ESSAYS

AND

OBSERVATIONS,

PHYSICAL AND LITERARY.

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ESSAYS

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ARTICLE I.

The Description of a new Plant; *by Dr.* ALEXANDER GARDEN, *Physician at* Charleston *in* South Carolina.

DOCTOR GARDEN writes Doctor *Whytt*, that, in Summer 1754, he met, about a mile from the town of *New York* in *New England*, with a plant; which, at first, he took to be a *hypericum*, but, on examining it, found it different: upon which he took down its characters, and sent them, some days after, to Miss *Jenny Colden* (daughter to the Honourable *Caldwallader Colden*), a very ingenious young Lady and curious Botanist. In return to this, Miss *Colden* sent Dr. *Garden* the characters of a plant which proves to be the ſame : it is N° 153. of her collection ; and was firſt found by her, Summer 1753. Uſing the privilege of a firſt diſcoverer, ſhe was pleaſed to call this new plant *Gardenia*, in compliment to Dr. *Garden*.

The Deſcription of the GARDENIA, *ſent by Dr.* Garden *to Miſs* Colden.

ANONYMOS.

POLYADELPHIA ENNEANDRIA.

RADIX perpendicularis, in deſcenſu fibras aliquot at plures cirros emittens, ſenſim attenuatur, ſimplex, mollis.

CAULIS ſimplex, fruticoſus, teres, rectus, ex alis foliorum internodiis ſeſquipollicaribus laterales emittens ramulos, lineam craſſus, cubitum plus minus altus, cavus, annuus, glaber.

FOLIA ſimplicia, ſeſſilia, patula, bina ex oppoſito alternata, integerrima, obtuſa, ovata, cordata in duo auricula, circa caulem ſupra ſe invicem expanſa, ad baſin extenduntur, nitida, ſupernè viridia, infernè glauca, nervo medio infrà

frà prominulo, punctis lucidis *hyperici* foliorum inftar perforata——dimidium pollicis plus minus latitudine, fefquipollicari fere longitudine.

PEDUNC. nunc alaris, nunc terminatrix, filiformis, femiuncialis.

CALYX perianthium pentaphyllum perfiftens, foliolis linearibus, lanceolatis, acutis.

COROLLA pentapetala pallidè rubra, bracteis lanceolatis, cum calyce fitum alternantibus et longioribus.

STAMINA. *Filàmenta* novem, leviter purpurea filiformia, in tria corpora ad bafin coalita. Fafciculi ifti filamentorum, interpofitione trium corporum *nectareis* fimilium, à fe invicem feparantur. Haecce corpora, colore funt bruneoluteo, obtufa, craffa, breviora, receptaculo feffilia, hinc modice cava, inde gibbofa. *Antherae* fubrotundae, parvae, luteae.

PISTILLUM germen trigonum; ftyli tres, parum reflexi, teretes; ftigma nullum, vel faltem nudis oculis imperceptibile.

RECEPTAC. parvum cyathiforme excavatum.

PERICARP. capfula oblonga, acuminata, obtufè triquetra, trilocularis, trivalvis,

tribus

tribus ſulcis per longitudinem decurrentibus.

SEMINA plurima, parva, obtuſè cylindrica, receptaculo proprio per funem umbilicalem breviſſimum in duobus ordinibus adhaerentia.

1. *Hypericum* inter *polyadelphia polyandria* collocat celeberrimus LINNÆUS; et optimo quidem jure: nam, in quacunque re variant inter ſe diverſae hujus ſpecies, conſtanter tamen exiſtunt filamenta numeroſa; at in hacce planta ſemper novem inveniuntur filamenta, et ſemper in tria corpora vel faſciculos concreſcunt.

2. In *hyperico* nunquam occurrunt *nectarea*; at in *anonymo* ſemper exiſtunt tria, conſpicua, et ſtamina ab invicem ſeparantia.

3. Germen et pericarpium figurâ ſubrotundâ donantur; at in *anonymo* uterque triquetra forma gaudet.

4. In *hyperico*, calyx perianth. quinquepartitum; at in *anonymo*, perianth. pentaphyllum.

Miſs

Miſs COLDEN'S *Deſcription of the ſame Plant.*

N° 153. GARDENIA.

COVER of the flower is a cup, compoſed of five lancet-ſhap'd leaves continuing.

FLOWER is five oval-ſhap'd leaves, longer than the cup, and ſpread out.

CHIVES are nine, placed in three bundles; every three are joined in one body for near half their length; their upper parts are fine threads; they are a little ſhorter than the flower-leaves. The *caps* are roundiſh.

There are three ſmall oval-ſhap'd bodies, of a bright red colour, placed on the *ſeat* of the flower alternately with the bundles of *chives*.

PISTILL. The *ſeed-bud (germen)* is of a long ovally ſhape, with three deep furrows. The *ſtiles* are three threads of the length of the chives. The *tips (ſtigma)* are plain.

COVER of the ſeed is a long oval-ſhap'd *box*, of a dark red colour, with three deep furrows along it, and opening at

three

three parts at top. Before the ſeeds are ripe, it contains three cells; but when the ſeeds are ripe, the diviſions ſeparate at the axis, and it contains but one cell.

SEAT of the ſeed is three ridges (being the inſide of the three furrows of the ſaid box) each with two rows of ſeeds.

SEEDS are numerous, ſmall, and oval-ſhap'd.

ROOT is fibrous; but, tho' fibrous, the moſt of them are about as thick as the ſtalk; they are white and branched; ſome of them are like fine threads.

STALK grows ſingle, is round and ſmooth, and is branched out oppoſitely from the arm-pits of the leaves.

LEAVES ſtand thinly in pairs oppoſitely on the ſtalk and branches, they have no leaf-ſtalks, are oval-ſhap'd, about half as long as they are broad, broadeſt in the middle, are ſmooth, have a rib along the middle, with ſmall tranſparent veins extending from it towards the edges, and the edges are intire.

The flowers are of a pale red colour; they ſtand in cluſters on the top of the ſtalk and

and *branches*, and ſometimes in pairs at the arm-pits of the *leaves*.

This plant grows in wet ground, and flowers in *Auguſt*.

OBSERVATION. The three chives *only* in each bundle, and the three oval-ſhap'd bodies on the ſeat of the flower, together with the ſeat to which the ſeeds adhere, diſtinguiſh this plant from the *hypericums*; and, I think, not only make it a different *genus*, but likeways makes an order which LINNÆUS has not.

ART. II.

A Deſcription of the Matrix *or Ovary of the* Buccinum Ampullatum *; *by* ROBERT WHYTT M. D. F. R. S. *Fellow of the Royal College of Phyſicians, and Profeſſor of Medicine in the Univerſity of* Edinburgh.

NATURALISTS inform us that the *buccina* and *purpuræ*, the former towards the cloſe of the Winter and the latter in the Spring, throw out a viſcid glutinous humor; which thickening and becoming dry, forms a congeries of cells or receptacles connected together, ſomething reſembling the cods of white vetches, and containing the naſcent *buccina* or *purpuræ*. This congeries of cells or pods is often found upon the ſhore, and ſometimes miſtaken for the hardened froth of the ſea.

BUT whatever may be the manner in which theſe cells are formed, the *matrix*, ovary

* The *buccinum ampullatum* is that in which the hermit-crab is found.

ovary or receptacle, in which the nafcent *buccina ampullata* are found, has fomething remarkably curious in its ftructure.

Tab. i. *fig.* 1. reprefents the *matrix*, as it is called, of the *buccinum ampullatum*, fent me from South-*Carolina* by Dr. *Garden*; the length of which was full two foot.

a a a a a, &c. are the cells, receptacles or pods, compofing this *matrix*, whofe number amounted to 94. Thefe pods, towards the extremity D, turned fmaller.

ABCD, a band or ligament connecting the feveral cells or pods, of much the fame kind of fubftance with the fides of the cells, but thicker and tougher.

The pods or cells are compofed of a thin, tough, tranfparent fkin or membrane, and contain in their cavities a confiderable number of fmall *buccina*.

Fig. 2. reprefents one of thefe cells intire, thro' the tranfparent membrane of which may be feen the *buccina* it contains.

Fig. 3. fhews one of thefe cells or pods opened, that its *buccina* may be diftinctly feen.

Fig. 4. exhibits ſeveral different views of the ſmall *buccina*, all of their natural ſize.

In ſome of the pods or cells, I found 32, in others, only 28 *buccina*: but, as towards the ſmall end of the *matrix* the pods were leſs, and had fewer *buccina* in them, we may ſuppoſe that each pod might contain 25 *buccina*; in which caſe, the number in the whole *matrix* muſt have been 2350.

Art.

ART.

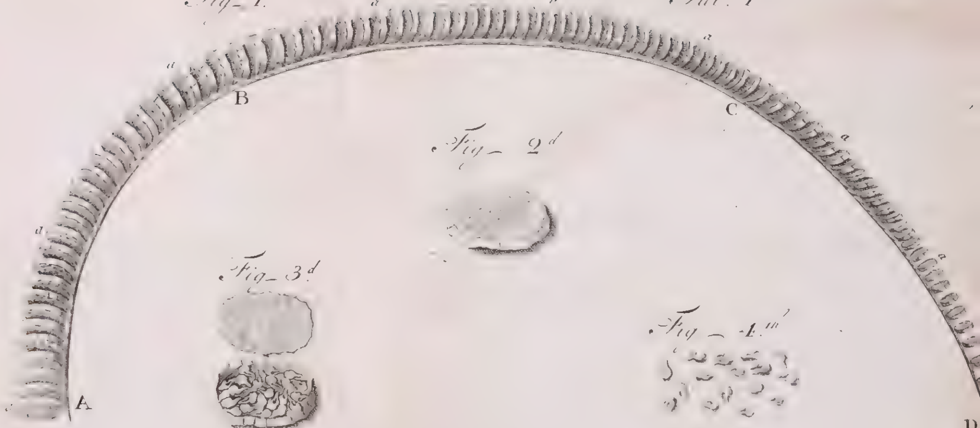
Fig. 1st
a
a
a
a
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a
a
B
C
A
D
Fig. 2d
Fig. 3d
Fig. 4th

ART. III.

Drawings of ſome very large Bones; *by* GEORGE CLERK Eſq;

TAB. II. *fig.* 1. repreſents the *ſcapula* or ſhoulder-blade of an animal, found in a ſhell marl-pit near the town of *Drumfries*, of a monſtrous ſize, meaſuring in length twenty one inches and eight tenths.

Fig. 2. is a rib twenty three inches long, and two and two tenths broad.

Fig. 3. and 4. are alſo pieces of broken ribs, found in the ſame place; where the whole or greateſt part of the bones of the animal were afterwards diſcovered, but loſt or deſtroyed by the country-people who found them.

As the bones deſcribed are at leaſt one third larger than thoſe of our biggeſt oxen or horſes, they are ſuppoſed to be the remains of an *Elk* or ſome other animal not at preſent an inhabitant of this country.

THEY are juſt now in the poſſeſſion of *George Clerk*.

ART. IV.

Observations on Light and Colours; *by* THOMAS MELVILL, M. A. *

SECT. I.

On the Mutual Penetration of Light.

1. ONE of the firſt and greateſt difficulties that occurs in reflecting on this ſubject, is, to conceive how it is poſſible that light

* Read *January* 3. *and February* 7. 1752.

Had the ingenious Author of this paper (who died *December* 1753, at the age of 27) lived to put the finiſhing hand to it, he would, probably, have added many things, and perhaps retrenched ſome others, by which it would have been rendered ſtill more deſerving of the approbation of the public. Mr. *Melvill* uſed to obſerve, that as, of all Sir *Iſaac Newton*'s diſcoveries, thoſe relating to light and colours were perhaps the moſt curious; it was ſomewhat remarkable, that few, if any, of his followers had gone one ſtep beyond him on theſe ſubjects, or attempted to compleat what *he* had left unfiniſhed. Our Author, therefore, propoſed to have applied himſelf particularly to the further illuſtration of the theory of light and colours. The following eſſay is a ſpecimen of what might have been expected from him, and ſufficiently ſhews the uncommon genius of its Author.

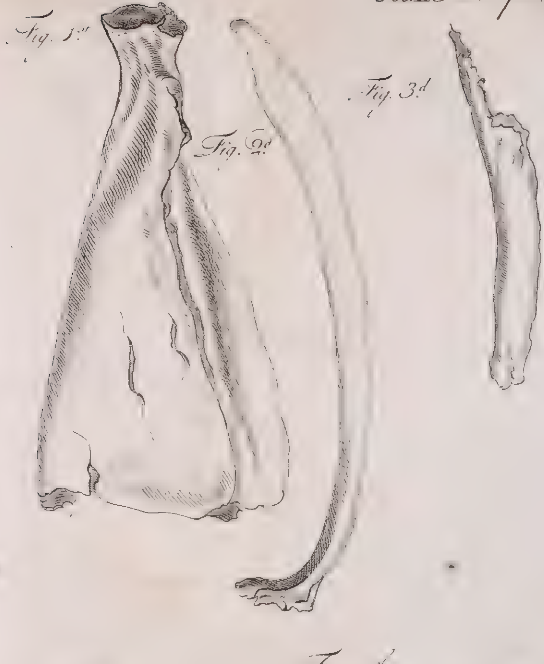


Fig. 1st
Fig. 2d
Fig. 3d
Fig. 4th

light can move thro' light in all imaginable directions, without occasioning the least perceivable confusion or deviation from its rectilinear course. Many have been induced, from this consideration, to believe it incorporeal; and all who have thoroughly weighed the difficulty, have seen the necessity of ascribing a subtility to it incomparably greater than we are led, by any *phænomena*, to ascribe to any other species of bodies in Nature. There is no physical point in the visible horizon which does not send rays to every other point; no star in the heaven which does not send rays to every other star: the whole horizon is filled with a sphere of rays from every point in it; and the whole visible universe, with a sphere of rays from every star. In short, for any thing we know, there are rays of light joining every two physical points in the universe, and that in contrary directions; except where opaque bodies interveen.

2. Those who suppose that light is nothing else than vibrations or pulses propagated thro' a subtile elastic *medium* from the visible object to the eye, may perhaps remove the difficulty by ascribing a sufficient minute-

ness

nefs to the particles of that *medium*; fince we fee, by experience, that found in the air, and waves in the water, are conveyed in different directions, without fenfibly interfering: but, as that hypothefis feems infupportable on other accounts *, we muft endeavour to accommodate our folution to the only other conception we can frame of it; namely, that of particles actually projected from the luminous body.

3. It is manifeft, that, tho' the mere fubtility of the particles of light may tend to account for its eafy paffage, in all directions, thro' denfe tranfparent bodies, it will not ferve to explain its eafy paffage thro' other light equally fubtile: but, for this purpofe, it feems neceffary to fuppofe light incomparably rare when at the denfeft; that is, that the femi-diameters of two of the neareft particles in the fame or in different rays, foon after their emiffion, are incomparably lefs than their diftance.

4. Let us confider a little the courfe of a particle of light from any of the remoter fixed

* *Newtoni Principia*, book 2. prop. 41. and 42. See alfo *Newton*'s Optics, query 28.

ed ſtars to the human eye; for inſtance, from the ſmall one called the *Rider* in the tail of the *Great Bear*: The particles by which we ſee that ſtar, have, in the firſt place, paſſed thro' the ſpace ſurrounding it, in which there are probably ſeveral planets revolving, and which muſt be therefore ſo filled with a ſphere of rays from each of them that they may be viſible to an eye any where ſituated in thoſe ſpaces; after that, they have paſſed laterally thro' the whole torrent of light flowing from the ſtar of the ſecond magnitude which we ſee beſide it; and laſtly, they have paſſed likeways acroſs the whole ocean of the ſolar light, and all that light with which the ſpace ſurrounding the ſun is filled from all the comets, planets, and ſatellites; and beſides, in every phyſical point of their numerous journey from the *Rider* to our eye, they have paſſed thro' rays of light flowing in all directions from every fixed ſtar in the viſible univerſe: and yet, during the whole, they have never juſtled againſt one particle of light; otherways they could not have arrived in their true direction to our eye. This reflection cannot fail to ſuggeſt a general notion of the rarity and tenuity

nuity of light, far ſurpaſſing all the ſuppoſitions which are uſually made about it.

5. The chance which any one body has to juſtle with others of like magnitude, is leſſened in proportion to the bulk of the bodies with reſpect to the ſpace in which they move. It muſt be therefore ſuppoſed, as we mentioned above, that the diſtance of the neareſt particles, flowing in the ſame and in different lines, muſt exceed their diameter, not indeed infinitely, but a number of times utterly incomparable with all our ordinary numbers, in order that a particle may eſcape in one phyſical point of its progreſs: but, that it may paſs freely on thro' the whole diſtance of the remoteſt fixed ſtars, it is evident, that this proportion of exceſs muſt be multiplied by a number again incomparable. But this exceſs, ſo increaſed, muſt be raiſed to a power whoſe exponent is a number equal to the number of all the fixed ſtars, planets, and comets. And laſtly, if there is an elaſtic *medium* diffuſed thro' the mundane ſpace, as the propagation of heat * and many other *phænomena* ſeem to inſinuate; this laſt number muſt be at leaſt doubled, if we would

* Newt. Opt. queries. *ad fin.*

would exprefs the proportion in which the diftance of the neareft rays exceed the diameters of their particles: and yet this diftance of the neareft rays, flowing from the fame center, is fo incomparably below our fmalleft meafures, that there is no poffibility of defining it.

6. HAD *Euler* confidered this extreme rarity, as well as tenuity of light, which muft be acknowledged by all who fuppofe that its particles are actually projected from the lucid body, he would not have alledged, that this opinion is inconfiftent with the freedom and perpetuity of the celeftial motions*.

7. SOME have thought, that, if the particles of light repel one another, their mutual perturbation may be prevented: but the contrary is manifeft upon the leaft reflection; for tho', by that means, the particles might be prevented from ftriking, they muft inftantly turn one another from their rectilinear courfes, as foon as they come, in different directions, within the reach of their mutual powers. Thus, we find by experience, it is impoffible to make one ftream of air penetrate another without confufion; for the two ftreams either unite into a common one with

* See his Nova theoria lucis et colorum.

an intermediate direction, or produce irregular eddies.

8. Here, by the bye, we may see that the ingenious system of *Boscovich*, the *Roman* Professor, concerning the elements of matter *, whatever may be said for it from other considerations, gives us no assistance in comprehending the mutual penetration of light; for indivisible points, endued with an insuperable repulsive power, reaching to a finite distance, are as subject to interfere, as solid particles of a finite magnitude.

SECT. II.

On the Heating of Bodies by Light.

9. It appears, by Sir *Isaac Newton*'s experiments on the inflexion of light, that bodies act upon it at some distance; and that the same power, variously exercised in various circumstances, is the cause, likeways, of refraction and reflexion. We know no instance of any kind of attraction or repulsion in Nature which is not mutual; we observe likeways that bodies are heated by the influence of the sun's rays: it is therefore natural

* See his Dissert. de lumine et de viribus vivis.

tural to look upon this as the effect of the reaction of light upon bodies, and that, at a distance from them; for, there is no reason to think that light produces heat by actually striking the solid parts of bodies, after we are satisfied that bodies produce the reflexion and refraction of light, without suffering it to come into contact with them.

10. From these principles it follows, that light, in passing out of one *medium* into another of different density, must always produce some degree of heat; because it is partly refracted and reflected at the common surface: secondly, that, in passing forwards thro' the same homogeneous or perfectly transparent *medium*, it can produce no heat; because there is no reflexion or refraction, no influence of the body upon the light, but every ray pursues its own right-lined course, as if it moved in a perfect void *.

11. Hence it appears, that, in water, glass, and other transparent *mediums*, which are

* Sir *Isaac Newton*, in the third book of his *Principia*, where he disputes concerning the tails of comets, lays it down as an obvious principle, *Quod radii solis non agitant media quæ permanant, nisi in reflexione et refractione.*

are warmed by the ſun's rays, the heat muſt be propagated from their ſurfaces towards their central parts *.

12. HENCE likeways we underſtand why opaque bodies are ſooner heated by the ſun-beams than tranſparent ones; ſince, there are innumerable reflexions and refractions within their ſubſtances, beſides what happen in common with tranſparent bodies at their ſuperficial parts. As each colorific particle of an opaque body, by the reaction of the particles of light, muſt be ſomewhat moved when the light is reflected backward and forward between the ſame particles, it is manifeſt that they likeways muſt be driven backward and forward with a vibratory motion; and the time of a vibration will be equal to that which light takes in moving thro' a particle, or from one particle of a body to another adjoining. This diſtance in moſt ſolid opaque bodies cannot be ſuppoſed greater than $\frac{1}{12500}$th of an inch, which ſpace

* I have found, by repeated trials, that the heat of water in deep lakes decreaſes regularly from the ſurface downwards.

ſpace a particle of light deſcribes in $\frac{1}{125000000000000}$th of a ſecond. With ſo rapid a motion therefore may the internal part of bodies be agitated by the influence of light, as to perform 125,000,000,000,000 vibrations or more in a ſecond of time! The arrival of different particles of light at the ſurface of the ſame colorific particle in the ſame or different rays, may diſturb the regularity of their vibrations, but will evidently increaſe their frequency, or raiſe ſtill minuter vibrations among the parts which compoſe theſe particles; by which means the inteſtine motion becomes more ſubtile and thoroughly diffuſed. If the quantity of light admitted into the body be increaſed, the vibrations of the particles muſt likeways increaſe in magnitude and velocity; till, at laſt, they may be ſo violent as to make all the component particles daſh one another to pieces by their mutual colliſions: in which caſe, the colour and texture of the body muſt be deſtroyed. Thus may we form, from known principles, ſome imperfect conception of the manner in which bodies are heated and burned by the action of light: More than an imperfect notion of theſe ſecret operations of Nature is not to be expected; for they certainly depend, in great meaſure, upon laws and principles utterly unknown to us.

13. If one beam or ray of light, by paſſing ſtraight onwards thro' the ſame pellucid ſubſtance, can communicate no heat to its internal parts; neither will the greateſt quantity of rays, tho' crowded into the narroweſt ſpace, by croſſing one another. From hence it follows, that the portion of air which lies in the *focus* of the moſt potent *ſpeculum* is not at all affected by the paſſage of light thro' it, but continues of the ſame temperature with the ambient air; altho' any opaque body, or even any tranſparent body denſer than air, when put in the ſame place, would be intenſely heated in an inſtant.

14. This conſequence, evidently flowing from the plaineſt and moſt certain principles, ſeems not to have been rightly underſtood by many philoſophers*: for which reaſon,

I

* See *Boerhaave* element. chem. tom. 1. on fire, coroll. 5. after exper. 14. and coroll. 1. and 7. after exper. 17. See alſo *Rutherforth*'s ſyſtem of natural philoſophy, prop. 366.

I thought it might be worth while to ſay ſomething in explication of it. The eaſieſt way to be ſatisfied of the matter experimentally, is, to hold a hair or down immediately above the *focus* of a *lens* or *ſpeculum*, or, to blow a ſtream of ſmoke from a pipe horizontally over it; for, if the air in the *focus* were hotter than the ſurrounding fluid, it would continually aſcend upon account of its rarefaction, and thereby ſenſibly agitate theſe ſlender bodies. Or a *lens* may be ſo placed as to form its *focus* within a body of water or ſome other tranſparent ſubſtance, the heat of which can be examined from time to time with a thermometer: but care muſt be taken in this experiment to hold the *lens* as near as poſſible to the tranſparent body; leſt the rays, by falling cloſſer than ordinary on its ſurface, ſhould warm it more than the common ſun-beams.

15. It is well known that the rays of light, by paſſing obliquely thro' our atmoſphere, [illegible] are

366. of the aſtronomical part; and *Nolet leçons de phyſique, tom.* 4. The ſilence of moſt phyſical writers, concerning this paradoxical truth, makes it probable that they were unacquainted with it.

are inflected into a curve by the continued infraction arising from the continual increase of its density; therefore they must produce some degree of heat in every part of their progress thro' it [N° 10.]. But, as the whole successive refraction is just equal to the single refraction that would be made in passing at once from the celestial spaces into a *medium* as dense as the lowest part of our atmosphere *, and all the successive reflexions that can be made from every different *stratum*, are but equal to what would be made at once from the surface of a *medium* of the same density; it easily appears, by comparing the densities of air and water, and their respective signs of refraction, that all the refraction and reflexion which the whole depth of our atmosphere produces is much less than what happens at one surface of water; and consequently, the heat produced in our atmosphere, by the immediate action of light upon it, must likeways be much less than what is raised in water. The air seems to have the greatest part of its heat communicated to it from the opaque vapours which

* *Newt.* Opt. book 2. part 2. prop. 10.

which float in it and the general ſurface of ſea and land to which it is contiguous.

SECT. III.

On the ſilver-like Appearance of Drops of Water on the Leaves of Colewort.

16. IT is common to admire the volubility and luſtre of drops of rain that lie on the leaves of *colewort* and ſome other vegetables; but no philoſopher, as far as I know, has put himſelf to the trouble of explaining this curious *phænomenon*. Upon inſpecting them narrowly, I find, that the luſtre of the drop ariſes from a copious reflexion of light from the flattened part of its ſurface contiguous to the plant: I obſerve further, that, when the drop rolls along a part which has been wetted, it immediately loſes all its luſtre; the green plant being then ſeen clearly thro' it: whereas, in the other caſe, it is hardly to be diſcerned.

17. FROM theſe two obſervations laid together, we may certainly conclude, That the drop does not really touch the plant when it has the mercurial appearance, but hangs

in the air at ſome diſtance from it, by the force of a repulſive power; for, there could not be any copious reflexion of white light from its under ſurface, unleſs there were a real interval between it and the ſurface of the plant *. [*See* TAB. iii. *Fig.* 2.]

18. IF

* *Newt.* Optics, query 29.

Let AB, TAB. iii. *Fig.* 4. repreſent the extremity of any repulſive body immerſed in water, for inſtance a ſlice of *colewort* leaf, CL and DM, the convex ſurfaces of water immediately ſurrounding it, and CD perpendicular to AB, the common tangent of theſe curves, which will be the continuation of the general ſurface of the water. The forces with which any two particles, E and F, are preſſed by the water in the directions EG, FH perpendicular to KB, are known to be as KG and KH, and the repulſive powers which balance them muſt be in the ſame proportion. If therefore the relation between the ordinate and abſciſs in the curve DM could be any way found by experiment, the law of the repulſive power might be determined, upon ſuppoſition that the particles are influenced by no force but the repulſive power of the line KB and the gravity of the ſuperincumbent fluid; but their mutual attraction, which tends to leſſen their lateral tendency, muſt be likeways taken into the account in order to an exact determination.

Before I leave this ſubject of attraction and repulſion, I beg leave to propoſe to the Society, the ſpontaneous motions of light bodies on the ſurfaces of fluids, as a thing worthy of being inquired into; for, tho' it be manifeſt in general that they depend upon the different figures of the ſurface, it is far from being an eaſy matter to explain the particular caſes by mechanical or hydroſtatical laws. The following account

of

18. If that ſurface were perfectly ſmooth, the under ſurface of the drop would be ſo likeways; and would therefore ſhow an image of the illuminating body by reflexion, like

of the *phænomena* may be uſeful towards ſuch an enquiry. *Caſe* 1. Suppoſe a fluid which is attracted to the ſide of its containing veſſel, and conſequently is elevated, at the ſides, into a concave ſurface: If a body be immerſed which attracts the fluid, and is therefore ſurrounded likeways with a concave elevation of the fluid; as ſoon as the two elevations begin to join, the body will move towards the ſide of the veſſel with an accelerated motion. *Caſe* 2. Suppoſe a fluid which is formed into a convex ſurface, either by the repulſive power of the containing veſſel or coheſive force of its own particles: If a light body be immerſed which attracts the fluid; as ſoon as its ſurrounding elevation begins to join with the lateral depreſſion of the fluid, it will begin to move towards the middle of the veſſel; and, if it be brought by force towards the ſide, it will recede from it again with an accelerated motion.

In both the firſt and ſecond *caſes*, if the attracting body be held faſt, and the whole fluid made eaſily moveable with its containing veſſel, it will remove to or from the attracting body in the ſame manner as the attracting body did with reſpect to it; *i. e.* in the firſt caſe, the whole fluid will move ſo that the attracting body may come to its edge; and in the ſecond, ſo that it may recede from it. *Caſe* 3. If, in a fluid which is attracted by the ſides of its veſſel, a body be immerſed which repels the fluid, and is therefore ſurrounded with a ditch or convex depreſſion of the fluid; as ſoon as that depreſſion begins to join the elevation of the fluid at the ſides, it will recede towards the middle; and, if forcibly like a piece of polished silver: but, as it is considerably rough and unequal, the under surface becomes rough likeways; and so, by [illegible] reflecting

forcibly brought to the side of the vessel, will fly from it with an accelerated motion. *Case* 4. If, in a fluid which is formed into a convex surface at the sides, a repelling body be immersed; as soon as its depression begins to unite with the lateral depression of the fluid, it will move towards the side with an accelerated motion. In these two last cases, the same observation holds as with respect to the first and second, *viz.* that the whole fluid will move with correspondent motions by the force of reaction, if the repelling body be held fast. *Case* 5. If two bodies be immersed in a fluid, which each of them attracts; as soon as their elevations begin to join, they will rush towards one another with equal forces and accelerated motions, and continue to adhere together. *Case* 6. If two bodies be immersed in a fluid which they repel; as soon as the two depressions that surround them begin to interfere, they will likeways rush together with an accelerated motion. *Case* 7. If two bodies be immersed in a fluid, the one of which attracts and the other repels it; as soon as the depression surrounding the one begins to join with the elevation of the other, they will mutually fly from each other. *Lastly*, If a body be immersed in a fluid which it attracts in one part and repels in another, it will approach to or recede from other bodies and parts of the fluid, differently according to its situation, by the rules above laid down.

The different figures ascribed to the surface of the water in these several cases are plainly discernible by the sight: if the experiments are made with candle-light, they are distinguished by the shadowy or luminous rings which they

project

reflecting the light copiously in different directions, assumes the resplendent white colour of unpolished silver.

19. AFTER it is thus proved by an optical argument that the drop is really not in contact with the plant which supports it, we easily conceive whence its wonderful volubility

project on the bottom of the vessel, according as they are convex or concave.

Some writers have been so inattentive as to ascribe the motions in the first case to an immediate attraction between the swimming body and the side of the vessel. *See Helsham's Lectures.* Before I had observed the fourth and sixth cases, I thought the *phænomena* might be all explained from this principle, that the light bodies always tend to the highest parts of the water. It has been suggested to me by some, that this tendency, combined with the greater or lesser immersion of the bodies, upon account of the ring of water which they elevate or depress, may produce all the different cases: and by others, that the whole is explicable from the single principle of attraction between the parts of water which causes two drops to run into one. I believe it will be found, on due consideration, that none of these accounts is satisfactory: but there is no reason to despair of coming to the bottom of these *phænomena*; since other motions of a like kind have been successfully explained. Thus the running of a drop of oil towards the concourse of two glass-planes and the motion of a bubble on the surface of liquors, when the glass is held obliquely towards that point, where the glass is inclined to the liquor in the smallest angle, are easily understood from the direction of the compound force with which the drop and bubble are acted.

bility arifes, and why it leaves no tract of moifture where it rolls.

20. From the like reafoning, we may conclude, That, when a fmooth needle is made to fwim, it does not any where touch the water, but forms around it, by its repulfive power, a ditch or bed, whofe concavity is much larger than the bulk of the needle. [*See* Tab. iii. *Fig*. 3.] And hence it is eafy to underftand how the needle fwims upon a fluid lighter than itfelf; fince the quantity of water, difplaced by it, may be equal to the weight of the needle. *Phænomena* of this kind, inftead of being reduced to hydroftatical principles, are commonly attributed to the mere tenacity of water, and even ufed for meafuring its cohefive power. *See Muffchenbroeck, Elementa Phyfices.*

21. This inftance furnifhes us with a juft and neceffary correction of the common hydroftatical law, That "the whole fwimming "body is equal in weight to a quantity of "the fluid whofe bulk is equal to that of "the part immerfed:" for, to comprehend this, as well as all ordinary cafes, it fhould be faid more generally, That "the whole "weight of the fwimming body is equal to

"the

"the weight of the quantity of the fluid dif-
"placed by it.

22. THESE *Phænomena* appeared to me worthy of obfervation here: both becaufe they fhew the fertility of optical principles in leading to the knowledge of things otherways inacceffible; and becaufe they exhibit a clear fpecimen of a repulfive power, fimilar to that which we fuppofe neceffary for the reflexion of light from the anterior furface of a denfer *medium*. Nor do I fee how it is poffible to account for the fufpenfion of the drop in the air by *comparative attractions*; into which fome other appearances of repulfion have been, perhaps, not unfuccefsfully, refolved *.

SECT. IV.

On the Change which coloured Bodies undergo in different Lights.

23. SIR *Ifaac Newton* has abundantly proved, by a variety of arguments, that the ordinary colours of natural bodies arife folely from

* See *Jurin on capillary attraction*, at the end of *Cote's hydroftatical Lectures*.

from the compounded colour of thoſe rays which they reflect; their colour being denominated by the ſpecies of thoſe primitive rays which they reflect in greateſt plenty: but this part of the *Newtonian* doctrine will receive further confirmation by examining the different colours which the ſame body aſſumes when illuminated by different lights; and which may be called, in diſtinction from the former, their extraordinary colours.

24. BODIES of all the principal colours, *viz.* red, yellow, green and blue, are very little altered when ſeen by the light of burning ſpirits: but, if ſalts be continually mixed with them during the burning, different changes enſue.

25. WHEN ſal ammon. potaſh or alum are infuſed, the colour of red bodies appears ſomewhat faded and dirty: green and blue appear much the ſame as in candle-light; both being faint and hardly diſtinguiſhible: white and yellow are ſcarcely at all affected.

26. WHEN nitre or ſea-ſalt are plentifully mixed with the burning ſpirits, and the whole is ſtirred about briſkly; the brighteſt red bodies, ſeen by the light then emitted, are reduced to a dirty tawny brown, that ſeems

ſeems to have nothing of redneſs in it: green is transformed into another ſort of brown, only diſtinguiſhable from the former by a certain inclination to a livid olive-colour; when nitre is mixed with the ſpirits, one may ſtill ſee ſome remains of a greeniſh colour, unleſs it be poured *in* very plentifully: dark blue is hardly to be known from black, except that it appears the deeper black of the two: light blue is changed into a very light brown of a peculiar kind: white aſſumes a livid yellowiſh caſt: and yellow alone appears unaltered and extremely luminous. Theſe experiments I made with different ſorts of rich-coloured bodies, as ſilks, cloths and paints. Poliſhed copper, which has contracted from the air a high-flaming colour, is reduced by the ſame light into the appearance of yellow braſs; the faces and hands of ſpectators appear like thoſe of a dead corpſe; and other mixed colours, which have red or green in their compoſition, undergo like changes.

27. Having placed a paſte-board with a circular hole in it between my eye and the flame of the ſpirits, in order to diminiſh and circumſcribe my object, I examined the

 conſtitution

conſtitution of theſe different lights with a priſm, (holding the refracting angle upwards) and found, that, in the firſt caſe [N° 25.], when ſal ammon. alum or potaſh fell into the ſpirits, all ſorts of rays were emitted, but not in equal quantities; the yellow being vaſtly more copious than all the reſt put together, and red more faint than the green and blue.

28. In the light of ſpirits mixed with nitre or ſea-ſalt, I could ſtill obſerve ſome blue, tho' exceſſively weak and diluted: with the latter, the green was equally faint; but, with the former, pretty copious. But, when either of theſe ſalts were uſed, I could hardly ſee any veſtige of the red at all, at leaſt when they were poured *in* plentifully, and the ſpirits conſtantly agitated. At every little intermiſſion indeed the red rays would ſhow themſelves very manifeſtly below the hole, and red bodies ſeen by that light reſumed ſomewhat of their ordinary colour: and it was very entertaining to obſerve how both would evaniſh again at once, as ſoon as the ſalting and ſtirring were renewed.

The proportion in which the bright yellow exceeds the other colours in this light,

is

is ſtill more extraordinary than in the former: inſomuch that the hole ſeen thro' the priſm appears uniformly of this yellow, and as diſtinctly terminated as thro' a plain glaſs; except that there is adjoining to it on the upper ſide a very faint ſtream of green and blue. White bodies illuminated with it, appear alſo thro' the priſm perfectly well defined; both which are very ſurprizing *phænomena* to thoſe who have been accuſtomed to the uſe of the priſm in other heterogeneous lights, where it never fails to throw confuſion on the extremities of all viſible objects.

29. BECAUSE the hole appears thro' the priſm quite circular and uniform in colour; the bright yellow which prevails ſo much over the other colours, muſt be of one determined degree of refrangibility; and the tranſition from it to the fainter colour adjoining, not gradual, but immediate.

30. UPON examining ſoap-water-films in the ſame light, I could only obſerve luminous bands ſeparated by dark ones; the green and blue being too weak to affect my eye in this view. It would be needleſs labour to enter here into a particular detail of the reaſons of the different transformations of co-

loured

loured bodies, above related [N° 24, 25, and 26.]; ſince, in general, it is evident enough, that they are owing to the different compoſitions of the lights with which they were illuminated: the experiments with the priſm [N° 27, 28.] are of themſelves a ſufficient commentary upon the reſt.

SECT. V.

A Remark on EULER's Nova Theoria Lucis et Colorum.

31. EULER, in that treatiſe, (publiſhed lately along with ſome other tracts, under the title of *Opuſcula Mathematica*) endeavours to amend the *Huygenian* hypotheſis of vibrations, and ſupport it againſt the objections which made *Newton* and his followers reject it: we ſhall not enter here upon the diſcuſſion of that queſtion; as it would require a diſcourſe of conſiderable length; and the rather, that the *Newtonian* theory of light and colours depends not on any particular hypotheſis with reſpect to the intimate nature of light (in like manner as his ſyſtem of univerſal gravitation is independent of all hypotheſes

pothefes concerning the caufe of gravity). In his Optics, he lays down his difcoveries at full length, without ever inquiring whether light confifts in vibrations propagated thro' a fluid or of particles projected in ftraight lines from the luminous body: and, in his queries, where he touches this matter *, he feems to be more pofitive in rejecting the hypothefis of vibrations, than in eftablifhing any other.

32. But *Euler* likeways advances a new notion with refpect to the origin of colours in opaque bodies, which is intirely inconfiftent with the principal part of Sir *Ifaac Newton's* doctrine. He fuppofes, that coloured bodies reflect the fun's incident white light from their anterior furface; but, that the particular fpecies of light, by which they appear coloured, is properly emitted by the parts of the body: for inftance, he imagines that vermilion does not appear red by a more copious reflexion of red than of other incident rays, but by the new emiffion of red rays from the particular velocity of vibration which its elaftic parts are capable of conceiving by the impulfe of the incident light.

33. It

* *Newt.* Opt. quer. 28. and 29.

33. It is a ſufficient refutation of this ſyſtem, that no *phænomena* prove or require its exiſtence: whereas *Newton*'s theory not only ſolves the *phænomena*, but is directly drawn from a multitude of experiments. According to *Euler's* hypotheſis, a body of one colour, placed in homogeneous light of another, ought not to appear of the colour of the light, but of a middle one between that and its own natural colour; which is contrary to experience *.

34. If it ſhould be ſaid, That none of the incident light is capable of qualifying the body for emitting its proper colour, but rays of the ſame colour: that which he calls new light emitted will be, in his ſcheme, more properly incident light reflected.

35. The chief or only fact which ſeems to have led him into that opinion, is, that there are many coloured bodies, ſuch as metals, which are capable of receiving a fine poliſh; and therefore of reflecting regularly the images of other objects, and at the ſame time retain their proper colour by which they are ſeen in all poſitions. *That* light by which we ſee in them the images of other objects, he

* *Newt.* Opt. book 1. part 2. prop. 10.

he acknowledges to be incident light properly reflected; but the other, he ſuppoſes, is properly emitted from the colorific parts of the body. But what neceſſity is there of recurring to this ſuppoſition, when we know, previouſly, that the component parts * of all opaque bodies are tranſparent; that, from every tranſparent body, there is a double reflexion; part of the incident light being reflected at the firſt ſurface, and a part of what paſſes thro' the firſt, reflected at the ſecond? and when we know, further, that very thin bodies, (as ſoap-bubbles, *Muſcovy*-glaſs, and air in a fracture of glaſs or ice, or between two *lenſes)* while they reflect ſome rays of all colour from the firſt ſurface, reflect only particular colours at the ſecond †? Do not theſe facts lead us naturally to ſuppoſe the firſt ſort of light to be only a part of the incident light reflected from the firſt ſurface of the body; and the ſecond, a part of what had paſſed on, reflected from the poſterior ſurfaces of the ſuperficial particles?

SECT.

* *Newt.* Opt. book. 2. part 2. prop. 2.

† Ibid. book 2. part 3. prop. 12.

SECT. VI.

Concerning the Cause of the different Refrangibility of the Rays of Light *.

36. In order to account for the different refrangibility of the differently-coloured rays, Sir *Isaac Newton* † and several of his followers have supposed, that their particles are of different magnitudes or densities: but, if there be any analogy between gravity and the refractive power, it will produce equal perpendicular velocities in all particles, whatever their magnitude or density be; and so all sorts of rays would be still equally refrangible.

37. It seems therefore a more probable opinion, which others have advanced, that the differently-coloured rays are projected with

* Altho' the doctrine contained in this section has been already published in the Philosophical Transactions for 1753, (vid. vol. xlviii. part 1. p. 262, &c.) having been communicated to the Royal Society, by the Author, in a letter to the Reverend Dr. *James Bradley* D. D. F. R. S.; yet it could not be omitted here, on account of its connexion with some of the queries that follow; besides that it contains several illustrations not to be found in the Transactions.

† *Newt.* Opt. query 29.

with different velocities from the luminous body: the red, with the greateſt; violet, with the leaſt; and the intermediate colours, with intermediate degrees of velocity: for, upon this hypotheſis, it is manifeſt, that they will be differently refracted in the priſmatic order; according to obſervation. Since, according to Sir *Iſaac Newton*'s doctrine of refraction now generally received, the velocity of a ray, after entering any new *medium*, is, to its former velocity, as the ſine of incidence to the ſine of refraction *; if all the colours move with equal ſwiftneſs in any one *medium*, their velocity will neceſſarily become unequal, upon entering a denſer *medium*, in the inverſe proportion of their ſeveral ſines of refrangibility: tho' we ſuppoſe, therefore, the ſun's rays to be emitted with one common velocity, it will follow that their velocities are unequal in air, glaſs, water, or any tranſparent body, whoſe refractive denſity differs from that of the ſolar atmoſphere †. This conſideration is ſufficient to take off the appearance of improbability from our *hypotheſis*.

* *Newtoni* Principia, lib. i. prop. 95.

† See below, query 3.

38. On ſuppoſition that the different refrangibility of the rays of light ariſes ſolely from their different velocities before incidence; theſe velocities muſt be, to one another, nearly as their ſines of refraction.

39. Sir *Iſaac* found their ſines of refraction from glaſs into air, beginning from the extreme violet, to be * as 78, $77\frac{7}{9}$, $77\frac{2}{3}$, $77\frac{1}{2}$, $77\frac{1}{3}$, $77\frac{1}{5}$, $77\frac{1}{8}$, 77; the ſine of incidence being 50: from whence their ſines of refraction out of air into glaſs, beginning from the extreme red, and ending with the extreme violet, are found to be as † 78000, 77873, 77797, 77663, 77496, 77330, 77220, 77000; the ſine of incidence being 120120. Theſe numbers, therefore, nearly expreſs the velocities in air, of the ſeveral rays, before their incidence ‡.

40. Hence

* *Newt.* Opt. book 1. part 2. prop. 3.

† The extreme ſines are plainly reciprocal to the former; and thoſe of intermediate colours are fourth proportionals to the ſine in Sir *Iſaac*'s experiment, 77 and 78.

‡ The quantities which give the accurate proportion of the velocities, before incidence, muſt be in a conſtant *ratio*: the ſines of refraction, by which the above calculations are made, have this condition: but, it is otherways manifeſt, that they give only a groſs approximation to the truth.

From

40. Hence their velocities in any other *medium*, may be found; for, they are, to these,

From what follows, perhaps, an exacter computation might be made, if a proper mean angle of incidence were made use of, altho' the quantities in the canon are really not in a constant *ratio*.

Tab. iii. *fig.* 1. Let two rays, falling in the same line of incidence IC, with different velocities, upon AB the surface of a denser *medium*, be refracted into different lines CR, CV. Taking any line CD in the perpendicular to represent the total action of the refracting power on the less refrangible ray, and CE on the more refrangible: If, thro' D and E, parallels to IC be drawn, meeting the refracted rays in V, R and G; it is plain, that CR, CV will be, as their respective velocities after refraction; and DR, EV, as their velocities before incidence. Since the whole acceleration which a given power produces in a body, is, *cæteris paribus*, as the time in which it operates; CD must be to CE nearly as the time which the swifter ray takes to pass thro' the refracting space, to that which the slower ray takes in passing thro' the same, inversely, as their velocities before incidence; that is, as EV to DR: but CD is likeways to CE as DG to EV; therefore DR, EV and DG, are continued proportionals; therefore DR is to EV in the subduplicate *ratio* of DR to DG: but DR is to DG in a *ratio* compounded of DR to DC, and DC to DG, that is, in the compounded *ratio* of S, DCR to S, DRC and of S, DGC to S, DCG; wherefore DR is to EV in the subduplicate *ratio* of S, DCR × S, DGC to S, DCG × S, DRC; that is, "The velocities before incidence are nearly in the direct "subduplicate *ratio* of these sines and the reciprocal sub- "duplicate *ratio* of the sines of the excesses of the common "angle of incidence above the several angles of refraction."

theſe, as the ſine of incidence to the ſine of refraction, when a ray paſſes from air into the given *medium* *.

41. WHILE the differently-coloured rays are ſuppoſed to move with one common velocity, any pulſes, excited in the ethereal *medium*, muſt overtake them at equal diſtances; and therefore the intervals of reflexion and tranſmiſſion, if they ariſe in this manner, as Sir *Iſaac Newton* conjectures, would be all equal: but, if the red move ſwifteſt, the violet ſloweſt, and the intermediate colours with intermediate velocities; it is plain, that the ſame pulſes muſt overtake the violet ſooneſt, the other colours in their order, and, laſt of all, the red; that is, the intervals of the fits muſt be leaſt in violet, and gradually greater in the priſmatic order; according to obſervation.

42. As the proportion between theſe intervals in red and violet can be aſſigned by experiment, and the proportion of their velocities in any *medium* likeways, by N° 40.; the velocity of the ethereal pulſes in any *medium*, and their diſtance from one another, may be thence computed by the following rule:

"Multiply

* *Newt.* Princip. lib. 2. prop. 0.

"Multiply the product under the velocities "of the red and violet rays by the difference "of the intervals of their fits; then divide "by the difference of the two products "which are formed by multiplying the in- "terval of the fits in red by the velocity "of the violet, and the interval of the fits "in violet by the velocity of red:" the quotient ſhall expreſs the velocity of the ethereal pulſes *.

43. THE velocities of the red and violet in air, are, by the above eſtimation, as 78 and 77 †; and the intervals of their fits are, by experiment ‡, as 100 and 63: from whence, by the canon now laid down, the velocity of the ethereal pulſes is found to be, to that of red light, as 79763 to 78000.

As

* Let C denote the celerity of the ethereal pulſes, V the velocity of red light, and v that of violet, I and i the intervals of their fits, and D the perpendicular diſtance of two ſucceeding pulſes: it is plain, from the nature of the hypotheſis, that I is to D as V to C—V, and again, D to i as C—v to v; therefore, *ex æquo*, I is to i as CV—Vv to Cv—Vv: from which ariſes the equation

$$C = \frac{\overline{I-i} \times Vv}{I \times v - i \times V}.$$

† In the celeſtial *medium* they are leſs, [No 40.] but very nearly in the ſame proportion.

‡ *Newt.* Opt. book 2, p. 1, obſervat. 14.

As light moves from the ſun to us, by Dr. *Bradley*'s lateſt computation *, in 8′, 12″, the pulſes of the ethereal fluid will be propagated thro' the ſame ſpace in 8′, 1″.

44. The diſtance between the ethereal pulſes, is, to † the interval of the fits in red, as the difference between the velocity of the ethereal pulſes and that of red light is to the velocity of red light; that interval, therefore, is not much more than $\frac{1}{44}$th of the interval of the fits in red, and therefore does not much exceed $\frac{1}{2457931}$, of an inch ‡.

45. The velocity of the ethereal pulſes being determined, as above, from the intervals of the fits in the two extreme colours, as found by experiment, the intervals of the fits in the ſix intermediate rays may be calculated from theory; for the interval in any one colour muſt be, to that in red, as a product under the velocity of the given colour and the exceſs of the velocity of the ethereal pulſes above that of red, is to a product under the velocity of red and the exceſs of the

the

* See *Eames* Abridg. tranſact. vol. vi. p. 157.

† See note * to foregoing page.

‡ See the table of the thickneſs of coloured plates in *Newt.* Opt. part 2. book 2.

the velocity of the ethereal pulſes above that of the given colour: but, even upon the ſuppoſition of the truth of our theory, an exact coincidence between calculation and experiment is not to be expected till the velocities of the rays be more accurately determined.

46. Upon the hypotheſis of the different velocities of different colours, we may underſtand, at leaſt in general, whence it is, that the intervals of the fits may bear a proportion ſome way related to the ſpaces* occupied by the ſeveral colours in the ſpectrum; an analogy otherways very unaccountable! Since, from the velocities of the ſeveral rays upon which the intervals of the ſeveral fits depend, ariſe likeways their ſeveral degrees of refrangibility, which determine the ſpace occupied by each in the ſpectrum.

And thus likeways we may conceive, how the different rays are qualified to produce different ſenſations in the mind: for, having different degrees of impulſive force, they may cauſe vibrations of different magnitude or velocity in the optic nerve; by which, according

* Compare *Newt.* Opt. b. i. part 2. prop. 3. with b. ii. part 3. prop. 16th.

according to the laws of our conſtitution, the ideas of different colours may be excited *; in like manner as the ideas of different tones ariſe from different vibrations of the air communicated to the auditory-organ. It has been ſaid, That the different ſenſations excited in the mind cannot ariſe from the different force of the particles of light; ſince the colour of homogeneal rays is not altered by paſſing thro' different *media*, tho' their velocity be thereby always increaſed or diminiſhed †. But it ought to be conſidered, that every ray, as it muſt paſs at laſt thro' the humours of the eye in order to viſion, falls upon the *retina* with one given velocity, whatever number of refractions it has previouſly undergone: for the velocity of any ray in any one *medium* being, to its velocity in any other *medium*, in a conſtant proportion, *viz.* the inverſe of the ſines of incidence and refraction, when a ray paſſes from the one into the other; it is manifeſt, that each ray muſt have a certain determined velocity in any given *medium*, which cannot be either increaſed or diminiſhed by making the ray paſs

* *Newton's* Optics, query 13.

† *Muſſchenbroeck*, Elementa Phyſices, § 1161.

paſs previouſly thro' any number of tranſparent bodies any how diſpoſed*.

It is impoſſible therefore to know, whether an alteration of the ſwiftneſs, with which a homogeneal ray ſtrikes the *retina*, would alter its colour; I mean the ſenſation of colour produced by it in the mind: ſince it is impoſſible to alter, at pleaſure, the denſity of that fluid which determines its final velocity.

One may diſtinguiſh two different effects of the refractive power on the rays of light, *viz.* the change of direction and change of velocity. Sir *Iſaac Newton* has proved with reſpect to the firſt, That it is different in the differently-coloured rays, and of a determined degree in each: he has further proved, That refraction, conſidered in its firſt effect, does not change the colour of any ſimple ray. But it appears, from what we have now ſaid, That none of his experiments prove the

 immutability

* Here it is proper to obſerve, that the *hypotheſis* which ſuppoſes the intervals of the fits to be determined by the velocity of the ray, agrees well with a remarkable obſervation of Sir *Iſaac Newton* (Optics, b. ii. part. i. obſerv. 21.): *viz.* That theſe intervals in any *medium*, at a given angle of incidence, are of a given magnitude, without regard to the denſity of the ſurrounding *medium*.

immutability of ſimple rays by the ſecond effects of refraction.

47. As it is of great conſequence in philoſophy to diſtinguiſh between facts and hypotheſes, however plauſible; it ought to be obſerved, that the various refrangibility, reflexibility, and inflexibility of the ſeveral colours, and their alternate diſpoſitions at equal intervals to be reflected and tranſmitted, which are the whole ground-work of the *Newtonian* ſyſtem, are to be conſidered as certain facts deduced from experiment: but whether the velocities of the different rays are exactly equal, or different in the manner now deſcribed, is no more than probable conjecture; and, tho' this point ſhould be decided by a method propoſed afterwards, it would ſtill continue uncertain, whether the fits of reflexion and tranſmiſſion are occaſioned by an alternate acceleration and retardation of the motion of light, or in ſome other manner *.

And,

* For inſtance, it might be ſuppoſed, that every particle of light has two contrary poles, like a load-ſtone; the one of which is attracted by the parts of bodies, and the other repelled; and that, beſides their uniform rectilineal motion, the particles of differently-coloured rays revolve in different periods round their center: for thus, their friendly and unfriendly

And, after all, it is no more than probable conjecture, that ſuch an alternate acceleration and retardation is brought about by the influence of pulſes excited in the ethereal *medium*: nay there are ſome circumſtances in theſe *phænomena* that ſeem hardly intelligible by that hypotheſis alone; as, why the intervals of the fits are leſs* in denſer *mediums*; and why they increaſe ſo faſt and in ſo intricate a proportion, according to the obliquity † of incidence.

48. ACCORDING to Dr. *Bradley*'s beautiful theory of the aberration of light, the ſtars appear to be removed from their true places to a certain diſtance, by the proportion which the velocity of the earth bears to the velocity of light: It is plain therefore, that, on our hypotheſis, a ſtar muſt have a different apparent place for every different colour; that is, its apparent diſk muſt be extended by the aberration into a longitudinal form reſembling the priſmatic ſpectrum, having [illegible] its

friendly poles being alternately turned towards the ſurfaces of bodies, they might be alternately diſpoſed to reflexion and tranſmiſſion; and that at different intervals, in proportion to the periods of their rotation.

