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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,  
By JOHN MARTYN, F. R. S.  
Professor of Botany in the University of Cambridge;

AND

EPHRAIM CHAMBERS, F. R. S.  
Author of the Universal Dictionary of Arts and Sciences.

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FROM 1720 TO 1780.  
WITH MANY CURIOUS AND INTERESTING NOTES  
ON THE HISTORY AND ANATOMY OF MAN, &c.

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A  
T A B L E

O F T H E

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.

- I. **O**N the light of bodies produced by friction.
- II. **O**N fire arms differently charged.
- III. On stones, and particularly on those of the sea.
- IV. Of an extraordinary cure performed by a concert of music.
- V. Of the multiplication of animalcules.
- VI. Of the circulation of the blood in insects.
- VII. Of worms voided by stool.
- VIII. Of the Iguana, an American lizard.
- IX. Of the difference of the milk of European women, and Negresses at Batavia.
- X. Of an aurora borealis seen at Berlin.
- XI. Of a new island near Santerini.
- XII. Of a new way of constructing the map of a country.

In the MEMOIRS.

- 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.
- 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.*
- III. *Of the irregularities of the apparent depression of the horizon of the sea, by M. Cassini.*
- IV. *Observations on spiders, by M. Homberg.*
- V. *Of the effects of gunpowder, chiefly in mines, by M. Chevalier.*
- VI. *A new construction of sluices, by M. de la Hire.*

A N  
A B R I D G M E N T

O F T H E

Philosophical Discoveries

A N D

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

I N T H E

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.*

**T**HE 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 privilege, 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, whercon 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 *ceteris 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 *Caieux*, 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 *Bonneuil*, *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 consistence.

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,

8 *The HISTORY and MEMOIRS of the*

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 *Tressport*, 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  $\frac{1}{2}$  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 chalk, 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 microscope, 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 fallads, 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 *folium*. 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. *Of the Iguana, an American lizard.*

The *iguana* is a kind of lizard found through all parts of *America*, it is described in *Piso's* book, *De utriusque Indiae re naturali & medica*. It is amphibious, it has two stomachs, in one of which there is often a stone, white on the outside, and the inside very much like the colour of the *American* bezoar. It has the virtue of expelling the stone and gravel in the kidneys, and cures the suppression of urine. It is administered in very fine powder, with an equal quantity of the powder of nut-shells, both together weighing a dram, in orange-flower water, if there is no fever, or suspicion of an inflammation in the ureters, or in the bladder; in which case it must be given in white wine, mixed with parsley water, or pellitory of the wall, or some other diuretick. It sometimes has effect in an hour's time, but at most, in three hours. A *Spanish* physician of *Caraccas*, having sent this account to M. *DePas*, a physician of *Montpellier*, who is with M. *DesLandes*, director of the *Assiento* company in *America*, and having related to him many experiments which he had made with the stone of the *iguana*, this letter has been sent to the academy.

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

M. *Hemberg* says, that *European* women who go to *Batavia*, cannot suckle their children, their milk being so salt that they will not take it; whereas, the milk of the *Negresses*, though their diet is the same, is sweet and pleasant as usual; therefore they suckle the children of the *Dutch*

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 filtering 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'Ablé 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

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 Chastucil 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 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 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 where 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 concentric 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 a leagues distance at most from

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 imperceptible in a little tract of land, will be less so if the work is performed in a tract of less latitude,

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 geometer 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.

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O F T H E

Philosophical Memoirs

O F T H E

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 these observations, which were made here during the preceding year, in all the same circumstances, and in the same manner as those of the foregoing years. The height of water which fell at the observatory, was in

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

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 considered, which commonly used to be between 19 and 20 inches: but it must be looked upon as one of the wettest years, if it is considered that the greatest rains commonly happen in the months of *July* and *August*, with storms, and that this year it did not rain in both these months together much more than 18 lines.

Dry years are always advantageous to the corn in this country, the greatest part of the land being moist 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 exposed to the air, but shaded from the sun. It is at the 48th degree of its division in the bottom of the caves of the observatory, where I suppose the air to be in a mean state of heat, and it begins to freeze when the liquor in the tube falls to 32 degrees. The lowest

which the thermometer fell at the beginning of this year, was to  $20 \frac{1}{2}$  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  $28 \frac{1}{2}$ . 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 height 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 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  $ABC^*$  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 pulley, 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 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. 5.

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 raises the surface of the sea an inclination of 13' 14".

The least apparent depression observed by father *Laval*, at this height 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 raised 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 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  $\frac{1}{4}$  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 *Palpebrae*, or eyelids; 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, 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 sponge, 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 sponges 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 rest 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, where-with 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 alike 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 wiew-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 *papilla*, 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 eruption 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 procedure 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

D 2

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 morte* 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, 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 fore-head, 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 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 subtilly 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*. 14

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 sustain

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 considerable, and perhaps the most considerable of all.

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

In this hypothesis, if fire is put to gunpowder, it will catch it successively, and the ball will not go out till there is a sufficient quantity thereof, not only to get the better of the weight of the ball, but also of the column of air which rests upon it. So that if the ball be six 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 successively augment the swiftness of the ball, till it has acquired its greatest velocity, which would be the same with the inflamed parts of the powder, did not the air resist it; but as the resistance of the air, which the ball expels, augments in the proportion of the square of the velocities of the ball, there is a fixed time when this resistance becomes equal to the new effort, which the ball receives from the powder. Thus when there is too great a quantity

tity of powder in the gun, it will not augment the velocity of the ball. Supposing therefore that there is in this gun only a sufficient quantity of powder, to give it the greatest velocity it is capable of acquiring, the effort of the powder will after that diminish successively, till it entirely ceases; and then did not the air resist the motion of the ball, it would continue to move with a uniform swiftness, equal to its greatest acquired velocity: but the air continually resisting, the swiftness of the ball diminishes each instant, so that there is a fixed time, wherein the remaining impression, which the powder has given to the ball, is equal to the resistance of the air, and then the ball can no longer move. But the weight of the air and of the ball acting against it, with an effort of 473 *lb.* as has been said, will repel the ball to the bottom of the gun, by accelerating its velocity, like all heavy bodies.

From what has been said, it may be concluded,

1. That the best powder (every thing else being equal) is that which soonest take fire.

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

3. That in a gun, whose barrel is cylindrical, of a given length, there is a determined quantity of gunpowder which drives the ball as far as possible; and this quantity is such, 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 bursting, because 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 effect 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 faucisse, 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 falsely 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 result of all the experiments which have been made, to know what quantity of powder must be used according to the different bodies, is to each cubic toise.

*lb. of powder.*

Of loose earth	9 or 10
Of firm earth, and strong sand	11 or 12
Of fat clayey earth	15 or 16
Of new masonry, but slightly bound	15 or 20
Of old masonry, 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 whose base 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, whose superior aperture is not greater than the chamber wherein the powder was lodged.

V. That besides the effort of the powder against the bodies which it raises, it also presses and crushes all the earth near it, both underneath and on the sides of it, and this pressure or crush extends so much the farther, as the surrounding bodies make less resistance.

To account for all the effects resulting from these experiments, and afterwards determine the quantity of powder with which mines ought to be charged, and the most advantageous disposition to produce the effects proposed by them.

Let us, 1. conceive a mine, whereof all the parts surrounding it are incapable of compression, and make equal resistance, such as a bomb of equal thickness every where would make, suspended in the air; it is evident that in this case, besides the resistance of the body, the effort of

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 buttress 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 of 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 flopped 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 fell. All the boards which stop the sluice, and are placed against the fell, 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.

*The HISTORY and MEMOIRS of the*

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 J. 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.

Fig. 1.

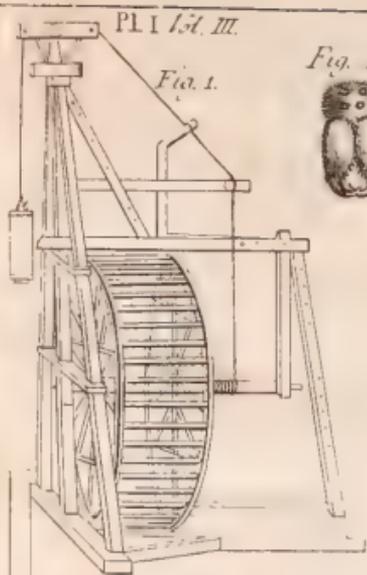


Fig. 4.



Fig. 5.



Fig. 6.



Fig. 7.



Fig. 9.



por.

Fig. 8.



Fig. 2.

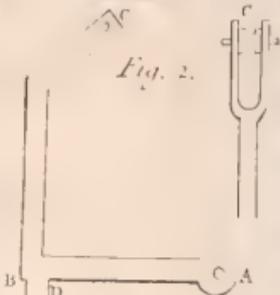


Fig. 9.



Fig. 11.

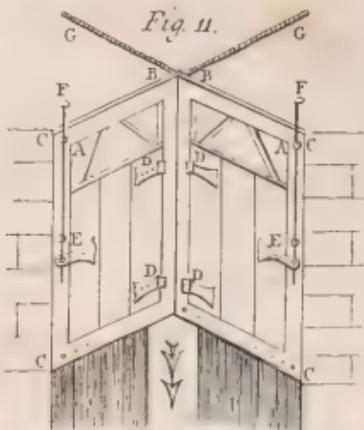
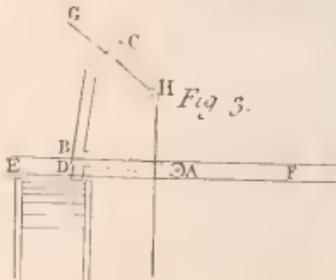
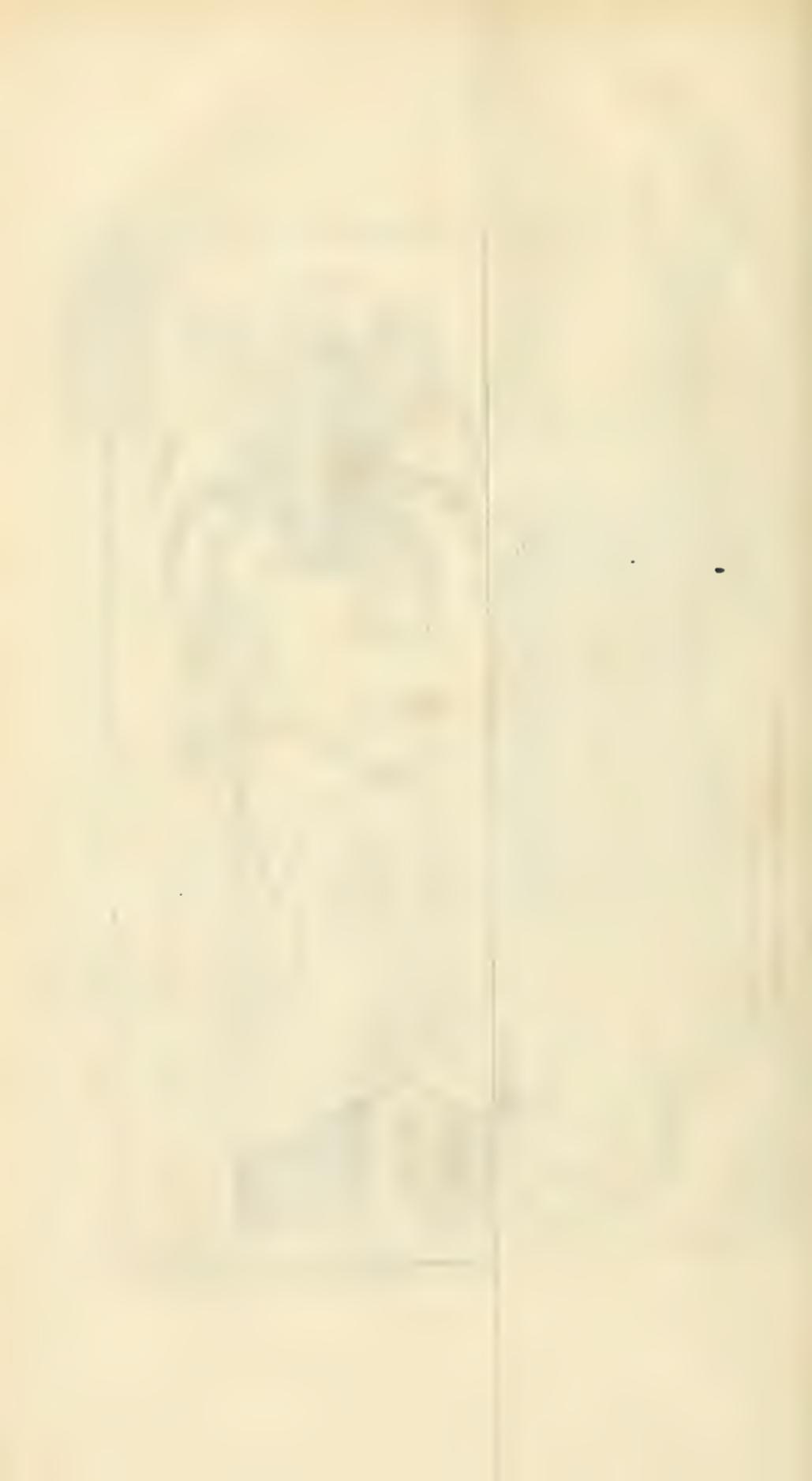


Fig. 10.



Fig. 3.





A  
T A B L E

O F T H E

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.

- I. **U**PON *thunder.*
- II. **U**f *some shells inclosed in stone.*
- III. *Of the force of rays of the sun in pressing and pushing.*
- IV. *Why in summer ice melts faster in vacuo, than in the air.*
- V. *Why the tenderest glasses are least subject to break by fire.*
- VI. *Of the effect of the sun's heat on a paste laid upon a piece of polished glass.*
- VII. *Of an extraordinary cure performed by musick.*
- VIII. *Of the new island formed near Santerini.*
- IX. *Of the method of measuring the heights of places by the barometer.*
- X. *Of a little shell-fish that feeds upon muscles.*
- XI. *An account of Dr. John Scheuchzer's dissertation on the origin of mountains.*
- XII. *An account of Dr. John James Scheuchzer's dissertation on crystal.*
- XIII. *An account of the same author's dissertation intituled, Piscium Querelæ & Vindiciz.*
- XIV. *On the generation of stails.*
- XV.

- XV. *Of the eggs of the cuttle-fish.*  
 XVI. *On the burning glasses of the ancients.*  
 XVII. *A method of stopping horses suddenly.*

In the MEMOIRS.

