

A

PROÆMIAL ESSAY,

WHEREIN, WITH SOME

Considerations touching EXPERIMENTAL ESSAYS
in general,

Is interwoven such an Introduction to all those written by the Author, as is necessary to be perused for the better understanding of them.

I KNOW not, *Pyropbilus*, whether what you will meet in the ensuing discourses will prove worthy of your taking notice of; yet I dare tell you, that if all my endeavours to serve you were not duties, I should think I might deserve your thanks for venturing to write them for your sake. For I am sufficiently sensible, both how unlearned I am, and in how learned an age I presume to write: nor has the great number of those escaped my observation, who finding it a much easier task to censure experimental compositions than to write such, endeavour to acquire the title of judicious, by condemning all things, that themselves have not written, or thought on. And indeed, *Pyropbilus*, I had, besides these, so many other discouraging considerations in my eye, whilst I was setting down the following essays, that I should scarce have prosecuted a design so full of trouble, and so unlikely by its success to make amends for it, if I had thought it free, for the securing of my own quiet and credit, to suppress observations which might prove serviceable to you, who having sufficiently conversed with books, are now desirous to begin to converse with things themselves. But I must confess, I look upon experimental truths as matters of so great concernment to mankind, that in spite of the just sense I have of my own disabilities, I am deterred from complying with those inclinations and motives that endear silence to me, by considering the fate of him, who though he had less entrusted to him than any of his companions, was yet severely punished for burying his single talent. And though, *Pyropbilus*, I could not without such reluctance resolve to write, yet I found it much more uneasy to resolve to write so soon. For I could not but consider, that being yet but very young, not only in years, but, what is much worse, in experience, I have yet much more need to learn, than ability to teach. And I considered too, that after a man is grown somewhat acquainted with things themselves, and has taken some general notice of the cognations, differences, and tendencies of their properties, he may every day so much improve his knowledge, that I am apt to think, that if God should be pleased to protract my life a few years longer, I shall at the end of them be able to look upon what I have hitherto written with pity, if not with blufhes. And I have often observed, that it is wont to happen in the productions of the mind, as in those of the body. For as those, that apply themselves to procreation too young,

and before they have attained to their full vigour and strength, do generally both hinder their own growth, and become the parents but of weak and short-lived children; so they, that too early, and before their judgment and experience be fully ripe, addit^t themselves to write books, do commonly both hinder their own proficiency in knowledge, and write but immature, and therefore seldom lasting treatises. Nor shoul^d I, *Pyrophilus*, have ever prevailed with myself to present you so early these discourses, since, by keeping them longer by me, I might easily by second thoughts, and fresh experiences, be enabled to correct and enrich them, did not the frequent and dangerous distempers, to which my very sickly constitution has of late rendered me too obnoxious, make me justly doubt, whether or no, if I should long forbear to write, death would not sooner come than the expected maturity of age and judgment. And though I had no such consideration to inove me to make hile to tender to you the ensuing discourses, yet this would suffice to engage me to present them you with all their present defects; that if I should keep them till I can make them les^t unworthy of you, I must keep them till you are grown past the need of them.

AND now that I have told you, *Pyrophilus*, both why I have written the ensuing discourses, and why I keep them not by me long enough to present them you with fewer imperfections; I suppose you will expect, that I should next tell you, why I have cast them into essays, rather than into any other form. To satisfy you about this particular, *Pyrophilus*, I must freely acknowledge to you, that it has long seemed to me none of the least impediments of the real advancement of true natural philosophy, that men have been so forward to write systems of it, and have thought themselves obliged either to be altogether silent, or not to write les^t than an entire body of physiology: for, from hence seem to have ensued not a few inconveniences.

AND first, when men, by having diligently studied either chymistry, anatomy, botanics, or some other particular part of physiology, or perhaps by having only read authors on those subjects, have thought themselves thereby qualified to publish compleat systems of natural philosophy, they have found themselves, by the nature of their undertaking, and the laws of method, engaged to write of several other things than those, wherein they had made themselves proficients; and thereby have been reduced, either idly to repeat what has been already, though perhaps but impertinently enough, written by others on the same subjects; or, else to say any thing on them rather than nothing, lest they should appear not to have said something to every part of the theme, which they had taken upon themselves to write of.

In the next place, the specious and promising titles and comprehensive method of these systems have been often found to persuade unwary readers, that all the parts of natural philosophy have been already sufficiently explicated; and, that consequently it were needless for them to put themselves to trouble and charges in making further inquiries into nature, since others having already sufficiently made it their busines to investigate and explicate physiological truths, our busines needs now be no more than to learn what they have taught, and thankfully to acquiesce in it.

Nor has the systematical way of writing been prejudicial only to the proficiency of some readers, but also to the reputation of some writers of systematical books. For it not unfrequently happens, that when a writer, to vent some few peculiar notions or discoveries of his own, pretences to write a whole body of philosophy, what is truly his own, though excellent in its kind, is either lost in the crowd of the things he has borrowed from others, and so comes to be over-looked, or at least not sufficiently taken notice of, by the reader; or else the unwelcome, and yet in such compotures

fearce evitable, repetition of many things, that others had, I know not how often, written before, occasions the laying aside of the whole book, as a rhapsody of trite and vulgar notions, scarce worth the perusing. And by this means the author often loses the reputation of his peculiar notions, as well as the reader the benefit of them; and that, which would have made an excellent and substantial essay, passes but for a dull and empty book.

BUT the worst inconvenience of all is yet to be mentioned; and that is, That whilst this vanity of thinking men obliged to write either systems or nothing is in request, many excellent notions or experiments are, by sober and modest men, suppressed; because such persons being forbidden by their judgment and integrity to teach more than they understand, or assert more than they can prove, are likewise forbidden by custom to publish their thoughts and obsevervations, unless they were numerous enough to swell into a system. And indeed it may be doubted, whether the sytematical writers have not kept the world from much more useful compourees than they have presented it with. For there are very few men, if any at all, in the world, that are enriched with a competent stock of experiments and and obsevervations to make out clearly and solidly, I say not all the phænomena of nature, but all those, that belong to chymistry, anatomy, or any such considerable subordinate doctrine of physiology. And those very men, that are diligent and judicious enough to study prospe-roufly any of those parts of physiology, are obliged to spend so much time in the accurate prosecution of that, and are wont to be thereby made so wary, and so thoroughly acquainted with the difficulty of physiological investigations, that they will least of all men be forward to write systems.

And what I say, *Pyrephilus*, of the inconveniences, that have hitherto been observed to flow from men's forwardness to write entire bodies of philosophy, may be, in its degree, applied to the practice of those, that pretend to give us compleat accounts of chymistry, or almost (I say almost) any other considerable and comprehensive part of natural philosophy: though I deny not, that in such attempts, which are much less difficult than the former, some men have done mankind considerable service, though they have not fully performed what the titles of their writings seem to promise. Nor am I so rigid as to be unwilling, that, from time to time, some very knowing writer should publish a system of physiology, or any part of it, according to the best authors and obsevervations of that time: for such a work may be useful, partly, for the instructing of youth in schools and academies; and partly, that men may have, from time to time, an inventory of what hath been already discovered; whereby the needless labour of seeking after known things may be prevented, and the progres of mankind, as to knowledge, might the better appear. But then it is to be wished, that such writings were not published by very intelligent persons, nor till some considerable improvement have been made in knowledge since the last work of that nature. Nor would I be thought to disallow such writings of very learned men, as though they may bear very general titles, yet are not published by their authors as compleat bodies or systems of physiology, but rather as general principles (almost like the hypotheses of astronomers), to affit men to explicate the already-known phenomena of nature. For of such kind of writings, if their authors be (as for the most part they are), subtle and inquisitive men, there may be very good use, not so much by their gratifying the intellect with the plausible account of some of nature's mysteries; as because on the one side their writers, to make good their new opinions, must either bring new experiments and obsevervations, or else must consider those, that are known already, after a new manner, and thereby make us take notice:

notice of something in them unheeded before ; and on the other side, the curiosity of readers, whether they like or disapprove the hypothesis proposed, is wont to be thereby excited to make trial of several things, which seeming to be consequences of this new doctrine, may, by their proving agreeable or repugnant to experiment, either establish or overthrow it.

And that you may know, *Pyropbilus*, what kind of writings I mean, I shall name to you the learned *Gaffendus* his little *Syntagma of Epicurus's philosophy*, and that most ingenious gentleman *Montfleur Descartes* his principles of philosophy. For though I purposelē refrained, though not altogether from transiently consulting about a few particulars, yet from seriously and orderly reading over those excellent (though disagreeing) books, or so much as Sir *Francis Bacon's Novum Organum*, that I might not be prepossessed with any theory or principles, till I had spent some time in trying what things themselves would incline me to think ; yet beginning now to allow myself to read those excellent books, I find by the little I have read in them already, that if I had read them before I began to write, I might have enriched the ensuing essays with divers truths, which they now want, and have explicated divers things much better than I fear I have done. But of such writers the number is yet (and will I fear always be) so small, that I shall not need to make many exceptions, when I treat of the usefulness of writing books of essays, in comparison of that of writing systematically : or, at least, *Pyropbilus*, whilst I presume not to judge of other men's abilities, I hope it may be lawful for me to confess freely to you concerning myself, that I am very sensible of my being far from having such a stock of experiments and observations, as I judge requisite to write systematically ; and I am apt to impute many of the deficiencies to be met with in the theories and reasonings of such great wits as *Aristotle*, *Campanella*, and some other celebrated philosophers, chiefly to this very thing, that they have too hastily, and either upon a few observations, or at least without a competent number of experiments, presumed to establish principles, and deliver axioms. For it very rarely otherwife happens, than that theories, that are grounded but upon few and obvious experiments, are subject to be contradicted by some such instances, as more free and diligent inquiries into what of nature is more abstruse, or even into the less obvious qualities of things, are wont to bring to light. I remember, that being once at *Leyden*, I was brought to the top of a tower, where, in a darkened room (such as is now used in many places to bring in the species of external object) a convex glafs, applied to the only hole, by which light was permitted to enter, did project upon a large white sheet of paper, held at a just distance from it, a lively representation of divers of the chief buildings in the town ; all which, upon the admission of more light into the room, by opening the window, did immediately disappear. And methinks, *Pyropbilus*, that in divers of the philosophical theories, that have been formerly applauded, something not unlike this may be easily observed. for though, whilst they are looked on with such a weak and determinate degree of light, they may appear very artificial and well-proportioned fabrics, yet they appear so but in that twilight, as it were, which is requisite to their conspicuosity. For if but a full light of new experiments and observations be freely let in upon them, the beauty of those (delightful, but fantastical) structures does immediately vanish.

And truly, *Pyropbilus*, if men could be persuaded to mind more the advancement of natural philosophy than that of their own reputations, it were not, methinks, very uneasy to make them sensible, that one of the considerablest services, that they could do mankind, were to set themselves diligently and industriously to make experiments and collect observations, without being over-forward to establish principles and axioms,

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believing it uneasy to erect such theories, as are capable to explicate all the phenomena of nature, before they have been able to take notice of the tenth part of those phenomena, that are to be explicated. Not that I at all disallow the use of reasoning upon experiments, or the endeavouring to discern as early as we can the confederations, and differences, and tendencies of things : for such an absolute suspension of the exercise of reasoning were exceeding troublesome, if not impossible. And, as in that rule of arithmetic, which is commonly called *regula falsi*, by proceeding upon a conjecturally-supposed number, as if it were that, which we inquire after, we are wont to come to the knowledge of the true number sought for ; so in physiology it is sometimes conducive to the discovery of truth, to permit the understanding to make an hypothesis, in order to the explication of this or that difficulty, that by examining how far the phenomena are, or are not, capable of being solved by that hypothesis, the understanding may, even by its own errors, be instructed. For it has been truly observed by a great philosopher, that truth does more easily emerge out of error than confusion. That then, that I wish for, as to systems, is this, that men, in the first place, would forbear to establish any theory, till they have consulted with (though not a fully competent number of experiments, such as may afford them all the phenomena to be explicated by that theory, yet) a considerable number of experiments, in proportion to the comprehensiveness of the theory to be erected on them. And, in the next place, I would have such kind of superstructures looked upon only as temporary ones ; which though they may be preferred before any others, as being the least imperfect, or, if you please, the best in their kind that we yet have, yet are they not entirely to be acquiesced in, as absolutely perfect, or incapable of improving alterations.

If were very possible, *Pyrephilus*, to let you see, that all that has been said to recommend to you that form of writing, which (in imitation of the French) we call essays, is but a part of what may be pertinently said to the same purpose. But because this introductory discourse itself is to be but an essay, not a book, I dare not long insist upon the advantages of this sort of discourses. Only because I think, that if I could engage you, *Pyrephilus*, and such other ingenious persons, to cast their physiological observations and reflections into experimental essays, I should thereby do real learning no trifling service, by bringing so useful a way of writing into the request it deserves ; upon this consideration, I say, I must beg leave to represent to you this great conveniency of essays, that as in them the reader needs not be clogged with tedious repetitions of what others have said already, so the writer, having for the most part the liberty to leave off when he pleases, is not obliged to take upon him to teach others what himself does not understand, nor to write of any thing but of what he thinks he can write well. And if such essays be but as they should be competently stocked with experiments, it is the reader's own fault, if he be not a learner by them : for indeed when a writer acquaints me only with his own thoughts or conjectures, without enriching his discourses with any real experiment or observation, if he be mistaken in his ratiocination, I am in some danger of erring with him, and at least am like to lose my time, without receiving any valuable compensation for that great loss : but if a writer endeavours, by delivering new and real observations or experiments, to credit his opinions, the case is much otherwise ; for let his opinions be never so false, his experiments bring true, I am not obliged to believe the former, and am left at liberty to benefit myself by the latter ; and though we have erroneously superstructured upon his experiments, yet the foundation being solid, a more wary builder may be very much furthered by it in the erection of more judicious and consistent

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sistent fabrics. Such a writer, if I be not wanting to myself, will certainly teach me useful truths, and if it be not my fault, he can lead me into no errors; and oftentimes the very experiments, that he delivers, besides that they may be applicable to many other purposes unthought of by him, may be either sufficient, or at least helpful to the very discovery of the erroneouſness of the opinions they are alleged to countenance: and I may account, that a man, that gives me, together with his conjectures (though erroneous) in matters of physiology, some noble experiment or observation, by which he pretends to verify them, does me no greater injury, than Galileo upon his first invention of the telescope would have done an astronomer, if he had told him, that he had discovered in heaven those imaginary new stars, which a late mathematician has fancied himself to have descried there, and at the same time had made him a present of an excellent telescope, with expectation thereby the receiver should be made of the giver's opinion; for by the help of his instrument the astronomer might not only make divers useful obſervations in the sky, and perhaps detect new lights there, but discern also his mistake, that gave it him.

AFTER what has been said, *Pyrrhius*, of the usefulness of experimental essays, we must proceed to lay something concerning the manner of writing them: but because you will also expect to receive some account of the ensuing discourses, I shall not treat of those two subjects a-part, but, in discoursing of the following essays, shall take occasion to acquaint you with part of my thoughts concerning such kind of compositions in general, the other considerations belonging to the same subject being already upon several occasions dispersed among, and to be met with in the ensuing discourses themselves.

AND first, as for the style of our experimental essays, I suppose you will readily find, that I have endeavoured to write rather in a philosophical than a rhetorical strain, as desiring, that my expressions should be rather clear and significant, than curiously adorned: for to a ſubject of the serious and important nature of physiology that saying may unquestionably be applied, *Ornari res ipsa negat, contenta doceri*. And certainly in these diſcourses, where our design is only to inform readers, not to delight or persuade them, perspicuity ought to be esteemed at least one of the best qualificatiōns of a ſtyle; and to affect needless rhetorical ornaments in ſetting down an experiment, or explicating ſomething abſtruse in nature, were little leſs improper, than it were (for him that designs not to look directly upon the ſun itſelf) to paint the eyeglasses of a telescope, whose clearneſs is their commendation, and in which even the moſt delightful colours cannot ſo much please the eye, as they would hinder the ſight. And that it may not be ſuſpected, that thoſe, that would not have it requisite to employ a florid ſtyle in treating of philosophical ſubjects, do but in their own excuse deny the neceſſity of ſuch rhetorical embellishments as they are not able to afford their compoſtures, give me leave to ſubjoin, that it was not an unpolished naturaliſt, but that prince of orators, *Cicerō* himſelf, who made this ſtudious declaration; *Omne (says he) quod de re bona dilucidē dicitur, preclarē mibi dici videtur: iſiūſmodi autem res velle ornācē dicere, pueri eſt: planū autem & perspicuū expedire posse, docti & intelligentis viſi.* But I muſt not ſuffer myſelf to ſlip unawares into the common place of the unfitness of too ſpruce a ſtyle for ſerious and weighty matters; and yet I approve not that dull and inſipid way of writing, which is praeticed by many chymists, even when they digreſs from physiological ſubjects. For though a philosopher need not be ſollicitous, that his ſtyle ſhould delight its reader with his floridneſs, yet I think he may very well be allowed to take a care, that it diſguſt not his reader by its flatneſs, especially when he does not ſo much deliver experiments or explicate them, as make reſlections

flections or discourses on them : for on such occasions he may be allowed the liberty of recreating his reader and himself, and manifesting, that he declined the ornaments of language, not out of necessity, but discretion, which forbids them to be used, where they may darken as well as adorn the subject they are applied to. Thus (to resume our former comparison) though it were foolish to colour or enamel upon the glasses of telescopes, yet to gild or otherwise embellish the tubes of them, may render them more acceptable to the users, without at all lessening the clearness of the object to be looked at through them.

AND as for exotic words and terms borrowed from other languages, though I expect, that persons not conversant in the philosophical compositions written (especially of late) in our language will be apt to suspect me for the willing author of divers new words and expressions ; yet as for you, *Pyrephilus*, who peruse other than moral, theological, and historical books in English, and find how much use is made in them of exotic terms, I hope you will find, that I have not at all affected them, but have rather studiously declined the use of those, which custom has not rendered familiar, unless it be to avoid the frequent and unwelcome repetition of the same word (so troublesome to the ear, and so much forbidden by orators) or for some peculiar significance of some such word, whose energy cannot be well expressed in our language, at least without a tedious circumlocution. And in such cases, *Pyrephilus*, I suppose a writer may be allowed to use exotic terms ; especially when custom has not only denominated them, but brought them into request. For, as in the fashions of clothes, though perhaps fools begin them, yet wise men, when they are once generally received, scruple not to follow them, because then obstinately to decline them would be as ridiculously singular as at first it would have been to begin them : so in exotic words, when custom has once made them familiar and esteemed, scrupulously to decline the use of them may be as well a fault, as needlessly to employ them : for it is not the use, but the affectation of them, that is unworthy a philosopher. And from the latter of those I hope I have kept myself far enough ; for I should think myself guilty of a very childish vanity, if the use I made of languages were so to write as to be the last understood. But besides the unintentional deficiencies of my style, I have knowingly and purposely transgressed the laws of oratory in one particular, namely, in making sometimes my periods or parentheses over-long : for when I could not within the compass of a regular period comprise what I thought requisite to be delivered at once, I chose rather to neglect the precepts of rhetoricians, than the mention of those things, which I thought pertinent to my subject, and useful to you, my reader. And for this fault, *Pyrephilus*, since I have made myself guilty of it but for your sake, I think I ought to obtain your pardon at least as easily as my own, since barely to keep you from losing any thing, that I conceived might be serviceable to you, I knowingly expose my style to be censured as disproportionate to itself.