* *Newt.* Optics, b. 2. part 3. prop. 17.

† Prop. 15. ibidem.

its red extremity neareſt to its true place. In th ſtars ſituated near the pole of the ecliptic, its length ſhould continue always the ſame, tho' directed along all the different ſecondaries of the ecliptic in the courſe of a year: but, in thoſe which lye in or near the plane of the ecliptic, it ſhould be greateſt at the limits of the eaſtern and weſtern aberrations; the ſtar recovering its colour and figure when the true and mean places coincide. But, there is no hope of diſcovering, whether our hypotheſis be true or falſe, by this conſequence of it; for the greateſt length of the dilated diſk, being, to the whole aberration, as the difference of the velocity of red and violet to the mean velocity of light, *i. e.* as 1 to 77 nearly, (N° 39.) cannot much exceed one fourth part of a ſecond; for the greateſt aberration is but about twenty ſeconds.

49. THE time which the extreme violet takes to move thro' any ſpace muſt be, to that which the red takes, as 78 to 77. If *Jupiter* be ſuppoſed in a quadrate aſpect with the ſun, in which caſe the eclipſes of his ſatellites are moſt commodiouſly obſerved, his diſtance from the earth being nearly equal

to

to his diſtance from the ſun; light takes about forty one minutes of time in paſſing from him to the earth: therefore the laſt violet light which a ſatellite reflects, before its total immerſion into the ſhadow of *Jupiter*, ought to continue to affect the eye for a 77th part of 41′, or 32″, after the red reflected at the ſame time is gone: that is, A ſatellite, ſeen from the earth, ought to change its colour above half a minute before its total immerſion from white to a livid greeniſh colour, thence into blue, and at laſt evaniſh in violet. I need ſcarcely obſerve, that the ſame *phænomenon* ſhould take place in the time of emerſion, by a contrary ſucceſſion of colours, beginning with red and ending in white.

50. If this *phænomenon* ſhould be actually perceived by aſtronomers, we ſhall have a ſufficient direct proof of the different velocities of the coloured rays; for I ſee not to what other cauſe the *phænomenon* could be rationally aſcribed: If it be not, we may conclude that rays of all colours are emitted and reflected with one common velocity.

SECT. VII.

On the Imperfection of our Knowledge concerning the Inflexions of Light.

51. Sir *Isaac Newton* went a very considerable length in examining the inflexions of light, as well as its reflexions and refractions; but did not bring his inquiry on this head to a conclusion. He tells us, that he intended once, if other business had not called him off, to have made more experiments; not for confirming himself in preconceived opinions, as many do; but for discovering the true manner in which light is inflected, for producing the coloured fringes with black lines between them. He adds, however, some queries which contain hints of what he had gathered on this subject from his own observations, *viz.* that the rays of light differ according to their colour in their degrees of flexibility, and that they are bent several times backwards and forwards with a serpentine motion in passing by the sharp edges of bodies: these thoughts he threw out "in "order to incite others to a further search *."

But,

* *Newt.* Opt. p. 313. see the first two or three queries.

But, ſo far have his intentions been diſappointed hitherto, that few phyſical writers ſeem to comprehend diſtinctly the hints which he has left concerning the manner of inflexions *; and none, as far as I know, has advanced

* When any opaque body is held at the diſtance of three or four inches from the eye, ſo that a part of ſome more diſtant luminous object, ſuch as the window or the flame of a candle, may be ſeen by rays paſſing near its edge: If another opaque body, nearer to the eye, be brought acroſs from the oppoſite ſide; the edge of the firſt body will ſeem to ſwell outwards and meet the latter, and, in doing ſo, will intercept a portion of the luminous object that was ſeen before.

This *phænomenon* has been raſhly aſcribed to the inflexion of light, by ſuch as underſtood not thoroughly the nature of inflexion, nor obſerved accurately the circumſtances of the fact.

Let AB repreſent the luminous object (TAB. iii. *Fig.* 5.) to which the ſight is directed, CD the more diſtant opaque body, GH the nearer, and EF the diameter of the pupil; join ED, FD, EG, FG, and produce them till they meet AB in K, N, M and L: It is plain, that the parts AN, MB of the luminous object cannot be ſeen. But, taking any point *a* between N and K, and drawing *a*D*d*; ſince the portion *d*F of the pupil is filled with light flowing from that point, it muſt be viſible: any point *b* between *a* and K muſt fill *f*F a greater portion of the pupil, and therefore muſt appear brighter. Again, any point *c* between *b* and K muſt appear brighter than *b*, becauſe it fills a greater portion *g*F with light. The point K itſelf, and every other point in the ſpace KL, muſt appear with compleat luſtre;

advanced one ſtep beyond them. It is ſurprizing, that, before Sir *Iſaac Newton*, the world continued ſo long entirely ignorant of the true theory of light and colours; and it is no leſs ſo, that, ſince he quitted the ſubject, no further diſcovery of any moment has yet been made amongſt all the philoſophical Societies in *Europe*.

52. MANY

luſtre; ſince they ſend entire pencils of rays EKF, ELF to the eye: and the viſible brightneſs of every point from L towards M muſt decreaſe gradually as from K to N: *i. e.* The ſpaces KN, LM will appear as dim ſhadowy borders or fringes adjacent to the edges of the opaque bodies. When the edge G is brought to touch the right line KF, the penumbra's unite; and, as ſoon as it reaches NDF, the above *phænomenon* begins: for it cannot paſs that right line without meeting ſome line *a*D*d* drawn from a point between N and K, and, by intercepting all its rays that fell upon the pupil, render it inviſible. In advancing gradually to the line KDE, it will meet other lines *b*D*f*, *c*D*g*, *&c.* and therefore render the points *b*, *c*, *&c.* from N to K ſucceſſively inviſible; and therefore the edge of the fixed opaque body CD muſt ſeem to ſwell outwards, and cover the whole ſpace NK, while GH by its motion covers MK. When GH is put to a greater diſtance from the eye, CD continuing fixed; the ſpace OP to be paſſed over for intercepting NK is leſs; and therefore, with an equal motion of GH, the apparent ſwelling of CD muſt be quicker; which is found true by experience If ML repreſents a luminous object, and REFQ any plane expoſed to its light; the ſpace FQ will be entirely ſhaded

52. MANY ingenious men have beſtowed infinite thought and labour on the more complex and aſtoniſhing *phænomena* of Nature, without arriving at any certain or definite diſcoveries; ſuch as earthquakes, thunder and other meteors, magnetiſm, electricity, vegetation, fermentation and other chemical operations: and the ſubtilty of thoſe matters will probably continue to elude the ſearch of lateſt poſterity. But, in the ſimpler, ſteadier, and more regular ſubjects, ſuch as light and colours, which are capable of accurate menſuration and mathematical reaſoning, a ſagacious and induſtrious obſerver can hardly fail of making ſome progreſs; eſpecially in a branch of the inquiry which is already puſhed to a conſiderable length. Diſcoveries of this kind are capable of a particular ſort of proof which is very beautiful and convincing, from the exact coincidence of the computed effects

 with

ſhaded from the rays, and the ſpace FE will be occupied by a *penumbra* gradually darker from E to F: Let now GH continue fixed, and CD move parallel to the plane EF; and, as ſoon as it is paſſes the line LF, it is evident, that the ſhadow QF will ſeem to ſwell outwards, and when CD reaches ME ſo as to cover with its ſhadow the ſpace RE, QF by its extenſion will cover FE. This is found to hold true likeways by experiment.

with the real ones, as to quantity. Many inſtances of this occur in Sir *Iſaac Newton*'s writings, and in all mathematical philoſophy: ſuch as the calculation of the moon's irregularities; of the tides; of the preceſſion of the equinoxes; of the reſiſtance of fluids; and, in optics, his computation of the dimenſions of the rainbow; of the aberration of colours; of the intervals of the fits of reflexion and tranſmiſſion; and of the coloured rings reflected by thick tranſparent *ſpeculums*.

WHAT further I have to offer concerning light and colours, conſiſting chiefly of doubts, difficulties or looſe conjectures, ſhall be propoſed under the form of queries.

SECT.

SECT. VIII.

Queries, consisting of Doubts, Difficulties, and Conjectures, concerning Light, Colours, and coloured Bodies.

QUERY I. Are not the rays, emitted by all sorts of luminous bodies, similar to those of the sun, both as to colour and degrees of refrangibility? And, do not luminous bodies differ from one another only according to the colours which they emit most plentifully, in like manner as opaque bodies are distinguished by the colours of incident light which they reflect in greatest abundance? (*See* N° 24, 25, 26, 27, 28, and 29.) But, to make our induction sufficiently strong, ought not experiments to be made with the lights of a greater variety of bodies? And would it not further conduce to the illustration of this question to form, by Sir *Isaac*'s method *, a beam of solar light, consisting of such colours and in such proportions as were seen in the lights of salts and burning spirits; and then to observe in it the appear-

ance

* *Newt.* Opt. book 1. part 2. prop. 11.

ance of coloured bodies? Further, are not the intervals of the fits, in rays of any one colour, the fame in the fame *medium*, from whatever luminous body they are emitted? For, if thefe intervals were different, would there not be changes in the colours of bodies not to be accounted for by the compofitions of the lights with which they are illuminated*?

QUER. II. Do not all luminous bodies, the moft languid as well as moft bright, emit their lights of any one colour with one determinate velocity; fince it is found by experience that they are all equally refracted by the fame *medium*? And therefore, does not the different fplendor of luminous bodies proceed wholly from the different denfity of their light at equal diftances? And is not this confirmed by the equality of *Bradley*'s aberration of light in fixed ftars of all magnitudes †? If this be fo, the comparative ftrength of different lights, fuch as of the fun, moon, a candle, *&c.* may be eafily eftimated by finding the greateft diftances to which the fame opaque body is vifible when illuminated by each of them, or the limits beyond

* *See* Art.

† *Eames*'s Abridg. of tranfact. vol. 6. p. 158.

yond which it is invifible to a given eye; for the denfities of the incident lights are nearly as the fquares of the diftances of thefe limits from the object *. Does not all light move with the fame velocity after reflexion as before; fince the angle of reflexion is always equal to the angle of incidence? The exception, made by fome, of electrical light is founded on no lefs a miftake than confounding the luminous body with its light †. But, the beft proof of this propofition is from the coincidence of the computations of the velocity of light, from the equation of the eclipfes of *Jupiter*'s fatellites and the aberration of the fixed ftars ‡.

QUER.

* Let A and *a* (TAB. iii. *fig.* 6.) denote the fame or two equal bodies of the fame colour illuminated with different lights, and B, *b*, the limits. As we fuppofe the light received by the eye, at thefe points, is juft fufficient to affect it fenfibly and no more, the two lights at thefe different diftances muft be nearly of the fame denfity; taking therefore in AB a line Aβ equal to *ab*, the denfity of the light at β muft be, to the denfity of the light at *b*, nearly as AB^2 to Ab^2: and, it is evident, that thefe denfities, at equal diftances, muft be as the whole quantities of light reflected; and thefe again very nearly as the whole quantities of light incident.

† *Muffchenbroeck*'s Elementa Phyfices, late edition, in his chapter on electricity.

‡ *Eames*'s Tranfact. vol. vi. &c.

QUER. III. Is light emitted with the ſame velocity, in whatever *medium* the luminous body be placed? Or, is it not rather emitted with greater velocity in denſer *mediums*, and that in proportion to their refractive powers? The ſame argument from whence we gather in general the equal velocity of light emitted by all ſorts of luminous bodies, ſeems to prove the truth of the latter ſuppoſition. For, ſince rays of any one colour, from the ſun and a candle, for inſtance, are equally refracted by a ſurface of glaſs or water, we may conclude, that their velocities in air are equal. Wherefore, if the denſity of the ſun's atmoſphere, contiguous to his ſurface, be different from the denſity of our lower air, as may be ſafely preſumed, his rays muſt have been emitted with more or leſs velocity than that of the candle; otherways, they could not have the ſame velocity afterwards in any common *medium*: for, the velocity with which any ray is emitted, is, by the laws of refraction, to its velocity in any given *medium*, as the ſine of refraction to the ſine of incidence, when a ray paſſes from the

the *medium* of emiſſion into the given *medium*.

QUER. IV. IF the atmoſphere is not much warmed by the paſſage of the ſun's light thro' it, but chiefly by its contact with the heated ſurface of the globe, as we ſhewed above (N° 15.); may we not hence give one very ſimple and plauſible reaſon, why it is coldeſt in all climates on the tops of very high mountains; namely, becauſe they are removed to the greateſt diſtance from the general ſurface of the earth? For it is well-known, that a fluid heated by its contact with a ſolid body decreaſes in heat, in ſome inverſe proportion to the diſtance from the body. But, to have this queſtion fully determined, the temperature of the air in the valley and on the mountain-top muſt be obſerved every hour both night and day, and carefully compared together.

QUER. V. FROM what has been laid down in Sect ii. concerning the manner of the action of light in heating bodies, is it not reaſonable to ſuppoſe that the heat produced by a given number of rays, in an opaque body of a given magnitude, muſt be greater

when

when the rays are more inclined to one another, than when they are leſs ſo? For the direction of the vibrations, raiſed by the action of the light, whether in the colorific particles or thoſe of an inferior order, will more interfere with one another; from whence the inteſtine ſhocks and colliſions muſt increaſe: beſides this, the colorific particles of opaque bodies being diſpoſed in various ſituations, perhaps, upon the whole, the rays will fall more directly on each, the more they are inclined to one another. Is not this the reaſon of what has been remarked by philoſophers* †, That the heat of the ſun's light, collected into a cone, increaſes in approaching the focus in a much higher proportion than according to its denſity? That the difference of the angle, in which the rays fall on any particle of a given magnitude placed at different diſtances from the focus, is but ſmall, is no proof that the *phænomenon* cannot be aſcribed to it; ſince we know not in what high proportion one or both the circumſtances now mentioned may operate. However, that

* *Boerhaave,* Element. chemic. de igne.

† *Muſſchenbr.* Elementa Phyſices, § 1040.

that it proceeds not from any unknown action of the rays upon one another, as has been insinuated *, is evident from this, that each particular ray, after passing thro' the focus, preserves its own colour and its own direction, in the same manner as if it were alone.

QUER. VI. MAY it not be inferred, that the component parts of opaque bodies are greater than those of transparent ones, as theory requires †, from this simple observation, that the former, such as metals, stones, woods, *&c.* when broken transversely, shew a visible roughness and inequality at the fracture; whereas the latter, such as glass, chrystal, gems, ice, *&c.* appear as smooth, almost, as when they are polished?

QUER. VII. Do not *Newton*'s experiments with the island and rock chrystal sufficiently prove, that the rays of light have different permanent properties in their different sides, relative to these two bodies? Must we not therefore conceive each particle of light to preserve its position invariably while it moves forward, at least so as not to revolve round its

* *Musschenbroeck*'s Elementa physices, § 1040.

† *Newton*'s Opt. Book 2. part 3. prop. 4.

its center perpendicularly to the direction of its motion? Would it not be proper to try how light is inflected in passing closely by the several angles and sides of these fossils?

QUER. VIII. Is it not possible to prove by experiment what Sir *Isaac Newton* takes for granted as a reasonable supposition, that thin transparent plates, of any uniform colour, divided into smaller fragments, would compose a powder of like colour *? And would not this tend to strengthen the analogy between the colours of such plates and those of natural bodies? For this purpose, I have tried to freeze soap-bubbles; but could never make any stand till they were turned to ice, except such as were too thick to have lively colours: however, I doubt not, but, with due care, the thing might be done; especially, if the soap-water, instead of being blown with a pipe into bubbles, were drawn out into a plain plate upon any wooden or metalline frame: for, the sides of a plain surface bearing a greater proportion to its *area*, than a base of a spherical segment to its surface, the frost would be sooner communicated

* *Newton*'s Opt. book 2. part 3. prop. 5.

municated to the whole water in the former caſe than in the latter. There is this advantage too in uſing a plain ſurface of ſoap-water, that, before it freezes, the obſerver may draw out any particular colour or ſeries of colours, which he chuſes, to a greater breadth, by ſtroaking it along with a wet finger. For this reaſon, amongſt others, I have found it a more convenient ſubject for examining the various orders of colours, than ſpherical bubbles adhering to a plane. Perhaps, melted roſin might be drawn out into a thin-coloured plate before it hardens; for I have often blown it into bubbles with a tobacco-pipe till it became coloured. I know no other ways in which the various orders of colours can be preſerved for deliberate inſpection, but either in a frozen plate of water or roſin, or in the permanent *ſcoria* that appear on heated metals. I have counted, on the ſide of a clean-poliſhed copper tea-kettle, the ſix firſt orders of colours diſtinctly and regularly ranged in the ſame ſucceſſion in which they appear in the ſoap-bubbles; the firſt order being formed on that part of the kettle that had been leaſt heated.

QUER.

QUER. IX. WHAT elſe is the inflexion of light towards the fine edges of bodies than a particular caſe of refraction, in which the rays, after being bent by the attractive power, are carried beyond the refracting ſurface, and miſs entring it, becauſe of its ſmall extent? For, if the ſurface of the edge be produced, it will meet the inflected rays; and thus the inflexion will become properly refraction. And, in like manner, we may conſider the inflexion of light off from the edges of bodies, as a ſpecies of reflexion.

QUER. X. Is it not impoſſible that an animal can ſee, if the diameter of its eye be much leſs than the interval between the fits of tranſmiſſion and reflexion in water, that is, than $\frac{1}{375000}$th of an inch?

QUER. XI. THERE are many experiments which ſhew that a yellow and blue ray mixed, make a green one; a yellow and blue powder, a green powder; and a mixture of rays or paints of all the priſmatic colours, a white ray or paint: Now, do not the ſame experiments equally demonſtrate, that the idea of green is a confuſion or mixture of the

ideas

ideas of yellow and blue; the idea of white, a mixture of the ideas of all the colours; and, in general, the ideas of all compound colours, a mixture of the ideas of their conſtituents? In the experiments which Sir *Iſaac Newton* performed with the toothed inſtrument, the component colours are not, indeed, preſented to the eye all at once; yet they follow one another in ſo rapid a ſucceſſion, that their reſpective impreſſions remain in the eye till they are renewed, and therefore they muſt affect the mind all at once*. If a piece of paper

* It is in this manner that philoſophers explain (*Newt.* (Opt. Quer. 16.) the appearance of a fiery circle, which is made by a burning body whirled about ſwiftly. We ſhall here give an account of ſome other *phænomena* that flow from the ſame principle.

If a white rod be moved rapidly backwards and forwards with an angular motion, the whole circular ſpace which it runs over will appear whitiſh; but not equally ſo, being fainteſt and moſt dilute in the middle, and brighter towards the two ſides, which ſeem to be diſtinctly terminated with two white rods interſecting each other in the center of rotation. (See TAB. iii. *Fig* 7.)

The total impreſſion made upon the eye by equal ſmall parts of the ſector muſt be, as the quantity of light emitted from it and the frequency of the returns of the rod to it; *i. e.* inverſely, as the time between the returns of the rod. Let paper be daubed all over with ſmall dots of blue and yellow, it will appear green to an eye which is placed at too great a diſtance to diſtinguiſh

Let ABC repreſent the circular ſector, and DC a line biſecting it; the rod always returns to DC after the time of of one vibration; and, to any other line EC between DC and AC or AB, the mean time of its return is the ſame; for it alternately returns in twice the time of deſcribing AE, and twice the time of deſcribing EB; ſo that two ſucceeding intervals of its returns are equal to the time of two vibrations: but the intervals of the returns to the lines AC or CB are manifeſtly equal to the time of two entire vibrations. The brightneſs of the ſector therefore in DC, or any line between DC and AB or BC, muſt be ſimply as the quantity of light emitted from equal ſmall portions of the ſector; that is, in the inverſe proportion of the velocities of the rod when in theſe lines. It is plain from this, that the ſector muſt be *incomparably* brighter in AC and BC, where it reſts, than any where elſe, notwithſtanding that the intervals of return thither are double; that is, it will appear to be bounded diſtinctly with a white rod on each ſide.

If the rod be agitated with ſmall and quick vibrations of its own, by ſticking it againſt ſome ſolid body immediately before it is hurried backwards and forwards with the angular motion, the ſector appears divided, at equal intervals, by a great many diſtinct rods, almoſt as bright as the two lateral ones (TAB. iii. *Fig.* 8.) reſembling the ſpokes of a ſpread fan. The reaſon of which curious *phænomenon* is plainly this; that its angular motion, being alternately in the ſame and in a contrary direction to its particular vibrations, is alternately accelerated and retarded or ſtopt. In the interval, where it is accelerated, the ſector muſt appear very dilute; and, where

it

ſtinguiſh the ſeparate points. In whatever manner ſenſation be performed, it is certain, that the organs which receive the firſt impulſe from external objects cannot convey to us any ideas, if they, or the impreſſions made by them, be leſs than of a certain definite magnitude. A number of things ſeparately intangible, if joined together, may be felt by the touch: A certain number of inviſible points become ſufficient to affect the ſight by their united rays; and a certain number of ſounds too ſmall to be heard ſeparately, at laſt form an audible ſound *.

QUER.

it is greatly retarded or brought to reſt, muſt appear very luminous or divided by white rods, for the ſame reaſon that they appear at the ſides.

* Some Sceptics have diſputed againſt the endleſs diviſibility of quantity, becauſe the imagination ſoon arrives at a *minimum*; alledging from thence, that our idea of extenſion involves the notion of indiviſibles, and is as it were compounded of them. Nothing corporeal can be imagined or conceived at all which is not conceived as *ſeen*, *handled*, or otherways ſenſibly perceived. Imaginative ideas are nothing elſe than tranſcripts or images of ſenſations, and therefore muſt be limited by the ſame bounds and in the ſame manner as ſenſation. Now the *minimum ſenſibile* is rather in all caſes a confuſed, indiſtinct and uncertain tranſition from *perceivable* to *not perceivable*, than the clear perception of a point indiviſible in magnitude; for its magnitude depends

on

QUER. XII. SINCE bodies derive their colours from the original and immutable qualities of thoſe rays which they reflect moſt copiouſly, ought they not to appear of the ſame colour, whether viewed at the greateſt or leaſt diſtances? Whence is it therefore, that the planets whoſe ſolid parts are probably covered with vegetables, and muſt therefore reflect a great ſuperiority of green

on the luſtre of the object. That nothing can be conceived or imagined which is leſs than a certain bulk, is no more an argument againſt the endleſs diviſibility of quantity, than that nothing can be felt or ſeen below that ſize; which, it is evident, from every magnifying glaſs and from every different diſtance of an object, depends not at all on the conſtitution of the thing perceived, but on that of the perceiver, or the means and circumſtances of his perception.

Nor, tho' it were granted that the *minimum viſibile* is diſtinctly ſeen as an indiviſible point, would it follow, that the idea of extenſion, received by ſight, is made up of the ideas of indiviſibles; for we receive the idea of extenſion by that motion of the eye which is neceſſary to direct its *axis* to different objects or parts of an object: and, it is well known, that the generation of quantity by motion is preferred by the beſt writers, for this very reaſon, that it neceſſarily excludes the notion of indiviſibles. It ſhould be remembered likeways, that a viſible object is not divided by the eye into a number of contiguous *minima viſibilia*; for, to whatever mathematical point in the object the eye is directed, a *minimum viſibile* may be ſeen there by means of a certain portion of the object immediately ſurrounding it.

green rays, appear almoſt intirely white when viewed from the earth? May not this be accounted for, in the ſame manner as the change of colour obſervable in earthly objects ſeen thro' a great tract of the atmoſphere? A mountain covered with the freſheſt verdure, at the diſtance of twelve or fifteen miles, looks blueiſh; and at twenty or thirty, eſpecially if the air be thickened, degenerates into a dim white, ſo that one can hardly diſtinguiſh it from the clouds that ſkirt the horizon. With reſpect to the primary planets, it may be likeways anſwered, that perhaps we ſee them chiefly by light reflected from the air and vapours that ſurround them.

QUER. XIII. Why is it ſo hard to diſtinguiſh green bodies from blue by candle-light?

QUER. XIV. Whence proceeds the blueneſs of the ſky? Since it is certain that no body aſſumes any particular colour, but becauſe it reflects one ſort of rays more abundantly than the reſt; and ſince it cannot be ſuppoſed that the conſtituent parts of pure air are groſs enough to ſeparate any colours of themſelves; muſt we not conclude, with

Sir *Isaac Newton* *, that the violet and blue-making rays are reflected more abundantly than the rest, by the finer vapours diffused thro' the atmosphere whose parts are not big enough to give them the appearance of visible opaque clouds? Do not those who say †, that the ethereal blue proceeds from the mixture of the sun's white light reflected faintly by the atmosphere with the perfect blackness of the celestial space behind, revive, without any necessity, the antient confused notion, that all colours may be formed by certain compositions of light and shade? Altho' the atmosphere reflects more blue rays than what go to the formation of perfect white, it is easy to conceive how coloured bodies, illuminated by it, may not be sensibly tinctured with blue. Let us suppose, that the atmosphere reflects $\frac{1}{4}$ more of blue rays than of the other colours, and that vermilion reflects $\frac{19}{20}$ of the red rays incident upon it, and $\frac{1}{20}$ of every other colour; then, it is clear, that the red rays, reflected by the vermilion, will still exceed the blue reflected by it, as 19 exceeds $1 + \frac{1}{4}$: so that

* Opt. book 2. part 3. prop. 7.

† Nature displayed, vol. iv. And *Muschen.* Phys. § 1403.

that the purity of its red colour will not be ſenſibly impaired. But, to ſhew that, in proper circumſtances, the blueiſh colour of ſky-light may be ſeen on bodies illuminated by it, as it is objected ſhould always happen *; expoſe to the ſun-beams, on a clear cloudleſs day, a ſheet of white paper, and place on it any opaque body; you will perceive that the ſpace of the ſhadow, which is illuminated only by the ſky, appears remarkably blueiſh, compared with the reſt of the paper which receives the ſun's direct rays. If certain white and black paints mixed together produce blue, it is becauſe the black is not perfect ſhade, but a dark blue or purple † ‡. Any mixture of whiteneſs and true black can only form a fainter white or grey, which has no more affinity with blue than with red or any other colour.

QUER. XV. Is not the opinion which Sir *Iſaac Newton* ſeems to have had ||, and, ſince him, the generality of philoſophers, concerning the cauſe of the various colours reflected

* *Muſchen.* Phyſ. § 1403.

† Ibid. § 1172.

‡ *Newt.* Opt. book 2. part 3. prop. 7.

|| Opt. book 2. part 3. prop. 5. near the end.

flected by the clouds at ſun-riſing and ſetting, liable to great difficulties? For, why ſhould the particles of the clouds become, at that particular time and never at any other, of ſuch magnitude as to ſeparate theſe colours? And why are they rarely, if ever, ſeen tinctured with blue and green, as well as red, orange and yellow? Is it not more credible that the ſeparation of rays is made in paſſing thro' the horizontal atmoſphere? and that the clouds only reflect and tranſmit the ſun's light, as any half tranſparent colourleſs body would do in their place? For, ſince the atmoſphere, as was ſaid in the laſt query, reflects a greater quantity of blue and violet rays than of the reſt, the ſun's light, tranſmitted thro' it, ought to draw towards yellow, orange, or red; eſpecially when it paſſes thro' the greateſt tract of air: accordingly, every one muſt have remarked, that the ſun's horizontal light is ſometimes ſo deeply tinctured, that objects directly illuminated by it appear of a high orange or even red; at that inſtant, is it any wonder that the colourleſs clouds reflect the ſame rays in a more bright and lively manner? It is obſervable, that the clouds do not commonly aſſume their

their brighter dyes till the ſun is ſome minutes ſet; and that they paſs from yellow to a flaming golden colour; and thence, by degrees, to red; which turns deeper and deeper, tho' fainter, till the ſun leaves them altogether. Now, it is plain, that the clouds, at that time, receive the ſun's light thro' a much longer tract of air than we do at the inſtant of ſetting, perhaps by the difference of a hundred miles or more; as may be computed from their height or the duration of their colours. Is it not, therefore, natural to imagine, that, as the ſun's light becomes always ſomewhat yellowiſh or orange in paſſing thro' the depth of the atmoſphere horizontally, it ought to incline more and more from orange towards red, by paſſing thro' a ſtill greater length of air; ſo that the clouds, according to their different altitude, may aſſume all the variety of colours, obſerved in them at ſun-riſing and ſetting, by barely reflecting the ſun's incident light as they receive it? I have often obſerved with pleaſure, when in *Switzerland*, that the ſnowy ſummits of the *Alps* turn more and more reddiſh after ſun-ſet, in the ſame manner as the clouds. What makes the

ſame

ſame colours much more rich and copious in the clouds, is their ſemi-tranſparency joined with the obliquity of their ſituation.

Does it not greatly confirm this explication, that theſe coloured clouds immediately reſume that dark leaden hue which they receive from the ſky as ſoon as the ſun's direct rays ceaſe to ſtrike upon them? For, if their gaudy colours aroſe, like thoſe of the ſoap-bubble, from the particular ſize of their parts, they would preſerve nearly the ſame colours, tho' much fainter, when illuminated *only* by the atmoſphere. About the time of ſun-ſet or a little after, the lower part of the ſky, to ſome diſtance on each ſide from the place of his ſetting, ſeems to incline to a faint ſea-green, by the mixture of his tranſmitted beams, which are then yellowiſh, with the ethereal blue: at greater diſtances, this faint green gradually changes into a reddiſh brown; becauſe the ſun's rays, by paſſing thro' more air, begin to incline to orange: and, on the oppoſite ſide of the hemiſphere, the colour of the horizontal ſky inclines ſenſibly to purple; becauſe his tranſmitted light which mixes with the azure, by paſſing thro' a ſtill greater length

of

of air, becomes reddiſh; as we have ſaid above.

To underſtand diſtinctly why the ſun's rays, by paſſing thro' a greater and greater quantity of air, change by degrees from white to yellow, thence to orange, and laſtly to red, we have only to apply to the atmoſphere, what Sir *Iſaac* ſays (Book I. of his Optics, part 2. prop. 10.) concerning the colour of tranſparent liquors in general.

Is it not the ſame coloured light of the riſing and ſetting ſun which tinctures the clouds, that, being thrown by the refraction of the atmoſphere into the earth's ſhadow, gives the moon ſometimes, in total eclipſes, the obſcure reddiſh colour of brick? As the rays which paſs thro' the greateſt tract of air, become reddiſh; thoſe which paſs thro' the leaſt, yellowiſh; and the intermediate ones, orange: the red muſt converge faſteſt into the ſhadow; after them, the orange; and laſtly, the yellow: ſo, that the whole ſpace of the earth's ſhadow, from the point of the cone to about   ſemidiameters from the earth, being filled with a faint light, whoſe colours verge always more to red in approaching the earth; the colour of the

moon,

moon, in total eclipſes, muſt needs vary likeways, according to her diſtance from the earth at the time of obſervation; and, if I miſtake not, be always more inclined to red at entering and leaving the ſhadow, than in the middle. Let Aſtronomers determine, whether the *phænomena* agree with this theory. It is not ſurprizing, that this refracted light is very faint and obſcure at the diſtance of the moon; ſince its mean denſity there, will be as much leſs than the denſity of the light of the ſetting ſun, as the annular ſpace of the lower air thro' which it paſſes, drawn into the moon's horizontal parallax, is leſs than the area of a great circle of the earth drawn into four times the exceſs of the horizontal refraction of the atmoſphere above the ſame parallax; that is perhaps as 1 to or more.

QUER. XVI. I have obſerved, when at ſea, that, tho' I preſſed my body and head firmly to a corner of the cabin, ſo as to be at reſt in reſpect of every object about me, the different irregular motions of the ſhip, in rolling or pitching, were ſtill diſcernible by the ſight: How is this fact to be reconciled to optical principles? Shall we conclude, that the eye, by the ſudden motions of the veſſel, veffel, is rolled out of its due pofition? Or, if it retains a fixed fituation in the head, is the perception of the fhip's motion owing to a vertigo in the brain, a deception of the imagination; or to what other caufe?

QUER. XVII. HAS not gold been reduced, by beating, to a degree of thicknefs little exceeding that which muft be afcribed to its colorific parts, according to Sir *Ifaac Newton*'s theory? But, how can it cohere into a continuous leaf, fo as to leave no vifible pores, unlefs there be many of its component particles contained in its thicknefs?

QUER. XVIII. WHEN one looks ftedfaftly at *Sirius* or any bright ftar not much elevated above the horizon, its colour feems not to be conftantly white, but appears tinctured, at every twinkling, alternately with red and blue: To what is this appearance owing? Is not the feparation of colours by the refraction of the atmofphere too fmall to be perceived?

QUER. XIX. BODIES become black by burning; becaufe they are reduced* into very fmall parts: but, whence is it, that

 moft

* *Newt.* Opt. Book iii. part 2. prop. 7.

most bodies, when further burned to ashes, assume a grey or whitish colour?

QUER. XX. SINCE the cause of blackness in bodies is the smallness of their transparent parts, which renders them incapable of reflecting any colour; how can black bodies, solid or fluid, be at the same time opaque? Can light be finally stifled by the refractive powers of the particles alone? or, ought it not rather to make its way thro' the body, if there be no reflexion, without any sensible loss, altho' the several rays might issue in various directions? And, may it not be demanded, in like manner, concerning all coloured opaqùe bodies, How all sorts of light can be stifled and stopt within a body, whose internal parts are fitted to reflect only one or two colours, and transmit all the rest?

QUER. XXI. IF the parts and pores of pellucid bodies be much less than the least interval between the fits of reflexion and transmission; it is plain, that rays of light, entering a part or pore in a fit of transmission, will not be reflected at its back surface: and thus it may be understood, how all rays that enter the first surface of a transparent body continue to be transmitted thro' its substance

to

to the greateſt diſtances, *viz.* if the rays are always put into a new fit of *moſt eaſy* tranſmiſſion at entering every new pore or particle. But is not that ſuppoſition contrary to what Sir *Iſaac* teaches elſewhere; That the fits of reflexion and tranſmiſſion continue to return at equal intervals, after a ray has entered a tranſparent body, and are thus regularly propagated to the greateſt diſtances*? And, if this be true, how can the rays be tranſmitted to any ſenſible diſtance, ſince they muſt often arrive in fits of eaſy reflexion at the common ſurfaces of pores and particles? But, altho' it could be underſtood by the doctrine of the fits in light why there is no reflexion from the interior parts of water and other pellucid *mediums* †, does not the rectilinear tranſmiſſion of light thro' theſe bodies in all directions, and conſequently in all degrees of obliquity, to their internal parts, prove, that theſe parts, upon account of their minuteneſs, loſe their powers of refraction as well as reflexion? And to what known property of light or bodies can this be attributed?

QUER,

* *Newt.* Opt. Book ii. part 2. prop. 12.

† Ibid. Book ii. part 3. prop. 4.

QUER. XXII. IF the fits are produced by an alternate acceleration and retardation of the particles of light, ſome of the particles, which are ſwift enough to be tranſmitted at the firſt ſurface of a tranſparent *medium*, muſt overcome the reflecting power more eaſily than others; namely, thoſe that happen to be in their point of greateſt celerity or neareſt to it: Now, muſt not rays that are moving with different velocities be differently bent from their courſe, as we argued above with reſpect to ſimple-coloured rays, by the ſame refractive power? Why therefore is not every beam of light, homogeneal or heterogeneal, diffuſed by refraction into innumerable rays, according to the reſpective velocities with which they entered the refracting ſurface? Is it a ſufficient anſwer to this query, That rays which are fartheſt from their point of greateſt ſwiftneſs will be moſt bent in a direction contrary to that of refraction, by the reflecting power, and will therefore only return to the direction of ſwifter rays by a greater degree of refraction?

QUER. XXIII. Sir *Iſaac Newton* juſtly argues, that light muſt be reflected at a diſtance from bodies; becauſe the moſt poliſhed ſurface,

face, being extremely rough and uneven in respect of the particles of light, would disperse them indifferently in all directions, if they rebounded from it by striking: But, will not the like difficulty still remain, *viz.* how light can be reflected or refracted regularly by the best-polished surface, if the power of the body proceeds from an attraction or repulsion belonging to each physical point? It might be perhaps supposed, that the repulsive power produces reflexion at a distance so great, in respect of the inequalities that are left in polished bodies, that the direction of force, resulting from their joint action, may be very nearly perpendicular to the general surface of the body; and this might tend to account for the regular reflexion from the anterior surface of a denser *medium*. But, will this supposition suffice for explaining the regularity of refraction, and of reflexion, from the posterior surface of a denser *medium*: in both which cases, the light must actually enter the pores of the attracting body, and therefore approach much nearer to one inequality than another; since the pores, by which it enters, are certainly much less than those inequalities? In

water

water and other tranſparent liquors, this muſt certainly be the caſe, if their globular particles touch one another, as is commonly concluded from their incompreſſibility: for, as a number of ſpheres laid together leave no rectilinear paſſages between them, the tranſmitted light muſt paſs thro' the component particles; and therefore the pores, by which it enters, muſt be much leſs than the whole hemiſpherical ſurfaces of the particles which evidently conſtitute the inequalities of the general ſurface of the liquor *.

QUER. XXIV. How does light preſerve its rectilinear courſe in paſſing thro' air, *æther* and other elaſtic fluids? Will not the difficulty ſtill continue, whatever ſubtility or rarity is aſcribed to theſe *mediums*; ſince the powers from whence their elaſticity ariſes, muſt prevail thro' all the free ſpaces that interveen their particles? Muſt we not, therefore, ſuppoſe, that the rays of light are not ſubject

* We are certain, that the inequalities of a craggy rock or rough wall are much greater than the particles of air or their diſtance from one another, by which their repulſive powers are probably terminated (*Newt.* Princip.): Why is ſound, therefore, reflected ſo regularly from ſuch bodies, that the echo is faintly heard, except at an angle of incidence equal to the angle of reflexion?

ſubject to theſe repulſive powers, tho' they paſs thro' the ſphere of their action? Does not the refraction of light towards the perpendicular, out of the celeſtial ſpaces into air, even prove that it is attracted by the particles of air? Would it not be extravagant and incongruous beyond meaſure, to imagine the *æther* ſo ſubtile, in reſpect of light, that, tho' it be driven out of the way by the rays, as air is by common projectiles, it is not capable of retarding them ſenſibly in their motion from the moſt diſtant fixed ſtars to our eye? Do not theſe and many other difficulties, in the phyſical part of Optics, whoſe ſolution is ſought for in vain from any principles hitherto diſcovered, ſhew the neceſſity of extending our views and inlarging our ſtock of principles by further experimental inquiry? Such objections are not to be conſidered as demonſtrations of the falſehood of our preſent theory; but as proofs of its narrowneſs, partiality and imperfection.

DES CARTES, contenting himſelf with a ſuperficial and inaccurate knowledge of the laws of impulſe, vainly dreamed, that he had got poſſeſſion of the univerſal cauſe

from

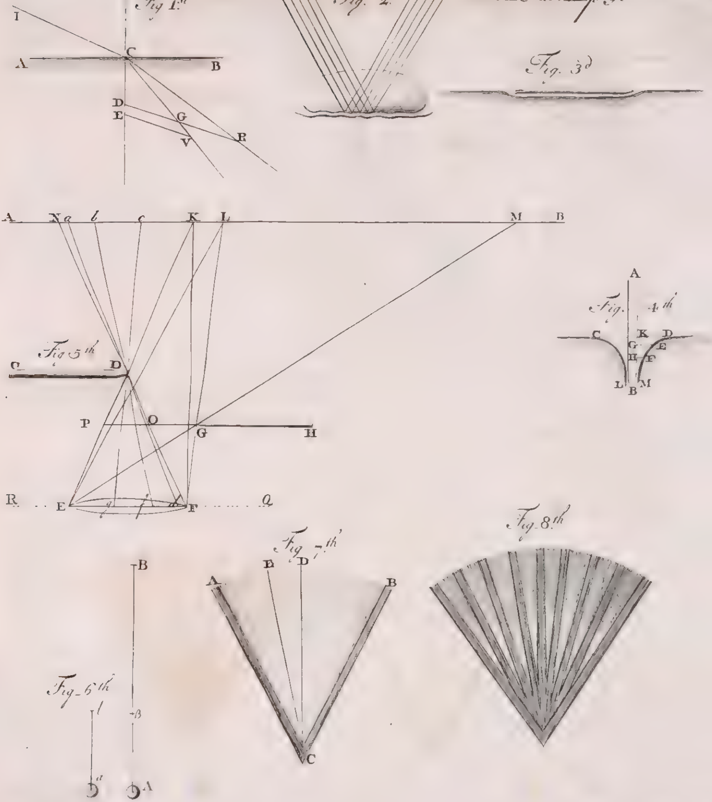
from whence all effects in Nature are derived; when, in truth, he was unable to deduce from them the simplest cases of collision. Many in this age, who write and speculate on physical subjects, seem to fall into a like error; while they employ their whole study in endeavouring to reconcile all *phænomena* with the new principles discovered by Sir *Isaac Newton:* and, when they find, to their mortification, that this will not always succeed; *phænomena* must be disguised, and Nature tortured, to hide their ignorance. From the lazy method of philosophizing in the closet, among books and diagrams, there never arose, there never will arise, any discovery of consequence: Great inventors usually understand the extent of their own principles too well, to leave much of the application of them to others.

The discovery of the different refrangibility of the rays, was an inestimable addition to natural knowledge; as it serves, at once, for explaining innumerable *phænomena* in Nature which flow from it as immediate and necessary consequences: and, if it shall be demonstrated by the observation proposed in N° 49. that the differently-coloured rays really

really move with different velocities, our theory of light will be ſtill farther improved; as the different refrangibility can be thence mechanically explained.

THE whole ſyſtem of Nature is one immenſe ſeries of cauſes and effects, whoſe beginning and end are equally hid in the depths of infinity. Only a ſmall, a very ſmall portion of it, comes under our immediate obſervation; being expoſed alike to the ſight and other ſenſes of all mankind. Almoſt every *phænomenon* is, at once, the cauſe of manifold effects; and one effect, among many, of a ſuperior cauſe. The buſineſs of Science is to extend our views, by unfolding the latent cauſes which exiſt in Nature; and thence explaining their manifeſt effects. The diſcovery of one ſuch real cauſe, unknown before, if it be of general or very extenſive influence, as that of univerſal gravity, is to be eſteemed a great advancement of natural philoſophy. To undervalue ſuch a diſcovery, as ſome have done, becauſe the cauſe of that cauſe cannot yet be aſſigned, is highly abſurd: ſince the ſame objection muſt for ever ly againſt all caufes, except *primary* ones; which are certainly removed far beyond the reach of human inquiry. The proper office, and higheft boaft of true philofophy, is, to bring us ftill nearer to the DEITY, by leading us upwards, ftep by ftep, in the mighty *fcale* of Nature.

ART.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ART. V.

*An easy Method of computing the Parallaxes of the Moon**; *by*——

TAB. IV. *fig.* 1. 1. IF from ⊙, the center of the ſun, a right line ⊙W be carried round, always touching the earth's ſurface, this line will form a conical ſurface; which, being cut, by a plane paſſing through the center of the earth, at right angles to the line joining the centers of the ſun and earth, the ſection, ſo made, is the diſk of the earth.

2. THE ſame conical ſurface will cut off a circular portion of the moon's ſphere, $w\ n\ e\ s$; within which, any arches, intercepted by lines drawn from the center of the ſun and extended to the diſk of the earth, will be nearly in the ſame proportion to one another, as the reſpective diſtances intercepted on the diſk.

3. HENCE, c being the center of the circular portion, wce the ecliptic; if the ſemidiameter of the diſk be expreſſed by the number of ſeconds in the arch cw or ce = horizontal parallax

* November 6th, 1755.

rallax of the moon from the ſun, every line drawn on the diſk will be expreſſed by the ſeconds of its correſpondent arch of the moon's ſphere.

4. Let C be the center of the diſk, W E, N S, the projections of *w e* the ecliptic, and *n s* a circle of latitude; W N E being the upper or northern ſemicircle, and W S E the lower or ſouthern. Let V be the place of any given vertex on the diſk, and *v* the correſpondent point in the moon's ſphere; V A, V B, perpendiculars to N S, W E, and *v a*, *v b* their correlatives. If the point *v* be the true place of the moon, *i. e.* if the viſible places of the ſun and moon be the ſame, then will V A be the par. lon. ☽ *à* ☉, and V B the par. lat. to a ſpectator at the point V on the diſk, or at the given vertex on the ſurface of the earth. For the par. lon. ☽ *à* ☉ is the difference of the viſ. lon. ☽ *à* ☉ obſerved at V and C: but the viſ. long. ☽ *à* ☉ obſerved at C, is the ſame as if ſeen from A; therefore the par. lon. ☽ *à* ☉ is equal to the difference of the viſ. lon. ☽ *à* ☉ obſerved at V and A, which is equal to the arch *v a* the meaſure of V A. In like manner, V B may be proved

to

to be the par. lat. ☽ *à* ☉; the viſ. lat. being the ſame when obſerved at B or C.

5. In any other poſition of the moon, if its diſtance from the point v exceeds not one degree, which it can never do in the time of an eclipſe; the parallaxes, to a ſpectator at the point V of the diſk, will continue nearly the ſame as before, without any ſenſible alteration. Let l be the place of the moon, $l\,d$, $l\,f$, perpendiculars to $c\,n$, $c\,e$, and let $f\,l$, $v\,a$, be produced till they meet in the point g, and let $l\,d$ meet with $v\,b$ in the point t; the viſ. lon. ☽ *à* ☉, will be nearly the ſame at the points g and l. Therefore the viſ. long ☽ *à* ☉, obſerved at V, is the angle under which the arch $v\,g$ is ſeen from that point. But the arch $v\,g$ will be ſeen nearly under the ſame angle from the points V and C; and conſequently it is the meaſure of the viſ. lon. ☽ *à* ☉. But the true lon. is $d\,l = a\,g$: therefore $v\,g - a\,g = v\,a$ is the par. lon. ☽ *à* ☉. Again, $a\,d = l\,g =$ viſ. lat. ☽ *à* ☉; and $c\,d = f\,l =$ tr. lat.: therefore $v\,b = a\,c = a\,d + d\,c =$ par. lat. ☽ *à* ☉.

6. But theſe parallaxes $v\,a$, $v\,b$, and the viſ. lon. and lat. $v\,g$, $v\,t$, ſuppoſe the ſpectator at the point V on the diſk; whereas his

true

true place is at the correfpondent point of the furface of the earth; confequently, the vif. lon. and lat. ☽ à ☉, vg, vt, muft be increafed in the ratio of the diftance of the point v from the fpectator's place on the furface, to its diftance from the point V on the difk. But, as the horizontal femidiameter of the moon fhould likewife be increafed in the fame ratio, it will equally anfwer the purpofe of finding the times and phafis of an eclipfe, to let thefe continue unaltered, and to diminifh the fun's apparent femidiameter in the fame ratio.

7. Let C P be the axis of the earth projected on the difk, F M D the ellipfe into which the parallel of the given place is projected, D F its greater axis, O M its leffer femiaxis, V G perpendicular to C O; M H, G K, perpendiculars to C W, and M I, G L, at right angles to C N; and, for fhortening the rules, put C W, the femidiameter of the difk, or the horizontal parallel ☽ à ☉, equal to the radius of the tables of fines and tangents.

8. For computing the parallaxes V A, V B, it will be convenient firft to fuppofe the fun in the meridian, and the place of the vertex

tex at M; then to compute the variation of parallaxes, for the given time, from noon.

9. THE meridian par. lon. ☽ à ☉ is MI or CH; and the meridian par. lat. ☽ à ☉ is MH or CI: for finding which the requiſites are,

$$\left.\begin{array}{l}\left.\begin{array}{l}\text{Sine}\\ \text{Coſ.}\end{array}\right\}\text{ of the lat. of the place,}\\ \left.\begin{array}{l}\text{Sine}\\ \text{Coſ.}\end{array}\right\}\text{ of the declination of the ſun,}\\ \left.\begin{array}{l}\text{Sine}\\ \text{Coſ.}\end{array}\right\}\text{ of PCN = complement of the meridian angle,}\end{array}\right\}\begin{array}{l}\text{To be found}\\ \text{from the}\\ \text{Tables.}\end{array}$$

$$\text{OC} = \frac{\text{S, lat. pl.} \times \text{coſ. decl.} \odot}{\text{R}} \begin{Bmatrix}\text{North}\\ \text{South}\end{Bmatrix} \text{ if the lat. pl. be } \begin{Bmatrix}\text{North.}\\ \text{South.}\end{Bmatrix}$$

$$\text{OM} = \frac{\text{Coſ. lat. pl.} \times \text{S, decl.} \odot}{\text{R}} \text{ of a contr. ſpec. to the decl. } \odot.$$

$$\text{CM} = \begin{Bmatrix}\text{Diff.}\\ \text{Sum}\end{Bmatrix} \text{ of OC and OM, if they are of } \begin{Bmatrix}\text{the ſame}\\ \text{different}\end{Bmatrix} \text{ Species.}$$

$$\text{Merid. par. lon. ☽ à } \odot = \text{MI} = \text{HC} = \frac{\text{CM} \times \text{coſ. merid. ang.}}{\text{R}}$$

$$\begin{Bmatrix}\text{Eaſt}\\ \text{Weſt}\end{Bmatrix} \text{ from the } \begin{Bmatrix}\text{Summer}\\ \text{Winter}\end{Bmatrix} \text{ to the } \begin{Bmatrix}\text{Winter}\\ \text{Summer}\end{Bmatrix} \text{ ſolſtice if CM is } \begin{Bmatrix}\text{North.}\\ \text{South.}\end{Bmatrix}$$

$$\text{Merid. par. lat. ☽ à } \odot = \text{MH} = \text{IC} = \frac{\text{CM} \times \text{S, merid. ang.}}{\text{R}} \begin{Bmatrix}\text{of a contr.}\\ \text{ſpec. to CM.}\end{Bmatrix}$$

10. THE variations from the merid. parallaxes for the given hour from noon, may be conceived to be made up, each of two parts; which are the projections of MG and GV upon CW for the lon. and upon CN for the lat.: the former being HK, KB;

K B; and the latter I L, L A. For finding which, besides the foregoing requisites, there is required

$\left.\begin{array}{l}\text{Sine}\\ \text{Verſ. Sine}\end{array}\right\}$ of the degrees from noon.

$$MG = \frac{OM \times \text{verſ. S, deg. à noon}}{R}$$

$$GV = \frac{\text{Coſ. lat. pl.} \times \text{S, deg. à noon}}{R}$$

Variation of the PAR. LON. ☽ *à* ⊙.

$$\text{1ſt PART} = \frac{MG \times \text{coſ. merid. ang.}}{R}$$

$\left.\begin{array}{l}\text{Weſt}\\ \text{Eaſt}\end{array}\right\}$ when the ſun is in $\left\{\begin{array}{l}\text{♑ ♒ ♓ ♈ ♉ ♊.}\\ \text{♋ ♌ ♍ ♎ ♏ ♐.}\end{array}\right.$

$$\text{2d PART} = \frac{GV \times \text{S, merid. ang.}}{R} \left\{\begin{array}{l}\text{Eaſt AM.}\\ \text{Weſt PM.}\end{array}\right.$$

Variation of the PAR. LAT. ☽ *à* ⊙.

$$\text{1ſt PART} = \frac{MG \times \text{S, merid. ang.}}{R} \left\{\begin{array}{l}\text{South}\\ \text{North}\end{array}\right\} \text{contrary to the decl. ⊙.}$$

$$\text{2d PART} = \frac{GV \times \text{coſ. merid. ang.}}{R}$$

Forenoon $\left\{\begin{array}{l}\text{South}\\ \text{North}\end{array}\right\}$ when the ſun is in $\left\{\begin{array}{l}\text{♑ ♒ ♓ ♈ ♉ ♊.}\\ \text{♋ ♌ ♍ ♎ ♏ ♐.}\end{array}\right.$

Afternoon $\left\{\begin{array}{l}\text{North}\\ \text{South}\end{array}\right\}$ when the ſun is in $\left\{\begin{array}{l}\text{♑ ♒ ♓ ♈ ♉ ♊.}\\ \text{♋ ♌ ♍ ♎ ♏ ♐.}\end{array}\right.$

11. BY

11. By art. 6. the part to be ſubtracted from the apparent ſemid. ⊙, muſt bear the ſame proportion to the whole, as the diſtance of the vertex on the ſurface of the earth from its point on the diſk, to the diſtance of the points V, *v*; which is nearly the ſame with the ſemidiameter of the moon's orbit, and may always be expreſſed by a conſtant number, *viz.* that of the 2ds of a degree equal to the radius of a circle. The diſtance of the vertex from its point on the diſk may be conceived to be made up of two parts, *poſitive*, or *negative*: the firſt of which is the perpendicular diſtance of the centre of the given parallel circle from the plane of the diſk; the ſecond part is the perpendicular diſtance of the given vertex from a plane paſſing thro' the foreſaid centre, and parallel to the diſk.

1ſt Part $= \dfrac{\text{S, lat. pl.} \times \text{S, decl.} \odot}{\text{R}} \left\{\begin{matrix}\text{Poſ.}\\ \text{Neg.}\end{matrix}\right\}$ when the lat. pl. and decl. ⊙ are of $\left\{\begin{matrix}\text{the ſame}\\ \text{different}\end{matrix}\right\}$ Species.

2d Part $= \dfrac{\text{Coſ. lat. pl.} \times \text{coſ. decl.} \odot \times \text{coſ. deg. } \grave{a} \text{ noon}}{\text{RR}}$

$\left.\begin{matrix}\text{Poſitive}\\ \text{Negative}\end{matrix}\right\}$ if the given hour from noon be $\left\{\begin{matrix}\text{leſs}\\ \text{more}\end{matrix}\right\}$ than 6.

