THE PHILOSOPHICAL

HISTORY

AND

MEMOIRS

OF THE

Royal Academy of Sciences at *Paris*:

OR,

An ABRIDGMENT of all the PAPERS relating to *Natural Philoſophy*, which have been publiſh'd by the *Members* of that *Illuſtrious Society*, from the Year 1699 to 1720.

With many Curious OBSERVATIONS relating to the Natural Hiſtory and Anatomy of Animals, *&c.*

Illuſtrated with COPPER-PLATES.

The Whole Tranſlated and Abridged,
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A TABLE OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCVII.

In the HISTORY.

In the MEMOIRS.

AN

AN ABRIDGMENT OF THE Philoſophical Diſcoveries AND OBSERVATIONS IN THE HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1707.

I. *On the light of bodies produced by friction; tranſlated by Mr.* Chambers.

THE new phoſphorus diſcovered by M. *Bernoulli*, and mentioned in the preceding papers, could not fail of raiſing the curioſity of philoſophers, and eſpecially thoſe of the academy, who had a ſort of right to a diſcovery made by one of its members. Among other experiments on this head, they came at length to the light which certain bodies yield, by rubbing in the dark; the reſult whereof is as follows.

As moſt of theſe experiments were only made on bodies which yield light the moſt eaſily, as a cat's back when rubbed againſt the hair in winter, or ſugar, or ſulphur pounded, *&c.* there are certain conditions to be obſerved.

1ſt, That of the bodies rubbed againſt each other, one of them at leaſt muſt be tranſparent, that the light may be ſeen through while it laſts, which uſually is during the time of friction.

2dly, The ſurface of the two bodies muſt be plain, ſmooth, and clean, that the contact may be the more immediate.

3dly, The two bodies muſt both be hard.

4thly, A great denſity, without a great degree of hardneſs, will alſo have its effect. Thus M. *Bernoulli* procures light, by rubbing an amalgama of mercury and tin upon a looking-glaſs.

5thly, One of the two bodies muſt be as thin as poſſible, that it may be the eaſier heated by friction, and may yield a quicker, as well as brisker, light. This M. *Bernoulli* tried on little copper-plates of different thickneſſes.

6thly, Gold rubbed upon glaſs, appeared the fitteſt of all metals to afford light; but no body yields ſo exquiſite a light as a diamond, which comes nothing behind that of a live coal, briskly blown by the bellows; nor is it any matter how thick the diamond is.

Hence M. *Bernoulli* concludes, that Mr. *Boyle*, notwithſtanding all his skill in experimental philoſophy, held a thing to be a kind of prodigy which was none, *viz.* a diamond of his, which yielded light when rubbed in the dark, to which he gave the magnificent appellation of *adamas lucidus*, yet had not this any particular priviledge, unleſs that its brightneſs continued a few moments after the friction, which we may add, was the foundation of part of Mr. *Boyle*'s eſteem for it.

On occaſion of theſe experiments of M. *Bernoulli*, M. *Caſſini* the younger made others on the ſame head to the effect following.

1ſt, A diamond, cut table-ways, being rubbed on a looking-glaſs, yielded a light almoſt equal to that of a live coal, and which even appeared larger than the face of the diamond.

2dly,

2dly, A diamond, cut facet-ways, yielded a leſs vivid light.

3dly, A crown, and ſome other pieces of ſilver, yielded leſs light than the facet-diamond.

4thly, A copper double and a ſol yielded very little.

All the bodies, in the fore-mentioned experiments, were rubbed upon glaſs.

5thly, The table-diamond, when rubbed on a plate of ſilver, yielded light.

II. *On fire-arms differently charged; tranſlated by Mr.* Chambers.

M. *Carré* informing the academy of ſome experiments made by a friend upon fire-arms charged in different manners, it was thought proper to verify them; which M. *Caſſini* the younger, accordingly undertook.

He made a kind of machine, wherein was a piece of wood armed at one end, with a pretty thick plate of talc, whereon the ſeveral ſhots were to be received. This plate was made moveable, ſo as to give way, more or leſs, according as a greater or leſs impulſe was made on it; and at the ſame time ſhew, by the ſtructure of the machine, how much it had given way.

Now from the experiments made by M. *Caſſini*, it appears, 1ſt, That the putting a wadding between the powder and ball, renders the effort the greater: the reaſon is evident, and accordingly we find it the common practice.

2dly, That *cæteris paribus*, thoſe balls which fit exactly the bore of the piece have the greateſt effect, by reaſon, doubtleſs, that they do not come out ſo readily, but give time for a greater quantity of powder to take fire.

3dly, That when the powder is rammed violently down, the effort is no greater, but rather ſomewhat leſs, than when barely preſſed down.

4thly, That gun-powder caſt upon the ball, diminiſhes its effect: the reaſon may be, that the powder making its effort every way, that which is upon the ball muſt needs give ſome oppoſition thereto, by acting counter to the motion which ſhould bring it forth.

5thly, That this powder, though it diminiſh the effect of the ball, increaſes the noiſe.

6thly, That the fire of the powder under the ball, communicates with that over it, even though the ball be exactly fitted to the bore, and lodged between two waddings: this appears from the great increaſe of noiſe.

7thly, That taking a ball ſomewhat leſs than the bore, and putting but little powder under it, and a good deal over it, one may ſhoot with a very great noiſe, but no ſenſible effect. They who have purchaſed ſecrets for becoming invulnerable, and have been ſo cautious as to make trials thereof, have doubtleſs been impoſed on by this artifice.

III. *Upon ſtones, and particularly thoſe of the ſea; tranſlated by Mr.* Chambers.

M. *Saulmon* making a tour about the coaſt of *Normandy* and *Picardy*, and the country adjoining, had occaſion to make ſome phyſical reflections which he communicated to the academy.

The *Galets* are a kind of pebbles, commonly flat, round, and always very ſmooth and poliſhed, driven by the ſea upon thoſe coaſts. It is eaſy to conceive, that their figure and poliſh had aroſe from their being long beaten and toſt by the waves, and

and rubbed one againſt another; but there are ſtore of them likewiſe found out at land. M. *Saulmon* learned, that when they dig their cellars at *Caïeux*, abundance of theſe *galets* tumble in; and that at *Brutel*, which is a league from the ſea, the ſame thing had befallen: upon digging a well he further obſerved, that the mountains of *Bonnueil*, *Broye*, and *Queſnoy*, which are eighteen leagues from the ſea, are all covered with *galets*, which he alſo found in the valley of *Clermont*, in the *Beauvaiſis*; but obſerved, that there were none on the top of the mountain, which is very high.

Among the *galets* out at land, there are ſeveral whoſe ſurface is very rough and irregular, beſet with points; and what is more, this ſurface is a kind of bark, or rind, different from the reſt of their ſubſtance; yet this ſeems to be their natural ſtate: for no external cauſe can ever have inveſted them with this rind, but may, on the contrary, have ſtripped them of it; and ſuch cauſe may be a long and violent friction. Add, That it is highly probable they are of the ſame ſpecies with pebbles, which have a like rind, conſiderably thick, and of a chalky conſiſtance.

M. *Saulmon* makes no doubt, but that all theſe lands were formerly covered by the ſea; a notion which had already been ſtarted in the hiſtory of 1706, with ſome of the arguments which ſeem to prove it. To render it ſtill more probable, at leaſt with regard to the country where M. *Saulmon* made his obſervations, he endeavours to ſhew by the diſpoſition of the place, that when the ſea did cover it, the currents formed between the mountains with the ſeveral eddies of water, muſt neceſſarily have thrown the greateſt or leaſt *galets* into the places where they are actually found; for it is obſervable, that the greater and ſmaller kinds are not uſually intermixed, and diſtributed ſome on the one ſide, and ſome on the other. It is evident upon M. *Saulmon*'s ſuppoſition, that the mountain, whoſe top was free of *galets*, had roſe above the ſea, and conſequently could not receive the driving ſtones upon its top; but to determine by the laws of motion of bodies circulating in, and with a fluid the ſeveral diſtributions of *galets* that muſt have been made in ſeveral places, would be both a topography and a phyſiology of ſo nice a kind, that we think it ought not to be attempted. We ſhall only relate two obſervations after M. *Saulmon*.

The 1ſt, That a hole 16 feet deep, being dug horizontally in the beach of *Treſport*, which is all ſtony, diſappeared in 30 years time; that the ſea had eat this thickneſs of 16 feet into the beach in that time. Now ſuppoſing, that it always gained at that rate, it would dig 1000 fathoms, or ½ a league of ſtone, in 12000 years; and it is evident from hiſtory, that the ſea has really advanced, or withdrawn, in a multitude of places; and that it has a general, though a very ſlow motion, whereby it changes its bounds.

2dly, Flints have not only a chalky rind, but their black and hard ſubſtance, which is properly the flint, may be ſuppoſed to have originally been no other than cha k, which had hardened by degrees, and changed its colour. M. *Saulmon* produced flints of different ages, ſome whereof had a greater or leſs quantity of chalk ſtill remaining in their centre, while others had chalky veins diſperſed through their black ſubſtance, and carried all the indications of their having arrived at their blackneſs and hardneſs by length of time. He even conjectures, that the flints, when too old, turn rotten; and that it is ſuch as theſe we find

with

with their black ſubſtance turned reddiſh, leſs firm, and as it were ruſty.———All which ſeems to tally with the ſyſtem of ſtones ariſing from ſeeds.

IV. *Of an extraordinary cure performed by a concert of muſick.*

An eminent muſician, who was a great compoſer, was ſeized with a fever, which ſtill increaſing, became continued. On the 7th day he fell into a violent delirium, having hardly any intermiſſion, attended with cries, tears, terrors, and a perpetual want of ſleep. The 3d day of this delirium, one of thoſe natural inſtincts which is ſaid to make animals ſeek for thoſe herbs which are proper for them, made him deſire to hear a little concert in his chamber; it was with much difficulty that the phyſician conſented to it. They played to him the cantata's of M. *Bernier*. From his firſt hearing them tune their inſtruments, his face aſſumed a ſerene air, his eyes were compoſed, the convulſions entirely ceaſed, he ſhed tears of pleaſure, and his ſenſes were affected with the muſick in ſuch a manner, as he never felt before nor ſince the cure; his fever ceaſed during the whole concert; but as ſoon as it was ended, he relapſed. They did not neglect to continue the uſe of this unexpectedly ſucceſsful medicine; the fever, and delirium always ſuſpended during the concert and muſick was become ſo neceſſary to him, that in the night he made a relation who watched with him, both ſing and dance, though her affliction made it difficult for her to comply with him. One night among the reſt, when none but his nurſe was with him, who could only ſing one miſerable ballad, he was forced to be content, and even re-

received ſome benefit from it. In ſhort, 10 days muſick quite cured him, without any other aſſiſtance, except bleeding in the foot, which was the ſecond time this had been done, and was ſucceeded with a great evacuation. M. *Dodart* related this hiſtory; which he had well atteſted: he does not pretend, that this ought to ſerve as an example or a rule, but it is pretty curious to ſee in a man, whoſe very ſoul, if I may ſo ſay, was become harmony, by a long continued cuſtom, how concerts by degrees reſtored his ſpirits to their natural courſe. It is not likely that a painter would have been cured thus by pictures; paintings have not ſo great an influence as muſick over the motions of the ſpirits, and no art in this reſpect can equal it.

V. *Of the multiplication of animalcules.*

A philoſopher, friend to M. *Carré*, who has been frequently mentioned in the preceding hiſtories, imagined from ſome experiments which he had made, that animalcules ſeen in the water with a microſcope do not multiply therein, but proceed from little inviſible flies, which lay their eggs in the air. And indeed, as theſe animals are a kind of little worms, it is natural enough, that, like many other worms, they ſhould all proceed from ſome of the winged ſpecies; but the obſerver was convinced of his miſtake. He boiled water and dung, and filled therewith two phials of equal ſize; when they were lukewarm, he put into one of theſe phials, two little drops of water taken out of a veſſel, wherein the water was full of theſe little animalcules; and 8 days after, he found this phial filled with an innumerable quantity of animalcules, of the ſame ſpecies with thoſe which were

were in the drops of water. There were none to be perceived in the other phial, though the dung might probably have produced ſome. They had both been ſtopped very cloſe. The multiplication of animalcules in water, is therefore hereby ſettled; but more ſo, if it is certain, that this philoſopher ſaw them couple; at leaſt, it is certain, that he ſaw them joined two and two. Perhaps this was to fight; but, do they always fight by pairs?

VI. *Of the circulation of blood in inſects.*

M. *Lewenhoeck* ſays, That he could not obſerve the circulation of blood in inſects, and therefore imagines another way, by which he believes their life is maintained. But the philoſopher, whom we juſt now mentioned, who is well ſkilled in the uſe of the miſcroſcope, ſays, That he has diſtinctly ſeen the circulation in the leg of a ſpider.

VII. *Of worms voided by ſtool.*

M. *Homberg* ſays, That a young man of his acquaintance, who is in good health, has during theſe 4 or 5 years, voided every day by ſtool, a great number of worms, about 5 or 6 lines long, though he eats neither fruits or ſallads, and has made uſe of all known remedies. He once or twice voided above an ell and a half of a flat worm, divided by joints, called the *ſolium*. It is hereby ſeen, how many eggs of inſects there muſt be in all that food which we leaſt ſuſpect to contain any, which want nothing but the ſtomach; or, as I may call it, an oven fit to hatch them.

VIII.

VIII. *Of the* Iguana, *an* American *lizard.*

The *iguana* is a kind of lizard found through all parts of *America*, it is defcribed in *Pifo*'s book, *De utriufque Indiæ re naturali & medica.* It is amphibious, it has two ftomachs, in one of which there is often a ftone, white on the outfide, and the infide very much like the colour of the *American* bezoar. It has the virtue of expelling the ftone and gravel in the kidneys, and cures the fuppreffion of urine. It is adminiftred in very fine powder, with an equal quantity of the powder of nut-fhells, both together weighing a dram, in orange-flower water, if there is no fever, or fufpicion of an inflammation in the ureters, or in the bladder; in which cafe it muft be given in white wine, mixed with parfley water, or pellitory of the wall, or fome other diuretick. It fometimes has effect in an hour's time, but at moft, in three hours. A *Spanifh* phyfician of *Caraccas*, having fent this account to M. *DePas*, a phyfician of *Montpellier*, who is with M. *DesLandes*, director of the *Affiento* company in *America*, and having related to him many experiments which he had made with the ftone of the *iguana*, this letter has been fent to the academy.

IX. *Of the difference of the milk of* European *women, and* Negreffes *at* Batavia.

M. *Homberg* fays, that *European* women who go to *Batavia*, cannot fuckle their children, their milk being fo falt that they will not take it; whereas, the milk of the *Negreffes*, though their diet is the fame, is fweet and pleafant as ufual; therefore they fuckle the children of the *Dutch*

 and

and *English*. He himſelf, who was born at *Batavia*, was ſuckled by a *black*. He thinks it probable, that when the *Europeans* are carried into ſo hot a climate, being not made for it, thoſe veſſels which in them are deſigned to filtrate the milk, dilate too much, and give paſſage to thoſe ſalts which are not intended to enter into the compoſition of this liquor; but that the women of theſe hot countries are, by their firſt formation, fitted to generate good milk; that is, either that the filtring veſſels are naturally leſs, and do not afterwards dilate more than is neceſſary, or are of a firmer texture, and leſs capable of dilatation; or ſomething, in ſhort, equivalent to this.

X. *Of an* aurora borealis *ſeen at* Berlin.

M. *Leibnitz* ſent an account from *Berlin*, to M. *l'Abbé Bignon*, that *March* 6, between 7 and 10 in the evening, there was ſeen in this city, and in the neighbouring country, an aurora borealis, which was ſomething like that mentioned by M. *Gaſſendi*, in the life of M. *Piereſc*. There were two luminous arches, one of which was higher than the other; both directly northward; their concavity turned downward; their chords parallel with the horizon. The ſuperior arch was interrupted; ſtreams of light went from the one to the other, which juſt appeared, and vaniſhed away.

XI. *Of a new iſland near* Santerini.

M. *De la Lanne*, conſul in *Candia*, ſent word to the conſul of *Tunis*, that two miles from the iſland of *Santerini*, which is ſeventy miles from *Candia*, a new iſland was perceived; which, at firſt ap-

appeared only like a little veſſel, but increaſing daily, it became as big as a large ſhip. It is ſurrounded with many other little iſlands; and there continually proceed great flames from it. This novelty is the more ſurpriſing, the water being in this place more than 60 braces deep; the ſubterraneous fires muſt therefore have ſtrange force to throw up ſuch a great heap of ſtones ſo high, through the ſea. As in ſome parts of *Santerini*, and of ſome other iſlands of the *Archipelago*, the ſoil wholly conſiſts of pumice ſtone, it is very likely, that theſe new iſlands are formed of theſe light ſtones. M. *de Chaſtueil Gallaup*, a gentleman of *Provence*, of great erudition and merit, has done me the honour to communicate this to me, of which he was informed by a letter from *Tunis*, which letter ſaid, this account was confirmed by the captain and ſailors of a ſhip newly arrived from the *Levant* to *Suſa*, in the kingdom of *Tunis*, who were all eye-witneſſes of the truth of what M. *De la Lanne* had written.

XII. *Of a new way of conſtructing the map of a country.*

The great expence which attends the conſtructing the map of a country geometrically, the length of time which it requires, and the ſmall number of thoſe who are capable, or who will take the pains to execute this work, are the reaſons why few maps are conſtructed geometrically; yet no others are abſolutely certain. Provided ſuch cannot be had, M. *Chevalier* propoſes another method, which is not far ſhort of the geometrical exactneſs; yet may be put in practice without any geometry, requiring only care and attention.

The

The arch of the horizon taken between the point where the ſun riſes or ſets, at any day whatſoever, and the point where it riſes or ſets, when it is in the equator, is called the *amplitude*. It is then, at firſt view, that the amplitude is greater in proportion to the diſtance of the ſun from the equator, or has a greater declination; and it is alſo ſeen from the different poſitions of the ſphere, that the more oblique it is, or the higher the pole is elevated for any place, the greater is the amplitude, all the reſt being equal. The declination of the ſun, and the elevation of the pole, are therefore the two elements whereon the ſize of the amplitude depends, and tables of the variation of the amplitudes are conſtructed according to that of their elements.

I ſuppoſe the place where I am, for example *Paris*, to be in the centre of a pretty large circle deſcribed on a paper, and divided into 360. As I know by the tables, that the ſolſtitial amplitude, the greateſt of all at *Paris*, is 37 degrees omitting the minutes, I take on my circle for the equinoctial amplitude, or 0, the point where its diviſions begin, and the 37 degrees following anſwers to the ſolſtitial amplitude. This ſpace of 37 degrees anſwers to three months, and I divide it according to the table of amplitudes, to each day of 3 months, or rather from 5 to 5 days, becauſe the amplitudes have not any ſenſible alteration from one day to another. I do the ſame for the amplitudes of the other 9 months of the year.

I alſo ſuppoſe that the *radius* of my circle repreſents an extent of two leagues, and I divide it into 8 equal parts, which are conſequently of a quarter of a league each, and through each of theſe diviſions I deſcribe circles concentrical with the

the firſt. M. *Chevalier* calls the papers, whereon theſe figures are, *chaſſis*, or *frames*.

This done, on any day whereon the riſing or the ſetting of the ſun can be obſerved, I have two wires on the frame directly perpendicular, one at the centre, the other at the outward circle, which anſwers to the day pitched upon, I place the frame exactly horizontally, and turn it in ſuch a manner, that at the moment of the ſun's riſing or ſetting, the ſhadow of the two wires is upon the ſame right line, and I fix it faſt in this ſituation. It is certain, that all the diviſions of the outward circle will anſwer exactly to thoſe of the horizon, that the 90th degree, for example, after an equinoctial amplitude, is a pole, *&c.* in a word, that the frame is well rectified. Then if I am in a place high enough to ſurvey an extent of two leagues round, I direct a rule, which can be moved round the centre, exactly to a ſteeple, at what place I pleaſe; and I am certain, that this ſteeple is in reſpect to *Paris*, in the poſition determined by the rule, to the ſouth-eaſt, for example, and conſequently it muſt be deſcribed in my frame on this line. It remains to be known at what point; now it is ſuppoſed, that I know pretty well the diſtance of all thoſe places which are within two leagues of the place were I dwell, and this knowledge is more familiar in the country, where the frame will be moſt in uſe. As it is divided into quarters of leagues, I place the ſteeple according to its known diſtance, either upon one of the concentrical circles, or between two circles, and cannot fall into any conſiderable error therein.

What I have done in relation to *Paris*, M. *Chevalier* would have 30 or 40 perſons do round about *Paris*, at 2 leagues diſtance at moſt from one

one another, each of them in relation to the place where they dwell; not that each ſhould be obliged to make a frame, that work requiring the hand of a geometrician, but they being once made by an artiſt, copies may be ſent to 30 or 40 perſons, who will then have only the trouble of laying out the lines of neighbouring places, as has been ſaid, and few are incapable of doing this. The 30 or 40 little maps being made, they muſt be returned to the geometrician, who underſtands how to put them together, and thereby compoſe a map of the country round *Paris*. As the ſame frame is to be ſent to all thoſe deſigned to be employed, it is ſuppoſed, that the amplitudes are the ſame, as to places which are but little diſtant, which is only ſenſibly true. Nor can this method of conſtructing maps be uſeful, except as to a little tract of land; and it is proper, that the city, or principal place, on which alone the amplitudes are regulated, ſhould be in the middle of that tract of land which is to be deſcribed, that ſmall errors of particular places may compenſate for one another.

It ſhould ſeem, that without making uſe of amplitudes, the frame might be rectified by means of the meridian of the place, which is commonly known in the country; but it is only known in a groſs manner, and if it was neceſſary to find it more exactly, few would ſucceed in it. The method of rectifying by amplitudes, when the frame is quite finiſhed, is more certain, and has no difficulty in it. Not but the other may alſo be uſed with ſucceſs.

It may be obſerved, in the method of the amplitudes, that an error, which may be imperceivable in a little tract of land, will be leſs ſo if the work is performed in a tract of leſs latitude,

 or

or in a time nearer to the equinoxes, for in these two circumſtances, there is leſs difference in the amplitudes of different places. The latitude is the circumſtance which makes leaſt difference in them; and as in *France* it is pretty extenſive, the operations near the equinoctial ought ſo much the more carefully to be obſerved there.

To have given here in general the ſpirit of M. *Chevalier*'s method is ſufficient. As a geometrician muſt of neceſſity be at the head of the work, he will eaſily imagine what alterations certain particular circumſtances require, and facilities which may be contrived for the operators. A biſhop, who has a genius for the ſciences, may in this manner conſtruct a map of his country, by help of his clergy, who will hardly be ſenſible themſelves, that they are making geometrical operations. Many uſeful things, and ſome which appear difficult, would almoſt execute themſelves, if they who are in place, would give a firſt motion to them.

AN

ABRIDGMENT

OF THE

Philoſophical Memoirs

OF THE

ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1707.

I. *Obſervations on the quantity of rain, which fell at the obſervatory during the year* 1706, *and on the thermometer and barometer, by M.* de la Hire *.

THE obſervation which I have long made on the quantity of water which falls on the earth during each year, the reſult of which I give in the memoirs of the academy, at the beginning of the ſucceeding year, have excited many curious perſons, in different parts of the kingdom, to do the ſame in the places where they dwell. Some of theſe obſervations have already been given in our memoirs, and have been compared with thoſe made at *Paris*; but the moſt conſiderable is, that made by the marſhal *de Vauban* at *Liſle*, in *Flanders*, during ten years ſucceſſively, which I related ſome time ago, and from thence concluded, that there is a little more rain in *Flanders* than at *Paris*.

* Jan. 8, 1707.

Here is the continuation of thefe obfervations, which were made here during the preceding year, in all the fame circumftances, and in the fame manner as thofe of the foregoing years. The height of water which fell at the obfervatory, was in

	Lines.		*Lines.*
Jan.	$8\frac{1}{4}$	July	13
Feb.	$15\frac{1}{4}\frac{1}{8}$	Aug.	$5\frac{1}{4}\frac{1}{8}$
March	$3\frac{1}{2}\frac{1}{8}$	Sept.	$18\frac{1}{4}\frac{1}{8}$
April	$7\frac{1}{2}$	Octob.	$19\frac{1}{4}$
May	$23\frac{1}{2}$	Nov.	17
June	$21\frac{1}{2}$	Dec.	$30\frac{1}{4}\frac{1}{8}$

Quantity of water in the whole year 183 lines $\frac{1}{2}\frac{1}{8}$, or 15 inches, 3 lines $\frac{5}{8}$.

This has been a very dry year, if the quantity of water, which has fallen, is in general confidered, which commonly ufed to be between 19 and 20 inches: but it muft be looked upon as one of the wetteft years, if it is confidered that the greateft rains commonly happen in the months of *July* and *Auguft*, with ftorms, and that this year it did not rain in both thefe months together much more than 18 lines.

Dry years are always advantageous to the corn in this country, the greateft part of the land being moift and cold; for the weeds do not then grow, or turn the corn up.

As to the heat, I compute it by the thermometer, called the *Florence* thermometer, which is fixed in a place expofed to the air, but fhaded from the fun. It is at the 48th degree of its divifion in the bottom of the caves of the obfervatory, where I fuppofe the air to be in a mean ftate of heat, and it begins to freeze when the liquor in the tube falls to 32 degrees. The loweft which the thermometer fell at the beginning of this year, was to 20 ½ degrees *Jan.* 21; but it almoſt immediately roſe again to 30 degrees, and the froſt was but inconſiderable, and of little continuance; and in the firſt 8 days of *February*, when the cold is commonly moſt ſevere, the thermometer always ſtood at about 30 degrees. The 9th of this month, it was at 45 degrees, which is almoſt its mean ſtate: the remainder of the month it was always near 30 degrees, which indicates a little froſt. As to cold at the end of the year, it was inconſiderable, for it only froze *Dec.* 21, the thermometer then falling to 28 ½. There fell only a little ſnow *Feb.* 4.

Though the cold was not great, nor of long continuance, the heat on the contrary was very conſiderable, and laſted long; for the thermometer almoſt always ſtood at near 60 degrees in the 3 months of *June*, *July*, and *Aug.* The hotteſt day was *Aug.* 8, wherein the thermometer was at 68 degrees about the ſun's riſing, which is the time when I always obſerve it, and wherein the air is the cooleſt of the day. This very day, at 2 in the afternoon, which is the time when the air is hotteſt, the thermometer roſe to near 82 degrees; whence the heat is known to have been very great, ſince the thermometer roſe to 34 degrees above the mean ſtate; and had it fallen as much below it in the winter, it would have come to 14 degrees, which commonly indicates the greateſt cold that we ever ſuffer in this country.

In theſe ſort of obſervations regard ought to be had to the wind, which partly cauſes heat and cold, therefore I alſo give much attention to that. In the month of *Jan.* the wind was always eaſterly, inclining ſometimes to the ſouth, and ſometimes to the north. At the beginning of *Feb.* it was weſterly; and towards the end of the month northerly. In *March*, it was pretty changeable, chiefly in the weſt, but little in the eaſt, and paſſing by the north. In *April*, at the beginning, towards the north-eaſt; and at the end, in the weſt. In *May*, the weſt wind prevailed. In *June*, it was almoſt always near the ſouth and weſt. In *July*, at the beginning and end, near the weſt; and in the middle near the north. In *Aug.* it was almoſt always weſt, inclining a little to the north, and very often to the ſouth; which contributed much to the great heat. In *Sept.* almoſt always ſouth-weſt. At the beginning of *Oct.* alſo ſouth-weſt; and at the end, near ſouth-eaſt. In *Nov.* the wind was almoſt always ſouth, and a little thereabouts; but chiefly weſtward. In *Dec.* almoſt always ſouth and ſouth-weſt.

The prevailing wind this year, was ſouth-weſt, as it is commonly in this country, becauſe of the neighbourhood of the ſea; but this ſouth-weſt wind has always been very violent.

There were ſome ſtorms this ſummer; but the moſt conſiderable happened *July* 27, in the morning, with thunder; which did much damage in many places.

The barometer which ſerves me for the weight of the air, is always placed at the top of the great hall of the obſervatory. *March* 10, the quick-ſilver roſe to 28 inches, 1 line $\frac{1}{2}$. *Dec.* 22, it fell to 26 inches, 9 lines: the difference between theſe 2 heights was therefore 1 inch, 4 lines $\frac{1}{2}$, which is pretty near as uſual; but it ſeldom falls ſo low, except with a very high ſoutherly wind of long continuance, as it was then. I have frequently obſerved, that the quick-ſilver has been very high, though

though the wind has been ſoutherly, which is contrary to the common rule.

The tube of the barometer which I always make uſe of, is very ſlender and long; and I ſuſpect that there is a little air therein, which I cannot get out; for I have another, whoſe tube is of a middling ſize, wherein the quickſilver always ſtands more than three lines higher. Light is ſeen in the vacuum of theſe barometers when the quickſilver is agitated; and one of them is that wherein M. *Picard*, of the academy, who was the firſt that obſerved it, made his firſt obſervation on the light in the vacuum of barometers. We have alſo other barometers, conſtructed in a manner different from the common ones; in which, even air has been ſuffered to enter, yet they alſo give a light.

I alſo obſerved, *Dec.* 31, of this year, 1706, the declination of the needle, 9 degrees, 48 minutes, weſtward, with the ſame needle of 8 inches, length, and in the ſame place where I uſed to obſerve it every year, as I have related in preceding years.

II. *A machine to retain the wheel, which ſerves to raiſe the rammer to drive the piles in the conſtruction of bridges, kays, and other works of this nature, by M.* de la Hire*.

The rammer, or beetle, which is uſed to drive great piles, is of 1000 or 2000 *lb.* weight; and is commonly raiſed by a roller, which compoſes a part of the crane or engine, which is to raiſe great weights.

* June 1, 1707.

This rammer runs freely between two grooves, that the whole force thereof may fall on the head of the pile which is to be driven. But as the common rollers of engines are moved by 4 arms fixed to them it is difficult and tedious to turn them, which hinders the work; therefore a great wheel is fixed to this roller of 10 or 12 feet in diameter, as there is to great cranes, that men by walking or climbing in this wheel, may cauſe the roller to turn more eaſily and conveniently, as may be ſeen in the figure*.

In conſtructing a great ſtone bridge at *Moulins*, in *Bourbon*, after a new manner, by the direction and plan of M. *Manſart*, ſurveyor of the buildings, they are obliged to ſink great piles, more than 20 feet deep, to get a good foundation; it is therefore neceſſary to make uſe of a rammer of 2000 *lb.* weight. But as the great wheel which is apply'd to the roller, on which the rope of the rammer winds in proportion as it is raiſed, is large enough to receive in it 4 men on a row, who climb up together on the croſs bars, or rounds, which form the breadth of this wheel, and almoſt always to keep them in at the heighth of the axis or roller, to make the greater effort, this wheel muſt be ſtopped every time the rammer goes down; for the weight of the men within it being no longer ſtopped by the weight of the rammer, would carry the wheel ſwiftly round; and the men therein might be thrown down, and perhaps killed; therefore they are obliged to ſtop this wheel with a hook faſtened to a rope, and fixed to ſome appointed place every time the rammer is let down. This is very troubleſome; beſides, it may happen, that the hook or rope may break by the force of the men's weight on the circum-

* Plate I. Fig. 1.

ference

ference of the wheel, and then the labourers run the riſque of their lives.

But the men in the machine have not only this to fear; for ſometimes the deſcent of the rammer, or the hook, by which it is ſupported, or the rope which is made uſe of, may break on a ſudden in raiſing it; and by theſe unforeſeen accidents the labourers are in great danger, as has ſometimes happened.

This obliged one of the king's chief architects, who has the direction of this edifice, to propoſe to me laſt winter, that I ſhould find out ſome remedy for all theſe inconveniencies, and make it ſo eaſy, that corpulent men, who are moſt commonly employed in theſe works, may receive no hurt by any negligence, or inadvertency whatſoever: This is what I thereupon contrived, and which is to be executed.

I firſt conſidered, that in all accidents which may happen to this machine, the rope which holds up the rammer, entirely ſlackens; and conſequently, a piece muſt be fixed in the timber work of the machine itſelf, which falling between the ſteps or rounds of the wheel, and being capable of reſiſting any effort whatever, may retain it when the rope of the rammer is ſlack; and on the contrary, this piece muſt diſengage itſelf from thence when the rope is tight.

For this purpoſe I made a ſquare A B C * of either wood or iron, much wider than it is thick, forked near the end C, which is a little bent. In this forked part I fixed a little roller or pully, ſo that the cable or rope of the rammer, may move freely in this part, paſſing under the pulley. To the other branch of the ſquare AB, near the

* Fig. 2.

angle

angle B, I fix a ſtrong pin D, or a bracket of the ſame thickneſs as the branch of the ſquare. To conclude, in the end A of this branch BA, a hole is bored that an iron pin may be put therein.

In the compoſing this engine, there are two pieces EF * mortiſed parallel to each other, which help to ſtrengthen it; there is a ſpace of 4 or 5 inches between them, in which I faſten the ſquare ABC, and fix it to the mortiſes by a pin placed at the end A, but in ſuch a manner that it has not too much room to play on the ſides, which depends on the diſtance between the mortiſes, and on the thickneſs of the branch of the ſquare.

Directly under theſe mortiſed pieces goes the great wheel of the roller, which carries the rope of the rammer; and the machine is diſpoſed in ſuch a manner, that when the cable GH, which comes from the top of the engine to the pully H, to be afterwards turned on the roller, is tight or ſtrained; it holds up the ſquare, paſſing through the pully at C, ſo that the bracket of the ſquare D, does not touch the rounds of the wheel. But as ſoon as the rope GH ſlackens, the weight of the ſquare itſelf, and the weight which the rope adds to it, by reſting on the forked part, makes it turn upon the pin at A, and fall into the fellows of the great wheel; and the bracket D falling immediately between the ſteps or rounds, retains the wheel in this ſtate, it being impoſſible for it to turn; for the branch AB of the ſquare, being faſtened between the mortiſed pieces, can bear a very great effort.

But when the cable or rope of the roller is again ſtrained, to fix the rammer to it, the ſquare immediately riſes, and the bracket D diſengages it-

* Fig. 3.

ſelf

ſelf from the rounds of the wheel, which is then at liberty to turn round and raiſe the rammer.

This machine is very ſimple and convenient, and may ſave the lives of labourers and workmen employed in this buſineſs, and without any precaution.

III. *Of the irregularities of the apparent depreſſion of the horizon of the ſea, by* M. Caſſini *; *tranſlated by Mr.* Chambers.

After examining the 1ſt obſervations of the apparent depreſſion of the ſenſible horizon of the ſea, made by father *Laval*, in his obſervatory at *Marſeilles*, finding them different at different times, I deſired him to continue his obſervations, to ſee whether this difference would ſtill continue equally irregular.

The teleſcope of the inſtrument which he uſes, is raiſed 144 *Paris* feet above the level of the ſea, according to a levelling made by himſelf; which 144 feet high give the direct ray, which razes the ſurface of the ſea an inclination of 13′ 14″.

The leaſt apparent depreſſion obſerved by father *Laval*, at this heighth during this winter, was 11′ 46″; and the difference between this height, and that of the direct ray, would be 1′ 28″; which might be owing to the greateſt refraction of the viſual ray, which razed the ſurface of the ſea. — But the greateſt apparent depreſſion obſerved by him was 14′ 30″, which exceeds that of the direct ray by 1′ 16″; and this contrary to the rules of refraction, which ſhould diminiſh this inclination inſtead of increaſing it.

* June 28, 1707.

We

We have already noted from ſeveral other obſervations, that a part of the ſurface of the ſea, contiguous to the ſenſible horizon, confounds itſelf as to ſight, with the heaven itſelf; and that on this occaſion, the apparent circumference of the ſenſible horizon falls in the ſea within our ſight. The viſual ray directed to this apparent circumference of the horizon of the ſea declines; therefore on this occaſion, from the direct ray which razes the ſurface of the ſea, towards the lower ſide, contrary to the inclination which the refracted ray raiſing this ſurface ought to have.

Having communicated this remark to father *Laval*, and he not having occaſion to diſtinguiſh this difference by any ſenſible ſign, it is evident how difficult it is to diſtinguiſh it; and how liable to error the method is of finding the magnitude of the diameter of the earth, by obſerving the tangent of the ſea without this circumſpection.

It appears by father *Laval*'s obſervations, that this difference between the ſeveral apparent depreſſions of the horizon of the ſea, viewed from the ſame place, does frequently exceed a 5th part of the leaſt apparent inclination; ſo that one might be deceived in this method by a 5th part of the ſemi-diameter of the earth.

I have endeavoured to reduce the difference between the apparent inclination of the refracted ray, which razes the ſurface of the ſea, and the real inclination of the direct ray, to certain rules; and it is evidently of great importance to examine, what degree of exactneſs a method is capable of, to prevent any expectation of more than it can afford.

By the multitude of obſervations made by father *Laval*, we learn, 1ſt, That when we attempt to determine a diſtance, or a ſmall height, upon the ſurface of the ſea, by a ſingle obſervation of the depreſſion

depreſſion of the horizon, we can only be ſure of being within $\frac{1}{5}$ of the truth; and accordingly this is pretty nearly the difference found between the height of the obſervatory at *Marſeilles*, as taken by obſervations made at *Marſeilles*, and the real height found by levelling, the former being 175 feet, and the latter 144. 2dly, That having ſeveral obſervations of the apparent depreſſion of the ſea, made in the ſame place at different times; and taking a *medium* between theſe obſervations, we ſhall have the inclination nearly equal to that of the direct ray, which raiſes the ſurface of the ſea, which may ſerve to determine the height and diſtance, by the common method, to a tolerable exactneſs. 3dly, That the variation of the apparent heights of the ſea bears no uniform relation to the variation obſerved at the ſame time in the barometer and thermometer, which ſeems to confirm what we have frequently obſerved, that the air which cauſes the refraction, is of a different nature from that which balances the weight of liquors in vacuo.

We have frequently obſerved the apparent depreſſion of the ſenſible horizon of the *Mediterranean* ſea, from an elevation 6 times greater than that of the obſervatory at *Marſeilles*, and conſtantly found it 42 min. without any ſenſible difference between one time and another, which ſhews, that the refraction is much more variable at moderate heights, than at very large ones.

IV. *Obſervations upon ſpiders, by M.* Homberg; *tranſlated by Mr.* Chambers.

The colour and figure of an extraordinary kind of ſpider, which I met withal, among the tuberoſes in a garden at *Toulon*, raiſed my curioſity to ex-

examine this; and afterwards all the other kinds of ſpiders I could find in. I made uſe of a microſcope for the diſcovery of certain parts which the naked eye cannot diſtinguiſh, and have procured deſigns of them larger than the life, to repreſent them ſuch as they appeared thro' the microſcope.

I ſhall here only give the deſcription of 6 principal kinds of theſe inſects; to which, all the reſt I have met withal, may be referred.

The 6 kinds are, 1ſt, the domeſtick ſpider; or that which makes its webb on the walls, and in the corners of rooms. 2dly, The garden ſpider; or, that which makes its webb out of doors, uſually of a roundiſh figure, and a looſe texture, in the centre whereof the animal lodges all day. 3dly, The black ſpider; found in caves, cellars, and holes of old walls. 4thly, The wandring ſpider, which does not lie ſtill in its neſt like the other kinds. 5thly, The field ſpider with long legs, uſually called the ſpinner. And 6thly, The raging ſpider, or the famous tarrantula.

By the way it may be proper to begin with a general deſcription agreeing to all the kinds of ſpiders; and afterwards to note the particular characters of each: nor ſhall I enter into a minute account of the ſtructure of all the external parts of this inſect, but confine myſelf to what is not eaſily diſcoverable by ſimple inſpection, and without the help of a microſcope.

The whole body of the ſpider may be divided into the anterior part, the poſterior part, and the paws or legs; the anterior part contains the thorax and head; the poſterior, the belly: theſe two parts are faſtened together by a choak, or very narrow rim. In the generality of ſpiders, the anterior part is covered with a hard ſcaly cruſt, and the posterior, with a soft skin; the legs arise from the *thorax*, and are hard like the rest of it. This structure is very different from that of diverse creeping and flying insects; for instance, the maids*, and others, whose belly and *thorax* adhere to each other by their whole extent without any choak, or contraction, tho' their *thorax* be invested with a hard crust, and the belly with a soft skin, yet their head adheres to their *thorax* by a very narrow choak; again ants, wasps, and most flies have their *thorax* and belly fastened by a choak, and their head and *thorax* by another.

All spiders are covered with hair, their hard, as well as their soft parts.

They have eyes on various parts of their heads, of different size and number, and differently placed; but all of them without *Palpebræ*, or eye-lids; and covered with a hard glossy transparent crust.

In the fore-part of the head is a kind of double claw, or gripe, like that of a lobster, which, with the front of this animal, makes the whole fore-part of the head. See fig. 4, 5, 6. This claw consists of two flattish branched pieces, covered with a hard crust or shell, and fastened perpendicularly to the lower part of the front by a soft skin, which serves them as a joint or hinge to open and shut upon. These pieces are beset with little hard eminences at the two edges that meet, and thus become fit to catch, and hold their prey near the mouth which is behind the claw, in order to draw their food therefrom.

At the lower end of each of the branched pieces, is a hooked nail, somewhat like the nails of a cat; these nails are very large, hard, and jointed, so that the animal can move them upwards and down-

* Adder-Bolts.

wards,

wards, without ſtirring the branches themſelves. It is probable theſe nails ſerve to ſhut or cloſe the ends of the claws, and gripe the prey to prevent its eſcape; for by their means, the aperture of the claws forms a triangle cloſed on all ſides, which otherwiſe would be open at one end. See fig. 6. Theſe nails being jointed, may likewiſe ſerve to raiſe or fall the prey, as the animal finds occaſion.

All ſpiders have 8 jointed legs, like the legs of lobſters; and at the extremity of each, are two large hooked jointed nails.

Between the two nails of each extremity, is a body not unlike a wet ſpunge, much like that found at the end of flies legs, and in all likelihood ſerving for the ſame purpoſes; *viz.* to walk with the feet upwards upon ſmooth poliſhed bodies, where the hooks or nails would be of no uſe: theſe ſpunges ſupply a ſort of viſcid liquor, which ſerves to make them ſtick or hang thereon: this viſcid liquor ſtops with age, both in flies, and ſpiders, ſo that they become unable to walk long up a perpendicular glaſs. And we even find, that an old ſpider or fly, happening to fall into a deep *China* jar, is unable to get out again, and muſt die of hunger.

And the ſame thing befalls ſpiders with reſpect to the matter, whereof they make their webb. An old ſpider has no more of this matter left in its body, nor can ſo much as refit its webb when broken, or diſplaced; all it can do is to expel ſome weaker ſpider of the ſame ſpecies, and poſſeſs its neſt, which I have frequently found it do. It is not unlikely, that the liquor at the extremities of the paws, is the ſame with that which makes its webb, or at leaſt near a-kin thereto, ſince both of them ceaſe about the ſame time; but of this we ſhall ſpeak more at large hereafter.

Beſides

Besides the eight legs above-mentioned, wherewith the spider walks, it has two others nearer the head, which are of no use in walking, but serve it in lieu of arms and hands, to place and take back the prey which they hold in their claws, in order to shift and present different parts of it to their mouth. This fifth pair of legs, or these arms, are not formed a like in all the kinds of spiders; in some, they are perfectly like the other legs; and in others quite different. Their difference will be noted when we come to the different characters of each species of spiders.

Around the *anus* of all spiders are four little muscular *papillæ*, or nipples, pretty broad about their bases, and pointed at their extremities*, having a pretty free motion; every way from the middle of these *papillæ*, as through a mould, or wier-drawer's iron, issues the vivid liquor, which produces the thread whereof their nests and webs are formed. This mould has a sphincter to open and shut it, by which means they can spin bigger or smaller at pleasure; and the spider, being suspended in the air by this thread, either stops when this mould closes, or continues to descend by its own weight when it opens.

The manner wherein they make their webs, is as follows: when a spider is to hang her work in a corner of a room, where she can easily go to all the places the threads are to be fastened on, she opens and detaches the four nipples above-mentioned immediately, upon which a little drop of viscid liquor appears upon the tip of each. This drop being forcibly pressed against the wall, sticks thereto by its natural gluten, and the spider removing from the place, new matter continues drawing thro' the hole; and thus is the

* Plate I. Fig. 10.

firſt thread form'd. Being arrived at the place of the wall, where ſhe would have her web terminate, ſhe preſſes the end of her *anus* againſt the ſame, and thus faſtens the other end of the thread, after the ſame manner as the firſt. This done, ſhe withdraws about half a line from the firſt, and here faſtens the end of a ſecond thread, which ſhe ſpins forth parallel to the former, till arriving at the other end of the firſt thread, ſhe faſtens the ſecond to the wall, and thus proceeds till the whole breadth of her intended web be finiſhed. Theſe parallel and longitudinal threads, which may be called the warp of the web being finiſhed, ſhe proceeds to croſs or traverſe them with other threads, and to this purpoſe faſtens one of their ends againſt the wall, and the other upon the firſt thread that had been drawn; thus leaving one ſide of the web quite open for the flies to come in at. Theſe latter croſs threads may be called the woof of the web; and being all of them but new ſpun, they eaſily ſtick to every thing they touch, and conſequently to the warp they paſs over, wherein all the ſtrength and firmneſs of this web conſiſts; whereas the firmneſs of our cloths depends on the interweaving of the threads of the woof between thoſe of the warp.

To make the croſs-threads ſtick the firmer, the ſpider works with its four *papillæ*, and ſqueezes cloſe all the parts where the interſections happen, as ſoon as one thread is laid upon another, remembering to triple, or quadruple the threads at the borders, to ſtrengthen them the more, and prevent an erupture being made in the web.

A ſpider may furniſh twice or thrice as much matter as is neceſſary to make a web, provided ſhe have not ſpent too much in the firſt; but if a new web be wanting after this, ſhe muſt either diſ-

diſpoſſeſs ſome other ſpider by force, or find a vacant web, which is no unuſual thing, by reaſon the young ſpiders always relinquiſh their firſt webs to make new ones. If the old ſpider be not ſupplied with any of theſe ways, it muſt periſh, for there being no living without a web, at leaſt for the domeſtick ſpiders, tho' ſome of the reſt need none. Thus much for the webs made in corners of rooms.

As to the webs made a-loft in gardens, *&c.* where the ſpider cannot eaſily come, the method of proceedure is thus: the animal places itſelf in a calm ſeaſon on the end of ſome branch of a tree, or any other body that projects far into the air, here ſtanding firm on its 6 fore-feet, with the 2 hind ones, it draws a thread from its anus two or three yards long, which it lets float in the air till ſuch time as the wind driving it againſt ſome ſolid body. It quickly ſticks thereto by its natural gluten; the animal from time to time pulls this thread towards it, to learn whether the looſe end have yet faſtened to any thing, which it learns by the reſiſtance it meets withal in pulling. Finding it fixed, it ſtrains the thread a little, and fixes it with its *papillæ* to the place where it ſtands. This thread now ſerves it as a bridge, or ladder, to go to the place where chance has caſt it, by which means ſhe doubles this firſt thread, which ſhe afterwards triples, or quadruples, according as its greater or leſs length requires more or leſs ſtrengthening. This done, the ſpider places itſelf about the middle of this thread, and with its two hind paws draws from its anus a new thread, which it lets float like the former, till finding it fixed to ſome body, ſhe ſtrains it a little, and then with a *papilla* faſtens the end as perpendicularly as ſhe can, on the middle of the firſt

 thread;

thread; proceeding afterwards to ſtrengthen it, by doubling, or tripling, as in the former caſe. The like proceſs ſhe repeats ſo often till the middle of the firſt thread become a centre; from whence proceed ſeveral radii, the work being continued till ſuch time as ſhe can go upon the croſs threads from the end of one of the radii, to the ends of all the reſt This done, ſhe fixes a new thread in the centre, and draws it along one of the radii, and from thence to the middle of one of the croſs threads, where ſhe faſtens it with her *papillæ*; and by this means makes as many radii as ſhe finds proper. The radii all made, ſhe returns to the centre, and there faſtens a new thread, which ſhe draws and faſtens down in a ſpiral direction upon the radii, from the centre to the magnitude ſhe would have the web. This done, ſhe takes up her lodging in the centre of the web, with her head always downwards, to avoid, as ſhould ſeem, the too great brightneſs of the heavens, as having no eye-lids to reſtrain and modify it, or rather, to ſuſtain and reſt her big belly on a large baſe of her thorax; whereas if ſhe remained with her head upwards, the belly would only hang by a ſlender thread, wherewith it is faſtened to the thorax, which might be incommodious.

The ſpider only keeps in the centre of her web during the day-time; in the night, or when it rains, or blows hard, ſhe retires into a little cell, built at the extremity of her web, under the leaf of a tree, or plant, or ſome other place ſtronger, and more ſtable than her web, and which may afford her ſhelter from the rain. This place ſhe uſually chooſes towards the higheſt part of the web, that ſhe may have immediate refuge there on occaſion; for moſt ſpiders aſcend with more eaſe and diſpatch than deſcend.

The

The ſpider lies in wait for flies, or other inſects, which entangle themſelves in her web, and which are to ſerve it for food. When the fly is ſmall, the ſpider takes it in its claws, and bears it into her neſt to ſuck its juice; but when the fly is too big, in proportion to the ſpider, and with its wings and claws might be liable to incommode her, the ſpider in this caſe wraps her round and round, with a number of threads, which ſhe draws from her anus, to fetter the fly, till ſhe can no longer ſtir either wing or feet; upon which the ſpider carries it peaceably into her den, and feeds of it. Sometimes the fly happens to be ſo big and ſtrong, that the ſpider cannot compaſs it, in which caſe, inſtead of entangling it more, the ſpider looſens it, or even, if that cannot well be, breaks the part of the web where the fly hangs; and, lets it go, applying herſelf in the next place, either to mend her damaged web, or make a new one.

All male ſpiders are ſmaller than the female ones of the ſame kind; and this to ſuch degree, that I have found five or ſix male garden ſpiders hardly balancing one female one. This is no uncommon thing in moſt inſects, tho' quite contrary to what we find in quadrupeds, where the males are always bigger and ſtronger than the females.

The ſpiders of all kinds are oviparous, with this difference, that ſome of them, as the garden ſpider and ſpinner, produce a great number of eggs; and others, as the houſe ſpider, very few; they lay their eggs on a piece of the web, which they bind together in a cluſter, and brood on them in their neſt. If they be driven out of the neſt, in the time when they are hatching, they take this cluſter of eggs in their claws above deſcribed, and carry it with them. As ſoon as the little ones are hatched, they begin to ſpin, and enlarge at ſuch rate, that one may almoſt ſee them grow; yet, without taking any food that I have been able to diſcover; if a very ſmall gnat happen to fall in their way, they fly upon and make ſhew, as if they fed on it; but if none come in a day or two, or even more, they ſtill continue growing as faſt as if they had fed, augmenting every day to more than double their bulk.

The peculiar characters of each ſpecies of ſpiders, are taken from the different diſpoſitions of their eyes; not but there are other conſiderable differences between them, but theſe not univerſal.

The domeſtick, or houſe ſpider, which makes the firſt ſpecies, has 8 little eyes nearly equal in its forehead, in an oval ſituation*. This ſpider makes a large web; its arms are perfectly like its legs, excepting that they are ſomewhat ſhorter, and that it never puts them to the ground. This ſpecies changes its ſkin yearly, even to the very legs, as lobſters do, which I have not obſerved of any other kind. It is very long-lived, I having known one of them above four years, which had not grown any thing conſiderable in body, but a great deal in legs. This ſpider is liable to a diſeaſe, which renders it frightful, being ſometimes covered with ſcales ſtanding out an end, and the intervals thereof ſwarming with vermin, much like the lice upon flies; but a deal ſmaller. When the diſtempered ſpider runs faſt, it throws off ſome of its ſcales, with the little vermin. The diſeaſe is very rare in our cold countries, nor have I ever obſerved it out of *Naples*. The ſpider, when ſeized therewith, never ſtays long in a place; and if it be ſhut up, ſoon dies.

* Fig. 4.

The ſecond is the garden ſpider, which makes a large round web in the air, and uſually poſſeſſes the middle thereof. It has 4 large eyes, placed ſquare in the middle of the forehead, and 2 ſmaller on either ſide of the head*. The females of this ſpecies have the largeſt bellies I have known in ſpiders; the males are very ſmall; they are of different colours, but uſually *feuille mort* ſpotted with white and brown; tho' ſometimes they are all white, as thoſe I obſerved at *Toulon*, among the tuberoſe flowers; and ſome I have known all green; nor are they all of the ſame ſize, but the green ones are ſmalleſt, and the brown ones biggeſt of all. Pouring ſpirit of wine on theſe ſpiders, they did not ſeem at all diſturbed thereby, no more than with *aqua fortis*, or oil of vitriol; but oil of turpentine killed them in a moment; which accordingly I have frequently applied to deſtroy broods of young ſpiders of this kind; ſome of them containing no leſs than a hundred a-piece, which, in a few days, will overrun a whole garden, and ſpoil a great number of plants.

The third ſpecies is that of ſpiders in vaults and old walls. Theſe ſeem only to have ſix eyes, all the other ſpecies having eight. The eyes are placed two in the middle of the forehead, and two of each ſide the head; all ſix being nearly of the ſame ſize †. The ſpiders of this ſpecies are all of them black, and very hairy; their legs are ſhort, and they are ſtronger and more miſchievous, as well as longer lived, than moſt other ſpiders. If you take one, it will defend itſelf and bite the inſtrument it is held withal, and though pierced in the belly, will ſometimes live two or three days; whereas all the other ſpiders die

* Fig. 5. † Fig. 6.

quickly upon piercing their belly; nor do they ever defend themſelves, or bite any thing when taken. In lieu of a web to catch flies, theſe only ſpin a few threads, 7 or 8 inches long, which iſſue from their neſts, like ſo many radii, and are faſtened to the wall around the hole where they inhabit; any inſect walking on the wall, and ſticking againſt any of theſe threads, advertiſes the ſpider, who lies perdue in her hole, and, upon this notice, inſtantly ruſhes out with prodigious ſwiftneſs, and ſeizes the inſect. I have ſeen a vigorous waſp carried off by one of theſe ſpiders, which none of the other ſpecies would have touched, both on account of the ſtings thoſe inſects are armed withal, and of the hard ſcales wherewith their whole body is defended; but the fore part and legs of this ſpider, being covered with a very hard ſhell, and the hind part, or belly, with a thick cloſe leather, it does not fear the waſp's ſting; and its gripes are ſo ſtrong and hard, that they are able to break the ſcales of the waſp.

The fourth ſpecies of ſpiders are thoſe we call vagrants, by reaſon they do not ſtay at home in their reſpective neſts, as all the other ſpiders do, who wait quietly for their prey to come home to them; but, on the contrary, go out in queſt of prey, and hunt it down with infinite wiles and ſtratagems. They have two large eyes in the middle of their forehead, and two ſmall ones at the extremities of the forehead, two of the ſame ſize on the back of the head, and two very ſmall ones between that and the forehead *. The ſpiders of this ſpecies are of different ſizes and colours, white, black, red, brown, and ſpotted. In one part of their body they are different from all

* Fig. 7.

other ſpecies, *viz.* the extremity of their arms, and 5 pair of legs, which terminates in a cluſter, or in a plume of feathers; whereas in all the other ſpiders, it terminates in two hooks, like the other legs. This plumage is uſually of the ſame colour with the reſt of the body, and ſometimes equal in bulk to the whole head. The animal makes uſe of it, to throw upon the wings of flies it has caught, in order to prevent their motion and fluttering, which would greatly incommode it, in as much as this ſpider wants the neceſſary means, which others are furniſhed withal, of tying and entangling its prey.

The fifth ſpecies is, That of field-ſpiders, vulgarly called, ſpinners. This ſpecies has its fore-part, or head, and *thorax*, flat horizontally, and almoſt tranſparent, being covered with a very fine whitiſh ſleek ſcale; it has a large black ſpot on its head, which I take for the brain, which appears through the tranſparent ſhell it is covered withal. This ſpider has 8 eyes ranged in a very extraordinary manner, two of them in the middle of the fore-head, ſo extremely ſmall and cloſe to each other, that they appear like one little oval body: at the right and left of the fore-head, are two little prominences; and at the top of each of theſe, are three eyes, placed very near each other *; theſe eyes are bigger than the two in the middle; their cornea is very prominent, white, and tranſparent, though the fund be black; whereas, the eyes in the middle, are quite black. From each of theſe prominences, as well as from the two eyes in the middle, ariſe three very ſenſible canals, which terminate in the black ſpot, ſuppoſed above to be the brain. As theſe canals recede from the eyes, they approach towards each

* Fig. 8.

other

other, ſo as to end almoſt in the ſame part of the brain: in them are the optic nerves probably lodged. The legs of theſe ſpiders are much ſlenderer and longer in proportion, than thoſe of other kinds; but their arms are much ſhorter, and more fleſhy, bearing little or no reſemblance to the legs, as they do in all other ſpiders: their legs are ſo full of hairs, that to the microſcope, they appear like writing quills.

The famous tarantula makes the ſixth ſpecies of ſpiders; it has the figure and appearance of a common houſe ſpider, but much ſtronger, and more robuſt in all its parts: the legs, and bottom of its belly, are ſpotted with black and white; but the top of its belly, with all the fore-part are quite black: its head and *thorax* are covered with one ſingle black ſhell, perfectly like a little tortoiſe: it has eight eyes, which are altogether different from thoſe of other ſpiders, both in colour, and conſiſtence. All the eyes of other ſpiders, are either black, or red, bordering on black; and are covered with a hard tranſparent ſcale, remaining ſuch after their death; whereas, theſe are covered with a ſoft and moiſt cornea, which withers and ſinks when they are dead: their colour is white, bordering ſomewhat on yellow, very bright, and ſparkling like cats eyes, when viewed in the dark; they are ſituate four in a ſquare figure, in the middle of the fore-head; and four in a horizontal line, below the four firſt: theſe laſt border the bottom of the forehead, and are placed immediately over the root of its gripe, or pinchers. Theſe eyes are of different bulk; the four firſt are nearly alike, being about a line in diameter, and ſufficiently viſible without a microſcope; but the latter are not above half the diameter of the former. The tarantula is very miſchievous, and will bite on its own accord, during the coupling ſeaſon. I have ſeen them at *Rome*, but they are not minded, as having never been known to do any harm; but in the kingdom of *Naples* they do a deal of miſchief, by reaſon we ſuppoſe the country is much hotter there than at *Rome*. The ſymptoms which befall thoſe wounded thereby, are very whimſical as well as the cure. They have been deſcribed by ſeveral *Italian* and *French* authors; and tho' their hiſtory appears ſomewhat fabulous, it is real nevertheleſs. An account of them has been given us by M. *Geoffroy*; and an extract thereof in the hiſtory of the academy for the year 1702, to which we refer the reader.

An explanation of the figures, tranſlated by J. M.

Fig. 4. Repreſents the eyes and claws of the houſe ſpider.

Fig. 5. The garden ſpider, which keeps in the air, in the middle of its web.

Fig. 6. The black ſpider, which inhabits in the holes of old walls.

Fig. 7. The wandering ſpider, which does not keep in one neſt like the others, and goes out to hunt flies and other inſects.

Fig. 8. The head and eyes of the field-ſpider, commonly called the ſpinner.

Fig. 9, 9. The tarantula.

Fig. 10. A ſpider reverſed, which ſhews the *papillæ* of its *anus*, which it makes uſe of for the thread.

V. Of

V. *Of the effect of gunpowder, chiefly in mines, by M.* Chevalier *.

Every one knows, that gunpowder is a compoſition of ſaltpetre, ſulphur, and charcoal, beat and mixed together; and that a certain proportion is to be obſerved in the mixture of theſe ingredients, and precautions taken in the choice of them, and in the manner of making the powder, which contribute to the goodneſs thereof. But this is not what we deſign here to examine. It is of the effect of the powder, and chiefly in mines, which I propoſe to treat.

The late marſhal *de Vauban* communicated to me a great number of experiments on this ſubject. This great man who was always employed in promoting the king's glory, and the grandeur of the ſtate, having obſerv'd on many occaſions, that the ſucceſs of mines did not always anſwer to expectation, thought it neceſſary by exact experiments to determine the different effects of mines in all the ſeveral circumſtances wherein they may be employ'd; and from thence conclude on certain rules to be obſerv'd on important occaſions. The ſucceſs has juſtified theſe rules; but before I lay them down, I muſt explain the reaſon why gunpowder when it takes fire, is capable of making ſuch great efforts.

Firſt I conſider, that air is neceſſary to the action of the powder; for by experiments made in the air-pump, it will not take fire from a flint in the vacuum; and though it takes fire from the ſun-beams, by means of a double convex-glaſs, yet it is almoſt without any noiſe or effort.

* Nov. 12, 1707.

Secondly,

Secondly, The bodies whereof gunpowder is compoſed, do not with equal facility take fire. Sulphur takes it more readily than charcoal, and charcoal than ſaltpetre, which is the predominant ingredient in the powder; there is commonly 3 parts of ſaltpetre, to one of both the other taken together. It is alſo to be ſuppoſed, that each of theſe bodies is compoſed of parts of unequal aptneſs in taking fire.

Thirdly, The powder muſt be very dry, that it may the ſooner take fire; it muſt be granulated that the flame may very ſubtilely communicate itſelf through the ſpaces left between the grains, which muſt all perform their effort almoſt at the ſame time.

I. This being ſuppoſed, it may be conceived, that firſt the different bodies whereof powder is compoſed, taking fire ſucceſſively, the fire directly impreſſes its action on the firſt or moſt ſubtile, which afterwards communicates a certain degree of velocity to the ſecond; and the ſecond to the third, and ſo on till the whole matter being kindled, makes its effort.

2. Moſt of thoſe bodies againſt which the powder acts, have alſo parts of unequal ſolidity capable of communicating to one another ſucceſſively the motion of the parts of the powder; and the effort of the parts of the powder will be ſo much the more conſiderable, the greater number there are of parts of unequal ſolidity, either in the ingredients of the powder, or in the bodies againſt which it acts; (all things elſe being equal) and that theſe parts have with one another, and nearer relation to a geometrical progreſſion, beginning at the moſt ſubtile, and proceeding to the moſt groſs, as has been ſhown by the learned M. *Huygens*, in his *Laws of Motion*, and after him by M. *Carré*.

It

It may therefore be concluded, that the bodies alone whereof powder is compoſed, being put in motion by fire, become capable by ſtriking againſt one another to contribute to the great effect which it produces: but I think it not poſſible to reduce to calculation what ſhare they have in it, becauſe the proportion of ſeveral parts of the bodies whereof powder is compoſed, are not known, nor that of the bodies on which it acts.

II. Let us now examine what effort the air contained in the grains of powder, and that which fills all thoſe little ſpaces between the grains, is capable of producing by its ſpring when it is dilated by the action of fire. Experiments have ſhewn, that the ſpring of the air becomes capable by the heat of boiling water, to ſuſtain a weight three times greater than what it will ſuſtain in a temperate degree of heat.

I ſuppoſe a certain bulk of powder, contains in all its pores, and between the ſpaces of the grains, as much air, as it contains proper ingredients of the powder; thus 2 cubic feet of powder, which weigh about 140 *lb.* contain 1 cubic foot of air. If a mine is conceived to be charged with 140 *lb.* of powder, and that the aperture of this mine is a foot ſquare, the air contained in the mine, will by the preſſure of the external air, with which it is *in equilibrio*, ſuſtain a weight of more than 2,200 *lb.* which is the weight of a priſm of quickſilver, whoſe baſis is a foot ſquare, and 28 inches high. If to this air contained in the mine, a degree of heat is communicated equal to that of boiling water, it will become capable by its ſpring, to ſuſtain a weight of about 2,900 *lb.* that is a third more than before; thus if the weight which reſiſts the effort of this air, is leſs than 700 *lb.* it will be lifted up. And if it is ſuppoſed

that

that the action of the fire, impreſſes on the air a degree of heat 100 times greater than that which it receives from boiling water, it will become capable of ſuſtaining a weight 100 times greater. In this caſe, one cubic foot of air will ſuſtain a weight of near 290,000 *lb.*

It has been ſuppoſed, that the action of fire augments the force of the ſpring of air only 100 times more than the heat of boiling water: but there is a probability, that it augments it conſiderably more; for it is certain that the force of the ſpring of the air when loaded, augments in the ſame proportion as its bulk would augment, if it was not loaded: thus by the heat of boiling water, the air would only augment its bulk one third; but, by M. *Amonton*'s experiments, powder, which has taken fire, augments its bulk 4000 times; and it muſt be imagined, that the air contained in the powder hath a great ſhare in this increaſe, which nevertheleſs I do not think it poſſible to determine exactly.

However, without having any regard to the motion, which may be produced from the different bodies, whereof the powder is compoſed, ſtriking againſt one another, for this cannot be brought to a calculation; and only ſuppoſing, that the action of fire augments the force of the ſpring of air 100 times more than the heat of boiling water, it has been juſt now ſhown, that one cubic foot of air, contained in two cubic feet of powder, is capable of ſuſtaining a weight of near 290,000 *lb.* but this effort being made from all parts againſt the ſurface of all the bodies which ſurround the powder, as from a centre to the circumference, it is divided among all theſe bodies; ſo that if a cubic mine is ſuppoſed, whoſe ſix faces equally give way, each face of the mine will ſuſ-

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tain the ſixth part of the whole effort of the powder which it contains; thus in the preceding ſuppoſition each face will ſuſtain a weight of about 48,000 *lb.* but if there were five faces of this mine immoveable, the effort would fall entirely on the ſixth, which would then ſuſtain the whole weight of 290,000 *lb.* This effort is much greater than what is found by experiments; for 140 *lb.* of powder raiſes only about 30,000 *lb.* weight of earth, as reſults from the experiments which ſhall afterwards be given.

The reaſon of this difference proceeded from many cauſes; 1. From the powder not taking fire all at once, the action of the firſt fire is finiſhed, or at leaſt conſiderably diminiſhed at the time of the effort of the ſecond.

2. A part of this effort is loſt by the paſſage which conveys the fire into the mine, and by the pores of thoſe bodies which encompaſs the mine. Experience ſhows, that in counter-mines 15 or 20 feet diſtance from mines which have been played, there is an inſupportable ſmell of burnt powder; nay, that even the ſmoak conveys itſelf through the earth.

3. The tenacity of the parts from being ſeparated is another obſtacle; ſo that a greater force is neceſſary, for example, to raiſe 1,000 *lb.* of old maſonry well bound, than the ſame quantity of new, or ſuch as is not well bound; for, beſides the weight of raiſing them, this coheſion muſt be alſo broken.

4. To ſuſtain the weight of the earth alone is not ſufficient; but a great part of the effort of powder is alſo employed in carrying it upwards with a certain velocity.

5. The reſiſtance of the ſurrounding air is another obſtacle to be ſurmounted, to which no re-

gard is had in practice, though it is very confiderable, and perhaps the moft confiderable of all.

III. To form a clear idea of the manner by which powder acts on bodies, let us fuppofe an immoveable gun fixed vertically with the mouth upwards, of an indefinite length, or at leaft long enough for a ball to make all the range which the powder can fend it; and having no regard to the friction of the ball in the barrel of the gun, let us fuppofe that it is applied immediately to the powder, and that it is fo perfect a caliber, as exactly to fit the barrel of the gun, fo that no air can pafs between; in order that we may only confider what can happen from the refiftance of the air, and the effort of the powder.

In this hypothefis, if fire is put to gunpowder, it will catch it fucceffively, and the ball will not go out till there is a fufficient quantity thereof, not only to get the better of the weight of the ball, but alfo of the column of air which refts upit. So that if the ball be fix inches in diameter, it will weigh near 33 *lb.* and the column of air will weigh about 440. Thus the ball will not be perceived to move, till that quantity of powder takes fire, which is able to move a weight of 473 *lb.* The powder continuing to take fire, it will fucceffively augment the fwiftnefs of the ball, till it has acquired its greateft velocity, which would be the fame with the inflamed parts of the powder, did not the air refift it; but as the refiftance of the air, which the ball expels, augments in the proportion of the fquare of the velocities of the ball, there is a fixed time when this refiftance becomes equal to the new effort, which the ball receives from the powder. Thus when there is too great a quantity of powder in the gun, it will not augment the velocity of the ball. Suppofing therefore that there is in this gun only a fufficient quantity of powder, to give it the greateft velocity it is capable of acquiring, the effort of the powder will after that diminifh fucceffively, till it entirely ceafes; and then did not the air refift the motion of the ball, it would continue to move with a uniform fwiftnefs, equal to its greateft acquired velocity: but the air continually refifting, the fwiftnefs of the ball diminifhes each inftant, fo that there is a fixed time, wherein the remaining impreffion, which the powder has given to the ball, is equal to the refiftance of the air, and then the ball can no longer move. But the weight of the air and of the ball acting againft it, with an effort of 473 *lb.* as has been faid, will repel the ball to the bottom of the gun, by accelerating its velocity, like all heavy bodies.

From what has been faid, it may be concluded,

1. That the beft powder (every thing elfe being equal) is that which fooneft take fire.

2. That the barrel of the gun, near the breech, ought to be fuch, that a greater quantity of powder may take fire therein before the ball goes out. This is the reafon why guns, with chambers, carry farther with an equal quantity of powder, or as far with a lefs quantity than thofe whofe barrel is entirely cylindrical.

3. That in a gun, whofe barrel is cylindrical, of a given length, there is a determined quantity of gunpowder which drives the ball as far as poffible; and this quantity is fuch, as may have time to take fire while the ball is in the gun. But the more powder there is on fire in the gun, the more danger there is of its burfting, becaufe its

effort is greater, and it remains longer againſt the ſide of the gun.

4. That the longer that part of the gun is, through which the ball is to run, ſuppoſing it does not attain its greateſt velocity, the more powder may be put into it; becauſe the ball taking up more time in paſſing, a greater quantity of powder has time to take fire, of which it receives the impreſſion. This is probably the reaſon, that ſome very long guns, ſuch as the culverin of *Nancy*, carry much farther than common guns of the ſame caliber.

5. That the quantity of powder with which a gun is charged, and the ſhape of its barrel being determined, there is alſo a length in the gun which has all poſſible advantages; ſo that a greater length would leſſen the range of the ball. This length is ſuch, that the ball may go out of the mouth of the gun, when all the powder has made its effort; and if the quantity of powder is undetermined, this length is ſuch, that the ball will go out of its mouth when it has acquired its greateſt velocity. Therefore guns of the new invention, whoſe barrel near the breech is ſpherical or ſpheroidal, in which the powder being more cloſe together, takes fire more readily, are not ſo long as thoſe whoſe barrel is cylindrical.

6. That the effort of the powder, towards one certain ſide, is greater in proportion to the reſiſtance it meets with from the others; and thus the more difficult it is for a gun to recoil, whether becauſe of its weight, or any other impediment, the farther will it ſend the ball. The difficulty of conveying very heavy guns by land, and the expence requiſite for this, cauſe them to be made as light as poſſible, provided they can reſiſt the effort of the powder; but guns made

for ſhips, are commonly much heavier than thoſe deſigned for land ſervice.

Let us now apply what has been ſaid of the action of powder in general, to its particular effort in mines. I ſuppoſe it is known what a mine is, and the different kinds thereof, as *Fourneaux*, *Fougades*, &c. The precautions which ought to be taken in digging and charging them, propping up the galleries and branches which lead to them, ſtopping them up, the way of diſpoſing the ſauciſſe, which conveys the fire to it; all which things are well deſcribed by thoſe who have treated on mines. It is chiefly to determine the moſt advantageous diſpoſition of them, and the quantity of powder with which they ought to be charged, that they may perform the effect propoſed, that we were obliged to make theſe experiments.

Mines are either made in the body of the earth, ſuch as are made by the beſieged to blow up the batteries and works of the beſiegers, before they make a lodgment on the covered way; or on riſing ground, where nothing joins to it either on the right or left, as to make a breach in ramparts made of earth; or to blow up walls, which may be dry or thrown down; to conclude, ſometimes they are made uſe of to tear up rocks.

All the experiments have diſcovered;

I. That the effect of the mine is always made on the weakeſt ſide; thus the diſpoſition of the chamber of a mine does not contribute to determine this effect, either to one ſide or another, as the miners had falſly imagined.

II. That a greater or leſs quantity of powder is requiſite, according to the inequality of the weight of thoſe bodies which the mine is to raiſe, and according to the inequality of their coheſion, and

and the refult of all the experiments which have been made, to know what quantity of powder muft be ufed according to the different bodies, is to each cubic toife.

	lb. of powder.
Of loofe earth	9 or 10
Of firm earth, and ftrong fand	11 or 12
Of fat clayey earth	15 or 16
Of new mafonry, but flightly bound	15 or 20
Of old mafonry, well bound	25 or 30

III. The aperture of a mine, which has played in the body of the earth, being properly charged, is made in a cone, the diameter of whofe bafe is double the height taken from the centre of the mine.

IV. That when a mine is over-charged, it makes only a hole or well, whofe fuperior aperture is not greater than the chamber wherein the powder was lodged.

V. That befides the effort of the powder againft the bodies which it raifes, it alfo preffes and crufhes all the earth near it, both underneath and on the fides of it, and this preffure or crufh extends fo much the farther, as the furrounding bodies make lefs refiftance.

To account for all the effects refulting from thefe experiments, and afterwards determine the quantity of powder with which mines ought to be charged, and the moft advantageous difpofifion to produce the effects propofed by them.

Let us, 1. conceive a mine, whereof all the parts furrounding it are incapable of compreffion, and make equal refiftance, fuch as a bomb of equal thicknefs every where would make, fufpended in the air; it is evident that in this cafe, befides the refiftance of the body, the effort of

 the

the powder muſt ſurmount the weight of the ſurrounding air; and then the body muſt be reduced to duſt, or at leaſt into very ſmall pieces.

By the way, it muſt be obſerved, that the bomb differs from this ſuppoſed mine, only in its being a little thicker at the bottom, oppoſite to the fuſee than elſe where.

The bottom of the bomb, is made moſt ſolid for two reaſons. 1. That this part being heavieſt, may turn towards the ground when the bomb falls, leſt it ſhould be broken by its ſhock againſt thoſe bodies which it meets with. 2. that it may not fall on the fuſee, which might extinguiſh it; either of which caſes happening, the bomb would not execute the principal effect deſigned, which is to convey the fire into the enemy's magazine, after having by its fall, made way through the vaults or boards of the places, which contain them. Bombs are alſo on many occaſions made uſe of in mines, as to blow up a buttereſs in the walls of a rampart, when a breach is to be made in an inveſted rampart, and in the *fougades* made for the defence of the outſide af a place.

Let us in the ſecond place conceive a mine, wherein all the bodies which encompaſs it, are equally capable of compreſſion, and make a reſiſtance with equal force on all ſides. In this caſe, the firſt effect of the fired powder, would be to cruſh and compreſs equally all theſe bodies, and they will not be divided or ſeparated, till by their compreſſion, they become capable of reſiſting its effort; ſo the powder therein may be in ſuch a ſmall quantity, that its whole effect may only terminate in the compreſſion of the adjacent bodies. This is the reaſon, why in mines made in the earth, the chamber is ſtopped up with ſtrong beams well ſupported; ſometimes even with

with ſtones that the adjacent parts may have more reſiſtance. It is eaſy to conceive, that if the adjacent parts to the chamber of ſuch a mine, as has been ſuppoſed, were unequally capable of compreſſion, inſtead of the compreſſion extending equally in a ſphere, as in the firſt caſe, it would in this ſecond caſe extend unequally.

