E S S A Y S

A N D

O B S E R V A T I O N S,

PHYSICAL AND LITERARY.

Academies &c. — Edinburgh Royal Society

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SECOND EDITION.

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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, beſides ſuch as more immediately relate to the branches of medicine. All the ſciences are remarked to have a 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 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

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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.

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, though, without this method of conveyance, they would be intirely loſt to the

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 phænomena, remarked by different perſons, there often reſult general truths, of which, from one of theſe phænomena, no man of the greateſt ſagacity could entertain any ſuſpicion. Though 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, through 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 through 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 they are founded are extremely obvious; and

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 it is 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 read 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 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

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leſs thoſe which have barely been read 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 their ſecretary Dr ALEXANDER MONRO Profeſſor of Phyſic and of Anatomy at Edinburgh.

Edinburgh, 1771.

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ESSAYS

ESSAYS

AND

OBSERVATIONS

PHYSICAL AND LITERARY.

ARTICLE I.

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

NOTHING has retarded the progress of philosophy more than an unlucky propensity that 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 phaenomenon must be accommodated to that principle, and eve-

ry oppoſite fact, however obſtinate, goes for nothing. And thus we endeavour to mould nature to our wiſh, inſtead of deſiring to know nature in her genuine appearance.

We ſee, then, that in ſcience, not leſs than in action, appetite and inclination prevail. 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; having no foundation but fancy and chimera. After ſo much wandering, philoſophers became more cautious; they began with doubting of every thing, and then with ſearching 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

expedient to keep the end in view. Facts and experiments 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

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 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 motae 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 the definitions that ever were attempted, Ariſtotle's definition of motion is the moſt unintelligible, "*Ac-* "*tus 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

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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, nor to their readers, upon this ſubject; for they have attempted what is utterly impoſſible, *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 inſtances.

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

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

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, though 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* is equally applicable to animate and inanimate beings, ſuppoſing them to be equally ſelf-movers.

WHETHER

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 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. Here

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is an effect 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 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 we are indebted for the knowledge of theſe facts to 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

ginning 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 superadded 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 driven against an obstacle, rebounds with 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 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 in matter various powers 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

ther incapable of active powers; that activity is confined to immaterial ſubſtances, and that inertneſs is implied in the very conception of matter. This makes them 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 philoſopy is not much affected by it, of whatever errors it may be productive in the more abſtract ſciences. For it is admitted,

mitted, that the Deity, in acting upon matter, ſubjects himſelf to the ſame 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 has no ſupport from reaſon more than from experience.

To put an end to a good deal of looſe reaſoning upon this ſubject, it will be neceſſary to aſcertain the meaning of the terms *material* and *immaterial;* which are uſed by writers without any accurate meaning. All beings and exiſtences muſt be either material or immaterial; or, in other words, muſt be matter or not matter: Therefore, if we know what is mat-

ter,

ter, 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 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 immateriality 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 the properties of ſuch ſubſtances are not objects of any external ſenſe.

FROM

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 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

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 after all, it muſt appear extremely whimſical 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

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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 whatſo-
"ever is done in the world, at leaſt where
"the rational ſoul intervenes not, is really
"effected by corporeal cauſes and agents,
"according to the laws ſettled by the om-
"niſcient author of things." * And he obſerves, "That as it more recommends
"the ſkill of an engineer to contrive an
"elaborate engine, ſo as that there need
"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
"employed to concur notably to the ope-
"rations of this or that part, or to hinder
"the engine 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

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

"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 em-"ployed, 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 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

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

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 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

to 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, though 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 preſer-

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ving 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 ea-

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ſy 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 nor 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

power continually exerted to preſerve a body in motion. Again, if motion be conſidered 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

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 though it ſhould be 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 plain-

ly 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 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 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 itſelf in motion. And this is a law which regards all matter without exception.

AND

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 diſtinguiſhed 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 reſiſtance. 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*.

THOUGH this *vis inertiae* 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 are, the reſiſtance will be the leſs: And they may be made ſmoother and ſmoother, till the reſiſtance be quite 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 inertiae*, when it can be accounted for by other cauſes; and that it is unphiloſo-

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phical 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 conceivable 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 no force ſo ſmall which it would not yield to. Theſe, and ſuch like experiments, look as if matter had no attachment to reſt, no *vis reſiſtentiae*, 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 propoſi-

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tions which may appear not to be very consistent: Yet both are true in fact. And the difficulty will vanish, by attending to the *vis inertiae* or *resistentiae*, that it is not measured merely by the size or bulk of the body in which it is inherent, but has a relation to the *momentum* of the impinging 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 resistance of a body at rest, shall be the subject of the next chapter.

OF

OF THE COMMUNICATION OF MOTION.

THE communication of motion from one body to another, has been extremely puzzling 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 impinging 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

ties of matter; and it is no leſs repugnant to the very conception of the thing, that the motion of one 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 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, though 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 phænomenon 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

produced is eaſily deducible from the principles above laid down. The *vis inſita* and the *vis reſiſtentiae* are ſufficient to account for all the effects that proceed from the colliſion of hard bodies which have no elaſticity. If the reſiſtance of a body be always leſs than the impreſſed force, the reſiſting body, after its reſiſtance is overcome, muſt neceſſarily be carried along with the body that impinges upon it. And, being once put in motion, its own *vis inſita* preſerves it in the ſame degree of motion. I here put the ſimpleſt caſe, that of a body in motion, impinging upon one at reſt; and, for the eaſe of the imagination, I ſhall ſuppoſe theſe two bodies equal. With reſpect to this caſe it is aſcertained by experiments, that the two bodies in contact go on with half the celerity which the impinging 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 impinging body

body is deſtroyed by the reſiſtance of the body at reſt, the other half remains, with which the impinging 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, by its impenetrability muſt be carried along with the other.

BUT, in order to form an accurate notion of percuſſion, two preliminary points muſt be ſettled. The firſt is, to ſettle 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 impinging body, and carry it backwards at leaſt for ſome ſmall ſpace; which cannot be; becauſe, once admitting a retrograde

grade 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 impinging body, must be merely by resistance. The other point to be settled is, whether the operation be instantaneous, or whether it be performed 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 impinging 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 analysed. Tho' the whole is performed in an instant, it may, however, be distinguish-

ed into different ſteps. The firſt is, that, by a certain degree of reſiſtance, a proportion of the *momentum* or force of the impinging 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 impinging body. And the laſt ſtep is, that it is carried on with the ſame degree of velocity by its own *vis inſita*.

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 impinging 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

the momentum of the body which is put in motion, is equal to the momentum loſt by the impinging body. Therefore the momentum of the impinging 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 expect ſome wiſe contrivance. Writers have given very little attention to the laws of motion, conſidered in the light of final cauſes, tho' a moſt beautiful ſpeculation. It would carry me too far from my preſent purpoſe, to enter directly upon it: I take liberty only to make a ſingle obſervation with reſpect to the 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.

own. The reſiſtance of bodies again is of admirable uſe to keep force within proper bounds. If the ſmalleſt body, impinging 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 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 puzzled 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?

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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 ſeem to make a figure. The firſt is, that, according to this law, there cannot be in matter ſuch a thing as reſiſtance without reaction; a bold aſſertion, that contradicts what in common apprehenſion appears to be the fact; and therefore ought not to have been taken for granted without offering any evidence. 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 in hard bodies, for example, as well

well as in elastic; the effects of collision must 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 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.

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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 entering upon 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

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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 pressure 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 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 prin-

ciple by which to account for this phænomenon. 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

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; and reſts it wholly upon this fact, that equal changes of motion are produced upon colliſion; which is, in other words, that, whatever force is loſt by the one body, an equal force is generated in the other. And indeed it appears probable, that the law has been invented

chiefly

chiefly to account for this effect: If ſo, it is extremely raſh in philoſophers to apply it, as they do, in every caſe where there is the leaſt appearance of action and reaction, as if it were an univerſal law of nature that muſt obtain in every caſe whatever. It was not diſcovered, it would ſeem, that the equality of motion before and after percuſſion, is the genuine effect of the *vis reſiſtentiæ*. Therefore, to account for this phænomenon, a new law muſt be invented; which, to diſguiſe the matter, muſt be conſidered as a general law: For, had it been plainly ſpoke out, what in effect is ſaid by Dr Keill, that this law is only applicable to the percuſſion of bodies; every perſon would be ſenſible, that accounting for the law of percuſſion in this manner, was doing no more than repeating the fact itſelf in different words. For, to ſay that the actions of two bodies in percuſſion are equal and in oppoſite directions, 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

In accounting for the preſſure of fluids *undequaque*, Muſchenbroeck 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 con-

ſequence

ſequence in fluids, but in 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, though 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

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 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, chuses 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 gravity must be the effect of some cause, which penetrates into the very center of the sun and planets, and which acts not in proportion to the surfaces, but the solid quantity of matter; its action only decreasing in a duplicate *ratio* of the

"distances.

"diſtances." And adds, "That he has "not been able to find out from phæno-"mena, the reaſon of theſe properties of "gravity, and that he does not chuſe 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 wonderful, when it was his own child. New diſcoveries are always received with ſome degree of heſitation: Becauſe it is the effect of novelty to produce doubts as well as ſurprize. But now, that this theory is fully eſtabliſhed by habit, as well as by reaſoning, and has got a firm hold of the mind; it is not obvious, why later philoſophers ſhould affect the ſame reſerve. For my part, I cannot ſee any difficulty of explaining the cauſe of attraction or gravity, more than of explaining the cauſe of a body's continuing in the ſame degree of motion with which it begins to move. And this I ſhall now attempt.

It is above eſtabliſhed, that the continuation of motion in bodies muſt be an effect of a power inherent in all matter,

of preſerving that degree of celerity which is once beſtowed upon it. The body, by this power, is directed to move in a ſtreight line. But we perceive nothing in the nature of this power to confine its operation to a ſtreight line, more than a curve. And we may ſuppoſe the power ſtill further varied, to make the body, inſtead of moving in a ſtreight line, or in a curve, to direct its equable motion towards any other given body, within a certain diſtance. 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. We have no ſuch conception of any power as to regulate its operations by one law more than by another. And this leads to the cauſe of gravity or attraction. We eaſily conceive the *vis inſita*, or a power in matter to continue its motion in a ſtreight line: It is equally eaſy to conceive a power in matter by which every particle has a tendency to be united with every other particle. Vary the *vis inſita* in the two following

lowing particulars, and we ſhall 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 its body not in a ſtreight line, but towards every body great and ſmall within its ſphere of activity.

THE grand difficulty that 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; whence 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.

If bodies act upon each other by the force of gravity, it is easy to conceive that the exertion of gravity should be fainter and fainter in proportion to the distance; but it is not so easily conceived that distance should have any influence, if the gravity of each body be exerted within itself, and not upon another body at a distance. 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 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. 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 ellipſe: 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 well founded; but, in my apprehenſion, he has overlooked the moſt ſolid and weighty objection. Whatever cauſe can be aſſigned for the motion of this fluid round the ſun, will equally account for 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 pow-

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er 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 the ſame inconſiſtency of a body acting where it is not, has moved our Britiſh philoſopher to invent an ætherial 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

"the empty celeſtial ſpaces between them. "And, in paſſing from them to great di- "ſtances, that it grows denſer and den- "ſer 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 me- "dium towards the rarer." I am not better ſatisfied with this hypotheſis than that of Des Cartes. For, without looſing time upon a minute inquiry into the very peculiar properties with which this ſuppoſed aether muſt be endued, in order 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

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28th query, at the end of his optics, may be urged with equal ſucceſs againſt this ſuppoſed aether, "It is of no uſe; "and, as there is no evidence for its ex-"iſtence, it ought to be rejected."

I muſt own, at the ſame time, great reluctance to 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 connection, 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: And the conſequence is, that the bit of matter involved in 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

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 increaſing the quantity of matter indefinitely, the ſum of the tendencies of any one 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 even for a moment; the curvilinear motion of the heavenly bodies, as well as the deſcent of bodies towards the centre of the earth, are obviouſly the effects of that power. With regard to any particular planet, the earth for example, we have no more to ſuppoſe, but that it is 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 planets, deſcribes

deſcribes an elipſis, in one of the foci of which the ſun is placed.

In the deſcent of heavy bodies towards the centre of the earth, the force of gravity is ſuppoſed to be invariable. For, though this force leſſens by diſtance, yet the diſtance of any two points, from which we have acceſs to drop a body, is ſo inconſiderable in reſpect of the diſtance of either from the centre, that it is left out in all calculations as imperceptible. Hence, the following queſtion is readily ſuggeſted: How comes it, that the power of gravity, which in this caſe is ſuppoſed invariable, has the effect of accelerating the motion of a falling body? It ſhould be thought, that the power of gravity, ſuppoſing it invariable, muſt produce an equable motion downwards, without acceleration; juſt as the *vis inſita* does, laying aſide gravity, in whatever directon the body is thrown. For, in general, as effects muſt always correſpond with their cauſes, every force which is uniformly exerted without diminution or augmentation, muſt produce an equable motion,

motion, without acceleration or retardation: And, on the other ſide, every varied effect 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 accelerations 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,

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

tion, 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 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,

body, the force of gravity varies every inſtant, and increaſes 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, though it continues invariable after the body is ſet in motion, 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

makes it evident that the power of gravity is continually increaſing from the beginning to the end of the motion.

In accounting for this phænomenon, I have neglected the *vis inſita*, though it may probably act in conjunction with gravity in the deſcent of bodies towards the centre 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 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 acquires 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

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,

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 downwards, gravity being exerted without an antagonist, every new impulse produces an acceleration of motion; and, these impulses being repeated incessantly, the acceleration goes on, till the stone touch the groud.

It remains only to be observed, that, as the force of gravity is accurately pro-

portioned to the velocity, gravity muſt produce the ſame effect, whether its force be exerted in the direction of the motion, or contrary to it; and that the acceleration in the former caſe muſt 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 ſame ſeries of numbers repreſents both; a progreſs from the ſmalleſt to the greateſt number being ſimilar to acceleration; the contrary progreſs, to retardation. And hence it is that theſe two contrary motions are performed preciſely in the ſame time.

It is now eaſy to account for the different velocities of a planet in the different points of its orbit. The motion of a planet may 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

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its upward motion is ſpent. And, indeed, by prolonging the greater axis indefinitely, an ellipſe 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 THE FORCE OF BODIES IN MOTION.

IN explaining the firſt law of motion, *viz.* 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 that 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 reſiſtentiæ* when the body is at reſt. There is no difficulty to conceive a body endued with ſuch a property,

perty, as to preſerve itſelf in its original degree of motion where there is no obſtruction, and yet to yield to the ſmalleſt reſiſtance or oppoſing force. But the *vis inſita* is plainly not of that 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 motion." 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 im-

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pulſive 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 ſolicitous 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

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 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

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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 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.

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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 velocities. 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 out-ſetting, 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, though 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 with, has but half the time in any given ſpace to produce its effect;

fect; 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,

dium, becauſe it is the effect of that reſiſtance. All the world knows, that, when bodies move through a fluid, or any 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

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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 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 that is the foundation of all theſe powers. But ſtill, it is gaining

ground

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, 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 journey's end.

ART.

ART. 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

inſtances of the ſame kind, are always preſent to our view.

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 in-
"active, the ſmalleſt force would be ſuffi-
"cient to move a great body and a little
"body, with equal velocity: And that
"the

"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 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.

* Eſſay I. p. 9. 21. 24.

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 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 the 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

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 muſt be different in different caſes. Thus the idea of reſiſtance, as it is 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*.

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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'Laurin'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 aught we know, matter may "be of kinds ſo different from each other, "that the ſolid elementary 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 a greater or leſs than a given quantity of matter. Two bodies, or two elementary particles of equal quantities

tities 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 themſelves ſome notion of force antecedent to

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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 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

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

act upon the water in one direction, and the water to react upon the oars in the oppofite direction, and to produce the motion of the boat. Yet it is plain, that the motion is intirely produced by the rower, who, in fo far as he contributes to the motion of the boat, employs the oar as a lever of the fecond 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 progreffion of animals, whether it be of walking, flying, or fwimming, Borelli and all other authors content themfelves 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 oppofite direction. This is at beft but too concife, not to call it a falfe theory: Seeing it muft be manifeft that the medium, by any reaction which takes place here, can only confume as much force as is impreffed upon it, and is utterly incapable of producing any kind of motion.

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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 behoves us to follow it out, and inquire 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 prepoſſeſſion, and are ready to imagine, that reſt is the proper ſtate of body. But a diligent re-

view of all the circumſtances ſoon diſcovers, that body is equally indifferent 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

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 be deſired to reflect on what a per-"ſon feels within himſelf in walking, du-"ring which a repeated activity is exert-"ed to continue the motion *." But this, ſurely, can only be intended as an illuſtration of what is meant by the ſuppoſed 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

* Eſſay I, p. 22.

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 itself, 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 o-"thers, nor can we compare it to any "thing that is not motion*." But that is no reason why it should change of itself,

* Essay I. p. 22.

ſelf, 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 motion: Why ought that motion to ceaſe without a cauſe? Action is neceſſary to the production of motion, and no wonder if nothing can deſtroy the effect of one action but another action. The trite maxim, *ſublata cauſa, tollitur effectus,* is not to be ſo literally interpreted, as that an effect may not continue, after its cauſe ceaſes to act.

As we know ſo little of the nature of that influence by which one being puts another in motion, or by which it excites motion in itſelf; with what reaſon can we poſitively conclude its effects to be only

ly momentary, when they everywhere appear to be ſo permanent? When motion is begun in any body, we attribute it to a certain action which we cannot poſſibly explain; when a body continues in the motion which it has once received, it appears to be a neceſſary conſequence of that action. We commonly indeed ſay, that the body then moves on of itſelf; but the only queſtion in debate is, Whether there be occaſion for a continual recruit of force, new impreſſions, new ſollicitations to motion, like to what were at firſt exerted by the moving power?

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?