12. It would be eaſy, from the foregoing rules, to conſtruct tables of the parallaxes for any given latitude. Such tables would be ſufficiently exact, if calculated for every third degree of the ſun's longitude, and every quarter of an hour from noon to 6 hours, aſſuming 10000 for radius, or the horiz. par. ☽ *à* ☉. The equation to be ſubtracted from the apparent ſemid. ☉ needs only be calculated for every 10th degree of longitude, and every half hour from noon to 6 hours, taking the mean ſemid. ☉.

13. If the given time from noon be more than 6 hours, ſubtract it from 12 hours, and the lon. ☉ from 12 ſigns; and, for the remaining time and longitude, ſeek the intire parallax, and the equation for the ſemid. ☉, changing its ſign.

14. Such tables, tho' conſtructed for a particular latitude, may eaſily be made to ſerve for any other latitude. For the variations from the meridian parallaxes are always proportional to the coſine of latitude. And the meridian parallax, whether of lon. or lat. is made up of the ſum or difference of two parts; the firſt of which is proportional to the ſine, the ſecond to the coſine of lat.

These

Thefe two parts for the tabular latitude are found thus. To the given lon. ☉ add 6 figns; and take out the merid. parallax anfwering to each longitude. Half the fum of thefe gives the 1ft part, and half their difference the 2d part, to be added to the firft when the merid. par. for the given lon. ☉ is greater than the other, and to be fubtracted when lefs. In like manner, the equation for the femid. ☉ confifts of two parts; the 1ft of which is proportional to the fine, the 2d to the cofine of lat. The 1ft part is the equation for the given lon. ☉, and 6 hours from noon; and this fubtracted from the equation for the given lon. ☉ and hour from noon, gives the 2d part.

15. THE fame rules will ferve for finding the moon's parallaxes from a fixt ftar or planet. If the femid. of the planet have no fenfible magnitude, there will be no place for the correction mentioned in art. 6. But, as the requifites could not be found from the common tables, if the ftar has latitude, it will be convenient, firft, to fuppofe it has none, and afterwards to apply a proper correction. Befides, the proceeding in this way will make the tables already defcribed to be

of

of the ſame uſe as in ſolar eclipſes, ſubſtituting the lon. ✱ for the lon. ☉, and, for the hour from noon, the equatorial diſtance of the ſtar's ecliptic place from the meridian, converted into time at the common rate of 15 degrees to an hour.

16. IF the ſtar have latitude, then the true lon. and lat. ☽ *à* ✱ are meaſured by arches, from the center of the moon, perpendicular to a circle of latitude, and to a great circle at right angles to it, both paſſing thro' the ſtar. Hence,

As radius to coſ. true lat. ☽, ſo tr. lon. ☽ *à* ✱ to the ſame projected. And,

As coſ. tr. lon. ☽ *à* ✱ to radius, ſo tan. tr. lat. ☽ to the ſame projected: or, without any ſenſible error, ſo tr. lat. ☽ to the ſame projected.

17. To find the correction of the parallaxes for the latitude of a ſtar: let CW (*fig.* 2.) be the ecliptic on the earth's ſurface, N its pole; CN a circle of latitude thro' the ſtar; W its pole; V the given vertex, CD, the lat. ✱; WVA a great circle thro' the vertex V, meeting with the circle of

of latitude in A, WD another great circle; VB, V*b*, arches perpendicular to WC, WD. Then WA is a quadrant, and the ſine of VA is the par. lon. ☽ *à* ✱, whether the ſtar has latitude, or not: the ſine of VB is the par. lat. if the ſtar be in the ecliptic; but, if it is in the circle WD, the par. lat. will be the ſine of V*b*; which may be found from the parallaxes of lon. and lat. given for the ecliptic place of the ſtar: thus,

$$\text{S, WV} = \text{Coſ. VA} = \sqrt{\overline{\text{R} + \text{par. lon.}} \times \overline{\text{R} - \text{par. lon.}}}$$

$$\text{S, VWB} = \frac{\text{R} \times \text{S, VB (par. lat.)}}{\text{S, WV}}$$

$$\text{VW}b = \text{VWB} \left\{\begin{matrix} + \\ - \end{matrix}\right\} \text{CWD, if the lat} \ast \text{ be} \left\{\begin{matrix} \text{South.} \\ \text{North.} \end{matrix}\right.$$

$$\text{S, V}b = \text{par. lat. ☽ } \grave{a} \ast = \frac{\text{S, WV} \times \text{S, VW}b}{\text{R}}$$

18. Since the radius of the tables of ſines and tangents was all along taken for the horiz. par. ☽ *à* ⊙ or ✱; the parallaxes and equation found muſt be altered proportionally.

EXAM-

EXAMPLES.

The beginning of the solar eclipse, July 14, 1748. *was observed by Doctor* BRADLEY, *at* Greenwich, *to be* $9^h\ 4'\ 30''$, a. m.

REQUISITES.

Sine	lat. of the place,	51^h	$29'$	$00''$	9.8934439
Cos.					9.7943083
Sine	decl. N. of the sun,	19	36	00	9.5256298
Cos.					9.9740774
Sine	meridian angle,	76	49	07	9.9884042
Cos.					9.3580017
Sine	degrees from noon,	43	52	30	9.8407880
V.s.					9.4458320

S, lat. pl.	9.8934439	V. Si. deg. *à* noon	9.4458320
Cos. decl. ☉	9.9740774	OM	9.3199381
OC = 73709 N.	9.8675213	MG	8.7657701
Cos. lat. pl.	9.7943083	Cos. lat. pl.	9.7943083
S, decl. ☉	9.5256298	S, deg. *à* noon	9.8407880
OM = 20890 S.	9.3199381	GV	9.6350963
CM = 52819 N	9.7227902		

Cof. merid. ang.		9.3580017
CM		9.7227902
Merid. par. lon. ☽ à ⊙, E.	12045, E,	9.0807919

S, merid. ang.		9.9884042
CM		9.7227902
Merid. par. lat. ☽ à ⊙, S.	= 51427, S.	9.7111944

Variation of PAR. LON.

Cof. merid. ang.		9.3580017
MG		8.7657701
1ft PART = 1330, E.		8.1237718
S, merid. ang.		9.9884042
GV		9.6350963
2d PART = 42024, E.		9.6235005
Total	43354, E.	
Merid. par.	12045, E.	
Par. lon.	55399, E.	9.7435019
Horiz. par. ☽ à ⊙ = 53′ 58″		3.5103768
Tr. par. lon. ☽ à ⊙ = 29 54, E.		3.2537787
Tr. lon. ☽ à ⊙, W. = 58 34, W.		
Vif. lon. ☽ à ⊙, W. = 28 40, W.		

Variation of PAR. LAT.

S. merid. ang.		9.9884042
MG		8.7657701
1ft PART = 5678, S.		8.7541743
Cof. merid. ang.		9.3580017
GV		9.6350963
2d PART = 9842, N.		8.9930980
Total	4164, N.	
Merid. par.	51427, S.	
Par. lat. =	47263, S.	9.6745213
Horiz. par. ☽ à ⊙		3.5102768
Tr. par. lat. ☽ à ⊙ = 25′ 30″, S.		3.1847981
Tr. lat. ☽ =	33, 56, N.	
Vif. lat. ☽ à ⊙ =	8, 26, N.	

Dist. VERTEX *from the* DISK.

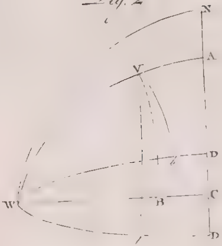
Cof. lat. pl.	9.7943083	S, lat. pl.	9.8934439
Cof. decl. ☉	9.9740774	S, decl. ☉	9.5256298
Cof. deg. *à* noon	9.8578476	1ft PART = 26247,	9.4190737
	9.6262333	2d PART = 42290.	

Total dift. — — — —	68537, —	9.8359251
2ds in radius, co. ar. — — — —		4.6855748
Horiz. par. ☽ *à* ☉ — — —		3.5102768
Semid. ☉ — — —	15′ 52″ —	2.9786369
		1.0104136
2ds to be fubtracted — —	10	
Curtate femid. ☉ — — —	15 42	

N. B. A late pamphlet on this fubject having been publifhed, (intitled, *A new and compendious Method of inveftigating the Parallactic Angle, without regard to the Nonagefimal Degree.* Addreffed to the R. Hon. the E. of *Macclesfield*) and afterwards inferted in the philofophical tranfactions: the Author of the foregoing paper thinks proper to intimate, that he had the fubftance of it by him in writing, long before he faw that pamphlet. And, tho' the two methods may feem to be pretty fimilar, yet he thinks the operations by the foregoing rules will, upon comparifon, be found to be rather more accurate, and, at the fame time, much eafier; as the trouble of working by the tables of fines and tangents is here, in a great ..e, avoided.

Fig. 1

Fig. 2



ART. VI.

A Solution of Kepler's *Problem; by* MATTHEW STEWART, *Professor of Mathematics in the University of* Edinburgh *.

THO' the problem proposed by the celebrated *Kepler* has been solved by several Mathematicians of great note: yet, as this is of considerable use in astronomy, it is hoped the following solution may be agreeable to some; as it requires a less degree of knowledge in the more difficult parts of Mathematics, than the other methods do.

PROP. I. *Tab.* V. *Fig.* 1.

Let there be a streight line AB, and CD a portion of a curve wholly concave towards AB, and draw AC, BD parallel to each other, meeting the curve in C, D; let CE, a tan-

 gent

* December 4, 1755.

gent to the curve at C, meet BD in E; join AD, AE, BC, and let AE meet the curve in F: the triangle ABC will be greater than the ſector ACF, but leſs than the ſector ACD.

Join CD. Becauſe AC, BE, are parallel, the triangles, ABC, AEC, will be equal: but the triangle AEC is greater than the ſector ACF; therefore the triangle ABC is greater than the ſector ACF. Again, becauſe AC, BD, are parallel, the triangles, ABC, ADC, are equal: but the triangle ADC is leſs than the ſector ACD; therefore the triangle ABC is leſs than the ſector ACD.

PROP. II. *Fig.* 2.

Let there be a curve AEB, wholly concave towards the ſtreight line AB, and let C, D, be two points in the line AB; draw DE to any point

E

E in the curve, and draw CF parallel to DE, meeting the curve in F; let EG, a tangent to the curve at E, meet CF in G, and join DF, DG, and let DG meet the curve in H; let the point D be between the points, A, C: the ſector ACE will be greater than the ſector ADH, but leſs than the ſector ADF; and the ſector BCE will be greater than the ſector BDF, but leſs than the ſector BDH.

Because the triangle DEC is greater than the ſector EDH, let the ſector ADE be added to both; and the ſpace ACE will be greater than the ſector ADH: and, becauſe the triangle DEC is leſs than the ſector EDF, let the ſector ADE be added to both; and the ſector ACE will be leſs than the ſector ADF.

Again, becauſe the ſector ACE together with the ſector BCE, is equal to the ſector ADF together with the ſector BDF, and the ſector

ſector ACE is leſs than the ſector ADF; therefore the ſector BCE is greater than the ſector BDF: and, becauſe the ſector ACE together with the ſector BCE, is equal to the ſector ADH together with the ſector BDH, and the ſector ACE is greater than the ſector ADH; therefore the ſector BCE will be leſs than the ſector BDH.

PROP. III. PROBL. I. *Fig.* 3.

Let there be a given circle AEB, and let D be a given point within the circle; and from D let there be drawn DE, DF, to two given points, E, F, in the circle: granting the quadrature of the circle, it is required to draw a line DG meeting the arc FE in G; ſo that the ſector EDG may be to the ſector GDF in a given ratio, ſuppoſe that of m to n.

LET C be the center of the circle, and join CE, CF; draw DH, DK, perpendicular to CE, CE, CF, meeting CE, CF in H, K; and let EL, FM, be tangents to the circle at the points, E, F.

BECAUSE the points, D, E, F, are given, and the circle is given, the fector DEF will be given; and, becaufe the fector EDG is to the fector GDF in the given ratio of m to n, the fectors, EDG, GDF, will, each of them, be given in magnitude. In the tangent EL take the point L towards F; fo that, joining HL, the triangle HEL may be equal to the fector EDG: again, in the tangent FM take the point M towards E; fo that, joining KM, the triangle KFM will be equal to the fector FDG: it is evident, the points, L, M, will be given. Join DL, DM, meeting the circle in N, O; the point G will fall between the points, N, O. For, becaufe EL, FM, are tangents to the circle at E, F, the angles HEL, KFM, will be right; and therefore DH, EL, will be parallel: likeways DK, FM, will be parallel; therefore the triangles, DEL, HEL, will be equal, and likeways the triangles, DFM, KFM, will be equal: and therefore the triangle DEL will be equal to the fector DEG, and the triangle DFM equal

to

to the ſector DFG; therefore the ſector DEG will be greater than the ſector DEN, and the ſector DFG greater than the ſector DFO; therefore the line DG will fall between the lines, DN, DO; therefore the point G will fall between the points, N, O.

AGAIN, Becauſe the points, L, M are given, and the point D likeways given, the ſectors, DEN, DFO, will be given; and, becauſe the ſectors, DEG, DFG, are given, the ſectors, DNG, DOG, will be given; and therefore the ſector NDG will be to the ſector GDO in a given ratio, ſuppoſe that of p to q. The problem, therefore, will be reduced to this again, To divide the ſector NDO by the line DG, ſo that the ſector NDG may be to the ſector GDO in the given ratio of p to q. And thus, repeating the operation, the point G will be found within a very narrow limit.

N. B. THE limit ON might have been found by taking in the tangents, EL, FM, the point L towards F, and the point M towards E; ſo that, joining DL, DM, meeting the circle in N, O, the triangle DEL would be equal to the ſector EDG, and the triangle DFM

DFM equal to the ſector DFG: but the other way points out the method of calculation.

PROP. IV. PROBL. 2. *Fig.* 4.

Let there be a ſemicircle whoſe diameter is AB and center C, and let D be a point in the diameter: granting the quadrature of the circle, it is required to draw a line DE meeting the circle in E, ſo that the ſemicircle may be to the ſector BDE in a given ratio, ſuppoſe that of p to q.

SUPPOSE the problem ſolved. Let the ſemicircle be to the ſector BCF as p is to q; join DF; draw CG parallel to DF, meeting FG, a tangent to the circle at F, in G, and join DG meeting the circle in H; let CG meet the circle in K, and join DK, FK, CH.

BECAUSE the ſemicircle is to the ſector BDE as p to q, that is, as the ſemicircle to the ſector

ſector BCF, the ſector BDE will be equal to the ſector BCF: but the ſector BCF is [*Prop.* 2.] greater than the ſector BDK, but leſs than the ſector BDH; therefore the ſector BDE is greater than the ſector BDK, and leſs than the ſector BDH; therefore the line DE falls between the lines, DH, DK; and therefore the point E falls between the points, H, K. Again, becauſe the ſector BDE is equal to the ſector BCF, and the ſector BCK is common to both, the ſpace KCDEK will be equal to the ſector KCF; and, becauſe the triangles, KCD, KCF, are equal, (becauſe KC, DF, are parallel), therefore the ſector KDE is equal to the ſpace KEFK; and, becauſe the triangles, GDK, GFK, are equal, taking the common ſpace GHK from both, the ſector KDH will be equal to the ſpace KEFK together with the ſpace GFHG; but the ſector KDE is equal to the ſpace KEFK, therefore the ſector EDH is equal to the ſpace GFHG.

Because the ſemicircle is to the ſector BCF as p to q, therefore the angle BCF is given, and therefore the angle FCD is likeways given: and, in the triangle FCD, becauſe the ſides, CF, CD, are given, and like-

ways

ways the angle FCD, the angles CDF, CFD will be given; and therefore the angles BCK, KCF will be given; therefore the arcs BK, KF are given: becaufe FG is a tangent to the circle at the point F, the angle CFG will be a right angle: and becaufe the angle FCG is given, and likeways the fide CF given, the fides CG, CF will be given: in the triangle GCD, becaufe the fides GC, CD are given, and the angle GCD given, the angles CGD, CDG will be given: and in the triangle GCH, becaufe the fides GC, CH and the angle CGH is given, the angle GCH will be given; and therefore the arc HK will be given: and becaufe the arc FK is given, therefore the arc FH is given. Again, becaufe the arc FK is given, the fector FCK will be given, and likeways the triangle FCK given, therefore the fpace KEFK will be given; and becaufe the triangles, GFC, GHC, are given, the fpace GFCHG will be given; and becaufe the arc FH is given, the fector FCH will be given; therefore the fpace GFHG will be given.

From this, the following construction may be deduced.

CONSTRUCTION.

In the ſemicircle take the point F, and let the arc AFB be to the arc BF as p is to q; join CF, DF, and draw CG parallel to DF, meeting FG, a tangent to the circle, at F in G; and the circle in K; join DG meeting the circle in H, and join FK, DK; draw the line DE [*Prop.* 3.] meeting the circle in E, ſo that the ſector KDE may be to the ſector EDH as the ſpace KEFK to the ſpace GFHG: the ſemicircle will be to the ſector BDE as p is to q.

BECAUSE the ſector KDE is to the ſector EDH, as the ſpace KEFK to the ſpace GFHG; the ſector EDH will be to the ſector KDH, as the ſpace KEFK to the ſum of the ſpaces KEFK, GFHG: but, becauſe the

the triangles, GDK, GFK are equal, and the ſpace GHK common to both, the ſector KDH will be equal to the ſum of the ſpaces KEFK, GFHG; therefore the ſector KDE is equal to the ſpace KEFK. And, becauſe the triangle KDC is equal to the triangle KFC, the ſpace KCDE will be equal to the ſector KCF; therefore (adding the ſector BCK to both) the ſector BDE will be equal to the ſector BCF. Again, becauſe the arc AFB is to the arc BF as p is to q, the ſemicircle will be to the ſector BCF as p is to q; therefore the ſemicircle will be to the ſector BDE as p is to q. Q. E. D.

BUT, as this would require a good deal of trigonometrical calculation, the following method may be uſed; which will give the point ſought very nearly, when this problem is of uſe in the planetary ſyſtem.

PROP. V. PROBL. 3. *Fig.* 5.

Let there be a ſemicircle whoſe diameter is AB and centre C, and let D be a point in the diameter not

very

very excentric; granting the quadrature of the circle, it is required to draw a line, DE, meeting the circle in E, ſo that the ſemicircle may be to the ſector BDE in a given ratio, ſuppoſe that of p to q.

Suppose the problem ſolved; and let the ſemicircle be to the ſector BCF as p is to q; join DF, and draw CG parallel to DF meeting FG a tangent to the circle at F in G; join DG, and let CG, DG meet the circle in H, K: join DH, FH; draw CM parallel to DH meeting HM a tangent to the circle at H in M; draw DL perpendicular to CH meeting CH in L; join LE, and let FN perpendicular to CH meet CH in N.

Because the ſemicircle is to the ſector BDE as p is to q, that is, as the ſemicircle to the ſector BCF, the ſector BDE will be equal to the ſector BCF: and becauſe the ſector BCH is common to both, the ſpace HCDE will be equal to the ſector HCF; but becauſe CH, DF are parallel, the triangle DCH is equal to the triangle FCH; therefore

fore the ſector EDH will be equal to the ſpace contained by the arc HF and the chord FH. Again, becauſe the ſector BDE is equal to the ſector BCF, and the ſector BCF is [*Prop.* 2.] greater than the ſector BDH, and leſs than the ſector BDK, the ſector BDE will be greater than the ſector BDH, and leſs than the ſector BDK: therefore the point E is in the arc HK between the points H, K. [Let the arc HK be called the limiting arc]. Becauſe the point D is not very excentric, the limiting arc will be little. In the orbit of *Mercury*, the moſt excentric of all the planets, the limiting arc will be leſs than ſixteen minutes in that part of the orbit where it is greateſt; therefore EH may be conſidered as coinciding with the tangent to the circle at the point H; and therefore the triangle ELH will be equal to the ſector EDH, that is, equal to the ſpace contained by the arc FH and the chord FH; therefore the rectangle LHE will be double of the ſpace contained by the arc FH and the chord FH. Becauſe the rectangle contained by CH and the arc FH is double of the ſector CFH, and the rectangle contained by CH, FN is double of the triangle CFH; the

rectangle

rectangle contained by CH and the excess of the arc FH above FN will be double of the space contained by the arc FH and the chord FH; therefore the rectangle LHE will be equal to the rectangle contained by CH and the excess of the arc FH above FN; therefore LH will be to HC as the excess of the arc FH above FN to HE: but because the triangles DLH, CHM are similar, LH will be to HC as DL or FN to HM; therefore FN will be to HM as the excess of the arc FH above FN to HE.

From this, the following construction may be deduced.

CONSTRUCTION.

Let the semicircle be to the sector BCF as p is to q; join DF, draw CH parallel to DF meeting the circle in H; join DH, draw CM parallel to DH meeting HM a tangent to the circle at H in M; and let FN perpendicular to CH meet CH in N; in the tangent HM

HM take HE towards F, ſo that FN may be to HM as the exceſs of the arc FH above FN to HE; join DE: the ſemicircle will be to the ſector BDE as *p* is to *q*.

Let DL, perpendicular to CH, meet CH in L; join FH, LE. Let CH meet FG, a tangent to the circle at F, in G; join DG meeting the circle in K.

Because the triangles DLH, CHM are ſimilar, LH will be to HC as DL or FN to HM; that is, as the exceſs of the arc FH above FN to HE: therefore the rectangle LHE will be equal to the rectangle contained by HC and the exceſs of the arc HF above FN: but, becauſe the rectangle contained by CH and the arc HF is double of the ſector HCF, and the rectangle contained by CH, FN is double of the triangle CFH; therefore the rectangle contained by CH and the exceſs of the arc FH above FN will be double of the ſpace contained by the arc FH and the chord FH; therefore the rectangle LHE will be double of the ſpace contained by the arc FH and the chord FH;

and

and therefore the triangle LHE will be equal to the ſpace contained by the arc FH and the chord FH: but, becauſe DL, HE are parallel, the triangles DHE, LHE are equal; therefore the triangle DHE is equal to the ſpace contained by the arc FH and the chord FH: and, becauſe DF, CH are parallel, the triangles CDH, CFH are equal; therefore the ſpace HCDE will be equal to the ſector CFH. Let the ſector BCH be added to both, and the ſector BDE will be equal to the ſector BCF: but, becauſe the ſector BCF is greater than the ſector BDH and leſs than the ſector BDK, therefore the ſector BDE is greater than the ſector BDH and leſs than the ſector BDK, and therefore DE falls between DH, DK: and, becauſe the point D is not very excentric, the limiting arc HK will be little; therefore the tangent HE may be conſidered as coinciding with the arc HK: becauſe the ſector BDE is equal to the ſector BCF, the ſemicircle will be to the ſector BDE as the ſemicircle to the ſector BCF: but the ſemicircle is to the ſector BCF as p is to q; therefore the ſemicircle is to the ſector BDE as p is to q. Q. E. D.

Kepler's

Kepler firſt of all diſcovered that the planets revolved in ellipſes round the ſun placed in one of the *foci*, and that they deſcribed equal areas in equal times round the ſun. Let the ſemi-ellipſe, [*Fig.* 6.] whoſe greater *axis* is AP, *focus* S, and centre C, repreſent half the orbit of a planet round the ſun in S; and ſuppoſe the planet at the point K in its orbit; join SK: half the periodic time of the planet round the ſun, is to the time the planet moves from A to P, as the area of the ſemi-ellipſe to the area ASK; and therefore to find the place of the planet at any given time, it is neceſſary to find the poſition of the right line SK, which ſhall cut off the area ASK proportional to the time, that is, To draw the line SK ſo that the area of the ſemi-ellipſe may be to the area ASK, as half the periodic time of the planet round the ſun to the given time.

From K let fall KH perpendicular to AP, meeting the ſemicircle deſcribed upon AP in G, and join SG: it is evident, from the nature of the ellipſe and circle, that the ſemicircle is to the ſector ASG as the ſemi-ellipſe to the ſector ASK; therefore the ſemicircle is to the ſector ASG as half the periodic time of the

the planet round the ſun to the time the planet moves from A to K: the problem therefore is reduced to this, To draw the line SG meeting the ſemicircle in G, ſo that the ſemicircle may be to the ſector ASG as half the periodic time of the planet round the ſun to the given time. In the ſemicircle, take the arc AB, ſo that the arc ABP may be to the arc AB as half the periodic time of the planet round the ſun to the given time. Join CB; the ſemicircle therefore will be to the ſector ACB as half the periodic time of the planet round the ſun to the given time, that is, as the ſemicircle to the ſector ASG; therefore the ſectors ACB, ASG, are equal; join CG.

The angle ACB is called by *Kepler* the mean anomaly, the angle ACG the anomaly of the excentric, and the angle ASK the coequate or true anomaly. The problem therefore is reduced to this: The mean anomaly of a planet being given, to find the anomaly of the excentric and the coequate anomaly.

PROP.

PROP. VI. PROBL. IV. *Fig*. 6.

Let AP be the greater axis of a planet's orbit, S the focus the place of the ſun, A the aphelion, P the perihelion; upon AP let the ſemicircle ABP be deſcribed; let C be the center, and let the angle ACB be the mean anomaly of the planet at any given time: it is required to find the anomaly of the excentric.

JOIN SB, and draw CD parallel to SB meeting the circle in D: join SD in BD: take the arc DG, ſo that the ſine of the angle BCD may be to the tangent of the angle CDS, as the exceſs of the arc BD above its ſine to the arc DG; and join CG: the angle ACG will be nearly the anomaly of the excentric.

JOIN SG; draw BE perpendicular to CD meeting CD in E, and draw CF parallel to SD meeting DF a tangent to the circle at D in F.

BECAUSE

Because BE is the ſine of the angle BCD, and DF the tangent of the angle DCF, that is, of the angle SDC; BE will be to DF as the exceſs of the arc BD above BE to the arc DG; therefore [*Prop.* 5.] the ſector ASG is equal to the ſector ACB; therefore the angle ACG is the anomaly of the excentric.

The computation is as follows: In the triangle BCS, as the ſum of the ſides BC, CS, is to the difference of the ſides BC, CS, ſo is the tangent of half the angle ACB to the tangent of half the difference of the angles, CSB, CBS; therefore the angles, CSB, CBS will be given, that is, the angles, ACD, DCB, will be given. Again, in the triangle CSD, the ſum of the ſides, CD, CS, is to the difference of the ſides CD, CS, as the tangent of half the angle ACD to the tangent of half the difference of the angles, CSD, CDS; therefore the angle CDS will be given. Again, becauſe as the ſine of the angle BCD is to the tangent of the angle CDS, ſo is the exceſs of the arc BD above its ſine to the arc GD; ſay as the radius is to the ſine of the angle BCD, ſo is $57^{\circ}.2957795$ *&c.* the number of degrees in an angle ſubtended by by an arc equal to the radius, to the number of degrees in an angle ſubtended by an arc equal to the ſine of the angle BCD; [let this angle be called A]. Again, as the ſine of the angle BCD to the tangent of the angle CDS, ſo is the exceſs of the angle BCD above the angle A, to the angle GCD. Therefore the angle ACG, the anomaly of the excentric, will be given.

EXAMPLE I.

In the orbit of *Mercury*, the mean diſtance is to the excentricity as 100000 to 20589. Suppoſe the mean anomaly from the aphelion to be 60°, it is required to find the anomaly of the excentric. In the triangle BCS, as 120589, the ſum of BC, CS, is to 79411 the difference of the ſides BC, CS, ſo is the tangent of 30°, half the ſum of the angles, CSB, CBS, to the tangent of half the difference of the angles CSB, CBS.

The log. tang. of 30° is	9.7614394
The log. of 79411 is	4.8998807
The ſum is	14.6613201
The log. of 120589 is	5.0813077
The difference	9.5800124 is the log.

log. tang. of 20° 49′.00894 half the difference of the angles CSB, CBS; therefore the angle CSB or ACD is 50° 49′.00894, and the angle CBS or BCD is 9° 10′.99106. Again, in the triangle DCS, as 120589 the ſum of DC, CS, is to 79411 the difference of DC, CS, ſo is the tangent of 25° 24′.50447 half the ſum of the angles CSD, CDS, to the tangent of half the difference of the angles CSD, CDS.

The log. tang. of 25° 24′.50447 is	9.6767070
The log. of 79411 is	4.8998807
The ſum is	14.5765877
The log. of 120589 is	5.0813077
The difference	9.4952800

is the log. tang. of 17° 22′.21093 half the difference of the angles CSD, CDS; therefore the angle CDS is 8° 2′.29354. Again, as the radius is to the ſine of 9° 10′.99106, ſo is 57° .2957795 &c. the number of degrees in an angle ſubtended by an arc equal to the radius, to the number of degrees and minutes in an angle ſubtended by an arc equal to the ſine of 9° 10′.99106.

The

The log. of 57.2957795 is	1.7581226
The log. ſine of 9° 10′.99106 is	9.2030097
The ſum is	10.9611323
The log. of rad. is	10.0000000
The difference	0.9611323

is the log. of 9° 8′.6349999, the number of degrees and minutes contained in an angle ſubtended by an arc equal to the ſine of 9° 10′.99106. The angle A therefore is 9° 8′.6349999; the exceſs therefore of the angle BCD above the angle A is 2′.35606. Again, as the ſine of 9° 10′.99106 is to the tangent of 8° 2′.29354, ſo is the angle 2′.35606 to the angle DCG.

The log. of 2′.35606 is	0.3721863
The log. tang. of 8° 2′.29354 is	9.1498998
The ſum is	9.5220861
The log. ſine of 9° 10′.99106 is	9.2030097
The difference	0.3190964

is the log. of 2′.084953 the angle DCG, the angle ACG the anomaly of the excentric is 50° 51′.093893, that is 50°. 51′ 5″.63358.

LET

Let SM, perpendicular to CG, meet CG in M: It is evident, becauſe the ſector ACG is common to both the ſectors ASG, ACB, that, if the ſector ASG be equal to the ſector ACB, the triangle SCG will be equal to the ſector BCG; and therefore the line SM will be equal to the arc BG: if the ſector ASG be greater than the ſector ACB, the triangle SCG will be greater than the ſector BCG; and therefore the line SM will be greater than the arc BG: and, if the ſector ASG be leſs than the ſector ACB, the triangle SCG will be leſs than the ſector BCG; and therefore the line SM will be leſs than the arc BG: that is, if the arc AG be greater, equal, or leſs, than the true anomaly of the excentric, the line SM will be greater, equal, or leſs, than the arc BG; and therefore, the leſs the difference is between the line SM and the arc BG, the leſs will the difference be between the arc AG and the anomaly of the excentric.

Because the triangles GCH, CSM, are ſimilar, CG is to GH as CS to SM; that is, the radius is to the ſine of 50° 51′.093893 as 20589 to CM.

The

The log. of 20589 is	4.3136353
The log. ſine of 50° 51′.093797 is	9.8895890
The ſum is	14.2032243
The log. of rad. is	10.0000000
The difference	4.2032243

is the log. of SM. Again, as CA to SM, ſo is 57°.2957795, &c. the number of degrees in an angle ſubtended by an arc equal to CA, to the number of degrees in an angle ſubtended by an arc equal to SM.

The log. of 57°.2957795, &c. is	1.7581226
The log. of SM is	4.2032243
The ſum is	5.9613469
The log. of CA	5.0000000
The difference	0.9613469

is the log. of 9° 8′.90625 the number of degrees and minutes in an angle ſubtended by an arc equal to SM: but, becauſe the arc AB is 60°, and the arc AG is 50° 51′.093797, the arc BG will be 9° 8′.906203; the difference therefore between SM and the arc BG

 is

is 0'.000047; the difference therefore is less than the 354th part of a second.

Mr. *Machin*, in his solution of this problem in the Philosophical Transactions, Number 447. makes the anomaly of the excentric to be 129°.14846, when the mean anomaly reckoned from the perihelion is 120°; and therefore, if the mean anomaly reckoned from the aphelion be 60°, the anomaly of the excentric will be 50°.85154, that is 50° 51'.0924.

In order to determine the difference between SH and the arc BG according to this computation,

The log. of 20589 is	4.3136353
The log. sine of 50° 51'.0924 is	9.8895888
The sum is	14.2032241
The log. of rad. is	10.0000000
The difference	4.2032241

is the log. of SM. Again, as CA to SM, so is 57°.2957795, &c. the number of degrees in an angle subtended by an arc equal to CA, to the number of degrees in an angle subtended by an arc equal to SM.

The

The log. of $57^\circ.2957795$, &c. is	1.7581226
The log. of SM is	4.2032241
The sum is	5.9613467
The log. of CA is	5.0000000
The difference	0.9613467

is the log. of $9^\circ\ 8'.905998$, the number of degrees and minutes in an angle subtended by an arc equal to SM: but, because the arc AB is 60°, and the arc AG is $50^\circ\ 51'.0924$, the arc BG will be $9^\circ\ 8'.9076$; the difference therefore between SM and the arc BG is $0'.001602$, very nearly one tenth of a second, and is more than thirty four times greater than the former difference.

Because AS is equal to the sum of BC, CS, and PS equal to the difference of BC, CS; it is evident, that, if, from the log. tang. of half the angle ACB, the difference of the logarithms of AS, SP be subtracted, the remainder will be the log. tang. of an angle, which, if added to half the angle ACB, will give the angle ASB or ACD; and, if subtracted from half the angle ACB, will give the angle CBS or BCD.

EX-

EXAMPLE II.

In the orbit of *Mars*, the mean diſtance is to the excentricity as 152369 to 14100. Suppoſing the mean anomaly to be 1°, it is required to find the anomaly of the excentric.

The log. tang. of 30′, half the angle ACB, is	7.9408584
The difference of the logs. of AS, SP is	0.0806086
The difference	7.8602498

is the log. tang. of 24′.9196814, half the difference of the angles CSB, CBS; therefore, the angle CSB, that is the angle ACD, is 54′.9196814; and the angle CBS, that is the angle BCD, is 5.0803186. Again, to determine the angle CDS,

The log. tang. of half the angle ACD is	7.8958249
The difference of the logs. of AS, SP is	0.0806086
The difference	7.8152163

is the log. tang. of 22′.4641426, half the difference of the angles CSD, CDS; therefore

fore the angle CDS, that is the angle DCF, is 4′.0956981.

Again, as the radius is to the ſine of 5′.0803186, ſo is 57° 2997795, &c. the number of degrees in an angle ſubtended by an arc equal to the radius, to the number of degrees and minutes in an angle ſubtended by an arc equal to the ſine of 5′.0803186.

The log. of 57°.2957795 is	1.7581226
The log. ſine of 5′.0803186 is	7.1633313
The ſum is	8.9214539
The log. of radius is	10.0000000
The difference	—2.9214539

is the log. of 5′.0073162, the number of minutes contained in the angle A; the exceſs therefore of the angle BCD, above the angle A, is 0′.0930024. Again, as the ſine of the angle BCD is to the tangent of the angle DCF, ſo is 0′.0930024 to the angle DCG.

The log. of 0′.0930024 is	—2.9684941
The log. tang. of the angle DCF is	7.1622795
The ſum is	6.1307736
The log. ſine of the angle BCD is	7.1633313
The difference	2.9674423

is

is the log. of 0′.0927774; the angle DCG therefore is 0′.0927774; therefore the angle ACG, the anomaly of the excentric, is 55′.0124588.

EXAMPLE III.

SUPPOSING the mean anomaly in the same orbit to be 45°, it is required to find the anomaly of the excentric.

The log. tang. of 22° 30′ is	9.6172243
The diff. of the logs. of AS, SP is	0.0806085
The difference	9.5366158

is the log. tang. of 18° 59′.1327325, half the difference of the angles CSB, CBS; the angle CSB, that is the angle ACD, therefore is 41° 29′.1327325; and the angle CBS or BCD is 3° 30′.8672675.

The log. tang. of half the angle ACD is	9.5783203
The diff. of the logs. of AS, SP is	0.0806085
The difference	9.4977118

is the log. tang. of 17° 27′.7082672, half the difference of the angles CSD, CDS; the angle CDS or DCF, therefore, is 3° 16′.858099.

3° 16′.858099. Again, as radius is to the fine of the angle BCD, fo is 57°.2957795, &c. to the angle A.

The log. of 57°.2957795, &c. is	1.7581226
The log. fine of the angle BCD is	8.7874623
The fum is	10.5455849
The log. of rad. is	10.0000000
The difference	0.5455849

is the log. of 3°.5122459; the angle A, therefore, is 3° 30′.734754; the excefs of the angle BCD above the angle A is 0′.1325135. Again, as the fine of the angle BCD is to the tangent of the angle SDC or DCF, fo is 0′.1325135 to the angle DCG.

The log. of 0′.1325135 is	—1.1222601
The log. tang. of the angle SDC is	8.7583537
The fum is	7.8806138
The log. fine of the angle BCD is	8.7874623
The difference	—1.0931515

is the log. of 0′.1239228; the angle DCG, therefore, is 0′.1239228; therefore the angle ACG, the anomaly of the excentric, is 41° 29′.2566553.

EX-

EXAMPLE IV.

AGAIN, in the ſame orbit ſuppoſing the mean anomaly to be 100° degrees, it is required to find the anomaly of the excentric.

The log. tang. of 50° half the angle ACB is	10.0761865
The difference of the logs. of AS, SP is	0.0806086
The difference	9.9955779

is the log. tang. of 44° 42′.4982192, half the difference of the angles CSB, CBS; therefore the angle CSB, that is the angle ACD, is 94° 42′.4982192; and the angle CBS, that is the angle BCD, is 5° 17′.5017808. Again, to determine the angle CDS,

The log. tang. of half the angle ACD is	10.0557285
The difference of the logs. of AS, SP is	0.0806086
The difference	9.9751199

is the log. tang. of 43° 21′.5819834, half the difference of the angles CSD, CDS; therefore the angle CDS, that is the angle DCF, is 3° 59′.6671212.

AGAIN,

AGAIN, as the radius is to the ſine of 5° 17′.5017808 the angle BCD, ſo is 57°· 2997795 to the angle A.

The log. of 57°.2957795 is	1.7581226
The log. ſine of 5° 17′.5017808 is	8.9642381
The ſum is	10.7223607
The log. of radius is	10.0000000
The difference	0.7223607

is the log. of 5° 16′.600752 the number of degrees and minutes contained in the angle A; the exceſs therefore of the angle BCD above the angle A is 0′.9010288. Again, as the ſine of the angle BCD is to the tangent of the angle DCF, ſo is 0′.9010288 to the angle DCG.

The log. of 0′.9010288 is	—1.9547387
The log. tang. of the angle DCF is	8.8440431
The ſum is	8.7987818
The log. ſine of the angle BCD is	8.9642381
The difference	—1.8345437

is the log. of 0′.6831934; the angle DCG therefore is 0′.6831934; therefore the angle ACG the anomaly of the excentric is 94° 43′.1814126.

 THE

The three laſt examples are taken from Dr. *Keil*'s aſtronomical Lectures, Lecture 23d. and the numbers agree very nearly with his.

In the orbits of *Mercury* and *Mars*, if the exceſs of the angle BCD above the angle A be added to the angle ACD, the ſum will be nearly the anomaly of the excentric reckoned from the aphelion.

In orbits of ſmall excentricity, the angle ACD is nearly the anomaly of the excentric; therefore the following rule will give the anomaly of the excentric very nearly.

From the logarithmic tangent of half the mean anomaly, ſubtract the difference of the logarithms of the aphelion and perihelion diſtances; the remainder is the logarithmic tangent of an angle, which call B: to the angle B, add half the mean anomaly; the ſum will give very nearly the anomaly of the excentric.

EXAMPLE V.

In the earth's orbit, the mean diſtance is to the excentricity as 100000 to 1691. Suppoſe the mean anomaly from the aphelion to be 30°, it is required to find the anomaly of the excentric.

The

The log. tang. of 15° is	9.4280525
The diff. of the logs. of AS, SP is	0.0146892
The difference	9.4133633

is the log. tang. of 14° 31′.3670421, half the difference of the angles CSB, CBS; therefore the angle CSB, that is the angle ACD, is 29° 31′.367042; therefore the anomaly of the excentric is nearly 29° 31′.3670421, that is 29° 31′.22″.022526, which agrees very nearly with Dr. *Keil*'s numbers in the foresaid Lecture.

EXAMPLE VI.

Again, in the earth's orbit, suppose the mean anomaly to be 60°, it is required to find the anomaly of the excentric.

The log. tang. of 30° is	9.7614394
The diff. of the logs. of AS, SP is	0.0146892
The difference	9.7467502

is the log. tang. of 29° 10′.081873, half the difference of the angles CSB, CBS; therefore the angle CSB, that is the angle ACD, is 59° 10′.081873; therefore the anomaly of the excentric is nearly 59° 10′.081873.

EX-

EXAMPLE VII.

In the orbit of *Venus*, the mean diftance is to the excentricity as 10000000 to 69855. Suppofe the mean anomaly reckoned from the aphelion to be 60°, it is required to find the anomaly of the excentric.

The log. tang. of 30° is	9.7614394
The diff. of the logs. of AS, SP is	0.0060677
The difference	9.7553717

is the log. tang. of 29° 39′.2739959, half the difference of the angles CSC, CBS; therefore the angle CSB, that is the angle ACD, is 59° 39′.2739959; therefore the anomaly of the excentric is nearly 59° 39′.2739959, that is 59° 39′.16″,439754.

If the mean anomaly reckoned from the perihelion be 120°, the anomaly of the excentric would be nearly 120° 20′.43″.560246, which agrees very nearly with Mr. *Machin*'s numbers in the forecited tranfaction.

The anomaly of the excentric being found, the coequate or true anomaly will be found by the refolution of the triangle GCS: Thus, from the log. tang. of half the anomaly of the excentric, fubtract the difference of the logs,

logs. of the aphelion and perihelion diſtances, the remainder will be the log. tang. of an angle; to this angle add half the anomaly of the excentric; let the ſum be called the angle C; to the log. tang. of the angle C, add half the ſum of the logs. of the aphelion and perihelion diſtances; from this ſum, ſubtract the log. of the mean diſtance; the remainder will be the log. tang. of the coequate or true anomaly.

LET CL be the leſſer axis of the planet's orbit. Becauſe, from the nature of the ellipſe, the ſquare of CL is equal to the rectangle ASP, the log. of CL will be equal to half the ſum of the logs. of AS, SP: and, becauſe the tangent of the angle KSH is to the tangent of the angle GSH as HK to HG; that is, from the nature of the ellipſe, as LC to CA; therefore, if to the log. tang. of the angle GSH, the log. of CL be added, and from the ſum the log. of AC be ſubtracted, the remainder will be the log. tang. of the angle KSH.

AGAIN, the ſine of the true anomaly is to the ſine of the anomaly of the excentric, as the leſſer axis of the orbit to the diſtance of the planet from the ſun.

BECAUSE

Because CL is to HK as CG to GH, that is, as the radius to the ſine of the anomaly of the excentric, and HK is to KS as the ſine of the true anomaly to the radius; therefore CL is to KS as the ſine of the true anomaly to the anomaly of the excentric.

The place of a planet in an elliptic orbit [granting the quadrature of the ellipſe] may be found at any given time within a ſmall limit, by the following theorem.

THEOREM. Fig. 7.

Let the ellipſe, whoſe greater axis is AP, foci S, K, and center C, repreſent the orbit of a planet round the ſun at S; and, ſuppoſing the periodic time of the planet round the ſun to be known, and likeways the time the planet paſſed thro' the aphelion A: As the periodic time of the planet round the ſun, is to the time elapſed ſince the planet paſſed thro' the point

point A, ſo let the area of the ellipſe be to the ſector ACB; join SB, and draw CD parallel to SB on the ſame ſide of AP that SB is; and let CD be equal to CA; join SD; let CD, SD, meet the ellipſe in E, F: the true place of the planet is between the points E, F; that is, the planet is paſſed the point E, but not come to the point F.

LET G be the place of the planet; join SG, and join BD meeting AP in H, and join KB; draw SL parallel to KB meeting BD in L. Becauſe CD is parallel to SB, CD will be to SB as CH to HS; therefore, twice CD will be to SB as twice CH to HS. But, becauſe twice CD is equal to AP, that is, equal to KB together with BS, and twice CH is equal to KH together with HS; therefore KB together with BS will be to BS as KH together with HS is to HS; and therefore KB will be to BS as KH to HS, that is, as KB to SL; therefore BS, SL are equal: therefore the angle SBL is equal to the angle SLB,

SLB, that is, equal to the angle KBD; and therefore, from a known property of the ellipse, BD is a tangent to the ellipse at the point B; and therefore [*Prop.* 2.] the sector ACB is greater than the sector ASE, and less than the sector ASF: but, because G is the place of the planet, the area of the ellipse will be to the sector ASG as the periodic time of the planet round the sun is to the time elapsed since the planet passed thro' the point A, that is, as the area of the ellipse to the sector ACB; therefore the sector ASG is equal to the sector ACB; and therefore the sector ASG is greater than the sector ASE, and less than the sector ASF; therefore the line SG falls between the lines SE, SF; and therefore G, the place of the planet, is between the points E, F; therefore the planet has passed the point E, but not come to the point F.

Fig. 1.

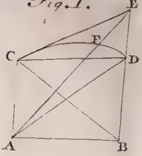


Fig. 2.

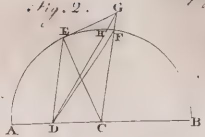


Fig. 3.

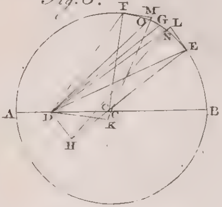


Fig. 4.

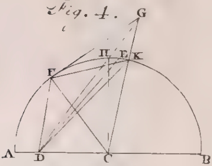


Fig. 5.

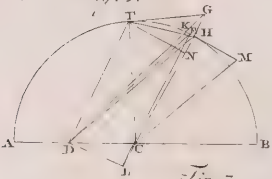


Fig. 6.

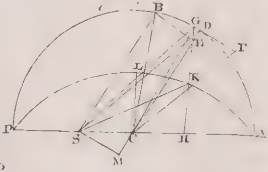
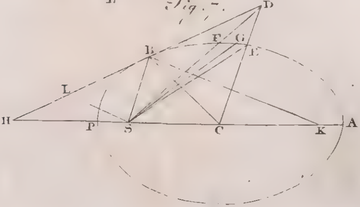


Fig. 7.



ART. VII.

Of the Cold produced by Evaporating Fluids, and of some other Means of producing Cold; by Dr. WILLIAM CULLEN *Professor of Medicine in the University of* Glasgow *.

A YOUNG Gentleman one of my pupils, whom I had employed to examine the heat or cold that might be produced by the solution of certain substances in spirit of wine, observed to me: That, when a thermometer had been immersed in spirit of wine, tho' the spirit was exactly of the temperature of the surrounding air, or somewhat colder; yet, upon taking the thermometer out of the spirit, and suspending it in the air, the mercury in the thermometer, which was of *Farenheit*'s construction, always sunk two or three degrees. This recalled to my mind some experiments and observations of *M. de Mairan* to the same purpose; which I had read some time before. *See Dissertation sur la glace, edit.* 1749. *pag.* 248, *& seq.*

* May 1. 1755.

When I firſt read the experiments of *M. de Miaran* in the place referred to, I ſuſpected, that water, and perhaps other fluids, in evaporating, produced, or, as the phraſe is, generated ſome degree of cold. The above experiment of my Pupil confirmed my ſuſpicion, and engaged me to verify it by a variety of new trials.

I began by repeating the experiment with ſpirit of wine; and found, when I had taken the utmoſt care to have the ſpirit exactly of the temperature of the air, that conſtantly however, upon taking the thermometer out of the ſpirit, the mercury ſunk ſeveral degrees, and indeed continued to ſink ſo long as the ball of the thermometer continued wet with the ſpirit of wine. I found alſo, when the ball began to dry, and the mercury to riſe again in the ſtem of the thermometer, that, if the ball was again dipped into the ſpirit and immediately taken out, the mercury in the thermometer might be again obſerved to ſink, and that thus, by repeated dippings, the cold produced might be rendered very remarkable. The cold produced was alſo obſerved to be ſtill greater, when, between each dipping, the thermometer was

moved

moved very nimbly to and fro in the air; or if, while the ball was wet with ſpirit of wine, it was blown upon by a pair of bellows; or indeed if the air about the ball was otherwiſe any how put in motion. If any of theſe means for putting the air in motion are employed, the repeated moiſtenings of the ball of the thermometer may be performed by dipping it into the ſpirit of wine. But, when a certain degree of cold has been produced by a firſt dipping, that is apt to be diminiſhed by dipping again into the warmer ſpirit; and therefore the thermometer ought either to be dipped into the ſpirit and taken out again very quickly, or, what is ſtill better, the ball of the thermometer ought to be moiſtened by a feather that has been dipped into the ſpirit of wine. By taking theſe methods, I have by ſpirit of wine made the mercury in the thermometer ſink from 44 degrees to below the freezing point; and, by employing ſome other fluids to be mentioned by and by, I have produced a ſinking of the thermometer much more conſiderable.

In making experiments of this kind, it is to be obſerved, that the cold produced is of very ſhort duration. On this account it

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is not proper to employ a thermometer inclofed in a glafs tube; and it is neceffary to employ one having a fmall ball, which may render it more fenfible. But, as a fmall ball occafions the divifions of the fcale to be the fmaller, a thermometer filled with quickfilver is not fo proper for thefe experiments, as one filled with fpirit of wine; having at the fame time both a fmall ball and a flender ftem. What beft of all fhews the cold produced, and is indeed, with refpect to feveral fluids, quite neceffary, is an *Air* thermometer. This too will be rendered more convenient by having the upper part of the tube bent as in the figure annexed, fo that the ball may be moiftened without the liquor's running down upon the ftem and fcale.

I have entered into this detail for the fake of thofe who may defire to repeat my experiments. Having now faid enough on the manner of making them, I go on to obferve, that in this way I have examined a great variety of fluids. Such as,

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The *quick-lime ſpirit* of *ſal. ammoniac*,
The *æther* of *Frobenius*,
The *nitrous æther*,
The *volatile tincture of ſulphur*,
Spirit of wine,
Spirit of ſal. ammoniac made with the fixed *alkali*,
Brandy,
Wine,
Vinegar,
Water,
Oil of turpentine,
Oil of mint,
Oil of pimento.

By each of theſe employed to moiſten the ball of the thermometer, ſome degree of cold is produced. I dare not however at preſent determine exactly what is the ſinking of the thermometer produced by each. For this purpoſe, it would be neceſſary to repeat the trials often and with preciſely the ſame circumſtances at each time: which I find to be very difficult. In the mean time I have endeavoured to give a notion of the comparative power of theſe fluids in producing cold, by the order in which I have ſet them down; having mentioned that fluid firſt which ſeemed

ſeemed to me to occaſion the greateſt ſinking of the thermometer, and the reſt follow in order as they ſeemed to occaſion leſs and leſs.

From the above enumeration I imagine it will appear, that the power of evaporating fluids in producing cold, is nearly according to the degree of volatility in each. If to this we join the conſideration, that the cold is made greater by whatever haſtens the evaporation, and particularly, that the ſinking of the thermometer is greater as the air in which the experiment is made is warmer, if dry at the ſame time; I think we may now conclude, that *the cold produced is the effect of evaporation.*

I did not think it neceſſary to diverſify my experiments further by examining a great many fluids, which are manifeſtly of a like nature with theſe above-mentioned. I preſume pretty confidently, that the ſeveral ſpirituous, watery, and oily fluids, akin to theſe already tried, will be found all of them to have ſimilar effects. And, conſidering how many fluids theſe claſſes comprehend, and that, in theſe already tried, the cold produced ſeems to depend more on the volatility of the

aggregate

aggregate than on the nature of the mixt; I was ready to conclude, that all fluids whatever, would, in evaporating, produce cold. But I have found a ſeeming exception. When the ball of the thermometer is moiſtened with any of the foſſil acids, a conſiderable degree of heat is produced. It is however to be doubted, if this affords an exception. We know that theſe acids attract water from the air; and alſo that theſe acids, mixed with water, always produce heat: it may therefore be ſuppoſed, that the heat produced, by moiſtening the ball of the thermometer with theſe acids, is to be imputed rather to their mixing with the water of the air, than to their evaporation ſingly. This perhaps cannot be poſitively determined, till the evaporation of theſe acids, in a very perfect vacuum, is examined; which I have not yet had an opportunity of doing. In the mean time, I have made an experiment which I think is to the purpoſe. To one part of ſtrong acid of vitriol, I added two parts of ſpring-water. When this mixture, which produces a great degree of heat, was returned to the temperature of the air, I uſed it for moiſtening the ball of the thermometer,

ter,

ter, and found it produced a ſenſible degree of cold, and ſeemingly a greater than water alone would have done. I need not here obſerve, that the mixture I uſed was ſtill a very acid liquor, only ſo much ſaturated with water, that it would not now attract any from the air. Whether it would not have had the ſame effect, tho' leſs diluted, I have not had time to examine. The experiment, as it ſtands, tends to prove, that the heat produced by acids, applied to the ball of the thermometer, is owing to the mixture of theſe with the water of the air; and therefore, it is ſtill very probable, that all fluids, which do not immediately affect the mixture of the air, will, in evaporating, produce cold.

When I had proceeded thus far, I began to conſider, whether the cold produced in the above experiments might not be the effect of the mixture of the ſeveral fluids with the air; and that therefore, to a liſt of cooling mixtures and ſolutions which I was then making up, I ſhould now add the ſeveral ſolutions made by the air. By one who ſuppoſes the evaporation of fluids to depend upon the action of the air as a menſtruum, this would be readily admitted; but, as I knew that fluids

fluids evaporate *in vacuo* as well as in the air, I resolved to suspend my opinion, till I should repeat my experiments in an exhausted receiver.

In prosecuting these, a number of new and to me curious *phænomena* have presented themselves; so many, that I find the experiments must be often repeated, and much diversified, before I can give the Society a proper account of them. In the mean time, I shall give you the following facts already sufficiently verified.

A thermometer hung in the receiver of an air-pump, sinks always two or three degrees upon the air's being exhausted. After a little time, the thermometer *in vacuo* returns to the temperature of the air in the chamber, and upon letting air again into the receiver, the thermometer always rises two or three degrees above the temperature of the external air.

