ESSAYS

AND

OBSERVATIONS,

PHYSICAL AND LITERARY.

Read before a Society in EDINBURGH, *and publiſhed by them.*

VOLUME I.

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PREFACE.

AFTER the medical ſociety of *Edinburgh* had publiſhed thoſe volumes of Eſſays, which have met with ſo favourable a reception from the public, a propoſal was made them to enlarge their plan, and to carry their diſquiſitions into other parts of nature, beſide ſuch as more immediately relate to the branches of medicine. All the ſciences are remarked to have cloſe connexion together; but none more than thoſe of medicine and natural philoſophy: And the ſociety ſoon obſerved, that, ſhould it turn its inquiries into more general knowledge, it could reap the advantage of preſerving all its old members, and needed but open its door to Gentlemen of other profeſſions, who might enrich it with their obſervations and diſcoveries.

SOON after the ſociety had received a new form, ſeveral misfortunes happened, which retarded its progreſs, and have hitherto prevented it from communicating any thing to the

the public. The rebellion broke out in this country; and both ſcattered the members for ſome time, and engaged their attention to ſubjects, leſs agreeable and more intereſting, than general diſquiſitions into nature. No ſooner were public affairs compoſed, than we met with an irreparable loſs in the death of Mr MACLAURIN, one of our ſecretaries. The great talents of that Gentleman are generally known and highly eſteemed in the literary world; but the ſociety have, alſo, particular reaſon to regrete in him the loſs of thoſe qualities, which form an excellent academician. Indefatigable himſelf, he was a perpetual ſpur to the induſtry of others; and was highly pleaſed with the promotion of knowledge, from whatever hands it came. At the time of his death, a number of diſcoveries, ſufficient to have formed a volume, had been communicated to him; but, being mingled with his other papers, have been diſſipated by various accidents; and the ſociety could recover but few of them.

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THE object of this ſociety is the ſame with that of the other academies, which have been eſtabliſhed in different parts of *Europe*, the promoting of *natural philoſophy*, and of *literature*, by communicating to the public ſuch diſſertations as ſhall be tranſmitted to them, either by their own members or by others. 'Tis allowed, that theſe two branches of learning, eſpecially the former, are more promoted by the obſervation of facts than by the moſt ingenious reaſonings and diſputations. To a diligent, and even ſometimes to a careleſs inquirer, many valuable experiments, no doubt, occur; and theſe would enrich our collections, tho', without this method of conveyance, they would be intirely loſt to the public. The united judgments too of men correct and confirm each other by communication, their frequent intercourſe excites emulation, and from the compariſon of different *phaenomena*, remarked by different perſons, there often reſult general truths, of which, from one of theſe *phaenomena*, no man of the greateſt ſagacity could entertain

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any ſuſpicion. Tho' the collection of experiments ſeems continually, by means of the learned ſocieties, and the labours of individuals, to be augmenting, we need not entertain any apprehenſions, that the world will ever be overwhelmed by the number of confuſed and independent obſervations. The heap does not always go on, increaſing in bulk and diſorder, thro' every age. There ariſe, from time to time, bold and happy geniuſes, who introduce method and ſimplicity into particular branches of ſcience; and reducing the ſcattered experiments to more general theorems, abridge the ſcience of nature. Hints of this kind, we hope, may be able to paſs thro' our hands; and at worſt, our collections will be a ſpecies of magazine, in which facts and obſervations, the ſole means of true induction, will be depoſited for the purpoſes of philoſophy.

THE ſciences of theology, morals, and politics, the ſociety are reſolved intirely to exclude from their plan. However difficult the inferences in theſe ſciences, the facts, on which

which they are founded, are extremely obvious; and we could not hope, by our collections, to be, in this respect, of any service to the public. The great delicacy of the subject, the imperfections of human understanding, the various attachments and inclinations of mankind will for ever propagate disputes with regard to these parts of erudition. And 'tis the peculiar happiness of geometry and physics, that, as they interest less the passions of men, they admit of more calm disquisition and inquiry.

It is not that the society expect or propose, that what they communicate will be intirely above doubt or disputation. The papers, indeed, which they print, were all red before them, and they gave their consent to the inserting them in their collections: But they pretend not to warrant the justness of every reasoning, nor the accuracy of every observation. The author alone of each paper is answerable for the contents of it: And the society are as willing to insert what may be communicated in opposition to the sentiments

ments of any of its members, as in confirmation of them.

The ſociety are ſenſible that it belongs alone to the public to decide concerning the value of any invention; and all the merit to which they pretend, is that of exciting the induſtry of the learned, and of conveying their productions to the notice of the world. They aſſume not ſuch authority as to ſtamp their approbation on any performance, even thoſe which they communicate, much leſs thoſe which have barely been red before them. If ever their teſtimony has been cited by any author in favour of his books, reaſonings, or machines, they hereby declare, that this liberty has been uſed entirely without their conſent or knowledge, and proceeded alone from the fond opinion, which the writer had entertained of his own performance.

Whoever will favour the ſociety with any diſcourſe which it comprehends in its plan, may ſend their papers to either of the ſecretaries, Mr Alexander Monro Profeſſor of Anatomy at *Edinburgh*, or Mr David Hume Library-keeper to the faculty of Advocates.

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ARTICLE I.

Of the Laws of Motion; by the Honourable HENRY HOME, *Esquire, one of the Senators of the College of Justice.*

NO thing has more perplexed philosophy, than an unlucky propensity, which makes us grasp at principles, without due regard to facts and experiments. Tho' fond of knowledge, we are willing to purchase it at the easiest rate; and general principles delight us, because they shorten the road to knowledge. This bent of the mind is productive of manifold errors. Prepossessed once by a favourite principle, we are no longer open to conviction. Every phenomenon muſt be accommodated to that principle, and every oppoſite fact, however obſtinate, muſt go for nothing. And thus we endeavour to mould nature to our wiſh, inſtead of deſiring to know nature in her genuine figure.

We ſee, then, that in ſcience, as well as in action, appetite and inclination generally gain the aſcendant. Even in natural philoſophy, theory was introduced before experiment, and every philoſopher urged his own notions, without regard to truth or reality. This produced a maſs of undigeſted and contradictory theory; which at length could not fail to bring on the diſcovery, that the whole was little better than fancy and chimera. The diſcovery had a remarkable good effect; which was, firſt to make us doubt of every thing, and then to make us ſearch after truth in the more painful road of induction. By this means, a greater number of important truths have been brought to light within a century or two, than before that time from the beginning of the world.

But tho' our only ſure guides to truth are facts and experiments, it is however expedient to keep the end in view. Facts and ex-

periments

periments are uſeleſs lumber, if we are not to reaſon about them, nor draw any conſequences from them. In all our operations, we may have an eye to theory: nay we muſt have it; for ſuch is the conſtitution of our mind. Theory becomes only a ſource of error, when we indulge in it too much, or attach ourſelves to it beyond what facts and experiments can juſtify. In ſhort, theory is vain without experiments, and experiments are beſt underſtood by applying them to theory.

NATURAL philoſophy, it muſt be acknowledged, is far from being ripe for firm and laſting theory. But we have facts and experiments in great abundance, to be a foundation for theoretical ſketches; which may be ſafely indulged, provided they be done in ſo ſlight a manner, as to leave the mind free from any wrong bias. Tho' we have not materials for a regular building, there is nothing to hinder us to venture upon a model.

ALMOST every author who treats of the laws of motion, has thought it incumbent upon him to ſet out with a definition of motion. And, in a thing ſo ſimple, it may appear ſtrange to find ſuch a variety of definitions: whence one thing may be gathered, that none

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of thoſe definitions have been approved of by ſucceeding authors. *Epicurus* defines motion to be "a paſſage from one place to another:" and he might as well have defined it to be motion from one place to another; becauſe paſſage in this ſentence means nothing different from motion. *Sturmeus* defines motion to be "*ſucceſſiva rei motæ in diverſis locis exiſtentia*;" which is juſtly blamed by *Leibnitz* as deſcribing the effect of motion, rather than the formal nature of it. And yet *Leibnitz*'s own definition, "*motus eſt continua loci mutatio*," is not more ſatisfactory. But, of all definitions that ever were attempted, *Ariſtotle*'s definition of motion is the moſt unintelligible, "*Actus entis in potentia quatenus in potentia*;" which *Locke* condemns as abſolute jargon; and which, he ſays, would puzzle any rational man, to whom it was not already known by its famous abſurdity, to gueſs what word it could ever be ſuppoſed to be the explication of.*

But it is no wonder theſe authors have not given ſatisfaction to each other, or to their readers, upon this ſubject; for they have attempted what is utterly impoſſible, *viz.*

* Locke concerning human underſtanding, B. 3. ch. 4. § 8.

viz. to give a definition of a ſimple idea. They might as well have attempted a definition of colour or ſound, of pain or any other ſimple feeling. It is to be regreted, that natural philoſophers and mathematicians are not always well ſkilled in logics, and of this truth we ſhall have but too often occaſion to give examples.

SCARCE any author that I know has thought of giving a definition of reſt; yet it is not a more ſimple object than motion. I can gueſs at the reaſon, that reſt is the more familiar object; a greater number of things appearing to be in a quieſcent ſtate than in motion. If every thing about us appeared to move, we ſhould be ſurpriſed the firſt time we ſaw a body at reſt, and probably would endeavour to explain the thing to others: but both are equally incapable of a definition. They are ſimple objects of ſight and perhaps of touch. Deprive us of theſe two ſenſes, and we could never have the leaſt notion either of reſt or motion.

WHEN a being moves itſelf or moves, we conceive the being as acting; and in this view motion is a ſpecies of action. But, when a body is moved by being acted upon, we conceive the motion of the body as an effect produced by a proper cause. In this case the body does not act, but is acted upon. This, tho' an evident distinction, does not exclude self-motion from being also considered as an effect in a certain light, *viz.* an effect produced by a being upon itself.

EVERY thing which moves, and is not barely moved or acted upon, must be endued with a power of motion. For this power is involved in the very conception of self-motion. And the term *power* may be equally applied to animate and inanimate beings, supposing them to be equally self-movers.

WHETHER matter in any case be endued with a power of motion, is a celebrated question, which is generally resolved in the negative: and, as this question cannot be overlooked in the present speculation, I shall endeavour to examine it with all the accuracy possible.

MATTER is generally represented as altogether unactive and inert; and indeed in a superficial view the fact appears to be so. The bulk of the things about us seem to be at rest, and we lay our account that they will continue in that state, unless acted upon

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by ſome external force. If we lock up any moveable object, we truſt to find it in the ſame place; and, if it be miſſing, we aſcribe our loſs to thieves, not to ſelf-motion in the body. Matter, ſo far as we can diſcover, is certainly not endued with thought or voluntary motion; and yet, that it is endued with a power of motion in certain circumſtances, appears to me an extreme clear point. Dropping a ſtone from a high tower, it falls to the ground without any external impulſe, ſo far as we can diſcover. Here is an effect produced, which every one, who has not ſtudied philoſophy, will attribute to a *power* in the ſtone itſelf. One would not heſitate to draw this concluſion, ſhould the ſtone move upwards; and yet, ſetting aſide habit and cuſtom, it muſt be evident, that a ſtone can as little move downwards as upwards without a *vis motrix*. And that this is a juſt, as well as natural way of thinking, will appear by analogy. When a man is in motion, we readily aſcribe the effect produced to a power which he poſſeſſes, to move his limbs. Why then do philoſophers deny to the ſtone, in the act of falling, the power of beginning motion, a power which they ſo readily aſcribe to the

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man? If it be objected that man is a being endued with a power of moving himſelf, and of moving other things, the plain anſwer is, that theſe are facts which we learn no other way than by experience; and we have the ſame experience for a voucher, that a ſtone ſet free in the air will move itſelf. And if it be farther urged, that man is a thinking being, the anſwer will readily occur, that a power of beginning viſible motion is no more connected with a power of thinking, than it is with any other property of matter or ſpirit. Nay, Mr *Locke* holds, that matter may be endued with a power of thinking; and, ſuppoſing this power ſuper-added to the other properties of matter, it cannot be maintained, that matter would be rendered thereby more or leſs capable of beginning or continuing viſible motion.

But this is not the only inſtance in which we diſcover an agency or active power in matter. A billiard ball ſtruck upon the ground, rebounds with a conſiderable force. A bow bent by the hand, reſtores itſelf with violence when the ſtring is let go. In both, there is an inſtant of reſt betwixt the oppoſite directions of the motion. The ball rebounds, and the bow

reſtores

reſtores itſelf to its former ſhape, without any external force, by an inherent power which is known by the name of elaſticity. But we need not dwell upon particular inſtances. Chemiſtry diſcovers various powers in matter of the moſt active kind; and every man who is converſant with the operations of chemiſtry, muſt have a ſtrong impreſſion that matter is extremely active.

FOR the ſake of illuſtration, let us ſuppoſe a ſubſtance or thing having the following properties, that it makes no reſiſtance to bodies impinging upon it; that when carried along in a man's hand, even with the moſt violent motion, it does not increaſe the *momentum* of the hand, and that it ſtops ſhort the inſtant the hand ceaſes to move. This would certainly be the moſt inert of all conceivable things. And if ſo, matter cannot be abſolutely inert or paſſive, when its properties differ ſo widely from theſe deſcribed. In many circumſtances matter begins motion, and acts often with great violence. It has a conſtant endeavour, when once ſet in motion, to preſerve itſelf in the ſame degree of motion, and, when at reſt, is not put in motion without reſiſtance.

But it is maintained by the bulk of our philosophers, that matter is altogether incapable of active powers; that activity is confined to immaterial substances, and that inertness is implied in the very conception of matter. This moves them to ascribe to some invisible agency, all that activity which we discover in matter. In every one of the above instances, matter, they say, does not act, but is acted upon by the deity, who interposes by general laws, to preserve the uniformity of nature. Thus, when a stone falls, it is not the stone which acts, but the deity. It is the continual action of the deity which keeps the planets in elliptical orbits: and, when a plague infests the world, it is the deity who spreads the infection, and directs inert matter to ravage and destroy. Arsenic is not of itself a poison; it is the immediate finger of God which makes it so.

With regard to this whimsical doctrine, which declares war against our senses, it may be observed, that natural philosophy is not much affected by it, of whatever errors it may be productive in the more abstract sciences. For it is admitted, that the deity, in acting upon matter, subjects himself to the same

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ſtrict and invariable laws which govern matter, ſuppoſing it to have a power of motion. Therefore, the hiſtory of nature will be the ſame, upon either ſuppoſition. At the ſame time, as this hypotheſis not only contradicts common ſenſe, but, in place of a beautiful chain of cauſes and effects, diſcoverable in the operations of matter, preſents a deformed and crude ſcene, which detracts from the wiſdom of the deity; I think it of conſequence to beſtow ſome thoughts upon it: and, I am hopeful to make it evident, that this hypotheſis is no better ſupported by reaſon than by experience.

To put an end to a good deal of looſe reaſoning upon this ſubject, it will be neceſſary to aſcertain the meaning of of the terms *material* and *immaterial*; which, ſo far as I know, has not been done with ſufficient accuracy. All beings and exiſtencies muſt be either material or immaterial; or, in other words, muſt be matter or not matter: therefore, if we know what is matter, we cannot be at a loſs about what is not matter. I take it for granted, that we have no conception of matter but as ſubſtance or body, endued with colour, figure, extenſion, impenetrability, or other

other properties of ſuch a nature as to be objects of our external ſenſes. The direct oppoſite, muſt of courſe be an accurate deſcription of an immaterial ſubſtance, *viz.* that which has properties of ſuch a nature, as not to lie open to any of our external ſenſes, more than colour to one born without ſight. Thus it comes out, that imm teriality is merely a negative term, comprehending every thing that is not matter. And it is of conſequence to be obſerved, that the diſtinction betwixt material and immaterial, not being founded on the nature of the things which are ſo diſtinguiſhed, but on the limited nature of our external ſenſes, has not the leaſt tendency to explain the nature or properties of immaterial ſubſtances, further than barely, that theſe properties are of ſuch a kind, as not to be the objects of any external ſenſe.

From theſe premiſes, the following reaſoning will, it is hoped, be found entirely concluſive. Size, figure, motion, weight, &c. are qualities of matter which are perceived by our external ſenſes: but there is none ſo fooliſh to maintain, that matter can have no qualities but what are objects of an external ſenſe. It would be the ſame as to deny the exiſtence

existence of immaterial substances, because these do not exhibit themselves to our senses. Power is a property or quality, of which none of our external senses afford us the perception; and therefore our want of perception of power, does not more conclude a negation of power to matter, than to spirit. In general, we have no means to come at the knowledge of a *cause*, but by the *effect* produced. We cannot, *a priori*, conclude, that animate beings are endued with any sort of powers, more than inanimate. Experience is here our only guide. We find by experience man to be a reasoning being, endued with many powers and faculties: and, by the same experience, we find matter to be endued with certain powers and faculties. Both are discovered by the effects produced; and we have no other means to make a discovery. We see a stone fall without any external impulse. From that effect we have a just foundation to conclude, that the stone has a power of moving itself. And, if we have not a just foundation to make this conclusion, we have not a just foundation to make this other conclusion, that a man has a power of self-motion when we see him walking. And af-

after all, it muſt appear extremely whimſi-cal to deny to matter a power of motion, merely becauſe matter diſcovers itſelf to our external ſenſes in part; when, at the ſame time, we are ſo ready to attribute powers of every ſort to immaterial ſubſtances, which cannot be reached by our external ſenſes, either in whole or in part.

It is ſuggeſted above, that to endue matter with a power of acting according to general and invariable laws, exhibits a more beautiful and compleat ſyſtem, than to leave it abſolutely inert, to require a continued interpoſition of the deity. To illuſtrate this point, I chuſe to borrow the honourable Mr *Boyle's* ſentiment, whoſe piety was not inferior to his knowledge. "It ſeems manifeſt "enough, that whatſoever is done in the "world, at leaſt where the rational ſoul in-"tervenes not, is really effected by corporeal "cauſes and agents, according to the laws "ſettled by the omniſcient author of things." * And he obſerves, "That as it more recom-"mends the ſkill of an engineer to contrive "an elaborate engine, ſo as that there need "nothing

* Inquiry into the vulgar notion of nature. p. 60.

"nothing to reach his ends in it, but the "contrivance of parts void of underſtand-"ing; than if it were neceſſary that, ever "and anon, a diſcreet ſervant ſhould be em-"ployed to concur notably to the operations "of this or that part, or to hinder the en-"gine from being out of order: ſo, it more "ſets off the wiſdom of God, in the fabric "of the univerſe, that he can make ſo vaſt a "machine perform all thoſe many things "which he deſigned it ſhould, by the mere "contrivance of brute-matter, managed by "certain laws of motion, and upheld by his "ordinary and general concourſe; than if he "employed, from time to time, an intelli-"gent overſeer to regulate and controul the "motion of the parts *." What may be the opinion of others, I cannot ſay; but, to me, this argument is perfectly concluſive. Conſidering this univerſe as a great machine, the workmanſhip of an intelligent cauſe; I cannot avoid thinking it is the more compleat, the leſs mending or interpoſition it requires. The perfection of every piece of workmanſhip, human and divine, conſiſts in its anſwering

* Inquiry into the vulgar notion of nature. p. 7.

ſwering the deſigned purpoſe, without beſtowing further labour upon it. And therefore, upon the whole, as we have no foundation, either in reaſon or experience, to deny activity to matter; I conclude, that the doctrine of the abſolute inertneſs of matter, is not only repugnant to truth, but tends, in an indirect manner, to arraign the deity of want of power or of wiſdom, or of both.

Of the VIS INSITA or VIS INERTIÆ.

HAVING thus brought our ſubject within bounds, by evincing that we have no occaſion to ſearch for any other immediate cauſe of natural appearances than the activity of matter itſelf; I ſhall proceed to examine ſome of the powers of matter, ſuch of them as are productive of the moſt remarkable effects: and ſhall begin with the *vis inſita*, or *vis inertiæ*, that ſignal property of matter, whereby, as philoſophers teach, "A body "always

" always perſeveres in its ſtate of reſt, or of " uniform motion, in a right line, 'till, by " ſome external influence, it be made to " change its ſtate." This property is handled by writers without any degree of accuracy. The bulk of them reſolve it into the inertneſs and paſſive nature of matter ; and conſider the preſent ſtate of a piece of matter, whether of motion or of reſt, to be an effect, which, once produced, muſt continue to exiſt as it is, 'till it be changed or deſtroyed by the operation of a new cauſe. But, as this conception of the matter is liable to ſtrong objections, which ſhall by and by be mentioned, the moſt rational writers admit of a power in matter to preſerve itſelf in its preſent ſtate ; but then, conſidering the perſeverence in reſt and in motion, to be effects of the ſame kind, they, without making any diſtinction, attribute both to the ſame power or cauſe.

I CANNOT ſubſcribe to either of theſe notions. It is obvious to me, that the mere negation of a cauſe, tho it may account for the continuance of a body at reſt, as it may account for its preſerving the ſame figure or colour ; yet can never account for the reſiſtance made in the change from reſt to motion, or from motion to reſt. And it is equally obvious, that a ſtate of motion is very different from a ſtate of reſt ; and that the preſerving a body in motion, muſt be an operation of a different kind, from that of preſerving it at reſt.

In order to handle this ſubject with ſome degree of accuracy, I ſhall endeavour to make out the three following propoſitions: *firſt*, That motion is a continued action, which muſt infer ſome power continually acting. *Secondly*, That as matter reſiſts a change from reſt to motion, as well as from motion to reſt, this reſiſtance is not to be accounted for by the mere negation of a cauſe, but is a poſitive effect to require a cauſe as much as motion does. And, *laſtly*, after explaining the cauſe of the continuance of motion, I ſhall endeavour to ſhow, that it is an effect of a different kind from the reſiſtance which matter exerts againſt any impulſe which puts it in motion.

With regard to the firſt propoſition, motion evidently differs from the other properties of matter. It is not like extenſion, figure, and ſuch like qualities eſſential to matter. Every body muſt be of a certain ſize and figure; but it is eaſy to ſeparate motion from body, by ſuppoſing it to be at reſt. But what we have principally to conſider in the preſent argument is, that the moulding of a body into a certain figure, or the giving it a certain colour, are not imperfect, but compleated effects; which, once produced, muſt continue invariably the ſame, until ſome power be exerted to make an alteration. This is involved in the very conception of colour and figure, and is ſo ſimple and clear a propoſition, as to be incapable of any illuſtration. The bare negation of a new cauſe, is ſufficient to account for the continuance of ſuch effects. Reſt is of the ſame kind. It requires no action or operation to preſerve a body at reſt, more than to preſerve its colour or figure. But motion, being the direct oppoſite of reſt, cannot be one of thoſe compleat effects, which, once produced, muſt continue invariably the ſame. Motion, by the very conception of it, is action. While a body is in motion, it is in continual action; and, as action implies power, there muſt be a power continually exerted to preſerve a body in motion. Again, if motion be conſidered

dered as an effect, it can only be compared to figure or colour, with regard to that part of it which is paſt: what part remains to be performed, is ſo far from a compleated effect, that it is not ſo much as commenced; and therefore muſt neceſſarily be conceived as a different effect, to the production of which there muſt be a new exertion of power. Motion then is a continued effect, or rather a ſeries of different effects, to require a cauſe in continued action. Accordingly, when we attend to our own perceptions, we do not inquire for a cauſe, when a body ceaſes to move. If the cauſe of the motion ceaſe, we ſeek no other reaſon for the ceaſing of the motion. If a thing ſo evident can admit of illuſtration, let us figure an animal in motion. It never came into the mind of any perſon, that a man is paſſive in the action of walking or running. In this caſe we reaſon juſtly, that walking or running requires a continued exertion of power. It requires no exertion of power to put an end to theſe motions. The man has but to forbear to exert his power, and reſt enſues. In this matter there is no difference betwixt animate and inanimate beings; not even tho' it ſhould be

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ſuppoſed, that matter is altogether paſſive. For, if matter itſelf has not a power of motion, the ſpirit which actuates matter, cannot otherways preſerve it in motion, but by a continued exertion of power.

THE ſecond propoſition is almoſt ſelf-evident. To account for continuance at reſt, nothing is required except barely the negation of a cauſe. But matter at reſt not only continues in that ſtate, but plainly reſiſts every force which tends to change its ſtate from reſt to motion. Matter makes no reſiſtance to any cauſe which produces an alteration in its figure or colour. Reſiſtance is a poſitive effect which requires a poſitive cauſe. And this property of reſiſtance which all matter is endued with, is a principle directly oppoſite to inertneſs or paſſeity, if I may uſe that term. Reſiſtance is plainly action, which every man may readily perceive in himſelf, when an impulſe is made upon him: and, were matter abſolutely paſſive and inert, it would make no reſiſtance, but be carried on by every impulſe, without diminiſhing the force of the impelling body.

WITH regard to the third propoſition, we cannot be at any loſs to aſſign a cauſe for the continuance

continuance of that quantity of motion which is once given to a body. If the intervention of ſpirit be rejected as above, there is but another hypotheſis to recur to, which is, That inanimate matter is endued with a power to preſerve itſelf in motion, as well as animated matter is, without any other difference, but that the former having no will or feeling, acts blindly and invariably by an eſtabliſhed law. It has no ſpontaneous motion, becauſe it is has no will. Neither has it in every circumſtance a power of beginning motion; but, when once ſet in motion, it has a power given it by the Author of nature to continue it ſelf in motion. And this is a law which regards all matter without exception.

And this power being once eſtabliſhed, it remains only to be made out, that it is of a different kind from that which reſiſts a change from reſt to motion, and from any degree of motion to a greater. The thing is obvious; for, as cauſes are beſt dſtinguiſhed by their effects, the cauſe cannot be the ſame of two effects diametrically oppoſite. *viz.* preſerving a body in motion, and oppoſing motion. It may ſerve to clear this matter, if we attend to the diſtinction betwixt impulſe and reſiſt-

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ance. They are ſimple ideas, and therefore incapable of a definition; but their difference will be obvious, by conſidering a man in the act of puſhing againſt an obſtacle, and barely reſiſting force applied againſt him, without puſhing again, or counteracting. The man, no doubt, is active in reſiſting, as well as in puſhing; but the actions are very different in their nature, and carefully to be diſtinguiſhed in accurate reaſoning. In a word, the tendency of impulſe is, to produce motion; the tendency of reſiſtance is, to prevent it. Theſe powers, therefore, ſo oppoſite to each other, ſhall hereafter be diſtinguiſhed by different names. The *vis inſita* ſhall be appropriated to that power which is favourable to motion; leaving the other power which reſiſts motion to be denominated by the *vis inertiæ*, or rather *vis reſiſtentiæ*.

Tho' this *vis inertiæ* be a power, as obſerved, inherent in all matter, there are certain experiments which may appear to claſh with it, and which, upon that account, merit attention. If a round ball be laid upon a horizontal table *in vacuo*, in which ſituation there will be no counteraction of gravity, it is certain, that the ſmoother the ball and table

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are, the reſiſtance will be the leſs: and they may be made ſmoother and ſmoother, till the reſiſtance be quit inſenſible. If the reſiſtance do not vaniſh altogether, it may be owing to our want of art to make any ſurface abſolutely ſmooth. There is no reaſon, it may be thought, to aſcribe the ſmall remaining reſiſtance to a ſuppoſed *vis inertiæ*, when it can be accounted for by other cauſes; and that it is unphiloſophical to ſuppoſe the exiſtence of a cauſe, when we cannot point out a ſingle effect that reſults from it, and from no other cauſe.

BUT there is another experiment of the very eaſieſt operation, and which is liable to no ambiguity. Let a body of any determined weight be ſuſpended by a thread or rope fix'd to a hook in the ceiling: the leaſt conceiveable force will put this body in motion. If any reſiſtance at all be felt, it ought to be attributed to the denſity of the air, not to the body. And indeed theſe experiments infer, no more than what is admitted by every philoſopher, that the ſmalleſt force is ſufficient to move the greateſt body. It is acknowledged, that, were the globe of the earth ſuſpended by a chain, there is not a force ſo

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ſmall which it would not yield to. Theſe, and ſuch like experiments, look as if matter had no attachment to reſt, no *vis reſiſtentiæ*, but that it readily yielded to motion. That a body is put in motion by the very ſmalleſt force, and yet that it reſiſts a change from reſt to motion, are propoſitions which may appear not to be very conſiſtent: yet both are true in fact. And the difficulty will vaniſh, by attending to the power of the *vis inertiæ* or *reſiſtentiæ*, that it is not meaſured merely by the ſize or bulk of the body to which it belongs, but has a relation to the *momentum* of the impingeing body. The leaſt force will put the greateſt body in motion: but from this it does not follow, that matter does not reſiſt a change from reſt to motion. It only follows, that this reſiſtance is always leſs than the *vis motrix*. To calculate the proportion betwixt the *vis motrix* and the reſiſtence of a body at reſt, ſhall be the ſubject of the next chapter.

OF THE COMMUNICATION OF MOTION.

THE Communication of motion from one body to another, has been extremely puzling to metaphyſicians; tho' not to practical philoſophers, who make no difficulty to ſuppoſe, that motion actually paſſes out of one piece of matter into another. Let us figure one billiard ball at reſt, and another directed upon it with a certain velocity. It is underſtood that part of the motion of the impingeing ball paſſes into that which is at reſt, after which it proceeds to move with that degree of velocity which is communicated to it. This account of the matter appears to me altogether abſurd. Motion cannot ſubſiſt by itſelf to be transferred from place to place, or from body to body. It is one of the qualities or properties of matter; and it is no leſs repugnant to the very conception of the thing, that the motion of one body

body can paſs from it to another, than that the figure of one body can paſs from it to another. In general, qualities, properties, and affections, are inſeparable from the particular bodies to which they belong. They have no ſeparate exiſtence, and therefore cannot be conveyed, even in the imagination, from one body to another. The green colour of this field, cannot be taken from it, and beſtowed upon another. All that can be done, is to give the other a ſimilar colour. My ideas or feelings cannot be conveyed from me to any other perſon, tho' ſimilar feelings or ideas may be produced in that other perſon. This is not a play about words; it leads to the explanation of a phoenomenon which natural philoſophers have not been able to explain with any ſatisfaction. When motion is produced in one body by the impulſe of another, there is no neceſſity to embrace ſo abſurd a doctrine, as that motion paſſes from the one to the other. The motion produced is eaſily deducible, from the principles above laid down. The *vis inſita* and the *vis reſiſtentiæ*, are ſufficient to account for all the effects that proceed from the colliſion of hard bodies which have no elaſticity. If the reſiſtance

ſiſtance of a body is always leſs than the impreſſed force, the reſiſting body, after its reſiſtance is overcome, muſt neceſſarily be carried along with the body which impinges upon it. And, being once put in motion, its own *vis inſita* preſerves it in the ſame degree of motion. I am here putting the ſimpleſt caſe, that of a body in motion, impingeing upon one at reſt; and, for the eaſe of the imagination, I ſhall ſuppoſe theſe two bodies equal. In this caſe it is aſcertained by experiments, that the two bodies in contact, go on with half the celerity which the impingeing body had before the colliſion. For it is a general rule in the percuſſion of hard bodies, that the *momentum* or force, directed to the ſame point, continues the ſame after contact as before. There is no difficulty here: for after half of the force of the impingeing body is deſtroyed, by the reſiſtance of the body at reſt; the other half remains, with which the impingeing body moves, and carries along with it the other body from reſt to motion. When the reſiſtance of the body at reſt is once deſtroyed, it yields to the impreſſed force without any further oppoſition; and

and, by its impenetrability, muſt be carried along with the other.

BUT, in order to form an accurate notion of this operation of percuſſion, two preliminary points muſt be ſettled. The firſt is, to determine the preciſe nature of the action exerted by a body at reſt when impelled by one in motion. Does the body at reſt return ſtroke for ſtroke, or impulſe for impulſe? Does it truly react, or does it barely reſiſt? That it reſiſts only, and does not react, will be evident from a ſingle conſideration, which is, that motion is eſſential to impulſe, not at all to reſiſtance. Now, if the body at reſt reacts, by returning ſtroke for ſtroke; it muſt ruſh upon the impingeing body, and carry it backwards at leaſt for ſome ſmall ſpace: which cannot be; becauſe, once admitting a retrograde motion, the *vires inſitæ* of the two bodies muſt carry them on in the ſame direction, which is contrary to fact. It is obvious, then, that there can be no repercuſſion or reaction of the body at reſt. All the obſtruction it gives to the action of the impingeing body, muſt be merely by reſiſtance. The other point to be ſettled is, whether the operation be inſtantaneous, or whether it be performed

formed in time? That it is inſtantaneous, will thus appear. A gradual change of motion ſuppoſes, that the foremoſt body paſſes through every degree of velocity from reſt till it acquires its greateſt velocity; and that the motion of the impingeing body is gradually retarded in proportion. But this ſuppoſition is inconſiſtent with a known fact, that the bodies continue conjoined after contact; whence it is evident, that both muſt move with the ſame velocity from the inſtant of contact.

THESE preliminary points being ſettled, the operation may be eaſily analyzed. Tho' the whole is performed in an inſtant, it may, however, be diſtinguiſhed into different ſteps. The firſt is, that, by a certain degree of reſiſtance, a proportion of the *momentum* or force of the impingeing body is deſtroyed, and conſequently of its velocity. The next ſtep is, that, with the remaining force and velocity, an impulſe is made upon the body at reſt, which now reſiſts no longer, whereby it begins to move with the remaining velocity of the impingeing body. And the laſt ſtep is, that it is carried on with the ſame degree of velocity by its own *vis inſita*.

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From what is ſaid, it will be eaſy to adjuſt the proportion betwixt the *vis motrix*, and the reſiſtance of a body at reſt. For tho' reſiſtance and *impetus* are ſo eſſentially different, as not to admit of a compariſon betwixt themſelves; yet, the effects produced by them, are capable of a compariſon. Now, the force loſt by the impingeing body is a juſt meaſure of the reſiſtance of the body at reſt; and if the *momentum*, before and after impulſe, continue the ſame; it muſt follow, that the *momentum* of the body which is put in motion, is equal to the *momentum* loſt by the impingeing body. Therefore the *momentum* of the impingeing body, is to the reſiſtance of the body at reſt, as the quantity of matter of both bodies, is to the quantity of matter in the latter.

This power of reſiſtance in matter, which is never exerted but when an impulſe is made upon it, is of ſo ſingular a nature, as to lead us to ſuſpect ſome very artful contrivance. It is ſurpriſing that writers have given very little attention to the laws of motion, conſidered in the light of final cauſes, tho' it is a moſt beautiful ſpeculation. It would carry me too far from my preſent purpoſe, to enter

ter profeſſedly upon this theme. And therefore I ſhall barely touch upon it, ſo far as concerns this property of reſiſtance. Gravity, an univerſal law of matter, ſerves the nobleſt purpoſes. In conjunction with the *vis inſita*, it preſerves the ſolar ſyſtem in order. The *vis inſita* is uſeful in another reſpect; becauſe by its means, we can apply force greatly ſuperior to our own. The reſiſtance of bodies again, is of admirable uſe to keep force within proper bounds. If the ſmalleſt body, impingeing upon the greateſt, had the effect of communicating its own velocity, which it would do, were there no reſiſtance, the ſmalleſt force would be ſufficient to give a new violent motion to the earth, and to put the lives of all mankind in the power of every individual.

OF

OF ACTION AND REACTION.

IT is laid down as an univerſal law of matter, " that reaction is always contrary " and equal to action." Or, " that the mutual " actions of two bodies upon each other, are " always equal, and with oppoſite directions." And Sir *Iſaac Newton*, in illuſtrating this law, gives particular inſtances of its taking place in all the different actions of matter; to wit, in impulſe, in preſſure, and in reſiſtance. For my part, I have always been puzled about this law, and am uncertain, after all, whether I have a juſt conception of it. Are we to underſtand it to be a property of matter in general, that when acted upon, it exerts an equal and contrary action? Or only, that in theſe particular caſes where matter reacts, the reaction is always equal and contrary to the action? The former is Sir *Iſaac*'s ſenſe of this law; which is evident, from his placing it among the laws that govern all matter in general; as well as from his various inſtances, importing that it holds in every caſe whatever. And now, taking the law in this ſenſe, difficulties croud upon me; of which I ſhall mention a few, that make the greateſt figure. The firſt is, that, according to this law, there cannot be in matter, ſuch a thing as reſiſtance without reaction; a bold aſſertion, which contradicts what appears to be the fact in common apprehenſion; and which therefore ought not to be admitted, without a very compleat induction of particular caſes; more compleat, ſo far as I can learn, then hitherto has been attempted. But, not to reſt upon the negative, it appears clear, that, in the colliſion of hard bodies, there is reſiſtance without reaction: and I flatter myſelf with having made out above, that, were there reaction in this caſe, the effects of colliſion would be very different from what they are in fact. And this leads to another difficulty, that if reaction were, in every caſe, equal to action, *viz.* in hard bodies as well as in elaſtic; the effects of colliſion behoved to be the ſame in both. Two elaſtic balls meeting in oppoſite directions, rebound; becauſe, after their force is ſpent by colliſion, the ſame force is reſtored

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to each, by their mutual reaction; and the same must happen to hard bodies, if they have the same power of reaction. In a word, I cannot distinguish elasticity from a power of reaction; and a hard body differs from an elastic only in this, that the former has no power of reaction, but only of resistance, A third difficulty is, that, in the bulk of the instances commonly given to illustrate this law, I cannot discover an equality of action and reaction. And lastly, what appears the most unsurmountable difficulty is, that, excepting the case of elasticity now mentioned, and that of fluidity to be mentioned afterwards; I have not found a single instance of an effect produced by the action of one body upon another, but what may be fairly explained by acknowledged principles, laying aside altogether this supposed equality of action and reaction. And if no fact can be condescended on, to prove the point with regard to hard bodies; why should we adapt this as an universal law of motion or of matter? For surely it is irrational to admit of any law or principle without evidence.

These difficulties will be best illustrated by going to particulars. And with regard to

Sir *Iſaac*'s firſt inſtance, of a man's preſſing a ſtone with his hand; we muſt diſtinguiſh betwixt a moveable and immoveable obſtacle. If a man preſs an immoveable obſtacle with his hand, I cannot here diſcover any thing but mere reſiſtance, which is not to be overcome by the force of the hand. This is an effect which muſt follow from the impenetrability of matter, without neceſſity of taking under conſideration, any other of its properties. I diſcover here no reaction. There is no counter-action or impulſe in the ſtone, to correſpond to the action or impulſe of the hand. There is nothing but reſiſtance, which is indeed an action, but an action which is not exerted upon the hand: the action is exerted within the reſiſting body itſelf, to preſerve it from motion. And, if this reſiſtance ſhall be conceived as a counter-action, which it may be in a looſe way of thinking; I cannot however conceive any equality betwixt the prſſure of my hand and this ſuppoſed counter-action of the ſtone. At this rate, the counter-action of the ſtone is equal to every degree of impreſſed force, provided the impreſſed force be not great enough to move the ſtone; and ſo it will be equal at the

the ſame time to a thouſand different degrees of force. To talk of the equality of action and reaction in ſuch a caſe, means really nothing more than that the hand has not force enough to move the ſtone.

AGAIN, if the obſtacle be moveable, it coincides with another inſtance given, that of percuſſion, or the colliſion of hard bodies. Here indeed it is true, that there is as much force loſt by the one body, as is generated in the other. But then this is plainly the effect of the *vis reſiſtentiæ* as explained above; and therefore it is unphiloſophical to introduce any other principle by which to account for this phoenomenon. At the ſame time, I cannot diſcover any equality of action and reaction in this caſe; on the contrary, it is eſtabliſhed by infallible experiments, that the impreſſed force or action is always greater in a determined proportion, than the reſiſtance or reaction. And it is this very proportion which produces the effect of an equality of motion before and after impulſe; or, in other words, the acquiſition of as much force by the one, as is loſt by the other.

ANOTHER inſtance is of a horſe drawing a ſtone by a rope; and it is ſaid, that the ſtone draws the horſe as much as the horſe the ſtone. Were this ſo, there could be no motion. All I can diſcover here, is mere reſiſtance in the ſtone, occaſioned partly by its *vis reſiſtentiæ*, and partly by friction. And after this reſiſtance is overcome, which ſpends or exhauſts ſo much of the horſe's ſtrength; the ſtone is drawn along with the horſe's remaining ſtrength, without any further reſiſtance.

WITH regard to gravitation, which is given as another inſtance of the equality of action and reaction; it is very true, that there is an equal tendency in every particle of matter to every particle; but I am at a loſs what further can be made of this, than that ſuch is the law of gravitation. Why then ſhould we endeavour to account for this effect by any ſeparate law? I might add, that this at beſt is a doubtful inſtance of the mutual actions of bodies upon each other. It will perhaps, upon examination, be found the more ſafe opinion, that bodies act not upon each other by the power of gravity. But of this afterwards.

KEILL, in explaining this law of nature, ſeems to confine it to the percuſſion of bodies;

bodies; and refts it wholly upon this fact, that equal changes of motion are produced upon collifion; which is, in other words, that, whatever force is loft by the one body, an equal force is generated in the other. And indeed it appears probable, that the law has been invented chiefly to account for this effect: if fo, it is extremely rafh in philofophers to apply it, as they do, in every cafe where there is the leaft appearance of action and reaction, as if it were an univerfal law of nature that muft obtain in every cafe whatever. It was not difcovered, it would feem, that the equality of motion before and after percuffion, is the genuine effect of the *vis refiftentiae*. Therefore, to account for this phoenomenon, a new law behoved to be invented; which, to difguife the matter, muft be confidered as a general law: for, had it been plainly fpoke out, what in effect is faid by Dr *Keill*, that this law applies only to the percuffion of bodies; every perfon would be fenfible, that accounting for the law of percuffion in this manner, was doing no more than repeating the fact itfelf in different words. For to fay that the actions of two bodies in percuffion are equal and in oppofite directi-

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ons, is, in an obſcure and indiſtinct manner, really ſaying no more, than that a force is generated in the one body, equal to that which is loſt in the other.

In accounting for the preſſure of fluids *undequaque*, *Muſchenbroke* reaſons thus; he ſuppoſes the water in a veſſel to be compoſed of perpendicular columns of ſpherical particles, one reſting upon another from the ſurface downwards, and all reſting upon the bottom of the veſſel. Thus the bottom of the veſſel is preſſed downwards with the weight of a column: and, becauſe action and reaction muſt be equal, therefore, ſays our author, the bottom of the veſſel muſt preſs upwards with the ſame force, *i. e.* with a force equal to the weight of the column; whence, ſays he, the whole preſſure upwards, muſt neceſſarily be equal to the whole preſſure downwards.

So far our author's argument appears concluſive, that, admitting the law of action and reaction, a preſſure *undequaque* muſt be the conſequence: and as the law is not confined to fluids, but is ſuppoſed to be an univerſal law of matter; a preſſure *undequaque* muſt not only be the conſequence in fluids, but in

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ſolids equally. Here then is a fair *dilemma*. We muſt either acknowledge a preſſure *undequaque* in ſand, powder, and indeed in all looſe bodies, as well as in fluids; or confine this law of the equality of action and reaction to fluids. Can we remain a moment in doubt betwixt theſe oppoſites? We are certain that a preſſure *undequaque* is not a property of ſolids. The equality of action and reaction is but an aſſertion without evidence. What remains then, but that we adhere to the former, and reject the latter, except as to fluids. And thus our author luckily, tho' without intention, has furniſhed a very convincing argument againſt the univerſality of this ſuppoſed law of action and reaction. If an *undequaque* preſſure is an effect of this law, it follows clearly, that this law takes not place in ſolids, at leaſt not univerſally, as in fluids.

OF GRAVITY.

GRAVITY is one of the moſt remarkable properties that belong to matter. By this property, which is univerſal, every particle of matter tends to every other particle; whereby, if not obſtructed, all matter would be gathered into one maſs. As every particle of matter has this tendency, the tendencies of any two bodies to a mutual union, muſt be in proportion to their quantities of matter. And it is obſerved that the force, which, at different diſtances, is exerted by two bodies in order to be conjoined, is in the reciprocal proportion of the ſquares of theſe diſtances. It is by this property that the heavenly bodies move round the ſun, and are kept from flying off in a tangent: and it is by the ſame property that bodies left free at any height, fall to the earth with an accelerated motion. When applied to the latter, it is commonly called *gravity*; when to the former, *attraction*. Sir *Iſaac*

Iſaac Newton, to whom the great diſcovery was reſerved, that the deſcent of heavy bodies, and the curvilinear motion of the planets, are effects of the ſame cauſe, chooſes to talk of this property of matter with great circumſpection and reſerve. He pretends only, in his *Principia*, to have aſcertained the facts, without venturing to point out the cauſe. In the general *ſcholium*, which concludes that elaborate work, he ſatisfies himſelf with having explained "the motion of the "celeſtial bodies, and of the ſea, by the force "of gravity, without aſſigning the cauſe of "gravity." He only obſerves, "That gravi-"ty muſt be the effect of ſome cauſe, which "penetrates into the very center of the ſun "and planets, and which acts not in propor-"tion to the ſurfaces, but the ſolid quantity "of matter; its action only decreaſing in a "duplicate *ratio* of the diſtances." And adds, "That he has not been able to find "out from phoenomena, the reaſon of theſe "properties of gravity, and that he does not "chooſe to deal in hypotheſes." It need not be ſurpriſing, that this great philoſopher ſhould be reſerved upon the cauſe of a theory ſo extenſive and ſo wonderfull, when it

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was his own child. New difcoveries are always received with fome degree of hefitation: becaufe it is the effect of novelty to produce doubts as well as furprize. But now, that this theory is fully eftablifhed by habit, as well as by reafoning, and has got a firm hold of the mind; it is not obvious, why later philofophers fhould affect the fame referve. For my part, I cannot fee any difficulty of explaining the caufe of attraction or gravity, more than of explaining the caufe of a body's continuing in the fame degree of motion with which it begins to move. And this I fhall now attempt.

It is above eftablifhed, that the continuation of motion in bodies muft be an effect of a power inherent in all matter, of preferving that degree of celerity which is once beftowed upon it. The body, by this power, is directed to move in a ftreight line. But we perceive nothing in the nature of this power to confine its operation to a ftreight line, more than a curve. And we may fuppofe the power ftill further varied, to make the body, inftead of moving in a ftreight line, or in a curve, to direct its equable motion towards any other given body, within a certain diftance ſtance. In general, as we have no means to diſcover power in any being but by the effects produced; ſo the nature and operation of the power are only to be diſcovered by the ſame means. There is nothing in our conception of any power to confine its operations to one law more than another. And this leads to the cauſe of gravity or attraction. If we admit of the *vis inſita*, the operations of which may be varied in a thouſand different ways; it is equally eaſy to conceive a power in matter by which every particle has a tendency to be united with every other particle. 'Tis but varying the *vis inſita* in the two following particulars to have an exact deſcription of gravity. Firſt, That it have the effect of beginning motion, as well as of continuing it; and, next, That it direct the body to which it belongs, not in a ſtreight line, but towards every body great and ſmall within its ſphere of activity.

THE grand difficulty which puzzles foreign philoſophers is, to admit of a power in a body to draw other bodies to it; for this is their conception of attraction, ſuggeſted by the term itſelf. They obſerve, that ſuch a power is inconſiſtent with a general maxim, that no being can act where it is not; which would involve this evident abſurdity, to ſeparate the being from its actions. And, from theſe premiſes, they juſtly conclude, that one body cannot act upon another at a diſtance. It muſt be confeſſed, that attraction is an unlucky term, ſince it has led philoſophers into the above miſtake; tho' Sir *Iſaac Newton* cannot be juſtly blamed, who made uſe of a term invented to his hand. By varying the conception of attraction, and by conſidering it as a power in matter not to draw other bodies to it, but to move itſelf towards other bodies, the difficulty vaniſhes.

But upon this idea of attraction or gravity, it may be ſuggeſted, that there can be no reaſon, why the power in exerting itſelf, ſhould keep pace with the diſtance of the object towards which its force is directed. The diſtance of the object, it will be ſaid, can have no effect to diminiſh the force, when, by the ſuppoſition, the action of the one body is not exerted upon the other, but upon itſelf. This has the appearance of a difficulty, and no more but the appearance. If matter has a power to act in any one caſe, its actions may be varied by any aſſignable law. And in particular to imagine a power in a body

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impelling it towards a body at hand, with a greater force than towards one at a diſtance, is in reality not more difficult, than to imagine it exerting always the ſame force, without regard to diſtance.

It is not improbable, that the above mentioned objection, of a body's acting where it is not, has led *Leibnitz* and other foreign philoſophers, to adopt the vortices of *Des Cartes*, rather than Sir *Iſaac Newton*'s theory; liable, according to their notion of it, to an inſuperable objection. Yet there cannot be conceived a more whimſical hypotheſis, than that of a fluid circulating about the ſun, in which the planets are involved and carried along, like a ſhip in a torrent. The ingenious *Mapertuis*, in his eſſay upon attraction, moves ſeveral objections to it, even as new moulded by later writers. He objects in the firſt place, that the planets do not move in a circle, but in an elipſe. In the next place, that they do not move with celerities proportionable to their diſtances from the ſun; which they behoved to do, if carried along in a *vortex* moving, like a wheel, equably round the ſun. Theſe objections are juſt; but, in my apprehenſion, he has over-

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looked the moſt ſolid and weighty objection. Whatever cauſe can be aſſigned for the motion of this fluid round the ſun, will equally apply to the motion of the planets, without neceſſity of introducing a new fund of matter, of the exiſtence of which we have no evidence. More particularly, motion is obviouſly an effect which muſt have ſome cauſe. This cauſe muſt either be a continued interpoſition of a voluntary agent, or a power given to matter to continue itſelf in motion. A piece of matter cannot move in a ſtreight line, without one or other of theſe cauſes; and as little in a circle. Therefore, to make this ſuppoſed fluid to circulate about the ſun, one or other of theſe cauſes muſt operate; and one or other of theſe cauſes is ſufficient to account for the motion of the planets, without neceſſity of inventing a fluid to produce the effect. *Des Cartes* therefore, and his followers, are guilty of the ſame ſort of abſurd reaſoning, for which we juſtly laugh at the poor *Indian*, who was forced to invent an overgrown elephant to reſt the earth upon, and an overgrown crab to be a footſtool to the elephant.

WHETHER

WHETHER the ſame inconſiſtency of a body acting where it is not, has moved out *Britiſh* philoſopher to invent an aetherial *medium* much rarer than air, as the cauſe of gravitation, I cannot ſay. In the twenty firſt query, at the end of his optics, he obſerves, "that this *medium* is much rarer within the "denſe bodies of the ſun, ſtars, planets and "comets, than in the empty celeſtial ſpaces "between them. And, in paſſing from them "to great diſtances, that it grows denſer and "denſer perpetually; and thereby cauſes the "gravity of thoſe great bodies towards one "another, and of their parts towards the "bodies; every body endeavouring to go "from the denſer part of the *medium* towards "the rarer." I am not better ſatisfied with this hypotheſis than that of *Des Cartes*. For, without loſing time upon a minute enquiry into the very peculiar properties with which this ſuppoſed aether muſt be endued, to produce the effects aſſigned it; the ſame objection lies againſt it, that is above urged againſt the vortices of *Des Cartes*, that it is a new ſpecies of matter invented without evidence, and indeed without neceſſity: for it is as eaſy to endue the planets with a power which impells them towards the ſun, as to endue this ſuppoſed aether with a power which repells it from the ſun. Therefore the argument which is juſtly urged by this author himſelf againſt a *plenum* in the 28th *query*, at the end of his opticks, may be urged with equal ſucceſs againſt this ſuppoſed aether, " It is of no uſe; and, as there is no " evidence for its exiſtence, it ought to be " rejected."

I muſt own, at the ſame time, great reluctance, to embrace a doctrine which overturns, or ſeems to overturn, the moſt beautiful part of Sir *Iſaac*'s own theory, and that which affords the ſtrongeſt conviction of its truth, *viz*. the connexion, by a common cauſe, betwixt the curvilinear motion of the planets, and the deſcent of bodies towards the center of this earth. Suppoſing an aetherial *medium* to be the cauſe of the former, it cannot well alſo be the cauſe of the latter. Among other reaſons, this occurs, that the different denſities of the ſuppoſed aether, on the oppoſite ſides of a bit of matter left free in the air, muſt be, *quam proxime*, nothing. This muſt be yielded; and the conſequence is, that the bit of matter involved in

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a *medium* which preſſes equally *undequaque* or *quam proxime*, will either remain at reſt, or move with a very ſlow pace. But this is contrary to fact; for the acceleration of falling bodies, within the leaſt perceivable diſtance, is perceivable.

THE power of gravity has a ſingular property not commonly attended to, that its force is not ſpent by being exerted upon any ſingle body. Its whole force is exerted upon a multitude of bodies, all at the ſame time. Thus the earth tends at once both to the ſun and moon; and the whole force of its tendency is directed to each, juſt as much as if the other were annihilated. Thus the ſun is impelled towards the whole ſyſtem of planets and comets; and its total impulſe is directed upon every one of them at the ſame inſtant. It cannot fail to appear ſingular and ſurpriſing, to find a force exerted in ſo many different and even oppoſite directions at one time, without being diminiſhed by diviſion. Yet all this proceeds from one ſingle property, that every particle of matter tends to every other particle; and conſequently, by encreaſing the quantity of matter indefinitely, the ſum of the tendencies of any one

particle

particle of matter, may be greater than any aſſignable quantity. But nature is wonderful, as well as various, in her operations; and we ought to ſubdue our incredulity, when, upon ſearching into her works, we find ſo many appearances different from what we are accuſtomed to in common life.

Having thus eſtabliſhed, that there is a power inherent in matter, by which every particle tends to be united to every other particle, and which power is in continual action without ever being ſuſpended but for a moment; the curvilinear motion of the heavenly bodies, as well as the deſcent of bodies towards the center of the earth, are obviouſly the effects of this power. With regard to any particular planet, the earth for example, we have only to ſuppoſe, that it is once ſet in motion by the hand of the Almighty; it will endeavour to preſerve itſelf in the ſame degree of motion by the *vis inſita*. Having likewiſe a tendency to the ſun, theſe two powers conſtantly acting in different directions, carry it round the ſun. At the ſame time, the proportion of theſe two forces is ſo adjuſted, that the earth, as well as all the other

other planets, defcribe an elipfis, in one of the *foci* of which the fun is placed.

In the defcent of heavy bodies towards the center of the earth, the force of gravity is fuppofed to be invariable. For, tho' this force leffens by diftance, yet the diftance of any two points, from which we have accefs to drop a body, is fo inconfiderable in refpect of the diftance of either from the center, that it is left out in all calculations as imperceptible. Hence the following queftion is readily fuggefted, How comes it, that the power of gravity which in this cafe is fuppofed invariable, has the effect of accelerating the motion of a falling body? It fhould be thought, that the power of gravity, fuppofing it invariable, muft produce an equable motion downwards, without acceleration; juft as the *vis infita* does, laying afide gravity, in whatever direction the body is thrown. For in general, as effects muft always correfpond with their caufes, every force which is uniformly exerted without diminution or augmentation, muft produce an equable motion, without acceleration or retardation: and, on the other fide, every varied effect

which

which is gradually diminiſhed or augmented, muſt proceed from a varied cauſe.

THIS difficulty is endeavoured to be explained in the following manner. Gravity, it is ſaid, has a remarkable property, "That "it acts with the ſame force upon a body "that is already in motion, as upon a body "that is at reſt, ſo as to produce equal acce-"lerations in falling bodies in equal times *." But this manner of conceiving the effect of gravity, whatever obſcurity it may occaſion, tends not to remove the difficulty. One thing is extremely clear, that we have no means afforded us, to eſtimate the force of any power, other than the effects produced by it; which holds in general, whether the power be internal or external. From the uniformity of the effects, we conclude the *vis inſita* to be an uniform power; and we have the ſame foundation, from its varying effects, to conclude gravity to be a varying power. In ſhort, when acceleration is produced by an external cauſe, it will be evident to every one, that the force of the impulſive cauſe, muſt be continually increaſing, to produce ſuch

* *M'Laurin's* account of *Newton's* philoſophy, p. 248.

ſuch an effect; and it will be a hard taſk to aſſign any juſt foundation for a difference in this particular, betwixt external and internal force.

We muſt therefore yield to the difficulty, becauſe it is unſurmountable. It is very true, that a body has, *quam proxime*, the ſame weight at the diſtance of ten foot from the ſurface of the earth, as at the diſtance of a hundred: and the force of gravity is, *quam proxime*, the ſame when a body is dropt from the hand at theſe different diſtances. But then, tho' gravity is ſuppoſed invariable, where the difference of the diſtances is ſo ſmall; yet this only holds where other circumſtances are the ſame: that is, where the body is either at reſt, or moves with the ſame celerity. For it is extremely clear, that, in the perpendicular deſcent of a body, the force of gravity varies every inſtant, and turns greater in its progreſs downwards. And indeed gravity cannot otherways produce acceleration, more than the *vis inſita* does. Nor is there any thing ſingular in this property of gravity. The *vis inſita* has a property ſomewhat ſimilar: for, tho' it continues invariable after the body is ſet in motion,

tion, yet it is not always the ſame. It keeps pace with the impreſſed force, and is very different, according to the different forces with which bodies are put in motion. In a word, ſince effects muſt always correſpond with their cauſes, the gradual increaſe of the force of a falling body, muſt infer the gradual increaſe of the power of gravity, which is the cauſe of the motion. Or rather, conſidering the matter in a different light, perhaps more accurately, the tendency or *niſus* of a body towards the center of the earth, which is greater in motion than at reſt, and which gradually increaſes with the velocity of the motion, being nothing elſe but the exertion of its power of gravity, makes it evident that the power of gravity is continually increaſing from the beginning, to the end of the motion.

In accounting for this phœnomenon, I have neglected the *vis inſita*, tho' it may probably act in conjunction with gravity in the deſcent of bodies towards the center of the earth, as well as in the curvilinear motion of the planets. For, if the force of gravity be ſuppoſed invariable, the addition of the *vis inſita*, which is alſo an invariable force, will only

only bring out a greater invariable force, which can never produce an acceleration of motion. The acceleration therefore muſt be attributed to gravity alone, the force of which, in the beginning of the motion, is ſuppoſed to be leſs than any aſſignable quantity, whereby a body, ſet in motion by the force of gravity, paſſes through every degree of velocity from reſt, till it acquire that velocity which it has when it touches the ground.

I have dwelt the longer upon this property of gravity, becauſe there is connected with it another property, which is, that in motion, the action of gravity is not to be conſidered as one action exerted through a length of time, but as a number of different actions exerted inceſſantly. For, if the gravity of a body in motion continues not the ſame any two ſucceſſive moments, but is continually varying, the action muſt vary with the power; and conſequently is not one, but a number of different actions. Gravity in a body at reſt, is, like the *vis inſita*, one invaried power, which produces one invaried action exerted through a length of time. But, when once the body is put in motion, we muſt neceſſarily admit, firſt, that gravity is exerted by repeated impulſes ; and next, that theſe impulſes, continually turning ſtronger, form an increaſing ſeries, holding an exact proportion with the growing velocity, of which theſe impulſes are the cauſe.

AND this leads us readily to conceive the operation of gravity in the aſcent and deſcent of bodies. A ſtone, in its perpendicular motion, is carried upwards by its *vis inſita* counter-acted by gravity : in its return, it is carried down by gravity without any counter-action. At firſt view, the aſcent will be conſidered as the operation of two powers acting in oppoſite directions ; the effect of which commonly is, to produce an equable motion with the difference of the forces. And, were the action of gravity one uniform action, like that of the *vis inſita* ; ſuch would truly be the effect : but, as gravity is exerted by repeated impulſes, a different effect muſt follow. The very firſt impulſe of gravity takes off from the force of the *vis inſita* ; what remains is leſſened by the ſecond impulſe ; and ſo on, till the *vis inſita*, gradually decreaſing by the repeated counter-actions of gravity, be annihilated altogether. In the

return

return downwards, gravity being exerted without an antagonift, every new impulfe produces an acceleration of motion; and, thefe impulfes being repeated inceffantly, the acceleration goes on, till the ftone touch the ground.

It remains only to be obferved, that, as the force of gravity is accurately proportioned to the velocity, gravity muft produce the fame effect, whether its force be exerted in the direction of the motion, or contrary to it; and that the acceleration in the former cafe muft be equal to the retardation in the latter, in any two points where the velocities are equal. Thus, upon the whole, the gradual retardation of upward motion, is equal to the gradual acceleration of downward motion. The fame feries of numbers reprefents both; a progrefs from the fmalleft to the greateft number being fimilar to acceleration; the contrary progrefs, to retardation. And hence it is that thefe two contrary motions are performed precifely in the fame time.

It is now eafy to account for the different velocities of a planet in the different points of its orbit. The motion of a planet may be be juſtly repreſented by perpendicular aſcent and deſcent. A planet moving from the ſun, reſembles a ſtone moving from the earth upwards; and, in its return towards the ſun, reſembles the ſame ſtone returning to the earth, after its upward motion is ſpent. And, indeed, by prolonging the greater axis indefinitely, an ellypſe coincides at laſt with a ſtreight line; and the motion in the former coincides with the motion in the latter. Conſidering then that gravity and the *vis inſita* are the two powers which govern motion in the curve as well as in the ſtreight line; it muſt follow, that the law of motion is the ſame in both.

OF

OF THE FORCE OF BODIES IN MOTION.

IN explaining the firſt law of motion, that every body perſeveres, as much as in it lies, in its preſent ſtate of reſt or motion; I have touched at that remarkable power which all matter is endued with, of moving on with its original velocity; and to which power I have choſen to confine the term *vis inſita*. I now proceed to unfold ſome further properties of this power. It appears clear, that the *vis inſita* might have been ſo ordered, as to produce an accelerated or retarded, inſtead of an uniform motion. And it might have been ſo ordered as to yield to the ſmalleſt oppoſing force, which is the caſe of the *vis reſiſtentiae* when the body is at reſt. There is no difficulty to conceive a body endued with ſuch a property, as to preſerve itſelf in its original degree of motion, where there is no obſtruction, and yet to yield to the

the ſmalleſt reſiſtance or oppoſing force. But the *vis inſita* is plainly not of this nature. The body not only preſerves itſelf in motion where there is no reſiſtance, but exerts a certain force againſt every thing that reſiſts its progreſs. And it is with a view to this force exerted in motion, as well as with a view to the reſiſtance exerted at reſt, that Sir *Iſaac Newton*, in his third definition, deſcribes the *vis inſita* to be "that power in matter, by "which every body, as much as in it lies, "perſeveres in its preſent ſtate of reſt or mo- "tion." The force, however exerted in motion, is eſſentially different from that exerted at reſt. The latter is mere reſiſtance; the former is an impulſive force, by which the body endeavours, *quantum in ſe eſt*, to overcome every obſtruction to its motion. The force of reſiſtance is above aſcertained, and a rule laid down for eſtimating it, about which all philoſophers are agreed. They differ widely about the impulſive force of the *vis inſita*: the *Germans* holding it to be as the ſquares of the velocities; the *Engliſh* and *French* holding it to be ſimply as the velocities. The diſpute is ſpun out to a great length: the parties, as is uſual, being more ſolici-

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tous to enforce their own arguments, than to give fair play to thoſe advanced by their antagoniſts. And, by profuſion of writing, the point is rendered ſo perplexed and intricate, that there appears not a better way to come at the truth, than neglecting the arguments on both ſides, to apply directly to facts for a ſolution, as one would do upon a point newly ſtarted.