To conclude, if is ſuppoſed that in a mine, all the encompaſſing bodies are equally capable of compreſſion, but that there is leſs reſiſtance on one ſide than the other, as it happens to all mines which are made in the body of the earth, a ſphere of compreſſion will immediately be made, whoſe diameter will be ſo much the greater, in proportion to the reſiſtance of the weakeſt part on its being raiſed ; on which three things may be obſerved.

1. If the effort of the powder is very great in proportion to the reſiſtance on the weak ſide, the compreſſion will not extend far ; and this part will be raiſed ſo ſuddenly, that the neighbouring parts having not time to ſhake, there will only be made a hole or well, whoſe diameter will be very near equal to that of the chamber of the mine, the earth of which will be thrown at a great diſtance. This is what happened when *Verue* was beſieged by M. *de Vendôme*, the beſieged ſprang two mines, which being overloaded, did not blow up the batteries which annoyed them ; theſe mines made holes or wells wherein the beſiegers made lodgements under ſhelter.

Secondly, if the mine is under charged, it makes only a ſimple compreſſion, or at moſt a little riſing near the weakeſt part, as it happened at the ſieges of *Ciudad Rodrigo*.

In ſhort, if the mine is charged with a quantity of powder, between theſe two extremes, it will raiſe a cone of earth, the diameter of whoſe baſe, will have a greater or leſs proportion to its height, from the centre of the mine, according as the effort of the powder is greater or leſs. And the moſt advantageous effect, is when the diameter of the baſe of this cone is double its height; for then almoſt all the earth which was raiſed, falling back into the aperture of the mine, the enemy cannot make uſe of it for a lodgment. In order to produce this effect, the quantity of powder neceſſary in proportion to the different bodies to be raiſed by mines, has been determined by experiments.

To charge a mine therefore, that it may perform its effect with all poſſible advantage, the weight of the bodies which are to be raiſed muſt be known; that is the ſolidity of the right cone muſt be found, whoſe baſe is double the height of the earth, over the centre of the mine, which is eaſy to be found by the rules of geometry; the little cone contained in the chamber of the mine, may be ſubtracted; but ſuch minuteneſſes are of no conſequence, and the cube of its height may even be taken for the ſolidity of this cone; theſe ſolidities are not ſo much unlike, as to cauſe any ſenſible difference in the effect of the mine. Having found the ſolidity of this cone in cubic toiſes, multiply the number of theſe toiſes, by the number of pounds of powder neceſſary to raiſe the bodies which it contains, as directed in the experiments; and if the cone to be raiſed, contains bodies of different weights, a mean weight muſt be taken between them all, having alſo regard to thoſe which have moſt coheſion. It is in general beſt to put rather a little too much powder than too little. As to the diſpoſition of mines, it muſt be obſerved for a general rule, that the part

towards

towards which we would determine its effect, ſhould be the weakeſt. We will not here enter into the particulars of this diſpoſition, it varies according to the variety of circumſtances, in which they are employed, and the effects we would have them produce; and may eaſily be concluded from the principles already laid down.

VI. *A new conſtruction of ſluices, by M.* de la Hire *.

Sluices are commonly made on ſmall rivers, which have no great fall, and but little water, the river is therefore ſtopped at ſome convenient place, that a ſufficient quantity of water may be collected above it, to carry a boat; and when the boats are come to the ſluice, they expeditiouſly open it, and the boats paſs through it, being ſupported by the collected water.

The common way of ſhutting ſluices is very ſimple, and of ſmall expence, it is placing ſeveral pieces of ſquare boards againſt a groundſel fixed croſs the bottom of the river, and on the top againſt another piece of wood, which alſo goes croſs the width of the river, and is parallel to the groundſel, but is eaſily moved on a great pin at one end thereof; and the other end faſtened to ſome ſolid and firm body, when it is in a ſituation parallel to the ſell. All the boards which ſtop the ſluice, and are placed againſt the ſell, and the tranſverſe beam at the top, are called *aiguilles*, and are retained or held only by the water, which riſes by degrees in the canal, or river, above the ſluice: but all theſe *aiguilles* are never placed ſo exactly cloſe to each other, as to prevent the

* Dec. 3, 1707.

waters

waters running between them; which is a great fault in these sort of sluices.

When the sluice is to be opened, they hasten to take out these *aiguilles*, and turn the transverse beam at the top, to give free passage to the boat; but this cannot be done so soon as not to endanger its running aground, or being fast on the groundsel in the middle of the sluice. Therefore it is the practice in many places to fasten ropes to the top of all the *aiguilles*, the more easily to draw them on shore, and more expeditiously than by standing on the beam.

But here is a way of opening and shutting sluices at once, and without trouble. They may be shut or stopped with two doors, such as are commonly made use of at the entrance and going out of great sluices. They are folding doors which bear against one another, and make a salient angle to the side up the river: but the whole art consists in the construction of the door.

Each folding door * AB is only a frame of wood, of sufficient strength, for the use and place. These frames are hung upon hinges at C, which are on the posts on each side of the sluice, in the common manner of doors, and open upwards of the river: but the real doors, which shut the open part of the frames, are hung on hinges at D, on the upright battens of the frames, which are to join or meet when the doors shut, and these doors open downward of the river contrary to the frames. Near E, they have each a little latch, or rather a hasp, with a hole, which admits a staple, thro' which a pin may be put F, with a long handle like a bolt, that it may be placed in the hole or eye of the staple, when they are standing on the top of the door.

* Plate I. Fig. 11.

It is seen by this construction, that the doors E D, being fastened to the frames A B; and the frames being one against another, the canal of the river will be shut or stopped, and the water will rise against these doors on the side up the river; and when the sluice is to be opened, they need only draw out both the pins or bolts at the same time; and immediately the two doors going with the stream, the frames may be easily placed on the sides of the canal, by drawing them with a chain or a rope G B, as they stand on the shore; for the water can have no great power over that part of the frame which is in it.

By this construction, it is also seen, that by drawing the frames to the sides of the canal, the doors E D will still continue with the stream, but at last when the frames are quite open, the doors E D will be shut and return to their place of themselves, where they need only be fastened with the bolt.

In short, there will be no difficulty in shutting up the sluice, for the water being then almost on a level on both sides, has not more power against the door on one side than on the other.

The parts of these frames may be strengthened by two binders placed at the top, higher than the level of the water when it is retained, that it may take less hold of the parts of the frame, when that is to be opened.

It will be observed, that it is not necessary that the door should be always as high as the opening of the frame, it is sufficient if it keeps up the water in the canal high enough to carry the boats. Let it also be observed, that two great latches may be put instead of the two hasps, which are in the figure, to fasten the door the stronger, and better to the upright of the frame.

frame. These latches will fasten into the catches which are to be fixed into the door post, and there must be to each of them a button fastened into the same rod, which must reach to the top of the door, and go through two staples, or rings, which are there to be fixed; so that by pulling this rod, both the latches will lift up at once, and the same rod will serve to shut them when the door is put again in its place, if the latches do not of themselves fall into the catches by their own weight, and that of the rod.

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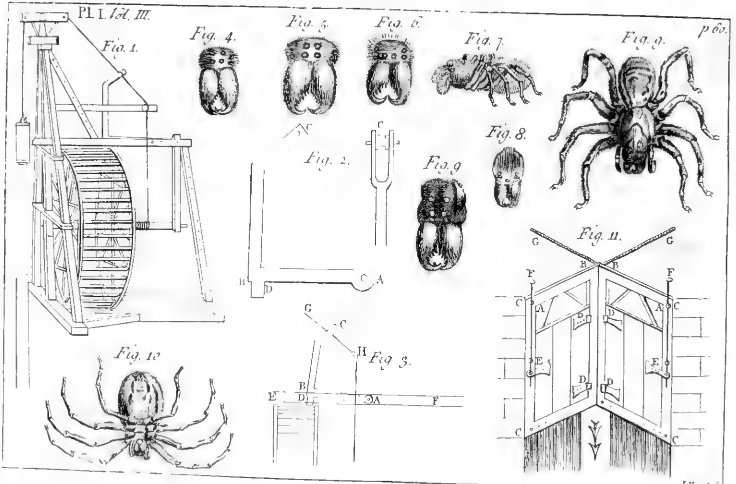
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J. Mynde f.

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OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCVIII.

In the HISTORY.

XV.

In the MEMOIRS.

AN

AN ABRIDGMENT OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1708.

I. *Upon thunder; tranſlated by Mr.* Chambers.

WE have chymical operations in the air, as well as in the laboratories, and ſometimes the very ſame: thus thunder is only an inflammation occaſioned by the mixture of a ſulphurous matter, with an acid ſpirit.

But a difficulty ſeems to ariſe hence, that thoſe two matters when mixed together by a chymiſt, after they are once ſet on fire, ſpend themſelves intirely, ſo that no new inflammation can be made without new materials; whereas, from one and the ſame cloud, we frequently find a multitude of flaſhes burſt one after another, which indicate as many different inflammations. Now the inflammable matter in the cloud being diſſipated in the firſt flaſh, how ſhould any new ones be formed?

M. *Homberg* is of opinion, that the ſame matters which take fire by their union, and by their firing become ſeparated again, may rejoyn each other anew, be kindled again; and thus for ſeveral times ſucceſſively. On earth this is impoſſible, by reaſon after they are once kindled, and by this means rendered extremely rare and light, the lower air being heavier than they, preſſes them on all ſides; and thus raiſes them to a region where they

they are found in *equilibrio* to a thinner air; and thus are loſt to us: but if the ſame matters be raiſed in exhalations from the boſom of the earth, they are already arrived at this region of *equilibrium*; and 'tis here they are kindled, where of conſequence they find no heavier air to raiſe them after the exploſion, ſo that they cannot be diſſipated, but will remain where they were, and may rejoyn each other till ſuch time as a ſhower caſt them down on the earth, and thus purge the air of them.

This explication is the more probable as it is founded on the operation itſelf, which repreſents thunder; if in lieu of pouring ſpirit of nitre haſtily on an eſſential oyl, which will produce a ſudden inflammation, it be poured on drop by drop, we ſhall only find an efferveſcence raiſed without any inflammation, and the mixture of the two liquors becomes a reſin; which if put in a retort, and diſtilled by degrees, will return the acid, and the oyl whereof it was formed: now this acid and oyl are ſtill capable either of being kindled by mixing them again, or of producing a new reſin, which will indure the ſame operation it had undergone before, as long as you pleaſe. Here the fire of the diſtillation makes the ſame ſeparation of the matters, as the exploſion would have done, if they had been ſuffered to kindle; whence it appears, that if they were not to fly from us, they would be as fit by their re-union to form a new flame, as a new reſin.

As in each moment that a flaſh of lightening ſtrikes the eye, there is a large quantity of matter ſet on fire, M. *Homberg* imagines, that ſo many repeated inflammations may give a certain determination to the air, and cauſe ſome of thoſe variable winds which blow indifferently from all points

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of the horizon, and which are the only ones which we experience in theſe temperate climates. Hence perhaps it is, that we have more ſouthern than northern winds, ſince there are always violent thunders betwixt the tropicks, which are ſouthwards, in reſpect to us; at leaſt it is certain, that this notion will very well account why our winds blow in puffs, or blaſts, ſince the flaſhes follow each other at ſome diſtance, and each gives its ſeveral blow; and if it were certain, that the regular or trade-winds blow more continuouſly, it would be a confirmation hereof.

II. *Of ſome ſhells incloſed in ſtone.*

M. *Tournefort* has ſhewed ſome ſhells incloſed in a bit of a rock, pierced by a great many cavities, which were as their habitation. The entrance of theſe cavities was often narrower than their bottom, ſo that theſe animals after being entered therein when ſmall, muſt have grown there, and preſſed the ſtone being yet tender, in proportion as they grew.

III. *Of the force of the rays of the ſun in preſſing and puſhing.*

We ſhould not have ſuſpected, that the rays of the ſun had the force of preſſing and puſhing, even when they are re-united by the burning mirrour. M. *Homberg* has obſerved, that if he expoſed to it a very light matter, ſuch as *Amianthus*, and in a pretty large quantity, it was reverſed by the rays of the *focus* above the coal which bore it, unleſs it was preſented very ſlowly, and one part after the other, ſo that it was not ſtruck too roughly by the *focus*, nor all at once in the whole ſur-

ſurface. Beſides, M. *Homberg* having ſtraitened a ſpring of a watch, and engaged one end of it in a block of wood, he drove by repeated ſtrokes againſt the free end of the ſpring, the *focus* of a *lens* of 12 or 13 inches diameter, and he ſaw that the ſpring made very ſenſible vibrations, as if it had been thruſt with a ſtick. This force of the matter of light agrees with the weight, that has been found in it by other experiments.

IV. *Why in ſummer ice melts faſter in* vacuo, *than in air.*

M. *Homberg* has found, that in ſummer, ice melts much ſooner in *vacuo*, than in the air. The reaſon of it is very plain; ice only melts by the action of the ſubtile matter or æther, and in *vacuo* the whole ſpace is filled only with this matter.

V. *Why the tendereſt glaſſes are leaſt ſubject to break by fire.*

M. *Homberg* has obſerved, that tender glaſſes, that is, ſuch as have more ſalt in their compoſition and leſs ſand, or thoſe which having more ſand are very thin, are leſs ſubject to break at the fire and burning mirrour. It is eaſy to ſee that glaſs is only brittle by the extreme heterogeneity of the particles of ſalt and ſand, of which it is compoſed, that it breaks by the difficulty that the ſubtile matter, when it is ſtrongly agitated, finds to move freely in the interſtices of its parts, and that it finds leſs reſiſtance in the particles of ſalt, than in thoſe of ſand, which are more ſolid.

VI. *Of the effect of the ſun's heat on a paſte laid upon a piece of poliſhed glaſs.*

A perſon having applied to a piece of poliſhed glaſs about half a foot ſquare, a paſte of *Spaniſh* white, and ſize, put it altogether in the ſun during the great heats of the ſummer. The paſte which was towards the ſun, having been ſtrongly heated, bent toward the ſun, and rolled upward, in ſuch a manner, that in this motion the lower ſurface, placed upon the glaſs, raiſed itſelf. But the ſingularity of it was, that this ſurface raiſed with it a flake of the glaſs. This flake made a ſort of varniſh upon the paſte like *Delft* ware; its thickneſs was unequal, but it did not exceed half a line. It is very ſurpriſing that the adherence of the paſte upon the glaſs ſhould be ſo ſtrong; and alſo that it ſhould be able to pull off from the glaſs ſo conſiderable a flake. It had been blown, and probably the cane, through which they blowed, had been plunged in the crucible at different times which had made it divide into ſeveral flakes, which however did not appear, becauſe they were very exactly applied to each other. We owe this obſervation to M. *Geoffroy.*

VII. *Of an extraordinary cure performed by muſick.*

The extraordinary cure which we have ſpoken of in the hiſtory of 1707, is not ſo much ſo, or at leaſt it is not any longer ſingle. Here is another example which we had from M. *de Mandajor*, mayor of *Alais*, in *Languedoc*, a man of ſenſe and merit. A dancing maſter of *Alais*, during

during the carnival of 1708, having been ſo much the more fatigued in the exerciſes of his profeſſion, as they are the moſt agreeable, fell ſick with it the beginning of lent. He was attacked by a violent fever, and the fourth or fifth day he fell into a lethargy, which he was a great while a coming out of. He came out of it only to enter into a furious and ſilent *delirium*, in which he made continual attempts of getting out of his bed, threatning with his head and looks thoſe who hindered him, and even all who were preſent; and obſtinately refuſed, conſtantly without ſpeaking, all the remedies that were offered to him. M. *de Mandajor* ſaw him in this condition; it came into his head, that perhaps muſick might recover a little this ſo diſordered an imagination, and he propoſed it to the phyſician. He did not diſapprove the thought, but he juſtly feared the ridicule of the execution, which would have been yet infinitely greater, if the patient had died in the operation of ſuch a remedy. A friend of the dancing-maſter, who was ſubject to none of theſe difficulties, and who could play on the violin, took that of the ſick perſon's, and began to play the airs that were moſt familiar to him. They took him to be more mad than the patient confined to his bed, and began to reproach him; but preſently the ſick perſon raiſed himſelf upon his ſeat, as a man agreeably ſurpriſed; his arms would beat time to the tunes; but becauſe they held him by force, he could only ſhew by his head the pleaſure he felt. By degrees, even thoſe who held his arms, finding the effect of the violin, ſlackened the violence with which they had held them, and gave way to his motion in proportion as they found he was no longer raving. At laſt, at the end of a quarter of an hour he ſlept ſoundly,

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and had during this ſleep a criſis, which brought him out of danger.

VIII. *Of the new iſland formed near* Santerini.

We are now better informed of the new iſland which has raiſed itſelf near that of *Santerini*, or *Santorin*, which has been mentioned in the hiſtory of 1707*. A letter, that F. *Bourgnon*, a miſſionary jeſuite at *Santorin*, an eye witneſs of all this phœnomenon, has writ to M. *de Feriol*, the *French* ambaſſador at the port, and that this miniſter has ſent into *France*, has been communicated to the academy.

May 23, 1707, at ſun-riſing there was ſeen from *Santorin*, 2 or 3 miles at ſea, ſomething like a floating rock which had not been ſeen before. Some believed it to be a veſſel which was going to break againſt ſome little iſlands or rocks which are there, and went to pillage it. They were ſurpriſed to find it a new ſhelf, and they were bold enough to get upon it, altho' it was yet moving, and encreaſed almoſt ſenſibly under their feet. They brought back, as a teſtimony of their courageous landing, ſome pumice-ſtones of an extraordinary fineneſs and delicacy; and ſome very large and exquiſite oyſters, that the rock where they were fixed, had raiſed with it, from the bottom of the ſea. They had a little earthquake in *Santorin* two days before the birth of this ſhelf; it increaſed very ſenſibly as well in breadth as height, till the 13th or 14th of *June*, without being accompanied with any accident. It was then almoſt half a mile in circumference, and 20 or 25 feet high. It was round and white; the earth was light, and had a little clay in it.

* Pag. 13 of this volume.

They

They began to believe, that this new labour of nature was finiſhed, but the water of the ſea became ſenſibly thick every day, and had the colour of various mineral ſubſtances; among which, ſulphur was predominant, the waves had an agitation, and boiling, which came from the bottom. Thoſe, who would approach the new iſland, felt an immoderate heat, which hindered their acceſs to it: at laſt there ſpread in the air a ſtink which infected the whole iſland of *Santorin*, and extremely incommoded the inhabitants; all this foretold ſome terrible change to this part of the world, and fear reigned in every mind. In effect there was ſeen on the 16th of *July*, at ſun-ſet, a great chain of 17 or 18 obſcure black rocks, a little from one another, which went out of the bottom of the ſea, towards the new iſland, and ſeemed to be going to join ſoon together, and with it, which actually happened ſome days afterwards. On the 18th there came out of it for the firſt time, a very thick ſmoke; and there were noiſes heard which came out of the bottom of the new earth, ſo much the more threatening, as they were alſo more hollow. The 19th the fire began to appear very weak at firſt, but it increaſed continually. Every night the new iſland ſeemed to be only made of a great number of furnaces, which vomited flames. And as if the heavens had a mind to contribute to this frightful illumination, there was ſeen one night toward the end of *July*, only for a few moments, a ſtream of fire which went from eaſt to weſt.

During this time, the iſland juſt forming increaſed very much, even in height. The waters of the ſea boiled more violently, they were more loaded with ſulphur and vitriol, and the infection was ſo great at *Santorin*, that they could not

 breathe,

breathe, eſpecially when the wind drove the ſmoak that way. Toward the end of *Aug.* the ſubterraneous noiſes became more frequent, and ſo terrible, that they equalled that of 6 or 7 great cannons diſcharged all at once, the fire made new openings every day, and it threw into the air ſometimes a prodigious quantity of fine aſhes, which did much damage to the harveſt of *Santorin*, ſometimes a like quantity of little ſtones inflamed, which cauſed a little iſland, whereon they ſometimes fell, to appear all on fire; ſometimes great burned rocks, which raiſed themſelves like bombs and carcaſſes, and afterwards plunged into the ſea at above 7 miles diſtance.

Theſe terrible diſcharges were become continually more frequent ſince the end of *Auguſt*; and, in fine, to the month of *November*, where F. *Bourgnon*'s relation ends. It is very remarkable, that then it did not any more throw out ſuch great ſtones, nor in ſo great a quantity, that the ſea was not troubled any more, that its boiling was calmed, that the ſtink was hardly ſmelt any more at *Santorin*; and, on the other ſide the ſmoak was every day blacker, thicker, and in greater abundance, the fire was greater, the ſhowers of aſhes were daily, and the ſubterraneous noiſes continual and ſo violently, that it was hard to diſtinguiſh them from thunder. The account goes no farther than the 20th of *November*; and it is likely, that the prodigies of the new iſland are not yet diſpoſed to ceaſe.

That of *Santorin* itſelf, which was formerly called *Thera*, has paſſed among the ancients for a new production. It is certain, that in 726, 1427, and 1573, it has received additions by ſubterraneous fires, or that the little neighbouring iſlands were formed as the laſt, which we have juſt mentioned.

tioned. There was alſo in 1650 a furious ravage in *Santorin* and thereabouts, but without any other new production than that of a great bank, which perhaps will be the foundation of another iſland. The ſubterraneous furnace, which is in this part of the globe, muſt be one of the moſt ardent.

IX. *Of the method of meaſuring the heights of places by the barometer.*

M. *John James Scheuchzer*, Doctor of Phyſick, at *Zurick*, and member of the royal ſocieties of *England* and *Pruſſia*, having ſent to the academy a great number of obſervations of the height of the barometer, which he has made in different towns of *Switzerland*, and upon ſome mountains of that country, during the years 1705, 1706, 1707, M. *Maraldi* made uſe of them to find, according to the method explained in the memoirs of 1703 *, how much the places where they have been made, are elevated above the level of the ſea. This method requires, that we know in what proportion the air is always dilated upwards; that we have correſpondent obſervations of the barometer, made in ſome place, whoſe elevation above the level of the ſea is known, as M. *Maraldi* had his made at *Paris*, and that we ſuppoſe in a great extent of country, ſuch as is that of *France* and *Switzerland*, that the barometer varies in the ſame manner and in the ſame time By this M. *Maraldi* found, for example, that mount *Joch* is elevated above the ſea 1340 toiſes, and as there is another pretty near it called *Tittliſberg*, always covered with ice and ſnow, which thoſe of the country ſay, is the higheſt mountain

* Vol. II. Page 85 of this abridgment.

of

of *Switzerland*, and which M. *Scheuchzer* believes to be elevated 2000 feet more than *Joch*; it follows from hence, that the higheſt mountains of *Switzerland* would be elevated 1660 toiſes. They would be more ſo than the *Canigou*, which is one of the *Pyrenean* mountains.

But it muſt be owned, that this method for meaſuring heights would be much more ſure; if we were not obliged to ſuppoſe that the barometer varied in the ſame manner and time in diſtant places, which is not always true; and if in the ſame country, where we would take a height, we had an obſervation of a barometer made at the ſame time on the ſea ſhore, or in ſome other place, whoſe elevation above the ſea was known, then there would not remain any more uncertainty than in the hypotheſis of the proportion, according to which the air that ſurrounds the earth dilates itſelf upwards.

And even this uncertainty begins to diſſipate a little; and the progreſſion, that M. *Caſſini* has eſtabliſhed for the dilatation of the air in the place above quoted, in 1703, is ſufficiently proved.

F. *Laval* having meaſured geometrically ſeveral heights at *Sainte Baume*, and thereabouts, he afterwards carried a barometer thither, and has obſerved how much lower it was there than at his obſervatory at *Marſeilles*, of which he knew the elevation above the level of the ſea. He has ſent his meaſures and obſervations to Meſſ. *Caſſini*, who have found what ought to be; according to their progreſſion, the height of the mountains, which gave the falling obſerved in the barometer; and they have found the ſame heights that F. *Laval* found elſewhere by geometrical meaſures. There was only two or three toiſes

toiſes difference, which is inconſiderable in proportion to great heights, and is beſides almoſt abſolutely unavoidable, becauſe in the leaſt dilatation of the air a line of quickſilver anſwers to 6 toiſes of air, and conſequently, if in the obſervation of the height of the barometer made in the loweſt place, we miſtake half a line, which is very eaſy, we miſtake three toiſes in the calculation, of the height, and much more, if the ſame error is in the obſervation made at the higheſt place. This is a general inconvenience of all the operations, where very ſmall magnitudes give great ones, to which they anſwer.

To meaſure the height by the barometer with the greateſt certainty poſſible, the two places where we obſerve the greateſt elevation and depreſſion of the quickſilver muſt be, as in F. *Laval*'s operation, ſo little diſtant that we may not ſuſpect the weight of the atmoſphere to be different.

Of a little ſhell-fiſh, that feeds upon muſcles.

M. *de Reaumur* has obſerved the way taken by a little ſhell-fiſh to feed upon muſcles, which is very ſingular and difficult to explain. This ſhell-fiſh is of the ſame ſpecies with thoſe which are called in *Latin Trochus*, or *Turbo*, that is, its ſhell is one piece, and turned ſpirally. The fiſh comes half out of it when it pleaſes, as the ſnails do out of theirs. The muſcle being incloſed in its two ſhells would not ſeem likely to be the prey of this animal; and yet it is. It faſtens itſelf to the ſhell of a muſcle, pierces it with a round hole very exact, about a line in diameter, and paſſes into it a ſort of trunk or little hollow cylindrical pipe, 5 or 6 lines long,

which

which it turns ſpirally, and ſucks the muſcle with it.

The difficulty is to know how it makes the hole. It is not with the trunk which ſucks, for that is too ſoft and too blunt to pierce a very hard ſhell. M. *de Reaumur*, by the diſſection of this animal, has not been able to find any part of it proper for this effect, though if it had any it muſt be as ſenſible as the hole; he has even met with many of theſe little ſhell-fiſhes faſtened to muſcles, which they have not yet pierced quite through, he has ſeparated them, and ſeen nothing. He has alſo obſerved, that theſe imperfect holes were almoſt as large in the bottom as at their opening, which does not agree with the figure of an inſtrument, which probably would be more pointed at its extremity. Laſtly, he has alſo ſeen oval holes, and it is difficult either for an inſtrument to make them, or for the ſame that makes round to make oval.

He believes therefore that the animal may throw upon the muſcle ſome drops of liquor capable of piercing the ſhell. This drop will naturally be round, and ſometimes it becomes oval, becauſe it does not fall perpendiculary upon the muſcle, or becauſe the muſcle gives it ſome little motion. To render this conjecture ſtill more probable, it is to be wiſhed, that in the imperfect holes, and where the animal ſeems ſtill to be working. M. *de Reaumur* had found there this ſort of *aqua fortis*.

He has obſerved, that there is never any hole in all the circumference where the two ſhells of the muſcle join, and he attributes this to a very ingenious precaution in the animal that attacks it. Which is, that if the muſcle ſhould open its ſhells, the trunk of the little fiſh would not be in the hole that it ſhould make, it would eaſily turn

turn it away, and then the muſcle in ſhutting its ſhells, would ſqueeze it, and perhaps cut it, or at leaſt would keep its enemy priſoner.

M. *de Reaumur* has ſometimes ſeen ſeveral holes upon the ſame muſcle, and when he has found empty muſcle-ſhells, he has almoſt always ſeen of theſe holes, which makes him believe, that theſe ſhell-fiſh do not a little contribute to the deſtruction of the muſcles.

XI. *An account of Dr.* John Scheuchzer's *diſſertation on the origin of mountains.*

M. *John Scheuchzer*, doctor of phyſick at *Zurick*, has done the academy the honour to dedicate to it a *Latin* diſſertation upon the *origin of mountains*, or upon the *formation of the earth*, which is not yet printed.

Deſcartes, for it often happens that the hiſtory of ſome inquiries, or of ſome diſcoveries begin by him, is the firſt who has thought of explaining mechanically the formation of the earth: afterward *Steno*, *Burnet*, *Woodward*, and at laſt *Scheuchzer* have undertaken either to extend or rectify his ideas, and have added them together.

If the globe of the earth was perfectly ſpherical, that is, without mountains, and if the different beds of ſand, clay and ſtones, of which it is compoſed, were every where, as they are in an infinite number of places, pretty exactly parallel between themſelves, and concentrical to the ſurface of this globe, we ſhould eaſily imagine that the whole had been formed of a troubled fluid, if I may ſo ſay, and heterogeneous, of which the different parts, unequally heavy, would naturally ſeparate from one another by the laws of gravity, and be ranked in different circular beds, which would all have had the centre of the globe for a common centre. Even this ſeparation would have made the fluidity ceaſe. This ſyſtem would not only be poſſible, but almoſt neceſſary, for we could hardly attribute to another cauſe the parallelism and concentricity of the *ſtrata*. That the the earth was at firſt a fluid, and that by the laws of motion it is become ſolid by time, and is diſpoſed as it is, or that God created it all at once in the ſtate to which the laws of motion would have brought it, is the ſame thing according to the ingenious reflection of M. *Deſcartes*. It is indifferent whether God created the egg, or the fowl firſt.

The parts of land and water animals, branches and leaves, *&c.* found in beds of ſtone, and that pretty deep, confirm this ſyſtem of the fluidity of the earth. By what other means than this, could they be incloſed where they are? but it is alſo true, that we muſt ſuppoſe a ſecond formation of the beds or *ſtrata*, much leſs ancient than the former, at the time of which the earth had neither plants nor animals. *Steno* eſtabliſhes ſeveral ſecond formations, cauſed in different times by extraordinary innundations, by earthquakes, and by the matter that the *Vulcano's* vomit. *Burnet*, *Woodward*, and M. *Scheuchzer*, chuſe rather to attribute to the univerſal deluge a ſecond general formation, which however does not exclude the particular ones of *Steno*.

But the mountains ſeem to ſubvert the ſyſtem of the fluidity, they could never have riſen, ſince all that is fluid becomes level. Nevertheleſs this ſyſtem is ſo probable in its ſelf, and ſo well ſupported in the greateſt part of the terreſtrial globe, that it deſerves ſome endeavours to preſerve it. It is for this,that M. *Scheuchzer* adopts the opinion of thoſe, who have believed that after the univerſal

verſal deluge, God being pleaſed to make the waters enter again, into the ſubterraneous reſervoirs, had broken and diſplaced with his all powerful hand, a great number of *ſtrata*, which were before horizontal, and had raiſed them above the ſurface of the globe. The whole diſſertation was made to ſupport this opinion.

As theſe heights or eminencies muſt have been of a very ſolid conſiſtance, M. *Scheuchzer* obſerves, that God raiſed them only in places where there were a great many beds of ſtone. From hence it comes, that the countries where there is a great quantity, as *Switzerland*, are very mountainous, and that on the contrary, thoſe which like *Flanders*, *Germany*, *Hungary*, and *Poland*, have only ſand or clay, and that to a very great depth, are almoſt intirely without mountains.

It was impoſſible that the broken, diſplaced, and elevated *ſtrata* ſhould remain horizontal; and we never find any in the mountains with this direction, but what remains of it, is that they are ſtill parallel between themſelves, and this, ſuppoſing the diſplacing, is in reality all that they could poſſible preſerve of it.

M. *Scheuchzer* has obſerved their different directions, in a whole chain of mountains of three leagues, upon the borders of the lake *Uri*, and has ſent to the academy a very curious map of them. There is no horizontal bed there, tho' they are all ſo in the plains, and hardly any that makes a right angle with the horizon; we find indifferently all the other angles. It is viſible that this is underſtood of the ſurface or ſlopes of the beds. As to their direction, which we ſhould ſee, if one ſide of the mountain was cut according to its inclination to the horizon, they are very different in different mountains, and ſometimes

times in the ſame. Some are in arches or vaulted, others are in a ſort of triangle, and have ſome very acute angles, but all the directions whatever of one bed, are always exactly parallel to thoſe of many other neighbouring beds. What is here the moſt ſingular in M. *Scheuchzer's* map, is the extreme direction of 2 different ſeries of beds, which meet at their convex parts, and form the figure of two branches of a curve that turns back.

M. *Scheuchzer* has made in the celebrated quarry of *Glaris*, from whence there has been drawn a great number of tables of ſtone, an obſervation not very favourable to the ſyſtem of the fluidity, which however he does not diſſemble. The beds of this quarry, which are but an inch thick, are of two different natures, and alternately hard and ſoft; and to make tables of it that may be uſed, they muſt cut a hard *ſtratum* with a ſoft one, without ſeparating them. The hard ſuſtains the ſoft which muſt be at the top, when they work it, as it is in the quarry. One would think that in a fluid, all the heavieſt part muſt have precipitated to the bottom, and that there could not have been beds alternately lighter and heavier. Nevertheleſs a ſingle bed, where the lighteſt is always at the top, proves alſo the fluidity, the whole difficulty remains in the alternate ſituation of the beds. To give a ſolid ſatisfaction of this difficulty, we had better wait for new obſervations which M. *Scheuchzer* ſeems to promiſe, than to imagine any ſolution, how ingenious ſoever. Beſides we have already launched too far upon a work which belongs to this able philoſopher, and which the academy has no right to aſſume

XII.

XII. *An account of Dr.* John James Scheuchzer's *diſſertation on cryſtal.*

M. *John James Scheuchzer*, brother to the former, and alſo doctor of phyſick at *Zurick*, a great natural philoſopher, has ſent alſo to the academya *Latin* diſſertation upon cryſtal, which he has not yet publiſhed.

There is a great deal of cryſtal in the mountains of *Switzerland*, and it is a journey which the author made thither in 1705, which occaſioned this diſſertation. We have but too few of theſe ſort of phyſical inquiries made by skilful perſons, who have ſeen them with their own eyes. M. *Scheuchzer* collected with great erudition all the different cryſtals, perfect, or imperfect, coloured, mixed, and differently figured, which the ancient as well as modern authors have ſpoken of; he ranges them under certain ſpecies, and relates the different names that have been given them, or their *ſynonyma*, which is known to be very uſeful in ſuch ſubjects, and was wanting in this.

He afterwards enters into the philoſophy of the formation of cryſtal, and even undertakes to prove geometrically the neceſſity of the hexagonal figure, which is common to it. M. *Scheuchzer* believes, according to the common ſyſtem, that the cryſtal, as well as the precious ſtones, has been liquid, and formed in ſtones which were ſo likewiſe. He ſeems perſuaded by experience, that there are no more new cryſtals produced. Upon this foundation he conjectures, that when the exteriour *cruſt* of the earth had been extremely ſoftened by the waters of the univerſal deluge, the fluid matter of cryſtal had penetrated it, and gathered together in the cavities and fiſſures of the ſtones, where it congealed by time. We muſt not be ſurprized, that ſo great a confuſion as that which was cauſed by the deluge upon the ſurface of the earth, is an epoch or an origin which frequently recurs in phyſical inquiries.

XIII. *An account of the ſame author's diſſertation, intitled,* Piſcium querelæ & vindiciæ.

In a diſſertation of the ſame author, printed under the title of *Piſcium querelæ & vindiciæ*, and ſent to the academy, the univerſal deluge is more ſenſibly pointed out.

M. *Scheuchzer* has made a ſort of catalogue, of all the ſtones that he knows, like thoſe which we have ſpoken of in the hiſt. of 1703*, and 1706†, that is, which incloſe fiſhes, or rather repreſentations, and at moſt the ſkeletons of fiſhes. We have already ſaid how far theſe ſorts of ſtones were from being, as has been commonly enough imagined, ſports of nature, or fortnuitous paintings; and thus M. *Scheuchzer* introduces the fiſhes complaining that theſe ſtones, which are really their tombs, are taken for meer ſtones, wherein their figures are found engraven by chance; and that theſe curioſities are referred to the *mineral kingdom*, by taking them away from the *animal kingdom* to which they belong. The author is perſuaded that theſe fiſhes buried in ſtones, have been there ever ſince the univerſal deluge, and this ſeems true, eſpecially with regard to thoſe which are found in places, where no other accident could have brought them, and where we cannot believe that there has ever been any water ſince that time. Such is the quarry of *Oningen* in the dioceſe of

* Vol. II. Page 13. † Page 356 of the ſame.

Conſtance.

Conſtance. Several of M. *Scheuchzer*'s ſtones have been taken out of it. The moſt remarkable both for ſize, and the perfection of the figure, is that which contains a great pike, of which there even remains in ſome places petrified fleſh. This proves alſo the reality of the animals if not more ſurely, at leaſt more palpably than thoſe delineations ſo fine and delicate, which have no ſubſtance.

It is not only fiſhes, that M. *Scheuchzer* ſhews in the cabinet of curioſities, which he expoſes to the publick view ; there are alſo two bones of the *vertebræ* of the back of a man, and alſo a feather of a bird found in ſtone, but becauſe there is always found more of fiſh, than any thing elſe, it is they that are the ſpeakers in the common ſubject of *complaint*. It is viſible, that there is nothing but fiſhes, that have been able to remain wrapped up in this deep mud or ſlime, which the deluge left upon the ſurface of the earth, and which afterwards hardning formed different beds. All that was not naturally able to penetrate at leaſt to a certain depth, remained expoſed to the air, or was uncovered ſoon after, and conſequently was deſtroyed. This is the reaſon that there is found a much greater quantity of ſhells than of fiſhes incloſed in ſtones, and almoſt always the heavieſt ſhells. Their weight makes them fall lower in this general ſlime, and that which is found the loweſt, is the beſt preſerved.

XIV. *On the generation of ſnails. Tranſlated by Mr.* Chambers.

The philoſopher that ſhould be reproached with too much application, to the ſtudy of ſuch contemptible things as inſects, might clear himſelf by only aſking, whether the ſmalleſt pieces of God's handy-work are below our concern; but it likewiſe happens, that theſe ſame works which the generality of men have been pleaſed to conſider as the ſmalleſt, are really thoſe where the moſt contrivance, and the greateſt miracles of mechaniſm appear, and if we henceforth prefer inquiries into the ſtructure of the human body, nothing but our intereſt can juſtify us therein.

If a common garden-ſnail be examined out of the coupling-ſeaſon, and its body diſſected with all the care poſſible, nothing will be found therein that ſeems to have relation to generation, and yet as has already been obſerved, in the memoir of M. *Poupart*, this animal is an hermaphrodite, and conſequently muſt have a greater *apparatus* of genital parts, than moſt others. Every thing too that paſſes in it, muſt be of a very ſingular nature; the chief of theſe ſingularities we ſhall here deliver, but without explaining the mechaniſm, whereby they are executed, which we reſerve for M. *du Verney*'s memoir on that ſubject, we ſhall there ſee with amazement how much a ſnail ſtands nature in.

This animal on the right ſide of its neck, has a little almoſt inſenſible cleft, which leads into the cavity of the body, where the inteſtines are found, very winding, and fluctuating, in its belly; but at the time of copulation, all this changes form and the animal is metamorphoſed, almoſt throughout. The little kind of gut being now driven from the bottom of the belly towards the neck ſwells, turns backwards, and diſpoſes itſelf in ſuch manner, as to preſent itſelf to the cleft of the neck, which is now much dilated after the manner of a male and a female part, each ready to do its office; but this does not proceed till after the ſnail has met with another, and by ſeveral preliminary motions more vigorous, and as it were paſſioned than one would expect from ſo cold an animal, they have raiſed each other into a proper diſpoſition, and are aſſured of a perfect underſtanding.

The better to aſſure themſelves of this, they have another very ſingular expedient which they never fail to put in practice together. With the male and female part there iſſues at the aperture of the neck, a kind of ſpear ſhaped like the head of a lance, and terminating in a very acute point; now the two ſnails turning the cleft in their neck towards each other, upon their coming to touch in that place, the ſpear iſſuing from one, pricks the other, and the mechaniſm which plays it, is ſuch, that it immediately hereupon leaves the part it belonged to, and either drops on the ground, or is carried off by the ſnail it has pricked. This ſnail inſtantly withdraws, but ſoon after rejoyns the other, which it pricks in its turn, and after ſuch mutual puncture, the copulation never fails of being conſummated; whereas all the other preludes might have failed. The ſpear emitted on either ſide, ſeems intended to advertiſe the two ſnails, that they are in equal readineſs, for in this hermaphrodite kind, there is not as in our's one principal and active ſex, whoſe diſpoſition alone might ſuffice.

Snails uſe to couple three times at the diſtance of about 15 days from each other, each time of copulation we find a new ſpear, nature being at the expence of producing it, tho' for a uſe ſeemingly of little importance. M. *du Verney* compares this re-generation to that of a deer's head, and in effect there ſeems ſome analogy between the ſubſtance of the one and the other.

After

After emitting, the ſpear follows the reciprocal penetration of the male part of each ſnail into the female one, and as they have each of them the two organs of generation diſpoſed alike, at the orifice of their neck, to make each organ correſpond to its reſpective one, 'tis neceſſary that one of the ſnails have its head upwards, and the other downwards, which they practice accordingly.

Their copulation laſts 10 or 12 hours, and produces eſpecially towards the beginning, either a ſtupor or a tranſport, which hinders their giving any ſign of ſenſe. During all this time they never ſeparate, nor can be brought to it, do what you will; indeed they have a very cogent reaſon for this firm embrace, which is, that the *glans* of the male part growing tumid, cannot get out at the paſſage by which it entered. It may be about an hour arriving at this extenſion and till then no ſeminar matter is emitted.

What is more, the *ſemen* is not yet formed, nor is it till after the copulation is begun that nature ſo much as goes about it, or employs any of the ſtructure neceſſary to provide it. There is a ſingularity likewiſe in the matter of the *ſemen*, which is not fluid, but of a conſiſtence like wax, and aſſumes the figure of the canals it paſſes thro'. It is expelled by a motion like that of the inteſtines, when they evacuate their contents; and during all the time of copulation, except the firſt hour, creeps gently forwards from each ſnail into the other.

The canal it iſſues from is longer than that of the female, which firſt receives it; but from hence it paſſes into other veſſels belonging to the female ſex, where at length it occaſions the fecundification, tho' not immediately after their firſt copulation,

pulation, nor even the ſecond, but only after the third.

At the end of about 18 days they bring forth their eggs, by the aperture of their neck, and hide them in the ground with the utmoſt care and induſtry; and what is further remarkable is, that upon opening a ſnail preſently before it lays, no eggs are found therein, but only a kind of little ligaments, or embryos, which ſwim in a very limpid liquor; and make brisk motions therein. Theſe embryos beome eggs in the road ere they get forth; that is, are inveſted with membranes, which has furniſhed them by certain liquors, and afterwards hardened.

All this is only the natural hiſtory of the generation of ſnails; 'tis only what is done, and not the manner of doing it. If this manner were left to the ableſt naturaliſt to divine, it would doubtleſs prove a very intricate *enigma*; accordingly it is thus far proved almoſt impenetrable, notwithſtanding we have all the pieces of the mechaniſm in our hand, and ſee them played under our eyes.

XV. *Of the eggs of the cuttle-fiſh.*

M. *Saulmon* having procured from the ſea ſome eggs of the cuttle-fiſh in bunches, there was found in all of them a little cuttle-fiſh, very well formed; they were each held by a pretty long ligament to a thick trunk or common cord, out of which all theſe ligaments came, very much twiſted together. We do not take them to be the ſame thing with that which is called *veſicaria marina*, and is believed by the ſailors to be this bunch of cuttle-fiſhes eggs, which the little fiſhes are gone out of, and have left it dried. There is not any remain-

der

der of thefe ligaments of the eggs feen in the *ve ficaria*, at leaft we cannot be affured of them. and the irregular veficles, or grains, which compofe them, feem glued together.

XVI. *On the burning-glaffes of the ancients.*

Altho' the academy does not propofe to make inquiries into antiquity, and is more employed in difcovering what is, than what was formerly thought, or what we may yet add to arts, than what has been practifed, it has however given a great deal of attention to an obfervation of M. *de la Hire*'s, on the burning-glaffes being known to the ancients. The burning mirrours certainly were; for fome hiftorians have pretended that *Archimedes* made ufe of one to burn a fleet, and altho' they attribute to it an impoffible effect, this proves that they were known. But it is certain, that thefe mirrours, which they invented, muft have been of metal, and concave, and had a *focus* by reflection, and we are commonly perfuaded, that the ancients did not at all know the *focus* by refraction of convex-glaffes. Neverthelefs M. *de la Hire* has found them in the firft fcene of the fecond act of the *Clouds* of *Ariftophanes*. *Strepfiades* is a dull, ftupid old fellow, who fays to *Socrates*, that he has thought of a fine invention not to pay his debts.

Strep. *Haft thou feen at the druggift's this fine tranfparent ftone, with which they kindle fire?*

Socra. *Is it not glafs that you mean?*

Strep. *True.*

Socra. *Well, what is it thou will do with it?*

Strep. *When they fhall give me a fummons, I will take this ftone, and putting it to the fun, I will*

will make the whole writing of the ſummons melt at a diſtance.

We ſee plainly, that this writing was drawn upon wax, with which ſome other more ſolid matter was covered. This glaſs, which kindled fire, and melted the wax at the ſun, was not concave; for altho' it had by virtue of this figure a *focus* by reflection, this reflection which is neceſſarily made upward, would have rendered the uſe of it very inconvenient, and very little popular; the ſummons muſt have been held raiſed in the air, that *Strepſiades* might have been able to have melted the writing, and it is not at all natural that he ſhould make this ſuppoſition; whereas, with a convex glaſs that burns downward, we might ſtrike what we pleaſe.

The ſcholiaſt on *Ariſtophanes* ſays upon this place, that it muſt be meant of a *round, thick glaſs, made on purpoſe for this uſe, which was rubbed with oil, and heated, to which a match was adjuſted*, or *brought near*; for the *Greek* expreſſion is equivocal, *and that in this manner the fire was kindled*. We do not very well underſtand what he means by his oil, unleſs it was made uſe of to give a greater poliſh to the glaſs; but in ſhort, which is enough in this place, he imagined this glaſs to be convex, and it is a proof that in his time, which was much later than that of *Ariſtophanes*, it was known that theſe glaſſes burned.

We have no deſign of making here a learned diſſertation, in which it would be ſhameful to have any ſtroke of erudition eſcape. We ſhall only obſerve, that *Pliny lib.* 36 and 37, ſpeaks of *balls of glaſs, and balls of cryſtal, which being expoſed to the ſun, burned either the cloaths or the fleſh of the ſick perſons*, whom they intended to cauterize. *Lactantius*, in his book *of the wrath of god*, says also, that a glaſs ball held to the ſun, kindled fire even in the greateſt cold. Here is the effect of convex glaſſes inconteſtably proved.

But if they knew that they burned, how were they ignorant of their magnifying the objects? For it is difficult to imagine, that an invention ſo agreeable, ſo neceſſary, and ſo ſimple was loſt, even in the greateſt barbariſm, and all the hiſtorical monuments concur in fixing the origin of it toward the end of the 13th century, when they began to diſcover the uſe of ſpectacles. If the *Greek* or *Latin* philoſophers had known this augmentation of objects, would they not have made uſe of them in their inquiries, and would they not have mentioned them in their works an infinite number of times? There would have been even ſpread into their language, as in ours, metaphors, and phraſes taken from them. It is true, that there are two or three paſſages in *Plautus*, which ſeem to prove optick glaſſes; but when we look upon them more nearly, they do not prove them any longer.

Why therefore were they ignorant of the moſt neceſſary uſe of the burning glaſs? In the firſt place, the falſe ideas of philoſophers upon viſion, may have contributed to it. They believed that it was made, either by the flowing of I know not what ſubſtance, which came out of our eyes, and went to ſearch for the objects, or by little repreſentations of objects in miniature which came out of them, and ſought our eyes; all their difficulty was only to chooſe one of theſe two ſyſtems, both equally falſe; they had no ſuſpicion of our pencils, and *foci*, and conſequently they did not ſee any relation between a burning-glaſs and the

manner

manner in which viſion is made, and one of theſe things was not like to conduct them to the other. Beſides, it ſeems that their burning-glaſſes were balls of glaſs, either ſolid or filled with water; and it is demonſtrated by dioptricks, that the *focus* of a glaſs ſphere is diſtant from it $\frac{1}{4}$ of its diameter. If theſe balls were but $\frac{1}{2}$ a foot in diameter, which is the greateſt they can have, we muſt then bring an object within 1 inch $\frac{1}{2}$ to perceive that it was magnified; and it is very natural, and even almoſt neceſſary, that when we have looked through theſe balls, we muſt only have ſeen very diſtant objects, which have not appeared greater, but only disfigured and confuſed; the clear augmentation of diſtant objects requires either very large ſpheres, which is impracticable, and does not fall into uſe, or very ſmall portions of very large ſpheres, which are in uſe at preſent with great ſucceſs, and cannot hardly ever be found by chance, nor eaſy to imagine by reaſoning. Beſides, to know this, the glaſs muſt be worked as we do, and according to all appearance, the ancients only knew how to blow it, and make veſſels of it. It is not therefore ſurprizing, that the knowledge of burning-glaſſes did not carry them farther; it is much more ſo, that there was not 300 years between the ſpectacles and teleſcopes. Every thing is ſlow enough among us, and perhaps we are juſt upon the border of ſome important diſcovery, which we ſhall one day be ſurprized that we were not arrived at.

XVII. *A method of ſtopping horſes ſuddenly.*

It is ſaid to be a known fact, that horſes which run away, ſtop all at once, if there is any thing thrown upon their head which hinders their ſeeing. This being ſuppoſed, M. *Daleſme* has ſhewn a very eaſy manner of diſpoſing two lines, which let fall at once upon the eyes of two coach horſes, two pieces of leather which are on the ſide, in ſuch a manner as immediately to hinder their ſeeing. Theſe cords may be pulled from within the coach, and this would be a very eaſy way of preventing a very fatal accident, and even the fear of it.

AN

AN

ABRIDGMENT

OF THE

PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1708.

I. *Observations on the quantity of rain, which fell at the royal observatory at* Paris, *during the year* 1707, *and on the heights of the thermometer and barometer, by M.* de la Hire *.

I Observed exactly each day of the year 1707, in the east tower of the observatory on a level with the great hall, the heights of the thermometer and barometer, with the quantity of rain which fell, and in the same manner as in the preceding years, and as I have there explained them. But it would be troublesome to relate these observations day by day, therefore I shall only give the result of each month: the height of the rain which fell was in

	Lines.		*Lines.*
Jan.	$4\frac{7}{8}$	July	38
Feb.	10	Aug.	$34\frac{3}{4}$
March	11	Sept.	$9\frac{1}{4}$
April	$4\frac{1}{8}$	Octob.	41
May	$11\frac{3}{8}$	Nov.	6
June	$16\frac{7}{8}$	Dec.	$27\frac{3}{4}$

The sum of the water of the whole year 215 lines or 17 inches, 11 lines.

* Jan. 7, 1708.

Which

Which varies very little from 19 inches, to which we have determined the mean height of the rain water of each year. Neverthelefs we may fay, that this year has been dry, at leaft the fpring, fince it has hardly rained at all in *April*, or $\frac{2}{3}$ of the beginning of *May*; however, the year has been fruitful in corn, as it generally happens in this country, becaufe the greateft part of the ground is cool and moift. The 12th of *Auguft*, there fell 21 lines $\frac{1}{2}$ of water; and during the 4, 5, 6 and 7th of *October*, it rained 34 lines in height with a weft wind without ftorms. There fell fnow on the 5th of *March* only; but it melted very foon, and gave $\frac{1}{2}$ a line of water.

The cold has not been confiderable during the whole year; for my thermometer fell at the loweft but to 27 parts $\frac{1}{3}$ the firft of *Feb.* and in the greateft cold it falls to 31, but very feldom, and it was at 48 at the bottom of the caves of the obfervatory; which we look upon to be the mean ftate of the air. It begins to freeze, when it is at 32; fo that it hardly froze this year, for the thermometer rofe again pretty foon; it was at the loweft at 31 inches only, the 1ft and 30th of *December*.

If the cold has not been great, the heat on the contrary has been exceffive; for the thermometer rofe to 69 $\frac{1}{3}$ the 21ft of *Aug.* the preceding day it was almoft the fame, and towards 3 in the afternoon, when the air is the hotteft, the thermometer marked 82; thus the heat has exceeded the mean ftate 34 parts or degrees, and the cold only 20 $\frac{2}{3}$. From whence we fee, that if the cold had been as great as the heat was in proportion to the mean ftate, the thermometer fhould have fallen to 14, as it fometimes happens; for we fuppofe that

that the ſpirit of wine may dilate itſelf above the mean ſtate, with the ſame eaſe that it contracts itſelf below it.

The reigning wind of the whole year has been between the S. and W. as it is always in this country; and it is that which commonly gives us rain, and in a greater quantity, for it comes from the ſea with regard to us. But in *April* and *May*, the wind was often to the N. and thereabouts.

The barometer, upon which I make the obſervations, is always placed at the top of the great hall of the obſervatory, which is about 22 toiſes above the mean height of the river, and this barometer marked 3 lines $\frac{1}{2}$ leſs height, than another which is at the ſide, altho' they both make light in the *vacuum* by ſhaking the quickſilver. This barometer was at 28 inches, 3 lines $\frac{1}{3}$ the 21ſt of *November*, the higheſt that it was the whole year, altho' the wind was then toward the W. and the sky very ſerene; but the days before and after it tended to the N. This is pretty near the greateſt height that it riſes to here. It fell at the loweſt the 4th of *December*, only to 27 inches, 1 line, which is much leſs than it falls ſometimes, and the wind was then towards the S. W. and with very little rain. I ſhall give in another memoir particular obſervations upon the barometer.

The declination of the magnetical needle was 10° 10′ towards the W. *December* 28, 1707, in the ſame place, and with the ſame needle, of 8 inches, which I always uſe.

II.

II. *A description of a new barometer, to know the weight of the air exactly; with some remarks on the common barometer, by M.* de la Hire *.

In philosophical inquiries, we have very often occasion to know exactly the weight of the air, what it is at a certain time and place, and it cannot be certainly known, but by the means of barometers. But in the simple barometers, which are commonly made use of, and which appear to be the most just, we cannot know exactly this weight, because of the little height of quicksilver which answers to a great height of air.

For as to the heat which dilates the air, or the cold which contracts it, they are only particular accidents in some particular space upon the surface of the earth, which do not increase or diminish the effect of the gravity of the whole mass of air, as may be demonstrated by what follows.

Let † there be the phial A, with the bent tube B D, which is fixed to it at the bottom in B; and let there be also the little capillary tube E F, which is fixed to it at the top. If quicksilver is poured into the tube B D, through the aperture D, it will enter into the phial, and raise itself to the same height as in the tube B D, the air being able to go out by the tube E F, and when there is a little in the phial, we may seal the extremity of the small tube.

Now, if the phial A is immerged in water, luke-warm or a little hot, the air which is inclosed will dilate, and the quicksilver will rise in B D, as to G, by the force of the spring of the dilated air, and it will descend a little, as to the

* March 21, 1708. † Plate II. Fig. 1.

height HH, in the phial, ſo that this dilatation will make it ſuſtain a height of quickſilver HG; and if any body was incloſed in the air of the phial, it would then be in a much thinner air than it was before, and yet this body would be more compreſſed than it was; for it will be beyond that which it was in the open air, by the weight of a column of quickſilver equal to HG, ſince this dilated air makes the ſame effort on all ſides, that it made to ſupport the column of quickſilver HG, and this according to the laws of liquid bodies, and it borrows this effort from the ſides of the phial; but if this thinner air was only in ſome open ſpace about the earth, it muſt be conſidered as being incloſed in a thinner air which ſurrounds it, of which it would borrow its effort, which could only be equal to that of the air, which is on the ſides and above it, and in this caſe, the bodies incloſed in this thinner air would be no more compreſſed, than if they were at the ſame height in the thinner air.

But to know exactly the weight of the air in a certain place, and in a certain time by the means of the barometer, there have been many invented, which give the difference much more ſenſibly than the ſimple barometer; but it does not ſeem to me that it has more conveniencies, or is more juſt than that of M. *Huygens*, which is commonly called the double barometer; perhaps, becauſe of two tubes, and two boxes, or phials, which compoſe it. M. *Huygens* has given a deſcription of it in the *Journal de Scavans* of 1672, which is as follows.

This * barometer is compoſed of two cylindrical glaſs boxes A and B, of equal thickneſs or diameter, of 14 or 15 lines, and an inch in height.

* Plate II. Fig. 2

Theſe boxes are joined by a tube E R of the ſame material, and two lines diameter on the inſide. This tube is bent at the bottom in R, where it joins to the box B. Above this box, there riſes another tube C D, whoſe inſide diameter muſt be but a little more than a line.

There muſt be between the middle of the box A, and the box B, about 27 inches $\frac{1}{2}$.

They fill firſt the box A, and the tube E R with quickſilver, holding it inclined, and having voided all the air that was incloſed with the quickſilver, they raiſe it up to put it in its vertical ſituation, where it muſt remain, the box A being upward, and the box B downward. Then the quickſilver muſt remain toward the middle of the box A, as alſo in the box B; and between the two ſurfaces of the quickſilver in both boxes there will be the ſame difference of height as in the ſimple barometers, which ſhews the gravity of the air with relation to the quickſilver ſuſpended in the box A, above that of the box B.

Afterwards there is common water poured through the tube D, in which there is mixed $\frac{1}{6}$ of *aqua fortis* to prevent its freezing in the winter, or ſome other liquor which is coloured; and there is as much poured in, as quite fills the lower box B, and raiſes the water in the tube to pretty near its middle in G, ſuppoſing the gravity of the air in its mean ſtate.

After this conſtruction, M. *Huygens* adds, that to find how much the differences marked by this barometer, are greater than thoſe that the common barometer can make, there is a general rule, which is, that the proportion of the differences of this new barometer, to thoſe of the common barometer, is as 14 times the ſquare of the diameter of the boxes, to once the ſame plus 28 times

times the ſquare of the diameter of the tube which contains the water.

This double barometer is very convenient for uſe, in its ſhewing the change of the weight of the atmoſphere much more ſenſibly than the ſimple ones; and if they are conſtructed according to the dimenſions and manner which M. *Huygens* propoſes, they will be about twelve times more ſenſible than the ſimple. Nevertheleſs it muſt be obſerved that the exactneſs, which we ought to hope from them, may be a little altered by the difficulty that the air may have to act upon the water of the little tube, and by the height where it may remain ſuſpended above the tube, either in riſing or falling, and this height may even change without the air changing its gravity.

For if the water is deſcended in its tube by the increaſe of weight of the atmoſphere, the little tube being moiſtened in the ſpace which the water has quitted, the water will at firſt ſupport itſelf higher than it ought, becauſe its parts are as it were hooked together, and to the inner ſides of the tube; but it afterwards ſinks a little without there happening any change to the atmoſphere. On the contrary, when the atmoſphere becomes lighter, the water does not riſe faſt enough, nor with the ſame eaſe that it ought, by reaſon that it does not act freely. But theſe cauſes of irregularity in this barometer might not be ſo conſiderable as thoſe of the dilatation or condenſation of the liquor, with relation to the quickſilver in the heat and in the cold, of which I have made very exact obſervations upon theſe very barometers, for two years, as I ſhall relate in another memoir, which muſt increaſe or diminiſh the charge upon the quickſilver of the lower box.

It was to avoid theſe accidents that I formerly propoſed to M. *Huygens*, who was then retired into *Holland*, and with whom I had a correſpondence, to make ſome alterations in his barometer, that I might not undertake any thing upon this ſubject, which might make him uneaſy, and alſo to have his approbation of it, if he thought that the thing deſerved it; and this is the anſwer he made to my propoſal. *Your thought for the double barometer appears to me very good and ingenious, and I ſee that it may be made to mark ſtill greater differences than in mine, by prolonging at the top the tube of water above the tube of quickſilver.* Hague, Aug. 24, 1690.

After that time, I had neglected this invention; but at laſt I have perfected it, and put it in ſuch a ſtate, that it will have pretty conſiderable advantages over the double barometers, as M. *Huygens* has judged; and that they may be made as ſenſible as we pleaſe, and if it is not more ſenſible than his own, it only wants $\frac{1}{9}$ of the quickſilver.

The figure * ſhews the conſtruction of this barometer, which is almoſt like the double barometer; but the boxes A and B are only about 4 lines and half in diameter; the thickneſs of the tube C D on the inſide is but a line in diameter, and the box K, which is joined to the top of the tube CD, is every way like and equal to both the others, but it muſt have a little opening at the top. The three boxes are each 2 inches high, and the diſtance between the middle of the two boxes A and B, ought to be pretty near 28 inch. $\frac{1}{2}$. As for the thickneſs of the tube, which joins the two boxes A and B, it is not at all determined, for this tube ſerves only for communication, and

* Plate II. Fig. 3.

it

it is ſufficient, that it be 1 or 2 lines inſide diameter.

The quickſilver is put into the boxes A and B, as in the double barometer, and from the quickſilver of the box B, to about the middle of the box K, there are two different liquors, which cannot mix together, and are diſtinguiſhed in G, in the little tube C D, towards the middle, when the atmoſphere is of mean weight.

By the conſtruction the three boxes being of equal diameter, they will always have the ſame height of liquor, or the ſame weight upon the quickſilver of the box B, in all its different heights, which will be $\frac{1}{14}$ of B K, which is the height of the liquors above the quickſilver of the box B, in ſuppoſing the liquors ſenſibly of the ſame weight between themſelves, and of the ſame weight as the water, which is 14 times lighter than the quickſilver, according to M. *Huygens*; for the liquor will riſe or fall as much in the box K, as the quickſilver in the boxes B and A, but it will riſe in A, when it falls in B, and on the contrary.

In the alterations of the height of quickſilver or weight of air, it is evident, that the lower liquor G B will act as in the double barometer.

My barometer may be uſed as the ſimple barometer, by ſticking a little ſlip of paper divided into $\frac{1}{2}$ lines upon the upper and lower boxes, which marks the entire lines of the height of the quickſilver, which anſwers to the weight of the atmoſphere; ſo that we may always compare the true alteration of the atmoſphere, with that which ſhall be marked by the lower liquor, and inſtead of dividing the height of the tube C D into parts at pleaſure, which have no proportion to the height of the atmoſphere, as is commonly done, divide it into two parts, which ſhall repreſent the heights of the atmoſphere, in lines of quickſilver, which may eaſily be done.

For example, having found that the difference of the height of the quickſilver in the boxes A and B, when the atmoſphere is light, and the air at a mean ſtate of heat, is 29 inches, 2 lines; and knowing the proportion of the weight of quickſilver to that of each of theſe liquors, and alſo their proportion of weight to the quickſilver according to their heights, which is 2 inches, 2 lines, take away 29 inches, 2 lines, and the remainder will be 27 inches for the true height of the quickſilver, which ſhews the weight of the atmoſphere in this ſtate, and in the time of obſervation.

This is the reaſon that there muſt be writ overagainſt the point M, where the lower liquor is, in its tube, this height of 27 inches; and place alſo upon the boxes A and B, the two little ſlips of paper divided into $\frac{1}{2}$ lines, ſo that their diviſion which ſhall be marked at the height of the quickſilver be marked 27 inches.

If you have no regard to the alterations of the bulk of the liquors and quickſilver in cold and heat, with regard to the mean ſtate, nor to the different heights of the lower liquor, which is a little heavier than the other, it is evident that the quickſilver will fall or riſe in its boxes, in the manner that I have before ſaid; for the liquors which we ſuppoſe very near of equal weight, will always charge equally the quickſilver of the lower box. We need therefore only know the motion of the lower liquor in its tube, in proportion to the motion of the quickſilver in its boxes. This may be found by experiment, if, after the firſt obſervation when the atmoſphere was light, there,

there be another made when it ſhall be heavy, the air being pretty near the ſame ſtate of heat: for we ſhall have upon the diviſion of the boxes the height of the quickſilver, which anſwers to the weight of the atmoſphere; and ſuppoſing it to be an inch, that is to ſay ½ an inch upon each box, mark the height of the liquor, as in Z, 28 inches; and divide the ſpace M Z into the number of lines, which have been obſerved for theſe two points M and Z, which is here 12, and continue theſe diviſions above M and below Z, which is eaſy to underſtand. We ſuppoſe, that the tube and the boxes are of equal thickneſs all the way; if not, for greater juſtneſs we muſt find by experiment other points of height of the liquor in different weights of the atmoſphere.

We ſee by what has been juſt explained for the diviſion, that if the true height of the quickſilver be known that anſwers at one time, as that which is here above marked, to the weight of the atmoſphere, there need only be at firſt put the little ſlips of paper upon the boxes A and B, which mark this height over-againſt the ſurface of the quickſilver, and alſo the ſame height at the ſide of the tube C D, over-againſt the ſurface of the lower liquor, and we have only to know the proportion of the weight of the liquors to that of the quickſilver, and the reſt is done of itſelf.

Obſerve that the *aqua ſecunda* made with ⅙ of *aqua fortis*, is to the quickſilver in weight, according to the obſervations of M. *Homberg*, as pretty nigh 1 to 12, which is alſo the proportion of oil of tartar, that is put into the double barometer, as I have found by the examinations that I have made.

There only remains for me to examine what muſt happen to my barometer, by the dilatation and

and condensation of the liquors, and of the quicksilver in the great cold and heat. By the experiment which I proposed before, where I took the mean state of the heat of the air, we found divisions with which we might be contented, if a great exactness was not required; and so much the more, as there being but little quicksilver and liquor in the boxes of this barometer, the alteration of heat and cold, beyond the mean state, can cause no great difference; yet we may draw two lines parallel to M Z on each side of it, and very near to it, and there mark also by experiment, in the great heat and cold, the divisions which shall answer to the heights of the quicksilver of the boxes, and do also the same thing for the divisions of the boxes; for as to the different constitutions of the air between the extremes and the mean, it will not be difficult to judge of them.

Lastly, oil of tartar may be put for the lower liquor, as in the double barometer; and spirit of wine, or oil of *petroleum*, for the upper; which I believe to be more proper than putting spirit of wine at the bottom, and oil of *petroleum* at the top, because the oil of tartar alters its bulk less than the spirit of wine by heat and cold; nevertheless the spirit of wine approaches nearer to the weight of the oil of *petroleum*, than to that of the oil of tartar. But there must be a mark made upon the box K N, where the upper liquor is in a certain disposition of the air and of the atmosphere, to know afterwards how much this liquor will be diminished by evaporation, and to put in again as much as there was at first; but the aperture of this little box may be lightly stopped, which will not hinder the air from acting upon the liquor; and even a small slender tube may be applied

applied to the top of this aperture, which will preſerve the liquor longer.

III. *Reflections on the variation of the needle, obſerved by the* Sieur Houſſaye, *captain commandant of the ſhip* L'Aurore, *during the expedition to the* Eaſt-Indies, *made by the ſquadron commanded by the baron* de Pallieres, *in* 1704 *and* 1705, *by M.* Caſſini *the ſon* *; *tranſlated by Mr.* Chambers.

A journal of the obſervations of the needle, made by M. *Houſſaye*, captain of the ſhip *Aurora*, in a voyage to the *Eaſt-Indies*, in the ſquadron commanded by the baron *de Pallieres*, was ſent by the commiſſioner of the marine in the eaſt to the count *de Pontchartrane*, according to the orders which he had received for that purpoſe. This officer, who has acquired great experience in eight ſeveral voyages to the *Eaſt-Indies*, not only relates the obſervations which he made in his laſt voyage, but alſo compares them with thoſe he had made in ſeveral places in his former ones, with deſign to ſhew the increaſe or diminution, to which the variation is ſubject in length of time. He has alſo taken care to note the obſervations, which he made in the ſight of the capes, iſlands, and coaſts in his paſſage, and informs us, that he made uſe of the *Mercator's* chart of *Pieter Goos*, where the firſt meridian paſſes thro' the pike of *Teneriff*. ——— Having therefore a copy of this author's chart of the *Eaſt-Indies*, we had an opportunity of comparing his obſervations with the variations laid down in

* April 25, 1708.

in Dr. *Halley's* chart, allowing for the difference of longitudes between the two.

At their departure from *Port Louis* on the *French* coaſts, the variation of the needle was found 5°—— north-weſt; in Dr. *Halley*'s chart it is laid down as 6°—½ north-weſt.

At 357 —long. and 22° north lat. the variation was found 0; where in Dr. *Halley*'s it is noted as 1°—½ north-weſt.

At 353°—45′ —long. and 16°——30′— ſouth lat. the variation was found 2°——½ north-eaſt; where in Dr. *Halley*'s chart it is noted as 3°——½ north-eaſt.

At 354°—— long. and 18°—— ſouth lat. the variation was found 3°———½ north-eaſt; where in Dr. *Halley*'s chart it is 3 ——¼ north-eaſt. —— This ſame variation of 3°—½ north-eaſt, continued as far as 23°— ſouth lat. Under the ſame longitude of 354°— where in Dr. *Halley*'s chart it is made 4°——⅔ north-eaſt.

In theſe places M. *Houſſaye* obſerves the variation in 1682 was found 11°—— north-eaſt, ſince which time it has conſtantly diminiſhed, ſo as now only to be 4 or 5°———

The greateſt variation north-eaſt, which he found in this voyage was 6 ——— the longitude being 367 — and the latitude 28 — ſouthward, in which place it is repreſented in the chart as ſomewhat under 5 — north-eaſt. This variation diminiſhes as you proceed eaſtward, and at length turns to the north-weſt; ſo that within ſight of the *Cape of Good Hope*, and along all the coaſts of *Angola*, as far as *Bengal*, it is 9 or 10°— north-weſtwards; and is laid down accordingly in Dr. *Halley*'s chart.

On the weſtern ſide of the bank *Des Aiguilles*, the variation was found 12°— north-weſt, and

on

on the eastern side of the same bank 13°—½ to 14°—— and is represented much the same in Dr. *Halley*'s chart.—— In 1680, M. *Houssaye* observes the variation at the *Cape of Good Hope* was only from 7 to 7°—½ north-west, since which time it has been continually increasing, as well as at the bank *Des Aiguilles*.

Through the whole channel of *Mosambicque*, from 25°— south lat. as far as within sight of the bay of St. *Augustine*, in the island of *Madagascar*, the variation is found from 22 to 23°—— north-west; and in the year 1682 was found from 18 to 19°—— Dr. *Halley*'s chart for the year 1700, represents it in the bay of St. *Augustine*, as 21°—¾, which is somewhat less than it was observed in 1704, as it ought to be, by reason of the annual increase of the variation in this place.

Within sight of the island of *Juan de Nour*, the variation was found 22°— north-west, where in the chart it is 20°——⅓ north-west.

Within sight of the islands *Mayotte*, *Amzuam*, and *Moely*, the variation was found 20°—30′— north-west; formerly it was only 18°—— and in Dr. *Halley*'s chart is 20°— north-west.

Under the line at 70°——long. the variation was found 16°— north-west; where in Dr. *Halley*'s chart it is 17°—⅔ north-west.

At 87°— long. and 15°— north-lat. the variation was found 10°—30′ north-west; where in Dr. *Halley*'s chart it is 12°— north-west.

Within sight of *Canary* at 16°——30′ north lat. and all along the coasts of *Malabar*, the variation was found 6°———30′—— north-west, where the chart makes it 8°— north-west.

At *Cape Comorin* the variation was found 7°— 30 —— north-west; in the chart it is 7°——⅔ north-west.

Within ſight of *Point Galle*, in the iſland of *Ceilon*, the variation was found 5°——½ north-weſt in the chart it is 6°——½.

Near the coaſt of *Coromandel*, the variation was found 5°—— north-weſt, exactly the ſame as in the chart.

In the iſlands *Andaman* and *Nicobar*, the variation was found 3°— north-weſt, preciſely as in the chart.

Within ſight of the iſland *Diego Rodrigues*, the variation was found 16°—30′ north-weſt, which in the chart is 19°—— north-weſt.

Within ſight of the iſland *Maurice*, the variation was found 21° ——— north-weſt, where the chart gives it 20°——½ north-weſt.

Within ſight of the iſland *Bourbon*, the variation was found from 21°——½ to 22°—— north-weſt, where the chart gives it 21°——— north-weſt.

At 74°——long. and 25°— ſouth lat. the variation was found 23°—½ north-weſt, where the chart gives it 22°—½ north-weſt.

At 72°—45′ long. and 27°—15′ ſouth lat. the variation was found 24°—30′— where the chart gives it 23°—north-weſt.

The ſame variation continued as far 65°—— 45′—— long. and 33°—10′ ſouth lat. where the chart makes it 23°—⅔ north-weſt.

From this place the variation continually diminiſhed, as they proceeded towards the *Cape des Aiguilles* about the middle whereof at 35°—— 30′ lat. the variation was found 13°—— north-weſt, and within ſight of the *Cape of Good Hope*, and all along the coaſts of *Angola*, as already mentioned, from 9°—½ to 10°— north-weſt; in Dr. *Halley*'s chart the variation at the middle of the bank *Des Aiguilles* is 12°—

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At the *Cape of Good Hope* ſomewhat above 10°— and along the coaſts of *Angola* from 9½ to 10°———

As you proceed hence towards the iſland of St. *Helena*, the variation gradually diminiſhes; ſo that within ſight of that iſland, on the eaſtern ſide, it was found 1°—— or 1°——½ north-weſt, where the chart gives it ſomewhat above 1°——— north-weſt.

At the iſland of *Aſcenſion*, there is no variation, or at moſt not a degree north-eaſtwards; the chart gives it ⅓ of a degree north-eaſtwards.

Proceeding hence for *France*, as you paſs the line at 357 or 358° of longitude, there is no variation. The chart gives it a variation of ½ a degree north-eaſtwards.

As we approach the *Azores*, the needle begins to decline north-weſtwards; ſo that within ſight of the iſlands *Corva* and *Flora*, we find a variation of 4°——to 4°——30′——— north-weſtwards, where the chart gives it 5°———⅓ north-weſtward.

As we approach *Terre-Neuve*, the variation increaſes to 7 or 8°— and at length on the coaſts of *Britany* dwindles to 5°— north-weſt, as was obſerved at our departure from *Port Louis*.

Many of theſe obſervations agree exactly with thoſe of Dr. *Halley*'s chart, and the generality of them only differ about a degree, which muſt be allow'd a very great pitch of accuracy, conſidering the difficulty of making exact obſervations of the variation at ſea. This difference may alſo in ſome meaſure ariſe from the annual change found in the variation of the needle, which increaſes in ſome places and diminiſhes in others, as appears from theſe obſervations.——— For at 354°— longitude, and 20°— ſouthern latitude, the variation,

which

which is north-eaſt, has in 22 years dwindled from 11°— to 5. At the *Cape of Good Hope*, the variation, which is north-weſt, has in 24 years increaſed 2 or 3°—— and in the channel of *Moſambicque*, near the bay of St. *Auguſtine*, it has in 22 years increaſed 4 or 5°—.

IV. *Experiments and obſervations on the dilatation of the air by boiling water, by M.* de la Hire *; *tranſlated by Mr.* Chambers.

M. *Amontons* had long diſcovered by experiments, that the heat of boiling water can only dilate air to a certain pitch, whatever degree of fire be employed to make it boil; when he propoſed to the academy a new thermometer, whereby to diſcover the relation between the heat of air over the whole earth.