ſelf? 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 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 proper-

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ly 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 therefore 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 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

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 the activity before deſcribed. This is conceived to be

* 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 and activity. Clark's Lett. to Leibnitz p. 327.

an action in a ſecondary ſenſe. It is only a conſequence of the firſt action. A motion once produced continues, till it be deſtroyed by an equal and oppoſite 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 periſh of itſelf. This quantity of motion is always proportional to the real ſpace deſcribed by the whole quantity of matter; and that ſpace having length, breadth, and thickneſs, is meaſured by multiplying the quantity of matter by the length of ſpace deſcribed by every particle, that is, by the velocity. If a body in motion ſtrikes directly upon another at reſt, the two move on as one body after the ſtroke. They cannot move together with the ſame velocity that the ſingle body had before the ſtroke, for then there would be an increaſe of quantity of motion. And whence ſhould that proceed? Such an appearance as that would diſcover an activity in bodies indeed.

deed. Upon the ſuppoſition 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 inertiæ*. 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

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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 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

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

preſſions 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, 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 bo-

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dy 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 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

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 velocities 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

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, though 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 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 arrives 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,

"That, by a profusion of writing, the "point is rendered so perplexed and "intricate, that there appears not a bet-"ter way to come at the truth, than, ne-"glecting the arguments on both sides, to "apply directly to facts for a solution, "as one would do upon a point newly "started." With this 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, though 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,

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. It is ſaid, "That, 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 with, has but "half the time, in any given ſpace, to "produce its effect." 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.

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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 in 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

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 mere *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

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

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 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

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 go 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 along a table; whereas, if a ſudden tug be given, or a greater force applied, the thread breaks, or the load-ſtone 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

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 indeed only by experience that we learn the laws of the communication of motion.

For whoever 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 from foretelling 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

* Philoſ. Eſſays.

trary direction. This, for the ſake of 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 aught 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

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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, though 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 forewards.

wards. 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 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

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. 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, though it were ever ſo agreeable to the natural appearances of things. But ſhall we maintain, that every particular body can only

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be moved by a power of its own; and ſhall we deny, that motion is communicated by one body to another, merely 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 repreſented, the public judgment need not be declined. Whatever obſcurity the learned may find in this ſubject of the communication of motion, a vulgar obſerver will be very little perplexed about the matter. When a ſtone is thrown out of the hand, and continues to move when left to itſelf; there is no fear, but "every perſon, who has not ſtu-"died philoſophy," will rather attribute this to a force ſome how impreſſed upon the ſtone, than to any action or operation

of

of the lifeleſs ſtone. It will never enter into his head, that the ſtone flies away with wings of its own. Such a perſon, judging from firſt appearances, that all motion languiſhes and decays, may, likely enough, be inclined to think that body rather affects a ſtate of reſt than a ſtate of motion; and that the impreſſed force would not remain for ever: But he will never entertain the ſmalleſt doubt, but, that as long as the motion continues, it is only an effect of the firſt impulſe. And, having once granted that motion can continue one moment as an effect after the action of the moving power is over, the ſame reaſon muſt remain for its continuance the next moment of time, and ſo on for ever. 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 ſtop 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

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 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, inquire 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 far the concurrence of the Deity is necessary to the support of created beings in general, is perhaps impossible for us

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, though

though not essentially 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 those 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 "therefore 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

that theſe bodies move themſelves? The contrary of this appears in ſo many inſtances, as gives good reaſon to believe it never 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, though 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:

fect: Yet there are a multitude of facts which indicate a mechanifm by means of fome effluvia; particularly the new method of making artificial magnets by attrition, and the well known experiment, in which an iron rod is prefented to a magnetic needle, the fame end being made to attract, fometimes one pole and fometimes another, by only changing the pofition of the rod, or inverting it upfide down; and much more by ftriking upon it with a hammer, or beating it againft the ground. Electricity is another more palpable inftance of the fame fort. That a fluid is concerned in producing the apparent attractions and repulfions, and other more aftonifhing effects of the electric globe, can hardly be doubted, however difficult it may ftill be to defcribe the laws to which it is fubjected in its operations. Why then fhould it be accounted "whimfical" or unphilofopical to demand a caufe for the attractive power of gravity? Though all the mechanical accounts hitherto given of the caufe of gravity fhould be found unfatisfactory,

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may it not ſtill be owing to ſome unknown mechaniſm, or the intervention of matter, moving other matter? Or, though 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 vi-"ſible motion is no more connected with "a power of thinking, than it is with any "other property of matter or ſpirit." This may poſſibly be admitted in a certain ſenſe, viz. that there may, for aught 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,

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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; though 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

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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 increaſes. A body placed by itſelf would move no way; but two bodies run together. A given body is more attracted to a large 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 diverſified according to different circumſtances, can proceed from nothing but an intelligent cauſe, either mediately or immediately exerted upon bodies.

WHEN

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 profound ſyſtem of the ſceptical philoſophy *; but hath not yet been adopted by

* 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

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 a property inherent in matter; from

mary whereof, for the benefit of vulgar capacities, we of this nation enjoy in the Philoſophical Eſſays, and the Eſſays Moral and Political.

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, though he expreſsly 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

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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 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

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 active force of its own; for that would be oppoſite to the active force of gravitation.

If, in order to evade the foregoing argument, it ſhould be ſaid, that a body never tends to two oppoſite bodies at the ſame time; but tends only to one ſide with the difference of the forces with which it would tend to each of theſe bodies taken ſeparately: This will at leaſt afford another remarkable inſtance of that exact regularity with which the power of gravity is varied and adapted to the different ſituations of bodies; and which it is quite inconceivable that blind matter ſhould perform without the ſuperintendence of an intelligent being.

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It has indeed been frequently aſſerted, that bodies move towards each other by virtue of a law originally eſtabliſhed. But law, that is to ſay, a mere abſtract name or complex notion, which is no real being, cannot impell a ſtone, and cauſe it to begin to move. Law by itſelf, with ſubmiſſion be it ſpoken, will avail nothing, unleſs either the ſubjects of it have underſtanding 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

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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 mat-

ter 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, 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 end-

leſsly

lessly 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, with this or that new direction, or velocity; or the like. All which ideas are quite different from that of think-

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ing; 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 rea-

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ſon in comparing one thing 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,

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will be found entirely consistent with the indivisibility and unity of thinking substance. They do indeed exhibit a remarkable peculiarity in the manner of the propagation of these 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, though 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

lency 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 acknowledged, even by every Theist, to exist without a cause. But the instances of a wise administration and superintendence, presented every moment to our view, strike the mind in the most forcible manner. Weare 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 ra-
" tione et numero moveri possit sine consi-
" lio, in quo nihil est temerarium, nihil va-
" rium, nihil fortuitum. Ordo autem side-
" rum et constantia, neque naturam signifi-
" cat; est enim plena rationis: Neque fortu-
" nam, quae amica varietati constantiam
" respuit. Quae qui videat, non impie
" solum, verum etiam indocte faciat, si
" Deos esse neget. Nec sane multum in-
" teresst, utrum id neget, an eos omni pro-
" curatione,

"curatione, atque actione privet: Mihi e-
"nim, qui nihil agit, esse omnino non vi-
"detur." 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 established in the material world, and by which it is conducted. The most successful inquirers 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

friends to religion have done. Plutrach, 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 fruitleſs attempts have formerly been made to explain all the phaenomena 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

* Coſmologie.

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 reflection 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 æther was poſſeſſed of a real activity of its own. The elaſticity of that 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

ſaac's intention, to aſcribe activity to matter in any ſhape; though 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 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, though one ſingle cypher be of no value, yet an infinite number of nothings can amount to

* Philoſophical Eſſays, p. 119.

to a real quantity. There is no other poſſible method whereby to judge of the intelligence or wiſdom of any being, but by its 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 the effects of theſe powers in general, and without regard to any particular application of them, that we muſt determine concerning their nature. And it hath been already attempted to

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ſhew, that they are all probably derived from an active intelligence, either immediately or mediately exerciſed upon matter. A connection manifeſtly appears amongſt different parts of the ſyſtem of the world; and many bodies in it, though dead and inanimated, are not only moved and agitated themſelves, but communicate motion to each other, according to a certain fixed and eſtabliſhed order, which is called the courſe of nature. But there is no foundation in mechanics for imagining, that a lifeleſs body, in conſequence of a law promulgated ſome thouſands of years ago, can move itſelf with as much regularity and variety, as if it was actually endued with thought and reflection.

THIS, to moſt people, will appear an impoſſibility, as much as to make a blind man to ſee, whilſt he continues blind. And there are few, but will at leaſt acknowledge the contrary opinion to be ſupported with ſo high a degree of probability, as juſtly to deſerve the preference. "Haec quidem, ſi non vera, ſaltem veri-"ſimillima videtur."

"IT

"It is alledged, that the ſyſtem of the "world would diſcover more contrivance, "if it went on of itſelf, without any con-"currence 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 concourſe." The leſs power is exerted to produce a given effect, the mechaniſm may juſtly indeed be eſteemed the more perfect. 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.

things. If all the motions and changes 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 inquiries 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

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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 more eaſily amongſt one another than thoſe 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.

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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 intirely conſiſtent with the moſt abſolute *inertia*. But, that real actions, and equal oppoſite reactions, obtain in the active powers of attraction and 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; otherwaysit muſt break or give way. The power of coheſion may indeed greatly 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 fixed body, the force

with

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

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.

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. Matth. 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

dicularis EN: Æqualis 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 angulus 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

autem eſt MX ipſi XH: Ergo et KE ipſi EL eſt aequalis.

Perpendenti autem hanc propoſitionem ejuſque pulcherrimam reſolutionem et compoſitionem ſtatim 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.

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 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,

culi, 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 angulo 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 i-

gitur 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 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

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 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 aequalis 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.

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 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 parallela rectae AC. Quod quidem ita ſe habet.

COMPONETUR autem ſic. Quoniam eſt recta DE parallela rectae AC, erit angulus

gulus 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 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.

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 quaevis recta circulo occurrens in F, G; et CF, CG jungantur rectae AB occurrentes in H, K; erit rectangulum 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

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, 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 rectan-

gulum

gulum 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 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;

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 quaevis 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, 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 and FC, hoc eſt, ut rectangulum CFG ad quadratum ex CF; erit rectangulum ONP ad quadratum ex CN

ut

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. 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 punctum D, ut sit DB ad DA, ut quadratum ex CE ad quadratum ex CF; bifariam secetur EF

in

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

Sit recta AB circulo occurrens in A, B; et sint rectae AC, BC circulum contingentes

gentes 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 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

dratum 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 autem 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 aequale 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; erit rectangulum FLG, hoc eſt, quadratum ex AL, ad quadratum ex LC ut rectangulum FMG ad rectangulum NMO,

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 occurrens 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 occurrentes in H, K; erit rectangulum HAK ad rectangulum HBK, ut quadratum ex AD ad quadratum ex DB.

FACTUM jam ſit. Et ducatur DL parallela rectae AC, rectis CB, CF, CG occurrens

currens 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

quadrato ex DL, hoc eſt, quadrato ex BL, erit rectangulum OLP ad rectangulum MLN 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 rectangulum LEM ad rectangulum LFM, ut quadratum ex GE ad quadratum ex GF.

Factum jam sit. Per punctum D ducatur recta parallela rectae AC, rectis EB, CH, CK occurrens 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

dratum 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 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

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 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 quaevis recta circulo occurrens in H, K, et FH, FK jungantur, rectae CG occurrentes in L, M; erit rectangulum LGM aequale quadrato ex CG.

Factum

Factum jam fit. 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 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 rectangulum EOB ad rectangulum SOT: Et quoniam eſt rectangulum HNK

aequale

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 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

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 CD circulo occurrens 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

rallela 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 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

lum 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 quadratúm 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 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 occurrentes 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 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

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 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

dratum ex HF, et eſt quadratum ex FP ad quadratum ex PC, ut quadratum ex 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, alternando, 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. 1. Sint [Tab. 2. Fig. 13.] duae rectae AB, CD, ſectionem conicam contingentes 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

lum 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 datum punctum verget, vel parallelum erit poſitione datae.

Ut

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

Sit AB diameter circuli [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.

ART. IV.

Of the Cause of the Variation of the Obliquity of the Ecliptic, by COLIN M'LAURIN, *late Fellow of the Royal Society, and 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

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 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 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 deſcri-

bing 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, 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

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 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

gradual rifing of the furface of the land of Egypt, from the annual fediments of the Nile, feems to afford a much better argument againft their boafted antiquity in Egypt, than any thing that can be drawn from aftronomical obfervations, to fupport a vanity fo univerfal, that we need not go very far from home to find abfurd enough inftances of it. As for what is faid of Pitheas and other ancients, their wrong notions concerning the refractions, and the imperfection of their inftruments, render any conclufions on fo nice a fubject, drawn from their obfervations, dubious. We ought to fhew more regard to the modern obfervations brought for this doctrine, than to repreffent 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

of the atmosphere, made the mean distance of the moon less by 4 semidiameters of the earth than it really is. It is true, Mr Cassini's observation in 1655, compared with late ones, makes it vary at the rate of 55″ in 79 years. Let us therefore see, if a variation, such as that arises from Mess. Cassini's and Goden's observations, be not a consequence of principles already established in the solar system; and offer some hints of a method by which it may be ascertained whether this be the just solution of this question, and how, from a proper series of observations, made with great exactness, 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 sun, to greater perfection. This will be the 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

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, though 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

kind is the motion lately diſcovered by Mr Bradley, which, on this account, lay ſo long concealed from aſtronomers. Of the ſame kind is the motion 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 centre of the ſyſtem, or fixed point; but the centre of gravity of the whole ſyſtem. That the ſun therefore moves about this centre; and that, when Jupiter and Saturn, the two biggeſt planets, are in the ſame right line, on the ſame ſide of the ſun, the centre of the ſun will be almoſt a diameter of the ſun, diſtant from this fixed point. Hence, though we ſuppoſe the earth to move always in the ſame plane, the ſun will appear to have

different

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 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 phaenomenon.

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

the common centre of gravity of the ſun and Jupiter, which we may conſider as the centre of the earth's orbit, becauſe of the minuteneſs of the earth compared with theſe, T the ſolſtitial point, PΓ*p* the axis of the earth, IS the right line joining the centres 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*, and 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$\int$, which is equal to 2CTS, may be ſomewhat more than a minute. And this may be ſufficient to account for any variation that is yet diſcovered by aſtronomers. But, ſince it may exceed one minute, it appears that it is of moment to have this theory proſecuted farther and aſcertained: For an error of a minute, in a matter of ſuch moment as the declination of the ſun, which is an element in moſt queſtions in aſtronomy, geography, and navigation, is not to be deſpiſed; eſpecially now when ſo great exactneſs in theſe matters is deſired, and, I believe, may be obtained. Such a variation, however, can have no ſenſible effect on our ſeaſons; and there ſeems to be no ground to apprehend, that our year will degenerate into a perpetual ſpring 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 on 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 hour'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

ſudden and great changes on his ſurface are owing to tides analogous to thoſe we 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

P
C
K

B
C
A
H

D
P
F

TAB. II.

Fig. 10.

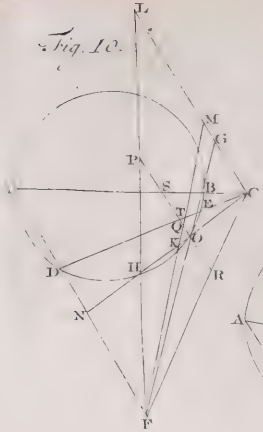

Fig. 11.

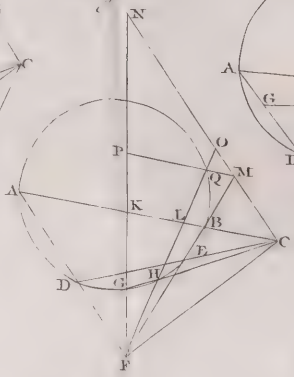

Fig. 12.

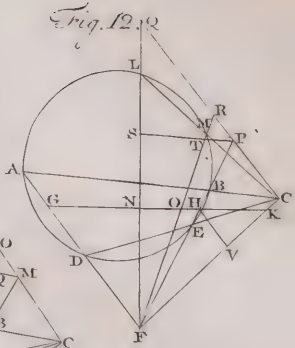

Fig. 13.

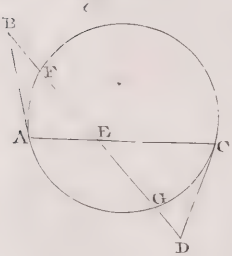

Fig. 14.

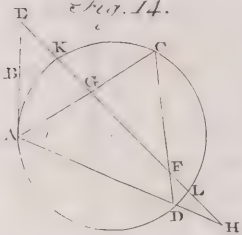

Fig. 15.

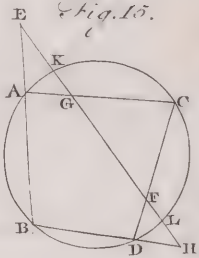

Fig. 16.

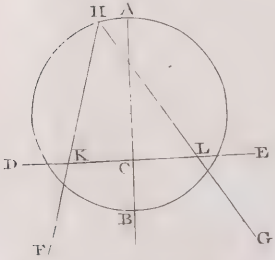

Fig. 17.

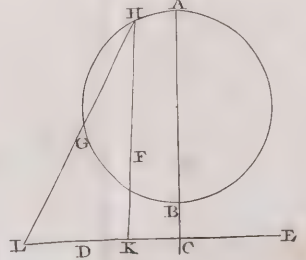

Fig. 18.

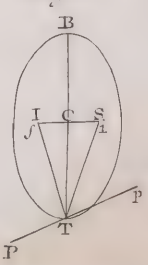

figure differs considerably from a sphere; and the gravitation towards him is not inversely as the square of the distance from his centre. Now, this variation from the regular course of gravity may produce some inequalities in their motions. And, in order to judge of these, I have computed the law of gravity towards such a spheroid as Jupiter; and will subjoin some theorems on this subject.

It seems somewhat extraordinary, 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 phaenomenon that passes over quickly, because of their swift motions, it may be the more useful, providing it can be seen.

No

No phaenomenon 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 phaenomenon, of that kind, of which it is perhaps beſt not to attempt any explication, till the ſame be confirmed by more obſervations.