CONSIDERING the matter abſtractly, I cannot find that there is any inconſiſtency in the *German* hypotheſis. As we may conceive the *vis inſita* to be exerted according to any aſſignable law, ſo we may conceive it to be exerted in any aſſignable degree. When we conſider the *vis inſita* as employed in preſerving the ſame degree of velocity, it follows neceſſarily, that its force, which is the cauſe, muſt be meaſured by the velocity, which is the effect. But when we conſider another branch of its operations, *viz.* its *niſus* to overcome every obſtruction to the equable motion, we have no other means to eſtimate this *niſus*, but its effects; for, however probable it may be, it does not neceſſarily follow, that this *niſus* muſt alſo be meaſured by the velocity. Facts and experiments, then, are the only means

means by which we can determine this controverſy; and, when we look about for facts and experiments, there can be none leſs liable to exception than thoſe, where the *vis inſita* of one body is oppoſed to the *vis inſita* of another, by colliſion and percuſſion. If a pendulum of one pound weight be let down from ſuch a height, as to acquire at the loweſt point the velocity of 10, it will exactly balance a pendulum of two pounds weight, let down from ſuch a height as only to acquire the velocity of 5, and the two bodies will be at reſt upon their colliſion. The remaining at reſt upon contact, is compleat evidence, that the forces of the two bodies are equal; conſequently that their forces are as their quantities of matter multiplied upon their velocities; and therefore that the force of the ſame body is always as its velocity. A thouſand different experiments of the ſame kind, make it evident, that the force of every moving body is in the preciſe ratio of its velocity. And thoſe experiments muſt be abſolutely unexceptionable, in which we compare the force of one body, with the force of another equal body moving with a different velocity; for this is in effect comparing the different

ferent forces of the ſame body moving with different velocities.

It being thus aſcertained, that the force of a body moving by the *vis inſita* is always in proportion to its velocity, we have reaſon to conclude, from analogy, that the ſame rule holds where bodies move by the force of gravity. And accordingly this is alſo made evident from a courſe of unexceptionable experiments.

The *German* philoſophers, in their experiments, have been miſled, by aſcribing to the force of the moving body, effects which obviouſly reſult from other cauſes. Not to lengthen out this paper too much, I ſhall ſatisfy myſelf with giving an inſtance or two. It is a fact agreed upon, that a pendulum ſwung upwards with a double velocity, will reach a quadruple height. Hence it is inferred by *Leibnitz*, and his followers, that the forces muſt be as the ſquares of the velocities; ſeeing the effects produced by the different forces, *viz.* the ſpaces gone through, are as the ſquares of the volocities. And could it be juſtly maintained, that the ſpace run through is purely the effect of the momentum or force, with which the body is thrown upwards, the argument would be conclusive. But it is not so. Laying aside gravity, and the resistance of the air, a body thrown upwards with the smallest force, will move on *in infinitum*. It is by the operation of gravity, and the resistance of the air, that motion ceases, when the body arrives to a certain height. The retardation, therefore, of motion, in bodies thrown up with different velocities, laying aside the resistance of the air, may be a measure of the force of gravity, of which it is the effect; but can never be a measure of the force with which the body is thrown up, of which it is not the effect. And, from the fact of a body's arriving at four times the height with double the velocity, to infer, that the momentum, at its outsetting, must be as the square of the velocity, is really not more just, than to infer, when one body is let drop from four times the height of another body, that it must acquire four times the force of the other body, tho' it acquire but double its velocity; which does not afford the shadow of an argument. When a body is thrown up with a double velocity, and consequently with a double force, the reason why it ascends four times its former height, is plainly this, that the counteraction of gravity, while it has a double force to

struggle

ſtruggle with, has but half the time in any given ſpace to produce its effect; and therefore this body, before its motion be totally ſubdued, muſt aſcend four times the height that it aſcends when thrown upwards with the ſingle velocity and ſingle force.

But the argument which the *Leibnitſians* truſt moſt to, is founded upon experiments of the falling of balls upon clay, or other ſoft body; where it is eſtabliſhed, that the impreſſions made, are in proportion to the heights from whence the balls are let fall, and conſequently to the ſquares of the velocities. From theſe experiments it is inferred, that the forces muſt alſo be as the ſquares of the velocities; it being taken for granted, that the impreſſions made upon the clay muſt be the meaſure of the forces or momenta, of which they are ſaid to be the immediate and direct effects. The error of this reaſoning is of the ſame kind with the former. The retardation of the motion of a body falling through a reſiſting medium, is not the effect of gravity, and therefore cannot be the meaſure of its force. It is the meaſure of the reſiſtance of the medium, becauſe it is the effect of that reſiſtance. All the world knows, that when bodies move through a fluid, or any

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ſoft matter, a double force produces a quadruple effect; which may be accounted for in the ſame manner with the retardation which happens in the aſcent of heavy bodies. A double force makes the body aſcend four times the height before it be ſtopt by the counteraction of gravity. And it is equally reaſonable to ſuppoſe, that when bodies fall into reſiſting mediums, a double force will carry the body a quadruple ſpace downwards before the motion be quite ſtopt by the reſiſtance of the medium.

I ſhall conclude this eſſay with the following obſervation, that the ſeveral powers I have aſcribed to matter, are in nothing ſimilar to occult qualities. The error of thoſe who dealt in the doctrine of occult qualities was, in attributing every different effect to ſome quality or cauſe confined to that ſingle effect; which was in reality ſaying no more, than, what all the world knows, that every effect muſt have a cauſe. This was not advancing a ſingle ſtep in knowledge, but amuſing one's ſelf with words in place of things. The powers I have attributed to matter, reſolve into general laws, each of them productive of a thouſand different effects. And we have

no

no reaſon to doubt that mere matter may be endued with various powers, as well as animals are. When we ſay, that ſeeing, hearing, taſting, touching, ſmelling, proceed from ſo many different powers or ſenſes, no perſon conſiders this as aſcribing effects to occult cauſes. Have we not the ſame reaſon to conclude, that there is a power in matter to continue itſelf in motion, another power to reſiſt a change from reſt to motion, and a third power to unite itſelf with every other piece of matter; when we perceive effects which as directly reſult from theſe powers, as ſeeing does from the ſenſe of ſeeing. It may be true, for ought we know, that there is ſome more general principle in matter, which is the foundation of all theſe powers. But ſtill it is gaining ground, and knowledge, to trace effects to their cauſes, and to diſcover that many different effects proceed from the ſame cauſe. To be diſſatisfied with ſuch diſcoveries, merely becauſe of the poſſibility of other cauſes ſtill more general, which lie hid from us, is in effect to be diſſatisfied with all knowledge whatever; becauſe, however far we penetrate, we never can be certain, that we are arrived at our journeys end.

Some

ARTICLE II.

Some Remarks on the Laws of Motion, and the Inertia *of Matter; by* JOHN STEWART, M. D. *Fellow of the Royal College of Physicians and Professor of natural Philosophy in the University of* Edinburgh.

THE Laws of motion, as delivered by Sir *Isaac Newton*, are all founded on the supposition, that body of itself is absolutely inactive. And inactivity is now commonly ascribed to matter as one of its general properties; body being defined to be whatever is extended, impenetrable, divisible, moveable, and inactive. At the same time, every one knows, that active powers are continually employed through all the parts of nature. The life and motion of animals, the production and growth of vegetables, the attractions of gravitation and cohesion, with other instances of the same kind, are always present to our view.

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PHILOSOPHERS who aſſert the inactivity of matter, ought therefore to be able to give ſome good reaſon, why they refuſe to allow it the free poſſeſſion of ſuch powers in its own right; and why it is only to be regarded as a paſſive inſtrument, under the direction, and ſubject to the dominion, of ſome ſuperior being. But, in order to aſcertain the true notion of the *inertia* of body, the proper method is to begin with the ſimpleſt caſe; and to conſider body as a lifeleſs inanimated maſs, without weight, attraction, repulſion, or any tendency to begin motion, till acted upon by ſome foreign external cauſe. And ſurely it would be ſomewhat ſurpriſing, if body even in theſe circumſtances, ſhould be found to diſcover any activity.

IT hath been imagined however, by ſome people, "That, if body were utterly unactive, "the ſmalleſt force would be ſufficient to "move a great body and a little body, with "equal velocity: and that the ſame power "might communicate a great velocity or a "ſmall velocity to any body.*" Mr *De Mairan* in *Mem. de l' Acad.* 1728 ſeems to entertain

* Eſſay I. p. 9. 21. 24.

tain the ſame opinion, when he aſks, if the *inertia* of matter (which he explains to be that reſiſtance which body makes to its being drawn out of reſt, and receiving a determined motion, and which is more or leſs in proportion to its maſs) may not be the effect of ſome motion; and if it ought not to be conceived as an actual force, acting by ſome ſecret mechaniſm. And the author of an *Eſſay on Spirit* lately publiſhed, maintains in like manner, that there is an active reſiſtance to the beginning of motion in every body; tho' indeed he attributes that activity to an immaterial cauſe. And Meſſ. *Buffon* and *Needham* too, who have carried the activity of matter to the higheſt pitch, have drawn one argument in ſupport of their ſyſtem from the reſiſtance of body to motion. *Needham*'s Obſervations p. 435.

To give the queſtion a fair examination, we ſhall, inſtead of ſpeaking about matter or body, ſuppoſe for once, a ſubſtance before us quite inactive of itſelf, which is extended, impenetrable, finite, and conſequently moveable. Let us try if any leſs reſiſtance to motion can be expected here, than is actually to be met with from common matter.

Place

Place any maſs of ſuch a ſubſtance at reſt. It cannot begin motion of itſelf by the ſuppoſition. But an active animated being, as for example, a man, can move it. Some effort muſt certainly be made, ſome power exerted, to produce this effect. It will never be pretended, that the ſame effort can move the maſs either with a great velocity or a ſmall velocity; that being as abſurd, as to ſay that a great velocity and a ſmall velocity are one and the ſame thing. In like manner, it muſt require one effort to move a ſmall quantity of this ſubſtance with a certain velocity, and a different effort to move a great quantity with the ſame velocity. The ſame energy of the agent, will never ſerve to move a given quantity of this ſubſtance, or double the quantity of this ſubſtance, with the ſame velocity; no more than it will move the ſame ſubſtance with different velocities. By this exertion of our own activity, we acquire the ideas of forces. The animated being (or the mind) is differently affected by different objects, whether of the ſenſes or underſtanding. And why ſhould it be affected in the ſame way, when a great ſubſtance and a little ſubſtance are moved by it, or when a great velocity and a little velocity are imparted to the ſame ſubſtance? When we endeavour to communicate motion to ſuch a ſubſtance, we muſt be conſcious of ſome kind of *feeling*; and theſe *feelings* moſt be different in different caſes. Thus the idea of *reſiſtance*, as it called, to motion, in the moſt inactive ſubſtance we can imagine, would be ſuggeſted to us from theſe *perceptions*; and is preciſely the ſame with what we experience daily in handling of matter. Nor does it ſeem poſſible to conceive an extended, impenetrable ſubſtance, diveſted of this kind of reſiſtance from *inertia*. The larger the ſubſtance is which we intend to move with a given velocity, the greater force muſt be applied: and, could we ſuppoſe it actually infinite, no finite force could move it at all.

THERE is a very extraordinary paſſage in Mr. *M'claurin's Account of Sir Iſaac Newton's Philoſophy*. P. 100. which, in reſpect of the high merit of the author, deſerves our attention. It is there ſaid, "That, for ought "we know, matter may be of kinds ſo different from each other, that the ſolid ele-

mentary

"mentary particles of the one, may have a " greater *inertia* than equal ſolid elementary " particles of the other kind." This conjecture, if true, would effectually overturn what hath been now advanced. But it appears to be equally inconſiſtent with his own account of *inertia*, and occurs only in a poſthumous work. The *inertia* of body is a negative quality, or a negation of all poſitive power, and therefore can admit of no degrees of greater or leſs in a given quantity of matter. Two bodies, or two elementary particles of equal quantities of matter, if they are inert at all, muſt have equal *inertia*, or require equal forces to move them with equal velocities. And, in every caſe, the *inertia* is proportional to the quantity of matter.

If a body left at reſt does not begin motion of itſelf, it is determined to remain in that ſtate, not from any real repugnance to motion, which is as conformable to its nature as a ſtate of reſt: but becauſe nothing is done without a cauſe. And, when acted upon by any external influence, it obeys without reluctance; the motion produced being in exact proportion to the moving cauſe. It has a conſtant ſuſceptibility of motion, and a perfect facility in receiving it. But we may as well aſk, why an inactive ſubſtance does not begin ſome degree of motion of itſelf? as, why different powers are requiſite to produce different motions? When people talk of the *reſiſtance* of matter at reſt, as of an *active power*, ſtruggling againſt any agent, and actively oppoſing it, they ſurely frame to themſeves ſome notion of force antecedent to all experience; and, they would do well to inform the world, in what manner this idea was ſuggeſted to them.

The *paſſive nature* of body is abundantly manifeſt, from its yielding to the leaſt conceivable action. The leg of a fly moves the whole globe of the earth. A man indeed cannot roll a tun ſo faſt as he can a tennis-ball: and we may find a horſe able to draw a loaded cart two miles in the hour, who cannot be prevailed upon to draw it four miles in the ſame time. But are not ſuch common *phaenomena* as theſe more naturally accounted for, from the ſluggiſhneſs or inactivity of matter, than from its ſuppoſed activity? A great body ſet in motion is one effect: a little body moved with the ſame velocity

velocity is another. A given body moved with a great velocity, is one effect; and when moved with a less velocity, it is a different effect. The old principle seems to apply well enough in this case, that effects are proportional to their causes.

It must be confessed, that authors in treating of the *inertia* of matter, and of its resistance to motion, are very apt to express themselves in terms that import a real activity; and which, if strictly understood, are inconsistent with *inertia*. Thus, when a person in a boat pulls a rope that is fastened to the shore, it is commonly said, that the man acts upon the shore in one direction, and that the shore, by its reaction in the opposite direction, pulls the man and boat towards it. Yet, notwithstanding such expressions, will any one seriously maintain, that the ground has an active power to produce any such effect? It is evidently the force of the man extending himself, that draws the shore with his hands one way, and at the same time pushes the vessel with equal force with his feet the opposite way. In like manner, if a person standing in a boat, pushes against the ground with a pole, it is not really the reaction of the ground that makes the boat recede; it is the force of the man that is the only moving power, acting equally upon both the boat and the ground. When a boat is rowed with oars, the oars are said to act upon the water in one direction, and the water to react upon the oars in the oposite direction, and to produce the motion of the boat. Yet it is plain, that the motion is intirely produced by the rower, who, in so far as he contributes to the motion of the boat, employs the oar as a *lever* of the second kind, where the body to be moved, or the boat, is placed betwixt the moving power and the *fulcrum*.

In the common account of the progression of animals, whether it be of walking, flying, or swimming, *Borelli* and all other authors content themselves with telling us, That the animal acts upon the ground, air, or water, in one direction, and that the reaction of the medium carries the animal forwards in an opposite direction. This is at best but too concise, not to call it a false theory: seeing it must be manifest that the medium, by any reaction which takes place here, can only consume as much force as is impressed

impreſſed upon it, and is utterly incapable of producing any kind of motion. The only immediate cauſe of the motion, being the active force of the animal which preſſes the medium one way, and its own body the other way, as might be particularly ſhown in every kind of progreſſion, if it were thought needful.

THUS, upon the moſt attentive examination into the ſtate and condition of body at reſt, we have found it to be perfectly *inert* or inactive. Body in motion next demands our conſideration. Suppoſing a body once put in motion by ſome agent or other, it behooves us to follow it out, and enquire what happens to it on this change of its ſtate. An extended, impenetrable, inactive ſubſtance has received a motion: Will it inſtantly ſtop when the immediate influence of the active power ceaſes? or will it perſevere in its new ſtate?

FROM what commonly happens in the motions of bodies, their gradual loſs of force and returning to a ſtate of reſt; people contract an early propoſſeſſion, and are ready to imagine, that reſt is the proper ſtate of body. But a diligent review of all the circumſtances, ſoon diſcovers, that body is equally in-

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different to either ſtate, of reſt or motion. Body at reſt does not change its ſtate of itſelf; no more does body in motion. A body at reſt requires no cauſe of its continuance; as little does a body in motion. This property of body, by which it perſeveres in its ſtate of reſt, or of uniform motion, in a ſtreight line, unleſs when forced to change it by ſome external influence, is called the *inertia* of matter.

We know not how motion is originally communicated at all; we know nothing of the nature of motion or force, but by experience. We can define neither; the ideas being quite ſimple. Natural philoſophy however takes it for granted, that ſuch a thing there is. Let a motion or force begin any way you pleaſe, we never ſee it ceaſe till it be deſtroyed. Why then ſhould we imagine a body ought to ſtop of itſelf; and that, to preſerve it in motion, a conſtant exertion is neceſſary, like that which produced it at firſt? What argument can lead us to aſcribe ſuch an activity to body? What ſhould determine the body to ſtop, if there be nothing to oppoſe its motion? Is there any experiment pointing that way? yes, "we

"may

"may be desired to reflect on what a person "feels within himself in walking, during "which a repeated activity is exerted to con- "tinue the motion *." But this, surely, can only be intended as an illustration of what is meant by the suposed activity of matter in motion, and not as a proof of its reality. For, every day's experience must teach us on the contrary, that it requires a great activity, sometimes more than we are masters of, to stop a begun motion in our bodies. How doth it appear, that the same effort is necessary to be continually exerted, which was employed at the beginning of the motion? We find a certain effort necessary to begin a motion in our own bodies; but, we should find no occasion for repeating it, were it not consumed or wasted upon other bodies. When we give ourselves one push forwards upon a smooth surface, such as ice for example, there is no need for a second immediately; and, were there no attrition nor resistance from the air, the motion would continue for ever. If a body set in motion, were to stop, retard, or any way change, its motion of it-

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* Essay I. p. 29.

ſelf; that would betray an inclination or tendency to one ſtate preferably to another; it would no longer appear equally indifferent to either, in which alone paſſivity conſiſts.

THE moſt general law of matter we diſcover by experience is, that every effect continues till deſtroyed by ſomething. Why then may not motion continue till it be deſtroyed, as well as the magnitude, figure, colour, or any other property of body? or even as well as the very exiſtence of matter? "Motion "is indeed a mode of exiſtence different "from all others, nor can we compare it to "any thing that is not motion*." But that is no reaſon why it ſhould change of itſelf, any more than theſe other modes. If the continuation of motion bore a nearer reſemblance to the continuation of any other effect, would that render it any plainer? Motion is an effect *ſui generis*; but we have an infinite variety of examples of its continuance. An active being preſſes forwards a certain quantity of an impenetrable inactive ſubſtance: is there not ſome effect produced here? ſomething then communicated to it? The body is put in

* Eſſay I. p. 18. 19.

in motion: why ought that motion to ceafe without a caufe? Action is neceffary to the production of motion, and no wonder if nothing can deftroy the effect of one action but another action. The trite maxim, *fublata caufa, tollitur effectus*, is not to be fo literally interpreted, as that an effect may not continue, after its caufe ceafes to act.

As we know fo little of the nature of that influence by which one being puts another in motion, or by which it excites motion in itfelf; with what reafon can we pofitively conclude its effects to be only momentary, when they every where appear to be fo permanent? When motion is begun in any body, we attribute it to a certain action which we cannot poffibly explain; when a body continues in the motion which it has once received, it appears to be a neceffary confequence of that action. We commonly indeed fay, that the body then moves on of itfelf; but the only queftion in debate is, Whether there be occafion for a continual recruit of force, new impreffions, new follicitations to motion, like to what were at firft exerted by the moving power?

UNLESS

Unless this account of the continuation of motion can be ſhewn to involve ſome abſurdity, why ſhould we ſeek for any other cauſe of it, than the force impreſſed by the external agent? We may be told perhaps, "That motion is a continued action:" and therefore body continuing to move, is active. But is not this either a begging of the queſtion, or taking hold of the ambiguity of language to ſupport it; inſtead of explaining, why a motion once excited ſhould ceaſe of itſelf? No doubt, we commonly ſay in the mechanical philoſophy, that one body acts upon another by impulſe, and the other reacts upon it. Nay, Sir *Iſaac Newton* himſelf ſpeaks of matter acting by *inertia*, or *vis inertiæ*, which, tranſlated literally, would ſeem to import an impotent power, or active inactivity. Such terms cannot eaſily be avoided, without introducing endleſs circumlocutions. If more accurate expreſſions can conveniently be ſubſtituted in their place, it would be no diſſervice done to philoſophy. But we are not to diſpute about words, when the meaning is clear; or, to confound things entirely diſtinct, becauſe they happen frequently to be called by the ſame name. The

true

true diſtinction between active force, properly ſo called, and the *vis inertiæ*, ſeems to conſiſt in this, that ſome beings can begin motion where there was none before, either in themſelves, or in the body to be moved: in other beings, the motion, when begun from ſome external cauſe, is continued for want of a power to ſtop it. The firſt of theſe may be properly denominated *active beings*; the other, even when in motion, have no proper activity of their own.

THERE can indeed be no action upon matter without a production of motion, or at leaſt a tendency to it. But it doth not therefor follow, that motion and action are ſynonymous terms. Why may not an active being communicate motion to matter, without moving itſelf? Is not that every whit as eaſy as ſelf-motion? But, whether the agent moves itſelf or not, it muſt have a power of acting, previous to all motion; otherways it would be impoſſible for matter or any other being to begin motion of themſelves. A due attention to this remark, will afford a direct anſwer to what hath been ſo much inſiſted on, "That motion is action." Motion is not action, but the effect of an action.

ACTIVITY

Activity may be variouſly applied. An action may be with-held by an oppoſite and equal action, as in the caſe of two contrary preſſures: or, it may be employed in deſtroying the effect of ſome former action, as when it ſtops or retards a moving body. But the genuine characteriſtic of an active being, is a power of beginning motion either in itſelf or another, without the means of preceeding motion*. Thus a man from a ſtate of reſt can begin a motion which ſhall move another body: or he can begin a motion which ſhall ſtop or retard another motion. And it will readily be admitted, that whatever active being can thus begin motion in another, without the means of previous motion, will of courſe be able to conſume motion in another, without receiving any itſelf.

When one body ſtrikes upon another and moves it, we commonly indeed call this an action: yet there is no reſemblance between this mechanical communication of motion, and

* *All mere mechanical* communications of *motion*, are not properly *action*, but mere *paſſiveneſs*, both in the bodies that impell, and that are impelled. *Action* is the beginning of a motion where there was none before, from a principle of *life* or activity. *Clark's Lett. to Leibnitz* p. 327.

and the activity before defcribed. This is conceived to be an action in a fecondary fenfe. It is only a confequence of the firft action. A motion once produced continues, till it be deftroyed by an equal and oppofite motion, or an action capable of having produced it.

THE effect produced by an agent is a certain quantity of motion or force, which, like other effects, continues without any tendency to perifh of itfelf. This quantity of motion, is always proportional to the real fpace defcribed by the whole quantity of matter; and that fpace having length, breadth, and thicknefs, is meafured by multiplying the quantity of matter, by the length of fpace defcribed by every particle, that is, by the velocity. If a body in motion ftrikes directly upon another at reft, the two move on as one body after the ftroke. They cannot move together with the fame velocity that the fingle body had before the ftroke, for then there would be an increafe of quantity of motion. And whence fhould that proceed? fuch an appearance as that, would difcover an activity in bodies indeed. Upon the fuppofition of perfect inactivity, the quantity of motion muſt remain unchanged: and we here abſtract entirely from elaſticity, whoſe effects proceed from a real activity, and have no relation to what is called the *vis inertiae*. To find the length of the ſpace deſcribed by the impinging body, we divide the ſpace by the quantity of matter, which is as it were the baſe. And to have the length of the ſecond ſpace deſcribed by the two bodies conjointly, we muſt divide the ſame ſpace by the maſs of matter in both. As the diviſor increaſes, the quotient muſt diminiſh in the ſame proportion.

Were we indeed to examine very minutely the gradual communication of motion from one body to another, we ſhall find a real activity concerned in the operation; but then it is not of that ſort againſt which we are now contending. It is that active force known by the name of the *attraction of coheſion*, which there takes place. When any part of a body is pulled or ſtruck upon by any moving power, it would neceſſarily be ſeparated from the other parts, were there no coheſion. But, where there is a coheſion, that attractive force muſt oppoſe the moving power. And if either the coheſion be ſtrong,

or

or the velocity of the moving power but ſmall, this oppoſition muſt continue, till ſuch time as all the parts of the body have acquired one common velocity. That part of the body which is immediately acted upon by the moving power, is firſt put into motion; and, drawing the other parts after it, gradually communicates motion to them, without ever getting beyond the reach of their attraction, or being broke aſunder. Yet, it is ſtill to the moving power that the motion of the whole body muſt be aſcribed; becauſe the attraction among the ſmall particles being mutual, will oppoſe the motion one way, as much as it promotes it the other; and ſo can neither forward nor obſtruct the motion upon the whole. The attraction ſerves only to connect and link the ſeveral parts together. The force loſt by one body, is preciſely equal to what is gained by the other; the whole effect or change, with regard to motion being the ſame, as if it had been inſtantaneouſly produced. And there is no argument to be drawn from hence, to prove the active reſiſtance of one body at reſt, to another in motion, or the tendency of any body in motion, to return to a ſtate of reſt.

FROM the experiment of Mr *Poleni* we learn, That ſpheres of equal diameters falling upon ſoft clay, make equal impreſſions, when the products of the quantities of matter into the heights from which they fall, are equal; that is, when the quantities of matter multiplied by the ſquares of the velocities are equal. As it is much eaſier to determine when two impreſſions are equal, than when they differ in any proportion of magnitude; the velocities of the different bodies in this experiment, are ſo adjuſted, in reſpect of the quantities of matter, as that the impreſſions may be equal. But, from thence we conclude, that ſuch impreſſions are always as the quantities of matter and ſquares of the velocities conjointly; and conſequently, when the quantities of matter are equal, the impreſſions will be as the ſquares of the velocities. The ſame experiment may be ſhewn perhaps in a more ſimple manner, by making pendulums of different quantities of matter, to ſtrike with different velocities upon a ſmooth ſurface of ſoft clay; the velocities being always aſſigned by help of a graduated arch, as is done in the common experiments of the colliſion of bodies. In this manner, likewiſe,

likewiſe, the impreſſions are found to be equal, when the products of the quantities of matter, by the ſquares of the velocities, are equal. Thus, let a hollow ball, with a quantity of matter 1, move with a velocity 20, and make a certain impreſſion. If you afterwards incloſe as much lead within the ſame ball, as to render it of double the maſs of matter, you muſt give it a velocity ſomewhat exceeding 14, before it make an impreſſion equal to the former one. If the body 2, receive only a velocity 10, the impreſſion will be remarkably leſs. In the former method, the gravity or weight of the balls, continuing to act after they touch the clay, may contribute ſomewhat in making the impreſſions; but, in the other way of proceeding, the impreſſions muſt be more exactly owing to the *vis inſita*, or force acquired by the bodies, in falling from the determined heights. This experiment can eaſily be reconciled with the old meaſure of forces, according to which the forces of equal bodies are to be eſtimated from the velocities ſimply. But, in order to do this, we ought carefully to diſtinguiſh betwixt two very different kinds of reſiſtance, *viz.* that from the *inertia* of the

the matter, and the other from the force of cohesion. When an impression is made by a stroke, upon the surface of any yielding substance, such as soft clay; the resistance to be overcome in making that impression, proceeds almost entirely from the tenacity or cohesion: any resistance arising from the *inertia* of the small quantity of matter displaced in forming the pit or cavity, is quite inconsiderable. The force of cohesion or attraction amongst the several particles of any homogeneous body, is equal and constant; and therefore, must produce an uniform resistance to the motion of any body applied to overcome it, or a resistance proportional to the time of the action. The continued resistance of a few attracting particles in a slow motion, and the successive resistance of a greater number of particles in a quick motion, will equally diminish the force of a given spherical body, in an equal time. If then the force of any ball, moving with a certain velocity, be consumed by the resistance from cohesion in a given time; the force of the same ball moving with double velocity, will be consumed in a double time; if the velocity be triple, the time will likewise be triple; and the veloci-

ties

ties muſt always be proportional to the times in which they are deſtroyed. From whence it is manifeſt, that a ball ſtriking with a velocity as 2, muſt make an impreſſion fourfold of what it makes with a velocity as 1; becauſe it continues likewiſe for double the time. In uniform motions, a double velocity and a double time, muſt always give a fourfold ſpace: and the ſame thing muſt obtain in equably retarded motions; the ſpaces deſcribed being exactly the half of what would have been deſcribed by the uniform motions. But, tho' the impreſſion be fourfold, the real effect, by which the force ought to be meaſured, is only double; for it has only undergone a double reſiſtance. For the ſame reaſon, triple the velocity makes a ninefold impreſſion. And, in general, the ſmall impreſſions made upon the ſurface of ſoft clay, (or even upon marble) by bodies ſtriking upon it, muſt, *caeteris paribus*, be as the ſquares of the velocities. Theſe impreſſions, made in oppoſition to the uniform reſiſtance of coheſion, are no better meaſures of forces, than the heights to which bodies are thrown near the earth, in oppoſition to the uniform power of gravity. The

heights

heights are known to be, as the ſquares of the velocities. When the velocities are, as 2 to 1, the heights are, as 4 to 1. But the forces are ſtill to be eſtimated, as 2 to 1; ſince the times which the uniform action of gravity takes to deſtroy them, are in that proportion. With a double velocity, and in a double time, the body arives at a fourfold height.

WHAT hath been here remarked, concerning the meaſure of forces, is not ſo much intended for the inſtruction of thoſe who have had leiſure to attend to this celebrated controverſy, as to enable every one who has the ſmalleſt acquaintance with theſe matters, to judge for themſelves; whether the common arguments, when rightly underſtood, be ſufficient for the deciſion of the queſtion? or, whether there be ſtill occaſion for a new ſolution of the difficulties? The debate has been cloſed long ago; after being managed by the ableſt advocates on both ſides: and the ſubject is generally thought to be exhauſted, though no formal reconciliation hath hitherto been declared amongſt the parties. We are now told, "That by a profuſion of writing, "the point is rendered ſo perplexed and intricate,

" tricate, that there appears not a better way " to come at the truth, than, neglecting the " arguments on both ſides, to apply directly " to facts for a ſolution, as one would do up- " on a point newly ſtarted." With this pompous introduction, or preamble, is uſhered *in* a new project for a treaty of peace amongſt the *Engliſh*, *French*, and *German* philoſophers. No mention is made of the *Italians*, the *Dutch*, and others, tho' they were pretty deeply engaged in the quarrel. The reaſon for which omiſſion may poſſibly be this, that the philoſophers in *Italy*, *Holland* and elſewhere, chuſe rather to preſerve the appearance of neutrality, and act as mediators. A preliminary article being firſt ſettled, namely, to conſider the ſeveral pretenſions of the contending powers, as ſo many points newly *ſtarted*, and that without any regard to their reſpective memorials; there aroſe a neceſſity in the next place, in order to render the negotiation the more conſiſtent with itſelf, for *ſtarting* new facts likewiſe, or at leaſt of new-modelling the old ones. The method in which the whole affair hath been conducted, and the jarring and oppoſite intereſts adjuſted, will beſt appear by an inſtance or

two.

two. It is ſaid, " That when a body is " thrown up with a double velocity, and " conſequently with a double force; the rea- " ſon why it aſcends four times its former " height, is plainly this, that the counter- " action of gravity, while it has a double " force to ſtruggle with, has but half the " time, in any given ſpace, to produce its ef- " fect." The natural meaning of which is, that if a body, thrown up with any velocity, riſes to a certain height in a given time; a body thrown up with double the velocity, ought to aſcend to an equal height in half that time. This indeed would hold true, if the upward motions could be ſuppoſed uniform: but, as a body thrown up, happens always to be equably retarded, the real fact comes out to be very different. The body thrown up with the double velocity, riſes to triple the height of the firſt body in an equal time, and to $\frac{7}{4}$ of that height in half the time. Let the height to which the firſt body riſes in any time, be called 1 yard; the height to which the ſecond body aſcends in half that time, is 1 yard and $\frac{3}{4}$. The former poſition however, as it was only taking a retarded motion for an uniform one, was pretty plausible, and good enough to pass upon *Germans* and other foreigners; especially if they were strangers to the *English* language. Another allowable artifice to deceive the adversaries, is in endeavouring to make them believe, that all the world knows, that "when bodies move through a fluid, or "any soft matter, a double force produces a "quadruple effect." If the word *effect* were taken its most proper sense, for the force communicated to the fluid or soft matter, by the moving body, this could never exceed the force of the body. A double force could never produce any more than a double effect in its own direction. All the world indeed may know, that when a body strikes upon clay or other such soft matter, a double force may produce a quadruple impression. But then, this can only happen in such cases, where no other resistance is worth the minding, but that arising from the cohesion of the matter, in the manner above explained. When a ball moves through a fluid, the resistance proceeds, almost entirely, from the density or *inertia*; and, the density being given, this resistance must increase as the squares of the velocity. In this case, it would be a vain attempt, to aſſign any meaſure of the impreſſions; becauſe, it follows clearly from the demonſtrations of Sir *Iſaac Newton**, that the body would go on for ever. And ſurely, it requires not much abſtract reaſoning to prove, that meer *inertia*, without the help of ſome active power, like gravity, or the attraction of coheſion, could never totally conſume any motion, or reduce a body from a ſtate of motion to a ſtate of abſolute reſt. Yet, after all, as every one has heard, that the reſiſtance of a perfect and incompreſſible fluid, was in the duplicate *ratio* of the velocities; this was foundation enough for a lover of peace, to aſſert roundly, that the impreſſions made in a fluid, were in that *ratio* likewiſe. Greater ſtretches might well be permitted for the accompliſhment of ſo deſirable an end, as a compleat union and harmony amongſt philoſophers.

When one body ſtrikes upon another with a great velocity, the parts immediately impelled are ſometimes broken off or driven away beyond the reach of attraction, before they have time to draw the other parts after them with any obſervable force; and the force

* Princip. Lib. II. Prop. v. et cor.

force of cohesion, having opposed the impinging body for so short a time, makes no sensible resistance to it. The attraction of cohesion acting without interruption; any resistance made, or any motion communicated by it, must be gradually produced; and consequently, in a very small time, the effect will be proportionally small. Of this we have many examples. Let us suppose a board set nearly upright on its end, and so slightly supported, that a bullet thrown against it, out of a man's hand, will tumble it over; if the same bullet be discharged from a gun, it will go through the board without moving it out of its place. In the same way, a bullet has been known to through a man's body, an arm has been taken off by a cannon ball, or even by the sail of a wind-mill, without any visible motion produced in the other parts of the body; though a much less force would be sufficient to drag the whole body forwards. The better to illustrate the manner in which these effects are performed; let it be observed, that by means of a small thread or a load-stone, a considerable mass of matter may be gently pulled alongst a table: whereas, if a sudden tug be given, or a greater force

force applied; the thread breaks, or the loadſtone ſeparates, without ſeeming at all to move the body. The nature of *inertia* and reſiſtance has been ſo much miſunderſtood, or miſrepreſented, that it was neceſſary to explain theſe *phænomena* from their true principles. In particular it might be thought, conſiſtently with the falſe notions advanced concerning the reſiſtance of matter, that the reaſon why a body did not move forwards when a part of it was broke off by a great force, was this, that the *inertia* or reſiſtance to motion in that caſe, became ſtronger than the power of coheſion.

PHILOSOPHERS have fondly perplexed themſelves, with many ſubtile queſtions concerning the communication of motion; and have perſiſted, with the utmoſt anxiety, in a very fruitleſs inquiry, how motion can paſs out of one body into another: as if motion was ſomething that could be ſeparated from the moving body, and infuſed from one into the other, like water poured into a phial. But, notwithſtanding all the intricacy of this affair, it would appear to be ſtill a greater myſtery, if one body in motion were not to move another lying freely at reſt. It is in-

deed

deed only by experience that we learn the laws of the communication of motion. For who ever ſuppoſed, that either the general or particular properties of matter could be diſcovered in any other way? How could it be known, whether body was penetrable or impenetrable, diviſible or indiviſible, elaſtic or non-elaſtic, animated or inanimated, but by trial? Suppoſing, however, that we were informed before hand, that the body at reſt was impenetrable, utterly inactive and ſoft, and had neither impediment nor tendency to motion; what would hinder us to foretell exactly every circumſtance that happens, when another body of the ſame kind ſtrikes upon it*?

When a body in motion ſtrikes upon another at reſt; the one loſes as much motion, as the other gains, in the ſame direction. This is ordinarily ſaid to proceed from a reaction of the body that acquires the motion. The effect is indeed the ſame upon the percutient body; there is the ſame change of its ſtate, as if it had actually received an impulſe in the contrary direction. This, for the ſake of ſhortneſs

* Philoſ. Eſſays.

ſhortneſs, we may, and always do call, *the effect of reaction*; but we can never imagine that there is any real active oppoſition of the body at reſt to the motion of the other. The great difficulty complained of, is in conceiving, how the one body has its motion diminiſhed by the ſtroke as much as the other is increaſed, while there is no active repugnancy allowed in the one to the other. It would be ridiculous enough to fancy, that the motion of the one was a part of the motion of the other; and that the very ſame motion could be transferred from the one into the other. "As well might the magnitude, figure, or colour, of one body be imparted "to another." But this we may venture to ſay, that, for ought appears to the contrary, it follows from the nature of a paſſive, extended and impenetrable ſubſtance, that motion is loſt in one by producing it in another. It will be eaſily admitted, that one motion may deſtroy an oppoſite motion, and by that means loſe of its own. Yet there is no leſs difficulty here, than in the communication of motion. If two pieces of clay oppoſing each other with equal forces, had any real activity of their own, it might be expected, that

that, tho' they both ſtopt at their firſt meeting, this was only done by ſuſpending the effect of each other's action; and that they would reſume their former degree of motion, upon being ſeparated by a man's hand. We find however, that the motion of both bodies is irrecoverably loſt by ſuch a colliſion. If then the force of one body is loſt or conſumed by deſtroying force in another: why may not one body as well loſe force by producing it in another. Beſides, were there any real active oppoſition in the body at reſt, to the body in motion; part of the force ought to be loſt on that account, and the ſum of the motions after the ſtroke, would be leſs than the motion before it.

WHEN a horſe pulls a ſtone forwards, it is commonly ſaid, that the ſtone pulls the horſe back, with a force equal to that with which the horſe pulls the ſtone forwards. But nothing more can poſſibly be meant, than that the horſe loſes as much force as the ſtone gains: and that, with reſpect to the horſe, the effect is the ſame, as if there was no ſtone tied behind him, but that he was puſhed back with a force equal to that acquired by the ſtone. The active force of the horſe's

limbs

limbs preſſes forwards both the horſe and the ſtone; what is called the reaction of the ſtone, is not equal to the whole force exerted by the horſe; for then indeed there could be no progreſſion: it is only equal to what is impreſſed upon the ſtone.

The equality of action and reaction in the caſe of preſſures, is to be underſtood in the ſame manner. When a finger is preſſed againſt any body, the body is ſaid to react upon the finger, as much as the finger is made to act upon the body. If the weight of the body be ſuſtained by the preſſure of the finger, there will be an active reſiſtance from the power of gravity. And, if the figure of the body be violently kept in a bent or compreſſed ſtate, there will be an active reſiſtance from the cauſe of coheſion. But if the finger be applied ſo as to communicate an equal motion to all the parts of the body, and without oppoſing its gravity; there will be no occaſion for imagining, that the body really reacts, or actively bears againſt the finger. The effect indeed upon the finger is the ſame, as if the body actually preſſed upon it; but that effect can eaſily be accounted for, without having recourſe to ſuch a ſuppoſition.

position. The surface of a soft body, like that of the finger pressed against a hard body, must undergo a change of figure, and receive the same impression, as if the hard body was pressed against it; because, when the parts that come first into contact with the body have their motion retarded, the other parts will advance farther forwards.

If there was any contradiction or absurdity in the common opinion of the communication of motion, this would be a sufficient reason for rejecting it, tho' it were ever so agreeable to the natural appearances of things. But shall we maintain, that every particular body can only be moved by a power of its own; and shall we deny, that motion is communicated by one body to another, meerly because we do not understand the precise manner in which motion is either conveyed or preserved? Why do we not, for the same reason, deny, that motion is begun at the command of any animated being? Or do we better conceive how any being or body moves itself, than how it moves another?

An appeal has been offered to the common sense of mankind, as ready to declare in favour of the activity of matter. If the facts be fully reprefented, the public judgment need not be declined. Whatever obfcurity the learned may find in this fubject of the communication of motion, a vulgar obferver will be very little perplexed about the matter. When a ftone is thrown out of the hand, and continues to move when left to itfelf; there is no fear, but " every perfon, who has not " ftudied philofophy," will rather attribute this to a force fome how impreffed upon the ftone, than to any action or operation of the lifelefs ftone. It will never enter into his head, that the ftone flies away with wings of its own. Such a perfon, judging from firft appearances that all motion languifhes and decays, may, likely enough, be inclined to think that body rather affects a ftate of reft than a ftate of motion; and that the impreffed force would not remain for ever: but he will never entertain the fmalleft doubt, but, that as long as the motion continues, it is only an effect of the firft impulfe. And, having once granted that motion can continue one moment as an effect after the action of the moving power is over, the fame reafon muft remain for its continuance the next moment of time, and fo on for ever.

When

When we ſee a body continuing to move with the ſelf-ſame velocity which is received from the hand, is it not more natural to ſay, that it does ſo for want of a power or activity to ſtopt itſelf, or to deſtroy this motion, than to imagine the body to be no way determined by the impulſe, but to be carried on with an equal force by a power of its own continually exerted after the impulſe. Such obſequious complaiſance of the ſtone, in imitating ſo exactly the motion of the hand, muſt appear extremely curious; eſpecially if we conſider what a violent oppoſition it is ſuppoſed to have made, before it yielded to the motion at firſt. It brings to remembrance the man in the parable, *who ſaid, he would not go, but went:* or perhaps it may be likened to a young fellow learning to dance, who bends his limbs but aukwardly at firſt ſetting out, till being led about for a while in the maſter's hand, he can afterwards continue the ſtep of himſelf. If, in the colliſion of bodies, the one body could thus adapt and conform itſelf in all caſes to the motion of the other; it ſeems to be the higheſt injuſtice to refuſe it the power of ſenſation and conſciouſneſs.

FROM

From experience and obſervation we learn, that body is equally indifferent to motion and reſt. And this indifference appears to be the natural conſequence of the moſt abſolute inactivity. Body muſt exiſt either in the one ſtate or the other. But, ſuppoſing it once exiſting in a moving ſtate, the continuance of the motion implies no activity in the body, any more than it requires activity to preſerve a quieſcent body in a ſtate of reſt. And we may, with equal reaſon, enquire for the cauſe of the continuation of reſt, as for a cauſe of the continuation of motion.

Philosophers have indeed entertained various opinions concerning the continuation of motion. *Ariſtotle* endeavoured to explain it by means of a vehicle or the circumpulſion of fluid matter, hereby only evading the queſtion, as the ſame difficulty returned, how the motion of this fluid continued. And many of the moderns have imagined reſt to be the proper ſtate of inactive matter, to which it naturally tended; and have aſcribed the continuance of motion to the conſtant and immediate agency of the Deity, or of ſome delegated intelligent power.

How

How far the concurrence of the Deity is neceſſary to the ſupport of created beings in general, is perhaps impoſſible for us to determine: but, unleſs ſome continual influence of that ſort be requiſite, there appears to be no occaſion for any ſpecial concurrence, in order to account for the continuance of motion. If however, it could be demonſtrated, that body cannot continue to move on in a ſtreight line, by virtue of the firſt impulſe; what more rational ſolution will be found, than to have recourſe to the efficiency of an intelligent principle? For tho' it ſhould be granted, that the continuation of motion, and the communication of it from one body to another, cannot proceed from the *inertia* of matter; will it therefore follow, that every atom of unintelligent and undeſigning matter is endued with an activity capable of moving it with the greateſt order and regularity, as well as variety and diverſity; conforming itſelf to the motion of other atoms with which it is connected; and adapting itſelf often to the will and intention of man? Why ſhould we not rather rank this power of the preſervation of motion, with thoſe other active powers employed upon matter, tho' not eſſentially

ſentially belonging to it (ſuch as gravity, attractions and repulſions of various kinds, *&c.*) and refer them all to an intelligent cauſe, if there be herein diſcovered the ſame ſignatures of thought and deſign?

THIS leads us to another ſource of objections againſt the inactivity of matter. As the very reſiſtance which matter makes to an external force, and its preſervation of the motion received, have been taken for ſymptoms of activity; we need wonder the leſs to find an activity ſuſpected in matter from its gravitation, and theſe other inſtances where motion begins in bodies, without any viſible cauſe.

IT may be objected, "That a ſtone falls "to the ground without any external im-"pulſe, ſo far as we can diſcover; and there-"fore dead matter begins motion of itſelf." Such manner of reaſoning would make ſhort work of natural philoſophy. Becauſe there are a variety of motions, changes and tranſformations, produced every day amongſt inanimate bodies; is it ſtraight way to be concluded, that theſe bodies move themſelves? The contrary of this appears in ſo many inſtances, as gives good reaſon to believe it ne-

ver

ver happens in any caſe. We ſee for certain, many motions begun by animated beings; we obſerve many bodies moved by the impulſe of other bodies; and the mechanical cauſes of ſome motions, have through time been diſcovered, which were not formerly perceived. Once in a day, it was found philoſophy to maintain that ſmoak and vapour mounted upwards of themſelves by a principle of levity, tho' now one might as well aſſert that cork riſes up of itſelf in water. Many *phænomena* were explained from an abhorrence of a void, which was fully as reaſonable a paſſion in a dead body, as the love of a center, or an inclination to meet with other bodies. The riſe of water in pumps was aſcribed to a ſelf-moving power in the water, and the pulſation of the arteries to a power they had of dilating themſelves. The power of magnetiſm bears ſome reſemblance to that of gravity; the theory of it is ſtill imperfect: yet there are a multitude of facts which indicate a mechaniſm by means of ſome effluvia; particularly the new method of making artificial magnets by attrition, and the well known experiment, in which an iron rod is preſented to a magnetic needle, the

ſame

ſame end being made to attract, ſometimes one pole and ſometimes another, by only changing the poſition of the rod, or inverting it upſide down; and much more by ſtriking upon it with a hammer, or beating it againſt the ground. Electricity is another more palpable inſtance of the ſame ſort. That a fluid is concerned in producing the apparent attractions and repulſions, and other more aſtoniſhing effects of the electric globe, can hardly be doubted, however difficult it may ſtill be to deſcribe the laws to which it is ſubjected in its operations. Why then ſhould it be accounted "whimſical" or unphiloſopical to demand a cauſe for the attractive power of gravity? Tho' all the mechanical accounts, hitherto given of the cauſe of gravity, ſhould be found unſatisfactory; may it not ſtill be owing to ſome unknown mechaniſm, or the intervention of matter, moving other matter? Or, tho' it were ſhown to be impracticable by any mechaniſm whatever, as is not improbably the caſe; why may we not attribute it to the immediate agency of an intelligent active being?

It may be urged further by way of objection, "That a power of beginning viſible "motion

"motion is no more connected with a power "of thinking, than it is with any other pro- "perty of matter or ſpirit." This may poſſibly be admitted in a certain ſenſe, *viz.* that there may, for ought we know, exiſt ſome ſpecies of thinking beings, deſtitute of the power of motion altogether. Oiſters have very little of it. But however this be, we know, with all the certainty attainable in phyſics, that many thinking beings have ſuch a power; we ſee them begin motion, a relative motion on the ground. When that motion is loſt, they renew it and vary it again at every ſtep. They not only begin new motion, but deſtroy old motion, at pleaſure: whereas no experience can ever tell us, that the beginning of the viſible motions of dead matter is original and underived. And there is this wide difference (which hath been often remarked by authors on this ſubject) betwixt animated and inanimated beings, with reſpect to motion, namely, that the thinking being can determine the direction and quantity of its motion: which is a power incompatible with dead matter; and conſequently it will of itſelf remain for ever incapable of the leaſt motion. Thus far therefore there is a connexion betwixt motion and thinking, that a power of beginning motion ſeems neceſſarily to infer a power of thinking; tho' we cannot affirm inverſely, that a power of thinking muſt infer a power of beginning motion.

But if the bare beginning of motion ſeems to require an intelligent cauſe, the power of gravity, ſurely, has the higheſt title to lay claim to that origin. The motions ariſing from gravity are evidently of ſuch a ſort, as cannot, without the greateſt violence to reaſon, be aſcribed to any blind tendency betwixt the attracting bodies. This will beſt appear upon ſtating ſome of its known effects. A ſtone is drawn towards the earth in *Europe* and in *America*; it changes its direction in different places, pointing always nearly to the center of the earth, (or exactly in a line perpendicular to the level ſurface) in the ſame manner as iron does towards the loadſtone, or a feather to the electric tube. The attractive force of a ſtone diminiſhes, the farther it is removed from the earth, according to a fixed rule, or as the ſquare of the diſtance encreaſes. A body placed by itſelf would move no way; but two bodies run together. A given body is more attracted to a large quantity

quantity of matter than to a leſſer. Is it then conceivable, *that* an unthinking being ſhould be endued with an activity which it regulates and varies in proportion to the ſituation, diſtance, and magnitude of another body, whilſt it is ſuppoſed to be not in the leaſt influenced or acted upon by that other body, or any other being whatſoever? If this ſhall be maintained, another queſtion will ariſe. By what actions, or what ſtronger language than this, can any man convince his neighbour of his own reaſon or underſtanding? It is preſumed, that a higher degree of evidence will hardly be required in phyſical matters, than what we have for the life and exiſtence of one another: the voice of nature as loudly declares the origin of gravity, that ruling principle which binds the parts of the ſyſtem together. From the circumſtances obſervable in the apparent mutual tendency of bodies, we are naturally led to conclude, that gravitation is the effect of the continued and regular operation of ſome other being upon matter; and that bodies are either drawn or preſſed together by ſomething external. A power ſo conſtant, ſo regular, and withal ſo uniformly varied and diverſi-

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fied according to different circumſtances, can proceed from nothing but an intelligent cauſe, either mediately or immediately exerted upon bodies.

WHEN motion is obſerved to begin in any body, it muſt be aſcribed to one or other of theſe four cauſes ; 1. Some external animated being; 2. An external inanimated being; 3. A ſelf-moving animated faculty; and 4. A ſelf-moving inanimated faculty. Of the former three, there are a variety of manifeſt examples. Of the laſt there can be no certain example at all; and conſequently we ſhould make a bad choice, in preferring it to the others: ſuch a ſuppoſition, namely the production of motion from an internal inanimate principle, is intirely without foundation; and ſeems to be much the ſame thing as to allow, that motion may begin without any cauſe at all.

THAT ſomething may begin to exiſt, or ſtart into being without a cauſe, hath indeed been advanced in a very ingenious and pro-

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found ſyſtem of the ſceptical philoſophy *; but hath not yet been adopted by any of the ſocieties for improvement of natural knowledge. Such ſublime conceptions are far above the reach of an ordinary genius; and could not have entered into the head of the greateſt phyſiologiſt on earth. The man who believes that a perception may ſubſiſt without a percipient mind or a perceiver, may well comprehend, that an action may be performed without any agent, or a thing produced without any cauſe of the production. And the author of this new and wonderful doctrine informs the world, that, when he looked into his own mind, he could diſcover nothing but a ſeries of fleeting perceptions; and that from thence he concluded, that he himſelf was nothing but a bundle of ſuch perceptions.

Mr *Baxter*, in his *Inquiry into the nature of the Human ſoul*, and likewiſe in his *Matho*, endeavours to prove, that gravity cannot be

* *Treatiſe on Human Nature*, 3. vols. octavo. This is the ſyſtem at large, a work ſuited only to the comprehenſion of Adepts. An excellent compend or ſummary whereof, for the benefit of vulgar capacities, we of this nation enjoy in the *Philoſophical Eſſays*, and the *Eſſays Moral and Political*. And to theſe may be added, as a farther help, that uſeful commentary, the *Eſſays on Morality and natural Religion.*

be a property inherent in matter; from this consideration, that, if body had an actual tendency to fall down, it could not at the same time resist the downward motion by its *inertia*. Which reasoning plainly supposes, that there is a real active resistance arising from *inertia*. And indeed, tho' he expresly intends to deny the smallest activity in matter, yet he every where speaks of *inertia*, as a *conatus* or tendency essential to matter, by which it makes a violent opposition to the power of gravity, and other active powers. This being a question of the utmost importance, it may not be amiss to offer an amendment upon that argument, which will free it from any inconsistency, and place it on its true bottom, on which alone it can stand its ground. To prove then that gravity does not proceed from any internal active force of any kind, either in the earth or such bodies as fall towards it; it is only necessary to assume one axiom, that no substance can actively tend to move to two opposite sides, at the same time. It is easy to conceive that a body may be pushed or drawn to opposite sides, by two contrary external forces: in which case, if they are equal, the body will remain at rest; if unequal, it will be moved

ved to one ſide, by the difference of the two forces. Thus, every part of a ſtagnating fluid is preſſed equally in every direction. But if a body actively tend to one ſide, it cannot actively tend to the other at the ſame time, that being a contradiction in terms. And in fact, we ſhall find it impoſſible, to make two ſuch efforts in our own bodies, to move them *wholly* to two oppoſite ſides. This being granted, it follows, that if the waters of the ſea gravitate towards the earth by an active force, they cannot, at the ſame time, gravitate by their own activity towards the moon, ſo as to produce the tides. And therefore, the gravitation in one or both caſes, is owing to an external influence. The ſame argument, if it ſhall be found concluſive, may be applied to the attraction of the moon towards the earth and ſun, in the conjunctions, and other ſuch like caſes. The attractions betwixt ſmall corpuſcles, will be ſhown to proceed from an external cauſe, in the ſame manner. And hence it will follow, that elaſticity and the chymical efferveſcencies, are not the reſult of the activity of attracting particles, And for the ſame reaſon a body thrown upwards, cannot move up by an an active force of its own; for that would be oppofite to the active force of gravitation.

If, in order to evade the foregoing argument, it fhould be faid, that a body never tends to two oppofite bodies at the fame time; but tends only to one fide with the difference of the forces with which it would tend to each of thefe bodies taken feparately: this will at leaft afford another remarkable inftance of that exact regularity with which the power of gravity is varied and adapted to the different fituations of bodies; and which it is quite inconceivable that blind matter fhould perform without the fuperintendance of an intelligent being.

It has indeed been frequently afferted, that bodies move towards each other by virtue of a law originally eftablifhed. But law, that is to fay, a mere abftract name or complex notion, which is no real being, cannot impel a ftone, and caufe it to begin to move. Law by itfelf, with fubmiffion be it fpoken, will avail nothing, unlefs either the fubjects of it have underftanding, to yield a willing obedience, or they be compelled to it by external force.

If the firſt appearances of things are to be truſted, there is full as good reaſon to aſſert, that the earth *draws* the ſtone, or the magnet the iron, as that the ſtone or iron move of their own accord. Yet the ſuppoſition of one body drawing another body at a diſtance, without the intervention of other matter, is univerſally rejected; and that merely becauſe of the natural impoſſibility of the thing. That a being cannot act where it is not, any more than when it is not, is an axiom or principle of reaſon and common ſenſe, and not a leſſon of experience. And is it not equally ſelf-evident, that dead matter can never begin motion of itſelf; far leſs regulate its motions according to a law? If bodies are not ſenſible of the neighbourhood of other bodies, of their quantities of matter, and of their preciſe diſtance from them; is it to be imagined that they will move themſelves with ſuch determined degrees of force, correſponding to the different quantities of matter and different diſtances?

The active powers both of attraction and repulſion are of ſuch a ſort, as could not be exerciſed by the bodies themſelves, without either diſtinct perceptions of their own ſituations and magnitudes with reſpect to other bodies, or a regular ſucceſſion of ſome kind of clear perceptions, correſponding to every variety of ſituation and magnitude, and all this accompanied with a memory and a power of comparing paſt with preſent perceptions. It would therefore make nothing to the preſent purpoſe to have recourſe to that extraordinary ſubterfuge, made uſe of by Mr *Hobbs* on a like occaſion, That all matter is endued with an obſcure ſenſe and perception, and wants only the organs and memory of animals. And it hath never yet been alledged, that all matter is eſſentially endued with a clear and diſtinct ſenſation or conſciouſneſs. If it ſhould, we need be at no loſs for an anſwer. For, were that the caſe, why have we no perception of this activity that is ſuppoſed to be exerted by our own bodies in falling to the ground?

Dr *Clark*, *Wollaſton*, and others, have ſo fully proved that matter is incapable of any degree of thinking, that it is impoſſible to confute their arguments, but by ſcornfully denying the force of all metaphyſical demonſtrations whatever. If matter thinks, then either thinking is eſſential to all matter, or it ariſes from the modification, magnitude, figure

gure or motion of certain parcels of matter. But what can be more ridiculous than to imagine, that matter is as eſſentially conſcious, as it is extended! Will it not follow from that ſuppoſition, that every piece of matter, being made up of endleſsly ſeparable parts, (that is, of parts which are as really diſtinct beings, notwithſtanding their contiguity, as if they had been at the greateſt diſtance one from another) is made up alſo of innumerable conſciouſneſſes and infinite confuſion? And farther, if every part of matter be ſelf-conſcious, it would be a contradiction to ſuppoſe that any ſyſtem could be ſo. The reſulting ſenſation or conſciouſneſs at laſt being but one diſtinct ſenſation or conſciouſneſs, (as is that of a man) the ſenſation or conſciouſneſs of every one of the conſtituent particles, would be the individual ſenſation or conſciouſneſs of all and each of the reſt. In the next place, the faculty of thinking cannot ariſe from the ſize, figure, texture, or motion of body; nor can be deſtroyed by any alteration of theſe qualities: becauſe bodies, by any change of theſe, only become greater or leſs, round or ſquare, rare or denſe, tranſlated from one place to another,

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with this or that new direction, or velocity; or the like. All which ideas are quite different from that of thinking; there can be no relation between them.

THIS sort of reasoning deserves at least some direct answer, and is not to be overthrown by any criticism on the meaning of the word *immateriality*. This, as is learnedly observed, most certainly "comes out to "be merely a negative term, comprehending "every thing that is not matter." And it is not to be doubted that if other terms, such as *immortality* and *infinity*, were but examined with the same "sufficient accuracy," the one might come out to be merely a negative term, comprehending every thing that is not mortal, and the other a negative term comprehending every thing that is not finite. Such premisses as these are undeniably true; the only difficulty lies in discerning the use and importance of them. But if the reasoning subjoined to these premisses "be found "entirely conclusive," the consequence must be, that, in judging of the qualities of matter, we are in every case to rely upon the report of our external senses, and never to employ our reason in comparing one thing

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with another, in order to correct our first impressions. *Epicurus* is to be applauded for believing on the credit of his eye-sight, that the sun and moon were no bigger than a cart-wheel or a cheese. The peasant, who thinks that the sun moves from east to west every day, is wiser in that respect, than the greatest philosopher; and *Copernicus* is to be regarded as a common enemy to mankind, "for declaring war against our senses."

The grand question to be resolved in this controversy, is not whether we have clearer conceptions of material or immaterial substances, as we are equally ignorant of both; but whether the power of thinking, which is a known faculty of some substance, be not absolutely indivisible? and as such, incompatible with the known properties of matter? The most plausible objection that can well be made against this argument, arises from the late observations made concerning the polypus, and other living creatures of that kind. Yet these, when duly considered, will be found entirely consistent with the indivisibility and unity of thinking substance. They do indeed exhibite a remarkable peculiarity in the manner of the propagation of these

animals;

animals; and perhaps give ſome ground for conjecturing, that a number of animals, or a ſyſtem of thinking beings, may probably be conjoined together, under the form of one animal. We were formerly acquainted with inſtances of two or more animals connected or adhering together, as in worms of different kinds, tho' they do not grow or extend themſelves to any larger dimenſions upon ſeparation. Other animals were known, which, upon loſing a limb, had it regenerated again, as is the caſe of craw-fiſh and lobſters. May it not then be ſuppoſed, that, in the polypus, both theſe caſes concur together?

He who allows, that the ſyſtem of the univerſe may proceed at preſent without any guidance or direction, will find it difficult to aſſign to himſelf any good reaſon why it might not always, or from eternity, have done the ſame. The excellency and perfection of the material world, can be no evidence to him of an original architect or contriver of all things: ſince the higheſt excellencies and perfections are acknowled, even by every Theiſt, to exiſt without a cauſe. But the inſtances of a wiſe adminiſtration and ſuperintendence, preſented every moment to

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our view, ſtrike the mind in the moſt forcible manner. We are eye witneſſes of providence, but not of creation. The contemplation of every part of nature, furniſhes us with irreſiſtible proofs of intelligence, counſel and deſign, ſtill employed in actuating, moving, conducting and governing the univerſe. *Nihil eſt enim, quod ratione, et numero moveri poſſit ſine conſilio, in quo nihil eſt temerarium, nihil varium, nihil fortuitum. Ordo autem ſiderum et conſtantia, neque naturam ſignificat; eſt enim plena rationis: neque fortunam, quæ amica varietati conſtantiam reſpuit. Quæ qui videat, non impié ſolum, verum etiam indocté faciat, ſi Deos eſſe neget. Nec ſane multum intereſt, utrum id neget, an eos omni procuratione, atque actione privet: mihi enim, qui nihil agit, eſſe omnino non videtur.* Cic. de Nat. Deor. lib. II. cap. 16.

We obſerve indeed, in various inſtances, a ſubordination of natural cauſes and effects, and a dependence of one thing upon another. To inveſtigate theſe, is the proper province of natural philoſophy. The philoſopher's buſineſs is, to learn the conſtitution of things as they really are; and to ſearch out the laws and order

order eſtabliſhed in the material world, and by which it is conducted. The moſt ſucceſsful enquirers into nature, have ſeen moſt reaſon to reſolve all things finally, into an incorporeal, intelligent, and powerful firſt cauſe: and have rejoiced in the perſuaſion, that they themſelves, and all parts of the univerſe, are the offspring, and under the adminiſtration, of the ſame great, wiſe, and beneficent parent. But they are not ſolicitous in limiting the number of ſecond cauſes, far leſs in excluding them altogether, as ſome over zealous friends to religion have done. *Plutarch**, in recommending the philoſophy of *Anaxagoras*, obſerves, that ſuperſtition proceeds from ignorance of cauſes, and want of experience; but that natural philoſophy baniſhes all ſuperſtition, and begets in the mind a well grounded piety, with comfortable hopes.

MANY

* Δεισιδαιμονια προς τα μετεωρα θαμβος εργαζεται τοις αυτων τε τουτων τας αιτιας αγνουσι. ὁ φυσικος λογος την ασφαλη μετ᾽ ελπιδων αγαθων ευσεβειαν ενεργαζεται. Vit. PERICL.

MANY fruitleſs attempts have formerly been made to explain all the *phænomena* of nature, on mechanical principles alone. But it is exceedingly ſurpriſing, that, in the preſent age, ſo eminent an aſtronomer as M. *Maupertuis* * ſhould again revive exploded notions; and ſuggeſt, that the planets may poſſibly be retained in their orbits, by the motion of a circumambient fluid, as an ultimate cauſe. There is nothing more demonſtrably certain, than that gravity cannot ariſe from the preſſure of a whirlpool, or vortex of a denſe fluid, once put in motion, and continuing to move round of itſelf, as M. *Des Cartes* imagined. And it is needleſs at preſent to examine, whether it can be better explained by means of a rare elaſtic æther. If Sir *Iſaac Newton* endeavours in that manner to account for gravity, the attraction of coheſion, the reflexion and refraction of light, *&c.* it is only with a view to point out ſome more general mechanical cauſe, upon which all theſe powers may poſſibly depend. He never could believe that his aether was poſſeſſed of a real activity of its own. The elaſticity of that

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* *Coſmologie.*

fluid, muſt itſelf either proceed from ſome higher mechanical cauſe, or flow immediately from ſome vital intelligent principle, which muſt be immechanical. The mutual repulſions betwixt one particle and another of the fluid, and betwixt the fluid and the bodies ſwimming in it, diſcover as much variety and regularity, as were before obſerved in the *phaenomena* of gravity; and muſt conſequently be regarded as the continual effects of thought and deſign.