- 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.*
- II. *A description of a new barometer, to know the weight of the air exactly; with some remarks on the common barometers, by M. de la Hire.*
- III. *Reflections on the variation of the needle, observed by the Sieur Houfflaye, captain commandant of the ship PAurore, 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.*
- IV. *Experiments and observations on the dilatation of the air by boiling water, by M. de la Hire.*
- V. *Reflections on the observations of the variation of the needle, made on board the Mau-repas, in the voyage to the South-sea; with some remarks of M. de la Verune, commander of that vessel, on the navigation of the coasts of America; and of the Terra del Fuego, by M. Cassini the son.*
- VI. *Conjectures on the position of the island of Meroe, by M. Delisle.*
- VII. *Reflections on the observations made by F. Laval, at the S. Baume; and other  
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*neighbouring mountains, by M. Cassini the son.*

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

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.*

A N  
A B R I D G M E N T  
O F T H E

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.*

**W**E 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 rejoyne 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 rejoin 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 endure 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 lightning strikes the eye, there is a large quantity of matter set on fire, M. *Homborg* 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

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. *Homburg* has observed, that if he exposed to it a very light matter, such as *Amiantus*, 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, threatenng 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 surpris'd; 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,

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 surpris'd to find it a new shelf, and they were bold enough to get upon it, altho' it was yet moving, and encreas'd 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 rais'd with it, from the bottom of the sea. They had a little earthquake in *Santorin* two days before the birth of this shelf; it increas'd 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.

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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, 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 horned rocks, which raised themselves like tombs 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. Bourguon'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 *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 *Marzeilles*, 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 fattens 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 Zurich, 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

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 inundations, 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 uni-  
 versal

verfal deluge, God being pleafed to make the waters enter again, into the fubterraneous refervoirs, had broken and displaced with his all powerful hand, a great number of *strata*, which were before horizontal, and had raifed them above the furface of the globe. The whole difertation was made to fupport this opinion.

As thefe heights or eminencies muft have been of a very folid confiftance, M. *Scheuchzer* obferves, that God raifed them only in places where there were a great many beds of ftone. From hence it comes, that the countries where there is a great quantity, as *Switzerland*, are very mountainous, and that on the contrary, thofe which like *Flanders*, *Germany*, *Hungary*, and *Poland*, have only fand or clay, and that to a very great depth, are almoft intirely without mountains.

It was impoffible that the broken, displaced, and elevated *strata* fhould remain horizontal; and we never find any in the mountains with this direction, but what remains of it, is that they are ftill parallel between themfelves, and this, fuppoſing the displacing, is in reality all that they could poſſible preferve 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 fo 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 furface or flopes of the beds. As to their direction, which we ſhould fee, if one ſide of the mountain was cut according to its inclination to the horizon, they are very different in different mountains, and ſome-

times

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. *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 *Zurich*, a great natural philosopher, has sent also to the academy a *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 exterior *crust* of the earth had been extremely dissolved 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, intituled, 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 fortuitous 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.* 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 him-

self 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 feminar 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 these ligaments of the eggs seen in the *vesicaria*, at least we cannot be assured of them, and the irregular vesicles, or grains, which compose them, seem glued together.

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

Altho' the academy does not propose to make inquiries into antiquity, and is more employed in discovering what is, than what was formerly thought, or what we may yet add to arts, than what has been practised, it has however given a great deal of attention to an observation of M. de la Hire's, on the burning-glasses being known to the ancients. The burning mirrors certainly were; for some historians have pretended that *Archimedes* made use of one to burn a fleet, and altho' they attribute to it an impossible effect, this proves that they were known. But it is certain, that these mirrors, which they invented, must have been of metal, and concave, and had a *focus* by reflection, and we are commonly persuaded, that the ancients did not at all know the *focus* by refraction of convex-glasses. Nevertheless M. de la Hire has found them in the first scene of the second act of the *Clouds* of *Aristophanes*. *Strepsiades* is a dull, stupid old fellow, who says to *Socrates*, that he has thought of a fine invention not to pay his debts.

*Strep.* *Hast thou seen at the druggist's this fine transparent stone, with which they kindle fire?*

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

*Strep.* *True.*

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

*Strep.* *When they shall give me a summons, I will take this stone, and putting it to the sun, 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 *Strepsades* 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 fought 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.

# A N A B R I D G M E N T

O F T H E

P H I L O S O P H I C A L M E M O I R S of the R O Y A L  
A C A D E M Y of S C I E N C E S 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

	<i>Lines.</i>		<i>Lines.</i>
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. Nevertheless we may say, that this year has been dry, at least the spring, since 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, because the greatest part of the ground is cool and moist. The 12th of *August*, 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 west wind without storms. There fell snow on the 5th of *March* only; but it melted very soon, and gave  $\frac{1}{2}$  a line of water.

The cold has not been considerable during the whole year; for my thermometer fell at the lowest but to 27 parts  $\frac{1}{3}$  the first of *Feb.* and in the greatest cold it falls to 31, but very seldom, and it was at 48 at the bottom of the caves of the observatory; which we look upon to be the mean state of the air. It begins to freeze, when it is at 32; so that it hardly froze this year, for the thermometer rose again pretty soon; it was at the lowest at 31 inches only, the 1st and 30th of *December*.

If the cold has not been great, the heat on the contrary has been excessive; for the thermometer rose to 69  $\frac{1}{3}$  the 21st of *Aug.* the preceding day it was almost the same, and towards 3 in the afternoon, when the air is the hottest, the thermometer marked 82; thus the heat has exceeded the mean state 34 parts or degrees, and the cold only 20  $\frac{1}{3}$ . From whence we see, that if the cold had been as great as the heat was in proportion to the mean state, the thermometer should have fallen to 14, as it sometimes happens; for we suppose 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 21<sup>st</sup> 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 4<sup>th</sup> 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  $10^{\circ} 10'$  towards the W. *December* 28, 1707, in the same place, and with the same needle, of 8 inches, which I always use.

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 immersed 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 Savans* 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  $\frac{1}{2}$  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  $\frac{1}{2}$  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 CD 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 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  $\frac{1}{4}$  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 over-against 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  $\frac{1}{2}$  an inch upon each box, mark the height of the liquor, as in Z, 28 inches; and divide the space MZ 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 CD, 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  $\frac{1}{2}$  of *aqua fortis*, is to the quicksilver in weight, according to the observations of M. *Homborg*, 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 Houffaye, 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. Casfini the son \*; translated by Mr. Chambers.*

A journal of the observations of the needle, made by M. *Houffaye*, 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

\* 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  $5^{\circ}$  — north-west; in Dr. *Halley's* chart it is laid down as  $6^{\circ} - \frac{1}{2}$  north-west.

At  $357$  — long. and  $22^{\circ}$  north lat. the variation was found  $0$ ; where in Dr. *Halley's* it is noted as  $1^{\circ} - \frac{1}{2}$  north-west.

At  $353 - 45'$  — long. and  $16^{\circ} - 30'$  — south lat. the variation was found  $2^{\circ} - \frac{1}{2}$  north-east; where in Dr. *Halley's* chart it is noted as  $3^{\circ} - \frac{1}{2}$  north-east.

At  $354^{\circ}$  — long. and  $18^{\circ}$  — south lat. the variation was found  $3^{\circ} - \frac{1}{2}$  north-east; where in Dr. *Halley's* chart it is  $3^{\circ} - \frac{1}{4}$  north-east. — This same variation of  $3^{\circ} - \frac{1}{2}$  north-east, continued as far as  $23^{\circ}$  — south lat. Under the same longitude of  $354^{\circ}$  — where in Dr. *Halley's* chart it is made  $4^{\circ} - \frac{2}{3}$  north-east.

In these places M. *Houffaye* observes the variation in 1682 was found  $11^{\circ}$  — north-east, since which time it has constantly diminished, so as now only to be  $4$  or  $5^{\circ}$  —

The greatest variation north-east, which he found in this voyage was  $6^{\circ}$  — the longitude being  $367$  — and the latitude  $28$  — southward, in which place it is represented in the chart as somewhat under  $5^{\circ}$  — 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  $10^{\circ}$  — 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  $12^{\circ}$  — north-west, and  
on

on the eastern side of the same bank  $13^{\circ} - \frac{1}{2}$  to  $14^{\circ}$  — and is represented much the same in Dr. Halley's chart. — In 1680, M. Housheer observes the variation at the *Cape of Good Hope* was only from  $7^{\circ}$  to  $7^{\circ} - \frac{1}{2}$  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  $25^{\circ}$  — 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  $23^{\circ}$  — north-west; and in the year 1682 was found from  $18$  to  $19^{\circ}$  — Dr. Halley's chart for the year 1700, represents it in the bay of St. *Augustine*, as  $21^{\circ} - \frac{3}{4}$ , 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  $22^{\circ}$  — north-west, where in the chart it is  $20^{\circ} - \frac{1}{3}$  north-west.

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

Under the line at  $70^{\circ}$  — long. the variation was found  $16^{\circ}$  — north-west; where in Dr. Halley's chart it is  $17^{\circ} - \frac{2}{3}$  north-west.

At  $87^{\circ}$  — long. and  $15^{\circ}$  — north-lat. the variation was found  $10^{\circ} - 30'$  north-west; where in Dr. Halley's chart it is  $12^{\circ}$  — north-west.

Within sight of *Canary* at  $16^{\circ} - 30'$  north lat. and all along the coasts of *Malabar*, the variation was found  $6^{\circ} - 30'$  — north-west, where the chart makes it  $8^{\circ}$  — north-west.

At *Cape Conorin* in the variation was found  $7^{\circ} - 30'$  — north-west; in the chart it is  $7^{\circ} - \frac{2}{3}$  north-west.

Within sight of *Point Galle*, in the island of *Ceilon*, the variation was found  $5^{\circ} \text{---} \frac{1}{2}$  north-west in the chart it is  $6^{\circ} \text{---} \frac{1}{2}$ .

Near the coast of *Coromandel*, the variation was found  $5^{\circ}$  north-west, exactly the same as in the chart.

In the islands *Andaman* and *Nicobar*, the variation was found  $3^{\circ}$  north-west, precisely as in the chart.

Within sight of the island *Diego Rodrigues*, the variation was found  $16^{\circ} \text{---} 30'$  north-west, which in the chart is  $19^{\circ}$  north-west.

Within sight of the island *Maurice*, the variation was found  $21^{\circ}$  north-west, where the chart gives it  $20^{\circ} \text{---} \frac{1}{2}$  north-west.

Within sight of the island *Bourbon*, the variation was found from  $21^{\circ} \text{---} \frac{1}{2}$  to  $22^{\circ}$  north-west, where the chart gives it  $21^{\circ}$  north-west.

At  $74^{\circ}$  long. and  $25^{\circ}$  south lat. the variation was found  $23^{\circ} \text{---} \frac{1}{2}$  north-west, where the chart gives it  $22^{\circ} \text{---} \frac{1}{2}$  north-west.

At  $72^{\circ} \text{---} 45'$  long. and  $27^{\circ} \text{---} 15'$  south lat. the variation was found  $24^{\circ} \text{---} 30'$  where the chart gives it  $23^{\circ}$  north-west.

The same variation continued as far  $65^{\circ} \text{---} 45'$  long. and  $33^{\circ} \text{---} 10'$  south lat. where the chart makes it  $23^{\circ} \text{---} \frac{2}{3}$  north-west.

From this place the variation continually diminished, as they proceeded towards the *Cape des Aiguilles* about the middle whereof at  $35^{\circ} \text{---} 30'$  lat. the variation was found  $13^{\circ}$  north-west, and within sight of the *Cape of Good Hope*, and all along the coasts of *Angola*, as already mentioned, from  $9^{\circ} \text{---} \frac{1}{2}$  to  $10^{\circ}$  north-west; in Dr. *Halley's* chart the variation at the middle of the bank *Des Aiguilles* is  $12^{\circ}$ .

At

At the *Cape of Good Hope* somewhat above  $10^{\circ}$ — and along the coasts of *Angola* from  $9\frac{1}{2}$  to  $10^{\circ}$ —

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  $1^{\circ}$ — or  $1^{\circ}$ — $\frac{1}{2}$  north-west, where the chart gives it somewhat above  $1^{\circ}$ — north-west.

At the island of *Ascension*, there is no variation, or at most not a degree north-eastwards; the chart gives it  $\frac{1}{3}$  of a degree north-eastwards.

Proceeding hence for *France*, as you pass the line at  $357$  or  $358^{\circ}$  of longitude, there is no variation. The chart gives it a variation of  $\frac{1}{2}$  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  $4^{\circ}$ — to  $4^{\circ}$ — $30'$ — north-westwards, where the chart gives it  $5^{\circ}$ — $\frac{1}{3}$  north-westward.

As we approach *Terre-Neuve*, the variation increases to  $7$  or  $8^{\circ}$ — and at length on the coasts of *Britany* dwindles to  $5^{\circ}$ — 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  $354^{\circ}$ — longitude, and  $20^{\circ}$ — southern latitude, the variation, which

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

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.

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. *Amon-ton*, 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  $\frac{1}{4}$  of a line on the inside. Thus far agrees with the phial and tube used by M. *Amon-ton*; 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. *Amon-ton*s 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  $\frac{1}{2}$ , 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 AB above that in the phial; but the third of 27 inches, 7 lines  $\frac{1}{2}$ , is 9 inches, 2 lines  $\frac{1}{2}$ , 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  $\frac{1}{3}$  of the weight of the atmosphere, but less by 9 lines  $\frac{1}{2}$ .

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  $\frac{1}{2}$  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 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, 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. *Amon-ton* 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-

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 of 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 ED, till it rise to E in the tube DE, which is open, and only to F in the tube DB, which is fastened to the phial AB underneath B. 'Tis certain, that the air in the phial, and in the part BF of the tube BD, will be compressed more than the external air, as being loaden with a height of mercury EF; and in this case, if the whole phial be taken away, or only its communication with the tube BD in B be stopped, 'tis easy to infer, that the mercury will still remain in F, and neither rise nor fall in the tube BD, tho' the compressed air in BF 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, 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 subtracting 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*

*vous* 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,  $\frac{2}{3}$  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 immersed 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 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 immersed, 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 water being placed over a good fire, I presently perceived a multitude of little bubbles begin to arise from the end of the syphon, which shewed that the air in the phial was beginning to dilate, and issue at the end of the syphon, by the heat it had conceived from the fire ; but as the water heated more and more, the air bubbles rose from the syphon with more precipitation, which continued till such time as the water boiled out right, when there were bubbles still seen to arise, tho' much less than before.

After the water had boiled some time, I took it off the fire, keeping the end of the phial and syphon still under water, that as the water in the copper, and the air in the phial should come to cool, no particle of air might get into the phial, either by the syphon, or any little pores, that might be found in the cork ; and to shorten the operation, I laded some of the hot water off, and supplied 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 same density as the external air, a little water was left in the part of the syphon, which traversed the cork, and was suspended and counterbalanced within the air in the phial and the external air.

