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Document Description: “Of the Unsuccessfulness of Experiments” by Boyle

Dated: undated

Archive: unknown

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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 profitablest 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 doubling Naturalist.

The First ESSAY,

Of the Unsuccessfulness of EXPERIMENTS.

I AM very sorry, *Pyrophilus*, 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, dis-

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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, Pyrophilus, 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 despondency 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 discouraged 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 grosly to distinguish the causes of that unsuccessfulness, into the particular or mistaken properties of the materials imployed 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 unskilfulness 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 unskilfulness may have miscarried.

THE materials to be employed about the experiments we are considering may also admit of several distinctions; as into natural and factitious, 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-grass, that of a greater quantity of the feed not any grass sprung up; which not being imputable

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to the soil, 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 subtilest 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* \*[[1]](#footnote-1) 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 a 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 sequestrations of phlegm, was not pure enough, to perform what we expected from from [*sic*] 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 passeth for a very honest man: but this spirit, besides its weakness, 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

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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 glass. 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 glasses (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 stiptical substance, 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, consists 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 imployed 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 progress 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

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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 saying 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 colliquation. 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 god 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 god, 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[[2]](#footnote-2), speaking of the metals of it, says, *Il y a bien,* &c. that is, there is certainly gold among the inhabitants of *Madagascar*, which has not been

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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 I this country *Voulamene Voutroüa*. He adds, that this gold, which they call god of *Malacasse*, 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 fineness from each other, and discriminated by the natives by three peculiar names. But that which he adds as most considerable, is, *that Malacassean gold is of so 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 case 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 several ores; for instance, of lead, some of which I can shew you so 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 aptness to vitrify, and serve the potters to glaze their earthen vessels, the miners call pottern-ore, 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 severer 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 cost 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 the most pure or unmixt, and as yet unwrought) I was desirous to try,

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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 cognation 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 strenghen the paradox I have proposed, by the authority of that great and candid chymist *Basilius 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 inriches the former; adds, that there is such a different 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 inriched, the possessor of it soon after imployed 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 he shewed me the cinnabar, which is not

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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 antimonies, 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, imploy themselves oftener than we in making it, divers times unsuccessfully attempt the preparing it. And it may perhaps also from some diversity either in antimonies or irons, that eminent chymists have (as we have observed) often failed in their endeavours to make the starry regulus of *Mars* and antimony. Insomuch 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 reguleses 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 stary 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 recompensed 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 tale, 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 talcs 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 show-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 alcalizate 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 suspicion among divers knowing naturalists, to whom I shewed it, for a fair solution of gold. *Paracelsus[[3]](#footnote-3)* 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 amiss, when he added in the same discourse, after he had mentioned great variety of marchasites, stones, and other minerals, *Sed & hoc verum est, in terra multa adhuc condi, quæ mihi incognita sunt, sed eadem nec alii norunt. Certum siquidem est, progressie temporis tot tamque varia à Deo adhuc proditum iri, de quibus nemo nostrum nedum unquam somniavit*.[[4]](#footnote-4)

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 less 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-

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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 sal terræ; generated in very differingly-qualified parcels of earth, may probably receive divers qualities from the particular soil, wherein it grows, though these qualities lie concealed and ususpected 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 exotick 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 qua-

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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, *Pyrophilus*, to put you in mind of taking notice, what a great change is made in that liquor, when upon the recess 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, discern no change in them? as may daily be observed in the superannuated 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 several 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 indiscriminately 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 distil it often in great quantities, assure 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 differing strength of urines, though it be very probable, is not so easily to be satisfactorily made out, we shall rather insist on this other observation confirmed to us by experience, 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 avolation 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 extricating themselves from the rest, will even upon a very gentle heat (as trial made on purpose has informed us) fly away, and leave a phlegmatick and unactive liquor behind them. In confirmation whereof I must acquaint you, *Pyrophilus*, with what lately befell 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 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 fie 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, that the covers of the vessels had not been, by him I employed to put them in, well luted on, and besides were I some placed cracked, I suspected, that the heat of the dunghill had not only looseed the saline parts of the liquor, but driven them away: and accordingly by dutilling it in a very gentle heat, and in a very high cucurbit, we obtained, instead of an active and saline spirit, a languid and nauseous 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 business of colours, and is acquainted with