It is worthy of our notice, that, from ſeveral phaenomena, aſtronomers conclude the ſatellites to revolve on their axes, 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

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; though we muſt alſo allow that the variety of nature is not to be limited by our conceptions.

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 centre 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

generating ellipse from the centre, d the distance of any body from the centre 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 MACFAIT, *M. D.*

THE experiment propoſed by Dr 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.

DR FRANKLIN 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 topmaſt 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 uſeful a propoſal deſerves to be examined: Variety of experiments may give hints for new improvements. For this reaſon the following obſervations are communicated, though 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

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

THE lightening 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

flame continued upon the end of the long rod, though 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 through the streets, no fire appeared upon 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

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 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.

THOUGH 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

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.

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

It ſeems plain that the glaſs-tubes were not of great uſe upon this occaſion, and that by being wet, they conducted the electrical fire nearly in the ſame manner as the iron rods.

It is probable, that thunder-guſts in cold or boiſterous weather are apter to ſtrike, becauſe the fire is more condenſed.

In warm weather the lightning expands itſelf more as it flies; and, by expanſion, loſes its vigour.

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,

more, in order to prevent thoſe blazing fires from fixing upon them, by the attraction of the ſharp points.

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 to Berwick, it lightened inceſſantly; but two claps of thunder only were heard, and thoſe very faint: Upon the whole, there is reaſon to think, that the fire of this ſtorm ſpread over the breadth of 130 miles, at leaſt. It began, at leaſt, as far ſouth as Liverpool, and went off upon the ſea at Inverneſs.

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

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

As

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.

But, becauſe proper inſtruments are not always at hand, it will be of uſe to mark the time when any principal range becomes vertical, or the time when the higheſt part of the range ſeems to touch any remarkable ſtar.

These vapours commonly aſſemble, at firſt, in a broad lucid yellow vein, flowing ſlowly eaſt and weſt. By degrees the vapours accumulate and turn brighter, and, by their mutual colliſions, are at laſt kindled and ſplit into thouſands of revolving cylindric columns, ſhewing a variety of colours, but principally red and green, and the red appears to be undermoſt. Thus they are ſcattered thro'

the ſky, and become faint in their colours; then they coaleſce and revolve, and are ſcattered again, or continue flaſhing hither and thither thro' the air, in a thin luminous vapour that ſeems to be compoſed of a groſſer and more volatile kind, and people imagine they ſometimes hear a faint crackling noiſe.

It is probable that the ſhining of the ſea is a phænomenon nearly related to thoſe above; namely, That it is owing to ſome volatile oily ſulphureous matter that ariſes from the ſea, and floats upon its ſurface when the evaporation is ſmall; as is the caſe at land with the *ignes fatui*, and thoſe fiery ſparks which often appear, when you tread in marſhy or moſſy low grounds, in damp weather. We may even ſuppoſe that thoſe particles are often carried down to the earth and ſea from the ſky, adhering to the particles of rain, hail, or ſnow, and continue floating there till they are evaporated again.

These things are agreeable to what obſervations I could make, and I leave them to be examined by thoſe who have better opportunity.

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

terjacent plains and valleys, one may often emerge out of the mist by going up some neighbouring 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.

DURING this kind of weather, there is another phænomenon observable, which I have seen many years ago; namely, a rainbow formed round one's shadow in the mist. The French and Spanish gentlemen who went to Peru, with a view to determine the figure of the earth, having, among many other difficulties, the embarrassment 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 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.

HAVING ſeen no account for this phænomenon, I ſhall now venture to offer my conjectures. As the ſun is a broad luminous ſurface, the ſhadows of bodies muſt be ſurrounded with a penumbra occaſioned by the different direction of the rays proceeding from each verge of the ſun and from the center; this becomes very ſenſible when you are upon ſome eminence with

the

the ſun near ſetting, and your ſhadow projected a good way on the other ſide.

When one has travelled for ſome time in the miſt, his cloths become, as it were, cruſted over with particles of dew or hoar froſt, and the rays of the ſun are refracted in paſſing through them. The rays which proceed from parts near each verge of the ſun form the exterior and interior bow, if I may call them ſo, and the rays that proceed from parts near the centre form a bow in the middle.

This agrees with the account of that phænomenon by Don Antonio D'Ulloa; but when there are only two bows, it would ſeem that they are formed only by the rays proceeding from each verge, which occaſioned the darkiſh interval above mentioned, and the great bending of the rays by refraction occaſioned the ſhadow to be contracted.

It would ſeem that this phænomenon was ſeen on the mountains of Peru always accompanied with the white bow above deſcribed; but the above obſervation ſhews they were not neceſſarily connected.

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 *fruſtum* 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

likewiſe made of braſs, and weighing about 20½ averdupois pounds, having the arms of Scotland and of Sterling marked *in relievo* upon 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 Stirling, 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 21¼ 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 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

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

	Troy Grains.
The Troy ounce being equal to	480
The Scotch ounce is equal to	476
The Averdupois 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 - -	$37\frac{2}{10}$
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 *." 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

* L. xi. c. 2.

more curious piece of workmanſhip than the body of the ſun, at leaſt as far as appears to us *. 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 ancients, 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 †; yea, and to reckon analogy a ſufficient proof, that ſuch properties as are generally obſervable in the one,

* Vid. Boyle's works, fol. edit. vol. 1. p. 428, and vol. 4. p. 523.

† Vid. TYROCIN bot. p. 3.

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 beings, capable of pleaſure and pain, deſire and averſion, &c *.

I

* " Vita et in animalibus et in plantis eſſe deprehenſa " eſt. ANAXAGORAS itaque et EMPEDOCLES deſiderio " plantas duci aiunt; ſentire item, ac triſtitia voluptate" teque affici, affirmant. Et ANAXAGORAS quidem, a" nimalia ipſas eſſe, et voluptate ac dolore moveri, do" cuit; E foliorum ſcilicet defluvio, et ex incremento, " iſtud colligens. EMPEDOCLES vero ſexum his admiſ" tum eſſe arbitratus eſt. Eodem modo PLATO quoque " appetitu ſolum illas duci, ob vehementem ſcilicet fa" cultatis altricis neceſſitatem, affirmabat. Quod ſi " conſtet plantas voluptate ac dolore affici, tum ſentire " quoque rationi erit conſentaneum: Et ubi hoc conſti" terit, tum appetitu quoque duci; ſi quidem ſomno re" ficiuntur, et vigiliis excitantur, rationi conſentaneum " erit. Ad eundem modum ſi quaeramus, an ſpiritum " ducant, et ſexuum miſtionem habeant, an contra ſit; " multam ſuper hoc ambiguitatem, et quaeſtionem pro" lixam excitabimus.—Cum in plantis reperiatur, quod " unaquaeque ſpecies maſculum genus habeat et femel" lum, et omnino quod maſculum eſt aſperius ac durius " rigi-

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 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 ancient, 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

"rigidiuſque, ſemellum debilius et fœcundius; quae-
"rendum rurſus eſt, inveniaturne haec duo genera ſi-
"mul commiſta 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 TIMÆUM, c. 2. Et librum de hiſtoria philoſophica, GALENO adſcriptum, ſub finem.

to be ſpurious; and that Crateva lived not before Mithridates, whom he complimented with the name of a plant, as Pliny * informs us. As for Hippocrates himſelf, I find in him a *conyza fœmina*, 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 φαϑι δε τινες, ſays he, και των ὁμοιογενων, τα μεν ἀνϑειν, τα δε ὁ, &c. "But "it is ſaid, that of plants of the ſame "kind, ſome flower, others not; as of "the palm-trees, that the male bloſſoms, "not the female, which about the ſame "time puts out the fruit. Plants there-"fore of the ſame kind are thus differ-"enced, as alſo all ſuch as cannot perfect "the fruit." † And elſewhere ‡, "The "moſt common diſtinction of trees, is in-"to female and male, whereof the one is "fertile,

* Lib. 25. c. 6.
† Hiſt. lib. 1. c. 22.
‡ Hiſt. lib. 3. c. 9.

"fertile, 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 *: So ſeems not much to have regarded analogy, in the diſtinction of ſexes, except it be in one ſpecies of the palm-tree.

3. BUT of this famous tree he ſays †, unleſs the *ſpatha* be cut from the male, and, while it retains the down, flowers, and duſt ‡, 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-

* Hiſt. lib. 2, c. 8.

† Hiſt. lib. 2. c. 9.

‡ Κονιορτὸς.

" tree and the palm-tree, is of uſe to the " female, ἀλλ' ἡ μεν ὁιον μιξις, ἡ δε κατ' ἀλλον τρόπον, " but, in the one* there is, *quaſi coitus*; in " the other it happens after a different " manner." And elſewhere " † That " the fruit will not continue on the fe- " male palm tree, unle's the flower of the " male with the κριστὸς be ſhaken over it, " as ſome affirm, is certainly ſingular: " For though there is an evident reaſon " for caprification, which it ſomewhat " reſembles; yet none can be aſſigned " for the effect of this ſprinkling." And, leſt 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 to appear, not in one or two inſtances ‡, but in all, or the greateſt part of the females, τὴν γὰρ ουσίν κτω κρίνομεν τὸ γειῶς. Of this tree more afterwards.

4. DIO-

* viz. Palm-tree.

† Cauſ. plant. lib. 3. c. 23.

‡ As here in the fig-tree and palm-tree.

4. DIOSCORIDES, who lived under Nero, has a male and female *mandragora*, *abrotanum*, *mercurialis*, *anagallis*, *ariſtolochia*, *ciſtus*, *filix*, *pæonia*, *polygonum*, *tithymalus*, *verbaſcum*, and an *arundo fœmina*: But, without any regard to analogy or to their fertility or barrenneſs, does he call them ſo; for his *mercurialis-mas* carries the ſeed, and the *fœmina* 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, foemina citra florem germinat tantum, ſpicae modo *." But, when he comes to mention particular plants, excepting what he has

* L. 13. c. 4.

"from

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 ancients, 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 entirely neglected. But Cæſalpinus * ſays expreſsly, "Fructum ferunt, non vero "florent oxycaedrus, taxus; in genere "herbaceo mercurialis, urtica, cannabis: "Quorum omnium ſteriles *mares* vocant, "*foeminas* autem fructioferas. In eo ta-"men genere foeminas melius provenire "et

* De plantis, l. 1 .c. 3.

" et foecundiores fieri aiunt, ſi juxta *ma-*
" *res* ſerantur; ut in palma eſt animad-
" verſum: Quaſi halitus quidam, ex ma-
" re efflans, debilem foeminae calorem
" expleat 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 thecae 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 very great and neceſſary; becauſe " even thoſe plants which have no flower " or foliature, are yet ſome way or other " attired;

" attired; ſo that it ſeems to perform its
" ſervice to the ſeed, as the foliature to the
" fruit. In diſcourſe hereof with our learn-
" ed Savilian profeſſor, Sir Thomas Mil-
" lington, he told me, he conceived that
" the attire doth ſerve as the male for the
" generation of the ſeed. I immediate-
" ly, replied, that I was of the ſame opi-
" nion; 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 be 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 * 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

* Hiſt. p. 17.

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 *.

9. AMONG the firſt who adopted this doctrine, was Mr John Ray, that great natural hiſtorian; at firſt indeed only as probable †, 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 8vo: 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 12mo ‡: This I have not ſeen, but only an abſtract of it in the appendix to Miſcl. nat. cur. Dec.

3.

* See Grew's anat. fol. p. 171.

† Vid. R. Hiſt. p. 18.

‡ Reprinted Francofurti, 1701, in 4to, Lin. bib. bot. 173.

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 ne-"ceſſaria eſt, ut ſi vel maris apices, vel foe-"minarum ſtyli, vel utraque deficiant, "nulla proles ſequi poſſit: Ut in frumen-"to turcico, cui juba praemature reſeca-"tur, et mercuriali, mare a foemina ſepa-"rata, conſtat." But then he propoſes 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 Valentini delivers them: "1*mo*, Plantae dantur apicibus co-"pioſis, aſt nullo ſtylo praeditae; adeo-"que mares ſine foeminis, ut in equiſeto, "lycopodio, &c. 2*do*, Videtur e contrario "ſibi obſervaſſe foemininum ſine pretenſo "virili ſemine. 3*tio*, Mercurialis, ſpina-

"chiae,

" chiae, et cannabis foemellas ſolitario,
" abſque vicini maris contagio excultas,
" plurima granula ſeminaque foecunda
" obtinuiſſe vidit: Et haec in cauſa erant,
" cur haec omnia cum aliquali oppoſiti
" formidine proponat."

11. In 1705, Mr Samuel Morland laid before the Royal Society, ſome new obſervations on the parts and uſe of the flower in plants *. Here, he ſays, " Dr " Grew falls ſhort, in that he ſuppoſes " the farina only to drop upon the out- " ſide of the uterus, and to impregnate " the included ſeed by ſome ſpirituous " emanations, 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 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

* Philoſ. Tranſact. No 287.

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

"plantarum expoſuit:" As the ſame profeſſor Heiſter * teſtifies.

13. In 1711, Mr Geoffroy preſented the Royal Academy at Paris with "obſervations on the ſtructure and uſe of the principal 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, 1mo, That, the germ is never to be ſeen in the ſeed till the apices ſhed their duſt. And, 2do, 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 conſequence was, ſome of the ears of the mays aborted altogether;

* Pref. p. 70.

together; others ripened a few ſeeds; as did alſo the mercurialis plants: Both which ſcemed to be foecund. "Perhaps, "ſays he, the duſt of the apices, brought "from ſome other place by the wind, fer-"tilized theſe few ſeeds: That this is not "impoſſible, we have a fine inſtance in "Jovianus Pontanus, preceptor to Alphon-"ſus King of Naples, who, in a poem, "tells us, that, in his time, there were two "palm trees, a male cultivated at Brin-"des and a female in the woods of Otran-"to, which carried no fruit for many "years. But at length, being elevated a-"bove 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 *." A very fine ſtory indeed!

I ſhall only further obſerve, concerning this learned author, that though he has added

* Vid. Mem. Acad. 1711.

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, though the one be Grew's and the other Morland's, he thus concludes:

"BUT, whichsoever of these conjectures "be pitched on, it remains always cer- "tain, by my observations, that the dust "of the apices, which has been hitherto "neglected as vile excrements, which in "a manner disfigured the flowers, are ne- "vertheless 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, entertained his audience with a discourse on the structure of

of flowers, and the uſe of their parts; which was printed in French and Latin at Leiden in 1718, in 4to: 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 to the uſe of the parts which I am to explain, I believe it will be much better 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 different ſexes of plants, &c." I ſhall notice only two or three things

things concerning this diſcourſe. 1mo, 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.

2do, 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 into the ſeeds, without penetrating their proper covers or ſhells. Beſides he aſſerts, and that truly too, that this 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, though 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 the fruit, continue perfectly white.

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,

3tio, 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 "igitur hae, quas Fallopianis comparo, "quod ad ova deferant, non exigua ipſa "illa pulveris foecundi grana, quae apices ſuper illas ejaculantur, aut in ipſarum excutiunt infundibulum, ut ſectatur "Leeuwen-

" Leeuwenhoekianorum atque Hartſoeke-
" rianorum phantaſmatum, voluit; ſed
" halitum modo, aut ſpiritum volatilem,
" qui pulvere hoc ſe expedit, ovaque ipſa
" foecundat. Credo enim, auditores, per-
" ſuaſum certumque habendum, non ma-
" teriam maſculinam, nec vermiculos ſup-
" poſititios, vel animalcula ſeminalia eſſe,
" quae impregnationem in fœmella abſol-
" vant: Quia Malpighius, narrante ana-
" tomico recente *, agnovit foetum reperi-
" ri in ovis ranarum et gallinarum ante
" copulam: Ut et certiſſimum eſt, ger-
" men adeſſe in ſeminibus plantarum quae
" non fuerunt impregnata, quorumque
" parenchyma facit cum germine ipſo
" continuum corpus. Non poterit ideo
" eſſe aliud quid, præter volatilem hunc
" ſpiritum, cui craſſior materies vehi-
" culi modo vicem praeſtat ſimplicis.
" Natura vero ſemper eaſdem ſectante le-
" ges, concludere oportet, id quod hac
" occaſione in animalibus contingit, idem

* M. DIONIS edit. 1715, p. 392.

"et vegetantibus accidere *." 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 Linnæus, 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: "Anthe-
"ras et ſtigmata † conſtituere ſexum
"plantarum, a palmicolis, Millingtono,
"Grewio,

* Vid. p. 17.

† That is, the apices, and extremity of the ſtylus.

"Grewio, Rayo, Camerario, Godofredo, "Morlando, Vaillantio, Blairio, Juſſievio, "Bradleyo, Royeno, Logano, &c. detec- "tum, deſcriptum, et pro infallibili aſ- "ſumptum: Nec ullum, apertis oculis "conſiderantem cujuſcunque plantae flo- "res, latere poteſt; quod demonſtratum "in Sponſalibus Plantarum, Upſaliae "1746, in 4to *." And elſewhere † "Generationem vegetabilium fieri, medi- "ante pollinis antherarum illapſu ſupra "ſtigmata nuda, quo rumpitur pollen, ef- "flatque auram ſeminalem, quae abſor- "betur ab humore ſtigmatis; quod con- "firmat 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: Gamerarius

* Syſtema naturae, edit. Lipſiae 1748, in 8vo, p. 216.

† Philoſoph. Botan. edit. Stockholm. 1751. in 8vo. page 91.

us himſelf doubted of it; Tournefort diſbelieved it; and Pontedera * 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 deſervedly 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, cannabis,

* Anthol. l. 2. p. 107.—185.

bis, &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 ſpinaciae, mercurialis, can-"nabis, abſque vicini maris contagio "excultam, ſemina foecunda 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

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 ſpecimen 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;

riantly; and, though 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, 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, though none of the ſpecies was

to

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 * of the bryonia, as noticed by Mr Millar †, yea of Mr Geoffroy's mays mentioned above.

18. THE learned Valentini ‡ 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 foeminina* 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

* Page 69.

† Gard. Dict. abridg.