Taking out the cork therefore and the syphon, and wiping the phial well on the outside, 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 ; so 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, subtracting the weight of the phial 6 drachms  $\frac{1}{2}$ , as I found it at first. The remainder is 35 drachms  $\frac{1}{2}$ , 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  $35 \frac{1}{2}$  to 8, which is somewhat less than  $4 \frac{1}{2}$  to 1. Yet is this dilatation much greater than what I had found before, which was only as  $2 \frac{1}{2}$  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  $\frac{1}{2}$ , and 11 grains; so that there were 11 grains of water sticking to its inside, then sitting 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  $35 \frac{1}{2}$  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 pre-

sent 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 immersing 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  $345^{\circ}$ — $44'$  longitude, and  $20^{\circ}$ — $44'$  south latitude, near the island of *Ascension*, the variation was found  $7^{\circ}$ — $30'$  north-east. In Dr. *Halley's* chart the variation at this place is somewhat above  $7^{\circ}$ —north-east.

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

At  $310^{\circ}$ — $30'$ —long. and  $52^{\circ}$ — $19'$ —south lat. near the islands of *Sebalt*, the variation

\* July 21, 1708.

was found  $23^{\circ}$  — north-east. In the chart it is  $21^{\circ}$  —  $30'$  — 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  $300^{\circ}$  —  $10'$  — longitude, and  $13^{\circ}$  —  $6'$  — south latitude, near the point *Canette*, and that of *St. Galland*, the variation was found  $7^{\circ}$  — north-east.

At  $297^{\circ}$  —  $27'$  — long. and  $14^{\circ}$  —  $1'$  — south latitude, near *Pisco*, the variation was found  $7^{\circ}$  — north-east.

At  $297^{\circ}$  —  $30'$  — long. and  $31^{\circ}$  —  $49'$  — south lat. near *Valparais*, the variation was found  $8^{\circ}$  — north-east.

At  $299^{\circ}$  —  $25'$  — long. and  $36^{\circ}$  —  $30'$  — south lat. near the *Conception*, the variation was found  $10^{\circ}$  — 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  $44^{\circ}$  —  $49'$  — the variation was found  $12^{\circ}$  north-east.

At the latitude of  $48^{\circ}-58'$ — the variation was found  $13^{\circ}$ —— north-east.

At the latitude of  $53^{\circ}-37'$ — the variation was found  $15^{\circ}$ — north-east.

And at the latitude of  $56^{\circ}-42'$ — the variation was found  $17^{\circ}$ — 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 *Schalt*, which he calls *Sible de Ward*, and describes them as 3 islands situate at  $51^{\circ}-25'$ — south latitude, he found the variation in the year 1683,  $23^{\circ}-10'$ — north-east. I have already mentioned, that the variation was found near these islands  $23^{\circ}$ —— $0'$ — 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  $47^{\circ}-10'$ — latitude in the *South-sea*, *Dampier* found the variation  $15^{\circ}-\frac{1}{2}$  north-east.

And

And at the latitude of  $36^{\circ}$  — he found the variation  $8^{\circ}$  — 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  $5^{\circ}$  — 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  $57^{\circ}$  —  $40'$  — southern latitude, is only  $55^{\circ}$  —  $40'$  southern latitude.

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He adds, that the islands of *Barnvelt*, which the charts place in the same latitude with *Cape Horn*, are situate west-north-west of that *Cape* in  $56^{\circ}—35'$ —latitude. — In which he likewise agrees pretty nearly with M. *Delisle's* chart above-mentioned.

He further observes, that the islands of *Barnvelt* 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 found, and the weather moderate along *Chili* and *Peru*.

M. *de la Verane* 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 Fuogo*, or to make a large circuit, when the wind does not allow it. Lastly, he ob-

erves, 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*, 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. Baltazar 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 *Égypt* 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 *Schicks*, 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 *Mercö* was indubitably upon the *Nile*. The source of the *Nile*, which was so long sought for in vain by the ancients, is in  $12^{\circ}$  of north-latitude. Its cataracts, a little less celebrated, but much better known than its source, are  $23^{\circ} \frac{1}{2}$ , and it is certainly between these two points, that the island of *Mercö* must be.

The ancients have said, that this island was formed by the concurrence 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 *Ataboras*, 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 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 impresson 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 *Mercë*, 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 *Mercë* 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 *Alaboras* 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 *Mercë*, 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  
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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  $23^{\circ} \frac{1}{4}$  of north latitude. Now from *Syene*, to the city of *Meroë*, 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 *Meroë*, at  $15^{\circ} \frac{1}{4}$  from the equator.

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

\* Lib. II. cap. 73.

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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  $18^{\circ}$  of *Taurus*, and  $14^{\circ}$  of *Leo*. *In Meroe, quæ est caput gentis Æthiopum, bis in anno absumi umbras, sole duodevicesimam 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  $\frac{1}{2}$  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 ancients have placed it in the middle of the first climate, of which the longest day is 13 hours, which gives by calculation  $16$  degrees  $\frac{1}{2}$ , 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. *Influant in Nilum, says Strabo, duo flumina ab oriente delata, & Meroem ingentem insulam complectuntur.* I cannot tell whether the ancients 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 *Darder*. The river *Tacaze* being as big as half the *Nile*, has very much the appearance of being the *Astaboras* of the ancients: this is the opinion of *Juan de Barros*, the *Liey* of the *Portugueze*; and two things will not permit me to doubt it. The first is, that according to

the jesuits, who have been in *Ethiopia*, it enters into the *Nile* at  $17^{\circ} \frac{1}{2}$  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 *Mercë*, *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 *Portugucze* 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. Gobien*s 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

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. da 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 *Mercö*, 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 *Albara*, 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 *Mercö* 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 *Mercö* 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.

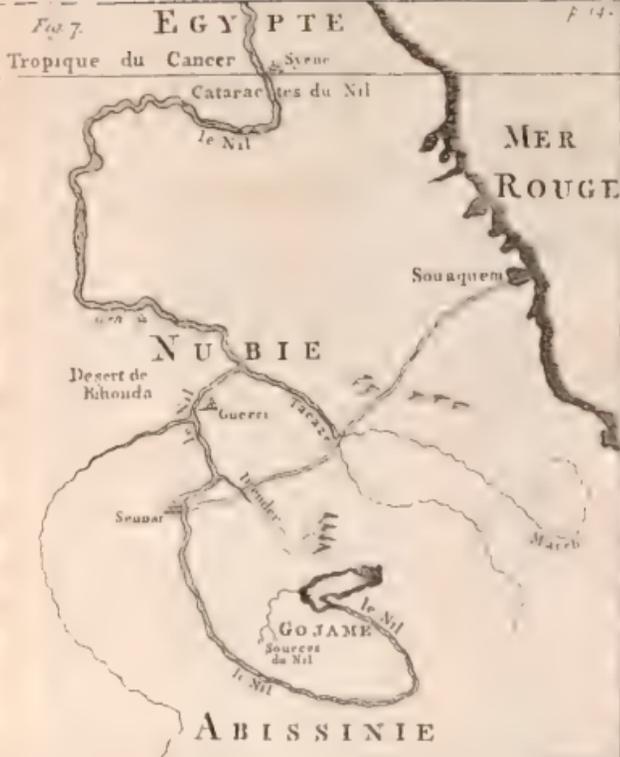
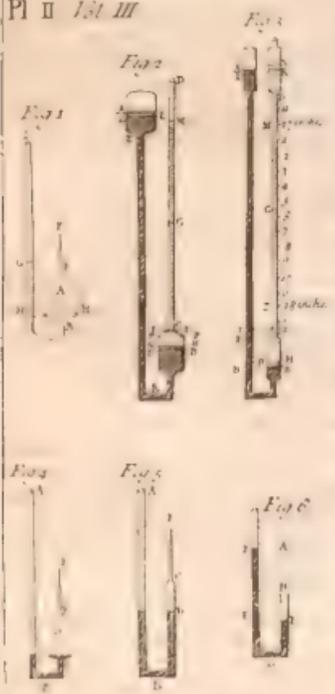
VII. *Reflections on the observations made by F. Laval, at St. Baum, and other neighbouring mountains, by M. Cassini, jun\*. translated by Mr. Chambers.*

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 height 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.



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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 extremities 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  $\frac{1}{4}$ , 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  $\frac{2}{4}$ , is the depression 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  $\frac{2}{3}$ , which added to 2 lines  $\frac{1}{3}$ , for the height of the observatory gives 2 inches 8 lines  $\frac{2}{3}$ , for the height of *St. Baum* above the sea, the number corresponding to which in the table is 415 fathom  $\frac{1}{2}$ , which subtracted 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  $\frac{1}{2}$  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 subtracted 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 subtract 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 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 *Marfeilles*; 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  $1^{\circ}45''$ —whereas at his observatory it has risen to  $3^{\circ}20''$ —Indeed as F. *Laval* has made a much greater number of such observations at *Marfeilles*, 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  $1^{\circ}30''$ —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  $36^{\circ}$  --- 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 *parbelia* on them, especially when the sun is very high, and the air well heated; as in effect the *phenomenon* 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 phenomenon 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 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. Casfini the son \**.

He set out from *Martinica*, 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 <i>Porto-cabeillo</i> , the meridian height	}	78	48	55
of the upper edge of the sun				
Refraction minus the parallax				9
Therefore the true height of the	}	78	48	46
upper edge				
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	}	100	30	48
height of the equator				
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

150 *The HISTORY and MEMOIRS of the*

July 13, the meridian height of the } 78 25 5  
 upper edge of the sun

From whence we take the height of } 10 30 50  
 the pole

These observations concur in deter- }  
 termining the height of the pole at } 10 30 50  
*Porto-cabeillo* to be

*Observations for the variations of the needle.*

F. Feuillée set out from this port the 14th 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 *Curacca* 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 } 81 46 5  
 of the sun was

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

ROYAL ACADEMY of SCIENCES. 151

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 } 86° 38' 17"  
 of the sun

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<sup>h</sup> 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<sup>h</sup> 33 5" at *Paris*, by the corrected calculation.

5<sup>h</sup> 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 staid 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  $\frac{1}{2}$ , 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  $\frac{1}{4}$  line less than what was observed at *Cairne* 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  $\frac{1}{2}$ , 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  $7^{\circ} 25'$  N. E.

This declination is marked in *Dr. Halley's* map above  $9^{\circ}$  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  $\left. \begin{array}{l} 56 \\ 8 \\ 10 \end{array} \right\}$

ROYAL ACADEMY of SCIENCES. 153

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 } 10 20 25  
fort of *Bocachica*

*Observations for the height of the pole at Carthage-  
na, 1705.*

*Jan. 1, 1705, at Carthage-  
na, meri-  
dian height of the upper edge of } 56 46 20  
the sun*

*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 Carthage-  
na* 10 30 25

*Observation of the eclipse of the moon, Dec. 11,  
1704, at Carthage-  
na.*

At 0<sup>h</sup> 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. Cou-  
plet* 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 *Carthage-  
na*, the  
beginning of the eclipse.

154 *The HISTORY and MEMOIRS of the*

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

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* 5 11' 50".

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

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

ROYAL ACADEMY of SCIENCES. 155

Jan. 16.

At 1<sup>h</sup> 20' 15" in the morning at *Carthagera*, 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 calcu-  
lation

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

The last observation having been made in se-  
rene weather it seems best to fix here, and deter-  
mine the difference of the meridians between *Pa-  
ris*, and *Carthagera* of 5<sup>h</sup> 11' 20"

*Observations for the variation of the needle at  
Carthagera.*

*F. Feuillée* has found by several observations  
the variation of the needle at *Carthagera* 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 up-  
per 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 Glos* 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 <i>Aug.</i>	—————	88	18	37
The 2d of <i>Sept.</i>	—————	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 <i>Oct.</i>	—————	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 <i>Nov.</i>	—————	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 <i>Dec.</i>	—————	52	10	14
The 31st	—————	52	28	2

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

tions at *Martinico* between  $14^{\circ} 42' 5''$  and  $14^{\circ} 43' 55''$ , 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  $14^{\circ} 43' 0''$ .

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

<sup>n</sup>  
3 10 41 in the morning at *Martinico*, the immersion of the second satellite into the shadow of *Jupiter*.

*The 19th 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 25th 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 emergence of the third from the shadow of *Jupiter*.

3 17 52 total duration in the shadow of *Jupiter*.

*The 26th 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.

158 *The HISTORY and MEMOIRS of the*

<sup>h</sup> 4 <sup>'</sup> 13 <sup>"</sup> 18 difference of the meridians between  
*Paris and Martinico.*

*The 4th of November, at*

1 13 57 in the morning at *Martinico*, the im-  
merſion 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.

2 12 54 difference of the meridians between  
*Paris and Martinico.*

*The 27th of November,*

1 19 36 in the morning at *Martinico*, the im-  
merſion of the firſt ſatellite into the  
ſhadow of *Jupiter*. The wind broke  
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 im-  
merſion 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 im-  
merſion 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.

$4^{\text{h}} 39' 18''$  at *Paris*, by the corrected calculation.  
 $4^{\text{h}} 12' 44''$  difference of the meridians between  
*Paris* and *Martinico*.

*The 23d of March, at*

$10^{\text{h}} 47' 33''$  at night at *Martinico*, emerfion of  
the firft fatellite out of the fhadow  
of *Jupiter*.

$14^{\text{h}} 59' 28''$  at *Paris*, by the corrected calculation.  
 $4^{\text{h}} 11' 55''$  difference of the meridians between  
*Paris* and *Martinico*.

*The 15th of April, at*

$11^{\text{h}} 7' 44''$  at night at *Martinico*, emerfion of the  
firft fatellite out of the fhadow of  
*Jupiter*.

$15^{\text{h}} 20' 44''$  at *Paris*, by the corrected calculation.  
 $4^{\text{h}} 13' 0''$  difference of the meridians between  
*Paris* and *Martinico*.

Almost all thefe obfervations concur in giving  
the difference of the meridians between *Paris* and  
*Martinico*  $4^{\text{h}} 13' 0''$ .

We had determined it by the comparifon of  
two obfervations, made at the fame time at *Paris*  
and at *Martinico* to be  $4^{\text{h}} 13' 28''$ .

Therefore we may for greater exactnefs deter-  
mine the difference of the meridians between  
*Paris* and *Martinico* to be  $4^{\text{h}} 13' 15''$ .

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

$8^{\text{h}} 12' 58''$  at night, beginning of the eclipse  
 $10^{\text{h}} 49' 0''$  end of the eclipse.  
 $2^{\text{h}} 36' 2''$  total duration

F. *Feuillée* obferved during the time of this  
eclipse, the immerfion and emerfion of feveral  
spots, of which we were not able to obferve the  
cor-

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

- $9^{\text{h}} 42' 2''$  at *Martinico promontorium acutum*, quite in the shadow.  
 $17^{\text{h}} 55' 0''$  at *Paris* the shadow was at *promontorium acutum*  
 $4^{\text{h}} 12' 58''$  difference of the meridians between *Paris* and *Martinico*.  
 $10^{\text{h}} 49' 0''$  the end of the eclipse at *Martinico*.  
 $15^{\text{h}} 2' 30''$  at *Paris*.  
 $4^{\text{h}} 13' 30''$  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*  $4^{\text{h}} 13' 15''$ .