IT ſeems to have been far from Sir *Iſaac*'s intention, to aſcribe activity to matter in any ſhape; tho' his meaning has been ſometimes miſtaken*. To do ſo, would be a manifeſt contradiction to the primary laws of motion, delivered by himſelf in the beginning of his *Principia*.

ACCORDING to *Spinoza*, one piece of matter is moved by another, and that by a third; and thus there is a progreſſion of cauſes and effects, *in infinitum*, all acting blindly, without intelligence and deſign. Not to repeat here, the metaphyſical argument againſt the poſſibility of an endleſs ſeries of dependent cauſes

* *Philoſophical Eſſays*. p. 119.

causes of any kind, it will be a sufficient confutation of this scheme to observe, that it supposes that an infinite number of undesigning beings can continually produce the greatest order, regularity and harmony: which is no better sense than to say, that, tho' one single cypher be of no value, yet an infinite number of nothings can amount to a real quantity. There is no other possible method whereby to judge of the intelligence or wisdom of any being, but by it's sensible effects; and we may as easily suppose one single lifeless being to produce by itself the ordinary effects of wisdom, as that they can result from the successive action of an infinite number of them.

BECAUSE the world is often compared to a machine, it hath been imagined, that, when once set a going, it may continue its motions without any further interposition of the Author of nature, or any other being. If we attend however to any machines of human contrivance, we only learn, that all their motions depend upon certain active powers, as gravity and elasticity. These are employed by ingenious artists, such as they find them, to many useful purposes in life. But it is by

considering

confidering the effects of thefe powers in general, and without regard to any particular application of them, that we muft determine concerning their nature. And it hath been already attempted to fhew, that they are all probably derived from an active intelligence, either immediately or mediately exercifed upon matter. A connection manifeftly appears amongft different parts of the fyftem of the world; and many bodies in it, tho' dead and inanimated, are not only moved and agitated themfelves, but communicate motion to each other, according to a certain fixed and eftablifhed order, which is called the courfe of nature. But there is no foundation in mechanics for imagining, that a lifelefs body, in confequence of a law promulgated fome thoufands of years ago, can move itfelf with as much regularity and variety, as if it was actually endued with thought and reflexion.

THIS, to moft people, will appear an impoffibility, as much as to make a blind man to fee, whilft he continues blind. And there are few, but will at leaft acknowledge the contrary opinion to be fupported with fo high a degree of probability, as juftly to deferve ſerve the preference. *Haec quidem, ſi non vera, ſaltem veriſimillima videtur.*

" It is alledged, that the ſyſtem of the " world would diſcover more contrivance, if " it went on of itſelf, without any concur- " rence of the Deity." That Mr *Boyle* gives any countenance to this opinion, cannot be poſitively concluded from his words. He does not ſay, that brute matter moves itſelf, but that " it is managed by certain laws, and " upheld by God's ordinary and general con- " courſe." The leſs power is exerted to produce a given effect, the mechaniſm may juſtly indeed be eſteemed the more pefect. But a machine going on without a moving power at all, is a thing quite unheard of. And where would be the beauty of every body, every wheel moving itſelf, without a dependence of one part upon another? There would then be no mechaniſm at all. Let the machine be as grand and perfect as poſſible; it muſt ſtand in need of a firſt mover, not only to begin the motion, but to preſerve it. If every part ſpontaneouſly moved itſelf, it is then no machine; ſome other name muſt be found for ſuch an arrangement or ſyſtem of things. If all the motions and changes

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of bodies are performed immediately by theſe bodies themſelves, without the influence of other matter, or any other power; there is an end of all enquiries into cauſes and effects; philoſophy muſt be degraded to a bare knowledge of facts, a hiſtory of nature.

THERE is an argument, in the form of a *reductio ad abſurdum*, brought againſt the received opinion of the equality of action and reaction, which had almoſt been overlooked. The ſubſtance of it is this, that as the preſſure *undequaque*, is a conſequence of the law of equal action and reaction in fluids; ſo the ſame ſort of preſſure ought to be a property of ſolid bodies likewiſe, if the ſame law be univerſally extended to all bodies, ſolids as well as fluids. For a ready anſwer to this objection, we need only recollect the definition of a fluid, as diſtinguiſhed from a ſolid body. An *undequaque* preſſure cannot obtain in a ſolid body, unleſs the particles of it did yield to any force exerciſed upon them; and, in yielding, were eaſily put in motion amongſt themſelves; or in other words, unleſs the ſolid body was converted into a fluid. A parcel of dry ſand may bear ſome ſmall reſemblance to a fluid; its particles may be moved

ved more eaſily amongſt one another, than theſe of a firm body, whoſe parts are cloſely united by attraction: yet ſtill there muſt be a conſiderable adheſion of the particles of ſand, on account of their irregular figures, and the inequalities on their ſurfaces. When a man applies his hand to a hole in the ſide of a veſſel of water, he ſuſtains a preſſure from the tendency of the particles to ſlide down and make their eſcape: but, when his hand is laid on the ſide of a ſand bank, there is no ſuch preſſure to be felt; becauſe the particles are intangled together, and ſupport one another. Thus a body laid upon a rough inclined plain, may remain fixt, by the reſiſtance from attrition alone; whereas, if the plain be ſmooth, ſome other power becomes neceſſary to hinder the deſcent of the body.

THE mechanical action and reaction of matter, or the changes with regard to motion and reſt, which take place in the colliſions of non-elaſtic bodies, have been already conſidered; and were found to be entirely conſiſtent with the moſt abſolute *inertia*. But that real actions and equal oppoſite reactions, obtain in the active powers of attraction and

repulſion,

repulſion, is agreeable to perpetual experience; and is no leſs certain, than that theſe powers themſelves are eſtabliſhed in nature. We find the loadſtone attracts iron, and that iron attracts the loadſtone, with equal force; and, becauſe they attract each other equally, they remain at reſt when they come into contact. If a mountain, by its gravity, preſſed upon the earth, and the earth did not react equally on the mountain; then the mountain would neceſſarily carry the earth before it, by its preſſure, with a motion accelerated *in infinitum*. The ſame is to be ſaid of a ſtone, or the leaſt part of the earth, as well as of a mountain. A mutual attraction too is obſerved amongſt the diſtant bodies of the ſolar ſyſtem.

WHEN a heavy body lies upon a table, and conſequently acts upon it with its whole weight; there muſt be an equal reaction from the power of coheſion of the parts of the table, in order to ſupport the burden. In the ſame manner, if the bottom of any veſſel ſuſtains the weight of a certain quantity of ſand, it muſt react with a force equal to that weight; otherways it muſt break or give way. The power of coheſion may indeed greatly

exceed

exceed the particular weight which it happens to counteract at any time: and the ſame table, at different times, may ſupport very different degrees of weight. But ſtill it is true, that the force exerted by this power upon the body, can neither be greater nor leſs than its weight, ſince the reaction barely ſupports the body without beginning motion in it. If this needs any illuſtration, we may ſuppoſe a man to have a weight hanging at his hand, while at the ſame time he preſſes that hand upwards againſt any fix'd body; the force with which the hand adheres to, or is preſſed againſt the body, may be greater or leſs at pleaſure; but the force employed againſt the weight, or that which directly oppoſes and counteracts it, muſt exactly be equal to the weight itſelf.

THE argument againſt the law of action and reaction may be made to conclude with equal force againſt the law of gravity itſelf; and, if it proves any thing at all, will even prove it impoſſible that all terreſtrial bodies ſhould be endued with weight. *Mutatis mutandis*, the reaſoning proceeds thus. Admitting the law of gravity, a ſmooth level ſurface muſt be the conſequence. And as the law is not confined to fluids, but is ſuppoſed to be an univerſal law of matter; a ſmooth level ſurface muſt not only be the conſequence in fluids, but in ſolids, equally. Here then is a fair *dilemma*. We muſt either acknowledge a ſmooth level ſurface to be always found in ſand, powder, and indeed in all looſe bodies, as well as in fluids; or confine this law of gravity to fluids. Can we remain a moment in doubt betwixt theſe two oppoſites? we are certain that a ſmooth level ſurface is not a neceſſary property of ſolids. The gravity of a whole hill of ſand, is but an aſſertion without evidence. What remains then, but that we adhere to the former, and reject the latter, except as to fluids? And thus our author luckily, tho' without intention, has furniſhed a very convincing argument againſt the univerſality of this ſuppoſed law of gravitation. If a ſmooth level ſurface is an effect of this law, it follows clearly, that this law takes not place in ſolids, at leaſt not univerſally, as in fluids.

It hath been judged a ſubject worthy "to "be regreted that natural philoſophers and "mathematicians are not always well ſkilled "in logics." A fair compariſon alone can ſhow,

ſhow, whether they be more obnoxious to this cenſure than other people, and who are the moſt guilty of fallacious reaſoning, and in particular of that ſpecies of it commonly called *Ignorantia Elenchi* or ἑτεροζητησις. It muſt indeed be avowed, that few of the mathematical philoſophers have teſtified any high admiration of thoſe ſpacious openings and enlargements lately ſtruck out by certain bold and enterprizing undertakers in the dialectic art. Nor can it reaſonably be expected that they ſhould entertain the moſt favourable opinion of ſuch performances. Men who puzzle themſelves with ſelf-evident axioms, and ſtumble at the plaineſt demonſtrations, raiſe a ſhrewd ſuſpicion that they may be liable to like human infirmities in other matters, and can have no pretenſions to be received as infallible guides. The fartheſt that complaiſance can go, is to transfer the compliment, and to regrete, that theſe *univerſal philoſophers* are not always well ſkilled in the elements of mathematics and natural philoſophy. * If their end in view be really the

* Meſſ. *Hobbs*, *Toland* and *Collins*, have made little other uſe of the mathematical philoſophy, than as a touch-ſtone for diſcovering their own metal. Witneſs *Elementa Phyſicae*, *Letters to Serena*, and *Reflections on Mr Clarke's ſecond defence*. p. 36

the inveſtigation of truth, as it is to be wiſhed; a little more converſation and familiarity with *Euclid* and other geometricians, might be of good ſervice to them, by accuſtoming their minds to the ſteady purſuit of real knowledge: but if their higheſt aim in life be vain diſputation, and an oſtentatious diſplay of their abilities, in attempting to involve the cleareſt truths in doubt and uncertainty; better were it for them to throw away the rule and compaſs altogether, and to exerciſe their faculties on other ſubjects, where there may be more room for ſubtile evaſions, and where miſtakes, tho' equally remote from truth, and perhaps of more pernicious conſequence to mankind, cannot, from the nature of the thing, be ſo eaſily detected.

ART.

ART. III.

Pappi Alexandrini collectionum mathematicarum libri quarti propositio quarta generalior facta, cui propositiones aliquot eodem spectantes adjiciuntur; auctore MATTHEO STEWART, *in Academia Edinensi Matheseos Professore.*

PROP. IV. *Lib.* 4. *Collect. Math. Pappi Alexandrin.*

Sit [*Tab* 1. *Fig.* 1.] circulus ABC, cujus centrum E, diameter BC et recta linea contingens AD, quae cum BC in puncto D conveniat. Ducatur autem DF, et juncta AE, producatur ad G, et FKG et GLH jungantur. Dico KE ipsi EL aequalem esse.

Factum jam sit, et ipsi KL parallela ducatur HXM. Ergo MX est aequalis XH; ducatur etiam a puncto E ad FH perpendicularis

EN:

EN: aequalis igitur eſt FN ipſi NH: erat autem et MX aequalis XH: ergo NX ipſi FM eſt parallela; et angulus HNX aequalis eſt angulo NFM, hoc eſt, angulo HAX, et in circulo ſunt puncta A, N, X, H; eſt igitur angulas ANH aequalis angulo AXH, videlicet angulo AEL: et propterea in circulo ſunt puncta A, E, N, D; rectus eſt enim uterque angulorum EAD, END.

Componetur autem ſic. Quoniam uterque angulorum EAD, END eſt rectus, puncta A, D, E, N in circulo erunt. Aequalis igitur eſt angulus AND angulo AED. Sed angulus AED eſt aequalis angulo AXH, propterea quod parallelae ſunt ED, XH: ergo in circulo ſunt A, N, X, H puncta: et angulus HAX angulo HNX eſt aequalis: angulus autem HAX aequalis eſt angulo HFM: ergo FM ipſi NX eſt parallela; et eſt FN aequalis NH: quare et MX ipſi XH aequalis erit; eſtque ut XG ad GE, et ita XM ad EK, et HX ad LE: ut igitur XM ad EK, ita HX ad LE: et, permutando, aequalis autem eſt MX ipſi XH: ergo et KE ipſi EL eſt aequalis.

Perpendenti autem hanc propoſitionem, ejuſque pulcherrimam reſolutionem et compoſitionem ſtatim mihi

mihi occurrit veram esse, etsi recta BC *non sit diameter; si vero bisecta sit in puncto* E, *et iisdem fere verbis quibus utitur Pappus ostenditur, ope sequentis Lemmatis.*

LEMMA. *Tab.* 1. *Fig.* 2. 3.

Sint duae rectae AB, CD circulo inscriptae, sibi mutuo occurrentes in puncto E extra circulum, et bifariam secentur rectae AB, CD in F, G punctis, a puncto E ducatur EH circulum contingens in H, erunt puncta E, F, G, H in circulo.

Et, iisdem manentibus, si sit punctum H in circumferentia, atque puncta E, F, G, H in circulo, continget juncta EH circulum in H.

Cas. 1. Si una [*Fig.* 2.] rectarum AB, CD, puta recta AB, sit diameter; jungantur FG, FH.

Quoniam recta CD bisecta est in G, et est F centrum circuli quoniam diameter AB bisecta est in F; rectus erit angulus FGE; rectus

rectus autem est angulus FHE, quoniam contingit EH circulum in H, quare erit angulus FGE aequalis angulo FHE: in circulo igitur sunt puncta E, F, G, H.

Et, iisdem manentibus, si sit punctum H in circumferentia, atque puncta E, F, G, H in circulo, continget juncta EH circulum in H.

Quoniam AB diameter bisecta est in F, erit F centrum circuli; et quoniam recta CD bisecta est in G, rectus erit angulus FGE; est autem angulus FHE aequalis angulo FGE, quoniam in circulo sunt puncta E, F, G, H; rectus igitur est angulus FHE; quare continget EH circulum in puncto H.

Cas. 2. Si neutra [*Fig.* 3.] rectarum AB, CD sit diameter sit K centrum circuli, et jungantur KF, KG, KH, FH, GH et KE.

Quoniam rectae AB, CD bisectae sunt in F, G punctis, erunt anguli KFE, KGE recti; et quoniam contingit EH circulum in H, rectus erit angulus EHK. Quoniam recti sunt anguli KGE, KHE in circulo sunt puncta E, K, G, H; quare erit angulus EGH aequalis angulo EKH; et quoniam recti sunt anguli EFK, EHK in circulo sunt puncta E, F, K, H; quare erit angulus EFH aequalis an-

gulo

gulo EKH, hoc eſt, angulo EGH: in circulo igitur ſunt puncta E, F, G, H.

Et, iiſdem manentibus, ſi ſit punctum H in circumferentia, atque puncta E, F, G, H in circulo, continget juncta EH circulum in H.

Quoniam rectae AB, CD biſectae ſunt in F, G, et eſt K centrum circuli, erunt anguli EFK, EGK recti; quare in circulo ſunt puncta E, F, G, K; in circulo igitur ſunt puncta E, F, K, G, H; quare erit angulus EHK aequalis angulo EGK; rectus autem eſt angulus EGK; rectus igitur eſt angulus EHK; quare contingit EH circulum in H.

Propoſitio Pappi aliter enunciari poteſt hoc modo.

PROP. *Tab.* I. *Fig.* I.

Sit circulus ABC et recta BC circulo occurrens in B, C; ſitque recta AD circulum contigens in A, et rectae BC occurrens in D bifariam ſecetur BC in E, et occurrat juncta AE

T circulo

circulo in G; per punctum D ducatur quaevis recta circulo occurrens in F, H; et GF, GH, jungantur rectae BC occurrentes in K, L; erit EK aequalis ipsi EL.

Factum jam sit, et ipsi KL parallela ducatur HM rectis GF, GA occurrens in M, X; et bifariam secetur FH in N, et jungantur NE, NX, NA et AH.

Quoniam est EK aequalis ipsi EL, erit et MX aequalis ipsi XH; est autem FN aequalis ipsi NH; parallelae igitur sunt rectae NX, FM; quare erit angulus HNX aequalis angulo HFM, hoc est, angulo HAX; in circulo igitur sunt puncta A, N, X, H: est igitur angulus ANH aequalis angulo AXH, hoc est, angulo AEL; quare in circulo sunt puncta A, E, N, D; contingit igitur AD circulum [per *Lem.* praec.] in A, quod quidem ita se habet.

Componetur autem sic. Quoniam contingit DA circulum in A, in circulo sunt puncta A, E, N, D [per *Lem.* praec.]; quare erit angulus AND aequalis angulo AED, hoc est angulo AXH; in circulo igitur sunt puncta

cta A, N, X, H; quare erit angulus HNX aequalis angulo HAX, hoc eſt, angulo HFM; parallelae igitur ſunt rectae NX, FM; et quoniam eſt FN equalis ipſi NH, erit et MX aequalis ipſi XH, quare erit et EK aequalis ipſi EL. Q. E. D.

Alias quoque propoſitiones huic ſpectantes excogitavi, quarum aliquot huic ſubjicere viſum eſt.

PROP. I. *Tab.* 1. *Fig.* 4.

Sit circulus ABC, et recta AB circulo occurrens in A, B; et ſit C punctum in circumferentia circuli; jungantur CA, CB, ſitque recta DE parallela rectae AC, rectis AB, CB occurrens in D, E; per punctum D ducatur quaevis recta circulo occurrens in F, G; et CF, CG jungantur rectae DE occurrentes in H, K; erit rectangulum HEK aequale rectangulo CEB.

Factum

Factum jam ſit, et jungantur BH, BF. Quoniam eſt rectangulum HEK aequale rectangulo CEB, in circulo ſunt puncta K, H, C, B, quare erit angulus BHD aequalis angulo BCG, hoc eſt, angulo BFD: in circulo igitur ſunt puncta B, H, F, D; aequalis igitur eſt angulus ADH angulo CFB, hoc eſt, angulo CAB; quare erit DE parellela rectae AC. Quod quidem ita ſe habet.

Componetur autem ſic. Qoniam eſt recta DE parallela rectae AC, erit angulus ADH aequalis angulo CAB, hoc eſt, angulo CFB: in circulo igitur ſunt puncta B, H, F, D; quare erit angulus BHD aequalis angulo BFD, hoc eſt, angulo BCG: in circulo igitur ſunt puncta K, H, C, B; quare erit rectangulum HEK aequale rectangulo CEB. Q. E. D.

LEMMA.

Sint recta A, B, C, D; E, F, G, H; ſitque ut A ad B, ita C ad D, et ut E ad F, ita G ad H; erit rectangulum A in E ad rectangulum B in F, ut rectangulum

rectangulum C in G ad rectangulum D in H.

Est enim rectangulum A in E ad rectangulum B in E, ut A ad B, hoc est, ut C ad D, hoc est, ut rectangulum C in G ad rectangulum D in G; et est rectangulum B in E ad rectangulum B in F, ut E ad F, hoc est, ut G ad H, hoc est, ut rectangulum D in G ad rectangulum D in H: quare, ex aequo, erit rectangulum A in E ad rectangulum B in F, ut rectangulum C in G ad rectangulum D in H. Q. E. D.

PROP. II. *Fig.* 5. *Tab.* 1.

Sit circulus ABC, et recta AB circulo occurrens in A, B; et sit C punctum in circumferentia circuli, et CA, CB jungantur, et sit punctum D in recta AB, sitque BE ad BC ut BD ad DA; per punctum D ducatur quævis recta circulo occurrens in F, G; et CF, CG jungantur rectæ AB occurrentes in H, K; erit rectangulum

lum HAK ad rectangulum HBK, ut quadratum ex AC ad rectangulum CBE.

Factum jam sit, et ducatur DL parallela rectae AC rectis CB, CF, CG occurrens in L, M, N; et per punctum L ducatur parallela rectae AB rectis CF, CG occurrens in O, P.

Quoniam est rectangulum HAK ad rectangulum HBK, ut quadratum ex AC ad rectangulum CBE, erit, alternando, rectangulum KAK ad quadratum ex AC, ut rectangulum HBK ad rectangulum CBE. Quoniam vero est OL ad LM, ut HA ad AC; et PL ad LN, ut KA ad AC; erit [per *Lem.* praec.] rectangulum OLP ad rectangulum MLN, ut rectangulum HAK ad quadratum ex AC, hoc est, ut rectangulum HBK ad rectangulum CBE; et, invertendo, erit rectangulum MLN ad rectangulum OLP, ut rectangulum CBE ad rectangulum HBK; est autem rectangulum OLP ad quadratum ex CL, ut rectangulum HBK ad quadratum ex BC; quare erit rectangulum MLN ad quadratum ex CL, ut rectangulum CBE ad quadratum ex BC, hoc est, ut BE ad BC; et quoniam est BE ad BC, ut BD ad DA, hoc

est,

eſt, ut BL ad LC, hoc eſt, ut rectangulum CLB ad quadratum ex CL; erit rectangulum MLN ad quadratum ex CL, ut rectangulum CLB ad quadratum ex CL; eſt igitur rectangulum MLN aequale rectangulo CLB. Quod quidem [per *Prop.* praec.] ita ſe habet.

Componetur autem ſic. Quoniam eſt rectangulum MLN, aequale rectangulo CLB, erit rectangulum MLN ad quadratum ex CL, ut rectangulum CLB, ad quadratum ex CL, hoc eſt, ut BL ad LC; et quoniam eſt BL ad LC, ut BD ad DA, hoc eſt, ut BE ad BC, hoc eſt, ut rectangulum CBE ad quadratum ex BC, erit rectangulum MLN ad quadratum ex CL, ut rectangulum CBE ad quadratum ex BC: eſt autem quadratum ex CL ad rectangulum OLP, ut quadratum ex BC ad rectangulum HBK; quare erit rectangulum MLN ad rectangulum OLP, ut rectangulum CBE ad rectangulum HBK; et, invertendo, erit rectangulum OLP ad rectangulum MLN, ut rectangulum HBK ad rectangulum CBE. Quoniam vero eſt HA ad AC, ut OL ad LM, et KA ad AC, ut PL ad LN, erit [per *Lem.* praec.] rectangulum HAK ad quadratum ex AC, ut rectangulum OLP ad rectangulum MLN, hoc eſt, ut rectangulum

gulum HBK ad rectangulum CBE, et, alternando, erit rectangulum HAK ad rectangulum HBK, ut quadratum ex AC ad rectangulum CBE. Q. E. D.

PROP. III. *Tab.* 1. *Fig.* 6.

Sit circulus ABC et recta AB circulo occurrens in A, B; et sit C punctum in circumferentia circuli; et CA, CB jungantur, et sit punctum D in recta AB, sitque quaevis recta EF rectis CA, CB occurrens in E, F; et sit FG ad CF, ut DB ad DA; per punctum D ducatur qaevis recta circulo occurrens in H, K; et CH, CK jungantur rectae EF occurrentes in L, M; erit rectangulum LEM ad rectangulum LFM, ut quadratum ex EC ad rectangulum CFG.

Factum jam sit, et ducatur DN parallela rectae AC rectis CB, CH, CK occurrens in N, O;

N, O, P; et per punctum N ducatur parallela rectae EF, rectis CO, CP occurrens in Q. R.

Quoniam est rectangulum LEM ad rectangulum LFM, ut quadratum ex EC ad rectangulum CFG, erit rectangulum LEM ad quadratum ex EC, ut rectangulum LFM ad rectangulum CFG. Quoniam vero est QN ad NO ut LE ad EC, et RN ad NP ut ME ad EC; erit [per *Lem.* ad Prop. 2.] rectangulum QNR ad rectangulum ONP, ut rectangulum LEM ad quadratum ex EC, hoc est, ut rectangulum LFM ad rectangulum CFG: et, invertendo, erit rectangulum ONP ad rectangulum QNR, ut rectangulum CFG ad rectangulum LFM: est autem rectangulum QNR ad quadratum ex CN, ut rectangulum LFM ad quadratum ex CF; quare erit rectangulum ONP ad quadratum ex CN, ut rectangulum CFG ad quadratum ex CF, hoc est, ut FG ad CF: et quoniam est FG ad CF ut DB ad DA, hoc est, ut BN ad NC, hoc est, ut rectangulum CNB ad quadratum ex CN; erit rectangulum ONP ad quadratum ex CN, ut rectangulum CNB ad quadratum ex CN: est igitur rectangulum ONP aequale rectangulo CNB. Quod quidem [per *Prop.* 1.] ita se habet.

Componetur autem ſic. Quoniam eſt rectangulum ONP aequale rectangulo CNB, erit rectangulum ONP ad quadratum ex CN, ut rectangulum CNB ad quadratum ex CN, hoc eſt, ut BN ad NC: et quoniam eſt BN ad NC ut BD ad DA, hoc eſt, ut GF ad FC, hoc eſt, ut rectangulum CFG ad quadratum ex CF; erit rectangulum ONP ad quadratum ex CN ut rectangulum CFG ad quadratum ex CF: eſt autem quadratum ex CN ad rectangulum QNR, ut quadratum ex CF ad rectangulum LFM; quare erit rectangulum ONP ad rectangulum QNR, ut rectangulum CFG ad rectangulum LFM: et, invertendo, erit rectangulum QNR ad rectangulum ONP, ut rectangulum LFM ad rectangulum CFG. Quoniam vero eſt LE ad EC ut QN ad NO, et ME ad EC ut RN ad NP; erit [per *Lem.* ad Prop. 2.] rectangulum LEM ad quadratum ex EC, ut rectangulum QNR ad rectangulum ONP, hoc eſt, ut rectangulum LFM ad rectangulum CFG: et, alternando, erit rectangulum LEM ad rectangulum LFM, ut quadratum ex EC ad rectangulum CFG. Q. E. D.

Corollarium.

Corollarium. Sit circulus ABC (*Tab.* 1. *Fig.* 6.) et recta AB circulo occurrens in A, B; et sit C punctum in circumferentia circuli, et CA, CB jungantur; sitque recta EF rectis CA, CB occurrens in E, F; in AB producta sumatur punctam D, ut sit DB ad DA, ut quadratum ex CE ad quadratum ex CF; bifariam secetur EF in T, et per punctum D ducatur quaevis recta circulo occurrens in H, K, et CH, CK jungantur rectae EF occurrentes in L, M; erit LT aequalis ipsi TM.

Sit FG ad FC ut DB ad DA. Quoniam est rectangulum CFG ad quadratum ex CF, ut FG ad FC, hoc est, ut DB ad DA; et est DB ad DA ut quadratum ex CE ad quadratum ex CF: erit rectangulum CFG ad quadratum ex CF ut quadratum ex CE ad quadratum ex CF: est igitur quadratum ex CE aequale rectangulo CFG; est autem [per *Prop.* praec.] rectangulum LEM ad rectangulum LFM ut quadratum ex CE ad rectangulum CFG; quare erit rectangulum LEM aequale rectangulo LFM: erit igitur EL ad LF ut FM ad ME, et, componendo, erit EF ad FL ut EF ad EM; aequalis igitur est EM ipsi FL: et quoniam est ET aequalis ipsi TF, erit LT aequalis ipsi TM. Q. E. D.

PROP.

PROP. IV. *Tab.* 1. *Fig.* 7.

Sit recta AB circulo occurrens in A, B; et ſint rectae AC, BC circulum contingentes in A, B; et per quodvis punctum D in AB ducatur DE parallela rectae AC, rectae BC occurrens in E. Si per punctum D ducatur quaevis recta circulo occurrens in F, G; et CF, CG jungantur, rectae DE occurrentes in H, K; erit rectangulum HEK aequale quadrato ex DE.

Factum jam ſit. Jungantur CD; et occurrat recta DF rectis AC, BC in L, M; per M ducatur recta rectis AC, DE parallela; occurratque rectis CH, CE, CD, et AD, in N, O, P et Q punctis.

Quoniam eſt rectangulum HEK aequale quadrato ex DE, et eſt rectangulum NMO ad quadratum ex MP ut rectangulum HEK ad quadratum ex DE; erit rectangulum NMO aequale quadrato ex MP: et eſt rectangulum

FMG

FMG aequale quadrato ex MB; quare erit rectangulum FMG ad rectangulum NMO, ut quadratum ex MB ad quadratum ex MP: et quoniam eſt FL ad LC ut FM ad MN, et GL ad LC ut GM ad MO; erit [per *Lem.* ad prop. 2.] rectangulum FLG, hoc eſt, quadratum ex AL, ad quadratum ex LC, ut rectangulum FMG ad rectangulum NMO, hoc eſt, ut quadratum ex MB ad quadratum ex MP: eſt autem quadratum ex MQ ad quadratum ex MP, ut quadratum ex AL ad quadratum ex LC, hoc eſt, ut quadratum ex MB ad quadratum ex MP; quare erit MQ aequalis rectae MB: eſt autum AC ad CB, ut MQ ad MB; aequalis igitur eſt recta AC rectae CB. Quod quidem verum eſt, quoniam rectae AC, BC circulum contingunt in A, B punctis.

Componetur autem ſic. Quoniam rectae AC, BC circulum contingunt in A, B punctis, erit recta AC aequalis rectae BC: et eſt MQ ad MB ut AC ad CB; quare erit MQ aequalis rectae MB; eſt igitur quadratum ex MQ eaquale quadrato ex MB, hoc eſt rectangulo FMG; quare erit rectangulum FMG ad rectangulum NMO ut quadratum ex MQ ad rectangulum NMO: et quoniam eſt FL ad LC ut FM ad MN, et GL ad LC ut GM ad MO;

MO; erit rectangulum FLG, hoc est, quadratum ex AL ad quadratum ex LC ut rectangulum FMG ad rectangulum NMO, hoc est, ut quadratum ex MQ ad rectangulum NMO: est autem quadratum ex MQ ad quadratum ex MP ut quadratum ex AL ad quadratum ex LC, hoc est, ut quadratum ex MQ ad rectangulum NMO; aequale igitur est rectangulum NMO quadrato ex MP: et quoniam est rectangulum HEK ad quadratum ex DE ut rectangulum NMO ad quadratum ex MP, erit rectangulum HEK aequale quadrato ex DE. Q. E. D.

PROP. V. *Tab.* 1. *Fig.* 8.

Sit recta AB circulo occurens in A, B, et sint rectae AC, BC circulum contingentes in A, B; per punctum quodvis D in AB ducatur recta circulo occurrens in F, G; et CF, CG jungantur, rectae AB occurentes in H, K; erit rectangulum HAK ad rectangulum HBK, ut quadratum ex AD ad quadratum ex DB.

Factum

Factum jam sit. Et ducatur DL parallela rectae AC, rectis CB, CF, CG occurrens in L, M, N; et ducatur LO parallela rectae AB, rectis CF, CG occurrens in O, P.

Quoniam est rectangulum HAK ad rectangulum HBK, ut quadratum ex AD ad quadratum ex DB, hoc est, ut quadratum ex CL ad quadratum ex LB; et est rectangulum HBK ad rectangulum OLP, ut quadratum ex CB ad quadratum ex CL; erit rectangulum HAK ad rectangulum OLP, ut quadratum ex CB vel AC ad quadratum ex BL: et, alternando, erit rectangulum HAK ad quadratum ex AC, ut rectangulum OLP ad quadratum ex BL. Rursus, quoniam est OL ad LM ut HA ad AC, et PL ad LN ut KA ad AC; erit [*Lem.* ad prop. 2.] rectangulum OLP ad rectangulum MLN, ut rectangulum HAK ad quadratum ex AC, hoc est, ut rectangulum OLP ad quadratum ex BL. Est igitur rectangulum MLN aequale quadrato ex BL, hoc est, quadrato ex DL. Quod quidem [*Prop.* praec.] verum est.

Componetur autem sic. Quoniam est rectangulum MLN aequale [*Prop.* praec.] quadrato ex DL, hoc est, quadrato ex BL, erit rectangulum OLP ad rectangulum MLN

ut

ut rectangulum OLP ad quadratum ex BL: quoniam vero est HA ad AC ut OL ad LM, et KA ad AC ut PL ad LN; erit rectangulum [per *Lem.* ad prop. 2.] HAK ad quadratum ex AC, ut rectangulum OLP ad rectangulum MLN, hoc est, ut rectangulum OLP ad quadratum ex BL: et alternando, erit rectangulum HAK ad rectangulum OLP, ut quadratum ex AC vel BC ad quadratum ex BL: est autem rectangulum OLP ad rectangulum HBK, ut quadratum ex CL ad quadratum ex CB; quare erit rectangulum HAK ad rectangulum HBK, ut quadratum ex CL ad quadratum ex BL, hoc est, ut quadratum ex AD ad quadratum ex DB. Q. E. D.

PROP. VI. *Tab.* 1. *Fig.* 9.

Sint duae rectae AC, BC, circulum contingentes in A, B, et sit D punctum in recta AB; sit recta EF, rectis AC, BC occurens in E, F, et juncta CD in G: si per punctum D ducatur quævis recta circulo occurrens in H, K, et CH, CK jungantur, rectae EF occurrentes in L, M; erit

erit rectangulum LEM ad rectangulum LFM, ut quadratum ex GE ad quadratum ex GF.

Factum jam sit. Per puctum D ducatur recta parallela rectae AC, rectis CB, CH CK occurens in N, O, P; per punctum N ducatur recta parallela rectae EF, rectis CH, CK occurrens in Q, R; et per punctum F ducatur FS parallela rectae AC, rectae CD occurrens in S.

Quoniam est rectangulum LEM ad rectangulum LFM, ut quadratum ex EG ad quadratum ex FG, hoc est, ut quadratum ex EC ad quadratum ex FS; erit alternando, rectangulum LEM ad quadratum ex EC, ut rectangulum LFM ad quadratum ex FS: quoniam est rectangulum QNR ad quadratum ex CN, ut rectangulum LFM ad quadratum ex CF, et est quadratum ex CN ad quadratum ex ND, ut quadratum ex CF, ad quadratum ex FS; erit rectangulum QNR, ad quadratum ex ND, ut rectangulum LFM ad quadratum ex FS; hoc est, ut rectangulum LEM ad quadratum ex EC. Quoniam vero est LE ad EC ut QN ad NO, et ME

 ad

ad EC ut RN ad NP, erit [per *Lem.* ad prop. 2.] rectangulum LEM ad quadratum ex EC, ut rectangulum QNR ad rectangulum ONP; quare erit rectangulum QNR ad rectangulum ONP, ut rectangulum QNR ad quadratum ex ND: est igitur rectangulum ONP aequale quadrato ex ND. Quod quidem [per *Prop.* 4.] verum est.

Componetur autem sic. Quoniam est rectangulum ONP aequale quadrato ex ND, erit rectangulum QNR ad rectangulum ONP ut rectangulum QNR ad quadratum ex ND; et quoniam est LE ad EC ut QN ad NO, et ME ad EC ut RN ad NP, erit rectangulum LEM ad quadratum ex EC, ut rectangulum QNR ad rectangulum ONP, hoc est, ut rectangulum QNR ad quadratum ex ND. Quoniam vero est rectangulum QNR ad quadratum ex CN, ut rectangulum LFM ad quadratum ex CF, et est quadratum ex CN ad quadratum ex ND, ut quadratum ex CF ad quadratum ex FS; erit rectangulum QNR ad quadratum ex ND, ut rectangulum LFM ad quadratum ex FS; quare erit rectangulum LEM ad quadratum ex EC, ut rectangulum LFM ad quadratum ex FS: et alternando, erit rectangulum LEM ad rectangulum LFM,

ut

ut quadratum ex EC ad quadratum ex FS, hoc eſt, ut quadratum ex EG ad quadratum ex FG. Q. E. D.

PROP. VII. *Tab. 2. Fig. 10.*

Sit recta AB circulo occurrens in A, B, et in ipſa ſumatur punctum C, et ducatur CD circulo occurrens in D, E, et AD, BE jungantur ſibi mutuo occurrentes in F; ducatur CG parallela rectae AF, rectae BF occurrens in G: ſi per punctum C ducatur quævis recta circulo occurrens in H, K, et FH, FK jungantur, rectae CG occurrentes in L, M; erit rectangulum LGM aequale quadrato ex CG.

Factum jam ſit. Jungantur CF, et occurrat recta CH rectis AF, BF in N, O; per punctum O ducatur recta parallela rectae CG, vel AF, rectis FL, FM, FC occurrens in P, Q, R, et rectis AB, DE in S, T.

Quoniam

Quoniam eſt rectangulum LGM aequale quadrato ex CG, et eſt rectangulum POQ ad quadratum ex OR, ut rectangulum LGM ad quadratum ex CG; erit rectangulum POQ aequale quadrato ex OR: et eſt rectangulum HOK aequale rectangulo EOB; quare erit rectangulum HOK ad rectangulum POQ, ut rectangulum EOB ad quadratum ex OR; et quoniam eſt HN ad NF ut HO ad OP, et KN ad NF ut KO ad OQ, erit [per *Lem.* ad prop. 2.] rectangulum HNK ad quadratum ex FN, ut rectangulum HOK ad rectangulum POQ, hoc eſt, ut rectangulum EOB ad quadratum ex OR: eſt autem quadratum ex FN ad rectangulum AND, ut quadratum ex OR ad rectangulum SOT; quare erit rectangulum HNK ad rectangulum AND, ut rectangulm EOB ad rectangulum SOT: et quoniam eſt rectangulum HNK aequale rectangulo AND, erit rectangulum EOB aequale rectangulo SOT; quare erit OB ad OS ut OT ad OE, hoc eſt, erit BG ad GC ut GC ad GE: eſt igitur angulus GEC aequalis angulo GCB, hoc eſt, angulo BAD; quod quidem verum eſt, quoniam puncta A, B, E, D, ſunt in circulo.

Componetur

Componetur autem ſic. Quoniam puncta A, B, E, D ſunt in circulo, erit angulus GEC aequalis angulo BAD, hoc eſt, angulo GCB; quare erit BG ad GC ut GC ad GE, hoc eſt, erit OB ad OS ut OT ad OE: eſt igitur rectangulum SOT aequale rectangulo EOB, hoc eſt, rectangulo HOK; quare erit rectangulum HOK ad quadratum ex OR, ut rectangulum SOT ad quadratum ex OR, hoc eſt, ut rectangulum AND, vel HNK, ad quadratum ex FN. Quoniam vero eſt HN ad NF ut HO ad OP, et KN ad NF ut KO ad OQ, erit rectangulum HNK ad quadratum ex FN, ut rectangulum HOK ad rectangulum POQ; quare erit rectangulum HOK ad rectangulum POQ ut rectangulum HOK ad quadratum ex OR; eſt igitur rectangulum POQ aequale quadrato ex OR: et quoniam eſt rectangulum LGM ad quadratum ex GC, ut rectangulum POQ ad quadratum ex OR, erit rectangulum LGM aequale quadrato ex GC. Q. E. D.

PROP. VIII. *Tab.* 2. *Fig.* 11.

Sit recta AB circulo occurrens in A, B, et in AB ſumatur punctum C,

ducatur

ducatur CD circulo ocurrens in D, E, et jungantur AD, BE, sibi mutuo occurrentes in F; per punctum C ducatur quaevis recta circulo occurrens in G, H, et FG, FH jungantur, rectae AB occurrentes in K, L; erit rectangulum KAL ad rectangulum KBL, ut quadratum ex AC ad quadratum ex BC.

Factum jam sit. Ducatur CM parallela rectae AF, rectis FB, FK, FL occurrens in M, N, O; et per punctum M ducatur parallela rectae AB, rectis FK, FL occurrens in P. Q.

Quoniam est rectangulum KAL ad rectangulum KBL, ut quadratum ex AC ad quadratum ex BC, hoc est, ut quadratum ex FM ad quadratum ex BM; et est rectangulum KBL ad rectangulum PMQ, ut quadratum ex FB ad quadratum ex FM; erit rectangulum KAL ad rectangulum PMQ, ut quadratum ex FB ad quadratum ex BM, hoc est, ut quadratum ex AF ad quadratum ex CM: et, alternando, erit rectangulum KAL ad ad quadratum ex AF, ut rectangulum PMQ ad quadratum ex MC. Quoniam vero est PM ad MN ut KA ad AF, et QM ad MO ut LA ad AF; erit [per *Lem.* ad prop. 2.] rectangulum PMQ ad rectangulum NMO, ut rectangulum KAL ad quadratum ex AF, hoc est, ut rectangulum PMQ ad quadratum ex CM: est igitur rectangulum NMO aequale quadrato ex CM. Quod quidem [per *Prop. praec.*] verum est.

Componetur autem sic. Quoniam est rectangulum NMO aequale quadrato ex MC, erit rectangulum PMQ ad rectangulum NMO ut rectangulum PMQ ad quadratum ex MC: quoniam vero est KA ad AF ut PM ad MN, et LA ad AF ut QM ad MO; erit rectangulum KAL ad quadratum ex AN, ut rectangulum PMQ ad rectangulum NMO, hoc est, ut rectangulum PMQ ad quadratum ex MC: et, alternando, erit rectangulum KAL ad rectangulum PMQ, ut quadratum ex AF ad quadratum ex MC, hoc est, ut quadratum ex FB ad quadratum ex BM; et est rectangulum PMQ ad rectangulum KBL, ut quadratum ex FM ad quadratum ex FB; quare erit rectangulum KAL ad rectangulum KBL ut est

eſt quadratum ex FM ad quadratum ex BM, hoc eſt, ut quadratum ex AC ad quadratum ex BC. Q. E. D.

PROP. IX. *Tab.* 2. *Fig.* 12.

Sit recta AB circulo occurrens in A, B, et in AB ſumatur punctum C; per C ducatur recta circulo occurrens in D, E; et AD, BE jungantur, ſibi mutuo occurentes in puncto F; et ſit recta GH rectis AF, BF occurrens in G, H, et juncta FC in K: ſi per punctum C ducatur quaevis recta circulo occurrens in L, M, et FL, FM jungantur, rectae GH occurrentes in N, O; erit rectangulum OGN ad rectangulum OHN, ut quadratum ex GK ad quadratum ex HK.

Factum jam ſit. Ducatur per punctum C parallela rectae AF rectis FB, FL, FM occurrens in P, Q, R; et per punctum P ducatur

catur parallela rectae GH rectis FL, FM occurrens in S, T, et ducatur HV parallela rectae PC rectae FC occurrens in V.

Quoniam est rectangulum OGN ad rectangulum OHN, ut quadratum ex GK, ad quadratum ex HK, hoc est, ut quadratum ex FG ad quadratum ex HV; erit, alternando, rectangulum OGN ad quadratum ex FG, ut rectangulum OHN ad quadratum ex HV: quoniam est rectangulum SPT ad quadratum ex PF, ut rectangulum OHN ad quadratum ex HF, et est quadratum ex PF ad quadratum ex PC, ut quadratum ex HF ad quadratum ex HV, erit rectangulum SPT ad quadratum ex PC, ut rectangulum OHN ad quadratum ex HV, hoc est, ut rectangulum OGN ad quadratum ex FG. Quoniam vero est NG ad GF ut SP ad PQ, et OG ad GF ut TP ad PR, erit [per *Lem.* ad prop. 2.] rectangulum OGN ad quadratum ex FG, ut rectangulum SPT ad rectangulum QPR; quare erit rectangulum SPT ad rectangulum QPR, ut rectangulum SPT ad quadratum ex PC. Est igitur rectangulum QPR aequale quadrato ex PC. Quod quidem [per *Prop.* 7.] verum est.

Componetur autem sic. Quoniam est rectangulum QPR aequale quadrato ex PC, erit

 rectangulum

rectangulum SPT ad rectangulum QPR, ut rectangulum SPT ad quadratum ex PC: et quoniam est NG ad FG ut SP ad PQ, et OG ad GF ut TP ad PR, erit rectangulum OGN ad quadratum ex FG, ut rectangulum SPT ad rectangulum QPR, hoc est, ut rectangulum SPT ad quadratum ex PC. Quoniam vero est rectangulum SPT ad quadratum ex PF, ut rectangulum OHN ad quadratum ex HF, et est quadratum ex FP ad quadratum ex PC, ut quadratum ex HF ad quadratum ex HV; erit rectangulum SPT ad quadratum ex PC, ut rectangulum OHN ad quadratum ex HV: quare erit rectangulum OGN ad quadratum ex FG, ut rectangulum OHN ad quadratum ex HV: et, alternanda, erit rectangulum OGN ad rectangulum OHN, ut quadratum ex FG ad quadratum ex HV, hoc est, ut quadratum ex GK ad quadratum ex HK. Q. E. D.

SCHOLIUM.

Propositiones ante dictae in sectionibus quoque conicis obtinent. Idque reputanti sequentia observata sunt theoremata, a quibus alia etiam multa deduci possunt.

THEOR.

THEOR. 1. *Sint* [Tab. 2. Fig. 13.] *duae rectae* AB, CD, *sectionem conicam contingentis in* A, C; *et* A, C *jungantur; sit quaevis recta, rectis* AB, CD, AC *occurrens in* B, D, E, *et sectioni in* F, G *punctis; erit rectangulum* BFD *ad rectangulum* BGD, *ut quadratum ex* EF *ad quadratum ex* EG.

THEOR. 2. *Sit* [Tab. 2. Fig. 14.] *recta* AB *sectionem conicam contingens in* A, *et recta* CD *sectioni occurrens in* C, D *punctis; et jungantur* AC, AD; *occurratque quaevis recta rectis* AB, CD, AC, AD *in* E, F, G, H, *et sectioni in* K, L *punctis; erit rectangulum* EKF *ad rectangulum* ELF, *ut rectangulum* GKH *ad rectangulum* GLH.

THEOR. 3. *In* [Tab. 2. Fig. 15.] *sectione conica inscribantur duae rectae* AB, CD; *et jungantur* AC, BD; *occurratque quaevis recta rectis* AB, CD, AC, BD, *in* E, F, G, H, *et sectioni in* K, L *punctis; erit rectangulum* EKF *ad rectangulum* ELF, *ut rectangulum* GKH *ad rectangulum* GLH.

Ex hisce patet sequens theorema.

Si in sectione conica inscribatur quaevis figura quadrilatera, cujus tria latera per data puncta in recta transeunt; reliquum latus, vel

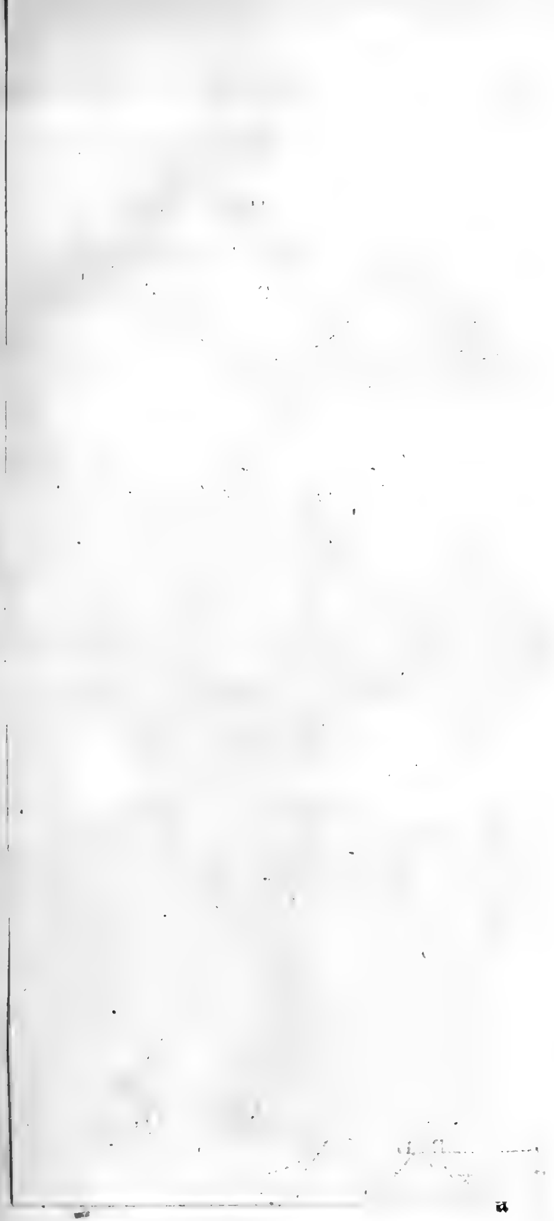
ad

ad datum punctum verget, vel parallelum erit positione datæ.

Ut finem faciam; theorema unum tantum subjiciam, observatione quidem haud indignum; et quod etiam in sectionibus conicis, paululum mutatum, obtinet; unde alia, à nullis, quod sciam, scriptoribus geometricis observata, deriventur.

Sit AB *diamete rcirculi* [Tab. 2. Fig. 16. 17.] *positione dati, occurratque recta* CD *ipsi normalis in dato puncto* C; *non sit autem* C *centrum circuli, et datum sit punctum* D *in recta* CD; *sitque quadratum ex* CD *majus rectangulo* ACB, *si sit punctum* C *intra circulum; si vero extra, sit quadratum ex* CD *minus rectangulo* ACB; *producatur* DC *ad* E, *ut sit* CE *aequalis ipsi* CD; *dabuntur duo puncta* F, G *extra rectam* DE, *ut, si ab ipsis inflectantur rectae* FH, GH *ad punctum quodvis* H *in circulo, rectae* DE *occurrentes in* K, L, *erit summa quadratorum ex* DK, EK *ad rectangulum* DLE *in ratione data.*

ART.



TAB. I.

Fig. 1.

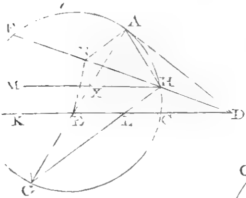


Fig. 2.

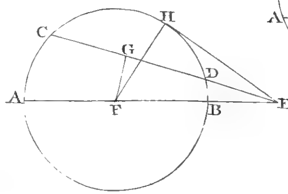


Fig. 3.

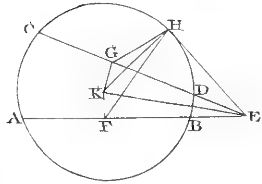


Fig. 4.

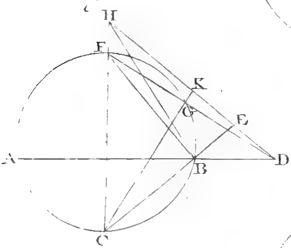


Fig. 5.

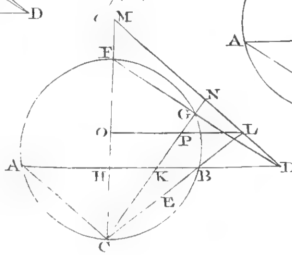


Fig. 6.

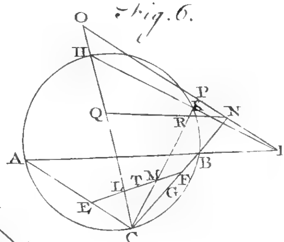


Fig. 7.

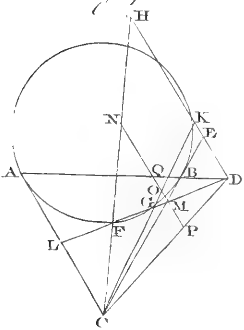


Fig. 8.

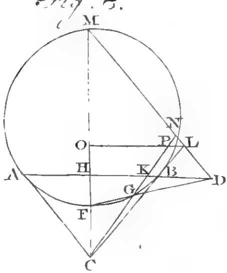
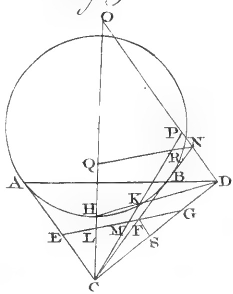


Fig. 9.



ART. IV.

Of the Cause of the Variation of the Obliquity of the Ecliptic, by COLIN McLAURIN, *late Fellow of the Royal Society, aud Professor of Mathematics in the University of* Edinburgh.

THE *French* aſtronomers in *Peru*, and Dr *Bevis* at *London*, and others elſewhere, have endeavoured to determine the obliquity of the ecliptic with greater accuracy than former aſtronomers had attained to. The reſult of their obſervations makes it to be now about $23° 29' 28''\frac{1}{3}$. It had been for ſome time reckoned $23° 29' 29''$; and before that, $23° 29' 30''$; and, by ſome antient aſtronomers, 19 or 20 minutes greater. Meſſ. *Caſſini* and *Bradley* had found it to vary a little: and this has given occaſion to a notion, that it is decreaſing continually; that there was a time when the axis of the earth lay in the plane of the earth's orbit; that it has been gradually riſing, till now it contains an angle with this plane of $66° 30' 31''\frac{2}{3}$; and that, in a a long period of time, it will become perpendicular to this plane; when we ſhall have every where a continual ſpring throughout the year. The Chevalier *de Louville* and M. *Godin* pretend even to compute the period of this motion; the former having gone to *Marſeilles* to meaſure the variation of this obliquity from the time when *Pitheas* meaſured it at the ſame place 2000 years ago; he found it diminiſhed by 20′, and thence eſtimates the period of this motion to be above 2 millions of years. M. *Godin* has ſince found, that, from the year 1655 to 1734, it decreaſed 55″, by comparing an obſervation of old M. *Caſſini*'s with his own, and thence deduces the period to be 1944000 years. And hence ſome authors have endeavoured to explain ſeveral old *Egyptian* and *Babyloniſh* traditions, concerning the ſun's having riſen twice in the weſt; and have taken occaſion to indulge themſelves in ſeveral ſpeculations.

I HAVE formerly ſeen a piece that was printed many years ago, but never, I think, publiſhed; containing a ſyſtem and a kind, of liturgy for the Pantheiſts or Spinoſiſts; wherein the author warmly eſpouſed this notion, concerning the variation of the obliquity ty of the ecliptic, and endeavoured to improve it, for ſupporting his favourite principle of equality; imagining, that, in the compaſs of this period, the ſtate and phænomena of all places on the earth would be upon the whole equal; an opinion that ſeems to ſuit the patrons of a blind and abſolute neceſſity: but whatever be thought of this continual and regular diminution of the angle in which the ecliptic and equator interſect each other, it is certain that ſuch an equality would not be the conſequence of it. Places of the earth would ſtill have their peculiarities: the people at the equator would have their days and nights conſtantly equal, how great or ſmall ſoever the obliquity of the ecliptic were; for this is a neceſſary conſequence of their deſcribing a great circle of the earth by the diurnal motion, that is always biſected by the boundary of light and darkneſs, which is likewiſe a great circle in conſequence of a neceſſary truth. The poles would have their ſix months day and ſix months night, as now; with this further ſingularity, that when the axis lay in the plane of the ecliptic, the heat muſt have been far more intolerable at the poles than is now known in any part of the earth,

earth, or could be known in any other part of it, during this long period: for the ſun muſt have then continued ten days together within 5° degrees of the zenith of the poles, without any intermiſſion of night, or abatement from a leſſer elevation, at which altitude he never continues above 40′ in the equinoctial days at the equator; a peculiarity as ſingular as it would have been fatal to the polar regions. The preſent obliquity of the ecliptic gives evidently great relief to the equatoreal regions; as by it the ſun is carried to both ſides of the zenith during the year, and is not permitted to dart conſtantly perpendicular rays upon them. It is to it likeways the more northern countries owe their ſummer and harveſt; which we are told is over with them in two months, or very little more, while the ſun's almoſt continued and inceſſant action ripens fruits which the heat of our ſun, much leſs theirs, on the 10th of *March*, the equinoctial day, could never be able to accompliſh. Thus it ſerves for diminiſhing their heat at the equator where it was uſeful to leſſen it, and for increaſing it towards the polar regions where it was proper to ſtrengthen it. But a regular and

and continued variation of the obliquity of the ecliptic would ruin ſome parts of the world, and diſturb an order well eſtabliſhed in nature, without ſerving the purpoſe of the Pantheiſts.

I WOULD not take up your time in making remarks on what is brought to ſupport this doctrine, from old fables of *Egyptian* or *Babyloniſh* prieſts delivered to *Herodotus*, that ſeem to have flown from their vanity. What is ſhewn by Dr. *Shaw*, in his travels, concerning the gradual riſing of the ſurface of the land of *Egypt*, from the annual ſediments of the *Nile*, ſeems to afford a much better argument againſt their boaſted antiquity in *Egypt*, than any thing that can be drawn from aſtronomical obſervations, to ſupport a vanity ſo univerſal, that we need not go very far from home to find abſurd enough inſtances of it. As for what is ſaid of *Pitheas* and other antients, their wrong notions concerning the refractions and the imperfection of their inſtruments, render any concluſions on ſo nice a ſubject, drawn from their obſervations, doubious. We ought to ſhew more regard to the modern obſervations brought for this doctrine, than to repreſent them in a level with the fables vented by the priefts of *Egypt*. According to feveral modern obfervations, the obliquity of the ecliptic varies: but we are to remember, that even the noble *Tycho Brache* committed an error of 18′ in drawing his meridian in his ifland of *Teraniburgum*; and, from his erroneous opinions concerning the refraction of the atmofphere, made the mean diftance of the moon lefs by 4 femidiameters of the earth than it really is. It is true, Mr *Caffini*'s obfervation in 1655, compared with late ones, makes it vary at the rate of 55″ in 79 years. Let us therefore fee, if a variation, fuch as that arifes from Meff. *Caffini*'s and *Goden*'s obfervations, be not a confequence of principles already eftablifhed in the folar fyftem; and offer fome hints of a method by which it may be afcertained whether this be the juft folution of this queftion, and how, from a proper feries of obfervations, made with great exactnefs, an improvement and correction in geography and navigation may be deduced from this inquiry, by bringing the theory of the earth's motion in her orbit, and particularly what relates to the declination of the fun to greater perfection. This will be the more

more uſeful, that the uſual methods of taking the latitude of places by the ſun's meridian altitudes, ſuppoſes the tables of his declination to be perfectly juſt; whereas there is ground to doubt of their preciſeneſs.

MATHEMATICAL inſtruments in general, and ſuch particularly as ſerve for aſtronomical purpoſes, have been, of late, brought to an exactneſs never known in former times. Hence ſome minute motions, or inequalities of motions, in the heavens, may be diſcovered, that have hitherto eſcaped the moſt diligent obſervers. They had diſcovered indeed ſome motions that are very minute or ſlow, ſuch as the preceſſion of the equinoxes; but this was owing to the continual increaſe of the effects of thoſe very minute motions, which, tho' inſenſible in one revolution, become very manifeſt in many revolutions. Motions of this kind are eaſily found, and are accurately determined, by comparing diſtant obſervations. But there are other minute motions which are hard to be diſcovered: theſe which do not grow up to be ſenſible in a number of revolutions; their effect in one part deſtroying their effect in another part of a revolution. Of this kind is the motion

tion lately diſcovered by Mr. *Bradley*, which, on this account, lay ſo long concealed from aſtronomers. Of the ſame kind is the moon of the fixed ſtars, that ought to ariſe from the parallax of the earth's orbit. And of this kind I ſuſpect the variation of the obliquity of the ecliptic to be.

To proceed therefore to what I take to be the cauſe of this motion, Sir *Iſaac Newton* has found, that the ſun and planets gravitate all towards each other mutually; that it is neither the ſun (according to the *Copernican* ſyſtem), nor the earth (according to *Ptolomy*), that is the center of the ſyſtem, or fixed point; but the center of gravity of the whole ſyſtem. That the ſun therefore moves about this center: and that when *Jupiter* and *Saturn*, the two biggeſt planets, are in the ſame right line, on the ſame ſide of the ſun; the center of the ſun will be almoſt a diameter of the ſun, diſtant from this fixed point. Hence, tho' we ſuppoſe the earth to move always in the ſame plane, the ſun will appear to have different declinations at the time of the ſolſtice: and, as the obliquity of the ecliptic is determined always from the declination of the ſun at the ſolſtice, it will therefore

therefore appear to vary, but not in a manner that will accresce and produce any sensible change in our seasons; but it will increase and decrease a little within small limits, and its variation will principally depend on the position of *Jupiter* and *Saturn* to the sun and earth, and will nearly return to the same magnitude when these return to the same position, with respect to one another and to the solstice. *Jupiter* has most effect in producing this variation; and, if it be found to have a connexion or dependence on his position to the sun and earth, it will be an indication that this is the true cause of the phænomenon.

Was the orbit of the earth perpendicular to the orbit of *Jupiter*, this variation would be much more considerable than it is, and might amount to above half a degree, or a diameter of the sun. Suppose [*Tab.* 2. *Fig.* 18.] BET to represent the orbit of the earth in such a case, C the common center of gravity of the sun and *Jupiter*, which we may consider as the center of the earth's orbit, because of the minuteness of the earth compared with these, T the solstitial point, PT*p* the axis of the earth, IS the right line joining

ing the centers of the ſun and *Jupiter*, which we ſuppoſe perpendicular to the orbit of the earth, while the earth is in the ſolſtice T. Then, if *Jupiter* be at I, and the ſun at S; the angle PTS will be the complement of the ſun's greateſt declination, or of the angle contained by the ecliptic and equator: but if *Jupiter* be at *i*, ad the ſun at *ſ*; the angle PT*ſ* will be the complement of this declination, and theſe would differ by the angle ST*ſ*, which, (becauſe CS is nearly equal to a ſemidiameter of the ſun) might amount to 32′.

But, becauſe the angle contained by the orbits of *Jupiter* and the earth is ſmall, the right line CS is ſeen very obliquely from the earth, and the variation of the obliquity thence ariſing is ſmall. The angle CTS is nearly to the inclination of *Jupiter*'s plane to the ecliptic, as the ſemidiameter of the ſun to the diſtance of the earth from the ſun: or the angle CTS is about $\frac{1}{215}$ of that inclination; therefore ST*ſ*, which is equal to 2CTS, may be ſomewhat more than a minute. And this may be ſufficient to account for any variation that it is yet diſcovered by aſtronomers. But, ſince it may exceed one minute, it appears that

that it is of moment to have this theory profecuted farther and afcertained: for an error of a minute, in a matter of fuch moment as the declination of the fun, which is an element in moft queftions in aftronomy, geography, and navigation, is not to be defpifed; efpecially now when fo great exactnefs in thefe matters is defired, and, I believe, may be obtained. Such a variation, however, can have no fenfible effect on our feafons; and there feems to be no ground to apprehend, that our year will degenerate into a perpetual fpring in any period of time; which, after all, would be far from being an advantage to us, or to the inhabitants of the earth in general.

ART.

ART. V.

Concerning the ſudden and ſurpriſing Changes obſerved in the Surface of Jupiter*'s Body; by the* ſame.

THERE have been more ſudden and ſurpriſing changes obſerved on the ſurface of *Jupiter*'s body, than on any other planet in the ſyſtem. *Caſſini* has obſerved ſeveral belts riſe on his body in one hours's time; and ſpots, in very different forms, are often ſeen upon him. I could wiſh that the poſition of his ſatellites were well conſidered when thoſe changes happen: for, ſince *Jupiter* has four moons, ſome of which are much nearer to him than our moon is to the earth; when they are all in conjunction or oppoſition to one another, very great tides muſt be produced in the ocean there, if there be any; and there is ground to ſuſpect that theſe ſudden and great changes on his ſurface are owing to tides analagous to thoſe we call

A a [illegible] in

TAB. II.

Fig. 10.

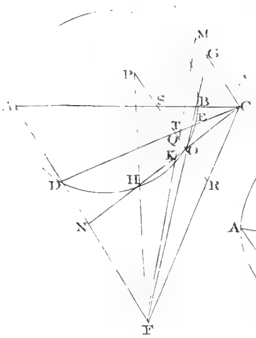


Fig. 11.

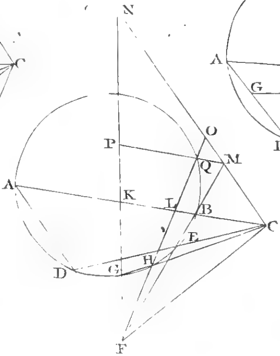


Fig. 12.