When a vessel containing spirit of wine, with a thermometer immersed in it, is set under the receiver of an air-pump; upon exhausting the air, the mercury in the thermometer sinks several degrees. It becomes more especially remarkable when the air in

 any

any plenty iſſues out of the ſpirit of wine. As the ſpirit continues long to give out air, ſo it is long before the thermometer immerſed in it returns to the temperature of the external air. But when after ſome time it appears ſtationary, if it is then drawn up out of the ſpirits and ſuſpended in the vacuum, the mercury ſinks very quickly eight or nine degrees; a good deal farther than it would have done in the ſame circumſtances in the air. In the ſame manner as in the air, the thermometer *in vacuo* may be made to ſink lower by repeated dippings into the ſpirit of wine: but here theſe repeated dippings have not ſo remarkable an effect as in the air; becauſe the dipping cannot be ſo quickly performed, and the thermometer is therefore more affected by the warmth of the ſpirit. It is ſometimes alſo affected by a drop of the ſpirit which the ball takes up along with it, and which, as I ſhould have obſerved above, ought always to be taken away in the experiments made in the air. This experiment with ſpirit of wine was often enough repeated, to ſhew clearly, that the evaporation of the ſpirit

in

in vacuo produces a greater degree of cold than the evaporation of the ſame in the air.

SATISFIED of this, I have tried alſo ſome other fluids; as the quick-lime ſpirit of ſal. ammoniac, and the two kinds of æther. Veſſels containing theſe with a thermometer immerſed in the liquor, but ſeparately and at different times, were ſet under the receiver. With regard to the different fluids, the *phænomena* were much the ſame. Upon exhauſting the receiver, the fluid gave out a great quantity of elaſtic air; and while this happened, the mercury in the immerſed thermometer ſunk very faſt and to a great length. In our trials, it generally ſunk below the ſcale applied ſo that we could not meaſure exactly how far. In one experiment before exhauſting the receiver, the thermometer had ſtood at 50 degrees, and, after exhauſting, we could judge very certainly that it ſunk to below 20. In another experiment made with the nitrous æther, when the heat of the air was about 53 degrees, we ſet the veſſel containing the æther in another a little larger containing water. Upon exhauſting the receiver, and the veſſel's remaining for a few minutes *in vacuo*, we found

found the moſt part of the water frozen, and the veſſel containing the æther ſurrounded with a thick and firm cruſt of ice.

SUCH a means of producing cold, and to ſo great a degree, has not, ſo far as I know, been obſerved before, and it ſeems to deſerve being further examined by experiments. Till that is done, I do not chuſe to give any account of ſome other remarkable *phænomena* that have occurred in the above experiments, nor to enter into the ſeveral ſpeculations that the ſubject ſeems to ſuggeſt.

SINCE writing the above, I have had occaſion to obſerve, that Mr. *Richman* of the Acadamy of *Peterſburg* has taken notice of the effect of evaporating fluids in producing cold; but does not impute it to the evaporation alone. His very exact account of the *phænomena*, and his theory with regard to them, may be ſeen in *Nov. Comment. Acad. Petropolitanæ ad ann.* 1747 & 1748. page 284.

ART. VIII.

Experiments upon Magnesia alba, *Quicklime, and some other Alcaline Substances*; *by* JOSEPH BLACK, M. D.*

PART I.

HOFFMAN, in one of his observations, gives the history of a powder called *magnesia alba*, which had been long used and esteemed as a mild and tasteless purgative; but the method of preparing it was not generally known before he made it public †.

IT was originally obtained from a liquor called the *mother of nitre*, which is produced in the following manner:

SALT-PETRE is separated from the brine which first affords it, or from the water with which it is washed out of nitrous earths, by the process commonly used in crystallizing salts. In this process the brine is gradually diminished, and at length reduced to a small quantity of an unctuous bitter saline liquor, affording

* June 5. 1755.

† Hoff. op. T. iv. p. 479.

affording no more ſalt-petre by evaporation; but, if urged with a briſk fire, drying up into a confuſed maſs which attracts water ſtrongly, and becomes fluid again when expoſed to the open air.

To this liquor the workmen have given the name of the *mother of nitre*; and *Hoffman*, finding it compoſed of the *magneſia* united to an acid, obtained a ſeparation of theſe, either by expoſing the compound to a ſtrong fire in which the acid was diſſipated and the *magneſia* remained behind, or by the addition of an alkali which attracted the acid to itſelf: and this laſt method he recommends as the beſt. He likewiſe makes an inquiry into the nature and virtues of the powder thus prepared; and obſerves, that it is an abſorbent earth which joins readily with all acids, and muſt neceſſarily deſtroy any acidity it meets in the ſtomach; but that its purgative power is uncertain, for ſometimes it has not the leaſt effect of that kind. As it is a mere inſipid earth, he rationally concludes it to be purgative only when converted into a ſort of neutral ſalt by an acid in the ſtomach, and that its effect is therefore proportional to the quantity of this acid.

ALTHO'

ALTHO' *magnesia* appears from this history of it to be a very innocent medicine, yet having observed, that some hypochondriacs who used it frequently, were subject to flatulencies and spasms, he seems to have suspected it of some noxious quality. The circumstances however which gave rise to his suspicion, may very possibly have proceeded from the imprudence of his patients, who, trusting too much to *magnesia*, (which is properly a palliative in that disease,) and neglecting the assistance of other remedies, allowed their disorder to increase upon them. It may indeed be alledged, that *magnesia*, as a purgative, is not the most eligible medicine for such constitutions, as they agree best with those that strengthen, stimulate and warm; which the saline purges commonly used are not observed to do. But there seems at least to be no objection to its use when children are troubled with an acid in their stomach; for gentle purging in this case is very proper, and it is often more conveniently procured by means of *magnesia* than of any other medicine, on account of its being intirely insipid.

THE above-mentioned Author observing, some time after, that a bitter saline liquor, similar

ſimilar to that obtained from the brine of ſalt-petre, was likewiſe produced by the evaporation of thoſe waters which contain common ſalt, had the curioſity to try if this would alſo yield a *magneſia*. The experiment ſucceeded: and he thus found out another proceſs for obtaining this powder, and at the ſame time aſſured himſelf by experiments, that the product from both was exactly the ſame*.

My curioſity led me ſome time ago to inquire more particularly into the nature of *magneſia*, and eſpecially to compare its properties with thoſe of the other abſorbent earths, of which there plainly appeared to me to be very different kinds, altho' commonly confounded together under one name. I was indeed led to this examination of the abſorbent earths, partly by the hope of diſcovering a new ſort of lime and lime-water, which might poſſibly be a more powerful ſolvent of the ſtone than that commonly uſed; but was diſappointed in my expectations.

I have had no opportunity of ſeeing *Hoffman*'s firſt *magneſia* or the liquor from which it

* Hoff. Op. T. iv. p. 500.

it is prepared, and have therefore been obliged to make my experiments upon the second.

In order to prepare it, I at firſt employed the bitter ſaline liquor called *bittern*, which remains in the pans after the evaporation of ſea water. But as that liquor is not always eaſily procured, I afterwards made uſe of a ſalt called *epſom-ſalt*, which is ſeparated from the bittern by cryſtallization, and is evidently compoſed of *magneſia* and the vitriolic acid.

There is likewiſe a ſpurious kind of Glauber ſalt, which yields plenty of *magneſia*, and ſeems to be no other than the epſom ſalt of ſea water reduced to cryſtals of a larger ſize. And common ſalt alſo affords a ſmall quantity of this powder; becauſe being ſeparated from the bittern by one haſty cryſtallization only, it neceſſarily contains a portion of that liquor.

Those who would prepare a *magneſia* from epſom-ſalt, may uſe the following proceſs.

Dissolve equal quantities of epſom-ſalt, and of pearl aſhes ſeparately in a ſufficient quantity of water; purify each ſolution from its dregs, and mix them accurately together

 by

by violent agitation: then make them juſt to boil over a briſk fire.

ADD now to the mixture three or four times its quantity of hot water; after a little agitation, allow the *magneſia* to ſettle to the bottom, and decant off as much of the water as poſſible. Pour on the ſame quantity of cold water; and, after ſettling, decant it off in the ſame manner. Repeat this waſhing with the cold water ten or twelve times: or even oftner, if the *magneſia* be required perfectly pure for chemical experiments.

WHEN it is ſufficiently waſhed, the water may be ſtrained and ſqueezed from it in a linen cloth; for very little of the *magneſia* paſſes thro'.

THE alkali in the mixture uniting with the acid, ſeparates it from the *magneſia*; which not being of itſelf ſoluble in water, muſt conſequently appear immediately under a ſolid form. But the powder which thus appears is not intirely *magneſia*; part of it is the neutral ſalt, formed from the union of the acid and alkali. This neutral ſalt is found, upon examination, to agree in all reſpects with vitriolated tartar, and requires a large quantity of hot water to diſſolve it. As much of it is therefore diſ-

ſolved

ſolved as the water can take up; the reſt is diſperſed thro' the mixture in the form of a powder. Hence the neceſſity of waſhing the *magneſia* with ſo much trouble; for the firſt affuſion of hot water is intended to diſſolve the whole of the ſalt, and the ſubſequent additions of cold water to waſh away this ſolution.

THE caution given of boiling the mixture is not unneceſſary; if it be neglected, the whole of the *magneſia* is not accurately ſeparated at once; and by allowing it to reſt for ſome time, that powder concretes into minute grains, which, when viewed with the microſcope, appear to be aſſemblages of needles diverging from a point. This happens more eſpecially when the ſolutions of the epſom-ſalt and of the alkali are diluted with too much water before they are mixed together. Thus, if a dram of epſom-ſalt and of ſalt of tartar be diſſolved each in four ounces of water, and be mixed, and then allowed to reſt three or four days, the whole of the *magneſia* will be formed into theſe grains. Or if we filtrate the mixture ſoon after it is made, and heat the clear liquor which paſſes thro'; it will become turbid, and depoſite a *magneſia*.

I had the curiofity to fatisfy myfelf of the purgative power of *magnefia*, and of *Hoffman*'s opinion concerning it, by the following eafy experiment. I made a neutral falt of *magnefia* and diftilled vinegar; choofing this acid as being, like that in weak ftomachs, the product of fermentation. Six drams of this I diffolved in water, and gave to a middle-aged man, defiring him to take it by degrees. After having taken about a third, he defifted, and purged four times in an eafy and gentle manner. A woman of a ftrong conftitution got the remainder as a brifk purgative, and it operated ten times without caufing any uneafinefs. The tafte of this falt is not difagreeable, and it appears to be rather of the cooling than of the acrid kind.

Having thus given a fhort fketch of the hiftory and medical virtues of *magnefia*, I now proceed to an account of its chemical properties. By my firft experiments, I intended to learn what fort of neutral falts might be obtained by joining it to each of the vulgar acids; and the refult was as follows.

Magnesia is quickly diffolved with violent effervefcence, or explofion of air, by the

acids

acids of vitriol, nitre, and of common ſalt, and by diſtilled vinegar; the neutral ſaline liquors thence produced having each their peculiar properties.

THAT which is made with the vitriolic acid, may be condenſed into cryſtals ſimilar in all reſpects to epſom-ſalt.

THAT which is made with the nitrous is of a yellow colour, and yields ſaline cryſtals, which retain their form in a very dry air, but melt in a moiſt one.

THAT which is produced by means of ſpirit of ſalt, yields no cryſtals; and if evaporated to dryneſs, ſoon melts again when expoſed to the air.

THAT which is obtained from the union of diſtilled vinegar with *magneſia*, affords no cryſtals by evaporation, but is condenſed into a ſaline maſs, which, while warm, is extremely tough and viſcid, very much reſembling a ſtrong glue both in colour and conſiſtence, and becomes brittle when cold.

BY theſe experiments *magneſia* appears to be a ſubſtance very different from thoſe of the calcarious claſs; under which I would be underſtood to comprehend all thoſe that are converted into a perfect quick-lime in a

ſtrong

ſtrong fire, ſuch as *lime-ſtone, marble, chalk,* thoſe *ſpars* and *marles* which effervefce with aqua fortis, all *animal ſhells* and the bodies called *lithophyta.* All of theſe, by being joined with acids, yield a ſet of compounds which are very different from thoſe we have juſt now deſcribed Thus, if a ſmall quantity of any calcarious matter be reduced to a fine powder and thrown into ſpirit of vitriol, it is attacked by this acid with a briſk effervefcence; but little or no diſſolution enſues. It abſorbs the acid, and remains united with it in the form of a white powder, at the bottom of the veſſel, while the liquor has hardly any taſte, and ſhews only a very light cloud upon the addition of an alkali*.

The ſame white powder is alſo formed when ſpirit of vitriol is added to a calcacarious earth diſſolved in any other acid; the vitriolic expelling the other acid, and joining

* Mr. *Margraaf* has lately demonſtrated, by a ſet of curious and accurate experiments, that this powder is of the nature, and poſſeſſes the properties, of the gypſeous or ſelenitic ſubſtances. That ſuch ſubſtances can be reſolved into vitriolic acid and calcarious earth, and can be again compoſed by joining theſe two ingredients together. Mem de l'Acad. de Berlin. an. 1750, p. 144.

joining itſelf to the earth by a ſtronger attraction; and upon this account the *magneſia* of ſea-water ſeems to be different from either of thoſe deſcribed by *Hoffman*. He ſays expreſsly, that the ſolutions of each of his powders, or, what is equivalent, that the liquors from which they are obtained, formed a coagulum, and depoſited a white powder, when he added the vitriolic acid *; which experiment I have often tried with the marine bittern, but without ſucceſs. The coagulum thus formed in the mother of nitre may be owing to a quantity of quick-lime contained in it; for quick-lime is uſed in extracting the ſalt-petre from its matrix. But it is more difficult to account for the difference between *Hoffman*'s bittern and ours, unleſs we will be ſatisfied to refer it to this, that he got his from the waters of ſalt ſprings, which may poſſibly be different from thoſe of the ſea.

MAGNESIA is not leſs remarkably diſtinguiſhed from the calcarious earths, by joining it to the nitrous and vegetable acids, than to the vitriolic. Thoſe earths, when combined with ſpirit of nitre, cannot be reduced to a cryſtalline

* Hoff. Op. T. iv. p. 480 & 500.

cryſtalline form, and if they are diſſolved in diſtilled vinegar, the mixture ſpontaneouſly dries up into a friable ſalt.

Having thus found *magneſia* to differ from the common alkaline earths, the object of my next inquiry was its peculiar degree of attraction for acids, or what was the place due to it in Mr. *Geoffroy*'s table of elective attractions.

Three drams of *magneſia* in fine powder, an ounce of ſalt ammoniac, and ſix ounces of water were mixed together, and digeſted ſix days in a retort joined to a receiver.

During the whole time, the neck of the retort was pointed a little upwards, and the moſt watery part of the vapour, which was condenſed there, fell back into its body. In the beginning of the experiment, a volatile ſalt was therefore collected in a dry form in the receiver, and afterwards diſſolved into ſpirit.

When all was cool, I found in the retort a ſaline liquor, ſome undiſſolved *magneſia*, and ſome ſalt ammoniac cryſtallized. The ſaline liquor was ſeparated from the other two, and then mixed with the alkaline ſpirit. A coagulum

coagulum was immediately formed, and a *magnesia* precipitated from the mixture.

THE *magnesia* which had remained in the retort, when well washed and dried, weighed two scruples and fifteen grains.

WE learn by the latter part of this experiment, that the attraction of the volatile alkali for acids is stronger than that of *magnesia*, since it separated this powder from the acid to which it was joined. But it also appears, that a gentle heat is capable of overcoming this superiority of attraction, and of gradually elevating the alkali, while it leaves the less volatile acid with the *magnesia*.

DISSOLVE a dram of any calcarious substance in the acid of nitre or of common salt; taking care that the solution be rendered perfectly neutral, or that no superfluous acid be added. Mix with this solution a dram of *magnesia* in fine powder, and digest it in the heat of boiling water about twenty four hours; then dilute the mixture with double its quantity of water, and filtrate. The greatest part of the earth now left in the filtre is calcarious, and the liquor which passed thro', if mixed with a dissolved alkali,

 yields

yields a white powder, the largeſt portion of which is a true *magneſia.*

From this experiment it appears, that an acid quits a calcarious earth to join itſelf to *magneſia*; but the exchange being performed ſlowly, ſome of the *magneſia* is ſtill undiſſolved, and part of the calcarious earth remains yet joined to the acid.

When a ſmall quantity of *magneſia* is thrown into a ſolution of the corroſive ſublimate of mercury, it ſoon ſeparates part of the mercury in the form of a dark red powder, and is itſelf diſſolved.

Imagining that I perceived ſome reſemblance between the properties of *magneſia* and thoſe of alkalis, I was led to try what change this ſubſtance would ſuffer from the addition of quick-lime, which alters in ſuch a peculiar manner the alkaline ſalts.

Twenty ſeven grains of *magneſia* in fine powder were mixed with eighteen ounces of lime-water in a flaſk, which was corked cloſe and ſhaken frequently for four days. During this time, I frequently dipp'd into it little bits of paper, which were coloured with the juice of violets; and theſe became green as ſoon as they touched the water, until the fourth

fourth day, when their colour did not ſeem to be altered. The water being now poured off, was intirely inſipid, and agreed in every chemical trial with pure water. The powder, after being perfectly well dried, weighed thirty ſeven grains. It did not diſſolve intirely in ſpirit of vitriol; but, after a briſk efferveſcence, part of it ſubſided in the ſame manner as the calcarious earths, when mixed with this acid.

WHEN I firſt tried this experiment, I was at the trouble of digeſting the mixture in the heat of boiling water, and did not then know that it would ſucceed in the heat of the air. But Dr. *Alſton*, who has obliged the world with many curious and uſeful diſcoveries on the ſubject of quick-lime, having had occaſion to repeat it, I learned from him that heat is not neceſſary; and he has moreover added an uſeful purpoſe to which this property of *magneſia* may be applied; I mean the ſweetening of water at ſea, with which lime may have been mixed to prevent its putrefaction.

THAT part of the dried powder which does not diſſolve in ſpirit of vitriol, conſiſts of the lime ſeparated from the water.

QUICK-LIME

Quick-lime itſelf is alſo rendered mild by *magneſia*, if theſe two are well rubbed together and infuſed with a ſmall quantity of water.

By the following experiments, I propoſed to know whether this ſubſtance could be reduced to a quick-lime.

An ounce of *magneſia* was expoſed in a crucible for about an hour to ſuch a heat as is ſufficient to melt copper. When taken out, it weighed three drams and one ſcruple, or had loſt $\frac{7}{12}$ of its former weight.

I repeated, with the *magneſia* prepared in this manner, moſt of thoſe experiments I had already made upon it before calcination, and the reſult was as follows.

It diſſolves in all the acids, and with theſe compoſes ſalts exactly ſimilar to thoſe deſcribed in the firſt ſet of experiments: but what is particularly to be remarked, it is diſſolved without any the leaſt degree of effervеſcence.

It ſlowly precipitates the corroſive ſublimate of mercury in the form of a black powder.

It ſeparates the volatile alkali in ſalt ammoniac from the acid, when it is mixed

with

with a warm ſolution of that ſalt. But it does not ſeparate an acid from a calcarious earth, nor does it induce the leaſt change upon lime-water.

LASTLY, when a dram of it is digeſted with an ounce of water in a bottle for ſome hours, it does not make any the leaſt change in the water. The *magneſia*, when dried, is found to have gained ten grains; but it neither effervеſces with acids, nor does it ſenſibly affect lime-water.

OBSERVING *magneſia* to loſe ſuch a remarkable proportion of its weight in the fire, my next attempts were directed to the inveſtigation of this volatile part, and, among other experiments, the following ſeemed to throw ſome light upon it.

THREE ounces of *magneſia* were diſtilled in a glaſs retort and receiver, the fire being gradually increaſed until the *magneſia* was obſcurely red hot. When all was cool, I found only five drams of a whitiſh water in the receiver, which had a faint ſmell of the ſpirit of hartſhorn, gave a green colour to the juice of violets, and rendered the ſolutions of corroſive ſublimate and of ſilver very ſlightly

turbid,

turbid. But it did not ſenſibly efferveſce with acids.

THE *magneſia*, when taken out of the retort, weighed an ounce, three drams, and thirty grains, or had loſt more than the half of its weight. It ſtill efferveſced pretty briſkly with acids, tho' not ſo ſtrongly as before this operation.

THE fire ſhould have been raiſed here to the degree requiſite for the perfect calcination of *magneſia*. But even from this imperfect experiment, it is evident, that of the volatile parts contained in that powder, a ſmall proportion only is water; the reſt cannot, it ſeems, be retained in veſſels, under a viſible form. Chemiſts have often obſerved, in their diſtillations, that part of a body has vaniſhed from their ſenſes, notwithſtanding the utmoſt care to retain it; and they have always found, upon further inquiry, that ſubtile part to be air, which having been impriſoned in the body, under a ſolid form, was ſet free and rendered fluid and elaſtic by the fire. We may therefore ſafely conclude, that the volatile matter, loſt in the calcination of *magneſia*, is moſtly air; and hence the calcined *magneſia*

nesia does not emit air, or make an effervescence, when mixed with acids.

The water, from its properties, seems to contain a small portion of volatile alkali, which was probably formed from the earth, air, and water, or from some of these combined together; and perhaps also from a small quantity of inflammable matter which adhered accidentally to the *magnesia*. Whenever Chemists meet with this salt, they are inclined to ascribe its origin to some animal, or putrid vegetable, substance; and this they have always done, when they obtained it from the calcarious earths, all of which afford a small quantity of it. There is, however, no doubt that it can sometimes be produced independently of any such mixture, since many fresh vegetables and tartar afford a considerable quantity of it. And how can it, in the present instance, be supposed, that any animal or vegetable matter adhered to the *magnesia*, while it was dissolved by an acid, separated from this by an alkali, and washed with so much water?

Two drams of *magnesia* were calcined in a crucible, in the manner described above, and thus reduced to two scruples and twelve grains.

grains. This calcined *magnesia* was dissolved in a sufficient quantity of spirit of vitriol, and then again separated from the acid by the addition of an alkali, of which a large quantity is necessary for this purpose. The *magnesia* being very well washed and dryed, weighed one dram and fifty grains. It effervesced violently, or emitted a large quantity of air, when thrown into acids, formed a red powder when mixed with a solution of sublimate, separated the calcarious earths from an acid, and sweetened lime-water: and had thus recovered all those properties which it had but just now lost by calcination: nor had it only recovered its original properties, but acquired besides an addition of weight nearly equal to what had been lost in the fire; and, as it is found to effervesce with acids, part of the addition must certainly be air.

This air seems to have been furnished by the alkali from which it was separated by the acid; for Dr. *Hales* has clearly proved, that alkaline salts contain a large quantity of fixed air, which they emit in great abundance when joined to a pure acid. In the present case, the alkali is really joined to an acid, but without any visible emission of air; and yet the

the air is not retained in it: for the neutral ſalt, into which it is converted, is the ſame in quantity, and in every other reſpect, as if the acid employed had not been previouſly ſaturated with *magneſia*, but offered to the alkali in its pure ſtate, and had driven the air out of it in their conflict. It ſeems therefore evident, that the air was forced from the alkali by the acid, and lodged itſelf in the *magneſia*.

These conſiderations led me to try a few experiments, whereby I might know what quantity of air is expelled from an alkali, or from *magneſia*, by acids.

Two drams of a pure fixed alkaline ſalt, and an ounce of water, were put into a Florentine flaſk, which, together with its contents, weighed two ounces and two drams. Some oil of vitriol diluted with water was dropt in, until the ſalt was exactly ſaturated; which it was found to be, when two drams, two ſcruples, and three grains of this acid had been added. The vial with its contents now weighed two ounces, four drams, and fifteen grains. One ſcruple, therefore, and eight grains were loſt during the ebullition, of which a trifling portion may be water, or

 ſomething

ſomething of the ſame kind. The reſt is air.

THE celebrated *Homberg* has attempted to eſtimate the quantity of ſolid ſalt contained in a determined portion of the ſeveral acids. He ſaturated equal quantities of an alkali with each of them; and, obſerving the weight which the alkali had gained, after being perfectly dryed, took this for the quantity of ſolid ſalt contained in that ſhare of the acid which performed the ſaturation. But we learn from the above experiment, that his eſtimate was not accurate, becauſe the alkali loſes weight as well as gains it.

Two drams of *magneſia*, treated exactly as the alkali in the laſt experiment, were juſt diſſolved by four drams, one ſcruple, and ſeven grains of the ſame acid liquor, and loſt one ſcruple and ſixteen grains by the ebullition.

Two drams of *magneſia* were reduced, by the action of a violent fire, to two ſcruples and twelve grains, with which the ſame proceſs was repeated, as in the two laſt experiments; four drams, one ſcruple, and two grains of the ſame acid were required to compleat

pleat the ſolution, and no weight was loſt in the experiment.

As in the ſeparation of the volatile from the fixed parts of bodies, by means of heat, a ſmall quantity of the latter is generally raiſed with the former; ſo the air and water, originally contained in the *magneſia*, and afterwards diſſipated by the fire, ſeem to have carried off a ſmall part of the fixed earth of this ſubſtance. This is probably the reaſon, why calcined *magneſia* is ſaturated with a quantity of acid, ſomewhat leſs than what is required to diſſolve it before calcination: and the ſame may be aſſigned as one cauſe which hinders us from reſtoring the whole of its original weight, by ſolution and precipitation.

I took care to dilute the vitriolic acid, in order to avoid the heat and ebullition which it would otherwiſe have excited in the water; and I choſe a Florentine flaſk, on account of its lightneſs, capacity, and ſhape, which is peculiarly adapted to the experiment; for the vapours raiſed by the ebullition circulated for a ſhort time, thro' the wide cavity of the vial, but were ſoon collected upon its ſides, like dew, and none of them ſeemed to reach the

the neck, which continued perfectly dry to the end of the experiment.

We now perceive the reafon, why crude and calcined *magnefia*, which differ in many refpects from one another, agree however in compofing the fame kind of falt, when diffolved in any particular acid; for the crude *magnefia* feems to differ from the calcined chiefly by containing a confiderable quantity of air, which air is unavoidably diffipated and loft during the diffolution.

From our experiments, it feems probable, that the increafe of weight which fome metals acquire, by being firft diffolved in acids, and then feparated from them again by alkalis, proceeds from air furnifhed by the alkalis. And that in the *aurum fulminans*, which is prepared by the fame means, this air adheres to the gold in fuch a peculiar manner, that, in a moderate degree of heat, the whole of it recovers its elafticity in the fame inftant of time; and thus, by the violent fhock which it gives to the air around, produces the loud crack or fulmination of this powder. Thofe who will imagine the explofion of fuch a minute portion of fixed air, as can refide in the *aurum fulminans*, to be infufficient for the

exceffive

exceſſive loudneſs of the noiſe, will conſider, that it is not a large quantity of motion communicated to the air, but rather a ſmart ſtroke which produces ſound, and that the exploſion of but a few particles of fixed air may be capable of cauſing a loud noiſe, provided they all recover their ſpring ſuddenly, and in the ſame inſtant.

The above experiments lead us alſo to conclude, that volatile alkalis, and the common abſorbent earths, which loſe their air by being joined to acids, but ſhew evident ſigns of their having recovered it, when ſeparated from them by alkalis, received it from theſe alkalis which loſt it in the inſtant of their joining with the acid.

The following are a few experiments upon three of the abſorbent earths, made in order to compare them with one another, and with *magneſia*.

Suſpecting that *magneſia* might poſſibly be no other than a common calcarious earth, which had changed its nature, by having been previouſly combined with an acid, I ſaturated a ſmall quantity of chalk with the muriatic acid, ſeparated the acid from it again by means

means of a fixed alkali, and carefully waſh-ed away the whole of the ſalt.

The chalk when dryed was not found to have ſuffered any alteration; for it effervеſ-ced with the vitriolic acid, but did not diſſolve in it; and when expoſed to a violent fire, was converted into a quick-lime, in all re-ſpects ſimilar to that obtained from common chalk.

In another experiment of the ſame kind, I uſed the vitriolic acid with the ſame event.

Any calcarious matter reduced to a fine powder, and thrown into a warm ſolution of alum, immediately raiſes a briſk effervеſcence. But the powder is not diſſolved; it is rather increaſed in bulk: and if the addition be re-peated until it is no longer accompanied with effervеſcence, the liquor loſes all taſte of the alum, and yields only a very light cloud upon the admixture of an alkali.

From this experiment we learn, that acids attract the calcarious earths more ſtrongly than they do the earth of alum; and as the acid in this ſalt is exactly the ſame with the vitriolic, it compoſes with the calcarious earth a neutral ſubſtance, which is very diffi-cultly ſoluble in water, and therefore falls

down

down to the bottom of the veſſel along with the earth of alum which is deprived of its acid. The light cloud formed by the alkali proceeds from the minute portion of the calcarious compound which ſaturates the water.

The earth of animal bones, when reduced to a fine powder and thrown into a diluted vitriolic acid, gradually abſorbs the acid in the ſame manner as the calcarious earths, but without any remarkable effervefcence. When it is added to the nitrous or to the muriatic acid, it is ſlowly diſſolved. The compound liquor thence produced is extremely acrid, and ſtill changes the colour of the juice of violets to a red, even after it is fully ſaturated with the abſorbent. Diſtilled vinegar has little or no effect upon this earth; for after a long digeſtion it ſtill retains its ſour taſte, and gives only a light cloud upon the addition of an alkali.

By dropping a diſſolved fixed alkali into a warm ſolution of alum, I obtained the earth of this ſalt, which, after being well waſhed and dried, was found to have the following properties.

It is diſſolved in every acid but very ſlowly, unleſs aſſiſted by heat. The ſeveral ſolutions, when thoroughly ſaturated, are all aſtringent with a ſlight degree of an acid taſte, and they alſo agree with a ſolution of alum in this, that they give a red colour to the infuſion of turnſol.

Neither this earth, nor that of animal bones, can be converted into quick-lime by the ſtrongeſt fire, nor do they ſuffer any change worth notice. Both of them ſeem to attract acids but weakly, and to alter their properties leſs when united to them than the other abſorbents.

Part II.

In reflecting afterwards upon theſe experiments, an explication of the nature of lime offered itſelf, which ſeemed to account, in an eaſy manner, for moſt of the properties of that ſubſtance.

It is ſufficiently clear, that the calcarious earths in their native ſtate, and that the alkalis and magneſia in their ordinary condition, contain a large quantity of fixed air, and this

this air certainly adheres to them with confiderable force, fince a ftrong fire is neceffary to feparate it from magnefia, and the ftrongeft is not fufficient to expell it entirely from fixed alkalis, or take away their power of effervefcing with acid falts.

These confiderations led me to conclude, that the relations between fixed air and alkaline fubftances was fomewhat fimilar to the relation between thefe and acids; that as the calcarious earths and alkalis attract acids ftrongly and can be faturated with them, fo they alfo attract fixed air, and are in their ordinary ftate faturated with it: and when we mix an acid with an alkali or with an abforbent earth, that the air is then fet at liberty, and breaks out with violence; becaufe the alkaline body attracts it more weakly than it does the acid, and becaufe the acid and air cannot both be joined to the fame body at the fame time.

I alfo imagined, that, when the calcarious earths are expofed to the action of a violent fire, and are thereby converted into quicklime, they fuffer no other change in their compofition than the lofs of a fmall quantity of water and of their fixed air. The remarkable acrimony which we perceive in them after this procefs, was not fuppofed to proceed from any additional matter received in the fire, but feemed to be an effential property of the pure earth, depending on an attraction for thofe feveral fubftances which it then became capable of corroding or diffolving, which attraction had been infenfible as long as the air adhered to the earth, but difcovered itfelf upon the feparation.

This fuppofition was founded upon an obfervation of the moft frequent confequences of combining bodies in chemiftry. Commonly when we join two bodies together, their acrimony or attraction for other fubftances becomes immediately either lefs perceivable or entirely infenfible; altho' it was fufficiently ftrong and remarkable before their union, and may be rendered evident again by disjoining them. A neutral falt, which is compofed of an acid and alkali, does not poffefs the acrimony of either of its conftituent parts. It can eafily be feparated from water, has little or no effect upon metals, is incapable of being joined to inflammable bodies, and of corroding and diffolving animals and vegetables; fo that the attraction both of the acid and alkali for thefe feveral

fubftances

fubftances feems to be fufpended till they are again feparated from one another.

CRUDE lime was therefore confidered as a peculiar acrid earth rendered mild by its union with fixed air: and quick-lime as the fame earth, in which, by having feparated the air, we difcover that acrimony or attraction for water, for animal, vegetable, and for inflammable fubftances.

THAT the calcarious earths really lofe a large quantity of air when they are burnt to quick-lime, feems fufficiently proved by an experiment of Mr. *Margraaf**, an exceedingly accurate and judicious Chemift. He fubjected eight ounces of *ofteocolla* to diftillation in an earthen retort, finifhing his procefs with the moft violent fire of a reverberatory, and caught in the receiver only two drams of water, which by its fmell and properties fhewed itfelf to be flightly alkaline. He does not tell us the weight of the *ofteocolla* remaining in the retort, and only fays, that it was converted into quick-lime; but as no calcarious earth can be converted into quick-lime, or bear the heat which he applied without lofing above a third of its weight, we may fafely

* Mem. de l'Acad. de Berlin. an. 1748, p. 57.

ſafely conclude, that the loſs in his experiment was proportional, and proceeded chiefly from the diſſipation of fixed air.

According to our theory, the relation of the calcarious earth to air and water appeared to agree with the relation of the ſame earth to the vitriolic and vegetable acids. As chalk for inſtance has a ſtronger attraction for the vitriolic than for the vegetable acid, and is diſſolved with more difficulty when combined with the firſt, than when joined to the ſecond; ſo it alſo attracts air more ſtrongly than water, and is diſſolved with more difficulty when ſaturated with air than when compounded with water only.

A calcarious earth deprived of its air, or in the ſtate of quick-lime, greedily abſorbs a conſiderable quantity of water, becomes ſoluble in that fluid, and is then ſaid to be ſlaked; but as ſoon as it meets with fixed air, it is ſuppoſed to quit the water and join itſelf to the air, for which it has a ſuperior attraction, and is therefore reſtored to its firſt ſtate of mildneſs and inſolubility in water.

When ſlaked lime is mixed with water, the fixed air in the water is attracted by the

the lime, and ſaturates a ſmall portion of it, which then becomes again incapable of diſſolution, but part of the remaining ſlaked lime is diſſolved and compoſes lime-water.

If this fluid be expoſed to the open air, the particles of quick-lime which are neareſt the ſurface gradually attract the particles of fixed air which float in the atmoſphere. But at the ſame time that a particle of lime is thus ſaturated with air, it is alſo reſtored to its native ſtate of mildneſs and inſolubility; and as the whole of this change muſt happen at the ſurface, the whole of the lime is ſucceſſively collected there under its original form of an inſipid calcarious earth, called the cream or cruſts of lime-water.

When quick-lime itſelf is expoſed to the open air, it abſorbs the particles of water and of fixed air which come within its ſphere of attraction, as it meets with the firſt of theſe in greateſt plenty, the greateſt part of it aſſumes the form of ſlaked lime; the reſt is reſtored to its original ſtate; and if it be expoſed for a ſufficient length of time, the whole of it is gradually ſaturated with air, to which the water as gradually yields its place.

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We have already ſhewn by experiment, that magneſia alba is a compound of a peculiar earth and fixed air. When this ſubſtance is mixed with lime-water, the lime ſhews a ſtronger attraction for fixed air than that of the earth of magneſia; the air leaves this powder to join itſelf to the lime. And as neither the lime when ſaturated with air, nor the magneſia when deprived of it, are ſoluble in water, the lime-water becomes perfectly pure and inſipid, the lime which it contained being mixed with the magneſia. But if the magneſia be deprived of air by calcination before it is mixed with the lime-water, this fluid ſuffers no alteration.

If quick-lime be mixed with a diſſolved alkali, it likeways ſhews an attraction for fixed air ſuperior to that of the alkali. It robs this ſalt of its air, and thereby becomes mild itſelf, while the alkali is conſequently rendered more corroſive, or diſcovers its natural degree of acrimony or ſtrong attraction for water, and for bodies of the inflammable, and of the animal and vegetable kind; which attraction was leſs perceivable as long as it was ſaturated with air. And the volatile alkali when deprived of its air, beſides this

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attraction for various bodies, discovers likeways its natural degree of volatility, which was formerly somewhat repressed by the air adhering to it, in the same manner as it is repressed by the addition of an acid.

This account of lime and alkalis recommended itself by its simplicity, and by affording an easy solution of many *phænomena*, but appeared upon a nearer view to be attended with consequences that were so very new and extraordinary, as to render suspicious the principles from which they were drawn.

I resolved however to examine, in a particular manner, such of these consequences as were the most unavoidable, and found, the greatest number of them might be reduced to the following propositions:

I. If we only separate a quantity of air from lime and alkalis, when we render them caustic they will be found to lose part of their weight in the operation, but will saturate the same quantity of acid as before, and the saturation will be performed without effervescence.

II. If

II. If quick-lime be no other than a calcarious earth deprived of its air, and whose attraction for fixed air is stronger than that of alkalis, it follows, that, by adding to it a sufficient quantity of alkali saturated with air, the lime will recover the whole of its air, and be entirely restored to its original weight and condition: and it also follows, that the earth separated from lime-water by an alkali, is the lime which was dissolved in the water now restored to its original mild and insoluble state.

III. If it be supposed that slaked lime does not contain any parts which are more firey, active or subtile than others, and by which chiefly it communicates its virtues to water; but that it is an uniform compound of lime and water: it follows, that, as part of it can be dissolved in water, the whole of it is also capable of being dissolved.

IV. If the acrimony of the caustic alkali does not depend on any part of the lime adhering to it, a caustic or soap-ley will consequently be found to contain no lime, unless the quantity of lime employed in making it were greater than what is just sufficient to extract the whole air of the alkali; for then as much of the ſuperfluous quick-lime might poſſibly be diſſolved by the ley as would be diſſolved by pure water, or the ley would contain as much lime as lime-water does.

V. We have ſhewn in the former experiments, that abſorbent earths loſe their air when they are joined to an acid; but recover it, if ſeparated again from that acid, by means of an ordinary alkali: the air paſſing from the alkali to the earth, at the ſame time that the acid paſſes from the earth to the alkali.

If the cauſtic alkali therefore be deſtitute of air, it will ſeparate magneſia from an acid under the form of a magneſia free of air, or which will not efferveſce with acids; and the ſame cauſtic alkali will alſo ſeparate a calcarious earth from acids under the form of a calcarious earth deſtitute of air, but ſaturated with water, or under the form of ſlaked lime.

These were all neceſſary concluſions from the above ſuppoſitions. Many of them appeared too improbable to deſerve any further attention: ſome however, I found upon reflection, were already ſeconded by experience. Thus *Hoffman* has obſerved, that quicklime does not efferveſce with ſpirit of vitriol*; and it is well known that the cauſtic ſpirit of urine, or of ſalt ammoniac, does not emit air, when mixed with acids. This conſideration excited my curioſity, and determined me to inquire into the truth of them all by way of experiment. I therefore engaged myſelf in a ſet of trials; the hiſtory of which is here ſubjoined. Some new facts are likeways occaſionally mentioned; and here it will be proper to inform the reader, that I have never mentioned any without ſatisfying myſelf of their truth by experiment, tho' I have ſometimes taken the liberty to neglect deſcribing the experiments when they ſeemed ſufficiently obvious.

DESIRING to know how much of an acid a calcarious earth will abſorb, and what quantity of air is expelled during the diſſolution, I ſaturated two drams of chalk with diluted ſpirit of ſalt, and uſed the Florentine flaſk, as related in a ſimilar experiment upon magneſia. Seven drams and one grain of the acid finiſhed the diſſolution, and the chalk loſt two ſcruples and eight grains of air.

THIS experiment was neceſſary before the following, by which I propoſed to inquire into

* Hoff. Op. T. iv. p. 480.

into the truth of the firſt propoſition ſo far as it relates to quick-lime.

Two drams of chalk were converted into a perfect quick-lime, and loſt two ſcruples and twelve grains in the fire. This quick-lime was ſlaked or reduced to a milky liquor with an ounce of water, and then diſſolved in the ſame manner, and with the ſame acid, as the two drams of chalk in the preceding experiment. Six drams, two ſcruples and fourteen grains of the acid finiſhed the ſaturation without any ſenſible effervеſcence or loſs of weight.

It therefore appears from theſe experiments, that no air is ſeparated from quick-lime by an acid, and that chalk ſaturates nearly the ſame quantity of acid after it is converted into quick-lime as before.

With reſpect to the ſecond propoſition, I tried the following experiments.

A piece of perfect quick-lime made from two drams of chalk, and which weighed one dram and eight grains, was reduced to a very fine powder, and thrown into a filtrated mixture of an ounce of a fixed alkaline ſalt and two ounces of water. After a ſlight digeſtion, the powder being well waſhed and dried,

dried, weighed one dram and fifty eight grains. It was ſimilar in every trial to a fine powder of ordinary chalk, and was therefore ſaturated with air which muſt have been furniſhed by the alkali.

A dram of pure ſalt of tartar was diſſolved in fourteen pounds of lime-water, and the powder thereby precipitated, being carefully collected and dried, weighed one and fifty grains. When expoſed to a violent fire, it was converted into a true quick-lime, and had every other quality of a calcarious earth.

THIS experiment was repeated with the volatile alkali, and alſo with the foſſil or alkali of ſea-ſalt, and exactly with the ſame event.

THE third propoſition had leſs appearance of probability than the foregoing; but, as an accurate experiment was the only teſt of its truth, I reduced eight grains of perfect quick-lime made of chalk, to an exceedingly ſubtile powder, by ſlaking it in two drams of diſtilled water boiling hot, and immediately threw the mixture into eighteen ounces of diſtilled water in a flaſk. After ſhaking it, a light ſediment, which floated thro' the liquor, was allowed to ſubſide; and this, when collected

collected with the greateſt care, and dryed, weighed, as nearly as I could gueſs, one third of a grain. The water taſted ſtrongly of the lime, had all the qualities of lime-water, and yielded twelve grains of precipitate, upon the addition of ſalt of tartar. In repeating this experiment, the quantity of ſediment was ſometimes leſs than the above, and ſometimes amounted to half a grain. It conſiſted partly of an earth which efferveſced violently with *aqua fortis*, and partly of an ochry powder, which would not diſſolve in that acid. The ochry powder, as it uſually appears in chalk to the eye, in the form of veins running thro' its ſubſtance, muſt be conſidered only as an accidental or foreign admixture; and, with reſpect to the minute portion of alkaline earth which compoſed the remainder of the ſediment, it cannot be ſuppoſed to have been originally different from the reſt, and incapable, from its nature, of being converted into quick-lime, or of being diſſolved in water; it ſeems rather to have conſiſted of a ſmall part of the chalk in its mild ſtate, or ſaturated with air, which had either remained, for want of a ſufficient fire to drive it out entirely, or had been furniſhed by the diſtilled water.

I indeed expected to see a much larger quantity of sediment produced from the lime, on account of the air which water constantly contains, and with a view to know whether water retains its air when fully saturated with lime, a lime-water was made as strong as possible; four ounces of which were placed under the receiver of an air-pump, together with four ounces of common water in a vial of the same size; and, upon exhausting the receiver, without heating the vials, the air arose from each in nearly the same quantity: from whence it is evident, that the air, which quick-lime attracts, is of a different kind from that which is mixed with water. And that it is also different from common elastic air, is sufficiently proved by daily experience; for lime-water, which soon attracts air, and forms a crust when exposed in open and shallow vessels, may be preserved, for any time, in bottles which are but slightly corked, or closed in such a manner as would allow free access to elastic air, were a vacuum formed in the bottle. Quick-lime therefore does not attract air when in its most ordinary form, but is capable of being joined to one particular species only, which is dispersed thro' the atmosphere,

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either in the ſhape of an exceedingly ſubtile powder, or more probably in that of an elaſtic fluid. To this I have given the name of fixed air, and perhaps very improperly; but I thought it better to uſe a word already familiar in philoſophy, than to invent a new name, before we be more fully acquainted with the nature and properties of this ſubſtance, which will probably be the ſubject of my further inquiry.

It is, perhaps, needleſs to mention here, that the calcarious ſubſtances uſed in making the above experiments ſhould be of the pureſt kind, and burnt with the utmoſt violence of heat, if we would be ſure of converting them into perfect quick-lime. I therefore made uſe of chalk burnt in a ſmall covered crucible with the fierceſt fire of a Black-ſmith's forge, for half an hour, and found it neceſſary to employ, for this purpoſe, a crucible of the *Auſtrian* kind, which reſemble black lead; for if any calcarious ſubſtance be heated to ſuch a degree in an ordinary or *Heſſian* crucible, the whole of it is melted down, together with part of the veſſel, into glaſs.

I now prepared to inquire into the properties of the cauſtic alkali; in order to which,

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I made a cauftic or foap ley in the following manner.

Twenty fix ounces of very ftrong quick-lime made of chalk, were flaked or reduced to a fort of fluid pafte, with eleven pounds of boiling water, and then mixed in a glafs veffel with eighteen ounces of a pure fixed alkaline falt, which had been firft diffolved in two pounds and a half of water. This mixture was fhaken frequently for two hours, when the action of the lime upon the alkali was fuppofed to be over, and nothing remained but to feparate them again from one another. I therefore added 12 pounds of water, ftirred up the lime, and, after allowing it to fettle again, poured off as much of the clear ley as poffible.

The lime and alkali were mixed together under the form of a very thick milky liquor or fluid pafte; becaufe they are thus kept in perpetual contact and equal mixture until they have acted fufficiently upon one another: whereas in the common way of ufing a larger quantity of water, the lime lies for the moft part at bottom, and, tho' ftirred up ever fo often, cannot exert its influence fo fully upon the

the alkali, which is uniformly diffuſed thro' every part of the liquor.

THE above ley was found upon trial to be ſaturated by acids without the leaſt effervescence or diminution of weight.

IT was now proper to examine whether the alkali ſuffered any loſs in becoming cauſtic, which I propoſed to attempt by aſcertaining the ſtrength of the ley, or the quantity of ſalt which a given portion of it contained; from which by computation ſome imperfect knowledge might be obtained of the quantity of cauſtic produced from the eighteen ounces of mild ſalt.

I therefore evaporated ſome of my ley, but ſoon perceived that no certain judgment could be formed of its ſtrength in this way, becauſe it always abſorbed a conſiderable quantity of air during the evaporation, and the dried ſalt made a pretty briſk effervescence with acids, ſo that the ley appeared ſtronger than it really was; and yet, upon proceeding in the eſtimate from this rude and unfair trial, it appeared that the ſalt had loſt above a ſixth in becoming cauſtic, and the quantity of acid ſaturated by two drams of it was to the quantity of acid faturated by two drams of falt of tartar, nearly as fix to five.

THESE experiments are therefore agreeable to that part of the fecond propofition which relates to the cauftic alkali.

UPON farther examining what changes the alkali had undergone, I found that the ley gave only an exceeding faint milky hue to lime-water; becaufe the cauftic alkali wants that air by which falt of tartar precipitates the lime. When a few ounces of it were expofed in an open fhallow veffel for four and twenty hours, it imbibed a fmall quantity of air, and made a flight effervefcence with acids. After a fortnight's expofure in the fame manner, it became entirely mild, effervefced as violently with acids, and had the fame effect upon lime-water as a folution of an ordinary alkali. It likeways agrees with lime-water in this refpect, that it may be kept in clofe veffels, or even in bottles which are but flightly covered, for a confiderable time, without abforbing a fenfible quantity of air.

IN order to know how much lime it contained, I evaporated ten ounces in a fmall filver ſilver diſh over a lamp, and melted the ſalt, after having diſſipated the water *.

THE cauſtic thus produced was diſſolved again in a ſmall quantity of water, and depoſited a trifling portion of ſediment, which I imagined at firſt to be lime; but finding that it could eaſily be diſſolved in a little more water, concluded it to be a vitriolated tartar, which always accompanies the fixed alkali of vegetables.

I then ſaturated the ſolution of the cauſtic ſalt with ſpirit of vitriol, expecting thus to detect the lime; becauſe that acid precipitates a calcarious earth from its ordinary ſolutions. During the ſaturation, a large quantity of white powder was formed; but this likeways turned out to be a vitriolated tartar, which had appeared in the form of a powder, becauſe there was not enough of water in the mixture to diſſolve it.

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* This evaporation was performed in a ſilver diſh, on account of the acrimony of the ſalt; which is ſo very great, that, having once evaporated a part of the ſame ley in a bowl of Engliſh earthen or ſtone ware, and melted the cauſtic with a gentle heat, it corroded and diſſolved a part of the bowl, and left the inſide of it pitted with ſmall holes.

Lastly, I expoſed a few ounces of the ley in an open ſhallow veſſel ſo long, that the alkali loſt the whole of its cauſticity, and ſeemed entirely reſtored to the ſtate of an ordinary fixed alkali; but it did not however depoſite a ſingle atom of lime. And to aſſure myſelf that my cauſtic ley was not of a ſingular kind, I repeated the ſame experiments with an ordinary ſoap-ley, and with one made by mixing one part of a pure fixed alkaline ſalt with three parts of common ſtone lime freſh ſlaked and ſifted; nor could I diſcover any lime in either. The firſt of theſe contained a ſmall quantity of brimſtone, and was far from being perfectly cauſtic, for it made a pretty briſk efferveſcence with acids; but the laſt was ſo entirely deprived of its air, that it did not diminiſh in the leaſt the tranſparency of lime-water.

These experiments ſeem therefore to ſupport the fourth propoſition, and to ſhew that the cauſtic alkali does not contain any lime.

As it ſeems probable, from the quickneſs and eaſe wherewith the alkali was rendered cauſtic, that more lime had been employed than what was juſt ſufficient to extract the whole

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of its air, we are ſurpriſed to find that little or none of the ſuperfluous quick-lime was diſſolved by the water. But this *phænomenon* will become leſs ſurprizing, by comparing it with ſome ſimilar inſtances in chemiſtry. Water may be made to depóſite a ſalt, by the admixture of a ſubſtance which it attracts more ſtrongly than it does that ſalt; ſuch as ſpirit of wine; and quick-lime itſelf may be ſeparated from water upon the ſame principle; for if that ſpirit is added to an equal quantity of lime-water, the mixture becomes turbid and depoſites a ſediment, which, when ſeparated and diſſolved again in diſtilled water, compoſes lime-water. We may therefore refer the above *phænomenon*, with reſpect to the ley, to the ſame cauſe with theſe, and ſay, that the water did not diſſolve the lime, becauſe it already contained a cauſtic alkali, for which it has a ſuperior attraction.

I alſo rendered the volatile alkali cauſtic, in order to examine what change it ſuffered in the operation, and obtained an exceedingly volatile and acrid ſpirit, which neither efferveſced with acids, nor altered in the leaſt the tranſparency of lime-water; and, altho' ve-

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ry ſtrong, was lighter than water, and floated upon it like ſpirit of wine.

I next inquired into the truth of the fifth propoſition, in the following manner.

Two drams of epſom-ſalt were diſſolved in a ſmall quantity of water, and thrown into two ounces of the cauſtic-ley; the mixture inſtantly became thick, like a decoction of ſtarch or barley, by the magneſia, which was precipitated. I then added ſpirit of vitriol by degrees, until the mixture became perfectly clear, or the whole of the magneſia was again diſſolved; which happened without any effervescence or emiſſion of air.

Half an ounce of chalk was diſſolved in ſpirit of ſalt, the quantity of which was ſo adjuſted, that the mixture was not acid in the leaſt degree; and the ſolution was thrown into twelve ounces of the cauſtic ley; which quantity I found, by experiment, to be ſufficient for precipitating almoſt the whole of the chalk. I now filtrated this turbid liquor, and laid the powder remaining in the paper upon a chalk-ſtone, in order to draw as much of the water from it as poſſible, and thereby reduce it to the form of a more denſe and heavy powder, that it might ſubſide the more perfectly

perfectly in the following part of the experiment. I then mixed it with about twenty ounces of pure water in a flask, and, after allowing the powder to subside, poured off the water, which had all the qualities of lime-water. And I successively converted eight waters more into lime-water, seven of these in the same quantity, and with the same management, as the first. The eighth was likeways in the same quantity; but I allowed it to remain with the chalk, and shook it frequently, for two days. This, after being filtrated, formed a cream or crust upon its surface when exposed to the air; changed the colour of the juice of violets into green; separated an orange-coloured powder from a solution of corrosive sublimate; became turbid upon the addition of an alkali; was entirely sweetened by magnesia; and appeared so strong to the taste, that I could not have distinguished it from ordinary lime-water. And when I threw some salt ammoniac into the lime which remained, the vapour of the volatile alkali immediately arose from the mixture.

In this experiment therefore the air is first driven out of the chalk by an acid, and then,

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in order to ſeparate this acid from it, we add an alkali which has been previouſly deprived of its air; by which means, the chalk itſelf is alſo obtained free of air, and in an acrid form, or in the form of ſlaked lime.

WE have alſo ſeveral proceſſes for obtaining the volatile alkali in a cauſtic form, which ſeem to be only ſo many methods of obtaining it in its pure ſtate, and free of fixed air. The firſt of theſe is the ſeparation of the alkali from an acid, merely by heat; an inſtance of which we have from Mr. *Margraaf* *. He prepared from urine an ammoniacal ſalt, the acid of which is the baſis of the phoſphorus, and is of ſuch a peculiar nature, that it endures a red heat without being diſſipated. Sixteen ounces of the neutral ſalt were ſubjected by him to diſtillation. The acid remained in the retort, and he found in the receiver eight ounces of an alkaline ſpirit, which, he tells us, was extremely volatile, very much reſembling the ſpirit of ſalt ammoniac diſtilled with quick-lime; and no cryſtals were formed in it, when expoſed to the cold air.

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* Mem. de l'Acad. de Berlin, an. 1746, p. 87.