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  $9^{\text{h}} 40'$ .

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

*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  $\frac{1}{2}$ , greater by  $\frac{1}{4}$  of a line than what he found at *Porto-Bello* of 3 feet, 5 lines  $\frac{1}{2}$ .

*Ob-*

*Observations of the variation of the needle.*

F. Feuillée at his return to *Martinico*, found the variation of the needle to be  $6^{\circ} 10'$  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  
T A B L E

O F T H E

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

In the HISTORY.

- I. **O**F the shagreen, which comes from Turkey.
- II. **O**F great cold coming with a south wind.
- III. Of the Seine not being entirely frozen in the hard winter of 1709.
- IV. Of a pullet with two hearts.
- V. Of the legs of sea-urchins.

In the MEMOIRS.

- 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.*
- II. *Observations on the quantity of rain water, and on the winds, by M. le Comte du Pontbriand, at his castle two leagues west from St. Malo; communicated to the academy by M. du Toray, 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.*

III.

- III. *Observations on the water which fell at Lyons, during the year 1708, by M. de la Hire.*
- IV. *A comparison of the barometrical observations made at Paris and at Zurich, in the year 1708, by M. Maraldi.*
- V. *Observations on the motions of the tongue of the wood-pecker, by M. Mery.*
- 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.*

- 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.*
- XVI. *Reflections on the observations of the eclipse of the sun, March 11, 1709, made in different countries, by M. Cassini the son.*

A N  
A B R I D G M E N T  
O F T H E

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. *Faugcon* 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-*

*The HISTORY and MEMOIRS of the*

*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  
lately,

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 *aorta* 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.

A N  
**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.*

**T**HE quantity of rain, which fell during the year 1708, was in

	<i>Lines.</i>		<i>Lines.</i>
Jan.	$28\frac{1}{8}$	July	32
Feb.	15	Aug.	$15\frac{1}{8}$
March	$15\frac{7}{8}$	Sept.	12
April	$17\frac{3}{8}$	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  $\frac{1}{2}$ ; 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  $\frac{1}{2}$  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  $\frac{1}{2}$  *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  $\frac{1}{2}$  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  $\frac{1}{2}$  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 height 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  $10^{\circ} 15'$  to the west. This needle is 8 inches long, and is that which I always use.

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 ———	1 ¼
May ——— 6½ ———	11½	26½ ———	30 ¼
June ——— 31 ¾ ———	17	24 ———	23 ⅝
July ——— 40 ———	38	10 ———	32
Aug. ——— 38 ———	34 ¾	6½ ———	15
Sept. ——— 20½ ———	9 ¼	43½ ———	12
Oct. ——— 32 ———	41	35½ ———	15
Nov. ——— 10½ ———	6	11 ———	6½
Dec. ——— 57½ ———	27 ¼	24½ ———	9 ¼
	Inch. Lin.		Inch. Lin.
Total at Pontbriand	24 10 ¼	Total at Pontbriand	24 6
Paris	17 11½	Paris	18 ¼

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 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 21<sup>st</sup> 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

ROYAL ACADEMY of SCIENCES. 175

At *Paris* the greatest rain was 21 lines  $\frac{1}{2}$  *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  $\frac{1}{2}$ , with a N. W. wind.

*On the winds in 1708.*

In *Jan.* the wind more to the S. at *Paris* than at *Pontbriand*, and sometimes the same.

In *Feb.* often the same.

In *March* generally the same.

In *April* the same, but on some days a little different.

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

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

In *July* very few observations at *Pontbriand*, so that nothing is discovered 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 these two places.

In *Nov.* a little different.

In *Dec.* the same.

We cannot make a very just comparison of all these winds; for *M. du Pontbriand* marks the rhumb only on those days when it rained.

At *Pontbriand* the greatest 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 strong 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  $\frac{1}{2}$ , *May* 24, with a N. N. W. wind, and at *Pontbriand* 4 lines  $\frac{1}{2}$ , 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.

	<i>In. Lin.</i>		<i>In. Lin.</i>
Jan.	2 0	July	1 6 $\frac{1}{4}$
Feb.	3 7 $\frac{1}{2}$	Aug.	3 6
March.	2 3 $\frac{1}{6}$	Sept.	7 7 $\frac{1}{4}$
April	3 9 $\frac{1}{2}$	Oct.	1 11
May	2 2 $\frac{1}{8}$	Novem.	0 10
June	4 10 $\frac{3}{4}$	Dec.	2 1 $\frac{3}{4}$

Sum of the whole year 35 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 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  $\frac{1}{2}$ , 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  $\frac{1}{2}$ ; 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 di-

minution

minution of the water of the *Limat* in height was 9 inches  $\frac{1}{2}$ , and the augmentation 1 inch  $\frac{1}{2}$ .

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  $\frac{1}{2}$ , 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  $\frac{3}{4}$  at *Zurick*. The *Limat* increased 24 inches, and fell but  $\frac{1}{2}$  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  $\frac{2}{3}$  ; at *Zurick*, 21 lines  $\frac{1}{2}$ . 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 height 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 *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 the thermometer 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 *Switzerland*, 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 *Switzerland* 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 *Tamina*, 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 diminution

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 *Switzerland* 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  $\frac{1}{2}$  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  $\frac{1}{2}$  long; the hinder, which was unknown to M. *Borelli*, is 5, or thereabouts, being united to a

\* March 13. 1709.

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 woodpecker 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 encompass 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 straight; 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 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-

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  $OB$  come as parallel to each other as far as the *cornea*  $BB$ , 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  $OB$  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  $BB D$  is plunged in the water  $AA$ , so that the surface  $AA$  is perpendicular to the rays  $OB$ , which come from the object  $O$  to the eye, then these rays  $OB$  meeting the surface of the water  $AA$  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  $FF$ , 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  $FF$  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 immersed 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 immersed 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 immersed in water.

Let *BB D*, as before, be the eye of the animal immersed in water, of which the surface is *AA*. It follows, that the rays *DB*, 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 *BB*, they meet the water *AA*, 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 immersed.

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 asslant. 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 immersed 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 *phenomenon* of gravity. A stone thrown up in the air falls down directly upon the surface of the earth; people do not use to be surpris'd at it: and yet to find the cause of this fall, is one of the most difficult problems to be resolv'd in physicks; and we are not yet arriv'd to a solution sufficiently demonstrat'd, 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 propos'd 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 *phenomenon* of gravity; and if we should not succeed in explaining it by this principle, it will certainly shew the insufficiency 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. *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 23.56', 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  $\frac{1}{2}$ . 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 impresson 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 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 subtracted 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  $\frac{1}{2500000}$  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

† Exactly 205,560  
lb. and 2,340

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24,336

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38,025

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 concurrence 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  
of

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 *phenomenon* 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, and 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*

*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 expressly 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  $\frac{1}{2}$ ; upon which descending by the iron grate in the great road leading to *Verfailles*, we found the liquor in the tube fallen to 28 divisions  $\frac{1}{2}$ , 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  $\frac{1}{2}$ , 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  $\frac{1}{2}$ .

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  $\frac{1}{2}$ , 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  $\frac{1}{2}$ ; but having found it in the morning at 33 divisions  $\frac{1}{2}$ , we learned that between morning and evening a change of 2 divisions  $\frac{1}{2}$  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  $\frac{1}{2}$  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  $\frac{2}{3}$ , which I take for 4 lines  $\frac{1}{2}$ , 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  
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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  $\frac{1}{2}$ , 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  $\frac{1}{4}$  by adding  $\frac{1}{2}$  the difference between morning and evening, and subtracting these 22 divisions  $\frac{1}{4}$  from 38  $\frac{1}{2}$ , the remainder 16 divisions  $\frac{1}{4}$  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  $\frac{1}{2}$  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  $\frac{1}{2}$ ) by 12, we shall have 12 lines  $\frac{1}{2}$  for the height of mercury, equivalent to 150 lines of liquor. We must subtract therefore 12 lines  $\frac{1}{2}$  from the 29 inches difference between the heights of mercury in the cisterns, and the remaining 27 inches, 11 lines  $\frac{1}{2}$  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 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  $\frac{2}{7}$  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 terrafs 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 height 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's* 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.

	<i>inch.</i>	<i>lin.</i>	<i>poi.</i>
I. The single barometer stand- ing 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 stand- ing 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 stand- ing 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	

	inch.	lin.	part.
IV. The single barometer stand- ing at _____	} 27	9	1
The double barometer in the great heats stood at _____			
And in a moderate degree of heat at _____	} 34		
The difference is			
	4		$\frac{1}{2}$
	Or	18	

V. The single barometer stand- ing at _____	} 27	10	0
The double barometer at a mode- rate degree of heat stood at _____			
And the barometer being removed to the open air when it began to freeze stood at _____	} 30		
The difference is			
	2		$\frac{2}{3}$
	Or	10	$\frac{2}{3}$

Being desirous likewise to find what would befall the double barometer, when expos'd to the sun about noon, in the greatest heats of July, in the year 1708, and the better to discover the effects thereof, I plac'd M. *Amontons's* little spirit of wine thermômetre aside of it.

I observ'd the liquor in the barometer at first rise very slowly in comparis'n 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 observ'd 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 comparis'n thereof; yet one would expect the same thing to be-

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 different; 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 *phenomena*, 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, *phenomena* 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 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  $\frac{1}{2}$  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 21<sup>st</sup>, 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 some-

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?

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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

ROYAL ACADEMY of SCIENCES. 233

By these observations the barometer fell, in 5 days, about 1 inch at *Paris* and at *Genoa*.

	At <i>Paris</i> .		At <i>Genoa</i> .	
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	4½ 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	2¼ W.	27	6½ 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.

	At <i>Paris</i> .		At <i>Genoa</i> .	
1708.	Barom.	Winds.	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 <sup>th</sup> . 27	5 rain
24.	27	11 $\frac{1}{4}$ N.W.	25 <sup>th</sup> . 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 than *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  $\frac{1}{2}$ , 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 observations

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 departure from *France*, that he had been assured that there was no sensible difference in the barometer, found in all the places situated between 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 barometer 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 observed, but often also in the same place, where the quicksilver was more or less elevated according to the different dispositions of the air, tho', to try 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 *Ma-*  
 “ *laca* 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 basin.

“ 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 immersed 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 in-  
 ches, 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 30 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*.

Befides theſe experiments made at a time when the air was pure and ſerene, *P. 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 ordinary, 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 basin, where it immersed 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 itself, 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 perpendicular 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 pre-

ceding experiments, which shews the exactness of both of them.

In the second experiment, 6 inches of natural air inclosed in the tube, after the reverfing 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 reverfing, 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  $\frac{3}{4}$ , 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 *phenomenon* 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 immersed 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 reverfed it in the quicksilver; fo that above an inch was immerged. Immediately after the reverfing, the quicksilver kept within a few lines of where it keeps by the dilatation alone without having rarefied it. But the quicksilver was feen to rife in the tube, as faft as the air was condensed in cooling; and when it was entirely cooled, the quicksilver rofe an inch and 2 lines more than it did immediately after the reverfing, and more than M. *Mariotte's* rule required; and confequently the rarefied air was lefs dilated than by the rule, by the fame 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 lefs, than by the rule; the air of *Malaca* therefore dilates lefs than our air rarefied by the heat of boiling water.

I made the fame experiment upon 6 inches, and afterwards upon 9 inches of air, and I always found that our air rarefied by heat, dilated much lefs 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 lefs dilatation of the air of *Malaca* comes not only from the great heats of the climate, but from its own nature being lefs apt to dilate than ours.

As the air dilates otherwife at *Malaca* than it does in *France*, at an almoft equal height with the furface of the fea; and as in *France* the dilatation is found at great heights different from that which happens to the lower air, as results from the obfervations made on the mountains of *Auvergne* and *Rouffillon*, we may infer, that the whole mafs of the air has not the property of dilating itfelf 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 ; 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. Geofroy, 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, 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 *Leclonius* 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 have found others, where the stones were quite formed, and the stomach solid and full of a brownish liquor, very mouldy and fetid.

Under the bag where the stones are inclosed, I have found a flat membranous vesicle, whose use I do not understand, only it has been observed, that when the stone disappears, this vesicle becomes full of a sweet limpid water, and possesses the same space as the stone possessed. In others, I have found large fair stones, and a new delicate membrane, inclosing the stones and the stomach. Upon raising this membrane, there were 3 new teeth visible thereon, similar in all respects to those of the old stomach; so that no doubt can any longer remain, whether this membrane becomes at length the real stomach.

In cray-fishes, which had cast their coats, I have found the stomach full of a brown liquor, the membrane of the stomach being here very tender, and no appearance of viscid matter in it, nor any remains of the former stomach. The stones were much lessened, and appeared as if corroded by some dissolvent; they were covered with a very fine membrane, which was the only thing that parted them from the cavity of the stomach.

In other cray-fishes, which had moulted a longer time, I did not find the stones in their usual places, but quite in the stomach, where they were joined together by their concave parts.

In others, where the new shell was almost arrived at its full hardness, I found nothing in the place where the stones used to be lodged, but a white spot, which was no more than the two membranes of the vesicle, that had contained the stone, shrunk close together. Upon opening the stomach, 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 waggon; 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, not easy to be altered by the bodies around, being not only in an under skin, clofer 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 enlarges 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 chrySTALLIFICATIONS, 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

certainly 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 *laminae*, whence the stones are formed that grow in its bosom; if such stony *laminae* 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,

the same thing is found to befall 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  $4\frac{1}{2}$ , to  $3\frac{1}{2}$ , 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 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 coagulating 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 consistence, 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 between the lesser *laminae* 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.

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 enlargement 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 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 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 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 extremely 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 extremely 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 filtering 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  
fea-

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 dissected by M. *Merry*; one whereof is represented by fig. 13, where the spaces *aaa*, 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 preceding 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 expresses 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.

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; 1<sup>st</sup>, 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 enrich 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 considered, that snails of all ages and degrees of growth below the highest, have no such ledge, the difficulty disappears, for the same thing doubtless happen to such shells, as that of *fig. 15*. This ledge is of the same colour with the stripes in the little striped snails, represented in *fig. 14*, and accordingly the extremity of the collar is of the same colour as the skin, which forms the stripes, as may be seen in *fig. 10*.

The curvity of the shell is unchangeable, unless that of the body of the animal, which is its mould, happen to change, 'tis easy to imagine probable causes of such a change in the growth of the snail. For instance, 'tis not unlikely, that the internal fibres of the collar may grow faster than the external ones; the consequence whereof must be the latter's pulling the collar of the snail towards them, and obliging it to bend outwards.