His experiments were chiefly made with a machine very ingeniouſly contrived, tho' ſomewhat compound and difficult of application, by means whereof he compreſſed the air, in a glaſs phial, with 27 inches of mercury, beyond its natural compreſſion from the weight of the atmoſphere. This phial was joined to a crooked glaſs tube, wherein was mercury 27 inches above that in the phial, the uſe of his machine was to bring the mercury to this height, then he plunged the phial with its crooked tube in cold water, which at laſt he placed over the fire till it boiled vigorouſly; and this experiment being performed before the academy, it was obſerved, that after the water boiled, the mercury ſuſtained in the tube roſe no higher, tho' the fire was conſiderably increaſed, than it did when it firſt began boiling.

* July 24, 1708.

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This experiment I thought very curious, but did not conceive why he made it with air compressed with 27 inches of mercury beyond its natural load. To conclude from thence, that the air, ſuch as on the ſurface of our earth, without further compreſſion than that of the weight of the atmoſphere, dilates itſelf by boiling water about $\frac{1}{3}$ of its former bulk, ſince in theſe concluſions ſeveral things muſt neceſſarily be ſuppoſed about the nature of air, whereof we have no ſatisfactory knowledge.

The firſt experiments made by M. *Amontons*, led him inſenſibly on to execute what he had projected, without giving him room to think of another ſimpler, and conſequently juſter method of attaining it. This was what induced me to make the following experiments of the dilatation of air, and its force, when heated by boiling water, to ſuſtain a certain height of mercury without introducing any foreign preſſure, more than what ariſes from the weight of the atmoſphere, at the time and place of experiment. I took a glaſs tube * A B C, bent in B, and to the extremity thereof C, faſtened a phial D 2 inches in diameter, the tube was open in A, and its diameter about $\frac{3}{4}$ of a line on the inſide. Thus far agrees with the phial and tube uſed by M. *Amontons*; but it being impoſſible to pour mercury into the tube, without compreſſing the air in the phial, I faſtened another very ſlender tube E F over the phial, which opening into it, let the air eſcape in proportion as the mercury entered the tube A, till having poured the mercury into the tube ABC, about 2 lines higher than the aperture of the leſſer tube in the phial, I ſealed the extremity F of this leſſer tube, the mercury being now

* Plate II. Fig. 4.

at the ſame height in the phial as in the tube AB, and conſequently the air in the phial, no more compreſſed than the external air, which M. *Amontons* had not been able to attain, in pouring his mercury into the tube, as he himſelf confeſſes in the memoirs of 1699, which was doubtleſs what led him to compreſs it with 27 inches beyond its natural load, to make its compreſſion about double of what it uſually is.

I obſerved at the ſame time the height of the barometer, which was 27 inches, 7 lines ½, and the thermometer ſtood at 42 degrees, which is always at 48 in the vaults of the obſervatory, and makes what I call the mean ſtate of the air between heat and cold, the weather being moiſt with a ſouth wind, and the day the 11th of *December*, 1705.—— Without more ado, I put the phial in water, and the water over the fire, till making it boil violently, the mercury roſe 8 inches, 5 lines in the tube A B above that in the phial; but the third of 27 inches, 7 lines ½, is 9 inches, 2 lines ½, and conſequently the air in the ſtate it was in, before its being dilated by the heat of the water, did not ſuſtain a height of mercury equal to ⅓ of the weight of the atmoſphere, but leſs by 9 lines ½.

This operation I repeated on the 16th of *Feb.* 1706, with the ſame phial as before, wherein the mercury had been left ever ſince, the little tube ſtill continuing ſealed; but this time the thermometer only ſtood at 38 degrees, and conſequently the air in the phial was more contracted than in the former experiment, when the weather was warmer, and beſides the barometer now ſtood at 28 inches, 5 lines, and conſequently the atmoſphere was 9 lines ½ of mercury heavier than before. On both theſe accounts, the mercury

ſhould

ſhould have fallen in the tube were it was left, and was found accordingly 1 inch, 6 lines lower than that in the phial.

Opening therefore the end of the little tube, to give room for the external air to preſs upon the mercury in the tube, it preſently roſe to the ſame height as that in the phial; then ſealing the little tube anew, I put the phial in water, which I made boil; but found that the mercury now only roſe 8 inches above that in the phial, which is 5 lines leſs than before, and 14 lines $\frac{1}{2}$ leſs than $\frac{1}{3}$ of the weight of the atmoſphere.

And yet as the air was colder and heavier, and conſequently a greater number of its ſpringy particles contained in the ſame compaſs of the phial, the heat of boiling water, which was the ſame in both experiments, ſhould rather have increaſed its effect, and made it ſuſtain a greater height of mercury; but the contrary being found, we muſt of neceſſity confeſs, that the nature of the air is unknown to us, unleſs we ſuppoſe that the weight of the atmoſphere acting on the mercury in the tube had more force to depreſs the air in the phial than the boiling water had to make the mercury riſe, by opening and unfolding the ſprings of the air incloſed in the phial.

'Tis true, that according to the ſuppoſition of M. *Mariotte*, which M. *Amontons* makes uſe of, to infer the dilatation of air by boiling water, to be $\frac{1}{3}$ more than what it naturally is, *viz.* That the ſprings of air are compreſſed in the reciprocal ratio of the weights, we ſhould always find the ſame ratio between the weight of the atmoſphere, and the weight of mercury raiſed in the tube, as between the compreſſion of the air by the weight of the atmoſphere, and the effort made by the ſame mercury in the tube to compreſs the quantity of air firſt contained in the phial, which effort makes what we call the dilatation of the ſprings of air by boiling water to ſuſtain a weight, tho' in reality theſe ſprings be not dilated; for the heat of boiling water acting on the air incloſed in the phial, makes no ſenſible alteration in its bulk, while it obliges the mercury to riſe a certain height in the tube, to make an equilibrium therewith. 'Tis therefore this height of mercury in the tube, that always balances the effort of the boiling water on the air in the phial; ſo that this air in the phial muſt now be conſidered as compreſſed by the weight of the atmoſphere, and the height of the mercury in the tube likewiſe, tho' before it was only compreſſed by the weight of the atmoſphere; and as the bulks of the air in the phial are to be in the reciprocal ratio of the incumbent weights, it will amount to the ſame as introducing into the phial, where the mercury undergoes no ſenſible change of height, a quantity of air compreſſed by the two cauſes, the weight of the atmoſphere, and of the mercury in the tube, which had the ſame ratio to the quantity of natural air in the phial; and this air likewiſe compreſſed by the two former cauſes, as the weight of the mercury in the tube would have to the weight of the atmoſphere over the ſame baſe. ——For an inſtance.

Suppoſing the weight of the atmoſphere equal to 27 inches of mercury, the height of the mercury in the tube 9 inches, and the capacity of the phial 4 inches, which laſt we ſuppoſe full of air, compreſſed by the weight of the atmoſphere, before the mercury riſes in the tube. When the mercury is riſen 9 inches in the tube, the phial ſtill remaining full of air, this air muſt be compreſſed therein beyond what it was before in the reciprocal

cal ratio of the incumbent weights, which are as 27 to 36, or 3 to 4; ſo that it amounts to the ſame thing, as if an inch of this compreſſed air had been introduced into the phial, which inch of compreſſed air would be the meaſure of the effort, with regard to the 3 inches, into which the air of the whole phial would be reduced, which would balance the 9 inches height of mercury in the tube; whence it follows, that this ſuppoſed quantity of air introduced into the phial, which is the meaſure of the effort of boiling water on the air in the phial (it being the boiling that makes this effort) will always bear the ſame ratio to the quantity of air naturally compreſſed in the phial, as the height of mercury in the tube bears to the height of mercury, which balances the weight of the atmoſphere.

Examining therefore our two experiments by this rule, we ſhall have for the firſt effort of the boiling water, with regard to the weight of the atmoſphere, 8 inches, 5 lines, to 27 inches, 7 lines $\frac{1}{2}$, which is nearly as 10 to 33; but for the ſecond, we ſhall have it as 8 inches to 28 inches, 5 lines, which is nearly as 10 to 35 $\frac{1}{2}$; whence it appears, that this ratio is far from $\frac{1}{3}$ of the weight of the atmoſphere; and farther in the ſecond experiment than the firſt. Accordingly M. *Amontons* does not call it $\frac{1}{3}$; for he only learned it by induction but nearly $\frac{1}{3}$.

All our reaſonings hitherto upon the dilatation of air by boiling water, is founded on the two known properties of air, *viz.* its being a fluid, and its parts being ſpringy; for as to its weight, it need not be regarded in theſe experiments, where its height in the phial is ſo inconſiderable; ſo that all the properties of fluid and ſpringy bo-

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dies may be attributed to the air in theſe experiments.

Hence the mercury ſhould only riſe in the tube to a certain height, where it has ſufficient force to bend, or ſtrain the ſprings of the air, to render it a balance to itſelf, which height will be the ſame upon the ſurface of the mercury contiguous to the compreſſed air, whether we ſuppoſe a great deal of air, or a multitude of ſprings, or only a few, for the ſprings will ſuſtain each other, and are all ſuſtained at laſt by the parietes of the veſſel they are contained in.

This appears the more probable, as in taking one of theſe phials with its tube ABDE *, and pouring mercury into it by the tube E D, till it riſe to E in the tube D E, which is open, and only to F in the tube D B, which is faſtened to the phial A B underneath B. 'Tis certain, that the air in the phial, and in the part B F of the tube BD, will be compreſſed more than the external air, as being loaden with a height of mercury E F; and in this caſe, if the whole phial be taken away, or only its communication with the tube B D in B be ſtopped, 'tis eaſy to infer, that the mercury will ſtill remain in F, and neither riſe nor fall in the tube B D, tho' the compreſſed air in B F have no longer any communication with that in the phial, which is compreſſed likewiſe by theſe experiments, therefore it appears indifferent, whether the phial be ſmall or great compared to the thickneſs of the tube.

Nevertheleſs as neither the contraction nor extenſion of ſprings is infinite, but both of them have their bounds, it follows, that ſtrictly ſpeaking, they muſt not obſerve the ratios of the incumbent weights, even for a little change of weight; hence we have room to ſuſpect, that

* Fig. 6.

this

this cauſe alone may make ſome alterations in the experiments of the compreſſion and dilatation of air, and as in ſuch a fluid as air, compoſed of ſpringy particles, there may be ſome particular property unknown to us, which may hinder its acting after the ſame manner as other fluids. I have endeavoured to make ſome diſcovery hereof, and with this view contrived the following experiment, which tho' it bears ſome reſemblance to the former, is very different in the proportion of the tube to the quantity of air to be dilated by the boiling water.

I took the glaſs tube * ABC, bent like a ſyphon, one branch whereof AB was 15 inches long, and the other BC only 8, its extremity was drawn into a capillary tube CF, and the inner diameter of the ſyphon was $\frac{1}{4}$ of an inch.

The ſyphon being inverted, I poured mercury into it, which riſing equally in both branches of the ſyphon, I only left 3 inches height of air in the ſhorter branch, *viz.* from D to C, then ſealing the extremity F of the capillary tube, I inſtantly plunged the tube in water, which I made to boil. Upon this, I found that the mercury in the long branch AB, only roſe 1 inch, 8 lines $\frac{1}{2}$ above the level of what was firſt in the ſhort branch BC; but the mercury now fell as faſt in the ſhort branch, as it roſe in the long one, which was open a-top, and conſequently the mercury roſe 3 inches, 5 lines; and the long branch above that in the ſhort one: when the boiling water had dilated the air contained in it, the barometer then ſtood at 28 inches, 3 lines, and the thermometer at 36 degrees $\frac{1}{2}$.

Now this experiment where the 3 inches height of air contained in the tube BC, repreſent a little

* Fig. 5.

phial

phial, with regard to the large tube AB, wherein the mercury rose, gives nothing like what we learned from the two former; but as the air dilated by the boiling water, possessed a greater space than it did before, which was not found in the former experiments, it could not here sustain so great a height of mercury, as it did there; and if we enquire by the rule of the air's being compressed in the reciprocal ratio of the weights, what quantity of mercury must be added to the long tube AB; to reduce the air, heated or dilated by the boiling water, to its former bulk of 3 inches, we shall find upwards of 21 inches required; for it will be as three inches of air contained in the tube are to 31 inches 8 lines, which is the weight of the atmosphere, with double the dilatation of the air in the close tube; so are 4 inches 8 lines $\frac{1}{2}$, which is the whole air dilated in the close tube to the height 49 inches 8 lines $\frac{1}{2}$, from whence substracting the weight of the atmosphere 28 inches, 3 lines, and likewise the fall of the mercury in the close tube, which is 1 inch, 8 lines $\frac{1}{2}$, the remainder is 19 inches, 9 lines, the height of mercury in the open tube, above that in the other tube, required to reduce the air in the close tube, which is dilated by the boiling water to its first bulk of 3 inches, and yet it should only be about 9 inches $\frac{1}{2}$, which is $\frac{1}{3}$ of the weight of the atmosphere. Hence therefore I learn, that the quantity of the inclosed air, upon which the boiling water acts, may occasion a great diversity in the result of these experiments, and it would even seem to follow, that a little quantity of air, dilated by boiling water, becomes more forcible than a great one.

Another experiment I made with regard to what M. *Nuguet* had published in the *Memoirs de Trevoux*

voux for *Oct.* 1705, obſerving what M. *Amontons* had advanced in the memoirs of the academy, that the air is dilated, by the heat of boiling water, $\frac{1}{3}$ of its natural bulk, he made three ſeveral experiments to be ſatisfied of it. ——— By the firſt, he found that air naturally compreſſed, as upon the ſurface of the earth, is dilated by boiling water in ſuch manner, that the ſpace it now poſſeſſes is to its natural ſpace, as 2 to 1, or 4 to 2, and not as 4 to 3, according to M. *Amontons*; and he obſerves very judiciouſly, that the air, in his experiment, was not dilated to its utmoſt extent, by reaſon part of this dilated air was encompaſſed with cold water, but makes no mention of another cauſe, which likewiſe prevented its dilating, *viz.* the weight of the cold water, which had roſe above a hole, made in the bottom of the phial immerged in the water.

M. *Nuguet*'s ſecond experiment was ſomewhat different from the former; and by this he found the dilated air to the natural air, as 16 to 1; but as he does not regard the height of the water in the boiler, whereby the air was dilated by means of a hole at the bottom of the phial, ſo great a dilatation muſt neceſſarily have enſued.

His third experiment likewiſe gives the ratio of dilated to natural air, as 16 to 1; but I do not conceive how he could make it after the manner he relates; for the cold water no ſooner enters the phial plunged in the boiling water, than the phial ſhould break.

He obſerves upon theſe three experiments, that the firſt is very wide from the other two, which could never have roſe from the ſingle cauſe aſſigned by him.

The laſt of them I repeated with all the circumſtance he mentions, and found that the bulk

of

of the air, dilated by the heat of boiling water, was to that of natural air as 5 to 2, or as 2 $\frac{1}{2}$ to 1 nearly, which is very far from 16 to 1, as found by him.

The great difference among these experiments shews, that there are some circumstances not attended to, which may produce great effects in the nature of air, and that we must be warned else, from drawing any general consequence from a few particular observations, and condemning others, drawn from observations in the same case; what then occurred to me, as to the reason of the difference between M. *Nuguet*'s observation and mine is as follows. —— M. *Nuguet* used a little phial, which only held 2 ounces, 7 drachms $\frac{1}{2}$ of water, whereas that I used, held 25 ounces, and as we can never judge so well from an experiment in little as in large, there might some diversity arise from this quarter. I also observed from M. *Nuguet*'s account of his observations, that he first filled his phial with water, and then emptying it, put it in the boiling water to dilate its air; now the little water, which might be left therein, being raised into bubbles, which would be put into a violent motion by the heat, I fancied, might not only extend the springs of the air, but that possessing a large bulk, they might have carried off, as they issued from the phial, most of the air contained therein; as we find in eolipiles, which blow so vehemently for a considerable time, till no more water is left in the bowl. By this means only a little air must have been left in M. *Nuguet*'s phial; whereas that which I used being first well dried, the heat had nothing to act on, but the air contained in it; but as all air abounds more or less with watry particles, if this effect had any place in these experiments, we we should always find great differences in those made like the two former at different times, when the air was probably more replete with water at one time than another: from whence thoſe of M. *Amontons* were exempted, by reaſon they were made with three different phials at the ſame time. This induced me to believe, that the moiſture of the air, when heated by boiling water, might poſſibly make conſiderable differences as to the dilatation of the air, tho' it could not get out of the phial, as being retained by the mercury.

But being aware how wide our reaſonings frequently are from the truth in phyſical matters, I reſolved to repeat the experiment I had made upon the dilatation of air by boiling water in a phial, and immediately after to make another with the ſame phial, with a little water in it, either to confirm or overthrow the notion I had conceived about the difference between our experiments. ——— Accordingly the 18th of *July*, 1708, the barometer ſtanding at 28 inches, and the thermometer at 55 degrees, which in the vaults of the obſervatory ſtood at 48. The wind being weſterly and very moiſt with a little rain, I took a new glaſs phial, as dry as the conſtitution of the air would allow of, and weighing it, found it 6 drachms $\frac{1}{2}$; then ſtopping it well with a cork, thro' which I had put one of the legs of a ſmall glaſs ſyphon, I cemented it well to the cork with ſealing-wax, leaving the other leg of the ſyphon on the outſide. This phial I put in cold water, holding it down, ſo as both the cork and the ſyphon were immerged, taking care only to ſink the mouth of the phial a little below the ſurface of the water, for fear the water ſhould make way by its weight into the phial.

 This

This water being placed over a good fire, I prefently perceived a multitude of little bubbles begin to arife from the end of the fyphon, which fhewed that the air in the phial was beginning to dilate, and iffue at the end of the fyphon, by the heat it had conceived from the fire; but as the water heated more and more, the air bubbles rofe from the fyphon with more precipitation, which continued till fuch time as the water boiled out right, when there were bubbles ftill feen to arife, tho' much lefs than before.

After the water had boiled fome time, I took it off the fire, keeping the end of the phial and fyphon ftill under water, that as the water in the copper, and the air in the phial fhould come to cool, no particle of air might get into the phial, either by the fyphon, or any little pores, that might be found in the cork; and to fhorten the operation, I laded fome of the hot water off, and fupplied its place with cold, which was continued till the water was entirely cooled; then taking the phial out, I found a good deal of water had entered it, while we were waiting for the cooling, and as a mark, that the air left in the phial, was of the fame denfity as the external air, a little water was left in the part of the fyphon, which traverfed the cork, and was fufpended and counterbalanced within the air in the phial and the external air.

Taking out the cork therefore and the fyphon, and wiping the phial well on the outfide, I found it weigh with the water in it 4 ounces, 2 drachms, then filling it with water to the height, whereat the bottom of the cork had been, which was equal to the bulk of air it contained when I put it in the water, I found it weigh 5 ounces, 2 drachms; fo that the air left in the phial, was equal

equal to an ounce of water, and from 5 ounces, 2 drachms, the weight of the water in the whole phial, with the phial itſelf, ſubſtracting the weight of the phial 6 drachms ½, as I found it at firſt. The remainder is 35 drachms ½, which is equivalent to the whole air in the phial, when I put it in water.

Hence I infer, that the whole air of the phial, naturally compreſſed by the weight of the atmoſphere, was to that which remained after its dilatation by boiling water, as 35 ½ to 8, which is ſomewhat leſs than 4 ½ to 1. Yet is this dilatation much greater than what I had found before, which was only as 2 ½ to 1: hence as the air was very moiſt in this laſt experiment, I had reaſon to imagine, that the particles of water diffuſed thro' the air, might be the occaſion, as I had before ſuſpected of this extraordinary dilatation; for further ſatisfaction therefore I inſtantly proceeded to my laſt experiment, as I had before reſolved.

I poured the water out of the phial, and contenting myſelf to ſhake it well without drying, I weighed it as before, and found it 6 drachms ½, and 11 grains; ſo that there were 11 grains of water ſticking to its inſide, then fitting in the cork and ſyphon, I repeated the experiment as before, without omitting the leaſt circumſtance. The reſult was, that the phial was found quite full of water, and that the ratio of the capacity of the phial to the remaining part, not poſſeſſed by the water, was as 35 ½ to 1, as I found by weighing as before. Hence I can no longer make any doubt, but that a little more or leſs water in the air, may occaſion great variations in theſe experiments, ſince bare 11 grains of water in the preſent one, produced an effect 8 times greater than in the former experiment.

But tho' the phyſical account ſhould be diſallowed, yet the experiments will ſtill ſtand inconteſtable, whereby ſuch different dilatations of air by boiling water are produced ; ſo that we may at leaſt infer hence, that no exact ſtandard of heat over the whole earth can be had by this method, not even with uſing phials and tubes, like that I firſt uſed, and which differs but little from thoſe of M. *Amontons*, which are hardly portable.

Upon the whole, were it not better, in lieu of this contrivance, to ſubſtitute good ſpirit of wine thermometers, all graduated alike by careful experiments, without minding thoſe equal diviſions commonly placed on them, which are of no ſervice for making an exact compariſon ; ſince there is no knowing whether the inſides of the tubes thro' their whole length, nor the proportion of the bowl to the tube ? All required to this end, is to make ſeveral ſuch thermometers nearly alike, and plunge them all into frozen water, leaving them ſome time therein, and then marking the height of the liquor in each tube, the other diviſions may be made after the ſame manner, by warming the water gradually, and immerging all the thermometers in it, care muſt be taken withal to mark a point, which may be called the mean degree between heat and cold ; as that where the ſpirit of wine ſtands in the tubes in the vaults of the obſervatory, where it continues alike all the ſeaſons of the year. Hence we might alſo learn, whether the deep mines and caverns of other countries, where the temperature of the external air cannot reach, afford the ſame degree of heat as ours, and whether the differences of ſoil occaſion any variation therein.

V. *Reflections on some observations of the variation of the needle, made in a voyage to the* South-sea, *aboard the ship* Maurepas, *by M.* de la Verune, *commander of the said ship, with some remarks on the navigation of the coasts of* America *and* Terra del Fuego, *by M.* Cassini *jun* *; *translated by Mr.* Chambers.

The abbot *Bignon* has lately given us the observations of the variation of the needle, made in the ship *Maurepas* in its voyage to the *South-sea*, in the year 1706, 1707, and 1708, wherein care is taken to note that the longitudes are reckoned from the meridian of the pike of *Teneriff*, which gave us an opportunity of comparing them with the variations laid down in Dr. *Halley*'s chart.

These observations being very numerous, we shall content ourselves to give the result thereof, and only note such as were made near, or in sight of any islands, or coast, and which will admit of an exact comparison.

On the 27th of *December*, 1706, at 345°——44′ longitude, and 20°—44′ south latitude, near the island of *Ascension*, the variation was found 7°——30′ north-east. In Dr. *Halley*'s chart the variation at this place is somewhat above 7°—— north-east.

In *December*, 1707, at 297°——12′ longitude, and 56°—6—— south latitude, near the island of the *Hermit*, the variation was found 20°—— north-east; where in Dr. *Halley*'s chart it is 20°——30′—— north-east.

At 310°——30′—— long. and 52°——19′—— south lat. near the islands of *Sebalt*, the variation

* July 21, 1708.

was

was found 23°—— north-eaſt. In the chart it is 21°—30′— north-eaſt.

In the other parts of his courſe, both going and returning from *Cape Horn* to the equinoctial, the variations obſerved, commonly agree with thoſe in the chart within a degree.

As to the variations in the *South-ſea*, Dr. *Halley* has not laid them down in his chart, for want of obſervations of them; for which reaſon I have endeavoured to ſupply in ſome meaſure that defect, by drawing lines to ſhew the degrees of variations, from the obſervations made along the weſtern coaſt of *America*. The obſervations I chiefly make uſe of, were made near the coaſts, which I ſhall here relate, according to the order of the latitudes.

In *Auguſt*, 1707, at 300°—10′— longitude, and 13°—6′— ſouth latitude, near the point *Canette*, and that of *St. Galland*, the variation was found 7°——— north-eaſt.

At 297°—27′— long. and 14°—1′— ſouth latitude, near *Piſco*, the variation was found 7°—— north-eaſt.

At 297°——30′— long. and 31°—49′— ſouth lat. near *Valparezo*, the variation was found 8°—— north-eaſt.

At 299°—25′—— long. and 36°——30′— ſouth lat. near the *Conception*, the variation was found 10°——— north-eaſt.

From theſe obſervations it appears, that the variation of the needle increaſes along the weſtern coaſt of *America*, as the ſouthern latitude increaſes, which is further confirmed by ſeveral obſervations, made at a little diſtance from this coaſt.——— For at the latitude of 44°—49′— the variation was found 12° north-eaſt.

At

At the latitude of 48°—58′— the variation was found 13°——— north-eaſt.

At the latitude of 53°—37′— the variation was found 15°— north-eaſt.

And at the latitude of 56°——42′— the variation was found 17°— north-eaſt.

In other parts of the ſhip's courſe, where it appears by the longitude expreſſed, that it was ſeveral degrees diſtant from the coaſts, the variation is laid down differently under the ſame parallels, which may ſerve in ſome meaſure to determine the direction of the lines of variation, which we hope to be enabled to rectify by the obſervations that ſhall hereafter be communicated; for beſide that there are ſeveral of theſe obſervations, which it is very difficult to reconcile, we ſhould have ſeveral made at different diſtances from the coaſts, ere we can pretend to determine the direction of thoſe lines with any preciſion.

I ſhall here add ſome obſervations of the variation delivered by *Dampier*, in his voyage round the world.

At the iſlands of *Sebalt*, which he calls *Sible de Ward*, and deſcribes them as 3 iſlands ſituate at 51°—25′— ſouth latitude, he found the variation in the year 1683, 23°—10′— north-eaſt. I have already mentioned, that the variation was found near theſe iſlands 23°——0′— in the year 1707, whence it appears that there has been no ſenſible difference in the variation during the ſpace of 24 years, which ſeems to confirm what we have elſewhere obſerved, that at *Cape Horn* the variation has not altered in the ſpace of 100 years.

At 47°—10′— latitude in the *South-ſea*, *Dampier* found the variation 15°—½ north-eaſt.

And at the latitude of 36°—— he found the variation 8° —— north-eaſt.

By which laſt obſervations it appears, that in the *South-ſea* near the weſtern coaſts of *America*, the variation continually increaſes, as you recede from the equinoctial, agreeably to what we have already infered from other obſervations.

To theſe obſervations of the needle, M. *Clairambaut*, who ſent them to the count *de Pontchartrane*, has joined ſome remarks on the navigation of the eaſtern coaſts of *South America* and *Terra del Fuego*, made by M. *de la Verune*; which, together with a particular map of thoſe countries, which he has promiſed to ſend, may ſerve to rectify ſeveral ſea-charts, wherein he finds the iſlands about *Cape Horn* prepoſterouſly placed.

His firſt remark is, that the coaſts from *Cape St. Anthony*, at the mouth of the river *de la Plata* to the ſtraits of *Magellan* are laid down, a point of the compaſs more eaſterly than they really are.

He alſo obſerves, that the diſtance between the ſtraits of *Magellan* to the ſtraits of *le Maire*, as well as the ſituation, are very ill expreſſed in the common charts; for by his account, thoſe two ſtraits are 55 or 56 leagues diſtant, and that of *le Maire* is ſituate to the north-weſt 5°— north of the ſtraits of *Magellan*. ——— But it may be here obſerved, that in Dr. *Halley*'s chart, printed in 1700, and M. *Deliſle*'s chart of the ſtraits of *Magellan*, printed in 1703, thoſe two ſtraits are laid down very agreeably to his obſervations.

He alſo obſerves, that the *Terra del Fuego* is not near ſo large, nor ſo much ſouthern as was imagined; and adds, that *Cape Horn*, which the common charts place 57°—40′— ſouthern latitude, is only 55°——40′ ſouthern latitude.

He

He adds, that the iſlands of *Barnevelt*, which the charts place in the ſame latitude with *Cape Horn*, are ſituate weſt-north-weſt of that *Cape* in 56°—35′—latitude. — In which he likewiſe agrees pretty nearly with M. *Deliſle*'s chart abovementioned.

He further obſerves, that the iſlands of *Barnevelt* are the moſt ſouthern lands; and that there is no danger in paſſing between thoſe iſlands and *Cape Horn*. The diſtance from the ſtraits of *le Maire* to *Cape Horn*, which is eaſt-north-eaſtwards of that ſtrait, he obſerves is 80 leagues, which agrees very well with the diſtance expreſſed in Dr. *Halley*'s chart; but much exceeds that in M. *Deliſle*'s.

After doubling *Cape Horn*, there is no farther difficulty, the charts being all good, as well as the coaſts ſound, and the weather moderate along *Chili* and *Peru*.

M. *de la Verune* makes ſeveral other curious and uſeful obſervations on the navigation of theſe ſeas; he points out the favourable ſeaſon for paſſing *Cape Horn*, and how to behave both in going and returning. The iſland *Hermit* he places 24 or 25 leagues from this *Cape* eaſtward, in the ſame latitude, and makes it 18 or 20 leagues in compaſs. He alſo determines the ſituation of the iſland *Sebalt*, whoſe eaſtern point is ſituate N.N.E. of the ſtraits of *le Maire*, at about 55 leagues diſtance; and he takes them to form a kind of *archipelago*. At his return he ſaw them very diſtinctly, and found their ſituation very different from what is commonly ſuppoſed. He gives them an extent of 55 or 60 leagues, and notes that to avoid them, they are obliged to range the *Terra del Fuego*, or to make a large circuit, when the wind does not allow it. Laſtly, he obſerves,

ſerves, that the lands of *Brazil* are laid down more eaſterly than they really are, by which means all the ſhips, which go from the ſtraits of *Magellan* or *le Maire*, find on their arrival at *Brazil* an error of ſome 200 leagues.

VI. *Conjectures on the poſition of the iſland of* Meroë, *by M.* Deliſle *.

In † all *Ethiopia*, which is a country of very great extent, there is nothing more celebrated among the ancients than the iſland of *Meroë*, nor any thing ſo difficult to find among the moderns, or that they leſs agree in. If what the ancients have ſaid of it be true, this iſland could arm 250,000 men, and maintain 400,000 artificers. It contained a great number of cities; the chief of which was that of *Meroë*, which has communicated its name to the iſland, and ſerved for a reſidence to the queens, *regia & metropolis Æthiopum.* I ſay to the queens, becauſe it ſeems the women reigned in this country to the excluſion of the men. In the time of *Auguſtus*, it was a princeſs with one eye indeed, but of a maſculine courage, *virilis ſane mulier, ſed altero oculo capta.* She made an irruption into *Egypt*, which at that time belonged to the *Romans*, but was obliged to ſend ambaſſadors to *Auguſtus.* At the death of our ſaviour, there reigned another, one of whoſe eunuchs was baptized by St. *Philip*, as may be ſeen in the *Acts of the apoſtles.* When *Nero* ſent ſome of his guards into this country, to ſearch for the ſources of the *Nile*, it was alſo a princeſs that reigned there, and all theſe three were called *Candace*; but we ſee by a paſſage of

* Nov. 14, 1708. † Plate II. Fig. 7.

 Pliny,

Pliny, that this name was for a long time become common to the queens.

If hiſtorical diſſertations were ſuffered here, inſtead of reciting only philoſophical and mathematical diſcuſſions, we ſhould relate what *Diodorus* and other authors have written to the advantage of this iſland; but we muſt paſs to the difficulty that there is to diſcover it in the modern geography.

This difficulty proceeds a little from the memoirs, that we have upon *Ethiopia*; for we muſt not hope, that without a reaſonable knowledge of the preſent ſtate of the world, we can make the relation of the ancient geography to the new. When they firſt began in *Europe* to have commerce with the kings of *Ethiopia*, there were ſome writers of no veracity, who, upon ſlight informations, ſaid many things far from the truth, which threw the world into numberleſs errors, from which we have hardly yet been able to recover; and it is upon the credit of theſe writers, that ſuch wretched maps have been made, and that theſe places have been ſo many ways diſfigured, that an ambaſſador of the king of *Ethiopia* ſaid, in *Egypt*, to young *Thevenot*, that our geographers had filled their country with monſters and *chimeras*.

It is true, that the jeſuits, who have been pretty long in this country, have given us better informations of it, and have made a map upon the ſpot very different from thoſe made in *Europe*. Beſides F. *Balthazar Tellez*, F. *Nicolas Godinho*, M. *Ludolf*, and others have given us deſcriptions of the country upon much ſurer memoirs; but they have only deſcribed that part of *Ethiopia*, which we call *Abyſſinia*, and not that which we call *Nubia*; and yet this was neceſſary to enable us to decide the queſtion with any perſpectuity.

I ſhall not therefore undertake here to decide it; but the memoirs that I have received from that country under the protection of M. *le Comte de Pontchartrain*, enabled me to propoſe at leaſt ſome conjectures. M. *du Roule*, the king's envoy into *Ethiopia*, as well to obey the orders of the miniſter, as to acquit himſelf with more honour of the glorious employment, with which his majeſty had honoured him, had taken in *Egypt* all the information neceſſary for the road he was to go, which was none of the leaſt difficult parts of his commiſſion. He had made a deſcription of *Nubia*, and of the courſe of the *Nile*, upon the depoſition of many *Scheicks*, or chiefs of families, who had travelled 15 or 20 times into *Ethiopia*, as well by the *Nile*, as through the deſerts. He did me the favour to communicate to me what he had learned; and it is upon his memoirs that I ſhall propoſe my conjectures.

The iſland of *Meroë* was indubitably upon the *Nile*. The ſource of the *Nile*, which was ſo long ſought for in vain by the ancients, is in 12° of north-latitude. Its cataracts, a little leſs celebrated, but much better known than its ſource, are 23° ½, and it is certainly between theſe two points, that the iſland of *Meroë* muſt be.

The ancients have ſaid, that this iſland was formed by the concourſe of the *Aſtaboras* and the *Nile*, and by another river named *Aſtape*, which falls in like manner into the *Nile*. That this iſland was terminated on the weſt by the *Nile*, and was bounded on two other ſides by the *Aſtape* and *Aſtaboras*; which ſhews that it was but improperly called an iſland, ſince it was not incloſed

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on all ſides, and it muſt be like that which we call here *l'Iſle de France*.

Notwithſtanding ſo formal a deſcription, *Mercator* and *Ortelius* have repreſented the iſland of *Meroë*, as formed by two arms of the *Nile*, and called it *Gueguere*; and almoſt every body have ſuffered themſelves to be carried away by the authority of theſe two geographers, upon whoſe credit they boldly pronounce, that the iſland of *Meroë* is now known under the name of *Gueguere*. Nevertheleſs the iſlands that are formed by the *Nile* alone above the cataracts are all ſmall, which cannot agree with what we have ſaid of the largeneſs of that of *Meroë*, nor with the number of its cities and inhabitants, and beſides there is not one that approaches to that of *Gueguere*.

The jeſuits, who have been in *Ethiopia*, are perſuaded that the iſland of *Meroë* is nothing elſe but the kingdom of *Gojame*, which is almoſt entirely incloſed by the river *Nile*, in the manner of a *peninſula*, as may be ſeen in the map; but this *peninſula*, which makes the kingdom of *Gojame*, is formed only by the *Nile*, not by the *Aſtape*, nor by the *Aſtaboras*, I mean not by any river that could be ſuppoſed to be the *Aſtape* and the *Aſtaboras*, which is contrary to the deſcription which the ancients have given of it. Beſides the city of *Meroë*, the capital of this iſland, muſt have been placed between the 16 and 17th degree of north latitude, as ſhall be hereafter ſhewn; and the kingdom of *Gojame* does not go beyond the 13th degree. In ſhort, if that which we now call the kingdom of *Gojame*, had been the iſland of *Meroë*, ſo known by the ancients, would they not alſo have known the ſources of the *Nile*, which are without diſpute in the middle of this kingdom?

Iſaac

Iſaac Voſſius, of the royal ſociety of *England*, is one of thoſe who lately has laboured the moſt uſefully at geography; and altho' his pretended reformation of the longitudes has done him no honour, he has nevertheleſs made excellent inquiries in theſe geographical works. He pretends that the *peninſula*, made by the river *Mareb* on the ſide of its ſource, by a circuit almoſt equal to that which the *Nile* makes about the kingdom of *Gojame*, is the iſland that we ſearch for; but beſides, that this iſland is formed only by one ſingle river, and not even by the *Nile*, contrary to what the ancients have ſaid of it, this peninſula, formed by the *Mareb*, has neither the extent nor the ſituation that the ancients have given to the iſland of *Meroë*. And what abſolutely deſtroys this opinion is, that the city of *Meroë*, the capital of the iſland, was upon the *Nile*; and that the iſland, or *peninſula* of *Mareb*, is very diſtant from it.

Cellarius, whoſe geographical works are now famous among the learned, has collected in his uſual manner all that the ancients have ſaid of the iſland of *Meroë*; but he does not give any intelligence of the preſent ſtate of that country, without which we cannot conclude any thing; he only ſeems to approve the opinion, which confounds the kingdom of *Gojame* with *Meroë*, which I have juſt refuted.

F. *Tellez*, a jeſuit, after having well conſidered all that the miſſionaries of his ſociety have written upon *Ethiopia*, is perſuaded, that this is an imaginary iſland. If I had believed, that ſuch an opinion could make any impreſſion upon one's mind, I would begin by refuting it; for it is uſeleſs to reaſon upon a thing that is not, or at leaſt whoſe exiſtence is doubtful: but it is ſtrange that

that any one can doubt of the exiſtence of the iſland of *Meroë*, after what has been noted by the ancients with relation to it. *Pliny* affirms, that *Simonides* ſtayed there 5 years; and that after him *Ariſtocreon*. *Bion* and *Baſilis* have deſcribed its length, breadth and diſtance from the city of *Syene* and the *Red-ſea*, its fruitfulneſs, capital city, and have even related the number of its queens.

Ludolf, who has not been able to find this iſland, any more than F. *Tellez*, has not however doubted but that in ſome meaſure it exiſted; but he pretends, that it muſt be ſought for more to the weſt than has yet been done, and that it is among the countries to which we do not travel. That if after all the inquiries that ſhall be made, it is not found, we may ſay that ſome arm of the *Nile* is dried up, and that this is the reaſon that we cannot diſcover it: but this author is not aware, that thoſe who have lately travelled over *Ethiopia*, have long coaſted the *Nile*, and that they muſt, on the contrary, have left the iſland of *Meroë* to the eaſt, ſince the *Nile* bounded it on the weſt; and that thus it muſt be looked for to the eaſt and not to the weſt, as he ſays. And as to the rivers drying up, I own that there are many of them in *Africa*, which having flowed ſome time through ſands or ſpongy grounds, weaken inſenſibly, and at laſt diſappear; but we do not put the *Nile* nor the *Aſtaboras* in the number of theſe rivers; and the power, or rather the licence of geographers, altho' great, does not go ſo far as to dry up rivers of this conſequence.

Since therefore we muſt find the iſland of *Meroë*, and as it is the duty of a geographer to make the parallel of the ancient geography with the new, we may conjecture, that it is this ſpace of of ground which is between the *Nile* and the rivers *Tacaze* and *Dender* ; and I am going to endeavour to establish this conjecture by the situation of this country, which appears to me conformable to that which the ancients have given to the island of *Meroë*, by the rivers of which it is formed, by its extent, by its figure, and by some other singularities common to the island of *Meroë*, and to the country I have just pointed out.

The situation of a place, or country, is proved by the degree under which it is situated; and by the distance of this place, or country, from other places that are known to us. The city which is the most known of all these countries is the city of *Syene:* the latitude of it is not at all doubtful; and this is a fixed point, from which we may without fear, measure the places about it. *Pliny* * affirms, that on the day of the summer solstice at noon, bodies do not make any shadow; and for a proof of it, they have caused a well to be digged, which at that time is quite light. *In* Syene *oppido, solstitii die medio, nullam umbram jaci, puteumque ejus experimenti gratiâ factum, totum illuminari.* *Strabo* has said the same things in other terms, which shews, that the city of *Syene* is just under the tropick of *Cancer*, at 23° ½ of north latitude. Now from *Syene*, to the city of *Meroe*, according to the same author, were reckoned 5,000 *stadia*, in going toward the south; and these 5,000 *stadia* reckoned in astronomical measures, make 7 degrees of a great circle, and give the position of the city of *Meroe*, at 15° ½ from the equator.

This position of the city of *Meroe*, which agrees pretty justly with that which *Ptolemy* gives it in the 4th book of his geography, is also confirmed

* Lib. II. cap. 73.

firm'd by another paſſage in *Pliny*, who ſays, that the city of *Meroë* has no ſhadow at all, any more than that of *Syene*; and that this happens twice in the year, when the ſun is in 18° of *Taurus*, and 14° of *Leo*. *In* Meroe, *quæ eſt caput gentis* Æthiopum, *bis in anno abſumi umbras, ſole duodevicesſimam Tauri partem, & quartam decimam Leonis obtinente.* Now it is certain, that when the ſun is in theſe degrees juſt mentioned, it has about 16 degrees ½ of declination, which is the latitude that the ancients have given to the city of *Meroë*, and which reſults from its diſtance from that of *Syene*.

I could alſo prove by the climates the poſition of the city of *Meroë*: The antients have placed it in the middle of the firſt climate, of which the longeſt day is 13 hours, which gives by calculation 16 degrees ½, which is the ſame latitude that we have given to *Meroë* upon obſervations, and upon its diſtance from the city of *Syene*. I have neglected in this calculation the refraction, becauſe it does not make any conſiderable difference.

The iſland of *Meroë* was formed by the river *Nile*, and two other rivers, which came from the eaſt, as we have ſaid. *Influunt in* Nilum, ſays *Strabo*, *duo flumina ab oriente delata, &* Meroem *ingentem inſulam complectuntur*. I cannot tell whether the antients knew any other rivers than theſe two, that flow into the *Nile* on the eaſt ſide; but we ſee by the memoirs of M. *du Roule*, that there are but two conſiderable ones, the rivers *Tacaze*, and *Dender*. The river *Tacaze* being as big as half the *Nile*, has very much the appearance of being the *Aſtaboras* of the antients: this is the opinion of *Juan de Barros*, the *Livy* of the *Portugueze*; and two things will not permit me to doubt it. The firſt is, that according to

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the jesuits, who have been in *Ethiopia*, it enters into the *Nile* at 17° ½ of latitude, which is within some minutes near the same height that *Ptolemy* gives to the outlet of the *Astaboras*, 700 *Stadia* below the city of *Meroë*, as we see by *Strabo*, *Diodorus*, and others.

The second thing that makes me believe the *Tacaze* to be the same with the *Astaboras* is, that this river is otherwise called *Atbara*, as we see by the relation of the scheiks of *Nubia*, and by that of a *Recollet* who has passed this river in going into *Ethiopia*. Now the names *Atbara* and *Astaboras* are not very different. I suppose that the *Atbara* is its true name, and that the *Greeks* have altered it as they have done many others, since that still happens pretty often to those who are obliged to use foreign names in their writings.

As for the river *Astape*, it will probably be that of *Dender*; for there are only the two rivers, *Atbara*, and *Dender*, at least that are of any consideration, which enter immediately into the *Nile* on the east side.

The extent of the country that I have pointed out, is pretty near the same as that which the ancients have allowed to the island of *Meroë*, *Diodorus*, and *Strabo* have made the length of it 3,000 stadia, and the breadth 1,000; that is to say they have allowed it 120 leagues of length, and 40 of breadth, which agrees here pretty well; whereas, neither the kingdom of *Gojame*, nor the *Peninsula*, formed by the river *Mareb*, approach to this extent.

And not only the extent is the same, but also the figure of a buckler, which *Diodorus* and *Strabo* give to the island of *Meroë*, sufficiently agrees with the country that I speak of. Perhaps a skilful painter might not think it exact; but we must not look for all the regularity of designing in the figures that antients have given to countries, no more than to thoſe that they have given to the conſtellations.

There would be but one thing to apprehend, that the plan, which I here repreſent, was not very certain; and that to prove what I have advanced, I had only accommodated it to the opinion of the ancients, like the *Leſbian* architects, who finding it difficult to ſuit the ſtones to their model, made their model conform to the ſtone. But to that I anſwer, that it is the rivers which make the figure, and the greateſt part of this plan, and that theſe rivers with their ſprings, their courſes, and their outlets, are drawn from the map that *F. Hieronymo Lobo*, *Franciſco d'Almeyda*, and other *Portugueze* jeſuits, have made upon the ſpot; that they are taken from the verbal depoſitions of the ſcheiks of *Nubia*, examined ſeparately by M. *du Roule* from the itineraries of our *French* jeſuits, and of the *Sieur Poncet*, whoſe travels *F. le Gobiens* cauſed to be printed, and from ſome other manuſcript travels of *Italian Recollets*, ſent into that country by the congregation *de propaganda fide*, of which I had collated copies.

Beſides the affinity that I have related between the iſland of *Meroë*, and the country that I propoſe to repreſent it, there are alſo ſome others, as the rains, the fruitfulneſs of the country, and the hunting of elephants.

Strabo ſays, that the regular rains begin only at *Mero*; and *Pliny*, that thoſe who were ſent by *Nero* to ſearch for the ſources of the *Nile*, began to find in theſe places trees, and plants, *herbas demum circa* Meroem *ſylvarumque aliquid apparuiſſe, cætera ſolitudines.* And it is exactly the obſervation, that *F. Brevedent* has made in theſe very places. We quitted, ſays he, the city of *Corti*, and the river *Nile*, to enter into the deſert

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of *Bibouda.* We began to ſee trees and plants, the rains being firſt met with in theſe quarters, whereas all the reſt to that place was only watered by the overflowing of the *Nile*, or by the means of machines which raiſed up the water to ſpread it upon the grounds, and this *Poncet* declares likewiſe in his itinerary. They might well ſay like *Pliny*, *cætera ſolitudines*, they who had walked many days in ſand, or parched grounds, where they neither found water, nor graſs, nor any thing but frightful ſolitudes. And without doubt it was in theſe deſert places, that *Cambyſes*, king of *Perſia*, having loſt part of his army, was obliged to return into *Egypt*, without arriving at that part of *Ethiopia* which begins to be cultivated and inhabited; whereupon we cannot enough admire the vanity of thoſe *Greek* authors, who would not willingly be ignorant of any thing, and who to find the origin of the name of *Meroë*, have written, that *Cambyſes* had taken this city, and had changed the name, which it formerly bore, into that of his ſiſter, who was called *Meroë*, and that this princeſs was buried there.

They have very much praiſed the fertility of the iſland of *Meroë*, and the great number of its inhabitants; and this agrees perfectly well with the country of which I ſpeak. *F. Paulet*, a jeſuit, ſays, that beyond the *Nile*, over-againſt *Senna*, the country ſwarms with people; and that there may be ſeen thouſands of little villages ſpread over the whole country. I have a journey from the ſame city of *Sennar* to *Souaquem*, an iſland and port of the *Red-ſea*, wherein it is ſaid, that the country which I deſcribe, is well cultivated, and peopled. And in the deſcription of *Nubia*, made by M. *du Roule*, upon the relation of the people of the country, it appears, that in theſe places the earth

earth is ſo fruitful, that they have three harveſts in a year.

In fine, it is a little above *Meroë*, that they began to ſee elephants according to *Pliny*. The *Ptolomys*, kings of *Egypt*, and among others the famous *Philadelphus*, who was ſo attached to the knowledge of nature and of the ſciences, ſent hither to hunt theſe great animals, and had built ſome places for the convenience of thoſe that were ſent thither; and it has been obſerved in the journeys from *Sennar* to *Souaquem*, which I have juſt mentioned, that beyond the river *Atbara*, toward the ſame height that is pointed out by *Pliny*, they found in the mountains great quantities of elephants, and many other ſorts of animals.

It ſeems to me that to complete the probability of my conjecture, there needs no more than to find the city of *Meroë* itſelf in the iſland that I have juſt ſpoken of, or at leaſt to diſcover the ruins or remains of it. If *Joſephus* and *Heliodorus* were to be credited, who place it at the uniting of the *Nile* and *Aſtaboras*, it would not be difficult; we need only look for the conflux of theſe two rivers which would not be doubtful; but it is well known, that *Heliodorus*'s hiſtory of *Ethiopia* is only a romance, and there is great likelihood, that the little ſtory which *Joſephus* makes concerning *Moſes*'s expedition into *Ethiopia*, when he was, ſays he, at the court of *Pharaoh* and general of his troops, does not merit any more credit, ſince it is not found in the ſcripture, nor in *Philo*; thus it will be better to have recourſe to *Strabo*, who ſays, that the city of *Meroë* was 700 *ſtadia* above the union of the *Aſtaboras* and the *Nile*, or to *Pliny* who makes it 70,000 paces. There is found toward theſe places the city of *Guerre*, that our travellers ſay is one of the moſt conſiderable of of the country. Might it not be what others call *Meroë*, or *Gueguerë*, by a ſort of reduplication? But there is perhaps a raſhneſs in carrying meer conjectures ſo far, and the academy profeſſes a ſevere exactneſs in the inquiries into truth.

VII. *Reflections on the obſervations made by* F. Laval, *at* St. Baum, *and other neighbouring mountains, by M.* Caſſini, *jun**. *tranſlated by Mr.* Chambers.

Among a number of aſtronomical and geographical obſervations ſent by *F. Laval*, to the count *de Pontchartrain*, there are ſeveral of the height of the barometer made at *St. Baum*, and *St. Pilon*, on different days, and at different times of the day, which he has compared with thoſe made at the ſame time at his obſervatory at *Marſeilles*.

The better to perceive the relation between thoſe obſervations he has made a table, in the 1ſt column whereof are expreſſed the days of the month, and times of the day, when ſuch obſervations were made. The 2d expreſſes the heights of the mercury at the obſervatory at *Marſeilles*. The 3d and 4th the heighth of the mercury at the ſame time at *St. Baum*, and *St. Pilon*. And the 5th of the height of the thermometer at *St. Pilon*.——To theſe are added 3 other obſervations of the barometer.

The 1ſt made at the foot of the rock *St. Pilon*, where it ceaſes to be perpendicular, and joins with the ſlope of the mountain.——The 2d on the mountain *des Beguignes*, eaſtward of *St. Pilon*.——And the 3d in the plain below *St. Baum*, called the plain *d'Aups*.

* Dec. 22, 1708.

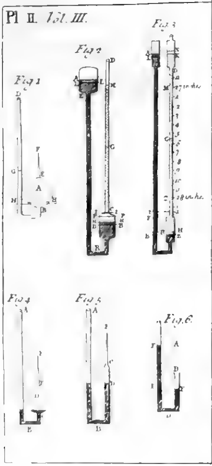
Fig. 1
Fig. 2
Fig. 3
Fig. 4
Fig. 5
Fig. 6.

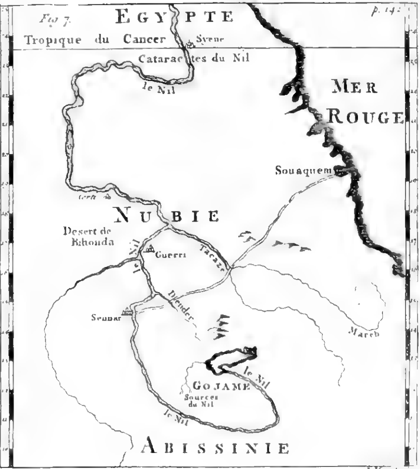
Fig. 7.
EGYPTE
Tropique du Cancer
Syene
Cataractes du Nil
le Nil
MER ROUGE
Souaquem
NUBIE
Desert de Bihouda
Guerri
Tacaze
le Nil
Dender
Sennar
Mareb
le Nil
GOJAME
Sources du Nil
le Nil
ABISSINIE

To determine the height of thoſe places with regard to each other, he meaſured a baſe line of 155 fathoms in the plain *d'Aups*, and from the extreams of that baſe obſerved the angles between *St. Pilon* and the mountain *des Beguignes*, then taking the angles of the apparent height of thoſe mountains, he found the height of the mountain *des Beguignes* geometrically to be 264 fathom above the plain *d'Aups*, and that of *St. Pilon* 181 fathom above the ſame plain, which ſets the mountain *des Beguignes* 83 fathom above *St. Pilon*. ———For the height of the bottom of the rock where the 1ſt obſervation was made, the rock being perpendicular from *St. Pilon* to this place, he had the convenience of meaſuring it with a cord, and found it 63 fathom.

Now to find the reſult of theſe obſervations of the barometer made at *Marſeilles*, and the places around, compared with the ſeveral elevations which were taken geometrically, we are firſt to conſider, that the height of the obſervatory at *Marſeilles* above the ſurface of the ſea is 24 fathom, to which 2 lines and $\frac{1}{3}$ of mercury correſpond, as appears from the table in the memoirs for 1705.

Having therefore taken the differences between the heights of the mercury found at the ſame time at the obſervatory, and at *St. Pilon*, which are 15 in number, and whereof the ſmalleſt is 2 inches, 9 lines $\frac{1}{2}$, and the greateſt 2 inches 11 lines $\frac{3}{4}$, we took the mean between them all, which is 2 inches 10 lines $\frac{13}{45}$, to which we added the 2 lines $\frac{1}{3}$ correſponding to the height of the obſervatory above the ſurface of the ſea. The ſum 3 inches 0 lines, are $\frac{28}{45}$, is the depreſſure of the mercury correſponding to the elevation of *St. Pilon* above the ſurface of the ſea.——To which, in the table above

above quoted, anſwer 481 fathom, the height of *St. Pilon* above the ſea according to the barometer.

So by comparing the obſervations of the barometer made at the ſame time at *Marſeilles*, and at *St. Baum*, which are 16 in number, the ſmalleſt difference in the height of the mercury is found 2 inches 5 lines $\frac{1}{2}$, and the greateſt 2 inches 6 lines $\frac{3}{4}$; and taking a *medium* between them all, we have 2 inches 6 lines $\frac{7}{48}$, which added to 2 lines $\frac{1}{3}$, for the height of the obſervatory gives 2 inches 8 lines $\frac{23}{48}$, for the height of *St. Baum* above the ſea, the number correſponding to which in the table is 415 fathom $\frac{1}{2}$, which ſubſtracted from 481 fathom, the height of *St. Pilon* above the ſea leaves 65 fathoms $\frac{1}{2}$ for the height of *St. Pilon* above *St. Baum.*———Yet this height by *Father Laval*'s menſuration with a cord, was only 53 fathom; but the difference between the heights of *St. Baum* and *St. Pilon* is too ſmall for any rules to be eſtabliſhed thereon; we ſhall therefore proceed to examine what reſults from the obſervations of heights, whoſe difference is greater, as the plain *d'Aups*, and the mountain *des Beguignes*, which is 284 fathom above the ſame.

The height of the barometer on this mountain was found on the 29th of *June* 24 inches 1 line, at which time the mercury at *Marſeilles* was 27 inches 4 lines high, which gives us a fall of 3 inches 3 lines between the obſervatory at *Marſeilles* and the top of the *Beguignes*, adding therefore the two lines $\frac{1}{3}$ for the height of the obſervatory above the ſea, to theſe 3 inches 3 lines, we ſhall have 3 inches 5 lines $\frac{1}{3}$ of mercury for the height of the mountain *des Beguignes* above the ſurface

face of the ſea, the number correſponding to which in the table is 559 fathom, 1 foot.

On the ſame day the height of the mercury on the plain *d'Aups* was found 25 inches 6 lines, at which time at the obſervatory at *Marſeilles* it was found 27 inches 4 lines $\frac{1}{4}$, the difference is 1 inch 10 lines $\frac{1}{4}$, which added to 2 lines $\frac{1}{3}$ for the height of the obſervatory above the ſea, gives 2 inches 0 lines $\frac{7}{12}$ for the height of the plain *d'Aups* above the ſea, the number correſponding to which in the table is 298 fathom, 1 foot $\frac{1}{2}$. This ſubſtracted from 559 fathoms, the height of the *Beguignes*, above the ſea, gives 261 fathoms for the height of the mountain *Beguignes* above the plain *d'Aups*, according to the different heights of the barometer, compared with the table above-mentioned,——— which height F. *Laval* determined geometrically to be 264 fathoms, the difference therefore is only 3 fathoms: which is a preciſion greater than we could ever have expected, conſidering that an error of $\frac{1}{4}$ of a line in the obſervations of the height of the barometer, ſuffices to make this difference.

So, if from 481 fathoms, the height of *St. Pilon* above the ſea according to the barometer, we ſubſtract the height of the plain *d'Aups* above the ſea, which we have found to be 298 fathoms, 1 foot $\frac{1}{2}$, we ſhall have the height of *St. Pilon* above the plain *d'Aups*, 182 fathoms, 5 feet, which is only 1 fathom, 5 feet more, than F. *Laval* had determined it geometrically.

From theſe obſervations therefore it appears, that the difference between the heights of two places may be found with ſufficient exactneſs, by the rule above laid down, provided the height of the barometer, at the ſurface of the ſea, be known at the ſame time. For want of an obſervation at the ſurface of the ſea, we may ſuppoſe the mean height of the mercury there to be 28 inches; but then we muſt not expect to arrive at this preciſion.

Reflections on the apparent depreſſion of the horizon of the ſea.

The height of *St. Pilon* above the ſurface of the ſea, being found by obſervations of the barometer, as we have elſewhere ſhewn, to be 481. An enquiry may be made into the obſervations of the apparent depreſſion of the horizon of the ſea, made by the ſame father on that mountain.

Theſe obſervations he has repreſented in a table, wherein are expreſſed the ſtate of the air and the wind, which blew at the time of obſervation, together with the correſpondent height of the barometer and thermometer.

The greateſt apparent depreſſion of the horizon of the ſea was obſerved on the 25th of *June*, at 3 in the afternoon, to be 57′—45″— the weather then being hazy, and the wind at north-weſt; the ſmalleſt was found on the 26th of *June* in the morning, to be 56′—0″—— the ſky being very clear, and the wind ſouth-weſterly: taking therefore a *medium* between theſe two obſervations, which differ 1′—45″— from each other, we ſhall find the mean apparent depreſſion to be 56′—52″

Suppoſing now the ſemi-diameter of the earth to be 3271600 fathoms, as we found it by our obſervations in prolonging the meridian, we ſhall find, that at the height of *St. Pilon* above the ſea, which is 481 fathoms, the real depreſſion of the horizon ſhould be 58′——57″—— which is greater by 2′——5″ than the mean apparent depreſſion 56′—52″——— This exceſs muſt be owing to the refraction, which raiſes the appa-

 rent

rent viſual ray above the true one, by about the 28th part of the angle of the mean apparent depreſſion.

F. *Laval* remarks on his obſervations, that there is a variation in the refraction, at heights greater than thoſe of the obſervatory at *Marſeilles*; but that this variation is not ſo conſiderable as in lower places; for in all the obſervations which he has had opportunity to make on *St. Pilon*, this variation never roſe above 1—45″— whereas at his obſervatory it has riſen to 3′—20″———Indeed as F. *Laval* has made a much greater number of ſuch obſervations at *Marſeilles*, than at *St. Pilon*, 'tis poſſible, that by further obſervation on that mountain, a greater difference might be found than he has yet met with.

The ſame father alſo notes, that his obſervations confirm what he had mentioned in the memoirs ſent to the academy, that the refraction is greateſt when there is a fog in the air occaſioned by a north-weſt wind; and that it is even greater or leſs, as the wind is more or leſs freſh. On the contrary, that the ſea never appeared leſs depreſſed than on the 26th of *June*, in the morning, when the wind blew weakly from the ſouth-weſt, and the horizon was very clear. On the evening of that ſame day, there being a great fog the refraction, was increaſed by 1′—30″— The weight and the heat of the air, ſeemed not to contribute any thing to the refraction, ſince the barometer and thermometer were pretty much at the ſame height on the 25th and 26th of *June*; and yet the difference of the refraction was as great as he had ever known it.

VIII. *An observation of a luminous circle about the sun, by M.* de la Hire *; *translated by Mr.* Chambers.

On the 9th of *April*, in the present year, 1708, at one in the afternoon, I perceived a large luminous ring about the sun very compleat in all its parts. The sun was in the centre of this ring, the diameter whereof was 36° —— and his breadth a degree and half; the inner edge of the ring was pretty well defined, and of a colour bordering on red; but the outer was whitish, and thus lost itself in the sky. So much of the sky as appeared within the circle was very dark, and especially in that part contiguous to the circle. On the outside it was much clearer and whiter, tho' the whole air was full of a sort of light fog, which had rose a great height, there was no *parhelion*, or mock-sun, on this circle, as is frequently found on such circles near the horizon at sun-rise, where there are commonly two diametrically opposite to each other, and of the same height with the sun; but 'tis very rare to observe such circles in the meridian, and still rarer to see *parhelia* on them, especially when the sun is very high, and the air well heated; as in effect the *phænomenon* can only be owing to particles of ice, which occasion this appearance by refracting the sun's rays; and as these circles have always the same diameter, it follows, that those icy particles must always be of the same figure; 'tis not so easy to give a physical solution of this phænomenon as of the rain-bow, the cause whereof is evidently in the little drops of rain, which are spherical, and of which we can make a perfect imitation by means of a little phial full of water,

* April 25, 1708.

nor

nor need we wonder, if there occur ſome differences in the obſervations of the diameters of theſe circles, as well as thoſe of the rain-bow, ſince in the latter, experience teaches us, that the different degrees of the heat of water produce a conſiderable alteration.

IX. *An extract of the obſervations made in the* Weſt-Indies *in* 1704, 1705, *and* 1706, *by* F. Feuillée, *a minim, mathematician to the king, compared with thoſe which were made at the ſame time, by M.* Caſſini *the ſon* *.

He ſet out from *Martinico*, *July* the 4th, 1704, and arrived the 12th at *Golfo-triſte*, which the *Spaniards* call *Porto-cabeillo.*

Obſervations for the height of the pole at Golfo Triſte, *or* Porto-cabeillo, July 12, 1704.

	°	′	″
At *Porto-cabeillo*, the meridian height of the upper edge of the ſun	78	48	55
Refraction *minus* the parallax			9
Therefore the true height of the upper edge	78	48	46
Semi-diameter of the ſun		15	50
Therefore the true height of the centre	78	32	56
Declination of the ſun	21	57	52
Therefore the ſupplement of the height of the equator	100	30	48
And the height of the pole	10	30	48

We ſhall content ourſelves in the following obſervations, to give the height of the pole, which reſults from the obſervations of the meridian height of the ſun, having regard to the refraction, parallax, ſemi-diameter, and declination of the ſun.

* December 20, 170[illegible].

July

	°	′	″
July 13, the meridian height of the upper edge of the ſun	78	25	5
From whence we take the height of the pole	10	30	50
Theſe obſervations concur in determining the height of the pole at *Porto-cabeillo* to be	10	30	50

Obſervations for the variations of the needle.

F. *Feuillée* ſet out from this port the 14th of of *July* to go to *Santa Marthe*, where he arrived the 21ſt. He obſerved, as he went along, the mountains of *Santa Marthe*, which are of a prodigious height, and had their tops ſtill covered with ſnow, altho' the ſun was near the zenith.

The 18th he obſerved, between *Porto-cabeillo* and *Curacoa* the variation of the needle, by the means of the amplitude, to be 6° 40′ N. E.

It is marked in Dr. *Halley*'s map of variations in this place for the year 1700, about 7° N. E.

The 20 near *Cape des Eguilles*, a little diſtant from *Santa Marthe*, he obſerved the variation of the needle to be 7° 6′.

It is marked in Dr. *Halley*'s map in this place above 8°——

Obſervations for the height of the pole at Santa Marthe.

July 24, 1704, at *Santa Marthe*, the meridian height of the upper edge of the ſun was	81	46	5
Aug. the third	84	8	35
Aug. the fourth	84	24	10

In taking a mean between the height of the pole, which reſults from theſe obſervations, we ſhall have the height of the pole

At St. *Marthe*	11	19	55

Theſe

Theſe obſervations were made 100 paces from the ſea.

Obſervations for the height of the pole at Porto-Bello.

	°	′	″
Sept. 7, 1704, at *Porto-Bello*, the meridian height of the upper edge of the ſun	86	38	17
The 12th	84	44	49
The 13th	84	22	0
Oct. the 3d	76	33	26
The 4th	76	11	0
The 22d	69	27	50

Taking a mean between the height of the pole which reſults from theſe obſervations, we ſhall have the height of the pole,

At *Porto-Bello.*	*9*	*33*	*5*

Obſervations of the ſatellites of Jupiter, *for the longitude of* Porto-Bello.

October 7, At $2^h\ 4'\ 25''$ in the morning at *Porto-Bello*, the immerſion of the firſt ſatellite into the ſhadow of *Jupiter*, the ſky ſerene and clear.

$7^h\ 33\ \ 5''$ at *Paris*, by the corrected calculation.

$5^h\ 28'\ 40''$ difference of the meridians between *Paris* and *Porto-Bello*, by which *Porto-Bello* is more eaſterly.

Obſervations of the length of the pendulums at Porto-Bello.

F. *Feuillée* applied himſelf during his ſtay at *Porto-Bello*, which was above 3 months, in finding the length of the pendulum. He had for this purpoſe ſuſpended a muſket-ball to a thread of of ſilk graſs, and having ſpent the greateſt part of the day, whilſt he ſtayed in this port, in comparing the vibrations of this pendulum with that which he had brought from *France*, he found that the length taken from the centre of the ball, being 3 feet, 5 lines $\frac{7}{12}$, agreed perfectly well with the mean motion.

According to this obſervation, the length of the pendulum at *Porto-Bello* is about 3 lines leſs than that which we obſerved at *Paris*. It is alſo 1 line $\frac{3}{4}$ leſs than what was obſerved at *Caienne* in 1672, by M. *Richer*, tho' this iſland is 4 or 5 degrees nearer to the equator than *Porto-Bello*.

The length of the pendulum at *Porto-Bello* only differs about a line from that which was obſerved in 1682 at *Goree* of 3 feet, 6 lines $\frac{5}{9}$, and at *Guadaloupe* of 3 feet, 6 lines $\frac{1}{2}$.

Obſervations of the variation of the needle at Porto-Bello.

F. *Feuillée* having with great care drawn a meridian line upon a horizontal plane, placed there 3 compaſſes of different ſizes, the biggeſt of which was 9 inches, 7 lines, and found the declination of the needle 7° 25′ N. E.

This declination is marked in Dr. *Halley*'s map above 9° N. E.

Obſervations for the height of the pole at the fort of Bocachica.

This fort is 3 leagues, or thereabouts, to the ſouth of *Carthagena*, built at the entrance of the gulph.

	°	′	″
Dec. 14, 1704. meridian height of the lower edge of the ſun	56	8	10

The

	°	′	″
The 20th, meridian height of the upper edge	56	26	20
By the mean of theſe obſervations, we have the height of the pole at the fort of *Bocachica*	10	20	25

Obſervations for the height of the pole at Carthagena, 1705.

Jan. 1, 1705, at *Carthagena*, meridian height of the upper edge of the ſun	56	46	20
Jan. 2	56	51	47
Jan. 3	57	3	2

Taking a mean between the height of the pole, which reſults from theſe obſervations, we ſhall have the height of the pole,

At *Carthagena* of	10	30	25

Obſervation of the eclipſe of the moon, Dec. 11, 1704, *at* Carthagena.

	h	′	″	
At	0	51	47	in the morning, the beginning of the eclipſe.
	3	36	32	end of the eclipſe.
	2	44	45	total duration.

F. *Feuillée* made this obſervation with M. *Couplet* the ſon. They had a more favourable time than we had at *Paris*, where the ſhadow of the earth did not appear well terminated; ſo that we could only obſerve the beginning of the eclipſe, and the immerſion of ſome ſpots. This is the reſult of the compariſon of this obſervation, with thoſe which were made at the royal obſervatory.

At 0 51 47 in the morning at *Carthagena*, the beginning of the eclipſe.

	h	′	″	
At	6	4	40	at *Paris*, the beginning with a telescope of 3 feet.
	5	12	53	difference of the meridians between *Paris* and *Carthagena*.
	0	59	21	at *Carthagena*, *mare humorum* enters
	6	12	0	at *Paris*, the ſhadow at the edge of *mare humorum*
	5	12	39	difference
	1	3	29	at *Carthagena*, the beginning of *Grimaldi*
	6	14	30	at *Paris*, by Meſſ. *de la Hire*
	5	11	1	difference
	1	6	45	at *Carthagena*, end of *Grimaldi*
	6	17	30	at *Paris*, by Meſſ. *de la Hire*
	5	10	45	difference
	1	9	9	at *Carthagena*
	6	21	0	at *Paris*, by Meſſ. *de la Hire*
	5	11	51	difference of the meridians between *Paris* and *Carthagena*

Taking a mean between the difference of the meridians which reſults from theſe obſervations, we ſhall have the difference of the meridians between *Paris* and *Carthagena* 5h 11′ 50″.

Obſervations of the ſatellites of Jupiter *at* Carthagena, Jan. 8, 1705.

	h	′	″	
At	11	28	46	in the evening at *Carthagena*, the emerſion of the firſt ſatellite out of the ſhadow of *Jupiter* through ſome fogs.
	16	39	54	at *Paris* by the corrected calculation.
	5	11	8	Difference of the meridians between *Paris* and *Carthagena*.

Jan. 16.

At 1h 20′ 15″ in the morning at *Carthagena*, the emersion of the first satellite out of the shadow of *Jupiter*, the heavens being clear and serene.

6 31 15 at *Paris* by the corrected calculation

5 11 20 difference of the meridians between *Paris* and *Carthagena.*

The last observation having been made in serene weather it seems best to fix here, and determine the difference of the meridians between *Paris*, and *Carthagena* of 5h 11′ 20″

Observations for the variation of the needle at Carthagena.

F. Feuillée has found by several observations the variation of the needle at *Carthagena* to be 7° 12′ N. E.

It is marked in that place in Dr. *Halley*'s map of variations 9° 0 N. E.

Observations for the height of the pole at fort St. Louis.

This fort is situated to the south of the island of *St. Domingo.*

Feb. 21, 1705, meridian height of the upper edge of the sun 61° 32′ 25″

Which gives for the height of the pole at fort *St. Louis* 18 18 5.

Observations for the height of the pole at the island of St. Thomas.

March 17, 1705, meridian height of the upper edge of the sun 70 41 0

Which gives the polar height for the island of *St. Thomas* 18 21 55

Obſervations made at Martinico.

F. *Feuillée* went at the return from his voyage to *Martinico*, where he made new obſervations during his ſtay.

He gives notice that his obſervations were made to the eaſt of the iſland at 7 or 8 leagues diſtance from the place where Meſſ. *des Hayes* and *du Gloſs* made theirs, ſo that the difference of the meridians between *Paris* and the place where he has made his obſervations, muſt be leſs than that which reſults from Meſſ. *des Hayes* and *du Glos*'s obſervations.

June 28, 1705, meridian height of the upper edge of the

	°	′	″
Sun ——— ———	81	39	10
The 19th of *Aug.* ———	88	18	37
The 2d of *Sept.* ———	83	26	37
The 14th ——— ———	78	54	28
The 16th ——— ———	78	8	55
The 20th ——— ———	76	11	42
The 22d ——— ———	75	48	20
The 30th ———	72	40	47
The 1ſt of *Oct.* ———	72	17	37
The 4th ——— ———	71	8	16
The 6th ———	70	21	18
The 9th ———	69	12	5
The 20th ——— ———	65	6	43
The 3d of *Nov.* ———	60	23	30
The 14th ——— ———	57	13	10
The 18th ———	56	11	48
The 20th ——— ———	55	32	25
The 29th ———	53	59	15
The 26th of *Dec.* ———	52	10	14
The 31ſt ——— ———	52	28	2

Theſe obſervations give the height of the pole in the place where *F. Feuillée* made his obſerva-

tions

tions at *Martinico* between 14° 42′ 5″ and 14° 43′ 55″, almoſt the ſame as that which reſults from the obſervations, which he had made at the beginning of his voyage; therefore we may determine the height of the pole at this place to be 14° 43′ 0″.

Obſervations of the ſatellites of Jupiter *at* Martinico, *the* 18*th of* October 1705, *at*

h	′	″	
3	10	41	in the morning at *Martinico*, the immerſion of the ſecond ſatellite into the ſhadow of *Jupiter*.
			The 19*th of* October, *at*
2	56	47	in the morning at *Martinico*, the immerſion of the firſt ſatellite into the ſhadow of *Jupiter*, the heaven being ſerene.
7	9	39	at *Paris*, by the corrected calculation.
4	12	52	difference of the meridians between *Paris* and *Martinico*.
			The 25*th of* October, *at*
2	0	54	in the morning at *Martinico*, the immerſion of the third ſatellite into the ſhadow of *Jupiter*.
5	18	46	in the morning at *Martinico*, the emerſion of the third from the ſhadow of *Jupiter*.
3	17	52	total duration in the ſhadow of *Jupiter*.
			The 26*th of* October, *at*
4	51	6	in the morning at *Martinico*, the immerſion of the firſt ſatellite into the ſhadow of *Jupiter* near the zenith.
9	4	24	at *Paris*, by the corrected calculation.

4

h ′ ″

4 13 18 difference of the meridians between *Paris* and *Martinico*.

The 4th of November, *at*

1 13 57 in the morning at *Martinico*, the immersion of the first satellite into the shadow of *Jupiter*.

5 26 51 at *Paris*, by the corrected calculation by an observation of the following day.

4 12 54 difference of the meridians between *Paris* and *Martinico*.

The 27th of November, *at*

1 19 36 in the morning at *Martinico*, the immersion of the first satellite into the shadow of *Jupiter*. The wind shook the telescope.

5 32 38 immersion observed at *Paris*.

4 13 2 difference of the meridians between *Paris* and *Martinico*.

The 27th of December, *at*

3 10 14 in the morning at *Martinico*, the immersion of the first satellite into the shadow of *Jupiter* near the zenith.

7 23 16 at *Paris*, by the corrected calculation.

4 13 2 difference of the meridians between *Paris* and *Martinico*.

The 28th of December, *at*

4 27 42 in the morning at *Martinico*, the immersion of the second satellite into the shadow of *Jupiter*.

The 28th of February, 1706, *at*

10 26 34 at night at *Martinico*, emersion of the first satellite out of the shadow of *Jupiter* near the zenith.

14

h	′	″	
14	39	18	at *Paris*, by the corrected calculation.
4	12	44	difference of the meridians between *Paris* and *Martinico.*
			The 23d of March, *at*
10	47	33	at night at *Martinico*, emerſion of the firſt ſatellite out of the ſhadow of *Jupiter*.
14	59	28	at *Paris*, by the corrected calculation.
4	11	55	difference of the meridians between *Paris* and *Martinico.*
			The 15th of April, *at*
11	7	44	at night at *Martinico*, emerſion of the firſt ſatellite out of the ſhadow of *Jupiter*.
15	20	44	at *Paris*, by the corrected calculation.
4	13	0	difference of the meridians between *Paris* and *Martinico.*

Almoſt all theſe obſervations concur in giving the difference of the meridians between *Paris* and *Martinico* 4ʰ 13′ 0″.

We had determined it by the compariſon of two obſervations, made at the ſame time at *Paris* and at *Martinico* to be 4ʰ 13 28″.

Therefore we may for greater exactneſs determine the difference of the meridians between *Paris* and *Martinico* to be 4ʰ 13′ 15″.

