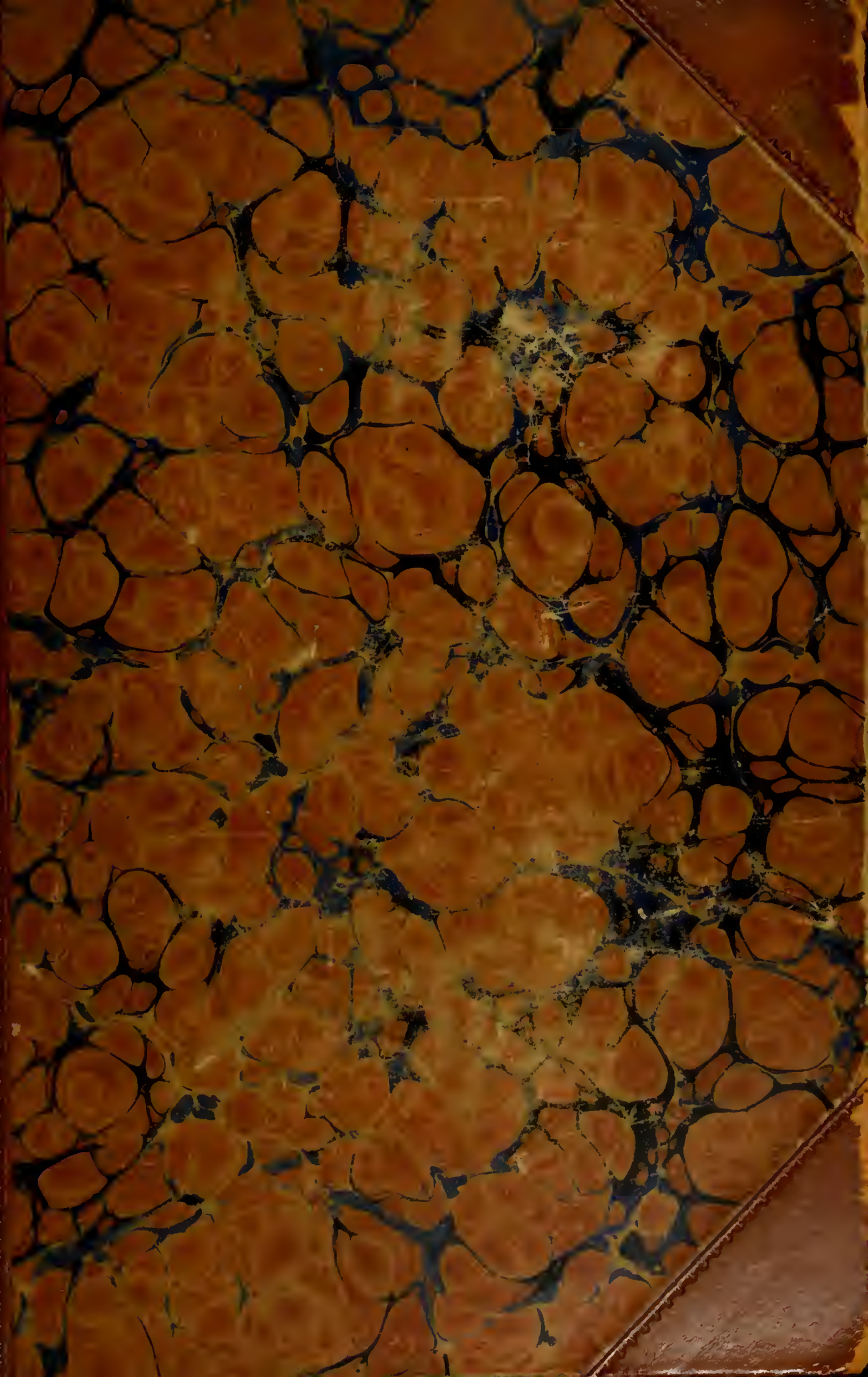

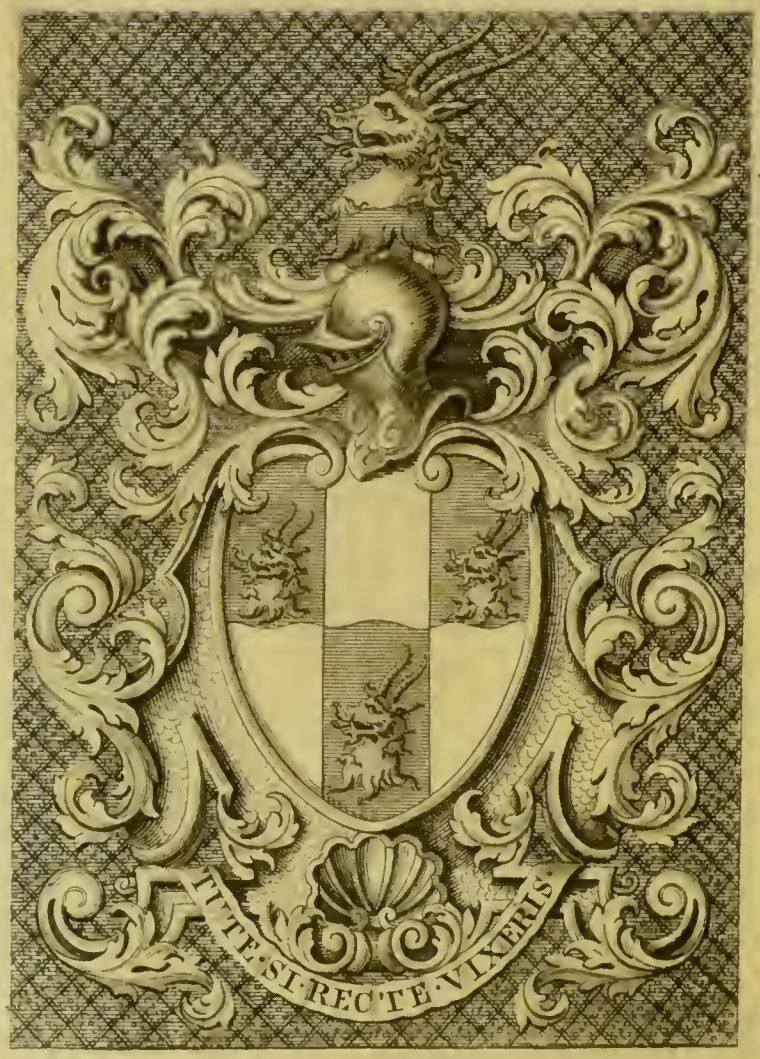
VTTE·SI·REC·TE·VIXERIS

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. L. PART I. For the Year 1757.

LONDON.

Printed for L. DAVIS and C. REYMERS,
Printers to the ROYAL SOCIETY,
against *Gray's-Inn Gate*, in *Holbourn.*

M.DCC.LVIII.

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ERRATA.

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Page 96. *line* 5. *read* expoſed the extremity of the umbilicus.
Page 168. *line* 4. *after* as *dele* well as.
Page 328. *line* 9 *from the bottom, for* ſtream *read* ſteam.

In the Tables of the Variation of the Magnetic Needle, Anno 1756,

Lat.	Long.			
0 —	70 E	*for Var.* $3\frac{1}{4}$ W	*read* $2\frac{3}{4}$ W.	*Read the ſame in p.* 333.
15 N	35 W	$3\frac{1}{2}$ W	$2\frac{1}{2}$ W	
5 S	40 E	17 W	18 W	
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35 S	10 W	$5\frac{1}{2}$ W	5 W	
35 S	45 E	$26\frac{1}{2}$ W	26 W	

PHILO-

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of the Earthquake felt in* New England, *and the neighbouring Parts of* America, *on the* 18*th of* November 1755. *In a Letter to* Tho. Birch, *D.D. Secret. R. S. by Mr. Professor* Winthrop, *of* Cambridge *in* New England.

Reverend Sir,

Read Jan. 13, 1757.

I Beg leave to lay before you the best account I am able to give of the great earthquake, which shook New England, and the neighbouring parts of America, on Tuesday the 18th day of November 1755, about a quarter after four in the morning. I deferred writing till this time, in order to obtain the most distinct information of the several particulars relating to it, both here and in the other places where it was felt; and especially the extent of it.

The night, in which this earthquake happened, was perfectly calm and ſerene. In the evening there was a fog over the marſhes bordering on the river Charles, which runs through this town: but this I found intirely diſſipated at the time of the earthquake, the air being then quite clear, and the moon, which wanted but 36^{h} of the full, ſhining very bright. The earthquake began with a roaring noiſe in the N.W. like thunder at a diſtance; and this grew fiercer, as the earthquake drew nearer; which was almoſt a minute in coming to this place, as near as I can collect from one of my neighbours, who was then on the road in this town. He tells me, that, as ſoon as he heard the noiſe, he ſtopt, knowing, that it was an earthquake, and waiting for it; and he reckoned he had ſtood ſtill about 2′, when the noiſe ſeemed to overtake him, and the earth began to tremble under him: but, as I doubted, whether it were ſo long, I counted ſeveral numbers to him as ſlowly as a clock beats ſeconds; and then he ſaid, he believed he could have counted half an hundred, at that rate, before the noiſe and ſhake came up to him. By his account, as well as that of others, the firſt motion of the earth was what may be called a pulſe, or rather an undulation; and reſembled (to uſe his own compariſon) that of a long rolling, ſwelling ſea; and the ſwell was ſo great, that he was obliged to run and catch hold of ſomething, to prevent being thrown down. The tops of two trees cloſe by him, one of which is 25, the other 30 feet high, he thinks waved at leaſt ten feet (and I depend on his judgment in this particular, becauſe he judged right of the height of the trees, as I found by actual

menſura-

menſuration); and there were two of theſe great wavings, ſucceeded by one, which was ſmaller. This ſort of motion, after having continued, as has been conjectured, about a minute, abated a little; ſo that I, who was juſt then waked, and, I ſuppoſe, moſt others, imagined, that the height of the ſhock was paſt. But inſtantly, without a moment's intermiſſion, the ſhock came on with redoubled noiſe and violence; though the ſpecies of it was altered to a tremor, or quick horizontal vibratory motion, with ſudden jerks and wrenches. The bed, on which I lay, was now toſſed from ſide to ſide; the whole houſe was prodigiouſly agitated; the windows rattled, the beams cracked, as if all would preſently be ſhaken to pieces. When this had continued about 2′, it began to abate, and gradually kept decreaſing, as if it would be ſoon over: however, before it had quite ceaſed, there was a little revival of the trembling and noiſe, though no-ways comparable to what had been before: but this preſently decreaſed, till all, by degrees, became ſtill and quiet. Thus ended this great ſhock. It was followed by another about an hour and a quarter after, viz. at 5^h 29′. This, though comparatively ſmall, was very generally perceived, both as to its noiſe and trembling, by thoſe who were awake. On the Saturday evening following, viz. the 22d of November, at 27′ after eight, there was a third, more conſiderable than the ſecond, but not to be compared with the firſt. And on Friday the 19th of December in the evening, exactly at ten o' clock, there was a fourth ſhock, much ſmaller than either of the former, though, like them, preceded by the peculiar noiſe of an earthquake.

The whole lafted but a few feconds; but the jarring was great enough to caufe the window-fhutters and door of the room, in which I then was, to clatter. The fky was perfectly clear, and there was a very gentle and fcarcely-perceptible gale at S.W. Thefe four are the only fhocks, that I have been fenfible of from the 18th of November laft to this date; tho' more are faid to have been felt in other parts of the country to the northward of us.

As to the duration of the great fhock, people have differed widely, viz. from 1′ to 6 or 7. Our printed accounts have generally fixed it to about 2′, or 3 at the moft; but as thefe were only the uncertain guffes of perfons, who had no rule to gufs by, no dependence can be had on them. I am well fatisfied, that with us it continued 4′, or rather 4′ 1-half, taking in the whole of the time, from the firft agitation of the earth till it was become perfectly quiet; tho' the violence of the fhock did not laft above half fo long. This I am affured of, partly from the obfervations of fome gentlemen, who were up, and looked on their watches, when it began and ended; one of whom tells me it was 4′, and another, that it was near 5; and partly from my own obfervations, which were as follow. The preceding noon I had adjufted both my clock and watch to the apparent time, by a meridian line; and the following noon I found, that the watch had kept time exactly. Being awaked by the earthquake, I lay till the violence of it feemed to be over for the fecond time, the firft abatement happening juft after I waked. Till then I forbore to rife, becaufe the agitation was fo vehement, that I concluded it would be difficult, if not impracticable, to

to go from the bed to the chimney, without being thrown down; and therefore thought it beſt not to attempt it. The ſpace of time, in which I lay awake, I cannot think to be much, if any thing, leſs than 2′. This was the conjecture I formed at that time; though, it being but conjecture, I would not lay very great ſtreſs upon it, were it not ſupported by concurring obſervations. On the ſecond abatement I roſe, and lighting a candle, looked on my watch, and found it to be 15′ after four. The ſhock then was not quite over, but the windows continued rattling for about a minute longer, as near as I can remember; for the ſhock went off very gradually. As ſoon as I had looked on the watch, I went directly to the clock, which was in another chamber, that I might ſee whether that agreed with the watch, and found that it was ſtopt at $4^h\ 11'\ 35''$. Its ſtopping, however, was not immediately owing to the violence of the ſhock, though ſeveral clocks, and watches too, at Boſton, are ſaid to have been ſtopped by it, but to the following accident: Having ſome time before uſed a pretty long glaſs tube, in a particular experiment, I had ſhut it up in the clock-caſe for ſecurity; and this tube, being overthrown by the earthquake, lodged againſt the pendulum, and ſtopt its motion. By this accident, the beginning of the earthquake, I conceive, is determined with all the exactneſs, that can be deſired; for, ſo far as I can learn, the firſt ſhake was violent enough to overſet ſo tall, ſlender a body, and ſtanding in a poſition ſo near a perpendicular, as that tube; and it was impoſſible for the pendulum to make one oſcillation, after the tube had ſtruck againſt it. But

I am

I am not able to fix the end, nor conſequently the duration, with the ſame exactneſs: however, from the time, when the clock ſtopt, to my looking on the watch, it was about 3′ 1-half; and the jarring was not quite over till about a minute after this: ſo that I think I ſpeak within bounds, if I ſay, that this ſhock with us laſted at leaſt 4′. In other places, its duration might poſſibly be different. I was careful to note the time, when we had it, as exactly as I could, in hopes, that, by comparing it with the like accounts from diſtant places, we might be able to judge, with a good degree of exactneſs, of the courſe of this earthquake, the place of its origin, and the velocity of its progreſs. But all the accounts of the time, which I have yet ſeen, are ſo very lax, that no juſt concluſions can be drawn from them, with reſpect to either of theſe points. What I have been able to collect with relation to them, I ſhall ſet down preſently.

Thoſe, who ſuppoſe the duration much ſhorter, as 1′ 1-half, or 2′, urge, that a minute is a longer ſpace of time than moſt people are aware of; which is very true: but it ſhould alſo be conſidered, that if we judge of the length of any ſpace of time by the number of ideas, which paſs through our minds in that time; a very great fright,---a fright ſo great, as to take intire poſſeſſion of the mind for a time, and ſhut out every idea except that of the preſent danger, will make us judge the time to be much ſhorter than it really is. And this, I make no doubt, was the caſe with many; the ſurpriſe, into which they were thrown, being ſuch, as to keep out every idea, except that, which happened to ſtrike their minds with the

greateſt

greateſt force at the beginning. Thus, ſeveral perceived no noiſe diſtinct from that, which was occaſioned by the crackling of their houſes, and the diſturbance of the moveables in them; while others, who were waked by the noiſe, and ran from one room to another, have told me, that they felt nothing at all of the ſhake. For this reaſon, the conjectures, which perſons in theſe circumſtances made, as to the duration of the ſhock, ought not to be put into the balance with the actual obſervations made by watches.

The courſe of this earthquake ſeems to have been nearly from N. W. to S. E. My neighbour beforementioned, who was then abroad, and informed me, that the noiſe began about the N. W. told me at the ſame time, that it paſſed off towards the S. E. and that he heard the noiſe in that quarter gradually abating, as it became more diſtant, for about the ſame ſpace of time after the ſhock was over here, as he heard it in the N. W. before the ſhock began here. Other accounts, which I have ſince met with, agree with this. Thoſe, who were in ſuch clear open places, could make the beſt judgment in this matter; for ſuch, as were within doors, or ſurrounded with buildings, might eaſily be miſled by the various reflections of the ſound. I am induced to give the greater credit to this information by what I obſerved myſelf: for a key, which was thrown from off a ſhelf in my houſe, was found at a place on the floor, which bore very near N. W. of the place, from which it fell; though the ſituation of it before its fall was ſuch, that it might have been thrown in any direction, except towards the S. E.

An

An account, which we have lately received from the Weſt-Indies, agrees very well with the ſuppoſition, that our earthquake proceeded ſouth-eaſtward. The account is, that 'on the 18th of November, 'about two o' clock in the afternoon, the ſea with-'drew from the harbour of St. Martin's, leaving the 'veſſels dry, and fiſh on the banks, where there 'uſed to be three or four fathom water; and con-'tinued out a conſiderable time; ſo that the people 'retired to the high land, fearing the conſequence 'of its return; and when it came in, it aroſe ſix 'feet higher than uſual, ſo as to overflow the low 'lands. There was no ſhock felt at the above time.' As this extraordinary motion of the ſea happened about 9^h after our great ſhock, it ſeems very likely to have been occaſioned by the ſame convulſion of the earth. Now if this earthquake went off ſouth-eaſtward into the Atlantic, it muſt have paſſed conſiderably to the eaſtward of St. Martin's; and, in fact, it did not reach that iſland, there being no ſhock felt there. The motion of the ſea then was owing to a great agitation raiſed at a conſiderable diſtance in ſome part or other of the ocean, where the earthquake paſſed, and from thence propagated to that iſland. Nor is the length of time greater than what ſeems to be neceſſary for this effect. The earthquake itſelf, at the rate it moved with us, would be ſome hours in going from hence to the diſtance of St. Martin's: for ſound would be about $2\frac{1}{4}^h$ in moving to ſuch a diſtance; and the progreſs of the earthquake was ſlower than that of ſound, as appears from hence, that the roar of this earthquake arrived here near a minute before the ſhake. The reſt of the

the 9h might well be ſpent in conveying the motion excited in the water, from the place where it was excited, to St. Martin's; for the waves raiſed thereby could not move with near the velocity of ſound.

It is worthy of remark, that, of the five great earthquakes, which this country has felt ſince its ſettlement by the Engliſh, two have gone nearly in the ſame track as this laſt did. The firſt, which was on June 2. 1638, 'came from the northward, and paſſed 'ſouthward.' By the deſcription given of it, it was very much like our late earthquake, only perhaps not quite ſo violent. 'The noiſe and ſhakes of the 'earthquake, October 29. 1727, ſeemed,' it is ſaid, 'to come from the north-weſtward, and to go 'off ſouth-eaſterly; and ſo the houſes ſeemed to 'reel.' As to the great earthquakes of 1658 and 1662, we have no account of the courſes, which they went in. But, from the other three, it may be reaſonably conjectured, that the ſource of our earthquakes, or the place in which they originate, is in ſome part of Canada, or perhaps beyond it.

The extent of this earthquake ſeems to have been greater than that of any of our former earthquakes. This province of the Maſſachuſetts-bay, or rather the province of New Hampſhire, about the latitude of 43° north on the ſea-coaſt, ſeems to have been the center of it, or the place of its greateſt violence; and the ſhake to have been leſs conſiderable each way from hence towards the S. W. and N. E. By the accounts we have from the S. W. the ſhock was leſs at New York than it was with us; and ſtill leſs at Philadelphia, which is farther towards the S. W. By the beſt information I can procure, the

limit toward the S. W. was Chesopeak-bay in Maryland, the shock having been felt on the eastern side of that bay, and not on the western. For the other limit toward the N. E. we are informed, that the earthquake was felt at Annapolis Royal in Nova Scotia, though in a much less degree than with us. It shook off a few bricks from the tops of some chimnies, but was not perceived by vessels on the water. And a letter from Halifax says, 'The earthquake, which 'happened in the W. extended itself to this place, 'tho' scarcely perceivable here.' But it was not at all felt by our army, which lay encamped at Seganecto, about 100 miles N. from Halifax. Thus Halifax seems to have been very near the N. E. limit. I am not able to ascertain its eastern and western limits; but it extended to all our back inland settlements; and was perceived, though in a very small degree, by our army at Lake George, distant from hence about 130 miles N. W. by W. But it was not felt at all at the British fort of Oswego, situate on the south-eastern shore of Lake Ontario, and distant from hence about 250 miles W. by N. So great was the shock in the Atlantic, 70 leagues to the E. of Cape Anne, that the people on board a vessel there were suddenly surprised, just at the time of our earthquake, supposing they had run a-ground; till, on throwing over the lead, they found they had more than 50 fathom water. The extent of the earthquake E. and W. from Halifax to Lake George was about 550 miles; and its extent along the sea-coast, from N.E. to S.W. at least 800 miles. But if the agitation of the water at St. Martin's was occasioned by our earthquake continued into the Atlantic, as was conjectured above,

above, its extent, in a direction toward the S. S. E. muſt have been at laſt 1900 miles.

I ſhall now proceed to mention the principal effects of this earthquake, for which I can find ſufficient vouchers; for many ſtrange things have been related, which, upon examination, appear to be without foundation. Beſides the throwing down of glaſs, pewter, and other moveables in the houſes, many chimnies were levelled with the roofs of the houſes, and many more ſhattered, and thrown down in part. Some were broken off ſeveral feet below the top, and, by the ſuddenneſs and violence of the jerks, canted horizontally an inch or two over, ſo as to ſtand very dangerouſly. Some others were twiſted, or turned round in part. The roofs of ſome houſes were quite broken in by the fall of chimnies; and the gable ends of ſome brick buildings thrown down, and many more cracked. Throughout the whole country, the ſtone fences were more or leſs thrown down. The vane upon the public market-houſe in Boſton was thrown down; the wooden ſpindle, which ſupported it, about five inches in diameter, and which had ſtood the moſt violent guſts of wind, being ſnapt off. A new vane, upon one of the churches in Boſton, was bent at its ſpindle two or three points of the compaſs; and another at Springfield, diſtant about 80 miles weſterly from Boſton, was bent to a right angle. A diſtillers ciſtern, made of plank, almoſt new, and very ſtrong put together, was burſt to pieces by the agitation of the liquor in it; which was thrown out with ſuch force, as to break down one whole ſide of the ſhed, that defended the ciſtern from the weather; as alſo to

ſtave off a board or two from a fence at the diſtance of eight or ten feet from it. In ſome parts of the country, particularly at Pembroke and Scituate, about 25 miles S.E. from hence, there were ſeveral chaſms or openings made in the earth, from ſome of which water has iſſued, and many cart-loads of a fine whitiſh ſort of ſand. Theſe are the principal effects of this earthquake on the land, ſome of which argue a very quick and violent motion of the earth. Tho' the degree of violence was doubtleſs different in different places, yet, that I might make ſome eſtimate of it with us, I meaſured the greateſt diſtance on the ground, to which any of the bricks, which were thrown off from the tops of my chimnies, had reached, and found it to be 30 feet, and the height from which they fell was 32 feet. Now ſince bodies fall thro' 16 feet nearly in ·1″ of time; and the times, in which they fall through other heights, are in the ſubduplicate ratio of thoſe heights; it follows, that the velocity, wherewith thoſe bricks were thrown off, was that of above 21 feet in ·1″ of time: for the ſubduplicate ratio of 32 to 16 is the ſame as the ſimple ratio of 30 to a little more than 21. But the velocity was leſs at leſs heights: for the key before ſpoken of, as thrown from off a ſhelf in a chamber in my houſe, was not thrown ſo far, in proportion to the height thro' which it fell, as the bricks were from the top of the chimnies; and in my lower rooms nothing was thrown down, but a ſmall bell in the garret was made to ring by it. Hence it appears, that our buildings were rocked with a kind of angular motion, like that of a cradle; the upper parts of them moving ſwifter, or thro' greater ſpaces in

in the ſame time, than the lower; the natural conſequence of an undulatory motion of the earth.

But the agitation occaſioned by this earthquake was not confined to the land: it was very ſenſible on the water, and even at conſiderable diſtances in the ocean. The veſſels in our harbours were ſo ſhaken, that it ſeemed to thoſe, who were in them, as if they were beating on the bottom. Some, that were in the bay, coming in from ſea, thought they had run upon rocks or ſands. One very uncommon effect of this concuſſion is related by ſeveral of our ſeafaring men, that almoſt immediately after the earthquake, large numbers of fiſh of different ſorts, both great and ſmall, came up to the ſurface of the water, ſome dead, and others dying.

The center of our former earthquakes, as well as of this, ſeems to have been near the river Merrimac, about the latitude of 43° north, and 40 miles north from hence; many ſhocks having been felt in that neighbourhood, which did not extend to this place. The late Rev. Mr. Plant of Newbury, which is ſituated at the mouth of that river, has given a very particular journal, in *Philoſ. Tranſact.* N°. 462. of the ſhocks felt there from 1727 to 1741, few of which were perceived here or at Boſton. I remember none after the memorable 29th October 1727, beſide that on 30th January 1728, about two in the afternoon; and that on 5th September 1732, which, by his account, did conſiderable damage at Montreal in Canada, but it was very ſmall at Boſton. That alſo on 6th February 1737, about a quarter paſt four in the afternoon, which he calls a conſiderable ſhock, was perceived at Boſton; and ſo was that on 7th

December

December following, a little before 11 in the night. From the conclusion of Mr. Plant's journal, till the earthquake which is the subject of this letter, I know of none, but that which happened on Sunday June 3. 1744, at a quarter after ten in the morning. The roar of this was as loud as any I ever heard, but the shake not so great. The day was very fair and hot, with a little wind in the morning at W. S. W. which in the afternoon came round to N.N.W. The season preceding was hot and dry, there having been no rain from 23d May. On the 1st June, at four in the afternoon, Hauksbee's thermometer stood at 5,2; on the 2d, at five in the afternoon, it was at 1, with high wind at S. W.; on the 3d, at eight in the morning, it was at 19,8; and at six 1-half in the afternoon at 3,8. From the 1st June to the 2d, at the hours just mentioned, the barometer had fallen from 29,92 to 29,82; from which time it continued rising till the 4th at eight in the morning, when it was got up to 30,12; being, at the time of the earthquake, at 29,94. The rest of the month the weather was in general very hot, with many thundershowers.

As the late Hon. Judge Dudley, who has given a very just account of the great earthquake of 29 Oct. 1727, in *Philos. Trans.* N°. 437, has inserted an account of the weather in the preceding part of that year; and as our last earthquake happened at the same time of the year as that did, within 8 or 9 days (regard being had to the difference between the Julian and Gregorian styles); I hope it will not be disagreeable, if I give an account of our weather the last year: in doing which, I shall follow, as near as may be,

be, Mr. Dudley's method, ſetting down the particulars in correſponding columns, that ſo a compariſon may more eaſily be made between theſe two years.