As the different length of the fibres of the collar gives us an easy conception, how it may come to be bent outwards; so, by attending to this different length of the same fibres, we may conceive how the bodies of several animals come to be twisted spirally; for supposing that from the production of such animals, the fibres of a certain part of their surface are longer than those of the opposite surface, 'tis evident the body will crook itself so as, the surface, whose fibres are shortest, will form the concave of the curvity; and the other surface, whose fibres are longest, the convex which is enough to make the body of the animal describe a spiral, since it cannot grow without always bending thus on itself, provided its long and short fibres grow in the same proportion. 'Tis true, in the case above-mentioned, it would only describe spirals, whose several 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 \* 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 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 *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. 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.

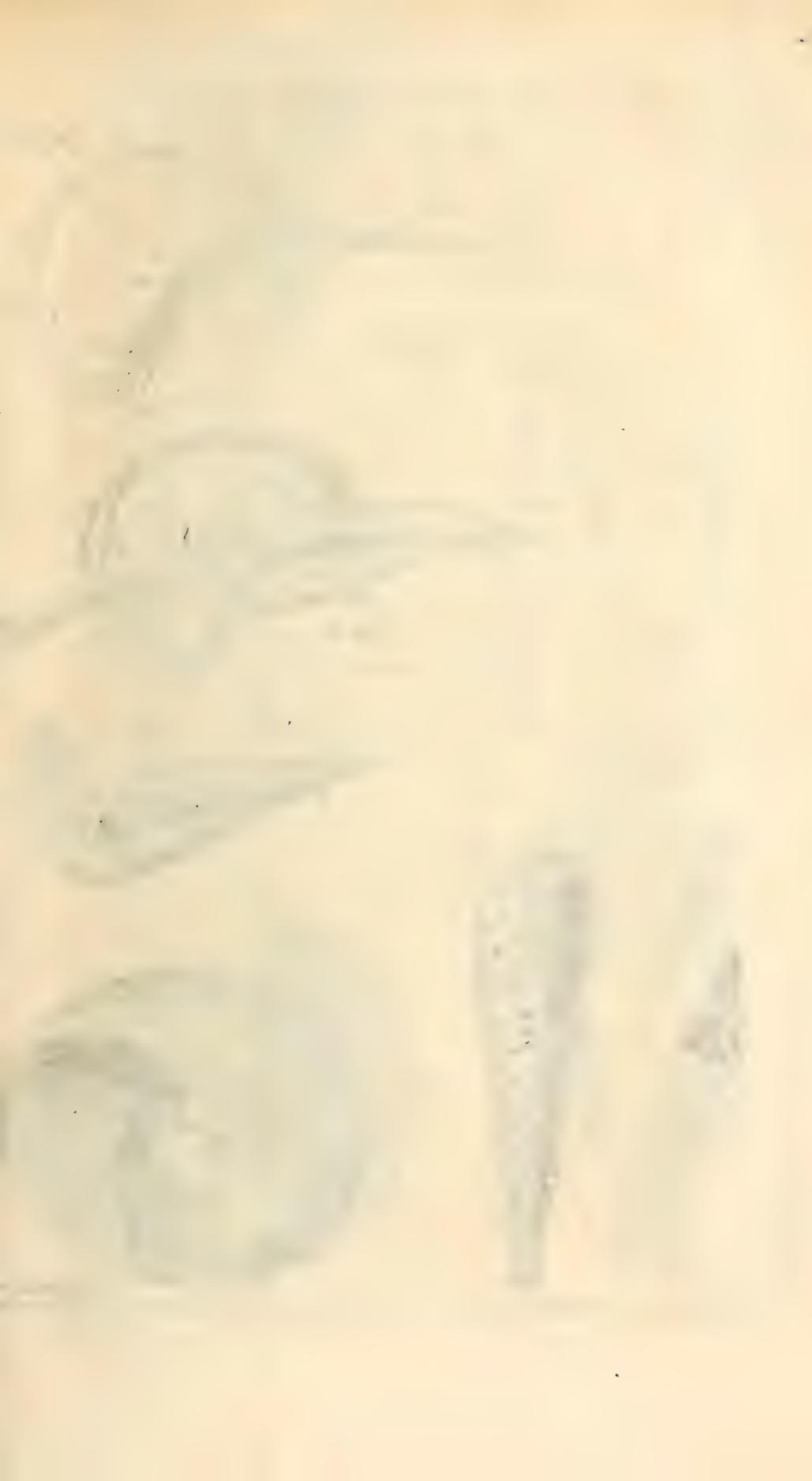
*Fig. 14.* is the section of a shell, where the tail of the animal has been obliged to abandon the first turns, because they are grown quite solid. The letters A A A A A A mark the spaces, which at first were occupied by the body of the animal, and afterwards filled up. It shews also that part of the space EB is become solid, namely that which is marked E, the body of the animal occupied only the spaces BB, D D D D, &c.

CCCC are those eminences of shells, which I have called horns, or sections of those eminences.

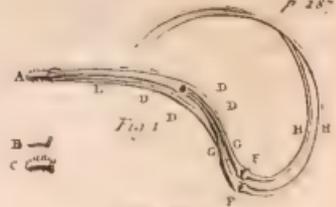
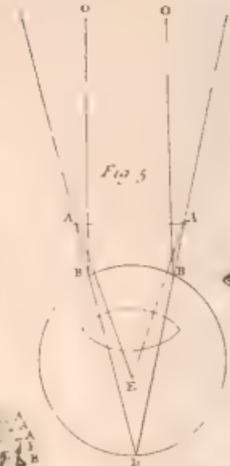
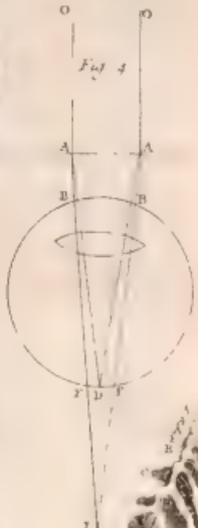
*Fig. 15.* is the transverse section of a shell, which after having made a certain number of spiral turns in C C C C one way, turns back again in D D D.

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.

CCC are eminences, or little horns. *Fig.*



Pl. III. C. III.



*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, CCC, DDD 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, DD, A A, C C. It appears, that the inner surface DD, 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 terrestrial 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

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 lose 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  $\frac{1}{4}$ th part of the whole, it is certain ere they reach the body exposed to the fire, they must pass through the vessel when in 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 siter 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 *phenomenon*.

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 repositied 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 perceivable,

able, it must force its prison doors, and make an attack upon some other body; but the cells it is repositèd 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 deliver'd 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 particles

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

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 confirmed 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 meshes thereof, will do it no damage; but if a body come which is too bulky to pass thro' those meshes, 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 hard'y 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.

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 their 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 alcali 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.

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 tempered

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 decomposing 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 surpris'd, that calcined metals, and all bodies in general, which have procur'd 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 diffus'd 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 pressures of the flame, to a distance greater or less; but when only little particles of luminous substance, are diffus'd from solid bodies, they presently become so darkened by the air around them, as to be disabled from making pressures efficacious, and extended enough, to become sensible to the eye.

Upon the whole we conceive, that the matter of light lodged in inflammable bodies, expos'd 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 to 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 expos'ing very inflammable bodies, as paper or straw, to a too small fire, they sometimes consume intirely without casting any flame, by reason the external agent being

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 *phenomena*, 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 inroach on the subject of future ones.

I shall only here observe, that all *phosphori* in general may be considered as a kind of sponges, 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  
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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  $10^{\circ}$ , and that of *M. Amontons's* at the  $53^{\circ}$ : 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, 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  $51^{\circ}$ —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 seem'd 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  
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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  $51^{\circ}$ —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 Ikan*, notwithstanding its natural storminess, 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  
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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 loses 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 befall 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.

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Now I allow, that fluids contain a great deal of air; that this air is more compressed in the fluids than in the open air; that the frost gives occasion for its springs to unbend themselves; and that these springs unbend with the more force, on account of the compression they are in, and what is more, that this unbending of the springs of the air is the cause of the lightness and rarefaction of ice, as well as of the bubbles and tubules mentioned in my observations; but I cannot allow, that the action of these springs is the cause of evaporation, when I consider that both the fluids which freeze, and such as withstand the frost, undergo an evaporation proportional to the tenuity of their parts, and that ice, several days old, looses full as much as water just beginning to freeze. In fluids which do not freeze, the unbendings of the springs of air cannot be very considerable; and in ice, formed many days, those springs must have had their full play, and now left incapable of any further action.

It has been observed, that when the ice begins to form, its surface is full of wrinkles, which are sometimes disposed in parallel lines, and sometimes after the manner of *radii*, under which surface is a multitude of little frozen particles, in form of needles, or rather of funnels, whose small end is turned to the surface of the water. These funnels are easily perceived in a cylindrical phial, when the liquor contained in it is entirely froze.

Now this disposition of the ice thus beginning to be formed, is favourable to the contained air's escaping out upon the spring's beginning to unbend, and seems at the same time to prohibit the entrance of the external air, which might otherwise take its place. Thus the air which re-

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; corrosive 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 ferous and ymphatick part, which will remain inclosed in its sulphurous ones, and only to be extricated threfrom by a general liquefaction. This l iuefaction 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, occasion some serous parts to be separated from them; whence the hoarseness and cough, which are frequently accompanied with an involuntary weeping, by reason of the lachrymal points, which are stopped by the thickening of the mucus in the nose. —

Accordingly the two catarrhs are to be treated after a very different manner; those from cold, are cured by remedies, which restore the humours to their fluidity; and where there is a stoppage of the head, the readiest remedy I know is, the perfume of amber, which doubtless acts by the quantity of volatile salt, and sulphur, contained therein; wine and brandy burnt with sugar, and tea, coffee, and chocolate, are proper for the same reason; several violent and very obstinate colds I also knew cured that winter with chicken broth, wherein an ounce of snakes flesh dried, with a handful of cresses had been boiled about  $\frac{1}{4}$  of an hour.

As to catarrhs caught in the thaw, care must be taken to prevent the too great dissolution of the humours, by boiled emulsions, rice-milk, water-gruel, barley-water, and yolks of eggs, with sugar-candy, whey, and milk itself; narcotics, and phlebotomy, are proper in either kind of catarrh; and especially where the patient is harrassed with a cough, or any inflammation of the breast 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 observes, that it has not increased since the year

316 *The HISTORY and MEMOIRS of the*  
 1703, when he observed it also to be 11 de-  
 grees.

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\*.*

8	43	36	Beginning of the eclipse at <i>Nuremberg</i> .
8	33	49	Beginning of the eclipse at <i>Genoa</i> .
8	20	45	Beginning of the eclipse at <i>Marseilles</i> .
0	9	47	Difference of the meridians between <i>Genoa</i> and <i>Nuremberg</i> .
0	22	51	Difference of the meridians between <i>Marseilles</i> and <i>Nuremberg</i> .
11	6	34	End of the eclipse at <i>Nuremberg</i> .
10	57	21	At <i>Genoa</i> .
10	41	26	At <i>Marseilles</i> .
0	9	13	Difference of the meridians between <i>Genoa</i> and <i>Nuremberg</i> .
0	25	8	Difference of the meridians between <i>Marseilles</i> and <i>Nuremberg</i> .

XVI. *Reflections on the observations of the*  
*eclipse of the sun, March 11, 1709, made*  
*in different countries, by M. Cassini the*  
*son\*.*

At <i>Montpellier</i> the end of the eclipse	"	"	"
was observed exactly at	2	55	49
We find by the figure drawn up			
for the meridian of <i>Paris</i> , that it must			
have happened there at	2	49	30

\* March 2, 1709.

† April 17, 1709.

Which

ROYAL ACADEMY of SCIENCES. 317

	h	'	"
Which gives the difference of the meridians	0	6	19
At <i>Genoa</i> the beginning of the eclipse was observed exactly at	0	59	52
It must have happened by the fi- gure at	0	34	0
Which gives the difference of the meridians	0	25	52
At <i>Bolonia</i> the end was observed with some ambiguity at	3	34	35
It must have happened by the fi- gure at	2	58	40
Which gives the difference of the meridians	0	35	55

A

# T A B L E

O F T H E

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.

- I. **O**N the progressive motion of several species of shell-fishes.
- II. Of the glass ware of India.
- III. Of a sort of acorn from Coromandel.
- IV. Of the virtues of a nut called Bicuiba.
- V. Of a woman delivered of a child, when above 80 years of age.
- VI. Of the fatal effects of some vapours in a baker's cellar.
- VII. Of a remarkable echo.
- VIII. Of figured stones.
- IX. Of M. John James Scheuckzer's Herbarium diluvianum.
- X. Of the count Marfigli's philosophical essay towards a history of the sea.
- XI. Of a tænia found in a tench.
- XII. The discovery of an extraordinary sort of insect.
- XIII. On the pond muscles.

In the MEMOIRS.

- I. Experiments on the elasticity of the air, by M. Carré.
- II. Ob-

- 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.*
- III. *A comparison of the observations which we have made here at the observatory on the rain and winds, with those which M. le Marquis de Pontbriand made at his castle near St. Malo, during the year 1709, by M. de la Hire.*
- IV. *A comparison of my observations with those of M. Scheuchzer, on the rain and constitution of the air during the year 1709, at Zurich, in Switzerland, by M. de la Hire.*
- V. *Of the necessity of centering well the object glass of a telescope, by M. Cassini the son.*
- VI. *Observations on the bezoar, and on other substances, which come near to it, by M. Geoffroy, jun.*
- VII. *An insect of snails, by M. de Reaumur.*
- 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 the son.*
- IX. *Observations on a sort of talc, which is commonly found near Paris, upon banks of plaister-stones, by M. de la Hire.*
- X. *Observations on the variation of the needle, with regard to Dr. Halley's map, with some geographical remarks made upon some sea-journals, by M. de Lisle.*
- XI. *Reflections on the observations of the flux and reflux of the sea, made at Havre de Grace, by M. Boissaye du Bocage, professor of hydrography, during the years 1701, and 1702, by M. Cassini the son.*
- XII. *Reflections on the observations of the tides made at Brest, and at Bayonne, by M. Cassini the son.*

- XIII. *An examination of the silk of spiders, by M. de Reaumur.*
- XIV. *Experiments on the effects of the wind, with regard to the thermometer, by M. Cassini the son.*
- XV. *Experiments on the thermometers, by M. de la Hire the son.*
- XVI. *An observation on the little hen-eggs without yolk, which are commonly called cock's-eggs, by M. Lapeyronie.*
- XVII. *A comparison of the observations of the eclipse of the moon, Feb. 13, 1710, made in different places by M. Maraldi.*

A N  
A B R I D G M E N T  
O F T H E

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.*

**A**Ltho' 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 fides. In looking at a muscle, we should not guefs 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 *sy-stole* 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.

324 *The HISTORY and MEMOIRS of the*  
*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, proceeding 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*.

CC the horns or tubes.

Fig. 13. a *tellina* bending its leg to raise itself.

Fig. 14. another species of *tellina* opened.

I the leg.

CC the horns or tubes.

Fig. 15. a *tellina*, with its leg thrust forth, ready to enter the sand.