THE next thing, *Pyrephilus*, of which I am to give you an account, is, why I have in the ensuing essays delivered many experiments and observations, which may seem slight and easie, and some of them obvious also, or else perhaps mentioned by others already. To satisfy you about this, I must inform you, that many of the particulars, which we are now considering, were in my first design collected in order to a continuation of the Lord *Verulam's* *Sylva Sylvarum*, or natural history. And that my intended centuries might resemble his, to which they were to be annexed, it was requisite, that such kind of experiments and observations, as we have been newly speaking of, should make up a considerable part of them. And indeed it were to be wished, that such inquisitive persons, as cannot be at the charge, or have not the opportunity,

portunity, of making new experiments, would busy themselves, as they have opportunity, in industriously collecting, and carefully setting down the phænomena to be met with without the assistance of new experiments, especially such particulars, as seem either to be of moment in order to the hinting or confirmation of some considerable truth, or to the detection of some applauded error, or else to have been, though obvious enough, yet little taken notice of. For I am confident, that very much may be done towards the improvement of philosophy by a due consideration of, and reflexion on, the obvious phænomena of nature, and those things, which are almost in every body's power to know, if he pleases but seriously to heed them; and I make account, that attention alone might quickly furnish us with one half of the history of nature, as well as industry is requisite, by new experiments, to enrich us with the other. And therefore I confess I think myself beholden to him, that first makes me take notice of what I might easily have known, but heeded not before: it not seldom happening, that we are prejudiced by, though we do not complain of that ignorance, from which we might relieve ourselves, if we did but diligently turn our eyes to the observations, wherewith even neighbouring and familiar objects would, if duly consulted, present us. But I digress; and therefore must step back into the way, and tell you, that the reasons, why I first designed the narrative of what I had tried and observed for a continuation of Sir Francis Bacon's Natural History, you will meet with in my preface to that specimen of the intended continuation, which I have given to those of my essays, that treat of promiscuous experiments: and the reason, why I have since declined that succinct way of writing, is for the sake of *Pyrephilus*, that I might have, in a more free and uncircumscribed way of discoursing, a greater liberty to insist on and manifest the reasonableness of such animadversions, as I thought seasonable for a person, who, though a great proficient in the other parts of philosophy, is but a beginner in experimental learning. And the second reason, why I have often made use of seemingly slight experiments, is, because such are more easily and cheaply tried, and they being alleged for the most part to prove some assertion, or credit some admonition, I thought their easiness or obviousness fitter to recommend them, than depreciate them; and I judged it somewhat unkind, or at least indiscreet, to refer you most commonly for proof of what I delivered, to such tedious, such difficult, or such intricate processes, as either you can scarce well make, unless you be already, what I desire my experiments should help to make you, a skilful chymist; or else are as difficult to be well judged, as the truth they should discover is to be discerned. I was also hopeful, that the easiness of divers things inviting you to make trial of them, and keeping you from being disappointed in your expectations, the succels of your first attempts would incourage you to make trial also of more nice and difficult experiments. And till you have tried them, do me the right to think, that I deal not unmercifully with you.

The reasons of my having divers times recorded experiments, which you may have formerly met with, and perchance even in printed books, I have elsewhere deduced in a peculiar discourse on that subject; and therefore shall now only add, that by reason of my being as yet a stranger to the German tongue, wherein the most and best chymical books are said to be written, I may have set down divers chymical experiments and observations, that are extant already in that Hermetical language, (if I may so call it) without having had them from their Dutch publishers, or so much as dreamed of their having been divulged by any man. I have likewise in my preface to the essays, that you will meet with under the title of *Promiscuous Experiments*, given you an account, why I have not refrained from mentioning divers things, which may seem

seem very slight, because very obvious : and I have long had thoughts to inform you in an intire discourse to be written on purpose, why I think, that even the trivial, and therefore slighted, truths of physiology ought not to be despised. And for my own part, I shall not scruple to confess to you, that I disdain not to take notice even of ludicrous experiments, and think, that the plays of boys may sometimes deserve to be the study of philosophers : for as when we go a hunting, though the flight of the hare and the pursuit of the dogs be to us but sport and recreation, yet the beasts themselves are extreamly earnest, the one to save his threatened life by flight, and the other to overtake his desired prey ; so nature acts very seriously in all the other things, that we make sports with, and is in very good earnest, whether we men be so or no.

PERHAPS you will wonder, *Pyropbilus*, that in almost every one of the following essays I should speak so doubtfully, and use so often, *perhaps, it seems, it is not improbable*, and such other expressions, as argue a diffidence of the truth of the opinions I incline to, and that I should be so shy of laying down principles, and sometimes of so much as venturing at explications. But I must freely confess to you, *Pyropbilus*, that having met with many things, of which I could give myself no one probable cause, and some things, of which several causes may be assigned so differing, as not to agree in any thing, unless in their being all of them probable enough ; I have often found such difficulties in searching into the cause and manner of things, and I am so sensible of my own disability to surmount those difficulties, that I dare speak confidently and positively of very few things, except of matters of fact. And when I venture to deliver any thing, by way of opinion, I should, if it were not for mere shame, speak yet more diffidently than I have been wont to do. It is not, that I at all condemn the practice of those inquisitive wits, that take upon them to explicate to us even the arbitrefest phenomena of nature ; for I am so far from censuring them, that I admire them, when their endeavours succeed, and applaud them even where they do but fairly attempt. But I think it is fit for a man to know his own abilities and weaknesses, and not to think himself obliged to imitate all that he thinks fit to praise. I know also, that the way to get reputation is to venture to explicate things, and promote opinions ; for by that course a writer shall be sure to be applauded by one sort of men, and be mentioned by many others ; whereas by the way of writing, to which I have condemned myself, I can hope for little better among the more daring and less considerate sort of men, should you shew them these papers, than to pass for a drudge of greater industry than reason, and fit for little more, than to collect experiments for more rational and philosophical heads to explicate and make use of. But I am content, provided experimental learning be really promoted, to contribute even in the least plausible way to the advancement of it ; and had rather not only be an under-builder, but even dig in the quarries for materials towards so useful a structure, as a solid body of natural philosophy, than not do something towards the erection of it. Nor have my thoughts been altogether idle and wanting to themselves, in framing notions, and attempting to devise hypotheses, which might avoid the deficiencies observed in other men's theories and explications : but I have hitherto, though not always, yet not unfrequently, found, that what pleased me for a while, as fairly comporting with the observations, on which such notions were grounded, was soon after disgraced by some further or new experiment, which at the time of the framing of those notions was unknown to me, or not consulted with. And indeed I have the less envied many (for I say not all) of those writers, who have taken upon them to deliver the causes of things, and explicate the mysteries of nature, since I

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have had opportunity to observe, how many of their doctrines, after having been for a while applauded, and even admired, have afterwards been confuted by the discovery of some new phenomenon in nature, which was either unknown to such writers, or not sufficiently considered by them. For I have found it happen, as well to many others (that have published their opinions) as to me (who have been more private in my guesses) in our theories built on either too obvious or too few experiments, what is wont to happen to the falsifiers of coin; for as counterfeit pieces of money will endure some of them one proof, as the touch-stone, others another, as aqua fortis, some a third, as the hammer or the scales, but none of them will endure all proofs; so the notions I mention (in which sort I fear too great a part of those hitherto extant may be comprised) may agree very fairly with this or that or the other experiment; but being made too hastily, and without consulting a competent number of them, it is to be feared, that there may still after a while be found one or other, (if not many) their inconstancy with which will betray and discredit them.

I HAVE notwithstanding all this, on some occasions, adventured to deliver my opinion; not that I am very confident of being less subject to err in those particulars, than in any of the others, wherein I have refrained from interposing any conjecture; but because I would manifest to you, that I scruple not to run the same venture with those incomparably better naturalists, that have thought it no disgrace in difficult matters rather to hazard the being sometimes mistaken, than not to afford inquisitive persons their best assistance towards the discovery of truth.

AND because, *Pythagoras*, in the reasons and explications I offer of natural effects, I have not for the most part an immediate recourse to the magnitude, figure, and motion of atoms, or of the least particles of bodies, I hold it not unfit to give you here some account of this practice; not so much for the sake of those few passages in my essays, that may be concerned in it, as for that of many learned men, especially physicians, whose useful writings being to be undervalued, and are in danger to be despised, by an opinion taken up from the mis-understood doctrine of some eminent Atomists, as if no speculations in natural philosophy could be rational, wherein any other causes of things are assigned than atoms and their properties. I consider then, that generally speaking, to render a reason of an effect or phænomenon, is to deduce it from something else in nature more known than itself; and that consequently there may be divers kinds of degrees of explication of the same thing. For although such explications be the most satisfactory to the understanding, wherein it is shewn, how the effect is produced by their more primitive and catholick affections of matter, namely, bulk, shape and motion; yet are not these explications to be despised, wherein particular effects are deduced from the more obvious and familiar qualities or states of bodies, such as heat, cold, weight, fluidity, hardness, fermentation, &c. though these themselves do probably depend upon those three universal ones formerly named. For in the search after natural causes, every new measure of discovery does both instruct and gratify the understanding; though I readily confess, that the nearer the discovered causes are to those, that are highest in the scale or series of causes, the more is the intellect both gratified and instructed.

I THINK it therefore very fit and highly useful, that some speculative wits, well versed in mathematical principles and mechanical contrivances, should employ themselves in deducing the chiefest modes or qualities of matter, such as are heat, cold, &c. and the states or conditions of it (if we think fit to distinguish these from its qualities) as fluid, firm, brittle, flexible, and the like, from the above-mentioned most primitive and simple affections thereof. And, I think, the commonwealth of learning

ing exceedingly beholden to those heroick wits, that do so much as plausibly perform something in this kind. But I think too, we are not to despise all those accounts of particular effects, which are not immediately deduced from those primitive affections of either atoms, or the insensible particles of matter; but from the familiar, though not so univeral, qualities of things, as cold, heat, weight, hardnes, and the like. And perhaps it would be none of the least advantages, which would accrue to Naturallists from a satisfactory explication of such qualities by the most primitive and simple ones, that it would much shorten the explication of particular phenomena. For though there be many things in nature, that may be readily enough made out by the size, motion, and figure of the small parts of matter; yet there are many more, that cannot be well explained without a great deal of discourse, and divers successive deductions of one thing from another, if the purposed effect must be deduced from such primary and univeral causes; whereas if we be allowed to take the notions of cold, heat, and the like qualities for granted, the explications and proofs may be much more compendiously made. He gives some reason, why stones and iron, and all other heavy bodies, will swim in quicksilver, except gold, which will sink in it; that teaches, that all those other bodies are *in specie* (as they speak) or bulk for bulk, lighter than quicksilver, whereas gold is heavier. He, I say, may be allowed to have rendered a reason of a thing proposed, that thus refers the phænomenon to that known affection of almost all bodies here below, which we call gravity, though he does not deduce the phenomenon from atoms, nor give us the cause of gravity; as indeed scarce any philosopher has yet given us a satisfactory account of it. So if it be demanded, why, if the sides of a blown bladder be somewhat squeezed betwixt one's hands, they will, upon the removal of that which compressed them, fly out again, and restore the blader to its former figure and dimensions; it is not saying nothing to the purpose, to say, that this happens from the spring of those aerial particles, wherewith the bladder is filled, though he, that says this, be not perhaps able to declare, whence proceeds the motion of restitution, either in a particle of compressed air, or any other bent spring.

AND as for the reasons of things assigned by physicians, they must be by most of them despised, unleis we will allow of such explications, as deduce not things from atoms or their affections, but only either from seconday qualities, or from the more particular properties of mixt bodies. If a physician be asked, why rhubarb does commonly cure loosenesses, he will probably tell you as a reason, that rhubarb is available in such diseases, because it hath both a laxative virtue, whereby it evacuates choler, and such other bad humours as are wont, in such cases, to be the peccant matter; and an astringent quality, whereby it afterwards arrests the flux. But, if you further ask him^r the reason, why rhubarb purges, and why it purges choler more than any other humour; it is ten to one he will not be able to give you a satisfactory answer. And indeed, not only the manner, whereby purgative medicines work, but those other properties, whereby some bodies are diuretick, others fodi-ristick, others farcotick, &c. are not, I fear, so easy to be intelligibly made out as men imagine, and yet a skilful physician would justly think himself wronged, if the reasons he renders of things in his own profession were denied the name of reasons, because made without recourse to atomical principles. And indeed, there are often-times so many subordinate causes between particular effects and the most general causes of things, that there is left a large field, wherein to exercise men's industry and reason, if they will but solidly enough deduce the properties of things from more general and familiar qualities, and also intermediate causes (if I may so call them) from

from one another. And I am the more backward to despise such kind of reasons, because I elsewhere declare, that there are some (for I do not say, many) things, as particularly the origin of local motion, of which, even by the atomical doctrine, no physical cause can well be rendered; since either such things must be ascribed to God, who is indeed the true, but supernatural cause of them; or else it must be said (as it was by the old Epicurians) that they did ever belong to matter: which, considering that the notion of matter may be compleat without them, is not to give a physical efficient cause of things in question, but in effect to confess that they have no such causes. But of this elsewhere more.

In the mean time, that you may not be drawn away to undervalue such writers as I have been pleading for, nor think you ought to refrain from writing what occurs to you, though true and useful, unless you deduce it, or at least can do so, from the Epicurian notions; I shall here briefly represent to you (what perhaps you will not hereafter think a despicable suggestion) that there are two very distinct ends, that men may propound to themselves in studying natural philosophy. For some men care only to know nature, others desire to command her; or, to express it otherwise, some there are, who desire but to please themselves by the discovery of the causes of the known phenomena; and others would be able to produce new ones, and bring nature to be serviceable to their particular ends, whether of health, or riches, or sensual delight. Now as I shall not deny, but that the atomical, the Cartesian, or some such principles, are likely to afford the most of satisfaction to those speculative wits, that aim but at the knowledge of causes; so I think, that the other sort of men may very delightfully and successfully prosecute their ends, by collecting and making variety of experiments and observations; since thereby learning the qualities and properties of those particular bodies they desire to make use of, and observing the power that divers chymical operations, and other ways of handling matter, have of altering such bodies, and varying their effects upon one another, they may, by the help of attention and industry, be able to do many things, some of them very strange, and more of them very useful in human life. When a gunner or a soldier employs gunpowder, it is not necessary, that he should consider, or so much as know, of what, and of how many ingredients (much less of what kind of atoms) it is made, and the proportion and manner, wherein they are mingled; but the notice experience gives him of the power of that admirable concrete, as it is made up and brought to his hands, suffices to enable him to perform things with it, than nothing but their being common and unheeded can keep from being admired. The physician, that has observed the medicinal virtues of treacle, without knowing so much as the names, much less the nature of each of the sixty and odd ingredients, whereof it is compounded, may cure many patients with it. And though it must not be denied, that it is an advantage as well as a satisfaction, to know in general, how the qualities of things are deducible from the primitive affections of the smallest parts of matter; yet whether we know that or no, if we know the qualities of this or that body they compose, and how it is disposed to work upon other bodies, or be brought on by them, we may, without ascending to the top in the series of causes, perform things of great moment; and such, as without the diligent examination of particular bodies, would, I fear, never have been found out *a priori*, even by the most profound contemplators. We see that the artificers, that never dreamed of the Epicurian philosophy, have accommodated mankind with a multitude of useful inventions. And Paracelsus, who (besides that he seems none of the most piercing and speculative wits) sure had little recourse to atomical notions, if he ever so much as heard of them, was able

able to perform some things, that were truly admirable, besides those he vainly boasted of; as may appear not only by what I elsewhere represent, but by what *Oporinus* himself (as severly as he otherwise writes against his deserted master) confesses he law of the stupendous cures, which *Paracelsus* wrought with his famous *Laudanum* (whatever he made it of). But we need not go far to find a noble example to our present purpose, since we see, that the bare making of trials with the loadstone, and irons touched by it, though the experimenters were ignorant (as some fear we yet are) of the true and first causes of magnetical phenomena, have produced inventions of greater use to mankind, than were ever made by *Leucippus*, or *Epicurus*, or *Aristotle*, or *Telius*, or *Campanella*, or perhaps any of the speculative devisers of new hypotheses; whose contemplations aiming for the most part but at the solving, not the increasing or applying, of the phenomena of nature, it is no wonder they have been more ingenuous than fruitful, and have hitherto more delighted than otherwise benefited mankind: I say hitherto, because though experience warrants me so to speak now, yet I am not unwilling to think, that hereafter, and perhaps in no long time, when physiological theories shall be better established, and built upon a more competent number of particulars, the deductions, that may be made from them, may free them from all imputation of barrennes. But of these things I otherwise (though not, as I remember, in any of the following essays) more fully discourse.

And therefore I shall now resume the subject, that occasioned this long excursion, and add to what I said in excuse of my venturing sometimes to deliver something as my opinion in difficult or controverted cases, that I must declare to you, *Pyropilus*, that as I desire not my opinions should have more weight with you, than the proofs brought to countenance them will give them; so you must not expect, that I should think myself obliged to adhere to them any longer, than those considerations, that first made me embrace them, shall seem of greater moment, than any that I can meet with in opposition to them. For *Aristotle* spoke like a philosopher, when to justify his dissent from his master *Plato*, he said, among other things, that for the sake of truth, men (especially being philosophers) ought to overthrow even their own tenets (*Δέιγες δὲ ἀντίθεσιν, οὐ διὸ τοι ευηγγείλης μη τῆς αἰνάπει, καὶ τὰ εἰκάσια αἴρεις, ἀλλας εἰθικόν φιλοσόφους θύεται.*) And though for a man to change his opinions, without seeing more reason to forsake them than he had to assent to them, be a censurable levity and inconstancy of mind; yet to adhere to whatever he once took for truth, though by accession of more light he discover it to be erroneous, is but a proud obstinacy, very injurious to truth, and very ill becoming the sense we ought to have of human frailties. And it ought to be esteemed much less disgraceful to quit an error for a truth, than to be guilty of the vanity and perverseness of believing a thing still, because we once believed it. And certainly, till a man is sure he is infallible, it is not fit for him to be unalterable.

You will easily discern, *Pyrophilus*, that I have purposely, in the ensuing essays, refrained from swelling my discourses with solemn and elaborate confutations of other men's opinions, unless it be in some very few cases, where I judged, that they might prove great impediments to the advancement of experimental learning; and even such opinions I have been wary of meddling with, unless I supposed I could bring experimental objections against them. For it is none of my delight to engage myself with, or against, any one sect of Naturalists, but barely to invite you to embrace or refuse opinions, as they are consonant to experiments, or clear reasons deduced thence, or at least analogous thereto; without thinking it yet seasonable to contend very earnestly for those other opinions, which seem not yet determinable by such experiments.

Some CONSIDERATIONS touching

ments or reasons. And indeed, to allude to our former comparison, I would endeavour to destroy those curious but groundless structures, that men have built up of opinions alone, by the same way (and with as little noise) by which such fantastical structures, as thole I mentioned to have seen at Leyden, may be demolished. To destroy which, it were needless to bring battering engines, since nothing is requisite to that effect but an increase of light. And experience has shown us, that divers very plausible and radicated opinions, such as that of the unhabitableness of the torrid zone, of the solidity of the celestial part of the world, of the blood's being conveyed from the heart by the veins (not the arteries) to the outward parts of the body, are generally grown out of request, upon the appearing of those new discoveries, with which they are inconsistent, and would have been abandoned by the generality of judicious persons, though no man had made it his busines, purposely, to write confutations of them: so true is that vulgar saying, that *Reputum est index sui & obliqui.*

But when at any time, *Pyropilus*, I have been induced to oppose others, as I have not denied myself the freedom, that is requisite unto loyalty to truth; so I have endeavoured to use that moderation, and civility, that is due to the persons of deserving men. And therefore you shall find me not only in one essay oppose an author, whom in another I applaud; but sometimes you may find me commanding a writer in the very same page, perhaps, where I am endeavouring to disprove his opinions: for I love to speak of persons with civility, though of things with freedom. Nor do I think it reasonable, either that any man's reputation should protect his errors, or that the truth should fare the worse for his sake, that delivers it. And as for the (very much too common) practice of many, who write, as if they thought railing at a man's person, or wrangling about his words, necessary to the confutation of his opinions; besides that I think such a quarrelsome and injurious way of writing does very much misbecome both a philosopher and a Christian, methinks it is as unwise, as it is provoking. For if I civilly endeavour to reason a man out of his opinions, I make myself but one work to do, namely, to convince his understanding; but, if in a bitter or exasperating way I oppose his errors, I increase the difficulties I would surmount, and have as well his affections against me as his judgment: and it is very uneasy to make a proselyte of him, that is not only a dissenter from us, but an enemy to us. And that which makes me the more dislike the bitter way of disputing, which I am reprehending, is, that I have often observed, that though one of the disputants alone be at first in the fault, yet the other is most commonly drawn to share in the guilt, though, to contract it, he must imitate his adversary. For as a mad dog, by biting others, is wont to make thole he bites run mad like himself, and do promiscuous mischief; so these so provoking writers are wont to enrage thole they offend, and infect them also with their own virulent distemper. But, *Pyropilus*, when I speak of dealing respectfully with those I dissent from, I would be understood of such, as have well deserved of experimental learning, or at least been candid and sober inquirers after truth. For, as I think, that it would much discourage any prudent perfon from venturing to communicate what he thinks he knows to the world, to find, that an error proceeding from human weakness, or the darkness and arbitrariness of things, seldom escapes being detected without being made matter of disgrace or reproach to the author: so, on the other side, when vain writers, to get themselves a name, have presumed to intrude upon the credulous world such things, under the notion of experimental truths, or even great mysteries, as neither themselves ever took the pains to make trial of, nor received from any credible persons that professed themselves to have tried them; in such cases, I see not how we are obliged to treat writers, that took no

pains

pains to keep themselves from mistaking or deceiving, nay, that cared not how they abuse us to win themselves a name, with the same respect, that we owe to those, who, though they have missed of the truth, believed they had found it, and both intended to deliver it, and took some (though not prosperous) pains, that they might convey nothing else to us.