Obſervations of the eclipſe of the moon, April 27, 1706, *at* Martinico *at*

8	12	58	at night, beginning of the eclipſe
10	49	0	end of the eclipſe.
2	36	2	total duration

F. *Feuillée* obſerved during the time of this eclipſe, the immerſion and emerſion of ſeveral ſpots, of which we were not able to obſerve the cor-

corresponding ones at *Paris*, because the sky was not very serene. This is the result of his observation with ours. At,

h	′	″	
9	42	2	at *Martinico promontorium acutum*, quite in the shadow.
17	55	0	at *Paris* the shadow was at *promontorium acutum*
4	12	58	difference of the meridians between *Paris* and *Martinico*.
10	49	0	the end of the eclipse at *Martinico*.
15	2	30	at *Paris*.
4	13	30	difference of the meridians.

Taking a mean between the differences which result from these two observations, we shall have the difference of the meridians between *Paris* and *Martinico* $4^h\ 13'\ 15''$.

The same that we determined by the satellites of *Jupiter*.

This eclipse was observed at the same time at the port *de Paix*, in the island of *St. Domingo*, where the end was seen at $9^h\ 40'$.

We shall therefore have the difference of the meridians between *Martinico* and the port *de Paix* of $1^h\ 9'\ 0''$; which being added to the difference of the meridians between *Paris* and *Martinico* of $4^h\ 13'\ 15''$, gives the difference of the meridians between *Paris* and the port *de Paix*, in the island of *St. Domingo*, of 5 22′ 15″.

Observations of the length of the pendulums at Martinico.

F. *Feuillée* having suspended a musket-ball to a thread of silk-grass, found by several observations, the length of the pendulum to be 3 feet, 5 lines $\frac{10}{12}$, greater by $\frac{1}{4}$ of a line than what he found at *Porto-Bello* of 3 feet, 5 lines $\frac{7}{12}$.

Ob-

Obſervations of the variation of the needle.

F. *Feuillée* at his return to *Martinico*, found the variation of the needle to be 6° 10′ N.E. pretty near the ſame, that he had obſerved in 1704 in the ſame place.

All the obſervations juſt related, added to thoſe which are inſerted in the travels of the academy, will ſerve to determine pretty exactly the coaſt of *South America* from *Caienne* to the *Iſthmus* of *Panama*, and the ſituation of many of its iſlands.

A
TABLE
OF THE
PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCIX.

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IV. *A comparison of the barometrical observations made at* Paris *and at* Zurich, *in the year* 1708, *by M.* Maraldi.

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VI. *An explanation of some facts in opticks, and of the manner in which vision is performed, by M.* de la Hire.

VII. *An examination of a considerable difficulty proposed by M.* Huygens, *against the* Cartesian *system of the cause of gravity, by M.* Saurin.

VIII. *Observations on the weight of the atmosphere, made at the castle of* Meudon *with M.* Huygens's *double barometer, by M.* de la Hire.

IX. *A comparison of the barometrical observations made in different places, by M.* Maraldi.

X. *Observations on cray-fish, by M.* Geoffroy, *jun.*

XI. *Of the formation and growth of the shells of land and water animals, either of the sea or of rivers, by M.* de Reaumur.

XII. *Conjectures and reflections on the matter of fire or of light, by M.* Lemery *the son.*

XIII. *Observations on the evaporation which happens to fluids during a great cold: with remarks on some effects of the frost, by M.* Gauteron, *of the royal society of sciences at* Montpellier.

XIV. *The variation of the needle at* Nuremberg, *by M.* Wurt-Z baur.

A N

AN

ABRIDGMENT

OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1709.

I. *Of the ſhagreen which comes from* Turkey.

M. *Jaugeon* having been curious to know what the ſhagreen is, which comes from *Turkey*, inquired of M. *Feriol*, ambaſſador at *Conſtantinople*, from whom he received all the information that he deſired. There is no animal of this name, as ſome have imagined. They make the ſhagreen of the ſkin of the buttocks of horſes and mules, which is well tanned, and rendered as thin as poſſible; it is preſſed for a certain time, after being ſtrewed with the fineſt muſtard-ſeed. When the ſeed takes well, the ſkins are beautiful; if not, there remain ſome ſmooth places called *mirrours*, which are a great blemiſh. The fineſt ſhagreens are made at *Conſtantinople*, and in ſome parts of *Syria*.

II. *Of great cold coming with a ſouth-wind.*

It has been thought ſurpriſing, that the cold of the winter 1709, which was ſo extraordinary, and rigorous, laſted ſeveral days at *Paris*, with the wind at ſouth. To aſſign the reaſon of it, M. *de la Hire* has ſaid, that the mountains of *Au-*

Auvergne, which are to the ſouth of *Paris*, were then all covered with ſnow; and M. *Homberg*, that a very cold north-wind, which came a great way, and extended very far, having preceeded, the ſouth wind was but a reflux of the ſame air, which the north had driven, and had not been heated in any country. Theſe two cauſes may eaſily be joyned together.

III. *Of the* Seine *not being entirely frozen in the hard winter of* 1709.

There were another wonder in that winter. Notwithſtanding the extreme violence of the cold, the *Seine* was not entirely frozen at *Paris*, and the middle of its current was always free, only there floated ſome great flakes of ice in it. And yet, in leſs ſevere winters, the *Seine* has been ſo frozen, that carts might go upon it. M. *Homberg* is of opinion, that in our climate at leaſt ſuch great rivers would not freeze of themſelves, except toward the edges, becauſe their current is always too ſtrong towards the middle; and therefore if they did not break the ice at the ſhoar, which they never fail to do for different reaſons, the middle would flow as uſual, and would not carry flakes of ice with it; ſuppoſing alſo that there fell no ſmall rivers into the great one; but as they do fall into it, the ice carried by it in the middle comes for the moſt part from the ſmall rivers, which are eaſily frozen, and where people break the ice; that theſe flakes being ſtopped either by a bridge, or bend of the river, or by any obſtacle whatſoever, hold and ſtick together by the cold, and afterwards form a ſort of cruſt, which covers the whole ſurface of the river; and laſtly,

laſtly, that as the cold of 1709 was very ſudden and ſharp from its very beginning, the ſmall rivers which fall into the *Seine* above *Paris*, froze all at once, and entirely; ſo that their flakes which would have faſtened on the ſurface of the *Seine*, could not be carried into it; at leaſt in a ſufficient quantity. It is pretty remarkable, that the very violence of the cold was partly the cauſe that the *Seine* did not freeze.

In the ſame winter, the ice of the port of *Copenhagen* was 27 inches thick, even in the places where it was not accumulated. This fact is the more worthy of attention, becauſe in the great froſt of 1683, the royal ſociety having cauſed the thickneſs of the ice of the *Thames* to be meaſur'd, when they went upon it in coaches, found it to be but 11 inches.

IV. *Of a pullet with two hearts.*

M. *Plantade*, of the royal ſociety of *Montpellier*, being at *Paris*, met with two pullets within a ſhort ſpace of time, each of which had two hearts. He gave thoſe of the laſt to M. *Caſſini* the ſon, who brought them to the academy. M. *Littre* ſoaked them in warm water, in order to examine them. They were of equal ſize; and each of them very little leſs than the heart of a pullet of the ſame age. They were placed even with each other, at the diſtance of half an inch; each of them had its ventricles, its auricles, and all the blood-veſſels, like common hearts; and had nothing ſingular, except their being both faſtened by their lower *vena cava* to one of the lobes of the liver. M. *Littre* conjectures, that the blood of the right ventricle of the right heart went into the right lobe of the lungs; and the blood of the right ventricle of the left heart into the left lobe. As for the other circulation, either the *aortæ* of both hearts might be united, and form but one, or the *aorta* of the right heart furniſhed blood to the parts of the right ſide, and that of the left heart to the left ſide; or both diſtributed themſelves equally through the whole body, ſo that there was always a double artery. Beſides, as each of the hearts had almoſt as much force as one ſingle heart, this pullet had twice as much life as another, and if one heart failed it, it would have another to ſupply the place. This confirmation, which, according to what has been ſeen, is probably not very rare in this ſpecies, cannot be impoſſible in men, and perhaps it has already produced ſome *phænomena*, which have confounded the naturaliſts.

V. *Of the legs of the ſea-urchins.*

Naturaliſts think that the ſpines, with which the ſea urchins are ſurrounded, ſerve them to walk upon inſtead of legs. But M. *Gandolphe* having obſerved at *Marſeilles* that theſe animals walked pretty quick at the bottom of the ſea, has diſcovered, that this motion is not executed by their ſpines, but by legs diſpoſed about their mouth, which is always turned againſt the bottom of the ſea; theſe legs immediately diſappeared as ſoon as the urchins are taken from the bottom of the water, and thence came the common error. It was known that they walked, and they were not ſeen to have legs, becauſe they had not been ſeen walking in the ſea. They are like thoſe of a flat inſect, called the *ſea-ſtar*, which

which M. *Gandolphe* has ſtudied at *Dunkirk*, and has promiſed a deſcription of it, which probably we ſhall never ſee; for the academy have been informed of his death this year, and are afraid of loſing, with ſo good a correſpondent, a great many fine obſervations.

AN

ABRIDGMENT

OF THE

Philosophical Memoirs of the Royal Academy of Sciences at *Paris*, for the Year 1709.

I. *Observations on the quantity of rain which fell at the observatory during the year* 1708, *with the alterations which happened to the thermometer and barometer, with regard to the heat and seasons, by M.* de la Hire.

THE quantity of rain, which fell during the year 1708, was in

	Lines.		*Lines.*
Jan.	$28\frac{1}{8}$	July	32
Feb.	15	Aug.	$15\frac{1}{8}$
March	$15\frac{7}{8}$	Sept.	12
April	$17\frac{2}{8}$	Octob.	15
May	$30\frac{2}{8}$	Nov.	$6\frac{4}{8}$
June	$23\frac{1}{8}$	Dec.	$9\frac{2}{8}$

Total 219 lines $\frac{1}{2}$, or 18 inches $\frac{1}{4}$.

This quantity of water is not very far from 19 inches, to which we have fixed the mean years; and as M. *Mariotte* had formerly determined by like observations, which he had caused to be made at *Dijon* by one of his friends.

The greatest quantity of rain that fell in one day, was but 10 lines about *May* 24, and *Oct.* 20; and with an almost north wind, which is observable; for this wind seldom brings us the greatest rain.

The prevailing wind of this whole year, was the south, and it seldom turned towards the north, and often to the eaſt and weſt. There were great fogs both at the beginning and end of this year.

There fell three inches of ſnow, *Feb.* 14, and about as much *Nov.* 14, and a little *Dec.* 5.

During the whole year there were ſeveral ſtorms, but not very violent.

My thermometer, which is at 48 parts of its diviſion, in the mean ſtate of the air, and at the bottom of the caves of the obſervatory, where it always remains in the ſame ſtate, being expoſed in an open place, but ſheltered from the wind and ſun, was at the loweſt at the beginning of the year, *Feb* 13. at 27 parts ½; and it begins only to freeze in the country when it is at 32 parts, which ſhews, that it was no very great cold at that time; for before that day, and afterwards, it was always towards 35 or 40 parts. At the end of the year, on *Oct.* 29, it froze, the thermometer being at 29 parts, but without continuing; and the whole month of *November* was pretty mild in proportion to the ſeaſon. The thermometer fell alſo to 25 parts *Dec.* 12; and on this day was the hardeſt froſt of the whole year, which was not very conſiderable, for the thermometer ſometimes falls to 13 parts.

The greateſt heats of this year were *Aug.* 15 and 16, as uſual; the ſpirit of the thermometer riſing to 66 parts ½ towards ſun-riſing, and to 76 parts about 3 in the afternoon. Thus the heat and cold of this year were nearly at the ſame degree with regard to the mean ſtate.

My barometer was at the loweſt at 26 inches 9 lines ½ *Jan.* 10, with a moderate ſouth eaſt wind, as it was on the days before and after; and it was at the higheſt *Nov.* 17, at 28 inches 1 line ⅚ with a low north north-eaſt wind, and on the days before and after towards the ſouth;

ſo that the difference between the loweſt and the higheſt was 1 inch 4 lines ½ nearly. I have alſo another barometer, in which the quickſilver keeps up at three lines higher than in that which I make uſe of to mark my common obſervations every day, though theſe 2 barometers make light in the *vacuum* by agitating the quickſilver, which ſhews there is no air in them, or very little in proportion to what is commonly thought. Thus this difference of heighth muſt come only from the different weight of the quickſilvers.

I obſerved the declination of the magnetical needle *Dec.* 27, and found it to be 10° 15′ to the weſt. This needle is 8 inches long, and is that which I always uſe.

II. *Ob-*

II. *Observations on the quantity of rain water and on the winds, by M.* le Comte du Pontbriand, *at his castle 2 leagues west from* St. Malo; *communicated to the academy by M.* du Torar *of the academy, and compared with those which we have made at* Paris, *at the royal observatory, during the years* 1707 *and* 1708, *by M.* de la Hire *.

Quantity of rain-water.

	In 1707.		In 1708.	
	At *Pontbriand.*	At *Paris.*	At *Pontbriand.*	At *Paris.*
	Lines.	*Lin.*	*Lines.*	*Lines.*
Jan.	9½	5	35	28
Feb.	20½	10	18½	15
March	22	11	22½	16
April	7½	4	36½	17¼
May	6½	11½	26½	30¼
June	31¾	17	24	23⅛
July	40	38	10	32
Aug.	38	34¾	6½	15
Sept.	20½	9¼	43½	12
Oct.	32	41	35½	15
Nov.	10½	6	11	6½
Dec.	57½	27¼	24½	9¼

	Inch.	*Lin.*		*Inch.*	*Lin.*
Total at *Pontibrand*	24	10¼	Total at *Pontbriand*	24	6
Paris	17	11½	*Paris*	18¾	

Some like observations, which M. *le Comte du Pontbriand* had communicated to us before, shewed that it rained a little more towards *St. Malo* than at *Paris*, which is confirmed by the 2 years which we have just compared.

* Fe 9. 1709.

On

On the winds in 1707.

In *Jan.* the winds were generally more to the S. at *Paris* than at *Pontbriand*, by a quarter of the compaſs.

In *Feb.* almoſt the ſame.

In *March* the quite contrary to the preceding months.

In *April* much the ſame as in *Jan.*

In *May* the winds were different in theſe two places.

In *June* pretty much alike; but ſometimes more to the S. at *Paris* than at *Pontbriand*, by a quarter of the compaſs.

In *July* the wind almoſt the ſame, with very great heats, the 21ſt at *Paris*, as at *Pontbriand*, the wind being S. E. S. and S. W.

In *Aug.* pretty often more to the S. at *Paris* than at *Pontbriand*.

In *Sept.* the winds a little different in theſe two places.

In *Oct.* ſometimes the ſame, and ſometimes oppoſite.

In *Nov.* often the ſame, but at *Paris* ſometimes more to the S. than at *Pontbriand*.

In *Dec.* often the ſame, ſometimes oppoſite, but often at *Paris* more to the S. than at *Pontbriand*.

At *Pontbriand* the greateſt rain on the ſame day was 10 lines *July* 3, with a N. E. wind: that day the wind at *Paris* was S. W. with thunder, but without rain. In all the reſt of the year, the greateſt rains on the ſame day roſe but to 6 lines at *Pontbriand*. But at *Paris* the rain was 16 lines *July* 15, with a ſtrong wind towards the S; but at *Pontbriand* there fell but 5 lines and half, with the ſame wind that day

At

At *Paris* the greateft rain was 21 lines ½ *Aug.* 12, with a low wind towards the W. and at *Pontbriand* 5 lines, with a N. wind. In *Oct.* at *Paris*, the 4th and 5th together gave 24 lines, with a wind toward the W. and at *Pontbriand* 6 lines ½, with a N. W. wind.

On the winds in 1708.

In *Jan.* the wind more to the S. at *Paris* than at *Pontbriand*, and fometimes the fame.

In *Feb.* often the fame.

In *March* generally the fame.

In *April* the fame, but on fome days a little different.

In *May* at *Pontbriand*, the night between the 6th and 7th a fharp froft, which blafted all the trees; but at *Paris* fine weather: the winds different.

In *June* the winds different, and at *Paris* ufually more to the S.

In *July* very few obfervations at *Pontbriand*, fo that nothing is difcovered of the difference.

In *Aug.* more to the S. at *Paris* than at *Pontbriand.*

In *Sept.* as in *Aug.*

In *Oct.* the winds different in thefe two places.

In *Nov.* a little different.

In *Dec.* the fame.

We cannot make a very juft comparifon of all thefe winds; for M. *du Pontbriand* marks the rhumb only on thofe days when it rained.

At *Pontbriand* the greateft rain on one day was but 9 lines; the 20 and 27 of *October*, the wind being S. E. and S. W. and 8 lines *April* 22, with a S. E. wind. *Oct.* 20, at *Paris*, it rained 10 lines, with a ftrong N. wind. On the

27th,

27th, at *Paris* no rain; wind N. *April* 22, at *Paris* no rain, foggy.

At *Paris* the greateſt rain on one day was 11 lines ½, *May* 24, with a N. N. W. wind, and at *Pontbriand* 4 lines ½, with a N. W. wind. At *Paris* 9 lines, *July* 2, wind S. W. at *Pontbriand* no rain. At *Paris* again, 10 lines *Oct.* 20, as was marked above.

III. *Obſervations on the water which fell at* Lyons, *during the year* 1708, *by M.* de la Hire *.

F. *Fulchiron* has obſerved exactly the quantity of rain water, and melted ſnow, which fell at *Lyons* at the obſervatory of the jeſuits, and in the ſame manner that I obſerve here; of which this is the reſult of each month which he has communicated to me.

	In.	*Lin.*		*In.*	*Lin.*
Jan.	2	0	July	1	6¼
Feb.	3	7½	Aug.	3	6
March	2	3⅙	Sept.	7	7¼
April	3	9½	Oct.	1	11
May	2	2⅛	Novem.	0	10
June	4	10¾	Dec.	2	1¾

Sum of the whole year 36 inches, 9 lines.

We ſee by this, that the quantity of rain water at *Lyons* was double what it was at *Paris*; and it is not probable, that this comes from the two great rivers which flow by it, and at moſt could only form a great many fogs; but rather from the great mountains, which are but little diſtant

* April 13, 1709.

from

from it, where there always falls much more water and ſnow than in the plains.

IV. *A compariſon of the barometrical obſervations made at* Paris *and at* Zurick, *during the firſt ſix months of the year* 1708, *by M.* Maraldi.

M. *Scheuchzer* has ſent to the academy a memoir, wherein are ſeveral obſervations, which he has made at *Zurick* during the firſt ſix months of the year 1708, on the barometer, thermometer, winds, conſtitution of the air, quantity of rain which has fallen, and on the augmentation and diminution of the *Limat*, a river which paſſes by *Zurick*. They were made every day of the month, and often twice on the ſame day. To all theſe obſervations he adds others at the end of each month, upon the diſeaſes, which prevailed during that month.

For the barometrical obſervations, he made uſe of two tubes, one upright, the other inclined, in which the motion of the quickſilver is twice as ſenſible as in the upright one. Theſe heights are divided into inches and lines of the *Paris* foot. Theſe two barometers often agree together, but ſometimes there is a difference of 4 lines. In the compariſon which we have made of theſe obſervations with our own, we have made uſe of the upright barometer. To meaſure the rain, he ſays, he made uſe of the method of the academy and of the *Paris* meaſure. He alſo uſed the ſame meaſure, to know the augmentation and diminution of the *Limat*.

Jan. 1, the barometer was at the obſervatory at 27 inches, 5 lines, the wind being S. At *Zurick*, with the ſame wind, the barometer was at

26 inches, 3 lines; ſo that the difference between the obſervatory and *Zurick* was 1 inch, 2 lines, by which the quickſilver was higheſt at the obſervatory. The moſt common and mean difference is 1 inch, 4 lines. After *Jan.* 1, the barometer roſe in both places till the 3d, and then fell till the 10th, when it was at *Paris* at 26 inches, 10 lines ½, at *Zurick*, at 25 inches, 11 lines, which are almoſt the loweſt to which it falls at either place; thus it had fallen about 6 lines; in this interval the wind was at *Paris* S. or S. W. at *Zurick* it was at the ſame time almoſt quite oppoſite; that is, N. or N. W. The barometer roſe the reſt of the month. At *Paris* the 19th and 20th, there were very violent S. W. winds. M. *Scheuchzer* obſerves alſo, that on the 19th there was a ſtrong S. W. wind; and adds, that on the 25th at 10 p. m. there was a very violent wind, which threw down a great many chimneys. His thermometer was *Jan.* 29, at 10 degrees, which is the loweſt to which it fell. During the month of *Jan.* it rained at *Zurick* 18 lines ½; at *Paris* it rained above 34 lines. The diminution of the *Limat* was 9 inches, the augmentation two.

At the beginning of *Feb.* the barometer being very low at both places, it roſe from the 6th to the 9th, in 3 days, a little more than 10 lines at *Paris*, and 8 lines at *Zurick*; it then fell till the 16th, and afterwards roſe till the 22d, being as it had been *Feb.* 9, at *Paris* at 28 inches, 1 line, at *Zurick* at 26 inches, 8 lines, which are almoſt the greateſt heights to which it uſually riſes. During the month of *Feb.* there generally prevailed the ſame N. and N. W. wind at *Paris* and at *Zurick*, and in both theſe cities there fell the ſame quantity of rain, that is, 19 lines. The diminution

minution of the water of the *Limat* in height was 9 inches ½, and the augmentation 1 inch ½.

There happened ſeveral variations in the height of the barometer in the month of *March*, and theſe variations happened on the ſame days, and were almoſt the ſame at *Paris* and at *Zurick*. It continued elevated the two firſt days, and ſunk the third : it roſe the three following days, and ſunk again till the eleventh. After having riſen till the ſixteenth, it ſunk a third time till the twenty-ſecond. The wind was N. at *Paris*, and N.W. at *Zurick*. It rained at both places 17 lines. The augmentation of the *Limat* was 5 inches, equal to the diminution.

April 10, at *Paris*, the barometer was at 27 inches 2 lines ½, with a weſt-wind; at *Zurick*, it was 25 inches 11 lines, with a north-wind. The barometer roſe a little the following day in both cities, and it ſunk again the 12th at *Zurick* and at *Paris*, where it continued to fall again the 13th with a violent ſouth-wind. It rained in *April* 26 lines at *Paris*; and 52 lines ¼ at *Zuri ck*.The *Limat* increaſed 24 inches, and fell but ½ an inch.

The days that the barometer continued higheſt in *May* at both places were, the 7, 8, 9, and 28th, and the days that it fell moſt were the 16th and 17th. The ſame at both places. It rained in *May*, at *Paris*, 27 lines ⅔; at *Zurick*, 21 lines ½. The diminution of the *Limat* was 4 inches, and the augmentation 18.

During the month of *June*, the barometer generally continued at a great height, except the 4, 27, and 30th, when it was at *Paris* at 27 inches 5 lines, at *Zurick* 26 inches 1 line. The days that it continued at the higheſt were the 14th and 15th, being at *Paris* at 28 inches, and at *Zurick* at 26 inches 5 lines. It rained at *Paris*

25 lines $\frac{1}{2}$, at *Zurick* 66 lines $\frac{1}{2}$. The augmentation of the *Limat* 21 inches, the diminution 7.

The greateſt height to which the barometer roſe in the 6 firſt months of this year was at *Paris*, on the 9th and 22d of *Feb.* to 28 inches 1 line; and the leaſt height to which it fell, was *Feb.* 1, when it was at 26 inches 10 lines. So that the variation from the greateſt to the leaſt heighth was 1 inch 3 lines at *Paris*. At *Zurick* the greateſt height was 26 inches 8 lines, *Feb.* 9, and 22. The leaſt was 25 inches 11 lines, *Feb.* 1. The difference is 9 lines, being leſs by 6 lines than what happened at *Paris*.

A compariſon of the barometrical obſervations made at Paris, *and at* Zurick, *the* 6 *laſt months of the year* 1708.

In *July* the barometer generally continued at a great height at both places; it was at a mean height only on the 6th and 7th, being at *Paris* at 27 inches 7 lines; at *Zurick* at 26 inches 2 lines $\frac{1}{2}$ and 3 lines; ſo that the difference was 1 inch 4 lines, as we have already concluded by other compariſons. The wind, which prevailed at the ſame time in theſe two cities, has generally been different, and often oppoſite. It was the ſame only for 4 days, the 11th, the 18th, and the 22d, being in both places north-eaſt, and the 16th ſouth-weſt. The thermometer was the higheſt at *Zurick* the 28th, at *Paris* the 29th. In *July* it rained at *Paris* 28 lines, at *Zurick* 48. The waters of the *Limat* augmented 10 inches, and diminiſhed 16; thus M. *Scheuchzer* ſays, that the augmentation of the rivers does not anſwer to the quantity of rain, ſince the *Limat* diminiſhed more than it increaſed, tho' there fell a great quantity of rain during the month of *July*.

In

In *Aug.* the variation which happened to the height of the barometer was 4 lines at *Paris*, and 3 at *Zurick*. The winds were moſt part of the time very different in theſe 2 cities. The day that the thermometer roſe the higheſt, was the 15th at *Paris*, the ſame as at *Zurick*. It rained at *Paris* 22 lines $\frac{1}{3}$, at *Zurick* 35 lines $\frac{1}{2}$. The waters of the *Limat* increaſed 3 inches in height, and diminiſhed 22 inches.

In *Sept.* the day that the barometer was the higheſt, was the firſt, both at *Paris*, and at *Zurick*; and the day that it fell the loweſt, was the 26th at both places. The 10th, a ſouth-eaſt wind prevailed in both places; the 20th, a ſouth-weſt wind; the 21ſt, a ſouth wind: on the other days the winds were different. It rained at *Paris* 12 lines, at *Zurick* 34. The *Limat* diminiſhed 12 inches without having increaſed.

In *Oct.* the barometer continued higheſt the 6th and 7th, the 18th and the 19th, both at *Paris*, and at *Zurick*. During almoſt the whole month there were north, north-eaſt, or north-weſt winds. It rained at *Paris* 14 lines $\frac{2}{3}$, at *Zurick* 27 lines $\frac{1}{2}$. The perpendicular height of the waters of the *Limat* diminiſhed 10 inches without having increaſed.

In *Nov.* the days that the barometer was the higheſt, were the 1ſt, and the 19th, the ſame at *Paris* and at *Zurick*; and the day that it fell the loweſt at both places was the 23d. The ſame wind prevailed the 24th and 26th. The coldeſt day was the 25th at both places. It rained at *Paris* 5 lines $\frac{1}{2}$, at *Zurick* 7. The diminution of the *Limat* was 6 inches without having increaſed.

In *Dec.* the 14th was the day that the barometer was the loweſt in both places. The days that thethermometer was the loweſt, were at *Paris* the

the 11th and 14th, at *Zurick* the 12th and the 29th. There was no day when the wind was the ſame in both places. It rained at *Paris* 9 lines $\frac{2}{3}$, at *Zurick* it rained 21 lines $\frac{1}{2}$. The diminution of the *Limat* was 4 inches without augmentation.

The total ſum of the rain which fell at *Paris*, according to our obſervations, was 20 inches one line; that which fell at *Zurick* is 30 inches; ſo that there fell almoſt $\frac{1}{3}$ of rain more at *Zurick*, than at *Paris*. M. *Scheuchzer* thinks it rains more in *Swiſſerland*, than in *France*, becauſe of the great quantity of mountains, where the clouds being driven by the winds, commonly pour down in rain and ſnow. The great quantity of rivers which proceed from theſe mountains, give room alſo to imagine that the rain falls there in greater abundance. He thinks alſo, that there falls more rain in the countries near the ſea, than in thoſe which are inland. He ſays, that at *Upminſter*, in *England*, according to Dr. *Derham*'s obſervations, it rains 19 inches of water, when at *Townley* in *Lancaſhire*, there fall 39 inches.

In the 6 firſt months of the year 1708, the augmentation of the waters of the *Limat* was 71 inches $\frac{1}{2}$; the 6 laſt it was 13; and the total augmentation 84 inches $\frac{1}{2}$. The diminution during the 6 firſt months was 35 inches, and 67 in the 6 laſt. The total diminution 102 inches, greater by 16 inches than the augmentation.

M. *Scheuchzer* ſays, that the augmentation of the waters in the rivers of *Swiſſerland* comes chiefly from the melting of the ſnows upon the mountains, which appears by ſeveral torrents of that country, and in particular, by thoſe which he calls *Taminna*, the waters of which increaſe every evening, in ſummer, often to a foot in height, tho' it has not rained all the day. From the di-

minution

minution of the waters of the *Limat* being greater than the augmentation, M. *Scheuchzer* infers, that his country is colder than that which is farther from the *Alps*, where it is winter the greateſt part of the year, there being in *Swiſſerland* but two months of ſummer, which ought rather to be called a ſpring.

V. *Obſervations on the motions of the tongue of the wood-pecker, by M.* Mery *.

In order to give a more juſt explanation of the motions of the tongue of the wood-pecker, than that which appears in the works of M. *Borelli* and M. *Perrault*, I ſhall deſcribe more exactly than they have done all the parts on which its motions depend.

Notwithſtanding the tongue of this bird ſeems to be very long, yet its proper length is certainly but 3 or 4 lines; for that of the body and branches of the *os hyoides*, which theſe authors have aſcribed to it, do not belong to it in anatomical ſtrictneſs.

The tongue of the wood pecker is made of a very ſhort little bone, covered with a horn of a ſcaly ſubſtance: its figure is pyramidal; it is articulated by its baſe, with the anterior extremity of the *os hyoides*.

The *os hyoides* is about 2 inches long, and ½ a line thick; it is articulated by its poſterior extremity, with 2 bony branches more ſlender than its body. Each branch is compoſed of 2 bony threads of unequal length, joined together, and cloſed at the end. The foremoſt thread is but 1 inch ½ long; the hinder, which was unknown to M. *Borelli*, is 5, or thereabouts, being united to a

* March 13. 1709.

little

little cartilage which terminates it; ſo that each branch is 3 times as long as the body of the *os hyoides* and that of the tongue together. Theſe branches which belong to the *os hyoides*, are bent in form of an arch; the middle of which occupies the ſides of the neck, the anterior extremities paſs under the beak, and are terminated in the body of the *os hyoides*; their poſterior extremities paſs over the head, and enter the noſe on the right ſide; but it is obſervable, that they are not articulated to it; which contributes very much to the egreſs of the tongue, as I ſhall ſhew hereafter.

The *os hyoides* and the anterior thread of its branches, are incloſed in a ſheath, formed of the membrane which lines the inſide of the lower beak. The extremity of this ſheath is united to the opening of the ſcaly horn of the tongue. This ſheath is prolonged, when the tongue comes out of the beak, and contracts when it returns.

The ſcaly horn, which covers the little bone of the tongue, is convex above, plane underneath, and hollow on the inſide: it is armed on each ſide with 6 very fine, tranſparent, and inflexible points: their extremity is a little turned towards the throat. It is probable that this horn, armed with theſe little points, is the inſtrument with which the wood-pecker catches its prey; which he does with ſo much the more eaſe, as this inſtrument is always lubricated with a glutinous matter, which is poured into the extremity of the lower beak by 2 excretory ducts, which go from 2 pyramidal glands ſituated at the inner ſides of this part.

To make uſe of this inſtrument, nature has given the wood-pecker ſeveral muſcles, of which ſome belong to the branches of the *os hyoides*: theſe draw the tongue out of the beak; others

belong

belong to the ſheath, which incloſes the body of the *os hyoides*, with the anterior threads of its branches; thoſe draw the tongue into the beak. Laſtly, the tongue has its proper muſcles, which draw it up and down, and to each ſide.

Each branch of the *os hyoides* has but 1 muſcle, which alone is as long as the tongue, the *os hyoides*, and one of its branches together; theſe 2 muſcles derive their origin from the internal, lateral, anterior part of the lower beak, and in retreating they involve the poſterior threads of the branches of the *os hyoides*, and paſſing above the head, they are at laſt inſerted at their extremities, whence proceed 2 elaſtic ligaments, which uniting together form a third, which faſtens them to the membrane of the noſe. Theſe ligaments are very ſhort; but are eaſily prolonged by being drawn. Now as the reſiſtance of theſe ligaments may eaſily be ſurmounted by the contraction of theſe muſcles, it is eaſy to conceive, that when they contract, they draw the poſterior extremities of the branches of the *os hyoides* out of the noſe, and carrying them away on the ſide of their origin, they drive the body of the *os hyoides*, the anterior threads of its branches, and the tongue out of the beak; which they could not have done, notwithſtanding the great flexibility of the *os hyoides*, if its branches had been ſtrictly faſtened, or articulated with the bones of the noſe; for tho' the arches, which they deſcribe, may be extended, they could not have been ſufficiently prolonged to drive the tongue 4 inches out of the beak; which they do with ſo much more eaſe, as they have their motion free in theſe muſcles, where they are incloſed as in a canal, and alſo are not articulated with the bones of the noſe.

To draw the tongue into the beak, nature has given to the ſheath which incloſes the *os hyoides*, and the anterior threads of its branches, two muſcles to pull it back; and becauſe their prolongation and contraction muſt be equal to thoſe of their antagoniſts; ſince the tongue makes the ſame way in retreating into the beak, as it does in going out of it, nature has taken care to place theſe muſcles in the little ſpace which is between the under part of the *larynx* and the end of the beak, to cauſe each of them to make two circumvolutions a contrary way, about the upper part of the *trachea*, whence theſe two muſcles draw their origin; after which they croſs one another behind the *larynx*, and at laſt line the inſide of the ſheath to which they are united; now as its extremity is joined to the opening of the ſcaly horn of the tongue, it happens that when theſe two muſcles contract, they pull and draw this ſheath backward, and thus drawing the tongue into the beak, they drive back the poſterior extremities of the branches of the *os hyoides* into the noſe. The 3 elaſtic ligaments, which I have mentioned, ſerve alſo to draw them back; for after having been prolonged by the muſcles, which draw the tongue out of the beak, they contract as ſoon as theſe muſcles are relaxed, and draw into the noſe the branches of the *os hyoides*, to which they are faſtened.

There is above the skull a groove, which with the skin forms a canal, which incloſes the hinder part of the branches of the *os hyoides*, with their muſcles, in which theſe parts have their motion free. This canal hinders the branches of the *os hyoides* from receding either way when they are drawn forwards, and makes them eaſily reſume their place, when they are drawn backwards.

If we do but reflect on the length of the tongue, the *os hyoides*, and its branches joined together, and on the origin and determinate insertion of the muscles, which make the tongue of the wood-pecker go in and out of the beak, it will be easy to judge that M. *Borelli* was mistaken; for if we consider, that the tongue of this bird, the *os hyoides*, and the branches joined together, are 8 inches in length, and that of this length there comes 4 inches out of the beak when it is drawn, we shall easily conceive, that the tongue making the same way in retreating, as it did in going out, the muscles, which pull it backwards and forwards, must each of them have prolongations and contractions of 4 inches, and that consequently they must be above 4 inches long, not being able to contract their entire length. Thus, of the 4 first muscles, which M. *Borelli* allows the tongue for its motions, two taking their origin from the extremity of their lower beak, and the two from the fore part of the skull, and all the four being inserted into the middle of this length of 8 inches, it is visible, that these muscles could never have such an effect, since at most they would be each of them no more than 4 inches.

M. *Borelli* would not have fallen into this opinion, if he had observed that the two muscles, which rise from the beak, run through the whole extent of the body and branches of the *os hyoides*. His mistake therefore comes from having divided each of these muscles into two, and from having known that the anterior threads of the branches of the *os hyoides*, at the end of which he places the insertion of the four first muscles of the tongue, which he has described. As for those, which turn about the *trachea*, he knew the true use of them.

As for M. *Perrault*, he was much more mistaken than M. *Borelli*. For first he makes no mention of the muscles which incompass the *trachea*, and yet it is by their action alone, that it is withdrawn into the beak. Secondly, he makes M. *Borelli*'s 4 first muscles rise from the *larynx*, and sends two of them to the posterior extremities of the branches of the *os hyoides*, and the other two to their anterior extremities, to draw the tongue in and out, and thereby he falls into the same inconvenience with M. *Borelli*; but this mistake is the greater, as there goes no muscle from the *larynx*, to be fastened to the branches of the *os hyoides*.

In short, the whole inquiry which these gentlemen have made to explain the motions of the tongue of the wood-pecker, is terminated in the muscles, which make it come in and out of the beak. It does not appear, that their anatomists gave themselves the trouble to penetrate farther into its structure: thence it comes that these gentlemen have told us nothing of the 4 muscles proper to the tongue of this bird, by which it is moved up and down, and to each side, whether it is placed within or without the beak.

All these muscles derive their origin from the anterior part of the branches of the *os hyoides*, two from one, and two from the other, and are terminated each of them in a long slender tendon; these four tendons embrace the body of the *os hyoides*, and are inserted into the base of the little bone of the tongue. When all these muscles act together, they hold the tongue strait; when the muscles of the upper part contract at the same time, they draw the tongue upwards; when those of the under part are in action, they draw it downwards. But when two muscles placed on the

the same side act together, they pull it to that side.

Now, as of all the muscles which serve for the different motions of the tongue, only these four last are inserted into it, it is visible, that the muscles, which pull it in and out, do not properly belong to it, but to the sheath and branches of the *os hyoides*, where these muscles are inserted as I have shewn; whence it follows, that the motions, which the tongue makes going in and out of the beak, belong also to these parts, and not to the tongue, since in these two motions it may remain unmoveable.

An explanation of the figures in Plate III. Fig. 1, 2, *and* 3.

A. The tongue of the wood-pecker.

B. The proper bone of its tongue.

C. The scaly-horn armed with points, in which this bone is received.

D. D. D. D. The four proper muscles of the tongue.

E. The body of the *os hyoides*.

F. F. Its two branches.

G. G. The anterior threads of these branches.

H. H. Their posterior threads.

I. I. The two glands, which emit the glutinous matter to lubricate the tongue.

K. K. The apertures of the excretory vessels of these glands.

L. The membranous sheath, which incloses the *os hyoides*, the anterior threads of its branches, the four muscles of the tongue, and the anterior part of the two muscles, which draw it back into the beak.

M. M. The two muscles, which pull the tongue out of the beak.

N. N.

N. N. The two muſcles, which pull it into the beak.

VI. *An explanation of ſome facts in opticks, and of the manner in which viſion is performed, by M.* de la Hire †.

We know that the pupil of the eye in moſt animals contracts with a ſtrong light, and opens conſiderably in the dark. It is eaſy to ſee in the diſſection of the eye, that the *iris*, which is perforated in the middle, where it is called the aperture of the pupil, is a circular muſcle, which can contract by retreating towards its circumference, which then increaſes the aperture of the pupil; but in relaxing, its parts return from the centre of the pupil by an elaſtic power; and this is what diminiſhes the pupil.

To underſtand rightly how this change can be made in the pupil by the action of the muſcle, we muſt conſider that the body of this muſcle is toward its circumference, where it is faſtened within the eye, and that all its fibres ſeem to tend from the circumference toward the centre, which they do not reach; for they are terminated at the little circle, which forms the pupil. But this muſcle having a pretty conſiderable thickneſs towards its head, if its fibres recede from each other according to the thickneſs of the muſcle, where there ought to be a great quantity of them, their extremity which forms the pupil, muſt draw nearer to the head, and conſequently dilate the pupil; but when the action of the muſcle ceaſes, the ſpring of the ſame fibres may replace them in their firſt ſtate, and cloſe the pupil; or there

† March 30, 1709.

might

might be ſome elaſtick fibres in this muſcle, which would ſerve only for this purpoſe; or laſtly, we might imagine another muſcle of but little thickneſs, couched upon the firſt, the fibres of which would be circular, and ſerve it for an antagoniſt; for the circular fibres of this muſcle, receding from each other according to their plane, would cloſe the pupil, the action of the other muſcle having ceaſed; and this opinion ſeems to me the moſt natural, or I am moſt inclined to follow it.

But of two antagoniſt muſcles the ſtrongeſt will always prevail, when there is no particular determination for either: whence it follows, that if that which dilates the pupil is the ſtrongeſt, as it appears to be, we ſhall judge that the natural ſtate of the pupil is to be dilated.

The action of opening and ſhutting the pupil is not of that kind which we call voluntary, but of that which is neceſſarily performed by a foreign cauſe, as it happens to ſeveral parts of the bodies of animals.

It ſeems probable, that a very great light making too ſtrong an impreſſion upon the bottom of the eye, hurting, and in a manner burning it, as when we look at the fire, or at a white body expoſed to the ſun, obliges us immediately to cloſe the pupil as much as poſſible, to receive fewer of theſe too luminous rays, and to remove the danger which threatens the eye. On the contrary, when we look attentively at any object in the dark, we do all we can to ſee diſtinctly, and perfectly to diſcern all the parts of it, which we cannot do without the help of a pretty vivid light; wherefore we dilate the pupil, that there may enter into the eye a greater quantity of theſe feeble rays, which altogether will make a ſtronger im-

impreſſion by reuniting themſelves in the principal organs of viſion.

But tho' we are expoſed to a pretty ſtrong light, we do not always cloſe the pupil, when we are attentive to look upon any object, the image of which is to be ſtrongly painted on the bottom of the eye, which is obſervable in thoſe animals, which can cloſe and dilate the pupil in an extraordinary manner, ſuch as cats; for when they are in a ſtrong light and quiet, their pupil is almoſt quite ſhut; and if any extraordinary object, which rouſes their attention, preſents itſelf, we ſee them open it at once as much as they can.

Nature ſeems to have given a particular ſtructure to the *iris* of this ſort of animals, that it ſhould not cloſe circularly, but ſidewiſe, that it may open readily and conſiderably in the dark, where they moſt often ſeek their nouriſhment.

What attention ſoever we give to ſee the ſmall parts of an object, the pupil will always be leſs open in a ſtrong light, than in the dark, eſpecially if this attention laſts any time; for a ſtrong light naturally obliges it to ſhut, to hinder the principal organ of viſion from being hurt. Thus, in the dark, or in a faint light, we cannot queſtion but that the pupil puts itſelf in its natural ſtate of dilatation, and that it does not open ſo much as the *equilibrium* of the muſcles, which compoſe the *iris*, permits, as it happens to all the parts of the bodies of animals, which are moved by antagoniſt muſcles.

The following obſervation is pretty common, and thoſe who have made it have always obſerved the ſame thing. If you plunge the head of a living cat into water, the pupil immediately quite opens itſelf, tho' the animal is expoſed to very bright

bright objects; and then you may ſee diſtinctly the leaſt parts that are at the bottom of the eye.

I undertake therefore to explain here by the laws of opticks:

1ſt. Why luminous objects do not by their preſence oblige the eye of this cat to ſhut.

2d. Why we ſee the bottom of the eye diſtinctly.

Let O * be a luminous or very bright object, of which the rays O B come as parallel to each other as far as the *cornea* B B, the object O being at a moderate diſtance from the eye. It is known that the eye being expoſed to the air, the greateſt refraction of the rays O B is made at firſt upon the *cornea*, and that afterwards, after two other refractions, much leſs than the firſt, upon the ſurfaces of the cryſtalline, theſe rays meet in D, upon the bottom of the eye, which we call a good conformation.

But if the eye B B D is plunged in the water A A, ſo that the ſurface A A is perpendicular to the rays O B, which come from the object O to the eye, then theſe rays O B meeting the ſurface of the water A A perpendicularly, will ſuffer no refraction therein, and will enter the eye a-croſs its humours, which are but little different from the water, ſuffering a little refraction therein; whence it follows, that they will have a direction to aſſemble towards E, very far beyond the eye, and conſequently that they will meet the bottom of the eye in points F F, diſtant from each other, inſtead of meeting in the ſame point D.

But the rays of the luminous point O, which are entered into the eye, occupying at that time a very conſiderable ſpace F F on the bottom of the eye, will make but a very faint impreſſion on it,

* Plate III. Fig. 4.

whereas they would have touched it very briskly, if they had met in D; wherefore this luminous object in this caſe muſt not oblige the pupil to contract. Beſides this animal being in a violent ſtate, gives attention to all that ſurrounds it, which muſt alſo oblige it to keep its pupil very open, as I have obſerved already.

For this reaſon nature has given fiſhes, which live in the water, a very convex, and almoſt ſpherical cryſtalline, that the rays of objects, which are in the water, and ſuffer but little refraction in paſſing thro' the *cornea*, may turn ſufficiently upon the ſurfaces of the cryſtalline, to be collected on the bottom of the eye. And if we find, that ſome divers perceive objects in the water at a greater diſtance than they would in the air, it can be nothing but a particular caſe of the conformation of the eye of theſe divers, who having the ſight very ſhort, becauſe of the very convex figure of their cryſtalline, can ſee very diſtinctly in the water, like fiſhes, diſtant objects of which the rays in the air would meet between the cryſtalline and the bottom of the eye, and meeting the bottom of the eye in a conſiderable ſpace would be there confounded, and conſequently would make a confuſed viſion.

We muſt now explain why, when the eye of the cat is immerged in water, we perceive diſtinctly all the parts of the bottom of the eye, as if it was not filled with humours.

It is certain, that the larger the windows of any room are, the brighter the objects will be therein, and the more diſtinctly ſeen; wherefore we have a better view of the parts of the bottom of the eye of the cat immerged in water, when the pupil is very much dilated, than if it was contracted. But it is not only the great aperture of the

the pupil, which makes us ſee objects diſtinctly, ſince in men, who have the *gutta ſerena*, and whoſe pupil is very open, we can perceive nothing at the bottom of the eye, which is expoſed to the air. It is therefore the water, which touches the eye, that makes us ſee theſe objects, and this is what we muſt explain by the ſame principles of opticks, which we uſed at firſt.

When an eye well formed is in the air, the rays which diverge from a point D * of its bottom, having paſſed thro' the three ſurfaces of its humours, turn from them in ſuch a manner, as to come out almoſt parallel to each other; wherefore we can ſee this object D diſtinctly; ſince rays that are parallel, or almoſt parallel, always make a diſtinct viſion in our eye, and yet we do not ſee this object D.

Let us now examine what muſt happen to the ſame rays, which diverge from the point D of the bottom of the eye of the animal, when it is immerged in water.

Let B B D, as before, be the eye of the animal immerged in water, of which the ſurface is A A. It follows, that the rays D B, which diverge from the point D of the bottom of the eye, being a little turned or refracted upon the two ſurfaces of the cryſtalline, muſt meet the *cornea*, while they are yet diverging: but as in coming out of the *cornea* in B B, they meet the water A A, the refraction of which is not ſenſibly different from that of the aqueous humour, where they paſſed in touching the *cornea*, they muſt continue their courſe in the ſame right line, and continue ſtill diverging quite to the ſurface of the water in A, whence at laſt they muſt go out to enter into the air, being yet more diverging than they were

* Fig. 5.

in the water by the laws of dioptricks ; and consequently wheresoever we place our eye to receive these diverging rays, which are then directed as if they came from the point E, nearer to the *cornea* than the point D, we may very distinctly perceive the point D, as placed at E and in the air.

This is what the plain surface of the water produces upon these rays ; but there is also another observation to be made, which shews us why we do not see the object D of the bottom of the eye, when it is out of the water ; and why we see it when it is immerged.

The surface of all well polished bodies sends back the light, and sends it back or reflects it so much the more strongly as it is more polished ; and if these polished bodies are also transparent, a part of the light will pass thro' the body, and another part will be reflected ; and this will always be in proportion to the transparence and polish. But as we have no bodies, whose surface is more polished than that of liquids, we might say there would enter into the eye exposed to the air, much fewer rays of light, than in the water, if the *cornea* was not always covered with a clear and unctuous liquor. This, therefore is not the reason that we do not see the bottom of the eye, when the *cornea* is exposed to the air ; and that we do see it, when the eye is in the water ; for if the rays of light are reflected upon the *cornea* in the air, they will be reflected also upon the surface of the water, and almost in equal quantity ; which is contrary to the opinion of some, who have pretended that a great many were lost upon the *cornea* in the air, and have not observed, that no fewer were lost upon the surface of the water.

But

But it is not ſo much the quantity of the rays that are reflected upon the *cornea*, or upon the water, that muſt be conſidered, in what may bring ſome interruption to a very clear viſion, tho' the rays are rightly diſpoſed to make it, as the direction of the ſame rays reflected. For if theſe reflected rays are parallel, or nearly ſo, to the *axis* of the eye, which meets the principal organ of viſion, where we ſee the objects moſt diſtinctly, and where the object, which we conſider attentively, is painted, we muſt ſee a pretty ſtrong light in this place, and this by its brightneſs will hinder the diſtinguiſhing of theſe objects, which otherwiſe are of a dark colour; and this will happen to the *cornea* of an eye, tho' the light illuminates it only aſlant. For the *cornea* being of a convex figure, ſome rays may ſtrike upon it obliquely, which will be directed almoſt according to the *axis* of the eye of him who looks; which does not happen to a plain ſurface, which would be perpendicular to this *axis*, where theſe rays would be reflected according to the ſame inclination to the ſurface, with which they had met it. Wherefore we can ſee much more diſtinctly, and without the mixture of this foreign light, the parts of the bottom of the cat's eye immerged in water, than if it was expoſed to the air. It is for this reaſon alſo, that when we are in the air out of a room, and look thro' glaſs, tho' ever ſo clean, upon the objects therein, we cannot ſee them without difficulty, becauſe of the inequality of the ſurface of the glaſs, which reflects the light every way.

We may make the experiment of what I here advance, by looking at an object thro' a round glaſs bottle, and afterwards thro' a piece of plain glaſs, the light playing in the ſame manner upon the ſpherical and plain ſurfaces of theſe two glaſſes: for

for the head of him that looks near would hinder the rays which ſhould fall upon the plain glaſs, and might be reflected in the eye towards the *axis* of viſion; but it will not be the ſame thing upon the ſurface of the glaſs bottle, where ſome will always enter the eye almoſt parallel to the *axis*, becauſe of the convex figure of the bottle.

In what I have hitherto ſaid, I did not think it neceſſary to expreſs what part of the eye I took for the principal organ of ſight. But one of the moſt famous anatomiſts of this company having examined the fact, which is the ſubject of this memoir, and having accounted for it very learnedly by the motion of the animal ſpirits in the eye of the cat, is for the *choroides* in oppoſition to the *retina*, following, as he ſays, the opinion of M. *Mariotte*.

M. *Mariotte*'s diſcovery is one of the moſt curious that has been made in philoſophy, and as the experiment is very eaſy to make, we could not doubt of it. Yet I here repeat, that the defect of viſion at the place where the *retina* is perforated by the *choroides*, proves nothing againſt the *retina*, and that the *choroides* can only be conſidered as an intermediate organ, which communicates to the *retina* the vibration or motion, which it receives from the light with its different modifications. And can we look for the principal organ of a ſenſe any where but in the nerves, which communicate with the brain, and can inform the ſoul under different appearances of what paſſes out of the body, and that by the interpoſition of a certain *medium* proper to move them; for the nerves are too delicate parts to be laid open.

It will be the ſame with regard to the other ſenſes, as to the ſight, and we cannot ſay, that the skin, which covers the whole body, is the prin-

principal organ of feeling, nor that the membrane of the drum of the ear is that of hearing; any more than that the skin of the tongue is that of tasting, because, when this skin is burnt, we have no sensation of tastes.

The black colour of the *choroides* is very proper to be sensibly shaken by all the different and least motions of light, as we see in the experiment of the white paper exposed to a burning mirror, which cannot be inflamed unless it is blackened; for the motion of the particles of the body which transmits the light, or the light itself, acts strongly among the points set with black bodies wherein it is engaged; whereas it is only reflected upon white bodies, which are composed only of very smooth parts like little mirrors. The *retina* therefore will not be shaken by a reflection of the luminous rays upon the *choroides*, which is black, as our anatomist pretends. In short, the conclusion of his memoir shews me, that he is not of M. *Mariotte*'s opinion, as he says he is, but that he has followed mine, changing only the definition of the principal organ of vision, which he ascribes to the *choroides*, and I to the *retina*. Thus the whole difference between him and me will be in the name of the principal organ, for he makes vision consist in a reflection of the luminous rays upon the *choroides*, and I in a shaking of the parts of the *choroides*, to be transmitted to the optic nerve or to the *retina*.

As for M. *Mariotte*'s opinion, he thinks, that the *choroides* is the principal and only organ of vision, and that this membrane alone carries to the brain the sensations of colours, since being a production of the *pia mater* it accompanies the optic nerve all the way to the eye, where being arrived, it forms the *choroides*; and lastly, that the optic nerve serves only

only to contain the ſpirits, and that it has no fibres.

But it ſeems to me not eaſy to conceive, how the ſoul can have a ſenſation of a very great quantity of objects, which are perceived all at once, and in the order in which they are ranged, without imagining an infinite number of very ſlender fibres, which compoſe the optic nerve, and are diſpoſed in order on the whole ſurface of the *retina*, which the membrane alone of the *pia mater*, or of the *choroides*, could not do without a great confuſion, even tho' it had fibres like thoſe of the optic nerve. But we ſee that the functions, which I have aſcribed to the *choroides* and to the *retina*, are both together neceſſary for viſion, and that one cannot be done without the other.

I could add alſo in this place, that we perceive colours only by a ſenſation of heat; for no body imagines there is light without heat, whether this light comes directly from the luminous body, or by reflection. But as this heat is uſually ſo faint, eſpecially if the luminous body is very diſtant from the body which it illuminates, there muſt enter into the eye a pretty large quantity of theſe rays, and at the ſame time they muſt meet in a point upon the black body of the *choroides*, to make a ſtronger impreſſion upon it, and to make no confuſion with thoſe which come from other luminous points, and quite near, and modified in different manners, which the ſenſe of feeling cannot perceive. This is a thought, which I think might be ſupported by very ſtrong reaſons.

VII. *An examination of a considerable difficulty proposed by M.* Huygens, *against the* Cartesian *system of the cause of gravity, by M.* Saurin *.

The most ordinary effects of nature, which are the least striking to the vulgar, are not always such as give the least degree of exercise to philosophers. Such is the *phænomenon* of gravity. A stone thrown up in the air falls down directly upon the surface of the earth; people do not use to be surprised at it: and yet to find the cause of this fall, is one of the most difficult problems to be resolved in physicks; and we are not yet arrived to a solution sufficiently demonstrated, which throws a full light upon all the difficulties.

I have undertaken a little treatise upon this subject, which I have begun to read in our particular assemblies. The academy may see, that I place the cause of gravity in the centrifugal effort of the celestial matter which surrounds us; and that I make this effort rise in it, from its circular motion about the *axis* of the earth, according to the notion of the *Cartesian vortices*. One of the principal objects that I have proposed in this little treatise, is to defend this opinion against the difficulties, which have made two of the greatest geometricians of our age, M. *Huygens*, and Sir *I. Newton* who reject the *hypothesis* of the *vortices*.

M. *Huygens* makes three objections against this *hypothesis*, in his discourse on the cause of gravity; but only two of them appear to me worthy of consideration. It is of one of these two, which has often been repeated after him by a great many authors of all sizes, that we find a solution in the

* April 10, 1709.

ſecond *Journal des Sçavans* for the year 1703. I was willing to expoſe this ſolution before-hand to the criticiſm of the learned, that I might be ſure not to deceive myſelf in thinking it ſupported by a true demonſtration, and to take advantage of the new lights, which their reflections might give me. It has merited the attention of two authors, who pretend, not without reaſon, to be profound in theſe matters, and are not much diſpoſed to favour me; but tho' they have combated it with a good deal of ſpirit, one in his *Recherches de Phyſique & de Mathematique*, and the other in the *Memoires de Trevoux*; I will venture to ſay, that they have not weakened the confidence that I had in the ſecurity of this ſolution.

The other objection of M. *Huygens* is to be the ſubject of this memoir, and I muſt own I am not yet perfectly ſatisfied about it. Therefore I ſhall not give this inquiry, as I did the former, the title of a ſolution, but of an examination.

In heavy bodies we perceive but two things clearly; one, that being let go in the air they move according to a direction which tends nearly to the centre of the earth; the other, that they endeavour to move according to the ſame line when they are retained: and it is exactly this effort with which they preſs, or puſh what retains them. that is called *gravity*.

It is evident, that theſe two things are the effect of one and the ſame cauſe. The force, of what nature ſoever, which makes the heavy bodies move according to the conſtant direction obſerved by them, is the very ſame that makes theſe bodies preſs according to the ſame direction, the plane oppoſed to retain them.

The queſtion therefore about gravity is, only to give the reaſon of a certain motion, namely, of that particular motion, which carries bodies towards the centre of the earth, which on that account are called heavy.

If we conſult our notions of the phyſical cauſe of motion, they will preſent us with nothing clear, nothing diſtinct but the ſhock or impulſe: thus it is by this principle that we muſt give a reaſon for the motion of which we are ſeeking for the cauſe, or abandon this inquiry, and give up the hope of ever being able to explain in an intelligible and reaſonable manner the *phænomenon* of gravity; and if we ſhould not ſucceed in explaining it by this principle, it will certainly ſhew the inſufficience of our knowledge, but not that of the principle.

See therefore according to this notion, in what manner we philoſophiſe upon gravity with M. *Huygens*. Heavy bodies move toward the centre of the earth; therefore they are driven thither. Bodies cannot be driven but by other bodies in motion which ſhock them; there are therefore other bodies in motion, which ſtrike thoſe which we call heavy, and by this ſhock drive them whither we ſee them tend. Theſe other bodies are not perceived; it is therefore a ſubtile matter, which the delicacy of its parts hinders us from ſeeing; and as we know beſides, by a thouſand other effects, that the earth ſwims in a fluid of an inconceivable ſubtilty, which ſurrounds it on all ſides, there is no room to queſtion its being to this ſubtile matter, that we are to aſcribe the impulſe, which produces the motion of the heavy bodies.

But how does it produce it? To explain this in order, I ſhould make long deductions; but I omit them, and come directly to the point. It is its circulating about the earth with an extreme rapidity: in this circulating it makes an effort to recede from the earth; and the groſs bodies not having the ſame motion, and not making the ſame effort, muſt be neceſſarily driven towards the earth. Thus far M. *Huygens* and I have gone hand in hand, and philoſophiſed in concert: but now we are going to part; and this is the point of our ſeparation. M. *Huygens* makes the celeſtial matter move circularly every way about the centre of the earth; that is, in his ſyſtem the centre of the earth is the common centre of all the circles deſcribed by the celeſtial matter: whereas, according to *Deſcartes*, it is all moved the ſame way about the *axis* from W. to E. and deſcribes circles, of which the planes are parallel to that of the equator. It is this *hypotheſis* that I defend againſt the two objections of M. *Huygens*.

The firſt is drawn from the direction which heavy bodies obſerve in their fall. M. *Huygens* pretends, that in the ſuppoſition of parallel circles, deſcribed by the celeſtial matter, the bodies ought to fall according to lines perpendicular to the *axis* of the earth, and that they would be driven toward the centre only in the plane of the equator; whereas we learn from the experiment, that they every where follow the ſame direction which tends to the centre. This is the objection, which I think I have ſufficiently anſwered in the *Journal des Sçavans*.

I ſhall now examine the ſecond. M. *Huygens* obſerves, that to produce the degree of gravity, which we find in the terreſtrial bodies, the velocity of the celeſtial matter, which moves circularly, muſt be much greater than the velocity of the daily motion of the earth about its own *axis*. Whence he concludes, that if the celeſtial matter

moved

moved the ſame way with a like velocity, it would be impoſſible for it not to carry away with it all the bodies which are upon the ſurface of the earth, by the continual effort of ſo rapid a motion, which does not happen.

I ſhall propoſe this objection in its full force. The bodies, which are upon the earth, being carried away with it about its own *axis* in 24 hours, neceſſarily make an effort themſelves to recede from the centre, and their effort is proportioned to the velocity, which carries them along. If the celeſtial matter moved circularly, only with the ſame velocity that the earth turns, it would make no more effort to recede from the centre of the earth, than the bodies do, which are upon the earth; and conſequently there would be no gravity; theſe bodies being thrown into the air would not fall back again. To whatſoever place of the ſurrounding fluid they ſhould be carried, and afterwards let go, they would remain ſuſpended and at reſt, as they would be *in equilibrio* with an equal bulk of the celeſtial matter.

The bodies therefore which are upon the earth, are heavy, and being thrown into the air, fall down again, only becauſe the celeſtial matter makes more effort to recede from the common centre than they do: and if we retrench their effort from that of the celeſtial matter, the quantity of effort which ſhall remain, and is the degree of force with which they are driven toward the centre, will be exactly equal to their degree of gravity. Thus the celeſtial matter muſt circulate faſter than the earth turns; and the exceſs of its velocity above that of the earth muſt be ſuch, that there may reſult from it this quantity of effort equal to the degree of gravity of the terreſtrial bodies.

M.

M. *Huygens* has found by an exact inquiry, that it required the circular motion of the celeſtial matter to be about 17 times as quick as that of the earth. This calculation is founded upon a curious propoſition; but it is a little embarraſſed. It may be made in an eaſier manner by ſuppoſing the truth of another theorem, which is very eaſily demonſtrated. This theorem is, that in equal time the ſpace run over by a body which falls perpendicularly, is to the ſpace or arch run thro' by the celeſtial matter, which moves circularly, and produces gravity, as the ſame arch is to the diameter of the circle which it deſcribes. And conſequently if the number of feet, which this diameter contains, is multiplied by the number of feet, which a body that falls perpendicularly runs over in a ſecond, this product will be equal to the ſquare of the arch, run over alſo in a ſecond by the celeſtial matter. We know by experiments made with a great deal of exactneſs, that a body, which falls perpendicularly, runs over about 15 feet in a ſecond: the diameter of the circle deſcribed by the celeſtial matter near the earth, not being ſenſibly different from that of the earth itſelf, is 39,231,600 feet. Therefore by the theorem, theſe 2 numbers multiplied together, will give a product equal to the ſquare of the arch run over by the celeſtial matter; and the ſquare root of this product, which is 24258, will be the number of feet equal to the arch run over. Therefore, to produce the degree of gravity, which we find upon the earth, the celeſtial matter runs over 24,258 feet in a ſecond.

The earth making a revolution in 23h 56′, or in 86,160″, and the circle which it deſcribes, being 123,249,600 feet, what it runs over in a ſecond muſt be 1430 feet ½. Thus the velocity of the

the celeſtial matter, which makes it run over 24,258 feet in a ſecond, is to that of the earth, which runs over only 1430 in the ſame time, as the firſt of theſe numbers is to the ſecond. Now if we divided theſe two numbers one by the other, we ſhall find they are nearly as 17 to 1. In meaſuring therefore the degree of gravity by the ſole centrifugal effort of the celeſtial matter, which comes from its circular motion, it is demonſtrated that the velocity of this motion muſt be 17 times as great as that of the daily motion of the earth, or ſurpaſs it 16 times.

But to know ſtill more exactly how far the difficulty goes, let us examine what impreſſion this prodigious velocity, which we are obliged to aſcribe to the celeſtial matter, can make upon the terreſtrial bodies, and we ſhall ſee if any means will offer to render it inſenſible.

The late M. *Mariotte* made a great number of experiments on the force of the ſhock of fluids, and in particular of water and air. He has found*, that water going with a velocity, which makes it run thro' 3 feet $\frac{1}{4}$ in a ſecond, and with this velocity ſtriking perpendicularly a ſurface of $\frac{1}{2}$ a foot ſquare, ſuſtains a weight of 3 lb. $\frac{3}{4}$. He has alſo determined, that the air going 24 times as faſt, made exactly the ſame effort. Thus the air running thro' 78 feet in a ſecond, and with this velocity ſhocking a ſurface of $\frac{1}{2}$ a foot ſquare, oppoſed perpendicularly to its courſe, would ſuſtain a weight of 3 lb. $\frac{3}{4}$: but if we allow it the velocity by which the celeſtial matter ſurpaſſes that of the earth, what weight will it ſuſtain? It is eaſy to calculate it. The efforts of the ſame fluid, which goes with different velocities, are to each other as the ſquares of the velocities. The velocity

* *Mouvement des eaux*, p. 187, and 195.

of

of the air, which makes it ſuſtain 3 lb. $\frac{3}{4}$, is 78 feet in a ſecond; that of the celeſtial matter, the velocity of the earth being ſubſtracted from it, is 22,827 $\frac{1}{2}$: ſay, as the ſquare of 78 is to the ſquare of 22,827 $\frac{1}{2}$, ſo is the weight of 3 lb. $\frac{3}{4}$, to a 4th term: this 4th term will give the effort of the air, or the weight ſought. In performing this operation, we find, that if the air went with the velocity of the celeſtial matter, it would ſuſtain a weight of above 320,000 lb.*

In this calculation, we have followed the determination of M. *Mariotte*, who allows to air a velocity only 24 times as great as that of water, to make it ſupport the ſame weight that water does; but other experiments prove, that it muſt go 30 times as faſt; and if we follow theſe experiments, the weight which the air will ſuſtain with the velocity of the celeſtial matter, will be diminiſhed, but yet it will be more than 200,000 lb†.

Such would be the force of the air carried along with the velocity, which agrees with the celeſtial matter to produce gravity. Whence we ſee, that tho' the effort of the celeſtial matter moved with this rapidity ſhould be but $\frac{1}{200000}$ part of that of the air, it would however ſuſtain the weight of one pound, by acting againſt a ſurface of $\frac{1}{2}$ a foot ſquare, and that if it was near 2,500,000 times weaker, it would ſtill ſuſtain the weight of an ounce; ſo that if a body that weighed but an ounce was ſuſpended in the air at the end of a thread, and oppoſed a ſurface of half a foot ſquare

* It is exactly 321,187 lb. and 14,508/24,336

† Exactly 205,560 lb. and 2,340/38,025

to

to the courſe of the celeſtial matter, it would drive it from weſt to eaſt, with an effort which would cauſe it to make that way an angle of 45 degrees, abſtracting all other reſiſtance but that of the ſuſpended body.

It would be impoſſible, becauſe of the reſiſtance and continual agitation of the air, and of ſeveral other conſiderations, to determine exactly how much the effort of the celeſtial matter with equal velocity muſt be weaker than that of the air to become inſenſible, but it appears to me, that it muſt be 3 or 4 millions of times. It remains to know, whether it can be ſuppoſed without abſurdity, or whether we can give any probable reaſon of the weakneſs of this effort.

We know that ſome fluids are more or leſs fluid than others, and that they make more or leſs reſiſtance to the motion of bodies; and conſequently more or leſs effort againſt bodies at reſt, when the fluids themſelves are in motion. Thus we have juſt ſeen, that the air muſt go 30 times faſter than water, to have an equal *impetus*: whence it follows, that going with the ſame velocity as water, it muſt make 900 times leſs effort than water, 900 being the ſquare of 30. The rule which is given upon this point, is, that the efforts of different fluids which go with the ſame velocity, are as their denſities; it is upon this principle alſo, that we make the air 900 times thinner than water. This conſequence however might be falſe; for the rule upon which it is founded, is not exactly true, but when the fluids compared differ only in denſity. In that caſe it is eaſy to comprehend, that if, of 2 fluids carried along with the ſame velocity, one is for inſtance twice leſs denſe than the other, it muſt make twice leſs effort; for at each time the body, against which it acts, is struck by twice fewer particles, and consequently is twice less struck. The rule therefore is certain and evident, but it is defective, because there are in fluids a great many other differences to be regarded. The force of the shock in those, which go equally fast, does not only depend on this, that in equal time they strike with the sum of the efforts a greater or less quantity of particles; but in this also, that they make more or less resistance to the division; that is, the particles have more or less ease to be separated or displaced. Now a greater or less facility of being displaced may have several causes, and by the concourse of all these causes become as considerable as we please.

The first cause that presents itself, is the different degree even of density. I have made use of density already: it is a double use that I make of it, but not a bad one; and it comes here under another consideration. It is plain, that a fluid must be so much the more easy to divide, as its particles are less close, and less near to each other; that is, so much the more as it is the less dense. The more or less asperity, or inequality in the surfaces of the particles, and their figures more or less irregular and embarrassing, are two other causes worthy of attention, which may produce great differences, with regard to the facility of fluids to divide, and consequently in the force of their shock.

I thought at first, that I might add to these articles the different degree of subtilty. And indeed it was natural enough to think, that, supposing all other things equal, the fluid, which had its particles the least gross, should be divided with most facility, and make least effort against the obstacles opposed to its course. This thought quite pleaſed me: it furniſhed me with the eaſieſt way in the world to reduce to nothing the force of the ſhock of the celeſtial matter, which we may make as ſubtile as we will: but in ſeeking to demonſtrate a propoſition, which appeared to me ſo probable, I have found, contrary to my expectation, tho' after Sir *I. Newton*, that it was falſe, and that two fluids of the ſame nature, and denſity, which differ only in the ſmallneſs of their particles, make an equal reſiſtance to the motion of bodies; or, if the fluids themſelves move, have an equal *impetus*. I confeſs I have been much grieved upon this head, and it was not till after thoroughly diſputing againſt my own demonſtration, that I conſented to throw it aſide.

However, let the notion be ever ſo falſe, from which I thought I might draw ſo great an advantage; yet the more or leſs ſubtilty is ſtill a material conſideration in another part; for a fluid, which ſhould be ſo ſubtile, that all bodies would give it a free paſſage thro' their pores, would without doubt ſtrike theſe bodies with much leſs force than another fluid of the ſame nature would, of which the particles would be too groſs to be able to paſs thro' the pores of the bodies. It is evident, that tho' theſe two fluids were of the ſame denſity, they would fall in proportion to the effect of the ſhock in the caſe of two fluids unequally denſe; all that in the ſubtile fluid continues its courſe thro' the pores of the bodies, freely and without ſhocking them, not being to be reckoned. Now how far may not that go?

The texture of the moſt ſolid bodies is perhaps infinitely more rare than we think. What is very certain, is, that the ſenſes and imagination deceive us therein. If we conſult them, who would ſay that what a bit of oak contains of its own proper matter makes but the 20th part of the bulk under which it appears? Perhaps it may not make the thousandth part, or the hundred thousandth; but at least it is easy to demonstrate that it does not make the 20th. The wood of oak weighs less than water, and water weighs near 19 times less than gold. A bit of oak therefore weighs more than 20 times less than a bit of gold of the same bulk: but it is a principle demonstrated by M. *Huygens* himself, that the specifick weight of bodies exactly follows the proportion of the quantity of proper matter which they contain under an equal bulk. Upon this principle, a bit of oak contains 20 times less proper matter than a bit of gold of equal bulk; and consequently, by supposing the gold perfectly solid, and without pores, which is very far from being true, the quantity of proper matter, which a piece of oak contains, is not the 20th part of its bulk; certainly our eyes do not tell us so. By the same reasoning, a body, which shall weigh 20 times less than an equal bulk of oak, and 400 times less than an equal bulk of gold, will also contain 20 times less of its proper matter than the oak, and 400 times less than the gold: do the eyes judge thus of it?

I have no light into the absolute solidity of bodies: I know by the weight the different proportions of density or rareness between them; but if we consider a body in itself, and without comparing it to others, it is impossible to know what its absolute degree of solidity is; that is, to determine what proportion there is between the quantity of proper matter that it contains, and its bulk: thus I know that a piece of oak is 20 times less solid than an equal piece of gold; but then to what degree is this piece of gold solid? How

many

many pores has it? How much proper matter? This I am abſolutely ignorant of; or rather I know with the utmoſt evidence, that it cannot be known; and I dare advance this propoſition, which may ſeem a paradox, that if we would maintain, that in a piece of gold, there is not of proper matter the hundred millionth part of its bulk, we might indeed maintain it without a poſitive proof, but we might boldly defy the natural philoſophers to demonſtrate the contrary.

I do not doubt but the imagination of thoſe, who judge of every thing by their ſenſes, is ſhocked at it. Gold is the moſt heavy of all the bodies that we know: it has always ſeemed very ponderous to them, and therefore very maſſy; this confuſed notion will always paſs among them for an experiment as evident as a demonſtration: but when we ſupport a weight, the ſenſe of gravity that we have is relative to the degree of ſtrength that we have to ſuſtain it: what a man finds light is an enormous weight for a child, and we might have ſuch a ſtrength, that the moſt heavy maſs would ſeem as light as a feather. Thus in judging by the ſenſe, men a thouſand times ſtronger than we, finding gold 1000 times leſs heavy than we find it, would alſo judge it to be 1000 times leſs ſolid than we judge it to be. To conclude, as neither the ſenſes nor the imagination are to be heard upon this point, and as reaſon does not fix any bounds for us, we may give to the texture of bodies all the rareneſs, as well as to the celeſtial matter all the ſubtleneſs of which we have need; provided only that the ſuppoſition which we ſhall make for the effect that we would explain, is not oppoſed by other effects.

Here is another article, upon which we cannot build too much, which is referred to that of the figures

figures more or leſs embarraſſing; that the particles of the celeſtial matter have neither a determinate figure nor bigneſs; each particle being able to divide, and dividing infinitely, according as there is occaſion, and with the utmoſt facility, they accommodate themſelves without difficulty to all ſorts of places; which diminiſhes infinitely in the fluid its reſiſtance againſt being diſplaced, and ſo much weakens its effort.

To all that we have ſaid upon the cauſes which may contribute to render the effort of the celeſtial matter inſenſible, we may add thoſe experiments of Sir *I. Newton*, which are in our favour. He made them to determine whether the celeſtial matter, which penetrates all bodies, and fills their pores, had any ſhare in the reſiſtance, which theſe bodies ſuffer when they are moved in a fluid; and he has not found more reſiſtance on that ſide, than if this matter did not exiſt, and the pores were entirely void. We ſhall not take advantage however of his diſcovery: what conſequence could we draw from an inſenſible reſiſtance in ſuch weak motions as thoſe of the experiments which we can make? But it is a matter of great ſurprize, that ſo able a man as Sir *I. Newton* ſhould conclude the *vacuum* from it, or be near concluding it, inviting us alſo to repeat the experiments, to convince ourſelves more and more of the pretended ſolidity of this concluſion.

If after all the conſiderations that have juſt been made, we ſhould be ſtruck as with an abſurdity, with this prodigious rapidity, which we aſcribe to the celeſtial matter near the earth, tho' it does not make itſelf felt there, there ſeems to be no other way to take, than to digeſt this abſurdity, as we are obliged to digeſt ſo many others in moſt phyſical ſubjects, and generally in moſt of the objects

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of our knowledge: for, in ſhort, this abſurdity, whether pretended or true, to which the opinion, that I defend, leads, is found to be a neceſſary conſequence of the moſt certain obſervations of the aſtronomers, as I am going to demonſtrate.

The planets, which turn about the ſun at different diſtances, go ſome of them faſter than the others: the famous *Kepler* was the firſt, who obſerved, that their velocities keep an inverted *ratio* of the ſquare roots of their diſtances. Suppoſe for example, that the diſtance of *Venus* from the ſun is to that of *Mercury*, as 9 to 4; I take theſe numbers, becauſe they are convenient, and not very different from the exact proportion that theſe two diſtances have between them; the ſquare root of 9 is 3, that of 4 is 2: the ſquare root of the diſtance of *Venus* being therefore to the ſquare root of the diſtance of *Mercury*, as 3 to 2, we find, according to *Kepler*'s rule, that in an inverted *ratio*, the velocity of *Venus* is to that of *Mercury*, as 2 to 3.

