
TUTE·SI·RECTE·VIXERIS

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. LXI. For the Year 1771.

PART I.

LONDON:

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M.DCC.LXXII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume. And this information was thought the more neceſſary, not only as it has been the common opinion, that they were publiſhed by the authority, and under the direction, of the Society itſelf; but alſo, becauſe ſeveral authors, both at home and abroad, have in their writings called them the *Tranſactions of the Royal Society*. Whereas in truth the Society, as a body, never did intereſt themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſeable, that a Committee of their Members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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PRESENTS

PRESENTS

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ROYAL SOCIETY

In the YEAR 1771;

WITH

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Feb.

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12.	Dr. Gower.	Materials for a new Hiſt. of Cheſhire.	4°
19.	Donald Monro, M. D.	Some ſpecimens of native natron, from Tripoli.	

PHILO-

PHILOSOPHICAL TRANSACTIONS.

I. *Remarks upon the Nature of the Soil of* Naples, *and its Neighbourhood; in a Letter from the Honourable* William Hamilton, *His Majesty's Envoy Extraordinary at* Naples, *to* Mathew Maty, *M. D. Sec. R. S.*

S I R,

Naples, Oct. 16, 1770.

Read Jan. 10. 17. 24. 1771.

ACCORDING to your desire, I lose no time in sending you such further remarks as I have been making with some diligence, for six years past, in the compass of twenty miles or more, round this capital. By accompanying these remarks with a map of the country I describe, and with the specimens of different matters that compose the most remarkable spots of it, I do not doubt but that I ſhall convince you, as I am myſelf convinced, that the whole circuit (ſo far as I have examined) within the boundaries marked in the map, is wholly and totally the production of ſubterraneous fires; and that moſt probably the ſea formerly reached the mountains that lie behind Capua and Caſerta, and are a continuation of the Appennines. If I may be allowed to compare ſmall things with great, I imagine the ſubterraneous fires to have worked in this country under the bottom of the ſea, as moles in a field, throwing up here and there a hillock, and that the matter thrown out of ſome of theſe hillocks formed into ſettled volcanos, filling up the ſpace between one and the other, has compoſed this part of the continent, and many of the iſlands adjoining.

From the obſervations I have made upon mount Etna, Veſuvius, and its neighbourhood, I dare ſay, that, after a careful examination, moſt mountains that are, or have been volcanos, would be found to owe their exiſtence to ſubterraneous fire; the direct reverſe of what I find the commonly received opinion.

Nature, though varied, is certainly in general uniform in her operations; and I cannot conceive that two ſuch conſiderable volcanos as Etna and Veſuvius ſhould have been formed otherwiſe, than every other conſiderable volcano of the known world. I do not wonder that ſo little progreſs has been made in the improvement of natural hiſtory, and particularly in that branch of it which regards the theory of the earth; nature acts ſlowly, it is difficult to catch her in the fact. Thoſe who have made this

ſubject

ſubject their ſtudy have, without ſcruple, undertaken at once, to write the natural hiſtory of a whole province, or of an entire continent; not reflecting, that the longeſt life of man ſcarcely affords him time to give a perfect one of the ſmalleſt inſect.

I am ſenſible of what I undertake in giving you, Sir, even a very imperfect account of the nature of the ſoil of a little more than twenty miles round Naples: yet I flatter myſelf that my remarks, ſuch as they are, may be of ſome uſe to any one hereafter, who may have leiſure and inclination to follow them up. The kingdom of the Two Sicilies offers certainly the faireſt field for obſervations of this kind, of any in the whole world; here are volcanos exiſting in their full force, ſome on their decline, and others totally extinct.

To begin with ſome degree of order, which is really difficult in the variety of matter that occurs to my mind, I will firſt mention the baſis on which I found all my conjectures. It is the nature of the ſoil that covers the antient towns of Herculaneum and Pompeii, and the interior and exterior form of the new mountain, near Puzzole, with the ſort of materials of which it is compoſed. It cannot be denied that Herculaneum and Pompeii ſtood once above ground; though now, the former is in no part leſs than ſeventy feet, and in ſome parts one hundred and twelve feet, below the preſent ſurface of the earth; and the latter is buried ten or twelve feet deep, more or leſs. As we know from the very accurate account given by Pliny the younger, to Tacitus, and from the accounts of other contemporary authors, that theſe towns were buried by an eruption of mount Vesuvius in the time of Titus; it must be allowed, that whatever matter lies between these cities and the present surface of the earth over them, must have been produced since the year 79 of the Christian æra, the date of that formidable eruption.

Pompeii, which is situated at a much greater distance from the volcano than Herculaneum, has felt the effects of a single eruption only; it is covered with white pumice stones mixed with fragments of lava and burnt matter, large and small; the pumice is very light, but I have found some of the fragments of lava and cinders there, weighing eight pounds. I have often wondered that such weighty bodies could have been carried to such a distance (for Pompeii cannot be less than five miles, in a strait line, from the mouth of Vesuvius). Every observation confirms the fall of this horrid shower over the unfortunate city of Pompeii, and that few of its inhabitants had dared to venture out of their houses; for in many of those which have been already cleared, skeletons have been found, some with gold rings, ear rings, and bracelets. I have been present at the discovery of several human skeletons myself; and under a vaulted arch, about two years ago, at Pompeii, I saw the bones of a man and a horse taken up, with the fragments of the horse's furniture, which had been ornamented with false gems set in bronze. The skulls of some of the skeletons found in the streets had been evidently fractured by the fall of the stones. His Sicilian majesty's excavations are confined to this spot at present; and the curious in antiquity may expect hereafter, from so rich a mine, ample matter for their dissertations: but I will confine myself

to

to ſuch obſervations only as relate to my preſent ſubject.

Over the ſtratum of pumice and burnt matter that covers Pompeii, there is a ſtratum of good mould, of the thickneſs of about two feet and more in ſome parts, in which vines flouriſh, except in ſome particular ſpots of this vineyard, where they are ſubject to be blaſted by a foul vapour or *mofete*, as it is called here, that riſes from beneath the burnt matter. The abovementioned ſhower of pumice ſtones, according to my obſervations, extended beyond Caſtel-a-mare (near which ſpot the ancient town of Stabia alſo lies buried under them), and covered a tract of country not leſs than thirty miles in circumference. It was at Stabia that Pliny the elder loſt his life, and this ſhower of pumice ſtones is well deſcribed in the younger Pliny's letter. Little of the matter that has iſſued from Veſuvius ſince that time, has reached theſe parts: but I muſt obſerve that the pavement of the ſtreets of Pompeii is of lava; nay, under the foundation of the town, there is a deep ſtratum of lava and burnt matter. Theſe circumſtances, with many others that will be related hereafter, prove, beyond a doubt, that there have been eruptions of Veſuvius previous to that of the year 79, which is the firſt recorded by hiſtory.

The growth of ſoil by time is eaſily accounted for; and who, that has viſited ruins of ancient edifices, has not often ſeen a flouriſhing ſhrub, in a good ſoil, upon the top of an old wall? I have remarked many ſuch on the moſt conſiderable ruins at Rome and elſewhere. But from the ſoil which has grown over the barren pumice that covers Pompeii, I was

enabled

enabled to make a curious obſervation. Upon examining the cuts and hollow ways made by currents of water in the neighbourhood of Veſuvius and of other volcanos, I had remarked that there lay frequently a ſtratum of rich ſoil, of more or leſs depth, between the matter produced by the exploſion of ſucceeding eruptions; and I was naturally led to think that ſuch a ſtratum had grown in the ſame manner as the one abovementioned over the pumice of Pompeii. Where the ſtratum of good ſoil was thick, it was evident to me that many years had elapſed between one eruption and that which ſucceeded it. I do not pretend to ſay that a juſt eſtimate can be formed of the great age of volcanos from this obſervation, but ſome ſort of calculation might be made; for inſtance, ſhould an exploſion of pumice cover again the ſpot under which Pompeii is buried, the ſtratum of rich ſoil abovementioned would certainly lie between two beds of pumice; and if a like accident had happened a thouſand years ago, the ſtratum of rich ſoil would as certainly have wanted much of its preſent thickneſs, as the rotting of vegetables, manure, &c. is ever increaſing a cultivated ſoil. Whenever I find then a ſucceſſion of different ſtrata of pumice and burnt matter like that which covers Pompeii, intermixed with ſtrata of rich ſoil, of greater or leſs depth, I hope I may be allowed reaſonably to conclude, that the whole has been the production of a long ſeries of eruptions occaſioned by ſubterraneous fire. By the ſize and weight of the pumice, and fragments of burnt erupted matter in theſe ſtrata, it is eaſy to trace them up to their ſource, which I have done more than once in the neighbour-
hood

hood of Puzzole, where explofions have been frequent. The gradual decreafe in the fize and quantity of the erupted matter in the ftratum abovementioned, from Pompeii to Caftle-a-Mare, is very vifible: at Pompeii, as I faid before, I have found them of eight pounds weight, when at Caftle-a-Mare the largeft do not weigh an ounce.

The matter which covers the ancient town of Herculaneum is not the produce of one eruption only; for there are evident marks that the matter of fix eruptions has taken its courfe over that which lies immediately above the town, and was the caufe of its deftruction. Thefe ftrata are either of lava or burnt matter, with veins of good foil between them. The ftratum of erupted matter that immediately covers the town, and with which the theatre and moft of the houfes were filled, is not of that foul vitrified matter, called lava, but of a fort of foft ftone, compofed of pumice, afhes, and burnt matter. It is exactly of the fame nature with what is called here the Naples ftone; the Italians diftinguifh it by the name of *tufa*, and it is in general ufe for building. Its colour is ufually that of our free ftone, but fometimes tinged with grey, green, and yellow; and the pumice ftones, with which it ever abounds, are fometimes large and fometimes fmall: it varies likewife in its degree of folidity.

The chief article in the compofition of this *tufa* feems to me to be, that fine burnt material, which is called *puzzolane*, whofe binding quality and utility by way of cement are mentioned by Vitruvius, and which is to be met with only in countries that have been fubject to fubterraneous fires. It is, I believe, a fort of lime prepared

pared by nature. This, mixed with water, great or ſmall pumice ſtones, fragments of lava, and burnt matter, may naturally be ſuppoſed to harden into a ſtone of this kind; and, as water frequently attends eruptions of fire, as will be ſeen in the accounts I ſhall give of the formation of the new mountain near Puzzole, I am convinced the firſt matter that iſſued from Veſuvius, and covered Herculaneum, was in the ſtate of liquid mud. A circumſtance ſtrongly favouring my opinion is, that, about two years ago, I ſaw the head of an antique ſtatue dug out of this matter within the theatre of Herculaneum; the impreſſion of its face remains to this day in the *tufa*, and might ſerve as a mould for a caſt in plaiſter of Paris, being as perfect as any mould I ever ſaw. As much may be inferred from the exact reſemblance of this matter, or *tufa*, which immediately covers Herculaneum, to all the *tufa's* of which the high grounds of Naples and its neighbourhood are compoſed; I detached a piece of it ſticking to, and incorporated with, the painted ſtucco of the inſide of the theatre of Herculaneum, and ſhall ſend it for your inſpection *. It is very different, as you will ſee, from the vitrified matter called lava, by which it has been generally thought that Herculaneum was deſtroyed. The village of Reſina and ſome villas ſtand at preſent above this unfortunate town.

To account for the very great difference of the matters that cover Herculaneum and Pompeii, I have often thought that in the eruption of 79 the moun-

* This piece is now in the Muſeum of the Royal Society, together with many other ſpecimens, mentioned in this and in the following letter. M. M.

tain muſt have been open in more than one place. A paſſage in Pliny's letter to Tacitus ſeems to ſay as much, "*Interim e Veſuvio monte pluribus locis latiſſimæ* "*flammæ, atque incendia relucebant, quorum fulgor et* "*claritas tenebras noctis pellebat*:" ſo that very probably the matter that covers Pompeii proceeded from a mouth, or crater, much nearer to it than is the great mouth of the volcano, from whence came the matter that covers Herculaneum. This matter might nevertheleſs be ſaid to have proceeded from Veſuvius, juſt as the eruption in the year 1760, which was quite independent of the great crater (being four miles from it), is properly called an eruption of Veſuvius.

In the beginning of eruptions, volcanos frequently throw up water mixed with the aſhes. Veſuvius did ſo in the eruption of 1631, according to the teſtimony of many contemporary writers. The ſame circumſtance happened in 1669 according to the account of Ignazzio Sorrentino, who, by his Hiſtory of Mount Veſuvius printed at Naples in 1734, has ſhewn himſelf to have been a very accurate obſerver of the phænomena of the volcano, for many years that he lived at Torre del Greco, ſituated at the foot of it. At the beginning of the formation of the new mountain, near Puzzole, water was mixed with the aſhes thrown up, as will be ſeen in two very curious and particular accounts of the formation of that mountain, which I ſhall have the pleaſure of communicating to you preſently; and in 1755 Etna threw up a quantity of water in the beginning of an eruption, as is mentioned in the letter I ſent you laſt

 year

year upon the ſubject of that magnificent volcano *. Ulloa likewiſe mentions this circumſtance of water attending the eruptions of volcanos in America. Whenever therefore I find a *tufa* compoſed exactly like that which immediately covers Herculaneum, and undoubtedly proceeded from Veſuvius, I conclude ſuch a *tufa* to have been produced by water mixing with the erupted matter at the time of an exploſion occaſioned by ſubterraneous fire; and this obſervation, I believe, will be of more uſe than any other, in pointing out thoſe parts of the preſent *terra firma*, that have been formed by exploſion. I am convinced it has often happened that ſubterraneous fires and exhalations, after having been pent up and confined for ſome time, and been the cauſe of earthquakes, have forced their paſſage, and in venting themſelves formed mountains of the matter that confined them, as you will ſee was the caſe near Puzzole in the year 1538, and by evident ſigns has been ſo before, in many parts of the neighbourhood of Puzzole; without creating a regular volcano. The materials of ſuch mountains will have but little appearance of having been produced by fire, to any one unaccuſtomed to make obſervations upon the different nature of volcanos.

If it were allowed to make a compariſon between the earth and a human body, one might conſider a country replete with combuſtibles occaſioning exploſions (which is ſurely the caſe here) to be like a body full of humours. When theſe humours concentre in one part, and form a great tumour out of which they are diſcharged freely, the body is leſs

* Phil. Tranſact. Vol. LX. p. 1.

agitated;

agitated; but when by any accident the humours are checked, and do not find a free paſſage through their uſual channel, the body is agitated, and tumours appear in other parts of that body, but ſoon after the humours return again to their former channel. In a ſimilar manner one may conceive Veſuvius to be the preſent great channel, through which nature diſcharges ſome of the foul humours of the earth; when theſe humours are checked by any accident or ſtoppage in this channel for any conſiderable time, earthquakes will be frequent in its neighbourhood, and exploſions may be apprehended even at ſome diſtance from it. This was the caſe in the year 1538, Veſuvius having been quiet for near 400 years. There was no eruption from its great crater from the year 1139 to the great eruption of 1631, and the top of the mountain began to loſe all ſigns of fire. As it is not foreign to my purpoſe, and will ſerve to ſhew how greatly they are miſtaken, who place the ſeat of the fire in the centre or towards the top of a volcano, I will give you a curious deſcription of the ſtate of the crater of Veſuvius, after having been free from eruptions 492 years, as related by Bracini, who deſcended into it not long before the eruption of 1631: "The crater was five miles in "circumference, and about a thouſand paces deep; its "ſides were covered with bruſh wood, and at the "bottom there was a plain on which cattle grazed. "In the woody parts, boars frequently harboured; "in the midſt of the plain, within the crater, was a "narrow paſſage, through which, by a winding path, "you could deſcend about a mile amongſt rocks and "ſtones, till you came to another more ſpacious

" plain covered with aſhes: in this plain were three " little pools, placed in a triangular form, one to- " wards the Eaſt, of hot water, corroſive and bitter " beyond meaſure; another towards the Weſt, of " water ſalter than that of the ſea; the third of hot " water, that had no particular taſte."

The great increaſe of the cone of Veſuvius, from that time to this, naturally induces one to conclude, that the whole of the cone was raiſed in the like manner, and that the part of Veſuvius, called Somma, which is now conſidered as a diſtinct mountain from it, was compoſed in the ſame manner. This may plainly be perceived by examining its interior and exterior form, and the ſtrata of lava and burnt matter of which it is compoſed. The ancients, in deſcribing Veſuvius, never mention two mountains. Strabo, Dio, Vitruvius, all agree, that Veſuvius, in their time, ſhewed ſigns of having formerly erupted, and the firſt compares the crater on its top to an amphitheatre. The mountain now called Somma was, I believe, that which the ancients called Veſuvius; its outſide form is conical, its inſide, inſtead of an amphitheatre, is now like a great theatre. I ſuppoſe the eruption in Pliny's time to have thrown down that part of the cone next the ſea, which would naturally have left it in its preſent ſtate, and that the conical mountain, or exiſting Veſuvius, has been raiſed by the ſucceeding eruptions: all my obſervations confirm this opinion. I have ſeen antient lavas in the plain on the other ſide of Somma, which could never have proceeded from the preſent Veſuvius. Serao, a celebrated phyſician now living at Naples, in the introduction of his account of the eruption of Veſu-

vius

vius in 1737 (in which account many of the phænomena of the volcano are recorded and very well accounted for) ſays, that at the convent of Dominican Fryars, called the Madona del Arco, ſome years ago, in ſinking a well, at a hundred feet depth, a lava was diſcovered, and ſoon after another, ſo that in leſs than three hundred feet depth, the lavas of four eruptions were found. From the ſituation of this convent it is clear beyond a doubt, that theſe lavas proceeded from the mountain called Somma, as they are quite out of the reach of the exiſting volcano.

From theſe circumſtances, and from repeated obſervations I have made in the neighbourhood of Veſuvius, I am ſure that no virgin ſoil is to be found there, and that all is compoſed of different ſtrata of erupted matter, even to a great depth below the level of the ſea. In ſhort, I have not any doubt in my own mind, but that this volcano took its riſe from the bottom of the ſea; and as the whole plain between Veſuvius and the mountains behind Caſerta, which is the beſt part of the Campagna Felice, is (under its good ſoil) compoſed of burnt matter, I imagine the ſea to have waſhed the feet of thoſe mountains, until the ſubterraneous fires began to operate, at a period certainly of a moſt remote antiquity.

The ſoil of the Campagna Felice is very fertile; I ſaw the earth opened in many places laſt year in the midſt of that plain, when they were ſeeking for materials to mend the road from Naples to Caſerta. The ſtratum of good ſoil was in general four or five feet thick; under which was a deep ſtratum of cinders, pumice, fragments of lava and ſuch burnt matter as abounds near Veſuvius and all volcanos.

The

The mountains at the back of Caſerta are moſtly of a ſort of lime-ſtone, and very different from thoſe formed by fire; though Signior Van Vitelli, the celebrated architect, has aſſured me, that in the cutting of the famous aqueduct of Caſerta through theſe mountains, he met with ſome ſoils, that had been evidently formed by ſubterraneous fires. The high grounds which extend from Caſtel-a-Mare to the point of Minerva towards the iſland of Caprea, and from the promontory that divides the bay of Naples from that of Salerno, are of lime ſtone. The plain of Sorrento, that is bounded by theſe high grounds, beginning at the village of Vico, and ending at that of Maſſa, is wholly compoſed of the ſame ſort of *tufa* as that about Naples, except that the cinder or pumice ſtones intermixed in it are larger than in the Naples *tufa*. I conceive then that there has been an exploſion in this ſpot from the bottom of the ſea. This plain, as I have remarked to be the caſe with all ſoils produced by ſubterraneous fire, is extremely fertile; whilſt the ground about it, being of another nature, is not ſo. The iſland of Caprea does not ſhew any ſigns of having been formed by ſubterraneous fire, but is of the ſame nature as the high grounds laſt mentioned, from which it has been probably detached by earthquakes, or the violence of the waves. Rovigliano, an iſland, or rather a rock in the bay of Caſtel-a-Mare, is likewiſe of lime ſtone, and ſeems to have belonged to the original mountains in its neighbourhood: in ſome of theſe mountains there are alſo petrified fiſh and foſſil ſhells, which I never have found in the mountains, which I ſuppoſe to have been formed by exploſion.

You

You have now, Sir, before you the nature of the ſoil, from Caprea to Naples. The ſoil on which this great metropolis ſtands has been evidently produced by exploſions, ſome of which ſeem to have been upon the very ſpot on which this city is built; all the high grounds round Naples, Pauſilipo, Puzzole, Baïa, Miſenum, the iſlands of Procita and Iſchia, all appear to have been raiſed by exploſion. You can trace ſtill in many of theſe heights the conical ſhape that was naturally given them at firſt, and even the craters out of which the matter iſſued, though to be ſure others of theſe heights have ſuffered ſuch changes by the hand of time, that you can only conjecture that they were raiſed in the like manner, by their compoſition being exactly the ſame as that of thoſe mountains, which ſtill retain their conical form and craters entire. A *tufa*, exactly reſembling the ſpecimen I took from the inſide of the theatre of Herculaneum, layers of pumice intermixed with layers of good ſoil, juſt like thoſe over Pompeii, and lavas like thoſe of Veſuvius, compoſe the whole ſoil of the country that remains to be deſcribed.

The famous grotto anciently cut through the mountain of Pauſilipo, to make a road from Naples to Puzzole, gives you an opportunity of ſeeing that the whole of that mountain is *tufa*. The firſt evident crater you meet with, after you have paſſed the grotto of Pauſilipo, is now the lake of Agnano; a ſmall remain of the ſubterraneous fire (which muſt probably have made the baſon for the lake, and raiſed the high grounds which form a ſort of amphitheatre round it) ſerves to heat rooms, which the Neapolitans make great uſe of in ſummer, for carrying off

diverſe

diverſe diſorders, by a ſtrong perſpiration. This place is called the Sudatorio di San Germano; near the preſent bagnios, which are but poor little hovels, there are the ruins of a magnificent ancient bath. About an hundred paces from hence is the Grotto del Cane; I ſhall only mention, as a further proof of the probability that the lake of Agnano was a volcano, that vapours of a pernicious quality, as that in the Grotto del Cane, are frequently met with in the neighbourhood of Etna and Veſuvius, particularly at the time of, before, and after, great eruptions. The noxious vapour having continued in the ſame force conſtantly ſo many ages, as it has done in the Grotto del Cane (for Pliny mentions this Grotto), is indeed a circumſtance in which it differs from the vapours near Veſuvius and Etna, which are not conſtant; the cone forming the outſide of this ſuppoſed volcano is ſtill perfect in many parts.

Oppoſite to the Grotto del Cane, and immediately joining to the lake, riſes the mountain called Aſtruni, which, having, as I imagine, been thrown up by an exploſion of a much later date, retains the conical ſhape and every ſymptom of a volcano in much greater perfection than that I have been deſcribing. The crater of Aſtruni is ſurrounded with a wall to confine boars and deer (this volcano having been for many years converted to a royal chace). It may be about ſix miles or more in cicumference; in the plain at the bottom of the crater are two lakes, and in ſome books there is mention made of a hot ſpring, which I never have been able to find. There are many huge rocks of lava within the crater of Aſtruni, and ſome I have met with alſo in that of Agnano;

the

the cones of both theſe ſuppoſed volcanos are compoſed of *tufa* and ſtrata of looſe pumice, fragments of lava and other burnt matter, exactly reſembling the ſtrata of Veſuvius. Bartholomeus Fatius, who wrote of the actions of king Alphonſo the firſt (before the new mountain had been formed near Puzzole), conjectured that Aſtruni had been a volcano. Theſe are his words: *Locus Neapoli quatuor millia paſſuum proximus, quem vulgo Liſtrones vocant, nos unum e Phlegreeis Campis ab ardore nuncupandum putamus.* There is no entrance into the crater of either Aſtruni or Agnano, except one, evidently made by art, and they both exactly correſpond with Strabo's deſcription of avenues; the ſame may be ſaid of the Solfaterra and the Monte Gauro, or Barbaro as it is ſometimes called, which I ſhall deſcribe preſently.

Near Aſtruni and towards the ſea riſes the Solfaterra, which not only retains its cone and crater, but much of its former heat. In the plain within the crater, ſmoke iſſues from many parts, as alſo from its ſides; here, by means of ſtones and tiles heaped over the crevices, through which the ſmoak paſſes, they collect in an aukward manner what they call *ſale armoniaco*; and from the ſand of the plain they extract ſulphur and alum. This ſpot well attended to might certainly produce a good revenue, whereas I doubt if they have hitherto ever cleared 200 *l.* a year by it. The hollow ſound produced by throwing a heavy ſtone on the plain of the crater of the Solfaterra ſeems to indicate, that it is ſupported by a ſort of arched natural vault; and one is induced to think that there is a pool of water beneath this vault (which boils by the heat of a ſubterraneous fire ſtill deeper)

 by

by the very moiſt ſtream that iſſues from the cracks in the plain of the Solfaterra, which, like that of boiling water, runs off a ſword or knife, preſented to it, in great drops. On the outſide, and at the foot of the cone of the Solfaterra, towards the lake of Agnano, water ruſhes out of the rocks, ſo hot, as to raiſe the quickſilver in Fahrenheit's thermometer to the degree of boiling water, a fact of which I was myſelf an eye-witneſs. This place, well worthy the obſervation of the curious, has been taken little notice of; it is called the *Piſciarelli*. The common people of Naples have great faith in the efficacy of this water, and make much uſe of it in all cutaneous diſorders, as well as for another diſorder that prevails here. It ſeems to be impregnated chiefly with ſulphur and alum. When you approach your ear to the rocks of the Piſciarelli, from whence this water ouzes, you hear a horrid boiling noiſe, which ſeems to proceed from the huge cauldron, that may be ſuppoſed to be under the plain of the Solfaterra. On the other ſide of the Solfaterra, next the ſea, there is a rock which has communicated with the ſea, till part of it was cut away to make the road to Puzzole; this was undoubtedly a conſiderable lava that ran from the Solfaterra when it was an active volcano. Under this rock of lava, which is more than ſeventy feet high, there is a ſtratum of pumice and aſhes. This ancient lava is about a quarter of a mile broad; you meet with it abruptly before you come in ſight of Puzzole, and it finiſhes as abruptly within about an hundred paces of the town. I have often thought that many quarries of ſtone upon examination would be found to owe their origin to the ſame cauſe, though time

time may have effaced all ſigns of the volcano from whence they proceeded. Except this rock, which is evidently lava and full of vitrifications like that of Veſuvius, all the rocks upon the coaſt of Baïa are of *tufa*.

I have obſerved in the lava of Veſuvius and Etna, as in this, that the bottom as well as the ſurface of it was rough and porous, like the cinders or ſcoriæ from an iron foundery, and that for about a foot from the ſurface and from the bottom, they were not near ſo ſolid and compact as towards the centre; which muſt undoubtedly proceed from the impreſſion of the air upon the vitrified matter whilſt in fuſion. I mention this circumſtance, as it may ſerve to point out true lava's with more certainty. The ancient name of the Solfaterra was, *Forum Vulcani*, a ſtrong proof of its origin from ſubterraneous fire. The degree of heat that the Solfaterra has preſerved for ſo many ages, ſeems to have calcined the ſtones upon its cone, and in its crater, as they are very white and crumble eaſily in the hotteſt parts.

We come next to the new mountain near Puzzole, which, being of ſo very late a formation, preſerves its conical ſhape entire, and produces as yet but a very ſlender vegetation. It has a crater almoſt as deep as the cone is high, which may be near a quarter of a mile perpendicular, and is in ſhape a regular inverted cone. At the baſis of this new mountain (which is more than three miles in circumference), the ſand upon the ſea ſhore, and even that which is waſhed by the ſea itſelf, is burning hot for above the ſpace of an hundred yards; if you take up a handful of the ſand below water, you are obliged to get rid of it directly, on account of its intenſe heat.

I had been long very deſirous of meeting with a good account of the formation of this new mountain, becauſe, proving this mountain to have been raiſed by meer exploſion in a plain, would prove at the ſame time, that all the neighbouring mountains, which are compoſed of the ſame materials, and have exactly or in part the ſame form, were raiſed in the like manner, and that the ſeat of fire, the cauſe of theſe exploſions, lies deep, which I have every reaſon to think.

Fortunately, I lately found two very good accounts of the phænomena that attended the exploſion, which formed the new mountain, publiſhed a few months after the event. As I think them very curious, and greatly to my purpoſe, and as they are rare, I will give you a literal tranſlation of ſuch extracts as relate to the formation of the Monte Nuovo. They are bound in one volume*.

The title of the firſt is *Dell Incendio di Pozzuolo, Marco Antonio delli Falconi all Illuſtriſſima Signiora Marcheſa della Padula nel* MDXXXVIII.

At the head of the ſecond is, *Ragionamento del Terremoto, del Nuovo Monte, del Aprimento di Terra in Pozzuolo nell' Anno* 1538. *é della ſignificatione d'eſſi. Per Piero Giacomo da Toledo*; and at the end of the book, *Stampata in Nap. per Giovanni Sulztbach Alemano, a* 22*di Genaro* 1539, *con gratia, é privilegio.*

" Firſt then (ſays Marco Antonio delli Falconi), " will I relate ſimply and exactly the operations of

* This very ſcarce volume has been preſented by Mr. Hamilton to the Britiſh Muſeum. M. M.

" nature,

" nature, of which I was either myſelf an eye-wit-
" neſs, or as they were related to me by thoſe who
" had been witneſſes of them. It is now two years
" that there have been frequent earthquakes at
" Pozzuolo, at Naples, and the neighbouring parts;
" on the day, and in the night before the appearance
" of this eruption, above twenty ſhocks great and
" ſmall were felt at the abovementioned places. The
" eruption made its appearance the 29th of Septem-
" ber 1538, the feaſt of St Michael the angel; it was
" on a Sunday, about an hour in the night; and as I
" have been informed, they began to ſee on that ſpot,
" between the hot baths or ſweating rooms, and
" Trepergule, flames of fire, which firſt made their
" appearance at the baths, then extended towards
" Trepergule, and fixing in the little valley that lies
" between the Monte Barbaro and the hillock called
" del Pericolo (which was the road to the lake of
" Avernus and the baths), in a ſhort time the fire
" increaſed to ſuch a degree that it burſt open the
" earth in this place, and threw up ſo great a quanti-
" ty of aſhes and pumice ſtones mixed with water,
" as covered the whole country; and in Naples a
" ſhower of theſe aſhes and water fell great part of
" the night. The next morning, which was Monday,
" and the laſt of the month, the poor inhabitants of
" Pozzuolo, ſtruck with ſo horrible a ſight, quitted
" their habitations, covered with that muddy and
" black ſhower, which continued in that country the
" whole day, flying death, but with faces painted
" with its colours, ſome with their children in their
" arms, ſome with ſacks full of their goods, others
" leading an aſs loaded with their frightened family

" towards

" towards Naples, others carrying quantities of birds " of various ſorts that had fallen dead at the time the " eruption began, others again with fiſh which they " had found, and were to be met with in plenty " upon the ſhore, the ſea having been at that time " conſiderably dried up. Don Petro di Toledo, Vice- " roy of the kingdom, with many gentlemen, went " to ſee ſo wonderful an appearance; I alſo, having " met with the moſt honourable and incomparable " gentleman, Signior Fabritio Moramaldo, on the " road, went and ſaw the eruption and the many " wonderful effects of it. The ſea towards Baïa " had retired a conſiderable way; though from the " quantity of aſhes and broken pumice ſtones thrown " up by the eruption, it appeared almoſt totally dry. " I ſaw likewiſe two ſprings in thoſe lately-diſcover- " ed ruins, one before the houſe that was the queen's, " of hot and ſalt water; the other of freſh and cold " water, on the ſhore, about 250 paces nearer to the " eruption: ſome ſay, that ſtill nearer to the ſpot " where the eruption happened, a ſtream of freſh " water iſſued forth like a little river. Turning to- " wards the place of the eruption, you ſaw mountains " of ſmoak, part of which was very black and part " very white, riſe up to a great height; and in the " midſt of the ſmoke, at times, deep-coloured flames " burſt forth with huge ſtones and aſhes, and you " heard a noiſe like the diſcharge of a number of " great artillery. It appeared to me as if Typheus " and Enceladus from Iſchia and Etna with inume- " rable giants, or thoſe from the Campi Phlegrei " (which according to the opinions of ſome were " ſituated in this neighbourhood), were come to

" wage

" wage war again with Jupiter. The natural hifto-
" rians may perhaps reafonably fay, that the wife
" poets meant no more by giants, than exhalations,
" fhut up in the bowels of the earth, which, not
" finding a free paffage, open one by their own force
" and impulfe, and form mountains, as thofe which
" occafioned this eruption have been feen to do;
" and methought I faw thofe torrents of burning
" fmoke that Pindar defcribes in an eruption of Etna,
" now called mon Gibello in Sicily, in imitation of
" which, as fome fay, Virgil wrote thefe lines :

" *Ipfe fed horrificis juxta tonat Ætna ruinis*, &c.

" After the ftones and afhes with clouds of thick
" fmoke had been fent up, by the impulfe of the
" fire and windy exhalation (as you fee in a great
" cauldron that boils), into the middle region of the
" air, overcome by their own natural weight, when
" from diftance the ftrength they had received from
" impulfe was fpent, rejected likewife by the cold
" and unfriendly region, you faw them fall thick,
" and by degrees, the condenfed fmoke clear away,
" raining afhes with water and ftones of different
" fizes, according to the diftance from the place:
" then by degrees with the fame noife and fmoke it
" threw out ftones and afhes again, and fo on by
" fits. This continued two days and nights, when
" the fmoke and force of the fire began to abate.
" The fourth day, which was Thurfday at 22 o'clock,
" there was fo great an eruption, that, as I was in
" the gulph of Puzzole coming from Ifchia, and
" not far from Mifenum, I faw, in a fhort time,
" many

" many columns of ſmoke ſhoot up, with the moſt " terrible noiſe I ever heard, and, bending over the " ſea, came near our boat, which was four miles or " more from the place of their birth; and the quan- " tity of aſhes, ſtones, and ſmoke, ſeemed as if they " would cover the whole earth and ſea. Stones, great " and ſmall, and aſhes more or leſs, according to the " impulſe of the fire and exhalations, began to fall, " ſo that a great part of this country was covered " with aſhes; and many that have ſeen it, ſay, they " reached the vale of Diana, and ſome parts of " Calabria, which are more than 150 miles from " Pozzuolo. The Friday and Saturday nothing but " a little ſmoke appeared, ſo that many, taking cou- " rage, went upon the ſpot, and ſay, that with the " ſtones and aſhes thrown up, a mountain has been " formed in that valley, not leſs than three miles in " circumference, and almoſt as high as the monte " Barbaro, which is near it, covering the Canettaria, " the caſtle of Trepergule, all thoſe buildings and " the greateſt part of the baths that were about " them; extending South towards the ſea, North as " far as the lake of Avernus, Weſt to the Sudatory, " and joining Eaſt to the foot of the monte Barbaro, " ſo that this place has changed its form and face in " ſuch a manner as not to be known again, a thing " almoſt incredible to thoſe who have not ſeen it, " that in ſo ſhort a time ſo conſiderable a mountain " could have been formed. On its ſummit there is a " mouth in the form of a cup, which may be a " quarter of a mile in circumference, though ſome " ſay it is as large as our market-place at Naples, from " which there iſſues a conſtant ſmoke; and though

" I have ſeen it only at a diſtance, it appears very
" great. The Sunday following, which was the
" 6th of October, many people going to ſee this
" phænomenon, and ſome having aſcended half the
" mountain, others more, about 22 o'clock there
" happened ſo ſudden and horrid an eruption, with
" ſo great a ſmoke, that many of theſe people were
" ſtifled, ſome of which could never be found. I
" have been told, that the number of the dead or
" loſt amounted to twenty four. From that time
" to this, nothing remarkable happened; it ſeems
" as if the eruption returned periodically, like
" the ague or gout. I believe henceforward it
" will not have ſuch force, though the eruption
" of the Sunday was accompanied with ſhowers
" of aſhes and water, which fell at Naples, and
" were ſeen to extend as far as the mountain of
" Somma, called Veſuvius by the ancients; and,
" as I have often remarked, the clouds of ſmoke
" proceeding from the eruption, moved in a
" direct line towards that mountain, as if theſe
" places had a correſpondence and connection one
" with the other. In the night, many beams and
" columns of fire were ſeen to proceed from this
" eruption, and ſome like flaſhes of lightening.
" We have then, many circumſtances for our obſer-
" vation, the earthquakes, the eruption, the drying
" up of the ſea, the quantity of dead fiſh and birds,
" the birth of ſprings, the ſhower of aſhes with
" water, and without water, the innumerable trees
" in that whole country, as far as the Grotto of
" Lucullus, torn from their roots, thrown down, and
" covered with aſhes, that it gave one pain to ſee
" them:

" them: and as all these effects were produced by
" the same cause that produces earthquakes; let us
" first enquire how earthquakes are produced, and
" from thence we may easily comprehend the cause
" of the abovementioned events." Then follows a dissertation on earthquakes, and some curious conjectures relative to the phænomena which attended this eruption, clearly and well expressed, considering, as the author himself apologizes, that at that time the Italian language had been little employed on such subjects.

The account of the formation of the monte Nuovo, by Pietro Giacomo di Toledo, is given in a dialogue between the feigned personages of Peregrino and Svessano; the former of which says, " It is now
" two years that this province of Campagna has been
" afflicted with earthquakes, the country about
" Pozzuolo much more so than any other parts, but
" the 27th and the 28th of the month of September
" last, the earthquakes did not cease day or night, in
" the abovementioned city of Pozzuolo; that plain
" which lies between the lake of Averno, the monte
" Barbaro, and the sea, was raised a little, and many
" cracks were made in in it, from some of which
" issued water; and at the same time the sea, which
" was very near the plain, dried up about two hun-
" dred paces, so that the fish were left on the sand,
" a prey to the inhabitants of Pozzuolo. At last, on
" the 29th of the said month, about two hours in
" the night, the earth opened near the lake, and dis-
" covered a horrid mouth, from which were vomited
" furiously, smoke, fire, stones, and mud composed
" of ashes; making, at the time of its opening, a

" noise

" noiſe like very loud thunder: the fire that iſſued " from this mouth, went towards the walls of the " unfortunate city; the ſmoke was partly black and " partly white, the black was darker than darkneſs " itſelf, and the white was like the whiteſt cotton; " theſe ſmokes, riſing in the air, ſeemed as if they " would touch the vault of heaven; the ſtones that " followed, were, by the devouring flames, con- " verted to pumice, the ſize of which (of ſome I ſay) " were much larger than an ox. The ſtones went " about as high as a croſs-bow can carry, and then " fell down, ſometimes on the edge and ſometimes " into the mouth itſelf. It is very true that many of " them in going up could not be ſeen, on account " of the dark ſmoke; but when they returned from " the ſmoky heat, they ſhewed plainly where they " had been by their ſtrong ſmell of fetid ſulphur, " juſt like ſtones that have been thrown out of a " mortar, and have paſſed through the ſmoke of in- " flamed gunpowder. The mud was of the colour " of aſhes, and at firſt very liquid, then by degrees " leſs ſo, and in ſuch quantities, that in leſs than " twelve hours, with the help of the abovementioned " ſtones, a mountain was raiſed of a thouſand paces " in height. Not only Pozzuolo and the neighbour- " ing country was full of this mud, but the city of " Naples alſo, the beauty of whoſe palaces were, in " a great meaſure, ſpoiled by it. The aſhes were " carried as far as Calabria by the force of the " winds, burning up in their paſſage the graſs and " high trees, many of which were borne down by the " weight of them. An infinity of birds alſo, and " numberleſs animals of various kinds, covered with

 " this

"this sulphureous mud, gave themselves up a prey "to man. Now this eruption lasted two nights and "two days without intermission, though, it is true, "not always with the same force, but more or less; "when it was at its greatest height, even at Naples "you heard a noise or thundering like heavy artillery "when two armies are engaged. The third day "the eruption ceased, so that the mountain made its "appearance uncovered, to the no small astonish-"ment of every one who saw it. On this day, "when I went up with many people to the top of "this mountain; I saw down into its mouth, which "was a round concavity of about a quarter of a mile "in circumference, in the middle of which the "stones that had fallen were boiling up, just as in "a great cauldron of water that boils on the fire. "The fourth day it began to throw up again, and "the seventh much more, but still with less violence "than the first night; it was at this time that many "people, who were unfortunately on the mountain, "were either suddenly covered with ashes, smothered "with smoke, or knocked down by stones, burnt by "the flame, and left dead on the spot. The smoke "continues to this day, and you often see in the night "time fire in the midst of it. Finally, to complete "the history of this new and unforeseen event, in "many parts of the new-made mountain, sulphur "begins to be generated." Giacomo di Toledo, towards the end of his dissertation upon the phænomena attending this eruption, says, that the lake of Avernus had a communication with the sea, before the time of the eruption; and that he apprehended that the air of Puzzole might come to be affected in summer

ſummer time, by the vapours from the ſtagnated waters of the lake, which is actually the caſe.

You have, Sir, from theſe accounts, an inſtance of a mountain, of a conſiderable height and dimenſions, formed in a plain, by mere exploſion, in the ſpace of forty-eight hours. The earthquakes having been ſenſibly felt at a great diſtance from the ſpot where the opening was made, proves clearly, that the ſubterraneous fire was at a great depth below the ſurface of the plain; it is as clear that thoſe earthquakes, and the exploſion, proceeded from the ſame cauſe, the former having ceaſed upon the appearance of the latter. Does not this circumſtance evidently contradict the ſyſtem of M. Buffon, and of all the natural hiſtorians, who have placed the ſeat of the fire of volcanos towards the center, or near the ſummit of the mountains, which they ſuppoſe to furniſh the matter emitted? Did the matter which proceeds from a volcano in an eruption come from ſo inconſiderable a depth as they imagine, that part of the mountain ſituated above their ſuppoſed ſeat of the fire muſt neceſſarily be deſtroyed, or diſſipated in a very ſhort time: on the contrary, an eruption uſually adds to the height and bulk of a volcano, and who, that has had an opportunity of making obſervations on volcanos does not know, that the matter they have emitted for many ages, in lavas, aſhes, ſmoke, &c. could it be collected together, would more than ſuffice to form three ſuch mountains as the ſimple cone or mountain of the exiſting volcano? With reſpect to Veſuvius, this could be plainly proved; and I refer to my letter upon the ſubject of Etna, to ſhew the quantity of matter thrown up in one ſingle eruption,

tion, by that terrible volcano. Another proof that the real ſeat of the fire of volcanos lies even greatly below the general level of the country whence the mountain ſprings, is, that was it only at an inconſiderable depth below the baſis of the mountain, the quantity of matter thrown up would ſoon leave ſo great a void immediately under it, that the mountain itſelf muſt undoubtedly ſink and diſappear after a few eruptions.

In the above accounts of the formation of the new mountain, we are told that the matter firſt thrown up, was mud compoſed of water and aſhes, mixed with pumice ſtones and other burnt matter: on the road leading from Puzzole to Cuma, part of the cone of this mountain has been cut away to widen the road. I have there ſeen that its compoſition is a *tufa* intermixed with pumice, ſome of which are really of the ſize of an ox, as mentioned in Toledo's account, and exactly of the ſame nature as the *tufa* of which every other high ground in its neighbourhood is compoſed; ſimilar alſo to that which covers Herculaneum. According to the above accounts, after the muddy ſhower ceaſed, it rained dry aſhes: this circumſtance will account for the ſtrata of looſe pumice and aſhes, that are generally upon the ſurface of all the *tufas* in this country, and which were moſt probably thrown up in the ſame manner. At the firſt opening of the earth, in the plain near Puzzole, both accounts ſay, that ſprings of water burſt forth; this water, mixing with the aſhes, certainly occaſioned the muddy ſhower; when the ſprings were exhauſted, there muſt naturally have enſued a ſhower of dry aſhes and pumice, of which we have been

likewiſe

likewiſe aſſured. I own, I was greatly pleaſed at being in this manner enabled to account ſo well for the formation of theſe *tufa* ſtones and the veins of dry and looſe burnt matter above them, of which the ſoil of almoſt the whole country I am deſcribing is compoſed; and I do not know that any one has ever attended to this circumſtance, though I find that many authors, who have deſcribed this country, have ſuſpected that parts of it were formed by exploſion. Wherever then this ſort of *tufa* is found, there is certainly good authority to ſuſpect its having been formed in the ſame manner as the *tufa* of this new mountain; for, as I ſaid before, nature is generally uniform in all her operations.

It is commonly imagined that the new mountain roſe out of the Lucrine lake which was deſtroyed by it; but in the above account, no mention is made of the Lucrine lake; it may be ſuppoſed then, that the famous dam, which Strabo and many other ancient authors mention to have ſeparated that lake from the ſea, had been ruined by time or accident, and that the lake became a part of the ſea before the exploſion of 1538.

If the above deſcribed eruption was terrible, that which formed the monte Barbaro (or Gauro, as it was formerly called), muſt have been dreadful indeed. It joins immediately to the new mountain, which in ſhape and compoſition it exactly reſembles; but it is at leaſt three times as conſiderable. Its crater cannot be leſs than ſix miles in circumference; the plain within the crater, one of the moſt fertile ſpots I ever ſaw, is about four miles in circumference; there is no entrance to this plain, but one on the

Eaſt

Eaſt ſide of the mountain, made evidently by art; in this ſection you have an opportunity of ſeeing that the matter, of which the mountain is compoſed is exactly ſimilar to that of the monte Nuovo. It was this mountain that produced (as ſome authors have ſuppoſed) the celebrated Falernian wine of the ancients.

Cuma, allowed to have been the moſt ancient city of Italy, was built on an eminence, which is likewiſe compoſed of *tufa*, and may be naturally ſuppoſed a ſection of the cone formed by a very ancient exploſion.

The lake of Avernus fills the bottom of the crater of a mountain, undoubtedly produced by exploſion, and whoſe interior and exterior form, as well as the matter of which it is compoſed, exactly reſemble the monte Barbaro and monte Nuovo. At that part of the baſis of this mountain which is waſhed by the ſea of the bay of Puzzole, the ſand is ſtill very hot, though conſtantly waſhed by the waves; and into the cone of the mountain, near this hot ſand, a narrow paſſage of about 100 paces in length is cut, and leads to a fountain of boiling water, which, though brackiſh, boils fiſh and fleſh without giving them any bad taſte or quality, as I have experienced more than once. This place is called Nero's bath, and is ſtill made uſe of for a ſudatory, as it was by the ancients; the ſtream that riſes from the hot fountain abovementioned, confined in the narrow ſubterraneous paſſage, ſoon produces a violent perſpiration upon the patient who ſits therein. This bath is reckoned a great ſpecifick in that diſtemper which is ſuppoſed to have

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made its appearance at Naples, before it ſpread its contagion over the other parts of Europe.

Virgil and other ancient authors ſay, that birds could not fly with ſafety over the lake of Avernus, but that they fell therein; a circumſtance favouring my opinion that this was once the mouth of a volcano. The vapour of the ſulphur and other minerals muſt undoubtedly have been more powerful the nearer we go back to the time of the exploſion of the volcano; and I am convinced that there are ſtill ſome remains of thoſe vapours upon this lake, as I have obſerved there are very ſeldom any water fowl upon it; and that when they do go there, it is but for a ſhort time, whilſt all the other lakes in the neighbourhood are conſtantly covered with them, in the winter ſeaſon. Upon mount Veſuvius, in the year 1766, during an eruption, when the air was impregnated with noxious vapours, I have myſelf picked up dead birds frequently.

The caſtle of Baïa ſtands upon a conſiderable eminence, compoſed of the uſual *tufa* and ſtrata of pumice and aſhes, from which I concluded I ſhould find ſome remains of the craters from whence the matter iſſued; accordingly, having aſcended the hill, I ſoon diſcovered two very viſible craters, juſt behind the caſtle.

The lake called the Mare morto was alſo, moſt probably, the crater from whence iſſued the materials which formed the Promontory of Miſenum, and the high grounds around this lake. Under the ruins of an ancient building near the point of Miſenum, in a vault, there is a vapour, or *mofete*, exactly ſimilar in its effects to that of the Grotto del Cane, as I have often experienced.

The form of the little iſland of Niſida ſhews plainly its origin. It is half a hollow cone of a volcano cut perpendicularly; the half crater forms a little harbour called the Porto Pavone; I ſuppoſe the other half of the cone to have been detached into the ſea by earthquakes, or perhaps by the violence of the waves, as the part that is wanting is the ſide next to the open ſea.

The fertile and pleaſant iſland of Procita ſhews alſo moſt evident ſigns of its production by exploſion, the nature of its ſoil being directly ſimilar to that of Baïa and Puzzole; this iſland ſeems really, as was imagined by the ancients, to have been detached from the neighbouring iſland of Iſchia.

There is no ſpot, I believe, that could afford a more ample field for curious obſervations, than the iſland of Iſchia, called Enaria, Inarime, and Pithecuſa, by the ancients. I have viſited it three times; and this ſummer paſſed three weeks there, during which time, I examined, with attention, every part of it. Iſchia is eighteen miles in circumference: the whole of its ſoil is the ſame as that near Veſuvius, Naples, and Puzzole. There are numberleſs ſprings, hot, warm, and cold, diſperſed over the whole iſland, the waters of which are impregnated with minerals of various ſorts; ſo that, if you give credit to the inhabitants of the country, there is no diſorder but what finds its remedy here. In the hot months (the ſeaſon for making uſe of theſe baths), thoſe who have occaſion for them flock hither from Naples.

A chari-

A charitable inſtitution ſends and maintains three hundred poor patients at the baths of Gurgitelli every ſeaſon. By what I could learn of theſe poor patients, thoſe baths have really done wonders, in caſes attended with obſtinate tumours, and in contractions of the tendons and muſcles. The patient begins by bathing, and then is buried in the hot ſand near the ſea. In many parts of the iſland, the ſand is burning hot, even under water. The ſand on ſome parts of the ſhore is almoſt entirely compoſed of particles of iron ore; at leaſt they are attracted by the load-ſtone, as I have experienced. Near that part of the iſland called Lacco, there is a rock of an ancient lava, forming a ſmall cavern, which is ſhut up with a door; this cavern is made uſe of to cool liquors and fruit, which it does in a ſhort time as effectually as ice. Before the door was opened, I felt the cold to my legs very ſenſibly; but when it was opened, the cold ruſhed out ſo as to give me pain, and within the grotto it was intolerable. I was not ſenſible of wind attending this cold; though upon mount Etna and mount Veſuvius, where there are caverns of this kind, the cold is evidently occaſioned by a ſubterraneous wind: the natives call ſuch places *ventaroli*. May not the quantity of nitre, with which all theſe places abound, account in ſome meaſure for ſuch extreme cold? My thermometer was unluckily broken, or I would have informed you of the exact degree of the cold in this *ventaroli* of Iſchia, which is by much the ſtrongeſt in its effects I ever felt. The ancient lavas of Iſchia ſhew, that the eruptions there have been very formidable; and hiſtory informs us, that its firſt inhabitants were driven out of the iſland

by

by the frequency and the violence of them. There are ſome of theſe ancient lavas not leſs than two hundred feet in depth. The mountain of St. Nicola, on which there is at preſent a convent of hermits, was called by the ancients Epomeus; it is as high, if not higher, than Veſuvius, and appears to me to be a ſection of the cone of the ancient and principal volcano of the iſland, its compoſition being all *tufa* or lava. The cells of the convent abovementioned are cut out of the mountain itſelf; and there you ſee plainly that its compoſition no way differs from the matter that covers Herculaneum, and forms the monte Nuovo. There is no ſign of a crater on the top of this mountain, which riſes almoſt to a ſharp point; time, and other accidents, may be reaſonably ſuppoſed to have worn away this diſtinctive mark of its having been formed by exploſion, as I have ſeen to be the caſe in other mountains, formed evidently by exploſion, on the flanks of Etna and Veſuvius. Strabo, in his 5th book, upon the ſubject of this iſland, quotes Timæus, as having ſaid, that, a little before his time, a mountain in the middle of Pithecuſa, called Epomeus, was ſhook by an earthquake, and vomited flames.

There are many other riſing grounds in this iſland, that, from the nature of their compoſition, muſt lead one to think the ſame as to their origin. Near the village of Caſtiglione, there is a mountain formed ſurely by an exploſion of a much later date, having preſerved its conical form and crater entire, and producing as yet but a ſlender vegetation: there is no account, however, of the date of this eruption. Nearer the town of Iſchia, which is on the ſea ſhore,

at

at a place called *Le Cremate*, there is a crater, from which, in the year 1301 or 1302, a lava ran quite into the ſea; there is not the leaſt vegetation on this lava, but it is nearly in the ſame ſtate as the modern lavas of Veſuvius. Pontano, Maranti, and D. Franceſco Lombardi, have recorded this eruption; the latter of whom ſays, that it laſted two months, that many men and beaſts were killed by the exploſion, and that a number of the inhabitants were obliged to ſeek for refuge at Naples and in the neighbouring iſlands. In ſhort, according to my idea, the iſland of Iſchia muſt have taken its riſe from the bottom of the ſea, and been increaſed to its preſent ſize by divers later exploſions. This is not extraordinary, when hiſtory tells us (and from my own obſervation I have reaſon to believe) that the Lipary iſlands were formed in the like manner. There has been no eruption in Iſchia ſince that juſt mentioned, but earthquakes are very frequent there; two years ago, as I was told, they had a very conſiderable ſhock of an earthquake in this iſland.

Father Goree's account of the formation of the new iſland in the Archipelago (ſituated between the two iſlands called Kammeni, and near that of Santorini) of which he was an eye-witneſs, ſtrongly confirms the probability of the conjectures I venture to ſend you, relative to the formation of thoſe iſlands and that part of the continent above deſcribed: it ſeems likewiſe to confirm the accounts given by Strabo, Pliny, Juſtin, and other ancient authors, of many iſlands in the Archipelago, formerly called the Ciclades, having ſprung up from the bottom of the ſea in the like manner. According to Pliny, in the

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4th year of the cxxxv Olympiad, 237 years before the Christian æra, the island of Thera (now Santorini) and Theresia were formed by explosion; and, 130 years later, the island Hiera (now called the great Kammeni) rose up. Strabo describes the birth of this island in these words: "In the middle space between "Thera and Therasia flames burst out of the sea for "four days, which, by degrees, throwing up great "masses, as if they had been raised by machines, "they formed an island of twelve stadia in circuit." And Justin says of the same island, *Eodem anno inter insulas Theramenem et Theresiam, medio utriusque ripæ et maris spatio, terræ motus fuit: in quo, cum admiratione navigantium, repente ex profundo cum calidis aquis Insula emersit.*

Pliny mentions also the formation of Aspronisi, or the White Island, by explosion, in the time of Vespasian. It is known, likewise, that in the year 1628, one of the islands of the Azores, near the island of St. Michael, rose up from the bottom of the sea, which was in that place 160 fathoms deep; and that this island, which was raised in fifteen days, is three leagues long, a league and a half broad, and rises three hundred and sixty feet above water.

Father Goree, in his account of the formation of the new island in the Archipelago, mentions two distinct matters that entered into the composition of this island, the one black, the other white. Aspronisi, probably from its very name, is composed of the white matter, which if, upon examination, should prove to be a *tufa*, as I strongly suspect, I should think myself still more grounded in my conjectures; though I must confess, as it is, I have scarcely a doubt

doubt left with respect to the country I have been describing having been thrown up in a long series of ages by various explosions from subterraneous fire. Surely there are at present many existing volcanos in the known world; and the memory of many others have been handed down to us by history. May there not therefore have been many others of such ancient dates as to be out of the reach of history?

Such wonderful operations of nature are certainly intended by all-wise Providence for some great purpose. They are not confined to any one part of the globe, for there are volcanos existing in the four quarters of it. We see the great fertility of the soil thrown up by explosion, in part of the country I have described, which on that account was called by the ancients *Campania Felix*. The same circumstance is evident in Sicily, justly esteemed one of the most fertile spots in the world, and the granary of Italy. May not subterraneous fire be considered as the great plough (if I may be allowed the expression) which nature makes use of to turn up the bowels of the earth, and afford us fresh fields to work upon, whilst we are exhausting those we are actually in possession of, by the frequent crops we draw from them? Would it not be found, upon enquiry, that many precious minerals must have remained far out of our reach, had it not been for such operations of nature? It is evidently so in this country. But such great enquiries would lead me far indeed. I will only add a reflection, which my own little experience in this branch of natural history furnishes me with. It is, that we are apt to judge of the great operations of nature on too confined a plan. When first I came to Naples,

my

my whole attention, with reſpect to natural hiſtory, was confined to mount Veſuvius, and the wonderful phænomena attending a burning mountain; but, in proportion as I began to perceive the evident marks of the ſame operation having been carried on in the different parts above deſcribed, and likewiſe in Sicily, in a greater degree, I looked upon mount Veſuvius only as a ſpot on which nature was at preſent active, and thought myſelf fortunate in having an opportunity of ſeeing the manner in which one of her great operations (an operation, I believe, much leſs out of her common courſe than is generally imagined) was effected.

Such remarks as I have made on the eruptions of mount Veſuvius, during my reſidence at Naples, have been tranſmitted to the Royal Society, who have done them more honour than they deſerved. Many more might be made upon this active volcano, by a perſon who had leiſure, a previous knowledge of the natural hiſtory of the earth, a knowledge of chemiſtry, and was practiſed in phyſical experiments, particularly thoſe of electricity. I am convinced that the ſmoke of volcanos contains always a portion of electrical matter, which is manifeſt at the time of great eruptions, as is mentioned in my account of the great eruption of Veſuvius in 1767. The peaſants in the neighbourhood of my villa, ſituated at the foot of Veſuvius, have aſſured me, that, during the eruption laſt mentioned, they were more alarmed by the lightening and balls of fire that fell about them with a crackling noiſe, than by the lava and the uſual attendants of an eruption. I find in all the accounts of great eruptions mention made of this ſort of

lightening,

lightening which is diſtinguiſhed here by the name of *Ferilli.* Bracini, in his account of the great one of Veſuvius in 1631, ſays, that the column of ſmoke, which iſſued from its crater, went over near an hundred miles of country, and that ſeveral men and beaſts were ſtruck dead by lightening, iſſuing from this ſmoke in its courſe.

The nature of the noxious vapours, called here *mofete*, that are uſually ſet in motion by an eruption of the volcano, and are then manifeſt in the wells and ſubterraneous parts of its neighbourhood, ſeem likewiſe to be little underſtood. From ſome experiments very lately made, by the ingenious Dr. Nuth, on the *mofete* of the Grotto del Cane, it appears that all its known qualities and effects correſpond with thoſe attributed to fixed air. Juſt before the eruption of 1767, a vapour of this kind broke into the king's chapel at Portici, by which a ſervant, opening the door of it, was ſtruck down. About the ſame time, as his Sicilian majeſty was ſhooting in a paddock near the palace, a dog dropped down, as was ſuppoſed, in a fit; a boy going to take him up dropped likewiſe; a perſon preſent, ſuſpecting the accident to have proceeded from a *mofete*, immediately dragged them both from the ſpot where they lay, in doing which, he was himſelf ſenſible of the vapour; the boy and the dog ſoon recovered. His Sicilian majeſty did me the honour of informing me himſelf of this accident ſoon after it had happened. I have met with theſe *mofetes* often, when I have been making my obſervations on the borders of mount Veſuvius,

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particularly in caverns, and once on the Solfaterra. The vapour affects the nostrils, throat, and stomach, just as the spirit of hartshorn, or any strong volatile salts, and would soon prove fatal if you did not immediately remove from it. Under the ancient city of Pompeii, the *mofetes* are very frequent and powerful, so that the excavations that are carrying on there are often interrupted by them; at all times *mofetes* are to be met with under ancient lavas of Vesuvius, particularly those of the great eruption of 1631. In Serao's account of the eruption of 1737, and in the chapter upon *mofetes*, he has recorded several curious experiments relative to this phænomenon. The Canonico Recupero, who, as I mentioned to you in a former letter, is watching the operations of mount Ætna, has just informed me, that a very powerful *mofete* has lately manifested itself in the neighbourhood of Etna; and that he found near the spot from whence it rises, animals, birds, and insects, dead, and the stronger sort of shrubs blasted, whilst the grass and tenderer plants did not seem to be affected. The circumstance of this *mofete*, added to that of the frequent earthquakes felt lately at Rhegio and Messina, makes it probable that an eruption of mount Ætna is at hand.

I am alarmed at the length of this letter. By endeavouring to make myself clearly understood, I have been led to make, what I thought, necessary digressions. I must therefore beg of your goodness, that, should you find this memoir in its present state, too tedious (which I greatly apprehend) to be pre-

sented

ſented to our reſpectable Society, you will make only ſuch extracts from it as you ſhall think will be moſt agreeable and intereſting. I am,

S I R,

With great truth and regard,

Your moſt obedient humble ſervant,

William Hamilton.

REFERENCES to the MAP, TAB. I.

1. Naples.
2. Portici.
3. Refina, under which Herculaneum is buried.
4. Torre del Greco.
5. Hermitage, at which travellers ufually reft, in their way up mount Vefuvius.
6. St. Angelo, a convent of Calmaldolefe, fituated upon a cone of a mountain formed by an ancient explofion.
7. Cones formed by the eruption of 1760, and lava that ran from them almoft into the fea.
8. Mount Vefuvius and Somma.
9. Village of Somma.
10. The convent of the Madona del Arco, under which lavas have been found at 300 feet depth, and which muft have proceeded from the mountain of Somma, when an active volcano.
11. Ottaiano.
12. Torre del Annunziata.
13. Caftel a Mare, near which the ancient town of Stabia is buried, and where Pliny the elder loft his life.
14. Vico.
15. Sorrento, and the plain formed evidently by fubterraneous fire.
16. Maffa.
17. Ifland of Caprea.


Capua
Caserta
PART OF THE APENNINES
THE CAMPAGNA FELICE
Cuma
New Mountain
Solfa Terra
Gulf of PUZZOLE and BAIA
P. of Misenum
Nisida
Procita
Ischia
NAPLES
Gulf of NAPLES
Herculaneum
M. Somma
M. Vesuvius
Pompeii
GULF OF CASTEL A MARE
Plain of Sorrento
Point of Minerva
Caprea I.
Gulf of SALERNO
Italian Miles

18. The Grotto of Paufilipo, cut through the mountain anciently, to make a road from Naples to Puzzole.
19. Point of Paufilipo.
20. The Gaiola, where there are ruins of ancient buildings, fuppofed to have belonged to Lucullus.
21. The ifland of Nifida, evidently formed by explofion.
22. The Lazaret.
23. The Bagnoli.
24. Puzzole, or Pozzuolo.
25. The Solfaterra, anciently called Forum Vulcani: between the Solfaterra and the lake of Agnano, are the boiling waters of the Pifciarelli.
26. The New Mountain, formed by explofion in the year 1538; the fand of the fea fhore at its bafis burning hot.
27. The lake of Agnano, fuppofed the crater of an ancient volcano: here are the baths called St. Germano, and the famous Grotto del Cane.
28. Aftruni, which has been evidently a volcano, and is now a Royal Chace, the crater being furrounded with a wall.
29. The monte Gauro or Barbaro, anciently a volcano.
30. The lake of Avernus, evidently the crater of an ancient volcano.
31. Lake of Fufaro.
32. Point of Mifenum, from whence Pliny the elder difcovered the eruption of Vefuvius that proved fatal to him; near this place, in a vault of an ancient building, is a conftant vapour or

or *mofete*, of the ſame quality with that of the Grotto del Cane.

33. The Mare Morto, the ancient Roman Harbour.
34. Baïa; behind the caſtle are two evident craters of ancient volcanos.
35. Iſland of Procita.
36. A perfect cone and crater of a volcano near Caſtiglione in the iſland of Iſchia.
37. Lava that ran into the ſea in the laſt eruption on this iſland, in the year 1301, or 1302; the place now called Le Cremate.
38. Town of Iſchia and caſtle.
39. Lake of Licola.
40. Lake of Patria.
41. The river Volturnus.
42. Capua.
43. Caſerta.
44. Averſa.
45. Mataloni.
46. Acerra.
47. Iſland of Iſchia, anciently called Ænaria, Inarime, and Pithecuſa.
48. The mountain of St. Nicola, anciently called Mons Epomeus, ſuppoſed the remains of the principal volcano of the iſland.
49. Caſtiglione, near which are the baths of Gurgitelli.
50. Lacco, near which is that very cold vapour called by the natives *ventarole*.
51. Ancient city of Pompeii, where his Sicilian majeſty's excavations are carrying on at preſent.
52. Rovigliano.
53. River of Sarno.

54. Cu-

54. Cuma.
55. Hot ſands and Sudatory called Nero's baths.
56. The Lucrine lake ſuppoſed to have been here, and of which there is ſtill ſome little remain.
57. Villa Angelica, Mr. Hamilton's villa, from whence he has made many of his obſervations upon mount Veſuvius.
58. Cones formed by an ancient eruption called *viuli*; here are likewiſe cold vapours called *ventaroli*.
59. High grounds, probably ſections of cones of ancient volcanos, being all compoſed of *tufa* and ſtrata of looſe pumice and burnt matter.
60. Plain of the Campagna Felice, four or five feet of excellent ſoil, under which are ſtrata of burnt and erupted matter.

. . . . . Marks the boundary of Mr. Hamilton's obſervations.

II. *Extract of another Letter, from Mr.* Hamilton, *to Dr.* Maty, *on the ſame Subject.*

Naples, March 5, 1771.

Read May 30, 1771.

SINCE I had the pleaſure of ſending you my letter, in which the nature of the ſoil of more than twenty miles round this capital is deſcribed; examining a deep hollow way cut by the rain waters into the outſide cone of the Solfaterra, I diſcovered, that a great part of the cone of that ancient volcano has been calcined by the hot vapours above deſcribed. Pumice calcined ſeems to be the chief ingredient, of which ſeveral ſpecimens of (as I ſuppoſe) variegated uniform marble are compoſed, and the beautiful variegations in them may have probably been occaſioned by the mineral vapours. As theſe ſpecimens are now ſent to the Royal Society, you will ſee that theſe variegations are exactly of the ſame pattern and colours as are met in many marbles and flowered alabaſters; and I cannot help thinking that they are marble or alabaſter in its infant ſtate. What a proof we have here of the great changes the earth we inhabit is ſubject to! What is now the Solfaterra, we have every reaſon to ſuppoſe, to have been originally thrown up by a ſubterraneous exploſion from the bottom of the ſea. That it was long an exiſting volcano, is plain, from the ancient currents of lava, that are ſtill to be traced from its crater to the ſea, from the ſtrata of pumice and erupted matter, of which its cone, in common with thoſe of all other volcanos, is compoſed, and from the teſtimony of many ancient authors. Its cone in many parts has been calcined, and is ſtill calcining, by the hot vapours that are continually iſſuing forth through its pores, and its nature is totally changed by this chemical proceſs of nature. In the hollow way, where I made theſe remarks, you ſee the different ſtrata of erupted matter, that compoſe the cone in ſome places perfectly calcined, in others not, according as the vapours have found means to inſinuate themſelves more or leſs.

A hollow way cut by the rains on the back of the mountain, on which part of Naples is ſituated, towards Capo di China, ſhews that the mountain is compoſed of ſtrata of erupted matter, among which are large maſſes of bitumen, in which its former ſtate of fluidity is very viſible. Here it was I diſcovered that pumice ſtone is produced from bitumen, which I believe has not yet been remarked. Some ſpecimens ſhew evidently the gradual proceſs from bitumen to pumice; and you will obſerve that the cryſtalline vitrifications, that are viſible in the bitumen, ſuffer no alteration, but remain in the ſame ſtate in the perfect pumice as in the bitumen.

In a piece of ſtratum, calcined from the outſide of the Solfaterra, the form and texture of the pumice ſtones is very diſcernible. In ſeveral parts of the outſide cone, this calcining operation is ſtill carried on by the exhalation of conſtant very hot

 and

and damp vapours, impregnated with ſalts, ſulphur, alum, &c. Where the above-mentioned vapours have not operated, the ſtrata of pumice and erupted matter, that compoſe the cone of the Solfaterra, are like thoſe of all the high grounds in its neighbourhood, which I ſuppoſe to have been thrown up likewiſe by exploſion. I have ſeen here, half of a large piece of lava perfectly calcined, whilſt the other half out of the reach of the vapours has been untouched; and in ſome pieces the center ſeems to be already converted into true marble.

The variegated ſpecimens then, above deſcribed, are nothing more than pumice and erupted matter, after having been acted upon in this manner by the hot vapours; and if you conſider the proceſs, as I have traced it, from bitumen to pumice, and from pumice to marble, you will think with me that it is difficult to determine the primitive ſtate of the many wonderful productions we ſee in nature.

I found in the *tufa* of the mountain of Pauſilipo, a fragment of lava: one ſide I poliſhed, to ſhew it to be true lava; the other ſhews the ſigns of the *tufa*, with which it is incorporated. It has evidently been rounded by friction, and moſt probably by rolling in the ſea. Is it not natural then to imagine that there muſt have been volcanoes near this ſpot, long before the formation of the mountain of Pauſilipo? This little ſtone may perhaps raiſe in your mind ſuch reflections, as it did in mine, relative to the great changes our globe ſuffers, and the probability of its great antiquity.

III. *A Letter from Dr.* Franklyn, *F. R. S. to the Astronomer Royal; containing an Observation of the Transit of* Mercury *over the* Sun, November 9*th* 1769: *By* John Winthrop, *Esq; F. R. S.* Hollisian *Professor of Mathematics and Natural Philosophy at* Cambridge, New England.

DEAR SIR,

Craven-street, Feb. 12, 1770.

Read Jan. 10, 1771.

I HAVE just received a letter from Mr. Winthrop, dated Dec. 7, containing the following account, *viz.*

" On Thursday the 9th of November, I had an " opportunity of observing a transit of Mercury. I " had carefully adjusted my clock to the apparent " time, by correspondent altitudes of the Sun, taken " with the quadrant for several days before, and with " the same reflecting telescope as I used for the transit " of Venus *. I first perceived the little planet " making an impression on the Sun's limb at $2^h\ 52'$ " $41''$; and he appeared wholly within at $53'\ 58''$ " apparent time. The sun set before the planet " reached the middle of his course; and for a con-

* See Phil. Transact. Vol. LIX. p. 352.

" ſiderable time before ſunſet, it was ſo cloudy, that
" the planet could not be diſcerned. So that I made
" no obſervations of conſequence except that of the
" beginning, at which time the Sun was perfectly
" clear. This tranſit compleats three periods of
" 46 years, ſince the firſt obſervation of Gaſſendi at
" Paris, in 1631."

I am, SIR,

With great eſteem,

Your moſt obedient ſervant,

B. Franklin.

IV. *Obſer-*

IV. *Obſervations on the Heat of the Ground on Mount* Veſuvius: *By* John Howard, *Eſq*; *F. R. S.*

Read Jan. 17, 1771.

I BEG leave to lay before this Society, ſome obſervations which I made laſt June, on the heat of the ground on mount Veſuvius, near Naples.

On my aſcending the mountain, I often immerged the bulb of a thermometer in the ground, but found no ſenſible heat for ſome time: the firſt riſing in my thermometer, was 114°; every two or three minutes, I obſerved the inſtrument, till I gained the ſummit. At thoſe times, I found it riſing to 122°, 137°, 147°, 164°, and 172°: on the top, in two places, where I made the obſervations, in the interſtices betwixt the hard lava, it was 218°. Such a degree of heat, after I had overcome the inconvenience of the exhalations, raiſed my curioſity to know if there was a ſtill greater degree of heat in the mouth of the ſaid mountain. Accordingly, I made a ſmall deſcent, and, by two obſervations I carefully and attentively made, my thermometer both times ſtood at 240°.

John Howard.

P. S. It

P. S. If it ſhould be aſked, how a perſon, either to their feet or in ſtooping or laying down to make the obſervations, could endure ſuch a degree of heat; I anſwer, that the heat, both at top and in the mouth of the mountain, was only in particular places. This was known by the fumes; the hard maſſes of lava are only warm, and even ſo tolerable as to permit me, to lay on them, as I was often obliged to do, when the thermometer was immerged, to make a true obſervation.

Philos.Trans. Vol. LXI. Tab.II. p. 55.

G. Edwards delineavit A° 1770.

Basire sc.

V. *Description of a Bird from the* East Indies; *in a Letter to* James West, *Esq; President of the Royal Society; from Mr.* George Edwards, *F. R. S.*

SIR,

Read Jan. 17, 1771.

IN August last, a friend of mine carried me with him to Valentine House, near Ilford in Essex, the seat of Charles Raymond, Esq; to see some curious birds and other animals, from the East Indies; amongst these, I discovered a rare bird, not before known to me *. It is of a new genus, and the only species of the genus hitherto come to my knowledge. It is about the bigness of a heron [see TAB. II.]; and has a good deal of the appearance of birds of the heron and crane kind, except that the neck is a little shorter. On first sight, I thought the bird belonged to that genus; but, on a closer view, I judged it to be no wader in the water, for though the legs be as long or longer than in herons, &c. yet they are feathered down to the knees, which we do not find in birds who wade in shallow waters, to seek their food.

* This bird was described, under the name of the *Sagittarius* from the Cape of good Hope, by Mr. Vosmaer, keeper of the Statholder's Museum at the Hague, in one of his publications in low Dutch, printed at Amsterdam, 1769, in 4to, with a coloured cut of the said bird. It seems to feed equally on flesh and fish; which accounts for his uniting the characters of birds of prey, and of waders in water. M. M.

food. The toes in this bird are alſo much ſhorter than they are in herons, ſo that I think it muſt be placed amongſt land birds. The bill is exactly like thoſe of hawks, and other birds of prey, which is the only inſtance I have diſcovered in any of the long legged kind of birds; the talons or claws are ſmall and unfit for a bird of prey, and the eyes are of a dark colour placed in ſpaces covered with a bare ſkin of an orange colour, on each ſide of the head. It hath a beautiful creſt compoſed of many long painted feathers tipped with black hanging backward. The beak, head, neck, back, breaſt, and upper covert feathers of the wings are of a blueiſh aſh colour, rather lighter on the breaſt than on the back. The belly, thighs, the greater wing-feathers, and tail, are black, the tail feathers being tipped with white; the legs and feet are of a reddiſh fleſh colour, the claws black. This bird was called a ſnake-eater, by thoſe who brought it from India. I believe it may prey on ſmall ſerpents, lizards, and other ſmall reptiles. Another bird was brought with this, ſuppoſed to be the male of this ſpecies, which died ſoon after it was landed Mr. Raymond's ſervant told me that it was ſomething larger, and the creſt longer, the head black, but that in other reſpects the two birds agreed.

I am, SIR,

Your moſt humble ſervant,

College of Phyſicians,
January, 1771.

Geo. Edwards.

VII. *An*

VI. *An Extract from the Register of the Parish of* Holy-Cross *in* Salop, *being a Second * Decade of Years from* Michelmas, 1760, *to* Michaelmas, 1770, *carefully digested in the following Table, by the Rev.* William Gorsuch, *Minister of that Parish.*

Read January 24, 1771.

		1761.	1762.	1763.	1764.	1765.	1766.	1767.	1768.	1769.	1770.	Total.	
Baptized,	Males.	17	18	20	18	19	23	22	17	22	18	194	382.
	Females.	14	20	22	21	19	16	30	9	20	17	188	
Buried.	Males.	11	11	16	14	19	33	11	19	12	15	161	365.
	Females.	14	13	20	13	19	54	18	20	14	19	204	

Increase 17.

m. Males. f. Females.

		1761 m.	f.	1762 m.	f.	1763 m.	f.	1764 m.	f.	1765 m.	f.	1766 m.	f.	1767 m.	f.	1768 m.	f.	1769 m.	f.	1770 m.	f.	Total
Under a month		3	1	2	0	0	3	3	0	0	2	1	5	1	1	0	2	—		1	0	25
From a month to a year		2	1	0	2	3	3	3	2	2	0	6	5	2	2	5	5	1	0	3	1	48
From 1	to 2	1	1	1	0	3	3	1	0	—		3	3	0	1	3	2	—		0	2	24
2	5	1	0	1	0	1	2	1	1	0	2	6	11	2	3	0	2	3	0	1	2	39
5	10	—		0	1	—		1	1	2	0	2	5	0	1	1	1	2	3	1	2	23
10	15	—		—		—		—		1	1	1	0	—		1	0	1	0	—		5
15	20	0	1	—		1	0	1	0	0	1	2	2	—		—		0	1	1	1	11
20	25	—		—		—		0	2	—		0	2	—		—		0	1	—		5
25	30	0	1	1	0	—		—		—		0	1	—		1	1	—		—		5
30	35	—		—		—		—		0	1	0	2	0	1	1	0	—		—		5
35	40	1	0	1	0	1	1	1	0	1	1	2	1	0	2	1	0	0	1	—		14
40	45	1	1	1	0	1	0	1	1	1	1	1	1	1	0	—		2	0	0	3	16
45	50	1	0	1	2	—		—		—		0	2	1	0	1	0	1	0	1	1	11
50	55	—		0	3	0	1	—		2	1	1	0	0	1	2	1	—		0	2	14
55	60	0	2	1	0	1	1	1	0	0	1	3	2	—		0	1	1	1	—		15
60	65	0	2	—		1	2	1	1	5	1	0	3	—		0	1	0	1	2	0	20
65	70	1	0	1	1	1	0	0	1	2	3	1	2	2	1	—		—		0	2	18
70	75	0	2	0	2	1	1	0	2	1	0	1	3	1	0	0	2	0	1	2	1	20
75	80	—		1	0	1	2	—		1	1	0	1	—		1	0	0	2	2	1	13
80	85	0	1	0	1	1	0	—		1	2	3	3	1	4	2	0	1	2	1	0	23
85	90	0	1	—		—		0	1	—		—		—		0	1	0	1	—		4
90	95	—		0	1	0	1	0	1	0	1	—		—		0	1	—		—		5
	96	—		—		—		—		—		—		0	1	—		—		—		1
	101	—		—		—		—		—		—		—		—		—		0	1	1

* *In Vol.* LII. *Part* i. *Art.* 25. *see the First Decade.*

There

There remain alive,

Under ten years of age, { Males 126, Females 122 } 248.

From 70 to 75	Males	12	33	From 80 to 85	Males	3	9
	Females	21			Females	6	
From 75 to 80	Males	8	11	From 85 to 90	Males	3	8
	Females	3			Females	5	

Diſtempers and Caſualties.

Accidents	5	Convulſions	31	Palſy	3
Ague	1	Dropſy	20	Small-pox	46
Apoplexy	5	Drowned	7	Teeth	5
Aſthma	3	Evil	4	Untimely	7
Cancer	2	Fever	35	Worms	4
Childbed	4	Jaundice	4		
Chincough	6	Impoſthume	2		
Conſumption	101	Meazles	15		

N.B. The remainder died of a natural decay, without any diſtemper.

Houſes or Families in 1765, 249—in 1770, 240.
Ditto, paying window-tax, in 1765, 70—in 1770, 65.
Void houſes, none.

Number of perſons in 1765,	1096
Ditto, in 1770,	1046
	Acres.
In arable and paſture land	1400
Gardens, yards, and houſes	300
	1700

There is no waſte land.

In the year 1767, nine houſes were pulled down, to open a way for building a new ſtone bridge over the river Severn, by which forty-four perſons were removed; ſix continued in the pariſh, and thirty-eight went out of the pariſh.

VII. *A Letter from Mr.* Stephen de Vifme *to* Mathew Maty, *M. D. Sec. R. S. containing an acount of the manner, in which the* Chinefe *heat their rooms.*

S I R,

Read Jan. 31, 1771.

THE great pleafure I have in obliging and rendering any fervice to your Royal and illuftrious Society, made me requeft the Rev. P. Gramont, miffionary, (a man of great family and fenfe, whom I was acquainted with at Canton before he was ordered to Pekin, by the Emperor) to make enquiries, and collect any curiofity ufeful to philofophy, mechanicks, and natural hiftory, that might be ufeful to mankind, and agreeable to the curious. He has been fo polite to tranfmit me the model of a Chinefe furnace, ufed to warm the apartments; together with a mufter of different coals, lime, &c. for the Royal Society, which I fend to my brother to be tranfmitted to you; and I fhall be happy that it comes fafe to your hands. Father Loreyro, miffionary and phyfician to the King of Cochin China (a gentleman we are greatly obliged to for his humanity, in cloathing, feeding, and getting a paffage to fome poor fhipwrecked feamen of the Earl Temple); is at work upon drawing a correct

I 2 draught

draught of that coaſt and Cambodia; and I hope, when finiſhed, to obtain one for the Society.

I embrace this opportunity* of alſo making you an offer of my ſervices, and ſubſcribing myſelf with the greateſt eſteem and regard,

Sir,

Your moſt obedient

humble ſervant,

Canton,
Oct. 8, 1770.

Step. de Viſme.

* Unfortunately this gentleman died at Canton ſoon after the date of this letter, which deprives the Royal Society, and the public, of the very curious and uſeful informations they might have expected from him.

VIII. *An Account of the* Kang, *or* Chinese *Stoves, by Father* Gramont, *translated from the* French.

Read Jan. 31, 1770.

THE greatest of all masters is want. It is a spur to genius, gives wings to industry, and points out such resources as neither learning nor curiosity would ever have contrived. This it is which has taught the Chinese to make use of sea coal to warm their houses, and to procure to themselves the benefit of its heat without being annoyed by its offensive smoak. This discovery of the Chinese might perhaps be of use in the great city of London, and those parts of England, where this fuel is burnt in rooms. The warm concern the Royal Society has always shewn for whatever affects the lives or welfare of the community, induces us to hope for a favourable acceptance of a model of the Chinese Kang, which we apprehend may be conducive to those beneficent purposes; we therefore have added such explanations as will give an insight into the theory of it, that it may be made known, and improved upon.

May the illustrious and celebrated Royal Society consider this trifle as a token of our profound respect, and accept it as a small acknowledgement of our gratitude for the favours bestowed upon us, wretched and afflicted as we are! As we have the honour to write to gentlemen eminent for their learning,

we

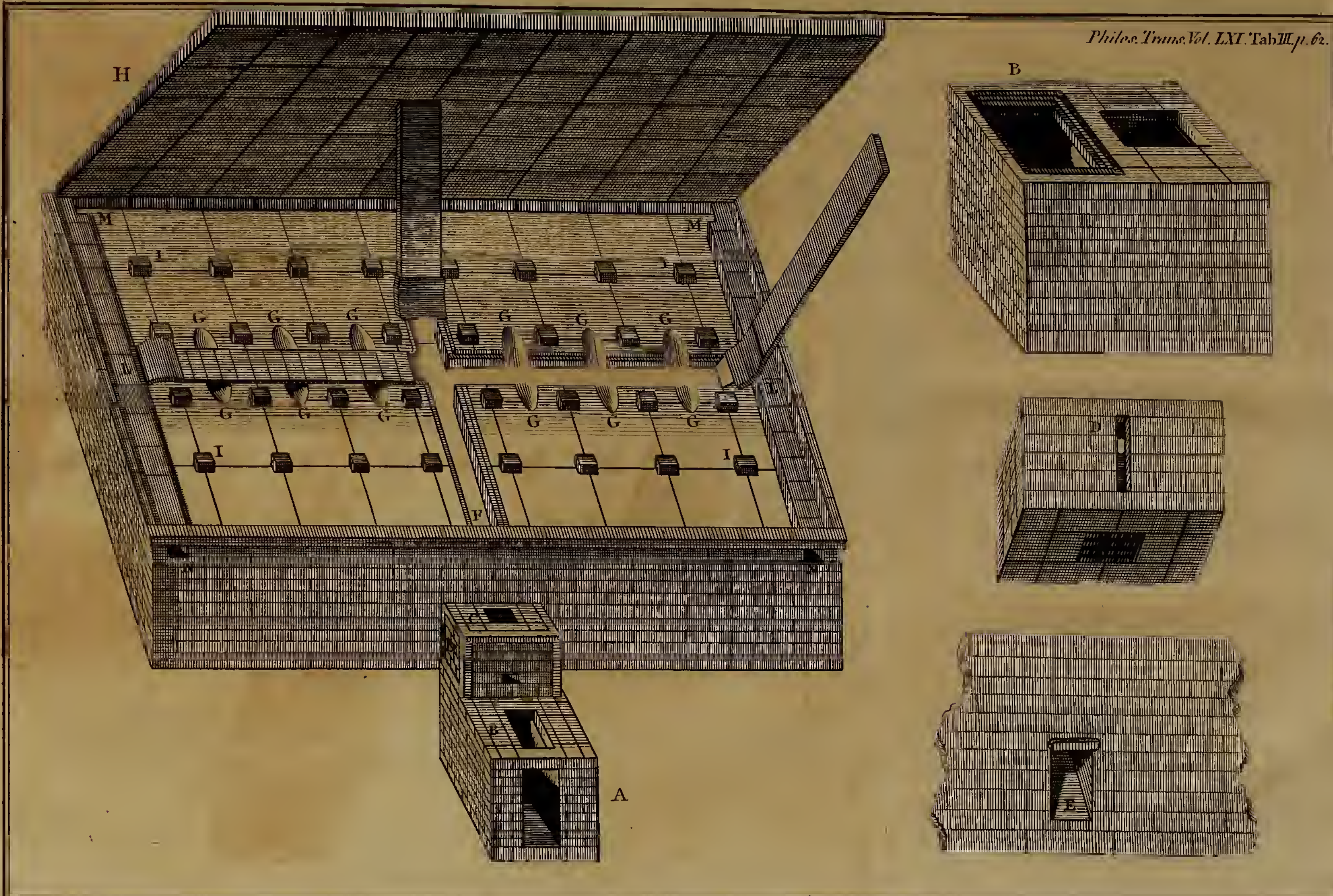
we ſhall only relate and deſcribe what is moſt eſſential.

1. A Kang is a kind of ſtove, that is heated by means of a furnace, which caſts all its heat into it. Many kinds of ſtoves, ovens, and furnaces, have indeed been contrived beyond ſea, which are ſomewhat like this; but the Chineſe ſeemed to have found means to unite all their conveniences and uſes in the Kang. They are of various ſorts, the *Kang* with a pavement, or *Ti-Kang*; the *Kang* for ſitting people, or *Koa-Kang*; and the chimney *Kang*, or *Tong-Kang*. As they are all made upon the ſame principle, we ſhall confine ourſelves to the deſcription of the *Kao-Kang*, from which the model (Tab. iii.) is taken.

The parts of a Kang are, 1. a furnace; 2. a pipe for the heat; 3. a brick ſtove; 4. two funnels for the ſmoke.

The furnace is proportioned to the ſize of the ſtove it is intended to heat. A is the aſh hole. B the cellar. C the furnace. D the ſlit, or mouth, that conveys the flame and heat into the ſtove. E The pipe or conductor for the heat. F begins at the mouth of the furnace, and forms a channel which falls in a right angle upon a ſecond, that goes quite through under the middle of the floor; and this laſt pipe has vent holes, G, here and there. The ſtove is a pavement made of bricks, H, which being ſupported at the four corners by little ſolid piles, I, leaves a hollow ſpace between them and the under pavement, where the heat remains pent up, and warms the floor. The ſmoke funnels are at both ends of the ſtove L, with a little opening M upon

 the



Basire del et sc.

the ſtove, and another N outward, which carries off the ſmoke.

Nothing can be more ſimple than the effect reſulting from the aſſemblage of all theſe parts. The heat of the furnace, impelled by the outward air, and attracted by the rarefied air of the ſtove, ruſhes through the ſlit, aſcends into the tube, ſpreads through the ſtove by the vent holes, heats the bricks, and from them the whole room. The ſmoke, which has a free paſſage, is carried off by the funnels.

2. Admitting this deſcription, which explains the model; let us next conſider what is requiſite for the conſtruction of a good Kang. The furnace may be placed either in the room itſelf, or in the next room, or without doors. The poor, who are glad to make the moſt of the firing that warms the *Kao-Kang*, on which they ſit by day, and ſleep by night, place the furnace in the ſame room; the middling ſort put it in an adjoining room; the rich and great have it on the outſide, and moſt commonly behind the north wall. The furnace muſt be much below the level of the ſtove, that the heat and flame may aſcend with the greater impetuoſity into the conductor, and not drive up the aſhes. The furnace is in the form of a cone, ſomewhat arched, that the activity of the heat and flame may be all impelled into the ſtove, and not fly off when the aperture at the top is left open. Note, that the two little moveable ſlips are planks, that take up occaſionally, when people want to go down into the cellar and empty out the aſhes. The opening in the furnace is narrow, and the lower end of the conductor muſt go quick up into the

ſtoves

ſtove. The conductor is to be walled in very cloſe on all ſides with bricks, and well cemented with mortar made of quick lime. That which the Chineſe uſe is made with one part of white lime to two of black. The black lime, of which we ſend a ſample, is found at the entrance of the coal pits, and ſeems to us to be no other than coals diſſolved by rain waters. We can atteſt that this ſubſtance mixed with white lime makes excellent mortar, nearly reſembling cement. It is proof againſt rain and ſun, and is uſed here to cover and ſhelter whatever is expoſed to the weather. We ſhould rejoice if this hint could prove uſeful to the Britiſh nation. If their country affords black lime, they are poſſeſſed of a great treaſure.

The ground or flooring of the ſtove may be of beaten clay, or, what is infinitely better, bricks placed edgewiſe, or large paving tiles.

The funnel for the ſmoke, or rather the two funnels, muſt be made with great care. Some make them terminate in little chimneys, that carry off the ſmoke above the roof. In the model, they open into the room, as the city poor have them; but in the country, and in gentlemen's houſes, they are on the outſide.

It is of conſequence that the little piles which ſupport the great ſquare bricks of the floor be very ſolid, and the bricks very thick and perfectly ſquare. The Chineſe bind them with a ſort of cement made of white and black lime, tempered with *Tong Yeou*, which is a kind of varniſh. We are apt to think walnut or linſeed oil boiled would do as well.

As

As ſoon as the Kang is compleated, fire is kindled in the furnace, to dry it quick and even. Great diligence muſt be uſed in examining it, in order to ſtop up all the little holes through which the ſmoke might eſcape. The wealthy, to make their Kang neater, and to moderate its heat, oil the bricks of the floor, and light the fire, to make the oil penetrate deeper, and to dry them the faſter. This oil is again the *Tong Yeou*, and may be ſupplied with walnut oil.

3. The *Ti Kang*, or paved Kang, is made like the *Kao Kang* juſt deſcribed; the only difference is, 1°, The pipe for the heat goes on riſing from the mouth of the furnace to the further end of the room. 2°, It does not communicate with a ſecond pipe, as in the model. 3°, The vent holes that convey the heat into the ſtove are all made narrow next the furnace, and go widening towards the ſtove. 4°, The funnels for the ſmoke always terminate without doors, or end in the little chimneys. 5°, In the Emperor's palace and thoſe of princes, the ſtove is covered with two rows of bricks, to confine the ſmoke, and to moderate the heat.

Note, That the bricks in the royal apartments are two feet ſquare, and four inches thick. They coſt near a hundred crowns apiece; and are ſo beautiful, good and ſolid, that you can have no conception of any ſuch thing beyond the ſeas. They are grey; but this is owing to the Chineſe manner of baking their bricks and tiles, which comes nearer to that of the antients than ours. Theſe bricks when coloured and glazed appear as fine as marble. The

Tong-Kang, or the *Kang* built in the wall, differ from the *Ti-Kang* only by its perpendicular poſition; the theory is the ſame.

4. The Kang is heated by lighting a fire in the furnace. The ſmoke and even the flame ruſhes violently into the pipe, and is carried off by the vent holes all through the ſtove, where, being pent up, it heats the bricks of the pavement in the ſpace of five or ſix hours: when a Kang is thoroughly heated, very little fire is required to keep it warm, though here the thermometer is almoſt all the winter at 9, 10, and even 12 or 13 degrees below the freezing point (in Reaumur's thermometer); and although all the rooms are on the ground floor, and have nothing but windows, and thoſe paper windows, all over the front, which is commonly to the ſouth, the warmth of the Kang is ſufficient to keep up their temperature at 7 or 8 degrees above froſt, with very little fire conſtantly kept up. It ſeldom riſes to more than 4 or 5 degrees in the Emperor's apartments, owing to the double row of bricks, but the warmth is very gentle and very penetrating.

As a Kang is heated by a furnace, any kind of fuel will do, *viz.* wood, charcoal, ſea coal, furze, &c. The Chineſe make the moſt of every thing. In the palace they burn nothing but wood, or a kind of coal which neither ſmoaks nor ſmells, and burns like tinder. The generality of people burn ſea coal: the poor in the country make uſe of furze, ſtraw, cow dung, &c.

A great ſaving may accrue from the following obſervation; the Chineſe, to ſave coals, pound them to the

the ſize of coarſe gravel, and mix them with one third, or even an equal quantity, of good yellow clay. This mixture being well kneaden, they make it up into bricks, which ſtrike a greater heat than wood, and come incomparably cheaper. The ſea coal thus tempered is far leſs offenſive; and beſides, the Chineſe, in order to draw off the noiſom vapours of the air, conſtantly heated by the coal fire, always keep bowls of water in the rooms, and renew them now and then. The gold fiſhes that are kept in theſe bowls are both an ornament and amuſement. In the palace, the Emperor's apartments are decorated with flower pots, and little orange trees, &c. The Chineſe philoſophers pretend that this is the beſt way to ſweeten the air, and abſorb the fiery particles diſperſed in it. They likewiſe leave two panes open night and day at the top of each window, to renew the air, which they think is too much rarefied by the heat. Theſe particulars may appear too trifling to be laid before the Royal Society; but, as they relate to the preſervation of their fellow citizens, we hope the worthy members will make allowances in favour of the motive.

5. The Kang is attended with many advantages and conveniencies. 1°, The rich and great are not expoſed to the troubleſome attendance on a fire in the chimney, and enjoy all its benefits. 2°, The poor uſe all ſorts of fuel without any other expence than what the kitchen requires, and have the comfort of ſitting warm by day, and lying warm by night. The fire in the furnace ſerves to dreſs victuals, and to heat the ſtove. The poor go further ſtill: they encloſe within the brick work of the Kang a veſſel, either

of copper tinned, or of iron, which ſupplies them with hot water for their tea. This water evaporates in the night, moiſtens the air of the room, and abſorbs the noxious particles of the ſea coal. We cannot forbear dwelling upon theſe little œconomical obſervations, as our aim is public utility. The Chineſe are wont to ſay, The Emperor can build a palace, and cannot make a ſhrub ; one word from his mouth makes a nobleman of a mere citizen, but all his wiſhes and prayers cannot prolong the lives of his favourites one ſingle moment.

It is not our part to point out what uſe might be made of the Chineſe Kang beyond the ſeas ; but we apprehend that the *Ti-Kang* might be a profitable improvement for hoſpitals, manufactories, &c. and a pleaſing one for the rich. The *Tong-Kang* properly managed would do very well in upper rooms, and would afford warmth for the bed-chamber, and fire for the dreſſing-room. The *Kao-Kang* ſeems leſs adapted to the cuſtoms and manners of Europe, but induſtry might find ſome uſe for it in the country. Should the Kang be rejected on account of its novelty, ſome hints might ſtill be taken from its conſtruction for the uſe of ſeveral kinds of handicraft.

The Chineſe ſea coal may give ſome inſight into the formation, qualities, uſes, and nature of this ſingural foſſil; but this would require a ſeparate paper. All we ſhall here obſerve is, that, as far as we can judge from the ſamples we have ſeen, it ſeems for the moſt part to be a ſtone diſſolved by the waters, and impregnated with ſulphur. Its hurtful qualities proceed from a mixture of antimony, copper, iron,

&c.

&c. The beſt coal, and that which burns fierceſt, is gloſſy, hard, and brittle. The Chineſe are very fond of that ſort that flies, and ſnaps in the fire, to burn in their forges, becauſe it contains a great deal of ſalt-petre. When the flame is blue, it is very fierce, but it is too dangerous, as the ſulphur is too predominant.

Peking, 22 Oct. 1769.

P. S. If we have expreſſed ourſelves improperly, which would not be very ſurprizing, conſidering how little we are verſed in theſe matters, and how little time we can ſpare for Europe, we are ready to retract whatever may be thought amiſs, and to give what further informations may at any time be deſired. Whoever has ſo loved the Chineſe for Chriſt's ſake as to come and ſeek them in this far country, has not diveſted himſelf of his attachment to Europe, and will ever be ſolicitous for the welfare of thoſe he has left behind, and endeavour to promote it, both by their prayers, and imparting whatever may conduce to alleviate the miſeries of this ſhort life.

In what a ſtriking light do we here ſee the vanity of the world, the intoxication of philoſophy, and the wretchedneſs of thoſe who know nothing of Jeſus Chriſt! Learning, vice, and idolatry, go hand in hand in the ſanctuary of policy; which knows nothing but the Creator of the world, whom the Chineſe worſhip on their knees, and diſhonour in their lives.

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As there is room left in the box that contains the model, we have put in ſome little ſpecimens of ſea coal.

N° A. 1, 2, 3, different ſorts of ſea coal, or rather ſtones diſſolved, and turned to coals.

N° B. 1, 2, ſea coal, ſuch as is burnt here. N°. 2 is the beſt. N° 3. is the ſame coal turned to black lime. This you may be convinced of, by diſſolving it in water, and mixing it with white lime.

N° C. 1, 2, 3, 4, ſeveral degrees of bad coal, which produces a dangerous ſmoke that occaſions fainting fits. N° 4. is the worſt, and is laid by for the uſe of forges, whenever it is found.

N° D. is a kind of Clinker, extracted from the aſhes of ſea coal. That which produces the greateſt quantity of it is reckoned the beſt.

If the Society ſhould be deſirous of further informations concerning the ſea coal, we muſt beg to be favoured with particular queſtions; but let it be remembered, that we are not within reach of ſuch helps as chemiſtry would afford, nor can elucidate the matter by experiments.

IX. *Account*

IX. *Account of a remarkable Thunder Storm: In a Letter from the Rev.* Anthony Williams, *Rector of* St. Keverne, *in* Cornwal, *to the Rev.* William Borlase, *D.D. F. R. S.*

DEAR SIR,

Keverne, Aug. 27, 1770.

Read Feb. 7, 1771.

I HAVE received yours, which, I muſt confeſs, I ought to have anſwered much ſooner.

For ſeveral days before the thunder ſtorm which fell on St. Keverne ſpire and church, on Sunday the 18th day of February laſt, the wind was very hard at North and North Weſt, accompanied with violent ſhowers of hail, which had done ſome damage to the roof of the church, and many houſes in the church-town. On the Sunday morning above-mentioned, the wind being at North-weſt, from five o'clock during almoſt the whole day the wind was exceſſive hard; and about ſix, I ſaw ſome few faint flaſhes of lightning, which, as the day came on, if it continued, became imperceptible. The weather being ſo bad, prevented a great number of people from coming to church, which in all human probability was a happy circumſtance; for, about a quarter

quarter after a eleven o'clock, while I was in the latter end of the Litany ſervice, we had a very fierce flaſh of lightning, followed at the diſtance of about four or five ſeconds by the loudeſt thunder I remember ever to have heard; but which did no damage, nor ſeemed in the leaſt to diſturb any of the congregation, though at the ſame time the roof of the church was rifling, and the hail made a noiſe terrible to be heard. In half a minute after this, as near as I can poſſibly gueſs, the whole congregation, except five or ſix perſons, were at once ſtruck out of their ſenſes. I myſelf received the ſhock ſo ſuddenly as not to remember I either heard the thunder or ſaw the lightning: the firſt thing that I can recollect with any degree of certainty is, that I found myſelf in the vicarage ſeat, which is very near the deſk, without either gown or ſurplice, bearing in my arms as I then thought a dead ſiſter, and God knows it was a miracle that ſhe was not ſo; I perceived a very ſtrong ſulphureous ſmell, almoſt ſuffocating, and a great heat. At this time the confuſion among the congregation was inconceivable, ſome running out of the church for ſafety, and returning into it again (for the ſtones from the roof were falling on our heads, both in and out of the church); ſome on their knees, imploring the divine aſſiſtance, giving themſelves up to certain deſtruction; and a great many, in different places of the church, lying quite motionleſs, whom I thought then to be quite dead.

In the afternoon, my thoughts being a little compoſed (I believe for full two hours I could not be ſaid to be rightly in my ſenſes), I walked to the church, to ſee what damage was done; and ſuch a ſcene

ſcene preſented, as is horrible to think of, much more to ſee. The church-yard was almoſt full of ruins; the ſpire, which was about forty-eight feet high from the battlements of the tower, was carried off half way down, and the remaining part cracked in four places very irregularly down to the bottom. The north ſide of the tower from the battlements to the arch of the bell chamber window was quite out, except the corner ſtones, which remained firm and unremoved; the lead on the top of the tower was greatly damaged, melted in ſeveral places, and as it were rolled together. The arch of the belfry door, which was very ſtrongly built with a remarkable hard iron ſtone, laid in lead, was alſo greatly damaged; ſome of the ſtones were cracked croſs-ways, and juſt removed out of their places, others were quite hove out, and the lead between the joints not only melted, but looſened ſo as that you might pick it out with your fingers. The traces of the lightning were here diſcovered along the ſurface of the earth; the ſtones were thrown from the ſpire on the tops of many houſes in the Church Town, but did no great hurt; in a gentleman's houſe, one ſtone weighing fourteen pounds fell through the roof into the chamber, but did no further hurt than to make a hole in the roof and plaiſtering. It is to be obſerved, that the ſtones from the ſpire were ſcattered in all directions, as well againſt the wind as with it, ſome of which, but not very large, were found but a little ſhort of a quarter of a mile. The ſpire from the top ſix feet downwards was ſolid, through which paſſed an iron ſpill to fix the weather-cock on. Did not the lightning firſt ſtrike on this ſpill, and was conducted

 through

through the ſolid part of the ſpire, and, having not iron to conduct it any further, burſt in the hollow part of the ſpire, and threw the ſtones about in all directions? It is remarkable that the ſpill was found in the bell-chamber, and the weather-cock in the battlements; and that the bells were not in the leaſt damaged, though a deal board, that lay acroſs the beams to which the bells were hung, was ſplit long ways in two pieces. The inſide of the church ſtill preſented a much more horrible ſpectacle; the roof of the church was almoſt all gone, and ſome of the timber work in the north iſle ſhattered to pieces; every ſeat in the church had rubbiſh in it, ſome more ſome leſs, and ſtones of large ſize, ſome of 150 pounds weight and upwards, ſcattered here and there amidſt the congregation, which damaged the ſeats, &c. but did no hurt to the people, though they ſat in thoſe very ſeats where the ſtones fell. The lightning entered at the three ends of the church at Weſt, made its way through the body of the church, and went out through the three ends of the church at Eaſt; the holes where it came in and where it went out are not large, neither are the walls much damaged. The belfry window was ſhattered to pieces, not one whole pane I believe to be found in it; many other windows alſo ſuffered greatly, the glaſs and munnions being much ſhattered. The lightning entered alſo through two places in the roof, one near the ſinging loft, and ſtruck upon the top of a pillar juſt by it; the traces of it are to be ſeen from the top of the pillar almoſt to the bottom: there were then ſitting by this pillar two young men, one in the ſinging loft, and other under him in the church, who were

were both lightly ſcorched; he in the loft from head to foot, and the other in the face only: but it is remarkable that his hat, which hung on a nail juſt above him, was cut in two pieces. In the other place, the lightning entered juſt over the deſk and pulpit, and fell in like manner on a pillar that ſtands in the vicarage ſeat; but here it was a great deal more violent, and, as the object of its fury was my ſiſter, I hope you will excuſe my being very particular. Upon this pillar reſted a large oak ſoil, the bottom of which was burſt into ſix pieces, and one of the pieces, being a very large one, was thrown from its place to the diſtance of about 20 feet, and appeared to be burnt, the other pieces did not fall. From thence the lightning came down the pillar with great force, tore the ſeat into many pieces, knocked down my ſiſter, and made its way through the bottom of the ſeat into the earth. She had pattens on, and the wooden part of one of them was broke into three pieces; the holes, through which the ribbon is put to tie them together, were quite burnt out, and the ribbon found in the ſeat without the leaſt damage, or ſo much as the knot looſened; her ſhoe was burnt, and rent from the toe to the buckle; but the buckle, which was of ſilver, remained unhurt; her ſtocking was burnt and rent in the foot, juſt in the ſame manner as her ſhoe, and ſcorched along to the garter, and two little holes were burnt through in the leg of it: her apron, petticoats, &c. were burnt through and through, and ſhe had ſeveral ſlight burns on ſeveral parts of her body, beſides two bruiſes on her head and breaſt, cauſed by the rubbiſh that fell into the ſeat. As ſhe was carrying out of church, ſhe greatly complained of a deadneſs in her legs, which, as ſhe could not move them at all, I ſuppoſed were broke; however, they were not broke, only a little burnt, and turned as black as ink; which, by timely care, not only came to their natural colour by Tueſday noon, but could ſupport her alſo to come down ſtairs; and, excepting a hurry of ſpirits, grew quite well that week.

Not more than ten perſons out of the whole congregation were hurt, and none of them to any great degree; one young fellow, who was more frightened than hurt, remained ill a long time, but I believe he is now quite well; the lightning touched his watch in his pocket, the marks of which may be ſeen on the cryſtal and ſilver part of it to this day. Nobody remembers to have heard any more thunder, or ſeen any more lightning after this, though the weather continued very ſtormy all that day; ſo that this thunder ſtorm, from beginning to end, could laſt but a very ſhort time. The damage we ſuffer by it (which is now repairing) will amount to about 450 l.

Thus, Sir, I have given you a particular account of this dreadful accident, by which a great number of people, had it not been for the favourable, I may ſay, miraculous interpoſition of Providence, muſt inevitably have periſhed. It muſt really excite our wonder to conſider that not only not one life was loſt, but that no perſon was hurt to ſuch a degree as to confine him for more than two or three days.

I remember to have ſeen an obſervation of yours: "How deplorable would be the conſequences of ſuch "blaſts of lightning, if they happened where are "large

" large congregations in time of divine ſervice!" Here you ſee, Sir, they have happened under the very circumſtances in which you then thought they muſt prove fatal. But Providence has let us know, in this remarkable caſe, that, let the danger be ever ſo great, and ſeemingly to us unavoidable, yet he is willing, as well as able, to ſave us.

I am, dear Sir,

Your moſt obedient,

humble ſervant,

Anth. Williams.

X. *Explication of an inedited Coin, with two Legends, in different Languages, on the Reverſe. In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Cuſtos Archivorum of the Univerſity of* Oxford, *Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

GOOD SIR,

Read Feb. 7, 1771.

THE coin I ſhall here attempt to explain on one ſide (See TAB. III. n. 1.) preſents to our view the head of Jupiter, and on the other the prow of a ſhip, which indicates the place wherein it was ſtruck to have been a maritime town. Above the prow of the ſhip, we ſee two characters, that are either Punic (1) or Phœnician. I ſay, either Punic or Phœnician, becauſe it may not perhaps be ſo eaſy to determine whether that town was occupied by the

(1) From the preſent ſtate of the Kabyles we may infer, that the ancient Africans, or Indigenæ, their progenitors, muſt have been a very rude uncivilized people, at the firſt arrival of the Carthaginians amongſt them. It is therefore utterly improbable, that they ever uſed any alphabetical characters, before the Phœnician letters were introduced into their country by the Carthaginians; or that any other characters, peculiar to themſelves, and different from the Punic, ever afterwards prevailed amongſt them. I cannot therefore but think, that thoſe learned men who ſuppoſe the reality of ſuch characters are egregiouſly miſtaken, as they can have nothing to advance in ſupport of ſuch an opinion. Shaw, *Travels*, *&c.* p. 288, 289, Oxford, 1738. Peller. *Suppl. quatr. & dern.* p. 55. A Paris, 1767.

Cartha-

Carthaginians, or the Phœnicians, and the Romans, when the piece firſt appeared. Beſides the two abovementioned characters, there is a monogram, formed of the three Latin letters V, A, B, very indifferently preſerved, in the exergue, with which the Punic or Phœnician elements perfectly correſpond. For the ſecond of thoſe elements is manifeſtly the moſt common Punic or Phœnician form of *Beth*, and I have many years ſince proved the firſt (2) to be a form of the Phœnician and Samaritan *Vau*; and (3) obſerved, that though it ſometimes anſwers to V conſonant, it is likewiſe not ſeldom taken for *Beth*, or B. Nor is this to be wondered at, as thoſe two letters ſo nearly in power approach one another. The middle element of the monogram, A, has nothing equivalent to it in the Punic or Phœnician inſcription; that vowel, in conformity to the genius of the oriental orthography, between the two conſonants, being there ſuppreſſed. But the two Punic or Phœnician characters, and the Latin monogram, clearly enough demonſtrate the name of the town where the piece was ſtruck. The monogram ſeems to be preceded by a ſort of date in the exergue, which reſembles the characters ⌊CXI; but, as theſe characters are ill preſerved and indiſtinct, I believe the powers of them will not be ſo eaſily aſcertained.

From what has been here laid down, the learned will eaſily admit the medal in queſtion to have been ſtruck at Vabar, a maritime city of Mauritania Cæſarienſis, after that place had been ceded to the Romans, and was inhabited by them, and either the Carthaginians or the Phœnicians. Vabars i mention-

(2) *De Num. quibuſd. Sam. et Phœn. &c. Diſſert.* p. 73, 74. Oxon. 1750. (3) Ibid. p. 74.

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ed by (4) Ptolemy, but in his days ſeems to have been a place of no conſiderable note. It, however, probably made a greater figure, when inhabited either by the Carthaginians, or the Phœnicians, and the Romans; for that it was occupied by two at leaſt of thoſe nations, when the medal before me firſt appeared, the legends on the reverſe, though ſomewhat imperfect, render ſufficiently clear. That the Carthaginians were poſſeſſed of this city in ancient times, is conſonant to the faith of hiſtory; ſince they were maſters of all that part of Africa extending from the pillars of Hercules, or ſtreights of Gibraltar, to the greater Syrtis, for a conſiderable period of time, as we learn from (5) Polybius. And that the Phœnicians were maſters of it in early times, is equally probable. For that they occupied the ſea-coaſt of Mauritania, from at leaſt the generation immediately preceding Homer to the time it fell into the hands of the Romans, we are informed by Strabo (6). It cannot therefore be eaſily determined, as already obſerved, whether the piece in queſtion was ſtruck by the Phœnicians or the Carthaginians. It muſt, however, be attributed to the town of Vabar, when inhabited by either the Carthaginians or the Phœnicians, not improbably the latter, and the Romans; the two legends on the reverſe, as well as the perfect agreement between them, rendering this inconteſtably clear.

That the piece I am conſidering was either of a Punic or an Africo-Phœnician origin, may be deemed probable from hence, that it exhibits a Latin legend

(4) Ptol. *Geogr.* Lib. IV. c. ii.

(5) Polyb. Megalopolit. *Hiſtoriar.* Lib. iii. p. 266, 267. Amſtelodami, 1670.

(6) Strab. *Geogr.* Lib. iii. p. 150, 151. Lutetiæ Pariſiorum, 1620.

on

on the reverse, as do several other Punic or Africo-Phœnician coins. This appears from some of the medals of the elder Juba, one of Achola, and another of Leptis, now in my small collection; to omit other similar instances, that might, with great facility, be produced: whereas, unless I am greatly deceived, none of the Asiatico-Phœnician coins have ever yet presented to our view any Latin characters. This is an additional proof in support of what has been here advanced; and seems farther to evince, that my medal must be assigned to the town of Vabar, and was struck there, when that place was occupied by either the Carthaginians or the African Phœnicians, and the Romans; though the time of that operation cannot, with any tolerable precision, be ascertained.

I shall only beg leave to add, that though Vabar was a place of no great note in the days of Ptolemy, it seems to have been a town of some consideration in the earlier times, as (7) Dr. Shaw saw some ancient ruins on the spot where it formerly stood; that a coin of this ancient city has never yet, I believe, been communicated to the learned world; that the medal before me, which at present has a place in my small cabinet, is one of those very few Punic or Phœnician coins that are adorned with a Latin legend, and consequently merits the particular attention both of the curious and the learned; and that I am, with the highest regard,

S I R, Your much obliged

and most obedient humble servant,

Christ-Church, Oxon.
Sept. 28, 1770.

John Swinton.

(7) Shaw, ubi sup. p. 89. Oxford, 1738.

XI. *Remarks upon Two* Etruſcan *Weights, or Coins, never before publiſhed. In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Cuſtos Archivorum of the Univerſity of* Oxford, *Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

Good Sir,

I.

Read Feb. 7, 1771.

THE firſt piece to be conſidered here is an Etruſcan as, (See Tab. III. n. 2.) or weight, exhibiting on one ſide the head of Janus, covered with a cap; and on the reverſe a club, attended by the mark of the As, and the legend I◁OA⅃Ǝꟻ, in Etruſcan characters. Between the two faces of Janus, the head of a bufolo, or wild ox, preſents itſelf to our view, as does alſo a ſort of concha marina, or ſea-ſhell, ſomewhat reſembling one on a didrachm of Tarentum, in my ſmall cabinet, and ſeveral other ancient coins, contiguous to the cap; both of which have, not a little, ſuffered from the injuries of time. The letters on the reverſe are

more

more rude and barbarous than thoſe of any ſimilar Etruſcan coins hitherto publiſhed, which is an inconteſtable proof of the exceeding high antiquity of this piece. The forms of ſeveral of them are likewiſe ſomewhat different from thoſe of the correſpondent elements on all the other ſimilar Etruſcan weights, hitherto communicated to the learned world. The concha marina, and perhaps the bufolo's head, is a ſingularity that will announce the weight before me an inedited coin. The piece weighs preciſely five ounces, and twelve grains; and is, in all reſpects, except what relates to the concha and bufolo's head, tolerably well preſerved.

The firſt riches of mankind were their flocks and their herds, and particularly their (1) oxen. Hence the firſt money in Italy, from pecus, was (2) called pecunia, and the moſt ancient braſs coins had the figure of an ox (3) impreſſed upon them. Hence alſo the Greeks, in the days of Homer (4), eſtimated the value of their properties according to the number of oxen they were equivalent to, as we learn from that celebrated poet. For he informs us, that Glaucus's golden armour was worth an hundred oxen, whereas that of Diomedes, for which it was

(1) Eraſm. Frölich. *Notit. Elementar. Numiſm. Antiquor. &c.* p. 2. Viennæ, Pragæ, & Tergeſti, 1758.

(2) Plin. *Nat. Hiſt.* Lib. xviii. p. 98. l. 6, 7. & Lib. xxxiii. p. 610. l. 6,7. Ed. Hard. Pariſiis, 1723. Fröl. ubi ſup. et alib. *Una lettera del* Annib. degli Abati Olivieri *al Sig. Abate Barthelemy, &c.* p. 28, 29. In Peſaro, 1757. Ezech. Spanhem. *De Uſ. et Præſtant. Numiſmat. Antiquor.* Diſſert. prim. p. 23. Londini, 1706.

(3) Iidem ibid.

(4) Hom. Il. z. 235, 236, 237. Vid. etiam Euſtath. in loc.

 exchanged,

exchanged, did not exceed the value of nine of thoſe animals. The figure of the ox on the moſt ancient money ſeems to have been ſoon converted in Etruria into the ſymbol of the head of that beaſt connected with the head of Janus, who is (5) ſaid to have firſt introduced the uſe of money into Italy. The head of the bufolo, or wild ox, may, as I conceive, have appeared on ſome of the moſt ancient coins of Tuſcany, and particularly the piece I am at preſent conſidering, becauſe the bufolo was formerly, and is ſtill, a native of that country. When I reſided there, above thirty years ſince, the woods between Leghorn and Piſa abounded with them. They were then likewiſe ſaid to have been very numerous in other parts of Tuſcany, and La Romagna; and ſeveral of them, at different times, both tamed and wild, I myſelf have there ſeen The reaſon here mentioned ſeems to extend to other remains of antiquity of the Etruſcans beſides their coins, on which the head of the bufolo appears, as the (6) authors here referred to have rendered ſufficiently clear.

From what has been obſerved, as well as from the thickneſs, high relief, and extreme rudeneſs of the workmanſhip, or rather in conjunction with theſe, we may conclude, that our As is either coeval with ſome

(5) Cytherius Poet. apud Athen. *Deipnoſoph.* Lib. xv.

(6) Anton. Franciſc. Gor. *Muſ. Etruſc.* Tab. CXXIII. Hamilton's *Collect. of Etruſc. Greek, and Rom. Antiqu.* Vol. II. pl. 63. I have a fine Etruſcan Vas Potorium, (See Tab. III. n. 3.) ending in a bufolo's head; which, as I apprehend, formerly belonged to Cardinal Gualtieri: as alſo another, terminating in (See TAB. III. n. 4.) the head of a gray-hound, ſimilar to one publiſhed by Mr. Hamilton, which had likewiſe a place aſſigned it in the Cardinal's collection of Etruſcan antiquities. I bought both of them of Sig. Barazzi, at Rome, in 1733. Hamilton's *Collect. of Etruſc. Greek, and Rom. Antiquit.* Vol. I. pl. 49.

of

of the earlieſt pieces, or weights, ever uſed in Italy, or but little poſterior to them. Father Gori (7) ſeems to be of the ſame ſentiments with me, in this particular; and neither Sig. (8) Olivieri, nor any other writer, has invalidated, or diſproved, what has been advanced on this head, by that celebrated author.

That the weight here conſidered is to be aſſigned to a maritime town, the concha marina, or ſea-ſhell, irrefragably proves. I ſhould therefore, with the very learned Sig. Olivieri, rather attribute it to Volterra, than to Velitræ (9), at preſent called Velletri, as Father Gori (10) ſeems to have done. For Velitræ was a town of Latium, and much leſs conſiderable than the city of Volterra; which (11) was the moſt ancient city of Etruria, the ſeat of a lucumo, and one of the moſt conſiderable places in Tuſcany. It was alſo a maritime city (12),

(7) Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. II. p. 419. et alib. Florentiæ, 1737.

(8) Sig. Oliver. *Una Letera, &c.* In Peſaro, 1757.

(9) Annib. degli Abati Olivieri, in *Eſame della Controverſia letteraria, ſopra il Muſeo Etruſco, ſtampato negli Opuſcoli ſcientifici*, Tom. XXI. et ubi ſup. p. 43. The old Etruſcan word VELATRI, FELATRI, or FELATERI, ſeems to have been tolerably well preſerved in the name of Monte Veltrajo, or Feltraio, a mountain in the territory of Volterra, and about two miles from that city. This, as I apprehend, may be conſidered as an additional argument in ſupport of Sig. Olivieri's and Monſignore Mario Guarnacci's opinion. *Notizie Iſtorich. della citta di Volterra, oper. del Sig.* Lorenz. Aul. Cecin. *dal Caval.* Flamin. Dal Borgo, p. 44, 49. In Piſa, 1758. Monſig. Mar. Guarnacci, in *Origin. Italich.* In Lucca, 1767.

(10) Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. II. p. 427.

(11) Chriſt ph. Cellar. *Geogr. Ant.* Lib. II. c. ix. ſect. 2. p. 573, 574. Lipſiæ, 1731. Vid. etiam Tho. Dempſt. *De Etrur. Regal.* et Anton. Franciſc. Gor. *Muſ. Etruſc.* paſſ.

(12) Strab. Lib. V. p. 223. Lutetiæ Pariſiorum, 1620.

as we learn from Strabo, being ſeated not far from the Vada Volaterrana, near the place were the river Cæcina threw itſelf into the Tyrrhenian ſea. I would therefore read the legend, on the reverſe of this coin, FELATHERI, FELATERI, or FELATERRI; the fifth letter being (13) ſometimes endued with the power of *Theta*, and ſometimes with that of *Tau*; and a duplication of conſonants, in writing, having been unknown to the moſt ancient Etruſcans. That the vowel E, between the fifth and ſixth elements of the Tuſcan legend, on the reverſe hereon, ſhould be ſuppreſſed, or omitted, will not be any matter of ſurprize to thoſe who are apprized, that ſuch a ſuppreſſion, or omiſſion, ſo conſonant to the genius of theHebrew and Phœnician orthography, from which that of the moſt (14) ancient Etruſcans could not have greatly differed, in old Tuſcan words, does not ſeldom (15) occur. Of this MENLE, HERCLE, MELACRE, PHVLNICE, MENREA, which were read MENELE, or MENELAE, HERCVLE, MELEACRE, PHVLVNICE, MENEREA, or MENERFA (16), are convincing proofs; to omit many other ſimilar inſtances, that might, with great facility, be produced. But this is ſo ſettled a point, that it

(13) Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. II. p. 408, 409.

(14) Swint. *De Linguâ Etrur. Regal. Vernac. Diſſertat.* Oxon. 1738.

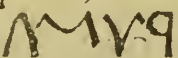
(15) Phil. Bonarot. Tho. Dempſt. et Anton. Franciſc. Gor. ubi ſup. paſſ. Vid. etiam Joan. Bapt. Paſſer. Piſaurenſ. *Helleniſm. Etruſcor. in Symbol. Litterar. &c.* Vol. II. p. 35—73. Florentiæ, 1748, & Carlo Antonioli in *Antic. Gem. Etruſc. ſpiegat. & illuſtrat. &c.* p. 70—78. In Piſa, 1757.

(16) Iidem ibid.

4 will

will not be conteſted in any part of the learned world.

II.

The ſecond piece, or weight, of which I propoſe to give an account in this paper, is a ſtips uncialis, (See TAB. III. n. 5.) as appears both from the weight and ſize of it, of the earlieſt date. I received it, as a preſent, from my worthy friend James Gilpin, Eſq; late recorder of the city of Oxford, with ſeveral other ancient braſs coins. On one ſide it has preſerved the head, or rather a full face, of the * ſun; the workmanſhip of which is more rude and barbarous than that of any other ſimilar piece that ever fell under my view, and done pefectly in the moſt ancient Etruſcan taſte. The reverſe had originally on it the prow of a ſhip, which has been ſo totally effaced by the injuries of time, that only a very few exceeding faint traces of it are now to be ſeen. The relief on the face-ſide is very high, as was undoubtedly at firſt that on the other; but the reverſe being in a manner quite ſmoothed, nothing there remains but the veſtiges of the prow of a ſhip, that are barely viſible. However, juſt over the prow, we may diſcover clearly enough the legend , in Etruſcan characters, though but very indifferently preſerved. That

* We meet with a full face of the ſun pretty much reſembling this on a denarius of the Plautian family, but done in a much more elegant taſte; which demonſtrates it to be vaſtly inferior, in point of antiquity, to the piece I am now offering my thoughts upon. Sig. Havercamp. *Theſaur.* Morel. Tom. II. p. 329. Amſtelædami, 1734.

word

word is apparently equivalent to ROMA, and consequently the piece itſelf muſt be deemed an uncia, or ſtips uncialis, of Rome, though the globule, or uncial mark, has not eſcaped the ravages of time.

That the piece in queſtion is an uncia of Rome, appears not only from the legend on the reverſe, as juſt obſerved, but likewiſe from another uncia of Rome, with the full face of the ſun upon it, as here, though done in the more modern Roman taſte, now in my ſmall collection. That uncia is likewiſe, I doubt not, in ſeveral (17) other collections; being, as I apprehend, a pretty common coin. The ſame concluſion is likewiſe deducible from another Roman uncia, with the word ROMA, juſt above (See Tab. III. n. 6.) the prow of the ſhip, on the reverſe, in the very ſame ſituation as the Etruſcan legend on the weight before me, in my little cabinet. We may therefore ſafely enough pronounce the coin here deſcribed a ſtips uncialis of Rome, of a very remote antiquity, with the Etruſcan name of that capital of the world on the reverſe.

The Etruſcan letters were, undoubtedly, the firſt alphabetic characters of Italy. Nay, they prevailed at Rome, and in every part of Italy, till after the regifuge. This I have fully proved in a Latin (18)

(17) Scipionis Maffeii *Origin. Etruſc. & Latin.* p. 61. Tab. II. Ord. II. n. ii. Lipſiæ, 1731. Annib. degli Abati Olivieri in *Indice Delle Antichiſſ. Monete di Bronzo Romane & Italich.* ubi ſup. p. 53. In Peſaro, 1757. See Haverc. in *Rom.* Tab. III. n. 4.

(18) *De Priſcis Romanorum Literis Diſſertat.* Oxon. 1746. Many curious particulars are deducible from the point here inſiſted upon. To omit others that occur, from hence it plainly follows, that thoſe inſcriptions on the Eugubian tables conſiſting of Latin letters, or the more modern characters of Italy, are

diſſertation,

diſſertation, printed at Oxford, in 1746. The piece at preſent engaging my attention is an additional, or rather an apodictical, proof of the truth of what was there advanced. It demonſtrates the Etruſcan letters to have been uſed at Rome, in very early times; and conſequently evinces, in the ſtrongeſt manner, the principal point inſiſted upon in that diſſertation.

The time when the medal I have been conſidering firſt appeared, for want of proper chronological characters, and ſufficient light from hiſtory, cannot, with any preciſion, be aſcertained. But, from a peruſal of the ſmall performance (19) above-mentioned, and what has been offered here, we ſhall, I believe, be induced to conclude, that it is at leaſt coeval with the regifuge, which happened in the year of Rome 245; or rather, as I ſhould apprehend, that it may be a conſiderable number of years anterior to that event.

The two braſs coins above deſcribed being extremely curious, eſpecially the ſecond of them, an Etruſcan coin of Rome having never been heard of before; and many curious points being deducible

more recent than the regifuge. Nay, in the diſſertation here referred to, they have been demonſtrated inferior, in point of antiquity, to the Duilian inſcription; and conſequently Father Gori muſt be egregiouſly miſtaken, when he makes all the inſcriptions on thoſe tables ſome generations older than the Trojan war. See the *Univerſal Hiſtory*, Vol. XVI. p. 48. Lond. 1748.

It may not be improper to remark here, that the Etruſcans had not the letter O in their alphabet, but conſtantly made uſe of V for that element. Hence it came to paſs, that the Etruſcan name of Rome was not Roma, but Ruma, as it appears on my very ancient coin. *Philoſoph. Tranſact.* Vol. LVIII. p. 256.

(19) *De Priſc. Romanor. Lit. Diſſertat.* p. 6, 7, 8, 9, 10. Oxon. 1746.

 from

from that coin, which I have not time at preſent even to touch upon; I was willing to flatter myſelf, that the ſhort account of them, in this paper, would not be unacceptable to the Royal Society. I have therefore taken the liberty to tranſmit it to you, in order to its being laid before that very learned and moſt illuſtrious body; and am, with great truth,

Good Sir,

Your much obliged,

and moſt obedient, humble ſervant,

Chriſt-Church, Oxon,
Sept. 29, 1770.

John Swinton.

XII. *Interpre-*

XII. *Interpretation of Two* Punic *Inscriptions, on the Reverses of two* Siculo-Punic *Coins, published by the Prince* di Torremuzza, *and never hitherto explained. In a Letter to* M. Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Custos Archivorum of the University of* Oxford, *Member of the Academy* degli Apatisti *at* Florence, *and of the* Etruscan *Academy of* Cortona *in* Tuscany.

DEAR SIR,

Read April 11, 1771.

THE two Punic legends, of which I am now to attempt an interpretation, have been published, together with five others, by the Prince (1) di Torremuzza, in his volume of ancient inscriptions, printed at Palermo, in 1769. As the coins, on which they have been preserved, seem extremely curious, and are unnoticed by any other author; the Royal Society will indulge me the liberty of transmitting them my sentiments of those very valuable remains of antiquity, in this paper, drawn up in the shortest and most concise manner possible.

(1) *Sicil. et objacent. insular. veter. inscript. nov. collect. &c.* class. XX. p. 292, 293. Panormi, 1769.

 The

I.

The firſt of theſe minute inſcriptions, (See TAB. III. n. 7.) which is the firſt of thoſe publiſhed (2) by the Prince di Torremuzza, in the place here referred to, adorns a fine Punic tetradrachm, as it ſhould ſeem, well enough preſerved; which on one ſide preſents to our view the head of a woman, and three fiſhes; but, on the reverſe, the head of an horſe, behind which ſtands a palm tree, attended by an inſcription, in the exergue, formed of ſeven Punic letters. The workmanſhip, as well as the types, is probably ſimilar to that of the ſilver medals of Menæ, by me deſcribed and explained, in (3) one of my former papers.

The firſt of the letters, of which this inſcription is compoſed, will be allowed an *Ain* of the uſual Punic form. This may be collected from the (4) coins of Menæ, juſt mentioned, as well as others, that might eaſily be produced. The ſecond ſeems to be *Nun*. But it has been inaccurately taken, and is in reality *Mem*. This is likewiſe clearly evinced by the (5) legends on the reverſes of other ſimilar Siculo-Punic coins. The third is undoubtedly the Punic

(2) Ibid. p. 292.

(3) *Philoſoph. Tranſact.* Vol. LIV. Tab. xi. n. 1. p. 99, 404.

(4) *Philoſoph. Tranſact.* ubi ſup.

(5) *Philoſoph. Tranſact.* ubi ſup. D. Bernard. Aldret. *Var. Antiguedad. de Eſpan.* &c. p. 177—180. D. Vincen. Juan de Laſtanoſ. *Muſ. de las Medal. deſconocid. Eſpañol.* Tab. 45. En Hueſca, 1645. Peller. *Recueil de Medaill* &c. Tom. Trois. p. 22. pl. 88. n. 5. A Paris, 1763. *Memoir. de Litterat. de l'Academ. des Inſcript. & Bell. Lettr.* &c. Tom. Trenuem. p. 417. pl. ii. n. 8, 9, 10, 12. A Paris, 1764.