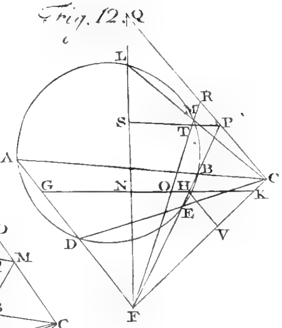


Fig. 13.

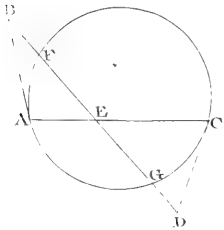


Fig. 14.

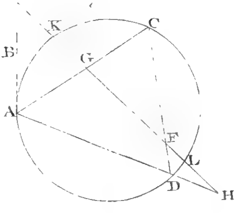


Fig. 15.

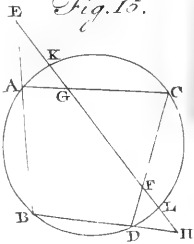


Fig. 16.

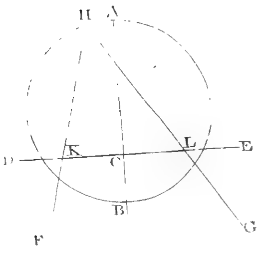


Fig. 17.

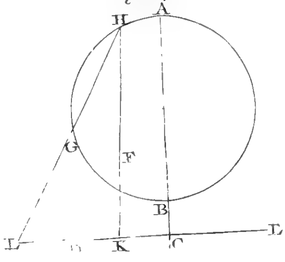
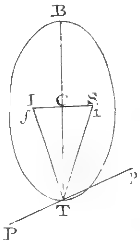


Fig. 18.



call ſpring tides. Further the velocity with which *Jupiter* revolves on his axis, and the greatneſs of his body, muſt contribute to the greatneſs of theſe tides, and of the inundations produced by them.

In *Jupiter*'s ſyſtem, the theory of gravity diſcovers itſelf in ſome particulars more manifeſtly than in any other part of the general ſyſtem. *Jupiter* is the only body that is, to ſenſe, ſpheroidical; the axis being leſs than the diameter of the equator by $\frac{1}{12}$. Poſſibly we may, by farther obſervations, diſcover the effects of the tides likewiſe; or rather may learn, that effects already viſible and known proceed from them.

The action of the ſatellites upon one another produces ſome irregularities in their motions; and from this it is ſuppoſed that their eclipſes do not anſwer accurately to the times computed from the tables. But there is another ſource, I believe, of their irregularities. *Jupiter*'s figure differs conſiderably from a ſphere; and the gravitatation towards him is not inverſely as the ſquare of the diſtance from his center. Now this variation from the regular courſe of gravity may produce ſome inequalities in their motions. And, in order to judge of theſe, I have computed the law of gravity towards ſuch a ſpheroid as *Jupiter*; and will ſubjoin ſome theorems on this ſubject.

It ſeems ſomewhat exraordinary, that in 130 years, ſince *Jupiter*'s ſatellites were diſcovered by *Galileo*, no eclipſe of any of theſe ſatellites by one another has been obſerved. Such an eclipſe could hold but for a ſmall time: but, as it is not difficult to compute nearly the time when they ought to happen, it would be worth while to look for them; ſince this might contribute to render their theory more perfect; which is of ſo much uſe in aſcertaining the longitudes of places. If it be found to be a phænomenon that paſſes over quickly, becauſe of their ſwift motions; it may be the more uſeful, providing it can be ſeen.

No phænomenon ſeems more extraordinary, relating to *Jupiter*, than that, according to *Caſſini*, ſome of his ſpots revolve in leſs time than others; the difference amounting to 4′. This is a phænomenon, of that kind, of which it is perhaps beſt not to attempt any explication, till the ſame be confirmed by more obſervations.

It

IT is worthy of our notice, that, from ſeveral phænomena, aſtronomers conclude the ſatellites to revolve on their axis, in the ſame time that they revolve around their primaries; by which means the exceeding great tides that would be produced in them are avoided. Thoſe ariſing from their various diſtances from the primaries, in their *apſides*, may be ſufficient for agitating their waters. The tides that would be produced in our moon, from this circumſtance alone, ought to be conſiderably greater than the tides produced in our ocean; and poſſibly, by a careful attention to the limits of thoſe black parts of the moon which formerly were conſidered as ſeas, but, becauſe of cavities diſcovered in them, with better teleſcopes, are ſuſpected by many to be only large planes, ſome deciſion of this queſtion might be obtained. Water is of too great importance, in natural operations, to ſuppoſe haſtily any planet to be deprived of it; tho' we muſt alſo allow that the variety of nature is not to be limited by our conceptions.

THEOR.

THEOR. I.

THE gravity towards an oblate ſpheroid in the axis produced, decreaſes in a leſs proportion than that in which the ſquare of the diſtance from the center increaſes: but the gravity in the plane of the equator decreaſes in a higher proportion than that in which the ſquare of the diſtance increaſes.

THEOR. II.

LET b denote the ſemidiameter of the equator, c the diſtance of the focus of the generating ellipſe from the center, d the diſtance of any body from the center of the ſpheroid in the plane of the equator; and the gravity at the diſtance d, will be to the gravity at the diſtance b, as $\frac{b^2}{d^2} \times 1 + \frac{3c^2}{10d^2} + \frac{9c^4}{56d^4}$ &c. to $1 + \frac{3c^2}{10b^2} + \frac{9c^4}{56b^4}$ &c. * * *

ART.

ART. VI.

Obſervations on Thunder and Electricity, by EBENEZER McFAIT M. D.

THE experiment propoſed by Mr *Franklin*, to prove that lightning and the electrical fire are the ſame, has often been repeated with ſucceſs both in *England* and abroad; ſo that the moſt noted electrical experiments have been performed by fire drawn from the clouds.

MR FRANKLIN alſo firſt diſcovered, that ſharp points attract and diſcharge the electrical matter moſt copiouſly; and from thence ſuppoſes, that a very ſharp pointed rod, fixed to the extremity of the top-maſt of a ſhip, with a wire conducted down from the foot of the rod round one of the ſhrouds, and over the ſhip's ſide into the ſea, would ſilently lead off the electrical fire, and ſave the ſhip from thunder in hot countries; and that, by a ſimilar method, buildings might be preſerved.

So

So uſeful a propoſal deſerves to be examined: variety of experiments may give hints for new improvements. For this reaſon the following obſervations are communicated, tho' not ſo compleat as might be wiſhed, being the reſult of one trial only.

It ſeldom thunders in this northern clime. In *June* 1752, there ſeemed to be ſome thunder at a diſtance from *Edinburgh*; but, from the beginning of *July* to the beginning of *October*, we had nothing almoſt but continual rains. The laſt ſummer was uncommonly warm and dry; and yet we had only a few claps of thunder at *Edinburgh*, one evening: and my attempts for making any of thoſe experiments were entirely unſucceſsful till *Saturday*'s night *September* 15. when we had a very great ſtorm.

I used a round iron rod, two tenths of an inch diameter, about eleven feet long, ſharpened at one end; the other end was inſerted into a glaſs-tube; and that tube ſtood in a common glaſs-bottle, which I held in my hand.

I used alſo another rod about three feet long, ſharpened in like manner, at one end; which ſtood with the other end in a glaſs-tube,

tube, which was ſtuck in the ground. I began upon the *Calton-hill*.

THE lightning and fire in the air abounded greatly; and yet it was ſome time before any thing elſe appeared. At laſt, ſome rain began to fall, and the air turned moiſter; then fire appeared upon the extremities of each of the rods in a ſmall pretty blaze: very like the fire which is diſcharged from the point of a ſword in the dark, when the perſon that holds it is electrified, and ſtands upon glaſs or roſin; or like that which appears upon any ſharp point, when preſented to an electrified gun-barrel, but in greater quantity. I touched the long rod with my finger; but had no ſparks from it. The ſhort rod was accidentally taken out of its tube, and yet continued to burn and blaze as formerly. In like manner the flame continued upon the end of the long rod, tho' I took hold of it any where at pleaſure above the glaſſes, till I moved my hand or finger along, within a few inches of the flame; then it was attracted by my hand, and vaniſhed.

I WENT from the *Calton-hill* to the *Caſtle-hill*, at the other end of the town; and, in paſſing thro' the ſtreets, no fire appeared upon on either of the rods: but almoſt immediately when I got clear of the houſes, upon the open hill, the point of the longer iron rod took fire. In the dark, I had loſt the tube belonging to the ſhorter rod; and the point of it did not catch fire when the longer one was kindled. Perhaps I did not wait long enough for a proper trial; for I ſoon touched the flame upon the long rod with the ſharp point of the ſhort one, and then it alſo took flame and continued burning, as before, without any further dependence upon the longer one.

I HELD the ſhorter rod by the ſharp end, and approached the blunt end of it to the flame, upon the point of the longer rod; then this blunt end catched the fire, and the flame upon the points of the two rods continued rather ſtronger, than on the ſingle one before, ſo long as I kept them in contact, and the fires within three or four inches of one another: but, when I drew them farther aſunder, the flame upon the extremity of the blunt rod vaniſhed. This happened as often as I tried it; and it is evident, that in like manner, I could have got the fire to fix upon the points of a great many rods, and

ſo

ſo have had them all flaming together. Once or twice, a flaſh of lightning ſeemed to dart directly againſt the point of the rod; then the fire, as I thought, expanded itſelf and united with the lightning, but it immediately began to ſhine again, when the lightning was paſt.

THO' it rained much in time of theſe obſervations, yet the fire upon the ends of the rods did not go out until it became ſo heavy, as if it were pouring down out of funnels.

AFTER this I went home for ſome time, reſolving to come abroad again when the ſtorm was more tolerable; but it continued to rain all night, ſo violently, that I was obliged, with regret, to leave ſeveral experiments to the chance of ſome future opportunity.

FOR example, I ſuſpected that the glaſs-tubes had not been of great uſe on this occaſion; and wanted to have tried, whether I ſhould have had the ſame appearances by uſing the rods alone, without any other *apparatus*. This is very probable; as alſo, that the glaſſes, by being wet, allowed the electrical fire to flow off as it was attracted.

I BEG leave to add a few remarks relative to this ſubject.

It would ſeem that experiments of this kind may be made without danger, when the thunder is at a moderate diſtance.

The lightning expands itſelf, as it flies; and, by expanſion, loſes its vigor.

Perhaps there is one ſimple and eaſy way of protecting maſts and ſpires from thunder, *viz.* to fix horizontally, upon the higheſt parts of them, a flat round piece of wood, of a foot diameter, or more, in order to prevent thoſe blazing fires from fixing upon them, and accumulating.

This ſtorm paſſed directly over *Edinburgh*, and came on from the ſouth by weſt, as nearly as could be eſtimated. There was a great deal of lightning, that night, above ſixty miles to the weſtward; but no thunder heard. At *Glaſgow*, there was very much lightning, and a few diſtant faint claps of thunder. On the road from *Belford* or *Berwick*, it lightned inceſſantly; but two claps of thunder only were heard, and thoſe very faint: ſo that there is reaſon to think, that the fire of this ſtorm ſpread over the breadth 130 miles, at leaſt. I wiſh I could alſo give ſome account where this thunder began, and how far it run before it was extinguiſhed.

On

On September 3. there were a great deal of ſtreamers, which roſe nearly from the ſame point that the thunder afterwards came from; and gradually worked north, till they deſcended below the horizon. The air had a thunder-like appearance, for ſeveral days before this ſtorm; and for ſome nights after it, the ſtreamery vapour appeared equally diffuſed, muddy, inert and languid, and not vibrating any variety of colours; as if the more volatile parts had been conſumed. It is highly probable that lightning and the *aurora borealis* are of the ſame materials. In hot countries ſtreamers are not ſeen, or but rarely; becauſe they are kindled into thunder and flaſhes of lightning: in cold countries ſtreamers abound, and it ſeldom thunders. The ſtreamers have ſerved to predict thunder to follow next day, in ſummer; and they have been alſo ſeen to break out into flaſhes of lightning. Thunder diſturbs the motion of the magnetic needle; and it has been lately found in *Sweden*, that ſtreamers do the ſame. Thus thunder, electricity, magnetiſm, and the *aurora borealis*, appear all wonderfully related; and many things

things remain undiscovered in this vast field, which is but just newly opened.

As it is probable that the height which some philosophers have assigned for the streamers in the atmosphere, is by several hundreds of miles too much; it were to be wished that people in various latitudes would carefully observe their altitude, at different times of the night; that, by comparing simultaneous observations, this matter may be determined with more certainty.

ART.

ART. VII.

Some Phænomena obſervable in foggy Weather; by the ſame.

ONE may often meet with opportunities of making obſervations: but he may not always have leiſure enough to judge of their importance, and make a proper uſe of them. This is the caſe at preſent, with reſpect to what follows.

I HAVE frequently obſerved, in foggy weather, during froſt, that, when the ſun was come to a proper altitude, there was a bow formed in the miſt, of a white colour; and the altitude of the bow ſeemed to ſubtend an angle equal to the elevation of the ſun above the horizon, and formed a ſemicircle.

THIS kind of miſt comes from the ocean, with a ſlow north-eaſterly wind; and overſpreads all the lower grounds: but, in a country like ours, ſo much diverſified with long ridges of hills, and interjacent plains and valleys, one may often emerge out of the miſt

mist by going up some neigbouring hill; then he will sometimes see the whole country around, as it were, buried under a vast deluge; and nothing but the tops of distant hills, appearing here and there above the flood; and he will think of diving down again into it, with a kind of horror. The air at that time, in these regions, is very pure, serene, and agreeable to breathe in.

In this, or a similar situation, there is a phænomenon observable, which I have seen many years ago; namely, a rainbow formed round one's shadow in the mist. The *French* Gentlemen who went to *Peru*, with a view to determine the figure of the earth; having, among many other difficulties, the embarassment of tedious and very frequent fogs to struggle with; had often occasion to observe this phænomenon. It seemed also worth mentioning here, that any one who has the curiosity may satisfy himself concerning it.

I had occasion to see it again on the 23d of *October* last, in travelling from *Glasgow*, northward. When I had ascended a very little out of the fog, so that my shadow was projected amongst it; the sun being behind, and almost on a level with me; there was a double

double range of colours, like thoſe of the rainbow, formed round the ſhadow. The colours of the uttermoſt range were broad and very diſtinct, and about two feet diſtant from the ſhadow every where; then there was a darkiſh interval, and after that another narrower range of colours, cloſely ſurrounding the ſhadow, which was very much contracted.

It is remarkable, that there is a ring of light, brighter than ordinary, that ſurrounds the ſhadows of all bodies; occaſioned by the inflection of the rays of light, as they paſs by the ſurfaces of thoſe bodies. But I dare not proceed to offer my conjectures in what manner this inflection of the rays contributes to the formation of thoſe ranges of colours.

ART.

ART. VIII.

Of the meaſures of Scotland, *compared with thoſe of* England, *by* JAMES GRAY.

THE ſtandard pint jug in the cuſtody of the burgh of *Sterling*, is made of braſs, in form of a *fruſtrum* of a cone; it weighs near ſixteen *averdupois* pounds. It appears, by its make, to be very old, and has two ſhields in relievo upon its ſide, with rude figures upon them, as repreſented in the margin. Its dimenſions, as near as could be taken from a veſſel of ſuch rude workmanſhip, are, *viz.*

Mean

	Inches
Mean diameter of its mouth,	$4\frac{3}{10}$
Mean diameter of the bottom,	$5\frac{3}{10}$
Mean depth.	6

It was five times carefully filled with clear river water, which was each time accurately weighed by a nice beam, that would turn, with leſs than a grain, when 100 ounces were in each ſcale; and its content of water, at a medium of theſe trials, which did differ but a few grains from one another, was found to weigh 26180 *troy* grains.

At the ſame time, a veſſel which was made to contain accurately, one hundred cubic inches, was filled with the ſame water: and after ſeveral trials, which did not differ one grain from one another; its content in water was found to weigh 25318 *troy* grains.

The ſtandard pint jug therefore contains $103\frac{404}{1000}$ cubic inches; and each cubic inch of water with which the jug was filled, weighs $253\frac{18}{100}$ *troy* grains.

The ſtandard *Sterling* jug, in the cuſtody of the Dean of Gild of *Edinburgh*, likewiſe made of braſs, and weighing about $20\frac{1}{2}$ *Averdupois* pounds, having the arms of *Scotland* and of *Sterling* marked in *relievo* upon

 its

its ſide, was in the ſame manner filled with water; and its content was found not to differ from that in the cuſtody of the burgh of *Sterling*, above a fiftieth part of a cubic inch. Perhaps it may not be improper to obſerve, that notwithſtanding the nicety of the beam, and the ſmall ſurface of the mouth of the veſſel, which contained 100 cubic inches; no difference was perceptible in the weight of its content of water, tho' the heat of this fluid was ſeveral degrees both above and below temperate in the thermometer; and that no difference was to be found between ſpring and river water; nor even between river water when clear, and when ſomewhat diſcoloured after a ſpeat.

By act. parl. of 19. *February* 1618. anent ſettling the meaſures and weights of *Scotland*, it is ſtatute and ordained, that the wheat firlot ſhall contain $21\frac{1}{4}$ of the *Sterling* jug; and that the bear firlot ſhall contain 31 of the ſame.

The *Engliſh* wine gallon contains, according to the Guild-hall ſtandard, 224 cubic inches; but, according to ſtatute, 231 cubic inches. The *Engliſh* ale gallon, both according to the Exchequer ſtandard and ſtatute, contains 282 cubic inches. The *Winchester*

chester bushel, according to act of parliament, 1697, and ratified in the first year of Q. *Anne*, contains $2150\frac{42}{100}$ cubic inches. Therefore

	Cubic Inches
The *Scotch* pint contains -	$103\frac{4}{10}$
The *English* wine pint, according to statute, contains -	$28\frac{7}{8}$
The *English* ale pint contains -	$35\frac{1}{4}$
The wheat firlot contains -	$2197\frac{34}{100}$
The bear firlot contains -	$3205\frac{54}{100}$
The *Winchester* bushel contains	$2150\frac{42}{100}$

According to act of parl. of 19th *February* 1618 above mentioned, the *Scotch* pint jug contains, of the clear running water of *Leith*, three pounds and seven ounces of *French Troas* weight, ordained to be the weight of *Scotland*; whence the *Scotch* pound is equal to 7616 *Troy* grains, or $15\frac{13}{15}$ ounces *Troy*.

By an accurate comparison of standard weights made at *London* (*vid. Philosoph. Transact.* N°470.) the *Averdupois* pound was found equal to 7000 *Troy* grains. Therefore

The

	Troy Grains,
THE *Troy* ounce being equal to	480
The *Scotch* ounce is equal to	476
The *Adverdupois* ounce is	$437\frac{1}{2}$

	English Inches.
THE *Scotch* foot is -	$12\frac{1}{15}$
The *Engliſh* foot is - -	12
The *Scotch* ell, according to the ſtandard of *Edinburgh*, is equal to - - - -	$37\frac{2}{15}$
The *Engliſh* ell is - -	45
The *Engliſh* yard is - -	36

	English Feet.
THE *Scotch* mile contains	5952
The *Engliſh* mile contains	5280
The *Scotch* acre	$55353\frac{6}{10}$ ſquare.
The *Engliſh* acre contains	43360 ſquare.

ART.

ART. IX.

A Dissertation on the Sexes of Plants; by CHARLES ALSTON, M. D. *King's Botanist in* Scotland, *Fellow of the Royal College of Physicians, and Professor of Medicine and Botany in the University of* Edinburgh.

THE smallest, and apparently most despicable productions of nature, are not so barren, but that they are capable, both to invite our speculations, and to recompence them. Even *Pliny* could say, "rerum natura nusquam magis quam in minimis tota est." (*a*) For how much soever the coelestial bodies declare magnificence, and immensity of power; yet do they not by far afford us so clear and cogent arguments of wisdom and design, as do the bodies of animals and plants. And for my part, says the great *Boyle*, I am apt to think, that the eye of a fly is a more curious piece of workmanship than the body of

(*a*) L. xi. c. 2.

of the ſun, at leaſt as far as appears to us. *(b)* Hence

THE exquiſite ſtructure of flowers, eſpecially of the moſt minute, and ſcarcely viſible duſt of the *apices*, commonly the ſport of the winds, has engaged the attention of the learned, both of this and the preceeding age: and yet, no body has been able to determine, with any certainty, whether it is abſolutely neceſſary to the fecundity of the ſeeds, or excrementitious only; which is the ſubject of the following inquiry.

ANIMALS and vegetables reſemble one another in ſo many things, that not only ſome of the antients, but even of the moderns alſo, ſeem to be at a loſs how to diſtinguiſh them, or fix the limits of each of theſe kingdoms *(c)*; yea, and to reckon analogy a ſufficient proof, that ſuch properties as are generally obſervable in the one, do belong to the other alſo. Thus *Empedocles* and *Anaxagoras* ſeem to have reaſoned, when they maintained, that both ſexes were conjoined in plants; that they were animated, yea and ſentient

(*b*) Vid. BOYLE's works, fol. edit. vol. I. p. 428. & vol. iv. p. 523.

(*c*) Vid. TYROCIN bot. p. 3.

tient beings, capable of pleaſure and pain, deſire and averſion, *&c.* (*d*)

I SHALL follow the philoſophers no further; my deſign being only to inquire into the riſe and progreſs of the doctrine concerning the ſexes of vegetables among botaniſts; and

(*d*) "Vita et in animalibus et in plantis eſſe deprehenſa "eſt. ANAXAGORAS itaque et EMPEDOCLES deſiderio "plantas duci aiunt; ſentire item, ac triſtitia voluptateque "affici affirmant. Et ANAXAGORAS quidem, animalia ip-"ſas eſſe, et voluptate ac dolore moveri, docuit; e foliorum "ſcilicet defluvio, et ex incremento, iſtud colligens. EMPE-"DOCLES vero ſexum his admiſtum eſſe arbitratus eſt. Eo-"dem modo PLATO quoque appetitu ſolum illas duci, ob ve-"hementum ſcilicet facultatis altricis neceſſitatem, affirmabat. "Quod ſi conſtet plantas voluptate ac dolore affici, tum ſen-"tire quoque rationi erit conſentaneam: et ubi hoc conſtite-"rit, tum appetitu quoque duci; ſi quidem ſomno reficiuntur, "et vigiliis excitantur, rationi conſentaneum erit. Ad eun-"dem modum ſi quaeramus, an ſpiritum ducant, et ſexuum "miſtionem habeant, an contra ſit; multam ſuper hoc am-"biguitatem, et quaeſtionem prolixam excitabimus.—Cum in "plantis reperiatur, quod unaquaeque ſpecies maſculum genus "habeat et femellum, et omnino quod maſculum eſt aſperius "ac durius rigidiuſque, femellum debilius et foecundius; quae-"rendum rurſus eſt, inveniaturne haec duo genera ſimul com-"miſta in plantis eſſe, ut EMPEDOCLES dicit. Id quod ego "ſane ita habere non arbitror." Thus ARISTOTLE de plantis, lib. 1. c. 1. et 2. See alſo Fragmentum GALENI in PLATONIS TIMAEUM, c. 2. Et librum de hiſtoria philoſophica GALENO adſcriptum ſub finem.

and whether it is ſupportable by experiments, or a mere hypotheſis?

Sect. I.

Theophrastus, the greateſt, as well as the eldeſt, of the *Greek* botaniſts whoſe works have been preſerved, can beſt explain, why ſome plants of the ſame kind were called females, and others males, by them. I know ſome reckon *Crateva* more antient, and cotemporary to *Hippocrates*, on the authority of ſome epiſtles fathered on the old man, and annexed to his works. But the learned have proven theſe letters to be ſpurious; and that *Crateva* lived not before *Mithridates*, whom he complimented with the name of a plant, as *Pliny* (*e*) informs us. As for *Hippocrates* himſelf, I find in him a *conyza femina*, but no other plant called either male or female.

2. But *Theophraſtus*, who ſucceeded *Ariſtotle* in his ſchool, in the 114. olympiad, very frequently mentions the ſexes of plants. Thus φασι δε τινες, ſays he, ϗ των ὁμοιογενων, τα μεν ανθειν, τα δε ὐ, &c. "But it is ſaid, that "of plants of the ſame kind, ſome flower, "others

(*e*) lib. 25. c. 6.

"thers not; as of the *palm-trees*, that the "male bloſſoms, not the female, which a- "bout the ſame time puts out the fruit. "Plants therefore of the ſame kind, are thus "differenced, as alſo all ſuch as cannot perfect "the fruit." *(f)* And elſewhere *(g)* "the "moſt common diſtinction of trees, is into "female and male, whereof the one is fer- "tile, but the other barren, in ſome. But, "when both are fertile, the female carries "the faireſt fruit, unleſs ſuch be called males, "for ſo ſome call them." And almoſt all the plants which he diſtinguiſhes into male and female, are equally fertile in both ſexes, as *abies*, *filix*, *cornus*, *tilia*, *cupreſſus*, *ciſtus*, *conyza*, &c. Even *palm-trees* he divides into fructiferous and barren; and the fructiferous again into female and male (*h*): ſo ſeems not much to have regarded analogy, in the diſtinction of ſexes, except it be in one ſpecies of the *palm-tree*.

(*f*) Hiſt. lib. 1. c. 22.
(*g*) Hiſt. lib. 3. c. 9.
(*h*) Hiſt. lib. 2. c. 8.

3. But of this famous tree he ſays, *(i)* unleſs the *ſpatha* be cut from the male, and, while it retains the down, flowers and duſt, (*k*) be ſhaken over the fruit of the female; it will never ripen, but fall off: which this ſprinkling prevents. "For the male, adds "he, of both the *fig-tree* and the *palm-tree*, "is of uſe to the female, ἀλλ' ἡ μεν οἷον μιξις, "ἡ δε κατ' ἄλλον τρόπον, but in the one *(l)* there "is, *quaſi coitus*; in the other it happens af-"ter a different manner." And elſewhere "(*m*) That the fruit will not continue on the "female *palm-tree*, unleſs the flower of the "male with the κονιορτὸς be ſhaken over it, "as ſome affirm, is certainly ſingular: for "tho' there is an evident reaſon for caprifica-"tion, which it ſomewhat reſembles; yet "none can be aſſigned for the effect of this "ſprinkling." And leaſt it ſhould hence be inferred in general, that female trees were of themſelves inſufficient for fructification, without the aſſiſtance of the males; he obſerves, that in order to ſuch a concluſion, this ought

(*i*) Hiſt. lib. 2. c. 9.

(*k*) Κονιορτὸς.

(*l*) *viz.* Palm-tree.

(*m*) Cauſ. Plant. lib. 3. c. 23.

ought to appear, not in one or two inſtances *(n)*, but in all, or the greateſt part of the females, τὴν γὰρ φυσίν ὕτω κρίνομεν τῦ γενῦς. Of this tree more afterwards.

4. DIOSCORIDES, who lived under *Nero*, has a male and female *mandragora*, *abrotanum*, *mercurialis*, *anagallis*, *ariſtolochia*, *ciſtis*, *filix*, *pæonia*, *polygonum*, *tithymalus*, *verbaſcum*, and an *arundo fæmina*: but, without any regard to analogy or to their fertility or barreneſs, does he call them ſo; for his *mercurialis-mas* carries the ſeed, and the *fæmina* is barren; and the male of all the reſt, equally fertile with the female. The ſame may be ſaid of *Galen* and the ſucceeding *Greeks*; none of them, ſo far as I have obſerved, mentioning the ſexes of the *palm-tree*.

5. PLINY, who flouriſhed under *Veſpaſian*, ſays indeed, "Arboribus, imo potius "omnibus quae terra gignet, herbiſque etiam, "utrumque ſexum eſſe, diligentiſſimi naturae "tradunt: nullis tamen arboribus (palma) "manifeſtius. Mas in palmite floret, fae-"mina citra florem germinat tantum, ſpicae "modo." *(o)* But when he comes to mention particular

(*n*) As here in the fig-tree and palm-tree.

(*o*) L. 13. c. 4.

particular plants, excepting what he has from *Theophrastus* of the *palm-tree*, he transgresses analogy as much as *Dioscorides*: his *mercurialis masculus* being the seed-bearing one; and his other male and female plants the same with those of *Theophrastus* and *Dioscorides*. Hence it appears that the *palm-tree* is the only instance, among the antients, where sexes are attributed to particular plants, on account of fertility or barrenness: other plants being called male or female, on various and very different accounts, and for distinctions sake only.

6. In a word, from *Theophrastus* down to *Cæsalpinus*, who died at *Rome* in 1603, the analogy between plants and animals, with regard to sexes, seems to have been intirely neglected. But *Cæsalpinus (p)* says expresly, "Fructum ferunt, non vero florent oxycae-" drus, taxus; in genere herbaceo mercuria-" lis, urtica, cannabis: quorum omnium ste-" riles *mares* vocant, *faeminas* autem fru-" ctiferas. In eo tamen genere faeminas " melius provenire et foecundiores fieri aiunt, " si juxta *mares* serantur; ut in palma est a-" nimadversum:

(*p*) De plantis, l. 1. c. 3.

" nimadverſum: quaſi halitus quidam, ex " mare efflans, debilem faeminae calorem ex- " pleat ad fructificandum."

SECT. II.

7. BUT of late it has been maintained by not a few, that there is as real a diverſity of ſexes in every ſpecies of plants, as in every ſpecies of animals; and, firſt of all by the celebrated naturaliſt Dr *N. Grew*, to whom therefore the honour of the invention is of right due. For this great man, in his anatomy of flowers, read before the Royal Society *November* 9. 1676, after noticing the ſecondary uſes of the duſt of the *apices*, which he calls *globulets* or ſmall particles within the *thecæ* of the attire; he adds, " But the " primary and chief uſe is ſuch as has reſpect " to the plant itſelf, and ſo appears to be ve- " ry great and neceſſary; becauſe even thoſe " plants which have no flower or foliature, " are yet ſome way or other attired; ſo " that it ſeems to perform its ſervice to the " ſeed, as the foliature to the fruit. In diſ- " courſe hereof with our learned *Savilian* pro- " feſſor, Sir *Thomas Millington*, he told me, " he

" he conceived that the attire doth ſerve as
" the male for the generation of the ſeed.
" I immediately, replied, that I was of the
" ſame opinion; gave him ſome reaſons for
" it, and anſwered ſome objections that might
" oppoſe them."

WHETHER, as ſome pretend, this compliment paid to Sir *Thomas Millington*, ſhows him to have been of this opinion before our author, I leave to the learned to determine; for my part, I think the words cannot bear it. And Mr *Ray* (*q*) gives it for Dr *Grew*, without naming Sir *Thomas*.

8. DR GREW then proceeds to give the ſum of his thoughts concerning this matter; and plainly aſſerts, as his opinion, that when the attire or *apices* break, or open, the globules or duſt falls down on the ſeed-caſe or *uterus*, and touches it with a *prolific virtue*; not by entering bodily, or as to its groſs ſubſtance, but only by communicating to it ſome *ſubtle* and *vivific effluvia* (*r*).

9. AMONG the firſt who adopted this doctrine, was Mr *John Ray*, that great natural hiſtorian; at firſt indeed only as probable (*s*),

(*q*) Hiſt. p. 17.

(*r*) See GREW's anat. fol. p. 171.

(s), but afterwards as proven by many arguments, which are collected in the preface to his *ſylloge ſtirpium Europearum extra Britannias naſcentium*, printed at *London* 1694, in 8*vo*: whether theſe arguments ſufficiently prove the doctrine will be conſidered below.

10. IN 1695, *Rudolphus Jacobus Camerarius*, profeſſor of botany and medicine at *Tubingen*, publiſhed there an *epiſtola de ſexu plantarum*, in 12*mo* (t): this I have not ſeen, but only an abſtract of it in the appendix to Miſcel. nat. cur. *Dec.* 3. *an.* 3. for 1695, and 1696, by M. *B. Valentini*. Here the generation of plants to be analogous to that of animals, he endeavours to ſhew in eight particulars; the laſt of which he calls the *palmarium* and *ponderoſiſſimum argumentum:* and it is this, "Certum eſt," ſays he, "ad animalium generationem copulam utriuſque ſexus exigi: quae in plantis adeo quoque neceſſaria eſt, ut ſi vel maris apices, vel faeminarum ſtyli, vel utraque deficiant, nulla proles ſequi poſſit; ut in frumento turcico, cui juba praemature reſecatur, et mercuriali mare, a faemina ſeparata, conſtat." But then he propoſes

three

(s) Vid. R. Hiſt. p. 18.

(t) Reprinted Francofurti, 1701, in 4to Lin. bib. bot. 173.

three objections againſt his own doctrine, which are ſtronger than all his arguments for it; yea, the third plainly confutes it: and they are theſe, as *Volentini* delivers them; "1*mo*, Plantae dantur apicibus copioſis, aſt "nullo ſtylo praeditae; adeoque mares ſine "faeminis, ut in equiſeto, lycopodio, &c. "2*do*, Videtur e contrario ſibi obſervaſſe faemi-"ninum ſine pretenſo virili ſemine. 3*tio*, Mer-"curialis, ſpinachiae, et cannabis femellas ſo-"litario, abſque vicini maris contagio excul-"tas, plurima granula ſeminaque foecunda "obtinuiſſe vidit: et haec in cauſa erant, cur "haec omnia cum aliquali oppoſiti formidine "proponat."

11. In 1703, Mr *Samuel Morland* laid before the Royal Society, ſome new obſervations on the parts and uſe of the flower in plants(*u*); here he ſays, "Dr *Grew* falls ſhort, in that "he ſuppoſes the *farina* only to drop upon "the outſide of the *uterus*, and to impregnate "the included ſeed by ſome *ſpirituous ema-"nations*, or *energetical impreſs*." So he paraphraſes *prolific virtue* and *ſubtle vivific effluvia*. He then propoſes a more probable hypotheſis, as he thinks, *viz.* "that the *farina*

"is

(*u*) Philoſ. Tranſact. No 287.

"is a congeries of ſeminal plants; one of "which muſt be conveyed into every *ovum* "or ſeed, before it can become prolific." He has the honour, I believe, of being the inventor of this hypotheſis; but is ſo far from making it probable, that the ſtructure and poſition of the parts of ſome of the flowers he inſtances, which are all the arguments advanced by him for it, ſufficiently confute it; e. g. *corona imperialis*. For allowing for once, which however cannot be allowed, that the *ſtylus* is always hollow, and that rain might waſh the duſt, or wind ſhake it down this cavity, into the heart of the ſeed in erect flowers; what muſt become of pendulous flowers, ſuch as the *crown imperial?* here the *farina* muſt be waſhed or ſhaken upwards.

12. As for *Waldschmidii diſſertatio de ſexu plantarum*, *Kiliae* 1705, *4to*, and *Gakenholzii diſſertatio de vegetabilium indole cognoſcenda*, *Helmſtadii* 1706, *in 4to*, mentioned by the learned *Heiſterus*, in his *praefatio in epiſtolam Burckhardi*, *Helmſtadii* 1750, *in 8vo*; I know nothing about them: but I have no reaſon to think, that they have been more ſucceſsful in demonſtrating the ſexes of plants than *Camerarius*, "qui, omnium planiſſime, ſexum

"plantarum expoſuit:" as the ſame profeſſor *Heiſter* (*x*) teſtifies.

13. In 1711, Mr *Geoffroy* preſented the Royal Academy at *Paris*, with "obſervati-"ons on the ſtructure and uſe of the princi-"pal parts of flowers;" wherein he attempts to prove, that the duſt of the *apices* really impregnates the ſeed. And after advancing almoſt all that had been ſaid on this ſubject by *Grew*, *Ray*, *Camerarius*, *Morland*, without mentioning them; he aſſerts, 1*mo*, That, the germ is never to be ſeen in the ſeed till the *apices* ſhed their duſt. And, 2*do*, That, if the *ſtamina* be cut out before the *apices* open, the ſeed will either not ripen, or be barren if it ripens: in proof whereof he gives two experiments that he made, one on the *mays*, by cutting off all the *ſtamina* before the *apices* opened; and the other on the *mercurialis*, by training up ſeparately ſome of the ſeed-bearing plants by themſelves. The conſe-quence was, ſome of the ears of the *mays* aborted altogether; others ripened a few ſeeds; as did alſo the *mercurialis* plants: both which ſeemed to be *foecund*. "Perhaps, ſays he, "the duſt of the *apices*, brought from ſome "other

(*x*) Pref. p. 70.

"other place by the wind, fertilized theſe few "ſeeds: that this is not impoſſible, we have "a fine inſtance in *Jovianus Pontanus*, precep- "tor to *Alphonſus* King of *Naples*, who, in a "poem, tells us, that in his time, there were "two *palm-trees*, a male cultivated at *Brindes* "and a female in the woods of *Otranto*, which "carried no fruit for many years. But at "length being elevated above the other trees "of the foreſt, ſo that it could ſee the male "*palm-tree* of *Brunduſium*, it then began to "bear good fruit in abundance. No doubt, "becauſe then it began to receive the duſt "of the *ſtamina*, carried by the wind from "the male *Palm-tree*, over the other trees of "the wood (*y*)." A very fine ſtory indeed!

I SHALL only further obſerve, concerning this learned author, that tho' he has added nothing new, of conſequence, on this ſubject, yet he ſeems to claim the honour of being the inventor of all; for after giving his conjectures, with relation to the manner how this duſt impregnates the ſeed, and as his own too, tho' the one be GREW's and the other *Morland*'s, he thus concludes,

"BUT

(*y*) Vid. Mem. Acad. 1711.

"BUT whichſoever of theſe conjectures "be pitched on, it remains always certain, "*by my obſervations*, that the duſt of the *a*-"*pices*, which has been hitherto neglected, "as vile excrements, which in a manner "disfigured the flowers, are nevertheleſs parts "eſſential, and neceſſary to the foecundity of "plants." The pains however he has been at, in examining and deſcribing the duſt of the *apices*, of a conſiderable number of plants, if accurately performed, might have procured him better treatment, than he met with from ſome of his countrymen.

14. In 1717, M. *Vaillant*, at the opening of the Royal garden, intertained his audience with a diſcourſe on the ſtructure of flowers and the uſe of their parts; which was printed in *French* and *Latin* at *Leiden* in 1718, in 4*to*: and thus he begins, "Since the flowers "are, without controverſy, the moſt eſſential "parts of plants, it is very fit that I entertain "you, in the firſt place, with them; and "the rather becauſe every botaniſt hitherto, "has given only confuſed ideas of them. The "language perhaps which I am to make uſe "of, may appear ſomewhat new in botany: "but, as it will abound in terms altogether

ſuitable

" ſuitable to the uſe of the parts which I am " to explain; I believe it will be much bet- " ter underſtood, than the old one, which " being ſtuffed with improper and equivocal " words, more apt to perplex than illuſtrate " the matter, precipitate into error thoſe, " whoſe clouded imagination has no right " notion of the functions of theſe parts. The " flowers, ſtrictly ſpeaking, are nothing elſe " but the organs which conſtitute the differ- " ent ſexes of plants, &c." I ſhall notice only two or three things concerning this diſcourſe. 1*mo*, That according to it, the *apices* of barren flowers ſhed their duſt all at once, by a kind of exploſion: but fertile flowers, ſlowly and by degrees, and commonly before they open, or expand their covers; but he gives only the *parietaria* for an inſtance.

2*do*, THAT he has demonſtrated that the duſt of the *apices* cannot enter the ſeeds; becauſe the *ſtylus* is not always hollow, but often ſolid: and although it were hollow or tubular into the ſeed-veſſel, it could not thus convey the duſt in the ſeeds, without penetrating their proper covers or ſhells. Beſides, he aſſerts, and that truly too, that this duſt

duſt does not enter even the cavity of the fruit; as may be ſeen by opening the heads of the *papaver orientale hirſutiſſimum flore magno, T. Cor.* p. 17. when the flower is fully blown. For, tho' the upper part of the heads, are then covered with the purple duſt of the *apices;* yet the ſeeds, partitions to which they adhere, and all within the fruit, continue perfectly white. I might add, that the duſt of the *apices* is ſometimes in ſo large grains, as to be viſible to the naked eye, as in ſome of the *malvaceae*: while no conduits are diſcoverable, by magnifying glaſſes, in the *ſtylus*, whoſe diameter does not much exceed that of the grains of the duſt, which reſembling prickly balls, muſt be very unfit to enter a ſtrait paſſage. And,

3*tio*, THAT the volatile ſpirit, or vapour of the duſt, may be conveyed to the ſeed, in this manner; it may enter the air veſſels of the *ſtylus*, paſs through them into the *placenta*, thence into the *funes umbilicales*, and ſo into the *ova* or ſeeds, along with the nutritive juices. But I cannot omit a remarkable paſſage in this diſcourſe, as it anſwers an aſſertion of Mr *Geoffroy*, and on other accounts which take in the tranſlator's words.

"Tubae

"Tubae igitur hae, quas fallopianis compa-
"ro, quod ad ova deferant, non exigua ipſa
"illa pulveris foecundi grana, quae apices ſu-
"per illas ejaculantur, aut in ipſarum excu-
"tiunt infudibulum, ut ſectatur *Leeuwen-*
"*hoekianorum* atque *Hartſoekerianorum* phan-
"taſmatum voluit; ſed halitum modo, aut
"ſpiritum volatilem, qui pulvere hoc ſe ex-
"pedit, ovaque ipſa foecundat. Credo enim
"auditores, perſuaſum certumque habendum,
"non materiam maſculinam, nec vermiculos
"ſuppoſititios, vel animalcula ſeminalia eſſe,
"quae impregnationem in femella abſolvant:
"quia *Malpighius*, narrante anatomico recen-
"te (z) agnovit foetum repiriri in ovis rana-
"rum et gallinarum ante copulam: ut et
"certiſſimum eſt germen adeſſe in feminibus
"plantarum quae non fuerunt impregnata,
"quorumque parenchyma facit cum germi-
"ne ipſo continuum corpus. Non poterit
"ideo eſſe aliud quid, preter volatilem hunc
"ſpiritum, cui craſſior materies vehiculi mo-
"do vicem praeſtat ſimplicis. Natura vero
"ſemper eaſdem ſectante leges, concludere
"oportet, id quod hac occaſione in anima-
"libus

(z) M. Dionis edit 1715. p. 392.

" libus contingit, idem et vegetantibus acci-
" dere (*a*)." Upon the whole, it is evident that Mr *Vaillant* aſſumes more the air of an original writer on this ſubject than belongs to him. He never mentions *Grew*, *Ray*, or *Camerarius*; and quotes only ſome paſſages from *Geoffroy*, for the pleaſure of confuting them.

15. I ſhall paſs a variety of later authors who have treated on this ſubject; and come to the moſt ſtrenuous defender of the ſexes of plants, who has collected all the arguments for it that perhaps can be advanced, and pretends to have demonſtrated it fully: I mean the famous and very learned *Carolus Linneus*, profeſſor of medicine and botany in the univerſity of *Upſal*, fellow of a great many philoſophical ſocieties; and certainly one of the greateſt botaniſts of this age. For this great man thus writes, " Antheras et ſtigmata (*b*) " conſtituere ſexum plantarum, a palmicolis, " *Millingtono*, *Grewio*, *Rayo*, *Camerario*, *Godofredo*, *Morlando*, *Vaillantio*, *Blairio*, *Juſſievio*, *Bradleyo*, *Royeno*, *Logano*, &c. detectum,

(*a*) Vid. p. 17.

(*b*) that is, the apices, and extremity of the ſtylus.

"detectum, descriptum, et pro infallibili assumptum: nec ullum, apertis oculis considerantem cujuscunque plantae flores, latere potest; quod demonstratum in *sponsalibus plantarum*, Upsaliae 1746, in 4to *(c)*." And elsewhere *(d)* "Generationem vegetabilium fieri, mediante pollinis antherarum illapsu supra stigmata nuda, quo rumpitur pollen, efflatque *auram seminalem*, quae absorbetur ab humore stigmatis; quod confirmat oculus, proportio, locus, tempus, pluviae, palmicolae, flores, nutantes, submersi, syngenesia; immo omnium florum genuina consideratio."

YET I cannot help thinking this doctrine not capable of demonstration, far less that the *genuina consideratio* of any flower can make it probable: *Camerarius* himself doubted of it; *Tournefort* disbelieved it; and *Pontedera* *(e)* uses many arguments to refute it. It remains therefore only, that the arguments for and against the sexes of plants, as understood by the moderns, especially the de-

 servedly

(*c*) Systema naturae, edit. Lipsiae 1748. in 8vo, p. 216.
(*d*) Philosoph. Botan. edit. Stockholm. 1751. in 8vo. p. 91.
(*e*) Anthol. l. 2. p. 107.—185.

ſervedly much commended *Carolus Linnaeus*, be fairly ſtated.

SECT. III.

16. THE ſtate of the controverſy is briefly this; Whether the influence of the duſt of the *apices*, is abſolutely neceſſary to the foecundity of the ſeeds, or not: or, Whether good and fertile ſeeds can be produced, when the duſt of the *apices* has no acceſs to the *ſtyli* or *ſtigmata* of the plants that carry them. Now, there being ſeveral ſpecies of vegetables, which bear flowers on one plant, and ſeeds on another, as *ſpinacia*, *mercurialis*, *cannibis*, &c.; I know no way, to determine the queſtion ſo certainly, as by training up one or more of theſe ſeed-bearing plants, at a ſufficient diſtance from ſuch as carry the flowers, and obſerving the conſequence: for,

17. IF a ſeed-bearing or female *ſpinacia*, *mercurialis*, or *cannabis*, ſtanding at a diſtance from any of the flowering or male plants, can produce fertile ſeeds; then the influence of their duſt is not neceſſary to the fertility of theſe ſeeds: but, "foemellam ſpi-"naciae, mercurialis, cannabis, abſque vici-

"ni

"ni maris contagio excultam, ſemina foe- "cunda producere vidit *Camerarius*;" *ergo* the duſt of the *apices* is not neceſſary to the foecundity of all ſeeds. Although this teſtimony of *Camerarius* is beyond all exception; yet, in confirmation of it, and to prevent cavilling, I ſhall relate the experiments I made with the ſame plants.

1. In the Spring 1737, I tranſplanted three ſets of the common *ſpinage*, long before it could be known, whether they were flowering or ſeed-bearing plants, from a little bed on which they were raiſed, into a place of the garden, full 80 yards diſtant, and almoſt directly South; there being two *hawthorn* and three *holly* hedges, all pretty thick and tall, between them and their ſeed-bed; and no other *ſpinage* in the garden, nor ſo near them by far: all the three proved fertile plants, and ripened plenty of ſeeds. I ſowed them, they grew, and proſpered as well as any *ſpinage* ſeed poſſibly could do. This, I own, made me, at firſt, call in queſtion the ſexes of plants, which I formerly too implicitly believed.

2. THE ſame year, a few plants of the common *hemp*, which I had raiſed for a ſpe-

cimen

cimen from the ſeed, being accidentally deſtroyed when very young; and finding afterwards, about the end of *June*, a pretty ſtrong but late plant of *hemp*, growing in the incloſure to the eaſt of *Holyrood-houſe*, commonly called the *Bowling-green*, by itſelf: I cauſed great care to be taken of it; there not being that year any *hemp* raiſed within a mile of it, that I could find. This plant grew luxuriantly; and, tho' bad weather in the Autumn made me pluck it up a little too ſoon, yet I got about thirty good ſeeds from it, which the ſucceeding Spring produced as thriving male and female plants, as if the mother *hemp* had ſtood ſurrounded with males. And,

3. In the Spring 1741, I carried two young ſeedling plants of the *French mercury*, long before there was any in flower, from the City Phyſic-garden, the only place where it was then to be found in this country, to the King's Garden at the Abbey, which are more than 700 yards diſtant from one another, with many high houſes, trees, hedges, and part of a hill between them: and planted one of them in one incloſure, where it was ſhaded from the ſun the greateſt part of the day; and the other, in another, 25 yards diſtant,

ſtant, expoſed to the South and Weſt. Both plants ripened fertile ſeeds; and the laſt ſhed them ſo plentifully, that it proved a troubleſome weed for ſeveral years: tho' none of the ſpecies was to be found in that garden, for more than twenty years preceeding.

HENCE the duſt of the *apices* of theſe three ſpecies of plants, cannot, on any account, be called their *genitura*. And the ſame may be ſaid of the *lupulus*, according to Mr *Tournefort*'s obſervation *(f)* of the *bryonia*, as noticed by Mr *Millar (g)*, yea of Mr *Geoffroy*'s *mays* mentioned above.

18. THE learned *Valentini* (*h*) attempts to remove *Camerarius*'s doubts two ways. 1. Perhaps, ſays he, there may be found latent *ſtyli* among the *apices* of the *equiſetum*: and 2. The wind may carry the *pollen ſeminale*, (or duſt of the *apices*) to as great a diſtance, as the *mercurialis*, *vel frumenti Turcici feminina* can be ſeparated from their proper males.

THAT the wind could have this effect, even on the *ſpinage* male duſt, is far from being probable: for, to ſay nothing of the globular

(*f*) Page 69.
(*g*) Gard. Dict. abridg.
(*h*) German. ephemer. l. c.

globular figure or ſpecific gravity of this duſt, a north wind only could drive it toward the females, which coming over the *Caltonhill*, muſt rather have driven it into the ground, or into the intervening hedges. And indeed this ſuppoſition is ſo evidently extravagant, that I find not any notice taken of it, by the moſt ſtrenuous defenders of the ſexual ſcheme.

19. BUT the other ſuppoſition of the latent *ſtyli*, *in equiſeto* &c. ſeems to have given occaſion to another as extraordinary, *viz.* of latent *ſtamina*, in *cannabis*, *lupuli*, &c. foemellis " Accidit interdum, ſays Mr *Wahlbom*, " or rather Mr *Linnaeus*, (*i*) ut cannabis ſe- " minifera, unum alterumve ferat florem " ſtaminiferum, quo nonnullae fæminae im- " pregnari poſſint; quod Camerarium lu- " ſit." Granting this ſhould happen ſometimes, and that theſe Gentlemen have ſeen it, (which, however, is not here aſſerted); yet I may ſay, that I believe they are the firſt that ever ſaw it, and that this is not enough to remove *Camerarius*'s doubt, nor to weaken the argument

(*i*) Amaen. Acad. vol. 1. edit. Lug. Bat. 2749. 8vo. p 99.

argument drawn from the above experiments, unleſs they can ſay, that the ſeed-bearing *hemp* carries always one or more ſuch ſtaminiferous flowers; or that the *cannabis* of *Camerarius*, and the other abovementioned, had really ſuch flowers in it, which they cannot aſſert. For my part, I can declare that tho' I ſearched very carefully for the ſeed of the *hemp* plant, (*k*) I could ſee nothing like *ſtamina* on it; neither on the ſeed-bearing plants of the *ſpinacia*, *mercurialis*, *bryonia*, either before or ſince I read Mr *Wahlbom*'s *Sponſalia Plantarum*.

20. Hence I think the poſition of that learned botaniſt *Julius Pontedera*, *viz.* "Apicum liquorem embryonis foecundationi "in omnibus plantis non eſſe neceſſarium," to be a demonſtrable truth, which neither authorities nor arguments can refute. However to clear up further this controverſy, I ſhall conſider the arguments advanced by the moſt famous ſexualiſts, as they are ſummed up by the very learned *Carolus Linnaeus*, in his *Fundamenta Botanica;* explained by *Jo. Geſnerus*

(*k*) Exp. 2.

nerus (*l*), and demonſtrated, they ſay, by *Jo. Wahlbom (m)*, and *Car. Linnaeus* himſelf *(n)*.

21. Generationem vegetabilium fieri, mediante pollinis antherarum, &c. ut ſupra, N° 15. *confirmat* (or, as in former editions, *dictitat*) 1. *oculus*, F. B. § 145. How the eye ſhews or confirms the generation of vegetables, to be thus effected, is explained three different ways. For,

1. "Ipſa outopſia (ſays *J. Geſner*) hunc "generationis actum edocet. Primus qui "florum connubia vidit, cel. D. Seb. Vaillant "(*o*) tam eleganter deſcribit, ut nemini de "hac re dubium ſupereſſe poſſit. Ita autem "cl. Vaillant, "Quoties acciderit, ut in ea- "dem ſtirpe flores gerantur ſimul, quorum "hi fœminina tantum, illi autem maſculina "et fœminina conjuncta organa cingunt, ar- "rectio tumorque organorum maſculinorum "in his tam ſubito contingit, ut lobuli gem- "mae

(*l*) Diſſertationes Phyſicae de Vegetabilibus, Lugd. Bat. 1743. 8vo conjunctum cum C. Linnaei oratione de neceſſitate peregrinationum. Explicant elementa botanica Linnaei. Lin. bib. vol. p. 174.

(*m*) In the Sponſalia Plantarum, Amaen. Acad. vol. 1. p. 61. ad 109.

(*n*) In Phil. Botan.

(*o*) In lib. de ſtructura florum.

"mæ floſculoſæ cedant illorum impetui, at-
"que hinc inde ſemet expandant, mirabili me-
"hercle velocitate, et extemplo quam vio-
"lentiſſime fœcundam explodant, omnem-
"que uno impetu ejaculentur genituram,
"diffuſa nimirum pulverulenta nubecula,
"ſpargente quaquaverſum foecundationem
"arvi genitalis. Vix venereus hic ludus ab-
"ſolutus eſt, quin illico florum labia, aut lo-
"buli, ad ſe invicem accedant eodem qui-
"dem, quo a ſe mutuo receſſerant, celeri-
"tatis impetu, veteremque ita formam ſta-
"tim renovent. Apparatum hunc artificio-
"ſum facili ſpectare datur in *parietaria*. Sed
"accedas opportet hora ſacra veneri, aurora
"eſt quae favet his congreſſibus; ubi vero
"agere renuunt ſatis opportune, aciculae a-
"pice, leniter modo, ſtimulus. In herma-
"phroditus, ubi duo ſexus conjuncti haben-
"tur, multum abeſt ut tanto impetu explo-
"datur. Plerique enim flores, praecipue
"nutantes, in quibus piſtillum obliquum
"intra ſtamina poſitum habet, actum gene-
"rationis floribus clauſis exercent, dum ſtig-
"ma adhuc intra medias antheras contine-
"tur." Thus Mr *Geſner*. (*p*)

(*p*) Diſſ. p. 86.

22. ALLOW me here to obſerve, in the firſt place that this artificial *apparatus* (*q*) was ſeen and deſcribed by *John Bauhinus*, long before *Vaillant* was born (*r*), and tranſcribed both by *Moriſon* and *Ray* in their hiſtories of plants, tho' *Vaillant* mentions none of them: and, ſecondly, that this ſeems to afford a ſtrong argument againſt the impregnating virtue of the duſt of the *apices*. For, according to *Linneus* himſelf, "parietariae *hermaphroditi*, "flores duo continentur involucro plano hex-"aphyllo. Calyx monophyllus, magnitudi-"ne involucri dimidiati. Stamina filamenta "quartuor calyce longiora, illumque expan-"dentia. *Faemineus* flos unus, inter herma-"phroditos ambos, intra involucrum; cum "calyce ut hermaphroditi (*ſ*)." Now, ſince the ſtamina of the harmaphrodite flowers, as well as the *germen* of the female flower betwixt

(*q*) Tout. cette mechanique, VAILL.

(*r*) "Parieteriae floſculi conferti circa caulem ex foliorum "alis, flocos coccinei ſerici imitantur, primulum e nodulo "emicantes: poſt ſe ſtamina oſtendunt obſcure, ex albo pur-"puraſcentibus apiculis, involu'a, quae ſi ſtylo evolvere co-"neris, ſubſultum excuſſo pulvere cum impetu, ſpectaculo "jucundo, ſe expandunt repanda, in medio ſeminis rudimen-"tum circundantia." I. B. 2. p. 975.

(*ſ*) Gen. pl. p. 494.

betwixt them, are contained, in one common *involucrum*, and the *apices* do not eject their duſt, until, by the ſtraitning, or ſtretching out of the formerly crooked *ſtamina*, the common *involucrum* is burſt open, and the *apices* raiſed in the air; ſince no ſooner do they find themſelves free, and erected high enough above the *involucrum*, than immediately, by a ſort of exploſion, all the duſt is entirely, with great violence, thrown out; and ſince the leaves of the *involucrum* cloſe up again, as quickly, and with as much force as they ſeparated, and reſume their former figure (*t*); does not all this ſhew, that the duſt is not by nature deſigned to impregnate the ſeed, but to be carried away by the winds, leſt it ſhould infect it, by falling on the *ſtigmata*; and that *autopſia* teaches the quite contrary to what Mr *Geſner* alledges.

23. "ITA

(*t*) "Ita quidem, (adds, Mr VAILLANT), ut difficillimum "foret credere, flores hoſce ullam vim paſſos eſſe, niſi vel ipſe "actum hunc vidiſſet oculus, vel adhuc cerneret caduca ſcele"ta magnanimorum heroum, aliquamdiu erecta in campo "conflictus, ubi apluſtrium inſtar, jocularios experiuntur luſus "volitantis zephyri."

23. "ITA sese in plantis habere, (says "M. *Wahlbom*) dictitat primo oculus. Flo-"re florescente, et polline antherarum vo-"litante, quod stigmati pollen inhæreat, pri-"ma fronte obvium est (*u*)." But it is not at all obvious, far less does the eye see, that generation is thus accomplished; for this dust as often bespatters the *petala*, &c. Neither does what he adds concerning the florescentia *violæ tricoloris*, *gratiolae*, *iridis*, *campunalae et syngenesiarum*, in the least favour him. For, tho' all he says were true, these make but a very inconsiderable part of the vegetable kingdom. Yet they seem rather to make against him. For in these compound flowers, which he calls *syngenesiae*, the "*antherae* sunt lineares, erectae, lateribus "coalitae in cylindrum tubulatum. *Stylus* "filiformis erectus staminum longitudine, "antherarum cylindrum perforans. *stigma* "bipartitum, laciniis revolutis, patentibus." *Lin. Gen. pl.* p. 370. To which if we add, that these *antherae* split, and emit their dust, on the outside, not inside, of this *cylinder*, while the *stigma* is commonly pretty far above

(*u*) Amæn Acad. 1. p. 90.

bove it; what can reaſonably be inferred from this ſtructure, but that Nature deſigned this duſt ſhould be thrown away as uſeleſs, if not hurtful to the *ſtylus?*

AGAIN, the *ſtylus* of the *campanula* is commonly much longer than the *ſtamina*, and briſtly a little above them, at it were to hinder their acceſs to the *ſtigma*. The *ſtamina* of the *iris* are hid under the outſide of the ſegments of the *ſtigma*, the depreſſed *petala* being frequently bearded or hairy under them. So no plants could be here more improperly alledged; for by the ſtructure of their flowers, it is evident, that the *pollinis illapſus ſupra ſtigmata nuda* is impoſſible: nor can Mr *Wahlbom* deny it; " *campanula*, (ſays " he) a caeteris in eo differt, quod pulvis la" teri hiſpidi ſtyli adfigatur, et exinde per " certos canales ſtigmati communicetur. *I*" *ris* particularem nobis oſtendit ſtructuram; " ſtigmata enim ſeſe dilatantia, antheras om" nino operiunt; illum tamen ad petala re" ferunt ſitum, ut, aura ſubeunte ſtigmata, " pollen per rimas illorum aſcendat." What eye ever ſaw theſe *canales*, or *rimae*, with the *pollen* riſing in them? how, or why riſe to the *ſtigma* thus ſituated?

As for his obſervations on the *viola tricolor*, I paſs them, becauſe nothing to the purpoſe; and alſo too ſmutty for *Britiſh* ears. But let us hear what the great *Linnaeus* by himſelf ſays:

24. "GENERATIONEM vegetabilium fieri, " &c. (ſays he) confirmat *oculus*. Pollinem " intrare germina credidit *Morilandus*; ejuſ- " dem eſſentiam extrahi, mediante ſtigmate " madido, ſtatuit *Vaillant*: pollinem *aceris* " rumpi in humore vidit *Bern. Juſſiaeus*: om- " nem pollinem in humore explodere auram " ſeminalem, confirmat *Needham* (*x*)." But *Morland*'s opinion is fully confuted by *Vaillant*, who maintains only that the vapour, or vola- tile ſpirit of the male duſt, enters the *trachiae* of the *ſtylus*; but not a word can I find in his *diſcourſe*, concerning the attraction of the eſſence of the duſt by means of the moiſt *ſtigma*; and, altho' it ſhould all burſt in moiſture I do not ſee how it thence follows, that it thus ſends out an *aura ſeminalis*; e- ſpecially, ſince it does not appear, that the *ſtigmata* muſt neceſſarily be moiſt in or- der to fertility: and it is obſerved by Mr *Wahlbom*

(*x*) Phil. Bot. p. 91.

Wahlbom (y), That, "in omnibus fere floribus conſpicitur, quomodo, aëre humido, floſculos complicant, ne aqua pollen attingat." By no means, therefore, can the eye be ſaid to confirm the ſexual ſcheme.

25. THE *ſecond* argument for this ſcheme is taken from the proportion which the *antherae* bear to the *ſtylus*.

"Ex proportione quoque, (ſays *Geſnerus*) (z), veroſimiliter judicamus, cum, pro magnitudine et numero ſeminum, ipſa quoque ſtamina majora ſint vel numeroſiora." But that this is evidently a miſtake, will appear to any one that will take the pains, to compare *Linnaei monandriae* with his *polyandriae*; or the *cannacorus* with the *prunus*, and other ſtoned fruits: the *ſyngeneſiae* have five *ſtamina* for one ſeed, and that, not ſeldom a very ſmall one: the *umbelliferae*, as many for two ſeeds, &c.

"ITA ſeſe in plantis habere dictitat ſecunda *proportio*: plerumque ſtamina et piſtilla eandem ferunt altitudinem, ut eo melius ad ſtigma pollen, mediante vento, accedat; "in

(y) Amaen Acad. 1. p. 93.
(z) Diſſert. p. 91.

"in quibuſdam vero non, ubi ſingularis ob-
"ſervatur proceſſus foecundationis (*a*)." But *plerumque* dictitat nihil, eſpecially ſince there are a great many *genera*, where no ſuch proportion or ſingular proceſs takes place, as in almoſt all the *floſculoſi*, *ſemifloſculoſi*, *radiati*, *liliacei*, *caryophyllæi*, &c. and of the ſix plants inſtanced as ſingular, in three, *viz.* *dianthus*, *nigella*, and *paſſiflora;* the *piſtilla*, any curvature notwithſtanding, continue high above theſe *ſtamina* (*b*.)

THIS argument is thus explained by *Linnæus.* "*Proportio*: ſtigmata ſeſe flectere ad "antheras, dein exſeri ex *diantho, paſſiflora,* "*nigella, patet.* Piſtillum ubi breviſſimum, "connivent antherae ſupra ſtigmata: *ſaxi-* "*fraga, parnaſſia.* Connivent dum efflant pol- "linem antherae in *celoſia.* Comprimit corolla "digitis antheras ad ſtigmata in *Teucrio* (*c*)." Here not a word of proportion confirming his ſcheme; no inſtance of it; but only reaſons for inequality. No matter, therefore, whether it be ſo in theſe or not.

26. THE

(*a*) WAHLBOM, Amaen. acad. 1. p. 90.

(*b*) See Pontedera for more inſtances. Anthol. l. 2. c. 8.

(*c*) Phil. Bot. p. 91.)

26. The *third* arg[t] is, " Ex *loco* ſtaminum " et piſtilli, non leve argumentum petere poſ- " ſumus; nam in pleriſque plantis, floribus " hermaphroditicis preditis, ſtamina ambiunt " ovarium, et ea ratione ut maxima pars ge- " niturae ad ſtigma accedat." *Geſnerus (c)*. Can the *ſtamina* ſurround the *ovarium*, in the *monandriae*, *diandriae*, &c.? but this needs no anſwer; neither what he adds concerning the *piſtilli fabrica*, and *antherarum materies*, which is nothing to the purpoſe.