A cauſtic volatile alkali may alſo be obtained, by mixing ſalt ammoniac with half its weight of a cauſtic fixed alkali, or of magneſia which has been previouſly deprived of its air by fire; and then ſubmitting theſe mixtures to diſtillation: Or merely by adding any ordinary volatile alkali to a proper quantity of a cauſtic ley; for in this caſe the air paſſes from the volatile to the fixed alkali, by a ſuperior attraction for the laſt, and, by a gentle heat, the compound yields a ſpirit ſimilar to that prepared from ſalt ammoniac and quick-lime.

It is therefore probable, that, had we alſo a method of ſeparating the fixed alkali from an acid, without, at the ſame time, ſaturating it with air, we ſhould then obtain it in a cauſtic form; but I am not acquainted with an inſtance of this ſeparation in chemiſtry. There are two indeed which, at firſt ſight, appear to be of this kind; theſe are the ſeparation of the fixed alkali from the nitrous acid by means of inflamed charcoal, in the proceſs for making *nitrum fixatum*, and of the ſame alkali, from vegetable acids merely by heat; but, upon examining the product of each proceſs, we find the alkali either fully or nearly ſaturated with air. In the firſt, either the charcoal or the acid, or both together, are almoſt wholly converted into air; a part of which is probably joined to the alkali. In the ſecond, the acid is not properly ſeparated, but rather deſtroyed by the fire: a conſiderable portion of it is converted into an inflammable ſubſtance; and we learn from Dr. *Hales*, that the bodies of this claſs contain a large quantity of fixed air.

WHEN we conſider that the attraction of alkalis for fixed air is weaker than that of the calcarious earths, and reflect upon the effects of heat in chemiſtry, we are led to imagine, that alkalis might be entirely deprived of their air, or rendered perfectly cauſtic, by a fire ſomewhat weaker than that which is ſufficient to produce the ſame change upon lime; but this opinion does not ſeem agreeable to experience.

THE alkalis do, however, acquire ſome degree of cauſticity in a ſtrong fire, as appears from their being more eaſily united with ſpirit of wine after having been kept in fuſion for ſome time. For that fluid, which cannot be tinctured by a mild ſalt of tartar,

will

will ſoon take a very deep colour from a few drops of a ſtrong cauſtic ley. The circumſtances which hinder us from rendering theſe ſalts perfectly cauſtic by heat, are their propenſity to diſſipation in the utmoſt violence of the fire, their extreme acrimony, and the imperfection of our common veſſels. For before the heat becomes very intenſe, the alkalis either evaporate, or diſſolve a part of the crucibles in which they are contained, and often eſcape thro' their pores; which happens, eſpecially as ſoon as they have already acquired ſome degree of additional acrimony, by the loſs of part of their air.

The fuſion alſo, which they ſo readily undergo, is well known by Chemiſts, as a ſtrong obſtacle to the ſeparation of the volatile from the fixed parts of a compound by fire; accordingly, in ſeveral proceſſes, we are directed to add to the fuſible compound ſome porous ſubſtance which is incapable of fuſion, and will retain the whole in a ſpongy form, thereby to facilitate the diſſipation of the volatile parts.

In order to know whether an alkali would loſe a part of its air, and acquire a degree of cauſticity, when expoſed, with this precaution, to the action of a ſtrong fire, I mixed an

ounce

ounce and a half of falt of tartar with three ounces of black-lead, a fubftance of any the moft unchangeable by chemical operations. This mixture I expofed, for feveral hours, in a covered crucible, to a fire fomewhat ftronger than what is neceffary to keep falt of tartar in fufion. When allowed to cool, I found it ftill in the form of a loofe powder; and taking out one half, I diluted it with water, and by filtration obtained a ley, which, when poured into a folution of white marble in *aqua fortis*, precipitated the marble under the form of a weak quick-lime: for the turbid mixture gave a green colour to the juice of violets, and threw up a cruft like that of lime-water; and the precipitated powder collected and mixed with falt ammoniac immediately yielded the fcent of the volatile alkali.

Lest it fhould here be fufpected, that the alkaline qualities of this mixture, and of the precipitated marble, were not owing to a lime into which the marble was converted, but to the alkali itfelf which was added, it is proper to obferve, that I mixed fo fmall a proportion of the ley with the folution of marble as made me fure, from certain experiments, that the whole of the alkali was fpent in performing

ing the precipitation, and was consequently converted into a neutral salt by attracting the acid. The properties therefore of the mixture can only be referred to a lime, as is indeed sufficiently evident from the crust which is peculiar to lime-water.

I was therefore assured by this experiment, that an alkali does really lose a part of its air, and acquire a degree of causticity, by the proper application of heat; but finding by several trials, that the degree of causticity which it had thus acquired was but weak, and that the quick-lime produced in this experiment was exhausted and rendered mild by a small quantity of water, I exposed the crucible together with that half of the alkali which remained in it to a stronger fire, in order to expel a larger quantity of air, and render it more remarkably caustic; but the whole of it was dissipated by the force of the heat, and the black lead, which still retained the form of a loose and subtile powder, yielded little or nothing to water.

We learn then from the above experiment the reason why the alkali newly obtained from the ashes of vegetables is generally of the more acrid kinds of that salt. It never

appears

appears until the ſubject be converted into aſhes, and is ſuppoſed to be formed by the fire, and to be the reſult of a particular combination of ſome of the principles of the vegetable; one of which principles is air, which is contained in large quantity in all vegetable matters whatever. But as ſoon as the ſmalleſt part of a vegetable is converted into aſhes, and an alkali is thus formed, this ſalt neceſſarily ſuffers a calcination, during which it is kept in a ſpongy form by the aſhes, and ſhews a very conſiderable degree of acrimony if immediately applied to the body of an animal; but if the aſhes are for any time expoſed to the air, or if we ſeparate the alkali from them by the addition of a large quantity of water and ſubſequent evaporation, the ſalt imbibes fixed air from the atmoſphere, and becomes nearly ſaturated with it: tho' even in this condition it is generally more acrid than ſalt of tartar, when this is prepared with a gentle heat.

Borax has ſometimes been referred to the claſs of alkalis, on account of ſome reſemblance it bears to thoſe ſalts: but it has been demonſtrated by accurate experiments, that we ſhould rather conſider it as a neutral ſalt;

that it is compofed of an alkali and of a particular faline fubftance called the fedative falt, which adheres to the alkali in the fame manner as an acid, but can be feparated by the addition of any acid whatever, the added acid joining itfelf to the alkali in the place of the fedative falt. As this conjunction of an acid with the alkali of borax happens without the leaft effervefcence, our principles lay us under a neceffity of allowing that alkali to be perfectly free of air, which muft proceed from its being incapable of union with fixed air and with the fedative falt at the fame time: whence it follows, that, were we to mix the fedative falt with an alkali faturated with air, the air would immediately be expelled, or the two falts in joining would produce an effervefcence. This I found to be really the cafe upon making the trial, by mixing a fmall quantity of the fedative falt with an equal quantity of each of the three alkalis, rubbing the mixtures well in a mortar, and adding a little water. It is however proper in this place to obferve, that, if the experiments be made in a different manner, they are attended with a fingular circumftance. If a fmall quantity of the fedative falt be thrown into a

large

large proportion of a diſſolved fixed alkali, the ſedative ſalt gradually diſappears, and is united to the alkali without any efferveſcence; but if the addition be repeated ſeveral times, it will at laſt be accompanied with a briſk efferveſcence, which will become more and more remarkable, until the alkali be entirely ſaturated with the ſedative ſalt.

THIS *phænomenon* may be explained by conſidering the fixed alkalis as not perfectly ſaturated with air: and the ſuppoſition will appear very reaſonable, when we recollect, that thoſe ſalts are never produced without a conſiderable degree of heat, which may eaſily be imagined to diſſipate a ſmall portion of ſo volatile a body as air. Now, if a ſmall quantity of the ſedative ſalt be thrown into an alkaline liquor, as it is very ſlowly diſſolved by water, its particles are very gradually mixed with the atoms of the alkali. They are moſt ſtrongly attracted by ſuch of theſe atoms as are deſtitute of air, and therefore join with them without producing an efferveſcence; or, if they expel a ſmall quantity of air from ſome of the ſalt, this air is at the ſame time abſorbed by ſuch of the

contiguous

contiguous particles as are deſtitute of it, and no efferveſcence appears until that part of the alkali, which was in a cauſtic form or deſtitute of air, be nearly ſaturated with the ſedative ſalt. But if, on the other hand, a large proportion of the ſedative ſalt be perfectly and ſuddenly mixed with the alkali, the whole, or a large part, of the air is as ſuddenly expelled.

In the ſame manner may we alſo explain a ſimilar *phænomenon*, which often preſents itſelf in ſaturating an alkali with the different acids: the efferveſcence is leſs conſiderable in the firſt additions of acid, and becomes more violent as the mixture approaches the point of ſaturation. This appears moſt evidently in making the *ſal diureticus* or regenerated tartar: The particles of the vegetable acid here employed, being always diffuſed thro' a large quantity of water, are more gradually applied to thoſe of the alkali, and during the firſt additions are chiefly united to thoſe that are freeſt of air *.

 That

* Boerh. Operat. Chem. proceſs. LXXVI.

That the fixed alkali, in its ordinary ſtate, is ſeldom entirely ſaturated with air, ſeems to be confirmed by the following experiment.

I expoſed a ſmall quantity of a pure vegetable fixed alkali to the air, in a broad and ſhallow veſſel, for the ſpace of two months; after which I found a number of ſolid cryſtals, which reſembled a neutral ſalt ſo much as to retain their form pretty well in the air, and to produce a conſiderable degree of cold when diſſolved in water. Their taſte was much milder than that of ordinary ſalt of tartar; and yet they ſeemed to be compoſed only of the alkali, and of a larger quantity of air than is uſually contained in that ſalt, and which had been attracted from the atmoſphere: for they ſtill joined very readily with any acid, but with a more violent efferveſcence than ordinary; and they could not be mixed with the ſmalleſt portion of vinegar, or of the ſedative ſalt, without emitting a ſenſible quantity of air.

As it now appeared that ſeveral alkaline ſubſtances have an attraction for fixed air, I tried a few experiments to learn the relative ſtrength of their ſeveral attractions.

Twenty four grains of magnefia in fine powder were mixed with five ounces of the cauftic ley in a fmall vial, which was immediately corked and fhaken frequently for four hours. The ley was then poured off, and the magnefia wafhed with repeated affufions of water, and dried. It had loft about the half of its weight, and when reduced to a fine powder was readily diffolved by acids with an effervefcence which was hardly perceivable: the alkali had therefore extracted its air. I alfo threw fome frefh magnefia into the ley which had been poured off, and thereby rendered it perfectly mild and fimilar to a folution of falt of tartar; fo that it effervefced brifkly with acids.

With an ounce of the mild fpirit of falt ammoniac, I mixed a dram of magnefia in very fine powder which had been previoufly deprived of its air by fire; and obferving that the magnefia had a tendency to concrete into a folid mafs, I fhook the vial very frequently. After fome days the powder was increafed to more than double its former bulk; and when the vial was opened, the alkaline fpirit emitted a moft intolerably pungent fmell. It likewife floated upon water, but was not perfectly

perfectly cauſtic; for it ſtill yielded ſome air when mixed with acids, and alſo rendered lime-water turbid: neither of which would probably have happened if I had uſed a greater quantity of magneſia, or had allowed the mixture to remain a longer time in the vial. I now waſhed out the whole of the mixture into a bowl, and dryed the magneſia until it loſt all ſmell of the alkali. It weighed a dram and fifty eight grains, effer-veſced violently with acids, and therefore contained a large quantity of air, which had been drawn from the alkali by a ſtronger attraction.

Having formerly ſhewn, that magneſia ſaturated with air ſeparates an acid from a cal-carious earth, which it is not able to do after being deprived of its air by fire; I now ſuſpected that the air was the cauſe of this ſeparation, becauſe I found that it was joined to the calcarious earth at the ſame time that the acid was joined to the earth of magneſia; and imagined that a pure calcarious earth might poſſibly have a ſtronger attraction for acids than a pure earth of mgneſia.

I therefore diſſolved two drams of magne-ſia in the marine acid, and thus obtained a compound of an acid and of the pure earth of this ſubſtance; for the air which was at firſt attached to it, was expelled during the diſſolution. I then added thirty grains of ſtrong quick-lime in exceeding fine powder, ſhook the mixture well, and filtrated it. The powder remaining in the paper, after being well waſhed, was found to be a magneſia, which, as I expected, was deſtitute of air; for it was diſſolved by the vitriolic acid without effervеſcence. And the filtrated liquor contained the lime united to the acid; for upon dropping ſpirit of vitriol into it, a white powder was immediately formed.

We muſt therefore acknowledge a ſtronger attraction between the calcarious earths and acids than between theſe and magneſia: but how does it then happen, that, if magneſia ſaturated with air be mixed with a compound of acid and calcarious earth, theſe two laſt, which attract one another the moſt ſtrongly, do not remain united; but the acid is joined to the magneſia, and the calcarious earth to the air which it attracts much more weakly than it does the acid? Is it becauſe the ſum of the forces which tend to join the magneſia to the acid and the calcarious earth to the air

air, is greater than the ſum of the forces which tend to join the calcarious earth to the acid, and the magneſia to the air: and becauſe there is a repulſion between the acid and air, and between the two earths; or they are ſomehow kept aſunder in ſuch a manner as hinders any three of them from being united together?

THE firſt part of this ſuppoſition is favoured by our experiments, which ſeem to ſhew a greater difference between the forces wherewith the calcarious earth and magneſia attract fixed air, than between thoſe which diſpoſe them to unite with the acid. The repulſions however hinted in the ſecond are perhaps more doubtful, tho' they are ſuggeſted in many other inſtances of decompoſition; but the bounds of my preſent purpoſe will not allow me to enter upon this ſubject, which is one of the moſt extenſive in chemiſtry.

WE meet alſo with a difficulty with reſpect to the volatile alkali ſimilar to the above. Thus a calcarious earth that is pure or free of air has a much ſtronger attraction for acids than a pure volatile alkali, as is evident when we mix quick-lime with ſalt

ammoniac;

for the alkali is then immediately detached from the acid: and agreeably to this I found, upon trial, that a pure or cauſtic volatile alkali does not ſeparate a calcarious earth from an acid. Yet, if we mix a mild volatile alkali, which is a compound of alkali and air, with a compound of acid and calcarious earth, theſe two laſt, which attract one another moſt ſtrongly, do not remain united; but the acid is joined to the alkali and the earth to the air, as happens in the precipitation of a calcarious earth from an acid, by means of the common or mild volatile alkali.

I remember likewiſe a parallel inſtance with regard to quick-ſilver. This metal has an attraction for the vitriolic acid, and when joined to it appears under the form of turbith mineral: but this attraction is weaker than that of the fixed alkali for the ſame acid; for if we mix a diſſolved ſalt of tartar with turbith mineral, the turbith is converted into a brown powder, and the alkali into vitriolated tartar; which change happens the ſooner, if the pure or cauſtic alkali is uſed. Yet, if to a compound of quick-ſilver and the nitrous acid, we add a compound of the fixed

alkali

alkali and the vitriolic acid, or a vitriolated tartar, and digest the mixture with a strong heat, the vitriolic acid does not remain with the alkali, but is joined to the quick-silver which it attracts more weakly, composing with it a turbith mineral; while the alkali is joined to the nitrous acid which it likeways attracts more weakly than it does the vitriolic, and is converted into salt-petre.

From some of the above experiments, it appears, that a few alterations may be made in the column of acids in Mr. *Geoffroy*'s table of elective attractions, and that a new column may be added to that table, according to the following scheme, where the alkaline substances are all considered as in their pure state and free of fixed air.

Acids.	Fixed air.
Fixed alkali,	Calcarious earth.
Calcarious earth,	Fixed alkali.
Volatile alkali and magnesia,	Magnesia.
* * * * * *	Volatile alkali.
	* * * *

At

At the foot of the firſt column ſeveral of the metals might follow, and after theſe the earth of alum; but as I don't know what number of the metals ſhould precede that earth, I have left it to be determined by further experience.

The volatile alkali and magneſia are placed in the ſame line of this column; becauſe their force of attraction ſeems pretty equal. When we commit a mixture of magneſia and ſalt ammoniac to diſtillation, the alkali ariſes and leaves the acid with the magneſia; becauſe this earth, by attracting the acid, repreſſes its volatility, and it ſeems alſo to diminiſh the coheſion of the acid and alkali, and to render them ſeparable by a gentle heat. If the magneſia be ſaturated with air, this likewiſe, on account of its volatile nature and attraction for the alkali, is driven up along with it, and makes it appear under a mild form, and in the ſame manner do the alkali and air ariſe from a mixture of ſalt ammoniac and of a crude calcarious earth.

ART. IX.

Of the Analyſis and Uſes of Peat, by ALEXANDER LIND, Eſquire *.

The Analyſis of Peat.

FIVE ounces of peat, hard, dry, and of a deep brown colour, found ſix or ſeven miles from *Edinburgh*, being diſtilled with a gradual heat, gave firſt an ounce and an half of clear water; after which the oil began to come over, firſt yellow, then darker coloured, till it became like tar, and along with it, a yellow or brown coloured acid oily liquor, ſtrongly empyreumatic, to the quantity of ſix drams: this liquor, towards the laſt, became alkaline: but of this there was only a ſmall quantity; for the diſtilled liquor that came over firſt, was found to be conſiderably acid, ſo that it took more than a dram of *ol. tart. per deliquium* to ſaturate it; if the diſtillation had been continued with a very violent heat, there would have come over more alkaline ſpirit with the pitchy oil: the quantity of oil

* 1744.

oil that came over was two drams and an half; it coagulates in the cold, and melts with a ſmall heat. There remained in the retort two ounces of coal; which being carefully burnt in a crucible, left eleven drams and a few grains of fine yellow aſhes.

THAT it may appear how far the *Scotch* and *Dutch* peats agree, I ſhall next give the *analyſis* of ſome *Dutch* peat made by *Degner*. He took twenty four ounces of peat, which being firſt reduced to a powder; and then put in a retort, yielded, by a gentle diſtillation, a good quantity of inſipid phlegm, with an empyreumatical ſmell. This being taken away, there followed next a yellow ſpirit, and about the neck of the retort, remained ſticking a certain white unctuous earth, reſembling a volatile ſalt. The degree of heat being increaſed, there came forth a white ſmoke, with which a red oil riſing at the ſame time, was collected in the receiver, in the form of a thick pitchy matter, ſwiming upon the ſpirit, and ſticking ſtrongly to the ſides of the veſſel. In the retort remained only a black coal.

HAVING weighed each of theſe ſeparately, the coal was nine ounces ſix drams; the

thick

thick pitchy oil, an ounce and an half; the volatile oily ſalt or ſpirit four ounces; the reſt, the inſipid phlegm received in an open receiver.

The phlegm being firſt examined, was nothing but a ſimple clear watery liquor, without taſte or ſmell.

The rediſh yellow liquor that came next, was a liquid volatile unctuous ſalt, compoſed of a watery phlegm, a volatile ſalt, and a little oil, which commonly goes under the name of ſpirit. This mixt with ſpirit of ſea ſalt occaſioned only a few bubbles; it raiſed with oil of vitriol an efferveſcence, and turned muddy; it precipitated a ſolution of mercury in *aqua fortis*, into a black powder, turned ſyrop of violets green, had no effect upon chalk, ſpirit of ſalt ammoniac: ſpirit of harts-horn made no other change upon it, but turning the liquor, which was before muddy, clear. From all which, he ſays, it appears, this ſpirit is of an alkaline nature; and that except a little oil, contains chiefly a volatile alkaline ſalt.

The oil which was found ſwiming upon the water or ſpirit had a ſtrong empyreumatical ſmell, tho' not ſo fetid as the diſtilled oil

of

of animals. yet ſtronger than that of oils diſtilled from bituminous mineral ſubſtances, and was in ſmell the moſt like that of peat ſmoke. In the cold, it congealed into a body of the conſiſtence of ſoap or honey, or rather into a pitchy ſubſtance of a rediſh brown colour, being of a cauſtic pungent taſte; upon the fire it melted like wax, and when cold, looked like the ſoot of peat.

THIS oil eaſily catches fire, but is ſoon again extinguiſhed. When kindled, it burns like weak ſpirit of wine, but not ſo intenſely. When put upon the fire in a ſpoon, if you approach a lighted candle, it kindles into a flame; and, upon the removal of the candle, is immediately extinguiſhed; ſo it muſt be frequently kindled before it wholly conſumes.

THE cinder that remained in the retort, being put in the fire, in a ſhort time turned red, and immediately, without ſmoke or flame, fell into grey aſhes, nothing different from ordinary peat-aſhes. Theſe aſhes contain leſs ſalt than thoſe of wood. All peat-aſhes do not yield the ſame quantity of ſalt, but differ according to the quality of the peat. From a pound of *Dutch* aſhes, *Degner*, after boiling, filtrating, and inſpiſſating in the ordinary

nary way, obtained only half an ounce of a rediſh impure ſalt, mixt with much earth: from other peats that leave after burning a red aſh, and that were taken from dryer ground, he had an ounce of a rediſh impure ſalt.

This ſalt, he ſays, has rather a ſaline than alkaline taſte; and when expoſed to the open air, runs ſlowly, after the manner of ſuch ſaline lixivious ſalt. Being diſſolved in water and mixed with oil of vitriol, it becomes turbid, with a ſmall bubbling up. With fixed ſalt, or ſpirit of harts-horn, it turns muddy; with ſpirit of ſea ſalt, there is no change, the liquor remaining clear, only a few bubbles. It coagulates ſoap when boiled with it, in the ſame manner that ſea ſalt does. This ſalt diſſolved again in water, filtrated and inſpiſſated until it begins to cryſtallize, gathers, when ſet in a cool place, into a ſalt of a cubical form, and when thrown into the fire, makes a noiſe like ſea ſalt that is decrepitated. The liquor being further inſpiſſated and ſet to cryſtallize a ſecond time, yielded a nitrous ſalt; the remaining liquor was a pure lixivious alkaline ſalt, that made a ſtrong efferveſcence with acids. From all which it appears,

pears, that this ſalt is principally compoſed of a ſalt like that of common ſea ſalt, with a ſmall proportion of the alkaline kind.

What remained after the ſalt was extracted out of the aſhes, was only a ſlimy earth and ſome ſand.

To theſe analyſes, I ſhall add two others. Dr. *Smidberg* a Phyſician at the *Hague*, having diſtilled ſome peat in a retort, obtained from it a ſpirit, an oil, a volatile ſalt, all of them much like that of hartshorn. *Ottman* an Apothecary at *Stutberg*, from a *Swabian* peat, had a volatile ſpirit, like that of ſpirit of tartar, a fetid oil, but no volatile ſalt.

The Uſes of Peat.

The principal uſe of peat is burning, not only for the ſervice of families, but likeways for that of a great many trades; ſuch as brewers, bakers, diſtillers, making of lime, *&c.*: and, as there is a good deal of difference in peats, and ſome kinds preferred to others, I ſhall here take notice of ſome of the principal differences.

The

The firſt is, with reſpect to the place out of which they are taken. Such as are got from brackiſh grounds, near the ſea, alſo ſuch as are impregnated with vitriol or ſulphur, have a diſagreeable ſmell, and are hurtful to the health. In *Zealand*, they have a kind of peat, which, when burning, makes every body in the room look like a dead perſon; and, when they ſit long by the fire, grow faintiſh: it alſo turns the bottom of their veſſels white. Peats taken from moſſes, free of all minerals, have none of the above mentioned, or any other bad effect.

As to the matter itſelf, that differs in many reſpects; ſo that in the ſame moſs, according to the different depth of it, there are three or four different kinds of peats found. In *North Britain*, in the province of *Groningen*, and in ſeveral other places, that which is uppermoſt, is light and ſpungy; further down, better; and at bottom, is a ſubſtance that is black, and makes a firm ſolid peat.

In *Holland*, that which lies uppermoſt is beſt, being of a dark or black colour, to which others ſucceed of different colours and ſubſtances not ſo good. That which is light and ſpungy, taken from a barren heathy ground,

ground, or from a dry ſandy ſoil; alſo ſuch moſs as is much mixed with pieces of rotten wood, roots, mud, gravel, or ſand, or which conſuming quickly, leaves behind a great many impurities mixed with its aſhes, is bad.

Peats differ conſiderably, according to the pains beſtowed in making them. Such as are perfectly freed from all hetrogeneous matter, well knead and wrought, are the beſt of all. Upon which account, peats, made in the province of *Holland*, where no labour is ſpared in the working them, are preferable to all others; tho', in other places, the ſubſtance may be equally good. A *Dutch* peat ſix inches long and three or four thick, will weigh a pound; a peat made at *Nimiguen*, of the ſame dimenſions, will not weigh above half a pound, often leſs. It is a general obſervation, that all peats made of moſs-mud, and well knead, are conſiderably heavier than ſuch as are only cut out of the moſs.

Peats that are of a dark colour, and ſolid, that continue longeſt in the fire without conſuming, that have a good cinder, and fall into white aſhes, are moſt eſteemed: on the contrary, ſuch as are light and porous, conſume quickly in the fire, leave no cinder, but a great many impurities and aſhes, are little valued.

THE aſhes of peats differ alſo conſiderably in colour, quantity, and weight; as to which nothing certain can be determined. Sometimes the white, ſometimes the grey, and at other times the red, are heavieſt. In *Frieſland*, the peat that leaves the red aſhes, *Degner* ſays, is heavieſt; about *Nimiguen* thoſe that are red are found lighter than the *Dutch* peat which leaves a grey aſh.

THE Brewer, Diſtiller, and other trades, prefer the peat that leaves a red aſh, which, tho' it ſeldom has a firm cinder, yet burns violently. The Baker makes choice of the light turf, and in *North-Holland*, where the inhabitants are extremely cleanly, they uſe the peat that has red aſhes, upon account of their being heavier than the white, and therefore not ſo apt to fly about and ſpoil their furniture.

BESIDES the uſes now commonly made of peats, there are two others in which, I think, they may be employed with great advantage. The firſt is, the ſmelting iron ore, the only fewel

fewel at prefent ufed in that operation is charcoal of oak, and other hard woods; any attempt made to do it with pit-coal, fo far as I can learn, has hitherto proved unfuccefsful; and indeed from the nature of that fubftance, there feems little hopes of ever bringing it to anfwer the end, the bituminous or inflamable part of pit-coal having nearly the fame effect upon iron, which common fulphur has. It deftroys, as experience fhews, the malleability of iron and all other metals. Pit-coal has likeways another bad quality, which I have often found to my coft. With a ftrong heat it runs into a glaffy fubftance, which in time, by its fticking fo clofely together, and to the fides of the furnace, quite choaks it up, and, by its tenacity, hinders the metallic parts from finking downwards, as they would do by their natural gravity. What is chiefly wanted in fmelting, is an open fire; the furnaces are commonly fufficiently clogged with the ftony and other hetrogeneous bodies united with the ore, which run into glafs without the addition of any fuch foreign matter as has a tendency to vitrification. The char'd wood, on the contrary, keeps always an open fire, the in-

flamable

flamable part of which is ſo far from hurting metals, that it preſerves their malleability, by ſupplying with its own ſulphur that which is deſtroyed or carried off from the metal, by the intenſe heat employed in the ſmelting; and this is ſo far true, that iron reduced to a *calx* or friable ſubſtance by calcination, has its malleability reſtored by being fluxed with powdered charcoal only. And this we may here obſerve, by the bye, as an inſtance of the difference of ſubſtituting a vegetable in place of a mineral ſulphur.

THE peat then being intirely a vegetable ſubſtance, there ſeems nothing more requiſite to make it a proper fewel for ſmelting iron, but the being able to raiſe by its means a heat ſufficient for that purpoſe. This, experience ſhews, cannot be done with the peats we now have. The moſt likely method of obtaining this end, I think, is, to bring them to be as ſolid and compact a ſubſtance as poſſible. The denſeſt bodies, *cæteris paribus*, when thoroughly heated, are, the hotteſt: hence it is, that metals as they are the heavieſt bodies, ſo they reach the greateſt degree of heat. The ſame holds in fewel; the hardeſt woods are made choice of,

when

when a ſtrong heat is wanted; and even in common peats, I have ſhewn you how far preferable the hard and ſolid are to the light and ſpungy. By ſome experiments which I have made, I find it to be no difficult matter, to bring peat to a conſiderable degree of ſolidity, as you yourſelves may ſee by the ſpecimen I now ſhow you. The ſimple operation of grinding, does the buſineſs; and as a peat, when taken out of the moſs, is a ſoft body, and eaſily grinded, a machine may be eaſily contrived to grind, at a moderate expence, ſeveral tuns in a day. The charge of digging peats, cutting them into ſquares or the form of bricks, when of a proper dryneſs, will be little different from that of making peats in the ordinary way. The ſolidity of peat prepared in the manner mentioned is ſurpriſing; its ſpecific gravity being ſomewhat greater than that of pit-coal. I compared a peat of this kind with a piece of coal brought from Baron *Clerk*'s coal-mines near *Edinburgh*, and by the hydroſtatical balance, reckoning water 1000, their ſpecific gravities were nearly as follows, pit-coal 1287, ſolid peat 1303.

FROM

From what has been ſaid, it appears, that, if iron could be made with peat, it would be of great ſervice, particularly in ſome places of *North Britain*, where peat is to be had in plenty, along with iron, which now lies unwrought for want of wood: and even where wood may be found, if peat brought to the conſiſtency I mention would do the buſineſs, it would come cheaper than char'd wood. Another advantage of this kind of peat, would be the ſmelting of lead with it alone, which cannot well be done at preſent, without the help of pit-coal, which in ſome places muſt be brought from a conſiderable diſtance, and at no ſmall charge.

The other uſe I would propoſe of peat, is the employing it as dung, for the fertilizing of ground, when prepared in the manner I ſhall afterwards mention. I am not ignorant, that the aſhes of peats are uſed for that purpoſe with great advantage, not only by themſelves, but likeways mixed with other dung; and even the duſt of peat, that remains at the bottom of peat-ſtacks; but in that ſtate it has not the effects of dung, nor are its effects equal to what they would be, were it rightly prepared. To ſet this matter

in

in a proper light, I muſt be allowed to ſay ſomething in general of vegetation, and of the uſe of dungs in promoting it.

VEGETABLES which increaſe by ſeed, as is the caſe with by far the greateſt part, if not all of them, are at firſt *plantulas* wrapt up in a very ſmall bulk in the end of the ſeed; which, when put in the ground, by the moiſture they find there, extend themſelves, and are firſt nouriſhed by part of the ſeed itſelf, which does the ſame office to the young plant, by affording it a finer nouriſhment, as the *placenta* does to the *embryo*. When the plant becomes ſtronger and ſhoots forth its roots, it then draws its nouriſhment from the earth. Thus it goes on growing until it has attained its utmoſt perfection: after which it gradually decays, dies, and at laſt rots and putrefies. By putrefaction, the parts of which the vegetable was compoſed, *viz.* its ſalts, oils, phlegm and earth, are ſeparated: part remains upon the ground where the plant falls; but the far greateſt part being volatile, flies up into the air, from whence it deſcends again upon the earth and incorporates with it. The ſame materials ſerve to nouriſh new plants, there being no part of them, as we all

know,

know, loſt. What we call a vegetable mould, is an earth in which there is ſtore of ſuch parts of vegetables lodged, the *matrix* in which they lie, being a fine but barren ſand. As long as there is a ſufficient ſtock of ſuch particles in any earth, that ground is fruitful; but when this is exhauſted, which happens ſooner or later, from the quantity of vegetables nouriſhed by it, and carried off for the uſes of life, it becomes barren. The only remedy, when no better can be had, is to allow it to reſt, until it receives a new recruit from the air, in which are perpetually floating, and falling down upon the earth, particles of all kinds, proper for the nouriſhment of plants. But as this is a tedious way of recovering the fertility of ground, the better and more expeditious one, is by laying dung upon it, which being wholly made up of putrefied vegetables, or animals, equally proper for nouriſhing plants, the parts, of which both are compoſed, being the ſame, and the tranſition from the one to the other eaſy; the ground by this new acquiſition becomes again fertile. Every vegetable then whoſe parts are ſet looſe, by that laſt fermentation of nature, putrefaction, affords

affords a proper *pabulum* for vegetables; and the great diſtinction of plants, which commonly lies in a very ſmall part, and that too the moſt volatile, being taken away by putrefaction, all vegetables, when reduced to that ſtate, ſeem to be pretty much upon a *par* for that purpoſe. Now, to return to what I intended to ſay, and to which what I have mentioned was only a kind of preamble; Peat moſs, being wholly a vegetable matter, muſt, if reduced to a thorough ſtate of putrefaction, anſwer the ſame purpoſes for fertilizing ground as other putrefied vegetables. While it lies in the moſs, there is too great a quantity of water, to raiſe a ſufficient degree of heat, to bring the vegetables of which peat-moſs is compoſed, whether actually growing, decaying, or decayed, to a compleat degree of putrefaction. But if it were taken out of the moſs, and laid in heaps like other vegetables to rot, with a degree of moiſture ſuitable for that purpoſe; and if, to begin and alſo quicken the putrefaction, green freſh ſucculent plants were employed in a ſufficient quantity firſt to raiſe a heat; this I make no doubt would, by communicating it to the moſſy ſubſtance, in a ſuitable

 time,

time, and by right management, reduce the whole maſs to the ſtate deſired. This already is in ſome meaſure practiſed in *Holland*, where they mix the duſt of peats with ordinary dung, in making of dung-hills.

I ſhall conclude this paper with only mentioning two others uſes of peat, which I had almoſt forgot, *viz.* that peat-duſt ſtrawed upon ground where peaſe or other ſeeds are ſown, in order to have an early crop, is an excellent preſervative of ſuch vegetables from the froſt; as it keeps the ground warm, by not allowing the cold to penetrate into it. And that there is nothing properer than peat to ſtop water, and to confine it, in the making of fiſh-ponds, *&c.* This I learned from his Grace the Duke of *Argyle*, who I obſerved uſed it with great ſucceſs for that purpoſe.

ART.

ART. X.

The Effects of Semen Hyoscyami albi, *by Dr.* ARCHIBALD HAMILTON *Physician in* Edinburgh *.

—————Student of physic, of a thin habit of body, about twenty years of age, had been, for two years, in the use of taking a small quantity of white henbane-seed to make him sleep, and without any bad effect. But on *Friday March* the 8th 1754, betwixt four and five in the afternoon, he, in order to procure sleep, swallowed about twice as much of this seed as he could take up betwixt his fore-finger and thumb, *i. e.* nearly 25 grains. He felt himself half an hour after, very heavy and much inclined to sleep; his eyes were oppressed, and spirits dejected, with a general lassitude and inactivity over his whole body. These symptoms still increasing, he went abroad and drank tea about six o'clock; and with great difficulty could keep awake, having sometimes let fall the tea-spoon insensibly. He complained of a great uneasiness and

* May 1. 1755.

and dryneſs of his throat, and that the tea in ſwallowing was like to choak him. He had alſo ſome gripes in his belly. After he drank tea, he was ſoon ſeized with convulſions and ſo great a degree of inſenſibility, that he did not know the people who were in company with him. He ſpoke many incoherent things, and at the beginning of his illneſs ſaid he was afraid he had taken poiſon. The people who were with him being alarmed, ordered him to be carried home, and ſent for me. I found him incapable of giving any account of his misfortune. His eyes were open and rolling, now and then he was ſeized with tremors, ſtartings, and convulſions, grapling the bed-cloaths, his head, face, noſe, and other parts of his body, in the manner patients frequently do in nervous fevers. His pulſe exceſſively ſmall and low with *ſubſultus tendinum*. The ſenſe of feeling ſeemed alſo impaired; for when I pinched his ſkin, he made no complaint. He had no inclination to vomit, nor had any ſtool from the time he took the ſeed. I ordered him immediately a vomit; and in the mean time Dr. *Boſwell* was ſent for. He ſpit out the vomit as ſoon as it was poured into to his mouth; so that it appeared he was either not deprived of taste, or could not let the vomit over. A second vomit was instantly given, which was also spilt or spit out. A solution of white vitriol was afterwards given, the most of which he swallowed without the desired effect. A strong clyster with antimonial wine was administred, which he kept about twenty minutes. He got a second injection an hour after. As he continued in the same miserable situation, a blister was applied to his head, and sinapisms to the soles of his feet. He passed the night in the same condition without sleeping, and was alternately seized with convulsions, startings, and catched with his hands at every thing about him. In the morning, he became more sensible, and began to speak a little distinctly, altho' his head was yet very confused and muddy. He told what quantity of the seed he had taken, and for what purpose. His pulse was now somewhat stronger. He got a purgative infusion, which operated four times that day. In the evening, he was still more distinct, altho' his eyes continued heavy and his head confuſed. His pulſe was now quick, full, and ſtrong; and he complained of a great pain and weight in his head. He was blooded about twelve o'clock that night to the quantity of twelve ounces. He ſweated plentifully, had good ſleep; and was altogether ſenſible and diſtinct next morning.

ART.

ART. XI.

The Effects of the Thorn-Apple, by Dr. ABRAHAM SWAINE *Physician at* Brentford *.

ROBERT BULMER, a man of a ſtrong conſtitution, 69 years of age, and who had enjoyed a good ſtate of health all his life, till about two years before, when he was firſt afflicted with the gravel; in *October* 1746, being adviſed by a friend to take a decoction of the fruit of the common burdock, as a remedy for his diſeaſe, by miſtake gathered the fruit of the *ſtramonium* or thorn-apple. After dividing three of theſe, each of which was as big as a ſmall hen's egg, into two parts, he boiled them in a pint of milk, which, when a little cooled, he drank off about eight o'clock in the morning faſting. Preſently afterwards, he became vertiginous or giddy; and therefore roſe from his chair to take the air, with an intention to pluck more fruit. In walking two or three hundred yards from his houſe, he ſtaggered as if

* May 1. 1755.

if drunk, feared he ſhould fall on his head, and that he was about to loſe his ſenſes; but had no ſickneſs nor the leaſt inclination to vomit. As ſoon as he got home he went to bed, and complaining of an exceſſive dryneſs of his tongue and throat, a little water mixed with wine was given him; he alſo felt an odd ſenſation of dryneſs in and violent girding a-croſs the *thorax*. In leſs than half an hour he began to faulter in his ſpeech, became inſenſible, reſtleſs, and muttered frequently; in which condition I found him. His extremities, and alſo the trunk of his body, were cold. His pulſe ſmall and quick. He often raiſed himſelf on his knees, continually ſtretched out his arms, and employed his hands as if ſearching for ſomething he wanted; his eyes were dull and heavy; after ſome time, he became dumb and more quiet, had almoſt no pulſe; and, upon his being taken out of bed that it might be put into better order, his limbs were viſibly paralytic. Altho' he changed poſtures a little, yet he remained ſtupid for ſix or ſeven hours; then he raged furiouſly, requiring two perſons to hold him in bed, notwithſtanding which, he raiſed himſelf up, toſſed greatly,

and

and ſeemed to catch at the byſtanders with his hands, uttering incoherent ſentences. At laſt he became ſenſible and more quiet, reſtleſs and delirious by turns; and about ten o'clock in the evening of the ſame day perfectly recovered. After taking a purgative, he ſlept well all night, and had ſeveral ſtools in the morning. For the ſpace of fourteen hours he neither ſlept, vomited, nor diſcharged any thing by ſtool or urine, tho' he frequently paſſes urine at other times, being grievouſly afflicted with the gravel.

Art. XII.

The Effect of Musk in curing the Gout in the Stomach; by James Pringle *Esquire, late Surgeon to the third Regiment of Foot-Guards**.

A Gentlewoman, aged 43 years, naturally of a delicate conſtitution, who has been for ſeveral years ſubject to hyſteric fits, attended with a dry aſthma, which her ſhape much contributed to; was frequently attack'd to a violent degree with the gout in her head and ſtomach, as well as in all her extremities; and with which ſhe was lame the moſt part of Summer 1745. On the 3d of *November* following, ſhe was violently ſeized with it in her ſtomach, which occaſioned violent hiccups and convulſions of the part. The deſcription ſhe gave of it was, that as ſoon as theſe fits ſeized her, there came on a violent working of her ſtomach, and ſo great an agitation of her back, that her Maid was not able to keep her hand on it. By degrees

* Aprile 3. 1746.

degrees it roſe to her throat, when ſhe was almoſt ſtrangled. She could by no means lie down, but was forced to ſit night and day in an eaſy chair; and even then if ſhe lean'd her head to the one ſide or other, it gave her great pain; ſo that ſhe was obliged to ſit in an erect poſture. Her legs were very much ſwelled, which ſubſided a little on laying them on a chair; but as ſoon as that happened, the aſthma returned. She did nothing all this while but keep herſelf warm, now and then drinking a little of ſome generous wine (as ſhe ſaid, to keep it out of her ſtomach), and once or twice took a little of the *tinct. ſacra.* On the 21ſt of *November* about 9 o'clock at night, a Lady of her acquaintance, who had ſeen her in this condition, deſired me to viſit her, tho' ſhe doubted if I ſhould find her alive. Accordingly I went, and as I had ſeen ſuch extraordinary effects of the *tonquin* medicine in the *ſingultus*, and had heard from Mr. *Read* of its efficacy in other nervous caſes, I imagined it might be of ſome ſervice here: and therefore I ſent her the following bolus.

℞ *Cinnab.*

℞ *Cinnab. nativ.*
——*Antimon. aua gr.* xxv.
Moſch. opt. gr. xvi.
Syr. balſ. q. ſ. F. bolus.

But altho' this is Mr. *Read*'s common doſe, yet, as ſhe was very weak, I ordered her to take only the one half of it immediately, drinking after it a cup of brandy, and the other half in ſix hours after. Next morning I found her much better, having from the firſt doſe no more convulſions in her ſtomach. I then ventured to give her a whole bolus at 9 o'clock in the morning, to be repeated every four hours until ſuch time as ſhe ſhould ſleep or ſweat: and notwithſtanding the coldneſs of the weather, and her being obliged to ſit in a chair, yet, by the time ſhe had taken four boluſes, a plentiful ſweat and ſleep enſued, and then ſhe was able to lie in a horizontal poſture on her couch without the return of her former ſymptoms. This ſweat continued from the afternoon of the 22d till the 24th at night, with very little intermiſſion. I gave over the boluſes and ordered her a julep, to eight ounces of which I put twelve grains of muſk,

to

to be taken *ad libitum.* In this method ſhe continued till the 27th, quite eaſy and free from all her former ſymptoms, and even the ſwellings of her legs almoſt gone: But as on this day ſhe fancied the gout in her ſtomach was returning, I gave her another bolus. She complained this time of the intolerable heat of the brandy, which was the firſt thing ſhe had found warm in her ſtomach during this illneſs. On the 29th, ſhe was apprehenſive of another attack, and took another bolus; after which ſhe found herſelf very well, and walked about the room, the ſwellings of her feet being quite gone. And on the 4th of *December* went out in a chair to thank the Lady who ſent me to her, and continues to be well to this day.

ART.

ART. XIII.

An Account of an uncommon Effect of antimonial wine; by Dr. JAMES WALKER *Surgeon and Agent for the Navy at* Edinburgh *.

THE *vinum benedictum* is universally known to be a strong emetic, when given in a large dose; and it has also been often prescribed in smaller quantities as an attenuant, sudorific, and diuretic: but an accident lately discovered to me a very different effect of it, from any of the above mentioned.

BEING one evening, in *December* 1755, a little hot and feverish, with a quick and full pulse, I went early to bed, and drank a full *English* pint of sack-whey which I had ordered one to make for me. Very soon after this, I fell asleep, and continued all night oppressed with an unusual drowsiness: at ten next morning, I with great difficulty got so far the better of this lethargic disposition, as to

* February 5. 1756.

to get out of bed; when I found myself ſtill heavy and inclined to ſleep, with a laſſitude and numbneſs in my limbs, ſo that I could ſcarce ſtand. Being thus incapable to go about buſineſs, I reſolved to ride out on horſeback; and having mounted with ſome difficulty, found my head very giddy, and had much ado to keep myſelf from falling aſleep. After having rode three hours, I returned home much in the ſame ſituation, and was ſurprized to find two apprentices with the ſame complaints I had myſelf: they could aſſign no cauſe of their being ſo affected, nor had they eat any thing but their uſual food, except the curd of which I had got the whey. Being led by this to ſuſpect ſomething uncommon in the wine with which the poſſet was made; I called for it, and had a bottle brought me containing *vinum benedictum*, which had been made about a month before; and the bottle having been put in an improper place, was the reaſon of its having been miſtaken for *Liſbon*. I found I had drank in my whey about a gill and a half of this emetic wine, and was ſurprized it had not vomited me ſtrongly. Thinking, however, that the finely attenuated particles of the antimony might

might be moſtly ſuſpended in the curd, I inquired particularly at the two young Gentlemen who had eat it, whether they were affected with any *nauſea* or vomiting; but they told me they found no other effect but a heavineſs and great inclination to ſleep. This drowſineſs continued with them two days; but I did not find myſelf quite free of it at the end of four days. I do not know if it be worth while to obſerve, that the day after I had drank the above whey, my pulſe was ten in the minute ſlower than it had been the night before. It may be proper however to take notice, that the *vinum benedictum* which was uſed, by miſtake, inſtead of *Liſbon*, was made exactly as is directed in the *Edinburgh Diſpenſatory*; and I have ſince found half an ounce of it vomit a patient very well.

ART. XIV.

An obſtinate Dyſentery cured by Lime-Water; *by* JAMES GRAINGER *M. D. Phyſician at* London *.

——— *Arſdale*, a ſtout, middle aged, but intemperate ſoldier, was ſeized *November* 1751 with ſhivering and other ſymptoms that precede an accute diſorder. The day following, he went frequently to ſtool, and evacuated blood to the quantity of a gill every quarter of an hour. Tho' the fever was inconſiderable, yet, as he was of a ſanguine habit, the lancet was not ſpared. He ſwallowed ſome ipecacuan vomits; was gently purged every third day with rhubarb; and had ſmall quantities of *opium* at bed-time to hinder his riſing in the night. The third week his fundament came down, attended with but little pain, unleſs when he went to ſtool. This ſymptom, however, was timeouſly removed by fomentations of a decoction of oak-bark. *December*; his ſtools, tho' leſs frequent, were mixed with blood and *mucus*. Then

* May 3. 1753.

the gripes ſeized him in good earneſt, ſometimes fixed and torturing like a ſtitch in the baſtard ribs, at other times wandering with *borborygmi*, now as it were twiſting his guts, then cutting him in two, as he expreſſed it, and bending him forward. They were always moſt ſevere before ſtool, eaſier in the time of evacuation, but eaſieſt after. With theſe ſymptoms were complicated a dyſury and piles, greatly inflamed, but bleeding none. Theſe diſmal complaints continued almoſt equally violent for three months, altho' *V. S.* emetics, *vitr. antimon. cerat.* mild antiphlogiſtic purgatives, opiates, lubricating and aſtringent remedies were uſed, as the ſymptoms indicated. Opiates, tho' they eaſed him, ſeldom procured undiſturbed repoſe, and always affected his head; this conſequence of *laud.* was more effectually prevented by *balſ. locatel.* than any thing elſe. Vomits always relieved him, but increaſed the pain of the hæmorroids. Aſtringents, tho' they bound him up for a little time, moſt generally brought on afterwards a more frequent inclination to ſtool, with increaſed *tormina.* Clyſters were impracticable on account of the piles, but he found conſiderable ſervice from the

the milder purgatives, with calomel.; and when the ſwelling of his fundament was removed by *ſulphur* and emollient, diſcutient applications, they became highly uſeful. In the 4th month, the region of the ſtomach ſwelled, with redneſs of his face, eſpecially after food, and continued for ſix months, altho' its removal was attempted by bitters; and the bark (cautiouſly combined with purgatives), mulled claret, and camomile tea, while proper external applications were not forgotten. *April* and *May*, the hæmorroids and dyſury left him; but his ſtools became thinner, more acrid, and intollerably fetid. Then he was ſeized with a hiccup, his face looked ghaſtly, his extremities turned cold, his pulſe became quick, ſmall, irregular, and his gripes were rather abated. Evacuations in theſe deplorable circumſtances were improper, but ſinapiſms were applied to the ſoles of his feet; and epiſpaſtics to the region of the ſtomach with ſome ſucceſs. A bolus of bark, caſtor, and camphire was given every third hour, and waſhed down with a glaſs of mulled claret; his guts too were fomented with anodyne, emollient, and antiſeptic clyſters. By theſe his deadly ſymptoms left

him,

him, he could ſit up at the fire, and had not above 12 ſtools in the 24 hours, which were however ſtill ichorous. I could not find from the nurſe that he ever paſſed any of the villous coat of the guts, tho' *ſcybala* were frequently evacuated. About the end of *May*, he was able to walk round the ramparts of *Fort-William*, and he told he thought he would recover, were he ſent from *Lochaber*. On this, he was carried, by water, to the Iſle of *Mull*, being provided with proper medicines to forward his recovery. Here, tho' the hiccup and *facies hippocratica* did not recur, yet his gripes did; and he purged blood and worms almoſt inceſſantly. The latter end of *July*, he was ſent back to the Fort a perfect ſkeleton; where, tho' I am convinced the air is inferior to that of *Caſtle-Dowart* in *Mull*, yet, as great care was taken both of his diet and medicines, he paſſed no more worms, his gripes only ſeized him at ſtool, whither he went much more ſeldom than formerly; what he paſſed now appeared to be *mucus* mixed with *pus* and ſtreaks of blood. Very ſmall doſes of ipecacuan, *viz.* eight grains three times a day were then adminiſtred to him every third day;

but

but, tho' they relieved by operating both up and down, yet they ſickened him ſo much, that I was obliged to ſubſtitute a bolus of calomel. over night, and a purging ptiſan next morning, in their place. The diſeaſe, however, did not yield; and when the regiment was ordered to march for *Berwick upon Tweed*, he was put aboard the veſſel that carried our baggage. This was a hardy ſtep, conſidering his weakneſs, and the length and danger of the voyage; but he could not be carried with his comrades over the *Black* mountain, and he deſired to die any where, rather than remain in *Fort-William*. Five or ſix weeks after he landed at *Berwick*, his ſtools were more numerous, and ſtill very painful. Sometimes blood, ſometimes ſlime, ſometimes ichor, and once he paſſed a great quantity of hardened excrements, which relieved him of a dull pain of his left hypochondre; his legs too, at night, ſwelled and pitted to the touch, and his ſtomach was often inflated. Bitters, with ſteel, were preſcribed, and camomile tea drank for breakfaſt, while the utmoſt regard was had to diet; the *vitr. antimon.* was again tried, and alum poſſet recommended. The

dyſentery

dyſentery baffled all our attempts; and now, deſpairing of his recovery, I ordered him to drink lime-water, with a third part milk, to the quantity of an *Engliſh* pint and a half every day: It was at the latter end of *November* 1752. In three days time, there was a ſenſible change to the better, his ſtools were leſs frequent, and his pains abated. Encouraged by this happy beginning, he was ordered to drink *lib.* iii. of lime-water a-day. This, in three weeks, made him ſo coſtive, that I was obliged to clyſter him, and diminiſh the *aq. calc.* to the quantity firſt ordered. In ſix weeks from his taking this medicine, he was ſo thoroughly recovered, that he was diſmiſſed the hoſpital, and ſoon after marched to his company at *Carliſle*, where he ſtill enjoys perfect health. The diſorder was on him full 14 months; and I have reaſon to think his recovery was chiefly owing to the lime-water, after the moſt celebrated antidyſenterics had been uſed in vain.

The dyſentery is endemic at *Maryburgh*, near *Fort-William*, and commonly attended with *procedentia ani*, piles, dyſury, abdominal inflations, *oidema's*, and hiccup. Many of the ſoldiers died, eſpecially the more intemperate;

temperate; between the 20th and 35th day, is the fatal period. Thofe who died had mortifications of the great guts: it began with us about the latter end of *October*; autumn however is its ufual feafon; it rages commonly two months, tho' many have it all the winter, and I always obferved them worft in rainy weather. It may be worth while to obferve, that of late lime-water principally conduced to cure an Officer of a dyfentery, while another was effectually cured of a weaknefs in the bladder, by the fame remedy.

ART.

ART. XV.

The anthelmintic Virtue of the Bark of the wild Cabbage or Bulge-water Tree; by the late Mr. PETER DUGUID *late Surgeon in* Jamaica, *in a Letter to* ALEXANDER MONRO *senior,* M. D. & P. A.

THE Writers on the diseases of the *West-Indies* generally take little notice of the Inhabitants of *Jamaica*, young and old, white and black, being much troubled with worms, especially the long round sort. They are however so frequent, that every practiser ought to have regard to them in treating most of his patients. I imagine these worms infest the inhabitants here on account of their sweet viscid bread-kind, to wit, plantains, yams, bananos, sweetish potatoes, &c. which are fit nourishment for these vermin. I was lately allowed to open a Gentleman's child, that, at seven months of age, died of vomiting and convulsions. In its intestines there were twelve large worms: one of them

* May 1. 1755.

them filled the *appendix vermiformis*, and three of them were intertwiſted in ſuch a manner as to block up the opening at the *valvula Tulpii*, ſo that nothing could paſs from the ſmall to the great guts.

NATURE has bountifully provided the people here with a powerful remedy againſt ſo great an evil. This is the bark of a tree growing plentifully in this Iſland. The inhabitants call it *wild cabbage* or *bulge-water*: and from what I have ſeen, it appears to be the moſt powerful vermifuge yet known; for it frequently brings away as many worms by ſtool as would fill a large hat. It is commonly given in decoction, but not in any regular quantity; the negroes being generally the preparers of this medicine, and therefore no wonder that it ſometimes has very violent effects. I am now making experiments for aſcertaining the doſe to patients of different ages, and ſhall ſoon ſend you the reſult of my trials, together with ſome of the bark itſelf.

ART. XVI.

The description of a monstrous Fœtus; by Mr. JOHN MOWAT *Surgeon at* Langholm, *in a Letter to* ALEXANDER MONRO senior, *M. D. & P. A* *.

SEVERAL learned men having of late years disputed about the formation of monsters, it is probable the histories of them may be of use in accounting for some *phænomena* in nature; on this account, I send you the following description of one, which will be better understood by the figures which young Professor *Monro* caused to be drawn of it, of half the natural dimensions. See *plate* VI. *fig.* 1.