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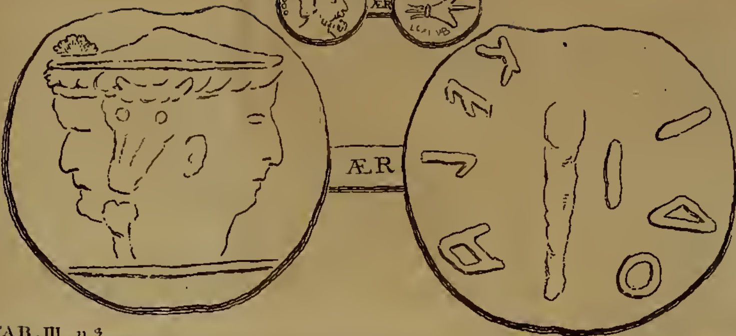
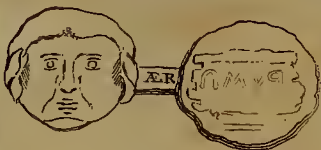
TAB. III. n. 1.

TAB. III. n. 2.



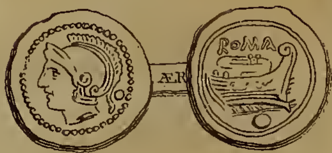
TAB. III. n. 3.

TAB. III. n. 5.

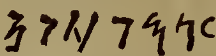


TAB. III. n. 4.

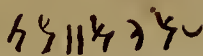
TAB. III. n. 6.



TAB. III. n. 7.



TAB. III. n. 8.



Basire sc.

or Pœnician *Samech* (6), nearly as it appears in the famous Maltese inscription, and not unlike the form of that element exhibited by one of those found at Citium, now in the Bodleian Library, Oxon. The fourth and sixth are so (7) like the Punic and Phœnician *Ghimel* that they cannot well pass for any other element. The fifth is manifestly *Hheth* (8), though it seems to have somewhat suffered from the injuries of time. The seventh (9) greatly resembles the most common figure of *Thau*, and therefore we cannot be much mistaken if we take it for that letter. The powers of the Punic characters forming the inscription standing thus, we may, I conceive, read the whole AM SEGHEGT, or SEGEGTH, which is but a small variation from the word SEGESTE, or SEGESTA, the Greek and Latin name (10) of a considerable maritime city of Sicily, not far from Eryx, where money was coined, after the Greeks (11) had possessed themselves of the place. The medal therefore adorned with this minute Punic inscription may, without any impropriety, be supposed to have been emitted from the mint at Segesta, as the Punic words, AM SEGHEGT, or SEGEGTH, POPVLVS SEGESTANVS, appear upon it, when the Carthaginians

(6) *Philosoph. Transact.* ubi sup. Vol. LIV. Tab. xxii. p. 394. *Memoir. de Litterat. &c.* ubi sup.

(7) *Philosoph. Transact.* ubi sup. Tab. xxiv. p. 408, 409.

(8) *Philosoph. Transact.* ubi sup. p. 404. & Tab. xxiv.

(9) *Philosoph. Transact.* Vol. LIV. Tab. xi. p. 99. & Tab. xxiv. p 404.

(10) Christ. Cellar. Notit. Orb. Antiqu. Lib. II. c. xii. p. 397, 398.

(11) Fil. Parut. *La Sicil. Num.* in Num. di Segest.

ginians were masters of that city, and occupied all the adjacent territory appertaining to it.

That the Carthaginians were actually possessed, for a certain period, of that part of Sicily where Eryx and Segesta had their situation, does not only appear (12) from antient history, but likewise from a long Punic inscription, found at the former of those places. This inscription has been (13) published by the Prince di Torremuzza, who extracted it from Sig. Antonio Cordici's manuscript history of Eryx, with a copy of which he was supplied by Sig. Dominico Schiavo, in the very valuable and learned work mentioned in the beginning of this paper.

That such rough and uncouth words as SEGHEGT, or SEGEGTH, with vowels scarce sufficient to form, or facilitate, the pronunciation, were not unknown to the Carthaginians, we may infer from the words SBAQTNI, ENGKARA, ESCQVAR, FIEGKV, GHERQ, IGHASESC, and many others that occur in the remains of the ancient Punic tongue, which (14) at present exist in the vernacular language of the Maltese.

From what has been here advanced, it is incontestably clear, that SEGESTE, or SEGESTA, is a word of a Punic origin; which, indeed, has been observed by the famous Bochart. That learned au-

(12) Polyb. Diod. Sic. Liv. Oros. &c. *Univ. Hist.* Vol. vi. p. 829. et alib. Lond. 1742.

(13) *Sicil. et objacent. insular veter. inscript. nov. collect. &c.* class. xx. p. 296, 297. Panormi, 1769.

(14) *Canonico* Gio-Pietro Francesco Agius de Soldanis, in *Dizionar. Punico-Maltes.* pass. In Roma, 1750.

thor has (15) ſufficiently exploded the fabulous account of Aceſta, the pretended founder of Segeſta, given us by ſome of the ancient writers; though, for want of the aſſiſtance of the Punic coin before me, he could not hit upon the true name the city now in view, at leaſt when the medal I am conſidering firſt appeared, went under amongſt the Carthaginians.

As no chronological characters occur on the piece conſidered here, the time when it was ſtruck cannot, with any preciſion, be aſcertained. That operation muſt, however, have preceded the concluſion of the firſt Punic war; ſince the Carthaginians, by the treaty of peace which terminated that war, ceded the (16) whole of their poſſeſſions in the iſland of Sicily to the Romans. Nay, the medal I am endeavouring to throw ſome light upon was probably prior, perhaps many years, to the ſurrender of Segeſta (17) to the Romans, in the beginning of the firſt Punic war, when the inhabitants of Segeſta put the African garriſon there to the ſword, about 258 years before the birth of CHRIST; the Carthaginians ſeeming never to have been poſſeſſed of this ancient city, after that tragical event.

(15) Sam. Bochart. *Chan.* lib. I. c. 27. p. 563, 564. Francofurti ad Mœnum, 1681.

(16) Polyb. Liv. Oroſ. Zonar. &c. *Univ. Hiſt.* Vol. vi. Lond. 1742.

(17) Polyb. Diod. Sic. Liv. Oroſ. &c. *Univ. Hiſt.* Vol. vi. p. 829. Lond. 1742.

II. The

II.

The ſecond of the two abovementioned (See TAB. III. n. 8.) inſcriptions, which is the ſixth of thoſe publiſhed, in the place above refered to, by the Prince di Torremuzza, is compoſed of ſeven letters. Of theſe the firſt and ſecond are undoubtedly *Ain* and *Mem*, as we may certainly infer from a ſimilar inſcription, on the reverſes of other Punic coins, that have been (18) heretofore explained. The third muſt be *He*, as it ſo (19) much reſembles a form of the Greek *Epſilon*; the antient figures of the fiſth element in the Greek, Phœnician, and Samaritan alphabets having (20) been originally the ſame. The ſenſe of the inſcription ſeems likewiſe abſolutely to require this. The fourth letter is apparently *Mem* (21), as will be allowed by every one in the leaſt acquainted with the ancient Siculo-Punic, and Siculo-Phœnician, characters. The fifth is *Hheth*, or *Heth*, as we may collect from (22) other medals, ſimilar to that on

(18) *Philoſoph. Tranſact. Memoir. de Litterat. de l'Acad. des Inſcript. & Bell. Lett. &c.* D. Bern. Aldret. D. Vincen. Juan de Laſtanos. Peller. ubi ſup.

(19) Sig. Haverc. *de Lit. Græc. Diſſert.* p. 248, 249 Vid. *Syllog. Scriptor. qui de ling. Græc. rect. pronuntiat. &c.* Lugduni Batavorum, 1736.

(20) Vid. Hadr. Reland. D. Bern. de Montfauc. Don Luis Joſeph Velazquez, Chiſh. alioſque Scriptor. quam plurim.

(21) *Philoſoph. Tranſact. Memoir. de Litterat. de l'Acad. des Inſcript. & Bell. Lettr. &c.* Aldret. Laſtanoſ. Peller. &c. ubi ſup.

(22) *Philoſoph. Tranſact.* Vol. LIV. Tab. xxiv. p. 408, 409. & Tab. xi. n. 1. p. 99, 404.

which

which this legend has been preserved. Part of this element, however, has been defaced by the injuries of time; which to demonstration appears (23), not only from coins already published, but likewise from others preserved in the cabinets of the great and the curious, to which easy access may be had. The sixth must be *Nun*, as we may conclude from all the abovementioned (24) coins. The draught of it, however, given us by the Prince di Torremuzza, seems somewhat to resemble one of the forms of *Mem*, and therefore it was probably not taken with the utmost accuracy; the Prince, perhaps, not being so thoroughly conversant with the various figures of the Siculo-Punic, and Siculo-Phœnician, letters, and learned men but little acquainted with those figures pretty frequently mistaking one similar letter for another. This character likewise is apparently different from the form of *Mem*, in the same inscription, and seems not a little to resemble the usual form of *Nun*; as will appear to every one, examining it with proper attention. The seventh is *Thau*, as will be admitted, I believe, by every one versed in this branch (25) of literature. The inscription, therefore, of which I am at present attempting an interpretation, is formed of the two words עמ המחנת, AM HAMMAHANOTH, HAMMEHNOTH, or HAMMENOTH, POPVLVS MENENIVS, or MENA-

(23) *Philosoph. Transact.* Vol. LIV. p. 99, 404, & Tab. xi. n. 1. p. 99. *Memoir. de Litterat. &c.* ubi sup. Pl. II. n. 7, 8, 9, 10, 12. p. 417. Aldret. Lastanos. Peller. &c. ubi sup.

(24) *Philos. Trans. Mem. de Litterat.* &c. Aldret. Lastanos. Peller. ibid.

(25) Barthel. Peller. &c.

RVM POPVLVS, as we may find rendered incontestable by other (26) similar coins.

In Hebrew the prefix ה is not seldom added to the (27) beginning of the proper names of provinces, cities, and towns. So וירגלו את־העי, AND VIEWED AI. Jehos. vii. 2. מן השטים עד הגלגל, FROM SHITTIM TO GILGAL. Mich. vi. 5. וכל־ערי הגלעד, AND ALL THE CITIES OF GILEAD. Jehos. xiii. 25. To which I could add many more instances of the same mode of expression, that might, with equal facility, be produced. As the Punic and Phœnician languages therefore (28) agreed in most points with the Hebrew, we may naturally suppose the Phœnician, or Carthaginian, inhabitants of Menæ to have impressed the words עם המחנת, AM HAMMENOTH, upon their most ancient coins.

The medal, which has conveyed down to us this inscription, through such a series of ages, is of the tetradrachmal form, and of a very considerable antiquity. It has a place assigned it in the (29) very valuable cabinet of the Prince di Torremuzza, though he has not favoured us with a draught of it. On one side it exhibits the head of a woman, goddess, or tutelary deity of the place where it was struck, with three fishes sporting round it; and on

(26) *Phil. Transf. Mem. de Litterat. &c.* Aldret. Lastanos. Peller. ubi sup.

(27) Johan. Buxtorf. *Thesaur. Grammat. Ling. Sanct. Hebr.* p. 385. Basileæ, 1651.

(28) Sam. Bochart. *Chan.* lib. II. c. 1. *Philosoph. Transact.* Vol. LIII. p. 292. & Vol. LIV. p. 134.

(29) *Sicil. et objacent. insular. veter. inscript. nov. collect. &c.* class. xx. p. 293. Panormi, 1769.

the

the reverſe a horſe's head, under which appears the inſcription, that is one of the principal objects of my attention, in this paper. It will be almoſt needleſs to remark, that the horſe's head is one of the moſt uſual ſymbols on the reverſes of the antient Carthaginian coins.

With regard to the third character here, taken by me for *He*, I would beg leave to remark, that it is the creſcent, or lunated form of the Greek *Epſilon*; which was a figure of that element of a pretty high antiquity, though not the (30) firſt uſed by the Greeks. That it was as early in Sicily as Julius Cæſar's days, has been proved from (31) the coins of Entella, coeval with that emperor. And that it was known in Italy many years, perhaps ſeveral generations, before (32) the finiſhing ſtroke was given to the Roman republic, is clearly evinced by a moſt curious minute Greek ſepulchral inſcription, publiſhed by P. D. Gianfranceſco Baldini, of which a particular account will be found in the very valuable work referred to here. Nay, in Greece it ſeems to have a long time preceded this monument (33), as may be inferred from two minute inſcriptions preſerved on two antient Greek ſtatues of Speuſippus and Xenocrates, mentioned by a very learned modern author. It may therefore, with ſufficient

(30) Sig. Haverc. *De Lit. Græc. Diſſert.* p. 248, 249. in *Sylloge Scriptor. qui de ling. Græc. ver. et rect. pronun.* &c. Lugdun. Batavorum, 1736.

(31) Fil. Parut. *La Sicil. num.* Tab. cxiv. n. 2, 3. Sig. Hav. ubi ſup.

(32) *Sag. di Diſſertaz. Accademich. di Cortòn.* Tom. II. p. 157. In Roma, 1738.

(33) Sig. Haverc. ubi ſup. p. 248.

pro-

propriety, be ſuppoſed as ancient as at leaſt the later ages of the Carthaginian empire in Sicily, if not much older. That the moſt ancient form of the Greek *Epſilon* was alſo ſometimes impreſſed upon the Punic medals of Menæ, is rendered inconteſtable by a moſt valuable tetradrachm of that city, publiſhed (34) by M. Pellerin. The firſt letter of the legend, on the reverſe of that tetradrachm, is the Punic *Ain*, not very accurately taken; and the third is undoubtedly the oldeſt Phœnician, Samaritan, and Greek figure of *He* or *Epſilon*, brought by Cadmus out of Phœnicia, and repreſenting, according (35) to Euri-

(34) Peller. *Recueil de Medaill. &c.* Tom. Troiſ. p. 22. pl. 88. n. 8. A Paris, 1763. The city of Menæ, the Μηναὶ of Ptolemy, was built by Deucetius, king of the Siculi, but ſubject to the Carthaginians, from the days of Dionyſius the elder, king of Syracuſe, to the time of Timoleon, the Corinthian, according to Diodorus Siculus; in ſome part of which interval, the piece I have been conſidering, as well as all others ſimilar to it, was probably ſtruck. From Menæ's being a town of the Siculi, and inhabited by them and the Greeks, M. Barthelemy infers, that it never was ſubject to the Carthaginians, and that therefore the piece in queſtion could not poſſibly have made its firſt appearance there. But the former of theſe aſſertions is expreſsly contradicted by Diodorus Siculus, and therefore the latter of them muſt neceſſarily fall to the ground. Of this the learned antiquary above-mentioned ſeems to have been ſufficiently aware, when he declares, that he does not give us for demonſtration what he has advanced on this head. I muſt beg leave here farther to remark, that the word מחנת cannot well be tranſlated Caſtra here, as the proper names of cities are generally, if not always, pointed out to us, by the legends on the reverſes of ſuch coins. Diod. Sic. lib. xi. c. 78. Vid. etiam lib. xii. xiii. xiv. See the *Univ. Hiſt.* Vol. VII. p. 535 Lond. 1747.

(35) Euripid. et Agath. Tragic. apud Athen. *Deipnoſoph.* lib. x. c. 20.

pides,

pides, and Agathon, in Athenæus, an obliquated trident. That inſcription is formed of the very ſame letters with thoſe that conſtitute the legend I am conſidering, and conſequently will admit of the ſame interpretation. Nor can this be matter of ſurprize to thoſe who conſider, not only that the firſt figure of the Greek *Epſilon* was (36) borrowed from the earlier Phœnicians, but likewiſe that the later form of that element, which was alſo ſufficiently ancient, as I have here inconteſtably proved, might likewiſe have been deduced from a later figure of the Phœnician, or Punic, *He*, in a country chiefly occupied by the Greeks (37), Phœnicians, and Carthaginians, for a very conſiderable period. And that this was really the caſe, from the legend on the reverſe of the coin of which I am now attempting an interpretation, ſeems abundantly clear. The Prince di Torremuzza has likewiſe (38) rendered the antiquity of this form of the Phœnician, or Punic, *He* inconteſtable. All which conſiderations being maturely weighed, and due attention given to the medals here deſcribed; the power of the third element of the legend, or inſcription, before me, will appear, I would flatter myſelf, to be ſufficiently aſcertained.

(36) Edm. Chiſhul. *Inſcript. Sig.* Sig. Haverc. ubi ſup.

(37) Herodot. Thucyd. Polyb. Diod. Sic. Liv. Strab. Oroſ. Zonar. &c.

(38) *Prolegom.* p. 39, 40. From the Siculo-Punic medals mentioned in this paper, as well as many others, it ſeems clearly to appear, that the prefix ה was never annexed to the word עם, AM, POPVLVS, on the Siculo-Punic coins, as M. Barthelemy has been pleaſed to aſſert, but to the proper name of the place immediately following it. This, if allowed, muſt be deciſive in favour of what I have formerly advanced, relative to the power of the character taken by that learned antiquary for *He*, but by me for *Mem. Philoſoph. Tranſact.* Vol. LIV. p. 397.

If

If what has been juſt advanced ſhould be admitted by the learned, they will readily allow the oldeſt forms of *He* on the antient Siculo-Punic coins, to have greatly reſembled, or rather to have been almoſt perfectly the ſame with, thoſe of that letter exhibited by the earlieſt Phœnician, Samaritan, Greek, and Etruſcan, coins. Nor can any thing be more conſonant to the faith of hiſtory than ſuch a notion. We cannot therefore ſuppoſe *He* to have reſembled any of the forms of *Mem*, or rather to have been repreſented by one of thoſe forms, as M. Barthelemy (39), without any juſt grounds for his opinion, has actually ſuppoſed, as nothing ſeems more remote from truth than ſuch a ſuppoſition. Antient hiſtory, antient coins, and the reaſon of the thing itſelf, notwithſtanding his exalted merit, and the great figure he ſo juſtly makes in the republic of letters, decide the point in queſtion moſt clearly and evidently againſt him.

Nor will it avail him to (40) aſſert, that the Greek coins of Menæ differ in ſeveral reſpects from thoſe conſidered by me in this paper; and that the (41) workmanſhip of the latter is better than that of the pieces which are the acknowledged productions of that city. For that the workmanſhip of ſeveral of the Punic and Phœnician coins is highly finiſhed and elegant, and that the taſte and genius of thoſe coins differ conſiderably from the manner of thoſe ſtruck by the Greeks and the Romans, will not admit of a doubt.

(39) Barthel. in *Memoir. de Litterat. &c. de l'Academ. des Inſcript. & Bell. Lettr. &c.* Tom. XXX. p. 409, 410, 411, 417.

(40) Barthel. *Lettre a Monſ. le Marquis Olivieri, &c.* p. 28, 29. A Paris, 1766.

(41) Id. ibid. p. 28.

The

The former have not their reverſes diverſified by ſuch a variety of ſymbols as have the latter. The Carthaginians, in particular, ſeem to have adorned their medals with very few ſymbols, or types; and thoſe ſuch as were, for the moſt part, common to all the cities and towns ſubject to their republic. As the legend therefore, on the reverſe of the medal I have been endeavouring to explain, is clear and expreſs in favour of my explication, and plainly points out to us the place where it was ſtruck; the abovementioned (42) objections, hinted at by M. l'Abbé, after what has been juſt obſerved, will fall to the ground of courſe, and not be allowed, by the beſt and moſt competent judges of the point in queſtion, to have the leaſt tendency to invalidate what has been advanced in favour of my opinion.

Several curious particulars, not hitherto touched upon, are deducible from the coin, or rather my explication of the coin, I have been conſidering. But as I have already exceeded the limits propoſed to myſelf, when I began this paper, it is time to conclude; which I ſhall beg leave to do, with aſſuring you, that I am,

Good Sir,

Your much obliged,

and moſt obedient, humble ſervant,

Chriſt-Church, Oxon.
October 2, 1770.

John Swinton.

(42) *Lettre à Monſ. le Marquis Olivieri, &c.* p. 27, 28, 29.

XIII. *Extract*

XIII. *Extract of two Letters from* M. Meſſier, *of the Royal Academy of Sciences, and F. R. S. to* M. de Magalhaens, *on a new Comet: Tranſlated by Dr.* Bevis, *F. R. S.*

Read Jan. 24, and Feb. 1, 1771.

PLEASE, Sir, to inform the Royal Society, that I diſcovered a new Comet, the 10th of Jan. inſtant, 1771, about eight o'clock in the evening; it was between the head of Hydra and the Little Dog, over the parallel of Procyon. The poſition whereof I determined by comparing it with that ſtar, and the ſtar δ in Hydra. The obſervations are as follow:

		h	m	s	
1ſt Obſ.	The 10th of Jan. 1771, at	10	16	45	true time.
	Right Aſcenſ. of the Comet	121°	47	16	
	North Declination	5	21	15	
2d Obſ.	Same night at	21	19	5	
	Right Aſcenſ. of the Comet	120	24	31	
	North Declination	6	4	46	

From which obſervations it appears, that in $3^h\ 2'\ 20''$ of time its motion in right aſcenſ. was $1°\ 22'\ 45''$ and $43'\ 31''$ in declination: this comet was perceived by the bare eye. In the teleſcope its nucleus is bright, of a whitiſh complexion, and not very well defined, ſurrounded with an atmoſphere ſeveral minutes wide, with a faint tail 5 or 6 degrees long. Its apparent motion among the fixt ſtars contrary

trary to the order of ſigns, from the equator towards the North pole.

This makes the twelfth Comet I have diſcovered and obſerved in thirteen years paſt.

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WITHOUT what I had done the 10th of January for determining the poſition, and forming any tolerable conjecture of the direction of the motion of the comet, it would have been impoſſible for me to have found it afterwards; for from the 10th to the 16th the ſky was every night quite clouded, during which the light of the comet had extremely abated, and its motion decreaſed, inſomuch that between the 16th and 17th of January the ſky clearing, I ſought for it two hours, without finding it; but though with little hopes of ſeeing it, I ſought for it in Perſeus and Andromeda. After a world of uſeleſs pains and look-outs, at length I ſaw it again, between the horns of the Bull, of a very feeble light: and it was neceſſary to be acquainted with the heavens as well as I was, to find where it was; nor have any of our aſtronomers, as far as I can yet learn, ſucceeded in their attempts. I obſerved it January 10th, 16th, 17th and 20th, on which laſt day it was extremely cloſe to the planet Mars, leſs than a minute of time, both in right aſcenſion and declination, and its light ſo languid as to be in a manner extinguiſhed by that of Mars. Without that excellent teleſcope which you procured for M. le Preſident de Saron, I ſhould not have perceived it with any of my inſtruments. Here follow my obſervations, which may poſſibly turn out the only good ones, and which I intreat you to lay before the Royal Society.

1771	True Time. h	′	″	Rt. Aſcenſiou °	′	″	No. Declin. °	′	″	
Jna. 10	8	44	24	22	27	46	5	4	37	Eſtimate poſition to δ Hydræ.
	10	16	45	121	47	16	5	21	15	Determined poſition by a ✳ of the 7th mag. then unknown, as alſo by δ Hydræ.
	10	46	3	121	35	31	5	36	51	By δ Hydræ.
	12	48	58	120	49	10	6	1	47	By the ſame ✳ of Hydræ, repeated.
	13	19	5	120	24	31	6	14	46	By Procyon.
16	7	16	4	84	3	17	22	39	21	By ζ the South horn of the Bull.
	7	41	39	84	1	32	22	40	51	By the ſame.
17	6	27	35	80	41	58	23	45	25	By Flamſteed's 121 of Taurus and the 163 of La Caille.
	6	40	34	80	40	28				
	7	2	37	80	37	13	23	46	51	By the ſame.
	7	21	23	80	35	28				By the ſame ✳.
20	8	18	30	72	52	2	25	55	25	By a ✳ of the 8th mag. obſerved on the merid.
	8	18	30	72	52	25	25	55	28	By the planet Mars.
	8	56	49	72	47	47	25	56	47	By the above ✳ of the 8th mag.
	8	56	49	72	48	17	25	56	46	By the planet Mars.
	9	19	24	72	47	17	25	57	3	By the above ✳ of the 8th mag.
	9	19	24	72	47	32	25	57	4	By Mars.

Such are my obſervations, from whence M. Pingré has deduced the following Elements of its orbit.

	s	°	′	″
Aſcending ☊	3.	18	42	10
Indication of the orbit		31	25	55
Place of the perihelion	8.	28	22	44
Log. of the perihel. diſt.	9,722833			

Paſſed the perihel. the 22d of November, 1770, at $22^h\ 5'\ 48''$ M.T. at the royal obſervatory, motiou retrograde. He adds, " that this " comet reſembles none of thoſe whoſe elements are determined, " on comparing its motion with the places of its Perihel and ☊ : it " is eaſy to ſee, that it was impoſſible to diſcover it at Paris before " the year 1771 ; and it may even be added, that it muſt frequently " have paſſed in the Sun's neighbourhood, imperceptible to the " Northern parts of the Earth."

XIV. *De-*

The Equatorial Telescope

E. Nairne inv. et fecit

XIV. *Description and Use of a new constructed Equatorial Telescope or portable Observatory, made by Mr.* Edward Nairne, London.

Read Feb. 7, 1771.

THE Inſtrument conſiſts of the following parts (ſee the annexed Plate, TAB. IV.) a mahogany triangular Stand A A A, and three adjuſting ſcrews B B B; a moveable azimuth Circle C, which is divided into degrees, and by a vernier index to every 6 minutes; above this azimuth Circle is the horizontal plate D, to the under part of which is faſtened the vertical conical axis E; on the middle of the upper ſurface of the horizontal plate, is placed a ground glaſs Level F, by which the plate D is ſet parallel, and the pillar E perpendicular to the horizon; from this plate riſe perpendicularly two quadrants G G, one of which is divided for the latitude into half degrees, and has a vernier index to 3 minutes; the equatorial plate H, with its hour circle, is ſupported by the two quadrants G G; its axis of motion (which is placed near the hours XII. XII.) paſſes through the centers of the quadrants, and carries the index I, pointing to the divided quadrant; the equatorial plate is divided into half degrees, and has a vernier index ſhewing every 3 minutes of right aſcenſion or 12 ſeconds of time; it is figured to ſhew

both

both degrees and time; to prevent misapprehension, it may be right to remark that the hours XII. XII. ought properly to have been placed according to the meridian line; they are here placed otherwise, for the convenience of better seeing the meridian distance shewn by the vernier; On the upper part of the equatorial plate is the plate K; upon this plate K, are fixed the two supporters M M, which support the axis N, under which is fastened the semicircle of declination O, divided into half degrees, and has a vernier index subdividing it to 3 minutes; on the upper part of this axis, is fixed an achromatic Telescope P, which magnifies about 50 times; to the eye End of this Telescope, is applied a small reflecting speculum making an angle of 45° with the axis of the telescope, whereby objects that are in the zenith or any other altitude may be observed, without putting the body in any inconvenient position; to the under part of the axis N, is fastened a brass arm carrying the weight Q, which counterbalances the telescope, and the brass work annexed to it; whilst the weights R R counterbalance in like manner the whole of the instrument that is moveable on the equatorial axis, so that whatever position the instrument is put in, it will there remain, being perfectly balanced; the four motions of this instrument may, when required, be moved extreamly slow, by means of the indented edges of the circle and semicircles, and the screws or worms to which the handles are fixed, *viz.* that for the horizontal motion marked S, called the horizontal handle, that marked T the handle of latitude, V the equatorial handle, and W the declination handle.

To adjuſt the inſtrument for obſervation, the firſt thing to be done is to make the horizontal plate D level, by means of the ſpirit level, and the three adjuſting ſcrews at the bottom of the ſtand; this being done, move the equatorial plate either with or without the latitude handle, until the index on the quadrant points to the latitude of the place; and then the equatorial plate will be raiſed, to the elevation of the equator of the place, which is equal to the complement of the latitude (and which, if not known, may likewiſe be found by this inſtrument, as will appear hereafter); and thus the inſtrument is ready for obſervation. The manner of uſing this inſtrument for the following obſervations, I ſhall borrow in part from the words of the late ingenious Mr. Short, in his deſcription of his equatorial teleſcope*, which, however, differs eſſentially in conſtruction from this.

To find the Hour of the Day, and the Meridian of the Place.

Firſt, find from aſtronomical Tables, the Sun's declination for the day; and for that particular time of the day; then ſet the declination ſemicircle to the declination of the Sun, taking particular notice whether it is North, or South; and ſet the declination ſemicircle accordingly, you then turn about both the horizontal handle and equatorial handle, until you find the Sun preciſely concentrical with the field of the teleſcope; if you have a clock or watch at hand, mark that inſtant of time; and by looking upon the equatorial plate and vernier index, you will find

* Vide Phil. Tranſ. Vol. XL. p. 243.

the

the exact apparent time of the day, which, comparing with the time ſhewn by the clock or watch, ſhews how much either of them differ from apparent time; in this manner you find the hour of the day.

To find the Meridian of the Place.

[The inſtrument remaining as in the laſt obſervation.]

You firſt move the plate K until the vernier on it cuts the 12 o'clock hour, and, diſcharging the ſcrew to which the declination handle is fixed, turn the teleſcope down to the horizon, and obſerve the point which is then in the middle of the field of the teleſcope, or cut by the interſection of the croſs wires, and a ſuppoſed line drawn from the center of this field, to that point in the horizon, is your meridian line, where a mark may be ſet up in order to preſerve it; you may likewiſe preſerve this line, by the azimuth circle, which being made moveable, ſhould be turned ſo as to bring the 0 of the azimuth plate to agree with its vernier, when the teleſcope is pointed to the meridian; this motion in the azimuth plate will be found very convenient, ſince you may thus recover the meridian line by it, and it will ſhew the exact azimuth of any object the teleſcope is directed to, without diſturbing any other part of the inſtrument: the beſt time of the day for making this obſervation for finding your meridian, is about three hours before noon, or as much after noon; the meridian of the place may be found by this method very nearly, and, if proper allowance be made for refraction,

fraction, it may be found to great exactness; this line once settled will save trouble afterwards, and is indeed the foundation of all astronomical observations.

To find a known Star or Planet at any proposed instant of time, whether in the day or night.

The instrument remaining rectified as in the last observation; set the declination semicircle to the declination of the planet, at the proposed instant, and bring the index of the equatorial plate, to point to the meridian distance of the star or planet, at the proposed instant if westward, or to the complement of the meridian distance if eastward of the meridian. (This distance is found by adding together the right ascension of the Sun in time, and the apparent time of the day, and taking the difference between the sum and the star's right ascension in time; when the star's right ascension in time is greater than the above sum, the meridian distance will be East; when the star's right ascension is less than the sum, the meridian distance is West.) Having thus set the Instrument, look through the telescope, you will see the star or planet; and if it should afterwards get out of the field, you will easily recover it, by moving the equatorial handle only, provided the star is above the horizon, because the diurnal motion of a star is parallel to the equator.

By this instrument most of the stars of the first and second magnitude may be seen even at midday, and the Sun shining bright, as also Mercury, Venus, and Jupiter; Saturn and Mars are not so easy to be seen, in the day time, upon account of the faintness of their

heir light, except when the Sun is but a few degrees above the horizon; in the ſame manner in the night time, when you can ſee a ſtar or planet, or any new phænomenon, ſuch as a comet, you may find its declination and meridian diſtance or complement thereof, by turning about the equatorial handle and declination handle, untill you ſee the ſtar, planet, or new phænomenon; and looking upon the equatorial plate, you find its meridian diſtance or complement thereto, and upon the declination ſemicircle its declination. In order to have the other uſes of this inſtrument, you muſt ſet it to the hour XII on the equatorial plate, and to 0 on the ſemicircle of declination, and ſet the axis E perpendicular to the horizon, and then this inſtrument becomes an Equal Altitude Inſtrument, a Tranſit Inſtrument, a Theodolite, a Quadrant, an Azimuth Inſtrument, and a Level; the manner of applying it to theſe different purpoſes is obvious.

The following is one Example of its Uſes in finding the Altitude of any Object.

Set 0 of the ſemicircle of declination, to agree with 0 of its vernier index, and faſten it there; fix likewiſe the vernier of the equatorial plate to 12 o'clock, or 0 degrees; then, having ſet the axis E perpendicular, by means of the level F, turn the inſtrument about horizontally upon the axis E, and vertically upon the axis of motion of the equatorial plate, until the object appears in the middle of the field; and the index I will point out upon the quadrant G, G, the zenith diſtance of the object, the complement of which to 90° is its altitude; hence the greateſt or meridian altitude may be found, from which the latitude of the place may be deduced in the uſual manner.

XV. *Experiments to ſhew the Nature of* Aurum Moſaicum: *By Mr.* Peter Woulfe, *F. R. S.*

Read, Feb. 14, 1771.

THE Aurum moſaicum is known by the names of Aurum muſivum, Aurum muſicum, and Purpurina. We can collect but very imperfect ideas of this preparation, from the writings of antient and modern chemiſts. It is true, that the method of making it has been deſcribed by many; but no one hitherto has led us into the knowledge of its nature, by a ſufficient number of experiments. It has been much uſed formerly as a pigment, but is now almoſt laid aſide, and the Bronzes ſubſtitued in its place. It is ſometimes uſed in medicine as a vermifuge, but how improper and uncertain it is for that purpoſe, will appear by the following experiments.

The beſt preparation deſcribed hitherto for making Aurum moſaicum, is ſet down in the London Diſpenſatory, and is as follows.

Take of tin twelve ounces, of flowers of ſulphur ſeven ounces, of ſal ammoniac ſix ounces, and of purified mercury ſix ounces; melt the tin, and add the mercury to it; and when cold, powder it, and mix with it the ſal ammoniac and ſulphur. Sublime the mixture in a matraſs. The Aurum moſaicum will

will be found under the sublimate, with some dross at the bottom *.

Ætiology of the Operation.

As soon as the mixture grows warm, the tin acts on the sal ammoniac, and sets free its volatile alkali; and this, having a great affinity with sulphur, joins with a great part of it †, rises in the sublimation, and is totally dissipated. The portion of tin, which acted on the Sal ammoniac and set free its volatile alcali, unites with the acid of salt of the sal ammoniac, and forms a salt of tin, which sublimes ‡. The mercury, which was added only in order to divide the tin, unites with some of the sulphur, and likewise sublimes and forms a cinnabar. The tin which remains, unites with the remaining sulphur, and forms the Aurum mosaicum, which is found in the bottom of the matrass. Instead of performing this operation in a matrass, I have used a glass retort, fixed in a black lead crucible, with sand round it; the crucible was put into a proper furnace, and a charcoal

* The proportion formely used was equal parts of each, which in the quantity of tin here employed, viz. ℥ 12, produced only ℥ 13½ of Aurum mosaicum; whereas the same quantity of tin in the proportion of the London dispensatory afforded ℥ 16.

Troy weight was made use of in all the following experiments.

† Sulphur combined with volatile alcali forms a volatile liver of sulphur, called by Mr. Boyle volatile tincture of sulphur and quick lime.

‡ Filings of tin, calx of tin, or tin divided by amalgamation with mercury, distilled with sal ammoniac, &c. decomposes it, whereby its acid of salt unites with the tin, and its volatile alkali is set free.

fire made round it, an adopter was luted to the retort, and to the adopter a quilled receiver; a long vial was fitted to the quill of the receiver, in order to hold the liquor that diſtilled.

It will be eaſy by this apparatus to perform the operation without any conſiderable loſs, provided the fire be well regulated. It is neceſſary to make a very ſlow fire at firſt, in order to condenſe the volatile fumes; for a great quantity of air is ſet free, when the ingredients begin to act on one another. The fire ſhould be rather ſlow for the firſt four or five hours, and then gradually encreaſed, until the crucible becomes moderately red hot; in which ſtate it is to be continued during the reſt of the operation, which commonly laſts about ſixteen hours from the beginning to the end.

If this operation be performed for the ſake of the Aurum moſaicum only, or for its ſublimate, it may as well be done in a matraſs or body; and in that caſe there is no great loſs of the ſublimate.

Twelve ounces of tin with ſulphur, ſal ammoniac and mercury in the proportion of the London diſpenſatory, prepared in this manner produced,

	℥	ʒ	℈
* Of volatile liver of ſulphur, liquid and dry	1	4	2
Of ſublimate found in the retort and adopter	13	2	0
Of Aurum moſaicum	16	0	0
Loſs in the operation	0	1	1
Weight of all the ingredients	℥ 31	0	0

* The volatile liver, for the moſt part, comes over liquid, and is often found in the long vial in form of moſt beautiful ramifications, which are a cryſtallization of the volatile liver.

The

This operation was often repeated, and always with ſome little variation in the products, owing to the management of the fire, which cannot be always made alike.

If this preparation be made with too great a degree of fire, the Aurum moſaicum will be partly melted, or of a dark colour; and if the fire be not continued for a ſufficient time, a portion of the cinnabar and ſalt of tin will remain with the Aurum moſaicum.

The Sublimate of Aurum moſaicum examined.

The ſublimate, which was obtained in preparing the Aurum moſaicum weighing ℥ 13 ʒ 2 was finely powdered and digeſted with diſtilled water ſharpened with ſome acid of ſalt *, and when cold was filtered; more water was added to what remained in the filter, by which means it was deprived of its ſoluble part.

† The undiſſolved part of the ſublimate dried, and then ſublimed in a retort, produced ℥ 9½ of cinnabar, which was of a dark colour, owing to an exceſs of ſulphur; there came over into the receiver a ſmall quantity of an acid liquor, and there was found in the retort ℥ ½ of Aurum moſaicum, which added to the former quantity makes ℥ 16½.

The ſoluble part of the ſublimate is compoſed of tin united to the acid of ſalt; in order to know the quantity of tin which it contains, a ſufficient quantity of fixed alcali diſſolved in water was added

* Salt of tin mixed with water becomes turbid, and a portion of the tin precipitates; therefore the acid of ſalt was added, to prevent the precipitation.

† This contains ſome running mercury; but in the ſublimation it diſappears, and forms cinnabar.

to it, by which means the tin was precipitated; this precipitate weighed ℥ 2 ʒ 7.

An ounce of tin diſſolved in the acid of ſalt *, being precipitated with a ſolution of fixed alcali in water, well waſhed and dried † weighed ℥ 1$\frac{1}{4}$; ſo that a precipitate of tin contains only $\frac{4}{5}$ of tin, and therefore the ℥ 2 ʒ 7 of precipitate obtained from the ſublimate contain only ℥ 2 ʒ 2$\frac{1}{2}$ nearly of tin; this being deducted from ℥ 12, the quantity of tin uſed in the operation, makes ℥ 9 ʒ 5$\frac{1}{2}$, which is the real quantity of tin contained in the ℥ 16$\frac{1}{2}$ of Aurum moſaicum obtained in this proceſs; therefore ℥ 1 $\frac{7}{10}$ of Aurum moſaicum contains ℥ 1 of tin, and ℥ $\frac{7}{10}$ of ſulphur; for ℥ 9 ʒ 5$\frac{1}{2}$ of tin is to ℥ 16$\frac{1}{2}$ of Aurum moſaicum, as ℥ 1 of tin is to ℥ 1 $\frac{7}{10}$ nearly of Aurum moſaicum. This will be further illuſtrated by other experiments.

The tin, which was precipitated by adding a fixed alkali to the ſoluble part of the ſublimate, was diſtilled with iron filings and fixed alkali; but no mercury was obtained. This ſhews that none of the mercury unites with the acid of the ſal ammoniac.

There was no volatile alkaline ſmell produced by the addition of fixed alkali to the ſoluble part of the ſublimate, though there were an exceſs of it added; which proves that the ſal ammoniac was totally decompoſed.

* The vapour, which ariſes in diſſolving tin in the acid of ſalt, becomes inflammable, when the ſolution is made in large quantity, by means of heat; the like happens alſo with regard to lead.

† This precipitate, if dried with too much heat, takes fire, and burns like a dried plant that contains nitre.

The ſoluble part of the ſublimate of Aurum moſaicum produces cryſtals of an irregular form, which do not deliqueſce in the air like all other ſalts of tin, owing chiefly to their having a leſs portion of acid. A drop of the ſolution of this ſublimate, cryſtalliſed on a piece of glaſs, and viewed with a microſcope, has very much the appearance of the cryſtals of alum.

Aurum moſaicum, when well prepared, is of a ſhining golden colour, has no taſte, and is not ſoluble in water. It is not acted on by acids, nor by fixed or volatile alkalis diſſolved in water. If melted with an equal quantity of fixed alkaline ſalt of tartar, it forms a liver of the colour of gumbouge, which is, for the moſt part, ſoluble in water, and may be precipitated by any acid.

If Aurum moſaicum be diſtilled with iron filings, no mercury will be obtained.

It is well known, that tin deflagrates violently with nitre; therefore it will not ſeem ſurpriſing, that the Aurum moſaicum ſhould have that property in a far greater degree, it being a compoſition of tin and ſulphur *.

Sulphur, combined with metallick ſubſtances, renders them inactive, as we ſee in cinnabar, antimony, &c.; therefore Aurum moſaicum well prepared muſt be a very unfit medicine for worms.

Aurum moſaicum is often found to have a very rough taſte; but that is owing to the ſalt of tin, which has not been ſufficiently diſſipated in the ſublimation, and, in that ſtate it may effectually deſtroy

* May not this ſubſtance be uſeful in fire-works, as alſo ſulphurated iron?

worms; but it muſt be allowed to be a very uncertain medicine, and perhaps dangerous, as it may contain too ſmall or too great a quantity of the ſalt of tin.

If ſalt of tin be good for worms, it would be right to aſcertain its doſe, and give it in a proper vehicle.

Tin combined with Sulphur, by Fuſion.

Four ounces of tin melted and ſaturated with ſulphur weighed ℥ 5, and formed a black ſhining flaky brittle ſubſtance when melted. The tin need not be made quite red hot for this operation; for, when the ſulphur is mixed with it, a deflagration enſues, and the mixture grows red hot. In order to ſaturate the tin with as much ſulphur as poſſible, it muſt be added to it at two or three different times. The tin, notwithſtanding, cannot be, by this means, perfectly ſaturated with ſulphur; for after powdering and ſifting it, there remains in the ſieve a portion of the tin, which will flat under the peſtle, and not powder any more, unleſs melted and combined with more ſulphur. If this operation be done with too great a degree of fire, the increaſe of weight will not be ſo conſiderable, on account of the great fire, which diſſipates ſome of the ſulphur.

Four ounces of Aurum moſaicum melted in a covered crucible loſes ʒ 6 of its weight, and becomes a maſs ſomewhat like melted ſulphurated tin, though it be not ſo ſhining, nor ſo flaky, but rather more of a needle form. If the fuſion be repeated two or three times, ſome of its ſulphur will be each time diſſipated, and have exactly the appearance of ſulphurated tin.

Aurum

Aurum mosaicum melts much more readily than sulphurated tin; and that, because it contains a greater quantity of sulphur.

Sulphurated tin, by calcination, is totally deprived of its sulphur and phlogiston. ℥ 4 of tin saturated with sulphur, and then carefully calcined weighed ℥ 4½, so that a calx of tin prepared in this manner, weighs $\frac{1}{9}$ more than the tin it contains *.

Four ounces of Aurum mosaicum calcined in the same manner, weighed ℥ 3, ʒ 2, ℈ 1, which, being a calx of tin, of the nature of that made with sulphurated tin, contains $\frac{1}{9}$ less of tin, which $\frac{1}{9}$ being deducted, makes ℥ 2, ʒ 7, ℈ 1, gr. 4½, the quantity of tin contained in ℥ 4 of Aurum mosaicum †. An ounce of Aurum mosaicum carefully calcined, and reduced with flux, produced only ℥ ½ and gr. 11 of tin: we may conclude from the foregoing experiments, that ℥ 1 of Aurum mosaicum contains more than this quantity of tin; but it is well known that metallick bodies, which have been much calcined, and especially tin and zinc, always lose in their reduction.

Aurum mosaicum will be of a black colour, if too small a quantity of sulphur be used, and if the fire be too strong and too long continued †.

* All metallick substances, and even zinc, though a good deal of it dissipates in flowers, increase in weight by calcination.

† It is almost impossible to calcine Aurum mosaicum, without some loss; for it is so light and subtil, that it cannot be stirred without dissipating some of it; therefore the quantity of tin which it contains, cannot be exactly ascertained by this means.

Receipts for making Aurum Moſaicum without Mercury.

1ſt. Take ℥ 8 of granulated tin ſifted through a fine ſieve, and mix it well with ℥ 6 of ſulphur, and ℥ 4 of ſal ammoniac; put the mixture into a matraſs or body, and calcine it for ſix or ſeven hours. The Aurum moſaicum hereby obtained is not of ſo bright a colour, as that made in the uſual manner; and that, on account of the tin not being ſufficiently divided in order to unite well with the ſulphur.

2d. Take ℥ 8 of tin reduced to a calx by calcination, and mix it well with ℥ 7 of ſulphur, and ℥ 4 of ſal ammoniac; calcine it as the former. This makes a good coloured Aurum moſaicum, though it be found here and there of an unequal colour, owing to ſome of the tin, which had been too much calcined, and thereby prevented from uniting with the ſulphur.

3d. Take ℥ 8 of tin, and ſaturate it by fuſion with ſulphur, powder and mix it well with ℥ 5 of ſulphur, and ℥ 4 of ſal ammoniac; calcine the mixture as before. This produces a good coloured Aurum moſaicum.

Receipts for preparing Aurum Moſaicum, without Mercury or Sal ammoniac.

1ſt. Take ℥ 8 of tin, ſaturate it by fuſion with ſulphur, and mix it with ℥ 4 more of ſulphur, and calcine as before, but with a leſs degree of fire. This forms rather a dark coloured Aurum moſaicum, owing to the ſulphur which melts, and in great

meaſure

meaſure ſeparates and ſwims on the ſurface of the ſulphurated tin.

2d. Take ℥ 10 of ſulphurated tin, powder and mix it well with ℥ 4 of ſulphur, and ℥ 2 of ſpirit of ſalt; calcine the mixture as the former. This forms an Aurum moſaicum of a tolerable good colour. This mixture ſoon grows warm of it ſelf after being put into the body, and produces a penetrating vinous ſmell, no ways like that which is afforded by diſſolving tin in the acid of ſalt.

In this operation, part of the tin unites with the acid of ſalt, and is at laſt diſſipated in the operation.

3d. Take ℥ 8 of tin, and ſaturate it with ſulphur, powder and mix it with ℥ 5 of ſulphur; put it into a body, and pour on it ℥ 2 of volatile liver of ſulphur obtained in making Aurum moſaicum, and calcine it as fore. This does not make ſo good a coloured Aurum moſaicum as the former, owing to the fuſion of the ſulphur, which in great meaſure ſeparates from the ſulphurated tin. Soon after the volatile liver was poured on the mixture of ſulphurated tin and ſulphur, it grew ſo hot that the body could ſcarcely be held in the hand.

4th. Take ℥ 4 of tin, ſaturate it with ſulphur, powder and mix it well with ℥ 2 of ſulphur, and ℥ 1 of tin diſſolved in the acid of ſalt and cryſtalliſed; calcine it as uſual. This produced ℥ 6½ of very good and quite taſteleſs Aurum moſaicum, ſo that ℥ 4 of tin by its union with ſulphur encreaſed in weight ℥ 2 ½.

This operation was done in a retort, to which there was luted an adopter and receiver, in order to collect the ſalt of tin, which for the moſt part came over congealed.

5th. Take ℥ 10 of ſulphurated tin, powder and mix it well with ℥ 16 of corroſive mercury ſublimate; put it into a retort, to which an adopter and receiver is to be luted, calcine it for ſix hours, at firſt with a middling fire, and for the laſt three hours the retort muſt be red hot.

In this operation, a portion of the tin unites with the acid of the mercury ſublimate; and forms the ſmoaking liquor of Libavius, which diſtills for the moſt part in a liquid form; the mercury contained in the mercury ſublimate unites with a ſmall portion of ſulphur (about $\frac{1}{7}$ of its weight), and ſublimes in form of cinnabar in the top of the retort; the remaining tin, having a ſufficiency of ſulphur, forms the Aurum moſaicum, which is found at the bottom of the retort of a moſt beautiful ſparkling golden colour.

The reaſon for obtaining Aurum moſaicum by this operation is, that the greateſt part of the tin unites with the acid of the mercury ſublimate, and riſes in diſtillation; the remaining tin has thereby a ſufficient quantity of ſulphur to form the Aurum moſaicum.

Some of the foregoing receipts did not well ſucceed at the firſt or ſecond trial; and many other experiments, which did not anſwer, have here been omitted.

From the foregoing experiments, we may certainly conclude, that Aurum moſaicum is a combination of tin and ſulphur; it contains better than $\frac{1}{3}$ of ſulphur. It alſo appears that the only uſe of the mercury,

mercury is to divide the tin; and that the ſal ammoniac ſerves only to prevent the fuſion of the ſulphur.

The following proportion will anſwer better than that of the London Diſpenſatory; for there will be a greater produce of Aurum moſaicum, though a leſs quantity of mercury and ſal ammoniac be uſed.

Tin ℥ 12, ſulphur ℥ 7, ſal ammoniac ℥ 3, and mercury ℥ 3.

This proportion yields ℥ 17½ of Aurum moſaicum, whereas that of the London Diſpenſatory gives only ℥ 16.

The ſoluble part of the ſublimate of Aurum moſaicum anſwers far better for dying than any ſolution of tin; a ſmall quantity of it with cochineal will dye ſilk, and eſpecially cloth, of a fine ſcarlet colour; ſilk may be dyed of a fine crimſon colour, by its means, with the addition of brazil wood, peach wood, or braziletto; but with logwood, ſilk and cloth may be made of a great variety of fine purple colours, which ſeem laſting.

The property, which this ſublimate has of making finer colours than any ſolution of tin, engaged me to make many trials, with other preparations of tin; and I found, that when tin was united to the acid of ſalt, and diſtilled or ſublimed, it would produce finer colours than any ſolution or combination of tin, unſublimed or undiſtilled.

I muſt be excuſed for the preſent, for not telling the reaſon of this; it may be diſcovered by examining well the products, which are obtained by making the liquor fumans of Libavius, in the common manner.

May not iron and copper, united to the acid of ſalt and ſublimed, anſwer better for dying, than other preparations of iron and copper?

Moſt other metallick ſubſtances may be after this manner more intimately combined with a greater portion of ſulphur than by fuſion. Biſmuth is the only one, which produced a golden colour, and that not ſo fine a one as Aurum moſaicum. Iron, copper, lead, and regulus of antimony, produce black combinations; arſenic forms a reddiſh maſs like realgar; zinc does not in this manner, nor in any other way that I know, combine with ſulphur.

An Apparatus for making Aurum Moſaicum in the cheapeſt manner.

A glaſs veſſel cannot be uſed for this operation more than once, becauſe it is neceſſary to break it, to get out the Aurum moſaicum. The following utenſil may be employed a great number of times, and ſave the expence of glaſs.

Take a black lead crucible, N°. 60; bore a round hole in its bottom about three inches diameter; and ſaw off an inch of its upper edge; if it has a lip, get a round piece of burnt clay, of an inch thick or rather more, to fit exactly into this edge; the compoſition, which is uſed for making paving-tiles, anſwers very well for this purpoſe. In order to make uſe of this apparatus, fit the round piece of burnt clay to the inner edge of the crucible, by means of ſome loam ſoftened with glue, and dry it ſlowly; then turn it upſide down, and lay it in a proper furnace on two iron bars. The mixture for the Aurum moſaicum is to be put in through the round

round hole at top, and then covered with an aludel and luted; this ſerves to collect the flowers and the ſublimate which riſes. The fire is to be made under and all round the crucible. 11 ℔. Troy of Aurum moſaicum may be made here at a time; and when the operation is over, the bottom or round piece of burnt clay will eaſily come out with the Aurum moſaicum. A large crucible may be made uſe of, if a larger quantity be required to be made at once. The operation cannot fail of ſucceſs, provided the fire be made of a ſufficient ſtrength, and of an equal degree from the bottom to the top of the crucible, which is eaſily done in a good furnace. The operation is finiſhed in eight hours, unleſs the volatile liver is wanted.

White arſenic, digeſted with a ſolution of tin in the acid of ſalt, becomes ſoon black; it does hereby regain its phlogiſton, and is reduced to the ſtate of regulus of arſenic, and will by this means readily combine with copper, and other metallick ſubſtances; which it would not do, without the help of phlogiſtic ſubſtances. This is the moſt eaſy and ready way of reducing arſenic to its metallick form: the arſenic may be deprived of the ſolution of tin, which adheres to it by waſhing it with water. It is to be dried ſlowly, for otherwiſe it is apt to catch fire.

A Method of dying Wool and Silk, of a yellow colour, with Indigo; and alſo with ſeveral other blue and red colouring Subſtances.

THE Saxon blues have been known for ſome time; and are made by diſſolving indigo in oil of

 vitriol,

vitriol, by which means the indigo becomes of a much more lively colour, and is extended to ſuch a degree, that it will go very far in dying.

A receipt for making the beſt Saxon blue will, I dare ſay, be agreeable to many; I will, therefore, give the following, which produces a very fine colour, and never fails of ſucceſs.

Mix ℥ 1 of the beſt powdered indigo, with ℥ 4 of oil of vitriol in a glaſs body or matraſs: and digeſt it for one hour with the heat of boiling water, ſhaking the mixture at different times; then add ℥ 12 of water to it, and ſtir the whole well, and when grown cold filter it. This produces a very rich deep colour; if a paler blue be required, it may be obtained by the addition of more water. The heat of boiling water is ſufficient for this operation, and can never ſpoil the colour; whereas a ſand heat, which is commonly uſed for this purpoſe, is often found to damage the colour, from its uncertain heat.

Indigo, which has been digeſted with a large quantity of ſpirit of wine, and then dried, will produce a finer colour than the former, if treated in the ſame manner, with oil of vitriol.

No one, that I know of, has heretofore made uſe of the acid of nitre, inſtead of the acid of vitriol; and it is by means of the former that the yellow colour is obtained: it was nevertheleſs natural to uſe it, on account of its known property of making yellow ſpots, when dropped on any coloured cloth.

The acid of ſalt does not diſſolve indigo, and therefore is of no uſe in dying.

Receipt

Receipt for making the yellow dye.

Take ℥ ½ of powdered indigo, and mix it in a high glaſs veſſel, with ℥ 2 of ſtrong ſpirit of nitre, previouſly diluted with ℥ 8 of water; let the mixture ſtand for a week, and then digeſt it in a ſand heat for an hour or more, and add ℥ 4 more of water to it; filter the ſolution, which will be of a fine yellow colour.

Strong ſpirit of nitre is liable to ſet fire to indigo; and it is on that account that it was diluted with water, as well as to hinder its frothing up. ℥ 2½ of ſtrong ſpirit of nitre will ſet fire to ℥ ½ of indigo; but, if it be highly concentrated, a leſs quantity will ſuffice.

If the indigo be digeſted twenty four hours after the ſpirit of nitre is poured on it, it will froth and boil over; but, after ſtanding a week or leſs, it has not that property.

One part of the ſolution of indigo in the acid of nitre, mixed with four or five parts of water, will dye ſilk or cloth of the paleſt yellow colour, or of any ſhade to the deepeſt, and that by letting them boil more or leſs in the colour. The addition of alum is uſeful, as it makes the colour more laſting; according as the ſolution boils away, more water muſt be added.

None of the colour in the operation ſeparates from the water, but what adheres to the ſilk or cloth; of conſequence this colour goes far in dying.

Cochineal, Dutch litmus, orchel, cudbear, and many other colouring ſubſtances treated in this manner, will all dye ſilk and wool of a yellow colour.

The indigo which remains undiſſolved in making Saxon blue, and collected by filtration, if digeſted with ſpirit of nitre, dyes ſilk and wool of all ſhades of brown inclining to a yellow.

Cloth and ſilk may be dyed green with indigo; but they muſt firſt be boiled in the yellow dye, and then in the blue.

XVI. *Account of an extraordinary Steatomatous Tumour, in the Abdomen of a Woman, by* P. Hanly, *M. D. Communicated by* Charles Morton, *M. D. Sec. R. S.*

Received November 8, 1770.

Read Feb. 21, 1771.

MRS. Reily, aged thirty ſix years, pale, tall, fleſhy, and formerly of a healthy conſtitution, was brought to bed of a ſtrong, lively daughter, on the twenty third day of May, 1770, in the pariſh of St. Anne, Dublin.

In the fifth month of her pregnancy, ſhe felt an uncommon lump in her ſtomach (as ſhe expreſſed it), about the ſize of a hen-egg, which did not then give her much pain or uneaſineſs, and ſhe was in hopes that her delivery would carry it off: ſhe had towards the end of her pregnancy frequent reachings, ſometimes puked, and became emaciated; three days after ſhe was brought to bed, ſhe found the lump and reachings had encreaſed; ſhe became very uneaſy, and ſent for me.

Upon examining her abdomen, I felt a conſiderable tumour contiguous to her ſtomach, which afterwards had greatly encreaſed, and was extended obliquely to her right ſide, as low as her navel; it lay immediately under the peritoneum and abdominal muſcles, and in the progreſs of its encreaſe, I could

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plainly feel one large, and other ſmaller protuberances of a firm ſubſtance, in ſome meaſure reſembling the head and ſuperior extremities of a fœtus, It could be eaſily moved from ſide to ſide, without giving her any pain; but it reſiſted, and made her uneaſy, when I attempted to move it downwards: her abdomen appeard plump and full, as if ſhe had not been brought to bed; but the hypochondres were more prominent and diſtended, than the region below her navel. I ordered for her the ſimple bitter infuſion with abſorbent powders, and delayed giving deobſtruent medicines, till ſhe had recovered her ſtrength after lying-in. I alſo deſired her not to ſuckle her infant; but, as her huſband was poor, ſhe did not comply, by which means ſhe quickly became greatly exhauſted and emaciated.

In a fortnight after her delivery, ſhe got up daily, walked about her room, ſometimes went abroad, and continued to ſuckle her child; but the reachings returned at intervals, the tumour increaſed in ſize, its protuberances became larger and more diſtinct, ſhe was often reſtleſs, and in pain at night on lying in bed, had a hectic fever, and daily became weaker and more emaciated, with a ſharp pinched-up noſe, hippocratic countenance, ſmall, quick, weak, thread-like pulſe, loſs of appetite, and night ſweats.

In five weeks after her delivery, the tumour had greatly increaſed in all its dimenſions; and its protuberances, which to the feel ſeemed to reſemble the head, trunk, and extremities of an extra-uterine fœtus, became more palpable and diſtinct, as the abdominal muſcles from their diſtenſion became thinner. I brought ten phyſicians, ſurgeons, and accoucheurs

coucheurs to viſit her; and we were all ſo much deceived as to be of opinion, that the tumour was an extra-uterine fœtus: however, we were deterred from attempting the Cæſarean operation, from a conviction that ſhe was too weak, hectical, and reduced, to encourage any hopes of her recovery, in caſe it had been performed; and therefore we determined to leave the event to nature, eſpecially as we could perceive no motion of any particular parts of the tumour, though it had greatly increaſed, and as it was poſſible that it might be ſome other tumour.

She continued gradually declining; the tumour and ſymptoms increaſing, during May and June; and the twenty-third day of July following, I perceived a ſmall fluctuation of water in her abdomen, and gave her an intimation thereof, which determined her to procure another nurſe for her infant; but the aſcites daily increaſed, and in nine or ten days after, her legs and feet became œdematous, her night-ſweats ſtill continued, though her dropſy augmented, and ſhe languiſhed under the acute pains, more frequent reachings, hectic fever, loſs of ſtrength, want of appetite, and reſtleſs nights, except when ſhe took an opiate, which often proved a great relief and refreſhment to her.

Her poſture in bed now was half ſitting, half lying, which was the only poſition ſhe could bear without great pain and ſhortneſs of breathing.

About ſeven days before her death, ſhe was ſeized with a ſmart lax, which, in a few days, carried off part of the ſwelling in her left leg; ſhe became ſomewhat lighter, and leſs diſtreſſed in her breathing, which made her vainly hope, that her diſorder might

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be carried off in that manner; but the tumour, weakneſs, and other ſymptoms encreaſed, till the ſecond day of September inſtant when ſhe expired.

After her death, on opening her abdomen, in preſence of ſeven gentlmen of the faculty, we found about a gallon of water, and a large ſteatomatous tumour juſt under the peritoneum, near three inches in thickneſs, ſeven inches in length from her ſtomach to the obtuſe angle of her ribs, and in ſome places near five inches in breath from her ſternum to the vertebras of her back, full of prominences of different ſizes. It was of a hard conſiſtence, like tallow in its anterior part, but ſofter poſteriorly, and divided by thin membranes into numerous cells, which were diſtended with hard and ſofter fat; it weighed ſeven pounds, was of an irregular figure, adhered to, and compreſſed, the anterior part of her ſtomach, and was ſo firmly united to the inferior ſurface of the liver, that it could not be ſeparated from it without force. It preſſed and concealed the colon, and extended from the ſtomach by her liver to the right ovarium, and vertebras of her back: the ſmall guts were greatly ſqueezed, and moſtly forced towards the left ſide; and the anterior lobe of her liver was ſo compreſſed between the diaphragm and tumour, that it appeared flattened, ſmaller than uſual, and in a withered, decaying ſtate.

There was nothing præternatural in the matrix, or any of the other bowels; but they were greatly compreſſed, and the tumour, from its membranes and contained fat, ſeemed to be a production and diſtenſion of that part of the omentum, which adheres to the ſtomach, although it reached and adhered

hered to the right ovarium, liver, aorta, and colon, as well as to the ſtomach.

The operator was for ſome time in ſearch of the colon, before he found it, adhering to, and almoſt forming a part of, the poſterior edge of the tumour.

Dublin,
Sept. 6, 1779.

P. Hanly, *M. D.*

Received February 4.

XVII. *A Letter from Dr.* Ducarel, *F. R. S. and F. S. A. to Dr.* William Watson, *M. D. and F. R. S. concerning Chesnut Trees; with two other Letters to Dr.* Ducarel, *on the same Subject.*

SIR,

Read Mar. 8, 1771.

IN a letter addressed to you, on the trees which are supposed to be indigenous in Great Britain, published in the *Philosophical Transactions* *, the Hon. Mr. Daines Barrington has attacked a prevailing notion among the learned; that chesnut trees are the native production of this kingdom. Mr. Barrington argues that they are not; and his reasonings on this, are now to be considered.

In my Anglo-Norman Antiquities, p. 96. I had observed that "many of the old houses (in Normandy) when pulled down, are found to have a "great deal of chesnut timber about them; as there "are not any forests of chesnut trees in Normandy, "the inhabitants have a tradition, that this timber "was brought from England: and there are some circumstances, which, when rightly considered, will

* Vol. LIX. p. 23.

" add ſtrength to this tradition; for many of the old " houſes in England are found to contain a great " deal of this kind of timber: ſeveral of the houſes " in Old Palace Yard, Weſtminſter, and in that neigh- " bourhood, which were taken down in order to " build Parliament and Bridge-ſtreets, appeared to " have been built with cheſnut; and the ſame was " obſerved with regard to the Black Swan Inn, in " Holborn, and many other old buildings lately " pulled down in different parts of England." And to this I had ſubjoined the following account in a note. " Cheſnut timber being at preſent rarely to be " found growing in the woods and foreſts of Eng- " land, many perſons are induced to think that the " ſweet cheſnut was never an indigenous tree of this " iſland: but a little conſideration will plainly evince, " that it always was, and is to this day, a native of " England. It is generally allowed, that all the " ancient houſes in the city of London were built of " this timber. Certainly it did not grow far off; " and moſt probably it came from ſome foreſts near " the town; for Fitz Stephens, in his deſcription of " London, written in the reign of king Henry the " Second, ſpeaks of a large and very noble foreſt, " which grew on the North ſide of it. Rudhall, " near Roſs, in Herefordſhire, an ancient ſeat of the " family of Rudhall, is built with cheſnut, which " probably grew on that eſtate; for although no tree " of the kind is now to be found growing wild in " that part of the country, yet there can be no " doubt, but that formerly cheſnuts trees were the " natural growth of the neighbouring wood lands, " ſince we find that Roger earl of Hereford, founder

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"of the abbey of Flaxley, in Gloucesterſhire, "by his charter, printed in Dugdale's monaſticon, "tom. i. p. 884. gave the monks there, the tythe "of the cheſnuts in the foreſt of Deane, which is "not above ſeven or eight miles from Rudhall. "The words, are *Singulis annis totam decimam caſta-"nearum de Dena.* In the court before the houſe "at Hagley Hall, in Worceſterſhire, the ſeat of "Lord Lyttelton, are two vaſt ſweet cheſnut trees, "which ſeem to be at leaſt two, if not three hun-"dred years old; and Mr. Evelyn, in his Sylva, p. "232. mentions one, of an enormous ſize, at Tortſ-"worth, in Gloucesterſhire, which hath continued "a ſignal boundary to that manor, from King Ste-"phen's time, as it ſtands upon record; and which "tree is ſtill living, and ſurrounded by many young "ones, that have come up from the nuts dropped "by the parent tree. Mr. Evelyn alſo aſſures us, "that he had a barn framed intirely of cheſnut tim-"ber, which had been cut down in its neighbour-"hood. In the foreſt of Kent, adjoining to Suſſex, "there ſtill remains ſeveral large old cheſnut ſtubbs, "which were left by the woodmen as termini, or "boundaries, either of pariſhes, or private property. "Beſides this, there are to this day, in the North "Eaſt part of Kent, ſeveral large woods, conſiſting "principally of cheſnut trees and ſtubs. In the "pariſh of Milton, near Sittingborne, is a manor "called Norwood Caſteney, otherwiſe Cheſteney, "from its ſitnation among cheſnut woods, which "reach to the highway from London to Dover, and "give name to a hill between Newington and Sit-"tingborne, it being called Cheſnut Hill, the cheſ-

"nut

" nut trees growing plentifully on each ſide of it, " and in woods round it for many miles. And " by the particulars for leaſes of crown lands in " Kent, temp. Eliz. Roll III. N° 8. now in the " Augmentation office, it appears that there is, " in the ſame pariſh of Milton, a wood containing " two hundred and ſeventy eight acres and a half, " called Cheſton, otherwiſe Cheſnut wood. To " conclude, my worthy friend, Edward Haſted, eſq; " of Sutton atHone, near Dartford in Kent, F.R.S. " and F.S.A. aſſiures me that one of his tenants at " Newington, a few years ſince grubbed up forty " acres of wood, which were intirely cheſnut."

In the very out-ſet of the argument, Mr. Barrington impoſes upon himſelf, by changing the terms of the queſtion. " Since you ſent me, ſays he to Dr. " Watſon, the ſpecimen of ſuppoſed cheſnut, which " was taken from the old hall of Clifford's Inn, I " have been at ſome pains to examine the authority " for the prevailing notion, with regard to this be- " ing an indigenous tree" (p. 23.)—but in p. 24. he ſays, " I ſhall begin by conſidering the proofs, which " are commonly relied upon to the *Spaniſh* or *ſweet* " cheſnut being indigenous in Great Britain."— though not one word has preceded, though not one word follows, of the Spaniſh and the common cheſnut being the ſame. He then alledges, " that the " very name of Spaniſh, ſeems ſtrongly to indicate " the country from which it was orignally introduced " here" (p. 24.) This is ſurely a ſtriking inſtance of an inaccuracy of language; the whole controverſy between us turns only upon that which is commonly called the cheſnut tree, and which is therefore de-

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nominated *Castanea Vulgaris*, by all the ancient Botanists. It is so called by Dr. Johnson in his *Mercurius Botanicus:* by the same author, in his *Iter Cantianum*; and by Blackstone, in his *Specimen Botanicum*; and in this true view of the controversy, let us examine the principal parts of it.

I have, Sir, in the abovementioned quotation, particularly noticed a large tract of chesnut woods, to continue to this day near Sittingborne, in Kent; in opposition to this, Mr. Barrington says, that he has taken a very minute inspection of these woods; and that, "finding them planted in rows, and without "any scattering trees to introduce them, he is con-"vinced that they are not natives." (p. 27 and 28) Such is the argument by which my assertion is endeavoured to be set aside.

I shall not here enter into an examination of the four general rules laid down by Mr. Barrington, "from which it may be decided, whether a tree is "indigenous or not in any country," p. 23. That I leave to the consideration of two of my particular friends, who have entered into the Botanical reasons produced by Mr. Barrington, and whose letters to me on this subject are hereunto annexed. I confine myself to the fact. "Remember, says Dr. Plot "in his MS. *Collectanea of Kent* (in the library of "Edward Jacob, Esq; of Feversham) the iron oar "smelted in Chesnut wood, in the confines of Borden "and Newington." Dr. Johnson, in his *Iter Cantianum*, 1632, speaks of the *Castanea Vulgaris inter Sittingbourne et Rochester*. And this Chesnut wood is equally mentioned as early as the 22d of Elizabeth, under the title of *Quædam Sylva, vocata Chestenwode*, in in a conveyance, which the reader may ſee below (1). This wood then is not very modern; and if ever it was planted by any human hand, muſt have been planted two or three ages ago; but it was certainly never planted by any human hand; the whole wood

(1) Ex. Orig. penes Edw. Jacob Arm. de Feverſham, S.A.S. Nov. 22, 1770. Sciant p'ſent. et futur. q'[d] ego Georgius Clyfforde, p'ochie de Bobbynge in com. Kanc. ar. p' quadam pecunie ſumma michi p'fato Georgio p' Georgium Ffylmer p' manibus ſolut. unde fateor me fore ſolut. et content. dictumq; Georgium Ffylmer hered. et exec. et admynyſtr. ſuos fore exonerat. et acquietat. p' p'ſent. dedi conceſſi vendidi et hac p'ſent. carta mea confyrmavi eidem Georgio Ffylmer quinque acr. ter. et boſc. ſive majus ſive minus ſcituat. jacen. et exiſten. in pochia de Borden in com. p'dicto videl't ad quandam ſilvam ib'm, voc. Cheſten woode verſus Weſt ad ter.* Garret, gen'; verſus Southe ad ter. hered. Alexandr. Cottye; verſus Eſt ad boſcū hered. Henrici Droumfylde; verſus Northe; Eſt et Weſt ad boſcū Thome Pettenden, verſus North; H'end. et Tenend. predict. quinque acr. ter. et boſc. cum omnibus et ſyngulis ſuis p'tin. p'fat. Georgio Ffilmer hered. et aſſign. ſuis ad opus et uſum ipſius Georgii Ffilmer hered. et aſſign. ſuor. imp'p'etū Caplitib. d'no feodi p' ſervis inde eis prius debit. et de jur. conſuet. Et ego p'dict. Georgius Clyfford et hered. mei p'dict. quinque acr. ter. et boſci cum omnibus et ſingulis ſuis p'tin. p'fat. Georgio Ffilmer hered. et aſſign. ſuis contra omnes gentes warrantizabimus et imp'petū defendemus p' p'ſentes. In cujus rei teſtimonium ego p'dictus Georgius Clyford huic p'ſent. cart. mee ſigillum meum appoſui; dat. viceſimo octavo die Maii anno regni dñe ñre Elizabeth dei gra' Angl. Frauncie, et Hib. Regine fidei defenſoris, &c. viceſimo ſecundo.

Georgius Clyfforde, (L. S.)

Sealed and delivered
in the preſence of

German Wake, &
Henry Whithead.

* Sic Orig.

covers

covers more than three hundred acres of land. In one part of Chefnut wood, upon the hanging banks of Chefnut-ftreet, and in the way from Kay-ftreet to Stockbury, are now the remains of large chefnut trees and pollards, which were plainly planted by the bold irregular hand of nature.

I had alfo mentioned a grant (or rather a confirmation of a grant) made to the abbey of Flexeley, which was the tithe of chefnuts in the foreft of Dean; " totam Decimam Caftanearum de Denâ." But Mr. Barrington objects to the fuppofition " of " Dena, in the record, meaning the foreft of Dean, " as there are fo many places of the name of Dean " in the kingdom." This however is furely an objection of no weight. The Ciftertian abbey of Flexeley, or Dene, was actually fituated in the foreft of Dean (2), and was anciently called Flaxlyn abbey of St. Mary de Dean (3). This abbey, together with Dean Magna (alias Mitchell Dean), and Dean Parva, all lie in the fame hundred with the foreft (the hundred of Saint Briannell), and are included in the ecclefiaftical deanery, called Foreft: where, therefore can the Dene of Flexely be placed, but at the foreft in which it was fituated, and from which it derived half of its appellation? And what pretence can a Dene in Hampfhire, or a Dean in Lancafhire, have to a place in a record, which relates only to the abbey of Saint Mary de Dene, in the foreft of Dean? But all fuch reafonings are unneceffary: the point is afcertained beyond the poffibility of a doubt, by Henry the Second's confirmation of the original

(2) Tanner's Notitia, p. 147.
(3) Atkin's Gloucefterfhire, p. 288. Edit. 1768.

grant,

grant, which may be ſeen below (4). The king, by this record, confirms to the monks, *locum qui dicitur Flexleia*

(4) Flexleyenſis Abbatia, in agro Glouceſtrenſi. Carta Henrici Normannorum Ducis, Donatorum conceſſiones recitans et confirmans.

H. Dux Normanniæ et comes Andegaviæ archiepiſc, &c. Salutem. Sciatis me conceſſiſſe et confirmaſſe Deo et Sanctæ Mariæ, et monachis ordinis Ciſtercienſis, pro ſalute anteceſſorum meorum, et mea propria, in elemoſinam perpetuam, omnes illas donationes quas Rogerus Comes Herefordiæ eiſdem monachis in elemoſinam dedit, juxta teſtimonium cartarum ſuarum, ſcilicet, locum quendam in valle Caſtiart, quæ dicitur Flexleia, ad conſtruendam abbatiam, et totam terram illam quæ dicitur Waſtadene, quæ fuit Walfrici, et quandam fabricam ferrariam apud Edlandam, et totam terram ſub veteri Caſtello de Dena ad ſartandam, et illam quæ eſt aſſartata, et quandam piſcariam apud Redliam, quæ dicitur Newerra, et quoddam pratum in Pulmede, et omnia aiſiamenta ſua in foreſta de Dena, et dominicum totum de Dimmoc, et terram illam quæ fuit Uthredi clerici, et terram Ernaldi, et terram Wolfrici, ita ſcilicet, quod ipſe Uthredus clericus remaneat in manu abbatis, cum eſcambio ſuo, ſcilicet duabus virgatis terræ quod nemini inde reſpondeat niſi abbati; et dimidium nemus apud Dimmoc; et ſingulis annis *totam decimam Caſtanearum de Dena*, et terram illam quam adquietavit ipſe Comes Herefordiæ de Gaufrido filio predicti Wulfrici, et aliam quam ipſe Comes adquietavit de Leſfrico, de Strattra. Quare volo, &c. Nos autem has prædictas donationes non tantum eis confirmo, ſed etiam omnes alias quas idem Rogerus Comes Herefordiæ illis in elemoſinam daturus eſt. Teſtibus Rogero Comite Herefordiæ, Willielmo de Crivecuer, Ricardo de Humet, Conſtab. Philippo de Columbariis, Roberto de Ivigum, Willielmo de Angervill, Willielmo Cumin, apud Eveſham.

Cart. Antiq. X. Num. 4.

Carta regis Henrici Secundi.

Henricus, Dei gratia, Rex Angliæ, et Dux Normanniæ et Acquitanniæ, et Comes Andegaviæ, Archiepiſopis, &c. et omnibus fidelibus ſuis Anglis et Normannis, tam præſentibus, quam

Flexleia ubi abbatia fundata est, by the title of *Locum quendam in foresta de Lenâ.* He afterwards goes on, to

quam futuris, salutem. Sciatis me dedisse et confirmasse Deo et Beatæ Mariæ et Monachis meis de Dena, quos in propria protectione suscepi, pro salute mea et antecessorum meorum, in elemosinam perpetuam, locum quendam in foresta de Dena, videlicet, totam vallem de Castiard, et locum qui dicitur Flexleia, ubi abbatia fundata est de ordine Cisterciensi, in honore beatæ virginis Mariæ, pro amore Dei, et pro anima regis Henrici avi mei, et Comitis Gaufridi Andegaviæ patris mei, et Matildis imperatricis matris meæ, et aliorum parentum et antecessorum meorum, et pro salute mea, et hæredum meorum, et pro stabilitate et pace regni Angliæ. Concessi etiam eis et confirmavi omnes illas donationes quas Rogerus comes Herefordiæ eisdem in elemosinam dedit sicut cartæ ejus testantur. Præterea dedi eis et confirmavi omnia aisiamenta in eadem foresta mea de Dena, scilicet pasturam juvencis suis et porcis suis, et omnibus aliis pecoribus suis, et ligna et materiem ad domos suas et ad ædificia sua facienda, et ad alias res usui suo necessarias, sine vasto in eadem foresta mea. Et de eadem foresta dedi eis *decimam castanearum mearum*, et grangeam quæ dicitur Wastedena, et unam forgeam ferrariam, ita liberam et quietam et operantem, per omnia, sicut meæ dominicæ forgeæ. Et totam terram sub veteri castello de Dene ad sartandam, et illam quæ est assartata; videlicet, centum acras, et quandam piscariam apud Reidleiam, que dicitur Nolwera, et quoddam pratum apud Reidleiam, quod vocatur Pulmede; scilicet quatuor acras, et terram quam illis dedit in elemosinam Leuvericus de Staura, et grangiam quam eis dedi apud Wallemere, de assartis meis; videlicet, ducentas acras, cum pratis et pascuis, et omnibus aliis aisiamentis, et quatuor acras de Northwoda, et totam dominicatum meum de Dimmoch, et quinque virgatas terræ et dimidiam, præter dominicatum, et dimidium nemus meum de Dimmoch, et dimidium retium in manu mea, propter aisiamenta hominum meorum, ea scilicet de causa, ut monachi mei habeant suam partem nemoris in bene et in pace, et sine omni communione aliorum hominum; et firmiter præcipio, ut nullus eos super hoc inquietet. Præterea dedi eis essartum quoddam subtus Castiard, quod vocatur Terra Vncentii. Hæc omnia dedi to give them *omnia asiamenta in eadem foresta mea de Denâ*; and then he particularly subjoins, et de *eadem foresta* dedi eis *Decimam Castanearum mearum*. Can any words possibly be more explicit than these? And can Mr. Barrington aver against the testimony of an authentic record? But, though the Dena of the record does mean the forest of Dean, Mr. Barrington has still an objection in reserve; and asserts that "there are not the least vestiges of any such trees in "this forest at present." (p. 29.) But is Mr. Barrington sure there are no vestiges of chesunt trees in the forest? Did Mr. Barrington inspect into every part of this ample area? And did no trees, no stumps, no stools, escape his eye in this wide unbounded range? But the fact appears otherwise. There are not merely stumps, not merely stools, of chesnut trees; but actual and absolute trees of chesnut existing at this day, in the forest of Dean.

In a letter to me, dated Dec. 10, 1770, from the Rev. Mr. William Crawley, resident at, and minister of Flaxley (uncle to Thomas Crawley Bovey, Esq; the present owner of Flaxley abbey); is the following account:—" In this very forest and " near Flaxley is a parcel of land, about three or

dedi Deo et beatæ Mariæ et monachis meis Deo devote servientibus, habenda et tenenda imperpetuum, soluta et quieta ab omni reguardo et exactione seculari. Quare volo, &c. Teste Ricardo de Humet, Willielmo de Creveca, Philippo de Columbariis, Willielmo de An;ervill, apud Evesham. (Monasticon Anglicanum, Tom. I. p. 884).

Pat. 22 R. II. part 3. m. 16. per Inspex. Vide Cart. antiq. N. N. 30. Et pat. 27 H. VI. par. I. m. 9.

 " four

"four hundred acres, which is ſtill denominated "*cheſnut:* though neither cheſnut, nor any other "kind of tree is to be ſeen there, excepting what "we call underwood or coppice, moſtly hazel. In- "deed in many places of the foreſt, I find cheſnut "trees are (ſparingly) to be met with; but within "a few yards of the above ſpot, in a wood of my "nephew, are many of remarkable fine growth." But, even if the fact had been as Mr. Barrington hath ſtated it, the faith of a record atteſting the exiſtence of cheſnut trees formerly, in the foreſt of Dean, was ſurely not to be ſuperſeded by the non-exiſtence of ſuch trees at preſent; they might have exiſted formerly, though they do not exiſt at preſent. And the record explicitly aſſures us that they did exiſt, and as early at leaſt as the reign of Henry the Second.

The cheſnut tree, therefore, may ſtill claim a natural relation to this iſland, notwithſtanding the two arguments of Mr. Barrington againſt it: and if we look into this kingdom, we ſee the cheſnut tree, not confined to Sittingbourne woods, or to Dean foreſt; but ſcattered with a free hand, through many parts thereof; ſhooting up with all the healthy vigour of genuine natives, and giving denomination to ſeveral places amongſt us. Thus the cheſnut wood of Sittingbourne, has given the name of Cheſnut-ſtreet, to the neighbouring road; and the old Saxon half of the name, Street, ſtrongly intimates the other half to be very ancient. The appellation occurs in the firſt map, that notices the names of the roads, the map of Kent by Morden. In Hertfordſhire is a town, called in old writings, Cheſton, Cheſthunte, Sheſterhunte, and Ceſtrehunt; and Norden (in his deſcription difcription of Hertfordfhire, p. 15,) fays, Cur non (5) Cheſtin? Caftanetum of Cheffe-nut trees?

The Saxons were well acquainted with this tree, and, according to Skinner and Lye, called it Cẏſtel and Cẏſz-beam; the fame word evidently with our prefent Chef-nut. Dr. Johnfon, in his *Mercurius Botanicus*, 1634, remarks the chefnut to have been not unfrequent in the woods, as well as in the plantations, of his own times; *Caftanea Vulgaris in fylvis nonnullis et viridariis*;—Mr. Dale, in his *Hiftory of Harwich*, mentions various chefnut trees to be growing in Stour wood, within the parifh immediately adjoining to Harwich. Blackftone, in his *Specimen Botanicum*, p. 12. fpeaks of chefnut trees growing in (6) Bulwin woods, between Dartford and Bexley, in Kent, plentifully; not twenty miles diftant from London. Mr. Philipot, in his *Villare Cantianum*, which was printed in 1659, fays in p. 237. "There "is a manor, called Northwood Chafteners, which "name complies with the fituation; for it ftands North "from the town, in a wood where chefnut trees "formerly grew in abundance." "The noble chef- "nut tree, fays Morton, (Northamptonfhire, p. 397.) "belonging to the Worfhipful Thomas Tryft, Efq; "of Marford, is the largeft of that kind I have any "where feen: the body of it is no lefs than fifteen "feet eight inches in circumference; and it extends "its branches proportionably." "On the outfide of "the Roman ftation at Temple Brough, near Sheffield, "in Yorkfhire, fays Gibfon's Camden, (Vol. II. p. "847.) "is a large bank, upon which are huge trees, "and upon the fide of the bank of the highway,

(5) Cheftin. (6) Now Baldwyn Woods.

 "there

" there grew a chefnut tree that had fcarce any bark " upon it, but only upon fome top branches which " bore leaves; it was not tall, but the bole could " fcarcely be fathomed by three men." " There was " ftanding, fays Evelyn (in his *Sylva*, Fol. London, " 1706, p. 223.) an old and decayed chefnut at " Fraiting, in Effex, whofe very ftump did yield " thirty fizeable loads of logs. I could produce you " another of the fame kind in Gloucefterfhire, which " contains within the bowels of it, a pretty wain- " fcotted room, enlightened with windows, and " furnifhed with feats, &c." And to thefe we may add two great chefnut trees flourifhing at Tortworth, in Gloucefterfhire, and at Writtlepark, in Effex; the former is allowed, even by Mr. Barrington, " to be " the oldeft tree that we have any account of, per- " haps in Europe." (p. 30.) And the following de- fcription of both, was publifhed about twelve or thirteen years ago (7); " At the feat of the Lord " Ducie, at Tortworth, in Gloucefterfhire, there is " now growing an Englifh chefnut, which meafures " fifty one feet about, at the height of fix feet above " the ground. This tree divides itfelf, at the crown, " into three limbs, one of which meafures twenty " eight feet and half in the girt, and five feet above " the crown of the tree. The foil is a foft clay, " fomewhat loomy; the fituation is the North Weft " fide of a hill; this tree was ftiled, in King John's " time, the great and old chefnut tree at Tortworth; " fo it is fuppofed to be now above one thoufand " years old."

(7) London Magazine, 1758, p. 482.

" There

" There is another ſtately cheſnut (8), but little " inferior to that at Torteworth, in Writtle park, " three miles to the left of Ingateſtone, in Eſſex. " The late Lord Petre meaſured this tree, and found " it forty five feet girth, five feet from the ground; " this vaſt trunk ſupports a lofty head, which, at a " diſtance, affords a noble proſpect, and well de- " ſerves to be ſurveyed by all that admire ſuch " wonderful productions." At Little Wymondley, near Hitchin, in Hertfordſhire, is an old decayed cheſnut tree, the trunk whereof (meaſured within theſe two years) was found to be forty two feet circumference in one part, and forty eight feet in another, as I am credibly informed.(9) And, to give additional force to an argument which is already deciſive of itſelf, we may obſerve, that in the New Foreſt, there are very many cheſnuts irregularly ſcattered among the oaks and other trees; and now to be ſeen in the road from Limington to Southampton.

In this great abundance of cheſnut trees formerly among us, we need not wonder that cheſnut timber was frequently uſed in old houſes, preferable to oak; it was then the timber moſt eſteemed by our joiners and carpenters. And, though very laſting, yet it has been juſtly diſcredited, in theſe later ages, for houſes, becauſe, when it begins to decay, the conſumption commences at the core, and the heart is the firſt deſtroyed. And we can produce ſome

(8) In a News Paper, called The Citizen, or General Advertizer, Sept. 21. 1758.

(9) This tree is ſituate in the grounds, and near the houſe of Little Wymondley Bury, late the eſtate of Lord Groſvenor, but purchaſed within two or three years by Col. Cracherode.

proofs,

proofs, additional to the many that have been formerly produced, of chesnut timber actually employed in buildings. "The old houses in the city "of Gloucester (as the Reverend Mr. Crawley informs me that he has often been assured) are "constructed of chesnut, derived assuredly from the "chesnut trees in the forest of Dean." In many of the oldest houses at Feversham is much genuine chesnut, as well as oak, employed. In the nunnery of Davington, near Feversham (now entire), the timber consists of oak intermingled with chesnut. And the great chesnut beam which supported the leads of the church tower at Feversham, when it was lately taken down, was found rotted for many feet at the extremity; and had, as it were, a mere shell of sound timber remaining about it.

Thus have I endeavoured, with all the respect due to genius and truth, to point out some of the mistakes into which, I apprehend, Mr. Barrington has fallen. I might have dwelt more largely upon the antiquarian part of my subject; but the botanical was more immedtately my point. And in the examination of this, I have shewn, that the chesnut tree flourishes greatly in this kingdom; that it appears wildly scattered over the face of the country; that it was actually settled among us many centuries ago; and used by our ancestors in buildings; and that it was even familiarly known to the Saxons. All these united evidences strongly co-operate to prove it a native of this island, and must absolutely be allowed to prove it, till Mr. Barrington, or some other person, can produce superior evidence to the contrary.

I beg

I beg leave to ſubmit theſe obſervations to your conſiderations; and have the honor to remain,

SIR,

Your moſt faithful

humble ſervant,

Doctors Commons,
Jan. 5, 1771.

And. Coltee Ducarel.

XVIII. *Copy*

XVIII. *Copy of Mr.* Thorpe's *Letter to Dr.* Ducarel, *concerning Chesnut Trees.*

Dear Sir,

Read March 8, 1771.

HAVING perused the Hon. Mr. Barrington's letter to Dr. Watson, published in the Philosophical Transactions, I find he lays down three or four general rules to determine whether a tree is indigenous or not in any country, as follows:

" I. They must grow in large masses, and cover " considerable tracts of ground; nor must such woods " end abruptly by a sudden change to other trees, " except the situation and strata become totally " different.

" II. If the trees grow kindly in copses, and " shoots from the stool, it must for ever continue in " such a wood, unless grubbed up, nor is it then " easily extirpated.

" III. The seed must ripen kindly: nature never " plants but where a succession may be easily con- " tinued, and in the greatest profusion.

" Lastly, many places in every country must re- " ceive their appellation from indigenous trees, which " grow there, &c. When the instances of this are " singular,

"singular, it will prove directly the contrary, as he "hopes to shew with regard to the chesnut, &c."

In answer to his objections, and agreeable to these his forgeoing rules; I shall endeavour to prove the chesnut to be an indigenous tree, in this island; and 1st, Mr. Barrington says, that he examined the woods near Sittingbourn himself; "and on a very "minute inspection of them, found those parts which "consist of chesnuts, to be planted in beds or rows, "about five yards distant from each other; nor are "there any scattering trees to introduce them, &c."

In what wood or woods, he observed these plantations, I must confess, I am quite at a loss to find, having never observed this regularity in any of the woods I have been in; and I very lately asked a person who has lived many years in that neighbourhood, deals largely in timber and underwood, and is over all these woods every year, who told me he knew of no such regular plantations in any of them; that the chesnut grew intermixed with other trees, as in all ancient woods.

Indeed, the amazing distance of the plants from each other, which Mr. Barrington mentions, is somewhat extraordinary; as the usual custom now, in planting sets of chesnut or ash, for hop poles, is about seven or eight feet distance, as has been lately done by John Cocking Sole, Esq; in his plantation of chesnuts, at Newington.

The woods, called the Chesnut woods, the property of the Earl of Aylesford, which lie in the parishes of Newington, Borden, and Bobbing, abound with these trees, which grow promiscuously with others, both from stubs and stools of a large size;

 twenty

twenty acres of which are annually felled for poles, &c.

Cranbroke Wood, belonging to Mrs. Mercer, in Newington, has the chefnut in plenty with other trees, which produce poles in abundance, from old ftubs and ftools.

The Squirrel Wood, the property of the Hon. Mr. Roper, in the parifh of Stockbury; thofe called Long Tun and Binbury, contain plenty of chefnut, intermixed with other trees, in which are very large chefnut pollards; to appearance fome hundred years ftanding; which grow on a poor foil, and are quite hollow fhells, having no nourifhment but from the rind or bark; yet throw out plenty of fhoots from the roots.

I have a farm in the parifh of Stockbury, called Nettlefted, forty acres of which are tithe free, which portion of tithes belonged to the great monaftery of St. Auftin, fituated without the walls of Canterbury. They were given in very ancient times to the ufe of the almonary or almonry of that abbey; as far back as the time of Archbifhop Walter, in the year 1193, how long before is uncertain, and are mentioned by William Thorn, a monk of that houfe, and publifhed by Sir Roger Twifden, in the Decem Scriptores; part of thefe tithes are woodland, and to this day called Almery or Ambry-Tanton. In this wood are very old ftools of chefnut, fome of which are ten feet circumference, and ftand promifcuoufly with oak, afh, and other trees. Thefe ftools yet produce very good poles, which were felled once in my father's time, and have twice fince they have been in my poffeffion.

In

In ſhort, all that vaſt range of woods, called Stockbury vallies, which extend from Key-ſtreet to Binbury Pound, produce the cheſnut in common with other trees; the woods formerly belonging to the abbey of Leſnes, founded by Richard de Lucie, chancellor and chief-juſtice to Henry II. in the pariſh of Earith, ſtill called the Abbey woods, having great plenty of cheſnut, both timber and ſtub wood, and from the ſtumps and ſtools of large timber trees formerly felled, which ſtools are now quite hollow and decayed, except the outward bark or ſhell, round the crowns of which ariſe many ſtools, and are cut for poles at the proper growth.

Church wood, in the ſame pariſh, has the like; and many others in this neighbourhood.

In Wrotham pariſh above Kemſing, is a wood belonging to a farm, called Cottons, which has cheſnut intermixed with other trees.

I could enumerate many more in different parts of this county, was it neceſſary; and I make no doubt, on due inſpection, the like may be found in other counties of this kingdom: it is moſt certain, the cheſnut does not grow in every wood, but in ſuch only, where the ſoil is adapted to it. Different ſtrata will produce different trees; as for example, the great wood called Jordens, in the pariſhes of Bexley and North Cray, the woods beyond Ruxley towards Farningham, have ſome acres nothing but birch, ſome only hazel, &c. Godden-wood, in the pariſh of Seal, is intirely birch. The woods on the Cold hills, of Chelsfield and Nockholt, run moſt upon beech; and thoſe in the Weald of Kent, upon a clayey ſoil, are chiefly oak.

 In

In anſwer to the third and laſt general rule; that the nuts of the cheſnut tree ripen kindly, and in great quantity, is manifeſt from the numbers of poor people at Earith, and the adjoining hamlet; going into the woods at the proper ſeaſon, and gathering ſome a quarter, others three ſacks each, to fat their hogs, eſpecially when pulſe and grain are dear. It is true, the nuts are not ſo large as on trees which ſtand ſingle and open to the ſun, in parks, courts, &c. Even the oak will not produce acorns in a wood, till it becomes an old tree; and then not ſo large and in ſuch plenty as on old trees and pollards which ſtand open in fields and hedge rows. But where the cheſnut, as before obſerved, ſtands ſingle and planted for ornament, as in the Wilderneſs park, the ſeat of Mr. Prat, in Seal; and in Bradbourn park, the ſeat of Sir Roger Twiſden, Bart, at Eaſt Malling, and divers other places, the nuts are large, well taſted, and in great plenty, yielding excellent food for the deer.

It is well known that trees cloſe planted in orchards will not produce fruit ſo large and fair, as in kitchen gardens, where they ſtand ſingle, are often digged about, and manured.

Mr. Barrington himſelf ſays, Dr. Watſon informed him, "that in Spain the cheſnut trees deſtined to "produce the beſt fruit, are engrafted upon the "wild cheſnut; and that the French call the com- "mon ſort Chataignier, and the improved one "Maronier." If ſo, the latter may be the ſort which are annually brought to England, and ſold at all the fruit ſhops, &c. and are called Spaniſh cheſnut. Mr. Barrington ſays, "the very name of Spaniſh, "ſeems

" ſeems moſt ſtrongly to indicate the country from " which this tree was introduced here." But why Spaniſh? I do not know that it is any where here ſo called, and none of the wood-men know it by that name. The old Botanical writers, John Bauhine, Gerhard, and Parkinſon, call it Caſtanea Vulgaris. Caſpar Bauhine, in his Pinax, Caſtanea Sylveſtris, the common or wild cheſnut tree. Ray indeed, in his Synopſis, the 3d Edit. publiſhed by Dillenius, p. 449, has the following, " in ſylvis quibuſdam prope " Sittingburn Cantii oppidum, & Woburn Bedfordiæ, " obſervavimus an ſpontaneam, an olim ibi ſatam, neſ- " cimus." It is ſomewhat ſtrange that ſo celebrated a Botaniſt ſhould treat of it in ſo ſlight a manner, and with ſeemingly ſo little attention, as to mention it only in thoſe two places.

Laſtly, Mr. Barrington ſays, " that many places, " in every country, muſt receive their appellation from " indigenous trees which grow there, &c."

There are many trees which give few, if any, appellation to places. It does not therefore follow that they are not indigenous. In ancient time, England abounded more in woods and foreſts than at preſent; and the oak and aſh being then two of the moſt common trees, occaſioned the names of the contiguous places and pariſhes to receive their derivation.

Notwithſtanding his trial of the ſpecimens of oak and cheſnut, I am well aſſured many old buildings were, and are, of the latter; eſpecially in places where theſe trees flouriſhed. When I repaired the old houſe at Nettleſted, in Stockbury, in ſawing off the end of the main girder, it was decayed at heart; and

and pronounced by the ſurveyor and carpenter then preſent to be cheſnut, as are the other timbers.

Cowſted, a very ancient ſeat in the ſame pariſh, is intirely of that wood; and Dr. Stukely, in his letter to the late Lord Hardwick, read at the Society of Antiquaries, and ſince publiſhed in the Archæologia, p. 44. ſays, " the curious roof of the large hall " of the manſion houſe at Leſnes is of cheſnut, which " no doubt was felled in the abbey woods there."

In latter times, the ſeat called Mount Maſcal, in the pariſh of North Cray, rebuilt by Sir Comport Fitch, Bart. about fourſcore years ſince, the girders and large timber of which are, as I am well informed, of cheſnut felled in the woods adjoining.

And why ſhould it not have been uſed in buildings, ſeeing it is very durable, and grows to a great ſize? witneſs the fine trees felled laſt ſummer, together with ſome oak and beech, in the park of Penſhurſt in this county; poſſibly in length of time, the characteriſtick of the cheſnut trees decaying inwardly, might be the reaſon of the oaks being moſtly uſed, as the more durable timber; and the former found to turn to better account for underwood and poles; eſpecially when hops came into uſe in Henry the Eighth's time, and are the beſt for that purpoſe. Even oak, by reaſon of its ſcarcity and dearneſs, is now little uſed in publick buildings; fir-timber altogether ſupplying its place.

The cheſnut tree yet alive in the court at Tortworth, in Glouceſterſhire, ſuppoſed by Evelyn and Bradley to have been planted in the time of King John, may poſſibly be the oldeſt tree of the kind extant in this kingdom; but is no proof of there

not

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not being chefnut trees before that time: Any more than the famous tree called Bears oak, in the park at Penfhurft abovementioned; or the well-known tree called Fifher's oak, in the parifh of Farnborough, in this county; or that in Welbeck park, the feat of the Duke of Portland, were fome of the firft trees of that kind here planted; the fituation and ornament of thefe trees protected them from the axe.

The common elm, Evelyn thinks not to be an indigenous tree, and it may not as it is feldom, if ever, found growing in woods; but in road ways, hedge rows, &c.; and not in the North of England, though, as Mr. Ray obferves, fome trees are only found in the North, fome in the South, and others in the Weft; neither does the elm, when an old tree, fhoot kindly from the ftool.

I agree with Mr. Barrington, that the box tree is an exotick; but the yew is certainly indigenous, as I think may be eafily proved, and which he affents to, but doubts whether the euonymus or fpindle tree, and liguftrum or privet, are fo; moft certainly they are, as no fhrubs are more common on dry banks, and in hedges, &c.: but, as he affigns no reafon for their not being indigenous, I fhall dwell no longer on that fubject, and conclude,

Dear Sir,

Your moft humble fervant,

Bexley,
Nov. 26. 1770.

J. Thorpe.

XIX. *Extract*

XIX. *Extract of a Letter from* Edward Hasted, *Esq; F. R. S. and F. S. A. to Dr.* Ducarel, *concerning Chesnut Trees.*

Dear Sir,

Read March 8, 1771.

IN anſwer to Mr. B's 1ſt rule—I muſt remark, inſtances are exceeding frequent of woods and coppices breaking off, by a ſudden change, to other trees, and that where the ſiutation and ſtrata are entirely the ſame; ſometimes without any mark of diviſion, and ſometimes with a ditch only, an old ſtub for a boundary, or perhaps diſtinguiſhed only by the difference in the growth of the underwood, or the like. It is a known fact, that particular ſorts of trees have grown in large tracts and maſſes in a country, which have been in ſucceeding times almoſt extirpated from thence, either from others being more diligently encouraged and preſerved, or from the preſent deſtructive method of too frequent cutting them down; and only ſcattered ſtubs or trees have remained of the ſort, thinly diſperſed in woods and hedges. The wich, elm, maple, and others, are indigenous trees; and yet ſeldom, if ever, grow in large maſſes, or cover conſiderable tracts of ground; the reaſon of which

which is, they never ſhoot from the ſtool ſo as to make any conſiderable progreſs.

As to the 2d—A tree, or particular wood, may grow very kindly in a coppice, and yet in proceſs of time, by the continual felling of the wood, may be entirely worn out, when other ſorts, which bear the woodman's cutting-bill more kindly, will increaſe, and overrun the former, ſo as to fill every vacancy made by it. Beſides, there are ſome kinds of wood which are poiſoned, and in time decay by the near affinity of others. The aſh is a particular inſtance of this poiſonous quality towards other trees.

As to the 3d rule of ſeeds ripening kindly; I muſt diſagree in this too, as I find very few, if any, whether indigenous or not, whoſe ſeeds do not ripen here ſufficient to continue the tree eaſily; and where it is not in profuſion, the indigenous tree will be found as deficient as ſome others, which are known to be otherwiſe.

Mr. B's laſt rule, of places taking their name from indigenous trees which grow there, may ſerve as well to prove all trees whatſoever ſo: there being but few trees which have grown in Britain, but our very ingenious etymologiſts have derived the names of ſome places from them.—Singular inſtances, I own, I do not recollect.

All kinds of things in general adopt the name of that country where they grow, or are made in the greateſt perfection.—Inſtances of this are obvious in every neceſſary of life. The cheſnut, whoſe fruit ripens in Spain in much more perfection than in this variable and colder climate, has gained the additional name of Spaniſh to it, among the merchants

 and

and venders of them, though in the country villages the woodmen will yet talk of the growth of this right ENGLISH CHESNUT. And as to Pliny's telling us that chefnuts were brought from Sardis to Italy long before his time; that does not make it lefs probable that they might have been the growth of Britain, at the very time they were brought from thence to Rome.

The ancient Norman buildings are moftly of this wood, which in all probability was fetched from this country; moft of the ftone wherewith our monafteries and buildings of fuch fort were erected came from Normandy. This feems to have been a mutual traffick for fome centuries between the two countries.

How the notion arofe firft, that the foreft mentioned by Fitz-Stevens to the Northward of London, was moftly of chefnut, I do not know, nor could I ever find any authority for it; though it continues the affertion of moft literary men. If I might conjecture, I fhould think it to have arifen from a blunder and miftake of the name of Norwood; there being many decayed ftubbs of chefnuts in the archbifhop of Canterbury's Norwood, not far from London; which is, no doubt, the place Mr. Miller means, when he mentions fuch having been feen in the neighbourhood of the metropolis.

Moft antiquarians affert that Old London was built of chefnut: that this tree grew near London, has been proved above from Norwood, and may from the name of Chefhunt, in Hertfordfhire; that it may have done fo in former times in great plenty, might be fuppofed from what I have faid before; but one reaſon of its decay may be aſſigned to the great increaſe of the metropolis, which conſumed moſt of the cheſnut timber near it; and the ſtubbs of ſuch being much ſubject to decay, few, if any of them, could naturally laſt to this time, ſo as to bring any profit to the owner, but have been grubbed up from time to time, till they are now almoſt totally eradicated; and I think, there is great probability that the univerſal decay and deſtruction of this kind of timber, throughout the realm, appeared in ſo ſerious a light to the legiſlature, as to give the firſt riſe for our laws for the preſervation of timber in general.

Oak timber is ſo entirely different from cheſnut, in the rings and ſpaces, which appear when cut tranſverſely, that it is impoſſible to miſtake the one from the other.

In a note, p. 96. of the Anglo-Norman antiquities, mention is made, of a large tract of cheſnut woods, near Sittingbourne, in Kent (and in the North Weſt part of Eaſt Kent, as it ſhould be printed), which is certainly right; theſe woods are a very large tract, which more or leſs have cheſnut ſtubbs ſpread over the whole ſpace of them. They extend ſome miles, from the environs of the town of Milton, by the old highway (now diſuſed), leading from thence to Maidſtone. The general name of the whole tract, is Cheſnut or Cheſtney Woods. The 40 acres mentioned in the ſaid note to have been grubbed up, were only felled; and were of ſuch a ſize and growth, as to be moſtly uſed as timber. On the top of Cheſnut Hill between Newington and Sittingbourne, there ſtood a cheſnut tree of prodigious ſize, which has been felled within theſe few years, the ſtool of which may now be ſeen cloſe to the high road.

The production of nature in this vaſt tract of woods is ſo plain, that it would be abſurd to uſe arguments to defend it; nor ſhall I bring examples of it from other countries, which might be had: I ſhall only take notice, with Dr. Ducarel, that in the ancient foreſts of Kent, which lay to the ſouth of it, adjoining to Suſſex and Surry, there remain large old cheſnut ſtubs or brocks, now almoſt worn out, and periſhed, which are left by the woodmen as termini or boundaries, either of pariſhes or of private property; which is the univerſal cuſtom every where made uſe of to diſtinguiſh the wood of different owners, and are never cut down or altered; ſo that they muſt have ſtood ſacred to this uſe, from the firſt introduction of private property into this iſland; and were no doubt even then of conſiderable age, by their being made choice of for this uſe, in preference to any others.

But to return to the neighbourhood of Milton.—The manor of Norwood, within that pariſh, is called, in the higheſt records we are acquainted with, Norwood-Cheſtney, Chaſtney, and Caſtney, no doubt from the great plenty of cheſnut within its bounds, even in thoſe early times. Nor is this a ſingular inſtance of any place in England being named from the cheſnut tree; Cheſhunt, in Suffolk; and Cheſhunt, in Hertfordſhire, having both their names from the plenty of cheſnuts near them: the laſt of theſe places, Chancy tells us, ſeems in old time to have abounded with them; and that moſt of the ancient houſes in that vill were built of them; and in the venerable

nerable book of Doomſday, we have an account of a quantityof woodland in this pariſh, ſufficient for the feeding of 1200 hogs, which ſhews us that this conſiderable tract of wood was of ſuch ſort, as to afford plenty of good food for ſwine; as it certainly muſt be to afford pannage for ſo large a number; and that theſe woods were cheſnuts, may in all probability be preſumed from the above circumſtances.

The ſame venerable record likewiſe mentions the village of Box, alias Boxbury, in Hertfordſhire; which, the learned Serjeant tells us, was ſo called from a large wood, which retains the name to this day; and I have now before me the names of more than a dozen pariſhes and places, which have taken their names from the box tree, and retain it to this time. The fir, no doubt, from every evidence that can be had of former times, and by the evidence of our own eyes, from the numbers of them which have been dug up in almoſt every part of Britain, was an indigenous tree of this county; notwithſtanding Cæſar's aſſertion to the contrary, who appears to have been but little acquainted with it, when he tells us, "this iſland had every kind of "tree the ſame as Gaul, except the fir and the "beech;" both of which were in the greateſt plenty here at that very time; the latter was particularly ſo within the county of Kent, the only ſpot he might be ſaid to be acquainted with: and yet, after this, no one ſure will aſſert that either of theſe trees are not indigenous; though the former of them is entirely extirpated (as the production of nature) from the Southern part of Britain, which the cheſnut is not; though it is made uſe of as an argument againſt

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its being the natural product of this country. The elm bears every mark of its being indigenous; and, according to one of Mr. B's general rules, it muſt be ſo, for there are near 40 places in England, which take their name from this tree, moſt of which are mentioned in the book of Doomſday.

Whoever has been much acquainted with the woods and tracts of ground lying on our Chalky Hills, will ſurely never contend that the yew is not the indigenous growth of this country. I am,

Dear Sir,

Yours, &c.

Huntingfield, in Kent,
Nov. 29, 1770.

Edw. Haſted.

XX. *A Letter from the Hon.* Daines Barrington, *F.R.S. to* Mathew Maty, *M.D. Sec. R.S. occasioned by the three preceding Letters.*

Dear Sir,

February 15, 1771.

Read March 8, 1771.

I HAVE lately had an opportunity of perusing three letters from Dr. Ducarel, Mr. Thorpe, and Mr. Hasted, which contend that the sweet chesnut is an indigenous tree of this country, and which are intended to be communicated to the Royal Society.

As I do not see any reason for altering the opinions which I have happened to form on this subject, from what is contained in these three letters, I should not trouble the Society with any answer to the contents of them, did not Mr. Thorpe contradict, on the testimony of another person, what I have asserted I was an ocular witness of.

I must therefore a second time repeat, that the chesnut woods near Newington, in Kent, are planted in rows at four or five yards distance (other trees often intervening); and for a proof of this fact, I refer Mr. Thorpe to the woods on the North East of the church;

church*; as alſo the wood to the eaſtward of the great road to Canterbury, immediately after you leave the town of Newington.

I ſpent very near a whole day in the examination of theſe woods; but I would more particularly refer to the two cheſnut plantations above ſpecified, as they were juſt then ſhooting from the ſtools, when I took this very minute view of them.

I have already ſaid, that I am willing to leave the point in controverſy, upon what hath been advanced on the one ſide, and on the other.

I will only beg leave to ſtate a ſingle obſervation, together with what ſeems to be an inference that is fairly deducible from it, and which is applicable to any diſputes, with regard to trees being of native growth, or otherwiſe.

I believe I may ſay, that I have been almoſt in every corner of the twelve Welſh counties; and never ſaw a beech tree in any of them, which had the leaſt pretence to be indigenous.

I will ſuppoſe, however, that a wood of any given number of acres, with beech in it, was found in the central part of the principality; and that theſe trees were not planted in rows (as at Newington and Sittingborne); but diſperſed, as happens in other indigenous woods.

Could it poſſibly be contended, that ſuch beech trees had not been introduced by ſome planter; notwithſtanding it might be proved to be a wood of great antiquity?

* I think, I can depend upon my memory ſo far, as to ſay that the cheſnuts I have alluded to, are at the North Eaſt of the church; but at all events, they are very near to it.

If this was inſiſted upon, it muſt at the ſame time be conceived, that when the beech maſt was wafted by the wind to ſuch a moſt ſelected ſpot, ſome preternatural cauſe muſt have prevented its being ſown in any intermediate place.

I am, Dear Sir,

Your moſt faithful

humble ſervant,

Daines Barrington.

Received November 15, 1770.

XXI. *An Account of the Nyl-ghau, an* Indian *Animal, not hitherto deſcribed: By* William Hunter, *M. D. F. R. S.*

Read Feb. 28, 1771.

AMONG the riches which, of late years, have been imported from India, may be reckoned a fine animal, the Nyl-ghau; which, it is to be hoped, will now be propagated in this country, ſo as to become one of the moſt uſeful, or at leaſt one of the moſt ornamental beaſts of the field. It is larger than any ruminant of this country, except the ox; its fleſh probably will be found to be delicious; and, if it ſhould prove docile enough to be eaſily trained to labour, its great ſwiftneſs, with conſiderable ſtrength, might be applied, one would think, to valuable purpoſes.

Good paintings of animals give much clearer ideas than deſcriptions. Whoever looks at the picture, which was done under my eye, by Mr. Stubbs, that excellent painter of animals (ſee TAB. V.), can never be at a loſs to know the Nyl-ghau, wherever he may happen to meet with it. However, I ſhall attempt a deſcription of the animal; and then give as much of its hiſtory as I have been hitherto able to learn. The account will be imperfect: yet it will give naturaliſts ſome pleaſure in the mean time to know

Geo. Stubbs Pinx.t Ja.s Basire Sc.

know even a little of a large and elegant animal, which has not hitherto been deſcribed, or painted.

At firſt ſight, the male Nyl-ghau ſtruck my imagination with being of a middle nature, between black cattle and deer; ſuch an animal as we might ſuppoſe a mule would be, that was the produce of thoſe two ſpecies of beaſts. In ſize, it is as much ſmaller than the one, as it is larger than the other: and in its form there is a very apparent mixture of reſemblance to both. Its body, horns, and tail, are not unlike thoſe of a bull; and the head, neck, and legs, are very like thoſe of deer.

Colour. The colour, in general, is aſh, or grey, from a mixture of black hairs and white: moſt of the hairs are half white, and half black; the white part is towards the root. The colour of its legs is darker than that of its body; the ſame thing may be ſaid of its head, with this peculiarity, that there the darker colour is not general and uniform, but ſome parts are almoſt quite black. In ſome parts to be mentioned hereafter, the hair is of a beautiful white colour.

Trunk. The height of the back, where there is a ſlight eminence over the ſhoulder-blade, is four feet and one inch; at the higheſt part, immediately behind the loins, it is only four feet. The general length of the trunk, as ſeen in a ſide view, from the root of the neck to the pendulous tail, is about four feet; which is nearly the height of the animal; ſo that, in a ſide view, when it ſtands with its legs parallel, its back and limbs make nearly three ſides

 of

of a ſquare, and the ground upon which it ſtands makes the fourth.

Round the body, immediately behind the ſhoulder, it meaſures four feet and ten inches; and a little more juſt before the hind-legs; but this laſt dimenſion, no doubt, will vary conſiderably, as it happens to be more full or empty of food and drink.

HAIR. The hair on the body in general is thinner, more briſtly, and ſtronger, than on our black cattle. On the belly, and upper part of the limbs, it is longer and ſofter than upon the back and ſides.

MANE. All along the ridge or edge of the neck and back, as far as the poſterior part of the hump which is over the ſhoulder-blades, the hair is blacker, longer, and more erect; making a ſhort, and thin, upright mane.

The umbilical and hypogaſtric regions of the belly, the inſide of the thighs, and all thoſe parts which are covered by the tail, are white. The *præputium penis* is not marked with a tuft of hair; and the ſheath of the *penis* projects very little.

TESTICLES. The teſticles are oblong and pendulous, as in a bull.

TAIL. The bones of the tail come down to within two inches of the top of the *os calcis*. The end of the tail is ornamented with long black hair, and likewiſe with ſome white, eſpecially on the inſide. On the inſide of the tail, except near its extremity, there is no hair; and on the right and left

left there is a border of long white hair, which makes it on the inſide look like a feather.

Legs. The legs are ſmall in proportion to their length; more ſo than in our black cattle, and rather leſs ſo than in our deer. The length of the fore-leg is a little more than two feet and ſeven inches. There is one white ſpot on the fore part of each foot, almoſt immediately above the large hoofs; and another ſmaller white ſpot before the ſmall hoofs: above each of the ſmall hoofs, there is a remarkable tuft of long white hair, which turns round in a flat curl. The large hoofs of the fore-leg, are of an awkward length. This was very obſervable in every one of the five individuals of this ſpecies which I have ſeen; yet it was ſuſpected to be the effect of confinement; and the examination of the hoof, in the dead animal, proved that it was ſo.

Neck. The neck is long and ſlender, as in deer; and when the head is raiſed, it has the double turn of the Italic letter *S*. At the throat, there is a ſhield-like ſpot of beautiful white hair; and lower down, on the beginning of the convexity of the neck, there is a mane-like tuft of long, black hair.

Head. The head is long and ſlender. From the horns, it riſes upwards and backwards to join the neck. Its length, from the horns only to the point of the noſe, is about one feet two inches and three quarters.

Nose,

Nose. The partition between the noſtrils was artificially perforated for fixing a cord, or bridle, according to the Eaſtern cuſtom of tying up or leading horned cattle. The noſtrils are very long, in a direction almoſt parallel to the mouth, and are wideſt at their anterior end.

Mouth. The *rictus oris* is long; and as far as this reaches, the lower jaw is white: ſo is the upper lip, as far as the noſtril.

Teeth. There are ſix grinders in each ſide of each jaw, and four inciſor teeth in each half of the lower jaw. The firſt of the inciſors is very broad; and the reſt ſmaller in gradation, as they are placed more outwards or backwards.

Eyes. The eyes in general are dark coloured; for all of the *conjunctiva* that can be commonly ſeen is of that complexion. In an oblique or ſide view, the *cornea*, and all that is ſeen through it, is blue, like burniſhed ſteel. The pupil is oval, or oblong, from ſide to ſide; and the *iris* is almoſt black.

Ears. The ears are large and beautiful, above ſeven inches in length, and ſpread to a conſiderable breadth near their end. They are white on their edge, and on their inſide; except where two black bands mark the hollow of the ear with a zebra-like variety.

Horns. The horns are ſeven inches long; they are ſix inches round at their root, and growing ſmaller

ſmaller by degrees, they terminate in a blunt point. At their root they have three flattened ſides, divided by ſo many angles: one of the angles is turned forwards, and conſequently one of the ſides backwards. This triangular ſhape is gradually leſs perceptible towards the extremity. At the root there are ſlight circular wrinkles, in proportion to the age of the animal. The body and point of the horn is ſmooth, and the whole of a very dark colour. They riſe upwards, forewards and outwards at a very obtuſe angle, with the forehead or face. They are gently bended, and the concavity is turned inwards, and a little forewards. The diſtance between them at the roots is three inches and a quarter, at the points ſix inches and a quarter, and at their moſt hollow middle parts leſs than ſix inches.

Food. It eats oats, but not greedily; is fonder of graſs and hay *; but is always delighted with wheat bread. When thirſty, it would drink two gallons of water.

Dung. Its dung is in the form of ſmall round balls, of the ſize of a nut-meg; and it paſſes a quantity of theſe together, with a ruſhing ſound.

Manners. Though it was reported to have been exceedingly vicious, it was in reality a moſt gentle creature while in my cuſtody, ſeemed pleaſed with every kind of familiarity, always licked the

* General Carnac informs me, that no hay is made in India; their horſes are fed with graſs freſh cut, and a grain of the pulſe kind, called *Gram.*

hand

hand which either ſtroaked, or gave it bread, and never once attempted to uſe its horns offenſively. It ſeemed to have much dependance on it's organs of ſmell, and ſnuffed keenly, and with noiſe, whenever any perſon came within ſight. It did ſo likewiſe when any food or drink was brought to it; and was ſo eaſily offended with a ſmell, or ſo cautious, that it would not taſte the bread which I offered, when my hand had touched oil of turpentine or ſpirits *.

Its manner of fighting is very particular: it was obſerved at Lord Clive's, where two males were put into a little incloſure; and it was related to me by his Lordſhip, thus: While they were at a conſiderable diſtance from each other, they prepared for the attack, by falling down upon their fore-knees; then they ſhuffled towards each other with a quick pace, keeping ſtill upon their fore-knees, and when they were come within ſome yards, they made a ſpring, and darted againſt each other.

All the time that two of them were in my ſtable, I obſerved this particularity, *viz.* that whenever any attempt was made upon them, they immediately fell down upon their fore-knees; and ſometimes they

* General Carnac, in ſome obſervations which he favoured me with upon this ſubject, ſays, " All of the deer kind have " the ſenſe of ſmelling very exquiſite. I have frequently ob- " ſerved of tame deer, to whom bread is often given, and " which they are in general fond of, that if you preſent them " a piece that has been bitten, they will not touch it. I have " made the ſame obſervation of a remarkable fine ſhe-goat, " which accompanied me moſt of my campaigns in India; and " ſupplied me with milk, and which, in gratitude for her ſer- " vices, I brought from abroad with me."

would

would do ſo when I came before them; but, as they never darted, I ſo little thought this poſture meant hoſtility, that I rather ſuppoſed it expreſſive of a timid or obſequious humility *.

FEMALE. The *Female* differs ſo much from the *Male*, that we ſhould ſcarcely ſuppoſe them to be the ſame ſpecies. She is much ſmaller, both in height and thickneſs. In her ſhape, and in her yellowiſh colour, ſhe very much reſembles deer; and has no horns. She has four nipples, and is ſuppoſed to go nine months with young. She commonly has one at a birth, and ſometimes twins.

The young male Nyl-ghau is like the female in colour, and therefore like a Fawn.

SPECIES. When a new animal is preſented to us, it will often be difficult, and ſometimes impoſſible, to determine its ſpecies, by the external characters alone. But when ſuch an animal is diſſected by an anatomiſt, who is a maſter in comparative

* The intrepidity and force with which they dart againſt any object may be conceived from the following anecdote, of the fineſt and largeſt of thoſe animals that has ever been ſeen in England. The violence which he did to himſelf, was ſuppoſed to occaſion his death, which happened ſoon after. A poor labouring man, without knowing that the animal was near him, and therefore neither meaning to offend, nor ſuſpecting the danger, came up near to the outſide of the pales of the incloſure; the Nyl-ghau, with the quickneſs of lightening, darted againſt the wood work, with ſuch violence, that he broke it to pieces; and broke off one of his horns cloſe to the root. From this piece of hiſtory and farther inquiry, I was ſatisfied that the animal is vicious and fierce in the rutting ſeaſon, however gentle and tame at other times.

anatomy, the queſtion is commonly to be decided with certainty.

From the external marks alone, I ſuſpected, or rather believed, the Nyl-ghau to be a peculiar and diſtinct ſpecies. Some of my acquaintance thought it a deer. The permanent horns convinced me that it was not. Others thought it an antelope. The horns, and the ſize of the animal, made me ſuſpect that it was not. It had ſo much of the ſhape of deer, eſpecially the female, that I could not ſuppoſe it to be of the ſame ſpecies with our black cattle. In rutting time, one of the males was put into a paddock with a female of the red-deer: but nothing like attraction or attention was obſerved between them. At length, in conſequence of the death of one of them, I was aſſured by my brother, who diſſected it, and who has diſſected with great attention almoſt every known quadrupede, that the Nyl-ghau, is a new ſpecies *.

HISTORY. Of late years ſeveral of this ſpecies, both male and female, have been brought to England. The firſt were ſent from Bombay, by Gov. Cromelen, as a preſent to Lord Clive: they arrived in Auguſt 1767. They were male and female, and continue to breed every year. Afterwards two were brought over, and preſented to the Queen by Mr. Sullivan. From her Majeſty's deſire to encourage every uſeful or curious enquiry in natural knowledge,

* Mr. Pennant, whoſe love of natural hiſtory heightens the enjoyment of an independent fortune, in his *Synopſis*, publiſhed ſince this paper was written, claſſes this animal (*White-footed*, p. 29.) as a ſpecies of the *Antelope*; but he now thinks it belongs to another *Genus*, and will claſs it accordingly in his next edition.

I was

I was permitted to keep these two for some time; which enabled me to describe them, and to get a correct picture made; and, with my brother's assistance to dissect the dead animal, and preserve the skin and skeleton. Lord Clive has been so kind to give me every help that he could furnish me with, in making out their history; so has General Carnac, and some other gentlemen.

At all the places in India, where we have settlements, they are rarities, brought from the distant interior parts of the country, as presents to Nabobs and great men. Lord Clive, General Carnac, Mr. Walsh, Mr. Watts, and many other gentlemen, who have seen much of India, tell me they never saw them wild. So far as I have yet found, Bernier is the only author who has even mentioned them *. In the 4th Vol. of his Memoires, he gives an account of a journey which he undertook, ann. 1664, from Delhi, to the province of Cachemire, with the Mogul Aurengzeb, who went to that terrestrial paradise, as it is esteemed by the Indians, to avoid the heat of the summer. In giving an account of the hunting, which was the Emperor's amusement in this journey, he describes, among others, that of *le Nylghau*; but without saying more of the animal, than

* Since the reading of this paper, I have received the following information from Dr. Maty. In the fourth Volume of Valentyn's description of the East Indies, published in Low Dutch, 1727, under the article of Batavia, p. 231, I find amongst the uncommon animals kept at the castle, this short indication, "There was a beast, of the size and colour of a "Danish ox, but less heavy, pointed towards the mouth, ash-"grey, and not less than an Elk, whose name he bore." It was a present from the Mogul.

that the Emperor ſometimes kills them in ſuch numbers, as to diſtribute quarters of them to all his Omrachs; which ſhews that they were there wild, and in plenty, and eſteemed good or delicious food.

This agrees with the rarity of theſe animals at Bengal, Madraſs, and Bombay: for Cachemire is the moſt northern province of the Empire; and it was on the march from Delhi to that place, that Bernier ſaw the Emperor hunt them.

NAME. The word *Nyl-ghau*, for theſe are the component letters correſponding to the Perſian, though pronounced as if it were written *Neel-gaw*, ſignifies a blue cow, or rather a bull, *Gaw* being maſculine; and the male animal of that name has a good title to the appellation, as well from the likeneſs he bears in ſome parts to that ſpecies of cattle; as from the bluiſh tinct which is very diſcernible in the colour of his body; but this is by no means the caſe with the female, which has a near reſemblance, as well in colour as in form, to our red deer. The Nyl-ghaus which have been brought to England have been moſt, if not all, of them received from Surat or Bombay; and they ſeem to be leſs uncommon in that part of India, than in Bengal; which gives room for a conjecture that they may be indigenous perhaps in the province of Guzarat, one of the moſt Weſtern and moſt conſiderable of the Hinduſtan empire, lying to the Northward of Surat, and ſtretching away to the Indian ocean.

A gentleman * who has been long in India, and has an extenſive acquaintance there, has written to

* General Carnac, who likewiſe favoured me with the preceding article upon the name of the animal.

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his friends, to collect all the intelligence they can possibly procure concerning this animal; and in the course of the next year, some satisfactory information may perhaps be received from thence, though the natives of that country, he says, have no turn whatever after natural history; and indeed are very little inquisitive after any kind of knowledge.

XXII. *Ob-*

XXII. *Obſervations on the* Aphides *of* Linnæus, *by Dr.* William Richardſon, *of* Ripon, Yorkſhire: *Communicated by* William Heberden, *M. D. F. R. S.*

Read Mar. 14, 1771.

THE learned *Linnæus* by his unwearied application having reduced the various productions of nature into one regular ſyſtem, and clearly diſtinguiſhed the numerous tribe of inſects into their diſtinct claſſes and ſubdiviſions, ſeems to me to have laid a more ſolid foundation for the natural hiſtory of theſe minute animals, than any other writer who has gone before him. Difficult, however, as it is to lay ſo firm a foundation, the ſuperſtructure muſt ſtill be eſteemed a more arduous undertaking; as it is eaſier to diſtinguiſh the outward form, even of the minuteſt inſects, than to diſcover their internal nature and diſpoſition. This is a knowledge not to be attained by any ſingle perſon, be his genius and diligence ever ſo great; but to bring it to any degree of perfection, will require the joint endeavours of the curious in all ages, and in all the different parts of the world. From which conſiderations, I am induced to throw in my mite towards promoting ſo uſeful an undertaking; by reducing my obſervations on this ſurprizing kind of inſect, into a more conciſe and regular form.

Though the Aphides are diſtinguiſhed by Linnæus into more than thirty ſpecies; ſtill I am ſatisfied, from my own obſervation, the diſtinct ſpecies are even double that number: nor can I altogether agree with this ingenious author, that there are a greater variety of plants producing Aphides, than there are different ſorts of this inſect. Where plants are of a like nature, they are uſually frequented by the ſame inſects; but many of theſe plants will be found to ſupport two or more quite different ſorts. On the peach and nectarine indeed the Aphides are the ſame, nor do I find on theſe trees more than one ſort. The plum tree, on the other hand, has two ſorts, very diſtinct from each other: one of a yellowiſh green, with a round ſhort body; the other of a bluiſh-green, as it were enameled with white, and the ſhape more oblong. On the gooſberry-buſh and currant the ſame Aphides may be found; but each of theſe is inhabited by two very different ſpecies: one being of a duſky green, with a ſhort plump body; the other of a paler green, the body more taper, and tranſverſly wrinkled. To theſe inſtances I muſt further add, that the roſe-tree ſupports not leſs than three diſtinct ſpecies: The largeſt of which is of a deep green, having long legs of a browniſh caſt, with the joints of a very dark brown, as are alſo the horns and antennæ; a ſecond ſort is paler green, has much ſhorter legs, and a more flat body; the third ſort is of a pale red, its body tranſverſly wrinkled, and is moſt frequently on the ſweet-brier. It not being, however, ſo much my intention to enumerate the different ſpecies of theſe inſects, as to give ſome inſight into their extraordi-

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nary nature; the inſtances I have already produced will, I flatter myſelf, be thought ſufficient.

The great variety of ſpecies which occur in the inſects now under conſideration may indeed make an enquiry into their particular natures ſeem not a little intricate and perplexed; having them, however, ſkilfully reduced under their proper genus, the difficulty is by this means conſiderably diminiſhed. All the inſects comprehended under any diſtinct genus, we may reaſonably ſuppoſe to partake of one general nature; and, by diligently examining any of the particular ſpecies, may thence gain ſome inſight into the nature of all the reſt. With this view I have choſen, out of the various ſorts of Aphides, the largeſt of thoſe found on the roſe tree; not only as its ſize makes it the more conſpicuous, but as there are few others of ſo long a duration. This ſort, appearing early in the ſpring, continues late in the autumn; while ſeveral are limited to a much ſhorter term, in conformity to the different trees and plants from whence they drew their nouriſhment.

SECTION I.

If at the beginning of February the weather happens to be ſo warm, as to make the buds of the roſe tree ſwell and appear green; ſmall aphides are frequently to be found upon them, not larger than the young ones in ſummer, when firſt produced. But there being no old ones to be found at this time of the year, which in ſummer I had obſerved to be viviparous; I was formerly not a little perplexed by ſuch

ſuch different appearances, and almoſt induced to give credit to the old doctrine of equivocal generation. That the ſame kind of animal ſhould, at one time of the year be viviparous, and at another oviparous, was an opinion I could then by no means entertain. This, however, frequent obſervation has at laſt convinced me to be fact; having found thoſe Aphides, which appear early in the ſpring, to proceed from ſmall black oval eggs, which were depoſited on the laſt year's ſhoots in autumn: though, when it happens that thoſe inſects make too early an appearance, I have obſerved the greateſt part to ſuffer from the ſharp weather that uſually ſucceeds; by which means the roſe trees are ſome years in a manner freed from them.

Thoſe which withſtand the ſeverity of the weather, ſeldom come to their full growth before the month of April; at which time they uſually begin to breed, after twice caſting off their exuviæ, or outward-covering. It then appears that they are all females, which produce each of them a very numerous progeny, and that without having intercourſe with any male inſect. As I obſerved before, they are viviparous; and what is equally uncommon, the young ones all come into the world backwards. When they firſt come from the parent, they are enveloped by a thin membrane, having in this ſituation the appearnce of an oval egg; which I apprehend muſt have induced Reaumur to ſuſpect that the eggs diſcovered by Bennet were norhing more than abortions. This egg-like appearance adheres by one extremity to the mother, while the young one therein contained extends the other; by that

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means gradually drawing the ruptured membrane, over the head and body, to the hind feet. During this operation, and for ſome time after, by means of ſomething glutinous, the fore part of the head adheres to the vent of the parent. Being thus ſuſpended in the air, it ſoon frees itſelf from the membrane in which it was confined, and after its limbs are a little ſtrengthened, is ſet down on ſome tender ſhoot, and then left to provide for itſelf.