" Tertio, *locus*. Etenim ſtamina plerumque " piſtillum ambiunt, ut ventorum ope ſemper " quidquam pulveris attingat ſtigma." *Wahlbom (d)*. But the learned author cannot but know, that the *ſtamina* ſtanding round the *piſtillum* can never prove the neceſſity of the duſt's falling on the *ſtigma* of every plant, in order to its foecundity; eſpecially when this is not always their ſituation. He adds, " Mo- " neciae flores maſculi *plerumque* ſupra flo- " res foemineos collocantur, ut pollen eo me- " lius in piſtillum decidat;" and inſtances *ricinus* among others: but, ſure I am, the *ricinus vulgaris* B. p. has all its female flowers

H h above

(*c*) Diſſert. p. 9.

(*d*) Amaen. Acad. 1. p. 91.

above the male flowers; the *ſtigma* of the uppermoſt being commonly ſome inches above the neareſt *ſtamina*.

" Locus. Nunquam piſtilliferae ſponte " naſcuntur ſine ſtaminiferis in eadem terra; " prodeunt ex eodem ſeminae ambae." *Linnaeus (e)*. But this cannot be proven. Yea, *Camerarius* aſſerts the contrary *(f)*. But, granting it true, it proves nothing, confirms nothing.

27. Argument 4th, " Ex *tempore* quoque " vegetationis harum partium concludere " datum eſt: namque in antheris, ſtamina " farinam foecundantem eo tempore conti- " nent, quo piſtillum viget, deinde, excuſſo " pulvere peracta foecundatione, perit ſtamen; " ſuccus copioſior in piſtillum devolutus, ef- " ficit ut fructum matureſcat," *Geſnerus (g)*. But the *ſtamina ſpinaciae, mercurialis, cannabis, mays, juniperi, violae martiae*, &c. ſhed their duſt commonly before their *ſtigmata* are viſible.

" Quarto *tempus*. Primum hic atten- " dendum venit quod ſtamina et piſtilla una " proveniant,

(*e*) Phil. Bot. p. 91.
(*f*) See No. 10. ſupra.
(*g*) Diſſert. p. 91.

"proveniant, exceptis tantum pauciſſimis. "Alterum, quod ubicunque flores maſculi "diſtinctis a foemininis gaudent thalamis, aut "in eadem aut diverſa planta, et *ubi maſculi* "*flores nec perpendiculariter ſupra foemineos* "*erecti ſunt*; ibi, floreſcentia ante foliorum "exortum peragatur, neceſſe eſt; ne, foliis "intervenientibus, inhibeatur foecundatio: "ex. gr. in *moro, viſco, mercuriali perenni*, &c." *Wahlbom* (*h*). How juſtly theſe three are inſtanced, any body may judge. But ſince he admits of exceptions, as to the firſt *notandum*; and many other plants emit the flowers before the leaves; this argument proves nothing.

"TEMPUS. In declinis flores ante germi-"nationem foliorum *plerumque* prodeunt, ne "folia tegant piſtilla; *ſalix, populus, cory-*"*lus*, &c." *Linnaeus* (*i*). Here, as commonly, we find *plerumque*; and an imaginary reaſon.

28. Argument 5th, *viz. pluviae*; "Fit in-"de ut, dum *pluvia* eluit pulverem ſtaminum, "germina piſtilli decidant, aut in fructus "naſcantur abortivos, ut ſtillant vites, perco-

"quantur

(*h*) Amaen. Acad. I. p. 93.

(*i*) Phil. Bot. p. 91.

"quantur, maraſmo exareſcant, locum con-"cedant inſectorum nidis et eorum evoluti-"oni, ut fruges uſtilagine pereant, et quae "ſunt alia vitia. Sedulo itaque Natura ubique "ſolicita fuit, ut genitura illibata ad piſtillum "perveniret," *Geſnerus* (*k*). But how does it appear that ſuch are the conſequences, of the duſt's being waſhed away by rain? Has too much moiſture no bad effects, after the *ſtamina* have ſhed their duſts? Does it not frequently rot the plants? &c.

"Quinto *pluviae*. IN omnibus *fere* flori-"bus conſpicitur, quomodo, urente ſole, ſeſe "expandant, veſpertino vero tempore, et "aëre humido, floſculos complicent, ne "aqua pollen antherarum attingat et coagu-"let, quo facto, ad ſtigmata efflari nequeat; "at ſtigmate, mirum ſane! ſemel foecunda-"to, nec veſpere, nec pluvia ingruente, "ſeſe contrahant flores," *Wahlbom* (*l*). But our author knows, that many plants cloſe their leaves in the night or in rain; as the *acaciae*, *mimoſae*, &c. that many open their flowers in the night, and ſhut them when

(*k*) Differt. p. 91.
(*l*) Amaen Acad. 1. p. 93.

when the ſun is hot; as ſome *cerei*, *ketmiae xyla*, *lychnis noctiflora*, *mirabilis peruviana*, &c. *(m)* Does not the *paſſion-flower* keep open in the night as well as day, until it ſhut up for good and all, and that whether it be ſun-ſhine or rain? He adds, "Secale florens "antheras filamentis inſidentes exſerit, quo "tempore, ſi pluvia cadit, pollen conglobatur, hincque annonam difficilem auguratur "agricola, nec immerito; grana enim im-"minuuntnr exinde, quod plerique floſculi "abortum paſſi ſint." That *ſecale*, *triticum*, many *gramina*, *plantagines*, *pimpinellae*, &c. thruſt out the *apices* on pretty long *ſtamina*, when in flower, I deny not; but that rain at that time cauſes ſcarcity of any of them, I never obſerved: and altho' it were granted, that this commonly happens, how does it appear that rain cauſes the *floſculi* to abort, or this abortion cauſes ſcarcity of *ſecale*? ſince, at the ſame time, the other plants are ſufficiently fertile, yea the manner of flowering in ſome of theſe plants, ſeems to afford an argument, not contemptible, againſt the ſexual

(*m*) "Mirabilis eſt planta quae tam ſpecioſos flores, nocti "atrae objicit, et ſereno diei ſubtrahit." Lin. H. Cliff. p. 54.

ual ſcheme. For in *ſecale* and *triticum* too, there are three *ſtamina* for one *germen*, both included within, and well covered by the ſame huſks (be they a *corolla* or *calyx*, or both, no matter here) before the flowering time. Now if the duſt be neceſſary to the impregnation of the ſeed, and, in order to this, it muſt fall on the *ſtigma*; Why is not this accompliſhed, before the covers open? Why are the *ſtamina* ſo long? Why do they thruſt out the *apices* ſo far from their *ſtigmata*, and throw out, if not all, at leaſt the greateſt part of their duſt, in the common air? Does not all this look as if this duſt, was not deſigned by Nature for the impregnation of the ſeed? as is often noticed.

" *Pluviae* (ſays *Linnaeus*) *(n)* combibunt " pollen, ut in ſtigmata cadere nequeat; " hortulanis notiſſimum in drupiferis, (ſo " he calls the *amygdalus*, *perſica*, *prunus*, " *armeniaca*, *ceraſus*, *lauroceraſus*, &c.) et " pomiferis. Agricolis deteſtabilis in agris " ſecalinis, (why not alſo *triticeis?) Fu-* " *mus* idem etiam facit, abſorbendo humi- " dum ſtigmatis." So the *ſtigmata* muſt neither be wetted nor dried. But this is fully anſwered

(*n*) Phil. Bot. p. 91.

answered above. *Non causa pro causa* is here very frequent.

29. THE *sixth* argument, which the Sexualists triumph in, as a demonstration of their doctrine, is taken from the culture of the *palm-tree*. "Instar omnium argumento-"rum esse potest (says *J. Gesnerus*) *(o)* mo-"dus quo foecundatio palmae dactyliferae, "ad obtinendos dactylorum fructus maturos, "apud Persas instituitur, a cl. Kaempfero, "in his locis, annotatus." It would be too long to transcribe all that modern authors have said concerning this tree. The reader may, if he pleases, consult *John Leo (p)*, *Prosper Alpinus (q)*, *Hadgi Mustapha Aga (r)*, *Engelbertus Kaempfer (s)*, Pere *Labat (t)*, *Christ. Got. Ludwig. (u)*, *&c.*

30. THESE authors are agreed only in this, that the date-bearing *palm-tree* has no flowers: and unless the flowering or male *palm-tree* be sufficiently near, or the dust of its *apices* be some

(*o*) Differt. p. 85.

(*p*) Harris collect. vol. 1. p. 347.

(*q*) De pl. Aeg p. 24.

(*r*) T. Just. p. 69.

(*s*) Amen. exot. p. 706.

(*t*) Voyage aux isles de L'Amerique, Hague edit. vol. 1. part. 2. p. 209.

(*u*) In J. Gesner. diss. p. 86.

ſome how conveyed into the *ſpatha* of the female; its date ſtones will not grow. I ſhall paſs the very wide differences among them, as to the manner of conveying it; and only obſerve, that without the influence of the male, the female will either be barren, or caſt her unripe fruit, according to the antients, and *Alpinus*; that the dates will want ſtones, be harſh, and not eatable, except by camels and cattle, *Hadgi*. "Omnia ſua fru-"ctuum rudimenta, indeclinabili abortu di-"mittunt," *Kaempfer*. "In fructa pulpae lo-"co adeſt cortex durior, ſiccus, adſtringens, "oſſiculum vel nullum vel tenue," *Ludwig*. which are all contradicted by *Labat*.

31. "IT is pretended, (ſays that reverend father,) that the *date-tree* is male and female; that the male bears bloſſoms, but no fruit, that being left to the care of the female; but that ſhe would carry none, if ſhe had not the male by her, or at leaſt within ſight of her." I am ſorry that I cannot ſubſcribe to this opinion of the naturaliſts, but it is a moſt certain experiment, directly oppoſite to their ſentiments, that hinders me; for we have a *date-tree* beſide our monaſtery in *Martinico*, which carries fruit, tho' ſingle: whether is is male or

or female, I know not; but this I know for certain, that there was not another of the kind within two leagues of it. Whence we may conclude, that the preſence of the male is not neceſſary to render this tree fruitful, as naturaliſts pretend." He ſays indeed alſo, that the ſtones of the dates of the iſlands will not grow; ſo that thoſe who would raiſe *palm-trees*, are obliged to plant the *Barbary* dates; and that dates do not ripen ſo perfectly in *Martinico* and *Guadaloupe*, as they do in *Africa*, in *Aſia*, or even in *St. Domingo*." For altho' our dates, ſays he, become ſoft, yellow, and as it were luſcious, and in a word, appear perfectly ripe; yet they ſtill retain a certain ſharpneſs, which ſhews they want at leaſt ſome degree of maturity." Thus *Pere Labat*. l. c.

Jo. Bauhinus (*u*) relates, that he ſaw only one date-bearing *palm-tree* at *Montpelier*; "Centeſimum annum ſuperare creditur, (ſays "he) et vulgo ibi perſuaſum, ante grandem "aetatem fructum non proferre, et vix ante "quinquageſimum annum, ut quidem per-"hibebant." Hence *Pontanus*'s fiction is eaſily accounted for, if there be any truth in it;

 and

(*u*) Hiſt. I. p. 360.

and perhaps ſome others, particularly concerning the amours of theſe trees; for which ſee *Pliny* (*x*), and *Caſſianus Baſſus* (*y*).

39. BUT I cannot omit the opinion of *Herodotus*, the moſt ancient author, who has left any thing concerning the uſefulneſs of the male, to the female *palm-tree*; "In *Babylonis* agris, ſays he, *palmae* magna ex parte "fructiferae proveniunt; ex quibus non ſolum "cibum, vinum, et mel conficiunt, ſed etiam eodem modo quo *ficus* curantur. *Palmarum* enim, quas *Graeci* maſculus vocant, "fructus *palmis* glandiferis alligant, ut earum "fructum maturet *culex* ſubiens, ne ex arbore is defluat. Ferunt enim *palmarum* mares (*z*) culices in fructu, quemadmodum "caprifici." Thus *Herodot*, as rendered by *Bod. a Stapel* (*a*). Who elſewhere (*b*) quotes a famous traveller for ſuch a culture of this tree, as confirms *Herodot*'s opinion (*c*).

AGAIN

(*x*) L. 13. c. 4.

(*y*) In Theoph. p. 103.

(*z*) ψῆνα.

(*a*) In THEOPH. p. 115.

(*b*) P. 103.

(*c*) "Agricolae etiamnum, ut refert doctiſſimus GUILLAND, in *Arabia, Ægypto, Meſopotamia, Iudaea, Phoenicia*, et "tota *Syria*, volentes cavere ne foeminae, aut ſterilitatis nox-

" am

AGAIN *Joan. Veslingius,* who was long in *Egypt*, differs not a little from *Alpinus*, as to the culture of *palm-trees* there: " Causa tam " foecundi proventus (says he) in telluris ha- " bitudinem referenda est, arenosam scilicet " et salsam, plantae huic gratissimam. Vidi " his locis in palmarum veluti sylvis, terram " copiosissimi nitri calida nive late consper- " sam, vel aquis Nili, vel roscida noctium " humiditate fervidiore sole perustis. Nec " opus hic *maritali cinere* palmae effoeminatae " vigorem incitare. Flantibus enim ab au- " stro per Æthiopiam, et steriles Arabiae de- " sertae campos urentibus, ventis; ingens ni- " trosi pulveris sublata vis, abunde cacumi- na

" am incurrant, aut fructum, quem aliquando imaginatione " libidinis, ut ita dicam, concipere solent, ante maturitatem " amittant, ita ordinant utriusque sexus palmas, ut mares eo " saltem intervallo a foeminis distent, quo pulvis, ventorum " flatibus a foliis mascularum sublatus, in foeminarum folia " incidat; idque satis ad foecunditatem, et fructus matura- " tionem facere compertum est. Sed mirum dictu! quod si " qua procul a mare abstiterit, ut neque pulvis, neque aura, " odorve ejus, ad eam permeare possit, excogitaverunt colo- " ni *funem a mare religatam ad fœminam usque producere;* at- " que ita quasi maritali vinculo copulatam, masculi virtute " clanculum per funem irrepente, foecundam fieri, quae prius " in ea sterilescebat solitudine." It is easy to conceive how insects may creep along a rope, but not how a powder or dust can be thus conveyed from one tree to another.

"na palmarum vegetat.—Meminique ſic o-"
"nuſtum fuiſſe dactylis ſuis unicum race-"
"mum, ut eum attollere a terra prae pondere"
"vix ſuſtinerem (*d*)." And *Alpinus* himſelf is obliged to own, that the fruitfulneſs of the *palm-trees* in the deſerts of *Arabia*, is not owing to any artificial culture, but to the winds carrying the duſt and flowers of the male to the female (*e*). Is it probable, that nature has left the fertiliſing of ſo uſeful a tree, to the uncertain motions of the air?

Besides, that great and curious botaniſt *Tournefort*, after mentioning the opinion of *Theophraſtus Alpinus*, and others, concerning the male *palm-trees*, adds, "Cum in Hiſpania "Baetica, palmarum feraci, a prudentioribus "viris de hac re ſciſcitarer, certum nihil ac-"cipere potui. De *lupulo* certius loquor. In "Horto Regio Pariſienſi, luxuriat fructibus "quotannis onuſtus. Qui vero floribus gau-"det, non occurrit niſi in inſulis Sequanae et "Matronae, longe diſtantibus: in Horto Re-"gio tamen ſemina profert" (*f*).

This

(*d*) Veſling in Alpin. c. 7. p. 11.
(*e*) V. Alpin. de plant Æg. p. 25.
(*f*) Inſt. p. 69.

THIS objection againſt the ſexes of plants drawn from the *lupulus*, appears to be ſtrengthened by the anſwer made to it; which is this, " *Humulus* duplex omnino eſt; " unus floribus ſuperbit ſtaminiferis, alter pi- " tilliferis; idque quod *fructum* vulgo vocant, " eſt *calyx* tantum explicatus et elongatus: " hinc *humulus*, quamvis foemina, nec foe- " cundata, conos taman proferre valet. Hoc " Tournefortium decepit, ne ſexum plantarum " agnoſceret, quum *lupulus* (foemina) in hor- " to Pariſienſi luxuriabat, fructibus quotannis " onuſtus; qui vero floribus gaudebat (mas) " non occurrebat niſi in inſulis Matronae et " Sequanae multum diſtantibus (*g*). Idem " fit in *moro* et *blito*, cujus baccae calyces ſunt " ſucculenti; minime paricarpia, ſeu ova- " ria" (*h*).

34. FOR I am at a loſs to find wherein *Tournefort* was deceived. He gives an accurate deſcription, as well as elegant figures, of the parts of the flower and fruit, as ſtanding on different plants (*i*), without which the character of the *humulus*, in *Linnaei Genera plantarum*

(*g*) Tournefort, Hag. p. 69.
(*h*) Amean. acad. 1. p. 99.
(*i*) Vid. T. p. 535 t. 309.

plantarum (*k*) is not very intelligible to a beginner. But he calls the enlarged *calyx*, *fructum*: the cones he calls *fructus*; and so they are in the most proper sense of the word. *Linnaeus* himself in his *Fundamenta bot.* teaches, that "essentia fructus in semine "consistit"(*l*); and in his *Philosophia Botanica* (*m*), "fructus ex semine sive pericarpio, "sive non tectum sit, dignoscitur." No matter therefore, whether ye call these cones *calyces elongatos*, or *fructus*, if they contain seeds: and *Tournefort* expressly adds, "In "horto regio *semen* profert;" which Mr *Wahlbom* is pleased to omit, for what reason I shall not say. As for the *morus* and *blitum*, I see not why their *baccae succulentae* may not be called *fructus* also; especially since *Linnaeus* (*n*) gives *blitum* a *pericarpium*; and describes a *periparpium* to be "viscus gravidum "seminibus, quae matura dimittit"(*o*): and consequently *Wahlbom* and he don't well agree. But, to return to the *palm-tree*,

35. SEXTO,

(*k*) P. 477.

(*l*) § 88.

(*m*) P. 56.

(*n*) Gen. pl. p. 5.

(*o*) Phil. Bot. p. 53.

35. "SEXTO, *Palmicolas* palmarum ſpadi-
"ces maſculos divellere, eoſdemque ſupra
"foeminas collocare, memoriae mandarunt,
"*Theophraſtus*, *Plinius*, *Alpinus Tournefor-*
"*tius*, *Kaempferus*, alii: quo neglecto da-
"ctyli acerbi, et nucibus deſtitutae, fiunt,"
Wahlbom (*p*). This is anſwered above (N° 31): then the author gives a long paragraph out of *Kaempfer* (*q*); as does *Geſnerus* (*r*), from the ſame page; yet they differ widely. I have not at preſented *Kaempfer* by me; but may be allowed to notice one thing, not very credible in each, according to their principles, *viz.* "Nemus foecundari univerſum poteſt ab una "phoenice florida," *Geſner.* "Singulare quod "ſpadices exſiccati ad thalamos apti ſunt, et "in annum poſterum, ſalva virtute, aſſervari "poſſunt," *Wahlbom.*

36. "PALMICOLÆ: notiſſima *Theophraſto* "*Plinio*, *Kaempfero*, aliiſque. *Piſtaciae* cultu- "ra in Archipelago; *Tournefortius*. *Caprifi-* "*catio* veterum, et adhuc in Archipelago, "per inſecta." *Vid.* diſſert. noſtr. de ficu, *Linnaeus*

(*p*) Amaen, Acad. I. p. 94.
(*q*) Amaen. p. 706.
(*r*) Diſſert. p. 85.

Linnaeus (*t*). Neither *Theophrastus*, *Pliny*, nor *Kaempfer*, relate the culture of *palm-trees*, as from their proper knowledge; and are contradicted by an eye witness, *Pere Labat*.

As for the culture of *pistacia* in the *Archipelago*, for which *Tournefort* is alledged, I cannot find any such thing in his works. Such a culture, indeed, is mentioned by the editor of M. *Geoffroy*'s *Materia Medica* (*u*), as practised in *Sicily*: but he neither says he was in that island himself, nor tells us who informed him. However, if such is the culture of the *pistacia*, either in *Sicily* or the *Archipelago*, it seems to be very modern, and founded on the imaginary impregnating virtue of the dust of the *apices*; and no doubt, the *terebinthus* will soon be treated the same way, it can do no harm; tho' it is not very probable, that *deficiente tali impregnatione fructus abortiant* in this tree, more than in the *palm-tree*.

37. CONCERNING *caprification*, if you consult *Theophrastus* (*x*), *Pliny* (*y*), and *Tournefort*

(*t*) Phil. bot. p. 92.
(*u*) Tom. 2. p. 417.
(*x*) De caus. pl. l. 2. c. 12.
(*y*) l. 15. c. 19.

fort (z), or *Pontedera (a)* only, who quotes all the three, together with the learned Dissertation our author refers to, which I take to be *Cornelii Hegardt Historia naturalis et medica ficus (b)*; it will be easy to judge, whether it is at all probable, that the fig insects carry the dust of the *caprificus* to the unripe figs, and thus impregnate the seeds *(c)*.

(*z*) Voy. let 8.

(*a*) Anthol. l. 2. c. 33. 34. 35.

(*b*) In C. LINNAEI, Amaen. Acad. vol. 1. p. 213. 243.

(*c*) " Cupido ficus nobis dicitur, quem antiqui *psenem* seu " *insectum* vocarunt *ficarium*, et PONTEDERA, Anthol. 172. " descripsit, estque species ichneumonis. Hisce ichneumo- " nibus jam mutatis, alisque instructis, tempus adest, quo " caprificus, seu ficus mas, florescit, h. e. farinam edit anthe- " rarum; tunc ichneumones e caprifici cavitatibus, farina, " *molitoris* instar e mola sua prodeuntis, obducti evolant, et " conjugibus acquisitis de ovis pariendis soliciti sunt: hinc " ad singulos grossos transvolantes, cavitates ficus foeminae, " dolii instar, clavis ferreis vel spiculis seu pistillis ab omnibus " lateribus intus completas, intrando, non possunt non fari- " nam illam, qua contecti sunt, excutere. Patet igitur hoc " modo, *ficum* hanc foeminam facillime impregnari." Thus Mr HEGARDT, Amaen. Acad. 1. p. 231. A fine appendix for PONTANUS's poem! But,

ACCORDING to PONTEDERA, " Ficarii culices, forma " ad vespas, nascendi autem modo ad muscas, accedere mihi " videntur. Ut primum grossi grandiusculi facti apertum fun- " dum ostendunt, ingrediuntur foemellae, et ex infimo ven- " tre producta tuba, foeturae in frumentis nidum excavant, et " ovula

38. If it be ſtill alledged, that the infoecundity of the date ſtones of *Martinico* demonſtrates the foecundating virtue of the male duſt; I anſwer, by no means: for *Pere Labat*

" ovula deponunt. Naſcuntur in his vermiculi, qui deinde " nymphae evadunt immobiles, durae, corpore oblongo, ca- " pite cum dorſo luteolo, caetera primum albae mox nigrae. " Perforato deinde nido, exit animalculum, plerumque non- " dum pinnis explicatis. Egreſſum ſtatim ſenectam deponita " capite incipiens. Tunc flavum cernitur, quod tamen " mox exſiccatum nigrum evadit. Dum vero involucris " ſpoliatur, huc et illuc revolvitur; et propterea apicum " pulviſculo, quo tota pomorum cavitas repleta eſt, infarci- " tur, quippe molliuſculum. Quare e groſſis egreſſum, et " ſole exſiccatum, pulverem diſcutit ad hunc modum: " ſtans quatuor anterioribus pedibus innititur, et duobus po- " ſtremis abdomen, lumbos, pinnas pulvere mundat, iterum " atque iterum cruribus detergens; deinde quatuor poſteri- " oribus ſeſe librans, duobus anterioribus caput, dorſum et " cornua purgat. Quemadmodum feles et alia elegantiora " animalia ſolent. Tunc, depoſito onere, evolat." Anthol. p. 174. And p. 175, he adds, " Hujuſmodi animalcu- " lis tota groſſorum caro et frumenta corrumpuntur. In ſativae " vero ficus pomis, haec animalcula nunquam inveni: num " vero ad haec volent ignoro; ſedulo hoc mihi inquirenti, " nullum ſane in ſativis ficubus apparuit. Neque enim Ita- " lia caprificationis indiget, ſed ſine groſſis ſativae ficus ſua " coquunt poma. In Graecia hoc culturae opus perpetuum " non eſt, ſerotina poma non caprificantur; neque praeco- " ciorum in macro ſolo, et in aquilonio, in ipſa Graecia ' ulla fit caprificatio. Pomum caprificatum bonitate inferius " eſt non caprificato et inſuavius. Hinc illi qui in Graecia " ficus *bat* does not ſay that he tried them; but in general, that the date ſtones of the *French* iſlands would not grow; and, it can ſcarce be ſuppoſed there were no males among all their *palm-trees*. Beſides many things concerning *palm-trees*, which appeared to be as well atteſted, are found to be vulgar errors. But, granting that they really are barren; this may be owing to the climate, to the ſoil, to bad culture, or to the want of *Pontedera*'s *culices*, rather than of the male duſt, for any thing yet appears.

39. ARGU-

" ficus venundabant, quo facilius emptores allicerent, συκα ά- " νερίνάςα iterum atque iterum clamitare ſolebant." And, after explaining the uſe of caprification, he adds, " Quare " concludendum, caprificationem in Graecia ob externas " cauſas eſſe neceſſariam, nequaquam ob ficus naturam, cum " alibi poma coquant non caprificata. Eadem etiam de " cauſa *palmas* in quibuſdam regionibus eſſe caprificandas, " in aliis minime; id autem per culices fieri, non vero per " affectionem, quam apices embryonibus communicent, ſatis " demonſtratum eſt." See Anthol. l. 2. c. 34, and 35. p. 172, &c. Now ſince M. HEGARDT's *Cupid*, or *Miller* does not go abroad, till well bruſhed, and freed of the duſt; ſince there is no caprification in *Italy*; and ſince, this notwithſtanding, he, and LINNAEUS alſo affirm, " Ficus in Hollandia " quotannis prolici e ſeminibus, vel fructu lacerato terrae " commendato, fructu tamen illo ex Italia allato." Amaen. Acad. 1. p. 233. Exam. Epicris. p. 16. &c.; I leave it to the reader to determine, whether caprification affords an argument for, or againſt the ſexes of plants.

39. ARGUMENT 7. "Septimo *flores nu-*
"*tantes.* Cum pollen maſculus, plerumque
"aëre ſpecifice gravior, difficile ſurſum ten-
"deret; apud *pleraſque* plantas, piſtillum lon-
"gius gerentes, florem nutantem fecit Crea-
"tor, ut ſtigma eo melius attingat pulvis:
"e. gr. in *galantho leucoio, cyclamine, narciſ-*
"*ſo, fritillaria, campanula, erythronio,* &c."
Wahlbom (*e*). "*Flores nutantes* gaudent
"piſtillo, ſtaminibus longiore, ut cadat pol-
"len in ſtigma: *campanula, leucoium, galan-*
"*thus, fritillaria,*" *Linnaeus* (*f*).

I readily grant, that the *pollen maſculum,* or *pulvis apicum,* is heavier than air, it being often heavier than water; and alſo, that ſome *flores nutantes* have the *piſtillum* longer than the *ſtamina*; but cannot allow the reaſon to be "ut cadat pollen in ſtigma:" for thus it muſt fall on the back of the *ſtigma,* when the *ſtylus,* is there thickeſt; or fall by it quite, when there ſlendereſt; and, in neither caſe, can this have ready acceſs to the ſeed. Again, in many *genera* of plants, not only in different *ſpecies,* but even on the ſame ſtem, ſome flowers hang down, others ſtand erect, others

(*e*) Amaen. acad. 1. p. 95.

(*f*) Phil. Bot. p. 92.

thers horizontal, while the *piſtillum* and *ſtamina* bear the ſame proportion to one another: *e. g.* ſome ſpecies of the *narciſſus*, *campanula*, *lilium*, &c. And altho' every *flos nutans* had the *piſtillum* longer than the *ſtamina*, yet it could not thence be inferred with any probability, that the deſign of the great Creator, in thus forming them, was what our authors alledge, unleſs every erect flower had its *piſtillum* ſhorter than the *ſtamina*, which is far from being fact, as is proven above (*g*).

40. ARGUMENT 8. "Videte et admiremini ſolertiam, quam Natura adhibet in plantarum aquaticarum, quae farinam foecundantem habent, floribus. Tempore floreſcentiae, flores ſpecifice leviores redditi, ultra aquae ſuperficiem attolluntur, ut in aëre foecundatio fiat, nec humiditate diluatur genitura: dum vero ſub aqua flores adhuc reconduntur ſubmerſi, ſolicite per petalorum commiſſuras clauduntur, apicibus verſus ſtigma inclinatis, et verſus interiora tantum farinaceis, exteriori ſuperficie membranacea et lata; ut in *nymphaea* et affinibus apparet," *Geſnerus* (*h*).

OCTAVO

(*g*) Vid. No 25.

(*h*) Diſſert. p. 92.

"Octavo *ſubmerſi.* Plantae haud paucae "caule ſub aqua latent; inſtante vero floreſ- "centia, enatant flores, ut *nymphaea*, &c. "Aliae vero ſub aqua omnibus ſuis partibus "occultantur; ut *myriophyllum*, *ſtratiotes*, *po-* "*tamogetones* plerique, qui omnes, ſub floreſ- "centia, ſpicam flores ſupra aquas exſerunt, "deinde iterum, peracta floreſcentia, de- "mergitur ſpica," *Wahlbom* (*i*).

"*Flores ſubmerſi* adſcendunt ſub floreſcentia: *nymphaea*, *ſtratiotes*, *myriophyllum*, *potamogeton*, *hydrocharis*, *valiſneria*," *Linnaeus* (*k*).

Although ſome aquatic plants blow only above the water, it cannot be proven that all do ſo: yea it is certain, that many ſubmarine ones fructify under water. But, granting all the alledged facts, it by no means follows, that the deſign of Nature is, to procure the better acceſs of the duſt to the *ſtigma*; but rather that it may be diſperſed in the air. For ſince, under water, the flowers "ſolicite per "petalorum commiſſuras clauduntur, apici- "bus verſus ſtigma inclinatis, et verſus interi- "ora tantum *farinaceis*;" the duſt or its *aura ſeminalis*, muſt there have much eaſier acceſs

to

(*i*) Amaen Acad. I. p. 96.

(*k*) Phil. Bot. p. 92.

to the *ſtigma*, than it can have, when the *ſtamina* are ſeparated, and expoſed to the winds; eſpecially, if it be true, as Mr *Vaillant* has it, that, in hermaphrodite flowers, and ſuch, according to the Sexualiſts, is the *nymphaea*, and ſome other aquatics, the duſt is not thrown out at once with ſuch violence, as it is where the ſexes are ſeparated, " ſed actum " generationis (adds Mr *Geſner*) floribus clau- " ſis exercent, dum ſtigma adhuc intra me- " dias antheras continetur." (*l*)

41. I mention theſe two learned authors, tho' I have the misfortune to differ from them, not only becauſe they are commended by *Linnaeus*, eſpecially *Vaillant*, of whom he ſays, " primus clare ſexum expoſuit (*m*)," but alſo that I may help them to a better inſtance, than the *parietaria*, of the ſudden exploſion of the duſt of the *apices* in barren flowers: it is the common flowering *nettle*, or *urtica urens maxima*, B. p. 232. (*n*); for one cannot obſerve

(*l*) Vid n. 21. ſupra.

(*m*) Bib. bot. 173.

(*n*) That is for a ſpecimen of botanical ſuperfluity; urtica foliis cordatis amentis, cylindraceis, ſexu diſtinctis, *mas*, Fl. lap. p. 299; urtica foliis oblongo-cordatis, dioica, H. Cliff. p, 440; urtica dioica foliis oblongo-cordatis, Fl. ſuec. p. 282; urtica maſcula, Syſt. Nat. 133; urtica perennis, Amaen. Acad, 2. 25. 99.

obſerve this ſtinging *nettle* for a few minutes, in almoſt any of the ſummer months, if the ſun ſhines, eſpecially before noon, but he muſt ſee many little clouds of duſt, thrown with a ſort of exploſion out of the burſting *apices*, which ſoon diſperſe and fall down. This, however, being a ſingle inſtance, and by me not obſerved in *ſpinacia, mercurialis, cannabis*, or any ſuch barren plant; no general concluſion can be drawn from it.

42. ARG. 9. " Nono, *Syngeneſia fruſtranea.* " Flores compoſiti variis modis fabricati ſunt. " — *Polygamia fruſtranea* foeminis exſultat " maritatis, totum diſcum occupantibus; floſ" culi vero foeminei radium conſtituentes, ob " defectum ſtigmatis, abundante licet diſci " pulvere, familiam propagare nequeunt," *Wahlbom* (*o*). I omit the reſt, ſince *Linnaeus* (*p*) ſays only, " *ſyngeneſia fruſtranea:* ubi " ſtigma deeſt, ibi nulla foecundatio: in ra" dio *centaureae, helianthi, rudbeckiae, coreo*" *pſidis.*" But, in the *radius* or *corona* of all theſe flowers, the *ſtylus* is alſo wanting, as well as the *ſtigma*; which may be many ways

(*o*) Amaen. Acad. 1. p. 96.
(*p*) Phil. Bot. p. 92.

ways neceſſary to the ripening the ſeed. It does not therefore appear that this proves or confirms the foecundating virtue of the duſt.

43. ARGUMENT 10. is a very extenſive one. "Ita ſeſe in plantis habere dictitat decimo *omnium florum genuina conſideratio.* "Brevitatis cauſa nonnullos tantum hic examinare lubet," *Wahlbom* (z). And, for brevity's ſake alſo, I ſhall here paſs them all; ſome of them having been noticed already, as others will be below; with this obvious note, that ſuch a general poſition can be proven only by an as general induction; which is altogether impracticable.

44. BUT ſince *Malpighius*, whom I reckon one of the moſt genuine contemplators of flowers, obſerves (*a*), that, "Turgentibus "orbicularibus corporibus quibus ſtaminum "capitula replentur *exſiccataque continente* "*capſula*, foras prodeunt globuli minimi et "diſperguntur;" and that this exſiccation does not, cannot well happen, before, by opening of their covers, the *ſtamina* be expoſed to the free air; and ſince in fact, the

 apices

(z) Amaen, Acad. 1. p 97.

(*a*) P. 63. edit. in 4to.

apices do not ſplit, at leaſt generally ſpeaking, ſo far as I have obſerved, until their covers open: it appears to be a more natural inference, that this duſt is not deſigned to fall on the *ſtigma*, or to impregnate the ſeed (*b*). And, conſequently, that " Omnium florum " genuina conſideratio, nec dictitat, nec con- " firmat, generationem vegetabilium fieri" in the manner the Sexualiſts pretend.

45. ALTHO' I have already, perhaps, been too tedious, and ſaid enough to overturn the modern doctrine of the ſexes of plants; yet there ſtill remain ſome arguments for it, which I cannot paſs. For *Linnaeus* ſays, " *An-* " *theras* eſſe plantarum *genetalia maſculina*, et " earum *pollen* veram *genituram*, docet eſſen- " tia, praecedentia, ſitus, tempus, locu- " lamenta, caſtratio, pollinis ſtructura: *ſti-* " *gmata*, germini ubique adnexa, eſſe *geneta-* " *lia foeminina*, probat eſſentia, praecedentia, " ſitus, tempus, caſus, abſciſſio (*c*)." Which in Phil. Bot. *(d)*, and Sponſ. Pl. *(e)*, are explained: but confirmed only, either by miſtakes, or by falſe conſequences. Thus,

" *Situs*:

(*b*) Vid. No. 40. ſupra.

(*c*) Lin. Fund. Bot. § 143. 144.

(*d*) P. 90.

(*e*) P. 84.

" *Situs:* Didynamiſtis ſtamina adſcendunt " ſub corollae labrum ſuperius, quo et ſe pi- " ſtillum flectit." But in the deſcription of his claſs 14. or *didynamia*, " Antherae ſub " labio ſuperiore *ſaepius* reconditae" *(f)*. And, " concerning the *ſitus*, he adds, " *Monaecciae* " *pleraeque* flores ſtamineos ſupra piſtilliferos " gerunt: *zea, ricinus*." But the *flores ſta-minei* are below the *piſtilliferi* in *ricinus*. What he ſays of *tempus*, is anſwered above; of *caſtratio*, below: and what he obſerves of the *loculamenta antherarum*, and *pollinis ſtructura*, teach nothing, but the author's conjectures.

46. But, whether theſe arguments be concluſive or not, *caſtratio florum* moſt certainly demonſtrates the uſe of the *farina foecundans*, and conſequently the ſexes of plants. " Veritatem hanc probat *caſtratio*. Si anthe- " ras alicujus plantae uniflorae auferamus, et, " ne aliqua alia ejuſdem ſpeciei adſit, cure- " mus; abortit fructus, vel ſaltem ova profert " ſubventanea; quod adeo certum, ut quiſ- " que nullo non ſucceſſu id experiri queat," *Wahlbom (g)*. " Caſtratio: *Melonis* flores ſta- " mineos

(*f*) Vid. Lin. Gen. pl. p. 261.
(*g*) Amaen. Acad. 1. p. 86.

"mineos qui diligenter auferunt, fructus non "obtinent. Tulipae solitariae si auferantur "antherae ante casum pollinis, sterilis eva-"det," *Linnaeus* (*h*).

So the winds seem here to forget their duty. But, granting all here alledged, it proves nothing. For plucking off the *petala* may have the same effect; "Saepius avulsis flo-"ris foliis, antequam hiarent, in tulipa prae-"cipue, expectavi an stylus incrementa ca-"peret; et interdum ejusdem incrementum "*remorari* observavi, quandoque quaedam "semina, absque noxa, debitam sortita sunt "magnitudinem," *Malpighius* (*i*). Wounds also in otherways necessary parts, may be the cause of such barrenness, by depriving the seeds of their proper juices, &c. Nevertheless there is reason to deny the fact: for surely M. *Wahlbom* did not make the experiment in every species of flowers: M. *Geoffroy's mays* ripened some seeds, tho' castrated (*k*): and I made the trial in tulips, *nullo cum successu*. Thus,

47. ONE

(*h*) Phil. Bot. p. 90. and 92.

(*i*) Oper. p. 70.

(*k*) Vid. Mem. Acad. 1711.

47. ONE year, obſerving two ſtrong *tulips* growing together, in an incloſure ſurrounded with a tall and thick quickſet *hawthorn-hedge*; I cut down two or three more *tulips*, which ſtood at ſome diſtance from them, ſo as to leave none within that incloſure, ſave the two I mentioned; out of theſe, gently opening the *petala*, I plucked all the *ſtamina* with their *apices* ſtill intire. The conſequences of this too rude *caſtration*, was a conſiderable extravaſation of the juices, in the bottom of the flower, and a ſudden decay of the *ovarium* or fruit, which never increaſed, but turned yellow, ſhrunk, and withered. In order to diſcover whether this abortion was owing to the wounds, or to the want of the duſt of the *apices*; I ſuffered theſe two *tulips* to remain in the place where they were: and next Seaſon, with the ſame precaution that no other *tulips* ſhould flower within the incloſure, I opened the *petala*, and took out carefully, not the *ſtamina*, but only all the *apices*; which prevented any ſenſible bleeding of the parts. This more gentle caſtration, they bore perfectly well; the *ovarium* ſuffered nothing, in either of them,

but

but increaſed, and came to maturity, quite full of ſeeds. See alſo *Gardener*'s dictionary, article *generation*, near the end.

48. THUS I think I have ſufficiently anſwered all the arguments for the ſexes of plants, taken either from the ſtructure of flowers, or experiments of any conſequence that I could meet with. But ſince no ſmall ſtreſs ſeems ſtill to be laid on the *analogy* between plants and animals, as much favouring this doctrine; I muſt beg leave, a little to conſider it alſo, altho' it is certainly true, to uſe Mr *Needham*'s words (*l*), that the method of reaſoning by analogy, is but too apt to lead us into miſtakes; and therefore we ought to be very diffident of conſequences deduced this way: for mere analogy, founded on facts, and extended by conjecture, however plauſible, can, at moſt, but furniſh motives for a reaſonable doubt, and further inquiry.

59. "OMNE vivum ex ovo; per conſequens etiam vegetabilia: ovum, non foecundatum germinare, negat omnis experientia; adeoque et ova vegetabilium," *Linnaeus* (*m*). I ſhall not here enquire, whether either of theſe propoſitions are certainly true; but

(*l*) Phil. Tranſ. No. 490.
(*m*) Fund. bot. § 132—150.

but only notice, that they neither prove nor explain any thing: for if *omne vivum* include vegetables as well as animals, as certainly, according to our author it does; then he might as well have ſaid, *Omnia animalia et vegetabilia ex ovis, et per conſequens etiam vegetabilia*: if *vivum* do not include vegetables, the conſequence does not follow. The ſame holds in the ſecond, and ſeveral others of his aphoriſms.

50. In how many things ſoever plants and animals may agree, certainly they do not agree in every thing. "Ad ſummum, (ſays "*Theophraſtus*) (*n*) non omnia ſimiliter atque "in animalibus accipi debent: nam (in plan-"tarum genere) vis undique germinandi ha-"betur; quoniam et undique animatum eſt." And *Malpighius* (*o*), "In vegetantibus, ubi "non eſt tantus organorum apparatus, et "cuilibet ſenſibili particulae, omnia inſunt "quae in toto deprehenduntur: *nulla interce-"dente generatione*, abſciſſi quicunque rami "frequenter in novam ſobolem excreſcunt; "vel naturae miniſterio, deciduae minimae "et compendiariae plantulae, a tenellis ſur-culis

(*n*) De hiſt. pl. l. 1. c. 1.
(*o*) Anat. pl. p. 76.

"culis ſub ſeminum ſpecie, propagationem "quocunque anno perpetuant." Hence many plants are much better and eaſier propagated by cuttings, layers, offsets, gems or buds, than by ſeeds: hence *garlicks, onions, leeks, &c.* carry gems frequently, on the top of the ſtalk, among, or in place of the ſeeds, as well as at their roots under ground. What are bulbuous roots but gems? and ſuch *Caeſalpinus* obſerved on the leaves of the *moly*; as they are frequently ſeen on the ſtalks of *tulips, lilies:* and it is to buds that the increaſe of many trees is owing.

51. Now, what is a bud? how does it differ from a ſeed? "Differt ſoboles a ſemi-"ne, ut foetus vivens ab ovo; ſemen enim "tanquam ovum eſt, in quo eſt principium "vitale, at vita nequaquam; ſoboles autem "vivit, primo quidem juxta parentem, ut "ejus germen, poſtea vero per ſeipſum, pro-"priis radicibus ex terra humorem trahens," *Caeſalp.* de plantis (*p*): and *Malpighius*, (*q*) "Gemmae ſunt velut infans, ſeu foetus ita "cuſtoditus, ut ſuo tempore auctus, in ſur-"culum excreſcens, tandem ova promat. "Erit

(*p*) Lib. 1. c. 5.

(*q*) Anat. Plant. p. 39. 77.

"Erit igitur probabiliter ſemen quaſi gemma "pendula et decidua, alieno germinatura "ſolo." And although the learned *Petrus Lofling*, in his treatiſe called *Gemmae Arborum*, (*r*), or rather the publiſher himſelf, is pleaſed to ſay, "qui gemmam ſibi repreſen-"tant ut alterum ſemen, fallunt et fallun-"tur" (*ſ*); yet his deſcription of a *gemma* ſeems not altogether to agree with this *notandum*: for (*t*) he ſays, "gemma eſt pars plan-"tae radici inſidens, quae occultat ſquamis, "foliorum rudimentis, embryonem futurae "herbae." And below (*u*), he explains what he means by *radix* thus, "Hae gemmae inſi-"dent, vel radici ſub terra reconditae, vel *ra-"dici ſupra terram in truncum ramoſum aſſur-"genti*; illae, ubi carnoſae fuerint atque mag-"nae, *bulbi* nomine veniunt, &c." And elſewhere (*x*), "Gemmam proinde concipio in-"ſtar herbae in compendium redactae, tectae "et contractae ſua inter extrema folia, ut ab aëris injuria conſervetur; cui herbae nihil

Mm "amplius

(*r*) Amaen. Acad. 2. p. 182. 224.

(*ſ*) P. 185. note k.

(*t*) P. 185.

(*u*) P. 186.

(*x*) P. 192.

"amplius deeſt, quam vis ſe extendendi, e-"am autem calor demum excitat." Is not this as applicable to ſeeds, as to gems? "Semen, (ſays *Linnaeus*) (*y*) proprie, novum vegetabilis rudimentum, humore rigatum, "veſica tunicatum." And is not a *gemma* the ſame? or a more perfect ſeed? however, call it *embryon*, *compendium*, *primordium plantae*, or what you pleaſe, with its covers: ſince *Linnaeus* owns, "Generationes plantarum ex "*ſemine* et *gemma* eſſe coevas (*z*)"; and "*gemmas*, proinde ac *ſemina*, in ſe continere pri-"mordium plantae (*a*);" it comes to the ſame thing for our purpoſe. For,

52. If gems contain the *primordia plantarum*, they contain the moſt eſſential part of ſeeds; becauſe the *ſeminis eſſentia conſiſtit in corculo* (*b*); and the *corculum* is nothing but the *novae plantae primordium* (*c*). Now ſince buds or gems are copiouſly produced by numberleſs plants, and often break out of the ſmootheſt part of the bark, eſpecially of pruned trees; and, ſince the ſmalleſt part

(*y*) Lin. Phil. bot. p. 54.

(*z*) Phil. Bot. p. 88.

(*a*) Gem. arb. Amaen acad. 2. p. 185.

(*b*) Phil. bot. p. 56.

(*c*) Ib. p. 54.

part of a plant may be made to grow and emit gems, whether it be naturally fertile or barren, *male*, *female*, or *hermaphrodite*; does it not clearly follow, that neither difference of sex, nor *illapsus pollinis antherarum supra stigmata nuda*, are necessary to the production of the *primordia plantarum*, the essence of seeds, as well as of gems; and that no imagined analogy between plants and animals, can warrant or excuse the fulsom and obscene names, imposed by the *Sexualists* on the different parts of the fructification of vegetables *(d)*?

53. NOT a few other arguments against the modern doctrine of the sexes of plants, might be brought from the structure of many compleat flowers, as well as from the numerous tribes of such as are called less perfect; some of which produce seed, but want flowers; others have neither flowers nor seed. For it requires more than an *ipse dixit* to prove, that "omnis species vegetabilium flo-"re et fructu instruitur, etiam ubi visus eas-"dem

(*d*) Vid Cl. LINNAEI fundamenta botanica, § 140, 143, 144, et 146, and the learned commentaries on these, in the *Sponsalia plantarum*, and *Philosophia botanica*.

"dem non aſſequitur," as *Linnaeus* aſſerts (*e*). I know he attempts to prove it thus (*f*), "*Muſ-*"*corum* ſemina *Nos; lemnae* flores delineati "a *Valliſnerio; fucorum* flores obſervavit *Reau-*"*mur; pilulariae* flores inveſtigavit *B. Juſſiae-*"*us; fungorum* ſtamina deſcripſit *Michelius*." And this is all, and affords but a lame proof, and moſtly conjectural.

Thus, although *Dillenius* (*g*), in the end of a particular deſcription of the *ſelago foliis et facie abietis R. ſyn. p.* 106. or upright *firr-moſs*, ſays, "plura non obſervavi, nec "ſemina in his vel calycibus, vel foliis bra-"cteatis invenire potui:" yet becauſe he adds, "Suſpicor autem bracteas illas folia ſemina-"lia eſſe, et novarum plantarum productioni "inſervire;" the learned author of the *ſe-mina muſcorum* (*h*), poſitively concludes, "Hae foliola, tempore autumnali matura de-"cidunt a calyce perſiſtente, et novam plan-"tam propagant, radiculas e baſi exſerentia." But, ſince below (*i*) he owns, that "Semi-"na muſcorum tanquam *nuda corcula*, ſine "cotyledonibus,

(*e*) Fund. bot. § 139.
(*f*) Phil. bot. p. 89.
(*g*) Hiſt. Muſc. p. 437.
(*h*) Amaen. acad. p. 2. 295.
(*i*) P. 299.

"cotyledonibus, ſine tunicis animo concipi-"enda ſunt.—Flos autem obſcure nobis per-"cipitur, cum nullum *ſtylum*, nullumque *ſtig*-"*ma* habeat:" if, what he calls a ſeed, really takes root and grows (for I find not that either he or *Dillenius* made the experiment); it has a better claim to the character of a gem, than of a ſeed. And, whether or not, it is evident that the "generatio muſcorum "minime fit, mediante pollinis antherarum "illapſu ſupra ſtigmata nuda."

54. THE ſame learned author (*k*) aſſerts, "Quod pulvis in capitulis muſcorum ſit pol-"len maſculum:" that the "ſemina lycopodii "officinarum, ſunt purum putum pollen (*l*);" tho' others think them all rather ſeeds. But, whether they are either, I ſhall not, cannot determine: only they ſeem to reſemble more the ſeed of the *lingua cervina*; which has been found fertile (*m*), without the foecundating influence of any *antherae* yet diſcovered, that I know of. But, granting the above cited aphoriſm to be true in its largeſt extent, it can never prove the alledged uſe of the

(*k*) P. 300.

(*l*) P. 293.

(*m*) Vid. Il. ox. 3. p. 555.

the duſt of the *apices* in imperfect plants, where it does not appear; ſince the contrary is demonſtrated in the moſt perfect of the vegetable kingdom.

55. I noticed above, that ſeveral arguments might be taken from the fructification of ſome of the moſt compleat plants: and the author of the *ſemina muſcorum* (*n*) gives ſome inſtances, and anſwers them; how ſatisfyingly, I leave the reader to examine. Only, ſince there are three ſpecies of the *viola*, I ſhall add a fourth, *viz. viola martia purpurea flore ſimplici odoro*, B. p. 199. or common *March violet.* This plant puts out its flowers with the *ſtamina* in *March*, on long foot-ſtalks; no fruit ſucceeds them, nor appears for ſome weeks, yea months after, when cloſe on the root, and well covered with the leaves, the fruit is formed, ſoon ripens, and contains plenty of ſeeds; tho' all the flowers were plucked in *March.* Does either *tempus* or *proportio*, here confirm the ſexual ſcheme?

56. To conclude: Had the modern doctrine of the generation of plants, continued only to influence the learned more accurately

to

(*n*) P. 186, &c.

to inquire into the ſtructure of vegetables, I would never been at ſo much pains to confute it: but, ſince it has given occaſion to an intire deformation of botany, and to the introduction of an infinite number of new names, and perplexing, tho' childiſh terms, whereby this moſt uſeful ſcience, is like to become not only vaſtly more difficult, but even ridiculous; I thought it high time to publiſh ſuch arguments as to me appeared ſufficient, by ſapping the foundation, to overturn this hideous ſuperſtructure. For ſurely no method at all, is much better, than ſuch an one, whoſe *nomenclature* is more difficultly acquired, than the knowledge of the plants themſelves, which alone is true botany. But of this more elſewhere (*o*). It remains only that we inquire a little into the real uſe of the duſt of the *apices*.

SECT. IV.

56. AUTHORS are ſo much divided in opinion, as to the primary uſe of this duſt, that it may be doubted whether it be of any to the plant which produces it. And, ſince vegetables ſerve only, or at leaſt chiefly, for the uſe

(*o*) Tyrocin. Bot. I. p. 40. 50.

ufe of animals; what the great Mr *Boyle* fays of the one, may well be applied to the other *(p)*. And fince we fee bees frequently come out of flowers loaded with their duft, and apparently carrying it to their hives; it is not improbable that they feed on it. For, if Mr *Geoffroy*'s experiments can be depended on *(q)*, it cannot be the *materia* of wax, without being digefted and prepared in the infect; and, how many other infects may live on it, I cannot tell.

57. BUT,

(p) "The whole animal is but a part of that greater body "the univerfe; and therefore cannot eafily be fuppofed to "have been framed and furnifhed with the parts it confifts "of, intirely for its own fake. And, when we fay, that all "its parts are contrived for the beft advantage for the animal, "I conceive it to be underftood in this limited fenfe, that "the parts are excellently framed for the welfare of the ani-"mal as far furth as that welfare, is confiftent with the ge-"neral ends of the Author of nature, in the conftitution "and government of the univerfe. All which ends it is not "an eafy tafk to difcover, tho' fome of them may be inve-"ftigable by us. And, it feems prefumption to fuppofe, "that the welfare of particular animals, is any further de-"figned and provided for, than will confift with the cofmi-"cal ends of the univerfe, and the courfe of God's general "providence, to which his fpecial or particular providence, "about this or that mere animal, ought, in reafon, to be "fubordinated." Vid. BOYLE's Works, vol. 4. p. 548.

(q) Vid. Mem. acad. k. 1711.

57. But, if it be inſiſted on, that its primary uſe muſt reſpect the plant itſelf; perhaps *Caeſalpinus* may be in the right, with relation to the plants, whereof ſome, in the ſame ſpecies, carry *ſtamina*, and others the ſeed *(r)*: and *Malpighius (ſ)*, and *Tournefort*, with relation to ſuch as bear both on the ſame individual plant: *viz.* that it carries off what is excrementitious, and unfit for nouriſhing the ſeed *(t)*. I own, indeed, that this opinion does not well agree with that of *Caeſalpinus*; it being more than probable, that the duſt of the *apices*, in barren as well as fertile flowers, is of the ſame nature, and deſigned for the ſame uſe.

58. And, from what has been obſerved of the *parietaria*, *ficus*, *viola*, &c. yea and from the general ſtructure of fertile, as well as barren flowers; it is alſo more than probable, that this duſt is rather excrementitious and noxious

(*r*) Vid. ſupra. No. 6.

(*ſ*) Vid. p. 70.

(*t*) "Petala (ſays Tournefort) alimentum a pediculo acceptum viſceris inſtar, perficiunt, et fructui naſcenti ſuppeditent; ineptis humoris partibus per ſtamina, ſeu vaſa excretoria, abeuntibus in apices ſeu receptacula. Diximus jam apices, quicquid minus apti continet alimentum, in ſe recipere rerumque valvas a congeſtis excrementis deduci." Inſt. p. 69. and 70.

noxious, than uſeful to the nouriſhment or foecundity of the ſeed. Nor is this inconſiſtent with its influence, on the production of the numberleſs varieties of new flowers, which yearly adorn the gardens of the curious: if the opinion, which has for ſome time prevailed, be well founded; to wit, that it is the beſt way to obtain new varieties of fine flowers, to plant near together ſome of the beſt kinds of the ſame ſpecies, differently variegated, and ſave their ſeeds: reckoning that the duſts of theſe flowers have ſuch influence on one another, that their ſeed will produce finer varieties, than the ſeed of the beſt of them would do, if it ſtood alone, however otherwiſe managed. And on the ſame foundation it is, that M. *Du Hamel*'s ſcheme, for producing new varieties of fruits, is built (*u*). I ſay, allowing all this to be fact, which I cannot confirm; it ſeems rather to eſtabliſh, than to be an objection againſt Mr *Tournefort*'s opinion.

59. FOR, ſince all double flowers are monſters, "Luxuriantes flores (ſays *Linnaeus*) "nulli naturales, ſed omnes monſtra ſunt; "pleni eunuchi evaſerunt, proliferi monſtro- "ſorum

(*u*) Vid. Mem. acad. an. 1728.

"ſorum augent deformationem (x)"; ſince the fineſt varieties of flowers are the moſt tender; ſince variegation of flowers, as well as of leaves, is preternatural; and ſince even the improvements of plants, or fruits by culture, with relation to their uſefulneſs to us, are but diſeaſes in the plants, conſidered in their natural ſtate: may it not be properly inferred, that, whatever influence the duſt of the *apices* has in producing theſe, it muſt be owing to its noxious, not foecundating vapours?

60. AND, to conclude, although it ſhould be granted that we have not ſufficient *data*, to enable us to determine poſitively of what real uſe this duſt is to the plant; yet, ſince it has appeared, to a demonſtration, certain, that fertile ſeeds may be produced without it; our ignorance herein, can never be an argument of any conſequence, for the modern ſexual ſcheme, or doctrine of the ſexes of vegetables, againſt which I having been arguing; with what ſucceſs, I ſubmit to the learned to determine.

(x) Fund. Bot. § 150.

ART.

ART. X.

Remarks on chemical Solutions and Precipitations, by ANDREW PLUMMER, *M. D.; Fellow of the Royal College of Physicians, and Professor of Medicine and Chemistry in the University of* Edinburgh. *

THAT I may not give the first instance of breaking in upon the order and rules established in this society, I shall lay before you, at this time, some miscellaneous observations, on the solutions and precipitations which frequently occur in the prosecution of chemical inquiries.

I shall lay down these remarks by way of propositions, and, after each, shall mention the facts or experiments which gave occasion to the remark, or which confirm and illustrate the proposition.

ALTHOUGH many of the instances which I am to adduce, are common, and well known to every one versant in chemical experiments; yet, as the history of chemical solutions

* *January 3d* 1738.

ſolutions is very extenſive, and as many ſingular conditions and appearances, are to be obſerved in the actions of various ſolvents, upon the bodies which they diſſolve; it may poſſibly be of ſome advantage, to any one who ſhall afterwards undertake to give a rational and ſatisfactory account of the cauſes which produce ſuch effects, and of the laws by which theſe cauſes act in particular circumſtances, to have in his view a great many inferior axioms or canons, comprehending as many particular inſtances as poſſible: becauſe theſe may enable him to aſcend to more general canons; and at length to the ultimate phyſical cauſe; and again, from thence to explain the *phænomena*, in particular inſtances.

By *ſolution*, is here meant, the ſeparation and diviſion of a ſolid body, or of a thick and conſiſtent maſs, by means of a ſenſible fluid, into parts ſo ſmall, as to diſappear and be equally diſperſed through the fluid.

That liquor which makes the ſeparation of parts, I call the *ſolvent*; or, in the ſtile of the Chemiſts, the *menſtruum*.

I muſt take the liberty, to make one preliminary remark, before I deſcend to particulars:

lars: for, if the aſſertion of certain Chemiſts was well founded, it would, in a great meaſure, ſuperſede any further inquiries.

I. THE notion of an univerſal *menſtruum*, capable to diſſolve equally every body to which it is applied, appears repugnant to the nature of things, and to theſe limited powers which we diſcover in all known bodies.

I ſhall not undertake, in this place, to adduce and examine the bold aſſertions and high pretenſions of ſome Chemiſts to the diſcovery of an univerſal ſolvent; becauſe it would exceed the limits which I propoſe to myſelf, and perhaps afford little entertainment or inſtruction. But I muſt be allowed to doubt of the poſſibility of an univerſal ſolvent, until more certain evidences are produced than what have hitherto appeared. For, as there is a vaſt variety of bodies which differ much from one another in denſity, ſolidity and texture, in the bulk, ſhape and compoſition of the conſtituent particles, in the degree of force with which theſe cohere among themſelves, and in the number, ſize, and figure of the pores or interſtices betwixt the ſolid parts; it is ſcarce conceivable, that

that any one liquor can be endued with powers correſponding to all the various circumſtances of ſuch a variety of bodies, ſo as indiſcriminately to diſſolve all: it is therefore much more agreeable to the nature of things, that there ſhould be a variety of liquors endued with different qualities, in different degrees, to ſeparate the particles of ſo many different ſolid bodies; and daily experience ſufficiently confirms the truth of this concluſion. There is perhaps no body, natural or artificial, but there may be found a *menſtruum* proper to diſſolve it: and, tho' ſome liquors are known to diſſolve ſeveral bodies which differ in many reſpects; yet even theſe ſolvents, which are allowed to be moſt extenſive, are found incapable of diſſolving a great many other bodies.

II. The denſity, ſolidity, hardneſs, fixedneſs and other manifeſt qualities of bodies, can afford us no certain mark or indication of the qualities of the liquors, requiſite to diſſolve them, if experience is wanting. Neither can we conclude, from the ſtrength, ſharpneſs or corroſiveneſs of liquors, diſcovered by their effects on the human body, the

the fitneſs of ſuch liquors to diſſolve different ſubſtances.

IRON and copper, the hardeſt of the metals, which require the greateſt force to extend them, and the ſtrongeſt fire to bring them to fuſion, will be corroded and diſſolved by liquors moſt harmleſs to the human body; as vinegar, juice of lemons, a ſolution of tartar, rheniſh wine, nay moiſt air: whereas quick-ſilver, whoſe particles are ſo mobile, eaſily ſeparable and diſſipated by a moderate heat, and tin, which is very ſoft and eaſily melted, require more corroſive *menſtrua*. Coral, and ſome of the hardeſt gems, will be better diſſolved in a weak vegetable acid, than in a much ſtronger one of the mineral kind. *Oleum vitrioli*, *ſpiritus nitri*, or *aqua fortis*, the moſt ponderous and corroſive of the acid liquors, cannot diſſolve bees-wax or crude ſulphur, which will readily relent in the mild and ſoft oil of olives; *ſal tartari*, or *oleum tartari*, *p. d.* is almoſt equally corroſive, with regard to the human body, as *aqua fortis*: yet the ſolvent powers of theſe liquors, with reſpect to other ſubſtances, are moſt oppoſite.

III. THE

III. The fitneſs of a liquor to diſſolve a certain body, cannot be certainly learned from the agreement of that body in ſeveral properties with another, which we know can be diſſolved in that liquor. Neither can we ſafely conclude, that two liquors, which nearly reſemble one another, will mutually diſſolve all the ſame bodies.

We meet with abundance of inſtances in chemiſtry, to make us cautious in drawing ſuch concluſions. Well rectified ſpirit of wine, makes a very compleat ſolution of colophony or any of the common roſins; but there are ſeveral ſubſtances which have a great affinity to roſins, as *lacca*, *copal*, *amber*, *ſulphur*, which being digeſted with highly rectified ſpirit of wine, will ſcarce give a tincture to the ſpirit; that is, theſe bodies remain either altogether, or, for the far greater part, indiſſoluble in that liquor, unleſs they are previouſly prepared and diſpoſed to a ſolution, by the interpoſition of another body, as a fixt alcaline ſalt, &c. Deſtilled vinegar will readily diſſolve copper, lead, iron; but, it would be raſh to conclude, that the ſame liquor will alſo diſſolve gold, ſilver, antimony, whatever agreement we find between theſe bodies and the former. On the other hand, *aqua fortis* and *aqua regia* appear to be much of the ſame nature; and the former, by a very ſmall change, may be converted into the latter; yea, further, they diſſolve equally ſeveral bodies, as copper, iron, ſpelter, tinglaſs: yet, it would be erroneous to conclude, that, whatſoever body one of theſe liquors diſſolves, the other would diſſolve the ſame; for *aqua fortis* diſſolves ſilver and not gold, and, *vice verſa*, *aqua regia* diſſolves gold and not ſilver.

IV. The ſolvent power of ſome liquors, with reſpect to certain bodies, may be increaſed, diminiſhed or deſtroyed, by a very ſmall and almoſt imperceptible change of the liquors.

I put two grains of gold into a drachm of *ſpiritus ſalis marini*, and ſet the glaſs on warm ſand; after ſome hours, I could obſerve no ſenſible change of the bits of gold, in colour or bulk, nor of the liquor: then I put into the glaſs, as cautiouſly as I could, one drop of *ſpiritus nitri*; after ſome time, I found the liquor had aſſumed a bright yellow, or golden colour, and the gold quite diſſolved; for there remained only a few white particles

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at the bottom, which were probably ſilver. Thus the very ſmall addition of *ſpiritus nitri* very much increaſed the ſolvent power of the *ſpiritus ſalis marini*. It is known, that *oleum vitrioli* poured on quick-ſilver, and kept in the common air, or in a very gentle heat, does not corrode or diſſolve it; but, if they are brought to a heat exceeding that of boiling water, then the ſolution takes place. Mr *Boyle* teſtifies, that, by his care to rectify or dephlegmate *aqua fortis* very much, he rendered it unfit to diſſolve ſilver. While *aqua fortis* is diſſolving copper with a briſk motion, if a little *alcohol* is poured in, the ſolution will be ſtopt.