—————when six months with child, had this abortion, which lived half an hour.

TAB. VI. *fig.* 1. A A The lower parts of the bodies of two female *fætuses*, with their lower extremities in a natural state.

B The navel-string common to both *fætuses.*

C The bodies joined immediately above the navel.

D D The

* May 1. 1755.

D D The ſuperior extremities of the more compleat *fœtus*, which is here repreſented moſt in view.

d d The ſuperior extremities of the other *fœtus*. All the four being of a natural form.

E The *ſternum* of the more compleat *fœtus* which had clavicles joined to it in the common way, as the other alſo had.

F One neck common in appearance to both *fœtuſes*.

The face and ears, being natural, need no letters of reference to their parts.

G The forehead had hair farther down than ordinary.

H The top of the head of an extraordinary breadth.

I The hairy ſcalp covering the parietal bones.

K The right temple of the *fœtus* D D.

L The teguments covering the occipital bone.

M The *occiput* of the other *fœtus*.

Fig. 2. The back view of theſe *fœtuſes* where the head, neck, and fore parts of the *thorax* only are repreſented.

N N The ſcalp over the four parietal bones.

O O ———The two occipital bones.

P P Two ears joined together.

Q Q The two *meatus auditorii*.

R A paſſage by which a probe could be put into the *œſophagus*.

S The neck of the *fœtus* D D.

T The *ſternum*.

U U The forepart of the arms.

The back parts of neither *fœtus* are drawn, having nothing preternatural. Where the *abdomen* of each *fœtus* was diſtinct at A A, the *viſcera* were in a natural ſtate, and in the undivided cavity of the belly above C, there were two ſtomachs, livers, ſpleens, *inteſtina duodena*, *jejuna* and *ilium*; but I ſaw only one *omentum* and one *pancreas*. One diaphragm divided the *abdomen* from the *thorax*, but it was pierced by two *venæ cavæ* and two *œſophagi*, and two *aortæ deſcendentes* paſſed between its *appendices* on each ſide where the *vertebræ* were.

There was a *ſternum* to the *thorax* of both ſides E *fig.* 1. and T *fig.* 2. From each *ſternum* a *mediaſtinum* was extended to a ligamentous fleſhy ſubſtance, which was continued tranſverſely from one ſpine to the other. In each

each *mediaſtinum* was a *pericardium* with its heart and veſſels as in natural *fœtuſes*, and on each ſide of each *mediaſtinum* was a *thorax* with lungs. Thoſe in the *thorax* of the more compleat child, the one, to wit, with the face, being of a pale colour and ſwimming when put into water, while the other lungs were of a redder colour and ſank in water.

THE ſpines gradually approached each other as they extended towards the neck, at the lower part of which the ſides of the bodies of the *vertebræ* ſeemed to be contiguous; but I puſhed a probe up between them as far as the ſecond *vertebra*.

WITHIN the head I ſaw a *cerebellum* on each ſide divided from a common *cerebrum* by a membranous *plexus* produced from the *dura mater*. The parts of the *encephălon* were too ſoft and tender for my diſſection, and I examined the anatomy no further, but have the monſter in ſpirits with the three great cavities filled with lint and bran where the bowels were taken out.

ART.

ART. XVII.

The Diſſection of the ſame Monſter continued; by ALEXANDER MONRO junior, *M. D. and Profeſſor of Anatomy in the Univerſity of* Edinburgh *.

MY father having ſhewed me Mr. *Mowat*'s deſcription of the monſtrous *fœtus*, and a model of its form in wax, I was curious to know how the parts of the head and neck were formed. The preſerved *fœtus* was obtained with difficulty, and under a promiſe that no diſſection ſhould be made which might ſpoil its form. The ſubject was unfavourable, and the promiſe rendered the diſſection difficult, and impoſſible to be performed ſo accurately as I wiſhed.

AFTER drawing aſunder the two parietal bones of each ſide I N N (ſee Mr. *Mowat's fig.*) to take out the lint, *&c.* with which the ſkull was filled, and having cleaned away the matter, membranes, *&c.* adhering to the bones, I ſaw the *cranium* of a natural

* May 1. 1755.

ral form on that ſide where the face is, ſo far back as the *ſella turcica*, behind which the ends of the cuneiform proceſſes of the two occipital bones united together. From each of theſe cuneiform proceſſes the occipital bones extended of a natural enough form to each ſide; their ſituation may be judged by conſidering the figures at L, M, or O, O, having each a *foramen magnum* for the ſpinal marrow. At the ſide of each of the occipital bones, neareſt to the conjoined preternatural ears, an *os petroſum* was placed, but without having any ſquammous part of the temporal bones, ſuch as were on the other ſide of each occipital. Between theſe *oſſa petroſa* there was a triangular little bone which ſuſtained theſe preternatural ears, and was inſtead of *os ethmoides*, *ſphenoides*, and two ſquammous bones. The *fœtus* with the face had therefore all the common nerves, but the *fœtus* with only the conjoined ears wanted the 1, 2, 3, 4, 5, and 6 pairs.

The *meatus* Q, Q *fig*. 2. led into the organ of hearing. The orifice R was the entry to a paſſage which opened into the *œſophagus* of the *fœtus* D D.

The

The mouth of the more compleat *fœtus* had all the ordinary parts, with the *uvula*, *nares*, *larynx* and *pharynx*. Behind this *pharynx* and the *œsophagus* descending from it, there was another *larynx* and *trachea*. From the back part of the *glottis* of this last mentioned *larynx*, a little excrescence resembling a tongue stood out, and behind it a canal descended of the form of one of the *nares* which joined with the one continued from R to form another *œsophagus*.

I could not prosecute the vessels and nerves placed on the neck and head without breach of promise, and therefore can give no account of them.

ART.

ART. XVIII.

Bones found in the ovarium *of a Woman; by Dr.* GEORGE YOUNG, *and communicated to the Society by Dr.* JOHN BOSWELL, *Fellow of the Royal College of Physicians in* Edinburgh *.

A Woman near fifty years of age, who had never had a child, being four months obſtructed, thought ſhe had conceived; but the *menſes* then returning, ſhe had exceſſive flooding, which was ſometimes in great quantity, at other times was leſs, but ſcarce ever intermitted for a year and a half. It was then ſtopped by ſome medicine; after which, her belly ſwelled to ſuch a degree in ſix weeks, that her urine was almoſt totally ſuppreſſed; ſhe was very coſtive unleſs when clyſters were given, and ſhe died in a few days more.

ON cutting the teguments of the *abdomen*, a large quantity of bloody water ruſhed out; and when the containing parts were fully opened,

* November 1. 1737.

opened, all the cavity was bloody, and the veſſels were large and turgid with dark-coloured blood. No bowel was now ſeen; all that appeared in view being a great number of irregular fleſhy lumps, which were bladders full of a red watery liquor. Some of them were of the ſize of the largeſt apples I have ſeen, others were as ſmall as pigeon-eggs; and there were of all the intermediate ſizes between theſe. Upon a ſtricter examination, theſe veſicles were found to be all contained in one common cyſt, of which I had cut the fore-part with the teguments of the *abdomen*. The large cyſt filled all the belly: when it was raiſed, the bowels appeared in a natural ſtate; except that, 1. The left Fallopian tube was very large. 2. No *ovarium* of that ſide could be ſeen unleſs the great cyſt was that *ovarium* immenſely diſtended. 3. The right *ovarium* was as big as the head of a new-born child. It contained a viſcid white-coloured ſubſtance reſembling maſhed brains, which run together like ſuet when put into water. In this ſtuff I found the bones herewith ſent.

THE

The bone repreſented in *plate* VI. *fig.* 3. is a piece of a jaw with three firm *dentes molares*, A, in it. *Fig.* 4. and 5. are two views of a part of a jaw, in which are three *dentes molares*, B, irregularly ſet, and an *inciſor* C.

ART.

ART. XIX.

Proofs of the Contiguity of the Lungs and Pleura; by ALEXANDER MONRO *ſenior, M.D. and P.A.**

THE experiment of opening the *thorax* without hurting the lungs of living animals, while the trunks of their bodies are immerſed in water, thro' which no bubbles of air riſe after the perforation, as propoſed by the ingenious *Lieberkuhn* and executed by the illuſtrious *Haller* †, is a deciſive one, if rightly performed, for proving no air between the *pleura* and lungs. But, as an unwary operator may wound the lungs in perforating the *thorax*, when air would certainly riſe in the water, from which the exiſtence of air in the *thorax* might be concluded, and ſeveral other circumſtances may, and have cauſed the concluſion from this experiment to be diſputed; it may not be amiſs to mention ſome eaſier ways of proving

* February 7. 1754.

† Opuſc. de reſpirat.

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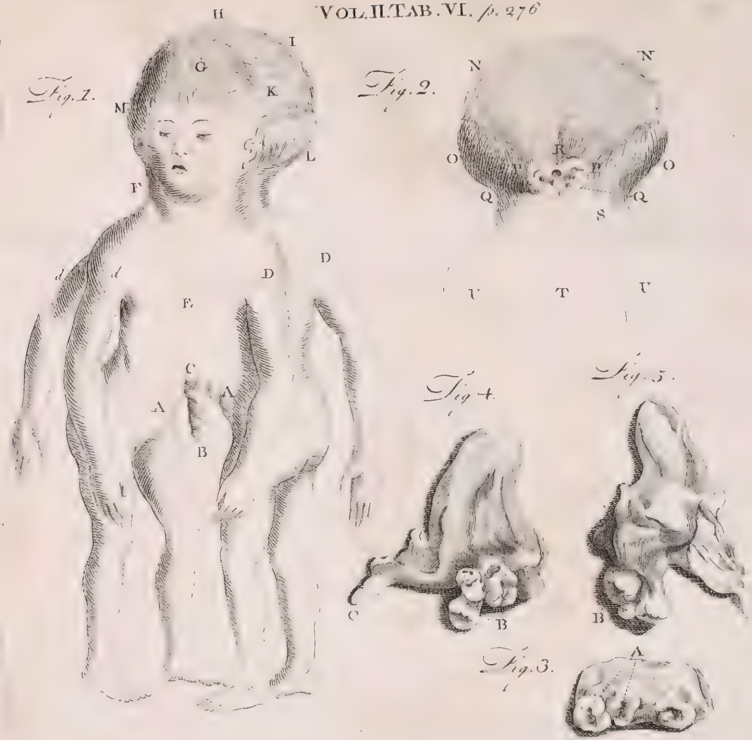
Fig. 1.
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Fig. 4.
Fig. 5.
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Fig. 3.
A

proving the non-exiſtence of air in this place. Such are the following:

1. DISSECT the teguments and intercoſtal muſcles from the *pleura* of either a dead man or quadruped without wounding this membrane, in which there is no difficulty; then pull up and depreſs alternately the *ſternum* and ribs as often as you will, the lungs are ſeen contiguous all the while to the *pleura*; but, on making a ſmall puncture thro' this membrane, the lungs, if they are not grown to the *pleura*, which is often the caſe in the human ſubject, fly from the *pleura*, and are no more ſeen.

2. THIS connection of the lungs and *pleura*, more or leſs of which is ſeen in moſt human bodies, implies ſtrongly a natural contiguity of theſe two parts.

3. LAY bare the *pleura*, without wounding it, between two ribs of any living quadruped, which requires no great dexterity; and then the contiguity of the lungs and *pleura* may be ſeen, tho' the lungs are conſtantly ſliding and changing place along the *pleura*, and tho' this membrane is in different ſtates: while the creature inſpires, it is concave;

during

during exſpiration it is convex and prominent outwards, for this plain reaſon, that while inſpiration is performing, the air does not paſs ſo quickly at the narrow *glottis* as to fill the lungs at once with air of denſity and weight equal to the atmoſphere; and during exſpiration, the air cannot eſcape ſo faſt at the *glottis* as to prevent its more than ordinary condenſation and expanſility in the lungs than the external air has.

If we were to find accurately what weight the *pleura* could raiſe when it is made convex during exſpiration in the preceeding experiment; would not this determine how much more preſſure, than that of the atmoſphere, the part of the lungs within this elevated *pleura*, on which this weight is ſuſtained, is expoſed to?

Would not the preſſure on the ſame part of the lungs during inſpiration be nearly as much leſs than the weight of the atmoſphere, as is the weight raiſed in exſpiration; ſince it is the ſame *glottis* which allows the air to paſs in both caſes?

Is not the force, by which the inſpiratory organs, acting with the greateſt energy of the mind, exceed the power of the exſpiratory

tory organs uninfluenced by the mind, confiderably lefs than the weight of the atmofphere; fince, if, after exfpiration, we prevent the entry of the air by the *glottis*, we cannot dilate the *thorax* as in infpiration, tho' there is ftill a good deal of rarefying air in the lungs?

Do not the infpiratory organs, during infpiration, overcome the refiftance of the expiratory organs, and likewife that fhare of the preffure of the atmofphere, which the air, rufhing into the lungs, does not balance?

Are not the ribs arched, and the fpaces between them narrow, to prevent ill effects from that unbalanced part of the atmofphere during infpiration?

Art.

ART. XX.

An Account of ſome Experiments made with Opium *on Living and Dying Animals; 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 ancient phyſicians imagined that *opium* extinguiſhed the flame of life in animals by its exceſſive cold; and in later times, there have not been wanting thoſe who deduced its effects from a quite oppoſite quality, whereby it was thought to rarefy the blood and to compreſs the brain or origin of the nerves. Theſe falſe notions, however, of the nature and action of *opium*, have been refuted by ſeveral of the moderns, whoſe writings have thrown conſiderable light upon this ſubject.

THE following experiments were made with a view ſtill further to illuſtrate the manner in which this wonderful drug produces its

* Auguſt 7. 1755.

its effects, and particularly to ſhew its influence upon the motion of the heart.

1. Having injected a ſolution of *opium* in water into the ſtomach and guts of a frog, I obſerved, that in little more than half an hour it ſeemed to have loſt all power of motion, as well as feeling; for there was no contraction produced in the muſcles of its limbs and trunk by irritating them. I opened the *thorax* an hour after the injection, and found the heart, inſtead of between 60 and 70, making only 17 pulſations in a minute. The auricle, which was much diſtended with blood, always contracted firſt, and after it the ventricle.

2. A frog continued to move its limbs, and leap about for above an hour after I had cut out its heart, and was not quite dead after two hours and a half.

Five minutes after taking out the heart of another frog, I injected a ſolution of *opium* into its ſtomach and guts. In leſs than half an hour, it ſeemed to be quite dead; for neither pricking nor tearing its muſcles produced any contraction in them, or any motion in the members to which they be-

 longed

longed. After cutting off its head, a probe puſhed into the ſpinal marrow, made its fore-legs contract feebly.

3. EIGHTEEN minutes paſt four in the afternoon, I injected a ſtronger turbid ſolution of *opium* in water than that uſed in the preceeding experiments *, into the ſtomach and guts of a frog; and as it ſquirted out moſt of the ſolution injected by the *anus*, I threw in ſome more in its place. At twenty four minutes paſt five, I opened this frog, and obſerved the heart with its auricle greatly diſtended with blood and beating very ſlowly, not above ſeven times in a minute. When the heart was touched with the point of a pair of ſciſſars, its motion was rendered quicker for two or three pulſations: after which it became as ſlow as before.

4. IMMEDIATELY after decollating a frog, I deſtroyed its ſpinal marrow, by puſhing a ſmall probe down thro' its ſpine, which occaſioned ſtrong convulſions of all

the

* *Viz.* half an ounce of *opium* diſſolved in eight ounces of water; which was alſo made uſe of in all the following experiments. The heat of the ſolution was nearly the ſame in all the experiments; *viz.* about 60 degrees of *Farenheit*'s thermometer.

the muſcles, eſpecially thoſe of the inferior extremities. Ten minutes after this, I opened the *thorax*, and found the heart beating at the rate of 45 times in a minute. Sixteen minutes after decollation, it moved 40 times in a minute. After half an hour it made 36, and after fifty minutes only 30 pulſations in the minute, which were now alſo become very ſmall and feeble.

N. B. WHEN I opened the *thorax* of another frog immediately after decollation, and deſtroying its ſpinal marrow, I obſerved its heart beating at the rate of 60 in a minute, which is four or five pulſations leſs than I have generally ſeen the hearts of frogs make in that time, when their *thorax* was opened without decollation.

5. AT nine minutes paſt eleven in the forenoon, immediately after decollating another frog, I deſtroyed its ſpinal marrow with a red hot wire, which produced terrible convulſions in all the muſcles, as in the laſt experiment. I opened the *thorax* of this frog *thirty five minutes* after decollation, and obſerved its heart beating 30 times in a minute. The contraction of the auricle regularly preceeded that of the heart: the

auricle

auricle was not near ſo much diſtended with blood, nor the heart ſo much ſwelled as in thoſe frogs which had a ſolution of *opium* injected into their ſtomach and guts *. At one o'clock (*viz.* an hour and fifty one minutes after decollation), the heart of this frog made 20 pulſations in a minute. At half an hour paſt two, when the room was become warmer by the ſhining of the ſun, it beat 25 times in a minute; and when placed in the ſun-beams, it performed 31 contractions in that time. After this, I removed the frog to an eaſt window, where it was expoſed to a cool breeze; upon which the motion of its heart became ſlower, ſo that in a ſhort time it only made 25 pulſes in a minute. I then expoſed it anew to the ſun-beams, by which its motion was ſoon quickened, ſo that it beat 30 times in a minute.

At twenty five minutes paſt five in the evening, (*viz. ſix hours and ſixteen minutes* after decollation and the deſtruction of its ſpinal marrow) the auricle of this frog's heart, which was ſtill filled with blood, contract-

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* See No. 3. above, and Eſſay on the Vital and other Involuntary Motions of Animals, p. 371 & 372.

ed twelve times in a minute; but the heart itſelf lay without motion, was ſwelled and very red: however, when pricked with a pin, it performed two or three pulſations, and then remained at reſt, till rouſed by a new *ſtimulus*. At thirty five minutes paſt five, the heart ſeemed to be quite dead, but the auricle continued its motion; nay, at half an hour paſt eight, near three hours after the heart had been without motion, the auricle, which was very near as much filled with blood as when I firſt opened this frog, beat 11 or 12 times in the minute; its pulſations, however, were not now ſo regular as to time, as they had been before.

Is it not probable, that the auricle of this frog's heart beat longer than uſual, becauſe it continued, to the laſt, to be filled with blood; whereas, generally, the auricles of frogs hearts, which are opened after decollation and the deſtruction of their ſpinal marrow, expell, after ſome time, the blood which they contain, and acquire the appearance of a ſmall pellucid bladder filled with air?

6. I laid bare the abdominal muſcles and *thorax* of a frog, by diſſecting off the ſkin,

and at twenty minutes before nine in the morning, I immerſed the whole body of the frog in a turbid ſolution of *opium* in water, in a ſmall baſon, which I covered, to prevent the frog from leaping out of it. *Thirty five minutes* after immerſion, I took it out of the ſolution and opened the *thorax* and *pericardium*. The heart's auricle, which was much diſtended with blood, beat 15 times in a minute, but the heart itſelf, only 6 times. Forty minutes paſt nine (*viz.* twenty five minutes after the frog was taken out of the ſolution of *opium*) the heart ſeemed to have recovered more life; for it performed eight pulſations in a minute: the contractions of the auricle now became feebler, and were ſcarce more numerous than thoſe of the heart, but always preceeded them ſome little time. Six minutes before ten this heart moved only ſix times in the minute. Twenty four minutes paſt ten it made only five pulſations in ſixty five ſeconds, the firſt, third, and fifth of which pulſations were after an interval of fifteen ſeconds, and the ſecond and fourth after a pauſe of ten ſeconds. Seventeen minutes before twelve, and *two hours and twenty eight minutes* after the frog

frog was taken out of the ſolution of *opium*, its heart moved only thrice in ſeventy five ſeconds, and performed its *ſyſtole* very ſlowly. Before two o'clock afternoon the heart was quite dead; but how long, I cannot ſay, not having had leiſure to obſerve it from a quarter before twelve to near two.

7. AFTER cutting off a frog's head and deſtroying its ſpinal marrow with a red hot wire, I laid bare the abdominal muſcles and *thorax*, as in the laſt experiment, and immerſed the whole body of the frog in a turbid ſolution of *opium*, at half an hour paſt nine in the morning. *Thirty ſix minutes* after immerſion I took it out of the ſolution, and opened its *thorax* and *pericardium*. The heart and its auricle beat, each, twenty ſix times in a minute, and the pulſations of the auricle preceeded thoſe of the heart regularly. The heart did not appear to be more ſwelled or redder than in a natural ſtate, and the auricle was not near ſo full of blood as in *Exp.* 6. Twelve minutes paſt ten, *viz.* ſix minutes after this frog was taken out of the ſolution of *opium*, its heart beat 27 times in a minute. At eleven o'clock it performed 18 vibrations in that time; and 16 at a

quarter

quater before twelve. At two o'clock afternoon, the auricle, which, having expelled all its blood, was now only filled with air, continued its motions; but the heart lay at reſt. Ten minutes paſt four, *i. e. five hours and forty four minutes* after the frog was taken out of the ſolution, the auricle of its heart beat 9 times in ſixty four ſeconds.

8. I laid bare the abdominal muſcles and *thorax* of another frog, and at fourteen minutes paſt eight in the morning, immerſed it as above in a turbid ſolution of *opium*. Fourteen minutes paſt nine, I took it out of the ſolution, and laid open its *thorax* and *pericardium*; after which the heart began to beat at the rate of nine times in a minute: but the auricle, which was greatly diſtended with blood, made no motion, except in ſo far as it was agitated a little by the pulſation of the heart: nor were the muſcles of the legs or thighs brought into contraction by cutting or tearing their fibres. At half an hour paſt nine the heart beat only 7 times in a minute; and the auricle, which was now pretty empty of blood, and, in place of it, filled with air, had a pulſation as well

as

as the heart. Thirteen minutes before ten, *i. e. thirty three minutes* after the frog was taken out of the ſolution, the auricle ſhewed, at conſiderable intervals, a very faint pulſation, but the heart lay without any motion.

9. THE ſame day, after cutting off the head and deſtroying the ſpinal marrow of another frog, I laid bare its abdominal muſcles and *thorax*; and, at eighteen minutes paſt ten, immerſed it in a ſolution of *opium*, as above. Eighteen minutes paſt eleven, I took it out of the ſolution and opened its *thorax* and *pericardium*, after which the heart began to move at the rate of 8 times in a minute. Twenty five minutes paſt eleven, the heart beat 15 times in a minute; and at twelve o'clock it performed between 13 and 14 vibrations in the ſame time. At two o'clock, (*viz. two hours and forty two minutes* after the frog was taken out of the ſolution) the auricle, which was now filled with air, continued to vibrate weakly, about 11 times in the minute; but the heart itſelf was without motion. At ten minutes before four in the afternoon, the auricle ſtill continued to

 move,

move, but more feebly than the auricle of N° 5.

10. I laid open the whole *abdomen* of a larger frog than any of the former; and, at twenty two minutes paſt ten in the morning, immerſed it in a ſolution of *opium*, as above. *Thirty five minutes* after immerſion, I took it out of the ſolution, and opened its *thorax* and *pericardium*. The heart was vaſtly red and much ſwelled, and its auricle greatly diſtended with blood; but both were without any motion: after two minutes, however, the heart began to vibrate at great leiſure, ſcarcely performing nine pulſations in a minute; but the overſtretched auricle made not the ſmalleſt motion. During every *ſyſtole*, the heart was remarkably paler, and in the time of its relaxation became much redder; which *phænomenon* I obſerved likeways in all the frogs hearts in the above experiments, but more remarkably in thoſe frogs who had been expoſed to the action of *opium*. Another thing, which I remarked in all theſe experiments, was, that the heart, during its *ſyſtole*, became manifeſtly ſhorter, and was lengthened in the time of its relaxation. But to return; at ſix minutes

paſt

paſt twelve, *(i. e. an hour and nine minutes after the frog was taken out of the ſolution)* its heart made only 6 pulſations in the minute; and at eleven minutes paſt twelve, obſerving it without motion, I pricked it with a pin, and breathed upon it, in order to renew its pulſation, but to no purpoſe.

11. TWENTY eight minutes paſt ſeven in the evening, I laid open the whole *abdomen* and *thorax* of a frog, and immediately after immerſed it in a ſolution of *opium* as above. Thirty eight minutes paſt ſeven, when I pricked its legs with the point of a penknife, it made very little motion. Two minutes after this, I turned it to its back, and obſerved its heart moving only between ten and eleven times in a minute. Having laid the frog again on its belly, that it might be more expoſed to the action of the *opium*; at forty eight minutes paſt ſeven, *i. e. twenty minutes* from the firſt immerſion, I turned it again to its back, and obſerving the heart without motion, I opened the *pericardium*; which producing no effect, I cut the heart out of the body, and laid it on a plate, when it gave two or three pulſes, and never after

moved,

moved, tho' it was pricked once and again with a pin.

No motion was produced in any of the other muſcles of this frog, by irritating them.

12. I cut off a frog's head and deſtroyed the ſpinal marrow with a hot wire, then laid open its *thorax* and *abdomen*, and immerſed it in a ſolution of *opium* at nineteen minutes paſt eleven. Eight minutes before twelve, *i. e. thirty three minutes* after immerſion, I obſerved its heart beating very ſlowly: but two minutes before twelve, when I took it out of the ſolution of *opium*, it had no motion. After, this I opened the *pericardium*, and irritated the heart two or three times with the point of a *ſcalpel*, which always produced a few pulſations. I then put the frog in the ſolution for five minutes more, and, upon taking it out, found its heart quite dead.

13. After cutting off a frog's head and deſtroying its ſpinal marrow, I laid open its whole *abdomen*, and immerſed it in a ſolution of *opium*, twenty three minutes before one. After it had lain *ſixteen minutes*, I cut up its *thorax* and *pericardium*; and obſerving the

heart

heart beating very regularly and pretty ſtrongly, 21 times in the minute, I immerſed it again in the ſolution, which had now immediate acceſs to the heart. After five minutes, I took it out of the ſolution; and finding the heart without motion, I pricked it with the point of a knife; upon which it began to beat at the rate of 14 times in the minute, and continued its motions very languidly, and not without ſome interruption, for about a quarter of an hour.

14. I cut out the heart of a frog, and put it in fountain-water at ten minutes paſt ten; immediately after immerſion, it beat about 28 times in the minute. Eighteen minutes paſt ten, it made 6 pulſations in thirty ſeconds. Twenty minutes after ten, I took it out of the water and laid it on a table, and obſerved, that as often as it was gently touched with any thing, it made one full and ſtrong contraction, and no more: however, in four or five minutes, it began to beat of its own accord, and, at twenty eight minutes after ten, performed 19 pulſations in a minute. Thirty five mi-minutes paſt ten, it beat 12 times in a minute.

15. TWENTY

15. TWENTY three minutes paſt twelve, I cut out the heart of another frog, and put it in fountain-water. After *twelve minutes* immerſion, I took it out of the water, when it beat above 20 times in a minute. Having put it in the water for five minutes more, it ceaſed from motion, and when taken out, did not move except when pricked, and then only performed one pulſation.

16. EIGHT minutes paſt eleven, I cut out the heart of a third frog, and put it into fountain-water. Eleven minutes after immerſion its heart beat 8 times in the minute, and four minutes after this it vibrated 11 times in thirty ſeconds; but the motion was confined to about one third part of the heart next its *apex*. *Twenty minutes* after immerſion, it continued to move much in the ſame way; but in two minutes more, obſerving no motion in it, I took it out of the water, and laid it on a table, where it remained at reſt, unleſs when touched. Soon after this, however, it began to move; and at twenty five minutes after immerſion, it made 9 pulſes in ſixty three ſeconds. Four minutes after this, it moved only thrice in fifty ſeconds, and then ceaſed altogether;

unleſs

unlefs that, when pricked with the point of a knife, it gave one very faint pulfation. At forty feven minutes paft eleven, it was quite dead.

17. I cut out the heart of a fourth frog, and at thirty minutes paft ten immerfed it in a turbid folution of *opium* in water of the fame degree of heat with the fountain-water ufed in the three laft experiments*. After this heart had been immerfed *ten minutes*, I took it out of the folution, and laid it on a table, but it made not the fmalleft motion; and when pricked with the point of a knife, tho' it quickly recovered its fhape, yet it was not excited into a proper contraction, as the heart of N° 14. I continued to obferve this heart from time to time for more than half an hour, but it never made the leaft motion.

18. I cut out the heart of a fifth frog, and put it into a folution of *opium* in water five minutes before eight. After *feven minutes* immerfion, I took it out, and laid it on a plate, where it remained at reft. When pricked with a knife, it did not perform a full pulfation like N° 14. but feemed to feel a

* *Viz.* Nearly fixty degrees of *Farenheit*'s thermometer.

a little, by a very faint kind of motion which was excited in ſome of its fibres.

19. At thirteen minutes before twelve, I cut out the heart of a ſixth frog, and immerſed it in a ſolution of *opium.* *Six minutes* after immerſion, it had no motion; but when pricked, made one pulſation. After lying five minutes more in the ſolution, it was quite dead.

20. I cut out the heart of a ſeventh frog, and at thirty ſeven minutes paſt nine in the morning, immerſed it in a ſolution of *opium*, as above. Forty two minutes after nine, when I took it out of the ſolution, it was without motion: but when touched with the point of a knife or probe, it performed one contraction, but with leſs vigour and more ſlowly than the heart of N° 14. Forty ſeven minutes paſt nine, it began to beat of its own accord. Two minutes after this, it moved 6 times in the minute, but much more feebly than N° 14. Six minutes before ten, it beat only 4 times in a minute: after this, it began to beat much faſter; but its motions ſoon returned to their former ſlowneſs. At ten, after having lain near a minute without motion, it began again, of its

its own accord, to beat at the rate of 17 times in the minute, and continued for eight or ten minutes after this to beat very feebly, and in an irregular manner as to time.

21. Mr. *Robert Ramſay* Student of medicine in this place, having diſſolved two ſcruples of *opium* in an ounce of water and a dram of liquid *laudanum*, injected it blood-warm into the *inteſtinum rectum* of a very ſmall dog near ſix months old. In leſs than a minute after the injection was made, the dog could not ſtand on his hinder legs; and in 3 or 4 minutes he had loſt the uſe of them ſo much, that when they were ſtrongly pinched, he neither moved them, nor ſeemed in the leaſt degree ſenſible of pain. He could, however, ſtill ſcramble about with his fore-legs; and when they or his ears were pinched, he howled remarkably, and ſeemed to feel conſiderable pain. Ten minutes after the injection, he lay as if he had been quite ſtupid; only when a noiſe was made by beating on the ground, he opened his eyes a little and howled, but preſently after fell into a profound ſleep. In a few minutes after this, he began to be convulſed; upon which Mr.

 Ramſay

Ramſay injected a ſtrong ſolution of ſea ſalt in water into his guts, which purged him ſeverely, and occaſioned a *prolapſus ani* ; ſoon after this, he awaked from his ſleep, and gradually recovered the uſe of his hinder legs; ſo that in leſs than an hour he could run about the room, tho' he often fell down, his legs bending under him. After three or four hours, he ſeemed to be quite well in every reſpect ; but altho' the experiment was made at mid-day, he could taſte no meat till late at night. When he was in the moſt ſtupid ſtate, he could make uſe of his fore-legs, and complained when his ears were pinched.

22. The ſame young Gentleman, at my deſire, made the following experiment. On the 9th of *April* 1755, after making an opening into the cavity of the *abdomen* of the dog on whom the laſt experiment was made, he injected by the wound a dram of *opium* diſſolved in two ounces and a half of water ; but before he could ſtitch up the wound, about an ounce of the ſolution eſcaped. The dog loſt the power of his hinder limbs almoſt inſtantaneouſly. Two minutes after the injection was made, he began to be convul-

ſed

fed; and, in two minutes more, after having raifed himfelf upon his fore-legs, he fell down fenfelefs. At this time Mr. *Ramfay* laid bare the *thorax*, by diffecting off the teguments, which did not feem to give the dog any pain, and could plainly feel the motion of his heart thro' the *pleura*: it beat 76 times in a minute, but became gradually flower *. Immediately after counting the pulfe, Mr. *Ramfay* cut the ribs on each fide of the *fternum*, which he laid back in the ufual way. The heart, which was thus brought in view, appeared quite turgid, and continued in motion about five minutes; during which time it performed only between 60 and 65 weak vibrations, for they were not compleat contractions. While the heart was thus moving, warm *faliva* was firft applied to it, then cold water, and laft of all oil of vitriol; which fhrivelled the parts it touched, almoft in the fame manner as a hot iron would have done; but none of them accelerated the heart's vibrations, which became gradually flower, till they ceafed altogether.

THE

* The dog's heart in a natural ftate, and before the injection of the folution of *opium*, beat 150 in the minute.

The fibres of ſome of the intercoſtal muſcles on the right ſide of the *ſternum* continued to be agitated with a weak tremulous motion near half an hour after the injection was made into the *abdomen*; but the intercoſtal muſcles attached to the ribs on the ſides of the *thorax* were not obſerved to move, nor did the diaphragm make any motion when its fibres were pricked or cut.

Nothing remarkable was ſeen in the *abdomen*; only, altho' it was opened ten minutes after making the injection, the inteſtines had no motion; whereas, in another young dog, which had got no *opium*, Mr. *Ramſay* obſerved the periſtaltic motion continue half an hour after laying open the *thorax*.

The dog loſt little or no blood in making the wound into his *abdomen*, nor were any of his bowels hurt by it.

23. A ſmall dog into whoſe ſtomach the late celebrated Dr. *Mead* had forced, at four different times, a ſolution of two drams of *opium* in water, lived above an hour and three quarters after getting the firſt doſe. *Vid. treatiſe on poiſons*, Eſſay IV.

24. It may not be improper to add here an experiment related by Dr. *Alſton* in his

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learned diſſertation on *opium* *. Into the crural vein of an old dog forty two pounds weight, he cauſed be injected at three different times, half an ounce of *opium* diſſolved in four ounces of water, filtrated, and of the ſame warmth with the blood of the animal. The firſt time, about fifteen drams were thrown in, and very ſlowly. It had no obſervable effect. About an hour after, eight drams more were injected ſlowly, and immediately the dog was ſeized with ſtrong convulſions; the pulſe was frequent and ſmall, and after ſome time he foamed at the mouth. But there appearing no ſigns of immediate death, after waiting an hour more, the laſt nine drams were thrown in quickly; upon which the pulſe became full and ſlow, and in a minute or ſo, the dog expired.

From the preceeding experiments, we may, I think, fairly draw the following concluſions.

(a) Opium applied to the ſtomach, guts, cavity of the *abdomen* and *thorax* and abdominal muſcles, ſoon leſſens, and after ſome time intirely deſtroys all feeling and power of

* Edinburgh Med. Eſſays, vol. v. p. 1. art. xii.

of motion, not only in the parts to which it is applied, but thro' the whole body. N° 1, 2, 3, 8, 11 & 22.

(b) Opium produces theſe effects much more quickly in animals which are ſoon killed by want of food and air, than in thoſe which can live long without them, and the parts of whoſe bodies preſerve a power of motion and appearances of life for a conſiderable time after they are ſeparated from each other. N° 1, 3, *&c.* compared with N° 21, 22 & 23.

(c) SINCE a ſolution of *opium* injected into the ſtomach and guts deſtroys the ſenſibility and moving power of frogs, fully as ſoon when they are deprived of their heart, as when this organ remains untouched; it follows, that *opium* applied to theſe parts, does not produce its effects by entering the blood, and being, by its means, conveyed to the brain, as ſome have imagined, but by its immediate action on the organs and parts which it touches. N° 1. compared with N° 2. See alſo *Edinburgh Medical Eſſays*, edit. 3. vol. 5. part 1. page 140.

(d) SINCE, after decollation and the deſtruction of the ſpinal marrow, *opium* operates

rates much more ſlowly in deſtroying the heart's motion in frogs, than it does when the animals are intire (N° 6. compared with N° 7.); it follows, that it muſt produce its effects chiefly, if not wholly, by its action on the brain, ſpinal marrow, and nervous ſyſtem. The heart of the frog N° 7. whoſe brain and ſpinal marrow had been deſtroyed, beat 27 times in a minute, after the animal had lain thirty ſix minutes in a ſolution of *opium*; which was only three pulſations leſs than the heart of the frog N° 5. performed thirty five minutes after the deſtruction of its brain and ſpinal marrow, altho' it was not expoſed to the action of *opium*.

(e) WHEN *opium* injected into the veins, and thus mixed with the blood, leſſens or deſtroys the ſenſibility and moving power of animals much in the ſame way as when it is applied to their ſtomach, guts, or cavity of the *abdomen* (N° 24); is it not probable, that it produces theſe effects by its action on the extremities of the nerves which terminate upon the internal ſurface of the heart and whole vaſcular ſyſtem; and perhaps, alſo, by affecting immediately the *medulla cerebri* itſelf? And when a ſolution of *opium*

applied

applied to the bare abdominal muscles of a frog deprived of its brain and spinal marrow, does, after a long time, considerably impair the heart's motion; is it not reasonable to think, that this is owing to the finer parts of the *opium* being absorbed by the bibulous veins and carried to the heart, and thus brought into contact with the nerves of this organ? N° 7. compared with N° 9.

(f) SINCE *opium*, without entering the blood or being carried to the several parts of the body, destroys the power of feeling in animals *merely* by acting on the nerves to which it is applied (*c*) *(d)*, it follows, that the nerves are the instruments of sensation, or, at least, necessary to it. Nor is it sufficient to destroy this conclusion, that there have been instances of animals endowed with feeling whose brains were so greatly diseased, as to *seem* incapable of performing their functions. It is far from being safe to build theories in physic upon a few monstrous appearances in nature.

(g) It appears from N° 4. and 5. compared with N° 3, 6, 8, 10 and 11. that decollation and the destruction of the spinal marrow does not weaken or destroy the heart's

heart's motion in frogs, near so soon as *opium* injected into their stomach and guts, or applied to the muscles and bowels of the lower belly and *thorax*.

(h) ALTHO' a solution of *opium* applied to the opened *thorax* and *abdomen* of a frog, after decollation and the destruction of its spinal marrow, soon weakens or destroys the motion of the heart; yet it does not produce these effects so speedily as when the brain and spinal marrow are intire, N° 11. and 12. In the former case, the *opium* can only affect the heart by its topical influence; in the latter, it not only acts this way, but also exerts its powers upon the brain, spinal marrow, and whole nervous system; and therefore must produce more sudden effects.

(i) IT appears, beyond doubt, from the preceeding experiments, that the heart is not exempted from the power of *opium*, as the learned Dr. *Haller* has affirmed *, but has its motion destroyed by it, as well as the other muscles, only not so soon. See N° 4. and 5. compared with N° 3. 6. 8. & 10. and

* Act. Gotting. vol. ii. p. 147 & 154.

and N° 14. 15. & 16. compared with N° 17. 18. 19. & 20.

'Tis true, that the fibres of the intercoſtals on the right ſide of the *ſternum* of the dog N° 22. continued to be agitated with a tremulous motion conſiderably longer than the heart, and when the intercoſtal muſcles attached to the ribs were quite dead. But did not this happen becauſe, after ſeparating the *ſternum* from the ribs, and thus cutting off all communication between it and the ſpinal marrow, the muſcles attached to it, could be no more affected by the *opium*, which had been injected into the cavity of the *abdomen*; while the heart and other muſcles whoſe communication, by means of the nerves, with the brain and ſpinal marrow, was intire, continued to be expoſed to its action?

(k) As Dr. *Langriſh* has obſerved, that the diſtilled water of laurel-leaves injected into the cavity of the *abdomen*, kills dogs ſooner than when it is taken into the ſtomach *; ſo N° 21. and 23. compared with N° 22. ſhew that *opium* injected into the ſtomach and great

* Phyſical experiments on brutes, p. 64.

great guts of dogs, does not produce either ſuch ſpeedy or powerful effects, as when thrown into the cavity of the *abdomen*. And N° 6. compared with N° 10. ſhews, that a ſolution of *opium* applied to the abdominal muſcles, does not kill frogs ſo ſoon as when all the *viſcera* of the lower belly are expoſed to its action.

(l) ALTHO' it ſeems probable, from N° 22. compared with N° 24. that a ſolution of *opium* injected into the veins of dogs does not kill them ſo ſoon as when thrown into the cavity of the *abdomen*; yet this cannot be certainly concluded, ſince the dog of N° 24. was much older and above ten times heavier than the other.

(m) IT appears, that a ſolution of *opium* injected into the great guts of a dog, affects the inferior part of the ſpinal marrow much more remarkably than its ſuperior part, or the brain; ſince the dogs of N° 21. and 22. not only loſt the power of motion ſooner in their hinder legs than in their fore ones, but alſo were inſenſible of any pain in them, and yet howl'd ſtrongly when their ears were pinched.

(*n*) A

(*n*) A ſolution of *opium* injected into the cavity of the *abdomen* or great guts of dogs, does not deſtroy the feeling and power of motion of their hinder limbs, by ſending any *effluvia* to their muſcles; otherways it could not produce theſe effects ſo inſtantaneouſly, (N° 21. & 22.) Beſides, ſince *opium* thrown into the ſtomach and guts of a frog after being deprived of its heart, deſtroys the ſenſibility and moving power of its muſcles equally ſoon, as if the animal had been intire (N° 2.); 'tis plain, that theſe effects cannot be owing to the finer parts of the *opium* being received into the blood, and by its means carried to the ſeveral muſcles and organs.

(*o*) Nor does a ſolution of *opium* injected into the great guts or cavity of the *abdomen* in dogs produce its effects by tranſmitting through the nerves any ſubtile *effluvia* to the ſpinal marrow; otherways its operation could not have been ſo inſtantaneous (N° 21. & 22.); nor could the ſpinal marrow and its nerves have recovered their functions ſo ſoon, after the *opium* was evacuated by a purgative clyſter, N° 21.

(*p*) It

(*p*) It remains, therefore, that *opium*, by affecting the extremities of the nerves of the parts to which it is applied, does, by means of their connexion and ſympathy with the brain and ſpinal marrow, deſtroy or prevent, through the whole nervous ſyſtem, the operation of that *power* upon which depends ſenſation and motion in the bodies of animals.

(*q*) Since *opium* applied to the abdominal muſcles of a frog, deprived of its brain and ſpinal marrow, does not deſtroy the motion of the heart ſo ſoon, as when it is applied to the abdominal muſcles of a frog whoſe brain and ſpinal marrow are intire, (N° 6. and 7.), it follows, that the brain and ſpinal marrow, and conſequently the nerves derived from them, have a greater influence than any other part of the animal ſyſtem, upon the motion of the heart

(*r*) Opium does not only deſtroy the moving power of the muſcles of animals by intercepting the influence of the brain and ſpinal marrow, but alſo by unfitting the muſcular fibres themſelves, or the nervous power lodged in them for performing its office: otherways a ſolution of *opium*, when applied

applied to the abdominal mufcles or *vifcera* of a frog, would not put a ftop to the heart's motion fooner, or indeed fo foon, as decollation and the deftruction of its fpinal marrow, (N° 4. & 5. compared with N° 8. & 10.). *Opium* therefore does not produce its effects, *folely*, by putting a ftop to the function of the brain and fpinal marrow, but its influence reaches to the fibres of the mufcles themfelves, or to the extremities of the nervous filaments which terminate in them.

When I fay the influence of *opium* reaches to the nervous filaments which terminate in the mufcular fibres, it is not meant, that any *effluvia* or fubtile parts of the *opium* are tranfmitted to them (See *n* & *o* above), but that it deftroys their powers, by means of that fympathy which they have, through the brain or fpinal marrow, with the nerves to which the *opium* is immediately applied.

(f) From the above experiments we may infer, that not only the power of voluntary motion in the mufcles, but alfo their *irritability* or power of motion, when ftimulated, proceeds from the nerves, or is at leaft immediately

mediately dependent on their influence; since *opium*, which produces its effects, *solely*, by affecting the nervous system *(m, n & o)*, destroys those powers so suddenly. I know it has been lately argued by a celebrated author, that the irritability of the muscles must be independent of the nerves, because the muscles of animals preserve a power of moving when irritated, for some time after the communication between them and the brain, by means of the nerves, is cut off*. But since a solution of *opium* applied to the abdominal muscles of frogs, *merely* by its action on the nerves, puts a stop to the irritability or moving power of the heart, much sooner than the destruction of the brain and spinal marrow *(g)*; is it not reasonable to conclude, that the tremulous motions of irritated muscles after their nerves are tied, proceed from the integrity of the nervous filaments below the ligature, and the nervous *power* still remaining in them or in the muscular fibres themselves?

The tying or cutting of a nerve, *only* prevents the derivation of any new *influence* [illegible] from [illegible]

* Acta Gotting. vol. ii. p. 134, &c.

from the brain, to the parts to which it belongs; but does not immediately deſtroy the *power* or *influence* remaining in the nerve itſelf. *Opium* applied in ſufficient quantity to the ſenſible parts of animals, not only quickly puts a ſtop to the function of the brain and ſpinal marrow, and thus produces in the muſcles all the effects of a ligature on their nerves, but alſo deſtroys the *power* of every nervous filament in the body *(r)*, and therefore puts a ſtop to the motion of the heart in frogs ſooner than the deſtruction of the brain and ſpinal marrow.

(t) THE almoſt inſtantaneous palſy brought on the hinder legs of a dog, by injecting a ſolution of *opium* into the cavity of its *abdomen* (N° 22.), and the effects of the ſame ſolution injected into the ſtomach and guts of a frog deprived of its heart (N° 2.), where no part of the *opium* could be conveyed to the muſcles, nor be conceived to alter the nature of their *gluten*; ſhew, that the *irritability* of the muſcles has not its ſeat in this *glue*, as ſome have lately imagined*. But if the motions of irritated muſcles be owing to a diſagreeable ſenſation excited in them or their

* Act. Gotting. vol. ii. p. 152.

their nerves, as we have elsewhere endeavoured to shew *, 'tis easy to see that *opium* must, by destroying the *sensibility* of the muscles, of consequence also destroy their *irritability*.

(u) In animals which have got a large dose of *opium*, the veins, especially those of the membranes of the brain, are observed to be much swelled; whence it has been thought, that *opium* produces its effects in the bodies of animals, partly, at least, by rarefying the blood and compressing the brain: but this distension of the veins seems to be no more than a consequence of the very slow motion of the blood through the heart, on account of the insensibility with which this organ is affected †.

(v) Since *opium* soon puts a stop to the vital motions of animals, which yet continue in

* Essay on the vital and other involuntary motions of animals, sect. ix. and Physiological Essays, p. 188, &c.

† In frogs, into whose stomach and guts I had injected a solution of *opium*, I not only found the heart's auricle, but also the great veins leading to it, much distended with blood. Vid. Essay on vital motions, &c. p. 371. and 372.

in time of ſleep with little or no diminution of their vigour; ſince it often eaſes pain without bringing on ſleep, and ſince, by its topical action on the heart, it deſtroys the motion of this organ after all communication between it and the origin of the nerves is cut off *; it follows, that the effects of *opium* are not owing, as ſome have thought, to its producing ſleep: on the contrary, the ſleep which it occaſions, ſeems to be only a conſequence of its impairing the ſenſibility of the whole nervous ſyſtem.

THE other effects of *opium* may be alſo deduced from the ſame cauſe, particularly its reſtraining all evacuations that are owing to an unuſual irritation of the parts of the body, and at the ſame time promoting thoſe natural ſecretions which have been diminiſhed or ſtopt by ſpaſmodic ſtrictures of the veſſels, from ſome uncommon *ſtimulus* affecting them.

(w) LASTLY, does not *opium* kill animals by rendering their ſeveral organs wholly inſenſible of the *ſtimuli*, which are deſtined by nature to excite them into action; whence not

* Vid. N° 12. 13. 17. 18. 19. & 20. above.

not only a ſtop is put to the periſtaltic motion of the guts, and to the propulſion of the chyle *, but the fluids alſo begin to ſtagnate firſt in the ſmaller and afterwards in the larger veſſels †; while the heart becoming gradually leſs ſenſible of the *ſtimulus*

* In a ſmall dog, which Dr. *Kaau Boerhaave* opened, after having given him three grains of *opium*, he obſerved ſcarce any periſtaltic motion in the guts: the ſtomach was much diſtended; the *pylorus* was ſhut, and the bread and milk, which the dog had taken with the *opium* about ten hours before, was indigeſted. There was nothing like chyle in the *duodenum*, nor any lacteal veſſels to be ſeen in the meſentery. The bladder of urine and great guts were much filled, nor had the animal evacuated either urine or *fæces* from the time he ſwallowed the *opium*. *Impetum faciens Hippocrati dictum*, p. 402. & 403. The learned Dr. *Haller* has alſo obſerved, that *opium* puts a ſtop to the periſtaltic motion of the guts in frogs and other animals, Act. Gotting. vol. ii. p. 154.

† This my worthy Collegue Dr. *Alſton* obſerved with a microſcope in frogs into whoſe ſtomach he had conveyed a few drops of a ſolution of *opium* in water. *Vid.* Medical Eſſays, vol. v. part 1. art. xii. And indeed the great diſtenſion of the heart and its auricle in frogs killed with *opium* (N° 5. compared with N° 3. 6. & 10. above) indicates a more than ordinary reſiſtance to the blood's motion in the arteries, as well as a leſs degree of irritability in the heart. Further, is not the ſlow, full pulſe, and dry parched mouth in thoſe who have got an overdoſe

lus of the blood with which it is diftended, contracts more feebly and at greater intervals, till at laft it ceafes from motion altogether?

ART.

dofe of *opium*, owing, partly to the flower motion of the fluids in the fmall arteries and fecretory veffels of the glands? Tho' it muft be confeffed, that the drynefs of the mouth may be in fome meafure owing to the perfpiration being greatly increafed by the *opium*.

ART. XXI.

The History of a compleat Luxation of the Thigh, in a Letter to Dr. JOHN RUTHERFOORD, *President of the Royal College of Physicians, and Professor of Medicine in the University of* Edinburgh; *by* JAMES MACKENZIE, *M. D. late Physician at* Worcester *.

SIR, *Feb.* 10. 1755.

"THE account which I gave you of a "compleat luxation of the thigh reduced at the *Worcester* Infirmary, I now "send in writing, agreeably to your request. "I send you also Doctor *Wall*'s Letter vouching this dislocation; and Mr. *Jefferys*'s "narrative of the same case. If you think "any further proof necessary, there are still "two Physicians and two Surgeons more, "alive and well, who were present at the "reduction, and will bear witness (if required) to the truth of this accident, as well "as the two Gentlemen above named, and "myself, who am, *&c.*"

WILLIAM

* March 6. 1755.

WILLIAM JONES, a tall, robuſt, healthy Butcher, fifty ſix years old, was carried to the *Worceſter* Infirmary on the ſeventeenth of *Auguſt* 1747, lame, and in grievous pain. His account of himſelf was, That he happened unfortunately, ſome hours before, to ride an unruly horſe, which ran away with him; and that, making an effort to check him, juſt as he was galloping over a ſtone bridge, the animal reared himſelf upon his hinder feet; and immediately fell backward on his rider. The man was ſtunned with the fall; but the horſe quickly recovered himſelf, and went off full ſpeed, dragging poor *Jones* after him, by his leg which was engaged in the ſtirrup. The horſe was ſoon providentially ſtopped, and the man ſet at liberty, but unable to move his thigh, which he believed was broken.

THE Surgeon in waiting, (for four Phyſicians and three Surgeons gave their attendance in rotation, namely, Doctors *Attwood*, *Mackenzie*, *Cameron*, and *Wall*; and Meſſ. *Edwards*, *Ruſſel*, and *Jefferys)* having thoroughly examined the ſituation and figure of the

the parts disabled and in pain, came to one of the Physicians, and told him, that the poor man's thigh was dislocated; that the head of the bone was struck quite out of the *acetabulum*, and lay fairly in the groin. The Physician having, in his younger days, attended *Boerhaave* (who, surely, understood physic and surgery as well as any man ever did), and knowing that, from the prodigious strength of the ligaments, and depth of the socket in that articulation, this learned Professor was of opinion, that the thigh-bone was never dislocated by external violence, but frequently broken near the head: which was the true reason why such accidents were seldom, or never, cured: the Physician, I say, persuaded, that his preceptor was in the right, observed to the Surgeon, that there must be a mistake somewhere, and that there was no instance on record, which could be depended upon, of such a luxation as he described: to which the Surgeon replied, "Sir, if you "will not believe me, you will believe your "own eyes and fingers presently."

The novelty of the case brought all the Physicians and Surgeons to the Infirmary. A Skeleton was fetched, and great care ta-

ken

ken, by a ſtrict inquiry into circumſtances, to ſatisfy all preſent, that the bone was not broken, but really and fairly diſlocated. There is no reaſoning againſt facts: the toes and knee were turned outwards, the diſabled limb was longer than the ſound, the hip-joint utterly inflexible, and the round large head of the bone lay obvious to the ſight and touch in the groin.

The next inquiry was, how this diſlocation ſhould be reduced. All were called to conſultation; not one of the Phyſicians or Surgeons had ever ſeen the caſe before. Some of the principal books of modern ſurgery were looked into; but one and all deſcribed the reduction, and recommended extenſion in ſuch a general, languid, hearſay, manner, that it was plain they were as unpractiſed in the caſe, as the Gentlemen preſent: nor was any better ſucceſs to be expected from *Galen*'s * method of reducing a

* Vid. Gal. in librum Hipp. de artic. commentarior. lib. 4. aph. 42.

And indeed the antients ſeem to have been acquainted with luxations of the hip-joint only in children, or diſtempered bodies; unleſs we ſhall except *Paulus Ægineta*, whoſe a luxation of this joint by hanging the patient to a ſtrong beam by the heels with his head near the ground.