When the ſpring proves mild, and conſequently favourable to this kind of inſect, I have obſerved not only the roſe trees, but various kinds of fruit-trees, to be greatly injured by them. Hence I was firſt introduced to inveſtigate the nature of theſe inſects; in order to find out ſome expedient, whereby ſo great an evil might be prevented. To avoid being tedious by deſcending to particulars, I ſhall recommend the following general rule; *viz.* to lop off the infected ſhoots before theſe inſects are greatly multiplied; repeating the ſame operation before the time their eggs are depoſited. By the firſt pruning, you will prevent a very numerous preſent increaſe; and by the ſecond, may intirely cut off the next year's ſupply.

SECTION II.

In the ſpring months, there appear on the roſe tree but two generations of Aphides, including thoſe which immediately proceed from the laſt year's eggs; the warmth of the ſummer adds ſo much to their fertility, that no leſs than five generations ſucceed each other during that interval. One is produced

duced in May, which twice caſts off its covering; while the months of June and July each ſupply two more, which caſt off their coverings three or four times, according to the different warmth of the ſeaſon. This frequent change of the outward covering is the more extraordinary, as it is the ofteneſt repeated when the inſects come the ſooneſt to their growth; which I have ſometimes obſerved to happen in ten days, where warmth and plenty of nouriſhment have mutually conſpired. From which conſiderations, I am thoroughly convinced, that theſe various coverings are not connate with the inſect; but that they are like, the ſcarf-ſkin, ſucceſſively produced.

Early in the month of June, ſome of the third generation, which were produced about the middle of May, after caſting off their laſt covering, diſcover four erect wings, much longer than their bodies: and the ſame is obſervable in all the ſuceeding generations, which are produced during the ſummer months; without however diſtinguiſhing any diverſity of ſex, as is uſual in ſeveral other kinds of inſects. For ſome time before the Aphides come to their full growth, it is eaſy to diſcover which of them will have wings, by a remarkable fulneſs in the breaſt, which in the others is hardly to be diſtinguiſhed from the body. When the laſt covering is rejected, the wings, which were before folded up in a very narrow compaſs, gradually extend themſelves in a moſt ſurprizing manner, till their dimenſions are at laſt very conſiderable. But theſe winged ones have this further peculiarity, that the number of them does not ſeem ſo much to depend on their

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original ſtructure, as on the quantity or quality of the nouriſhment wherewith they are ſupplied: it being frequently obſervable, that thoſe on a ſucculent ſhoot have few or none with wings among them; while others of the ſame generation, on a leſs tender branch, are moſt of them winged: as if the firſt rudiments of the wings were compoſed in the former, while nature thought proper to expand them in the latter, that they might be more at liberty to ſupply their wants.

The increaſe of theſe inſects in the ſummer time is ſo very great, that, by wounding and exhauſting the tender ſhoots, they would frequently ſuppreſs all vegetation, had they not many enemies which reſtrain them. To enumerate the variety of other inſects, that in their worm and fly ſtate are conſtantly deſtroying them, would exceed the bounds of my preſent deſign: there is one, however, ſo ſingular in the manner of executing its purpoſe, that I cannot paſs it by without ſome further notice. This is a very ſmall black ichneumon fly, with a ſlender body, and very long antennæ; which darts its pointed tail into the bodies of the Aphides, at the ſame time depoſiting an egg in each. This egg produces a worm, which feeds upon the containing inſect, till it has acquired its full growth; when it is uſually changed to that kind of fly from whence it had its origin. In this, however, it is ſometimes prevented by another ſort of ſmall black fly, which wounds this worm through its pearl-like habitation; and by laying one of its eggs therein, inſtead of the former fly, produces its own likeneſs.

I muſt however further obſerve, notwithſtanding theſe inſects have many enemies, they are not without friends; if we may conſider thoſe as ſuch, who are very officious in their attendance, for the good things they expect to reap thereby. The ant and the bee are both of this kind, collecting the honey in which the Aphides abound; but with this difference, that the ants are conſtant viſitors, the bee only when flowers are ſcarce. To which let me alſo add, that the ants will ſuck in the delicious nectar, while the Aphides are in the act of diſcharging it from the anus; but the bees only collect it from the leaves, on which this honey-dew has fallen.

SECTION III.

In the autumn, I find three more generations of Aphides to be produced; two of which make their appearance in the month of Auguſt, and the third uſually before the middle of September. As the two firſt differ in no reſpect from thoſe which we meet with in ſummer, it would be waſting time to dwell any longer upon them; but the third, differing greatly from all the reſt, demands our giving it a more ſerious attention. Though all the Aphides which have hitherto appeared were females, in this tenth generation are found ſeveral male inſects; not that they are by any means ſo numerous as the females, being only produced by a ſmall part of the former generation. To which I muſt further add, that I have obſerved thoſe which produce males, previouſly to have produced a number of females; which in all reſpects reſembling thoſe already deſcribed,

ſcribed, I ſhall decline taking into any further conſideration.

The females have at firſt altogether the ſame appearance with thoſe of the former generations; but in a few days their colour changes from a green to a yellow, which is gradually converted into an orange-colour, before they come to their full growth. They differ likewiſe in another reſpect, at leaſt from thoſe which occur in the ſummer, that all thoſe yellow females are without wings. The male inſects are however ſtill more remarkable; their outward appearance readily diſtinguiſhing them, from the females of this and all other generations. When firſt produced, they are not of a green colour like the reſt, but of a reddiſh brown; and have afterwards, when they begin to thicken about the breaſt, a dark line along the middle of the back. Theſe male inſects come to their full growth in about three weeks time, and then caſt off their laſt covering; the whole inſect being after this operation of a bright yellow, the wings only excepted. But they ſoon change to a darker yellow, and in a few hours to a very dark brown; if we except the body, which is ſomething lighter coloured, and has a reddiſh caſt. They are all of the winged ſort; and the wings, which are white at firſt, ſoon become tranſparent, and at length appear like very fine black gauze.

The males no ſooner come to maturity, than they copulate with the females; in which act they are readily diſcovered, as they remain in conjunction for a conſiderable time, and are not eaſily diſturbed. The commerce between them continues the whole month of October, and may be obſerved at all times

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of the day; though I have found it moſt frequent about noon, eſpecially when the weather is moderately warm, with the ſun overcaſt. The females, in a day or two after their intercourſe with the males, I have obſerved to lay their eggs; which they uſually do near the buds, when they are left to their own choice. Where there are a number crowded together, they of courſe interfere with each other; in which caſe, they will frequently depoſit their eggs on other parts of the branches, or even on the ſpines with which they are beſet. I do not however find that the eggs produced by theſe inſects bear any proportion to the number of young ones which proceed from the females of other generations; not having obſerved any one inſect to produce more than two or three, and that in appearance with great difficulty.

Having now traced their progreſs through the different ſeaſons of the year, and obſerved the various metamorphoſes which they ſucceſſively undergo; I cannot help ſuſpecting the inſufficiency of human reaſon, in ſetting any ſcheme to which the different changes of inſects may be accurately reduced. Though the indefatigable Swammerdam ſeems to have been fully convinced that there is no inſect, whoſe changes may not be reduced to one or other of the four orders he has deſcribed; ſtill the inſect now under conſideration, having at different ſeaſons quite different appearances, cannot, I think, with ſtrictneſs be confined to any of them. In the ſpring they ſeem in ſome meaſure to coincide with the firſt order, though in ſummer thoſe with wings more properly belong to the ſecond; but in autumn,

autumn, the males may ſeem to come under one order, and the females under another; or, I ſhould rather think theſe inſects are not clearly reducible to any order.

SECTION IV.

Some of the inſects now under conſideration continuing to lay their eggs till the beginning of November, I chooſe to deſer giving a more particular account of them, till the ſeaſon for which they ſeem by nature to have been deſigned. Theſe eggs are of a regular oval figure, being about the tenth part of an inch in length, and the twentieth in breadth; which, though it may ſeem a very inconſiderable bulk, is certainly large for ſo minute an inſect. When they are firſt produced, their colour is green, but in a few days turns to brown, and by degrees becomes quite black. The covering of the eggs may be called thick, if compared with its ſmall ſize; which at firſt is rather of a yielding nature; but, after being expoſed to the air, ſoon contracts a greater firmneſs. If this covering is wounded, there iſſues forth a mucilaginous fluid, which is very tranſparent, and in appearance of a uniform conſiſtence. Theſe eggs adhere firmly to the branches on which they are depoſited, by means of ſomething glutinous wherewith they are beſmeared, and in a moſt ſurprizing manner reſiſt all the ſeverity of the winter.

Though I have juſt now obſerved, the contents of the eggs to have the appearance of an uniform fluid; that this cannot in reality be the caſe, ſufficiently appears from the Aphides they produce in the

the ſpring, without any other aid than the warmth of the ſeaſon. Nor is a ſingle inſect to be eſteemed the whole product of an egg, ſince it has been clearly ſhewn, that ten generations ſucceed each other; the firſt rudiments of which muſt have been originally in the egg, as the females have no communication with the males but in autumn. The wonder however becomes ſtill greater, when we conſider the number of individuals in each generation; this being, I am fully convinced, at a medium, not leſs than fifty. Whoever pleaſes to multiply by fifty, nine times over, may by this means form ſome notion of the great number of inſects produced from a ſingle egg; but will at the ſame time find that number ſo immenſe, as to exceed all comprehenſion, and indeed to be little ſhort of infinity. How far this can be reconciled with any theory of generation which the ingenuity of man has hitherto invented, may be a contemplation not altogether unworthy our curioſity, though I fear it will not turn out much to the credit of our reaſoning faculties.

The ancient doctrine of equivocal generation, as alſo that from an admixtion of the ſeminal matter of both ſexes, being now quite rejected by all modern naturaliſts; two other opinions ſeem to have ſprung up in their ſtead. While one party aſſerts, that the original organization of the fœtus exiſts in the ovary of the female, and that it is vivified by a ſubtile ſpirit in the ſpermatic fluid of the male; the other lays it down for a certainty, that the eggs of the female are only to be conſidered as a proper nidus, provided for the reception of thoſe minute animalcules, with which the male ſemen is found to abound. As the former opinion does not appear to have any certain fact to ſupport it, we may well ſuſpect an inſufficiency in the cauſe to produce the effect aſſigned; but, ſuppoſing it adequate to the production of one generation, who can conceive a ſubtile ſpirit to remain in force for ten generations, and that through all the various ſeaſons of the year? With regard to the latter, I muſt obſerve, that the animalcules of Leeuwenhock being compared with Malpighi's firſt rudiments of the chick, their reſemblance is not ſo ſtriking as to afford me the leaſt conviction: but ſhould we allow theſe animalcules requiſite to produce the firſt generation, how then are the ſubſequent nine generations produced without them? Not being able to anſwer theſe queries myſelf, nor expecting them to be readily anſwered by others; it ſeems moſt prudent to obſerve with diligence what nature does, without being over anxious to diſcover by what means. Let us reſt ſatisfied in admiring the wonderful effects of generation, while we refer the primary efficient cauſe to the eternal will and power of an Almighty Creator.

Read March 21, 1771.

XXIII. *Meteorological Obſervations at* Ludgvan *in* Mount's-Bay, Cornwall, 1770: *By* William Borlaſe, *D. D. F. R. S. Communicated by Dr.* Jeremiah Milles, *Dean of* Exeter, *and F. R. S.*

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Ombr.
			Med.	Inches.
January	Higheſt 28 30,51 Very high Loweſt 7 29,36	Snow from the 5th to the 9th; 10th, ſnow all gone. Violent ſtorm on the 11th; nine days calm; reſt miſty, with fogs, ſhowers, and wind. Eaſt and Weſt winds nearly equal, latter rather more; hard froſt on the 7th day only.	Higheſt 2 51 Loweſt 7 32 } $43\frac{18}{31}$	$3\frac{160}{1000}$
February	Higheſt 14 30,39 Loweſt 18 29,1	Calm 12 days; a violent ſtorm all night the 18th; a ſlight froſt only on the 24th; wind Weſterly 21 days.	Higheſt 14 51 Loweſt 24 36 } $44\frac{3}{28}$	3,300
March	Higheſt 2 30,6 Loweſt 10 28,71	Sleet, deep ſnow and lying, the 16th, 17th, 18th, 19th, 20th, with hard froſt; froſt, and hard ſnowing and windy, 23d, 24th; ſnow, hail, and froſt, 26th, 27th, 28th, 29th. Froſt and ſnow gone the 30th, with hazy miſts. Wind moſtly from the North and Eaſt, being to the Weſt as 19 to 12.	Higheſt 31 $51\frac{1}{2}$ Loweſt 21 32 } $41\frac{5}{31}$	4,040
April	Higheſt 29 30,31 Loweſt 6 28,90	Mixture of all weathers, but that of froſt and ſnow we had none. Wind 22 days from the North; reſt nearly equal.	Higheſt 30 54 Loweſt 9 39 } 46	3,400

May

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Med.	Ombr. Inches.
May	Higheſt 1 29,98 Loweſt 5 28,99	Firſt 7 days windy and ſtormy ſhowers; in the reſt 20 days of calm, moſtly fair. Wind 16 days from the Eaſt equally mixed with North and South.	Higheſt 28 60 Loweſt 3 $40\frac{1}{2}$	$52\frac{1}{31}$	3,000
June	Higheſt 14 30,7 Loweſt 19 29,17	Moſtly hazy and miſty. Fair, windy, rain and cloudy, nearly equal. Wind 26 days from the Weſt, chiefly mixed with the South.	Higheſt 9 61 Loweſt 20 49	$54\frac{29}{30}$	3,200
July	Higheſt 12 30,20 Loweſt 18 29.32	Calm 24 days, ſome hazy and miſty, but little or no rain. Wind Weſterly 22 days, its mixture moſtly with the South.	Higheſt 13 66 Loweſt 4 52	$58\frac{6}{31}$	1,950
Auguſt	Higheſt 4 30,10 Loweſt 20 29,62	18 calm days; reſt miſty, hazy, or fair. On the 6th at 8 P. M. much lightning; violent about a quarter paſt 9, with very rapid flaſhes, and loud thunder, till two A. M. when it began to rain with a violent flood for a quarter of an hour; afterwards, much rain, but no more thunder or lightning. Wedneſday the 8th, cloſe calm morning; about five A. M. thunder, with much lightning; about ſix rained violently, at nine cleared off, and a fair day. Thurſday morning the 9th, 2 A. M. lightning and thunder, but not ſo violent; wind Eaſterly; wind Eaſt and Weſt nearly equal, with North and South nearly equal.	Higheſt 6 71 Loweſt 31 55	$60\frac{1}{31}$	1,850
September	Higheſt 28 30,10 Loweſt 26 28,84	Weather moſtly variable, and of all kinds. On the 18th hazy and calm, A. M.; P. M. a ſtorm and hard rain; on the 25th and 26th a continued ſtorm to the 27th; the 28th calm with miſts; wind twenty-five days South, with a greater mixture of the Weſt.	Higheſt 6 63 Loweſt 22 53	$58\frac{1}{2}$	2,840

October

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Med.	Ombr.
October	Higheſt 7 30,24 Loweſt 22 28,69	Firſt eleven days calm and mild; next twelve days violent rains and windy; next eight days ſhowery, and rains. Wind twenty-five days Weſterly, with eighteen days mixture of the North; reſt equal.	Higheſt 1 60 Loweſt 22 43	$50\frac{20}{31}$	Inches. 6,550 at leaſt, the receiver being brim-full when looked to: whether overflowed uncertain.
November	Higheſt 4 29,94 Loweſt 8 28 44 Very low here.	Rain twenty-two days; reſt ſtormy, windy, and cloudy. Wind Weſterly twenty-ſix days; mixed moſtly with the South. An extraordinary fall of rain this month; on the 25th a ſtorm all night.	Higheſt 3 53 Loweſt 20 37	$47\frac{1}{30}$	7,250
December	Higheſt 1 30,0 Loweſt 23 29,20	26 days rain and ſhowers; much ſtormy weather. On the 18th in the evening, began a violent ſtorm from Weſt, and W. S. W.; the extreme was at eleven P. M. Wind twenty-ſix days blew from the Weſt; moſtly mixed with the South.	Higheſt 15 52 Loweſt 25 37	$45\frac{30}{31}$	4,100

Total Rain this Year 1770, at leaſt $44,\frac{640}{1000}$

XXIV. *Deſcription*

XXIV. *Defcription of a new Hygrometer*: *By Mr.* John Smeaton, *F. R. S.*

Read March 21, 1771.

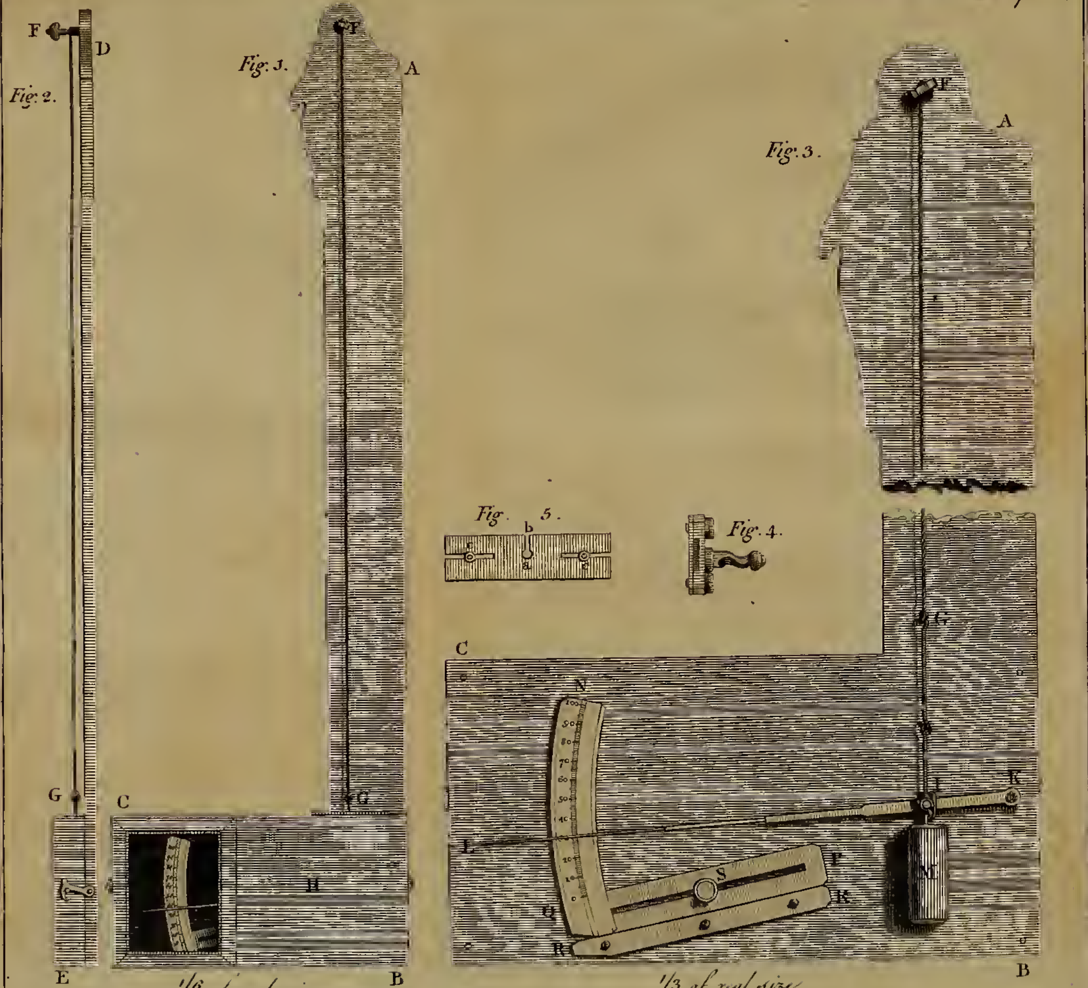
HAVING fome years ago attempted to make an accurate and fenfible hygrometer, by means of a hempen cord, of a very confiderable length; I quickly found, that, though it was more than fufficiently fufceptible of every change in the humidity of the atmofphere, yet the cord was, upon the whole, in a continual ftate of lengthening. Though this change was the greateft at firft, yet it did not appear probable that any given time would bring it to a certainty; and futhermore it feemed, that, as the cord grew more determinate in mean length, the alteration by certain differences of moifture grew lefs. Now as, on confidering wood, paper, catgut, &c. there did not appear to be a likelihood of finding any fubftance fufficiently fenfible of differences of moifture, that would be unalterable under the fame degrees thereof; this led me to confider of a conftruction which would readily admit of an adjuftment; fo that, though the cord whereby the inftrument is actuated may be variable in itfelf, both as to abfolute length, and difference of length under given degrees of moifture, yet that, on fuppofition of a material departure from its original feale, it might be readily reſtored thereto, and in conſequence that any numbers of hygrometers, ſimilarly conſtructed, might, like thermometers, be capable of ſpeaking the ſame language.

The two points of heat, the more readily determinable in a thermometer, are the points of freezing and boiling water. In like manner, to conſtruct hygrometers which ſhall be capable of agreement, it is neceſſary to eſtabliſh two different degrees of a moiſture which ſhall be as fixed in themſelves, and to which we can as readily and as often have recourſe as poſſible. One point is given by making the ſubſtance perfectly wet, which ſeems ſufficiently determinable; the other is that of perfect dry; but which I do not apprehend to be attainable with the ſame preciſion. A readineſs to imbibe wet, ſo that the ſubſtance may be ſoon and fully ſaturated, and alſo a facility of parting with its moiſture, on being expoſed to the fire to dry; at the ſame time that neither immerſion in water, nor a moderate expoſition to the warmth of the fire, ſhall injure its texture; are properties requiſite to the firſt mover of ſuch an hygrometer, that in a manner exclude all ſubſtances that I am acquainted with, beſides hempen and flaxen threads or cords, and what are compounded thereof.

Upon theſe ideas, in the year 1758 I conſtructed two hygrometers, as near alike as poſſible, in order that I might have the means of examining their agreement or diſagreement on ſimilar or diſſimilar treatment. The interval or ſcale between dry and wet, I divided into 100 equal parts, which I call the degrees of this hygrometer. The point of 0 denotes

denotes perfect dry; and the numbers increafe with the degrees of moifture to 100, which denotes perfect wet.

On comparing them for fome time, when hung up near together in a paffage or ftair-cafe, where they would be very little affected by fire, and where they would be expofed to as free an air as poffible in the infide of the houfe, I found that they generally were within one degree, and very rarely differed two degrees; but, as thefe comparifons neceffarily took up fome time, and were frequently interrupted by long avocations from home, it was fome years before I could form a tolerable judgement upon them. One thing I foon obferved, not altogether to my liking; which was, that the flaxen cords, which I made ufe of, feemed to make fo much refiftance to the entry of fmall degrees of moifture (fuch as is commonly experienced within doors in the fituation abovementioned) that all the changes were comprized within the firft 30 degrees of the fcale; but yet, on expofing them to the warm fteam of a wafh-houfe, the index quickly mounted to 100. I was therefore defirous of impregnating the cords with fomething of a faline nature, which fhould difpofe them more forcibly to attract moifture; in order, that the index might, with the ordinary changes of moifture in the atmofphere, travel over a greater part of the fcale of 100: how to do this in a regular and fixed quantity, was the fubject of many experiments, and feveral years interrupted enquiry. At laft, I tried the one here- after defcribed, which feemed to anfwer my intentions in a great meafure; and though, upon the whole, it does not appear



J. Smeaton delin.

Basire sc.

pear likely that this inſtrument will ever be made capable of ſo accurate an agreement, as mercurial thermometers are made to be; yet, if we can reduce all the diſagreements of an hygrometer within $\frac{1}{40}$th part of the whole ſcale, it will probably be of uſe in ſome philoſophical enquiries, in lieu of inſtruments which have not as yet been reduced to any common ſcale at all.

Deſcription of the Hygrometer.

Fig. 1 and 2, A B C is an orthographick delineation of the whole inſtrument ſeen in front in its true proportion.

D E is that of the profile, or the inſtrument ſeen edgeways.

F G, in both, repreſents a flaxen cord, about 35 inches long, ſuſpended by a turning peg F, and attached to a loop of braſs wire at A, which goes down into the box cover H, which defends the index, &c. from injury, and by a glaſs expoſes the ſcale to view.

Fig. 3. ſhews the inſtrument to a larger ſcale, the upright part being ſhortened, and the box cover removed; in which the ſame letters repreſent the ſame parts as in the preceeding figures; G I are two loops or long links of braſs wire, which lay hold of the index K L, moveable upon a ſmall ſtudd or center K. The cord F G is kept moderately ſtrained by a weight M, of about half a pound avoirdupoize.

It is obvious, that as the cord lengthens and ſhortens, the extreme end of the index riſes and falls, and ſucceſſively paſſes over

N 2 the ſcale, diſpoſed in the arch of a circle, and containing 100 equal diviſions. This ſcale is attached to the braſs ſliding ruler Q P, which moves upon the directing piece R R, fixed by ſcrews to the board, which makes the frame or baſe of the whole; and the ſcale and ruler, N Q P, is retained in any place, nearer to or further from the center K, as may be required, by the ſcrew S.

Fig. 4. repreſents in profile, the ſliding piece, and ſtudd I. (fig. 3.), which traverſes upon that part of the index next the center K; and which can, by the two ſcrews of the ſtudd, be retained upon any part of the index that is made parallel; and which is done for 3 or 4 inches from the center, for that purpoſe. The ſtudd is filed to the edges, like the fulcrum of a ſcale beam, one being formed on the underſide, the other upon the upper, and as near as may be to one another. An hook formed at the lower end of the wire loops C I, retains the index by the lowermoſt edge of the ſtudd, while the weight M hangs by a ſmall hook upon the upper edge: by theſe means the index is kept ſteady, and the cords ſtrained by the weight, with very little friction or burthen upon the central ſtudd K.

Fig. 5. is a parallelogram of plate braſs, to keep out duſt, which is attached to the upper edge of the box cover H, and ſerves to ſhut the part of the box cover neceſſarily cut away, to give leave for the wire G I to traverſe with the ſliding ſtudd (fig. 4.) nearer to, or further from, the center of the index K; and where in (fig. 5.) *a* is an hole about $\frac{1}{5}$ of an inch diameter, for the wire G I to paſs through, in the riſing and falling of the index,

freely

freely without touching; *b* is a ſlit of a leſſer ſize, ſufficient to paſs the wire, and admit the cover to come off without deranging the cord or index; *c c* are two ſmall ſcrews applied to two ſlits, by which the plate ſlides lengthways, in order to adapt the hole *a* to the wire G I, at any place of the ſtudd I upon the index K L.

Remarks on the preceding Conſtruction.

1ſt. In this conſtruction the index K L being 12 inches long, 4 inches from the extreme end are filed ſo narrow in the direction in which it is ſeen by the eye, that any part of theſe 4 inches, lying over the diviſions of the ſcale, becomes an index thereto. The ſcale itſelf ſlides 4 inches, ſo as to be brought under any part of the 4 inches of the index, attenuated as before mentioned.

2dly. The poſition of the directing piece R R is ſo determined, as to be parallel to a right line drawn through the point o upon the ſcale, and the center K of the index; conſequently, as the attenuated part of the index forms a part of a radius, or right line from the ſame center, it follows, that whenever the index points to *o* upon the ſcale, though the ſcale is moved nearer to or further from the center of the index, yet it produces no change in the place to which the index points.

3dly. When the divided arch of the ſcale is at 10 inches from the center (that is, at its mean diſtance) then the center of the arch and the center of the index are coincident. At other diſtances, the extremes of which are 8 or 12 inches, the center of the divifions and center of the index, pointing thereto, not being coincident, the index cannot move over fpaces *geometrically* proportionable to one another in all fituations of the fcale; yet, the whole fcale not exceeding 30 degrees of a circle, it will be found on computation, that the error can never be fo great as $\frac{1}{100}$th part of the fcale, or 1 degree of the hygrometer; which in this inftrument being confidered as an indivifeable, the mechanical error will not be fenfible.

Choice and Preparation of the Cord.

The cord here made ufe of is of flax, and betwixt $\frac{1}{20}$th and $\frac{1}{30}$th of an inch in diameter; which can readily be afcertained by meafuring a number of turns made round a pencil or fmall ftick. It is a fort of cord ufed in London for making nets, and is of that particular kind called by net makers *flaxen three threads laid.* I do not imagine that the fabrick of the cord is of the moft material confequence; but yet I fuppofe, when cords can be had of fimilar fabrick, and nearly of the fame fize, that fome fmall fources of variations will be avoided. In general I look upon it that cords, the more they are twifted, the more they vary by different degrees of moifture, and the lefs we are certain of their abfolute length; therefore thofe moderately twifted, I fuppofe, are likely to anfwer beft.

A competent quantity of this cord was boiled in one pound avoirdupoife of water, in which was put two pennyweights troy of common falt; the whole was reduced by boiling to 6 ℥ avoirdupoife, which was done in about half an hour. As this aſcertains a given ſtrength of brine on taking out the cord; it may be ſuppoſed that every fibre of the cord is equally impregnated with ſalt. The cord being dried, it will be proper to ſtretch it; which may be done ſo as to prevent it from untwiſting, by tying three or four yards to two nails, againſt a wall, in an horizontal poſition, and hanging a weight of a pound or two to the middle, ſo as to make it form an obtuſe angle. This done for a week or more in a room, will lay the fibres of the cord cloſe together, and prevent its ſtretching ſo faſt after being applied to the inſtrument, as it otherwiſe would be apt to do.

I have mentioned the ſizes and principal dimenſions that I have uſed; as the inſtruments may as well be ſimilarly conſtructed as otherways; but I do not apprehend it to be very material to agree in any thing but the ſtrength of the brine on taking the cord out of it. If the cord is adapted to the inſtrument ſome days before its firſt adjuſtment, I apprehend it will be the more ſettled.

Adjuſtment of the Hygrometer.

The box cover being taken off, to prevent its being ſpoiled by fire, and chuſing a day naturally dry, ſet the inſtrument nearly upright, about a yard from a moderate fire; ſo that the cord may become dry, and the inſtrument warm, but not ſo near as would ſpoil the fineſt linen by too much heat, and yet fully evaporate the moiſture; there let the inſtrument ſtay, till the index is got as low as it will go,

now

now and then ſtroking the cord betwixt the thumb and finger downwards, in order to lay the fibres thereof cloſe together, and thereby cauſing it to lengthen as much as poſſible: when the index is thus become ſtationary, which will generally happen in about an hour (more or leſs as the air is naturally more or leſs dry), by means of the peg at top raiſe or depreſs the index, till it lays over the point *o*; this done, remove the inſtrument from the fire, and having ready ſome warm water in a teacup, take a middling camel's hair pencil; and dipping it in the water, gently anoint the cord, till it will drink up no more, and till the index becomes ſtationary, and water will no more have effect upon it; which will alſo generally happen in about an hour. If in this ſtate the index lays over the degree marked 100, all is right: if not, ſlack the ſcrew S, and ſlide the ſcale nearer to or further from the center, till the point 100 comes under the index, and then the inſtrument is adjuſted for uſe: but, if the compaſs of the ſlide is not ſufficient to effect this, as may probably happen on the firſt adjuſtment, ſlack the proper ſcrews, and move the ſliding ſtudd I nearer to or further from the center of the index, according as the angle formed by the index, between the points of dry and wet, happeneth to be too ſmall or too large for the ſcale; the quantity can eaſily be judged of, ſo as the next time to come within the compaſs of the ſlide of the ſcale; the quantity of ſlide being $\frac{1}{3}$ of the length of the index, and conſequently its compaſs of adjuſtment $\frac{1}{3}$ of the whole variable quantity. Now as ſliding the ſtudd I will vary the poſition of the index

dex refpecting the point of *o*, this movement is only to be confidered as a rough or preparatory adjuftment, to bring it within the compafs of the flide of the fcale; which will not often happen to be neceffary after the firft time; but in this cafe, the adjuftment muft be repeated in the fame manner, by drying and wetting as before defcribed.

It is to be remarked, that, as the cord is fuppofed impregated in a given degree with common falt, and this not liable to evaporate, care muft be taken in wetting, that no drops of wet be fuffered to fall from the cord: for, by the obfervance hereof, the original quantity is preferved in the cord.

Obfervations made upon two original Hygrometers.

Thefe hygrometers were firft adjufted, after the impregnation of the cords with common falt, in February 1770; they were kept together in a ftaircafe till the fummer following; they were frequently obferved, and rarely found to differ more than one degree.

In fummer, one of them remaining in the former place, the other was removed into a paffage through a building; which having no doors, and the inftrument being hung fo that neither rain nor the direct rays of the fun could fall upon it, thereby it became expofed to the winds, and the free paffage of the open air. In thefe fituations the two hygrometers not only differed very greatly in quantity, but even frequently were moving different ways. They were thus continued till January 1771, in which fpace of time I obferved, that the moft ordinary place of the

index

index was between 15° and 25° in the open air; that at 40° the atmoſphere felt very ſenſibly moiſt; but yet it was frequently above 60°; and more than once at 70°, or very near. I have therefore marked the point of 0 *dry*; 20° the *mean*, 40° *moiſt*, 70° *very moiſt*, 100° *wet*. I do not, however, mean thoſe words (that of dry and wet excepted) as of any other intent, than that of general direction, in like manner as thoſe upon the barometer; leaving the relative degrees of moiſture to be judged of by the ſcale.

In the month of January laſt, I reſtored the expoſed hygrometer to its former place in the ſtaircaſe, when both inſtruments were again compared together; and they rarely differed more than 1 degree, and never ſo much as 2°. After this, they were both removed together to the out paſſage; and there they agreed nearly in the ſame manner, the utmoſt difference not exceeding 2 degrees. After ſome tryal here, one of them was readjuſted, leaving the other hanging in its place. On reſtoring the new adjuſted inſtrument to the other, they now differed about 5°, the new adjuſted one ſtanding ſo much higher. The day following the other was readjuſted alſo, and afterwards reſtored to its place with the former, which had been left in the out paſſage; and after this readjuſtment they both agreed to 1°. This being obſerved for ſome days, one of them was taken down, in order to be packed up for London; this I have now the honour of exhibiting to the Royal Society; and I beg to leave it in the Society's houſe, that in caſe any one ſhould be

deſirous

desirous of having an instrument made on the same plan, they may have recourse thereto.

It appears from the foregoing observations, that, in the compass of 11 months, the cords had stretched the value of 5°: and I also observed that they both had contracted their compass about 10°. I would, therefore recommend, that an hygrometer should from its first adjustment, be readjusted at the end of three months, and again, at the end of six months from the first; after that, at the interval of about six months, to the end of two years from the beginning; and after that, I apprehend that once a year will suffice; the best time of adjustment, being in the dry and warm weather of July or August: and by these means, I apprehend the instrument will be always kept within 2° of its proper point.

Respecting the sensibility of this instrument, it has that in a greater degree than its constancy to its scale can be depended upon, which was all that I intended; where greater degrees of sensibility are required, to make comparisons at small intervals of time, the beard of a wild oat, and other constructions may be used, with advantage; this instrument being considered as a cheque upom them as to more distant periods.

General Conclusion.

I am aware that an hygrometer actuated by any principle of the kind here made use of may not be a measurer of the quantity of moisture, actually dissolved in, and intimately mixed, with the air; but only indicates the disposition of the air to part with, or precipitate the water contained in its ſubſtance; or, on the contrary, to diſſolve and imbibe a greater quantity: but as it is by ſeparating the effects of natural cauſes, that we are enabled to judge of theſe cauſes, and from thence their effects when again compounded; every attempt to aſcertain the operations of a ſimple cauſe will have its value in the ſearch into nature: nor can we *a priori* determine the value of any new inſtrument; for, if it ſhould lead to a ſingle diſcovery, or even to aſcertain a ſingle fact, this may again lead to others of great importance, of which we might have, either none, or an imperfect idea of before. For my own part, I have always looked on a thick fog, and the ſweating, or condenſation of the water's vapours upon the walls in the inſide of buildings, to be the greateſt marks of a moiſt atmoſphere: whereas I have not always found the hygrometer affected at theſe times in the higheſt degree. On the contrary at the cloſe of a fine day, and the fall of the dew on the ſudden approach of a froſt, I have found the hygrometer more affected by moiſture than in ſome of the preceeding caſes; and ſtill more by a falling dew in the time of an hard froſt. I juſt mention theſe matters of hints for the enquiry of others; not having had length of time, ſince I brought the inſtrument to anſwer my intention, to make any abſolute concluſions.

I am ſorry I have been obliged to take ſo much compaſs, to deſcribe and explain a very ſimple inſtrument; but as I meant at the ſame time to give ſome idea of what is to be expected from it, I thought

it

it more excusable to be prolix than not sufficiently explicit.

London,
March 21, 1771.

J. Smeaton.

P. S. It is to be noted, that, after each readjustment, though the hygrometers would generally within a few hours come near their point, yet it was not till the next day that they could be depended on, as having come to their nearest agreement.

XXV. *Letter from Mr.* John Baptiſt Beccaria, *of* Turin, *F. R. S. to Mr.* John Canton, *F. R. S. on his new Phoſphorus receiving ſeveral Colours, and only emitting the ſame.*

Clariſſimo Viro

Joanni Canton, M. A. et Lond. Soc. Membro meritiſſ.

Joannes Baptiſta Beccaria, ex Scholiis Piis, S. P. D.

Read April 11, 1771.

THECAS plures confici curavi ex lamina ferrea cylindraceas intus nigerrimas. Operculum late pertuſum cryſtallo occluditur, colore in theca quaque diverſo. Singulis thecis offas immiſi ex phoſphoro calcareo-ſulphureo ſingulas omnia pares. Hæ clauſæ ſoli objiciuntur ſimul omnes; aſportatas in tenebras aperio, atque offam, quæ per cryſtallum viridem, video vireſcere; rubeſcere, quæ per rubram; flaveſcere, quæ per flavam cryſtallum lucem imbuit: videlicet conſit hoc experimento jam non quantam ſolum lucem ebiberit phoſphorus, ſed et qualem, eam ipſum unice emittere. Quod etiam experimentum Regiæ Societati obveniet fortaſſe non injucundum. Vale.

Summo Franklinio obſequium meo nomine, et ſalutem plurimam dicas rogo.

XXVI. *Some*

XXVI. *Some Remarks on the Effects of the late Cold in* February *last: in a Letter from the Rev.* R. Watfon, *Fellow of* Trinity College, *and Profeffor of Chemiftry at* Cambridge, *to* Mathew Maty, *M.D. Sec. R. S.*

Dear Sir,

Trin. Coll. Cam. March 21, 1771.

Read April 11, 1771.

ON the 12th of laft February, about an hour after fun rifing, I obferved at Cambridge a degree of cold which is very unufual in England, though common enough in more northern climates. Fahrenheit's thermometer, made by Dollond, as well in the open air, as when covered with fnow, ftood as low as 6° above 0. The Cam, by no means a rapid river, remained unfrozen; at the fides indeed there was a little ice, and fome fmall flakes floating in the middle. This is no very uncommon phænomenon. The Seine was not frozen at Paris in 1709, though the cold continued for two days one degree greater than in the prefent cafe. Various reafons have been produced, in order to account for this feeming deviation from the ufual courfe of nature. It hath been generally believed

that

that the ſtrong current in the Seine impeded the congelation: motion will certainly hinder the parts of fluid bodies from acquiring a regular arrangement; but it may be doubted whether it will wholly prevent their coaleſcence, in any caſe where the degree of heat is leſs than what would keep them fluid if they were quieſcent. We have frequent inſtances in chemiſtry, of ſaturated ſolutions of ſalts remaining perfectly fluid whilſt at reſt, and of forming thick coagulums upon the leaſt motion. Melted metals, glaſs, reſins, &c. appear to continue fluid for a longer time, after being taken from the fire, by having their parts moved, than if they are left at reſt; becauſe the ſuperficies which is expoſed to the air is conſtantly changing, and the whole maſs becomes uniformly cold and fixed at once, as ſoon as it has parted with the heat neceſſary for its fuſion. The moſt rapid rivers would probably experience a ſimilar change, did the cold in the atmoſphere continue long enough to be communicated to the whole body of the water: for upon taking the thermometer out of the ſnow, which laid upon the bank of the river, and immerſing it into the water, it ſuddenly roſe 26°, and ſtood at 32°, or higher; ſo that the air was very conſiderably colder than the water: nor is this at all to be wondered at, when we conſider that great degrees of cold may be ſuddenly produced in the atmoſphere by cauſes which do not immediately operate upon other bodies. Thus the influx of colder air from the northern latitudes, or the deſcent of that which always remains exceedingly cold in the upper parts of the atmoſphere in the ſame

ſame latitude, may in a few hours wholly change the air of a particular diſtrict: or, if from any peculiar circumſtance the air ſhould become unuſually dry, and conſequently diſpoſed to diſſolve much water, a great degree of cold might be almoſt inſtantaneouſly produced; but which could not be communicated to other bodies, in a little time, by ſo rare a fluid as the air.

During the forementioned degree of cold, a thick vapour was ſeen riſing from the ſurface, and marking as it were the courſe of the river. If we attribute the elevation of this vapour to the attraction of the air, rather than to the comparative warmth of the water (for water juſt beginning to freeze is obſerved not to loſe of its weight by evaporation *in vacuo)* the great cold may be thought perhaps to have proceeded from the ſolution of water in air which was then carrying on; for the earth was glutted with humidity, and the air was become dry, having been freed from its water by an almoſt inceſſant precipitation for three days, under the form of ſnow or ſleet. It is very remarkable, that the extreme cold of January 13, 1709, came on at Paris, with a gentle ſouth wind, and was diminiſhed when the wind changed to the north; this is accounted for by M. de la Hire, from the wind's having paſſed over the mountains of Auvergne to the ſouth of Paris, then covered with ſnow; and by Mr. Homberg, from the reflux of that air, which had been flowing for ſome time from the north. I do not ſee from what philoſophical principle it can be ſuppoſed, that the ſame air in its regreſs from a ſouthern latitude ſhould

be

be colder than in its progreſs from a northern; and as to the other opinion, the phænomenon of the cold's increaſing upon the wind's changing from north to ſouth, hath been taken notice of in other places, where there was no ſnow to refer it to. May it not deſerve to be conſidered, whether the ſudden ſolution of large quantities of aqueous vapours, brought from the ſouth into a dry northern air, be not a cauſe adequate to the effect produced? The ſolubility of water in air is diſtinctly mentioned by Dr. Halley, in the Philoſ. Tranſ. N° 192; and in the 6th Vol. of the French Encyclopedie, publiſhed in 1756; and more fully and ingeniouſly treated of by Dr. Hamilton in 1765: the cold attending the ſolution is a phænomenon ſimilar to that attending many other chemical ſolutions, and is in a leſs degree ſenſibly felt by every one who goes into a room newly waſhed, or ſtreet in the ſummer time lately watered.

Upon taking the thermometer out of the river, its bulb was quickly covered with a thin cruſt of ice, which defended it ſo much from the cold ſubſiſting in the atmoſphere, that it did not ſink two degrees in ten minutes; whereas, when it was wiped dry after immerſion in water, it ſunk above 20° in a leſs ſpace of time: this circumſtance ſhews that ice doth not tranſmit cold, and is explained by the experiments of M. Richmann, who hath eſtabliſhed it as a principle, that metallic ſubſtances are far more quickly affected in their dimenſions by the tranſitions from heat to cold, and the contrary, than any other bodies yet known.

Being defirous of obferving the effect of this extraordinary degree of cold upon various faline folutions, I haftened to my elaboratory, where I happened to have a great many folutions of falts corked up in quart bottles; the bottles were not all full, but the folutions were perfectly faturated; the ftate in which I found them is expreffed in the following table.

Frozen wholly	Frozen nearly	Wholly fluid
Alum	Green vitriol	Sea falt
Cream of Tartar	Blue vitriol	Sal gemmæ
Arfenic	Rochelle falt	Sal ammoniac
Corrof. fublimate	Glauber's falt genuine	Volatile alkaline falt
Borax	White vitriol, a few glacial fpicula	Fixt alkali per deliq.
Nitre		Epfom falts } Lymington.
		Glauber falts }

Thefe experiments agree upon the whole very well with thofe of profeffor Braunius, related in the Peterfburgh Commentaries for 1763: for, though his faturated folutions of Epfom falts, and of fixt alkali, had begun to freeze in a lefs degree of cold, yet it is probable that his Epfom falts might have been different from thofe manufactured at Lymington, and the folution of his fixt alkali not fo well faturated as that which is made per deliquium.

During the fame froft, I endeavoured to find out the powers, by which different falts, when they are diffolved in water, refift congelation. With this view I diffolved equal weights of falts, equally dry, in equal quantities of water, and expofed the folu-

 tions

tions, when they were arrived at the ſame degree of heat, in veſſels of equal and ſimilar figures to the ſame freezing atmoſphere; and accurately marking the times in which they began to freeze, I found them obſerving the following order: firſt alum, then Rochelle ſalt, green vitriol, ſugar refined, white vitriol, vitriolated tartar, Glauber's ſalt, mineral fixt alkali, nitre, blue vitriol, volatile alkali, ſal ammoniac, laſt of all, ſea ſalt. Theſe experiments were repeated once or twice with ſome attention; yet I would not be thought to propoſe the order in which I have arranged the ſeveral ſalts, as wholly to be relied on. It were to be wiſhed, that a ſufficient number of experiments were accurately made upon this ſubject; ſome general truths relative to metallic earths, and alkaline neutral ſalts, would probably be obtained therefrom, which, however unimportant in themſelves, might ſerve, upon ſome occaſion or other, as connecting links, to extend the chain of our ideas. By this compariſon of equal quantities of different ſalts diſſolved in equal quantities of water, we might be enabled to ſpeak with as much preciſion, concerning the powers by which they reſiſt congelation, as we do concerning thoſe by which they reſiſt putrefaction. I know not whether it may not be thought too curious a remark to obſerve, that the Ocean is impregnated with that ſpecies of ſalt which reſiſts congelation with the greateſt power, and in ſuch a quantity as tends not to preſerve entire, but to accelerate the diſſolution of the numberleſs animals which are daily dying in it. Beccher, it hath been aſſerted, was acquainted with this property of common ſalt;

but

but he seems only to speak of it as a far less efficacious anti-septic than sugar; at least, the honour of ascertaining the proportion in which it acts as a septic undoubtedly belongs to Sir John Pringle; for Beccher, in his *Physica Subterranea, lib.* I. *sect.* v. *cap.* 1. where he is speaking of this matter, says, "quod "nimius salis usus corpus putrescere faciat, sicut "modicus a putredine præservat."

To a table exhibiting the relative powers of neutral salts in resisting congelation, another might be usefully added, denoting the powers of all the known acids and alkalies when diluted to a given density; as also of vinous spirits, from highly rectified spirits of wine to water impregnated with the minutest quantity of spirit. Not but that it may be conjectured *a priori*, that in this last case the resistance to congelation would be directly as the quantity of spirit contained in given quantities of water. I made an experiment of this kind with sea salt; in equal quantities of water were dissolved quantities of sea salt, increasing in the arithmetical progression, 0, 5, 10, 15, 20, &c.; the times in which the solutions began to freeze, reckoning from the time in which simple water began, increased accurately in the same progression: hence it may be inferred, that, in salt of the same kind, the resistance to congelation is in the direct simple proportion of the quantity of salt dissolved; this conclusion cannot be extended to salts of different kinds, since water saturated with sea salt is more difficultly congealed than when saturated with various other salts, which it dissolves in greater quantities.

These obſervations, which are only propoſed as hints to thoſe who have more leiſure for experimental enquiries, you will be ſo obliging as to communicate to the Royal Society, or not, as you think proper. I am,

Dear Sir,

Your moſt faithful

and obedient ſervant,

R. Watſon.

XXVII. *A Letter from* Thomas Barker, *Esq; of* Lyndon *in* Rutlandshire, *to* James West, *Esq; Pres. R. S. concerning Observations of the Quantities of Rain fallen at that Place for several Years.*

S I R,

Lyndon, March 22, 1771.

Read April 18, 1771.

ON the other side is the quantity of rain, which has fallen at Lyndon in Rutland, since May 1736, with a table of the mean rain in the first four or five years, and every ten years since; which shews that there has been more rain in the latter part of this period, than in the former. But the least four years were from 1740 to 43, little more than 16½ inches a year; and the greatest four years from 1767 to 70, above 25½ inches a year. For comparing of dry seasons and wet ones, I have made a table of the three driest months, the three driest two months, three, four, &c. to twelve successive months; and a like table of wet ones: but as the years 1763, 68, 70, exceeded any others, I have made another like table of them. There are no three months come up to the last quarter of 1770,

$7\frac{1}{2}$ inches of which came in three weeks, from Nov. 6 to 26; but 1763 and 68, were wetter than 70, except thofe three months: and in this country 63 was the wetteft; yet, by what I heard, I fuppofe 68 exceeded it in many places. In common fpeaking, thofe are called wet years, in which the fummer, the growing feafon, was wet and cold; and thofe dry ones, wherein the fummer was dry and burning; fo that though 1740, 1, 2, and 3, had all but little rain, yet 42 and 43 were not properly called dry years, becaufe the ground never burned long together; and as the different degrees of heat, and frequency of rain, do not appear in this table, one cannot certainly judge, from the quantity of rain, which were the drieft fummers. Thofe complained of for dry, were, 1737, 40, 41, 50, 60, 62, and 65; but the hotteft and moft burning were 1750, 60, and 62; and 40 and 65 were cold and dry. On the other hand, the wet years were 1738, 39, 51, 52, 56, 63, and 66 to 70; but the wetteft 1751, 56, 63, and 68; and above all the laft quarter of 1770.

Feb. 12 laft, the thermometer abroad, was down at 4 of Fahrenheit's fcale, which is lower than I have obferved it in above 20 years paft; the loweft I had before obferved, was $10\frac{1}{2}$, Jan. 5, 1768. I have therefore given the rife and fall of the thermometer for above a week in the froft.

	Morn	Afternoon
Feb. 8	27 N.by W.	36 E.N.E.
9	26½ N.E.	29½ N.E.
10	28 N.E.	33 E.by W.
11	24½ E.S.E.	29½ S.W.
12	4 W.by N.	31 N.W.
13	15½ W.	30½ S.
14	9 S.W.	25 S.S.W.
15	12 S.W.	27½ E.
16	31 E.by W.	35½ E.

It was remarkable, that as long as the wind continued N. E. the froſt was moderate, when it turned S. W. it became very ſevere; and when the wind turned back into the Eaſt again, the froſt went away. This looks as if the weather was ſeverer Southward than here; as I think was likewiſe the caſe in Feb. 1754, which was alſo a very cold ſeaſon.

I am, Sir,

Your humble ſervant,

Tho. Barker.

…llon, mean rain at different periods

4 years / 5 years 36–40	10 years 40–50	10 years 50–60	10 years 60–70	34 years / 35 years 36–70
1.271	1.410	1.722	1.606	1.544
1.194	0.856	1.146	1.715	1.234
1.101	1.374	1.472	1.143	1.303
1.341	1.394	1.905	1.298	1.510
1.408	1.196	1.609	1.661	1.476
1.406	2.272	2.158	2.614	2.213
2.623	2.052	2.974	2.478	2.518
3.074	1.105	2.701	2.302	2.194
2.128	1.765	1.370	1.731	1.694
1.517	1.741	1.561	2.673	1.924
0.985	1.939	1.614	2.325	1.820
1.7[illegible]2	1.443	1.898	1.729	1.698
9.790	18.547	22.130	23.275	21.118

Annual quantities of rain.

1736	1737	1738	1739	1740	
	0.615	1.788	2.430	0.250	Jan.
	1.660	0.568	2.487	0.060	Feb.
	1.768	1.189	0.814	0.632	Mar.
	0.676	1.230	2.585	0.872	April
0.985	1.000	2.100	1.860	1.036	May
0.922	0.720	2.420	1.537	1.430	June
6.550	0.306	0.624	1.965	3.668	July
2.500	6.300	1.418	2.350	2.800	Aug.
1.540	3.465	2.110	1.903	1.620	Sept.
2.350	2.025	1.640	0.522	1.050	Oct.
0.620	9.570	0.692	1.557	1.488	Nov.
1.500	1.830	1.320	1.540	2.412	Dec.
16.967	20.935	17.159	21.660	17.318	

Annual

Annual quantities of rain at Lyndon.

	1741	1742	1743	1744	1745	1746	1747	1748	1749	1750	
Jan.	1.088	1.435	0.406	1.198	0.827	1.758	2.862	0.938	2.483	1.107	Jan.
Feb.	0.618	0.863	0.365	0.941	0.572	1.706	1.211	0.369	1.017	0.894	Feb.
Mar.	0.568	0.055	1.193	1.428	2.541	1.880	1.240	1.946	1.870	1.020	Mar.
April	0.270	1.908	1.252	2.759	1.708	0.762	1.017	1.367	0.548	2.348	April
May	0.441	1.546	0.868	1.257	1.137	0.546	2.829	1.178	1.107	0.995	May
June	1.366	1.430	0.379	3.479	3.451	2.900	1.562	3.044	3.039	2.069	June
July	0.873	3.136	5.230	0.820	0.724	1.442	2.248	3.484	1.049	1.510	July
Aug.	1.633	0.160	1.124	0.957	3.934	0.456	0.071	1.305	0.767	0.640	Aug.
Sept.	4.935	1.778	0.008	3.298	0.899	1.633	1.922	0.553	0.618	1.003	Sept.
Oct.	1.460	2.386	3.088	3.142	1.460	2.274	0.582	1.060	1.086	0.875	Oct.
Nov.	1.960	2.417	0.724	2.276	2.067	1.789	4.920	0.430	0.688	2.124	Nov.
Dec.	0.490	0.163	1.427	1.168	1.233	1.279	3.624	1.549	1.674	1.827	Dec.
	15.702	17.277	16.064	22.723	20.553	18.425	24.088	17.223	16.946	16.412	

Annual quantities of rain at Lyndon.

	1751	1752	1753	1754	1755	1756	1757	1758	1759	1760	
Jan.	3.098	2.518	1.692	0.925	1.021	2.016	2.144	1.867	0.876	1.062	Jan.
Feb.	0.924	1.377	1.841	0.887	0.835	0.689	0.594	2.060	0.379	1.872	Feb.
Mar.	2.046	1.203	1.172	1.247	1.657	1.370	1.905	1.792	1.874	0.452	Mar.
April	3.086	0.827	1.395	1.455	1.965	3.899	2.090	0.917	3.026	0.389	April
May	2.656	2.134	0.980	1.400	1.393	1.258	1.371	1.269	2.739	0.890	May
June	1.847	3.084	1.007	2.883	1.811	2.973	0.375	2.160	2.970	2.470	June
July	4.989	3.678	2.595	3.849	1.585	3.197	3.002	5.023	0.927	0.895	July
Aug.	1.580	1.334	3.380	1.060	2.258	4.257	6.057	1.711	3.729	1.644	Aug.
Sept.	2.614	0.480	0.706	0.107	2.546	2.080	0.518	1.465	0.854	2.333	Sept.
Oct.	1.819	0.295	1.458	1.866	1.628	1.528	1.954	1.032	1.500	2.531	Oct.
Nov.	1.338	1.090	2.112	1.960	3.138	0.975	1.498	0.912	0.980	2.134	Nov.
Dec.	1.161	3.127	3.865	2.218	1.408	0.944	2.175	1.386	1.085	1.613	Dec.
	27.158	21.147	22.203	19.857	21.245	25.186	23.683	21.594	20.939	18.285	

Annual quantities of rain at Lyndon.

1761	1762	1763	1764	1765	1766	1767	1768	1769	1770	
0.191	1.727	0.600	3.984	1.435	0.164	3.079	2.834	1.194	0.852	Jan.
1.469	0.968	2.882	1.134	1.240	2.102	2.002	3.062	1.556	0.736	Feb.
0.529	1.527	0.919	0.829	2.767	0.785	1.052	0.391	0.693	1.934	Mar.
0.490	0.595	0.692	1.524	2.111	1.955	0.845	2.023	0.843	1.900	April
2.035	0.738	2.304	1.095	0.406	3.286	2.123	1.622	1.451	1.553	May
3.487	0.764	2.426	2.182	0.788	2.279	2.163	4.521	4.769	2.765	June
0.566	1.119	5.657	4.624	0.582	2.363	3.682	2.402	1.994	1.788	July
3.614	3.615	2.929	1.770	2.805	0.409	1.527	1.720	2.360	2.270	Aug.
2.349	1.525	3.307	0.830	0.696	1.080	0.687	3.025	2.583	1.223	Sept.
3.685	4.154	1.606	1.359	4.842	0.829	2.822	3.119	1.202	3.114	Oct.
1.443	0.923	1.894	1.765	1.281	1.938	0.926	4.040	1.224	7.818	Nov.
1.541	0.233	3.525	2.398	1.048	1.776	0.400	2.146	1.608	2.613	Dec.
21.399	71.888	28.741	23.494	20.001	18.966	21.308	30 905	21.477	28.566	

Three drieſt ſeaſons from one month to twelve.

1 Month	Sept. 43	0.008	Mar. 42	0.055	Feb. 40	0.060
2	Jan. and Feb. 40	0.310	Dec. 42–Jan. 43	0.569	Feb and Mar. 40	0.692
3	Dec. 42–Feb. 43	0.934	Jan. — Mar. 40	0.942	Mar. — May 41	1.279
4	Jan. —April 40	1.814	Feb. — May 41	1.897	Dec. 42–Mar. 43	2.127
5	Jan. — May 40	2.850	Jan. — May 41	2.985	Dec. 42–April 43	3.379
6	Feb. — July 41	4.136	Dec. 42–May 43	4.247	Jan. to June 40	4.280
7	Dec. 42–June 43	4.626	Jan. — July 41	5.224	Dec. 39–June 40	5.930
8	Oct. 39–May 40	6.579	Jan. — Aug. 41	6.857	Nov. 42–June 43	7.043
9	Oct. 39–June 40	8.009	Nov. 36–July 37	8.865	Sept. 59–May 60	9.084
10	Sept. 39–June 40	9.912	Oct. 40–July 37	10.174	Aug. 42–May 43	10.988
11	Aug. 42–June 43	11.367	Sept. 40–July 41	11.794	Aug. 39–June 40	12.262
12	Aug. 40–July 41	13.427	Sept. 59–Aug. 60	14.093	July 39–June 40	14.227

Three wetteſt ſeaſons, from one month to twelve, except 1763, 1768, and 1770, which are below.

1 Month	July 36	6.550	Aug. 37	6.300	Aug. 57	6.057
2	Aug. Sept. 37	9.765	July Aug. 57	9.059	July Aug. 36	9.050
3	Aug. Sept. Oct. 37	11.790	July Aug. Sept. 36	10.590	June July Aug. 56	10.427
4	July — Oct. 36	12.940	April — July 51	12.578	June — Sept. 56	12.507
5	April — Aug. 56	15.584	Mar. — July 51	14.624	Aug. — Dec. 37	14.190
6	April — Sept. 56	17.664	April — Sept. 51	16.772	Aug. 37–Jan. 38	15.978
7	April — Oct. 56	19.192	Mar. — Sept. 51	18.818	May — Nov. 61	17.179
8	Mar. — Oct. 51	20.637	Mar. — Oct. 56	20.562	July 57–Feb. 58	19.131
9	Jan. — Sept. 51	22.840	Jan. — Sept. 56	21.739	July 57–Mar. 58	20.923
10	Dec. 50–Sept. 51	24.667	Nov. 55–Aug. 56	24.205	Nov. 47–Aug. 48	22.175
11	Nov. 50–Sept. 51	26.791	Nov. 55–Sept. 56	26.285	Aug. 37–June 38	23.545
12	Nov. 50–Oct. 51	28.610	Sept. 55–Aug. 56	28.379	Aug. 57–July 58	27.290

Wetneſs of the Seaſons in

	1763		1768		1770	
1 Month	July	5.657	June	4.521	Nov.	7.818
2	July Aug.	8.586	Oct. Nov.	7.159	Oct. Nov.	10.932
3	July — Sept.	11.893	Sept. — Nov.	10.184	Oct. — Dec.	13.545
4	June — Sept.	14.319	Sept. — Dec.	12.330	Sept. — Dec.	14.768
5	May — Sept.	16.623	June — Oct.	14.787	Aug. — Dec.	17.038
6	July — Dec.	18.918	June — Nov.	18.827	June — Nov.	18.978
7	July 63–Jan. 64	22.902	June — Dec.	20.973	June — Dec.	21.591
8	June — Jan.	25.328	May — Dec.	22.595	May — Dec.	23.144
9	May — Jan.	27.632	April — Dec.	24.618	April — Dec.	25.044
10	May — Feb.	28.766	Feb. — Nov.	25.925	Mar. — Dec.	26.978
11	May — Mar.	29.595	Jan. — Nov.	28.759	Feb. — Dec.	27.714
12	Feb. 63–Jan. 64	32.125	Jan. — Dec.	30.905	Jan. — Dec.	28.566

XXVIII. *Obſer-*

XXVIII. *A ſecond Letter from Mr.* Barker *to the Preſident, on the ſame Subject; together with the Determination of the Latitude of* Stamford, *in* Lincolnſhire.

S I R,

Lyndon, June 4, 1771.

Read June 13, 1771.

I AM glad the letter I ſent to you for the Royal Society, was thought worth their acceptance. I have, on the other ſide, ſent, as you deſired, the height my rain meaſurer ſtands above the ground, which, if you think proper, may be added to my former letter. Mr. Edward Lawrence, who obſerved the rain at Stamford part of the time which I have done here, generally found more water in his meaſurer which ſtood on the ground, than I did in mine; but I cannot depend on his obſervations, becauſe I have been told the ſervants at the houſe uſed to play him tricks, and pour into his ciſtern more water than fell in, to which a thing on the ground is very liable.

Mr. Lawrence alſo obſerved the latitude of Stamford with a quadrant of Mr. Siſſon's making; and as it is uſeful to preſerve ſuch things, I have extracted the obſervations from his book, and ſhewn the latitude deduced from them.

My rain ciſtern has all along ſtood on the top of a wall, where another meets it at right angles. The top of the ciſtern on the North ſide, is 7 feet 3 inches;

 on

on the Southweſt ſide, 8 feet 6 inches; and on the Southeaſt ſide, 10 feet above the ground; it is all open Southward for 25 yards; the North ſide is an orchard, but no tree hangs over it.

The latitude of Mr. Neal's houſe, at the South end of St. Martin's, adjoining to Stamford in Lincolnſhire, as taken by Mr. Edward Lawrence, in 1736.

1ſt, By the Sun's meridian Altitude Incl. of Eclipt. 23° 28′ 20″.

	°	′	″		°	′	″		°	′	″
May 1. Alt.	55	37	0	June 18 Alt.	60	35	0	Aug. 7 Alt.	50	19	50
☉'s Decl. N.	18	16	15	Decl. N.	23	14	28	Decl. N.	12	59	16
Alt. of the Equat.	37	20	45	Equator	37	20	32	Equator	37	20	34
6	56	47	26	July 7	58	20	30	11	49	0	18
	19	27	11		20	59	16		11	39	25
	37	20	15		37	21	14		37	20	53
13	58	24	30	10	57	46	53	18	46	34	0
	21	3	10		20	25	36		9	13	7
	37	21	20		37	21	17		37	20	53
31	60	28	31	15	56	44	0	Sept. 8	38	38	30
	23	8	36		19	22	45		1	18	1
	37	19	55		37	21	15		37	20	29
June 1	60	33	32	17	56	16	26	9	38	16	0
	23	12	24		18	55	16		0	54	34
	37	21	8		37	21	10		37	21	26
2	60	37	2	20	55	33	0				
	23	15	49		18	11	50				
	37	21	13		37	21	10	greateſt	37	21	26
9	60	49	0	26	53	58	0	mean	37	20	55
	23	28	12		16	36	56	leaſt	37	19	55
	37	20	48		37	21	4				

2d, By the Meridian Altitude of the Pole ſtar.

May

	° ′ ″		° ′ ″
May 14, Alt.	50 33 0	Oct. 29, below	50 33 30
below the Pole	2 5 45	above	54 44 30
Latitude	52 38 45	Latitude	52 39 0

	° ′ ″
Mean Latitude by the Sun	52 39 5
by three obfervations of the Pole ftar	52 38 55
Mean Latitude of the S. of St. Martin's	52 39 0
St. Mary's, the middle of Stamford, is half a mile further North, therefore its Latitude is	52 39 30

I remain, Sir,

With all proper refpect,

Your humble fervant,

Tho. Barker.

XXIX. *Obfer-*

XXIX. *Obſervations on ſome Bivalve Inſects, found in common Water, by Mr.* Muller, *of the new Academy of Sciences in* Bavaria, *and the Oeconomical Society at* Bern; *communicated by* R. H. A. Bennet, *Eſq; F. R. S.*

Read April 18, 1771.

THE name of Bivalve is given only to thoſe ſhell-fiſh, whoſe houſes are compoſed of two parts, ſuch as muſcles and oyſters. Few of theſe are to be met with in freſh water, whereas a vaſt number are inhabitants of the ſea. I am acquainted with no more than four different ſpecies, like the ſea bivalve; they are found in the waters of Fridrickſdal, near Copenhagen, and amongſt them one has hitherto eſcaped the reſearches of conchiliogiſts.

In return, nature has liberally ſtocked the ſame waters with ſmall inſects, much more perfect than the inhabitants of the ſea-ſhells, and likewiſe provided with a double ſhell. It is ſufficiently known, that muſcles and oyſters are animals extremely ſimple; ſince they want ſeveral of the moſt perfect organs, and conſequently enjoy life in an incomplete manner. The want of eyes, arms, legs, &c. obliges them to lead an idle life, deprived of all the advantages, which

which ariſe from ſight and motion. Nature, from which they received an habitation ſufficient to protect them from external injuries, ſeems to have fixed for life their abode to one dark ſpot. Our bivalve inſects, on the contrary, by opening their two folding gates, enjoy both ſight and motion, alternately dipping in the mud, and darting though their element the water; whenever they meet with bad company, they hide themſelves in their ſhells, and ſhut up the valves, which force and diſtreſs attempt in vain to force open.

I have diſcovered ſeveral different ſpecies of theſe animals in the waters of Friderickſdal, one only of which is known to the naturaliſts. Mr. Baker, of the Royal Society of London, is the firſt, that I know of, who mentions it; " he ſays *, that the " inſect ſwims very faſt; that it procures its nouriſh- " ment by means of a whirlpool, which it raiſes in " the water by means of its arms; that, upon meet- " ing with a ſolid body, it ſtops itſelf by means of " its feet; that upon the ſlighteſt touch it ſhrinks into " its ſhell; and laſtly that it bears much reſemblance " to a bivalve ſhell-fiſh." To this deſcription he joins a figure, which, though imperfect †, repreſents the inſect. *Linnæus* ‡, and Geoffroi || call it the *Monocle*, and without taking notice that Mr. Baker knew it already, they obſerve that its *antennæ* are compoſed of ſmall white threads; and that the ſhell is oblong, ſmooth, and greyiſh, round on one ſide, flat on the other,

* Microſcope made eaſy.

† Tab. XV. f. viii.

‡ *Fauna Suecica*, 1761; 2060.

|| Hiſtoire des inſectes, tom. ii. p. 657. 4°.

other, and nearly of the ſame ſize at each end. None of the above-mentioned writers have had the ſatisfaction of inſpecting the inhabitant of the ſhell, which indeed is very difficult. Now as this inſect bears a ſtrong likeneſs to the new ſpecies, which I am about to deſcribe, we ſhall take a view of both together.

As I was walking in the month of November 1767 along the ſhore, out of the Weſtern gate of Copenhagen, I ſaw in a ditch of freſh water, a *conferva* *, which I carried home with me. I immediately put a lump of it to dry upon the ſtove; after which, upon looking at it through a glaſs, I diſcovered here and there ſeveral ſmall white points, very ſmooth and ſhining. Theſe I took up upon the point of a pin, and on a cloſer view found them to be two valved ſhells hardly diſcernible. The hinge, together with the opening and figure of them, juſtified my opinion. I ſeparated the valves, and the riſing part of the hinge to the edge ſhewed them to be ſhells. I regretted that the inſect, a ſight of which was abſolutely neceſſary to rank them among the teſtaceous kind, had been deſtroyed by the heat of the ſtove. The froſt came on, and prevented my making any further enquiries. I ſhewed my ſhells to three naturaliſts of known abilities, who agreed in aſſuring me that they were of the muſcle kind. I had ſtill ſome doubt ariſing from the recollection of the inſect above-mentioned †, which I had found formerly: and I put off the deciſion, till

* Flora Fridrickſdalina, 1016.

† Fauna Fridrickſd. 851.

I had

I had ſeen the inhabitant alive. In the beginning of April 1768, as ſoon as the froſt broke, I got ſome more of the *confervas*, which I diſſolved in a glaſs of water without diſcovering the bivalve; nor had I any better ſucceſs upon trying the effect of the ſtove. During the ſpring, I continued my ſearch in the country, and found ſeveral ſpecies of bivalve inſects, which led me to think the inhabitant of the ſhell was like them. At laſt, in autumn, after I had given up my hopes, I found it in the Park, at the bottom of a ditch full of ſtanding waters. The tranſparency of the ſhell gave me an opportunity of examining the inhabitant; and the examination cleared up the doubt I had about its ſpecies.

The new ſhell is a bivalve; white, ſmooth, ſhining, and tranſparent, without the leaſt ſpot, hair, or down. Its figure is oblong, rounded at both ends, and the hinge ſomewhat ſinuated at the opening, and convex at the ſides, in ſuch a manner as, when ſeen out of water, it is very like the ſeeds of ſome plants; and this is common to all the ſpecies of this genus. The ſubſtance is coriacious, or like hardened glue; thin, and very brittle when dryed. When ſeen by the microſcope, ſome of them appear very like net work. The valves are equal, a little broader at one end than at the other, and ſomewhat flatted at the ſlope; they are not however more elevated at the opening than at the hinge, but rather the contrary; for on the inſide they ſhew another edge, leſs elevated than that of the outſide, and which grows leſs and leſs towards the hinge. I call by this name the place, where the valves join, though I have not been able to diſcover either the membrane or teeth,

 which

which ſeem to ſhut the valves in common ſhells. They are however ſtrictly joined to this place during the animal's life; which makes one think there is a ligament at the tail of the inhabitant, by which he ſhuts himſelf in. The length of the ſhell is half a line, and its greater breadth above a quarter of a line. That ſpecies mentioned by the above writers is three times longer before it comes to its full growth. It is hairy, though ſmooth to the naked eye, more indented at the ſlopes where the valves are projecting, and more depreſſed towards the hinge; it is opaque, and of a changeable colour. Some of theſe inſects are of a light and others of a dark green, marked with an oblique ſtripe of a lighter than the reſt. Some of theſe are bright, and others grey and dirty; but the down with which the ſhell is covered, and to which the dirt ſticks, is only viſible with the microſcope. I have examined ſeveral of theſe, at different ages, and at different times of the year, and have found them all rough; whereas every one of thoſe of the new ſpecies is ſmooth. I ſhall call this new ſpecies the *white ſmooth* bivalve, to diſtinguiſh it from another, the ſhell of which is white and rough; and from that of the above mentioned authors, which I call the *ſordid*, in alluſion to the dirty ſhell in which it is often found.

I have already obſerved how difficult it is to diſcover the ſhape of the inhabitants of theſe bivalves: however, the tranſparency of the *ſmooth white* one, gave me an opportunity of examining the lateral part of its inhabitant with the microſcope; and a happy accident, by which I catched the *ſordid* one at reſt upon the back of its own valves, enabled

abled me to examine its fore part through a glaſs. I ſuſpect that it was ſhedding its ſkin, and for this reaſon was quieter, and had its valves more open than uſually; be that as it will, I ſhall now deſcribe the remarkable animal I obſerved.

The head is broad towards the bottom, but decreaſes gradually in bulk, and terminates in a tapering point; it has on each ſide a ſmall long white thread, in the form of *antennæ*. The animal ſeems to lower and raiſe the point at pleaſure.

The *antennæ*, are about the length of the ſhell, and reſt on a tranſparent cylindrical baſis, which ends in white long capillary filaments. They appear to be ſtuck on at the extremity of the head, but in fact are tied to the ſides, as I have often obſerved the animal to lower the point of its head towards its breaſt, without the *antennæ* following the motion. The *ſmooth white* bivalve has five capillary threads at each *antenna*, four of which are at top, and the fifth ſomewhat lower. The *ſordid* appears to have ten at each *antenna*; in ſeveral, the *antennæ* appear yellowiſh, and their baſis ſeems to conſiſt of four rings.

It is by means of theſe *antennæ*, which are real fins, that the animal changes its poſition, from one place to another, being able to move them ſeveral ways; when it has a mind to move faſt, they are firſt extended ſtreightways, and appear like two briſtles; in an inſtant the threads are unfolded, and the animal ſwims with great quickneſs. As for walking, it ſometimes joins the threads, ſometimes unfolds only a ſingle one, and ſometimes ſcatters them about all together; ſometimes it bends them between the

 valves,

valves, which are opened towards the place of the eye; it often hides one or both of them under the breaſt between the four legs; theſe *antennæ* ſeem to afford as great an amuſement to the animal, as they do to the ſpectators.

At the place where the head joins the body, towards the border of the hinge of the ſhell, one may perceive a little black ſpot, which is the animal's eye. This extraordinary ſituation of the organ of ſight upon the neck ſeems aſtoniſhing; every thing that is new is ſo, but the ſurprize ariſes only from the narrowneſs of our ideas. Many people would give very ſpecious reaſons for this poſition; others might ſuppoſe, that if the Creator had conſulted us upon the matter, the eyes ſhould have been placed in a quite contrary poſition, towards the extremity of the head. How childiſh and weak would this be! What God does, is undoubtedly moſt perfect; and what he orders the beſt poſſible: but what we term final cauſes, are ſeldom any more than conjectures, though ſometimes they happen unexpectedly to be true. Some aquatic inſects have the eye in the forehead, others at the bottom, on the fore or back part of the head, at the ſide or under it; nay there are ſome, whoſe head conſiſts of the eye only. The plain reaſon to be given of the different poſitions of the principal organs, is at the ſame time the moſt probable, or at leaſt the moſt within our compaſs. The Governor of the world is pleaſed to give infinite variety to his works, and only obſerves the laws of uniformity in the generation of each diſtinct ſpecies.

The breaſt jets out a good deal towards the opening of the ſhell, and conſtitutes the greater part of the animal's body. The feet, mouth, and little briſtles are placed upon it.

There are four feet, whoſe poſition reſembles a good deal that of quadrupeds, only that their reciprocal bent is more marked. The two foremoſt are at the top of the breaſt, in the part where it appears moſt ſloped. I took them a great while for feelers, becauſe the animal employs them to touch things with; but another uſe it makes of them, together with the diſcovery of ſome true briſtles, makes me judge them to be legs. They are white, tranſparent, and jointed, bent towards the back legs, and terminated by two points in the ſhape of claws. The joints have very thin hair on the inferior part. The two hind legs are tied to the lower part of the breaſt. They are longer than the fore legs. Each joint has a couple of ſmall threads at the end, and each leg terminates in a claw ſomewhat lengthened; as to the reſt, they are like the fore legs, and bend towards them.

The bivalve inſect makes uſe of its claws, not only to walk upon the *conferva*, ſome parts of which are true labyrinths, and others foreſts to him; but likewiſe to remove the dirt, to ſeize its prey, and to faſten itſelf to other animals of its kind, or to neighbouring bodies.

Under the breaſt, and near the fore feet, is a black ſpot, which is the inſect's mouth; it is covered with a ſmall tranſparent ſkin, which opens in the middle, and ſhews a couple of jaws, marked with a very black ſpot at the place where they join. Between theſe jaws hang very ſmall white beards like thoſe of the *tipula*; and above theſe again, there appears a ſmall black tranſverſal line. About the mouth there are ſeveral other little beards, ſomewhat in the ſhape of feet, which are conſtantly in motion.

There is no doubt but that theſe ſerve to procure a free paſſage to the water, and to carry the food to the animal's mouth; which employment we can by no means aſſign to the hinder legs, as Mr. Baker, who did not ſee the parts concealed between the valves of the ſhell, has done.

The belly is almoſt as broad as the breaſt, but has ſcarce above half its length. The breadth decreaſes towards the tail. When ſeen from before, the belly appears compoſed of two conical lobes, marked in the middle with a black circle. It moves alternately to, and retires from, the breaſt.

The tail comes out between theſe two lobes; it is of the ſame length with the body, and conſiſts of two ſtreight white and tranſparent canals, which are joined together till towards the end, where they ſeparate, and each terminates in two curved points. Towards the middle of the tail, there is a little hard briſtle, upon each of the canals. The animal commonly keeps this hid under his breaſt and belly; nor have I ever ſeen it extend it, unleſs when upon the point of wanting the neceſſary water, when the animal brings it out, to put himſelf in an eaſier ſituation; after which, it is immediately drawn in back again.

Upon the back of the inſect, are likewiſe ſeen two large round bodies, which I take to be the *ovaria*.

No body, after this deſcription, will diſpute the ſuperiority which our bivalve inſect has over the bivalve

ſhell-fiſh,

ſhell-fiſh, by the wonderful conſtruction of its body, and the advantages which ariſe from it. But the difference of make is not the only one, ſince the ſhell too is formed in a quite different manner.

The ſeveral hypotheſes of naturaliſts, on the formation of ſhells, are known; ſome will have them increaſe by *intuſſuſception*, and others by *juxtapoſition*. This latter opinion, which M. de Reaumur patronized, and which nature ſeemed to juſtify, became, in conſequence, the moſt general; but if the friends of the other ſyſtem were thought to loſe their cauſe, it was only for want of obſerving with a ſufficient degree of accuracy the operations of nature, whoſe variety would have furniſhed them with inſtances in their favour. Our bivalve inſect offers one, which the deſertion of the old ſhell and the formation of a new one, in proportion as the animal grows, put beyond a doubt. The fact itſelf appears, not only from the obſervation of empty ſhells of different ſizes, which are to be met with in waters, and are nothing more than the ſpoils of our bivalve inſects; but, from the ſingular good fortune I had, in ſeeing one of the animals ſtrip itſelf, entirely, in my preſence, of the membrane of its ſhell, and of the exterior parts of its body, and ſhew itſelf at laſt before me abſolutely renewed. The *exuviæ* both of the ſhell and the animal's body were tranſparent as the brighteſt cryſtal. The joints of the *antennæ*, the briſtles, the feet, the ſmalleſt hairs, were more diſtinguiſhable than in the animal itſelf.

How infinitely ſmall are the organs, which, hid as it were in ſheaths and caſes, only become viſible when they are magnified ſome thouſands of times! and how many are there which eſcape the beſt microſcope!

In

In the cleareſt water that we drink, one can often ſee with the naked eye ſpoils of this inſect, joined to thoſe of its ſhell, floating along, like fine white cotton.

This adheſion proves that the body of the animal is joined to the ſhell by ſome ligaments, which poſſibly too may keep the valves to the hinge, as I conjectured above.

I have not yet ſucceeded in diſcovering the organs of generation ; nor have I ſeen the inſects in the act of copulation (which cannot be leſs extraordinary than that of the other ſpecies of the *monoculi*) : ſo that I can ſay nothing of their ſex. I have obſerved that they lay eggs, but this does not prevent their being likewiſe viviparous: I have ſeen other ſpecies of *monoculi*, ſome of which had their *ovaria* full of eggs, and others of little live beaſts, which at times they hatched, and at others put down in the ſhell.

The *ſordid* ſpecies is the moſt commonly met with ; one finds it all the year, even in the time of froſt, from under which I have often drawn it.

It is found in all pure waters, and even in the little ditches which are expoſed to be overflowed by the ſea. I have preſerved it from May to November, full of life and motion, in a glaſs of water, which I did not renew the whole time.

The *ſmooth white* inſect lives at the bottom of marſhes, and pools, in which the *conferva* I have mentioned grows.

As the entomologiſts have ranked the bivalve inſects under the genus of the *monoculi*, I am naturally led to ſay ſomething about this genus.

Syſtema-

Syſtematical writers have confounded aquatic inſects, very different, both in ſpecies and genus, under the general arbitrary name of *Monoculus*. They have not been contented with giving the ſame denomination to ſeveral ſpecies, whoſe properties and attributes did not at all correſpond with the known characters of the genus, but have likewiſe given as ſpecific marks thoſe which nature tells us are generic. I ſhall only mention at preſent the *ſordid*, which furniſhes me with a ſtriking example. M. Geoffroy, as well as Linnæus, has ranked it under the genus of the *monoculi*. According to the latter, the generic character of this, is to have two eyes and twelve feet, ſix of which are fixed; whereas the former gives it only one eye and ſix feet. Beſides the difference as to the number of eyes, my deſcription proves that the number of feet does not agree with this account. Let me add, that the particular make of the *antennæ*, the feet, the tail, and the whole body, give this inſect a claim to form a genus of its own. As to the ſpecific definition, *Antennis multiplicibus capillaceis*, *teſta bivalvi*, and whatever elſe is ſaid of it, if one excepts the colour only, belong equally to all my ſpecies with capillary *antennæ*, and conſtitute rather a definition of the whole genus, than of a particular ſpecies.

The ſame miſtake is to be met with in ſeveral other ſpecies brought under this genus; and the reaſon of it is that the authors, not having known more than four of all the different ſpecies, which I have reckoned up in the following table, have generalized the characters of theſe four, though they were not well acquainted even with theſe.

The *water parrot*, which is the beſt known, both on account of its colour, ſometimes red, which makes the vulgar believe that the water is changed into blood, and from the works of *Schæffer Baker*, *Geoffroy*, and *Swammerdam*, is repreſented by the latter as hermaphrodite, though it be different in ſex, and have the parts of generation double.

The knowledge of theſe inſects has been almoſt entirely neglected, though in reality very intereſting; not to ſpeak of their wonderful make, the difference of their motion, and their ſingular mode of copulation, are worthy of our enquiries. Let it be ſufficient to ſay, that we ſwallow them and their ſhells, either living or dead, both in our victuals and drink; ſo that I ſhould not be ſurprized, if ſome time or other they were found in our inteſtines, or in thoſe of beaſts, and ſeveral of our diſeaſes attributed to them.

I propoſe giving the deſcription and hiſtory of theſe inſects, with their figures drawn to the life, as ſeen by the microſcope: this I ſhall do in a work which I am projecting. To render it more compleat, I beg the favour of all naturaliſts to communicate their obſervations, which I ſhall not omit to give them the credit of, and at the ſame time, if they ſhould find any other ſpecies, to ſend them to me. It is very eaſy to tranſport theſe inſects, as they live very well in a ſmall quantity of water for ſeveral weeks, without a neceſſity of a change. With theſe hopes I have added a liſt of the ſeveral ſpecies, which I have met with in the waters of *Friderikſtall*. It is after having examined and compared them, at different intervals, at all ages, and in all ſeaſons of the year, that I venture to pronounce upon their ſpecific differences.

I ſhall

I ſhall take another opportunity of fixing the general ones.

Gens Monoculorum,

aquarum Friderichſdalenſium.

a Conchacei.

* Antennis capillaribus ſuperis: capite abſcondito.

1. Antennis binis: teſta ovata, tomentoſa. Fig. IV, V, VI.
2. Antennis binis: teſta ovata fuſca, ciliata.
3. Antennis binis: teſta ſubovata, candidiſſima.
4. Antennis binis: teſta reniformi, pellucida. Fig. I, II, III.
5. Antennis binis: teſta ſubreniformi, fuſca: faſciis tribus albis.
6. Antennis binis: teſta elongata; faſcia viridi.
7. Antennis binis: teſta antice truncata: ſtrigis nigris.
8. Antennis binis: teſta globoſa, glaberrima.
9. Antennis binis: teſta globoſa, faſciis tribus nigris.

** Antennis capillaribus inferis: capite exſerto.

10. Antennis binis: cauda inflexa: teſta globoſa.
11. Antennis binis: cauda inflexa: teſta oblonga.
    N.B. Duas tantum antennas omni ratione proſpicere licuit, etiamſi quatuor adeſſe vix dubitem.
12. Antennis quaternis: cauda truncata: teſta globoſa.

13. Antennis quaternis: cauda inflexa lamellata: testa ventricosa.
14. Antennis quaternis: cauda erecta: testa elongata.
15. Antennis quaternis: cauda inflexa: testa antice aculeata.
16. Antennis quaternis: cauda inflexa serrulata: testa ventricosa, mutica.
17. Antennis quaternis: cauda recta: testa univalvi.
N.B. Hi potius binoculi & ultimus quidem proprii generis.

*** Antennis ramosis: capite manifesto.

18. Antennis dichotomis: cauda inflexa: testa subrhombea mutica.
Pulex non caudatus Schæff. monog. t. 1. f. 9.
19. Antennis dichotomis: cauda inflexa: testa gibba quadrangulari.
20. Antennis dichotomis: cauda inflexa verrucosa: testa postice aculeata.
Pulex caudatus Schæff. monog. t. 1. f. 1—8.
21. Antennis dichotomis: cauda inflexa: testa antice serrulata, postice aculeo longo.
22. Antennis dichotomis: cauda inflexa: testa antice ciliata: corniculis porrectis longis.
23. Antennis dichotomis: cauda inflexa: testa antice pilosa: corniculis pendulis.
24. Antennis dichotomis: cauda inflexa appendiculata: testa postice acuta.
25. Antennis dichotomis: cauda inflexa appendiculata: testa antice aculeata.
26. Antennis dichotomis: cauda deflexa: testa mutica: corniculis porrectis brevibus.

27. An-

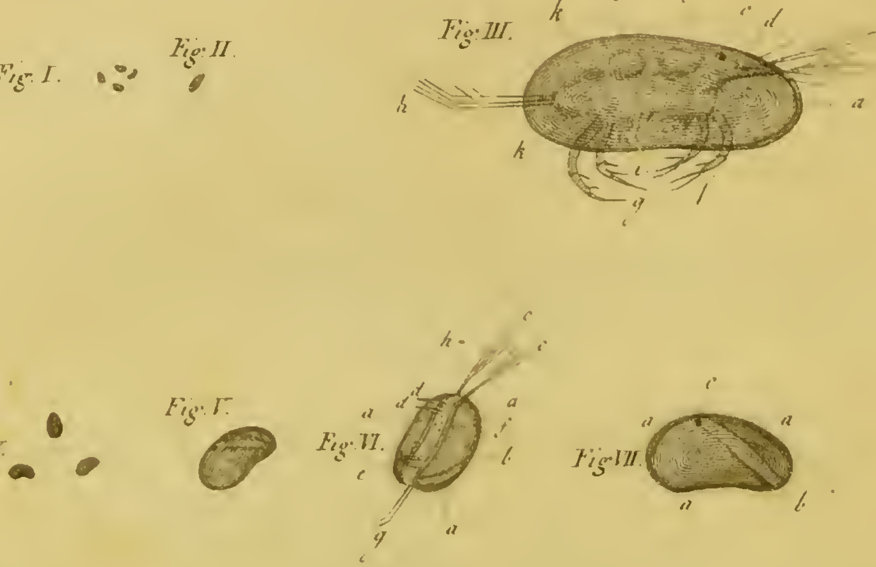
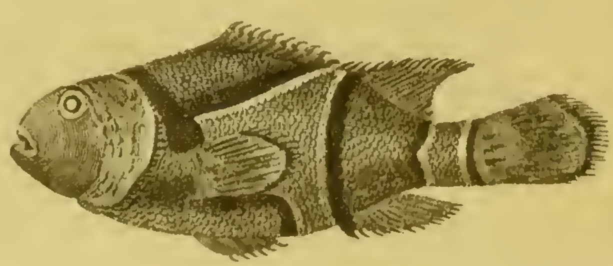


Fig. I. Fig. II. Fig. III. Fig. IV. Fig. V. Fig. VI. Fig. VII.

Fig. VIII.



M. Tyson del.

27. Antennis dichotomis: cauda recta: testa ovata mutica.
28. Antennis trichotomis: cauda recta: testa angulis anticis setiferis.

b Cataphracti.

* Antennis binis.

29. Antennis binis simplicibus: cauda recta bifurca.
30. Antennis binis simplicibus: cauda curva bifurca, laciniis pendulis.
31. Antennis binis simplicibus: cauda biseta.
32. Antennis binis simplicibus rigidis: cauda bifida.
33. Antennis binis dichotomis: cauda inflexa.

* * Antennis quaternis.

34. Antennis quaternis simplicibus: cauda recta bifida.

Baker microscop. t. 15. f. 1—4.

Hafniæ, 24 Nov. 1768. Otto Fridericus Müller.
Acad. Cæs. N.C. Scientiarum Boicæ,
ac Societ. Oecon. Bernens.

Explanation of the Bivalve Insect, Tab. VII.

Fig. I. The smooth white insect as it is naturally.
Fig. II. The same, seen through the magnifier.
Fig. III. The same magnified by the microscope.
The transparent shell shews the inhabitant lying at its full length; with the *antennæ*, legs and tail, out of the valves.

a The edges of the two valves.
b The *antennæ*.
c The eye.

d The

d The head.
e The *ovaria.*
f The fore legs.
g The hind legs.
h The tail.
i The fore part of the breaſt, where the beards and mouth are placed.
k The belly.

Fig. IV. The *ſordid* ſhell of its natural ſize.
Fig. V. The ſame, as ſeen through the glaſs.
Fig. VI. The ſame, with the ſhell a little opened, and more magnified.
a The rough ſhell.
b The oblique ſtripe.
c The *antennæ.*
d The fore legs.
e The hind legs.
f The mouth and joints.
g The tail.
Fig. VII. The ſame, with the ſhell ſhut.

XXX. *A Letter to the Rev.* M. Lort, *B. D. F. R. S. containing an Account of a ſingular Fiſh, from the* South Seas, *by the Rev. Mr.* Michael Tyſon.

Reverend SIR,

Read May 9, 1771.

THE Rev. Mr. Farmer, Fellow of Emanuel college, Cambridge, very obligingly lent me a curious fiſh, preſerved in ſpirits, which was brought by his relation Commodore Byron, from the new-diſcovered iſlands in the South Sea. As I have the greateſt reaſon to believe that it has never been figured or deſcribed by any author, and indeed never before ſeen in Europe; I have taken the liberty to ſend you the following deſcription and drawing of it (TAB. VII. fig. VIII.). I could not count the branchioſtegous rays, without greatly injuring the ſpecimen; but there is no doubt of its being one of the *Perca* genus of Linnæus. It is called by the Commodore the *Zebra* fiſh, he not knowing its proper name. The drawing is exactly meaſured from the real fiſh, and is in every part of the ſame ſize.

Piſcis

Piſcis thoracicus.

Perca * * * * * *.

CAPUT obtuſum, anticè nudum. Os aſcendens, labiis carnoſis marginatum, mandibula inferiore longiore. Dentes in maxillâ utrâque æquales, aceroſi, approximati. Sutura maxillarum utrinque obliqua, dentata.

Opercula branchiarum ſpinis ſerrato-ciliata. Nares unicæ, rotundæ, marginatæ.

Corpus ovatum, compreſſum, ſquamoſum.

Pinnæ baſi ſquamoſæ, margine nigræ, ramentis ultra radios porrectis. Dorſales 2 ſubunitæ: prima, rotundata, radiis 10 ſpinoſis, ſecunda angulata, radiis 16 mollibus. Pectorales rotundatæ radiis 14. Ventrales radiis 6. Analis angulata radiis 14, anticis 2 ſpinoſis. Caudalis rotundata 18.

Color griſeus. Faſciæ 6 nigræ tranſverſæ totum piſcem cingunt: prima per caput ducitur, pone oculos; ſecunda per operculorum marginem; tertia angulata, obliqua, inter pinnam primam dorſi atque anum; quarta recta ab unione pinnarum dorſalium ad ſpatium pone anum; quinta arcuata inter pinnam dorſalem ſecundam et pinnam analem; ſexta rectiuſcula in baſi pinnæ caudalis.

Diagnoſis.

Diagnosis.

Perca (* * * *) Pinnis dorsalibus subunitis, caudâ rotundatâ, corpore ovato: fasciis 6 transversis nigris.

I am,

REVEREND SIR,

Your most obedient

humble servant,

Bennet Coll. Camb.
March 11, 1771.

Michael Tyson.

XXXI. *An Account of* Elden Hole *in* Derbyshire; *By* J. Lloyd, *Esq; with some Observations upon it, by* Edward King, *Esq; F. R. S.; in a Letter to* Matthew Maty, *M. D. Sec. R. S.*

To Edward King, Esq;

Dear Sir,

Read Feb. 21, 1771.

THE inclosed is some account of Elden Hole, in Derbyshire; with the observations I made, upon being let down into it, in June last, at the time I was at Buxton-wells. If you think it any way curious, as a new account, you will be pleased to communicate it to the Royal Society.

I am, Dear Sir,

Your much obliged

humble servant,

Soughton in Flintshire,
August 4, 1770.

J. Lloyd.

A De-

A Defcription

of Elden Hole, in Derbyshire.

Having often heard, and feen, feveral accounts of the unfathomable depth of Elden Hole, in Derbyfhire, and being in that neighbourhood, I was inclined to make what enquiries I could about that noted place, of the adjoining inhabitanrs; who informed me, that about fourteen or fifteen years ago, the owner of the pafture in which this chafm is fituated, having loft feveral cattle, had agreed with two men for to fill it up; but they, finding no vifible effects of their labour, after having fpent fome days in throwing down many loads of ftones, ventured to be let down into it, to fee if their undertaking was practicable; when upon finding at the bottom a prodigious large cavern, they defifted from their work, as it would have been almoft impoffible to have procured a fufficient quantity of ftones to have filled it up.

Upon enquiry of one of thefe men whether there were any damps at the bottom; and being affured in the negative; I procured two ropes of forty fathom nearly in length, and eight men to let me down.

As the entrance is fo well known, I fhall fay nothing further of it, than merely, that it lies near North and South in its direction lengthways; and that the opening from one of thofe points to the other, at the ſurface, is about thirty yards, and eight or nine yards broad.

For the firſt twenty yards I was let down (which was at the South ſide), I could aſſiſt myſelf with my hands and feet, as it was a kind of confined ſlope; but after that, the rock jetted out into large irregular pieces, on all the three ſides next me; and on that account I met with ſome difficulty in paſſing, for about the ſpace of ten yards more; at which depth the rope was moved at leaſt five or ſix yards from the perpendicular. Thence down, the breadth was about three yards, and the length at leaſt five or ſix, through craggy irregular ſlits in the rock, which was rather dirty, and covered with a kind of moſs, and pretty wet, until I came within about twelve or fourteen yards of the bottom, and then the rock opened on the Eaſt ſide, and I ſwung, till I deſcended to the floor of the cave, where I perceived there was light enough came from the mouth of the pit (though at the diſtance of ſixty-two perpendicular yards) to read any print. When I was at the bottom, I perceived that the cavern conſiſted of two parts; the firſt (into which I deſcended, at the place I began to ſwing) being a cave, in ſhape not much unlike to that of an oven; and the latter, a vaſt dome of the form of the inſide of a glaſs-houſe; with a ſmall arched paſſage from the one to the other, through which a ſlope of looſe ſtones (that have been thrown in from time to time) extends from the wall at the Weſt ſide of the firſt dome, to almoſt the bottom of the ſecond cave, or dome, with ſuch an angle, that the further end of the cave is lower by twenty-five yards, than the place where I firſt landed.

The

The diameter of this cavern I take to be nearly fifty yards: the top I could not trace with my eye; but had reafon to believe it extended to a prodigious height; for, when I was nearly at the top of one of the incrufted rocks, at the height of (I dare fay) twenty yards, I could find no clofure of the dome, though I then faw much further than when I ftood at the bottom.

As to the particular curiofities to be met with in the fmall cavern, they are not worth mentioning; indeed I did not meet there with any ftalactitical incruftations whatfoever; but the wall confifted of rude and irregular fragments of rock.

Amongft the fingularities in the fecond cavern, I particularly obferved the following; climbing up a few loofe ftones on the South fide, at the place marked Q. (in the plan fig. II.), I defcended again, through a fmall flit, into a little cave, four yards long, and irregular, as to height not exceeding two yards; and the whole lined with a kind of fparkling ftalactites, of a fine deep yellow colour, with fome fmall ftalactitical drops hanging from the roof.

Facing the firft entrance is a moft noble column, of the fame kind of incruftation (fee D. fig. II. and IV.) which I could perceive to be above thirty yards high: and proceeding on to the North, I came to a large ftone (marked E. fig. II. and IV.) covered with the like matter; and under it I found a hole two yards deep, lined with the fame; from whence fprung a rock confifting of vaft folid round maffes, like the former in colour, though not in figure, on which I eafily afcended to the height of twenty yards, and got fome fine pieces of ftalactites, pendent from the cragged fides which joined this rock. At the upper part I

 perceived

perceived a ſmall hole, or cleft; but could not, without being in danger of my life, get at it; and I found great difficulty in coming down again.

After this, proceeding forward, I came to another pile of incruſtations, different from the two former, and much rougher; and which was not tinged with ſuch a yellow, but rather with a brown colour; and at the top of this alſo is a ſmall cavern, into which I went.

The laſt thing I took notice of was the vaſt drops of ſtalactites, hanging like icicles from every part of the vault; ſome of which were as large as a man's body, and at leaſt four or five feet long.

I obſerved the greateſt part of the walls of the large cavern was lined with incruſtations, and that they were of three kinds: the firſt, being the deep yellow ſtalactites; the ſecond, being a thin coating, like a kind of light ſtone-coloured varniſh upon the ſurface of the limeſtone, and which glittered exceedingly by the light of the candles; and the third being a ſort of rough effloreſcence, every minute ſhoot reſembling a kind of roſe-flower.

Having ſatisfied my curioſity with a view of this aſtoniſhing vault, I began to return (obſerving the whole floor to be covered with vaſt quantities of looſe ſtones); and reaſcending that heap, which I firſt mentioned, and ſo returning through the arch which ſeparated the two vaults, I perceived, that though it is now only about three yards high, yet it muſt formerly (before the ſtones were flung in), have been a very magnificent entrance.

Once more faſtening the rope to my body, I gave the ſignal to be drawn up, which I found to be a much more difficult and dangerous taſk than my deſcent,

descent, owing to my weight drawing the rope into clefts, betwixt the fragments of the rock, which made it stick; and to my body jarring against the sides, which I could not possibly prevent with my hands. Another circumstance also increased the danger, which was, the rope loosening the stones over my head, whose fall I every instant dreaded. As I was obliged to keep my face towards the side on which I was let down, I could not make any very particular observations on either of the rocks on each side of me, nor any whatsoever on the opposite one, except at a few resting places, either in my descent or ascent.

For the sake of conveying a clearer idea of the description, I have added two or three drawings, and a plan; which are as exact a resemblance of the place, as my recollection will enable me to give.

And, before I conclude, I ought to mention, that under the projection of the rock at A (fig. I.) where the passage first grows narrow, and which may with difficulty be seen from the top, is the entrance of a cavern, that seems to go a great way; but I could not get into it, and therefore am not able to say any thing further about it.

P. S. Since writing the above, I have been informed, that a gentleman, who lives near the spot, affirms, there was formerly the mouth of a second shaft in the floor of the great cavern, somewhere under the great heap of stones; and that it was covered up by the miners, at the time when so many loads were thrown in from the top. It is reported to have gone down a vast depth further, and to have had water at the

the bottom; but I did not perceive any remaining appearance of ſuch opening myſelf, nor did the miners, who went down with me, ſay any thing about it.

To Doctor Maty, Sec. R. S.

SIR,

Bedford-Row, Sept. 1, 1770.

I Have taken the liberty to ſend you, in conſequence of Mr. Lloyd's requeſt, his curious and exact account of Elden Hole in Derbyſhire: and I hope it will not be thought improper, if I venture to add a few ſhort obſervations upon it.

Mr. Lloyd, in his poſtſcript, mentions the report of there being a ſecond ſhaft, at the bottom of the great heap of ſtones: and when I was myſelf in Derbyſhire, about four years ago, and went to view the ſpot, I had an opportunity of receiving ſome information, from the wife of one of the miners, who had been down; and ſhe deſcribed the cavern in a manner agreeable to ſuch an account: for ſhe mentioned a very ſteep ſhelf, or deſcent, in the midway; at the bottom of which (ſhe ſaid) her huſband went down again a great way further, till he came to ſome water.

I do therefore conclude, that there really is ſuch a ſecond ſhaft; which having been covered up with large

large flag ſtones, or timber (probably by the miners) to facilitate, if poſſible, the filling up of Elden Hole, ſtill remains buried under the heap of ſtones. And I do alſo ſuppoſe, that the great ſlope of ſtones, which Mr. Lloyd deſcribes, is not entirely compoſed of looſe ſtones from time to time flung in; but that under them is the original ſhelf of ſolid rock, much ſteeper than the preſent ſlope, and ſomething in the direction S D fig. V. with the mouth of the ſecond ſhaft near the end of it. And this ſuppoſition, together with Mr. Lloyd's exact deſcription of the parts of the cavern which he ſaw, will perhaps reconcile all the accounts that have been given of this moſt aſtoniſhing chaſm.

For, ſtones flung down, or let down by ropes, in a proper direction, would certainly ſlide along the ſhelf of rock, and deſcend into the ſecond ſhaft, before it was covered up; whereas others would reſt at the bottom of the firſt ſhaft, or in the great cave: and hence the depths obſerved by different perſons, at different times, muſt have varied greatly from one another.

And if it be further conſidered, that, in ſounding ſuch great depths, the weight of the rope may often be miſtaken for the weight of the plummet; and that hence the rope may continue deſcending, and coiling up, firſt at the bottom, and afterwards at other places where it is accidentally ſtopped, till it be at length hindered in its deſcent by ſome projections of the rock nearer the mouth of the ſhaft; this will account for Mr. Cotton's letting down 884 yards; whilſt the water at the bottom of the ſecond ſhaft will account for 80 yards being wet; as ſo many

 might

might coil up in the water (let it have been ever so shallow), and as the reſt, beyond the real depth of the chaſm, might coil up either in the great or little cave.

Again, the many craggs on each ſide the firſt ſhaft, (and probably alſo on each ſide the ſecond) muſt retard any ſtone in its fall; and by that means will account for the length of time a body takes in deſcending; which muſt be a great deal longer than if it fell in open ſpace: and hence Dr. Short (who has given us a calculation, formed from the time of the deſcent of heavy bodies, according to the Newtonian principles of gravitation) was miſled to conclude, though very ingeniouſly, that this chaſm was 422 yards deep.

And laſtly; the falling of ſtones into the water, at the bottom of the ſecond ſhaft, and the increaſe of the ſound made thereby, partly from the reverberation at the ſides of the great cavern, and partly from the form of the upper ſhaft (which is not very unlike that of a ſpeaking trumpet, ſee fig. I.) might occaſion that aſtoniſhing noiſe, which is ſaid to have been heard at various times formerly, on throwing ſtones into this gulph; but which has not been heard of late years, in a manner at all agreeable to old reports.

And now, Sir, I cannot forbear to take notice, that as both Mr. Lloyd, and alſo the miner's wife, from whom I had my information, mentioned there being water at the bottom of the ſecond ſhaft, it appears highly probable, that this water is the continuation of a ſubterraneous river; and indeed of that very river which runs out of the mouth of the great cavern.

cavern at Caſtleton: for it is obſerved by the country-people in the neighbourhood, that there is a large quantity of grit ſtone grows in the earth near Eiden Hole, but none near Caſtleton; and yet, on high floods, the river at Caſtleton waſhes great quantities of fragments of that very grit-ſtone, out of the mouth of Caſtleton cavern.

There is alſo a tradition, which, however ridiculous it appears at firſt ſight, ought to have ſome little weight; eſpecially if compared with what Keyſler and Dr. Brown * relate of the Zirchnitzer ſea in Carniola. The tradition is this, that many years ago, a poor old woman, hunting her gooſe, it fled from her, and at laſt fell down into Elden Hole, to her great ſorrow; but ſome days after, ſhe heard it was ſeen at the mouth of Caſtleton cavern, and actually received it ſafe again from thence: the gooſe having, by the fluttering of its wings, preſerved itſelf from being daſhed to pieces in its fall; and having found its paſſage ſafely through the ſubterraneous river.

I have added theſe few obſervations, for the ſake of preſerving the tradition cocerning the ſecond ſhaft, which otherwiſe perhaps would very ſoon be loſt; and alſo for the ſake of ſhewing how great a probability there is of its being true: and to explain the matter more fully, I have ventured to add a fifth drawing, though merely from conjecture.

But before I conclude, I muſt beg leave to obſerve, that the diſpoſition of the maſſes of ſtalactites in this cavern, ſeems to me to deſerve ſome attention. Of the three great piles of incruſtations, two manifeſtly

* See Keyſler's Travels 8vo. Vol. IV. p. 140, and Lowthorp's Abridgement of the Philoſophical Tranſactions, Vol. II. p. 306.

defcend from two chafms (H and G fig. IV.) in the fides of the cavern; and therefore feem to have been formed by the water draining, and dripping at times, through thofe chafms, and carrying with it the ftalactitical matter: and it is remarkable that the pile (I, Fig. IV.) from the larger chafm, is coarfer, and rougher, and of a more earthy colour, than that from the fmaller chafm. But the third and largeft column of ftalactites, (D, Fig. IV.) has no chafm in the rock at its top; and is of a finer kind than the two others; and confifts of perpendicular fpires; whereas the others confift of large mif-fhapen lumps. And it is moft remarkable, that this ftands very near the end of the flope of ftones; and confequently, that fomewhere near it muft be the mouth of the fecond fhaft, if fuch really exifts.

As therefore Dr. Browne, in his travels (p. 96, 4to.) mentions ftalactites formed on the irons in the cupola of the baths at Buda, by the *exhalations* from the baths; and as fo many of the waters in Derbyfhire are warm, and mineral; I would beg leave to fubmit it to the confideration of the curious, whether this column, in particular, and the thin coating of ftalactites, on all the walls of the great cavern, mentioned by Mr. Lloyd, were not moft probably formed by *exhalations* from the fecond fhaft; whilft the other two columns, and the ftalactites pendent from the roof, were formed by water and ftalactitical matter tranfuding through the chafms above-mentioned, and through the pores of the ftone? I am, Sir,

With great refpect, your much obliged,

and moft obedient humble fervant,

Edward King.

Basire Sc.

Explanation of PLATE VIII.

Fig. I.

A Section of the Great Shaft, and First Cave.

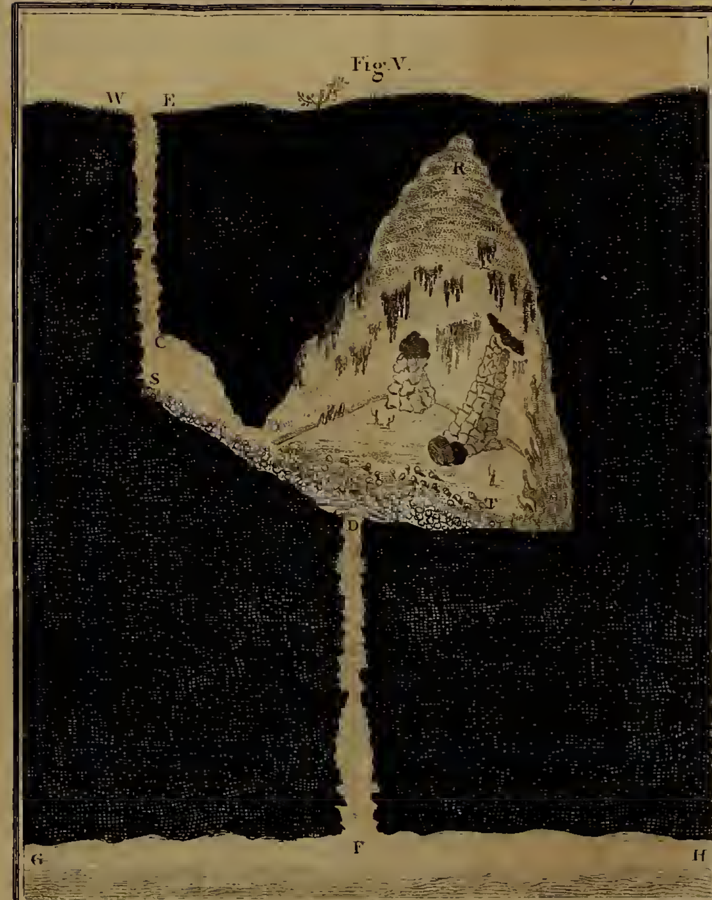
N S	The mouth of the Chasm lengthways from North to South.
S	The South end, at which I was let down.
A	The first great cragg, where the passage grows narrow. Opposite to it is a large projection of the rock, which may be just seen from the top: and under that is,
F	The entrance of a cavern, which seemed to go a great way; but I could not get into it.
C	A large projection of the rock at the bottom of the shaft. As soon as I had passed this, I swung; being come into the first Cave.
D E	The heap of stones, with which the whole bottom of the first cave is filled.
B	The arched passage into the great vault, being about three yards high.

Fig.

Fig. II.

The Plan of the Caves.

YZB	The floor of the firſt cave. Y The North end. Z The South end.
YZ	The flat wall of rock, on the Weſt ſide.
B	The entrance into the great cavern, on the Eaſt ſide.
BXQKVPI	The floor of the great cavern.
Q	A little, narrow, irregular cave.
D	The baſe of a column of ſtalactites, above 30 yards high.
P	The baſe of the rock of more ſolid and round maſſes of ſtalactites.
E	The great ſtone, which covers part of the mouth of
F	A pit, or hole, two yards deep, lined with yellow ſtalactites like ſtucco.
I	The baſe of a pile of encruſted ſtones, which lead up to a ſmall cave.
SK	The direction of the ſection fig. III.
XV	The direction of the ſection of the great cavern, fig. IV.



Basire sculp.

Fig. III.

AC The ſhaft where I was let down.

C The place where I began to ſwing, in the ſmall cave.

AS The perpendicular wall of rock on the Weſt ſide.

B The entrance into the great cavern on the Eaſt ſide.

RT The great Vault or Dome.

ST The heap of ſtones, which have been flung in from time to time.

I Large drops of ſtalactites, in cluſters.

Explanation of Plate IX.

Fig. IV.

The inſide of the great Cavern.

B The entrance on the Weſt ſide.

K The continuation of the vaſt heap of ſtones.

D The column of ſtalactites, above 30 yards high.

E The great ſtone which covers part of the mouth of

F A pit or hole, two yards deep, lined with ſtalactites like ſtucco.

P The rock of more ſolid and round maſſes of ſtalactites, on which I aſcended about 20 yards.

G A cleft in the rock at the top of the pile.

I A pile of incruſted ſtones, which leads up to a ſmall cave H.

L Large drops of ſtalactites, hanging in large cluſters.

R The roof (probably ſomewhat of this form) but too high to be ſeen.

Fig. V.

A View of all the Caverns, with the ſecond Shaft.

WE The mouth of Elden Hole. W The Weſt ſide. E The Eaſt.

C The entrance into the leſſer cave.

B The paſſage into the great cavern.

R The roof of the great cavern, reaching nearly to the ſurface of the ground.

ST The ſlope formed by the heap of ſtones.

SD The ſteep ſhelf of rock, probably, under it.

D The mouth of the ſecond ſhaft, which very probably ſtill exiſts; but is covered up, ſomewhat in the manner here repreſented.

F The bottom of the ſecond ſhaft.

G H A ſubterraneous river, at the bottom of the ſecond ſhaft; which probably communicates with ſome of the rivers in that great cavern at Caſtleton: a circumſtance there is reaſon to ſuſpect, from hence, that thoſe rivers, on great floods, are obſerved to caſt up ſmall fragments of a kind of grit ſtone, which grows plentifully in the parts of the country near Elden Hole; but is not to be found in the parts near the cavern at Caſtleton.

XXXII. *An Account of two new Tortoises; in a Letter to* Matthew Maty, *M. D. Sec. R. S.: By* Thomas Pennant, *Esq; F. R. S.*

S I R,

Read May 2, 1771.

I BEG the favour of you to lay before the Royal Society, an account of two tortoises that have just fallen into my hands.

The first was communicated to me * by my worthy and learned correspondent Doctor Garden, of Charles Town, in South Carolina; a gentleman to whom the world is indebted for various information relating to the natural history of that province; and whose assiduity promises fair to enable me to make considerable additions to the accounts already given of the new world. He has favoured me with an ample description of this new animal; together with some relation of its manners; both of which are now delivered to the Society in the words of the ingenious writer.

* The specimen now under the inspection of the Society, was lent me, by my good friend Mr. Ellis; my own specimen being in the country: to Mr. Ellis I was also indebted for the elegant drawing of the animal, done from the life, in South Carolina.

"I

"I now come to ſpeak of a ſpecies of Turtle or
"Tortoiſe, peculiar to our ſouthern rivers. We call
"it the *ſoft ſhelled* Turtle; becauſe, when alive, the
"covering looks like leather, very ſmooth and pli-
"able, without any appearance of bone in it. It is
"very ſwift and fierce. They are not commonly
"got here in Charles-town, though by chance
"this laſt ſummer, I had two ſent me. One of
"them I had preſerved entire and ſent to our friend
"Mr. Ellis; the other, leſs perfect, I have ſent you.
"This is a very curious animal, and I think, a non-
"deſcript, for there is none of Linnæus's fifteen
"ſpecies, that reſemble it, except the firſt; and
"that, he particularly mentions, is found in the
"* Mediterranean; but this always inhabits freſh
"waters, remote from the ſea. The head and ſnout
"are particularly diſtinguiſhed from every other
"Turtle; and what is more, I am told they ex-
"ceed any turtle in the delicacy of their taſte and
"flavour. I never eat any of them; but have heard
"many ſpeak of them who were great epicures, and
"they have aſſured me, that they were far preferable
"to the green kind."

* There are two ſpecies of Tortoiſes in that ſea, a coriaceous one, and another reſembling that of the Weſt Indies, which is ſcarce eatable. The laſt I procured from Leghorn, and at this time am doubtful whether it differs ſpecifically from the Weſt Indian Turtle.

Fresh Water Turtle, commonly called Soft Shelled Turtle. TAB. X.

" THEY are found in large quantities in Savannah and Alatamaha rivers; and I have been told that they are very common in the rivers in East Florida.

" They grow to very large sizes, though the largest that ever I heard of was seventy pounds.

" The Turtle, which I now have by me, weighs twenty pounds; and probably, when I first got it, it might have weighed from twenty-five to thirty pounds, as I have observed that it has grown poorer every day. I have had it now near three months, and I never could observe that it has eaten any thing that has been given it, though a variety of things have been tried.

" It is twenty inches long from one end of the shell or covering to the other, and fourteen inches and a half broad. The colour of this shell or covering, in general, is dark brown, with a greenish cast.

" The middle part is hard, strong, and bony; but all round the sides, especially towards the tail and hindermost part, it is cartilaginous, soft and pliable, resembling thick tanned sole-leather, yielding very easily to any force in any direction whatever, but thick enough and strong enough to defend the animal from any injury. All the hind part of the back is full of oblong smooth knobs; and the fore part, just where it covers the head and neck, is studded full of large knobs. The under side of this plate is very beautiful,

Fig. 1.

Fig. 4.

Fig. 2.

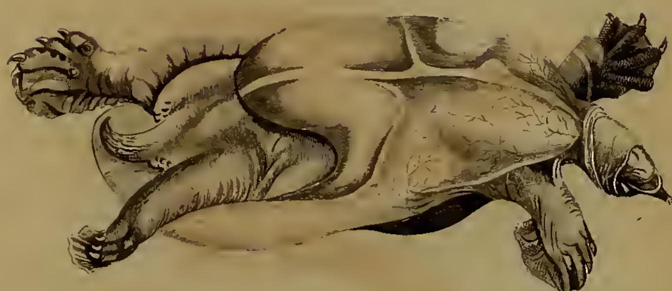


Fig. 3.

beautiful, of a lively whitiſh colour, interſperſed with innumerable very fine ramifications of blood veſſels, running from the margin of the plate into larger and larger branches, until the ſight of them is at once loſt by their entering the body of the animal.

" The under, or belly plate, or rather *ſternum*, is of a fair whitiſh colour, and extended forward two or three inches more than the back plate, ſo that the head reſts on it very conveniently. The hind part of this plate is hard and bony, ſhaped very much like a man's riding ſaddle, with two pieces for the thighs to reſt on. The fore part of the plate is pliable and cartilaginous.

" The head is ſomewhat triangular and attenuated, rather apparently ſmall for the animal, but growing gradually larger towards the neck, which is thick and long, and eaſily extended out (the neck of the preſent ſubject was thirteen inches and a half long) to a great length, or drawn back again under the ſhell or plate.

" The eyes are placed in the fore and upper part of the head, near to one another, having pretty large looſe palpebræ. The pupil is ſmall and lively, ſurrounded by a lemon-coloured *iris*, perfectly round, and giving much life and fire to the eyes. When danger approaches, or when it goes to ſleep, it covers its eyes, by bringing the inner and looſe part of the lower *palpebræ* over its eye, like a *membrana nictitans*.

" The upper lip and under lip are both large, but eſpecially the upper. The *mandibula* are both entire, each being one entire bone all round, of the ſame ſhape as the mouth.

" The

" The noſtrils are the moſt ſingular part, being a cartilaginous production of at leaſt three quarters of an inch, beyond the upper and fore angle or point of the upper lip, perforated with two apertures reaching back and opening into the roof of its mouth, having a ſmooth *ſeptum* but fimbriated upon each ſide. This, at firſt ſight, in ſome manner reſembles the ſnout of the mole; but it is tender, thin and tranſparent, and cannot be intended for digging in the earth or land.

" The arms are thick and ſtrong, conſiſting of three diſtinct joints, *viz.* the upper, the fore arm, and hand. The hands have each five fingers, of which the three firſt are ſhorter and ſtronger, and furniſhed with ſtrong nails, or rather claws. The two laſt fingers have more joints, but are ſmaller, and, inſtead of being furniſhed with claws, are covered with the membrane, which is extended even beyond their extremities. Towards the back or hind part, there are two ſpurious fingers, which juſt ſerve to ſupport the membrane when extended. The upper ſide of theſe arms and hands are covered with a wrinkled looſe ſkin, of a duſky greeniſh colour. The legs conſiſt of the ſame number of joints, and have the ſame number of toes as there are fingers on the fore-feet, and theſe are furniſhed with nails in the ſame manner, only there is but one ſpurious toe. Both the fore and hind legs are thick, ſtrong, and muſcular; and as the animal is very fierce, when it is attacked or diſturbed, it often raiſes itſelf on its legs, and will leap forward to bite its diſturber or enemy, which it does with great fury and violence.

" They

" They are likewiſe very ſtrong, and of a lively whitiſh colour, becauſe they are generally, if not always, covered with the upper plate, which, as I ſaid before, is extended a great way behind.

" The tail is large and thick, and generally as long as the hind part of the upper plate. The anus is placed about an inch from the extremity of the tail on the inſide.

" The Turtle, from which theſe characters were taken, was a female; after ſhe came into my poſſeſſion ſhe laid fifteen eggs, and about the ſame number were taken out of the belly when ſhe died. The eggs were nearly an inch diameter, and perfectly ſpherical.

" It is eſteemed very good eating, and ſaid by many to be more delicate than the green turtle."

The other Species of Tortoiſe, which I name

the Tuberculated,

was communicated to me by Mr. Humphries, of St. Martin's-Lane, merchant of minerals, ſhells, and inſects. He was unacquainted with its place and hiſtory; therefore I muſt content myſelf with giving a meer deſcription of it, deprived as I am of the knowlege of its manners and uſes, without which even natural hiſtory is as replete with dulneſs as with inutility.

Its length, from nofe to the extremity of the back, is three inches three lines; its greateft breadth, one inch and a half.

The head is large and fcaly. The neck thick and wrinkled. Eyes full; noftrils fmall and oval; the end of the upper mandible long and bifurcated, lapping very far over the lower.

The back is divided length-ways, with five prominent ribs covered with large yellow tubercles, the intervening part is dufky and divided by multitudes of leffer and more depreffed tubercules. The whole circumference of the back bounded by a tuberculated rib, like thofe on the upper part. The extremity furcated. The whole is coriaceous and pliant.

The tail is depreffed fideways, tapers to a point, and reaches beyond the end of the back.

The belly is yellow, tuberculated like the back, but marked with fix rows greatly prominent.

The prior fins are longer than the whole body, very thin, dufky, and edged on their interior fides with white, and both the furfaces are covered with depreffed tubercles. The hind fins are broad, much dilated near their end, and flightly bilobated: none of thefe fins had the leaft marks of toes or nails.

This may probably be the fame with the *Teftudo coriacea* of *Linnæus*, p. 350, or the coriaceous one above mentioned: but, as I have not at prefent before me the authors cited by that able naturalift, I will not pretend to pronounce with certainty whether it is the fame.

Expla-

Explanation of the Figures.

TAB. X.

Fig. 1. The ſoft-ſhelled Tortoiſe.
2. The ſame on its back.
3. The ſame with its neck exerted; drawn from the dried animal.
4. The tuberculated Tortoiſe.
5. Exhibits the form of the mouth.

XXXIII. *Meteorological Obſervations at* Caën *in* Normandy; *for* 1765, 1766, 1767, 1768, 1769. *By* Nathanael Pigott, *Eſq*; *communicated by the late Dr.* Bevis, *F. R. S.*

Read May 9, 1771.

1765.

Months	Days	Barometer	Inches	Mean	Ther.	Mean	Remarks
January	31	Greateſt height	30,20	29,65			Wind chiefly S. S. W. 15th froſt in the morning; the reſt of the month was cloudy.
	9	Leaſt ditto	29,10				
	8	Greateſt ditto			46,5	42	
	31	Leaſt ditto			37,5		
February	24	Greateſt ditto	30,14	29,505			Wind chiefly N. and E. N. E. 4th froſt and cloudy; 18th, 19th, 20th, 21ſt, 24th, 26th, ſmall froſt; 27th thaw; 28th rainy.
	28	Leaſt ditto	28,87				
	27	Greateſt ditto			44,5	39	
	19	Leaſt ditto			33 5		
March	8 23	Greateſt ditto	30 02	29,495			Wind chiefly S. S. W. 11th, 12th, 14th, 19th, 20th, 27th, 28th, high wind; 13th, 18th, 30th, 31ſt, ſtormy.
	1	Leaſt ditto	28,97				
	24	Greateſt ditto			51,0	45,5	
	1	Leaſt ditto			40,0		
April	12	Greateſt ditto	30,38	29,975			Wind variable; 1ſt ſtormy; 4th, 6th, 7th, 8th, 9th, 10th, 13th, 14th, 15th, 22d, high wind; ſky almoſt always cloudy: 2d ſome ſwallows: 26th ſome thunder: fruit trees begin to bloom.
	20	Leaſt ditto	29,57				
	27 28	Greateſt ditto			67,5	57,5	
	13	Leaſt ditto			47,5		
May	12	Greateſt ditto	30,24	29,90			Wind moſtly in the N. points: this month remarkable for fine ſun, and clear ſky.
	15 24	Leaſt ditto	29,56				
	22 23	Greateſt ditto			67,0	60 0	
	5	Leaſt ditto			53,0		
June	9	Greateſt ditto	30,22	30,00			Wind moſtly in the North points: weather changeable the whole month: 3d ſome thunder and lightening with heavy rain at 4h P. M.
	16 17	Leaſt ditto	29,78				
	4 5	Greateſt ditto			70,0	65,5	
	29 30	Leaſt ditto			61,0		

Month

Meteorological Obſervations, at Caën in Normandy.

1765.

Months	Days	Barometer	Inches	Mean	Ther.	Mean	Remarks
July	4	Greateſt height	30,28	29 99			Wind moſt in the northern points; beginning, cloudy ſky; middle, ſun with clouds; end, windy and ſun with flying clouds; 25th at 7h. P.M. therm. 77°.
	7	Leaſt ditto	29,70				
	19 25	Greateſt ditto			72,0	67,0	
	1	Leaſt ditto			62,0		
Auguſt	18	Greateſt ditto	30,30	29,66 +			Theſe obſervations are from the 10th to the 24th only. Wind variable; the beginning ſhowery; 12th ſtormy; end, ſunſhine and clouds.
	22	Leaſt ditto	29,03				
	23	Greateſt ditto			76,0	67,5	
	12	Leaſt ditto			59,0		
November	21	Greateſt ditto	30,27	29,645			Wind variable: beginning rainy; middle foggy; end, white froſt in the mornings.
	25	Leaſt ditto	29,02				
	1	Greateſt ditto			56,0	49,0	
	23 24	Leaſt ditto			42,0		
December	16	Greateſt ditto	30,34	29,73			Wind chiefly in the Eaſt points: beginning froſty: 5th hard froſt; 8th, 9th, 10th, 11th, rain: remainder, almoſt conſtant froſt.
	11	Leaſt ditto	29,12				
	8	Greateſt ditto			51,0	44,0	
	25	Leaſt ditto			37,0		

1766.

Months	Days	Barometer	Inches	Mean	Ther.	Mean	Remarks
January	29	Greateſt ditto	30,72	30,375			Wind chiefly E. S. E. from the beginning till the 20th, foggy and cloudy till the 30th; 31ſt, froſt, 29th, 30th, 72 inches: height of barometer carefully ſet down.
	2 3	Leaſt ditto	30,03				
	31	Greateſt ditto			40,0	33,5	
	11	Leaſt ditto			27,0		
February	19	Greateſt ditto	30 61	30,065			In the beginning wind in the North points; in the middle in the South; at the end to the Northern points; froſt till 11th; wind and rainy till the end.
	17	Leaſt ditto	29,52				
	15	Greateſt ditto			45,0	37,5	
	8	Leaſt ditto			30,0		

Meteorological Obſervations at Caën in Normandy.

1766.

Month	Days	Barometer	Inches	Mean	Ther.	Mean	Remarks
March	7 26	Greateſt height Leaſt ditto	30,29 28,84	29,565			Wind variable; 1ſt, 22d, ſtormy; 26th hurricane; 23d, 24th, 25th, wind and ſnow in the nights. The whole month moſtly cloudy: from 25th to 26th, quickſilver fell in barometer 0,67 inches.
	13 1 3	Greateſt ditto Leaſt ditto			53,0 40,0	46,5	
April	7 24	Greateſt ditto Leaſt ditto	30,40 29,35	29,875			Wind variable; moſt part of the month rain and wind.
	27 1	Greateſt ditto Leaſt ditto			57,0 48,0	52,5	
July	4 9	Greateſt ditto Leaſt ditto	30,08 29,69	29,885			Theſe obſervations from the 2d to the 16th only. Wind variable; cloudy and rainy.
	10 3	Greateſt ditto Leaſt ditto			70,5 63,0	66,75	
Auguſt	26 22	Greateſt ditto Leaſt ditto.	30,27 29,86	30,065			Wind at beginning in the North points; middle variable; end in the South points, Clouds and wind almoſt the whole month.
	9 17	Greateſt ditto Leaſt ditto			76,0 64,0	70,0	
September	17 7	Greateſt ditto Leaſt ditto	30,38 29,60	29,99			Wind till towards the end varying; from the 25th S. S. E. Clouds and wind the whole month.
	27 16	Greateſt ditto Leaſt ditto			72,0 60,0	66,0	
October	2 8	Greateſt ditto Leaſt ditto	29,84 29,27	29,555			Wind moſtly E. S. E. the end of the month variable; clouds and wind; 6th ſtormy: theſe obſervations from the 1ſt to the 10th only.
	5 9	Greateſt ditto Leaſt ditto			70,0 62,0	66	

Mete-

Meteorological Obſervations, at Caën in Normandy.

1767.

Months	Days	Barometer	Inches	Mean	The.	Mean	Remarks
January	21 13	Greateſt height Leaſt ditto	30,10 29,05	29,575			Wind moſtly S. S. E. this month froſty; ſometimes ſharp and high winds.
	30 13	Greateſt ditto Leaſt ditto			52,0 30,5	41,25	
February	1 2 8	Greateſt ditto Leaſt ditto	30,03 29,30	29,665			Wind moſt in the South points. Rainy and windy from the 9th to the 15th. Theſe obſervations only down to the 15th.
	1 5 10	Greateſt ditto Leaſt ditto			54,0 50,0	52,0	
May	24 30	Greateſt ditto Leaſt ditto	33,30 29,42	29,86			Wind varying the whole month. moſtly rainy and windy.
	16 4	Greateſt ditto Leaſt ditto			62,0 55,0	58,5	
June	10 3	Greateſt ditto Leaſt ditto	30,15 29,39	29 77			Wind varying. 3d ſtormy; the whole month windy and rainy.
	29 3	Greateſt ditto Leaſt ditto			65,5 55,0	60,25	
July	18 21	Greateſt ditto Leaſt ditto	30,02 29,63	29 825			Wind varying. 21ſt ſtormy; from the 11th to the 30th cloudy and rainy. Few flies yet in the appartments, and thoſe cannot fly.
	16 12	Greateſt ditto Leaſt ditto			67,0 63,0	65,0	
September	13 4	Greateſt ditto Leaſt ditto	30,16 29,62	29,89			Theſe obſervations only from the 1ſt to the 15th; wind varying; weather changeable.
	4 15	Greateſt ditto Leaſt ditto			72,0 64,0	68,0	

Mete-

Meteorological Obſervations at Caën in Normandy.