WEATHER in

1727, O. St.	1755, N. St.
January and February very moderate.	January, but eſpecially February, very moderate.
Beginning of March, a great deal of ſnow, and ſome cold weather: afterwards, pleaſant, rain at times, and once thunder and lightning.	4th of March the greateſt ſtorm of ſnow we had all winter. The whole month colder than February.
April, for the moſt part, fair, pleaſant. Plentiful rain, beginning and end of the month.	April, nothing very remarkable. No hot weather. Each of theſe four months afforded more ſnow and rain, than the common quantity, taken at a medium for 7 years together.
May, beginning, pleaſant; then a great deal of rain; afterwards, cold and very dry.	Firſt 20 days of May, dry; 14 to 18 incluſive, uncommonly hot; latter part, frequent thunder-ſhowers. The whole month drier than the medium.
June, abundance of thunder and lightning.	June, ten thunder ſhowers; 15 to 20 incluſive, uncommonly cool. The drieſt June ſince 1749.

July,

WEATHER in

1727, O. St.	1755, N. St.
July, very dry; a great deal of thunder and lightning.	July, ſeven thunder-ſhowers, and a little more rain than the medium.
Auguſt, exceeding hot and dry. One plentiful rain.	Auguſt, not very hot; much drier than the medium.
September, till the middle, very hot. More hot weather than in any ſummer. Middle, a violent north-eaſt ſtorm, with a great deal of rain.	September, variable; 10 to 14 incluſive, uncommonly hot; ſeveral other days uncommonly cool. Upon the whole, the ſummer rather cool than hot. The hotteſt weather was in the middle of May. No great rains; but rather more than the medium.
October, a pretty deal of cold weather.	October, a great deal of cold weather. Thunder-ſhowers on the 13th and 17th. Snow on the 20th, 25th, 29th, & 30th. But the quantity of rain and ſnow in the whole month leſs than the medium.
23, a great deal of rain, with the S. wind.	
25, at night a hard froſt.	
26, Winteriſh weather, and a little ſnow.	
28, Cold. Wind N.W.	
29, Cold. Little wind at N.W. Evening quite calm, and a clear ſky.	November began with cold and wet, there falling, in the eleven firſt days 3,404 inches of rain; and on the 11th in the morning there was thunder and lightning with the rain;

rain; and at a quarter paſt two in the afternoon, the barometer was at 29,46; which was lower than it had been ſince the 15th of October. From thence, till the day of the earthquake, my diary ſtands thus:

November 1755.

D.	H.	Barom.	Hauksb. Therm.	Wind.	Weather.	Rain, &c.
12	7¾ M	29,78	63,7	W 1	Very fair. Somewhat foggy.	,003
	2¾ E	82	51,7	W 1	Fair.	
13	9 M	30,14	68	NW 2	Very fair.	
	6¼ E	21	56,8	NNW 1	Clear.	
14	8½ M	42	69,4	0	Fair.	
	1½ E	45	59	NE 2	Very cloudy.	
	8¾ E	5	. .	NE 1	Clear.	
15	8½ M	4	74,6	0	Cloudy. Hazy. White froſt.	
	4 E	32	60,5	E 1	Very fair.	
16	0¼ M	27	70,8	0	Foggy.	
	2 E	28	59,9	N 1	Fair.	,013
	9¼ E	32	. .	N 1	Fair with clouds. Foggy.	
17	8 M	3	70,1	N 1	Cover'd. Foggy.	
	1½ E	27	59	E 1	Very fair. Evens ſomewh^t foggy	,001
18	4¼ M	17	74,1	0	Clear. A violent earthquake.	
	8 M	16	78	0	Very fair. Great white froſt.	
	3¼ E	11	58,3	E 1	Very fair and hazy.	,017
	10 E	08	69,1	0	Clear. Somewhat hazy.	

From this time the barometer roſe till the 20th, when, at 8¼ M. it was up at 30,44, the ſky covered, wind N 2. Then it fell till the 23d at 6½ E, when it was ſo low as 28,87; which was lower than it had been ſince the 6th of February laſt. The afternoon of the 22d, and night following, when we had another ſhock, it was calm, and rained 1,205 inches. This leads me to obſerve, that though the *ſerenity*, as well as *calmneſs*, of the air, is a circumſtance taken notice of in many earthquakes, both in this and in other parts of the world; yet it does

 not

not always obtain, at leaſt in the ſmaller ſhocks; and, ſo far as I have had opportunity to obſerve, the *calmneſs* of the air has more conſtantly attended upon earthquakes, than its *clearneſs*. The white froſt on the morning of the earthquake, which, when melted, I found to be of the depth of $\frac{17}{1000}$ of an inch, was almoſt double of any white froſt we have had for ſeven years paſt, and about five or ſix times as great as we commonly have. The barometer and thermometer underwent no alteration at the time of the earthquake: only, my barometer, which has an open ciſtern of quickſilver, and ſtood in a chamber, was ſo agitated, that part of the quickſilver was daſhed over the ſides of the ciſtern, and ſcattered upon the floor. This ciſtern was a cylindric cup, whoſe ſides were an inch higher than the ſurface of the quickſilver.

I ſhall not pretend to make a compariſon between the weather of the two fore-mentioned years, nor inquire how far Mr. Dudley's conjecture (*Phil. Tranſ.* N°. 437. p. 66.), as to the influence of the weather in producing the earthquake of 1727, might be affected by ſuch a compariſon. I chooſe to leave this to you, Sir, and to the other gentlemen of the Royal Society, who, I know, are much better able to make a proper judgment in this matter; and beg leave to ſubſcribe, with the greateſt reſpect to that illuſtrious Society and yourſelf,

Reverend Sir,

Your moſt obedient,

and moſt humble Servant,

John Winthrop.

Cambridge *in* New England, 10 Jan. 1756.

II. *The*

II. *The ſtrange Effects of ſome effervescent Mixtures; in a Letter from Dr.* James Mounſey, *Phyſician of the* Ruſſian *Army, and F.R.S. to Mr.* Henry Baker, *F.R.S. Communicated by Mr.* Baker.

Moſcow, Sept. 20th, 1756.

Read Jan. 20, 1757.

MR. Butler, a paper-ſtainer, trying to make ſome diſcoveries for the better fixing of colours, was put in great danger of his life by the following experiments:

Having put into one gallipot a quarter of an ounce of verdegris, and into another pot two leaves of falſe-gold leaf, to each he poured about a ſpoonful of aqua-fortis. They began immediately to ferment, eſpecially the gold-leaf. He was very aſſiduous in ſtirring them, to make the ſolution perfect. Having nothing elſe at hand, he did this with a pair of ſmall ſciſſars, at arm's length, carefully turning away his face, to prevent the fumes from entering his lungs. He was called away, about other buſineſs, before he had quite ended his proceſs; and ſoon after waſhed and ſhifted himſelf: but had ſcarce finiſhed before he felt a burning pain in the ring-finger of his right hand, which he imputed to his having inadvertently touched the aqua-fortis. This increaſed every moment, and affected the whole hand with burning pain and ſwelling, which very ſoon ſubſided: but then it flew into the left hand, and, a few minutes afterwards, into the inſides of his legs, as if ſcalding water had

been thrown on them. His ſtockings being immediately pulled off, there appeared a great many red ſpots, as large as ſix-pences, ſomething raiſed above the ſkin, and all covered with very ſmall bliſters.

In about two hours after the accident, I firſt ſaw him: he was very uneaſy, complaining of pain, and great anxiety, at the pit of the ſtomach, as if a burning hot iron was laid on it: ſo he expreſſed himſelf. His pulſe was regular, but ſlower and weaker than natural: he had a nauſea, and complained of a very coppery ſmell and taſte. I ordered ſome alcaline volatile medicines, and to drink ſmall ſack-whey. He vomited once, and had four or five ſtools, and then his ſtomach grew eaſy. But the ſcene ſoon began again with lancing pain in the left eye. He continued the ſame medicines, drank plentifully of the whey, and was kept in a breathing ſweat, by which he found ſome eaſe at night: but whenever the ſweating leſſened, the burning pains returned in broad flakes, changing from one part of the body to the other; ſometimes with ſhootings in his eye, and ſometimes along the penis, but he had no heat of urine. His pulſe continued regular, but weak; and in ſeveral places of his body ſuch kind of ſpots ſtruck out as thoſe on his legs.

Monday, the third day, in the morning, after ſleeping well, his pulſe was ſomewhat raiſed, and he continued eaſy till about eleven o' clock, when the burning pains returned, ſhooting from place to place; but always ſo ſuperficial, that he could not diſtinguiſh whether it was in or under the ſkin. Rubbing the part affected with one's hand gave eaſe: but when the ſweating went off, and the burnings and ſhootings

ings became insufferable, I always put him into a bath of hot water, with some wood ashes, kept ready in the room; which gave him great relief. This afternoon he felt violent burning pain in his great toes, and sometimes in his left hand, with shootings up to the shoulder. Once he cried out, in great pain, that his shoulder was burst; for he felt something fly out with a sort of explosion: but, examining the part, I found nothing particular. He observed, when the flaky burnings began, they were as if they kindled from a point, and flashed like lightning, as he termed it. He was very often tormented with such pains on the pit of the stomach; and this evening had shootings thro' the back, with a pain in the belly. He complained of a strong sulphurous smell, which, he said, was like to suffocate him; tho' his breathing seemed easy, and his lungs no way affected. In the night he was seized with great pain about the heart, and cried out violently, that his heart was on fire: but after taking a dose of nervous medicines, and being put into the bath, he was soon freed from this, and passed the rest of the night tolerably well. At the time of such violent attacks the pulse continued regular, but still slower and softer than usual.

Tuesday. He complained most of his toes, and now and then burning pains in the forehead.

Wednesday. This whole day it continued most in the toes of the left foot; but in the evening the pain on the stomach returned, which lanced to the left side, with dartings inwardly. He became so uneasy and restless, that I was obliged to add some opium to the other medicines; which answered very well.

Thursday.

Thurſday. The pains kept moſt in the toes of the left foot.

Friday. Nothing particular, except his feeling, with ſharp pain, a ſpark (as he called it) fly out of his right cheek, in the ſame way, he ſaid, as that, which burſt on his ſhoulder, but much leſs. He perceived no pain in that part before this; nor any thing after, beſides a ſoreneſs, which laſted for ſome days. Hitherto he had been kept in a continual ſweat: his appetite was greater than his allowance; his digeſtion good; and his reſt indifferent. From this time he was not attacked by any violent ſymptoms; and could be quiet, tho' he did not ſweat.

On Sunday he began to get out of bed; but was often ſeized with glowing pains, ſuddenly affecting different parts of the body; which ſeldom continued an hour in one part, but ſhifted from place to place: theſe he was troubled with, in a leſs degree, even long after he went abroad.

By care and watchfulneſs the violence of the ſymptoms were kept under; and, by the uſe of antidotes for poiſons of the nature of what he received this from, the diſeaſe was overcome, and the patient recovered his perfect health and ſtrength.

III. *Ex-*

III. *Extract of a Letter of* J. Wall, *M. D. to the Rev. Dr.* Lyttelton, *Dean of* Exeter, *and F.R.S. concerning the good Effects of* Malverne *Waters in* Worceſterſhire.

SIR, Worceſter, Dec. 22. 1756.

Read Jan. 20, 1757.

THE Malverne Water much deſerves encouragement, ſeveral very extraordinary cures having been done by it lately. I propoſe to make a collection of the principal, and publiſh them, as an appendix to my little treatiſe. Amongſt other remarkable inſtances of their great effects are the following, which have happened this year. A poor woman, formerly a patient in our infirmary for a fiſtulous ulcer in the hip, and another in the groin, which penetrated the abdomen, has received her cure there, tho' ſhe was reduced to ſo great a degree, as to be thought incurable, and ſent into the country on a milk-diet, *&c.* as the laſt reſource. The diſcharge from the ſores was prodigiouſly great, and ſo offenſive, that ſhe could hardly be borne in a room. The water took off the ill ſmell almoſt inſtantly; the diſcharge ſoon leſſened, and grew thick and well-conditioned; her hectic ſymptoms went off in proportion; and, by continuing the uſe of the water for five or ſix months, ſhe is cured.

A woman with a phagedenic ulcer in the cheek, throat, and noſe, from an ozæna in the hollow of the cheek-bone, received great relief this year, in five or ſix weeks time; the external ulcer, which had

had almoſt deſtroyed the whole cheek, being healed in that time, and the other parts much amended. Her affairs would not permit her a longer continuance at the well; but ſhe continues the uſe of the water at home, and finds great relief from it there. I hope another ſeaſon will complete the cure.

Mr. Parry, of Clent, had his ſkin cleared, and perfectly healed, in five weeks; tho', when he came to the well, he was covered with an elephantiaſis; for which he had tried moſt of the purging waters, and ſea-water, under the direction of Dr. Ruſſell, without effect. So bad was he, that he could not move a limb but the ſkin cracked, and ouzed out a filthy ſanies; and he left the mark of his body every night in his bed. The waters have alſo had another very ſurpriſing effect on him: for they have been his Helicon, and converted him into a poet; he having written a poem on the occaſion, which he ſhewed to Lord Foley and Dr. Dalton.

I know a Lady, who, we had great reaſon to fear, had an internal cancer, who has lately received great advantage from the uſe of theſe waters, after other things had been tried unſucceſsfully.

I could ſend many more inſtances; but the compaſs of a letter will not admit of it: and I ſhould be afraid of having tired you already, did I not know, that it muſt give you pleaſure to hear of its extenſive utility. I am, Sir,

Your moſt obliged

humble Servant,

J. Wall.

IV. *An*

IV. *An Account of the* Carlſbad *Mineral Waters in* Bohemia: *In a Letter to the Right Honourable the Earl of* Macclesfield, *Preſident of the R. S. by the Rev.* Jeremiah Milles, *D. D. F. R. S.*

My Lord,

Read Jan. 20, 1757.

MR. Watſon having favoured the Society with an Account of Dr. Sprengsfeld's treatiſe on the Carlſbad waters, I have taken the liberty to ſubmit to your Lordſhip ſome obſervations on the ſame ſubject, which I made during my ſtay in that place; together with ſome ſpecimens of different ſorts of incruſtations, which are formed by thoſe waters.

Carlſbad is a ſmall town, ſituated on the confines of Bohemia, at the diſtance of 14 German, or 28 French, leagues weſt of Prague. It is remarkable for its warm mineral ſprings, which are ſaid to have been accidentally diſcovered, in the year 1370, by the Emperor Charles the IVth, as he was hunting; from whom they received their preſent name of Carlſbad, or Charles's bath. Theſe waters ſoon growing into repute, occaſioned the building of a ſmall neat town, conſiſting chiefly of houſes calculated for the accommodation of the company, who frequent this place in the ſummer time. There are two warm ſprings, which riſe in the middle of the town, very near each other: and tho' they are ſuppoſed to be of the ſame

quality, yet, as one is much warmer, it is thought likewiſe to be more efficacious than the other. The former of theſe, called the Brudel, riſes very near the bed of the ſmall river Tepel, which runs thro' the middle of the town, and is ſometimes overflowed by it. The water iſſues with great force from the bottom of this ſpring, riſing in a conſiderable body to the height of ſix feet perpendicular; and would force itſelf much higher, if it were confined within a narrower compaſs. The ſpring is incloſed with a ſquare wall, within which are fixed three wooden pipes, which convey the water from the bottom of the ſpring into a reſervoir; which diſtributes it into a number of ſmall troughs, communicating with the ſeveral bathing-houſes, which are built on both ſides of the river for the uſe of the patients. This ſpring is ſo impetuous, that they are obliged to pave and ramm the bed of the river, leſt it ſhould force itſelf up in the channel: and I obſerved one place on the river ſide, where it had burſt thro' the rock; and they had been obliged to confine it, by faſtening down a large ſtone on the orifice.

The water of this ſpring is ſo hot, that you cannot bear your hand in it; and the inhabitants make uſe of it for ſcalding their pigs and their poultry.

The water, when put into a glaſs, has a bluiſh caſt, not unlike that of an opal: and tho' I could not diſcover, that in 24 hours it had depoſited the leaſt ſediment, yet there was a thin whitiſh ſcum collected on the ſurface; and I obſerved the ſame in the baths, where it was much thicker; and was of the colour, and almoſt of the conſiſtence, of a wafer.

It

It has a ſalt taſte when firſt taken from the water, and is made uſe of by the inhabitants for cleaning of teeth and ſcouring ſilver: it is called Baden Flaum.

Tho' this water does not depoſit any ſediment, yet it is remarkable for the ſpeedy and ſtrong incruſtation of all bodies, which are put into it. Little plaiſter figures are ſold here, on purpoſe to verify the experiment; which, tho' perfectly white when put into the ſpring, are, in eight-and-forty hours, entirely covered with a yellow incruſtation. The ſame effect is obſerved on the pipes and channels, thro' which the water is conveyed. If care were not taken to clean them four or five times a year, they would be intirely choaked up; and in ſome parts, where it has not been neceſſary to clean them ſo often, I have ſeen them covered with an incruſtation two inches thick. In ſurrounding and covering theſe wooden pipes, they do not change the nature of the wood; but it is obſervable, that they add great hardneſs and ſolidity to it: ſo that it is affirmed a piece of deal will laſt a hundred years in this water. The head ſpring is cleared out once in 30 or 40 years, with a very great expence: at which time they are obliged to break off all the ſtony incruſtation, which had been made by the water ſince the laſt cleaning; and if neglected would (as it has ſometimes actually done) choak the paſſages, and oblige the ſpring to find vent in ſome other place. The incruſtations formed by theſe waters are of different kinds: that, which is made in the troughs and pipes, thro' which the water is conveyed after it comes above ground, is of a light ſandy nature, of a looſe contexture, and a

bright yellow. It is uſed by the inhabitants as a gentle corroſive for eating off proud fleſh. There is another of a darker colour, and a much harder nature, which is found at the very mouth of the ſpring, where it burſts out of the rock. There are other ſorts taken out of the ſubterraneous cavities of the ſpring at the time it was cleaned. In what manner they are formed, is not ſo eaſy to determine; unleſs there were an opportunity of obſerving in what manner and direction they lie within the ſpring. They ſeem to be an alabaſtrine ſpar, and are beautifully marked with ſtrait veins of different colours, which may be ſuppoſed to have received their tinge from the different colour of the ſpring-water at the time when this ſediment, or rather ſcum, was formed upon it. They find pieces of this kind moſt beautifully variegated; and ſome of them large enough, by fineering, to make tables: theſe poliſh very well, and are not much inferior to jaſper in appearance. It is a part of the manufacture of the place, to work this ſort of ſtone into ſnuff-boxes, cane-heads, and ſleeve-buttons.

There is likewiſe another ſort of incruſtation different from all theſe, which was found ſome years ago, in digging for the foundations of the new pariſh-church, which is about 300 yards diſtant from the Brudel ſpring. They found there the ſame kind of water; but it did not riſe with ſo great force as in the other ſpring: and they diſcovered in the cavities large maſſes of a ſtony concretion, which were a ſort of piſolithi, moſt of them in a globular, but ſome in an oval form, from the ſmalleſt ſize to the bigneſs of a nutmeg; the former ſort lying in maſſes, the latter generally ſingle and detached: they are perfectly

perfectly white, hard, and ſmooth, and appear to conſiſt of a great number of lamellæ formed round a ſmall nucleus. This ſort of incruſtation has been found in no other place; but there are ſome of a browner ſort; and more irregular ſhapes, which are taken out of the Brudel.

The medicinal virtues of theſe waters have been been treated of by German authors. They are eſteemed to be particularly efficacious in removing obſtructions, and in caſes of the ſtone and gravel; of which the treatiſe lately produced to the Society contains many remarkable proofs. They are much frequented in theſe and in other caſes; ſo that they have generally 200 perſons in a ſeaſon drinking the waters. The ſeaſon begins in May, and ends in Auguſt. They drink them in the following method. They begin with a purge; and aſſiſt its operation with ten or twelve chocolate-cups of the water taken within five minutes of each other. The day following they take the waters in the ſame quantity, and at the ſame intervals, keeping themſelves all the time in a warm room; which, with the warmth of the waters, occaſions a moſt plentiful perſpiration. This is repeated for ſeven or eight days, increaſing daily two or three cups of the water, till they come to drink 25 or 30 cups a day. The operation continues from eight of the clock in the morning till noon. Some bleed once in the middle of the courſe, others not at all. After they have finiſhed this courſe of drinking, they bathe two days ſucceſſively, continuing in the bath half an hour, or longer, as their ſtrength permits them, or their caſe requires. This is the whole courſe; which is repeated two or three times,

times, or oftener, as they find neceſſary. The whole is concluded with a gentle purge, tho' the waters themſelves are of a laxative nature.

There is another ſpring in the town of the ſame nature, but not ſo warm, as the Brudel: it is called the Mill-ſpring, and is only tepid. Thoſe of a warm or weak conſtitution make uſe of this inſtead of the other, both for drinking and bathing.

There are likewiſe ſeveral chalybeat ſprings in the neighbourhood of Carlſbad; one at half a mile, and the other at two leagues diſtance from the town. Both of them ſeem to reſemble the water of the Pohun ſpring at Spa; but are not near ſo ſtrong. They do not uſe them medicinally on the ſpot; but they are brought to Carlſbad, and ſold, in order to be drank with their wine. I am,

My Lord,

With the greateſt reſpect,

Your Lordſhip's

Moſt obedient humble Servant,

Jeremiah Milles.

Groſvenor-ſtreet,
Jan. 19th, 1757.

V. *An Eſſay towards aſcertaining the ſpecific Gravity of living Men. By Mr.* John Robertſon, *F. R. S.*

Read Jan. 27, 1757.

SOME time laſt autumn I had occaſion to draw up a few examples on the uſe of a table of the ſpecific gravities and weights of

of ſome bodies. Among other things, that occurred then to me, I thought it might be uſeful to know the ſpecific gravity of men. In order to make ſome experiments on this ſubject, I got a ciſtern made, of 78 inches in length, 30 inches wide, and 30 inches deep: it was conſtructed as near a parallelopiped as the workman could, to prevent tedious operations in computing the horizontal ſections of the ciſtern by the ſurface of the water. I then endeavoured to find ten perſons, ſuch as I propoſed to make the experiments withal; namely, two of ſix feet high, two of five feet ten inches, two of five feet eight inches, two of five feet ſix inches, and two of five feet four inches. One of each height I propoſed ſhould be a fat man, and the other a lean one; but I could not ſucceed in procuring ſuch men; and, after waiting till near the middle of October, I was obliged to put up with ſuch, as would ſubmit themſelves to the experiment at that ſeaſon of the year. They were all labouring men, belonging to the ordinary of Portſmouth yard, and, except one or two of them, who were middling ſized men, were for the moſt part very thin and ſlim made perſons. I had alſo provided a ſliding meaſure to take their heights, and ſcales to weigh them in. Every thing being prepared, each man ſtript himſelf in an adjoining room, and put on a pair of trowſers for decency's ſake: his height was firſt taken, then his weight, and then he immerſed (fortified with a large dram of brandy). A ruler, graduated to inches, and decimal parts of an inch, was fixed to one end of the ciſtern, and the height of the water noted before a man went in, and to what height it roſe when he ducked himſelf

under

under its ſurface; and of theſe ſeveral obſervations is the following table compoſed.

N°.	Heights. Ft.	Heights. In.	Wt. Pds.	Ht. Water before immerſed. Inches.	Ht. Water when immerſed. Inches.	Water raiſed. Inches.	Solidity.	Weight Water. Pounds.
1	6	02	161	19,30	21,20	1,90	2,573	160,8
2	5	10$\frac{3}{8}$	147	19,25	21,16	1,91	2,586	161,6
3	5	9$\frac{1}{2}$	156	19,21	21,06	1,85	2,505	156,6
4	5	6$\frac{3}{4}$	140	19,17	21,21	2,04	2,763	172,6
5	5	5$\frac{7}{8}$	158	19,13	21,21	2,08	2,817	176,0
6	5	5$\frac{1}{2}$	158	19,09	21,26	2,17	2,939	183,7
7	5	4$\frac{3}{8}$	140	19,05	21,06	2,01	2,722	170,1
8	5	3$\frac{1}{8}$	132	19,01	20,86	1,85	2,505	156,6
9	5	4$\frac{1}{8}$	121	18,97	20,76	1,79	2,424	151,5
10	5	3$\frac{1}{4}$	146	18,93	20,66	1,73	2,343	146,4

In making of theſe experiments, I remarked ſome inconveniencies, which I did not at firſt advert to, and which, at that time, I could not prevent. I intended, that each man ſhould have got gently into the water, immerſed himſelf all but his head, and ſo have ſtaid until the motion of the water had ceaſed; then he was ſuddenly to have ducked his head under, and have continued ſo a few ſeconds of time, until I had noted the riſe of the water; and, after his leaving the ciſtern, another was not to go in until the water was free from motion. Could theſe things have been done, as I had projected, I could have recommended the foregoing table as ſufficiently complete: but I muſt obſerve, that beſide the men's being of different ſizes from what I had deſired, they were in too much haſte to be diſmiſſed (with another dram

after dreſſing); ſo that the water was not quite ſtill when they got into the ciſtern: neither could I perſuade all of them to lay themſelves down gently, much leſs to keep their heads under water ſo long a time as one ſecond: ſo that, in moſt of the obſervations, the ſurface of the water was far from being quite ſo ſtill, as to render the meaſures perfectly exact, I being obliged to catch them, as it were, by taking the mean height between the librations. Moreover, the great area of the ciſtern was no inconſiderable bar to the accuracy I expected. However, as I do not recollect experiments of this kind anywhere recorded, theſe, perhaps, may give ſome ſatisfaction to ſuch perſons, who may have the curioſity to deſire ſome knowlege on this ſubject. Were I to make any more obſervations of this kind, I would chuſe an upright parallelopiped, not above 18 or 20 inches in the ſide of the ſquare; into which the perſon ſhould let himſelf down by ſteps nailed to the ſide: for in ſo ſmall an area the motion of the water would ſooner ſubſide; neither would the librations be any thing near ſo large as on a ſmaller ſurface.

One of the reaſons, that induced me to make theſe experiments, was a deſire of knowing what quantity of fir or oak timber would be ſufficient to keep a man afloat in river or ſea water, thinking that moſt men were ſpecifically heavier than river or common freſh water; but the contrary appears from theſe trials: for, excepting the firſt and laſt, every man was lighter than his equal bulk of freſh water, and much more ſo than his equal bulk of ſea-water: conſequently, could perſons, who fall into water, have preſence of mind enough to avoid the fright

uſual on ſuch accidents, many might be preſerved from drowning; and a piece of wood, not larger than an oar, would buoy a man partly above water ſo long as he had ſpirits to keep his hold. Some things herein advanced will perhaps more readily appear from the following relation.