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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, *Pyrophilus*, 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 menstruums 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 smell 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 amber, we poured some drops of it into beer, or water, into which the spirit of wine suddenly diffusing 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 likewise, 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 subsiding salt was by the interposition of that saline liquor protected from the action of the spirit of wine; which being by this new way dephlegmed, 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, *Pyrophilus*, 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 siege 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 allayed 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 sky-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 deceive 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 soever it is reputed, is not altogether free from the pollutions of other metals; for our gold-beaters, though for their own profit sake they are wont to use the finest 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

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many goldsmiths and chymists, who think their gold most requisitely 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 imbase 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 lucriferous. 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 burning, singing, or as much as discolouring 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 phænomenon this plausible reason has been assigned, that the flame of the spirit of wine is so pure and subtile, 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 sulphurous 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 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 dephlegmed. It hath been likewise observed, that aqua fortis will work more readily on lead, if it be allayed with water, than if it be purely rectified. I otherwhere 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,

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we found to our trouble and loss, 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 needless, 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 artists 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, the 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 not necessary to reduce it to a salt) and very dry, into the bottom of a tall and slender glass 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 processes, 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

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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 nauseous 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 needlesly that we prescribed bodies tall and slender; for we found not the experiment to succeed in large and low ones, and much less in retrorts, 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 glass, though very broad, and but moderately high, would seve 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) unless it be skilfully attempted, the highly rectified liquor, that is poured on, will rather leave some of its most spiritous parts behind, than carry up so fixt a salt.

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The Second E S S A Y,

Of Unsucceeding 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 artists irreparably. And *Glauber* alone, if you will therein credit him, tells us of several ways, by which

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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 Novi[[5]](#footnote-5)*(says he) *qui mihi asseruerit seme! se ex mobili argento, quod vivum dicitur, stabile verumque argentum confecisse succis & foliis herbarum, idque vendidisse peritis explorandæ metallicæ veritatis ; eisdem mox usum se foliis frustra, & quod semel perfecerat, nunquam alias, quanquam id sæpe tentaverit, perficere potuisse.*

*Alium novi* (says he further) *qui adhuc apud vivos moratur, cui cum aurum & argentum circiter quindecies per artem effectum esset, amisit artem eam, accepitque oraculo socii per quietem habito, id ingratæ mentis vitio contigisse. Ut hinc 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. *Retulist quidam mihi* (subjoins he) *sese aurum ex argento fecisse semel magna copia ; secundo se usum eisdem rebus, fecisse quidem, sed minima semper quantitate, sic ut detrimentum lucro majus esse supputaverit. Venisse in mentem, uti detrimentum effugere possit, si non ex argento, sed ex ære 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 consequeretur.*

*Idem* (continues the prince) *affirmavit ab amico, qui expertus hoc ipsum suerat accepisse, qui cum ex cinnabari argentum fecisset optimum, sæpenumero sese postea insistentem operi majore cum diligentia semper eventu rei fuisse 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 artists call that silver, which wanting but the tincture of gold abides the trial of aqua fortis, &c.) or some other lucriferous 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 unwieldly 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 HAE 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,

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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 mis-ascribed 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 a graft, which a divine, to whom the garden belongs, affirmed to have been grafted upon a quince-tree; and the industrious *Kirchir* tells us, that *Experientia docet Persicum moro insitum fructus proferre, &c. de quo nullum est dubium utpote vulgare pene:* [[6]](#footnote-6) ‘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.’

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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

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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 glass 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 most 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 a 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 sadder 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 loss, which sometimes he knows not to what to impute, of which I have heard several of them complain: and therefore divers of our less-expert dyers, to avoid those 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 oftentime 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 tincted 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, *Pyrophilus*, 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 loss 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 tincted parts would fasten, though they would not, whilst (according to the custom of refiners) it was poured on cold.