1767

Months	Days	Barometer	Inches	Mean	The.	Mean	Remarks
October	15 8	Greateſt height Leaſt ditto	30,16 29,68	29 92			Theſe obſervations from the 7th to the 20th only. Wind moſt in the North points, chiefly cloudy, ſome rain.
	8 14	Greateſt ditto Leaſt ditto			62,0 53,0	57,5	
November	28 15	Greateſt ditto Leaſt ditto	30,37 29,21	29.79			Theſe obſervations from 14th to 29th only. Wind chiefly in the S. points; 18th, 21ſt, 22d, ſun-ſhine and ſmall froſt. 16th the leaves ſtill green on the trees.
	27 23	Greateſt ditto Leaſt ditto			54,0 47,0	50,5	
December	3 20	Greateſt ditto Leaſt ditto	30,44 29,19	29,815			23d began to freeze very hard with N. N. E. wind. See an account of this froſt lower down. 1ſt, moſt trees have leaves, but yellow and begin to fall. 13th, the trees ſtripped of their leaves.
	8 27	Greateſt ditto Leaſt ditto			54,0 33,0	43,5	

1768

Months	Days	Barometer	Inches	Mean	The.	Mean	Remarks
January	5 8	Greateſt ditto Leaſt ditto	30,00 29.10	29,55			Wind moſtly in the Eaſt points. 1ſt, much ſnow: high wind; hard froſt to the 8th; 8th, thaws. The reſt of the month rainy and windy.
	30 6 7	Greateſt ditto Leaſt ditto			52,5 22,0	37,25	
February	6 10	Greateſt ditto Leaſt ditto	30,37 29,56	29,965			In the beginning wind in the N. points: at the end in the S. This month moſtly windy, cloudy and rainy.
	14 16 4 5	Greateſt ditto Leaſt ditto			57,0 44,0	50,5	

Mete-

Meteorological Obſervations at Caën in Normandy.

1768

Months	Days	Barometer	Inches	Mean	The.	Mean	Remarks
March	4 11	Greateſt height	30.39	29 705			Wind chiefly E. towards the N. greateſt part of this month froſty.
	15	Leaſt ditto	29 02				
	1	Greateſt ditto			57,0	50,5	
	11. 12	Leaſt ditto			44,0		
April	11	Greateſt ditto	30,16	29,795			Wind changing; 6th, ſtorm of thunder, lightening, and hail, at 5 h. P.M. lightning fell on the church, called Abbaie aux Dames, ran over the choir with quick ſerpentine motion, and then diſappeared without any further miſchief; 29th, rain and thunder at 11h. ½ A. M.
	29	Leaſt ditto	29,43				
	17	Greateſt ditto			61,0	56,0	
	1	Leaſt ditto			51,0		
May	23	Greateſt ditto	30,17	29,76			Wind chiefly in the N. points. This month was windy, and the ſun ſhone with white broken clouds. 5th, at 10 h. P.M. ſtorm of thunder, lightning and rain.
	29	Leaſt ditto	29.35				
	23	Greateſt ditto			70.0	63,0	
	2 19	Leaſt ditto			56.0		
June	21	Greateſt ditto	30,16	29,855			Wind chiefly in the Weſterly points. 7th, at 4 h. P. M. ſtorm of hail thunder and lightning; from 9th to 22d, windy and rainy; 24th, ſtormy, with rain thunder and lightning.
	26	Leaſt ditto	29,55				
	24	Greateſt ditto			69,0	65,8	
	20	Leaſt ditto			62,5		
July	21	Greateſt ditto	30,14	29,845			Wind chiefly in the Weſterly points. This month cloudy and windy, with ſome rain.
	7	Leaſt ditto	29,55				
	1	Greateſt ditto			73,0	69,0	
	20	Leaſt ditto			65 0		
Auguſt	9	Greateſt ditto	30,13	30,015			Theſe obſervations from the 3d to the 13th only. Wind moſt in the Weſtern points; ſky partly cloudy, partly clear.
	4	Leaſt ditto	29,96				
	12	Greateſt ditto			70,0	68,0	
	9	Leaſt ditto			66,0		

Mete-

Meteorological Obfervations at Caën in Normandy.

1768.

Months	Days	Barometer	Inches	Mean	The.	Mean	Remarks
September	14 12	Greateft height Leaft ditto	30,03 29,45	29 74			Thefe obfervations from the 7th to the 15th only. Wind unfettled. 12th, fome thunder in the morning; the other days wind, clouds or rain.
	14 8	Greateft ditto Leaft ditto			65,0 60,5	62,7+	
October							October 30th, betwixt 12 h. and 13 h. at Harcourt, about 5 leagues S. W. of Caën, feveral gentlemen faw a ball of fire of a whitifh colour, apparently of about a foot diameter, which caft a great light in the room, though the curtains were drawn, and there was a great fire and feveral wax lights in it: its direction from N. to S. It fell with great velocity, and feemingly about 30 or 40 yards from the room, without any explofion. I had juft left the apartment, but was immediately informed of it; about 3 hours after, there was a violent hurricane of wind, hail and rain.
November	19 23	Greateft ditto Leaft ditto	30,22 28,60	29,41			Barometer at noon the 21ft 29,43 ditto — — 22d 28,46 fell in 24 hours 0,97 22d, continued rain; at 10½ P. M. fmart flafhes of lightning with thunder. Wind varying this month and the weather cloudy, rainy and windy; 15th leaves begin to fall.
	15 30 23	Greateft ditto Leaft ditto			54,0 44,0	49,0	
December	12 2	Greateft ditto Leeft ditto	30,40 29,14	29,77			Wind chiefly in the Southern points. 2d ftorm of wind and rain; 14th hard froft. The weather this month unfettled.
	1 15	Greateft ditto Leaft ditto			52,0 32,0	42,0	

Meteorological Observations, at Caën in Normandy.

1769.

Months	Days	Barometer	Inches	Mean	The.	Mean	Remarks
January	14	Greatest height	30,16	29,825			Wind chiefly in the Southern points, cloudy, wind and rain; 24th white frost; 22d white frost: the weather this month unsettled.
	1	Least ditto	29,49				
	13	Greatest ditto			50,0	44,7 +	
	22 24	Least ditto			39,5		
February	20	Greatest ditto	30,19	29,725			Wind mostly in the Eastern points. 4th at 11 h. P. M. hurricane, morning generally white frost. From the 22d to the end of the month, rainy, cloudy, and high wind.
	5	Least ditto	29,26				
	28	Greatest ditto			53,0	46,0	
	1	Least ditto			39,0		
March	3	Greatest ditto	30,47	29,835			Wind in the Northern and West points. All this month clouds and wind; little rain.
	11	Least ditto	29,20				
	4	Greatest ditto			55,0	50,5	
	10 31	Least ditto			46,0		
April	24	Greatest ditto	30,13	29,605			Wind at beginning in the North points; at the middle in the South points; at the end in the North points; 22d, white frost in the morning; the rest of the month chiefly rainy and windy.
	8	Least ditto	29,08				
	28	Greatest ditto			62,5	54,2 +	
	2.3.4.5.6	Least ditto			46,0		
May	2	Greatest ditto	30,39	29.945			Wind varying; 18th, at 3 h. P.M. thunder; 20th at 4 h. P. M. rain with thunder and lightning; 23d, at 11 h. P. M. thunder and lightning; 23d, at 3 h. $\frac{1}{2}$ P. M. ther. 75°; 27th, at 4 h. P. M. a very heavy rain. This month mostly windy and rainy.
	14	Least ditto	29,50				
	23	Greatest ditto			73,0	63,5	
	13	Least ditto			54,0		
June	11	Greatest ditto	30,19	29,92			These observations from 2d to the 19th only; wind chiefly in the W. points. This month cloudy, windy and rainy.
	15	Least ditto	29,65				
	9.10.13	Greatest ditto			65,0	63,0	
	15	Least ditto			61,0		

Mete-

Meteorological Obſervations at Caën in Normandy.

1769.

Month	Days	Barometer	Inches	Mean	Ther.	Mean	Remarks
July	10	Greateſt height	30,16	29,60			Theſe obſervations from 6th to 16th only; wind variable; firſt days clouds; the laſt days clear ſun.
	8	Leaſt ditto	29,04				
	7	Greateſt ditto			76,0	72,5	
	10	Leaſt ditto			69,0		
Auguſt	18	Greateſt ditto	30,12	29,81			Theſe obſervations from the 14th to the 23d only. Wind chiefly in the Weſt points; clouds and wind.
	22	Leaſt ditto	29,50				
	15	Greateſt ditto			70,0	66,7+	
	21	Leaſt ditto			63,5		
September	16	Greateſt ditto	30,17	29,785			Shifting winds; 9th, at 3h. P. M. a very heavy ſhower of hail from the W. the hailſtones about the ſize of a middling walnut, of different irregular ſhapes; the largeſt of ſome, brought to me after the ſtorm, diminiſhed in volume, and weighed ſtill 2 pennyweights and 2 grains; the ſtorm laſted about 4 or 5 minutes.
	12	Leaſt ditto	29,40				
	6 19	Greateſt ditto			71,0	65,5	
	26	Leaſt ditto			60,0		
October	10	Greateſt ditto	30,30	29,98			Wind variable; weather unſettled; often changing.
	22	Leaſt ditto	29,66				
	19 20	Greateſt ditto			63,0	55,8	
	7	Leaſt ditto			48,6		
November	28	Greateſt ditto	30,52	30,035			Theſe obſervations from 18th to 30th only; wind ſhifting; 19th, 20th, hard froſt; the reſt of the month moſtly wind with ſome rain.
	22 23	Leaſt ditto	29,55				
	27	Greateſt ditto			55,0	47,5	
	20	Leaſt ditto			40,0		
December	4	Greateſt ditto	30,38	29.59			Theſe obſervations from the 3d to 14th only: the wind almoſt always in the South points; wind and rain; 13d, ſqualls at 8h. P. M. the barometer being at 28,69 inches.
	23	Leaſt ditto	28,69				
	23	Greateſt ditto			55,0	49,0	
	8	Leaſt ditto			43,0		

Mete-

Meteorological Observations at Caën in Normandy.

Inches 1765	Mean height of Barometer
29,650	January
29,505	February
29,495	March
29,975	April
29,900	May
30,000	June
29,990	July
29, 66+	Auguſt
	September
29,645	November
29,730	December
29,655	mean of theſe means
30,375	January 1766
30,065	February
29,565	March
29,875	April
29,885	July
30,065	Auguſt
29,990	September
29,555	October
29,992—	mean of theſe means
29,575	January 1767
29,665	February
29,860	May
29,770	June
29,825	July
29,890	September
29,920	October
29,790	November
29,815	December
29,790	mean of theſe means

Inches	Mean height of Barometer.
29,550	January 1768
29,965	February
29,705	March
29,795	April
29,760	May
29,855	June
29,845	July
30,015	Auguſt
29,740	September
29,410	November
29,770	December
29,828+	mean of theſe means
29,825	January 1769
29,725	February
29,835	March
29,605	April
29,915	May
29,920	June
29,600	July
29,810	Auguſt
29,785	September
29,98	October
30,035	November
29,590	December
29,805	mean of theſe means

Mean height	In the years
29,665	1756
29,922	1766
29,790	1767
29,828+	1768
29,805	1769
29,802	mean of theſe means

Remarks

Theſe obſervations were made at noon, in a South-Weſt room, with a barometer, whoſe tube is about $\frac{3}{10}$ of an inch diameter; in which the motion of the quick-ſilver, in ſqualls and guſts of wind, is extremely perceptible; yet, for further ſatisfaction, I ordered another to be made in London, with the greateſt care, by Heath and Wing, with a nonius giving the $\frac{1}{100}$ part of an inch. I placed this barometer by the other, in July 1769; and having compared them every day for a year, I find that the ancient one marks $\frac{5}{100}$ of an inch more than the other; therefore, if from the mean height as

above	—	29,802
be deduced	—	0,050
the true mean height is		29,752

The greateſt height obſerved, at noon, was, Jan. 29, 1766, 30,72.
The leaſt, Dec. 23, 1769, at 8 h. P. M.

Limits of the motion of quickſilver	—	2,03

Hence

Hence it appears, that if the mean height of barometers, on a level with the surface of the sea, be supposed, with Dr. Scheuchzer, Phil. Transf. N° 405, 406, — 29.993 inches
and the mean height at Caën, — — 29,752 ditto

0,241 the diff.

will be the greater mean weight of the atmosphere at the surface of the sea, then at Caën: and if, with the Doctor, we allow, for each tenth of an inch depression of the quicksilver, 90 feet elevation, my room, which is in the highest part of the town, will be about 217 feet above the level of the sea.

Monsieur de Luc, of Geneva, has given a method to measure the different elevation of places by barometers, grounded on his own observations, far more exact than any other before him; his rule is, "the difference of the logarithms of the height of "the quicksilver, in the two places, reduced into French lines, "and the logarithms carried to five places, including the charac-"teristicks, will give the difference of elevation in toises, if "Fahrenheit's thermometer be nearly at 66°; but about $\frac{1}{18}$ must "be deducted from the elevation so given, if the thermometer "be at 55° or temperate."

29,993 English inch. = 337,824 French lines log. 25286,8
29,752 ditto = 335,110 ditto log. 25251,9

diff. 34,9 toises

or 209 4 French feet = 223 English feet nearly; from which if $\frac{1}{18}$ = 12 $\frac{1}{2}$ nearly be deducted, 210 $\frac{1}{2}$ feet remain for the difference of elevation of my room and the surface of the sea, which differs 6 $\frac{1}{2}$ feet from the result given by the first hypothesis.

The greatest height observed, in these five years, with a good Fahrenheit's thermometer screened from the sun, in a S. W. room, was as follows at noon:

$$\left.\begin{array}{l}\text{1765 August 23d}\\ \text{1766 August 9th}\\ \text{1765 June 7th}\end{array}\right\}76°$$

the least height of ditto Jan. 6th 1768 — — 14

1765 August 23, exposed the thermometer, at noon, to the sun, suspended on a thread between two sticks, in the middle of my garden at Caën, which may be about two English acres, so that the thermometer received the least reflected heat possible in that place; the quicksilver stood as follows,

at

at 1^h P.M. 97°
at 2 ditto 96

1765 Auguſt 26, at a village, called *les Iles Bardelles*, 7 leagues from Caen, the ſame thermometer, in a South room, from which the ſun was excluded by the window ſhutters, roſe to 90°.

An Account of a remarkable degree of cold obſerved at Caën in Normandy.

1767	Ther.	Hours	Barometer
December	°	h ′	Inch.
24	+ 12	at 8 0	30,02
	+ 16	at 9 0	30,02
25	+ 18	at 11 0	
	+ 15	at 12 44	
	+ 18	at 21 45	
		at 22 00	29,9 little wind at E. S. E. ſmall fog. cloudy.
26	+ 15	at 10 00	
	+ 11	at 19 00	
	+ 12	at 21 00	
		at 22 00	29,67 wind N. E.
27	+ 17	at 7 00	
	+ 16½	at 8 00	
	+ 12	at 10 00	
	+ 11	at 11 00	
	+ 10	at 18 00	
	+ 10	at 20 00	
		at 22 00	29,49 no wind.
28	+ 15	at 8 20	
	+ 14	at 11 00	
	+ 13	at 19 00	
	+ 18	at 22 00	29,46 little wind S. S. E. flying clouds.
30	+ 23	at 7 00	
	+ 20	at 10 00	
	+ 11	at 19 00	
	+ 14	at 21 00	
		at 22 00	29,84 little wind S. flying clouds

An

An Account of a remarkable degree of cold, obſerved at Caën in Normandy.

1768	Ther.	Hours	Barometer.
January	°	h ′	
3	+ 10	at 11 00	
	+ 9	at 20 00	
4	+ 11	at 5 00	
	+ 10	at 6 00	
	+ 8	at 8 00	
	+ 6	at 9 15	
	O	at 12	
	— 8	at 19 30	
	O	at 21 0	30,03 clear ſky, no wind.
		at 19 0	30,00 wind W. by S.
5	— $2\frac{1}{2}$	at 5 30	
	— 2	at 6 30	
	— 2	at 8 00	
	+ 1	at 9	
	O	at 9 30	
	— 3	at 10 00	
	O	at 10 35	
	O	at 11 00	
	+ 3	at 19 30	
	+ 6	at 20 00	
	+ 7	at 21 00	
	+ 8	at 21 30	
	+ 10	at 22 15	
6	+ 14	at noon	
	+ 12	at 7 15	
	+ 14	at 8 45	
	+ 13	at 10 00	

N. B. The ſign + ſignifies, that the degrees marked by the quickſilver were above 0, or the beginning of the diviſion.

The negative ſign — ſignifies, that the degrees were below 0, or the beginning of the diviſions.

The thermometer was expoſed in the open air to the North.

The hours are aſtronomical hours.

Experi-

Experiments on ſome Liquids.

1768	H. M.	
Jan. 3	11 30	I placed a wine glaſs half full of rack, another half full of rum, in a North window, to the open air: the next day, at the ſame hour, the rack was frozen to a thick jelly, with icey particles; the rum alſo to a jelly, but leſs thick.
	4 50	I expoſed, as above, a wine glaſs, half full of cyder: ditto of beer.
		Ditto of red wine, called Beaujenci.
	5 13	The cyder began to freeze.
	6 30	Ditto conſolidated.
4	5 5	The beer began to freeze.
	6 30	Ditto conſolidated.
	5 12	The wine began to freeze.
	6 30	Ditto nearly conſolidated.
		The icey ſurface of the cyder being broken, it froze over again, in three or four ſeconds, the ice forming, with a progreſſive motion, very perceptible to the eye.
	9 00	I expoſed, as above, a wine glaſs half full of Malaga wine.
		Half ditto of Burgundy, called Migraine.
		Half ditto of Rouſſillon wine.
		Half ditto of ſpring water.
5		Half ditto of cyder-brandy.
	9 15	The water began to freeze.
	9 30	Ditto conſolidated.
	9 30	The Malaga, Migraine, Rouſſillon, began to freeze.
	11 00	Ditto ditto ditto conſolidated.

N. B.

N. B. The brandy did not freeze; but the 6th at noon, the quantity was diminiſhed, and ſome icey particles ſtuck to the glaſs, above the ſurface of what remained. I found, by weighing the above liquids hydroſtatically, in a temperate ſtate of heat, that the migraine, cyder, beaujenci, beer, and the water, were very nearly of the ſame ſpecific gravity; and that a piece of gold, which

weighed in the water	- -	224,25 grains,
weighed in the Malaga	- -	224,00 ditto,
in the Rouſſillon	- - -	224,50 ditto,
in the brandy	- - -	225,00 ditto.

XXXIII. Nyctanthes

XXXIII. Nyctanthes elongata, *nova Planta Indica, quam, descriptione atque icone illustratam, illustrissimæ Societati Regiæ* Londinensi *reverenter offert* Petrus Jonas Bergius, *M. D.* Suecus, *R. Soc.* Lond. *aliarumque Societ. Membr.*

Read May 9, 1771.

ETSI Botanicis plantarum in India orientali nascentium multa dudum messis fuit, tamen non dubitandum arbitror, quin adhuc satis amplus variis locus detur spicilegiis. Loquor jam potissimum de oris oceano vicinis, quæ plantarum curiosis identidem patuerunt; non de regionibus istius terræ ab oceano remotioribus, utpote quæ, a nullo botanico in hunc usque diem calcatæ, adeo non novarum minusve cognitarum plantarum offerrent spicilegia, si cuiquam oculatiori contingeret eas adire, ut potius integram atque abundantem earundem sine dubio præberent messem. Ut autem harum regionum gaza herbaria admodum foret exoptanda summoque nisu desideranda, ita nec illarum exilior forte jam residua supellex ullatenus est contemnenda, quin potius attentis, quoad liceat, oculis consideranda ab unoquoque, qui contemplatione naturæ, uti par est, delectatur.

Ego proinde cupide ac libenter excepi oblatas mihi aliquoties ab amicis, ex itinere Chinensi reversis, haud paucas stirpes, in diversis oceani Indici insulis

 deprehensas;

deprehenſas; quas ubi ſtudioſius contemplor, video-euq nonnullas raritate præſtantiſſimas, atque idcirco novas, quod Botanicorum antea innotuerunt nemini, congruens meis puto ſtudiis eas omnes impenſe examinare apteque deſcribere & delineare. Cæterum ne hæcce res juſto diutius duci per alias meas occupationes videatur, occaſionem ſectabor quam potero primam delicias haſce cum orbe erudito communicandi.

Patiatur itaque illuſtriſſima Societas Regia Londinenſis, quod ad acta ipſius nunc illico tranſmittam exactiſſimam iconem idoneamque deſcriptionem novæ cujuſdam plantæ Indicæ, quam ad Nyctantis genus amandare debui, etiamſi illius habitus ſatis abludit a cunctis dudum cognitis Nyctanthis ſpeciebus. Una quidem harum, nempe Nyctanthes multiflora, a Cl. *N. L. Burmanno*, *Flor. Ind. tab.* III. *fig.* 1. delineata, quodammodo cum mea convenit ipſo florum ſitu; ſed tamen valde ab ea diſcrepat, non ſolum florum magnitudine, ſed etiam foliorum figura & reliquo habitu, unde quoque diverſitas ſatis elucet ſpecierum.

Icon adjuncta (Tab. XI.) ramulum refert naturali magnitudine expictum; & vero ramulum ipſum accepi ab amico *C. G. Ekeberg*, ex itinere in Chinam domum non ita pridem feliciter redeunte.

Nyctanthes *(elongata)* foliis cordato-lanceolatis acutis elongatis minoribuſque, ramis teretibus.

Descr. *Caulis* fruticoſus. *Rami* ſubprocumbentes, oppoſiti, teretes, inferiores glabri, ſuperiores villoſi, ramoſi: ramulis oppoſitis. *Folia* oppoſita, cordato-lanceolata, ſubbipollicaria, acuminata, integerrima, utrinque glabra, nervoſa, margine paululum undulata, ſaturate viridia; inferiora ramulorum ſenſim minora,

2 Nyctanthes elongata.

minora, infima vero cordato-ovata, parva. *Flores* in ramulis terminales, 5 vel 6 congesti, subumbellati, breviter pedicellati. CALYCIS perianthium monophyllum, tubulatum, minimum, persistens, sex vel septemfidum: laciniis subulatis, pilosis. COROLLA monopetala; *Tubus* cylindricus, striatus, longus, pollicaris, superne incrassatus; *Limbus* planus, octo vel novempartitus: laciniis ovato-oblongis, acutis. STAMINA, *Filamenta* bina, brevia; *Antheræ* lineares, obtusæ, utrinque sulcatæ, intra tubum corollæ occultatæ. PISTLLUM. *Germen* subrotundum, truncatum, retusum, glabrum. *Stylus* filiformis longitudine staminum. *Stigma* incrassatum, bifidum.

 XXXIV. *Account*

XXXIV. *Account of a Mole from* North America: *In a Letter to Dr.* Maty, *Sec. R. S. from the Hon.* Daines Barrington, *F. R. S.*

Dear Sir,

May 15, 1771

Read May 15, 1771.

I Send herewith a mole from North America, which Mr. Kuckahn (who hath before preſented ſeveral birds and inſects to the Society) deſires they will do him the honour to place in their Muſeum.

This ſpecies of mole much reſembles that of Europe in its general appearance, except in point of colour: to ſhew, however, that there is a very material and ſpecific difference between the two animals, I have incloſed the head of the common Engliſh mole, which contains all the teeth belonging to each jaw.

The American ſpecimen is not indeed ſo perfect in this reſpect; but a ſufficient number of teeth remains, to ſhew the diſtinction between theſe two ſorts of moles.

In the European, you will obſerve ſix cutting teeth in the upper jaw, which are followed by two canine ones.

In the American (on the other hand) there are two very long and large cutting teeth in the centre, calculated to fill the vacancy in the lower jaw, which contains only two ſhort cutting teeth, followed immediately by two long canine ones.

In the lower jaw of the European mole, however, there are eight ſmall cutting, without the intervention of any canine teeth. I am,

Dear Sir,

Your moſt faithful

humble ſervant,

Daines Barrington.

XXXV. *Letter*

XXXV. *Letter from the Hon.* Daines Barrington, *F. R. S. to* William Heberden, *M. D. F. R. S. giving an Account of ſome Experiments made in* North Wales, *to aſcertain the different Quantities of Rain, which fell in the ſame Time, at different Heights.*

S I R,

December 24, 1770.

Read June 6, 1771.

AS I happened to be at the meeting of the Royal Society, when your experiments were read, relative to the different quantities of rain, which fell within receivers of the ſame dimenſions at different heights from the ground; it occurred to me, that the ſame trials might be made at more diſproportionate heights, though at the ſame diſtance from the ſurface of the earth.

I accordingly directed two rain-gages of exactly the ſame dimenſions to be made by your inſtrument-maker, which you was ſo obliging as to take the trouble of examining.

As I propoſed to keep them at the top and bottom of a Welch mountain, and am not ſtationary a ſufficient time in the Principality to attend to a long courſe of ſuch obſervations; I ſent the rain-gages to

Mr.

Mr. Meredith Hughes, of Bala, in Merionethſhire, who is a very ingenious land-ſurveyor, and, from his philoſophical turn, would be pleaſed with executing the commiſſion, though a very troubleſome one.

I directed him to place one of the rain-gages at the top of Rennig, which is about four miles Weſt of Bala, and is commonly conſidered as the fifth mountain of North Wales, in point of height *.

I directed the other rain-gage to be fixed near a houſe, called Bochyrhaidr, at about half a mile's diſtance from Rennig; and ſo as that the rain might not be impeded, when the wind blew over the mountain towards the point where the lower rain-gage was placed. Proper precautions were alſo taken, that neither cattle, nor any other accident, ſhould interfere with the experiment.

Being deſirous to know with ſome degree of preciſion the height of this mountain, I directed Mr. Hughes to aſcertain it in the common method, by examining the fall of the mercury in the barometer, at the top, when compared with its ſtate at the bottom. Having made this experiment, he informed me, that the difference was one inch and ſixten ths, which according to Dr. Halley's method of computation, would give about 450 yards in height, from the adjacent plain.

By the following table it will appear, that the quantities of rain, which had fallen in the two rain-

* I rather ſuppoſe it, however, to be only the ſixth, and ſhould range them thus, according to their comparative heights: Carnedd Llewelin, Snowdon, Cader Idrys, Arran Mowddy, Glider, and Rennig. I place Carnedd Llewelin before Snowdon, becauſe I carried a water level to the top of the latter, and conceived Carnedd Llewelin to be higher; perhaps the difference may be only a few yards.

gages

gages were weighed ſix ſeveral times; in three of which the contents of the upper receiver exceeded thoſe of the lower; and in the three others, the quantity in the lower exceeded that of the upper. On the whole, however, the contents of the lower rain-gage exceeded that of the upper above half an inch. This trifling difference therefore ſeems to ariſe from a ſhower's laſting perhaps a little longer on the bottom of the mountain, and not from any permanent cauſe, as in your obſervations.

I am perſuaded, that theſe experiments have been made with the greateſt attention and accuracy, as I was at Bala in Auguſt laſt, and found that all my directions had been moſt punctually followed.

The inference to be drawn however from them (ſuch as they are) ſeems to be, that the increaſe of the quantity of rain depends upon its nearer approximation to the earth, and ſcarcely at all upon the comparative height of places, provided the rain-gages are fixed at about the ſame diſtance from the ground.

Poſſibly alſo a much controverted point between the inhabitants of mountains and plains may receive a ſolution from theſe experiments; as in an *adjacent valley, at leaſt*, very nearly the ſame quantity of rain appears to fall within the ſame period of time as upon the neighbouring mountains. I am, Sir,

Your moſt faithful

humble ſervant,

Daines Barrington.

1770.	Bochyraidr. Grains	Inches.	The top of Rennig. Grains.	Inches.
From July 6th to 16th	5080 =	0,709	4643 =	0,648
July 16th to 29th	15654 =	2,185	15217 =	2,124
July 29th to Aug. 10th	4370 =	0,610	4698 =	0,656
Sept. 9th both bottles had run over.				
Sept. 9th to 30th	23167 =	3,234	17648 =	2,464
Oct. 17th both bottles had run over.				
Oct. 17th to 22d	5353 =	0.747	6336 =	0,885
Oct. 22d to 29th	9179 =	1,281	9944 =	1,388
Nov. 20, both bottles were broken by the frost.		8,766		8,165

N O T E.

It may not be improper to ſubjoin to the foregoing account, that, in the places where it was firſt obſerved, that a different quantity of rain would be collected, according, as the rain-gages were placed above or below the tops of the neighbouring buildings, the rain gage below the top of the houſe, into which the greater quantity of rain had for ſeveral years been found to fall, was above fifteen feet above the level of the other rain-gage, which in another part of London was placed above the top of the houſe, and into which the leſſer quantity always fell. This difference therefore does not, as Mr. Barrington juſtly remarks, depend upon the greater quantity of atmoſphere, through which the rain deſcends: though this has been ſuppoſed by ſome, who have thence concluded, that this appearance might readily be ſolved by the accumulation of more drops, in a deſcent through a greater depth of atmoſphere. W. H.

XXXVI. *A Disquisition concerning certain Fluents, which are assignable by the Arcs of the Conic Sections; wherein are investigated some* new *and* useful *Theorems for computing such Fluents: By* John Landen, *F. R. S.*

Read June 6, 1771.

MR. Mac Laurin, in his Treatise of Fluxions, has given sundry very elegant Theorems for computing the Fluents of certain Fluxions by means of Elliptic and Hyperbolic Arcs; and Mr. D'Alembert, in the Memoirs of the Berlin Academy, has made some improvement upon what had been before written on that subject. But some of the Theorems given by those Gentlemen being in part expressed by the difference between an Arc of an Hyperbola and its Tangent, and such difference being not directly attainable, when such Arc and its Tangent both become infinite, as they will do when the *whole* Fluent is wanted, although such Fluent be at the same time finite; those Theorems therefore in that case fail, a computation thereby being then impracticable, without some farther help.

The supplying that defect I considered as a point of some importance in Geometry, and therefore I earnestly

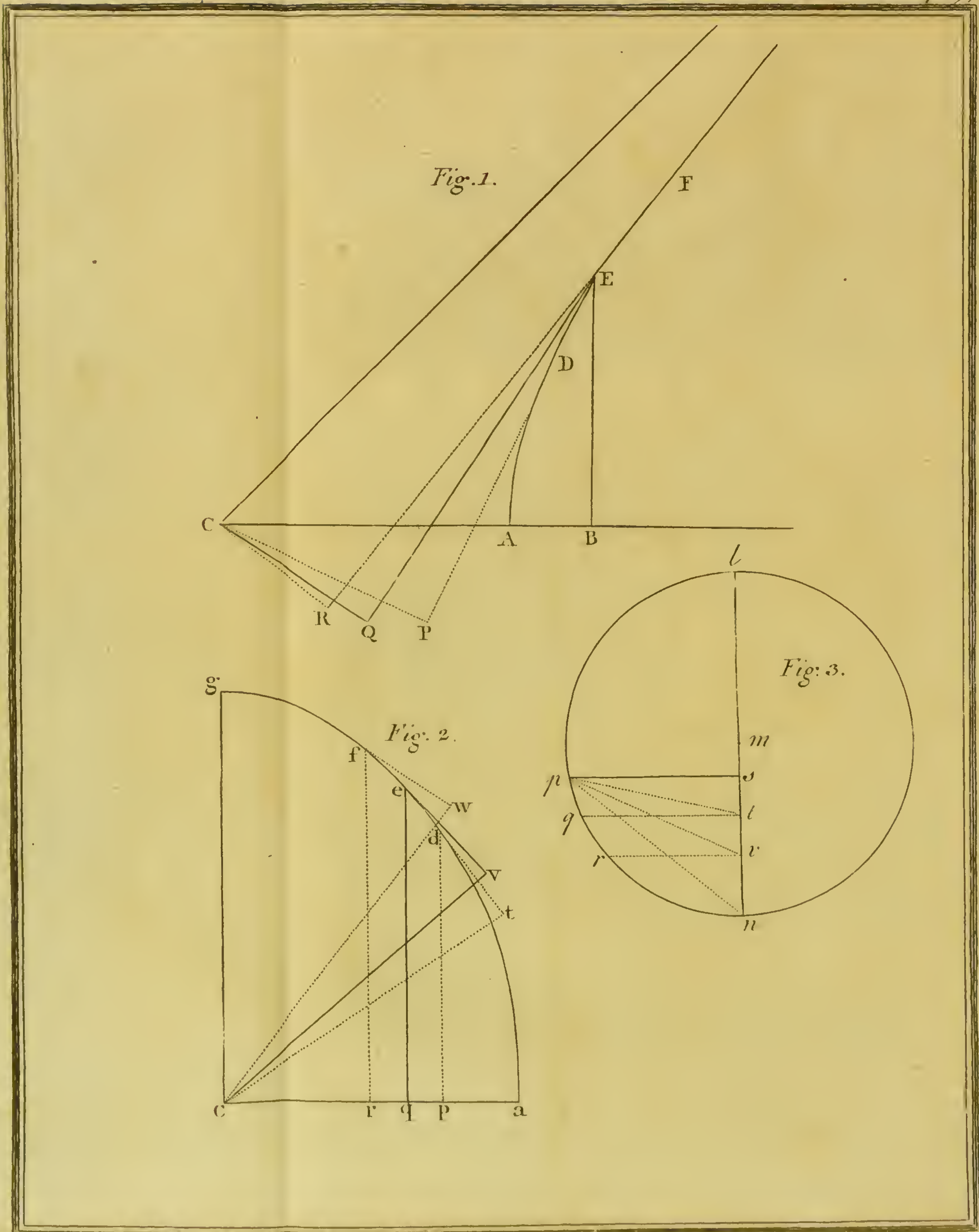


Fig. 1.
F
E
D
C
A
B
R
Q
P
Fig. 2.
g
f
e
w
d
v
t
c
r
q
p
a
Fig: 3.
l
m
p
s
q
t
r
v
n

earneſtly wiſhed, and endeavoured, to accompliſh that buſineſs; my aim being to aſcertain, by means of ſuch arcs, as above-mentioned, the *Limit* of the difference between the Hyperbolic Arc and its Tangent, whilſt the point of contact is ſuppoſed to be carried to an infinite diſtance from the vertex of the curve, ſeeing that, by the help of that *Limit*, the computation would be rendered practicable in the caſe wherein, without ſuch help, the before-mentioned Theorems fail. And having ſucceeded to my ſatisfaction, I preſume, the reſult of my endeavours, which this Paper contains, will not be unacceptable to the Royal Society.

1.

Suppoſe the curve ADEF (Tab. XII. fig. 1.) to be a conic *Hyperbola*, whoſe ſemi-tranſverſe axis AC is $= m$, and ſemi-conjugate $= n$.

Let CP, perpendicular to the tangent DP, be called p; and put $f = \frac{m^2 - n^2}{2m}$, $z = \frac{p^2}{m}$. Then (as is well known) will DP — AD be = the fluent of $\frac{-\frac{1}{2} m^{\frac{1}{2}} z^{\frac{1}{2}} \dot{z}}{\sqrt{n^2 + 2fz - z^2}}$, p and z being each = to m when AD is $= o$.

2.

Suppoſe the curve adefg (fig. 2.) to be a quadrant of an *Ellipſis*, whoſe ſemi-tranſverſe axis cg is $= \sqrt{m^2 + n^2}$, and ſemi-conjugate ac $= n$. Let ct be perpendicular to the tangent dt, and let the abſciſſa cp be $= n \times \overline{\frac{z}{m}}\big|^{\frac{1}{2}}$. Then will the ſaid tangent dt be $= m \times \overline{\frac{mz - z^2}{n^2 + mz}}\big|^{\frac{1}{2}}$; and the fluxion thereof will be found

$$= \tfrac{1}{2} mn^2 z^{-\frac{1}{2}} \dot{z} \times \frac{\overline{m - z}|^{\frac{1}{2}}}{\overline{n^2 + mz}|^{\frac{3}{2}}} - \frac{\frac{1}{2} m^{\frac{1}{2}} z^{\frac{1}{2}} \dot{z}}{\sqrt{n^2 + 2fz - z^2}}.$$

3.

In the expreſſion $\frac{y^{q-1}\dot{y}}{\overline{a+by}|^r \times \overline{c+dy}|^s}$, let $\frac{c+dy}{a+by}$ be ſuppoſed $= z$. Then will $\frac{az-c}{d-bz}$ be $= y$, and the propoſed expreſſion will be

$$= \frac{\overline{ad - bc}|^{1-r-s} \times z^{-s}\dot{z}}{\overline{az - c}|^{1-q} \times \overline{d - bz}|^{1+q-r-s}}.$$

4.

Taking, in the laſt article, r and s each $= \frac{1}{2}$, $q = \frac{3}{2}$, $a = -d = \frac{n^2}{m}$, $b = 1$, and $c = n^2$, we have

$$\frac{y^{\frac{1}{2}}\dot{y}}{\overline{\frac{n^2}{m} + y}|^{\frac{1}{2}} \times \overline{n^2 - \frac{n^2}{m} y}|^{\frac{1}{2}}} \left(= \frac{m^{\frac{1}{2}} n^{-1} y^{\frac{1}{2}} \dot{y}}{\sqrt{n^2 + 2fy - y^2}}\right)$$

$$= - mnz^{-\frac{1}{2}} \dot{z} \times \frac{\overline{m - z}|^{\frac{1}{2}}}{\overline{n^2 + mz}|^{\frac{3}{2}}}.$$

It

It appears therefore, that, y being $= n^2 \times \frac{m - z}{n^2 + mz}$,

$$- \frac{\frac{1}{2} m^{\frac{1}{2}} y^{\frac{1}{2}} \dot{y}}{\sqrt{n^2 + 2fy - y^2}} - \frac{\frac{1}{2} m^{\frac{1}{2}} z^{\frac{1}{2}} \dot{z}}{\sqrt{n^2 + 2fz - z^2}} \text{ is}$$

$$= \tfrac{1}{2} m n^2 z^{-\frac{1}{2}} \dot{z} \times \frac{\overline{m - z}|^{\frac{1}{2}}}{\overline{n^2 + mz}|^{\frac{3}{2}}} - \frac{\frac{1}{2} m^{\frac{1}{2}} z^{\frac{1}{2}} \dot{z}}{\sqrt{n^2 + 2fz - z^2}};$$

which, by Art. 2. is = the *fluxion of the tang.* d t.

Confequently, taking the fluents, by Art. 1. and correcting them properly, we find

$$DP - AD + FR - AF = L + dt.$$

CP, in fig. 1. being $= m^{\frac{1}{2}} z^{\frac{1}{2}}$; cp, in fig. 2. $= n \times \overline{\frac{z}{m}}|^{\frac{1}{2}}$;

CR, perpendicular to the tangent FR $= m^{\frac{1}{2}} y^{\frac{1}{2}}$;

DP — AD = the *fluent* of $\frac{-\frac{1}{2} m^{\frac{1}{2}} z^{\frac{1}{2}} \dot{z}}{\sqrt{n^2 + 2fz - z^2}}$;

FR — AF = the fluent of $\frac{-\frac{1}{2} m^{\frac{1}{2}} y^{\frac{1}{2}} \dot{y}}{\sqrt{n^2 + 2fy - y^2}}$;

and L the *Limit* to which the difference DP — AD, or FR — AF, approaches, upon carrying the point D, or F, from the vertex A *ad infinitum*.

5.

Suppofe y equal to z, and that the points D and F then coincide in E, the points d and p being at the fame time in e and q refpectively. Then cv being perpendicular to the tangent ev, that tangent will be a *maximum* and equal to cg — ac $= \sqrt{m^2 + n^2} - n$; the tangent EQ (in the hyperbola) will be $= \sqrt{m^2 + n^2}$; the

the abſciſſa BC $= m\sqrt{1+\frac{n}{\sqrt{m^2+n^2}}}$; the ordinate BE $= n \times \sqrt{\frac{n}{\sqrt{m^2+n^2}}}$; and it appears, that L is $= 2\text{EQ} - 2\text{AE} - \text{ev} = n + \sqrt{m^2+n^2} - 2\,\text{AE}$! Thus the *Limit* which I propoſed to aſcertain is inveſtigated, m and n being any right lines whatever!

6.

The *whole fluent* of $\frac{\frac{1}{2} m^{\frac{1}{2}} z^{\frac{1}{2}} \dot{z}}{\sqrt{n^2+2fz-z^2}}$, generated whilſt z from o becomes $= m$, being equal to L; and the fluent of the ſame fluxion (ſuppoſing it to begin when z begins) being in general equal to $L + \text{AD} - \text{DP} = \text{FR} - \text{AF} - \text{dt}$; it appears, that, k being the value of z correſponding to the fluent $L + \text{AD} - \text{DP}$, $\frac{mn^2 - n^2 k}{n^2 + mk}$ will be the value of z correſponding to the fluent $L + \text{AF} - \text{FR}$, and FR — AF will be the part generated whilſt z from $\frac{mn^2 - n^2 k}{n^2 + mk}$ becomes $= m$. It follows therefore, that the *tang*. dt, together with the fluent of $\frac{\frac{3}{2} m^{\frac{1}{2}} z^{\frac{1}{2}} \dot{z}}{\sqrt{n^2+2fz-z^2}}$ generated whilſt z from o becomes equal to any quantity k, is equal to the fluent of the ſame fluxion generated whilſt z from $\frac{mn^2 - n^2 k}{n^2 + mk}$ becomes $= m$; cp being taken $= n \times \overline{\frac{k}{m}}\big|^{\frac{1}{2}}$.

Suppoſe

Suppose $k = \frac{mn^2 - n^2k}{n^2 + mk}$, its value will then be $\frac{n}{m}\sqrt{m^2 + n^2} - \frac{n^2}{m}$. Consequently the fluent of $\frac{\frac{1}{2}m^{\frac{1}{2}}z^{\frac{1}{2}}\dot{z}}{\sqrt{n^2 + 2fz - z^2}}$ generated whilst z from o becomes $= \frac{n}{m}\sqrt{m^2 + n^2} - \frac{n^2}{m}$, together with the quantity $\sqrt{m^2 + n^2} - n$, is equal to the fluent of the same fluxion generated whilst z from $\frac{n}{m}\sqrt{m^2 + n^2} - \frac{n^2}{m}$ becomes $= m$: and these two parts of the *whole fluent* being denoted by M and N respectively; M will be $= n - \text{AE}$, and $\text{N} = \sqrt{m^2 + n^2} - \text{AE}$.

7.

The fluent of $\frac{\frac{1}{2}m^{\frac{1}{2}}z^{\frac{1}{2}}\dot{z}}{\sqrt{n^2+2fz-z^2}}$ being $L + \text{AD} - \text{DP}$, the fluent of $\frac{\frac{1}{2}m^{\frac{1}{2}}z^{\frac{1}{2}}\dot{z}}{\sqrt{n^2+2fz-z^2}} + \text{DP} - \text{AD} - L$ will be $= o$. Therefore, the fluent of $\frac{\frac{1}{2}m^{\frac{1}{2}}z^{\frac{1}{2}}\dot{z}}{\sqrt{n^2+2fz-z^2}}$ + the fluent of $\frac{\frac{1}{2}m^{-\frac{1}{2}}n^2z^{-\frac{1}{2}}\dot{z}}{\sqrt{n^2+2fz-z^2}}$ being = the fluent of $\frac{1}{2}z^{-\frac{1}{2}}\dot{z} \times \overline{\frac{n^2+mz}{m-z}}\Big|^{\frac{1}{2}}$, it is obvious, that the fluent of $\frac{\frac{1}{2}m^{-\frac{1}{2}}n^2z^{-\frac{1}{2}}\dot{z}}{\sqrt{n^2+2fz-z^2}}$ is $= \text{DP} - \text{AD} - L$ + the fluent of $\frac{1}{2}z^{-\frac{1}{2}}\dot{z} \times \overline{\frac{n^2+mz}{m-z}}\Big|^{\frac{1}{2}}$

$= \text{DP}$

$= DP - AD - L +$ the *elliptic arc* dg (fig. 2.) whoſe abſciſſa cp is $= n \times \overline{\frac{z}{m}}|^{\frac{1}{2}}$.

Conſequently, putting E for $\frac{1}{4}$ of the periphery of that ellipſis, it appears that the *whole fluent* of $\frac{\frac{1}{2} m^{-\frac{1}{2}} n^2 z^{-\frac{1}{2}} \dot{z}}{\sqrt{n^2 + 2fz - z^2}}$, generated whilſt z from o becomes $= m$, is equal to $E - L = E + 2AE - n - \sqrt{m^2 + n^2}$.

8.

By taking, in Art. 3. q, r, and s, each $= \frac{1}{2}$; and $a = -d = \frac{n^2}{m}$, $b = 1$, and $c = n^2$; we find, that, if y be $= \frac{mn^2 - n^2 z}{n^2 + mz}$, $\frac{z^{-\frac{1}{2}} \dot{z}}{\sqrt{n^2 + 2fz - z^2}} + \frac{y^{-\frac{1}{2}} \dot{y}}{\sqrt{n^2 + 2fy - y^2}}$ will be $= o$.

It is obvious therefore, that the fluent of $\frac{z^{-\frac{1}{2}} \dot{z}}{\sqrt{n^2 + 2fz - z^2}}$, generated whilſt z from o becomes equal to any quantity k, is equal to the fluent of the ſame fluxion, generated whilſt z from $\frac{mn^2 - n^2 k}{n^2 + mk}$ becomes $= m$.

Now, ſuppoſing $k = \frac{mn^2 - n^2 k}{n^2 + mk}$, its value will be $\frac{n}{m}\sqrt{m^2 + n^2} - \frac{n^2}{m}$.

Conſequently, the fluent of $\frac{z^{-\frac{1}{2}} \dot{z}}{\sqrt{n^2 + 2fz - z^2}}$, generated whilſt z from o becomes $= \frac{n}{m}\sqrt{m^2 + n^2} - \frac{n^2}{m}$,

is

is equal to *half* the fluent of the same fluxion, generated whilst z from o becomes $= m$; which *half fluent* is known by the preceding article.

9.

It appears, by Ar. 4. that

$$\frac{\frac{1}{2}m^{\frac{1}{2}}y^{\frac{1}{2}}\dot{y}}{\sqrt{n^2+2fy-y^2}}+\frac{\frac{1}{2}m^{\frac{1}{2}}z^{\frac{1}{2}}\dot{z}}{\sqrt{n^2+2fz-z^2}} \text{ is} = -\text{the } \textit{flux. of the tang.}\ \mathrm{dt};$$

and it appears, by the last article, that

$$\frac{\frac{1}{2}m^{-\frac{1}{2}}n^2y^{-\frac{1}{2}}\dot{y}}{\sqrt{n^2+2fy-y^2}}+\frac{\frac{1}{2}m^{-\frac{1}{2}}n^2z^{-\frac{1}{2}}\dot{z}}{\sqrt{n^2+2fz-z^2}} \text{ is} = o;$$

$mn^2 - n^2y - n^2z - myz$ being $= o$.

Therefore, by addition, we have

$$\frac{1}{2}y^{-\frac{1}{2}}\dot{y}\times\overline{\frac{n^2+my}{m-y}}\Big|^{\frac{1}{2}}+\frac{1}{2}z^{-\frac{1}{2}}\dot{z}\times\overline{\frac{n^2+mz}{m-z}}\Big|^{\frac{1}{2}}$$

$=$ — the *fluxion of the tangent* dt.

Consequently, by taking the correct fluents, we find the *tang.* dt ($=$ the *tang.* fw) $=$ the *arc* ad — the *arc* fg! the abscissa cp being $= n\times\overline{\frac{z}{m}}\Big|^{\frac{1}{2}}$, the abscissa cr $= n\times\overline{\frac{y}{m}}\Big|^{\frac{1}{2}}$, and their relation expressed by the equation $n^6 - n^4u^2 - n^4v^2 - m^2u^2v^2 = o$, u and v being put for cp and cr respectively. Moreover, the tangents dt, fw, will each be $= \frac{m^2uv}{n^4}$; and $\mathrm{ct}\times\mathrm{cw} = \overline{\mathrm{cv}}^2 = \mathrm{ac}\times\mathrm{cg}$.

If for the semi-transverse axis cg we substitute h instead of $\sqrt{m^2+n^2}$, the relation of u to v will be expressed

expreſſed by the equation

$$n^6 - n^4 u^2 - n^4 v^2 - \overline{b^2 - n^2} \times u^2 v^2 = o, \text{ and}$$

$$dt \; (= fw) \text{ will be } = \frac{b^2 - n^2}{n^3} \times uv.$$

If u and v be reſpectively put for fr and dp, their relation will be expreſſed by the equation

$$b^6 - b^4 u^2 - b^4 v^2 + \overline{b^2 - n^2} \times u^2 v^2 = o, \text{ and}$$

$$dt \; (= fw) \text{ will be } = \frac{b^2 - n^2}{b^3} \times uv.$$

10.

Suppoſe $y =$ to z, (that is, $v = u$) and that the points d and f coincide in e. In which caſe the tangent dt will be a *maximum*, and $=$ cg — ac. It appears then that the *arc* ae — the *arc* eg is $=$ cg — ac.

Conſequently, putting E for the quadrantal arc ag, we find that the *arc* ae is $= \frac{E + b - n}{2}$!

the *arc* cg $= \frac{E - b + n}{2}$!

There are, I am aware, ſome other parts of the *arc* ag, whoſe lengths may be aſſigned by means of the whole length (ag) with right lines; but to inveſtigate ſuch other parts is not to my preſent purpoſe.

11.

Taking m and n each $= 1$; that is, ac ($=$AC) $= 1$, and cg $= \sqrt{2}$; let the arc ag be then expreſſed by e: put c for $\frac{1}{4}$ of the periphery of the circle whoſe radius is 1; and let the *whole fluents* of $\frac{-\frac{1}{2}z^{\frac{1}{2}}\dot{z}}{\sqrt{1-z^2}}$ and $\frac{\frac{1}{2}z^{-\frac{1}{2}}\dot{z}}{\sqrt{1-z^2}}$, generated whilſt z from o becomes $=1$, be denoted by F and G reſpectively. Then, by what is ſaid above, $F+G$ will be $=e$; and, by my theorem for comparing curvilineal areas, or fluents, publiſhed in the *Philoſ. Tranſact.* for the year 1768, it appears that $F \times G$ is $=\frac{1}{2}c$. From which equations we find $F=\frac{1}{2}e-\frac{1}{2}\sqrt{e^2-2c}$, and $G=\frac{1}{2}e+\frac{1}{2}\sqrt{e^2-2c}$.

But m and n being each $=1$, L is $=F$; therefore $1+\sqrt{2}-2AE$, the value of L, from Art. 5. is, in this caſe, $=\frac{1}{2}e-\frac{1}{2}\sqrt{e^2-2c}$. Conſequently, in the *equilateral hyperbola*, the *arc* AE, whoſe abſciſſa BC is $=\sqrt{1+\frac{1}{\sqrt{2}}}$, will be $=\frac{1}{2}+\frac{1}{\sqrt{2}}-\frac{1}{4}e+\frac{1}{4}\sqrt{e^2-2c}$, by what is ſaid in the article laſt mentioned. Hence the *rectification* of *that arc* may be effected by means of the *circle* and *ellipſis*!

The application of theſe *Improvements* will be eaſily made by the intelligent Reader, who is acquainted with what has been before written on the ſubject. But there is a theorem (demonſtrable by what is proved in Art. 8.) ſo remarkable, that I cannot conclude this diſquiſition without taking notice of it.

12.

Let *lpqn* (fig. 3.) be a circle perpendicular to the horizon. whose highest point is *l*, lowest *n*, and center *m*: let *p* and *q* be any points in the semi-circumference *lpqn*: draw *ps*, *qt* parallel to the horizon, intersecting *lmn* in *s* and *t*; and, having joined *lp*, *pt*, make the angle *lpv* equal to *ltp*, and draw *rv* parallel to *qt*, intersecting the circle in *r*, and the diameter *lmn* in *v*. Let a pendulum, or other heavy body, descend by its gravity from *p* along the arc *pqrn*: the body so descending will pass over the arc *pq* exactly in the same time as it will pass over the arc *rn*; and therefore, *qt* and *rv* coinciding when *lt* is equal to *lp*, it is evident that the time of descent from *p* to *q* will then be precisely equal to *half* the time of descent from *p* to *n*!

And it is farther observable, that, if *pqn* be a quadrant, the *whole* time of descent will be $=\overline{\frac{a}{b}}\Big|^{\frac{1}{2}} \times \overline{\frac{1}{2}e + \frac{1}{2}\sqrt{e^2 - 2c}}$; the radius *lm*, or *mn*, being $=a$; and *b* being put (for 16 $\frac{1}{12}$ feet) the space a heavy body descending freely from rest falls through in one second of time.

In general, *ns* being denoted by *d*, and the distance of the body from the line *ps*, in its descent, by *x*, the fluxion of the time of descent will be expressed by $\frac{\frac{1}{2}ab^{-\frac{1}{2}}x^{-\frac{1}{2}}\dot{x}}{\sqrt{2ad - d^2 - \overline{2a - 2d}\,.\,x - x^2}}$; the fluent whereof, corresponding to any value of *x*, may be obtained by Art. 7. By which article it appears, that the *whole* time

time of deſcent from any point p will be

$$= \frac{a}{b^{\frac{1}{2}} d^{\frac{1}{2}} \times \overline{2a - d}} \times \overline{E + 2\,\mathrm{AE} - pn - ps}.$$

The ſemi-tranſverſe A C (fig. 1.) being $= ns$;
the ſemi-tranſverſe c g (fig. 2.) $= np$;
and the ſemi-conjugate in each figure $= ps$.

Since writing the above, I have diſcovered a general theorem for the rectification of the Hyperbola, by means of two Ellipſes; the inveſtigation whereof I purpoſe to make the ſubject of another Paper.

XXXVII. *A*

XXXVII. *A Letter from Mr.* John Reinhold Forſter, *F. A. S. to the Hon.* Daines Barrington, *Vice-Preſ. R. S. on the Management of Carp in* Poliſh Pruſſia.

Somerſet-houſe Stable-yard, May 29, 1771.

DEAR SIR,

Read June 13, 1771.

YOU was ſo kind as to judge favourably of the few hints I threw out in a converſation, about *the management of Carp* in Pruſſia and in the electorates of Brandenburg and Saxony, and deſired me to collect my obſervations upon that ſubject, into a ſmall memoire. Though I am very ſenſible, that there are many more capable of giving a ſatisfactory account of the management of carp; I will, however, to obey your friendly commands, communicate to you ſuch obſervations as I can collect from my own experience; from the methods obſerved in Pruſſia, Brandenburg, and Saxony, where I had opportunities to enquire into the ſubject, during my ſtay in theſe countries; and laſtly, from the inſtructions of an anonymous German patriot, in a book, intitled, *A Syſtem of all the Sciences relative to Oeconomy and the Finances.* In caſe you find theſe obſer-

obſervations deſerving to be laid before the Royal Society, I ſhall think myſelf very much honoured by it.

I am, with the ſincereſt ſentiments of gratitude and regard,

Dear Sir,

Your much obliged humble ſervant,

John Reinhold Forſter.

Obſervations on the beſt way of managing Carp, from real experience, and the beſt methods now in uſe.

IT would be needleſs to ſpeak of the natural hiſtory of this well-flavoured fiſh, after the ſatisfactory account given of it in the *Britiſh Zoology* *, by that moſt accurate zoologiſt Mr. Pennant. I will only obſerve this, that though the carp is now commonly found in ponds and rivers, and generally thought to be a freſh-water fiſh †, the ancient zoologiſts ranged

* Britiſh Zool. Vol. III. p. 300, &c.

† I have great reaſon to think, that many other fiſh, which, it is commonly conceived, can only live in the ſea, may alſo exiſt, at leaſt for ſeveral years, and perhaps breed, in freſh water.

the

the fame among the fea-fifh: and I know inftances of its being caught in the harbour of Dantzig, between that city and a little town called Hela; which

The fmelt or fparling (Salmo Eperlanus Linnæi) never comes up our rivers, but for a fhort time; and then does not penetrate much further than where the water continues to be brackifh.

I have, however, been informed by Sir Francis Barnard (the late Governor of New England) that in a large pool which he rented not far from Bofton, and which had not the leaft communication with the fea, feveral of thefe fifh, originally introduced from the falt water, had lived many years, and were, to all appearance, very healthy.

I have alfo the following well-attefted fact with regard to the common grey mullet, which it is believed was never before taken in frefh water.

Mr. Kymer hath made, near Kidwelly in Carmarthenfhire, a communication between his collieries and an arm of the fea, by means of a canal.

Before this canal was compleated, the falt water filled it at every tide, and feveral mullets were by this means introduced.

For thefe three or four years, the fea hath been entirely excluded; and the canal, from the conftant influx of frefh water, hath ceafed to be brackifh for more than two years.

The mullets, however, continue to live in this canal; though Mr. Kymer informs me, they do not look in fo good condition, as when frefh from the fea.

We are fo much in the dark about the natural hiftory of fifh, particularly thofe of the falt water, that it is to be wifhed fea ftews were made on fome of our coafts, as I am told is very commonly practifed in North America, and for a very trifling expence.

Nothing more is requifite, than either to find or dig a proper cavity, perhaps a yard below the low water mark, at fpring tides, from which the fea fhould be excluded, except at a narrow entrance, where large ftones fhould be piled from the beach to above the high water mark.

Through fuch an inlet, the ftew would be every twelve hours, fupplied with frefh falt water, at the fame time that the fifh would not be able to make their efcape.

is situated at the extremity of a long, narrow, sandy promontory, projecting Eastwards into the sea, and forming the gulf before Dantzig, of about 30 English miles diameter. These carp were forced, as I suppose, by a storm, from the mouth of the Vistula, which here enters the Baltic, into the sea: and as the other two branches of the Vistula or Weixel disembogue into a large fresh water lake, called the Trish-Haff, which has a communication with the sea at Pillau; it is equally probable, that these fish came round from Pillau, to the harbour of Dantzig; especially as they are frequently found in the Trish-Haff.

The sale of carp makes a part of the revenue of the nobility and gentry in Prussia, Pomerania, Brandenburgh, Saxony, Bohemia, Mecklenburgh, and Holstein; and the way of managing this useful fish is therefore reduced in these countries into a kind of system, built on a great number of experiments, made during several generations, in the families of gentlemen well skilled in every branch of husbandry.

The first thing which must be attended to, in case a gentleman chooses to have carp-ponds, is to select the ground where they are to be made:

By this very easily-contrived reservoir, sea-fish, when caught in too great numbers, might be kept for the supply of the table or market, when perhaps the weather will not permit them to be taken; and many ingenious experiments might be tried.

It is not impossible (for example) that the fish of the fresh water might be improved, by continuing in such a stew for a fortnight or three weeks, as horses are said to thrive by feeding on the salt marshes.

Daines Barrington.

 for

for upon the ſoil, water, and ſituation of a pond, the ſucceſs in the management greatly depends. The beſt kind of ponds ought to be ſituated in a well-manured, fertile plain, ſurrounded by the fineſt paſtures and corn fields of a rich black mould, having either mild or ſoft ſprings on the ſpot, or a rivulet that runs through the plain; the water ought to be mild and ſoft, by no means too cold, or impregnated with acid, calcareous, ſelenitic, or other mineral particles. The expoſure muſt be ſheltered againſt the cold blaſting Eaſterly or Northern winds, by a ridge of hills, ſituated at ſome diſtance from the pond, enjoying fully the benign influence of the ſun, far from any thick ſhady wood, that might intercept the beams of the ſun, or where the leaves of trees might cauſe a putrefaction, or impregnate the water with aſtringent particles.

Such ponds as are ſurrounded by poor, cold, and ſtiff ſoils, are open to the Eaſt and North winds, have a wood on one or two ſides, and hard or cold water, or ſuch as iſſues from mines, moors, or moſſes, are inferior in goodneſs.

Ponds in a poor, dry, or ſandy ſoil, ſurrounded by pines or firs, with the juſt-mentioned inconveniences, are conſidered as the worſt of all.

The ground towards the pond ought to have a gentle ſlope; for deep vallies are ſubject to great floods, and will then endanger the dikes in a wet rainy ſeaſon; and often the expectations of many years are carried away.

The ſoil cannot be altered: it is therefore a chief qualification of a pond, to be contrived in a good ſoil.

The

The ſun is a leſs material article; provided therefore a pond can enjoy the morning and noon-tide ſun, it matters not much if the wood be on one or two of its ſides.

The water is a material point; but in caſe the ſprings that ſupply the ponds are very cold and hard, it may be ſoftened and tempered by expoſing it to the ſun and air in a large reſervoir above the pond, or by leading it for a long way in an open expoſure, before it enters the pond.

The quantity of water to ſupply the pond with, is another requiſite; too much water makes too great a canal neceſſary, for carrying its ſuperfluity off; and this is very expenſive: too little water has another inconvenience, *viz.* that of keeping the water too long in the pond, and to cauſe a ſtagnation, without any ſufficient freſh ſupplies; and often, in a dry ſeaſon, the ſcantineſs of freſh water diſtreſſes the fiſh, and cauſes diſeaſes and mortality among them.

The above remarks are general, and muſt be applied to all kinds of ponds; but now I will enter into a more minute detail: it is found by experience moſt convenient, to have three kinds of ponds for carp. The firſt is called the ſpawning-pond; the nurſery is the ſecond; and the main-pond is the third and largeſt.

There are two methods for ſtocking the ponds with carp; either to buy a few old fiſh, and to put them into a ſpawning-pond; or to purchaſe a good quantity of one year's old fry, for the nurſery. I will treat of both methods, and will add ſomething about the management of carp in the main-pond.

A pond intended for ſpawning, muſt be well cleaned of all other kinds of fiſh, eſpecially ſuch as are of a rapacious nature, *viz.* pike, pearch, eel, and trout; and alſo of all the newts or *larvæ* of lizards, and the *dytiſci* or water-beetles, which frequently deſtroy quantities of the fry, to the great loſs of the owner.

A rich ſoil, gentle ſloping banks, mild ſprings, or a conſtant ſupply of good ſoft water, with a fine expoſure in regard to ſun and air, are the chief requiſites for a good ſpawning-pond.

A pond of the ſize of about one acre, requires three or four male carp, and ſix or eight female ones; and thus further, in proportion to each acre, the ſame number of males and females.

The beſt carp for breeders are five, ſix, or ſeven years old, in good health, in full ſcale, without any blemiſh or wound (eſpecially ſuch as are cauſed by the *lernæa cyprini* Linn. a kind of cartilaginous worm) with fine full eyes and a long body. Such as are ſickly, move not briſkly, have ſpots as if they had the ſmall-pox, have either loſt their ſcales, or have them ſticking but looſely to the body, whoſe eyes lie deep in their heads, are ſhort, deep, and lean, will never produce good breed.

Being provided with a ſet of ſuch carp as are here deſcribed, and ſufficient to ſtock a pond with, it is beſt to put them, on a fine calm day, the latter end of March or in April, into the ſpawning-pond. Care muſt be taken, that the fiſh be not too much hurt by being tranſported in a hogſhead, nor put into the pond on a ſtormy day; for they are eaſily thrown

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upon the ſhallows on the ſides, being weak and harraſſed by being caught, removed, and not yet acquainted with the deep holes for their retreat, in the new habitation.

Carp ſpawn in May, June, or July, according as the warm ſeaſon ſets in earlier or later. The warm weather expands and ſwells gently the bodies of the fiſh; and their bellies being diſtended with roe and milt, they feel an itching about thoſe turgid parts, and therefore ſwim to a ſhallow, warm, ſheltered place, where the bottom of the pond is either ſomewhat ſandy or gritty, where ſome graſs and aquatic plants grow, or where ſome ozier branches and roots hang in the water; they gently rub their bodies againſt the ground, the graſs, or oziers, and by this preſſure, the ſpawn iſſues out; and as the milter, by a natural inſtinct, follows the ſpawner, and feels the ſame itching, the calls of nature are gratified in the ſame manner, and the ſoft roe or milt is ſpread over the ſpawn, and thus impregnated. Carp in this ſeaſon are frequently ſeen ſwimming, as if it were in a circle, about the ſame ſpot, which is merely done with an intention of repeating the rubbing of their expanded bellies. The fineſt and calmeſt ſummer days are commonly thoſe on which carp ſpawn; providence having thus made a proviſion for the greater ſecurity of the fry of ſo uſeful a fiſh; as otherwiſe, in a ſtormy day, the ſpawn would be waſhed towards the banks, where it would be eaten up by birds, or trampled upon by men and quadrupeds, or dried up by the heat of the ſun, and a whole generation of carp entirely deſtroyed. In a pond of my uncle's, I frequently found the carp in a warm ſummer evening, round a

large

large ſtone, rubbing their bellies againſt the hard ſandy ground; I often approached with as much ſilence as poſſible, put my hands and feet among the ſporting carp, and had the ſatisfaction to ſee them paſs and repaſs through my hands, without being in the leaſt diſturbed; but at the leaſt noiſe or quick motion occaſioned by me, they moved away with ſurprizing velocity.

About the ſpawning ſeaſon, great care muſt be taken, to keep out all aquatic fowl, wild and tame, from the ponds; for geeſe and ducks not only ſwallow the ſpawn, but deſtroy ſtill more of it, by ſearching the weeds and aquatic plants. It is therefore a general rule, to ſend twice a day, a man round the ponds, to ſcare all wild fowl, *viz.* ſwans, geeſe, ducks, cranes, and herons.

Sometimes cruſians and carp, or tench and carp, being put together in a pond, and the males and females of each kind not being in a juſt proportion one to another, the different ſpecies mix their roe and milt, and thus produce mules or mongrel breeds.

The mules, between carp and cruſians*, ſeldom and ſlowly attain the ſize, which carp are capable of;

* The fiſh thus named is ſuppoſed to be the ſame with the rud or finſcale (See Br. Zool. Vol. III. p 310.). It is not very common in England, and is generally eſteemed to be much inferior to a carp in point of flavour, which I rather conceive to ariſe from its being placed in improper ponds, or eaten when it is not fully in ſeaſon, as our countryman Mr. Henſhaw gives the following account of the karouſſe (*cyprinus carauſius* of Linnæus). "The crawfiſh of that country (meaning Den-"mark) are at leaſt twice as big as ours, and are excellent "meat; but the choiceſt pond fiſh they have, is called *karouſe*,

they

they are very deep, and ſhorter in proportion than carp, but of a very hardy nature.

The mules between carp and tench, partake of the nature of both fiſh, come to a good ſize; but ſome part of their body is covered with the ſmall ſlimy ſcales of a tench, and ſome other part has the larger ſcales of carp; their fleſh approaches nearer to that of a tench, and they are likewiſe of a leſs tender nature than the common carp: this latter kind of mule is called in Germany *ſpiegel-karpe*, i.e. the *mirror-carp*, the blotches with large ſcales among the ſmaller ones being conſidered as mirrors.

Whether theſe mules are capable of propagating their ſpecies, I cannot affirm; never having made any experiments on that ſubject; nor have I heard any thing ſaid on that head with any degree of preciſion, or founded on experience. In ſome ponds in Lancaſhire, I was told, by a gentleman of great worth and honour, both theſe kinds of mules are now and then found.

I think it, however, not adviſeable, to put carp and tench, or carp and cruſians, in one pond, unleſs it be done for experiment's ſake; in which latter caſe, a ſmall pond, free from other fiſh, with one or two fiſh of each kind, will be ſufficient to gratify curioſity, without debaſing a generation of carp in a large pond.

"ſomewhat reſembling a roach, with his red fins; but it is "near as big as the largeſt carp, and much better meat."

Dr. Birch's Hiſt. R. S. Vol. III. p. 187.

D. B.

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The young fry being hatched from the ſpawn, by the benign influence of the ſun, they are left the whole ſummer, and even the next winter, in the ſpawning-pond, in caſe the pond be ſo deep, that the ſuffocation of the young tender fry under the ice in a ſevere winter, is not to be apprehended, for it is by no means advantageous to take them out in the firſt months of their exiſtence. However, if the ſhallowneſs of the pond, its cold ſituation and climate, make it neceſſary to ſecure the fry againſt the rigours of the enſuing winter, the water of the pond muſt be let off; the fry and old fiſh will gradually retire to the canal and ditches, which communicate with the hole in the middle of the pond, and a net, with ſmall meſhes, is then employed to catch both the fry and old ones. The old breeders are then ſeparated from the fry, and both kinds put in ſeparate ponds, that are warmer and more convenient for the wintering of theſe delicate fiſh. Care muſt be taken, to fix upon a calm, mild day, at the latter end of September, for the catching of the fry out of the ſpawning-pond.

The nurſeries are the ſecond kind of ponds intended for the bringing up the young fry. The beſt time to put them into the nurſery is in March or April, on a fine and calm day. A thouſand or twelve hundred of this fry may be allotted to each acre of a pond. The choice of the fry muſt be made according to the above enumerated characters of good and healthy fiſh, and muſt be carefully removed from one pond to another It is likewiſe requiſite to ſend people with long ſticks, all the firſt day, round the pond, in order to drive the tender and weak fry from the ſides into the pond, becauſe they are bewildered in a ſtrange place, and often become the prey of rapacious birds *.

In caſe the pond be good, and not overſtocked before, and the fry well-choſen and preſerved, it is almoſt certain, they will grow within two ſummers ſo much as to weigh four, five, and ſometimes ſix pounds, and to be fleſhy and well-taſted. A great many Pruſſian gentlemen make a good profit, by ſelling their carp, after two years ſtanding in the nurſery, and export them even to Finland and Ruſſia.

The main-ponds are the laſt kind. In theſe, carp are put, that meaſure a foot, head and tail incluſive. Every ſquare of fifteen feet in the pond is ſufficient for one carp, and will afford food and room for the fiſh to play in. The more room carp have, and conſequently the more food the pond affords, the quicker will be the growth of the fiſh. The longer the pond has been already in uſe, the longer you intend to keep the carp in it, the more you deſire to quicken the growth of them, the more you ought to leſſen the number of fiſh deſtined for the pond. Spring and autumn are the beſt ſeaſons for ſtocking your main-ponds. The growth of your fiſh will always be in proportion with the food they have: for carp are obſerved to grow a long time, and to come to a very conſiderable ſize, and a remarkable weight. I recollect to have ſeen carp above a yard long, and of 25 pounds weight; but I

* I have reaſon to think that the common carrion crow ſhould be added to the liſt of birds, which Mr. Forſter hath before ſuppoſed deſtroy fiſh when in ſhallow waters, as I once ſaw this bird taken by a trap, which was baited with a fiſh for a heron.

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had no opportunity to afcertain their real age. In the pond at Charlottenburg, a palace belonging to the king of Pruffia, I faw more than two or three hundred carp between two and three feet long; and I was told by the keeper, they were between 50 and 60 years ftanding: they were tame, and came to the fhore in order to be fed; they fwallowed with eafe a piece of white bread, of the fize of half a half-penny roll.

During winter, ponds ought to have their full complement of water; for the deeper the water is, the warmer lies the fifh. In cafe the pond be covered with ice, every day fome holes muft be opened, for the admiffion of frefh air into the pond, for want of which frequently carp perifh.

In the fummer, obferve to clean the rails and wire-works, in the water-courfes, of the weeds and grafs, which frequently ftop them up. Birds that feed on fifh muft be carefully kept out of the ponds. In a great drought, provifion ought to be made, to keep the water at the fame height as it commonly ftands in the pond, *i. e.* between four and five feet. If the water ftagnates and grows putrid, it muft be let off, and a fupply of frefh water be introduced from the refervoirs. If the weeds, efpecially reed and flags, and fome of the aquatic graffes, over-run too much the pond, fcithes fixed on poles of 16 or 20 feet, with a lead faftened to them to keep the fcithes on the bottom of the pond, are thrown out, and then again drawn to the perfon that works with them, and the weeds will all be cut; after which operation, they muft be drawn up by long harrows, and fet in heaps on the fhore for putrefaction, and in length of time,

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for manure. This cleaning of ponds, muſt never be done in a ſpawning-pond, where it would be the deſtruction of thouſands of fiſh.

Autumn is the beſt ſeaſon for catching ſuch carp as are intended for the market. After the pond has been for five or ſix years in conſtant uſe, it is likewiſe time to let the water entirely off, and clear the pond of the mud, which often increaſes too much, and becomes a nuiſance. When the pond is dry, it may be ploughed before the froſt ſets in, and next ſpring oats or barley ſhould be ſown in it, after a new ploughing; and it will repay the trouble to the owner with a rich and plentiful crop. When the looſe ſuperfluous mud is carried off out of the pond, care ought to be taken not to take the ſoil below the original level of the pond.

Some people ſow a pond, which hath been laid dry for ſome months, with oats; and when they are growing, they fill the pond with water, and introduce carp for ſpawning, and think, by this contrivance, to procure food for the fiſh and ſomething to rub their bellies againſt. But this practice ſeems to be more noxious than beneficial; for the growing oats will putrefy, and communicate putridity to the water, which can by no means be ſalutary to the fiſh.

The epicures ſometimes feed carp, during the colder ſeaſon, in a cellar. The following method is the beſt that can be obſerved for that purpoſe. A carp is laid on a great quantity of wet moſs, ſpread on a piece of net, which then is gathered into a purſe, and the moſs ſo contrived, that the whole fiſh be entirely wrapt up in it: however, care muſt be taken to give the fiſh eaſe, and not to ſqueeze it, ſo that it may have room to breathe in this confined attitude. The net with the fiſh and moſs is then plunged into water and hung up to the cieling of the cellar. In the beginning, this operation muſt be very frequently repeated, at leaſt every three or four hours; by length of time the fiſh will be more uſed to the new element, and will bear to be out of water for ſix or ſeven hours *. Its food is bread ſoaked in milk, which, in the beginning, muſt be adminiſtred to the fiſh in ſmall quantities; in a ſhort time the fiſh will bear more and grow fatter. I ſaw the experiment tried in a nobleman's-houſe, in the principality of Anhalt-Deſſau; and during a fortnight, I viſited myſelf, every day, the fiſh, together with the young nobleman, my friend, whom I accompanied to his ſeat from the univerſity, during the Chriſtmaſs-vacation. After the fiſh had been kept in the above manner during a fortnight, it was dreſſed and ſerved up at dinner, when every one preſent found it excellent in its flavour. At my late uncle's, I had an opportunity of repeating the experiment on a carp

* It is known to every one that a carp will live a great while out of water; but perhaps it may not be ſo notorious, that the keeping him ſeveral hours in the common air, without any precautions, may be repeated from day to day, without any apparent inconvenience to the fiſh.

There is a fiſhmonger near Clare-market, who, in the winter, expoſes for ſale, a buſhel at leaſt of carp and tench, in the ſame dry veſſel: but a ſmall proportion of theſe can be ſold in a day; and I have frequently been informed, that the fiſh continue in good health, notwithſtanding their being thus expoſed to the air ſix or ſeven hours for ſeveral ſucceſſive days.

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that had been brought 20 miles wrapt up in wet moſs; but after the fiſh had been kept three days in wet moſs, during which it was freſh and healthy, it was employed to regale a friend, whoſe unexpected arrival accelerated its fate, before the experiment was finiſhed.

John Reinhold Forſter.

XXXVIII. *An*

XXXVIII. *An Account of the remarkable Cold observed at* Glaſgow, *in the Month of* January, 1768; *in a Letter from Mr.* Alexander Wilſon, *Profeſſor of Aſtronomy at* Glaſgow, *to the Rev. Mr.* Nevil Maſkeline, *B. D. F. R. S. and Aſtronomer Royal.*

Reverend SIR,

College, Glaſgow, May 29, 1771.

Read Nov. 7, 1771.

HAVING of late had ſome leiſure time, I have made out from my minutes, a detail of the remarkable cold which prevailed here in the month of January, 1768; the intenſity of which being ſo extraordinary for this climate, an account of it may perhaps be thought worthy of a place in the Philoſophical Tranſactions.

Whilſt in bed, on ſunday morning, January 3, 1768, about 8 o'clock, it felt ſomehow unuſually cold. A little while after, on reaching out for a decanter which I had placed near me the preceding night,

night, with fome water in it, I was furprized to find the furface of the water frozen over, the like not having happened before in that place. Upon this, I defired my fon to try the cold by a thermometer, as I imagined it behooved to be very intenfe. The experiment was foon after made, by expofing a thermometer at a high North window, and free from the walls of the houfe; in which fituation it had not remained for a quarter of an hour, when we found the mercury had fallen fo low as to 5 deg. of Fahrenheit's fcale.

Although I had expected a great degree of cold, yet I was not quite prepared for fo extraordinary a report as that which the thermometer now gave me. My doubts were, however, foon fettled, by examining matters with more attention, and by finding the firft thermometer verified by my ftandard one, which was now hung out befide it.

Being thus fatisfied that there was no fallacy in this preliminary obfervation, it naturally occurred, that the cold, however intenfe it now was, might have been much more fo at fome earlier hour of the morning. But how to afcertain this, and to recover the loft obfervation, was the difficulty. In the eagernefs of our difappointed curiofity, we were difpofed to magnify this golden opportunity, which had now efcaped us, and to reflect upon it with regret, when luckily a little invention helped us out. A notion fuggefted itfelf, that, if we went very warily to work, we might perhaps furprize thofe imagined colds ftill lurking under the furface of the fnow, which at that time lay thick upon the ground.

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I need not mention upon what principles of the heating and cooling of bodies this expectation was founded, as they will readily occur of themſelves. The fact was, that I immediately repaired to the fields, and ſought out a low place, upon which the ſun had not then riſen; here I laid the thermometer in the ſnow, almoſt upon the very ſurface, when preſently the mercury ſunk from + 6 deg. to — 2 deg. which therefore I concluded to have been pretty nearly the coldeſt temperature of the air over night.

The next thing was, to make regular obſervations with the thermometer, ſo long as the cold promiſed to continue remarkable. The inſtrument was hung upon a pole near to the obſervatory, and to the windward of it, care having been alſo taken to keep it under a proper ſhade, ſo long as the ſun ſhone out.

Regiſter of the Thermometer, kept at the M'Farlane obſervatory, of the college of Glaſgow, on ſunday January 3, *and monday January* 4, 1768.

Sunday	10 o'clock	+	5 deg.
morning	11		7
	12		9
afternoon	1		10
	2		11
	3		9½
	3½		6½
	4		3½
	4½		2
	5		1½
	5½		2½
	6		1½
	6½		0½
	7	—	1
	7½	—	0½

The temperature of the ſnow on ſunday morning, at about ten inches below the ſurface, was near to 30 deg.

8 —

	8	—	$0\frac{1}{2}$	
	$8\frac{1}{2}$	—	1	
	9	—	2	
	$9\frac{1}{2}$	—	1	
	10	—	2	
	$10\frac{1}{2}$	—	2	
	11	—	2	
	$11\frac{1}{2}$	—	1	Some appearance of clouds in the S. E.
	12	—	0	
	$12\frac{1}{2}$	—	$0\frac{1}{2}$	
Monday	1	—	1	
morning	2	—	0	
	$2\frac{1}{2}$	+	3	Clouds gathering, and ſome wind from E.
	3		6	
	$3\frac{1}{2}$		7	
	4		9	Quite overcaſt, wind E.
	$4\frac{1}{2}$		10	Ditto
	5		12	Ditto

It was obſervable, that after ſun ſetting, the atmoſphere had a tendency ſometimes to turn a little foggy, and again quickly to clear up, balancing, as it were, betwixt theſe two different ſtates. It is worthy of notice, that the minute variations of the thermometer, as ſet down in the above regiſter, ſeemed to depend upon theſe different conſtitutions of the air; the mercury always riſing in the thermometer a ſmall matter, when the miſtineſs came on, and *vice verſa*.

In the intervals of obſervations, we made ſome other experiments, which the preſent intenſity of the froſt ſuggeſted; particularly one relating to the evaporation of ice, which was tried in the following manner. I took a ſquare reflecting metal belonging to my own two foot teleſcope, and expoſed it on the balluſtrade of the obſervatory, till it had acquired the temperature of the place, which was then at 0 deg. after it was thus cooled, I breathed on it repeatedly, till its polifhed furface was covered over with an incruftation of ice or frozen vapour, of a very palpable thicknefs. In this condition the fpeculum was replaced in its former fituation, having its incrufted furface expofed to the ftill open air; when, in a little time, we found the frozen pellicle begining to difappear at the outer edge, all around, leaving the metal quite clear. Gradually more and more of the fpeculum was bared in a regular progreffion, from the circumference towards the centre; and at laft, in about 50 minutes, the whole furface had parted with its ice. This experiment was repeated when the fpeculum was defended from the open air, by a large thin box, with a cloth over it. The event turned out the fame as before, only it required longer time.

This progrefs of the evaporation from the outward parts towards the centre of the fpeculum, was likely owing to the original plate of ice being thickeft towards the center, a circumftance which might arife from the manner of fixing it at firft breathing on it. Or perhaps it may be imputed to fome more curious caufe, and may be fome effect of the repulfive force belonging to the polifhed furface; but this point we did not fufficiently examine into, by a due repetition of experiments. I may juft mention, that, partly with a view to this matter, we expofed as above, a fet of bodies, having their furfaces of different degrees of polifh, and as equally covered with frozen moifture as we could judge. The refult of which experiments feemed to favour the idea of the ice being lefs attached to the more polifhed furface than to the coarfer. This appeared particularly in the cafe case of a comparison made betwixt the speculum above-mentioned, and the brass end or cover of the same telescope; for the ice was found still to cleave to its surface a good while after the speculum was entirely cleared. These imperfect experiments are only mentioned by the bye, and may perhaps serve as hints to others, who may be disposed to prosecute this part of natural philosophy.

Some particular reasons have occurred, which will hinder me from transmitting to you the paper on the solar spots, till some time next winter, by which time I shall have finished every thing I have to say on that subject. Wishing to hear from you at your leisure, I ever am, with much respect,

Reverend Sir,

Your most obedient servant,

Alexander Wilson,

Professor of Astronomy at Glasgow.

Received November 15, 1770.

XXXIX. *Some Experiments on Putrefaction*; *by* F. L. F. Crell, *M. D. and Professor of Chemistry at* Brunswick.

Read Nov. 7, 1771.

THE celebrated lord Bacon [a] has, without doubt, shewn a very great sagacity, in pointing out to posterity, putrefaction, as a subject, worthy of making further inquiries into; and certainly, as there happen daily so many changes, not only in the inanimate, but also in the animate world, carried on by its means; the knowledge of every thing relating to it must clear up a great many points in natural philosophy, not thoroughly understood before. But these inquiries ought to be still of more consequence to mankind, as health depends greatly upon keeping in due bounds putrefaction, which the body naturally tends to. For these reasons, Sir John Pringle deserves, besides his other eminent merits, very great praises, on his having made many experiments on this subject; and medicine is indebted to him for considerable improvements resulting from them. He has besides opened

[a] Nat. Hist. Cent. IV.

the way to many other gentlemen, among whom excell Dr. Gaber, and Dr. M'Bride, whoſe numerous experiments ſhew the ingenuity, and ſagacity, they are poſſeſſed of: but the ſubject is not yet exhauſted, nor will it be very eaſily. I have made ſome experiments relating to it; and ſhould be very glad, if they threw a new light on ſome points of the greateſt importance to medicine.

Dr. Gaber has proved, by his experiments, the preſence of a volatile alcali produced by putrefaction; but as he did not diſcover by the ſame proceedings [*b*] any in its beginning or end, though there was a very putrid ſmell, he denies its exiſtence in theſe ſtates, and concludes, that this volatile alcali is not a neceſſary product of putrefaction [*c*]. This doctrine ſeemed to me not quite conformable to the phænomena: for, as all ſmell, as much as we know at leaſt till now, depends on a ſaline matter, joined with a

[*b*] Acta Taurinenſ. Vol. I. p. 78. Cum attegerint ſummum effervefcentiae gradum, continuato ejuſdem loci calore effervefcentiae vim amiſerunt. P. 79. Citius plerumque prodiit foetor, quam alkali, idemque tardius deſiit. P. 82. Maſſam inde relinqui foetentiſſimam, ſed emiſſo alkali ad effervefcentiam ineptam.

[*c*] Id. p. 83, 15. Quum foeteret graviſſime reſiduum deſtillationis, quamquam omni alkali orbatum, manifeſtum videtur, ab alkali foetorem exaltari quidem poſſe, & magis penetrantem effici, non autem ab eodem produci, quandoquidem ſupereſt eo ſublato—16. Videtur is odor a volatilibus admodum particulis proficiſci, ſed quae ab alkali diſſimiles ſunt, plerumque citius gignantur, tardiuſque diſſipentur — alcaleſcentia adeſſe poteſt medico foetori conjuncta—viciſſim maximus foetor abſque alcali— Ex quibus differentia inter foetidas alcalinaſque partes confirmari videtur.—P.84, 17. Videtur alcali non eſſe productum neceſſarium putrefactionis neque gradum alcaleſcentiae gradui putrefactionis reſpondere.

phlogiſton,

phlogiſton, and the ſaline matter producing the putrid ſtench, was not very likely an acid; I ſuppoſed it to be a volatile alkali, which, involved in phlogiſtic matter, might fly off, before the alkali was developed. I wanted to know by experiment, if I was right; for this purpoſe, I put, the 19th of June (the thermometer being 58° of Fahrenheit, and continuing between 58° and 62° all the time I obſerved), in a pretty large receiver, ſome beef cut in very ſmall pieces; I covered the bottom with it thinly, and poured upon it water, about two inches high. The 22d, the putrid ſmell was very ſenſible: but I let it ſtand till the 24th, when I poured off the fluid [*d*], adding again about the ſame quantity of water to the fleſh. I filtrated then the fluid through a piece of fine linen, and mixed with ſome of it the ſyrup of violets, which it did not alter; neither did it effervefe with the ſpirit of vitriol, diluted to a ſharpneſs near that of the vegetable acid. I thought of keeping it in digeſtion for ſome days; but, for fear that ſome little ſolid particles might have paſſed through the linen, and by that means, in growing putrid, might give ſome alcali, and render the trial inaccurate, I diſtilled the fluid by a heat of about 160°, after which, I repeated the trial with the ſyrup of violets and the ſpirit of vitriol; but it produced no

[*d*] It requires ſome attention to find out the proper time when to pour off the liquor; if it is done too ſoon, it will give too little volatile alcali to be much ſenſible by experiments; for, though it ſmells ſtrongly, it is known how little matter is required to produce a ſtrong ſmell. If it is delayed too long, it ſhews already ſigns of an alcali. For that reaſon, I made many experiments in vain.

change.

change. I then put it, the 25th, into a retort, fitted to it a receiver, applied to the jointure a ring of paſte made of flower and water, covered it with a piece of wet bladder, and expoſed it in a *balneum arenæ* to a heat of 108° to 116°, till the 29th of June, when the whole fluid was diſtilled over. I perceived during this operation, that the liquor, from being quite tranſparent, grew turbid; the firſt diſtilled tranſparent fluid grew alſo turbid in the receiver, and at the bottom of the retort there was a ſmall ſettlement of a whitiſh earth. The liquor had a particular ſmell, but quite different from a putrid one, inclining to the volatile alcali; and ſhewed a ſlight but ſenſible degree of effervefcence with the ſpirit of vitriol; and the ſyrup of violets was turned evidently green by it.

In the mean time, the fleſh with the water continued to emit a putrid ſtench; and the 28th of June I found the fluid colouring the ſyrup of violets greeniſh, and ſhewing a kind of effervefcence with the acid. Both theſe qualities were increaſing every day, till the 8th of July, when, on account of a journey, I could not obſerve it any longer. I had left the mouth of the receiver open; and on my return the 1ſt of Auguſt, I found an exceeding putrid ſmell; I covered the veſſel; and the 2d, examined the fluid, but it did not effervefce any more. I then filtrated the liquor; but the fleſh was ſo rotten, that a great many particles paſſed through the linen, and rendered it turbid. I put it into a retort, adapted a receiver, and luted it, as before-mentioned; the heat was alſo the ſame, between 108° and 116°. In this warmth it continued for about four days, when the fluid was

diſtilled

diftilled over. On opening the veffels, the fmell was again entirely changed, not near fo difagreable as before. In the receiver I obtained a fluid, which turned the fyrup of violets green, effervefced very fmartly with the very fame fpirit of vitriol I had ufed before; gave the fmell of a volatile alcali, on adding to this the fixed alcali; praecipitated the calces of metals diffolved in acids, and fhewed itfelf by all proofs a true volatile alcali. In the retort remained a yellowifh matter, almoft without any fmell. I put to it fome water; and after 24 hours, it gave the herbaceous fmell, but fhewed no figns of any alcali. I let it ftand four days longer: the herbaceous fmell continued; but there was no alcali to be difcovered. I diftilled it with a gentle fire: but neither then did there appear an alcali [*e*]; and by applying a ftronger fire, I got nothing but a kind of empyreumatic oil.

I had poured, the 3d of Auguft, fome frefh water on the putrid matter; its putrid fmell continued; the 7th I decanted the fluid, filtrated it, and made it undergo the fame operation, with exactly the fame effect as before; which I did again the 11th, with the very fame effect. I did not repeat it oftener, as I had occafion for this putrid flefh to fome other purpofe.

Thefe experiments fhew, I think, that the volatile alcali is prefent as long, at leaft, as the putrid fmell

[*e*] What this herbaceous fmell did depend on, I did not enquire any farther, as not relating to medicine, fince a living body never was found in fuch a ftate: but very likely it depends on fome volatile alcali, which is perhaps in fo very fmall a quantity as not to be perceptible by experiments.

continues,

continues; and that this volatile alcali is the basis of it, because, as this was distilled over, the residuum, being still in intestine motion, got only the herbaceous smell. The reason, why the volatile alcali has been distinctly observed at a certain period of putrefaction, and not in the others, is, I believe, this; the volatile alcali has, it seems, a tendency, to disintangle itself, by intestine motion, of all such matter as it is involved with; but if it is not combined with such fixed matter as retains it till it has gone through all its evolutions, it is, being itself volatile, carried off by the still more volatile phlogistic matter with which it is commonly joined. For this reason, I suppose, the putrefying matter shews in its beginning no sign of a volatile alcali; because its smell depends only on those particles, which have been on the surface, without any strong cohesion with the substance. In the farther progress of putrefaction, the matter involving the alcali, or forming it, is intermixed, and in cohesion with the solid particles of the substance, and is by these means retained till the alcali is come to its purer state. Towards the end of putrefaction, the cohesion of the particles being almost entirely taken off, the volatile alcali is carried off before it can go through all its states.

If it is therefore true, that the volatile alcali is essential to, or at least always present in putrefaction, it seems to follow, that the alcalies never can be used in living bodies, as antiseptics [*f*], for laying

[*f*] It is very difficult, methinks, to account for the antiseptic power of the volatile alcali, and other salts, on dead animal sub-

aside their stimulating quality, which must prevent their use in most of the putrid diseases, they would increase the morbific matter, by being intimately mixed by circulation with phlogistic matter, which they find in abundance in such bodies. It has been objected to this, that the exhalation of stale urine, though shewing a great quantity of volatile alcali, is inoffensive to health [g]: and that some persons have taken the volatile alcali in very great quantity, without its bringing on a putrid disease [h]: but there

stances: I once thought, that as the ammoniac salt, nitre, &c. bring down the thermometer several degrees, perhaps all these salts acted by instantly absorbing the heat produced by the beginning intestine motion; and that, as a certain degree of warmth is necessary to putrefaction, in preventing this degree from coming on, it might hinder the whole operation. To see by experiment how far this might be true, I put into phials a certain quantity of water, with that proportionate quantity of alcalies, fixed and volatile, sal ammoniac, &c. which Sir John Pringle had found (Append. p. xvi. xvii.) to be antiseptic; and in one as much pure water as a standard. I stopped every one of them with a cork, in which I had made a hole for a thermometer of Fahrenheit. I exposed all these phials to the same heat; Sir John had used about 112°; but I found, that both those with the salts and that without it marked the same degree of heat; and that therefore the absorption of heat can by no means be the reason of the putrefaction being stopped. May this phænomenon not depend upon the salts penetrating the body, and giving to the particles more *puncta contactus* (according to their greater or less affinity)? and may not these salts, in augmenting cohesion, hinder the fluids from separating themselves from one another, and, in consequence, prevent intestine motion? Is this not somewhat confirmed by the action of adstringents? and by the most powerful actions of metallic salts, as being of the greatest specific gravity?

[g] Sir J. Pringle, Append. p. vi.

[h] Id. ibid. p. xcii.

are

are however ſome examples [*i*], where it has been hurtful. It is urged further, that a perſon, being only for a ſhort time expoſed to really putrid exhalations, may be infected with putrid diſeaſes; and therefore that this effect of putrid exhalations does not depend on the volatile alcali, as it may be taken pure in very large doſes, without producing ſuch effects. To this I reply, by an analogous inſtance; a ſmall quantity of ferment will bring on fermentation in a large maſs of fermentable matter, and yet as much acid as could be obtained from the ferment, far from exciting an inteſtine motion in the fermentable matter, would rather check it; but can it, for all that, be denied, that the involved acid in the ferment is the chief cauſe of ſetting the whole maſs in fermentation? In the ſame way, the alcali combined with phlogiſtic matter may produce ſuch inteſtine motion as the pure alcali cannot; and very likely the firſt would not produce it, if the volatile alcali in it could be changed.

To bring this about, the moſt powerful means ſeem to be the uſe of acids; and the moſt celebrated phyſicians agree in the good effect they have obſerved from acids in putrid diſeaſes, and recommend them ſtrongly. Dr. M'Bride thinks otherwiſe, and his reaſons are theſe: *firſt* that if the acids came unchanged to the abſorbent veſſels, they would not admit of them [*k*]; *ſecondly*,

[*i*] Huxham on the ſore throat, p. 67, 68. Ejuſd. Eſſay on fevers, p. 118, edit. 5.

[*k*] Experiment. Eſſays, edit. ſec. p. 20. The auſtere acid (generated in the firſt paſſages of weakly perſons) is exactly in the ſame ſtate with a foreign acid, for the lacteals will admit none of it.

if they did, they would be dangerous [*l*]; and *thirdly* that they are quite changed, before they leave the *primæ viæ* [*m*]. As for the *first*, I do not know what reasons Dr. M'Bride founds his assertions upon, as acids never are given in so concentrick a state, as by their astringency to make these vessels shut up their orifice; and as metallic salts themselves are absorbed in their very compound state (which seems clear with regard to the corrosive sublimate, and other such saline preparations), I do not see, why the simple acids could not be absorbed. The *second* reason seems to be founded upon some of Dr. M'Bride's experiments (p. 132, 133), *viz.* that putrid flesh, sweetened by distilled vinegar and spirit of vitriol, was firm; but on being boiled went quite to pieces, whereas that sweetened by volatile alcali did not. But, I conceive, these experiments are not applicable to a living body: for the acid being there mixed with the fluids, cannot act in this way on the solids, till the fluids are (if I may use that

[*l*] Ibid. p. 134. the acids dissolve the elementary earth, and thus destroy the texture of that substance, whose soundness they are supposed to restore.—P. 148. we are not to expect, that they are to pervade the minute branches of the vascular system; when indeed it is evident, that they ought not to be allowed to pass into the blood in their acid form; since it is plain, that, from their dissolvent nature, the body must be destroyed, and its most solid parts melted down to a jelly, if naked acids were to be received into the general mass of fluids.

[*m*] Ibid. p. 148. acids are neutralized during the alimentary fermentation; and therefore they cannot act as acids, by saturating any thing of the alcaline kind that they meet with in their course of circulation.

expression)

expreſſion) ſupra-ſaturated with the acid [*n*], which in putrid diſeaſes cannot be the caſe. And farther, a heat of 212° of Fahrenheit never can increaſe the action of the acids in living bodies, as it did in the experiments; for, though Dr. M'Bride denies this conſequence, and will prove the contrary, as the fleſh with the alcali did not diſſolve; yet this circumſtance proves nothing more, than that the volatile alcali has not ſuch power of diſſolving the gluten of animal fibres, as acids have; for, if the effect depended only on the action of the acids by themſelves, the fleſh would rather have been diſſolved when immerſed in them, than when boiled in water.—The Doctor beſides ſeems not quite conſiſtent on this head; for, p. 151, he ſays, "Adſtringents can only "be of importance in thoſe caſes, where, from "extreme relaxation and reſolution of the ſolids, the "diſſolved fluids are ſuffered to tranſude, and either "form ſpots of different hues, or run off by actual "hæmorrhage; here, indeed, the acid of vitriol, as "an aſtringent, not as an acid, is found of great uſe "in gaining time." As the acid could not exert its aſtringent power on the veſſels, without coming to the *ſecundæ viæ* (p. 153.) he ſeems not afraid, in this caſe, of its melting down the moſt ſolid parts to a jelly.

In proof of his *third reaſon*, he alledges ſome experiments; *viz.* the third, p. 40, where a mixture of *fleſh*, bread, lemon juice, and ſaliva, did not efferveſce, after fermentation with an alcali; and the 5th,

[*n*] This has, it ſeems, happened in ſome rare caſes quoted by Dr. M'Bride, and Dr. Haller, p. 148.

p. 42,

p. 42, where a mixture of bread, water, ſaliva, and ſpirit of vitriol efferveſced ſmartly, before the inteſtine motion; but not at all after it. I could object againſt theſe experiments, and eſpecially the 5th, that perhaps the proportion of the ſaliva to the acid was too great, and that a perſon in a putrid diſeaſe ought to take more acids than could be neutralized by the inquiline liquors. However, I will not inſiſt on this; and ſuppoſe theſe experiments to be quite applicable to the caſe: but if theſe mixtures do not efferveſce any more, does it follow, "that "they are neutralized, and therefore act as acids, by "ſaturating any thing of the alcalinous kind, that "they meet with, in their courſe of circulation?" There are ſome ſaline bodies, which do not efferveſce when mixed together; which will, however, change one another's nature. Thus *f. e.* brimſtone, mixed with a ſtrong fixed alcali, does not efferveſce [*o*], but changes, on being diſſolved, the nature of the alcali. A ſolution of ſoap does not efferveſe on the addition of an acid, but joins with the acid, and neutralizes it. Theſe inſtances made me ſuſpect the concluſion drawn by Dr. M'Bride from his experiments; and to clear up theſe doubts, in this particular caſe, I referred to experiments. For this purpoſe, I mixed, the 4th of Auguſt, the thermometer being at 64°, three ounces of ſaliva, a dram of the liquor of

[*o*] This applies alſo to the ſolution of brimſtone in limewater, out of which the lime particles have been precipitated, by the intoduction of fixed air.

putrid

putrid flefh, and a very fmall quantity of bread: and added as much of the diluted fpirit of vitriol, as to make it four, and effervefce definitely with the alcali. There was not any fign of inteftine motion till the 7th of Auguft, when from time to time fome air bubbles, and alfo fome folid particles, rofe to the top; and this continued till the 8th. Not perceiving any farther motion, I poured off the clear liquor, which did not effervefce any more with the alcali. I mixed, the 9th, fix drams of the putrid liquamen, with about the double of this liquor, and put in befides four folid pieces of flefh, which had lain three days in the liquamen: thefe pieces were of a prodigious ftench, and fo rotten, that with the leaft force they were torn to pieces. There appeared no figns of inteftine motion: the 10th, the putrid fmell was very much abated: the 11th, it was changed, and there remained only a fmell much like that of found flefh: the pieces were without any fmell, and had acquired again fome degree of firmnefs. In this condition they remained for a week, and I did not obferve them any longer.

This experiment proves, I believe, that acids, though changed in the alimentary canal fo far, as not to effervefce with alcalies, may notwithftanding check putrefaction; and that, therefore, their ufe is of great confequence, and ought not to be omitted in putrid difeafes. Though Dr. M'Bride believes that thefe difeafes may be cured with fermentable fubftances only; I muft own that I do not agree with him, and am not quite convinced of his opinion, that putrefaction depends only on the loſs of fixed air. I rather believe this an effect than the cauſe of putrefaction; but I ſhall refer this ſubject to another occaſion.

END OF PART I.

PHILOSOPHICAL TRANSACTIONS.

VOLUME LXI. PART II.

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Preſent Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Conſiderable Parts of the WORLD.

VOL. LXI. For the Year 1771.

PART II.

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M.DCC.LXXII.

PHILOSOPHICAL TRANSACTIONS.

PART II.

XL. *Obſervations upon Five antient* Perſian *Coins, ſtruck in* Paleſtine, *or* Phœnicia, *before the Diſſolution of the* Perſian Empire. *In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Cuſtos Archivorum of the Univerſity of* Oxford, *Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

SIR,

Read June 20, 1771.

THE coins before me, as well as ſeveral others ſimilar to them, were undoubtedly ſtruck, in ſome of the cities of Syria, Paleſtine, or Phœnicia, before the reduction of thoſe provinces, and the conqueſt of the Perſian empire, by Alexander the Great. This, if I am not miſtaken, was firſt remarked by (1) M. Baudelot; who has been lately followed herein by (2) M. l'Abbé Barthelemy, and (3) M. Pellerin. I intirely agree whith all thoſe learned men in what they have advanced relative to thoſe coins, and ſhall now beg leave to ſubmit a few curſory obſervations upon five of them (as the ſubject is extremely curious) to the conderation of the Royal Society; which may poſſibly ſerve

(1) Baudel. *l'Utilit. des voyag.* &c. p. 638. A Paris, 1693.

(2) *Journ. des Sçav.* &c. T. LIII. Aout, 1760. p. 279, 280.

(3) Peller. *Recueil de Medail. de Rois,* &c. *Explicat. de la Vignette*, p. iii. iv. A Paris, 1762.

ſerve to evince the truth of what has been offered by thoſe celebrated antiquaries, on this head, and ſet this matter in the cleareſt light.

I.

The firſt of the medals to be conſidered here (ſee Tab. XIII. n. 1.) was given me ſome years ſince, by my worthy friend, the Reverend and learned Mr. Thomas Crofts, late chaplain to the Britiſh factory at Aleppo, and formerly of Wadham College, Oxford; who brought it with him to England, out of the Eaſt. On one ſide we diſcover Atergatis, Adergatis, or Derceto, taken by ſeveral learned (4) men, for the Dagon of Scripture, nearly as we find that pagan divinity deſcribed (5) by Diodorus Siculus, and Lucian, with a pigeon before her, and a fiſh in her right hand. On the other, we perceive a galley, or ſmall veſſel, on the ſea, with rowers in it; under which there appears a ſea-horſe, or rather a ſea-monſter, of a very particular form. Near the face of Adergatis, the two Phœnician letters, M A, preſent themſelves to our view. The piece is in good conſervation, having ſuffered very little from the injuries of time.

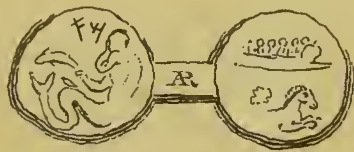
That this ſilver medal muſt have been anterior to the diſſolution of the Perſian empire, we may fairly collect from the reverſe; which agrees in every particular, but the ſea-horſe, with the reverſe of a Daric, that undoubtedly preceded the abovementioned event, and exhibits the very ſame Phœnician letters, with which it is adorned. But this will be rendered inconteſtable by a bare inſpection of the draught of that

(4) Seld. *de Diis Syris*, Syntagm. II. cap. iii. Andr. Beyer. ad Joh. Seld. ubi ſup. p. 300.

(5) Diod. Sic. *Bibl. Hiſt.* lib. II. Lucian *de Dea Syr.* apud Johan. Selden. ubi ſup. Vid. Athen. *Deipnoſoph.* lib. VIII.

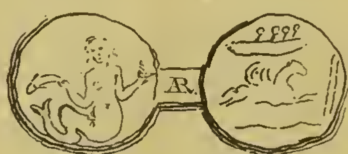
Daric,

TAB. XIII. n. 1.

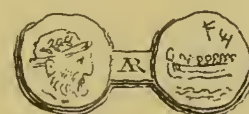


TAB. XIII. n. 2.

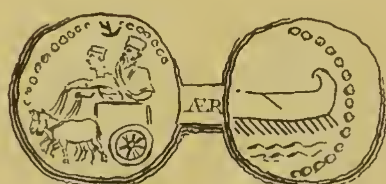
TAB. XIII. n. 3.



TAB. XIII. n. 4.

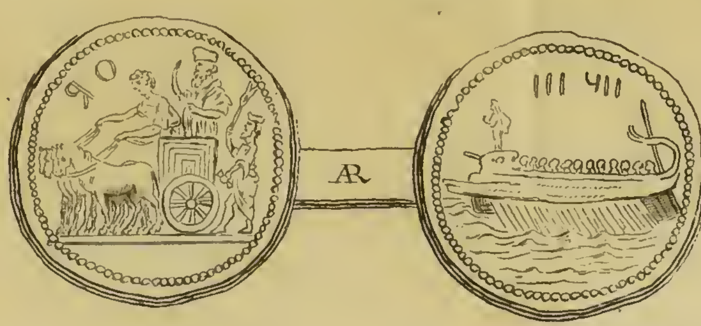


TAB. XIII. n. 5.



TAB. XIII. n. 6.

TAB. XIII. n. 7.



Daric, and others of ſimilar coins, in the (6) plate referred to here.

That this piece was ſtruck at Aſcalon, a very antient and celebrated city of Paleſtine, there is, I think, little reaſon to doubt. Dagon, or Atergatis, was a deity of the Philiſtines, to whom Aſcalon appertained, as we learn from (7) Scripture; and therefore may very naturally be ſuppoſed to have been worſhiped there, as well as in the other principal cities belonging to that people. We are aſſured by (8) Diodorus Siculus, and Lucian, that Aſcalon was famous for the worſhip of Atergatis, or Derceto, and the ſuperb temple of that deity there. The coins of Aſcalon (9) not infrequently exhibit Atergatis, with a pigeon, as here; pigeons (10), as well as fiſhes, having been conſidered as ſacred animals, bearing a near relation to Atergatis, if not as objects of religious worſhip, in that city. The reaſon of this is given us in few words, by a (11) very learned author, who ſets the point here inſiſted on beyond diſpute. I own, indeed, the divinity in queſtion is ſaid to have had a temple at Hierapolis, and to have been worſhiped there; but this, according to the great (12) Mr. Selden, ſeems to be a miſtake. Beſides, the goddeſs of Hierapolis was worſhiped under (13) a human form, and not with the tail of a

(6) *Numiſm. Antiqu.* &c. à Thom. Pemb. et Mont. Gom. *Com. collect.* p. 2. T. 75.

(7) 1 Sam. v. 2, 3, 4, 5, &c.

(8) Diod. Sic. & Lucian. ubi ſup.

(9) Joan. Vaill. *Nnmiſm. Imperator. &c. à Pop. Rom. Dit. Græcè loquent. &c.* p. 81. Henr. Noris, *An. et Epoch. Syro-Maced.* p. 510. Lipſiæ, 1696. (10) Joh. Selden. ubi ſup. p. 192—202. Amſtelodami, 1680. (11) Id. ibid. (12) Seld. ubi ſup. p. 192. (13) Id. ibid.

fiſh, as Derceto is repreſented on the medal I am conſidering. Nor do I remember ever to have ſeen Atergatis, or Derceto, in that form, or attended by a fiſh and a pigeon, as on my medal, on any of the Hierapolitan coins. Laſtly, I have a braſs medal of Aſcalon (ſee TAB. XIII. n. 2.), in my ſmall collection, with a galley, or little veſſel, on the water, and rowers in it, as we find exhibited by the piece before me, over which the two Greek letters ΑΣ plainly appear; which ſeems moſt clearly to evince, at leaſt the high probability of, the point in view. It muſt therefore be allowed extremely probable, if not abſolutely certain, that the coin conſidered here was ſtruck at Aſcalon, though current throughout Syria, Paleſtine, and Phœnicia, before the reduction of thoſe provinces by the arms of Alexander the Great.

As no chronological characters on the piece in queſtion preſent themſelves to our view, it will be extremely difficult, if not impracticable, to aſcertain, with any preciſion, the time when it firſt appeared. However, I cannot help thinking it probable, that the coin was ſtruck about 351 years before the birth of CHRIST, when the (14) provinces of Paleſtine and Phœnicia were ſubdued by Artaxerxes Ochus, ſoon after they had revolted from him. The people of thoſe provinces might then have uſed money ſimilar, at leaſt in ſome reſpects, to that which was current in Perſia, either out of compliment to, or by the poſitive order of, that prince. This, I ſay, might not improbably have been the caſe; but that it really was ſo, I muſt not preſume abſolutely to affirm. Be this, however, as it will, a circumſtance

(14) Diod. Sic. *Bibl. Hiſt.* Lib. XVI.

will occur, in the explication of the fourth medal to be confidered here, that will bring no fmall acceffion of ftrength to the notion I would now recommend to the attention of the learned world.

With regard to the two Phœnician letters exhibited by this coin, they feem either to form the word מא, MA, which in Phœnician not improbably denoted WATER, or the SEA, (15) as in Arabic, or to be the two firft elements of the word MAIVMA, in Syriac (16) fignifying likewife WATER, the name of the port and place of the magazine of naval ftores, fuch a port and place having formerly appertained both to Afcalon (17) and Gaza. For thofe two Phœnician letters, preferved on many medals, (18) can fcarce always be looked upon either as the whole or part of the proper name of the place where the coins were ftruck, as they feem firft to have appeared in different towns; though it muft be owned, that MAIVMA, as applied to the ports of Afcalon and Gaza, was (19) confidered as the proper name of a town, erected at a fmall diftance from each of thofe two cities. However, I fhould rather take the word MA to have denoted WATER, or THE SEA, as a fmall veffel on the fea is vifible on thefe coins, and the word MAIVMA, as applicable to the ports of Afcalon and Gaza, feems not to have (20) been ufed long before the time of Conftantine the Great:

(15) Gol. Lex. Arab. p. 210. Val. Schind. Lex. Pent. p. 994. Hanoviæ, 1612.

(16) Gothofred. apud Henric. Noris, ubi fup. p. 511.

(17) Sozom. Lib. ii. c. 4. Henr. Noris, ubi fup. p. 511, 512.

(18) *Numifm. Antiqu.* &c. *à* Thom. Pemb. *et* Mont. Gom. *Com. collect.* p. 2. T. 75. n. 7, 8.

(19) Henr. Noris, ubi fup. p. 511. (20) Id. ibid.

unlefs

unleſs we would ſuppoſe it to denote AN HVN-DRED, and to refer to ſome remarkable occurrence, from whence the Perſians dated their computation of time, in the days of Ochus, to us at preſent utterly unknown.

II.

The ſecond medal (ſee TAB. XIII. n. 3.) I receiv-ed, as a preſent, from the Reverend and learned Mr. Crofts, who brought it with him from Syria, at the ſame time that he gave me the firſt. Atergatis, or Der-ceto, on this ſilver piece, holds a *concha-marina*, or ſea-ſhell, in her left hand; but, in all other reſpects, it is ſo ſimilar to the former as ſufficiently appears from the draughts of them both, that it may almoſt, if not abſo-lutely, paſs for a duplicate of the ſame coin. The piece, however, has been but indifferently preſerved; ſo that without the aſſiſtance of the medal already deſcribed, it would have been of no great ſervice to the learned world.

As the two Phœnician characters, that occur on the firſt medal, have been intirely defaced on this, by the injuries of time, I can offer nothing relative to them here. It may not, however, be improper to obſerve, that neither this nor the former medal has yet, as far as I can find, been ever communicated to the learned world.

III.

The third medal (ſee TAB. XIII. n. 4.) is a very ſmall ſilver piece, and was preſented to me by my worthy and learned friend, the Reverend Mr. Thomas Crofts, who brought it with the other two, above de-ſcribed, out of the Eaſt. The reverſe, which exhibits the two Phœnician elements 𐤀 𐤌, MA, and a galley, or ſmall veſſel, full of rowers, on the water, almoſt intirely agrees with that of two Perſian Darics (21), as will appear from an inſpection of the draughts of them, in the plate here referred to. This indicates the piece to have been ſtruck in Paleſtine, or Phœnicia, before the diſſolution of the Perſian empire, probably at the ſame time that the two former firſt appeared. On the other ſide we obſerve a laureated antient head, which I take to repreſent Jupiter Marnas, a deity worſhiped (22) at Gaza, a celebrated antient city, at no great diſtance, 3 paraſangs only, from Aſcalon; who might therefore probably have had divine honours paid him there, as well as at Gaza; and conſequently we may attribute the coin to either of thoſe cities, though, on account of the head of Jupiter Marnas, I ſhould rather prefer Gaza. This, I ſay, appears to me extremely probable; but that either the laureated head really repreſents Jupiter Marnas, or the piece was certainly ſtruck at Gaza, I muſt by no means take upon me abſolutely to decide.

If the head on this medal ſhould not be imagined to point out Jupiter Marnas, the local deity worſhiped at Gaza, it may perhaps be ſuppoſed to repreſent ſome hero, or the founder of Aſcalon, or Gaza. Be that, however, as it will, as either this very coin, or one exceedingly ſimilar to it, has a place aſſigned it in (23) Lord Pembroke's noble collection, it can ſcarce be allowed to paſs for an inedited coin.

(21) *Numiſm. Antiqu. &c. à* Tho. Pemb. *&* Mont. Gom. *Com. collect.* p. 2. T. 75. n. 7, 8.

(22) Henr. Nor. ubi ſup. p. 494. Golii not. ad Alfragan. p. 142.

(23) *Numiſm. Antiqu.* &c. ubi ſup. p. 2. T. 75. n. 8.

IV.

IV.

The fourth (ſee TAB. XIII. n. 5.) is a ſmall braſs medal, that may paſs for an inedited coin, though one not unlike it has been publiſhed by M. Baudelot. On one ſide a human figure, that probably repreſents a king of Perſia, with a Perſian tiara on its head, in a triumphal car, drawn by two horſes, and driven by a ſimilar figure, with a Perſian tiara likewiſe on its head, preſents itſelf to our view. On the other, a veſſel navigated by rowers, reſembling that exhibited by the three foregoing coins, may be clearly diſcerned. The piece has been well preſerved, and was undoubtedly anterior to the reduction of Syria and Phœnicia by Alexander the Great. For that the perſon in the car is a Perſian, we may infer from the tiara on his head, which occurs on the heads of ſeveral Perſian figures (24) in the ruins of Perſepolis; and that he was a royal perſonage, appears from hence, that the kings of Perſia only had their effigies impreſt on the Perſian coins. It is true, ſome of the figures in the ruins of Perſepolis are the produce of the Parthian times, and ſeveral of them even coeval with ſome of the princes of the houſe of Saſſan, and conſequently of ſtill a later date. But, notwithſtanding this, the figures now in view were undoubtedly Perſian, and fully evince the point they are brought to prove; but had they been Parthian, that would have made no alteration in the preſent caſe, as the Perſians and the Parthians ought to be conſidered as one and the ſame nation, and their attire as one and the ſame.

(24) Engelbert. Kæmpfer. *Amœnitat. Exoticar.* Faſcic. p. 345, 312, 340, &c. Lemgoviæ, 1712.

That

That the piece then was ſtruck in Paleſtine, or Phœnicia, whilſt under the domination of the Perſians, there is, I think, little reaſon to doubt; though it may, perhaps, be not altogether ſo eaſy to aſcertain, with any preciſion, the time when it firſt appeared. There is, however, one period, and one only, as I apprehend, in the Perſian hiſtory, to which this may, with the ſtricteſt propriety, be referred; and that is, immediately after the reduction of Sidon, by Artaxerxes Ochus, when the Phœnicians, who had before entered into an alliance with Nectanebus, king of Egypt, and aſſerted their independency, made their (25) ſubmiſſion to him. This happened in the year of the Julian period 4363, about 351 years before the (26) birth of Chriſt. That prince having then intirely ſubdued the Phœnicians, who had revolted from him, and reinforced his army with a body of 10000 Greeks, reſumed his deſign of invading Egypt; and, (27) after the ſurrender of Jericho, probably advanced at the head of his forces to Aſcalon and Gaza, through which he might have paſſed, on his route to that country, though he ſeems to have undertaken the Egyptian expedition, or rather to have entered Egypt, the following year. Upon his arrival at Aſcalon, he may naturally be ſuppoſed to have ſtruck ſome of the pieces, at leaſt, conſidered here; and particularly that which is the preſent object of my attention, with the repreſentation of a Perſian king, in a triumphal car, upon it. For this muſt ſeem naturally to have announced the intire reduction of Syria and Phœnicia, that had juſt before ſubmitted to him.

(25) Diod. Sic. ubi ſup.

(26) Jac. Uſſer. *Annal.* ad An. Jul. Period. 4363. p. 146, 147. Genevæ, 1722.

(27) Solin. cap. 35.

As therefore the coins in queſtion are found in (28), and come from, thoſe parts of the Eaſt; I would willingly flatter myſelf, that what is here advanced will meet with the approbation of the learned.

There is one farther circumſtance relative to this coin, which muſt not be omitted here. Over the Perſian monarch's head, at a ſmall diſtance from it, we may eaſily diſcover a kind of letter (29), not appertaining, as it ſhould ſeem, to the Phœnician alphabet; which, as I apprehend, may be taken for an antient Perſic character. It not a little reſembles that endued with the power of the ſhort A, deduced from the Zend and Pazend, by (30) Dr. Hyde; as alſo

(28) Peller. ubi ſup.

(29) This coin ſeems to have been ſtruck either at Aſcalon, Gaza, or Acco; as moſt of the ſimilar coins hitherto publiſhed probably firſt appeared in one of thoſe cities. But the names of all thoſe places begin with an *Ain*, not an *Aleph*, and therefore the character in queſtion cannot be ſuppoſed to have appertained to any of thoſe names. Whereas Ardſhîr, Ardſchîr, or Artaxerxes, the name of the king, in whoſe reign the coin ſeems to have been ſtruck, has for its firſt letter, an *Aleph*, not an *Ain*; and therefore the character on this piece may be preſumed, after what has been ſaid, to point at that name. And this will be rendered ſtill more probable by the poſition of the Perſian character I am here conſidering. It is placed over the head of the great perſonage in the triumphal car, or rather almoſt contiguous to the tiara with which it is covered. This appears to me almoſt deciſive in favour of what is here advanced. If any learned man, however, ſhould diſſent from me in this particular, I ſhall not quarrel with him for adhering to his own opinion. Baudel. Peller. paſſ. Barthel. Schikard. *Proſap. Perſ. Proem. et alib.* Tubingæ, 1628.

(30) Vid. Lit. in Libr. Zend & Pazend, juxta apographum D. Hyde, uſitat. &c. apud D. Hyd. in *Hiſt. Relig. Vet. Perſ.* Ed. Oxon. 1760. *Memoir de Litterat.* Tom. XXXI. p. 358. A Paris, 1768. That the Perſians uſed the Aſſyrian letters in the days of Herodotus, we learn from that celebrated hiſtorian. But, notwithſtanding this, they might likewiſe have uſed other antient alphabetic characters, at leaſt in ſome part of the inter-

two others belonging to two of the antient Perſian alphabets publiſhed by M. Anquetil du (31) Perron, to which he aſſigns the power of A. All which if we admit, it may probably be conſidered as the

val between the commencement and the diſſolution of the Perſian empire, if not throughout that interval. And that they had actually then ſuch characters, the coin under conſideration here gives us good reaſon to believe. But we muſt not poſitively aſſert this, ſince nothing certain or deciſive, without the farther aſſiſtance of antient coins, or other genuine monuments of antiquity, can be offered in ſupport of ſuch an opinion.

The very learned M. la Croze, takes the antient letters of the Perſians, ſo many ſpecimens of which have been given us by Dr. Hyde, to have been originally deduced from the Syriac alphabet. But this is by no means certain. That they are, however, more antient than has been hitherto generally imagined, will be readily acknowledged, by all who admit the character in queſtion to be one of the letters of the antient Perſic alphabet, as I am inclined to believe it is. But neither muſt this be laid down as a fact, till it is confirmed by other antient coins, or by other authentic monuments of antiquity, that may be intirely depended upon. Herodot. lib. IV. c. 87. Matur. Veys. la Croze, apud Joan. Chamberlayn. *Diſſertat.* &c. p. 129, 130. Amſtelædami, 1715.

(31) M. Anquetil du Perron has lately publiſhed, at Paris, his tranſlation of the Zend-Aveſta of Zeratuſht. In the preliminary diſcourſe, or introduction to which, he has been pleaſed to abuſe and ridicule two of the moſt reſpectable members of our Univerſity, as well as me. This conduct is the more extraordinary, as, by his own account, the polite and friendly reception he met with from us, during the two days he reſided in Oxford, towards the cloſe of January, 1762, ought to have inſpired him with ſentiments very different from thoſe of hatred and averſion to the whole Engliſh nation; which he, though greatly obliged to them both in the Eaſt Indies and in England, diſcovers in many parts of his firſt volume. But I mean not at this time to recriminate. Nor indeed is there any need of a recrimination. His own account of the treatment he met with at Oxford, from the gentlemen he has ſo groſsly abuſed, is a much ſeverer reflection upon him than any thing I have ſaid, or could have ſaid, of him, here.

In my preſent circumſtances it would be improper for me to characterize his tranſlation of the Zend-Aveſta, as I might ſeem to be acted by too vindictive a ſpirit, if I delivered here my true ſentiments of it. But I would beg leave to recommend to the initial letter of Ardſhîr, Ardſchîr, or Artaxerxes, the name of the prince in whoſe reign, and by whoſe command, the piece was ſtruck. This, if allowed, will be an additional proof of the truth of what has been here advanced, relative to the time in which the medal firſt appeared, as well as to the occaſion of that appearance, hinted at above. It is certain, ſuch initial letters ſometimes repreſented the names of kings and great men amongſt (32) the Greeks, who were neighbours to the Perſians, and were imitated by them in ſeveral particulars, even as early as the year wherein I believe this coin to have been ſtruck. This obſervation will throw no inconſiderable light upon what I would now propoſe to the conſideration of thoſe that are the beſt verſed in this branch of literature, and render my elucidation of the medal before me, as well as of the others conſidered here, not altogether unworthy the attention of the learned.

V.

The fifth medal (ſee TAB. XIII. n. 6.) is extremely ſimilar to the fourth, but very ill preſerved (33). The

learned world the peruſal of a little piece lately publiſhed *, highly meriting their attention; the ingenious author of which, a moſt valuable member of our Univerſity, though a very young man, is at leaſt a match for M. Anquetil, in the knowledge of the Arabic, Perſic, Turkiſh, &c. languages, as well as other branches of oriental literature, and ſeems to have done him tolerable juſtice, by giving us a clear and adequate idea of this performance. However, I may poſſibly hold him up to the public, in his proper colours, on ſome future occaſion.

(32) Eraſm. Frœl. *Not. Element. Numiſm. Ant.* p. 138, 142. Viennæ, Pragæ, et Tergeſti, 1758.

(33) I ſhall beg leave in this place to remark, that we find a noble Perſian ſilver medallion (ſee TAB. XIII. n. 7.), publiſhed by M. Pellerin, with a Perſian monarch, in a triumphal car, drawn

* *Lettre à Monſieur A—— du P——*, à Londres, 1772.

former,

former, however, differs from the latter in this, that it exhibits a lacquey, or ſlave, as it ſhould ſeem, following the triumphal car. This renders it ſtill more probable, that the figure in that car was intended to repreſent a perſon of the firſt diſtinction, or rather a Perſian monarch. Both of theſe coins have been hitherto unpubliſhed (34), though of medals ſimilar to them, in conjunction with ſeveral others of the ſame kind, one (35) of the moſt valuable parts of Lord Pembroke's ineſtimable collection has been formed.

If there were originally any characters on the piece laſt deſcribed, they have all been effaced by the injuries of time. From the fourth medal I have attempted to elucidate in this paper, it ſeems to appear, that certain antient Perſian letters, as well as Phœnician, are ſometimes handed down to us, by this ſpecies of coins. A draught of one of them, in the (36) plate here referred to, may poſſibly be thought, by ſome, to bring a freſh acceſſion of ſtrength to ſuch an opinion.

Thus have I endeavoured to illuſtrate five very valuable antient coins, ſtruck in Paleſtine, or Phœ-

by two horſes, and followed by a lacquey, or ſlave, on one ſide, as here, attended by the Phœnician legend ϘO ; which is manifeſtly equivalent to the three letters ערכ, ARC, the ſecond character being a Phœnician monogram, formed of *Reſch* and *Caph*. On the other ſide, we diſcover a galley, navigated by rowers, and adorned with one of the *Dii Patæci*, at the ſtern. Over the galley, there appears a date; which may, perhaps, denote 203, or 213, if we ſuppoſe the character repreſenting *Ten*, now defaced, to have originally occupied the chaſm in the middle of the numerical characters, though the æra to which this refers I cannot take upon me to aſcertain. The letters ערכ, ARC, evince the piece to have been ſtruck at ARCA, or ARCE, a city of Phœnicia, between Byblus and Heliopolis, called by the Greeks Ἄρκη, or Ἄρκαι, as we learn from Stephanus Byzantinus. *Steph. Byzant. in voc.* Ἄρκη. *Vid. etiam Lucæ Holſtenii not. in loc.*

(34) *Numiſm. Antiqu.* &c. p. 2. T. 75.

(35) *Numiſm. Antiqu.* &c. p. 2. T. 75. & 87. n. 2.

nicia, before the time of Alexander the Great, almoſt intirely unnoticed by any other writer; and this purely in order to excite others of more learning, greater abilities, and better verſed in theſe matters, to conſider with proper attention, and treat more copiouſly, a very curious ſubject, hitherto but barely touched upon by the learned. And as the paper now ſent you may not improbably produce ſuch an effect, it may poſſibly contribute to the farther extenſion of the knowledge of antient medals, at leaſt by introducing to future diſcuſſion ſeveral important points, relative to that ſpecies of literature, at preſent utterly unknown. You will therefore excuſe the trouble given on this occaſion, by,

S I R,

Your much obliged,

and moſt obedient, humble ſervant,

Chriſt-Church, Oxon.
March 16, 1771.

John Swinton.

(36) *Numiſm. Antiqu.* &c. ubi ſup. p. 2. T. 87. n. 2.

Received

Received January 4, 1771.

XLI. *A Letter from* Richard Waring, *Esq*; *F. R. S. to the Hon.* Daines Barrington; *on some Plants found in several Parts of* England.

S I R,

Read Nov. 14, 1771. I WOULD sooner have acknowledged the honour of your very obliging anfwer to the letter, I took the liberty of writing to you, if I had not waited a while, in hopes of having fomething to communicate, that might, in fome degree, merit your acceptance. Nothing better, than the following, having yet fallen in my way, I prefent you with

A catalogue of fome indigenous plants, in places not heretofore mentioned, in the counties of Salop, Stafford, Chefter, Flint, Denbigh, Carnarvon, and Merioneth, that are fcarce in this ifland, or have been generally fuppofed to be fo, or not indigenous; and occafionally of fuch in other counties; and fome, that, though common in fome other counties, are fcarcely, or not at all to be found in thefe; and alfo of fuch as may be doubtful, perhaps originally foreign, though generally fuppofed to be natives of Britain.

 Acer

Acer majus Ger. Nullibi quod ſciam in Anglia ſponte oritur. Raii Syn. A ſtranger in England, only where planted. Gerard. Perhaps we have not any tree more hardy, or more apt to be propagated from the ſeeds; ſince thoſe of this tree do not often fail of taking root, upon whatever ſoil they fall, and if they were not heavier than ſome other winged ſeeds, or leſs coveted by birds than ſome of the more ſolid or pulpy kinds, there would perhaps be no reaſon, why this tree ſhould not be as much diſperſed throughout the country as the moſt common tree we have; except that it may have been more lately introduced among us.

Agaricus parvus lamellatus, pectunculi forma, elegans. Dill. Cat. Giſſ. Here in November, on ſticks rotted on the ground.

Agaricus trilobatus, ſupernè albus, lævis, infernè ferrugineus, foraminibus oblongis et rotundis eleganter punctatus. An Fungus arboreus lobis rubellis, diverſimodè figuratis et punctatis. R. Syn. On a hazel tree near Congleton, Cheſhire, in October.

Angelica ſativa C B. Ray in Cat. Angl. tit. Hypposelinum, ſays, "By this, when young, I ſuppoſe "they were deceived who gave information to the "compilers of Phyl. Brit. that Angelica ſativa "grew on the rocks near Berwick." They might have taken it from Dr. Johnſon's Merc. Botan. However it is wild in many places by the Thames-ſide, particularly at Stangate, Lambeth; and I have found it far from any water, in a gravel-pit near the end of St. Edmund's-bury, toward Sudbury.

Apium petræum, ſive montanum album J. B. On Halton-caſtle, and on a wall at the chamber of the foreſt De-la-mer, Cheſhire, and on the garden-wall of the Abbey of Perſhore, Worceſterſhire.

Aquilegia flore cæruleo Ger. In ſylvis et dumetis. R. Syn. I have not ſeen it wild.

Aquilegia fl. purpureo. Some years ago I found one only in a wood here, where none has ſince appeared.

Aquilegia fl. atro rubido. Four of this kind on a ſmall bank by the high-way about half a mile from Wore, Salop (towards Namptwich).

Berberis dumetorum. C B. In the hedges on each ſide of the road from Ludlow, oppoſite to Hawford-chapel, and thence to a brook about a mile off, plentifully, and leſs ſo about half a mile further, toward Stretton, Salop; alſo in ſome places between Shrawarden and Neſs-cliff, and between Blacker's-ford and Prees in that county: but whether elſewhere than in hedges, or there ſpontaneous, or ſown, or planted, I do not know.

Betulus ſive Carpinus Ger. In ſylvis et ſepibus. R. Syn. I have not obſerved it more northward than Bedfordſhire, though Gerard ſays, "plentifully in Northamptonſhire."

Biſtorta major Ger. In pratis humidis et umbroſis. Ph. Brit. In a trench without the Weſtern wall of the caſtle of Ruthin, Denbighſhire.

Blattaria lutea J.B. By the road ſide a little ſhort of a place called the Marſh between Ludlow and Stretton, Salop, I once obſerved a ſingle plant; and this year in a wild and long neglected part, perhaps three perches, of a gentleman's garden, near

near Ofweftry in that county, I faw, I believe, above a hundred. Neither the gentleman, nor his gardener, could account for the production of them; or had ever obferved them before that inftant.

Cardamine tripla, fc. ad gradum quartum, prolifera, floribus plenis. I have often found it fo hereabout, and near Ofweftry, and fufpect it may not be uncommon, though unobferved elfewhere.

Cardamine flore majore elatior Tourn. Inft. Ñafturtium aquaticum, amarum. Park. In the bog near the bath at Willowbridge, Staffordfhire.

Carduus Mariæ Ger. Ad agrorum margines et in aggeribus foffarum non raró occurrit. Ray Hift. Though frequent about London, it is fo rare in thefe counties, that I have feen of it only in the church-yard at Ince in Chefhire, and about the caftle at Caergwrle, and in two other places, in this county of Flint. I have not feen one good figure of this elegant plant.