V. SOME bodies which we know can be eaſily diſſolved in certain liquors, may be ſo changed by the fire, or otherways, that the ſame liquors will not afterwards diſſolve them. On the other hand, the fire renders ſome bodies fit to be diſſolved in certain liquors which could not be diſſolved in them before.

THE oil of vitriol, as I ſaid before, aſſiſted by a proper degree of heat, readily enough diſſolves quick-ſilver: but when quick-ſilver is, by long digeſtion in a moderate heat, deprived

deprived of its fluidity, and turned into a red *calx*, or has been firſt diſſolved in *aqua fortis*, and afterwards, by evaporation and calcination, freed from the acid, and reduced to a red powder; if either of theſe red powders, I ſay, is digeſted with oil of vitriol, and made to boil, this liquor will not now diſſolve the mercury, or ſo much as change its colour. On the other hand, tho' *ſpir. ſal. mar.* is not eſteemed a proper *menſtruum* of quick-ſilver in the immerſive way, but when applied to it in form of vapour in ſublimation; yet if this ſpirit is poured upon the red precipitate mercury, it preſently changes its colour to white, and quickly diſſolves it. Again, *ſpir. ſal. mar.* readily diſſolves clean and freſh filings of iron: yet if theſe are frequently moiſtened with water, and by trituration and calcination turned to a *crocus*; *ſp. ſal. mar.* poured upon this, raiſes an effervеſcence indeed, but does not ſeem to make a ſolution; for the iron lies at the bottom not ſenſibly diminiſhed, and the colour of the liquor is not changed. Some ores, as they are taken from the mine, tho' they appear pretty rich and clean, yet will not readily diſſolve in the ſame *menſtrua*, which are fit to diſſolve the pure metals, until

til the ores are roaſted, waſhed and ſmelted. The *calces* of lead and tin will more eaſily diſſolve in ſpirit of vinegar, than the thin plates or ſhavings of the ſame metals.

VI. Some liquors which diſſolve ſeveral different bodies, while they perform the ſolutions, ſeem to act in a different manner upon each body, require different helps, and diſſolve different quantities each.

This is very remarkable in the action of oil of vitriol upon iron and quick-ſilver: to make oil of vitriol diſſolve iron ſucceſsfully, it muſt be diluted with a quadruple quantity of water; when the clean fileings are put into this liquor by degrees, there ariſes a ſtrong ebullition and effervesſcence, with a conſiderable heat and thick ſteams of a penetrating ſulphurous ſmell, which are apt to kindle and make a fulminating noiſe upon the approach of the flame of a candle; and, laſtly, one ounce of oil of vitriol thus diluted will diſſolve an equal weight of pure fileings of ſteel: but, when oil of vitriol is employed to diſſolve quick-ſilver, it muſt be ſtrong and well dephlegmated: when it is poured upon the quick-ſilver, it raiſes no inteſtine motion or heat; it does not begin to corrode or diſſolve

the

the quick-ſilver, till there is ſuch a heat applied, as will almoſt bring the liquor to boil, and even then the ſolution goes on ſlowly and quietly: laſtly, to diſſolve one ounce of quick-ſilver, it requires, at leaſt, three ounces of oil of vitriol; and yet the quick-ſilver will not be entirely ſuſpended in this ponderous liquor.

Even water, tho' it diſſolves all kinds of ſalts, yet it does not act upon all of them with the ſame eaſe; that is, the ſame quantity of water cannot diſſolve an equal quantity of each ſalt, in the ſame time, or with the ſame degree of heat; for an ounce of cold water will ſoon diſſolve half an ounce of ſalt of tartar; but half an ounce of crude tartar cannot be diſſolved in leſs than ten ounces of water; and that quantity of cold water will make no ſolution until it is brought to boil, and continues to boil for ſome time, new water being added to ſupply the waſte. Even that compound ſalt, made of the fixt ſalt and chryſtals of tartar, which, from its ſolubility, with reſpect to common tartar, gets the name of *tartarus ſolubilis*, requires little leſs than ten times its weight of warm water to diſſolve it fully.

VII. Some

VII. Some bodies can be diſſolved in liquors of very different qualities, but in different proportions, with different helps, appearances and effects.

Common ſulphur or its flowers diſſolve in any oil, but more eaſily, and in greater quantity, in a thick, mild, expreſt oil, than in a thin, hot, aromatic, diſtilled oil. When flowers of ſulphur are digeſted with oil of olives, or lint-ſeed, it becomes a thick balſam; three ounces of oil will diſſolve one ounce of ſulphur; and any ſmall part of ſulphur which remains, has the appearance of melted ſulphur. But when the flowers are digeſted with æthereal oil of turpentine, ſix ounces of oil will ſcarce diſſolve one of ſulphur, and what remains is not like the ſulphur in the former caſe; for, if the veſſel is cooled gradually before the balſam is poured off, the ſulphur appears almoſt like nitre when it chryſtallizes, or a vegetating ſalt branched out into long ſlender ſtalks. Sulphur likeways diſſolves in ſpirit of hartſhorn, but ſtill in leſs quantity, and with a ſmaller degree of heat. And it is to be obſerved, that the unrectified or oily ſpirit will diſſolve more ſulphur than the clear and pure

ſpirit.

ſpirit. To make ſulphur ſoluble in water or ſpirit of wine, it muſt be melted, then mixed and incorporated with a fixed alcaline ſalt, by which means it will moſtly diſſolve in warm water, and give a deep and ſtrong tincture to ſpirit of wine.

THE *calx* of lead, *litharge* or *minium*, diſſolve in diſtilled vinegar; but more fully and quickly in *aqua fortis* diluted with ſix times its weight of water. The ſolution of lead in ſpirit of vinegar, being evaporated ſlowly, to a due conſiſtence, and then removed to a cool place, never ſhoots into fair diſtinct chryſtals; but thickens, as it were, into a ſaline maſs, like coarſe ſugar, ſomewhat moiſt; and, with difficulty, can be brought to a tolerable whiteneſs: but the ſolution in diluted *aqua fortis*, treated in the ſame manner, gives large, ſolid, ſhining, white chryſtals, regularly ſhaped. The *calx* of lead or *minium* will likewiſe diſſolve by digeſting or boiling in oil of olives or lintſeed; but as it diſſolves, it thickens into the conſiſtence of an unguent or plaiſter.

GOOD *aqua fortis* or *ſpiritus nitri* will diſſolve about an equal weight of quick-ſilver, and the ſolution is performed with a great commotion

commotion, heat, and thick red ſmoke: ſtrong oil of vitriol diſſolves ſcarce a third part of its weight of quick-ſilver, and the ſolution is made in a ſlow and imperceptible manner, and requires the aſſiſtance of a great external heat.

THE manner in which the acid of ſea ſalt is applied to quick-ſilver, in order to diſſolve and be united to it, is ſtill more extraordinary: there are two methods practiſed, but both depend on the ſame principle, and produce the ſame effect. For either the quick-ſilver is rubbed in a marble mortar with green vitriol calcined to whitineſs, decrepitate ſea ſalt, and a ſmall proportion of dry ſalt-petre, till the quick-ſilver is extinguiſhed and diſappears, this maſs is put into a ſubliming glaſs, a gentle heat is made at firſt, and gradually increaſed; in the beginning ariſe white ſteams, if theſe are collected, they condenſe into a liquor, which proves a weak *aqua regia*; then there will ſublime a white ſhining chryſtalline body, which is a vitriol of quick-ſilver, and goes under the name of *mercurius ſublimatus corroſivus*: that the quick-ſilver is here diſſolved by, and united to the acid of ſea ſalt, with a ſmall quantity of the nitrous a-

 cid,

cid, is evident, becauſe the ſame materials, without the addition of quick-ſilver, properly treated, yield a good *aqua regia* which diſſolves gold. Or quick-ſilver is firſt diſſolved in *aqua fortis*, the ſolution is evaporated till their remains a white, dry, vitriolic body, which is to be mixed and rubbed with an equal weight of decrepitate ſea ſalt, and ſublimed as before: here likewiſe the acid of ſea ſalt, with a portion of the nitrous, is joined to the mercury; for one way of preparing an *aqua regia*, is by pouring *aqua fortis* upon decrepitate ſea ſalt, and drawing off a liquor by diſtillation, which is in effect done in this caſe, only the acid incorporates with the quick-ſilver into a vitriolic form.

VIII. The ſolvent powers of certain *menſtrua* are not deſtroyed in the act of ſolution, but can be exerted on other bodies; and the *menſtrua* ſeparated from the diſſolved bodies, may retain or recover all their former qualities.

This property of ſeveral ſolvents, will be found to obtain, in many more inſtances than is commonly imagined. Many ſolvents, when they are actually diſſolving bodies, excite a ſtrong and remarkable effervеſcence, with a great expanſion, hiſſing, bubling, heat and ſmoke:

ſmoke; and, after the ſolution, the liquors frequently acquire new qualities which were not conſpicuous before, either in the ſolvents or in the bodies diſſolved: whence it is generally imagined, that, from this ſtruggle or action and reaction betwixt the bodies, the particles of the *menſtruum* are broken, their points blunted, and their figures changed, by penetrating into the pores of the ſolid body, by disjoining and dividing the parts very minutely. But, I ſhall endeavour to make it appear, from experiments, that the active particles, of moſt liquors, employed as ſolvents, are extremely tenacious of their figure, bulk and denſity, at leaſt of the powers and properties which depend on theſe, and conſtitute the particular marks and characters of theſe liquors: that, tho' they have diſſolved ſeveral different bodies, have entered into many compoſitions, and have paſt thro' a great variety of forms; yet, after all, moſt of them can be brought back to their primitive ſtate, and made to exert the ſame powers which they did at firſt, and again run thro' all the ſucceſſive changes; ſo that it may be doubtful whether art can

intirely

intirely change the nature of ſome of theſe ſolvent liquors.

To begin with ſome familiar inſtances to confirm our remark. Water can diſſolve a certain quantity of any ſalt; yet, when it is fully impregnated with one ſalt, it will ſtill diſſolve a portion of another ſalt, tho' not ſo much as the ſame quantity of pure water would do. From ſea water, by evaporation and chyſtallization, are procured three diſtinct ſalts, beſides other ſubſtances, as the ingenious Mr *Brown* has obſerved, *viz.* the muriatic, or ſalt for common uſe, the bitter purging ſalt, and a fiery calcarious ſalt, different from the other two; yet ſea-water will diſſolve, at the ſame time, a pretty conſiderable quantity of ſeveral other ſalts, as ſugar, ſalt ammoniac, ſalt petre and ſalt of tartar; and, after it has diſſolved ſufficiently of two or more of theſe ſalts, it will, even then, diſſolve ſome more ſea ſalt, and ſtill more of the epſom or *Glauber*'s ſalt. Hence, by the way, we find ſuch a variety of mineral or medicinal waters, impregnated in various proportions, with different ſalts, vitriols, metals, earths, &c. The acid of ſea ſalt, which has diſſolved quick-ſilver, and chryſtallized with

with it in the ſublimation of corroſive mercury, will diſſolve the metallic or reguline part of antimony into a liquor, called *butyrum antimonii*; and this will afterwards diſſolve gold. Diſſolve ſilver in *aqua fortis*, when the ſolution is finiſhed, add to it ſome crude ſalt ammoniac, in powder, and ſet the veſ- in hot ſand; the ſilver will all fall to the bottom: then pour the clear liquor into another glaſs; this will be found a good *aqua regia* fit to diſſolve gold, *regulus antimonii*, &c. There is another noted experiment which evidently illuſtrates and confirms the remark. If fine copelled ſilver is diſſolved in proof *aqua fortis*, and the ſolution is diluted with an equal quantity of diſtilled water; it will continue clear and limpid, without any precipitation: if then a piece of poliſhed copper is put into the ſolution, the ſilver falls gradually down, and the copper is diſſolved, as appears by the green colour which the liquor acquires in a little time. When the ſilver is all ſeparated, take out the remaining copper, and put in a piece of clean iron; the green colour goes gradually off, by the copper falling down, and the iron is diſſolved in its place: after allowing a ſufficient

ſufficient time for the ſolution of the iron, and precipitation of the copper, put into the ſolution a piece of lime-ſtone; the *aqua fortis* immediately works upon it with freſh vigour: laſtly, add ſome ſalt of tartar, or *oleum tartari, p. d.*; this precipitates the ſtony matter, and renews the effervescence. This ſeems to finiſh the work, and quite to abſorb the acid, and deſtroy all its ſolvent powers: yet even here the acid only lurks and conceals itſelf by its ſtrict union with an oppoſite ſalt. But art can again produce it to view, and reſtore all its former powers. If in this proceſs, five or ſix ounces of *ſpir. nitri*, or *aqua fortis*, were employed in the beginning, and, after the laſt ſtep, the liquor is diluted with ſome more water, ſtrained thro' brown paper, fully ſatiated with the alcaline liquor or ſalt, and then ſlowly evaporated and chryſtallized: there will be procured a ſalt perfectly reſembling common ſalt-petre, in the ſhape of the chryſtals, exploſive quality, and other marks. This, factitious ſalt-petre, then may be employed in place of the common; and I diſtilled ſome ounces of a ſalt, made much in the ſame way (for I imagine the intermediate ſteps will

will make no alteration) with oil of vitriol, in *Glauber*'s manner, and obtained a ſtrong ſmoking ſpirit, which anſwers all the characters of *Glauber*'s ſpirit of nitre, and diſſolves the ſame bodies; and therefore may run thro' the ſame courſe, over and over, without any change, unleſs that it may be gradually waſted, ſome part of it being diſſipated in every operation.

I found the ſame thing hold in other acid liquors, as ſpirit of ſea ſalt, ſpirit of vinegar, the acid of tartar, *&c.* which I joined to alcaline ſalts, and recovered the acid by diſtillation. Theſe and ſome other experiments of the like nature, which I have not yet had leiſure to conſider in their full extent, I ſhall take another opportunity to lay before the ſociety. In the mean time I ſhall proceed to make ſome

Remarks on Precipitation.

By precipitation is meant the parting or ſeparating the ſmall particles of a diſſolved body from the ſolvent liquor by means of a third body ſolid or fluid, which is therefore called the precipitant.

There

There is indeed another manner of parting the diſſolved body from the ſolvent, which is ſometimes, tho' leſs properly called precipitation; when, by drawing off part of the ſolvent liquor by evaporation or diſtillation, the diſſolved body falls down gradually in proportion to the quantity of the liquor waſted.

I ſhall chiefly conſider the firſt or more proper precipitation, in which there is both greater variety, and more difficulty to account for the change.

Remark I. All precipitations do not happen from the oppoſition of an *alcali* to an *acid*; but there are different means of precipitating different ſolutions.

It is certain, from experience, that whatever bodies are capable of being diſſolved in any acid liquor, may be precipitated from it by an alcaline ſalt or liquor. On the other hand, any body that is diſſolved in an alcaline liquor may be parted from it by the addition of an acid. This then is a pretty general method of precipitating: however this oppoſition of acid and alcali is not the ſole cauſe of precipitation; for many metals and metallic

metallic ſubſtances diſſolved in their proper acid *menſtrua*, may be precipitated by other bodies that are not alcaline, ſome by plain water, ſome by neutral ſalts, natural or artificial; ſome by another metal, ſome by another acid liquor; and, laſtly, ſome of theſe ſolutions may be precipitated in two or three different ways; of all which I ſhall give inſtances afterwards. But further, acid and alcaline liquors are not the only ſolvents; for water, ardent ſpirits, oils, and ſeveral compound liquors, neither acid nor alcaline, diſſolve a great many different bodies, which likeways may be ſeparated from them by precipitants of different qualities. Therefore an acid and alcali do not always concur to make a precipitation.

II. THE weakening or diluting ſolutions, by a liquor ſpecifically lighter than the ſolvent, is not the ſole cauſe of precipitation.

SOME who would account for precipitations upon mechanical principles, contend that diminiſhing the ſpecific gravity of the ſolvent liquors is the cauſe of all precipitations: but this, altho' it obtains in ſome inſtances, yet, in many more, it will be found contrary to experience. If any ſalt is diſſolved in water, and if ſpirit of wine, a liquor ſpecifically lighter than water, is added to the ſolution; no precipitation will enſue.

When ſilver is diſſolved in good *aqua fortis*, the ſolution may be diluted with fair water to any degree you pleaſe; yet the particles of ſilver continue ſuſpended in the liquor, tho' rendered ſpecifically lighter: whereas, when the thineſt and lighteſt parts of the ſolvent are carried off by evaporation, the ſilver falls down. In the ſame manner, a ſolution of quick-ſilver in *aqua fortis*, a ſolution of gold in *aqua regia*, of lead in ſpirit of vinegar, and ſome other ſolutions of metallic ſubſtances, may be diluted with ſimple water without precipitation. Further, ſometimes a liquor ſpecifically heavier than the ſolvent is added to the ſolution, whereby the ſpecific gravity of the compound liquor is increaſed; yet the particles of the diſſolved body are no longer ſuſpended in it, but fall to the bottom, quite contrary to this hypotheſis. When roſin of jalap is diſſolved in highly rectified ſpirit of wine or *alcohol*, if water, which is ſpecifically heavier than the ſpirit, is poured in, the reſinous particles can no longer be ſuſtained. The ſublimate corroſive mercury

mercury can be diſſolved in lime-water or common water: upon pouring in oil of vitriol, a very ponderous acid liquor; the mercury ſubſides. Coral diſſolved in ſpirit of vinegar, will be thrown down by the ſame oil of vitriol. Theſe inſtances naturally lead us to another remark.

III. SOME bodies diſſolved in one acid liquor may be precipitated by another acid.

To the two former examples of this manner of precipitation, I ſhall add ſome others. Silver, diſſolved in *aqua fortis*, will be thrown down, if *ſpir. ſal. marini* is put into the ſolution: on the other hand, a ſolution of gold in *aqua regia*, ſuffers a precipitation by the affuſion of *ſpir. nitri*. That corroſive acid liquor, called *butyrum antimonii*, is known to be a ſolution of the metallic part of antimony in *aqua regia*: if we pour into it *ſpir. nitri*, after a pretty ſtrong efferveſcence, we ſhall find a metallic powder at the bottom.

IV. SOME metals diſſolved in an acid liquor, may be precipitated thence by another metal or metallic, ſtony or calcarious ſubſtance, capable of being diſſolved in the ſame *menſtruum*.

UNDER

Under the head of ſolution, I had occaſion to adduce a well known inſtance, to ſhow that a *menſtruum* having diſſolved one body, will ſucceſſively diſſolve ſeveral others in a certain order. I am obliged to mention the ſame again, as a confirmation of the preſent remark. If pure ſilver is diſſolved in proof *aqua fortis*, and the ſolution diluted with fair water; the ſilver will be precipitated by copper, the diſſolved copper by iron, and that by lime-ſtone. This however is not the ſole inſtance of this way of precipitation: others may be found. The ſublimate corroſive mercury may be juſtly looked upon as a ſolution of quick-ſilver in the acid of ſea ſalt or *aqua regia*, brought into a chryſtalline or vitriolic form by ſublimation: when this corroſive mercury is mixed with an equal weight of common antimony, and theſe rubbed together, left to moiſten in the air, and then diſtilled; there happen ſuch changes to this mixture, as evidently ſhow a real precipitation of the quick-ſilver by the reguline part of the antimony; for the acid forſakes the quick-ſilver, and diſſolves the *regulus*, which makes the *butyrum antimonii;* and the quick-ſilver freed from the acid, is by the fire incorporated

incorporated with the ſulphur of antimony into a black powder like *Æthiops mineralis*, which may be ſublimed into the *cinnabaris antimonii*. Quick-ſilver diſſolved in *aqua fortis*, may likeways be precipitated by another metallic body, as zink or biſmuth.

V. Some bodies diſſolved in their proper *menſtrua*, may be precipitated thence, by ſeveral bodies of different qualities.

WE meet with a great many inſtances in chemiſtry to confirm this remark. I ſhall mention a few examples, in which the greateſt variety obtains. Pure ſilver being diſſolved in good *aqua fortis*, and the ſolution diſtributed into ſix or eight glaſſes, we may make as many precipitations by different precipitants; for ſolutions of ſea-ſalt and crude ſalt ammoniac, ſpirits of ſea-ſalt, vitriol and ſalt ammoniac, a fixt alcaline ſalt, or its lee, and a piece of copper, will, each of them ſeparately, make a precipitation of the ſilver.

QUICK-SILVER diſſolved in *aqua fortis*, may be precipitated by a ſolution of ſea ſalt, by a ſolution of crude ſalt ammoniac, by freſh urine, by ſpirit of ſalt ammoniac, by *ol. tart. p. d.* and by zink.

REGULUS

Regulus of antimony diſſolved in *aqua regia*, will be precipitated by plain water, by a ſolution of ſalt-petre, by ſpirit of nitre, and, by the alcaline liquors, fixt and volatile.

VI. The matters precipitated from the ſame ſolution, by different precipitants, differ from one another in ſeveral remarkable qualities.

When either a muria of ſea-ſalt, a ſolution of ſalt ammoniac, or ſpirit of ſea-ſalt (for theſe three have nearly the ſame effects) is put into a ſolution of ſilver; it becomes milky, and, as it were, cruddled, and, at length, a white powder ſubſides to the bottom: this powder being waſhed with warm water and dried, is ſoft and impalpable; its weight exceeds that of the ſilver diſſolved, by more than on ſixth part. This powder comes much ſooner to fuſion than ſilver, but does not recover the appearance or properties of that metal; for it looks like a piece of yellowiſh glaſs, ſemi-opaque and brittle, yet bending or yielding a little, whence it gets the name of *luna cornea*, or horny ſilver; and it is with great difficulty that it can be reduced to true malleable and ductile ſilver.

When

When a plate of copper is put into a solution of silver diluted, the surface of the copper is immediately covered, as it were, with a coat of silver; this becomes gradually thicker, and appears tufted, spungy and loose: upon shaking the plate, large pieces of this crust fall off, which either rise to the top, or swim about in the liquor for a while, till discharging many bubbles of air, they grow heavier, and fall down in form of a white powder; the silver being in this manner separated from the liquor, then washed and dried, is nearly of the same weight with the silver dissolved; and, when melted, runs into a lump of pure malleable silver, without any considerable loss.

If an alcaline liquor, as *spir. sal. ammoniaci*, or *ol. tartari p. d.* is dropt into a solution of silver, it excites a strong effervescence, which will be renewed when more of the alcaline liquor is dropt in at intervals, until the acid is satiated; and there appears a white, bulky and frothy matter, which gradually subsides: all being thrown into a cap of brown paper, fitted to a funnel, and warm water poured in, the frothy matter diminishes considerably in bulk; and there is left a

powder

powder of an ash colour, which being dried, a little exceeds the weight of the silver dissolved.

The precipitations of mercury shew still a greater variety, at least, as to their effects, being used as medicines. If sea salt, dissolved in a good quantity of water, is put into a solution of quick-silver in *aqua fortis*, it turns the clear and limpid solution turbid, opaque and milky; at length the white matter subsides, which being separated from the fluid, washed and dried, remains a very white powder, and a little exceeds the weight of the quick-silver dissolved: but it is to be observed, that the *muria* does not precipitate all the mercury; for the liquor, thus separated from the white powder, is known to have the effects of a cosmetic or detersive mercurial water; besides if *ol. tart. p. d.* is dropt into this liquor, a small quantity of a brown mercurial powder will be precipitated. Physicians know that this white precipitate is a pretty active and brisk medicine, and produces greater effects, even in a smaller dose, than *calomel*, but is more mild and tractable than the *merc. sublim, corrosivus*; for, when this last is somewhat tempered by *spir. sal. ammon.* it becomes

comes a medicine much of the ſame nature with the preceeding white precipitate. A ſolution of crude ſalt ammoniac, and *ſpir. ſal. mar.* have the ſame effects on the ſolution of quick-ſilver, as the *muria* has.

If a little freſh urine is put into a ſolution of quick-ſilver in *aqua fortis*, there will likewiſe happen a precipitation; but the powder is not white, but of a pink or fleſhy colour, it appears pretty bulky, and has acquired a little additional weight. This *precipitate* is not uſually preſcribed by phyſicians: but M. *Lemery* pretends that it is milder than the white, and only operates by ſtool.

It would appear then, that, in many inſtances of precipitation, the particles of the diſſolved body are increaſed in denſity and ſpecific gravity; by reaſon that ſome parts either of the precipitating matter only, or of the compound, made up of the ſolvent and precipitant, are very firmly united to them, ſo that they cannot longer be ſuſtained in the liquor. But, in other caſes, where the matter precipitated has acquired no addition of weight, no new or remarkable properties, and is eaſily reducible to the native form of the body diſſolved; there muſt be ſome other cauſe of precipitation, which may probably be, that there is a greater affinity betwixt the precipitating body and the ſolvent, than betwixt this and the body diſſolved; whence, from the diſpoſition of theſe bodies to unite, they ſhake off, or drive away from them, the particles already joined to the ſolvent.

ART.

ART. XI.

Experiments on Neutral Salts, compounded of different acid Liquors, and alcaline Salts, fixt and volatile; by the ſame.

* AT a former meeting of the ſociety (*January* 1738) I read ſome remarks on chemical ſolutions; the laſt of which gave me occaſion to mention ſome experiments, in which I was then employed, but had not finiſhed: and therefore, at that time, I could not conſider them in their full extent, or have a juſt view of all the conſequences that might be drawn from them.

I now proceed to relate the experiments, and to obſerve how far they may be uſeful to extend our knowledge.

I prepared a parcel of factitious, or (as the Chemiſts are pleaſed to call it) regenerated nitre, in this manner; I took two ounces of an extemporaneous fixed alcaline ſalt, made of equal parts of nitre and tartar; I diſſolved

* *June* 7. 1739.

diſſolved this ſalt in warm water, filtred the ſolution, and poured into it ſtrong ſpirit of nitre, a little at a time, until the alcaline liquor was perfectly ſatiated with the acid, which required one ounce and a half of the ſpirit of nitre. From this ſaline liquor, by evaporation and chryſtallization, I procured two ounces and a half of ſalt, when it was all collected and well dried. The chryſtals were long, ſlender and priſmatical; the ſalt had a pretty mild and cooling taſte; and bits of it put upon a burning coal, made it ſparkle and give a bright flame.

We may obſerve by the way, that the compound ſalt exceeded the weight of the fixed ſalt by half an ounce or 240 grains; this additional weight it procured from the ſpirit of nitre: therefore one ounce and a half, or 720 grains of the ſpirit of nitre which I uſed, contain 240 grains of true acid, which is capable to ſatiate four times its weight of alcaline fixt ſalt.

I put two ounces and a half of this factitious nitre into a ſmall glaſs retort, and poured upon it one ounce of oil of vitriol, which immediately begun to raiſe an ebullition with heat and red fumes; a receiver was inſtantly joined

joined to the retort with paſte; the glaſs ſet in a ſand furnace, and the fire increaſed gradually. After the operation, there was found in the receiver, a ſmoaking acid liquor, of an orange colour, which weighed one ounce, two drams, three grains: there remained at the bottom of the retort, a cake of white ſolid ſalt; which being diſſolved in warm water, the ſolution filtered, and part of the water again exhaled, ſhot into chryſtals; and theſe being all carefully collected, weighed two ounces, twenty two grains.

The ſpirit obtained in this manner was fiery, active, and conſtantly emitting red fumes; it diſſolved ſilver, kindled with oil of cloves, and agreed in all other characters with ſtrong ſpirit of nitre. The ſalt which remained in the retort when purified and chryſtallized, no way reſembled ſalt-petre, or the ſalt employed in this operation, in the ſhape of the chryſtals, taſte, or other properties; for theſe chryſtals were polyhedras, having ſeveral triangular faces and ſolid angles; their taſte is rough, bitter and ſomewhat aſtringent; they do not readily melt or ſparkle in the fire, as common or factitious nitre does.

I made another factitious ſalt, by impregnating a ſolution of the ſame alcaline fixt ſalt with ſpirit of ſea ſalt, pouring it in by degrees, till there appeared no effervefſcence, and the liquor taſted briny. Four ounces and a half of this ſpirit were employed to ſatiate two ounces of the fixt ſalt: when the two ſaline liquors were duly proportioned, there appeared, before evaporation, a great many ſmall grains of white ſalt at the bottom which, as well as the liquor ſurrounding them, had the true muriatic taſte.

The ſalt obtained out of this liquor, had the genuine figure of the common ſea ſalt; but the chryſtals were ſmaller, more hard, white and opaque: the ſalt is not altogether ſo pungent as what we commonly uſe; but more agreeable and cooling, yet without any acidity. This ſalt, while moiſt, weighed two ounces, ſix drams and a half; but, when well dried, and ſlightly decrepitate, the weight was reduced to two ounces and two drams. I put this quantity of powdered ſalt into a ſmall glaſs retort, and poured upon it two ounces of ſtrong ſpirit of nitre, which occaſioned an ebullition with heat and ſmoke. I proceeded to the diſtillation, with a fire gra-

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dually increaſed; when the veſſels were cooled, I found in the receiver, two ounces four drams and a half, of an acid liquor, of a faint greeniſh colour, which ſmoked much when the veſſels were firſt disjoined; but the fumes were paler and whiter than thoſe of ſpirit of nitre, and ſooner ceaſed. I put a few drops of this liquor into a ſolution of ſilver in *aqua fortis;* it made it white and cruddled, and at length a white powder fell down. I poured a ſmall quantity of it upon a few grains of filings of gold; with the aſſiſtance of a little heat, the ſpirit pretty quickly diſſolved it, leaving only a few particles at the bottom. From theſe characters I think we may conclude, that the acid liquor, produced in this operation, is a true *aqua regia:* I chuſe rather to call it ſo, than ſpirit of ſea ſalt; becauſe I found it had a greater and quicker effect in diſſolving gold than the plain ſpirit of ſalt had, therefore it is probable, that a ſmall quantity of the nitrous acid had accompanied the acid of ſea ſalt.

THERE remained in the retort, a cake of ſolid white ſalt, like melted nitre, or *ſal prunellae:* after it had been diſſolved in warm water, the ſolution filtered and evaporated; there

there appeared many regularly ſhaped chryſtals, long, ſlender, and priſmatical, of the nitrous kind; which being removed, and more of the moiſture exhaled, the chryſtals which formed were not ſo large nor ſo regularly ſhaped as the peceeding, but the whole appeared like a maſs of white ſalt coarſely powdered: however I could diſcover, among theſe chryſtals, ſome that had a quadrangular baſe, but long ſides, like two cubes joined, others variouſly compounded or broken: even this part of the ſalt had the cooling nitrous taſte; and, part of it put upon a live-coal, ſparkled and excited a bright flame like ſalt-petre.

Again, I took two ounces of that neutral ſalt, called *tartarus ſolubilis*, the preparation of which is very well known; in this compoſition, the native acid of white tartar, or the chryſtals of tartar, is impregnated with an alcaline ley, or *ol. tart. p. d.* from the union of theſe in due proportion, ariſes a mild neutral ſalt, which diſſolves more eaſily in water, than tartar or its chyſtals; whence it hath the epithet of ſoluble. I put two ounces of this preparation into a ſmall glaſs retort, and poured upon it one ounce of oil of vitriol

vitriol, which raiſed a bubbling and efferveſcence. In diſtillation there came over ſix drachms of liquor, of a browniſh colour, with a little oilineſs upon it: its taſte was ſowre, but diſagreeable and empyreumatic; it had an unpleaſant ſmell, but not very ſtrong or pungent; it efferveſced with alcaline ſalts fixt or volatile: in all which properties it agrees with the acid liquor drawn by diſtillation from crude tartar, which is commonly called its *ſpirit*. I obſerved, in diſtilling this mixture, that there came up ſome volatile ſalt, but in very ſmall quantity, which ſpread itſelf upon the neck of the retort, in very fine and ſubtile ramifications: the upper part of the body of the retort was ſullied with ſome thick black oil; and the ſaline matter at the bottom was of a dirty aſh colour, like tartar half-burnt. I poured warm water into the retort, and left it on hot ſand, to diſſolve the ſalt; I found it neceſſary to paſs this ſolution through a filter, and there remained in the paper a good deal of black earthy matter. The liquor was very acid, and had a vitriolic roughneſs, becauſe more oil of vitriol had been poured upon the *tartarus ſolubilis* than was ſufficient: to overcome the acidity, I dropt into the liquor some *cl. tart. p. d.* till the effervescence ceased; and then, by evaporation and chrystallization, I procured a salt, in every respect, like the *tartarus vitriolatus*, as usually prepared.

Upon two ounces of the same *tart. solubil.* I poured one ounce of spirit of sea salt, made after *Glauber's* manner; by distillation there was produced a liquor of the same kind, with that of the preceeding experiment. The remaining salt indeed was not dissolved nor chrystallized; but from its taste, and by its precipitating a solution of silver in *aq. fort.* it appeared to to be of the muriatic kind.

After this, I made an experiment on another neutral salt in which the acid of vinegar is united to the alcaline salt of tartar, which is commonly known by the name of *tartarus regeneratus*. I took one ounce and an half of salt of tartar, and poured distilled vinegar upon the dry salt at several times, shaking the glass frequently, till fifteen ounces of the acid were employed: the vessel was then set on a sand-bath, and the liquor gradually inspissated till there remained four ounces 27 grains of a moist, greasy, saline substance, of a brownish colour, like coarse sugar.

ſugar. I did not think it for my preſent purpoſe to beſtow more time and pains to bring this ſalt to a greater degree of whiteneſs and dryneſs; but I put it into a retort, and poured upon it one ounce and an half of oil of vitriol, diluted with three ounces of water: in diſtillation there came over near ſeven ounces of a thin limpid acid liquor, which, in taſte, reſembled diſtilled vinegar, but was more ſtrong. The ſaline maſs left in the retort being diſſolved in warm water, the ſolution filtred and evaporated, the chryſtals could not be diſtinguiſhed from the ordinary *tartarus vitriolatus* in ſhape, colour, or taſte.

To ſatisfy myſelf, that a volatile alcaline ſalt or ſpirit may likewiſe be diſengaged from an acid to which it has been united, and reſtored to its former ſtrength and qualities, I made the following experiment. I took eight ounces of that compound ſaline liquor, which is deſcribed in our College diſpenſatory under the title of *Spiritus Mindereri*; it is made by pouring diſtilled vinegar upon volatile ſal-ammoniac, until the ſalt is diſſolved and ſatiated by the acid. This neutral ſaline liquor, however, will not chryſtallize or coaleſce

alesce into a dry solid salt, nor will the volatile *alcali* separate from the acid, by a simple distillation, but they rise together. I put eight ounces of this spirit into a bolt-head; and, adding half an ounce of dry salt of tartar, I presently fitted a capital and receiver to the body, luted the joinings, and set the vessels in a sand heat; in a little time a white volatile salt sublimed into the capital, which was soon washed down by a watery vapour into the receiver. This liquor being tried, was found equally strong, pungent, clear and volatile, as the best spirit of sal-ammoniac: the saline matter left in the body was of a greyish colour, soft and greasy to the touch, and would hardly dry or chrystallize, resembling *tartarus regeneratus*.

I need not confirm this by any further experiment, because it is known to every body, that the volatile salt and spirit of sal-ammoniac are procured by adding a fixt alcaline salt to the crude sal-ammoniac: now, as we can prepare an extemporaneous or artificial salt, by impregnating the volatile salt or spirit of sal-ammoniac with the spirit of sea salt; we have no reason to doubt, but, by means

of

of a fixt alcaline ſalt, we may again diſengage the volatile ſalt or ſpirit from the acid.

But it may be of more importance to diſcover what kind of acid is joined to the volatile alcali in the common or crude ſal-ammoniac: for this purpoſe I made the following experiment. When all the volatile ſalt and ſpirit are drawn off from crude ſal-ammoniac by the addition of a fixt alcaline ſalt and water, there remains a ſaline maſs; which, when diſſolved in warm water, the ſolution ſtrained and evaporated, the chryſtals very much reſemble thoſe of ſea ſalt, in ſhape, taſte, and other diſtinguiſhing characters of that ſalt, eſpecially when it is well depurated and carefully chryſtallized. This ſalt, however, has been dignified with the title of *ſal febrifugum Sylvii.* I put three ounces of it into a retort, and poured in one ounce of oil of vitriol, and as much water. In the diſtillation, the receiver was filled with white fumes, which condenſed into a liquor of a pale colour, ſomewhat turbid at firſt, of which I had near three ounces. The liquor had all the appearances of the ſpirit diſtilled in *Glauber*'s manner, from common ſea ſalt; and when ſome of it was dropt into the ſolutions

tions both of ſilver and quick-ſilver, it turned them both milky, and made a white precipitation. From all which I have no difficulty to conclude, that the acid contained in crude ſal-ammoniac is of the muriatic kind, or the ſame which is produced from ſea ſalt. But, whence this acid ſhould proceed, is a matter of greater intricacy; ſince we are aſſured that no ſea ſalt is neceſſary to, nor employed in, the preparation of that ſal-ammoniac which is brought to us from *Egypt*.

THIS queſtion I ſhall not undertake to diſcuſs, as being foreign to my preſent purpoſe; but ſhall ſubjoin ſome remarks on the experiments above related, or deduce ſome corollaries which ſeem naturally to ariſe from them.

I. THE acid ſpirits, as thoſe of vitriol, nitre, ſea ſalt, vinegar, &c. are ſaline liquors, or fluid ſalts, made up of the very minute particles of an acid ſalt, and a certain proportion of plegm or water.

II. THE particles of theſe acid ſalts cannot be brought to unite, among themſelves, by chryſtallization or ſublimation, ſo as to appear in the form of a ſolid or dry ſalt: but they

they readily lay hold on the particles of certain metals or ſemi-metals, calcarious or abſorbent earths, or oppoſite ſalts, fixt or volatile, which ſerve them as the baſis; and, joined to theſe, they aſſume a chryſtalline form.

III. When any of the acid liquors are joined, in due proportion, to any alcaline ſalt, whether fixt or volatile; the particular acrimony and pungency of both, as to our ſenſes, and their power of diſſolving certain bodies, in a ſeparate ſtate, are quite deſtroyed, at leaſt, overcome and ſuſpended, by their union.

IV. When an acid liquor has diſſolved a metallic body, and is united to it; the ſolution and the vitriol made of it, are frequently more acrid and corroſive, with reſpect to the human body, than the acid by itſelf. I do not affirm this univerſally: but we have inſtances of it in the ſolutions and vitriols of ſilver, quick-ſilver, copper, antimony.

V. When the ſpirits of nitre or ſea ſalt are joined to ſalt of tartar, or pot-aſh, and chryſtallized; the chryſtals aſſume the peculiar ſhape, and moſt remarkable properties of the natural ſalt, from which the acid ſpirit

rit was originally produced. Consequently there appears to be some affinity betwixt the native fixt bases of these salts, and the fixt salts of plants.

VI. The artificial vitriols compounded of the same acid spirits and metallic particles, do not resemble the original salts; but the vitriols of different metals, or metallic bodies, have different shapes, tho' they are joined to the same acid salts.

VII. The acid salts, tho' combined with the alcaline, into mild neutral salts, yet are not so changed by this union but, when they are again artfully disjoined, they may exert the same powers and produce the same effects as before their union: therefore we may conclude, that the figure, bulk, and density of the acid particles are not changed, tho' the liquors may differ somewhat in strength from the greater or smaller proportion of phlegm, or water accompanying the real acid.

VIII. One acid which has been united to an alcaline salt may be set at liberty, or expelled from its cohesion with the alcaline salt, by another acid of a different origin. But this does not happen indiscriminately; for

for the oil of vitriol expels any other acid, but cannot be diſlodged by any of the reſt. A volatile alcaline ſalt is expelled from its union with an acid, by a fixt alcaline, but not contraryways.

General Scholium.

We can have no other idea of a ſolid or conſiſtent body in general, as diſtinguiſhed from a fluid, than that the parts of a ſolid cohere together by ſome power, which retains the particles in their ſituations, with reſpect to one another, and makes them to reſiſt their ſeparation or diviſion, till that power is overcome by a ſuperior force.

When we obſerve that a certain fluid, as water, poured upon a hard ſolid body, as a lump of ſalt-petre, in ſome hours, makes ſuch a change upon the lump, that it wholly diſappears, if the glaſs is ſhaken a little, that is, the ſalt is divided into particles, ſo ſmall, that they eſcape our ſight, and are equally diſtributed through the liquor, and ſuſpended in it; when we obſerve this, I ſay, we cannot but conclude, that the particles of water are endued with a power capable to overcome the force by which the particles of ſalt-petre cohered with one another.

If a known quantity of cold water has diſſolved as much ſalt-petre as it is capable to hold, in this ſolution the water is to the ſalt as $5\frac{1}{3}$ to 1; (in ſcalding water the proportion is as 4 to 1). If then $\frac{4}{5}$ of the water is waſted by evaporation, many of the ſmall particles of nitre being brought into contact, join together, and form chryſtals in ſhape and other properties reſembling the nitre which was diſſolved in the water. It follows, therefore, that the particles of nitre mutually attract one another, when the interpoſing matter is removed, the diſtance betwixt them very much diminiſhed, and no ſuperior power acting upon them; and, when in contact, they cohere with a certain degree of force. Again, it appears, that the particles of water do attract the particles of nitre more ſtrongly than theſe laſt attract one another; and, conſequently, overcome their coheſion, and ſuſpend their mutual attraction. Laſtly, it is evident, from the above appearances, that tho' the parts of nitre which ſwim in the water are ſo ſmall, that they are

are not perceptible ſingly, even when viewed thro' a pretty good lens; yet they muſt be ſimilar to one another, and retain the properties of real nitre.

LET us next conſider the effects of another liquor, upon the ſame ſalt-petre.

IF oil of vitriol is poured upon a parcel of nitre, there happens likewiſe a ſolution, but with appearances different from thoſe in the former; for this is accompanied with a conſiderable inteſtine motion and expanſion, ſmoak and heat, and produces a much greater change on the nitre: for, by the help of external heat and cloſe veſſels, there riſe from the mixture copious red ſteams, which are condenſed into a corroſive acid liquor, very active and volatile, and there is left a white ſaline maſs, no way reſembling nitre.

FROM what has been ſaid formerly of this experiment, it appears that the oil of vitriol makes a more minute and ſubtile diviſion of nitre than what ſimple water can do; that the fluid acid ſalt of the oil of vitriol attracts one part of each real particle of nitre, *viz.* the fixt and alcaline baſis, while this ſame vitriolic acid ſeems to repell another part of nitre, that is the acid and volatile

part,

part, which attracts, or is attracted by the phlegm or watry part of the mixture, is easily raised up in vapour by heat, and turned into a saline acid liquor.

It may be justly questioned, whether this is an ultimate division of nitre, or a separation of it into its most simple principles or first elements. It is very hard to find an experiment that can give full satisfaction in this point; for there are certain bounds set to art, as to the division of bodies: but if that part of nitre, which unites with the acid of vitriol into a neutral salt, is the same with nitre fixt by deflagrating with char-coal; or if it is no other than that salt which can be extracted from the remains of the distillation of nitre with bole; then we have some reason to conclude, that this part of nitre is not a simple elementary matter: since fixt nitre can, by frequent solutions and fusions, be so changed, that little of it will remain, but a mere earth.

Many of the solutions which happen in chemistry, are of the first kind, that is, the solvents act no otherways upon their proper objects than water does upon nitre or any other salt, by dissolving the cohesion of the

larger

larger particles which compose the body, without resolving the particles into their primary elements.

When spirit of nitre or *aqua fortis* is poured upon pieces of silver, the appearances plainly point out that there is an attraction betwixt the particles of the solvent and the silver, and that there is likewise a repulsion of the particles already dissolved from the silver, to make way for the application of new particles of the solvent to other parts of the silver; for there is a remarkable motion and agitation in the liquor, the surface of the silver soon becomes rough and discoloured, air-bubbles are formed upon it, which soon fly off, and a constant succession of these arise, break and diffuse themselves: if a little of this liquor is carefully taken off from the top, even before half the silver is dissolved, and dropt into a *muria* of sea salt; it will give evident marks that it contains a portion of silver. When all the metal is dissolved, the solution becomes clear and limpid, and all is at rest. These things evidently shew, that such metallic particles as are freed from their cohesion by the saline parts of the *menstruum*, recede with them from the silver, and are ſucceeded by other acid particles to perform their part of the operation, until all the parts of the ſolvent are equally charged with particles of ſilver.

THE attractive force between the ſaline particles of the ſpirit of nitre and the ſilver, muſt be ſuperior to the power by which the particles of ſilver attract one another; for it diſſolves their coheſion, ſeparates them, and keeps them ſuſpended in a fluid which has only about one tenth part of the ſpecific gravity of ſilver: yet theſe particles, thus ſuſpended, differ in nothing, when freed from the acid, but bulk from granulated ſilver or the bits rubbed off plate by a fine file. That this power of attraction between the particles of ſpirit of nitre and thoſe of ſilver muſt be very great, will appear, if we conſider, firſt, that it requires a very great heat to melt ſilver; but this melting is nothing elſe, than by the force of fire, to overcome the coheſion among the particles of ſilver, and bring the maſs to a ſtate of fluidity, ſo that the particles may eaſily move and ſlide one upon another while that degree of heat laſts; but ſpirit of nitre produces the ſame or rather a greater effect. In the next place, tho' ſpirit

rit of nitre is of itſelf pretty volatile, yet, when it has diſſolved ſilver, the ſaline parts adhere ſo firmly to the metallic, that a very ſtrong degree of heat is required to ſeparate them; for, if a ſolution of ſilver is ſet in a heat that would make water boil, the light and watry parts indeed fly off, but afterwards the evaporation goes on ſlowly, unleſs the fire is conſiderably increaſed; and, after ſeveral hours, the matter has ſtill the appearance of a very moiſt ſalt, which is very corroſive, and, in bulk and weight, greatly exceeds the ſilver that was diſſolved. When the ſand-heat can make no further change on the maſs, put it into a crucible, and ſet it in a pretty briſk and clear fire: this expells the remaining moiſture and ſuperfluous acid; after this the matter melts like wax, and, when cold, is turned to a hard ſubſtance; the cauſtic quality of which, the increaſe of weight above that of the ſilver, and its aptneſs to moiſten, ſhew, that, after all the heat which the maſs has ſuſtained, there is ſtill a good quantity of the ſaline part of the ſpirit of nitre intimately incorporated with the ſilver. Now, when a piece of this hard ſtony ſubſtance is put upon a piece of well kindled

char-coal

char-coal, and blown a little, it makes an explosion with a ſparkling and flame, like a piece of common ſalt-petre; the ſaline matter is conſumed or turned into flame or ſmoke, and the pure ſilver is left on the coal. By the way, this may be ſome ſort of proof, that the volatile acid part of nitre is not more a ſimple elementary matter than is the fixt part.

NOTWITHSTANDING what has been ſhewn, concerning the ſtrong attraction between the particles of ſpirit of nitre and thoſe of ſilver, and the difficulty of ſeparating them by the force of fire; yet, if a piece of clean copper is put into a ſolution of ſilver in ſpirit of nitre, the ſaline particles moſt readily ſhake off the ſilver, and diſſolve the copper: which ſhews that the particles of copper have a ſtronger attractive power, with reſpect to the ſpirit of nitre, than the particles of ſilver have; which difference of attraction will probably ariſe from the different bulk, figure or denſity of the particles of the two metals, the acid being the ſame. The ſame thing may be ſaid of iron, zinc, chalk, volatile alcaline ſalt and fixt ſalt, each of which attract ſpirit of nitre more ſtrongly than

than ſilver does, and the laſt more than the preceeding.

HERE then we have a ſeries of bodies from ſilver to ſalt of tartar, whoſe attractive powers, with reſpect to the ſame ſaline liquor, are continually increaſing; but, whether uniformly or in any certain proportion, is not eaſy to determine: there may be found a body which attracts the acid of nitre more than the firſt, and leſs than the ſecond, and ſo thro' other degrees; which can only be aſcertained by a great variety of trials.

As the ſalt of tartar, or any ſtrong fixt alcaline ſalt, is the ſtrongeſt attracter of the nitrous acid, and throws down or ſeparates any other body that has been diſſolved in it; ſo it likeways attracts every other acid, and disjoins it from whatever it has diſſolved: therefore we may conſider *that* ſalt as a ſtanddard whereby to compare the ſeveral attractive powers of the different acid liquors. But it is certain, from the experiments that have been related, that, if three parcels of the ſame fixt alcaline ſalt, are joined to the three different acids of nitre, ſea ſalt, and vinegar, and if oil of vitriol is poured upon each of theſe ſaline mixtures or compound ſalts; the vitriolic acid will attract, or be attracted more ſtrongly by, the alcaline ſalt than the other acids, which will be driven from their places; and the acid of vitriol will be ſo firmly united to the fixt ſalt, that no other acid or known body is capable to ſeparate them.

AGAIN, If ſpirit of nitre is poured upon a compound ſaline liquor, or ſalt made of ſpirit of ſea ſalt and ſalt of tartar, and upon another made of ſpirit of vinegar and the ſame ſalt of tartar; the ſpirit of nitre, by its ſuperior attractive power, will join itſelf to the alcaline ſalt, and drive away the other acids; but is not able to reſiſt the ſtill greater force of the vitriolic acid.

LASTLY, If ſpirit of ſea ſalt is poured upon a compound of ſpirit of vinegar, or any other vegetable acid and a fixt alcaline ſalt; this will be more ſtrongly attracted by the acid of ſea ſalt, than by the vegetable acid which will be expelled: but the ſpirit of ſea ſalt may likewiſe be diſlodged by the acids of vitriol or nitre.

IN moſt ſolutions, precipitations, and other operations by which the texture of bodies are much changed, there are ſome appearances, ſuch as an expanſion and bubbling, the production

duction of an elaſtic fluid like air, and a commotion ſo great, as to produce ſome remarkable degree of heat, and in ſome caſes ſmoak and flame: which appearances, I ſay, plainly diſcover, that the ſmall particles of bodies are endowed with ſome very active powers, capable to introduce certain motions tending to union or disjunction, attraction or repulſion.

THAT many parts of matter are endowed with an attractive power tending to union and coheſion, may be deduced from the inſtances already mentioned: and many more might be brought from occurrences in chemiſtry to confirm the ſame. That other parts of matter are endowed with an elaſtic or repelling force, may be concluded from the properties of air and light, demonſtrated by the greateſt philoſophers of the laſt and preſent age. The curious experiments of the ingenious Dr *Hales*, in his *Analyſis of the air*, and the ſurpriſing experiments on electricity now ſo much in vogue, may ſatisfy us, that particles of the ſame nature and qualities with thoſe of air and light, may be joined to particles of a groſſer nature and attractive quality, in various maſſes of matter, to ſome adhering more looſely, to others

thers more firmly connected. For ſome time they may lie concealed without manifeſt motion or action; their repelling or elaſtic power being overcome or balanced by the attractive force of the ſurrounding corpuſcles, till by certain concurring circumſtances, as external motion, heat, moiſture, the addition of other matter, *&c.* the attractive force is diminiſhed, or the repelling increaſed: then they are rouſed, as it were into action, and diſcover themſelves by great and remarkable effects, in changing the texture of the maſſes to which they were united. Such changes we daily ſee happen in fermentations, putrefactions, efferveſcences, ſolutions, accenſions, exploſions, *&c.*

THESE principles of motion in matter, are not the vain fictions of men merely ſpeculative in philoſophy, but evidently deduced from obſervations and experiments on a great variety of bodies in many different circumſtances; and every one who reflects upon the moſt ordinary occurrences in the works of nature and art, will be convinced of the exiſtence of ſuch cauſes, and find that they are ſo univerſal and unalterable, that they can only be referred to the firſt cauſe, that is, to the will of the Supreme Being.

ART.

ART. XII.

Experiments and Obſervations upon the Hartfell Spaw, *made at* Moffat 1750; *and an Account of its Medicinal Virtues, ſo far as they have hitherto been diſcovered from experience; by* WILLIAM HORSEBURGH, *M. D.*

THIS *Spaw* ſprings from the *Hartfell* mountain, about three miles north of *Moffat*; and is commonly called *Williamſon's water*, from Mr *John Williamſon*, who diſcovered it in 1748, and who has been at a good deal of pains, and ſome expence, to make it acceſſible, by mending the road, clearing away the dirt about the ſpring, and fitting it with a ſpout, that it may be more conveniently taken up.

THE water on which I made the following experiments, had been taken up three or four days from the ſpring, and was contained in bottles well corked and waxed; it was taken up on the 26th of *Auguſt*, after a very rainy ſeaſon.

1. IT was quite clear and pellucid.

2. IT,

2. It had a ſharp, aluminous, ſtrong chalybeate, and ſtyptic taſte.

3. It cruddles with ſoap.

4. Its ſpecific gravity to that of *Annan* river water (which is eſteemed the beſt water they have hereabout) is, as four ounces five grains, to four ounces eleven grains.

5. By ſhaking it in a vial, or mixing it with tartiſh wine and ſugar, it gives no ſigns of its containing a ſpirit more than river-water.

6. When boiled with an equal quantity of new or ſweet milk, it does not cruddle the milk.

7. With ſyrup of violets, it gives a faint green.

8. With powder of galls, it preſently became blue, and turned gradually darker, till it became of a deep purple, and then of an ink-colour: it does the like with an infuſion of tea, tho' not in the ſame degree as with the galls.

9. When *oleum vitrioli*, or *ſpiritus vitrioli*, is dropt into it, no efferveſcence, or change of colour, is produced.

10. With *oleum tartari per deliquium*, it made no efferveſcence; but light green clouds gathered,

gathered, and were ſuſpended, in the middle of the water; and a great number of ſmall air-bubbles, like particles of quick-ſilver, appeared adhering to the bottom of the glaſs, to its ſides, and upon the ſurface of the water; multitudes of the like air-bubbles were intermixed with the green clouds: after ſtanding about an hour, a great many of the air-bubbles diſappeared, and the clouds in the middle of the water grew thicker, and of a darker green, and afterwards precipitated.

11. AN aqueous ſolution of *ſaccharum Saturni*, turned it a little whitiſh, but not ſo much as it did the river-water; and much leſs then it did the river-water, when there was a very little ſea ſalt diſſolved in it; in all theſe three trials, a white powder precipitated.

12. ON *September* 1ſt, I poured a chopin or *Engliſh* quart of this water into a clean, white ſtone-bowl; and covered it with a piece of paper, to keep out the duſt.

ON the 2d, there was precipitated a brown powder, like brick-duſt; and, on the ſurface of the water, there were many ſhining ſpangles of a copper-like colour.

ON

On the 3d, there was more of the browniſh powder at the bottom; all the parts of the bowl, which were covered with the water, were tinged yellowiſh; a pellicle covered the ſurface of the water, and all the ſpangles were gone.

On the 6th, the ſharp aluminous taſte was pretty ſtrong, but the chalybeate taſte much weaker; there ſeemed to be no more of the browniſh powder at the bottom than there was on the 3d; the yellow ſubſtance that adhered to the bowl was alſo thicker, and had a ſmooth oily look; the pellicle on the top had now acquired the thickneſs of writing paper; it was eaſily taken up, as it adhered to any thing put into it, and was of a golden colour: I took up as much of the water as filled a wine glaſs; and, with powder of galls, it immediately ſtruck a blue.

On the 13th, the ſharp aluminous taſte ſtill remained ſtrong, but the chalybeate was ſcarce to be diſtinguiſhed; the quantity of the precipitate, and the pellicle on the top, were much the ſame; but the yellow ſubſtance, adhering to the bowl, was rather more: into a glaſs of this water, I dropt ſyrup of violets; it turned of a faint green: to another glaſs of of the ſame, I put powder of galls; it immediately ſtruck a blue tincture, but not ſo deep as on the 6th.

On *October* 9th, it had a more aluminous taſte, than the freſh *Spaw* water; the chalybeate taſte was ſcarce to be diſtinguiſhed; the precipitate, yellow ſubſtance adhering to the bowl, and the pellicle on the ſurface, much the ſame as on the 13th *ult*; the water was as limped as on the firſt day; ſyrup of violets changed its colour to a deeper green than before, and, with powder of galls, it immediately gave a blue tincture.

13. I boiled a chopin of this *Spaw* water, in a clean tin pan, till there remained of it only one fourth part; it turned of a yellowiſh colour and was muddy; when this was cold, and the okry flakes precipitated, it had a harſher and more aluminous taſte than the freſh *Spaw* water, or than that which had been ſo long expoſed to the air (12.). To a glaſs of this I put powder of galls; it changed very ſlowly, and, after it had ſtood about eighteen hours, it exhibited a faint blue.

14. In a clean tin pan, over the fire, I evaporated a chopin of the ſame *Spaw* water, till there remained only about one ounce and

a half; this I immediately poured into a China tea-cup, and, when cold, filtrated it: the filtrated liquor was clear and limpid, and had a ſtrong chalybeate taſte; this was evaporated by a ſand-heat, in a wide-mouthed glaſs; there was left a ſalt of whitiſh-brown colour, which had an aluminous and ſtrong chalybeate rough taſte. The whitiſh-brown colour of the ſalt was owing, I believe, to its having been a little ſcorched at the bottom of the glaſs; for, when I afterwards gently exhaled the water before the fire, the ſalt was of a pure white.

15. A little of this ſalt put into a glaſs of water, made the water of a yellowiſh colour; and, in a little time, it began to ſeparate into ſmall yellowiſh coloured flakes, which growing gradually larger, and thicker, precipitated; after ſtanding a night, the water was quite clear, with a few detached, ſmall flakes, floating cloſe to the ſides of the glaſs; the water had the original taſte of the *Spaw*, but rather ſtronger.

I poured off the clear water, half into one glaſs, and half into another: into the firſt poured off, which was the cleareſt, I dropt ſyrup of violets; after ſtanding a little, it it gave a faint green tincture: to the other half, I added powder of galls; it ſlowly changed to a violet colour, then to a purple, and, after ſtanding all night, it was of a very dark blue, inclining to black: into the remaining water, with the ſediment, I dropt ſyrup of violets, and ſtirred all about; it cauſed a tincture of a deeper green than the clear water: all theſe three mixtures, which did not exceed an ounce and a half, I poured into a chopin of common water; and the whole became of a deep violet colour.

16. THIS ſalt, after *ol. tartari p. d.* has been dropt upon it, and then diſſolved in a little water, yields a deep green.

17. THE ſalt of this *Spaw*, after it is burnt on a hot iron, exhibites the like *phaenomena* (15), and always loſes its aluminous taſte.

18. THIS ſalt turned moiſt and bubbled on a hot iron; and, when calcined in a crucible, it became of a purple colour, exactly like that of the *vitrioli Martis calx rubra;* and there was a manifeſt attraction of ſeveral of its particles by the magnet.

19. NEITHER the ſalt nor earth of this *Spaw*, make any effervefcence with *ſpiritus*

vitrioli,

vitrioli, *oleum vitrioli*, or *oleum tartari*, *p. d.* when by themſelves, or when diluted with water; nor does the ſalt emit any fumes with *ol. vitrioli.*

20. A glaſs of this water, into which ten gutts of *ol. tart. p. d.* were dropt, and another glaſs of the ſame water, turned, with powder of galls, to a faint violet colour; being mixed together, produced exactly the colour of old red port-wine.

Experiments made on the ſame Spaw *Water, taken up* October 12. 1750, *after five or ſix Weeks of dry Weather.*

21. The ſharp aluminous taſte was much weaker, and the chalybeate ſtiptic taſte ſcarce to be diſtinguiſhed.

22. WITH ſyrup of violets, even after ſtanding all night, there was a little, and but very little, tendency to green.

23. WITH *ol.vitrioli*, no efferveſcence, or change of colour.

24. WITH *ol. tart. p. d.* no efferveſcence, but the like clouds appeared as (10), tho' in ſmaller quantity, and of a yellow colour, with ſcarce any green.

25. WITH

25. WITH the ſolution of *ſaccharum Saturni*, it gave a little milkineſs, but ſcarce to be diſtinguiſhed.

26. With powder of galls, it changed very ſlowly; and, after ſtanding a night, it exhibited but a faint violet colour, ſomething inclining to purple.

27. A chopin of it, evaporated in a clean tin pan, yielded a very white ſaline ſubſtance, of a ſharp, aluminous, chalybeate and ſtiptic taſte.

28. A chopin of this *Spaw* water, taken up from the ſpring, after a long rainy ſeaſon, yielded of ſalt, nine grains; and of earth, ſomething more than one grain: the like quantity taken up after three weeks of dry weather, gave of ſalt, but ſeven grains; and of earth, about half a grain: the like quantity taken up after, between five and ſix weeks of dry weather, afforded of ſalt, only five grains and a half; and of earth, about a quarter of a grain.

29. THERE is another ſpring, about forty foot from this, which ſeems to be of the ſame kind; for it has the like taſte; and, with ſyrup of violets, powder of galls, *ol. vitrioli*, *ol. tartari*, *p. d.* and the ſolution of

of *saccharum Saturni*, it exhibites the like *phænomena*.

From the preceeding Experiments we may observe,

1. THAT this *Spaw* contains a principle of iron, which is volatile; for its chalybeate quality is much weakened by boiling (13), or even by being exposed for some time to the air (12).

2. IT contains an iron principle, which is fixed; for its chalybeate, quality is not intirely destroyed, by being exposed to the air (12), nor by boiling (13), nor by evaporation to a dryness (14, 15), nor by burning the *residuum* (17); and, when its salt is calcined a sufficient time, it is attracted by the magnet (18).

3. IT seems, by turning green with syrup of violets, to contain an alcaline principle, which is likewise fixed; for neither by being exposed to the air, nor by evaporation to a dryness, nor by burning the *residuum*, does it lose this quality.

As neither the water nor its salt or earth, make the least visible effervescence with *spiritus*, or *ol. vitrioli*, either by themselves, or when

when they are diluted with water; ſo it may be ſaid, that it contains no alcali, and that its turning green with ſyrup of violets, is of itſelf no proof; becauſe common water does, with ſyrup of violets, change to a faint green colour, after it has ſtood ſometime: but this I imagine rather to be a proof, that there is in common water an alcaline principle, *viz.* abſorbent earth; for rain-water, or ſnow-water, have not the leaſt tendency to green, with the ſaid ſyrup.

4. IT ſeems to contain a very little ſea ſalt (leſs than common water) by its precipitating the ſolution of *ſaccharum Saturni* (11).

5. IT ſeems to contain ſome portion of *alum*.

α. FROM the taſte of the *Spaw* water, as it comes from the ſpring, which is remarkably aluminous.

β. FROM its taſte becoming more aluminous, when part of the water is exhaled, by being expoſed to the air, for between five and ſix weeks (12).

γ. FROM its taſte after three fourths of the water was evaporated, which was more harſh and aluminous, than that of the freſh *Spaw*,

or

or than that which had been ſo long expoſed to the air (13).

δ. FROM the aluminous taſte of the ſalt (14. 27).

ε. FROM its ſalt ſwelling and riſing into a bliſter on the hot iron (18).

ζ. FROM this ſalt loſing its aluminous taſte when calcined a ſufficient time (17), which alſo happens to *alum*, when treated in a like manner.

THE *nitrum murale*, it is true, alſo ſwells and riſes in bliſters on the hot iron; likeways *borax*, and the purging ſalt of *Scarborough* water: but the *nitrum murale* differs from this ſalt in taſte, and, when calcined, falls down into a looſe powder, like lime; which this ſalt did not do, tho' calcined for a conſiderable time, in a ſtrong fire.

Borax likeways differs in taſte from this ſalt, and vitrifies in a moderate heat, in a few minutes; which this ſalt did not, after being calcined in a ſtrong heat for a conſiderable time. This ſalt has not the bitter penetrating taſte of the purging *Scarborough* ſalt, neither its purging quality.

I don't however pretend that all thoſe marks amount to a demonſtrative proof that there

there is *alum* in this water, but only to a great probability: for, to be ſure of it, we muſt ſee the *alum* chryſtalliſed, which is not eaſily done *; and requires, for that proceſs, a greater quantity of ſalt than I had.