AA the bounds of growth, marked so distinctly on the shell, that they look like small pieces stuck upon larger.

CC 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.

AAA, &c. that part of the animal which is uncovered by the shell.

T the head.

CC 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 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.

AO 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'anés*.

BB 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.

AHIFBD a space where only the strait canals appear.

ACIFRA a space where only the circular canals appear.

IFT O 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. 25.* the mouth reversed.

CC the circumference where the horns are fastened.

OOOO 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.

DD the circumference or base.

CD 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.

RR, &c. the trunk divided into 8 branches.

PP 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.

RR, &c. the branches into which the trunk is divided.

OO, &c. the apertures of each of these branches.

*Fig. 29.* a sea-star.

RR one of its rays laid open.

BB 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.

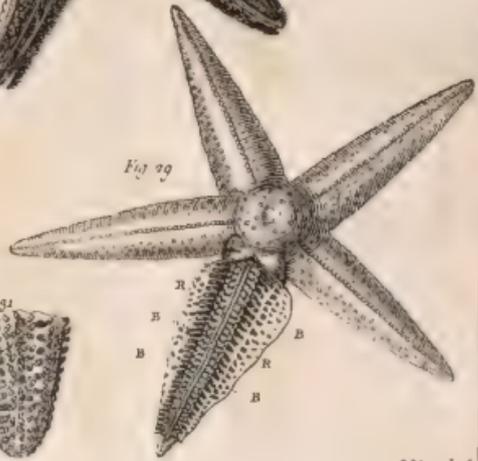
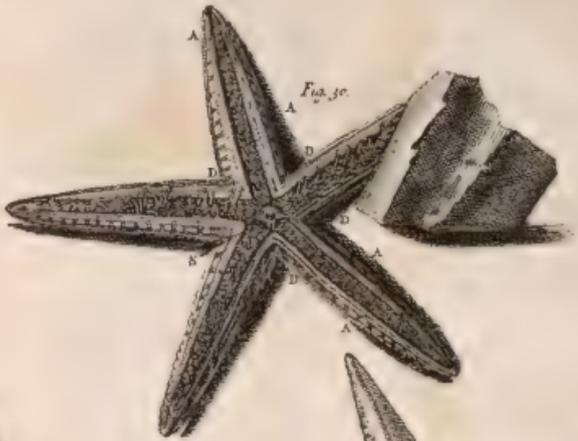
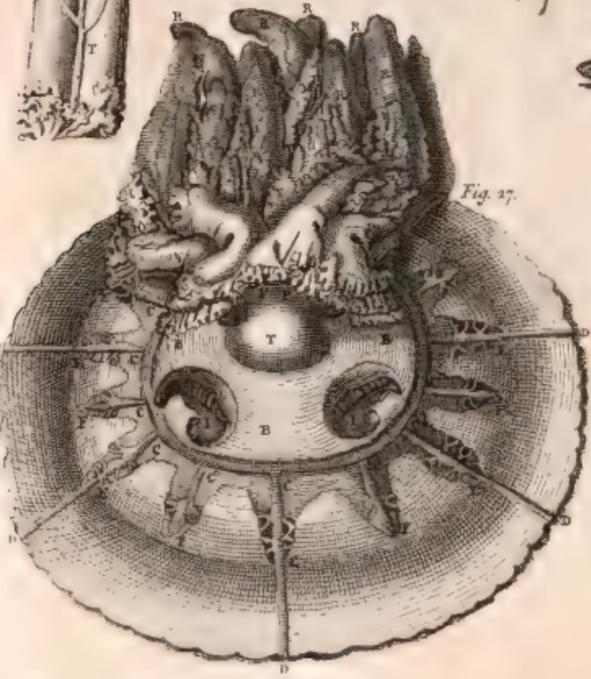
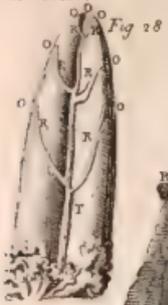
R R R the skin.

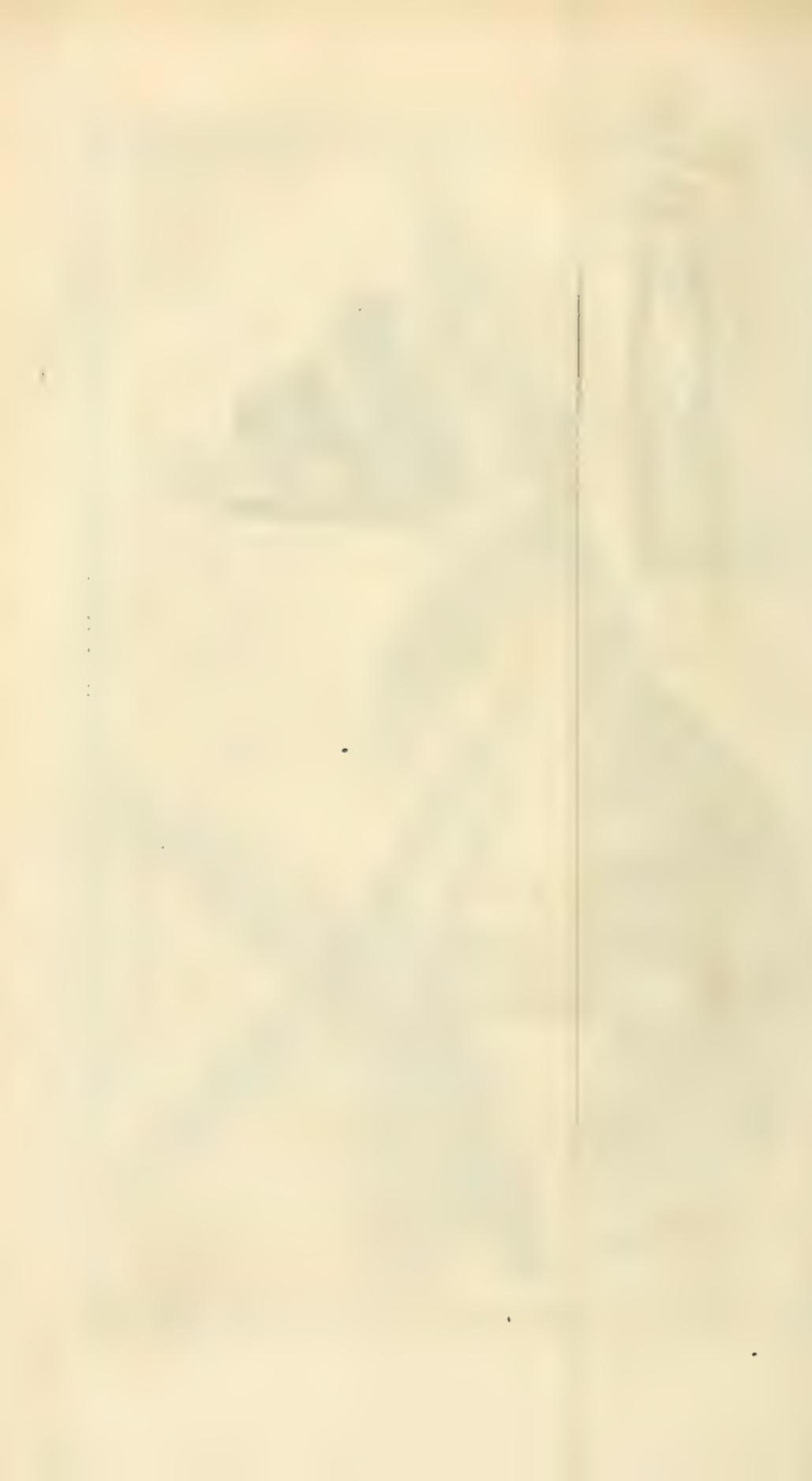
## II. *Of the glass-ware of India.*

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





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 fallads, 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 pulley, 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

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 *Switzerland*, 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 often 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 basin half

\* Vol. I. pag. 410. † Vol. III. pag. 77. ‡ Vol. III. p. 6.

full of water, turn pretty fast round its centre; till at last the water has taken all the swiftness of the basin, 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 supercedes 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* 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 forests 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-

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 Marfigli's philosophical essay towards a history of the sea.*

The count *Marfigli* 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 *a'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; 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 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 unwholesom 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 32<sup>d</sup> 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 29<sup>th</sup> 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  $\frac{1}{30}$ , 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 *Marfigli* 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 lithophytions, a marine plant. He has begun experiments at different times upon the tartarifications of the sea, which could not be carried far enough.

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-

344 *The HISTORY and MEMOIRS of the*  
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 be-  
tween *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 percussive  
that it makes against the rocks, drives it some-  
times 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  $\frac{1}{2}$  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 surpris'd 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 appear-  
ances, would hardly be taken for an animal, and  
at most but for an egg, should however leap in a  
garden, raising itself  $\frac{2}{3}$  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.

346 *The HISTORY and MEMOIRS of the*  
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 respire 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 ani-  
mal of its own *species*, and is itself alone both fa-  
ther and mother of what it produces. Here is a  
quite new idea of an animal; it is the pond mus-  
cle, the structure of which, M. *Mery* has disco-  
vered, notwithstanding its shape is so odd, and  
discourageing, on account of its excessive sin-  
gularity.

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 refer-  
voirs or canals contained between the interior sur-  
face 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 ca-  
nals to receive the water. One throws sever-  
al 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 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 it is generally sunk at the bottom of the water.

It has ovaries, and feminal 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 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 shell's did not grow in the same manner as the flesh, these which are fastened at first in certain places of the growing muscle, must continually 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.

A N  
A B R I D G M E N T  
O F T H E

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. Cham-  
bers.

**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 impor-  
tance to be given up, either upon M. Parent's  
experiments, or his reasonings without some fur-  
ther examination; for it can never be too much  
considered, how liable we are to fall into errors,  
in drawing conclusions from one or two experi-  
ments, 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 extraor-  
“ dinary and surprizing 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 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 physical truths;  
 “ which time alone can give them.”

Here follow my experiments. ——— I procured 4 little glass phials to be made with long necks, like those used by *M. Parent*, and prepared after the same manner. The first was full of common air; the 2d exhausted 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 sealed them all hermetically, and laying one after another upon the burning coals, the consequence was, that the phial, which contained nothing but common air, and which remained some time without shewing any effect, as being somewhat thicker than the rest, opened at a place where it was somewhat stretched before, and produced a kind of hissing by the air issuing from it, without any great violence. The second had pretty much the same effect, but the hissing was somewhat more considerable, the part of the phial most heated having stretched somewhat further, and yielded more quickly. The third made a violent detonation, and burst into very little pieces in a very short time. The fourth likewise burst with some noise, and very quickly, tho' only a very small hole was made in it.

After this, I made 4 other little phials like the former. The first, which was full of air, remained on the coals a considerable time, ere it produced its effect; but it stretched till at length it burst, with a considerable noise, and discovered a large aperture.

The second, which was likewise full of air, produced much the same effect, but with less noise, the part at which it opened was stretched more, and the hole smaller.

The third and fourth, which were emptied of air, sunk inwards without bursting; and especially

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 saltpetre, 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 accordingly 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 mechanisim, 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 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.

	<i>Lin.</i>		<i>Lin.</i>
Jan.	22 $\frac{1}{8}$	July.	18 $\frac{2}{8}$
Feb.	13 $\frac{7}{8}$	Aug.	10 $\frac{1}{8}$
March	20 $\frac{2}{8}$	Sept.	29 $\frac{2}{8}$
April	37 $\frac{6}{8}$	Oct.	17 $\frac{1}{8}$
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 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 lots 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 comparifon of thermometers in different countries, and for the fame time, unlefs the thermometers have been rectified by one another in all forts of degrees of heat and cold, and I believe it will not 'be poffible to find two equal; that is, of which equal degrees in the divifions 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 obfervatory; I found it at the higheft at 28 inches, 3 lines  $\frac{1}{2}$  *Jan.* 19, with a calm and ferene sky, which was about the time of the greateft cold; and the 31ft of *December*, it was at 28 inches, 3 lines  $\frac{1}{6}$ , with a very thick fog and calm. It was alfo feveral times beyond 28 inches, with different winds, partaking rather of the N. than of the S. and always without rain. I obferved this barometer at the loweft at 26 inches, 7 lines  $\frac{1}{2}$ , with a ftrong fouth wind, and moderate rain, *Dec.* 16. The difference between the greateft and leaft heights of the barometer, was therefore 1 inch, 8 lines, which is a little more than the mean difference that is obferved here, and is 1 inch, 6 lines. This instrument was pretty exact in foretelling rain and fair weather, according to the common notion.

I obferved the declination of the loadftone with the fame needle of 8 inches long, and in the fame place where I ufed, and as I have fhewed in the memoirs of the preceding years. The 24th of *Dec.* laft, I found this declination  $10^{\circ} 30'$  toward the W. from whence we know that this declination increafes almoft the fame quantity each year.

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

	<i>Lines.</i>		<i>Lines.</i>
Jan.	33 $\frac{1}{4}$	July	18 $\frac{1}{4}$
Feb.	17 $\frac{1}{2}$	Aug.	5 $\frac{1}{4}$
March	30 $\frac{1}{4}$	Sept.	5
April	30 $\frac{1}{2}$	Octob.	14
May	26 $\frac{1}{4}$	Nov.	3 $\frac{3}{4}$
June	23 $\frac{3}{4}$	Decem.	17 $\frac{1}{4}$

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  $\frac{1}{4}$  of water, and at *Paris* only 22 lines  $\frac{1}{2}$ . 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, becaule 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  $\frac{3}{4}$  of water, and at *Paris* 45 lines  $\frac{1}{7}$ : also at *Paris* the 25th and 26th, it rained 9 lines, and at *Pontbriand* only 2  $\frac{1}{7}$ .

In *August* we had a storm in the night between the 11th and 12th, with 7 lines  $\frac{1}{2}$  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 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  $\frac{2}{3}$ . 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  $\frac{1}{2}$ , 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  
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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 impressiion 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 *GH*, in such a manner, that the rays *GL*, *HL*, which proceed from this point, and fall upon the surface of the lens *GH*, 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 *ABCD* to *abcd*, 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* *SFT*, which passes through the centre of the glass *F*, and the rays which proceed from the point *T*, and fall upon the eye-glass *GH*, 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* *EL* 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 irregularities,

larities, which cannot be remedied, but by centring the two object-glass's 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 *Chaldian* 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 *juvial 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 browses 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 *castia*, 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 founded like eagle-stones. I have attempted to prick some of these stones with a red-hot needle to see whatehy were counterfeit, but it did

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 flattened, 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 *Agypt*, *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 *Clytus* calls it *Caprivaiva*.