The Lords of the Admiralty have appointed, for the exerciſe of the ſcholars belonging to the royal academy at Portſmouth, a ſmall yacht; wherein, during the ſummer months, thoſe young gentlemen are taught the practice of working a veſſel at ſea, under the directions of one of the maſter-attendants, aſſiſted by eight or ten ſeamen. The laſt time this yacht was out, which was about the beginning of laſt October, one of the ſcholars was ordered to heave the lead. The youth was about thirteen years old, ſmall of his age, and far from being fat; as he was ſtepping on the gunnel, he fell over-board: the ſea was rough, and the yacht had great way; ſo that he was preſently at a conſiderable diſtance from the veſſel. The ſkiff was immediately let down; but the painter not being faſt, the rope run an end, and the ſkiff went adrift. One of the ſeamen jumpt over-board, got into the boat, brought her alongſide the veſſel, took in another man, and then went after the youth, whom they recovered, after he had been in the water more than half an hour. The young gentleman, relating the affair, ſaid, that as he could ſwim very little, and judging he ſhould ſink if he ſtrove againſt the waves, he turned on his back, and committed himſelf to their mercy. He kept himſelf perfectly calm; and obſerved, when a wave was likely to break over him, to hold his

 breath,

breath, and to ſpurt out the water forced into his mouth. His hat, which happened to be tied by a piece of ſtring to one of his coat button-holes, he often held up with his hand, as a ſignal where he was. Juſt before the boat came up to him he began to be faint, his eyes became dim, and he thought himſelf on the verge of ſinking. This youth, who, by his prudence, ſaved himſelf from drowning, muſt, at that time, have been ſpecifically lighter than water.

VI. *An Inſtance of the Gut* Ileum, *cut thro' by a Knife, ſucceſsfully treated by Mr.* Peter Travers, *Surgeon, at* Liſbon. *Communicated by* John Huxham, *M.D. F.R.S.*

Liſbon, Auguſt 3d, 1756.

Read Jan. 27, 1757.

ANtonia Joſée da Coſta, one of the King's meſſengers, was attacked by two men, and, after receiving two blows on the head, was ſtabbed with a knife in the right hypogaſtric region, about three fingers breadth above the os pubis; the external wound being larger, as the knife was drawn obliquely towards the navel, and might be an inch and half in length, the perforation thro' the peritonæum about three quarters of an inch; the inteſtine ileum hanging out about ten or twelve inches, and quite pierced thro', the wound in the gut being large enough to admit my fore finger. After clearing the grumous blood with warm

water and Hungary water, the uninterrupted ſuture was made on both perforations; then dilating the common integuments of the belly, the inteſtine was reduced, leaving the ends of the two threads at the ſuperficies of the wound; and the external inciſion was ſewed up by the interrupted ſuture, and common dreſſings of lint and bandage applied. A clyſter was given him immediately after the above operation, of oil of olives, the yolk of an egg, and warm water.

4th. This day I found he had paſſed in the moſt excruciating pains, attended with continual vomitings: his fever very high, pulſe full and irregular: he was bled ten ounces this morning, and the like quantity this evening. The clyſters were continued thrice a day, with a decoction of wormwood and camomile inſtead of the warm water, and an anodyne mixture of mint-water, liquid laudanum, and ſugar, to be taken occaſionally; alſo three ounces of ſyrup of rhubarb, with an ounce of the freſh-drawn oil of ſweet almonds, to be taken, a common ſpoonful, every two hours.

5th. The bleedings were continued twice this day, three ounces each time, and the clyſters were adminiſtered as yeſterday. His pulſe and fever very high; he vomited ſome excrements; and towards night complained of a ſingultus.

6th. His bleedings and clyſters were continued as before. Finding his ſingultus and vomiting ſo very troubleſome, I ordered him Dr. Huxham's tincture of the bark; which was taken, a tea-ſpoonful, ſix times a day, in a little mint-water; which indeed greaty relieved him: his ſingultus and vomiting became leſs frequent.

7th.

7th. I found his ſkin moiſt, and pulſe ſoftened. I remained with him about an hour, and found a plentiful perſpiration throughout the body; on which I omitted his bleedings: the clyſters were continued; and towards night he had a proper diſcharge by ſtool, very fœtid, and inſpiſſated.

8th. I found, for the firſt time, he had ſlept laſt night, and ſeemed much in ſpirits: the ſymptomatic fever ſomething leſſened; and he had purged laſt night, and this day, eight times.

9th. He had five ſtools; his neauſea much abated; and a gentle diaphoreſis continued.

10th. The ſingultus ceaſed; his vomiting very little; his pulſe low, accelerated, and thread-like in its ſtroke; his purging violent; and he greatly complained of a moſt acute pain of the wounded parts. A paper of the following abſorbent powders was given him every three hours in rice-water. Crabs-eyes and red coral prepared, of each one drachm, crude opium two grains: theſe were made for three doſes, and given as above.

11th. He ſlept well; leſs pain; pulſe more equal; his diarrhæa much the ſame.

12th. The threads, with which I had made the ſuture of the inteſtine, came out of themſelves: the wound well-conditioned; fever very little; his diarrhæa rather increaſed. He ſent for me in the evening, being much alarmed, as he thought ſome liquids he had taken to have paſſed thro' the wounded parts.

13th. Yeſterday he complained of great pains in his belly: the diſcharge from his wound was laudable matter, and in good quantity.

14th.

14th. He rested well, and was seemingly well beyond expectation. His diarrhæa still continuing troublesome, he took the hartshorn decoction, with an addition of diascordium.

15th. I cut off the threads of the external wound, and continued dressings of digestive in the common method.

16th. He grew visibly better each day after; and on Sept. 7th I discharged him from any further attendance, his wound being intirely healed over, and he is in all respects very well, free from pain, or any inconvenience from the wound. He was kept seven and twenty days on chicken-broth, and never admitted to use any solids during that time: afterwards he was indulged with young chickens, &c.

VII. *An Account of a Visitation of the leprous Persons in the Isle of* Guadaloupe: *In a Letter to Mons.* Damonville, *Counsellor and Assistant-Judge at* Martinico, *and in the Office of King's Physician at* Guadaloupe. *By* John Andrew Peyssonel, *M.D. F.R.S. Translated from the* French.

SIR,

Read Feb. 3, 1757.

I Received the letter, which you honoured me with, and the order for visiting the persons afflicted with the leprosy. I was sensible of the misfortune of being ordered upon that commission:

commiſſion: I ſay misfortune; for ſuch you will perhaps think it, when you have read this letter.

It is now about 25 or 30 years ſince a very particular diſeaſe ſhewed itſelf in many perſons in this iſland Grande Terre. Its beginning is imperceptible: there appear but a few livid-red ſpots upon the ſkins of the white people, and of a yellowiſh red upon the blacks. Theſe ſpots in the beginning are not accompanied with pain, or any other ſymptom; but nothing can take them away. The diſeaſe increaſes inſenſibly, and continues ſeveral years in ſhewing itſelf more and more. Theſe ſpots increaſe, and extend indifferently over the ſkin of the whole body. Sometimes they are a little prominent, but flat. When the diſeaſe makes a progreſs, the upper part of the noſe ſwells, the noſtrils are enlarged, the noſe becomes ſoftened; tuberoſities appear upon the cheek-bones; the eyebrows are inflated; the ears grow thick; the ends of the fingers, and even the feet and toes, ſwell; the nails become ſcaly; the joints of the feet and hands ſeparate and mortify: ulcers of a deep and of a dry nature are found in the palms of the hands and ſoles of the feet, which grow well, and return again. In ſhort, when the diſeaſe is in its laſt ſtage, the patient becomes frightful, and falls to pieces. All theſe ſymptoms come on by very ſlow degrees, one after another, and ſometimes require many years to ſhew themſelves: the patient is ſenſible of no ſharp pain; but feels a kind of numbneſs in his hands and feet. Theſe people perform their natural functions all the while, eating and drinking as uſual: and when even the mortification has taken off the fingers and toes, the only ill

conſe-

conſequence, that attends, is the loſs of thoſe parts, that drop off by the mortification; for the wound heals of itſelf, without any application: but when it comes to its laſt period, the poor ſick perſons are horribly deformed, and truly worthy of compaſſion.

This ſhocking diſeaſe is obſerved to have ſeveral other unhappy characters; as, 1ſt, that it is hereditary, and that ſome families are more apt to be ſeized with it than others: 2dly, that it is infectious, being communicated *per coitum*, and alſo caught by keeping company with thoſe ſo diſeaſed: 3dly, that it is incurable, or at leaſt that no remedy has yet been found to cure it. They have in vain tried mercurials, ſudorifics, and every other regimen uſed in venereal complaints, under a notion, that this leproſy was the conſequence of ſome venereal taint: but, inſtead of being of ſervice, theſe methods rather ſerved to deſtroy the patients; for, far from leſſening the diſeaſe, the antivenereal medicines unlocked the diſtemper, the moſt dreadful ſymptoms appeared, and all thoſe ſo treated periſhed ſome years ſooner than the others, who did not take theſe medicines.

A very juſt fear of being infected with this cruel diſtemper; the difficulty of examining infected perſons before the diſeaſe came to its ſtate; the length of time of its lying concealed, by the care of the patients to keep it ſecret; the uncertainty of the ſymptoms, which diſtinguiſh it in the beginning; produced an extrordinary dread in all the inhabitants of this iſland. They ſuſpected one another, ſince virtue and merit had no ſhelter from this cruel ſcourge. They called this diſtemper the leproſy; and conſequently preſented ſeveral memoirs to the generals and intendants,

intendants, laying before them all theſe facts above-mentioned; their juſt apprehenſions; the public good; the trouble, that this diſtruſt cauſed in this colony; the complaints and hatred, that theſe accuſations occaſioned among them; the laws made formerly againſt ſuch leprous perſons, and their expulſion from civil ſociety. They required a general viſitation of all perſons ſuſpected of this diſtemper, that ſuch, as were found infected, might be removed into particular hoſpitals, or into ſome ſeparate places.

Theſe memorials were ſent to court, which, giving due attention to theſe juſt repreſentations, iſſued orders for the required viſitations in the moſt convenient manner, for the good of the public and of the ſtate.

In the mean time, the poſt of phyſician-botaniſt became vacant in the iſland of Cayenne. The miniſter was pleaſed to name me for it; and altho' this iſland was much more fertile in philoſophical diſcoveries than all the others, he thought proper to change my deſtination, and ſent me to this iſle Guadaloupe; and did not forget the article of the leproſy in my inſtructions.

When I arrived at Martinico in 1727, Monſieur Blondel de Juvencourt, then intendant of the French iſles, communicated to me both the orders of the court, and all the memoirs, that related to this affair. A tax was then laid upon the Negroes of the inhabitants of the Grande Terre, to raiſe a neceſſary fund for this viſitation, thus made at the expence of the colony; and Monſ. le Mercier Beauſoleil was choſen treaſurer of this fund.

Being arrived at Guadaloupe, the Count de Moyencourt, and Monſ. Meſnier, ordinator and ſubdelegate to this intendance, communicated to me the orders of the general and intendant. I began then to inform myſelf of the neceſſary inſtructions for acquiting myſelf of this dangerous commiſſion, the diſagreeable conſequences of which I eaſily foreſaw. I had ſo often heard of theſe leprous ſpots, that I judged it neceſſary to know, whether what was ſaid was true: for I could not comprehend, that a diſeaſe, which has ſo dreadful an end, and the ſymptoms then ſo terrible, ſhould continue ten or fifteen years without any other appearance than theſe ſimple ſpots; which, in themſelves, had nothing very bad. I demanded an inqueſt to be made, in order to ſatisfy myſelf of this fact: ſeveral ſurgeons, as practitioners, and ſeveral honeſt inhabitants, as obſervers, were accordingly called together, who all proved the ſame fact in this inqueſt; which you, Sir, may, and muſt, have ſeen in the regiſter of the ſubdelegation of this iſland. I am, moſt ſincerely,

SIR,

Your moſt humble and obedient Servant,

Auguſt 10. 1748. Peyſſonel.

RESULT *of the* VISITATION.

1ſt, NONE of the patients, whom we viſited, had any fever; and they all declared, that they found no inconvenience nor pain; but, on the contrary, eat, drank, and ſlept well, performing every

natural function; which was proved by their plumpness, which appeared even when the disease was most confirmed.

2. The disease began to shew itself in the Negroes by reddish spots, a little raised, upon the skin, being a dry kind of tetter, neither branny nor scabbed, and without any running, but of a livid-red, and very ill-conditioned. The Negroes sometimes bring these spots with them from their own country. The spots are constantly found upon every person troubled with this disease; and are in greater numbers, in proportion as the disease grows more inveterate.

3. Among the whites the disease shews itself at the beginning by spots of a livid violet colour, without pain; which are followed by little watery bladders, particularly upon the legs, which burst, and leave small ulcers with pale edges, and different in their natures from the common ulcers.

4. In proportion as the disease increased, the hands and feet grew larger, without any signs of inflammation; since neither redness, nor pain, nor any oedematous appearance accompanied it; but it was the very flesh, that increased in bulk. And this growth of the hands and feet was not attended with any sharp pain, but only a kind of numbness.

5. This bloated state of the hands and feet was succeeded by white deep ulcers under the skin, which became callous and insensible; and which emitted only a clear serous matter like water, and were but little painful. Afterwards the ends of the fingers became dry, the nails became scaly, and, I don't know how, they were eaten away; the ends of the fingers dropt off; then the joints separated

without pain, and the wounds cicatrized of themſelves, without the leaſt need of medicines. In the increaſe of the diſtemper hardneſſes and lumps were formed in the fleſh, the colour became tarniſhed, the noſe ſwelled, and the noſtrils grew wide: at laſt the noſe ſoftened like paſte, the voice became hoarſe, the eyes round and brilliant, the forehead covered with tetters and lumps, as well as the face; the eyebrows became very large, the countenance was horrible, the breath fœtid, the lips ſwelled, large tubercles were formed under the tongue; the ears grew thick and red, and hung down; and, ſuch was the inſenſibility of all the parts, that we run pins thro' the hands of ſeveral, without their feeling any thing of it. In ſhort, we were aſſured, that theſe people periſhed by degrees, falling into a mortification; and the limbs dropt off of themſelves, without any conſiderable pain, continuing ſtill to perform well their natural functions.

6. Theſe leprous people lived thus eaſy, if I may be allowed the expreſſion, for ſeveral years, even fifteen or twenty; for the diſeaſe begins inſenſibly, and ſhews itſelf but very ſlowly.

7. Antivenereal remedies, which were ordered for almoſt every patient we ſaw, were of no ſervice: if they ſometimes palliated ſome ſymptoms, they very often haſtened the progreſs of the diſeaſe: beſides, we never found the parts of generation at all infected, nor any thing, that looked like the pox, about them.

8. Some of theſe people had indeed particular ſymptoms. In ſome the hair fell off; which was replaced by a finer kind: in others, worms were found

found in their ulcers: want of ſleep, or frightful dreams, afflicted ſome; while others quite loſt their voice, or it became effeminate like that of eunuchs; and others, we found, ſtunk extremely.

9. Almoſt all of them, being deſirous of concealing their diſorders, endeavoured to deceive us, by alleging falſe excuſes for the cauſes of their ſores and ulcers: the greater part of them pretended, that the rats had eaten off their toes, and that burns had cauſed their ulcers. Theſe were the figures, that every where preſented to us.

10. We were confirmed in our opinions by experience, ſupported by verbal proceſs, that this was the ſtate of the diſeaſed; that the diſtemper could neither be the pox, nor the effect of an inveterate one: that it had no ſymptom of that diſeaſe; but that it had every character of what the ancients called leproſy, elephantiaſis, or ſuch other names, as they were pleaſed to give it. So that we do not heſitate to pronounce, that thoſe infected with this diſeaſe, as we have deſcribed it, ought to be treated as leprous perſons, and ſubject to the ordinances, which his majeſty was pleaſed to iſſue againſt ſuch perſons.

11. Again, we are well aſſured, from our obſervations, that the diſtemper is contagious, and hereditary; and yet the contagion is not ſo active, nor poiſonous, as that of the plague, ſmall-pox, nor even as the ring-worm, itch, ſcald, and other cutaneous diſorders: for, if that were the caſe, the American colonies would be utterly deſtroyed; and theſe perſons ſo infected, mixed as they are in every habitation, would have already infected all the Negroes, whom they come near.

12. We believe, that this contagion does not take place but by long frequenting the company of the infected, or by carnal knowlege. Besides, we have observed, that even such long frequenting, or cohabiting with them, are not always sufficient to communicate the disease; because we have seen women cohabit with their husbands, and husbands with their wives, in the distemper, while one is sound, and the other infected. We see families communicate and live with leprous persons, and yet never be infected; and thus, altho' experience, and the information of the sick, prove the contagion, we are of opinion, that there must be a particular disposition in people to receive the poison of the leprosy.

13. As to what regards the distemper's being hereditary, it is assuredly so. We have seen intire families infected; and almost every child of a leprous father or mother fall insensibly into the leprosy; and yet, in several other families, we have seen some children sound, and others tainted; the father has died of the disease, and the children grew old without any infection: so that, tho' it is certainly hereditary, yet we believe it is of the same nature with those in families troubled with the consumption, gravel, and other hereditary distempers; which are transmitted from father to son, without being so very regular, as to affect every one of the family.

14. We could never find out any certain rule of judging, at what age the disease shews itself first in those, who were begotten by infected parents: but we have, as far as we could, observed, with regard to women or girls, that the symptoms begin with the menses, and continue slightly till they have lain-

in

in of one or two children: but that then more visible, and indeed more cruel, symptoms appeared. As to men, or infants, there is no rule to know it in them.

15. For the explanation of the causes, symptoms, and what we think the most likely means of cure, we refer to a particular dissertation. Let it suffice here to observe, that we do not imagine, that the air, water, or manner of living, can produce it; for we have found as many sick in the low marshy places, as in more airy saline places: and if many Negroes were infected in the Grand Terre, where they drink the foul waters of ponds and lakes, we see an equal number ill in places, where they have fresh rivers and running waters; but they may prove proper causes for unlocking, and disposing persons to receive, the disease.

16. We believe, and are persuaded, that the origin of this disease among the Negroes comes from Guinea: for almost all the Negroes from that country told us they came from thence with these reddish spots, the first and certain signs of the distemper begun.

17. As to the infected Whites and Mulattoes of this island, we were informed, that the disease was not known among the Whites till about 25 or 30 years ago; when, out of charity, they received a miserable object from the island of St. Christopher's, whose name was Clement; who, about the year 1694, fled hither. It was the family of the Josselins, called the Chaloupers, that protected him; which family, as also that of the Poulins, we found infected by communication with this sick man, as old Poulin declared to us.

It

It is thought, that others were infected by communication with the Negro women, especially in the beginning, when the disease is much concealed, and at a time when they did not mistrust one another; which is very probable, since we saw many Mulatto children, born of female Negroes, infected and leprous.

18. However this be, this distemper has had its progress; and in this visitation, which we made, we examined 256 suspected persons; that is, 89 Whites, 47 free Mulattoes, and 120 Negroes: among whom we found 22 Whites, 6 Mulattoes, and 97 Negroes, infected with the leprosy, amounting to 125. There were six Whites and five Negroes more, whom we could not visit, for reasons set forth in the verbal process. The remaining persons, which were 131, appeared to us very sound: not that we can answer for the consequences, especially with respect to the children, who are the offspring of leprous persons; whether declared such by us, or dead before the visitation, suspected of infection.

This is the opinion, declaration, and result of the visitation made by us, the physicians and surgeon appointed for that purpose. At Basseterre, the day above-mentioned.

PEYSSONEL.
LEMOINE.
MOULON.

A second visitation was made in October 1748.

VIII. *An*

VIII. *An Account of the late Discoveries of Antiquities at* Herculaneum; *in an Extract of a Letter from* Camillo Paderni, *Keeper of the* Herculanean Museum, *and F. R. S. to* Thomas Hollis, *Esq*; *dated* Naples, Dec. 16, 1756.

Read Feb. 10, 1757.

IT is probable, that the first volume of antique paintings will be published at Easter; in which there will be fifty copper-plates, with observations by the academy lately established here for illustrating the antiquities.

Two volumes of the ancient papyrus have been unrolled. One treats of *rhetoric*, and the other is upon *music*; and both are written by the same author, Philodemus. Il Signor Canonico Mazzocchi, a very learned gentleman of this city, is now translating them from the Greek. There are two persons constantly employed in unrolling other volumes.

In the month of April were found two fine busts of women, the subjects unknown. Also a young stag, of excellent workmanship, upon a base. The height of it, from the feet to the top of the head, is three palms and a half. Likewise its companion; but broken in many pieces; which however I hope to restore.

In May, a small young hog.

In October, a female statue, of middling workmanship. Also a Silenus, a palm and three inches high, standing upon a square base raised upon three

rows of ſteps, which are ſupported at the angles by lions claws. He has a bald head, a long curled beard, a hairy body, and naked feet. The drapery about him is looſe and flowing: the fore-finger of each hand is extended, and all the reſt are cloſed. From his back ariſes a branch above the head, where it divides into two, which, twiſting their foliage round it, fall and ſpread themſelves below the ſhoulders, on each of which a ſtand is placed to fix a lamp. In the middle, betwixt the extremities of theſe two ſmall branches, is a bird reſembling a parroquet. The whole of this figure is in a very good taſte. All theſe things above-mentioned are of bronze.

In November was diſcovered a beautiful marble Terminus, of Greek workmanſhip, as big as the life. It is dreſt in a chlamys; has a young countenance; and the head is covered with a Grecian helmet.

Many other things have alſo been found, as lamps, vaſes, and ſuch-like, in bronze. And we have often met with paintings. If any farther diſcoveries are made, which are remarkable, you may depend on being informed of them.

At preſent my time is much taken up, in a work extremely difficult and tedious; which is this: When the theatre was firſt diſcovered, there were found in it, among other things, ſeveral horſes in bronze, larger than the life; but all of them bruiſed, and broken into many pieces. From this ſad condition they are not yet reſtored. But his majeſty having expreſſed a particular deſire to ſee that effected, if poſſible, with regard to one of them, I reſolved to attempt it; and accordingly have ſet about it.

IX. *An Account of ſome Trees diſcovered under-ground on the Shore at* Mount's-Bay *in* Cornwall: *In a Letter from the Rev. Mr.* William Borlaſe, *F. R. S. to the Rev. Dr.* Lyttelton, *Dean of* Exeter.

Reverend Sir, Ludgvan, Jan 24. 1757.

Read Feb. 10, 1757.

BEING an airing the other day with Mrs. Borlaſe, on the ſands below my houſe, we perceived the ſands betwixt the Mount and Penzance much waſhed into pits, and bare ſtony areas, like a broken cauſey. In one of the latter, Mrs. B. as we paſſed by, thought ſhe ſaw the appearance of a tree; and, upon a review, I found it to be the roots of a tree, branching off from the trunk in all directions. We made as much haſte down to the ſame place in the afternoon as we could, and with proper help to make a farther examination. I meaſured and drew the remains; and about 30 feet to the weſt found the roots of another tree, but without any trunk, tho' diſplayed in the ſame horizontal manner as the firſt. Fifty feet farther to the north we found the body of an oak, three feet in diameter, reclining to the eaſt. We dug about it, and traced it ſix feet deep under the ſurface; but its roots were ſtill deeper than we could purſue them. Within a few feet diſtance was the body of a willow, one foot and a half in diameter, with the bark on; and one piece of a large hazel-branch, with its bark

on. What the two firſt trees were, it was not eaſy to diſtinguiſh, there being not a ſufficiency remaining of the firſt, and nothing but roots of the ſecond, both pierced with the teredo, or augur-worm. Round theſe trees was ſand, about ten inches deep, and then the natural earth, in which theſe trees had formerly flouriſhed. It was a black marſh-earth, in which the leaves of the juncus were intirely preſerved from putrefaction. Theſe trees were 300 yards below full-ſea-mark; and, when the tide is in, have at leaſt 12 feet of water above them: and doubtleſs there are the remains of other trees farther towards the ſouth, which the ſea perpetually covers, and have more than 30 feet water above them. But theſe are ſufficient to confirm the ancient tradition of theſe parts, that St. Michael's mount, now half a mile incloſed with the ſea, when the tide is in, ſtood formerly in a wood. That the wood conſiſted of oak, very large, hazel and willow trees, is beyond diſpute. That there has been a ſubſidence of the ſea-ſhores hereabouts, is hinted in my letter to you, p. 92; and the different levels and tendencies, which we obſerved in the poſitions of the trees we found, afford us ſome material inferences, as to the degree and inequalities of ſuch ſubſidences in general; as the age, in which this ſubſidence happened (near 1000 years ſince at leaſt) may convince us, that when earthquakes happen, it is well for the country, that they are attended with ſubſidences; for then the ground ſettles, and the inflammable matter, which occaſioned the earthquake, has no longer room to ſpread, unite, and recruit its forces, ſo as to create frequent and ſubſequent earthquakes: whereas, where

there

there are earthquakes without proportionable ſubſidences, there the caverns and ducts under-ground remaining open and unchoaked, the ſame cauſe, which occaſioned the firſt, has room to revive and renew its ſtruggles, and to repeat its deſolations or terrors; which is moſt probably the caſe of Liſbon. I am, Sir,

Your moſt affectionate

and obliged humble Servant,

Wm. Borlaſe.

X. *Experiments on applying the Rev. Dr.* Hales's *Method of diſtilling Salt-water to the Steam-Engine. By* Keane Fitzgerald, *Eſq; F. R. S.*

Read Feb. 17. 1757.

ON reading Dr. Hales's account of purifying ſalt water, by blowing ſhowers of air thro', it occurred to me, that ſomething of the kind might be applied with advantage to the ſteam or fire-engine, by increaſing the quantity of ſteam, and conſequently diminiſhing the quantity of fuel otherwiſe neceſſary.

As the ſtrength of ſteam raiſed from boiling water is always in a fluctuating ſtate, and, by the beſt experiments hitherto made, has never been found above $\frac{1}{10}$ ſtronger, or weaker, than air; I was in doubt, whether ſteam, produced by this method, would

would be ſufficiently ſtrong for the purpoſe of the ſteam-engine.

I made an experiment firſt on a ſmall boiler, about 12 inches diameter, made in the ſhape of thoſe commonly uſed in ſteam-engines, with a funnel at the top, of about 1 inch diameter, for the ſteam to paſs thro'; the aperture of which was covered with a thin plate, fixt at one end with a hinge, and a ſmall leaden weight to ſlide on the other, in the nature of a ſteel-yard, to mark the ſtrength or quantity of the ſteam. A tin pipe made for this purpoſe, with ſeveral ſmall holes towards the end, paſſed from a ſmall pair of bellows, through the upper part of the boiler, to within about an inch of the bottom. The boiler was half filled with water, which covered the holes in the pipe about ſix inches. From the beſt obſervation I was capable of making with this machine, by blowing air thro' the boiling water, it produced about $\frac{1}{6}$ more ſteam than was produced by the ſame fire without blowing air thro'.