After mature deliberation, it was agreed, that, in caſe the uſual extenſion did not ſucceed, the *vis percuſſionis* (which is well known to increaſe the force to a ſurpriſing degree by accelerating the motion) ſhould next be tried. In order to both, therefore, we provided a large ſtrong table, of a proper length and height, which we faſtened with ſcrews to the floor, and covered with ſuch blankets and bolſters as we wanted; a piece of ſtrong cloth alſo was laid upon the blankets, under the patient's back, of length ſufficient to turn up between his thighs, and paſs over his ſhoulders down to the floor, where both ends were ſecurely fixed, with a view to reſiſt or counteract the neceſſary extenſion. We provided alſo two towels of a convenient length and thickneſs; one of which, at the middle, was tied with a tight, but eaſy knot above the patient's ankle, and the two ends twiſted together, were given to three

whoſe various methods of extenſion, in this caſe, whether real or ſuppoſed, ſome of the moderns have copied, and ſome have altered.

ſtrong men to hold. The other towel was in the ſame manner faſtened above the knee, and the double end given to three more; while the Surgeons ſtood ready, one with his hand on the ball of the diſlocated bone to direct it into the ſocket, one at the knee, and another at the foot to turn them inwards.

When all things were ready, the extenſion was begun, in the common method, by the towel-men; but tho' they exerted their utmoſt ſtrength, the head of the bone was not moved in the leaſt, and their effort ſerved only to increaſe the poor man's torture to an intolerable degree.

FINDING thus the extenſion of no ſignificancy; and the patient's courage reviving after ſome reſpite; the *vis percuſſionis* was was carried into execution after the following manner. The towel-men were directed to ſlacken their towels to a certain point, to ſtand with their feet firm, their arms ſtreight, and their bodies bending a little forward; and, upon a certain ſignal agreed on, were ordered to pull with a vehement and quick jerk, throwing themſelves back with all their might.

AFTER

After every thing was in good order, and the aſſiſtants appriſed of the nature and neceſſity of the operation which they were about to perform; the ſignal agreed on was at laſt given. The towel-men pulled in a moment with a ſtrong and ſudden ſpring; the Surgeons performed their parts dexteroufly; and inſtantly there was a loud craſh heard, which made one of the Phyſicians call out, Alas! the table is broken.—But at that very moment the patient, with a thundering voice, cried, It's in, it's in, it's in. And ſo it really was; for we immediately found the limb reſtored to its natural poſition, length, and flexibility. The patient was put to bed; and, by a proper diet and care, recovered his former health, and could walk perfectly well in three weeks. One of the Phyſicians often met him afterwards on foot driving cattle, and always aſked, How he did? To which his conſtant anſwer was, Very well, thank God and the Gentlemen. And he can now, upon occaſion, walk twenty miles in a day, without fatigue or pain, tho' the injured limb ſtill remains near a quarter of an inch longer than the other.

ATR. XXII.

Some Obſervations on the new Method of curing the Cataract, by extracting the Cryſtalline Humour; by THOMAS YOUNG *Surgeon in* Edinburgh *.

TO reſtore loſt ſight, is recovering one of the moſt uſeful of all the ſenſes, and the couching of the cataract, would be one of the moſt valuable operations, could it always be done with ſafety; but the bad ſucceſs, and dreadful conſequences which often attend it, have deterred many good Surgeons from performing this operation, and thrown it much into the hands of empyrics.

I have couched but few in the old way, and thoſe with ſuch bad ſucceſs, that I was fully determined to operate no more on the eyes; nor did the ſucceſs of the new method performed by the ingenious M. *Daviel*, alter my reſolution for a conſiderable time, till, at the importunity of ſome of my beſt friends, I

* March 4. 1756.

I consented to try this new operation. Six cataracts luckily cast up last summer in the Royal Infirmary at *Edinburgh*, which I extracted in the following manner.

The patient being seated in a chair, with an assistant at his back, to support his head, and keep up his eye-lid, as in the old operation, the operator may stand or sit in a chair, as he finds most convenient.

He should keep down the under eye-lid with two fingers of the one hand, while with the other, he takes the small knife A (*Plate* VII. *Fig*. 3.) with which he pierces the transparent *cornea* at the external angle of the eye, near to where the *cornea* joins with the *sclerotica*, taking great care not to wound the *iris*. Run the knife in a horizontal direction across the anterior chamber, and bring it out about the same distance from the white of the eye, as where it entered; then cut that part of the *cornea* which lies below the two orifices, as much in the form of a crescent as possible, this makes the incision larger, and keeps the cicatrice more off the sight; lift up the flap of the cut *cornea* with the scoop B (*Fig*. 4.) or any other convenient instrument; introduce at the

same

ſame time a common couching needle C, (*Fig.* 5.) thro' the pupil, to open the *capſula* of the cryſtalline *lens*, that the latter may come the more eaſily out. A ſmall aperture generally ſerves this purpoſe; if the *lens* is of a firm conſiſtence, it often ſticks to the point of the needle, ſo that when the inſtrument is withdrawn the cryſtalline comes along with it; if it does not, a very gentle preſſure upon the eye forces it out. The operation may be frequently performed with the knife alone, the *capſula* of the cryſtalline being ſometimes ſo thin, that, after the *cornea* is cut, a ſmall preſſure on the eye makes the *lens* come away.

THIS method of operating is much the ſame with that practiſed by M. *Daviel*, which you'll find at large in the Memoirs of the Academy of Surgery, *vol.* ii. *p.* 337. I have followed the example of the famous Mr. *Sharp*, and ſhunned the great multiplicity of inſtruments M. *Daviel* makes uſe of, which renders this operation more ſimple, leſs tedious, and leſs dangerous.

I ſhall next mention the ſucceſs of each operation in the order they were performed.

1. ROBERT

1. ROBERT LAURIE, aged about 30 years, was admitted into the Royal Infirmary with a cataract in both eyes.

I operated on the left eye the 23d of *July* 1755.

As soon as I had passed the knife into the anterior chamber, he turned his eyes so much upwards, that the *cornea* was quite out of sight; I waited till the eye returned to its former position, when I found the point of the instrument in the *iris*, which I immediately disengaged, and finished the operation without any other accident.

I expected a great inflammation from the *iris* being touched, but was agreeably disappointed, finding the man recover with little pain, no fever, and the inflammation inconsiderable.

ABOUT three weeks after the operation, he could distinguish colours, and large objects tolerably well; but could not bear much light. His eye continued weak and watery for about three weeks more, when he could easily see a pin in the sleeve of his own coat; his eye was clear, but the pupil not quite round, which was certainly owing to the *iris* being hurt.

2.

2. ——— ——— was admitted into the Royal Infirmary about the middle of *September*, with a cataract in the one eye, and the *cornea* of the other quite opaque.

The pupil of the cataracted eye was contracted to above the ſize of a large pin head, but quite immoveable.

He was viſited by ſeveral Surgeons in town, who were of opinion, that the diſeaſe was incurable, and that the bottom of the eye was affected, as well as the cryſtalline *lens*.

I propoſed trying the new operation, before he ſhould be diſmiſſed incurable; to which they very readily conſented.

I performed it without any accident, and the man recovered in a few days, without any fever, pain, or inflammation. He was diſmiſſed the houſe about a fortnight after the operation, when his eye was quite clear, but the pupil ſtill immoveable; and he could only perceive a glimmering of light, which is more than was expected from the appearance of the pupil before the operation.

3. & 4. John Craig, aged about 40 years, was admitted into the Royal Infirmary with

with a cataract in both eyes, which had much of the milky appearance.

I operated on both eyes the 28th of *September* 1755, and nothing extraordinary occurred during the operation; only, upon dividing the *capsula* of the cryſtalline, a ſort of milky liquor came out, and the *lens* was of a dark-brown colour. He had a very ſpeedy recovery; ſix days after the operation, I uncovered his eye; he was capable of diſtinguiſhing colours. I looked again into his eyes on the 13th day, when I found his ſight ſtill better, and his eyes more able to look at ſmall objects, without complaining.

He was diſmiſſed the houſe the 10th of *November*, when he could read without the aſſiſtance of glaſſes.

5. Robert Laurie, whom I have already mentioned, had the operation performed on the right eye the 12th of *October*, when nothing extraordinary happened; he had a very good recovery, with ſcarce any pain or inflammation; he was diſmiſſed from the houſe the 19th of *November*, when he ſaw very diſtinctly with both eyes.

6. Agnes Barrowman, aged about 30 years, was admitted into the Royal Infirmary, with cataracts in both eyes.

I operated on the left eye the 26th of *October* 1755.

The ſpace betwixt her eye-lids, when raiſed up, was ſo ſmall, that I could with difficulty ſee all the *cornea*, which, in this patient, was remarkably flat.

As ſoon as I had paſſed the knife into the anterior chamber, ſhe was ſeized with a fit of coughing, which obliged me to cut the *cornea* in a very great hurry. The opening in the *cornea* was but ſmall, which gave me more difficulty in extracting the cryſtalline, than I had in any of the former.

Notwithstanding this unlucky accident, ſhe had a tolerably good recovery; her eye was pained, and ſomewhat inflamed, for ſometime after the operation, but never violently. She was diſmiſſed the houſe about ſix weeks after the operation, being then able to diſtinguiſh very ſmall objects.

N. B. Some eyes are more proper for this operation than others; the larger the eye, and the more convex the *cornea*, the operation will be the eaſier. This woman had a remarkably

remarkably bad eye in this reſpect; it was ſmall, the *cornea* flat, and the diſtance between the eye-lids, when open, was very little: perhaps the *ſpeculum oculi* would be of uſe to help all theſe faults while the *cornea* is cutting, but no longer, for fear of preſſing out the vitreous humour.

There was nothing particular in the treatment of theſe patients after the operation; it conſiſted chiefly in blooding, ſpare diet, now and then a gentle laxative, and cloths dipt in vinegar and water applied frequently to the eyes; they were not confined to their beds above a day or two, and none of them required fomentations.

I do not pretend, from the above caſes, to make a compariſon betwixt the ſucceſs of couching, and the new method; this requires more caſes than I have had occaſion to ſee.

According to the trials made by ſome of the *French* Surgeons, which you'll find in the Memoirs of the Academy of Surgery, *vol.* ii. *p.* 578. the couching was the moſt ſucceſsful.

Mr. *Morand* couched ſix patients.

3 of them ſaw diſtinctly.

3 of the cataracts roſe again.

M.

M. *la Faye* extracted ſix cataracts in the new way.

2 of the patients ſaw diſtinctly.

2 of them ſaw leſs diſtinctly.

2 of them were quite blind.

M. *Poyet* extracted ſeven cataracts after the new method.

2 of his patients ſaw diſtinctly.

2 of them leſs diſtinctly.

1 could diſtinguiſh light.

2 of them were quite blind.

Were I to judge from my own experience in both operations, the new method certainly claims the preference; ſince I have only operated upon ſix cataracts, and all of them have ſucceeded, tho' ſome were not very promiſing.

This, I hope, will excite others to make further trials and improvements in this operation.

ART. XXIII.

A Hernia *from the* Omentum *falling down into the* Scrotum; *by* THOMAS LIVINGSTON, *M. D. Phyſician at* Aberdeen *.

A Young man, aged about 28, of a very thin habit of body, and naturally of a healthy conſtitution, was ſeized with a pain and uneaſineſs about the region of the ſtomach; for which complaints, (without any regular advice) he took a vomit, which operated in the uſual manner, but without alleviating his former uneaſineſs. In the evening of that day, about ſix hours after the operation of the vomit, he was ſeized with ſharp pains over all his belly, for which, by the advice of a Phyſician, he was ordered an emollient clyſter, and an anodyne at bed-time. The clyſter operated gently, and he paſſed the night pretty free of pain, till towards morning that the pain returned more violent than ever; particularly in the left ſide of the umbilical region, and in the left teſticle.

* February 5. 1755.

teſticle. I was called to viſit him that forenoon, and upon examination found a ſcrotal *hernia* on the left ſide, about the ſize of a large fiſt, extremely hard over all its ſurface, and very painful on the ſlighteſt touch. He had a hard frequent pulſe, intenſe thirſt, and all the other ſymptoms of inflammation. He was immediately blooded to a conſiderable quantity, warm ſtupes wrung out of an emollient decoction were alternately applied upon his *ſcrotum* and belly, and an emollient purgative clyſter was injected. The application of the warm ſtupes gave him ſome ſmall relief, and he imagined the parts were ſofter; but, upon uſing the *taxis*, the pain was intolerable, and it was in vain to endeavour to reduce the *hernia*. He continued in this ſtate till the evening, when his fever demanded a ſecond blooding, which was accordingly performed; another ſtimulating clyſter was thrown up, and ſoon after he had two or three copious dejections of indurated *fæces*. The operation of the clyſter gave him ſome eaſe; but the ſtate of the *hernia* was not in the leaſt altered. He continued reſtleſs and much pained all night, and in the morning he drank the *decoct. tamarindor.*

marindor. cum dupl. ſennæ, which gave him ſeveral looſe ſtools throughout the day, the *hernia* continuing as hard and painful as formerly.

He would not agree to have the operation of the *bubonocele* performed, and in the evening his pulſe became feeble; he had frequent returns of a *ſingultus*, and died next morning. It may be neceſſary to mention, that ſoon after the vomit he cried out, that ſomething was tearing his ſtomach and guts towards the bottom of his belly.

During his illneſs I was much perplexed about the nature of the *hernia*. As the clyſters and ptiſan had operated very naturally, I could not imagine that it was any portion of the inteſtines; and as he was of a remarkably thin habit of body, I could ſcarcely imagine that is was the *omentum*; however, as his friends gave me the liberty of examining the body, my doubts and ſcruples were ſoon ſatisfied. I firſt looked the ſtate of the *abdomen*, where the following appearances were very obvious. The *omentum* fallen down, greatly ſtretched, and ſo tenſe, that one ſhould have imagined it would have broke. The ſtomach much diſtended

tended with air, the great curvature of it much lower down than its natural ſituation; the great arch of the *colon* quite out of its place, and lying as low as the middle of the ſmall guts; the *jejunum* and *ileum* conſiderably inflamed and much diſtended with air, and the meſenteric veſſels much more turgid than uſual. Theſe were the principal things to be obſerved in the *abdomen*. Upon making an inciſion through the teguments of the *ſcrotum*, (in the ſame direction as is ordered in the operation for the *bubonocele*) I ſoon diſcovered the hernial ſac, which was very thin, tenſe, and rigid; and, upon laying the ſac open, there was nothing to be found but the *omentum*, which was compleatly mortified as high as the ring of the muſcle. Upon dilating the ring itſelf, I found a convolution of the *ileum* ſticking in the very mouth of the opening, but the one half of the tranſverſe diameter of the canal was only engaged, and that part of it which was ſtrangulated was in a mortified ſtate. The remaining part of the gut betwixt that and the *caput coli* was much ſmaller than uſual, conſiderably inflamed, and contained a little putrid

putrid viſcid chyle. Upon taking out the *omentum*, I found a reſiſtance towards the lower part of the *ſcrotum*, which I imagined was owing to ſome adheſion; but, upon uſing a very gentle force, it was eaſily extracted, and plainly came out of the *tunica vaginalis teſtis*, in which there was a pretty large hole or perforation. The teſticle appeared to be quite ſound. That portion of the *omentum* which was contained in the *ſcrotum*, weighed ſix ounces and a half.

Art. XXIV.

A Child brought forth at a Rent of the Belly *.

IN *April* 1736, *Elspet Grant*, in the parish of *Moy*, being with child, took her labour-pains. After they had continued three days with the child in the birth, two cracks, as if the rafters of the house had broke, were heard about the sick wife, and her belly was rent from near the navel, with a squaint downwards and to the left side, to near the share-bone. At this rent the child came into the world, the after-burthen was brought away, and the intrails were seen.

The rent was cured without any other application, than that of butter mixed with white sugar, and its scar was only as the scratch of a big pin.

These facts are attested by the judicial oaths of *Anna Kennedy* a midwife, and *Mary Ogilvie* a neighbour, who were present when the rent was made and the child came out of it; of *Margaret Dallas*, who assisted to bring

* May 1. 1755.

bring away the after-burthen; of *Robert Smith* who ſaw the rent and intrails immediately after this; and of *Iſobel Tarrel*, who afterwards examined the ſcar: taken and ſubſcribed by *James Macqueen* younger of *Corribrough*, Bailie to the Laird of *Mackintoſh*, at *Moyhall*, *November* 22. 1738; of which the original ſubſcribed copies are kept by the Secretaries of the Philoſophical Society of *Edinburgh*.

A Child eſcaping at a Rent of the Womb into the Abdomen; *by* ALEXANDER MONRO, *ſenior*, *M. D. & P. A.*

IN *March* 1744, I was deſired by Mr. *Ramſay* Surgeon here, to witneſs the examination of the body of a woman who died in child-labour without being delivered. The account given me of this woman was, that ſhe was about 35 years of age, and had born two dead children, and a living one. Being at her full reckoning, her pains had begun on *Tueſday* morning, and continued in a natural way, the child advancing towards the birth, and

and ſome of the waters coming away all that day and *Wedneſday*, till *Wedneſday* evening, when ſitting in a chair in labour, ſhe gave a ſudden ſpring from the chair, complaining of violent pain in her belly. The child never was felt afterwards by thoſe who attempted to aſſiſt her delivery. She conſtantly complained of violent pain in her belly, with her ſenſes and judgment intire, till *Friday* morning, when ſhe died. On *Saturday* her body was opened.

AFTER cleaning away with ſpunges a conſiderable quantity of blood floating in the *abdomen*, we ſaw a ripe child and its ſecundines lying in the lower part of it, a little to the right ſide. The child, *placenta*, and umbilical rope were intire, and the membranes were as uſual after birth. The woman's *uterus* had its *fundus* raiſed as high as the navel, with its ſubſtance ſoft and ſpongy as is common in pregnants, nothing preternatural appearing in its fore-ſide; but when the *fundus* was turned down and forwards towards the *oſſa pubis*, a large rent four inches long was ſeen towards the neck of the womb; which being again put into its natural ſituation, was opened its whole length, in the middle

middle of its forepart, when we had a better view of the rent, extending from very near the *os uteri* upwards, a little obliquely to the right ſide. The *os uteri* was then very little open. The *cervix* which is diſtinguiſhable plainly from the *fundus* in a woman not with child, was here extended into the ſame common ſac with it. The inner ſurface of the womb was all ſmooth, ſeeming to be covered with a fine villous membrane. From the larger ſize of the ſinuſes at the back and upper part of the womb, I judged the *placenta* to have been formerly applied there.

ART.

ART. XXV.

A preternatural Collection of Waters in the Womb with Twins; by STEPHEN FELL *Surgeon in* Ulverſtone *.

IN *November* 1747, I was called to *Hannah Salthouſe*, mother of ſeveral children, then, according to her reckoning, in the firſt week of the eighth month of her pregnancy. She had obſerved her belly increaſing in bulk very faſt during the preceeding month, and particularly in the two laſt weeks, in every day of which a ſenſible difference was ſaid to have been obſerved. At my firſt viſit her *abdomen* appeared to me much more diſtended than ever I had ſeen in a woman with child, eſpecially from the navel to the upper part of the epigaſtric region, where it was more tenſe than towards the *oſſa pubis*, and the moſt ſtretched part had little ſenſe of feeling. Her pulſe was high and quick, her breathing was difficult, her face

* February 6. 1755.

face was cadaverous, and ſhe had continued unnatural labour-pains, with much thirſt and little urine. The *os tincæ* was a little opened, but without any forming of water that could be felt. I ordered clyſters and an opiate at night, which made her paſs it quietly.

NEXT day the pains were more natural, and ſhe was tolerably eaſy. In the evening, ſhe took an opiate with two ſcruples of *pulv. ad partum*, after which, there were intervals between her pains; but at four in the morning, ſhe became delirious, and appeared like one who had not an hour to live. On taking rich hot negus, theſe ſymptoms went off; and then having felt a child's head floating in water, I broke the membranes, and brought away two male children, who were ſcarce half the ſize of children born at the uſual time. One of them was dead, ſwell'd and livid; the other lived 64 hours, but without taking food, or making any diſcharge by ſtool or urine. The quantity of water voided in this delivery was computed by all preſent not leſs than ſix wine gallons, 48 or 50 *lib.*

MY

My patient's belly ſubſiding to the natural dimenſions, was ſwathed immediately after the after-burthen was brought away. She had ſtrengthening cordials and anodynes given her, with a proper diet, and ſometimes clyſters. Now, on the 12th day after her delivery, ſhe appears out of danger.

ART.

ART. XXVI.

Hiſtories of tophaceous Concretions in the alimentary Canal; by ALEXANDER MONRO *ſenior, M. D. F. R. S. and Profeſſor of Anatomy in the Univerſity of* Edinburgh *.

THE number of hiſtories of tophaceous concretions in the alimentary canal is not ſo great, but that your collection might, in my opinion, admit of ſome few, eſpecially if there is a variety in them.

Hiſt. 1. A healthy boy, about twelve years of age, began to complain of colic pains, which increaſing with frequent gripes, *borborygmi* and vomiting, had ſuch an effect, that his parents aſſerted, he was ſcarce of ſo large ſtature after ſix years of his diſeaſe as he was at the beginning of it. Vomits, purges, vermifuges, attenuants, and a variety of other medicines had been given in that time without any benefit.

HIS father, one of the town-officers or ſerjeants, having then aſked my worthy friend

* February 6. 1755.

and collegue Dr. *Plummer*'s advice, he desired my affiftance. We were told by the lad, who was greatly emaciated and very weak, that fome years paft he had not had the vomiting, but found a hard painful *tumor* above the left groin, which fometimes fhifted place a little, where he fancied often he felt fomething like the ftriking of two hard bodies on each other. He had been of late much fatigued with *tenefmus*. Sometimes he had no excretion of *fæces* for feveral days, and often he could fcarce make any water, and that only in drops. During two days before our vifit, the *tenefmus* was conftant, and he felt fomething hard within the *rectum* near to the *anus*, which he and feveral others had endeavoured in vain to bring away with their fingers.

On extracting this fubftance with a *forceps*, fuch as is ufed for extracting ftones from the bladder, he was much eafier than he had been of a confiderable time. Next day, he paffed two other balls, and on each of the two following days, a ball, which he could not force out at the *anus*, was extracted with the *forceps*. After this, he had no uneafinefs, neſs, and ſoon became a heal ſtrong young man.

The largeſt of theſe five balls which was the firſt extracted, is five inches in circumference and ſomething globular, but with ſeveral prominences and flat ſurfaces. Moſt of the flat parts had a ſmooth ſhining tartarous thin coat, the reſt of it was more rough and of a ſpongy appearance. The two laſt brought away, are leſs in bulk, and without ſo much tartarous cruſt. The two ſmall ones are all covered over with the ſhining tartarous cruſt, which in ſeveral places is prettily variegated with different ſhades of an aſhy colour. One of them has ſome reſemblance in its ſhape to the ſhell of a tortoiſe. The other or ſmalleſt may be compared to two pyramids joined by a common baſe.

The ſecond in ſize and the ſmalleſt are cut thro' near to the middle, where there is a ſmall flat bone, that probably has been the the *nucleus* about which theſe balls were formed, tho' they are not of the ſame ſhape.

We were informed by the parents, that they had often chid their ſon for ſwallowing the ſmall bones of ſheep and lambs feet, the

ſinewy

ſinewy parts of which, when boil'd, the family frequently took for food.

Hiſt. 2. A man who had been long tortured with a painful hard ſwelling in his belly a little above the right groin, which frequently cauſed vomiting and *diarrhœa*, tho' at other times he was very coſtive, aſked my advice, when he was very weak and emaciated. The ſeat of this *tumor*, the kind of feel it had thro' the containing parts, and its tumbling, as the patient ſaid, from one place to another, when he changed poſtures, made me ſuſpect a concretion to be lodged in the great ſac of the *colon*.

In hope to puſh it forwards in the *colon*, I cauſed his great guts to be filled with whey injected by the *anus*, and then directed himſelf and aſſiſtants to preſs repeatedly the *tumor* upwards. This *manœuvre* being ſeveral times renewed without ſucceſs, I preſcribed a briſk purgative, and ordered the injections with the preſſure to be repeated as ſoon as the cathartic began to operate. But this and ſeveral other ſuch operations failing, my patient died.

Having obtained leave to examine his body, my conjecture proved to be right; for

in

in the *caput coli* there was a ball of more than ſeven inches circumference, with a depreſſion at oppoſite ends. The inteſtine had contracted ſo much at the ſide of the ball next to the cavity of the *colon*, that I could not force it thro' the aperture there, but was obliged to cut the ſac in which it was lodged to take it out.

THE ball had no tartarous cruſt on its ſurface, and when it was cut thro', its *nucleus* was a chalky or limy ſubſtance about the ſize of a common pea.

Hiſt. 3. DR. *John Stevenſon* Phyſician gave me a concretion ſix inches in circumference, the *nucleus* of which is a plumſtone, taken out of the inteſtines of a boy of five years old. Tho' the ſtone had been ſwallowed long before the boy's death, the kernel of it was freſh when the ſtone was taken from the middle of the ball. A clyſter had brought away ſeveral other plumſtones from this boy ſome months after the plums had been eat.

Hiſt. 4. DR. *Stevenſon* alſo gave me another ſuch concretion, which has four flat ſides with ſeveral depreſſions in them, meaſuring about five inches in circumference,

formed

formed alſo on a burnet plumſtone, which he took out of the inteſtine of a girl.

Hiſt. 5. FROM the ſame Gentleman I likeways had a third concretion pretty like to, but a little larger than, the one deſcribed and painted by Dr. *Simſon* in *Med. Eſſ. vol.* i. *art.* 32. which, with three ſuch others, he took out of the inteſtines of another patient. Each of them had a ſmall ſtone in the middle, the patient having formerly ſwallowed ſmall ſtones and pebbles, for what he called a colic in his ſtomach.

THE Dr. tells me, that all theſe three patients waſted, without being ſick or loſing their appetite. They were fond of fleſh for food, and were averſe to ſlops. They ſeldom were free of *borborygmi*, which made the *abdomen* to change almoſt conſtantly its appearance, the parts of it riſing and ſinking as the air went from one place to another.

Hiſt. 6. IN a ball of this kind eight inches one way and ſix the other, taken from a Gentleman's inteſtines, whoſe hiſtory I do not know, the *nucleus* is a little round piece of wood about the ſize of a common hazle nut.

NONE

None of the balls mentioned in thefe four laft hiftories have any tartarous cruft, but they and all the other cut ones have the appearance of being compofed of *ftrata* furrounding the *nucleus*, their colour differing in fhades from a dark rufty to a pale afhy colour. Their fubftance, except where there is tartarous cruft, refembles a fine hat or chamois-leather when cut.

Hift. 7. In the collection of curiofities kept by the Surgeons of this place, there is a ball taken out of the ftomach of a horfe, which is nearly fpherical, and nineteen inches in circumference. Its furface has fomething of the mulberry form, being compofed of a great number of hemifpherical knobs, about a quarter of an inch diameter contiguous to each other. Their outward fhell looks like a thin cruft of fandy clay; but within this the fubftance has the fame matted appearance as the human concretions have.

Hift. 8. Balls are alfo frequently formed in the ftomachs of cows. Three of them which were given to me, are almoft exact fpheres of a black colour, compofed posed of an external hard tartarous crust, which is about $\frac{1}{20}$ of an inch thick. Within this there is nothing but short black hairs matted compactly together. The circumference of the largest is 9 inches; of the second, 6; and that of the third, is $5\frac{1}{2}$.

ART.

ART. XXVII.

Remarks on Procidentiæ Ani, Intuſuſceptio, *Inflammation, and* Volvulus *of the Inteſtines; by* ALEXANDER MONRO *ſenior, M. D. & P. A**.

AT ſtool more or leſs of the inſide of the *rectum* is generally thruſt out beyond the verge of the *anus*, which aſcends when the preſſure of the diaphragm and abdominal muſcles ceaſes. If the protruded inteſtine is not then retracted, it is ſqueezed by the *ſphincter ani*, ſo that the return of liquors from the part of it which is beyond this ſtricture, muſt be rendered difficult; on which account, this part ſwells, becomes of a colour more red than natural, and a larger than ordinary quantity of ſlime flows from the ends of the veſſels that open on its inverted villous ſurface. This ſtate is called *procidentia ani*; a diſeaſe, to which children, old people, thoſe weakened by diſeaſes, or ſuch as are attacked by *teneſmus*, from

* February 6. 1755.

whatever irritating cauſe, are more ſubject than others.

The ſpeedy reduction of the inverted protruded part of the inteſtine, is the effectual cure; nor ſhould time be loſt, as is often adviſed, in trying to diminiſh the ſwelling by warm fomentations and poultices, which relax the veſſels, and rarify the liquors, and therefore produce an effect very different from what they are intended to have. If the prolapſed inteſtine is ſo much ſwelled, that it cannot be made to paſs thro' the contracting *ſphincter*, inciſions may be made in its ſurface, by which part of the ſlime and blood contained in the cellular membranes, may be ſqueezed out, to diminiſh its volume, and thus to make it capable of returning again within the body.

The practice of the nurſes and other good women, in making the reduction of a *procidentia*, is very faulty. They apply a warm cloth to the protruded part of the gut, and preſſing on it, endeavour to thruſt it all up at once. Before the inteſtine ſwells, this operation ſometimes ſucceeds with children whoſe *ſphincter* is weak. But dry cloths or fingers are liable to adhere to the villous coat and

and give great pain in taking them off. The cloths or fingers applied here ought always to be befmeared with oil, unfalted butter or axunge, to prevent this adhefion. The bulk of the prolapfed part is often fo great, that it is impoffible to make it all pafs at once thro' the *fphincter*, and a fruitlefs attempt of reduction generally increafes the fwelling. The reduction ought to be made by preffing a fmall part of the fides of the orifice, with a greafy finger, and when that part is thruft within the orifice, another finger is applied to what is then the verge of the orifice to pufh it upwards, while the firft applied finger is withdrawn: by fuch an alternate fucceffion of two fingers, the whole may be introduced in moft cafes without incifions, fo that this difeafe is feldom fatal; and for that reafon, the patient is generally too much neglected after the reduction is made, which is fometimes attended with bad confequences.

If, after the reduction, the part continues to be pained and the patient's pulfe is quick, blood-letting, and a low cooling diet, are neceffary to prevent inflammation and its confequences. In all cafes, too much coftivenefs and

and its contrary a *diarrhœa*, eſpecially with *teneſmus*, are equally to be guarded againſt; ſeeing a return of the *procidentia* may be cauſed by either of them. The relaxed parts are to be braced by ſtrengthening topical medicines. In the aſtringents commonly preſcribed, I can have no confidence; their effect goes no deeper than the ſkin; but ſtimulants, ſuch as ardent ſpirits or tinctures of the aromatic reſins made with them, give a ſpring to all the parts, and excite a glowing heat whenever they are applied, ſo as to touch any part of the extremity of the gut, which they can always be made to do.

I have ſaid, that this diſeaſe, the *procidentia ani*, is ſeldom fatal; and the reduction of the prolapſed part of the inteſtine into the body, is generally regarded as a cure of it; but that this is not true, when the doubling of the inteſtine is high up, will appear from the following hiſtory.

A large-ſized ſtrong healthy boy, a year and an half old, after a *diarrhœa* of ſome days, with *teneſmus*, was obſerved to have a *procidentia ani*, which was treated two days by the women who attended him; after which Mr. *Adam Drummond*, Surgeon in this place,

was

was called to his aſſiſtance. He reduced the *procidentia* frequently; but it ſoon returned, which made him deſire I ſhould be conſulted.

THE inverted inteſtine ſtood out four inches from the *anus*, without being much ſwelled or of a deep red colour, and the child ſeemed to have no other diſeaſe. Mr. *Drummond* moſt eaſily introduced all the *tumor* into the body; but ſoon after it was puſhed out again, upon the child's having a deſire to ſtool, notwithſtanding a ſervant's keeping a finger on each ſide of the *anus* near to each other, while ſome liquid excrement was paſſed. After the reduction was again made, I put my finger, which is long, up the *rectum*, puſhing the orifice of the inverted gut on the point of it, and then found the orifice of the inverted gut reſembling the feel of the *os tincæ* of an unimpregnated womb reſting on it, which I could throw up ſome way further with a ſudden jirk of the laſt joint of the finger, but without being able to invert it. We then cauſed a large quantity of milk and water to be injected with force, while the two ſides of the *anus* were preſſed firmly on the pipe of the ſyringe introduced by the

anus.

anus into the *rectum*, to prevent the liquor's recoiling, in hope that the liquor would carry the inverted gut before it, to cause its return to the natural situation. This operation being repeated several times in vain, the *procidentia* always returning with the *tenesmus*, a very long probe of whale-bone was made, a sponge was fastened round the probe, this was wet in oil, the probe was introduced into the orifice, which, I said, resembled an *os tincæ*, the sides of which rested on the spunge; and with this the intestine was pushed a great way up into the body in the direction of the *rectum*, but without success. Several attempts of the same kind failing, we despaired of a cure; and the child, sometime after, being attacked with severe vomiting and perpetual *tenesmus*, died in few days.

Mr. *Drummond*, who opened the body, told me, that the inversion began a little below the upper part of the sigmoid flexure of the *colon*; and that the mescolon was torn away from the inverted part.

WHEN a doubled part of an intestine is extended into the cavity of this alimentary canal,

canal, without appearing externally at the *anus*, it is called *intuſuſceptio*, which, I am perſuaded, is a much more frequent diſeaſe than is generally thought. I have ſeen ſeveral whom I judged to have died by it, and ſhall now relate the caſes of four people whoſe bodies were examined after death.

1. A middle aged woman, during ſixteen months before her death, ſuffered greatly from colic-pains, diſtenſion of her belly, vomiting, and *teneſmus*. In the latter part of her life, when I firſt ſaw her, ſhe had no ceſſation from pain, except by the force of *opium*.

In the great arch of the *colon* was a doubled part of that gut, ſeven inches long. The containing inteſtine had a very ſlight adheſion to the inverted doubled part contiguous to it. The doubled part was of a dark red colour, but not very hard. The paſſage for the *fæces* thro' it was very narrow, not allowing a finger puſhed with force to paſs.

2. A woman about fifty years of age lived two years with ſuch ſymptoms as were narrated in the preceeding caſe. We found a doubled part of the *colon* four inches long in the

the left loin, with the ſame appearance as deſcribed in the former hiſtory.

3. A girl ſeven year old, after eating a carrot and ſome curren-berries, had a colic which continued with a diſtended belly, vomiting, and paſſing little *fæces*, from *July* till the middle of *December*, notwithſtanding various medicines were given.

Mr. *Malcolm* Surgeon in *Dalkeith*, whoſe patient ſhe was, being allowed to open her corpſe, cut out the affected part of the inteſtine, and ſent it to me. The end of the *ilium*, *valvula Tulpii*, *caput coli*, and *appendix vermiformis*, were raiſed twelve inches within the *colon*, to which they had a ſlight adheſion. The outer ſurface of the contained inteſtines was dark-coloured, and very unequal. The orifice of the prolapſed part was not at its end, but at one ſide an inch and a half from the end, with a ſoft flexible prominence at each ſide of the aperture, which I judge to have been *Tulpius*'s valve. The doubled parts were ſo grown together, that I could not diſtinguiſh one from the other. The paſſage within them was ſo ſmall and crooked, that I could not puſh a probe thro'

thro' it; but cutting it open gradually, I found it was ftill pervious.

4. Dr. *Cullen*, Profeffor of Medicine in the Univerfity of *Glafgow*, communicated the following cafe to me.

A boy about twelve years of age complained of wandering colic-pains, which he imputed to blows received on his belly from fome of his companions. Thefe pains returned frequently with *diarrhœa*, and fometimes bloody ftools, for near a year, when his parents confulted Mr. *James Muir* Surgeon in *Glafgow* about him. The boy was then much emaciated, had a quick pulfe, and was fo weak as to be confined to his bed. Two weeks after this, a livid membranous fubftance, paffed by the boy at ftool, was brought to Mr. *Muir*; who obferving it to be tubular, tied one end of it, and blowing into the other, diftended it into fuch a convoluted tube thirteen inches long, as you fee reprefented A B C, *Fig.* 1. of *Tab.* VII. which I caufed to be drawn from the original which was fent me. As it has the mefentery D connected to all its concave fide, it appears to have been an intire piece of gut, and not the villous coat only. Befides this large portion of inteſtine, there were ſeveral ſhreds and ſmaller pieces paſſed by the patient; notwithſtanding which, Mr. *Muir* ſaw afterwards, among the boy's *fæces*, ſkins of potatoes which he had eat after theſe parts of the inteſtine came away, ſo that they had not made any diſcontinuity in the alimentary canal. The ſymptoms continuing, the boy died in ſix weeks.

Mr. *Muir* opened the body of his patient, in preſence of ſeveral Gentlemen of the faculty, who ſaw what I am now to deſcribe, with the aſſiſtance of a figure, which I cauſed to be taken of the dried preparation of the inteſtine ſent me.

The folds of the inteſtines and *omentum* were all glued together by a fatty curdy matter. Within four inches of the valve of the *colon*, the *ilium* A B C, *Fig.* 2. *Tab.* VII. formed into the uſual curve by the meſentery D, ſuddenly roſe perpendicularly at E, where it was much contracted and had the appearance of a cicatrice. When the inteſtine was opened, this contracted part of it was found much thicker and harder than it was any where elſe, eſpecially on one ſide, where

where it ſtood ſo far into the cavity, as to leave a very ſmall paſſage for the aliment. Along this contracted part, the meſentery F was firm and thick. After this, the inteſtine G became of a natural enough form and make.

The Gentlemen in *Glaſgow* were, I think, juſtly of opinion, that the part of the inteſtine inflated by Mr. *Muir*, delineated in *Fig.* 1. was an intuſuſcepted part fallen away by gangrene from the inteſtine at E in *Fig.* 2. where, if there was a concretion, as is related in caſe 3. it might have ſeparated without leaving any diſcontinuity in the alimentary canal.

I have ſeveral times ſeen an *intuſuſceptio* in the ſmall guts of children, a little below which I obſerved ſeveral worms; but the inverted part was neither ſwelled nor diſcoloured, which made me think this diſorder had happened ſoon before death. In one of them a *lumbricus teres* had paſſed the half of its length thro' a hole made in the gut; but, as there was no redneſs or other mark of inflammation at this part, I judged the perforation to have been made by the worm after the death of the ſubject.

THERE

There is little difficulty in conceiving how a piece of a gut ſhould enter doubled into the part below it, and how it may be gradually protruded downwards to a conſiderable extent by the food or *fæces*, in their deſcent towards the *anus*; but it is not eaſy, when this diſeaſe begins, to diſtinguiſh it from a variety of other diſorders which happen in the alimentary canal, or to find a remedy when it is ſuſpected, eſpecially if the *intuſuſceptio* is in the ſmall guts; and from the obſervation above narrated of the child with the fatal *procidentia ani*, it would appear difficult to cure it; nay, if the doubled parts of the inteſtine are grown together, as in moſt of the hiſtories, a reduction of it is impoſſible. Nature ſeldom will perform what I imagine ſhe did in the laſt caſe, ſeparate the doubled part, and unite what contained it. Nor do I believe any will be ſo hardy as to adviſe the amputation of the affected part of the gut.

'Tis ſurprſing how the people in the preceeding hiſtories lived ſo long as they did, with ſuch large doublings of the inteſtine, and its meſentery preſſed together within an-

other

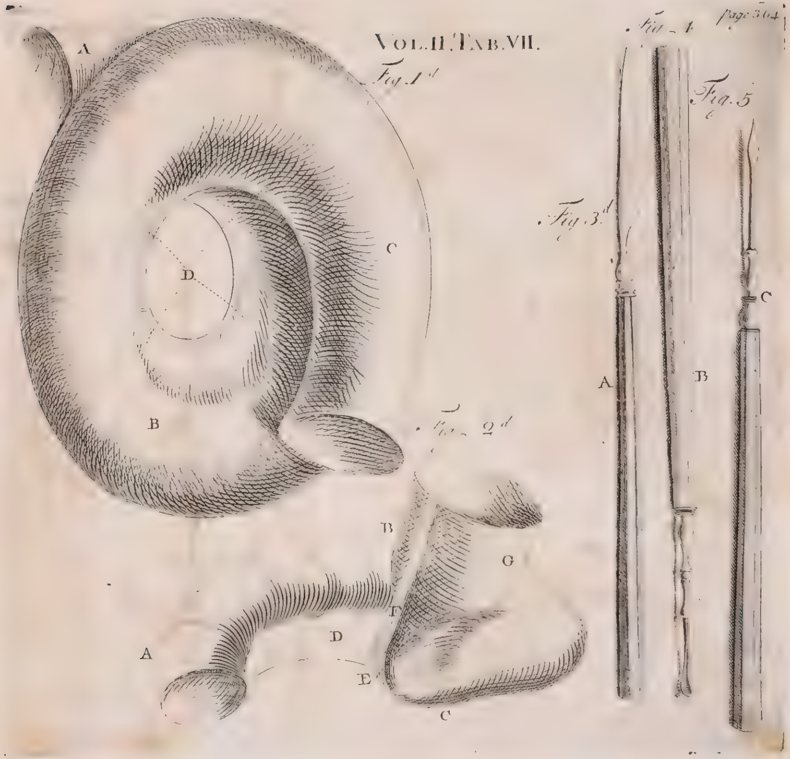
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Fig. 1
Fig. 2
Fig. 3
Fig. 4
Fig. 5
A
B
C
D
E
F
G

other piece of inteſtine, when the common inflammation of the inteſtine often kills ſoon thoſe it attacks; of which I could give numerous examples, but ſhall relate only one. A Gentleman of weak nerves, and ſubject to *flatus* and pain in his ſtomach, was ſeized with a colic-pain about ten o'clock at night, for which he ſwallowed a ſmall quantity of an ardent ſpirit. At three in the morning, twelve or fourteen ounces of blood were let from a vein in his arm, and a laxative clyſter was injected, and operated well. At eleven that forenoon I firſt ſaw him, when his friends thought him much better, being free of pain; but as his belly was greatly ſwelled and very tenſe, his pulſe quick, ſmall and intermitting, his eyes languid, his countenance faded, and a cold clammy ſweat was over all his body, I made the *prognoſis* of his having very few hours to live. He died before five of the afternoon, ſo that his diſeaſe killed him in eighteen hours; and I have heard of others who died in leſs than twelve hours after the firſt appearance of inflammation.

The common practice of taking ſpirituous liquors, or the warm carminatives, when people feel colic-pains, is often unlucky, and public warning ſhould be given againſt it; for tho' relief is found from ſuch things in the windy or ſpaſmodic colics, which is not a deadly diſeaſe, yet they hurry on the inflammatory ones ſo faſt, that they ſoon prove mortal. I muſt likeways think, that writers on the inflammation of the inteſtines don't repreſent ſtrongly enough the languor and low ſmall pulſe which ſuch patients generally have more than in moſt other diſeaſes. It is ſuch, that I have ſeen ſeveral caſes, where people of ſkill, deceived by theſe ſymptoms, have been afraid to order blood-letting, leſt the patient had not ſtrength to bear it, and thereby neglected this evacuation till it was too late. When there is a fixed pain in the ſtomach or inteſtines, with a quick tho' ſmall pulſe, no time is to be loſt; blood ought immediately to be let plentifully, and veneſection ſhould be repeated till the pulſe becomes full and free, which is a hopeful ſign of a cure's being made, tho' neither pain nor fever have yet ceaſed.

THE *intususceptio* or inflammation, but especially the latter, is generally the cause of what is commonly called the iliac passion or *miserere*; for the *volvulus* or twisting a part of the intestines into a knot, which was formerly said to be the case, is generally thought now, when anatomy is more cultivated, and inspection of morbid bodies is more universally allowed, to be an imaginary evil. It is very rare, but not impossible, as will appear from the history subjoined to this, and communicated to me by my Son.

ART.

ART. XXVIII.

A Hiſtory of a genuine Volvulus *of the Inteſtines*; *by* ALEXANDER MONRO *junior, M. D. and Profeſſor of Anatomy* *.

AN old man complained of a colic, which was neglected more than two days; when Mr. *William Wood* Surgeon in *Edinburgh* being called, found him in the agonies of death. Next day I obtained leave to open his *abdomen*, in preſence of ſeveral ſtudents of phyſic. We ſaw fourteen inches long of the *inteſtinum ilium* hanging in a *ſinus* down in the *pelvis*, all black and mortified, occaſioned by a ſtrangulation at the upper part of the two pieces of the *ilium* which formed the *ſinus*. The firm ſtricture there was made by the *appendix vermiformis*, the body of which lay behind the conſtricted parts of the *ilium*, while the end of it paſſing over and before them, had ſunk back again into a *plica* of the meſentery, from which, with great difficulty, I could draw it

* February 6. 1755.

it out, for it was there extended into a globular ſhape of three fourths of an inch diameter, by a glairy liquor, and was lodged in a depreſſion of the meſentery, the entry to which was ſmaller than the cavity where the globular end of the *appendix* had been lodged.

ART. XXIX.

A Deſcription of the American *Yellow Fever, in a Letter from Dr.* JOHN LINING *Phyſician at* Charles-town *in* South Carolina, *to Dr.* ROBERT WHYTT *Profeſſor of Medicine in the Univerſity of* Edinburgh *.

SIR, CHARLESTOWN, *December* 14. 1753.

" IN obedience to your deſire, I have ſent " you the hiſtory of the yellow fever as " it appeared here in the year 1748, which, " as far as I can remember, agreed in its " ſymptoms with the ſame diſeaſe, when it " viſited this town in former years. In *this* " hiſtory, I have confined myſelf to a faithful " narration of facts, and have avoided any " phyſical inquiry into the cauſes of the ſe- " veral ſymptoms in this diſeaſe; as that " would have required more leiſure than I " am, at preſent, maſter of, and would per- " haps have been leſs uſeful than a plain de- " ſcription.

* March 7. 1754.

"I wrote this hiſtory, ſo far as it relates "to the ſymptoms and prognoſtics in the year "1748, when we had the diſeaſe laſt in "this place; intending afterwards, if it re- "turned, to add, from further experience, "the method of cure, and likeways an ac- "count of any other ſymptoms which might "attend it; but, as no ſuch opportunity "has offered, I muſt now omit that part. "However, I hope the deſcription which I "have given of this dreadful malady, which "ſo frequently rages like the plague in the "ſouthern parts of *America*, is ſo full, that "a phyſician may, from thence, not only "form a true judgment of its nature, but "likeways be able to deduce and communi- "cate ſome more certain method of cure, "than has perhaps hitherto been uſed.

"I am ſorry I could not give a fuller "account of the diſſections of thoſe who "died of this diſeaſe, having unfortunately "loſt my notes taken from thoſe diſſections.

I am, &c.

I. THAT

I. THAT *fever*, which continues two or three days, and terminates without any critical difcharge by fweat, urine, ftool, &c. leaving the patient exceffively weak, with a fmall pulfe, eafily depreffible by very little motion, or by an erect pofture; and *which* is foon fucceeded with an icteritious colour in the white of the eyes and the fkin, vomiting, hæmorrhages, &c. and thefe, without being accompanied with any degree of a febrile pulfe and heat, is called in *America*, the *yellow fever*.

II. THIS fever does not feem to take its origin from any particular conftitution of the weather, independent of *infectious miafmata*, as Dr. *Warren** has formerly well obferved,

FOR within thefe twenty five years, it has only been four times epidemical in this town, namely, in the autumns of the years 1732, 39, 45 and 48, tho' none of thefe years (excepting that of 1739, whofe fummer and autumn were remarkably rainy) were either warmer or more rainy (and fome of them lefs

* In his Treatife concerning the malignant fever in *Barbadoes*, page 8.

lefs fo) than the fummers and autumns were in feveral others years, in which we had not one inftance of any one being feized with this fever ; which is contrary to what would probably have happened, if particular conftitutions of the weather were productive of it, without infectious *miafmata*. But that this is really an infectious difeafe, feems plain, not only from this, that almoft all the nurfes catched it and died of it; but likewife, as foon as it appeared in town, it foon invaded new-comers, thofe who never had the difeafe before, and country-people when they came to town, while thofe who remained in the country efcaped it, as likewife did thofe who had formerly felt its *dire* effects, tho' they walked about the town, vifited the fick in all the different *ftadia* of the difeafe, and attended the funeral of thofe who died of it. And laftly, whenever the difeafe appeared here, it was eafily traced to fome perfon who had lately arrived from fome of the *Weft-Indian* Iflands, where it was epidemical. Altho' the infection was fpread with great celerity thro' the town, yet if any from the country received it in town, and fickened on their return home, the

infection

infection ſpread no further, not even ſo much as to one in the ſame houſe.

III. THE ſubjects which were ſuſceptible of this fever, were both ſexes of the white colour, eſpecially ſtrangers lately arrived from cold climates, *Indians*, *Miſtees*, *Mulattoes* of all ages, excepting young children, and of thoſe only ſuch as had formerly eſcaped the infection. And indeed it is a great happineſs that our conſtitutions undergo ſuch alterations in the ſmall-pox, meaſles and yellow fever, as for ever afterwards ſecure us from a ſecond attack of thoſe diſeaſes. There is ſomething very ſingular in the conſtitution of the Negroes, which renders them not liable to this fever; for tho' many of theſe were as much expoſed as the nurſes to the infection, yet I never knew one inſtance of this fever amongſt them, tho' they are equally ſubject with the white people to the *bilious fever*.

IV. THIS fever began in the middle or rather towards the end of *Auguſt*, and continued till near the middle of *October*, when the weather became cold enough to prevent its further progreſs. In the beginning of *Auguſt*, the weather was warmer than I had ever

ever known it in that month: the mercury in *Farenheit*'s thermometer, for ſome days at 2 o'clock *p. m.* roſe, in the ſhaded air, to the 96th degree, at which time ſeveral people died of apoplexies. The latter part of *Auguſt* and the firſt week in *September* were much more temperate; the weather being then much as uſual at that ſeaſon of the year. The ſecond week in *September* was cold, the wind being conſtantly eaſterly and the weather cloudy; after which time I kept a regiſter of the heat of the ſhaded air; an abſtract of which follows.

In the latter part of *September*, and from the 1ſt to the 18th of *October*,

	Septemb.	*Octob.*
The mean heat at 2 *p. m.* was	72	65
The mean nocturnal heat was	68	54
The greateſt heat at 2 *p. m.* was	79	75
The leaſt heat at 2 *p. m.* was	60	52
The greateſt nocturnal heat was	71	70
The leaſt nocturnal heat was	62	42
The greateſt increaſe of heat in 24 hours was	13	17
The greateſt decreaſe of heat in 24 hours was	9	22

In

In all the month of *September*, and in the greateſt part of *October*, the wind was eaſterly.

The depth of the rain in *Auguſt*, *September* and *October* reſpectively, was 6.881, 7.442 and 5.550 inches; which, tho' it exceeded the rain of theſe three months taken together at a medium, from the ten preceeding years, by 5.570 inches, yet it was inferior to that which fell in the ſame months in ſeveral other years; for in the years 1747, 50, 51 and 52, there fell reſpectively, in thoſe three months of theſe years, above 21, 22, 24 and 26 inches of rain.

V. For a day or two before the attack of the fever, people in general complained of a headach, pain in the loins and extremities, eſpecially in the knees and calves of the legs, loſs of appetite, debility and a ſpontaneous laſſitude.

Some however were ſeized ſuddenly, withany ſuch previous ſymptoms.

VI. After a chillneſs and horror, with which this diſeaſe generally invades, a fever ſucceeded, in which,

1. The *pulſe* was very frequent till near the termination of the fever, and was generally

rally full, hard, and confequently ftrong: in fome, it was fmall and hard, in others, foft and fmall; but in all thofe cafes, it frequently varied in its fullnefs and hardnefs. Towards the termination of the fever, the pulfe became fmaller, harder, and lefs frequent. In fome there was a remarkable throbbing in the carotids and in the *hypochondria*; in the latter of which, it was fometimes fo great, that it caufed a conftant tremulous motion of the *abdomen*.

2. THE *heat*, generally, did not exceed 102 degrees of *Farenheit*'s thermometer; in fome it was lefs, it varied frequently, and was commonly nearly equal in all parts, the heat about the *præcordia* being feldom more intenfe than in the extremities, when thefe were kept covered. In the firft day of the difeafe, fome had frequent returns of a fenfe of chillnefs, tho' there was not any abatement of their heat. In a few, there happened fo great a remiffion of the heat for fome hours, when at the fame time the pulfe was foft and lefs frequent and the fkin moift, that one from thefe circumftances might reafonably have hoped that the fever would only prove a remittent or intermit-

 tent

tent. About the end of the ſecond day, the heat began to abate.

3. THE *ſkin* was ſometimes (tho' rarely) dry; but oftner, and indeed generally, it was moiſt and diſpoſed to ſweat.

ON the firſt day, the ſweating was commonly profuſe and general; on the ſecond day, it was more moderate: but on both theſe, there happened frequent and ſhort remiſſions of the ſweatings; at which times the febrile heat increaſed, and the patient became more uneaſy. On the third day, the diſpoſition to ſweat was ſo much abated, that the ſkin was generally dry; only the forehead and backs of the hands continued moiſt.

4. THE *reſpiration* was by no means frequent or difficult, but was ſoon accelerated by motion, or the fatigue of drinking a cup of any liquid.

5. THE *tongue* was moiſt, rough and white, even to its tip and edges. On the ſecond day, its middle in ſome was brown. On the third day, the whiteneſs and roughneſs of the tongue began to abate.

6. THE *thirſt* in very few was great.

7. A

7. A *naufea, vomiting* or frequent *reachings* to vomit, efpecially after the exhibition of either medicines or food, came on generally the third day, as the fever began to leffen; or rather as the fulnefs of the pulfe, heat, and difpofition to fweat began to abate. Some indeed, but very few, on the firft day, had a vomiting either bilious or phlegmatic.

8. VERY few complained of *anxiety* or oppreffion about the *præcordia* or *hypochondria*, nor was there any tenfion or hardnefs about the latter.

9. ON the firft day they generally *dozed* much, but afterwards were very *watchful.*

10. *Reftlefsnefs* and almoft continual *jactations* came on the fecond day.