‡ German. ephemer. l. c.

the 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 interveening 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 Linnæus, * "ut cannabis ſeminifera, unum al-"terumve ferat florem ſtaminiferum, "quo nonnullae foeminae impregnari "poſſint; quod Camerarium luſit." Granting this ſhould happen ſometimes, and that theſe gentlemen have ſeen it, (which, however, is not here aſſerted); yet

* Amaen. acad. vol. 1. edit. Lug. Bat. 1749 8vo. p. 99.

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 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 above mentioned, had really ſuch flowers in it, which they cannot aſſert. For my part, I can declare that, though I ſearched very carefully for the ſeed of the hemp-plant *, 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

* Exp. 2.

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 Linnæus, in his Fundamenta Botanica; explained by Jo. Geſnerus *, and demonſtrated, they ſay, by Jo. Wahlbom †, and Car. Linnæus himſelf ‡.

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 autopſia (ſays J. Geſner) " hunc generationis actum edocet. Pri- " mus qui florum connubia vidit, cel. D. " Seb.

* Diſſertationes phyſicae de vegetabilibus, Lugd. Bat. 1743. 8vo, conjunctum cum C. Linnæi oratione de neceſſitate peregrinationum. Explicant elementa botanica Linnæi. Lin. bib. p. 174.

† In the ſponſalia plantarum, Amaen. acad. vol. 1. p. 61. ad 109.

‡ In phil. botan.

" Seb. Vaillant * tam eleganter deſcribit,
" ut nemini de hac re dubium ſupereſſe
" poſſit " Ita autem cl. Vaillant, " Quo-
" ties acciderit, ut in eadem ſtirpe flores
" gerantur ſimul, quorum hi fœminina
" tantum, illi autem maſculina et fœmi-
" nina conjuncta, organa cingunt, arrectio
" tumorque organorum maſculinorum
" in his tam ſubito contingit, ut lobuli
" gemmae floſculoſæ cedant illorum im-
" petui, atque hinc inde ſemet expandant,
" mirabili mehercle velocitate, et extem-
" plo quam violentiſſime fœcundam ex-
" plodant, omnemque uno impetu ejacu-
" lentur genituram: diffuſa nimirum pul-
" verulenta nubecula, ſpargente quaqua-
" verſum fœcundationem arvi genitalis.
" Vix venereus hic ludus abſolutus eſt,
" quin illico florum labia, aut lobuli, ad
" ſe invicem accedant, eodem quidem
" quo a ſe mutuo receſſerant, celeritatis
" impetu, veteremque ita formam ſtatim
" renovent: Apparatum hunc artificio-
" ſum facili ſpectare datur in *parietaria*,
" Sed

* In lib. de ſtructura florum.

"Sed accedas opportet hora ſacra veneri; "aurora eſt quae favet his congreſſibus; "ubi vero agere renuunt ſatis opportune, "aciculae apice, leniter modo, ſtimulus. "In hermaphroditis, ubi duo ſexus con-"juncti habentur, multum abeſt ut tan-"to impetu explodatur. Plerique enim "flores, praecipue nutantes, in quibus "piſtillum obliquum intra ſtamina poſi-"tum habet actum generationis floribus "clauſis exercent, dum ſtigma adhuc in-"tra medias antheras continetur." Thus Mr. Geſner *.

22. ALLOW me here to obſerve, in the firſt place, that this artificial apparatus † was ſeen and deſcribed by John Bauhinus, long before Vaillant was born ‡, and transf-

* Diſſ. p. 86.

† Tout cette mechanique, Vaill.

‡ "Parietariae floſculi conferti circa caulem ex foli-"orum alis, floccos coccinei ſerici imitantur, primulum "e nodulo emicantes: Poſt ſe ſtamina oſtendunt obſcure, "ex albo purpuraſcentibus apiculis involuta, quae ſi "ſtylo evolvere coneris, ſubitultum excuſſo pulvere cum "impetu, ſpectaculo jucundo, ſe expandunt repanda, "in medio ſeminis rudimentum circundantia." I. B. 2. p. 976.

tranſcribed both by Moriſon and Ray in their hiſtories of plants, though 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 Linnæus himſelf, " parietariae hermaphroditi, flores duo continentur involucro plano hexaphyllo. Calyx monophyllus, magnitudine involucri dimidiati. Stamina filamenta quatuor, calyce longiora, illumque expandentia. Foemineus flos unus, inter hermaphroditos ambos intra involucrum; cum calyce ut hermaphroditi *." Now, ſince the ſtamina of the hermaphrodite flowers, as well as the germen of the female flower 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

* Gen. pl. p. 494.

ſelves 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*; 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 ſeſe in plantis habere, (ſays "M. Whalbom) dictitat primo oculos. "Flore floreſcente, et polline antherarum "volitante, quod ſtigmati pollen inhæreat, "prima fronte obvium eſt †." But it is not at

* "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 ſceleta magnanimorum heroum, aliquamdiu "erecta in campo conflictus, ubi apluſtrium inſtar, jo- "cularios experiuntur luſus volitantis zephyri."

† Amaen. Acad. I. p. 90.

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 *violae tricoloris*, *gratiolae*, *iridis*, *campunalae*, *et ſyngeneſiarum*, in the leaſt favour him. For, though 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 fili-"formis erectus, ſtaminum longitudine, "antherarum cylindrum perforans; ſtig-"ma bipartitum, laciniis revolutis, pa-"tentibus." *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 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,

AGAIN, the ſtylus of the campanula is commonly much longer than the ſtamina, and briſtly a little above them, as 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 communice-"tur Iris particularum nobis oſtendit "ſtructuram; ſtigmata enim ſeſe dilatan-"tia, antheras omnino operiunt; illum "tamen ad petala referunt ſitum, ut, au-"ra 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 Mo-"rilandus; ejuſdem eſſentiam extrahi, "mediante ſtigmate madido, ſtatuit Vail-"lant: Pollinem aceris rumpi in humore "vidit Bern. Juſſiaeus: Omnem pollinem "in humore explodere auram ſeminalem, "confirmat Needham *." But Morland's opinion is fully confuted by Vaillant, who maintains only that the vapour, or volatile 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, although it ſhould all burſt in moiſture, I do not ſee how it thence follows, that it thus ſends out an *aura ſeminalis*; eſpecially, ſince it does

* Phil. Bot. p. 91.

does not appear, that the ſtigmata muſt neceſſarily be moiſt in order to fertility; and it is obſerved by Mr Walhbom*, That, "in omnibus fere floribus conſpicitur, "quomodo, aëre humido, floſculos com-"plicant, 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 Geſne-"rus) †, 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

* Amaen Acad. 1. p. 93.

† Diſſert. p. 91.

ſeldom a very ſmall one: The umbelliferae, as many for two ſeeds, &c.

"ITA ſeſe in plantis habere dictitat "ſecunda proportio: Plerumq ie ſtamina "et piſtilla eandem ferunt altitudinem, "ut eo melius ad ſtigma pollen, mediante "vento accedat; in quibuſdam vero non, "ubi ſingularis obſervatur proceſſus foe-"cundationis *." 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 the ſtamina †.

THIS argument is thus explained by Linnæus. "*Proportio:* Stigmata ſeſe flec-"tere ad antheras, dein exſeri ex diantho, "paſſiflora,

* Wahlbom. Amaen acad. 1. p. 90.

† See Pontedera for more inſtances. Anthol. l. 2. c. 8.

" paſſiflora, nigella, patet. Piſtillum ubi
" breviſſimum, connivent antherae ſupra
" ſtigmata: Saxifraga, parnaſſia. Con-
" nivent dum efflant pollinem antherae
" in celoſia. Comprimit corolla digitis
" antheras ad ſtigmata in teucrio *."
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 third argument is, " Ex loco
" ſtaminum et piſtilli, non leve argumen-
" tum petere poſſumus; nam in pleriſque
" plantis, floribus hermaphroditicis prae-
" ditis, ſtamina ambiunt ovarium, et ea
" ratione ut maxima pars geniturae ad
" ſtigma accedat;" Geſnerus †. 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,

* Phil. Bot. p. 91.

† Diſſert. p. 9.

"Tertio, *locus*. Etenim ſtamina plerum-
" que piſtillum ambiunt, ut ventorum ope
" ſemper quidquam pulveris attingat ſtig-
" ma;" Wahlbom *. 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 fœcundity; eſpecially when this is not always their ſituation. He adds, "Mone-
" ciae flores maſculi plerumque ſupra flo-
" res fœmineos collocantur, ut pollen eo
" melius in piſtillum decidat;" and inſtances ricinus among others: But, ſure I am, the ricinus vulgaris B. p. has all its female flowers 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 ſemine am-
" bae;" Linnæus †. But this cannot be proven. Yea, Camerarius aſſerts the contrary.

* Amæn. Acad. 1. p. 91.

† Phil. Bot. p. 91.

trary *. But, granting it true, it proves nothing, confirms nothing.

27. ARGUMENT 4. "Ex *tempore* quoque "vegetationis harum partium concludere "datum eſt: Namque in antheris, ſtami-"na farinam fœcundantem eo tempore "continent, quo piſtillum viget, deinde, "excuſſo pulvere, peracta fœcundatione, "perit ſtamen; ſuccus copioſior in piſtil-"lum devolutus efficit ut fructum ma-"tureſcat;" Geſnerus †. But the ſtamina ſpinaciæ, mercurialis, cannabis, mays, juniperi, violæ martiæ, &c. ſhed their duſt commonly before their ſtigmata are viſible.

"Quarto, *tempus*. PRIMUM hic attenden-"dum venit, quod ſtamina et piſtilla una "proveniant, exceptis tantum pauciſſimis. "Alterum, quod ubicunque flores maſ-"culi diſtinctis a fœmininis gaudent "thalamis, aut in eadem aut diverſa plan-"ta, et *ubi maſculi flores nec perpendicula-"riter ſupra foemineos erecti ſunt*; ibi, flo-"reſcentia ante foliorum exortum pera-"gatur,

* See No. 10. ſupra.

† Diſſert. p. 91.

" gatur, neceſſe eſt; ne, foliis interveni-
" entibus, inhibeatur fœcundatio: Ex.
" gr. in *moro, viſco, mercuriali perenni,*
" &c;" Wahlbom *. 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 ger-
" minationem foliorum plerumque pro-
" deunt, ne folia tegant piſtilla; ſalix,
" populus, corylus &c.;" Linnæus †. Here, as commonly, we find *plerumque*, and an imaginary reaſon.

28. ARGUMENT 5. viz. *pluviæ*: " Fit
" inde ut, dum pluvia eluit pulverem ſta-
" minum, germina piſtilli decidant, aut
" in fructus naſcantur abortivos, ut ſtil-
" lant vites, percoquantur, maraſmo ex-
" areſcant, locum concedant inſectorum
" nidis et eorum evolutioni ut fruges u-
" ſtilagine pereant, et quae ſunt alia vitia.
" Sedulo itaque natura ubique ſolicita
" fuit,

* Amæn. Acad. 1. p. 92.
† Phil. Bot. p. 91.

"fuit, ut genitura illibata ad piſtillum "perveniret." Geſnerus *; 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 *pluviæ*. In omnibus fere "floribus conſpicitur, quomodo, urente "ſole, ſeſe expandant; veſpertino vero "tempore, et aëre humido, floſculos com-"plicent, ne aqua pollen antherarum at-"tingat et coagulet, quo facto, ad ſtigmata "efflari nequeat; at ſtigmate, mirum ſane! "ſemel fœcundato, nec veſpere, nec plu-"via ingruente, ſeſe contrahant flores;" Wahlbom †. But our author knows, that many plants cloſe their leaves in the night or in rain; as the *acaciæ*, *mimoſæ*, &c. that many open their flowers in the night, and ſhut them when the ſun is hot; as ſome cerei, ketmiæ xyla, lychnis noctiflora, mi-

* Diſſert. p. 91.

† Amœn. Acad. 1. p. 93.

rabilis peruviana, &c. * 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 tem-"pore, ſi pluvia cadit, pollen conglobatur, hincque annonam difficilem auguratur agricola, nec immerito; grana "enim imminuuntur exinde, quod plerique floſculi abortum paſſi ſint." That ſecale, triticum, many gramina, plantagines, pimpinellæ, &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

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

of these plants seems to afford an argument, not contemptible, against the sexual 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.

"*Pluviæ* (says Linnæus) * combibunt "pollen, ut in stigmata cadere nequeat; "hortulanis notissimum in drupiferis, (so "he calls the *amygdalus, persica, prunus,* "*armeniaca, cerasus, laurocerasus*, &c.) et "pomiferis.

* Phil. Bot. p. 91.

"pomiferis. Agricolis deteſtabilis in "agris ſecalinis (why not alſo triticeis?) "fumus idem etiam facit, abſorbendo "humidum ſtigmatis." So the ſtigmata muſt neither be wetted nor dried. But this is fully 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 "argumentorum eſſe poteſt (ſays J. Geſ-"nerus *) modus quo fœcundatio palmae "dactyliferæ, ad obtinendos dactylorum "fructus maturos, apud Perſas inſtitui-"tur, a cl. Kæmpfero, in his locis, an-"notatus." 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 †, Proſper Alpinus ‡, Hadgi Muſtapha Aga ‖, Engel-

* Diſſert. p. 85.

† Harris collect. vol. 1. p. 347.

‡ De pl. Aeg. p. 24

‖ T. Juſt. p 69.

Engelbertus Kaempfer *, Pere Labat †, Chriſt. Got. Ludwig. ‡, &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 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 fructuum rudimenta, inde-"clinabili abortu dimittunt;" Kaempfer. "In fructa pulpæ loco adeſt cortex durior, "ſiccus, adſtringens, officulum vel nul-"lum vel tenue;" Ludwig: Which are all contradicted by Labat.

31. "IT

* Amæn. exot. p. 706.

† Voyage aux iſles de L'Amerique, Hague edit. vol. 1 part 2. p. 209.

‡ In J. Geſner. diſſ. p. 86.

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, though ſingle: Whether it is male 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

St Domingo "For, although 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 * relates, that he ſaw only one date-bearing palmtree at Montpelier: "Centeſimum annum ſuperare "creditur (ſays he) et vulgo ibi perſua-"ſum, ante grandem aetatem fructum "non proferre, et vix ante quinquageſi-"mum annum, ut quidem, perhibebant." Hence Pontanus's fiction is eaſily accounted for, if there be any truth in it; and perhaps ſome others, particularly concerning the amours of theſe trees; for which ſee Pliny †, and Caſſianus Baſſus ‡.

39. But I cannot omit the opinion of Herodotus, the moſt ancient author, who has

* Hiſt. 1. p. 360.

† L. 13. c. 4.

‡ In Theoph. p. 103.

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 maſculos vocant, fructus "palmis glandiferis alligant, ut earum "fructum maturet culex ſubiens, ne ex "arbore is defluat. Ferunt enim palma-"rum mares * culices in fructu, quem-"admodum caprifici." Thus Herodot, as rendered by Bod. a Stapel †. Who elſewhere ‡ quotes a famous traveller for ſuch a culture of this tree, as confirms Herodot's opinion ||.

AGAIN

* ἄρσενα.

† In Theoph. p. 115.

‡ P. 103.

|| "Agricolae etiamnum, ut refert doctiſſimus GUIL-"LAND, in Arabia, Ægypto, Meſopotamia, Judaea, "Phoenicia, et tota Syria, volentes, cavere ne foeminae "aut ſterilitatis noxam incurrant, aut fructum, quem "aliquando imaginatione libidinis, ut ita dicam, con-"cipere ſolent, ante maturitatem amittant, ita ordi-"nant

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 habitudinem refe-"renda eſt, arenoſam ſcilicet et ſalſam, "plantae huic gratiſſimam. Vidi his "locis, in palmarum veluti ſylvis, terram "copioſiſſimi nitri calida nive late con-"ſperſam, vel aquis Nili, vel roſcida noc-"tium humiditate fervidiore ſole peru-"ſtis. Nec opus hic maritali cinere pal-"mae effoeminatae vigorem incitare. "Flantibus enim ab auſtro per Æthiopi-

"am,

"nant utriuſque ſexus palmas, ut mares eo ſaltem inter-"vallo a foeminis diſtent, quo pulvis, ventorum flati-"bus a foliis maſcularum ſublatus, in foeminarum folia "incidat; idque ſatis ad foecunditatem, et fructus ma-"turationem 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 coloni *funem a mare religatam ad* "*foeminam uſque producere*; atque 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.

" am, et ſteriles Arabiae deſertae campos " urentibus, ventis ; ingens nitroſi pulve- " ris ſublata vis, abunde cacumina pal- " marum vegetat.—Meminique ſic onu- " ſtum fuiſſe dactylis ſuis unicum race- " mum, ut eum attollere a terra prae " pondere vix ſuſtinerem *." 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 †. 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, palma- " rum feraci, a prudentioribus viris de " hac re ſciſcitarer, certum nihil accipere " potui. De lupulo certius loquor. In " horto

* Veſling in Alpin. c. 7. p. 11.

† V. Alpin. de plant Æg. p. 25.

"horto Regio Parisiensi luxuriat fructi-
"bus quotannis onustus. Qui vero flo-
"ribus gaudet, non occurrit nisi in insu-
"lis Sequanae et Matronae, longe distan-
"tibus: In horto Regio tamen semina
"profert" *.

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 sta-
"miniferis, alter pistilliferis; idque quod
"fructum vulgo vocant, est calyx tantum
"explicatus et elongatus: Hinc humulus,
"quamvis foemina, nec foecundata, co-
"nos tamen proferre valet. Hoc Tour-
"nefortium decepit, ne sexum plantarum
"agnoscerit, quum lupulus (foemina) in
"horto Parisiensi luxuriabat, fructibus
"quotannis onustus; qui vero floribus
"gaudebat (mas) non occurrebat nisi in
"insulis Matronae et Sequanae multum
"distantibus †. Idem fit in moro et bli-
"to,

* Inst. p. 69.
† Tournefort, Hag. p. 69.