All the obſervations confirm this rule; it is not only followed by the principal planets, which turn about the ſun; but alſo exactly by the little planets, which make their revolutions about a principal one; this M. *Caſſini* has fully verified in the ſatellites of *Jupiter*, and given us a theory of them, and by his learned and uſeful diſcoveries has a very great ſhare in the glory of the progreſs, which aſtronomy has made in our days, and a great one in the glory even of that which it ſhall make after. It is the ſame with the 5 ſatellites of *Saturn*, as with the 4 of *Jupiter*. It is therefore a law inviolably obſerved by the celeſtial bodies, in the ſmall particular *vortices*, as well as in the great one: and as the moſt reaſonable *hypotheſis* of the motion of the planets, or rather

rather the only reaſonable one is, that they follow the courſe of the celeſtial matter, which carries them along, it is to the different velocities of the celeſtial matter taken at different diſtances from the centre of the *vortex*, that *Kepler*'s rule muſt be applied.

To come now to the demonſtration, which I have promiſed; if by this rule we find the velocity, which agrees with the celeſtial matter near the earth, we ſhall find that it muſt be to that of the earth almoſt as 17 to 1, ſuch exactly as we have already ſeen that the degree of gravity of the terreſtrial bodies required: the calculation of it is neither long nor difficult.

The moon being diſtant from us, or from the centre of our particular *vortex* about 60 ſemi-diameters of the earth, the circle, which it runs thro' about this centre, is 60 times as great as that deſcribed by a point of the ſurface of the earth under the equator; and conſequently it has 60 times more way to go to finiſh its revolution, than this point has. Thus, if the moon ſhould finiſh its revolution only in 60 days, it would go as faſt as the earth which turns in a day: if the revolution of the moon was finiſhed in 30 days, its velocity would be double that of the earth under the equator: the moon employing but a little more than 27 days and half in its courſe, it follows that its velocity is a little more than double that of the earth. This being ſuppoſed, the diſtance of the celeſtial matter, which circulates here below, and is diſtant from the centre of the *vortex* only one ſemidiameter of the earth, and the diſtance of the moon, which we have made 60 of theſe ſemidiameters, are to each other, as 1 to 60, and their ſquare roots nearly as 1 to 8, or as 2 to 16, or as a little more than 2 to 17; therefore in an

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inverted *ratio*, conformably to *Kepler*'s rule, the velocity of the celeſtial matter near us is to the velocity of that, which carries on the moon, as 17 to a little more than 2 ; but we have found that the velocity of the moon, or of the celeſtial matter, of which it follows the courſe, was really to the velocity of the earth, as a little more than 2 to 1 ; therefore the velocity of the celeſtial matter here below, is to the velocity of the earth nearly, as 17 to 1. *Q. E. D.*

Such is the perfect agreement, between what velocity the *phænomenon* of gravity requires in the celeſtial matter, and what we find elſewhere, that it muſt have in virtue of a law eſtabliſhed by the obſervations, aud demonſtrated as the fundamental law of the whole ſyſtem of the univerſe, by the ingenious author of *The New Explanation of the Motion of the Planets.* If ſo wonderful an agreement does not entirely deliver the mind from the trouble, which this rapid motion of the celeſtial matter near the earth gives it, of which however we do not perceive any ſenſible effect ; it muſt at leaſt diſpoſe it to receive more favourably the conſiderations, which we have propoſed to reſolve, or weaken the objection of M. *Huygens.*

It is true, that a great many difficulties preſent themſelves ; and I ſhall not diſſemble, that this very law, which the velocities of the planets follow, when conſidered in the celeſtial matter, is ſurrounded with difficulties ; there are ſeveral which a little attention diſſipates ; it would be tedious and uſeleſs to dwell upon them : there are others more conſiderable, and among theſe two principal ones, which I ſhall touch upon in a few words.

The firſt offers itſelf immediately, and it is impoſſible not to be ſtruck with it. According to *Kepler*'s rule added to the *hypothesis* of our *vortices*, the celestial matter makes 17 revolutions about the earth in a day, whence comes it, that the earth makes but one? Why does not it follow the rule? This difficulty is common to the other *vortices*; *Jupiter* and *Saturn* turn each about his own centre; and both of them infinitely less quick than they ought to do according to the rule. The sun, which occupies the centre of the great *vortex*, turns in like manner about his own *axis*, and takes about 27 days and half in turning; whereas, according to the rule, it ought to employ but a little more than 3 hours. I confess I am not satisfied with the lights that I have into this difficulty, and that I have not any more solid answer to give, than that which may be seen in the new explanation of the motion of the planets, a work which it would be more easy to criticize, than to make a better.

The other difficulty is Sir *Isaac Newton*'s. In the midst of an uniform fluid, and at rest, that is, which has no other motion than the mere agitation of its parts every way, which renders it fluid, he conceives a solid sphere, which turns about an *axis*, almost like the earth. This sphere, as it turns, makes a continual impression on a first surface of the fluid, and this upon another, and this last upon another, and so on. On this arbitrary supposition, he inquires geometrically in what proportion the motion is communicated from each surface to the next, or what should be the proportion of the velocities at different distances from the common centre; and his *analysis* giving him a different proportion from that which is observed in the planets, he concludes that they are not carried along by the fluid, and that the *Cartesian*

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vortices are incompatible with the law eſtabliſhed by *Kepler*.

I paſs over a great number of reflections that might be made on Sir *Iſaac Newton*'s demonſtration: I am willing to admit it, but when I do admit it, I reject however the concluſion that he draws from it againſt our *vortices*. It has no force but in virtue of the ſuppoſition, which Sir *I. Newton* takes for granted, of a fluid perfectly uniform, and every where of equal fluidity, and of a reſiſtance on the ſide of the ſurfaces, in the *ratio* of the velocity. But if we ſuppoſe the fluidity to augment in proportion as it recedes from the centre, or a reſiſtance greater than in the *ratio* of the velocity, we ſhall find without difficulty the ſame proportion that is given by the rule.

What we ſay here has not eſcaped Sir *Iſaac Newton*'s exactneſs; he has expreſly obſerved it; but he contents himſelf with ſaying theſe ſuppoſitions would not be reaſonable; and tho' the laſt is inconteſtable, he chooſes rather to conſider gravity as a quality inherent in bodies, and to renew the exploded notions of occult qualities and attraction. We muſt not flatter ourſelves, that in all our phyſical inquiries, we can ever ſurmount all difficulties: but however let us always philoſophize upon clear, mechanical principles; if we quit them, all the light that we can have is extinguiſhed, and we are plunged anew into the old darkneſs of peripateticiſm, from which heaven preſerve us.

VIII. *Obſervations of the weight of the atmoſphere, made at the caſtle of* Meudon, *with M.* Huygen's *double barometer, by M.* de la Hire * ; *tranſlated by Mr.* Chambers.

The *Abbé de Louvois* having a curioſity to ſee the practice of levelling, and how the weight of the atmoſphere is found by obſervations of the barometer, I made the following ones in his preſence, with all the accuracy poſſible.———We had the uſe of a very good teleſcope level, and one of M. *Huygens*'s double barometers, which we found in the caſtle.

One morning at the bottom of the caſtle, the liquor in the tube of the barometer ſtood at 33 diviſions ½ ; upon which deſcending by the iron grate in the great road leading to *Verſailles*, we found the liquor in the tube fallen to 28 diviſions ½, the ſpace deſcended being 159 feet, 3 inches, and the fall of the liquor 5 diviſions from the firſt ſtation.

Continuing then to deſcend in the great road leading to *Paris*, as far as the opening of a little path, which goes to the river, we found the liquor in the tube at 24 diviſions ½, where the ſpace we had deſcended was 106 feet, 3 inches, and the fall of the liquor from the former ſtation was 4 diviſions.

From this ſtation to the river near the mills, we deſcended 134 feet, 3 inches, when the liquor was found in the tube at 21 diviſions, and conſequently had fallen 3 diviſions ½.

After noon the barometer was carried to the wall of the mill-pond, at the top of the park,

* June 5, 1709.

where

where the liquor in the tube ſtood at 38 diviſions ½, and by the levelling it appeared, that we had aſcended above the level of the caſtle 112 feet, 4 inches.

But returning in the evening to the caſtle, I found the liquor in the tube at 36 diviſions, and conſequently for theſe 112 feet, 4 inches, the liquor had altered 2 diviſions ½; but having found it in the morning at 33 diviſions ½, we learned that between morning and evening a change of 2 diviſions ½ had happened in the weight of the atmoſphere.

The whole height from the river to the mill-pond was exactly level at ſeveral ſtations, and found agreeably to the preceding obſervations 512 feet, 1 inch, or 85 fathoms, 2 feet, 1 inch, being the greateſt elevation about *Paris*.

Towards the evening I found the difference between the ſurface of the mercury in the two ciſterns of the barometer at the bottom of the caſtle was exactly 29 inches, and the liquor in the tube was 12 inches ½ above the mercury in the lower ciſtern. The diviſions of the tube for meaſuring the height of the liquor, were equal to 4 lines and $\frac{5}{9}$, which I take for 4 lines ½, on account of the ſmallneſs of the difference, and for the eaſe of calculation.

Now to deduce the exact weight of the atmoſphere from theſe obſervations, they muſt be reduced according to the ſtructure of this barometer, as already explained in our memoir of barometers. But firſt, in order to compare the heights of the liquor between the ſide of the river, and the top of the mill-pond, the obſervations muſt be reduced to the ſame hour, by reaſon of the change which happened in the weight of the atmoſphere between morning and evening; and as the

the obſervation at the river-ſide was made about noon, I ſhall reduce it to that made at the mill-pond in the evening, when the liquor was at 38 diviſions ½, on a ſuppoſition that the diminution of the atmoſphere proceeded uniformly from morning to evening. Hence inſtead of 21 diviſions obſerved by the river-ſide, I take 22 ¼ by adding ½ the difference between morning and evening, and ſubtracting theſe 22 diviſions ¼ from 38 ½, the remainder 16 diviſions ¼ gives the alteration of the height of the liquor in an aſcent of 512 feet under a conſtitution of air, ſuch as that in the evening of the ſame day.

The reduction of the diviſions of the tube to the real height of mercury, correſponding to the weight of the atmoſphere, will be eaſily made by the rules I have already given, and the obſervation I made in the evening of 29 inches difference between the heights of mercury in the ciſtern, when the liquor was 12 inches ½ above the mercury in the lower; for ſuppoſing what I have actually found, that the weight of mercury is to the weight of the liquor in the tube, as 12 to 1, dividing 150 lines (to 12 inches ½) by 12, we ſhall have 12 lines ½ for the height of mercury, equivalent to 150 lines of liquor. We muſt ſubſtract therefore 12 lines ½ from the 29 inches difference between the heights of mercury in the ciſterns, and the remaining 27 inches, 11 lines ½ will be the height of mercury, which weighs as much as the atmoſphere on the day of obſervation towards the evening at the height of the plain of the caſtle at *Meudon*, which is 66 fathoms, 4 feet above the river *Seine*, againſt the mills in the month of *September*, when it is uſually very low.

It

It remains to find the value of the diviſions of the tube, with regard to the heights of mercury, which repreſent the weight of correſpondent diviſions of the atmoſphere. In theſe barometers, which are formed according to the proportions given by M. *Huygens*, where the diameter of the ciſterns is 14 lines, and that of the tube 1 line, we ſhall have it by a rule found in my former memoir, as 12 times the ſquare of the diameters of the ciſterns to the ſquare of the ſame diameter + 23 times the ſquare of the tube; ſo are the diviſions of the tube, or the heights of the liquor to the heights of mercury repreſented by them, which is here as 2352 to 219; wherefore the 16 diviſions $\frac{1}{4}$, found between the higheſt and loweſt, which anſwer nearly to 73 lines, has about 6 lines $\frac{3}{4}$ for the true height of mercury, correſponding to the change of weight of the atmoſphere, between the river-ſide and the wall of the mill-pond in the park; ſo that dividing 512 feet, which is that height by 6 $\frac{3}{4}$, we ſhall have 75 and $\frac{23}{27}$ or 12 fathoms, and nearly 4 feet height of atmoſphere for a line of mercury, at a time when the weight of the whole atmoſphere was 27 inches, 11 lines $\frac{1}{2}$, at the ſurface of the ground in the caſtle of *Meudon*; and above the river, when it is low againſt the mills, at the foot of the mountain, 66 fathoms, 4 feet. ——— We here make no account of the different weights of the atmoſphere, in the different parts of this height, nor of the different aſcents of the liquor, which might have aroſe from the different heat at different times of the day, which dilates all liquors more or leſs, and even mercury itſelf; for that the heat was pretty much the ſame at the beginning and ending of the obſervations.

But as in the making theſe barometers they might have deviated a little from the proportion above-mentioned, between the diameters of the cisterns and the tube, I have made a *calculus* of what would ensue upon other proportions, and find that the difference would be very inconsiderable, tho' the diameters of the cisterns were 1 or 2 lines either bigger or less.

Tho' it cannot be doubted, that to find the weight of the atmosphere, it is much surer to go upon great heights than upon small ones, provided such heights be exactly known by reason of the difficulty of making an exact estimate of the heights of the mercury in the tube, yet I have not thought it amiss to make observations of lesser heights in order to find how they would agree with those of *Meudon*.

Accordingly I have several times observed the height of the mercury in different seasons, and different years, at the top of the terrass of the observatory, and the bottom of the vaults and cellars thereof, in the single barometer; and taking a medium between all these heights which agreed with an observation I had made in *Sept.* 1705, the time when the air is nearly of the same heat in the vaults, as at the top of the mercury of the barometer then standing at 28 inches in the large hall; and consequently the atmosphere being very heavy, as it was when the observations were made at *Meudon*; and the season being likewise the same, I found a change of 2 lines $\frac{1}{4}$ in the heighth of the mercury, for 28 fathoms or 168 feet height of the atmosphere; and consequently for 1 line of mercury we have 74 feet $\frac{2}{3}$, or 12 fathom 2 feet $\frac{2}{3}$; and by the observations made at *Meudon* I found, for the same line of mercury 12 fathoms 4 feet, the difference between which, *viz.* 1 foot $\frac{1}{3}$ is very inconsiderable in such observations.

Another

Another obſervation which I made at *Toulon* in 1682, upon the mountain *Claret*, which is 257 fathoms above the ſurface of the ſea, gave me in that ſeaſon, and under thoſe circumſtances of air, ſuppoſing the air equally denſe in this whole height, 12 fathoms for 1 line of mercury.

But it being certain that heat and cold may occaſion ſome alterations in barometers wherein the weight of the atmoſphere has no concern, as I have ſhewn in the memoir already cited; by reaſon ſome part of the air next the earth being heated more or leſs than the reſt, will make a change in the bulk both of the mercury and the liquor; beſides, that a moiſt air when heated, dilates more forcibly than a drier, and conſequently will ſuſtain the mercury to a height beyond what it would have from the bare weight of the atmoſphere, *&c.* I have therefore made ſeveral obſervations and experiments to bring all theſe effects to ſome further rules.

Placing a ſingle barometer aſide of one of M. *Huygens*'s double ones, and of M. *Amontons* thermometers by them, I obſerved their ſeveral heights every day for 3 years together, without over-looking the leaſt circumſtance that might have any concern therein, but there having happened no conſiderable cold in all that time, but only violent heats in the ſummer, I compared the ſtate of theſe barometers in the great heat, with that they were in at the mean heat of the air, as it is found in the vaults of the obſervatory, or at moſt when it begins to freeze; and I found that the mercury in the ſingle barometer does not undergo any ſenſible change of height, whether it be expoſed to the open ſun in the heat of ſummer, or be in the ſhade in a place moderately cold.

In the following obſervations I expreſſed the height of the mercury in the ſingle barometer, by inches, lines, and points, which are 6th parts of lines, the heights of the liquor in the double barometer being expreſſed by the diviſions on that barometer which are each equivalent to 4 lines, but are reduced afterwards into lines.

	inch.	lin.	poi.
I. The ſingle barometer ſtanding at ———	27	6	4
The double barometer in the great heats ſtood at ———	45 div.		$\frac{2}{3}$
And in a moderate degree of heat at	41		$\frac{1}{2}$
The difference is	4		$\frac{1}{6}$
Or		16	$\frac{2}{3}$
II. The ſingle barometer ſtanding at ———	27	8	0
The double barometer in the great heats ſtood at ———	42 div.		
And in a moderate degree of heat at	37		$\frac{1}{4}$
The difference is	4		$\frac{3}{4}$
Or		19	0
III. The ſingle barometer ſtanding at ———	27	11	1
The double barometer in the great heats ſtood at ———	33 div.		
And in a moderate degree of heat at	28		$\frac{1}{4}$
The difference is	4		$\frac{3}{4}$
Or		19	

VI.

	inch.	lin.	poi.
IV. The ſingle barometer ſtanding at ———	27	9	1
The double barometer in the great heats ſtood at ———	38		½
And in a moderate degree of heat at	34		
The difference is	4		½
Or		18	
V. The ſingle barometer ſtanding at ———	27	10	0
The double barometer at a moderate degree of heat ſtood at ——	33		⅓
And the barometer being removed to the open air when it began to freeze ſtood at ———	30		⅔
The difference is	2		⅔
Or		10	⅔

Being deſirous likewiſe to find what would befall the double barometer, when expoſed to the ſun about noon, in the greateſt heats of *July*, in the year 1708, and the better to diſcover the effects thereof, I placed M. *Amontons*'s little ſpirit of wine thermometer aſide of it.

I obſerved the liquor in the barometer at firſt riſe very ſlowly in compariſon of the ſpirit of wine in the barometer, but after they had ſtood upwards of an hour in the ſun, carrying them back to their former place, which is in the ſhade, I obſerved the liquor in the barometer ſtill continued riſing, while the ſpirit of wine, on the contrary, kept deſcending apace, to recover its former ſtate. Now tho' ſpirit of wine be very ſenſible of heat,and water very little in compariſon thereof; yet one would expect the ſame thing to befal

fal the water in the barometer, and the ſpirit in the thermometer, and that the cauſe ceaſing, the effect muſt ceaſe likewiſe. We find, however, that the mercury having received a much greater degree of heat than the liquor, and preſerving it longer withal, by reaſon of its greater denſity, continues ſtill to heat the liquor, even when removed from the ſun, and thus raiſes it conſiderably higher, than it was therein; and the rather, as the bulk of the mercury undergoes no ſenſible change by heat and cold, as I found by expoſing the ſingle barometer to a hot ſun.

As to the ſpirit in the thermometer, the caſe is very diffetent; for being a fluid very eaſy to be dilated by the ſmalleſt heat, it condenſes again with equal eaſe upon the ſmalleſt cold.

'Tis beyond doubt, that the different heights above-mentioned, between the liquor in the double barometer, while the ſingle barometer remained at the ſame height, and conſequently the atmoſphere was equally heavy, ariſe chiefly from the dilatation of the liquor; whereof there is ſufficient quantity in the phial, at the bottom of the barometer, and its tube ſlender: for upon the ſmalleſt ſwelling of this liquor, by the heat, a very ſenſible proof of it muſt appear in the little tube, which however does not obtain ſo much in my barometer; where, the liquor being but little, the elevation, occaſioned by the heat, is inconſiderable. I have ſhewn however how it may be applied, without falling into any error, by confounding the effect of the barometer with that of the thermometer, which, in the double barometer, occaſions great irregularities.

IX. *A comparison of the barometrical observations, made in different places, by M.* Maraldi *.

To arrive at the knowledge of the cause of the *phænomena*, observed by the means of the barometer, it is not sufficient to have observations made only in one place, it is necessary to make them also in different countries, to compare these observations together, to observe the conformity between them, and their differences.

Without a great number of these observations, we are liable to mistakes in explaining by causes, which would suit only a particular country, *phænomena* which may have more general causes; and we might consider, as a property of the whole mass of air, what agrees with it only in some circumstances, or in a certain extent of country.

Several learned men, who have perceived the physical uses that may be drawn from barometrical observations, have applied themselves for some time to make them in different countries. The marquis *Salvago* having communicated to me those which he had made at *Genoa* 3 years ago, I have compared them with our own, which were made at the same time at the observatory. In comparing these observations, we have found some, which had particulars in them, which I have thought worthy of being remarked. I shall afterwards relate some experiments on the dilatation of the air, made near the equinoctial, which I have had occasion to examine.

The marquis *Salvago*, in his observations made at *Genoa*, used a simple barometer, divided into

* July 20, 1709.

inches

inches and lines of the *Paris* foot. This barometer is situated in an apartment, where the quicksilver remains a line lower than at the sea shore, as has been found by observation; so that if we would reduce the observations of *Genoa* to the level of the sea, we must add a line to each height of quicksilver, which I shall hereafter mention.

In the relation of these observations, we shall not follow the order of time in which they were made; but I shall begin with the most remarkable.

In 1707, at *Paris*, from *Nov.* 15 to 18, the barometer continued for 4 days at the height of 28 inches within about ½ a line; the next day, *Nov.* 19, it fell to 27 inches, 4 lines, having fallen 8 lines in 24 hours; the next day, it rose again 10 lines, being on *November* 20, at 28 inches, 2 lines; during this variation the constitution of the air did not change the sky having been very calm and serene.

The same year, at *Genoa*, from *Nov.* 15 to 18, the quicksilver continued at the height of a little more than 28 inches, as it had been the same days at *Paris*. The next day, *Nov.* 19, at *Genoa*, the wind being S. the barometer fell to 27 inches, 5 lines, having fallen in one day 7 lines at *Genoa*, almost as it did the same day at *Paris*. It remained only that day in the same situation; but it rose again the next day to 28 inches; and the 21st, to 28 inches, 2 lines, as it happened at *Paris*, the wind was turned to the N.

The same year, from *Nov.* 20 to 28, the barometer remained at *Genoa* and at *Paris* generally at 28 inches, 1 line. During these 8 days at *Paris*, the wind was sometimes at W. and sometimes

times at N. W. at *Genoa*, the wind was always N.

Nov. 30, at *Paris*, the barometer fell to 27 inches, 0 lines, the wind being N. W. *Dec.* 1, it roſe again to 27 inches, 10 lines, the wind being W, and the weather fair; the next day, it roſe 2 lines more, having been at 28 inches; ſo that at *Paris*, from *Nov.* 28 to 30, it fell above an inch in two days; and from *Nov.* 30 to *Dec.* 1, it roſe 10 lines in 24 hours.

The ſame variations almoſt happened alſo at *Genoa* on the ſame days. By the obſervations of the marquis *Salvago*, from *Nov.* 28, when the barometer was at 28 inches, 1 line, it fell the 30th to 27 inches, 4 lines, having fallen 9 lines in two days, the wind being N. E. the next day, it roſe 5 or 6 lines, a little leſs than it had the ſame day at *Paris*.

It appears from theſe obſervations, that great variations happen in a little time in the height of the barometer, both at *Paris* and *Genoa*; and that there is a great conformity in theſe variations, which happened at the ſame time in ſuch diſtant countries. It appears alſo, that they have no great relation to the changes of the winds; for the variations of the barometer, which happened from *Nov.* 19 to 20, happened at *Paris* without any remarkable alteration of wind; and if on that day the barometer fell at *Genoa* with a S. E. wind, and roſe with a N. wind; in the variation which happened the 28th, the quickſilver fell with a N. E. wind which commonly makes it riſe. So at *Paris*, the barometer fell with a N. W. wind, and roſe with a W. with which it uſed to fall. But what rapidity muſt we aſcribe to the winds, to cauſe ſuch quick alterations in cities ſo diſtant?

It

It is not only in theſe ſudden variations, which happen very ſeldom, that we find this conformity; there is the ſame agreement alſo in the changes of the barometer, which are made more ſlowly, and happen in theſe two cities during the whole year.

As it would be tedious to relate all the obſervations made for the three laſt years, in which this agreement is found, I have made choice of the moſt remarkable.

		At *Paris.*			At *Genoa.*		
1706.		*Barom.*		*Winds.*	*Barom.*		*Winds.*
Jan.	1.	27	0	S.	27	3	
	7.	28	0	calm.	28	0½	N.

From *Jan.* 1 to 7, in the ſpace of 6 days, the quickſilver roſe 12 lines at *Paris*, and 9 ½ at *Genoa*.

Feb.	13.	27	3	S.	27	5	S. E.
	19.	28	1	calm	27	11½	N.

From *Feb.* 13 to 19, in 6 days, the barometer roſe 10 lines at *Paris*, and 6 at *Genoa*.

Oct.	31.	28	0	calm	28	0	calm.
Nov.	4.	26	9	S.E. rain	27	1	S.W.
	20.	27	11	S.	28	1	N.

By the obſervations of *Oct.* 31, and *Nov.* 4. in 4 days, the barometer fell at *Paris* 13 lines; it fell at the ſame time at *Genoa* 11 lines, tho' the winds were different. *Nov.* 20, the barometer roſe to a great height, being the ſame within about 2 lines in both theſe cities, tho' the wind was S. at *Paris*, and N. at *Genoa*.

Dec.	10	28	1	calm	28	4	N.
	15	27	1	W.	27	5	S.E.

By

By these observations the barometer fell, in 5 days, about 1 inch at *Paris* and at *Genoa*.

	At *Paris*.			At *Genoa*.		
1707.	*Barom.*		*Winds.*	*Barom.*		*Winds.*
Mar. 13.	27	11	W.	28	0	N.
17.	27	5	N. E.	27	8	S. E.

In 4 days, the barometer fell 6 lines at *Paris*, and 4 at *Genoa*, tho' the wind was very different.

July 20.	27	11	S. gentle	28	0	N. gentle
24.	27	$4\frac{1}{2}$	N. W.	27	6	S.E.

July 20, the winds being opposite at *Paris* and *Genoa*, the barometer rose pretty equally; it fell afterwards 6 lines in each place, in 4 days, the winds having changed, and being still opposite, that, is N. W. at *Paris*, and S. E. at *Genoa*.

Dec. 22.	27	10	S. W.	28	0	S. E.
27.	27	2	calm.	27	2	N. E.

In 5 days the barometer fell 8 lines at *Paris*, and 10 at *Genoa*.

1708.						
Jan. 11.	26	10	cal. and fair	27	3	S.W. clo.
17.	27	8	S.W.	27	11	S.E. fair.

The barometer rose in 6 days at *Paris* 10 lines, at *Genoa* 8, the winds being very different in these two cities.

Feb. 6.	27	$2\frac{3}{4}$	W.	27	$6\frac{1}{3}$	N.
10.	27	10	calm	28	0	N.

By these observations the barometer rose 6 lines in these two cities, the wind having been variable at *Paris*; at *Genoa* it was always N.

1708.	At *Paris.* Barom.	Winds.	At *Genoa.* Barom.	Winds.
Mar. 20.	27 8$\frac{3}{4}$	calm and fair	27 7	calm
22.	27 2	N.	27 3$\frac{1}{3}$	N.

The barometer being at a mean height, ſunk in 2 days 6 lines at *Paris*, 4 at *Genoa*, with a N. wind at both cities.

May 8.	27 11		28 0	N.
17.	27 4	S.W	27 5$\frac{3}{4}$	S.E.
Nov. 19.	28 2$\frac{1}{2}$	calm	28 2	calm
23.	27 6	N.W.	24*th.* 27 5	rain
24.	27 11$\frac{3}{4}$	N.W.	25*th.* 27 10	N.

The 19th, the barometer being at a great height fell till the 23d, and from the 23d to the 24th roſe 6 lines in a day. But at *Genoa* this variation was a day after it happened at *Paris*.

Dec. 10.	27 11$\frac{1}{2}$	calm	27 11	N.
15.	27 1	rain	27 5	rain

By all theſe obſervations, and a great many others, which I do not relate, it is manifeſt, that there is a great agreement in the variations, which happen at the ſame time at *Paris* and at *Genoa*, whether theſe alterations were quick and ſudden, like thoſe which were firſt related, or more ſlow like the laſt.

This correſpondence of the alterations of the barometer, ſeems to have no great relation to the conſtitution of the air, or to the winds, which prevail at the ſame time in different countries; for the quickſilver riſes at *Genoa*, when it riſes at *Paris*, and falls in like manner, whether there is the ſame conſtitution of the air, or whether the ſame wind prevails in both theſe cities, which is very rare;

rare; or whether both of them are different. It would be a thing worth examining by obſervations made in very diſtant places, to what diſtance ſuch a conformity in the variations of the barometer is found.

This long ſeries of obſervations at *Paris* and *Genoa*, compared together, ſhews, that to find the height of mountains, by barometrical experiments made at the ſame time in different places, after the manner propoſed in the memoirs of the academy, thoſe muſt be made uſe of, where the quickſilver keeps in the barometer at a mean height, and prefer theſe before others, where the quickſilver is found near to greater and ſmaller elevations, becauſe in the mean heights of the quickſilver the differences between different countries are more uniform.

By the compariſon of the obſervations made with this choice, we find between *Paris* and *Genoa* a difference of 3 lines height of quickſilver, which it has at *Genoa* more than at *Paris*; and as in the obſervations at *Genoa*, the barometer is a line lower than it ſhould be at the ſea-ſide, there reſults a difference of 4 lines of quickſilver between the *Paris* obſervations, and thoſe which ſhould be made at *Genoa* by the ſea-ſide. This difference between the level of the ſea at *Genoa* and at *Paris* agrees with what had been concluded by the obſervations of *Paris* and *Colioure*, related in the memoirs of 1703.

It has been obſerved in this memoir, that the differences which happen to the barometer in the ſame place, between the greateſt and ſmalleſt elevation, are greater in the northern than in the ſouthern countries, where theſe differences leſſen; ſo that towards the equinoctial they are reduced to a trifle.

Several obſervations received ſince that time from ſeveral places, are conformable to this obſervation. At *Upminſter*, in *England*, which is more N. than *Paris*, the variations of the barometer are alſo greater than at *Paris*; thoſe at *Paris* are greater than at *Genoa*; and the variations obſerved at *Genoa*, are alſo greater than thoſe which reſult from the obſervations of F. *Laval* made laſt year at *Marſeilles*, which is more ſouthern then *Genoa*.

This remark, which is confirmed by a great number of obſervations made at the ſame time in different places, does not agree with the obſervations made by M. *Scheuchzer* at *Zurick* theſe 3 laſt years; for tho' *Zurick* is much more to the N. than *Genoa*, the variations have been obſerved to be ſomething ſmaller at *Zurick*, far from having been greater than at *Genoa*. In 1706, the difference between the greateſt and leaſt elevation of the barometer was at *Zurick* 10 lines. At *Genoa*, the ſame year, this difference was 1 inch, 1 line. In 1707, at *Zurick*, it amounts to 11 lines; at *Genoa*, it was 1 inch. In 1708, by the obſervations made at *Zurick* with the upright barometer, which I think preferable to the inclined one, the variation was 10 lines; at *Genoa* 1 inch; at *Marſeilles* 10 lines ½, as at *Zurick*.

It muſt be obſerved, that the places of the obſervations, where this rule is found, are ſituated at heights very little different from each other, and are but little elevated above the ſurface of the ſea, as appears by the difference of the heights of the barometer, which is found between theſe obſervations, and with regard to thoſe which have been made near the level of the ſea. But it is not ſo with regard to the obſervations of *Zurick*, which are not conformable to this rule. For by the ob-

ſervations

ſervations made during the whole year 1708 at *Genoa* and *Zurick* compared together, there is a difference of 1 inch, 8 lines of quickſilver, found between the level of the ſea and *Zurick*; which ſhews that the place of the *Zurick* obſervations is very much elevated above the places of the other obſervations, and ſtill more above the level of the ſea.

This variation of the barometer being leſs in the high places than in the low ones, is alſo confirmed by the obſervations ſent laſt year by F. *Laval* to the academy: for having made barometrical obſervations for 10 days together on the mountain of *St. Pilon*, which is more northward by 2′ of a degree than *Marſeilles*, and is elevated above the level of the ſea about 480 toiſes; having compared them with thoſe which were made at the ſame time at the obſervatory at *Marſeilles*, he found that at *Marſeilles* the barometer varied 2 lines $\frac{3}{4}$, when it varied but 1 line $\frac{3}{4}$ at *St. Pilon*.

F. *Laval* aſcribes this difference partly to the heat, which is leſs in elevated than in low places, partly to the nature of the air, which being more rarefied in the elevated places, is leſs ſubject to the alterations, which contribute either to its heavineſs or lightneſs.

We might ſuppoſe, that it is ſome heterogeneous matter diſperſed in the air, which cauſes a part of theſe variations, and has a greater effect in the lower air than in the upper.

Having compared together the barometrical experiments, which have been hitherto made in different parts of the earth during the whole year, I have found that the variations of the barometer obſerved at *Zurick* approach much nearer to the variations obſerved near the equinoctial, than the others made hitherto in *Europe*.

I have examined on this occasion various experiments made near the equinoctial on the dilatation of the air, to see whether the air of this climate by dilating followed the reciprocal *ratio* of the weights from which it is relieved, according to M. *Mariotte*'s rule.

These experiments were made at *Malaca* by *F. de Beze*, during a stay of 7 months which he made at that place, which, tho' situated in 2 degrees of north latitude, enjoys, according to the report of the same father, a pretty temperate air for the climate, the heat being temperate, and not very variable.

These experiments are related among the *Observations Physiques & Mathematiques*, printed in 1692, with *F. Gouie*'s notes in the following terms.

" An able philosopher told me before my de-
" parture from *France*, that he had been assured
" that there was no sensible difference in the ba-
" rometer, found in all the places situated be-
" tween the tropicks, provided the observation
" was made in a place on a level with the sea.
" I was willing to examine the truth of this when
" I arrived in the *Indies*; and as I had no baro-
" meter mounted, I made use of a glass tube 29
" inches long, sealed hermetically, and exactly
" divided into inches and lines, with which I
" made the *Toricellian* experiment in different
" places between the tropicks; but I have found
" every where a pretty sensible difference in the
" elevation of the quicksilver, not only with
" regard to the different places where I ob-
" served, but often also in the same place, where
" the quicksilver was more or less elevated ac-
" cording to the different dispositions of the air;
" tho', to say the truth, this difference does not

" equal

" equal that which is found out of the tropicks, " ſince, according to what I have been able to " obſerve, it does not exceed 5 or 6 lines.

" I have already ſent to *France* the experiments " which I had made on this ſubject at *Siam* and " *Pondichery*. Theſe are what we made at *Malaca* and *Batavia*.

" Having choſen at *Malaca* a day when the " air appeared very pure, and the heaven was " not covered with any clouds, to make the ex- " periment: we found, that the quickſilver in " the tube kept up conſtantly to the height of " 26 inches 6 lines above the ſurface of that " which was in the baſon.

" The heat was at that time pretty great for " the climate, and the thermometer was at 69 deg.

" As I have obſerved by ſeveral experiments, " that the quickſilver uſually kept up to a greater " height when the heat was leſs, and that it fell " when the heat increaſed, tho' the ſky was " equally ſerene and clear, I thought it would be " proper to mark the degrees of the thermometer " at the ſame time, tho' there is not an exact " proportion between them.

" Being afterwards willing to try the elaſtic " force of the air, we left three inches of air at " the top of a tube, and having reverſed it in " the quickſilver, where it immerged 7 lines, " that of the tube remained at the height of 20 " inches 7 lines above the ſurface of the other; " and the air dilated occupied 7 inches 10 lines."

Having afterwards left 7 inches, 6 lines of air, the quickſilver remained at the height of 16 inches, and the air dilated occupied 12 inch. 5 lin.

In conſidering theſe obſervations, it is eaſy to ſee that they do not follow M. *Mariotte*'s rule; for in the firſt experiment, 7 inches 10 lines of

air

air dilated after the reverſing of the tube, to 3 inches of natural air before the reverſing, has not the ſame proportion as 26 inches 6 lines in *vacuo*, to 5 inches 5 lines exceſs of 26 inches 6 lines, to 20 inches 7 lines, the height which the quickſilver had with the dilated air, as it ought to be according to the rule. It is the ſame with the ſecond experiment; but in theſe 2 experiments, the proportion of the dilated air to the natural air, is leſs than the atmoſphere, to the difference between the height of quickſilver in the *vacuum*, and the height of the quickſilver with the dilated air.

Having calculated theſe 2 experiments, to know what the dilatation of the air ſhould be by the common rule; in the firſt where the natural air was 3 inches, after the reverſing the dilated air ought to occupy according to the rule 9 inches 11 lines; but by the experiment it occupied no more than 7 inches 10 lines; the difference between the experiment and the rule is, 2 inches 1 line, by which the ſpace occupied by the dilated air, was leſs.

In the ſecond experiment, 7 inches 6 lines of natural air after the reverſing, ought according to the rule, to be dilated, and fill the ſpace of 15 inches 1 line; but by the obſervation it occupied no more than 12 inches 5 lines; the difference between the obſervation and the rule is, 2 inches 8 lines, by which the obſervation is leſs; and conſequently, according to theſe experiments, the air of *Malaca* does not follow the rule, and is leſs dilated than that of *Europe*.

Beſides theſe experiments made at a time when the air was pure and ſerene, *F. de Beze* made others alſo, whilſt the ſky was leſs clear, and very cloudy, and that the height of the quickſilver in the *vacuum* was greater than in the preceding obſervations.

They

They are related after the firſt in the following manner.

"At the end of the moon, the ſky being
" very cloudy, and the air leſs clear than ordi-
" nary, I repeated theſe experiments in the ſame
" place, the thermometer was at 63 degrees.

"Having filled the tube with quick-ſilver,
" and reverſed it in that of the baſon, where it
" immerged one inch; it kept up at the height
" of 26 inches 10 lines $\frac{1}{4}$ above the ſurface of the
" quickſilver.

"Having afterwards put ſome quickſilver in
" the tube to the height of 26 inches, that there
" might remain 3 inches of air, and having
" plunged it in the quickſilver, the air dilating it-
" ſelf, occupied 7 inches 5 lines $\frac{1}{2}$, and the
" quickſilver 20 inches 6 lines $\frac{1}{2}$.

"Having left 6 inches of air, the quickſilver
" kept at the height of 17 inches 2 lines $\frac{1}{4}$, and
" the dilated air filled the reſt of the ſpace 10
" inches 9 lines $\frac{3}{4}$.

"Having left 9 inches of air, the quickſilver
" occupied but 14 inches 6 lines, and the dilated
" air 13 inches 6 lines. Theſe experiments
" were made in a place raiſed 15 or 20 feet per-
" pendicular above the level of the ſea."

By the compariſon which we have made of theſe obſervations with the rule, we find the ſame difference between them as in the preceding obſervations; for the 3 inches of natural air after the reverſing, dilated in ſuch a manner as to occupy only 7 inches 5 lines $\frac{1}{2}$, whereas, by the rule, it ought to contain a ſpace of 9 inches 6 lines $\frac{1}{2}$. The difference between the obſervation and the rule is 2 inches 1 line $\frac{1}{2}$, within about $\frac{1}{2}$ a line of what was found in the firſt of the preceding experiments, which ſhews the exactneſs of both of them.

In the ſecond experiment, 6 inches of natural air incloſed in the tube, after the reverſing fills the ſpace of 10 inches 9 lines $\frac{3}{4}$; this ſpace, by the calculation founded upon the rule, ſhould be 13 inches 3 lines. The difference is 2 inches 5 lines $\frac{3}{4}$, by which the dilatation is found leſs by the obſervation than by the rule.

In the laſt experiment, 9 inches of natural air incloſed in the tube being dilated by the reverſing, occupied 13 inches 6 lines, and by the calculation founded upon the rule, it ought to fill 16 inches 1 line $\frac{1}{4}$. The difference is 2 inches 7 lines $\frac{1}{4}$, by which the experiment gives leſs than the rule.

It is therefore manifeſt by all the experiments of *F. de Beze*, that the dilatation of the air, which reſults from them, is much ſmaller than that of our air, and that it does not follow the proportion found by the experiments of *Europe*.

It might be ſuppoſed that this *phænomenon* comes from the particular conſtitution of the air of *Malaca*, which being very much rarified by the heat of the climate, is afterwards leſs ſuſceptible of ſo great a dilatation as ours; but ſo far as we may judge by experiments made in *Europe*, this explanation alone is not ſufficient to give the reaſon of the great difference between the dilatation of our air, and that of *Malaca*, even though we ſhould ſuppoſe the heat which cauſed this rarefaction to be as great as that of boiling water. Theſe are the obſervations which we made.

I took a tube 38 inches long, in which I put quickſilver to the height of 35 inches, ſo that there remained 3 inches of air; I immerged this whole tube in boiling water, to rarefy the air contained in it; I afterwards ſtopped the aperture with

with my finger, and having taken the tube out of the water, I reverſed it in the quickſilver; ſo that above an inch was immerged. Immediately after the reverſing, the quickſilver kept within a few lines of where it keeps by the dilatation alone without having rarefied it. But the quickſilver was ſeen to riſe in the tube, as faſt as the air was condenſed in cooling; and when it was entirely cooled, the quickſilver roſe an inch and 2 lines more than it did immediately after the reverſing, and more than M. *Mariotte*'s rule required; and conſequently the rarefied air was leſs dilated than by the rule, by the ſame quantity of 1 inch and 2 lines. We have found by the experiments of *Malaca*, that the 3 inches of air dilated 2 inches 1 line leſs, than by the rule; the air of *Malaca* therefore dilates leſs than our air rarefied by the heat of boiling water.

I made the ſame experiment upon 6 inches, and afterwards upon 9 inches of air, and I always found that our air rarefied by heat, dilated much leſs than the air of *Malaca*, and that the difference found with regard to the rule is twice as great in the air of *Malaca* as in ours rarefied. Whence we may infer, that this leſs dilatation of the air of *Malaca* comes not only from the great heats of the climate, but from its own nature being leſs apt to dilate than ours.

As the air dilates otherwiſe at *Malaca* than it does in *France*, at an almoſt equal height with the ſurface of the ſea; and as in *France* the dilatation is found at great heights different from that which happens to the lower air, as reſults from the obſervations made on the mountains of *Auvergne* and *Rouſillon*, we may infer, that the whole maſs of the air has not the property of dilating itſelf according to the *ratio* of the weights.

We may alſo infer from theſe different dilatations, that the air is heterogeneous in theſe different parts, and that we ſhould therefore be cautious of founding a general ſyſtem upon particular experiments, let them be ever ſo certain and numerous.

It muſt be obſerved, that at *Cayenne*, the parallel of which differs from that of *Malaca* but 2 degrees ½ towards the N. the refractions of the ſtars have been found ſmaller than in *Europe*. It would be a thing worth examining, whether any relation is found between the manner, in which the air is dilated under ſeveral climates, and the different refractions of the celeſtial objects obſerved at equal heights above the ſurface of the ſea.

X. *Obſervations on cray-fiſh, by M.* Geoffroy, *junior* *; *tranſlated by Mr.* Chambers.

Among the multitude of obſervations on the ſeveral parts of natural hiſtory, there are ſome ſtill obſcure, and as it were unknown, for want of being confirmed by new experiments: and yet the making new diſcoveries is not enough to make philoſophy flouriſh, unleſs we prevent the old ones from being loſt. Hence there is a neceſſity for handling a-new ſome ſubjects which ſeem to have been neglected for a certain ſpace of time, and of which nothing is known, but upon the credit of ſome writer, whom it may not always be ſafe to truſt.

In purſuing this method, one has the pleaſure either of confirming the vulgar opinion, or of confuting it, or at leaſt of clearing and explaining it; for when only a few perſons have treated a

* Aug. 23, 1709.

ſubject,

ſubject, it rarely proves to be exhauſted. This was what induced me to make obſervations a-new on the cray-fiſh, and particularly on the ſtones found therein at the time when they change their covers, and which, by reaſon of their figure, are called *crabs-eyes*.

The common opinion touching theſe ſtones is, that they are found in the brain of the animal, which is what *Geſner*, *Agricola*, and *Bellonius* affirm; and yet ſo far are they from being in the brain, that they are rather found about its ſtomach.

Van Helmont ſeems to have been the firſt who apprehended this, but he having rendered himſelf ſuſpective on other occaſions, his opinion could not make its way; but the vulgar one ſtill prevailed, except in a few perſons who could ſee that experience was for him.

This author had obſerved, that towards the middle of *June* the cray-fiſhes begin to grow ſick, as being the time when they are to change their coats, or covers. For nine days, and upwards, they continue languiſhing, and as it were dead; in which compaſs of time, *Helmont* affirms, that a new membrane is formed, which incloſes the ſtomach, and that between this and the former ſtomach, a milky liquor is diſcharged, which, falling on either ſide, hardens into ſtone. This new membrane, according to him, ariſes from the pellicle formed on the ſurface of the milky liquor, and growing into a new ſtomach; the old one within it, and the remainder of the liquor with the ſtones themſelves, reſolve by little and little, and ſerve the animal for food during 27 days that theſe ſtones laſt; for the animal eats nothing all this while, nor is there any thing elſe found in its ſtomach.

I have not been able to trace all theſe matters related by *Van Helmont*, but have made ſome obſervations which agree with his.

I have found cray-fiſhes very ſoft, and ſo ready to quit their ſhell, that it was quite raiſed, ſo as to let the new one appear under it, like a very thick membrane, which only wanted time to become as hard as that going off.

This outer ſhell when it roſe, I found very thin, and the inner membrane, which uſes to line it, no longer adhering thereto, but forming a new ſhell.——And the like I have obſerved in the tail, commonly called the neck of the cray-fiſh, where the ſhells readily aroſe, and let the membrane that was to ſucceed them appear.

The ſame I have found upon breaking the claws; ſo that upon the whole we may ſay, that while the craw-fiſh is putting off its ſhell, the inner membrane thereof ſeparates therefrom, and growing gradually thicker, at length forms a new ſhell.

And I have ſince obſerved, that thoſe which are beginning to quit their ſhells, and whoſe inner membrane is come to a competent thickneſs, have ſtones in them perfectly formed, reſembling in figure the heads of young muſhrooms.

To aſcend to the origin of theſe ſtones, I have opened cray-fiſhes at other times of the year without finding any thing in them: but in my laſt obſervations made this month of *Auguſt*, opening ſome vigorous cray-fiſhes, which were only beginning to moult, in lieu of each ſtone, I found a film or *lamina*, ſwimming in the middle of a ſlimy ſubſtance, and which was perfectly the *embryo* of the ſtone. This ſtone, with its ſlime, were incloſed in a little ſlender membranous bag.

I

I have found others, where the ftones were quite formed, and the ftomach folid and full of a brownifh liquor, very mouldy and fetid.

Under the bag where the ftones are inclofed, I have found a flat membranous veficle, whofe ufe I do not underftand, only it has been obferved, that when the ftone difappears, this veficle becomes full of a fweet limpid water, and poffeffes the fame fpace as the ftone poffeffed. In others, I have found large fair ftones, and a new delicate membrane, inclofing the ftones and the ftomach. Upon raifing this membrane, there were 3 new teeth vifible thereon, fimilar in all refpects to thofe of the old ftomach; fo that no doubt can any longer remain, whether this membrane becomes at length the real ftomach.

In cray-fifhes, which had caft their coats, I have found the ftomach full of a brown liquor, the membrane of the ftomach being here very tender, and no appearance of vifcid matter in it, nor any remains of the former ftomach. The ftones were much leffened, and appeared as if coroded by fome diffolvent; they were covered with a very fine membrane, which was the only thing that parted them from the cavity of the ftomach.

In other cray-fifhes, which had moulted a longer time, I did not find the ftones in their ufual places, but quite in the ftomach, where they were joined together by their concave parts.

In others, where the new fhell was almoft arrived at its full hardnefs, I found nothing in the place where the ftones ufed to be lodged, but a white fpot, which was no more than the two membranes of the veficle, that had contained the ftone, fhrunk clofe together. Upon opening the ftomach, I found it full of a yellow liquor and food,

food, without any remains of a ſtone; and have ſometimes even found pieces of ſhells, and claws of other cray-fiſhes half digeſted therein. In theſe laſt are likewiſe found the ſpace formerly poſſeſſed by the ſtones, taken up by another veſicle, full of water, already mentioned. —— All which obſervations prove,

1ſt, That the ſtones taken from the heads of cray-fiſhes, are not lodged in their brain, but cloſed in the ſtomach, which is placed below.

2dly, That they are not the ſeed or origin of the new ſhell, as ſome have imagined, ſince they ſubſiſt after the ſhell is formed.

3dly, That upon caſting their ſhells, they likewiſe change their ſtomach, without any apparent renovation in the other parts, excepting the inteſtine, which ſeems to ſhare the fate of the ſtomach.

4thly, That the ſtones are not found till their ſeaſon of moulting, and that they are afterwards lodged in the new ſtomach, where they continue leſſening, and at length are totally conſumed.

5thly, That theſe ſtones, together with the old ſtomach, ſerves the animal as food during its ſickneſs, occaſioned by the moulting.

Some authors imagine, that the blue colour of ſome of theſe ſtones ariſes from a peculiar malady, incident to ſome of them at the time of their moulting. If this be not the real cauſe, 'tis at leaſt certain, that the ſtones of this colour aſſume a fleſh colour by boiling; and I have even known them turn red by the mere heat of the ſun.

Hence it is, that among thoſe uſed in the ſhops, ſome are blue, and others carnation; for I can ſcarce conceive that the greateſt part of the ſtones, commonly ſold, are counterfeit, as ſome have aſſerted, on account that the great quantity

thereof

thereof in uſe, ſince we find cray-fiſhes enough almoſt every where; beſide, that theſe ſtones conſiſt of layers, or *ſtrata*, like bezoar, which art would have much ado to imitate; not to mention that they turn black, exfoliate and yield a urinous ſmell upon calcination; a proof of their being really derived from the animal kingdom. To which may be added, that in the *analyſis*, they yield an urinous ſpirit, with a little volatile ſalt. ——— Upon the whole, 'tis more than probable, that the crabs eyes uſed among us, are taken from the living animals; and that the blue or ruddy ones, mixed among them, come from the ſick and dead ones.

The virtues of crabs eyes are commonly ſuppoſed to be no other than as meer abſorbents; but the following experiment will prove, that they have other properties, which carry them into the very maſs of blood.

A perſon having taken a potion, wherein crabs eyes were an ingredient for ſome acrimonies which incommoded him, found himſelf ſeized all at once with an eryſipelas in the face, which hereby became ſtrangely bloated, attended with violent prickings, the bloating reached his throat, and hindered his ſwallowing. At firſt, it was feared, that ſomething had been mixed among the crabs eyes, or that they had been pounded in a braſs mortar, and had imbibed the pernicious quality thereof; upon which the ſame potion was ordered with other crabs eyes, which ſtill produced the ſame effect, till at length the patient being informed that there were crabs eyes in the draught, eaſed the phyſician of his perplexity, by telling him, that ſhe had found the like every time ſhe had taken crabs eyes; upon which, the crabs eyes being diſcontinued, the ſymptom ceaſed, and it has been ſince obſerved, that crab's eyes had the ſame effect on her ſon; upon which it may not be amiſs to obſerve, how much the effect of remedies may be diſturbed by conſtitutions. Tho' we only ſpeak of the ſtones found in cray-fiſhes: yet there is a ſpecies of lobſters, called *aſtacus marinus*, where they are likewiſe found. This ſpecies is perfectly like our cray-fiſhes, ſetting its bulk aſide.

To conclude; if ſome people have an averſion for cray-fiſhes, *Van Helmont* obſerves, that thoſe animals, in their turn, have ſo great a one for hogs, that if any come near them, they preſently die. Hence, ſays he, it is that in *Brandenburg*, where ſtore of them are caught, the waggoners, who carry them, are obliged to keep watch all night, to prevent any hogs from paſſing under their waggons; for that if only one paſſed, there would not be a cray-fiſh alive next morning.

XI. *Of the formation and growth of the ſhells of land and water animals, either of the ſea or of rivers, by M.* de Reaumur †; *tranſlated by Mr.* Chambers.

The wiſdom of nature would not have done enough for the preſervation of animals, if, contenting herſelf to have framed their internal parts with wonderful art, ſhe had not employed the ſame addreſs to defend them againſt other bodies around them, the too rude touches of ſuch bodies would have quickly deſtroyed thoſe ſo ſlender canals, and thoſe fibres ſo very ſubtle, whereon their whole mechaniſm depends. Hence we find thoſe delicate parts inveſted with diverſe coats, or

† Nov. 1709.

covers

covers, not eaſy to be altered by the bodies around, being not only in an under skin, cloſer and firmer than the reſt; but this uſually covered with hairs, feathers, ſcales, or ſhells. Under theſe little ramparts, if I may uſe the term, the animal machines are ſheltered from all the attempts of bodies, which are continually rubbing and beating upon them; and the care of nature is even gone ſo far, as to proportion a ſtrength of theſe defences to the weakneſs of the parts within; I mean, that thoſe animals, which either by their figure, or their ſoftneſs of their ſubſtance, lay them moſt open to the bodies around, have the ſtrongeſt coverings. Thus we find ſhells on thoſe whoſe ſubſtance is very ſoft and moiſt, and figure almoſt flat or ſpiral, which would otherwiſe, by this double diſadvantage, be liable to lacerations from the ground, ſand, or ſtones they creep upon. The number of different kinds of animals, both in land and water, preſerved by means of ſuch ſhells is immenſe; as is alſo the art and ingenuity they are framed withal. Nature ſeems to have taken pleaſure in varying their ſtructure, colour, and ſhape; inſomuch that the admirers of the beauties of the creation have moſt of them made it their buſineſs to collect all they could meet withal, every new ſhell furniſhing ſome new curioſity; their cabinets, tho' they only contain a ſmall part of thoſe which deck the univerſe, yet have enough to excite the admiration of all, who know how to admire. Hitherto indeed they ſeem to have confined themſelves to the bare contemplation of this beautiful piece of workmanſhip, no body that I know of, having explained the manner wherein it is produced; ſo that finding nothing to be learned on this head among authors, I conſulted nature herſelf by ſeveral experiments; and 'tis on the reſult thereof the following ſyſtem is formed.

Tho' at firſt ſight, it may appear moſt natural to explain the formation of ſhells before their growth; yet I ſhall here obſerve a contrary order, and begin with explaining the manner in which they grow, by reaſon this was eaſier to be diſcovered by experiments, and that it afforded an eaſy inſight into their formation; which, as one may ſay, is only the firſt degree of growth.

A body may grow in two different manners, or to ſpeak more preciſely, the little parts of matter which unite themſelves to thoſe a body before conſiſted of, and hereby augment its bulk, may be joined to it in two different manners. The former when they have firſt paſſed thro' the body itſelf, and are prepared therein, and hereby rendered fit to poſſeſs the place they are carried to, ere they become united thereto, which is commonly called, growing by vegetation, and in ſchools, by intuſſuſception. Thus it is the ſap mounts in plants, by little canals in the plants themſelves; which, after preparing it, ſuitably conveys it to diverſe parts of the plant, where it ſtops and adheres, and conſequently inlarges the body of ſuch plant: and 'tis thus that the blood in an animal, being conveyed by the arteries to the extremes of the body, adheres to the fleſh and augments its bulk.

The 2d ſpecies of growth is, when the new parts are applied to the body, without paſſing through, or undergoing any preparation in the body itſelf, which is called growing by appoſition, and in the ſchools, by juxtapoſition. Thus it is all thoſe artificial plants grow produced by the chymiſts, as likewiſe all chryſtalliſations, ſalts, *&c.*

Now

Now the growth of ſhells muſt be performed after one of theſe two manners; they who make every thing vegetate, even to ſtones, would hardly have ſuſpected that ſhells, which are wrought with ſo much art, ſhould be produced by a ſimple juxtapoſition, and the analogy, which ſeems to be between them and bones (for may they not be conſidered as external bones) ſeems to confirm the opinion, ſince bones really vegetate; but there is no great ſtrength in bare conjectures, and 'tis experiments alone, made on the things themſelves in queſtion, that can ſupport ſuch reaſonings; 'tis they alone muſt ſhow the way nature has been pleaſed to take to arrive at her end; and by them we ſhall hereafter ſhew, that ſhells are formed by a ſimple appoſition. My experiments indeed have only been made on ſome ſpecies of ſhells, both of the ſea, river, and land kinds; but this I apprehend ſufficient to intitle me to an explanation of the growth and formation of ſhells in general, for the ſame reaſon, as the explaining how one plant vegetates, or in what manner nutrition is performed in one animal, would be allowed ſufficient for all.

Hence I ſhall content myſelf with relating the experiments, I have made on diverſe kinds of land ſnails, to prevent the tedious repetitions I muſt fall into, were I to give the like experiments upon water-ſnails, both ſea and river kinds, upon ſeveral ſpecies of two leaved ſhells, as muſcles, pallourdes, pectongles, *&c.* which it would not be eaſy for many people to repeat after me; whereas every body may make them on land ſnails. All I think neceſſary to note is, that I incloſed the ſeveral kinds of ſea and river ſhell-fiſhes in little tubs, which I ſunk in the ſea or river, after firſt piercing them full of little holes, big

big enough to let in the water, but not to let out the fiſhes; by which means I was enabled to make much the ſame experiments, and with the ſame ſucceſs upon their ſhells, as thoſe I am going to relate upon the ſhells of land ſnails. Thus much laid down, I paſs on to explain the growth of ſhells.

When the animal, which before exactly filled its ſhell, grows, the ſhell can no longer cover it all over, but neceſſarily leaves part of the body bare, which bare part is always that next the aperture of the ſhell; for the animal can only grow on that ſide. All animals, which, like the ſnails, inhabit twiſted or ſpiral ſhells, are only capable of augmenting on the ſide of their head, which is that of the orifice of the ſhell; whereas the fiſhes of two leaved ſhells, as muſcles, are capable of growing in their whole circumference. Now in all the ſpecies of ſhell-fiſhes, 'tis this ſame part of the body, thus uncovered by the growth of the animal, that makes the ſhell grow, and the mechaniſm whereby it is effected, is as follows.

'Tis a neceſſary effect of the laws of motion, that in liquids flowing in canals, when the the little part of ſuch liquids, or any little foreign bodies mixed with them, which by reaſon of their figure, or their likeneſs, moves ſlower than the reſt, muſt recede from the centre of motion; that is, range themſelves near the ſides of thoſe canals; and it frequently happens, that ſuch particles do likewiſe adhere to the inner ſurface of ſuch canals, when they happen to be viſcid enough for that end. Of this we have inſtances in the common water-pipes, whoſe *parietes* upon opening them, are frequently found covered over with a little cruſt of viſcid ſubſtance; and ſome

wherein

wherein certain waters are conveyed, with a ſtony cruſt; it is certain withal, that the liquids, flowing in ſuch canals, preſs, or impel their *parietes* on all ſides, or which amounts to the ſame, preſs the little viſcid or ſtony particles of the cruſts above-mentioned againſt the ſides; ſo that if theſe canals were pierced like ſieves, with a multitude of little holes of a proper figure, to give paſſage only to ſuch little viſcid and ſtony bodies, they would break out of the canals, and place themſelves on the external ſurface thereof, and there form the cruſt, as is ſeen on the inſide; with this only difference, that the former is capable of becoming much thicker and ſtronger, as being leſs expoſed to the friction of the liquor, than that formed in the inſide of the canal.

Now the growth of ſhells is the work of a mechaniſm of this kind; the external ſurface of the new-formed part of the body left bare by the old ſhell, is full of a multitude of canals, wherein the proper fluids are circulating, that are to ſuſtain the animal; and a great number of viſcid and ſtony particles are intermixed therewith, which being leſs fluid than thoſe which compoſe the liquids they are among, are caſt neareſt the ſides of the veſſels, which being full of an infinite number of pores at the external ſurface of the body proper to give them paſſage, they eaſily eſcape out of their containing veſſels, as being continually driven againſt the ſide by the circulating liquor, and place themſelves on the external ſurface of theſe canals, or rather over all the ſurface of the body not covered by the ſhell, where they arrive with the more eaſe, as all the pores give them a free exit; whereas ſeveral of theſe pores may be ſtopped on the reſt of the body by the ſhell it is covered with.

These particles of viscid and stony matter being arrived at the external surface of the body, easily adhere to each other, as well as to the extremity of the shell; and when the most subtle and fluid part of them is evaporated, they compose a little solid body, which is the first layer, or *stratum*, of the new piece of shell, and other particles of a like matter to that of the first *stratum*, whereof the circulating fluid contains enough, issue from the same vessels, by the same mechanism; here it being no danger, that the first *stratum* should have stopped all the pores, and thus form a second *stratum* of shell; and after the like manner arises a third and a fourth, till the new shell have arrived at a certain thickness, which is usually much less than that of the old one, when the further growth of the animal gives rise to another new piece of shell.

'Tis the experiments I am now going to deliver, that are to shew whether this be the real manner of nature's proceeding, or whether all I have advanced be only matter of imagination.

I began with supposing that the animal grows before its shell, of which it is easy to be satisfied, by observing a garden snail at the time when its shell is about to grow, or enlarge; for here it is visibly too small to cover the body. On this occasion, they fasten themselves to the wall, where they remain at rest, and give opportunity for observing a part of their body come beyond the shell all around; and this like all the rest of their body, is full of a prodigious number of little canals, as appears by the naked eye; but much more by the microscope.

The pores I have supposed in these canals, are too small to be visible; but their existence may be evinced from their effects, with as much certainty

certainty as if one ſaw them ever ſo plainly. To do this, we need only break off a piece of the ſhell of a ſnail, without wounding its body, which may always be eaſily done, by reaſon it only ſticks to it in one place; for in a little time after we ſhould find the skin of the animal covered with a liquid ſubſtance, which could not have come from the veſſels it was contained in, unleſs there had been pores in thoſe veſſels to let it paſs; and if for further ſatisfaction this liquor be wiped off the skin with a linnen cloth, in a few hours more, you'll have a dew liquor of the like kind ſucceeding it, which coming at once over the whole bare part, can only have paſſed through its pores.

'Tis this liquid, or rather the leſs fluid and moveable particles therein, that ſerved to make the ſhell grow; of this there will be no room to doubt, when it is conſidered how it repairs the loſs of a piece of its ſhell, which may be clearly ſeen *, by putting a ſnail, thus ſtripped of a piece of ſhell, in a place where it may be commodiouſly obſerved. In a veſſel, for inſtance, where it does not remain long, ere it faſtens againſt the ſides of the veſſel, as it does againſt a garden wall, when its ſhell grows in the uſual courſe. Upon this the liquor is ſeen to thicken and fix, that is, its more volatile parts evaporate, and leave the groſſer behind, which form a thin kind of cruſt over all the naked part of the animal. This cruſt may be perceived in four and twenty hours times, in which ſtate it may be compared for its fineneſs to a ſpider's web. 'Tis this cruſt that forms the firſt *ſtratum* of the new ſhell, which in a few days more grows thicker by the appoſition of new layers under the firſt,

* Plate III. Fig. 6.

till in 10 or 12 days that the new piece of ſhell is arrived at much the ſame thickneſs as the reſt.

When you would obſerve the new piece of ſhell arrive at this thickneſs, care muſt be taken to put up a proper food with the animal, eſpecially if the fracture were made near the aperture; for otherwiſe the bulk of its body will diminiſh conſiderably: ſo that what ſhell is left them, being large enough to cover them over, there are only the firſt leaves of a new ſhell formed; and it may in ſome caſes be likewiſe proper to pull them from the ſides of the veſſels, when they continue there for ſeveral days together, in order to induce them to uſe the food, and repair the expence made in producing the firſt leaves of a new piece of ſhell.

For their food one may give them herbs, or even earth, and paper frequently ſprinkled with water, for they will eat indifferently any of thoſe things, which may ſupply particles of matter firm enough to form a ſhell; and the earth, for inſtance, muſt needs abound with a multitude of little *laminæ*, whence the ſtones are formed that grow in its boſom; if ſuch ſtony *laminæ* circulate with the liquors in the veſſels of the ſnail, they muſt doubtleſs be very fit to form the ſeveral *ſtrata* of ſhells. Now it may be ſhewn, by a very eaſy experiment, that ſuch little ſtony particles do circulate with the liquors: in order to this, one need only put a certain quantity of the liquor in a veſſel, and expoſe it ſome days to the open air. After the ſubtleſt part is evaporated, a ſolid matter will be found at the bottom, among which a multitude of little white friable corpuſcles, like grains of ſand, only thinner, will be found. 'Tis known likewiſe, that ſnails at the beginning of winter make of this ſame liquor a little lid, for the orifice of their ſhell, to cover themſelves

themſelves cloſe up. This lid indeed is of a different texture, from that of the ſhell; but it is ſolid, which is enough to ſhew that there is plenty of ſolid particles mixed among the liquor; all the difference of texture between the lid and the ſhell, probably ariſes from the difference of the pores thro' which it paſſed, in order to form them.

The ſingle manner of forming a new piece of ſhell, in the room of another broken off, might ſuffice to prove, that theſe bodies do not vegetate; for if they grew by vegetation, there are only two ways for it, neither of which is compatible with the preceding experiment: for either theſe liquids, which the animal furniſhes for the growth of its ſhell, and which on this *hypotheſis*, can only be conveyed to it, by the little part it is faſtened by, which may here be conſidered as the root of the ſhell; either, I ſay, theſe liquids muſt here meet with canals to carry them to all parts of the ſhell, or canals to carry them only to the extremity, which is to be enlarged. Now in both thoſe ſuppoſitions, it would come to paſs, that when a piece of the ſhell had been broke off, the liquid, flowing in ſuch ſhell, muſt extravaſate and pour forth at the rupture made in it. In which caſe, it would be on the circumference of the hole made in the ſhell, that this liquid would be found, which, in reality, we only find on the body of the animal; and this liquor, after fixing, would make a kind of *callus*, which gradually enlarging, would at length cloſe the hole. 'Tis thus the *callus's* have broken; bones are formed by the extravaſation of the juice, which before ſerved to feed, and make them grow; and 'tis thus that after cutting a piece of fleſh from any part of the body, the adjacent fleſh extends, and at length covers the part before left bare. Laſtly,

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the ſame thing is found to befal trees; for upon cutting off a part, the juice oozing from it, forms a *callus*, which, by degrees, covers over the whole wound; but the quite contrary paſſes in the production of the new piece of ſhell, nothing comes out of the ſhell, and the whole compaſs of the hole cloſes at the ſame time, by the liquor oozing from the ſubjacent body, and to prevent any ſuſpicion, that this liquor iſſues from the ſhell in ſome inſenſible manner, and falling by its own weight, by the body of the animal, gathers in ſufficient quantity, to compoſe at length a new piece of ſhell, always placed directly under the old one, I ſhall ſubjoin two experiments, which, at the ſame time, will remove this ſcruple, and demonſtrate what has been already advanced.

* I have broke ſeveral ſnail-ſhells in two different manners, the firſt by making a large hole between the two extremities of the ſhell; that is between the ſhell and its orifice, and thro' the hole thruſting a piece of thin ſkin between the animal and its ſhell and faſtening this ſkin to the inner ſurface of the latter, ſo as to cloſe very accurately the hole made therein: here it is evident, that if the ſhell were not formed of a liquid ſpringing immediately from the body of the animal, but of another ozing from the ſhell, a piece of new ſhell muſt have form'd itſelf on the external ſurface of the ſkin; and no ſhell could poſſibly be formed between the body of the ſnail and this ſkin: the contrary, however, came to paſs; the ſide of the ſkin which immediately touched the body, becoming lined with ſhell, while nothing appeared on the other ſide.

* Fig. 7.

The

The ſecond experiment is no leſs deciſive than the former for breaking ſeveral ſnail ſhells, ſo as to leſſen the number of their circumvolutions: reducing for inſtance a large garden-ſhell *, which uſually conſiſts of 4, or 4 ½, to 3 ½, or 4; and thus rendering them too ſmall to cover the animal, I put them much in the ſame condition as they were in, when the growth of the body left part of it bare; this done, I took, as in the former experiment, a piece of the thin ſkin, as large as the aperture of the ſhell; and thruſting part of it between the body of the ſnail and the ſhell, and faſtening it to the inner ſurface of the latter, I turned the reſt of the ſkin over the external ſurface of the ſhell, and faſtened it in like manner thereto, ſo that the whole circumference of the aperture of the ſhell was covered with the ſkin. Now if the ſhell grew by a principal vegetation, one of theſe two things muſt happen, either that the piece of ſkin thus clinging about it, would have hindered its growth; or the ſhell growing and extending, would have carried the ſkin with it. But the contrary happened, for the ſhell grew, and the skin remained as I left it; the growth of the ſhell being ſo conducted, that the thickneſs of the skin remained between the new piece of ſhell and the old; which latter therefore could contribute nothing to the formation of the former.

Nor is there any difficulty in conceiving how the little parts of ſolid matter, mixed among the fluid, ſhould faſten themſelves to each other, in order to form a firſt *ſtratum* of the new ſhell; nor how a ſecond *ſtratum* ſhould unite itſelf to this firſt; a third to the ſecond; and ſo of the reſt. At leaſt this difficulty is no other than what we meet withal, in explaining the *nexus* of the parts

† Fig. 8.

of

of all ſolid bodies; in effect, whatever ſyſtem we adopt, 'tis obvious, that ſuch ſolid particles floating in a very viſcid liquor, are greatly diſpoſed to unite together, and form ſeveral *ſtrata*, as above-mentioned, I proceed now to give an experiment, which may let ſome light into the manner wherein this is effected.

I pounded ſome ſnail ſhells in a mortar, and after reducing them into a very fine powder, paſſed it thro' a very cloſe ſieve, in order to ſeparate the coarſer parts. This powder being put in a veſſel, and vinegar caſt thereon, a fermentation aroſe, and a kind of paſte was formed, which being left to dry in the air, attained a conſiderable hardneſs, eſpecially the firſt layer, or that next the air; on the contrary, when I moiſtened the ſame powder with water, a paſte indeed aroſe; but upon its drying, the little particles of the powder crumbled again, and ceaſed any longer to adhere. Hence it appears, that the acids analogous to thoſe of vinegar, are proper to bind the particles whereof the ſhell conſiſts together; they who make uſe at every turn of the acids in the air, may here find room for them, by ſuppoſing that they contribute to the coagaulating of the liquid, which fixes itſelf on the body of the ſnail. But to make this conjecture carry a face of probability, it ſeems neceſſary, that there ſhould be ſome acids found mixed with ſea-water, to help coagulate the liquids whereof ſea-ſhells are formed; whereas if this were true, the powder of a ſea-ſhell, mixed up with ſea-water, and then dried, muſt come to a better conſiſtance, than what we obſerved the ſnail-ſhell did, when mixed with river-water, which in fact it does not.

Nor need we apprehend, that the firſt leaf of a ſhell ſhould ſtop all the paſſages, by which the liquor

liquor is to iſſue to form a ſecond leaf, or *ſtratum*; and ſo of others, till it have arrived at a thickneſs. 'Tis hardly poſſible, that the new leaf ſhould cloſe ſo exactly about the body of the ſnail, as intirely to ſtop all the little pores thereof; but the difficulty vaniſhes at once, upon conſidering that this firſt leaf could not be formed without a diminution in the bulk of the ſnail's body, both on account of the ſolid particles, whereby the ſhell is formed, and of a much larger quantity of fluid matters mixed among them, which had ſince evaporated. Hence it follows, that there muſt be room enough left between this new leaf and the body of the animal, for new liquor to place itſelf between them, and thus form a ſecond *ſtratum* by the ſame mechaniſm as the firſt, and ſo a third, and as many more as is neceſſary to give the ſhell its due thickneſs.

The ſeveral *ſtrata*, which compoſe the thickneſs of ſhells, become very ſenſible upon throwing a ſhell in the fire, and taking it out again, after it is a little burnt; for here its thickneſs ſubdivides into a great number of different leaves, which are at a little diſtance from each other, the fire having found an eaſier paſſage between theſe leaves than betweeen the leſſer *laminæ* each of theſe conſiſts of; and the like uſually happens in other bodies formed of *ſtrata*. Witneſs all thoſe kinds of paſtries, formed of what we commonly call puff-paſte, the whole ſtructure whereof is to be formed of alternate layers of paſte and butter laid one over the other; which, upon baking, divide into ſeveral leaves or ſhivers, by reaſon paſſages are eaſieſt opened by the fire, or are even found already open between the ſeveral layers, which can never be exactly applied one over the other thro' their whole extent.

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The ſeveral leaves may be eaſily faſtened to each other, without their faſtening likewiſe to the body of the animal they are to cover, which the moiſture of its skin muſt neceſſarily prevent; and if any ſlight adheſion ſhould happen, the various motions of the animal within its ſhell, would be enough to break them again.

'Tis a neceſſary conſequence of this ſyſtem of the growth of ſhells, that their inlargement ſhould only proceed by increaſing the number of their ſpiral wreaths or circumvolutions; and that the length of each circumvolution ſhould always remain the ſame, which accordingly is a matter of fact, one may eaſily be convinced of, by only reducing the ſhell of a ſnail, arrived at its utmoſt growth, to the ſame number of circumvolutions as that of a young ſnail of the ſame ſpecies, the two ſhells will be found of the ſame ſize. I have frequently compared the ſhells of ſnails newly hatched, or which I had even taken out of their eggs before hatching *, with other ſhells of the largeſt ſnails of the ſame ſpecies, from which I had retrenched all but the like number of ſpiral circumvolutions, as were in the little ones, in which caſe they appeared both equal. It may be added, that the number of theſe circumvolutions makes a conſiderable addition to the ſize of a ſnail's ſhell a ſingle circumvolution, more or leſs occaſioning a very ſenſible difference, for the diameter of each circumvolution is near double that of the preceding one, and but half of the following one; whence it follows, that a half or even a $\frac{1}{4}$ of a circumvolution more muſt make a conſiderable enlargement; and yet it frequently proves difficult enough to diſcover, whether a ſhell contain $\frac{1}{2}$ or a $\frac{1}{4}$ of a circumvolution more or leſs

* Fig 9.

than

than another. The only ſure way to compare the number of circumvolutions between two ſhells of the ſame ſpecies, is to compare large ones with very ſmall ones, in which caſe the difference eaſily appears.

What has been hitherto ſaid of the growth of ſhells, will exempt us from the neceſſity of entering into the detail of their firſt formation; for 'tis eaſy to conceive, that when the body of a little embryo, which is one day to fill a large ſhell, is arrived at a certain ſtate, wherein the ſeveral skins that incloſe it are of conſiſtence enough to let paſs thro' their pores the only liquor fit to form a ſhell, this liquor muſt place itſelf on ſuch skins, and thicken and fix there; and in one word, begin the formation of a ſhell in the ſame manner as it afterwards continues its growth. Snails do not leave their eggs, till they have firſt covered themſelves with ſuch a ſhell, which now conſiſts of one circumvolution, and ſomewhat more.

It remains to ſolve two difficulties, which ſeem pretty conſiderable: the firſt naturally ariſes from the experiments above related, and ſtands thus, the new piece of ſhell, formed in lieu of the old one which had been pulled off, is of a whitiſh colour, and conſequently very different from the reſt of the ſhell, whence it ſhould ſeem to be of a different texture, and may hence be inferred to have been formed after a different manner; ſo that the foregoing experiments will determine nothing as to the ordinary way of growth.

To obviate this difficulty, it will be neceſſary to account for the regular variety of the colours in certain ſhells, or the ſame experiments, which ſhew the cauſe of ſuch regularity, will effectually remove this objection.