I FEAR it will be requisite, *Pyrophilus*, to tell you, why in some of the following essays you will meet with many passages transcribed out of other authors, and in some very few or none at all. And therefore to give you first a short account of the particular mentioned last, I must mind you, that it was most suitable both to my humour and design, to deliver only those things, wherewith my own observations, or trials, or thoughts, had furnished me, without troubling you with the repetition of those things, which had been delivered by others already; those kind of repetitions, unless they be made upon some such grounds as we shall presently mention, seeming to me to be as vainly as ambitiously affected by many writers, and being deservedly troublesome to judicious readers, who can easily discern, that they add much more to the bulk of books than of knowledge. But this notwithstanding, *Pyrophilus*, I thought myself obliged on some occasions, for your sake, to overcome my natural aversion to stuff any writings of mine with passages transcribed from those of others, partly for the reasons elsewhere insisted on, and partly for divers others. As first, because some particulars are of that strangeness, and of that moment, that they need and deserve to be verified by more than a single attestation. Next, because according to the Greek proverb, *καὶ τὰ κακά*, it is not properly to say over the same thing again, when the observation, experiment, or other passage of an author, is either illustrated, or otherwise improved by the repetition, or else applied to some purpose differing from that, to which the author brought it: that being applicable to many a single experiment or observation, which Seneca somewhere says, *Nulla res consummata est dum incipit*; and, *Etiam omnia à veteribus inventa sunt, hoc semper novum erit, usus, & inventorum ab aliis scientia dispositio*. And thirdly, because as the planets and stars have (according to astrologers) in their great synods or conjunctions, much more powerful, and sometimes other influences on the air and some other sublunar parts of the world, than are ascribed to one or two of them out of that aspect; so divers particulars, which whilst they lay single and scattered among the writings of several authors were incon siderable, when they come to be laid together in order to the same design, may oftentimes prove highly useful to physiology in their conjunction, wherein one of them may serve to prove one part or circumstance of an important truth, and another to explicate another, and so all of them may conspire together to verify that saying, *Et que non profunt singula, multa juvent*. It may then, I hope, suffice to justify me on this occasion, that not appealing to other writers as to judges, but as to witnesses, nor employing what I have found already published by them barely as ornaments to embellish my writings, and much less as oracles by their authority to demonstrate my opinions, but as certificates to attest matters of fact, I may hope, that their testimonies will as well be illustrated by mine, as mine by their's, and that all of them may contribute to your better information.

AND if, *Pyrophilus*, you grant, that upon these considerations I have not done amiss to apply to my purpose divers of those things, which I found delivered pertinently to them by those writers, which I chanced to cast mine eyes on, I suppose you will not think I need to make you an apology, for my having made most use of the passages of those writers, which I suppose will be most difficult to be met with (such as are many books of navigation and other voyages) and especially of French books not

yet translated into English or Latin. And I think I shall less need to make an excuse for my having for the most part set down the passages I recited in the author's own words, that being one of the readiest ways I know to satisfy the reader, and avoid injuring the writer. And indeed, I have met with abundance of quotations, wherein the transcriber doth so mistake, or so misrepresent the cited author's meaning, sometimes out of inadvertence, but sometimes too I fear out of indulgence to his own hypothesis, that if ever I should be tempted to trouble the world with any of my thoughts, I would beseech my readers, not to look upon any thing as my opinion or assertion, that is not delivered in the entire series of my own words; lest a transcriber should make me deliver those things resolutely and dogmatically, which I deliver but hesitantly and conjecturally; and lest I should seem to set down those things positively as processes, for whose success I undertake, which I record but by way of narrative.

For my so frequently mentioning what I have borrowed from other writers, or received from my friends, I expect to be excused by that of *Pliny, Benignum est (ut arbitror) & plenum ingenui pudoris, confiteri per quos proficeris.* Though I have seen divers modern writers, that so boldly usurp the observations and experiments of others, that I might justly apply to them what the same *Pliny* annexes; *Scio enim, conferentes autores me deprehendisse à juratissimis & proximis veteres transcriptos ad verbum neque nematos, &c.* If other writers should not prove more equitable (for I will not say more thankful) than such as these, they would quickly discourage those, whose aims are not very noble and sincere, from gratifying the public with inventions, whose praise and thanks would be usurped by such as will not name them. But perhaps they would be more just, if they reflected on what our author adds, *Obnoxii profecto animi & infelicitis ingenii est, deprehendi in furto malle, quam mutuum reddere, cum præfertim fons fet ex usura.*

And now I have said this concerning the passages I have borrowed from other authors, it will not be improper to add something about those I have declined to borrow. For you may possibly marvel, that in divers of the historical parts of my writings I have omitted such testimonies either of *Pliny, Solinus, Aristotle, Theophrastus, Alcian,* or, perchance, some of the antient physicians themselves (who yet, as more converfant with things, are usually more credible) as seem very pertinent to my discourse, and fit to prove what I design. But when I shall come to entertain you about natural history, I doubt not but to satisfy you with the reasons I shall offer you of this practice. In the mean time, I shall only tell you in short, that though I have a just respect for those great names I have mentioned; yet the sense I have of the difficulties I have found to make and relate an observation accurately and faithfully enough for a naturalist to rely on; and the occasions I have had of looking into divers matters of fact delivered in their writings, with a bold and impartial curioſity; have made me conclude so many of those traditions to be either certainly false, or not certainly true, that except what they deliver upon their own particular knowledge, or with peculiar circumstances, that may recommend them to my belief, I am very shy of building any thing of moment upon foundations, that I esteem so unsure, and much less upon the suspected passages, that *Wecker, Paracelſus, Porta, &c.* abound with. And therefore (though I well enough know, how much I impoverish my discourses by this niceſſe) yet I do not think it fair to imploy that as an argument to convince you, that has not that operation upon me myſelf. And I rather take notice of my forbearing to make uſe of the historical traditions and chymical or magical ſecrets, that I meet in the above-mentioned authors, or any other makers of collections, unleſs the narrative

narrative be (as I was saying) expressly enough delivered upon the writer's personal knowledge, or that of some other credible witness; not only because I would give you an account, why several of my writings are unfurnished with what most readers look on as the richest ornaments of other men's; but because if this wariness could be introduced, it would be the most effectual way of persuading men to write those kind of tract[s] I would recommend, physiological essays. For he, that will confine himself so strictly, will scarce be often tempted, on physical subjects, to write either systems or volumes.

ANOTHER thing, *Pyropbilus*, I must needs advertise you of in reference to the ensuing discourses; which is, that besides those deficiencies in point of ratiocination, which are due to my personal disabilities, I have purposely let pass some few (and but very few) inferences, which I discerned well enough not to be cogent, because I was willing to acquaint you upon some particular occasions with all the experiments then occurring to me, which I thought might contribute to the illustration of the subject in hand; though each of them apart did not irrefragably, nor indeed so much as strongly infer the conclusion, in order to which they seemed to have been mentioned as premisses. And this practice I made the less scruple of, because I was willing to exercise thereby your reasoning faculty, and try how far you would discern the tendency of several things, all of them pertinent enough to the subject in hand, but not all of them concluding to the main design, in order whereunto they were alleged. And I supposed, that the things by me mentioned, though not conclusive, being yet experimental, the mention of them, which in a strictly logical way of reasoning must have been forbore, might well make you amends for the exercise, to which I intended they should put your reason.

THERE remains yet one thing, *Pyropbilus*, of which I suppose you will expect I should give you an account; and that is, why in the ensuing essays I have mentioned divers experiments, which I have not plainly and circumstantially enough delivered. To satisfy you concerning which, I must represent to you, first, that though for your sake I have oftentimes, contrary to my reason and genius, delivered things, to make them more clear, in such a multitude of words, that I now seem even to myself to have in divers places been guilty of verbiage; yet in some other passages, treating of things, which use had rendered very familiar to me, I may have, to shun prolixity, unawares slip into the contrary extreme. Secondly, there are some mechanical experiments, wherein I have purposely omitted some manual circumstances, because I was unwilling to prejudice some ingenious tradesmen, who inake either a livelihood, or at least a gain, by the sale of the productions of such experiments. And I made the less scruple to conceal such mechanical circumstances (if I may so call them) because they were not necessary to the phisiological knowledge of the experiments; in naming of which, my intention was to teach you rather philosophy than trades. Thirdly, I mention some things but darkly, either because I received them upon condition of secrecy, or because some ingenious persons, that communicated them to me, or others to whom I imparted them, do yet make, and need to make, a pecuniary advantage of them. Fourthly, and some things, that, either having been the fruits of my own labours, or obtained in exchange of such, are freely at my own disposal, I have not yet thought fit so plainly to reveal, not out of any envious design of having them buried with me, but that I may be always provided with some rarity to barter with those secretists, that will not part with one secret but in exchange for another, and think nothing worth their desiring, that is known already to above one or two persons. And I think it very lawful to reserve always some concealed experiments

riments by me, wherewith to obtain the secrets of others, which being thereby gained, the other (as being no longer necessary to the former end) may freely be communicated.

AND think not, *Pyrophilus*, that the bare mention of an experiment as having been performed, though the way of making it be concealed, is of no use, if the relator of the experiment be a person, that may safely be credited: for it is something, to be assured, that such and such things have been really performed, and consequently are possible to be done, though we be not particularly acquainted with the means of performing them. And he tells you something, that tells you upon his own knowledge, that in such or such bodies, or ways of operating on them, considerable things of such or such a nature are to be met with. And for my part, when I go a hawking or setting, I think myself beholden to him, that assures me, that in such a field there is a covey of partridges, though he does no more towards the giving me them. And it is obvious, how much *Europe* is beholden to *Columbus* for the detection of many countries in *America*, which were not discovered by him, nor perhaps till long after his death, because he first informed us knowingly, that there were unknown regions beyond that vast ocean, which severs the old world from the new. But I begin to digress, and therefore shall proceed to tell you, that I am the less troubled at my omission of the circumstantial parts of some experiments, because I think it will be much for your advantage to try them over again yourself. And as I have taken care by the truth of the experiments I have delivered, to secure your success, in case you try them aright; so I cannot be very sorry, that you should in some particulars have a kind of necessity laid on you to exercise your own industry, and thereby increase your experience.

BUT besides all that has been said, *Pyrophilus*, I must freely confess to you, that there is one thing particularly relating to yourself, which has made me refrain from delivering, in the ensuing essays, some of the chief chymical processes, wherewith they might have been enriched. For not yet knowing with what seriousness you will additc yourself to promote experimental philosophy, nor what use you will make of what has been unveiledly communicated to you, I was somewhat unwilling, that some things, which had cost me a great deal of pains, should yet fall into any man's hands, that scorns to purchase knowledge with some pains; and I was desirous, in case you shall prove as industrious as I hope you will, to have something by me to encourage and cherish your industry, which may be more suitable to your improved knowledge. For I must confess to you, that in reference to the chymical processes extant in the following discourses, I look upon most of them but as trifles, not only in comparison of those things, which a knowing chymist might have delivered on the same subjects, but even in regard of divers processes (not impertinent to those discourses) wherewith I myself (as little as I am a pretender in these matters) am not unacquainted: and perhaps I would have given to the following treatises the title of trifles, instead of that of essays, if I had not been afraid of discouraging you thereby, and if the chymical part of them had been the chief thing, wherewith I intended to acquaint you in them. But if the reception you give to what we have already written, prove such as may encourage us to proceed, we may perhaps, if God be pleased to vouchsafe us life and opportunity, be invited to impart to you those more considerable chymical experiments, which either the communication of our friends, or our own labours, have presented us. For it will be much in the power of the entertainment, which these papers shall meet with, to make them either the beginning of our labours of this nature, or the end. And in the mean time, I think I may venture to tell you, that,

as inconsiderable as I have confessed divers of the chymical processes mentioned in the essays to be, yet if ever you take the pains (as I hope you will) to write experimental essays, and confine yourself to take as little upon trust as I have done, you will perhaps be ready to believe, that sometimes a short essay of this nature, not to say some one single experiment, may have cost me more than a whole treatise written on such a subject, whereon to be able without discredit to write books, it is almost sufficient to have read many. And give me leave to add, that as in such kind of compositions, oftentimes the enabling himself to give a considerable advertisement, or even hint, may cost the writer more than the making of several experiments; so it may be also more beneficial to the reader than the knowledge of them. For we must not always measure the considerableness of things by their most obvious and immediate usefulness, but by their fitness to make or contribute to the discovery of things highly useful. As, if it be true, what is reported by good authors of the hazel wand, or *virgula divinatoria*, though the hazel tree be much less considerable in reference to its fruit, or immediate productions, than a peach-tree, an orange-tree, or even an apple-tree; yet may it be made much more valuable than any of them, because, whereas they only present us with fruits, this may assist us to discover in latent mines inestimable treasures.

I had almost forgot to advertise you, *Pyropbilus*, that whereas I have not been so solicitous as most writers are wont to be, to swell the ensuing essays with the enumeration of the various opinions and arguments of authors about the subject I treat of, or to adorn them with acute sentences, fine expressions, or other embellishments borrowed from eminent writers; it has not been, because I utterly dislike the making use of those passages in classic or other authors, that may either give (among the admirers of those writers) some authority to our thoughts, or very handsomely and emphatically express them. For I remember, I have known it reprehended by learned men in *Epicurus*, that though he wrote very much himself, he would not vouchsafe in his writings to quote those of other men. And that I have not refrained from making use, now and then, of those philological ornaments of discourse, when they readily occurred to me, and appeared neither impertinent nor prolix, may, I hope, suffice to keep me from being suspected of the vanity of thinking myself above other men's assistance. But the reasons of my so much declining to make use of other men's authority, or expressions, were chiefly these: first, That the weaknes of my eyes has this long time kept me from reading almost any books, save the Scripture, with some critical expositions of it, and, here and there, some portions of the writings of those that pretend to teach their readers experimental matters: and the unfaithfulness of my memory, as to things of no great moment, has made me forget almost all the little philological and florid learning I was formerly acquainted with. And really, *Pyropbilus*, as for the books, that treat of natural philosophy, I am so sensible of the smallness of the advantage, which my disabilities have suffered me to make of them, that instead of being ambitious to appear a great reader, I could be very well content to be thought to have scarce looked upon any other book than that of nature. And in the next place, *Pyropbilus*, though I ignore not, that by this plain and unadorned way of writing, I unkindly deny my essays many imbellishments, which I might give them, and which perhaps you will think they do abundantly need; yet my frequent distempers, journeys, and other avocations, not allowing me so much time as I desired, to entertain you on philosophical subjects, I thought it more requisite to spend those confined hours in acquainting you with my own thoughts, such as they are, than with those of other men; unless (as I formerly intimated) I can some way or other more:

more than barely recite what I recite of theirs. And you will easily pardon me the injury, which for your sake I do my own reputation by this naked way of writing, if you, as well as I, think those the most profitable writers, or, at least, the kindest to their perusers, who take not so much care to appear knowing men themselves, as to make their readers such.

T W O E S S A Y S,

Concerning the

Unsuccessfulness of EXPERIMENTS,

Containing divers Admonitions and Observations (chiefly Chymical) touching that SUBJECT.

Advertisement about the two following Essays.

THE author of these discourses had enlarged them in this second edition, with divers observations and experiments, but that he has made use of them already in other papers belonging to his Sceptical or Doubting Naturalist.

The First ESSAY,

Of the Unsuccessfulness of EXPERIMENTS.

I AM very sorry, *Pyropilus*, that to the many (elsewhere enumerated) difficulties which you may meet with, and must therefore surmount, in the serious and effectual prosecution of experimental philosophy, I must add one discouragement more, which will perhaps as much surprize you as dishearten you; and it is, that besides that you will find (as we elsewhere mention) many of the experiments published by authors, or related to you by the persons you converse with, false or unsuccessful (besides this, I say) you will meet with several observations and experiments, which, though communicated for true by candid authors or undistrusted eye witnesses, or perhaps recommended to you by your own experience, may upon further trial, disappoint

appoint your expectation, either not at all succeeding constantly, or at least varying much from what you expected.

This advertisement may seem of so discouraging a nature, that I should much scruple the giving it you, but that I suppose the trouble at that unsuccessfulness, which you may meet with in experiments, may be somewhat lessened by your being forewarned of such contingencies : and that if you should have the luck to make an experiment once, without being able to perform the same thing again, you might be apt to look upon such disappointments as the effects of an unfriendliness in nature or fortune to your particular attempts, as proceed but from a secret contingency incident to some experiments, by whomsoever they be tried.

But because, *Pyropbilus*, the advertisement, which I am about to give you, may seem, as paradoxical, as discouraging : it will be but reasonable, that I present you with some instances of the requisiteness of it : which I shall the more willingly do, because thereby I may not only evince the truth of it, but somewhat lessen the dependency it is apt to produce, by letting you see, that though some of your experiments should not always prove constant, you have divers partners in that infelicity, who have not been disengaged by it.

To make nice and curious distinctions of the several grounds and occasions of the unsuccessfulness of experiments, would, perhaps, prove a work of greater difficulty than use ; and therefore I shall content myself grotly to distinguish the causes of that unsuccessfulness, into the particular or mistaken properties of the materials employed about them, and some such error committed in the handling of these materials, as though it hinder the success of the experiment, is not easy to be discerned. Which therefore I mention, that I may distinguish these kind of errors, that I am now to consider, from those more obvious ones, which proceeding barely from the unskillfulness of the tryers of the experiments, may be easily enough discerned, and either rectified or avoided by a knowing artist, or a person well versed and expert in making those particular experiments, which through that unskillfulness may have mis-carried.

The materials to be employed about the experiments we are considering may also admit of several distinctions ; as into natural and fictitious, sincere and adulterate, simple and compound, &c. But we shall likewise purposely forbear the insisting on, any of these, and content ourselves to cast what we have to say on this part of our theme, into a few and comprehensive observations.

And in this first place we will observe, that divers experiments succeeded not, because they were at one time tried with genuine materials, and at another time with sophisticated ones : and in this case it may be all one, as to the event of the experiment, whether the materials, wherewith it was successfully tried, were sophisticated or not, if those made use of in the latter trial were of differing qualities from those employed in the former ; because it may very well happen, that sophisticated bodies (as we may have occasion to shew hereafter) by the addition of those things, or by that deceitful way of preparation, whereby they have been sophisticated, may acquire an aptitude to produce such effects, as, had they not been adulterated, they would not have been fit to do. Now it is scarcely imaginable to him, that has not been very conversant with the drugs and simples sold in shops, how generally they are adulterated by the fraudulent avarice of the sellers, especially if they be such, whose preciousness may make their sophistication very beneficial to them, that practise it. It has been lately much complained of by some of the cultivators of clover-grafs, that of a great quantity of the seed not any grafts sprung up ; which not being imputable

to the foil, nor the sower, proceeds, as some analogical observations make me suppose, from the effeteness (if I may so speak) of the superannuated seed sometimes sold in the shops. And upon this subject I cannot conceal from you what was lately affirmed to me by one of the eminentest and soberest chymists of *Amsterdam*, who was also an Indian merchant, who assured me, that the most of the cinnamon and cloves, that is brought into these western regions, is defrauded in the *Indies* of much of the finest and subtlest aromatical parts, before it be sent into *Europe*. And to give a more familiar instance to our present purpose, you may be pleased to remember, *Pyrophilus*, that in one of the first of these essays, we have made mention to you of great store of living creatures, which we had observed in vinegar; of the truth of which observation we can produce divers learned and severe witnesses, who were not to be convinced of it, till we had fully satisfied them by ocular demonstration: and yet, *Pyrophilus*, there are divers parcels of excellent vinegar, wherein you may in vain seek for these living creatures: and we are now distilling some of that liquor (which if we did not think to be of the strongest and best sort, we should scarce think worth the being distilled for spirit) wherein nevertheless we can neither by candle-light nor by day-light discern any of those little creatures, of which we have often seen swarms in other vinegars. Of such fraudulent tricks as those lately mentioned, I could easily give you divers instances, if I were not afraid of teaching fallacies by discovering them. But some are so notorious, or otherwise of such a nature, as that it may be more useful than dangerous to mention them.