"to, cujus baccae calyces ſunt ſuccu-
"lenti; minime pericarpia, ſeu ova-
"ria." *

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 †, without which the character of the humulus, *in Linnaei Genera Plantarum* ‡ 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" ‖; and in his *Philoſophia Botanica* **, "fructus ex ſemine ſive peri-"carpio, ſive non tectum ſit, dignoſcitur." No matter therefore, whether ye call theſe cones *calyces elongatos*, or *fructus*, if they contain

* Amaen. acad. 1. p. 99.

† Vid. T. p. 535. t. 309.

‡ P. 477.

‖ § 88.

** 56.

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 * gives blitum a pericarpium; and deſcribes a pericarpium to be "viſcus gra-"vidum ſeminibus, quae matura dimit-"tit" †: And conſequently Walhbom and he don't well agree. But, to return to the palm-tree,

35. "SEXTO, Palmicolas palmarum "ſpadices maſculos divellere, eoſdem-"que ſupra foeminas collocare, memo-"riae mandarunt Theophraſtus, Plinius, "Alpinus, Tournefortius, Kaempferus, "alii: Quo neglecto, dactyli acerbi, et "nucibus deſtituti, fiunt;" Wahlbom ‡. This is anſwered above (No. 31.). Then the author gives a long paragraph out of Kaempfer

* Gen. pl. p. 5.

† Phil. Bot. p. 53.

‡ Amaen, Acad. 1. p. 94.

Kaempfer *; as does Gesnerus †, from the same page; yet they differ widely. I have not at present 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, as-"servari possunt;" Wahlbom.

§36. "PALMICOLÆ: notissima Theophra-"sto, Plinio, Kaempfero, aliisque. Pistaciae "cultura in Archipelago; Tournefortius. "Caprificatio veterum, et adhuc in Archi-"pelago, per insecta." Vid. dissert. nostr. de ficu; Linnaeus ‡. Neither Theophrastus, Pliny, or 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 Archi-

* Amaen. p. 706.

† Dissert. p. 85.

‡ Phil. bot. p. 92.

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 *, 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; though 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 †, Pliny ‡, and Tournefort §, or Pontedera ** only who quotes

* Tom. 2, p. 417.

† De cauſ. pl. l. 2. c. 12.

‡ l. 15. c. 19

§ Voy let 8.

** Anthol. l, 2. c. 33. 34. 35.

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* *; 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 †.

38. If

* In C. Linnaei, Amaen. Acad. vol. 1, p. 213. 243.

† "Cupido ficus nobis dicitur, quem antiqui psenem seu insectum vocarunt ficarium; et Pontedera, Anthol. 172. descripsit, estque species ichneumonis. Hisce ichneumonibus jam mutatis, alisque instructis, tempus adest, quo caprificus, seu ficus mas, florescit, i. e. farinam edit antherarum; 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 farinam 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 sun-

38. If it be ſtill alledged, that the infoecundity of the date-ſtones of Martini-

ſundum oſtendunt, ingrediuntur foemellae, et ex infimo ventre producta tuba, ſoeturae in frumentis nidum excavant, et ovula deponunt. Naſcuntur in his vermiculi, qui deinde nymphae evadunt immobiles, durae, corpore oblongo, capite cum dorſo luteolo, caetera primum albae, mox nigrae. Perforato deinde nido, exit animalculum, plerumque nondum 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, infarcitur, quippe molliuſculum. Quare e groſſis egreſſum, et ſole exſiccatum, pulverem diſcutit ad hunc modum: Stans quatuor anterioribus pedibus innititur, et duobus poſtremis abdomen, lumbos, pinnas, pulvere mundat, iterum atque iterum cruribus detergens; deinde quatuor poſterioribus ſeſe liberans 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 animalculis 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 Italia caprificationis indiget, ſed ſine groſſis ſativae ficus ſua coquunt poma. In Graecia hoc culturae opus perpetuum non eſt, ſerotina poma non caprificantur; neque praecociorum in macro ſolo, et in aquilonio,

in

co demonſtrates the foecundating virtue of the male duſt; I anſwer by no means: For Pere Labat 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,

in ipſa Graecia ulla ſit caprificatio. Pomum caprificatum bonitate inferius eſt non caprificato et inſuavius. Hinc illi qui in Graecia ficus venundabant, quo facilius emptores allicerent, συκα α ψεριναςα 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 feminibus, 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.

ſides, 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. ARGUMENT 7. "Septimo, flores "nutantes. Cum pollen maſculus, plerumque aëre ſpecifice gravior, difficile "ſurſum tenderet; apud pleraſque plantas, piſtillum longius gerentes florem "nutantem fecit Creator, ut ſtigma eo "melius attingat pulvis; e. gr in galantho leucoio, cyclamine, narciſſo, fritillaria, campanula, erythronio, &c.;" Wahlbom *. "Flores nutantes gaudent "piſtillo, ſtaminibus longiore, ut cadat "pollen in ſtigma: Campanula, leucoium, galanthus, fritillaria;" Linnaeus †.

I readily grant, that the *pollen maſculum*, or *pulvis apicum*, is heavier than air, it being

* Amæn. Acad. 1. p. 95.

† Phil. Bot. p. 92.

ing 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 horizontal, while the piſtillum and ſtamina bear the ſame proportion to one another: e. g. ſome ſpecies of the narciſſus, campanula, lilium, &c. And although 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 *.

40. Ar-

* Vid. No 25.

40. ARGUMENT 8. "Videte et ad-
"miremini ſolertiam, quam Natura adhi-
"bet 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 recondun-
"tur ſubmerſi, ſolicite per petalorum
"commiſſuras clauduntur, apicibus ver-
"ſus ſtigma inclinatis, et verſus interio-
"ra tantum farinaceis, exteriori ſuperfi-
"cie membranacea et lata; ut in nym-
"phaea et affinibus apparet;" Geſne-
rus *.

"OCTAVO, ſubmerſi. Plantae haud
"paucae caule ſub aqua latent; inſtante
"vero floreſcentia, enatant flores, ut
"nymphaea, &c. Aliae vero ſub aqua
"omnibus ſuis partibus occultantur; ut
"myriophyllum, ſtratiotes, potamogeto-
"nes plerique, qui omnes, ſub floreſcen-
"tia, ſpicam flores ſupra aquas exſerunt,
"deinde

* Diſſert. p. 92.

" deinde iterum peracta floreſcentia, de-
" mergitur ſpica;" Wahlbom *.

" Flores ſubmerſi adſcendunt ſub flo-
" reſcentia: Nymphaea, ſtratiotes, my-
" riophyllum potamogeton, hydrocharis,
" valiſneria;" Linnaeus †.

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 pe-
" talorum commiſſuras clauduntur, api-
" cibus verſus ſtigma inclinatis, et verſus
" interiora tantum farinaceis;" the duſt, or its *aura ſeminalis*, muſt there have much eaſier acceſs to the ſtigma, than it can have, when the ſtamina are ſeparated and expoſed to the winds; eſpecially, if it

* Amaen Acad. 1. p. 96.

† Phil. Bot. p. 92.

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 ac-
"tum generationis (adds Mr Geſner) flo-
"ribus clauſis exercent, dum ſtigma ad-
"huc intra medias antheras contine-
"tur *."

41. I mention theſe two learned authors, though I have the misfortune to differ from them, not only becauſe they are recommended by Linnaeus, eſpecially Vallaint, of whom he ſays, "primus cla-
"re ſexum expoſuit †." 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. ‡; for one can-

* Vid. n. 21. ſupra.

† Bib. bot. 173.

‡ That is for a ſpecimen of botanical ſuperfluity; urtica foliis cordatis amentis, cylindraceis, ſexu diſtinctis, mas,

cannot 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 fru-
"ſtranea. Flores compoſiti variis mo-
"dis fabricati ſunt.—Polygamia fruſtra-
"nea foeminis exſultat maritatis, totum
"diſcum occupantibus; floſculi vero
"foeminei radium conſtituentes, ob de-
"fectum ſtigmatis, abundante licet diſ-
"ci pulvere, familiam propagare neque-
"unt;" Wahlbom *. I omit the reſt, ſince

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. 138.; urtica perennis, Amaen. Acad. 2. 25. 99.

* Amæn. Acad. 1. p. 96.

ſince Linnaeus * ſays only, "ſyngeneſia "fruſtranea: Ubi ſtigma deeſt, ibi nulla "foecundatio: In radio centaureae, he-"lianthi, rudbeckiae, coreopſidis." 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 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 dic-"titat, decimo, omnium florum genuina "conſideratio. Brevitatis cauſa nonnul-"los tantum hic examinare lubet;" Wahlbom †. 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.

* Phil. Bot. p 92.

† Amaen. Acad. 1. p. 97.

44. But ſince Malpighius, whom I reckon one of the moſt genuine contemplators of flowers, obſerves, * 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 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 *. And, conſequently, that "Omnium florum genuina conſideratio nec dictitat, nec confirmat, generationem vegetabilium fieri" in the manner the ſexualiſts pretend.

45. Although I have already, perhaps, been too tedious, and ſaid enough to

* P. 63. edit. in 4to.

† Vid, No. 40. ſupra.

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, "Antheraseſſe "plantarum genitalia maſculina, et ea-"rum pollen veram genituram, docet eſ-"ſentia, praecedentia, ſitus, tempus, lo-"culamenta, caſtratio, pollinis ſtructura: "Stigmata, germini ubique adnexa, eſſe "genetalia foeminina, probat eſſentia, "praecedentia, ſitus, tempus, caſus, ab-"ſciſſio *." Which in Phil Bot. †, and Sponſ. Pl. ‡, are explained: But confirmed only, either by miſtakes, or by falſe conſequences. Thus,

"Situs: Didynamiſtis ſtamina adſcen-"dunt ſub corollae labium ſuperius, quo "et ſe piſtillum flectit." But in the deſcription of his claſs 14. or didynamia, "Antherae ſub labio ſuperiore ſaepius "reconditae §". And, concerning the ſitus,

* Lin. Fund. Bot. § 143. 144.

† P. 90.

‡ P. 84.

§ Vid. Lin. Gen. pl. p. 261.

tus, he adds, " *Monœciae pleraeque* flores " ſtamineos ſupra piſtilliferos gerunt: Zea, " ricinus." But the *flores ſtaminei* 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 fœcundans*, and conſequently the ſexes of plants. " Veritatem hanc pro- " bat caſtratio. Si antheras alicujus plan- " tae uniflorae auferamus, et, ne aliqua " alia ejuſdem ſpeciei adſit, curemus; a- " bortit fructus, vel ſaltem ova profert " ſubventanea; quod adeo certum, ut " quiſque nullo non ſucceſſu id experiri " queat;" Wahlbom *. " Caſtratio: " *Melonis* flores ſtamineos qui diligenter " auferunt, fructus non obtinent. Tuli- " pae ſolitariae, ſi auferantur antherae an- " te

* Amaen, Acad. I. p. 86.

"te casum pollinis, sterilis evadet;" Linnæus *.

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 floris foliis, antequam hiarent, "in tulipa praecipue, expectavi an stylus "incrementa caperet; et interdum ejus-"dem incrementum remorari observavi, "quandoque quaedam semina, absque "noxa, debitam sortita sunt magnitudi-"nem;" Malpighius †. 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 ‡: And I made the trial in tulips, *nullo cum successu*. Thus,

47. One year, observing two strong tulips growing together, in an inclosure surrounded

* Phil. Bot. p. 90. and 92.

† Oper. p. 70.

‡ Vid. Mem. Acad. 1711.

ſurrounded with a tall and thick quick-ſet 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,

them, 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 *, 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.

49. "OMNE vivum ex ovo; per con-
"ſequens etiam vegetabilia: Ovum, non
"fœcun-

* Phil. Tranſ. No 490.

"fœcundatum germinare, negat omnis "experientia; adeoque et ova vegetabi- "lium;" Linnæus *. I ſhall not here enquire, whether either of theſe propoſitions are certainly true; 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) † non omnia ſimi- "liter atque in animalibus accipi debent: "Nam (in plantarum genere) vis undi- "que germinandi habetur; quoniam et "undique animatum eſt." And Malpighius ‡, "In vegetantibus, ubi non eſt "tantus

* Fund. bot. § 132.—150.

† De hiſt. pl. 1. c. 1.

‡ Anat. pl. p. 76.

" tantus organorum apparatus, et cuili-
" bet ſenſibili particulae, omnia inſunt
" quae in toto deprehenduntur: *Nulla in-*
" *tercedente generatione*, abſciſſi quicun-
" que rami frequenter in novam ſobolem
" excreſcunt; vel naturae miniſterio, de-
" ciduae minimae et compendiariae plan-
" tulae, a tenellis ſurculis 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 bulbous roots but gems? And ſuch Cæſalpinus 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 ſobo es a
" ſemine, ut fœtus vivens ab ovo; ſemen
" enim tanquam ovum eſt, in quo eſt prin-

"cipium vitale, at vita nequaquam; ſo-
"boles autem vivit, primo quidem juxta
"parentem, ut ejus germen, poſtea vero
"per ſeipſum, propriis radicibus ex terra
"humorem trahens;" Cæſalp de plantis *:
And Malpighius †, "Gemmæ ſunt velut
"infans, ſeu fœtus ita cuſtoditus, ut ſuo
"tempore auctus, in ſurculum excreſcens,
"tandem ova promat. Erit igitur pro-
"babiliter ſemen quaſi gemma pendula et
"decidua, alieno germinatura ſolo." And
although the learned Petrus Loſling, in
his treatiſe called *Gemmæ Arborum* ‡, or ra-
ther the publiſher himſelf, is pleaſed to
ſay, "qui gemmam ſibi repreſentant ut
"alterum ſemen, fallunt et falluntur ‖;"
yet his deſcription of a gemma ſeems not
altogether to agree with this *notandum*:
For § he ſays, "Gemma eſt pars plantae
"radici inſidens, quae occultat ſquamis,
"foliorum rudimentis, embryonem futu-
"rae

* Lib. 1. c. 5.

† Anat. Plant. p. 39. 77.

‡ Amaen. Acad. 2. p. 182. 224.

‖ P. 185. note k.

§ P. 185

"rae herbae." And below *, he explains what he means by *radix* thus: "Hae gem-
"mae insident, vel radici sub terra recon-
"ditae, vel *radici supra terram in truncum*
"*ramosum assurgenti*; illae, ubi carnosae
"fuerint atque magnae, *bulbi* nomine
"veniunt, &c." And elsewhere †, "Gem-
"mam proinde concipio instar herbae in
"compendium redactae, tectae, et con-
"tractae sua inter extrema folia, ut ab
"aëris injuria conservetur; cui herbae ni-
"hil amplius deest, quam vis se exten-
"dendi, eam autem calor demum exci-
"tat." Is not this as applicable to seeds, as to gems? "Semen, (says Linnæus) ‡
"proprie, novum vegetabilis rudimen-
"tum, humore rigatum, vesica tunica-
"tum." 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 Linnæus owns, "Generationes plantarum
"ex

* P. 186.

† P. 192.

‡ Linn. Phil. bot. p. 54.

"ex ſemine et gemma eſſe coevas *; and "gemmas, proinde ac ſemina, in ſe con-"tinere primordium plantae †;" 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* ‡; and the corculum is nothing but the *novae plantae primordium* ‖. 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 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

* Phil. Bot. p. 88.

† Gem. arb. Amaen. acad. 2. p. 185.

‡ Phil bot. p. 56.

‖ Ibid. p. 54.

imagined analogy between plants and animals, can warrant or excuſe the fulſome and obſcene names, impoſed by the ſexualiſts on the different parts of the fructification of vegetables *?

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 flore et fructu in-"ſtruitur, etiam ubi viſus eaſdem non "aſſequitur," as Linnæus aſſerts †. I know he attempts to prove it thus ‡: "Muſcorum ſemina nos; lemnae flores "delineati a Valliſnerio; fucorum flores "obſervavit Reaumur; pilulariae flores "inveſtigavit B. Juſſiaeus; fungorum ſta-"mina

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

† Fund. bot. § 139.

‡ Phil. bot. p. 89.

"mina deſcripſit Michelius." And this is all, and affords but a lame proof, and moſtly conjectural.

THUS, although Dillenius *, in the end of a particular deſcription of the *ſelago foliis et facie abietis R. ſyn.* p. 106. or upright fir-moſs, ſays, "Plura non ob-"ſervavi, nec ſemina in his vel calycibus, "vel foliis bracteatis invenire potui:" Yet, becauſe he adds, "Suſpicor autem "bracteas illas folia ſeminalia eſſe, et no-"varum plantarum productioni inſervi-"re;" the learned author of the *ſemina muſcorum* † poſitively concludes, "Hae "foliola, tempore autumnali matura de-"cidunt a calyce perſiſtente, et novam "plantam propagant, radiculas e baſi ex-"ſerentia." But, ſince below ‡ he owns, that "Semina muſcorum tanquam nuda "corcula, ſine cotyledonibus, ſine tu-"nicis animo concipienda ſunt.—Flos "autem obſcure nobis percipitur, cum "nullum ſtylum, nullumque ſtigma ha-"beat,

* Hiſt. Muſc, p. 437.

† Amaen. acad. p. 2. 295.

‡ P. 299.

"beat:" 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, medi-"ante pollinis antherarum illapſu ſupra "ſtigmata nuda."

54. THE ſame learned author * aſſerts, "Quod pulvis in capitulis muſcorum ſit "pollen maſculum:" That the "ſemina "lycopodii officinarum ſunt purum pu-"tum pollen †;" 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 ‡, without the fœcundating 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

* P. 300.

† P. 293.

‡ Vid. II. ox. 3. 555.

of 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* * 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, 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

* P. 186, &c.

56. To conclude: Had the modern doctrine of the generation of plants continued only to influence the learned more accurately to inquire into the ſtructure of vegetables I would never have 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 *. It remains only that we inquire a little into the real uſe of the duſt of the apices.

* Tyrocin. Bot. 1. p. 40. 50.

SECT. IV.