This regular variety of colours is peculiarly obſervable in a little ſpecies of garden ſnails *; the ground of their ſhell is white, citron coloured, yellow, or ſome intermediate colour between theſe, and on this ground appear various ſtripes, which twiſt ſpirally like the ſhell, and in ſome ſhells are black, in others brown and reddiſh, in others the breadth of each ſtripe gradually increaſes as it approaches towards the aperture of the ſhell; and it ſometimes happens, that two of them ſpread ſo much as to meet, and form only one broad ſtripe afterwards. In ſome ſhells there are 5 or 6 ſuch ſtripes; others have but 3 or 4, and others only 2, or even a ſingle one. A ſort of white and brown ſtripes may alſo be ſeen on the large garden-ſnails; but they are much leſs conſpicuous, and muſt be viewed with ſome attention; to diſtinguiſh one from another in each kind of ſhells, the ſtripes are not all of the ſame breadth in the ſame part of the ſhell. There ſeems but one plauſible way of accounting for the variety of theſe colours on the principles we have here eſtabliſhed of the growth of ſhells by juxtapoſition; for having conſidered the skin of the animal as a kind of ſieve, which gives paſſage to the particles, which are to form the ſhell, 'tis obvious, that if we conceive the skin as differently pierced in different parts, or which amounts to the ſame, that it is compoſed of different ſieves, ſome whereof paſs particles of different figures or natures, from thoſe paſſed by others, and deny entrance to theſe, it will follow, that ſuch particles of different nature or figure, muſt form bodies, which will reflect the like differently, that is form pieces of ſhell of different colours.

* Fig. 9 10.

'Tis

'Tis likewiſe a neceſſary conſequence of the manner of growing of a ſnail's ſhell, that the whole ſurface of this ſhell (I do not ſay its whole thickneſs) ſhould be formed by the collar of a ſnail, as being the part next the head, and which therefore, upon the leaſt growth of the animal, muſt be left uncovered, 'tis this therefore that is to enlarge it; and we may conſider this as the manufacturer of the whole ſurface or circumference of the ſhell: ſo that it will ſuffice, if this collar be compoſed of different ſieves, to form a ſhell of different colours. For inſtance, if it had two or three little ſieves proper to tranſmit black or brown particles, and the ſides of thoſe ſieves be parallel to each other, while the reſt of its ſurface tranſmits other particles proper to exhibit yellow or citron colour; for the ſhell formed of particles paſſed thro' theſe ſeveral ſieves, muſt evidently have a yellow or citron colour'd ground, with black or brown ſtripes thereon, almoſt parallel or approaching each other inſenſibly, and which will become larger in proportion, as theſe ſieves are enlarged.

Tho' we were to diſcern nothing like theſe different ſieves juſt mentioned on the collar of the ſnail, they afford us ſo probable a ſolution of the diverſity of colours in ſhells, that one would be induced to admit them, but fortunately enough they diſcover themſelves, eſpecially in the little ſpecies of ſnails, ſo remarkable for the diſtinctneſs of its ſtripes *. Upon the ſtripping one of theſe ſnails of a part of its ſhell, all the reſt of the body appears of one uniform white colour, excepting the collar, where the white has more of a yellowiſh caſt; and beſide this is beſet with a number of black or brown ſtripes, equal to that

* Fig. 10.

of the ſtripes of the ſhell, and placed in the ſame direction. Thoſe ſnails which have only one black ſtripe on their ſhell, having but one black ſpot on their collar, and thoſe which have 4 ſtripes on their ſhell, having 4 likewiſe on their collar: theſe ſtripes are placed immediately under thoſe of the ſhell, and begin at about a line's diſtance from the extremity of the collar, which itſelf is uſually ſpotted with black all around; but the length of theſe ſtripes in the collar differs in different ſnails of the ſame ſpecies; one cannot overlook the ſieves I have above-mentioned, in obſerving theſe ſtripes, whoſe different colour abundantly proves the difference of their textures.

To remove all doubt, whether theſe ſpots do the office of ſieves different from thoſe of the reſt of the collar, and that the reſt of the collar, which likewiſe appears of a different colour from the reſt of the skin of the body, does alſo tranſmit particles of a different nature or figure, the buſineſs muſt be to learn, whether experiments agree with this reaſon; and all neceſſary thereto, is to let a ſnail repair the ſhell, which has been torn from it; for if it appear, that ſo much of their ſhell as is formed over theſe black ſtripes is black, and what is formed between them is of a different colour, both from thoſe ſtripes, and from the reſt of the body, it muſt be allowed inconteſtible, that theſe different parts do the different offices above aſſigned them. Now experience agrees perfectly with the reaſon already laid down, * the ſhell growing on the collar over the brown or black ſtripes, is itſelf brown or black; that formed between them is white or yellow; and that on all the reſt of the body white, but a different white from that of the collar when it happens to

* Fig. 11.

be

be white alſo : the ſame is obſervable in the large garden-ſnails, where all the ſhell formed over their collar is brown, or of a colour like that of the old ſhell, and the ſhell on all the reſt of their body white.

We come now to a ſecond ſcruple, which may ariſe upon repeating the experiments here related. The new ſhell formed over the collar, in the room of the old piece broken off, ſometimes proves of a different colour therefrom, which ſeem a contradiction to the account here laid down.

But there will be no great difficulty in reconciling this kind of irregularity with the reaſonings and experiments above, when 'tis conſidered that the new ſhell formed over the collar never differs in colour from the old, unleſs its external ſurface be extreamly rough, and as it were furrowed over, while the reſt of the ſhell is quite ſmooth.

This inequality of ſurface of the new ſhell is occaſioned by the motions the ſnail puts forth, when it would re-enter its houſe before this part be thick enough to ſuſtain itſelf, without bearing on it; for 'tis evident upon thus ſhrinking it, when there is only one or a few leaves formed of the new piece of ſhell, it muſt bring the extremity of ſuch pliant leaves towards the old ſhell, and thus reducing them into a leſs compaſs, makes diverſe folds therein, which of itſelf were almoſt ſufficient to change the colour of the new ſhell; but there is ſomething more in it; for the firſt new *ſtratum* formed upon breaking off a large piece of old ſhell is uſually white, by reaſon the particles of the liquid diſpoſed to form a ſhell of this colour, are tranſmitted more readily thro' the pores than thoſe which form a ſhell of any other colour, as is evident enough, the reſt of the body of the animal being palpably covered over with liquid ere any be perceived on the collar; whence it happens, that this liquid ſpreading upon the collar, forms the firſt leaf of the ſhell white; but this leaf being extreamly thin, is tranſparent likewiſe, and rarely hinders the ſhell, which the collar itſelf produces afterwards, from appearing of its natural colour. Now, if the ſnail happen to ſhrink into its ſhell when only this firſt white layer is formed, it is clear, that it muſt draw the extremities of ſuch leaf towards each other, by reaſon it adheres to it in ſome places; and will occaſion it to make pleats or folds, and increaſe its thickneſs by diminiſhing its breadth and tranſparence, which muſt give the new ſhell a kind of middle colour, between that uſually formed on the collar, and that on the reſt of the body; but the internal ſurface of the new piece of ſhell being always ſmooth, muſt always be of the colour naturally produced by the pores correſponding to it, and accordingly we find its colour diverſified after the ſame manner as that of the old ſhell, even when the external ſurface is of a different colour from what it ſhould naturally have.

It would be wrong to conclude from what has been here ſhewn of the formation of the ſtripes which adorn certain ſpecies of ſhells; that the external ſurface of all ſhells ſhould either be ſtriped, or have one uniform colour; and that there ſhould be no ſhells, whoſe external ſurface exhibits ſpots or ſtains differently placed, irregularly figured, and ſeparated from each other by unequal diſtances, ſuch as the ſhell, fig. 12. upon this ground, that ſuch ſpots cannot be produced on the ſurface of the ſhell, without different ſieves on the collar of the animals to tranſmit a different liquid from what paſſes thro' the other places,

places, and consequently without the apparatus necessary to produce a striped shell; for it is obvious, that the sieves must have subsisted during the whole formation of the shell, in order to render this shell striped in its whole compass; but if it happens on the contrary, that these sieves change so as the pores which before transmitted a liquid matter proper to form a brown shell, become either too wide, or too narrow, or alter their figure in any other manner after filterating a certain quantity of their first liquor, and the like alteration befall the rest which transmitted a liquid proper to form a white shell; the consequence must be, that the shell now formed, will exhibit several black and white spots, combined with the same irregularity as the sieves had been altered.

This will not appear a supposition without all foundation, to such as consider, that certain alterations befall even the sieves of the collar of snails, which produce striped shells; for some of these shells may be found wherein the stripes are very strong and vivid towards their aperture, while there is no appearance of any stripes on the first circumvolutions of the spiral; that is, on those next the *vertex* of the shell: now this change of colour can only proceed from a like change in the sieves of the collar; 'tis true, we are to conceive much more considerable changes on the collar of the animals which inhabit such shells as that of fig. 11.; but these changes are equally possible with the other.

The fluidity of the liquor whereof the shell is formed, may also have some share in the irregular distribution of the colours on some kinds; for it is easy to conceive, that if the liquid which some animals yield for the formation of their shell, be fluid enough to run easily from one place

place to another, ſhells may eaſily come to be irregularly marked, provided there be ſieves on their collar, which tranſmit different liquids; ſince in that caſe it muſt frequently happen, that the liquid will not remain in the place where it was firſt lodged; but that what for inſtance was deſtined to form a white ſhell, ſhall remove itſelf to a place where a liquid iſſues that is to form a black ſhell; as on the other hand, that which forms the black ſhell, they run into a place where another liquor iſſues to make a white ſhell: now, as this muſt happen very irregularly, according to the different poſitions the animal is in, where the ſhell is formed, the ſpots muſt likewiſe be diſpoſed very irregularly.

Recourſe however muſt be had to the firſt of the two cauſes above aſſigned, *viz.* a change of the texture of the ſieves of the collar, in order to account for the regular poſition of the red ſpots, in a ſquare or rectangular figure, which adorn the ſhell repreſented in fig. 13, it being neceſſary to form it ſuch, that the ſieves in this ſquare, or rectangular figure, which tranſmit the liquid proper to give ſuch colour to the ſhell, ſtop, and open again at a certain rate.

Tho' the collar of the ſnail trace out the whole circumference of the ſhell, and tho' this ſuffice to diſtribute colours regularly thereon, yet it does not give it all its thickneſs, which receives a conſiderable augmentation from the particles of the liquid iſſuing from the pores of the reſt of the ſkin: this is eaſily ſhewn, for upon reducing the ſhell of a large ſnail to the ſame number of circumvolutions, as that of a ſmall one, tho' they appear equally large, yet that of the large one will be found the thicker: this increaſed thickneſs of the ſhell is particularly obſervable in ſome ſpecies of ſpiral

ſea-

ſea-ſhells, where it ſometimes riſes to ſuch a pitch, that the firſt circumvolutions of the ſhell grow up; ſo that the animal is obliged to withdraw its tail into the circumvolutions further off, as appears very ſenſibly in ſome ſhells diſected by M. *Merry*; one whereof is repreſented by fig. 13, where the ſpaces *a a a*, formerly poſſeſſed by the body of the animal, are become quite ſolid.

The animal's tail not adhering to the vertex of the ſhell, as ſome have imagined, it can eaſily diſplace it; eſpecially while the part whereby the animal is faſtened to the ſhell, is changing (for this part changes according as the body of the animal, makes more, or fewer, ſpires:) thus a little ſnail, for inſtance, ſhall be faſtened by a part of its firſt circumvolution, and when its grown bigger, ſhall only be faſtened by the ſecond.

The laſt *ſtrata* formed by the ſkin which does not cover the collar of the ſnail ought to be white agreeably to all that has been hitherto advanced, and they are ſo accordingly, as may eaſily be perceived by rubbing off the firſt *ſtrata* of the external ſurface of theſe ſhells, with a file; thoſe which then remain appearing white, or the ſame may be proved with leſs trouble by conſidering, that the colours of the empty ſhells found in gardens, are frequently almoſt effaced, and ſometimes appear quite white; the firſt ſtrata which are the only coloured ones, having been carried off by too much attrition againſt the ground.

The growth of ſhells being proportionate to that of the animals inhabiting them, is hardly ſenſible; yet in the generality of ſhells, we can eaſily diſtinguiſh their ſeveral ſtages, or degrees of growth: theſe are expreſſed by ſeveral little parallel eminences, which one would be apt to take for the fibres of the ſhell; they are ſpread over the whole ſurface in ſuch as are flat, or two leaved; and over the whole breadth in thoſe twiſted ſpiralwiſe. The leaſt reflection on the manner above explained of the formation of ſhells, will let us ſee, that they cannot grow without producing the little eminences juſt mentioned; for each new piece of ſhell muſt be faſtened immediately under that preceeding it, which, of conſequence, will be higher than this, by the whole thickneſs it had attained when the growth of the animal gave riſe to this laſt; under which likewiſe muſt be placed the piece produced next to this; by ſuch means the ſhell muſt be covered with a multitude of little eminences parallel to each other, which may be diſtinctly ſeen on the ſhells of ſnails†, where they are very near together.

Each ſhell * has uſually ſome of theſe eminences much more diſtinct than others, and further aſunder, which expreſs the different times when the ſhell ceaſed growing, and bears ſome analogy to the different ſhoots obſervable on each branch of a tree, the heat of ſummer, or the cold of winter, putting a ſtop to the growth of the animal which inhabits the ſhell, as is eaſily obſerved in ſnails, its ſhell is ſtopped of courſe while thoſe ſeaſons laſt; I mean the extent or compaſs of it, not its thickneſs, which is continually increaſing by the flux of fluid particles from the body of the animal: hence when it begins to grow again, in a more favourable ſeaſon, the new piece of ſhell it now produces, is faſtened under a much thicker ſhell, than when its growth proceeds gradually; and conſequently, that former term muſt be expreſſed by a larger eminence.

† Fig. 6. * Fig. 7, 18.

I There

There is one other thing which renders the ſeveral places where the ſhell began growing, after having ceaſed for ſome time, ſenſible, *viz.* a change of colour on the ſtripes above-mentioned; the black or brown ſtripes are in theſe places, of a much brighter colour, and ſometimes ſcarce different from the reſt of the ſhell; nor will the cauſe of ſuch change be far to ſeek, if it be remembered, that the † ſieves of the collar which tranſmit the liquid proper to form theſe black or brown ſtripes, have their origin at the extremity of the collar; whence it is obvious, that the firſt ſtratum of ſhell drawn by the extremity of this collar, muſt be of a different colour from that of the ſtripes; but as the growth of the animal occaſions the ſtripes of the collar to be found under this firſt ſhell, while it is yet very thin, but conſequently tranſparent; it does not hinder the ſhell produced under it, from appearing black where it is ſo; but when the animal has ceaſed growing for ſome time, it increaſes the thickneſs of this ſhell produced by the extremity of the collar, ſo that the ſhell which the ſtripes of the collar produce under this laſt, when the animal begins to grow again, being placed under a piece of ſhell much thicker, and leſs tranſparent, the colour of theſe ſtripes is the leſs diſcernable: and thus appears different here, from what it is in the reſt of the ſtripes.

The figure of certain ſhells is what may now ſeem the moſt difficult to reconcile with this theory of their growth, and accordingly make the ſecond difficulty which I propoſe to ſolve; the chief objections drawn from the figures of ſhells againſt their growing by juxtapoſition, may be reduced to 4; 1ſt, The change of the curvity

† Fig. 7.

in certain parts of ſome ſhells; for how on this ſyſtem ſhould the curvity of ſome ſhells be produced, which, after extending for ſome time outwards, turns again upon itſelf, as in fig. 15, which repreſents the tranſverſe ſection of a ſhell of this kind, where it may be ſeen? But after the ſhell has twiſted from A through CCC to EEE, it turns back again to DDD, a meer appoſition of parts ought rather to continue the ſame curvity. 2dly, How are the horns produced which we find on certain ſhells? By horns, I mean a kind of eminences ſeen on ſome ſpecies of ſhells, which by their figure reſemble the horns of ſome animals, ſuch are the eminences in fig. 14 and 15, repreſented by the letters CCC. 3dly, How can the furrows, or flutings, be formed, which inrich the external ſurface of certain ſhells, while their internal ſurface is perfectly ſmooth? For why ſhould ſuch ſhells be thicker through their whole length in ſome places, than in others, as are thoſe of fig. 17, 18, 19? Laſtly, How can a cavity be formed, wherewith the body of the animal has no communication, and which runs all along the acclivity of the ſhell, as that repreſented by E, fig. 7.

The ſhells of land ſnails will yet furniſh an anſwer to the firſt of thoſe difficulties†. The laſt ſtage of growth of theſe ſhells is a kind of rim or ledge, about a line broad, which turns outwards; whereas all the reſt of the ſhell turns inwards; this ledge formed, the growth of the ſhells is at an end; they who may never have ſeen a ſnail's ſhell without ſuch a ledge, ſeem to have ſome reaſon to conclude, that theſe ſhells can never be produced by a ſimple juxtapoſition; for in that caſe they ſhould twiſt a contrary way from

† Fig. 6.

what they do; but if it be confidered, that fnails of all ages and degrees of growth below the higheft, have no fuch ledge, the difficulty difappears, for the fame thing doubtlefs happen to fuch fhells, as that of *fig.* 15. This ledge is of the fame colour with the ftripes in the little ftriped fnails, reprefented in *fig.* 11, and accordingly the extremity of the collar is of the fame colour as the skin, which forms the ftripes, as may be feen in *fig.* 10.

The curvity of the fhell is unchangeable, unlefs that of the body of the animal, which is its mould, happen to change, 'tis eafy to imagine probable caufes of fuch a change in the growth of the fnail. For inftance, 'tis not unlikely, that the internal fibres of the collar may grow fafter than the external ones; the confequence whereof muft be the latter's pulling the collar of the fnail towards them, and obliging it to bend outwards.

As the different length of the fibres of the collar gives us an eafy conception, how it may come to be bent outwards; fo, by attending to this different length of the fame fibres, we may conceive how the bodies of feveral animals come to be twifted fpirally; for fuppofing that from the production of fuch animals, the fibres of a certain part of their furface are longer than thofe of the oppofite furface, 'tis evident the body will crook itfelf fo as, the furface, whofe fibres are fhorteft, will form the concave of the curvity; and the other furface, whofe fibres are longeft, the convex which is enough to make the body of the animal defcribe a fpiral, fince it cannot grow without always bending thus on itfelf, provided its long and fhort fibres grow in the fame proportion. 'Tis true, in the cafe above-mentioned, it would only defcribe fpirals, whofe feveral circumvolutions

tions would be almoſt in the ſame plane; whereas few animals have the ſhell or the body, which ſerves it as a mould, twiſted in this manner, but have the ſeveral ſpires, both of their body and ſhell, in different planes; but with one ſuppoſition more, we ſhall eaſily conceive how thoſe laſt ſpirals are formed; for ſuppoſing, beſide the two ſurfaces, whoſe fibres have been laid down as longer one of them than the other, that there are two other directly oppoſite ſurfaces, each of them comprehended between the preceding ones, but ſmaller than them; and that theſe two laſt ſurfaces are alſo formed in ſuch manner, that the fibres of the one are longer than the correſponding fibres of the other. This muſt needs oblige the body of the animal to incline itſelf on one ſide, and hereby form ſpires ſituate in different planes.

If land ſnails happened to produce a ledge like that found at their laſt term of growth, after the formation of each quarter of a circumvolution, and that their external fibres relaxing hereupon, they produced another quarter of a circumvolution, bent the ſame way as the former; after which they produced a new ledge, and ſo in a ſucceſſion their ſhell would be divided from ſpace to ſpace, by a number of ſuch ledges, which would be a pretty ornament to it. 'Tis † by a like artifice, that the ſhells of the ſeveral ſpecies of ſea ſnails, which appear ſo wonderfully wrought, are formed, the working being only ſo many little ledges of ſhell diſpoſed at certain diſtances, which yet beautified in ſuch manner, as if nature had been at the pains to carve it.

† Fig. 16, 17.

The

The * horns found on ſome ſpecies of ſhells, are alſo produced by the ſame mechaniſm as the reſt of the ſhell, certain fleſhy tubercles growing on the body of the fiſhes, which inhabit them, ſerve them as moulds; and according as more or fewer of theſe tubercles are formed, while the animal grows one circumvolution, there are more or fewer of ſuch horns in the ſame circumvolution. They are hollow when theſe tubercles have remained on the body of the animal all its life-time; partly hollow, and partly ſolid, when the ſame tubercles had been partly diſſipated, and quite ſolid, when the tubercles had been quite vaniſhed during the animal's life.

To the ſame formation, and that of the ledges, we are to aſcribe much ſmaller eminences, which from their figure, may be called prickles, uſually found at the end of the terms of the ſenſible growth of theſe ſhells, as may be obſerved in *fig.* 18.

The flutings found on the external ſurface of ſhells, while their internal ſurfaces are perfectly ſmooth, will not be leſs eaſy to explain. It will ſuffice to obſerve, that the whole extremity of the ſurface of the animal's body is likewiſe fluted; and hence we may find the ſhell likewiſe fluted in its internal ſurface to ſome diſtance from its extremity †; but in regard the reſt of the ſurface of the animal's body is ſmooth and ſoft, the animal growing, and the part of its body not fluted, coming to correſpond to that of the ſhell, which is what this part furniſhes; for the ſhell ſerves to fill or ſtop the internal flutings, whence the ſhell is only found fluted on its external ſurface, excepting only the firſt lines of the breadth of its internal one.

* Fig. 14, 15. † Fig. 17, 18, 19.

There

There * is a flat ſea ſhell, much like the kind called *St. James*, whoſe formation would have appeared very difficult; but for what we have ſhewn of the formation of the flutings in other ſhells, this ſhell is likewiſe fluted, but the two ſides of each flute are little canals incloſed on all ſides with ſhells, and perforated from the vortex of the ſhell to its extremity. Tis eaſy to ſhew how theſe little canals may be formed, all required being to conceive, that the firſt extremity of the body of the fiſh is deeply fluted, and the reſt of its body quite ſmooth, and its ſubſtance too hard to enter the channel or fluting, formed by the extremity; ſo that the reſt of the body only produces a few leaves or ſhells, which are applied over this fluting, without cloſing it intirely, but leaving a little canal ſuch as above related.

Before we come to explain the formation of the cavity running along the flight of certain ſpecies of ſhells, between which and the body there is no communication, it may be neceſſary to define what we mean by flight. To form a preciſe idea thereof, it muſt be obſerved, that when the collar of the animal draws the ſeveral ſpiral circumvolutions of the ſhell, that part of the external ſurface neareſt the axis it winds about, forms ſpires, whoſe diameter or width is leſs than that of the ſpires deſcribed by the other points of the collar. Now that part of the ſea-ſhell formed by theſe ſmaller ſpires, is called its flight; a tolerable notion whereof may be conceived from the flight of a ſtair-caſe.

To unfold the myſtery of the formation of this cavity along the flight, it muſt be firſt obſerved, that the upper ſurface of the collar is convex, and the lower concave; as is evident hence, that the

* Fig. 19.

firſt

first is placed under the concavity of the shell, and the second over its convexity: * now the upper surface of the collar being always left bare, by the growth of the animal, 'tis this that forms the new shell, and that part of the upper surface of the collar, which traces the smallest spires, is likewise that which produces the flight of the shell; imagine now the collar of the animal to spread and extend, in order to produce a new piece of shell, and consequently a new piece of the flight, as the animal is twisted within its whole shell, we are to conceive at the same time that a certain part of its body extends and winds about a part of the flight it had not before reached to; this part thus applied to a new place of the flight is that where the lower surface of the collar makes an angle with the upper. Now if we conceive this part of the animal to be neither crooked nor flexible enough, to mould itself perfectly upon the part of the flight, it is new applied on, 'tis evident a little void space will be left between the flight, a part of the body of the animal, and a little piece of the old shell found between this part of the body and the flight. The part of the body which contributes to inclose this cavity, not being covered with shell, will yield a liquor proper to form one; and by the production of this new piece of shell, the little hole will be surrounded on all sides; and 'tis apparent this same hole must run all along the flight by reason the shell cannot grow, but it must be formed at the same time.

If the little part, which helps to inclose the hole, emits store of liquor, the hole by this means will become quite solid, being stopped up by the new shell; this accordingly befalls several new

* Fig. 7.

ſhells, whoſe flights are much thicker than it ſeems they ſhould be.

If the curvity of the flight diminiſh enough to give the body of the animal room to mould it ſelf thereon, after the ſhell has made a certain number of ſpires, 'tis evident no more hole muſt be formed, and that what is already formed muſt be ſtopped towards its upper ſurface. This accordingly actually befalls ſnails, which have attained their laſt degree of growth, or to whoſe ſhell the ledge is formed, as may be ſeen in *fig.* 11. The little ſhell there repreſented has a little ledge B B B, and the hole which ſhould appear in E, were it not arrived at its period of growth, is ſtopped up, by reaſon of its arrival thereat. The ſame thing befalls large ſnails, and the only reaſon why we ſee the holes E in *fig.* 7 and 8, upon the flight of their ſhell is, that they had not attained their utmoſt growth; otherwiſe thoſe holes would have been covered over as in *fig.* 11.

When the collar draws the ſeveral ſpires round a little cone, 'tis evident a little conical ſpace muſt be left vacant in the middle of the ſhell; that is, a little cavity will appear, round which all the ſpires are placed. Several ſpecies of ſea-ſhells, as that of *fig.* 12, and diverſe kinds of land ſnails have ſuch a conical aperture.

If the *vertex* of the cone, round which the collar of the animal winds, be at the origin of the ſhell, 'tis evident this hole muſt terminate in the point of the ſhell, which will cloſe it here. Such is the hole of the ſnail ſhells above-mentioned, and that of *fig.* 12, which terminates where the ſhell commences; but if the *vertex* of the cone be beyond the origin of the ſhell, it muſt be perforated throughout; and after this manner are ſeveral ſea-ſhells formed.

Laſtly,

Laſtly, if we ſuppoſe the collar of the animal twiſt round a ſolid of ſome crooked figure, in lieu of the cone above ſuppoſed, and the *vertex* of this ſolid to be at the origin of the ſhell, 'tis likewiſe evident, that a hole will be formed in the ſhell of the figure of ſuch ſolid.

If the animal inhabiting ſuch a ſhell, form a cavity all along the flight thereof, ſuch as we have already repreſented on the ſhells of large garden-ſnails, this its ſhell muſt be perforated with two ſeveral holes through its whole length, and conſequently will have two oblong apertures*, wherewith the body of the animal has no communication.

Theſe two holes may ſometimes alſo be produced after the ſame manner as that running along the flight. To conceive this, we need only imagine, that the part which afterwards poſſeſſes the place of that which has formed the hole, by reaſon it could not mould itſelf upon the flight, that the part I ſay of the animal's body that ſucceeds this, cannot adapt itſelf exactly to the ſhell it has produced.

A volume would hardly ſuffice to relate all the remarkables in the figures of ſhells, I have preſcribed myſelf narrower bounds, and the more willingly I do it, as there is ſcarce any thing extraordinary in them, whoſe formation may not be reduced to ſomething already laid down.

An explanation of the figures, tranſlated by J.M.

Fig. 6. repreſents a ſhell of a great garden ſnail, broken in two different places. The letters A A A mark the circumvolution of the holes that have been made in it. We ſee theſe holes

* Fig. 15.

ſtopped by new pieces of ſhell, placed immediately under the old one. It muſt be obſerved, that this new ſhell is not coloured like the old one, that it has not alſo different little lines, which may be called fibres of the ſhell, though improperly becauſe of their figure; and theſe fibres are marked diſtinctly upon the old one.

Fig. 7. The letters A A A mark the circumvolution of an aperture made in the ſhell. It is a piece of thin skin, which ſtops this aperture; it is paſted to the inner ſurface of the ſhell. B repreſents the new ſhell, which has formed itſelf upon the ſurface of the skin which touched the body of the ſnail.

D D is the circumvolution of the aperture of the ſhell, which is not turned back like that of of *fig.* 6.

E marks by a prick'd line the aperture of a hole, which runs along the whole flight of the ſhell, quite to its ſummit or point P.

C C is one of the notable bounds of the growth of the ſhell. We there ſee the rays almoſt interrupted, or faintly traced.

Fig. 8, is the ſhell of a great garden ſnail, of which the circumvolution of the aperture went juſt to A, but broken according to the turn of this aperture, which is bounded by the letters B C C. C C C is a bit of thin skin, which here appears paſted upon the outer ſurface of the ſhell, but we muſt alſo imagine it paſted upon the inner ſurface of the ſame ſhell; ſo that it covers the whole edge of the ſhell, which is conſequently contained between the two extremities of this piece of thin skin. E D D D Q mark the new ſhell which has been produced, and ſeparated from the old one by the thickneſs of the skin upon which it is applied.

Fig.

Fig. 9. repreſents the ſhell of a ſmall ſnail, newly come out of its egg.

Fig. 10. is a ſmall garden ſnail, with 5 black or brown rays painted upon its ſhell; the intervals between theſe rays are of a lemon colour. This ſnail appears diveſted of a part of its ſhell, which went before to A A A, and is at preſent terminated in B B, which was done on purpoſe to ſhew the collar of this ſnail, which is alſo marked with 5 rays C C C C C of a brown colour, but not ſo deep as that of the ſhell; the origin of theſe rays is at ſome little diſtance from the extremity of the collar; and they uſually are but a line or two in length. The ſpace between theſe rays, and that which is between the neareſt extremity to the edge of the collar, and that edge of the collar A A is of a much brighter colour than that of the rays, and alſo more brown than that of the reſt of the skin, which is from the extremity of the rays C C C C C the moſt diſtant from A A A, quite to the ſummit P of the ſhell.

The edge A A A of the collar of the animal is of a browniſh colour.

Fig. 11. is alſo a ſtriped ſhell, with only 3 rays. There have two holes been made in this ſhell, of which the fartheſt from the collar is marked A, and the neareſt D C C. The ſhell which was formed to ſtop the hole A, is of a different colour from the rays and their intervals. But that which ſtopped the hole D C C is of the ſame colour with the old one; ſo that the black rays are continued in C C, and D is of a lemon colour. This laſt hole however is here painted not quite ſo near the edge of the ſhell as it ſhould be.

B B B mark the return of this ſhell, which was arrived at its laſt degree of growth. This return is of a brown colour; it has alſo been ſeen (in *fig.* 10.) that the extremity of the edge of the collar is brown. The origin of the rays of the ſhell is not at this return, as the origin of the rays of the collar is not at the extremity of this collar.

E marks the ſhell, which then ſtops the cavity along the flight.

Fig. 12. repreſents a ſhell, called *la Veuve*; it is marked with different black ſpots, of irregular figures, and placed irregularly on a white ground.

At A there is a hole, which goes juſt to the ſummit of the ſhell. This hole is formed very differently from that of *fig.* 7 and 12.

Fig. 13. is a ſpecies of *turbinites*, upon which appear different little ſquares, of a red colour, diſpoſed in a pretty regular proportion.

Fig. 14. is the ſection of a ſhell, where the tail of the animal has been obliged to abandon the firſt turns, becauſe they are grown quite ſolid. The letters A A A A A A mark the ſpaces, which at firſt were occupied by the body of the animal, and afterwards filled up. It ſhews alſo that part of the ſpace E B is become ſolid, namely that which is marked E, the body of the animal occupied only the ſpaces B B, D D D D, &c.

C C C C are thoſe eminences of ſhells, which I have called horns, or ſections of thoſe eminences.

Fig. 15. is the tranſverſe ſection of a ſhell, which after having made a certain number of ſpiral turns in C C C C one way, turns back again in D D D.

A A are two holes, which are in the whole length of the ſhell, with which the body of the animal does not communicate, which occupies the ſpaces B B B, &c.

C C C are eminences, or little horns. *Fig.*

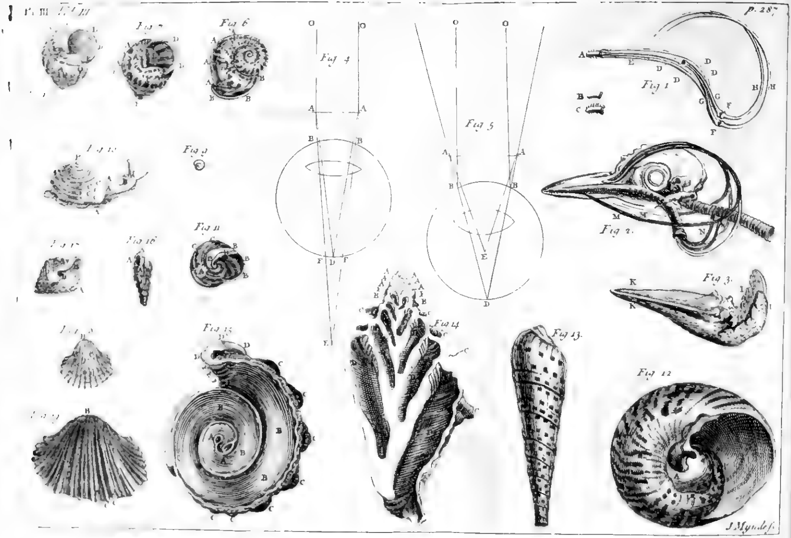
Fig 1
Fig 2.
Fig 3.
Fig 4
Fig 5
Fig 6
Fig 11
Fig 12
Fig 13.
Fig 14
Fig 15
Fig 16
J. Mynde f.

Fig. 16. is a ſpecies of *turbinites*, which ſeems very artificially wrought. This ornament comes from different returns, ſuch as the laſt A A A diſpoſed from ſpace to ſpace.

Fig. 17. has alſo ſeveral returns like the preceding. But we may alſo obſerve, that each of theſe returns is fluted.

BB is the inner ſurface of the ſhell, which is ſmooth, tho' the returns are fluted.

Fig. 18. is a ſhell with the outer ſurface fluted, tho' the inner ſurface is ſmooth.

CC, C C C, D D D are 3 bounds of very ſenſible growths, the laſt of which D D D D is adorned with ſeveral little eminences, which I have called points, becauſe of their figure.

Fig. 19. is alſo a fluted ſhell, but it has this ſingularity, that each of the ribs of the flutings are themſelves little canals; that is, there remain void ſpaces in the middle of theſe ribs through their whole length; and theſe holes are ſurrounded with ſhell in ſuch a manner, that the body of the animal does not enter within. We have opened one of theſe canals marked B, D D, A A, C C. It appears, that the inner ſurface D D, which is applied to the body of the animal, is terminated in A A, that is, theſe long holes are not ſhut up from A A to the extremity C C, into which the body of the animal enters.

XII. *Conjectures and Reflections upon the matter of light, or fire, by M.* Lemery, *jun. tranſlated by Mr.* Chambers.

The matter of fire is the 1ſt, and moſt powerful diſſolvant of terreſtial bodies, we having no other that penetrates ſo deep, and disjoins the component principles ſo compleatly; it is to this matter the chymiſt is indebted for the ſecrets he ex-

* Nov. 13, 1709.

extorts from nature which he would never reveal unleſs forced, and as it were tortured by ſo active a diſſolvant.———Now a matter which contributes ſo much to our knowledge of other bodies, does certainly deſerve to be itſelf ſtudied in its turn.

It is allowed to be the real principle of heat, light, and even of the fluidity or fuſion of ſeveral terreſtrial bodies, which, without the mixture and action of this matter would always remain in a ſolid form; but as it is not always found in ſufficient plenty, or meets with bodies which make too much reſiſtance, we ſometimes find, that inſtead of liquifying or keeping them in their former fluidity, it engages itſelf in them, and becomes incloſed in ſuch a manner as to remain impriſoned, and to need ſome external cauſe to come to its aſſiſtance, and open the cells on the outſide, wherein it was retained.

There are 2 remarkable circumſtances in this impriſoned matter; the firſt, That it ſometimes makes a ſenſible increaſe in the weight of the body it is contained in; and the 2d, That it retains all its peculiar properties during the whole time of ſuch captivity, whereof it gives evident proofs, when ever occaſion is given it, of breaking looſe from its confinement, and making an effect upon ſome other body.

Every body will not allow of what I here attribute to the matter of fire, it is even alledged, that ſuch doctrine is repugnant to our idea of what conſtitutes the proper nature of this matter, and yet it is ſupported by ſo many and ſolid experiments, that ſeveral chymiſts of the firſt claſs, have been obliged to adopt it.———To ſet it in a further light, and have the more pretence for applying it to certain *phænomena*, which I propoſe

poſe to account for in this memoir, and ſome others, I ſhall relate the experiments it is grounded upon, and anſwer ſuch objections as are brought againſt it; objections, which, notwithſtanding all the veriſimilitude given it by experiments, are of force ſufficient to bring its truth in queſtion.

Every body knows, that ſeveral metalline bodies when expoſed to the fire, as regulus of antimony, lead, tin, and even mercury; notwithſtanding that they looſe a great deal of their own ſubſtance, which flies into the air during the operation, are ſo far from weighing leſs than they did before, which one would naturally expect, that they weigh a great deal more. Now, the queſtion is, whence this augmentation of weight ſhould ariſe? And whether the fire, which reduces theſe bodies into the calcined ſtate we ſee them do not likewiſe give them this additional weight?

It may perhaps be anſwered, That this augmentation of weights ariſes from the acids of the wood, or coals, which are introduced into theſe bodies, by means of the fire, and remain in them, when the particles of fire are gone off.—— But it is difficult to conceive, how a ſufficient quantity of theſe acids ſhould arrive at a calcined body to produce an augmentation, which, as M. *Homberg* obſerves, ſometimes amount to $\frac{1}{1}$th part of the whole, it is certain ere they reach the body expoſed to the fire, they muſt paſs through the veſſel wherein the matter is contained; and yet the veſſels uſed in theſe operations are ſuch as will hold the moſt violent acids, without letting them eſcape thro' their pores; if therefore ſome acids of wood find means to paſs along with the particles of the fire, thro' the pores above-mentioned, yet the difficulty of paſſing is ſuch, as to make their number very ſmall; ſo that much the greateſt part of the acids muſt be ſtopped, and retained by the particles of the veſſel itſelf, which is uſually of a nature diſpoſed to abſorb them; the matter of fire, on the contrary, paſſing freely and plentifully through all kinds of veſſels, muſt be allowed much fitter to make this augmentation, which being very conſiderable, will ſuppoſe a copious cauſe, ſuch as fire alone can furniſh; but what proves the point ſtill more fully is, that upon expoſing theſe bodies to the ſun's rays collected by a burning-glaſs, their weight is no leſs increaſed than if they had been expoſed to a common fire: now in this caſe all acids of wood, and coals, are effectually precluded; and whatever ſuppoſition we go upon, it will be equally difficult to exclude the fire from its ſhare in this *phænomenon*.

But beſides proving that the matter of fire inſinuates itſelf into certain bodies, and augments their weight, it muſt likewiſe be ſhewn, that this matter in being thus repoſited in bodies, alters not its nature, but retains all the particular properties which conſtitute it matter of fire.—— The proof of this ſecond article will be a confirmation of the firſt; for if what is introduced into the bodies during their calcination, be the real matter of fire, when we conceive, that this matter engages itſelf, and reſides therein, with all its native properties, it will be eaſily allowed, that the augmentation of weight ariſes chiefly therefrom.

Now the matter of fire retained in metalline bodies, is kept too cloſe to be able to manifeſt itſelf by any of the ſenſible ſigns, which ſhould make it known, and diſtinguiſh it from other matters; the reaſon is, that to become perceive-

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able, it muſt force its priſon doors, and make an attack upon ſome other body; but the cells it is repoſited in, are ſo ſtrong, and ſolid, that nothing leſs than a fire of fuſion will ſuffice to break them, and diſengage the fiery particles contained in them.

It is otherwiſe with thoſe which had inſinuated into ſtony or ſaline bodies, by means of calcination; for theſe bodies being of a laxer texture, water alone ſuffices to make them a paſſage out; for that by impinging againſt the particles of thoſe bodies, it not only deſtroys the union, but reduces them into a fine powder, capable of being ſuſtained in the fluid: thus the reaſon why lime-water for inſtance is a drier, and abſorbent, is owing to the ſtony particles it is replete with, and if lime ſteeped in water be unfit for the uſes of building, it is by reaſon its particles having been much attenuated by the fluid, unite again ſo intimately as to form one compact and durable maſs.

As water therefore diſunites the particles of ſaline and ſtony bodies when calcined, and grinds them ſo very ſmall, if there be any matter of fire lock'd up between the particles thereof, it muſt eſcape by means of this diſunion; and this it does accordingly, throwing itſelf into the aqueous fluid which had delivered it, and which becomes more or leſs heated thereby, in proportion to the quantity of this matter.

Another remarkable effect is obſerved in ſome of theſe bodies; *viz.* That making a very ample proviſion of the matter of fire, and being liable to let it looſe again upon the ſlendereſt occaſion, when they are applied upon an animal body, the fiery particles which iſſue from them, and inſinuate into the texture of the part, burn, and make an eſchar differing only in degree from that produced by a live coal, or a hot iron.

The eaſineſs for the accounting for the effects above-mentioned, on the ſuppoſition of particles of fire latent in ſuch bodies, is a violent preſumption in favour of the hypotheſis; but what renders it inconteſtable is, the manner wherein calcined bodies become diſpoſed for ſuch effects, which is in conſequence of their being expoſed to the matter of fire———Add to this, that the properties they acquire hereby, are the ſame as thoſe of natural fire; and that none of theſe effects are unaccountable for, without any tolerable ſatisfaction upon any other footing.

For to take a particular inſtance, when lime caſt in water turns that liquid hot, and makes it boil as fire would do, ſhall this effect be attributed to any fermentative particles contained in the lime, and brought into action by the fire? With what ground can this be done, when we find nothing in lime but a pure earth, ſtripped of all ſalts, the fire ſeeming to have expelled all other matters to make room for itſelf? And how ſhould a pure earth, when ſteeped in water, be able to heat it? But the particles of fire, ſay they, are only ſuch, by reaſon of the rapid motion they are agitated with. Now ſuppoſing them engaged in the texture of groſs bodies, they muſt quickly looſe their motion, and conſequently ceaſe to be fire, and thus become incapable of the effects attributed to them, ſo that ſome other cauſe muſt be had recourſe to.

I anſwer, that the matter of fire muſt be conſidered as a fluid of a certain nature, and endued with properties peculiar to it, which diſtinguiſh it from all other fluids. Now I agree, that theſe properties depend on the rapid motion of the par-

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ticles of this fluid; but conceive withal, that the figure of each of theſe particles muſt be taken into the account: be this at it will, when this fluid happens to be detained in the texture of any groſs bodies, its condition, I ſuppoſe, is no worſe than that of other fluids, and conſequently muſt have the ſame fate: now water is likewiſe a liquid, whoſe fluidity, as ſhall hereafter be ſhewn, depends upon the matter of fire, and conſequently whoſe fluidity muſt be much ſhort of that of fire; and yet we ſee water daily incloſed in numerous bodies without looſing its fluidity, or any of the properties which characterize it; ſo that upon bringing it forth, we find it the ſame matter as before; and much more muſt the matter in queſtion, when in the ſame circumſtances, retain its nature, and be found upon its enlargement with the ſame properties as before.

But it will be replied, that the buſineſs here is not about a compariſon, but to ſhew how the particles of fire detained in a groſs body can preſerve their motion. This we ſhall conſider accordingly, after firſt diſpatching the following difficulty, the anſwer to which will naturally lead to that ſolution.

'Tis eaſy to conceive how a groſs fluid, whoſe particles are in a moderate agitation, ſhould be retained in the texture of a ſolid body; but it is ſcarce conceivable, but that a matter ſo ſubtil and active as fire, ſhould not find ſome paſſage out of the bodies it has been introduced into, or ſhould not even make itſelf a paſſage by the rapidity of its motion.

I anſwer, that as to what regards the activity of the matter of fire, it is certainly very great; and that when this matter is in a ſufficient quantity to

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ſurmount the reſiſtance of a ſolid body, it makes its way thro' by breaking the continuity of its parts; but it is not always that it is in quantity ſufficient for this purpoſe; in which caſe its force being inferior, or only equal to the reſiſtance of the ſolid body it is incloſed in, all its activity and efforts remain uſeleſs, unleſs they be aſſiſted by ſome foreign cauſe acting on the outſide.

As to the ſubtilty of the particles of this matter, it muſt be allowed very conſiderably; but the queſtion will be, Whether the pores of the cells they are incloſed in may not be ſtill ſmaller? As we have no microſcope fine enough, nor any meaſure exact enough to decide this point, and there being withal no inconvenience in ſuppoſing the pores above-mentioned ſmaller than the particles of fire, I inclined to this ſuppoſition, by reaſon of the ſtrong arguments we have, that the matter of fire is actually retained in the texture of ſeveral bodies.

Nor do I pretend, that the pores thro' which the particles of fire cannot paſs, ſhould be impenetrable to all other kinds of matter, for how ſmall ſoever theſe particles be, I can conceive others 100 times ſmaller, which can eaſily pervade all pores, and whoſe office may perhaps be to fill the vacuities of the univerſe; but notwithſtanding that their ſmallneſs ſurpaſſes that of fire, I do not apprehend them ſo proper to produce the effects here treated off, as the matter of fire——My reaſon is, that one of the chief properties of fire is to diſſolve and liquify terreſtrial bodies, which it effects by dividing and diſuniting the particles, and giving each the neceſſary motion to conſtitute it a fluid; but the ſubtil matter above-mentioned, finds ſo open a paſſage thro' all bodies, that it eſcapes on every ſide without making ſo ſtrong an

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impreſſion on thoſe bodies as we find from the matter of fire, which being leſs ſubtil than the former, and conſequently unable to purſue the ſame roads, is forced to break the obſtacles in its way, and thus deſtroy the natural texture of the bodies; this reaſoning might be confimed by ſeveral ſenſible facts, of which, the following is one: if a net be ſpread in the ſtream of a river, the particles of water finding an eaſy paſſage thro' the holes or maſhes thereof, will do it no damage; but if a body come which is too bulky to paſs thro' thoſe maſhes, it muſt either be ſtopped thereby, or break the net; and the ſame befalls the matter of light, which, according to its quantity and ſtrength, is either detained in bodies, or diſſolves them.

Now to conceive without the help of any compariſon, how the matter of fire incloſed in the cells of a ſolid body, ſhould be able to preſerve its motion, we need only obſerve, that there is a more ſubtil matter continually pervading the pores of theſe cells, and which of conſequence muſt keep up the agitation of the particles reſiding therein.

M. *Saurin* has ſhewn, that we may ſafely affirm, that the proper matter, even of the moſt ſolid and heavy bodies, does hardly make the 100,000th part of their bulk. Now, though we ſhould abate a good deal of this ſuppoſition, yet there would ſtill be room enough in the moſt ſolid bodies to give paſſage, or even lodging, to a large quantity of foreign matter, in which caſe the ſubtile matter abovementioned, paſſing more copiouſly than can well be imagined, the fiery particles, notwithſtanding their impriſonment, will not want cauſes ſufficient to maintain their fluidity and motion.

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In effect, tho' it should be granted, that the particles of fire engaged in a solid body, could not always preserve their motion therein, it would not follow hence, that they must loose their proper nature of fire; for it is not only to the rapidity of their motion, but also to their figure, and their smallness, that their peculiar properties are owing: thus the particles of water are at rest when frozen; and yet no body will say, that they are essentially different now, from what they were before, since we find the least agitation, or the smallest degree of heat, enables them again to produce effects which they had still remained fit for by their peculiar figure, and whereof no other body, though exposed to the same heat, would ever be capable.

We likewise know, that salt is the matter of tastes, and has certain properties arising from the peculiar figure of its parts, and yet it only acts when dissolved; or which amounts to the same, when it floats in a fluid, which keeps its particles in motion. Now will any one alledge, that salt, when undissolved, is not the matter of tastes, nor has the special properties which characterize a salt? This can never be said while its particles retain their essential figure, the chief source of these properties.

Hence, tho' it were true, that the retention of particles of fire in a solid body, sometimes robbed them of their motion, they would only be in the case of frozen water, or solid salt; and might be restored to their former effects by recovering their motion.

It may perhaps be demanded, why the matter of fire, which had penetrated into a solid body, should not be able to get out again without the help of a foreign cause to facilitate its escape, the passages

paſſages having been open enough to let it in, cannot be too narrow for its exit.

I anſwer, that while the body is expoſed to the fire, its pores are opened, and dilated, and ſeveral of the fiery particles which are continually entering it, go out again with the ſame liberty ſo long as the dilatation of the pores remains; but when the fire ceaſes to act, the cauſe of this dilatation ceaſing likewiſe, the particles of the body which before had been ſwelled, do now ſhrink, and their pores return to their firſt ſtate; upon which the particles of fire which had inſinuated into the cells of ſuch body, are now utterly ſhut up, beyond a poſſibility of eſcaping, till ſome new dilatation of the pores, or a fuſion of the body ſet them free.

'Tis no wonder, that bodies, which, by their calcination have ſtored up a large quantity of fire, ſhould not afford any ſenſe of heat upon touching; for as the particles of fire incloſed within them, cannot reach the hand, which is only applied on their ſurface, the effect will be the ſame, as if they had no fire at all; as we find that ſalt is only ſenſible to the taſte, when it is diſengaged enough from all other bodies, to make an immediate impreſſion upon the organ of that ſenſe: and hence if a body newly taken from the fire, give a vehement ſenſe of heat, this is not owing to the particles of fire impriſoned in it, but to thoſe which have found paſſages open enough to let them out: for we may ſuppoſe two kinds of pores, ſome which are naturally big enough to give free paſſage to the matter of fire at all times, and others which only afford it, when dilated by heat.

Laſtly, it may be further asked, why the matter of fire incloſed in ſaline and ſtony bodies, does not break the texture of the parts, which oppoſe its eſcape, ſince we find water do it, which yet is incomparably leſs active than fire.

I anſwer, that if the quantity of matter in fire contained in lime, were as great as that of water poured on it, it would probably need no foreign aſſiſtance to get forth; but notwithſtanding all its activity, its quantity may be found ſo ſmall, compared to that of water, that the particles of water ſhall be more effectual than thoſe of fire. Now 'tis evident, that the fire procured from the bodies above-mentioned, is much leſs in quantity than the water uſed to procure them.

Further as to fixed alcali ſalts, which likewiſe contain particles of fire; water, 'tis known, diſſolves them with ſurprizing quickneſs, and fire itſelf would hardly be able to bring them ſooner to fuſion. If then water make ſo perfect a diſunion on the particles of theſe ſalts, it will hereby afford a free paſſage to the matter of fire retained among thoſe particles; and if nothing than a fire of fuſion ſuffice to prove the ſame diſunion in theſe ſalts, the matter of fire contained therein, being in much leſs quantity, and conſequently much leſs powerful than that of a fire of fuſion, 'tis evident on this occaſion it muſt act leſs effectually than water; nor muſt we ſuppoſe, that the liquid thus poured upon lime and alcaly ſalts, does alone open a paſſage for the matter of fire, but there being all the room imaginable to ſuppoſe, that this matter ſtill retains its motion within the bodies, we conclude, that 'tis continually at work in its priſon to force a way thro' the ſame; and that if it prove unable, notwithſtanding all its efforts, to make its eſcape, without an extraneous aid, yet it contributes conſiderably, and facilitates the effect of this aid.

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The ſun only ſeems a vaſt fund of the matter of fire, or if you had rather, a huge flame of the ſame eſſential nature as ours, ſince we find, that both the one and the other produce the very ſame effects; but this luminary being at a great diſtance from us, can only act on terreſtrial bodies, in 2 manners, *viz.* either by emanations and effluxes of his ſubſtance, emitted from thence to us, an hypotheſis liable to ſeveral difficulties, and inadequate to certain of the phænomena, or by trains of the matter of this fire diffuſed thro' all the intervals of the fluid maſs, between the ſun and us, which trains come to act upon terreſtrial bodies, when preſſed or impelled towards them by the preſence of the ſun.——Each train may be conſidered as a little ſun continued, but ſtill depending upon the large one, which is the ſource of their motion or action, upon terreſtrial bodies.

Theſe trains which form the luminous rays, and are immediate agents of light, do not differ as to the matter from that of the ſun itſelf, as we find by certain experience: hence as the ſun is a flame which produces the ſame effects as a culinary flame, we may infer the manner of its acting upon terreſtrial bodies, from that wherein our flame is found to act: now we know, that upon plunging one of the abovementioned bodies in a common flame, 'tis the proper matter thereof, without any foreign aſſiſtance, that penetrates, heats, and modifies them according to their peculiar nature; and when the ſame bodies are preſented to the fire without touching the flame, the impreſſions they receive therefrom, are eſſentially the ſame as thoſe, which the flame, if immediately applied thereon, would have produced: the only difference is, as to more and leſs, ſo that a body acted on immediately by a little flame, will be heated and altered after the ſame manner, as if placed at a conſiderable diſtance from a large flame.

All this gives a ſufficient indication, that the matter of fire or light, interpoſed between the flame and us, is of the ſame nature as the flame itſelf; and why then ſhould the luminous rays, which tranſmit the action of the ſun to us, and ſeem only to be continuations thereof, be of a different matter from the ſun's body? In effect, when collected by means of a burning glaſs, they act with an equal or even more vigour, upon terreſtrial bodies, than the moſt violent flame could do, if immediately applied on the ſame bodies; a proof not only that the matter of theſe rays is the ſame as that of the flame; but alſo, that the flame conſiſts in a collection of a vaſt quantity of the matter of light, which acts the more forcibly, as it is more copious, and collected cloſer. On this footing, the ſun only ſeems to differ from the rays of light, collected by a burning-glaſs; in this, that the matter of light being there much more copious, and more collected than it is in the rays, would act more readily and forcibly upon bodies immediately applied thereto.

The vehement action of the rays united by a burning glaſs, ſhews, that the fluid, which in their natural ſtate ſeparates and extends them, does likewiſe ſerve to moderate this action, and render it more ſupportable; for without ſuch medium, inſtead of enlightening and exciting a gentle warmth, they would conſume all bodies, and even deſtroy the organ of ſight. To explain this by a ſenſible compariſon, the air is that to the rays of light, which water is to the particles of fire, in a *Balneum Mariæ*, the rays being tem-

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pered in their paſſage thro' the air, as the fire is in its paſſage thro' the water; or the rays of light might be compared to coroſive ſpirits, which tear and lacerate when they are pure, but produce an agreeable ſharpneſs when diluted with a ſufficient quantity of ſome other fluid.

The matter of fire driven by the ſun upon terreſtrial bodies, modifies them differently according to their reſpective natures; ſome it eaſily puts and preſerves in a ſtate of fuſion, and ſuch are the particles of water, which originally are ſolid, and owe all their fluidity to the action of the matter of fire lodged amongſt them: this we prove hence, that their fluidity remains while the ſun determines a ſufficient quantity of this matter, to convey his action upon terreſtrial bodies; but in thoſe ſeaſons when he only ſends a little, ſuch little being inſufficient to maintain the fuſion of theſe particles, they relapſe into their firſt ſtate of immobility, from whence they recover, preſenting them to the fire; or which amounts to the ſame; and the ſun begins to ſhed a greater quantity of the matter of fire upon terreſtrial bodies.

From what has been ſaid, we learn, 1ſt, that ice is only a reſtoration of the particles of water into their natural ſtate; 2dly, that the bare abſence of the matter of fire ſuffices to effect this reſtoration; and 3dly, that the fluidity of water is a real fuſion like that of metals expoſed to the fire, only differing from it in this, that metals require a large quantity of particles of fire to liquify and ſupport them in a ſtate of liquification, whereas the particles of water ſeldom receive ſo little fire, as to allow them to reſume their natural ſolidity.

Another effect of the matter of fire ſhed upon terreſtrial bodies is, to engage itſelf in certain

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compoſitions of ſalt, earth, water, *&c.* together with them to form oils, fats, and in fine all inflammable bodies, which only become ſuch by the great quantity of particles of fire lodged in them. What leads me to this ſentiment is, that upon decompounding theſe bodies, they turn intirely into ſalt, earth, water, and a fine ſubſtance, which paſſes thro' the cloſeſt veſſels, and maugre all the care of the beſt artiſt, ſpends itſelf in ſufficient quantity to produce a conſiderable diminution in the weight of what remains.

'Tis certain, that ſalt, earth and water, whether united together, or ſeparated, never become inflammable, but even uſually hinder, or retard the inflammability of bodies, which naturally have that property; it may even be aſſerted, that the effect of theſe principles in the compoſition of inflammable bodies, is only to ſtop and arreſt the matter of light or flame, which never riſes into the air under this form, except when the inflammable body having been firſt expoſed to the fire, that agent has broke the cells thereof, and given room for the incloſed matter to fly off.

'Tis the real matter therefore of fire or flame, which ſteals from the artiſt in the *analyſis* of inflammable bodies, all that remains of thoſe bodies after the decompoſition, being the materials whereof the cells were formed, in which this matter was retained; it will be eaſily allowed, that this matter, when free and left to itſelf, muſt pervade the cloſeſt veſſels, when we conſider that there is no veſſel but what the fire will readily penetrate, ſo as to heat a fluid contained in it; and as to the cauſe of inflammability, experience ſhewing us, that ſalt, earth, and water, in whatever circumſtance is found, never becomes inflammable;

mable; to what can we more probably attribute the effect abovementioned, than to the matter of fire, which, as already proved, forms the flame, and gives it all its properties.

Nor need we be ſurpriſed, that calcined metals, and all bodies in general, which have procured a ſtock of light by calcination, do not kindle by the fire as oils do; for to make a body kindle ſo as to be perceived, the luminous ſubſtance iſſuing continually from it, muſt be copious enough, and form a maſs ſufficiently firm to preſs the matter of light diffuſed thro' the air vigorouſly, and on all ſides, ſo that the particles of this matter ſtriking each other ſucceſſively, and according to the direct determination communicated to them, do hereby tranſmit the preſſions of the flame, to a diſtance greater or leſs; but when only little particles of luminous ſubſtance, are diffuſed from ſolid bodies, they preſently become ſo darkened by the air around them, as to be diſabled from making preſſions efficacious, and extended enough, to become ſenſible to the eye.

Upon the whole we conceive, that the matter of light lodged in inflammable bodies, expoſed to the fires, iſſues out every moment in much greater quantity, than the ſame matter lodged in calcined metals; whether it be that ſuch metals contain leſs of this matter than the oils, or whether having a cloſer texture of parts, they do not allow it ſo free egreſs; but at each effort of the agent which obliges them to let go, they let only ſmall parcels exhale, incable of ſenſibly affecting the eye.

This reaſoning perfectly agrees with a known fact, which is, that upon expoſing very inflammable bodies, as paper or ſtraw, to a tooſmall fire, they ſometimes conſume intirely without caſting any flame, by reaſon the external agent being too

too weak to expel a great quantity of the matter lodged in ſome bodies all at once, this whole matter flies off ſucceſſive in little inviſible portions, anſwerable to the force which procures them deliverance.

We might here take occaſion to account for ſeveral curious *phænomena*, wherein this ſyſtem of impriſoned fire perfectly quadrates, and which are even ſo naturally deducible herefrom, that each *phænomenon* ſeems a kind of proof of the truth thereof. How preciſely, for inſtance, does the matter of light ſeem to agree with the *phoſphori*, both natural and artificial; and to thoſe violent fermentations accompanied with flame, which the oyls uſed in ſuch experiments, are obliged to exhale, when penetrated by nitrous, or vitriolic acids? But were I to enter into a preciſe detail of all the experiments of this kind, and the particular circumſtances which accompany each, I ſhould go far beyond the bound preſcribed for this paper, and incroach on the ſubject of future ones.

I ſhall only here obſerve, that all *phoſphori* in general may be conſidered as a kind of ſpunges, filled with the matter of light, which is ſo feebly retained therein, as to need but very little external help to become capable of exhaling under a luminous form, and even of burning and ſetting on fire ſuch bodies as come in its way.

It follows from the whole, that if the ſun ſeem to be a kind of large receptacle, or fund of the matter of fire, we have an infinite number of petty receptacles in inflammable bodies, which ſeem to have been formed to ſupply the want of the ſun; in effect the preſence of fire being indiſpenſibly neceſſary to light and heat, and the great lumi-

luminary not being always in our hemiſphere, but retiring to a great diſtance from us in certain ſeaſons, or which amounts to the ſame, only determining a little quantity of the matter of light upon terreſtrial bodies, we find a happy ſubſtitute in the boſom of the earth, whereby to remove all the evils, into which the abſence or diſtance of the ſun would unavoidably throw us; I mean, a ſufficient quantity of the matter of light, to form a ſort of little ſuns, which warm and illuminate as well as the great one.

XIII. *On the evaporation of fluids in cold weather, with remarks on ſome effects of the froſts, by M.* Gauteron, *of the royal academy of* Montpellier; *tranſlated by Mr.* Chambers.

We uſually conſider the evaporation of fluids as an effect of the heat, or motion of the ambient air, and it will appear ſurprizing, that a quite oppoſite cauſe ſhould produce the ſame effect; and that a fluid ſhould loſe more of its parts in the ſevereſt froſt, than while the air is in a temperate ſtate. ——— Yet this is what I found in the great froſt of this winter.

I have even obſerved, that the greater the cold is, the greater has the evaporation been, and that ice itſelf loſt conſiderably, as much in proportion as the fluids which withſtood the froſt.

It began to freeze at *Montpellier*, on the 12th of *December*, 1708, the wind being at north, $\frac{1}{4}$ from north-eaſt, and the common thermometers ſtanding at 10°, and that of M. *Amontons's* at the 53°: At 6 o'clock this evening, I expoſed an ounce of common water, in a china cup, to be froze, which was done accordingly before

 morning,

morning, and weighing it at 8 a-clock the next day, I found that the water in freezing had loſt 24 grains of its weight. This diminution was very real, ſince, upon melting the ice, the water was found to have loſt 12 grains more, notwithſtanding all our precaution to prevent any ſecond evaporation. —— The ſame experiment repeated ſeveral days running, gave me much the ſame thing; with this difference, that the evaporation was much greater in a ſtormy night, or when the wind was ſtrong.

The thaw enſuing thereon, prevented the further proſecuting of my experiments; but it taking certainly again to froſt on the night between the 6th and 7th of *January*, I took occaſion to make the following ones.

For on the night between the 7th and 8th, I expoſed common water, brandy, oil of olives, oil of wallnuts, oil of turpentine, and mercury, an ounce of each, to the open air, the common thermometer ſtanding at the ſecond degree, and that of M. *Amontons*'s at 51°—6 lin. — The water was preſently froze, and in an hour loſt 6 grains; the oil of wallnuts loſt 8; and the brandy and oil of turpentine each of them 12, in the ſame ſpace of an hour, while the oil of olives and the mercury ſeemed rather to have gained than have loſt their weight. Next day the diminution of the frozen water was found 36 grains; that of the oil of wallnuts, which did not freeze, 40 grains; and thoſe of the brandy and oil of turpentine, which alſo withſtood the froſt, 54 each; the mercury and oil of olives remaining much in the ſame ſtate.

'Tis needleſs to note the evaporation produced day by day during the great cold; ſince, under equal circumſtances, the evaporation was nearly the

the ſame ; but a vehement cold and ſtrong winds, always made it greater than a leſs cold and calm weather.

'Tis obſervable, that the firmeſt ice is not exempt from evaporation in a ſevere cold ; for we find it looſe 36 grains from 8 in the morning to 1 in the afternoon, and 36 grains more from that time to 8 in the evening ; and the evaporation in the night was much at the ſame rate ; ſo that the ice loſt 100 grains in 24 hours, notwithſtanding its ſeeming firmneſs and ſolidity, and this at a time, which ſeemed more proper to bind, than to looſen the ſmalleſt of its parts.

The night between the 10th and 11th of *Jan.* proved the coldeſt that has been felt in this country, the liquor in the common thermometer ſunk intirely into the bowl ; and that of M. *Amontons*'s ſtood at 51°—1 lin. which is almoſt the extreme cold of the 8th climate ; in effect, the cold was felt very pinching in the warmeſt houſes, and few people could ſleep ſoundly how well ſoever they were covered. ——— This night the evaporation was very great, the common water loſt 48 grains, the oil of wallnuts 54, and the oil of turpentine and brandy 72.

This is a ſhort ſtate of what I obſerved on the evaporation of fluids in the great cold : my remarks upon froſt are,

Firſt, That the ſurface of freezing water appears wrinkled over, and that theſe wrinkles ſometimes form parallel lines, and ſometimes *radii*, which ſeem to go from the centre to the circumference, and upon freezing it in a cylindrical phial, I have found hollow tubes, formed around the cylinder from top to bottom, and ſeeming to go from the circumference to the centre.

Secondly, That water covered a-top and at the ſides with oil, froze about half an hour later than the water expoſed naked to the air, and in freezing formed a bunch of ice about an inch above the ſurface of the oil.

Thirdly, That oil of wallnuts preſerved water from a moderate froſt, which oil of olives had not been able to do.

Fourthly, That hot water, ready to boil, froze about half an hour later than cold.

Fifthly, That brandy, oil of wallnuts, and oil of turpentine did not freeze at all.

Sixthly, That tho' the sky was very clear during the froſt, yet the ſun appeared a little pale.

Seventhly, That the orange and olive trees loſt their leaves and branches, and moſt of them died to the very root; and what is more, the laurels, yews, grannate trees, fig-trees, jeſſimines, and ſome oaks themſelves underwent the ſame fate. The *Rhone* was froze 12 feet deep, and the pond *de Thau*, notwithſtanding its natural ſtorminеſs, and its communicating with the ſea, by a very ſhort and broad canal, was fixed from end to end, and ſeveral perſons went from the baths of *Balaruck* to *Sette* over the ice, a road unknown to our forefathers, and which perhaps will be ſo to our poſterity.

Eighthly, That the thaw on the 23d of *Jan.* as alſo that on the 26th of *Feb.* were followed with an epidemical catarrh, which ſcarce any body eſcaped.

All theſe effects muſt have aroſe from the ſame cauſe, *viz.* from the change in the air during the froſt — My ſentiment of this change is as follows.

The ſun's rays emitted in the winter, falling all obliquely on the ſurface of the earth, take up

more

more room thereon, and are leſs reflected upon themſelves; whence it follows, that the earth muſt be leſs heated in the winter time, and that the ætherial matter, moſt ſuſceptible of motion, will recede to that part where the ſun is moſt perpendicular to the earth, leaving ſuch ætherial matter as is leaſt diſpoſed for motion, on that part of the earth where it is winter.

Now the ætherial matter is commonly allowed the cauſe of the motion of fluids, and that the air of itſelf owes its motion and fluidity to the ſame: hence all fluids muſt remain in a ſtate of ſtiffneſs or condenſation, when this matter looſes part of its force; and hence the air itſelf muſt be denſer in winter, than in any other ſeaſon.

But we likewiſe find by ſeveral experiments, that the air contains a ſalt, which is ſuppoſed to be of a nature approaching that of nitre; now this, and the condenſation of the air being ſuppoſed, I ſay, that the molecules of this nitrous ſalt muſt be brought nearer, and conſequently their bulks enlarged upon a condenſation of the air; as on the contrary, they muſt be divided and further attenuated by the motion of that fluid, if the ſame thing befal all fluids, which have diſſolved any ſalt; that is, if the heat of the fluid keep the ſalt exactly divided, and the coolneſs of ice, or of a ſubterraneous place, give room for the particles of the diſſolved ſalt to gather together and cryſtallize, why muſt the air, which is capable of rarefaction and condenſation, be exempted from this general law?

And if the nitrous particles in the air be enlarged in a great cold, as cannot eaſily be denied, they muſt of conſequence have a leſs ſhare of velocity; but the product of their maſſes thus augmented by the velocity remaining, muſt ſtill give

them

them a greater quantity of motion.—— Nothing further is required to make the ſalt act more forcibly againſt the particles of the fluid; and this I apprehend the real cauſe of the great evaporation they undergo in cold weather.

Yet this aerial nitre cannot hinder a fluid from turning into ice, but on the contrary muſt be a means of promoting the ſame; for it is not the air or the nitre contained in it, but the ætherial matter that gives fluids their motion, and conſequently 'tis on a diminution of the force of this latter, that the loſs or diminution of the motion of the former depends. Now the ætherial matter, beſide its natural feebleneſs in the winter, muſt looſe a great part of its force, by acting againſt the condenſed air, which is further replete with large molecules of ſalt; and thus muſt neceſſarily be rendered feeble in a ſevere cold, and by no means in a condition for maintaining the motion of fluids; in a word, we may conſider the air in froſty weather, as that ice charged with ſalt, commonly uſed for the freezing of certain liquors in ſummer. Theſe liquors probably freeze by a diminution of the motion of the ætherial matter, which acting againſt the ice, and the ſalt mixed together, the air, with all its heat, cannot hinder the concretion.

It may perhaps be urged, that fluids contain particles of air, which, according to M. *Mariotte*'s obſervations, are in a ſtate of compreſſion 10 times greater than in the open atmoſphere; that the ſprings of the air thus compreſſed, unbend themſelves in the froſt by a diminution of the motion of the fluid; and that 'tis to the exploſion of theſe ſprings, that the evaporation of the particles of fluids in the froſt are owing.

Now

Now I allow, that fluids contain a great deal of air; that this air is more compreffed in the fluids than in the open air; that the froft gives occafion for its fprings to unbend themfelves; and that thefe fprings unbend with the more force, on account of the compreffion they are in, and what is more, that this unbending of the fprings of the air is the caufe of the lightnefs and rarefaction of ice, as well as of the bubbles and tubules mentioned in my obfervations; but I cannot allow, that the action of thefe fprings is the caufe of evaporation, when I confider that both the fluids which freeze, and fuch as withftand the froft, undergo an evaporation proportional to the tenuity of their parts, and that ice, feveral days old, loofes full as much as water juft beginning to freeze. In fluids which do not freeze, the unbendings of the fprings of air cannot be very confiderable; and in ice, formed many days, thofe fprings muft have had their full play, and now left incapable of any further action.

It has been obferved, that when the ice begins to form, its furface is full of wrinkles, which are fometimes difpofed in parallel lines, and fometimes after the manner of *radii*, under which furface is a multitude of little frozen particles, in form of needles, or rather of funnels, whofe fmall end is turned to the furface of the water. Thefe funnels are eafily perceived in a cylindrical phial, when the liquor contained in it is intirely froze.

Now this difpofition of the ice thus beginning to be formed, is favourable to the contained air's efcaping out upon the fpring's beginning to unbend, and feems at the fame time to prohibit the entrance of the external air, which might otherwife take its place. Thus the air which re-

 mains

mains in the freezing water, muſt dilate with the more freedom, as being no longer compreſſed by the external air; and hence probably ariſe the levity and rarefaction of ice, but not the evaporation of its particles.

It would be tedious to enter into an explication of all I have obſerved upon ice; beſides, that it may eaſily be deduced from the principals already laid down. From hence, for inſtance, it appears, that the particles of oil of olives are more ramoſe than thoſe of oil of wallnuts; and that 'tis owing to theſe branches, which lock the parts faſt together, that the aerial nitre is not able to carry them off; that the particles of wallnuts are more groſs, though leſs branchy, than thoſe of oil of olives; and that it ariſes hence, that the former is heavier, and dries quicker than the latter; and further, that the particles of the oil of wallnuts muſt be ſmoother, and more ſlippery, and only touch in a few points of their ſurface; whence it is, that the ætherial matter, with all its weakneſs, can eaſily move them, and hinder the oil from freezing; and hence it is, thoſe particles are not firm enough to reſiſt the impulſe of the aerial nitre, which carries them off; hence alſo appears that the tenuity of the particles of brandy and oil of turpentine, favours their fluidity and evaporation; as for the heavy and globular particles of mercury, it appears, that ſome more powerful agent, than the nitre of the air, is required to ſeparate them from their maſs.

Since the ætherial matter ſtill maintains the fluidity of oil of wallnuts, 'tis no wonder, that the water covered with it, ſhould withſtand the froſt; oil of wallnuts, on this occaſion, doing the office of a kind of filtre, and giving entrance to a quantity of this matter, ſufficient to maintain the

the fluidity of water; and if oil of olives likewiſe defend water a little while from the froſt, 'tis by reaſon this oil, which only condenſes by the cold, contains a little of the ætherial matter in its branches, by means whereof the water thus covered with oil of olives, is able to ſuſtain the cold longer, than if entirely deſtitute of that aſſiſtance; and if hot water freeze half an hour later than cold, 'tis by reaſon ſome time is ſpent in laying aſide the motion which the fire had given it; and as to the paleneſs of the ſun in a ſevere froſt, who does not perceive, that the condenſation of the air, and the groſſneſs of the nitrous particles contained in it, muſt reflect abundance of its rays, and prevent their penetrating to us. Laſtly, if a kind of gangreen appear on the frozen parts of trees and other plants, is not this owing to a corroſive ſalt, corrupting the texture thereof? The relation is ſo near between this gangreen, and that which befalls the parts of animals, that their cauſe muſt be near a-kin; coroſive humours burn the parts of animals, and the aerial nitre has the ſame effect on the parts of plants *penetrabile frigus adurit*.

I ſhall cloſe this memoir with ſome reflections upon the epidemical catarrh, which ſucceeded the thaws of the 23d of *Jan.* and the 26th of *Feb.* —— So many perſons were ſeized with it all at once, that it can be owing to nothing leſs than ſome general cauſe, which acted at the ſame time upon all men. This cauſe we find in the air reſpired after the thaw, whoſe nitre having been much divided, was now reſtored to its natural form: to explain myſelf,

The air conveyed into the lungs by the *trachea*, fills the veſicles, whereof that *viſcus* is compoſed; and tho' the blood do not enter into theſe vesicles, excepting a preternatural case; yet the blood in the pulmonary vein, being found more brisk and florid, than that in the artery, shews, that it has undergone a considerable change from the air in respiration. Hence, as the air has no immediate action upon the blood, we may suppose, that the texture of the vesicles of the lungs does the office of a kind of filtre, by separating the nitrous part of the air; and that 'tis this nitrous part to which the florid lively state of the blood in the pulmonary vein is owing, if now the nitrous particles in the air happen to be grosser than usual, as we have shewn must be the case in a severe cold, their proportion will be changed with respect to the filtre, which is to separate them; and hence only a small quantity will enter the blood, which, together with the external cold, will occasion that fluid to remain in a state of inactivity, during which the passages of perspiration being stopped, the blood must retain most of its serous and lymphatick part, which will remain inclosed in its sulphurous ones, and only to be extricated threfrom by a general liquefaction. This liquefaction of the humours must happen upon a thaw; for the nitre on this occasion dividing into little molecules, a great quantity of this salt must mix hastily with the blood, and animating it, excite a fermentation, which suffices to make an instant separation of a large quantity of lymph and serum, which being thrown upon all the glands of the body, produces a head-ach, nausea, stoppage of the nose, cough, crudity, and abundance of urine, weariness, and sometimes a little feverishness.

The catarrh above described is very different from what happens in a violent cold; in this latter, the humours circulate with difficulty; and by their thickening, occafion fome ferous parts to be feparated from them; whence the hoarfenefs and cough, which are frequently accompanied with an involuntary weeping, by reafon of the lachrymal points, which are ftopped by the thickening of the mucus in the nofe. ——

Accordingly the two catarrhs are to be treated after a very different manner; thofe from cold, are cured by remedies, which reftore the humours to their fluidity; and where there is a ftoppage of the head, the readieft remedy I know is, the perfume of amber, which doubtlefs acts by the quantity of volatile falt, and fulphur, contained therein; wine and brandy burnt with fugar, and tea, coffee, and chocolate, are proper for the fame reafon; feveral violent and very obftinate colds I alfo knew cured that winter with chicken broth, wherein an ounce of fnakes flefh dried, with a handful of creffes had been boiled about $\frac{1}{4}$ of an hour.

As to catarrhs caught in the thaw, care muft be taken to prevent the too great diffolution of the humours, by boiled emulfions, rice-milk, water-gruel, barley-water, and yolks of eggs, with fugar-candy, whey, and milk itfelf; narcotics, and phlebotomy, are proper in either kind of catarrh; and efpecially where the patient is harraffed with a cough, or any inflammation of the breaft is apprehended.