Carduus Mariæ non maculatus R. Hift. hirfutus non maculatus Ph. Brit. The word *hirfutus* feems to have been improperly added, for Ray in Hift. fays, "non alia in re à vulgari Carduo Mariæ differt "quam quod folia edat maculis deftituta;" and I have feen it about Kennington-common, Surry, as fmooth as the former fpecies, which is rather veined than fpotted.

Carduus tomentofus, Acanthium dictus, vulgaris R. Syn. Spina alba fylveftris Fuchfio J. B. "In "aggeribus foffarum, ad fepes et in incultis non "raró occurrit." R. Hift. I have feen it only in the neighbourhood of London.

Carduus capite tomentoſo J. B. A little ſhort of Gate-houſe-green, in the road from Middlewich, Cheſhire, plentifully; and not any where elſe, that I have ſeen, in theſe counties.

Carduus nutans J. B. "In agris incultis et reſtibilibus "non rarò provenit." R. Hiſt. It is not frequent in this part of the kingdom, where I have obſerved it only on the Bailey-hill at Mold (there but ſparingly), and about Pont-newidd near Kilken in this county, plentifully.

Carduus ſtellatus ſive Calcitrapa J. B. "Juxta vias "publicas, circa oppidà et pagos, inque incultis et "ſterilioribus frequens eſt." R. Hiſt. And, according to Gerard, this and Carduus ſolſtitialis are "in barren places by cities and towns, almoſt every "where." The laſt is a ſtranger to me, and I have not ſeen the former more Northward than between Northampton and Leiceſter.

Caryophyllata vulgaris majore flore, C. B. Vulgaris major Park. I once found it on a rock in a wood near this place.

Caryophyllata aquatica nutante flore C. B. aquatica flore rubro, ſtriato J. B. montana, ſive paluſtris, purpurea Park. In many damp parts of the woods here, ſo abundantly, that a great deal of ground is intirely covered with it. Parkinſon's figure, which is taken from that of Caryophyllata aquatica Matth. repreſents the leaves very different from thoſe of this plant, and not at all anſwerable to his own deſcription of it. He there very properly obſerves, that they are ſomewhat like to, but with longer foot-ſtalks than, his Caryophyllata montana, the figure of which he has copied from that of Caryophyllata major rotundifolia, Caryophyllata montana Matthioli L'Ob.Icon. but Matthiolus's figure of Caryophyllata montana reſembles them only in the leaves and root. L'Obel's (and conſequently Parkinſon's) figure is much better in reſpect of the flower; though the petala are not very diſtinct, nor is the thrum prominent enough, which two faults are in all the figures, I have ſeen, of this plant.

I once found here, together with this kind, one of a fine bright yellow; but not otherwiſe different. An Caryophyllata montana flore luteo nutante Hort. Reg. Par.? and once alſo one of the uſual hue, double and proliferous. An Caryophyllata montana flore pleno prolifero Breynii R. Hiſt.? But I ſuſpect, they were only accidental varieties.

Caryophyllus montanus minor C. B. marinus minimus l'Ob. Icon. Not only on the ſalt marſhes in our maritime counties, and on the higher rocks of Snowden, as already obſerved: there are two large incloſures, about two miles by computation, from Dôlgelle, and a large mooriſh place about two miles further, toward Feſtiniog, almoſt covered with it.

Caſſida paluſtris vulgatior fl. cœruleo Tourn. Lyſimachia galericulata l'Ob. In this part of England, I have obſerved it only by the ſides of a ditch and rill dividing the parts and the home-houſe paſtures at Ince, Cheſhire, and about thirty paces from Thornton-brook. There plentifully.

Centaurium luteum perfoliatum J. B. &c. Not very common in theſe parts; but we have it here in ſome of the upper paſtures.

Ceraſus

Cerafus avium nigra et racemofa, Ger. Very common in woods, thickets, and hedges hereabout. I have alfo feen it in hedges in Denbigh and Merionethfhires.

Chamæciftus vulgaris, fl. luteo, C. B. Heleanthemum vulgare, fl. luteo, I B. On the rocky hills about Llanveras, Denbighfhire. Though ufually on fuch dry, and efpecially on chalky foils, I have feen it even in a damp and very fhady part of a woody dingle, at Hayes near Ofweftry.

Chamædrys vulgaris Park. feu fativa R. Syn. An Chamæmorus minor repens C.B. ? Plentifully on a wall of the caftle at Whittington, Salop.

Chamæmorus ———. Gerard has the Knot or Cloudberry twice, under the feveral articles of Chamæmorus and Vacinia nubis; and in Phyt. Brit. it is feverally underftood under thofe of Chamæmorus, Ger. and Vaccinia nubis, Ger. et Chamæmorus ejufdem, the former of which, according to Gerard and Phyt. Brit. after him, is "upon Stanmore, between Yorkfhire and Weft-"moreland," the other, according to Ph. Brit. "upon the tops of the high mountains both of the "North and in Wales."

Parkinfon makes two fpecies or varieties of Chamæmorus, *viz.* Chamæmorus Anglica, and Chamæmorus Cambro-Britannica, five Lancaftrenfe Vaccinium nubis, and fays, "Dr. L'Obel found "the laft growing in Wales, and the fhires near "thereunto, as well as in Lancafhire:" but L'Obel has not given a figure of either. Parkinfon's figure of the former is like that of Rubo Idæo minori affinis Chamæmorus J. B.

Childrey,

Childrey, in Brit. Bac. ſays, the Cloudeſberry is peculiar to the top of Pendle-hill in Lancaſhire, and I do not know that any later author places it in Wales.

Upon the 10th of Auguſt laſt, I found about Cader Vron-wen, the higheſt point of Berwin mountains, in Denbighſhire, one ſpecies of Chamæmorus in vaſt abundance. The principal ſtalks, and the boſoms or wings of the footſtalks of the leaves, were like thoſe of the figure mentioned; but the leaves like thoſe of Chamæmorus Cambro-Britannica Park. The calix, or rather the perianthemum, in ſome plants, had five, in others only four diviſions. Not only the flowers were paſt, but the berries were fallen or had been gathered.

Clematitis ſylv. latifol. ſive Viorna Park. "Ubique "in ſepibus," Merr. Pin. I have not obſerved it more Northward than between Stouton and Perſhore, Worceſterſhire; and between Plumb-park corner and Cuttle-mill, Northamptonſhire.

Cochlearia rotundifol. minima, Park. In the graſs, near the top of Snowdon.

Conyza major Matthioli, ſive Baccharis quibuſdam, J. B. More ſparingly in theſe than in the more Southern counties. In the inner court, and the walls above the leads of the caſtle of Ludlow, Salop, and along the highways, many miles about that town; alſo about Stretton in that county: in the road about a quarter of a mile from Denbigh toward Llanſanan, and on a rock almoſt at the bottom of Snowdon (near Llyn. ſynnon glâs).

Conyza

Conyza paluſtris ſerratifol. C. B. paluſtris major Park. By the Eaſtern ſide of the river Dee, juſt above Eaton-boat, Cheſhire, ſparingly.

Coronopus vulgaris ſive Cornu cervinum, Park. In this part of the county, I have not ſeen it far from the ſea.

Cratægus fol. laciniato Tourn. Sorbus torminalis, C. B.

Cratægus fol. ſubrotundo, ſerrato, ſubtus incano Tourn. Sorbus ſylveſtris Aria Theophraſti dicta, Park. I have not obſerved either this or the former in this part of the kingdom; though Gerard ſays, Sorbus (he ſeems to mean Sorbus ſativa, C B. which is ſtill more rare, if at all of natural growth in this land) and Sorbus torminalis grow " in woods and groves, in moſt places " of England."

Cratægus Fennica Kalmii, Lin. Fl. Suec. On the wall of Caſtell Dinas brân, Denbighſhire, two plants in 1764, and in 1770, a few younger, but the larger of the two former then dead.

Cruciata vulgaris, Park. From ſome particular native places mentioned by authors, it ſeems not to be common in the counties about London: but there is ſcarcely any herb more ſo in this part of the kingdom.

Cyanus fl. albo, maculis cœruleis ſive purpureis notato, ſtaminibus cœruleis. About St. Edmund's-bury.

Cymbalaria, C. B. On many walls, beſide thoſe already mentioned, by the Thames-ſide; particularly from Mill-bank to Weſtminſter-bridge, and at Lambeth, oppoſite: at the Savoy, and on Somer-

ſet-houſe

ſet-houſe garden-wall. On the brick-walls at Brinkinnalt, the ſeat of lord Dungannon, in Denbighſhire, abundantly. I could learn only, that it was known to have been there above fifty years.

Dipſacus minor ſeu Virga paſtoris, Ter. Emac. "In humidis et aquoſis, ad ſepes et rivulos." R. Syn. We have it hereabout, and I have ſeen it in a few places in Denbighſhire and Shropſhire. It is however far from being general.

Echium vulgare, J. B. &c. "Secus vias et ſemitas, "inque muris non rarò, et arvis ſterilioribus nimis "etiam frequens." R. Syn. By the road-ſide, about a mile from Hamſtead (toward Wolverhampton) Staffordſhire, and at Wrine-hill, Cheſhire; not elſewhere, that I have ſeen, nearer this way, than Broadway-hill, Worceſterſhire.

Elaphoboſcum Matth. By the ſides of Stanny-brook, in many places toward Ince, Cheſhire.

Equiſetum ſylvaticum tenuiſſimis ſetis, C. B. By the road-ſide (between Trevalin and Hope, Flintſhire) in the townſhip of Llai, Denbighſhire.

Erica vulgaris hirſutior, Park. That on one of the Camp-hills, Staffordſhire, ſeems to be intirely ſuch (though not on the adjoining hills), and it is very frequent, with the common ſort, in many other places.

Erica baccifera procumbens nigra, C. B. On Salatin-mountain, Salop; on the drieſt parts only of the mountain called Gwern-to in Flintſhire, and on the bog at Willowbridge, Staffordſhire.

Euonimus vulgaris, Park. "In ſepibus frequens." R. Hiſt. It is very infrequent this way. I have ſeen it growing ſingly, not in many places, in Shropſhire,

Shropſhire, and in one place only in this county of Flint.

Fagus, Park. "This groweth through moſt woods "in England." Park. Not *through* many in this part of England, though I have ſometimes, even this way, ſeen a young tree, ſpring from the fallen maſt, among old ones, that had been planted; and this being more frequently the caſe in ſome parts, where the ſoil is more ſuitable, the obſervation of Cæſar may ſtill be juſt, that in his time there were not any trees of this kind in Britain.

Flos Adonis, Park. Plentifully on the ground among the ruins of the Abbey, at St. Edmund's-bury.

Fœniculum ſylveſtre, Park. & vulgare ejuſdem; for according to R. Hiſt. they do not differ. On the caſtle-bank at Shrewſbury; in the trench within the caſtle of Ruthin; about the gravel-pit at Hyde-park corner; and in the year 1756, almoſt all over the church-yard of Warden in the iſle of Shepey.

Frangula ſive Alnus nigra baccifera, Park. "In "ſylvoſis, udis perſæpè reperitur." R. Hiſt. Though pretty common in ſuch places about London, and in ſome parts of Kent, eſpecially about Tunbridge-town and Wells, I have not obſerved it this way, except in the coppice, called the park, at Willowbridge, Staffordſhire, where there is a good deal of it in a very dry gravelly ſoil.

Fumaria cum capreolis, J. B. On Caergwrle caſtle-hill, and on other hills there-about in this county; on the Wreken, and on Arcal-hill, near it; under a rock in the Foreſt De-la-mer, and on

 Helſby

Helſby Tor, and moſt abundantly among the oak-buſhes on two ſmall hills at the foot of the Tor, Cheſhire. It ſtill grows where Mr. Ray obſerved it, near Bala, and alſo in many hedges about that town and neighbourhood; and in and about Dôlgelle, the roofs of many buildings are covered with it.

Fungus albus ovum referens R. Syn. oviformis Merr. ovatus, J. B. Martyn, in his Tranſlation, &c. of Tournefort's Hiſtoire des Plantes aux env. de Paris, ſays, he found it in great plenty at Cheſterton, in May, and J. Bauhin ſays, it appears in April. We have it plentifully every year on the graſs-plots about this houſe, and I have ſeen it in two places in Denbighſhire; but never earlier than September, or later than November.

Fungus muſcarius, Park. Fungi muſcas interficientis 4. ſpecies, C. B. &c. R. Hiſt. venenati, muſcarii dicti, quarta et elegantiſſima ſpecies J. B. About two beech-trees on the common oppoſite to Hayes near Oſweſtry, Salop; on Auger-heath, Cheſhire; and moſt abundantly in a grove at the top of lord Chetwynd's garden at Mare, or Meer, Staffordſhire, and in the coppice, called the park, at Willowbridge in that county; in October.

This is the moſt beautiful Fungus, I have ſeen; and I do not know that it has before been mentioned as of this land, though Parkinſon generally ſays, " in the woods that have been *felled.*" Perhaps this, that I have met with, is a variety of the above cited authors; for the ſtriæ, or lamellæ, are white; whereas, according to J. Bauhin and Mr. Ray, they are black; and the pedicle, of which

which they give us only the dimenſions, is alſo white.

Fungus anguloſus et velut in lacinias ſectus, C. B. luteus ſive pallidus, Chanterelle dictus, ſe contorquens, eſculentus, J. B. In woods here, and elſewhere, in July, Auguſt, September, and October. It is very beautiful.

Fungus piperatus albus, lacteo ſucco turgens, J. B. In woods here in September.

Fungus maximus, albus, lacteſcens, non acris. In Willowbridge-park, and Broughton-wood, Staffordſhire. October.

Fungus parvus parvi galeri formam exprimens, rufus, C. B. On Caergwrle caſtle-hill in this county. October and November.

Fungus mediæ magnitudinis, pileolo ſupernè è rufo flavicante, lamellis ſubtus ſordidè virentibus. R. Syn. With the laſt preceding.

Fungus medius pileo muco æruginei coloris obducto R. Syn. Frequent hereabout. September.

Fungus acetabulorum modo cavus, radice carens, C B. Fungi Pericæ Plinii Col.

Fungus Hepaticæ ſaxatilis forma, C. B. Fungi Pericæ altera ſpecies, Col. This and the laſt preceding in the ſoil about the ſtools of oaks cut down, hereabout, and about Willowbridge, Staffordſhire, in November. They are of a fine orange colour, and of great beauty.

Fungus ophioglоſſoides non deſcriptus, Ph. Brit. ophioglоſſoides niger, R. Hiſt. Abundantly on the graſs-plots about this houſe, and at Hayes, Salop. I have ſeen it elſewhere on dry ground in both theſe counties, and believe it is often overlooked.

Fungi parvi lutei ad ophiogloſſoidem accedens, R. Hiſt.

Fungus terreſtris, digitatus dictus, minor, Park.

Fungus candidus digitatus minor, C.B.

Fungus ramoſus candidiſſimus ceranoides, ſive digitatus minimus, nonnunquam corniculatus. Plot. Nat. Hiſt. Staff.

Of theſe, and of many other, approaching to them, and of ſeveral colours, I have obſerved a great variety hereabout and elſewhere, in October and November.

Fungus ramoſus, flavus et albidus, J. B. In a dry thicket by the brook Terrig, in Treithen in this county. Auguſt.

Fungus parvus luteus, ramoſus, R. Syn. On the graſs-ſlopes here; on the aſcent from the river Alen to Gresford, Denbighſhire; in the Park at Willowbridge, and on Heyley-caſtle-bank, Staffordſhire.

Fungus pyriformis, Merr. Pin. In paſtures hereabout. September.

Fungus pulverulentus, Crepitus Lupi dictus, major, pediculo longiore ventricoſo, R. Syn. In paſtures hereabout, at the end of March, and in September.

Fungus phalloides, J. B. fœtidus, penis imaginem referens, C. B. I ſuppoſe, it is common in September, in moſt counties, though oftener ſcented than ſeen. It is hereabout known by the Welſh name, Gingroen, or Cingroen.

Geniſtella tinctoria, Ger. "In paſcuis nimis fre-"quens." R, Syn. I have not ſeen of it in this county, though I have often in thoſe of Cheſter and Stafford.

Geniſtella aculeata, Park. Not ſo frequent this way as about London. Abundantly about Horton, near Malpas, Cheſhire, and between Dôlgelle and Feſtiniog.

Gentianella fugax autumnalis elatior, Centaurii minoris foliis, R. Syn. Variat floris colore, carneo, cœruleo, albo. On the bank at Clomendy in Llanveras, Denbighſhire. It ſeems to differ only in ſize from Gentianella autumnalis Centauriæ minoris foliis Park.

Geranium batrachoides, J. B. In the field, which is next above the bridge, called Pont-llong, and between the rivers Alen and Terrig in this townſhip, Leeſwood; on the bank at Clomendy in Llanveras, and in the church-yard of Llanveras, Denbighſhire, and moſt abundantly about the Derbyſhire end of Burton-bridge.

Groſſularia, Park. Not unfrequently in hedges and thickets and upon walls.

Helenium, ſive Enula campana, J. B. I have ſeen it in a few fields in this county; in ſome waſte places about Freſh-water, and elſewhere, in the Iſle of Wight; but no where ſo plentifully as in the fields and lanes about Aber in Carnarvonſhire.

Herba Paris, J. B. Sparingly in a wood in Treithin, and in one in Broncoed, both near this place, with 4, 5, and 6, leaves.

Hippoſelinum ſive Smyrnium vulgare, Park. "In "moſt places of England." Ger. It is certainly very rare. I have ſeen of it only in the two inner courts of the caſtle of Ludlow, Salop; near St. Edmund's-

Edmund's-bury, in a field at the angle between two roads leading to Newmarket and Farnham, where it grows abundantly; between the town and cliff of Harwich, as mentioned by Mr. Dale; and ſparingly by the Thames-ſide between the planks ſupporting the bank at Stangate, Lambeth.

Horminum ſylv. Lavendulæ flore, C. B. "In gla-"reoſis frequens." R. Syn. In this part of the kingdom I have ſeen it only on Denbigh caſtle hill.

Hypericum pulchrum Traji, J. B. Plentifully on the hills about Cader Uronwen, Denbighſhire.

Hypericum maximum, Androſæmum vulgare dictum R. Syn. In a thicket at Hays, Salop, and elſe-where; but not common.

Hypericum elegantiſſimum non ramoſum folio lato, J. B. Upon an hour's ſearch, I found only two plants of it, where Mr. Ray obſerved it near Denbigh.

Juniperus vulgaris baccis parvis purpureis, J. B. I have not ſeen it more Northward than between Woodſtock and Enſton, though I have heard that it grows on ſome mountains in Wales; nor could I find Juniperus Alpina, J. B. upon Snowdon, where we read it grows.

Laureola, Park. "In ſylvis et ſepibus ſatis frequens." R. Hiſt. Though frequent in ſome counties; I have not ſeen it this way.

Liguſtrum, J. B. "In ſylvis, vepretis et ſepibus fre-"quens." R. Hiſt. It ſeems to be rare in theſe counties. In ſome hedges about Bridgenorth, Salop; between Ivetſea-bank and Canock-heath, Staffordſhire. I have ſeen only three or four ſhrubs, and theſe in one hedge, in this county of Flint.

Litho-

Lithoſpermum majus erectum, C. B. "Ad vias et "ſepes." Cat. Angl. I have met with it but twice in this county, and not often elſewhere.

Lychnis, Saponaria dicta, R. Syn. Among the ruins of the monaſtery at Greenfield in this county; on the caſtle-hill at Denbigh; by the road-ſide between Llanamonerch and the new-bridge, Salop, and in a gravel-pit near the end of St. Edmund's-bury, toward Sudbury.

Lyſimachia purpurea trifol. caule hexagono Spigel. Iſag. Much more frequent than the quadrangular about Ince, Thornton, Stoke, and Stanny, Cheſhire. Some of the ſtalks are pentagonal, and have their leaves ſingly at the joints, alternate. The quadrangular have only two oppoſite leaves. I have alſo obſerved ſome diſtinctly heptagonal; apparently only ſo, with four leaves at a joint: but I ſuſpect that they were in reality octagonal. All the other varieties frequently riſe from one root.

Lyſimachia lutea, J B, By Thornton-brook in two places between the Mill and Ince-common, Cheſhire. The ſide ſhoots in pairs, in threes, and in fours: but ſo, as far as I obſerved, in different plants.

Lyſimachia ſpecioſa, quibuſdam Onagra dicta, ſiliquoſa, J. B. Chamænerion flore Delphinii, Park. Par. On the left ſide of the hollow road juſt beyond Knowles-park, between Sevenoak and Tunbridge.

Mentha cattaria, J. B. It is common in a chalky ſoil: but of theſe counties I have obſerved it only in Shropſhire, and there about Shifnal, Norton, Atcham, Neſſcliff, Oſweſtry, and on the brow of the

the hill above the river Ceiriog on the road from Oſweſtry to Chirk.

Menthaſtrum fol. rugoſo rotundiore ſpontaneum, fl. ſpicato, odore gravi, J. B. By the high-way-ſide near, and Southward of, this houſe, plentifully.

Menthaſtrum ſpicatum fol. longiore candicante, J. B. By the brook-ſide, in a field called Maes-Madoc iſſa, near this place, plentifully.

Meſpilus vulgaris, J. B. Between Tunbridge-town and wells, and I heard it was common in woods thereabouts.

Myrrhis major, vel Cicutaria odorata, C. B. magno ſemine, longo, ſulcato, J. B. Major, ſive vulgaris, Park.

" In Haſſia circa Caſſellas ſponte provenire " Myrrhidem referebat Gillenius. Hæc, ut ait " Lobelius, non adeo frequens aut procera occurrit, " ſed raro et in pratis. Fertur autem apud Ger- " manos in pratis alicubi naſci, Dodonæo perhi- " bente, cùm apud Belgos hortenſis tantummodo " ſit." I. Bauhin.

" It is thought to grow wild in ſome fields in " Germany; but I doubt the report." &c. Park. Theat.

Dr. Hill, in Br. Herb. has given a figure of garden Myrrhis, and alſo one of garden Chervill. It is hard to know which he intends for this. The firſt he has left undeſcribed: the other he has given ſome account of, under the name of Chærophyllum ſativum, which, he ſays, is ſo called by C. Bauhine, and, by our gardeners, Sweet Fern. But Chærophilum ſativum, C. B. is an annual. This, though

though the ſtalks are annually removed, has a perennial root.

" In the foreſt of Savernac (Wilts) grows a kind " of ſweet fern." Brit. Bac. Whatever that may be.

This grows plentifully here for the length of about 300 yards on both ſides of the brook Terrig, in a field called Y Parki, and ſparingly by the ſame brook, at a conſiderable diſtance below, in a field called Maes-Madoc. The ſoil of a dry ſand, but ſometimes overflown. I have alſo ſeen it ſparingly in the church-yard at Kilken, in this county, and abundantly among the ruins of the Abbey of Llanegwaſt or de Valle crucis, in Denbighſhire.

Myrtus Brabantica, ſive Elæagnus Cordi Ger. On a bog near the road from Mold to Northop, in this county; and on one near the Decoy, between Oſweſtry and Babin's-wood, Salop.

Narciſſus ſylv. pallidus calyce luteo, C. B. I have ſeen it often in waſte places; but never, as I remember, far from a houſe.

Oenanthe aquatica minor, Park. Common in marſh ditches, and there ſeem to be ſome varieties of it.

Orobanche, ſive Rapum Geniſtæ, Ger. &c. " Not " to be found, but where broom doth grow." Ger. " More often where no broom groweth." Park. I have not ſeen it in this neighbourhood, where we have no broom: but it may be found at Hayes in Shropſhire, with broom, and very plentifully with furze, where there is no broom, over the ſand rocks at Ince in Cheſhire.

 Pedicularis

Pedicularis pratenſis fl. candidis, C. B. quibuſdam Criſta galli flore albo, J. B. Hereabout, and elſewhere, with the common red ſort, ſparingly.

Pentaphyllum, vel potius Heptaphyllum, fl. rubro, J. B. "No where near London." Martyn on Tourn. Par. On the bog at Willowbridge, Staffordſhire; and on the moſs near Hampton, Salop. In pits about Stoke and Stanny, Cheſhire, plentifully.

Phyllitis multifida, Ger. I have ſometimes ſeen it on moiſt and ſhady rocks, in this county, and in Denbighſhire. Whether ſpecifically diſtinct from the common ſort, or not, I cannot determine; but in ſome places where the common is in great plenty, and I have obſerved it many years, it has not varied in any one inſtance.

Populus ———. Though Populus tremula, C. B. ſeems not to be very common, I believe but ſeldom planted, I have reaſon to think that we have it of ſpontaneous growth; but I am not ſo well ſatisfied with regard to any other ſpecies of this genus.

Pulmonaria anguſtifol. rubente cœruleo flore, C. B. foliis Echii L'Ob. Icon. Among the ruins of Greenfield monaſtery in this county.

Pyraſter, ſeu Pyrus ſylveſtris, J. B. "In ſylvis et "ſepibus paſſim." R. Syn. I have but very rarely ſeen it.

Ranunculus montanus Aconiti folio, fl. globoſo, C. B. Helleborus niger Ranunculi folio, fl. globoſo majore Tourn. About Dôlgelle, Feſtiniog, Llanrhwſt, &c. in Merioneth and Denbigh ſhires, and in the meadows at Hayes, Salop, plentifully.

4 Rhamnus

Rhamnus catharticus, J. B. "Locis ſenticoſis, ſepi-"bus et ſylvis non rarò." R. Hiſt. I have met with it moſt often in our Southern counties; ſeldom in Shropſhire, and not at all in this neighbourhood.

Rorella longifolia perennis, R. Hiſt. ſi non Rorella longifol. maxima ejuſdem; plentifully, but leſs ſo than Ros ſolis fol. rotundo, C. B. on the hillocks, called Triddle-bogs, on the great bog at Willowbridge, Staffordſhire.

Roſa ſylv. fructu majore hiſpido, R. Syn. Almoſt as frequent as the common hep-tree.

Roſa Pimpinellæ folio, Ger. ſylv. pomifera minor, C. B. "In ſabuloſis et petroſis frequens." R. Hiſt. Not generally ſo; on Twittenham-common, among the furze plentifully, and on other commons in Surry: the hedges along the road from the 11th to the 12th mile-ſtone from Worceſter toward Kiderminſter, are almoſt intirely of it; and it is almoſt all over the rocky-hill by the North-weſt ſide of Denbigh.

Rubus Idæus ſpinoſo fructu rubro, J. B. Very common in theſe, and I have often ſeen it in woods and thickets in ſome of the Southern counties.

Ruſcus, Park. "groweth generally throughout this "land, in copſes and upon heaths and waſte-"grounds." Park. On the ſides of the road, frequently, between Bromly-town and common, and between that common and Farnborough, Kent. It has not been my hap to meet with it elſewhere.

Sambucus vulgaris, Park. "groweth not wild, but "is planted in all places, to ſerve, &c." Park. It is certainly wild in woods and thickets, and not unfrequently even in the clifts of rocks and upon walls.

Sambucus humilis, five Ebulus, C. B. At Fern-hill near Whittington, and about Whittington-caſtle, Salop; about a mile from Hamſtead toward Wolverhampton, ſparingly; on Tamworth caſtle-bank, plentifully; on the walls of Colcheſter-caſtle; in the ditches at the entrance of Long-Milford (from Sudbury); in Durſon-field near Northampton, and on Slaton-hill at the croſſing of Watling-ſtreet, between Northampton and Daventry; between Newport and Careſbrook-caſtle, Iſle of Wight; ſparingly by the ditches between Kennington-common and the Aſylum. Mr. Blackſtone, in his Specimen Botanicum, ſays, "near the upper gate in Cuper's-gardens abundantly." I ſearched diligently, both within and without the gate, September 16, 1759, and found not one plant of it; but within I found ſmall plants of Sambucus vulgaris among the large, in vaſt abundance; yet I hardly think he could miſtake one ſpecies for the other.

Sedum minimum, fl. mixto ex albo et rubro, Merr. On rocks, walls, and dry banks, almoſt throughout Carnarvon and Merioneth ſhires; and on and about Caſtell dinas-brân, near Llangollen, and in other parts of Denbighſhire adjoining to thoſe two counties.

Senecio hirſutus viſcidus major odoratus, J. B. On dry banks, walls, and rocks, at Ince, Cheſhire.

Solanum lethale, Park. Within the remains of Hawarden-caſtle, and abundantly in and about Hope in this county; about the Abbey de Valle crucis, and

3

and at Vron, and other places near Chirk-caſtle, Denbighſhire, and by the brook under the North-weſt ſide of the town of Denbigh.

Sorbus ſylv. foliis domeſticæ ſimilis, C. B. Ornus Dod. ſive Fraxinus ſylv. Park. "In montoſis "et uliginoſis in Cambria et Septentrionali Angliæ "parte." R. Syn. I may add, in the South too, having ſeen it in abundance even in the moſt ſouthern parts of Kent and Suſſex.

Sphondylium majus aliud laciniatis foliis, Park. "found by Mr. G. Bowles in Shropſhire, as I take "it." Park. On both ſides of the dingle, in the lower part of the wood, at Hayes, near Oſweſtry; it ſeems to be as common as Sph. vulgare; than which, ſcarcely any weed there is more ſo, though in other parts of that wood, where the common ſort is no leſs plentiful, this is hardly to be found. I have not ſeen it elſewhere. The apparent difference is ſo great, that I cannot but ſuſpect them to be ſpecifically diſtinct. I have the ſeeds of both now upon trial.

Parkinſon's figure of this plant is very bad, and I know none other of it.

Taxus, Park. "This groweth in many places of this "land; but planted in all, whether at home, or "abroad." Park. It is very common, of natural growth, not only in woods and thickets, as well as in hedges, but upon rocks and walls.

Telephium vulgare, C. B. Far from being univerſal. Abundantly about Llangollen, and in Carnarvon and Merioneth ſhires.

Tilia —— I do not believe that any ſpecies of this genus is wild in this part of the iſland, however it be in other parts.

Tithy-

Tithymalus linifol. Cam. An Tith. ſegetum linifol. R. Syn. With the oats hereabout.

Trachelium majus Belgarum, Park. In ſhady places hereabout, and elſewhere in Mold's-dale; but moſt abundantly in a field here, called y Parki, through which the Terrig runs; alſo about Egluyſeg and Llangollen, Denbighſhire.

Trachelium majus fl. purpureo, Park. Par. In Shropſhire toward Tenbury, as well as beyond, in Worceſter and Hereford ſhires, plentifully; in thickets among the rocks by Clomendy in Llanveras, Denbighſhire.

I am not certain of Trachelium minus, Park. which is ſaid to be common: but there are certainly in this iſland, and even in this neighbourhood, many ſpecies of this genus, that I cannot find to have been deſcribed or mentioned.

Trichomanes, ſive Polytrichum, J. B. " In petris " umbroſis et roſcidis, inque aggeribus umbroſis et " muris antiquis magnus ejus per totam Angliam " eſt proventus." According to my obſervation, it is rare in moſt of the Engliſh counties; but common hereabout, and much more ſo in ſome parts of Wales. The walls of the bridge and the walls and roofs of ſome houſes at Bethgelert in Carnarvonſhire, are almoſt covered with it.

Vaccinia nigra vulgaris, Park. Frequent on moſt heathy hills; but, as far as I have obſerved, more on dry than wet. It is ſcarcely two inches high on the moſt barren ground toward the top of Snowdon, with an extremely ſmall leaf, having frequently a large red excreſcence, the nidus of an inſect.

Vaccinia

Vaccinia rubra buxeis foliis, Park. On the ſummit only of the moſt pointed of the Camp-hills, Staffordſhire, in a dry gravel. This too is very minute in all its parts upon Snowdon, and ſo it is about Cader Vron-wen, Denbigh.

Verbaſcum, fl. albo parvo, J. B. Abundantly along the roads about Gresford and Acton, Denbighſhire.

Verbaſcum nigrum, fl. parvo luteo, apicibus purpureis, J. B. By the highway and park-ſide, almoſt oppoſite to Shavington (vulgò Shenton) hall, Salop, for about 100 yards, plentifully; rather above a mile beyond Hamſtead, toward, and near, Bar, Staffordſhire; in many places between Sudbury and St. Edmund's-bury: at the deſcent from Gerard's Croſs, and alſo in many places between the Chalk-hills, ſhort of Loud-water, and a mile or two beyond Weſt-Wickham, Bucks.

Viburnum, Park. "In ſepibus paſſim," R. Cat. Angl. I have not obſerved it more Northward than about Southam, and between Stratford upon Avon and Henley, Warwickſhire, and ſomewhat ſhort of Hiſham, Northamptonſhire.

Viola montana lutea grandiflora, C. B. Pretty plentifully on the mountains about Llanarmon, Dyffryn Ceiriog, Denbighſhire, and thence about half way to Oſweſtry; ſparingly about mid-way between Ruthin and Llangollen; between Bala and Feſtiniog, in ſome places, and abundantly between Feſtiniog and Dòlgelle.

Virga aurea anguſtifol. minus ſerrata, C. B. Plentifully throughout the park at Willowbridge, Staffordſhire, and in rocky places at Ince, Helſby, and

Frodſham,

Frodſham, Cheſhire; alſo, but of a much more humble growth, on dry ſandy banks about Dereſbury, Norton, and Halton, Cheſhire.

Virga aurea latifol. ſerrata, C. B. With the former at Willowbridge, but leſs frequent.

Ulmus ———. I have not been able to aſſure myſelf that any ſpecies of this comes up ſpontaneouſly with us. I need not except the caſe of ſuckers; they being, till removed, but parts of trees, and many of the kinds, frequently planted, are ſo productive of ſuckers, even from the extremities of the roots, that one might expect theſe trees to be much more general than they are. It is certain, there are many very extenſive woods without any of them. I have often ſown the ſeeds of ſome kinds (that have viſible ſeeds, which, I think, is not the caſe of all) and ever without ſucceſs: but, having been informed that ſuch have been raiſed this way, I mention my diſappointments only as ſome kind of teſtimony, that they are not apt to be increaſed from ſeeds that caſually fall, and I have not obſerved ſuch offspring.

Umbilicus Veneris, Ger. Much more common in Shropſhire, Staffordſhire, Cheſhire, and in this county, than more Southward; ſtill more in Denbighſhire, and on almoſt every rock and old wall in Carnarvon and Merioneth ſhires.

Umbilicus Veneris minor, Ger. Umbilicus Veneris, 2. Hiſt. des Plantes de l'Europe. Alter Cam. in Matth. & Matth. in Dioſc. Cotyledon minus montanum Sedi folio L'Ob. Icon. 469, where the figure of this and that of Umbilicus Veneris are tranſpoſed. This is a ſpecies of Saxifraga. An

Saxifraga

Saxifraga Sedi folio anguſtiore, ſerrato Tourn.? I believe, it is not now to be found on Bieſton-caſtle, Cheſhire, where Gerard ſays he found it.

In this Catalogue, I have thought it needleſs to mention, except comparatively, the particular native places already pointed out by others. I have paſſed over the maritime plants, unleſs found alſo in inland places; becauſe ſuch as I have obſerved, are not peculiar to any part of our coaſt; and I have endeavoured to avoid a needleſs multipicity of ſynonyma, uſing generally the ſhorteſt name, if ſufficiently deſcriptive, or that uſed by the author, whoſe icon, or deſcription, ſeemed to me to be the beſt; and therefore I have not adhered to any one ſyſtem.

Upon the whole, it may be difficult to determine what plants, if any, are originally Britiſh.

With regard to biennials, if there has been immemorially a conſtant annual flowering in waſte places, or in ground that does not appear, or is not known to have been cultivated for the purpoſe, it may perhaps be reaſonably preſumed that they are the natural and ſpontaneous product of ſuch places; for, in this caſe, I underſtand natural and ſpontaneous, according to common acceptation, to be ſynonymous, and applicable to any ſeminal production, however happening, or effected, without the aſſiſtance of art, whether from ſeeds depoſited there, or in that ſoil, at the creation, or from ſuch as are conveyed by the wind, by birds, or any other caſual means. Otherwiſe, in ſtrictneſs, there may be no ſuch thing in nature as a ſpontaneous production; for as to the old doctrine of equivocal generation, I ſup-

 poſe

pofe it to be univerfally exploded; though I do not difpute that the ftamina, or firft rudiments, have exifted, in the parent plants, from the beginning of all things, the vegetative principle being latent, till prepared to exert itfelf.

And upon the firft conjecture, a difficulty may arife.

It is, perhaps, not eafy to conceive that the fecundity of feeds, once perfected, can be retained inert through many ages. Our experience feems to fhew, that there are fome kinds of feeds, that, at a certain age, or nearly fo, either vegetate or perifh; that, if kept out of their proper matrix, or in it at too great a depth, beyond that time, whatever we do with them afterward, will not grow; and if there be really fuch (fo depofited *ab origine)* thofe kinds cannot, even in that fenfe, and in that cafe, be faid to come up fpontaneoufly.

Befides, if the feeds were fo depofited in the earth, and in a perfect ftate, fo numerous as they muft be; the larger kinds, efpecially, could not efcape our notice.

As to the antediluvian nuts, cones, and ftone-fruits, that, we hear, are fometimes found at vaft depths within the earth, however they may fuit the cabinets of the curious, I fear they are too antique to be prolific.

But in the other way, the feeds may be conveyed, from whatever diftance, in different years (for aught we know they are in every year), to the places where we fee the plants; and not only thither, but to many places, that are not proper to receive and cherifh them.

It

It is evident that the oak, aſh, and other our moſt common trees, are not naturally increaſed in any other way, except ſuch as are productive of ſuckers at a conſiderable diſtance from the ſtems; and many of theſe do not generally perfect their ſeeds: to ſay nothing of inferiour plants, that ſometimes, in the phraſe of gardening, *lay themſelves*.

But thoſe ſuckers, till parted from the parent trees, and removed from the place, are not often better than underwood, which may be one reaſon, why theſe kinds do not increaſe ſo extenſively as the former.

And if our forefathers had not induſtriouſly raiſed and increaſed (if not previouſly introduced) the moſt common and moſt uſeful trees, perhaps we ſhould not obſerve them to increaſe naturally more, or have found them more numerous, than many that we know to be exotic, and yet are as eaſily increaſed, and do of themſelves increaſe as faſt, proportionally, and are as hardy, as any trees we have.

Yet it is not to be expected, that theſe of exotic origin, more than thoſe that have been long familiar to us, ſhould increaſe alike in all ſoils, or in all counties, ſince there are ſome ſoils that are far from being general.

Mr. Da Coſta, in his Nat. Hiſt. of Foſſils, obſerves, that "Chalk is found chiefly in the South-eaſt part "of this iſland," ſo that, "if a ſtraight line were "drawn from Dorcheſter (in Dorſetſhire) to the coaſt "of Norfolk, it would almoſt include our chalky "ſtrata;" and I believe his obſervation to be juſt, except that, though the line be drawn even to the moſt Weſtern part of that coaſt, this ſoil extends conſiderably beyond it, into Wiltſhire.

We know, that of all ſoils this is the moſt favourable to beech, white-beam, juniper, viburnum, traveller's-joy, and to many of the herbaceous tribe; though not only ſuch, but many foreign plants, will increaſe alſo in ſoils that are not the moſt ſuitable to them.

In the woods here, and at a diſtant place, I find, not unfrequently, ſeedlings of the Scotch-pine (which whether indigenous of Scotland, or not, may be doubtful), ſpruce-fir, horſe-cheſnut, walnut, and perhaps more than I can at this time recollect. Of the four kinds mentioned, ſome trees, notwithſtanding the tread and the browſing of cattle, now grown to a conſiderable height, I am certain were not planted. Of the three firſt there are many not far off, that were planted, and probably may in moſt ſeaſons bear perfect ſeeds: but of the walnut I do not know that there is, or has been, within half a mile of the firſt-mentioned woods, a tree that has produced a nut mature enough for vegetation. It is, however, eaſy to conceive, that the nuts may have been brought from a much greater diſtance by birds, or other animals, and dropped accidentally, or hoarded and forgotten, or perhaps not needed.

In a ſhrubbery, many years left to nature, I have obſerved very numerous progenies of various foreign ſhrubs, both from the ſeeds and roots; and it is well known to gardeners, that many of their once *choice* flowering herbs are apt to multiply in the way of ſuckers, while the ſeeds of others *ſow themſelves* ſo plentifully, as not eaſily to be kept within bounds.

It, therefore, ſeems to me not unlikely, that all theſe kinds, and many more perhaps yet unimported, may in future ages be ſo far naturalized as to be deemed indigenous of this land.

But this conjecture, and the former, are offered with ſubmiſſion. I am,

SIR,

Your moſt obedient

and moſt humble ſervant,

Leeſwood,
Dec. 24, 1770.

R. H. Waring.

XLII. A

XLII. *A Catalogue of the Fifty Specimens of Plants, from* Chelſea *Garden: preſented to the* Royal Society, *for the Year* 1770, *purſuant to Direction of the late Sir* Hans Sloane, *Bart. from the Society of Apothecaries*, London: *By* Stanesby Alchorne, *Member of the ſaid Society.*

Read Dec. 6, 1771.

2401 ÆTHUSA, *Cynapium.* Lin. Spec. plant. 367. 1.
Cicuta minor petroſelini ſimilis. Bauh. pin. 160.

2402 Ajuga, *reptans*, ſtolonibus reptantibus. Lin. Spec. plant. 785. 4.
Conſolida media pratenſis cœrulea. Bauh. pin. 260.

2403 Anemone, *nemoroſa*, ſeminibus acutis, foliolis inciſis, caule unifloro. Lin. Spec. plant. 762. 15.
Anemone nemoroſa flore majore. Bauh. pin. 176.

2404 Anemone, *Thalictroides*, foliis caulinis ſimplicibus verticillatis, radicalibus duplicato-ternatis. Lin. Spec. plant. 763. 21.
Ranunculus nemoroſus aquilegiæ foliis virginianus aſphodeli radice. Pluck. Alm. 310. tab. 106. fig. 4.

2405 Antirrhinum, *bipunctatum*, foliis linearibus glabris, inferioribus quaternis, caule erecto paniculato, floribus ſpicato-capitatis. Lin. Spec. plant. 853. 13.

Linaria lutea parva annua. Bauh. hiſt. III. 457.

2406 Antirrhinum, *Orontium*, corollis ecaudatis, floribus ſubſpicatis, calycibus digitatis, corolla longioribus. Lin. Spec. plant. 860. 36.

Antirrhinum arvenſe majus. Bauh. pin. 212.

2407 Apium, *graveolens*, foliolis caulinis cuneiformibus, umbellis ſeſſilibus. Lin. Spec. plant. 379. 1.

Apium paluſtre ſive Apium officinarum. Bauh. pin. 154.

2408 Berberis, *vulgaris*, pedunculis racemoſis. Lin. Spec. plant. 471. 1.

Berberis dumetorum. Bauh. pin. 454.

2409 Bryonia, *alba*, foliis palmatis, utrinque calloſo-ſcabris. Lin. Spec. plant. 1438. 1.

Bryonia alba baccis nigris. Bauh. pin. 297.

2410 Buxus, *ſempervirens*. Lin. Spec. plant. 1394. 1.

Buxus arboreſcens. Bauh. pin. 471.

2411 Centaurea, *Cyanus*, calycibus ſerratis, foliis linearibus integerrimis, infimis dentatis. Lin. Spec. plant. 1289. 14.

Cyanus ſegetum. Bauh. pin. 273.

2412 Cheiranthus, *Cheiri*, foliis lanceolatis acutis glabris, ramis angulatis. Lin. Spec. plant. 924. 2.

Leucojum luteum vulgare. Bauh. pin. 202.

2413 Chryſanthemum, *Leucanthemum*, foliis amplexicaulibus oblongis, ſuperne ſerratis, infime dentatis. Lin. Spec. plant. 1251. 4.

Bellis

Bellis ſylveſtris caule folioſo major. Bauh. pin. 261.

2414 Convallaria, *verticillata*, foliis verticillatis. Lin. Spec. plant. 451. 2.

Polygonatum anguſtifolium ramoſum. Bauh. pin. 304.

2415 Ellisia, *Nyctelea*, Lin. Spec. plant. 1662. 1.

2416 Eryſimum, *officinale* ſiliquis ſpicæ adpreſſis, foliis runcinatis. Lin. Spec. plant. 922. 1.

Eryſimum vulgare. Bauh. pin. 100.

2417 Fragaria, *veſca*, flagellis reptantibus. Lin. Spec. plant. 708. 1.

Fragaria vulgaris. Bauh. pin. 326.

2418 Glecoma, *hederacea*, foliis reniformibus crenatis. Lin. Spec. plant. 807. 1.

Hedera terreſtris vulgaris. Bauh. pin. 306.

2419 Haleſia, *tetraptera*, foliis lanceolato-ovatis, petiolis glanduloſis. Lin. Spec. plant. 636. 1.

Haleſia fructibus membranaceis quadrangulatis. Phil. Tranſ. LI. 931.

2420 Helleborus, *fœtidus*, caule multifloro folioſo, foliis pedatis. Lin. Spec. pl. 784. 4. *β*.

Helleborus niger trifoliatus. Mor. Hiſt. III. 460.

2421 Hypoxis, *erecta*, piloſa, capſulis ovatis. Lin. Spec. plant. 439. 1.

Ornithigalum herbaceum luteum parvum virginianum foliis gramineis hirſutis. Pluk. Alm. 272. tab. 350. fig. 12.

2422 Lamium, *album*, foliis cordatis acuminatis petiolatis, virticillis vigintifloris. Lin. Spec. plant. 809. 5.

Lamium

Lamium album non fœtens, folio oblongo. Bauh. pin. 231.
2423 Lamium, *purpureum*, foliis cordatis obtusis petiolatis. Lin. Spec. plant. 809. 6.
Lamium purpureum fœtidum folio subrotundo. Bauh. pin. 230.
2424 Laurus, *nobilis*, foliis venosis lanceolatis planis perennantibus, ramulis tuberculatis cicatricibus, floribus racemosis. Lin. Spec. plant. 529. 5.
Laurus vulgaris. Bauh. pin. 460.
2425 Ligusticum, *Scoticum*, foliis biternatis. Lin. Spec. plant. 359. 2.
Ligusticum humilius Scoticum. Pluk. Alm. 217. tab. 96. fig. 2.
2426 Myrsine, *Africana*. Lin. Spec. plant. 285. 1.
Buxus africana rotundifolia serrata. Pluk. Alm. 74. tab. 80. fig. 5.
2427 Narcissus, *minor*, spatha uniflora, nectario obconico erecto crispo sexfido æquante petala lanceolata. Lin. Spec. plant. 415. 4.
Narcissus parvus totus luteus. Bauh. pin. 53.
2428 Narcissus, *Pseudo-Narcissus*, spatho uniflora, nectario campanulato erecto crispo æquante petala ovata. Lin. Spec. plant. 414. 2.
Narcissus sylvestris pallidus calyce luteo. Bauh. pin. 52.
2429 Ononis, *crispa*, fruticosa, foliis ternatis subrotundis undulatis dentatis viscoso-pubescentibus, pedunculis unifloris muticis. Lin. Spec. plant. 1010. 14.
2430 Peltaria, *alliacea*. Lin. Spec. plant. 910. 1.

Thlaſpi montanum glaſti folio majus. Bauh. pin. 106.

2431 Potentilla. *Anſerina*, foliis pinnatis ſerratis, caule repente. Lin. Spec. plant. 710. 2.

Potentilla. Bauh. Pin. 321.

2432 Primula, *veris*, foliis dentatis rugoſis. Lin. Spec. plant. 204. 1. γ.

Verbaſculum ſylveſtre majus ſingulari flore. Bauh. pin. 241.

2433 Ranunculus, *bulboſus*, calycibus retroflexis, pedunculis ſulcatis, caule erecto, foliis compoſitis. Lin. Spec. plant. 778. 25.

Ranunculus pratenſis radici verticilli modo rotunda. Bauh. pin. 179.

2434 Ranunculus, *Illyricus*, foliis ternatis integerrimis lanceolatis. Lin. Spec. plant. 776. 17.

Ranunculus lanuginoſus anguſtifolius grumoſa radici. Bauh. pin. 181.

2435 Rhexia, *Virginiana*, foliis ſeſſelibus ſerratis, calycibus glabris. Lin. Spec. plant. 491. 1.

Lyſimachia non papposa virginiana tuberariæ foliis hirſutis flore tetrapetalo rubello. Pluk. Alm. 235. tab. 202. fig. 8.

2436 Rumex, *acetoſa*, floribus dioicis, foliis oblongis ſagittatis. Lin. Spec. plant. 481. 24. δ.

Acetoſa montana maxima. Bauh. pin. 114.

2437 Rumex, *acetoſella*, floribus dioicis, foliis lanceolato-haſtatis. Lin. Spec. plant. 481. 25.

Acetoſa arvenſis lanceolata. Bauh. pin. 114.

2438 Sambucus, *nigra*, cymis quinquepartitis, caule arboreo. Lin. Spec. plant. 385. 3.

Sambucus fructu in umbella nigro. Bauh. pin. 456.

2439 Sa-

2439 Saponaria, *orientalis*, calycibus cylindricis villosis, caule dichotomo erecto patulo. Lin. Spec. plant. 585. 5.

Lychnis orientalis annua supina antirrhini folio, flore minimo purpurascente. Dil. Hort. Elth. 205.

2440 Scandix, *cerefolium*, seminibus nitidis ovato-subulatis, umbellis sessilibus lateralibus. Lin. Spec. plant. 368. 3.

Chærophyllum sativum. Bauh. pin. 152.

2441 Scilla, *bifolia*, radice solida, floribus erectiusculis paucioribus. Lin. Spec. plant. 443. 6.

Hyacinthus stellaris bifolius germanicus. Bauh. pin. 152.

2442 Sisymbrium, *Sophia*; petalis calyce minoribus, foliis decomposito-pinnatis. Lin. Sp. pl. 920, 18.

Nasturtium sylvestre tenuissime divisum. Bauh. pin. 105.

2443 Swertia, *perennis*, corollis quinquefidis, foliis radicalibus ovalibus. Lin. Sp. pl. 328. 1.

Gentiana palustris latifolia. Bauh. pin. 188.

2444 Syringa, *laciniata*, foliis lanceolatis, integris dissectisque. Lin. Spec. pl. 12. 2. β.

Lilac laciniato folio. Mill. Dict. pl. 164. 2.

2445 Syringa, *vulgaris*, foliis ovato cordatis. Lin. Spec. pl. 11. 1.

Lilac flore saturate purpureo. Mill. Dict. p. 163.

2446 Trifolium, *Melilotus-cærulea*, spicis oblongis, leguminibus seminudis mucronatis caule erecto. Lin. Spec. pl. 1077. 1.

Lotus hortensis odorata. Bauh. pin. 331.

2447 Vinca *major*, caulibus erectis, foliis ovatis, floribus pedunculatis. Lin. Sp. pl. 304. 2.
Clematis Daphnoides major. Bauh. pin. 302.

2448 Viola, *canina*, caule adultiore adſcendente, foliis oblongo-cordatis. Lin. Spec. plant. 1324. 9.
Viola martia inodora ſylveſtris. Bauh. pin. 199.

2449 Viola, *hirta*, acaulis, foliis cordatis piloſo-hiſpidis. Lin. Spec. pl. 1324. 6.
Viola martia hirſuta inodora. Mor. Hiſt. II. pl. 475.

2450 Zinnia, *multiflora*, floribus numeroſis. Lin. Spec. pl. 1269. 2.
Zinnia floribus pedunculatis. Jacq. Obſ. 2. tab. 40.

XLIII. *Tran-*

XLIII. *Obſervations made, by appointment of the Royal Society, at* King George's Iſland *in the* South Sea; *by Mr.* Charles Green, *formerly Aſſiſtant at the Royal Obſervatory at* Greenwich, *and Lieut.* James Cook, *of his Majeſty's Ship the* Endeavour.

Read November 21, 1771.

1769

April 13 WE came to an anchor in Royal Bay in King George's iſland.

15 Fixed upon the North point of the bay, which is the moſt Northern point of the iſland, for the place of obſervation; here we built a ſmall fort, to ſecure us againſt the natives, which we called fort Venus: it was not finiſhed and the inſtruments ſet up in proper order until the 10th of May, therefore the time for all obſervations made before this day, was taken by a watch with a ſecond hand, the going of which was aſcertained by altitudes of the ſun as often as were neceſſary.

The aſtronomical clock, made by Shelton and furniſhed with a gridiron pendulum, was ſet up in the middle of one end of a large tent, in a frame of wood made for the purpoſe at Greenwich, fixed firm and as low in the ground as the door of the clock-caſe would admit, and to prevent its being diſturbed by any accident, another framing of wood was made round this, at the diſtance of one foot from it. The pendulum was adjuſted

justed exactly to the same length as it had been at Greenwich. Without the end of the tent facing the clock, and 12 feet from it, stood the observatory, in which were set up the journeyman clock and astronomical quadrant: this last, made by Mr. Bird, of one foot radius, stood upon the head of a large cask fixed firm in the ground, and well filled with wett heavy sand. A centinel was placed continually over the tent and observatory, with orders to suffer no one to enter either the one or the other, but those whose business it was. The telescopes made use of in the observations were—Two reflecting ones of two feet focus each, made by the late Mr. James Short, one of which was furnished with an object glass micrometer. Thus furnished, the following observations were made.

vations of equal Altitudes of the Sun for the Time, made with the Aftronomical Quadrant.

	Time per clock of the Sun's Limb paffing the Wires at equal Altitudes.							Obfervatory		Side of the clock		Remarks.
	h ′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	Mean noon per clock.	Baro.	Th.	Baro.	Th.	
	9 39 5½	41 43	42 20	44 25	45 00	47 42½		30 18	86	30 19	82	
10	2 04 11½	2 35	00 57	1 58 53	58 18	55 34½		30 16	83	30 15	82	C. G.
	11 51 38½	51 39	51 38½	51 39	51 39	51 38½	11 51 38,7					
	9 09 08	11 36	12 09	14 00	14 35	17 01		30 22	89	30 20	84	
12	Cloudy	2 30 08	29 36	Cloudy	27 10	24 45		30 29	85	30 14	88	C. G.
		11 50 52	50 52½		50 52½	50 53	11 50 52,5					
	9 11 38	14 04	14 40	16 31	17 04½	19 33		30 35	89	30 15	86	
10	2 29 22	20 58	26 24	too late for	thefe wires.			30 1..	83	30 09	86	C. G.
	50 30	50 31	50 32				11 50 31					
	9 39 23	42 08	42 47	44 52	45 30	48 16		30 16	87	30 14	86	
14	Cloudy			1 55 24	54 46::	1 52 00:		30 20	86	30 19	88	C. G.
				11 50 08	50 08	50 08	11 50 08					
at {	9 15 34	18 05	18 40	20 36	21 11	23 44						
	2 23 14	20 43	20 09	18 12	17 38	15 05						
	11 49 24	49 24	49 24½	49 24	49 24½	49 24½	11 49 9¼					
17	9 27 52	30 30	31 05	33 09	33 45½	36 25		30 19	88	30 15	84	
	2 10 27	7 50	7 14	5 9½	4 33½			30 11	87	30 00	84	C. G.
utftop	11 49 9½	49 10	49 9½	49 9¼	49 9½		11 49 9,55					
{	8 21 02	23 15	23 45	25 27	25 56	28 11						None
	3 16 41:	Cloudy	14 00::	12 15:	11 45	Cloudy						good but
18	11 48 51½		48 52½	48 51	48 50½		11 48 51,4					the firft,
	8 36 31½	48 47	49 19	41 05	41 36	43 53		30 10	82	30 10	84	all the
	3 01 11	2 59 00::	58 26::	Cloudy	56 06½::	53 52::		30 09	82	30 08	86	reft in a
ng off	11 48 51¼	48 53,5	48 52.5		48 51¼	48 52,5	11 48 51,2					confufed haze C G
	9 44 51	47 44	48 23	50 39	51 19	54 17:						
	1 51 07	48 12	47 27	45 19	44 36	41 42						
21	11 47 59	47 58	47 55	47 59	47 57½	47 59½	11 47 58					
	10 3 39							30 25	81	30 22	84	
	1 32 17							30 19	82	30 17	87	C. G.
	11 47 58						11 47 58					
	9 12 24	14 59½	15 34	17 33	17 20	20 43		30 20	84	30 12	83	
	2 22 27	19 54	19 20	17 20		14 11		30 17	82½	30 11	82	C. G.
23	11 47 25½	47 26¾	47 27	47 26½		47 27	11 47 26½					
	9 30 46½	33 31	34 07	36 16	36 55	39 42½						
	2 03 57	1 15	2 00 39	58 28	57 52	55 05						
	11 47 21¾	47 23	47 23	47 22	47 23½	47 23¾	11 47 22,8					

Obfervations

Obſervations of equal Altitudes of the Sun for the Time, made with the Aſtronomical Quadrant.

Time per clock of the Sun's Limb paſſing the Wires at equal Altitudes.								Obſervatory		Side of the clock.		Remarks.
1769	h ′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	Mean noon per clock.	Baro.	Th.	Baro.	Th.	
May	7 43 17	45 26	45 55	47 33	48 02	50 10		30 19	82½	30 12	77½	
Reading off	3 51 01	48 52	48 26	46 45	46 16	44 09		30 13	80	30 07	83	J. C.
☿ 24	11 47 9	47 09	47 10½	47 09	47 09	47 9½	11 47 9⅓					
	8 05 29	7 39	8 10	9 52	10 21	12 33						
	3 28 52	26 41	26 11	24 29	23 59	21 49						
With ſtop	11 47 10½	47 10	47 10½	47 10½	47 10	47 11	11 47 10,4					
	10 04 21½	7 41½	8 2(½	11 02½	11 50½	15 16		30 19	81½	30 15	81½	
♃ 25	1 29 32	26 12	25 27	22 49	22 03	Cloudy		30 07	80	30 10	82	J. C.
	11 46 56¾	46 5(¾	46 56¼	46 55¾	46 56¾		11 46 56,55					
	8 15 59	18 20	18 42	20 35½	21 11	23 20		30 21	82	30 15	80½	
♄ 27	3 16 51	14 33	14 7	12 21	11 46	9 35		30 18	84	30 14	86	C. G.
	11 46 25	46 26½	46 24½	46 28¼	46 28½	46 27½	11 46 26,7					
	8 41 17	43 45	44 9	46 0	46 38	48 57		30 24	82	30 17	81½	
☉ 28	2 51 17	48 49	48 25	46 31	45 54	43 37		30 20	82	30 17	86	C. G.
	11 46 17	46 17	46 17	46 15½	46 16	46 17	11 46 16,6					
	9 05 39	8 18	8 44½	10 48½	11 28	13 57½		30 28	84	30 23	82	At 2h. 30′ P.M. wound up the clock and put it forward 10′ 57″. C.G.
☽ 29	2 26 25	23 46	23 20	21 17½	20 39	18 18		30 20	79	30 17	88	
	11 45 02	45 02	46 02¼	46 03	46 03½	46 2¼	11 46 2½					
Reading off	9 19 13½	21 55	22 23	24 23½	25 04½	27 45		30 29	78	30 22	80	
	2 34 28	31 48	31 18	29 16	28 37	26 06½		30 24	79	30 16	81½	J. C.
	11 56 50¾	56 51½	56 50½	56 49¾	56 50¾	56 50¾	11 56 50⅔					
	9 36 52	39 48	40 16½	42 27½	43 11	45 53						
♂ 30	2 16 38	13 47	13 19	11 07	10 24	too late						
	11 56 45	56 47½	56 47¾	56 47¼	56 47		11 56 46,9					
	9 55 57	59 06½	59 40½	10 02 02½	2 50	5 50						
	1 57 38	54 32	late	51 36	50 48	47 48						
	11 56 47½	56 49¼		56 49¼	56 49	56 49	11 56 48,8					
	8 30 09	32 30	33 7	34 42	35 17½	37 31		30 28	80½	30 21	78	
	3 23 01	20 40	20 04	18 27	17 53	15 39		30 21	79	30 16	82	J. C.
☿ 31	11 56 35	56 35	56 35½	56 34½	56 35¼	56 35	11 56 35					
	9 36 08	38 59	39 29	41 39	42 21½	45 06						
	2 17 3½	14 13	13 43	11 32	10 51	08 06						
	11 56 35¾	56 36	56 36	56 35½	56 36¼	56 36	11 56 36					
June	8 24 00	25 19	26 46	28 31	29 06	31 18		30 24	89½	30 17	76½	
♃ 1	3 28 51	27 33	26 06	24 21	23 46	21 34		30 32	91	30 20	84	J. C.
	11 56 25½	56 26	56 26	56 26	56 26	56 26	11 56 26					

Obſervations

Obſervations of equal Altitudes of the Sun for the Time, made with the Aſtronomica Quadrant.

1769	Time per Clock of the Sun's Limb paſſing the Wires at equal Altitudes. h ′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	Mean noon per clock	Obſervatory Baro.	Obſervatory Th.	Side of the clock. Baro.	Side of the clock. Th.	Remarks.
June ♀ 2	7 39 46 4 12 53½ 11 56 14¾ 7 54 39 3 57 54½ 11 56 16¾	41 48 10 43 56 15½ 56 52 55 42 56 17	42 12 10 18 56 15 57 17 55 17 56 17	43 52 8 37 56 14½ 58 59 53 34 56 16½	44 24 8 6 56 15 59 32 53 02 56 17	46 29 6 1 56 15 8 01 38 50 55 56 16½	11 56 15 11 56 16,8	30 22 30 18	80½ 81	30 12 30 15	73½ 82½	J. C.
♄ 3	7 36 43 4 15 17 11 56 00 7 46 20 4 5 40 11 56 00	39 01 13 00 56 00½ 48 32½ 3 28 56 00¼	39 25 12 35 56 00 48 57 3 05 56 01	41 03 10 57 56 00 50 38½ 1 21 55 59¾	41 37 10 26 56 01½ 51 10 00 50 56 00	43 42 8 22 56 02 53 15 58 45 56 0	11 56 00⅔ 11 56 00⅐	30 20 30 20	86½ 86½	30 13 30 15	75 98	Note, the clo was expoſed the ſun from or 10 o'clock the morning 4 in the aft noon. J. C.
With ſtop ⊙ 4 With ſtop	8 11 43 3 39 58 11 55 50½ 8 29 50 3 21 50 11 55 50	14 02 37 40 55 51 32 13 19 28 55 50½	14 26 37 16 55 51 32 38 19 04 55 51	16 12 35 29 55 50½ 34 28 17 13 55 50½	16 45 34 56 55 50½ 35 02 16 39 55 50½	18 56 32 45 55 50½ 37 16½ late	11 55 50⅔ 11 55 50½	30 22 30 20	80 88½	30 13 30 13	77½ 86½	The obſerva ons of both y ſterday and t day are as gc as can be ma J. C.
☽ 5	9 11 41 2 39 28 11 55 34½	14 15 36 53 55 34	14 45 36 22½ 55 34¼	16 47½ 34 20 55 33¾	17 16 33 52 55 34	19 47 31 21 55 34	″ +1 +7 to reduce journeyman clock to the other clock. 11 55 38,1	30 19 30 17	83 80	30 14 30 15	83 84	C. G.
♂ 6	8 13 19 3 37 36 11 55 27½		16 1 44 57 55 29	17 48 33 10 55 29	18 22 32 37 55 29½	20 34: 30 26 55 30	11 55 29	30 18 30 14	79 81	30 16 30 10	80 88	C. G.
☿ 7	8 58 22 2 52 13 11 55 17½	00 45 49 50 55 17½	9 01 12 49 24 55 18	3 09 47 25 55 17	3 44 46 51 55 17½	6 10:: 44 26:: 55 18::	11 55 17½	30 20 30 17	85 82	30 18 30 16	83 86	C. G.
♃ 8	7 47 45- 4 02 34 11 55 09½	49 57 00 20 55 8½	Cloudy 3 59 57	52 05 58 14 55 09½	52 36 57 41 55 08½	54 40 Cloudy	11 55 09	30 20 30 17	84 82	30 17 36 14	83 83	C. G,
♀ 9	9 50 00 1 59 49 11 54 54½	53 02 56 44 54 53	53 35 56 11 54 53	56 04 53 46 54 55	56 47 53 02 54 54½	59 48 50 00 54 54	11 54 54	30 19 30 11	81 79	30 17 30 14	84 83½	C. G.
⊙ 11	9 57 43 1 51 38 11 54 40½	Cloudy in the afternoon.					11 54 40½	30 24	74	30 20	74	C. G.

Obſervati

Obfervations of equal Altitudes of the Sun for the Time, made with the Aftronomical Quadrant.

Time per Clock of the Sun's Limb paffing the Wires at equal Altitudes.								Obfervatory		Side of the clock		Remarks.
1769 June	h ′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	Mean noon per clock	Baro.	Th.	Baro.	Th	
	7 52 56	55 33	55 36		57 52:			30 20	78½	30 15	77	
☽ 12	3 56 14	53 35						30 17	80½	30 12	82	C. G.
	11 54 35	54 34					11 54 34,5					
	9 14 52	17 34	18 02	20 06	20 45			30 16	82	30 16	83	
♂ 13	2 34 10	31 28	30 59	28 54	28 15							C. G.
	11 54 31	54 31	54 30½	54 30	54 30		11 54 30½					
	8 34 03	36 31	36 57	38 49	39 23	41 40		30 16	81	30 10	80	
☿ 14	3 14 37	12 10	11 45	9 54	9 21	late		30 16	82	30 12	87	C. G.
	11 54 20	54 20½	54 21	54 21½	54 22		11 54 21					
	8 15 31	17 52	18 15	Cloudy	20 36	22 47		30 08	71	30 04	70	
	Cloudy	3 30 04	29 41	27 52	Cloudy	25 07		30 10	83	30 05	80	C. G.
♄ 17		53 58	53 58			53 57	11 53 57,7					
	8 29 32	31 59	Cloudy	34 16	34 48	37 05						
	Cloudy	3 15 55:	15 33::	13 40:	13 06	late						
		53 57		53 58	53 57		11 53 57,3					
	9 13 48	16 32	16 59	19 04	19 42	22 17		30 26	78½	30 14	79	
☉ 18	2 33 48	31 05	30 39	28 32	27 55	25 20		30 14	76	30 10	77	C. G.
	11 53 48	53 48½	53 49	53 48	53 48½	53 48½	11 53 48,5					
	8 52 55	55 28	55 55	57 49	58 25	9 00 50		30 17	74	30 13	72	
☽ 19	2 54 30		51 30	49 34	48 57	46 33		30 12	76	30 10	79	C. G.
	11 53 42½		53 42½	53 41½	53 41	53 41½	11 53 41,8					
	8 13 26	15 47	16 11	18 00	18 33	20 47		30 12	77	30 10	77	
♂ 20	3 33 45	31 24	31 00	29 11	28 38	26 25		30 18	84	30 08	83	C. G.
	11 53 35½	53 35½	53 35½	53 35½	53 35½	53 36	11 53 35,6					
	7 49 01	51 15	51 38	53 22	53 54	56 01		30 07	70	30 07	70	
☿ 21	3 57 50	55 35	55 12	53 27	52 55	50 49		30 10	78	30 12	80	C. G.
	11 53 25½	53 25	53 25	53 24½	53 24½	53 25	11 53 25					
	8 25 23	Cloudy in the afternoon.						30 17	82	30 16	80	
♃ 22	3 21 14											C. G.
	11 53 18½						11 53 18½					
	7 57 50	8 00 07	0 30	2 15	2 49	4 58		30 16	80	30 11	74	
♄ 24	3 48 10	45 54	45 31	43 46	43 12	41 01		30 11	78	30 09	82	C. G.
	11 53 00	53 00½	53 00½	53 00¼	53 00½	53 01½	11 53 00½					

Obſervations of equal Altitudes of the Sun for the Time, made with the Aſtronomical Quadrant.

1769	Time per clock of the Sun's Limb paſſing the Wires at equal Altitudes. h ′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	Mean noon per clock.	Obſervatory Baro.	 Th.	Side of the clock. Baro.	 Th.	Remarks.
June ☉ 25	10 06 49 1 39 00 11 52 54½	10 12 35 31 52 51½	10 50 34 55 52 52½	13 35 32 10 52 52½	14 27 31 20 52 53½	17 50 27 53 52 51¼	11 52 52⅔	30 18	84	30 15	81	C. G.
♂ 27	9 27 17 2 17 53 52 35	30 10 15 01 52 35½	30 39 14 33 52 36	32 49 12 21 52 35	33 31 11 39 52 35	36 15 8 56 52 35½	11 52 35⅓	30 14 30 07	78 78	30 13 30 10	78 80	C. G.
July ☉ 2	8 02 29 3 41 10 11 51 49½	} Cloudy in the afternoon.					11 51 49½	30 18 30 15	76 83	30 12 30 11	75 84	C. G.
☽ 3	9 07 13 2 36 10 11 51 41½	9 51 33 34 51 42½	10 19 33 06 51 42½	12 21 31 02 51 41½	13 00 30 25 51 42½	15 31 27 52 51 41½	11 51 42	30 18 30 14	82 81	30 11 30 11	82 86	C. G.
♂ 4	7 33 21 4 09 43 11 51 32	35 33 7 31 51 32	35 55	37 36 5 27 51 31½	38 07 4 57 51 32	40 13 2 50 51 31½	11 51 31,8	30 11 30 10	71 84	30 11 30 13	70 84	C. G.
♃ 6	7 29 44 4 12 35 11 51 09½	31 57 10 24 51 10½	32 19 10 03 51 11	34 01 8 19 51 10			11 51 10¼	30 22 30 18	72 78	30 13 30 17	71 80	C. G.
♄ 8	Took down the Clocks and Obſervatory; the Pendulum vibrated 1° 55′ on each Side the Center, the Bob remained as at Greenwich.											

Account

Account of the going of the Aftronomical clock at King George's Ifland, deduced from the foregoing Obfervations.

Day of the Month		Corrected noon per clock			Mean Time			Clock flow for M. T.		Clock lofes		Inter-val of	Daily lofs of clock
1769		H.	M.	S.	H.	M.	S.	M.	S.	M.	S.	Days	S.
May	10	11	51	44,8	11	56	2,8	4	18,0	0	43,4	2	21,7
	12	11	50	58,3	11	55	59,7	5	1,4	0	20,9	1	20,9
	13	11	50	36,7	11	55	59,0	5	22,3	0	22,7	1	22,7
	14	11	50	13,7	11	55	58,7	5	45,0	1	1,4	3	20,5
	17	11	49	15,0	11	56	1,4	6	46,4	0	20,6	1	20,6
	18	11	48	56,4	11	56	3,4	7	7,0	1	2,5	3	20,8
	21	11	48	3,0	11	56	12,5	8	9,5	0	42,5	2	21,2
	23	11	47	29,4	11	56	21,4	8	52,0	0	20,5	1	20,5
	24	11	47	14,1	11	56	26,6	9	12,5	0	18,7	1	18,7
	25	11	47	1,1	11	56	32,3	9	31,2	0	45,9	2	22,9
	27	11	46	28,1	11	56	45,2	10	17,1	0	14,6	1	14,6
	28	11	46	20,7	11	56	52,4	10	31,7	0	22,2	1	22,2
	29	11	46	6,3	11	57	0,2	10	53,9				
The clock was put forward 10′ 57″.										0	19,0	1	19,0
	30	11	56	52,5	11	57	8,4	0	15,9	0	22,2	1	22,2
	31	11	56	39,0	11	57	17,1	0	38,1	0	19,0	1	19,0
June	1	11	56	29,1	11	57	26,2	0	57,1	0	19,6	1	19,6
	2	11	56	18,9	11	57	35,6	1	16,7	0	25,5	1	25,5
	3	11	56	3,2	11	57	45,4	1	42,2	0	20,1	1	20,1
	4	11	55	53,3	11	57	55,6	2	2,3	0	23,0	1	23,0
	5	11	55	40,8	11	58	6,1	2	25,3	0	20,8	1	20,8
	6	11	55	30,9	11	58	17,0	2	46,1	0	22,3	1	22,3
	7	11	55	19,8	11	58	28,2	3	8,4	0	20,2	1	20,2
	8	11	55	11,0	11	58	39,6	3	28,6	0	26,5	1	26,5
	9	11	54	56,1	11	58	51,2	3	55,1	0	37,5	2	18,7
	11	11	54	42,3	11	59	14,9	4	32,6	0	18,7	1	18,7
	12	11	54	35,8	11	59	27,1	4	51,3	0	16,4	1	16,4
	13	11	54	31,8	11	59	39,5	5	7,7	0	22,2	1	22,2
	14	11	54	22,1	11	59	52,0	5	29,9	1	2,0	3	20,7
	17	11	53	58,1	0	0	30,0	6	31,9	0	22,0	1	22,0
	18	11	53	48,9	0	0	42,8	6	53,9	0	19,8	1	19,8
	19	11	53	42,0	0	0	55,7	7	13,7	0	19,3	1	19,3
	20	11	53	35,6	0	1	8,6	7	33,0	0	23,6	1	23,6
	21	11	53	24,8	0	1	21,4	7	56,6	0	19,4	1	19,4
	22	11	53	18,2	0	1	34,2	8	16,0	0	43,6	2	21,8
	24	11	53	0,0	0	1	59,6	8	59,6	0	20,9	1	20,9
	25	11	52	51,7	0	2	12,2	9	20,5	0	42,5	2	21,2
	27	11	52	34,1	0	2	37,1	10	3,0	1	45,6	5	21,1
July	2	11	51	47,5	0	3	36,1	11	48,6	0	18,9	1	18,9
	3	11	51	39,7	0	3	47,2	12	7,5	0	20,9	1	20,9
	4	11	51	29,6	0	3	58,0	12	28,4	0	42,5	2	21,2
	6	11	51	7,7	0	4	18,6	13	10,9				

Hence the daily rate of the clock's lofing on mean time, by a mean of thefe 40 refults, is 20,8 feconds. By the firft and laft days obfervations compared together, the clock loft 19′ 49,″9 on mean time in 57 days, which is at the rate of 20,″88 or 20,″9 per day. The fwing of the pendulum

pendulum on each fide of the perpendicular during this time, varied between 1° 50′ and 1° 55′.

REMARK. The fame clock, when fixed up at the Royal Obfervatory at Greenwich, before the voyage, with the pendulum of the fame length, got at the rate of 1′ 45,8″ per day, on mean time, between April 19 and July 18, 1768. Therefore the force of gravity at Greenwich is to that at King George's Ifland, as 1000000 to 997075. N. M.

Obfervations of meridian zenith diftances of the fun and fixed ftars for finding the latitude of the Obfervatory.

Day of the month	Name of the object	Meridian zen. dift.	Latitude South	Mean
1769		D. M. S.	D. M. S.	° ′ ″
May 6	Sun's lower limb	34 33 7	17 29 17	17 28 20
May 27	Sun's upper limb	38 39 10	17 27 52	
May 28	Ditto	38 50 0	17 29 9	
29	Ditto	38 59 0	17 29 2	
30	Ditto	39 8 12	17 29 26	
31	Ditto	39 16 21	17 29 11	
June 7	Ditto	40 3 32	17 29 29	
8	Ditto	40 9 0	17 28 42	
9	Ditto	40 13 0	17 27 51	
10	Ditto	40 17 0	17 27 54	
11	Ditto	40 21 0	17 27 21	
12	Ditto	40 26 0	17 28 42	
13	Ditto	40 29 0	17 28 28	
15	Ditto	40 34 0	17 28 14	
17	Ditto	40 36 30	17 27 10	
18	Ditto	40 38 30	17 27 59	
19	Ditto	40 39 0	17 27 48	
20	Ditto	40 39 30	17 27 54	
22	Ditto	40 39 30	17 28 27	
25	Ditto	40 44 56	17 27 48	
27	Ditto	40 30 0	17 27 33	
June 21	Arcturus	37 53 0	17 30 29	17 29 9
22		37 50 0	17 27 29	
24		37 51 40	17 29 9	
July 4		37 52 0	17 29 29	
June 24	α Lyræ	56 3 20	17 29 53	17 29 43
27		56 3 0	17 29 33	
June 24	γ Aquilæ	27 32 20	17 28 45	17 28 59
28		27 32 48	17 29 13	
June 28	α Aquilæ	25 44 30	17 28 20	17 28 20
June 28	β Aquilæ	23 19 0	17 28 30	17 28 30
June 24	α Cygni	61 56 0	17 29 36	17 28 56
28		61 54 40	17 28 16	

The

The ſun and foregoing ſtars paſſed the meridian to the North; the following ſtars paſſed the meridian to the South above the pole.

Day of the month	Name of the object	Meridian zen. diſt.	Latitude South	Mean
1769		D. M. S.	D. M. S.	° ′ ″
June 23	Fomalhaut	13 20 0	17 29 37	17 29 37
June 2[illegible]	α Crucis	44 20 0	17 28 44	17 28 44
June 23	γ Crucis	38 19 0	17 29 50	17 29 50
June 21 23	β Crucis	40 54 30 40 54 45	17 30 36 17 30 21	17 30 28
June 22 24	α Centauri	42 22 0 42 21 40	17 29 59 17 30 19	17 30 9
June 21 24 27	β Centauri	41 44 10 41 44 26 41 44 32	17 30 9 17 29 53 17 29 47	17 29 56
June 2[illegible] 24	β Gruis	30 33 40 30 35 0	17 30 18 17 28 18	17 29 38
June 23	β Hydri	61 1 15	17 29 54	17 29 54
June 24 28	α Pavonis	39 57 36 39 56 44	17 28 5 17 28 57	17 28 31

The mean of the ſeven mean reſults from the ſun and ſix ſtars, to the North, gives the latitude 17° 28′ 51″ S. The mean of the nine reſults from the nine ſtars to the South, gives the latitude 17° 29′ 38″ S. The mean of theſe two means is 17° 29′ 15″ S. which may be taken for the latitude of the obſervatory.

N. B. Before any obſervations were made with the quadrant, the line of collimation was adjuſted, by means of a diſtant object, by inverting the quadrant.

Remark. It muſt be confeſſed, that the reſults of theſe obſervations (moſt of which were made by Mr. Green) differ more from one another than they ought to do, or than thoſe do made by other obſervers, with quadrants of the ſame ſize, and made by the ſame artiſt, the cauſe of which, if not owing to want of care and addreſs in the obſerver, I don't know how to aſſign. N. M.

Lunar Obſervations for the Longitude.

Month	Day	Time per clock	Alt. or Z.D. of ⊙ or ✳	Alt. or Z.D. of the ☽	Diſt. of ☽ a. ⊙ or ☽ and ✳	Whether Alt. or Z. D. and what Limb	Error of Quadrant	Apparent Time cor.	Longitude given	Mean of each days ſets
		h ′ ″	° ′ ″	° ′ ″	° ′ ″		′ ″	h ′ ″	° ′ ″	° ′ ″
1769 April		22 17 30	47 34 00	54 30 50	57 31 30	⊙'s Alt. L. L.	☽ an. ⊙ — 2 30	22 25 40	149 23 15	
☽	30	22 18 46	51 10 00	52 00 10	26 40	☽'s Alt. U.L.	☽ — 5 30	37 24	20 30	
		22 27 54	52 16 40	49 52 50	23 55			46 24	25 45	149 23 10
					✳'s N. L.					
May		9 52 57	27 29 40	79 54 00	52 55 00	✳'s Altitude	✳ — 2 00	10 03 19	148 59 00	
✳ Regulus	16	10 02 34	24 58 20	77 51 40	52 58 10	☽'s Alt. L.L.		12 56	149 14 15	
♂		10 12 57	22 34 20	75 21 00	53 01 05			23 19	39 15	149 17 30
					✳ F. L.					
✳ Antares		10 44 26	25 17 17	68 14 00	46 44 27	✳'s Zen. Diſt.	✳'s Z.D. + 2 00	10 54 48	149 34 15	
		10 59 00	22 07 40	64 55 00	40 00	☽'s Alt. L.L.		11 09 22	19 15	
♂	16	11 09 48	19 51 00	62 27 00	37 00			11 20 10	07 30	149 20 30
✳ Regulus		8 05 46	47 28 30	26 53 10	64 29 30	✳'s Altitude		8 16 21	149 57 30	
		21 54	44 35 00	23 25 50	34 55	☽'s Z.D. U.L.	✳'s Alt. — 2 00	32 29	43 30	
☿	17	30 03	43 06 40	21 11 50	38 10			40 38	150 30 15	150 03 45
☽ and ⊙		21 28 54	38 22 00	26 55 10	100 37 05	⊙'s Alt. L.L	⊙'s Alt. — 6 00	21 42 22	148 01 45	
		42 29	40 31 20	24 05 50	30 45	☽'s Alt. U.L.	☽'s Alt. — 5 00	21 55 59	21 45	
♀	26	55 34	42 25 40	21 03 40	24 40			22 09 04	27 30	148 17 30
☽ and ⊙		22 24 35	45 50 00	32 16 50	75 25 30			22 38 30	149 52 00	
		29 51	46 24 00	31 06 00	23 50	⊙'s Alt. L. L.	☽ — 4 00	43 46	30 00	
⊙	28	39 19	47 21 00	28 57 20	20 10	☽'s Alt. U.L.	☽ and ⊙ + 1 00	53 14	41 45	
		44 48	47 52 20	27 40 40	18 20			58 43	30 30	
		51 33	48 22 20	26 08 20	15 40			23 05 28	21 30	149 35 09
✳ α Aquilæ		18 49 32	43 12 20	52 53 40	67 03 30	✳'s Altitude	☽ and ✳ + 1 30	18 52 44	149 54 15	
☽	29	56 38	41 56 20	54 06 40	05 26	☽'s Alt. L.L.	☽'s Alt. — 4 00	59 50	50 15	149 52 15
☽ and ⊙		22 10 57	42 36 20	45 37 40	62 31 50			22 14 09	150 05 15	
		18 03	43 36 20	44 05 40	30 20	⊙'s Alt. L. L.	☽'s Alt. — 4 00	21 15	149 36 30	
		25 28	44 29 00	42 32 20	27 40	☽'s Alt. U.L.	☽ and ⊙ + 1 30	28 40	28 15	
☽	29	30 46	45 10 20	41 28 50	25 20			33 58	42 30	
		35 53	45 48 00	40 28 20	23 50			39 05	34 30	149 41 24
June		4 18 53	13 36 00	42 52 20	111 32 50	⊙'s Alt. L. L.	☽'s Alt. — 3 00	4 24 17	151 03 00	
☽ and ⊙		28 12	11 37 00	40 36 40	34 10	☽'s Z.D. U.L.	☽ and ⊙ + 1 30	33 56	150 19 30	
☽	12	37 56	9 39 50	38 24 00	36 20			43 20	18 30	150 33 40
		3 17 21	25 56 40	66 39 40	122 20 45			3 22 53	148 43 30	
		22 54	24 35 50	65 03 20	24 30	⊙'s Alt. L. L.		28 25	149 35 20	
		29 43	23 23 40	63 33 20	26 50	☽'s Z.D. U.L.		35 15	22 20	
♄	13	44 36	20 25 20	60 01 40	32 10			50 08	08 15	
		50 48	19 07 00	58 34 40	35 00			56 20	29 00	149 15 43

Lunar Obſervations for the Longitude.

Month	Day	Time per clock	Alt. or Z.D. of ☉ or ∗	Alt. or Z. D. of the ☽	Diſt. of ☽ a. ☉ or ☽ and ∗	Whether Alt. or Z. D. and what Limb	Error of Quadrant	Apparent Time cor.	Longitude given	Mean of each Days Sets
1769		h ′ ″	° ′ ″	° ′ ″	° ′ ″		′ ″	h ′ ″	° ′ ″	° ′ ″
June		10 09 34	47 59 40	14 26 40	55 33 20	∗'s Altitude	∗'s Alt. + 5 00	10 15 40	149 00 51	
∗ Spica		20 14	45 26 10	12 14 00	36 50	☽'s Z.D.U.L.	☉ and ∗ + 1 30	26 20	17 30	
♄	17	28 34	43 30 00	10 36 20	40 00			34 40	39 00	149 18 55
					∗ N. L.					
∗ Fomalh.		13 27 30	46 46 00	68 20 00	63 08 20	∗'s Zen.Diſt.		13 33 46	150 01 15	
		38 49	44 20 26	65 53 00	5 10	☽'s Alt. L.L.	☽ and ∗ + 1 30	45 05	26 00	
☉	18	46 56	42 36 00	64 15 20	3 30			53 12	10 15	150 12 30
					∗ F. L.					
∗ α Aquilæ		15 10 21	51 09 20	27 08 40	51 01 00	∗'s Altitude	∗'s Alt. − 3 00	15 17 26	150 15 30	
		20 04	49 51 40	25 24 40	3 00	☽'s Z.D.L.L.	☽ and ∗ + 1 30	27 09	149 52 00	
♄	24	27 32	48 22 40	24 02 00	4 47			34 57	46 00	149 57 50
		21 46 12	38 41 20	30 49 20	79 45 31			21 53 37	149 29 00	
		54 20	39 53 40	29 08 00	42 20	☉'s Alt. L. L.	☽'s Alt. − 3 00	22 01 45	34 15	
☽	26	59 42	40 39 00	28 00 10	40 20	☽'s Alt. U.L.	☽ and ☉ + 1 30	07 07	23 15	
		22 4 03	41 12 20	27 05 00	38 10			11 28	33 30	
		7 48	41 40 00	26 17 20	37 00			15 13	10 15	149 26 03
		20 36 16	26 56 00	50 01 20	66 54 10			20 43 47	149 09 45	
		44 07	28 23 20	48 56 20	50 30	☉'s Alt.L.L.	☽'s Alt. − 3 00	51 38	43 00	
♂	27	50 31	29 31 40	48 00 40	49 20	☽'s Alt.U.L.	☽ and ☉ + 1 30	58 02	12 15	
		55 31	30 25 20	47 14 40	47 26			21 03 02	27 45	
		59 52	31 09 40	46 34 40	45 40			07 23	30 00	149 24 33
		21 13 57	33 43 20	40 28 00	39 06 10	☉'s Alt. L.L		21 21 44	149 52 30	
♀	30	23 52	35 24 00	40 45 40	02 07	☽'s Z.D.cent.		31 39	150 14 45	
		30 12	36 25 20	41 00 20	00 10			37 59	10 15	150 05 50

Note. Every line of the Lunar Obſervations is the mean of three, which we call a ſet. We take three or five ſuch ſets at a time, and calculate the mean of each ſeparately. The ground where all the Altitudes were taken, is 13 feet 6 inches above the horizontal level: the zenith diſtances are all taken with the Aſtronomical Quadrant. Theſe diſtances of the Moon from the Sun and fixt ſtars, were obſerved with a braſs Hadley's ſextant, fitted with edge-bars, made by Mr. Ramſden.

Mean of theſe Obſervations gives George's Iſland to be in Long. 149° 36′ 38″ W. of Greenwich Obſervatory, at ♀'s fort.

Obſervations

Obfervations of the Eclipfes of Jupiter's Satellites, with reflecting Telefcopes of 2 Feet Focus, and the Longitude of the Obfervatory thence deduced.

1769	Time per clock	Apparent time cor.		Phænomena and Sat.	Time at Green. per Naut. Alm.	Long. W. of Green. in time
	h ′ ″	h ′ ″				
May 10	10 02 30	16 11 1		Emerf. of the 1ft Sat.		
	16 03 3c	Capt. Cook	16 12 1			
12	10 27 55	10 37 6		Ditto		
	10 28 05	Capt. Cook	10 37 16			
27	11 44 04	11 57 39	11 57 40	Second Satellite		
	11 44 05	Capt. Cook	very clear			
	11 47 15	12 00 51	12 1 44	Third ditto		
	11 48 08	Capt. Cook				
June 4	10 41 19	10 45 31		Firft ditto	20 44 39	9 59 4
	10 41 28	Capt. Cook	10 45 40			
13	7 02 45	7 08 19		Ditto	17 6 31	9 58 14
	7 02 45	Capt. Cook	7 8 16			
18	14 27 21	14 33 36		Ditto	24 31 41	9 57 41
	14 28 09	Capt. Cook	14 34 24			
20	8 55 15	9 1 43		Ditto	19 0 2	9 58 19
21	8 46 45	8 53 22				
	8 47 44	Capt. Cook	8 54 21	Second Satellite		
27	10 48 45	10 56 15		Firft ditto	20 53 43	9 57 28
July 4	12 42 40	12 51 16		Ditto	22 47 33	9 56 17
6	7 09 20	7 18 16		Ditto	17 16 05	9 57 47
	7 09 25	Capt. Cook	7 18 21			

Mean of the feven obfervations of the firft Satellite, rejecting thofe of the 10th and 12th of May, as too near Jupiter's oppofition to the Sun, gives the longitude of Venus's Fort $9^h\ 57'\ 50'' = 149°\ 27'\ 30''$. Add $20''$ for the correction of the times in the nautical almanack, as found by the obfervations of March 29 and April 12, at Greenwich, the true longitude will be $9^h\ 58'\ 10'' = 149°\ 32'\ 30''$.

Eclipfe of the Moon.

June 18	8 18 5	8 24 18	Beginning of the eclipfe
	11 52 30	11 58 44	End of the eclipfe
	11 52 10	11 58 24	Ditto by Capt. Cook
	11 55 37	12 1 48	The ☽ clear of the penumbra
	11 55 10	12 1 21	Ditto by Capt. Cook.

Tranfit of Venus by Mr. Green, with a reflecting telefcope of 2 feet focus, magnifying power 140 times.

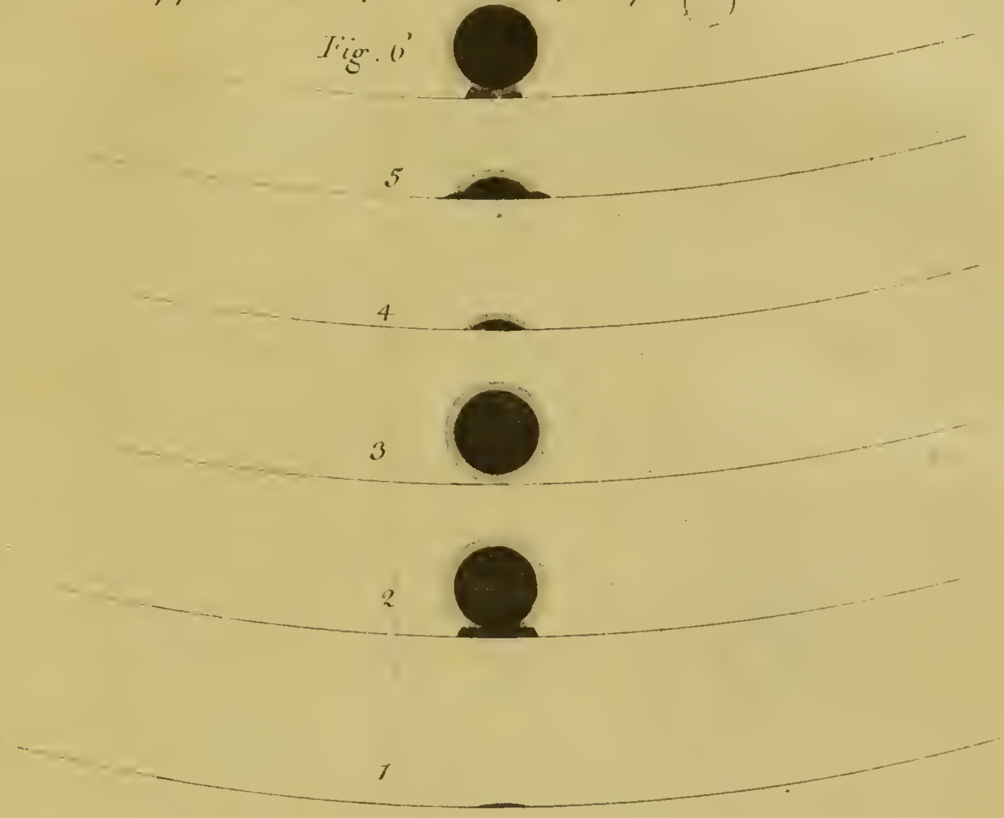
Time per clock h ′ ″		App. time June 2
9 21 45	Light thus on the ☉'s limb, TAB. XIV. fig. 1.	21 25 40
22 00	Certain, fig. 2.	21 25 55
39 20	Firft internal contact of ♀'s limb and the ☉ fee fig. 4.	21 43 15
40 00	Penumbra and ☉'s limb in contact, fee fig. 5.	21 43 55
		June 3
3 10 05	First contact of penumbra, undulating, but the thread of light vifible and invifible alternately	3 14 3
10 53	Second internal contact of the bodies	3 14 51
27 30	Second external contact	3 31 28
28 16	Total egrefs of penumbra, ☉'s limb perfect	3 32 14

Tranfit of Venus by Capt. Cook, with a reflecting telefcope of 2 feet focus, and the magnifying power 140.

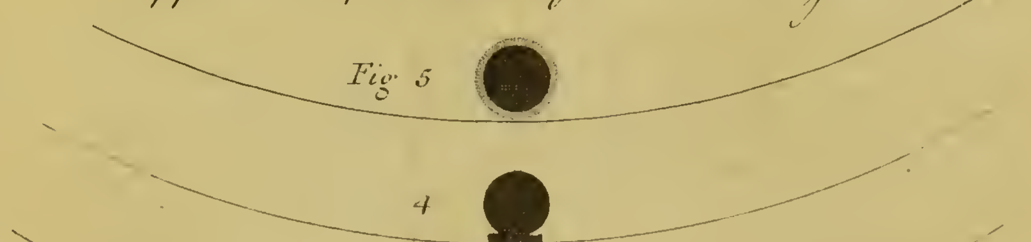
Time per clock h ′ ″		App. time June 2
9 21 50	The firft vifible appearance of ♀ on the ☉'s limb, fee fig. 1.	21 25 45
39 20	Firft internal contact, or the limb of ♀ feemed to coincide with the ☉'s, fig. 2.	21 43 15
40 20	A fmall thread of light feen below the penumbra, fig. 3.	21 44 15
		June 3
3 10 15	Second internal contact of the penumbra, or the thread of light wholly broke	3 14 13
10 47	Second internal contact of the bodies, and appeared as in the firft	3 14 45
27 24	Second external contact of the bodies	3 31 22
28 04	Total egrefs of penumbra, dubious	3 32 2

The firft appearance of Venus on the Sun, was certainly only the penumbra, and the contact of the limbs did not happen till feveral feconds after, and then it appeared as in fig. the 4th; this appearance was obferved both by Mr. Green and me; but the time it happened was not noted by either of us; it ap-

Appearances of Venus by Capt. Cook.



Appearances of Venus by Mr. Charles Green.



Basire sc.

peared to be very difficult to judge precifely of the times that the internal contacts of the body of Venus happened, by reafon of the darknefs of the penumbra at the Sun's limb, it being there nearly, if not quite, as dark as the planet. At this time a faint light, much weaker than the reft of the penumbra, appeared to converge towards the point of contact, but did not quite reach it, fee fig. 2. This was feen by myfelf and the two other obfervers, and was of great affiftance to us in judging of the time of the internal contacts of the dark body of Venus, with the Sun's limb. Fig. the 5th, is a reprefentation of the appearance of Venus at the middle of the egrefs and ingrefs, for the very fame phænomenon was obferved at both: at the total ingrefs, the thread of light made its appearance with an uncertainty of feveral feconds; I judged that the penumbra was in contact with the Sun's limb 10″ fooner than the time fet down above; in like manner at the egrefs the thread of light was not broke off or diminifhed at once, but gradually, with the fame uncertainty: the time noted was when the thread of light was wholly broke by the penumbra. At the total egrefs I found it difficult to diftinguifh Venus's limb from the penumbra; which of courfe made the fecond external contact a little doubtful, and the precife time that the penumbra left the Sun could not be obferved to any great degree of certainty, at leaft by me. Some of the other gentlemen, who were fent to obferve at different places, faw at the ingrefs and egrefs the fame phænomenon as we did; though much lefs diftinct, which no doubt was owing to their telefcopes being of a lefs magnifying power; for the penumbra was vifible through my telefcope during the whole Tranfit; and Dr. Solander, whofe telefcope magnified more than ours, faw it, I have reafon to think, diftincter than either Mr. Green or myfelf; though we both of us faw enough to convince our fenfes, that fuch a phænomenon did indifputably exift, and we had a good opportunity to obferve it, for every wifhed-for favourable circumftance attended the whole of that day, without one fingle impediment, excepting the heat, which was intolerable: the thermometer which hung by the clock and was expofed to the fun as we were, was one time as high as 119°. The breadth of the penumbra appeared to me, to be nearly equal to $\frac{1}{8}$th of Venus's femidiameter.

Tranfit

Tranſit of Venus by Dr. Solander, with a 3 feet reflecting teleſcope.

Timepe clock h ′ ″		App. time
9 22 11	Firſt external contact plainly convex, a wavering haze ſeen ſome ſeconds before	
9 39 33	Ingreſs, light ſeen glimmering under Venus	21 43 28
9 40 07	♀'s free from the ☉'s limb	21 44 2
3 27 51	♀'s true limb out	3 31 49
3 28 15	♀'s atmoſphere out	3 32 13

Obſervations of the Tranſit of Venus, made by Mr. Charles Green, with Dollond's micrometer fitted to a reflecting teleſcope of 2 feet focus.

June 2 1769.

In.	pts.	ver.		In.	pts.	ver.	
0	10	24	Venus's diameter meaſured off the ſcale.	0	10	5	Venus's diameter meaſured on the ſcale.
0	10	24		0	10	5	
0	10	24		0	10	$4\frac{1}{2}$	
0	15	0		0	10	4	
0	10	24		0	10	4	
0	10	24		0	10	5	
0	10	24		0	10	4	
Mean 0	10	24,14		Mean 0	10	4,50	

Half the difference of theſe two means is $+9{,}82$ ver. $=+8{,}4''$ the correction of the adjuſtment of the micrometer to be added to all obſervations made on the ſcale; and half the ſum of the two means is 10 pts. 14,31 ver. $= 54{,}97''$ Venus's apparent diameter.

After the above meaſurements of Venus' diameter, I fixed my teleſcope on an equatoreal ſtand, which was ſcrewed down to a large caſk filled with ſand and water; and by repeated trials a day before, an object (as the ſun) would move on along the wire a quarter of an hour without any ſenſible difference. Thus equipped, I took the following obſervations, a careful perſon noting the time by the clock and another writing down. By repeated

peated trials fome days before, I found the telefcope at diftinct vifion, when it ftood at o on the fcale; therefore I put it to this before I meafured Venus's diameter.

I read them all off myfelf and faw each written down.

Time by the clock			Apparent time			Meafure by micrometer			D°. red. & cor. in min. & fec.		
H	M	S	H	M	S	In.	pts.	V.	M	S	
22	35	28	22	39	24	0	50	18	3	57:4	Difference of declination between the North limb of Venus and the North limb of the Sun.
22	41	0	22	44	56	0	50	15	3	54,8	
22	45	34	22	49	30	0	50	12	3	52.3	
22	50	54	22	54	50	0	50	20	3	59,1	
22	57	44	23	1	40	0	50	8	3	48,8	
23	6	47	23	10	43	1	75	2	12	37,7	Diftance of the Eaftern limbs of the Sun and Venus in lines parallel to the equator; or rather the tranflation of Venus, in order to produce an artificial internal contact with the Sun to the Eaft.
23	11	5	23	15	1	1	75	18	12	51,4	
23	14	51	23	18	47	1	80	7	13	3,3	
23	18	36	23	22	32	2	35	19	17	8,5.	Diftance of the Weftern limbs of the Sun and Venus in lines parallel to the equator.
23	23	21	23	27	17	2	30	16	16	44,5	
23	25	49	23	29	45	2	30	0	16	30,8	
23	31	9	23	35	6	0	55	24	4	23,8	Difference of declination between the Northern limbs of the Sun and Venus.
23	35	50	23	39	47	0	60	3	4	27,3	
23	40	9½	23	44	6	0	60	5	4	29,0	
23	45	2	23	48	59	3	75	12	27	0,5	Difference of declination between the South limb of the Sun and the North limb of Venus.
23	47	53	23	51	50	3	75	14	27	2,2	
23	50	0	23	53	57	3	75	7	26	56,2	

I now took my telefcope from the equatoreal ftand, and placed it on its own proper ftand and took the following obfervations.

June 3 1769											
0	2	10	0	6	7	0	80	3	5	52,7	Neareft dift. of ☉'s N. limb from ♀'s S. limb
0	7	53	0	11	50	3	75	2	26	51,9	Greateft dift. of ☉'s S. limb from ♀'s N. limb
0	11	42	0	15	39	0	80	2	5	51,8	Neareft dift. of ☉'s N. limb from ♀'s S. limb
0	14	17	0	18	14	3	75	3	26	52,8	Greateft dift. of ☉'s S. limb from ♀'s N. limb
0	18	19	0	22	16	0	80	0	5	50,1	Neareft dift. of ☉'s N. limb from ♀'s S. limb
0	20	14	0	24	11	3	70	16	26	42,5	Greateft dift. of ☉'s S limb from ♀'s N. limb
0	23	13	0	27	10	0	80	6	5	55,2	Neareft dift. of ☉'s N. limb from ♀'s S. limb
0	25	28½	0	29	25	3	70	18	26	44.2	Greateft dift. of ☉'s S. limb from ♀'s N. limb
0	27	37	0	31	34	0	80	5	5	54,4	Neareft dift. of ☉'s N. limb from ♀'s S. limb

Time

Time by the clock			Apparent time			Meaſure by micrometer			D°. red. & cor. in min. & ſec.		
H	M	S	H	M	S	In.	pts.	V.	M	S	
0	29	35	0	33	32	3	70	20	26	45,9	Greateſt diſt. of ⊙'s S. limb from ♀'s N. limb
0	34	18	0	38	15	0	80	5	5	54,4	Neareſt diſt. of ⊙'s N. limb from ♀'s S. limb
0	36	59	0	40	56	3	70	19	26	45,1	Greateſt diſt. of ⊙'s S. limb from ♀'s N. limb
0	38	53½	0	42	50	0	80	3	5	52,7	Neareſt diſt. of ⊙'s N. limb from ♀'s S. limb
0	40	37	0	44	34	3	70	24	26	49,3	Greateſt diſt. of ⊙'s S. limb from ♀'s N. limb
0	42	19	0	46	16	0	80	4	5	53,5	Neareſt diſt. of ⊙'s N. limb from ♀'s S. limb
0	44	34	0	48	31	3	70	23	26	48,5	Greateſt diſt. of ⊙'s S. limb from ♀'s N. limb
0	46	12	0	50	9	3	75	0	26	50,2	Greateſt diſt. of ⊙'s S. limb from ♀'s N. limb
0	48	11	0	52	8	0	80	0	5	50,1	Neareſt diſt. of ⊙'s N. limb from ♀'s S. limb
0	58	13	1	2	10	0	75	15	5	41,6	Neareſt diſt. of ⊙'s N. limb from ♀'s S. limb
1	0	55	1	4	52	3	75	10	26	58,8	Greateſt diſt. of ⊙'s S. limb from ♀'s N. limb
1	3	4	1	7	1	0	75	9	5	36,5	Neareſt diſt. of ⊙'s N. limb from ♀'s S. limb
1	5	51	1	9	48	3	75	15	27	3,0	Greateſt diſt. of ⊙'s S. limb from ♀'s N. limb
1	8	21½	1	12	19	0	75	2	5	30,5	Neareſt diſt. of ⊙'s N. limb from ♀'s S. limb
1	10	47	1	14	44	3	75	21	27	8,2	Greateſt diſt. of ⊙'s S. limb from ♀'s N. limb
1	12	34	1	16	31	0	70	21	5	25,4	Neareſt diſt. of ⊙'s N. limb from ♀'s S. limb
1	15	57	1	19	54	3	80	8	27	18,3	Greateſt diſt. of ⊙'s S. limb from ♀'s N. limb
1	17	12	1	21	9	0	70	16	5	21,1	Neareſt diſt. of ⊙'s N. limb from ♀'s S. limb
1	19	34½	1	23	32	0	70	15	5	20,2	Neareſt diſt. of ⊙'s N. limb from ♀'s S. limb

At the laſt obſervation I looked at the thermometer which was cloſe by me, and it was 113 degrees high.

With my teleſcope as before, I meaſured the following horizontal diameters of the Sun and Venus.

	4 35 24		The Sun's horizontal diameter.
	4 35 24		
	4 40 0		
	4 40 0		
	4 35 24		
	4 35 24		
	4 40 0		
Mean	4 35 24,43	31 27,4	

	0 10 24	Venus's diameter meaſured off the ſcale.
	0 10 23½	
	0 10 24	
	0 10 24	
	0 10 24½	
Mean	0 10 24	

	0 10 3	Venus's diameter meaſured on the ſcale.
	0 10 4	
	0 10 4	
	0 10 5	
	0 10 6	
	0 10 3	
Mean	0 10 4,16	

Half

Half the difference of thefe two means, is + 9,92 vern. = +8,5″ the correction of the adjuftment of the micrometer, which only differs $\frac{1}{10}$th of a fecond from what was found by the meafures of Venus's diameter before. Half the fum of the two means is 10 14,08 = 54,77″ Venus's apparent diameter, which was found before 54,97″. The mean of the two refults is 54,87″ or 54,9″.

After the laft meafurements of the Sun and Venus's diameters, I replaced my telefcope on the equatoreal ftand, and took the following obfervations.

Time by the clock			Apparpnt time			Meafure by micrometer			D°. red. & cor. in min. & fec.		
H	M	S	H	M	S	In.	pts.	V.	M	S	
1	59	37	2	3	35	0	85	21½	6	29,9	Dift. of W. L. of ☉ and ♀ in lines parall. to the equat.
2	6	32	2	10	30	3	45	0	24	42,0	Dift. of E. L. of ☉ and ♀ in lines parallel to the equat.
2	10	44	2	14	42	0	95	10	7	2,8	Diff. of declin. of N. L. of ☉ and ♀
2	14	30	2	18	28	3	25	4	23	20,0	Diff. of declin. of S. L. of ☉ and ♀
2	17	55	2	21	53	1	0	2	7	17,3	Diff. of declin. of N. L. of ☉ and ♀
2	21	5	2	25	3	3	25	1	23	17,5	Diff. of declin. of S. L. of ☉ and ♀
2	24	7	2	28	5	0	75	20	5	45,9	Dift. of W. L. of ☉ and ♀ in lines par. to the equat.
2	27	54	2	31	52	3	65	0	26	7,4	Dift. of E. L. of ☉ and ♀ in lines par. to the equat.

Here follows the Table of the value of the fcale of the object glafs micrometer, which was delivered in by Mr. Short, together with the telefcope, by which the reductions of the foregoing obfervations were made.

Table for the object glafs micrometer; the focal length of which object glafs is = 482,867 inches.

Inches	Correfponding angle in min. and fec.		Dec. of an inch	Angle in min. and fec.		Parts of the ver.	Angle in feconds
	′	″		′	″		″
1	7	7,2	,05	0	21,4	1	0,9
2	14	14,3	,10	0	42,7	2	1,7
3	21	21,4	,15	1	4,1	3	2,6
4	28	28,6	,20	1	25,4	4	3,4
5	35	35,8	,25	1	46,8	5	4,3
			,30	2	8,1	6	5,1
			,35	2	29,5	7	6,0
			,40	2	50,9	8	6,8
			,45	3	12,2	9	7,7
			,50	3	33,6	10	8,6
			,55	3	54,9	11	9,4
			,60	4	16,3	12	10,3
			,65	4	37,6	13	11,1
			,70	4	59,0	14	12,0
			,75	5	20,4	15	12,8
			,80	5	41,7	16	13,7
			,85	6	3,1	17	14,5
			,90	6	24,4	18	15,4
			,95	6	45,8	19	16,3
						20	17,1
						21	18,0
						22	18,8
						23	19,7
						24	20,5

Obfervations

Obſervations on the Tranſit of Venus, June 3, 1769, by Dollond's micrometer fitted to a reflecting teleſcope of 18 inches focus, by Capt. James Cook.

Venus's diameter, ſoon after the ingreſs.

Off the ſcale			On the ſcale			
In.	Dec.	Ver.	In.	Dec.	Ver.	
0	10	4	0	10	4	By theſe meaſurements the correction of adjuſtment of the micrometer —0¾ of a diviſion of the vernier, and ♀'s diameter 10 d. 4½ v. = 56,8.
0	10	3	0	10	6	
0	10	4	0	10	6	
0	10	4	0	10	5	
0	10	3¾	0	10	5¼	

June 2 Time per Cl. h	′	″	Appar. Time h	′	′	In.	D.	V.	M	S	
23	3	1	23	6	57	3	20	20	28	6,6	Greateſt diſtance of ♀ and ☉ in outer contact.
23	6	46	23	10	42	3	20	18	28	4,6	
23	10	8	23	14	4	3	20	15	28	1,4	
23	14	36	23	18	32	3	20	10	27	56,2	
23	24	36	23	28	32	0	55	23	5	8,8	Leaſt diſtance of ♀ and ☉ in outer contact.
23	26	38	23	30	35	0	60	3	5	14,0	
23	29	38	23	33	35	0	60	4	5	15,1	
23	31	54	23	35	51	0	60	9	5	20,3	

Venus's diameter June 3.

	Off the ſcale			On the ſcale			
	In.	D.	V.	In.	D.	V.	
	0	10	2	0	10	6	By theſe meaſurements the correction of adjuſtment is —2¼ and Venus's diameter 0 10 4 = 56,″28.
			1			7	
			2			6	
			2			6	
Mean	0	10	1¾	0	10	6¼	

The Sun's horizontal diameter at 0^h 22′.

In.	D.	V.
3	60	18
		16
		19
		17

The

The Sun's horizontal diameter at 0^h 22′.

In.	D.	V.
		16
		17
		17
		20
		18
		17
		20
		21

Mean 3 60 18 From which fubtract $2\frac{1}{4}$ leaves In. 3 D. 60 V. $15\frac{3}{4}$

Time per Cl.			App. Time			Meafure by micrometer			D°. red. and corrected			
h	′	″	h	′	″	In.	D.	V.	M	S		
1	4	29	1	8	26	3	10	11	27	3,8	Greateft diftance of ♀ and ☉ in outer contact.	
	7	17	1	11	14	3	10	18	27	11,5		
	8	33	1	12	30	3	10	23	27	16,3		
1	14	16	1	18	13	0	60	15	5	25,1	Leaft diftance of ♀ and ☉ in outer contact.	
	15	45	1	19	42	0	60	15	5	25,1		
	16	55	1	20	52	0	60	9	5	18,8		
1	25	25	1	29	22	3	15	18	27	37,2	Greateft dift.	of the limbs of the Sun and Venus meafured externally.
1	27	29	1	31	26	0	55	18	5	2,3	Leaft diftance	
1	32	15	1	36	13	3	20	1	27	45,5	Greateft dift.	
1	34	12	1	38	10	0	55	4	4	47,7	Leaft diftance	
1	36	5	1	40	3	3	20	10	27	54,9	Greateft dift.	
1	38	19	1	42	17	0	55	0	4	43,5	Leaft diftance	
1	40	5	1	44	3	3	20	15	28	0,1	Greateft dift.	
1	41	48	1	45	46	0	50	21	4	39,4	Leaft diftance	
1	43	24	1	47	22	3	20	22	28	7,4	Greateft dift.	
1	46	0	1	49	58	0	50	15	4	33,1	Leaft diftance	

The Sun's diameter at 2^h 10′,

3	60	18
		18
		$16\frac{1}{2}$
		17
		17
		20
		16

Mean 3 60 $17\frac{1}{2}$ from which fubtract $3\frac{3}{4}$ leaves 3 60 $13\frac{3}{4}$

Venus's diameter.

0	10	2	0	10	8
	5	24			6
	5	24			8
	10	0			8
0	10	0	0	10	$7\frac{1}{2}$

By these measurements, the correction of adjustment of the micrometer, is — $3\frac{3}{4}$ and Venus's diameter 10 D. $3\frac{3}{4}$ V. = 56″,02.

The mean of the three separate deductions of Venus's observed diameter, is 56″,4.

A TABLE for reducing the foregoing observations deduced from the measures of the Sun's horizontal diameter, supposed = 31′ 31″.

Inches	Angle ′	Angle ″	Decimals of an In.	Angle M	Angle S	Div. of Vern.	Angle S
1	8	41,1	,05	0	26,1	1	1,0
2	17	22,2	,10	0	52,1	2	2,1
3	26	3,3	,15	1	18,2	3	3,1
			,20	1	44,2	4	4,2
			,25	2	10,3	5	5,2
			,30	2	36,3	6	6,2
			,35	3	2,4	7	7,3
			,40	3	28,5	8	8,3
			,45	3	54,5	9	9,4
			,50	4	20,6	10	10,4
			,55	4	46,6	11	11,5
			,60	5	12,7	12	12,5
			,65	5	38,7	13	13,5
			,70	6	4,8	14	14,6
			,75	6	30,8	15	15,6
			,80	6	56,9	16	16,7
			,85	7	22,9	17	17,7
			,90	7	49,0	18	18,8
			,95	8	15,1	19	19,8
			1,00	8	41,1	20	20,8
						21	21,9
						22	22,9
						23	24,0
						24	25,0
						25	26,1

N. B. The observations made by Mr. Green with Dollond's micrometer, particularly those concerning the difference of declination of Venus and the Sun's limbs, and the distances of Venus from the Sun's limb in lines parallel to the equator, will be better understood by consulting a paper intitled Directions for observing the Differences of Declination &c. with Dollond's Micrometer, by N. Maskelyne, Astronomer Royal, a copy of which was given to Mr. Green, before his departure from England; which will appear in this volume.

Observa-

Obfervations on the Dipping Needle.

Time when		Place where.	Dip of the North or South point	
			°	′
1768				
Sept.	13	In Funchal Bay, dip of N. end of needle	77	18
October	25	Croffing the line in long. 30° 18′ W. of Greenwich	26 to 28 N. point	
1769				
January	10	At fea in lat. 52° 54′ S. and long. 63° 10′ W.	63	S. point
	20	Good Succefs Bay in Straits Le Maire	68	51 Ditto
	24	On board the fhip at anchor in the above bay	65	00 Ditto
	30	At fea in lat. 60° 04′ S. long. 74° 10′ W.	65	17 Ditto
March	3	Ditto, ditto, 36 49 S. ditto 111 54 W.	65	52 Ditto
	13	Ditto, ditto, 30 46 S. ditto 125 28 W.	64	25 Ditto
April	5	Ditto, ditto, 18 25 S. ditto 140 51 W.	30	00 Ditto

N. B. Each of the above Obfervations is the mean of ten, twelve, or more; with the face of the inftrument turned alternately Eaft and Weft: thofe made at fea are a little dubious on account of the motion of the fhip; but, by means of a fwinging table we had made to fet the compafs upon, we could, in a tolerable fmooth fea, be certain of the dip to a degree, or at the moft two, by taking the mean of a great number of trials.

			°	′		
1769						
May	30	George's Ifland	29	26	South point Face	Eaft
			29	40		Weft
			30	10		Eaft
			31	45		Weft
			31	00		Eaft
			31	00		Weft
			30	51		Eaft
			30	40		Weft
			30	18		Eaft
			30	25		Weft
			30	21		Eaft
			30	40		Weft
			31	00		Eaft
			30	42		Weft
			30	45		Eaft
			31	30		Weft
			31	50		Eaft
			30	16		Weft
			30	16		Eaft
			30	48		Weft
			31	45		Eaft
		Mean	30	43		

1770 January 19, in Queen Charlotte's Sound, lat. 41° 5′ S. long. 184 35 W. The dip of the South end of the needle 54° 40′.

Obfervations on the Dipping Needle.

	° ′		
May 1 Botany Bay Lat. 34 00 S. Long. 208 37 W.	67 20	South Point Face	Eaft
	66 40		Weft
	66 55		Eaft
	67 08		Weft
Mean	67 01		

July 18 Endeavour River Lat. 15 26 S. Long. 214 48 W.	36 54	South Point Face	Weft
	40		Eaft
	06		Weft
	35 14		Eaft
	35 14		Weft
	36 00		Eaft
	00		Weft
Mean	36 0		

James Cook.

Obfervations on the Tides at K. Georges Ifland.

Day of the month	Time of low water	Time of high water	Height of tides	The moon paffes the merid. above the horizon	The moon paffes the merid. below the horizon
1769	H M	H M	Inches	H M	H M
June 4	6 0 A. M.	Noon	9½	0 36 P. M.	
5	6 0 A. M	Noon	8½	1 40 P. M.	
6	7 30 A. M.			2 40 P. M.	
7	8 0 A. M.	1 45 P. M.	9	3 34 P. M.	
8	8 41 A. M.	2 10 P. M.	8½	4 25 P. M.	
9	8 42 A. M.	3 15 P. M.	9¼	5 12 P. M.	
10		4 0 P. M.	8½	5 57 P. M.	
12		5 0 P. M.		7 23 P. M.	
14	7 41 A. M.				8 29 A. M.
17	8 40 A. M.	1 16 P. M.	9½		10 50 A. M.
18	8 50 A. M.	11 40 A.M.	10		11 38 A. M.
19	8 10 A. M.	0 15 P. M.	9		0 27 A. M.
20	8 0 A. M. water ftands at 5 inches on mark	0 30 P. M. water ftands at 14 inches on mark	9		1 26 P. M.

Obfervations

Obſervations on the Tides at K. George's Iſland.

Day of the month	Time of low water	Time of high water	Height of tides	The moon paſſes the merid. above the horizon	The moon paſſes the merid. below the horizon
1769	H M	H M	Inches	H M	H M
June 21	7 30 A. M. water at 5 in.				2 4 P. M.
22	8 30 A. M. water at 5 in				2 50 P. M.
25	10 15 A.M.				5 8 P. M.
27		7 0 A. M.	12½	6 19 A. M.	
28		8 0 A. M.	13	7 11 A. M.	
July 2	6 30 A. M.	Noon		11 13 A. M.	
3	6 30 A. M. water at 3 inches	0 30 P. M. water at 13 inches	10	0 15 P. M.	
4	7 15 A. M. water at 3 inches	1 0 P. M water at 13 inches	10	1 13 P. M.	
5	7 30 A. M. water at 3 inches			2 7 P. M.	

Hence the mean height of the ſides is about 10 inches, and the greateſt height ſcarcely exceeds one foot, in the middle of this wide-extended ocean; which falls far ſhort of what might have been expected from phyſical principles. The cauſe of this remarkable differenee deſerves farther inquiry. The time of high water alſo appears to precede the moon's paſſing the meridian by 45 minutes at a medium, and the time of low water to precede the ſame, by $6^h\ 31'$. But the mean difference of high and low water, ſhould be $6^h\ 12'$, which ſubtracted from $6^h\ 31'$, leaves $0^h\ 19'$, by which the time of high water ſhould precede the moon's paſſing the meridian; the mean of this and $0^h\ 45'$ is 32', by which the time of high water precedes the moon's paſſing the meridian, by a medium of all the obſervations. The times of high and and low water ſeem to be ſubject to great irregularity on particular days; no doubt owing to the ſmall riſe of the water, and the ſmallneſs of its force in conſequence, which renders it more liable to be diſturbed by the action of the winds and other cauſes: part of the irregularity may be attributed to the difficulty of obſerving the time of the flood or ebb, with any degree of certainty. N. M.

N. B. The iſland here named King George's Iſland, is called by the natives Ota-heite, by which name it will henceforth be called, the name of K. George's Iſland having been given before to another iſland in lat. 14 S. diſcovered by Commodore Byron.

*** Mr. Green having died at ſea in the paſſage home from Batavia, all the aſtronomical and other obſervations were partly arranged by Capt. Cook, and partly by the Aſtronomer Royal, from the original manuſcripts, and calculated by the latter.

Read

Read November 21, 1771.

XLIV. *Variation of the Compaſs, as obſerved on board the* Endeavour *Bark, in a Voyage round the World. Communicated by Lieut.* James Cook, *Commander of the ſaid Bark.*

N. B. THE day of the month is noted according to the nautical account, which therefore in all the obſervations noted P. M. is one day forwarder than the civil account. The latitude *in* is deduced from the laſt preceding meridian altitude of the ſun; and the longitude *in* is corrected by the laſt obſervations of the diſtances of the moon from the ſun and ſtars.