57. 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 uſe to the plant which produces it. And ſince vegetables ſerve only, or at leaſt chiefly, for the uſe of animals; what the great Mr Boyle ſays of the one, may well be applied to the other *. And ſince

* "The whole animal is but a part of that greater "body the univerſe; and therefore cannot eaſily be ſup- "poſed to have been framed and furniſhed with the "parts it conſiſts of, intirely for its own ſake, And, "when we ſay, that all its parts are contrived for the "beſt advantage for the animal, I conceive it to be un- "derſtood in this limited ſenſe, that the parts are ex- "cellently framed, for the welfare of the animal as far "forth as that welfare is conſiſtent with the general "ends of the Author of nature, in the conſtitution and "government of the univerſe. All which ends it is "not an eaſy taſk to diſcover, though ſome of them "may be inveſtigable by us. And, it ſeems preſump- "tion to ſuppoſe, that the welfare of particular animals "is any further deſigned and provided for, than will "conſiſt with the coſmical ends of the univerſe, and "the

ſince we ſee bees frequently come out of flowers loaded with their duſt, 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 *, it cannot be the *materia* of wax, without being digeſted and prepared in the inſect; and, how many other inſects may live on it, I cannot tell.

58. 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 †; and Malpighius ‡, and Tournefort, with relation to ſuch as bear both on the ſame individual plant, viz. That it carries off what is excrementitious,

"the courſe of God's general providence, to which "his ſpecial or particular providence, about this or that "mere animal, ought, in reaſon, to be ſubordinated." Vid. Boyle's Works, vol. 4. p. 548.

* Vid. Mem. acad. an. 1711.

† Vid. ſupra. No. 6.

‡ Vid. p. 70.

tious, and unfit for nourifhing the feed *. I own indeed, that this opinion does not well agree with that of Caefalpinus; it being more than probable that the duft of the apices, in barren as well as fertile flowers, is of the fame nature, and defigned for the fame ufe.

59. AND, from what has been obferved of the parietaria, ficus, viola, &c. yea, and from the general ftructure of fertile as well as barren flowers, it is alfo more than probable, that this duft is rather excrementitious and noxious, than ufeful to the nourifhment or foecundity of the feed. Nor is this inconfiftent with its influence on the production of the numberlefs varieties of new flowers, which yearly adorn the gardens of the curious: If the opinion, which has for fome time prevailed,

* "Petala (fays Tournefort) alimentum a pediculo "acceptum, vifceris inftar, perficiunt, et fructui nafcenti "fuppeditant: ineptis humoris partibus per ftamina, "feu vafa excretoria, abeuntibus in apices, feu recepta-"cula. Diximus jam apices, quicquid minus apti con-"tinet alimentum in fe recipere rerumque valvas a "congeftis excrementis deduci;" Inft. p. 69. and 70.

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 *. 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.

60. For, ſince all double flowers are monſters, "Luxuriantes flores (ſays Linnaeus) nulli naturales, ſed omnes monſtra ſunt; pleni eunuchi evaſerunt, proliferi monſtroſorum augent deformationem

* Vid. Mem. acad. an. 1728.

"tionem *"; 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?

61. 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.

ART.

* Fund. Bot. § 150.

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

* *January* 3. 1738.

mical experiments; yet, as the hiſtory of chemical 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 phaenomena 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

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: 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 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, though ſome liquors are known to diſſolve ſeveral bodies which differ in many reſpects; yet even theſe ſolvents, which are allowed

to

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 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

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 beeswax 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 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 chymiſtry, to make us cautious in drawing ſuch concluſions. Well rectified ſpirit of wine makes a very compleat

ſolution

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 fixed alkaline ſalt, &c. Diſtilled 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

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 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

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 of its fluidity, and

turned

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 fileings 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 effervescence 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.

ged. 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 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 of 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 effervescence, with a conſiderable heat and thick ſteams of a penetrating ſulphureous 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 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

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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 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, expreſſed 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;

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. 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

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fully and quickly in *aqua fortis* diluted with ſix times its weight of water. The ſolution of lead in ſpirit of vinegar, being evaporated ſlo ly 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 quickſilver; and the ſolution is performed with a great commotion, heat, and thick red ſmoke: Strong oil of vitriol diſſolves ſcarce a third part of its weight of quickſilver; and the ſolution is made in a ſlow

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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 whiteness, 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 acid, is evident; because the same materials,

rials, 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 there 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,

ſolvents, when they are actually diſſolving bodies, excite a ſtrong and remarkable effervescence, with a great expanſion, hiſſing, bubbling, heat, and 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 paſſed thro' a great variety of forms; yet,

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 through all the ſucceſſive changes; ſo that it may be doubtful whether art can 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, though not ſo much as the ſame quantity of pure water would do. From ſea water, by evaporation and chryſtallization, 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 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 veſſel 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

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is diluted with an equal quantity of diſtilled water, it will continue clear and lympid, 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 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

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with an oppofite falt. But art can again produce it to view, and reftore all its former powers. If, in this procefs, five or fix ounces of *fpir. nitri*, or *aqua fortis*, were employed in the beginning, and, after the laft ftep, the liquor is diluted with fome more water, ftrained through brown paper, fully fatiated with the alcaline liquor or falt, and then flowly evaporated and chryftallized: There will be procured a falt perfectly refembling common falt-petre, in the fhape of the chryftals, explofive quality, and other marks. This factitious falt-petre then may be employed in place of the common; and I diftilled fome ounces of a falt, made much in the fame way (for I imagine the intermediate fteps will make no alteration) with oil of vitriol, in Glauber's manner, and obtained a ftrong fmoaking fpirit, which anfwers all the characters of Glauber's fpirit of nitre, and diffolves the fame bodies; and therefore may run through the fame courfe, over and over, without any change, unlefs that it may be gradually wafted, fome

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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 is indeed another manner of parting the diſſolved body from the ſolvent, which is ſometimes, though leſs properly, called precipitation; when, by drawing

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 a 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 ſubſtances, diſſolved in their proper acid menſtrua, may be precipitated

ted 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, although it obtains in ſome inſtances, yet, in many more,

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, though rendered ſpecifically lighter: Whereas, when the thinneſt and lighteſt parts of the ſolvent are carried off by evaporation, the ſilver falls down. In the ſame manner, a ſolution of quickſilver 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

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 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 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

to 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 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 zinc 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

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 zinc.

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 precipitations, differ from one another in ſeveral remarkable qualities.

WHEN

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 one 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 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

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

The

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.

ſpir. ſal. ammon. it becomes 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 pre-

cipi-

cipitated 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. XI.

Experiments on Neutral Salts, compounded of different acid Liquors, and alcaline Salts, fixed 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) regene-

* June 7. 1739.

nerated nitre, in this manner: I took two ounces of an extemporaneous fixed alcaline ſalt, made of equal parts of nitre and tartar; I 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 fixed 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 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

bled 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 fixed ſalt with ſpirit of ſea ſalt, pouring it in by degrees, till there appeared no effervescence, and the liquor taſted briny. Four ounces and a half of this ſpirit were employed to ſatiate two ounces of the fixed 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 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 alto-

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 gradually 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 ſmoaked 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 ſpi-

rit

rit 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 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 preceeding, but the whole appeared like a maſs of white ſalt coarſely powdered: However I could diſcover, among theſe chryſtals, ſome that had a quadran-

gular

gular 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 chrystals; 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, which raised a bubbling and effervescence. In distillation there came over six drams of liquor, of a brownish colour, with a little oiliness upon it: Its taste was sour, but disagreeable and empyreumatic; it had an unpleasant smell, but not very strong or pungent; it effer-

vesced

vesced with alcaline salts fixed or volatile: In all which properties it agrees with 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 *ol. 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 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

coarſe 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 limped 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-

ſal-ammoniac, until the ſalt is diſſolved and ſatiated by the acid. This neutral ſaline liquor, however, will not chryſtallize or coaleſce 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 fixed

fixed 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 a fixed 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 fixed 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

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 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

duce 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 phlegm 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 readily lay hold on the particles of certain metals or ſemi-metals, calcarious or abſorbent earths, or oppoſite ſalts, fixed, 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 fixed 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

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, quickſilver, 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 was originally produced. Conſequently there appears to be ſome affinity betwixt the native fixed baſes of theſe ſalts, and the fixed 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, though they are joined to the ſame acid ſalts.

VII. The acid ſalts, though combined with the alcaline, into mild neutral ſalts,

yet

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, though 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 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 fixed alcaline, but not contrary-ways.

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

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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

their mutual attraction. Laſtly, it is evident, from the above appearances, that though the parts of nitre which ſwim in the water are ſo ſmall, that they are not perceptible ſingly, even when viewed through 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

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 fixed and alcaline baſis, while this ſame vitriolic acid ſeems to repell another part of nitre; that is, the acid and volatile 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 fixed by deflagrating with charcoal; or if it is no other than that ſalt which can be extracted from the remains of the diſtil-

lation

lation of nitre with bole; then we have ſome reaſon to conclude, that this part of nitre is not a ſimple elementary matter: Since fixed 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 coheheſion of the 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

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

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, though ſpirit 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 ſlow-

ly,

ly, 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 charcoal, 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 a 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 fixed 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 fixed ſalt, each of which attract ſpirit of nitre more ſtrongly than ſilver does, and the laſt more than the preceeding.

HERE

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 through other degrees; which can only be aſcertained by a great variety of trials.

As the ſalt of tartar, or any ſtrong fixed 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 ſtandard 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 fixed alcaline ſalt are joined to the three different acids of nitre, ſea-ſalt, and vinegar, and, if oil of vitriol is

poured

poured upon each of thefe faline mixtures or compound falts, the vitriolic acid will attract, or be attracted more ftrongly by the alcaline falt than the other acids, which will be driven from their places; and the acid of vitriol will be fo firmly united to the fixed falt, that no other acid or known body is capable to feparate them.

AGAIN, If fpirit of nitre is poured upon a compound faline liquor, or falt made of fpirit of fea falt and falt of tartar, and upon another made of fpirit of vinegar and the fame falt of tartar; the fpirit of nitre, by its fuperior or attractive power, will join itfelf to the alcaline falt, and drive away the other acids, but is not able to refift the ftill greater force of the vitriolic acid.

LASTLY, If fpirit of fea falt is poured upon a compound of fpirit of vinegar, or any other vegetable acid and a fixed alcaline falt; this will be more ftrongly attracted by the acid of fea falt, than by the vegetable acid which will be expelled:

But

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 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 great-

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eſt 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 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,

tations, 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

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 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, though 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, 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 than it did the river-water, when there was a very little sea salt dissol-

ved

ved 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 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

was eafily 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 glafs; and, with powder of galls, it immediately ftruck a blue.

On the 13th. the fharp aluminous tafte ftill remained ftrong, but the chalybeate was fcarce to be diftinguifhed; the quantity of the precipitate, and the pellicle on the top, were much the fame; but the yellow fubftance, adhering to the bowl, was rather more: Into a glafs of this water, I dropped fyrup of violets; it turned of a faint green: To another glafs of the fame, I put powder of galls; it immediately ftruck a blue tincture, but not fo deep as on the 6th.

On October 9th, it had a more aluminous tafte than the frefh fpaw water; the chalybeate tafte was fcarce to be diftinguifhed; the precipitate yellow fubftance adhering to the bowl, and the pellicle on the furface, much the fame as on the 13th *ult.*; the water was as limpid as on the firft day; fyrup of violets changed its colour to a deeper green than before, and, with

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 a whitiſh brown colour, which had an aluminous and ſtrong chalybeate rough taſte. The whitiſh brown

colour

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 dropped ſyrup of violets; after ſtanding a little, 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,

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 dropped 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, exhibits the like phænomena (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 effervescence with *ſpiritus vitrioli*, *oleum vitrioli*, or *oleum tartari*, *p. d.* when by themſelves, or when diluted

ted 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 dropped, 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 feet 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 *ſaccharum Saturni*, it exhibits 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 entirely 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 likwiſe fixed; for neither by being expoſed to the air, nor by evaporation

poration 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 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 ſome time: 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

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 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, though calcined for a conſiderable time in a ſtrong fire.

BORAX

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 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,

* Vid. Shaw's Inquiry into Scarborough water, part 1. § 4. Art. 43.

ances, 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 though okry earth is commonly allowed to be a conſtituent principle in chalybeate waters, 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.

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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 ocry, 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 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

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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, though there is no viſible effervescence in this experiment (10.), yet the many air-bubbles generated, ſeem 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, 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 effervescence, or ſenſible change upon the

the water; but a few air-bubbles gathered at the bottom of the glaſs; nothing, however, in compariſon of what appeared when the *ol. tart. 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 may 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, *vritriol. 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, than there is in the *vitriolum Martis*; for, if you add an alkali,

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 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, though 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

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 has been diſcovered to contain ſuch a ſalt.

IN the Philoſophical Tranſactions, No. 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 Lancaſhire; but theſe waters were found by Dr Cay, who made particular inquiry into the matter, not to be mineral ſprings, but drifts made for the draining of coal-pits; and that the vitriol found in theſe waters was owing to their running over marcaſite beds, which had been expoſed for ſome time to the air. I was ſuſpicious that ſomething of this kind had communicated the vitriol to the Hartfell ſpaw; but Mr Williamſon, who diſcovered the ſpring, aſſured me, that the water, on which

which I made the preceeding experiments, had run over nothing at all, after its exit from the ſpring, but was taken up by himſelf immediately as it iſſued out of the rock. Dr James Hunter, Phyſician in Moffat, afterwards ſent me ſome of the ſame water, which he received into bottles, by applying their mouth to the orifice of the vein in the rock from which it iſſued; and from it I obtained cryſtals of *vitriolum 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 turn-

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 therefore 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 water 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 do not remember which) of *ol. tart.*; and, having mixed it by ſtirring, it immediately produced a very deep blue.

FROM this experiment, I think it appears, that a water may be very ſtrong of

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*, 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 bot-

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, though not ſo deep as at firſt; and, with *ol. tart.* the water which had not been ſhaken gave a green, though 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 principle of this water, though not much nor ſuddenly.

THIS water ſeems to belong to the claſs of the *aquae Martiatae*; for it effervesces not with acids, like the acidulæ and thermae; neither is its volatile iron principle

ſo

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; though 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 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

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

groſs matter. In the beginning of February, the ſpitting was ſo extremely fœtid and ill-taſted, 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 Barntympan, 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 ſpoonful

ticular circumſtances of the patient: Or, if the patient's body has been long coſtive, and the excrements hardened, emollient glyſters may be uſed. Such as are plethoric,

ſpoonful 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 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

thoric, or have their veſſels full, ſhould let a little blood.

THIS water may be drunk at all times of the year: Though 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

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.

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, 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.

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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; though 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 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 inconveniencies 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 guts 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.

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 pro-

ſpect of its working a cure, its uſe ſhould be perſiſted in; and, though 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 linen 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,

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 colleague, 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

* Philoſoph. Tranſact. vol. 47. p. 266. and Diſſertation on quick-lime, &c. p. 4. 5 & 6. where the ſame thing is affirmed of oiſterſhell lime,

† Diſſert. on quick-lime, p. 11. & 53.

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 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,

* Diſſert. on quick-lime, p. 11.

balance, found it weighed 25320 Troy grains *, besides the weight of the vessel itself, which amounted to 13055 grains.

(*b*) I poured upon 90 grains of calcined oister-shells, 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 through a piece of a very thick linen-cloth doubled; by which means it was rendered free of any crusts, and equally pellucid with fountain water. With this lime-water, I filled

* 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. This difference may have arisen from our having put a few more drops of water into the vessel than Mr Gray did. But although, 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.

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 oiſter-ſhells, 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 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 oiſter ſhells, 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

* 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.

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*) 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-water, one 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

as

* Eſſay on the virtues of lime water, p. 47.

a half of (*d*), to give it the full gun-powder colour. Theſe experiments, though not ſo accurate as thoſe made with the balance, yet clearly demonſtrate 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 double lime-water (*d*), which had

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

* 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.

dram of freſh calcined oiſter ſhells, reduced to a powder, 520 drams of boiling water.

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 oiſter-ſhells; 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 No. 1. afforded very near four grains, No. 2. near 12 grains, and No. 3. rather more than 13 grains.

ALTHOUGH theſe three lime-waters had, at the time they were filtrated, quite loſt their taſte; yet, obſerving that No. 2. and 3. became turbid when mixed with

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 filtered the lime-waters of No. 2. and 3. into the ſame bottle, before 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 No. 3.; in which caſe, ſixteen ounces of it muſt have contained 17 gr. of the earthy part of the lime, and No. 2. only 12 gr.

SINCE No. 2. and 3. were not quite free of the lime, although 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,

* Phyſical experiments on brutes, p. 11.

rated, the quantity of this may be determined by experiment; though, 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 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, though 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

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 its finer parts more readily, and in greater abundance.

UPON comparing the experiments No. 1. with those of No. 3. and 4. 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

* As lime-water, after its earthy part has been precipitated by an alcaline salt, continues to taste strongly of the

ſome other way unknown to us, alter the 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 univer-

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?

ſal 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, although 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, 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

ſoftened with a little ſweet milk, the iſſolution 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.

ART. XIV.

Of the anthelmintic Virtues of the Root of the Indian Pink, *being Part of a Letter from Dr* JOHN LINING *Physician at Charlestown 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

root in ſubſtance is a moderate doſe: And I repeat it morning and evening for ſome days; ordering, at the ſame 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. Though 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,

ing, that morning; ℞. *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 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

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. 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

had no reaſon to doubt that my patient's diſtemper was the venereal 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 decoction of the woods; by which means, he ſeeming to be very well, I flattered myſelf that the diſeaſe 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, though 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 ſhin-bone 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 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

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 the ſame through 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, though 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 there-

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 do not doubt, but moſt of the diſeaſes, which uſually yield to mercury, may be ſubdued by this medicine. I ſhall only add, that I now omit the *reſina guajac.* and increaſe the quantity

tity 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 ALEXANDER MONRO, *Student of Medicine in the University of Edinburgh.*

THOUGH all anatomists agree, that the seed separated in the testicle, passes through 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

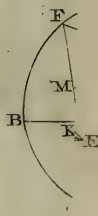
† De Vir. Organ. Tab. 1. Fig. 3. et Tab. 4. Fig. 3.

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 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

* Vol. v. Art. xx. § 29.