XIV. *The variation of the needle at* Nuremberg, *by M.* Wurtzelbaur.

M. *Wurtzelbaur* finds the variation of the needle at *Nuremberg*, to be near 11 degrees; and obferves, that it has not increafed fince the year

 1703,

1703, when he observed it also to be 11 degrees.

XV. *A comparison of the observation of the eclipse of the moon*, Sept. 29, 1708, *made at* Nuremberg, Genoa, *and* Marseilles, *by M.* Cassini *the son**.

h	′	″	
8	43	36	Beginning of the eclipse at *Nuremberg*.
8	33	49	Beginning of the eclipse at *Genoa*.
8	20	45	Beginning of the eclipse at *Marseilles*.
0	9	47	Difference of the meridians between *Genoa* and *Nuremberg*.
0	22	51	Difference of the meridians between *Marseilles* and *Nuremberg*.
11	6	34	End of the eclipse at *Nuremberg*.
10	57	21	At *Genoa*.
10	41	26	At *Marseilles*.
0	9	13	Difference of the meridians between *Genoa* and *Nuremberg*.
0	25	8	Difference of the meridians between *Marseilles* and *Nuremberg*.

XVI. *Reflections on the observations of the eclipse of the sun*, March 11, 1709, *made in different countries, by* M. Cassini *the son**.

	h	′	″
At *Montpellier* the end of the eclipse was observed exactly at	2	55	49
We find by the figure drawn up for the meridian of *Paris*, that it must have happened there at	2	49	30

* March 2, 1709. † April 17, 1709.

Which

	h	′	″
Which gives the difference of the meridians	0	6	19
At *Genoa* the beginning of the eclipſe was obſerved exactly at	0	59	52
It muſt have happened by the figure at	0	34	0
Which gives the difference of the meridians	0	25	52
At *Bolonia* the end was obſerved with ſome ambiguity at	3	34	35
It muſt have happened by the figure at	2	58	40
Which gives the difference of the meridians	0	35	55

A

TABLE

OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCX.

In the HISTORY.

In the MEMOIRS.

II. *Ob-*

XIII.

A N

AN
ABRIDGMENT
OF THE
PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1710.

I. *On the progressive motion of several species of shell-fishes.*

ALtho' animals in general have an indispensable occasion for the progressive motion, either to seek for food, or for the males and females to meet together; yet many of them seem incapable of it meerly by their figure: of this sort are several species of shell-fishes; and therefore M. *de Reaumur* has observed them with a great deal of care, for they might walk in secret; and an external action is often as difficult to discover, as the internal structure of a part.

The late M. *Poupart** had observed, that the river-muscles being laid upon the flat of their shells, thrust out at pleasure a part, which on account of its use, may be called a leg or an arm, that they made use of it to hollow the sand under them, and consequently to sink softly on one side, so as to be found at last upon the edge of their shell; after which, they advanced this arm as far as possible, and then rested upon its extremity to draw their shell to them, and so to trail themselves in a sort of groove which they themselves formed in the sand, and which sustained the shell

* Vol. II. p. 376, of this abridgment.

on both ſides. In looking at a muſcle, we ſhould not gueſs at this expedient, this mechanical reſource.

M. *de Reaumur* has ſeen a like motion in the ſea-muſcles, what may be called their leg or their arm, and which in its natural ſtate is 2 lines long, may come 2 inches out of the ſhell; and the animal having ſeized upon ſome fixed point with this arm ſo extended, contracts it afterwards, and conſequently advances by trailing.

By an almoſt ſimilar mechaniſm, which M. *de Reaumur* has been very minute in deſcribing, the *chama*, or *parr*, another ſort of ſhell-fiſh, walks upon the mud, or plunges into it. But he has obſerved, that if it plunges therein, it is no farther than is admitted by the length of two horns, or tubes, which it can puſh out of its ſhell, and with which it takes in, and throws out the water, which in all probability it ſtands in need of for its reſpiration. Theſe horns muſt always be able to communicate with the water that is above it, and thence it happens, that even when it does not make uſe of them, for they are not always in action, there is in the mud which covers it, one or two little holes of the diameter of its horns, which diſcovers it.

The length of theſe horns in the other ſhell-fiſhes, determines alſo the depth to which they ſink in the mud.

The *patella*, *lepas* or *limpet*, which is an univalve ſhell-fiſh, always faſtened to a ſtone upon which the lower circumference of the ſhell may be exactly applied, ſeems to have no other motion than the raiſing of this ſhell the height of a line, ſo that its body may have a circumference of this magnitude, uncovered and naked. As ſoon as one touches it, the ſhell falls and covers it. But yet M. *de Reaumur* has found in this animal, a progreſſive motion upon the ſtone to which it ſticks.

The ſea-nettle, which has the figure of a truncated cone, is in like manner always applied to a ſtone, by the greateſt baſe of this cone. Some circular muſcles form the plane of the two baſes, and ſome ſtrait muſcles go from one baſe to the other. The whole play of the progreſſive motion conſiſts in general in this, that one half of the muſcles both ſtrait and circular, on that ſide to which the animal would go, ſwells and extends itſelf, and conſequently occupies a ſmall part of a new place, whilſt the other half ſinks, and is drawn by that which advances, or puſhes it the ſame way. This motion is no more quick, nor more ſenſible than that of the hand of a clock.

There is another ſea-nettle, which faſtens itſelf to nothing, and is the moſt odd of all animals with regard to its figure; and is the moſt ſingular in the thinneſs of its conſiſtence, for it melts in one's hands. It would not be reckoned in the number of animals, if we did not ſee in it a motion of *ſyſtole* and *diaſtole*, the only ſign of life that it gives.

In the laſt place, the ſea-ſtar with its 304 legs to each of the 5 rays of which it is compoſed, goes never the faſter. Its 1520 legs give it no advantage over the muſcle, which has but one. What a prodigious variety is there in the works of nature! not only the great quickneſs of the motion, but even the extreme ſlowneſs is executed after different manners.

An explanation of the figures.

Plate IV, *fig*. 1. a ſea-muſcle opened after the natural manner.

L its leg.

Fig. 2. a ſea-muſcle gaping, and putting forth its leg.

Fig. 3. a *chama* or *purr* opened, to ſhew the parts ſerving for the progreſſive motion.

S the *vertex* of the ſhell.

M M two muſcles, which are cut thro'.

I the leg, placed in the middle of the ſhell, proceding from the *vertex*. Its whole extremity I is ſtrait and ſharp, it is only rounded over-againſt the 2 fleſhy tubes marked C C; whereas, on the other ſide it advances a little, and forms a ſort of blunted point marked P.

O O the inner aperture of the tubes C C.

Fig. 4. a *purr* embracing the mud with its leg R C O r.

Fig. 5. A *purr* prolonging the horns or tubes C C, to draw in the water.

S the *vertex*.

B B B the baſe.

S B the breadth.

L L the length.

Fig. 6. a ſort of ſhell-fiſh found on the coaſts of *Poitou*, *Aunis*, and *Saintonge*, and there called *palourde*. It is different from the *chama peloris*, and from the *pelorde* of the coaſts of *Provence*.

C C the two horns or tubes.

Fig. 7. a *palourde* opened.

O the interior aperture of that horn which is fartheſt from the *vertex*.

I the leg.

Fig. 8. a ſort of ſhell-fiſh found on the coaſts of *Poitou* and *Aunis*, where it is called *ſourdon*.

C C the horns or tubes.

Fig.

Fig. 9. a *ſourdon* opened.

I a part reſembling a leg.

P the foot.

T the heel.

Fig. 10. a *ſourdon*, with the leg, foot, and heel thruſt out for walking.

Fig. 11. a *tellina* opened ſo as ſhew the leg.

Fig. 12. a *tellina* with the leg thruſt forth, ready to open a way in the ſand.

S the *vertex*.

C C the horns or tubes.

Fig. 13. a *tellina* bending its leg to raiſe itſelf.

Fig. 14. another ſpecies of *tellina* opened.

I the leg.

C C the horns or tubes.

Fig. 15. a *tellina*, with its leg thruſt forth, ready to enter the ſand.

A A the bounds of growth, marked ſo diſtinctly on the ſhell, that they look like ſmall pieces ſtuck upon larger.

C C the horns or tubes.

I the leg.

Fig. 16. the ſhell of a *limpet* faſtened to a ſtone.

Fig. 17. the animal taken out of the ſhell.

A A A, *&c.* that part of the animal which is uncovered by the ſhell.

T the head.

C C two little horns bent towards it.

P a thick fleſhy part in the middle of the opening of the ſhell, which it makes uſe of for its progreſſive motion.

Fig. 18. a ſmall *whelk*, in which the organ of progreſſion, or leg, is like that of a ſnail.

E the leg.

C the lid with which it ſhuts its ſhell.

P

P the part which it puts upon its head, when it draws its leg into the ſhell.

Fig. 19. the *cancellus*, called in *Engliſh*, the *wrong heir*, or *Bernard the hermit*. It is a ſea-animal without any ſhell of its own, which lodges in the ſhells of *whelks*, and other turbinated ſhell-fiſhes.

DG its claws, like thoſe of crabs and lobſters.

Fig. 20. III three little bodies near its *thorax*, with which it faſtens itſelf to the ſhell.

A O is that part of the animal, which is covered only with a thin ſkin, the reſt having a ſofter ſhell than that of cray-fiſhes.

Fig. 21. a ſea-nettle.

A a part of the ſea-nettle, repreſented in this and the two following figures, reſembling the vent of a large beaſt, on which account theſe animals are called, by the common people in *France*, *culs de chevaux*, *&* *culs d'anes*.

B B the baſe which does not appear in this figure, becauſe the animal reſts upon it; but it may be ſeen diſtinctly in *fig*. 24.

Fig. 22. a ſea-nettle with all its horns extended.

Fig. 23. another ſea-nettle.

AIIFBD a ſpace where only the ſtrait canals appear.

ACIFRA a ſpace where only the circular canals appear.

IFTO a ſpace where the ſtrait canals partly appear, the circular canals being but partly ſwoln.

COTR a ſpace where the circular canals are ſwoln.

Fig. 24. a ſea-nettle reverſed, to ſhew its baſe.

Fig.

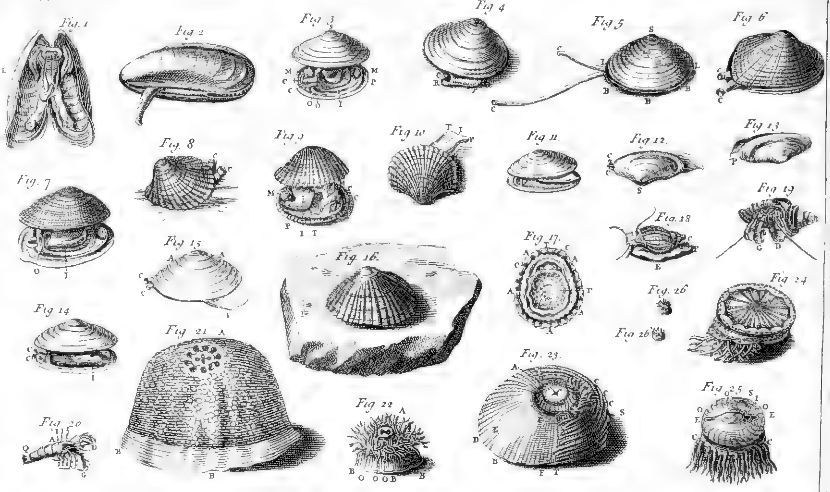
Fig. 1
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Fig 20
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Fig. 23.
Fig 24
Fig 25
Fig. 26
Fig 26

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Fig. 25. the mouth reverſed.

C C the circumference where the horns are faſtened.

O O O O the circumference of the mouth reverſed.

It is by this mouth, that the animal takes in its food, and excludes its young.

I a little ſort of inteſtine turned ſpirally.

Plate V. *fig.* 27. another ſort of ſea-nettle called ſea-blubber, or ſea-gelly.

D D the circumference or baſe.

C D the grand reſervoirs or canals.

The circular circumferences DDD, *&c.* EFEF, *&c.* receive the water only by the portion ED of the canals D; whereas the band CCCC, *&c.* EFEF, *&c.* the thickneſs of which increaſes gradually EFEF, *&c.* to CCCC, *&c.* receives the water from 16 canals marked CE and CF.

BBBB four columns, which divide the ſea-nettle as it were into 4 parts.

T a trunk, in which the 4 columns are united.

R R, *&c.* the trunk divided into 8 branches.

P P an appendage to one of theſe branches.

L a ſmall part of the canal left between the apertures of the columns.

Fig. 28. repreſents ſome of theſe parts more at large.

T the trunk of the canal.

R R, *&c.* the branches into which the trunk is divided.

O O, *&c.* the apertures of each of theſe branches.

Fig. 29. a ſea-ſtar.

R R one of its rays laid open.

B B two rows of tranſparent bodies like pearls.

Fig. 30. another ſea-ſtar reverſed, creeping under a ſtone.

A A in these two rays the ends of the legs appear.

S the mouth or sucker.

D D, *&c.* five teeth about the sucker.

Fig. 31. an end of a ray magnified.

C C C bundles of tubes.

R R R the skin.

II. *Of the glass-ware of* India.

M. *de la Hire* has been informed by a memoir that has been sent him from *Pondichery*, in the *Indies*, by F. *Tachard*, a missionary jesuit in 1709, that the glass-ware of *India*, which is not so fine as that of *China*, or *Japan*, is made of the gum of a tree of the colour of white amber, or karabé, which they melt in two quarts of linseed oil.

III. *Of a sort of acorn from* Coromandel.

M. *de la Mare*, a sea-officer, having brought from the *East-Indies*, *Brazil*, and *Peru* several sorts of drugs, gave them to M. *Sauveur*, who shewed them to the academy. M. *Geoffroy* took upon himself the examination of them. They were roots, seeds, woods, stones, *&c.* He compared these drugs, as he saw them himself, and what was said of them in the memoirs of M. *de la Mare*, with what was said of them by the authors, who have treated of these subjects, and by that he endeavoured to find out, if what he had before him was what those authors have described. We shall suppress the principal part of this work, altho' inquired into with a great deal of care, being only mere erudition, and we shall only

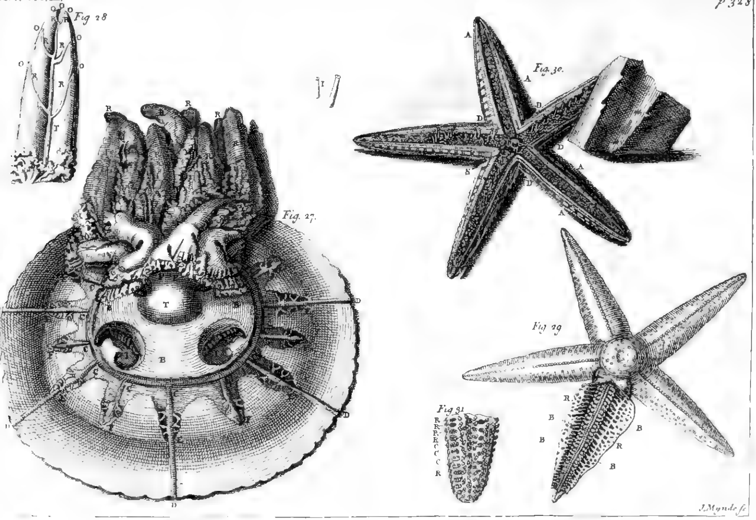
Fig. 28
Fig. 27.
Fig. 30.
Fig. 29
Fig. 31
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only take from it here, and in ſome other places what belongs to philoſophy.

There is on the coaſt of *Coromandel*, a tree pretty much like our oaks, which bears a ſort of acorn, out of which they draw an oil, like oil of olives. The *Malabars* make uſe of it in their food, for burning, and to colour their linnen. M. *de la Mare*, by their example eat of it in ſallads, and fiſh fried with it; and he had taught all the other officers of the coaſt to eat it, who found it to be very good.

IV. *Of the virtues of a nut called* Bicuiba.

The nuts called *bicuiba* burn like cloth ſoaked in pitch, and it is in burning that they extract the oil, as M. *de la Mare* has tried at M. *Boudin*'s, firſt phyſician to the late Dauphineſs. M. *Jean Verdois*, conſul of the *French* nation affirms, that he has cured many *cancers* with this oil, and that by eating one of theſe nuts, the colick is eaſed.

V. *Of a woman delivered of a child, when above* 80 *years of age.*

The late biſhop of *Sees* has affirmed, that a man in his dioceſe, whom he knew, being 94 years of age, had married a woman of 83, who in due time was delivered of a boy.

VI. *Of the fatal effect of ſome vapours in a baker's cellar.*

A baker of *Chartres* had put into his cellar, which was 36 ſteps deep, and well vaulted, 7 or 8 tubs of embers out of his oven. His ſon, a ſtrong and robuſt young man, going to carry ſome more embers, with a candle in his hand, the candle went out half way the ſtairs, he came up, lighted it and went down again. When he was at the bottom of the cellar, he cried out for help, after which they heard no more of him. His brother, as ſtrong as he, went down quickly, cried in the ſame manner, and then ceaſed. His wife went down after him, and a maid ſervant after her, and it was ſtill the ſame thing. So ſtrange an accident alarmed the whole neighbourhood; but no body was in haſte to go down into the cellar. There was only one neighbour, more zealous and bold, who not believing that theſe four perſons were dead, went down to give them his hand, and help them out. He cried out, and they ſaw no more of him. A paſſenger, a very vigorous man, aſked for a hook to draw ſome of the people out of the cellar, without going down to the bottom, he threw the hook and drew up the maid, who, upon coming to the air, gave a ſigh: they opened a vein, but ſhe did not bleed, and died upon the ſpot.

The next day a friend of the baker's out of the country, ſaid, that he would draw up all the bodies with a hook, but for fear that he ſhould find himſelf ill, without being able to get up again, he was let down into the cellar with a rope upon a wooden pully, and as ſoon as he ſhould cry out, they were to draw him up again: he very ſoon cried out; but as they drew him up, the rope unhappily broke, and he fell back again; they mended as faſt as they could this rope, which was broken pretty near the top of the cellar, but they could only bring him up dead. They opened him, his brains were in a manner dry, the *meninges* exceſſively ſtretched, the lungs marked with black ſpots, the bowels blown up, and as thick as one's arm, inflamed, and as red as blood; and what was the moſt particular, all the muſcles of the arms, thighs and legs, as it were, ſeparated from their parts.

The magiſtrates took cogniſance of this accident, for the publick intereſt, and forbid any one's going into the cellar, till they had taken the advice of phyſicians, ſurgeons, and even of maſons. It was concluded, that the embers, which the baker had put into his cellar, were not well extinguiſhed, that as there is a great deal of ſalt-petre in all the cellars of *Chartres*, the great heat had raiſed in that a very malignant vapour, which had cauſed ſo many fatal effects, that they ought to throw a great quantity of water into it, which would extinguiſh the fire, and make the nitrous vapours fall. This was executed, and at the end of ſome days they let down into the cellar a dog, tied to a plank, with a lighted candle. The dog did not die, and the candle did not go out, certain ſigns that all the danger was over. They took up the dead bodies, but ſo corrupted by the water, that they could not be examined; they were very much ſwoln, and one had his tongue out of his mouth, as if he had been ſtrangled. The academy had this hiſtory from M. *de la Hire*. There is one almoſt of the ſame ſort in the hiſtory of 1701 *.

VII. *Of a remarkable echo.*

M. *l'Abbé Teinturier*, the arch-deacon of *Verdun*, has ſent to M. *Caſſini* the ſon, the account of an echo, that he has ſeen 3 leagues from *Verdun*. It is formed by two thick towers, detach'd from the body of the houſe, and 26 toiſes diſtant

* Vol. I. pag 253 of this abridgment.

from one another. One has a lower apartment of free-ſtone vaulted; the other has only the porch ſo. Each has its ſtair-caſe. As all that belongs to echos may be called catoptricks of ſound, becauſe the ſound reflects itſelf according to the ſame laws, as the light does, we may look upon theſe two towers as two looking-glaſſes, placed over-againſt one another, which mutually ſending back the rays of a light object, multiply the image of it, altho' continually weakening it, and make it always appear farther diſtant. Thus when we are upon the line that joins the two towers, and pronounce a word with a pretty high voice, we hear it repeated 12 or 13 times, by equal intervals, and always weaker and weaker. If we go out of this line to a certain diſtance, we hear no more of this echo, from the ſame reaſon, that we ſhould ſee no more of the image, if we ſhould remove ourſelves too much from the ſpace which is between the two looking-glaſſes. If we are upon the line, which joins one of the towers to the body of the houſe, we only hear one repetition, becauſe the two echos do not any more play together with regard to thoſe that ſpeak, but one alone.

VIII. *Of figured ſtones.*

M. *John Schuchzer* being come to *Paris*, and having been preſent many times at the aſſemblies of the academy, of which he is one of the moſt learned, and uſeful correſpondents, read a *Latin* diſſertation, which he addreſſed to it, upon the figured ſtones, that he has obſerved in his journey into *Flanders* and *France.*

The quarries about *Paris* have at different depths of the beds, ſometimes pretty thick, different

ferent ſorts of ſhells, ſtrongly bound together either by earth or ſand. When theſe ſhells have preſerved their ſubſtance, or their natural conſiſtence, they do not yet merit the name of *figured ſtones*, that is only proper when they are petrified; but they deſerve it ſtill better, when after having ſerved for a mould to a ſubſtance yet fluid, which has intirely filled them, and afterwards grown hard, their ſubſtance has been abſolutely deſtroyed by time, and there remains only this petrified matter, which very exactly repreſents their interior figure. Then the whole that we ſee is, in reality, only a figured ſtone; and this probability is ſo ſtrong, that there is need only to prove, that ſome part of an animal has contributed to the formation of this ſtone. The perfect conformity of the figures is a demonſtration of it; to which M. *Scheuchzer* adds, that about theſe ſtones there is always in the quarries an empty ſpace, which is exactly that which the ſhells filled.

There may figured ſtones be found, whoſe moulds may be unknown to us at preſent. The ſhells, which have formed them, are not any more in our ſeas, or they have eſcaped us. The great quantity of ſtones, which certainly have been moulded in this manner, gives us a right to make this ſuppoſition. Perhaps even ſome moulds may have been loſt; that is, ſome ſpecies of ſhell may have periſhed; but to admit this thought, which is a little bold, we muſt perceive in a ſtone pretty ſenſible traces of this ſort of formation.

Thus we do not admit it at preſent in explaining a ſtone, which was thought to be found only in *Hungary* and *Tranſylvania*, but has been found by M. *Scheuchzer* in *Swiſſerland*, and in a ſtill greater

greater quantity in *Picardy* about *Noyon.* *Clusius* has called it *Numismale*, because of its figure; however, it does not so much resemble a medal, or piece of money, as a glass convex on both sides, but more elevated in the middle than the spherical curve requires. Its two convex halves easily separate, and sometimes are found naturally separated. Then we see in the stone turns made spirally, like those of a cord twisted about itself. These turns are fastened by a sort of little filaments, which extend themselves obliquely toward the circumference. The exterior surface of the stone is sometimes polished, but oftner set round with little points, whose different series are sorts of irregular flutings. The generation of these sorts of stones, if we could never suspect them to have been moulded, will perhaps reduce the philosophers to the hypothesis of seeds, ventured by the late M. *Tournefort* *.

To explain the shells petrified, and sometimes buried under ground at great depths, or those which by a long series of ages are consumed, after having left only the print of their figures, M. *Scheuchzer* has recourse to his hypothesis of the deluge already explained in the history of 1708 †, which he has in common with his brother upon these sorts of subjects. If what we have related after M. *Saulmon*, in the history of 1707 ‡, does not absolutely require this hypothesis, at least a considerable part of what is land now, must formerly have been sea.

We shall not here pass over in silence an idea, upon which, however, M. *Scheuchzer* has declared that he did not pretend to insist, and which he has only proposed as a sort of philosophical dream. If we make a great round bason half

* Vol. I. pag. 410. † Vol. III. pag. 77. ‡ Vol. III. p 6.

 full

full of water, turn pretty faſt round its centre, till at laſt the water has taken all the ſwiftneſs of the baſon, and ſtop it ſuddenly, the water will not ceaſe continuing to move, and even with ſo much force, that it would ſurmount the edge of the veſſel. Thus if God ſhould, at an inſtant, ſtop the turning of the earth about its axis, the waters of the ſea would ſpread themſelves with violence over the whole earth. This manner of explaining the deluge is not leſs ſimple than new; even when God exerts his extraordinary power, and ſuperſedes theſe laws which he has framed with ſo much ſimplicity, we may imagine, that the miracle is performed alſo with the greateſt ſimplicity poſſible.

IX. *Of M.* John James Scheuchzer's Herbarium Diluvianum.

M. *John James Scheuchzer*'s *Herbarium Diluvianum*, printed at *Zurick* in 1709, and ſent to the academy by the author, turns upon the ſame principle with the work which we have juſt mentioned, and with all thoſe of both theſe brothers, mentioned in the hiſtory of 1708 †. This extraordinary herbal is only compoſed of plants, which, from the time of the deluge, having been buried in ſoft ſubſtances, have left the print of their figure upon them, when they were afterwards become petrified. They are only ſimple figures without ſubſtance, but ſo perfect and ſo exact, even in the moſt minute particulars of what they repreſent, that it is impoſſible to miſtake them. Among a great number of plants, which are all of theſe countries, there is an *In-*

† Vol. III. pag. 77, 81, 82.

dian

dian one, the ſtone of which was found in *Saxony*, which agrees with an obſervation already made in the hiſtory of 1706 *. The ſtrange confuſion that the deluge muſt have cauſed upon the ſurface of the earth, renders the tranſportation of an *Indian* plant into *Germany* very poſſible. According to the manner in which the holy ſcripture explains it, we may equally place the beginning of the deluge, either in the ſpring or the autumn; but M. *Scheuchzer* removes this uncertainty by ſome of the plants of his herbal, and chiefly by an ear of barley. Their age is only that which they have here at the end of *May*. This is alſo confirmed by an inſect or two, of which we alſo know the life ſufficiently, and which are not older. Theſe are a new ſort of medals, whoſe dates are without compariſon more ancient, more important, and more ſure, than thoſe of all the *Greek* and *Roman* medals.

There are certain ſtones which repreſent upon their ſurface, not like thoſe of this herbal, a ſingle part of a plant, or a ſingle leaf, but ſhrubs and little forreſts very beautiful. Thoſe repreſent ſo much, that they repreſent nothing, and in effect on examining them ever ſo little, we ſee that theſe trees, or ſhrubs, do not repreſent any real plant. They are even ſometimes accompanied with little caſtles, or figures, which adorn the picture indeed, but render it unworthy of the herbal of the deluge. Theſe are true ſports of nature. M. *Scheuchzer* undertakes to explain the philoſophy of theſe ſports; that is, how certain juices which exuded from the pores of a ſtone, as faſt as it was formed, could ſpread themſelves between two of the leaves, or *ſtrata*, which com-[illegible] it, and trace there certain repreſentations

[illegible]ol II. pag. 357.

almoſt

almoſt regular, to which afterwards our imagination lends a little of what it wants. He has even rendered his explication ſenſible to the eyes, by an experiment quite like it of two ſlabs of poliſhed marble, which he rubs together, after having put ſome oil between them. It ſpreads there ſo as to form trunks and branches.

Among the remains of the deluge, M. *Scheuchzer* reckons a great trunk of a tree, which he knows to be laid upon the ſummit of mount *Stella*, the higheſt mountain of the *Alps*. M. *John Scheuchzer* has twice attempted to go and ſee it with his own eyes, altho' the moſt determined hunters have never been there but with fear; but the ſnows have been an invincible obſtacle. According to his eſtimate, this trunk is raiſed 4000 feet above the moſt elevated place of theſe mountains, where any trees naturally grow, for beyond a certain height there grow none. Who could have carried it thither? With what deſign? And what machine muſt they have uſed?

X. *Of the count* Marſigli's *philoſophical eſſay towards a hiſtory of the ſea.*

The count *Marſigli* has ſent to the academy a manuſcript work, intitled, *A Phyſical Eſſay on the Hiſtory of the Sea*, of which he has done it the honour of a dedication. He turned to the advantage of philoſophy a ſtay that he made on the coaſt of *Provence* and *Languedoc*, and took that opportunity of ſtudying the ſea particularly. The manner in which he engaged in it, is ſufficient to ſhew what the genius of obſervation is; and to give a model of it, he has formed a deſign as extenſive as the ſubject, he has embraced all the parts of it, and has undertaken to make by himſelf all the experiments which can have relation to it. If we had a ſufficient number of as good memoirs made by obſervers, who had been placed in different parts of the world, we ſhould at laſt have a natural hiſtory.

The work of count *Marſigli* is ſo conſiderable, that the extracts which the academy has cauſed to be made from it, by M. *Maraldi* and *Geoffroy*, were themſelves pretty large works. We ſhall here only give an idea of it incomparably ſhorter, and we ſhall be greatly aſſiſted by their labours. The hiſtory of the ſea is divided into 5 parts. The firſt treats of the diſpoſition of the bottom, or baſon of the ſea. The ſecond, of the nature of the water. The third, of its motions. The fourth, of the plants that grow there. The fifth, of the fiſhes. This laſt part is not finiſhed, and the academy have not yet ſeen any thing of it. The whole is accompanied with a great number of figures, made with a great deal of care.

To diſcover the nature and diſpoſition of the coaſts, he has made different ſmall voyages in barks, which are all contained between the cape of *Siſſe* near *Toulon*, and the cape *d'Agde* in *Languedoc*. He has made others at ſea, and ſometimes at 11 leagues diſtance, to examine the depth and nature of the bottom. He has found that the gulph of *Lyons* is cut aſunder by a ridge, hid under the water, that the part which is from the land to this ridge is not above 70 braces deep, and that the other which is towards the main is 150 in ſome places, and ſometimes ſo much that it cannot be ſounded. He calls it the abyſs[1]; he has ſearched what the diſpoſition of the ſoil was, that is to ſay, the order of different banks, or beds of earth, ſand-rocks, *&c.* not only in the coaſt, but alſo in the iſlands or neighbouring ſhelves.

This diſpoſition is found alike, ſo that all the iſlands are only fragments of the firm land, and probably the bottom of the ſea is a continuation of it. From hence we may conjecture, with count *Marſigli*, that the globe of the earth has a determinate organical ſtructure, which has not ſuffered great alterations, at leaſt in a conſiderable time.

He ſhews, that ſome beds of ſalt and bitumen are mixed among the beds of ſtone; and that upon the natural bottom of the ſea there is formed an *accidental* bottom, by the mixture of different matters, ſand, ſhells, mud, *&c.* which the glutinoſity of the ſea has ſtrongly united, and ſtuck together, and which are afterwards hardened ſometimes even till it petrifies. As theſe incruſtations are neceſſarily formed in *ſtrata*, there are ſome in which the fiſhers diſtinguiſh the annual augmentations; they have a ſurprizing variety of colours, which ſometimes penetrate even into the ſtony ſubſtance, but are oftener only ſuperficial, and diſſipate out of the water.

Some of the matters which form theſe incruſtations, have afforded by chymiſtry, principles ſo like to thoſe of marine plants, that we might ſuſpect them to be ſo; and much more, as they are ſometimes wholly fibrous. Such are the hard ſea moſſes, or *lichens* which faſten to the ſtone, and have almoſt the ſame hardneſs.

Count *Marſigli* found by a thermometer plunged in the water, that the degree of heat there is equal at different depths; that in the winter it is ſomething greater in this ſea, than in the air; and on the contrary in ſummer; but pretty often equal. Nevertheleſs count *Marſigli* has obſerved alſo, that many marine plants agree with thoſe of the land, in ſhooting in the ſpring oftener than in

 other

other ſeaſons. An accident prevented the experiments of the heat of the ſea from being continued ſo long as they ſhould have been.

According to him, the ſea-water, ſuppoſing it to be well choſen, is more clear and bright than any other water. As to its colour, it depends both upon its bottom, and the ſky; and ſo many other circumſtances hitherto leſs known, that all the experiments of count *Marſigli* leave him ſtill a great deal to deſire upon this ſubject.

It is more eaſy to determine the cauſes of its ſaltneſs and bitterneſs, for we may well obſerve the bitterneſs as different from the ſaltneſs. One is produced by the diſſolution of beds, or banks of ſalt, the other by the diſſolution of beds of bitumen.

Water is much more proper to diſſolve the ſalt, than the bitumen, which is an oily matter: and in the ſea-water the doſe of ſalt is much ſtronger than the bitumen. Count *Marſigli* having taken 23 ounces 2 drams of ciſtern-water, to make ſea-water of it, he put 6 drams of common ſalt into it, and only 48 grains of ſpirit of pit-coal; for pit-coal is bituminous; and beſides there are mines of it found in the mountains of *Provence*: and with this mixture he had an artificial ſea-water of the ſame taſte with the natural. Theſe 48 grains did not at all increaſe the weight of the water weighed by the areometer.

The ſmall quantity and lightneſs of this bituminous matter, are the cauſes, that the ſea-water diſtilled, ſo as to looſe its ſaltneſs, has not however loſt its bitterneſs, and a diſagreeable taſte, nor even as is pretended an unwholſom quality. The diſtillation which is naturally made by the ſun, and which is very different from that of an alem-

alembic, perfectly purges the ſea-water from its bitumen.

There are in the earth ſo many different matters, that the ſea waſhes, and of which it muſt raiſe ſome particles, that we may pretty juſtly believe, that bitumen is not the only principle which mixes with the ſalt.

By what has been juſt ſaid, we ſee that in 24 ounces of ſea-water, there are 6 drams of ſalt; or, which is the ſame thing, that it contains the 32d part of its weight of ſalt. But this is only true of the water taken at the ſurface of the ſea, that at the bottom is more ſalt, and has the 29th part of its weight of ſalt. The ſalteſt waters are alſo the heavieſt. Thoſe which are upon the ſurface of the ſea at the outlet of the *Rhone*, are lighter by $\frac{1}{303}$, than the waters farther diſtant, and equally ſuperficial; and theſe ſtill lighter than thoſe which are farther diſtant from land.

It is ſurpriſing that the water of the ſea, which does not want ſalt, has not diſſolved all that it can diſſolve. By count *Marſigli*'s experiments, a quantity of water which ought to contain 6 drams of it, diſſolved 4 $\frac{1}{2}$ more; and the artificial ſea-water 5. He conjectures, that the animals and plants of the ſea, conſume part of its ſalt; that another part of it diſſipates in the air; that the ſoft waters, which it receives not only from the rivers, but from the ſprings of its bottom alſo, freſhen it; but with all this he does not pretend, that the difficulty is intirely removed.

He has made 14 lb. of ſea-water paſs through 15 earthen pots, which he ſucceſſively filled with garden-mould and ſea-ſand. If they had been joined together, they would have made a caſcade of 75 inches long, and 5 broad. The 14 lb. of water having paſſed both through the ſand, and through

through the mould, were equally reduced to 5 lb. 2 ounces; but they were better freſhened by the ſand, and deprived of a greater part of their weight. If the caſcade of ſand had been twice as long, we might believe, that it would become almoſt inſipid. By this means the ſea-water might become freſh by filtering through the bowels of the earth, if at the end of a certain time the filters ſhould not fill with the ſalt which has been depoſited in them.

The ſalt of the ſuperficial waters is white, and that of the deep waters of a dark aſh-colour. The firſt is the only one in which there is found an acid, and is of a more biting ſaltneſs, and a much leſs ſenſible bitterneſs. From hence it comes, that at *Peccais*, in *Languedoc*, where they extract ſalt from deep well-waters, it muſt be left expoſed to the air for 3 years at leaſt, before it is vended. This time is neceſſary for it to loſe a bitterneſs which would be inſupportable. We ſhall ſuppreſs a great number of obſervations upon the marine ſalt, becauſe this ſubject is more known.

Count *Marſigli* has not had leiſure to content himſelf fully upon the fact of the bitumen contained in the ſea-water: however he believes it is this which produces the natural unctuouſneſs of this water, which even the diſtillation does not take away from it. The great quantity of glue which fixes upon the ſtones and plants, the union of ſo many heterogeneous bodies which glue together, the tartar which hardens in ſome places the bottom of the ſea, or incloſes ſeveral ſorts of matters, and chiefly the lithophytons, a marine plant. He has begun experiments at different times upon the tartariſations of the ſea, which could not be carried far enough.

He

He has obſerved, that pulſe boiled in ſea-water, came out of it more hard than when it was put in; that the fleſh of mutton becomes white, and more tender than in ſoft water, but very ſalt and bitter; that the bread made with ſea-water is ſalt, and may very well be eaten while it is new; but when it is ſtale, it acquires an exceſſive bitterneſs.

The ſea has three ſorts of motions, the flux and reflux, the currents, and the undulations. We know that the *Mediterranean* has no flux or reflux, at leaſt univerſally; and in effect, according to the common ſyſtem, it muſt not have any, ſince it is not under the courſe of the moon. However, as an almoſt inſenſible flux and reflux might eaſily eſcape the obſervations which are commonly made, count *Marſigli* has made new ones, which this motion could not have eſcaped; and it was not at all perceived in the places where the obſervations were made.

Count *Marſigli* has not diſcovered any rule in the currents, altho' he has not ſpared his voyages, nor his trouble. He has not been able to verify what is commonly ſaid of this famous current, which coaſts the whole *Mediterranean*, as if it was formed by the entrance of the waters of the ocean, and by their return. But he believes he has diſcovered ſomething very ſingular. During the ſummer, and in the time of the coral fiſhing, they perceive at the ſide of the abyſs, a current which ſeems to have a relation to the motion of the ſun upon the horizon, but ſo that it is always oppoſite to it. When the ſun is in the eaſtern part of its diurnal courſe, that is to ſay, from its riſing till noon, the current goes to the weſt; and at noon it turns to the north, and afterwards to the eaſt. They have not obſerved if it goes to the ſouth at mid-

midnight; this would agree with the reſt, and even appears neceſſary.

As to the undulation, it is ſufficient to know the exceſſes of it. Count *Marſigli* has obſerved between *Maguelone* and *Peyrole*, that in a great tempeſt the waves roſe 7 feet above the common level of the ſea. At the mountainous ſhores, ſuch as thoſe of *Provence*, a furious ſouth-weſt wind raiſes the water only 5 feet, but the percuſſion that it makes againſt the rocks, drives it ſometimes to 8. This is not comparable to the poetic tempeſts.

XI. *Of a* tænia *found in a tench.*

M. *Geoffroy*, junior, ſhewed a *tænia* found in a very ſound and fat tench, like to thoſe which are found in man, only it was not divided by rings. It had only ſtripes, or folds, perpendicular to its length, according to which another great ſtripe went from the head to the tail, dividing it into equal halves. It was intire, and 2 feet ½ long. We do not know that there has been hitherto any *tænia* found in fiſhes.

XII. *The diſcovery of an extraordinary ſort of inſect.*

We muſt be ſurpriſed to ſee that a little body pretty exactly oval, and whoſe great diameter, which is of above a line, is to the ſmall as 3 to 2; which has a very poliſhed ſurface of the colour of roaſted coffee, with a ſmall pearl-coloured band in the middle; and which, with theſe appearances, would hardly be taken for an animal, and at moſt but for an egg, ſhould however leap in a garden, raiſing itſelf ½ an inch, and ſometimes

leap-

leaping as high as 2. When we would have it leap, we need only expoſe it to the ſun, or hold it in the hand when it is hot. M. *Carré*, to whom we owe this obſervation, opened the bag of one of theſe little bodies, it is thick and ſolid in proportion to its bigneſs, and it had need be ſo to bear their leaps, and it incloſes a very white little worm, of which the back is cut with tranſverſe and parallel rings, and the belly very flat, and without feet. We perceive two little black points on the ſide of the head. As the figure of its belly hinders it from entirely filling the bag, it has room to make a leap there by gathering up its body, and afterwards opening it haſtily. It is thus that it raiſes up its houſe in the air. It muſt be very vigorous, for this houſe is a very great weight in proportion to it; and yet it raiſes it very high, and carries it a great way, and that very often. M. *Carré* kept one for two months in a box, without perceiving any alteration in it. This little animal is a riddle pretty difficult to explain. How does it nouriſh itſelf in this bag ſo well cloſed? How does it multiply in this priſon? For, although it ſhould multiply in the manner that muſcles do*, how ſhould its eggs get out?

XIII. *On the pond muſcles.*

We know well enough, at leaſt to a certain point, the animals that are moſt expoſed to our eyes, and with which we have the greateſt commerce. But there is an infinite number of others, which the little need that we have of them, the difficulty of obſerving them, a certain contempt which we have for them on account of their little-

* See the following article.

neſs, or their figure makes us neglect them, or abſolutely deprive us of them. Theſe are chiefly inſects, and ſhells.

Who would imagine that there is an animal which receives its nouriſhment, and reſpires only at the *anus*, which has neither veins nor arteries, and has no circulation in it? We need not mention its being a hermaphrodite, for that is a wonder at preſent too common; but it differs from all the other hermaphrodites hitherto known, in its multiplying independently of another animal of its own *ſpecies*, and is itſelf alone both father and mother of what it produces. Here is a quite new idea of an animal; it is the pond muſcle, the ſtructure of which, M. *Mery* has diſcovered, notwithſtanding its ſhape is ſo odd, and diſcourageing, on account of its exceſſive ſingularity.

What we may call the head of the muſcle, tho' we can find neither eyes, nor ears, nor tongue in it, but only an aperture which may be called its mouth, is an immoveable part, faſtened to one of the ſhells, in ſuch a manner, that it cannot go out to ſeek for nouriſhment, but nouriſhment muſt come to it. This nouriſhment is nothing but water, which, when the ſhells open, enters into the *anus* of the muſcle, which opens at the ſame time, and paſſes thence into certain reſervoirs or canals contained between the interior ſurface of the ſhell, and the exterior ſurface of the animal, and at laſt goes into the mouth, when compelled by a certain motion.

At the bottom of the mouth are two canals to receive the water. One throws ſeveral branches into the body of the muſcle, one of which terminates in the heart. The other is a ſort of inteſtine, which firſt paſſes through the

the brain, then makes ſeveral circumvolutions in the liver, and at parting from thence, traverſes the heart in a right line, and ends in the *anus*.

This brain and liver are ſuch no otherwiſe than as we pleaſe to call them ſo, but the heart deſerves that name a little better. It has a ventricle, and 2 auricles; and the alternate motions of *ſyſtole* and *diaſtole* in the ventricle and auricles; but it has neither veins, nor arteries: the water brought to it by its canal, enters from the ventricle into the auricles, and returns from the auricles into the ventricle, and makes a ſlight repreſentation of circulation, without any apparent effect; for being once arrived at this heart, it has no way to get out again. What muſt become then of the quantity there collected? Probably there is no collection made, becauſe the animal does not make the water flow continually thro' the mouth into the heart; and when a certain quantity has entered, the contractions of the heart ſqueeze it thro' the pores, and drive it into the neighbouring parts, which abſorb it, and are thereby nouriſhed.

The canal which M. *Mery* calls the inteſtine, and which, as well as the other, receives the water immediately from the mouth, does not ſeem fit to carry the nouriſhment to the parts, becauſe it has no branches to diſtribute it. However it contains at its beginning and end, two different ſubſtances; the firſt of which may be water digeſted, that is, the nutritious juices drawn from it; and the others may be the excrement.

The muſcle cannot breath, till it is raiſed upon the ſurface of the water, and it raiſes itſelf like other fiſhes, by dilating the cavity in which it contains the air. Then it is its *anus* too that receives the air from without, and carries it into the lungs, for is is generally ſunk at the bottom of the water.

It has ovaries, and ſeminal veſicles. Theſe two ſorts of organs are equally compoſed of tubes ranged by the ſide of each other, all ſhut at the ſame end, and opened at the oppoſite end. We do not diſtinguiſh theſe parts by their ſtructure, which is all alike to the eye, but the difference of their contents, and ſo much the more as the ovaries are always full of eggs in winter, and empty in ſummer, and as the veſicles are in all ſeaſons in like manner, but little filled with their milt, which conſequently ſeems to flow out continually. All the tubes diſcharge themſelves into the *anus*, and M. *Mery* imagines, that when the eggs are depoſited in their ſeaſon, they cannot fail of meeting with the milt or ſeed which fertilizes them. The animal therefore has no need of another to aſſiſt in its generation.

M. *Mery* does not agree with the late M. *Poupart* concerning the progreſſive motion of pond muſcles*. He apprehends, that their whole belly, which comes two inches out of their ſhells whenſoever they will, in the form of a keel of a ſhip, creeps upon the mud, juſt as the belly of a ſerpent does upon the ground. He deſcribes the muſcles, which, by their alternate contractions, make the whole play of this mechaniſm.

He is alſo of opinion, that the ſhell of the muſcle is not formed as M. *de Reaumur* has found the ſnail-ſhell to be formed†. Here the firſt circumvolutions are no larger in a great old ſnail, which proves that the ſhell is not a member of the animal, and that it is formed by a ſucceſſive addition of foreign parts; but ſome bands that

* See p. 321 of this volume. † See p. 250 of this volume.

we

we perceive on a muſcle ſhell, are largeſt in the biggeſt muſcles. Beſides the muſcle has 8 tendons faſtened to the inner ſurface of its ſhell; if the ſhells did not grow in the ſame manner as the fleſh, theſe which are faſtened at firſt in certain places of the growing muſcle, muſt continally change their faſtening to the laſt growth of the animal; and how could that be poſſible? The difficulty is conſiderable, but perhaps it is no more than a difficulty.

AN

AN
ABRIDGMENT
OF THE
PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1710.

I. *Experiments upon the elasticity of the air, by M.* Carré * ; *translated by Mr.* Chambers.

M. *Parent* gives us some experiments in the history of the academy for the year 1708, whereby he pretends to prove, that the air has no spring; but the point seems of too much importance to be given up, either upon M. *Parent*'s experiments, or his reasonings without some further examination; for it can never be too much considered, how liable we are to fall into errors, in drawing conclusions from one or two experiments, which may have succeeded agreeably to our opinion; especially when they go counter to an established doctrine, warranted by a multitude of experiments.

My intention therefore is to repeat M. *Parent*'s experiments, together with some others tending to the same matter, in order to which it may be necessary to transcribe the account thereof, given by M. *Fontenelle*. —— "A very extraordinary and surprising experiment, agrees with "or rather proves this sentiment. M. *Parent* "took several round glass phials about an inch in "diameter, and having long narrow necks, from "8 to 10 inches long, and a line wide; in each

* July 1709.

"of

" of these, he put a little quantity of a different " liquor as water, wine, spirit of wine, oil of " tartar, petrol and mercury; then putting their " necks thro' holes made in the receiver of an " air-pump, he exhausted the air, after which " melting that part of the neck, which was on " the outside, with a lamp, and twisting it about " the weight of the ambient air quickly sealed it " hermetically; so that there could be no " doubt but the phials were all well emptied of " air. At the same time there were other like " phials, sealed after the same manner, but full " of air, both the one and the other were laid " upon burning coals, whereupon those full of air, " by the great augmentation which the heat must " have occasioned in the strength of the spring, " should have burst with great noise; whereas, " in reality, they only melted gently through " the aperture; and on the contrary, those which " contained no air, but only a little liquor, made " all a great detonation, and burst in pieces. " Now, what becomes of the spring of the air " in this experiment? The ætherial matter car- " ried by the fire into the former phials, could " not make so great an effort against their inter- " nal parietes, by means of such subtile and deli- " cate particles, as those of air are, as by means " of more massive particles of the other liquors.

" Hereby we can easily explain how moisture " may produce those extraordinary effects, com- " monly attributed to the spring of the air, nor " need we any longer be in pain to understand " how such a spring should act in great rarefacti- " ons, where the particles of air do not seem to " touch, or bear upon one another; but this " perhaps would be to extend our consequences " further, than as yet may be allowed of. There

" is

" is a certain maturity required in phyfical truths, " which time alone can give them."

Here follow my experiments. ——— I procured 4 little glafs phials to be made with long necks, like thofe ufed by M. *Parent*, and prepared after the fame manner. The firft was full of common air; the 2d exhaufted of its air; the 3d full of air, with a little common water; and the 4th empty of air, but containing a little quantity of water. I fealed them all hermetically, and laying one after another upon the burning coals, the confequence was, that the phial, which contained nothing but common air, and which remained fome time without fhewing any effect, as being fomewhat thicker than the reft, opened at a place where it was fomewhat ftretched before, and produced a kind of hiffing by the air iffuing from it, without any great violence. The fecond had pretty much the fame effect, but the hiffing was fomewhat more confiderable, the part of the phial moft heated having ftretched fomewhat further, and yielded more quickly. The third made a violent detonation, and burft into very little pieces in a very fhort time. The fourth likewife burft with fome noife, and very quickly, tho' only a very fmall hole was made in it.

After this, I made 4 other little phials like the former. The firft, which was full of air, remained on the coals a confiderable time, ere it produced its effect; but it ftretched till at length it burft, with a confiderable noife, and difcovered a large aperture.

The fecond, which was likewife full of air, produced much the fame effect, but with lefs noife, the part at which it opened was ftretched more, and the hole fmaller.

The third and fourth, which were emptied of air, funk inwards without burfting; and efpecially

cially the fourth, in ſuch manner, as that half the convexity which touched the coals became cloſed exactly to the other half, and only made a hollow hemiſphere. The ſame ſhould always happen in this experiment, ſince the external air, though much dilated by the heat, muſt preſs more ſtrongly than the thin air included can poſſibly reſiſt, and conſequently the part moſt heated muſt be driven inwards; and if the ſame did not hold in the firſt experiment, 'twas probable, by reaſon there was air, or ſome other matter enough left in the phial to make it burſt.

Not being yet fully ſatisfied with theſe experiments, I made 15 other little phials like the former; an account whereof, and of the effects they yielded in the fire, follows.

The firſt, which was full of common air, being laid on the coals burſt in pieces in a very ſhort time, and with a little noiſe, which had not been found in any ſimilar experiment before.

The ſecond, which was emptied of its air, melted without burſting, and turned into a hollow hemiſphere, as above-mentioned.

The third, which was full of air with a little water, burſt quickly with a great noiſe.

The fourth, which was void of air, but had a little water, burſt in a ſhort time, with a noiſe ſomewhat greater than the former.

The fifth, which was full of water, remained but a little time on the coals ere it burſt, and threw them all around with a very great noiſe.

The ſixth, being full of water, exhauſted of air, its neck broke off, and it became a kind of eolipile, which continued a conſiderable time; and tho' the fire were very vehement, the phial ſuffered no change.

The ſeventh, being exhauſted of air, had a little coloured ſpirit of wine in it, this burſt almoſt as ſoon as laid on the coals with conſiderable noiſe.

The eighth, which was full of air, with a little ſea-ſalt in powder therein, melted, and yielded a little hole with ſome noiſe.

The ninth, being full of air, with a little ſalt-petre, made a ſmall hole in a very ſhort time, with a little noiſe.

The tenth, which was full of air, with a little urine, burſt in a ſhort time, with a conſiderable noiſe.

The eleventh, having no air, but a little ſalt water in it, burſt in a ſhort time, with a great noiſe.

The twelfth, having no air, but a little *aurum fulminans*, burſt as ſoon as laid on the coals, with a little noiſe.

The thirteenth, having no air, but a little ſulphur, melted and ſunk inwards, without burſting, the ſulphur alſo melted, and roſe to the top of the neck of the phial.

The fourteenth, being full of air, together with a little lamp-oil, remained a conſiderable time on the coal, but burſt at length with a conſiderable noiſe.

The fifteenth, was exhauſted of air, but had a little drop of mercury about a line in diameter therein, this remained 3 minutes on the coals without undergoing any change, and when it had been cooled, was laid on the fire again for 7 or 8 minutes without any effect, the mercury ſtill keeping to the top of the neck, only a little flaw was perceived in it.

It appears therefore, that all theſe experiments, inſtead of deſtroying the ſpring of the air, tend rather to confirm it; but it likewiſe appears, that neither the ſpring nor the dilatation of the included air, are the immediate cauſe of the noiſe and fracture of the glaſſes; ſince ſome of the phials, which were full of air, burſt without making any noiſe; the reaſon whereof may be, that the ſtrength of the air's ſpring, as well as that of other bodies, conſiſting only in the unbending of its parts, and acting equally every way, and this ſucceſſively in proportion to the action of the ſubtil matter, in its pores. This power diſtributing itſelf thro' all the parts of the phial it is contained in, that moſt heated coming at length to melt, yields and gives the air paſſage, which accordly iſſues out much after the manner as out of an eolipile; for that it does not dilate ſuddenly enough to burſt the ſides of the phial; but when the air is mixed with other particles of matter ſuſceptible of a great motion, and a quick and ſudden dilatation, it then produces the noiſe above-mentioned, and ſhatters the veſſel to pieces. We do not well conceive the mechaniſm, whereby theſe little particles of matter make this havock, and it muſt be confeſſed, the ſmalleſt experiments are often ſufficient to perplex a naturaliſt, who owns no other power, or virtue in bodies, but what ariſes from the motion and figure of their parts.

Not foreign to this purpoſe, are two other experiments, which prove the ſurprizing force of the dilatation of air, which thoſe who deal in ſuch experiments, will do well to obſerve, for fear of taking harm. —— An eolipile, being placed on the coals, and the fire raiſed to a conſiderable pitch, it flew from off the trevit againſt the foot of a table, a yard off, with force ſufficient to batter it, and continued whirling for ſome time after.

 The

The ſecond experiment was made in the academy *Del Cimento*, where a glaſs tube was taken about a foot and half long, whoſe extremities terminated in two globules of equal capacity; one whereof was open, as if the tube had been continued thro' it, then a quantity of brandy was poured into the tube, ſufficient to fill the lower globule and half the tube; after which the aperture of the upper globule was ſealed hermetically, the whole being plunged in a veſſel full of oil, which was made boiling hot, by continually blowing on the fire, the brandy roſe into the upper globule, and burſt the whole with ſo much violence, that uſing another time a copper veſſel, in lieu of a glaſs one, its bottom was broke out; and another time when an iron veſſel was made uſe of, near the thickneſs of a crown-piece, it burſt in like manner, and carried with it a ſplinter broke off from the pavement.

II. *Obſervations of the quantity of water, which fell at the obſervatory during the year* 1709, *with the ſtate of the thermometer and barometer, by M.* de la Hire *.

The quantity of water which fell, either in rain or melted ſnow was in.

	Lin.			*Lin.*	
Jan.	22	$\frac{3}{8}$	July	18	$\frac{2}{8}$
Feb.	13	$\frac{7}{8}$	Aug.	10	$\frac{7}{8}$
March	20	$\frac{2}{8}$	Sept.	29	$\frac{2}{8}$
April	37	$\frac{6}{8}$	Oct.	17	$\frac{5}{8}$
May	32		Novem.	1	$\frac{5}{8}$
June	45	$\frac{4}{8}$	Dec.	11	$\frac{6}{8}$

The ſum of the water of the whole year 1709, is 261 lines $\frac{1}{8}$, or 21 inches, 9 lines $\frac{1}{8}$, which is

* Jan. 8, 1710.

a

a little more than the mean years, which we have determined to be 19 inches.

We ſee by theſe obſervations, that the 3 months of *April*, *May*, and *June* have afforded almoſt as much water as the other 9, and it is what generally happens in *June*, *July*, and *Auguſt*; and this is the reaſon that the ſummer corn, which is ſown very late, has yielded a great deal. The great quantity of ſnow, which fell during the winter, has perhaps contributed to the fertility of the land; and if the wheat and rye had not been frozen in the root, this year would have been very plentiful.

The thermometer which I uſe for meaſuring the heat and cold, is the ſame which I have preſerved for about 40 years; but as it has been placed at different expoſures to the heavens, except for the laſt 15 years, we cannot make a very exact compariſon of the firſt obſervations with the laſt. However all theſe obſervations being always made at the day-break when the air is the coldeſt, we may conclude by them pretty exactly all that we can know by the means of this inſtrument. I ſhall only obſerve, that the judgment, which we commonly make of the cold, depends upon many particular circumſtances, as the wind, the humidity of the air, the heat or cold of the preceding days, the expoſure of the place where one is, and the conſtitution of bodies, which may conſiderably alter it; therefore it will be always more ſure to refer to the thermometer.

The cold, at the beginning of this year, was exceſſive, with a great deal of ſnow; for my thermometer fell to 5 parts, the 13th and 14th of *January*; and the following days being a little riſen, it returned to 6 parts the 20th, and the 21ſt to 5 $\frac{3}{4}$, but afterwards the cold diminiſhed gradually,

gradually. This great cold was very ſenſible, for the 4th of this month this thermometer was at 42 parts, which is very near a mean ſtate, which I have determined to 48; the 6th, it came to 30; the 7th, to 22; the 10th, to 9; and at laſt, the 13th, to 5. It was without doubt this ſudden change which appeared ſo extraordinary, and what is ſtill more ſurprizing is, that this great cold came without any conſiderable wind, or it had only a gentle one towards the ſouth, and then when the wind increaſed, and turned towards the N. the cold leſſened. This cold S. wind muſt ſhew us what really happened in the countries to the ſouth of us, where the ſea was frozen in ſome parts of the coaſt of *Provence*, and where the greateſt part of the fruit-trees, died as well as in this country.

I had never obſerved this thermometer to fall ſo low as this year. I only find in my regiſters, that the 6th of *February*, 1695, the thermometer was fallen to 7 parts in the place where it is at preſent; and the cold of that year, which had begun in 1694, was looked upon as one of the greateſt that had been for a great while, but we ſee that it is not at all to compare to that of this year. I have alſo obſerved ſometimes this thermometer at 13 parts, but very ſeldom.

The winter of this year laſted a great while; for the 13th of *March* it froze again very hard, the thermometer being at 24 parts, and the froſt beginning when it is at 32.

We find in *Mezeray*'s Hiſt. of *France*, that the winter of the year 1608 was very long and very ſevere, and that the greateſt part of the young trees were frozen: however that year was very plentiful, altho' they call it the year of the great froſt; but by the compariſon of the plenty

and

and of the loſs of trees, the laſt winter muſt have ſurpaſſed it.

The thermometer was at the higheſt at 63 parts the 11th of *Auguſt*, half an hour after 4 in the morning, and towards 3 in the afternoon at 75 parts. In the mean ſtate, it is at 48 degrees at the bottom of the caves of the obſervatory. The heat of this year has been much leſs than that of 1707, when the thermometer roſe to almoſt 70 parts, *July* 21, in the morning, and in the afternoon to 82, which is the higheſt it has been in this country, without being expoſed to the ſun.

To compare the obſervations of my thermometer with thoſe that ſhould be made with M. *Amontons*'s, of which he has had a great many diſtributed in ſeveral places, I have placed one, which he has made with a great deal of care, next to that which I commonly uſe; but as it had ſerved for ſome particular obſervations, I had not put it cloſe to mine till laſt *May*. We know that all M. *Amontons*'s thermometers have their 54th degree, or 54 inches, which marks the temperature of the air of the caves of the obſervatory, as in mine the 48th does. I obſerved then, that when M. *Amontons*'s thermometer was at 55 inches, 8 lines, mine was at 63 parts; ſo that 15 parts of mine anſwer to 20 lines of M. *Amontons*'s. But when mine marked 28 parts laſt *December*, M. *Amontons*'s marked 51 inches, 6 lin. which gives in mine 20 parts below the mean ſtate, and in that of M. *Amontons*'s 30 parts, which is a very different proportion from the firſt, and might be cauſed by the inequality of the inſide of the tubes; and as that of M. *Amontons*'s is very ſmall, and mine middling, I ſhould believe that the inequality might be greater in

M.

M. *Amontons*, than in mine. However, we may know by this, that we can have nothing very exact in the comparison of thermometers in different countries, and for the same time, unless the thermometers have been rectified by one another in all sorts of degrees of heat and cold, and I believe it will not be possible to find two equal; that is, of which equal degrees in the divisions answer to equal degrees of heat or cold.

As for my barometer, it is always placed at the top of the great hall of the observatory; I found it at the highest at 28 inches, 3 lines $\frac{1}{2}$ *Jan.* 19, with a calm and serene sky, which was about the time of the greatest cold; and the 31st of *December*, it was at 28 inches, 3 lines $\frac{1}{6}$, with a very thick fog and calm. It was also several times beyond 28 inches, with different winds, partaking rather of the N. than of the S. and always without rain. I observed this barometer at the lowest at 26 inches, 7 lines $\frac{1}{2}$, with a strong south wind, and moderate rain, *Dec.* 16. The difference between the greatest and least heights of the barometer, was therefore 1 inch, 8 lines, which is a little more than the mean difference that is observed here, and is 1 inch, 6 lines. This instrument was pretty exact in foretelling rain and fair weather, according to the common notion.

I observed the declination of the loadstone with the same needle of 8 inches long, and in the same place where I used, and as I have shewed in the memoirs of the preceding years. The 24th of *Dec.* last, I found this declination 10° 30′ toward the W. from whence we know that this declination increases almost the same quantity each year.

III.

III. *A comparison of the observations which we made here at the observatory on the rain and winds, with those which M.* le Marquis de Pontbriand *made at his castle near* S. Malo, *during the year* 1709, *by M.* de la Hire *.

It is for some years, that M. *du Torar* has communicated to us the observations, that M. *le Marquis de Pontbriand* makes at his castle, in the same manner that I make them here, upon the rain. He found, that there fell in melted snow and water in the month of

	Lines.		*Lines.*
Jan.	33 ¼	July	18 ¼
Feb.	17 ½	Aug.	5 ¼
March	30 ¼	Sept.	5
April	30 ½	Octob.	14
May	26 ¼	Nov.	3 ¾
June	23 ¾	Decem.	17 ¼

and during the whole year 225 lines, or 18 inches, 9 lines.

This quantity of water is less than what we found here, which was 21 inches, 9 lines, and this is extraordinary; for we had observed the preceding years that it rained much less here than in that country, which is upon the border of the sea.

We see by the memoirs of M. *de Pontbriand*, that the hard frost began some days sooner in that place than at *Paris*; but it snowed here at the same time with a N. W. wind. There was

* March 1, 1710.

but little wind at *Paris*, and that was towards the ſouth.

The month of *Jan.* gave there 33 lines ¼ of water, and at *Paris* only 22 lines ½. The memoir ſays, that the hard froſt diminiſhed at the end of *Jan.* and began again in *Feb.* and that the night between the 23d and 24th, it was as hard as from the 6th to the 18th of *Jan.* At *Paris*, it began again alſo in *Feb.* pretty near the ſame time; but it was much leſs than in *Jan.*

He adds alſo that the winds were very violent at N. W. but at *Paris* they were only very gentle, and toward the S.

He ſays, in fine, that the cold has not been ſo great with him, as in the middle of *Bretagne*; which muſt appear to have been ſo, becauſe of the proximity of the ſea, the humid vapours of which abſorb a part of the great cold, as we learn from all the experiments; for during the hard froſt, the air is extremely dry, and as ſoon as it becomes damp, it thaws.

I ſhall alſo here obſerve that I ſaw, in 1679, in the king's garden at *Breſt*, ſome very fine *ananas*, or pine-apples in the open ground, and I believe they had paſſed the winter there; perhaps the maritime ſoil contributed to it, for I do not believe they can be raiſed in this country.

In *June* they had at *Pontbriand* only 23 lines ¾ of water, and at *Paris* 45 lines ½: alſo at *Paris* the 25th and 26th, it rained 9 lines, and at *Pontbriand* only 2 ½.

In *Auguſt* we had a ſtorm in the night between the 11th and 12th, with 7 lines ½ of water, and they had none at *Pontbriand*.

In *September* we had again a ſtorm here the night between the 13th and 14, which gave 9 lines of water, and none at *Pontbriand*; beſides there

there fell only 5 lines of water during this whole month at *Pontbriand*, and above 29 lines at *Paris*.

In *Nov.* the quantity of water at *Pontbriand* was 3 lines $\frac{1}{4}$, and at *Paris* a little leſs than 1 line $\frac{1}{2}$.

In *Dec.* we had here during the night of the 15th and 16th a ſort of hurricane.

In general, all the winds of the year are a little different at *Pontbriand* and at *Paris*, and pretty often they tend more to the N. at *Pontbriand* than at *Paris*; which may be occaſioned by the direction of the *Engliſh* channel, and by all the coaſts of *Germany*, *Denmark* and *Norway*, and chiefly when the wind comes between the N. and W.

IV. *A compariſon of my obſervations with thoſe of M.* Scheuchzer, *on the rain and and conſtitution of the air, during the year* 1709, *at* Zurick, *in* Switzerland, *by M.* de la Hire *.

M. *Scheuchzer* has ſent me the obſervations that he has made upon the quantity of water which fell at *Zurick*, where he ſtayed during the year 1709; by which we ſee that the firſt ſix months have given him 172 lines $\frac{1}{2}$ of water, *Paris* meaſure, and the laſt 208 lines, which make in all 390 $\frac{1}{2}$ lines, or 32 inches, 6 lines $\frac{1}{2}$; but at *Paris*, there fell only 21 inches, 9 lines $\frac{1}{8}$: he adds, that this year has furniſhed 1 inch, 10 $\frac{1}{2}$ lines more than the preceding.

By the compariſon of theſe obſervations we

* May 24, 1710.

know, that it rains much more in *Switzerland* than at *Paris*.

I had already remarked by the obſervations of the rain made at *Lyons*, that it rained there much more than at *Paris*, which I attributed to the mountains of *Switzerland*, which are not very diſtant from it; and which is confirmed by theſe laſt obſervations. For it is not to be doubted but that the vapours, which are ſupported in the air in a flat country, and are much lower than the high mountains, when they come to meet, ſtop, and condenſe there in a cold ſeaſon into ſnow, which muſt produce much more water, being driven by the winds againſt the rocks, than in the places where they do not ſtop at all; and if the air is hot enough to hinder theſe vapours from freezing, they gather together and fall in rain, beſides the ſnow which then melts, and of which one part riſes alſo in vapour, cauſes there very great rains.

As for M. *Scheuchzer*'s obſervations upon the increaſes and diminutions of the river *Limage*, they naturally follow thoſe of the rains and meltings of the ſnow in the ſeaſon when that happens.

He alſo adds his obſervations upon the barometer and thermometer, where he ſhews that the greateſt height of the quickſilver of the barometer was 26 inches, 10 lines $\frac{1}{2}$, the 19th of *Jan.* and the loweſt 26 inches the 20th and 28th of *Feb.* and conſequently the difference was only 10 lines $\frac{1}{2}$, as in the year 1708.

The moſt remarkable thing is, that my barometer was alſo at the higheſt the 19th of *Jan.* at 28 inches, 3 lines $\frac{1}{2}$, with a calm, which is the ſame day that it was at the higheſt at *Zurick*, and that the difference is 17 lines; and if we would conclude from hence the different heights of of the places where these observations have been made, in supposing for one line of this difference 12 toises, 3 feet, as I have determined in these quarters, we should say, that the place where M. *Scheuchzer* observed, is higher than the middle of the observatory where my barometer is, by 212 toises ½. But the different heights at which we see the same quicksilver keep in different tubes, altho' in the same place, may leave us some suspicion of the true difference of the heights of these places.

As to the least heights of M. *Scheuchzer*'s barometer, which was at 26 inches the 20th and 28th of *Feb.* it does not altogether agree with mine in the same days; for *Feb.* the 28th, I had 27 inches, 2 lines, with a moderate wind, and consequently the difference of our barometers will be that day 14 lines instead of 15, which I found in the greatest height: perhaps our observations were not made in the same hour, and the wind might also occasion some alteration; M. *Scheuchzer* does not mark these circumstances. But *Feb.* 20, mine was at 26 inches, 10 lines, with a high wind at sun-rising; thus the difference would be only 10 lines, instead of 14 or 15 by the other observations, and mine would be lower than it ought by 4 or 5 lines. Nor was my barometer at the lowest on those days, for I observed it *Dec.* 16, at 26 inches, 7 lines ½, with a high south wind; thus the quicksilver of the barometer has much greater alterations at *Paris*, than at *Zurick* in *Switzerland.*

I think we might attribute these sorts of inequalities, to particular causes; for it is not probable that they can come from the different heights of the atmosphere, which make the weight of it, in places not very distant from one another. May we we not believe, that when there is a high wind and many clouds, and chiefly in the mountains, as in *Switzerland*, the wind ſhould compreſs and condenſe the air incloſed between the ſurface of the earth, the rocks and the clouds; ſo that it will then make a much ſtronger impreſſion upon the quickſilver of the barometer, than if there had not been any wind? But as in theſe ſorts of places, where there is a great deal of water, it is ſeldom that they have neither winds nor clouds, ſo the quickſilver of the barometer will for theſe reaſons ſupport itſelf there almoſt always higher than in the plains.

I can ſay nothing to M. *Scheuchzer*'s obſervations of the thermometer, altho' I have one of M. *Amontons*'s like his, which is a thick glaſs phial with a little quickſilver, which riſes into a little tube open at the top, as he had conſtructed them to make the experiment of boiling water, but I never make uſe of it, becauſe it is ſubject to the different changes of the weight of the air.

V. *Of the neceſſity of centring well the object glaſs of a teleſcope, by M.* Caſſini *the ſon**.

For the obſerving the apparent diſtances of the ſtars, they formerly made uſe of circles, ſemi-circles, or quadrants, divided into degrees and minutes, and furniſhed with four ſights, of which two were fixed and placed, one at the beginning of the diviſion, and the other diametrically oppoſite. The other two were born upon a rule moveable about the centre of the inſtrument, by the moderns called alhidade.

Since the invention of teleſcopes, they have ſubſtituted to the ſights two teleſcopes, one of

* March 26, 1710.

which is fixed upon a line parallel to the *radius*, which paſſes through the beginning of the diviſion, the other is placed upon a rule which turns about the centre. They place at the *focus* of the object-glaſſes of theſe teleſcopes, two threads which croſs one another in the *axis* at right angles, one of which is parallel to the plane of the inſtrument, and the other is perpendicular to it. They put the eye-glaſs into a tube, which ſinks into that of the teleſcope, ſo that the threads which croſs one another are at its *focus*, that their interſection may be well diſtinguiſhed.

Theſe teleſcopes thus diſpoſed have a great advantage over ſights, becauſe we diſtinguiſh by their means the terreſtrial and celeſtial objects, with much more perſpicuity, and obſerve more exactly their diſtance between themſelves, by placing them exactly in the interſection of the threads which croſs one another at their *focus* at right angles; but it is neceſſary, that the object-glaſſes be well centred, that is to ſay, that they be every where of equal thickneſs at their circumference. For let, 1, 2, 3, 4 *, be the tube of a teleſcope, which has at one end of its extremities an object-glaſs A, B, C, D well centred, ſo that the centre E of this glaſs be exactly in the *axis* S, E, I, O, of the teleſcope; let there be at the other extremity an eye-glaſs G H, of which let the centre I be alſo in the *axis* of the teleſcope. Let S be a very diſtant object, out of which proceed the rays S B, S D ſuppoſed to be parallel, which falling upon the ſurface of the glaſs B D, are refracted and reunited in the *axis* in L, which is the interſection of the two threads of ſilk M N, P R, which cut one another at right angles, and of which M N is vertical, and P R horizontal. We

* Plate VI. Fig. 1.

ſuppoſe

ſuppoſe the point L to be placed to the *focus* of the lens G H, in ſuch a manner, that the rays G L, H L, which proceed from this point, and fall upon the ſurface of the lens G H, are reunited in O. The eye being at O will ſee the object S in L in the *axis* of the teleſcope, and conſequently in its true ſituation.

If we move the object-glaſs A B C D to a b c d, ſo that the centre of the glaſs be, for example, in F; then the rays, that proceed from the object S, will be reunited at the point T, to the extremity of the *axis* S F T, which paſſes through the centre of the glaſs F, and the rays which proceed from the point T, and fall upon the eye-glaſs G H, will be reunited at the point V, where the eye being placed, will ſee the object S in T, in a very different ſituation from that where it appeared, when the object-glaſs was at the centre of the teleſcope.

If we now ſuppoſe, that we would obſerve the diſtance between two ſtars with two teleſcopes, one of which has its object-glaſs well centred, and the other not; if we incline the inſtrument to obſerve the apparent diſtance of the two ſtars, the well-centred teleſcope turning by this motion about its *axis*, the centre E of the object-glaſs reſts in the *axis* of the teleſcope, and its *focus* falls upon the point L the interſection of the threads; but the centre F of the object-glaſs not well centred, will, by this motion deſcribe a little circle about the *axis* E L of the teleſcope, and the point T, where the rays are then reunited will deſcribe alſo a like circle about the centre L; ſo that the apparent diſtance between theſe two ſtars obſerved with two teleſcopes, one of which has its object-glaſs well centred, and the other not, will not be their true diſtance, and will be ſubject to irregu-

larities,

larities, which cannot be remedied, but by cenring the two object-glaſſs exactly, or directing them one upon the other to the ſame object, which comes to the ſame thing.

VI. *Obſervations on the bezoar, and on other ſubſtances which come near to it, by M.* Geoffroy, *junior.*

The *bezoar* is thought by ſome to derive its name from the *Perſian* word *pazar* or *pazan*, which ſignifies a *goat:* and according to ſome others, it comes from the *Hebrew* or *Chaldean* word *beluzaar*, which ſignifies *counterpoiſon.*

The firſt ſtones, known under the name of *bezoar*, were brought from the eaſt. After the diſcovery of *America*, there came ſome, which bearing ſome reſemblance to the former, both in ſtructure and virtue, had the ſame name alſo, with this difference, that the firſt are called oriental, and the others occidental bezoars. There are other ſtony ſubſtances alſo taken from animals and diſpoſed in ſtrata, which have been called *bezoar*, with the addition of the name of the animal, as bezoar of the ape, and bezoar of the *cayman.* Some taking the word bezoar in the ſignification of counterpoiſon, have applied it indifferently to all ſubſtances endued with that virtue; hence it has been given to chymical compoſitions, as *mineral* and *jovial bezoar.* Others have called the powder of the heart and liver of vipers, *animal bezoar.* The name of *bezoar* or *bezoartic*, has alſo been given to ſome artificial powders or ſtones, in which bezoar is an ingredient. Such are the different bezoartic powders, the counteſs of *Kent*'s powder, the ſtones formed of this powder, and the *Goa* ſtone.

As the bezoar has been obſerved to be diſpoſed in *ſtrata*, the name has been given to a ſort of figured ſtone, found in *America* in ſeveral places, to which alſo the ſame virtues are aſcribed. There are bezoars found alſo in *Italy*, *Sicily*, ſeveral parts of *France*, and eſpecially in *Languedoc*.

Theſe are the different ſubſtances in general, which we know under the name of *bezoar*. But properly ſpeaking, the bezoar is a ſtony ſubſtance taken from ſome animal, compoſed of ſeveral *ſtrata*, or coats like onions, and endowed with ſome power of reſiſting poiſon. The two principal ſpecies of it are, as we have ſaid, the oriental and occidental. It is not eaſy to diſtinguiſh what animals they are that produce them; becauſe what agrees with only one of them, may have been aſcribed to both. We know in general, that this ſtone is found in the ſtomach of a ſort of wild goat which browzes upon aromatic plants. If we may believe *Tavernier*, there are ſeveral found in the ſame animal, as may be known by feeling. Theſe ſtones are of different ſhapes and ſizes: ſome are ſhaped like a kidney; others are round, or oblong, or of an irregular figure. Each ſtone is compoſed of ſeveral plates, and formed of a greeniſh or olive-coloured ſubſtance, ſpeckled with white. Theſe plates adhere to each other in ſuch a manner, as to ſhew upon breaking ſeveral *ſtrata* of ſubſtances of a different thickneſs, and ſometimes of a different colour. In breaking theſe ſtones, ſome plates part with great evenneſs from the reſt. The ſame thing happens upon rubbing them pretty briſkly. The middle or centre of the ſtone is commonly a hard, gravelly, ſmooth ſubſtance. The bezoartic *ſtrata*, which cover this maſs, are eaſily cruſhed by the teeth; and

and ſtick to them as if they were ſomething glutinous, and tinge the ſpittle.