It is commonly known, that sublimate is wont to be sophisticated with arsenick: and how differing the effects of such sublimate may be from those of that, which is faithfully prepared, not only upon metals, but (when mercurius dulcis and other preparations are made of it) upon human bodies, they, and scarce any but they, who are acquainted with the noxious qualities of arsenick, both to metals and men, can readily imagine. And indeed as for chymical preparations, *Helmont*^{*} was not much in the wrong, when he affirmed, there were scarce any, vulgarly sold in shops, to be relied on as faithfully prepared. And for my part, I have so often met with chymical preparations, which I have found unsincere, that I dare scarce trust any, either in the administration of physic, or so much as in the trial of considerable experiments, which either my own furnaces do not afford me, or wherewith I am not supplied by some person, of whose skill and faithfulness I have a good opinion. The other day, having occasion to use some spirit of salt, whereof I was not then provided, I sent for some to a chymist, who making it himself, was the likelier to afford that, which was well made: but though I gave him his own rate for it, at the first rectification even in retort, a single pound afforded us no less than six ounces of phlegm; and afterwards being further rectified in a high body and gentle heat, the remaining spirit parted with a scarce credible quantity of the like nauseous liquor, and after all these lesecurations of phlegm, was not pure enough, to perform what we expected from it. Of which complaining to an excellent chymist of my acquaintance, he sent for spirit of salt to a very eminent distiller of it, who gets much by his profession, and pasleth for a very honest man: but this spirit, besides its weaknes, discovered itself to be sophisticated with either spirit of nitre, or aqua fortis, which betrayed itself by its peculiar and odious smell; whereas spirit of salt skilfully and sincerely drawn is

* *Accipe pulvrem Johannis de Vigo propria manu paratam, nam alioquin admittit minio est adulteratus, prout qualemque medicamen chymicum quod venale existat fraude plenum est.* *Helmon.* de Febr. c. 14.
Sunt nempe illa essentialia venalia, quaeque magno arte penduntur, adulterata immia arqua singula, &c. Idem de Febr. c. 15.

commonly

commonly of a greenish colour, bordering upon yellow, and hath usually a peculiar, and sometimes (as I can exemplify to you in some of mine) a not unpleasing smell. And let me on this occasion advertise you, *Pyrophilus*, that in divers cases it is not enough to separate the aqueous parts by dephlegmation, as many chymists content themselves to do; but some liquors contain also an unsuspected quantity of small corpuscles of somewhat an earthy nature, which being associated with the saline ones, do clog or blunt them, and thereby weaken their activity: and therefore such liquors to be well depurated require the being distilled off, and that with a slow fire, that the dry fæces may be left behind in the bottom of the glafs. To satisfy some persons, that this observation is not groundless, we have sometimes taken of the better sort of spirit of salt, and having carefully dephlegmed it, removed it into lower glafses (that the less heat will suffice to make the liquor ascend) and having gently abstracted the whole spirit, there remained in the bottom and the neck of the retort, whence it was distilled, so great a quantity of a certain dry and stipical sublance, for the most part of a yellowish colour, that it seemed strange to the beholders, that so clear a spirit should conceal so much of it: and we ourselves should have wondered at it too, had we not remembered, that in what the chymists are wont to call the oil or rectified butter of antimony made with sublimate, the liquor, though distilled and very limpid, almost like fair water, consits in great part of the very body of the antimony: which we would here manifest, but that we elsewhere do it; and therefore chuse rather in this place to take notice, that the spirit of salt after this second depuration was so changed, that it seemed to be a much nobler, and almost another liquor than it was before.

BUT to return to our sophisticated spirit; what differing effects would be produced by true spirit of salt, and that which is mixt with the spirit of nitre, he that knows the great disparity in the operations of those two liquors, whereof (to mention now no other instances) the former will precipitate silver, when the latter has dissolved it, may easily inform you. Which instances I mention not as the considerablest, which may be produced on this subject, but as the freſhest in my memory.

In the next place, *Pyrophilus*, I observe, that even when the materials employed about experiments are no way sophisticated, but genuine, and such as nature has produced them, or art ought to prepare them; even then, I say, there may be a very considerable disparity betwixt concretions of the same kind and name, and which pass without suspicion for bodies of perfectly the same nature.

This may, to you, *Pyrophilus*, seem a great paradox; but perhaps upon examination it will appear a great truth: which because I am, perchance, the first, that has solemnly asserted, I hope I shall obtain your pardon, if I insist somewhat the longer upon the making it out. For though antimony (and the like is to be understood of quicksilver, gold, copper, tin, &c.) is wont by almost all men without hesitancy to be looked upon as being all of it of the same nature as well as denomination; yet he, that will take the liberty to suspect, that they may be deceived in that opinion, and then heedfully observe the differing progres and event of experiments, may very well discern, that there is as well a difference in minerals of the same kind, as there is in vegetables and animals of the same species. And as the white rose, the red rose, and the damask rose differ much from one another, though all three be roses; and as the sour and sweet orange are very differing betwixt themselves, and yet both of them from the *China* orange, though all be oranges; and as the hound, the grey-hound, the spaniel, the tumbler, the mastiff, and the water dog, &c. are very diversly qualified, though all of them be dogs: so neither are all the parcels of antimony to be

Of the Unsuccessfulness

met with in mines or shops of altogether the same qualities, though all of them be antimonial concretes. There is indeed this difference betwixt the variety to be observed in vegetables and animals, and that which is to be found in minerals, that the former is wont to be more obvious to the eye, and betray itself by some difference to be observed, either in the size of creatures of the same kind, or in some peculiar shape or colour, by which it is easy for nature conspicuously to discriminate bodies, that consist of many discernably distinct parts; whereas minerals appearing to the eye either to be perfectly similar, as metals, or at least to consist but of two or three distinct ingredients, as cinnabar, and some other mineral concretions, the diversity to be found betwixt minerals of the same denomination is hardly to be discerned, before experience have discovered it.

AND on this subject I consider, that the womb (if I may so speak) of a mineral body is not always like that of an animal, a place by a competent and peculiar involving fence secured from the intrusion of all bodies not of kin to that included in it: but a mineral being generated in the bowels of the earth, its womb is oftentimes accessible and open to other mineral juices or steams, that pass that way, though of never so differing natures from that of the more copious mineral. Insomuch that not only I have had the opportunity to observe (not without some wonder) minerals of differing kinds, as marchasites and metals, marchasites and stones, (I mean stones properly so called) salt and sulphur, and the like, blended in the same small lump of matter; but I have sometimes found, in a great mass of one sort of mineral, small parcels of a mineral of a quite differing kind perfectly inclosed in the substance of the other. But to resume what we were laying before, these intruding bodies (if I may so speak) being coagulated, and perhaps ripened together with the former by length of time, are not easily either separable, or so much as distinguishable at their first digging out of the ground, and much less after their colligation. For the ignorant or heedless mine man aiming only at the obtaining a quantity of such a metal, or other mineral, as may be vendible under such a determinate name, has neither the design, nor perhaps the skill, to make nice separations of the heterogeneous bodies to be met with in his ore, but melts so much of them as he can promiscuously together; and then sells them, not only to the merchant, but the chymist, for that metal or mineral, whose outward form and properties (as colour, consistence, weight, sound, &c.) it has: though that metal, under whose name it passes, be indeed but the predominant ingredient of the lump, wherein divers other minerals may in small quantities lie concealed, and yet upon occasion be discovered by exquisite separations, or discover themselves by unexpected operations, when they meet with bodies fit to act on them, or disposed to receive impressions from them.

I WAS lately visited by an ingenuous goldsmith of my acquaintance, who complained to me, that being wont to buy parcels of gold brought in small pieces, and as it were sandy corpuscles, from Guinea, or some country of that coast, though he found it upon all trials very right gold, yet was it so very pale, that few but expert goldsmiths would meddle with it, as fearing it had been some sophisticated metal; adding, that this exceeding paleness of it sometimes reduced him to melt it with very high coloured gold, or to heighten its tincture with that of copper, to bring it to the colour of ordinary gold.

THE probability of this may be proved by what is related by Monsieur Flacourt, governor of the French plantation in Madagascar, who, in his newly published history of that island, speaking of the metals of it, says, *Il y a bien, &c. that is, Madagascar, there is certainly gold among the inhabitants of Madagascar, which has not been brought*

brought hither by foreign ships: for it is not possible, that such ships should have left them so much of that metal as they have; and besides, it is of a differing nature from that of Europe, which they call in this country *Voulamene Voutrœua*. He adds, that this gold, which they call gold of *Malacaffe*, is pale, and is not worth above ten crowns (or about fifty shillings) an ounce; also, that the Negroes affirm, that there are many mines of it in the country, where it was formerly digged; that there is three sorts of it differing in sinewes from each other, and discriminated by the natives by three peculiar names. But that which he adds as most considerable, is, that *Malacaffean gold is of so very easy a fusion, that it is almost as easily melted as lead*; whereas we here find the gold we deal with to require considerably strong fires, and are wont to cast in borax to facilitate the fusion.

HAVING, upon occasion, had the curiosity not long since to visit some mines of lead, and other metals, I find that there is a great difference, and discernible even to the eye, betwixt the severall ores; for instance, of lead, some of which I can shew you to like steel, and so unlike common lead-ore, that the workmen upon that account are pleased to call it steel-ore, which being of more difficult fusion than ordinary, they are wont to mix it with other ore, which they call firm-ore, to facilitate the melting of it. And I likewise took notice of an ore, which for its aptnes to vitrify, and serve the potters to glaze their earthen vessels, the miners call potternore, and sell it (at least where I saw it digged up) dearer than other ore, from which it differs both visibly enough, and as the workmen affirm, in divers other (and those less obvious) qualities; and yet all these ores, after fusion, do pass indiscriminately under the name and notion of lead. In which therefore it is no wonder, that severall inquiries find a great deal of disparity. I remember I did not long since cause some lead-ore to be tried, which being the most promising that ever I saw, made me suppose it might contain some considerable quantity of silver: but though it proved so rich in lead, as to yield after the rate of seventy pound to the hundred, yet one of the most expert artists in Europe could not extract one grain of silver out of it; whereas the lead of very many mines, being skilfully examined, will leave behind it, upon the test, a proportion of pure silver. And though this quantity of silver be not considerable enough to make such mines as yield it pass for silver mines, (or, as we are wont to call them, mines-royal) because the silver will not quit the coit of extracting it; yet such mines, though they pass but for lead mines with the metalist, may appear to be mixt mines to the naturalist, who may meet with divers experiments, wherein the little silver that is in them, may make their lead operate differently from that of those ores, which are wholly destitute of silver.

AND as this disparity is discernible in lead-ores, so it may well be supposed, that the like would be discovered in the ores of other metals, if they were but purposely and skilfully examined. On which subject I remember, that a very experienced person in these affairs, and otherwise very candid and sober, was lately very desirous I should procure him some tin-ore, alleging, that he had met with a sort of it, which, after a long digestion in lixiviate liquors, afforded him a very considerable proportion of the richer metals; insomuch that having a large quantity of that ore, and finding the experiment on it to succeed constantly, he promised to himself a vast income by it: but when that stock of ore was spent, the next that he procured, though with great carefulness managed as the former, would by no means be brought to afford either so considerable a benefit, or so much as any at all. Which brings into my mind, that having once bought a parcel of block-tin (as the tradesmen call that, which is of the most pure or unmixt, and as yet unwrought) I was desirous to try,

if I could not make a menstruum to dissolve it in such manner as aqua fortis dissolves silver, and aqua regis gold; because chymists are wont to complain, that though they have a menstruum or two that will dissolve crude tin, yet they want one, that will keep it dissolved, and will not, which aqua fortis will, let it fall into a calx. Having therefore (by a way that I elsewhere teach) prepared such a liquor as was desired, I evaporated a solution of the forementioned tin, and setting it to shoot, found somewhat, to my wonder, that the crystals it afforded were not at all like any kind of vitriol, but broad, flat, and exceeding thin, just like those of silver. Whereupon for further trial, having examined this salt by the tongue, we found not, that it had any such taste as skilfully made calx of tin in spirit of vinegar, (wherein it is not every calx of *Jupiter* that is soluble) which (the last time we tried) seemed to us to have, as it were, a chalybeate taste, but such an excessive bitterness as may be met with in the crystals of silver made with aqua fortis. Finding also this further resemblance betwixt the salts of these two metals, that they did both of them presently dye upon the nails and skin a blackness, that could not in a short time be washed off; we should have suspected, that the menstruum had exalted the metal dissolved in it to a greater cognition to silver; but having afterwards prosecuted the same trial with the same menstruum, and another parcel of block-tin, (the former being casually lost) this metal, though bought very soon after the other, and, as I remember, at the same place, made us conclude, that the event of our trials proceeded from our having lighted upon a lump of tin, that was of a peculiar nature.

I REMEMBER also, that a while since a learned and inquisitive friend of mine found in his land a parcel of ore, part of which he shewed me, and some of which I can shew you, but have not yet made trial of it; which seemed to me, among others that looked upon it, to be copper ore, and which did indeed, after fusion, yield very good copper; but the persons, to whom he committed the examination of the mine, being very inquisitive, and extraordinary skilful, they did (as one of themselves immediately after confessed to me) find in that ore, besides the copper, no inconsiderable quantity of silver; and in that silver (having dissolved it in aqua fortis) a considerable proportion of gold.

BUT to detain you no longer on this subject; give me only leave to strengthen the paradox I have proposed, by the authority of that great and candid chymist *Bafilius Valentinus*, who speaking of antimony, after he hath told us, that there are several kinds of it, and especially two; the one more mercurial, and of a golden property, witnessed by the shining streaks or beams it abounds with; the other more full of sulphur, but destitute of the golden nature that enriches the former; adds, that there is such a differenter goodness betwixt the several sorts of antimony, as there is betwixt the several sorts of flesh or fish, which, though agreeing in name, and, if you please, in nature, do exceedingly differ in point of goodness. Which brings into my mind the great difference which I have found, even visible to the eye, betwixt the several sorts of antimony; and makes me also remember, that the other day being by an excellent chymist shewed a parcel of antimony as a rarity, upon the score of the various coloured sulphur, wherewith it was conspicuously enriched, the professor of it soon after employed it to make butter of antimony: but though he were very expert in that kind of distillation, yet instead of the liquor he expected, upon the approach of a gentle fire, he found the neck and body of his retort lined with an antimonial cinnabar, (or at least a red substance, by him concluded to be sulphur;) at which being surprized, he was pleased to withdraw his fire, till he had acquainted me with this accident, and in the yet unbroken retort shewed me the cinnabar, which is not wont

wont (as you know) to arise till after the butter of antimony is come over, and the remaining matter be urged with a vehement fire. And it is perhaps to the undiscerned difference of antimonials, that we may sometimes ascribe that contingency, which we have divers times had occasion to take notice of in the making of antimonial cinnabar: for though in our furnaces it hath been very successfully made, yet not only we have afterwards failed of making it, but we have seen much more expert chymists, and who, because of the high value they do (not undeservedly) place upon that medicine, employ themselves oftener than we in making it, divers times unsuccessfully attempt the preparing it. And it may be perhaps also from some diversity either in antimonials or irons, that eminent chymists have (as we have observed) often failed in their endeavours to make the starry regulus of *Mars* and antimony. Infomuch that divers artists fondly believe and teach (what our experience will not permit us to allow) that there is a certain respect to times and constellations requisite to the producing of this (I confess admirable) body. Upon which subject I must not omit to tell you, that a while since an industrious acquaintance of ours was working on an antimony, which unawares to him was, as we then supposed, of so peculiar a nature, that making a regulus of it alone without iron) the common way, (for his manner of operation I inquired of him) he found, to his wonder, and shewed me his regulus adorned with a more conspicuous star, than I have seen in several stellate reguluses of both antimony and *Mars*. Yet I dare not be too confident that this depended only upon the peculiar nature of that antimony, because since that, my own laboratory has afforded me divers such parcels of regulus without *Mars*, (some of which I have yet by me very fairly stellated) which though made of antimony, that seemed (by its various colours) to be more rich than ordinary in sulphur; yet in regard the antimony did not constantly afford a starry regulus, though by the same person ordered as near as could be after the same manner, it did not so clearly appear to me, whether the different event of the several trials proceeded from the peculiar nature of this or that parcel of antimony, or from some odd and scarce discoverable circumstance in the management of the operation. But in either case, the mention of these uncertain events will properly enough belong to our present discourse.

As in antimony, so (as I intimated above) in divers other minerals a considerable diversity may be observed: and I remember I was lately presented with a piece of a mineral, which to me, as well as to the rest who looked on it, seemed to be an ordinary and worthless marchasite; and yet a Dutch merchant (a skilful mineralist) who was the possessor of it, was very industrious to procure a greater quantity thereof, having in some of it, on which he had made trials, found a rich proportion of pure gold. And the same gentleman, whose copper ore I formerly mentioned, digging for more of that ore, found lately a quantity of red earth, which by knowing mineralists was guessed to be but bolus, and indeed looked very like it; but being melted with *regulus Martis stellatus* by a skilful trier of metals, it divers times richly compensated the examiner's curiosity, by affording him many grains of fine gold: and though I doubt, whether this gold proceeded from the bolus, or the regulus melted with it, yet however it may serve for an instance to shew, that some mineral bodies, which pass without dispute for minerals of such and such a precise nature, may have lurking in them minerals of a quite other nature, which may manifest themselves in some particular experiments (wherein they meet with proportionate agents or patients) though not in others.

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THAT the talc, which is wont to be employed about cosmeticks, is of very difficult calcination, is so known a thing to those that have tried to calcine it, that I have met with good chymists, that have looked on all the calces of tales but as impostures, Nor indeed have we calcined Venetian talc without some length of time, and much violence of heat. But among many sorts of talc we have here in *England*, there is none, which a moderate fire will in less than an hour reduce into a snow-white calx, of which I had lately a parcel by me; and some days since I met with another sort of English talc which I could suddenly calcine even with the flame of a candle. And my experienced friend Dr. K. assures me, that out of a German talc he met with, he did by digesting it in a strong solution of alcalize salts separate pretty store of good gold, and might have made it a very gainful experiment, if all the talc growing in the same place had been of the same richness. The like almost has been affirmed to me by a gentleman of eminency, who told me, that from a certain talc he had out of *Norway*, he had once drawn a pretty quantity of very good gold: and it seems indeed, that though some have been pleased to laugh at all attempts of sequestering any thing from any kind of talc; yet some parcels of that mineral afford good store of a tincture, which may, for aught I know, be of a golden nature. For I remember I have met with a kind of darkish coloured talc (whereof I can yet shew you a piece) which when I cast but into aqua regis, the menstruum manifestly worked upon it, and dissolved its coloured parts in such plenty, that the filtrated solution passed without sulphuric among divers knowing naturalists, to whom I shewed it, for a fair solution of gold. *Paracelsus* himself reckons four kinds of talc, red, white, black, and of that colour, which his interpreter translates luteous: and perhaps each of these colours comprises several kinds of that mineral. And therefore that mineralist did not answl, when he added in the same discourse, after he had mentioned great variety of marshasites, stones, and other minerals, *Sed & hoc verum est, in terra multa aducendi, qua mibi incognita sunt, sed eadem nec alii norunt. Certum siquidem est, progreesse temporis tot tamque varia à Deo adhuc proditum iri, de quibus nemo nostrum nedum unquam somniavit.*

*Parcel.
de Minerali.
T. acta 1.*

*Parcel,
ibidem.
temporis tot tamque varia à Deo adhuc proditum iri, de quibus nemo nostrum nedum unquam somniavit.*

It is vulgarly known, that there is a great difference between vitriols, that are reputed to be merely of the same metal. And not to mention those vitriols, that I have either made or seen, of les usual colours; nor to take notice of the veins, slate, and even loose earth, impregnated with copperas that I have had: to pass by all this (I say) as for those vitriol stones, whereof we in *England* are wont to make our vitriol, I have seen, at the chief work where copperas is made, so great a variety of them, (divers of which I have yet lying by me) that I could scarcely believe the workmen, when they affirmed them to be all copperas stones; and cannot but think it both very likely, that some of them contain other mineral substances besides vitriol, and very possible, that the saline parts of those stones, upon their solution by the rain, may work upon those other substances formerly concoagulated with them, and thereby imbue some parcels of the vitriol made of them with qualities other than are essential to the nature of vitriol, or belong ordinarily to it.