† Phil. Tranſ. No. 494. § xii.


F
M
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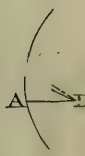
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A
D

TAB. I.

Fig. 1.

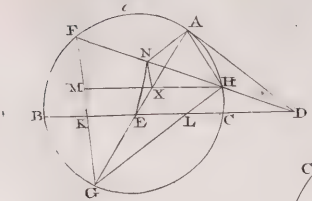


Fig. 2.

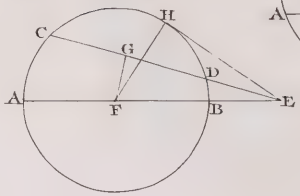


Fig. 3.

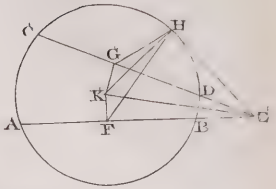


Fig. 4.

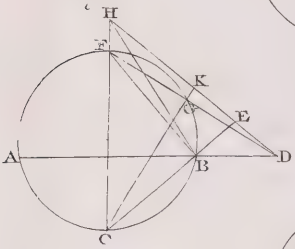


Fig. 5.

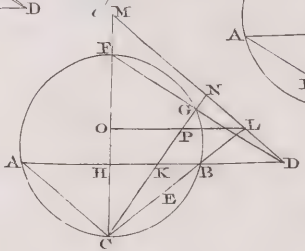


Fig. 6.

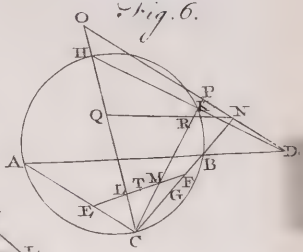


Fig. 7.

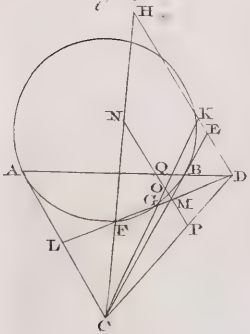


Fig. 8.

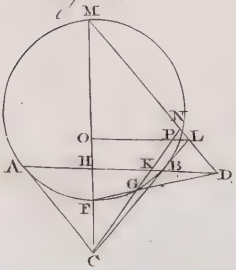
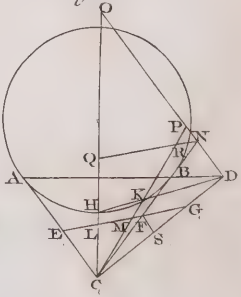


Fig. 9.



firm 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, though he ſometimes obſerved, that it entered a few convoluted 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.* 1. 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. 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 quickſilver to flow either way in it.

D. The *corpus pyramidale*.

E. The

E. The body of the epidydimis, where the convolutions ſeen through 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 convoluted 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. 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

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 in the epidydimis; and as it ſeems difficult to conceive how the ſeed can be propelled through

* Exp. anat. traitè dé bas ventre. § 488.

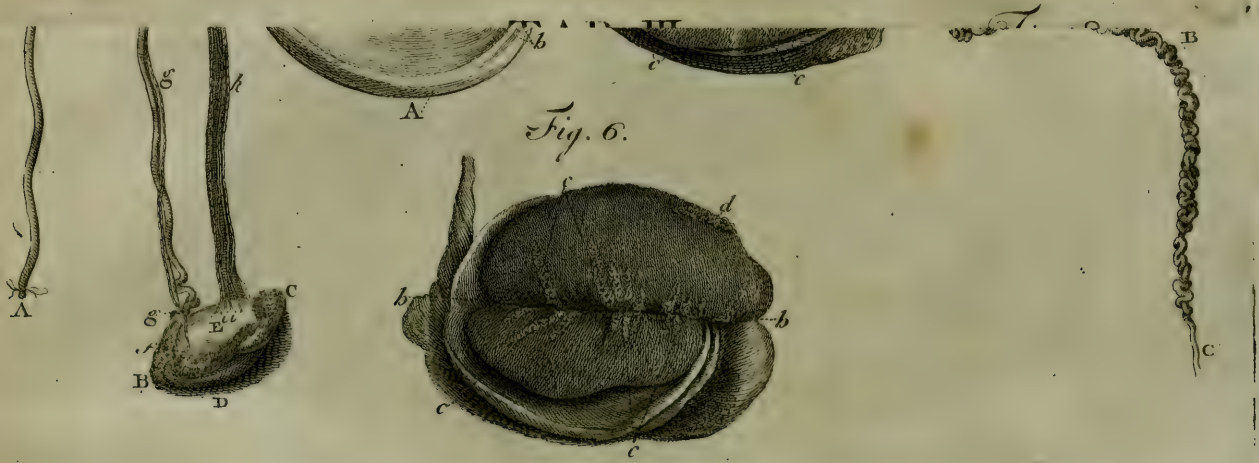

A
g
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Fig. 6.
c
d
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b
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c
B
C

TAB. III.


Fig. 7.
Fig. 1.
Fig. 8.
Fig. 9.
Fig. 2.
Fig. 3.
Fig. 10.
Fig. 4.
Fig. 5.
Fig. 11.
Fig. 12.
Fig. 6.

through 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 quick-ſilver 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.

THOUGH my injection penetrated far into the ſeminal pipes; yet ſtill we are unacquainted

acquainted with their origin. De Graaf* attempted in 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, though 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.

* De vir. org. p. 42.

ART.

ART. XVII.

The Diſſection of a Woman with Child; and Remarks on gravid Uteri, *by Dr* 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,

dines, if there were any ſuch, as Nortwyk * affirms there are, and Haller, in his *Primæ Lineæ* ‡, ſeems to alledge; though 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 de-

* Hiſt. uteri human. p. 1. § 7. and p. 2. § 83.

† § 830.

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 vermilion, into the aorta: And, after that, a coarser injection 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 through cautiouſly, a little farther forwards 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 puſhing 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

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 through 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 a continuation of the peritoneum, this adhered by a cellular membrane, through 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

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 through 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 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

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 through, the fine amnios appeared, thro' which was seen the fœtus 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, through which is seen the fœtus in its waters.

The membranes being all cut through, and the waters let out, we more plainly observed, as in Tab. v. fig. 1. the fœtus

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 fœtus, 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 fœtus 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 fœtus 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

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 tied.

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; 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. Though 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,

centa; 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; 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.

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 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 fimbriæ 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

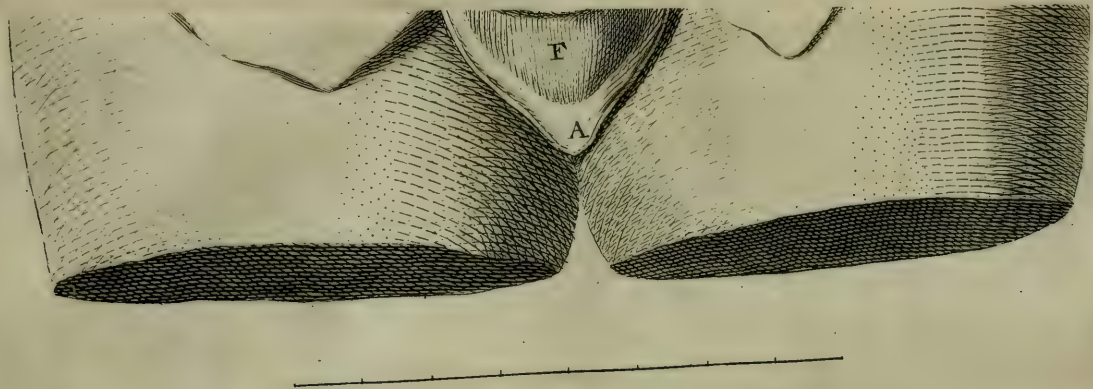
lowed to add ſome few remarks upon the whole.

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 than its top: And Veſalius *, Nortwyk † and others, have obſerved, that, the further advanced the woman is in her pregnancy, the lower they deſcend.

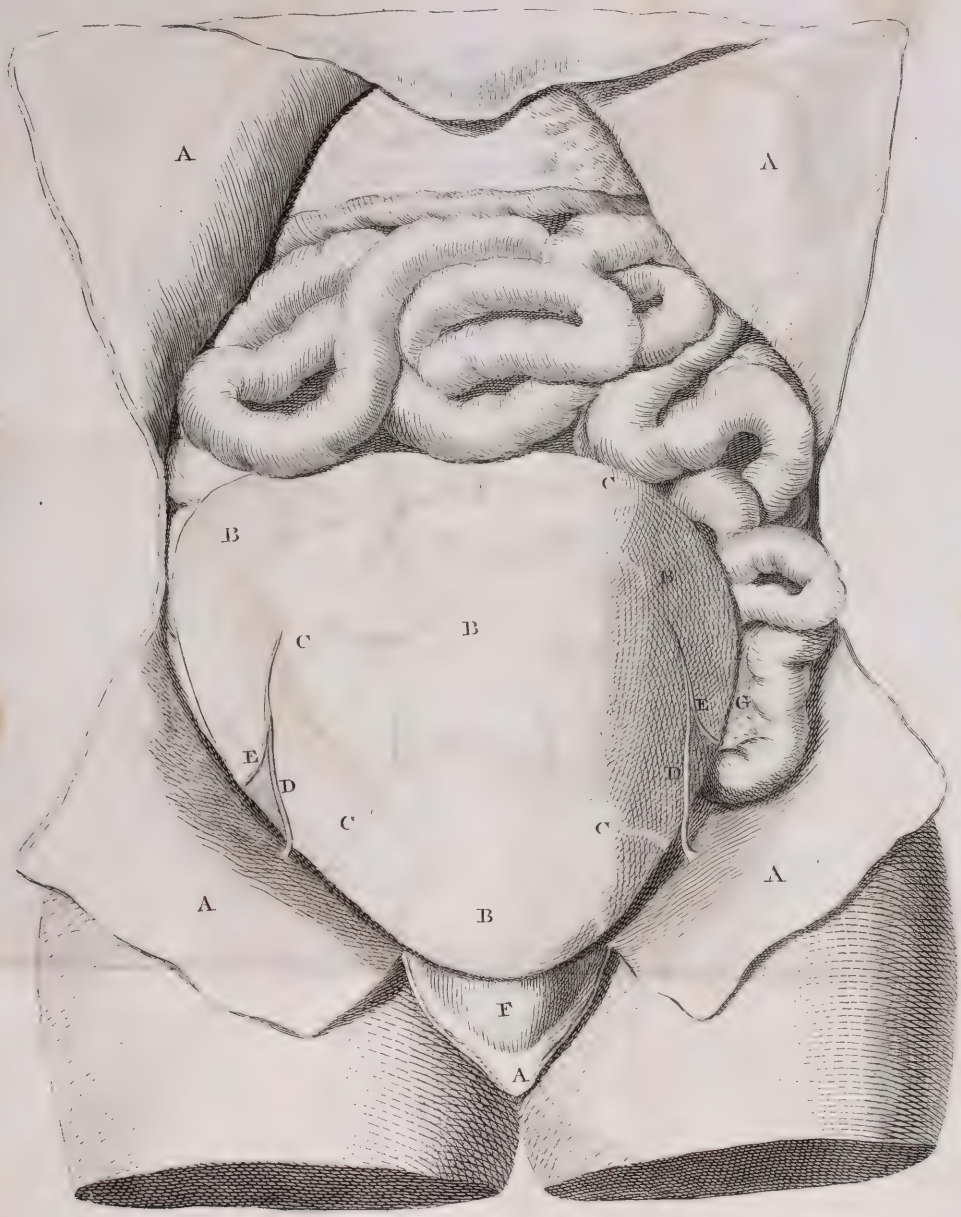
THE uterus, by impregnation, may be forced into different ſhapes. My figure and that of Bidloo, which reſemble each other greatly, ſhew, that one ſide of the womb may be raiſed much higher than the

* lib. 5. cap. 17.

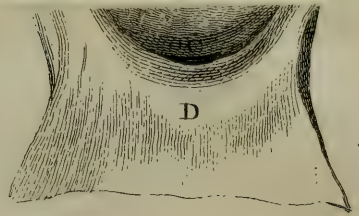
† Hiſt. Uteri, pars 2. § 77. p. 70.


F
A

TAB. IV.




B


D

TAB. V.

Fig. 1.

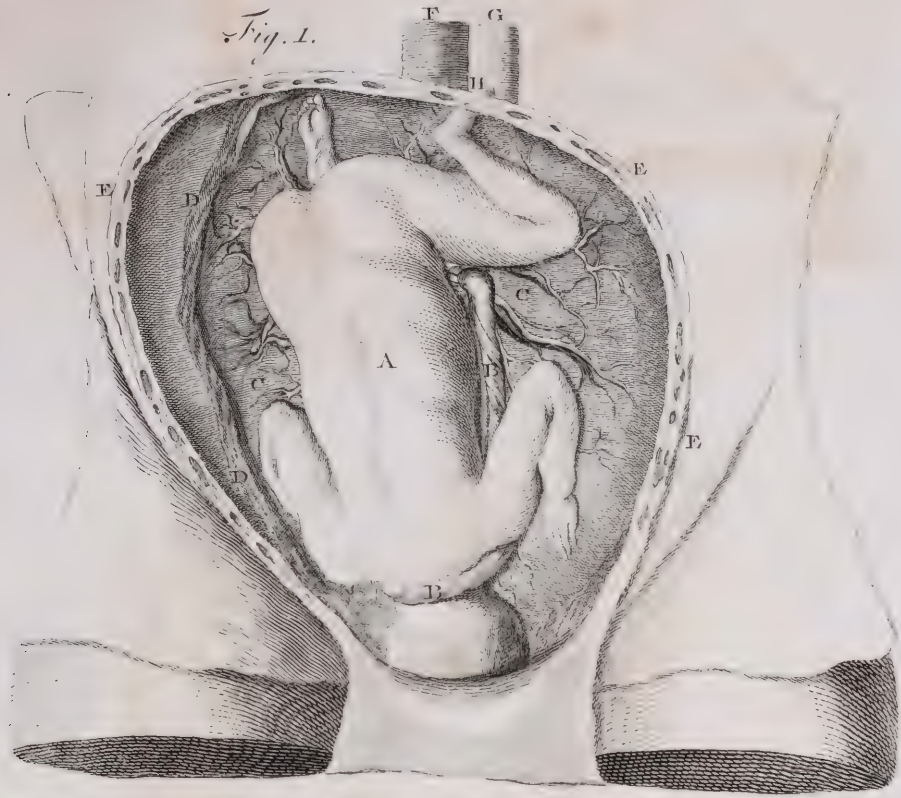


Fig. 3.

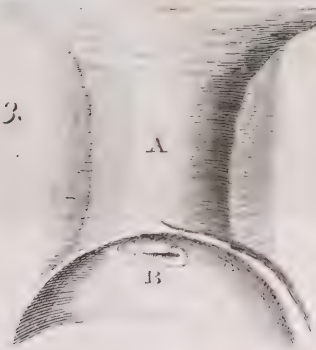
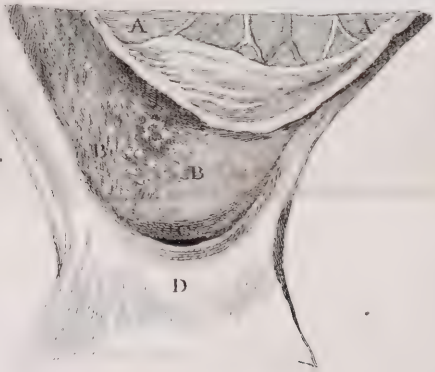


Fig. 2.