It may be objected, that, when equal quantities of this water and new milk were boiled together, the milk did not curdle (6.); therefore there can be no *alum* in it. This objection is ſpecious; its validity, however, like that of many other things which have very plauſible appearances, can only be determined from experiment: wherefore I diſſolved in water a greater proportion of *alum*, than there is, or ſeems to be, in this *Spaw*; and, when equal quantities of this ſolution, and freſh milk, were boiled together, the milk did not curdle.

6. This water contains very little earth, it gives not the leaſt ſign of it, when *ol. tart. p. d.* is dropt into it; but the little it contains, ſeems to be of the white calcarious kind: and tho' okry-earth is commonly allowed to be a conſtituent principle in chalybeate wa-

 ters,

* Vid. Shaw's Inquiry into *Scarborough* water, part 1. § 4. art. 43.

ters, yet I am of opinion, that no ſuch thing exiſts in them in a ſound, natural ſtate; and that it is never to be found, till the acid, or whatever it is, that diſſolves the iron-principle, leaves the water, or, at leaſt, its union with the iron-principle, then it appears in the form of an okry ſubſtance; but, while the diſſolvent of the chalybeate principle remains united to it, that is, as long as the water continues in a ſound ſtate, okry earth is never to be found in it. The earth, therefore, of this kind, which is left in the filter after evaporation and filtration, is owing to its diſſolvent principle being loſt in that proceſs. And I have reaſon to believe, from experiments which I have made, that, by repeated ſolutions, evaporations, and filtrations, the pureſt and moſt genuine cryſtals of *vitriolum Martis* may be all reduced to an okry, or bolar earth; eſpecially if the ſolutions be made in common water.

7. THE quantity of mineral principles in this water varies; and is leſs after a dry ſeaſon, than a wet one (28); an unuſual circumſtance, which probably proceeds from this cauſe, that the water in a wet ſeaſon, riſes higher in the veins, which contain the mineral

ral principles, and consequently dissolves and carries a greater quantity of these principles along with it. Whence it is likewise evident, that experiments made on this water at different times, will not always exactly correspond, but will vary more or less, in proportion to the mineral contents of the water.

8. The tenth experiment seems to be an indication, that this water contains a *vitriolum Martis nativum*; no other mineral water, that I know of, turns green, with *ol. tart. p. d.*: but, when this oil is dropt into a solution of *vitriolum Martis*, it becomes green. And tho' there is no visible effervescence in this experiment (10.), yet the many air-bubbles generated seems to be the effect of the alkali's uniting itself to the acid, which kept the chalybeate principle dissolved, and which chalybeate principle, being now by the *ol. tart.* disengaged from its dissolvent, appears in the form of green or yellow clouds; and at last is found, at the bottom of the glass, under the form of a subtile okry, or bolar earth.

It may perhaps be said, that since there is probably *alum* in this water, the air-bubbles are generated by the alkali's uniting itself to the acid of the alum. To destroy this supposition

poſition, I diſſolved *alum* in water, and that in a greater proportion, than it can be in this *Spaw*, and then dropt into it *ol. tart. p. d.*; there enſued no effervefcence, or ſenſible change upon the water; but a few air-bubbles gathered at the bottom of the glaſs; nothing, however, in compariſon of what appeared when the *ol. tartar. p. d.* was dropt into the *Spaw*; and no more than what formed upon dropping *ol. tart. p. d.* into a glaſs of pure river-water.

9. As chalybeate waters, in ſeveral particulars, reſemble a ſolution of *vitriolum Martis* in water, hence many have been induced to think, that thoſe waters contained a *vitriolum Martis* ſuch as is prepared by art: but ſome of the lateſt, and moſt learned authors, as *Hoffman*, *Boerhaave*, *&c.* are of a contrary opinion, and for a very good reaſon, becauſe no *vitriolum Martis* could ever be found, upon evaporation of chalybeate waters: the experiments however made on this *Spaw*, ſeem to prove that it contains a fixed vitriol of iron; for it not only turns green, upon the addition of *ol. tart.* (10).), but likeways, upon evaporation, there is a ſalt left, which has all the properties of *vitriolum Martis*.

I diſſolved, in a chopin of fountain-water, *vitriol. Mart. gr.* xv. and in every reſpect treated it as I had done the *Hartfell Spaw* water, *viz.* it was evaporated over the fire, till there remained about two ounces; this was filtrated, and exhaled, in a wide glaſs, before the fire: there was left in the glaſs *gr.* xi*ſs* of a light-yellowiſh coloured ſalt, and in the filter a dark-brown earth. With this ſalt, I repeated all the experiments I had made with the ſalt of the *Hartfell Spaw*; and I had the pleaſure to ſee, that they correſponded in every thing, without any material difference; only the ſalt left upon evaporation of *vitriolum Martis*, did not riſe in a bliſter on the hot iron, and, when diſſolved in water, gave, with powder of galls, a rediſh tincture inclining to purple: whereas the ſalt of the *Hartfell Spaw*, being diſſolved in water, with galls, ſtruck a blue. The reaſon of which I take to be, that there is a greater proportion of acid, in the compoſition of the ſalt of the *Hartfell Spaw*, then there is in the *vitriolum Martis*; for, if you add an alkali, as *ol. tartari*, to the blue tincture, produced by the ſalt of *Hartfell Spaw*, it changes to a rediſh; and if an acid, as *ol. vitrioli*, is added

to

to the *vitriolum Martis*, (*e. g.* if to a ſolution of two, three or four grains of *vitriolum Martis* in eight ounces of water, be dropt one gutt. of *ol. vitrioli*) this, with powder of galls, tho' indeed it changes but ſlowly, will give a blue, not a rediſh tincture; and again, if the acid is weakened, by dropping into this blue tincture *ol. tartari*, the blue will change to a rediſh. And this conjecture ſeems to be confirmed by the following experiment, *viz.* if *vitriol. Mart.* be burnt on a hot iron, till it becomes quite dry, and of a yellowiſh brown, and diſſolved in water, it gives much the ſame rediſh purple with galls, as before it was burnt; if this burnt *ſal Martis* be powdered, and the dry powder wrapt up in a ſingle paper, and left in a room without a fire, for three or four weeks, it will become moiſt, and, when diſſolved in water, ſtrikes with galls no more the former colour, but a blue; which, I preſume, is owing to the *acidum vagum* which it has imbibed.

Seeing therefore the ſalt of *Hartfell Spaw* has all the properties of *vitriolum Martis*, I cannot ſee why it may not be allowed, that this *Spaw* contains a native, fixed vitriol of iron: and it is the only water (I know of) which

which has been difcovered to contain fuch a falt.

In the Philofophical Tranfactions, N° 245. mention is made of two mineral waters, which yielded a real fixed vitriol of iron, one near *Eglingham* in *Northumberland*, the other near *Haigh* in *Lancafhire*; but thefe waters were found by Dr *Cay*, who made particular inquiry into the matter, not to be mineral fprings, but drifts made for the draining of coal-pits; and that the vitriol found in thefe waters, was owing to their running over marcafite beds which had been expofed for fome time to the air. I was fufpicious that fomething of this kind had communicated the vitriol to the *Hartfell Spaw*; but Mr *Williamfon*; who difcovered the fpring, affured me, that the water, on which I made the preceeding experiments, had run over nothing at all, after its exit from the fpring, but was taken up by himfelf immediately, as it iffued out of the rock. Dr *James Hunter*, Phyfician in *Moffat*, afterwards fent me fome of the fame water, which he received into bottles, by applying their mouth to the orifice of the vein, in the rock from which it iffued; and from it I obtained cryftals of *vitriolum*

olum Martis, which were of a paler green than the artificial.

In *Auguſt* 1752, this *Spaw*, with ſyrup of violets, ſtruck no green tincture.

With *ol. tart. p. d.* it immediately turned green, and clouds of a deeper green and more in quantity than in *Auguſt* 1750, were formed, and that much ſooner than in the preceeding experiments.

With powder of galls it ſcarce changed its colour; and, even after ſtanding ſome time, it exhibited but a faint blue. I was at a loſs for the reaſon of its giving ſo faint a blue with galls; when, at the ſame time, I knew, by its immediately turning green with the *ol. tart.*, together with the quantity of the clouds that preſently formed, and the deepneſs of their green, that it was very ſtrong of the chalybeate principle: I therefor ſet the mixture before the fire, to ſee what change a little heat would produce; and, when I obſerved that a ſmall heat had no effect, I gradually increaſed the heat, till bubbles appeared on the ſurface of the water; but this occaſioned no change on the colour of the water, the faint blue remaining much the ſame. I then conjectured, from the wa-

ter

ter not turning green with ſyrup of violets, as in the former experiments, that there was a greater quantity of acid than uſual united to the chalybeate principle: and, to diſcover whether my ſuſpicion was right, I dropt into the ſame glaſs, containing the water and powder of galls, one or two gutts (I don't remember which) of *ol. tart.*; and, having mixed it by ſtirring, it immediately produced very deep blue.

FROM this experiment, I think it appears, that a water may be very ſtrong of the chalybeate principle, and yet diſcover very little of it by the common experiment with galls; ſo that it may be proper always to try it with the *ol. tart.* alſo: for the ſtrength of the iron-principle is, as the colour and quantity of the clouds formed by this oil; that is, if the clouds are of a light yellowiſh colour and ſmall in quantity, the chalybeate principle is weaker; if they are of a dark brown and more in quantity, it is ſtronger; if they are of a green colour, the deeper the green and more in quantity, the ſtronger is the chalybeate principle; which I have found to correſpond with truth, by repeated experiments, not only on this *Spaw*, but on ſolutions of *vitriolum*

 Martis

Martis of different ſtrengths, in common water: but, how far this will hold in chalybeate waters that are volatile, I have had no opportunity to try.

I took a chopin bottle of this water, three months after it had been carried fifty ſix miles, and poured one half of it into another clean chopin bottle, and then corked them both very tight; the one bottle I ſhook frequently and ſtrongly for two days; the third day I tried both, with powder of galls and *ol. tart. p. d.*; with the galls, both gave a very deep blue; but the water which had been ſhaken, with the *ol. tart.* rather yielded a fainter green, than that which had not: I let both bottles ſtand corked up as before for ten or twelve days longer, without ſhaking; then tried them again with the galls and *ol. tart.*; they both ſtruck much the ſame deep blue tho' not ſo deep as at firſt; and, with *ol. tart.* the water which had not been ſhaken, gave a green, tho' fainter than at firſt; the other ſcarce offered any green; the clouds formed in it were fewer, and of a lighter yellow, than thoſe of the other. From which it ſeems, that ſhaking ſomewhat weakens the chalybeate

chalybeate principle of this water, tho' not much nor ſuddenly.

THIS water ſeems to belong to the claſs of the *aquae Martiatae*; for it effervefces not with acids, like the *acidulae* and *thermae*; neither is its volatile iron principle ſo volatile as theirs: hence in general, it may be ſaid to be aperient and ſtrengthening, both when uſed internally and externally: whence it muſt be of uſe in diſeaſes where the ſolids are relaxed and the blood too watery and weak. But, altho', when the principles of a mineral water are known, we may, from analogy, deduce its virtues in particular diſeaſes; yet, as this method is not ſo certain, no reaſoning being equal to experience, I ſhall confine myſelf to the laſt alone; tho' it is to be preſumed, that there have not been yet ſufficient opportunities of diſcovering all the virtues of a water ſo lately found out. However, it has been obſerved to be of great uſe in curing itchy, hot, tettarous eruptions, old obſtinate ulcers and ſores, internally uſed, and externally applied: it has likeways been of great ſervice in diſorders of the ſtomach and bowels; in the bloody flux; bloody urine; ſpitting of blood; immoderate flux

of

of the menſes; obſtruction of the menſes; the fluor *albus*; gleet; rheumatic pains; in the firſt ſtage of conſumptions, and even when they have been further advanced; in preventing miſcarriages; and in reſtoring health, when the conſtitution has been impaired by long illneſs: all which will appear from the following hiſtories, moſt of which were wrote down from the patients own mouths, and atteſted by the phyſician or ſurgeon who attended them*.

BEFORE

* There were given in to the Society, with this paper, twenty two well atteſted hiſtories of patients cured of the above mentioned diſeaſes, by means of the *Hartfell Spaw*; which it was thought proper to omit, ſince they would have ſwelled this article to a bulk too great for a work of this kind. But as the good effects of this water in conſumptions of the lungs are ſomewhat extraordinary, and may, perhaps, be doubted of by thoſe, who have imbibed early prejudices againſt all chalybeates and aſtringents, in ſuch caſes; it may not be amiſs to give a place, here, to the two following hiſtories.

1. Mrs *Glendinning*, wife to Mr *Robert Glendinning*, ſchoolmaſter in *Moffat*, aged 51, was, on the 20th of *December* 1750, attacked with pains in her right ſide; a conſtant ſharp pricking pain under the middle of the breaſt-bone; a hard, tickling, dry cough; thirſt, difficulty of breathing, and frequent gripes in her guts: all which ſymptoms increaſed, till the firſt of *January* 1751, when ſhe began to ſpit, with difficulty, a little thick groſs matter. In the beginning of *February*, the ſpitting was ſo extremely fœtid and ill-taſted, that

BEFORE one enters upon a courſe of the water, it will be proper to cleanſe the *primæ viæ*, by an emetic and a gentle purge or two; unleſs forbid by ſome particular circumſtances of

that it made her often vomit: ſhe frequently waſhed her mouth with ſalt and water; but ſtill felt a taſte and ſmell which ſhe thought reſembled that of ſtinking fleſh: ſhe now became very weak and emaciated; had hectic fits and night-ſweats: which ſymptoms continued increaſing till the middle of *April*; about which time (having tried no medicines before) ſhe began to drink the *Hartfell Spaw*, to the quantity of half an *Engliſh* pint every morning. She had ſcarcely uſed it a week, when her breathing became much eaſier; after a fortnight, the ſpitting loſt the fœtid ſmell and ill taſte; and, in three weeks, ſhe was perfectly free of the pain of her breaſt, cough, ſpitting, difficulty of breathing, hectic fits and morning-ſweats: and has continued ever ſince in good health.

2. Mrs *Halliday* in *Barntymipan*, aged 28, of a plethoric habit, complained, in the beginning of *September* 1750, of a pain under the upper part of her breaſt-bone, which frequently darted to the point of her right ſhoulder; a pain in her head and neck; her breathing was not quite free, and ſhe could not lie upon her right-ſide. About the middle of *October*, ſhe began to ſpit florid blood, which continued three weeks, at the rate of a ſpoon-ful every day; then ſtopped a week, and returned again. In this manner it went on about four months: after which, beginning to fear the conſequence might be fatal, as many of her neareſt relations had been carried off by conſumptions of the lungs; ſhe applied to Mr *Johnſton* ſurgeon-apothecary in *Moffat*, who, finding the ſpitting of blood increaſing, and her pulſe pretty

of the patient: or, if the patient's body has been long coſtive, and the excrements harden-ed, emollient glyſters may be uſed. Such as

pretty full and frequent, ordered her to be blooded, to take the *decoctum tamarindorum* of the *Edinburgh* Diſpenſatory, and to ſwallow four of the following pills thrice a-day. *R. Extract. cort. Peruvian. drach.* ii. *balſam. Peruvian. ſcrup.* i. *pulv. cort. Peruvian. q. ſ. ut. f. maſſa, ex cujus ſingul. drachm. formentur pil.* xii. The blooding and decoction were repeated as often as they ſeemed neceſſary; and the pills were continued ten weeks without ſucceſs: for ſoon after ſhe left off uſing them, ſhe began to cough up purulent matter with blood. Whenever the ſpitting diminiſhed, her breathing became very difficult; and the pain of her breaſt increaſed, with a particular ſoreneſs all along the breaſt-bone when ſhe coughed.

She drank goat's and ewe's whey, from the beginning of *June*, to the end of *Auguſt*, without any ſenſible benefit; for ſhe continued ſtill to ſpit blood and matter: ſoon after this, ſhe was attacked with ſudden fluſhes of heat and morning ſweats, which increaſed till the middle of *January* 1752, when ſhe was become feebler, much emaciated, and often faintiſh. At this time, ſhe was adviſed to drink the *Hartfell Spaw* in ſmall quantity; but, finding it ſit eaſy on her ſtomach, ſhe drunk an *Engliſh* pint of it daily. During the firſt week after drinking the water, ſhe expectorated dark, blueiſh, putrid ſtuff, without blood; and found her breaſt much eaſier than it had been any time from the beginning of her illneſs. Soon after, ſhe began to breathe freely; and, after having drank the water ſix weeks, was perfectly recovered: ſince which time, ſhe walked, or rather run, three *Scotch* miles in an hour, without being either hurt, or much fatigued by it.

as are plethoric, or have their veſſels full, ſhould let a little blood.

THIS water may be drunk at all times of the year: tho' I prefer the ſummer or warm months; becauſe perſpiration is then freeſt, and the operation of the water, as an alterative, is aſſiſted by the warmth of the weather: beſides, this ſeaſon is more convenient for the uſe of exerciſe, and the patients are leſs apt to catch cold. The propereſt time of the day for drinking it, is, when the ſtomach is moſt empty, as in the morning, faſting, before dinner, and ſometimes in the evening. Some patients have a cuſtom of drinking it with their victuals: but I don't approve of this; as it may retard, or, in ſome meaſure, hurt digeſtion.

THE quantity to be drunk is to be determined by the age, ſtrength, and other circumſtances of the patient: they ought to begin with a ſmall doſe at firſt, and increaſe it gradually: ſuch as are very weak and much extenuated, ſhould begin with a gill or leſs; this quantity they may take twice a-day; and, by degrees, increaſe it, in proportion as they recover their ſtrength till they come to drink an *Engliſh* pint a-day; a pint and an half,

half, or two pints, which is about a *Scotch* chopin, and is the largeſt quantity uſually drunk of this water in twenty-four hours. If the water ſhould be uneaſy on the ſtomach, the patient may mix with it an equal quantity, or leſs, of new milk, or ſuch a proportion of it as may be found moſt convenient: it may alſo be proper to warm the water.

As few of the patients, whoſe caſes I have collected, obſerved any rule in their diet or exerciſe, and nevertheleſs met with a cure; this ſhows that there is no great ſtrictneſs required in theſe matters: and, indeed, ſuch as have any tolerable degree of health and ſtrength, need not much recede from their ordinary way of living; tho' temperance in eating, a moderate uſe of wine, and gentle exerciſe, is what all infirm people ſhould conform themſelves to: but, ſuch as are very weak, and much emaciated, or that labour under dangerous diſeaſes of the lungs, or abdominal *viſcera*, muſt obſerve a very ſtrict regimen; their diet ſhould conſiſt of what is lighteſt and eaſieſt of digeſtion, ſuch as new milk, broths, panadoes, thin jellies, freſh eggs, chicken, veal, fowls, &c.; and a little wine may be allowed, when not prohibited

bited by the circumſtances of the patient; moderate exerciſe may be proper for ſome, and reſt for others; they muſt avoid the night air, ſitting on the graſs, and every thing that diſcompoſes the mind.

UPON the firſt uſe of this water, ſome complain that it makes them ſick, others that it makes their head giddy; but theſe inconveniences almoſt always proceed from their having taken too large a quantity: the doſe is therefore to be diminiſhed; and if, in a ſmaller quantity, it ſhould be uneaſy on the ſtomach, chewing cinnamon, or caraway ſeeds, or comfits made of them, are frequently uſeful; or any other aromatic, moſt agreeable to the patient. It ſometimes occaſions gripes, when the inteſtines are weak, on account of its coldneſs; it ought therefore to be warmed; the beſt way of doing it, is, to put as much as the patient intends to drink at one time, into a vial; cork the vial, and ſet it before the fire, or put it into warm water, till it acquires a ſufficient degree of heat. On its firſt uſe, it ſometimes purges ſuch as are troubled with a *diarrhœa*; but that is to be reſtrained by a gentle emetic, or by giving ſome gutts of the *tinct. opii* in every doſe of the water, or rather ſome little time before it; or three or four grains of the *pil. Matthæi* may be given at bed-time, and as much, if requiſite, in the morning: it will be convenient likeways to warm the water. But it oftener occaſions coſtiveneſs; for which it will be proper, now and then, to give a little *ſal. Glaub. polychr. manna*, or cream of tartar; or, which perhaps is preferable to any of them, the purging ſalt of *Scarborough* water. If, upon catching cold, any feveriſh fits ſhould ariſe, the uſe of the water muſt be ſuſpended till theſe ſymptoms are gone. When the water does not paſs eaſily, it will be proper to looſen the belly, or give diuretics; and ſuch whoſe ſtrength will permit, and whoſe veſſels are full of blood and juices, ſhould let blood.

There is no determining preciſely how long it ſhould be drunk; the advantage the patient reaps from it muſt decide that: ſome have uſed it only twelve days, others three weeks, others four, others ſix, ſome two months, and others many months; and yet they have all been ſo happy as to meet with a cure: ſo that, in general, as long as there is any proſpect of its working a cure,

its

its uſe ſhould be perſiſted in; and, tho' the patient may think himſelf well, I am of opinion he ſhould not immediately diſcontinue its uſe, but gradually diminiſh the quantity, and leave off by degrees. Some recommend purging and other medicines, after the courſe is finiſhed; but, if the patient has been cured, all medicines are uſeleſs, and rather hurtful; if the cure has been imperfect, then ſuch helps may be called in as the caſe requires.

As to the external uſe of this water: the practice has been, to waſh with it, when warmed, itchy, hot, tettarous eruptions, and old obſtinate ulcers, two or three times a-day; and, in ſome caſes, during the whole day, to keep linnen cloaths wetted in the ſame water, applied to the parts affected; which method has been very ſucceſsful; but, it muſt be remarked, that the water was always uſed internally alſo, in the ſame caſes.

IT ſeems proper to obſerve, that, as this water, put in bottles, well corked and waxed, may be carried to any diſtance, and will keep good a long time; longer than either the *Pyrmont* water or *Liege Spaw*: people living in any part of *Great Britain*, or even beyond ſeas, may reap almoſt the ſame benefit from it, as thoſe who reſide in its neighbourhood.

ART.

ART. XIII.

Of the various Strength of different Lime-waters, by ROBERT WHYTT, M. D. F. R. S. *Fellow of the Royal College of Physicians, and Professor of Medicine in the University of* Edinburgh.

THE Reverend and ingenious Dr *Stephen Hales*, having informed me, in a letter dated *May* 1751, that he had found the strength of lime-water much increased, by pouring it a second time on quick-lime, fresh from the fire; I thought it might be worth while to make a few experiments, in order to determine, with some degree of certainty, the different strength of different lime-waters; from these experiments it appeared, that lime-water acquired a considerable addition of strength by being poured on quick-lime newly taken from the fire; and that the first water got off quick-lime, was sensibly stronger than the fourth and succeeding ones *.

ON

* Essay on the virtues of lime-water, &c. p. 38. 39.

ON the other hand, my worthy friend and Collegue Dr *Alston*, having observed, several years since, that quick-lime continued to communicate its virtues to water much longer, than any one, before, had imagined; tells us, that he found afterwards, by experiments, that half a dram of stone quick-lime yielded forty ounces of lime-water; and that, after a pound of the same quick-lime had afforded five hundred pounds of lime-water, the water procured from it was as strong of the lime as ever *. Hence he imagines, that as water can only be impregnated to a certain degree, by quick-lime, so this will happen equally, whether the quick-lime be fresh from the fire, or has had five hundred times its weight of water poured on it before; provided the water be allowed time enough to extract the virtues of the lime †. And further affirms, that the strength of lime-water cannot be increased by slaking new-made lime in it, because the water can take up no more of the lime than it had before ‡.

As

* Philosoph. Transact. vol. 47. p. 266, and Dissertation on quick-lime, &c. p. 4. 5. & 6. where the same thing is affirmed of oisterstell lime.

† Dissert. on quick-lime, p. 11. & 53.

‡ Id. p. 11.

As theſe experiments and concluſions appeared inconſiſtent with what I had advanced, the *Doctor* has endeavoured further to weaken the credit of my experiments, by ſome arguments drawn, chiefly, from the imperfection of the hydroſtatical balance, and from the nature of quick-lime and its water. In order therefore to know whether I might not have been miſtaken in what I had ſaid, concerning the ſtrength of different lime-waters, I thought it neceſſary to make ſome new experiments; an account of which I beg leave to lay before the ſociety.

I. (*a*) HAVING got from my ingenious friend Mr *James Gray*, a cylindrical copper-veſſel ending in a narrow neck, which contained exactly 100 cubical inches; I filled it with the fountain-water of this city, and, by means of a very nice balance, found it weighed 25320 *Troy* grains *, beſides the weight of the veſſel itſelf, which amounted to 13055 grains.

(*b*) I

* According to Mr *Gray*'s experiments, the water which this veſſel contains, only weighs 23518 grains. *i. e.* two grains leſs than we have made it. See above, p. 201. This difference may have ariſen from our having put a few more drops of water into the veſſel than Mr *Gray* did. But altho'

(*b*) I poured upon 90 grains of calcined oisterſhells, newly taken from the fire, and reduced to a powder, 96 ounces, or five hundred and twelve times their weight of boiling water. After 92 hours, during which time the infuſion was frequently ſtirred and ſhaked, I decanted off the clear water, and filtered it thro' a piece of a very thick linnen-cloth doubled; by which means it was rendered free of any cruſts, and equally pellucid with fountain-water. With this lime-water I filled the above *veſſel*, and found its weight to be 25356 grains*.

(*c*) MONDAY, at ſeven in the evening, I poured upon a pound of calcined oiſterſhells, freſh from the fire, ten times their weight of water; next morning at ten I decanted off the clear lime-water; and having filtered it

altho', in weighing fluids with this veſſel, one might err ſix times more than this, yet it would not affect the point we have in view, which is not to determine with the greateſt accuracy the different ſpecific gravities of different lime-waters, but *only* to ſhew that they *are different*.

* The oiſterſhells made uſe of in this, and the following experiments, were got from among the rubbiſh on the ſouth ſide of the Caſtle of *Edinburgh*, and were quite free of any ſea ſalt.

it as above, filled the *veſſel* with it; it weighed 25397 grains.

(d) TUESDAY at mid-day, I poured ſeven pounds of the ſingle lime-water, *(c)* upon one pound of calcined oiſterſhells, newly taken from the fire, ſtirring them well for ſome time after; at three quarters paſt ſix in the evening, I decanted off, and filtered as above, the clear lime-water; and having filled the *veſſel* with it, found its weight to be 25457 grains.

HENCE it appears, that 100 cubical inches of the lime-water *(b)* exceeds, in weight, that quantity of fountain-water by 36 grains: *(c)* exceeds it by 77 grains, and *(d)* by 137 grains.

THE ſpecific gravity therefore of the weak lime-water *(b)* is to that of fountain-water nearly, as 704 to 703. The ſpecific gravity of the ſingle lime-water *(c)* is to that of common water, nearly as 329 to 328; and the double lime-water *(d)* is in ſpecific gravity to water nearly, as 186 to 185.

IT is obſervable that the ſpecific gravities of the ſingle and double lime-waters *(c)* and *(d)*, are conſiderably leſs than the ſpecific gravities of the ſingle and double lime-waters *(a)*

and

A. and B. mentioned *p.* 39. and 40. of my Eſſay on the virtues of lime-water, *&c.* But, if it be conſidered, that, in making the latter, a much leſs proportion of water was added to the quick-lime, than in making the former; it will appear that this difference of their ſpecific gravities, does not infer any thing againſt the accuracy of the hydroſtatic balance; but clearly ſhews, that the ſtrength of lime-water varies according to the quantity of water poured on the quick-lime.

It may be worth while to obſerve, that the ſpecific gravities of the lime-waters, (*b*) (*c*) and (*d*) did not differ more than their taſtes. The firſt was weakeſt and leaſt diſagreeable; the ſecond was ſtronger; and the third ſtill ſtronger and ſomewhat pungent. Further, while the double lime-water (*d*) gave, in a few minutes, a copper-colour to ſilver; the weak lime-water (*b*) produced no ſenſible change upon it.

II. Having, formerly, found that lime-water and claret wine, mixed together, in a certain proportion, acquired a colour like that of gun-powder*: I thought, that, by mixing claret with different lime-waters, one

 might

* Eſſay on the virtues of lime-water, p. 47.

might judge whether they were all equally ſtrong of the lime or not. The reſult of the experiments was, that one tea-ſpoonful of claret required four tea-ſpoonfuls of the lime-water (*b*); two and about one third of (*c*); and one and a half of (*d*), to give it the full gun-powder colour. Theſe experiments, tho' not ſo accurate as thoſe made with the balance, yet clearly demonſtate a remarkable difference of ſtrength betwixt the above lime-waters.

III. Twenty grains of ſalt of tartar being mixed with eight ounces and two drams of the weak lime-water (*b*), after it had ſtood five days on the lime, the mixture became immediately white and turbid, and ſoon precipitated a white powder; which, being ſeparated from the water, by filtration, and dried, weighed $2\frac{2}{3}$ gr.

The ſame quantity of ſalt of tartar, mixed with eight ounces and two drams of double lime-water, that had ſtood eight days on the lime, became conſiderably thicker and whiter than the former; and afforded rather more than 7 grains of white powder.

The ſame quantity of ſalt of tartar being mixed with eight ounces and two drams of the

the double lime-water (*d*), which had ſtood 24 hours on the lime, gave eight grains of a white powder.

It was obſervable, that theſe three lime-waters retained the taſte of the lime, after being mixed with the ſalt of tartar, and this equally after precipitation, as before it.

Since the earthy powder precipitated by theſe different lime-waters, proceeds *wholly*, or *almoſt* wholly, from the waters, and not from the fixed alcaline ſalt; * theſe experiments ſhew, beyond doubt, that double lime-water may contain thrice as much lime, as lime-water made by pouring, on quick-lime, 512 times its weight of water.

IV. 1. Monday 24th *December*, at eight in the evening, I poured upon a dram of freſh calcined oiſter-ſhells, reduced to a powder, 520 drams of boiling water.

At

* What proves this, is, that the calcarious matter precipitated by mixing ſalt of tartar with lime-water, is greater or leſs, in proportion to the ſtrength and quantity of the lime-water; but not in proportion to the quantity of the ſalt. Thus 12 grains of ſalt of tartar, mixed with four ounces of ſtrong lime-water, yielded as much of this matter, as the ſame quantity of this lime-water, mixed with 18 grains of the ſalt.

2. At the ſame time, I poured upon a pound of the ſame calcined ſhells, 8 pounds of boiling water.

3. Tuesday, at eleven before noon, I poured fifty ounces of the lime-water N 2, on nine ounces freſh calcined oiſterſhells; and, at eight in the evening, I filtered, through brown paper, theſe three waters, and put ſixteen ounces of each of them into a baſon by itſelf; and, having placed the baſons in a cloſet, where they might be pretty free from duſt, I let them ſtand 19 days. After this, I filtered the ſeveral waters through brown paper, and having collected the earthy cruſts, and dried them well; I found, that N° 1. afforded very near four grains, N° 2. near 12 grains, and N° 3. rather more than 13 grains.

Altho' theſe three lime-waters had, at the time they were filtrated, quite loſt their taſte; yet, obſerving that N° 2. and 3. became turbid when mixed with ſalt of tartar, I added eight grains of this ſalt to twelve ounces of theſe two waters; and the white powder which was precipitated, when dried, weighed juſt one grain and a half.

Having

HAVING filtered the lime-waters of N° 2. and 3, into the ſame bottle, befor I ſuſpected that any thing of the lime remained in them, it became impoſſible to know, which of them afforded moſt of the calcarious powder precipitated by the alcaline ſalt, or whether it did not proceed wholly from N° 3.; in which caſe, ſixteen ounces of it muſt have contained 17 gr. of the earthy part of the lime, and and N°2. only 12 gr.

SINCE N°2. and 3. were not quite free of the lime, altho' they had ſtood expoſed to the open air 19 days, and had loſt above $\frac{1}{4}$ by evaporation; it follows, that the ſureſt way of knowing the quantity of calcarious earth, contained in lime-water, is to evaporate it, as Dr *Langriſh* did*: and, if it be objected to this, that all water affords ſome earth, when evaporated; the quantity of this may be determined by experiment: tho' in many waters, it may well be neglected, on account of its ſmallneſs.

IT has been argued that quick-lime muſt, after many repeated affuſions of water, yield as ſtrong lime water as at firſt; becauſe, as long as there remains any virtue in the lime, the

* Phyſical experiments on brutes. P. 11.

the water will extract it, and continue to do so, till it has taken up as much of the lime as it can bear. But to this we cannot agree: for, tho' there is undoubtedly a certain degree of strength which lime-water can never exceed; yet, in order to communicate to water, this degree of strength, slaked lime may not only be insufficient, but repeated additions of quick-lime may be necessary; unless perhaps a very small proportion of water is poured upon it. Quick-lime, fresh from the fire, yields its virtues more easily, than when weakened by long exposition to the air, or by many affusions of water: the water must extract the virtues of the latter, while the former, by a sort of explosive force of its own, quickly impregnates the water. Nor is it to be wondered at, that quick-lime, fresh from the fire, should, at first, impregnate water more strongly with its virtues, than it does afterwards. This is as easily conceived, as that boiling water should extract more of the virtues of tea or coffee, than cold water. The only difference is, that the *menstruum* in the latter case acts more powerfully, while in the former the substance to be extracted affords

affords its finer parts more readily, and in greater abundance.

UPON comparing the experiments N° I. with thoſe of N° III. and IV. it appears, that the difference between the ſpecific gravities of different lime-waters and common water, is much more than the weight of the calcarious matter contained in theſe lime-waters: there muſt, therefore, be ſomething elſe beſides this earthy matter, which quick-lime communicates to water, by which its weight is increaſed *. Perhaps quick-lime may alſo, in ſome other way unknown to us, alter the ſpecific

* As lime-water, after its earthy part has been precipitated by an alcaline ſalt, continues to taſte ſtrongly of the lime; it follows, that, beſides this earth, it contains ſome more active and ſubtile part, to which its taſte and virtues are chiefly owing: for we know that the calcarious matter of lime-water, is perfectly inſipid and void of any other virtue, than what all abſorbent earths poſſeſs. This active and more ſubtile part of lime-water, ſeems to be ſeparated from its earth by the alcaline ſalt, which ſtrongly attracts and embraces it. And hence lime-water mixed with ſalt of tartar, does not loſe its taſte of the lime by being expoſed to the open air. Does not a ſolution of a fixed alcaline ſalt in water, poured on quick-lime, ſeparate this ſubtile active matter of quick-lime from its earthy part, by ſtrongly attracting it? And do not ſoap-leys conſiſt of water and fixed alcaline ſalt united with this active part of quick-lime, without *any*, or *almoſt* any, of its earthy part?

ſpecific gravity of water. But, whatever may be in this, it is evidently unreaſonable to deny that lime-water is as much ſpecifically heavier than common water, as the hydroſtatic balance, or other accurate experiments ſhew; becauſe we cannot account for this exceſs of gravity from any thing we know of the contents of lime-water. This is no leſs unphiloſophical, than if, one was to doubt of *univerſal* gravity, becauſe philoſophers have hitherto attempted, in vain, to account for it. If we miſtake not the matter much, the contrary has always been the opinion of mankind, *viz.* that every well atteſted fact is to be believed, altho' we are ignorant of its cauſe, or cannot ſhew the particular way in which it happens.

ENOUGH, it may, perhaps, be thought more than enough, has been ſaid, to ſhew that the ſtrength of lime-water is very different, according to the different quantities of water poured on quick-lime. However, I muſt be allowed to ſay, that this point, which has been diſputed by my good friend, is of that conſequence, as to deſerve to be fully cleared up; ſince, to ſuch as drink lime-water, with a view to the cure of the ſtone,

ſtone, it is of no ſmall importance to know, how it may be prepared, ſo as to have the ſureſt and ſpeedieſt effects. And, as lime-water, injected into the bladder, will undoubtedly diſſolve a ſtone lodged there; it is evident, that, after the bladder has been accuſtomed to the weaker lime-waters, or to theſe even ſoftened with a little ſweet milk, the diſſolution of the ſtone may be much haſtened, by injecting ſuch as are more ſtrongly impregnated with the virtues of the lime.

WITH regard to the lithontriptic powers of oiſter-ſhell and ſtone-lime-water, I ſhall only ſay, that, as in a variety of experiments made during the courſe of ten years, I had always obſerved the ſuperior efficacy of the oiſter-lime-water: I thought it to no purpoſe to make a new trial: any one, who doubts on which ſide the truth is, may eaſily ſatisfy himſelf. But, in making the experiment, the *calculi* ſhould either be immerſed in a large quantity of lime-water, or elſe it ſhould be renewed upon them every three or four days.

ART. XIV.

Of the anthelmintic Virtues of the Root of the Indian Pink, *being Part of a Letter from Dr* JOHN LINING *Physician at* Charles-town *in* South Carolina, *to Dr* ROBERT WHYTT, *Professor of Medicine in the University of* Edinburgh.

THERE is a plant, commonly called, by the *Carolinians*, *Indian Pink*, which grows in this province, the root of which is used as an anthelmintic, and was first communicated to the *English* by the *Indians*.

THE root is either given in powder, or an infusion is made of it in boiling water: but the powder is most effectual. When I give the powder, I add some rhubarb (a sufficient quantity to keep the belly open) and a little of some of the essential oils, as the *ol. rut. sabin.* or *absinth.* To a child of three years of age, 12 grains of the root in substance is a moderate dose: and I repeat it morning and evening for some days; ordering, at the same time,

time, a proper regimen. When the root is given in infuſion, 20 grains of it make a ſufficient doſe for a child of the ſame age.

It has an advantage above all other anthelmintics that I am acquainted with, as it is leſs nauſeous, and may ſafely be given in fevers without heating too much: likeways children, when they awake in the morning, may be deceived with it, by giving the infuſion with a little milk, and a ſufficient quantity of ſugar, in place of tea. Tho' it does not always ſucceed in expelling worms; yet I have obſerved, that children's complaints were remarkably relieved after having taken it for ſome days.

I ſhall only trouble you with one ſhort hiſtory, where I gave it the other day with ſucceſs. I was ſent for to a Negro-child about four years of age, on the ſeventh day of a continual fever: and, as I ſuſpected that the fever was kept up by the irritation of worms, I ordered the following, that morning; R. *aq. theriac. drach.* ii. *aq. font. unc.* ii*ſs. pulv. rad. anthelmiæ* (for ſo I called the *Indian* pink) *tart. regenerat. ana ſcrup.* i. *pulv. croci anglic. gr.* v. *ſacch. q. ſ. M. ſumat coch. unum ſecunda quaque hora.* Next morning, I ordered

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a repetition of the ſame; and, in the afternoon, when the child had a ſtool, 30 large worms, the *teretes*, were at once voided; and that afternoon, the fever went intirely off, and did not return. Next morning I repeated the ſame mixture; and, that day, the child voided 9 more of the ſame kind of worms.

WHEN too great a doſe of the powder or infuſion is given, it has ſome ſtrange effects, which appear very ſoon after the doſe is taken. Children become vertiginous, complain of a pain in and over their eyes, and the *muſculi adductores* and *abductores oculorum*, ſeem to be greatly affected, from their irregular actions; while the other muſcles of the eye, ſo far as I have had an opportunity of obſerving, remain unaffected. Theſe ſymptoms, however, ſoon go off; and their removal may be haſtened by giving ſome weak ſpirituous liquor, to which volatiles or ſaffron, or any of the warm eſſential oils, are added. But, to prevent any of theſe bad effects of this medicine, ſome guts of the oils before mentioned are added.

I have been the more particular in my account of this medicine, as I imagine it may, when properly introduced into practice, be of great uſe in ſaving the lives of many. It has been, for many years, uſed in this part of the world, not only by all the practitioners, but likeways univerſally by the planters.

ART.

ART. XV.

The History of a Cure performed by large Doses of an alterative mercurial Medicine, communicated to Dr PLUMMER; *by Mr* GEORGE DENNISTOUN *Surgeon in* Falkirk.

ABOUT the month of *December* 1737, a gentleman applied to me for cure: he complained, that, for some time past, he had felt pricking pains in several parts of his body, especially about his shoulders and shin-bones, which were now become intolerable. He had dry scurfy scabs over the whole inside of the thighs, the glands of both groins were a little tumified; and hard crusty scabs were here and there scattered upon the *scrotum*, hips, and about the *podex*. From these, and other circumstances, I had no reason to doubt that my patient's distemper was the veneral *lues*; upon which, I raised a salivation with *merc. dulc.* in the course whereof, all the symptoms gradually disappeared. I then put him upon a course of the *pil. Æthiopic.* and

and decoction of the woods; by which means, he seeming to be very well, I flattered myself that the diseafe was intirely eradicated. He had no complaints for above twelve months; but then he began to feel the old pricking pains, especially in his legs; and, in a short time, he discovered a swelling on the most prominent part of his shin-bone, about the bigness of a walnut, tho' flat; with such scabs, *&c.* as were formerly mentioned: and all this (as he told me) without reason to suspect a new infection.

In short, he applied to me a second time, and having advised with a physician, very eminent in his profession, he desired me to put him upon a new course of *merc. dulc.* but to give such small doses, at such intermediate distances, as to raise the salivation very slowly; and when once raised to the quantity of lib. iii. or lib. iv. in a day, to keep it up at that rate, by now and then dropping in a bolus, for two months at least; and, at the same time, to anoint the node on his shinbone with *ung. mercurial. &c.* I followed his directions punctually; and, indeed, all the symptoms went gradually off, except the forementioned node, which was little or nothing

thing altered, only he felt no pain in it: I was then ordered to give him the *pil. Æthiopic*; and *decoct. lignor.* after the uſe of which, for above a month, he began to feel ſome ſtinging pains in the node, and even through the whole leg, which daily increaſed. I had read the hiſtory of your alterative pills ſome time before: and, judging they might be more forcible than the *pil. Æthiopic.* in removing theſe ſymptoms, and the remains of the diſtemper; I hinted the ſame to the Doctor, and he chearfully agreed to uſe the one in place of the other. I ſhall here obſerve that the firſt ſalivation was raiſed with *ſcrup.* iv. of *merc. dulc.* and that he only ſwallowed *ſcrup.* viii. through the courſe of the other; by which it would appear that that medicine, had by no means become familiar to him. I prepared your pills preciſely as directed in Medical Eſſays vol. I. art. vi.; only inſtead of *calomel*, I took the ſame quantity of *merc. dulc.* and indeed of the ſame kind with which I had raiſed the laſt ſalivation: and, having divided the maſs, as there is alſo directed, I ordered him to take three pills morning and evening with a draught *decoct. lignor.* after each doſe, and to drink plentifully of

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the same thro' the day. This course, with a due regimen, he observed regularly for a month; but as he was quite wearied with the tedious course he had formerly undergone; and, as he afterwards told me, was resolved his disease should terminate soon, either in death or recovery; he frequently begged I would increase the dose of pills: upon which I allowed him to take four, morning and evening, but told him I durst venture no further; and withal what quantity of mercury they contained: he at the same time asked me, what would be the effect of over-dosing them? I told him, I believed they would either cause him vomit, or raise a salivation: this he pocketed up, as being signs sufficient to know when to stop: in short, as he had access to get the pills renewed at my shop when he pleased, without my knowing any thing of the matter; he had taken 24 pills each day for the continued space of six weeks, without being attended with the smallest inconvenience: and in that time the erratic pains and node had entirely vanished: but he afterwards made use of the pills, tho' in smaller doses. Having thus conquered his disease by his rashness; he triumphed as having alſo conquered the too timorous caution of his phyſicians. Upon a calculation, I find he had taken *unc.* xiv. of the alterative pills, which may contain about *unc.* iii. of ſweet mercury, from the firſt day of *December* 1739, to the firſt of *May* immediately thereafter; but that he had taken *unc.* ix. (conſequently above *ſcrup* xv. of ſweet mercury) within the ſpace of ſix weeks without intermiſſion. And ever ſince he has enjoyed perfect health. From this accidental experiment, I conceived ſuch a good opinion of thoſe pills, that I have frequently uſed them ſince in many different caſes; and with much ſucceſs. I have found moſt cutaneous diſeaſes yield very ſoon to this remedy; amongſt others, what is vulgarly called *Sibbens*, which rages in the moorlands near this place: whether this diſtemper is really a pox, I ſhall not pretend to ſay; only it is attended with the ſame ſymptoms: and I now begin to think, that, if theſe pills are given in ſuch large doſes as before mentioned, in which I accidentally found there was no danger; I don't doubt, but moſt of the diſeaſes which uſually yield to mercury, may be ſubdued by this medicine. I ſhall only

add

add, that I now omit the *resina guajac.* and increase the quantity of the gum; and I boil the *lign. guajac.* much longer than is directed in the *Edinburgh* Dispensatory, which, I am sensible, makes the decoction much stronger.

ART.

ART. XVI.

A Deſcription of the Seminal Veſſels, by ALEXANER MONRO, *Student of Medicine in the Univerſity of* Edinburgh.

THO' all anatomiſts agree, that the ſeed ſeparated in the teſticle, paſſes thro' the *epidydimis* to riſe in the *vas deferens*; yet few have a right notion of even the groſſer tubes in which it is conveyed. I have luckily made quickſilver paſs farther in theſe ſeminal veſſels, and thereby brought more of them into view, than I ſee deſcribed in any anatomical book; as you may judge by the preparations and pictures which I now ſend you, at my father's deſire.

De Graaf had formerly endeavoured, by diſſection, to unravel the *epidydimis*; and paints it *, as riſing from the teſticle in ſix or ſeven branches, which join at its *caput* or firſt remarkable turn into one pipe, the numerous contortions of which form the body of

* *De Vir. Organ. Tab.* 1. *Fig.* 3. *et Tab.* 4. *Fig.* 3.

of the *epidydimis*, terminating at its other end in the *vas deferens*.

This ſeems to have been overlooked, or not well underſtood, by moſt of the modern anatomiſts, who have differed widely, or talked with uncertainty, about theſe pipes; till of late, that the ingenious Dr *Haller*, by injecting quickſilver from the *vas deferens*, in the manner propoſed by my father in the *Medical Eſſays* *, and cauſing it to paſs as far as the teſticle, has been able to explain to us, with greater accuracy, the ſtructure of this intricate organ †. He agrees with *De Graaf*, that the *epidydimis*, from the *vas deferens* to its *head*, ſeems to be compoſed of a ſingle pipe, which he thinks might poſſibly be unlooſened, as *De Graaf* has repreſented; but does not affirm his having executed it. At its *head* indeed, he could divide it into ten or more vaſcular cones, from which veſſels go out, that, after forming a network with communications, give off ſtreight pipes which ſeem to plunge into the body of the teſticle. —His injection here generally failed, tho' he ſometimes obſerved, that it entered a few convoluted

* Vol. v. Art. xx. § 29.

† Phil. Tranſ. No. 494. § xii.

voluted tubes.—He mentions his having thrice ſeen a veſſel going off from about the middle of the *epidydimis* towards the ſpermatic cord, which he imagines is a *lymphatic*.

THAT this accurate anatomiſt's improvements, and the additions I am to make to them, may be better underſtood; I have firſt copied his elegant figure, and then ſubjoined ſome others I cauſed to be drawn of thoſe parts.

TAB. 3. *fig.* I. from Dr *Haller*.

A. The *vas deferens*.

B. The end or laſt turn of the *epidydimis*.

C. Its body, as it appears when the external membrane is diſſected off.

D. Its *head* unlooſened.

eee. &c. The vaſcular cones into which it divides.

ff. The *vaſa efferentia*, as he calls them.

gg. The network with communications.

hh. The ſtreight veſſels which ſeem to plunge into the teſticle.

IN *fig.* 2. The *tunica vaginalis* of the teſticle is cut off, and the teſticle, injected *epidydimis* and ſpermatic cord, are repreſented covered with their membranes.

A.

A. The ſerpentine beginning of the *vas deferens.*

B. The *globus minor*, or laſt turn of the *epidydimis*, from which the ſmall convoluted veſſel, *c*, which Dr *Haller* ſuppoſes to be a lymphatic, riſes.——If it is ſo, it has not the valvular ſtructure common to thoſe veſſels; for I can cauſe the quick-ſilver to flow either way in it.

D. The *corpus pyramidale.*

E. The body of the *epidydimis*, where the convolutions ſeen thro' the membranes, are larger than they appear in *fig.* 1.

F. The *head, globus major*, or firſt remarkable turn of the *epidydimis.*

G. Its riſe from the teſticle.

In *fig.* 3. and 4. The *tunica albuginea* is taken off from the ſides of the teſticle, and turned over upon the *epidydimis.*

A. The *tunica albuginea.*

B. The teſticle.

bb. Dr *Haller*'s ſtreight pipes from which

ccc. The ſmall convulted tubes upon the ſides of the teſticle are continued.

dd. In *fig.* 4. Repreſents a number of them upon the convex part of the teſticle, at the greateſt diſtance from the *epidydimis*

Fig.

FIG. 5. Repreſents the teſticle drawn aſunder in its middle from its convex ſurface towards the *epidydimis*.

FIG. 6. A ſmall piece is in the ſame manner ſeparated from the ſide of it.

IN both we obſerve Dr *Haller's* ſtreight veſſels entering at *bb*; and then dividing into *ccc* innumerable convoluted pipes, which compoſe almoſt the whole ſubſtance of the teſticle.

dd. A number of them where the injection has gone very far.

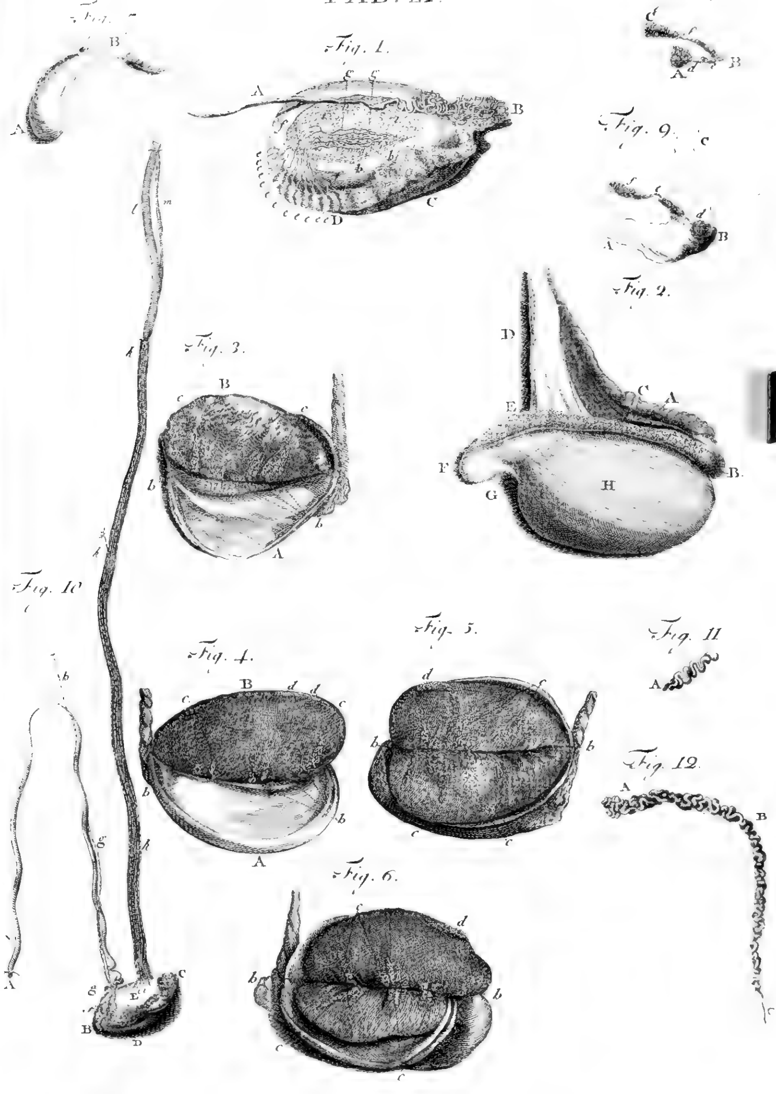
N. THESE preparations had been kept ſeveral months in ſpirits, and ſo often handled before the drawings were made, that many of the injected veſſels are deſtroyed, and the painter could not diſtinguiſh the membranous partitions of the teſticle.

As many of the moſt accurate anatomiſts, and even *Winſlow* *, have imagined, that the body of the *epidydimis* is compoſed of a number of pipes.——As Dr *Haller* does not poſitively affirm his having unlooſed it.——As the *vas deferens* is much larger than any pipe

* Exp. anat. traité de bas ventre. § 488.

* De vir. org. p. 42.

TAB. III.



pipe in the *epidydimis*, and as it ſeems difficult to conceive how the ſeed can be propelled thro' a tube ſo ſmall, ſo much convoluted, and of ſuch a length as the ſingle one, which, according to *De Graaf*, forms the *epidydimis*: on theſe accounts, I think, there is reaſon to doubt this ſtructure which he aſſerts, till we have more ſatisfying proof of it than has hitherto been adduced.——Such the following appears to be. If, in attempting to make preparations like to thoſe above deſcribed, an extravaſation happens in any part of the *epidydimis*, or if any of its convolutions are tied, not a drop of the injection can be puſhed farther towards the teſticle than this extravaſation or ligature: and, if the *epidydimis* is cut acroſs, while quickſilver is poured into the tube fixed in the *vas deferens*, it ſquirts out only at one orifice in this cut part; which is the plaineſt and moſt convincing demonſtration of the body of the *epidydimis*, being intirely compoſed of a ſingle tube convoluted in a moſt wonderful manner.

THO' my injection penetrated far into the ſeminal pipes; yet ſtill we are unacquainted with their origin. *De Graaf* * attempted in vain

* De vir. org. p. 42.

vain to diſcover this by maceration in water; nor have anatomiſts ſince his time been more ſucceſsful.

I have endeavoured ſeveral times to force quick-ſilver from the ſpermatic arteries into the ſeminal veſſels; but, tho' I made it return by the ſpermatic veins, I could never cauſe it to paſs into theſe convoluted tubes.

THIS I attempted the oftener, becauſe of the ſeeming probability of our being able to determine, better in this than in other glands, whether ſecretion is performed in continued cylindrical canals, according to *Ruyſch*, or, by the intervention of follicles, with *Malpighius*; ſince we can ſeparate the parts of the teſticle, without cutting, and even almoſt without laceration.

ART.

ART. XVII.

The Dissection of a Woman with child; and Remarks on gravid uteri, *by Doctor* DONALD MONRO *Physician at* London.

GENTLEMEN,

THE few histories of dissections and figures there are of *gravid uteri*, make me imagine, that an account of what I observed in one that fell under my examination, illustrated with a few figures which I took from the subject itself, would not be unacceptable to you.

WHILE I was at *Edinburgh* last year, the body of a woman, said to be six months gone with child, was brought to the anatomical theatre; which my father desired me to examine accurately, and particularly, to endeavour what I could to shew the *anastomosis* between the vessels of the womb and those of the secundines, if there were any such, as *Nortwyk* * affirms there are, and *Haller*, in his *Primæ Lineæ* †, seems to alledge; tho'

* Hist. uteri human. p. 1. § 7. and p. 2. § 83.

† § 830.

tho' in private converſation with this laſt named gentleman, when I was his ſcholar at *Gottingen*, he told me, that, what he had wrote concerning that ſubject, was upon the authority of others, and not from his own obſervation; for that he had had three women with child for ſubjects, whoſe bodies he had injected and diſſected; but had found nothing like an *anaſtomoſis* between the uterine and ſecundary veſſels in any of them: ſo that he now believed there was no ſuch *anaſtomoſis* as was alledged.

I opened the body of this woman; and, at firſt, imagined we had been miſtaken about her being with child; for the inteſtines came as low down as the *oſſa pubis*, and covered intirely the *uterus*: but, in turning up the inteſtines, we were ſoon undeceived, and ſaw the diſtended womb.——I then laid bare the deſcending *aorta*; and, having fixed a large pipe of a ſyringe into it, and warmed the *uterus* well, by pouring warm water upon it, and by keeping afterwards applied to it ſpunges filled with the ſame; I injected hot oil of turpentine, coloured with very fine powder of vermillion, into the *aorta*: and, after that, a coarſer injection of of melted tallow, diluted and coloured with part of the former injecting materials.——My father then demonſtrated to his pupils the parts in their natural ſituation; and I made the ſketch of the *uterus*, as repreſented in *tab.* iv. where the containing parts AAAAA, being laid aſide, you ſee the womb BBBB, with its *fundus*, raiſed higher in the right ſide than in the left;—CCC, the larger branches of the arteries on the foreſide of the womb, of a very ſerpentine form, and anaſtomoſing with each other;——DD, the round ligaments; EE, the Fallopian tubes fixed to the *uterus*, a good way below the *fundus*, inſtead of being fixed to its angles; as they are in an unimpregnated womb;—F, the urinary bladder, which, in this ſubject, roſe a good way above the *pubes*, turned down over it with a piece of the integuments;—G, the left *ovarium*.

NEXT day, the ſubſtance of the womb was cut thro' cautiouſly, a little farther forewards than the round ligaments; and, when the inciſion had penetrated into the *uterus*, a ſubſtance appeared which ſeparated quite eaſily from the more external parts, by being gently preſſed with the finger; and, by puſh-

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ing the finger alternately upwards and downwards, ſo much of this ſubſtance was ſeparated from the womb, as to allow a probe-pointed pair of ſciſſars to be introduced for enlarging the inciſion, parallel with the ſide of the *uterus.* I then ſaw that this ſubſtance was a villous membrane, which ſeparated almoſt of itſelf from the cut ſides of the womb when we attempted to raiſe them. In doing of this, we could not obſerve any tearing of veſſels or fibres, except in ſome places where ſmall convoluted arteries were broke off from the forepart of the *uterus* to adhere to this villous membrane. In this manner, this outer *lamella* of the *chorion* was every where ſeparated from the forepart of the *uterus*, which was cut thro' with a pair of ſciſſars all around its *fundus* and edges, as low down as the *oſſa pubis*, and was turned down over theſe bones.

The *uterus* was, as near as I could judge, about the thickneſs of an unimpregnated one: ſome of the gentlemen who were preſent at the diſſection, thought it was thicker, and others, that it was thinner.

The ſubſtance of the womb was compoſed of its firm ſtrong outer membrane which

is

is a continuation of the *peritoneum*: this adhered by a cellular membrane, thro' which a number of arteries ran, to a red coloured ſubſtance, which was about one half the thickneſs of the womb, and had a muſcular appearance; but I could not obſerve in it any real muſcular fibres, laid in regular *ſtrata*.—Within this there was a whiter coloured ſubſtance, which, on ſtretching, appeared plainly to be a cellular membrane, with its inſide ſmooth and poliſhed; but with a cellular appearance whenever it was ſtretched, by endeavouring to raiſe it from the ſubſtance of the *uterus*; for the internal very thin membrane, which covers the inſide of the womb, and is continued from the external *cuticula*, is ſo fine, that it is ſcarce to be diſtinguiſhed. Numerous veſſels, both arteries and veins, ran every where thro' this cellular ſubſtance.

In the middle of the cut ſides of the *uterus*, the empty veins were of a great ſize, in compariſon to what the arteries, diſtended with injection, were.

On the internal ſurface of the part of the womb which was turned down over the *pubes*, convoluted arteries were ſeen; and ſome orifices

fices of veins appeared. The outer *lamina* of the *chorion*, was ſcabrous, ſucculent, ſomewhat villous, of a pale fleſhy colour, and not at all tranſparent. The convoluted arteries, which I formerly ſaid had adhered to it, when the womb was ſeparated from it, were taken off with a pair of diſſecting forceps, without any diſcernible tearing of fibres. Croſs inciſions were then made into this outer covering of the *ovum*, and it was laid to a ſide, when we ſaw the ſecond *lamella* or clear tranſparent proper *chorion*, which ſeparated almoſt of itſelf, from the outer membrane; they being only connected together by a very fine and tender cellular ſubſtance. This internal *lamella* or proper *chorion* being cut thro', the fine *amnios* appeared, thro' which was ſeen the *foetus* ſwimming in its waters.

In Tab. vi. *fig.* 1. are repreſented AAA, the edges of the cut *uterus*, with large oblong orifices of the cut empty collapſed veins, and of the ſmaller round injected arteries;—B, the forepart of the *uterus* turned down between the thighs;—*CC*, part of the outer *lamina* of the *chorion*;—DD, part of the inner *lamina* of the *chorion*;—E, the *amnios* unopened,

opened, thro' which is ſeen the *foetus* in its waters.

THE membranes being all cut thro', and the waters let out, we more plainly obſerved, as in TAB. V. *fig.* I. the *foetus* A, with its head downwards; B, the navel-ſtring twiſted round its neck;—the *placenta* CC adhering to the back part of the womb, with the membranes DDD, lying folded upon its edges; and the cut ſides of the *uterus* EEE, where the orifices of the veins and arteries appear, as in the figure formerly referred to;—F, the *vena cava*;—G, *aorta deſcendens*;—H, the inferior meſenteric artery, to ſhew how high the *fundus* of the womb was raiſed.

THAT a compariſon of the ſituation of this *foetus*, with others, might be made; I ſketched in miniature the outlines of *Bidloo*'s, and *Albinus*'s figures. *Fig.* 2. of TAB. VI. is *Bidloo*'s, where the *foetus* lies in a bended poſture, with its head uppermoſt, and its *placenta* adhering to the forepart of the womb. And, *fig.* 3. of the ſame table, is copied from *Albinus*, where the *foetus* is with its head more perpendicularly downwards than in my ſubject; and no more, than the edges

 of

of the *placenta* adhering to the back part of the *uterus*, can be ſeen.

We ſought diligently for an *allantois*; but could ſee nothing that bore any reſemblance to it.

The following day, tallow diluted with oil of turpentine, was thrown into the umbilical arteries towards the *placenta*; and ſome of the ſame materials, coloured with *rad. anchuſae*, was injected into the umbilical vein. ———Tallow, diluted with oil of turpentine, and coloured with verdigreaſe, was likeways thrown into the uterine veins, after its cut edges were all tyed.

The *foetus* being taken out, we ſaw how far down the *placenta* adhered to the womb, as in Tab. v. *fig.* 2. where AA, is the loweſt part of the *placenta*, with ſome of the membranes turned upon it;—BB, a cluſter of *ſinuſes*, which appeared where theſe membranes had been taken from;—C, the entry to the *cervix uteri*;—D, a ſhare of the forepart of the *uterus* turned down.

Upon taking the *uterus* out of the body, and opening the back part of the *vagina*, there was, as in Tab. v. *fig.* 3. A, a conſiderable part of the *cervix uteri* very little diſtended;

diſtended; and the *os tincae* B, appeared like a ſmall *rima*, beſet all around with a glandular ring, which was filled with a thick *mucus*.

AFTER this, the upper half of the *placenta* was ſeparated from the womb, to which it adhered, by a cellular membrane of a ſtronger appearance than what connected the womb to any other part of the *ovum*.

EXTRAVASATED injection of all the four different kinds, was found between the *placenta* and *uterus*; the greateſt part of it, however, was of the vermilion injection, that had been thrown into the deſcending *aorta*. Tho' the extravaſated injection had made its way into the cellular ſubſtance both of the *uterus* and *placenta*; yet not one ſingle veſſel of the *placenta* was filled with any of the injection that came from the *uterus*; nor was a veſſel of the womb filled with that which came from the ſecundines.—In ſome places where there was none of the extravaſated injection adhering to the *placenta*; I obſerved that it was covered with the outer *chorion*; and ſome arteries broke off from the womb, adhered to it, but were taken away without laceration.

Most of the apertures commonly called *ſinuſes*, were full of the green injection; and ſome few of them had a very little of the vermilion in them. The internal membrane of the *uterus* was extended over part of each of their orifices, and there formed a ſort of valve.—Some of theſe orifices were ſo large, as to have allowed one's little finger to have been put into them; while others were ſo ſmall, as ſcarce to admit the point of a probe: and there were of all the different ſizes between theſe. — The veins continued from them were of a conſiderable diameter; but not ſo large as the *ſinuſes* themſelves.