THAT there is also a difference betwixt those bodies, that pass under the general name of common salt, cannot but be obvious to any chymist, that hath occasion to make accurate trials on that subject. And as for those concretes, that pass under the name of salt-petre, there is probably no small disparity among them: for besides the difference which we have observed, and which is obvious enough betwixt good English nitre, and that which is brought us over from *Barbary*, (which before it is much refined abounds very much with an adventitious salt, that tastes much like sea-salt)

salt) besides this I say, those that do use both good European and good East-India salt-petre assure me, they find much difference betwixt them, and give the preference to the latter. And indeed I have often thought I discerned a considerable difference in the operations of several kinds of salt-petre even after purification: and probably that sort of salt-petre, which near *London* an ingenious man of my acquaintance does sometimes (but cannot always) make, chiefly of our sea-salt, hath some differing qualities from that, which is drawn the common way out of the earth. And indeed salt-petre being but a kind of *fal terra*, generated in very differently-qualified parcels of earth, may probably receive divers qualities from the particular soil, wherein it grows, though these qualities lie concealed and unsuspected under the wonted exterior appearance of nitre. Which consideration brings into my mind what was lately told me by a very ingenious gentleman, concerning one of the eminentest of our *London* physicians, who was wont, as this confidant of his assured me, as an excellent secret, to employ in some of his choice remedies that peculiar salt-petre, which he had drawn out of the earth digged up in church-yards.

And such kind of differences would probably in other mineral bodies be taken notice of, if men's prepossessions did not make them ascribe the variations they meet with in their experiments, rather to any other cause, than the unsuspected difference of the materials employed about them.

Nor is it only, *Pyrophilus*, among mineral bodies of the same name, that such a diversity is to be found; but, if narrowly looked into, it is very probable, that a greater disparity may be discovered both among vegetables and animals, reputed of the same nature, than hath been yet taken notice of. Herbarists indeed have exercised a commendable curiosity in sub-dividing plants of the same denomination, and few naturalists ignore, that there are (for instance) many sorts of roses, and of apples, which differ widely betwixt themselves, as we see the difference betwixt the red rose and the white, betwixt the crab, the pippin, and the pearmain. But besides these differences, which are obvious enough to be registered by botanick authors, there may be more undiscerned ones (which yet may be considerable ones) betwixt the individuals of the same ultimate subdivision of plants, arising partly from the temperature of the air, which makes (for example) senna growing in *England* to differ much from that, which is denominated from *Alexandria*; partly from the nature of the soil, as is obvious in the change produced in wild simples transplanted into gardens; and partly from many other causes, which we have not now leisure to insist upon. But we see oftentimes, that one rose much differs from another of the same kind, and one pearmain from another pearmain. To which we may add, that the upper crust or surface of the earth being impregnated with subterraneal exhalations of several sorts, and tempered with variety of juices, it may very possibly be, that some particular plant may attract such juice out of a determinate spot of ground, as may give it exotic qualities, and make it differ even from the neighbouring plants of the same kind: to which purpose I remember, that travelling divers years since from *Geneva* towards *Italy*, I was in my passage through *Switzerland* by a gentleman of those parts (whose brother had been formerly my domestick) invited to his castle, and entertained among other things with a sort of wine, which was very heady, but otherwise seemed to be sack; and having never met with any such liquor during my long stay in those parts, I was inquisitive to know whence it was brought: and being answered, that it grew amongst those mountains, I could not believe it, till they assured me, that growing on a little spot of ground, whose entrails abounded with sulphur, it had from the soil acquired its inebriating property, and those other qualities,

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lities, which made it so differing from the wine of the rest of the vineyards of that country. And now I mention wine, give me leave, *Pyropbilus*, to put you in mind of taking notice, what a great change is made in that liquor, when upon the receſſes of the ſpirits and more volatile ſulphurous parts, or else the new texture they make with the others, it degenerates into vinegar; and yet how little either diminution of quantity or any other alteration doth appear upon this change to the beholder's eye. And though no body is like to loſe an experiment by miſtaking vinegar for wine, because both thoſe liquors and the changes of them are ſo familiar unto us; and because we are wont to taste each of them before we employ it; yet who knows what charges there may be in other bodies, with whose alterations we are unacquainted, though the eye, which is oftentimes the only ſenſe employed about judging of them, diſcerns no change in them? as may daily be obſerved in the ſuper-annuated ſeeds of plants, which, after their having been kept long beyond their due time, loſe all their germinating power, without loſing any of their obvious qualities. And here let me further obſerve to you, that urine is made much uſe of, not only by dyers, but feveral other tradeſmen, in divers operations (ſome of which we may elſewhere have occaſion to treat of) belonging to their profeſſions. Now theſe men being wont indiſcriminateſly to employ urine, without examining, whether it be rich in ſalt or not, and how long it hath been kept, it may not be impertinent to take notice, that chymists, who have occaſion to diſtill it often in great quantities, affiue me, that they find a notable diſparity betwixt urines, that of healthy and young men abounding much more with volatile ſalt, than that of ſickly or aged perſons; and that of ſuch as drink wine freely being much fuller of ſpirituouſ and active parts than that of thoſe, whose drink is but beer or water. But because the diſſering ſtrength of urines, though it be very probable, is not ſo eaſily to be ſatisfactorily made out, we ſhall rather iñſit on this other obſervation confirmed to us by expe‐rience, which is, that though tradeſmen are often wont to boil ſuch and ſuch things indifferently in any urine, as if it were all one how new or stale it is, they may ſometimes thereby commit conſiderable errors. For recent urine, wherein the ſaline parts are yet entangled among the reſt, will ſuffer itſelf to be boiled above one half or two thirds away, without the avoiation of its volatile ſalt and ſpirits. Whereas urine that has been divers weeks kept, is liable to a putrefaction, whereby the cement (if I may ſo speak) of the ingredients that it conſiſts of, perishing, or ſome change of texture occaſioning their diſjunction, (if not alſo concurring to produce them) the component parts fall aſunder, and the ſaline particles extracting themſelves from the reſt, will even upon a very gentle heat (as trial made on purpoſe has informed us) fly away, and leave a phlegmatische and unaſſeſtive liquor behind them. In confirmation whereof I muſt acquaint you, *Pyropbilus*, with what lately beſel me in reference to the diſtillation of urine: for having cauſed ſome of it to be buried in earthen veſſels in a dunghill, to be there putrified, for five or ſix weeks, I was by divers occaſional journeys kept from employing it, till it had lain there between four and five months; and obſerving, when I cauſed it to be taken out, that the covers of the veſſels had not been, by him I employed to put them in, well luted on, and beſides were in ſome places cracked, I ſuſpected, that the heat of the dunghill had not only loofened the ſaline parts of the liquor, but driven them away: and accorſingly by diſtilling it in a very gentle heat, and in a very high cucurbit, we obtained, inſtead of an active and ſaline ſpirit, a languid and naueſous phlegm. And how great odds there may be betwixt ſome experiments made with recent and putrified urine, may be eaſily conceived by him who knows what operation ſalts have in the buſineſſes of colours, and is acquainted with their

their efficacy in those other mechanical experiments, wherein urine is wont to be employed. But I fear I have dwelt too long upon this theme, and therefore I shall proceed to the next.

AND in the third place, *Pyropbilus*, I shall observe to you, that there is a great difference to be found among many things prepared by art, that pass under the same general name: which difference may proceed partly from that, which we have already observed to be found in the materials of which such factitious bodies are made, and partly from the way used in preparing them. To these heads many particulars may be reduced. But we shall at present restrain ourselves to the mention of two sorts of prepared bodies, namely, of such as are not purified and exalted enough, and of such as are so too much.

AND to begin with the first of these; it is very certain, that divers chymical experiments delivered by sober authors have been believed false, only because the instruments or other materials employed in the unsuccessful trials of them were not as highly rectified, or otherwise as exquisitely depurated, as those that were used by the deliverers of those experiments; so that oftentimes the fault of a bad menstruum is injuriously imputed to a good artist. That experienced chymist *Van Helmont*, in his paradoxical treatise of the stone, endeavours (as we have elsewhere mentioned) to explicate the manner of its being generated, by the coagulation immediately ensuing upon the mixture of the two volatile spirits of urine and of wine. This noble experiment has been by many unsuccessfully tried, and has been therefore by them discredited as a chymical fiction: and indeed the first, and I think the second time we attempted to make that coagulum, we found nothing at all of any such thing as we expected upon the confusion of the two fore-mentioned liquors; which though never so much shaken, and afterwards permitted to rest, did never in the least measure coagulate, which made us long suspect the experiment; till at length our favourable thoughts of that expert chymist making us think it possible, that the spirits we employed had not been sufficiently exalted, we dephlegmated, some by more frequent, and indeed tedious rectifications (which yet proved but necessary) and then were satisfied by more accurate trials, that *Helmont* had not misinformed us.

So likewise the same author in his treatise *de Peste* much extolling, as a friend to the stomach, the entrails, the nervous parts, and even the head, the tincture or solution of amber made with spirit of wine (which medicine is indeed no ignoble one, when administered to constitutions, that can well bear the heat of it) divers physicians and chymists have attempted the preparing of this tincture with such bad success, that they have given out, that either *Helmont* delivered what was not true, or concealed some considerable circumstance of the process.

WHEREAS having digested sufficiently dephlegmed spirit of wine upon very finely powdered amber (which, if it be the higher-coloured, yields the deeper tincture) in a very gentle heat (for the neglect of which caution even expert artists have often lost their pains and glasseſ) we have several times had a good yellow tincture of amber, which was discernable in the menstruum both by the ſnell and taste; and to ſatisfy ſome, that ſuspected the tincture to proceed but from the exaltation of the menstruum itſelf by digestion, and to manifest, that it was a real ſolution of the ſubtiler parts of the amber, we poured ſome drops of it into beer, or water, into which the spirit of wine ſuddenly diſſulfing itſelf, the diſſolved amber was plainly discernable ſwimming like a thin film upon the ſurface of the liquor, whence, little by little, it ſteamed away into the air.

THERE is likewife, as we have tried, to be drawn with spirit of wine from pure ſalt of tartar a pretty high tincture, and of a taste, which I thought not unworthy

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the taking notice of: but having a while since tried to draw this tincture with spirit of wine, which (unknown to me) was much too weak for that purpose, after I had kept the glass a while in digestion, coming to look whether or no the spirit was tinged, I found, that the salt of tartar had drawn to itself and imbibed the aqueous particles of the spirit of wine, and being thereby (for a great part of it) dissolved into a liquor like that, which is commonly called oil of tartar *per deliquium*, the subfiding salt was by the interposition of that saline liquor protected from the action of the spirit of wine; which being by this new way dephegmed, would not mix with the saline liquor, but swam entirely above it. To which I shall only add in general, that the German chymists are divers of them so accurate in the rectification of their spirit of wine, that in *England*, where we are wont to be less careful about that particular, it is usual enough for those experiments of theirs to be unsuccessfully tried, wherein the alcohol of wine (as they call it) is requisite.

And as spirit of wine, so many other menstruums are made unfit for the perfecting of divers real experiments, barely by their not being sufficiently freed from their weakening aquosity.

Nor is it only, *Pyropbilus*, in menstruums, but in divers other bodies, that the want of an exquisite depuration may produce in experiments variety of events. As for instance, it has been complained of by sober men, that their preparations of silver, though never so carefully made, have been apt to produce violent vomits; whereas we have not observed a well-prepared medicine of duly refined silver to work emetically, even in women and girls, but by seige or urine. But we cannot wonder at the violent operation of medicines made of ordinary silver: for not only that, which is coined, is wont, as the mint-masters themselves have confessed to me, to be alloyed with sometimes about a twelfth part, sometimes a smaller or greater proportion of copper, for the greater conveniency of the coin; but even that silver, which is commonly at great rates sold for refined silver, is not wont to be sufficiently freed from its copper. Which I not long since manifested in the presence of one of our richest and eminentest refiners, by dissolving some of his purest silver in his own aqua fortis; for the greenness of the solution quickly betrayed the adherency of *Venus* to the silver. And no wonder, for I have seldom seen our chiefeſt refiners blow off from their silver upon the test above half its weight of lead, whereas we think not our silver sufficiently refined for ſome purpoſes, till it have been freed from five or ſix times its weight of *Saturn*; and then it has ſometimes afforded a ſolution almost as clear as water, with only now and then a light touch of iky-colour, but nothing near ſo high as the ceruleous (liquor that is ſuppoſed to be a true) tincture of silver, artificially ſeparated from the reſt of the body.

Now that ill effects by the mixture of copper may be produced in ſuch medicines, as ought to be of pure silver, he, that is acquainted with the violent emetic qualities of *Venus*, can scarcely doubt. And as in men's bodies, ſo in other ſubjects, thoſe experiments may eaſily deceiver the artiſt's expectation, when he hopes to perform with silver and copper together thoſe things, which ſuppoſe and require silver without copper, or any adventitious metal: and as silver, ſo gold is very often employed for pure, when it is not ſo: for even the foliated gold, which is commonly ſold here in *England*, how fine foever it is reputed, is not altogether free from the pollutions of other metals; for our gold-beaters, though for their own profit fake they are wont to uſe the neatest coined gold they can get (as that which is capable of the greatest extenſion under the hammer) yet they ſcruple not to employ coined gold, and that the mint-masters (as themſelves inform me) are wont to allay with copper or silver, to make the coin more ſtiff, and leſs ſubject to be wasted by attrition. And as for thoſe

many

many goldsmiths and chymists, who think their gold most requisite refined, when they have blown from it on the test a due proportion of lead, they may be therein sometimes mistaken: for though *Saturn* may carry away with him all the copper, that did imbibe the gold, yet he does not likewise free it from the silver (for which purpose aqua fortis is therefore wont to be used) nay, the skilfullest refiner, that I ever yet knew, hath several times affirmed to me, that cupelling fine gold with lead, the gold has after retained and protected from the fire a proportion of silver, that lay lurking in the lead, and was afterwards separated from the gold by aqua fortis, but in so small a quantity, that the experiment (the cost and pains considered) was not luciferous. And of this sort of instances, *Pyrophilus*, more might be presented, if we did not think prolixity might be unwelcome to you.

BUT as many experiments succeed not according to expectation, because the menstruums employed about them were not pure enough; so some miscarry, because such menstruums are but too exactly depurated: for it is not so much the purity of liquors in their kind, as their fitness for the particular purpose, to which they are designed, that is in experiments to be principally regarded. For instance, we have sometimes, for recreation sake, and to affright and amaze ladies, made pieces of white paper and linen appear all on a flame, without either burning, singeing, or as much as discoloring them. This is performed by plunging the paper very thoroughly in weak spirit of wine, and then approaching it to the flame of a candle; by which the spirituous parts of the liquor will be fired, and burn a pretty while without harming the paper. But if this experiment be tried with exquisitely rectified spirit of wine, it will not succeed. Of this phenomenon this plausible reason has been assigned, that the flame of the spirit of wine is so pure and subtle, that, like an *ignis lambens*, it will not fasten upon the paper. But experience has informed us, that this conjecture is but a mistake, for the flame of spirit of wine is so hot, that I have in lamp-furnaces employed spirit of wine instead of oil, and with the same flame I have not only lighted paper, but candles, and even melted foliated gold. The true reason therefore, why that paper is not burned by the flame, that plays about it, seems to be, that the aqueous part of the spirit of wine being imbibed by the paper, keeps it so moist, that the flame of the sulphureous parts of the same spirit cannot fasten on it. And therefore when the deflagration is over, you shall always find the paper moist; and sometimes we have found it so moist, that the flame of a candle would not readily light it. And on the other side, having purposely made trials of plunging paper into sufficiently dephlegmated spirit of wine, the paper not having aqueous moisture to defend it, was very readily kindled and burned by the flaming spirit. And one of our best ways to try the pureness of spirit of wine is grounded on this very supposition: for dipping it in a cotton-wick like that of a candle, and setting it on fire, if the flame fasten on the wick, it is a sign of the goodness of the spirit; but if it do not, we conclude it to be weak, and not sufficiently dephlegmated. It hath been likewise observed, that aqua fortis will work more readily on lead, if it be alloyed with water, than if it be purely rectified. I elsewhere also mention an aqua fortis I have used, which was so strong, that it would not well dissolve silver itself, unless I first diluted it with fair water. And within this very week, wherein I write these things, I have had an unwelcome proof, that liquors may by too exquisite a depuration be made unfit for our purposes. For having, to gratify some ingenious friends, made a certain menstruum, wherewith we had formerly done some things upon gold, which were (not altogether without cause) thought strange enough, we took care at this time to separate it from whatever was either of an aqueous or an earthy nature, more exactly than ever we had formerly done. But coming to make use of this sort of menstruum,

Of the Unsuccessfulness

we found to our trouble and los, that instead of performing its wonted operations upon gold better than before, we could do nothing at all with it: for it will not now by heat itself be brought to touch gold, though that metal were wont to be dissoluble in it even *in frigido*. And to satisfy you, that it was the too exquisite depuration of this liquor, especially from its terrestrial parts, that thus unfitted it to touch a metal, which is otherwise wont to melt as it were naturally in it without ebullition (almost like ice in luke-warm water;) we will subjoin, that not only we in vain tried to make it serviceable by weakening it with fair water; but having, for trial-sake, taken a little of this numerical parcel of liquor before it was so carefully rectified, we found, that it dissolved crude gold as well as we had reason to expect. And it would be considered, whether or no in the extraction of the tinctures of several bodies, chymists do not only put themselves to a needles, but to a prejudicial trouble, when they refuse to employ any other spirit of wine, than that which is highly rectified. For, though in many bodies the parts desired by the artits being the sulphureous ones, the menstruum is the better for an exquisite dephlegmation; yet in divers other concretes the useful and efficacious parts have in them something of saline, which makes them more free to impregnate copiously such liquors, as have some aqueous mixed with their sulphureous parts.

But because there is nothing more easy than by diluting spirit of wine, though never so strong, to make it as weak as one pleases; and because pure spirit of wine is that of all other menstruums, that chymists generally make most use of, and which costs them most of charge and trouble (insomuch that here in *London* that, which is perfectly dephlegmed, is valued, in their shops that sell both, at ten times the price of common spirit of wine;) I presume you will not take it ill, that without being obliged to it by the title of this discourse, I take this occasion to acquaint you with the way I employ to obtain dephlegmed spirit of wine; especially since the practice of the common way of frequent rectifications is (not to mention other inconveniences) wont to prove either exceeding tedious, or insufficient. Put then about an inch thick of tartar calcined to whiteness (for I find it is not necessary to reduce it to a salt) and very dry, into the bottom of a tall and slender glas body, and pour on it as much spirit of wine, that has been but once rectified, as will, when they have been shaked together, swim above the tartar a finger's breadth (more or less in proportion to the tartar you put in) and then the head and receiver being carefully fastened on again, in a gentle heat draw off the spirit of wine, shifting if you please the receiver, when about half is come over, and if need be, rectifying once more all that you distil upon dry calx of tartar as before. Whether or no you may meet with this method in some chymical books, I know not: but it seems, that either it has not been clearly taught, or has been proposed by suspected authors, or else among other proceesses, by being found in whose company it has been discredited. For the most antient and experienced distillers I have met with, have either contented themselves to follow the common way of repeated rectifications, though thereby they lose much time, and much spirit of wine; or else have had recourse to peculiar vessels of such a height, as besides that they are neither easily nor cheaply to be procured, do not, as far as I have hitherto seen, excuse the need of reiterated rectifications. Whereas, when we considered, that the fixed salt of tartar readily imbibes aqueous bodies, and that yet it will not at all mix with pure spirit of wine, it was easy to conclude, that the phlegmatic part of the spirit of wine would be soaked up by the alcalizate salt, whereby the inflammable part would be freed from it. And accordingly when we proceeded after the manner above prescribed, we found, that the liquor, that was produced upon the first rectification from the salt, being fired in a warm silver-spoon, did not leave

leave behind it one drop of phlegm, or so much as the least moisture upon the spoon; nay, and indeed did endure a severer examen, to which for curiosity's sake we thought fit to put it. And when the distillation was carefully made, we found by frequently (for trial-sake) shifting the receiver, that all the spirit that ascended was (to sense) equally pure, since that which came up last of all, even till the calx seemed to begin to grow dry, by beginning to cleave at the top, did burn all away, as well as that which came over first. And having for further trial taken out the calcined tartar, and distilled it with a good fire, it yielded us pretty store of a naufeous and strongly scented liquor, which seemed to be but phlegm, both to the taste, and by its not being at all inflammable, though carefully tried. The same calx of tartar being kept in some earthen vessel upon the fire till it be well dried, which will require a good heat, may be employed more than once in this operation. And it was not needfully that we prescribed bodies tall and slender; for we found not the experiment to succeed in large and low ones, and much less in retorts, in which the phlegm is wont to rise together with the spirit; yet we found, that provided the distillation were made with a sufficiently mild heat, a glafs, though very broad, and but moderately high, would serve the turn so far, as that the first half that ascended (the other being very weak) proved a spirit, that in a silver-spoon would burn perfectly all away. And because white calx of tartar is sometimes not so easy to be procured, we will add, that we have for trial-sake sometimes substituted quick-lime, or salt of pot-ashes (made by a single solution, filtration, and coagulation) with no bad success, especially in case of removing the receiver before the ascension of the last part of the liquor, though even that itself has sometimes from quick-lime come up inflammable enough. And therefore this alcohol of wine we peculiarly call the alcalizate spirit of wine; and the rather, because *spiritus vini tartarizatus*, which perhaps may be thought the properest name for it, is employed by eminent chymical writers to signify a different thing. And a practicable way of making such an alcalized and pure spirit of wine we thought not unfit to teach you here once for all, in regard the menstruum is so highly useful, not only for tinctures, extracts, and many other chymical operations, but in the making of divers philosophical experiments, and particularly some of those, which you may meet with in our writings. And an eminently ingenious person (but to me a stranger) chancing to get a sight of this essay, was pleased to give me thanks for this last part of it; because, though he had very often made use of salt of tartar to improve spirit of wine, yet he did it before, not to dephlegm the weaker liquor, but to acuate the strong with the alcali: which though I deny not to be a thing feasible, yet (as I told him) unlesl it be skilfully attempted, the highly rectified liquor, that is poured on, will rather leave some of its most spirituous parts behind, than carry up so fixt a salt.