I then applied a machine of this kind to the engine at the York-buildings water-works, the boiler of which is 15 feet diameter. This is a patent-boiler, a ſection and plan of which is annexed. It has a double concave, with a kind of door-way or paſſage from one to the other, in order to let the flame paſs, as it were, thro' and round the water; by which means there is no-where above nine inches of water to be heated thro', tho' the boiler is ſo large; and which, by three years experience, has been found to require $\frac{1}{4}$ leſs fuel, than any other fire-engine of equal bigneſs.

I fixt

J. Mynde sc.

I fixt a pipe of an inch and a half diameter to a pair of double bellows three feet diameter; which pipe reached about one foot under the ſurface of the water in the boiler, to the end of which are fixed horizontally two branches, each about eight feet long, tapering from one inch diameter to about $\frac{1}{4}$ of an inch. Theſe branches are bent in a circular manner, as in the plan, to anſwer the form of the concave, and are perforated with ſmall holes about four inches diſtant at the thickeſt part, and decreaſing gradually in diſtance, to within $\frac{1}{4}$ of an inch, towards the ſmall end. The reaſon of theſe branches being made taper, and the diſtance between the holes decreaſing to the ſmall end, was in order to give the greater power to the air forced by the bellows to diſcharge the water lodged in ſuch a length of pipe; and I obſerved by this method, that the water was gradually forced thro' the holes to the end of each branch, and ſeemed to throw an equal quantity of air thro' the water.

The length of the pipe, to which the branches are fixed horizontally, is about 18 feet to the noſle of the bellows: notwithſtanding which length, the ſteam, that paſſed thro' the pipe into the bellows, was ſo hot before the water boiled, as to force thro' the leather: but this I eaſily remedied, by fixing a braſs cock of one inch and a half diameter to the pipe, which hindered the ſteam from aſcending, until the engine was ready to work; and being opened, the air continually keeps it cold until the engine has done working; then the cock muſt be ſhut again.

The bellows is worked by means of a ſmall lever, and pullies applied to the great lever of the fire-

engine,

engine, which keeps a continual blaſt whilſt the engine works; the ſtrength of which is increaſed or diminiſhed, by adding or taking off the weights on the bellows.

The effect produced, according to the beſt obſervations I could make, was, firſt, a very viſible alteration for the better in the working of the engine. When the fire was ſtirred, as it muſt be every time fuel is added, the ſteam generally became too fierce, which occaſioned great irregularity, and ſometimes, if not watched, great damage to the engine; and when the fire abated, the ſtroke became immediately much ſhorter, or ſtopped intirely, if fuel was not ſoon added: whereas, by blowing air thus thro' the water, it keeps, with any moderate care, an equal ſtroke to its full length, from the beginning to the end; and by that means diſcharges a conſiderably greater quantity of water. A proof of which was very evident, tho' I could not aſcertain the exact quantity: for the engine, before this improvement, ſupplied but two main pipes at once, which conveyed the water to the houſes ſerved by them; but ſince could not take off the quantity of water thrown up, part of which was obliged to be diſcharged into a third main.

As to the quantity of fuel, that may be ſaved by this method, it is not eaſy to determine from any experiment on this engine, the boiler and fire-place of which is made very different from all others, and the quantity of fuel already thereby greatly leſſened. The fire-place, which may be ſaid to be within the boiler, and is but barely large enough to contain a quantity of the roundeſt and ſtrongeſt burning coals

ſufficient

ſufficient to work the engine, cannot in this be made leſs; and conſequently will not admit ſuch a ſaving from this model, as from one properly conſtructed for the purpoſe: a proof of which I made, by trying ſome coals of a weaker kind, which were alſo cheaper; but on trial were not ſtrong enough to work the engine, and had therefore been laid by. Theſe coals anſwered extremely well; and, as it was a ſlower-burning coal, I found the conſumption, whilſt they laſted, was between two and three buſhels leſs in every ſix hours, which is about the time the engine works each day: and I am ſatisfied, if the perſon, who attends the engine, would take the proper care, more coals could ſtill be ſaved. For at ſeveral different times, when I had the coals exactly meaſured, and marked the time, I conſtantly found, that it required half a buſhel in the hour leſs than he generally uſed, and the engine threw up as much water.

As this method of blowing air thro' boiling water, in order to increaſe the quantity of ſteam for a fire-engine, has, I believe, never before been attempted, and produces already a very good effect, I am in hopes it may be ſtill further improved.

XI. *Extract of a Letter of Mr.* Abraham Trembley, *F. R. S. to* Tho. Birch, *D. D. Secret. R. S. Tranſlated from the* French.

SIR, Hague, 1 Feb. 1757.

Read Feb. 17, 1757.

I Wrote to you on the 26th of November laſt, concerning the earthquake felt ſome time before between the Rhine and the Meuſe. I have been ſince informed by Profeſſor Donati of Turin, that a ſlight ſhock had been perceived there on the 13th of Auguſt 1756, at a quarter after nine in the morning. It was likewiſe felt in other parts of Piedmont. He has alſo communicated to me an extract of a letter of a profeſſor of Genoa, one of his friends, of which the following is a tranſlation.

" On the 9th of November we felt here two " ſhocks of an earthquake; one at 20 $\frac{3}{4}$ hours, accord- " ing to the Italian way of reckoning; the other at " about 4 $\frac{1}{2}$ hours at night. I did not perceive the " firſt, being then walking in the houſe; but I felt " the ſecond. I was then laid down, and going to " ſleep. The direction of the undulations was from " north to ſouth, as far as I could judge."

Monſ. Donati took laſt ſummer, according to his cuſtom, a journey, in order to proſecute his reſearches into natural hiſtory. He was accompanied by Dr. Aſcanius, Fellow of the Royal Society; who was ſtill in doubt about coral's being a compoſition of animals. Monſ. Donati carried him to the ſea of Provence. He ordered coral to be fiſhed up in his

preſence. He placed it in a large veſſel full of water; and carried this veſſel on ſhore; where he ſoon convinced Dr. Aſcanius, by his own eyes, that coral is a maſs of animals of the polype-kind.

Monſ. Donati has written to me, that he has thoroughly ſatisfied himſelf by his laſt obſervations, that the polypes are fixed to their cells; of which he had before doubted. What he ſays afterwards of coral appears to me to expreſs with more truth and preciſion what we ought to think of this kind of animals, than any of the deſcriptions, which have been given ſince the new diſcoveries have changed our ſentiments on that ſubject. Polype-beds, and the cells, which they contain, are commonly ſpoken of as being the work of polypes. They are compared to the honeycomb made by bees. It is more exact to ſay, that coral, and other coralline bodies, have the ſame relation to the polypes united to them, that there is between the ſhell of a ſnail and the ſnail itſelf, or between the bones of an animal, and the animal itſelf. Monſ. Donati's words are as follow. "I am now of opinion, that coral is nothing elſe "than a real animal, which has a very great number "of heads. I conſider the polypes of coral only as "the heads of the animal. This animal has a bone "ramified in the ſhape of a ſhrub. This bone is "covered with a kind of fleſh, which is the fleſh "of the animal. My obſervations have diſcovered "to me ſeveral analogies between the animals of "kinds approaching to this. There are, for inſtance, "keratophyta, which do not differ from coral, ex-"cept in the bone or part, that forms the prop of the "animal. In the coral it is teſtaceous, and in the "keratophyta it is horny."

The obſervations, which I have made upon ſome kinds of polype-beds, lead me to think, that what are called polypes, in thoſe bodies, which are obſerved to come out of and return into the cells, are more than the heads of the animal. I have ſeen ſome, which had a bag, into which paſs'd their food, which I ſaw them ſwallow; and another bag, into which paſſed the groſſeſt part of that food, after it was digeſted. This is the caſe, for inſtance, of the plumed polypes, which I deſcribed at the end of the third memoir, in the work publiſhed by me on one kind of freſh-water polypes.

Monſ. Donati has obſerved divers very curious facts in the journey, which he made into the mountains. He has, in particular, traced out an immenſe bed of marine bodies. This bed croſſes the higheſt mountains, which ſeparate Provence from Piedmont, and loſes itſelf in the plains of Piedmont.

He has likewiſe obſerved a maſs of rock, which forms the extremity of a pretty high mountain, the foot of which is waſhed by the ſea. This rock is, at a conſiderable height, intirely pierced by pholades, that ſpecies of marine ſhell-fiſh ſo well known, which digs cells in the ſtones. It appears from hence, that this rock was ſome time covered by the ſea. According to Monſ. Donati, the ſea has inſenſibly retired from the parts, which were waſhed by it; and he thinks, that there muſt have been a very conſiderable ſpace of time between that and the time, when this mountain, pierced by pholades, was covered by the waters of the ſea. He deduces his opinion from the following fact. There is in this rock, pretty near the ſurface of the ſea, a natural

cavern

cavern fill'd with earth. In this earth have been found ancient Roman ſarcophagi and lamps. It follows from hence, that even in the time of the Romans this part of the rock, in which this cavern is ſituated, was not under water. As there is but a ſmall diſtance between the cavern and the ſurface of the water, it follows, that the water has ſunk but very little ſince the time of the Romans. If it has ſunk in the ſame proportion ſince the time, when it covered the top of the rock, there is no doubt, but that the time, when it was intirely covered by the ſea, muſt have been very diſtant. If the ſame manner of reaſoning be uſed, with reſpect to the bed of marine bodies, mentioned above, which croſſes the mountains, that ſeparate Provence from Piedmont, we ſhall be obliged to preſume, that the time, when thoſe mountains were under the waters of the ſea, was at a very great diſtance from the preſent.

Monſ. Donati concludes from theſe facts, and the conſequences deduced from them, that the Mediterranean ſea is a very ancient, and not a modern one, as Monſ. de Buffon imagines.

Thoſe, who explain all the phænomena of marine bodies found out of the ſea, by an univerſal deluge, do not admit the conſequences drawn by Monſ. Donati from thoſe marine bodies now under conſideration. It is plain, that moſt of the naturaliſts, who have obſerved a great number of theſe marine bodies, are not of opinion, that all thoſe phænomena can be explained by an univerſal duluge. Upon theſe ſubjects, before we undertake to judge, it is proper to be well informed of the nature of marine foſſile bodies, which are found in divers parts, and of their ſituation and arrangement. It is neceſſary likewiſe to be acquainted

quainted with the ſtate of thoſe, which are found actually under the ſea, and the revolutions, to which they are ſubject, while they are covered by it. It is ſtill farther requiſite to have an attention to the revolutions, which have been and are conſtantly obſerved, with reſpect to the ſea-ſhores, which change their ſituation in ſeveral parts, ſome advancing upon the land, and others retiring. If all theſe different facts be compared together, it will not be doubted, but there are actually under the earth marine bodies, which are found there only in conſequence of theſe ſlow revolutions, and not of an univerſal deluge. Perhaps this notion might be extended to the greateſt part of the marine foſſile bodies, which are known to us.

Monſ. Donati informs me, that he would be glad to preſent to the Royal Society an hiſtory of coral, if he thought, that it would be agreeable to them.

XII. *A brief Botanical and Medical Hiſtory of the* Solanum Lethale, Bella-donna, *or* Deadly Nightſhade, *by Mr.* Richard Pultney. *Communicated by Mr.* William Watſon, *F. R. S.*

Read Feb. 17. 1757.

BELLA-DONNA is the name, which the Italians, and particularly the Venetians, apply to this plant; and Mr. Ray (1) obſerves, that it is ſo called becauſe the Italian ladies

(1) Nom. Etymol. ad Calcem. Cat. Cant. p. 43. item Hiſt. Plant. p. 680.

make

make a cosmetic from the juice, or distilled water, which they use to make their complexion fair and white. Others (2) suppose it derives its name from its intoxicating quality. With us it is generally known by the name of Deadly Nightshade, or Dwale, tho' this last term is seldom used for it; and the old French word *Morelle*, which Lobel applies to it, seems to be quite forgotten amongst us.

Classical Distribution.

The Deadly Nightshade was very soon discovered by the revivers of botany after the restoration of learning; and, agreeable to the fashion of those days, it was greatly debated among the commentators, whether it was known, and by what name, to the fathers of botany Theophrastus and Dioscorides. Several of the writers of that time, as Dodonæus, Guilandinus, Fuchsius, and Cordus, were of opinion, that it was the Mandragora morion of Theophrastus; and their sentiments were espoused by his learned commentator Bodæus à Stapel (3), who moreover supposes it the plant, which Dioscorides describes, lib. iv. cap. 69. under the name of Στρύχνος μανικὸς. On the other hand, Matthiolus (4) has taken great pains to prove, that it is not the Mandragora of Theophrastus; and both he and Ruellius (5) are

(2) Bella-donna dicitur quod imaginationes non injucundas efficiat, vel ut honeste satis Plinius, quod lusum generet. Bod. Comment. in Theophr. p. 586. quod in somnis pulchras ostendat virgines feminasque. Ibid. p. 1078.

(3) Locis citatis.

(4) Oper. omnia edit C. B. p. 756.

(5) Ruell. in Dioscor. p. 536.

inclined

inclined to think, that the Bella-donna was not known to either of the Grecian Fathers; who are ſo ſhort, vague, and immethodical, in their deſcriptions, that it is very difficult, not to ſay impoſſible, to apply them to particular ſpecies with juſtneſs and preciſion.

Be this as it will, our reſtorers of botany agreed in general to rank it with the Solana, or Nightſhades; and as moſt of them took it to be the Στρύχνος μανικὸς of Dioſcorides, ſo we find thereto the addition of ſome epithet, expreſſive of its deleterious quality, in moſt of their writings; ſuch as lethale, ſomniferum, furioſum, &c. Its general agreement with the plants of that genus, and alſo the knowlege the world ſoon had of its poiſonous quality, when it is conſidered, that ſyſtematic diſtributions, from the parts of fructification, had not been thought of at that time: theſe, I ſay, were ſufficient reaſons for referring it to the Nightſhades. By ſuch names therefore is it found in moſt of the old writers; till Cluſius, who, obſerving perhaps, that it differed in its parts of fructification from the Solana, adopted the indigenous Italian name, as a generical one, and called it Bella-donna. Cæſalpinus, the firſt inventor of a botanic ſyſtem, did not ſeparate it from the Nightſhades. Moriſon and Ray, the revivers of method almoſt an hundred years afterwards, were aware of the difference; the former having placed it in a chapter among the *Solanis affines*, and the latter conſtituted a diſtinct genus of it, tho' he retained the old name in his hiſtory of plants. Tournefort adopted Cluſius's name *Bella-donna*, and was followed by all the ſyſtematic botaniſts, who have ſince wrote; as Boerhaave, Rivini, Ruppius, Knaut,

Knaut, Magnol, Ludwig, and Haller; until Linnæus, conformable to the 229th rule of the Fundamenta Botanica (6), rejected it, and very expresly calls it Atropa (7); in which he is followed by all succeeding writers, who have chosen his method.

Cæsalpinus, Morison, Ray, Herman, and Boerhaave, who range these plants according to the fruit, place the Deadly Nightshade among the *Herbæ Baciferæ* in their respective systems.

Rivinus, Ludwig, and Christian Knaut, who adopt the number and regularity of the petals in the corolla, for their classical character, refer it to such as have regular monopetalous flowers. Ruppius, whose method is upon the same plan, brings it among the irregular monopetalous ones.

Tournefort's method, which is established upon the figure of the flower, takes it into the first class among such plants as have campaniform or bell-shaped flowers.

Dr. Van Royen, whose system is undoubtedly a very elegant attempt towards the natural method in botany, arranges it among such as he calls Oliganthera; namely, such plants as have the stamina equal to, or fewer in number than, the segments of the corolla.

Dr. Haller, whose method is upon the plan of a natural one also, includes the Bella-donna among the Isostemones, such plants as have the number of the stamina equal to the segments of the corolla.

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(6) Nomina generica quæ ex Græca vel Latina lingua radicem non habent rejicienda sunt.

(7) Atropos una furiarum. Crit. Botan. p. 75.

In the ſexual ſyſtem of Linnæus, at this time ſo generally received, and ſo well eſtabliſhed, it belongs to the Pentandria monogynia, or ſuch plants as have five ſtamina and one ſtyle. The plants of this order are arranged into five ſubdiviſions. The Atropa comes in among thoſe, that have declinated ſtamina. According to this method, we ſhall give its generical characters from the laſt edition of Linnæus's Genera Plantarum.

The moſt obvious and eſſential character of the genus is the *globoſe berry, and open calyx* (8). The general character is as follows.

ATROPA Linn. Gen. Plant. Ed. 5. N°. 222.

The calyx is a gibbous permanent perianthium, formed of a ſingle leaf divided into five acute ſegments.

The corolla is formed of a ſingle bell-ſhaped petal, the tube of which is very ſhort; the limb ventricoſe, of an oval figure, and longer than the calyx. The mouth is ſmall, expanded, and divided into five pretty equal ſegments.

The ſtamina are five ſubulated filaments proceeding from the baſe of the flower, and are of the ſame length: at the baſe they are connivent, and at the top bent outwardly. The antheræ are thick and aſſurgent.

The germen is of a ſemiovated figure: the ſtyle is filiform, of the length of the ſtamina, and inclinated. The ſtigma is capitated, tranſverſely oblong, and aſſurgent.

(8) See Lin. Syſt. Naturæ, edit. Lugd. Bat. 1756. p. 97. N°. 222.

The

The fruit is a globose berry, ſtanding in a large cup, and containing three cells. The receptacle is convex on both ſides, and kidney-ſhaped.

The ſeeds are numerous, and kidney-ſhaped alſo.

The SPECIES.

1. Atropa caule herbaceo, foliis ovatis integris. Linn. Spec. Plant. p. 181.
   Atropa. Linn. Hort. Cliff. 57. Roy. Lugd. 423. Hort. Upſ. 45. Dalib. Pariſ. 70.
   Bella-donna majoribus foliis et floribus. Tourn. Inſt. 77. Boerh. Lugd. II. 69. Miller, plate 62.
   Bella-donna dicta Solanum lethale. Hill. Herb. Britan. p. 328. tab. 47.
   Bella-donna. Cluſ. Pan. p. 503. Bod à Stap. p. 586. Cat. Giſſen. 142. Raii Syn. ed. 3. p. 265. Vaillant. Botan. Par. p. 20. Hall. Helv. 507. Dale Pharmacol. 4° ed. p. 72. Wilſon. Synop. p. 122.
   Solanoc ongener flore campanulato vulgatius, latioribus foliis. Hiſt. Oxon. III. p. 532. ſect. 13. tab. 3. fig. 4.
   Solanum ſomniferum. Fuchſ. 689. Icon. opt.
   Solanum maniacum multis ſive Bella-donna. J. B. III. p. 611.
   Solanum melanoceraſos. C. B. pin. 166.
   Solanum lethale. Ger. 169. emac. 340. Park. 346. Raii Hiſt. Plant. 679.
   Solanum majus ſive Herba Bella-donna. Matthiol. Oper. Omn. p. 756.
   Solanum ſomniferum et lethale. Lobel. Adverſar. p. 102.
   Deadly Nightſhade, or *Dwale*.

2. Atropa caule fruticoſo. Spec. Plant. 182.
Bella-donna fruteſcens rotundifolia Hiſpanica. Tourn. Inſt. 77.
Solanum frutex rotundifolium Hiſpanicum. Barril. Obſ. 2. Icon. 1173.
Round-leaved ſhrubby Spaniſh Bella-donna.

3. Atropa foliis ſinuato-angulatis, calycibus clauſis acutangulis. Spec. Plant. 181.
Bella-donna flore magno violaceo. Hill. Herb. Brit. 329.
Alkekengi amplo flore violaceo. Few. Per. 724. tab. 16.
Large violet-flower'd Bella-donna, or Deadly Night-ſhade.

The firſt of the ſpecies here enumerated is the plant in queſtion. The ſecond has been found growing naturally in no other country than Spain. The third was firſt diſcovered by Father Feuillée in Peru, and is therefore only an inhabitant of the gardens in this part of the world.

The Description.

The root is perennial. It is pretty long, and divided into many branches of a brown colour, ſucculent, and of a diſagreeable ſmell. The radical leaves are frequently a foot long, and five inches broad, of an oval acuminated figure, and not ſinuated on the edges. The ſtalk riſes to three or four feet: it is much divaricated and branched. The cauline leaves ſtand alternately upon it, in ſhape like the radical, of

of a dufky-green colour on the upper part, and a paler green underneath, being a little hairy on both fides. The flowers ftand on fingle footftalks, in the alæ of the leaves: they are large, of a campanulated figure, and ftriated, of a dufky-purple colour within, with a yellow variegated bafe; the outer furface of the flower is of a greenifh red. After the flower fucceeds a fine beautiful large berry, which is black when ripe. For the reft, take in the generical character.

Moft of the old authors give us figures of this plant, which, tho' they convey a general idea of it, are yet fcarce any of them exact. This fault in general runs thro' all, that I have had an opportunity of examining; namely, that the flowers and fruit are reprefented by much too large in proportion to the leaves. Morifon's is perhaps one of the beft among the old figures: it is, upon the whole, tolerable, but not accurate on account of the beforementioned objection. Petiver's does by no means repref ent the plant juftly, in that the alæ of the leaves are not properly filled up. The moft accurate figure of all, that I have feen, is Mr. Miller's, in his plates adapted to the Gardeners Dictionary, which is undoubtedly taken from nature itfelf.

Place *of* Growth.

The Deadly Nightfhade is found in many parts of Europe, efpecially in England and in Auftria; and yet in our own country it is happily not very plentiful, inafmuch as our botanical writers ufually reckon it among the *more rare* plants, and fpecify particularly the places where they have obferved it.

Here

Here in England it is chiefly found in uncultivated places: in church-yards, about old walls, among rubbiſh in ſhady places, about dunghills, in lanes, and ſometimes about woods and hedges. It begins to flower in June, and maintains a ſucceſſion of flowers for two months. The berries are ripe in September and October.

It is of great importance, that the knowlege of poiſonous plants ſhould be extended as much as poſſible, that they may the better be avoided, and their fatal effects thro' miſtake be guarded againſt: there can therefore be no impropriety in enumerating particularly ſome of thoſe places, where our Engliſh botaniſts have obſerved it. Mr. Ray mentions its being found in the church-yard and lanes about Fulburn in Cambridgeſhire, Sutton-Colefield in Warwickſhire: in the Downs: at Cuckſtone, near Rocheſter in Kent, all the yards and backſides are over-run with it. *Ray. Syn.* Upon Clifton-hill, near Nottingham; alſo in a quarry near the cold-bath at Mansfield. *Catal. Notting.* In Currenwood-kins, near Burton in Kendal, and other places in Weſtmorland. *Wilſon's Syn.* Dr. Wilmer found it among the bogs going down to Dorking in Surrey, plentifully. In Preſton church-yard, near Feverſham in Kent. Mr. Watſon found it by the wood-ſide, under the park-wall, between Temsford-mills and Welwyn, Hertfordſhire; and near the road between Rocheſter and Maidſtone. Mr. Blackſtone found it in a ſhady gravel-pit near the old park-wood at Harefield, and in the gardens at More-park near Rickmanſworth, plentifully. *Specim. Botan.* About Rocheſter and Chatham, where it grows in the joints of

of old walls, and in moſt of the unfrequented lanes: alſo in Woodſtock-park in Oxforſhire, and Up-park in Hampſhire. I have obſerved it four or five years ſince on the edge of Charley-foreſt: about Grace-Dieu, Leiceſterſhire. It grows about North Luffenham in Rutland.

Its Poisonous Quality.

There have been many fatal inſtances of the narcotic and deleterious effects of the berries of this plant. They are upon record in almoſt all botanical, and many medical authors. Children have unhappily been the principal ſufferers this way, being tempted to eat by the enticing aſpect of the berries, or by miſtaking them for ſome other fruit. The berries, however, are not the only part of the plant, which partake of this intoxicating and poiſonous property: the whole plant is endued with it, and that in no ſmall degree.

If the Bella-donna is allowed to be the Στρύχνος μανικὸς of Dioſcorides (9), this quality of it was not unknown to that writer. It was very ſoon known to the firſt writers in the medical and botanic way after the reſtoration of letters; and they have not failed to inform us of it.

Tragus and Fuchſius, who wrote about the middle of the ſixteenth century, both relate inſtances of the poiſonous effects of theſe berries: the former, of a man, who went mad after having eaten of them;

(9) Mat. Med. lib. iv. cap. 69.

the latter, of two children, who perifhed by the fame means (10).

Lobel (11) tells us, that the berries of this plant are prefent death; and informs us of fome youths, who, after eating them, became ftupified, and died as from an over-dofe of opium.

Matthiolus (12) relates, from his own knowlege, of fome children poifoned by the fame means.

Among all the inftances of the intoxicating nature of this plant, there is none more memorable than that mentioned by the Scotch hiftorian Buchanan (13), of the deftruction of the army of Sweno; which is quoted by almoft all authors, who have wrote upon this plant. It is there faid, that the Scots mixed a quantity of the juice of thefe berries with the drink, which, by their truce, they were to fupply the Danes with; which fo intoxicated them, that the Scots killed the greateft part of them while they were afleep. How far this anecdote is to be depended upon, or whether other concurrent circumftances ought not to be taken into the account, I cannot determine.

Our own herbalift Gerard (14) mentions the cafe of three boys in the Ifle of Ely, who, having eaten of thefe berries, two of them died in lefs than eight hours; but the third, by drinking plentifully of honey and water, and vomiting after it, recovered.

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(10) See Sennert. lib. vi. par. 7. cap. 9.
(11) Stirpium Adverfar. p. 103.
(12) Oper. Omn. p. 754.
(13) Rerum Scoticar. lib. vii.
(14) Ger. em. p. 341.

Bodæus

Bodæus à Stapel, in his comment upon Theophraſtus (15), tells us of two youths, that eat two or three of theſe berries, which they got in the Leyden garden, miſtaking them for black currants: one of them periſhed, and the other recovered with great difficulty.

Simon Pauli relates two or three examples to the ſame effect (16). Wepfer gives us a circumſtantial account of a child about ten years old, who was thrown into a great variety of convulſive ſymptoms after eating of this fruit: but proper care being taken by vomiting, and afterwards giving alexipharmics and anti-epileptic medicines, he recovered (17).

M. Boulduc (18) laid before the Royal Academy of Sciences at Paris, the caſe of ſome children, who, upon eating theſe berries, were ſeized with a violent fever, palpitations of the heart, convulſions, and loſt their ſenſes. One of them, a little boy of four years old, died the next morning.

Boerhaave has inſtances to the ſame effect (19): and it was the misfortune of Dr. Abraham Munting, a noted botaniſt and profeſſor of phyſic in the univerſity of Groningen, to have his own daughter poiſoned with the berries of the Bella-donna.