Time		Lat. in North	Long. in Weſt (Weſt from Greenwich)	Variation: How found	Variation: Means (° ′)	Remarks
1768						
Auguſt	8	Engliſh Channel			23 0W.	From the Downs to Madeira
September	5	C. Fineſtre S. by ½ W. 6 leagues		pr. Azimuths	18 42	
	6	42 1	9 50W.	pr. Azimuths	21 40	
	7	40 29	10 11	pr. Azimuths	21 4	
	9	37 4	11 34	pr. Azimuths	19 50	
P. M.	10	36 46	11 58	pr. Azimuths	21 19	
A. M.	10	35 40	13 4	pr. Amplitudes	20 39	
P. M.	11	34 58	13 50	pr. Azimuths & Amplitudes	18 32	
A. M.	11	34 20	14 20	pr. Azim. & pr. Amplitudes	17 27	
		Funchal iſland of Madeira 32 33 33	16 49		15 30	Theſe are taken from Dr. Heberden, F. R. S. who reſided upon the iſland.
	20	Funchal iſland of Madeira N. 76 E. diſt. 19 leag.		pr. Azimuths	16 30	
	22	Salvages S. diſt. 9 or 10 leag.		pr. Azimuths	17 50	

Time

Time	Lat. in	Lon. in	Variation: How found	Variation: Means	Remarks
1768 September	North ° ′	West		° ′	
23	29 40	15 30		17 30 W. 17 00 17 15	Found by taking the ☉'s Azim. at equal Altitudes, before and afternoon.
P. M. D°.			pr. Amplitudes	18 30	
A. M.	29 7	15 50	pr. Azimuths	16 30	Paſſage to Rio de Janeiro.
24 P. M.	Pico Teneriff N. 18 E. Diſtance 140 miles		pr. Azimuths pr. Amplitudes	16 43 15 46	
24 A. M.	26 50	17 12	Ditto	14 58	
25 P. M.	25 20	18 50	pr. Amplitudes	15 1	
28 A.M.	20 56	20 40	pr. Sev. Azim.	12 46	
28 P.M.	19 33	20 50	Ditto	12 43	
29 P. M.	18 38	21 0	pr. Azimuths	12 33	
October					
1 P. M.	15 40	22 0	Ditto	10 37	
1 A.M.	14 35	22 8	Ditto	10 0	
	by another Compaſs		Ditto	11 40	
3	12 24	22 22	Amplitudes	8 49	
5 P. M.	11 53	22 0	pr. Azimuths pr. Amp. 5 59	6 10	
6 A. M.	9 45	22 20	pr. S. Az. 6 21	8 52	Towards Rio Janeiro.
8 P. M.	9 42	22 19	Ditto	9 0	
8 A.M.	8 46	22 4	Ditto 8 12 7 47	8 0	
9 P. M.	8 12	22 4	Ditto 8 23 8 20	8 21½	
9 A.M.	8 6	22 13	pr. Amplitudes 7 48	7 48	
10 P. M.	7 48	22 13	pr. Sev. Azim.	8 39	
13 P. M.	7 13	22 33	Ditto	8 54	
16 P. M.	6 50	23 46	Ditto	8 40	
21	3 4	26 30	Ditto	4 2	
22 A. M.	2 0	27 55	Amplitudes 3 13 pr. Azimuths 3 21	3 17	
25 P. M.	0 55	28 55	pr. Am. 2 16 pr. Azimuths Ditto 2 33	2 24½	

Time

Time		Lat. in	Lon. in	Variation: How found	Variation: Means	Remarks
1768 October		° ′ South	° ′ West		° ′	
27	A. M.	2 3	31 0	Ditto	2 48 W.	
29	P. M.	3 59	32 30	Ditto	2 25	
30	P. M.	5 46	32 48	pr. Sev. Azim.	1 31	
31	P. M.	7 30	33 4	Ditto	0 15	
November						
1	P. M.	9 22	33 16	Ditto	0 58	
	A. M.			Ditto	0 18	
2	P. M.	10 3	33 0	Ditto	0 34 E.	
3	P. M.	12 27	33 0	Ditto	0 47	
4	P. M.	15 25	33 40	Ditto	1 23	
7	P. M.	18 30	36 10	Ditto	4 41	
8	P. M.	20 4	37 18	Ditto	5 26	In foundings off the coaft of Brazil.
	A. M.	21 16	37 50	Ditto	7 52	
12	P. M.	Cape Forio W.N.W. dift. 12 leagues			6 40	
13	P. M.	Entrance Rio de Janeiro W. N. W. dift. 5 leagues			7 34	
December						
12	P. M.	25 44	41 4	pr. Amplitudes	8 40	Paffage to Terra dell Fuego.
	A. M.	26 0	41 20	pr. Sev. Azim.	8 23	
13	P. M.	26 34	41 33	Ditto	8 23	
16	P. M.	30 20	41 49	Ditto	9 36	
18	P. M.	32 30	42 48	Ditto	11 3	
19	P. M.	32 54	43 38	Amplitudes	11 3	
20	P. M.	34 34	45 38	pr. Sev. Azim.	13 44	
22	P. M.	36 50	48 32	Azim. 15 1 Amp. 15 1	15 1	
	A. M	37 8	49 1	pr. Amplitude	16 1	
23	P. M.	37 8	49 0	pr. Az. 15 24 Amp. 16 5	15 45	
	A. M.	36 46	49 2	pr. Amplitude	15 30	
29	P. M.	41 40	56 25	pr. D°. 16 12 pr. Azimuth 16 32	16 22	In foundings off the coaft of South America.
31	P. M.	42 40	60 25	pr. D°. 18 44 pr. Amplitude 18 22	18 36	

Time

Time	Lat. in	Lon. in	Variation: How found	Variation: Mean	Remarks
1769 January	° ′ South	° ′ West		° ′	
5 P. M.	48 42	60 51	pr. Am. 20 0; Amp. 20 9	20 4½ E.	
9 P. M.	51 30	63 30	pr. Several Az.	22 24	
10 P. M.	52 40	65 20	Ditto	21 57	
11 A. M.	54 0	67 30	Ditto	23 30	
	10 leag. from Terra del Feugo				
21 A. M.	Strait le Mare		Ditto	24 9	
22 A. M.	56 7	65 45	per Several Az.	25 4	
24	55 40	Near fome ifle on the coaft of Terra del Feugo E. of C. Horne.	ditto	21 0	Here the variation feems to be affected by the land, as thefe obfervations were well made.
25 A. M.	55 40	C. Horne S.W. 3 W. 8 leag.	pr. Amp.	21 16	Paffage from Cape Horne to Ota-hitee.
28 P. M.	57 0	69 0	pr. everal Az.	22 0	Mean refult of many azim. the fea calm and fmooth.
30 P. M.	60 10	74 26	Ditto	27 9	
February					
1 P. M.	59 23	76 45	Ditto	24 53	
3 P. M.	58 30	80 58	Ditto	24 4	
13 P. M.	49 13	89 36	Ditto	17 0	
15 P. M.	48 56	91 27	Ditto	12 0	
15 A. M.	48 10	92 0	Ditto	11 0	
21 A. M.	44 39	103 0	Ditto	6 30	
23 P. M.	39 43	105 52	Ditto	5 34	
27 A. M.	39 43	110 26	pr. Amp. 2 17; pr. Azim. 2 24	2 20½	
March					
4 P. M.	36 49	111 54	Ditto 2 6; Amp. 2 46	2 26	
8 P. M.	37 8	116 8	pr. Several Az.	3 13	Paffage from Cape Horne to Otahitee
9 P. M.	37 24	117 41	Ditto	4 41	
10 P. M.	35 30	119 30	Ditto	1 42	
11 Ditto	34 0	121 0	Ditto	4 12	
12 Ditto	32 40	123 0	Ditto	4 23	
13 P. M.	31 20	124 40	Amplitudes	3 20	
13 A. M.	30 56	125 20	pr. Azimuths	3 0	

Time		Lat. in	Lon. in	Variation How found	Variation Means	Remarks
1769 March		° ′	° ′		° ′	
		South				
15	P. M.	30 30	126 0	pr. Azimuths	3 45 E.	
	A. M.	29 36	126 50	Ditto	3 22	
16	P. M.	29 32	126 48	pr. 21 Azimuths	1 30	
	A. M.	29 28	127 4	Ditto	2 18	
17	P. M.	29 10	127 16	Ditto	3 27	
19	A. M.	27 40	129 20	Ditto	3 14	
21	A. M.	25 21	129 28	Amplit. 3 0 pr. Azim. 3 43	3 21½	
22	Ditto	25 21	129 32	Azimuth	3 10	
28	P. M.	21 14	127 38	Amplitudes	3 56	
29	P. M.	20 29	127 44	pr. Several Az.	2 27	
31	P. M.	19 30	129 10	Ditto	2 25	
April						
1	P. M.	19 7	131 40	Ditto	2 32	
4	P. M.	18 46	138 0	Ditto	2 54	
	A. M.	18 36	139 10	Ditto	2 54	
5	P. M.	18 36	139 40	Amplitude	3 30	
8	P. M.	17 48	143 50	Several Azim.	6 32	
9	P. M.	17 36	145 30	Ditto	4 54	
10	P. M.	17 42	146 16	Ditto	5 41	
11	P. M.	18 0	147 59	Ditto	6 30	
August		17 29	149 30	Meridian line	4 45½	On ſhore at Fort Venus by four of Dr. Knight's compaſſes, which appeared to be all good.
10	A. M.	17 15	151 41	pr. Several Az.	5 50	
13	Ditto	21 20	151 15	Ditto	5 40	From the Society Iſles to New Zeland.
15	P. M.	22 8	150 55	Amplitude	5 37	
	A. M.	23 37	150 37	Ditto	6 7	
18	P. M.	26 10	149 46	Ditto	8 8	
	A. M.	26 30	Ditto	Ditto	7 58	
23	P. M.	30 43	148 0	Azimuths	7 30	
24	A. M.	32 40	147 14	Ditto	7 18	
27	Ditto	33 8	147 25	Ditto	6 40	
30	Ditto	38 3	147 6	Ditto	7 9	
September						
5	P. M.	38 29	145 32	Ditto	7 0	
13	Ditto	33 0	153 0	Ditto	8 8	

Time	Lat. in	Lon. in	Variation: How found	Variation: Means	Remarks
1769 September	° ′ South	° ′		° ′	
19 P. M.	29 0	159 42	pr. Amplitudes	8 36 E.	
19 A. M.	29 0	159 25	pr. Sev. Azim.	8 29	
25. P. M.	33 30	163 40	Ditto	10 48	
October					
3 P. M.	36 50	173 46	Ditto	13 22	
4 P. M.	37 6	174 46	Ditto	12 48	
6 P. M.	38 33	179 0	Ditto 12 50 / Amplit. 12 48	12 59	
6 A. M.	39 0	180 0	1 Ditto	14 2	
7 P. M.	39 11	180 30	Azim. 15 4 / Ampl. 15 5	15 4½	In ſight of the Eaſt coaſt of New Zeland.
15 P. M.	39 37	182 30	Azimuths	14 10	Eaſt coaſt of New Zeland.
17 A. M.	40 0	182 0	Ditto	10 22	
November					
9	36 48	184 12	Several Azim.	11 9	On ſhore in Mercury Bay, N. W. coaſt.
25	35 50	185 15	Ditto	12 40	
26	35 15	Ditto	Amplitudes	13 10	
28	35 0	185 30	pr. Azimuths	11 45	Off the Northern parts of New Zeland.
December					
8	34 42	185 30	pr. Amplitude	12 51	
10 P. M.	34 40	186 15	Azim. 12 40 / Ampl. 12 40	12 40	
11 P. M.	34 40	186 45	Amplitude	12 20	
25 A. M.	34 0	188 0	Several Azim.	11 25	
1770 January					
6 A. M.	35 8	188 0	Ditto	12 26	
12 P. M.	38 12	185 3	Ditto	15 0	Weſt coaſt.
12 A. M.	39 0	Ditto	Ditto	14 15	
14 P. M.	39 40	Ditto	pr. Amplitudes	13 0	
15 A. M.	40 30	186 0	Azimuths	13 5	
February					
11 A. M.	41 0	183 0	pr. Amplitudes	14 0	
12 P. M.	41 26	184 0	Ditto	14 0	
14 Ditto	42 8	184 15	Azim. 15 8 / Amp. 15 0	15 4	

Time	Lat. in	Lon. in	Variation How found	Variation Means	Remarks
1770 February	° ' South	° '		° '	
17 A. M.	44 0	186 30	Ditto 14 32 Ditto 14 16	14 24 E.	East coast.
18 Ditto	45 0	186 15	pr. Amplitudes	15 36	
March					
1 P. M.	47 34	187 30	pr. Azimuths	16 34	
4 A. M.	46 30	189 0	Ditto	16 16	
7 P. M.	46 54	191 0	Amplitude	15 10	
7 A. M.	47 0	Ditto	pr. Azimuths	15 56	
9 P. M.	47 12	191 30	pr. Amplitudes	16 29	
15 P. M.	45 0	192 30	Ditto	15 2	West coast.
16 Ditto	44 27	191 15	pr. Azimuths	13 48	
25 A. M.	40 30	186 0	pr. Amplitudes	12 20	
April					
7 P. M.	37 15	196 40	Several Azim.	13 50	Between New Zeland and New Holland.
8 P. M.	37 40	197 40	Ditto	13 56	
10 A. M.	38 45	202 23	Ampl. 11 25 Azim. 11 20	11 22½	
13 P. M.	39 15	203 40	Amplitude Azimuth	12 25	
13 A. M.	39 23	204 0	Ditto	12 29	
14 P. M.	39 24	204 4	Amplitude	11 28	
A. M.	39 23	204 15	Azimuth	11 30	
20 A. M.	37 0	210 0	Ditto	10 40	Coast of New South Wales on the East coast of New Holland.
21 P. M.	36 35	210 0	Ditto	10 42	
22 A. M.	35 35	209 23	Ditto	9 50	
24 Ditto	35 35	209 0	Ditto	7 41	
25 P. M.	35 18	209 11	Ditto	9 15	
25 A. M.	34 0	208 50	Ampl. 9 36 Azim. 9 7	9 21½	
26 P. M.	34 18	208 49	Several Azim.	8 48	
May					
7 P. M.	33 50	208 37	Ditto	8 0	
7 A. M.	33 22	208 20	D°. with needles	7 56	Mean of all the Compasses.
8 P. M.	Ditto	Ditto	Azimuths	8 25	
10	33 13	207 20	Ditto	8 0	
11 A. M.	32 40	206 36	pr. d°. and Amp.	9 10	
18	26 20	206 46	pr. Azimuths	8 40	
19 P. M.	25 34	206 45	Ditto	8 36	
19 A. M.	25 24	206 38	pr. Amplitudes pr. Azimuths	8 21½	

Time	Lat. in	Lon. in	Variation		Remarks
			How found	Means	
1770 May	° ′	° ′		° ′	
	South				
20 P. M.	25 12		pr. Azimuth	8 45 E.	
22 P. M.	24 34	307 40	pr. Amplitude	8 3	
22 A. M.	24 25	208 0	Azimuths	7 50	
26 P. M.	23 24	209 10	Ditto	7 28	
June					
3 A. M.	20 20	211 20	Ditto	6 57	
5 A. M.	19 18	212 30	Amplitude	5 35	
6 A. M.	19 4	212 50	Ditto	5 31	
7 P. M.	19 0	213 15	Amplitude	5 25	
7 A. M.	18 52	213 35	Ditto	5 0	
9 A. M.	16 59	213 55	Ditto	4 53	
Auguſt					
23 P. M.	10 36	219 8	Amplitude	2 54	Strait between New Holland and New Guinea.
27 A. M.	10 3	220 45	Ditto	2 30	
28 P. M.	9 51	221 5	Ditto	2 51	
September					
2 P. M.	7 39	222 40	Ditto	2 34	Coaſt of N. Guinea.
3 Ditto	7 2	222 30	Ditto	2 4	Between N. Guinea and Timor.
4 P. M.	6 18	222 10	Ditto	2 30	
9 P. M.	9 36	232 13	Azim. 0 12 Amp. 0 5	0 8¾ W.	
10 A. M.	9 50	232 57	Ditto	0 2	Eaſt coaſt of Timor.
13 Ditto	9 40	235 45	Amp. 1 10 Azim. 1 27	1 18½	
14 Ditto	9 50	235 45	Amp. and Azim.	2 4	
17 P. M.	10 8	236 0	Amplitude	1 49	Iſland of Savre.
24 Ditto	11 10	241 30	pr. Several Azim.	2 44	South coaſt of Java.
27 Ditto	11 10	246 50	Ditto	3 10	
1771 January					
27 P. M.	10	256 32	Ditto	2 51	
February					
3 P. M.	15 52	264 36	Ditto	2 56	
7 P. M.	18 34	274 50	Ditto	3 24	
14 P. M.	21 56	287 10	Ditto	4 10	

Time

Time		Lat. in	Lon. in	Variation: How found	Variation: Means	Remarks
1771 February		° ′ South	° ′ West		° ′	
17	P. M.	23 20	297 18	pr. Several Azim.	10 20 W.	
20	P. M.	24 57	304 31	Ditto	12 15	
23	P. M.	26 59	311 28	pr. Amplitude	17 30	Java head to the Cape of Good Hope.
25	P. M.	27 55	314 0	pr. Azimuth	24 20	
	A. M.	28 40	316 0	pr. Amplitude	24 0	
26	P. M.	28 54	316 30	Azimuths	26 10	
March						
4	P. M.	31 8	326 30	Ditto	25 35	
8	A. M.	34 20	333 0	Amp. 28 30 Azim. 28 8	28 19	
10	P. M.	35 40	337 10	pr. Amplitude	24 0	
12	P. M.	34 54	339 0	Ditto	22 30	
		Table Bay, Cape of Good Hope.			20 30	By several observations.
April						
23	P. M.	27 12	349 30	Amplitude	17 40	From the Cape of Good Hope to England.
	A. M.	26 34	250 32	Azimuth	18 37	
24	P. M.	26 12	350 46	Amplitude	17 —	
	A. M.	25 26	351 16	Amp. and Azim.	17 30	
28	P. M.	19 50	357 0	Azimuth	14 0	
29	Ditto	18 30	359 6	Ditto	13 53	
May						
5	A. M.	15 25	7 0	Ditto	13 10	
7	Ditto	12 30	9 45	Ditto	12 50	
9	P. M.	10 24	12 0	Ditto and Amp.	11 00	
13	A. M.	3 18	17 46	Azimuth	10 00	
		North				
19	Ditto	4 20	21 51	Amplitude	7 40	
23	Ditto	7 40	26 0	Azimuth	9 40	
26	Ditto	10 38	29 22	Ditto	6 30	
31	Ditto	18 25	35 30	Ditto	5 9	
June						
1	A. M.	20 0	36 30		6 40	
2	Ditto	21 4	38 0	Ditto	5 4	
4	Ditto	23 30	40 0	Ditto	4 30	
6	Ditto	25 40	43 18	Ampl. 5 5 Azim. 6 4	5 34½	

Time		Lat. in	Lon. in	Variation How found	Variation Means	Remarks
1771 June		° ′ North	° ′ West		° ′	
7	A. M.	27 22	43 43	20 Azimuths	5 20 W.	
8	Ditto	28 30	43 42	Ditto	5 24	
9	Ditto	29 51	44 9	Amp. 7 3 / Azim. 7 30	7 17	
10	P. M.	30 26	44 15	Ampl. 9 18 / Azim. 9 —	9 9	
12	P. M.	32 16	45 14	Amplitude	7 0	
	A. M.	32 40	45 0	Azimuths	6 55	
13	P. M.	33 16	44 53	Azim. and Amp.	8 23	
13	A. M.	33 53	44 25	Ampl. 8 15 / Azim. 8 14	8 14½	
14	P. M.	34 36	Ditto	Amplitude	8 14.	
17	Ditto	38 26	40 20	Azimuth	9 1	
18	P. M.	39 12	39 0	Azim. 14 13 / Ampl. 14 18	14 15½	
18	A. M.	39 22	38 0	Amplitude	14 24	
30	P. M.	43 55	17 16	Azimuth	18 30	
30	A. M.	44 30	16 18	Ditto	19 30	
July						
1	P. M.	44 40	15 44	Azimuths	23 0	
1	A. M.	44 50	16 10	Ditto	22 50	
3	P. M.	45 —	13 0	Ditto	20 36	
4	P. M.	45 30	10 45	15 Azimuths / 2 Amplitudes	21 25½	
4	A. M.	45 20	9 37	20 Amplitudes / 12 Azimuths	21 10	
7	P. M.	45 45	8 38	Amplitude	22 30	

Extract.

Extract from Capt. Cook's Journal.

Nov. 9 1771 At 8 A. M. Mr. Green and I went on ſhore, to obſerve the Tranſit of Mercury, which came on at 7^h 20′ 58″ apparent time, and was obſerved by Mr. Green alone; I at this time was taking the Sun's altitude in order to aſcertain the time.

		h	′	″	
Mr. Green	Internal contact	12	8	58	P. M.
	External contact	—	9	55	
C. Cook	Internal contact	—	8	45	
	External contact	—	9	48	

Lat. obſerved at noon 36° 48′ 28″, the mean of this and yeſterday obſervations gives 36° 48′ $5\frac{1}{2}$″ South, the latitude of the place of obſervation. The variation of the compaſs was found to be 11° 9′ Eaſt.

*** Theſe obſervations were made by the help of a Graham's watch with a ſecond hand; corrected by obſerved altitudes of the Sun.

XLV. *Transitus* Veneris & Mercurii *in eorum Exitu è Disco* Solis, 4*to Mensis* Junii & 10*mo* Novembris, 1769, *observatus. Communicated by Capt.* James Cook.

Read Nov. 21, 1771.

INSTRUMENTA quæ obſervandis hiſce phænomenis utriſque deſtinaveram, erant ſequentia: horologium nempè Aſtronomicum à Domino *Shelton* fabrefactum, ſed pendulo & anchorâ Domini *Graham* inſtructum: quadrans porrò aſtronomicus, cujus radius eſt $2\frac{1}{2}$ pedum, à Domino *Dollond* confectus: teleſcopium denique Gregorianum trium pedum, micrometro objectivo ſive heliometro inſtructum, atque ab eodem confectum, quod maximè excellit.

Quod ad pendulum ſupradictum, quo ferè per ſeptennium uſus ſum, motus ejus oſcillatorius, qui motum ſolis medium exactè ſequitur & indicat, quemque per plures altitudines æquales ſolis & fixarum, ante & poſt habitam obſervationem denuò examinavi, in metiendis minutis ſecundis vere ἰσόχρονος eſt, et in eo ita uniformiter procedit, ut ab uno ſolſtitio ad alterum vix tribus ſecundis ab illo recedat.

 Quadrantem

Quadrantem porrò probè examinatum, qui in ejusmodi quoque observationibus magni momenti est, & ab astronomis vulgò applicari solet, huic ipsæ observationi eum in finem destinaveram, ut in horologio differentiam temporis elapsi inter mutuos limborum solis & planetarum contactus ad fila serica, in communi vitrorum foco tensa, habere atque eo ipso differentiam altitudinis & azimuthi inter solis & planetarum centra determinare possem; observato enim tempore elapso inter contactum limbi solis & planetæ ad unum idemque filum, concluditur inde differentia altitudinis eorum, si nempe fuerit filum horizontale; differentia verò azimuthi inter utrumque eodem modo concludetur, si filum fuerit verticale. Denique ex observata ejusmodi differentia altitudinis & azimuthi inter centra solis & planetæ, ipsam quoque differentiam longitudinis & latitudinis inter utrumque, tempore observationis habitæ, deducere licet.

Sic equidem methodo prædictâ situm & motum Veneris ac Mercurii, non tam in disco Solis, quam in ipsa eorum orbita, ope quadrantis determinare studui. Illud ipsum verò præprimis ope micrometri objectivi efficiendum putavi, ut nimirum distantias planetæ à proximo limbo Solis in ejus disco successive metiri possem; à quo elemento cætera pendent: quam quidem methodum præ cæteris exactam præferendam duco.

Interim eventus spem frustravit; vota & conamina in utroque casu irrita fuere, neque unicam ejusmodi sive Veneris sive Mercurii observationem in eorum transitu obtinere potui; cœlum namque minus serenum jam ab exoriente Sole ad horam usque octavam (in transitu Veneris) mutabilem valde faciem præ-

bebat,

bebat, et aliquando nubibus prorſus obductum erat: ſed faciem brevi poſt mutavit, Soliſque imaginem, quæ antea vix per aliquot minuta diſtinctè videri poterat, nunc ad finem uſque phænomeni pleno quaſi jubare conſpiciendam dedit.

Cœlo ita favente, exitum Veneris è diſco Solis, teleſcopio ſupra dicto, clarè, diſtinctè, nec minus accuratè, hunc in modum obſervari:

		h	′	″
1769 4 Junii ante meridiem	Contactus interior ſive initium exitus videbatur	8	30	13
	Contactus exterior ſive exitus totalis viſus	8	48	31

Eadem cœli facie ſerenâ, eodemque ſucceſſu exitum quoque Mercurii obſervare licuit: nempe

10 Novembris ante meridiem	Contactus interior ſive initium exitus videbatur	7	33	32
	Contactus exterior ſive exitus totalis viſus	7	35	11

Tempora in utroque tranſitu hic notata, ubique tempus verum indicant. Cæterùm plura de his videantur in *Actis Societatis Scientiarum Batavæ*, quæ *Harlemi* floret [a].

Situm geographicum obſervatorii non ita pridem exſtructi, ſive ejus *latitudinem* & *longitudinem*, exactè (ni fallor) determinavi.

Prior ſive *Elevatio Poli Antarctici* in illo, per plures altitudines Solis meridianas & quidem ſolſtitiales, nec non fixarum aliquot, determinata quam proximè accedit ad 6° 10′.

Poſterior autem, ſive *differentia meridianorum* hoc inter & Regium obſervatorium Pariſienſe, per aliquot immerſiones & emerſiones primi ſatellitis Jovis, per eclipſes binas lunares, atque per occultationem fixæ à

[a] Vol. XII. A. 1770. p. 123.

 Luna,

Luna, determinata, inventa fuit 104° 30′, ſive in tempore 6h 58′.

Obſervationes eum in finem habitas, nondum quidem in Europam tranſmiſi, brevi autem & infra paucos dies ad Societatem Batavam, volente Deo, tranſmittentur.

Bataviæ, in Obſervatorio recens exſtructo, 25 Decembris 1770.

Johan Maurits Mohr,

V.D.M. Senior, & Societatis Scientiarum Batavo-Harlemenſis Membrum.

Received

Received December 5, 1771.

XLVI. Kepler's *Method of computing the Moon's Parallaxes in Solar Eclipſes, demonſtrated and extended to all Degrees of the Moon's Latitude, as alſo to the aſſigning the Moon's correſpondent apparent Diameter, together with a conciſe Application of this Form of Calculation to thoſe Eclipſes; by the late* H. Pemberton, *M.D. F.R.S. Communicated by* Matthew Raper, *Eſq; F.R.S.*

Read Dec. 5, 1771.

THE calculation of ſolar eclipſes having been generally reputed a very operoſe proceſs, from the repeated computations required of the moon's parallaxes by their continually varying during the progreſs of the eclipſe, I was once induced to conſider Kepler's compendium for performing this, delivered in his Rudolphine tables, of which he had given a demonſtration in his treatiſe entitled *Aſtronomiæ pars optica.* But this demonſtration is perplext, and the method itſelf wants correction, to render it perfect. Both theſe defects I endeavoured to ſupply by the following propoſitions, by by which may be determined with ſufficient exactneſs the moon's apparent latitude, not only in eclipſes, but in all diſtances of the moon from the ecliptic.

But to theſe propoſitions I ſhall here premiſe the method I have generally uſed for computing the nonageſime degree, and its diſtance from the zenith; this form of calculation not being encumbered with any diverſity from the difference of caſes.

L E M M A.

To find the nonageſime, or 90th degree of the ecliptic from the horizon, and its diſtance from the zenith, the latitude of the place, and the point of the equinoctial on the meridian being given.

In TAB. XV. Fig. 1. 2. 3. 4. let A B be the equinoctial, A C the ecliptic, D the zenith, D E the meridian, and D F perpendicular to the ecliptic, whereby F is the nonageſime degree, and DF the diſtance of that point from the zenith. Then from D E, the latitude of the place, and A E the diſtance of the meridian from Aries, the arch of the ecliptic A F, and the perpendicular D F may be thus found.

Let I be the pole of the equinoctial, and H the pole of the ecliptic. Then A E augmented by 90° is the meaſure of the angle D I H, or of its complement to four right angles: And the ſquare of the radius is to the rectangle under the ſines D I, I H, as the ſquare of the ſine of half the angle D I H, or of half its complement to four right angles, to the rectangle under the radius, and half the exceſs of the coſine of the difference between D I and I H, above the coſine of D H, or the ſine of D F.

4 In

Fig. 1.

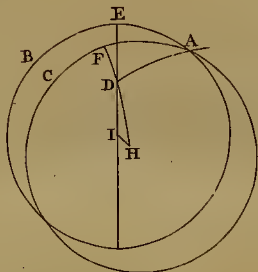


Fig. 2.

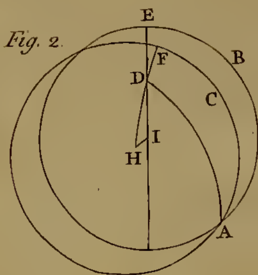


Fig. 3.

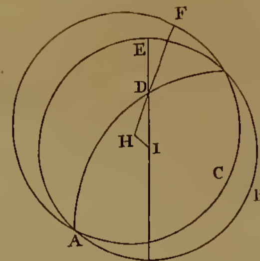


Fig. 4.

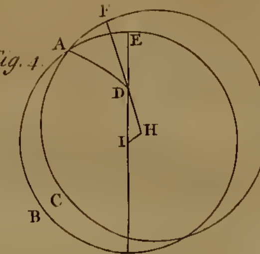


Fig. 5.

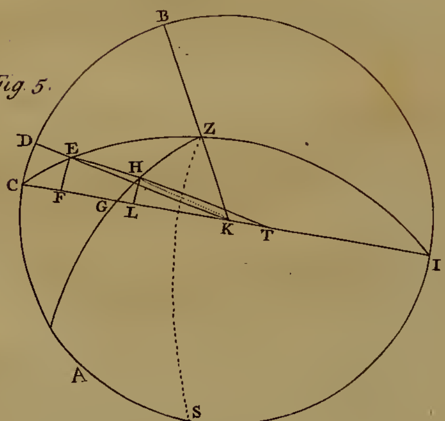


Fig. 6.

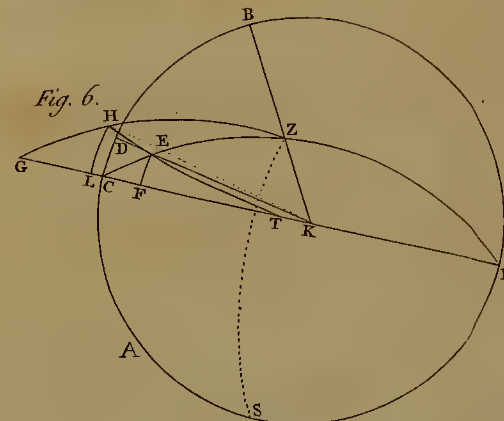


Fig. 7.

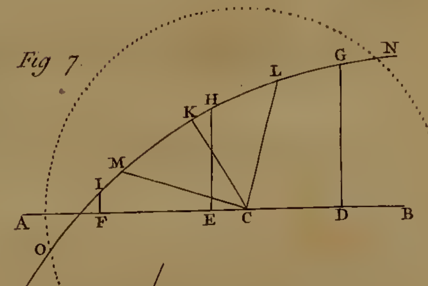
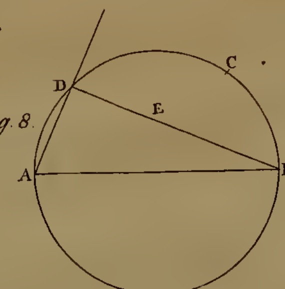


Fig. 8.



In the next place, the arch A D being drawn, in the rectangular triangle AED, the radius is to the cofine of D E, as the cofine of A E to the cofine of AD; and in the rectangular triangle AFD, the cofine of DF is to the radius, as the cofine of AD to the cofine of AF; therefore, by equality, the cofine of D F is to the cofine of D E as the cofine of A E to the cofine of AF [a], the arch AF counted according to the order of the figns being to be taken fimilar in fpecies to AE: For when AE is lefs than a quadrant (as in fig. 1.), AF will be lefs than a quadrant; and when AE fhall be greater than 1, 2, or 3 quadrants, AF counted according to the order of the figns, fhall exceed the fame number of quadrants. For, fince DE and DF are each lefs than quadrants, when A E in the triangle DEA is alfo lefs than a quadrant, the hypothenufe A D is lefs than a quadrant, when in the triangle DFA the legs DF and FA are fimilar, that is, FA will be lefs than a quadrant; (as in fig. 1.) but if AE is greater than a quadrant; (as in fig. 2.) that is, diffimilar to DE, the hypothenufe DA will be greater than a quadrant, and the arches DF, FA likewife diffimilar, and AF greater than a quadrant; alfo in fig. 3 and 4, the arches AE, AF counted from A, in confequence, will be the complements to a circle of the arches AE, AF in the triangles A DE, ADF.

For an example, let the cafe be taken in Dr. Halley's aftronomical tables, where an occultation of the moon with a fixed ftar is propofed to be computed, the lati-

[a] The fame may be concluded from the f. HD being to f. TD as f. HID to f. IHD.

tude

tude of the place being 65°. 50′. 50″, and the point of the equinoctial culminating 25°. 36′. 24″, from the firſt point of Aries.

This caſe relates to fig. 1. and the computation will ſtand thus,

For the diſtance of the nonageſime degree from the zenith,

	° ′ ″	
Diſtance of E in conſequnce from A, the equinoctial point	25.36.24	
Add	90. 0. 0	
Gives the angle HID	115.36.24	L. Sines
Half HID	57.48.12	9.92749
		9.92749
HI, the obliquity of the ecliptic uſed by Dr. Halley	23.29. 0	9.60041
ID, the complement of the latitude	24. 9.10	0.61190
Natural number correſponding	0.11676	9.06729
Its double, to be deducted from	0.23352	Sum, thrice
the nat. coſine of ID∽IH (0°.40′.10″.)	0.99993	rad. deducted
leaves the nat. coſine of HD (39. 58. 0.)	0.76641	
Therefore DF is 50. 2. 0.		
For the arch AF		
Coſine of DF, or ſine of HD (co. arith.)		0.19223
Coſine of the latitude, or ſine of ID		9.61191
Coſine of AE		9.95510
Coſine of the long. of the 90th deg. (54°.56′.24″.)		9.75924

The arch HD might have been computed by the verſed ſine of the angle HID. But I chuſe the method above; very few logarithmic tables having the logarithmic verſed ſines. Sir J. Moore, and Sherwin have given indeed ſuch tables, but they are imperfect, extending only to a quadrant.

Moreover, if a table of natural ſines is not at hand, the arch AD may be found logarithmically thus [a].

Take half the ſum of the four firſt logarithms in the preceding computation of HD, *viz.*	19.53364
Deduct the ſine of half DI ∽ IH	7.76675
the remainder	11.76689
This remainder ſought in the table of logarithmic tangents gives the correſpondent ſine	9.99994
This ſine deducted from the firſt number leaves the ſine of half HD, that is, 19°. 59′. 0″.	9.53370

PROPOSITION I.

In fig. 5, 6. Let BCA be the ecliptic, E the moon appearing in the ecliptic in C, from the place of the earth whoſe zenith is Z; B the nonageſime degree, the arch ZB being perpendicular to the ecliptic, ZEC the circle of altitude; ED the moon's latitude, the arch DE being perpendicular to the ecliptic CB; and DC the parallax in longitude: then DE is to the horizontal parallax, as the ſine of ZB, the diſtance of the nonageſime degree from the zenith, or the altitude of the pole of the ecliptic, to the radius; alſo DC is to the moon's horizontal parallax as ſ. BC × coſ. ZB to the ſquare of the radius.

The arch CE is to the moon's horizontal parallax as ſ. ZC to radius, and DE is to CE as ſ. ZB to ſ. ZC; whence by equality DE is to the horizontal parallax as ſ. ZB to the radius.

[a] See Philoſophical Tranſactions, Vol. LI. P. II. p. 927, 928.

Again, ſ. ZB is to radius as the tangent of ZB to the ſecant of ZB; therefore DE is to the horizontal parallax, as t. of ZB to ſec. ZB: but DC is to DE as ſ. BC to t. ZB; whence by equality DC is to the horizontal parallax as ſ. BC to the ſec. ZB, or as ſ. BC × coſ. ZB to the ſquare of the radius.

COROLLARY.

If the point S be taken 90 degrees from the apparent place of the moon, and the arch SZ be drawn, in the ſpherical triangle SBZ, the cſ. ZB × cſ. BCS, that is, cſ. ZB × ſ. BC is equal to rad. × cſ. ZS: therefore DC is to the horizontal parallax as cſ. ZS, or the ſine of the diſtance of S from the horizon to the radius. And if the point S is taken in conſequence of the moon, it will be above the horizon, when the nonageſime degree is alſo in conſequence of the moon; otherwiſe below.

PROPOSITION II.

Let G be the apparent place of the moon out of the ecliptic in the circle of latitude CK, K being the pole of the ecliptic, and H her true place. Then EF, the diſtance of the moon from the circle of her apparent latitude, when ſhe is ſeen in the ecliptic, is equal to HL, her diſtance from the circle of her apparent latitude, when her apparent place is G.

If a great circle EHT be drawn through E and H, till it meet the circle of the apparent latitude in T, the four great circles CZ, GZ, CT, ET, interſecting each other, the ratio of ſ. ZC to ſ. CE is compounded

2 of

of the ratio of ſ. ZG to ſ. GH and of the ratio of ſ. SHT to ſ. ET [a]. But CE and GH being the parallaxes in altitude at the reſpective diſtances from the zenith ZC, ZG, ſ. ZC is to ſ. CE as ſ. ZG to ſ. GH: therefore the ſine of HT will be equal to the ſine of ET, and the arches HT, ET together make a ſemicircle: whence ET is equal to HL.

COROLLARY.

The arch KH being drawn, the parallax in longitude, when the moon is in H, will be to HL as rad. to ſ. KH, or the coſine of the latitude; and EF, or its equal HL, to CD as ſ. KE to the radius. Therefore the moon's parallax in longitude, when in H, is to the parallax in longitude, when ſhe appears in the ecliptic, as the ſine of KE to the ſine of KH, that is, as the coſine of the latitude, when the moon appears in the ecliptic, to the coſine of her latitude in H.

PROPOSITION III.

When the moon appears out of the ecliptic, if her latitude is ſmall, the difference of the moon's latitude, when the moon appears in the ecliptic under the ſame apparent longitude, if both latitudes are on the ſame ſide of the ecliptic, otherwiſe their ſum, will be to the moon's apparent latitude, nearly as the ſine of the moon's diſtance from the zenith, when appearing in

[a] Ptolem. Almag. L. i. c. 12. Menel. Spheric. L. iii. pr. 1.

the ecliptic under the ſame apparent longitude, to the ſine of the correſponding apparent diſtance.

Fig. 6. When the moon appears out of the ecliptic in G, the four great circles CZ, GZ, CT, ET, interſecting each other as before, the ratio of ſ. CZ to ſ. ZE will be compounded of the ratio of ſ. CG to ſ. EH, or of CG to EH in theſe ſmall arches, and of the ratio of ſ. HT to ſ. GT, which laſt ratio, when the latitude is ſmall, and HT near a quadrant, is nearly the ratio of equality. Now in the triangle EKH the arch EH exceeds the difference of KE and KH, that is, the difference of the latitudes, when both the latitudes are on the ſame ſide of the ecliptic, and their ſum, when the latitudes are on the oppoſite ſides. But here the exceſs will be inconſiderable. Therefore if an arch X be taken, whoſe ſine ſhall be to the ſine of the difference, or ſum of the latitudes, as ſ. ZC to ſ. ZE, X ſhall be nearly equal to CG, the apparent latitude in G.

COROLLARIES.

1. If the arches DE, BZ be continued to K, the pole of the ecliptic, the four great circles CB, CZ, DK, BK, will interſect each other, and ſ. BD will be to the ſine of BC in the ratio compounded of the ratio of ſ. ZE to ſ. ZC, and of ſ. DK to ſ. EK, the leaſt of which ratios, the arch DE being ſmall, and DK a quadrant, is nearly the ratio of equality: therefore ſ. BD is to ſ. BC nearly as ſ. ZE to ſ. ZC; ſo that ſ. BD will be to ſ. BC nearly as the difference of the moon's true latitude, when ſhe appears in G from her latitude DE, wherewith ſhe would appear

in the ecliptic, if the points H and E are both on the ſame ſide of the ecliptic, or as the ſum of thoſe latitudes, when H and E are on different ſides of the ecliptic, to the moon's viſible latitude.

2. The moon's apparent diameter, is to her horizontal diameter, as the ſine of her apparent diſtance from the zenith to the ſine of her true diſtance. Therefore, when the moon is in C, her apparent diameter is to her horizontal diameter as ſ. ZC to ſ. ZE, and ſ. ZC being to ſ. ZE nearly as ſ. BC to ſ. BD; the moon's apparent diameter in C will be to her horizontal diameter nearly as ſ. BC to ſ. BD.

Again, the ratio of ſ. CG to ſ. EH is compounded of the ratio of ſ. ZG to ſ. ZH, and of the ratio of ſ. CT to ſ. ET; and is alſo compounded of the ratio of ſ. ZC to ſ. ZE, and of the ratio of ſ. GT to ſ. TH; but the ſine of ET is equal to the ſine of TH; the arches ET and TH compoſing a ſemi-circle; alſo the ſine of CT there differs little from the ſine of GT; therefore ſ. ZG is to ſ. ZH, that is, the moon's apparent diameter, when in G, to her horizontal diameter, nearly as ſ. ZC to ſ. ZE, or nearly as ſ. BC to ſ. BD.

3. In all latitudes of the moon, EH will not greatly exceed the difference, or ſum of the moon's latitude in H, and the latitude wherewith ſhe would appear in the ecliptic. Therefore the ratio of ſ. ZC to ſ. ZE being compounded of the ratio of ſ. CG to ſ. EH, and of the ratio of ſ. HT to ſ. GT, if X be taken, that its ſine be to the ſine of the difference or ſum of the latitudes, as ſ. ZC to ZE, ſ. X will be nearly to ſ. CG as ſ. HT to ſ. GT. Hence the difference of ſ. X and ſ. GC will be to ſ. CG nearly as the differ-

ence

ence of ſ. HT and ſ. GT to ſ. GT, HT not ſenſibly differing from TL. Now FT and TL together make a ſemi-circle, and the ſum of FG and GL is twice the difference of TL from a quadrant, and the difference between FG and GL equal to twice the difference of TG from a quadrant, alſo the difference between the ſines of TL and TG is equal to the difference of the verſed ſines of the differences of thoſe arches from quadrants; and moreover the rectangle under the ſines of two arches is equal to the rectangle under half the radius, and the difference of the verſed ſines of the ſum and difference of thoſe arches: therefore the difference of the ſines of X and of CG will be to the ſine of CG as the rectangle under the ſine of half FG and the ſine of half GL to the rectangle under half the radius and the ſine of GT, and in theſe ſmall arches the difference of X and CG will be to CG nearly as the rectangle under the ſines of FG and GL to the rectangle under twice the radius and the ſine of GT, or even twice the ſquare of the radius, this difference being to be added to X, when the moon's apparent latitude, and that by which ſhe would appear in the ecliptic, are on the ſame ſide of the ecliptic, otherwiſe deducted from X for the final correction of the apparent latitude. And in the laſt place this correction will be always ſo ſmall in quantity, that in computing it CF may be ſafely ſubſtituted for GL.

4. Moreover, the exceſs of the moon's apparent diameter, when ſeen in G, above her apparent diameter in C, bears a leſs proportion to her horizontal diameter than the rectangle under the ſine of her horizontal

horizontal parallax, and twice the ſine of half the apparent latitude CG to the ſquare of the radius.

The ſine of CE is to the ſine of ZC as the ſine of the horizontal parallax to the radius; and CE, the difference of ZC and ZE, being very ſmall, the difference of the ſines of thoſe arches may be eſteemed to bear to the ſine of CE, the ratio of the coſine of ZC to the radius; and thus the difference of the ſines of ZC and ZE, will be to the ſine of ZC as the rectangle under the ſine of the horizontal parallax and the coſine of ZC to the ſquare of the radius. And in like manner the difference of the ſines of ZG and ZH, will be to the ſine of ZG, as the rectangle under the ſine of the horizontal parallax and the coſine of ZG to the ſquare of the radius. But ſ. ZE is to ſ. ZC as the moon's horizontal diameter to her apparent diameter in C, and ſ. ZH to ſ. ZG as the moon's horizontal diameter to her apparent diameter in G. Therefore the difference of the apparent diameter in G from the apparent diameter in C, is to the horizontal diameter, as the rectangle under the ſine of the horizontal parallax, and the difference of the coſines of ZC and ZG, to the ſquare of the radius. But in the triangle CZG, the difference of ZC and ZG is leſs than the third ſide CG: therefore the chord of the difference of thoſe arches, and much more the difference of their coſines, will be leſs than the chord of CG, or twice the ſine of half CG. Hence the ratio of the augmentation of the apparent diameter in G to the apparent diameter in C, will be leſs than the rectangle under the ſine of the horizontal parallax and twice the ſine of half CG, the apparent latitude, to the ſquare of the radius.

More-

More accurately, the chord of the difference of ZC and ZG being to the difference of their cofines, as the radius to the cofine of half their fum, the difference of the moon's apparent diameters in C and G may be confidered as nearly bearing to the horizontal diameter, the ratio of the parallelopipedon, whofe altitude is the fine of the horizontal parallax, and bafe the rectangle under the chord of CG and the cofine of ZC, to the cube of the radius; the cofine of ZC being to the cofine of ZB, the diftance of the nonagefime degree from the zenith, as the cofine of BC, the apparent diftance of the moon from the nonagefime degree to the radius. But this difference can never be any fenfible quantity.

5. When the moon is in the longitude of the nonagefime degree, the parallax in longitude ceafes, and the apparent latitude is the difference of the moon's apparent diftance from the zenith, and the diftance of the nonagefime degree from the fame.

But now fince DC is to the horizontal parallax as the rectangle under the fine of BC, and the cofine of ZB to the fquare of the radius; if an arch be taken to the horizontal parallax as f. BD × cf. ZB to the fquare of the radius, this arch will differ but little from the parallax in longitude, and is ufed by Kepler as fuch; however, it ought to be corrected by adding it to BD, and taking an arch to this in the proportion of the fine of BD thus augmented to the fine fimply of BD; and this laft arch will be equal to the parallax in longitude without fenfible error.

Again, DE taken to the horizontal parallax as the fine of ZB to the radius, is confidered by Kepler as the

the moon's parallax of latitude in eclipſes; but this being deducted or added as the caſe requires gives EH, which being augmented in the proportion of the ſine of BD+DC to the ſine of BD, gives truly the apparent latitude without ſenſible error, when the latitude is ſmall: But, when greater, requires to be corrected by adding together the logarithmic ſine of the latitude now found, the ſine of EH and the logarithm of DE, the ſum of which is the double of the correction required.

In the laſt place the moon's horizontal diameter augmented in the proportion of the ſine of BC to the ſine of BD exhibits the moon's apaprent diameter.

And here the calculation will proceed thus:

In the example above choſen for computing the nonageſime degree,

The moon's longitude is given from ♈ 62°. 2.′38″

The longitude of the nonageſime degree was found above to be 54°.56′.24″

Therefore BD = 7. 6. 14	
its ſine	9.09226
BZ, as found above, 50°.2′.0″. its coſine	9.80777
The horizontal parallax in ſeconds	3.52387
° ′ ″ 4.25	2.42390
This added to BD gives 7.10.39	
Its ſine	9.09673
Diff. from the firſt ſine	447
This added to the log. of 4′. 25″, gives the log. of 4′. 28″, for the moon's parallax in longitude, ſuch as is derived from the parallax in altitude by the parallactic angle,	2.42837

 Again,

Again,		
The ſine of ZB. 50°. 2′. 0″.		9.88447
Horizontal parallax 55′.41″.=3341 ſeconds		3.52387
Their ſum, rejecting the radius, gives DE=42′.40″		3.40834 A
The moon's latitude 4°. 50′.18″		
Their ſum, (EH) the lat. being ſouth, 5°. 32′.58″		
Its ſine		8.98546 B
From the preceding calculation		447
For the apparent latitude, were the moon's lat. ſmall	5°. 6′.25½″	8.98993 C
But the moon's latitude being here great, the numbers markt A, B, C, being added together, give twice the correction.	0°. 0′.24″	1.38373
Its half	0°. 0′.12″	
This deducted from N° C, the moon's latitude being ſouth, gives for the apparent lat.	5°.36′.13½″	
Laſtly,		
From the moon's horizontal parallax her horizontal diameter is	0°.30′.37½″ or 1837½″	3.26423
The number from the firſt calculation		447
The moon's apparent diameter 1856½″ or 30′.56½″		3.26870

NOW in ſolar eclipſes the moſt regular method of treating them would be to conſider the viſible way of the moon from the ſun, as a line of continued curvature, which it really is; and as it differs not greatly from a ſtreight line, an arch of a circle may ſafely be uſed for it. But to form a computation in the ſphere upon this principle would require a proceſs ſomewhat intricate; but all the particulars uſually inquired into in ſolar eclipſes may readily be aſſigned graphically with ſcale and compaſs after this manner.

Firſt, find the time nearly of the conjunction of the luminaries, without being ſolicitous to inveſtigate the time

time with exactness. To this point of time assign in some crude manner the moon's parallax in longitude, by which a time may easily be assumed, not very distant from the visible conjunction. This may very commodiously be performed instrumentally by the proposition, with which I shall conclude this paper. To this point of time compute the place of the sun and moon, also for an hour before and after, or rather for such an interval of time as may include the whole ~~eclipse,~~ and not too much exceed, of which an estimate may easily be made by the forementioned proposition here subjoined. But all these places of the luminaries may be deduced from the calculation for finding the true conjunction, by means of the horary motions. In the next place, to each of these points of time compute the distance from the zenith and the place in the ecliptic of the nonagesime degree. Then from each position of the nonagesime degree, compute by the method described, the moon's parallax in longitude, her apparent latitude, and apparent diameter.

Fig. 7. After this, assuming upon any straight line, as AB, the point C for the sun, from thence lay down for the three points of the ecliptic, for which the preceding computations were made, the three distances CD, CE, CF, which shall be the measures in seconds, taken from a scale of equal parts sufficiently large, of the distances of the moon from the sun in each, compounded with their respective parallaxes in longitude, so as to represent the respective apparent distances of the moon from the sun in longitude. Upon these points erect the perpendiculars DG, EH, FI, for the moon's correspondent apparent latitudes, and describe through these three points the arch of a circle, as representing the visible way of the moon from the sun during the eclipse.

 Then

Then if from C the line CK be drawn from the centre of this circle, K will be the place of the moon at the greateſt obſcuration. The beſt method for aſſigning this point K is to deſcribe the arch of a circle to the center C with any interval, whereby it may cut the arch GHI, as in N and O; for the point K biſects the intercepted arch NKO. Again, if CL, CM be applied from C to the arch IHG, each equal to the ſum of the ſemidiameter of the ſun, and apparent ſemidiameter of the moon, L will be the place of the moon's center at the beginning, and M the ſame at the end of the eclipſe.

In the laſt place, for finding the time, when the moon ſhall be in each of the points L, K, M, meaſure the chords of the arches HG, HL, HM, HI, as not ſenſibly differing from the arches themſelves. Then A denoting HL or HM, and B the ſum of GH and HI, the time ſought for the greater chord may be conſidered equal to $\overline{\frac{A}{\frac{1}{2}B} - \overline{\frac{A}{\frac{1}{2}B}}\Big|^{q} \times \frac{GH \backsim HI}{B}}\Big|$ $\times$ the time of the moon's paſſing from G to H, or from H to I. The time for the leſſer chord will be $\overline{\frac{A}{\frac{1}{2}B} \times \overline{\frac{A}{\frac{1}{2}B}}\Big|^{q} \times \frac{GH \backsim HI}{B}}\Big|$ $\times$ the time above named; and in the laſt place, the time of the moon's paſſage between H and K equal to $\overline{\frac{HK}{\frac{1}{2}B} \mp \overline{\frac{HK}{\frac{1}{2}B}}\Big|^{q} \times \frac{GH \backsim HI}{B}}\Big|$ $\times$ the time ſpecified.

This calculation I deduced from Sir Iſaac Newton's Differential Method; and in the laſt caſe — or + $\overline{\frac{HK}{\frac{1}{2}B}}\Big|^{q}$ $\times$, &c. is to be taken, as K ſhall fall within the greater or leſſer of the arches GH, HI: but for the moſt part the term may be wholly omitted.

If

If this method be applied to the occultation of a ſtar, the diſtances CD, CE, CF muſt be the parallaxes in longitude computed according to the firſt of the preceding propoſitions united with the reſpective diſtances of the moon from the ſtar in longitude, contracted in the proportion of the coſines of the moon's latitudes, or at leaſt of the ſtar's latitude to the radius. Alſo the moon's apparent latitudes muſt, for the moſt part, be corrected by the third corollary of the third propoſition, and the apparent diameters, if the correction could amount to any ſenſible quantity, by the 4th corollary.

THE propoſition, I made mention of above for eſtimating the diſtance of the true conjunction from the viſible, is this. Fig. 8. In any circle, whoſe diameter is AB, let the arch AC meaſure twice the complement of the declination of any point in the ecliptic CD; in like manner meaſure twice the complement of the latitude, and AD, BD being drawn, let DE be the verſed ſine of the diſtance in right aſcenſion, of that point of the ecliptic from the meridian taken to a radius equal to the perpendicular let fall from C upon the chord AD; then BE will be the ſine of the diſtance of the point aſſumed in the ecliptic from the horizon, to a radius equal to the diameter of the circle.

Therefore, if the diameter of the circle be the meaſure, upon any ſcale of equal parts, of the moon's horizontal parallax, and the point taken in the ecliptic be 90° diſtant from the moon's apparent longitude; the right aſcenſion and declination of this point being firſt taken from tables of right aſcenſion and decli-

nation.

nation, BE, found as above, will be the meafure of the parallax in longitude, as affigned in the Coroll. to Prop. I. and if the point affumed in the ecliptic be 90° diftant from the moon's true place, BE will approach near enough to that parallax for the purpofe intended.

After the fame manner may the parallax in longitude be found for any other time affumed. Alfo if the arch AC be taken equal to twice the complement of the obliquity of the ecliptic, that is, BC equal to twice that obliquity, BE will be nearly equal to the parallax in latitude, provided DE be taken equal to the verfed fine, to the like fcale, as before, of the complement of the right afcenfion, of the point of the ecliptic on the meridian. And thus may be found the fitteft interval of time for the three calculations of the parallaxes, &c. I have above propofed in general an hour; but in great eclipfes it would be beft to affume this interval fomething greater, and in fmall eclipfes lefs.

Moreover thefe conftructions may be performed with very little trouble, any fmall fector being fufficient for the purpofe.

XLV. *Of*

XLVII. *Of Logarithms, by the late* William Jones, *Esq; F. R. S. Communicated by* John Robertſon, *Lib. R. S.*

Read Dec. 5, 1771.

THE following paper on the nature and conſtruction of Logarithms, was communicated to me many years ſince, by that eminent Mathematician the late William Jones, Eſq. The familiar manner in which he explains their nature, and the great art with which he obtains the modes of computation, not being exceeded, if equaled, by any writer on this ſubject, may claim a place in the Philoſophical Tranſactions, to be preſerved among the multitude of excellent papers, of which that moſt invaluable work is a ſafe repoſitory.

Of Logarithms.

1. Any number may be expreſſed by ſome ſingle power of the ſame radical number.

For every number whatever is placed ſomewhere in a ſcale of the ſeveral powers of ſome radical number r, whoſe indices are $m-1$, $m-2$, $m-3$, &c. where not only the numbers r^m, r^{m-1}, r^{m-2}, &c. are expreſſed; but alſo any intermediate number x is repreſented by r, with a proper index z.

 The

The index z is called the Logarithm of the number x.

2. Hence, to find the logarithm z of any number x, is only to find what power of the radical number r, in that ſcale, is equal to the number x; or to find the index z of the power, in the equation $x = r^z$.

3. The properties of logarithms are the ſame with the indices of powers; that is, the ſum or difference of the logarithms of two numbers, is the logarithm of the product or quotient of thoſe numbers.

And therefore, n times the logarithm of any number, is the logarithm of the nth power of that number.

4. The relation of any number x, and its logarithm z being given; To find the relation of their leaſt ſynchronal variation $\dot{x}$ and $\dot{z}$

Put $1+n$ for r, the radical number of any ſcale, and

$$q = \frac{n}{1+n}.$$

Let $a = q + \frac{1}{2}q^2 + \frac{1}{3}q^3 + \frac{1}{4}q^4$, &c.; $f = \frac{1}{a}$.

Then $f\dot{x} = x\dot{z}$ ſhews the relation required.

For $x = r^z = \overline{1+r}|^z$.

Now, let x and z flow ſo that x becomes $x+\dot{x}$, at the ſame time as z ſhall become $z+\dot{z}$,

Then $x + \dot{x} = \overline{1+n}|^{z+\dot{z}} = \overline{1+n}|^z \times \overline{1+n}|^{\dot{z}}$

$$= x \times \overline{1 + \dot{z}q + \tfrac{1}{2}\dot{z}q^2 + \tfrac{1}{3}\dot{z}q^3 + \tfrac{1}{4}\dot{z}q^4, \&c.}$$

Therefore $\dot{x} = x\dot{z} \times \overline{q + \frac{1}{2}q^2 + \frac{1}{3}q^3 + \frac{1}{4}q^4, \&c.}$

$= x\dot{z}a = x\dot{z} \times \frac{1}{f}$. Conſequently $f\dot{x} = x\dot{z}$.

5. If $1+n=r=10$, as in the common logarithms of Briggs's form,

Then a will be found to be 2,302585092994, &c.

And $f =$ . . . . . 0,434294481903 25, &c.

If $a=1=f$, the form will be that of Napier's logarithms.

6. Let B, $\grave{\text{B}}$, be the logs of the numbers x, $\grave{x}$, in the form $f=\frac{1}{a}$,

And N, $\grave{\text{N}}$, the logs of the same numbers, in the form $\varphi=\frac{1}{\alpha}$.

Then $\text{B}\varphi=\text{N}f$; $\text{B}a=\text{N}\alpha$; $\text{B}\grave{\text{N}}=\text{N}\grave{\text{B}}$; $\grave{\text{B}}\varphi=\grave{\text{N}}f$.

For $\grave{\text{B}}:\grave{\text{N}}::(f\times\frac{\grave{x}}{x}:\varphi\times\frac{\grave{x}}{x}::)f:\varphi::\text{B}:\text{N}::\frac{1}{a}:\frac{1}{\alpha}::\grave{\text{B}}:\grave{\text{N}}$.

If $x=10$; $\text{B}=1$; $a=2{,}30258$, &c.; or $f=0{,}43429$, &c.; $\alpha=\varphi=1$.

Then $\text{N}=\text{B}\times\frac{a}{\alpha}=2{,}30258$, &c.

$$\grave{\text{N}}=\grave{\text{B}}\times\frac{\text{N}}{\text{B}}=2{,}30258,\ \&c.\times\grave{\text{B}}.$$

$$\grave{\text{B}}=\frac{f}{\varphi}\times\grave{\text{N}}=0{,}43429,\ \&c.\times\grave{\text{N}}.$$

 7. Putting

7. Putting $x = q \pm v$; $N = \frac{v}{q}$.

Then $z =$ log. of x, or the log. of $q \pm v$, will be

$$= \pm \frac{v}{q} - \frac{v^2}{2q^2} \pm \frac{v^3}{3q^3} - \frac{v^4}{4q^4} \pm \frac{v^5}{5q^5} - \frac{v^6}{6q^6}, \text{ \&c. } \times f;$$

$$= \pm N - \tfrac{1}{2}N^2 \pm \tfrac{1}{3}N^3 - \tfrac{1}{4}N^4 \pm \tfrac{1}{5}N^5 - \tfrac{1}{6}N^6, \text{\&c.} \times f.$$

For $\dot{z} = \dot{L}, x = \dot{L}, \overline{q \pm v} = f \times \frac{\dot{x}}{x} = f \times \frac{\pm \dot{v}}{q \pm v}$,

$$= \pm \frac{\dot{v}}{q} - \frac{\dot{v} v}{q^2} \pm \frac{\dot{v} v^2}{q^3} - \frac{\dot{v} v^3}{q^4} \pm \frac{\dot{v} v^4}{q^5} - \frac{\dot{v} v^5}{q^6}, \text{ \&c. } \times f.$$

8. In three quantities p, q, r, increasing by equal differences, the logarithm of any one of them being given, the logarithms of the other two are also given.

For, let $v = q - p = r - q$; $N = \frac{v}{q} = \frac{q-p}{q} = \frac{r-q}{q}$;

P, Q, R, the logarithms of p, q, r.

I. $L = \frac{q}{p} = (L, \frac{q}{q-v} =) Q - P = f \times \overline{N + \tfrac{1}{2}N^2 + \tfrac{1}{3}N^3 + \tfrac{1}{4}N^4 + \tfrac{1}{5}N^5, \text{\&c.}} = fV$.

For $\dot{L}$, $\frac{q}{q-v} = f \times \frac{\dot{v}}{q-v}$.

II. $L, \frac{r}{q} = (L, \frac{q+v}{q} =) R - Q = f \times \overline{N - \tfrac{1}{2}N^2 + \tfrac{1}{3}N^3 - \tfrac{1}{4}N^4 + \tfrac{1}{5}N^5, \text{\&c.}} = fX$.

For $\dot{L}$, $\frac{q+v}{q} = f \times \frac{\dot{v}}{q+v}$.

III. $L, \frac{r}{p} = 2f \times \overline{V + X} = R - P = 2f \times \overline{N + \tfrac{1}{3}N^3 + \tfrac{1}{5}N^5 + \tfrac{1}{7}N^7 + \tfrac{1}{9}N^9, \text{\&c.}} = 2fZ$.

Where $N = \left(\frac{v}{q} =\right) \frac{r-p}{r+p}$.

Or, $L, \frac{r}{p} = L, \frac{q+v}{q-v} = R - P = 2fZ$.

For $\dot{L}$, $\frac{q+v}{q-v} = 2f \times \frac{q\dot{v}}{qq - vv}$.

9. Hence,

9, Hence, in two quantities, r the greater, p the less.

Putting $N = \frac{r-p}{r+p}$; $A = 2fN$; $B = AN^2$; $C = BN^2$;
$D = CN^2$, &c.

And $S = A + \frac{1}{3}B + \frac{1}{5}C + \frac{1}{7}D$, &c.

Then L, $\frac{r}{p} = S$; Or $R - P = S$.

Or, putting $N = \frac{r-p}{r+p}$; $A = fN$, &c.

Then L, $\frac{r}{p} = 2S$.

Where $p = 1$; $N = \frac{r-1}{r+1}$; let $A = 2fN$, &c.

Then L, $r = S$.

Or, in this case, putting $N = \frac{r-1}{r+1} = A$; $B = AN^2$, &c.

Then L, $r = 2fS$.

Where $p = 1$, and $f = 1$; $N = \frac{r-1}{r+1}$; let $A = 2N$, &c.

Then L, $r = S$.

10. In

10. In three quantities p, q, r, increasing by equal differences, the logarithms of any two of them being given, the logarithm of the third is also given.

I. For L, $\frac{qq}{pr} = 2f \times \overline{V - X} = 2Q - \overline{P + R}$

$= 2f \times \frac{1}{2}N^2 + \frac{1}{4}N^4 + \frac{1}{6}N^6 + \frac{1}{8}N^8$, &c. $= 2fY$.

Where $N = \frac{r-p}{r+p}$.

Or L, $\frac{qq}{pr} = $ L, $\frac{qq}{qq - vv} = 2fY = 2Q - \overline{P + R}$.

Because $\dot{L}$, $\frac{qq}{qq - vv} = 2f \times \frac{\dot{v}v}{qq - vv}$.

II. Putting $N = \frac{qq - pr}{qq + pr} = \frac{vv}{qq + rp} = $ (where $v = 1$) $\frac{1}{qq + pr}$;

$A = fN$; $B = AN^2$, &c.

Then L, $\frac{qq}{pr} = 2S = 2Q - \overline{R + P}$; Or $Q - \frac{R + P}{2} = S$.

For since $vv = qq - pr = 1$; put qq for r; pr for q.

Then $r - p = qq - pr = vv = 1$; $r + p = qq + pr$.

III. Putting $N = \frac{v}{q} = A$, &c. $a = \frac{1}{2}$.

$b = \frac{1}{4} - \frac{1}{3}a$.

$c = \frac{1}{6} - \frac{1}{5}a - \frac{1}{3}b$,

$d = \frac{1}{8} - \frac{1}{7}a - \frac{1}{5}b - \frac{1}{3}c$.

$e = \frac{1}{10} - \frac{1}{9}a - \frac{1}{7}b - \frac{1}{5}c - \frac{1}{3}d$, &c.

And $M = aA + bB + cC + dD$, &c.; $\Sigma = \frac{1}{2}\overline{R + P}$; $\Delta = \frac{1}{2}\overline{R - P}$.

Then $Q = \Sigma + \Delta M$.

For $Q - P = fV = \alpha$; $R - P = 2fZ = 2\Delta$;

but $\left(\frac{\alpha}{\Delta} = 1 + M = \frac{Q - P}{\frac{1}{2}R - \frac{1}{2}P}\right.$; Therefore, &c.

I

11. Any

11. Any numbers p, q, r, &c. and as many ratios a, b, c, &c. compoſed of them, the difference of whoſe terms is 1; as alſo the logarithms A, B, C, &c. of thoſe ratios, being given: To find the logarithms P, Q, R, &c. of thoſe numbers, where the form is 1.

For inſtance, if $p=2$, $q=3$, $r=5$,

$$a=\left(\tfrac{9}{8}=\right)\frac{3^2}{2^3};\ b=\left(\tfrac{16}{15}=\right)\frac{2^4}{3.5};\ c=\left(\tfrac{25}{24}=\right)\frac{5^2}{3.2^3}.$$

Now, the logs A, B, C, of theſe ratios, a, b, c, being found, the log. of either 2, 3, 5, or of any number compounded of them, may be found directly, by making each ſucceſſively equal to a^x, b^y, c^z.

Thus, for the log of $10=2.5$.

Let $a^x b^y c^z = \dfrac{3^{2x}}{2^{3x}} \times \dfrac{2^{4y}}{3^y.5^y} \times \dfrac{5^{2z}}{3^z.2^{3z}}$.

$$= 3^{2x}.2^{-3x} \times 2^{4y}.3^{-y}\ 5^{-y} \times 5^{2z}.3^{-z}.2^{3z} = 2.5.$$

Therefore $2^{4y-3x-3z-1} \times 3^{2x-y-z} \times 5^{2z-y-1} = 1$.

Conſequently $4y-3x-3z-1=0$; $2x-y-z=0$; $2z-y-1=0$

Therefore $x = 10$; $y= 13$; $z = 7$;

and $a^{10} \times b^{13} \times c^7 = (2 \times 5 =)\ 10$.

Therefore $10A + 13B + 7C =$ log. of 10, to the form 1.

Or, ſince $a = \dfrac{3^2}{2^3}$; $b = \dfrac{2^4}{3.5}$; $c = \dfrac{5^2}{3.2^3}$.

Therefore $A=2Q-3P$; $B=4P-Q-R$; $C=2R-Q-3P$.

Conſequently $\left.\begin{array}{l} P=3A+4B+2C=\text{log. of } 2 \\ Q=5A+6B+3C=\text{log. of } 3 \\ R=7A+9B+5C=\text{log. of } 5 \end{array}\right\}$ to the form 1.

Therefore $P+R=10A+13B+7C=$ log. of $(2 \times 5=)$ 10.

And fP, fQ, fR, are the logarithms of 2, 3, 3, reſpectively, in the ſcale of logarithms whoſe form is f.

 XLVIII. *An*

XLVIII. *An Inquiry into the Value of the ancient* Greek *and* Roman *Money: By* Matthew Raper, *Esq; F. R. S.*

INTRODUCTION.

Read Dec. 5, 1771.

THE firſt writers, who, after the revival of learning in Europe, made the Greek and Roman money an object of their inquiries, took great pains to collect and explain ſuch paſſages in antient authors as related to it; but very little to diſcover its true value. In ſo much, that ſome of them have ſuppoſed the Roman Aureus to have been heavier than the Greek Philippic (1); and others, that the Denarius was heaver than the Attic Drachm; but moſt of them agreed in this, that the two laſt mentioned coins were exactly equal. All which opinions are proved to be erroneous by the coins themſelves now in being.

(1) See Gronovius, de pecunia vetere, l. ii. c. 8.

Our

Our learned countryman John Greaves, was, I believe, the first who discovered that the Attic Drachm was heavier than the Denarius (2). He seems to have examined a greater number of Greek and Roman coins than any other writer on the subject. His balance turned with the 80th part of a grain (3); and his weights were correctly adjusted to the English standard (4), as appears from the comparison the Royal Society of London caused to be made, in the year 1742, of the Troy Ounce with that of Paris, which was found to agree precisely with what Greaves had so long before determined (5).

His care and diligence in weighing the coins, and his fidelity in reporting them, have never been doubted; but he is not always sufficiently explicit; as, where he says he had perused many hundred Denarii Consulares, and found the best of them to amount to 62 grains English (6); it is probable he found many such, for there are many of this weight and upwards in that noble repository the British Museum; but when he says in the same paragraph, that, weighing many Attic Tetradrachms, he found the best of them to be 268 grains, he may mean only one, for very few come up to that weight. Nor hath he given a particular description of this

(2) See the dedication of his Discourse of the Roman Foot and Denarius, printed in the year 1647, and reprinted, with other of his works, by Dr. Birch, in 1736. I quote the original edition, which contains 134 pages numbered after the dedication. That of Dr. Birch, begins at p. 181 (excluding the dedication), and ends at p. 356. (3) Ibid. (4) See his Discourse, P. 61.

(5) Philosophical Transactions, N° 465. (6) p. 61.

heavy

heavy Tetradrachm, but ſeems to think the weight of that coin was in all ages the ſame, which probably it was not.

He allows that ſilver is more liable to be overſized at the mint than gold (7); yet he determines the weight of the Attic Drachm from the Tetradrachm to be 67 grains (8), though no gold coin, he ever ſaw, comes up to it by a quarter of a grain in the Drachm (9).

He hath likewiſe made his Denarius above half a grain heavier than any he had peruſed, to agree with Villalpandus's weight of the Congius (1); which led him to ſuppoſe, that the Roman Aureus was juſt double the weight of the Denarius (2), contrary to the expreſs teſtimony of Pliny. And he hath not given a clear account of the Conſular Aureus.

In the year 1708, John Caſpar Eiſenſchmid, of Straſburg, publiſhed his book de ponderibus & menſuris veterum, &c. He is an accurate and a faithful writer, but wanted materials. He uſed Paris weights, which ſeem to have been correctly ſized to that ſtandard. Having ſeen no Roman gold older than the reign of Tiberius, which was not too imperfect to diſcover its original weight (3), and finding the moſt perfect Conſular Denarii to be very unequally ſized, he took a mean from a pretty large heap of ſuch as he thought unexceptionably perfect, rejecting ſome, which, though apparently ſo, were

(7) P. 103. (8) P. 66. (9) P. 72.
(1) Compare p. 94 and 120, with p. 61. (2) P. 103.
(3) Eiſenſchmid, p. 34.

very

very deficient in weight, and thence determined the weight of the Consular Denarius to be $74\frac{2}{7}$ Paris grains, equal to $60\frac{15}{16}$ Troy (4). But, as he hath not told us what number of pieces his large heap contained, nor the weight of the heaviest and lightest of them, his conclusion is not satisfactory.

Having no perfect Greek coins, either gold or silver, except one very ancient Attic Tetradrachm weighing 333 Paris grains, he derived the weight of the Attic Drachm from his Denarius, by a proportion between the Roman Pound and the Attic Talent, mentioned in the 38th book of Livy's History, which happened to agree with the weight of his ancient Tetradrachm, giving a Drachm of $83\frac{1}{4}$ Paris grains, equal to almost $68\frac{3}{10}$ Troy (5).

Neither he nor Greaves have taken notice of the Roman Scrupular gold coin, nor made such use of the Constantinopolitan Solidus, as might be expected, from the great number now remaining in the most perfect preservation, though the latter hath given the weights of 29 of them.

Greaves, very justly, observes, that, " gold coins " are not subject to be consumed by time and rust, " but only *ex intertrimento*; and therefore we may " the safelier give credit to them. And because " the difference, though but of a grain, is of some " consideration in gold, the masters of the mint use " to be more circumspect about them: whereas, in " silver coins, since it is hardly worth the pains to " stand precisely on the excess or defect of a grain, " there are few of these so exact, but either exceed

(4) P. 33. (5) P. 40 and 42.

" or

" or want in the very mint one or two grains, and
" ſometimes more (6)."

I found, the heavieſt of twenty new guineas, of the year 1768, freſh from the mint, to outweigh the lighteſt $1\frac{2}{5}$ grains. The didrachmal gold of Philip and Alexander is about 4 grains heavier than our guinea; and I never found the difference between any two of them, that appeared to be perfect and unworn, amount to two grains. The ſilver, likewiſe, of theſe two Princes is more correctly ſized, than any other ancient ſilver money I have ſeen.

The Roman Conſular Aureus is between 3 and 4 grains lighter than a guinea, and is not ſo correctly ſized as the Greek gold; but much more ſo than the Denarius, which is ſo unequal, that the Roman mint-maſters ſeem to have contented themſelves, with ſtriking a certain number of pieces out of the pound of ſilver, with very little regard to their equality. Therefore, as far as the diſcovery of the weight of the Roman pound depends on their coin, it muſt be obtained from the gold alone.

Eiſenſchmid ſuppoſes, that gold coins may have loſt a ſenſible part of their original weight, though no appearance of wear can be diſcovered on them, even with a glaſs (7). On the contrary, I have found guineas of George II, and Ann, whoſe wear, on the moſt prominent parts of the head, was viſible at the firſt glance of the naked eye, which were above ſtandard weight; therefore, where no appearance of wear, or other diminution, can be diſcovered

(6) Greaves, p. 103. (7) Eiſenſchmid, p. 34, 35.

on a coin, I ſee no reaſon to ſuppoſe it hath loſt any ſenſible part of its original weight.

In the following diſcouſe, I have collected the moſt authentic evidence I could find, of the weights of the Attic Drachm and the Roman Denarius; part of which I have taken from that very valuable publication of the Pembroke collection of coins. But, valuable as it is, it would have been more ſatiſfactory to the accurate peruſer, if the Noble Editor had diſtinguiſhed the degree of preſervation the ſeveral coins were in, and given the weights of the moſt perfect, nearer than to half a grain.

In the year 1759, by the favour of the learned and ingenious Dr. Gowin Knight, Principal Librarian of the Britiſh Muſeum, I weighed a conſiderable number of the moſt perfect Greek and Roman coins in that noble Repoſitory.

The ſcales I uſed were good workmanſhip, of the common conſtruction, made by Read; the beam 8 inches, and they turned freely with leſs than the 20th part of a grain. To avoid any error, I weighed each piece in both pans. My weights were moſt accurately ſized; and, upon comparing the Troy ounce I uſed, with that in the archives of the Royal Society, in an exquiſite balance of my late much eſteemed friend, Dr. Henry Pemberton, it was found to be $\frac{3}{8}$ of a grain heavier, which I have allowed for in the following diſcourſe.

This eſſay hath received very conſiderable additions from the ineſtimable treaſury of ancient coins, in the poſſeſſion of the learned Matthew Duane Eſq; who moſt obligingly aſſiſted me in taking the weights of ſuch as were for my purpoſe. And it

 was

was from the coins in this collection only, that I discovered the Eginean Talent to have been the money-ſtandard of Macedon, before Philip changed it for the Attic.

Dr. Hunter, likewiſe, very politely favoured me with the inſpection of his curious cabinet of ancient coins, ſome of which I ſhall have occaſion to mention in the following diſcourſe; as well as ſome brought from Greece, by my learned friend James Stuart, Eſq; who, it is hoped, will ſoon favour the Public with the ſecond volume of his Antiquities of Athens.

§ 1. *Of the Attic Drachm.*

THE Greek coins were not only money, but weights. Thus their Drachm was both a piece of money, and a weight; their Mina was 100 Drachms as a ſum, and the ſame number as a weight; and their Talent contained 60 Minas, or 6000 Drachms, both by weight and tale.

This way of reckoning 100 Drachms to the Mina, and 60 Minas to the Talent, was common to all Greece; and where the Drachm of one city differed from that of another, their reſpective Talents differed in the ſame proportion (8).

Of all the Greek cities and free ſtates, both in Europe and the leſſer Aſia, that of Athens was the moſt famous for the fineneſs of their ſilver, and the

(8) Pollux, L. IX. c. 6. § 86.

juſtneſs

juſtneſs of its weight (9): Xenophon tells us, that whitherſoever a man carried Attic ſilver, he would ſell it to advantage (1). And their money deſerves our more particular attention, both becauſe we have the moſt unexceptionable evidence of its ſtandard weight; and what little we know of the money of other Greek cities, is chiefly by compariſon with this.

The current coin of Athens, was the ſilver Drachm, which they divided into 6 Oboles, and ſtruck ſilver pieces of 1, 2, 3, 4, and 5 Oboles, of half an Obole, and a quarter of an Obole (2). Their larger coins above the Drachm were, the Didrachm, the Tridrachm (3), and the Tetradrachm; which laſt they called Stater, or the ſtandard.

It does not appear that they coined copper till the 26th year of the Peloponneſian war, when Callias was a ſecond time Archon (4). It was ſoon after publickly cried down; and the concluſion of the proclamation was to this effect, that, ſilver is the lawful

(9) See Ariſtophanes, Ranæ ver. 733. Polybius, in Excerpt. Leg. § 28. Δότωσαν δὲ Αἰτωλοὶ ἀργυρίου μὴ χείρονος Ἀττικοῦ κ. τ. λ. and § 35. Ἀργυρίου δὲ δότω Ἀντίοχος Ἀττικοῦ Ῥωμαίοις ἀρίστου. κ. τ. λ.

(1) Xenophon περὶ προσόδων. c. 3. Καὶ οἱ ἀργύριον ἐξάγοντες, καλὴν ἐμπορίαν ἐξάγουσιν· ὅπου γὰρ ἂν πωλῶσιν αὐτὸ, πανταχοῦ πλεῖον τοῦ ἀρχαίου λαμβάνουσιν.

(2) The piece of 50 gr. in P. II. T. 48. of the Pembroke collection, ſeems to be a Pentobolon; and the firſt in that plate a Hemiobolion. Mr. Stuart brought both half and quarter Oboles of ſilver from Athens.

(3) Pollux, L. IX. c. 6. § 60. There is a half Tridrachm of Alexander in the Britiſh Muſeum.

(4) See the Schol. on ver. 737 of Ariſtophanis Ranæ.

money

money of Athens (5). But they feem to have had copper money not long after; for Theophraftus, Demofthenes, and fome of the Comic Poets, quoted by Athenæus and Pollux, mention the Chalcus, which was the name of the copper coin (6). Many pieces of Attic copper are now in being (7); and Vitruvius fays, they coined copper Oboles, and quarter Oboles (8).

Authors differ in the value of the Chalcus; fome fay, it was the fixth part of an Obole (9), others the 8th (1); Pliny (fpeaking of it as a weight) the 10th (2); and Vitruvius, in the place before quoted, fays, fome called the quarter of an Obole Dichalcon, others Trichalcon. According to Polybius, it feems to have been the 8th part, for he makes a quarter of an Obole equal to half a Roman *As* (3); but the Denarius paffing for 16 *Affes*, and the Drachm for 6 Oboles, if a quarter of an Obole was equal to half an *As*, the Denarius fhould be greater than the Drachm, which it never was. Polybius, therefore, gives this

(5) Ariftoph. Ecclef. ver. 810 and the following.

(6) Theophraft. περὶ ἀπονοίας, and περὶ βδελυρίας. Demofthenes c. Midiam. Athenæus, L. III. c. 32. and elfewhere. Pollux, L. IX. c. 6. § 65.

(7) Pembroke Coll. P. II. T. 48.

(8) Vitruv. L. III. c. 1.

(9) Suidas, v. Ὀβολός. v. Τάλαντον. and one of the fragments in the appendix to Stephens's Greek Thefaurus, col. 217.

(1) Pollux, L. IX. c. 6. § 65, 67. Suidas, v. Τεταρτημόριον. The fragments afcribed to Galen and to Cleopatra in Stephens's Greek Thefaurus, col. 215, 217. That afcribed to Diofcorides fays, the third part. Thefe fragments fpeak of it as a weight, not a coin.

(2) Pliny, Nat. Hift. L. XXI. near the end of the laft chapter.

(3) Polybius, L. II. p. 103. of Cafaubon's edit.

for

for the neareſt value of half an *As* in Greek money, as it was if the Obole paſſed for 8 Chalci; but had it paſſed for 10, he would have ſaid one 5th of an Obole, which is nearer to the true value of half an *As*; or had it paſſed for 6, he would have ſaid one ſixth, which is ſtill nearer; in either caſe, he would not have ſaid one fourth, as neither 10 nor 6 admits of that diviſion. But though, when Polybius wrote, the Obole might paſs for 8 Chalci, it is not impoſſible that at different times, or in different places, it may have paſſed for 6, 10, and 12.

It is a common opinion, that the Athenians coined gold, for which I can find no good authority; and from the beſt information I have been able to get, there does not appear to be any Attic gold coin now remaining, that was ſtruck while they were a free and flouriſhing people.

The lexicographers, indeed, tell us, the Χρυσȣς Ἀττικὸς was equal to the Daric (4), and ſpeak of gold mines at Laurium (5); but no ancient writer mentions ſuch a coin, and all agree that the mines at Laurium were ſilver (6).

A paſſage in the Frogs of Ariſtophanes is, I believe, the only poſitive proof that can be produced from any ancient author in favour of this opinion. In ver. 732 of that comedy, he mentions a new gold coin. The ſcholiaſt on this paſſage tells us, that in

(4) Pollux, L. IX. c. 6. § 53. Suidas, v. Γλαύξ. v. Δαρεικός. Harpocratio, v. Δαρεικός.

(5) Suidas, v. Γλαύξ. Heſychius, v. Λαύρεια.

(6) Thucyd. L. II. § 55. and L. VI. § 91. Xenoph. περὶ προσόδων. Strabo, L. IX. p. 399, and Pauſanias at the beginning of his firſt book.

the Archonſhip of Antigenes, the Athenians coined their golden images of Victory; and the author of the treatiſe Περὶ ἑρμηνείας, § 298, praiſes an orator for the happy choice of his expreſſion, when he propoſed this expedient; but he neither mentions the orator's name, nor the time when this happened, nor whether the Athenians followed his advice; though the ſcholiaſt's ſhort quotation from Philochorus ſeems to imply that they did. But if in ver. 732. above mentioned, for χρυσίον, we read χαλκίον, it will agree better with ver. 737. where the Poet calls this money πονηρὰ χαλκία; and the ſcholiaſt on theſe words ſays, *perhaps the Poet means the copper money of Callias*; and this comedy was acted in his ſecond Archonſhip, when that copper money was coined.

That they had no gold coin at the beginning of the Peloponneſian war, appears from the account Thucydides gives of the treaſure then in the Acropolis, which conſiſted of ſilver in coin, and gold and ſilver bullion (7); but he would certainly have mentioned gold in coin, had there been any.

Therefore the ἀρχαῖον νόμισμα of Ariſtophanes could not be gold, nor the baſe καινὸν χρυσίον of equal value with the Daric; whence I conclude, καινὸν χαλκίον to be the true reading; and that it was the copper money above mentioned, which was afterward cried down.

Athenæus tells us that gold was extremely ſcarce in Greece, even in the time of Philip of Macedon; but that, after the Phoceans had plundered the

(7) Thucyd. L. II. § 13.

Pythian

Pythian temple, it ſhone forth among the Greeks (8). Philip conquered theſe Phoceans, and put an end to the holy war, as it was called.

About the time this war broke out, he took the city Crenides, on the borders of Thrace, which he enlarged, and called Philippi, after his own name; and he ſo improved the gold mines in its diſtrict, which before were of ſmall account, that they produced above a thouſand talents yearly, and enabled him to coin gold, which he called Philippics (9).

What Athenæus ſays of the ſcarcity of gold, may be true, if confined to Macedon, and the poorer ſtates of Greece; but muſt not be extended to Corinth or Athens; for though Thucydides does not ſpecify the quantity of gold that was in the Athenian treaſury at the beginning of the Peloponneſian war, it was, probably, not inconſiderable; for the gold about the ſtatue of Minerva weighed 40 talents, which valued (according to Herodotus) at 13 times its weight in ſilver, will be found to amount to above 120,000 pounds ſterling.

There is a gold coin in the Britiſh Muſeum, of elegant workmanſhip, with the head of Minerva on one ſide, and the owl and oil bottle on the other, the inſcription ΑΘΕ, and under the oil bottle the letters ΜΗ. It weighs 109½ Troy grains; but being a little worn, it probably, when new, came up to the juſt weight of the Roman Imperial Aureus. Whence we may conclude, that, when this piece was ſtruck, the Athenians had reduced their money to the

(8) Athenæus, L. VI. p. 231. See Diodorus, L. XVI. p. 527. Stephens's edit. (9) Diodorus, L. XVI. p. 514.

 Roman

Roman ſtandard, and that their Drachm was then equal to the Denarius. But I cannot find there is any Attic gold now extant, that was coined before Greece became ſubject to the Romans.

The Perſian Daric ſeems to have been the gold coin beſt known at Athens in ancient times. This they called Stater (1), probably becauſe it was the ſtandard to which their Drachm was originally adjuſted, which the Lexicographers tell us was half its weight (2).

Though Greaves ſays, the Daric is ſtill found in Perſia, it is certainly very ſcarce, and perhaps of doubtful antiquity.

For want, therefore, of the Daric, we muſt have recourſe to the gold of Philip, who took either that coin or the Attic Drachm for his ſtandard; as will appear, when I come to compare his money, and that of his ſon Alexander, with the Attic ſilver. This he probably did, with a view to his intended invaſion of Aſia; for the ancient ſtandard of Macedon was very different from that of Athens, as I ſhall ſhew hereafter.

Philip and his ſon Alexander coined gold of 4, 2, 1, and half an Attic Drachm. Mr. Duane hath a coin of Berenice, the wife of the ſecond Antiochus, weighing a quarter of a Drachm. In the Pembroke collection is a gold medal of Lyſimachus, of 8 Drachms; and Mr. Duane hath another of the like weight. But the Daric or χρυσȣ̃ς was didrachmal,

(1) Herodotus, L. VII. § 28. and Thucyd. L. VIII. § 28. call it Στατὴρ δαρεικός.

(2) Pollux, L. IV. c. 24. Heſychius, v. Χρυσοῦς.

and there are more of that ſpecies now remaining, than of any other.

In the Britiſh Muſeum are three gold coins of Philip, which have all the ſharpneſs of new money freſh from the mint. The heavieſt of them weighs above $132\frac{3}{5}$ Troy grains. A fourth, in the ſame collection, hath a hole punched through it; but in other reſpects, ſeems as perfect as the reſt, and is the heavieſt but one, of the four. There is likewiſe, a double Philippic of Alexander, perfect and unworn, which weighs $265\frac{3}{10}$ grains.

There are two more of Philip, in this collection, each weighing 132 grains, one of Alexander, of $132\frac{1}{5}$, and another of $131\frac{3}{5}$; but theſe are all a little worn, therefore I ſhall make no uſe of them.

Mr. Stuart brought home a Philippic, which, though not ſo fair in appearance as the beſt in the Britiſh Muſeum, weighed $133\frac{7}{10}$ grains.

Out of ſeven of the moſt perfect gold coins of Philip and Alexander, in Mr. Duane's collection, four weighed 133 grains each. He hath a moſt beautiful coin of Alexander of Epirus, brother to Olympias, the mother of Alexander the Great, weighing $132\frac{1}{3}$ grains; the workmanſhip is exquiſitely fine, and as perfect as when it was firſt ſtruck.

Greaves tells us, he bought at Alexandria a Philippic of Alexander, which he thought the faireſt in the world, weighing exactly $133\frac{1}{2}$ Engliſh grains. But, to bring it up to the ſtandard of his Tetradrachm of 268 grains, he ſuppoſes it might want half a grain, either by time, or the mint (3). His mentioning

(3) Greaves, p. 72.

 the

the mint ſhews, he could not diſcern any appearance of wear upon it, therefore I ſuppoſe it was perfect.

He found two of the ſame weight in the poſſeſſion of Sir Simonds D'Ewes.

He bought another at Conſtantinople, which weighed 133 grains; with which comparing one of Sir John Marſham, he found the latter a grain deficient.

He quotes Snellius for two gold coins, one of Philip, the other of Alexander, each weighing 179 Dutch grains, which, he ſays, anſwer to $134\frac{1}{2}$ Engliſh (4). But in this he is miſtaken, for they anſwer to no more than $132\frac{17}{20}$ (5). Snellius, to favour an ill founded hypotheſis of his own, ſuppoſes they had loſt ſomewhat of their firſt weight (6), but does not ſay they had any ſuch appearance; and as they outweigh the heavieſt in the Britiſh Muſeum, it is probable they were perfect.

In the Pembroke collection are two gold coins, one of Philip, weighing 134 grains, the other of

(4) P. 71.

(5) Eiſenſchmid. p. 16. ſays, Budelius, who was maſter of the mint at Cologne, found the money-ounce uſed in Flanders and the United Provinces, to weigh $579\frac{1}{5}$ Paris grains (equal to $475\frac{1}{3}$ Troy) and that Gaſſendus found it but 577. See alſo the Memoires of the Royal Academy of Science, for the year 1767. pp. 364, 370. I weighed the Dutch half marc of 4 ounces, from Amſterdam, in an excellent balance, and found it to weigh 3 ounces, 19 p. weight, and 4 grains Troy; which divided by 4, gives 19 p. weight, 19 grains, or 475 Troy grains, for the weight of the Dutch ounce. This ounce contains 640 Dutch grains; and

As 640 to 179, ſo are 475 to $132\frac{17}{20}$, the weight of Snellius's coins.

(6) See Snellius de re nummaria, Vol. IX. of the Theſaurus Antiquitatum Græcarum, col. 1583.

Alexander,

Alexander, weighfng 266, which, by their weights, ſhould be perfect

The difference between the heavieſt and the lighteſt of theſe pieces ſuppoſed to be perfect, does not amount to two grains in the Philippic; and a mean, taken from ſuch a number of coins, ſo equally ſized, muſt be very near their original ſtandard weight.

In the following table, I have not inſerted any piece, that I had reaſon to believe was ſenſibly deficient of its original weight. Therefore I have omitted Sir John Marſham's coin of 132 grains, which being deficient of the leaſt weight I have found in any perfect piece, it is moſt probable it was a little worn. I have likewiſe omited three coins in the Pembroke collection, of 132 grains each, for the ſame reaſon.

The pieces under the letter M, are from the Britiſh Muſeum; thoſe under D, from Mr. Duane's collection; that marked S, Mr. Stuart's; G, is the mark for thoſe mentioned by Mr. Greaves; Sn. for the two of Snellius; and P, for two from the Pembroke collection. The parts of a grain are given in decimals, for the convenience of adding them.

Troy

		Troy grains.
Philip . .	M.	132,62
		132,23
		132,08
with a hole		132,46
Alexander . .		265,3
Philip . . .	D.	133
		133
		133
Alexander . . .		133
		132,5
		132,5
		265,5
Philip . . .	S.	133,7
Alexander .	G.	133,5
		133,5
		133,5
		133
Philip . . .	Sn.	132,85
Alexander . . .		132,85
Philip . . .	P.	134
Alexander . . .		266
		24)3190,09
Mean Philippic		132,92

As none of theſe pieces can have increaſed their original weight, but, on the contrary, ſome may have loſt a ſmall part of it, we may fairly conclude, that the ſtandard weight of the Philippic was not leſs than 133 Troy grains; but probably ſomewhat greater.

In

In the Pembroke collection is a gold coin, or rather medal, of Lyſimachus, weighing 540 grains. Mr. Duane hath another of them, which wants but $2\frac{2}{5}$ grains of the ſame weight. This piece ſhould weigh 8 Drachms, and is of great importance on that account, as large weights and meaſures are more to be depended on, in inquiries of this kind, than ſmaller. According to this coin the Philippic ſhould weigh 135 grains, and the double Philippic 270: but none have yet been found to come up to theſe weights. Some few ſilver Tetradrachms exceed 270 grains, but they are very uncommon, and far the greateſt number of ſuch as ſeem moſt perfect, fall ſhort of 266. Neither is the ancient ſilver ſo correctly ſized, as to ſtand in competition with the gold of Philip and Alexander. Therefore, either the mint-weights of Lyſimachus were heavier than the Philippic ſtandard, or his money was leſs carefully ſized: or, laſtly, this piece, being intended rather for a medal than a coin, was purpoſely over ſized.

The ſilver coins of Philip and Alexander confirm what the lexicographers tell us, that the golden Stater of Philip, weighed two Drachms.

In the Britiſh Muſeum is a Drachm of Philip, weighing $67\frac{3}{10}$ grains, and another of Alexander of $66\frac{3}{5}$, both perfect. In the Pembroke collection is one of Alexander, which weighs 67 grains. Theſe give a didrachm of $134\frac{3}{5}$, $133\frac{1}{5}$, and 134 grains.

$267\frac{1}{2}$ Seven perfect tetradrachms of Alex-
266 ander, out of a much greater number,
$265\frac{1}{2}$ in Mr. Duane's collection, give a mean
$264\frac{1}{2}$ Didrachm of a little more than $132\frac{1}{2}$
$264\frac{1}{2}$ grains, as in the margin; which
264 answers near enough to the gold
$263\frac{1}{2}$ coins, to prove, that the Drachm was
——— the common standard, both for the
$14)1855\frac{1}{2}$ gold and silver money.

Mean $132\frac{1}{2}+$

I shall now shew, that this was the Attic Drachm.

The silver Stater, or Tetradrachm, is the most common Attic coin now remaining, and some of them are in very perfect preservation. They all have the head of Minerva on one side, and an owl on the other, with the inscription ΑΘΕ. Eisenschmid observes, that they appear, by the workmanship, and other circumstances, to be of different ages (7).

The most ancient are very rude work (8), of a small diameter and thick. He had one of them in the most perfect preservation, weighing $273\frac{1}{8}$ Troy grains; and there is one like it in the British Museum of $272\frac{9}{10}$ grains.

The second sort is somewhat better work, though rude, and the owl stands in a square; but in other respects is like the former. The eighth and ninth coins P. II. T. 48. of the Pembroke collection,

(7) Eisenschmid. p. 44.

(8) See Eisenschmid's figure, and c. 7. of P. II. T. 48. of the Pembroke collection.

seem

ſeem to be of this ſort. The eighth weighs 266 grains; and by having the weight put to it, and not to either of the other two in the ſame plate, I ſuppoſe it is well preſerved, and perhaps perfect. Theſe and the above mentioned have an olive branch coming from the edge to the owl; and both, by the rudeneſs of the work, ſhould be older than the time of Pericles, under whoſe adminiſtration ſculpture flouriſhed at Athens.

The work of a third ſort is more elegant, though not highly finiſhed. Its diameter is equal to that of an Engliſh half crown. The face of Minerva is beautiful; the owl ſtands on an oil bottle, and is encompaſſed by two olive branches, and, beſides the inſcription ΑΘΕ, hath ſome monograms and ſymbols near the owl.

A fourth ſort, of the ſame ſize, is generally higher finiſhed; and beſides the inſcription ΑΘΕ, hath inſtead of the monograms, a name or names about the owl, perhaps of the mint-maſters, or, as Mr. Stuart conjectures, of the owners of the mine that produced the ſilver. Theſe likewiſe have commonly ſome ſymbol near the owl. Some of them have a letter on the belly of the oil bottle, and two letters under it, as it were in an exergue. They ſeem to be of a later date than the laſt mentioned; for none that I have ſeen have the Ε for Η, or the Ο for Ω in the names, though they retain the Ε in ΑΘΕ; but the long vowels did not come into uſe at Athens till after the Peloponneſian war, as appears by inſcriptions now remaining (9), therefore, these must have been struck after that time; and if any now remain, that were struck during that war, they must be those with monograms.

The Attic money is not so equally sized as the Philippic silver. Mr. Duane hath a Tetradrachm with the letter K on the oil bottle, and ΔI under it, inscribed ΜΕΝΤΩΡ ΜΟΣΧΙΩΝ, which weighs 271¾ grains, and another with the same letters on and under the oil bottle, inscribed ΚΛΕΟΦΑΝΗΣ ΕΠΙΘΕΤΗΣ, in as perfect preservation, which weighs but 265 grains.

An Attic Tetradrachm in the British Museum, which appears to be but little worn, and not otherwise diminished, weighs but 247½ grains. We can hardly suppose, that this was struck to the same standard as Eisenschmid's ancient Tetradrachm of above 273 grains. That in the Pembroke collection, of 207 grains, hath probably been filed on the edge.

There are, however, a considerable number of Attic Tetradrachms, that answer in weight to those of Philip and Alexander, as nearly as can be expected, from coins so unequally sized. Mr. Stuart brought a very ancient one from Greece, weighing 265½ grains; Mr. Duane hath one of the like age, which weighs 265¼; they are both well preserved, and can have lost very little of their original weight: one, with a monogram and symbol, of 266¼ grains; another, inscribed ⊣ANI of the same weight, two of 265 grains, and one of 265¼. These answer so nearly

(9) See Montfaucon's Palæographia Græca, p. 135. and the Marmor Atheniense, lately published by Mr. Chambers. The Scholiast on ver. 688 of Euripides's Phœnissæ dates the introduction of the long vowels into Athens, in the Archonship of Euclides.

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to the weights of Alexander's Tetradrachms, that we cannot doubt of the equality of his ſtandard to that of Athens. And the gold Philippics of him and his father are ſo correctly ſized, and ſo perfect, that the mean Didrachm derived from them, of 133 Troy grains, muſt be very near its juſt weight; and its half, $66\frac{1}{2}$, that of the Attic Drachm.

§ II. *Of the Eginean and Euboïc Talents.*

THE Attic was not the only money-talent uſed in Greece. Hiſtorians and others mention the Eginean and the Euboic Talents. The former weighed 10000 Attic Drachms, but, like other Talents, contained only 6000 of its own; which being ſo much heavier than the Attic, the Athenians called it παχεῖαν δραχμὴν, or the thick drachm (1). This Talent was uſed at Corinth, as appears by a paſſage in A. Gellius, where the Corinthian Talent is valued at 10000 Attic drachms (2): and as Corinth was a place of great trade, it was probably uſed in moſt of the cities of the Peloponneſus.

If the Attic Drachm weighed $66\frac{1}{2}$ Troy grains, the Eginean ſhould weigh $110\frac{5}{6}$; which, to avoid fractions, and becauſe our Attic Drachm is rather underſized than otherwiſe, I ſhall call 111.

There are Macedonian coins, ſtruck before Philip coined gold, that anſwer to this ſtandard. One of Philip, in the Pembroke collection, weighs 224 grains.

(1) See Pollux, L. IX. c. 6. § 86 and 76.
(2) A. Gellius, L. I. c. 8.

Mr. Duane hath a ſilver coin, of either the firſt or ſecond Alexander, which weighs 447½ grains; three of Philip, of 221 each; another of Philip, of 223¼; and a fifth, 223¾. The mean Drachm from theſe ſix coins is 111¼ grains, which comes as near to the Eginean drachm, as can be expected from ſo ſmall a number of ſilver coins. Therefore, the Eginean Talent muſt have been the ſtandard of the Macedonian money, till Philip changed it.

It appears likewiſe to have been the ſtandard of the Ptolemaic money in Eygpt. Mr. Duane hath a gold coin of the Ptolemies, like c. 1. T. III. of the Pembroke collection, weighing nearly 27½ grains; Mr. Stuart another, weighing 27 3/20: ſuppoſing each was a quarterof the Drachm, the former will give it almoſt 110 grains, the latter 108½; but they are both a little worn. Mr. Duane hath a gold coin of Arſincë, like c. 3. T. III. of the Pembroke collection, which weighs 430 grains; and Dr. Hunter hath another of the ſame weight, which give a Drachm of 107½ grains. Dr. Hunter hath likewiſe a perfect ſilver coin of one of the Ptolemies, weighing 221 grains, another of 220, and a third of 109¼; but the two laſt are a little worn. The Ptolemaic gold coins in the Pembroke collection give the Drachm from 107 to 108 grains. As the piece of 221 grains wants but half a grain in the Drachm of the Eginean ſtandard, and that of 107 but four grains, we may fairly conclude that Talent to have been the money ſtandard of the Ptolemies. And not only ſo, but that it was originally Egyptian. For what ſhould induce Ptolemy, to relinquiſh the ſtandard eſtabliſhed by Alexander, and uſed all over Aſia and the

the greater part of Greece, but that he found the Eginean Talent eſtabliſhed in Egypt, when he poſſeſſed himſelf of that opulent kingdom.

Yet ſo imperfect are the accounts now remaining, of the ancient weights, that no writer hath mentioned this Talent, or one like it, as uſed in Egypt. On the contrary, Pliny tells us, on the authority of Varro, that the Egyptian Talent weighed 80 Roman pounds (3). But this is undoubtedly a falſe reading, and for *Ægyptium* we ſhould read *Euboïcum*; for Pliny is ſpeaking of the riches of Aſia, where the Euboïc Talent was uſed for weighing gold; and we know the weight of that Talent was ſettled at 80 Roman Pounds, by the treaty between the Romans and Antiochus.

The fragment of weights and meaſures aſcribed to Galen, makes the Egyptian Mina to weigh 16 Ounces (4); and conſequently, the Talent 80 Roman Pounds. But this Talent could not be the ſtandard of the Ptolemaic coins.

There is a paſſage in Pollux which makes the Egyptian Talent contain 1500 Attic Drachms (5). But this is an injudicious interpolation in the laſt edition of that author.

The fragment aſcribed to Cleopatra, and one that follows it, mention a Ptolemaic Mina of 18 Ounces, whoſe Drachm ſhould weigh $75\frac{3}{5}$ Troy grains; and Cleopatra ſays, there was an Egyptian Drachm, which weighed but the ſixth part of the Attic.

(3) Nat. Hiſt. L. XXXIII. c. 3.
(4) Stephani Theſ. Græc. t. IV. col. 25.
(5) L. IX. c. 6. § 86.

Galen

Galen (6) and the fragment aſcribed to Dioſcorides ſay, the Mina of Alexandria weighed 20 Ounces, or 120 Drachms. By Drachms, Galen certainly meant Denarii of 8 in the Ounce; for he tells us, that, in his time, a Drachm was always underſtood to mean what the Romans call a Denarius (7). The Drachm of this Mina ſhould weigh 84 grains.

Laſtly, Feſtus ſays, the Alexandrian Talent contained 12000 Denarii (8). If by Denarii he meant Attic Drachms, this Talent ſhould be juſt double the Attic.

None of theſe talents could be the ſtandard of the Ptolemaic money. Though, if Galen's Alexandrian Mina weighed 160 ancient Attic Drachms, its Drachm would weigh $106\frac{2}{5}$ Troy grains, which comes near to the Ptolemaic ſtandard. But the coins require a greater weight, and the Eginean Mina ſhould weigh $166\frac{2}{3}$ Attic drachms.

The Euboïc Talent certainly came from Aſia; for, Herodotus tells us, the Kings of Perſia weighed their gold by that Talent (9). In the ſame place he informs us, that the Babylonian Talent weighed 70 Euboic Minas. Pollux ſays, it weighed 70 Attic Minas (1). Therefore the Euboïc Talent ſhould be equal to the Attic. But Ælian tells us, it weighed

(6) See the word Μνᾶ in the index to Stephens's Greek Theſaurus.

(7) Πρόδηλον ὅτι δραχμὴν λέγομεν νῦν ἐν τοῖς τοιούτοις ἅπασιν, ὅπερ Ῥωμαῖοι δηνάριον ὀνομάζουσιν. Galen, L. VIII. De compoſ. medicam. as quoted by Gronovius, L. II. c. 6. De Pecun. Vet.

(8) Feſtus, De Verborum Signif. v. TALENTUM.

(9) Herod. L. III. § 89. (1) Pollux, L. IX. c. 6. § 86.

72 Attic Minas (2); and if ſo, the Euboïc Talent ſhould be heavier than the Attic, in the proportion of 72 to 70.

An article in the treaty between the Romans and Etolians, recorded by Polybius (3), whereby the latter were to pay a certain number of Euboïc Talents, in ſilver of Attic fineneſs, ſeems to favour this inequality of the two Talents: for, had they been equal, there would have been no occaſion to ſpecify the quality of the ſilver by the ſtandard of one country, and its weight by that of another.

But, if the Euboïc Talent was the ſtandard uſed in the commerce between Greece and Aſia (as it ſeems to have been) both countries were concerned to keep it up to its juſt weight; which was a ſufficient reaſon for the preference given to it by the Romans, on account of its authenticity, whether the Attic Talent was equal to it or not.

And there is a circumſtance very ſtrongly in favour of their equality, which is, that if Philip changed the money-ſtandard of his own country, with a view to the invaſion of Aſia, (as is highly probable), he certainly adopted the ſtandard of the Daric, which was the Euboïc Talent, by which the Kings of Perſia weighed their gold. But his money anſwers to the Attic Talent, as I have ſhewn above.

Pollux no where mentions the Euboïc Talent; and if he took his eſtimate of the Babylonian Talent from Herodotus, he certainly thought the Euboïc Talent was equal to the Attic.

(2) Var. Hiſt. L. I. c. 22.
(3) Polyb. Excerpt. Legat. § 28.

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But the numbers in the account Herodotus hath given of the revenue of Darius, as they now ſtand, diſagree with each other, and muſt be faulty in more places than one; and as probably in his value of the Babylonian Talent as elſewhere.

He tells us, the King of Perſia weighed his ſilver by the Babylonian Talent; therefore, that muſt have been reckoned the ſilver Talent of the empire, and was probably the ſtandard of their ſilver coin.

Xenophon, in his account of the expedition of Cyrus, ſays, the Aſiatic Siglus was worth $7\frac{1}{2}$ Attic Oboles (4). This coin ſeems to have been the Drachm of the Babylonian Talent; and if that Talent weighed 72 Attic Minas, the Siglus was really worth but $7\frac{1}{5}$ oboles; but the place Xenophon here ſpeaks of was near Babylon, where the Attic money was unknown and conſequently undervalued in common currency, This however ſhews, that, if the Babylonian Talent was the ſtandard for the ſilver coinage in Perſia, its weight probably exceeded 70 Attic Minas.

The ſame author tells us, that Cyrus paid Silanus the Ambraciot 3000 Darics for ten Talents. Therefore, the Talent of ſilver was worth 300 Darics. And if 3000 Darics were coined out of the Euboïc Talent of gold, 300 weighed ſix Euboïc Minas: and ſuppoſing the Babylonian Talent to weigh 72 ſuch Minas, the price of gold, at that time, was twelve times its weight in ſilver, as Plato, who was Xenophon's contemporary, tells us it was (5).

By the former of theſe paſſages, it appears probable that the Babylonian Talent weighed above 70

(4) Xenoph. Exped. L. I. (5) Plato, in his Hipparchus.

Attic Minas; by the latter, that it weighed above 70 Euboïc Minas; and if Pollux took his value of the Babylonian Talent from Herodotus, as the text now ſtands, and Ælian his value of the ſame, from a more correct copy of that author, or from ſome better authority, the Euboïc Talent muſt have been equal to the Attic.

§ III. *Of the Roman Money.*

PLINY hath given the following hiſtorical account of the Roman coinage: " Silver was firſt " coined at Rome in the 485th year of the City, " when Q. Ogulnius and C. Fabius were Conſuls, " five years before the firſt Punic war. And the " denarius was made to paſs for ten pounds of copper; the quinarius, for five; and the ſeſterce, for " two and a half. But the weight of the *As* was " reduced in the firſt Punic war, when the republic, " being unable to defray its expences, reſolved to " coin ſix *Aſſes* out of the pound; whereby they " gained five parts, and paid their debts. The " ſtamp of the *As* was a double-faced Janus on one " ſide, and the prow of a ſhip on the other: on the " triens and quadrans a boat. After this, when they " were preſſed by Hannibal, Quintus Fabius Maximus being dictator [*about the year* 537], the *As* " was reduced to one ounce, and the ſilver denarius " made to paſs for 16 *Aſſes*; the quinarius, for eight; " and the ſeſterce, for four. And the republic gained " one half [*upon the copper money*]. But in the pay

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" of the army, the ſoldier always received a *ſilver* " denarius for ten *Aſſes*. The ſtamp of the ſilver " money was a chariot and a pair, or a chariot and " four horſes; whence they were called *Bigati* and " *Quadrigati*. The *As* was ſoon after reduced to half " an ounce, by the Papirian Law.——What is now " called the Victoriat, was coined by the Clodian Law; " before which, it was imported from Illyricum as " merchandize: its ſtamp is a Victory, whence it " takes its name. The gold money was coined ſixty " two years after the ſilver, and the ſcruple paſſed " for twenty ſeſterces, which, as the ſeſterce was " reckoned at that time [2½ *Aſſes*], made the pound of " gold worth nine hundred *ſilver* denarii (1) [*of* 16 " *Aſſes each*]. It was afterward thought proper to coin " forty pieces out of the pound of gold. And our " Princes have, by degrees, diminiſhed their weight " to 45 in the pound (2)."

Thus far Pliny, whoſe date of the firſt coinage of ſilver is confirmed by Livy (3).

The Denarii now remaining are of various kinds. The moſt ancient are the *Bigati* and *Quadrigati*, having on one ſide the head of a woman in a helmet, with

(1) The common reading is ſeſtertios DCCCC, which I ſhall conſider hereafter.

(2) Plin. Nat. Hiſt. L. XXXIII. c. 3. In moſt editions of Pliny before Hardouin, the numbers 40 and 45, are thus written X. XL. M. and X. XLV. M. whence Agricola and Snellius have ſuppoſed the M. after the former number, to be a miſtake of the tranſcriber for II. and that after the latter for III. But Hardouin in his note on this paſſage hath ſhewn the M. in both places, to be ſuperfluous. In the laſt clauſe, I read *minutiſſimè vero*, not *minutiſſimè Nero*.

(3) See the epitome of L. XV.

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the inſcription ROMA, and the mark of the Denarius X or X̶, and ſome few XVI, and a Biga or Quadriga on the other. The next to theſe in antiquity have the head of Roma, or ſome other Deity, on one ſide, and on the reverſe, the name of the mintmaſter, or mintmaſters, with hiſtorical or emblematical figures. Many of theſe have the X or X̶, which continued to be the mark of the Denarius long after it paſſed for 16 *Aſſes*; whence ſome have concluded that it was reduced again to ten *Aſſes*, contrary to the expreſs teſtimony of Vitruvius (4); and Tacitus tells us that the mutinous legions in Pannonia demanded, to have their pay raiſed from ten *Aſſes*, to a Denarius. A third ſort hath the head of a Conſul or a General on one ſide, with an hiſtorical or emblematical reverſe. Few, if any, of theſe have the mark X or X̶ upon them. Theſe three ſorts are called Conſular Denarii, as having been ſtruck during the republican government by Conſuls. The Imperial Denarii have commonly the head of the reigning Emperor, with his name and titles on one ſide, and ſome emblematical figures on the reverſe, with a ſuitable inſcription.

The Romans coined their firſt gold money by the Scruple, as appears from Pliny's account, which is confirmed by the coins; for he tells us the Scruple paſſed for twenty Seſterces, and the reare gold coins now remaining with the numerals XX, and XXXX, which anſwer to the weight of one, and two ancient Roman Scruples. Theſe have the head of Mars on one ſide, with the numeral letters denoting their value,

(4) Vitruvius, L. III. c. 1. So likewiſe Voluſius Mætianus. Taciti Annal. L. 1. § 17. & 26.

and, on the reverſe, an Eagle ſtanding on a Thunderbolt. The latter coins of this ſcrupular ſtandard are like the Denarii of the age in which they were ſtruck; as was the gold of the different ſtandards that ſucceeded it.

The Romans did not uſe the Denarius for a weight, as the Greeks did their Drachm; till the Greek phyſicans coming to Rome, and finding the two coins nearly equal, preſcribed by it, as they had been accuſtomed to do by the Drachm in their own country. Neither did the Roman Pound depend on the weight of the Denarius, as the Greek Mina did on that of the Drachm; but the weight of the Denarius depended on the Pound.

The antient Roman Pound was divided into 12 Ounces, and the Ounce into 24 ſcruples (5). And we learn from Celſus and Pliny, that 84 Denarii were coined out of the Pound of ſilver (6); therefore, if we knew the true weight of the Roman Pound, we ſhould thence know that of the Denarius.

There are many antient Roman weights now remaining, from under an Ounce to 100 Pounds (7); ſome of them with inſcriptions have the appearance of ſtandards.

Lucas Pætus, from an antient weight of 10 Pounds, another of 4 pounds, and a third of 1 pound, inſcribed EX. AVC.D.CAS. in letters of ſilver, beſides three ſmaller of 3, 6, and 9 ounces, all ſix perfect and

(5) Varro de Re Ruſtica, L. I. c. 10. Collumella, L. V. c. 1. and Voluſius Mæcianus.

(6) Celſus de Medicina, L. V. c. 17. Pliny, Nat. Hiſt. L. XXXIII. c. 9.

(7) See Theſ. Antiq. Roman. Vol. XI. col. 1661.

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agreeing together, determined the antient Pound to contain 11 ounces, 10 ſcruples, modern Roman weight (8). But where he gives the weight of Veſpaſian's Congius (9), he makes ten antient Roman Pounds to weigh 9 pounds 6 ounces 10 ſcr. 10 gr. modern weight. The modern Roman ounce contains, like the antient, 24 ſcruples, the ſcruple 24 grains. Therefore, according to this determination, the antient Roman Pound ſhould weigh 11 ounces, 10 ſcr. $15\frac{2}{5}$ gr. modern weight, which is equal to $5012\frac{1}{5}$ Troy grains, if the exact weight of the modern Roman ounce be 438 Troy grains, as Greaves reckons it. But Pætus uſed a ſteelyard, which is a very fallacious inſtrument.

Gruter hath exhibited a conſiderable number of ancient Roman weights (1). Such of marble, from 1 to 10 pounds, as were intire, have neither mark nor inſcription. His two heavieſt weigh 9 pounds 8 ounces each, modern Roman weight, which give an antient Pound of 5081 Troy grains. Such of the reſt as are ſuppoſed to be intire, make it under 5000. His leſſer weights vary conſiderably. The Triens of Ruſticus gives a pound of 5092 Troy grains; his Sextans one of 5246. Among the braſs weights are two inſcribed AD. AVGVST. TEMP. C. P. One of five Pounds, weighing 5 pounds $2\frac{1}{2}$ ounces, makes the ancient Pound equal to 5475 Troy grains; the other is a Triens, and weighs 3 ounces, 19 ſcr. 4 gr. which gives 4992 Troy grains for the Roman Pound.

(8) Theſ. Antiq. Roman. Vol. XI. col. 1619.
(9) Ibid. col. 1635.
(1) Gruter's Inſcriptions, p. ccxxi.

Fabretti

Fabretti blames Pætus for making the ancient Roman Pound lighter than the modern (2), and produces ten ancient weights, to prove the contrary. Three of them are of braſs, and by their inſcriptions have the appearance of public ſtandards. One, with the mark X, weighs 10 pounds 5 oz. 14 ſcr. modern Roman weight, which, reduced to Troy grains, give $5500\frac{1}{2}$ for the antient Pound. Another, marked V, weighs 5 pounds, $2\frac{1}{2}$ oz. and gives 5475 Troy grains for the antient Pound. A third marked II, weighs 2 pounds, 1 oz. 9 ſcr. which makes the ancient Pound amount to 5557 Troy grains. His white marble weight hath no other inſcription but the mark I, for one pound, and weighs 13 ounces, $1\frac{1}{2}$ ſcr. equal to 5721 Troy grains. The reſt of his weights are from five ounces to three ſcruples, and give an ancient Roman Pound from almoſt 5500 Troy grains to above 5780.

At the end of Eiſenſchmid's preface, we find two *Aſſes librales*, one equal to $5407\frac{1}{2}$ Troy grains, the other to $5315\frac{5}{8}$; and a *Quadruſſis* of 21351 Troy grains, which gives a pound of $5337\frac{3}{4}$.

According to Fabretti's weights, the ancient Roman Pound could not weigh leſs than 5475 Troy grains, which is much greater than can be derived from any other evidences, as I ſhall ſhew hereafter. But, as many of the abovementioned weights have the appearance of public ſtandards, I have thought proper, to take more particular notice of them, than writers on this ſubject have commonly done.

Both Villalpandus and Greaves relied on the Congius of Veſpaſian for the ſtandard weight of the

(2) Fabretti Inſcript. p. 523.

Roman

Roman Pound, not doubting its authenticity, though the note in Gruter ſays, ſome have ſuſpected it (3). What foundation they had for ſuch ſuſpicion, does not appear; but it is very difficult, to counterfeit the genuine cracks and corroſions of antiquity, in a veſſel of this kind; and Greaves tells us, that while he was in Italy, there was found, among the ruins at Rome, a Semicongius in braſs, of the ſame figure with this of Veſpaſian, the ſides much corroded with ruſt. This he alſo meaſured, and found it to be half of Veſpaſian's Congius (4). But weights are eaſily counterfeited; and when the remains of antiquity were ſo eagerly ſought after, that artiſts found it worth their while to counterfeit the ancient coins, others might counterfeit the weights.

The Roman Congius contained ten Pounds weight of wine (5). Veſpaſian's ſtandard is of braſs; Pætus, Villalpandus, and Greaves, have given drawings of it; and Gruter tells us, the inſcription was in letters of ſilver.

Pætus filled this veſſel to the narrow part of the neck with rain water, and weighed it with a ſteelyard. But this inſtrument is liable to great errors; therefore his weight, which wants 5½ modern Roman ounces of what Villalpandus found it, is of ſmall authority.

Villalpandus filled it to the ſame height with ſpring water, and found it to contain juſt ten modern Roman pounds, which are equal to 52560 Troy grains.

Auzout, filling it likewiſe to the ſame height with ſpring water, weighed its contents twice; and the near

(3) Gruter's Inſcriptions, p. ccxxiii.
(4) Greaves, p. 92. in a note.
(5) Feſtus de verb. ſignif. v. PUBLICA PONDERA.

agreement

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agreement of its capacity deduced from his weights, with Greaves's meaſure, by Millet (6), is a proof of their being very near the truth.

Auzout's greater weight was 63024 Paris grains, equal to $51699\frac{3}{8}$ Troy; his leſſer, 62760 Paris grains, equal to $51482\frac{4}{5}$ Troy (7). It is not ſaid, at what time of the year either of theſe weights was taken; but the heat in ſummer, and the cold in winter, might have made a much greater difference between them.

The mean between both is $51591\frac{1}{10}$ Troy grains, which, divided by 10, give $5159\frac{1}{10}$ ſuch grains for the weight of the ancient Roman Pound.

Fabretti inſiſts, that this veſſel ought to have been filled up to the brim (8); but the part above the neck ſeems to have been deſigned, either to prevent the liquor from ſpilling when poured out, or for a ſecurity againſt the diminution of the ſtandard, which ſuch a finiſhing rendered impracticable.

Several objections have been made to this Pound derived from the Congius, of which the following are the moſt material.

Firſt, whereas the ſide of the Quadrantal containing 8 Congii, ſhould be equal to the Roman Foot; the ſide of a cube, containing 8 times this veſſel, exceeds the moſt authentic meaſures of that foot now remaining. But, as this relation of the two ſtandards to each other was of an ancient date, when all work-

(6) See Philoſoph. Tranſ. Vol LI. p. 790.

(7) Divers ouvrages de Mathematique & de Phyſique par Meſſ. de l'Academie Royale, Paris, 1693, in folio. p. 366. 371.

(8) Fabretti Inſcript. p. 527.

manſhip

manſhip was probably very rude and inaccurate at Rome, we cannot wonder at ſuch a diſagreement; eſpecially as both the ſhape of this veſſel and the inſcription ſhew it was not adjuſted by the foot meaſure, but by weight.

Secondly, the ſame bulk of any liquor being found to weigh more in winter than in ſummer, we cannot determine the preciſe weight of the Roman Pound from the contents of this veſſel, unleſs we knew the ſeaſon of the year in which it was originally adjuſted.

Thirdly, Villalpandus ſeems to have made his experiment carefully (9); but his weight exceeds Auzout's leſſer weight by above 1000 Troy grains; though both uſed ſpring water. Now if two curious perſons, who endeavoured to diſcover the exact weight of the antient Roman Pound, could differ ſo much in weighing the contents of the ſame veſſel, can it ſeem improbable, that the Roman officer, to whoſe department the adjuſting this ſtandard might happen to belong, ſhould differ as much from its juſt weight? But if he happened to be a perſon of accuracy, he would take care, that the ſtandard of a meaſure of capacity ſhould not fall ſhort of its ancient dimenſions, which is extremely unpopular; and, though he might endeavour to be exact, he would rather chuſe to err in exceſs than defect. Therefore, this veſſel is more likely to give too great a Roman Pound, than too ſmall a one.

Fourthly, this veſſel was by law to contain ten Pounds weight of wine; which being lighter than water, the weights above-mentioned muſt be too

(9) See Greaves, p. 92.

great. But probably the Romans of that unphilosophical age when this ſtandard was firſt eſtabliſhed were ignorant of this difference; and it might not be generally known, or not attended to, even in Veſpaſian's time; for Remnius Fannius, who lived long after, treating of the weights of various liquids, ſuppoſes the weight of wine to be equal to that of water,

Nam libræ, ut memorant, beſſim ſextarius addet,
Seu puros pendas latices, ſeu dona Lyæi.

And though he afterward tells us that ſome wines and ſome waters are heavier than others, he does not ſay that water is in general heavier than wine. And even at this day, when the ſpecific gravities of different liquors are ſo generally known, our books of Pharmacy call a wine pint of any liquor a pound. Therefore it is not improbable that this ſtandard was adjuſted by ſpring water in the reign of Veſpaſian.

But if it was really adjuſted by wine, the difference may be conſiderable; for, according to Eiſenſchmid's table of the ſpecific gravities of various liquids (1), that of pump-water is to Burgundy wine in the proportion of 371 to 355; and Auzout's mean weight of 5159 Troy grains diminiſhed in this proportion, gives but 4936½ ſuch grains for the antient Roman Pound.

All the above circumſtances conſidered, it ſeems more probable that this ſtandard ſhould give too great a Roman Pound, than too ſmall a one. But as no-

(1) Eiſenſchmid, p. 174, 175.

thing

thing certain can be determined from it, we muſt have recourſe to the coins, eſpecially the gold, which though not ſo correctly ſized as the Greek Philippics, are much more ſo than the ſilver Denarii.

Pliny tells us, that when the Romans firſt coined gold, they made the Scruple paſs for 20 Seſterces.

In the tables VI, VII. and X. of the Pembroke collection, we find nine pieces, weighing 17 grains, $26\frac{1}{2}$, $33\frac{1}{2}$, $51\frac{1}{2}$, 53, 105, 107 twice, $107\frac{1}{2}$.

That this was the ſcrupular coin mentioned by Pliny appears from the numeral letters XX for 20 Seſterces, on the ſmalleſt, and XXX on that of $33\frac{1}{2}$ grains, which ſhould be its double; and all the reſt are multiples of ſomewhat between 17 and 18 grains, except the ſecond, which is a Scruple and half. What the mark ↓ X on that of $51\frac{1}{2}$ grains denotes, I cannot tell. Savot, and Hardouin (2) call this figure ↓ a V, and ſay VX ſtood for 15; but though the Greeks often placed their numerals from right to left, I cannot find that the Romans ever did.

Theſe nine pieces ſhould contain $34\frac{1}{2}$ Roman ſcruples: Their weight amounts to 608 Troy grains, which, divided by $34\frac{1}{2}$, give $17\frac{43}{69}$ for the Scruple; whence the Roman Pound ſhould weigh $5075\frac{11}{23}$.

(2) Savot, P. III. c. 7. Hardouin's note on Pliny. This piece is 3 Roman Scruples, which valued at 60 Seſterces of $2\frac{1}{2}$ *Aſſes* to the Seſterce, was worth 150 *Aſſes*, or 9 ſilver Denarii and 6 *Aſſes*, wanting but 2 *Aſſes* of $9\frac{1}{2}$ Denarii. Now in Ptolomy's geographical tables, where the degree is divided *unciatim*, after the Roman manner; this character ↓, ſtands for one half; therefore being placed before the X (as on the coin) it might denote $9\frac{1}{2}$, as I before the X ſtands for 9. But Mr. Duane hath a gold coin with the ſame mark, and of the ſame impreſſion as this, which weighs but $45\frac{1}{2}$ grains, though it ſeems to be perfect.

But theſe pieces are too ſmall, and too few in number, to determine this point. Mr. Duane hath that of one Scruple, in fine preſervation, weighing almoſt $17\frac{1}{2}$ grains. Mr. de la Nauze hath given the weight of the piece of 3 ſcruples with the mark ↓ X in the French king's cabinet, which he ſays is exactly 64 Paris grains (3), equal to $52\frac{1}{2}$ Troy, and gives $17\frac{1}{2}$ grains for the Scruple.

This ſcrupular ſtandard ſeems to have continued till Sulla introduced one which Pliny hath not mentioned, on account, perhaps, of its ſhort duration. It was probably occaſioned by the riſe of the value of gold; for when the ſcrupular ſtandard was firſt eſtabliſhed, gold was worth but about ten times its weight in ſilver, as I ſhall ſhew hereafter; but in Sulla's time it was much dearer.

Cicero plainly alludes to this alteration in the coin, when, ſpeaking of his kinſman Marius Gratidianus, he ſays, *At that time the money was in ſuch a fluctuating ſtate that no man knew what he had* (4): and both he and Pliny relate, that the law Gratidianus made in Sulla's abſence from Rome, for the regulation of the coin, was ſo popular, that ſtatues were erected to him in every ſtreet, and incenſe burnt before them (5). The intent of this law ſeems to have been, to reſtore the ancient ſtandard in oppoſition to Sulla; for it ſo provoked him, that, on his return to Rome, he cauſed all the ſtatues to be thrown down (6), and Gratidi-

(3) Memoires de l'Academie des Inſcriptions, Vol. XXX. p. 359.

(4) Cicero de Officiis, L. III. § 20.

(5) Cicero, ibid. Pliny, Nat. Hiſt. L. XXXIII. c. 9.

(6) Pliny, L. XXXIV. c. 6.

anus

anus to be cruelly butchered by the hand of Catiline (7).

Three coins in the Pembroke collection bear the name of Sulla, and weigh 166, 167, and 168 grains (8). Bouteroue mentions one of 204 Paris grains (9), equal to $167\frac{1}{3}$ Troy. If thirty of these were coined out of the Roman Pound, the heaviest of the four pieces gives a Pound of 5040 grains.

The standard of forty in the pound, mentioned by Pliny, seems to have succeeded to this of Sulla, and continued to the establishment of the monarchy under Augustus; for Pliny says, *Principes imminuere pondus*; and the two heaviest pieces I can find of this standard, are, one of Pompey, in whose time it seems to have been introduced, the other of Antony and Octavius, struck after the expiration of the Triumvirate, which differ but the tenth part of a grain in weight. They are both in the British Museum, in fine preservation. The former is like coin 4 Tab. XI. of the Pembroke collection; the latter like coin 11. Tab. XII. But such as bear the name Augustus, which he assumed with the monarchy, are lighter than those of the Triumvirate.

Pompey's coin weighs $128\frac{1}{2}$ Troy grains, the other $128\frac{2}{5}$. Mr. Duane hath both these coins in fine preservation, the former weighing $126\frac{1}{2}$ grains, the latter 127. Those in the Pembroke Collection weigh 125 grains each.

(7) Seneca de Ira, L. III. c. 18.

(8) Tab. VIII.

(9) Recherches curieuses des monnoyes de France. Paris, 1666. in folio.

There

There are befides, in the Britifh Mufeum, two of 125 grains, like c. 2, and 4. in Tab. IX. of the Pembroke collection; one of $124\frac{1}{3}$ like coin 3. all very little worn; and a fourth of $124\frac{3}{5}$ grains, like c. 4. Tab. VII. which feems to be perfect. Dr. Hunter hath two perfect gold coins, one like c. 3. Tab. VIII. weighing $125\frac{3}{4}$ grains; the other like c. 2. Tab. IX. which weighs $125\frac{1}{2}$.

Thefe ten coins give a mean Aureus of $126\frac{1}{17}$ grains.

The Pembroke collection contains forty Aurei, from Pompey to the end of the Commonwealth. One of them weighs 127 grains; two $126\frac{1}{2}$; fix 126; and the reft from $125\frac{1}{2}$ to 123; except two of 121, which, being probably fomewhat worn, or otherwife diminifhed, may fafely be rejected. The remaining 38 added to the ten above-mentioned, give a mean Aureus of $125\frac{5}{48}$ grains.

But confidering that thirteen of the forty-eight weigh from $128\frac{1}{2}$ to 126 grains, and that many of the reft are probably fomewhat worn, we may fairly take 126 grains for the ftandard weight of this coin; and the number of pieces under 125 grains, that are vouched for perfect, will not allow it to be greater.

Bouteroue mentions two perfect Aurei of Julius Cæfar, each weighing 152 Paris grains, equal to $124\frac{}{10}$ Troy. And Greaves in his firft Table hath marked three of Julius for perfect, which weigh $122\frac{1}{4}$, $123\frac{7}{12}$, end $124\frac{3}{4}$ grains.

If the Aureus of forty in the Pound weighed 126 Troy grains, the Roman Pound muft weigh 5040.

The weight of this coin was gradually diminifhed by the Emperors, till in Pliny's time forty-five were ftruck ſtruck out of the Pound. He died in the reign of Titus; and the mean Aureus of Greaves's table from Nero to that Prince, incluſive, is under 112 grains. That of the Pembroke Collection for the ſame period amounts to 113; but Nero's coins (contrary to Hardouin's reading of Pliny's text) appear to have been heavier than thoſe of Veſpaſian or Titus.

Snellius, in his book *De re numaria*, hath given the weights of eleven Aurei, from Nero to Commodus, which he ſays were all as perfect as when they came from the mint. The lighteſt weighed 149 Dutch grains, the heavieſt 153; which anſwer to $110\frac{1}{2}$ and $113\frac{1}{2}$ Troy. The mean taken from all the eleven, is almoſt 112 Troy grains.

Bouteroue found the Aureus from Nero to Septimius Severus, to weigh from 133 Paris grains to 138; that is, from $109\frac{1}{10}$ to $113\frac{1}{5}$ Troy. The mean of theſe two weights is $111\frac{3}{20}$ grains.

This ſtandard continued beyond the reign of Septimius Severus; and the Pembroke coins from Nero to that time, give a mean Aureus of almoſt 112 grains. But we cannot ſuppoſe all of them to be perfect. Greaves's tables make it $113\frac{2}{5}$ for the ſame period; but four of his pieces of Hadrian and the Antonines weigh from $117\frac{3}{4}$ to 121 grains; which is an uncommon weight for that age, and might poſſibly proceed from an alteration of the ſtandard, which did not continue long. Excluding theſe four, the reſt give a mean Aureus of $112\frac{1}{5}$ grains.

Eiſenſchmid weighed a great number of ſuch as ſeemed perfect to the naked eye, and found the beſt of them to exceed 136 Paris grains, or $111\frac{5}{9}$ Troy. But, upon examining them with a glaſs, they all appeared

peared somehow damaged; which, says he, in so heavy a metal, might amount to the loss of a grain or two (1). But the loss of less than a grain is very discernible, without the help of a glass.

Upon the whole, if the standard weight of the imperial Aureus of forty-five in the Pound, did not exceed 112 grains, the Roman Pound will weigh 5040 Troy grains, as we found it from the consular Aureus.

Alexander Severus coined pieces of one half and one third of the Aureus, called Semisses, and Tremisses (2); whence the Aureus came to be called Solidus, as being their integer.

Soon after the reign of this prince, the coinage became very irregular, till Constantine entirely new modeled it, by coining 72 Solidi of four Scruples, out of the Pound of gold (3), and for the Denarius substituting the Miliarensis, of which I shall give some account hereafter.

Greaves's second table exhibits twenty-nine of these Solidi from Constantine to Heraclius, weighing from $67\frac{1}{2}$ grains to $70\frac{3}{4}$. The mean from the twenty-nine pieces is 69 grains, which, multiplied by 72, gives but 4968 grains for the weight of the Roman Pound.

(1) Eisenschmid, p. 34.

(2) Lampridius, in Alex. Severo.

(3) Siquis solidos appendere voluerit, auri cocti VI solidos quaternorum scrupulorum, nostris vultibus figuratos, adpendat pro singulis unciis, XII pro duabus: eadem ratio servanda & si materiam quis inferat, ut solidos dedisse videatur. Cod. Theod. de Ponderatoribus, § 1. Again, Illud autem cautionis adjicimus, ut quotiescunque certa summa solidorum pro tituli quantitate debetur, & auri massa transmittitur, in LXXII solidos libra feratur accepta. Cod. Justin. L. X. Tit. 70. de Susceptoribus, § 5.

Pound. But if the ſtandard weight of this coin amounted to 70 grains, the Pound will weigh 5040, agreeable to what we found it from the Aurei.

The Pembroke Collection contains 57 of theſe pieces from Conſtantine to Juſtinian. Five of them amount to 70 grains, and 29 to 69; the reſt are lighter, even to 64 grains. But we do not know what preſervation they are in. And unleſs the ſtandard weight of this coin amounted to 70 Troy grains, Conſtantine's Pound muſt have been ſomewhat deficient of the ancient Standard.

Having thus given as compleat an account of the Roman gold, as I have been able to collect from authors of credit, and my own obſervation, I ſhall proceed to examine the evidence we have of the weight of their ſilver money.

The Conſular ſilver is ſo unequal, that the Romans muſt have been very negligent in ſizing their pieces. Villalpandus tells us, that weighing many Denarii of the ſame form, inſcription, and apparent magnitude, and ſo like to each other, that they ſeem to have been ſtruck, not only in the ſame age, but even on the ſame day, he found them to differ in weight, 5, 9, or 10 grains from each other (4).

There is a piece in the Pembroke Collection, Coin 2. P. 3. Tab. 18, with the head of Roma, and X, the mark of the Denarius, on one ſide, on the other

(4) Cum plures Denarios appenderemus ejuſdem formæ, inſcriptionis, & penè magnitudinis, atque ita ſimiles, ut non ſolum eodem tempore, ſed eodem prorſus die, percuſſos fuiſſe conjiceres, tamen eos deprehendimus quinis, novenis, aut denis granis pondere a ſe invicem diſtare. Villalp. De apparatu urbis & templi, p. 357. Ten Roman grains are equal to about $7\frac{3}{7}$ Troy.

 Caſtor

Caſtor and Pollux, with R O M A in the exergue, which weighs 81 grains. Another with the like impreſs on each ſide, and V the mark of the Quinarius behind the head, which weighs 33 grains. A third in the ſame page hath the mark XVI behind the head of Roma, a biga on the reverſe, with R O M A in the exergue, which weighs but 54 grains. As theſe pieces ſeem to be exhibited chiefly on account of their uncommon weight, we muſt ſuppoſe the lighteſt to be perfect.

In the Britiſh Muſeum is a coin like the tenth in P. 3. Tab. 2. of the Pembroke Collection, which weighs above 73 grains. Another like the ſecond in P. 3. Tab. 18, which weighs 66½ grains; and a third, which ſeems perfect in all reſpects, with the head of Roma and X on one ſide, on the other a Quadriga with the inſcription C N.G E, which weighs but 55 grains.

It is difficult to account for theſe differences in the weight of the ſame coin, eſpecially as Pliny ſeems to have been ignorant of ſuch inequalities; for he tells us of an Eaſtern King, that wonderfully admired the juſtice of the Romans in coining all their Denarii of the ſame weight, though the impreſſes ſhewed them to be the money of different Emperors (5). Perhaps the King only admired the invention of coining, which was not known in his country; but Pliny, who tells the ſtory, certainly ſuppoſed all the Denarii were of equal weight.

Perhaps the heavy pieces of 73 and 81 grains were ſtruck at the mint for private perſons, to give

(5) Pliny, Nat. Hiſt. L. VI. c. 22.

away

away in presents on Birth-days, and New-years, as was the custom at Rome; and some of them may be modern forgeries: but the light pieces of 54 and 55 grains, must have been owing to the negligence or roguery of the coiners; though some of these too may be counterfeits.

The following Table exhibits the weights of forty-six of the fairest Denarii in the British Museum. Such of them as are marked with two dots, are a little worn, though very little. The Bigati and Quadrigati are distinguished by the letters B. and Q.

Troy grains

66,5 :	61,15
66,1	61,12 :
64,15 :	61,12
63,33	61,1 : Q
63,15 :	61,07 : B
63,07 :	60,85 :
63,05	60,75
62,7	60,5 : B
62,43 : Q	60,33 : B
62,27	60,3 : Q
61,95	60,2 B
61,93 :	60,05
61,8 :	59,95 :
61,8 B	59,15 : B
61,73 :	58,92 : Q
61,73 :	58,85 :
61,55 : Q	58,67 : Q
61,52 Q	58,2 Q
61,5 :	58,15
61,5	57,37 :
61,35	56,87 :
61,33	56,55 Q
61,2	55,0 Q

46) 2803,86 Sum total.

60,95 Mean Denarius.

The mean weight of the Denarius from all these pieces is 60,95 Troy grains; therefore, had all of them been perfect, it might have exceeded 61 grains. But the mean from the twenty-one that are so, amounts but to 60,92. Either of them comes very near to what Eisenschmid found it by the like method; though he rejected some pieces for no other reason but because he thought them too light.

But a mean from pieces so unequally sized is not to be relied on. And it may be questioned whether those of above 63 grains ever passed as common coin. Greaves, who had examined many hundred Denarii Consulares, says the best amounted to 62 grains; but had he met with any of 63, or even of $62\frac{1}{2}$, it cannot be doubted that he would have mentioned them in support of his Denarius of $62\frac{4}{7}$ grains from the Congius. Therefore the pieces of 63 grains and upward must be very uncommon, whereas they make above a seventh part of the number in this table.

Hence I conclude, that the mean derived from this table is of very small authority.

But if we take 5040 Troy grains for the weight of the Roman Pound, as determined from the Gold coins; the scruple will weigh $17\frac{1}{2}$ grains; the Consular Aureus, 126; the Imperial Aureus, 112; and the Solidus, 70: all which are probable weights of the several Coins; and the Consular Denarius of 84 in the Pound will weigh just 60 Troy grains.

And this must be very near its true standard weight; for were we to add only half a grain to it, the Consular Aureus would exceed 127 grains, which is certainly too great a weight for that coin.