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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 inestimable 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.’[[7]](#footnote-7)

IF there be any trade that obliges the artificers to be assiduously conversant with the materials they employ, it is that of the glass 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 glass-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 glasses) 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 unluckiness. But the next time I came, though it were upon appointment, his metal proved again unserviceable, and instead of being colourless, when it was cold, looked as if it had been stained with blue and yellow, and was besides brittler 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 *Asellius*, 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 mesentery of a dog dissected alive, to be some irregular ramification 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, *Pyrophilus*, 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 affrriction 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 the middle of the load-stone, you shall find, that the point of the knife has, 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 affrictions of such irons upon the pole of a naked terella, and we found it to succeed there likewise: how strange soever 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

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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 phænomena 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 artist 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 *Damasco*-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 insition, 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 insition is by many gardeners looked upon as a thing merely 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 insition 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:

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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 abstruse 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 observation, 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 a 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 streight 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 metaline vein, the want 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, that 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 long debate concerning it, gives us this account of his sense; *Metallicus igitur* (says he) *quia eum virum bonum & gravem esse volumus, virgulâ incantatâ non utetur, quia rerum naturæ peritum & prudentem, furcatam sibi usui non esse, sed, ut supra dixi, habe: naturalia venarum signa, quæ observat*. [[8]](#footnote-8) The diligent *Kircherus* informs us in his *Arte Magneticâ*, 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 ourselves having several times made trial of it in the presence of the confidentest assertors of the truth of it, could not satisfy ourselves, 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 out diligent country-man *Gabriel Plat*, though wont to be somewhat too severe to chymists, does ascribe very much to this detecting wand; and divers persons, in other things very far from credulous, have as eye witnesses 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 a vein of ore, and protested, that the motion of his hand did not at all contribute to the inclination of the rod, but that sometimes, when he held it very fast, it would bend so strongly as to break in his hand. And to convince me, that he believed himself, he did, upon the promises

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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 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 the less 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

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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 observators, by insisting 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, preremptorily 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 profess 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 accuratest of our modern writers, I suppose you will readily allow me to reckon Dr. *Harvey* and Dr. *Highmore*; 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 progress 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 sitting 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. *Highmore*, 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,

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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[[9]](#footnote-9)*, as another informs us) be pleased to affirm, that the loadstone declines 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 *Corvo*, 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 venturous countryman Captain *Thomas James* observed it in 63 degrees north-latitude to be no less than 27 degrees, 48 minutes[[10]](#footnote-10); 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, sometimes 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 succeeders 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 mountain *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[[11]](#footnote-11) tells us in more 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

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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 *Ilva* (upon the coast of *Italy*) where they dig up iron and store of load-stones, 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 pæony-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 pæony root unseasonably gathered without success; but having lately gathered it under its proper constellation, as they speak, (which is when the

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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 last of his observations communicated to the famous practical physician *Riverius*, solemnly professes his having divers times freed his patients from the falling-sickness by the single outward application of pæony-roots, collected and applied as is above mentioned. But though he thence infers the usefulness of observing stars in the practice of physic, yet before much weight be laid upon such improbable notions, as most of those of judiciary astrologers, the influence of constellations 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 observations to be taken notice of in trials medical, I could tell you, that I have observed even mathematical writers themselves to deliver such observations 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 accurateness, 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 accurateness, when they deliver observations 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 observations, as, being made by the help of material instruments framed by the hands and tools of men, cannot but in divers cases be subject to some, if not many, imperfections upon their account. Divers of the modern astronomers have so written of the spots and more shining parts or (as they call them) *Faculæ*, that appear upon or about the surface of the sun, as to make their readers presume, that at least some of them are almost always to be seen there. And I am willing to think, that it was their having so often met with such phænomena 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 observations made at several times, that I could detect any of these solary spots, which having during many months at least appeared so much seldomer than it seems they did before, that I remember a most ingenious professor of astronomy, excellently well furnished with dioptrical glasses, 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 *Faculæ*, that are written of, as such ordinary phænomena, I must profess to you, *Pyrophilus*, that a multitude of observations made with good telescopes, at several places and times whilst the sun was spotted, has scarce made me see above once any of the looked for brightnesses.