It would be almoſt endleſs to recite all the inſtances to be met with upon this head. The German Ephemerides, the Commercium Literarium, and other periodical works, furniſh us with farther proofs

(15) Page 586.
(16) Quadripart. Botan. p. 488.
(17) Cicut. Aquat. Hiſtoria et Noxæ. Baſil. 1716. p. 228.
(18) Hiſtoire de l'Academie Royale. 1703.
(19) Hiſt. Plant. Lugd. Bat. Hort. p. 510.

of the deadly quality of the Bella-donna; and they are unhappily corroborated by more recent inſtances in modern authors. The Gentleman's Magazine (20), Mr. Miller in his Gardeners Dictionary, and Dr. Hill in his Britiſh Herbal (21), exhibit to us ſeveral melancholy caſes of this kind.

The effects of this plant have been ſo extraordinary, that ſeveral diſtinct treatiſes have been publiſhed profeſſedly upon it. The moſt remarkable of theſe is that of J. M. Faber's, printed at Augſburg in 1677, under the following title; *Strychnomania explicans Strychni manici antiquorum, vel Solani furioſi recentiorum hiſtoriam.* In this tract the author has collected a number of caſes from various hands, concerning the poiſonous quality of the plant in queſtion. In the year 1724. C. Sicelius publiſhed a treatiſe upon this plant, under the title of *Diatribe de Bella-donna. Jenæ.* 8vo.

Medical History.

Who it was, that was bold enough to venture firſt upon the internal uſe of this plant as a medicine, I cannot ſay; chance very probably led to it, as in many other caſes. In the mean time, there is reaſon to believe, that it is not altogether a modern practice. One would be led to think, by the accounts given us in Matthiolus and Bodæus, that in their days its operation was very well known; and that they knew how to doſe it very exactly, ſince they give us an

(20) For Auguſt and September 1747, and for Sept. 1748.
(21) Page 329.

account

account of tricks being played with it, by infuſing the quantity of a ſcruple of the root in wine, and intoxicating people therewith. The former of theſe authors relates, that the diſtilled water from this plant, in a doſe of about two or three ſpoonfuls, was exhibited by ſome people in inflammations of the viſcera; and, he obſerves, with good ſucceſs. Parkinſon ſeems to have tranſcribed this account, reſpecting this uſe of it; but neither of them ſpeak of it from their own knowlege. It may be queſtioned, however, whether this could act otherwiſe than as mere water; ſince the principles with which this plant is endued, do not ſeem capable (if one may judge from its ſenſible qualities and effects upon thoſe who have taken it) of riſing in a ſtill.

Mr. Ray (22), from the German Ephemerides, an. 13. obſ. 64. preſents us with the relation of a ſhepherd in Denmark, who adminiſtered an infuſion of the berries in wine in the dyſentery, which was there very common, and very obſtinate; adding, that it was attended with great ſucceſs, not only reſtraining the flux, but carrying off the diſorder by ſweat. Mr. Ray obſerves further, that, correſpondent with this practice, Conrade Geſner actually prepared a ſyrop from the berries, and gave it in dyſenteric caſes with great ſucceſs. This account is found in Geſner's Epiſtles, and is quoted alſo by Dr. Haller, (23) when treating of this plant. Poſſibly its efficacy in theſe caſes may be accounted for, from conſidering it merely in the quality of an opiate; and

(22) Raii Hiſt. Plant. I. p. 681.
(23) Enumerat. Stirp. Helvet. p. 507.

therefore it cannot be adviſeable to uſe it, when ſafer medicines are always at hand.

Its external uſe ſeems to be of as long a date as its internal; and it was on account of its cooling and repellent quality, that it came into credit as a fucus among the Italian ladies. Matthiolus recommends it in the eryſipelas, the ſhingles, and other inflammatory diſorders of the ſkin. The leaves, applied in the form of a cataplaſm, are much celebrated by many writers, as of great uſe in reſolving tumors, particularly of the breaſt, and even ſuch as are of a ſchirrous and cancerous nature. Many of the old authors (24) mention this application of it, among other of the cooling and narcotic herbs; ſuch as the common nightſhade, henbane, hounds-tongue, *&c.* which it was uſual to apply on ſuch occaſions. Mr. Ray informs us, that Mr. Percival Willughby experienced its efficacy repeatedly, in diſcuſſing hardneſſes and cancerous tumors in the breaſt.

Its relaxing quality is very ſurpriſing, as appears by that memorable caſe related by the laſt-mentioned author, of a lady's applying a leaf of it to a little ulcer, ſuſpected to be of the cancerous kind, a little below her eye, which rendered the pupil ſo paralytic, that it loſt all its motion for ſome time afterwards: and that this event was really owing to that application, appears from the experiment's being repeated with the ſame effect three times.

The German phyſicians have gone much further: they have even ventured to give it inwardly in cancerous caſes. Dr. Haller, when treating of the qua-

(24) See Foreſtus, Etmuller, and the old chirurgical writers.

lity

lity of this plant, refers to Junker, and others of the modern phyſicians, as recommending the decoction of it with caution, that it be not given in ſuch quantity as to cauſe ſleep. So long ſince as the year 1739. there was a theſis publiſhed at Hall, by Michael Albert, in which the Bella-donna is propoſed as a ſpecific in cancerous caſes. What other phyſicians patronize this uſe of it, I cannot ſay, having but little opportunity of conſulting thoſe academic pieces, which are of ſuch eminent uſe in compilations of this kind. Thus much is certain, that its uſe, in ſuch caſes, rather gains ground; and the caſe, publiſhed in the French Bibliotheque (25), printed at the Hague, of an ulcerated cancer being radically cured by an infuſion of the leaves of this plant in water, deſerves particular attention, on account of its being ſo well atteſted. The caſe is extracted from an inaugural theſis of Profeſſor Lambergen's, who was the phyſician concerned (26). The event was ſo ſingularly happy and ſucceſsful in this inſtance, that we hope it will need no apology, if we give a particular detail of it.

The perſon afflicted with this miſerable diſeaſe was a widow of 34 years of age, and mother of four children. She had but weak nerves, and had been ſubject to inflammatory diſorders. She informed M. Lambergen, upon examining her, that ſhe had had a quinzy ſix times, which had twice ended in ſuppuration: that eight years before her right breaſt

(25) Bibliotheque des Sciences et des beaux Arts pour les mois. Jan. Fevr. Mars. 1755.

(26) Tiberii Lambergen Lectio inauguralis, ſiſtens Ephemeriden perſanati Carcinomatis. Groning. 1754.

had

had ſuppurated, and diſcharged much matter: that two years after it ſuppurated again; and that at the end of another year both breaſts underwent the ſame fate; ſince when the right had remained ſchirrous, but was without pain, except when ſhe handled it. She had ſuckled her youngeſt child about ſix months, when ſhe was ſeized with a fever; and the left breaſt (with which only ſhe could ſuckle ſince the other had ſuppurated) ſoon ſwelled, inflamed greatly, was very painful, and ſoon became almoſt as large as a child's head. Dr. Lambergen being called in, ordered copious bleeding, and that the child ſhould ſuck as little as poſſible. She took ſome medicines, and ſoon recovered.

A year paſſed after this without any bad accident; when the lunar evacuations, which ſhe had had from her 18th year, beginning to diminiſh, ſhe felt a pricking pain in her left breaſt, and her right began to ſwell. Upon a fright, ſhe had a fall, which accident increaſed both the pain and ſwelling; and ſhe had recourſe again to Dr. Lambergen.

He found the tumors in her right breaſt much enlarged, and ſo connected together, as to feel like one large one only. On the upper part of the breaſt, upon the pectoral muſcle, it felt rugged, unequal, and almoſt as hard as a ſtone. The patient complained of a conſtant itching in the part, and at times a pungent pain, which ſeemed to ſhoot from the armpit, and end in the tumor. Under this armpit the glands were hard and ſchirrous; and the left breaſt was not exempt from the like indurations. A vein or two on the right breaſt was a little enlarged, otherwiſe no alteration. It was no hotter than common;

mon; nor had it undergone any change of colour. To mitigate the pain of the ſchirrous, Dr. Lambergen ordered the following plaiſter:

℞ *Ung. Diapomphol.* ℥ ij. *Amalgam. merc. et Plumb.* ʒ iij. *Sperm. Cet.* ʒ j. *M.*

With this external application he preſcribed likewiſe the following powders, to be taken night and morning, and gave directions relating to the non-naturals.

℞ *Coral. rub. Antimon. Diaphoret. illot. Sper. Ceti a* ʒ ij. *Laud. gr.* vj. *M.* for 12 doſes.

Under this method the pain remitted, but the tumor inlarged, and a little riſing was obſerved on the upper part of it; and towards the nipple, where there was the leaſt hardneſs, a ſmall ſpot was perceived, which, at the next return of the catamenia, inflamed, and became the ſeat of the moſt excruciating pain. Dr. Lambergen, during this period, in the room of the powders, ſubſtituted emmenagogic pills, and ordered the pediluvium. She loſt ten ounces of blood from the foot: and by theſe means the ſwelling of the breaſt diminiſhed, and the patient ſuffered very little for ſome days. This truce, however, was but temporary: the riſing on the upper part of the tumor began to inflame, itched intolerably, the pain returned, was almoſt perpetual, and inſupportably pungent.

In this dreadful ſtate was the patient, when Dr. Lambergen deſired the late Dr. du Bois, Dr. Winter, phyſician to the houſe of Orange and profeſſor at Leyden, together with Dr. Van Arum of Leewarden, phyſician in ordinary to the Princeſs dowager, to viſit her. Theſe gentlemen examined her many times, and unanimouſly agreed, that it was now no leſs than

a confirmed cancer. It was Profeſſor Winter, who acquainted Dr. Lambergen, that he had heard M. Degner, a celebrated phyſician at Nimeguen, ſpeak of the Bella-donna, as a ſovereign remedy againſt inveterate ſchirri; adding, nevertheleſs, that he had never tried it himſelf.

In ſuch a caſe as this, where death ſeemed inevitable, a dangerous remedy is to be preferred to none at all. Dr. Lambergen therefore determined to try it upon his patient; but, knowing the character, which the plant bore, he reſolved to try the effects of it upon himſelf firſt. To this end, he poured ten tea-cups of water upon a ſcruple of the leaves, which had been gathered and dried three years: he let it ſtand all night lukewarm. Of this infuſion he took half a tea-cup full, being the twentieth part of the whole, in the morning faſting; but perceived no effect from it. This determined him the next morning to double the doſe; which produced a ſlight vertigo, and for an hour or two an uncommon dryneſs in his mouth. Being thus prepared, as he knew his patient had but a weak nervous ſyſtem, he determined to begin with caution.

It was the 14th day of January 1745, that ſhe took the firſt doſe, being one tea-cup full. It had the ſame effect upon her, as it had had on her phyſician; and moreover rendered her pulſe weaker and quicker than uſual. For ſeven mornings ſucceſſively ſhe took the ſame doſe, which, in general, produced the ſame effect. At the ſame time the plaſter was renewed, with the addition of a few grains of opium. Under this method her pain was mitigated; but, before the latter end of the week, returned again

more

more frequently, and more acute; ſo that ſhe was reduced to a moſt deplorable condition. The riſing on the upper part of the breaſt became livid; the place near the nipple before-mentioned inflamed, and was very painful; and two little pointed riſings were obſerved upon it, together with a ſlight fiſſure or opening. As the menſtrual period was approaching, the infuſion and the powders were omitted, and the pediluvium ſubſtituted. A mixture with crabs-eyes, ſp^{t}. nitri. d. and ſyr. e mecon. relieved the patient from ſome ſpaſmodic complaints ſhe had at this time, and the menſes returned more copiouſly than ever. The 27th ſhe took ſomething more than a tea-cup of the infuſion, being the firſt doſe of the ſecond ſcruple: her body was ſoluble; her breaſt leſs ſwelled, but the pain returned very acute, and ſeemed to terminate in the little callous eminence on the upper part of the breaſt, which now likewiſe became more pointed. The 28th ſhe took the ſame quantity of the infuſion. The two little pointed places near the nipple were now become two little holes, but had not diſcharged any matter. The other ſore on the upper part of the breaſt was more livid ſtill, and more painful, and had riſen into two little whitiſh points. The powders were omitted this night, as they had been now and then at other times. The 29th, very little ſleep the foregoing night, great pain from the upper ſore, the holes near the nipple were become larger, and had run a yellowiſh matter. The ſame doſe of the infuſion as before. At night ſhe had moſt acute pain from the upper ſore. The 30th both ſores were nearly in the ſame ſtate. All remedies were

this day laid aſide, except the infuſion; of which ſhe took a cup-full and an half; but her mouth ſoon became ſo dry, that ſhe could ſcarcely ſwallow a little tea; and the vertigo was ſo violent, that ſhe ſtaggered: her ſight was ſo weakened, that ſhe could ſcarcely read. Notwithſtanding this, ſhe had no anxiety, nor nauſea, nor pain; but her appetite was leſs, and her pulſe quicker. The 31ſt all the laſt-mentioned ſymptoms continued the ſame: the two pointed eminences on the upper part of the breaſt were become two little holes likewiſe, and had diſcharged a few drops of good matter.

The 1ſt of February the upper ſore had diſcharged but little matter, and that thinner than before. That near the nipple was become more livid, and the two holes were larger; but there had been no diſcharge for ſeveral days. The pulſe and appetite were good, ſleep natural, the body open. The pain, indeed, was continual, but leſs acute. The 2d, little alteration. The ſores diſcharged but little. From this time they were dreſſed twice in a day with *Nutritum*, and over all the *Emp. Saturnin.* The pain was not ſo violent, but was felt in another place, which began to ſwell. From the 6th to the 28th better and worſe: the pain more or leſs acute; and the catamenia paſſed without any bad effects. At the end of this month the ſchirrus all over the breaſt was much ſoftened, and ſenſibly diminiſhed. This was the opinion of Dr. du Bois and Dr. Winter, as well as of Dr. Lambergen.

The 1ſt of March an inflammation aroſe on the ſole of the right foot, and extended up the leg about four fingers above the heel. It ended in two great

bliſters,

blifters, as if from a burn, which were embrocated with wine and oil, and nothing bad followed. To the 22d, the patient was better and worfe. The menftrual period did not pafs without fome difturbance. The 26th fhe began with the infufion of the tenth fcruple, and every thing went on for the better to the end of the month.

From the 1ft of April to the 6th the ulcers were firm and dry; but the pain in the breaft increafed. Several blifters arofe on the foot, along the leg, and even upon the thigh, on the left fide. One upon the fole of the left foot, for 24 hours difcharged an incredible quantity of thick whitifh lymph. The pain from thefe blifters was beyond all fhe had felt before. It continued the 7th, 8th, and 9th; and new blifters arofe on the thigh. The excoriated parts were all dreffed with fpirit of wine. During the difturbances from thefe new complaints, the breaft was likewife painful, and fwelled, tho' the ulcer near the nipple was dry, and the other difcharged little or nothing. The 10th fhe had lefs pain both in her foot and in her breaft. The upper fore was clofed; the foot difcharged lefs. From the 11th to the 15th, notwithftanding the weather was very cold, her pain ftill leffened. The ulcer remained firm, and the whole breaft was foftened: her foot mended; and all went on for the better till the 18th, when the ulcer on the upper part of the breaft opened again in three places, and difcharged a thick yellowifh matter. The nipple of the left breaft alfo became inflamed, and furrounded with pimples, which difcharged a little lymph. In the mean time the cancerous breaft was more painful than on the preceding days. The

19th the pain leſs, tho' continual. Some diſcharge from the foot ſtill; but the ulcers on the thigh were healed, and another bliſter aroſe. The 20th the upper ſore on the breaſt cloſed again; but that near the nipple ſeemed to threaten another opening, and in fact it did, on the 24th, in three places. On the 20th the catamenia returned very copiouſly, and ſuperſeded the uſe of the pediluvium. The 25th ſhe began with the infuſion of the fourteenth ſcruple of the Bella donna, which, it is to be obſerved, was ſcarcely ever omitted. The 26th a bliſter aroſe at the end of the fore-finger on the left hand, was very painful, and diſcharged a great quantity of ſerous matter. The next day both ulcers on the breaſt diſcharged a ſmall quantity of lymph; otherwiſe the breaſt was leſs painful.

From the 28th of April to the 7th of May every thing went on for the better: the cancerous breaſt was almoſt without pain. The ulcers ran very little; but the excoriation and pain were much worſe from the nipple of the left breaſt, which alſo diſcharged a great quantity of lymph. The 8th the upper ulcer on the right breaſt cloſed; but the other opened again. From the 8th to the 16th no change for the worſe: on the contrary, the left breaſt was well; the right leſs painful, and diſcharged but very little. From the 18th to the 22d the menſtrual period: all things on the mending hand; the ulcer healed, and the patient had little or no pain: but, from the 23d to the 27th, the pain returned ſomething worſe, and there was ſome diſcharge from the breaſt.

The *Nutritum* was now diſcontinued, as too emollient. The 27th the infuſion from the eighteenth ſcruple of the Bella-donna was begun with.

From

From the 28th of May to the 12th of June the breaſt ſtill painful: in the mean time, however, the ulcers remained firm and dry. The tumor and ſchirroſity of the breaſt diminiſhed in ſuch a manner, that, excepting its being a little bigger than the other, it had intirely reſumed its natural form and colour. No indurations in the left breaſt, nor of the glands in the right armpit.

The 13th of June ſhe took a journey, was abſent ſome weeks, and returned in perfect health. Dr. Lambergen adviſed her, but in vain, to continue the infuſion. Nevertheleſs, ſhe was obliged now and then, when ſhe felt pain, to have recourſe to it, and was always relieved by it: and in the courſe of another year the remains of the ſchirrus were totally waſted.

It is now (1754) eight years ſince, and ſhe has had no relapſe, no pain, no hardneſs in her breaſt; has married a ſecond huſband, by whom ſhe has had a child, which ſhe ſuckled. What more can be requiſite to aſcertain a cure?

Thus we have given a detail of this memorable caſe; wherein we ſee, that ſix drachms of one of the moſt poiſonous vegetables that the world produces actually cured a woman, whom the moſt able phyſicians had given up as incurable; and who muſt otherwiſe have finiſhed her miſerable days in the moſt deplorable ſufferings.

It muſt not be omitted, that notwithſtanding the daily uſe this woman made of the Bella-donna, ſhe was not accuſtomed to it in the manner as people are who take opium. Dr. Lambergen always prepared the infuſion himſelf, and never had occaſion to make

it ſtronger than at the firſt, as the patient always found the like effects from the ſame doſe.

So ſingular and happy an event, as attended Dr. Lambergen's adminiſtration of this plant, certainly merits the attention of the medical profeſſion; and ſurely, one may add, entitles the medicine to future trials. And as the authenticity of the caſe will not be diſputed, it is therefore greatly to be wiſhed, that thoſe gentlemen, who belong to the public hoſpitals, and others that have frequent opportunities of attending patients labouring under this deplorable diſeaſe, would give it a further trial. A cancer, even in its latent, but much more in an ulcerated ſtate, is allowedly one of the moſt terrible and formidable diſorders to which human nature is liable; and hath long been ranged, very juſtly, among the *opprobria medicorum*, inſtances of a radical cure being rarely met with: indeed, one of the firſt phyſicians (23) of our age tells us, that it is not known to have been cured at all, but by a total extirpation of the part; and all, who are converſant in phyſic and ſurgery, know very well, that that operation is frequently no ſecurity againſt its return.

I have here endeavoured, in as conciſe a manner as might be, to exhibit the hiſtory of this extraordinary plant. The being able barely to know and diſtinguiſh one plant from another, however praiſeworthy in itſelf, ought not to be the only view of our botanical reſearches: we ſhould do more, and

(23) Dr. Van Swieten Comment. in Aphor. Boerh. ſect. 492.

endeavour

endeavour to inveſtigate, in the moſt attentive manner, the properties of vegetable productions, in order to accomodate them to the various exigencies of human life.

Several claſſes of vegetables, from their merely herbaceous taſte, and, as far as we can conjecture, from their other ſenſible qualities, ſeem to be formed by the great Author of nature principally for the nouriſhment of animals: but thoſe plants, which are endued with principles ſo highly active, as, when taken in ſmall quantities, to be able to put an end to animal life; ſuch deſerve to be more minutely inquired into, as under certain circumſtances theſe principles, properly directed, may conduce to great and good ends. We ſhould endeavour, therefore, diligently to inform ourſelves, in what quantities, and under what circumſtances, the poiſon ends, and where the medicine begins. In this reſpect we have certainly a notable inſtance in the hiſtory before us in Profeſſor Lambergen; whoſe induſtry, more eſpecially as it was attended with ſucceſs, merits our greateſt acknowlegements; inaſmuch as he has informed us, with no ſmall degree of accuracy and preciſion, that the plant under conſideration, which is well known to be of a highly deleterious nature, and that even in a ſmall quantity, may be ſo managed, as to be productive of good effects, not to be found poſſibly by any other means.

Some of the moſt efficacious medicines are ſuch, as, being poſſeſſed of highly active principles, do greatly diſturb the animal œconomy in their operation: nevertheleſs, however rough the *modus operandi* of any medicine be, if its efficacy by repeated

trials

trials be approved and confirmed, this is ſo far from proving a diſcouragement to its uſe, that we ought to regard the diſcovery of ſuch a one as a valuable acquiſition to the province of phyſic, eſpecially if it is applicable in deſperate and obſtinate caſes. The Bella-donna, on the contrary, ſuppoſing future trials ſhould prove it as happily ſucceſsful as Profeſſor Lambergen has experienced it, is a medicine of a different kind; inaſmuch as its operation is mild, when compared with that, which attends the exhibition of many others: we ſhould therefore have double reaſon to rejoice at the diſcovery.

XIII. *An Account of ſome of the Antiquities diſcovered at* Herculaneum, &c. *In a Letter to* Thomas Birch, *D. D. Secret. R. S. By* John Nixon, *A. M. F. R. S.*

Reverend Sir,

Read Feb. 24, 1757.

THE ſubject of this letter are ſome curſory obſervations made by me laſt ſpring, upon viewing the curioſities found at Herculaneum, and the places adjacent. I deferred putting them into any order, till I came to town, and had ſeen, by peruſing the Tranſactions of the Royal Society, whether ſome abler hand had not already prevented me, and made any further communication needleſs: but as I now find, that no notice has been hitherto taken of ſeveral particulars, which, in my humble

humble opinion, deſerved it, as tending to throw new light upon antiquity; I beg leave to trouble you with my thoughts upon them.

I ſhall begin with the muſeum in the King of the Two Sicilies' palace at Portici; wherein, amongſt a great number of other ancient and valuable remains, are theſe that follow, *viz.*

I.

Several *tali luſorii.* The *tali* are ſuppoſed to have been known to the Greeks (1) by the name of Ἀςράγαλοι as early as the Trojan war. But as the monuments before us are undoubtedly Roman, I ſhall confine my remarks upon them to the uſages received among that people; and being guided partly by what appears upon the face of theſe antiquities, and partly by what the Latin claſſics have delivered in general upon this ſubject, beg leave to obſerve, in the firſt place, that the *tali* had each of them but four ſides, two broader, and the other two more narrow, on which they would ordinarily reſt; as the rounding of their ends did not eaſily permit them to ſtand upon thoſe parts. However, the poſſibility of ſuch a poſition (tho' it did not occur to me to make the experiment with theſe pieces) may be deduced from a paſſage in Tully (2).

Further, with regard to the manner of diſtinguiſhing the ſeveral ſides of the *tali*, ſome learned (3) writers

(1) Hom. Iiad. 23. *v.* 88.

(2) *Ut enim —— ſi hoc fingamus, eſſe quaſi finem —— ita jacere talum, ut rectus aſſiſtat; qui ita talus erit jactus, ut cadat rectus* —— Cic. de Fin. L. 3. § 16. Ed. Verb.

(3) Vid. Dacier not. on Hor. L. ii. Od. 7. *v.* 25, &c.

ſpeak of it according to ideas taken from the faſhion of marking the modern dice, and (I may add) the ancient *tyſſeræ* likewiſe: but, as I did not obſerve the traces of any engraving, painting, *&c.* upon the pieces under conſideration, it ſeems to me more probable, what others aſſert (4), that this diſtinction was effected by the different configuration of the ſides themſelves, and not by any numbers marked upon them. And concerning this notation, the common opinion is, that the appearances expreſſing *one* and *ſix*, as alſo thoſe repreſenting *three* and *four*, were oppoſed to each other reſpectively.

But leaving theſe (however probable) conjectures, we can with certainty determine the number of the *tali* uſed in this game to have been four; and likewiſe, that among the various chances reſulting from them, the moſt fortunate one was that, wherein each of the ſides exhibited a different aſpect. The former of theſe circumſtances we learn from Tully (5), as we do the latter from Martial, who, in a diſtich ſent with a preſent of a ſet of *tali* to a friend, ſays,

Cum ſteterit nullus vultu tibi talus eodem,
Munera me dices magna dediſſe tibi (6).

It may further be collected from Horace, that the throw above deſcribed had the appellation of *Venus:* for when he intimates, that the preſident of the feaſt was elected by the *tali* (7), he muſt be ſuppoſed to

(4) Τὸ δὲ σῆμα τῶ κατὰ τὸν Ἀστράγαλον Πτώματος Ἀείθμυ Δόξαν εἶχεν. Jul. Pollux. L. ix. c. 7.

(5) *Quatuor tali jacti caſu venereum efficiunt.* Cic. de Div. L. i. §. 13. Ed. Verb.

(6) Mart. L. xiv. epig. 14.

(7) *Nec regna vini ſortiere talis.* L. i. od. 4. v. 18.

mean

mean the moſt favourable chance upon them. But he (8) elſewhere gives us to underſtand, that the chance, which determined that election, was called *Venus*.

Propertius is ſomewhat more explicite in aſſigning the title of this throw, as above; and at the ſame time informs us further, that the contrary (and conſequently moſt unlucky) one was termed *canes*.

Me quoque per talos Venerem *quærente ſecundos,*
Semper damnoſi ſubſiluere canes (9).

Now it ſeems to be agreed among the antiquaries, (10) that *canis* on the *tali* was unity: and indeed this opinion is countenanced by Perſius (11), who contraſts *canicula* with *ſenio*. If this be admitted, then the *canes* of Propertius muſt have been the chance, wherein all (or at leaſt the greater number of) the *tali* came up (as we ſhould expreſs it) aces.

There have been ſeveral other conjectures propoſed by learned writers upon this ſubject, which I chooſe to omit, for want of proper authorities from the claſſics to aſcertain them. This is likewiſe the caſe with regard to the rules obſerved by the ancient Romans at this diverſion. It is not at all improbable, that as we have ſeveral ſpecies of games upon the ſame ſet of dice, cards, *&c.* ſo they might have the ſame

(8) *Quem Venus arbitrum dicet bibendi?* L. ii. od. 7. *v.* 25. Some think, that this caſt was alſo named *baſilicus* from the uſage here mentioned. Sanad. in Loc.

(9) Prop. L. iv. el. 9. *v.* 18.

(10) Jul. Pollux. L. ix. c. 7. Lubin. on Perſ. ſat. 3. *v.* 49, &c.