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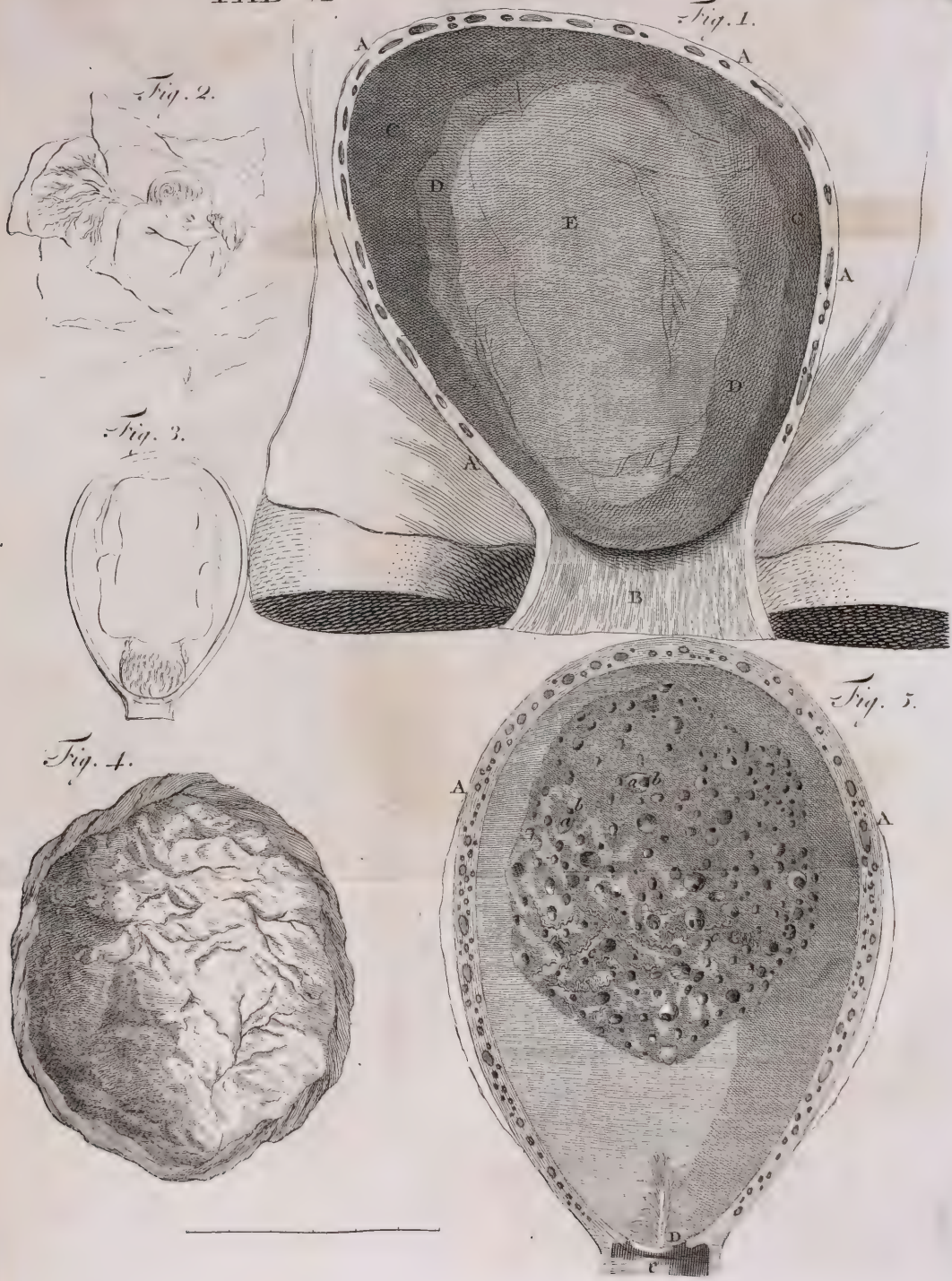
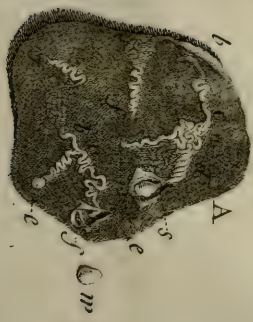
TAB VI





The strong outer membrane and cellular one immediately within it, did not

seem

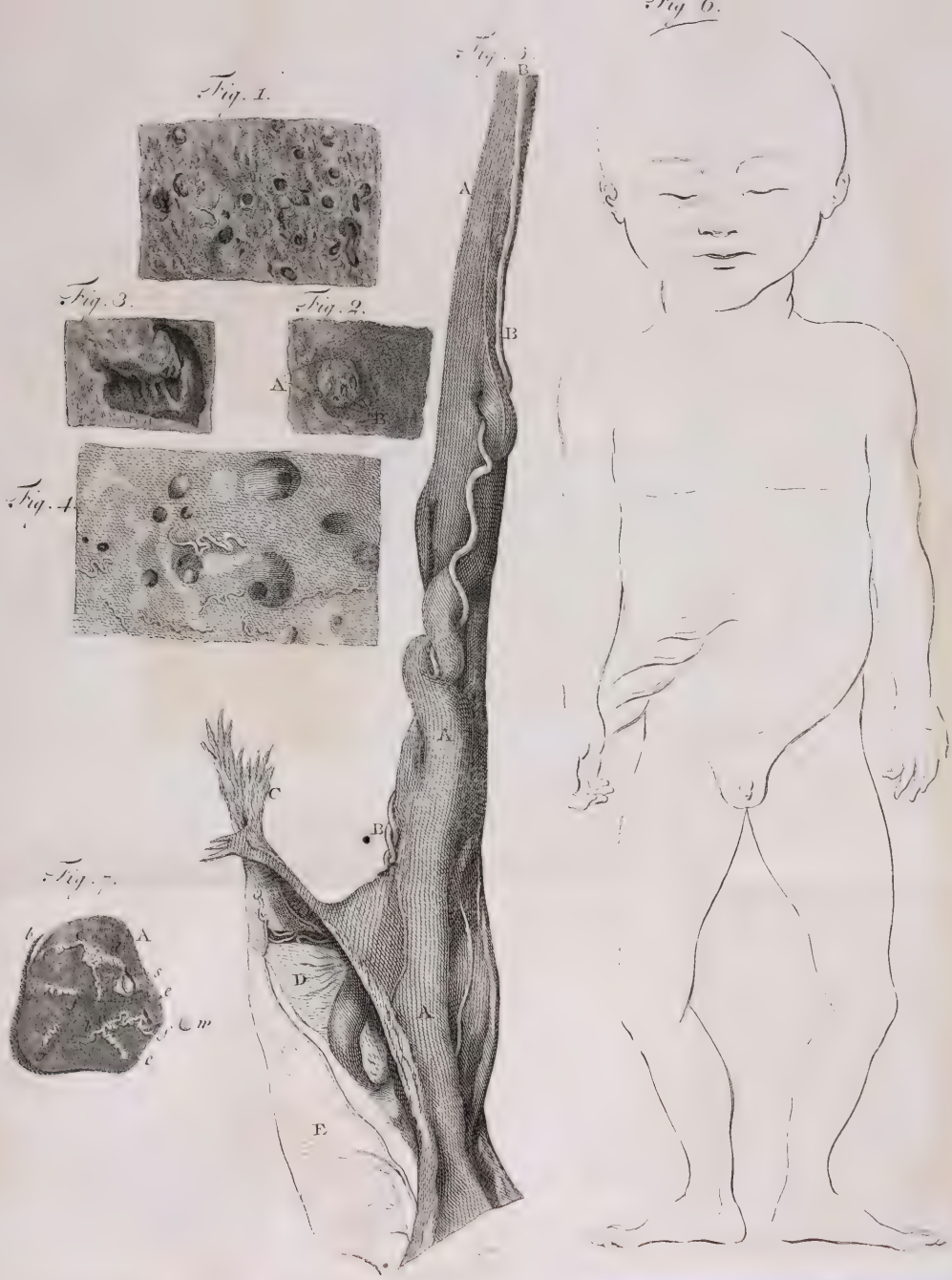


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
A
B
C
D
E
b
s
e
m
c

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 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

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, though Nortwyk § ſeems to think, that, if they are to be ſeen in any uterus, it muſt be in ſuch a one. Some few fibres there were, that went in an irregular, though 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

* 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.

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 Prælect. 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 ſentiments about this matter; and only obſerve, that Dr Smellie, Mr Hunter, Mr M'Kenzie, and others, who practiſe midwifery here, and have

* Pars 2. § 80.

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; though 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

injected 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, though 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 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

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 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 hap-

* Vol. 2. Art. 9.

† 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.

happens to be contiguous to, where it caufes a confiderable flow of liquors, by the abforbing powers of its veffels; in the fame way as the fucking of the nipple is the moft effectual way of caufing a flow of milk to the breafts. Thus the part of the human womb where the placenta is applied has its extreme veins enlarged into finufes: And the fmooth furface of the womb and its cornua in cows is raifed into glandular-like bodies where each placenta is placed.

THOUGH the contrivance of the blood returning from the foetus being poured into finufes, is a very good precaution againft haemorrhagies; yet the veins of the womb that communicate with thofe finufes being fo 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 moft internal coats of the womb are placed by way of valves over the orifices of thefe finufes; fo that whenever the womb collapfes or contracts, thefe orifices are

almoft

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 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, through 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 the diaphragm during labour-pains, and that, by being turned, its feet may act with advantage, by puſhing againſt the *fundis 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

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 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

* Arantius de foetu, cap. iii. p. 7.

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.

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, though 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 tincæ*, then ſeen from the vagina, was conſiderably larger, and the *cervix uteri* 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 vermilion, 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 men-

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 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

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.: Though 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.* to it, no remarkable 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,

air, water, or quick-ſilver, to enter it from the bladder; though 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 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

and which, in the foregoing paper, are mentioned under the name of ſinuſes: The real ſtructure and ſituation of which, being little underſtood, though 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 liquor into the veins; or they may be filled from the arteries, though 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.

ſes.——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, though 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 covered 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

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 through 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

* Prim. Lin. phyſ. § 804.

veins have been mistaken for sinuses, and their branches for canals opening 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 phænomenon from such a structure.

As the sinuses were filled by our coarse 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 deſcribed theſe openings, though, in the imaginary ſtructure of the ſinuſes, they are ſuppoſed; I have cauſed a few of the moſt diſtinct to be repreſented in Tab. 7. fig. 7. where A repreſents the inner ſurface of the womb, to which the edge of the placenta had been fixed; —*bb*, two ſmall arteries appearing after having pierced the muſcular ſubſtance; —*ccd*, their ſerpentine and ſpiral turns; —*ſſ*, their openings into the ſinuſes; which were plain beyond diſpute, when the wax was taken out of the ſinus, and the artery gently preſſed, the ſinus being again in part filled from it, as is repreſented at *ſ. w.* Nay, it is evident, that the arteries had communications with the ſinuſes alſo in my brother's ſubject; ſince the placenta was covered over with the extravaſated oil of turpentine and vermilion, which was injected into the *aorta deſcendens*;

ſcendens; and which therefore ſeems to have filled the ſinuſes, but to have been thruſt out of them by the coarſe green injection afterwards thrown into the veins. —*ee*, ſmall arteries which were obſerved every where opening upon the internal membrane of the womb;—*ff*, others which ſeem to have been torn in ſeparating the placenta.——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.

ART. XIX.

Of the difference between Respiration and the Motion of the Heart, in sleeping and waking Persons, by ROBERT 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 become less fit to receive the impressions of external objects; and when the muscles of voluntary motion are more relaxed and remain

main at reſt *. But, as reſpiration and the motion of the 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 inquire 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

* Eſſay on the Vital and other Involuntary motions of animals, Sect. xii.

† Boerhaave Inſtitut. Med. § 599.

‡ Vid. Eſſay on Vital and other Involuntary motions of animals, Sect. iii. and iv.

§ Ibid. Sect. x. p. 271. &c.

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. 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 through 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

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 the pulſe is quicker when we ſit or ſtand than when we lie; 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.

ſes. But, if no farther circumſtance, tending to retard the heart's 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, though 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 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 diminu-

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 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 con-

tractions 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 through 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 ſame time the other muſcles of this animal were ſo intirely deprived of feeling,

as

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

* Eſſay on the vital and involuntary motions, &c. p. 372.

† Kauu impet. faciens Hippocrat. dictum, No 434. 435.

ſlowly than when we are awake *. 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, through 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 through 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

* Boerhaave inſtitut. med. § 597.

† Exercit. de ſomno et vigilia, § 40.

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 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

* Eſſay on vital and involuntary motions, &c. p. 176. &c.

mulated 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 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

* Eſſay on the vital and involuntary motions, &c. p. 194.

owing partly to the ſenſe of feeling in the lungs being then ſomewhat diminiſhed, though 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 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 parallelism 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 parallelism

* Anat. raisonn. part 2. chap. 14.

rallelism of the ribs to remain the same, is a good one, to account for the defect of the internal intercostals between the spine and the angle of the ribs, and for the deficiency of the external intercostals from where the ribs begin to turn upwards to the *sternum*; a fact which anatomists are now all agreed in. The application of the demonstration to this purpose, is the intention of this paper.

LET AB (fig. 1. Tab. viii.) be the spine, CD the sternum, EFGH, and IKLM two ribs; then EK and GM are lines in the direction of the external intercostal muscles, and FI, and HL are in the direction of the internal intercostals.—Now, says Bayle, if both ribs rise, the angle FEI must be enlarged, and its hypotheneuse FI, the fibres of the internal intercostal, must be longer: Whereas muscles generally are shortened when they act.—Near to the spine, where the ribs cannot be brought nearer at one time than another, this reasoning is just; and therefore the internal intercostals are not placed nearer to the spine than where the motion of the

ribs is large enough to allow the lower rib to approach ſo much to the one above it, as to do more than compenſate for the elongation of the hypotheneuſe of the angle FEI, *i. e.* the internal intercoſtal is only to be found farther forward 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 internal intercoſtal may be ſhortened, becauſe the angle HMI. 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; amongſt 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

obſervable ſhriveling in my left leg where the *tendo Achillis* was broken: Whereas moſt of the other gentlemen have ſome of thoſe uneaſineſſes, 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

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 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

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 pye-holes are here repreſented on one ſide, either of which is to be uſed according to the thickneſs of the leg.

HAVING then wrapped ſoft flannel, ſmoaked 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, through 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 occa-

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 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

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 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.

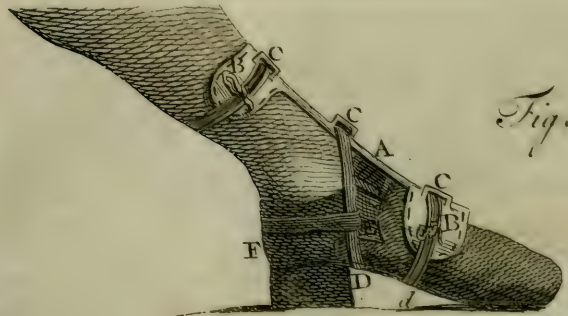
BEING

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. 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 through 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 rib-band, 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, through 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 through 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: Though, 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 in-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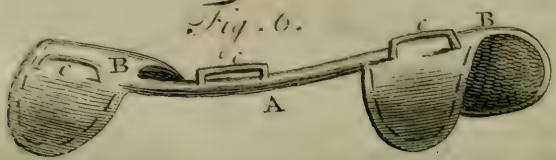
Fig. 6.
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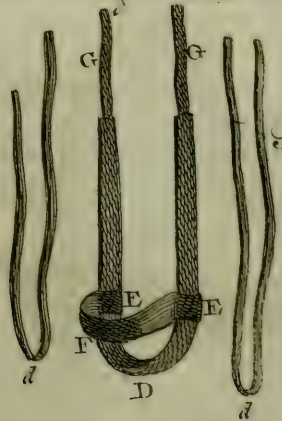
Fig. 7.
d
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G
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Fig. 8.
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TAB. VIII.

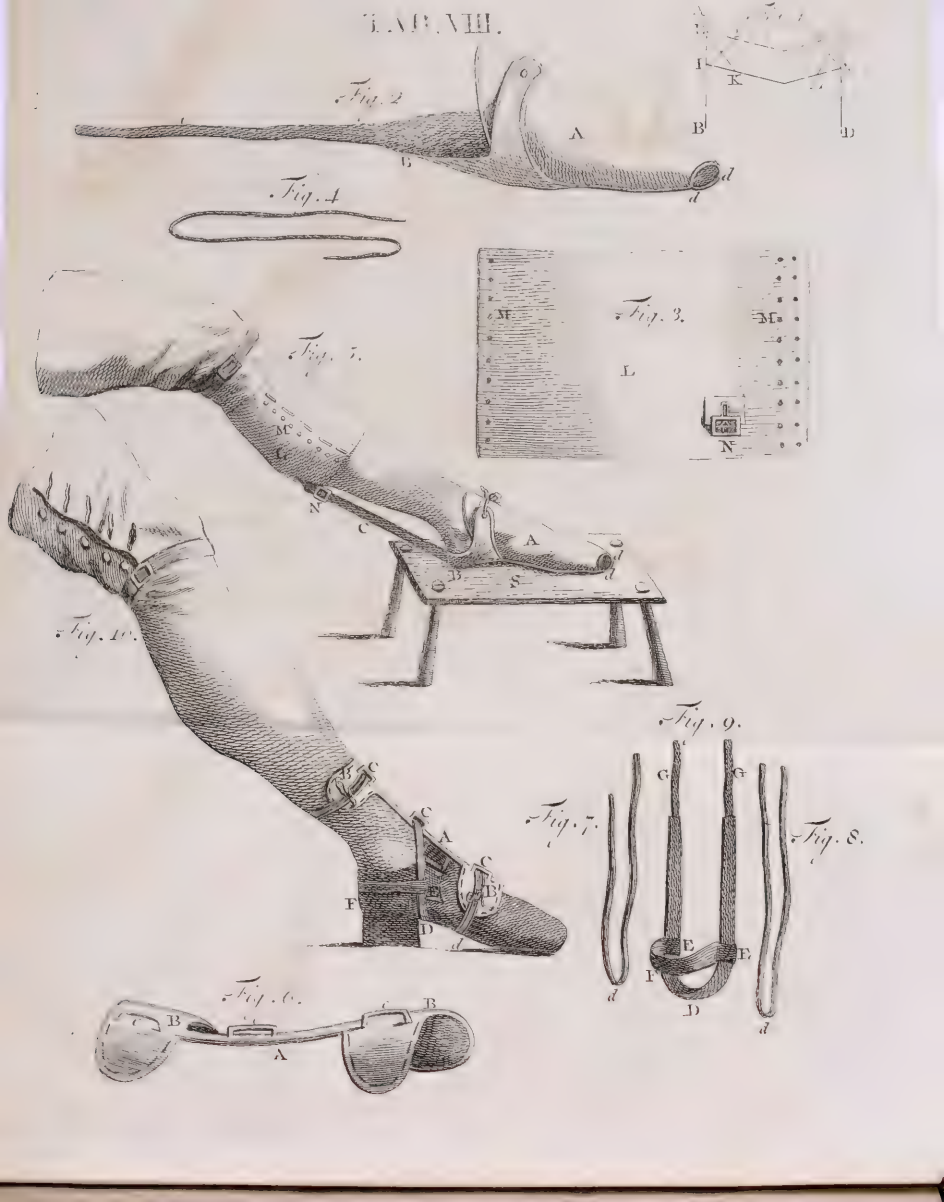
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
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Fig. 6.
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Fig. 8.
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Fig. 10.
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inſtead of it, without inconvenience.——All that time I never walked the ſtreets, 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 weak-

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: Through 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 Duriſdeer, 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

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; and a clammy ſweat frequently breaks out.——The legs become feeble with a prickling numbneſs; and there is a debility and lazineſs in all the body.——The appetite goes away, and they do not 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 ileum* 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;

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, 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 through 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

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 upon which they lie, after they cannot raiſe themſelves on their legs.

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 inflamed in ſome places, and in others a mortification was begun with holes thro' them.——His fœces were very hard; and, where they were of little quantity, the coats of the guts were thick, and the paſſage through them leſs.

IF

* The fine particles of lead which ſubſide ſlowly in water, in which lead has been waſhed.

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-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 im-

mediately 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. 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

cipally 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 ipecacuanha 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 be given ſoon.—— If it vomits, but does not purge, a cathartic of the antimonial kind, or of jallap and mercury, in greater than ordina-

ry

ry 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, lest 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 fœces, 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

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 on horſe-back ſome miles every day; and,

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.

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