The Second E S S A Y,

Of Unsuccessing E X P E R I M E N T S.

WHAT has been already said, *Pyrophilus*, may, I hope, suffice to shew you, how experiments may miscarry upon the account of the materials employed in trying them. And therefore we shall now pass on to consider the contingencies, to which experiments are obnoxious, upon the account of circumstances, which are either constantly unobvious, or at least are scarce discernible till the trial be past. And because these circumstances can hardly be discoursed of in an accurate method (which their nature will scarce admit of) I shall not tie myself to any other order in setting down the instances, which occur to me on this occasion, than that wherein they offer themselves to my memory.

And first I must acquaint you with what was not long since seriously related to me by doctor K. a person exceeding far both from the custom, and, in this particular, from the temptation of telling untruths. He then assured me, that lending his laboratory in Holland to a friend of his during his own absence, and leaving in that laboratory among other things great store of aqua fortis of several compositions, which he had made, to employ about his famous scarlet-dye, this friend of his sent him word a while after his departure, that by digesting gold with an aqua fortis, he had separated the tincture or yellow sulphur from it, and made it volatile (the remaining body growing white) and that with this golden tincture he had, not without gain, turned silver (as to part of it) into very perfect gold. Upon which advertisement the doctor speedily returning to his laboratory, did himself with the same aqua fortis divers times draw a volatile tincture of gold, which did turn silver into true gold: and (that I may add that upon the bye, to gratify your curiosity) when I demanded, whether or no the tincture was capable to transmute or graduate as much silver, as equalled in weight that gold, from whence the tincture was drawn, he assured me, that out of an ounce of gold he drew as much sulphur or tincture, as sufficed to turn an ounce and a half of silver into that noblest metal. Which I am the more disposed to believe, partly because my trials permit me not to doubt of the separableness of a yellow substance or tincture from gold; and partly because I am tempted to think, that silver may have in it a sulphur (to speak in the chymists language) which maturation is capable to graduate into a golden one, by having been certified by the observations of men very experienced in metalline affairs (and perhaps too by my own) that sometimes by corrosive liquors (which Sir Francis Bacon also, if I mistake not, somewhere observes) and sometimes by the operation of common sulphur (especially well opened and associated with fit salts) silver has afforded some grains of very pure gold. But our doctor found himself much mistaken in the hopes of growing rich by this experiment; for a while after endeavouring to make it again, his hopes were frustrated, which he ascribes to the aqua fortis, and therefore has attempted the same work afresh. But since all his trials have been hitherto fruitless, it is not improbable, that the disappointment proceeded from some other more abstruse cause; for we find such adventures to have sometimes befallen artifices irreparably. And Glauber alone, if you will therein credit him, tells us of several ways, by which he

he made gold once, and could not do it again. Upon which subject I must not omit those very illustrious testimonies and instances of this nature, that I find recorded by that ornament of his age and quality, the prince of *Mirandola*, in his treatise *de Auro*. Liber. 3. cap. 6. *Novi* (says he) qui mibi afferuerit semel se ex mobili argento, quod vivum dicitur, stabile verumque argentum consecisse suicis & foliis berbarum, idque vendidisse peritis explorande metallicae veritatis; eisdem mox usum se foliis frustra, & quod semel perfecerat, nunquam alias, quanquam id sepe tentaverit, perficiere potuisse.

Alium novi (says he further) qui adiuc apud vivos moratur, cui cum aurum & argentum circiter quindecies per artem effectum est, amissi artem eam, accepitique oraculo facii per quietem habito, id ingrato mentis vicio contigisse. Ut bina etiam veritatem apostolici dicti condiscamus, Neque qui plantat, neque qui rigat, est aliquid, sed incrementum dat Deus. And to both these narratives our learned prince does in the same book add divers others. *Retulit quidam mibi* (subjoins he) *sese aurum ex argento fecisse semel magnâ copia*; *secundo* *se usum eidem rebus, fecisse quidem, sed minimâ semper quantitate, sic ut detrimentum lucro majus esse supputaverit.* *Venisse in mentem, uti detrimentum effugere possit, si non ex argento, sed ex æræ melioris conditione metalli, sese consequi experiretur, idque se conjecturis firmis nixum tentavisse: cumque in eo fuisset, ut rem sese adepturum speraret, miris modis evenisse, ut nihil omnino conquequeretur.*

Idem (continues the prince) affirmavit ab amico, qui expertus hoc ipsum fuerat accepisse, qui cum ex cinnabari argentum fecisset optimum, se penumero sese possea insuffitentem operi majore cum diligenter semper eventu rei suisse frustratum. And to these relations of this famous prince I could add others of some acquaintances of mine, who having either once or twice made *luna fixa* (as artifices call that silver, which wanting but the tincture of gold abides the trial of aqua fortis, &c.) or some other luciferous experiment, have since in vain attempted to do the like again; and yet have their eyes so dazzled by the gold and silver they have (either separated or) made, that they are not to be prevailed with to desist from prosecuting their uncertain hopes.

THAT divers experiments succeed, when tried in small quantities of matter, which hold not in the great, it may save you something to be advertised of; divers projectors, especially chymists, having already very dearly bought the knowledge of that truth, for oftentimes a greater and unwieldy quantity of matter cannot be exposed in all its parts to a just degree of fire, or otherwise so well managed, as a less quantity of matter may be ordered. But this is so manifest a truth to those, that have dealt much in experiments, that whereas many chymists would be vastly rich, if they could still do in great quantities what they have sometimes done in little ones, many have undone themselves by obstinately attempting to make even real experiments more gainful.

I HAVE not been very solicitous to subjoin particulars to the foregoing observations, because that by reason of the contingency of such experiments, as would be the most for my present purpose, you might possibly be tempted to lose toil and charges upon trials, very likely not only to delude your hopes, but perhaps to make you distrust the fidelity of our relations. Yet for illustration-sake of what we have delivered, I am willing to mention some few contingent experiments, that occur to my thoughts.

AND first, it is delivered by the Lord *Verulam* himself, as I remember, and other naturalists, that if a rose-bush be carefully cut as soon as it has done bearing, it will again bear roses in the autumn. Of this many have made unsuccessful trials, and thereupon report the affirmation to be false; and yet I am very apt to think, that the Lord *Verulam* was emboldened by experience to write as he did. To clear up which difficulty, let me tell you, that having been particularly solicitous about the experiment,

I find by the relation both of my own and other experienced gardeners, that this way of procuring autumnal roses will in most rose-bushes most commonly fail, but in some, that are good bearers, it will succeed; and accordingly having this summer made trial of it, I find, that of many bushes, that were cut in June in the same row, the greater number by far promise no autumnal roses, but one, that hath manifested itself to be of a vigorous and prolific nature, is at this present indifferently well stored with damask-roses. And there may be also a mistake in the kind of roses; for experienced gardeners inform me, that the musk-rose will, if it be a lusty plant, bear flowers in autumn without the help of cutting. And therefore that may be misattributed to art, which is the bare production of nature. And cinnamon rose-bushes do so much better thrive by cutting than several other sorts, that I remember, this last spring, my gardener having (as he told me) about mid April (which was as soon as that kind of rose-bush had done bearing) cut many of them in my garden, I saw about the middle of June store of the same bushes plentifully adorned both with buds and with blown flowers.

An uncertainty not unlike that, which we have newly taken notice of in the experiment of producing autumnal roses, has been likewise observed in the attempts, that have been made to make divers sorts of fruit grow upon the same tree. And as for differing sorts of fruits of the same denomination, as apples, pears, &c. though some severe naturalists are unwilling to believe, that they can be made to grow upon the same tree; yet we dare not imitate their severity, having lately seen various sorts of pears fed by the same tree, and elsewhere three and twenty sorts of apple-grafts flourishing upon the same old plant, and most of them adorned with fruit. Nay, and though the fruits be not of the same denomination, yet if they be of kin in nature, they may very possibly be brought to grow on the same tree: for we lately gathered ripe apricocks and ripe plums upon one tree, from which we likewise expect some other sorts of stone-fruit. But to make fruits of very differing natures be nourished prosperously by the same stock, is so difficult a thing, that we can at most but reckon it among contingent experiments. For though Pliny and *Baptista Porta* relate their having seen each of them an example of the possibility of producing on one tree great variety of differing fruits; and though such a person as the deservedly-famous astronomer Dr. Ward assures me, that he has particularly taken notice of pears growing upon an apple-tree; and I elsewhere add a resembling observation of ours; yet certainly this experiment has been for the most part but very improsperously attempted; nor have I yet ever seen it succeed above once, though tried with very much care and industry. And I remember, that this very year, in the same garden where I gathered the apricocks and plums above mentioned, I saw the cions of a pear-tree so skilfully grafted upon an apple-stock, that it flourished very much with blossoms in the spring, and gave me great hopes, that it would bear fruit this newly-past summer, but has deceived my expectation; as divers other plants so grafted in the same garden have for many years deluded the hopes of the skilful master of it, who assures me, that though divers of them did for some years successively afford promising blossoms, yet they all decayed away without bearing any of them any fruit. Which yet may seem somewhat strange, since not only we have this summer gathered pears upon graft, which a divine, to whom the garden belongs, affirmed to have been grafted upon a quince-tree; and the industrious Kircher tells us, that *Experientia docet Persicum moro infustum fructus proferre, &c. de quo nullum est dubium utpote vulgare penè*: but experience tells us, that as little as a white-thorn and a pear-tree seem of kin, a cion of the latter will sometimes prosper will, being grafted upon a stock of the former.'

Aris Mag. Lactis & fructus proferre, &c. de quo nullum est dubium utpote vulgare penè: but experience tells us, that as little as a white-thorn and a pear-tree seem of kin, a cion of the latter will sometimes prosper will, being grafted upon a stock of the former.'

To contingent experiments, *Pyrophilus*, you may, if you please, refer what is delivered by those learned writers, who affirm, that if a lixivium made of the ashes or fixed salt of a burned plant be frozen, there will appear in the ice the idea of the same plant; for we have divers times purposely made trial of this experiment without the promised success. And I remember, that in the last cold season, proper for such trials, I purposely made a lixivium of fair water and salt of wormwood; and having frozen it with snow and salt after the manner of congelation elsewhere declared, I could not discern in the ice any thing more like to wormwood than to several other plants. And having about the same time, and after the same manner, exposed to congelation a thin phial full of a strong decoction of wormwood (from which an idea of the plant may be more probably expected) those, to whom I shewed it, after it was frozen, could discern as little like wormwood in it as myself. It is true, that in both these phials the ice seemed somewhat oddly figured; but it is true also, not only that we have observed that water, wherein a saline body, as salt petre, or sea-salt, or sugar, &c. has been dissolved, has afforded us ice, which seemed to shoot into several figures, but even in ordinary water congealed we have often seen ice figured, as if the water had been no elementary body; which needs not be admired since (to omit other causes, which may concur to the production of this effect) many waters gliding through earths abounding in saline particles of this or that nature, may be easily, in their passage, impregnated with them; whence perhaps it comes to pass, that dyers find some waters very fit, and others very unfit for the dying of scarlet and some other colours. And therefore we cannot but think, that the figures, that are oftentimes to be met with in the frozen lixivium or decoction of a plant, will afford but uncertain proofs, that the idea of each, or so much as of any determinate plant, displays itself constantly in that frozen liquor. And I much fear, that most of those, that tell us, that they have seen such plants in ice, have in that discovery made as well use of their imagination as of their eyes. And it is strange to observe what things some men will fancy, rather than be thought to discern less than other men pretend to see. As I remember Mr. R. the justly famous maker of dioptrical glasses, for merriment telling one, that came to look upon a great tube of his of thirty foot long, that he saw through it in a mill six miles off a great spider in the midst of her web; the credulous man, though at first he said he discerned no such thing, at length confessed he saw it very plainly, and wondered he had discovered her no sooner. But yet, *Pyrophilus*, because two or three sober writers do seriously relate some stories of that nature upon their own observation, I am content for their sakes to reckon their experiments rather among the contingent than the absolutely false ones: for it is not impossible, but that among the many figures, which frozen liquors do sometimes put on, there may appear something so like this or that plant, that being looked upon with the favourable eye of a prepossessed beholder, it may seem to exhibit the picture of the calcined vegetable: and we ourselves, not very long since, setting to freeze in snow and salt a fine green solution of good verdegrease which contains much of the saline parts of the grapes coagulated upon the copper by them corroded) obtained an ice of the same colour, wherein appeared divers little figures, which were indeed so like to vines, that we were somewhat surprized at the experiment; and that which increased our wonder was, that another part of the same solution being frozen in another phial by the bare cold of the air, afforded us an ice angularly figured as we have observed the ice of saline liquors oftentimes to be) but not at all like that made by the application of snow and salt. And having, for further trial sake, suffered that ice, wherein the vines appeared to thaw of itself, and having then frozen the liquor a second time in

the same phial, and after the same manner as formerly, we could not discern, in the second ice, any thing like that, which we had admired in the first. And in wine and vinegar, as much as those liquors partake of the nature of the vine, we have not, after congelation, observed any peculiar resemblance of it in figure.

The mention we have been making of ice brings into my memory another experiment, which may perhaps be reckoned likewise among contingent ones, and that is the experiment of burning with ice as with a glafs lens; which though some eminent modern writers prescribe to be done, without taking notice of any difficulty in it, yet both we and others, that have industriously enough tried it, have met with such defeating circumstances in it, especially from the uniform texture wont to be met with in moist ice, that the making of such burning-glasses may be well enough referred to those experiments, whose constant success is not to be relied on, as we elsewhere more particularly declare.

In the trade of dying there is scarce any tinging ingredient, that is of so great and general use amongst us as woad or glastum; for though of itself it dye but a blue, yet it is used to prepare the cloath for green and many other of the fadder colours, when the dyers have a mind to make them permanent, and last without fading: but yet in the decocting of woad to make it yield or strike its colour, there are some critical times and other circumstances to be observed; the easy mistake of which oftentimes defeats the dyer's expectation to his very great los, which sometimes he knows not to what to impute, of which I have heard several of them complain: and therefore divers of our leis-expert dyers, to avoid thole hazards, leave off the use of woad, though growing plentifully enough here in *England*, and instead of it employ indico, though it cost them dearer, as being brought hither sometimes from *Spain*, sometimes from the *Barbadoes*, and oftentimes even from the *East-Indies*.

Our *London* refiners, when, to part silver and copper, they dissolve those mixed metals in aqua fortis, are wont afterwards to dilute the glutted menstruum with store of fair water, and then with copper-plates to strike down the dissolved silver. But because by this manner of proceeding much copper is wont, after the separation of the silver, to remain in the menstruum, as may appear by its high tincture, that this thus impregnated liquor may be improved to the best advantage, they are wont to pour it upon what they call whiting which is said to be a white chalk or clay finely powdered, cleansed, and made up into balls) wherewith the tinted parts incorporating themselves, will, in some hours, constitute one sort of verditer fit for the use of painters, and such other artificers as deal in colours, leaving the remaining part of the menstruum an indifferently-clear liquor; whence they afterward, by boiling, reduce a kind of salt-petre fit, with the addition of vitriol (and some fresh nitre) to yield them a new aqua fortis.

And these things I mention, *Pyropbilus*, that you may know what I mean, when I tell you, that sometimes the refiners cannot make this verditer for a great while together, and yet cannot tell, whence their disability to make it proceeds. Of which contingency I remember I lately heard one of the eminentest and richest of them sadly complain, affirming, that neither he, nor divers others of his profession, were able, not long since, to make verditer for divers months together; and that several others were yet at a los in reference to that particular: though for his part he had, without knowing the cause of this contingency, found a remedy for it, namely, to warm the menstruum well before it be poured on the whiting; on which, when the liquor was warm, the tinted parts would fasten, though they would not, whilst (according to the custom of refiners) it was poured on cold.

MAKING

MAKING likewise the other day a visit to the chief copperas work we have in *England*, one of the overseers of it, who went along with me to shew me the contrivance of it, assured me, that divers times, by the mistake or neglect of a circumstance in point of time, they had lost, and are yet subject to lose, some thousands of pounds of vitriol at a time, which in spite of their wonted, but not sufficiently attentive and skilful care, would degenerate into an unctuous substance, not to be reduced into good vitriol again; unless by the tedious way of throwing it abroad, and exposing it with the unprepared stones, from which they draw their vitriol, to the rain and sun to be opened anew, and fitted for the yielding of vitriol after the same manner with those crude minerals.

UPON this occasion I must not omit, because much conducing to the scope of our present discourse, a memorable relation, that I have met with in the Indian History of the learned *Josephus Acosta*, who diligently surveyed the famous and almost inimitable mines of *Peru*, and (for one that was not a chymist) has delivered divers considerable and judicious observations about them. That which I am now to mention, is in that chapter, where he treats of the silver of the *Indies*, set down in these words: ‘ It is strange to see not only the difference betwixt the refining of metal by fire, and without it by quick-silver, but also that some of these metals, which are refined by the fire, cannot well be molten with any artificial wind, as with bellows, but when it is kindled, and blown with the natural air or natural wind. The metal of the mines of *Porco* is easily refined with bellows; and that of the mines of *Potosi* cannot be molten with bellows, but only by the breath of their guayars, which are small furnaces upon the sides of the mountains, built expressly where the wind lies, within the which they melt this metal: and though it be hard to yield a reason of this difference, yet it is most certain and approved by long experience.’

If there be any trade that obliges the artificers to be assiduously conversant with the materials they employ, it is that of the glafs men; and yet even to them, and in their most ordinary operations, there happen now and then little accidents, which, though they know not well to what to ascribe, are not yet capable of hindering them from doing sometimes what they have done a thousand times. And I remember, that among the last times I have been at a glafs-house, an eminently-skilful workman, whom I had purposely engaged to make some vessels for me, that required more than ordinary dexterity, was not able, when I came thither, to make metal (as they call that colliquated mixture of sand and fixt salt, whereof they blow their glases) tolerably fit to be employed: wherefore he desired me to take the pains to come again another day, and he would try to repair his unluckines. But the next time I came, though it were upon appointment, his metal proved again unserviceable, and instead of being colourles, when it was cold, looked as if it had been stained with blue and yellow, and was besides brittle than it ought to have been. So that it need be no such wonder, if philosophers and chymists do sometimes miss of the expected event of an experiment but once, or at least but seldom tried, since we see tradesmen themselves cannot do always, what, if they were not able to do ordinarily, they could not earn their bread.