(11) —— *Quid dexter* ſenio *ferret,*
——— *Damnoſa* canicula *quantum*
Raderet ——— Perſ. ſat. 3. *v.* 48.

variety on the *tali*: and if there were any laws eſtabliſhed by cuſtom for the regulation of this game in public, yet private parties might be at liberty to innovate at pleaſure, and agree upon whatever terms of play were moſt agreeable to their inclinations or circumſtances. In this light (according to (12) Eraſmus) we are to conſider the account, which Auguſtus gives of himſelf and his friends, in an epiſtle to Tiberius (13): *Inter cœnam luſimus* γεροντικῶς *heri et hodie: talis enim jactatis, ut quiſque canem aut ſenionem miſerat, in ſingulos talos ſingulos denarios conferebat, quos tollebat univerſos, qui Venerem jecerat.* And it is obvious to remark, that (upon this hypotheſis) the critics, perhaps, need not have been ſo much embaraſſed (as we find they have been) in endeavouring to reconcile this paſſage of Suetonius with that other of Perſius (14) produced above.

I ſhall conclude with noting, that in order to prevent any fraud or ſlight of hand in managing the *tali*, it was uſual to put them into a box (15), and, after ſhaking them together, to throw them out upon a table. Thus Martial introduces one of theſe *turriculæ*, as recommending its own uſefulneſs for the purpoſe above-mentioned:

Quærit compoſitos manus improba mittere talos,
Qui per me mittit, nil niſi vota facit (16).

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(12) Dial. Ἀσεργαλισμός.
(13) Sueton. C. Aug. §. 71.
(14) Sat. 3. *v.* 48. See Prat. not. in uſ. Delph. in loc.
(15) Hor. L. ii. ſat. 7. *v.* 17.
(16) Mart. L. xiv. epig. 16.

However,

However, this caution does not ſeem to have been ſo univerſally obſerved, but that ſometimes, *viz.* when the party conſiſted of ladies, it was (I preſume, for a reaſon greatly to their honour) ſuperſeded. Thus, in one of the firſt paintings found at Herculaneum, and now in the royal apartments at Portici, we ſee a young female figure exhibited, as playing at this game, with one or more of the *tali* lying upon the back part of her hand, while the reſt appear as having fallen off from thence towards the floor.

II.

A rule with four joints, each of which contained about 5 inches 9-tenths of our meaſure. I think there was another in two parts, which anſwered to the ſame proportion.

III.

A weight, inſcribed on one ſide EME, and on the other HABEBIS.

IV.

A ſmall *bolla d'oro*, which (after that in the late Dr. Middleton's collection, and another preſerved at Rome) is the third known to be extant in Europe. As this ornament was worn by ſo great a number of young perſons at Rome, and made of gold, which is ſo capable of reſiſting the injuries of the weather, moiſture, *&c.* one cannot but wonder at the extreme ſcarcity of theſe monuments in the cabinets of the curious. The moſt probable way of accounting for this (according to (17) Dr. Middleton) is, that the

(17) Germ. Ant. Mon. p. 38.

value

value of the materials, of which theſe *bullæ* were made, induced the poor labourers, as ſoon as they had found one to ſell it to the firſt goldſmith they met with for its real value (however ſmall it might be) by weight.

V.

A little figure like a Faunus, excepting that about the head it had ſomething of the character of the minotaur, *viz.* large curls upon the forehead, and ſeveral muſcular protuberances, or *tori*, under the throat.

VI.

A figure in relievo of a man ſitting with a bowl in his hand, which has been thought a Socrates. And indeed the features of the face bear a ſtriking reſemblance to thoſe of that ſage expreſſed in ancient monuments; as the bowl might properly refer to the well-known circumſtance of his death. But the other inſignia are not ſo ſuitable to the character of the ſubject, as one could wiſh: for he holds, partly in his hand, and partly under his arm, a ſhort ſtaff full of knots, and curved at the end like a ſhepherd's crook, ſuch as we find borne by ſatyrs in ſome Bacchanalian pieces: and the ſkin of a beaſt appears hanging from the ſeat of his chair.

VII.

An antique painting of a muſe, with a *capſula* near her containing ſome volumes, from which hang labels ſhewing the titles of the works. The ſame repreſentation appears in another painting kept in a different part of the palace. Signor Paderni obſerved

obſerved to me, that theſe remains would help the curious to form a more certain idea of the manner, in which the ancients affixed titles to their volumes, than they have hitherto been able to obtain. The moſt complete deſcription, that I can recollect, of an ancient book, with its appurtenances and decorations, is that of Martial addreſſed to one of his own.

Fauſtini fugis in ſinum? Sapiſti.
Cedro nunc licet ambules peruncſus,
Et frontis gemino decens honore
Pictis luxurieris umbilicis:
Et te purpura delicata velet,
Et cocco rubeat ſuperbus index. L. iii. ep. 2.

The *ſuperbus index* in the laſt verſe, curiouſly illuminated with ſcarlet, was undoubtedly the title of the book; but to what part of it it was annexed has hitherto been difficult to aſcertain: for as (according to the paintings under conſideration) it was inſcribed on a detached piece of paper or parchment, it muſt ſoon have been loſt from the book; eſpecially if the latter had ſuffered by damps, or any other injuries ſimilar to thoſe, that have affected the volumes found in Herculaneum, of which not only the title, but even the umbilici, tho' conſiſting of more ſolid materials, as horn, ivory, *&c.* are intirely deſtroyed: ſo that no light could be had from the original antiquities with relation to this point. The only means, whereby the connoiſſeurs could form any conjectures in this caſe, muſt have been, I preſume, from the faſhion of books among the ancients, *viz.* their being long ſcrolls rolled round upon a ſtick with ornaments at each end, as deſcribed in the epigram

produced above. This form required, that the books ſhould be laid at their length upon the ſhelves, where they were depoſited with either their ſide, or one of their ends, appearing outwardly. Now of theſe two poſitions the latter, which expoſed the umbilicus to view, might be thought (all circumſtances duly conſidered) the moſt convenient. To this part therefore it might with probability be conjectured, that the index or title was faſtened; but the paintings mentioned above plainly demonſtrate, that it actually was ſo.

Monſ. Dacier ſays (18), that the titles of books were anciently inſcribed upon the leathern covers, wherein they were wrapt, and which, by the means of thongs faſtened to them, kept the volumes cloſe and compact together. If that learned gentleman had ſupported this fact by proper evidences, then it muſt have been concluded, upon the joint authority of ſuch evidences, and of the antiquities under conſideration, that the practice of the ancients was, beſides the title on the ſides of the volume, to affix another on a label at one of its extremities. And indeed this additional notation (whatever we determine concerning its uſefulneſs, while the books lay on a ſhelf in a library) muſt have been very neceſſary, when ſuch books ſtood upright in a *capſula* (like thoſe in the painting before us), where no part of them, but one end alone, could poſſibly be ſeen.

VIII.

Some pieces of fine paper, coloured red on one ſide, and black on the other, found upon the breaſt

(18) Not. on Hor. L. i. ep. 20. *v.* 2.

of

of a ſkeleton. Signor Paderni told me, that they had been viewed with great admiration by ſuch of the virtuoſi, as he had ſhewn them to; and that their admiration proceeded from thoſe fragments appearing not to be of the *charta papyracea*, but of that of ſilk, cotton, or linen. And indeed, if they ſhould prove to have been made of any of the materials laſt mentioned, it would contradict the generally received opinion (according to (19) Montfaucon), that paper of ſilk or cotton, denoted by the common appellation of *charta bombycina*, was firſt found out in the 9th century; as that compoſed of linen rags *(ex linteolis contritis et aquâ maceratis*, as Pancirollus (20) expreſſes it) was about the 12th; and that the former ſupplied the place of the *charta papyracea* in the eaſt, as the latter ſuperſeded the uſe of it in the weſtern parts of the world.

IX.

A flat piece of white glaſs, taken off from towards the extremity of the ſheet, as appears from the curvature and protuberant thickneſs of one of its ſides above the other parts. I have ſeveral obſervations by me, with regard to this fragment, which I have not yet had leiſure to digeſt. I ſhall therefore proceed to the other parts of this collection.

To enter into a detail of the paintings found at Herculaneum, and depoſited in a different part of the palace at Portici, would be tedious, as their

(19) Mem. lit. de l'Acad. des Inſcrip. V. 9.
(20) Rerum Mem. L. ii. tit. 13.

number, when I ſaw them, exceeded 800; and it would be ſuperfluous, as the principal of them will ſoon make their appearance in the world by prints taken from them, and executed in a manner, which (as far as I could judge by the ſpecimens ſhewn me) will in no-wiſe diſcredit the originals, I ſhall therefore only mention two of them, *viz.*

I.

Theſeus with the Minotaur dead, and lying on his back at his feet, while ſeveral Athenian youths are embracing the knees, and kiſſing the hand, of their deliverer. We may obſerve, that the fabulous being above-mentioned appears in this piece with the intire body of a man, and only the head of a bull, which agrees with the manner, in which he is repreſented in an antique ſardonyx of Greek ſculpture in the cabinet at Vienna, and in moſt of the works of the ancient artiſts. Tho' I have by me the copy of an antique gem, wherein the Minotaur is exhibited as ſtanding in the center of the famous labyrinth, and having below the body of a bull as far as to the waiſt, and from thence upwards an human form: which repreſentation is further countenanced by Ovid, who deſcribes that monſter, as

Semibovemque virum, ſemivirumque bovem.
Art. Am. L. ii. *v.* 12.

II.

Chiron and Achilles. The latter of theſe is ſtanding, and has a *plectrum* in his right hand: the former ſeems to embrace his noble pupil with his left arm, and with his right hand to ſtrike the lyre, as teach-

ing him to play upon that inſtrument. But the moſt remarkable circumſtance in the figure of Chiron is his repoſing his hinder parts on his left haunch upon the ground. Yet this attitude, as well as the other particulars mentioned above, is expreſſed in an antique gem, of which I have ſeen a copy at Rome.

I ſhall conclude this paper with an account of the ſtatues, which ſtand in ſeveral rooms adjoining to the unfiniſhed part of the palace, and were found (as to the far greater number) at or near Herculaneum.

In the Firſt Room.

An equeſtrian marble ſtatue of M. Nonius Balbus the elder, which is intended to be placed in a large entrance on the eaſt ſide of the palace, to anſwer to that of his ſon, which is already ſet up on the other ſide, facing the bay of Naples.

In the Second.

Nero and Germanicus, conſiderably larger than the life, but ſqueezed ſomewhat flat by the weight of the lava, or other ruins, with which they were once overwhelmed.

A man in a ſacrificing habit.

Two others in the toga, and two women in the palla.

All theſe are of bronze.

Statues of marble depoſited here are the following, *viz.*

At the entrance, a matron larger than nature, with ſtrong expreſſion in her face.

Two coloſſal trunks in a ſitting poſture.

Three ſtatues of one of the Agrippina's.

A Roman matron, or empreſs, with remains of red painting on the extremities of her palla.

Three other matrons.

In the Third Room.

Bacchus. A muſe. A fragment of a ſtatue in the pallium. A fine ſtatua togata with the head veiled, larger than the life.

Another very remarkable figure, whoſe face reſembles in beauty that commonly attributed to Venus, tho' the dreſs and other inſignia plainly indicate a Pallas: for her head is covered with an helmet, below which her hair falls down long and diſhevelled. Her left arm is enveloped with her ægis, which is large and expanded, ſo as to form a kind of mantle. Her garments are thin, and ſit cloſe to her body in ſtrait plaits. She is in a poſture of running, or ſtriding, with her feet at a conſiderable diſtance from each other, and her arms extended different ways; an attitude ſtrongly marking the utmoſt eagerneſs and haſte.

Next appears a Vertumnus. A fine figure of a philoſopher. Volumnia and Veturius. A lady with a thin ſtola. A Venus. A boy of exquiſite workmanſhip. A ſmall ſtatua togata.

In another part is a Faun of bronze, reclined, with his right hand lifted up, and his leg extended. This figure (as we were informed) was found accompanied with ſeven others of the ſame metal, which now ſtand in another chamber, *viz.* two young men in a running a poſture; four females ſomewhat

reſembling

reſembling veſtals in their habit, excepting that all their heads were uncovered, and thoſe of two of them were adorned with *vittæ*, or filets. Laſtly, a young man of a ſmall ſize, cloathed, with his arms ſomewhat extended.

There remains but one more figure to be taken notice of in this collection, *viz.* that of Serapis, with Cerberus at his right hand. Ancient writers (21) enable us to account for this appearance, by informing us, that Serapis (beſides his other characters of Æſculapius, Sol, Oſiris, and Jupiter) was accounted the ſame as Dis Pater, or Pluto. Upon this hypotheſis none can doubt of the propriety of Cerberus's attending upon this deity in the figure before us, as well as in three others given us by Montfaucon (22).

If we deſire to enter into the myſtical reaſon of this repreſentation, we may learn it from Porphyry, *viz.* that Serapis (23), being the ſame as Pluto, had dominion over the evil dæmons; and that thoſe beings were figured by a dog with three heads; meaning the dæmon ſubſiſting in the three elements of water, earth, and air.

Give me leave to add further, that I find, by my journal, that upon viewing this figure, I took notice of a diſſimilitude in the heads of it: but as it did

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(21) *Deum ipſum (Serapidem) multi Æſculapium —— quidam Oſirim — plerique Jovem — plurimi Ditem patrem inſignibus, quæ in ipſo manifeſta, aut per ambages, conjectant.* Tac. Hiſ. L. iv. Εἷς Ζεὺς, εἷς Αἰδής, ἕις Ἥλιος ἐστὶ Σάραπις, Oraculum Apollinis apud M. A. Cauſ. Muſeum Rom. vol. ii. §. 6. tab. 13.

(22) Antiq. T. ii. P. 2. pl. 121, 122.

(23) Porphyr. apud Euſeb. Præp. Evang. L. iv. c. 23. Τὸς δὲ πονηρὸς δαίμονας ἐκ εἰκῶν ὑπὸ Σάραπιν ὑποπτεύομεν, ἔτε.

not then occur to me, that they were ever expreſſed in any other form than the canine, I did not examine minutely into the difference: but, upon recollection, I am now inclined to think, that that monſter might have the heads of three ſeveral animals in this piece, as he has in another, given us by (24) Montfaucon: which mode of exhibiting him was (according to that learned (25) antiquary) invented by the Egyptians; a circumſtance not to be wondered at in a people, whoſe imagination teemed ſo plentifully with monſtrous ideas of all kinds, as theirs is known to have done.

To the ſame original we may refer the ſerpent twiſting round Cerberus in this monument; as we ſee two of the ſame ſpecies encircling his heads and body in that mentioned above (26). As I know no particular relation, that the ſerpent bears to Serapis, conſidered as Pluto, I can regard it here only as a ſacred ſymbol in the theology of the ancient Egyptians; and, as ſuch, properly attributed to an attendant of one of their chief divinities.

I ſhall trouble you but with one more obſervation upon this article, *viz.* that (if I may truſt my memory for a particular omitted in my notes) this is the ſtatue, which being the principal one found in an ancient magnificent building diſcovered about ſeven years ago at (27) Pozzuoli (in conjunction with other circumſtances) occaſioned it to be called The Temple of

(24) Suppl. T. ii. L. vi. c. 10. Tab. xlviii.
(25) Montfaucon, ibid.
(26) Montfaucon, ibid.
(27) Vid. Obſervations ſur les Antiquités d'Herculaneum, &c. par Meſſ. Cochin & Bellicard, p. 83. Paris 1755.

Serapis.

Serapis. As this place ſeemed greatly to merit the attention of the curious in antiquity, we procured a plan of it, drawn by a native, who has free acceſs to it; and (if I thought it would be acceptable to that learned Society, of which I have the honour to be a member) the ſaid plan ſhould wait upon them, accompanied with ſome obſervations upon it by,

SIR,

Your moſt obedient,

humble Servant,

John Nixon.

London, Feb. 24. 1757.

P. S. A long room is deſigned to be fitted up in the King's palace at Portici, for the reception of all the antiquities found at Herculaneum, *&c.* This apartment will be lighted by thirteen windows on the ſide towards the Cortile, and adorned with forty columns, partly of verde antique, partly of alabaſter with browniſh veins, and other beautiful marbles, found in divers parts of the King's dominions. Between every two of theſe columns will be placed a group, ſtatue, or buſt. The compartments in the walls will contain the ancient paintings. The other curioſities are to be depoſited in caſes made for that purpoſe; and the pavement will conſiſt intirely of the fineſt pieces of Moſaic work, that have been found in Herculaneum, or any places within the Neapolitan ſtate.

XIV.

XIV. *An Account of the Effects of a Storm of Thunder and Lightning, in the Parishes of* Looe *and* Lanreath, *in the County of* Cornwall, *on the 27th Day of* June, 1756. *Communicated to the Rev.* Jeremiah Milles, *D.D. F.R.S. in two Letters, one from the Rev. Mr.* Dyer, *Minister of* Looe, *and the other from the Rev. Mr.* Milles, *Vicar of* Duloe, *in* Cornwall.

Read Feb. 24, 1757.

ON Sunday the 27th of June last it grew on a sudden as dark as a winter evening: soon after, the lightning began to flash, and the thunder to roar. The claps were near, and extremely loud; and the lightning was more like darting flames of fire, than flashes of enkindled vapour. Happily no damage was done to the town of Looe, which lies very low; but at Bucklawren, a village situated on the top of a hill, about two miles from hence, a farm-house was shattered in a most surprising manner. The house fronts the south. The windows of the hall and parlour, and of the chambers over them, which are in the front of the house, are sashed. The dairy window is the only one on the west side of the house. The chimnies are on the north side; and at the south-west corner there is a row of old elms on a line with the front, the nearest of which is ten feet distant from the house. The lightning seems to have had a direction from

from the ſouth-weſt to the north-eaſt. It firſt ſtruck the bevilled roof of the ſouth-weſt corner, near the eaves of the houſe; made a large breach, and tore up the floor of the garret, near the place where it entered, and deſcended by the weſt wall, in oblique lines, into the chamber over the parlour; but not having ſufficient vent that way, it darted in a line from S. W. to N. E. againſt the north wall of the garret, where meeting with reſiſtance, it broke down the floor near the north wall many feet wide, and carrying the cieling of the parlour-chamber before it, ran down by the wall of that room in direct lines. Where it deſcended on the weſt and north walls it made large and deep furrows in the plaiſter, and even tore out the ſtones and mortar. A large ſplinter was ſtruck off from the bed-poſt contiguous to the north wall, and the bed was ſet on fire. The chimney-piece was broken into many parts; the window-frame was moved out of the wall, every pane of glaſs was broken, the under ſaſh was torn in pieces, and a large piece of the chimney-board was thrown out of the window againſt an oppoſite garden wall, about 20 feet from the houſe. As the lightning ſhot thro' the window, it found a ſmall cavity between the wall and the ſlating with which the wall is covered, where it burſt off the ſlates as far as it continued in a direct line downward, and threw them at a great diſtance from the houſe. Notwithſtanding this dreadful havock, the force of the lightning was not ſpent; the window gave it not a ſufficient diſcharge. From the chamber over the parlour, it deſcended by the north wall to the room under it, which is wainſcotted, tore off the cornice the whole breadth of

the room, and ſome mouldings from the wainſcot; broke the glaſſes and Delft ware in the beauffet; ſhivered the ſhelves of a bottle-room; and, ripping off a ſmall ſtock-lock from the door, burſt it open, and made its way chiefly thro' the window, the frame of which was moved from the wall, and the glaſs ſhattered to pieces. Near the bottle-room there was a hole ſtruck in the partition-wainſcotting, which divides the parlour from the hall, about eight inches long and an inch broad: through this crevice the lightning entered the hall, which ſerves at preſent for a kitchen, and meeting with ſome pewter in its way, it flung it from the ſhelf about the room; threw down a large iron bar, that ſtood in a corner, and which ſeemed to have a trembling and deſultory motion; carried the tongs into the chimney, and threw a tea-kettle, that ſtood there, into the middle of the floor; moved a large braſs pot out of its place, which was under a table; and then darted thro' the windows, carrying away a pane of glaſs intire out of the upper ſaſh to the diſtance of many feet. The miſtreſs of the houſe and her ſon were ſitting at this window. They were the only perſons in the houſe, and providentially received no hurt. Some part of the lightning found a way between the door and door-caſe of the hall. The door is pannelled: and the lightning, in paſſing thro', penetrated into a cloſe mortiſe, and ſplit off a large ſplinter from the outſide of the door, cloſe to the tenon. In its courſe it left a ſmoaky tinge on the wall and timber, like that of fired gunpowder. A ſulphureous ſmell remained in the houſe many hours. Another (or probably a part of the ſame) flaſh of lightning

ſtruck

ſtruck the dairy window, melted the lead, and burnt the glaſs where it penetrated, and ſet the window-frame on fire. From thence it darted in a line from S. W. to N. E. downward, made a large hole in a plaiſtered partition near the floor into the barn, ſhattered a large paving rag-ſtone in pieces, and tearing up the ground, I ſuppoſe, ſunk into the earth. The elms were affected with the lightning, particularly that neareſt the houſe, from the top of which to the root appeared large furrows in the moſs, which grew on the bark, in ſome places in an irregular ſpiral, but for the moſt part in a perpendicular line; and from the root of it the ground was torn up in furrows, as if done with a plough-ſhare, about ſix feet long, the furrows gradually leſſening according to their diſtance from the tree. All this was done inſtantaneouſly. How amazingly ſwift, ſubtle, and powerful is the force of lightning! I am,

Reverend Sir,

Your moſt obedient Servant,

James Dyer.

A Letter from the Rev. Mr. Milles *on the ſame Subject.*

ABOUT four of the clock on Sunday afternoon, the ſame day that the lightning ſtruck the farm-houſe at Bucklawren, it fell upon another houſe called Pelyne, in the pariſh of Lanreath, about ſix miles diſtant. The houſe fronts the eaſt. The chimney, which is at the north end, is cracked, and

opened about two or three inches wide, from the top to the roof, where it entered the ſlating thro' a ſmall hole on the eaſtern ſide; forced its way thro' the upper chamber, where it melted an old copper ſkillet, a pair of ſheepſhears, and ſome odd braſs buckles and candleſticks that lay on the wall; conſumed the laths adjoining, and then made its way thro' a ſmall crevice in the upper part of the window. Another and more ſevere part of the ſame lightning deſcended the chimney; ſtruck two women down, who were ſitting on each ſide of it, without any further hurt; overturned a long table, that was placed before the window in the ground room, upon two men, who were ſitting on the inſide, with their backs towards the window. One of theſe men was miſerably burnt in his right arm. The lightning ſeems to have ſtruck him a little above the elbow, making a ſmall orifice about the bigneſs of a pea: the burn from thence to the ſhoulder is near an inch deep. His right thigh was likewiſe burnt on the inſide, and the outſide of his right leg, from a little below the knee, quite over the ancle to his toes. Both knees were burnt acroſs ſlightly, and his left thigh. His ſhirt-ſleeve, and the upper part of his waiſtcoat, were reduced to tinder: the buckles in his ſhoes were melted in different parts, and in different directions. He has not been able to uſe his arm ſince; and is under the care of a ſurgeon, who has reduced the wound to a hand's breadth, which was in the beginning advancing faſt towards a mortification. The other man was but ſlightly wounded. The lightning afterwards found its way thro' the window in three different places; melted the glaſs, leaving a ſmutty tinge, like that of

fired

fired gunpowder. A boy, about ten years old, ſon to the under-tenant, was alſo ſtruck down, as he was ſtanding at the door, but not hurt. The father and his daughter felt no ill effects; but ſaw the lightning roll on the floor, and thought the room was on fire.

XV. *An Account of the Peat-pit near* Newbury *in* Berkſhire; *in an Extract of a Letter from* John Collet, *M. D. to the Right Reverend* Richard *Lord Biſhop of* Oſſory, *F. R. S.*

My Lord, Newbury, Decemb. 2, 1756.

Read Feb. 24, 1757.

NOW I am mentioning the peat, I beg leave to aſſure your Lordſhip, that tho' ſome perſons have aſſerted, that after the peat has been cut out, it grows again after ſome years; yet this is not true of the peat found here, none of the peat-pits, which were formerly dug out, and have lately been opened again, affording the leaſt reaſon to juſtify ſuch an opinion; but, on the contrary, the marks of the long ſpade (with which they cut out the peat) are ſtill plainly viſible all along the ſides of the pits, quite down to the bottom; and are now as freſh as if made but yeſterday, tho' cut above fifty years ago: which ſhews alſo, that our peat is of too firm a texture to be preſſed together, and to give way, ſo as to fill again the empty pits: which perhaps may be the caſe in ſome of the moſſes, where the pits are found after ſome years to be filled up again. The

The town of Newbury lies north and ſouth, in the ſhape of a Y, croſs a valley; which valley runs eaſt and weſt, and is here about a mile broad, the river Kennet running along the middle of it. The peat is found in the middle of this valley, on each ſide of the river, extending in all from between a quarter of a mile to about half a mile in breadth; and in length, along the valley, about nine miles weſtward, and about ſeven eaſtward; and I believe much further, tho' not yet diſcovered, and perhaps with ſome intermiſſions.

The ground it is found in is meadow land, and conſiſts chiefly of a whitiſh kind of earth: under this lies what they call *clob*, being a peat-earth, compounded of clay, of a ſmall quantity of earth, and ſome true peat: it is from four to eighteen inches thick; and where the earth above it is but thin, it is ſometimes full of the roots of plants, that grow on the ſurface of the ground: and if the meadow alſo be mooriſh, the ſedge and flags will ſhoot their roots quite thro' it into the true peat, which lies directly under this clob.

The top of the true peat is found at various depths, from one foot to eight feet below the ſurface of the ground; and the depth or thickneſs of this peat is alſo very different, from one foot to eight or nine feet, the ground below it being very uneven, and generally a gravel. My friend Mr. Oſgood has dug two feet into this gravel, to ſee if any peat lay below it, but could not find any.

The trueſt and beſt peat has very little (if any) earth in it; but is a compoſition of wood, branches, twigs, leaves, and roots of trees, with graſs, ſtraw,

plants, and weeds; and lying continually in water makes it ſoft and eaſy to be cut thro' with a ſharp peat-ſpade. The colour is of a blackiſh brown; and if it be chewed between the teeth it is ſoft, and has no gritty matter in it, which the clob has. It is indeed of a different conſiſtence in different places, ſome being ſofter, and ſome firmer and harder; which may perhaps ariſe from the different ſorts of trees it is compoſed of.