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 distinguished by its being paler. It is sometimes of a light grey, ingenerated 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 Baubinus* 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 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 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 intestines;

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 *CC*, 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 top of the boards bordering the key, near the sluice of the basin, 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. *Bacri'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 subtracted 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 subtract 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<sup>h</sup>—13' in the evening, and it was high water at 11<sup>h</sup>—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 — 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 — 20'  $\frac{1}{2}$  in the morning, the new moon for that day being at 10<sup>h</sup>—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 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<sup>h</sup>—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<sup>h</sup>—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<sup>h</sup>—30', within 37' of the observation, which, in some measure, reconciles those two observations, which were 1<sup>h</sup>—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. Baer'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

392 *The HISTORY and MEMOIRS of the*  
the conjunctions or oppositions immediately following.

For an instance, in the full moon which happened on the 21<sup>st</sup> 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 26<sup>th</sup> 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 8<sup>th</sup> of *April*, the distance of the sun from the moon's apogee being 1<sup>st</sup>. ——— 0<sup>th</sup>. ——— 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 10<sup>th</sup> 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 26<sup>th</sup> of *March*, than on the 10<sup>th</sup> of *April*; but for the same reason, in the following full moon of the 22<sup>d</sup> of *April*, the distance from the equinox being increased, the tide should have been lower than on the 10<sup>th</sup> 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 8<sup>th</sup> of *April*, than on the 22<sup>d</sup>: whence it appears, that the highest or lowest tides bear a nearer relation to the distance of the moon from

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 of 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 $\frac{1}{2}$	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	$\frac{1}{2}$ 11 33 $\frac{1}{2}$	6 8
☉	15 Dec. 10 16 M.	11 55	11 57	6 11
●	29 Dec. 10 47 M.	11 51	$\frac{1}{2}$ 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	$\frac{1}{2}$ 12 14	5 6
●	28 Mar. 11 7 M.	12 10	$\frac{1}{2}$ 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 conjunction and Oppof.		Day of the highest tide.	Height of the water herein.
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
22	22	55	93519	15 April.	3 10 $\frac{1}{2}$
0	5	52	106496	26 April.	5 4
				13 May.	4 4
				29 May.	6 4

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 quar-

ter, 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.	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	5 11 $\frac{1}{2}$	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 $\frac{1}{2}$	5 2	6 6 6
3	□ 28 July	2 23 M	5 20 $\frac{1}{2}$	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 $\frac{1}{2}$	5 28	7 5
1	□ 7 Dec.	7 31 M	5 58	5 25	7 5
3	□ 22 Dec.	8 48 E.	5 15 $\frac{1}{2}$	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 $\frac{1}{2}$	7 8
3	□ 19 Feb.	6 33 M.	5 23	5 27	6 2
1	□ 6 Mar.	10 24 E.	4 33 $\frac{1}{2}$	4 55	9 2
3	□ 20 Mar.	3 47 E.	4 35	5 9	7 10
1	□ 5 Apr.	2 9 F.	4 57	5 12	5 2
3	□ 19 Apr.	3 39 M.	5 27 $\frac{1}{2}$	5 33	8 2
1	□ 5 May	1 59 M.	5 45 $\frac{1}{2}$	5 36 $\frac{1}{2}$	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
			5 January.	8 5
8 28 20	106425		20 January.	7 8
9 10 2	97717		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 subtract  $2'$  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 5 — 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 5 — 6', give 5 — 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 11 — 54' in the morning, and in the quadratures at 11 — 6', we have 5 — 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 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 — 8' in the evening. After which the sea sunk some inches, but rose again at 0 — 57' to the same height; it had been at 49' before, where it remained till 1 — 10'; and *M. Baert* fixed the time of high water for this day at 1 — 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.

h 11—54 mean time of high wa- ter at <i>Dunkirk</i> , on the day of new and full moon.	h 5—6 mean time of high wa- ter at <i>Dunkirk</i> , on the days of the quadratures.
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A TABLE of the retardation  
of the tides.

Day and hour at ter new r full moon.				Retarda- tion of the tides				Day and hour af- ter first or last quater.				Retarda- tion of the tides				Dif.
D.	H.	H.	M.	M.	D.	H.	D.	H.	M.	D.	H.	D.	H.	M.		
0	00	0	26		0	00	0	0	32							
	12	0	26	24		12	0	32	36							
1	00	0	50		1	01	8		41							
	12	1	11	21		12	1	49								
2	01	30	19		2	02	32		43							
	12	1	48	18		12	3	11	39							
3	02	6	18		3	03	44		33							
	12	2	24	14		12	4	14	30							
4	02	42	18		4	04	40		26							
	12	3	1	19		12	5	4	24							
5	03	21	20		5	05	28		24							
	12	3	41	20		12	5	50	22							
6	04	2	21		6	06	12		22							
	12	4	21	19		12	6	34	22							
7	04	39	18		7	06	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 subtracted 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<sup>h</sup> — 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 subtracted from 11<sup>h</sup> — 54, on account of the full moon's coming after the time of high water, the remainder is 11<sup>h</sup> — 43', the true time of high water in the morning of the 22d of *April*. *M. Baert* observed it that day at 11<sup>h</sup> — 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<sup>h</sup> — 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 — 58', whose double 30 is the number of min. to be added to 5 — 6', by reason the time of quadrature anticipates the mean time of high water. The sum, *viz.* 5<sup>h</sup> — 36' in the evening, gives the true time of high water on the 16th of *April*. *M. Baert* found it that day at 5<sup>h</sup> — 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 subtract 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 — 36 in the morning, the difference between 8 — 36 in the morning, and 11 — 54 the mean time of high water at *Dunkirk*, is 3' — 18"; the double whereof 6' — 36", being added to 11 — 54. gives the time of high water at 12 — 1', on the 24th of *March*, the day of full moon. To this add, 1 — 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 — 30' and 1 — 14', *viz.* 19', which subtracted from 1<sup>h</sup> — 31', the time of high water in the evening, gives 1<sup>h</sup> — 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  $6^{\circ}-32'$  in the morning, and  $5^{\circ}-6'$  in the evening, the mean time of high water at *Dunkirk*, in the quadratures, is  $10^{\circ}-34'$ ; whose double  $21^{\circ}-8''$  being added to  $5^{\circ}-6'$ , by reason the time of the 3d quadrature comes before  $5^{\circ}-6'$ , gives the time of high water at *Dunkirk*. On the 31st of *March*, 1701, the day of the last quadrature, at  $5^{\circ}-27'$  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  $6^{\circ}-12'$ , which added to  $5^{\circ}-27'$ , gives the time of high water at *Dunkirk*, for the 6th of *April*, 1701, at  $11^{\circ}-39'$  in the evening. — To find the time of high water, which happened in the morning, take the difference between  $6^{\circ}-12'$ , and  $5^{\circ}-50'$ , viz.  $22'$ , which subtracted from  $11^{\circ}-39'$ , the time of high water on the 6th of *April*, in the evening, gives  $11^{\circ}-17'$ , for the true time of high water in the morning of the same day. M. *Baert* found it  $11^{\circ}-21'$  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  $8^{\circ}-36'$  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  $1^{\circ}-31'$  in the evening. M. *Baert* observed it high water this day at  $1^{\circ}-31'$  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 paralleliped, and consequently its fragments will

\* July 19, 1710.

all be parallelipeds, 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  $78^{\circ} - 30'$ : this I have learnt by my observations.

In this paralleliped 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  $75^{\circ}$  each, which are the complements of the former.—— These measures are somewhat different from those of Mess. *Bartolin* 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 *phenomenon* 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 pa-

rallel to that whereby the obtuse angle of the face the object is upon is bisected.

This double image of the same object shews, that there must be a double refraction in these bodies, and accordingly two very different ones have been distinctly observed. The first, common to that found in all transparent 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 second, peculiar to this crystal, and arising from another inclination of the incident ray, to another line inclined to the same face. — Hence it follows, that if the incident ray be united with one of these lines, it will not undergo the refraction belonging to that line, but will undergo that depending on the other; and consequently the image will always be double in every other inclination.

I have made several experiments, and have likewise repeated them several ways; the result whereof is, that in the first of the two refractions, the sine of the angle of incidence in the air, is to the sine of the angle refracted in the body as 5 to 3; whence we learn, that this refraction of the body, notwithstanding its softness, surpasses that of the glass, 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, *Bartolin* takes it to depend upon a line, or ray, always parallel to the edges of the faces next those where the refraction is made; but *Huygens* denies this line to be parallel to those edges; for myself, after examining the point with great attention, I find this line more perpendicular to the surface of the crystal by  $1^{\circ}$ , which is no great matter in an enquiry of this kind: further I find the sines 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  $4\frac{1}{2}$  to 3, which is much like that of glafs.

'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  $\frac{1}{2}$  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 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, proceeding 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 *laminae*, which are always covered with a thin yellowish crust; and

\* Plate VI. fig. 9.

hence objects only appear very confusedly thro' these sides unless the crust be removed, and a varnish laid over, which is not easy to execute, by reason of the small connection between the *laminae*.

One of the points of the fork is sometimes found a little separate from its piece, being only joined irregularly thereto, by a little of the fatty earth; and upon separating them quite, we find that these points only adhered to the rest by pieces of *laminae*, about a line thick, which enter more or less into the body of the stone, and make bond as the masons call it therewith.

Upon removing some of the rough *lamellæ* on the surface of this talc, we clearly discern lines therein, as EF proceeding from the middle line AB, towards the edges on either side; and making an acute angle AEF, with the same middle line towards the fork A, -of about  $60^{\circ}$ : we also perceive other lines as GH proceeding from the middle towards the edges, and making an acute angle BGH towards the point B of  $50^{\circ}$ , so that the acute angle formed at the meeting of those 2 lines is  $70^{\circ}$ .

Hence it always happens, that upon cleaving the talc into thin pieces, which can only be done with a sharp knife, beginning at the exterior edges after first removing the crust: most of these *laminae* break into triangles, whose angles are constantly 50, 60, and 70 degrees, which is a very singular property of this stone. We also find certain fragments of those thin *lamellæ*, in figure of a parallelogram composed of 2 of these triangles joyned together.

Hence we may probably infer, that the mass of these talc stones, consists only of thin *lamellæ* slenderly fastened 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  $50^{\circ}$ ,  $60^{\circ}$ , and  $70^{\circ}$ , as appears from the pieces of broken *lamelle*, 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 arrangements in forming the *lamelle*, the sides of those *lamelle* will either be parallel to the line in the middle, or inclined thereto in  $10^{\circ}$ , which also forms the point of the piece; the faces whereof are always inclined  $10^{\circ}$  to the middle line on either side, when they are inclined at all, which happens almost universally; for the angle  $AEF$  being constantly  $60^{\circ}$ , and the angle  $BGH$ , or  $BEI$ , or  $BEK$   $50^{\circ}$ , the angle  $FEI$  or  $FEK$ , will necessarily be  $70^{\circ}$ ; and if the triangle  $FEI$ , whose angle  $FEI$  should be  $70^{\circ}$ , have its angle  $EFI$   $60^{\circ}$ , 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  $10^{\circ}$ , 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 *lamelle* might become inclined to each other, in an angle of 10 degrees; for I only here consider  $\frac{1}{2}$  the intire *lamelle* which is always divided into two by a line, as AB; but if in such formation of the *lamelle*, 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 *lamelle* parallel to each other.

It was in this formation of the *lamelle*, that they acquired their hardness, which became pretty considerable by their elements joining to each other at their sides; but the *lamelle* 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  $50^{\circ}$  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  $10^{\circ}$  towards the point, it follows, that the angle of the fork CAD must be 120 de-

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 *laminæ*: in fine, we find some pieces of talc which have others fastened on their sides; in others the point is extended into a parallelliped 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 halves 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 wiew 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  $110^{\circ}$ , 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 prominent 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

**A** *Lbidade*, is an *Arabic* word, used to express a moveable rule applied to an instrument for observing heights and lengths. It is also called a *dioptr*.

*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 immersed 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  $\frac{1}{2}$  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





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 faucisse.

*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

## 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.

A

# GENERAL INDEX

OF THE

## CONTENTS

OF THE

### THIRD VOLUME.

A

	pag.
<b>A</b> CORN <i>from</i> Cotomandel —	328
Air, <i>the dilatation of it by boiling water considered.</i>	110
———— <i>Experiments on the elasticity of it.</i>	350
America, <i>remarks on the navigation of its coasts.</i>	125
Amplitude, <i>what it is.</i>	15
Animalcules, <i>the multiplication of them.</i>	10
Astaboras <i>of the ancients thought to be the Tacaze.</i>	137
Astape <i>of the ancients thought to be the Dender.</i>	138
Atbara, <i>a name given to the Tacaze.</i>	ib.
Atmosphere, <i>the weight of it.</i>	220
Aurora borealis <i>seen at Berlin.</i>	13

B

Baker's cellar, <i>the fatal effects of some vapours there.</i>	329
Barometer <i>observed.</i>	22, 95, 171, 360, 364
———— <i>luminous.</i>	23
———— <i>a new one described.</i>	96
———— <i>the method of measuring the heights of places by</i> <i>it.</i>	73
Barometrical observations, <i>made at Paris and at Zurick,</i> <i>compared.</i>	177 Baro-

# INDEX.

	pag.
Barometrical observations upon St. Baum, and the neighbouring mountains.	142
————— in several places compared.	229
Batavia, the difference of the milk of European women there, and that of Negresses.	12
St. Baum, barometrical observations made there.	142
Beguignes, the height of it.	142
Berlin, an aurora borealis seen there.	133
Bezoar, observations on it.	369
Bernard the hermit, a sort of fish.	326
Bicuiba, a sort of nut, its virtues.	329
Bitumen contained in the sea-water.	342
Boca Cinca, its latitude.	152
Blood, circulation of it in insects.	11
Bolonia, the difference of its meridian from that of Paris.	317
Bombs considered.	84
Brazil, its position in the maps erroneous.	130
Burning-glasses of the ancients.	88

## C

Cancellus, a sort of fish.	326
Cancers cured.	329
Candace, the common name of the queens of Meroe.	130
Cape Horn, its position in the maps erroneous.	128
Cartesian system of the cause of gravity vindicated.	201
Carthage, its latitude.	153
————— longitude.	153, 154, 155
Catarrh, an epidemical one, which succeeded a thaw.	313
Cats, the particular structure of their eyes.	192
———— plunged in water, the effect on their eyes considered.	193
Centring well the object-glass of a telescope, the necessity of it.	366
Chama or purr, a sort of shell-fish.	322
Circulation of blood in insects.	11
Cobwebs, how made.	33
Cold, coming with a south wind.	165
Colick eased	329
Coromandel, a sort of acorn from that country.	328
Crabs-eyes, observations on them.	245
Cray-	

# I N D E X.

	pag.
Cray-fish, <i>observations on them.</i>	244
Crystal, <i>a dissertation on it.</i>	81
————— <i>of Island.</i>	408
Cuttle-fish, <i>the eggs of it considered.</i>	87

## D

Declination of the needle observed.	23, 95, 105, 172, 315, 360
Dender, <i>a considerable river flowing into the Nile.</i>	137
————— <i>thought to be the Astape of the ancients.</i>	138
Dunkirk, <i>the tides observed there.</i>	381