Though Pliny gives no particular account of any alteration in the weight of the Denarius, it was undoubtedly diminiſhed by the Emperors as well as the Aureus, though by what degrees is uncertain; for Galen tells us, that the writers on weights and meaſures differed in the number of Drachms [*Denarii*] they aſſigned to the Ounce; moſt of them making it to contain 7½, ſome but 7, and others 8 (6). The later writers make it contain 8 Denarii, of 3 ſcruples each (7).

Greaves "found by examining many Imperial "Denarii, that from Auguſtus's time to Veſpaſian "they continually almoſt decreaſed, till, from being "the ſeventh part of the Roman Ounce, they came "now to be the eighth part: and therefore 96 were "coined out of the Roman Libra, whereas before, "under the Conſuls, 84. From Veſpaſian to Alex. "Severus, as far as he had obſerved, the Silver con- "tinued at a kind of ſtay in reſpect of weight, ex- "cepting only ſuch coins as upon ſome extraordi- "nary occaſion, both then, and in the firſt Emperors "time, were ſtamped, either in honour of the Prince, "or of the Empreſs and Auguſta familia, or elſe in "memory of ſome eminent action. Theſe laſt moſt "uſually were equal to the Denarii Conſulares, and "many of them had theſe characters EX. S. C, or "elſe S. P. Q. R. Under Severus and Gordianus, "the Denarii began to recover their primitive weight, "but moſt commonly with a notable abaſement, "and mixture of allay (8)." Eiſenſchmid hath

(6) Galen, de med. comp. ſec. genera, L. III. c. 3.
(7) Rhemnius Fannius, Cleopatra, Dioſcorides, &c.
(8) Greaves, p. 113.

given

given the like account of the Imperial Denarius, and ſays he found its weight from Nero to Sept. Severus, to be to the Conſular Denarius in the proportion of 7 to 8 (9).

Having determined the weight of the ancient Roman Pound from the gold coins, to be 5040 Troy grains, it ſeems requiſite to ſay ſomething concerning the heavy weights exhibited by Gruter and Fabretti, which are irreconcileable to every other evidence.

Thoſe with inſcriptions are not older than the reign of Auguſtus; but neither his coins, nor thoſe of his ſucceſſors, will by any means anſwer to ſuch ſtandards.

Fabretti's mean pound of 5500 Troy grains, exceeds Auzout's mean Pound from the Congius by above three fourths of the antient Roman Ounce, though that veſſel is greater than can be derived from the greateſt probable meaſure of the antient Roman foot.

The weight of ſpring-water contained in the cube of half that foot (which was the legal meaſure of the Congius) is thus determined.

According to Eiſenſchmid's Table of ſpecific gravities (1), a cubic Paris inch of ſpring-water ſhould weigh 374 Paris grains in winter, when liquors are heavieſt. Therefore the cube of half the Paris foot (or 216 cubic Paris inches) muſt weigh 80784 ſuch grains.

The greateſt probable meaſure of the antient Roman foot, does not exceed 974 ſuch parts as the Paris foot contains $1065\frac{2}{5}$ (2).

(9) Eiſenſchmid, p. 33. (1) Eiſenſchmid, p. 175.
(2) See the Diſcourſe on the Roman Foot, Phil. Tranſ. Vol. LI.

And

And as the cube of the Paris foot, is to the cube of the Roman foot, ſo are 80784 Paris grains, to $61725\frac{1}{2}$ ſuch grains, the weight of the ſpring-water contained in the cube of half the Roman foot.

But $61725\frac{1}{2}$ Paris grains, are equal to 50634 Troy; therefore the Roman Pound, according to this calculation, ſhould weigh $5063\frac{2}{5}$ Troy grains, exceeding that derived from the coins, but by $23\frac{2}{5}$ ſuch grains.

If, on the other hand, we take Fabretti's Pound of 5500 Troy grains (equal to $6704\frac{3}{4}$ Paris) and reckon the weight of a cubic Paris inch of ſpring-water 374 Paris grains (as before), a Congius of ten ſuch Pounds will require a Roman foot of 1001 ſuch parts as the Paris foot contains $1065\frac{2}{5}$; which exceeds any probable meaſure of that foot.

Thus theſe heavy weights neither agree with the Roman money nor with the Congius; which is a circumſtance not eaſily to be accounted for, as the authorities for the larger Pound are indiſputable, and we do not know that the Romans uſed two weights like our Troy and Averdepoids.

The Denarius continued to be the current ſilver money of the Empire, till Conſtantine ſubſtituted the Miliarenſis in its ſtead.

The price of gold had been increaſing a conſiderable time before his reign, which made a new regulation of the money neceſſary. For this purpoſe, Conſtantine divided the Pound of gold into ſeventy-two Solidi (3), which was a more commodious

(3) See the Theod. and Juſtinian Codes quoted in p. 504.

number

number than either 40 or 45, as it divided the Ounce and half Ounce without a fraction. He likewise altered the weight of the silver coin, and fixed the price of the Pound of gold at 1000 pieces of his new silver, which were thence called Miliarenses (4). This he seems to have done in imitation of the ancient coinage; for when the Aureus of forty in the Pound passed for 25 Denarii, the Pound of gold passed for 1000.

But it was attended with this inconvenience, that his Solidus could not be exchanged for its true value in silver; for 1000 divided by 72 is $13\frac{8}{9}$; but it passed for 14 (5), which was more than it was worth, and made two prices of gold at the same time; one the legal price of 1000 Miliarenses for the Pound; the other, the current price, of 14 for the Solidus, which must have occasioned disputes in the payment of small sums.

To remedy this inconvenience, it was thought proper to alter the weight of the silver money, and having fixt the price of the Pound of silver at five Solidi (6), to coin 60 pieces out of it (7); which

(4) Μιλιαρήσιον, τὸ χιλιοςὸν τῆς τȣ̃ χρυσȣ̃ λίτρας· μίλη γὰρ οἱ Ῥωμαῖοι τὰ χίλια καλȣ̃σι, καὶ ȣ̃τω καlεκερμάτωσαν τὸ πόσον τῆς λίτρας, ἵνα δι᾽ αὐτȣ̃ σώζηlαι τὰ χίλια μιλιαρήσια, ὥςε καlὰ νόμισμα λαΓχάνειν μιλιαρήσια ιδ᾽. Glossæ nomicæ, quoted by Gronovius, L. IV. c. 16. de pecunia vetere.

(5) See the preceeding note.

(6) Jubemus ut pro argenti summa quam quis thesauris fuerit illaturus, inferendi auri accipiat facultatem, ita ut pro singulis libris argenti, quinos Solidos inferat. Cod. Theod. De argenti pretio, & Cod. Justin. L. X. Tit. 76.

(7) Cum publica celebrantur officia, sit sportulis nummus argenteus,—nec majorum argenteum nummum fas sit expendere,

retained

retained the name Miliarenſes, though the Pound of Gold was worth but 864.

A ſcholiaſt on the Baſilics tells us, that "One "Siliqua [*of gold*] is worth 12 Folles [*of copper*], or "half a Miliarenſis: therefore 12 Siliquas are half "a Solidus, for the whole Solidus is worth 12 Mi- "liarenſes, or 24 Siliquas (8)." The Róman Pound contained 1728 Siliquas (9), therefore there were 72 of theſe Solidi in the Pound; and each of them being worth 12 Miliarenſes, the Pound of ſilver, which was valued at 5 Solidi, muſt have contained 60 Miliarenſes.

How many Miliarenſes Conſtantine coined out of the Pound of ſilver is no where ſaid; but if the price of Gold was nearly the ſame in his reign, as when 5 Solidi were worth a Pound of ſilver, the Pound muſt have been worth $14\frac{2}{5}$ Pounds of ſilver; and 1000 divided by $14\frac{2}{5}$, gives $69\frac{4}{9}$ for the number of Miliarenſes coined out of the Pound. Therefore it is probable Conſtantine's number was either 69 or 70. If the former, each piece ſhould weigh $73\frac{8}{23}$ Troy grains; if the latter, $72\frac{3}{10}$.

Eiſenſchmid found the larger ſilver of Conſtantine to come up to 90 Páris grains, or $73\frac{83}{100}$ Troy; but the ſmaller (which ſhould be its half) ſeldom amounted to 40 Paris grains, or $32\frac{4}{5}$ Troy; which

quam qui formari ſolet, cum argenti libra una in argenteas ſexaginta dividitur. Cod. Theod. De expenſis ludorum.

(8) Χρὴ γινώσκειν ὅτι τὸ ἓν κεράτιον φόλλεις εἰσὶ ιϐ', ἤτοι μιλιαρησίου τὸ ἥμισυ· τὰ οὖν ιϐ' κεράτιά ἐστι νομίσματος ἥμισυ· τὸ δὲ ἀκέραιον νόμισμα ἔχει μιλιαρήσια ιϐ', ἤτοι κεράτια κδ'. Schol. in L. XXIII. Βασιλικῶν, apud Gronov. L. IV. c. 16. De pecunia vetere.

(9) See Rhemnius Fannius, and others.

 leaves

leaves it uncertain whether 69 or 70 of these Miliarenses were coined out of the Pound. If 69, the proportion of gold to silver was almost $14\frac{1}{2}$ to 1; if 70, $14\frac{2}{7}$ to 1.

In the Glossæ nomicæ, quoted by Gronovius and others, we have an attempt to settle the exchange between the two Miliarenses and the Solidus. The Glossographer, giving an account of the different sums called Folles, says, "There is likewise another "Follis, consisting of the smaller silver which was "paid to the soldiers, and thence called Miliarenses, "each of which is worth $1\frac{3}{4}$ Siliquas [*of gold*], and "the Follis contains 125, which make 218 Siliquas "and 9 nummi; or 109 of the Miliarenses now "current and 9 nummi; which are worth 9 Solidi, "1 Miliarensis, aud 9 nummi, and the Purse of 125 "pieces of this lesser silver, was called a Follis (1.)"

This Gloss appears to come from a different hand from that before quoted, by the absurd etymology here given of the word Μιλιαρήσιον; and the author did not understand his subject. For the Miliarensis of 60 in the Pound, was undoubtedly worth 2 Siliquas of gold, and if Constantine's was worth $1\frac{3}{4}$ when this Glossographer wrote, the two coins must have been in the proportion of 8 to 7, and the exchange

(1) Ἐςὶ δὲ καὶ ἕτερος φόλλις συναγόμενος ἐξ ἀργυρίων λεπτῶν, τῶν τοῖς ςρατιώταις διδομένων, καὶ διὰ τοῦτο μιλιαρησίων καλουμένων· ἔχει δὲ ἕκαςον τῶν τοιούτων λεπτῶν ἀργυρίων κεράτιον ἓν, ἥμισυ, τέταρτον· ὁ δὲ φόλλις, ἀργύρια τοιαῦτα ρκέ. ἃ ποιοῦσι κεράτια σιή, καὶ νούμμους θ'. ἤτοι, πρὸς τὸ νῦν κρατοῦν, μιλιαρήσια ρθ'. καὶ νούμμους θ'. γινόμενα ἐν χαράγμασι νομίσματα θ', μιλιαρήσιον ἓν, καὶ νούμμους θ'. τὰ τοίνυν ρκέ ἀργύρια συνήγετο εἰς ἀπόδεσμον ἕνα, καὶ ἔτος ἐκαλεῖτο φόλλις. Glossæ nomicæ apud Gronov. L. IV. c. 16.

I. made

made in ſmaller numbers without fractions; for 7 Solidi being worth 84 of the new Miliarenſes, would exchange for 96 of Conſtantine's.

But this Follis of 125 Miliarenſes, ſeems to have been intended for a more correct adjuſtment of Conſtantine's ſilver to his gold than 14 Miliarenſes for the Solidus; for it was the true value of 9 Solidi, which, at the rate of 14 for the Solidus, ſhould have exchanged for 126, which was one more than they were worth. And nine was the leaſt number of Solidi that could be exchanged for their true value in Conſtantine's ſilver; which this gloſſographer ſeems not to have known. For the Roman Pound containing 1728 Siliquas, Conſtantine's Miliarenſis was worth but the thouſandth part of them, or $1\frac{728}{1000}$, which multiplied by 125 make juſt 216 Siliquas without a fraction, which were equal to 9 Solidi. Or, ſuppoſing the proportion of Gold to ſilver the ſame when the new Miliarenſes were coined, as when the old ones were, 1000 of the latter and 864 of the former, being each worth a Pound of gold; divide both numbers by 8, and we ſhall have 125 of the old, worth 108 of the new, which paſſed for 9 Solidi.

Having mentioned the Follis, I ſhall endeavour to explain what it was. The word is Latin, and it anciently ſignified a little bag, or purſe; whence it afterward came to be uſed for a ſum of money, and very different ſums were called by that name. Thus, the Scholiaſt on the Baſilics mentions a Follis which was worth but the 24th part of the Miliarenſis; the Gloſſæ nomicæ, one of 125 Miliarenſes, and another of 250 Denarii (which was the ancient Seſtertium);

 and

and three different ſums, of 8, 4, and 2 pound of gold, were each called a Follis (2).

The Gloſſographer laſt quoted makes 9 Nummi equal to $\frac{3}{4}$ of a Siliqua; for 1$\frac{3}{4}$ multiplied by 125, is 218$\frac{3}{4}$, which he calls 218 Siliquas, and 9 nummi. Therefore 12 nummi were equal to a Siliqua; but the ſcholiaſt on the Baſilics makes 12 Folles equal to a Siliqua; conſequently, this Nummus and the ſcholiaſts Follis are the ſame.

The Scholiaſt tells us, the Miliarenſis was equal to 24 Folles of copper, therefore the Ounce of ſilver containing 5 Miliarenſes of 60 in the pound, was worth 120 ſuch Folles.

The Gloſſographer, deſcribing a Follis of 250 Denarii, ſays, it was equal to 312 Pounds, 6 Ounces *of copper* (3). The Denarius of that age was the eighth part of an Ounce, therefore an Ounce of ſilver muſt have been worth 120 Ounces of copper.

But according to the Scholiaſt, the Ounce of ſilver was worth ſo many Folles; therefore the Scholiaſt's Follis was an Ounce of copper. And this Follis being equal to the Gloſſographer's Nummus, that Nummus was likewiſe an Ounce of copper.

By a Reſcript of Arcadius and Honorius in the Theodoſian Code, the treaſury was impowered to

(2) See the Gloſſæ nomicæ, quoted by Gronovius near the end of c. 16. of L. IV.

(3) Φόλλις ςαθμός ἐςι, λεγόμενος καὶ βαλάντιον· ἕλκει δὲ δηνάρια διακόσια πεντήκοντα, τουτ᾽ ἔςι λίτρας τιβ´, καὶ οὐγγίας ἕξ, ὡς ἔχοντος ἑκάςου δηναρίου λίτραν α´ καὶ οὐγγίας γ´. Gloſſæ nomicæ, apud Gronov. L. IV. c. 16.

receive

receive a Solidus for 25 Pounds of copper (4), which ſets the price of that metal at the 125th part of its weight in ſilver. But the ſame Reſcript in Juſtinian's Code (5) for XXV *libris æris*, hath XX *libris æris*. Both cannot be right, perhaps neither; and the true reading may be XXIV *libris æris*, agreeable to theſe commentators.

Eiſenſchmid found Conſtantine's copper money to weigh a quarter of a Roman Ounce (6); therefore the Scholiaſt's Follis, and the Gloſſographer's Nummus contained four of them, as the ancient Nummus contained four *Aſſes*; but whereas the Denarius formerly paſſed for four Nummi, it now paſſed for 15, and the writers of this age ſay it paſſed for 60 *Aſſes* (7).

§ 4. *Of the value of Gold in Greece and Rome.*

Herodotus reckons the value of gold to ſilver in the proportion of 13 to 1 (1). Plato, who wrote about fifty years after him, ſays it was 12 times the value of ſilver (2); and Xenophon, Plato's contemporary, relates, that Cyrus paid Silanus the Ambraciot 3000 Darics for the ten talents he had promiſed

(4) Æris pretia quæ a provincialibus poſtulantur, ita excipi volumus, ut pro XXV. libris æris, Solidus a poſſeſſore reddatur. Cod. Theodoſ. de collatione æris.

(5) Cod. Juſtin. L. X. Tit. 29.

(6) Eiſenſchmid, p. 141.

(7) Hero, Epiphanius, &c.

(1) Herodotus, L. III. § 95.

(2) Plato in his Hipparchus.

him

him (3); which being Babylonian talents, agrees with Plato's eſtimate, as I have ſhewn above.

After the conqueſt of Aſia by Alexander, the immenſe treaſures of the Kings of Perſia circulating in Aſia and Greece, reduced the price of gold to ten times its weight in ſilver, at which it ſeems to have continued two hundred years, or more.

The Romans did not coin gold till above a hundred years after the death of Alexander: and Pliny gives the following account of its firſt coinage. *Aureus nummus poſt annum* LXII *percuſſus eſt quam argenteus, ita ut ſcrupulum valeret Seſtertiis vicenis: quod efficit in libras, ratione Seſtertiorum qui tunc erant, Seſtertios* DCCCC (4). Now if the Scruple was valued at 20 Seſterces, the Pound, inſtead of being worth 900, muſt have been worth 5760 ſuch Seſterces: but if for *Seſtertios* DCCCC, we read *Denarios* DCCCC, the account will be clear and intelligible. The words *ratione Seſtertiorum qui tunc erant*, imply that the Seſterce of that age was different from the Seſterce of Pliny's time: but the quarter of the ſilver Denarius, or Nummus Seſtertius of 4 *Aſſes*, was the ſame at both times, and we know of no other Seſterce but the ancient one of 2½ *Aſſes*. Twenty ſuch Seſterces make 50 *Aſſes* for the value of the Scruple of gold; which multiplied by 288 (the number of Scruples in the Roman Pound) give 14400 *Aſſes* for the value of the Pound of Gold. And reckoning 16 *Aſſes* to the *ſilver* Denarius (which it paſſed for at the time of this coinage) 14400 *Aſſes*

(3) Xenophon in his Expedition of Cyrus, L. I.
(4) Pliny Nat. Hiſt. L. XXXIII. c. 3.

make

make just 900 such Denarii; which is Pliny's number.

That the Romans kept their accounts in copper Sesterces of $2\frac{1}{2}$ *Asses*, long after the silver Sesterce passed for 4, appears not only from this passage, but from what Pliny says of the pay of the Army, that notwithstanding the *silver* Denarius passed for 16 *Asses*, it was paid to the soldier for 10: which implies that the Quæstor's accounts were kept in copper money, as all the public accounts probably were. Cæsar is said to have doubled the pay of the soldiers (5), and it appears from the account Tacitus gives of the mutiny of the legions in Pannonia (6), that at the accession of Tiberius to the empire, their pay was but ten *Asses* a day; and they demanded a Denarius, not upon pretence that the legionary soldiers had ever received so much, but that ten *Asses* were not an equivalent for the dangers and hardships a soldier underwent. Hence 5 *Asses* appear to have been their pay before Cæsar raised it; but if this was their pay on the Quæstor's book, they actually (according to Pliny) received a Quinarius of 8 *Asses*, and Cæsar only nominally doubled it; which is more probable than that their pay at the time he raised it, should be under two-pence three-farthings English a day. Polybius tells us, that in his time the pay of a Roman foot soldier was two Oboles a day; that of a centurion twice as much; and that of a horseman a Drachm (or Denarius) (7). This must be understood of what

(5) Suetonius in Julio, c. 26.
(6) Taciti Annal. L. I. § 17.
(7) Polybius, L. VI. p. 484 of Casaubon's edition.

they

they received, not of their nominal pay on the Quæſtor's book. The foot ſoldier, therefore, was paid at the rate of $5\frac{1}{3}$ *Aſſes* a day, which, in a country where a traveller might have his lodging and all neceſſaries on the road for half an *As* (8), would be great pay, had not their cloathing, arms, and tents, been deducted out of it, as they were (9). But both the public and private riches of the Romans were increaſing very faſt when Polybius wrote, and the prices of all the neceſſaries of life muſt have increaſed in proportion, therefore it is probable that the ſoldier's pay was raiſed to 5 *Aſſes* on the Quæſtor's book, for which they received a Quinarius, before Cæſar augmented it.

If the Pound weight of gold was worth 900 Denarii, 84 of which were coined out of the Pound of ſilver, the value of gold to ſilver muſt have been in the proportion of 900 to 84, or as $10\frac{5}{7}$ to 1. And if this was the value of gold at Rome ſixty-two years after their firſt coinage of ſilver, it proves that no fewer than 84 Denarii were then coined out of the Pound. Now by an article in the treaty with the Etolians, about eighteen years after this firſt coinage of gold at Rome, that people were permitted to pay one third of their tribute in gold, at the rate of one Pound of gold for ten of ſilver (1). Therefore gold was then but ten times the value of ſilver in Greece; and it could not be much higher at Rome, where ſilver was eſteemed the more uſeful metal, as appears by the limitation of the ſum to be paid in gold, to one

(8) Polybius, L. II. p. 103.
(9) Polybius, L. VI. p. 484. Taciti Annal. L. I. § 17.
(1) Polybius, Excerpt. Legat. § 28. Livy L. XXXVIII.

third

third of the whole; and Pliny obſerves, that the Romans always required the tribute they impoſed on conquered countries ſhould be paid in ſilver, not in gold (2); therefore it is not probable that gold ſhould bear a much higher price at Rome than elſewhere, as it would, according to this account of its firſt coinage, if fewer than 84 Denarii were coined out of the Pound of ſilver.

There is another paſſage in Pliny relating to the value of gold, which requires correction. Speaking of the Byſſine thread, he ſays, *Quaternis denariis ſcripula ejus permutata quondam, ut auri, reperio* (3). When 96 Denarii were coined out of the Pound, each of them weighed 3 Scruples; therefore 4 Denarii weighed 12 Scruples, which was nearly the value of a Scruple of gold when Pliny wrote. But Pliny knew no ſuch Denarius; for he ſays, the lawful weight of that coin was the eighty-fourth part of the Pound; beſides, he ſpeaks here of former times. Therefore for *Quaternis*, we ſhould read *Ternis*; for 3 Denarii of 84 in the Pound weighed $10\frac{2}{7}$ Scruples, which was nearly the ancient value of a Scruple of gold.

From a paſſage in Tacitus, compared with Suetonius, we learn that in Galba's time the Aureus paſſed for 25 Denarii; the former ſays ——*ut per ſpeciem convivii quoties Galba apud Othonem epularetur, cohorti excubias agenti viritim centum nummos divideret*; which the latter expreſſes thus, *quoties cœnâ principem exciperet, Aureos excubanti cohorti viritim dividebat* (4). But 100 Nummi were equal to 25 Denarii; there-

(2) Pliny, Nat. Hiſt. L. XXXIII. c. 3.
(3) L. XIX. c. 1.
(4) Taciti Hiſt. L. I. § 24. Suetonius in Othone, c. 4. See alſo Dio Caſſius, L. LV.

fore when 40 Aurei were coined out of the Pound of gold, and 84 Denarii out of the Pound of ſilver, the Pound of gold paſſing for 1000 Denarii, was worth $11\frac{19}{21}$ Pounds of ſilver.

When the Aureus of 45 in the Pound paſſed for 25 Denarii of 96 in the Pound, the proportional value of gold to ſilver was as 375 to 32, or a little under $11\frac{3}{4}$ to 1.

Suetonius tells us, that Cæſar brought ſo great a quantity of gold from Gaul, that he ſold it throughout Italy and the Provinces for 3000 nummi the Pound (5). 3000 nummi make 750 Denarii; and 750 is to 84, as $8\frac{13}{14}$ to 1. This was its price as merchandize, when the market was overſtocked, and the ſeller in haſte to diſpoſe of his goods; but what effect it had on the coin, we do not know.

By the diminution of the Aureus for above half a century before the reign of Conſtantine (6), the price of gold appears to have been riſing, till it came to above 14 times its weight in ſilver; for five Solidi of 72 in the Pound, being valued at a Pound of ſilver (7), the proportion between the two metals was as $14\frac{2}{5}$ to 1.

§ V. *Of the value of the ancient Greek and Roman money.*

IT does not appear that either the ancient Greeks or Romans allayed their money, but coined the

(5) Suetonius in Julio, c. 54.
(6) See the Pembroke Collection, from Tab. XX. to XXIV.
(7) See Cod. Juſtinian. L. X. Tit. 76. quoted above.

metals

It

metals as pure as the refiners of thoſe times could make them: for though Pliny mentions two inſtances of the contrary at Rome (1), the example was not followed, till the later Emperors debaſed the coin: and his expreſſion, *miſcentur æra falſæ monetæ*, ſhews he thought the practice illegal.

Though the ancients had not the art of refining ſilver, in ſo great perfection as it is now practiſed, yet, as they mixed no baſe metal with it, and eſteemed what they coined to be fine ſilver, I ſhall value it as ſuch.

Sixty-two Engliſh Shillings are coined out of 11 ounces 2 p. wt. Troy of fine ſilver, and 18 p. wt. allay. Therefore, the Troy grain of fine ſilver is worth $\frac{62}{111}$ths of a Farthing. Hence the Attic Drachm of $66\frac{1}{2}$ grains will be found worth a little more than Ninepence farthing; the Obole, a little more than Three halfpence; and the Chalcus, about $\frac{7}{9}$ of a Farthing.

But, for the reduction of large ſums to Engliſh money, the following numbers are more exact.

	£.	s.	d.
The Attic Drachm	0.	0.	9,286
The Mina	3.	17.	4,6
The Talent	232.	3.	0.

Hence the Mina expreſſed in Pounds Sterling and decimals of a Pound will be £. 3,869; the Talent £. 232,15.

The Romans reckoned by *Aſſes* before they coined ſilver, after which they kept their accounts in Seſterces. The word Seſtertius is an adjective, and ſigni-

(1) Pliny Nat. Hiſt. L. XXXIII. c. 3. & c. 9.

fies two and a half of any ſubſtantive to which it refers. In money matters its ſubſtantive is either *As*, or pondus; and Seſtertius *As*, is two *Aſſes* and a half; Seſtertium pondus, two pondera and a half, or 250 Denarii (2).

When the Denarius paſſed for ten *Aſſes*, the Seſterce of 2½ *Aſſes* was a quarter of it; and the Romans continued to keep their accounts in theſe Seſterces long after the Denarius paſſed for ſixteen Aſſes; til, growing rich, they found it more convenient to reckon by quarters of the Denarius, which they called Nummi, and uſed the words Nummus and Seſtertius, indifferently as ſynonymous terms, and ſometimes both together, as Seſtertius nummus; in which caſe, the word Seſtertius, having loſt its original ſignification, was uſed as a ſubſtantive; for Seſtertius nummus was not two Nummi and a half, but a ſingle Nummus of four *Aſſes*.

They called any ſum under 2000 Seſterces ſo many Seſtertii, in the maſculine gender; 2000 Seſterces they called duo or bina Seſtertia, in the neuter; ſo many quarters making 500 Denarii, which was twice the Seſtertium; and they ſaid dena, vicena, &c. Seſtertia, till the ſum amounted to a thouſand Seſtertia, which was a million of Seſterces. But, to avoid ambiguity, they did not uſe the neuter Seſtertium in the ſingular number, when the whole ſum amounted to no more than 1000 Seſterces, or one Seſtertium.

They called a million of Seſterces Decies nummûm, or Decies Seſtertiûm, for Decies centena mil-

(2) See Gronovius, De pecunia vetere, L. I. c. 4.

lia nummorum, or Seſtertiorum (in the maſculine gender) omitting centena millia, for the ſake of brevity: they likewiſe called the ſame ſum Decies Seſtertium (in the neuter gender), for Decies centies Seſtertium, omitting Centies for the reaſon above-mentioned; or ſimply Decies, omitting centena millia Seſtertiûm, or centies Seſtertium; and with the numeral adverbs, Decies, Vicies, Centies, Millies, and the like, either centena millia, or centies, was always underſtood.

Theſe were their moſt uſual forms of expreſſion, though for Bina, Dena, Vicena Seſtertia, they frequently ſaid, Bina, Dena, Vicena millia nummûm (3); and Cicero, in the paſſage quoted in the margin, hath uſed Mille Seſtertia, for Decies Seſtertium. But Gronovius ſays, that expreſſion is not to be found elſewhere, and ſuppoſes it to be a falſe reading.

If the Conſular Denarius contained 60 Troy grains of fine ſilver, it was worth ſomewhat more than Eightpence farthing and a half Sterling; and the *As*, of ſixteen to the Denarius, a little more than a Halfpeny.

To reduce the ancient Seſterces of $2\frac{1}{2}$ *Aſſes*, when the Denarius paſſed for 16, to pounds Sterling, multiply the given number by 5454, and cut off ſix figures on the right hand for decimals.

To reduce Nummi Seſtertii, or quarters of the Denarius, to pounds Sterling; if the given ſum be Conſular money, multiply by 8727, and cut off

(3) Suetonius in Julio, c. 38. Cicero in Verrem, L. I. § 14.

ſix

ſix figures on the right hand for decimals; but for Imperial money, diminiſh the ſaid product by one eighth of itſelf.

For example, Cicero ſays, Verres had received *Vicies, ducenta triginta quinqe millia, quadringentos decem & ſeptem nummos,* or 2.235.417 Seſterces: this being Conſular money, multiply by 8727, and cutting off ſix figures from the product, £. 19508,484159, or 19508 *l.* 9 *s.* 8 *d.* will be their value in Engliſh money.

Again, Suetonius relates, that when Veſpaſian came to the Empire, he found the treaſury ſo exhauſted, that he declared Quadringenties millies, or 40.000.000.000 nummi, were wanted to ſupport the Government (4). This was Imperial money, which, multiplied by 8727, and cutting off ſix figures from the product, gives 349.080.000
One eighth of which, 43.635.000
being ſubtracted, leaves £. 305.445.000 Pounds Sterling.

But Budæus ſuppoſes, that for *Quadringenties millies,* we ſhould read *Quadragies millies,* which reduces it to £. 30.544.500, and is a much more probable ſum.

If the Miliarenſes of 60 in the pound were fine ſilver, and weighed 84 Troy grains, they were worth 46,918918 . . . Farthings and decimals, or almoſt 11 pence 3 farthings Sterling; and the Solidus paſſing for 12 of them, was worth a little more than 11 *s.* 8 *d.* 3 *f.*

The Pound of gold was worth 864 of theſe Miliarenſes, amounting to 40537,94 Farthings and decimals,

(4) Suetonius, in Veſpaſiano, c. 16.

cimals, which, divided by 1000, give 40,538, or above 10 pence and half a farthing for the value of Conſtantine's Miliarenſis in Engliſh money.

The Conſtantinopolitans kept their accounts in Solidi, which are reduced to pounds Sterling, by multiplying the given number by 58648, and cutting off five figures on the right hand for decimals.

CONCLUSION.

THE Greeks had no money at the time of the Trojan war; for Homer repreſents them as trafficking by barter (1), and Priam (an Aſiatic) weighs out the ten talents of gold, which he takes to ranſom his ſon's body of Achilles (2).

This ponderal Talent was very ſmall, as appears from Homer's deſcription of the Games at the Funeral of Patroclus, where two Talents of gold are propoſed as an inferior prize to a mare with foal of a mule. Whence I conclude it was the ſame that the Dorian Colonies carried to Sicily and Calabria; for Pollux tells us, from Ariſtotle, that the ancient Talent of the Greeks in Sicily contained 24 Nummi, each of which weighing an Obole and a half, the Talent muſt have weighed ſix Attic Drachms, or three Darics; and Pollux elſewhere mentions ſuch a Talent of gold. But the Daric weighed very little more than our Guinea; and if 2 Talents weighed about 6 Guineas, we may reckon the mare with foal worth 12 *l*. which was no improbable price, ſince

(1) Iliad H. ver. 472.
(2) Iliad Ω. ver. 232.

we

we learn from a paſſage in the Clouds of Ariſtophanes, that, in his time, a running horſe coſt 12 Minas, or above 46 pounds Sterling.

Therefore, this ſeems to have been the ancient Greek Talent, before the art of ſtamping money had introduced the greater Talents from Aſia and Egypt.

Herodotus tells us, the Lydians were reputed to be the firſt that coined gold and ſilver money (3); and the Talent, which the Greeks called Euboïc, certainly came from Aſia. Therefore, the Greeks learned the uſe of money from the Aſiatics.

The Romans took their weights and their money, either from the Dorians of Calabria, or from Sicily; for their Libra, Uncia, and Nummus, were all Doric words, their Denarius was the Sicilian Δεκάλιτρον; and Pollux tells us, from Ariſtotle, that the Sicilian Nummus was a quarter of the Attic Drachm (4); and the Romans called a quarter of their Denarius by the ſame name.

The weights I have produced of the Greek and Roman coins, ſo fully prove the ancient Attic Drachm to have been heavier than the Denarius, that it may ſeem ſuperfluous to quote any authorities in ſupport of their evidence: nor ſhould I do it here, but in order, at the ſame time, to anſwer an objection which may be made to the weight I have aſſigned to the Attic Drachm.

In the treaty between the Romans and Antiochus, recorded by Polybius and Livy (5), the weight of

(3) Herodot. L. I. § 94.

(4) See Pollux, L. IX. c. 6. § 80, 81. 87. & L. IV. c. 24. § 175.

(5) Polybius, Excerp. Leg. § 35. Livy, L. XXXVIII. c. 38.

the

the Euboïc talent is ſet at 80 Roman Pounds. The Talent is not, indeed, called Euboïc, in the Treaty, which was ſuperfluous when its weight was ſpecified; but both hiſtorians, in relating the terms offered by Scipio to Antiochus, on which this treaty was founded, call it ſo (6). Therefore in Livy's recital of the treaty, for *Argenti probi* XII *millia Attica talenta*, we ſhould read, with Gronovius, *Argenti probi Attici* XII *millia talenta.*

In § II of this diſcourſe, I have endeavoured to prove that the Euboïc Talent was equal to the Attic; and if ſo, it contained 6000 Attic Drachms; but 80 Roman pounds contained 6720 Denarii; therefore, according to this treaty, the weight of the Attic Drachm muſt be to that of the Denarius, as 6720 to 6000.

And, even if the Euboïc Talent was heavier than the Attic, in the proportion of 72 to 70, the Attic Drachm would ſtill be heavier than the Denarius; for in that caſe, the Euboïc talent ſhould contain 6171 Attic Drachms, and the two coins would be in the proportion of 6720 to 6171.

But an anonymous Greek fragment publiſhed by Montfaucon (7), makes 100 Attic Drachms equal to 112 Denarii; which proportion of the two coins being the ſame with that of 6000 to 6720, ſeems to have been taken from this treaty; and if it was, that writer certainly thought the Talent therein mentioned, equal to the Attic.

(6) Polyb. Exc. Leg. § 24. Livy, L. XXXVII. c. 45.
(7) Analecta Græca, p. 393. Paris, 1688, in Quarto.

This proportion, however, does not agree with the weights I have aſſigned to the two coins; for if the Denarius weighed 60 Troy grains, and the Attic Drachm 66½, 6650 Denarii ſhould weigh 6000 Attic Drachms, or a Talent; but this number of Denarii is deficient of 80 Roman Pounds, by juſt 10 Ounces.

Now, this adjuſtment of the Talent to Roman Pounds, was probably occaſioned by the Greeks attempting to impoſe light weights upon the Romans, who finding the Talent to exceed 79 Pounds, might take what it wanted of 80 in their own favour, to puniſh the Greeks for their unfair dealing. Or, the ſtandard the Romans pitched upon for the Euboïc Talent might be ſomewhat over-weight; and the Coin of Lyſimachus above-mentioned, makes this conjecture not improbable; for that in the poſſeſſion of Mr. Duane weighs 537,6 Troy grains, which divided by 8 gives a Drachm of 67,2, exactly the weight required by this treaty, ſuppoſing the Denarius to weigh 60 grains. But the gold coins of Philip and Alexander are ſo perfect, and ſo correctly ſized, that their authority is indiſputable; and if the mean Drachm of 66½ grains derived from them were ſomewhat too ſmall, it cannot be increaſed by above a quarter of a grain.

Therefore, I ſuppoſe the great weight given to the Talent by this Treaty, may ariſe, partly from too heavy a ſtandard, and partly from the Romans taking the turn of the ſcale in their own favour.

After the Romans became maſters of Greece and Aſia, the Athenians might find it their intereſt to lower their Drachm to the weight of the Denarius,

rius, long before they were reduced into the form of a Roman Province, by Veſpaſian. When they did this, and whether they did it gradually, as may ſeem probable from ſome Tetradrachms now remaining, is uncertain; but that they did ſo, ſooner or later, cannot be doubted.

Pliny and Scribonius Largus expreſly ſay, the Attic Drachm was equal in weight to the Denarius (8): and A. Gellius, who, having reſided long at Athens, could not be ignorant of the value of the current money of that city, ſays 10000 Drachms were in Roman money, ſo many Denarii (9). And the Attic gold coin above-mentioned, in the Britiſh Muſeum, is a proof of their having reduced their money to the Roman ſtandard.

Theſe are the moſt authentic teſtimonies that the two Coins ever were equal; for though all the Greek writers of Roman affairs, call the Denarius, Drachma, it is no proof of their equality; for one being the current coin of Rome, as the other was of Athens, and not very unequal in value, a Greek might conſider the Denarius, as the Drachma of Rome, and tranſlate it by that word, which was familiar to his countrymen; as we call the French Ecû, or the Roman Scudo, a Crown; which hath no more affinity to the French or Italian names, either in ſound or ſignification, than Drachma hath to Denarius.

(8) Pliny, Nat. Hiſt. at the end of L. XXI. Scr. Largus, in his Preface.

(9) A. Gellius, L. I. c. 8. Hoc facit nummi noſtratis Denariûm decem millia.

But the opinion that the ancient Attic Drachm was really equal to the Denarius, hath occafioned much confufion in the writers on this fubject. Hence it is, that Rhemnius Fannius hath told us of an Attic Libra, or Mina (for he calls it by both names) of 75 Drachms; for the Roman Pound being reckoned to weigh 75 ancient Attic Drachms, Fannius, fuppofing them to be equal to fo many Denarii, concluded it muft be an Attic weight, as it could not, on fuch fuppofition be the Roman Pound.

An anonymous fragment fays, *The Attic Mina weighs* 12 *Ounces, the other* 16 (1): the former was the Roman Pound; the latter, the ancient Attic Mina. Which makes it probable, that when the Athenians reduced their money to the Roman ftandard, they adopted the Roman Weights; and this may have occafioned many miftakes in the later writers.

The great difproportion between the copper and filver money, when the Romans firft coined the latter, hath induced many to believe that the firft Denarii muft have been heavier than the eighty-fourth part of their Pound; thinking it incredible that filver fhould ever be valued at 840 times its weight of copper. But they can produce no ancient author of credit, in fupport of this opinion.

On the contrary, Dionyfius of Halicarnaffus, who made diligent enquiry into the antiquities of Rome, while all, or moft of the evidences relating to them were in being, giving an account of the firft infti-

(1) See the Appendix to Stephens's Greek Thefaurus, col. 219.

tution

tution of the Claſſes by Servius Tullius, hath valued what the Romans called *centum millia æris*, or 100000 Pounds weight of copper, no higher than 100 Minas (2), which is at the rate of a Drachm for every 10 Pounds of copper; and this valuation he muſt have taken from the price of copper when the Romans firſt coined ſilver, reckoning the Denarius of that time equal to what it was when he wrote. But had the firſt Denarius been Didrachmal or Tetradrachmal, ſo well-informed a writer muſt have known it, and would have valued the copper money accordingly. Neither is it probable that Pliny, who hath given ſo particular an account of the diminution of the *As*, ſhould omit that of the Denarius.

But it is not impoſſible that ſilver might be ſo ſcarce at Rome when it was firſt coined there, as to bear the above-mentioned proportion to copper; and the Romans, not being a trading people, might have no regard to its value elſewhere. It is likewiſe probable, that, through ignorance and inexperience in money matters, they ſet too high a value on it at firſt; which ſeems to have been the caſe, by its quick reduction from 840 times its weight in copper, to 140, in leſs than thirty years; and again to 112 in between twenty and thirty years more; and not very long after to 56, at which price it remained during the continuance of the republican government.

But we are little intereſted in the weight of the Denarius for the firſt ſixty years after it was coined; and I have ſhewn that when the Romans began to

(2) Compare Dionyſius with Livy.

coin

coin gold, it did not exceed the eighty-fourth part of their Pound.

The learned have differed much concerning the grammatical conſtruction and uſe of the word *Pondo*; moſt of them have ſuppoſed it to be a neuter indeclinable; but Gronovius hath produced many authorities to ſhew that it was the old ablative caſe of *Pondus, pondi*, for which they afterward uſed *Pondere*. Livy has, *Coronam auream libram pondo*, and the like in many places. Columella, *medicaminis pondo unciam*. Celſus, *pondo denariorum trium*. And Plautus in Menæchmis, *Pondo duum nummum*. In all which *pondo* ſeems to be the ablative caſe for *pondere*. And Feſtus tells us, *Centenas pondo dicebant antiqui, referentes ad libras* (3). Thus Livy ſays, *ſex millia pondo*, for, *ſex millia librarum pondere*, and *Pondo bina & ſelibras*, for, *Pondere bina librarum pondera & ſelibras*. In the former of theſe paſſages, Livy ſeems to have valued the Libra at 100 Denarii. For relating how Scipio was accuſed of having received a bribe from Antiochus of *ſex millia pondo auri, quadringenta octoginta argenti*, he calls it in a round ſum *Ducenties quadragies*, or 24000 Seſtertia (4). Now reckoning 100 Denarii to the Libra, and the value of gold decuple that of ſilver, it ſhould amount to 24192 Seſtertia; whereas reckoning 84 Denarii to the Libra, it would amount to no more than 20352. And Plutarch in his Life of Fabius, tranſlates what Livy calls *Pondo bina & ſelibras*, by 250 Drachms, which is a Seſtertium.

(3) Gronovius, De pec. vet. L. I. c. 6.
(4) Livy, L. XXXVIII. § 55.

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The learned Budæus, and others after him, have called this ſum of 100 Denarii, *Libra centenaria*, and *Libra nummaria*; though he confeſſes he had never found either the word Libra or Pondo uſed to ſignify a ſum of money; but always, when applied to gold or ſilver, a weight of Plate or Bullion; and how the Libra, which certainly weighed but 84 Denarii when Livy wrote, ſhould be valued at 100, is a paradox I cannot account for.

XLIX. *Deſcription of a Method of meaſuring Differences of Right Aſcenſion and Declination, with* Dollond's *Micrometer, together with other new Applications of the ſame: By the Rev.* Nevil Maſkelyne, *B. D. F. R. S. Aſtronomer Royal.*

Read Dec. 12, 1771.

THE divided object glaſs micrometer, as happily applied by the late Mr. John Dollond to the object end of a reflecting teleſcope, and now with equal advantage adapted by the preſent Mr. Dollond, his ſon, to the end of an achromatic teleſcope, is ſo eaſy of uſe, and affords ſo large a ſcale, that it is generally looked upon by aſtronomers as the moſt convenient and exact inſtrument for meaſuring ſmall diſtances in the heavens. But, as the common wire micrometer is peculiarly adapted for meaſuring differences of right aſcenſion and declination of celeſtial objects, and is not near ſo convenient or exact for meaſuring their abſolute diſtances; ſo on the contrary the object glaſs micrometer is peculiarly fitted for meaſuring diſtances, and has, I believe, generally been ſuppoſed incapable of or unfit for meaſuring differences of right aſcenſion and declination. Thus the two micrometers, as mutually ſupplying each other's defects, have been eſteemed both

both equally neceſſary in their turn to be uſed by the practical Aſtronomer, and conſequently to have a place in every well-furniſhed Obſervatory. Far be it from me to ſay any thing to the diſparagement of either of theſe valuable inſtruments, or to envy it the place which it is ſo juſtly entitled to. Every Aſtronomer, who has time and inclination for making a variety of obſervations, would undoubtedly wiſh to be ſupplied with, and to make uſe of both. But, as every perſon deſirous of making obſervations for his own amuſement or public utility may not happen actually to be furniſhed with, nor chuſe to be at the expence of providing himſelf with both, it is certainly a very deſirable thing, if he could be enabled to make that uſe of the inſtrument he has, which might ſupply, in ſome meaſure at leaſt, the want of the other which he has not. Therefore, as I find that the object-glaſs micrometer may be applied with little trouble and but ſmall additional expence to the meaſuring differences of right aſcenſion and declination, with an exactneſs little, if at all, inferior to what they can be obtained with the common micrometer, I propoſe to give here the directions neceſſary to be followed when it is uſed in this manner. I ſhall afterwards ſhew how differences of right aſcenſion and declination between the limbs of the Sun and Venus or Mercury, and diſtances of the limbs both in lines parallel and perpendicular to the equator, may alſo be obſerved in the tranſits of theſe planets over the Sun. Examples of the ſecond and third of theſe methods may be ſeen in the obſervations of the late tranſit of Venus at the North Cape, and in the South Seas, made according to theſe di-

 rections,

rections, which were previoufly communicated to the obfervers.

A fmall addition will be neceffary to be made to the apparatus of the object-glafs micrometer, to enable it to anfwer thefe purpofes, viz. a cell, containing two wires interfecting each other at right angles, placed in the focus of the eye-glafs of the telefcope, and moveable round about, by the turning of a button. Let E N W S, TAB. XVI. Fig. 1. reprefent the field-bar of the telefcope, E W and N S two wires interfecting each other at right angles at C, and moveable about the fame as a center, in manner above-mentioned. Suppofe it be required to meafure the difference of right afcenfion and declination of two ftars, whofe difference of declination does not exceed the extent of the fcale of the micrometer, and the diftance of the meridians paffing through the ftars does not exceed C W, the femidiameter of the field of the telefcope. Turn the wires E W, N S about, till one of the ftars (the wefternmoft ftar will generally be beft for this purpofe) runs exactly along the wire E W, by the diurnal motion. Then feparate the two fegments of the divided object-glafs to a convenient diftance, and turn the micrometer round about, by means of the proper handle, till the two images of the fame ftar, formed by the two fegments of the object-glafs, pafs the horary wire N S at the fame inftant. Laftly, partly by feparating the glaffes, and partly by touching the rack-work fcrews of the ftand of the telefcope, caufe the fouthernmoft image of the northernmoft ftar, and the northernmoft image of the fouthernmoft ftar, to appear both upon and run along the wire E W, as A, B. The numbers ftanding upon the fcale

of

Fig. I.

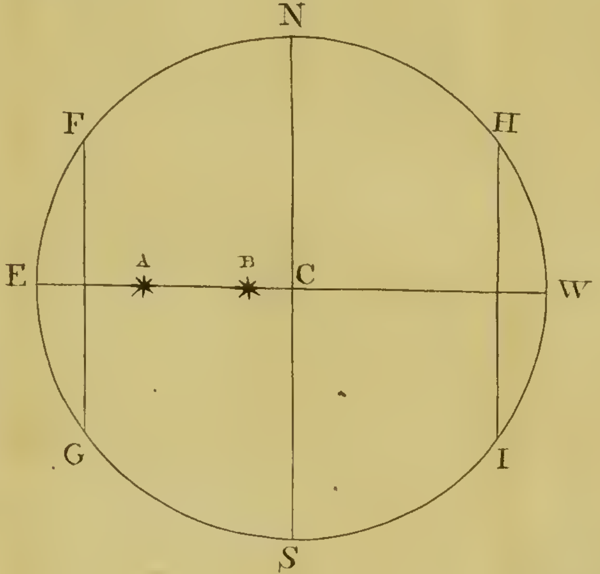


Fig. II.

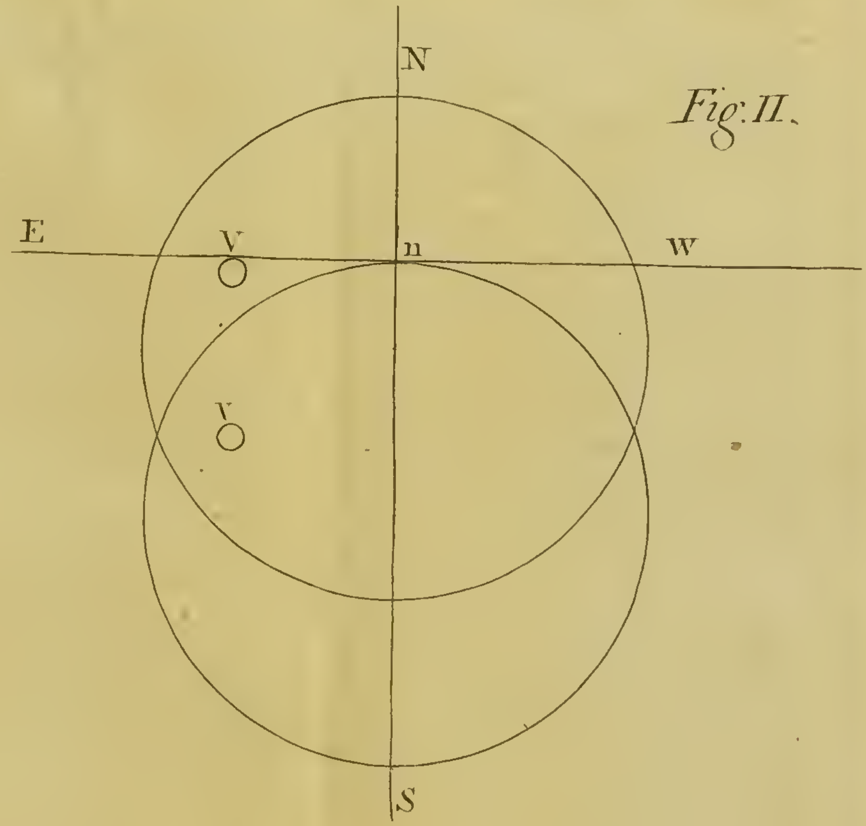


Fig. III.

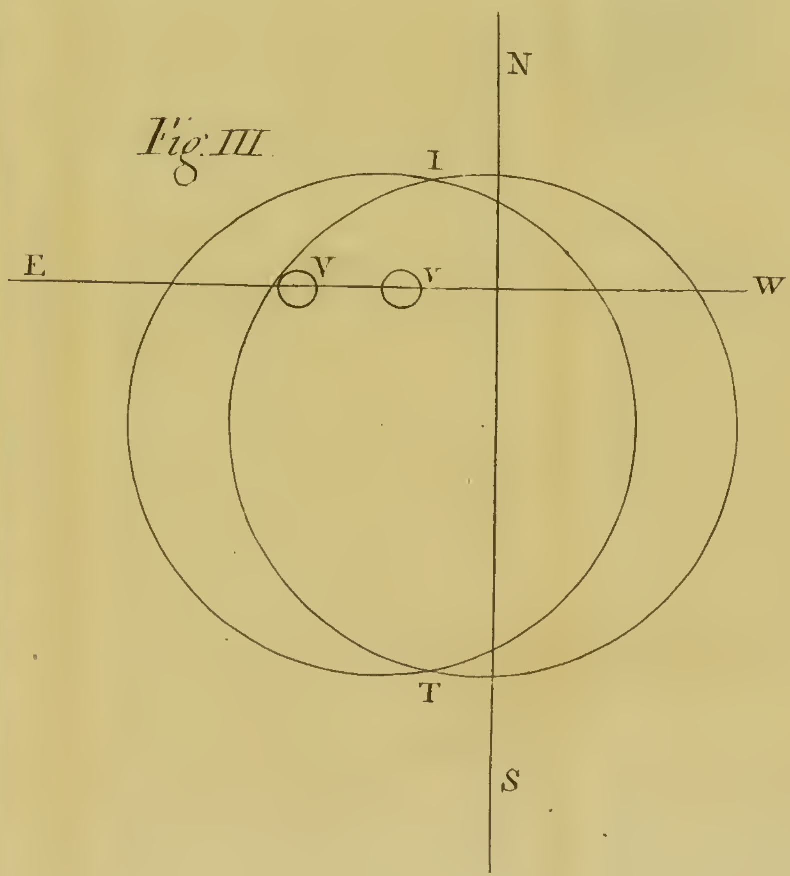
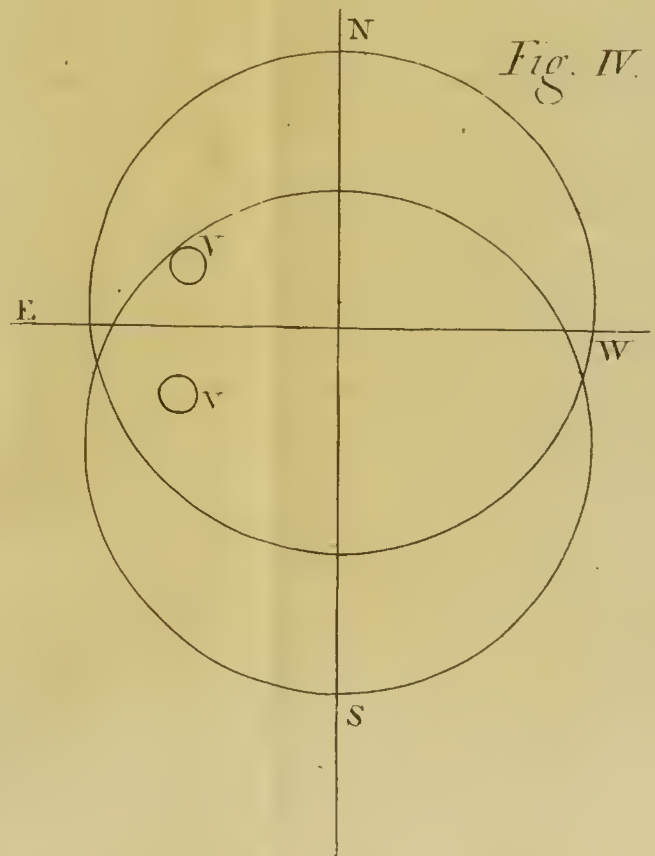


Fig. IV.



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of the micrometer will ſhew the difference of declination of the ſtars; and if the times be noted when they paſs the horary wire N S the difference of the times will give the difference of their right aſcenſion. For E W on account of the ſtar's running along it, is parallel to the equator; and conſequently N S which is perpendicular to it, repreſents a meridian or horary circle. And becauſe the two images of the ſame ſtar paſs the horary wire N S at the ſame inſtant, it follows that the centres of the two ſemicircular glaſſes lie in the ſame meridian, and conſequently when A, B, the two contrary images of the two ſtars are brought to the ſame parallel of declination E W, the ſcale will ſhew the difference of their declinations. And, for the ſame reaſon, the times of the images of the two ſtars paſſing the meridian wire N S will not be affected by the ſeparation of the glaſſes of the micrometer, and conſequently the difference of the times will give the difference of their right aſcenſion. It will be eaſily underſtood that in performing the operations above deſcribed, it will be neceſſary from time to time to turn the ſcrews of the rack-work which move the whole teleſcope together. Theſe operations will be much facilitated and rendered more exact, if the teleſcope be ſupported by and moveable on a polar axis; for the wires and micrometer may be thereby more readily brought into the requiſite poſitions, and the turning of the teleſcope about in order to follow the diurnal motion will not diſturb thoſe poſitions; which will afford this farther advantage to the obſerver, of being able to repeat the obſervations without loſs of time.

If two additional horary wires F G, H I parallel to N S be placed near E and W the two extremities of the wire, E W the adjuſtment of the wires and micrometer may be more readily performed, and the obſervation may be made on two ſtars, although their meridian diſtance from one another ſhould be almoſt equal to E W the diameter of the field of the teleſcope. It is evident, that if two ſtars be thus obſerved whoſe difference of declination is well ſettled, the value of the ſcale of the micrometer may be thereby determined.

In the foregoing directions it has been ſuppoſed that the images of the two ſtars can be brought to appear within the field of the teleſcope on the wire E W at the ſame time; but this is not abſolutely neceſſary. For if the micrometer be ſet to the difference of declination nearly, and then the ſtar which paſſes firſt through the teleſcope be made to run along the wire E W by touching one of the handles of the rack-work of the teleſcope, and afterwards the other ſtar, when it comes into the teleſcope, be brought to the wire E W by altering the opening of the glaſſes of the micrometer, the difference of the declination will be had, by taking half the ſum of the numbers ſhewn by the micrometer, at the two ſeparate obſervations of the two ſtars on the wire E W. This will be true, in caſe it can be depended upon that the two ſemicircular glaſſes recede equally in contrary directions; which may indeed be doubted, the work on which the motion of the glaſſes depends not being deſigned for ſuch a purpoſe, and therefore probably not made ſufficiently accurate for it.

The manner in which Mr. Dollond has contrived the motion of the glaſſes in his new improvement of the object-glaſs micrometer intirely obviates this

difficulty,

difficulty, and the difference of right aſcenſion and declination of any two ſtars or other points in the heavens may be thereby accurately meaſured, let the difference of right aſcenſion be what it will, provided the difference of declination does not exceed the extent of the ſcale of the micrometer; and thus the object-glaſs micrometer is put pretty much on a footing with the common micrometer, even with reſpect to the meaſuring right aſcenſions and declinations.

The difference of right aſcenſion and declination between Venus or Mercury and the Sun's limb, in their tranſits over the Sun, are to be obſerved nearly in the ſame manner as the difference of right aſcenſion and declination of two ſtars. But the proceſs will perhaps be rendered clearer by the following deſcription.

1. Turn the moveable wires E W, N S, into ſuch a poſition that the Sun's North limb *n* (ſee fig. II.) or the planets North limb V may run along the wire E W, which thereby becomes a tangent to the peripheries of their diſcs.

2. The ſemicircular glaſſes being ſeparated to a convenient diſtance, turn the micrometer about, till the two images of the planet V, *v*, paſs over the horary wire N S at the ſame inſtant.

3. Separate the glaſſes of the micrometer to that diſtance, that the North limb V of the Northernmoſt image of the planet may touch the wire E W at the ſame time that the Northernmoſt limb *n* of the Southernmoſt image of the Sun touches the ſame wire; and the ſcale of the micrometer will ſhew the difference of declination of the Northern limbs of the

the Sun and planet. In like manner, if the glaſſes of the micrometer be opened to a greater or leſs diſtance (according as the planet is nearer the North or South limb of the Sun) every thing elſe remaining unmoved, the difference of declination of the Southern limbs of the Sun and planet may be obſerved, by bringing the Southernmoſt limb of the Southernmoſt image of the planet to run along the wire EW, at the ſame time that the Southernmoſt limb of the Northernmoſt image of the Sun runs along the ſame. Half the difference of theſe two meaſures (if taken immediately after one another) is equal to the difference of the declination of the centers of the Sun and planet at the intermediate time, without any regard to the quantities of the diameters of the Sun or planet, or the error of adjuſtment of the micrometer.

The difference of the tranſits of the Eaſtern or Weſtern limbs of the Sun and planet will give the difference of right aſcenſion, as in the common micrometer.

Inſtead of differences of right aſcenſion, diſtances of the planet from the Sun's limb in lines parallel to the equator may be more accurately obſerved as follows.

The glaſſes being ſeparated to a convenient diſtance, turn both the wires and micrometer about, ſo that the two images of the planet may both run along the wire E W (ſee Fig. III) and ſeparate the glaſſes, ſo that V one of the images of the planet may touch the limb of the Sun to the Eaſt or Weſt, or rather both alternately. Or perhaps the following method may be preferable : ſeparate the two images of the Sun to any convenient diſtance, ſo as to produce a conſiderable angle of interſection of the circumferences at I and T;

Turn the wires about, ſo that the planet's centre, North, or South limb, may run along the wire E W; Then turn the micrometer about till the two interſections I T paſs the horary wire N S at the ſame inſtant, and the micrometer will be in a proper poſition for meaſuring diſtances in a line parallel to the equator; and the diſtance of the planet from the Sun's limb in a line parallel to the equator will be obtained by only bringing the glaſſes nearer together, or ſeparating them farther, till the planet's limb is in contact with the Sun's limb. If diſtances of the planet's near limb from the Sun's limb be thus taken to the Eaſt and Weſt alternately, and reduced to a given time, by allowing for the motion of the planet by calculation, half the difference of the two reduced meaſures will be the diſtance of the planet's centre from the middle of the chord of the Sun's diſc paſſing through the planet's centre parallel to the equator at the given time, without any regard to the quantities of the diameters of the Sun or planet, or the error of the adjuſtment of the micrometer. It may be proper to remark, that when the planet is brought to touch the Sun's limb, the point of contact will be North or South of the planet's centre according as the planet itſelf is North or South of the Sun's centre.

In like manner, diſtances of Venus or Mercury from the Sun's limb may be meaſured in lines perpendicular to the equator, ſee Fig. IV. (the micrometer being brought into the proper poſition, in the very ſame manner as for meaſuring the difference of declination from the Sun's north or ſouth limb, before deſcribed); and if the planet be brought into contact with

with the Sun's limb to the north and ſouth alternately, half the difference of the two meaſures, reduced to a given time by allowing for the motion of the planet by calculation, will be the difference of declination of the centers of the Sun and planet at that time, without any regard to the diameters of the Sun or planet, or the error of adjuſtment of the micrometer. And this would be a better obſervation than meaſuring the difference of declination of the limbs of the Sun and planet by bringing them both in contact with the ſame wire parallel to the equator deſcribed above; as the meaſuring diſtances from the Sun's eaſt or weſt limb in lines parallel to the equator is a better obſervation than meaſuring differences of right aſcenſion of the limbs by time.

By theſe two obſervations of diſtances of an inferior planet from the Sun's limb in lines parallel and perpendicular to the equator, its true place with reſpect to the Sun's center may be accurately aſcertained during any part of its tranſit over the Sun's diſk; and conſequently its neareſt approach to the Sun's center and the time of the ecliptic conjunction may be deduced with great exactneſs, although the middle of the tranſit ſhould not be ſeen, and the Sun ſhould be viſible only for a ſmall ſpace of time ſufficient for taking theſe obſervations.

The following order of making the ſeveral obſervations with Dollond's micrometer in the late tranſit of Venus was recommended to the obſervers who went on the part of the Royal Society to the North Cape and to the South Sea, which may ſerve to elucidate their obſervations. See Phil. Tranſ. Vol. LIX. p. 266, 267. and this Vol. LXI. p. 397, 418

Inſtructions

Inſtructions to the like effect were alſo given to the other obſervers, ſent by the Royal Society to Hudſon's Bay and the North of Ireland, on the ſame occaſion. See Phil. Tranſ. Vol. LIX. p. 480—482. and Vol. LX. p. 488.

" 1ſt, Immediately after the firſt internal contact,
" you are to obſerve ſeveral diameters of Venus
" (ſuppoſe 12) with o of the vernier placed al-
" ternately to the right and left hand of the be-
" ginning of the diviſions of the ſcale.

" 2dly, You are to obſerve ſeveral differences of de-
" clination of the northern limbs of the Sun and
" Venus, and the ſouthern limbs of the Sun and
" Venus alternately.

" 3dly, If there be conſiderable time left before the
" middle of the tranſit, you are to obſerve diſ-
" tances of Venus from the Sun's limb to the
" eaſt and weſt alternately, in lines parallel to the
" equator.

" 4thly, If there ſtill remain conſiderable time be-
" fore the middle of the tranſit, you are to ob-
" ſerve ſeveral times the horizontal diameter of
" the Sun.

" 5thly, You are to begin at leaſt half an hour (an
" hour would be better) before the middle of the
" tranſit, to meaſure the neareſt diſtance of Venus
" from the Sun's limb, and the fartheſt diſtance
" of Venus from the Sun's limb, alternately.

" N. B. The ſame poſition of the micrometer
" will ſerve for both, without turning it
" about. Theſe obſervations are to be con-
" tinued till the very middle of the tranſit,

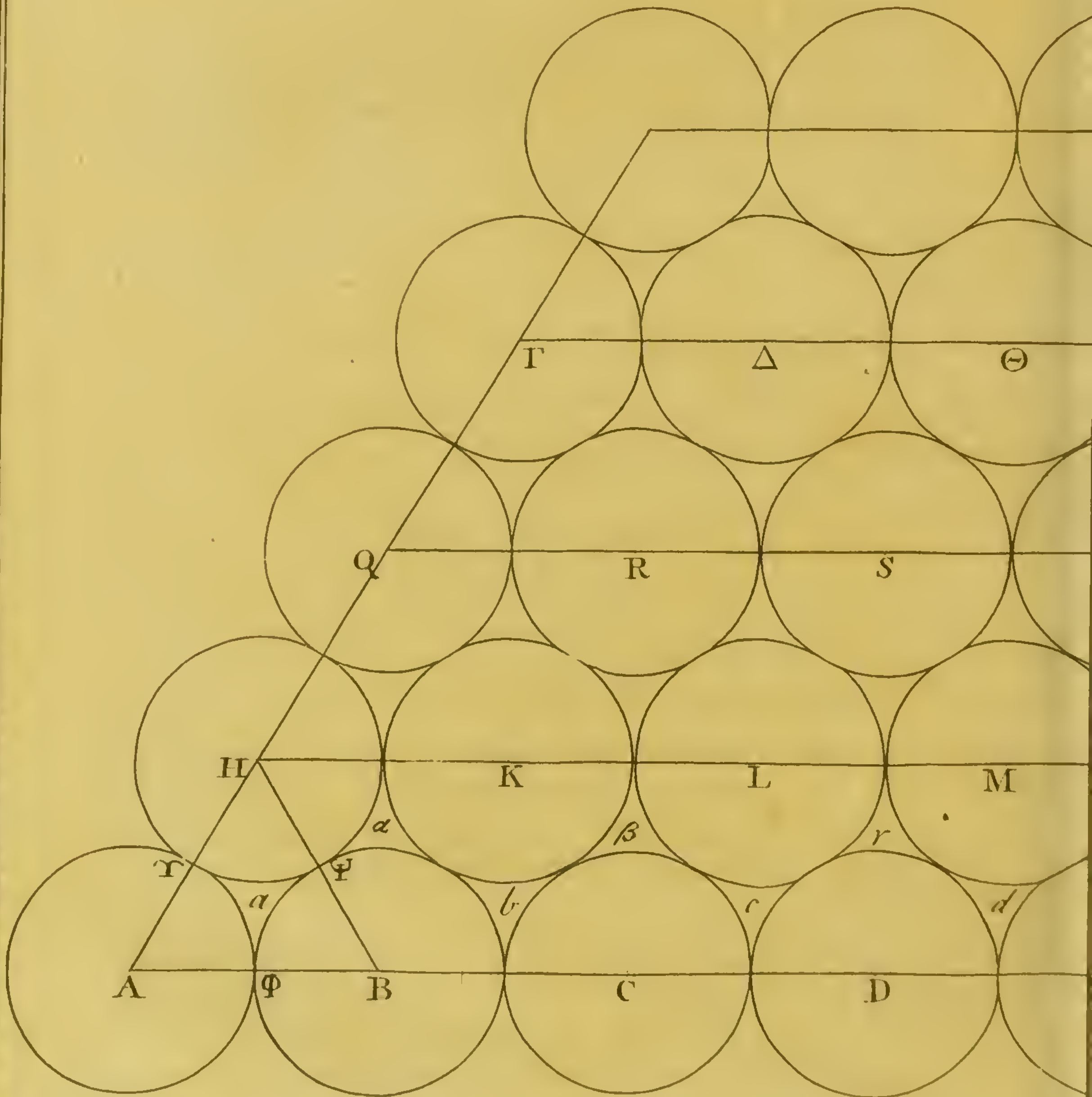
 " when

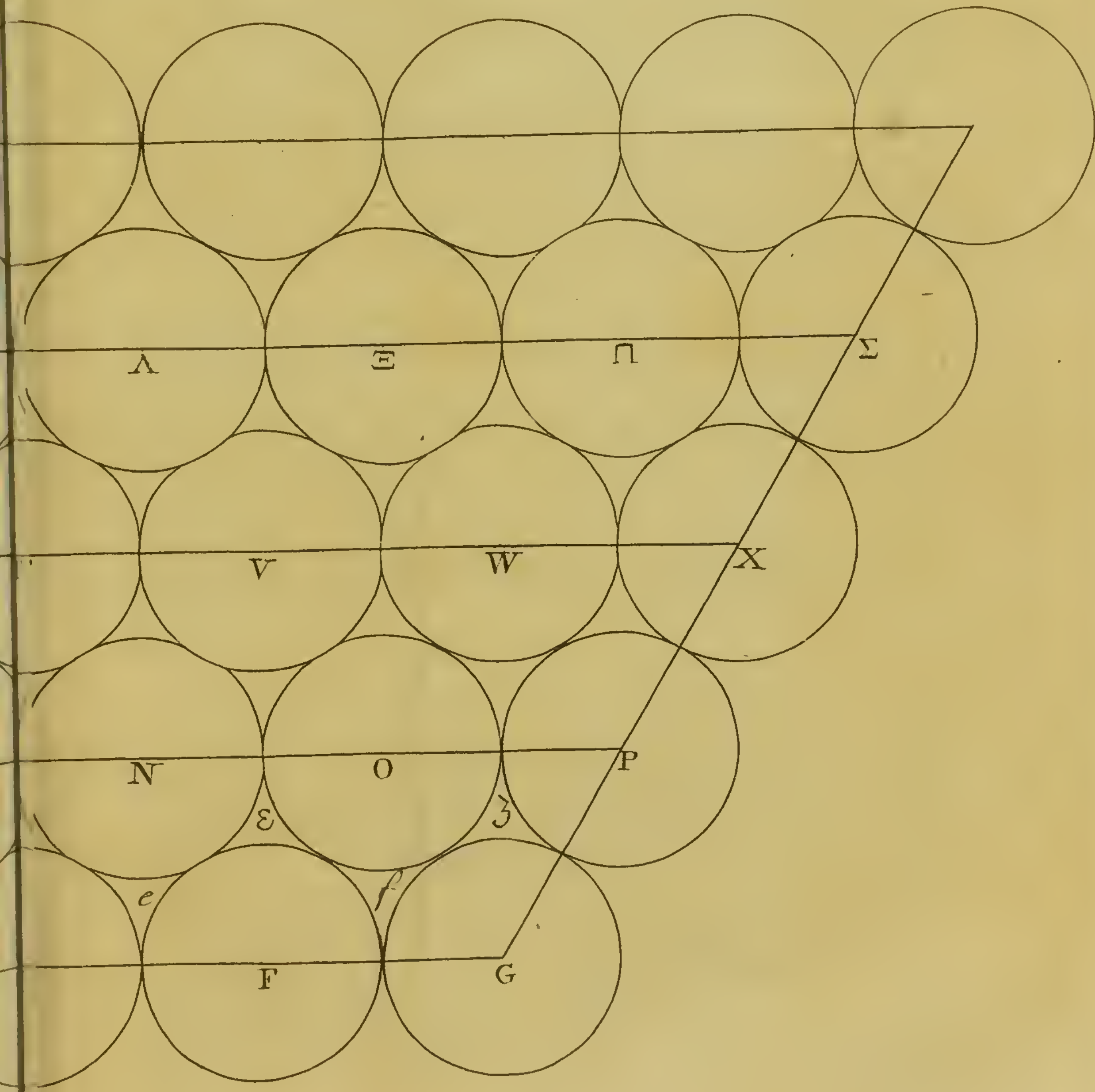
" when the diſtance will continue the ſame
" for a little ſpace of time; but it will be
" better to continue them for ſome time
" longer.

" 6thly, The ſame obſervations which were taken
" before the middle of the tranſit, or ſuch as could
" not, through ſome impediment, be obſerved
" before, may be proper to be obſerved after the
" middle of the tranſit.

" 7thly, It will be adviſeable to practiſe obſervations
" ſimilar to thoſe here recommended, previous to
" the tranſit of Venus, by means of ſpots in the
" Sun."

Philos. Trans. Vol. LXI. Tab. XVII. p. 517.





Basire Sc.

L. *A Supplement to a former Paper, concerning Difficulties in the* Newtonian *Theory of Light: by the Rev.* S. Horſley, *L L. B. F. R. S.*

PROBLEM I.

Read Dec. 19, 1771.

A PARCEL of equal Circles being diſpoſed upon a plane ſurface, of any figure whatſoever, in the cloſeſt arrangement poſſible, to determine the ultimate proportion of the ſpace covered by all the Circles, to the ſpace occupied by all their Interſtices, when each circle is infinitely ſmall, and the ſpace, over which they are diſpoſed, is of a finite magnitude.

The cloſeſt manner, in which a parcel of equal circles can be diſpoſed upon a plane, is when the centers of every three contiguous circles are ſituated at the angles of an equilateral triangle, which hath each of its ſides equal to a diameter of any one of the circles.

A number of circles, thus diſpoſed, may be divided, as TAB. XVII. ſhews, into ſeveral rows of circles, having their centers ranged upon parallel right lines, AG, HP, QX, ΓΣ, &c. Every

 circle,

circle, which is not in an outermoſt row, or at the extremity of any other row, touches ſix others, namely two in its own row, and two in the row on either ſide of its own: and each adjacent pair of theſe ſix do alſo touch each other. In the outer rows, every circle, which is not at one extremity of its row, touches four others, two in its own row, and two in the row next beſide it; which laſt two do likewiſe touch each other. A circle at either extremity of an outer row, touches only a ſingle circle in its own row, but either one or two in the row next beſide it. The bare inſpection of the figure (TAB. XVII.) will make theſe aſſertions manifeſt.

Now, imagine the equal circles, exhibited in the figure, to be each infinitely ſmall, the number of them being infinitely great, and the whole ſpace over which they are diſpoſed being of a finite magnitude. The ultimate proportion of the ſpace covered by all the Circles, to the ſpace occupied by all their Interſtices, is that of $\frac{1}{2}$ the area of one of the circles to the whole of one interſtitial area, *i. e.* the proportion of 39 to 4 very nearly.

DEMONSTRATION.

The circles ranged along the parallel right lines AB, HP, form two rows of interſtices; the row marked *a*, *b*, *c*, *d*, &c. and the row marked α, β, γ, δ, &c. and, in like manner, two rows of interſtices are formed by every two contiguous rows of circles.

Now, the numbers of the circles ranged along the ſeveral parallel right lines, AG, HP, QX, &c. are either equal or unequal, according to the figure of the ſpace over which they are diſpoſed.

I Caſe I.

Case 1. First suppose, that an equal number of circles is ranged along each of the parallel lines; in which case, the figure, in which they are included, must be a parallelogram. The number of circles, ranged along the parallel right lines A G, H P, being equal, the number of interstices in each of the rows, *a*, *b*, *c*, *d*, &c. α, β, γ, δ, &c. is less by unity than the number of circles upon either line, A G, or H P, be that number what it will. Thus the two circles A, B, upon the line A G, with the two circles H, K, upon the line H P, have the single interstice *a*, in the row *a*, *b*, *c*, *d*, &c. and the single interstice α, in the row α, β, γ, δ, &c. Again, the three circles A, B, C, upon the line A G, with the three, H, K, L, upon the line H P, have the two interstices *a*, *b*, in the row *a*, *b*, *c*, *d*, &c. and the two α, β, in the row α, β, γ, δ, &c. And universally, if the number of circles in each row be m, the number of interstices, in each of the two rows of interstices, will be $m - 1$. Consequently, the whole number of interstices formed by these two rows of circles is $2m - 2$. In like manner, the two rows of circles H P, Q X, form two more rows of interstices. And the number of circles upon each line, H P, Q X, being m, the number of interstices in each row is $m - 1$, and the whole number in both rows $2m - 2$. Therefore, the whole number of interstices formed by the three rows of circles, A G, H P, Q X, is $2m - 2$ twice taken, or $\overline{2m - 2} \times 2$. By the same reasoning if a fourth row of m circles, $\Gamma\Sigma$ be added, the number of interstices formed by the four rows is $\overline{2m - 2} \times 3$. And universally, if there be n rows

of

of equal circles, and m circles in each row, the number of interſtices formed by all the rows is $\overline{2m-2} \times \overline{n-1}$. Now, when the circles are infinitely ſmall, their diameters are infinitely ſmall. Therefore, the ſpace which they cover being of finite magnitude, it is neceſſary, that both the number of circles in each row, and the number of rows, that is, that each of the numbers, m and n, ſhould be infinitely great. But when m and n are each infinitely great, $\overline{2m-2} \times \overline{n-1}$, that is, the number of interſtices, becomes ultimately $2mn$; and the interſtices being all equal one to another, if the area of one be called P, the ſum of their areas will be $2mn \times P$. But the number of circles in n rows, each row conſiſting of m circles, is mn; and the circles being equal, if the area of one be called A, the ſum of their areas will be $mn \times A$. Hence the ſpace covered by all the circles is to the ſpace covered by all their interſtices, when the magnitude of each circle is infinitely diminiſhed, and the number of them ſo infinitely augmented, as that they ſhall cover a ſpace of finite magnitude, ultimately, as $mn \times A$ to $2mn \times P$, that is, as A to 2 P, or as $\frac{1}{2}$ A to P, that is, as $\frac{1}{2}$ the area of one circle to the whole area of one interſtice.

Caſe 2. Now, ſuppoſe, that unequal numbers of circles are ranged along the ſeveral lines A G, H P, Q X, &c. which muſt always be the caſe, if the figure of the ſpace, in which they are contained, be any other than a parallelogram; and let the number upon A G be the greateſt of all, and call that number, as before, m. If from the row H P, the extreme

treme circle P be taken away, all the reſt being left, the interſtice ζ will be taken away, and all the other interſtices, formed by m circles upon HP, with m circles upon AG, will remain. If again the circle O be taken away, beſides the interſtice ζ already taken away, the two ϵ, f will diſappear; and every circle more that is taken away, of thoſe remaining upon HP, from the extremity of the line, two more interſtices will diſappear. If from the row of circles HP, the extreme circle H be taken away, the two interſtices a, α, will diſappear. And if the circles K, L, M, be taken away ſucceſſively, every new circle that is taken away, two more interſtices will diſappear, of thoſe formed by the two rows AG, HP. Again, if the two circles P and H be taken away, the three interſtices ζ, a, α, will diſappear; and every circle more that is taken away, from either extremity, two more interſtices will diſappear. Hence whatever number of circles be taken away out of m circles upon HP, provided they be taken ſucceſſively, from either or both ends of the row (and when the number of circles upon HP is ſuppoſed leſs than that upon AG, the deficiency muſt be at the end, not in the middle of the row, otherwiſe the circles remaining would not be in the cloſeſt arrangement), it is evident that the number of interſtices which diſappear, of thoſe which would be formed by m circles upon HP, with m circles upon AG, muſt be either double the number of circles taken away, or leſs than the double of that number by 1. That is, if $m - a$ be the number of circles left upon HP, the number of interſtices formed by them, with m circles upon AG, is leſs than the number which would be

 formed

formed by m circles upon HP, with m circles upon AG, either by $2a$, or by $2a-1$. The number of interſtices formed by m circles upon each row would be, as hath been ſhewn in the preceding caſe, $2m-2$. Therefore, the number formed by m circles upon AG, with $m-a$ circles upon HP, is either $2m-2-2a$, or $2m-2a-1$. That is, ultimately (when the number $m-a$ is infinitely increaſed) $2m-2a$. Now, ſuppoſe the number of circles upon QX to be $m-a-b$. The number of circles upon the two rows AG, HP, is $2m-a$. Upon the three rows AG, HP, QX, the number is $3m-2a-b$. And if the number of circles upon ΓΣ be $m-a-b-c$, the number of circles upon the four rows AG, HP, QX, ΓΣ, will be $4m-3a-2b-c$. And, univerſally, the number of rows being n, and the number of circles upon the ſeveral rows, m, $m-a$, $m-a-b$, $m-a-b-c$, $m-a-b-c-d$, &c. ſucceſſively, the whole number upon all the n rows will be

$nm - a \times \overline{n-1} - b \times \overline{n-2} - c \times \overline{n-3}$, &c.

But, as it hath been ſhewn that m circles upon AG, with $m-a$ circles upon HP, form $2m-2a$ interſtices, if the number $m-a$ be infinite, in the ſame manner it may be ſhewn, that $m-a$ circles upon HP, with $m-a-b$ circles upon QX, form $2m-2a-2b$ interſtices, when the number $m-a-b$ is infinite. Therefore, the whole number of interſtices formed by the three rows upon AG, HP, QX, is $\overline{2m-2a} \times 2 - 2b$. And, in like manner, the number of interſtices, formed by the circles of four rows, will be

$\overline{\overline{2m-2a} \times 3 - 2b} \times 2 - 2c$. And, univerſally, n being

n being the number of rows, the number of the interftices will be

$\overline{2m-2a}\times\overline{n-1}-2b\times\overline{n-2}-2c\times\overline{n-3}$, &c. That is, $2m\times\overline{n-1}-2a\times\overline{n-1}-2b\times\overline{n-2}-2c\times\overline{n-3}$, &c. By comparing this expreffion with the former expreffion of the number of the circles, it will appear, that when n, the number of the rows of circles, is infinitely augmented, the number of interftices is to the number of circles, ultimately, as 2 to 1. For the two expreffions always confift of an equal numbers of terms. The fame numerical terms in both are affected with the fame figns. The firft term of the latter ($2m\times\overline{n-1}$) is ultimately double the firft term of the former (mn), when n is infinitely increafed, and each fucceeding term of the latter is double the correfponding term of the former. Therefore, the whole of the latter expreffion is ultimately to the whole of the former, as 2 to 1. That is, the number of interftices is ultimately double the number of circles: whence it follows, as in the former cafe, that the whole fpace covered by the circles is to the whole fpace occupied by the interftices, as $\frac{1}{2}$ the area of one circle to the whole area of one interftice.

In this Demonftration I have fuppofed the number of circles upon the feveral lines AG, HP, QX, &c. to decreafe continually. Had I fuppofed them to decreafe by fits, and in any manner imaginable, ftill the conclufion would have been the fame (a). There-

(a) Suppofe the number of circles upon the 1ft row to be m, upon the 2d, $m-a$, upon the 3d, $m-a+b$, upon the 4th, $m-a+b-c$, upon the 5th, $m-a+b-c+d$, and fo on, and

 fore,

fore, let the figure of the finite ſpace, including the circles thus cloſely arranged, with their interſtices, be what it will, the proportion of the ſpace covered by all the circles, to the ſpace taken up in interſtice, is ultimately that of $\frac{1}{2}$ the area of one circle to the whole area of one interſtice.

Now, that this is the proportion of 39 to 4, very nearly, will appear by computing one of the interſtitial areas.

The method of computing the interſtitial area is obvious. Let A, B, H be the centers of the three circles, which cloſe the interſtice ϒ Φ Ψ. Join A B, A H, B H. The right lines A B, A H, B H, paſs through the points of contact ϒ, Φ, Ψ, reſpectively.

and each of theſe numbers to be infinitely increaſed. Then, n being the number of rows, the whole number of circles will be $nm - a \times \overline{n-1} + b \times \overline{n-2} - c \times \overline{n-3} + d \times \overline{n-4}$, &c. Number the interſtices formed by every two contiguous rows, and add them all together, and the whole number of interſtices will be found to be

$2m \times \overline{n-1} - 2a \times \overline{n-1} + 2b \times \overline{n-3} - 2c \times \overline{n-3} + 2d \times \overline{n-5}$, &c. Now, by comparing theſe two expreſſions, it appears, that both conſiſt of the ſame number of terms: That the ſame numerical terms in order from the firſt, have the ſame ſigns: That the firſt term of the latter ($2m \times \overline{n-1}$) is ultimately the double of the firſt term of the former, when n is infinitely increaſed: That of the terms following the firſt, the negative terms of the latter are each double the correſponding negative terms of the former: and each poſitive term of the latter differs from the double of the correſponding poſitive term of the former by a number which vaniſhes with reſpect to either of thoſe correſponding terms, when n becomes infinite. Therefore, when n becomes infinite, the whole of the latter expreſſion becomes the double of the whole of the former. Hence the concluſion is as before.

The

The area of the triangle, A H B, is equal to the areas of the three ſectors A Υ Φ, B Φ Ψ, H Ψ Υ, added to the interſtitial area Υ Φ Ψ. But the triangle A H B is equilateral. Therefore each of the ſectors A Υ Φ, B Φ Ψ, H Ψ Υ is $\frac{1}{6}$ of the circle to which it belongs: and, the circles being equal, the three ſectors are equal to the half of any one of the circles. Therefore, the area of the triangle A H B is equal to $\frac{1}{2}$ the area of one circle (as of A) added to the interſtitial area Υ Φ Ψ. Therefore, from the area of the triangle A H B take $\frac{1}{2}$ the area of the circle A, and there will remain the interſtitial area Υ Φ Ψ.

Now, if the radius A Φ be put $= 1$, each ſide of the triangle A H B will be 2.

Therefore, the area of the triangle AHB will be	$= 1{,}73205$
But the radius being 1, $\frac{1}{2}$ the area of the circle A is	$= 1{,}5708$
The difference is	0,1612

And this is the interſtitial area Υ Φ Ψ, the half area of the circle A being 1,5708. Therefore, the ſemicircle is to the interſtice as 1,5708 to 0,1612, or as 9,74 to 1, or as 39 to 4, very nearly.

Corollary.

If a parcel of equal circles be ſo diſpoſed upon a plane ſurface of any figure whatſoever, that the centers of every three adjacent circles are ſituated at the angles of equal equilateral triangles, having ſides greater than the diameters of the circles, but greater in a finite proportion, the ultimate proportion of the ſpace

 covered

covered by all the circles to the ſpace occupied by all the interſtices, when each circle is infinitely diminiſhed, and the number of them ſo infinitely increaſed, that the ſpace over which they ſpread is of a finite magnitude, is that of $\frac{1}{2}$ the area of one circle to the whole area of one interſtice. And the area of any one interſtice is equal to the difference of the area of the equilateral triangle, formed by the right lines joining three adjacent centers, and $\frac{1}{2}$ the area of one of the circles.

PROBLEM II.

To determine the greateſt poſſible denſity of an infinitely thin cruſt compoſed of equal ſpherules, having their centers all in the ſame plane.

From the number 39 ſubtract its third part. To the number 4 add the third part of 39. The remainder is to the ſum, that is, 26 is to 17, very nearly, as the ſpace occupied by all the matter to the ſpace occupied by all the pore, in an infinitely thin cruſt, of the greateſt poſſible denſity, compoſed of equal ſpherules, having all their centers in the ſame plane.

DEMONSTRATION.

Upon a baſe of innumerable infinitely ſmall circles, arranged in the cloſeſt manner poſſible, (according to Prob. I.) imagine right cylinders to be erected, each cylinder having one of the little circles for its baſe, and its altitude equal to the diameter of its baſe. Theſe

Thefe cylinders are in the clofeft arrangement poffible for equal cylinders; and the fpheres, which they circumfcribe, are in the clofeft arrangement poffible for equal fpheres, which have their centers in the fame plane. The folid fpace occupied by the cylinders, is to the folid fpace occupied by their interftices, as the furface covered by their circular bafes, to the furface covered by the interftices of their bafes: That is, as 39 to 4, very nearly, by the firft Problem. But the fpheres contained within thefe cylinders are each but $\frac{2}{3}$ of the containing cylinder. The folid content therefore of all the fpheres is but $\frac{2}{3}$ of the folid content of all the cylinders; and the remaining third part of the folid content of the cylinders, together with the interftices between the cylinders, makes up the whole of the interftices between the fpheres. Therefore, the fpace occupied by the fpheres is to the fpace occupied by their interftices, as $39 - \frac{39}{3}$ to $4 + \frac{39}{3}$, or as 26 to 17, very nearly.

The fpheres being in the clofeft arrangement poffible, if each be a folid atom, or without pore within its own dimenfions, then, the infinitely thin cruft, which thefe atoms compofe, is plainly the moft denfe that can be compofed of equal fpherules, having their centers in one plane. And the fpace occupied by its matter is to the fpace occupied by its pore, as 26 to 17, very nearly.

SCHOLIUM.

If the component fpherules, inftead of being folid, be fuppofed to be each of the denfity of gold, in which one half of the bulk may reafonably be fuppofed to

be

be pore, then only $\frac{1}{2}$ of the ſpace, which they occupy, is filled with matter, and the other half is to be added to the pore. Hence ſpherules of the denſity of gold, arranged in the cloſeſt manner poſſible, having their centers in one plane, compoſe a cruſt, in which, $\frac{13}{43}$ds, or ſomewhat more than $\frac{12}{40}$ths, of its bulk is matter. Therefore, the denſity of ſuch a cruſt is ſomewhat greater than 12 times that of water, ſince $\frac{1}{40}$th only of the bulk of water is ſuppoſed to be matter, and $\frac{39}{40}$ths is pore.

S. Horſley.

The firſt of theſe two Problems, enabled me to determine the greateſt poſſible number of ſpherical particles of a given magnitude, that could find room to lie at one time upon the ſurface of the Sun; and, by the ſecond, I found the denſity of the cruſt, which ſuch particles, in the cloſeſt arrangement poſſible, with a given denſity of each particle ſeparately, would compoſe.

LI. *An*

Received November 14, 1771.

LI. *An Account of the going of an Astronomical Clock: By the Rev.* Francis Wollaston, *F. R. S.*

Read Dec. 19, 1771.

HAVING heard it often lamented, that very few registers of the going of clocks have been communicated to the public; I take the liberty to lay before the Society such observations as I have made to ascertain mine; and shall be happy if my amusements can in any way be of the least service to any one.

My clock was made by Holmes. The pendulum rod is of deal, to which the ball is screwed fast; and it is adjusted by a smaller weight underneath. The clock beats dead seconds; and is fastened to a principal wall, independent of the floor. The room never has a fire in it.

The transit telescope, with which I made the observations, has an achromatic object glass, of only 14 inches focal length, and magnifies about 15 times; its transverse axis is but 12 inches long, and it is mounted on a vertical axis of 18; being designed for an equal altitude instrument likewise, and so used

in

in ſome of the following obſervations. It is faſtened to a large ſtone pillar, bedded on the wall of the houſe; and is adjuſted in the meridian, to a mark 700 feet diſtant. I mention theſe particulars, becauſe the obſervations ſhew that even ſo ſmall an inſtrument is capable of tolerable exactneſs: and it is for that reaſon I have ſet down the reſult of all the tranſits I have taken for a year paſt; though much fewer would have ſufficed for ſhewing the rate of the clock. The obſervations themſelves are not here; becauſe I would not trouble the Society with ſuch a detail; elſe they ſhould readily have had them. It can be of no ſervice to falſify calculations, which might have been with-held; and I believe to the beſt of my abilities, theſe are done accurately. I am ſure they are delivered faithfully.

The 1ſt and 2d columns require no explanation.

The third ſhews how much the clock proved to be + or too faſt, — or too ſlow, for mean ſolar time upon each obſervation, when it came to be calculated afterwards. The marks : : or : are ſet down as any one appeared to me to be more or leſs doubtful at the time of obſerving. The calculations will ſhew which are really moſt to be ſuſpected.

The fourth column ſhews how much the clock varied per day, when compared with the preceeding obſervation of the ſame object. The ſmall variations in theſe, are owing probably to errors in obſerving, rather than to the clock itſelf. I do not pretend in obſerving to diſtinguiſh nearer than to half a ſecond; though the calculations are ſet down in decimals.

In

In the middle of February, when the firſt change was, the froſt was intenſe; and the pendulum did not, for ſome days, throw-out ſo far by about 7′ as it generally did; which was about 1° 37′ on one ſide, and 1° 40′ on the other. At the change in Auguſt, I obſerved no difference. It appears by theſe trials as if the clock gained in warm and loſt in cooler weather: but this is not clear. It began to gain before the weather grew warm. Whether this be owing to damp, or any other cauſes; longer experience and abler obſervers may diſcover.

1770			Clock + too fast — too slow	Varies from mean time per day
			′ ″	″
♃ Nov. 1	β Pegasi	Cl.	+ 0,5	
	α Andromedæ		+ 0,5	
♀ 2	☉ pr. eq. Alt. & Tr.		— 2,6	
☉ 4	Fomalhaut		— 2,7	
	β Pegasi		— 1,8	—0,8
♂ 13	☉	: :	—13,4	—1,0
	β Pegasi		—10,0	—0,9
	α Andromedæ		—10,5	—0,9
♄ 17	☉		—18,0	—1,1
	β Pegasi	:	—16,9	—1,7
	α Andromedæ		—16,9	—1,6
♂ 20	☉		—21,5	—1,2
	β Pegasi		—19,6	—1,0
	α Andromedæ		—21,2	—1,8
♄ 24	ε Pegasi		—22,5	
	β Pegasi		—21,5	—0,5
☽ 26	α ♒		—23,9	
♂ 27	γ Ceti		—24,4	
☿ 28	☉		—25,6	—0,5
	α ♒		—25,7	—0,9
☉ Dec. 16	γ Andromedæ	: :	—51,	
♄ 22	Fomalhant		—1 6,4	—1,3
	β Pegasi		—1 6,3	—1,6
☉ 23	β Pegasi		—1 7,4	—1,1
	γ Andromedæ		—1 6,3	
♂ 25	α Ceti		—1 14,0	
♃ 27	Rigel		—1 16,3	
	γ Orionis	:	—1 15,9	
	δ Orionis		—1 17,3	
	ε Orionis		—1 17,0	
☉ 30	α Andromedæ	:	—1 22,2	
	β Andromedæ		—1 22,0	
1771				
☉ Jan. 6	β Andromedæ		—1 37,5	—2,1
	γ Andromedæ		—1 37,5	—2,2
♂ 8	α Andromedæ		—1 40,5	—2,0
	β Andromedæ		—1 39,3	—0,8
	Rigel		—1 40,4	—2,0
☿ 9	☉	:	—1 43,3	
	β Andromedæ		—1 40,1	—0,8
	γ Andromedæ		—1 40,8	—1,1
	β Medusæ		—1 41,	
	Rigel		—1 41,5	—1,1
♃ 10	γ Andromedæ		—1 41,9	—1,1

1771			Clock + too fast — too slow	Varies pr. day
			′ ″	″
♃ Jan. 10	Rigel	Cl.	—1 43,7	—2,2
♀ 11	☉		—1 44,2	—0,5
	β Andromedæ		—1 42,3	—0,8
☉ 13	η Pleiadum		—1 45,6	
♂ 22	Rigel		—1 54,9	—1,0
♀ 25	Rigel		—2 0,7	—1,9
☉ 27	γ Andromedæ		—2 1,3	—1,2
♀ Feb. 1	Rigel		—2 9,7	—1,3
☉ 3	☉ per equal Alt.		—2 12,	
☽ 4	☉	: :	—2 13,3	: : —1,3
♂ 5	☉	:	—2 15,5	: —2,2
	η Pleiadum		—2 16,1	—1,3
	Rigel		—2 17,5	—1,9
♃ 7	Sirius		—2 19,3	
☽ 11	☉		—2 17,0	—0,5
♂ 12	☉		—2 15,5	+1,5
☿ 13	Sirius		—2 16,4	+0,5
♃ 14	☉		—2 15,1	+0,2
	Rigel		—2 15,4	+0,2
	Sirius		—2 16,6	—0,2
☉ 17	Sirius		—2 14,	+0,8
♄ 23	Rigel		—2 13,4	+0,2
♄ Mar. 9	Castor		—2 5,2	
	Procyon		—2 5,5	+1,7
☉ 10	Castor		—2 3,5	: +1,2
	Procyon	:	—2 4,3	+2,0
☽ 11	Procyon		—2 2,3	
☿ 13	α Orionis		—2 0,4	+1,1
	Castor		—2 0,2	+0,5
	Procyon		—2 1,3	+0,4
♀ 15	α Orionis		—1 59,6	: +0,5
	Sirius	:	—2 0,1	: +0,5
	Procyon	:	—2 0,5	+0,5
♄ 23	Sirius		—1 56,1	+0,5
	Procyon		—1 56,3	
☉ 24	α Hydræ		—1 55,6	+0,6
☿ 27	☉ per equal Alt.		—1 51,8	+1,4
♃ 28	α Hydræ		—1 50,1	+1,0
♄ 30	☉ pr. eq. Alt. & Tr.		—1 48,8	+0,8
☉ 31	Sirius		—1 49,8	+0,3
☽ Apr. 1	☉		—1 48,2	+0,4
	Sirius		—1 49,4	±0,0
♂ 2	☉		—1 48,2	+1,4
	Sirius		—1 48,0	

1771		Clock + too fast — too slow	Varies from mean time per day
		′ ″	″
Apr. 2	Procyon	Cl.—1 48,4	+0,8
	α Hydræ	—1 48,3	+0,4
3	☉	: —1 47	: +1,2
5	☉	—1 45,3	+0,9
8	☉	—1 43,7	+0,5
	Sirius	—1 43,5	+0,7
	Procyon	—1 44,3	+0,7
9	☉	—1 41,7	+2,0
	Sirius	—1 42,4	+1,1
	Procyon	—1 42,4	+1,9
11	☉	—1 38,5	+1,6
	Sirius	—1 39,5	+1,4
13	☉	—1 37,5	+0,5
	α Hydræ	—1 37,5	+1,0
17	☉	—1 30,0	+1,9
19	Regulus	: —1 26,4	
20	☉	—1 25	+1,7
25	β ♌	—1 19,4	
27	☉ per eq. Alt.&Tr.	—1 16,2	+1,3
May 5	β ♌	— 59,5	+2,0
12	β ♌	— 47,6	+1,7
13	β ♌.	— 45,4	+2,2
16	β ♌	: — 42,4	+1,0
	Spica ♍	— 40,9	
18	Spica ♍	— 38,6	+1,2
19	Spica ♍	— 36,2	+2,4
21	☉	— 34,0	+1,8
	Spica ♍	— 33,4	+1,4
24	☉	— 27,8	+2,0
27	Spica ♍	— 19,8	+2,3
	Arcturus	— 20,9	
	β ♏	— 19,6	
28	Arcturus	— 18,3	+2,6
29	☉	— 15,4	+2,5
June 1	Spica ♍	— 5,5	+2,9
2	Arcturus	— 3,2	+3,0
	γ Bootis	— 3,0	
5	☉	+ 5,3	+2,9
8	☉	+ 13,2	+2,6
	Spica ♍	+ 14,1	+2,8
10	☉	+ 18,0	+2,4
18	☉	+ 35,0	+2,1
	Antares	+ 36,1	

1771		Clock + too fast — too slow	Varies pr. day
		′ ″	″
♃ June 20	α Coronæ	Cl. + 38,4	
♀ 21	☉	+ 39,7	+1,6
	α Coronæ	+ 39,3	+0,9
	Antares	+ 39,3	+1,1
♄ 22	☉	+ 40,6	+0,9
♂ 25	☉	+ 45,4	+1,4
	Antares	: + 45,5	+1,5
	α Ophiuchi	+ 46	
♃ 27	☉	+ 47,75	+1,2
♄ 29	☉	+ 51,2	+1,7
	Arcturus	+ 51,0	
	Antares	+ 51,6	+1,5
♂ July 2	☉	+ 56,8	+1,9
☿ 3	Antares	+ 58,7	+1,8
♀ 5	☉	: +1 0,2	+1,1
☉ 21	α Ophiuchi	+1 30,9	+1,7
☽ 22	α Ophiuchi	+1 32,8	+1,9
♂ 23	β Lyræ	+1 34,3	
☿ 24	☉	+1 35,1	+1,8
♄ 27	☉	: +1 40,8	+1,9
☽ 29	α Lyræ	+1 47,7	
♃ Aug. 1	☉	+1 52,0	+2,2
♄ 3	α Aquilæ	: +1 58,1	
☉ 4	α Lyræ	+1 59,2	+1,9
	α Aquilæ	+1 59	+0,9
☽ 5	☉	+2 0,3	+2,1
	α Lyræ	+2 1,1	+1,9
♂ 6	α Lyræ	+2 3,0	+1,9
☿ 7	☉	:: +2 3,9	+1,8
♃ 8	α Lyræ	+2 6,9	+1,9
♀ 9	☉	+2 7,2	+1,7
♄ 10	☉	: +2 9,0	+1,8
	α Lyræ	+2 9,7	+1,4
♃ 15	☉	: +2 18,9	+2,0
♀ 16	α Aquilæ	+2 20,9	+1,8
♄ 17	☉	+2 20,3	:+0,7
	α Lyræ	+2 21,3	+1,7
	α Aquilæ	+2 21,8	+0,9
☽ 26	☉	+2 26,1	+0,6
♂ 27	☉	+2 25,5	−0,6
	α Lyræ	+2 26,4	+0,5
♃ 29	α Aquilæ	+2 25,7	+0,3
♀ 30	☉	+2 26,1	+0,2

1771		Clock + too fast — too slow	Varies from mean time per day
		′ ″	″
♀ Aug. 30	δ ♑	Cl. +2 24,7	
♄ 31	☉	+2 24,3	Cl. —1,8
☽ Sept. 9	α Aquilæ	:: +2 27	
	β Aquilæ	+2 25,5	
	α ♑ 2	+2 25,5	
	β ♑	+2 25,6	
♂ 10	α ♑ 2	+2 25,4	—0,1
	β ♑	+2 25,5	—0,1
♃ 12	☉	+2 25,0	
♄ 14	☉	:: +2 22,6	:: —1,2
	α Lyræ	: +2 22,8	—0,2
	γ Aquilæ	: +2 22,9	
☉ 15	☉ Equal Alt.	:: +2 20,4	
	α Aquilæ	+2 22,4	—0,2
☽ 16	β ♒	+2 21,5	
	γ ♑	+2 21,1	
	Fomalhaut	+2 21,5	
♂ 17	☉ per Transit	+2 20,9	
	per equal Alt.	+2 20,5	
☿ 18	β ♑	+2 19,8	—0,7
♃ 19	☉ pr.eq. Alt. & Tr.	+2 19,0	—0,9
	γ Aquilæ	+2 18,4	—0,9
	α Aquilæ	+2 18,7	—0,9
	β ♑	+2 18,7	—1,1
♀ 20	α Lyræ	+2 17,9	—0,8
	α Aquilæ	+2 18,1	—0,6
♄ 21	☉	: +2 16,1	: —1,4
	γ Aquilæ	+2 10,3	—1,1
	α Aquilæ	+2 17,0	—0,8
☿ 25	β ♒	+2 13,7	—0,8
	γ ♑	+2 14,1	—0,8
♀ 27	☉	+2 12,0	—0,7
♄ 28	☉	+2 11,9	—0,1
☽ 30	η Pegasi	+2 11,6	
♂ Oct. 1	☉	+2 11,1	—0,3

1771		Clock + too fast — too slow	Varies pr. day
		′ ″	″
♂ Oct. 1	α ♑ 2	Cl. +2 10,7	—0,7
☿ 2	☉	+2 10,3	—0,8
	α Lyræ	+2 11,6	—0,5
	α Aquilæ	+2 11,6	—0,6
♀ 4	☉	+2 11,3	+0,5
	α Aquilæ	+2 11,4	—0,1
	Fomalhaut	+2 10,9	—0,4
♄ 5	☉ pr. equal Alt.	+2 10,9	—0,4
♃ 10	☉	+2 7,4	—0,7
	γ Aquilæ	+2 7,5	—0,5
	α Aquilæ	+2 7,4	—0,7
♀ 11	☉	+2 6,6	—0,8
	γ Aquilæ	+2 6,6	—0,9
	α Aquilæ	+2 6,8	—0,6
♄ 12	☉	: +2 6,7	: +0,1
	γ Aquilæ	+2 6,3	—0,3
	α Aquilæ	+2 6,4	—0,4
☽ 14	γ Aquilæ	+2 4,6	—0,9
	α Aquilæ	+2 4,3	—1,0
	γ ♑	+2 4,6	—0,5
♂ 15	γ ♒	+2 3,4	
☿ 16	☉	+2 2,4	—1,0
	α Lyræ	+2 2,1	—0,7
♀ 18	☉	: +1 59,9	—1,2
☽ 21	☉	+1 55,3	—1,5
♀ 25	☉	+1 47,5	—1,1
♄ 26	☉	+1 46,4	—1,9
	γ Aquilæ	+1 46,5	—1,5
	α Aquilæ	+1 46,7	—1,5
♂ 29	☉	: +1 40,4	—2,0
☿ 30	γ Aquilæ	+1 39,1	—1,8
	α Aquilæ	+1 39,3	—1,8
♃ 31	β Pegasi	+1 38,1	

From these Observations it appears that the rate of the clock was as follows.

1770		Clock + too fast —too slow ′	″	Grain or Loss ″	Numb. of Days	Rate per Day ″
Nov.	1	+	0,5	—17,4	16	—1,1
	17	—	16,9	— 8,8	11	—0,8
	28	—	25,7	—40,7	24	—1,7
Dec.	22	—1	6,4	—15,6	8	—1,9
	30	—1	22,0			
1771				—23,6	14	—1,7
Jan.	13	—1	45,6	—24,1	19	—1,3
Feb.	1	—2	9,7	— 5,7	13	—0,4
	14	—2	15,4	+10,1	23	+0,4
March	9	—2	5,5	+ 5,5	6	+0,9
	15	—2	0,0	+10,6	17	+0,6
April	1	—1	49,4	+11,9	12	+1,0
	13	—1	37,5	+38,0	22	+1,7
May	5	—	59,5	+20,9	13	+1,6
	18	—	38,6	+33,1	14	+2,4
June	1	—	5,5	+41,6	17	+2,4
	18	+	36,1	+22,6	15	+1,5
July	3	+	58,7	+32,2	18	+1,8
	21	+1	30,9	+26,2	13	+2,0
Aug.	3	+1	57,1	+23,8	13	+1,8
	16	+2	20,9	+ 3,8	14	+0,3
	30	+2	24,7	— 2,3	16	—0,1
Sept.	15	+2	22,4	—11,7	16	—0,7
Oct.	1	+2	10,7	— 7,3	14	—0,5
	15	+2	3,4	—25,3	16	—1,6
	31	+1	38,1			

I will here add a few other observations I have made since I settled in this place, the lat. of which is 51° 24′ 33″ North, and the long. is 18,″5 in time, East of the Observatory at Greenwich.

Occultations of stars by the Moon.

1770					App. time	
♄ Apr.	7	e ♌	Imm.	11 29 25	observed with a 12 inch reflector.
♄	28	ζ ♉	Imm.	9 51 56;	windy and doubtful; same telesc.

1771

1771

♂ June 18 The Moon's lower limb juſt covers a ſmall ſtar. The imm. on the dark part, to which the ſtar ſeemed to adhere above two minutes; and, though not at all diſcoloured, loſt a little of its brightneſs, but diſappeared at laſt inſtantaneouſly, Apparent time 10 1 49

The Em. on the light part and doubtful 10 10 46

Obſerved with a $3\frac{1}{2}$ feet achrom. magnifying 100 times.

♂ July 23 2 μ ♐ I believe

Imm.	10 41 36,5	certain		$3\frac{1}{2}$ achrom. mag. 150
Em.	11 43 27 ::	doubtful		

☿ Sept. 18

* ♑	Imm.	11 56 51	good	$3\frac{1}{2}$ achrom. mag. 150
β ♑	Imm.	12 2 47	good	

The emerſions not till after the Moon was ſet.

Eclipſes of Jupiter's Satellites.

1770	App. time		
♀ July 13	9 6 24	Firſt Sat. Em.	12 In. Reflector mag. 55
♄ 21	9 3 8::	Fourth Sat. Imm.	Ditto
	9 57 43::	Em.	
☉ Aug. 5	9 20 42	Firſt Sat. Em.	Ditto
☉ 28	9 43 3::	Firſt Sat. Em. ♃ near ☽	Ditto
☿ 29	9 1 41	Second Sat. Em.	Ditto

1771	App. time		
☽ July 22	8 46 20::	Second Sat. Em. cloudy	$3\frac{1}{2}$ Achrom. mag. 100
♃ Aug. 1	9 8 5::	Third Sat. Em. cloudy	$3\frac{1}{2}$ Achrom. mag. 100
	10 30 54::	Firſt Sat. Em. cloudy	$3\frac{1}{2}$ Achrom. mag. 100
♄ 17	8 51 9	Firſt Sat. Em.	12 Inch Refl. mag. 35
♂ 27	9 32 3	Fourth Sat. Imm. hazy	$3\frac{1}{2}$ Achrom. mag. 100
♀ 30	11 3 20	Second Sat. Em.	Ditto
☽ Sept. 9	9 12 18	Firſt Sat. Em.	Ditto
☽ 16	11 11 15:	Firſt Sat. Em. ♃ but 3° 30′ high	Ditto
☿ Oct. 2	9 35 56	Firſt Sat. Em.	Ditto
♀ 11	6 3 16	Firſt Sat. Em.	Ditto
♄ 26	6 14 25	Third Sat. Imm.	Ditto
	8 4 16	Second Sat. Em.	Ditto

Chiſlehurſt, Nov. 2, 1771.

Francis Wollaſton.

LII. *An*

LII. *An Account of a pure native cryſtaliſed Natron, or foſſil alkaline Salt, which is found in the Country of* Tripoli *in* Barbary: *By* Donald Monro, *M. D. Phyſician to the Army, and to* St. George's *Hoſpital, Fellow of the Royal College of Phyſicians, and of the Royal Society.*

Read Dec. 19, 1771.

IT is well known that the nitre, or natron, of the antients, which they uſed for making of glaſs (*a*), and in their baths (*b*), and for other purpoſes, was not the ſalt which now goes by tha name of nitre, or ſaltpetre; but a ſalt of an alkaline nature, which, at preſent, is commonly called the natron of the antients, or the foſſil alkali.

(*a*) See an account of the making of glaſs with nitre and ſand in C. Plinii Secundi Hiſt. natural. Tom. III. lib. xxxvi. cap. 26, —and an account of its medicinal virtues, ibid. lib. xxxi. cap. 10. —And Tacitus, in mentioning the river Belus in India, ſays, "Circa cujus os collectæ arenæ, admixto nitro, in vitrum excoquuntur." Lib. v. Hiſt. ſect. 7.

(*b*) Nitre is mentioned as uſed in baths, in ſeveral parts of the Holy Scripture, particularly by the prophet Jeremiah. See chap. ii. ver. 22. The nitre, or natron, is likewiſe taken notice of by many other of the ancient authors.

The

The knowlege of it was entirely loſt for ſeveral centuries, but was revived in the laſt, by the Honourable R. Boyle, formerly a diſtinguiſhed member of this Society, who, in his Short Memoirs for the natural experimental Hiſtory of Mineral Waters (*c*), after telling us that it is of an alkaline nature, ſays, "that he had ſome of it brought from Ægypt, and "a neighbouring country, whoſe name he did not "remember."

However, it was afterwards neglected, and its properties as a diſtinct ſpecies of alkaline ſalt not known for many years; for although chemiſts obſerved, that a Glauber ſalt and cubic nitre were formed by diſlodging the marine acid from ſea ſalt, by means of the vitriolic and nitrous acids; and from thence ſuſpected that there was ſomething particular in the baſis of this ſalt; yet its true nature was not diſcovered till Monſ. du Hamel du Monceau gave an account, in the Memoirs of the French Royal Academy of Sciences for the year 1736, of his having obtained it pure, in two different ways. 1ſt, By diſlodging the marine acid by means of the vitriolic, and then ſeparating it by the addition of a phlogiſton, and forming a hepar ſulphuris, from which he precipitated the ſurphur by means of the vegetable acid, and then ſeparated this acid from the baſis of ſea ſalt by the force of fire. 2dly, By diſlodging the marine acid from the ſea ſalt by the addition of the nitrous, and ſo forming a cubic nitre, from which he diſlodged the acid, by deflagrating it with charcoal;

(*c*) See his Notes on Title 26, page 86, of the edition printed at London 1684-5.

and

and then he purifyed the remainder by diſſolving it in water, and by filtrating and evaporating the liquor and cryſtalliſing the ſalt.

After he had obtained the baſis of ſea ſalt quite pure, he tried a number of experiments with it, and with the natron of Egypt; and found that they were entirely of the ſame nature, and that they were of a diſtinct ſpecies of alkaline ſalt, different in their properties from the potaſh, and other alkaline ſalts, commonly obtained by burning wood, and moſt other vegetable ſubſtances; and that they formed different neutral ſalts with the three mineral acids, and with the vegetable.

This ſalt is likewiſe got from burning the Barilla, the Kali, and other marine plants; and all that is at preſent uſed in this country, by our manufacturers, has been prepared in this manner.

Hitherto it has not been found native in the weſtern parts of Europe, except in mineral waters, and in the neighbourhood of volcanoes, or at places where they are alledged to have exiſted formerly; but it has long been found in Egypt, and near to Smyrna, and in other eaſtern countries, commonly mixed with earth, in a floury or concrete form; in ſome places pretty pure, in others more mixed (*d*).

In the year 1764, a reſpectable member of this Society, Dr. Wm. Heberden, gave an account of a ſalt of this kind, which was found on the Pic of Tenerif, where there is a volcano, and added ſeveral very ingenious experiments of the Honourable Henry Ca-

(*d*) See Hoffman. Phyſ. Chem. lib. ii. obſ. 1.—Geoffroy, Mater. Medica, part i. cap. 2.—Dr. Shaw's Travels, Excerpt. pag. 55. and other authors.

 vendiſh,

vendiſh, to prove that the vegetable alkali has a greater affinity with acids than the foſſil or natron.

It is probable, that this ſalt got at the Pic of Tenerif is the baſis of ſea ſalt, whoſe acid has firſt been diſlodged, either by the force of fire, or by the acid of decompoſed ſulphur, which has afterwards been attracted by a freſh phlogiſton, and both ſeparated by the force of fire; though it is not at all impoſſible but that there may be magazines of this foſſil ſalt lodged native in the bowels of this mountain.

Hitherto we have no account, that I know of, of its being found any-where native in a cryſtalline form, and in large quantity; and therefore I imagined that the following hiſtory would be agreeable to the Society.

In the year 1765, Mrs White, widow to the late Conſul White of Tripoli, on her return to this country, ſhewed me a ſubſtance which, ſhe ſaid, had a very particular property of bubbling up, or fermenting, when mixed with lemon juice. Immediately, on ſeeing and taſting it, I ſuſpected it to be a pure native natron, or foſſil alkali; and was confirmed in this opinion, by mixing it with different acids; and I have ſince had a few pounds of it ſent home to me, and ſome gentlemen in the city have imported ſome hundred weight of it.

On enquiring into the hiſtory of this ſalt, I was told that it was brought yearly to Tripoli, in large quantities, from the mountains in the inland part of the country, and that it went by the name of Trona; that the inhabitants ſometimes took an ounce, or more of it, by way of phyſic, and that it commonly operated both as an emetic and purgative medicine;

that

that the principal uſe they made of it, was to mix it with their ſnuff, to give it, what they think, an agreeable ſharpneſs; and that it was yearly ſent to Conſtantinople, in large quantity, to be employed for the ſame purpoſe. But, ſo far as I can learn, the Turks are entirely ignorant of its nature, and employ it for no other uſes.

It is well known that this ſalt does not run *per deliquium*, but falls down into a white floury powder, when expoſed to the air; and that it makes a harder and firmer ſoap than the common vegetable alkali, and is alledged to make a purer and a finer glaſs.

This ſalt, which I have the honour now to preſent to the Society, is extremely pure, diſſolves entirely in water, leaving only a ſmall quantity of a reddiſh earth behind. I tried what quantity of acid an ounce of this ſalt would ſaturate, and found that it ſaturated as much as near two ounces and a half of the common groſs barilla, in the form it is commonly imported. I had it likewiſe tried by callico printers, and it was found to anſwer all their purpoſes, and nearly in the ſame proportion with reſpect to the groſs barilla, as above-mentioned, and I was told that it was thought to anſwer better than any other ſalt they had ever tried.

Moſt of the neutral ſalts made with this alkali and acids (except the cubic nitre) keep long without running *per deliquium*, even thoſe made with vegetable acids; for moſt of the neutral ſalts made with vegetable acids, and with ſome of the ſalt now before you, which I had the honour to preſent to this Society in the year 1767, ſtill remain entire, though kept only in a close drawer, in the same tea-cups and small basons, without any cover, as they were shewn to the Society.

I have not hitherto been able to learn in what particular place of the inland part of Tripoli in Barbary this salt is found, nor how it is disposed of in the bowels of the earth: but it should seem to run in thin veins, of about half an inch, or a little more thick, in a bed of sea salt; for all of it that has hitherto been imported into this country is covered with sea salt on each side. The one side is always smoother than the other, and appears as if it had been the basis on which it rested; the other, which should seem to be the upper side, is rougher, by the shooting of the crystals. The pieces of the thin veins appear almost as if the salt had been dissolved in water, and afterwards boiled up into thin crystallised cakes, only that the crystals are much smaller, and disposed in a manner that cannot easily be imitated by art; for when this salt is dissolved, and evaporated to a pellicle, and left to crystallise, it always shoots into crystals resembling those of Glauber salt.

Brown paper dipt into a solution of this salt, after it is dry burns almost as if it had been dipped in a solution of true nitre, as Dr. Heberden had observed of the salt got at the Pic of Tenerif; which shews, that it contains more of an inflammable principle than the common vegetable alkali.

There are great mines of sea salt in the country of Tripoli, the salt of which should seem to contain a large proportion of this natron; for, I am told, that all the meat salted with it acquired a red colour.

This native alkaline salt having never been subjected to the force of fire, is perfectly mild, and contains

tains no cauſtic parts, as the barilla, and the common potaſhes prepared by burning wood and plants, or the ſalts thrown out by volcanoes commonly do; and therefore, it will be found to be much more uſeful for bleaching and waſhing linens, and for cleaning and ſcowering cotton or woollen ſtuffs, and for many other purpoſes, than any other alkaline ſalt hitherto known, at the ſame time that it will anſwer every purpoſe for which the other kinds of the foſſil alkali are employed

When this ſalt is to be uſed for making rochelle or other neutral ſalts, or for waſhing or bleaching linen, it ought firſt to be diſſolved in pure water, and the ſolution be allowed to ſtand for ſome time, till the reddiſh or brown earth has all precipitated to the bottom, and then the pure liquor ought to be poured off, and what remains at the bottom be thrown into a filter; for, if this precaution is not taken, the reddiſh earth is in danger of giving a ſlight brown or reddiſh colour to the neutral ſalts, or to affect the colour of the linen.

LIII. *The Quantity of the Sun's Parallax, as deduced from the Obſervations of the Tranſit of* Venus, *on* June 3, 1769: *By* Thomas Hornſby, *M. A.* Savilian *Profeſſor of Aſtronomy in the Univerſity of* Oxford, *and F. R. S.*

Read Dec. 19, 1771.

THE uncertainty as to the quantity of the Sun's parallax, deduced from the obſervations of the tranſit of Venus in 1761 (whether it aroſe from the unfavourable poſition of the planet, ſo that a ſufficient difference of time in the total duration of the tranſit was not, and indeed could not be, obtained from obſervations made at different places; or from the diſagreement of the obſervations of different aſtronomers, which were to ſerve as terms of compariſon) ſeems now to be entirely removed: and from the obſervations made in diſtant parts by the aſtronomers of different nations, and eſpecially from thoſe made under the patronage and direction of this Society, the learned of the preſent time may congratulate themſelves on obtaining as accurate a determination of the Sun's diſtance, as perhaps the nature of the ſubject will admit.

The

The two following Tables give not only the obſervations themſelves, but alſo the computed differences of time from which the parallax was deduced.

Table I.

Places.	Latitude.	Obſervers names.	Int. Cont. at Ingreſs	Int. Cont. at Egreſs	Obſ. Dur.
	° ′ ″		H. ′ ″	H. ′ ″	H. ′ ″
Wardhus.	70 22 36 N.	F. Hell.	9 34 10,6	5 27 24,6	5 33 14
Kola.	68 52 56 N.	M. Rumonſky.	9 42 4	5 35 23	5 53 19
Hudſon's Bay.	58 47 32 N.	M. Wales.	1 15 21,3	7 0 45,5	5 45 24,2
		M. Dymond.	1 15 25,3	7 0 48,5	5 45 23,2
California.	23 3 37 N.	Abbè Chappe.	0 17 27,9	5 54 50,3	5 37 32,4
K. George's Iſland.	17 28 55 S.	Capt. Cook.	21 44 15,5	3 14 13	5 29 57,5
		Mr. Green.	21 43 55,5	3 14 3	5 30 7,5
		Dr. Solander.	21 44 2,5		

Table II.

	Obſerved durations.	Difference of comp. durat.	Difference of obſerv. durat.	Sun's parallax.
	H. ′ ″	′ ″	′ ″	″
King George's Iſland.	5 29 52,5			
Wardhus.	5 53 14	23 31,36	23 21,5	8,639
Kola.	5 53 19	23 41,09	23 26,5	8,611
Hudſon's Bay.	5 45 23,7	15 51,90	15 31,2	8,511
California.	5 37 32,4	7 42,43	7 29,9	8,464
California.	5 37 32,4			
Wardhus.	5 53 14	15 48,93	15 51,6	8,724
Kola.	5 53 19	16 4,41	15 56,6	8,629
Hudſon's Bay.	5 45 23,7	8 9,47	8 1,3	8,555
Hudſon's Bay.	5 45 23,7			
Wardhus.	5 53 14	7 39,46	7 50,3	8,905
Kola.	5 53 19	7 49,19	7 55,3	8,813
Mean of all				8,650

The

The ſecond column of the ſecond Table contains the obſerved duration, or interval of time, between the two internal contacts; the third contains the difference of each duration, deduced by computation upon a ſuppoſition that the Sun's parallax was $=8'',7$ on the day of the tranſit; the fourth, the difference of that duration, as determined by actual obſervation: In the laſt column is given the horizontal parallax on the day of the tranſit, reſulting from a compariſon of the third and fourth columns.

In the above compariſon, I have uſed Captain Cook's obſervation at the ingreſs, and a mean of his and Mr. Green's obſervations at the egreſs; becauſe, upon a compariſon of the obſerved times at the ingreſs and egreſs, made at the ſeveral places, when reduced to the center of the Earth, upon a ſuppoſition that the Sun's parallax on the day of the tranſit was $=8'',65$, the difference of meridians, as deduced from Captain Cook's obſervation at the ingreſs, agrees much better with the ſame differences deduced from a mean of the two obſervations at the egreſs, than thoſe derived either from the obſervation of Mr. Green, Dr. Solander, or even from a mean of all the three obſervations, as appears from the following compariſon.

Wardhus.

	Wardhus.		California.	
	Ingreſs.	Egreſs.	Ingreſs.	Egreſs.
	H. ′ ″	H. ′ ″	H. ′ ″	H. ′ ″
Obſerved times.	9 34 10,6	15 27 24,6	0 17 27,9	5 54 50,3
Effect of parallax.	+ 6 35,6	— 4 35,9	+ 24,9	+ 4 52
Reduced times.	9 40 46,2	15 22 48,7	0 17 52,8	5 59 42,3

	Kola.		Hudſon's Bay.	
	Ingreſs.	Egreſs.	Ingreſs.	Egreſs.
Obſerved times.	9 42 4	15 35 23	1 15 23,3	7 0 47
Effect of parallax.	+ 6 37,4	— 4 45,1	+ 4 15,9	+ 0 38,7
Reduced times.	9 48 41,4	15 30 37,9	1 19 39,2	7 1 25,7

King George's Iſland.

	Ingreſs.				Egreſs.
	Capt. Cook.	Mean.	Dr. Solander.	Mr. Green.	Mean.
Obſerved times.	21 44 15,5	21 44 4,5	21 44 2,5	21 43 55,5	3 14 8
Effect of parallax.	5 40,4	5 40,4	5 40,4	5 40,4	+ 6 23,8
Reduced times.	21 38 35,1	21 38 24,1	21 38 22,1	21 38 15,1	3 20 31,8
Ditto at Wardhus.	9 40 46,2	9 40 46,2	9 40 46,2	9 40 46,2	15 22 48,7
Difference of meridians.	12 2 11,1	12 2 22,1	12 2 24,1	12 2 31,1	12 2 16,9

	Ingreſs.				Egreſs.
	Capt. Cook.	Mean.	Dr. Solander.	Mr. Green.	Mean.
Reduced times at K. G. Iſl.	21 38 35,1	21 38 24,1	21 38 22,1	21 38 15,1	3 20 31,8
Ditto at California.	0 17 52,8	0 17 52,8	0 17 52,8	0 17 52,8	5 59 42,3
Difference of meridians.	2 39 17,7	2 39 28,7	2 39 30,7	2 39 37,7	2 39 10,5

	Ingreſs.				Egreſs.
	Capt. Cook.	Mean.	Dr. Solander.	Mr. Green.	Mean.
Reduced times at K. G. Iſl.	21 38 35,1	21 38 24,1	21 38 22,1	22 38 15,1	3 20 31,8
Ditto at Kola.	9 48 41,4	9 48 41,4	9 48 41,4	9 48 41,4	15 30 37,9
Difference of meridians.	12 10 6,3	12 10 17,3	12 10 19,3	10 10 26,3	12 10 6,1

	Ingreſs.				Egreſs.
	Capt. Cook.	Mean.	Dr. Solander.	Mr. Green.	Mean.
Reduced times at K. G. Iſl.	21 38 35,1	21 38 24,1	21 38 22,1	21 38 15,1	3 20 31,8
Ditto at Hudſon's Bay.	1 19 39,2	1 19 32,9	1 19 39,2	1 19 39,2	7 1 25,7
Difference of meridians.	3 41 4,1	3 41 15,1	3 41 17,1	3 41 24,1	3 40 53,9

The

The near agreement of the difference of meridians between King George's Iſland and the four other places, as deduced from Captain Cook's obſervation at the ingreſs, and from a mean of his and Mr. Green's obſervations at the egreſs, ſufficiently, I think, ſhew that the obſerved duration at King George's Iſland is at leaſt $5^h\ 29'\ 52'',5$: And, from a compariſon made in the ſame manner with the obſervations at Hudſon's Bay, it might be ſhewn that the time of the egreſs is uncertain to a few ſeconds, owing, perhaps, to the hazineſs of the air peculiar to that climate, even at the altitude of 10 or 12 degrees.

By the end of the Sun's eclipſe on the morning after the tranſit, the longitude of Wardhus from Paris, according to Father Hell, is $1^h\ 55'\ 6''$ E. of Paris, or $2^h\ 4'\ 22''$ E. of Greenwich: and, according to the obſervation of Mr. Rumouſky, Kola is $2^h\ 2'\ 55''$ E. of Paris, or $2^h\ 12'\ 11''$ E. of Greenwich. The point therefore at King George's Iſland, where the tranſit was obſerved, is $9^h\ 57'\ 53'',6 = 149°\ 28'\ 24''$ W. of Greenwich; Vill St. Joſeph in California is $7^h\ 18'\ 42\frac{1}{2}'' = 109°\ 40'\ 37''$ W. of Greenwich; and Prince of Wales's Fort in Hudſon's Bay $6^h\ 16'\ 49\frac{1}{2}'' = 94°\ 12'\ 22''$ W. of Greenwich.

From the near agreement of the ſeveral reſults before found, which are independent of the knowledge of the longitude of each place, and affected only by the neceſſary error in obſerving, the accuracy of the obſervation made at the Cape of Good Hope in 1761, by Meſſieurs Maſon and Dixon, is abundantly confirmed; by comparing which with the

the beſt obſervations made in the places whoſe longitudes were very nearly aſcertained, the Sun's parallax on the 5th of June was found = 8″,692 *. And Mr. Pingré, notwithſtanding the ſeveral arguments very ſpeciouſly produced in favour of his own obſervation at the Iſland of Rodrigues, as repreſented in his learned Memoire on the Sun's Parallax, will probably be of opinion, that an error of one minute was committed in writing down the time of his obſervation, as was conjectured by many perſons, as well as myſelf; a miſtake to which the moſt experienced obſerver is ſometimes liable, when at the time of obſervation the minute is nearly compleated.

The parallax on the 3d of June being 8″,65, the mean parallax will be found to be = 8″,78; and if the ſemidiameter of the Earth be ſuppoſed = 3985 Engliſh miles, the mean diſtance of the Earth from the Sun will be 93,726,900 Engliſh miles. And, as the relative diſtances of the planets are well known, their abſolute diſtances, and conſequently the dimenſions of the Solar Syſtem, will be as follows.

	Relative diſtance.	Abſolute diſtance.
Mercury,	387,10	36,281,700
Venus,	723,33	67,795,500
Earth,	1000,00	93,726,900
Mars,	1523,69	142,818,000
Jupiter,	5200,98	487,472,000
Saturn,	9540,07	894,162,000

Oxford, Dec. 17, 1771.

* See Phil. Tranſ. Vol. LIII. for the Year 1763. p. 491.

LIV. *A Letter from Mr.* R. E. Raſpe, *F. R. S. to* M. Maty, *M. D. Sec. R. S. containing a ſhort Account of ſome Baſalt Hills in* Haſſia *.

Dear Sir,

Caſſel, November 29, 1769.

Read Feb. 8, 1770.

I HAVE lately diſcovered in the neighbourhood of this city, ſeveral hills, compoſed of baſalt rocks, formed in polyedrous and moſtly pentagonal columns. As this ſort of ſtone has hitherto met with few obſervers, and affords many curious ſingularities, I deſire you to lay before the Royal Society, the following account of my reſearches.

Our baſalt rocks differ from thoſe of the Giant's Cauſeway in Ireland, by their want of articulation; and from thoſe anciently found at Syena in Egypt, and deſcribed with tolerable exactneſs by Strabo, Lib. xvii. by their being leſs thick, and not ex-

* The printing of this paper was poſtponed, on account of the delays and difficulties which attended the ſending of the plates, which the author deſired might be executed in the beſt manner, under his eyes.

ceeding

ceeding eight or ten inches in breadth, on unequal lengths from five to thirty feet.

The colour, hardnefs, weight, and fubftance of thefe ftones fufficiently fhew them not to belong to the genus of the marbles, amongft which Mr. Dacofta ranked them in imitation of the ancients.