They kindle eaſily in the fire, and ſeem to contain ſome volatile ſalt and oil. The matter which remains is like the *caput mortuum* left in the retort after the diſtillation of animal ſubſtances. Theſe ſtones are very ſmooth on the outſide, but ſometimes a little rugged, and like ſhagreen in ſome of their circumvolutions. They are pretty tender, and give a yellow, greeniſh, or olive-coloured tinge to paper rubbed with chalk, ceruſe, or lime, on being drawn pretty hard over them, becauſe they wear away, and leave ſome of their parts upon thoſe materials. I have ſteeped 2 of theſe ſtones cold; 1 in water, and the other in ſpirits of wine, for 12 hours, without finding any alteration in them. I have left the ſame ſtone in water for ſeveral days, and there came only a little matter from it, which juſt troubled the water, and yet the water or ſpirit of wine had penetrated both of them.

In the great number of bezoar ſtones which I have opened, I have found that many, as ſome authors relate, had chaff, hair, marcaſites, ſtones, or gravelly ſubſtances united together, and as hard as a ſtone in the middle. I have alſo found talc, wood, kernels almoſt like cherry ſtones, alſo myrobalan ſtones, quarters of other fruit-ſtones, kernels of *caſſia*, and kidney-beans incloſed in a coat, or outer membrane, hardened by the matter which has formed the bezoar, and having their own membrane drawn back, and dried, after having been ſwoln. In others, the firſt coat of the kidney-bean was conſumed; and the ſtones ſounded like eagle-ſtones. I have attempted to prick ſome of theſe ſtones with a red-hot needle to ſee whethey were counterfeit, but it did

 not

not enter, and only imbrowned the place where it was applied; which authors propoſe as one of the principal marks by which the good bezoar may be known, imagining that thoſe are to be rejected, in which kidney-beans are found, which they look upon as a proof that they have been falſified by the people of the country.

They adviſe us to chooſe the bezoar in ſtones of a middling bigneſs, of a brown colour, turning quick lime yellow, and chalk green, not diſſolving in water, and not riſing in bubbles about the part pierced with a red-hot iron, for that would ſhew it to be mixed with ſome reſins. The plates alſo muſt be fine, diſpoſed in *ſtrata*, and the ſtones muſt be taken from animals which live upon the mountains, ſuch as thoſe of *Perſia*. After all it ſeems pretty difficult to me, to counterfeit the bezoar; and with a little practice we may eaſily diſcover the cheat, if there is any. For if it was counterfeited with plaſter, or any ſuch like matter, it would not change either with the fire or water, it might colour the quick-lime with any tincture that was given it; and, in a word, undergo all the proofs, though it was counterfeit.

Nor is it to be imagined, that in order to counterfeit them, they pick out all theſe different ſubſtances which ſerve as a baſe to the *ſtrata*, of which they are compoſed, ſince they need only begin a little ball of the ſame paſte, which probably is not ſo rare, that they have occaſion to be ſaving of it.

I think the ſubſtances incloſed in the bezoar ſerve perfectly well to inform us of the manner in which it is produced, as is obſerved by *Tavernier*; who tells us, theſe ſtones are formed about little buds, or tops of the branches of a plant: theſe buds of *Tavernier* may be the kidney-beans ſpoken of by *Monard*, which I have obſerved. Theſe ſolid and undigeſted bodies remaining in the ſtomach of the animal, may irritate its glands, of which the lymph thickened with the leaven of the ſtomach, ſtill loaded with the juice of the aromatic plants on which it has juſt brouzed, may have been able to form the *ſtrata*, ſo ſmooth and exactly united, that art would find a difficulty in imitating it. I obſerve alſo, that whatſoever body makes the centre of this ſtone, the *ſtrata* of it are ſo fine, and ſo well turned, that the ſtone outwardly takes the figure of the ſubſtance contained within.

If, for example, there is a ſtraw, the ſtone will be long; if it is a ſtone, it will preſerve the figure of it; if it is a kidney-bean, the radicle will appear on the outſide; and a line which ſeparates very diſtinctly the 2 lobes of the bean; in ſhort, we may know by the ſhape and weight of them, what they contain. Thus, as in the choice of ſo precious a ſubſtance as the bezoar, we have not the liberty of opening all, after having been well aſſured of a certain number of the moſt doubtful, upon which we ſhall have made the preceding experiments, we muſt refer to the ſight and feeling. By the ſight we examine the colour immediately, which muſt neither be too pale, nor too deep; in the ſecond place, the fineneſs of the grain, the ſmoothneſs, and the cloſeneſs of its texture, which keeps the plates from riſing eaſily above one another. It muſt alſo be obſerved, that they have a regular ſhape, as of a kidney, a bird's egg, or ſomething like theſe. The touch may alſo judge of the matter incloſed within the bezoar, which we may eaſily determine by the weight of it. If, for example, the

the bezoar is heavy, the baſis will be a ſtone, or ſome other matter, which fills up the greateſt part of it; but if it is light, it will be hollow within, or contain only ſome light matter, as hair, or ſome of the vegetable ſubſtances already mentioned. The ſtones which rattle, ſhew there is a fruit within, which being dried, takes up leſs room; and ſometimes it is rotted or broken into a duſt, which ſome authors greatly eſteem.

I have alſo obſerved, that when bezoars are ſhaped like kidneys, are light and rattle, they have uſually a kidney-bean in the middle. Thoſe which are light, round, and a little flatted, contain a round flat fruit, almoſt of the ſhape of a caſſia ſtone. Moreover, though theſe ſtones ſhould incloſe a ligneous kernel, or even bits of wood, the lightneſs ſhould make them preferable to thoſe which contain ſtones, which alſo will be a great deal heavier, provided the bezoartic matter anſwers the other proofs.

The whole preparation of bezoar for common uſe in medicine is, to reduce it to a fine powder, either to give it in ſubſtance, or to make it enter into ſome compoſitions, obſerving to powder only the bezoartic part, and to ſeparate all the foreign matters which may be found in the heart of the bezoar, eſpecially when they are ſtones or other ſubſtances, which have no bezoartic virtue.

There is a great diverſity of opinions about the animal which yields the bezoar. It appears, that the oriental, which is brought to us from *Egypt*, *Perſia*, *India*, and *China*, is produced by a ſort of goat, called by the *Perſians*, *Pazan*, or by a wild goat of a larger ſize than ordinary, as nimble as a ſtag, with its horns reverſed on its back, whence *Cluſius* calls it *Capricerva*.

That

That which is brought from *America*, is produced by a ſort of goat, which is not at all, or but very little different from the other, except in its horns.

The different opinion of authors concerning the names and figure of this animal make me believe, that theſe ſtones may be found in ſeveral ſpecies of animals, and that each has deſcribed what he has ſeen. The ſame reaſon may ſerve to prove the cauſe of the different colours of the bezoar.

The occidental bezoar is eaſily diſtinguſhed by its being paler. It is ſometimes of a light grey, ingendered upon foreign ſubſtances, like the oriental bezoar. The plates are ſometimes thicker and ſtriped in their thickneſs.

The foſſil bezoars are a ſort of ſtones formed in *ſtrata*, having the figure of the animal bezoar. They are uſually of a light grey, their *ſtrata* are very thin, they have no ſmell, and are uſed in the ſame diſeaſes with the other bezoars. *America*, as I have already ſaid, furniſhes us with a great many of theſe bezoars, as well as *Italy*, and ſeveral parts of *France*.

Thoſe who have treated of the bezoar, as *Caſpar Bauhinus* have comprehended under this name, a great many ſubſtances that have no relation to it, which can only cauſe confuſion in natural hiſtory. If therefore we would range in a convenient order, all that can partake of he the name of bezoar, I believe it would be proper to make 5 claſſes of them.

The firſt would contain the true bezoars, which are the oriental and occidental.

In the ſecond we might place all the ſtones taken from animals, which reſemble the bezoar in their ſtructure and vertue, as the bezoar of the ape, that of the *Cayman*, alſo the different ſorts of pearls, and the crab's eyes.

In

In the third, the different ſorts of foſſil bezoars.

In the fourth, ſubſtances figured like the bezoar without its virtues, as the human ſtone, either of the bladder, kidneys, or gall-bladder, with thoſe which are found in the gall-bladder of oxen, and other animals.

In the fifth, the *egagropilæ*, which are a ſort of balls of different figures, pretty light, formed of a maſs of hairs and fibres of plants, which the animals could not digeſt. Theſe fibres and hairs are ſo interwoven as to form but one body, which reſembles a ball of felt. There are ſome which are covered again with a thin bezoartic cruſt. They commonly grow in the firſt ſtomach of all ruminating animals, or in the ſtomach of thoſe which do not ruminate. Such are the ſtone of the wild porcupine, and the other balls of hair found in goats, cows, oxen, and other animals.

VII. *An inſect upon ſnails, by M.* de Reaumur; * *tranſlated by Mr.* Chambers.

All the animals hitherto obſerved which live upon other animals, may be reduced to two kinds; for either they live on the external ſurface of the body of the animal, as the lice found on quadrupeds, birds, and even ſeveral inſects, as flies, beetles, hornets, *&c.* or they live in the body of the animal, under which kind may be ranged the ſeveral ſorts of worms which have been diſcovered by diſſection in the bodies of ſeveral animals.

The new inſects I have obſerved on ſnails does not come under either of theſe kinds, but has ſomething in common to both; for it ſometimes

* July 9, 1710.

inhabits the external ſurface of the body of the ſnail, and ſometimes hides itſelf in the *viſcera* thereof.

By the collar of a ſnail is meant, that part which encompaſſes its neck. This collar is of a conſiderable thickneſs; and 'tis little other than the thickneſs of this collar we perceive when the ſnail ſhrinks into its ſhell, ſo as neither to let its head nor baſis be ſeen, of which you may conceive an idea by *fig.* 2, the triangular ſpace B ſituate in the middle of the aperture of the ſhell is a remainder of the baſis of the animal, which is ſurrounded on all ſides by the thickneſs of the collar; and 'tis on this part of the collar, that the inſects we are to ſpeak off are found, they are repreſented in the ſame figure by the letters CCCC, *&c.* or rather by the dotted lines which proceeding from thoſe letters, terminate in theſe animalcules; they are never eaſier to obſerve, than when the ſnail is thus totally incloſed in its ſhell, tho' they may be perceived in ſeveral other circumſtances. The bare eye without any aſſiſtance of the microſcope, ſuffices to diſcover them; but they are rarely ſeen at reſt, being in a continual hurry, running about with great agility, which is ſomewhat ſingular; the motion of ſuch kinds of inſects being uſually very ſlow.

Notwithſtanding the ſmallneſs of theſe animalcules, there is not room for them to go upon the upper ſurfaces of the body of the ſnail, the ſhell being too exactly fitted thereon; but there is territory enough beſides to travel in; the ſnail giving them entrance, as oft as it opens its *anus*. This *anus* is likewiſe placed in the thickneſs of the collar, in the place marked by A; it is here repreſented ſhut; but the animal rarely comes out of its ſhell without opening it; beſides, that it opens

 it

it on ſeveral other occaſions, it may be ſeen open in *fig*. 3. where it is alſo denoted by the letter A.

It ſeems as if the little inſects waited with impatience for the favourable minute, when entrance ſhould be given them into the ample theatre of the inteſtines of the ſnail, at leaſt they never miſs the opportunity of preſenting themſelves when occaſion offers; gathering to the edge of the hole, they immediately ſlip into the ſame, running along the *parietes* thereof, ſo that a few minutes after, not one inſect is left on the collar. The letter D in *fig*. 3. ſhews ſome of theſe animalcules preparing to enter into the inteſtines by the *anus*.

The eagerneſs wherewith they endeavour to get in, ſeems an indication that this is their moſt commodious place of reſidence, how then ſhould they come on the collar? 'Tis poſſible they never do it but againſt their inclination, of which the continual hurry they are under ſeems a proof. In effect the ſnail obliges them to go lodge there, as often as it voids its excrements; for thoſe excrements poſſeſſing almoſt the whole width of the inteſtine, muſt neceſſarily drive before them every thing they meet in their way. The little inſects therefore upon their arrival at the edge of the *anus*, are forced to go upon the collar, and in regard this operation of the ſnail continues ſome time they walk about all this while on the collar, as having it not in their power to re-enter when they pleaſe, in regard the ſnail has frequently ſhut the door, while they were friſking on the outſide.

What has been hitherto ſaid, may be obſerved of all the ſpecies of ſnails, tho' moſt frequently of the large garden ſnails repreſented in *fig*. 2 and 3. But there are ſome ſorts wherein this inſect may be diſcovered, even in the middle of the inteſ-

tines;

tines; as in the little ſpecies of ſnails repreſented by *fig.* 4 and 5. The Characteriſtick of this ſpecies is a kind of lid denoted by O, conſiſting of a matter equally ſolid with that of the ſhell, and by means whereof the animal can incloſe itſelf all around when it pleaſes, as ſea ſnails do, whereas the collar of the common land-ſnails is bare, unleſs in winter and ſome dry ſeaſons, when they ſtop the aperture of their ſhell with a kind of foam which comes to a conſiſtence as it dries; but this occaſional lid never adheres to the body of the animal, like that above mentioned; nor is it comparable thereto in ſolidity. Breaking the ſhell of one of theſe little ſnails about the place E *fig.* 4. and thus laying the ſkin of the animal bare as in *fig.* 5. the inſect will be frequently diſcovered in the very body of the ſnail, by reaſon this coat or ſkin is tranſparent, and lets us ſee thro' it, as thro' a glaſs; the letter C repreſents two inſects, as viewed thro' the ſkins of the ſnail.

Tho' we find theſe inſects on all the ſpecies of ſnails, yet not at all times indifferently, and very rarely in rainy ſeaſons. Not to give our ſelves uſeleſs trouble, we are only to look for them after a drought, which perhaps may be proper to hatch them, or even to prevent the deſtruction of thoſe already formed. When the earth is very moiſt, the body of the ſnail is ſaturated with water, which afterwards oozing much more viſcid thro' the collar and baſe of the ſnail, forms ſeveral drops thereon, the ſmalleſt of which drops ſuffices to deſtroy ſeveral of theſe inſects; not that they are afraid of being drowned therein, as in a kind of little ſea, this liquor is to them a ſolid body, and each drop may be to them, what the fall of a building is to us. I mean it may overwhelm and cruſh them by its weight, whenever by the motion of the ſnail, one of theſe drops happen to be tumbled from one place to another.

Be this as it will, 'tis certain that dryneſs promotes their formation, as appears from the following fact, which I have repeated ſeveral times; gathering ſnails in moiſt weather, and after a careful examination finding no inſects in them, I put them in veſſels where the loſs of the watry humour, continually evaporating from them, could not be repaired, and viewing the ſame ſnails ſometime after, I never failed to find ſeveral inſects thereon, having ſometimes told twenty on the ſame animal. In 5 or 6 day; I have ſometimes found a few, but in 3 weeks never failed of a large quantity.

The body alone of the ſnail is a ſoil proper for theſe inſects, which are never ſeen on the ſhell; or if they be compelled thither are not long 'ere they recover the collar, from whence they were driven.

To the bare eye they uſually appear of a very white colour, though ſome of them ſeem a little browniſh, and others lightly tinged with red.

A good microſcope is neceſſary to perceive their ſeveral parts diſtinctly; by this they appear as in *fig.* 6 and 7; the former whereof repreſents their upper ſide, and the latter their under ſide. The letter T in each figure ſhews their trunk, which however only appears in part in *fig.* 6; but the manner in which it bends under may be ſeen. This trunk in all likelihood ſerves them to ſuck the ſnail; it is placed in the middle between two little horns C C, which are very movable, like thoſe of other inſects, both upwards, downwards, and laterally; and what is more, are capable of extending and contracting, like the horns

horns of ſnails; whence the animalcule is frequently ſeen without perceiving its horns.

Its body is divided into 6 *annuli*, and the anterior part to which the trunk and horns are joyned. It has 4 legs on each ſide, the 2 foremoſt whereof are articulated to the anterior part; and the 2 hind ones to the firſt ring; the ſecond and third are faſtened further from each other, than the firſt and ſecond; or the third and fourth: theſe legs are beſet with large hairs, and ſeem to terminate in three or four points, much like the legs of ſeveral kinds of beetles, when the laſt articulation is removed, which terminates in two little hooks. Their back is round, and raiſed with regard to their ſides, which are likewiſe rounded, and have 3 or 4 large hairs upon them; their *anus* is likewiſe ſurrounded with 4 or 5 hairs of an equal length; but there are none on its belly.

VIII. *Reflections on the obſervations of the flux and reflux of the ſea made at* Dunkirk, *by M.* Baert, *profeſſor of hydrography, during the years* 1701, *and* 1702; *by M.* Caſſini, *jun.* * *tranſlated by M.* Chambers.

Obſervations of the ebbing and flowing of the ſea, being of great importance for the ſecurity of navigation, and for the choice of times moſt ſuitable for coming in, or going out of ports, and it being withal of great conſequence to the ſciences, to learn whether they have any connection with the motions of the moon; and whether the variations to which they are ſubject, are reducible to any rules, a circular memoir was drawn up

* July 12, 1710.

by

by the academy, and at their requeſt, ſent by the count *de Pontchartrane*, into ſeveral ports of *France*, with orders to make exact journals of ſuch obſervations.

Among others, M. *Baert*, profeſſor of hydrography at *Dunkirk*, was intruſted with this care, of which he acquitted himſelf with all the application and accuracy that could be deſired.——— He choſe a place for his obſervations in the incloſure of the admiralty, where the ſea has no other conſiderable motion, but that of the flux and reflux; here he built a lodge both for ſhelter from the weather, and to prevent being diſturbed in his obſervations: this done, he fixed a ſquare tube * EFGH, perpendicular to the ſurface of the ſea, being compoſed of 4 boards open at bottom in GH, that the water might enter freely in, and riſe to a level with the ſea, and cloſed a-top in EF, by a lid EAF, which had a little hole in A, 14 lines in diameter, thro' which paſſed a wooden ruler TK, on the lower extremity whereof was a little ſquare board LM, ſomewhat blunted at the corners to prevent friction; under which board was faſtened a piece of cork 4 inches thick, which floating on the ſurface of the water, made the wooden ruler TK riſe and fall according as the tide roſe and fell. This ruler was divided into feet and inches, whereby to eſtimate the increaſe or diminution of the tide.———We omit in this account ſeveral circumſtances of this machine, which ſhews the great accuracy of M. *Baert*'s obſervations, and are related at large in a letter to father *Gouye*.

It may be neceſſary here to obſerve, that all the meaſures of the height of the ſea were taken with regard to a fixed point, which is on a level with

* Fig. 8.

the

the top of the boards bordering the key, near the ſluice of the baſon, directly on the aſcent towards the citadel, which is a part of the key which the ſea never goes beyond ; nor muſt it be omitted, that the direction of the canal at *Dunkirk*, is north-weſt by north, that its length from the mole-heads near the road to the place of obſervation is 1435 fathoms, and its breadth 36 fathoms at the mouth, and 16 where narroweſt ; notwithſtanding which, there is no conſiderable difference between the time of high water at the place of obſervation, and that againſt *Risbanc*, as was found 5 ſeveral times, by the fineſt days of ſummer, by minute watches.——For underſtanding of what follows, it muſt be obſerved, that we call it high water when the flood is roſe to its greateſt height ; and low water when the ebb is fallen to its greateſt depth. The greateſt tides are thoſe when the flood is the higheſt poſſible ; and the ſmalleſt tides thoſe when the flood is the loweſt poſſible.

The journal of M. *Baert*'s obſervations of the tides, begins on the 24th of *March*, 1701, and ends on the 31ſt of *May*, 1702 ; it expreſſes for every day the height of the water in the time of flood, and ſome hours before and after, with regard to the fixed point abovementioned, increaſing in number downwards, in order to find the proportion between all the heights of tides which he had occaſion to obſerve. To find the preciſe time he had drawn a meridian line with great exactneſs, whereby to regulate his clock from time to time ; thus obſerving the hour and minute wherein the water was at the ſame height, both in riſing and falling, he took the middle between the two obſervations which were neareſt the high water, the one before, and the other after it, for the preciſe time of high water, which he found

found more convenient than to make uſe of remoter diſtances, having obſerved in many experiments, that the ſea falls ſomewhat more ſlowly than it riſes. He alſo obſerved the winds, and the temperature of the air on each day of obſervation.

As to the irregularity of the progreſſion obſerved both in the riſing and falling of the tide, M. *Baert* dares not determine, whether the winds be the cauſe, or whether we are to ſuppoſe that the ſea is moved by waves far diſtant from each other; and by others which follow cloſe together. ——— As to that balancing upwards and downwards, obſerved at each high-water, he takes the cauſe to be natural; for as the ſea in approaching the coaſts meets with an obſtacle, it may riſe a little above its level, which will oblige it to return again; and thus make a ſlow ſort of vibrations near the place where the obſtacle is, which will ſcarce be perceivable elſewhere, by reaſon of the winds.

To be able to compare the obſervations of high water, and ſee whether their irregularity be reducible to any rule more certain than has yet been done, M. *Baert* has drawn a table wherein is expreſſed for every day from the 24th of *March*, 1701, to the laſt of *May* 1702, the moon's place at noon in longitude and latitude in two ſeparate columns; her age at the time of high water in a third column; the preciſe time of high water in a fourth; the height of water below the fixed point in the fifth; the moon's paſſage over the meridian; in the ſixth, and in the ſeventh, and eighth, the direction and ſtrength of the wind and ſtate of the weather.

The firſt thing that occurs upon conſidering the times of high water at *Dunkirk* is, that on the days

days of full moon the flood happens about noon, though not ſo exactly, but that we ſometimes find a difference of a whole hour, as may be obſerved in the 15 ſucceſſive obſervations made thereof; the high water which came the earlieſt, was on the 19th of *July*, at 24 minutes paſt 11 in the morning; and the lateſt on the 17th of *September*, at 24 minutes paſt 12 in the afternoon, which gives a variation of an hour in the times of the tides on the days of full moon; which variation being divided into 2, gives the mean time of high water at *Dunkirk*, about 6 minutes before noon.

To fix ſome rule in this variation of the time of the tides, on the days of full moon it muſt be obſerved, that the retardations of the tide from one day to another, bears ſome analogy to the motion of the moon, whoſe paſſage over the meridian is retarded about 49 minutes daily. On this footing, when the times of full moon concurs with the time of high water, there muſt neither be anticipation, nor retardation, in the time of high water; but when the full moon happens in the morning before high water, the moon's paſſage over a horary circle, is retarded two minutes in an hour, with regard to the ſun; and conſequently there muſt be an equal retardation in the time of high water; whereas, when the full moon happens after high water, the moon being not yet at its full, when the water is at its height, there muſt be an acceleration in the time of high water obſerved.

Suppoſing this acceleration, or retardation of 2 minutes in an hour, we have a rule for determining the variation of the tides on the days of full moon.———For an inſtance, on the 19th of *July*, 1701, the high water was found at 24 minutes paſt 11 in the morning, which is the greateſt acceleration obſerved by M. *Baert*; and full moon for that day is marked in the almanack at 50 minutes paſt 11 in the evening; hence the high water muſt have gained about 24 minutes, which being ſubſtracted from 11 hours, 54 min, the mean time of the tides at *Dunkirk*, gives 11 hours, 30 minutes for the time of high water, within 6 minutes of that found by obſervation.—Again, on the 17th of *Sept.* 1701, the day on which the greateſt retardation of the tides was found, high water happened at 24 min. paſt 12, and full moon at 56 min. paſt 5 in the morning, conſequently high water, by the rule above aſſigned, muſt have been retarded 12 min. which added to 11 hours, 54 min. give 12 hours, 6 min. for the time of high water, within 18 min. of that found by obſervation.

It muſt be obſerved, that whereas in the obſervation of the 19th of *July*, the wind was north-north-eaſt; on the 17th of *September*, it was ſouth and very freſh at the time of high water, which might have contributed to the retardation of the tide; for the waves being driven by the tide againſt the coaſts of *Dunkirk*, from north to ſouth, their motion might eaſily be retarded by the ſouthern wind, which coming from ſhore, blew directly againſt the tide; ſurmiſing from this obſervation, that the winds, according to their different directions, may occaſion either accelerations, or retardations of the tide, we examined the obſervations made on the 15th of *Nov.* 1701, the day of full moon, the wind being at ſouth, and very freſh, according to the rule above laid down, full moon having happened at 4 minutes paſt 5 in the evening, we muſt ſubſtract 10 min. from

from 11 hours, 54 min. which gives the time of high water at 11 hours, 44 min. in the morning, 16 min. earlier than in the obſervation, which fixed it at 12 hours, 0 min. In this obſervation therefore, as well as in that of *September*, there was a retardation in the tide, which may likewiſe be attributed to the wind, which blowing at ſouth-weſt, muſt have checked the motion of the tide. On the contrary, in the high water on the 12th of *April*, 1702, full moon happened at 0^h—13′ in the evening, and it was high water at 11^h—45′ in the morning, the wind being at north-north-weſt, and very freſh. By the rule therefore, high water ſhould have happened at 11^h—54′, which is 9′ later than was actually obſerved: ſo that in this obſervation was an acceleration, which may be attributed to the north-north-weſt wind, which blowing directly on the coaſt, concurred with the tide, and made it earlier than it would otherwiſe have been.

In the other tides, obſerved by M. *Baert* at full moons, the winds were either weak, or ſo diſpoſed, that they could neither hinder nor promote the motion of the tide any thing conſiderably, ſo that no regard was had to the effects produced by them.

When a like compariſon of M. *Baert*'s obſervations of high water for 15 ſucceſſive new moons, from the 8th of *April*, 1701, to the 26th of *May*, 1702, we find, that the earlieſt came on the 29th of *Nov.* at 11^h——20′ $\frac{1}{2}$ in the morning, the new moon for that day being at 10^h——11′ in the evening; and that the lateſt was found on the 27th of *April*, 1702, at 0—47′ in the evening. A new moon happening that day at 3—54 in the morning, the difference between the

 times

times of theſe two tides being divided into two, we have the mean time of high water at *Dunkirk* in the new moons, at 12—4′, which only differs 10′ from the mean time of the tides at full moon.

This difference being inconſiderable, high water at *Dunkirk* may be ſuppoſed in the new moons, as well as in the full moons, to happen at 11^h—54′ in the morning; ſo that uſing the rule above preſcribed for determining the variations of the tides, on the days of full moon, we ſhall have the time of high water on the 8th of *May*, 1701, at 12^h—15, which is within 20 min. of what was actually obſerved; and the time of high water on the 27th of *April*, 1701, at 12^h—30′, within 37′ of the obſervation, which, in ſome meaſure, reconciles thoſe two obſervations, which were 1^h—26 diſtant from each other.

As to the winds obſerved at the time of high water in the new moons, they do not ſeem to haſten or retard the flood, ſo regularly as was obſerved in the full moons, which may ariſe hence, that the motion of the tide ariſes from a complication of ſeveral cauſes, ſome whereof may be unknown; beſides, that 'tis difficult to aſcertain the preciſe time of high water. The time while it remains full flood, without either ſenſibly riſing, or falling, being according to M. *Baert*'s obſervation, from 12 to 20, or 30′, it muſt be obſerved, that the tides happening on the days of full and new moons, are not the higheſt tides; but that the higheſt happen 1, 2, or 3 days after, as appears from 30 obſervations made thereof, only two of which happened the day before full moon; ſo that upon a medium, one may ſuppoſe, that the higheſt tide at *Dunkirk* happens

pens two days after new or full moon, as M. *Baert* has obſerved.

'Tis commonly ſuppoſed, that the higheſt tides happen in the new and full moons next the equinoxes, and yet by comparing the obſervations made at *Dunkirk*, we find, that the higheſt tide happened on the 30th of *Nov.* 1701, when its height above the fixed point a day after full moon, was found 3 feet, 2 inches; and on the 27th and 28th of *Feb.* 1702, when it was found 3 feet, 3 inches.

The great height of theſe two tides, 'tis true, may be attributed to ſome extraordinary cauſe; for on the 29th of *Nov.* 1701, the day of new moon, the high water was found 6 feet, 8 inches, below the fixed point, which was one of the loweſt tides that had been obſerved; and on the day following, it was found 3 feet, 2 inches, which, as above noted, was the higheſt tide that had been known at *Dunkirk*. On this and the preceding day, there was a violent ſouth-weſt wind, which on the day of new moon, might have driven back the waters, and hindered their riſing to the uſual height, till returning with more impetuoſity on the day following, they roſe even beyond their cuſtomary pitch, and thus made a kind of balance; in effect on the next day, *viz*, the firſt of *December* was obſerved 4 feet, 2 inches below the fixed point, which is above a foot lower than on the day before; and on the ſecond of *December*, it was 3 feet, 11 inches higher than on the firſt; whereas, according to the common rule, it ſhould all along have been on the ſinking hand, ſo that we may ſuppoſe this alternate motion cauſed by a violent ſouth-weſt wind to have laſted 4 days.

Much

Much the ſame fluctuation was obſerved on the 27th and 28th of *Feb.* 1702, when the height of the fixed point above the ſea was found 3 feet, 3 inches; for on the 26th of *Feb.* the day of new moon, the high water was obſerved 5 feet, 6 inches below the ſame fixed point, occaſioned by a great north-weſt wind. On the 27th in the morning, the wind was ſouth-weſt, and at 10 a-clock turned to north-weſt, high water on this day was obſerved 3 feet, 3 inches below the fixed point, which is 2 feet. 3 inches higher, than on the day preceding. On the 28th, it was found at the ſame height; but on the firſt of *March*, high water was found 2 feet, 7 inches lower, than on the 28th of *Feb.* and on the ſecond of *March*, it was a full foot higher, than on the firſt, tho' it ſhould rather have ſunk; ſo that here was a kind of vibration, excepting that there was no variation between the heights of the 27th and 28th of *Feb.* which might be owing to the winds ſhifting ſo ſuddenly, from ſouth-eaſt to north-weſt, on the 27th in the morning.

We have ſufficient grounds therefore to ſuppoſe, that the winds may increaſe or diminiſh the height of the tides, after the ſame manner as they have been ſhewn to occaſion accelerations and retardations therein; and 'tis probable likewiſe, that the diſpoſition of the channel of the ſea, and the ſituation of the ſhores, may contribute their ſhare to the producing variations very difficult to be reduced to any certain rules.

As the higheſt tides after new and full moon, do not always happen at *Dunkirk* about the equinoxes, it has been enquired, whether ſome other cauſe, for inſtance, the different diſtance of the moon

moon from the earth, might not contribute to their increaſe, or diminution. —— For ſuppoſing, as we may very eaſily do, that the cauſe of the ebbing and flowing of the ſea ariſes from the preſſion of the moon, upon the fluid matter between the moon and the earth, it will follow, that the further diſtant the moon is from the earth, the leſs will this preſſion be, and conſequently the tide the lower; on the contrary, the nearer the moon is to the earth, the greater will be its preſſion, and conſequently the tide higher.

According to our theory of the moon, which gives an exact repreſentation of the motion of that planet, and its ſeveral diſtances from the earth, ſuch as found from the apparent variation of its diameter, 'tis ſuppoſed, that when the ſun's place meets with the place of the moon's apogee, the moon being now in conjunction, is at her greateſt diſtance from the earth; and, on the contrary, at its ſmalleſt diſtance, when in oppoſition. About 6 months after, when the ſun meets with the moon's perigee, the moon is then at her leaſt diſtance from the earth in conjunctions, and at the greateſt in oppoſitions; and when the ſun is 3 ſigns diſtant from the moon's apogee or perigee, on each ſide the moon is at the ſame diſtance from the earth, whether ſhe be in conjunction or oppoſition.

If we now compare M. *Baert*'s obſervations, made when the ſun was near the apogee and perigee of the moon, for about the mean diſtances, we ſhall find, that the high and low tides, both in the new and full moons, correſpond to the different diſtances of the moon from the earth; and that when the ſun is in the mean diſtances, the tides are pretty nearly of an equal height in the

the conjunctions or oppositions immediately following.

For an instance, in the full moon which happened on the 21st of *March*, 1701, the sun was near the moon's apogee, being 17 degrees 16′ therefrom, the moon therefore being then in opposition, was according to our theory near the earth, and consequently the tide must have been high accordingly. On the 26th of *March*, two days after full moon, the height of the fixed point above the surface of the water, was 4 feet, 3 inches, which was one of the highest tides that had been observed. ——— And in the next new moon that happened on the 8th of *April*, the distance of the sun from the moon's apogee being 1 sig.——0 deg.——21′ the moon was farther distant from the earth than in the preceding opposition; whence it follows, that the tide must have been lower, as it was observed accordingly, the height of the fixed point above the level of the sea on the 10th of *April* being found 5 feet, 8 inches.

'Tis true, according to the common opinion, which supposes that the highest tides happen nearest the equinoxes, the tide must have been higher on the 26th of *March*, than on the 10th of *April*; but for the same reason, in the following full moon of the 22d of *April*, the distance from the equinox being increased, the tide should have been lower than on the 10th of *April*; whereas it was really higher by 1 foot, 1 inch, conformably to what should have been from the situation of the moon, which was farther from the earth on the 8th of *April*, than on the 22d: whence it appears, that the highest or lowest tides bear a nearer relation to the distance of the moon from the

 the

the earth, than to the diſtance of the ſun from the equinoxes.

For the eaſier making this compariſon, we have drawn up the following table; in the firſt column whereof are expreſſed the days and hours of the new and full moons; in the 2d, the time of high water obſerved at *Dunkirk*, on the days of new and full moon; in the 3d, the time of high water calculated according to the preceding rule; in the 4th, the height of the fixed point above the ſurface of the ſea at the time of high water; in the 5th, the ſun's diſtance from the apogee of the moon; in the 6th, the diſtance of the moon from the earth, at the time of new and full moon, with regard to the mean diſtance, which is ſuppoſed to be 100000 parts; in the 7th, the day of the higheſt tide; and in the 8th, the height of the fixed point above the ſurface of the ſea.

A TABLE of the times and heights of the

Time of new and full moons.					Time of high water found by observation.		Time of high water found by calculation.		Height to the fixed point.		
	1701.	H.	M.		H.	M.	H.	M.	F.	In.	L.
☉	24 Mar. at	8	36	M.	11	45	12	1	4	11	
●	8 Apr.	10	54	M.	12	21	11	56	5	11	
☉	22 Apr.	5	16	E.	11	44	11	43	5	3	
●	8 May,	1	42	M.	12	35	12	15	6	2	
☉	22 May,	2	18	M.	12	8	12	13	5	3	
●	6 June,	2	28	E.	11	50	11	49	6	6	
☉	20 June,	0	26	E.	11	43	11	53	6	2	
●	6 July,	0	58	M.	12	9	12	16	5	10	
☉	19 July,	11	50	E.	11	24	11	30	6	6	
●	4 Aug.	10	15	M.	11	48	11	57 ½	5	7	6
☉	18 Aug.	2	6	E.	12	2	11	50	5	10	
●	2 Sept.	6	5	E.	11	37	11	42	5	7	6
☉	17 Sept.	5	56	M.	12	24	12	6	6	1	
●	2 Oct.	2	20	M.	11	46	12	13	3	11	
☉	16 Oct.	11	24	E	11	42	11	31	6	5	
●	31 Oct.	11	24	M.	11	39	11	55	4	6	4
☉	15 Nov.	5	4	E.	12	0	11	44	5	10	
●	29 Nov.	10	11	E.	11	20 ½	11	33 ½	6	8	
☉	15 Dec.	10	16	M.	11	55	11	57	6	11	
●	29 Dec.	10	47	M.	11	51 ½	11	56	5	0	
	1702.										
☉	14 Jan.	1	12	M.	12	9	12	16	5	6	
●	28 Jan.	1	38	M.	11	46	12	15	5	9	
☉	12 Feb.	3	2	E.	11	32	11	48	6	2	
●	26 Feb.	6	15	E.	11	57	11	41	5	6	
☉	14 Mar.	1	48	M.	12	13 ½	12	14	5	6	
●	28 Mar.	11	7	M.	12	10 ½	11	56	5	10	
☉	12 Apr.	0	13	E.	11	45	11	53	4	3	
●	27 Apr.	3	49	M.	12	47	12	10	5	11	
☉	11 May,	4	59	E.	11	36	11	44	5	6	
●	26 May,	8	27	E.	11	47	11	37	6	10	

tides, in the new and full moons at Dunkirk.

Distance of the sea from the moon's apogee	Distance of the moon from the earth in conjunct. and Opposit.	Day of the highest tide.	Height of the water therein.
S. D. M.			F. In. L.
0 17 16	93778	26 March.	4 3
1 0 21	105589	10 April.	5 8
		24 April.	4 7
		11 May.	6 1
		23 May.	5 1
		7 June.	6 0
3 2 33	99713	21 June.	5 7 6
3 16 2	98372	7 July.	5 5 6
		22 July.	5 10
		6 August.	4 9 6
		22 August.	5 1 6
		6 September.	3 10 6
5 18 10	106340	19 September.	5 3
6 1 13	93460	2 October.	3 11
		17 October.	4 0
		30 October.	3 10 6
		16 November.	5 6
		30 November.	3 2
		17 and 18 Dec.	6 3
		30 December.	3 7
		1702.	
9 4 37	100013	14 January.	5 6
9 17 19	100477	30 January.	5 6
		13 February	4 6
		27 and 28 Feb.	3 3
		15 March.	4 6
		30 March	5 2
[illegible] 22 55	93519	15 April.	3 10 ½
0 5 52	106496	26 April.	5 4
		13 May.	4 4
		29 May.	6 4

 By

By this table it appears, that when the ſun's diſtance from the apogee of the moon is about 3 or 9 ſigns, the height of the water on the day of the higheſt tide, is nearly equal both in conjunctions and oppoſitions.

As to the loweſt tides, out of the new and full moons, they do not uſually happen in the quadratures, but 1, 2, or 3 days after; ſo that we may ſuppoſe them at a medium, to happen 2 days after the firſt and laſt quarter; as we likewiſe obſerve, that the higheſt tides commonly happen 2 days after the new and full moon.

The loweſt tide happened on the 8th of *Feb.* 1702, the high water being then 10 feet, 2 inches below the fixed point; and the higheſt tide, as already obſerved, was on the 30th of *Novem.* 1701, when the high water was 3 feet, 2 inches below the ſame point; ſo that the difference between the higheſt and loweſt tides at *Dunkirk* was 7 feet. ——— But what is further remarkable is, that the height of the tides, which happen in the quadratures, ſeems likewiſe to depend on the diſtance of the moon from the earth, the flood being found higher when the moon is near the earth, and lower when ſhe is farther diſtant from it; ſo as the height is much the ſame in the firſt and laſt quarter, when the moon is equally diſtant from the earth.

According to the theory of the moon, when the ſun is about three ſigns diſtant from the moon's apogee, the moon in her quarter is in perigee, and in her laſt quarter in apogee, and conſequently the flood ſhould be higher in the firſt quarter, and lower in the laſt. On the contrary, when the ſun is about 9 ſigns diſtant from the moon's apogee, the moon, if in her firſt quarter, is in apogee, and in her laſt in perigee ; conſequently high water muſt be lower in the firſt than in the laſt quarter ; and when the ſun is either in the moon's apogee, or perigee, the moon is at an equal diſtance from the earth, both in the firſt and laſt quarter, and conſequently the tides muſt be equal in each.

But the agreement between the heights of the tides, and the different diſtances of the moon from the earth in the quadratures, will be more eaſily obſerved, by means of the following table.

A TABLE of the times and heights of

Day and hour of the quadratures.						Time of high water by observation.	Time of high water by calculation.	Height of the fixed point.		
		1701.	H.	M.		H. M.	H. M.	F.	In.	L.
3	□	31 Mar. at	6	32	M.	5 36 E.	5 27	8	6	
1	□	16 Apr.	2	8	M.	5 40	5 36	9	3	6
3	□	29 Apr.	10	25	E.	4 44	4 55	8	1	6
1	□	15 May	9	6	M.	5 30	5 22	8	4	6
3	□	29 May	3	38	E.	5 26	5 9	8	7	
1	□	13 June	2	26	E.	5 7 ½	5 11 ½	7	7	4
3	□	28 June	9	14	M.	5 16 M.	5 22	8	7	4
1	□	12 July	7	17	E.	4 48 ½	5 2	6	6	6
3	□	28 July	2	23	M	5 20 ½	5 36	8	8	2
1	□	11 Aug.	1	14	M.	5 23	5 38	7	0	6
3	□	26 Aug.	6	6	E.	4 31	5 4	8	0	8
1	□	9 Sept.	9	20	M.	5 39	5 22	5	7	6
3	□	25 Sept.	7	58	M.	5 18	5 24	9	2	6
1	□	8 Oct.	8	47	E.	4 51	4 59	7	0	
3	□	24 Oct.	7	50	E.	4 43	5 1	8	5	6
1	□	7 Nov.	0	17	E.	5 34	5 16	6	11	
3	□	23 Nov.	6	0	M.	5 24 ½	5 28	7	5	
1	□	7 Dec.	7	31	M	5 58	5 25	7	5	
3	□	22 Dec.	8	48	E.	5 15 ½	4 59	7	6	8
		1702.								
1	□	6 Jan.	5	47	M.	5 51	5 29	8	4	6
3	□	20 Jan.	10	41	E.	4 49	4 55	7	8	
1	□	5 Feb.	2	34	M.	5 18	5 35 ½	7	8	
3	□	19 Feb.	6	33	M.	5 23	5 27	6	2	
1	□	6 Mar.	10	24	E.	4 33 ½	4 55	9	2	
3	□	20 Mar.	3	47	E.	4 35	5 9	7	10	
1	□	5 Apr.	2	9	E.	4 57	5 12	5	2	
3	□	19 Apr.	3	39	M.	5 27 ½	5 33	8	2	
1	□	5 May	1	59	M.	5 45 ½	5 36 ½	9	3	3
3	□	18 May	4	4	E.	5 17	5 10	8	4	

the tides in the quadratures at Dunkirk.

Distance of the sun from the moon's apogee.			Moon's distance from the earth in the quadratures.	Time of lowest water.	Time of the lowest tide.	Height of the fixed point above the water.		
S.	D.	M.				F.	In.	L.
					2 and 3 April.	9	2	
					17 April.	9	5	
					1 May.	9	10	
					16 May.	8	8	
					30 May.	9	4	
2	26	40	97615		14 June.	7	11	2
3	9	17	106250		30 June.	9	3	
					15 July.	8	1	6
					29 July.	9	0	
					15 August.	8	6	
					29 August.	9	3	
					11 September.	8	7	
5	25	8	102165		27 September.	10	1	
6	6	30	102275		11 October.	9	7	
					26 October.	9	5	
					9 November.	9	1	
					26 November.	8	4	6
					8 December.	9	6	6
					23 December	7	11	
8	28	20	106425		5 January.	8	5	
9	10	2	97717		20 January.	7	8	
					8 February.	10	2	
					21 February.	8	7	
					8 March	10	1	
					22 March.	9	7	
					8 April.	8	10	
11	28	56	101725		20 April.	9	3	
0	12	40	100856		5 May.	9	3	3
					19 May.	9	1	3

If now we consider the retardation of the tides, from day to day, we shall find it liable to several irregularities, there being a retardation of 1^h—54′, between the 2d and 3d of *April*, 1701, and an anticipation of 30′, between the 15th and 16th of *October*; so that it would be difficult to give rules for finding the time of high water daily at *Dunkirk*, within a few minutes of truth, as we have done for the days of new and full moon.

Our first enquiry was, whether those irregularities bore any analogy to those of the true motion of the moon, which gains or loses, with regard to the mean motion; but finding, that they were frequently a contrary way, I have been obliged to look elsewhere for the causes of such variations. — In order hereto, we have compared the times of high water, observed on the days of the quadratures, and find, that on the day of the first and last quarter of the moon high water happens at *Dunkirk*, nearly about the same time as we had before observed, that high water happens nearly at the same time in the new and full moons.

Among the 29 observations made at the quadratures, that, wherein the flood was most accelerated, happened on the 26th of *August*, 1701, at 4^h——31′; and that wherein it was most retarded, on the 7th of *December*, 1701, at 5^h—58′: so that there is a variation of 1^h——27′, in the time of high water at the quadratures, which is greater by 1′, than that observed at new and full moons.

To assign some rule for this variation, we suppose, that the mean time of high water in the quadratures happens at *Dunkirk* at 5^h—6′ in the evening, and to or from this time add, or substract 2′ for every hour, which the time of the quadrature,

quadrature, expreſſed in the almanack, anticipates or comes behind this mean time of high water.——For an inſtance on the 31ſt of *March*, 1701, the day of the quadrature, high water was obſerved at *Dunkirk* at 5h——36′ in the evening. Now the laſt quarter of the moon is fixed for that day at 6 ——32 in the morning, by the almanack; and the difference between 6 —32′ in the morning, and 5 —6′ in the evening, the mean time of high water in the quadratures is 10 —34′; to which, at the rate of 2′ *per* hour, anſwer 21′, which added to 5h—6′, give 5h—27′ for the time of high water on the 31ſt of *March*, 1701, which is within 9′ of the time obſerved.

The mean time of high water at *Dunkirk*, in the new and full moons, being at 11h——54′ in the morning, and in the quadratures at 11 --6′, we have 5h—12′ for the interval between the times of the tides from the new and full moons to the quadratures, which is much leſs than that of the quadratures, to the new and full moons; accordingly a greater retardation, from one day to another, is obſerved in the tides ſucceeding the quadratures, than in thoſe which ſucceed the new and full moons; the cauſe whereof may be attributed to this, that the tides being lower about the quadratures, than about the full moons, the ſea, which grows higher every day, as it approaches the new or full moon, ſpends more time in ſurpaſſing the height of the preceding day; whereas, from the new and full moons to the quadratures, the ſea finding no obſtacle, but being aſſiſted by its own weight, deſcends with the greater velocity, and conſequently renders the intervals between the tides ſhorter.

After aſcertaining the time of high water in the new and full moons, and quadratures, we conſi-

 dered

dered anew all the obſervations made at *Dunkirk*, during the ſpace of 14 months, and determined the mean time of high water for ever, both after new and full moon, and after the quadratures. We alſo formed rules of the variations they are liable to, with regard to the times of the new and full moons, and quadratures preceding the given day.

By theſe rules, among 434 obſervations related by M. *Baert*, there are only two wherein the time of high water, determined by the rule, is 54 minutes different from the time obſerved. This difference will not appear very great, conſidering what a number of irregularities may occur in the obſervations, there being ſometimes a doubt of a whole hour in determining the time of high water, as was obſerved on the 27th of *February*, 1702; when it was firſt found high water at 0^h—$8'$ in the evening. After which the ſea ſunk ſome inches, but roſe again at 0^h—$57'$ to the ſame height; it had been at $49'$ before, where it remained till 1^h—$10'$; and M. *Baert* fixed the time of high water for this day at 1^h—$7'\frac{1}{2}$.

For the eaſier finding the time of high water on any given day, we preſent the following table of the retardation of the tides, both after the new and full moons, and after the quadratures; the tides are here laid down for every 12 a clock, for the conveniency of finding the morning and evening tides. ——— By means of this table, and of the rules ſubjoined, the true time of high water may be found at *Dunkirk* for any given day, which may be of ſervice to pilots for chuſing the moſt proper times to enter, or come out of that port.

11^h——

h ′ 11——54 mean time of high water at *Dunkirk*, on the day of new and full moon.	h ′ 5——6 mean time of high water at *Dunkirk*, on the days of the quadratures.

A TABLE of the retardation of the tides.

Day and hour after new or full moon.		Retardation of the tides		Dif.	Day and hour after first or last quarter.		Retardation of the tides		Dif.
D.	H.	H.	M.	M.	D	H.	D.	H.	M
0	0	0	0	26	0	0	0	0	32
	12	0	26	24		12	0	32	36
1	0	0	50		1	0	1	8	41
	12	1	11	21		12	1	49	
2	0	1	30	19	2	0	2	32	43
	12	1	48	18		12	3	11	39
3	0	2	6	18	3	0	3	44	33
	12	2	24	14		12	4	14	30
4	0	2	42	18	4	0	4	40	26
	12	3	1	19		12	5	4	24
5	0	3	21	20	5	0	5	28	24
	12	3	41	20		12	5	50	22
6	0	4	2	21	6	0	6	12	22
	12	4	21	19		12	6	34	22
7	0	4	39	18	7	0	6	54	20

Rule firſt, To find the time of high water at *Dunkirk* for the days of new and full moon, and of the quadratures.

Find in the almanack the time of new or full moon, and of the quadratures, and take the difference between this and the mean time of high water, expreſſed for the day of that phaſis, the double of this difference will be the number of minutes to be added to the mean time of high water, in caſe the time of the phaſis anticipate the mean time of high water, or, on the contrary, to be ſubſtracted in caſe ſuch time come after that of high water, the reſult whereof will be the true time of high water required.

For an inſtance, firſt, ſuppoſe the time of high water required for the day of full moon in *April*, 1701.

Full moon by the almanack falls on the 22d of *April*, at 5—16′ in the evening, the difference between this 5—16′ in the evening, and 11^h—54′ in the morning, the mean time of high water in the new and full moons at *Dunkirk*, as laid down in the table, is 5—22′; the double whereof, *viz.* 10′—44″ is the number of minutes to be ſubſtracted from 11^h—54, on account of the full moon's coming after the time of high water, the remainder is 11^h—43′, the true time of high water in the morning of the 22d of *April*. M. *Baert* obſerved it that day at 11^h—44′.

For a ſecond inſtance, ſuppoſe the time of high water required for the day of the firſt quadrature of the moon in *April*, 1701.

The almanack fixes the firſt quadrature of the moon to the 16th of *April*, at 2^h—8′ in the morning, the difference between which time and 5—6′ in the evening, the mean time of high water in the quadratures at *Dunkirk* is 14^h—58′, whoſe double 30 is the number of min. to be added to 5^h—6′, by reaſon the time of quadrature anticipates the mean time of high water. The ſum, *viz.* 5^h—36′ in the evening, gives the true time of high water on the 16th of *April*. M. *Baert* found it that day at 5^h—40′.

Second rule to find the time of high water at *Dunkirk*, for any given day.

Find by the firſt rule the time of high water, the day of full or new moon, or of one of the qua-

quadratures, immediately preceding the given day; to this add the retardation of the tides, correſponding to the difference between the given day, and the day of the preceding phaſis, the ſum will be the time of high water for the day required. ——— To find the time of high water immediately preceding, or following that now found, we muſt ſubſtract or add the difference correſponding to 12 .

For an inſtance, ſuppoſe the hour of high water required for the 26th of *March*, 1701.

In the almanack, we find that the *phaſis* immediately preceding the 26th of *March*, is full moon, which happens on the 24th of *March*, at 8^h—36 in the morning, the difference between 8^h—$36'$ in the morning, and 11^h—$54'$ the mean time of high water at *Dunkirk*, is $3'$—$18''$; the double whereof $6'$—$36''$, being added to 11^h—54. gives the time of high water at 12 —$1'$, on the 24th of *March*, the day of full moon. To this add, 1^h—$30''$, the retardation of the tides correſponding to two days after full moon, the reſult gives the time of high water, on the 26th of *March*, 1701, at 1 —$31'$ in the evening, the very ſame as was obſerved by M. *Baert*. ——— To find the time of high water, which happened in the morning, and the ſame day, take the difference between 1^h—$30'$ and 1 —$14'$, *viz.* $19'$, which ſubſtracted from 1^h——$31'$, the time of high water in the evening, gives 1^h—$12'$ for the true time of high water in the morning.

For a ſecond inſtance, ſuppoſe the time of high water required for the 6th of *April*, 1701.

By the almanack we learn, that the third quadrature of the moon, which is the phaſis immediately preceding the given day, happened on the 31ſt of *March*, 1701, at 6 —$32'$ in the morning,

ing, the difference between this 6^h—$32'$ in the morning, and 5^h—$6'$ in the evening, the mean time of high water at *Dunkirk*, in the quadratures, is 10^h—$34'$; whose double $21'$—$8''$ being added to 5 —$6'$, by reason the time of the 3d quadrature comes before 5^h—$6'$, gives the time of high water at *Dunkirk*. On the 31st of *March*, 1701, the day of the last quadrature, at 5^h—$27'$ in the evening, the difference between the 31st of *Mar.* the day of the third quadrature, and the 6th of *April*, the day given, is 6 days the corresponding retardation, to which in the table is 6^h—$12'$, which added to 5^h—$27'$, gives the time of high water at *Dunkirk*, for the 6th of *April*, 1701, at 11^h—$39'$ in the evening. ——— To find the time of high water, which happened in the morning, take the difference between 6^h—$12'$, and 5^h—$50'$, *viz.* $22'$, which substracted from 11^h—$39'$, the time of high water on the 6th of *April*, in the evening, gives 11^h—$17'$, for the true time of high water in the morning of the same day. M. *Baert* found it 11 —$21'$ this morning.

Third rule, to find in any given month the times of the highest tides, most proper for entering or coming out of the port of *Dunkirk*.

Find, by the preceding rule, the time of high water for the 2d days after the new and full moons of that month, and you will have the time required.

For an instance, the full moon of the month of *March*, happening on the 24th at 8 —$36'$ in the morning, we seek by the 2d rule the time of high water on the 26th of *March*, which, in the instance there given, is found at 1 —$31'$ in the evening. M. *Baert* observed it high water this day at 1^h—$31'$ in the evening; and the tide

tide was higher than in any of the preceding or following days.

Fourth rule, to find the day and hour of the highest tide, which will happen in any given month.

By the astronomical tables, take the moon's diameter for the day of new and full moon; if this diameter be greater on the day of the new than of the full moon, the tide will be highest this month 2 days after the new moon; but if the moon's diameter be greater on the day of full than of new moon, the tide will be highest this month 2 days after the full moon.

For an instance, suppose the highest tide required in the month of *April*, 1701——

By the table, we find the diameter of the moon, on the 8th of *April*, the day of new moon, to be 14′——53″, and on the 22d of *April*, the day of full moon, to be 16′——24″, consequently the highest tide in this month will be on the 24th, which agrees with M. *Baert's* observation.

Fifth rule, to find the day and hour of the lowest tide, which will happen in any given month.

Find, by the astronomical tables, the moon's diameter for the day of the first and last quadrature, if this diameter be smaller on the day of the first quadrature, the least tide this month will be 2 days after the first quadrature. On the contrary, if the moon's diameter be the least on the day of the last quadrature, the lowest tide that month will be 2 days after the last quadrature.

For an instance, suppose the least tide required for the month of *June*, 1701.

By the tables, we find the diameter of the moon, on the 13th of *June*, the day of the first quadrature, to be 16′——6″, and on the 28th of

June, the day of the laſt quadrature, to be 14′——47″, conſequently the ſmalleſt tide in the month of *June,* 1701, muſt have happened on the 30th of that month, agreeably to the obſervations of M. *Baert.*

IX. *Obſervations on a kind of talc, commonly found near* Paris, *over the banks of plaſter-ſtones, by M.* de la Hire *; *tranſlated by Mr.* Chambers.

One of the moſt curious among tranſparent ſtones, and that which may give moſt employment to the naturaliſts to account for its effects is what we commonly call *iſland cryſtal.* 'Tis extremely tranſparent, and clearer even than the fineſt glaſs; but might be more properly called a talc than a cryſtal, for the reaſons alledged hereafter. Its diſcovery we owe to *Eraſmus Bartholin,* a celebrated *Daniſh* mathematician, who firſt laid it before the publick, in a treatiſe upon the ſubject, printed in 1670. M. *Huygens* has alſo been very large on the properties of this ſtone, in his treatiſe of light, printed in 1690.

Having two large pieces of this ſtone in my poſſeſſion, I was willing to examine it, by ſeveral experiments, and in different manners, both for my own ſatisfaction, and for the aſcertaining of what thoſe gentlemen have ſaid of it. It ought, as already hinted, to be called a talc, rather than a cryſtal, it being one of its chief properties to cleave readily every way, but ſtill parallel to one of thoſe 6 phaſes, whereof its figure conſiſts, which is always an oblique angled parallelipiped, and conſequently its fragments will

* July 19, 1710.

all

all be parallelipipeds, whose 8 solid angles are placed similarly in the smallest pieces, as in the largest.

The 6 faces, whereof it is formed, are oblique angled parallelograms, whose two opposite obtuse angles are each 101 degrees, 30′, and consequently the two others being the complements thereof, must be each 78°——30′: this I have learnt by my observations.

In this parallelipiped are only 2 solid angles, which are opposite to each other, and formed by 3 of the obtuse angles of the faces; the other 6 are each comprehended between one obtuse angle and two acute ones, there being in all 12 equal obtuse angles, and as many equal acute ones.

The inclinations of the faces make two kinds of angles; 6 whereof are obtuse, comprehending each 105 degrees, and 6 acute ones of 75° each, which are the complements of the former.——These measures are somewhat different from those of Mess. *Bartholin* and *Huygens*, which may arise from the difficulty of making exact observations thereof, by reason the acute angles are not so well defined as the obtuse ones.

Thus much for the figure of the stone.——But what is more remarkable in it is, that it represents all the objects seen thro' two of its parallel faces double; the distance between the two images appearing so much the greater, as the faces are further distant from each other, or the crystal thicker. This *phænomenon* is the most sensible, when the object is a black point, or a line drawn on the face of the stone.

The doubling of the object, however, is not the only thing to be considered in this stone; but the manner wherein this is done, which is always in the line passing thro' the object, which is parallel to that whereby the obtufe angle of the face the object is upon is bifected.

This double image of the fame object fhews, that there muft be a double refraction in thefe bodies, and accordingly two very different ones have been diftinctly obferved. The firft, common to that found in all tranfparent bodies, and depending on the inclination of the incident ray, to the line perpendicular to the face of the body when the refraction is made. The fecond, peculiar to this cryftal, and arifing from another inclination of the incident ray, to another line inclined to the fame face. —— Hence it follows, that if the incident ray be united with one of thefe lines, it will not undergo the refraction belonging to that line, but will undergo that depending on the other; and confequently the image will always be double in every other inclination.

I have made feveral experiments, and have likewife repeated them feveral ways; the refult whereof is, that in the firft of the two refractions, the fine of the angle of incidence in the air, is to the fine of the angle refracted in the body as 5 to 3; whence we learn, that this refraction of the body, notwithftanding its foftnefs, furpaffes that of the glafs, which is only as 4 $\frac{1}{2}$ to 3.

As to the 2d refraction which is peculiar to this body, and makes the object double, *Bartholin* takes it to depend upon a line, or ray, always parallel to the edges of the faces next thofe where the refraction is made; but *Huygens* denies this line to be parallel to thofe edges; for myfelf, after examining the point with great attention, I find this line more perpendicular to the furface of the cryftal by 1°, which is no great matter in an enquiry of this kind: further I find the fines of the angles

angles of incidence in the air with regard to this line; and in this 2d refraction to be to the ſines of the refracted angles nearly as 4 ½ to 3, which is much like that of glaſs.

'Tis obſervable, that the image produced by the ſecond refraction, always appears lower than that produced by the firſt; the reaſon whereof is eaſily aſſigned from the laws of dioptricks, as alſo why each of the two images only appears with ½ the ſtrength it would have, if viewed without the interpoſition of any other body. Hence it is, that when the parts of the two images cover each other, as will happen in a certain ſituation to a black ſtroke upon the cryſtal, this part will appear twice as ſtrong as any where elſe.

My examination of the Iſland talc led me to conſider that found in this country over the banks of plaſter-ſtone; for we muſt not neglect what is committed unto us, and which would appear curious in a foreign country to beſtow all our attention upon what comes afar.

This plaſter talc is a tranſparent ſtone which bears a near reſemblance to that brought from the *Levant*, except in point of figure, which is very ſingular, and is conſtantly the ſame in all the pieces we have ſeen; its relation to the real talc conſiſts in its cleaving readily into thin leaves, or *lamellæ*, equally tranſparent with thoſe of common talc, but ſmaller, and more brittle.

We meet with ſtore of pieces of this ſtone, of a moderate bulk, in a *ſtratum* of white fatty earth, over the blocks of ſtone whereof the plaſter of *Paris* is made; they are diſtributed thro' this earth, wherein it is known they are formed without any order or uniformity, being thrown as it were at random, and ſeveral almoſt joining one to

 another

another with the intervention only of a little of the fatty earth.

The figure * of this talc reſembles the barbed point of an arrow, as appears by ABCD which repreſents one of its faces; for there are always two parallel to each other, according to which the ſtone cleaves into leaves; and one of theſe faces is bigger than the other. We find pieces from 12 to 15 inches long, all forked at the broad end as in CAD, the other end B terminating in a point; the pieces in thickneſs of a moderate ſize, are about 1 inch: thro' the 2 parallel faces we perceive objects very clearly, at leaſt in the white pieces; for there are ſome yellow and browniſh ones which are but little tranſparent.

Each piece is naturally divided into two lengthways, as appears by the right line AB, proceding from the cleft A to the point B; and the plane which parts them is perpendicular to the faces; but the two pieces are uſually united, being only diſtinguiſhed from each other by the inequality of the ſubſtance found in this part, where there is ſometimes likewiſe found, a little of the earth wherein the talc is formed: in ſome parts of it we alſo find a hard ſtony kind of cruſt.

The ſides which terminate this ſtone, do not uſually make right angles with the faces; but an acute angle of 75 degrees on the broad ſide of the face, and its complement on the other; and hence it is, that the two faces are not of the ſame bigneſs in every piece: the ſides naturally are not ſmooth and poliſhed, being only formed of the extremities of the ſeveral *laminæ*, which are always covered with a thin yellowiſh cruſt; and

* Plate VI. fig. 9.

hence

hence objects only appear very confufedly thro' thefe fides unlefs the cruft be removed, and a varnifh laid over, which is not eafy to execute, by reafon of the fmall connection between the *laminæ*.

One of the points of the fork is fometimes found a little feparate from its piece, being only joined irregularly thereto, by a little of the fatty earth; and upon feparating them quite, we find that thefe points only adhered to the reft by pieces of *laminæ*, about a line thick, which enter more or lefs into the body of the ftone, and make bond as the mafons call it therewith.

Upon removing fome of the rough *lamellæ* on the furface of this talc, we clearly difcern lines therein, as E F proceeding from the middle line A B, towards the edges on either fide; and making an acute angle A E F, with the fame middle line towards the fork A, of about 60°: we alfo perceive other lines as G H proceeding from the middle towards the edges, and making an acute angle B G H towards the point B of 50°, fo that the acute angle formed at the meeting of thofe 2 lines is 70°.

Hence it always happens, that upon cleaving the talc into thin pieces, which can only be done with a fharp knife, beginning at the exterior edges after firft removing the cruft: moft of thefe *laminæ* break into triangles, whofe angles are conftantly 50, 60, and 70 degrees, which is a very fingular property of this ftone. We alfo find certain fragments of thofe thin *lamellæ*, in figure of a parallellogram compofed of 2 of thefe triangles joyned together.

Hence we may probably infer, that the mafs of thefe talc ftones, confifts only of thin *lamellæ* flenderly faftened to each other, each whereof is formed

formed of little triangular *lamellæ*, as the elements thereof which are ſtrongly faſtened to each other at their edges, whence they have a conſiderable firmneſs; each of which little elementary triangles has 3 unequal acute angles; *viz.* of 50, 60, and 70°, as appears from the pieces of broken *lamellæ*, which are only aſſemblages of the ſame elementary triangles, and form triangles like their elements; for theſe *lamellæ* are very brittle, and yet afford the ſame angle, when, or howſoever broken.

If the ſides of theſe elementary triangles do not make a right angle with their face, but an angle of 75 degrees on one ſide, and its complement on the other, which however is more than can be obſerved, it would likewiſe follow, that upon joining together in the ſame order, the whole ſide of a piece formed by them, would have this inclination to the face, which is eaſily obſerved.

From the difference of the angles in the elementary triangles, it will likewiſe follow, that according to their ſeveral arangements in forming the *lamellæ*, the ſides of thoſe *lamellæ* will either be parallel to the line in the middle, or inclined thereto in 10°, which alſo forms the point of the piece; the faces whereof are always inclined 10° to the middle line on either ſide, when they are inclined at all, which happens almoſt univerſally; for the angle AEF being conſtantly 60°, and the angle BGH, or BEI, or BEK 50°, the angle FEI or FEK, will neceſſarily be 70°; and if the triangle FEI, whoſe angle FEI ſhould be 70°, have its angle EFI 60°, and conſequently the other EIF of 50, it will follow, that the ſide FI will be parallel to AB; but if the angle EFI, or EFK be 50 degrees, and the other EKF 60, the line FK will make an angle with

with the middle an angle of 10°, which we uſually find accordingly. Theſe 2 caſes may happen in the firſt formation of the leaves by the triangles, as FEK taking an inverted ſituation, the angle in E ſtill remaining the ſame; and as we may ſuppoſe, that before the forming of theſe leaves, their elementary triangles floated in a liquid ſubſtance; by the motion whereof they were ranged aſide of each other, in a certain order, agreeable to their figure; whence it happened, that the ſides of the *lamellæ* might become inclined to each other, in an angle of 10 degrees; for I only here conſider ½ the intire *lamellæ* which is always divided into two by a line, as AB; but if in ſuch formation of the *lamellæ*, one of them by any accident happened to take a different poſition, the reſt adapting themſelves thereto by the motion of the fluid, formed the ſides of the *lamellæ* parallel to each other.

It was in this formation of the *lamellæ*, that they acquired their hardneſs, which became pretty conſiderable by their elements joining to each other at their ſides; but the *lamellæ* having ſtill a liquid matter between them, which could only be drained off in time, it hence happened that they did not adhere any thing conſiderably to each other by their ſurfaces; ſo that if there be the leaſt foreign matter left between them, they will always be eaſier to ſeparate from each other than to be broke a-croſs.

As to the fork CAD, its formation ſeems to be as follows, the angle formed by each horn as ACH or ADH is uſually 50° which is the ſmalleſt of the 3 angles of the element, and if the exterior ſide of the piece make an angle with the middle line of 10° towards the point, it follows, that the angle of the fork CAD muſt be 120 degrees,

grees, which is very near what we actually find in ſome pieces of this talc.———Now if any foreign body have been found about A to hinder the 2 elementary triangles, which ſhould have been diſpoſed therein from uniting to thoſe of the ſides (ſomething of which kind we actually find in the diſunion of the horns, from the body of the talc by a piece of earth as abovementioned) in this caſe, the connection between the *laminæ* being interrupted, the reſt muſt have continued forming, and terminate at length in the point of the horn, by lines parallel on one ſide to EF, and on the other in AC and AD; for the natural figure of the elementary triangles in joining together, will always form triangles ſimilar to the elements.

What has hitherto been obſerved of the quantities of the angles in the talc, is only what obtains in the general, there being ſeveral irregularities found therein, occaſioned in the formation by foreign bodies, which diverting the elementary triangles, have made them aſſume external figures different from what would naturally have aroſe from the aſſemblage of elements; yet without any ſuch thing being perceivable in the body, on account of the ſmallneſs of thoſe elements, as we find by ſome of the ſides which are a little crooked, and by certain angles which are leſs or greater than thoſe of the elements, in which caſe theſe ſides muſt have little dentures; ſome whereof are perceivable in the irregular fractures of the *lamellæ*: in fine, we find ſome pieces of talc which have others faſtened on their ſides; in others the point is extended into a paralellipiped only on one ſide, and towards the point of others, we find another piece form'd as uſual, but oppoſite to the firſt, with a thouſand other varieties which are as it were the *luſus* of this formation.

After

After examining the figure of this talc, I applied my ſelf to the obſervation of its refractions. Theſe, I firſt conſidered between the 2 parallel faces, the only way wherein the ſtone is naturally tranſparent; and then in planes perpendicular to thoſe faces, as is uſually done in meaſuring the refraction. Then in all the other directions, as lengthwiſe from the middle of the point, towards the fork; then ſide-wiſe, breadth-wiſe, perpendicularly to the middle line, *&c.* and every where under all the different angles of inclination, found the ſine of the angle of incidence in the air, to the ſine of the angle refracted in the body as 5 to 3 $\frac{1}{3}$, which is the ſame as that from air into glaſs; and the ſame likewiſe with that peculiar to *Iſland* cryſtal, which deſerves a ſpecial attention; laſtly, ſeparating a piece of talc into two, by the plane which divides its length, and is perpendicular to the faces, I examined what the refraction would be a-croſs the thickneſs of its ſide, ſuch refraction being made in a plane parallel to the faces, which is impracticable while the two halfs are joyned together, both by reaſon of the too great thickneſs, and of the foulneſs of the middle part where the ſeparation is; but having cleanſed this part, and ſmeared it over with a little gum-water, as alſo the outer edge which is commonly rough, till a black body might be diſcerned thro' it, I found, that the refraction this way was the ſame as before; *viz.* as 5 to 3 $\frac{1}{3}$.

But being ſcarce ſatisfied with all theſe obſervations, I was further willing to know whether the clefts or flaws, perceived on the ſide of this ſtone, might not produce ſome particular effect; to make which the more apparent, I applied an iron wire lengthwiſe over theſe clefts, and looking thro' the talc, found its image appear in two different places, or at leaſt much larger than it really was, with a clear ſpace between the two; then moving the wire gently, but ſtill in its former direction, I perceived the image jump, as it were, from one place to another, but ſtill double; to render theſe obſervations the more conſpicuous, by reaſon the talc is very dim when viewed ſideways, it muſt be held near the light of a candle, and the iron wier applied full upon it.

Here it will be required to produce phyſical reaſons of all theſe effects, not only of the talc, but of *Iſland* cryſtal, which it ſo nearly reſembles; from whence light might perhaps be let into moſt other tranſparent bodies, as diamond rock, cryſtal, allom, *&c.* which are all natural productions; and in all appearance are formed of an aſſemblage of elements, ſimilar to each other, which determine their figure: but this I reſerve for another memoir.

At *Paſſy* near *Paris*, round the mineral ſpring, are likewiſe found little pieces of talc, of the ſame ſpecies as that of the plaſter quarries, being fiſſile like them into thin *lamellæ*; 'tis very clear and tranſparent, and ſenſibly formed of the ſame triangular elements, as that of plaſter; but the figure of its two parallel faces, according to which it cleaves, is a parallelogram with two acute angles of 50 degrees each. Its ſides make an angle, with the faces of 125 degrees on each ſide, tho' 'tis difficult to meaſure them exactly, by reaſon the ſides are not ſmooth, as being only formed of the extremities of *lamellæ*, which leaves ſeveral inequalities along the ſides.

What is moſt remarkable in this talc, is a prominent angle, of about 110°, which it makes about the middle of its thickneſs on either ſide;

ſide; ſo that its figure would be a parallelipiped, with 6 faces, provided its two ends, or baſes, were plain; but they alſo make a prominant angle about the middle of 140 degrees.

As to the refraction of this talc, I have not been able to find the exact quantity thereof, by reaſon the pieces are too ſmall; nor do I find, that objects appear double through its parallel faces.

Sir *Iſaac Newton* gives his obſervations upon *iſland* cryſtal, with an abſtruſe ſort of ſolution of its effects in his opticks.

An Explanation *of the* Terms of Art *used in this volume, which were not explained at the end of the former volumes.*

A

ALbidade, is an *Arabic* word, used to express a moveable rule applied to an instrument for observing heights and lengths. It is also called a *diopter*.

Apogeum, or *apogee*, is a point in the heavens, in which the sun, moon, or any planet, is at its greatest possible distance from the earth.

Areometer is an instrument used to measure the density or gravity of fluids. That which is used by the royal academy of sciences at *Paris*, is a glass bottle balanced with quicksilver, having a very narrow neck, divided all along into equal parts. It is immerged in liquors, which they would compare together, and the weight of them is determined by the degree to which the areometer sinks; that in which it sinks most being the lightest liquor.

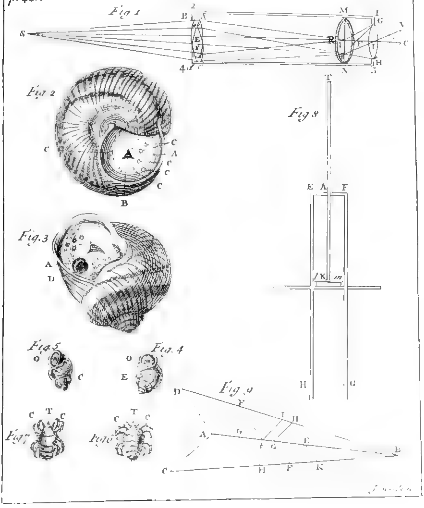
B

Brace, a measure of 5 *Paris* feet and 4 inches, or about 5 feet, 7 inches ½ of our measure.

C

Caliber, the bore or width of a fire-arm, or the diameter of its mouth, or of the ball that it carries.

Chamber, or *fourneau*, or *furnace* of a mine, is that part in which the powder is placed. The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cavity of it is about 5 or 6 cubical feet, and is charged with about 1000 *lb.* of powder, or leſs, according to the earth that is to be raiſed.

Choroides, the inner coat of the eye.

Cornea, one of the coats of the eye, ſo called, becauſe it is tranſparent like horn.

D

Diaſtole, the motion of the heart, by which it dilates itſelf: it is oppoſite to the *ſyſtole*, by which it contracts itſelf.

E

Eolipyle, a hydraulic inſtrument, conſiſting of a hollow metalline ball, with a ſlender neck or pipe, having a very ſmall aperture. It is heated red hot, in order to rarefy the air, and then thrown into water. There will enter as much water into it, as may ſerve to fill the *vacuum* left by the air condenſed by the coldneſs of the water. It is then ſet before the fire again, and the air ruſhes out with a ſurpriſing impetuoſity, and for a conſiderable time.

F

Fougade, or *fougaſſe*, is a little *fourneau*, or chamber of a mine made in form of a well, 8 or 10 feet broad, and 10 or 12 deep, which is digged under ſome work intended to be blown up. It is charged with ſacks or barrels of gunpowder, and is ſet fire to like other mines with a ſauciſſe.

Fourneau of a mine, *ſee* chamber.

M

Mortiſe, or *mortoiſe*, an inciſion into the thickneſs of a piece of wood, which is to receive another piece called a tenon.

O

Os hyoides, a bone ſo called, becauſe it reſembles the *Greek* letter Y. It lies at the root of the tongue.

P

Perigeum, or perigee, is a point of the heavens, in which the ſun, moon, or any planet is at its leaſt poſſible diſtance from the earth.

Pia mater, a thin and delicate double membrane, which lies under the *dura mater*, and immediately covers the ſubſtance of the brain.

S

Sauciſſe, or ſauſage, is a little roll of pitched cloth, 2 inches in diameter, filled with good powder, and having a ſlow fuſee faſtened to it. It reaches quite to the chamber of the mine, and is uſed to ſet it on fire.

Syſtole, that motion of the heart, by which it contracts itſelf. It is oppoſite to the *diaſtole*, by which it dilates itſelf.

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