To get at the peat, they firſt dig up the ſurface of the ground till they come to the clob, throwing the earth into the empty pits, from which they have already cut out the peat: they then dig up the clob, and either ſell it to the poor for firing, or lay it in heaps, to burn to aſhes, to be ſold to the farmers. Then they cut out the true peat, with a peculiar kind of ſpade, in long pieces, vulgarly called long ſquares, about three inches and a half broad every way, and four feet long, if the thickneſs of the peat will allow that length: and as they cut it out in long pieces, they lay them in a regular order carefully, in rows upon the ground, to be dried by the ſun and wind. If the peat be thick, when they have cut one length of the ſpade for ſome diſtance, they return again, and cut down another length of it (or four feet), and ſo on, till they reach the gravelly bottom, if they can ſufficiently drain it of the water, which continually comes in, tho' proper perſons are employed to pump out as much of the water as they can all the time. As the peat dries, and is turned by perſons appointed for that purpoſe, to dry it the better, it breaks into ſmaller lengths, and then it ſerves not only the poor; but many other perſons, for firing, and gives a good heat.

heat. It is ſold for about ten ſhillings a waggon-load, delivered at their houſes in the town. The aſhes alſo prove very good manure for both graſs and arable land; and the farmers give from four pence to ſix pence a buſhel for them, which renders this firing very cheap.

Great numbers of trees are plainly viſible in the true peat, lying irregularly one upon another; and ſometimes even cart-loads of them have been taken out, and dried for firing: but the nearer theſe trees lie to the ſurface of the ground, the leſs ſound is the wood: and ſometimes the ſmall twigs, which lie at the bottom, are ſo firm, as not to be eaſily cut thro' with the uſual peat-ſpade. Theſe trees are generally oaks, alders, willows, and firs, beſides ſome others not eaſily to be known. The ſmall roots are generally periſhed; but yet have ſufficient ſigns to ſhew, that the trees were torn up by the roots, and were not cut down, there being no ſign of the ax or ſaw; which, had they been felled, would have been plainly viſible.

No acorns are found in the peat, tho' many cones of the fir-tree are, and alſo a great number of nut-ſhells. They are all of a darkiſh colour; and the nuts are hollow within, and ſome of them have a hole at the broad end.

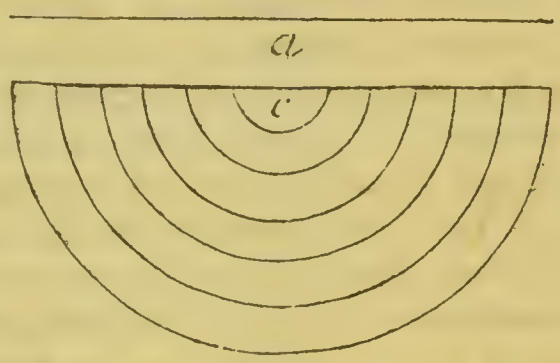
A great many horns, heads, and bones of ſeveral kinds of deer, the horns of the antelope, the heads and tuſks of boars, the heads of beavers, *&c.* are alſo found in it: and I have been told, that ſome human bones have been found; but I never ſaw any of theſe myſelf, tho' I have of all the others.

But

But I am aſſured, that all theſe things are generally found at the bottom of the peat, or very near it. And indeed, it is always very proper to be well and faithfully informed of the exact depth and place, where any thing of theſe kinds is found; whether it is in the earth above the peat, or in the clob; or in the true peat, or at the bottom of it; which will greatly aſſiſt us in forming a juſt judgment of the real antiquity of the things that are found, or at leaſt of the time they have lain there. Beſides this, as they formerly uſed to cut out the peat in large plots here and there, leaving ſpaces full of peat between thoſe pits (whereas now they draw off the greateſt part of the water by pumps, and ſo clear out all the peat regularly as they go on); ſo it muſt be carefully obſerved, whether whatſoever is found here be dug out of theſe old peat-pits, or not; for axes, and other things, may have been formerly dropt into theſe pits, before they were filled up again with earth, and may now be dug out of them again. My father has now in his poſſeſſion an iron hatchet, not greatly differing from the modern form, which was found lying flat at the very bottom of the peat: it was covered with a ruſt near half an inch thick, and the handle was to it, which ſeemed to be of beech-wood, but was ſo ſoft, that it broke in bringing it up: but as the perſon is dead, who found it, I can't ſay whether it lay in an old peat-pit, or no.

Mr. Oſgood found, ſome years ago, an urn, of a light brown colour, and large enough to hold above a gallon, in the true peat, about eight or ten feet from the river, near a mile and a half weſt of this town, in Speen-moor. It lay about four feet below

the level of the ground, and about one foot within the peat; and over it was raiſed an artificial hill, about eight feet higher than the neighbouring ground; and as the whole hill conſiſted of both peat and meadow-ground intermixed together, it plainly appeared, that the peat was older than the urn; and that the perſons, who raiſed the hill, muſt firſt have dug a large hole in the peat, to bury the urn there, and ſo formed the hill of the peat and meadow-ground mixed together. Round the hill, where the urn lay, they had made alſo many half-circular ridges, with trenches between them, one beyond another, in this manner:



Where *a* is the river, and *c* the hill; and the half circles ſhew ſome of the ridges, the number of which Mr. Oſgood has now forgot. The urn was broke by the peat-ſpade, and it came up only in ſmall pieces, ſo that nothing was found in it; and no body happened to be there at that time but the peat-cutters.

No coins of any ſort have been found in the peat. But there may, perhaps, be a variety of things at the bottom of it: but as the peat is always full of water, which is never quite drained off, ſo it is not an eaſy matter to examine the bottom.

 I beg

I beg leave to aſſure you, my Lord, that I am, with great reſpect and eſteem,

Your Lordſhip's moſt obliged,

and obedient humble Servant,

John Collet.

XVI. *An Account of the Alterations making in the* Pantheon *at* Rome: *In an Extract of a Letter from* Rome *to* Thomas Hollis, *Eſq*; *Communicated by* John Ward, *LL.D. R. S. Vice-Præſ.*

Read Mar. 3. 1757.

A Project was lately laid before the government by Paolo Poſi, an architect, for modernizing the inſide of the Pantheon, and unfortunately approved. In conſequence of which, the dome has been already cleaned, and rough caſt; and the remainder of the lead taken away, which ſerved as a lining to the ſilver work, that originally covered it. The veſtiges of the cornices, and other ornaments of the ſilver work, were ſtill diſcernible in the lead, which was faſtened by very large iron nails. All this was effected by a moveable ſcaffold, that was fixed to the bronze cornice of the open circle above, whereby the temple is illuminated, and deſcended to the cornice of the Attic order, being as curious in the contrivance, as deteſtable for the purpoſes intended by it. It is true, we could not before ſee the dome in its priſtine glory;

but we had the ſatisfaction of viewing the traces and remains of what it had been. Nor could the adepts in architecture ſufficiently admire the ſkill and ſagacity of the builder, who, compoſing it of a number of ſmall arches, which together formed a kind of net-work, and filling up the intervals between with pumice-ſtones and mortar, gave it that ſtrength and lightneſs, whereby it has probably ſtood ſo many ages.

The evil would be comparatively ſmall, had the project extended no farther, than what has been related; but they are now buſy in removing the Attic order, to make room for a new invention, ſuitable to the trifling taſte, which at this day prevails. And not content with that, they think of taking away the ancient pavement; and, what is ſtill worſe, its peculiar beauty, the open circle at the top, to place a lanthorn inſtead of it, as is uſual in modern cupola's.

You had the good fortune, Sir, to view this remarkable temple, in that ſtate, wherein it was left by the ancient barbarians: but thoſe, who ſee it hereafter, will find it in a much more deplorable condition, ſtripped of its precious marbles and ornaments; and ſo diſguiſed by modern alterations, that the noble form given it by Agrippa will be no longer diſtinguiſhable.

It is ſaid Il Signor J. B. Piraneſi, the architect, who publiſhed the antiquities of Rome, and divers ingenious works of that kind, has taken accurate plans of the Attic order, and every other particular relating to it. Theſe he propoſes to engrave and publiſh, with exact explanations annexed to them; together

together with a plan of the whole, as he believes it appeared in its original ſplendor and perfection; that poſterity may not be deprived of informations, which are of ſo great benefit and importance to all lovers of architecture. It is alſo ſaid, that the engineer [carpenter], who invented the ſcaffolding, has made an exact model of it for him; which he intends to publiſh as a part of the work before-mentioned.

XVII. *An Account of a new medicinal Well, lately diſcovered near* Moffat, *in* Annandale, *in the County of* Dumfries. *By Mr.* John Walker, *of* Borgue-Houſe, *near* Kirkudbright, *in* Scotland.

Read Feb. 10, & Mar. 3, 1757.

THIS mineral ſpring was found out by one Mr. Williamſon; a few years ago, when he was overſeeing a mine, which was at that time carrying on in its neighbourhood. It is ſituate about four miles diſtant from Moffat, in the bottom of a deep ſcar, which is on the weſt ſide of a large mountain called Hartfell, from which it has acquired the name of Hartfell-ſpaw. This ſcar is a part of the mountain, thro' which a ſmall ſtream of water has worn its way to a conſiderable depth; by which it has laid open, and expoſed to view, the ſtrata of the earth on each ſide: and in the bottom of this ſcar, and near to the brink of this ſmall brook, the mineral water ſprings up. When

When I ſaw it, it conſiſted of two ſprings, which were very well ordered by Mr. Williamſon, ſo as to run from two wooden ſpouts, immediately at their riſe out of the earth; which indeed muſt be of very great advantage to all mineral waters: and I am perſuaded there are many, whoſe medical qualities are greatly impaired by falling into reſervoirs, and continuing in them for ſome time after they ſpring from the earth. The one of theſe ſprings was ſituate about ten or twelve yards further up the brook than the other; and they were then diſtinguiſhed by the names of the upper and lower ſpring: but I have been ſince informed, that their ſituation is now altered. Each of theſe ſprings did at that time run nearly the ſame quantity of water, which, as I thought, was above an Engliſh quart in a minute, and that during a ſeaſon of very dry weather.

As there are many inſtances of mineral waters ſpringing out of the earth very near each other, which at the ſame time are impregnated with very different principles; it therefore ſeemed not at all improbable, that as theſe waters did appear to run, for ſome part of their courſe, in different channels, they might in ſome reſpects be different from each other. And this ſuſpicion I found not to be altogether groundleſs with regard to theſe ſprings, as will be ſhewn afterwards. For which reaſon it may be obſerved, that the following trials were all made upon the water of the upper fountain, except where the other is particularly mentioned; and alſo that they were made within 24 hours after the water was taken from the ſpring, being carried to Moffat in bottles carefully ſealed.

According

According to what may be inferred from the following experiments, it may be premiſed, that this water appears to contain in it a large proportion of iron, but in two different forms; and an aluminous ſalt, which is conjoined with a terreſtrial principle.

As the contents of ſeveral mineral waters have been the cauſe of many different opinions, and of great diſputes among phyſicians and chymiſts; as the inquiry I made into the principles of theſe waters, which I am now conſidering, was not performed with that nicety and exactneſs I could have wiſhed; and as I am perſuaded, that to dogmatize in any branch of philoſophy can never tend to its advancement; I ſhall not therefore pretend to determine with certainty in any part of this ſubject, where the contrary opinion can be admitted with the leaſt degree of probability. Theſe trials are indeed but few and imperfect, and are no-way ſufficient to form an exact account of this mineral water; yet I believe they may afford ſome concluſions, which may be ſerviceable in compiling a more compleat hiſtory of it. They render it pretty evident, that the above-mentioned principles are contained in theſe waters: and tho' I will not pretend abſolutely to exclude all others, yet I muſt ſay, that, by what inquiry I made, I could not obſerve them to be in the leaſt impregnated with any other kind of mineral ſubſtance.

After a good deal of obſervation upon the water of this Spaw; and after many fruitleſs attempts, which I have at different times made upon ſeveral other waters of the chalybeat kind in Scotland, in queſt of the volatile ſpirit, which has been commonly artributed to them; I muſt own, that I have been induced

induced to think, that there is no ſuch thing exiſts in theſe waters at all. What has been generally called the ſpirit of ſteel waters, ſeems to me to be very unintelligible; altho' the exiſtence of it in theſe waters has been aſſerted by all the writers on this ſubject, which I have yet had occaſion to ſee. The ſpirit of a mixed body is commonly taken to be a ſubtile, penetrating, light, and volatile ſubſtance, more ſuſceptible of motion than any other of its parts, and moſt eaſily ſeparable from them by avolation. But that any chalybeat water contains ſuch a ſubſtance, I think has never been made evident, unleſs where the water has been found to be impregnated with ſome other mineral principles. Some ſteel waters, I believe, contain a large proportion of air, whoſe elaſticity may occaſion it to break forth with an exploſive force; ſome others there are, which contain a volatile and ſulphureous halitus; and to one or other of theſe two cauſes, or to ſome other mineral principle, I think moſt of the phænomena may be referred, which have been attributed to a ferrugineous or vitriolic volatile ſpirit.

As the firſt thing obſervable in a mineral water is its outward form, we muſt therefore take notice, that the water of this Spaw equals the clearest ſpring-water in tranſparency; and is likewiſe as free of any colour or odour: yet its taſte is very ſtrong, and may be diſcerned to be compounded of a ſweet, ſubacid, and aſtringent taſte. Its ſweetneſs and acidity appear ſenſibly to ariſe from alum; and its high ſtyptic and aſtringent taſte does as evidently proceed from that mineral ſalt, joined with ſome earthy or ferrugineous parts. I muſt likewiſe obſerve, that when

I firſt

I firſt compared the taſte of theſe two ſprings, I could plainly diſcern, that the water of the lower ſpring was more acid, and leſs aſtringent, than that of the upper one; and, on the contrary, the water of the upper ſpring ſeemed more aſtringent, and leſs acid. This induced me to think, that the mineral parts, which cauſed the acid and aſtringent taſtes, were mixed in the waters of theſe two ſprings in different proportions. And what I obſerved of them afterwards ſtill confirmed this conjecture.

But, in order to give ſome evidence for the exiſtence of the above-mentioned minerals in the waters of theſe ſprings, we ſhall conſider them ſeparately, by relating the experiments, which ſeemed to indicate, that they are contained in theſe waters in a very conſiderable proportion, and by offering ſome concluſions, which may be reaſonably drawn from them.

And as the firſt trials were made in queſt of iron, it may perhaps be moſt proper to conſider it in the firſt place.

Experiment 1. Some pieces of galls being added to equal quantities of the water of the two ſprings, an exceeding deep and bright blue colour was immediately produced in the water of the upper ſpring, which in a little time turned to a perfect black. The water of the lower ſpring, tho' indeed it was turned of the ſame colour, yet was not of ſo deep a ſhade, but was ſomewhat lighter than the former. The tincture of galls cauſed the ſame appearances.

2. A tincture of balauſtine-flowers produced the above blue colours in both waters.

3. A quantity of the water being thoroughly tinged with galls, was allowed to ſtand 24 hours: being then filtrated thro' brown paper, the water, tho' almoſt quite colourleſs, would not again receive any tincture with galls.

4. After elixation the water became of a turbid yellow colour with ochre, and afforded very little tincture with galls.

5. A ſolution of ſal Martis, chemically prepared, being mixed with galls, immediately turned of a bright dark blue colour, exactly ſimilar to that produced in the water.

6. A ſolution of common and rock alum was noways changed in its colour with galls.

7. A ſolution of ſal Martis and alum being mixed in equal quantities, the mixture was turned of a bright blue colour with galls; yet not of ſo deep a hue, but of a more diluted colour than the ſolution of ſal Martis, without alum.

From theſe experiments we muſt firſt of all obſerve, that the colour, which theſe waters afford with galls and pomegranate-flowers, is very uncommon. The more iron, that any mineral water contains, it will afford the deeper colour with ſuch aſtringents: but tho' I have tried this experiment upon a great many of the ferrugineous waters in Scotland, and alſo upon the water of ſome of the foreign Spaws, I never obſerved one, that afforded ſo deep a colour as this, which we now conſider. Some of the weakeſt of them gives only a red or faint purple tincture, and the ſtrongeſt only a deep purple: but I never ſaw or heard of any chalybeat water, but this, either in

Scotland

Scotland or elſewhere, that afforded an intenſe black and inky colour with galls. From which, I think we may venture to conclude, that the water of this Spaw contains a far larger proportion of iron than moſt, or perhaps than any, other chalybeat water hitherto diſcovered: and for this reaſon, I dare ſay, it will likewiſe be ſo much the more preferable to moſt others in medicinal virtues; which has indeed already appeared by many ſurpriſing cures it has performed, and which, I am perſuaded, will more fully appear, when its medicinal effects ſhall be better known.

There muſt needs be a very great quantity of iron in this water, when it yields as deep a colour with galls as a ſtrong ſolution of ſal Martis. I was indeed at firſt apprehenſive, that this perhaps might not be owing ſo much to a large and uncommon proportion of chalybeat parts, as to the commixture of alum, which I judged to be in the water. But we ſee the contrary appears by theſe trials: for alum of itſelf affords no tincture with aſtringents, and, inſtead of rendering a ſolution of ſal Martis with galls of a more intenſe colour, rather makes it lighter and more diluted.

We ſee here, that the ferrugineous matter is intirely ſeparated from the water by an infuſion of galls. The like alſo happens by elixation; after which it is almoſt deprived of its tinging quality. Yet other chalybeat waters loſe this quality by a much leſs degree of heat.

As there is an ochrous earth ſeparated from all ſteel waters, when expoſed to the air, which ſubſides

to the bottom, and a metalline ſcum or cremor, which ſwim on their ſurface; we ſhall next conſider the appearances, which they make in this water.

Exp. 8. A ſolution of ſaccharum Saturni being dropt into common ſpring-water, left the upper parts of the water clear and colourleſs, but formed a lacteſcency towards the bottom. The ſame ſolution being added to the mineral water, ſoon turned it of a turbid yellow colour, which afterwards ſubſided, and formed a deep yellow cloud in the bottom of the glaſs; and below this yellow ſediment there adhered to the bottom of the glaſs a whitiſh ſubſtance, which I took to be the metalline parts of the ſaccharum Saturni ſeparated from the purer parts of the ſalt, which were ſtill ſuſpended in the water, and which made it of a muddy whitiſh colour.

9. Forty drops of oleum tartari per deliquium being added to an ounce of the water, made it of an uniform light yellow colour; but in an hour afterwards there were many ſmall yellow terrene *nubeculæ* formed in it. Theſe the next day were more conſpicuous, being thoroughly ſeparated from the water, and precipitated to the bottom, leaving the water quite clear, as it was before the mixture. A ſmall quantity of this limpid water being taken, it would afford no tincture with galls. It was then all poured off, except ſo much in the bottom of the glaſs as contained the above-mentioned clouds: to this there were ſome galls added, which in half an hour turned theſe clouds from a light yellow to a deep red colour, but did not change the colour of the water, in which they ſwam.

10. Im-

10. Immediately after the affuſion of ol. tart. p. d. to the water, galls were added to the mixture, which tinged it of a deep and bright red colour. After ſtanding for ſome time, there were red clouds precipitated to the bottom, and the water continued of a duſky opake red colour.

11. There is a ſmall brook, formerly mentioned, which runs near by theſe ſprings; into which the water, that flows from them, is diſcharged. I obſerved the ſtones and channel of this brook all tinged with ochre of a deep yellow colour, ſo far up as the water of theſe ſprings flowed into it; but the channel, which the mineral water ran over before it was mixed with the water of the brook, was very little or nothing diſcoloured with ochre. As I conjectured what this was owing to, I afterwards took two equal quantities of the mineral water, into one of which I put an equal quantity of common water. In two hours the mixture became leſs tranſparent, and appeared yellowiſh, while the ſimple mineral water retained its clearneſs. Next day there was much ochre ſeparated from the mixture, which ſubſided to the bottom of the glaſs: but the unmixed mineral water remained ſtill clear and colourleſs, as at firſt.

All chalybeat waters ſeparate their ochrous parts, when expoſed ſome time to the air; but this ſeparation is made ſooner by the commixture of ſeveral kinds of ſalts. Thus we ſee the ochre in this water is immediately ſeparated and precipitated by the ſolution of ſaccharum Saturni.

The oil of tartar cauſes a precipitation of theſe ferrugineous parts in the ſame manner. Which parts muſt

muſt be the ſole cauſe, that the water receives a tincture from galls; ſince, after they are precipitate, it loſes that quality, which they notwithſtanding retain even after they are ſeparated from the water. This precipitation of the ochrous parts of the water were the only viſible effects that I could perceive to follow from the affuſion of the ol. tart. p. d. I remember indeed, when I was at Moffat, I ſaw the manuſcript of Dr. Horſburgh's experiments upon this mineral water; which appeared to be very accurate; and which I underſtand are ſince printed, in a volume lately publiſhed by the Philoſophical Society at Edinburgh. Amongſt theſe I obſerved one, which I thought ſo very remarkable, that I particularly adverted to it. It was the effects of the affuſion of ol. tart. p. d. to the water, producing in it clouds, or a coagulation of a green or graſs-green colour. I think theſe were the words; and I own I was ſomething ſurpriſed at them. A ſolution of vitriolum Martis, mixed with this alkaline oil, does indeed produce a green coagulum: but I could ſcarcely think, that this, or any other chalybeat water, contained ſo large a proportion of that vitriol, as to be ſufficient to produce theſe effects, when I conſidered, that ſo many writers, which I had ſeen, upon this ſubject, have all failed in their attempts of extracting a conſpicuous martial vitriol from ſuch mineral waters. I had tried this experiment upon four or five chalybeat ſprings in Scotland, and likewiſe upon the Spa and Pyrmont waters, which had been well preſerved; but there never reſulted any ſuch effects from the mixture of theſe with oil of tartar, as are related in the above experiment. All the alteration it pro-

duced in theſe waters was the precipitation of an ochrous earth, but without the leaſt appearance of any green colour. As I looked upon this as a leading experiment in the hiſtory of vitriolic waters; as I had often tried it, and as often ſeen the green coagulum produced with the ſolution of the factitious vitriol, and never could obſerve it produced in any of the above water; I began to ſuſpect, that theſe waters were either not poſſeſſed of a vitriolic ſalt at all, or elſe, that it was in ſome reſpects very different from the factitious vitriol. For theſe reaſons, Dr. Horſburgh's experiment appeared very extraordinary; tho' at the ſame time I was greatly pleaſed, that I ſhould have the opportunity of repeating it, and of obſerving thoſe phænomena in this ferrugineous water, which I had ſought for in vain in ſeveral others. But when I came to make the trial, I was yet more ſurpriſed, when I found it miſgive, and that the ol. tart. p. d. produced no green colour or coagulum in this mineral water, nor cauſed any other alteration in it, than the ſeparation of a large quantity of ochrous earth of a yellow colour, exactly the ſame with what I had obſerved in the other ſteel waters. This failure made me immediately conclude, that I had ſomehow or other committed an error in the experiment: and tho' I was pretty ſure, that the mineral water, which I had uſed in it, was quite freſh, yet I could not be ſo poſitive as to the oil of tartar, which I ſuſpected to have been long kept. Yet that this could have been the cauſe of my being ſo unſucceſſful, I could ſcarcely believe, tho' indeed I could aſſign no other. I was ſorry, that I had not afterwards an opportunity of repeating this experiment with more

accuracy,

accuracy, from which I might have expected to reap more ſucceſs, as it is perhaps one of the moſt conſequence, that can be performed on this mineral water, as it is capable of demonſtrating the exiſtence of a ſubſtantial vitriolum Martis in it; which is more than has been hitherto done, or perhaps ever will be done, concerning any one of the vaſt number of chalybeat waters, which have been yet diſcovered.

When galls are added to the water, at the ſame time with oil of tartar, inſtead of its deep blue colour, it affords only a red tincture.

It appears from the 11th experiment, that an addition of common water cauſes the mineral water to precipitate its ochre; and the reaſon of this is obvious: for if theſe ochrous parts be altogether terrene, as they appear to be, and exiſt in the water unconnected with any other principle, then it muſt happen, that as theſe parts are uniformly diffuſed thro' the water, in which they are ſuſpended as in a menſtruum; by the addition of common water, this menſtruum being diluted, the coheſion of theſe terrene parts muſt be thereby weakened, and their contact deſtroyed; ſo that their menſtrual equilibrium being thus taken off, they can be no longer ſupported in the fluid, but muſt be precipitated by the force of their own gravity.

Exp. 12. When the water was expoſed for ſome days to the air, there was a cremor ſeparated from it of a ſhining chalybeat colour. This, like other kinds of cremor, takes a conſiderable time to compleat its intire ſeparation from the fluid, out of which it is expelled: for when it was deſpumated, a new cremor

cremor always ſucceeded, until the whole quantity, which the water contained, was exhauſted.

13. When this cremor firſt appeared on the water, it was of a faint blueiſh colour: but as it increaſed, it changed into a deeper and more bright ſhining blue: and, after longer ſtanding, it became blotched with various colours, as red, orange, yellow, green, blue, purple, and violet.

14. A quantity of the water being put in a gentle heat, this cremor was quickly ſeparated from it, and appeared on the ſurface of the water. A like quantity of the water, with its cremor already upon its ſurface, was put over a gentle heat, which by degrees broke the cremor into very ſmall parts; but whether they did evaporate, or precipitate in the water, I could not be certain. But, by other trials, this cremor was found to have a great degree of fixity, bearing a conſiderable heat without avolation; yet not without the appearance of ſome of its parts flying off, altho' moſt of them were fixed; becauſe what remained loſt its fine colours, and was changed into a ſhining chalybeat colour.

15. The water of the lower ſpring afforded a much leſs quantity of the cremor, than the water of the upper ſpring. It took alſo a longer time to ſeparate, was of a blueiſh colour, and had not the vivid colours, which the water of the upper ſpring ſhewed.

16. When ol. tart. p. d. and ſpirit of ſal ammoniac were added to the water, it did not ſeparate its cremor.

This cremor, which is ſeparated from the water, is the ſame with that, which appears on the ſurface of

a ſolution of vitriolum Martis, when expoſed for ſome time to the air: and an infuſion of iron in common water alſo emits a cremor of the ſame kind. I remember, as I was once carefully obſerving a large glaſs full of a chalybeat water, which contained much of this cremor; ſoon after it was expoſed to the air, I obſerved a tenuicus blueiſh vapour riſing in the parts of the water next the ſurface, which very much diminiſhed its tranſparency; and by degrees this vapour was emitted by the loweſt parts of the water: but as the cremor increaſed on its ſurface, the water became gradually deprived of the blueiſh tincture, which it received from this halituous body; which was apparently nothing elſe but the parts of the cremor ſeparating from the water, and aſcending upwards. From whence we may conclude, that this cremor conſiſts of the very fineſt parts of iron attenuated to the higheſt degree.

It has been the opinion of moſt naturaliſts, that theſe kind of mineral waters do abound in ſulphureous parts. This they have conjectured from the foetor, that often attends them. But in what quantity or form theſe parts exiſt in the fluid, or by what means they can be rendered conſpicuous, has not as yet been ſufficiently determined. Yet, I think, we may ſuſpect ſome of the parts of this cremor to be ſulphureous. They are volatile, and, being heated, do fly off from the pure metalline parts, which being more fixed, are thereby left deſtitute of thoſe vivid colours, which they enjoyed from the ſulphureous parts. Theſe are evident marks of ſulphur, and are altogether analogous to ſome other appearances of that mineral. Another obſervation tending to ſupport this is the want of thoſe vivid colours in the cremor,

cremor, which appears on an infuſion of iron; the reaſon of which ſeems to be the loſs of the ſulphureous parts of the chalybeat minerals by avolation, during the operations of the fire, which they undergo in refining.