It is affirmed by *Helmont* and others, that treat of the *Lapides Cancrorum*, that they grow within the skulls of those craw-fishes, from whence they have their name: but I have known good anatomists complain, that they have sought them in vain in the heads of those fishes, which may well make them distrust the veracity of those, that ascribe them to that sort of animals; yet we have often taken those stony concretions

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out of the heads of craw-fishes. But passing lately through *Hungerford*, a town famous for the plenty of such kind of fish, we made diligent enquiry concerning their nature, and were there informed by those that looked to them, that the concretions above-mentioned are to be found in their heads but about that season of the year, wherein they shift their shells, and that at other times of the year, several persons had in vain endeavoured to store themselves with crabs eyes at *Hungerford*. And indeed, having at the last time of my being there (which was about the latter end of June) caused divers large ones to be taken out of the water, we found these little stones but in the head of one of them; whereas about a fortnight before, which was near the summer solstice, passing by that place, we found in the wonted parts of the heads several such concretions, as to bigness and shape, but so soft, that we could easily crush and discind them betwixt our fingers. And certainly the mistake of the circumstance of time has much prejudiced the reputation of many truths: and I remember, that *Afilius*, to whose anatomical fortune the world is so much beholden, ingenuously acknowledges, that he had like to have lost the discovery of the milky veins, because having at first suspected those unlooked-for white vessels, which he took notice of in the mesenterie of a dog dissected alive, to be some irregular ramifications of nerves, he was much confirmed in his conjecture by the next dog he opened; for having dissected him at an inconvenient distance of time from the dog's repast, the slender vessels he looked for being destitute of the chyle, which is it, that makes them conspicuous, did not appear. So that he had lost the benefit of his first lucky observation, had not his sagacity inclined him to suspect, that if a dog was plentifully fed at a convenient distance of time before his being dissected, the vessel swelled with alimental juices would be the better discernible: whereupon, having feasted another dog some hours before he opened him, he manifestly detected those milky vessels, whose discovery has since set anatomists so usefully on work.

BUT, *Pyropilus*, not to exceed the limits of an essay, I must not multiply instances of the contingencies of experiments, but content myself to tell you in general, that in divers cases such circumstances as are very difficult to be observed, or seem to be of no concernment to an experiment, may yet have a great influence on the event of it. If on either of the extremes or poles of a good armed load-stone, you leisurely enough, or divers times, draw the back of a knife, which has not before received any magick influence, you may observe, that if the point of the blade have in this affliction been drawn from the middle of the æquator of the load-stone towards the pole of it, it will attract one of the extremes of an equilibrated magnetick needle; but if you take another knife, that has not been invigorated, and upon the self-same extremity or pole of the load-stone thrust the back of the knife from the pole towards the æquator, or middle of the load stone, you shall find, that the point of the knife bas, by this bare difference of position in the blade, whilst it past upon the extreme of the load-stone, acquired so different a magnetick property, or polarity, from that, which was given to the former knife by the same pole of the load-stone, that it will not attract, but rather seem to repel or drive away that end of the magnetick needle, which was drawn by the point of the other knife. And this improbable experiment not only we have made trial of, by passing slender irons upon the extremities of armed load-stones, the breadth of whose steel caps may make the experiment somewhat less strange; but we have likewise tried it by afflictions of such irons upon the pole of a naked terella, and we found it to succeed there likewise: how strange ever it may seem, that the same point or part of the load-stone should imbue iron with contrary properties, barely as they are, during their passing over it, drawn from the æquator

of the load-stone, or thrust towards it. But whether, or how far this observation insinuates the operation of the load-stone to be chiefly performed by streams of small particles, which perpetually issuing out of one of its poles, do wheel about and re-enter at the other; we shall not now examine, (though this seem one of the most likely phenomena we have met with, to hint a probable magnetical hypothesis) contenting ourselves to have manifested, by what plainly appears, how much influence a circumstance, which none but a magnetick philosopher would take notice of, may have on an experiment. We have also, with pleasure, observed, how artificers in the tempering of steel, by holding it but a minute or two longer or lesser in the flame (or other competent heat) do give it very differing tempers, as to brittleness or toughness, hardness or softness: for as when it is taken out of the flame to be extinguished, it looks either red, yellow or blue; so they esteem and find it fit to make knives, engraving tools, or springs for watches, &c. and yet it passes from one colour to another so swiftly, that none but an artifit expert in tempering of iron would suspect, that so small a difference of time of its stay in the flame could produce so great a difference in its tempers. On which occasion, *Pyrophilus*, I call to mind, that making a while since some trials concerning gravers in the shop of a famous artificer, he presented me, as a great rarity, a graver (which I yet keep) that would make the usual experiments about tempering of gravers appear false to him, that should never try them but upon it; for with all the care, wherewith I tried upon it the known ways of softening gravers, I could not soften this: which men eminently skilled in these matters (together with the person that made it) affirmed to have been made of *Damasceno-steel*, the strength whereof in cutting iron I have (not without some wonder) made trial of. But whether this singularity, which we have mentioned in this graver, proceeded from the nature of the steel, or from the temper, that it had afterward given it, is not yet agreed upon by those skilful men, to whom I have shewed it: but one of them, who by making instruments for navigators, has had the opportunity of making more than ordinary inquiry into matters of this nature, assures me, that he can easily soften this kind of steel, by only taking it off the fire at a certain nick of time, differing from that, which is wont to be observed in order to the softening of common gravers. And who knows, but that in many other experiments, seemingly despicable and unheeded circumstances may be of great concernment, though by reason of want of such particular observations, as the frequent dealing with the same body has given magnetick philosophers and artificers occasion to make, men have not yet taken notice of their importance?

To give you one instance to this purpose, *Pyrophilus*, let me take notice to you, that divers planters of fruit-trees have with wonder observed, that some grafts of cherry-trees, for example, have borne fruit the same year that they were grafted, (nay, I have observed some plants to bear fruit the same quarter of the year) and others not till the year after their infition, though neither in the goodness of the graft, nor in that of the stock, they had observed any disparity, to which the difference abovementioned could be ascribed; and therefore the bearing or not bearing of the cions of a cherry-tree the first year of its infition is by many gardeners looked upon as a thing miserly contingent. And yet indeed it scarce deserves to be reckoned among such contingent experiments, as we have been hitherto treating of; for I am informed by the trials of more than one of the most skilful and experienced grafters of these parts, that a man shall seldom fail of having cherries borne by his graft the same year, in which the infition is made, if he take care, that his graft, which must be of a good kind, have blossom-buds, as they are wont to be called, upon it: whereas

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whereas if it were only leaf-buds, as they may be termed, it will not bear fruit till the second season. And this not being taken notice of by vulgar gardeners, makes them, as we have said, impute a needless contingency to the fruitfulness of such kind of grafts. Now to discern such buds as are fit to produce blossoms, from such as will display themselves but in leaves, is no difficult matter, the former sort being more full, and big, and round than the latter, which are wont also to lie more flat and close to the graft. And it was, *Pyrophilus*, such observations as this, that induced us, after the beginning of the former essays, to discriminate from such contingent experiments as those, wherein the cause of the contingency is very arbitrary and difficult to be discerned, such other experiments, whose seeming contingency proceeds from more easily discoverable causes; for such, by diligent observation of circumstances, may be reduced to a greater certainty than the others seem capable of. Though I dare not deny, that even divers of those contingent experiments, which to us yet seem to belong to the first sort, by men's future skill and diligence in obseruation, may be made fit to be reduced to the second sort.

BEFORE I leave this subject, *Pyrophilus*, I dare not omit to say something to you of the *Virgula Divina*, or rather *Divinatoria*, by which many mineralists pretend to discover the latent veins of metals. Some use forked hazel, whose horns they hold by the ends one in each hand; and others content themselves to chuse a hazel rod (which some will have to be all of the same year's shoot) and this they bind on to another straight stick of any other wood, and walking softly with it over those places, where they suspect the bowels of the earth to be enriched with metals, they say, that if they pass over a metalline vein, the wand will, by bowing towards, discover it. And some dealers in metals I know, who affirm, that by holding the metals successively in that hand, wherein a man holds the rod, he may discover what determinate metal is predominant in the vein: for when he puts into his hand that metal, wherewith the mine chiefly abounds, the wand will manifestly bow more strongly, than when it is held in the hand with any other metal. What to determine concerning the truth of this perplexing experiment, I confess I know not. For *Agricola* himself, after a

De re metallica, lib.
20. p. 48.
lib. 3. pars rerum nature peritum & prudentem, furcam sibi usui non esse, sed, ut supra dixi, habet natura venarum signa, que obseruat. The diligent *Kircherus* informs us in his *Arte Magnetica*, that having exactly tried the experiment with metals, (for he mentions not his having tried it with mines) he could not find it in any measure succeed; and we ourselvses having several times made trial of it in the presence of the confident assertors of the truth of it, could not satisfy ourselvses, that the wand did really stand either to the metals, when placed under it, or to the metalline veins, when we carried it over mines, whence metalline ore was at that very time digging out. But on the other side, divers good authors, and even our diligent country-man *Gebriel Plat*, though wont to be somewhat too severe to chymists, does ascribe very much to this detecting wand; and divers perfsons, in other things very far from credulous, have as eye witneses with great asseverations asserted the truth of the experiment before us: and one gentleman, who lives near the lead-mines in *Somersetshire*, leading me over those parts of the mines, where we knew that metalline veins did run, made me take notice of the stooping of the wand, when he passed over vein of ore, and protested, tha. the motion of his hand did not at all contribute to the inclination of the rod, but that sometimes, when he held it very f st, it would bend so strongly as to break in his hand. And to convince me, that he believed himself, he did, upon the promises made

made him by his stooping wand, put himself to the great charge of digging in untried places for mines, (but with what success he has not yet informed me.) Among the miners themselves I found some made use of this wand, and others laughed at it. And this I must take notice of, as peculiar to this experiment, that the most knowing patrons of it confess, that in some men's hands it will not at all succeed, some hidden property in him that uses the wand being able, as they say, to overpower and hinder its inclinatory virtue. To which I must add what a very famous chymist, who affirms himself to have tried many other things with it besides those that are commonly known, very solemnly professed to me upon his own knowledge; namely, that in the hands of those very persons, in whose hands the rod will (as they speak) work, there are certain unlucky hours, governed by such planets and constellations, (which I confess I believed not enough to remember their names) during which it will not work, even in those hands, wherein at other times it manifestly will. But of this experiment I must content myself to say, what I am wont to do, when my opinion is asked of those things, which I dare not peremptorily reject, and yet am not convinced of; namely, that they that have seen them can much more reasonably believe them, than they that have not.

Nor is it only in experiments, *Pyrophilus*, but in observations also, that much of contingency may be: witness the great variety in the number, magnitude, position, figure, &c. of the parts taken notice of by anatomical writers in their dissections of that one subject the human body, about which many errors would have been delivered by anatomists, if the frequency of dissections had not enabled them to discern betwixt those things, that are generally and uniformly found in dissected bodies, and those which are but rarely, and (if I may so speak) through some wantonness, or other deviation of nature, to be met with. I remember, that a while since being present at the dissection of a lusty young thief, we had opportunity to observe, among other things, that the interval betwixt two of his ribs was near the back-bone filled up with a thick bony substance, which seemed to be but an expansion of the ribs, and appeared not to have grown there upon occasion of any fracture, or other mischance. About the same time being at a private dissection of a large and young human body with some learned men, an ingenious person, professor of anatomy, there present, chancing to cut a great nerve, spied in the substance of it a little of a very red liquor, which he immediately shewed me, as wondering what it might be: but I concluding it to be blood, presently suspected that it might have proceeded from some small unheeded drop of blood wiped off by the brushy substance of the nerve from the knife wherewith it was cut. Wherefore carefully wiping a dissecting knife, I did in another place cut the nerve asunder, and found another very little drop of pure blood in the substance of it as before. This I did again elsewhere with like success, shewing it to the by-standers, who admired to see a vessel carrying blood (for such they concluded it to be) in the body of a nerve, in regard they remembered not to have ever met with such an accident; though I let admire it, because I have in an ox's eye or two observed in that coat, which the moderns commonly call the retina, and which seems to be but an expansion of the pith of the optick nerve, little turgent veins manifestly full of blood.

We further observed in that lately mentioned body, in which we took notice of the irregular conjunction of two ribs, that the lungs, which were very sound, had a supernumerary lobe on one side, which did so little differ from its companions, that we did not, till we had displayed the lungs, take notice of it. And I remember, that a while before, being invited by a company of physicians to a private dissection, and the

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the lungs, which otherwise seemed not unsound, appearing in divers places fastened to the ribs, two ingenious anatomists, that were there present, did so little agree in their observations concerning such cases, that the one affirmed, that he had never seen any lungs (which had not been excessively morbid) tied to the thorax; and the other protested, that he had scarce ever opened a diseased body, wherein the lungs did not so adhere. But if it were not improper to mind a young gentleman of venereal observations, I could easily give you an eminent proof of the disagreement of anatomical observers, by insiting on the controversy betwixt the famous writers on that subject, concerning the anatomical notes or tokens of virginity; many eminent authors affirming, that they have seldom failed of finding them in one amongst very many dissected maids; and many other artists, both conspicuous and experienced, preremptarily professing, that they have seldom or never met with the pretended marks in persons even of the most undoubted virginity. And certainly it is very strange, that about a matter, which seems so easily determinable by sense, accurate and sober men should so widely disagree; as that the one should profes he has very rarely, if ever, met with in a human body, what another affirmeth himself to have as seldom, if ever, missed. But because, *Pyrophilus*, this subject is, perhaps, somewhat improper to be insisted on either to, or by, a young man, I shall pass on to tell you, that amongst the accuratess of our modern writers, I suppose you will readily allow me to reckon Dr. *Harvey* and Dr. *Higmore*; and that though in their excellent treatises of generation they both insist on the production and changes observable in hens eggs, as the patterns, whereunto the generation of other annimals may be referred; yet have we divers times, in the progres of nature in her formation of a chick, observed considerable variations in point of time and other circumstances (though in the main our observations commonly agreed) from what is by them delivered: which diversity may easily proceed from the different constitution of hens, their differing assiduity in fitting on their eggs, the differing qualifications of the eggs themselves, and several other particulars of the like nature. And I remember, that the other day taking notice of this to my learned friend Dr. *Higmore*, he readily acknowledged to me, that he himself had likewise observed divers circumstances in eggs whilst they were hatching, which varied from those set down by him in his book; though he had there accurately expressed the changes he discerned in those eggs, which at the same time afforded him his observations. And indeed there are certain things of such a nature, that scarce any single man's accurateness in making a single observation about them can secure him from appearing unskilful or unfaithful in his observations, unless those, that shall afterwards examine them, chance to be endowed with a somewhat more than ordinary either equity, or sagacity, or both. For instance, he that first affirmed, that a needle animated by a loadstone did constantly convert its extremes to the opposite poles of the earth, could scarce suspect himself of having delivered any thing, which he had not carefully tried. And yet of those pilots, *Gonzales Oviedo* and *Sebastian Cabot*, (who are said to have in *America* first taken notice of the declination of the mariner's needle) he that did first in those far distant parts of the world compare the meridian line afforded by magnetical needles with one mathematically drawn, (which may be readily found by accurate sun dials) and thereby observe the variation of the needle, or its declination from the true meridian line, might easily conclude the observer formerly mentioned to have been faulty, by reason of his finding the needle's variation differing (perhaps by divers degrees) from that delivered by the first observer. And this second man's observation might appear to have been as carelessly made to a hundred other observers, if

if the observations of navigators had not made it apparent, that the declination of the needle is far from being the same in all places: for though *Cardan* (as *Kircher* and *Fracastorius*, as another informs us) be pleased to affirm, that the loadstone declines <sup>*Fracastorius*
Hydro. I. 11.</sup> as many degrees, as the pole-star is distant from the pole of the world; yet besides divers reasons, common experience sufficiently manifests the inconsiderableness (not to speak more harshly) of that assertion. For about the islands of the *Azores*, especially that of *Cerro*, over which the first meridian is by many supposed to pass, the magnetick needle hath been observed directly to respect the poles, without any sensible declination from them; but in other places it is wont to vary sometimes eastward, sometimes westward, more or less. Insomuch that not only our venturesome countryman Captain *Thomas James* observed it in 63 degrees north-latitude to be no less than 27 degrees, 48 minutes; but a learned mathematical writer, that is lately come forth, makes the declination at the *Fretum Davis* to amount to, what is almost incredible, 50 degrees. And this deflexion of the needle sometimes to one side of the meridian, sometime to the other, happens with so much seeming irregularity, as has made both the diligent *Kircher* himself, and divers other magnetick writers, almost despair of reducing these kind of observations to any general hypothesis.

To which we may add, that perhaps very few even of the exactest observations of this nature made an age since, would now appear accurate to them, that should try them in the self-same places wherein, and the self-same manner after which they were formerly made. So that the diligentest of those observers would appear to us to have been negligent, if the sagacity of some of their successors had not prompted them to suspect, that even in the same place the needle's variation may vary. And I remember, that having not long since enquired of an English contriver of mathematical instruments for the use of seamen, what he had observed concerning this alteration of the needle's variation, he told me, that by comparing of ancient and modern observations made by himself and other accurate mathematicians at *London*, he had found the declination constantly to decrease, and, as he conjectured, about 12 or 13 minutes (though that methinks be much) in a year. And it will be yet more difficult to set down any observation of this nature, which will appear exact to posterity, if that strange thing be true (as it may well be) which was related to *Kircher* by a friend of his, who affirms himself to have observed a notable change of the needle's variation at *Naples*, after a great *incendium* of the neighbouring mountaine *Vesuvius*; which alteration he not absurdly suspects to have proceeded from the very great change made in the neighbouring subterraneal parts by that great conflagration. And it seems the same observation has been taken notice of by mathematicians elsewhere. For the learned Jesuit *Fournier* in his French hydrography tells us in more <sup>*Livres 11.*
chap. 10.</sup> general terms, that since the *incendiums* of *Vesuvius* the declination (of the needle) has notably changed in the kingdom of *Naples*. The same author somewhere delivers what (if it be true) is remarkable to our present purpose, in these words: 'There are persons, who have observed, that the same needle, that declined 5 degrees upon the surface of the earth, being carried down very low into certain caves, declined quite otherwise.' I added those words, if it be true, not to question the veracity of the author, but because it is very possible the makers of the observation (though learned men) may have been mistaken in it, without suspecting themselves in danger of being so. For I should scarce have imagined, unless my own particular observation had informed me, in how great a variety of stones and other fossiles the ore of iron may lurk disguised: so that it is no way incredible, that knowing chymists themselves, and much more mathematicians and others, not being aware

aware of the observation of what I have newly delivered, may presume, because they saw not in the deep caves abovementioned any minerals like the vulgar iron ore, that there is nothing of that metal there, when indeed there may be enough to occasion that deflexion of the needle; which (especially if it be strongly excited) may be often drawn aside by iron or other magnetick bodies, at a greater distance than those, that have not tried, will be apt to suspect. Which may perhaps be the reason, why in the little island of *Hiva* (upon the coast of *Italy*) where they dig up iron and store of loadstones, of which I have seen in *Tuscany* of a prodigious bigness, there is indifferent, but neighbouring places, such a strange disparity of the needle's variation as curious men have recorded.

Nor are magnetical and anatomical observations the only ones, which are subject to disagree now and then, without the negligence of those that make them: but I want time, and I fear you would want patience, to consider at present as many of them, as might be easily enumerated to you.

I suppose, *Pyrophilus*, you may have observed, how I in the past discourse have forborn to insist on medicinal experiments; which I have purposely done, because they are so many, and almost all of them subject to such uncertainties, that to insist on them would require much more time, than my occasions will allow me to spend upon this essay. And indeed in physick it is much more difficult than most men can imagine, to make an accurate experiment: for oftentimes the same disease proceeding in several persons from quite differing causes, will be increased in one by the same remedy by which it has been cured in another. And not only the constitutions of patients may as much alter the effects of remedies, as the causes of diseases; but even in the same patient, and the same disease, the single circumstance of time may have almost as great an operation upon the success of a medicine, as either of the two former particulars; as we may elsewhere have occasion by sundry instances to manifest. But besides the general uncertainty, to which most remedies are subject, there are some few, that seem obnoxious to contingencies of a peculiar nature: such is the sympathetick powder, of which not only divers physicians and other sober persons have assured me they had successfully made trial, but we ourselves have thought, that we were eye-witnesses of the operation of it; and yet not only many, that have tried it, have not found it answer expectation, but we ourselves trying some of our own preparing on ourselves, have found it ineffectual, and unable to stop so much as a bleeding at the nose; though upon application of it a little before, we had seen such a bleeding, though violent, suddenly stopped in a person, who was so far from contributing by his imagination to the effect of the powder, that he derided those that he saw apply it to some of the drops of his blood. Wherefore that the sympathetick powder and the weapon-salve are never of any efficacy at all, I dare not affirm; but that they constantly perform what is promised of them, I must leave others to believe. But making mention of remedies of this nature, though I am willing, *Pyrophilus*, to put a period both to your trouble and my own, yet I must not omit to tell you, that whereas the peony-root has been much commended both by ancient and modern physicians of no mean account, as an amulet against the falling sickness, and yet has been by many found ineffectual; we have been apt to suspect, that its inefficacy, if it be but infrequent, might possibly proceed from its having been unseasonably gathered: and when I was last in the west of *Ireland*, acquainting the eminentest of the Galenists there with my conjecture, he confirmed me in it, by assuring me, that he had often tried the peony root unseasonably gathered without success; but having lately gathered it under its proper constellation, as they speak, (which is when the decreasing

decreasing moon passes under *Aries*) and tied the slit root about the necks and arms of his patients, he had freed more than one, whom he named to me, from epileptical fits. Agreeable whereunto I find, that a famous physician of *Grenoble*, Monsieur des *Grands Prez*, in the laft of his obſeruations communicated to the famous practical physician *Riverius*, ſolemnly profeſſes his having divers times freed his patients from the falling-fickneſſe by the ſingle outward application of paenony-roots, collected and applied as is above mentioned. But though he thence infers the uſeulneſſe of obſerving ſtars in the practice of phyſic, yet before much weight be laid upon ſuch improbable notions, as moſt of thoſe judiciary astrologers, the influence of conſteſtations upon ſimples, &c. ought by ſevere and competent experiments to be better made out than hitherto it has been.