Their fubftance is vitreous, analogous to that of the horny ftones; they refift aqua fortis, and the chizzel: and only yield to a violent fire and the engravers wheel. Being worked in this manner they acquire the polifh of the ancient bafaltes, named by the Italians *Marmo paragone*. I have not yet completed a chemical analyfis of thefe ftones, which they richly deferve, chiefly as they contain fmall nefts of cryftals of tin ore, yellow, green, and black. Thefe probably greatly contribute towards giving to our ftones their fingular and conftant form. They feem to have acquired that form, in a different manner from that which influenced the ftrata and veins of other mountains. Laftly, no marks or impreffions of any organical bodies are found either in the out or infide of thefe ftones.

From all thefe confiderations I was induced to attribute their origin, to a watery cryftallifation, which might have taken place, either at the firft fettling of the chaos, or at the time of a diffolution of a great part of our globe. I had faid the fame thing in regard to the Giant's Caufeway, in my account of the formation of new iflands. But I now begin to entertain fome doubts about that opinion, for thefe two reafons.

1. In the explanation of the plates of the French Encyclopedie, I find that an obfervation made by Mr.

Mr. Defmareft, has induced him to attribute the origin of thefe ftony columns to the matter of volcanoes refrigerated from fufion, having found the Auvergne bafaltes placed on beds of lavas and fcoriæ, juft clofe to the opening of an extinguifhed volcanoe.

2. I difcovered the fame appearance at Habichfwald about Weiffenftein near Caffel. The top of the mountain, on which the famous cafcades of the Landgrave Charles are built, and which the Englifh troops made the place of their encampment after the battle of Willemftahl, is hardly compofed of any thing but enormous pieces of lavas and fcoriæ. Somewhat lower, and near the middle of the mountain, are found the bafaltes. Many of thefe are formed in polyedrous pillars; but fome, which are the neareft to the aforefaid lava, only confift of fhapelefs roundifh maffes. On the other fide of the mountain, and at a fmall diftance from the lavas and fcoriæ, is found one of the richeft coal mines I ever faw, in a bed of the thicknefs of eighteen feet.

The Duke of Rochefoucault, at Paris, an eminent lover and encourager of natural hiftory, has likewife affured me, that at Bolfena in Italy, the bafaltes are found near the lavas of an ancient volcanoe, and that the whole ifland of Sicily, chiefly on the fide of mount Etna, abounds with the fame.

Hence, it may be allowable to attribute with Mr. Defmareft the origin of the bafaltes to volcanoes. This opinion is further fupported from many circumftances; *viz.* the vitreous, and hitherto problematical fubftance of thefe ftones; the want of marine bodies, and laftly, the well-known experiment of fome melted metals, which, when hardened, appear

J. H. Tischbein del.

A Basalt-Rock ...

...r Gudensberg in Hassia

J. H. Tischbein del. 1769.

J. W. Mail Sculp. Berolini 1771

Felsberg a castle in Hassia, situated on a Basalt-hill in cryſtallizations not unlike thoſe of watery congelations.

I muſt not however omit that the other baſalt-mountains, which I have ſeen in Haſſia, about Felſberg, Aldenberg, and Gudenſberg, have ſhewn me baſaltes without any addition; theſe mountains ſtanding by themſelves, and ſhewing no traces of either lavas or ſcoriæ.

For the illuſtration of this paper, I have cauſed two engravings to be made; *viz.* TAB. XVIII. of the baſalt-rock near Gudenſberg; and TAB. XIX. of the baſaltes of Felſberg.

I am, with particular regard,

Dear Sir,

Your very obedient ſervant,

R. E. Raſpe.

LV. *An*

LV. *An Attempt to explain ſome of the principal Phænomena of Electricity, by Means of an elaſtic Fluid: By the Honourable* Henry Cavendiſh, *F. R. S.*

Read Dec. 19, 1771, and Jan. 9, 1772.

SINCE I firſt wrote the following paper, I find that this way of accounting for the phænomena of electricity, is not new. Æpinus, in his Tentamen Theoriæ electricitatis & magnetiſmi, has made uſe of the ſame, or nearly the ſame hypotheſis that I have; and the concluſions he draws from it, agree nearly with mine, as far as he goes. However, as I have carried the theory much farther than he has done, and have conſidered the ſubject in a different, and, I flatter myſelf, in a more accurate manner, I hope the Society will not think this paper unworthy their acceptance.

The method I propoſe to follow is, firſt, to lay down the hypotheſis; next, to examine by ſtrict mathematical reaſoning, or at leaſt, as ſtrict reaſoning as the nature of the ſubject will admit of, what conſequences will flow from thence; and laſtly, to examine how far theſe conſequences agree with ſuch experiments as have yet been made on this ſubject. In a future paper, I intend to give the reſult of ſome experiments I am making, with intent to examine ſtill further the truth of this hypotheſis, and to find out the law of the electric attraction and repulſion.

J HYPO-

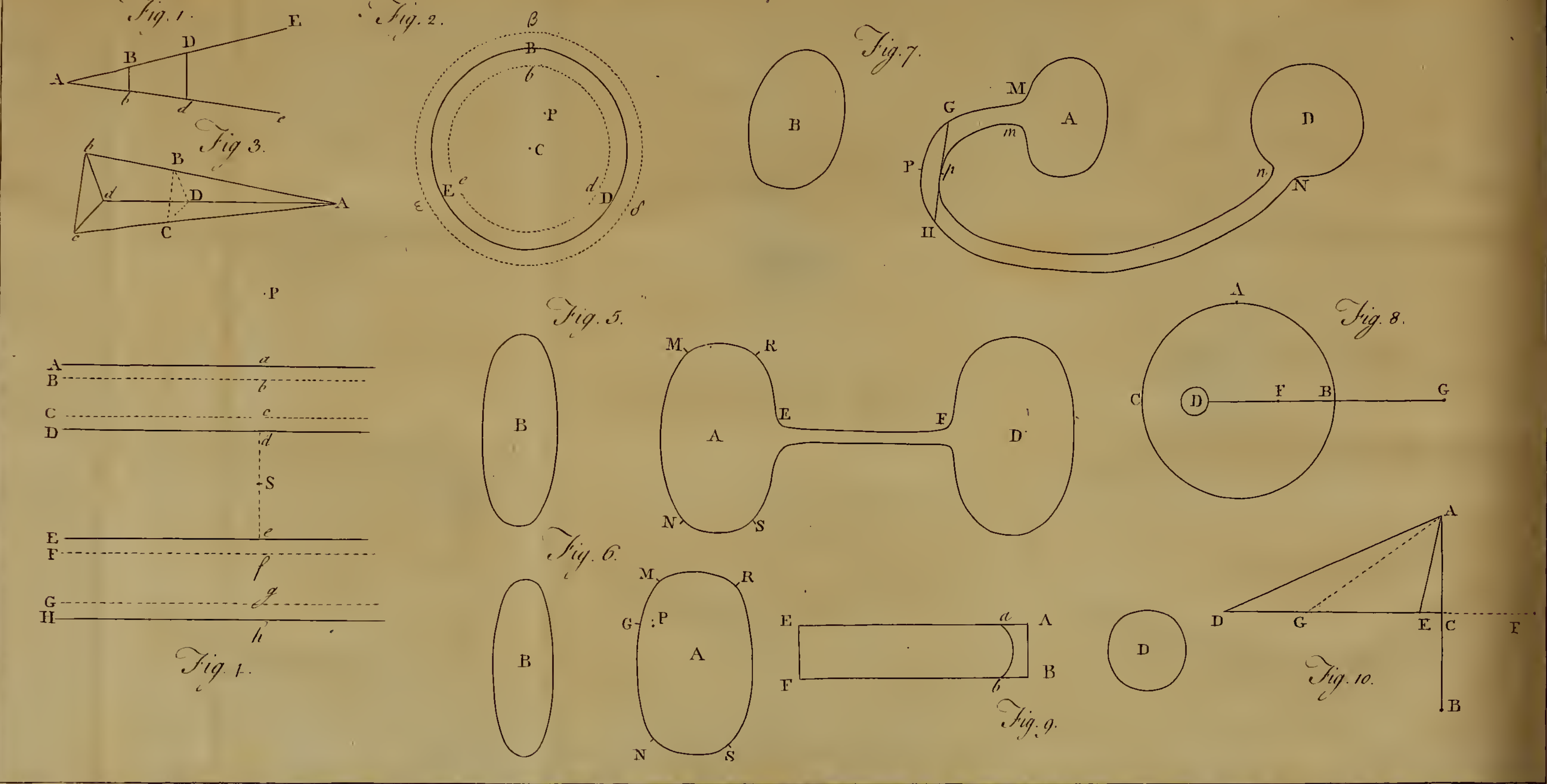


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
Fig. 10.

Hypothesis.

There is a ſubſtance, which I call the electric fluid, the particles of which repel each other and attract the particles of all other matter, with a force inverſely as ſome leſs power of the diſtance than the cube: the particles of all other matter alſo, repel each other, and attract thoſe of the electric fluid, with a force varying according to the ſame power of the diſtances. Or, to expreſs it more conciſely, if you look upon the electric fluid as matter of a contrary kind to other matter, the particles of all matter, both thoſe of the electric fluid and of other matter, repel particles of the ſame kind, and attract thoſe of a contrary kind, with a force inverſely as ſome leſs power of the diſtance than the cube.

For the future, I would be underſtood never to comprehend the electric fluid under the word matter, but only ſome other ſort of matter.

It is indifferent whether you ſuppoſe all ſorts of matter to be indued in an equal degree with the foregoing attraction and repulſion, or whether you ſuppoſe ſome ſorts to be indued with it in a greater degree than others; but it is likely that the electric fluid is indued with this property in a much greater degree than other matter; for in all probability the weight of the electric fluid in any body bears but a very ſmall proportion to the weight of the matter; but yet the force with which the electric fluid therein attracts any particle of matter muſt be equal to the force with which the matter therein repels that particle; otherwiſe the body would appear electrical, as will be ſhewn hereafter.

To explain this hypotheſis more fully, ſuppoſe that 1 grain of electric fluid attracts a particle of matter, at a given diſtance, with as much force as n grains of any matter, lead for inſtance, repel it: then will 1 grain of electric fluid repel a particle of electric fluid with as much force as n grains of lead attract it; and 1 grain of electric fluid will repel 1 grain of electric fluid with as much force as n grains of lead repel n grains of lead.

All bodies in their natural ſtate, with regard to electricity, contain ſuch a quantity of electric fluid interſperſed between their particles, that the attraction of the electric fluid in any ſmall part of the body on a given particle of matter ſhall be equal to the repulſion of the matter in the ſame ſmall part on the ſame particle. A body in this ſtate I call ſaturated with electric fluid: if the body contains more than this quantity of electric fluid, I call it overcharged: if leſs, I call it undercharged. This is the hypotheſis; I now proceed to examine the conſequences which will flow from it.

LEMMA I.

Let EAe (TAB. XX. fig. 1.) repreſent a cone continued infinitely; let A be the vertex, and Bb and Dd planes parallel to the baſe; and let the cone be filled with uniform matter, whoſe particles repel each other with a force inverſely as the n power of the diſtance. If n is greater than 3, the force with which a particle

at

at A is repelled by E B be or all that part of the cone beyond B b is as $\frac{1}{AB^{n-3}}$.

For fuppofing A B to flow, the fluxion of E B be is proportional to $-\dot{AB} \times AB^2$, and the fluxion of its repulfion on A is proportional to $\frac{-\dot{AB}}{AB^{n-2}}$; the fluent of which is $\frac{1}{n-3 \times AB^{n-3}}$; which when A B is infinite is equal to nothing; confequently the repulfion of E B be is proportional to $\frac{1}{n-3 \times AB^{n-3}}$ or to $\frac{1}{AB^{n-3}}$.

Corollary.

If A B is infinitely fmall, $\frac{1}{AB^{n-3}}$ is infinitely great; therefore the repulfion of that part of the cone between A and B b, on A, is infinitely greater than the repulfion of all that beyond it.

Lemma II.

By the fame method of reafoning it appears, that if n is equal to 3, the repulfion of the matter between B b and D d on a particle at A, is proportional to the logarithm of $\frac{AD}{AB}$; confequently, the repulfion of that part is infinitely fmall in refpect of that between A and B b, and alfo infinitely fmall in refpect of that beyond D d.

LEMMA III.

In like manner, if n is leſs than 3, the repulſion of the part between A and Bb on A is proportional to AB^{3-n}: conſequently the repulſion of the matter between A and Bb on A, is infinitely ſmall in reſpect of that beyond it.

COROLLARY.

It is eaſy to ſee from theſe three lemmata, that, if the electric attraction and repulſion had been ſuppoſed to be inverſely, as ſome higher power of the diſtance than the cube; a particle could not have been ſenſibly affected by the repulſion of any fluid, except what was placed cloſe to it. If the repulſion was inverſely, as the cube of the diſtance, a particle could not be ſenſibly affected by the repulſion of any finite quantity of fluid, except what was cloſe to it. But as the repulſion is ſuppoſed to be inverſely as ſome power of the diſtance leſs than the cube, a particle may be ſenſibly affected by the repulſion of a finite quantity of fluid, placed at any finite diſtance from it.

DEFINITION.

If the electric fluid in any body, is by any means confined in ſuch manner that it cannot move from one part of the body to the other, I call it immoveable: if it is able to move readily from one part to another, I call it moveable.

PROPOSITION I.

A body overcharged with electric fluid attracts or repels a particle of matter or fluid, and is attracted or repelled by it, with exactly the same force as it would, if the matter in it, together with so much of the fluid as is sufficient to saturate it, was taken away, or as if the body consisted only of the redundant fluid in it. In like manner an undercharged body attracts or repels with the same force, as if it consisted only of the redundant matter; the electric fluid, together with so much of the matter as is sufficient to saturate it, being taken away.

This is evident from the definition of saturation.

PROP. II.

Two over or undercharged bodies attract or repel each other with just the same force that they would, if each body consisted only of the redundant fluid in it, if overcharged, or of the redundant matter in it, if undercharged.

For, let the two bodies be called A and B; by the last proposition the redundant substance in B impels each particle of fluid and matter in A, and consequently impels the whole body A, with the same force that the whole body B impels it: for the same reason the redundant substance in A impels the redundant substance in B, with the same force that the whole body A impels it. It is ſhewn therefore, that the whole body B impels the whole body A, with the ſame force that the redundant ſubſtance in B impels the whole body A, or with which the whole body A impels the redundant ſubſtance in B; and that the whole body A impels the redundant ſubſtance in B, with the ſame force that the redundant ſubſtance in A impels the redundant ſubſtance in B; therefore the whole body B impels the whole body A, with the ſame force with which the redundant ſubſtance in A impels the redundant ſubſtance in B, or with which the redundant ſubſtance in B impels the redundant ſubſtance in A.

Corollary.

Let the matter in all the reſt of ſpace, except in two given bodies, be ſaturated with immoveable fluid; and let the fluid in thoſe two bodies be alſo immoveable. Then, if one of the bodies is ſaturated, and the other either over or undercharged, they will not at all attract or repel each other.

If the bodies are both overcharged, they will repel each other.

If they are both undercharged, they will alſo repel each other.

If one is overcharged and the other undercharged, they will attract each other.

N. B. In this corollary, when I call a body overcharged, I would be underſtood to mean, that it is overcharged in all parts, or at leaſt no where under-

undercharged: in like manner, when I call it undercharged, I mean that it is undercharged in all parts, or at leaſt no where overcharged.

PROP. III.

If all the bodies in the univerſe are ſaturated with electric fluid, it is plain that no part of the fluid can have any tendency to move.

PROP. IV.

If the quantity of electric fluid in the univerſe is exactly ſufficient to ſaturate the matter therein, but unequally diſperſed, ſo that ſome bodies are overcharged and others undercharged; then, if the electric fluid is not confined, it will immediately move till all the bodies in the univerſe are ſaturated.

For, ſuppoſing that any body is overcharged, and the bodies near it are not, a particle at the ſurface of that body will be repelled from it by the redundant fluid within; conſequently ſome fluid will run out of that body; but if the body is undercharged, a particle at its ſurface will be attracted towards the body by the redundant matter within, ſo that ſome fluid will run into the body.

N. B. In Prob. IV. Caſe III. there will be ſhewn an exception to this propoſition; there may perhaps be ſome other exceptions to it: but I think there can be no doubt, but what this proposition must hold good in general.

Lemma IV.

Let B D E, *bde*, and $\beta\delta\varepsilon$ (fig. 2.) be concentric spherical surfaces, whose center is C: if the space * B *b* is filled with uniform matter, whose particles repel with a force inversely, as the square of the distance, a particle placed any where within the space C *b*, as at P, will be repelled with as much force in one direction as another, or it will not be impelled in any direction. This is demonstrated in Newt. Princip. liber I. prop. lxx. It follows also from his demonstration, that if the repulsion is inversely, as some higher power of the distance than the square, the particle P will be impelled towards the center; and if the repulsion is inversely as some lower power than the square, it will be impelled from the center.

Lemma V.

If the repulsion is inversely as the square of the distance, a particle placed any where without the sphere B D E, is repelled by that sphere, and also by the space B *b*, with the same force that it would if all the matter therein was collected in the center of the sphere;

* By the space B *b* or B β, I mean the space comprehended between the spherical surfaces B D E and *bde*, or between B D E and $\beta\delta\varepsilon$: by the space C *b* or C β, I mean the spheres *bde* or $\beta\delta\varepsilon$.

ſphere; provided the denſity of the matter therein is every where the ſame at the ſame diſtance from the center. This is eaſily deduced from prop. 71. of the ſame book, and has been demonſtrated by other authors.

PROP. V.

Problem 1. Let the ſphere B D E be filled with uniform ſolid matter, overcharged with electric fluid: let the fluid therein be moveable, but unable to eſcape from it: let the fluid in the reſt of infinite ſpace be moveable, and ſufficient to ſaturate the matter therein; and let the matter in the whole of infinite ſpace, or at leaſt in the ſpace B β, whoſe dimenſions will be given below, be uniform and ſolid; and let the law of the electric attraction and repulſion be inverſely as the ſquare of the diſtance: it is required to determine in what manner the fluid will be diſpoſed both within and without the globe.

Take the ſpace B b ſuch, that the interſtices between the particles of matter therein ſhall be juſt ſufficient to hold a quantity of electric fluid, whoſe particles are preſſed cloſe together, ſo as to touch each other, equal to the whole redundant fluid in the globe, beſides the quantity requiſite to ſaturate the matter in B b; and take the ſpace B β ſuch, that the matter therein ſhall be juſt able to ſaturate the redundant fluid in the globe: then, in all parts of the ſpace B b, the fluid will be preſſed cloſe together, ſo

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that its particles ſhall touch each other; the ſpace Bβ will be intirely deprived of fluid; and in the ſpace Cb, and all the reſt of infinite ſpace, the matter will be exactly ſaturated.

For, if the fluid is diſpoſed in the above-mentioned manner, a particle of fluid placed anywhere within the ſpace Cb will not be impelled in any direction by the fluid in Bb, or the matter in Bβ, and will therefore have no tendency to move: a particle placed anywhere without the ſphere $\beta\delta\epsilon$ will be attracted with juſt as much force by the matter in Bβ, as it is repelled by the redundant fluid in Bb, and will therefore have no tendency to move: a particle placed anywhere within the ſpace Bb, will indeed be repelled towards the ſurface, by all the redundant fluid in that ſpace which is placed nearer the center than itſelf; but as the fluid in that ſpace is already preſſed as cloſe together as poſſible, it will not have any tendency to move; and in the ſpace Bβ there is no fluid to move, ſo that no part of the fluid can have any tendency to move.

Moreover, it ſeems impoſſible for the fluid to be at reſt, if it is diſpoſed in any other form; for if the denſity of the fluid is not everywhere the ſame at the ſame diſtance from the center, but is greater near b than near d, a particle placed anywhere between thoſe two points will move from b towards d; but if the denſity is everywhere the ſame at the ſame diſtance from the center, and the fluid in Bb is not preſſed cloſe together, the ſpace Cb will be overcharged, and conſequently a particle at b will be repelled from the center, and cannot be at reſt: in like manner, if there is any fluid in Bβ, it cannot

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be at reſt: and, by the ſame kind of reaſoning, it might be ſhewn, that, if the fluid is not ſpread uniformly within the ſpace C b, and without the ſphere $\beta\delta\varepsilon$, it cannot be at reſt.

Corollary I.

If the globe B D E is undercharged, every thing elſe being the ſame as before, there will be a ſpace B b, in which the matter will be intirely deprived of fluid, and a ſpace B β, in which the fluid will be preſſed cloſe together; the matter in B b being equal to the whole redundant matter in the globe, and the redundant fluid in B β, being juſt ſufficient to ſaturate the matter in B b: and in all the reſt of ſpace the matter will be exactly ſaturated. The demonſtration is exactly ſimilar to the foregoing.

Corol. II.

The fluid in the globe B D E will be diſpoſed in exactly the ſame manner, whether the fluid without is immoveable, and diſpoſed in ſuch manner, that the matter ſhall be everywhere ſaturated, or whether it is diſpoſed as above deſcribed; and the fluid without the globe will be diſpoſed in juſt the ſame manner, whether the fluid within is diſpoſed uniformly, or whether it is diſpoſed as above deſcribed.

 PROP.

PROP. VI.

PROB. 2. To determine in what manner the fluid will be diſpoſed in the globe BDE, ſuppoſing every thing as in the laſt problem, except that the fluid on the outſide of the globe is immoveable, and diſpoſed in ſuch manner as everywhere to ſaturate the matter, and that the electric attraction and repulſion is inverſely, as ſome other power of the diſtance than the ſquare.

I am not able to anſwer this problem accurately; but I think we may be certain of the following circumſtances.

CASE 1. Let the repulſion be inverſely as ſome power of the diſtance between the ſquare and the cube, and let the globe be overcharged.

It is certain that the denſity of the fluid will be everywhere the ſame, at the ſame diſtance from the center. Therefore, firſt, There can be no ſpace as C *b*, within which the matter will be everywhere ſaturated; for a particle at *b* is impelled towards the center, by the redundant fluid in B *b*, and will therefore move towards the center, unleſs C *b* is ſufficiently overcharged to prevent it. Secondly, The fluid cloſe to the ſurface of the ſphere will be preſſed cloſe together; for otherwiſe a particle ſo near to it, that the quantity of fluid between it and the ſurface ſhould be very ſmall, would move towards it; as the repulſion of the ſmall quantity of fluid between

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it and the ſurface, would be unable to balance the repulſion of the fluid on the other ſide. Whence, I think, we may conclude, that the denſity of the fluid will increaſe gradually from the center to the ſurface, where the particles will be preſſed cloſe together: whether the matter exactly at the center will be overcharged, or only ſaturated, I cannot tell.

COROLLARY.

For the ſame reaſon, if the globe be undercharged, I think we may conclude, that the denſity of the fluid will diminiſh gradually from the center to the ſurface, where the matter will be entirely deprived of fluid.

CASE 2. Let the repulſion be inverſely as ſome power of the diſtance leſs than the ſquare; and let the globe be overcharged.

There will be a ſpace B*b*, in which the particles of the fluid will be everywhere preſſed cloſe together; and the quantity of redundant fluid in that ſpace will be greater than the quantity of redundant fluid in the whole globe BDE; ſo that the ſpace C*b*, taken all together, will be undercharged: but I cannot tell in what manner the fluid will be diſpoſed in that ſpace.

For it is certain, that the denſity of the fluid will be everywhere the ſame, at the ſame diſtance from the center. Therefore, let *b* be any point where the fluid is not preſſed cloſe together, then will a particle at *b* be impelled towards the ſurface, by the redundant

redundant fluid in the ſpace Bb; therefore, unleſs the ſpace Cb is undercharged, the particle will move towards the ſurface.

Corollary.

For the ſame reaſon, if the globe is undercharged, there will be a ſpace Bb, in which the matter will be intirely deprived of fluid, the quantity of matter therein being more than the whole redundant matter in the globe; and, conſequently, the ſpace Cb, taken all together, will be overcharged.

Lemma VI.

Let the whole ſpace comprehended between two parallel planes, infinitely extended each way, be filled with uniform matter, the repulſion of whoſe particles is inverſely as the ſquare of the diſtance; the plate of matter formed thereby will repel a particle of matter with exactly the ſame force, at whatever diſtance from it, it be placed.

For, ſuppoſe that there are two ſuch plates, of equal thickneſs, placed parallel to each other, let A (fig. 3.) be any point not placed in or between the two plates: let BCD repreſent any part of the neareſt plate: draw the lines AB, AC, and AD, cutting the furtheſt plate in b, c, and d; for it is plain, that if they cut one plate, they muſt, if produced, cut the other: the triangle BCD is to the triangle bcd, as AB^2 to Ab^2; therefore a particle of matter at A will be repelled with the ſame force by the matter in the triangle BCD, as by that in bcd. Whence it appears, that a particle at A will

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be repelled with as much force by the neareſt plate, as by the more diſtant; and conſequently, will be impelled with the ſame force by either plate, at whatever diſtance from it it be placed.

Corollary.

If the repulſion of the particles is inverſely as ſome higher power of the diſtance than the ſquare, the plate will repel a particle with more force, if its diſtance be ſmall than if it be great; and if the repulſion is inverſely as ſome lower power than the ſquare, it will repel a particle with leſs force, if its diſtance be ſmall than if it be great.

PROP. VII.

Prob. 3. In fig. 4. let the parallel lines A*a*, B*b*, &c. repreſent parallel planes infinitely extended each way: let the ſpaces * AD and EH be filled with uniform ſolid matter: let the electric fluid in each of thoſe ſpaces be moveable and unable to eſcape: and let all the reſt of the matter in the univerſe be ſaturated with immoveable fluid; and let the electric attraction and repulſion be inverſely as the ſquare of the diſtance. It is required to determine in what manner the fluid will be diſpoſed in the ſpaces AD and EH, according as one or both of them are over or undercharged.

* By the ſpace AD or AB, &c. I mean the ſpace comprehended between the planes A*a* and D*d*, or between A*a* and B*b*.

Let

Let A D be that ſpace which contains the greateſt quantity of redundant fluid, if both ſpaces are overcharged, or which contains the leaſt redundant matter, if both are undercharged; or, if one is overcharged, and the other undercharged, let A D be the overcharged one. Then, firſt, There will be two ſpaces, A B and G H, which will either be intirely deprived of fluid, or in which the particles will be preſſed cloſe together; namely, if the whole quantity of fluid in A D and E H together, is leſs than ſufficient to ſaturate the matter therein, they will be intirely deprived of fluid; the quantity of redundant matter in each being half the whole redundant matter in A D and E H together: but if the fluid in A D and E H together is more than ſufficient to ſaturate the matter, the fluid in A B and G H will be preſſed cloſe together; the quantity of redundant fluid in each being half the whole redundant fluid in both ſpaces. 2dly, In the ſpace C D the fluid will be preſſed cloſe together; the quantity of fluid therein being ſuch, as to leave juſt enough fluid in B C to ſaturate the matter therein. 3dly, The ſpace E F will be intirely deprived of fluid; the quantity of matter therein being ſuch, that the fluid in F G ſhall be juſt ſufficient to ſaturate the matter therein: conſequently, the redundant fluid in C D will be juſt ſufficient to ſaturate the redundant matter in E F; for as A B and G H together contain the whole redundant fluid or matter in both ſpaces, the ſpaces B D and E G together contain their natural quantity of fluid; and therefore, as BC and F G each contain their natural quantity of fluid, the ſpaces C D and E F together contain their

natural

natural quantity of fluid. And, 4thly, The ſpaces BC and FG will be ſaturated in all parts.

For, firſt, If the fluid is diſpoſed in this manner, no particle of it can have any tendency to move: for a particle placed anywhere in the ſpaces BC and FG, is attracted with juſt as much force by EF, as it is repelled by CD; and it is repelled or attracted with juſt as much force by AB, as it is in a contrary direction by GH, and, conſequently, has no tendency to move. A particle placed anywhere in the ſpace CD, or in the ſpaces AB and GH, if they are overcharged, is indeed repelled with more force towards the planes D*d*, A*a*, and H*h*, than it is in the contrary direction; but as the fluid in thoſe ſpaces is already as much compreſſed as poſſible, the particle will have no tendency to move.

2dly, It ſeems impoſſible that the fluid ſhould be at reſt, if it is diſpoſed in any other manner: but as this part of the demonſtration is exactly ſimilar to the latter part of that of Problem the firſt, I ſhall omit it.

Corol. I.

If the two ſpaces AD and EH are both overcharged, the redundant fluid in CD is half the difference of the redundant fluid in thoſe ſpaces: for half the difference of the redundant fluid in thoſe ſpaces, added to the quantity in AB, which is half the ſum, is equal to the whole quantity in AD. For a like reaſon, if AD and EH are both undercharged, the redundant matter in EF is half the difference of the redundant matter in thoſe ſpaces; and if AD is

 over-

overcharged, and EH undercharged, the redundant fluid in CD exceeds half the redundant fluid in AD, by a quantity ſufficient to ſaturate half the redundant matter in EH.

COROL. II.

It was before ſaid, that the fluid in the ſpaces AB and GH (when there is any fluid in them) is repelled againſt the planes A*a* and H*b*; and, conſequently, would run out through thoſe planes, if there was any opening for it to do ſo. The force with which the fluid preſſes againſt the planes A*a* and H*b*, is that with which the redundant fluid in AB is repelled by that in GH; that is, with which half the redundant fluid in both ſpaces is repelled by an equal quantity of fluid. Therefore, the preſſure againſt A*a* and H*b* depends only on the quantity of redundant fluid in both ſpaces together, and not at all on the thickneſs or diſtance of thoſe ſpaces, or on the proportion in which the fluid is divided between the two ſpaces. If there is no fluid in AB and GH, a particle placed on the outſide of the ſpaces AD and EH, contiguous to the planes A*a* or H*b*, is attracted towards thoſe planes by all the matter in AB and GH, *id eſt*, by all the redundant matter in both ſpaces; and, conſequently, endeavours to inſinuate itſelf into the ſpace AD or EH; and the force with which it does ſo, depends only on the quantity of redundant matter in both ſpaces together. The fluid in CD alſo preſſes againſt the plane D*d*, and the force with which it does ſo, is that with which

which the redundant fluid in C D is attracted by the matter in E F.

Corol. III.

If A D is overcharged, and E H undercharged, and the redundant fluid in A D is exactly sufficient to saturate the redundant matter in E H, all the redundant fluid in A D will be collected in the space C D, where it will be pressed close together: the space E F will be intirely deprived of fluid, the quantity of matter therein being just sufficient to saturate the redundant fluid in C D, and the spaces A C and F H will be everywhere saturated. Moreover, if an opening is made in the planes A *a* or H *b*, the fluid within the spaces A D or E H will have no tendency to run out thereat, nor will the fluid on the outside have any tendency to run in at it: a particle of fluid too placed anywhere on the outside of both spaces, as at P, will not be at all attracted or repelled by those spaces, any more than if they were both saturated; but a particle placed anywhere between those spaces, as at S, will be repelled from *d* towards *e*; and if a communication was made between the two spaces, by the canal *d e*, the fluid would run out of A D into E H, till they were both saturated.

PROP. VIII.

Prob. 4. To determine in what manner the fluid will be difpofed in the fpace A D, fuppofing that all the reft of the univerfe is faturated with immoveable fluid, and that the electric attraction and repulfion is inverfely as fome other power of the diftance than the fquare.

I am not able to anfwer this Problem accurately, except when the repulfion is inverfely as the fimple or fome lower power of the diftance; but I think we may be certain of the following circumftances.

Case 1. Let the repulfion be inverfely as fome power of the diftance between the fquare and the cube, and let A D be overcharged.

Firft, It is certain that the denfity of the fluid muft be everywhere the fame, at the fame diftance from the planes A*a* and D*d*. 2dly, There can be no fpace as BC, of any fenfible breadth, in which the matter will not be overcharged. And, 3dly, The fluid clofe to the planes A*a* and D*d* will be preffed clofe together. Whence, I think, we may conclude, that the denfity of the fluid will increafe gradually from the middle of the fpace to the outfide, where it will be preffed clofe together. Whether the matter exactly in the middle will be overcharged, or only faturated, I cannot tell.

Case 2. Let the repulſion be inverſely as ſome power of the diſtance between the ſquare and the ſimple power, and let A D be overcharged.

There will be two ſpaces A B and D C, in which the fluid will be preſſed cloſe together, and the quantity of redundant fluid in each of thoſe ſpaces will be more than half the redundant fluid in A D; ſo that the ſpace B C, taken all together, will be undercharged; but I cannot tell in what manner the fluid will be diſpoſed in that ſpace. The demonſtrations of theſe two caſes are exactly ſimilar to thoſe of the two caſes of Prob. 2.

Case 3. If the repulſion is inverſely as the ſimple or ſome lower power of the diſtance, and A D is overcharged, all the fluid will be collected in the ſpaces A B and C D, and B C will be intirely deprived of fluid. If A D contains juſt fluid enough to ſaturate it, and the repulſion is inverſely as the diſtance, the fluid will remain in equilibrio, in whatever manner it is diſpoſed; provided its denſity is everywhere the ſame, at the ſame diſtance from the planes A *a* and D *d*: but if the repulſion is inverſely as ſome leſs power than the ſimple one, the fluid will be in equilibrio, whether it is either ſpread uniformly, or whether it is all collected in that plane which is in the middle between A *a* and D *d*, or whether it is all collected in the ſpaces A B and C D; but not, I believe, if it is diſpoſed in any other manner.

The demonſtration depends upon this circumſtance; namely, that if the repulſion is inverſely as the diſtance, two ſpaces A B and C D, repel a particle.

ticle, placed either between them, or on the outſide of them, with the ſame force as if all the matter of thoſe ſpaces was collected in the middle plane between them.

It is needleſs mentioning the three caſes in which A D is undercharged, as the reader will eaſily ſupply the place.

Though the four foregoing problems do not immediately tend to explain the phænomena of electricity, I choſe to inſert them; partly becauſe they ſeem worth engaging our attention in themſelves; and partly becauſe they ſerve, in ſome meaſure, to confirm the truth of ſome of the following propoſitions, in which I am obliged to make uſe of a leſs accurate kind of reaſoning.

In the following propoſitions, I ſhall always ſuppoſe the bodies I ſpeak of to conſiſt of ſolid matter, confined to the ſame ſpot, ſo as not to be able to alter its ſhape or ſituation by the attraction or repulſion of other bodies on it: I ſhall alſo ſuppoſe the electric fluid in theſe bodies to be moveable, but unable to eſcape, unleſs when otherwiſe expreſſed. As for the matter in all the reſt of the univerſe, I ſhall ſuppoſe it to be ſaturated with immoveable fluid. I ſhall alſo ſuppoſe the electric attraction and repulſion to be inverſely as any power of the diſtance leſs than the cube, except when otherwiſe expreſſed.

By a canal, I mean a ſlender thread of matter, of ſuch kind that the electric fluid ſhall be able to move readily along it, but ſhall not be able to eſcape from it, except at the ends, where it communicates with other bodies. Thus, when I ſay that two bodies com-

communicate with each other by a canal, I mean that the fluid ſhall be able to paſs readily from one body to the other by that canal.

PROP. IX.

If any body at a diſtance from any over or undercharged body be overcharged, the fluid within it will be lodged in greater quantity near the ſurface of the body than near the center. For, if you ſuppoſe it to be ſpread uniformly all over the body, a particle of fluid in it, near the ſurface, will be repelled towards the ſurface, by a greater quantity of fluid than that by which it is repelled from it; conſequently, the fluid will flow towards the ſurface, and make it denſer there: moreover, the particles of fluid cloſe to the ſurface will be preſſed cloſe together; for otherwiſe, a particle placed ſo near it, that the quantity of redundant fluid between it and the ſurface ſhould be very ſmall, would move towards it; as the ſmall quantity of redundant fluid between it and the ſurface would be unable to balance the repulſion of that on the other ſide.

From the four foregoing problems it ſeems likely, that if the electric attraction or repulſion is inverſely as the ſquare of the diſtance, almoſt all the redundant fluid in the body will be lodged cloſe to the ſurface, and there preſſed cloſe together, and the reſt of the body will be ſaturated. If the repulſion is inverſely as ſome power of the diſtance between the ſquare and the cube, it is likely that all parts of the body will be overcharged : and if it is inverſely as ſome leſs power than the ſquare, it is likely that all parts of the body, except thoſe near the ſurface, will be undercharged.

COROLLARY.

For the ſame reaſon, if the body is undercharged, the deficiency of fluid will be greater near the ſurface than near the center, and the matter near the ſurface will be intirely deprived of fluid. It is likely too, if the repulſion is inverſely as ſome higher power of the diſtance than the ſquare, that all parts of the body will be undercharged : if it is inverſely as the ſquare, that all parts, except near the ſurface, will be ſaturated : and if it is inverſely as ſome leſs power than the ſquare, that all parts, except near the ſurface, will be overcharged.

PROP. X.

Let the bodies A and D (fig. 5.) communicate with each other, by the canal E F; and let one of them, as D, be overcharged; the other body A will be ſo alſo.

For as the fluid in the canal is repelled by the redundant fluid in D, it is plain, that unleſs A was overcharged, ſo as to balance that repulſion, the fluid would run out of D into A.

In like manner, if one is undercharged, the other muſt be ſo too.

PROP. XI.

Let the body A (fig. 6.) be either ſaturated or over or undercharged; and let the fluid within it be in equilibrio. Let now the body B, placed near it, be rendered overcharged, the fluid within it being ſuppoſed immoveable, and diſpoſed in ſuch manner, that no part of it ſhall be undercharged; the fluid in A will no longer be in equilibrio, but will be repelled from B: therefore, the fluid will flow from thoſe parts of A which are neareſt to B, to thoſe which are more diſtant from it; and, conſequently, the part adjacent to M N (that part of the ſurface of A which is turned towards B) will be made to contain leſs electric fluid than it did before, and that adjacent to the oppoſite ſurface R S will contain more than before.

It muſt be obſerved, that when a ſufficient quantity of fluid has flowed from M N towards R S, the repulſion which the fluid in the part adjacent to M N exerts on the reſt of the fluid in A, will be ſo much weakened, and the repulſion of that in the part near R S will be ſo much increaſed, as to compenſate the repulſion of B, which will prevent any more fluid flowing from M N to R S.

The reaſon why I ſuppoſe the fluid in B to be immoveable is, that otherwiſe a queſtion might ariſe, whether the attraction or repulſion of the body A might not cauſe ſuch an alteration in the diſpoſition of the fluid in B, as to cauſe ſome parts of it to be undercharged; which might make it doubtful, whether B did on the whole repel the fluid in A. It is evident, however, that this propoſition would hold good, though ſome parts of B were undercharged, provided it did on the whole repel the fluid in A.

COROLLARY.

If B had been made undercharged, inſtead of overcharged, it is plain that ſome fluid would have flowed from the further part R S to the nearer part M N, inſtead of from M N to R S.

PROP. XII.

Let us now ſuppoſe that the body A communicates by the canal E F, with another body D, placed on the contrary ſide of it from B, as in fig. 5; and let theſe two bodies be either ſaturated, or over or undercharged; and let the fluid within them be in equilibrio. Let now the body B be overcharged: it is plain that ſome fluid will be driven from the nearer part M N to the further part R S, as in the former propoſition; and alſo ſome fluid will be driven from R S, through the canal, to the body D; ſo that the quantity of fluid in D will be increaſed thereby, and the quantity in A, taking the whole body together, will be diminiſhed; the quantity in the part near M N will alſo be diminiſhed; but whether the quantity in the part near R S will be diminiſhed or not, does not appear for certain; but I ſhould imagine it would be not much altered.

COROL-

COROLLARY.

In like manner, if B is made undercharged, ſome fluid will flow from D to A, and alſo from that part of A near RS, to the part near MN.

PROP. XIII.

Suppoſe now that the bodies A and D communicate by the bent canal M P N *n p m* (fig. 7.) inſtead of the ſtraight one EF: let the bodies be either ſaturated or over or undercharged as before; and let the fluid be at reſt; then if the body B is made overcharged, ſome fluid will ſtill run out of A into D; provided the repulſion of B on the fluid in the canal is not too great.

The repulſion of B on the fluid in the canal, will at firſt drive ſome fluid out of the leg MP*pm* into A, and out of NP*pn* into D, till the quantity of fluid in that part of the canal which is neareſt to B is ſo much diminiſhed, and its repulſion on the reſt of the fluid in the canal is ſo much diminiſhed alſo as to compenſate the repulſion of B: but as the leg NP*pn* is longer than the other, the repulſion of B on the fluid in it will be greater; conſequently ſome fluid will run out of A into D, on the ſame principle that water is drawn out of a veſſel through a ſyphon: but if the repulſion of B on the fluid in the canal is ſo great, as to drive all the fluid out of the ſpace GPH *p*G, ſo that the fluid in the leg MG*pm* does not

 join

join to that in N H pn; then it is plain that no fluid can run out of A into D; any more than water will run out of a veſſel through a ſyphon, if the height of the bend of the ſyphon above the water in the veſſel, is greater than that to which water will riſe in vacuo.

Corollary.

If B is made undercharged, ſome fluid will run out of D into A; and that though the attraction of B on the fluid in the canal is ever ſo great.

PROP. XIV.

Let ABC (fig. 8.) be a body overcharged with immoveable fluid, uniformly ſpread; let the bodies near ABC on the outſide be ſaturated with immoveable fluid; and let D be a body incloſed within ABC, and communicating by the canal DG with other diſtant bodies ſaturated with fluid; and let the fluid in D and the canal and thoſe bodies be moveable; then will the body D be rendered undercharged.

For let us firſt ſuppoſe that D and the canal are ſaturated, and that D is nearer to B than to the oppoſite part of the body, C; then will all the fluid in the canal be repelled from C by the redundant fluid in A B C; but if D is nearer to C than to B, take the point F, ſuch that a particle placed there would be repelled from C with as much force as one at D is repelled towards C; the fluid in D F, taking the whole

whole together, will be repelled with as much force one way as the other; and the fluid in FG is all of it repelled from C: therefore in both cafes the fluid in the canal, taking the whole together, is repelled from C; confequently fome fluid will run out of D and the canal, till the attraction of the unfaturated matter therein is fufficient to balance the repulfion of the redundant fluid in ABC.

PROP. XV.

If we now fuppofe that the fluid on the outfide of ABC is moveable; the matter adjacent to ABC on the outfide, will become undercharged. I fee no reafon however to think that that will prevent the body D from being undercharged; but I cannot fay exactly what effect it will have, except when ABC is fpherical and the repulfion is inverfely as the fquare of the diftance; in this cafe it appears by Prob. I. that the fluid in the part DB of the canal will be repelled from C, with juft as much force as in the laft propofition; but the fluid in the part BG will not be repelled at all: confequently D will be undercharged, but not fo much as in the laft propofition.

Corollary.

If ABC is now fuppofed to be undercharged, it is certain that D will be overcharged, provided the matter near ABC on the outfide is faturated with immoveable

moveable fluid; and there is great reaſon to think that it will be ſo, though the fluid in that matter is moveable.

PROP. XVI.

Let A E F B (fig. 9.) be a long cylindric body, and D an undercharged body; and let the quantity of fluid in A E F B be ſuch, that the part near E F ſhall be ſaturated. It appears from what has been ſaid before, that the part near A B will be overcharged; and moreover there will be a certain ſpace, as A *a b* B, adjoining to the plane A B, in which the fluid will be preſſed cloſe together; and the fluid in that ſpace will preſs againſt the plane A B, and will endeavour to eſcape from it; and by Prop. II. the two bodies will attract each other: now I ſay that the force with which the fluid preſſes againſt the plane A B, is very nearly the ſame with which the two bodies attract each other in the direction E A; provided that no part of A E F B is undercharged.

Suppoſe ſo much of the fluid in each part of the cylinder as is ſufficient to ſaturate the matter in that part, to become ſolid; the remainder, or the redundant fluid remaining fluid as before. In this caſe the preſſure againſt the plane A B muſt be exactly equal to that with which the two bodies attract each other, in the direction E A: for the force with which D attracts that part of the fluid which we ſuppoſed to become ſolid, is exactly equal to that, with which it repels

repels the matter in the cylinder; and the redundant fluid in E *a b* F is at liberty to move, if it had any tendency to do ſo, without moving the cylinder; ſo that the only thing which has any tendency to impel the cylinder in the direction E A is the preſſure of the redundant fluid in A *a b* B againſt A B; and as the part near E F is ſaturated, there is no redundant fluid to preſs againſt the plane E F, and thereby to counteract the preſſure againſt A B. Suppoſe now all the electric fluid in the cylinder to become fluid; the force with which the two bodies attract each other will remain exactly the ſame; and the only alteration in the preſſure againſt A B, will be, that that part of the fluid in A *a b* B, which we at firſt ſuppoſed ſolid and unable to preſs againſt the plane, will now be at liberty to preſs againſt it; but as the denſity of the fluid when its particles are preſſed cloſe together may be ſuppoſed many times greater than when it is no denſer than ſufficient to ſaturate the matter in the cylinder, and conſequently the quantity of redundant fluid in A *a b* B many times greater than that which is required to ſaturate the matter therein, it follows that the preſſure againſt A B will be very little more than on the firſt ſuppoſition.

N. B. If any part of the cylinder is undercharged, the preſſure againſt A B is greater than the force with which the bodies attract. If the electric repulſion is inverſely as the ſquare or ſome higher power of the diſtance, it ſeems very unlikely that any part of the cylinder ſhould be undercharged; but if the repulſion is inverſely as ſome lower power than the ſquare, it

6 is

is not improbable but ſome part of the cylinder may be undercharged.

LEMMA VII.

Let AB (fig. 10.) repreſent an infinitely thin flat circular plate, ſeen edgeways, ſo as to appear to the eye as a ſtraight line; let C be the center of the circle; and let D C paſſing through C, be perpendicular to the plane of the plate; and let the plate be of uniform thickneſs, and conſiſt of uniform matter, whoſe particles repel with a force inverſely as the *n* power of the diſtance; *n* being greater than one, and leſs than three: the repulſion of the plate on a particle at D is proportional to $\frac{DC}{DC^{n-1}} - \frac{DC}{DA^{n-1}}$; provided the thickneſs of the plate and ſize of the particle D is given.

For if C A is ſuppoſed to flow, the correſponding fluxion of the quantity of matter in the plate, is proportional to $CA \times C\dot{A}$; and the correſponding fluxion of the repulſion of the plate on the particle D, in the direction D C, is proportional to $\frac{CA \times C\dot{A}}{DA^n} \times \frac{DC}{DA}, = \frac{D\dot{A} \times DC}{DA^n}$; for $D\dot{A}$ is to $C\dot{A}$: : C A : D A; the variable part of the fluent of which is $\frac{-DC}{\overline{n-1} \times DA^{n-1}}$: whence the repulſion of the plate on the particle D is proportional to $\frac{DC}{\overline{n-1} \times DC^{n-1}} - \frac{DC}{\overline{n-1} \times DA^{n-1}}$, or to $\frac{DC}{DC^{n-1}} - \frac{DC}{DA^{n-1}}$.

COROL-

COROLLARY.

If DC^{n-1} is very ſmall in reſpect of CA^{n-1}, the particle D is repelled with very nearly the ſame force as if the diameter of the plate was infinite.

LEMMA VIII.

Let L and l repreſent the two legs of a right angled triangle, and b the hypothenuſe; if the ſhorter leg l is ſo much leſs than the other, that l^{n-1} is very ſmall in reſpect of L^{n-1}, $b^{3-n} - L^{3-n}$ will be very ſmall in reſpect of l^{3-n}.

For $b^{3-n} = \overline{L^2 + l^2}|^{\frac{3-n}{2}}$, $= L^{3-n} \times \overline{1 + \frac{l^2}{L^2}}|^{\frac{3-n}{2}}$, $=$ $L^{3-n} \times 1 + \frac{3-n \times l^2}{2L^2} - \frac{3-n \times n-1 \times l^4}{8L^4}$, &c. therefore $b^{3-n} - L^{3-n} = \frac{3-n \times l^2}{2L^{n-1}} - \frac{3-n \times n-1 \times l^4}{8L^{n+1}}$, &c. $=$ $\frac{l^{3-n} \times 3-n \times l^{n-1}}{2L^{n-1}} - \frac{l^{3-n} \times 3-n \times n-1 \times l^{n+1}}{8L^{n+1}}$, &c. which is very ſmall in reſpect of l^{3-n}; as l^{n-1} is by the ſuppoſition very ſmall in reſpect of L^{n-1}.

LEMMA IX.

Let DC now repreſent the axis of a cylindric or priſmatic column of uniform matter; and let the diameter of the column be ſo ſmall, that the repulſion of the plate AB on it ſhall not be ſenſibly different from what it would be, if all the matter in it was collected in the axis: the force with which the plate repells the column, is proportional to $DC^{3-n}+AC^{3-n}-DA^{3-n}$; supposing the thickness of the plate and base of the column to be given.

For, if DC is supposed to flow, the corresponding fluxion of the repulsion is proportional to $\frac{\dot{DC}}{DC^{n-2}} - \frac{DC \times \dot{DC}}{DA^{n-1}} = \frac{\dot{DC}}{DC^{n-2}} - \frac{\dot{DA}}{DA^{n-2}}$; the fluent of which, $\frac{AC^{3-n}+DC^{3-n}-DA^{3-n}}{3-n}$, vanishes when DC vanishes.

COROLL. I.

If the length of the column is so great that AC^{n-1} is very small in respect of DC^{n-1}, the repulsion of the plate on it is very nearly the same as if the column was infinitely continued.

For by Lemma 8, $AC^{3-n}+DC^{3-n}-DA^{3-n}$ differs very little in this case from AC^{3-n}; and if DC is infinite, it is exactly equal to it.

COROLL. II.

If AC^{n-1} is very small in respect of DC^{n-1}, and the point E be taken in DC such that EC^{n-1}, shall be very small in respect of AC^{n-1}, the repulsion of the plate on the small part of the column EC, is to its repulsion on the whole column DC, very nearly as EC^{3-n} to AC^{3-n}.

LEMMA.

Lemma X.

If we now ſuppoſe all the matter of the plate to be collected in the circumference of the circle, ſo as to form an infinitely ſlender uniform ring, its repulſion on the column DC will be leſs than when the matter is ſpread uniformly all over the plate, in the ratio of $\frac{\overline{3-n}\times AC^2}{2}\times\overline{\frac{1}{AC^{n-1}}-\frac{1}{DA^{n-1}}}$ to $DC^{3-n}+AC^{3-n}-DA^{3-n}$.

For it was before ſaid, that if the matter of the plate be ſpread uniformly, its repulſion on the column will be proportional to $DC^{3-n}+AC^{3-n}-DA^{3-n}$, or may be expreſſed thereby; let now A C, the ſemidiameter of the plate, be increaſed by the infinitely ſmall quantity $A\dot{C}$; the quantity of matter in the plate will be increaſed by a quantity, which is to the whole, as $2\,A\dot{C}$ to A C; and the repulſion of the plate on the column, will be increaſed by $\overline{3-n}\times A\dot{C}\times AC^{2-n}-A\dot{C}\times\frac{AC}{DA}\times\overline{3-n}\times DA^{2-n}, =\overline{3-n}\times A\dot{C}\times AC\times\overline{\frac{1}{AC^{n-1}}-\frac{1}{DA^{n-1}}}$: therefore if a quantity of matter, which is to the whole quantity in the plate, as $2\,A\dot{C}$ to A C be collected in the circumference, its repulſion on the column DC, will be to that of the whole plate, as $\overline{3-n}\times A\dot{C}\times AC\times\overline{\frac{1}{AC^{n-1}}-\frac{1}{DA^{n-1}}}$, to $DC^{3-n}+AC^{3-n}-DA^{3-n}$; and conſequently the repulſion of the plate when all the matter is collected in its circumference, is to its repulsion when the matter is spread uniformly, as $\frac{\overline{3-n} \times AC^2}{2} \times \overline{\frac{1}{AC^{n-1}} - \frac{1}{DA^{n-1}}}$, to $DC^{3-n} + AC^{3-n} - DA^{3-n}$.

Coroll. I.

If the length of the column is so great, that AC^{n-1} is very small in respect of DC^{n-1}, the repulsion of the plate, when all the matter is collected in the circumference, is to its repulsion when the matter is spread uniformly, very nearly as $\frac{\overline{3-n} \times AC^{3-n}}{2}$ to AC^{3-n}, or as $3-n$ to 2.

Coroll. II.

If EC^{n-1} is very small in respect of AC^{n-1}, the repulsion of the plate on the short column EC, when all the matter in the plate is collected in its circumference, is to its repulsion when the matter is spread uniformly, very nearly as $\frac{\overline{3-n} \times \overline{n-1} \times EC^2}{4AC^{n-1}}$ to EC^{3-n}, or as $\overline{3-n} \times \overline{n-1} \times EC^{n-1}$ to $4\,AC^{n-1}$; and is therefore very small in comparison of what it is when the matter is spread uniformly.

For by the same kind of process as was used in Lemma 8, it appears, that if EC^2 is very small in respect of AC^2, $AC^2 \times \overline{\frac{1}{AC^{n-1}} - \frac{1}{EA^{n-1}}}$ differs very

little

little from $\frac{\overline{n-1} \times EC^2}{2EA^{n-1}}$, or from $\frac{\overline{n-1} \times EC^2}{2AC^{n-1}}$; and if EC^{n-1} is very ſmall in reſpect of AC^{n-1}, EC^2 is *a fortiori* very ſmall in reſpect of AC^2.

COROLL. III.

Suppoſe now that the matter of the plate is denſer near the circumference than near the middle, and that the denſity at and near the middle is to the mean denſity, or the denſity which it would everywhere be of if the matter was ſpread uniformly, as δ to one; the repulſion of the plate on EC will be leſs than if the matter was ſpread uniformly, in a ratio approaching much nearer to that of δ to one, than to that of equality.

COROLL. IV.

Let every thing be as in the laſt corollary, and let π be taken to one, as the force with which the plate actually repels the column DC (DC^{n-1} being very great in reſpect of AC^{n-1}) is to the force with which it would repel it, if the matter was ſpread uniformly; the repulſion of the plate on EC will be to its repulſion on DC, in a ratio between that of $EC^{3-n} \times \delta$ to $AC^{3-n} \times \pi$, and that of EC^{3-n} to $AC^{3-n} \times \pi$, but will approach much nearer to the former ratio than to the latter.

LEMMA

LEMMA XI.

In the line DC produced, take CF equal to CA: if all the matter of the plate AB is collected in the circumference, its repulſion on the column CD, infinitely continued, is equal to the repulſion of the ſame quantity of matter collected in the point F, on the ſame column.

For the repulſion of the plate on the column in the direction CD, is the ſame, whether the matter of it be collected in the whole circumference, or in the point A. Suppoſe it therefore to be collected in A; and let an equal quantity of matter be collected in F; take FG conſtantly equal to AD; and let AD and FG flow: the fluxion of CD is to the fluxion of FG, as AD to CD; and the repulſion of A on the point D, in the direction CD, is to the repulſion of F on G, as CD to AD; and therefore the fluxion of the repulſion of A on the column CD, in the direction CD, is equal to the fluxion of the repulſion of F on CG; and when AD equals AC, the repulſion of both A and F on their reſpective columns vaniſhes; and therefore the repulſion of A on the whole column CD equals that of F on CG; and when CD and CG are both infinitely extended, they may be looked upon as the ſame column.

PROP. XVII.

Let two ſimilar bodies, of different ſizes, and conſiſting of different ſorts of matter, be both overcharged,

Fig. 11

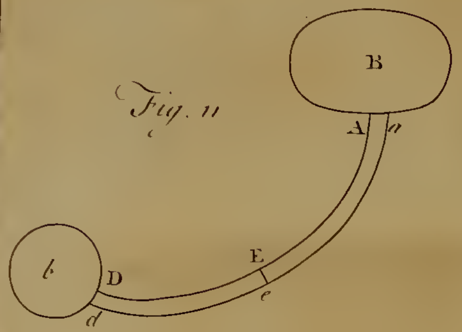


Fig. 12.

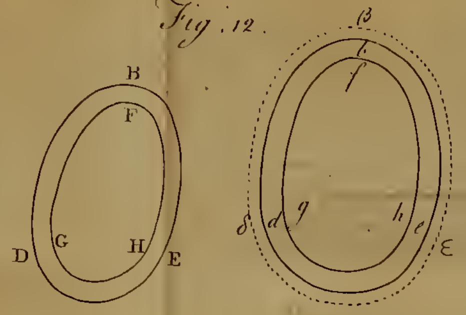


Fig. 13.

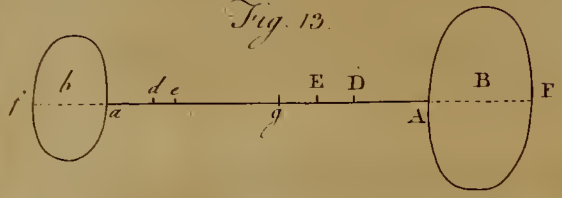


Fig. 14.

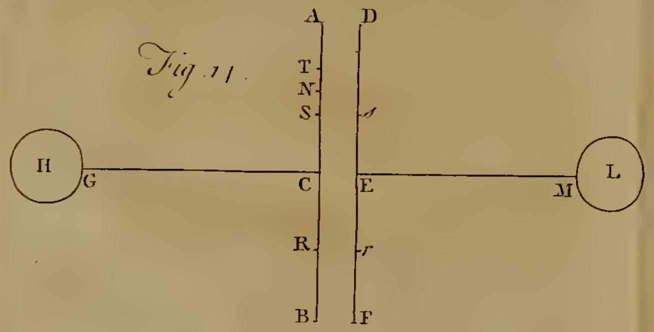


Fig. 15

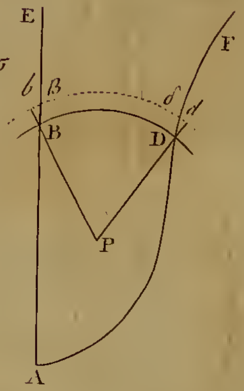


Fig. 16.

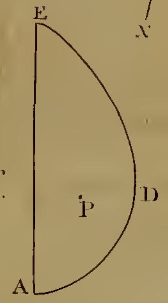


Fig. 17.

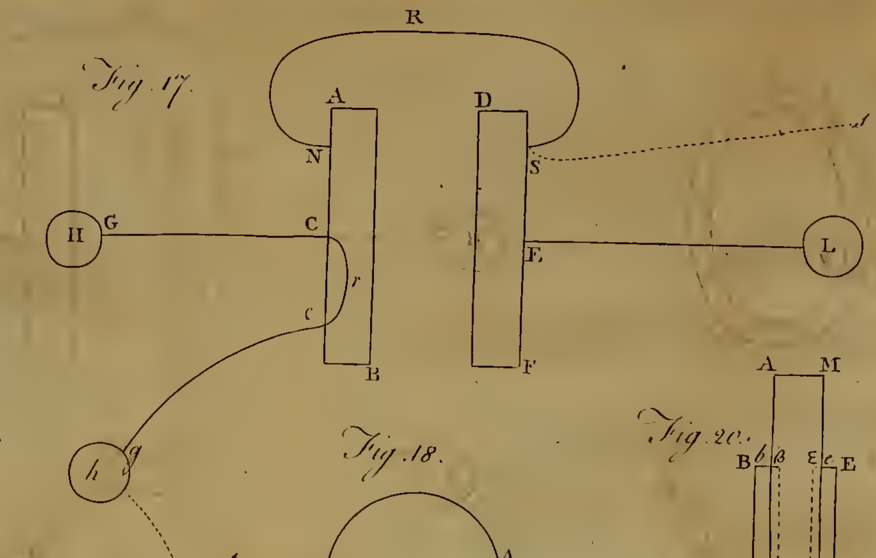


Fig. 18.

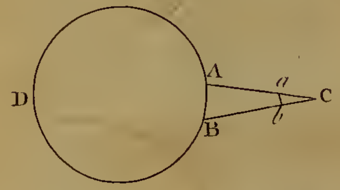


Fig. 19.

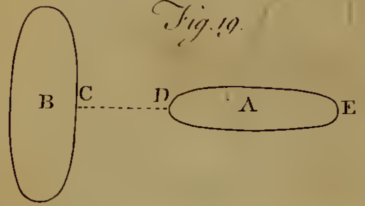
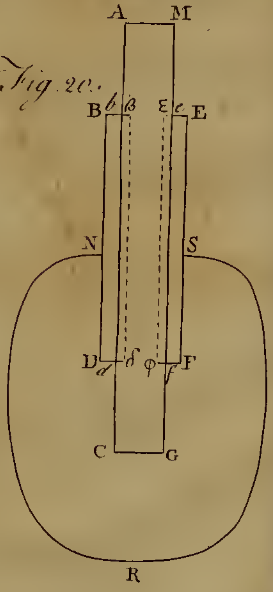


Fig. 20.



charged, or both undercharged, but in different degrees; and let the redundance or deficience of fluid in each be very ſmall in reſpect of the whole quantity of fluid in them: it is impoſſible for the fluid to be diſpoſed accurately in a ſimilar manner in both of them *; as it has been ſhewn that there will be a ſpace, cloſe to the ſurface, which will either be as full of fluid as it can hold, or will be intirely deprived of fluid; but it will be diſpoſed as nearly in a ſimilar manner in both, as is poſſible. To explain this, let B D E and *b d e* (fig. 12) be the two ſimilar bodies; and let the ſpace comprehended between the ſurfaces B D E and F G H (or the ſpace B F as I ſhall call it for ſhortneſs) be that part of B D E, which is either as full of fluid as it can hold, or intirely deprived of it: draw the ſurface *f g h*, ſuch that the ſpace *b f*, ſhall be to the ſpace B F, as the quantity of redundant or deficient fluid in *b d e*, to that in B D E, and that the thickneſs of the ſpace *b f* ſhall every where bear the ſame proportion to the correſponding thickneſs of B F: then will the ſpace *b f* be either as full of fluid as it can hold, or

* By the fluid being diſpoſed in a ſimilar manner in both bodies, I mean that the quantity of redundant or deficient fluid in any ſmall part of one body, is to that in the correſponding ſmall part of the other, as the whole quantity of redundant or deficient fluid in one body, to that in the other. By the quantity of deficient fluid in a body, I mean the quantity of fluid wanting to ſaturate it. Notwithſtanding the impropriety of this expreſſion, I muſt beg leave to make uſe of it, as it will frequently ſave a great deal of circumlocution.

intirely

intirely deprived of it; and the fluid within the ſpace fgh will be diſpoſed very nearly ſimilarly to that in the ſpace F G H.

For it is plain, that if the fluid could be diſpoſed accurately in a ſimilar manner in both bodies, the fluid would be in equilibrio in one body, if it was in the other: therefore draw the ſurface $\beta\delta\epsilon$, ſuch that the thickneſs of the ſpace βf ſhall be every where to the correſponding thickneſs of B F, as the diameter of bde to the diameter of BDE; and let the redundant fluid or matter in bf be ſpread uniformly over the ſpace βf; then if the fluid in the ſpace fgh is diſpoſed exactly ſimilarly to that in F G H, it will be in equilibrio; as the fluid will then be diſpoſed exactly ſimilarly in the ſpaces $\beta\delta\epsilon$ and B D E: but as by the ſuppoſition, the thickneſs of the ſpace βf is very ſmall in reſpect of the diameter of bde, the fluid or matter in the ſpace bf will exert very nearly the ſame force on the reſt of the fluid, whether it is ſpread over the ſpace βf, or whether it is collected in bf.

P R O P. XVIII.

Let two bodies, B and b, be connected to each other by a canal of any kind, and be either over or undercharged: it is plain that the quantity of redundant or deficient fluid in B, would bear exactly the ſame proportion to that in b, whatever ſort of matter B conſiſted of, if it was poſſible for the redundant or deficient fluid in any body, to be difpofed accurately in the fame manner, whatever fort of matter it confifted of. For fuppofe B to confift of any fort of matter; and let the fluid in the canal and two bodies be in equilibrio: let now B be made to confift of fome other fort of matter, which requires a different quantity of fluid to faturate it; but let the quantity and difpofition of the redundant or deficient fluid in it remain the fame as before: it is plain that the fluid will ftill be in equilibrio; as the attraction or repulfion of any body depends only on the quantity and difpofition of the redundant and deficient fluid in it. Therefore, by the preceeding propofition, the quantity of redundant or deficient fluid in B, will actually bear very nearly the fame proportion to that in *b*, whatever fort of matter B confifts of; provided the quantity of redundant or deficient fluid in it is very fmall in refpect of the whole.

PROP. XIX.

Let two bodies B and *b* (fig. 11.) be connected together by a very flender canal A D *d a*, either ftraight or crooked: let the canal be everywhere of the fame breadth and thicknefs; fo that all fections of this canal made by planes perpendicular to the direction of the canal in that part, fhall be equal and fimilar: let the canal be compofed of uniform matter; and let the electric fluid therein be fuppofed incompreffible, and of fuch denfity as exactly to faturate the matter

 therein;

therein; and let it, neverthelefs, be able to move readily along the canal; and let each particle of fluid in the canal be attracted and repelled by the matter and fluid in the canal and in the bodies B and *b*, juft in the fame manner that it would be if it was not incomprefsible *; and let the bodies B and *b* be either over or undercharged. I fay that the force with which the whole quantity of fluid in the canal is impelled from A towards D, in the direction of the axis of the canal, by the united attractions and repulfions of the two bodies, muft be nothing; as otherwife the fluid in the canal could not be at reft: obferving that by the force with which the whole quantity of fluid is impelled in the direction of the axis of the canal, I mean the fum of the forces, with which the fluid in each part of the canal is impelled in the direction of the axis of the canal in that place, from A towards D; and obferving alfo, that an impulfe in the contrary direction from D towards A muft be looked upon as negative.

For as the canal is exactly faturated with fluid, the fluid therein is attracted or repelled only by the redundant matter or fluid in the two bodies. Suppofe now that the fluid in any fection of the canal, as E*e*,

* This fuppofition of the fluid in the canal being incompreffible, is not mentioned as a thing which can ever take place in nature, but is merely imaginary; the reafon for making of which will be given hereafter.

is

is impelled with any given force in the direction of the canal at that place, the section D d would, in consequence thereof, be impelled with exactly the same force in the direction of the canal at D, if the fluid between E e and D d was not at all attracted or repelled by the two bodies; and, consequently, the section D d is impelled in the direction of the canal, with the sum of the forces, with which the fluid in each part of the canal is impelled, by the attraction or repulsion of the two bodies in the direction of the axis in that part; and consequently, unless this sum was nothing, the fluid in D d could not be at rest.

COROLLARY.

Therefore, the force with which the fluid in the canal is impelled one way in the direction of the axis, by the body B, must be equal to that with which it impelled by b in the contrary direction.

PROP. XX.

Let two similar bodies B and b (fig. 13.) be connected by the very slender cylindric or prismatic canal A a, filled with incompressible fluid, in the same manner as described in the preceding proposition: let the bodies be overcharged; but let the quantity of redundant fluid in each bear so small a proportion to the whole, that the fluid may be considered as disposed in a similar manner in both; let the bodies also be similarly situated in respect of the canal A a; and let them be placed at an infinite distance from each

 other,

other, or at ſo great an one, that the repulſion of either body on the fluid in the canal ſhall not be ſenſibly leſs than if they were at an infinite diſtance: then, if the electric attraction and repulſion is inverſely as the n power of the diſtance, n being greater than one, and leſs than three, the quantity of redundant fluid in the two bodies will be to each other, as the $n-1$ power of their correſponding diameters AF and af.

For if the quantity of redundant fluid in the two bodies is in this proportion, the repulſion of one body on the fluid in the canal, will be equal to that of the other body on it in the contrary direction; and, conſequently, the fluid will have no tendency to flow from one body to the other, as may thus be proved. Take the points D and E very near to each other; and take da to DA, and ea to EA, as af to AF; the repulſion of the body B on a particle at D, will be to the repulſion of b on a particle at d, as $\frac{1}{AF}$ to $\frac{1}{af}$; for, as the fluid is diſpoſed ſimilarly in both bodies, the quantity of fluid in any ſmall part of B, is to the quantity in the correſponding part of b, as AF^{n-1} to af^{n-1}; and, conſequently, the repulſion of that ſmall part of B, on D, is to the repulſion of the correſponding part of b, on d, as $\frac{AF^{n-1}}{AF^n}$, or $\frac{1}{AF}$, to $\frac{1}{af}$. But the quantity of fluid in the ſmall part DE of the canal, is to that in de, as DE to de, or as AF to af; therefore the repulſion of of B on the fluid in DE, is equal to that of b on the fluid in de: therefore, taking ag to Aa, as af to AF, the repulſion of b on the fluid in ag, is equal to that of B on the fluid in Aa; but the repulſion of b on ag may be conſidered as the ſame as its repulſion on Aa; for, by the ſuppoſition, the repulſion of B on Aa may be conſidered as the ſame as if it was continued infinitely; and therefore, the repulſion of b on ag may be conſidered as the ſame as if it was continued infinitely.

N. B. If n was not greater than one, it would be impoſſible for the length of Aa to be ſo great, that the repulſion of B on it might be conſidered as the ſame as if it was continued infinitely; which was my reaſon for requiring n to be greater than one.

Corollary.

By juſt the ſame method of reaſoning it appears, that if the bodies are undercharged, the quantity of deficient fluid in b will be to that in B, as af^{n-1} to AF^{n-1}.

PROP. XXI.

Let a thin flat plate be connected to any other body, as in the preceding propoſition, by a canal of incompreſſible fluid, perpendicular to the plane of the plate; and let that body be overcharged, the quantity of redundant fluid in the plate will bear very nearly the ſame

3 proportion

proportion to that in the other body, whatever the thickneſs of the plate may be, provided its thickneſs is very ſmall in proportion to its breadth, or ſmalleſt diameter.

For there can be no doubt, but what, under that reſtriction, the fluid will be diſpoſed very nearly in the ſame manner in the plate, whatever its thickneſs may be; and therefore its repulſion on the fluid in the canal will be very nearly the ſame, whatever its thickneſs may be.

PROP. XXII.

Let AB and DF (fig. 14.) repreſent two equal and parallel circular plates, whoſe centers are C and E; let the plates be placed ſo, that a right line joining their centers ſhall be perpendicular to the plates; let the thickneſs of the plates be very ſmall, in reſpect of their diſtance CE; let the plate AB communicate with the body H, and the plate DF with the body L, by the canals CG and EM of incompreſſible fluid, ſuch as are deſcribed in Prop. XIX; let theſe canals meet their reſpective plates in their centers C and E, and be perpendicular to the plane of the plates; and let their length be ſo great, that the repulſion of the plates on the fluid in them may be conſidered as the ſame, as if they were continued infinitely; let the body H be overcharged, and let L be ſaturated. It is plain, from Prop. XII. that DF will be undercharged, and AB will be more over-

charged

charged than it would otherwise be. Suppose, now, that the redundant fluid in A B is disposed in the same manner as the deficient fluid is in D F; let P be to one as the force with which the plate A B would repel the fluid in C E, if the canal M E was continued to C, is to the force with which it would repel the fluid in C M; and let the force with which A B repels the fluid in C G, be to the force with which it would repel it, if the redundant fluid in it was spread uniformly, as π to 1; and let the force with which the body H repels the fluid in C G, be the same with which a quantity of redundant fluid, which we will call B, spread uniformly over A B, would repel it in the contrary direction. Then will the redundant fluid in A B be equal to $\frac{B}{2P\pi - P^2\pi}$, and therefore, if P is very small, will be very nearly equal to $\frac{B}{2P\pi}$; and the deficient fluid in D F will be to the redundant fluid in A B, as $1 - P$ to one, and therefore, if P is very small, will be very nearly equal to the redundant fluid in A B.

For it is plain, that the force with which A B repels the fluid in E M, must be equal to that with which D F attracts it; for otherwise, some fluid would run out of D F into L, or out of L into D F: for the same reason, the excess of the repulsion of A B on the fluid in C G, above the attraction of F D thereon, must be equal to the force with which a quantity

quantity of redundant fluid equal to B, ſpread uniformly over AB, would repel it, or it muſt be equal to that with which a quantity equal to $\frac{B}{\pi}$, ſpread in the manner in which the redundant fluid is actually ſpread in AB, would repel it. By the ſuppoſition, the force with which AB repels the fluid in EM, is to the force with which it would repel the fluid in CM, ſuppoſing EM to be continued to C, as $1 - P$ to one; but the force with which any quantity of fluid in AB would repel the fluid in CM, is the ſame with which an equal quantity ſimilarly diſpoſed in DF, would repel the fluid in EM; therefore, the force with which the redundant fluid in AB repels the fluid in EM, is to that with which an equal quantity ſimilarly diſpoſed in DF, would repel it, as $1 - P$ to one: therefore, if the redundant fluid in AB be called A, the deficient fluid in DF muſt be $A \times 1 - P$: for the ſame reaſon, the force with which DF attracts the fluid in CG, is to that with which AB repels it, as $A \times 1 - P \times 1 - P$, or $A \times \overline{1 - P}^2$, to A; therefore, the exceſs of the force with which AB repels CG above that with which DF attracts it, is equal to that with which a quantity of redundant fluid equal to $\overline{A - A \times \overline{1 - P}^2}$, or $\overline{A \times 2P - P^2}$, ſpread over AB, in the manner in which the redundant fluid therein is actually ſpread, would repel it: therefore, $\overline{A \times 2P - P^2}$ muſt be equal to $\frac{B}{\pi}$, or A muſt be equal to $\frac{B}{2P\pi - P^2\pi}$.

Corol.

Corol. I.

If the denſity of the redundant fluid near the middle of the plate AB, is leſs than the mean denſity, or the denſity which it would everywhere be of, if it was ſpread uniformly, in the ratio of δ to one; and if the diſtance of the two plates is ſo ſmall, that EC^{n-1} is very ſmall in reſpect of AC^{n-1}, and that EC^{3-n} is very ſmall in reſpect of AC^{3-n}; the quantity of redundant fluid in AB will be greater than $\frac{B}{2} \times \overline{\frac{AC}{EC}}\Big|^{3-n}$, and leſs than $\frac{B}{2\delta} \times \overline{\frac{AC}{EC}}\Big|^{3-n}$, but will approach much nearer to the latter value than the former. For, in this caſe, $P\pi$ is, by Lemma X. Corol. IV. leſs than $\overline{\frac{EC}{AC}}\Big|^{3-n}$, and greater than $\overline{\frac{EC}{AC}}\Big|^{3-n} \times \delta$, but approaches much nearer to the latter value than the former; and if EC^{3-n} is very ſmall in reſpect of AC^{3-n}, P is very ſmall.

Remarks.

If DF was not undercharged, it is certain that AB would be conſiderably more overcharged near the circumference of the circle than near the center; for if the fluid was ſpread uniformly, a particle placed anywhere at a diſtance from the center, as at N, would be repelled with conſiderably more force towards the circumference than it would towards the center. If the plates are very near together, and, consequently, D F nearly as much undercharged as A B is overcharged, A B will ftill be more overcharged near the circumference than near the center, but the difference will not be near fo great as in the former cafe: for, let N R be many times greater than C E, and N S lefs than C E; and take E *r* and E *s* equal to C R and C S, there can be no doubt, I think, but that the deficient fluid in D F will be lodged nearly in the fame manner as the redundant fluid in AB; and therefore, the repulfion of the redundant fluid at R, on a particle at N, will be very nearly balanced by the attraction of the redundant matter at *r*, for R is not much nearer to N than *r* is; but the repulfion of S will not be near balanced by that of *s*; for the diftance of S from N is much lefs than that of *s*. Let now a fmall circle, whofe diameter is S T, be drawn round the center N, on the plane of the plate; as the denfity of the fluid is greater at T than at S, the repulfion of the redundant fluid within the fmall circle tends to impel the point N towards C; but as there is a much greater quantity of fluid between N and B, than between N and A, the repulfion of the fluid without the fmall circle tends to balance that; but the effect of the fluid within the fmall circle is not much lefs than it would be, if D F was not undercharged; whereas much the greater part of the effect of that part of the plate on the outfide of the circle, is taken off by the effect of the correfponding part of D F: confequently, the difference of denfity between T and S will not be near fo great, as if D F was not undercharged. Hence I fhould imagine, that if the two plates are very

very near together, the denſity of the redundant fluid near the center will not be much leſs than the mean denſity, or δ will not be much leſs than one; moreover, the leſs the diſtance of the plates, the nearer will δ approach to one.

Corol. II.

Let now the body H conſiſt of a circular plate, of the ſame ſize as A B, placed ſo, that the canal C G ſhall paſs through its center, and be perpendicular to its plane; by the ſuppoſition, the force with which H repels the fluid in the canal C G, is the ſame with which a quantity of fluid, equal to B, ſpread uniformly over A B, would repel it in the contrary direction: therefore, if the fluid in the plate H was ſpread uniformly, the quantity of redundant fluid therein would be B, and if it was all collected in the circumference, would be $\frac{2B}{3-n}$; and therefore the real quantity will be greater than B, and leſs than $\frac{2B}{3-n}$.

Corol. III.

Therefore, if we ſuppoſe δ to be equal to one, the quantity of redundant fluid in A B will exceed that in the plate H, in a greater ratio than that of $\overline{\frac{AC}{CE}}\big|^{3-n} \times \frac{3-n}{4}$ to one, and leſs than that of $\overline{\frac{AC}{CE}}\big|^{3-n} \times \frac{1}{2}$ to one; and from the preceding remarks it appears, that the real quantity of redundant fluid in A B can

 hardly

hardly be much greater than it would if δ was equal to one.

Corol. IV.

Hence, if the electric attraction and repulsion is inversely as the square of the distance, the redundant fluid in A B, supposing δ to be equal to one, will exceed that in the plate H, in a greater ratio than that of A C to 4 C E, and less than that of A C to 2 C E.

Corol. V.

Let now the body H consist of a globe, whose diameter equals A B; the globe being situated in such a manner, that the canal C G, if continued, would pass through its center; and let the electric attraction and repulsion be inversely as the square of the distance, the quantity of redundant fluid in the globe will be 2 B: for the fluid will be spread uniformly over the surface of the globe, and its repulsion on the canal will be the same as if it was all collected in the center of the sphere, and will therefore be the same with which an equal quantity, disposed in the circumference of A B, would repel it in the contrary direction, or with which half that quantity, or B, would repel it, if spread uniformly over the plate.

Corol. VI.

Therefore, if δ was equal to one, the redundant fluid in A B would exceed that in the globe, in the ratio of A C to 4 C E; and therefore, it will in reality exceed that in the globe, in a rather greater ratio than that of A C to 4 C E; but if the plates are very near together, it will approach very near thereto, and the nearer the plates are, the nearer it will approach thereto.

Corol. VII.

Whether the electric repulsion is inversely as the square of the distance or not, if the body H is as much undercharged, as it was before overcharged, A B will be as much undercharged as it was before overcharged, and D F as much overcharged as it was before undercharged.

Corol. VIII.

If the size and distance of the plates be altered, the quantity of redundant or deficient fluid in the body H remaining the same, it appears, by comparing this proposition with the 20th and 21st propositions, that the quantity of redundant and deficient fluid in A B will be as $AC^{n-1} \times \overline{\frac{AC}{EC}}|^{3-n}$, or as $\frac{AC^2}{EC^{3-n}}$, supposing the value of δ to remain the same.

PROP.

PROP. XXIII.

Let A E (fig. 15.) be a cylindric canal, infinitely continued beyond E; and let A F be a bent canal, meeting the other at A, and infinitely continued beyond F: let the ſection of this canal, in all parts of it, be equal to that of the cylindric canal, and let both canals be filled with uniform fluid of the ſame denſity: the force with which a particle of fluid P, placed anywhere at pleaſure, repels the whole quantity of fluid in A F, in the direction of the canal, is the ſame with which it repels the fluid in the canal A E, in the direction A E.

On the center P, draw two circular arches B D and $b\,d$, infinitely near to each other, cutting A E in B and β, and A F in D and δ, and draw the radii P b and P d. As P B = P D, the force with which P repels a particle at B, in the direction B β, is to that with which it repels an equal particle at D, in the direction D δ, as $\frac{Bb}{B\beta}$ to $\frac{Dd}{D\delta}$, or as $\frac{1}{B\beta}$ to $\frac{1}{D\delta}$; and therefore, the force with which it repels the whole fluid in B β, in the direction B β, is the ſame with which it repels the whole fluid in D δ, in the direction D δ, that is in the direction of the canal; and therefore, the force with which it repels the whole fluid in A E, in the direction A E, is the ſame with which it repels the whole fluid in A F, in the direction of the canal.

COROL-

COROLLARY.

If the bent canal ADF, inftead of being infinitely continued, meets the cylindric canal in E, as in fig. 16. the repulfion of P on the fluid in the bent canal A D E, in the direction of the canal, will ftill be equal to its repulfion on that in the cylindric canal A E, in the direction A E.

PROP. XXIV.

If two bodies, for inftance the plate A B, and the body H, of Prop. XXII. communicate with each other, by a canal filled with incompreffible fluid, and are either over or undercharged, the quantity of redundant fluid in them will bear the fame proportion to each other, whether the canal by which they communicate is ftraight or crooked, or into whatever part of the bodies the canal is inferted, or in whatever manner the two bodies are fituated in refpect of each other; provided that their diftance is infinite, or fo great that the repulfion of each body on the fluid in the canal fhall not be fenfibly lefs than if it was infinite.

Let the parellelograms AB and DF (fig. 17.) reprefent the two plates, and H and L the bodies communicating with them: let now H be removed to *h*; and let it communicate with A B, by the bent canal *gc*; the quantity of fluid in the plates and bodies

bodies remaining the ſame as before; and let us, for the ſake of eaſe in the demonſtration, ſuppoſe the canal *g c* to be every where of the ſame thickneſs as the canal G C; though the propoſition will evidently hold good equally, whether it is or not: the fluid will ſtill be in equilibrio. For let us firſt ſuppoſe the canal *g c* to be continued through the ſubſtance of the plate A B, to C, along the line *c r* C; the part *c r* C being of the ſame thickneſs as the reſt of the canal, and the fluid in it of the ſame denſity: by the preceding propoſition, the repulſion or attraction of each particle of fluid or matter in the plates A B and D F, on the fluid in the whole canal C *r c g*, in the direction of that canal, is equal to its repulſion or attraction on the fluid in the canal C G, in the direction C G; and therefore the whole repulſion or attraction of the two plates on the canal C *r c g*, is equal to their repulſion or attraction on C G: but as the fluid in the plate A B is in equilibrio, each particle of fluid in the part C *r c* of the canal, is impelled by the plates, with as much force in one direction as the other; and conſequently the plates impel the fluid in the canal *c g*, with as much force as they do that in the whole canal C *r c g*, that is, with the ſame force that they impel the fluid in C G. In like manner the body *h* impels the fluid in *c g*, with the ſame force that H does the fluid in C G; and conſequently *h* impels the fluid in *c g*, one way in the direction of the canal, with the ſame force that the two plates impel it the contrary way; and therefore the fluid in *c g* has no tendency to flow from one body to the other.

COROLLARY.

By the ſame method of reaſoning, with the help of the corollary to the 23d propoſition, it appears, that if A B and H each communicate with a third body, by canals of incompreſſible fluid, and a communication is made between A B and H by another canal of incompreſſible fluid, the fluid will have no tendency to flow from one to the other through this canal; ſuppoſing that the fluid was in equilibrio before this communication was made. In like manner if A B and H communicate with each other, or each communicate with a third body, by canals of real fluid, inſtead of the imaginary canals of incompreſſible fluid uſed in theſe propoſitions, and a communication is alſo made between them by a canal of incompreſſible fluid, the fluid can have no tendency to flow from one to the other. The truth of the latter part of this corollary will appear by ſuppoſing an imaginary canal of incompreſſible fluid to be continued through the whole length of the real one.

PROP. XXV.

Let now a communication be made between the two plates AB and DF, by the canal NRS of incompreſſible fluid, of any length; and let the body H and the plate AB be overcharged. It is plain that the fluid will flow through that canal from A B to D F. Now the whole force with which the fluid in the canal is impelled along it, by the joint action of the two plates, is the same with which the whole quantity of fluid in the canal CG or *cg* is impelled by them; supposing the canal N R S to be every where of the same breadth and thickness as CG or *cg*.

For suppose that the canal N R S, instead of communicating with the plate DF, is bent back just before it touches it, and continued infinitely along the line S*s*; the force with which the two plates impel the fluid in S*s*, is the same with which they impel that in EL, supposing S*s* to be of the same breadth and thickness as EL; and is therefore nothing; therefore the force with which they impel the fluid in NRS, is the same with which they impel that in N R S*s*; which is the same with which they impel that in C G.

PROP. XXVI.

Let now *x y z* be a body of an infinite size, containing just fluid enough to saturate it; and let a communication be made between *h* and *x y z*, by the canal *hy* of incompressible fluid, of the same breadth and thickness as *gc* or GC; the fluid will flow through it from *h* to *x y z*; and the force with which the fluid in that canal is impelled along it, is equal to that with which the fluid in N R S is impelled by the two plates.

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If the canal *hy* is of ſo great a length, that the repulſion of *h* thereon is the ſame as if it was continued infinitely, then the thing is evident: but if it is not, let the canal *hy*, inſtead of communicating with *xyz*, ſo that the fluid can flow out of the canal into *xyz*, be continued infinitely through its ſubſtance, along the line *yv*: now it muſt be obſerved that a ſmall part of the body *xyz*, namely, that which is turned towards *h*, will by the action of *h* upon it, be rendered undercharged; but all the reſt of the body will be ſaturated; for the fluid driven out of the undercharged part will not make the remainder, which is ſuppoſed to be of an infinite ſize, ſenſibly overcharged: now the force with which the fluid in the infinite canal *hyv*, is impelled by the body *h* and the undercharged part of *xyz*, is the ſame with which the fluid in *gc* is impelled by them; but as the fluid in all parts of *xyz* is in equilibrio, a particle in any part of *yv* cannot be impelled in any direction; and therefore the fluid in *hy* is impelled with as much force as that in *hyv*; and therefore the fluid in *hy* is impelled with as much force as that in *gc*; and is therefore impelled with as much force as the fluid in N R S is impelled by the two plates.

It perhaps may be aſked, whether this method of demonſtration would not equally tend to prove that the fluid in *hy* was impelled with the ſame force as that in N R S, though *xyz* did not contain juſt fluid enough to ſaturate it. I anſwer not; for this demonſtration depends on the canal *yv* being continued, within the body *xyz*, to an infinite diſtance beyond any over or undercharged part; which could not be if xyz contained either more or lefs fluid than that.

PROP. XXVII.

Let two bodies B and *b* (fig. 13.) be joined by a cylindric or prifmatic canal A *a*, filled with real fluid; and not by an imaginary canal of incompreffible fluid as in the 20th propofition; and let the fluid therein be in equilibrio: the force with which the whole or any given part of the fluid in the canal, is impelled in the direction of its axis, by the united repulfions and attractions of the redundant fluid or matter in the two bodies and the canal, muft be nothing; or the force with which it is impelled one way in the direction of the axis of the canal, muft be equal to that with which it is impelled the other way.

For as the canal is fuppofed cylindric or prifmatic, no particle of fluid therein can be prevented from moving in the direction of the axis of it, by the fides of the canal; and therefore the force with which each particle is impelled either way in the direction of the axis, by the united attractions and repulfions of the two bodies and the canal, muft be nothing, otherwife it could not be at reft; and therefore the force with which the whole, or any given part of the fluid in the canal, is impelled in the direction of the axis, muft be nothing.

Corol. I.

If the fluid in the canal is difpofed in fuch manner, that the repulfion or attraction of the redundant fluid or matter in it, on the whole or any given part of the fluid in the canal, has no tendency to impel it either way in the direction of the axis; then the force with which that whole or given part is impelled by the two bodies muft be nothing; or the force with which it is impelled one way in the direction of the axis, by the body B, muft be equal to that with which it is impelled in the contrary direction by the other body; but not if the fluid in the canal is difpofed in a different manner.

Corol. II.

If the bodies, and confequently the canal, is overcharged; then, in whatever manner the fluid in the canal is difpofed, the force with which the whole quantity of redundant fluid in the canal is repelled by the body B in the direction A *a*, muft be equal to that with which it is repelled by *b* in the contrary direction. For the force with which the redundant fluid is impelled in the direction A *a* by its own repulfion, is nothing; for the repulfion of the particles of any body on each other have no tendency to make the whole body move in any direction.

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Remarks.

When I firſt thought of the 20th and 22d propoſitions, I imagined that when two bodies were connected by a cylindric canal of real fluid, the repulſion of one body on the whole quantity of fluid in the canal, in one direction, would be equal to that of the other body on it in the contrary direction, in whatever manner the fluid was diſpoſed in the canal; and that therefore thoſe propoſitions would have held good very nearly, though the bodies were joined by cylindric canals of real fluid; provided the bodies were ſo little over or undercharged, that the quantity of redundant or deficient fluid in the canal ſhould be very ſmall in reſpect of the quantity required to ſaturate it; and conſequently that the fluid therein ſhould be very nearly of the ſame denſity in all parts. But from the foregoing propoſition it appears that I was miſtaken, and that the repulſion of one body on the fluid in the canal is not equal to that of the other body on it, unleſs the fluid in the canal is diſpoſed in a particular manner: beſides that, when two bodies are both joined by a real canal, the attraction or repulſion of the redundant matter or fluid in the canal, has ſome tendency to alter the diſpoſition of the fluid in the two bodies; and in the 22d propoſition, the canal C G exerts alſo ſome attraction or repulſion on the canal E M: on all which accounts the demonſtration of thoſe propoſitions is defective, when the bodies are joined by real canals. I have good reaſon however to think, that thoſe propoſitions actually hold good very nearly when the bodies are

are joined by real canals; and that, whether the canals are ſtraight or crooked, or in whatever direction the bodies are ſituated in reſpect of each other: though I am by no means able to prove that they do: I therefore choſe ſtill to retain thoſe propoſitions, but to demonſtrate them on this ideal ſuppoſition, in which they are certainly true, in hopes that ſome more ſkilful mathematician may be able to ſhew whether they really hold good or not.

What principally makes me think that this is the caſe, is that as far as I can judge from ſome experiments I have made, the quantity of fluid in different bodies agrees very well with thoſe propoſitions, on a ſuppoſition that the electric repulſion is inverſely as the ſquare of the diſtance. It ſhould alſo ſeem from thoſe experiments, that the quantity of redundant or deficient fluid in two bodies, bore very nearly the ſame proportion to each other, whatever is the ſhape of the canal by which they are joined, or in whatever direction they are ſituated in reſpect of each other.

Though the above propoſitions ſhould be found not to hold good, when the bodies are joined by real canals, ſtill it is evident, that in the 22d propoſition, if the plates AB and DF are very near together, the quantity of redundant fluid in the plate AB will be many times greater than that in the body H, ſuppoſing H to conſiſt of a circular plate of the ſame ſize as AB, and DF will be near as much undercharged as AB is overcharged.

Sir Iſaac Newton ſuppoſes that air conſiſts of particles which repel each other with a force inverſely as the diſtance: but it appears plainly from the foregoing pages, that if the repulſion of the particles was

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in this ratio; and extended indefinitely to all diftances, they would compofe a fluid extremely different from common air. If the repulfion of the particles was inverfely as the diftance, but extended only to a given very fmall diftance from their centers, they would compofe a fluid of the fame kind as air, in refpect of elafticity, except that its denfity would not be in proportion to its compreffion: if the diftance to which the repulfion extends, though very fmall, is yet many times greater than the diftance of the particles from each other, it might be fhewn, that the denfity of the fluid would be nearly as the fquare root of the compreffion. If the repulfion of the particles extended indefinitely, and was inverfely as fome higher power of the diftance than the cube, the denfity of the fluid would be as fome power of the compreffion lefs than $\frac{3}{5}$. The only law of repulfion, I can think of, which will agree with experiment, is one which feems not very likely; namely, that the particles repel each other with a force inverfely as the diftance; but that, whether the denfity of the fluid is great or fmall, the repulfion extends only to the neareft particles: or, what comes to the fame thing, that the diftance to which the repulfion extends, is very fmall, and alfo is not fixed, but varies in proportion to the diftance of the particles.

PART

PART II.

Containing a comparifon of the foregoing theory with experiment.

§ 1. It appears from experiment, that fome bodies fuffer the electric fluid to pafs with great readinefs between their pores; while others will not fuffer it to do fo without great difficulty; and fome hardly fuffer it to do fo at all. The firft fort of bodies are called conductors, the others non-conductors. What this difference in bodies is owing to I do not pretend to explain.

It is evident that the electric fluid in non-conductors may be confidered as moveable, or anfwers to the definition given of that term in p. 588. As to the fluid contained in non-conducting fubftances, though it does not abfolutely anfwer to the definition of immoveable, as it is not abfolutely confined from moving, but only does fo with great difficulty; yet it may in moft cafes be looked upon as fuch without fenfible error.

Air does in fome meafure permit the electric fluid to pafs through it; though, if it is dry, it lets it pafs but very flowly, and not without difficulty; it is therefore to be called a non-conductor.

It appears that conductors would readily fuffer the fluid to run in and out of them, were it not for the air which furrounds them: for if the end of a conductor is inferted into a vacuum, the fluid runs in and out of it with perfect readinefs; but

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when it is ſurrounded on all ſides by the air, as no fluid can run out of it without running into the air, the fluid will not do ſo without difficulty.

If any body is ſurrounded on all ſides by the air, or other non-conducting ſubſtances, it is ſaid to be inſulated: if on the other hand it any where communicates with any conducting body, it is ſaid to be not inſulated. When I ſay that a body communicates with the ground, or any other body, I would be underſtood to mean that it does ſo by ſome conducting ſubſtance.

Though the terms poſitively and negatively electrified are much uſed, yet the preciſe ſenſe in which they are to be underſtood, ſeems not well aſcertained; namely, whether they are to be underſtood in the ſame ſenſe in which I have uſed the words over or undercharged, or whether, when any number of bodies, inſulated and communicating with each other by conducting ſubſtances, are electrified by means of excited glaſs, they are all to be called poſitively electrified (ſuppoſing, according to the uſual opinion, that excited glaſs contains more than its natural quantity of electricity); even though ſome of them, by the approach of a ſtronger electrified body, are made undercharged. I ſhall uſe the words in the latter ſenſe; but as it will be proper to aſcertain the ſenſe in which I ſhall uſe them more accurately, I ſhall give the following definition.

In order to judge whether any body, as A, is poſitively or negatively electrified: ſuppoſe another body B, of a given ſhape and ſize, to be placed at an infinite diſtance from it, and from any other over or undercharged body; and let B contain the ſame quantity of electric fluid, as if it communicated with A by a canal of incompreſſible fluid: then, if B is overcharged, I call A poſitively electrified; and if it is undercharged, I call A negatively electrified; and the greater the degree in which B is over or undercharged, the greater is the degree in which A is poſitively or negatively electrified.

It appears from the corollary to the 24th propoſition, that if ſeveral bodies are inſulated, and connected together by conducting ſubſtances, and one of theſe bodies is poſitively or negatively electrified, all the other bodies muſt be electrified in the ſame degree: for ſuppoſing a given body B to be placed at an infinite diſtance from any over or undercharged body, and to contain the ſame quantity of fluid as if it communicated with one of thoſe bodies by a canal of incompreſſible fluid, all the reſt of thoſe bodies muſt by that corollary contain the ſame quantity of fluid as if they communicated with B by canals of incompreſſible fluid: but yet it is poſſible that ſome of thoſe bodies may be overcharged, and others undercharged: for ſuppoſe the bodies to be poſitively electrified, and let an overcharged body D be brought near one of them, that body will become undercharged, provided D is ſufficiently overcharged; and yet by the definition it will ſtill be poſitively electrified in the ſame degree as before.

Moreover, if ſeveral bodies are inſulated and connected together by conducting ſubſtances, and one of theſe bodies is electrified by excited glaſs, there can be no doubt, I think, but what they will all be poſitively electrified; for if there is no other over or undercharged body placed near any of theſe bodies, the thing is evident; and though ſome of theſe bodies may, by the approach of a ſufficiently overcharged body, be rendered undercharged; yet I do not ſee how it is poſſible to prevent a body placed at an infinite diſtance, and communicating with them by a canal of incompreſſible fluid, from being overcharged.

In like manner if one of theſe bodies is electrified by excited ſealing wax, they will all be negatively electrified.

It is impoſſible for any body communicating with the ground to be either poſitively or negatively electrified: for the earth, taking the whole together, contains juſt fluid enough to ſaturate it, and conſiſts in general of conducting ſubſtances; and conſequently though it is poſſible for ſmall parts of the ſurface of the earth to be rendered over or undercharged, by the approach of electrified clouds or other cauſes; yet the bulk of the earth, and eſpecially the interior parts, muſt be ſaturated with electricity. Therefore aſſume any part of the earth which is itſelf ſaturated, and is at a great diſtance from any over or undercharged part; any body communicating with the ground, contains as much electricity as if it communicated with this part by a canal of incompreſſible fluid, and therefore is not at all electrified.

If any body A, inſulated and ſaturated with electricity, is placed at a great diſtance from any over or undercharged body, it is plain that it cannot be electrified; but if an overcharged body is brought

brought near it, it will be poſitively electrified; for ſuppoſing A to communicate with any body B, at an infinite diſtance, by a canal of incompreſſible fluid, it is plain that unleſs B is overcharged, the fluid in the canal could not be in equilibrio, but would run from A to B. For the ſame reaſon a body inſulated and ſaturated with fluid, will be negatively electrified if placed near an undercharged body.

§ 2. The phænomena of the attraction and repulſion of electrified bodies ſeem to agree exactly with the theory; as will appear by conſidering the following caſes.

Case I. Let two bodies, A and B, both conductors of electricity, and both placed at a great diſtance from any other electrified bodies, be brought near each other. Let A be inſulated, and contain juſt fluid enough to ſaturate it; and let B be poſitively electrified. They will attract each other; for as B is poſitively electrified, and at a great diſtance from any overcharged body, it will be overcharged; therefore, on approaching A and B to each other, ſome fluid will be driven from that part of A which is neareſt to B to the further part: but when the fluid in A was ſpread uniformly, the repulſion of B on the fluid in A was equal to its attraction on the matter therein; therefore, when ſome fluid is removed from thoſe parts where the repulſion of B is ſtrongeſt to thoſe where it is weaker, B will repel the fluid in A with leſs force than it attracts the matter; and conſequently the bodies will attract each other.

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Case II. If we now ſuppoſe that the fluid is at liberty to eſcape from out of A, if it has any diſpoſition to do ſo, the quantity of fluid in it before the approach of B being ſtill ſufficient to ſaturate it; that is, if A is not inſulated and not electrified, B being ſtill poſitively electrified, they will attract with more force than before: for in this caſe, not only ſome fluid will be driven from that part of A which is neareſt to B to the oppoſite part, but alſo ſome fluid will be driven out of A.

It muſt be obſerved, that if the repulſion of B on a particle at E, (fig. 19.) the fartheſt part of A, is very ſmall in reſpect of its repulſion on an equal particle placed at D, the neareſt part of A, the two bodies will attract with very nearly the ſame force, whether A is inſulated or not; but if the repulſion of B, on a particle at E, is very near as great as on one at D, they will attract with very little force if A is inſulated. For inſtance, let a ſmall overcharged ball be brought near one end of a long conductor not electrified; they will attract with very near the ſame force, whether the conductor be inſulated or not; but if the conductor be overcharged, and brought near a ſmall unelectrified ball, they will not attract with near ſo much force, if the ball is inſulated, as if it is not.

Case III. If we now ſuppoſe that A is negatively electrified, and not inſulated, it is plain that they will attract with more force than in the laſt

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caſe; as A will be ſtill more undercharged in this caſe, than in the laſt.

N. B. In theſe three caſes, we have not as yet taken notice of the effect which the body A will have in altering the quantity and diſpoſition of the fluid in B; but in reality this will make the bodies attract each other with more force than they would otherwiſe do; for in each of theſe caſes the body A attracts the fluid in B; which will cauſe ſome fluid to flow from the farther parts of B to the nearer, and will alſo cauſe ſome fluid to flow into it, if it is not inſulated, and will conſequently cauſe B to act upon A with more force than it would otherwiſe do.

CASE IV. Let us now ſuppoſe that B is negatively electrified; and let A be inſulated, and contain juſt fluid enough to ſaturate it; they will attract each other; for B will be undercharged; it will therefore attract the fluid in A, and will cauſe ſome fluid to flow from the fartheſt part of A, where it is attracted with leſs force, to the nearer part, where it is attracted with more force; ſo that B will attract the fluid in A with more force than it repels the matter.

CASE V, and VI. If A is now ſuppoſed to be not inſulated and not electrified, B being ſtill negatively electrified, it is plain that they will attract with more force than in the laſt caſe: and if A is poſitively electrified, they will attract with ſtill more force.

In theſe three laſt caſes alſo, the effect which A has in altering the quantity and diſpoſition of the fluid

fluid in B, tends to increaſe the force with which the two bodies attract.

Case VII. It is plain that a non-conducting body ſaturated with fluid, is not at all attracted or repelled by an over or undercharged body, until, by the action of the electrified body on it, it has either acquired ſome additional fluid from the air, or had ſome driven out of it, or till ſome fluid is driven from one part of the body to the other.

Case VIII. Let us now ſuppoſe that the two bodies A and B are both poſitively electrified in the ſame degree. It is plain, that were it not for the action of one body on the other, they would both be overcharged, and would repel each other. But it may perhaps be ſaid, that one of them as A may, by the action of the other on it, be either rendered undercharged on the whole, or at leaſt may be rendered undercharged in that part neareſt to B; and that the attraction of this undercharged part on a particle of the fluid in B, may be greater than the repulſion of the more diſtant overcharged part; ſo that on the whole the body A may attract a particle of fluid in B. If ſo, it muſt be affirmed that the body B repels the fluid in A; for otherwiſe, that part of A which is neareſt to B could not be rendered undercharged. Therefore, to obviate this objection, let the bodies be joined by the ſtraight canal DC of incompreſſible fluid (fig. 19.). The body B will repel the fluid in all parts of this canal; for as A is ſuppoſed to attract the fluid in B, B will not only be more overcharged than it would otherwiſe be, but it will alſo be more over-

overcharged in that part neareſt to A than in the oppoſite part. Moreover, as the near undercharged part of A is ſuppoſed to attract a particle of fluid in B with more force than the more diſtant overcharged part repels it; it muſt, *a fortiori*, attract a particle in the canal with more force than the other repels it; therefore the body A muſt attract the fluid in the canal; and conſequently ſome fluid muſt flow from B to A, which is impoſſible; for as A and B are both electrified in the ſame degree, they contain the ſame quantity of fluid as if they both communicated with a third body at an infinite diſtance, by canals of incompreſſible fluid; and therefore, by the corollary to Prop. 24, if a communication is made between them by a canal of incompreſſible fluid, the fluid would have no diſpoſition to flow from one to the other.

Case IX. But if one of the bodies as A is poſitively electrified in a leſs degree than B, then it is poſſible for the bodies to attract each other; for in this caſe the force with which B repels the fluid in A may be ſo great, as to make the body A either intirely undercharged, or at leaſt to make the neareſt part of it ſo much undercharged, that A ſhall on the whole attract a particle of fluid in B.

It may be worth remarking with regard to this caſe, that when two bodies, both electrified poſitively but unequally, attract each other, you may by removing them to a greater diſtance from each other, cauſe them to repel; for as the ſtronger electrified body repels the fluid in the weaker with leſs force when removed to a greater diſtance, it will not be

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able to drive ſo much fluid out of it, or from the nearer to the further part, as when placed at a leſs diſtance.

CASE X, and XI. By the ſame reaſoning it appears, that if the two bodies are both negatively electrified in the ſame degree, they muſt repel each other: but if they are both negatively electrified in different degrees, it is poſſible for them to attract each other.

All theſe caſes are exactly conformable to experiment.

CASE XII. Let two cork balls be ſuſpended by conducting threads from the ſame poſitively electrified body, in ſuch manner that if they did not repel, they would hang cloſe together: they will both be equally electrified, and will repel each other: let now an overcharged body, more ſtrongly electrified than them, be brought under them; they will become leſs overcharged, and will ſeparate leſs than before: on bringing the body ſtill nearer, they will become not at all overcharged, and will not ſeparate at all: and on bringing the body ſtill nearer, they will become undercharged, and will ſeparate again.

CASE XIII. Let all the air of a room be overcharged, and let two cork balls be ſuſpended cloſe to each other by conducting threads communicating with the wall. By Prop. 15, it is highly probable that the balls will be undercharged; and therefore they ſhould repel each other.

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These two last cases are experiments of Mr. Canton's, and are described in Philos. Trans. 1753, p. 350, where are other experiments of the same kind, all readily explicable by the foregoing theory.

I have now considered all the principal or fundamental cases of electric attractions and repulsions which I can think of; all of which appear to agree perfectly with the theory.

§ 3. On the cases in which bodies receive electricity from or part with it to the air.

Lemma I.

Let the body A (fig. 6.) either stand near some over or undercharged body, or at a distance from any. It seems highly probable, that if any part of its surface, as MN, is overcharged, the fluid will endeavour to run out through that part, provided the air adjacent thereto is not overcharged.

For let G be any point in that surface, and P a point within the body, extremely near to it; it is plain that a particle of fluid at P, must be repelled with as much force in one direction as another (otherwise it could not be at rest) unless all the fluid between P and G is pressed close together, in which case it may be repelled with more force towards G than it is in the contrary direction: now a particle at G is repelled in the direction PG, *i. e.* from P to G, by all the redundant fluid between P and G; and a particle at P is repelled by the same fluid in the contrary direction; so that as the particle at P is repelled with not less force in the direction PG than in the

 contrary,

contrary, I do not ſee how a particle at G can help being repelled with more force in that direction than the contrary, unleſs the air on the outſide of the ſurface M N was more overcharged than the ſpace between P and G.

In like manner, if any part of the ſurface is undercharged, the fluid will have a tendency to run in at that part from the air.

The truth of this is ſomewhat confirmed by the third problem; as in all the caſes of that problem, the fluid was ſhewn to have a tendency to run out of the ſpaces A D and E H, at any ſurface which was overcharged, and to run in at any which was undercharged.

Corol. I.

If any body at a diſtance from other over or undercharged bodies, be poſitively electrified, the fluid will gradually run out of it from all parts of its ſurface into the adjoining air; as it is plain that all parts of the ſurface of that body will be overcharged: and if the body is negatively electrified, the fluid will gradually run into it at all parts of its ſurface from the adjoining air.

Corol. II.

Let the body A (fig. 6.) inſulated and containing juſt fluid enough to ſaturate it, be brought near the overcharged body B; that part of the ſurface of A which is turned towards B will by Prop. 11. be rendered

dered undercharged, and will therefore imbibe electricity from the air; and at the oppofite furface R S, the fluid will run out of the body into the air.

Corol. III.

If we now fuppofe that A is not infulated, but communicates with the ground, and confequently that it contained juft fluid enough to faturate it before the approach of B, it is plain that the furface M N will be more undercharged than before; and therefore the fluid will run in there with more force than before; but it can hardly have any difpofition to run out at the oppofite furface R S; for if the canal by which A communicates with the ground is placed oppofite to B, as in figure 5, then the fluid will run out through that canal till it has no longer any tendency to run out at R S; and by the remarks at the end of Prop. 27, it feems probable, that the fluid in A will be nearly in the fame quantity, and difpofed nearly in the fame manner, into whatever part of A the canal is inferted by which it communicates with the ground.

Corol. IV.

If B is undercharged the cafe will be reverfed; that is, it will run out where it before run in, and will run in where it before run out.

As far as I can judge, thefe corollaries feem conformable to experiment: thus far is certain, that bodies at a diftance from other electrified bodies receive

ceive electricity from the air, if negatively electrified, and part with some to it if positively electrified: and a body not electrified and not insulated receives electricity from the air if brought near an overcharged body, and loses some when brought near an undercharged body: and a body insulated and containing its natural quantity of fluid, in some cases, receives, and in others loses electricity, when brought near an over or undercharged body.

§ 4. The well-known effects of points in causing a quick discharge of electricity seem to agree very well with this theory.

It appears from the 20th proposition, that if two similar bodies of different sizes are placed at a very great distance from each other, and connected by a slender canal, and overcharged, the force with which a particle of fluid placed close to corresponding parts of their surface is repelled from them, is inversely as the corresponding diameters of the bodies. If the distance of the two bodies is small, there is not so much difference in the force with which the particle is repelled by the two bodies; but still, if the diameters of the two bodies are very different, the particle will be repelled with much more force from the smaller body than from the larger. It is true indeed that a particle placed at a certain distance from the smaller body, will be repelled with less force than if it be placed at the same distance from the greater body; but this distance is, I believe, in most cases pretty considerable; if the bodies are spherical, and the repulsion inversely as the square of the distance, a particle placed at any distance from the surface of the ſmaller body leſs than a mean proportional between the radii of the two bodies, will be repelled from it with more force than if it be placed at the ſame diſtance from the larger body.

I think therefore that we may be well aſſured that if two ſimilar bodies are connected together by a ſlender canal, and are overcharged, the fluid muſt eſcape faſter from the ſmaller body than from an equal ſurface of the larger; but as the ſurface of the larger body is greateſt, I do not know which body ought to loſe moſt electricity in the ſame time; and indeed it ſeems impoſſible to determine poſitively from this theory which ſhould, as it depends in great meaſure on the manner in which the air oppoſes the entrance of the electric fluid into it. Perhaps in ſome degrees of electrification the ſmaller body may loſe moſt, and in others the larger.

Let now ACB (fig. 18.) be a conical point ſtanding on any body DAB, C being the vertex of the cone; and let DAB be overcharged: I imagine that a particle of fluid placed cloſe to the ſurface of the cone anywhere between *b* and C, muſt be repelled with at leaſt as much, if not more, force than it would, if the part A*ab*B of the cone was taken away, and the part *a*C*b* connected to DAB by a ſlender canal; and conſequently, from what has been ſaid before, it ſeems reaſonable to ſuppoſe that the waſte of electricity from the end of the cone muſt be very great in proportion to its ſurface; though it does not appear from this reaſoning whether the waſte of electricity from the whole cone ſhould be greater or leſs than from a cylinder of the ſame baſe and altitude.

All

All which has been here ſaid relating to the flowing out of electricity from overcharged bodies, holds equally true with regard to the flowing in of electricity into undercharged bodies.

But a circumſtance which I believe contributes as much as any thing to the quick diſcharge of electricity from points, is the ſwift current of air cauſed by them, and taken notice of by Mr. Wilſon and Dr. Prieſtly (*vide* Prieſtly, p. 117 and 591); and which is produced in this manner.

If a globular body A B D is overcharged, the air cloſe to it, all round its ſurface, is rendered overcharged, by the electric fluid, which flows into it from the body; it will therefore be repelled by the body; but as the air all round the body is repelled with the ſame force, it is in equilibrio, and has no tendency to fly off from it. If now the conical point A C B be made to ſtand out from the globe, as the fluid will eſcape much faſter in proportion to the ſurface from the end of the point than from the reſt of the body, the air cloſe to it will be much more overcharged than that cloſe to the reſt of the body; it will therefore be repelled with much more force; and conſequently a current of air will flow along the ſides of the cone, from B towards C; by which means there is a continual ſupply of freſh air, not much overcharged, brought in contact with the point; whereas otherwiſe the air adjoining to it would be ſo much overcharged, that the electricity would have but little diſpoſition to flow from the point into it.

The ſame current of air is produced in a leſs degree, without the help of the point, if the body, inſtead of being globular, is oblong or flat, or has knobs

knobs on it, or is otherwise formed in such manner as to make the electricity escape faster from some parts of it than the rest.

In like manner, if the body ABD be undercharged, the air adjoining to it will also be undercharged, and will therefore be repelled by it; but as the air close to the end of the point will be more undercharged than that close to the rest of the body, it will be repelled with much more force; which will cause exactly the same current of air, flowing the same way, as if the body was overcharged; and consequently the velocity with which the electric fluid flows into the body, will be very much increased. I believe indeed that it may be laid down as a constant rule, that the faster the electric fluid escapes from any body when overcharged, the faster will it run into that body when undercharged.

Points are not the only bodies which cause a quick discharge of electricity; in particular, it escapes very fast from the ends of long slender cylinders; and a swift current of air is caused to flow from the middle of the cylinder towards the end: this will easily appear by considering that the redundant fluid is collected in much greater quantity near the ends of the cylinders than near the middle. The same thing may be said, but I believe in a less degree, of the edges of thin plates.

What has been just said concerning the current of air, serves to explain the reason of the revolving motion of Dr. Hamilton's and Mr. Kinnersley's bent pointed wires, vide Phil. Trans. vol. LI, p. 905, and vol. LIII, p. 86; also Priestly, p. 429: for the same repulsion which impels the air from the thick part of the wire towards the point, tends to impel the wire in the contrary direction.

It is well known, that if a body B is positively electrified, and another body A, communicating with the ground, be then brought near it, the electric fluid will escape faster from B, at that part of it which is turned towards A, than before. This is plainly conformable to theory; for as A is thereby rendered undercharged, B will in its turn be made more overcharged, in that part of it which is turned towards A, than it was before. But it is also well known that the fluid will escape faster from B, if A be pointed, than if it be blunt; though B will be less overcharged in this case than in the other; for the broader the surface of A, which is turned towards B, the more effect will it have in increasing the overcharge of B. The cause of this phænomenon is as follows:

If A is pointed, and the pointed end turned towards B, the air close to the point will be very much undercharged, and therefore will be strongly repelled by A, and attracted by B, which will cause a swift current of air to flow from it towards B; by which means a constant supply of undercharged air will be brought in contact with B, which will accelerate the discharge of electricity from it in a very great degree: and moreover, the more pointed A is, the swifter will be this current. If, on the other hand, that end of A which is turned towards B, is so blunt, that the electricity is not disposed to run into A faster than it is to run out of B, the air adjoining to B may be as much overcharged as that adjoining to A is undercharged; and therefore may by the joint repulsion

of

of B and attraction of A, be impelled from B to A, with as much or more force than the air adjoining to A is impelled in the contrary direction; so that what little current of air there is may flow in the contrary direction.

It is easy applying what has been here said to the case in which B is negatively electrified.

§ 5. In the paper of Mr. Canton's, quoted in the second section, and in a paper of Dr. Franklin's (Phil. Trans. 1755, p. 300, and Franklin's letters p. 155.) are some remarkable experiments, shewing that when an overcharged body is brought near another body, some fluid is driven to the further end of this body, and also some driven out of it, if it is not insulated. The experiments are all strictly conformable to the 11th, 12th, and 13th propositions: but it is needless to point out the agreement, as the explanation given by the authors does it sufficiently.

§ 6. On the Leyden vial.

The shock produced by the Leyden vial seems owing only to the great quantity of redundant fluid collected on its positive side, and the great deficiency on its negative side; so that if a conductor was prepared of so great a size, as to be able to receive as much additional fluid by the same degree of electrification as the positive side of a Leyden vial, and was positively electrified in the same degree as the vial, I do not doubt but what as great a shock would be produced by making a communication between this conductor and the ground, as between the two surfaces of the

 Leyden

Leyden vial, ſuppoſing both communications to be made by canals of the ſame length and ſame kind.

It appears plainly from the experiments which have been made on this ſubject, that the electric fluid is not able to paſs through the glaſs; but yet it ſeems as if it was able to penetrate without much difficulty to a certain ſmall depth, perhaps I might ſay an imperceptible depth within the glaſs; as Dr. Franklin's analyſis of the Leyden vial ſhews that its electricity is contained chiefly in the glaſs itſelf, and that the coating is not greatly over or undercharged.

It is well known that glaſs is not the only ſubſtance which can be charged in the manner of the Leyden vial; but that the ſame effect may be produced by any other body, which will not ſuffer the electricity to paſs through it.

* Hence the phænomena of the vial ſeem eaſily explicable by means of the 22d propoſition. For let ACGM, fig. 20, repreſent a flat plate of glaſs or any other ſubſtance which will not ſuffer the electric fluid to paſs through it, ſeen edgeways; and let B*bd*D, and E*ef*F, or B*d* and E*f*, as I ſhall call them for ſhortneſs, be two plates of conducting matter of the ſame ſize, placed in contact with the glaſs oppoſite to each other; and let B*d* be poſitively electrified; and let E*f* communicate with the ground; and let the fluid be ſuppoſed either

* The following explication is ſtrictly applicable only to that ſort of Leyden vial, which conſiſts of a flat plate of glaſs or other matter. It is evident, however, that the reſult muſt be nearly of the ſame kind, though the glaſs is made into the ſhape of a bottle as uſual, or into any other form: but I propoſe to conſider thoſe ſort of Leyden vials more particularly in a future paper.

able

able to enter a little way into the glaſs, but not to paſs through it, or unable to enter it at all; and if it is able to enter a little way into it, let $b\beta\delta d$, or $b\delta$, as I ſhall call it, repreſent that part of the glaſs into which the fluid can enter from the plate B*d*, and $e\phi$, that which the fluid from E*f* can enter. By the abovementioned propoſition, if *be*, the thickneſs of the glaſs, is very ſmall in reſpect of *bd*, the diameter of the plates, the quantity of redundant fluid forced into the ſpace B*d*, or $B\delta$, (that is, into the plate B*d*, if the fluid is unable to penetrate at all into the glaſs, or into the plate B*d*, and the ſpace $b\delta$ together, if the fluid is able to penetrate into the glaſs) will be many times greater than what would be forced into it by the ſame degree of electrification if it had been placed by itſelf; and the quantity of fluid driven out of $E\phi$, will be nearly equal to the redundant fluid in $B\delta$.

If a communication be now made between $B\delta$ and $E\phi$, by the canal N R S, the redundant fluid will run from $B\delta$ to $E\phi$; and if in its way it paſſes through the body of any animal, it will by the rapidity of its motion produce in it that ſenſation called a ſhock.

It appears from the 26th propoſition, that if a body of any ſize was electrified in the ſame degree as the plate B*d*, and a communication was made between that body and the ground, by a canal of the ſame length, breadth and thickneſs as N R S; that then the fluid in that canal would be impelled with the ſame force as that in N R S, ſuppoſing the fluid in both canals to be incompreſſible; and conſequently, as the quantity of fluid to be moved,

and

and the reſiſtance to its motion is the ſame in both canals, the fluid ſhould move with the ſame rapidity in both: and I ſee no reaſon to think that the caſe will be different, if the communication is made by canals of real fluid.

Therefore what was ſaid in the beginning of this ſection, namely, that as great a ſhock would be produced by making a communication between the conductor and the ground, as between the two ſides of the Leyden vial, by canals of the ſame length and ſame kind, ſeems a neceſſary conſequence of this theory; as the quantity of fluid which paſſes through the canal is, by the ſuppoſition, the ſame in both; and there is the greateſt reaſon to think, that the rapidity with which it paſſes will be nearly if not quite the ſame in both. I hope ſoon to be able to ſay whether this agrees with experiment as well as theory.

It may be worth obſerving, that the longer the canal N R S is, by which the communication is made, the leſs will be the rapidity with which the fluid moves along it; for the longer the canal is, the greater is the reſiſtance to the motion of the fluid in it; whereas the force with which the whole quantity of fluid in it is impelled, is the ſame whatever be the length of the canal. Accordingly, it is found in melting ſmall wires, by directing a ſhock through them, that the longer the wire the greater charge it requires to melt it.

As the fluid in Bδ, is attracted with great force by the redundant matter in Eφ, it is plain that if the fluid is able to penetrate at all into the glaſs, great part of the redundant fluid will be lodged in $b\delta$,

$b\delta$, and in like manner there will be a great deficience of fluid in $e\varphi$. But in order to form ſome eſtimate of the proportion of the redundant fluid, which will be lodged in $b\delta$, let the communication between Ef and the ground be taken away, as well as that by which Bd is electrified; and let ſo much fluid be taken from $B\delta$, as to make the redundant fluid therein equal to the deficient fluid in $E\varphi$. If we ſuppoſe that all the redundant fluid is collected in $b\delta$, and all the deficient in $e\varphi$, ſo as to leave Bd and Ef ſaturated; then, if the electric repulſion is inverſely as the ſquare of the diſtance, a particle of fluid placed anywhere in the plane bd, except near the extremities b and d, will be attracted with very near as much force by the redundant matter in $e\varphi$, as it is repelled by the redundant fluid in $b\delta$; but if the repulſion is inverſely, as ſome higher power than the ſquare, it will be repelled with much more force by $b\delta$, than it is attracted by $e\varphi$, provided the depth $b\beta$ is very ſmall in reſpect of the thickneſs of the glaſs; and if the repulſion is inverſely, as ſome lower power than the ſquare, it will be attracted with much more force by $e\varphi$, than it is repelled by $b\delta$. Hence it follows, that if the depth to which the fluid can penetrate is very ſmall in reſpect of the thickneſs of the glaſs, but yet is ſuch that the quantity of fluid naturally contained in $b\delta$, or $e\varphi$, is conſiderably more than the redundant fluid in $B\delta$; then, if the repulſion is inverſely as the ſquare of the diſtance, almoſt all the redundant fluid will be collected in $b\delta$, leaving the plate Bd not very much overcharged; and in like manner Ef will be

not

not very much undercharged: if the repulſion is inverſely as ſome higher power than the ſquare, B d will be very much overcharged, and E f very much undercharged: and if the repulſion is inverſely, as ſome lower power than the ſquare, B d will be very much undercharged, and E f very much overcharged.

Suppoſe, now, the plate B d to be ſeparated from the plate of glaſs, ſtill keeping it parallel thereto, and oppoſite to the ſame part of it that it before was applied to; and let the repulſion of the particles be inverſely, as ſome higher power of the diſtance than the ſquare. When the plate is in contact with the glaſs, the repulſion of the redundant fluid in that plate, on a particle in the plane $b d$, *id eſt*, the inner ſurface of the plate, muſt be equal to the exceſs of the repulſion of the redundant fluid in $b \delta$ on it, above the attraction of E ϕ on it; therefore, when the plate B d is removed ever ſo ſmall a diſtance from the glaſs, the repulſion of the redundant fluid in the plate, on a particle in the inner ſurface of that plate, will be greater than the exceſs of the repulſion of $b \delta$ on it, above the attraction of E ϕ; for the repulſion of $b \delta$ will be much more diminiſhed by the removal, than the attraction of E ϕ: conſequently, ſome fluid will fly from the plate to the glaſs, in the form of ſparks: ſo that the plate will not be ſo much overcharged when removed from the glaſs, as it was when in contact with it. I ſhould imagine, however, that it would ſtill be conſiderably overcharged.

If one part of the plate is ſeparated from the glaſs before the reſt, as muſt neceſſarily be the caſe, if it conſiſts of bending materials, I ſhould gueſs it would be be at leaſt as much, if not more, overcharged, when ſeparated, as if it is ſeparated all at once.

In like manner, it ſhould ſeem that the plate E*f* will be conſiderably undercharged, when ſeparated from the glaſs, but not ſo much ſo as when in contact with it.

From the ſame kind of reaſoning I conclude, that if the repulſion is inverſely, as ſome lower power of the diſtance than the ſquare, the plate B*d* will be conſiderably undercharged, and E*f* conſiderably overcharged, when ſeparated from the glaſs, but not in ſo great a degree as when they are in contact with it.

§ 7. There is an experiment of Mr. Wilke and Æpinus, related by Dr. Prieſtly, p. 258. called by them, electrifying a plate of air: it conſiſted in placing two large boards of wood, covered with tin plates, parallel to each other, and at ſome inches aſunder. If a communication was made between one of theſe and the ground, and the other was poſitively electrified, the former was undercharged; the boards ſtrongly attracted each other; and, on making a communication between them, a ſhock was felt like that of the Leyden vial.

I am uncertain whether in this experiment the air contained between the two boards is very much overcharged on one ſide, and very much undercharged on the other, as is the caſe with the plate of glaſs in the Leyden vial; or whether the caſe is, that the redundant or deficient fluid is lodged only in the two boards, and that the air between them ſerves only to prevent the electricity from running from one board to the other: but whichever of thefe is the cafe, the experiment is equally conformable to the theory.

It muft be obferved, that a particle of fluid placed between the two plates is drawn towards the undercharged plate, with a force exceeding that with which it would be repelled from the overcharged plate, if it was electrified with the fame force, the other plate being taken away, nearly in the ratio of twice the quantity of redundant fluid actually contained in the plate, to that which it would contain, if electrified with the fame force by itfelf; fo that, unlefs the plate is very weakly electrified, or their diftance is very confiderable, the fluid will be apt to fly from one to the other, in the form of fparks.

§ 8. Whenever any conducting body as A, communicating with the ground, is brought fufficiently near an overcharged body B, the electric fluid is apt to fly through the air from B to A, in the form of a fpark: the way by which this is brought about feems to be this. The fluid placed anywhere between the two bodies, is repelled from B towards A, and will confequently move flowly through the air from one to the other: now it feems as if this motion increafed the elafticity of the air, and made it rarer: this will enable the fluid to flow in a fwifter current, which will ftill further increafe the elafticity of the air, till at laft it is fo much rarified, as to form very little oppofition to the motion of the electric fluid, upon which it flies in an uninterrupted mafs from one body to the other.

In

In the ſame manner may the electric fluid paſs from one body to another, in the form of a ſpark, if the firſt body communicates with the ground, and the other body is negatively electrified, or in any other caſe in which one body is ſtrongly diſpoſed to part with its electricity to the air, and the other is ſtrongly diſpoſed to receive it.

In like manner, when the electric fluid is made to paſs through water, in the form of a ſpark, as in Signor Beccaria's * and Mr. Lane's † experiments, I imagine that the water, by the rapid motion of the electric fluid through it, is turned into an elaſtic fluid, and ſo much rarified as to make very little oppoſition to its motion: and when ſtones are burſt or thrown out from buildings ſtruck by lightning, in all probability that effect is cauſed by the moiſture in the ſtone, or ſome of the ſtone itſelf, being turned into an elaſtic fluid.

It appears plainly, from the ſudden riſing of the water in Mr. Kinnerſley's electrical air thermometer ‡, that when the electric fluid paſſes through the air, in the form of a ſpark, the air in its paſſage is either very much rarified, or intirely diſplaced: and the burſting of the glaſs veſſels, in Beccaria's and Lane's experiments, ſhews that the ſame thing happens with regard to the water, when the electric fluid paſſes through it in the form of a ſpark. Now, I ſee no means by which the diſplacing of the air or

* Elettriciſmo artificiale e naturale, p. 110. Prieſtly, p. 209.

† Phil. Tranſ. 1767, p. 451.

‡ Phil. Tranſ. 1763, p. 84. Prieſtly, p. 216.

water can be brought about, but by ſuppoſing its elaſticity to be increaſed, by the motion of the electric fluid through it, unleſs you ſuppoſe it to be actually puſhed aſide, by the force with which the electric fluid endeavours to iſſue from the overcharged body: but I can by no means think, that the force with which the fluid endeavours to iſſue, in the ordinary caſes in which electric ſparks are produced, is ſufficient to overcome the preſſure of the atmoſphere, much leſs that it is ſufficient to burſt the glaſs veſſels in Beccaria's and Lane's experiments.

The truth of this is confirmed by Prop. XVI. For, let an undercharged body be brought near to, and oppoſite to the end of a long cylindrical body communicating with the ground, by that propoſition the preſſure of the electric fluid againſt the baſe of the cylinder is ſcarcely greater than the force with which the two bodies attract each other, provided that no part of the cylinder is undercharged; which is very unlikely to be the caſe, if the electric repulſion is inverſely as the ſquare of the diſtance, as I have great reaſon to believe it is; and, conſequently, if the ſpark was produced, by the air being puſhed aſide by the force with which the fluid endeavours to iſſue from the cylinder, no ſparks ſhould be produced, unleſs the electricity was ſo ſtrong, that the force with which the bodies attracted each other was as great as the preſſure of the atmoſphere againſt the baſe of the cylinder: whereas it is well known, that a ſpark may be produced, when the force, with which the bodies attract, is very trifling in reſpect of that.

One

One may frequently obſerve, in diſcharging a Leyden vial, that if the two knobs are approached together very ſlowly, a hiſſing noiſe will be perceived before the ſpark; which ſhews, that the fluid begins to flow from one knob to the other, before it paſſes in the form of a ſpark; and therefore ſerves to confirm the truth of the opinion, that the ſpark is brought about in the gradual manner here deſcribed.

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*** The Number of PLATES in this Volume is Twenty-two.

F I N I S.

E R R A T A.

Vol. LIX.

Pag. 69, line 23, *for* ſhorteſt *read* longeſt.

Vol. LX.

Pag. 194,	8,	effort	effect.
	11,	knots	knobs.
205,	13,	exploſion	expulſion.
211,	9,	mere	more.
216,	12,	ſort	ſoot.
218,	22,	fitted	filled.
219,	28,		*read*, I let the air into the receiver,
224,	17,	*read* from one conducting particle to another.	
227,	2,	*for* proof	*read* proceſs.
502,	middle column, l. 3 from bottom,	7.45.5	*r.* 7.43.55.

Vol. LXI.

Pag. 17,	14,	*for* avenues	*read* avernus.
78,	12,	Tab. III.	Tab. III. *
79,	laſt line,	Vabars i	Vabar is.
145,	11,	cheſunt	cheſnut.
168,	note 3,	at the North Eaſt	to the North Eaſt.
219,	25,	ſalt	ſalts.
274,	3,	*after* Eſq; *add* F. R. S.	
275,	20,	30th 72 inches	Barom. 30 72 inches.
280,	13,	in it	in the apartment.
282,	13,	*after* ſtorm *r.* although diminiſhed in form, weighed.	
290,	2,	euq	que.
295,	23,	ſixteen ths	ſix tenths.
359,	2,	*read*	Richard Hill Waring.
360,	26,	Phyl	Phyt.
362,	6,	tripla	triplò.
365,	16,	Vacinia	Vaccinia.
366,	26,	and the	and on the.
367,	5,	county	country.
368,	5,	Ter	Ger.
371,	22, 23,	Pericæ	Pezizæ.
377,	1,	removed	renewed.
379,	9,	Triddle	Friddle.
383,	5,	prepared to	prepared and at liberty to.
454	at bottom	XLV.	XLVII.