AND as the nature of the material objects, wherewith the mathematician is conversant, may thus deceive the expectations 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 observations, whereon the mixed mathematics (as astronomy, geography, optics, &c.) are in great part built. This is but too manifest in the disagreeing supputations, that famous writers, as well modern as antient, have given us of the circuit of the terrestrial globe, of the distance and bigness 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: *Facilè* (says he) *lapsus aliquis penè insensibilis in instrumentis etiam majoribus conficiendis subrepit, qui inter observandum aliquot scrupulorum primorum jacturam faciat; insuper si ipse silus & tractandi modus non tam absoluta norma perficiatur, ut nihil prorsus desideratur, intolerabilis nec facilè animadvertenda deviatio sese insinuat. Adde quod instrumenta usu & ætate à prima perfectione degenerent. Nihil enim, quod hominum manibus paratur, ab omni mutatione undiquaque existit. Organa enim ejuscemodi, nisi è solido metallo affabre elaborentur, mutationi aëreæ obnoxia sunt; & si id quoque detur, ut è metallica materia constent, nisi ingentia fuerint, divisiones minutissimas graduum non sufficientur exhibent; dumque hoc præstant, sua magnitudine & pondere se ipsa ita aggravant, ut facile tum extra planum debitum aut figuram competentem, dum circumducuntur, declinent, tum etiam sua mole intractibilia redduntur. Quare magis requiritur in instrumentis astronomicis, quæ omni vitio careant construendis, artificium pari judicio conjunctum, quam haclenus à quamplurimis animadversum est. Id quod nos ipse usus longaque docuit experientia, non parvo labore nec mediocribus sumptibus comparata.[[12]](#footnote-12)*

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 observations 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 about 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, *Pyrophilus*, 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-

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structures either theorical 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 discouragement 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 falshood, 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

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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, that 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 fæces, that after all I have written in the former essay concerning the 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\*[[13]](#footnote-13) 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 sallet-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 common 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

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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 so 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 lyar, whensoever 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 mis-remember not) *Gascony* their venom is so pernicious, that they sometimes poison those that treat 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

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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 Alenis* (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 carraque, and accompanied by the famous pilot *Vincent Rodrique*, who had then 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.’[[14]](#footnote-14) 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, had 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*.’

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. [[15]](#footnote-15) But to draw at length to a conclusion of this already too tedious essay; the ends above mentioned, *Pyrophilus*, 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 despondency 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,

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but for those that we 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.

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1. Accipe pulverem Johannis de Vigo propria, manu paratam, nam alioquin admistus minio est adulteratus, prout qualecunque medicamen chymicum quod venale exstat fraude plenum est. Helmon. de Febr. c. 14.

   Sunt nempe olea essentialia venalia, quæque mange ære penduntur, adulterata omnia atque singula, &c. Idem de Feb. c. 15. [↑](#footnote-ref-1)
2. *Histoire de Madigascar, c. 37.* [↑](#footnote-ref-2)
3. Paracel. *de Mineral T[r]act.* 1. [↑](#footnote-ref-3)
4. Paracel. *Ibidem*. [↑](#footnote-ref-4)
5. Lib. 3. cap. 6. [↑](#footnote-ref-5)
6. Artis Mag. Luis & Umbræ, lib. 1 p 3 cap. 6. [↑](#footnote-ref-6)
7. Josephus Acosta, [\_] 4. cap. 5. [↑](#footnote-ref-7)
8. De re metellica, lib. 30. p. 28. Lib. 3. part 4. cap. 3. [↑](#footnote-ref-8)
9. Fournier Hydr. l. 13, c. 11. [↑](#footnote-ref-9)
10. In the Table annexed to his voyage [↑](#footnote-ref-10)
11. Livre [?] 11. chap. 10. [↑](#footnote-ref-11)
12. Tycho Brahe [?] l. 2. de Cometa An. 1577. p 153 [↑](#footnote-ref-12)
13. \* This is one of those, that make up the book of the usefulness of Experimental Philosophy. [↑](#footnote-ref-13)
14. De la Longitude, c. 34 [?]. [↑](#footnote-ref-14)
15. Fourn. Architecture Navale [?], c 22. [↑](#footnote-ref-15)