BUT to ſay no more of the contingent obſeruations to be taken notice of in trials medical, I could tell you, that I have obſerved even mathematical writers themſelves to deliver ſuch obſeruations as do not regularly hold true. For though it hath been looked upon as their privilege and glory to affirm nothing, but what they can prove by no leſs than demonstration; and though they uſed to be more attentive and exact, than moſt other men in making almost any kind of philosophical obſervation: yet the certainty and accuratenefs, which is attributed to what they deliver, muſt be reſtrained to what they teach concerning thoſe purely-mathematical disciplines, arithmetic and geometry, where the affections of quantity are abſtractedly conſidered: but we muſt not expect from mathematicians the same accuratenefs, when they deliver obſeruations concerning ſuch things, wherein it is not only quantity and figure, but matter, and its other affections, that muſt be conſidered. And yet leſs muſt this be expected, when they deliver ſuch obſeruations, as, being made by the help of material instruments framed by the hands and tools of men, cannot but in divers caſes be ſubjeſt to ſome, if not many, imperfections upon their account. Divers of the modern astronomers haſe ſo written of the ſpots and more ſhining parts or (as they call them) *Facule*, that appear upon or about the ſurface of the ſun, as to make their readers preſume, that at leaſt ſome of them are moſt always to be seen there. And I am willing to think, that it was their having fo often met with ſuſh phænoſena in the ſun, that made them write as they did. And yet when I firſt applied myſelf to the contemplation of theſe late diſcoveries, though I wanted neither good teleſcopes, nor a dark room to bring the ſpecies of the ſun into, yet it was not till after a great while, and a multitude of fruitleſs obſeruations made at feſeral times, that I could detect any of theſe ſolary ſpots, which having during many months at leaſt appeared fo muſh ſeldomeſ than it ſeems they did before, that I remember a moſt ingenious professor of astronomy, excellently well furnished with dioptrical glaſies, did about that time complain to me, that for I know not how long he had not been able to ſee the ſun ſpotted. And as for the *Facule*, that are written of, as ſuſh ordinary phenomena, I muſt profeſſ to you, *Pyropbilus*, that a multitude of obſeruations made with good teleſcopes, at feſeral places and times whiſt the ſun was ſpotted, haſe ſcarce made me ſee above once any of the looked for brightneſſeſ.

AND as the nature of the material objects, wherewith the mathematician is converfant, may thus deſceive the expeſtations grounded on what he delivers; ſo may the like happen by reaſon of the imperfection of the instruments, which he muſt make uſe of in the ſenſible obſeruations, whereon the mixed mathematics (as astronomy, geography, optics, &c.) are in great part buiſt. This is but too maniſt in the diſagreing ſuppoſitions, that famous writers, as well modern as antient, haſe given us of the circuit of the terreftrial globe, of the diſtance and bigneſſe of the fixed stars

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and some of the planets, nay, and of the height of mountains : which disagreement, as it may oftentimes proceed from the differing method and unequal skill of the several observers, so it may in divers cases be imputed to the greater or less exactness and manageableness of the instruments employed by them. And on this occasion I cannot omit that sober confession and advertisement, that I met with in the noble Tycho, who having laid out, besides his time and industry, much greater sums of money on instruments than any man we have heard of in latter times, deserves to be listened to on this theme, concerning which he has (among other things) the following passage :

*Tycho
Brache, l. 2.
de Cometa
An. 1577.
p. 153.*

Facile (says he) lapsus aliquis penitensibilis in instrumentis etiam majoribus conficiendis subrebit, qui inter observandum aliquot scrupulorum primorum jacturam faciat; infuper si ipse situs & tractandi modus non tam absoluta norma perficiatur, ut nihil prorsus defideretur, intolerabilis nec facile animadvertenda deviatio sepe insinuat. Adeo quod instrumenta usu & estate à prima perfectione degenerent. Nihil enim, quod hominum manus paratur, ab omni mutatione undique existit. Organis enim ejusmodi, nisi ē solidi metallo affabre elaborentur, mutationes aërea obnoxia sunt; & si id quoque detur, ut ē metallica materia conflent, nisi ingentia fuerint, divisiones minutissimas graduum non sufficientur exhibent; dumque hoc prestant, sua magnitudine & pondere se ipsa ita aggravant, ut facile tum extra planum debitum aut figuram competentem, dum circumducuntur, declinet, tum etiam sua mole intractabili redduntur. Quare magis requiritur in instrumentis astronomiis, que omni visio careant construendis, artificium pari iudicio conjunctum, quam balenus à quamplurimis animadversus est. Id quod nos ipse usus longaque docuit experientia, non parvo labore nec mediocribus sumptibus comparata.

HITHERTO our noble author. And as for the observations made at sea, the diligent Fournier advertises, that however many sea-captains and others may brag of their mathematical observations made on ship-board, yet he, upon trial of many instruments both at sea and ashore, makes bold to affirm, that no astronomer in the world can be sure to make his observation at sea within ten minutes of the precise truth, no not (says he) upon the sand itself, within one minute of it.

BUT instead of acquainting you with what may be drawn from the writings of our hydrographer, to prove, that his assertion is rather modest than too bold, I shall observe, that the observations even of skilful mathematicians may hold so little, or disagree so much, when they pretend to give us the determinate measures of things, that I remember of three very eminent modern mathematicians, who have taken upon them, by their experiments, to determine the proportion betwixt air and water, the one makes not the weight of water to exceed above 150 times that of air; the other reckons water to be between 13 and 14 hundred times; and the third no less than 10,000 times the heavier. Not to mention a modern and famous writer or two, who have been so mistaken as to think, that the weight of the water in comparison of the air is I know not how much under-reckoned, even by this last (overbold) estimate. And, if I had leisure, I could annex an experiment partly statical, and relating to the weight of the air, which though we made divers times in an hour, yet we missed of the like success twice as often in the same hour, without being able to know beforehand, whether the experiment would succeed within some pounds weight. But of this more perhaps elsewhere.

THE ends, *Pyropbilus*, which we have proposed to ourselves in setting down the things by us delivered in this and the former essay, are principally two.

AND first, we desire, that the instances we have given you of the contingency of experiments, may make you think yourself obliged to try those experiments very carefully, and more than once, upon which you mean to build considerable super-structures.

structures either theoretical or practical; and to think it unsafe to rely too much upon single experiments, especially when you have to deal in minerals: for many to their ruin have found, that what they at first looked upon as a happy mineral experiment, has proved in the issue the most unfortunate they ever made. And I remember, that the most experienced mineralist I have hitherto been acquainted with, though his skill has been rather gainful than prejudicial to him, has very seriously told me, that he could quickly grow an extraordinary rich man, if he could but do constantly whatsoever he has done, not only two or three, but many times.

The other end, *Pyrophilus*, to which I had an eye in writing the past discourses, was, that they may serve for a kind of apology for sober and experimental writers, in case you should not always upon trial find the experiments or observations by them delivered answer your expectations. And indeed it would prove a great encouragement to wary and considerate naturalists from enriching the world with their observations, if they should find, that their faithfulness in setting down what they observed is not able to protect them from blasting imputations of falsehood, but that by publishing any thing for the good of others, they must expose their reputation to all the uncertainties, to which any of their experiments may chance to prove obnoxious. It is true indeed, that if a writer be wont to be fabulous or transcriptive, and to deliver things confidently by hear-say, without telling his readers when he does so; if his experiments upon trial succeed not, we may be allowed to impute their unsuccessfulness rather to him, than to ourselves, or to chance, and need not think ourselves obliged to have so much a greater care of his reputation, than he had of his own, as for his sake to try more than once, what he for our sakes never tried so much as once. But if an author, that is wont to deliver things upon his own knowledge, and shews himself careful not to be deceived, and unwilling to deceive his readers, shall deliver any thing, as having tried or seen it, which yet agrees not with our trials of it; I think it but a piece of equity, becoming both a Christian and a philosopher, to think (unless we have some manifest reason to the contrary) that he set down his experiment or observation as he made it, though for some latent reason it does not constantly hold; and that therefore though his experiment be not to be relied upon, yet his sincerity is not to be rejected. Nay, if the author be such an one, as has intentionally and really deserved well of mankind, for my part I can be so grateful to him, as not only to forbear to distrust his veracity, as if he had not done or seen what he says he did or saw, but to forbear to reject his experiments, till I have tried, whether or no by some change of circumstances they may not be brought to succeed. Thus a while since finding in Sir Francis Bacon, that he delivers as a somewhat unlikely truth, that spirit of wine will swim upon oil (of almonds) we forthwith made trial of it, but found the oil swim upon the spirit of wine, and this upon several trials before witnesses: but our tenderness of the reputation of so great and so candid a philosopher made us to bethink ourselves, that (though he mentions it not, nor perhaps thought of any such thing, yet) possibly he may have used spirit of wine more pure than ordinary; and thereupon having provided some that was well rectified, we found, that the oil, that was wont to swim upon spirit of wine, not freed from its aqueous parts, did readily sink, and quietly lie in the bottom of that, which was carefully dephlegmed. And so having been informed, that the learned Dr. Brown somewhere delivers, that aqua fortis will quickly coagulate common oil, we poured some of those liquors together, and let them stand for a considerable space of time in an open vessel, without finding in the oil the change by him promised (though we have more than once with another liquor presently thickened common oil). Whereupon being,

being unwilling, that so faithful and candid a naturalist should appear fit to be distrusted, we did again make the trial with fresh oil and aqua fortis in a long-necked phial left open at the top, which we kept both in a cool place, and after in a digesting furnace; but after some weeks we found no other alteration in the oil, than that it had acquired a high and lovely tincture: notwithstanding which, being still concerned for the reputation of a person, that so well deserves a good one, the like contingencies we have formerly met with in other experiments, made us willing to try, whether or no the unsuccessfulness we have related might not proceed from some peculiar though latent quality, either in the aqua fortis or the oil by us formerly employed: whereupon changing those liquors, and repeating the experiment, we found after some hours the oil coagulated almost into the form of a whitish butter. Nor dare I allow myself to be confident, that I shall not need to be dealt with by you upon some occasions, with the like equity, that I have been careful to express towards others. And since the writing of thus much of this very essay, having desired a very skilful and candid chymist to do me the favour to provide me some of the purest and strongest spirit of salt, that could be made; he kept some salt in a vehement fire for divers days and nights together, and freed the extracted liquor so carefully and so skilfully both from its phlegm and its terrestrial faeces, that after all I have written in the former essay concerning that menstruum, I must freely confess to you, that I am now satisfied, that a spirit of sea salt may without any unsincerity be so prepared, as to dissolve the body of crude gold, though I could not find, that the solutions I made of that metal were red, but rather of a yellow or golden colour, much like those made with common aqua regis. But neither this artist nor I have been since able to make another spirit of salt capable of dissolving gold, notwithstanding all the industry we have employed about it; which makes me refer this to contingent experiments; unless the prosperous event of our former trial may be ascribed to the quality of the salt, that was distilled, which was brought from the island of Mayo, where the scorching sun makes out of the sea-water a salt, that is accounted much stronger and more spirituous than that, which is wont to be made in France and other more temperate climates. And let me, *Pyrophilus*, take this opportunity to add, that if I had not very cautiously set down the observation I related in another essay * concerning the little fishes or worms I there teach you to discover in vinegar, I should perhaps need much of your equity, to keep me from being thought to have imposed upon you in what I there delivered. For I have since met with divers parcels of vinegar, wherein the observation could not be made, for one wherein it held; so that I am glad to keep by me some vinegar stocked with those scarce visible animals, to satisfy ingenious men, among whom some have been fain, after their own fruitless trials, to come to me to show them the things delivered in that observation. What I mentioned a little above to have been tried upon falter-oil, puts me in mind of telling you, that among our experiments concerning the changes of colours, we were about to acquaint you with one, which we had formerly made upon commen oil-olive, it seeming to us a not inconsiderable one; since it was a way, that we devised of instantly changing the colour of the oil from a pale yellow to a deep red, with a few drops of a liquor, that was not red, but almost colourless. This experiment, as we were saying, *Pyrophilus*, we were about to set down among others concerning colours: but because we do not willingly rely on a single trial of such things, as we know not to have been ever tried before, we thought it not amiss for greater security to make the experiment

* This is one of those, that make up the book of the usefulness of *Experimental Philosophy*.

the second time, but could not then find it to succeed, nor even since upon a new trial (probably by reason of some peculiar quality in that particular parcel of liquor we first made use of) which made us think fit to omit the intended mention of it: but if I had upon my first trial acquainted you with it without any further scruple, you might upon trial have suspected, if not concluded, that I had misinformed you, though I had really delivered nothing but what I had tried. And indeed, *Pyrophilus*, though I have not the vanity to pretend to have deserved so much of you, as such naturalists as Sir *Francis Bacon* have deserved from every ingenious reader of their books; yet perhaps you will do me but right to believe, that though some of the experiments I have delivered may prove contingent, yet I have not delivered them unfaithfully, in reference to what I thought I observed in them, and remembered of them. And though I desire you should read my writings, as to give no farther assent to my opinions, than the reasons or experiments produced on their behalf require; yet in matters of fact, which I deliver as having tried or seen them, I am very willing you should think, that I may have had the weakness to be mistaken, but not an intention to deceive you.

THERE is yet one thing more, that I shall venture to acquaint you with before I conclude this essay, though you may think it relishes of a paradox, and it is this: that when I am satisfied of the abilities and circumspection of a writer, delivering a matter of fact as upon his own knowledge; I do not presently reject his observation as untrue, much less condemn the person himself as a liar, whenever I find, that it seems to be contradicted by a contrary and more undoubted observation, or to contradict a received and plausible either hypothesis or tradition; but rather try, if by fit distinction or limitation I can reconcile them; unless I can imagine something or other, which might probably lead him to mistake. And of this indulgence to an intelligent writer I have this reason to give, that sometimes there happen irregularities contrary to the usual course of things, as is evident in monsters; and sometimes the received hypothesis, though perhaps not to be rejected as to the main, will not hold so universally as men presume; and sometimes too the contradiction betwixt the observations may be but seeming (by reason of the want of some unheeded circumstance necessary to make them inconsistent) and so they may both be true.

We might dilucidate and confirm what we have newly delivered by several instances, were it not, that this essay is already but too prolix. Wherefore we shall only recommend to your consideration these few particulars.

THAT the Irish spiders (of which, whatever is vulgarly believed to the contrary, myself have in *Ireland* seen divers) are not poisonous, is not doubted by the inhabitants, who have had many ages experience of their harmlessness: and yet I dare not deny what the learned *Scaliger* somewhere affirms, that in (his country, if I misremember not) *Gascony* their venom is so pernicious, that they sometimes poison those that tread upon them through the very soles of their shoes. And that even here in *England* (though a country so near to *Ireland*) some spiders (at least) are venomous even without biting, I may elsewhere have occasion to give you an experimental proof.

It is so much taken for granted by divers authors, who pretend likewise to give reasons of it, and by the generality of their readers, that under the same meridian the magnetic needle keeps every where the same variation, without changing it by being carried northwards or southwards, that it is like, if many persons better acquainted with magnetic speculations than trials should read in the relations of the Hollanders, that under the meridian, that passes by the island of *Corvo*, where the needle

needle points directly at the poles, and which is therefore wont to be reckoned the first meridian, they found at two places, the one about 46°, the other about 55° degrees of northern latitude, a declination in the former of those elevations of no less than 7 or 8 degrees, and in the latter of a far greater number; and also that they found under the twentieth parallel of southern latitude under the same meridian of the Azores 10 or 11 degrees of declination; many, I say, if they should meet with these particulars, probably would suppose the Dutch to have been very bad observers, because these observations do not (as we intimated above) agree with the theory of the needle's declination. And yet if we confer these observations with others of the like nature, made by good navigators and other skilful men along other meridians, we may, I suppose, find cause rather to rectify the general opinion, than reject the Dutch observations for their disagreeing with it; especially if we take into consideration what is affirmed by the Jesuit *Jules Alen* (whom *Fournier*, amply treating of longitudes, extols for the accuratest observer of the needle's variation that ever was) sailing into *China* in a great Portugal caraque, and accompanied by the famous pilot *Vincent Rodriguez*, who had them made twenty-eight voyages to the Indies. For out of one of this father's letters *Fournier* has preserved this memorable passage: ' You must
 ' (says he) take notice of one thing very considerable, namely, that the further you
 ' go from the æquator in the same meridian, the greater you will find the magnetical
 ' variation.' There are some eminent modern naturalists, who affirm, that they have
 assuredly tried by weather-glasses, that cellars and other subterraneal places are colder
 in winter than in summer: and yet not to oppose to this experiment the common
 tradition to the contrary, I remember, that the bold and industrious Capt. *James* (formerly mentioned) in the relation of his strange voyage published by his late
 Majesty's command, has this notable observation, where he relates the excessive coldness
 of the water they met with in summer in that icy region, where they were forced
 to winter in the year 1632. ' Moreover our well (says he) out of which we had
 ' water in December, had none in July.'

De la Lou-
gritude, &c.
24+

Fourn. Ar-
oblectare
flavore, &c.
22.

LASTLY, though in the western parts it have been observed, that generally the inside, or heart, as they call it, of trees, is harder than the outward parts; yet an author, very well versed in such matters, treating of the building of ships, gives it us for a very important advertisement touching that matter, that they have observed at *Marseilles*, and all along the Levantine shores, that that part of the wood, that is next the bark, is stronger than that, which makes the heart of the tree. But to draw at length to a conclusion of this already too tedious essay; the ends above mentioned, *Pyrephilus*, being those, which I proposed to myself in writing the past discourse, you will make an use of it, which I was very far from intending you should, if you suffer it to discourage you from the vigorous prosecution of your inquiries into experimental knowledge. Nor indeed is any thing, that hath been said, fit to persuade you to other than watchfulness in observing experiments, and wariness in relying on them; but not at all to such a defection of mind, as may make you forbear the prosecution of them: for neither doth the physician renounce his profession, because divers of the patients he strives to cure are not freed from their diseases by his medicines, but by death; nor doth the painful husbandman forsake his cultivating of the ground, though sometimes an unseasonable storm or flood spoils his harvest, and deprives him of the expected fruit of his long toils. For as in physic and husbandry, those, that exercise them, are kept from deserting their professions, by finding, that though they sometimes miss of their ends, yet they oftentimes attain them, and are by their successes requited not only for those endeavours that succeed,

but for those that were lost; so ought we not by the contingencies incident to experimental attempts, to be deterred from making them, because not only there are many experiments scarce ever obnoxious to casualties, but even among those, whose event is not so certain, you may very probably make an experiment very often, without meeting with any of those unlucky accidents, which have the power to make such experiments miscarry. And sure the prosperous success of many succeeding attempts is well able to make amends for the fruitless pains employed on those few, that succeed not; especially since in experiments it not frequently happens, that even when we find not what we seek, we find something as well worth seeking as what we missed. Of this last mentioned truth we may elsewhere have occasion to discourse more largely; and therefore shall now conclude with barely minding you, that even merchants themselves are not wont to quit their profession, because now and then they lose a vessel at sea, and oft times their ships are by contrary winds and other accidents forced to put in at other ports than those they were bound for. Which example I the rather make use of, because that as the American navigators employed by the European merchants, having been by storms forced from their intended course, have been sometimes thereby driven upon unknown coasts, and have made discovery of new regions much more advantageous to them, than the fairest and constantest winds and weather could have been; so in philosophical trials, those unexpected accidents, that defeat our endeavours, do sometimes cast us upon new discoveries of much greater advantage, than the wonted and expected success of the attempted experiment would have proved to us.