The extravaſation having made the contiguous ſurfaces of the *placenta* and womb of my ſubject leſs diſtinct than I could have wiſhed; I drew ſmall figures of theſe parts, from *Albinus*'s elegant large original ones. *Fig*. 4. of Tab. vi. is the *placenta*, exhibited with its ſurface contiguous to the *uterus* in view; where the engraver has not done juſtice, in repreſenting the *monticuli* and *ſulci* of the *placenta*.

Fig. 5. Is better copied, where the oviform figure of the *uterus* of his ſubject, ſtrikes the eye, with the orifices of the cut veſſels in its ſubſtance,

ſubſtance; and the inſertion of the round ligaments are ſeen, at AA. The numerous *ſinuſes* ſufficiently ſhew where the *placenta* adhere to it. To ſome few of the valvular membranes of thoſe *ſinuſes*, I have put *a*; and *b* is placed near to what repreſents their orifices;—*cc*, point ſome of the ſerpentine ſmall arteries;—D, the valvular protuberance at the back part of the *cervix uteri*;—*e*, the *os tincae* with part of the *vagina* opened.

Tab. vii. *fig.* 1. Is the appearance which a part of the *uterus* of my ſubject had, where the *ſinuſes* were not diſtended; and ſome of the ſerpentine uterine arteries were diſtinctly ſeen.

Fig. 2. A *ſinus* A, with its valvular membrane cut away, when the orifices of three veins B are ſeen.

Fig. 3. The three veins ſlit open, till where they meet in one trunk.

Fig. 4. Is a copy of a little piece of *Albinus*'s figure, where the *ſinuſes* and ſerpentine arteries are repreſented of the natural ſize.

The ſpermatic vein appeared of ſuch a monſtrous ſize, and ſo much larger than the ſpermatic artery, that I diſſected and made a draught

draught of them, as repreſented in *fig.* 5. where the vein AA, and the artery BB, appear twiſted frequently round each other; and ſometimes as if the artery entered into the vein;—C, the *fimbriae* of the *tuba Fallopiana*;—D. the *ovarium;*—E, part of the *ligamentum latum.*

THAT others may judge how long my ſubject had been with child, I have added the outlines of the *foetus* of the natural dimenſions, in *fig.* 6.

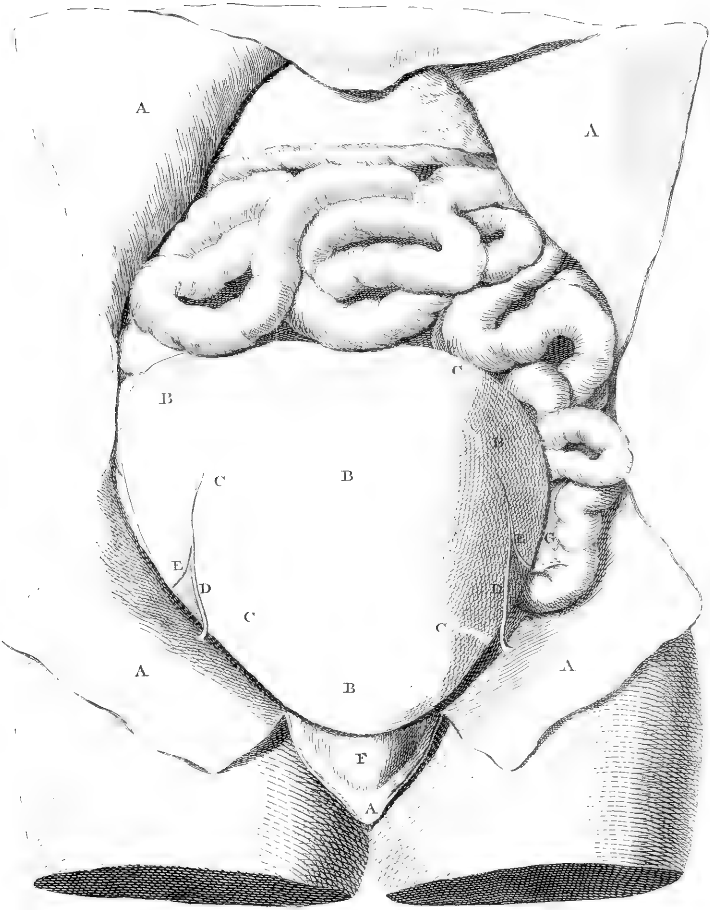
THUS, Gentlemen, I have fairly related all that I obſerved in the diſſection of this pregnant *uterus*; and beg leave to be allowed to add ſome few remarks upon the whole.

FROM the figure of the womb I diſſected, compared with thoſe of *Cowper*, *Nortwyk*, *Albinus*, *Hunter* and others, it appears, that the *fundus* of the impregnated womb, is always greatly extended upwards, in the time of gravidation, by which the round ligaments and Fallopian tubes, which are inſerted into the angles of the unimpregnated womb, are then much lower than the *fundus*: for, in my figure, theſe ligaments and tubes, are almoſt one third of the length of the womb

lower

† Hist. Uteri, pars 2. § 77. p. 70.

TAB. IV.



† Hist. Uteri, pars 2. § 77. p. 70.

TAB. V.

Fig. 1.

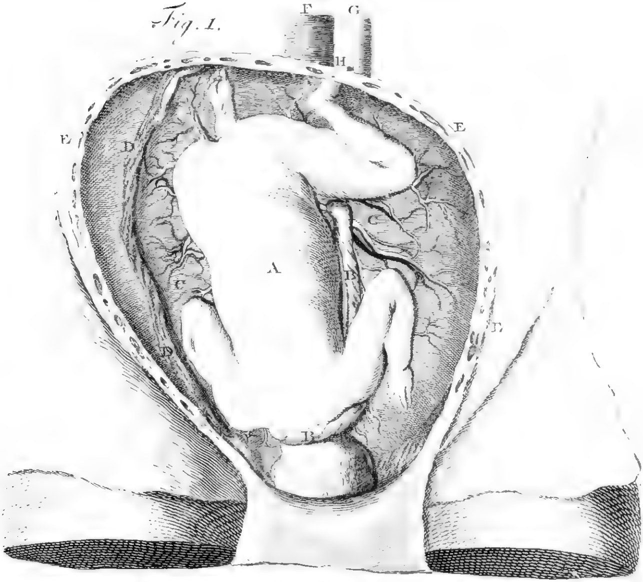


Fig. 3.

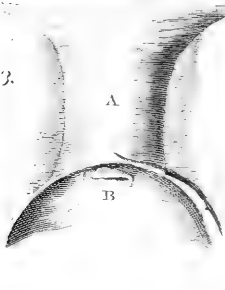
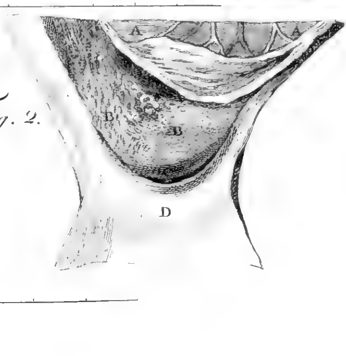


Fig. 2.



† Hist. Uteri, pars 2. § 77. p. 70.

TAB VI

Fig. 1.

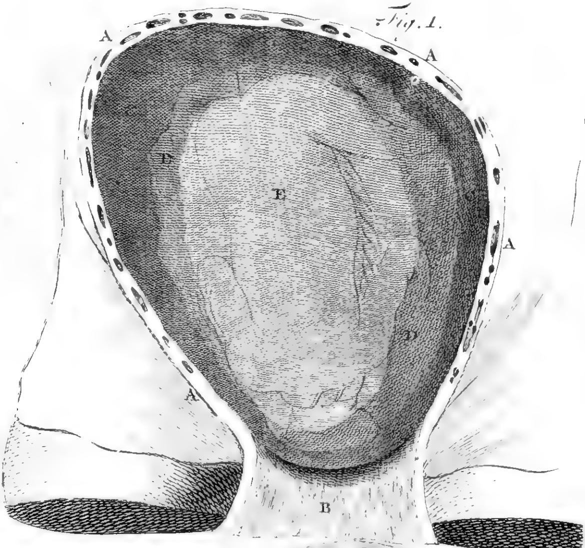


Fig. 2.

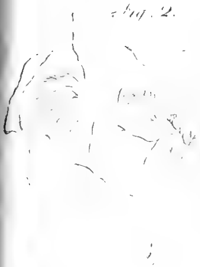


Fig. 3.

Fig. 4.

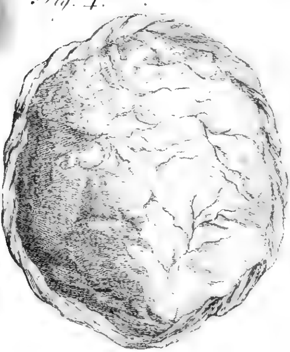
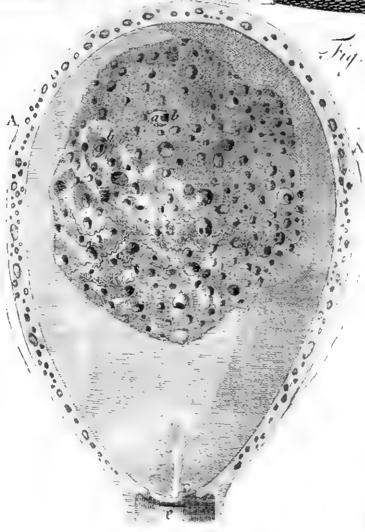


Fig. 5.



† Hist. Uteri, pars 2. § 77. p. 70.

TAB. VII.

Fig. 1.

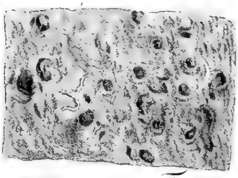


Fig. 3.

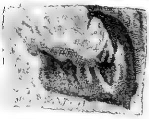


Fig. 2.

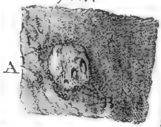


Fig. 4

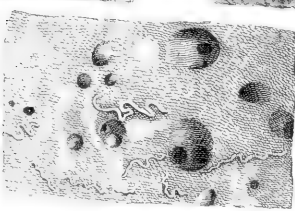


Fig. 5.

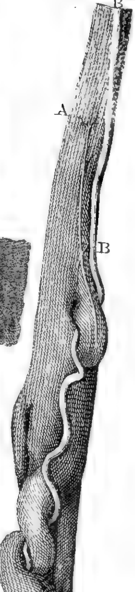
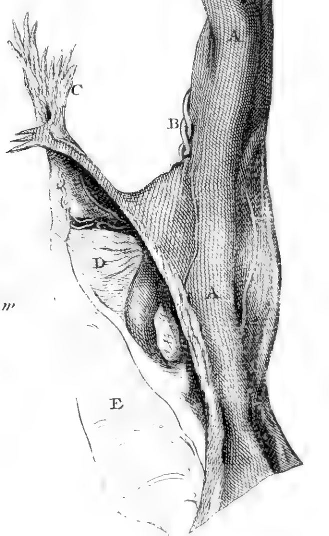



Fig 6.

Fig. 7.

lower than its top: and *Vesalius* *, *Nortwyk* † and others, have obſerved, that the further advanced the woman is in her pregnancy, the lower they deſcend.

THE *uterus*, by impregnation, may be forced into different ſhapes. My figure and that of *Bidloo*, which reſemble each other greatly, ſhew, that one ſide of the womb may be raiſed much higher than the other. *Vater* and *Albinus*, have painted the *uterus* as oval; and *Nortwyk* has repreſented it flat at top: but, as all theſe three Gentlemen's figures were done from wombs which were previouſly taken out of the body, we can form no judgment of the ſhape of the womb from their draughts; becauſe when the *uterus* is taken out of the body, it aſſumes a different ſhape, according to the manner in which it is laid down. In Mr *Hunter*'s elegant figure, which was taken from the *uterus in ſitu*, the womb is more oblong than in mine. And in two other pregnant women I ſaw diſſected, the one ſeven, and the other four months gone with child; the *uterus* was in both more globular

* lib. 5. cap. 17.

† Hiſt. Uteri, pars 2. § 77. p. 70.

globular than in the present subject. Possibly the figure of the womb may gradually become more oval, as the woman approaches to her time. The part likewise to which the *placenta* adheres, or the different situation of the child, may cause great variety here.

The strong outer membrane and cellular one immediately within it, did not seem in any of the pregnant *uteri* I saw dissected, or by the accounts of any authors, to have been much altered by the great distension they underwent.

Heister *, *Nortwyk* †, and Mr *Hunter*, observed the same muscular appearance under this outer cellular membrane, as I did, without any regular layer of fibres; as *Ruysch* has described and painted ‡: nor could I observe any such regular fibres in a woman who died a few hours after child-birth, whose body I dissected while I was in Mr *Le Cat*'s house at *Rouen*, tho' *Nortwyk* || seems to think, that if they are to be seen in any *uterus*,

* Anat. Compend. Tom. 2. p. 80.

† Hist. Uteri pars 2. § 87. p. 106.

‡ Advers. Dec. iii. Tab. 3. Fig. 1.

|| Hist. Uteri pars 2. § 87. p. 107.

rus, it muſt be in ſuch a one. Some few fibres there were, that went in an irregular, tho' ſomething circular manner; which, with the help of my diſſecting knife, I could have carved into a very pretty muſcle. Authors have differed much about this muſcle; ſome affirming that they have ſeen it, while others have denied that any ſuch thing ever exiſted. Theſe different opinions are all taken notice of, and quoted in *Boerhaave*'s *Praelect.* publiſhed by *Haller*; Vol. v. § 664. to which book I ſhall refer you.

I have already mentioned, the womb's being much about the thickneſs of an unimpregnated one: this however is not always the caſe; and authors have differed vaſtly in their opinions in this point: ſome alledging, that the *uterus* is always thicker; others that it turns thinner; while others have affirmed, that it does not alter in its thickneſs by impregnation. This great difference of opinions, muſt have been owing to the different *uteri* which have been examined. As Dr *Nortwyk* has quoted the paſſages of different obſervators concerning this diſpute; I ſhall refer you to his *Hiſtoria Uteri* * for their ſen-

* Pars 2 § 80.

timents about this matter; and only obſerve, that Dr *Smellie*, Mr *Hunter*, Mr *M'Kenzie* and others, who practiſe midwifery here, and have had occaſion to ſee a good number of impregnated wombs, are of opinion, that in general, the *uterus* does not alter much in its thickneſs by being diſtended; tho' ſometimes it is found thicker, and ſometimes thinner, than ordinary: and in a collection of *uteri* in Dr *Smellie*'s poſſeſſion, there are wombs which ſeem to favour all the three different opinions. One of the wombs in this collection, is remarkably thin, not being above the third part ſo thick as an unimpregnated *uterus* generally is. If I was to form a judgment, from the few *gravid uteri* I have ſeen, I would be inclined to think, that if the womb alters in its thickneſs at all, it rather turns thinner; but the difference is ſo ſmall, for the moſt part, that it is difficult to form a judgment about the matter. I ought however to obſerve, that the *gravid uteri* will be conſiderably thicker during life, when they are full of blood, than they are in dead bodies, where the veſſels are all collapſed. The difference was conſiderable, in the bulk and thickneſs of the womb, before we injected the

the veins, from what they were, after we had filled them with tallow.

By ſome few of the *ſinuſes* having a little of the red injection in them, it may be conjectured, that ſome of the ſmall branches of the uterine arteries opened into them: but I could not obſerve their orifices; and they themſelves were ſo ſmall, that I only ſaw one or two ſmall twigs running along the membrane that lined ſome of the *ſinuſes*.

Since, neither in this ſubject, the leaſt drop of either the injection that was thrown into the arteries or veins of the mother; nor in another woman, who died when four months gone with child, whoſe uterine arteries I injected with coloured tallow; any of the injections were found in the ſecundary veſſels, tho' a great deal of injection, in both ſubjects, was found extravaſated between the *placenta* and *uterus*; I muſt be of opinion, that there is no *anaſtomoſis* between the uterine and ſecundary veſſels; and I could eaſily ſee, how the extravaſated tallow, injected with force, could make its way into the cellular ſubſtance both of the *placenta* and womb.

Such ſerpentine convoluted very ſmall arteries, which Dr *Albinus* has taken notice of, and

and which I ſaw likewiſe, in this ſubject, on the inner ſurface of the womb, are not to be obſerved in any other organ of the body, ſo far as I remember.

NOTHING remarkable was obſerved in the membranes of the *ovum*, more than what is mentioned by the accurate anatomiſts *Albinus* and *Haller*: but Mr *Hunter* obſerved a number of veins going from the *uterus* in continued trunks, into the ſubſtance of the outer *lamella* of the *chorion*, in a pregnant woman he diſſected, and whoſe veins he had previouſly injected with yellow wax: theſe he has delineated in one of his figures, which will ſoon be publiſhed. We could obſerve none of them in our ſubject; but then, all the forepart of the *uterus* was ſeparated from the outer *lamella* of the *chorion*, before the veins were injected. Tho' Mr *Hunter* obſerved veins, he could not ſee one artery continued from the womb to the *chorion*; and the arteries of the *uterus* had been filled with injection, as well as the veins.

IN five of the ſubjects mentioned by my father, in *Medical Eſſays* *, two of which I ſaw, the *placenta* adhered to the forepart of the

* Vol. 2. Art. 9.

the womb; while in our preſent ſubject, and thoſe of *Vater* and *Albinus*, the *placenta* was fixed to the back part of it, and Dr *Smellie* aſſures me, he has found it attached to the *os uteri*. I think therefore it is plain, that there is no particular part to which we can ſay the *placenta* is always fixed: from which it may be juſtly concluded, that the *chorion* is not all equally capable of being *placenta*; and that the *placenta* does not owe its exiſtence to the energy of any particular ſpot of the womb; but that it is an original part which adheres to whatever place, not only of the womb, but of the Fallopian tubes * or *abdomen*, † it happens to be contiguous to; where it cauſes a conſiderable flow of liquors, by the abſorbing powers of its veſſels; in the ſame way as the ſucking of the nipple is the moſt effectual way of cauſing a flow of milk to the breaſts. Thus the part of the human womb where the *placenta* is applied, has its extreme veins enlarged into *ſinuſes*: and the ſmooth ſurface of the womb and its *cornua* in cows, is raiſed into glandular-like bodies where each *placenta* is placed.

THO'

* See two examples of this in DE GRAAF de mulier. organis cap. xiv. quoted from RIOLAN.

† An example of this, in *Hiſt. de l'Acad. des Sciences* 1716.

THO' the contrivance of the blood returning from the *foetus* being poured into *ſinuſes*, is a very good precaution againſt haemorrhagies; yet the veins of the womb that communicate with thoſe *ſinuſes*, being ſo large as they are, one would think, that more frequent haemorrhagies might happen at delivery, was it not for the oblique manner in which the moſt internal coats of the womb are placed by way of valves over the orifices of theſe *ſinuſes*; ſo that whenever the womb collapſes or contracts, theſe orifices are almoſt intirely covered by theſe membranes.

THE oblique ſituation of the *foetus*, the erect, or what is generally called the natural one, in the two I ſaw formerly, the tranſverſe one of *Nortwyk* and *Cowper*, perſuade me, there is no determined ſituation for a *foetus*, as is commonly thought; but that it may change places in its ſtirrings, as women generally feel.

IT has long been the received opinion, that *foetuſes* are moſtly placed in the womb, in what is called the erect or natural ſituation; that is, with the child's forepart towards the mother's belly; its head uppermoſt, and reclined

clined forwards and downwards, and that in the last months of pregnancy, or immediately before birth, the head, by its weight, falls down towards the *os uteri*, so that the face of the child is turned towards the *os sacrum* of the mother, and in this manner is delivered. All this account, both of the child's position, and falling down of the head, I must doubt of; for, when I attended courses of midwifery, I examined a great number of women, in all the different times of pregnancy, from six to nine months gone with child, and in the greater number of them I felt the head down; and Dr *Smellie* and Mr *Hunter* assure me, they have generally observed the same thing: so that this seems to be rather what ought to be called the natural situation. If the child be sometimes in the erect situation, and its head falls down, I cannot think that this change of posture is owing to its greater weight at this, more than at any other time; for the head of a *foetus* is proportionally larger and heavier, the younger it is: but to the child, thro' a natural instinct, endeavouring to avoid the pressure which its head will suffer by the contraction of the bottom of the womb, and the detrusion

of

of the *diaphragm* during labour-pains, and that, by being turned, its feet may act with advantage, by pushing against the *fundus uteri*, to assist the delivery.

THE enlargement of the veins of the womb during pregnancy, is really surprising, as may be judged by observing their transverse sections in their collapsed state, as represented in the cut edges of the womb; see TAB. v. and vi. and by the size of the spermatic veins, in TAB. vii. *fig.* 5. I ought however to observe, that the trunk of the vein is here represented less than it should have been; for when the womb was cut out of the body, some of the injection was, by accident, pushed out of the veins, before the figure was made. The situation of this vein and artery, may let us see, how easily *Arantius* * and other anatomists, before the injecting art was found out, might imagine the artery entered into the vein; that the arterious and venous blood might be blended together before they reached the womb. Injections plainly shew, there is no such uncommon *anastomosis* between the vein and artery.

THE

* ARANTIUS de foetu, cap. iii. p. 7.

The womb, by being diſtended, preſſes on the iliac veins, and makes a difficulty to the blood returning from the lower extremities, and even to the blood returning by the branches of the iliac, from the womb itſelf, long before the *uterus* has roſe ſo high as to preſs upon the ſpermatic vein: is not then this difficulty of the blood's returning by the iliacs, the cauſe of the vaſt diſtenſion of the ſpermatic veins during gravidation, as well as it is often the cauſe of the oedematous legs, and ſwelled *labia pudendorum*, at that time?

ART. XVIII.

Additional Obſervations on Gravid Uteri, *by* ALEXANDER MONRO, *Student of Medicine in the Univerſity of* Edinburgh.

GENTLEMEN,

HAVING wrote to my brother a few obſervations which I had made in examining a *gravid uterus*; he deſires me to add ſuch of them, as I thought might be a proper ſupplement to the paper which he ſent you lately on this ſubject.

THE woman I diſſected was about forty years of age, had born four children, and was ſaid to be five months gone with child; her *uterus* being almoſt as large as in my brother's ſubject, tho' more of an oval ſhape, with the forepart of it contiguous to the *peritoneum*.

HAVING obſerved ſo far haſtily, I was obliged to remove it from the body.

THE *os tincae*, then ſeen from the *vagina*, was conſiderably larger, and the *cervix uteri* was

was diſtended lower down than is repreſented in your TAB. 5. *fig*. 3. which poſſibly might be owing to this woman's weakneſs, and to the number of children which ſhe had born.

A liquor, of a thicker conſiſtence than the oil of turpentine and vermillion which my brother made uſe of, being injected into the *hypogaſtric* arteries, filled the ſpermatics, and a conſiderable artery in the *round ligament*. The joining of the trunks of the two former upon the ſides of the *uterus* was ſo large, that it was impoſſible to ſay, from which of them the branches diſtributed to its ſubſtance were derived. The right ſpermatic was ſmall, but the left was as large as a gooſe-quill; the hypogaſtrics being of a middle ſize betwixt the two.—By blowing air into any of the veins, we had ſufficient proof of their monſtrous ſize, and of their numerous and very large communications; the whole *uterus* being at once inflated.—Correſponding to the artery before mentioned in the *round ligament*, I obſerved a large vein in it, which probably diſcharged itſelf into the external iliac.

WHEN the *uterus* was cut open upon its fore part, near three fourths of its thickneſs had plainly the appearance of a muſcle; the fibres

fibres being of a pale red or fleſh-colour: and when part of it was boiled in water, we could not have diſtinguiſhed it from any other piece of boiled fleſh; moſt of its fibres then ſeeming to run obliquely from the *cervix* towards the *fundus*; others appearing tranſverſe. To which if we add the known ſenſibility of the womb, and its ſtrong and ſudden contraction at birth, we have enumerated all the eſſentials required in the conſtitution of a muſcle.

As the internal cellular, or rather ſucculent fungous membrane of the womb, eſpecially where the *placenta* is fixed, is of a conſiderable thickneſs; I cannot conceive, how any thing like to an orbicular or radiated muſcle could ever be alledged to be ſeen upon its inner ſide, without diſſection.

THE *placenta* and *foetus* were ſituated nearly in the ſame manner as is repreſented by *Bidloo*; a ſmall ſketch of whoſe figure is in your TAB. vi. *fig.* 2.: tho' poſſibly the ſituation of the *foetus* might have been altered, by our having carried the *uterus* above half a mile before we examined it.

THE *liquor amnii* was of a clear brown colour; upon adding *oil* of *vitriol*, or *ol. tart.*

p. d.

p. d. to it, no remarakble change could be observed: which proves that it contains little serous or urinous; for the *oil of vitriol* would have coagulated the former, and the latter would have betrayed itself upon the addition of the fixed alcali.——When it was brought to a boiling heat, it threw up a thin whitish scum; and an ounce of it boiled dry, left about a scruple of what appeared to be a serous *coagulum*.

We could not observe any thing like an *allantois*: and, when the *urachus* was afterwards examined, it neither allowed air, water, or quick-silver, to enter it from the bladder; tho' I have seen it pervious for an inch or more in children, born at the full time.

The *cervix uteri* being now cut open; the whole inner side of it was covered over, especially near to the *os tincæ*, with clusters of vesicles, some of which were of a considerable size, filled with a dark brown-coloured mucus; and small orifices, containing the same kind of liquor, appeared in their interstices.

When the *placenta* was taken off, the injection, which had been thrown into the hypogastric arteries, was found extravasated betwixt

twixt it and the *uterus*; many lumps of the wax being, as it were, wedged into the ſubſtance of the *placenta*, without having entered any of its veſſels.—We then ſaw many ſmall ſerpentine arteries with their orifices opening upon the internal membrane of the *uterus*; and other, generally larger, convoluted arteries, whoſe extremities we could not as yet obſerve, appearing prominent in the interſtices of cavities filled with the wax; and which, in the foregoing paper, are mentioned under the name of *ſinuſes:* the real ſtructure and ſituation of which, being little underſtood, tho' often talked of, I ſhall relate all that I obſerved in this ſubject; and then, by comparing it with the common deſcription, we may better underſtand what has miſled moſt anatomiſts.

The *ſinuſes* are ſeen without any previous diſſection upon the inner ſide of the *uterus*, chiefly where the *placenta* has been fixed; for there are but few of them, and theſe very ſmall, in any other part of it.—Their ſides are membranous, that, next to the cavity of the womb, being in each very thin, with a large orifice in it.—They can be readily diſtended, by blowing air, or injecting a li-

quor

quor into the veins; or they may be filled from the arteries, tho' with much greater difficulty, as happened in our injection: when ſtretched, they appear of a ſpheroidal ſhape; and, the diameter of their orifices being leſs than that of their cavities, we may give them the name of *ſinuſes*.—Upon diſſection, we obſerve arteries opening, at leaſt into ſome of them, as I ſhall afterwards prove, and conſiderable veins are continued from all of them; the veins and *ſinuſes* having all large communications with each other; and, tho' ſome of the ſinuſes may have only two or three ſmall veins opening into them, yet, in general, the diameter of the veins continued from them, is not greatly inferior to that of the ſinus.

SINCE, from this deſcription, it appears, that theſe cavities are only the ſomewhat dilated extremities of the branches of the veins; we may account for their formation, from the more than ordinary difficulty there is in the return of the blood from the *gravid uterus*; which being therefore accumulated, muſt ſtretch all the veins greatly, but ſuch parts of them moſt as reſiſt the leaſt; conſequently their extremities, which are only co-

vered

vered by the lax internal cellular membrane of the womb, will be moſt dilated, or put on the appearance of ſinuſes ; and this effect will be moſt conſpicuous where the greateſt flow of liquors is, that is, at the place where the *placenta* is attached to the womb ; as my brother has juſtly remarked.

If we compare the above deſcription with the common notion of a ſinus, we ſhall find they differ widely.—They are generally defined to be large cavities in the middle of the ſubſtance or fleſhy part of the womb, that have ſmall branches of arteries and veins opening into them, with canals, whoſe diameter is conſiderably leſs than that of the ſinus, running obliquely thro' the ſubſtance of the womb to open upon its inner ſide.—But ſince, after a diligent ſearch, no ſinuſes of this form could be ſeen in this ſubject; as neither my brother nor the accurate *Albinus* paint or deſcribe any ſuch ; and, as the ingenious and diligent Dr *Haller* poſitively affirms, in one of his lateſt works *, that after repeated experiments, he could not obſerve them ; it ſeems probable, that the trunks of the veins have been miſtaken for ſinuſes ; and their branches, for canals opening into

* Prim. Lin. phyſ. § 804.

into the cavity of the womb; nay, a late learned author applies theſe names of ſinuſes and canals, to the trunks and branches of the veins painted in *Albinus*'s 7th table of the *gravid uterus*.

THE exiſtence of ſinuſes and canals being then imaginary, and the cavities which we have deſcribed under the name of *ſinuſes* being intirely the creatures of impregnation, and not to be found in the *uterus virgineus*; or being at any rate, from the ſize of their orifices, unfit receptacles of the blood; we cannot ſurely account for the *menſtrual flux* or any other phœnomenon from ſuch a ſtructure.

As the *ſinuſes* were filled by our courſe injection, thrown into the arteries, I imagined, that, by a careful diſſection, the openings of the arteries into them might poſſibly be obſerved; but, I found more difficulty than I expected, from the ſize and very large communications of moſt of the *ſinuſes* and veins. Near to the edges of the *placenta*, where they were not ſo frequent, I was lucky enough to diſcover, with certainty, ſeveral orifices of arteries, ſome of which were of a conſiderable diameter, opening directly into the *ſinuſes*: and ſince none of the anatomiſts have painted or defcribed thefe openings, tho', in the imaginary ftructure of the *finufes*, they are fuppofed; I have caufed a few of the moft diftinct to be reprefented in TAB. 7. *fig*. 7. where A reprefents the inner furface of the womb, to which the edge of the *placenta* had been fixed;—*bb*, two fmall arteries appearing after having pierced the mufcular fubftance;—*ccd*, their ferpentine and fpiral turns;—*fs*, their openings into the *finufes*; which were plain beyond difpute, when the wax was taken out of the *finus*, and the artery gently preffed, the *finus* being again in part filled from it, as is reprefented at *f. w*. Nay, it is evident, that the arteries had communications with the *finufes* alfo in my brother's fubject; fince the *placenta* was covered over with the extravafated oil of turpentine and vermilion, which was injected into the *aorta defcendens*; and which therefore feems to have filled the *finufes*, but to have been thruft out of them by the coarfe green injection afterwards thrown into the veins.—*ee*, fmall arteries which were obferved every where opening upon the internal membrane of the womb;—*ff*, others which feem to have been torn in feparating the *placenta*.—And

And, in all that part of the womb to which the *placenta* had been fixed, when the cellular membranes and extremities of the veins were diſſected off; innumerable arteries, of all ſizes, from that of a crow-quill downwards, convoluted in a moſt ſurpriſing manner, were obſerved; which were nothing ſo conſpicuous in any other part of the womb, treated in the ſame manner.

ART. XIX.

Of the difference between Reſpiration and the Motion of the Heart, in ſleeping and waking Perſons, by ROBRT WHYTT, M. D. *&c.*

OF all the parts of the human body, there is none whoſe ſtructure is more ſubtile, and whoſe ſeveral functions and uſes are leſs known, than thoſe of the brain. It is little to be wondered at, therefore, if authors have failed of giving a ſatisfactory account of *ſleep*, which is one particular ſtate of this unknown organ.—We have elſewhere offered our conjectures concerning the reaſon why the vital motions continue in time of ſleep; when the organs of ſenſe becomes leſs fit to receive the impreſſions of external objects; and when the muſcles of voluntary motion are more relaxed and remain at reſt *. But, as reſpiration and the motion of the heart,

* Eſſay on the Vital and other Involuntary motions of animals, ſect. xii.

heart, ſuffer ſome change in time of ſleep, *i. e.* become then ſlower, fuller and more equable, than when we are awake *, it may be worth while to enquire a little into the reaſon of this *phaenomenon.*

IT has been ſhewn, that as the dilatation of the ventricles of the heart, is owing to the force of the refluent venous blood; ſo their contraction is produced, by the ſame blood acting upon them as a *ſtimulus* †: and that the heart can only be affected by *ſtimuli*, in ſo far as it is a ſentient organ, *i. e.* endued with feeling ‡. Whence it muſt follow, that the ſlowneſs of the pulſe in ſleep, and indeed in every other caſe, can only ariſe from one or more of the following general cauſes. *viz.* 1. A diminution of the ſtimulating quality of the blood. 2. Its ſlower return to the heart; or, 3. A leſs degree of ſenſibility or aptitude for motion in the heart itſelf.

I.

* BOERHAAVE Inſtitut. Med. § 599.

† Vid. Eſſay on Vital and other Involuntary motions of animals, ſect iii. and iv.

‡ Ibid. ſect. x. p. 271. *&c.*

I. When one has eat or drunk a great deal before ſleep, his pulſe will be, not ſlow, but quick and full; becauſe the ſtimulating power of the blood is increaſed, by a large quantity of chyle received into it. Much the ſame thing may happen from ſleeping in too hot an air, or under too great a weight of cloaths: for we know, that heat quickens the circulation of the fluids in all animals. On the other hand, when one has faſted long before ſleep, and lies very cool, his pulſe will, in time of it, be unuſually ſlow.

But when the blood is neither loaded with new chyle, nor altogether deſtitute of it; neither too much heated by cloaths or the external air, nor too cool, thro' want of proper covering; its ſtimulating quality will neither be augmented nor diminiſhed by ſleep; but will continue the ſame as in a perſon who is awake in the ſame circumſtances. It remains therefore, that the ſlowneſs of the heart's motion in time of ſleep, be owing, either to the ſlower return of the venous blood to it, or to ſome diminution of its ſenſibility.

II. Every one knows, that the affections of the mind diſturb the motion of the heart; that

that the pulſe is quicker when we ſit or ſtand than when we lye; and that the action of the muſcles of voluntary motion, not only promotes the return of the blood to the heart, but determines it thither, with much greater force than uſual. In ſleep therefore, where the horizontal poſture of the body, the quieſcence of the voluntary muſcles, and compoſure of the mind, all concur to render the return of the venous blood to the heart, more equable and ſlow, the contraction of this muſcle muſt be renewed at greater intervals, and with more regularity, than when we are awake, and the circulation is quickened or diſturbed by ſome, or all, of the above-mentioned cauſes. But, if no farther circumſtance, tending to retard the hearts motion, were found in ſleep, the pulſe ſhould be equally ſlow and full in a waking perſon lying at reſt in a horizontal poſture, and whoſe mind is compoſed, as in the ſame perſon in time of ſleep; which, however, does not ſeem to be entirely the caſe: for tho' the difference may be ſmall, and there may be many cauſes which may render it difficult, by obſervation, to determine that difference with any degree of certainty; yet the remarkable

ſlowneſs

ſlowneſs and fulneſs of the pulſe in the deep ſleep, accompanying an apoplexy or occaſioned by *opium*, makes it highly probable, that even in the much gentler ſleep of perſons in health, the pulſe is ſomewhat ſlower and fuller, than it would be, merely from the compoſure of mind, horizontal poſition of the body, and quieſcence of the muſcles of voluntary motion. Let us therefore ſee, whether the ſlowneſs and fulneſs of the heart's motion in time of ſleep, may not be in part owing, to ſome diminution of the ſenſibility of this organ.

3. In time of ſleep, as the exerciſe of the ſeveral ſenſes, is either ſuſpended or much impaired; ſo the ſenſibility or feeling, with which the organs of the body are more or leſs endued, ſeems to be rendered leſs acute. Thus we feel ourſelves affected with a kind of *ſtupor*, when we are juſt falling aſleep, and are then inſenſible of leſſer *ſtimuli*. The thin rheum, which, by irritating the windpipe, keeps us almoſt perpetually coughing when awake, gives us little or no diſturbance in ſleep: any extraordinary *ſtimulus* in the guts is alſo leſs perceived then; and hence it is, that a doſe of any purgative taken at night, is much

much longer before it operates, than when it is ſwallowed in the morning. If the heart, therefore, like the other organs of the body, becomes leſs ſenſible or irritable in time of ſleep, it will not be ſo quickly excited into contraction as uſual, by the venous blood ruſhing into its cavities; and hence its contractions will not only be more ſlowly repeated, but the pulſe will be full, becauſe the ventricles do not contract till they are much diſtended with blood. This will ſtill further appear, if we conſider how remarkably ſlow and full the pulſe is, in an apoplexy, where the ſenſibility of all the parts and their aptitude for motion, are much more impaired than in common ſleep, and how *opium*, which occaſions ſleep, and leſſens the ſenſe of feeling every where thro' the body, when given in a large doſe, renders the pulſe uncommonly ſlow and full. Thus the heart of a frog, into whoſe ſtomach and guts I had, an hour before, injected a ſolution of *opium*, was obſerved to beat near four times ſlower than uſual; and the auricle and great veins leading to the heart, were remarkably diſtended with blood, as was alſo the ventricle of the heart before every contraction. At

 the

the ſame time the other muſcles of this animal were ſo intirely deprived of feeling, as not to be, in any degree, affected by pricking or tearing their fibres*.

Does not the ſlower digeſtion of the aliment in time of ſleep, proceed, partly, from the periſtaltic motion of the ſtomach and guts being then repeated after longer intervals? In dogs who have got a large doſe of *opium*, this motion is very much leſſened or totally ſuſpended, the food laſt received into the ſtomach remains there indigeſted, the guts are more than uſually empty, and the lacteal veſſels inviſible†. In ſleep, therefore, not only the heart, but the ſtomach and guts alſo, become leſs ſenſible of the *ſtimulus* uſually affecting them, and conſequently repeat their contractions more ſlowly.

De Gorter, differing from *Boerhaave* and other authors, thinks, that the pulſe muſt be, not fuller, but ſofter in time of ſleep, becauſe the circulation of the blood is allowed, then, to proceed more ſlowly than when we are awake.

* Eſſay on the Vital and Involuntary motions, &c. p. 372.

† Kauu impet. faciens Hippocrat. dictum, N° 434. 435.

wake *. But, from what has been ſaid, it appears, that the fulneſs of the pulſe in ſleep, is not owing to the quicker circulation of the blood, but to a leſs degree of ſenſibility in the heart, whereby its ventricles are not excited into contraction, till they have been more fully dilated, than uſual, by the returning blood. It is, however, to be obſerved, that the fulneſs of the pulſe in ſleep, may be owing, partly, to the fluids paſſing, with greater difficulty, thro' the very ſmall lateral arteries, and the ſecretory tubes of the glands †. For we know that the fulneſs or ſoftneſs of the pulſe does not depend, ſolely, upon the quantity of blood thrown out by the left ventricle of the heart, but alſo upon the more or leſs difficult paſſage of this fluid thro' the extreme arteries; ſince, in proportion as theſe are obſtructed or open, a greater or leſs reſiſtance will be oppoſed to the blood projected by the heart.

AFTER what has been ſaid of the ſlowneſs of the heart's motion in ſleep, it will be eaſy to ſhew why reſpiration ſhould be performed, then, at greater intervals.

THE

* BOERHAAVE Inſtitut. med. § 597.

† Exercit. de ſomno et vigilia. § xl.

The cauſe exciting the alternate contraction of the inſpiratory muſcles, is an uneaſy ſenſation in the lungs, occaſioned by the blood puſhed into their veſſels by the right ventricle of the heart *. If then leſs blood is ſent, in a given time, into the lungs, in ſleep, than when we are awake; the neceſſity of new ſupplies of freſh air will be leſſened, and conſequently inſpiration will be performed at greater intervals.

Further, as in time of ſleep, the ſenſibility of the lungs, like that of the heart and guts, muſt be ſomewhat impaired, reſpiration muſt alſo, on this account, be performed more ſlowly; for the inſpiratory muſcles will not be excited into action till a greater degree of irritation, than uſual, be occaſioned by the blood accumulated in the pulmonary veſſels. And, to this it is owing, that reſpiration is not only ſlower, but ſomewhat deeper in time of ſleep, than in a waking perſon at reſt in a horizontal poſition.

In comatous and apoplectic caſes, where all the feelings of the body are much more impaired than in ordinary ſleep, reſpiration is not only much ſlower and deeper than uſual,

but

* Eſſay on Vital and Involuntary motions, &c. p. 176. &c.

but ſometimes, after expiration is finiſhed, a pauſe of 15, 20, 30, or more ſeconds will intervene, before a new inſpiration is begun. Much the ſame thing happens to animals who have ſwallowed too great a quantity of *opium* *.

Now, if it be reaſonable to aſcribe the ſlow, deep and interrupted breathing, in ſuch caſes, to the inſenſibility which attends thoſe diſeaſes of the head; and which *opium* never fails to produce, when taken too liberally; are we not hence led to conclude, the leſs remarkable change of breathing which happens in ſleep, to be owing, partly, to the ſenſe of feeling in the lungs, being then ſomewhat diminiſhed, tho' in a much leſs degree than in thoſe morbid caſes?

To conclude with ſumming up what has been ſaid in a few words; in ordinary ſleep the ſenſibility of the heart and lungs ſuffer ſo ſmall a diminution, that their motions will be very little more affected by it, than they would be from the horizontal poſition and reſt of the body, and compoſure of mind attending it. In the deeper ſleep, which ſucceeds great fatigue, the motions of the heart

* Eſſay on the Vital and Involuntary motions, &c. p. 194.

heart and lungs will be more obſervably altered. And, in the moſt profound ſleep, occaſioned by *opium* or a morbid ſtate of the brain, where a general inſenſibility reigns over the whole body; the pulſe will become much more remarkably ſlow and full, and reſpiration ſlower and deeper.

ART.

ART. XX.

Remarks on the Intercoſtal Muſcles, by ALEXANDER MONRO, F. R. S. *Profeſſor of Anatomy in the Univerſity of* Edinburgh.

*TAUVRY**, ſhewed the miſapplication of Mr *Bayle*'s propoſed demonſtration of the action of the internal intercoſtal muſcles, from his inattention to the ribs not keeping in the ſame parallelіſm when they are raiſed, as when they are depreſſed, becauſe of the greater motion of the lower than of the higher ribs; and he took notice of the ſame demonſtration, proving equally that the anterior part of theſe internal intercoſtals muſt act in inſpiration, as *Bayle* would have their poſterior part to act in expiration: but neither he, nor any other writer of my acquaintance, has applied this demonſtration of *Bayle*'s, which, granting the parallelіſm of the ribs to remain the ſame, is a good one, to account for the defect of the internal

* *Anat. raiſonn. part* 2. *chap.* 14.

nal interco∫tals between the ∫pine and the angle of the ribs, and for the deficiency of the external interco∫tals from where the ribs begin to turn upwards to the *∫ternum*; a fact which anatomi∫ts are now all agreed in. The application of the demon∫tration to this purpo∫e, is the intention of this paper.

LET AB (*fig.* I. TAB. viii.) be the ∫pine, CD the *∫ternum*, EFGH, and IKLM two ribs; then EK and GM are lines in the direction of the external interco∫tal mu∫cles, and FI, and HL are in the direction of the internal interco∫tals.——Now, ∫ays *Bayle*, if both ribs ri∫e, the angle FEI mu∫t be enlarged, and its hypoteneu∫e FI, the fibres of the internal interco∫tal, mu∫t be longer: whereas mu∫cles generally are ∫hortened when they act.—Near to the ∫pine, where the ribs cannot be brought nearer at one time than another, this rea∫oning is ju∫t; and therefore the internal interco∫tals are not placed nearer to the ∫pine than where the motion of the ribs is large enough to allow the lower rib to approach ∫o much to the one above it, as to do more than compen∫ate for the elongation of the hypoteneu∫e of the angle FEI, *i. e.* the internal interco∫tal is only to be

found

found farther foreward than the angles of the ribs.

NEAR to the *ſternum*, the cartilages cannot be brought nearer; and therefore, when they are raiſed up, HL, the fibres of the interal intercoſtal may be ſhortened, becauſe the angle HML is made leſs: but, if there were fibres of the external intercoſtal here, as GM, they would be made longer, becauſe the angle GHM is enlarged by raiſing the ribs; and therefore no external intercoſtal muſcles are placed at this forepart of the *thorax*.

Art. XXI.

The Cure of a fractured Tendo Achillis; *by the* ſame.

OUR predeceſſors were either not ſubject to the breaking or tearing the *tendo Achillis*, or it was miſtaken for a ſprain, or ſome ſuch other diſeaſe by phyſicians and ſurgeons; for it is rarely taken notice of in books; and the oldeſt and beſt practiſers here aſſure me, they had not obſerved it or heard of it, till between twenty and thirty years ago; ſince which this accident has happened to a conſiderable number of people of this country, of whom I know ſixteen, and ſeveral of thoſe have broke the tendons of both legs; among the reſt, I have been a ſufferer this way; and, upon a compariſon with my brethren in affliction, my cure is the moſt compleat of any I know; there being neither pain, ſtiffneſs, weakneſs nor obſervable ſhriveling in my left leg where the *tendo Achillis* was broken: whereas moſt of the other gentlemen have ſome of thoſe uneaſineſſes,

eaſineſſes, and ſeveral have all of them. On this account, I think it my duty, for the benefit of future patients, to make my method of management as public as I can; which probably will be beſt done, by communicating it to your ſociety.

When my tendon was torn, it cracked as loud as if I had ſuddenly broke a nut under my heel; which the company believed had happened: and I had ſuch a ſenſation as made me imagine that the heel of my ſhoe had ſtruck a hole in the floor; which is the feeling that ſeveral have had as well as I: while others complain as if they had received a ſmart ſtroke with a ſtone or cane on the part.——I ſuſpected immediately what the caſe was; and therefore, after feeling where the hollow was, between the ends of the broken tendon, I took the left foot in my right-hand, with which I extended the foot ſtrongly; and, as ſoon as I was put in a ſeat, preſſed down the calf of my leg with my other hand: in which poſture I kept them till Meſſ. *John Douglas* and *James Ruſſel* ſurgeons of this place came; and, after being ſatisfied of the rupture, by preſſing their fingers into the hollow between the two ends

of

of the tendon, they applied compreſſes, and a bended board on the upper part of my foot and fore part of my leg, which they kept, as near as they could, in a ſtraight line, by a tight bandage made with a long roller.—This dreſſing became ſoon too uneaſy for me to bear, and the board was liable to ſhuffle, whatever care was taken in the application of the bandage: therefore I changed it for the following compound one, conſiſting of two pieces. (See TAB. viii.)

Fig. 2. is a foot-ſock or ſlipper A, of double quilted tiking; from the heel of which B, the quilted ſtrap C, is of ſuch a length as to come up above the calf.

Fig. 3. A ſtrong quilted calf-piece L, with pye holes MM on each ſide, through which a lace, *fig.* 4. is to be paſſed; and, with a buckle N, ſo placed on its back-part, that, when the lacing was on the outſide of the leg, the buckle was in the middle of the lower part. Two rows of pyeholes are here repreſented on one ſide, either of which is to be uſed according to the thickneſs of the leg.

HAVING

HAVING then wrapped ſoft flannel, ſmoak-ed with the fumes of benzoin, round my foot and leg; I put on, as in *fig.* 5. the foot-ſock A, and calf-piece L, and bringing the ſtrap C, thro' the buckle N, I could by it extend the foot, and pull down the calf to what degree I thought fit, and there ſecure it with the buckle.

THIS bandage anſwering my intention quite well, I wore it night and day, drawing the ſtrap tighter when I inclined to ſleep, and relaxing it when I was fully awake and on my guard; at which time, likeways, I placed my foot on a ſtool, in the poſture repreſented at S, and frequently ſhifted the calf-piece or made the lacing of it looſer, to prevent the ſwelling of my foot, which it was in danger of occaſioning, when it became too tight, by being drawn down by the ſtrap.—After a day or two, I found my toes uneaſy, from the foot-ſock preſſing them; and therefore I undid the ſowing of it at the toes, from *d*, to *d*; and have cauſed it always ſince to be made open there, for others who uſed it.

DURING two weeks, I made no motion nor effort with my foot; but was carried on a chair, running on caſtors, from one part of of my houſe to another. After this, I began to move the foot backwards and forewards ſo gently as not to give pain, and gradually increaſed thoſe motions; but always ſtopped the flexion of the foot, or extenſion of the leg whenever I felt the leaſt uneaſineſs: frequently I continued theſe motions for half an hour together, with the diſeaſed limb; but kept the other as much at reſt as I could.

WHEN I began to walk, I always put the left leg ſome way before the right, that the left foot might be well extended; making uſe of a cane in my right-hand, to prevent any danger of falling.

THE void between the two ends of the divided tendon, became inſenſible in few days, except that a ſoftneſs was felt there, more than any where elſe; but this part turned gradually thicker and harder, till a knot was formed in it, which one, in feeling thro' the teguments, would have thought to be a piece of cartilage, as large as a middle ſized plumb, which has greatly decreaſed, and become ſofter, for ſome years paſt; ſo that, at preſent, it is much leſs perceptible than formerly.

SOME

Some weeks after the accident, I began to pour cold water on my leg and foot, cauſing them to be well rubbed immediately after; but the water, inſtead of ſtrengthening the member, as I expected it would have done, made it cold and weak; for which reaſon I ſoon forebore the uſe of it, and cauſed the leg to be rubbed twice a-day ſtrongly, with *unguentum altheae*, or ſome ſuch greeſy ſtuff, to protect the ſkin from excoriation by the friction. This *manoeuvre* was continued till I began to employ the limb freely.

Being obliged to go abroad after ſix weeks, I put on a pair of ſhoes with heels about two inches high, and applied the machine, which I am juſt now to deſcribe, in the day time, inſtead of the former bandage; which, however, was always put on at night, for a month more.

The new machine, *fig.* 6. was a piece of ſteel, the middle ſtalk of which A, is narrow but ſtrong: the ends BB, are thin, broad and concave, adapted to the convexity of the foot and forepart of the leg. Three ſtapples CCC, ſtand up from the forepart of the ſteel, one being in the middle of each of the broad ends, and the third in the middle of the ſtalk,

ſtalk. All the ſteel except the ſtapples was covered with chammoy leather, and the concavities of BB, were well buffed, as the ſofter rupture bands commonly are.

After I had put on my ſhoes and ſtockings, one end of this machine was put on the broad of my foot, nearer the toes than the buckle of the ſhoe; and the other end was placed on the forepart of the leg: then one ribband, or a thong of leather, *fig*. 7. 8. was put round the foot, and another round the leg, to paſs thro' the two ſtapples near the ends of the machine, and there ſecured with ſtraps or buckles, but without being drawn tight,—A third ſtrap or ribband, *fig*. 9. having its middle D, applied in the hollow of the foot, immediately before the heel had its ends paſſed on each ſide of the foot, thro' a *ſinus* or nooſe EE, of a fourth thong of leather F, that came round the quarter-heel of the ſhoe, to be afterwards put thro' the middle ſtapple; where, after theſe ends GG were drawn as tight as was thought convenient for extending the foot, they were ſecured with the buckle, or with knots. See the application in *fig*. 10.—I wore this machine always in the day-time, for five months: tho', horſe at the right ſide inſtead of the left one,

TAB. VIII.

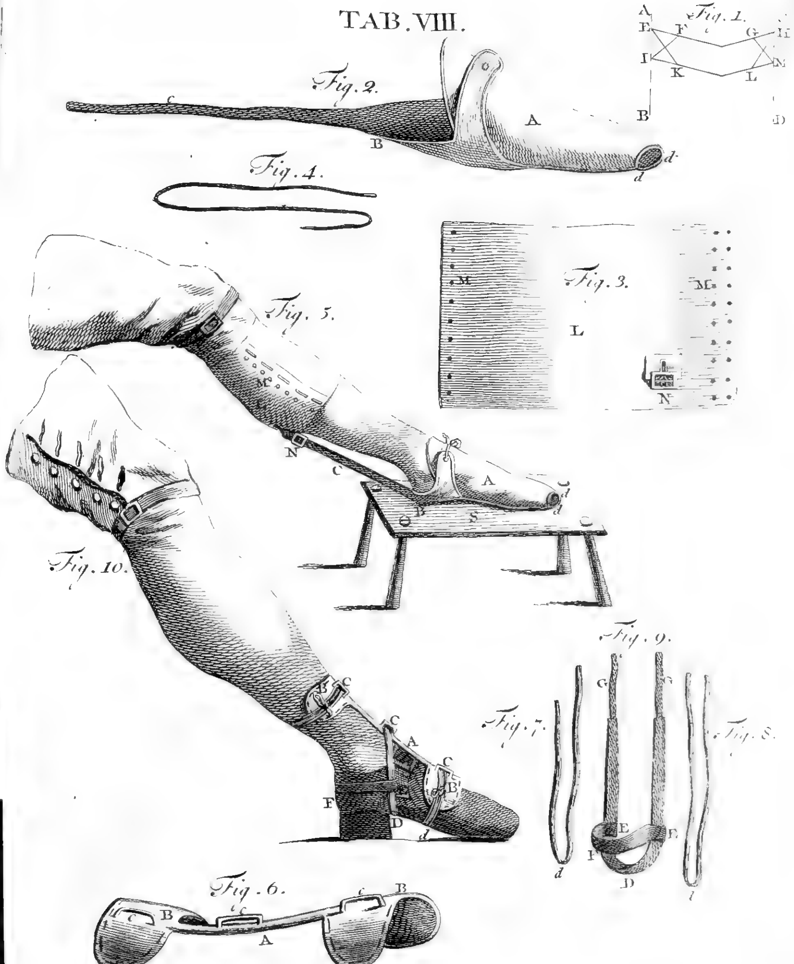


Fig. 1.
A E F G H I K L M B D
Fig. 2.
c B A d d
Fig. 4.
Fig. 3.
M L M N
Fig. 5.
M N C A B S d d
Fig. 10.
B C C A C B F D d
Fig. 9.
G G E E F D
Fig. 7.
d
Fig. 8.
l
Fig. 6.
c B c c B A

as it is inconvenient, by ſhuffling out of its place ſometimes; I think a thong of leather ſowed at one end to the upper middle part of the quarter-heel of the ſhoe, and faſtened at the other end to a garter, put above the calf of the leg, would ſerve inſtead of it, without inconvenience.——All that time I never walked the ſteets, but was carried in a chair.—— In going down ſtairs, I always put the diſeaſed leg firſt down at each ſtep; and, in coming up, I put the ſound leg foremoſt, by which I ſhunned the ſtretching and retearing of the new ſouldered ſinew, which, I knew from what others had ſuffered, might have made the caſe worſe than it was at firſt.—The habit I had got of walking after this manner in ſtairs, became ſo much a ſecond nature, that I had afterwards difficulty to learn to go up and down in the ordinary way, with the feet alternately.

I continued the high-heeled ſhoes for two years, cauſing my boots to be made after the ſame faſhion, when I began to ride; but have gradually ſince retrenched their height, till now I wear them of the common make.

During all that time too, I mounted my horſe at the right ſide inſtead of the left one, that I might ſhun the rearing my whole weight, by the force of the weakened *gaſtrocnemei* muſcles; and to prevent the accident of ſtraining them, in caſe the horſe ſtumbled, I kept the left foot deep in the ſtirrup. In walking up hill, I put that foot a-croſs; and, in ſhort, guarded againſt every other ſtretching ſtep or motion.

ON comparing the ſize of the calfs of the two legs at preſent, the left one is a little ſmaller than the right one, eſpecially in the morning; but ſo little, that it is ſcarce perceptible: thro' the day, the difference becomes leſs.—The *tendo Achillis* that was broken, is, as moſt other tendons cured after breaking, conſiderably thicker and harder than the other; but one muſt be told it is ſo, before he can diſcover it by the eye.

ART.

ART. XXII.

An Account of the Diſeaſe called Mill-Reek *by the Miners at* Leadhills, *in a Letter from Mr* JAMES WILSON, *Surgeon at* Durriſdeer, *to* ALEXANDER MONRO, *P. A.*

IN obedience to your deſire, I ſend what I have obſerved concerning the diſeaſe which the people at *Leadhills* call the *mill-reek*, and which all the inhabitants there are ſubject to; but it moſtly ſeizes, and violently affects the men whoſe daily buſineſs it is to melt down the lead. The melting-houſes, where this is done are called *mills*; becauſe the bellows there are worked by water-mills.

In the ſlighter ſtage of this diſeaſe, an uneaſineſs and weight is found about the ſtomach, eſpecially near the *cartilago enſiformis*; and ſometimes it appears like a cholic in the inteſtines. The ſpittle of the ſick is ſweet, and ſomething of a blueiſh colour, reſembling what one obſerves when he chews lead. —The pulſe is a little low; the ſkin is all over

cold

cold; and a clammy ſweat frequently breaks out.—The legs become feeble with a prickling numneſs; and there is a debility and lazineſs in all the body.—The appetite goes away, and they don't digeſt what food they take.—Sometimes a *diarrhœa* makes a cure; but, if it continues too long, it is very hurtful.—In this *ſtadium* the ſick are yet able to go about and to work.

But, if theſe ſymptoms continue long, and ſpirituous liquors are drunk with an empty ſtomach, or after working lead; the diſeaſe comes to its ſecond ſtage: in which, to the former complaints, are added a fixed pain in the ſtomach and guts, eſpecially in the lower part of the abdomen, extending itſelf from the one *os ilium* to the other.—The patients become very coſtive, with the ſenſe of ſomewhat gnawing their inteſtines; and the pulſe turns quick with heat on the ſkin.—Giddineſs, with vehement pain, ſeizes the head; which is ſucceeded by an inſenſibility and *delirium*, like madneſs of the worſt kind; in ſo much that they tear their own fleſh, and bite their hands; the extremities tremble, and are convulſed: at laſt they fall

low

low, the pulſe intermits at every 3d or 4th ſtroke, and they die in a *coma* or apoplexy.

THE reek or ſmoak riſing from the melting lead, is believed to be the cauſe of this diſeaſe; becauſe the melters who are moſt expoſed to the ſmoak which comes out often full in their faces, are moſt ſubject to this diſeaſe, the *mill-reek*.—The people here ſay they have ſeen birds, in a calm moiſt day, attempting to fly thro' the ſmoak of ſuch a chimney, fall down dead.—Cattle, which paſture near to mills, are often killed; and therefore ſhepherds take great care to keep their ſheep at a diſtance; which, if not by the ſmoak, muſt be hurt by the graſs, which I often ſee made blue by the ſmoak falling on it. And other animals ſuffer from the water impregnated with the fumes, or with the lead waſhed in it.

THE ſymptoms in them are very like to what men ſuffer. Dogs, in the laſt ſtage, lie either dull and ſtupid, or bite and ſnatch at every creature that comes near them: nay, they will gnaw and tear up the ground on which they lie, after they cannot raiſe themſelves on their legs.

THE

The people at *Leadhills* are ſo averſe to opening of dead bodies, that I could never prevail upon them to allow me to open any who died of this diſeaſe. But, in a dog who had it, I found *ſludge** lying on the inner coat of the ſtomach and inteſtines; and, in ſeveral parts, it was turned to a cruſt.—The guts were much inflammed in ſome places, and in others a mortification was begun with holes thro' them.—His *fæces* were very hard; and, where they were of little quantity, the coats of the guts were thick, and the paſſage thro' them leſs.

If proper medicines are given in the firſt ſtage of this diſeaſe, the patient generally recovers.—If it goes on till giddineſs begins, the ſucceſs is doubtful; and when the cure is delayed to be attempted a little longer, the diſeaſe almoſt conſtantly proves mortal.

If the work-people at *Leadhills* would uſe the following precautions, they might ſave themſelves from this diſeaſe, at leaſt would have it very mild.

I. No man ought to go to work faſting; and he ought to take oily or fat food: the *Engliſh* mill-

* The fine particles of lead which ſubſide ſlowly in water, in which lead has been waſhed.

mill-men on this account hold much better out than our countrymen. A glaſs of ſweet oil pure, or mixed with a little *aqua vitae*, would be a good morning draught.

II. PHYSIC ſhould be taken Spring and Harveſt, and whenever any effects of the reek are felt.

III. ARDENT ſpirits ought to be drunk very ſparingly; and ought never to be taken in time of work at the mill, or immediately after it. They increaſe and fix the bad effects of the leady ſmoak.

IV. No mill-man, when heated by work, ought to go into cold air; but to put on his cloaths immediately, and return to his lodging, to change his working cloaths for others, and cool gradually: by which he would prevent catching cold. In this article they are very careleſs.

V. IMMEDIATELY after coming from work, the aliment ſhould be moſtly liquid, as broths.

VI. LOW and poor diet makes them more liable to be affected, and leſs able to undergo a cure: theſe workmen ought to feed on good meat.

VII.

VII. When their buſineſs can allow, they ſhould go out of the reach of the reek, to breathe an untainted air, and to take victuals free from lead. But I muſt caution the labourers at *Leadhills* not to take long journeys: they are more hurt by travelling one day, than by working two.

The cure of this diſeaſe depends principally on cleanſing the *primae viae*: and therefore, after blooding ſuch patients, if they are plethoric, I give them a vomit of *emetic wine* or *tartar*; which muſt be a doſe double to what would vomit another perſon; otherways it would have no effect, eſpecially when the diſeaſe is violent or has continued long. My father, who has had long practice with ſuch patients under the mill-reek, has always finely powdered *vitrum antimonii* ready; of which he gives half a drachm for a doſe; and during its operation, warm water is to be drunk plentifully. If the vomit operates well, and purges briſkly too, the patient is in a fair way of recovering; which a ſecond, but a milder doſe of *ipecacuanna* with ſome *tartar emetic* mixed, often makes compleat: but, if the emetic neither vomits nor purges, the patient is generally worſe for it; and a ſtronger doſe ſhould

ſhould be given ſoon.—If it vomits but does not purge, a cathartic of the antimonial kind, or of jallap and mercury, in greater than ordinary quantities, ought to be given: and during the time of purging by the emetic or cathartic medicine, the patient ought to drink warm broth plentifully.—The vomits and purgatives ought to be repeated at proper intervals, till the uneaſineſs in the ſtomach and guts, from the diſeaſe, is gone.—If theſe medicines over-do, an opiate may be given at night; but this is to be adminiſtred ſparingly, leaſt it bring or increaſe coſtiveneſs, which is the worſt thing can befal the patient. At the ſame time, emollient, anodyne, and laxative clyſters, are frequently to be injected for emptying the guts, if the purgatives do not their duty.

WHEN blood or matter are paſſed with the *faeces*, the emetics and purgatives are to be abſtained from, till, by healing, balſamic, but laxative clyſters, and mild food, this appearance ceaſes.

WHEN the belly is much ſwelled, emollient fomentations ſhould be often applied to it; but, if the madneſs is begun, little elſe can be done, than to endeavour to keep the patient quiet during the little time he has to live.

SOMETIMES it happens, that, after the complaints of ſtomach and guts are gone, a prickling pain and feebleneſs remain in the legs, much like to a rheumatiſm; for which, friction, with a coarſe cloth or fleſh-bruſh, is neceſſary. If that fails, *ung. nervinum* with *ol. terebinth.* mixed, is to be rubbed on every night, before the fire; after which, the member is to be wrapped in flannel.—If theſe pains are violent, or the feebleneſs reſembles palſy; bliſters ought to be applied to the ſkin under which the large nerves are ſituated, and the medicines proper in a palſy are to be given.

SOME are ſo waſted before the cure is compleated, that they remain afterwards emaciated, weak, and as if they were hectic, with a giddineſs in their head: and ſometimes they chat to no purpoſe, or ſeem hypochondriac. In this condition, the patient ſhould go to the country, to ride a-horſe-back ſome miles every day; and, at the ſame time, ſhould take bitters with bark and ſteel. If the giddineſs continues, I have given with ſucceſs, *pilul. de myrrh.* with a ſmall proportion of *camphor.*

End of the firſt Volume.

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