11. A great *defpondency* attended the fick from the firft attack.

12. THE *ftrength* was greatly *proftrated* from the firft attack.

13. THE *pain* in the *head, loins, &c.* of which they had complained (V) before the attack, were greatly increafed, and in fome, the pain in the forehead was very acute and darting; but thofe pains went generally off the fecond day.

14. THE

14. THE *face* was flushed, and the eyes were hot, inflamed and unable to bear much light.

15. ON the first day, many of them, at times, were a little *delirious*, but afterwards not until the recess of the fever.

16. THE *blood* saved at venæsection had not any inflammatory crust; in warm weather, it was florid like arterial blood, and continued in one soft homogeneous-like mass, without any separation of the *serum* after it was cold. When there was any separation, the *crassamentum* was of too lax a texture.

17. THE *stools*, after the first day, were fetid, inclined to a black colour, and were very rarely bilious, soft or liquid, excepting when forced by art; for an obstinate costiveness attended the febrile state.

18. THE *urine* was discharged in a large quantity, was pale, sometimes limpid, and rarely of a higher than a straw colour, except when the weather was very warm, and then it was more saturated, of a deep colour, and discharged in smaller quantities. It had a large cloud, except when it was very pale or limpid; but more generally it had a copious,

white

white ſediment, even in the firſt day of the fever.

ON the ſecond day, the urine continued to be diſcharged very copiouſly; in ſome, it was then turbid, and depoſited a more copious ſediment, than on the firſt day; this ſediment was ſometimes of a browniſh colour; in which caſe it was generlly followed by bloody urine, either about the end of the ſecond or beginning of the third day. The colour and quantity of the urine, diſcharged in equal times, were remarkably variable, being now limpid, then of a deeper colour, now diſcharged in a larger, then in a ſmaller quantity, which could not be aſcribed to any change made either in the quantity or quality of the drink, *&c.*

VII. THE fever accompanied with thoſe (VI.) ſymptoms, terminated on the third day, or generally in leſs than 72 hours from the firſt attack, not by any aſſimulation, or coction and excretion of the morbid matter; for if by the latter, there would have been ſome critical diſcharge by ſweat, urine, ſtool, or otherways, none of which happened; and if, by the former, nothing then would have remained but great debility. No; this fever

did

did not terminate in either of thefe falutary ways, excepting in fome, who were happy enough to have the difeafe conquered in the beginning by proper evacuations, and by keeping up a plentiful fweat, till the total folution of the fever, by proper mild diaphoretics and diluents. But thofe who had not that good fortune, however tranquill things might appear at this period, (as great debility and a little yellownefs in the white of the eyes, feem'd then to be the chief complaints, excepting when the vomiting continued), yet the face of affairs was foon changed; for this period was foon fucceeded by the fecond *ftadium*; a ftate, tho' without any fever, much more terrible than the firft: the fymptoms in which were the following.

VIII. 1. The *pulfe*, immediately after the recefs of the fever, was very little more frequent than in health, but hard and fmall. However, tho' it continued fmall, it became, foon afterwards, flower and very foft; and this foftnefs of the pulfe remained as long as the pulfe could be felt. In many, in this ftage of the difeafe, the pulfe gradually fubfided, until it became fcarce perceptible; and this, notwithftanding all the means ufed to

fupport

ſupport and fill it; and when this was the caſe, the icteritious-like ſuffuſion, the vomiting, *delirium*, reſtleſsneſs, *&c.* increaſed to a great degree. In ſome, the pulſe, after being exceedingly ſmall and ſcarce perceptible, recovered conſiderably its fullneſs; but that favourable appearance was generally of but ſhort continuance.

2. THE *heat* did not exceed the natural animal heat; and when the pulſe ſubſided, the ſkin became cold, and the face, breaſt and extremities acquired ſomewhat of a livid colour.

3. THE *ſkin* was dry when the weather was cold, but was moiſt and clammy when the weather was hot.

4. THE *reſpiration* was natural or rather ſlow.

5. THE *tongue* was moiſt and much cleaner than in the former (VI. 5.) ſtage, its tip and edges, as alſo the gums and lips, were of a more florid red colour than uſual.

6. VERY few complained of *thirſt*, tho' they had a great deſire for cold liquors.

7. THE *vomiting* or *reaching* to vomit increaſed, and in ſome was ſo conſtant, that neither medicines nor aliment of any kind were retained. Some vomited blood; others only

only what was laſt exhibited, mixed with phlegm; and others again had what is called the *black vomit* *. The reaching to vomit continued a longer or ſhorter time, according to the ſtate of the pulſe; for as that became fuller, and the heat greater, the reaching to vomit abated, and *è contra*.

8. THE

* That which is called the *black vomit*, at firſt ſight, appears to be black; but on a more careful examination, I obſerved, that this colour proceeded from a great quantity of ſmall, flakey, black ſubſtances which floated in the liquor thrown up by vomit; but the colour of this liquor was much the ſame with that which the patient had laſt drank, and was by no means black. Thoſe black flakey ſubſtances are the bile mixed with, or adhering to the *mucus* which lined the ſtomach. For, upon diſſection of thoſe who died of this diſeaſe, not only in this but former years, I always obſerved that the *mucus* of the ſtomach was abraded, and the bile in its *cyſtis* was black and ſometimes very viſcid. In a Lad who died of this diſeaſe in the beginning of the fourth day, and who was immediately opened, the bile was not only black, but had the conſiſtence of thick *Venice*-turpentine, and was exceedingly tough. On the inſide of the ſtomach, there were ſeveral carbuncles or gangrenous ſpecks. And in all thoſe I have diſſected, who have died of this diſeaſe, I have not only always obſerved the ſame, but likeways that the blood was very fluid, and the veſſels of the *viſcera* much diſtended; from whence I have been very inclinable to think, when the diſeaſe was not conquered in its firſt *ſtadium*, that, about the time of the termination of the fever, there was a *metaſtaſis* of the morbid matter to the *viſcera*.

8. The inquietude was very obſtinate, and when they dozed, their ſlumbers were but ſhort and unrefreſhing. There were ſome who were drowſy; but theſe always awaked, after the ſhorteſt ſlumbers, with a great dejection of ſpirits and ſtrength.

9. The *jactations* or reſtleſsneſs was ſurpriſing; it was frequently ſcarce poſſible to keep the patients in bed, tho', at the ſame time, they did not complain of any anxiety or uneaſineſs; but if aſked how they did, the reply was, *Very well.*

10. The *debility* was ſo great, that, if the patient was raiſed erect in the bed, or, in ſome, if the head was only raiſed from the pillow, while a cup of drink was given, the pulſe ſunk immediately, and became ſometimes ſo ſmall, that it could ſcarce be felt; at this time, they became cold, as in a *horripilatio*, but without the anſerine-like ſkin: their ſkin became clammy, the *delirium* increaſed, their lips and ſkin, eſpecially about the neck, face and extremities, together with their nails, acquired a livid colour.

11. The *delirium* returned and increaſed; it was generally conſtant in thoſe whoſe pulſe was ſmall and ſubſiding.

12. THE inflammation of the *tunica conjunctiva* or white of the eyes increaſed much, but without pain.

13. A *yellowneſs* in the white of the eyes, if it did not appear before in the febrile ſtate, became now very obſervable, and that icteritious-like colour was ſoon diffuſed over the whole ſurface of the body, and was continually acquiring a deeper ſaffron-like colour. In ſome indeed no yellowneſs was obſervable, excepting in the white of the eyes, until a little before death, when it increaſed ſurpriſingly quick, eſpecially about the breaſt and neck.

14. THERE were many ſmall *ſpecks*, not raiſed above the ſkin, which appeared very thick in the breaſt and neck; but leſs ſo in the extremities, and were of a ſcarlet, purple or livid colour.

15. IN women the *menſtrua* flowed, and ſometimes exceſſively, tho' not at their regular periods.

16. THERE was ſuch a putrid diſſolution of the blood in this *ſtadium* of the diſeaſe, that, beſides the vomiting of blood formerly mentioned, and the bloody urine ſoon to be taken notice of, there were *hæmorrhagies* from

from the noſe, mouth, ears, eyes, and from the parts which were bliſtered with *cantharides*. Nay, in the year 1739 or 1745, there was one or two inſtances of an hæmorrhage from the ſkin, without any apparent puncture or loſs of any part of the ſcarf-ſkin.

17. An obſtinate *coſtiveneſs* continued in ſome; in others, the ſtools were frequent and looſe; in ſome, they were black, liquid, large and greatly fatiguing; in others, when the ſtools were moderate, even tho' they were black, they gave great relief; in others again, the ſtools nearly reſembled tar in ſmoothneſs, tenacity, colour and conſiſtence.

18. The *urine* was diſcharged in a large quantity, in proportion to the drink retained by the patient: it was pale if the patient was not yellow; but if yellow, then it was of a deep ſaffron-colour; in either caſe, it had a ſediment, or at leaſt a large cloud, which remained at the bottom of the glaſs; in ſome, it was very turbid, in others, it was bloody, and the quantity of blood diſcharged with the urine bore always ſome proportion to the ſtate of the pulſe; when that became fuller, the quantity of blood in the urine was diminiſhed:

diminiſhed: when the pulſe ſubſided, the bloody urine increaſed, and even returned after it had ceaſed ſome days, ſoon after the pulſe became ſmaller.

THIS ſtage of the diſeaſe continued ſometimes ſeven or eight days before the patient died.

IX. WHEN this *ſtadium* (VIII.) of the diſeaſe terminated in health, it was by a receſs or abatement of the vomiting, hæmorrhagies, *delirium*, inquietude, jactations, and icteritious-like ſuffuſion of the ſkin and white of the eyes; while, at the ſame time, the pulſe became fuller, and the patient gained ſtrength, which, after this diſeaſe, was very ſlowly.

BUT when it terminated in death, thoſe (VIII.) ſymptoms not only continued, but ſooner or later increaſed in violence, and were ſucceeded with the following, which may be termed the *third ſtadium* of the diſeaſe, which quickly ended in death.

X. THE pulſe tho' ſoft became exceedingly ſmall and unequal; the extremities grew cold, clammy and livid; the face and lips, in ſome, were fluſhed; in others, they were of a livid colour; the livid ſpecks increaſed

ſo

ſo faſt, that in ſome, the whole breaſt and neck appeared livid; the heart palpitated ſtrongly; the heat about the *præcordia* increaſed much; the reſpiration became difficult, with frequent ſighing; the patient now became anxious, and extremely reſtleſs; the ſweat flowed from the face, neck and breaſt; blood flowed from the mouth, or noſe or ears, and in ſome, from all thoſe parts at once; the deglutition became difficult; the hiccoughs and *ſubſultus* of the tendons came on, and were frequent; the patients trifled with their fingers, and picked the naps of the bed-cloaths; they grew comatous, or were conſtantly delirious. In this terrible ſtate, ſome continued eight, ten or twelve hours before they died, even after they had been ſo long ſpeechleſs, and without any perceptible pulſation of the arteries in the wriſts; whereas, in all other acute diſeaſes, after the pulſe in the wriſts ceaſes, death follows immediately. When the diſeaſe was *very* acute, violent convulſions ſeized the unhappy patient, and quickly brought this *ſtadium* to its fatal end. After death, the livid blotches increaſed faſt, eſpecially about the face, breaſt, and neck, and the putre-

faction

faction began very early, or rather increafed very quickly.

XI. This was the progrefs of this terrible difeafe thro' its feveral *ftadia*. But in hot weather, and when the fymptoms in the firft ftage were very violent, it paffed thro' thofe ftages, as Dr. *Warren* has likewife obferved, with fuch precipitation, that there was but little opportunity of diftinguifhing its different *ftadia*; the whole tragedy having been finifhed in lefs than 48 hours.

XII. It was remarkable, that, 1. The infection was increafed by warm and leffened by cold weather. 2. The fymptoms in the feveral *ftadia* were more or lefs violent, according to the heat or coolnefs of the weather. In hot days, the fymptoms were not only more violent, but in thofe who feemed, in moderate weather, to be on the recovery, or at leaft in no danger, the fymptoms were all fo greatly heightened, when the weather grew confiderably warmer, as frequently to become fatal. In cool days, the fymptoms were not only milder, but many, who were apparently in great danger in hot days, were faved from the very jaws of death by the weather becoming happily cooler. 3. The

difeafe

diſeaſe was generally more fatal to thoſe who lay in ſmall chambers not conveniently ſituated for the admiſſion of freſh air, to thoſe of an athletic and full habit, to ſtrangers who were natives of a cold climate, to thoſe who had the greateſt dread of it, and to thoſe, who, before the attack of the diſeaſe, had overheated themſelves by exerciſe in the ſun, or by exceſſive drinking of ſtrong liquors; either of which indeed ſeemed to render the body more ſuſceptible of the infection. Laſtly, the diſeaſe proved moſt certainly fatal to valetudinarians, or to thoſe who had been weakened by any previous diſeaſe.

XIII. The *prognoſtics* in the firſt *ſtadium* are theſe, 1. The more acute and conſtant the pains are in the head, loins, knees, *&c.* the more the eyes are inflamed; the greater their inability is to bear light, and the more the face is fluſhed at the firſt attack, the fever and all the ſymptoms (VI.) in the firſt *ſtadium* will be the more violent. 2. The more intenſe the ſymptoms are in the firſt ſtate, the ſooner will the fever terminate. 3. The ſooner the diſeaſe runs thro' the firſt *ſtadium*, the ſhorter will be the duration of the

the ſecond, *& è contra* 4. The ſhorter the duration is of the firſt, the greater and more certain is the danger in the ſecond ſtate. For when the fever terminated before the beginning of the third day, death ſeemed inevitably to be the conſequence, as there was then no poſſibility of ſupporting the pulſe, and as all the bad ſymptoms were then hurried on with ſuch precipitation, that the patient generally died before the end of the fifth day, excepting a conſiderable coolneſs of the weather happily interveened; but on the contrary, it was a favourable circumſtance when the fever was protracted to the end of the third day, without any remarkable hardneſs or depreſſion of the pulſe. 5. A great depreſſion of the pulſe, about the termination of the fever, is bad, ſince, from that circumſtance, the vomitings, inceſſant jactations, the coldneſs and lividneſs of the extremities, hæmorrhagies, *delirium, &c.* are uſhered in with ſurpriſing celerity. 6. The more the ſtrength is proſtrated from the firſt attack, the greater is the danger. 7. A vomiting coming on early in the diſeaſe, and continuing or increaſing, is bad, and generally preſages the *black vomit.* 8. A ſediment

ſediment in the urine in the firſt and ſecond day of the diſeaſe is bad, and the more copious the ſediment is, the greater is the danger.

XIV. THE prognoſtics in the ſecond *ſtadium* are theſe: 1. An early yellowneſs in the white of the eyes is bad: when it is obſervable about the end of the ſecond day, in the firſt *ſtadium*, the patient generally dies about the beginning of the fourth day from the firſt attack of the diſeaſe. But when the yellowneſs does not appear till the end of the third day, if the patient does not recover, the diſeaſe ſometimes continues to the 9th or 10th day of the ſecond *ſtadium* before the patient dies. When the yellowneſs of the ſkin and eyes increaſes faſt and acquires ſoon a deep icteritious-like colour, the greateſt danger is to be apprehended. 2. If the inflammation of the white of the eyes increaſes, it is bad. 3. The more inflamed and bloody-like the ſkin is where it has been bliſtered, the greater is the danger. 4. If the vomiting continues or increaſes, it is bad, but the *black vomiting* is generally mortal. 5. When the pulſe varies frequently in its fullneſs, being ſometimes ſmall, then

 fuller

fuller, it is bad. But there was leſs dependence to be had on the pulſe in this than is common in other diſeaſes; for in ſome patients, in the ſecond ſtage of the diſeaſe, even within a few hours of their death, the pulſe, with reſpect to its fullneſs, ſoftneſs, equality and frequency, has continued like that of one in perfect health, altho', from the other ſymptoms, the death of the patient could be foretold with great certainty. 6. The more the ſtrength of the patient is reduced in the firſt, the greater is the danger in the ſecond *ſtadium*. 7. Great reſtleſneſs, inquietude, an early *delirium* and a continuation of it are very bad. 8. Livid blotches about the neck and breaſt, a lividneſs of the lips and nails, fluſhing of the face, or a livid colour thereof, are ſure ſigns of the quick approach of death. 9. Frequent looſe ſtools, which give not any relief, are bad, and the ſooner they ſpontaneouſly happen, the greater is the danger: but thoſe which are black, and continue ſo without any abatement of the ſymptoms, are generally mortal. 10. Bloody urine and all hæmorrhagies, excepting ſlight ones from the noſe, are bad; and the more copious they

they are, the greater is the danger. But a flux of the *menſes*, tho' not at their regular period, if attended with an abatement of the ſymptoms, is a favourable circumſtance, otherwiſe it is bad. 11. A ſuppreſſion of urine, eſpecially in thoſe, who, in the courſe of the diſeaſe, have had large diſcharges that way, is a certain ſign of the quick approach of death.

XV. As to the prognoſtics in the third (X) *ſtadium*, it is ſufficient to ſay

"Nature, alas! was now ſurpriz'd,
"And all her forces ſeiz'd,
"Before ſhe was how to reſiſt advis'd *.

* Dr. *Sprat*'s Account of the plague of Athens.

ART.

ART. XXX.

Anſwer to an Objection againſt Inoculation; by EBENEZER GILCHRIST *M. D. Phyſician at* Dumfries *.

INOCULATION has been practiſed here about five and twenty years, twenty almoſt conſtantly. The ſucceſs of it ſoon demonſtrated the great advantage and neceſſity of it, eſpecially in a place long remarkable for bad ſmall-pox. A letter I wrote two or three years ago to a Gentleman, who aſked my opinion and advice for his children, was a means of introducing it into a part of the country where it had never been tried. While many were diſpoſed to come into it, there were not wanting ſome, as in all ſuch caſes, to oppoſe it; tho' in a pretty large trial of it, at the time, it had ſucceeded to the joy and ſatisfaction of all who wiſhed well to it. Amongſt other objections, one was more poſitively inſiſted on; which, indeed, were it as well founded as it is ſpecious, would

* February 5. 1756.

would effectually put to ſilence the warmeſt advocates for inoculation, and overturn altogether the practice. A proper anſwer was made to it; and perhaps I ſhould have thought no more of it, had I not been informed ſometime after, that, in a ſociety of ingenious Gentlemen, who made matters of this kind the ſubjects of their debates and inquiry, the ſame objection was taken notice of, and ſeemed to make an impreſſion. Pains were taken to undeceive them alſo.

THE objection, to give it in the words of my friend's Letter, is this: "The ſmall-"pox, in the ordinary way, is deſigned by "nature as a drain to clear the conſtitution "of ſome groſs humours, which, if not car-"ried off in this way, would bring on other "dangerous diſeaſes; and for moſt part end "in death, before perſons arrive at middle "age. Now, ſay the Objectors, the ſup-"puration where the ſmall-pox is inoculated, "is ſo inconſiderable, that it cannot be ſup-"poſed ſufficient to clear the body of thoſe "humours which are the parent of other "deſtructive diſtempers. Beſides, ſay they, "this theory is juſtified by facts and expe-"rience. Upon inquiry, it is found, that

"in

"in thofe places where inoculation has moft "prevailed, *particularly in and about Dumfries*, there are as many that die in childhood, and before they arrive at the age of "twenty, as formerly, even including thofe "who are cut off by the fmall-pox. If this "is the cafe, then inoculation is to no purpofe." I fhall not trouble you with what occurred to me in anfwer to this objection. I greatly fufpect the foundnefs of the principle on which it is built, and have ventured to deny the truth of the fact. How trifling foever you may think the objection, yet, as there is great ftrefs laid upon it, and by perfons of rank, I thought it might not be improper to apprife you of it. Thus far my friend.

An objection fo plaufibly formed, and with fuch particular application, feems to affect, more than any thing I have feen advanced, at once inoculation itfelf, and indirectly all, who, from a ferious perfuafion of its ufefulnefs, have fhewn themfelves induftrious to promote it. To encourage and recommend a practice more hurtful in its confequences, than the immediate good of it can be of fervice, which could not efcape the obfervation

of

of every one, ſuppoſes want of attention at leaſt, or ſomething worſe. Yet not ſo much to obviate a reflexion of this kind, becauſe I believe undeſigned, but for a more important reaſon; have I thought it demanded a public animadverſion.

In order to ſatisfy myſelf fully and others, I have not truſted wholly to my own opinion; but converſed with all who have been long and principally concerned in inoculating, thro' an extenſive country: and we can affirm that of the inoculated, few are dead. Two or three of a hundred are the utmoſt we can recollect: but ſuppoſing them more, it is far ſhort of the number that in ordinary circumſtances die before twenty. Nor are we miſtaken, do we think, when we ſay, that they are uncommonly healthy; which the ſmall proportion that are dead will readily ſuggeſt to every one. It is impoſſible to be very exact; but it is ſufficiently evident to us, that the ſtate of the inoculated is much the reverſe of what is objected. If this is true every where, as here it certainly has been hitherto, we are led to a very material diſcovery; and that which was intended as an unanſwerable objection, by giving occaſion

casion to a pretty careful inquiry, has accidentally furnished a new argument in favour of inoculation, and a further proof of the great benefit of it. Long use has shewn it to be immediately a real security against the prevailing malignity of a very mortal distemper; and the present instance affords a strong presumption that it is, in its consequences, no less a preservative from many diseases incident to a period of life the most fatal to mankind.

As to the theory in the objection, it is more philosophical perhaps to argue thus: The fever of the small-pox, communicated in the infant-state, not only destroys or expells the latent seeds of diseases, before they are, by time and accidents, perfected and put into action, but causes such an alteration of the humours as may make them less susceptible of any morbid impressions: and the vessels being so soon accustomed, before they become rigid, to certain motions and extensions, the body is rendered ever after more passive to the impulses of any subsequent distemper; which therefore will be attended with less danger. This is agreeable

able to experience; for one who has suffered an acute illneſs, will bear ſickneſs better than another who never had the like diſtemper, and be leſs overcome by it.

FROM ſuch reaſoning it will ſeem to follow, that the ſooner inoculation can be performed with ſafety, the greater will be the advantage. I cannot aſſert, that to this is owing the more than ordinary healthfulneſs of the inoculated with us, and that ſo few of them are dead: but if nothing forbid, I always adviſe it, the child yet unweaned; and with me it has always happily and pleaſantly ſucceeded. Though many have been inoculated before the ſixth month, I chuſe to delay it till this time, or any convenient time after, unleſs the ſubject is big and ſtrong. Then, beſides the greater pliancy, or kindly yielding of the ſolids, the blood has not, from the uſe of animal food, acquired an inflammatory diſpoſition, and the humours a vicious taint. The firſt paſſages are not, as at a more advanced age, diſordered by worms or other foulneſſes. A formal preparation, the juices being all ſweet and ſound, is very little neceſſary: and the continual dread and danger of the natural infection is early removed; a matter, in this case, both for parents and children, not least to be considered. Teething, I know, is made the great objection here; but from this I have met with so little difficulty, that I make no scruple, except in a few circumstances, which may be easily discovered and avoided.

ART.

ART. XXXI.

A Propoſal of a new Method of curing obſtructed Menſes; *by Dr.* ARCHIBALD HAMILTON *Phyſician in* Edinburgh *.

IN a converſation I had ſometime ago with my friend Dr. *Hunter*, now Phyſician in *Beverley*, concerning the cure of ſome particular diſeaſes, I remember he propoſed a method of removing obſtructions of the *menſtrua* by a mechanical compreſſion of the external iliacs. I thought, from the anatomical ſtructure of the parts, the propoſal pretty reaſonable in ſome caſes; and reſolved to put it into practice, the firſt favourable caſe that occurred to me. About ſix months ago, I was ſent for to viſit a girl betwixt nineteen and twenty years of age, who had been obſtructed for near ſeven months, occaſioned by ſuddenly expoſing herſelf to cold, during the time ſhe was menſtruating. From the firſt appearance of her *catamenia*, to the time of their ſtoppage, ſhe had enjoyed a

* May 1. 1755.

a very good ſtate of health. She had conſulted no regular practitioner ; but had taken a few things, without any relief, that ſome of her female acquaintance had deſired her. Her complexion was a little pale and wan. Her appetite and digeſtion bad, with eructations, and ſometimes ſwelling of her ſtomach. She had now and then ſickiſh and ſqueamiſh fits, with inclination to vomit. Her pulſe was ſlow and languid, with a great laſſitude and inactivity of body, not having a deſire to take her uſual exerciſe. On inquiry, I found ſhe never had any pulmonic diſorder, nor at preſent any complaint or uneaſineſs of her breaſt. She had alſo no pain or ſwelling about the *pudenda*. It now wanted about twelve days of the uſual time of the approach of her *menſes*. I deſired her to receive the ſteam of warm water, every night at bed-time, upon the *pudenda*, in order to relax theſe parts, ſo that the blood might more eaſily flow that way. I ordered her alſo ten days after, an aloetic purgative, to clean the *primæ viæ*, that the blood might find leſs reſiſtance in its courſe when determined to the *uterus*. Next day, after ſhe had taken the purge, I went and

ſaw

ſaw her, and found it had operated four times. About ſeven o'clock that evening, I applied a compreſs and bandage to the crural arteries, at the ſame place where they put the *tourniquet* in amputations of the thigh, but not ſo tight as to endanger a mortification of the inferior extremities. At the ſame time, I deſired her to ſit above the ſteam of warm water. I intended to have ſtaid with her, to obſerve the gradual effects of the bandage; but unluckily was ſent for in a hurry to ſee another patient. I left ſtrict orders, with a woman who was with her, to untie the bandage, in caſe ſhe complained of any difficulty of breathing. On my return, about twenty minutes after, I found her in the ſame ſituation I left her in. Her pulſe indeed beat about ſix ſtrokes in the minute faſter than before the application of the bandage. At the expiration of half an hour, ſhe began to feel a ſenſe of weight and fullneſs in the uterine region, and turned ſickiſh. As her head and breaſt continued pretty eaſy, I begged of her to allow the bandage to continue ſomewhat longer, and gave her a ſpoonful of a cordial-julep. An hour and a half after the firſt application of the bandage, we

found

found a viſible appearance of the return of her *menſes*, by applying a piece of ſoft clean linen to the parts, which, when removed, was ſtained in ſeveral places. I ſlacked the bandage, as her legs were ſomewhat be-numb'd, but was unwilling to remove it alto-gether till the diſcharge ſhould continue to flow for ſometime. I put her to bed; and, on my return next morning, found her ſtill menſtruating and eaſy. I now removed the rollers. The *menſes* continued to flow for three days, and returned regularly next period. Since that time, I underſtand ſhe has been very healthy.

ART.

ART. XXXII.

A Dropſy unexpectedly cured; by THOMAS LIVINGSTON *Phyſician at* Aberdeen *.

AS thoſe who are much converſant in the practice of phyſic and ſurgery, may be frequently diſappointed in their favourable prognoſtics, ſo they may ſometimes be agreeably ſurpriſed with the unexpected recovery of patients, whoſe ſymptoms they had pronounced mortal; an inſtance of which, I preſume, will plainly appear in the hiſtory of the following caſe.

ROBERT DUNCAN, aged about 20 years, a labouring ſervant in the country, was brought to the Infirmary of *Aberdeen June* 19. 1753. He was ſo weak, that he could give no account of the origin or progreſs of his diſeaſe; but thoſe who attended him from the country informed me, that during the preceeding winter he had been much expoſed to cold, after which his belly and lower extremities began to ſwell; he contracted an intenſe thirſt, with a paucity of urine; loſt his

* February 6. 1755.

his ſtrength and appetite; and the above ſymptoms continued to increaſe, without any means being tried for his recovery. He was admitted into the Infirmary, not with any proſpect of relieving his complaints; but as he was deſtitute, and wanted the common neceſſaries of life, he was ordered a place on purpoſe to let him die in peace. He had a very large *aſcites*, with an univerſal *anaſarca*, particularly of his thighs and legs, which ſeemed ready to burſt, and gangrenous-like veſicles appeared upon ſeveral parts of his legs and feet. His *ſcrotum* was diſtended to ſuch an enormous ſize, that I could hardly at firſt diſtinguiſh the *penis*; and the above ſymptoms were attended with a *dyſpnœa* and ſuch a defect of *vis vitæ*, that I could not diſcover a pulſation in any of the arteries, and was only ſenſible of a tremulous ſort of motion about his heart. In this diſmal ſituation I gave him over to the care of a nurſe, ordered him a little warm wine and water, and expected to hear of his death next morning; but I was diſappointed.

June 20. His countenance appeared rather more lively, he ſpoke a little with great difficulty, and I could diſcover a very languid

pulſation

pulſation at his wriſts ; I ſniped the veſicles on his feet and legs, and made ſeveral ſmall punctures with a common lancet, into the moſt depending parts of the *ſcrotum* ; there was a conſiderable diſcharge of a bloody-coloured *ſerum* from the veſicles ; but the diſcharge from the punctures in the *ſcrotum* was clear and pellucid. I ordered warm ſtupes wrung out of a hot aromatic decoction, to be alternately applied to his *ſcrotum* and legs, and he got a large ſpoonful of the *julap. diuretic. Pharmacop. Paup.* every two hours, with wine and water for his ordinary drink.

——21. THERE was a conſiderable quantity of water diſcharged by the punctures *in ſcroto*, the ſize of which was ſenſibly leſſened ; but there came nothing from the veſicles on his legs and feet. I made ſeveral ſmall punctures into the calfs of his legs, and continued the *fotus* and julep ; his other ſymptoms much as yeſterday.

——22. THERE was an incredible quantity of water evacuated from the punctures in his legs and *ſcrotum* ; he had more ſtrength, ſpoke more diſtinctly, and his breathing eaſier. Ordered his medicines to be contined as above.

 ——23. THE

———23. The difcharge from the punctures continued as yefterday, the fwelling of his trunk and extremities was confiderably diminifhed, his breathing eafier, and pulfe more diftinct: he had fome appetite: the fame medicines were continued.

From the 23d to the 26th, the difcharge continued; but turned daily lefs, and on the 27th was intirely ftopped; his head, *thorax*, arms, *fcrotum*, thighs and legs, quite free of fwelling; but his belly ftill confiderably diftended, tho' not near fo large as when he was admitted. He was reduced to the loweft ebb of weaknefs, and had a light nourifhing reftorative diet ordered him.

From *June* 27. to *July* 4. there was no very fenfible change on the fize of his belly; he ufed the *cerevifia diuretica* of the *Pharmacop. Paup.* was daily growing ftronger, and took fmall dofes of rhubarb and *pulv. fcillitic.* at fuch intervals as his ftomach could bear them.

From *July* 4. to *July* 20. he continued the ufe of the *cerevifia diuretica*, and took three of the *pilul. mercur. laxant.* every other night; thefe gave him two or three loofe watery ftools in the morning, which he bore very well;

well; and from the time he began to uſe them, he paſſed his urine in much greater quantity than formerly.

July 25. He was diſmiſſed the Infirmary quite cured, and continued ſtrong and healthy, on the 22d of *October* 1754, when this was wrote.

ART.

ART. XXXIII.

History of a Patient affected with Periodic Nephritic Convulsions; by CORNWELL TATHWELL *M. D. Physician at* Stamford *.

A Fair healthy looking girl, of a full habit of body, aged 17, about the beginning of last *July*, without any warning, fell into violent and general convulsions, and a strong dilirium, which lasted about six hours, then ended in sleep, and left a giddiness behind them for some time. As soon as the fit was off, she was blooded and blistered by her Apothecary, and seemed to have got well. In about three weeks after, a *diarrhœa* came on, and she complained a little of a a slight pain in her back and bowels; and in a month from the first seizure, being a few days before the full moon, she had a second fit about as long as the former, which ended in the same manner. Soon after this fit, her tongue was of a dusky colour,

* May 1. 1755.

colour, her pulſe quick, and her urine pale and ſmall in quantity. The blooding was repeated, a fœtid plaiſter applied to the *abdomen*, and a few ſtools procured by pills of ſoap and aloes; after which there appeared a white ſediment in the urine.

Half an ounce of valerian was got down in ſaline draughts every day; in a week, a ſeton was put into her neck, and the week after ſhe began to go into the cold bath every morning. Some bark and bitters, with a light chalybeate water, were added to the valerian, and the opening pills were uſed occaſionally.

About the latter end of *Auguſt*, after ſeveral ſtools, a giddineſs and ſleepineſs came on with a ſlight delirium, but without any convulſions.

When ſhe was waked out of this ſleep, ſhe complained greatly of a pain in her ſtomach and back; which neither V. S. the *ſemicupium*, nor *tinctura thebaica*, would relieve for any long time. The next day, on repeating the *ſemicupium*, ſhe vomited, and then firſt complained of a *dyſury*: I now gave her ſome pills of ſoap and *ſal. martis*. On the evening after, the pain returning in her

her back and *os pubis*, ſhe was again put into the *ſemicupium*, and afterwards voided a conſiderable quantity of pale urine, which at firſt ſeemed to contain ſome rags, but ſoon hung with a white cloud, and let fall a whitiſh powder. The pain going off next day, the urine was more ſaturated, and as the *ſemicupium* ſoon made her faint and ſick, it was omitted.

ALL this while the *catamenia*, which had firſt appeared about two years before, were entirely regular, and generally came on at their uſual period about a week after each fit. She now continued the uſe of the pills, with *German Spaw-water* and the cold bath. The *ſeton* was dried up about the end of *September*, and ſhe had only a ſlight return of pain in *November*, which was carried off by fomenting the *abdomen*. I recommended lime-water with her pills; but ſhe has continued perfectly well ever ſince, without taking any thing.

1. WERE not theſe a kind of periodical *nephritic convulſions?* I have known nephritic ſymptoms particularly troubleſome to other female patients about the time of menſtruation;

menſtruation; which ſeems to be owing to the greater turgeſcence of the neighbouring parts of the *uterus* at that period, by which the urinary veſſels become at the ſame time fuller and more ſtraitened.

2. It is remarkable theſe convulſions were ſcarce accompanied with any ſymptom of the nephritic kind till the pain and *dyſury* plainly appeared ſo, after the third fit, which the intermediate means ſeem to have contributed to make much ſlighter than the two former.

This will lead us, eſpecially in nervous caſes, which are often found to be ſympathic, to be more particularly attentive to, even, the ſmaller ſymptoms, which are ſometimes a better clue to guide us to the origin of the diſorder, than thoſe violent accidents that are more apt to engroſs our attention.

8. The *diarrhœa* preceeding the fits, was probably only an effect of the irritation of the urinary paſſages by conſent of parts: ſince the keeping of her open afterwards ſeemed of ſervice, inſtead of bringing on a relapſe. Whence I ſhall only beg leave to obſerve, how neceſſary it is in practice to diſtinguiſh

diſtinguiſh ſymptoms that only precede others from thoſe that produce others: in ſhort, of what conſequence it is not to miſtake that for a cauſe which is only prior in point of time; eſpecially as this, I fear, is too common a caſe, and often requires our utmoſt care, in obſerving the *juvantia* and *lædentia*, to avoid it.

ART.

ART. XXXIV.

History of a Fever after Child-bearing; by the ſame *.

A Delicate young woman (who, for two years before ſhe was married, had, from a fright, been ſubject, at times, to a kind of hyſteric epilepſy, eſpecially at the approach of the *catamenia)* about four days after being ſafely delivered of her firſt child, had a milk-fever, which came on attended with languor, catchings and reſtleſsneſs. She was carefully treated with mild diaphoretic and antiſpamodic remedies both internally and externally. A miliary eruption came out upon her arms; the *lochia* flowed regularly, and the belly was kept moderately open: yet ſhe got no reſt, was delirious, her tongue dry and black, and her pulſe quick and ſmall. A bliſter was applied over her head, which, without any pain or ſtrangury, produced a very large diſcharge. The next day, being the ninth from the beginning of the

* May 1. 1755.

the fever, ſhe got ſome ſleep, and became more ſenſible. A gentle perſpiration was promoted by *ſp. mindereri* in diluents, and hot bricks applied occaſionally to her feet. The pulſe was more quiet and regular, the tongue moiſter, and a white cloud ſubſided in the urine. On the 12th day, theſe flattering ſymptoms vaniſhed—the *delirium* roſe higher than ever: ſhe was perpetually comatous or convulſed; breathed with difficulty; often rubbed her hands together, and picked the bed-cloaths; and the pulſe could ſcarce be felt. A ſtimulant clyſter was not retained, veſicatories would not riſe upon her, yet ſhe was excoriated with involuntary urine. In this deplorable extremity, *ſinapiſms* raiſed a bliſter on her feet, and a large flux was promoted from them by an emollient cataplaſm, to which *unguent. ad veſicator.* was added *pro re natâ.*

As no medicine could be got down by the mouth, *pulv. cort. Peruv.* unc. ſs. *in decoct. capit. papav. alb.* unc. vi. was thrown up by way of clyſter, and repeated twice the next day with ſix drams each. Theſe three, containing in all two ounces of bark, were retained for a week; in which time the bad

ſymptoms

ſymptoms went off by degrees, and ſhe took freely of both whey and other nouriſhment. Her feet ſtill continued running, and were dreſſed with *unguent. alb.* Three more of the clyſters with *ſcrup.* vi. each were given in as many days. She now reſted well, and her pulſe was become regular, was much emaciated, and ſo weak, ſhe could ſcarce be raiſed without bringing on ſome hyſteric ſymptoms. By degrees ſhe began to recover her ſtrength: the clyſters were repeated now and then, increaſing the quantity of bark in each to *unc.* i. Her appetite returned: in a fortnight ſhe could be got out of bed: in another week, inſtead of the clyſters, ſhe took *extr. cort. Peruv. ſcrup.* ii. twice-a-day, occaſionally interpoſing ſome *rhubarb*; in about ten days more ſhe was got pretty well. The next month ſhe had a ſlight fit of the kind ſhe had been uſed to before marriage; but, by the help of ſome volatile fetid tincture in bitter wine, ſhe got rid of theſe, and has continued free from them ever ſince, tho' ſhe has had ſeveral more children.

ART.

Art. XXXV.

History of a Fever with bad Symptoms; by the same *.

A Middle-aged man, generally employed in a malt-kiln, about the latter end of *July* 1749, fometime after having fatigued himfelf, and got into a quarrel in a crowd at a public diverfion, was feized with a fever; in the beginning of which, he was blooded, then had a vomit, and afterwards a great variety of diaphoretic medicines were tried with him. He was kept fweating, and feveral crops of eruptions fucceeded each other, without any relief. Blifters were applied in feveral places, yet ftill a *delirium* and *vigiliæ* continued. Mufk, and even the bark in fubftance, was given him; but all without any effect for above a month. The Apothecary who attended him in the mean time often urged the neceffity of further advice; but his friends would not confent to it, till, at laft, I was called in.

I

* May 1. 1755.

I found him in continual tremors and *ſubſultus tendinum*; his pulſe was ſmall and obſcure, his tongue foul and duſky; and his urine had only a ſlight cloud in it. His eyes were ſo dim, he could ſcarce diſtinguiſh the perſons about him; and it was very difficult to make him hear, eſpecially on the right ſide. He was reſtleſs, languid, and delirious; often groaned and ſighed, and was ſtill bathed in a perpetual ſweat. Some red ſpots appeared upon his breaſt, and many purple and livid ones on his back.

As he had been coſtive for ſome days, firſt a ſtool or two was procured by a warm clyſter, ſome antimonial drops (which I have often experienced to deſerve very juſtly the encomiums Dr. *Huxham* * has beſtowed upon them as an excellent deobſtruent) were given now and then in red wine. Elixir of vitriol and volatile tincture of bark were adminiſtred, firſt in ſaline draughts, and afterwards in tincture of roſes. Within three days, his ſweats were gradually checked, the red *papulæ* came to a kindly ſuppuration, the livid ſpots diſappeared, the tremors, *ſubſultus*, and *delirium* went off. An abſceſs (which I had foretold

* Obſ. de aëre, vol. i. p. 141.

foretold from the particular deafneſs on that ſide) broke in his right ear, and, together with a bliſter behind it, was kept conſtantly running. In a day or two more, all the bad ſymptoms were vaniſhed; his pulſe grew fuller and ſtronger, a good ſediment appeared in his urine, and his tongue regained its natural colour. The volatile tincture was ſoon changed for a much larger proportion of the ſimple tincture of bark. In a week, not only his ſenſes returned, but an appetite; and he began to recover his ſtrength. A ſtrong decoction of bark was added to the laſt mentioned tincture, by which, with a few doſes of rhubarb interpoſed occaſionally, the cure was compleated.

ART. XXXVI.

Accounts of extraordinary Motions of the Waters in ſeveral Places of North Britain, *and of a Shock of an Earthquake felt at* Dunbarton.

ONE great deſign of the inſtitution of *this* Society being faithfully to record every remarkable *phænomenon* in nature that occurs, it has been thought proper to inſert the following accounts of the effects of the late earthquakes, as they have been obſerved in our diſtrict of *North Britain*; nor do we regrete, that they are not more ſingular or intereſting in their kind. When all the facts and circumſtances ſhall be collected by the united labours of the learned in different places, there is reaſon to expect, they may furniſh materials for a more compleat and accurate hiſtory, than hath been tranſmitted of any like event that ever happened in any preceeding age of the world.

THAT a tremor, which is hardly to be felt at land, or which may altogether eſcape notice

notice there, may be very perceptible on the waters, will eaſily be believed; nor is it more incredible, that a ſmall concuſſion given to a great body of water will produce a very remarkable agitation in the narrow creeks and ſhallows. And it is obſervable, that theſe commotions were moſt violent in the deepeſt lakes, particularly in *Loch-neſs*; the extraordinary depth of which hath been ſometimes aſſigned as a reaſon for its never freezing, the ſevereſt winters not being able to reduce it to the coldneſs of ice.

1. *Letter from Mr.* ROBERT GARDINER *Commiſſary to the Army in* North Britain *to Dr.* JOHN STEVENSON *Phyſician in* Edinburgh, *giving an Account of the Agitation of the Waters of* Loch-neſs *on the 1ſt of* November 1755, *when the City of* Liſbon *was deſtroyed by an Earthquake.*

SIR, EDINBURGH, *December* 22d 1755.

"I have your favour of the 20th, and, in "compliance with your requeſt, I give "you the following reply to your queries.— "I arrived at *Fort-Auguſtus* from *Fort-* "*William*

" *William* on the 31ſt of *October* laſt. Next
" morning about ten, I walked abroad, when
" the Barrack-Maſter and ſeveral others came
" and acquainted me, that they had ſeen a
" very extraordinary agitation in the waters
" of *Loch-neſs*. I refuſed giving credit to their
" ſtory, and a little afterwards returned to
" the *Fort*.

" ABOUT eleven, my Clerk and the Brewer
" at that place came and acquainted me, that
" a more extraordinary agitation than the
" former had happened, and they apprehend-
" ed ſome danger to our brewery, which is
" ſituated where the river *Oich* diſcharges it-
" ſelf into the lake. I walked then to that
" place; but before my arrival, the water had
" returned to its uſual channel. I ſaw very
" clearly the marks on the banks, to which
" it had flowed. The banks were quite wet,
" and a ſtrip of leaves of trees and twigs,
" *&c.* left on them. I inquired then into
" this affair, and the account the ſpecta-
" tors gave, were; That they obſerved
" the river *Oich*, which runs from weſt
" into the head of the lake, ſwell very
" much, and flow up the river from the

" lake with a pretty high wave, about 2 or
" 3 feet higher than the ordinary ſurface,
" with a pretty quick motion againſt the
" wind, and a rapid ſtream about 200 yards
" up the river; then broke on a ſhallow,
" and flowed about 3 or 4 feet upon the
" banks on the north-ſide of the river, and
" returned again gently to the lake: That
" it continued ebbing and flowing in that
" manner, for about an hour, without any
" waves ſo remarkable as the firſt, till about
" eleven o'clock, when a wave higher than
" any of the reſt came up the river; and,
" to the great ſurprize of all the ſpectators,
" broke with ſo much force on the low
" ground on the north ſide of the river, as
" to run upon the graſs 30 feet from the ri-
" ver's bank.

" LIEUTENANT *Smith* of the artillery,
" Mr. *Gwyn* ſun of Captain *Gwyn* of the *Loch-*
" *neſs*-galley, Mr. *Lumſden* Barrack-maſter
" at *Fort-Auguſtus*, Mr. *Forbes* Barrack-ma-
" ſter at *Bernera*, *Thomas Robertſon* Brewer
" at *Fort-Auguſtus*, and *George Bayne* my
" Clerk, and ſeveral others, were the ſpecta-
" tors of this extraordinary *phænomenon*;
" ſome

"some of them saw the whole progress of it, "others only a part.

"*Loch-ness* is about 20 miles in length, and "from 1 to 1½ miles broad, and bears from "south-west to north-east *. There was "no extraordinary muddiness in the water, "tho' it did not appear quite so clear as "usual. The morning was cold and gloomy, "and a pretty brisk gale blowed from "west south-west. The river *Oich* lies on "the north side of the *Fort*, and on the "south side, runs the river *Tarff* from west "south-west, and discharges itself into the "head of the lake, which was observed to "be quite agitated at the same time and "manner as the other. But there was no "shaking or *tremor* felt upon the land.

2. *Letter from Lieutenant* ISAAC BARRE' *to* DAVID ROSS, *Esq; Sheriff-Depute of the Shire of* Banff, *concerning the Motion of the Waters of* Loch-ness, November 1. 1755.

SIR, FORT-AUGUSTUS, *January* 15. 1756.

"I Have been here eight days, and, by "the strictest inquiries, have at last "picked

* It lies between the 57th and 58th deg. of northern latitude.

" picked up the following account. Perhaps the whole may be of little confequence; or, if it is, may come too late to be of any ufe to the Gentlemen who defired it.

" About half an hour after ten in the " morning of the 1ſt of *November*, *Lochneſs* was obferved to rife in a moſt extraordinary manner at *Fort-Auguſtus*; it ruſhed with great rapidity above two hundred " yards up the river *Oich*, which runs into " the Loch near the *Fort*; it laſted by the " beſt accounts about three minutes: the " people of the country, unaccuſtomed to " fuch an appearance, imagined at firſt it " was caufed by a great number of felks " ruſhing up the Loch. There was no " ſhock of any fort felt on ſhore, and the " air was remarkably clear, and a very mild " day, the wind then being weſterly (and " of courfe againſt the ſtream.)

" I could get no account of any perfon's " being in a boat on the Loch at the time; " but what follows has an air of truth, be" cauſe the relater is reckoned an honeſt " man and of great veracity; and befides, " had told the fame circumſtances frequent-

" ly

"ly before he could fuſpect that this extra-
"ordinary *phænomenon* would be the ſubject
"of a ſerious inquiry.

"He went with a boat for wood, from "*Glen-morriſon* to the *Old General's Hut* on the "oppoſite ſide; and, about the time above-"mentioned, his boat being near loaded, as "he was coming to it from the hill, he "obſerved a ſudden and violent wave co-"ming, which drove her ſeveral yards on "the beach, and returning, drew her back "into the Loch; a ſecond came with rather "more violence, and pretty near the ſame "effect; a third came with a much more "extraordinary rapidity than either of the "former, and with more ſurpriſing effects; "it drove the boat further on the beach, "took the wood intirely out of her, broke "off the rudder, and left her aſhore full of "water. At this time there was a gentle breeze from the weſt, and a mild ſerene ſky.

"This account was given by *James Fer-"guſon* a Miller at *Folt-Soy* near *Glen-Morri-"ſon*, and alſo atteſted by his companion "*James Macdonald*.

"They aſſert, that the water, in their o-"pinion, upon examining the ground after-

"wards,

" wards, muſt have riſen at leaſt three feet " perpendicular; and the time of the whole " agitation was about three minutes.

3. *Letter from Mr.* JOHN ROBERTSON *to* JAMES SMOLLET *of* Bonhill, *Eſq; one of the Commiſſaries of* Edinburgh, *concerning the Agitation of the waters of* Loch-lomond, November 1. 1755 *.

SIR, ROSDOB, *December* 17. 1755.

" AS I have been from home for a fortnight paſt, your's came to hand only " yeſterday, or would have anſwered it " ſooner. The taſk you impoſe upon me, " *viz.* the giving an account of the commotion that was obſerved in the waters of " *Loch-lomond* on the 1ſt of *November*, is already executed by Mr. *Macfarlane* the Miniſter of *Arrochar*, according to the beſt " accounts he could get of it from the ſpectators; as I was not an eye-witneſs of " this uncommon *phænomenon*, and as the account he gives of it is perfectly agreeable " to the relations of all thoſe who ſaw it, " I

* *Lochlomond* lies upwards of fifty *Engliſh* miles weſt of *Edinburgh*, about the 56th degree of north latitude.

" I will write you an exact copy of his re-
" lation, which, I think, is as full and dif-
" tinct an account as can be got of it.

" On the 1ft day of *November* laft, *Loch-*
" *lomond*, all of a fudden, and without any
" guft of wind, rofe againft its banks with
" great rapidity, and immediately retired;
" and in five minutes time fubfided, till it
" was as low in appearance as any body then
" prefent had ever feen it in time of the
" greateft fummer drought, and then it in-
" ftantly returned toward the fhore, and in
" five minutes time rofe as high as it was
" before; and the agitation continued at
" this rate from half an hour paft nine fore-
" noon till fifteen minutes paft ten, taking
" five minutes to rife and as many to fub-
" fide; and from fifteen minutes after ten,
" till eleven, every rife came fomewhat fhort
" in height of the immediately preceeding
" rife, taking five minutes to flow, and five
" to ebb, until the water fettled as it was
" before the agitation. After the agitation
" was over, the height to which the water
" had rofe was meafured, and found to be
" 2 feet 4 inches perpendicular. *Loch-long*
" and *Loch-keatrin* were agitated on the fame
" day

"day, and about the fame time ; but the "*phænomenon* was not fo minutely obferved "as that any exact account can be got of it.

"This relation of Mr. *Macfarlane*'s of the "*phænomenon*, contains in it every thing re-"markable I have as yet heard concern-"ing it.

4. *Letter from Mr.* Mark M^cCallum *to* George Clerk, *Efq; concerning an unufual motion of the Waters of the Frith of* Forth *at* Queens-ferry, *feven miles weft of* Edinburgh, *on the 1ft of* November 1755.

SIR,

"TO the beft of my remembrance, on "*Saturday* the firft of *November*, a-"bout ten o'clock before noon, being then "on the pier at *Queens-Ferry*, I obferved the "water to rife very fuddenly, and return "again with the fame velocity about a "foot or eighteen inches perpendicular, as "near as I can remember ; the barks and "boats then afloat ran forwards and back-"wards with great rapidity, and this conti-"nued for the fpace of three or four mi-"nutes ; but after the fecond or third rufh "of

" of the water, it gradually abated. There " was no wind at the time the waters were " agitated as above."

5. *An Account of an unuſual Motion in the Waters of* Cloſeburn-*Loch in* Dumfries-*Shire, on* Sunday *the 1ſt of* February 1756, *in a Letter to* George Clerk, *Eſq*; *.

SIR, Closeburn, *February* 4. 1756.

" About a quarter before nine on *Sunday* " morning, we were alarmed with an " unuſual motion in the waters of *Cloſeburn*-" Loch. The firſt thing that appeared to " me in this wonderful ſcene, was a ſtrong " convulſion and agitation of the waters from " the weſt ſide towards the middle, where " they toſſed and wheeled in a terrible man-" ner. From thence proceeded two large " currents, or more properly rivers, which " ran with a ſwiftneſs and rapidity beyond " all deſcription quite contrary ways; one " from the middle to the ſouth-eaſt, and

* It may not be amiſs to obſerve, that, on the 1ſt of *February* 1756, according to advices from abroad, a conſiderable earthquake happened at *Oporto* in *Portugal.*

"the other to the north-eaſt points of the "Loch. There they were ſtopped ſhort, "as the banks are pretty high, and ob- "liged to turn; which occaſioned a pro- "digious tumbling and agitation at both "ends of this body of water. There was "likeways a current which roſe ſome- "times conſiderably above the ſurface, near "the weſt ſide, that I frequently obſer- "ved running with great velocity a hun- "dred yards to the ſouthward, and re- "turning in a moment with as great ve- "locity the other way. What I obſerved "in the next place, was the toſſing of "the waters in the ponds, which were "more or leſs moved as the agitation of "the Loch came near this ſide, or kept "at a greater diſtance from it. But, as "it is beyond my capacity to give a particu- "lar deſcription of all that happened on this "occaſion, I ſhall conclude with telling you, "that the agitations and currents above men- "tioned continued, without intermiſſion, for "at leaſt three hours and an half, or four "hours, when they began to abate a little in "their violence, tho' the toſſings and cur- "rents were not quite over at ſunſet. I had

"almoſt forgot to tell you, that this *phæno-*
"*menon* was renewed on *Monday* morning a
"little before nine, and laſted for an hour
"and an half; but the motion of the waters,
"*&c.* were not ſo violent as the day before.
"What is very remarkable, there was not
"the leaſt gale of wind on *Sunday* till one
"o'clock, which helped us not a little in our
"obſervations."

6. *Letter from Mr.* PANTON *relating to the Earthquake felt at* Dunbarton.

SIR, DUNBARTON, *January* 17. 1756.

"IN anſwer to your's relative to the earth-
"quake here, there happened but one
"ſhock, and that very moderate, before one
"of the clock *December* 31ſt 1755; it con-
"tinued for a very ſmall ſpace of time. It
"agitated ſome people in bed very percepti-
"bly, and was felt by Mrs. *Weir* and ſome
"others, who were not gone to bed. It
"had a ſenſible effect upon tame birds in
"cages, and fowls: it ſo alarmed both, as
"to make the former flutter prodigiouſly,
"and the latter to croak in a frightful man-
"ner: it ſhook the board out of one cage,

"and

" and ſpilt the water in the glaſs thereof. It " was equally felt by thoſe who lived in " ground-ſtoreys, as by thoſe in ſecond and " third ſtoreys. There were ſome ſconces " in Mrs. *Colquhoun*'s houſe obſerved to vi- " brate during the ſhock. Nothing more " happened worth noticing.

N. B. This earthquake was felt at the ſame time in *Glaſgow*, *Greenock*, and other places of the neighbouring country.

End of the ſecond Volume.

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Loch-lomond

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W.

Y.

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