## E

Echo, <i>a remarkable one near Verdun.</i>	331
Eclipse, of the moon at Nuremberg, Genoa, and Mar- seilles.	316
————— <i>sun in different countries.</i>	316
Egagropilæ, <i>what they are.</i>	376
Ethiopia, <i>the maps of that country very erroneous.</i>	131

## F

Fire, <i>conjectures on the matter of it.</i>	287
Fire-arms differently charged.	6
Fishes petrified.	82
Flints, <i>their generation.</i>	8
Fluids, <i>the evaporation of them in cold weather.</i>	305
Flux and reflux of the sea observed at Dunkirk.	381
Friction, <i>light of bodies produced thereby.</i>	3
Frosts, <i>some effects of them.</i>	305

## G

Galets, <i>a sort of stones.</i>	6
Genoa, <i>difference of its meridian from those of Mar- seilles and Nuremberg.</i>	316
————— <i>from that of Paris.</i>	317
Glasses, <i>why the tenderest are least subject to break by fire.</i>	67
Glass-ware of India.	328
Vol. IV. N <sup>o</sup> . 34.	D d d
	Gojame

# I N D E X.

	pag.
Gojame <i>falsly supposed to be the Meroe of the ancients.</i>	133
Golfo-triste, <i>observations made there.</i>	149
Gravel <i>expelled from the kidnies.</i>	12
Gravity, <i>the Cartesian system of the cause of it vindicated.</i>	201
Gueguere <i>falsly supposed to be the Meroe of the ancients.</i>	133
Guerre, <i>a city of Ethiopia.</i>	141
Gunpowder, <i>the effect of it, chiefly in mines.</i>	44

## H

Hearts, <i>two in a pullet.</i>	167
Hogs, <i>injurious to cray-fish.</i>	250
Horizon of the sea, <i>the irregularities of its apparent depression.</i>	27
————— <i>the apparent depression of it.</i>	146
Horses, <i>a method of stopping them suddenly.</i>	91

## I

Ice, <i>why it melts faster in vacuo, than in air.</i>	67
— at Copenhagen, <i>its thickness in the hard winter of 1709.</i>	167
— <i>the thickness of it on the Thames in 1683.</i>	ibid.
Iguana, <i>a sort of American lizard.</i>	12
India, <i>the glass-ware of that country.</i>	328
Insects, <i>circulation of the blood in them.</i>	11
————— <i>an extraordinary sort.</i>	344
————— <i>upon snails.</i>	376
Island, <i>a new one near Santerini.</i>	13, 70

## L

Latitude of Bocachica.	152
————— Carthagena.	153
————— Fort St. Louis in Domingo.	155
————— Santa Marthe.	150
————— Martinico.	156
————— Porto-Cabeillo.	149, 150
————— Porto-Bello.	151
————— St. Thomas <i>Island.</i>	155
I	Lepas,

# I N D E X.

	pag.
Lepas, <i>a sort of shell-fish.</i>	322
Light of bodies produced by friction.	3
—— of the barometer.	23
—— conjectures on the matter of it.	287
Limat, <i>a river of Switzerland, the augmentation and diminution of it observed.</i>	177
Limpet, <i>a sort of shell-fish.</i>	322
Longitude of Carthagena.	153, 154, 155
—— Martinico.	157
—— Porto-Bello.	151
St. Louis, <i>a fort in St. Domingo, its latitude.</i>	155
Lyons, <i>the quantity of rain observed there.</i>	176

## M

Machine to retain the wheel, which serves to raise the rammer to drives piles.	23
Malaca, <i>the air dilates otherwise there than in France.</i>	242
Map of a country, <i>a new way of constructing it.</i>	14
Marseilles, <i>difference of its meridian from those of Genoa and Nuremberg.</i>	316
St. Marthe, <i>its latitude.</i>	150
Martinico, <i>its latitude.</i>	156
Meroe, <i>conjectures on the position of that island.</i>	130
—— governed only by queens.	ibid.
—— its true situation.	132, 135
—— not an island but a peninsula.	132
—— suspected by some to be an imaginary island.	134
—— its distance from Syene.	136
—— its position by the climates.	137
—— by what rivers formed.	ibid.
—— its figure.	138
Milk, <i>the difference between that of European women and Negresses.</i>	12
Mines, <i>the effect of gunpowder in them.</i>	44
Montpellier, <i>the difference of its meridian from that of Paris.</i>	316
Moon eclipsed at Nuremberg, Genoa, and Marseilles.	316
Motion progressive of several species of shell-fishes.	321
Mountains, <i>the origin of them.</i>	77

# I N D E X.

	pag.
Muscles of the pond.	346
—— a little shell-fish that feeds upon them.	75
Mufick, an extraordinary cure performed by it.	9, 68

## N

Needle magnetical, its declination	23, 95, 105, 125, 150, 152, 155, 172, 315, 360
Negresses, the difference between their milk and that of European women.	12
Nile, the sources of it where situated.	133
Nuremberg, the variation of the needle observed there.	315
—— difference of its meridian from those of Marfeilles and Genoa.	316

## O

Object-glass of a telescope, the necessity of centring it well.	366
Opticks, some facts in them explained.	190

## P

Palourde, a sort of shell-fish.	324
Patella, a sort of shell-fish.	322
Pendulum, the length of it at Porto-Bello.	151
—————————— Martinico.	160
Piles, a machine to retain the wheel, which serves to raise the rammer to drive them.	23
St. Pilon, barometrical observations made there.	142
Pont-briand, the weather observed there.	173
Porto-Bello, its longitude and latitude.	151
Porto-cabeillo, observations made there.	149
Port de Paix, in St. Domingo, its longitude.	160
Progressive motion of several species of shell-fishes.	321
Pullet with two hearts.	167
Purr, a sort of shell-fish.	322

## R

Rain, the quantity of it observed.	19, 93, 170, 173, 176, 356, 361, 363
------------------------------------	---

# I N D E X.

## S

	pag.
Santerini, <i>a new island near it.</i>	13, 71
Sea, <i>the physical history of it.</i>	337
— <i>nettles, their progressive motion.</i>	323
— <i>urchins, considerations on their legs.</i>	168
Seine, <i>why it was not entirely frozen in the hard winter of 1709.</i>	166
Shagreen, <i>an account of it.</i>	165
Shells <i>inclosed in stone.</i>	66
— <i>of land and water animals, how they are formed and grow.</i>	250
Shell-fish, <i>a small one that feeds upon muscles.</i>	75
— <i>the progressive motion of several of them.</i>	321
Sight, <i>the principal organ of it.</i>	198
Sluices, <i>a new construction of them.</i>	57
Snails, <i>their generation.</i>	83
— <i>an insect upon them.</i>	376
Solium, <i>a worm in the human body.</i>	11
Sourdon, <i>a sort of shell-fish.</i>	324
South-sea, <i>a voyage thither.</i>	125
Spectacles, <i>when discovered.</i>	90
Spider, <i>the circulation of blood in its leg.</i>	11
— <i>observations on these insects.</i>	29
— <i>their manner of making their webs.</i>	33
— <i>their manner of catching flies.</i>	37, 40
— <i>their different species.</i>	38
Spinner, <i>a sort of spider.</i>	41
Stone <i>expelled from the kidneys.</i>	12
Stones <i>of the sea.</i>	6
— <i>figured.</i>	332
Sun <i>eclipsed in different countries.</i>	316
— <i>a luminous circle observed about it.</i>	148
— <i>the force of its rays in pressing and pushing.</i>	66
— <i>the effect of its heat on a paste, laid upon a piece of polished glass.</i>	68
Suppression of urine <i>cured.</i>	12
Switzerland, <i>a great deal of crystal found in the mountains there.</i>	81
Syene, <i>its position.</i>	136

# INDEX.

## T

	pag.
Tacaze, <i>a considerable river, that flows into the Nile.</i>	137
——— <i>called also Atbora.</i>	138
——— <i>thought to be the Astabora of the ancients.</i>	137
Tænia <i>found in a tench,</i>	344
Talc, <i>observations on a sort of it.</i>	408
Tarantula, <i>a species of spider.</i>	42
Telescope, <i>the necessity of centring well its object-glass.</i>	366
Tench, <i>a tænia found in one.</i>	344
Terra del Fuego, <i>remarks on the navigation of its coasts.</i>	125
Thermometer <i>observed.</i>	20, 94, 171, 357
St. Thomas <i>island its latitude.</i>	155
Thunder <i>considered.</i>	64
Tides <i>observed at Dunkirk.</i>	381

## V.

Vapours <i>in a baker's cellar, the fatal effects of them.</i>	329
Vesicaria Marina <i>not the eggs of the cuttle-fish.</i>	87
Vision, <i>the principal organ of it.</i>	198

## W

West-Indies, <i>observations made there.</i>	149
Whelk or buccinum, <i>a sort of shell-fish.</i>	325
Winds <i>observed.</i>	22, 95, 170, 174
Woman <i>delivered of a child, when above 80 years of age.</i>	329
Woodpecker, <i>observations on its tongue.</i>	183
Worms <i>voided by stool.</i>	11
Wrong-heir, <i>a sort of fish.</i>	326

## Z

Zurick, <i>barometrical observations made there.</i>	177
--	-----

A N

I N D E X

O F

AUTHORS NAMES.

A.

- Agricola 245.  
F. Francisco Almeyda 139.  
M. Amontons 47, 110 to 113, 115, 119, 121, 124,  
225, 227, 305, 306, 307, 359, 366.  
Archimedes 88.  
Aristophanes 88, 89.

B.

- M. Baert 381 to 384, 386 to 389, 391, 402, 404, 405,  
406, 408.  
Juan de Barros 137.  
Bartholin 408, 409, 410.  
Bafilis 135.  
Caspar Bauhinus 375.  
Bellonius 245.  
M. Bernier 9.  
M. Bernouli 3, 4.  
F. de Beze 238, 240, 242.  
M. l'Abbe Bignon 13, 125.  
Bion 134.  
M. Borelli 183, 187, 188.  
M. Boudin 328.

F. Bourg-

*An* INDEX of AUTHORS NAMES.

F. Bourgnon, 70, 72.  
Mr. Boyle 4.  
F. Brevedent 139.  
Burnet 77, 78.

C.

M. Carré 5, 10, 45, 345.  
M. Cassini 4, 5, 27, 74, 105, 125, 142, 149, 167,  
215, 316, 331, 366, 381.  
Cellarius 134.  
M. de Chasteuil Gallaup 14.  
M. Chevalier 14, 16, 18, 44.  
M. Clairambaut 128.  
Clusius 334, 374.  
M. Couplet 153.

D.

M. Dalefme 92.  
Dampier 127.  
M. Delisle 128, 129.  
M. Descartes 77, 78, 204.  
Diodorus 131, 138.  
M. Dodart 10.

F.

M. Felibien 8.  
M. de Feriol 70, 165.  
F. Feuillée 149, 150, 151, 153, 155, 156, 159, 160,  
161.  
M. Fontenelle 350.

G.

Gandolphe 168, 169.  
Gassendi 13.  
M. Gauteron 305.  
M. Geoffroy 68, 244, 328, 338, 344, 369.  
Gesner 245.

M.

*An* INDEX of AUTHORS NAMES.

- M. des Glos 156.  
F. le Gobiens 139.  
F. Nicola Godinho 131.  
Pieter Goos 105.  
F. Gouye 238, 382.  
Dr. Halley 106, 107, 108, 109, 125, 126, 128, 129,  
150, 152, 155.  
M. des Hayes 156.  
Heliodorus 141.  
Van Helmont 245, 246, 250.  
M. de la Hire 19, 57, 88, 93, 96, 110, 148, 154, 165,  
170, 173, 176, 190, 220, 328, 331, 356, 361, 363,  
408.  
M. Homberg 11, 12, 29, 64, 65, 66, 67, 103, 166,  
289.  
Sieur Houffaye 105, 106, 107.  
M. Huygens 20, 45, 97, 98, 99, 100, 101, 202, 203,  
204, 206, 212, 217, 220, 223, 225, 408, 409, 410.

I.

- M. Jaugeon 165.  
Josephus 141.

K.

- Kepler 215 to 219.

L.

- Lactantius 90.  
M. des Landes 12.  
M. de la Lanne 13, 14.  
F. Laval 27, 28, 74, 75, 141, 145, 147, 236, 237.  
M. Leibnits 13.  
M. Lemery, jun. 287.  
M. Lewenhoeck 11.  
M. Littre 167.  
F. Hieronymo Lobo 139.  
Abbé de Louvois 220.  
M. Ludolf 131, 135.

*An* INDEX of AUTHORS NAMES.

M.

- M. de Mandajor 69.  
M. Manfart 24.  
M. Maraldi 73, 177, 229, 338.  
M. de la Mare 328, 329.  
M. Mariotte 113, 170, 198, 199, 207, 208, 238, 239,  
243, 310.  
Count Marfigli 337 to 344.  
Mercator 133.  
M. Mery 183, 273, 346, 347, 348.  
Mezeray 358.  
Monard 373.

N.

- Sir Iſaac Newton 201, 111, 214, 218, 219, 419.  
M. Nuguet 118, 119, 120.

O.

- Ortelius 133.

P.

- M. Parent 350, 352.  
M. de Pas 12.  
F. Paulet 140.  
M. Perrault 183, 188.  
M. Picard 23.  
Piſo 12.  
M. Plantade 167.  
Plautus 90.  
Pliny 89, 131, 135, 136, 137, 139, 140, 141.  
Sieur Poncet 139, 140.  
M. la Comte de Pontbriand 173, 175, 361.  
M. de Pontchartrain 382.  
M. Poupart 84, 321, 348.  
Ptolemy 136, 138.

*An* INDEX *of* AUTHORS NAMES.

R.

- M. de Reaumur 75, 76, 77, 250, 321, 322, 323, 348,  
376.  
M. Richer 152.  
M. du Roule 132, 137, 139, 140.

S.

- Marquis Salvago 229, 231.  
M. Saulmon, 6, 7, 8, 87, 334.  
M. Saurin 201, 295.  
M. Sauveur 328.  
M. John Scheuckzer, 77, 78, 79, 80, 177, 178, 180,  
182, 236, 332, 333, 334, 337, 363 to 366.  
M. John James Scheuchzer 73, 74, 81, 82, 83, 335,  
336, 337.  
Steno 77, 78.  
Sirabo 136, 137, 138, 139, 141.

T.

- F. Tachard 328.  
Tavernier 370, 373.  
Abbe Teinturier 331.  
F. Balthazar Tellez 131, 134, 135.  
M. du Torar 173, 361.  
M. Tournefort 66, 334.

V.

- M. de Vauban 19, 44.  
M: Jean Verdois 328.  
M du Verney 84, 85.  
M. de la Verune 125, 129.  
Isaac Voffius 134.

W.

- Woodward 77, 78.  
M. Wurtzelbaur 315.

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