It appears from the fifteenth experiment, that the water of theſe two ſprings contains a very different proportion of this cremor: and from the laſt, that it is precipitated along with the ochrous parts, which happens upon the affuſion of theſe alkaline liquors.

The next trials were in queſt of alum.

Exp. 17. A quantity of the water being kept for ſome time in a boiling heat, and after it was cool being filtred quite clear from its ochrous matter, it ſtill retained a ſubacid and aluminous taſte in a very ſtrong degree.

18. To an ounce of common ſpring-water there was added two gutts of freſh ſweet milk. This mixture being ſhaken, the milk mixed intimately with the water, without any kind of coagulation.

19. The ſame experiment being made with the mineral water, the milk, upon its affuſion, was ſo curdled, or ſeparated into clouds, that the greateſt ſhaking could not mix or incorporate it with the water.

20. This experiment being alſo made with a weak ſolution of alum in ſpring-water, its effects upon the milk were not in the leaſt different from thoſe of the mineral water.

21. And the ſame trial being again repeated with

the water, when boiled and filtred from its ochrous parts, the milk was in the ſame manner coagulated as before elixation.

22. One part of ſweet milk being added to four parts of the mineral water, the milk ſubſided, and formed a cloud in the bottom of the glaſs, leaving the upper parts of the water clear. This mixture being heartily ſhaken, the milk mixed ſo well with the water, that it appeared to be but a very little curdled.

23. When a larger quantity of milk was added to a ſmaller quantity of water, and even when equal parts of the milk and mineral water were mixed and ſhaken together, there could be no curdling or coagulation obſerved.

24. An equal quantity of the water and milk being boiled together, the greateſt part of the milk was coagulated into a thick white curd; and the remainder, with the mineral water, turned of a pure white milky colour, which drank like whey, and was very agreeable.

25. Eight gutts of ſweet milk being added to four ounces of the water, and the mixture boiled, part of the milk was thereby curdled, and ſwam upon the top of the water. The ochrous parts of the water were likewiſe ſeparated, and falling to the bottom, their colour did not appear of a clear yellow, as uſual, but was ſomething milky.

All theſe experiments ſtrongly indicate the exiſtence of alum in this water. It retains its aluminous taſte, and coagulates milk, after the chalybeat parts are almoſt all expelled by elixation. The coagulation

tion of the milk demonſtrates an acidity in the water, and the other appearances ſhew that acidity to be owing to an aluminous ſalt.

It appears, that the milk requires a large quantity of the water, to make a ſenſible coagulation in it: for, in the 22d experiment, one part of the milk being added to four parts of the mineral water, the coagulation was ſcarcely diſcernible: and in the 23d, when an equal or larger quantity of milk was added to the water, the coagulation was not at all obſervable. I have heard it confidently averred, that this mineral water did not at all curdle milk; which, I ſuppoſe, has been thro' a miſtake in the experiment, in adding too large a proportion of milk to the water: for in this way the coagulation cannot be obſerved.

I imagined, that when the water was boiled with milk, the mixture would have become of a muddy yellow colour, by the ſeparation of the ochre: but it did not even appear, that the ochre was at all ſeparated from the mixture, as it is from the water when boiled by itſelf. On the contrary, not only the coagulum, but alſo the liquor, was of a pure white colour, and of a pleaſant taſte: and this makes me think it worth the inquiring into, whether or not the water does retain its medical qualities after it is prepared in this manner with milk? For, if it does, ſuch a preparation might certainly be very ſerviceable in many caſes.

Theſe experiments, which we next relate, do not only aſcertain the exiſtence of alum in the water with greater certainty, but alſo, that there is a particular kind of earth conjoined with this ſalt.

Exp.

Exp. 26. An Engliſh quart of the water being kept boiling for a quarter of an hour, it turned thick, muddy, and yellow, by the ſeparation of its ochrous parts; and, being ſet to cool in a clean bowl, the next day all the ochre was ſubſided to the bottom, from which the water was carefully filtred: whereby it became almoſt as clear and limpid as before the elixation, retaining a ſharp aluminous taſte, but was deprived of the ſtrong ferrugineous taſte, which it had at firſt. This water was again boiled; by which means it was again turned a little yellow, by the ſeparation of ſome more ochre. It was therefore again filtred, and rendered clear, and its aluminous taſte was ſtronger than before. After this filtration, the water was evaporated in a ſand-heat to about a ſixteenth part of the original quantity, and then it taſted like a ſtrong ſolution of alum joined with a ſmall degree of a chalybeat taſte. And this being totally evaporated in a glaſs, there adhered upon its ſides a pure white ſalt; and a larger quantity of the ſame ſalt remained in the bottom of the glaſs, which was not ſo white, but more impure than the former, and of a brown colour.

27. This ſalt, thus procured from the water, being mixed with diſtilled vinegar and ſpirit of vitriol, there was not the leaſt effervescence produced.

28. Some of the brown-coloured ſalt being put upon a red-hot iron, it did neither ſparkle nor decrepitate; but was turned into a blackiſh cineritious ſubſtance, which in a ſhort time became a white calx. And tho' ſome of the ſalt was put upon the iron finely powdered, yet it concreted, and run together in a cinder, whoſe coheſion was afterwards deſtroyed when calcined by a further degree of heat.

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29. As I was accidentally deprived of the opportunity of obtaining the cryſtals of this ſalt, which would have been the beſt means of knowing to what ſpecies it was to be referred; I diſſolved the whole maſs in a ſmall quantity of ſpring-water, and, by filtrating this ſolution, I obtained a large proportion of fine earth of a brown colour.

30. This ſolution of the ſalt afforded a deep blue tincture with galls.

31. The ſame ſolution, being mixed with ſyrup of violets, became of a reddiſh colour.

32. Saccharum Saturni being added to the ſolution, precipitated a thick lacteſcent cloud.

33. Ol. tart. p. d. being alſo added to this ſolution, it cauſed no viſible effervescence, yet raiſed ſome bubbles of air, and cauſed a coagulation of many ſmall brown terrene nubeculæ in the water; which, after ſtanding ſome time, ſubſided to the bottom, and left the water clear.

Theſe experiments do plainly evince, that this water contains an aluminous ſalt, conjoined with a fine terrene ſubſtance, which is probably a part of the matrix, from whence the ſalt has been formed.

This ſalt gives no ſigns of any alkaline principle; but, on the contrary, of an acidity, as its ſolution reddens with ſyrup of violets.

With this ſalt there are alſo intimately conjoined ſome very ſubtile chalybeat parts, which are not ſeparable from it by elixation or evaporation.

Alum is diſtinguiſhable from all other mineral ſalts, by liquifying and bubbling upon a red-hot iron, and turning into a white calx. But this could not be well expected from this aluminous ſalt, which we

had extracted from the water, becauſe it was extremely foul, by being combined with ſo large a proportion of earth; which earthy parts were the occaſion of turning the ſalt of a blackiſh colour upon the iron. However, we ſee it turns white by a further degree of heat. But if the ſalt had been diſſolved, filtrated, and cryſtallized, till it had been purified and freed from this terrene matter, it would then certainly have had the ſame appearance upon the red-hot iron, as a pure aluminous ſalt. Again, as it is peculiar to an aluminous ſalt to liquify in ſome degree with fire, ſo we ſee, that this was evidently the caſe of this ſalt. Its eliquation indeed could not be ſo remarkable, as in pure alum, becauſe of its being mixed with ſo much earth; but that it did liquify in ſome degree is plain, becauſe the whole maſs of ſalt and earth, even when reduced to a powder, ran all together like a cinder.

The experiment upon the ſolution of this ſalt with ol. tart. p. d. is alſo a further proof of what we have already aſſerted: for tho' there was no viſible effervescence, yet the bubbles of air ſhew, that there was an inteſtine conflict of the oil with the acid principle in the ſolution; which being abſorbed by the alkali, the earth was precipitated, to which it formerly adhered.

The two next experiments were made in order to diſcover, whether an acid or alkaline principle prevailed in the water.

Exp. 34. Forty gutts of the ſyrup of violets being added to an ounce of the water, the mixture became of a bright ſea-green colour.

35. A quantity of the water being kept boiling for five minutes, and afterwards allowed to ſtand till it became clear, was carefully filtrated from its ochrous ſediment: after which, upon its mixture with ſyrup of violets, it turned of a faint reddiſh colour.

From theſe experiments we infer, that this mineral water contains both an alkaline and an acid principle; the former conſiſting of the ochrous and ferrugineous parts, which are ſeparated from the water by elixation; and the latter of the aluminous ſalt, which remains in the water after elixation.

The following trials were made in order to know what effects are produced in the water by being expoſed to the air; and in what reſpects the waters of the two ſprings differed from each other.

Exp. 37. An Engliſh quart of the water of each of the ſprings being fully expoſed to the air in two China bowls, the next day that of the under ſpring was neither altered in its taſte, colour, or tranſparency, nor in any other ſhape whatever; but that of the upper ſpring appeared of a yellowiſh colour, altho' it was clear and tranſparent as the other.

On the ſecond day the taſte of the waters ſcarcely appeared to be any way diminiſhed. No ſenſible change could be obſerved in the lower water; but the upper water was become more yellow than it was the day before, yet without any loſs of its tranſparency. They both tinged of a deep blue colour with galls; which tinctures appeared equally deep

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and ſtrong, as they did before the waters were expoſed to the air.

The third day the lower water appeared clear and colourleſs as before, only its ſurface was covered with a few ſmall ſpots of cremor. The upper water appeared more yellowiſh than formerly, and its ſurface was almoſt wholly covered over with the cremor. They both afforded a tincture with galls, which was not ſenſibly different from what they gave before their expoſure.

On the fourteenth day the water of the under well had precipitated a yellow ochrous ſediment, but the other water a more conſiderable quantity. A large quantity of cremor continued alſo to ſwim upon the ſurface of the upper water, but there was very little ſeparated from the water of the under well. Both waters being now tried with galls, inſtead of the deep blue colour, which they did formerly exhibit, they now became only of a deep purple colour.

On the twentieth day the viſible appearance of both waters was the ſame as when laſt obſerved.

On the thirty-eighth day they both afforded as deep a purple colour with galls, as they did three weeks before; and during that time alſo they had not precipitated any more of their ochrous parts, nor ſuffered any other ſenſible alteration.

The water of the upper well being filtrated from all the ferrugineous matter, which it had ſeparated during theſe thirty-eight days, was rendered almoſt as limpid and clear as when newly taken from the well: but, being boiled for ſome time, it became of a turbid yellow colour; and being allowed to ſtand,

it

it again precipitated abundance of an ochrous ſediment; and being filtrated, and mixed with galls, it received a faint purple colour of a blueiſh hue.

38. A bottle of the water of each of theſe ſprings, being carefully ſealed, carried to Moffat, and kept for two months, ſuffered not the leaſt alteration during that time, but was as freſh as when immediately taken from the fountain. And I am informed, that after it is carried to Edinburgh, and to places at a greater diſtance, it will keep a much longer time without being any way ſpoiled.

I believe it will appear from theſe obſervations, that this mineral water continues longer intire, and particularly that it retains the quality of tinging with galls longer, than moſt others of the chalybeat kind: at leaſt, of a great number, which I have ſeen deſcribed, I do not remember one, that retains it near ſo long, when expoſed to the open air. Many of them loſe this quality intirely in a few hours; and it is greatly impaired in the ſame time, even in thoſe which retain it longeſt. But this water, we ſee, remains expoſed to the open air for days, without almoſt any alteration. This may perhaps be owing either to the larger proportion of ferrugineous parts, with which it is impregnated; to their being attenuated to a greater degree; or to their more perfect commixture with the water, by means of the aluminous ſalt. The longer time, that any mineral water does remain intire, without any ſeparation of its mineral parts; or the longer it retains the ſame form, which it has when newly taken from the ſpring; the more perfect is the commixture of theſe parts with their

fluid vehicle: and I believe, upon that account, will be more effectual for medicinal uſes: for which reaſon, I ſuppoſe, theſe waters may prove a more beneficial medicine, than any others of the ferrugineous kind, whoſe mineral contents are not ſo intimately commixed with the aqueous fluid.

As theſe waters are ſo long in ſeparating their mineral contents, they appear particularly well adapted for being tranſported to diſtant places: for by this quality they are fitted to undergo a long carriage, and to be kept a conſiderable time, without any diminution of their medicinal virtues. It muſt alſo be noticed, that the water of the under well is by much the beſt of the two for carriage, or for being long kept, as it is longer in ſeparating its mineral contents than the upper one.

From theſe experiments it is evident, that there is a conſiderable difference betwixt the waters of the two ſprings. The upper one contains a much larger quantity of the ochrous earth, and metalline cremor, than the under one; which is the reaſon, why it yields a deeper colour with galls, as may be obſerved in the firſt experiment. I ſuſpected, on the other hand, that the under water contained a greater proportion of alum, than the water of the upper ſpring; but this I cannot affirm, as I find I had neglected to make the experiment, which would have determined whether it be ſo or not. Tho' the mineral contents of theſe two waters be ſimilar, yet, if they be thus mixed in them in different proportions, this muſt certainly create a difference between them, which deſerves to be attended to, as it may be ſufficient to diſallow of their being uſed promiſcuouſly, ſince their medicinal effects may be thereby different.

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But now, to ſum up the evidence, which theſe experiments, taken all together, do afford, concerning the mineral ingredients of this Spaw; I think they determine, with ſome degree of certainty, that it contains two different principles of iron, both of which are fixed. The one, which is the ochrous earth, is a true *minera ferri*, and, altho' it be a crude mineral, exiſts in the water in a very fine and ſubtile form; the other, which is the cremor or pellicle, whoſe parts are alſo extremely attenuated in the water, appears to be iron, not in its mineral, but in its metalline form, and, when thrown up upon the ſurface of the water, ſhews itſelf like an extreme thin *lamina* of that metal. There ſeems alſo to be ſome ſmall proportion of ſulphur joined with the metalline cremor. The other mineral ingredient, which enters into the compoſition of this Spaw, is a conſiderable proportion of an aluminous ſalt, which is conjoined with a ſmall quantity of a light brown-coloured earth (probably a part of the matrix whence the ſalt is formed), and ſtill more intimately connected with ſome of the chalybeat parts of the water, which are not ſeparable from it either by elixation or evaporation. Whether theſe be ſaline or terreſtrial, I cannot determine.

Having thus endeavoured to diſcover, by ſome plain and ſimple experiments, the mineral principles, with which this medicinal water is impregnated; I ſhall now only add ſome obſervations, with reſpect to the origin of ſteel waters, and particularly of this Spaw, whoſe origin, I think, is thereby diſcovered and aſcertained in a very obvious manner.

Among

Among ſeveral things, that are ſtill deficient in the hiſtory of mineral waters, an exact knowlege of their origin ſeems to be the chief; that is, from what foſſils, and in what manner, theſe waters do acquire the mineral ſubſtances, with which they are impregnated. As this happens in the bowels of the earth, and is therefore far removed from our view, it is not ſurpriſing, that there has been ſo little diſcovered concerning it; tho' indeed there have been many elaborate hypotheſes framed in order to account for it.

The writers on mineral waters have been of very different and oppoſite opinions concerning their origin. They have diſagreed widely amongſt themſelves; and I very much ſuſpect, that the accounts, which moſt of them give of this matter, are not agreeable to truth: particularly with reſpect to chalybeat waters, I have ſeen none, who have given a ſatisfactory account of their origin. They have all agreed, that iron, or the vitriol of that metal, does exiſt in mineral waters; but they have never yet agreed, how they came to exiſt in them, or in what manner mineral waters come to be imbued with theſe foſſils.

Some of the more ancient writers cannot comprehend, how ſimple water ſhould be intimately impregnated with ſo many different kinds of minerals, except by the means of ſome powerful agent. And as they thought nothing more proper for communicating and mixing mineral ſubſtances with water, than violent heat, they therefore termed all mineral waters, of whatever kind, by the name of *thermæ*. They ſaw ſome ſpring from the earth extremely hot, others moderately hot, others tepid, others exceſſively cold:

cold: they concluded from this, that all ſuch various degrees of heat in theſe waters were owing either to the different degree of ſubterranean fire, which they had undergone; or elſe to the great diſtance, which ſome of them had run in the earth, after they had been ſufficiently heated. They therefore maintained, that thoſe waters particularly termed *acidulæ* (the greateſt part of which are impregnated with iron), or thoſe, which, tho' intenſely cold, contained a large proportion of mineral matter, had in ſome part of the earth been impregnated with it, by means of an intenſe heat, which they had been gradually deprived of by a long paſſage thro' the colder parts of the earth.

Some naturaliſts again, of a later date, having exploded the former notion as chimerical, have thought, that a vapour riſing from vitriolic minerals, and mixed with the neighbouring ſtreams of water in the bowels of the earth, has imbued them with ſome of the parts, and with the properties, of vitriol.

Others are of opinion, that the exhalations of vitriolic minerals, paſſing thro' the cavities of the earth, are there condenſed by the ſubterraneous cold into a limpid fluid, containing the very fineſt parts of that mineral ſalt: which fluid, mixing with the præterlabent ſtreams of water, and iſſuing out of the earth with them, produce thoſe mineral ſprings called vitriolic.

The laſt opinion I ſhall mention on this ſubject, and which indeed appears the moſt plauſible, is of thoſe, who think, that the iron is corroded and diſſolved in theſe waters by means of an acid: for, as they imagine ſimple water incapable of doing this,

they ſuppoſe, that it is firſt imbued with an acid in the bowels of the earth; and then, by the corroſion of the chalybeat minerals, thro' which it runs, it comes to be impregnated with them. I once received this opinion, as the moſt probable I could then obſerve, concerning the origin of theſe ſprings: yet not as being ſatisfactory; for there are many objections againſt it, which it is difficult either to elude or to anſwer.

The ſuppoſition of an acidity in ferrugineous waters, I thought but ill confirmed, becauſe, upon trial, they diſcover no veſtiges of it, but rather appear to be alkaline. Beſides, in conſidering the cauſes of mineral waters, it ſeems more probable, that whatever minerals they contain, they muſt be ſuch, as can be received or extracted by common water in its paſſage thro' the earth, by ſolution, abraſion, or the like ſimple operations; and in this way alone I think we may come to account not only for the commixtion of the ſaline and terrene minerals, which are found in medical waters, but likewiſe of thoſe, that are metalline or ſulphureous; for which ſimple water, at firſt ſight, may perhaps ſeem to be an inſufficient ſolvent.

It was this notion, that firſt induced me to make trial upon various mineral and metallic bodies, in order to know how far they could communicate their virtues to common water by infuſion. I thought this might throw ſome light upon the origin of mineral waters: yet, tho' I made a great many experiments of this ſort, and particularly upon ſeveral kinds of native chalybeat minerals, I was as little ſatisfied concerning their origin as before. I at length, however, met,

met, by accident, with what I had inquired after with ſo little ſucceſs.

As I happened to be at a gentleman's houſe near Edinburgh, in whoſe eſtate there was a great deal of coal, and who was at that time working a level or adit, in order to drain off the water, I obſerved, that the current of water, which flowed from this level, ſeparated a great quantity of ochre, and, emptying itſelf into a river ſoon after it came from the entry of the level, tinged all the ſtones and the channel of the river, for a good way, of an ochrous colour. The taſte of this water was exactly like that of a common ſteel Spaw; and it afforded a purple colour with galls *. As I knew, that this water flowed off a great body of coal, I often infuſed that foſſil, taken from the pits near this level, in common water; but the infuſions never yielded any tincture with galls. I tried in the ſame way another mineral, that the miners call *blaes*; which is a cliffery ſtratum of a blueiſh colour, that often lies both above and below the coal: alſo another foſſil of a brown colour, which is very ponderous, and is called by the miners *dogger*; a thin ſeam of which often lies in the midſt of the coal. However, neither of theſe would afford an infuſion, that would tinge with galls. At laſt I got another mineral out of theſe coal-pits, which is ſometimes found amongſt the coal, but is not ſo frequent as any of the former; and this fully

* Within two miles of this place there is a ſteel Spaw of good repute for the performance of ſeveral extraordinary cures, which gives the ſame tincture with galls, and appears in every reſpect to be the ſame with the water, that flows from this level.

 anſwered

anſwered my expectation. It is found either in round or broad pieces, is exceeding ponderous, and of a ſhining yellow colour, and is called by the miners *braſs lumps*. When I infuſed this mineral for a ſhort time in common water, it communicated to it all the properties of a ſteel Spaw; its taſte was exactly the ſame; and it received a tincture from galls, which was of a more diluted or intenſe purple, according to the proportion of the mineral added to the water, or to the time of the infuſion. This ſimple experiment does therefore clearly diſcover to us the origin of ſteel waters, and the manner, in which they are impregnated with their mineral contents in the bowels of the earth.

This obſervation, which I had made concerning the origin of ſteel waters, led me, when I firſt viſited Hartfell-Spaw, to inquire into the adjacent foſſils: which was the more eaſily done, as the ſtrata of the earth about the well, for a conſiderable depth, are expoſed to view. After ſome ſearch among theſe, I found a ſtratum of cliffery rock, about three or four feet thick, of a grey colour, and, I think, about twenty paces from the ſpring. In ſome of the hollow places of this rock, where the rain and wind did not reach, I obſerved a white ſaline efflorescence on its ſurface, which when I had taken off and taſted, I concluded, from its ſtyptic and chalybeat taſte, that it was a native vitriolum Martis, notwithſtanding its white colour; but I found it, upon trial, to be alum, having ſome fine attenuated parts of iron conjoined with it, and the ſame ſalt with that contained in the Spaw water.

Having

Having taken ſome pieces of this rock, which were quite free from the ſaline effervefcence; and infufed them in common ſpring-water for ſome hours, this water did thereby acquire the true taſte of the Hartfell-Spaw. It likewiſe in the ſame manner received a deep blue tincture with galls, and contained all the other qualities of that mineral water, without the leaſt difference, that I could obſerve: which, I think, aſcertains the true origin of this mineral ſpring in the moſt obvious and undeniable manner.

I am perſuaded, that this plain and eaſy method of inveſtigating the origin of mineral ſprings is not only ſuperior to the moſt learned diſcuſſions and elaborate theories, but will be found to be the ſureſt, yea, the only way of extending and compleating our knowlege concerning them. As I have not yet had the opportunity of making the experiments, which I deſigned, upon the two foſſils, that we find to be the cauſe of the above mineral waters, and which will be neceſſary to elucidate and compleat theſe obſervations, which we have made concerning their origin; I ſhall now only add one thing, and recommend it to the obſervation of others: "*Whether or* "*not, from ſuch a knowlege of the origin of mineral* "*waters, we may not acquire artificial ones of as* "*great, or perhaps of greater, medicinal uſe, than* "*thoſe, which are naturally produced?*"

XVIII. *An Account of the State of the Thermometer at the* Hague *on the 9th of* January 1757. *Extracted from a Letter of Mr.* Abraham Trembley, *F.R.S. to* Tho. Birch, *D.D. Secret. R.S.*

Hague, Febr. 15. 1757.

Read Mar. 3, 1757.

I Carefully obſerved the thermometer during the cold days, which we have had this winter. I made uſe of the ſame thermometer, with which I made my obſervations in 1740, and for that purpoſe fixed it in the ſame place, where it was that year, *viz.* in a window directly expoſed to the north, and open to a large ſquare. In 1740 I ſaw Fahrenheit's thermometer at two degrees below 0. This year, on the 9th day of January in the morning, it was at three degrees above 0; that is, only five degrees higher than in 1740.

XIX. *Experimental Examination of* Platina. *By* William Lewis, *M.B. F.R.S.*

PAPER V.

Read Mar. 17, 1757.

THE account of this extraordinary mineral, formerly read to this illuſtrious Society, and honoured with their approbation, being ſince publiſhed in the *Philoſophical Tranſactions*,

actions, renders any recapitulation of the diſcoveries hitherto made unneceſſary.

The near and remarkable relation betwixt platina and gold, not only in point of gravity, but in many leſs obvious properties, hitherto ſuppoſed to belong to gold alone; and their as manifeſt diſagreement in others, particularly colour, ductility, and fuſibility; induced me to examine, what effects they might have in combination with one another in different proportions; and whether there is reaſon to credit the report of great frauds having been committed by mixing them together; how far ſuch abuſes are practicable; and, what is of more importance, the means by which they are diſcoverable.

Experiments of the Mixture of Platina and Gold.

Experiment I.

1. Twelve carats * of fine gold, and the ſame quantity of the purer grains of platina, were urged in a blaſt-furnace, for near an hour, with a fire ſo ſtrong, that a ſlip of Windſor brick, with which the crucible was covered, tho' defended by a thin coating of pure white clay, had begun to melt. Upon breaking the veſſel, the metal was found in one ſmooth lump or bead; which, after being nealed by the flame of a lamp, and boiled in alum-water, appeared,

* The proportions were adjuſted according to the carat weights, as it is by theſe, that the fineneſs of gold is uſually expreſſed. A carat is the twenty-fourth part of the whole compound: thus gold of ſo many carats is a compoſition, of which ſo many twenty-fourths are fine gold, and the reſt an inferior metal.

both

both in the maſs, and upon the touchſtone, of a pale bell-metal colour, without any reſemblance to gold. It bore ſeveral ſtrokes, and ſtretched conſiderably under the hammer, before it begun to crack about the edges. On viewing the fracture with a magnifying glaſs, the gold and platina appeared unequally mixed; and ſeveral ſmall particles of the latter were ſeen diſtinct: nor was the mixture intirely uniform after it had again and again been returned to the fire, and ſuffered many hours of ſtrong fuſion.

2. Eighteen carats of gold and ſix of platina (=3 : 1) were melted together as the foregoing, in an intenſe fire continued about an hour. The bead, nealed and boiled, was leſs pale-coloured than the former, but had nothing of the colour of gold. It forged tolerably well, like coarſe gold. To the naked eye it appeared uniform; but a good magnifier diſcovered in this, as well as in the other, ſome inequality of mixture, notwithſtanding the fuſion was two or three times repeated, with the ſtrongeſt degrees of heat we were capable of exciting by large bellows.

3. Twenty carats of gold and four of platina (=5 : 1) were kept in ſtrong fuſion for above an hour and a half. Theſe united into an equal maſs, in which no granule of platina, or diſſimilarity of parts, could be diſtinguiſhed. The colour was ſtill ſo dull and pale, that the compound could ſcarcely be judged by the eye to contain any gold. It hammered well into a pretty thin plate; but we could not draw it into wire of any conſiderable fineneſs.

4. Twenty-two carats of gold were melted in the ſame manner with two of platina (=11 : 1) the

ſame that ſtandard gold contains of alloy. The mixture was uniform, of a tolerable golden colour, but eaſily diſtinguiſhable from that of ſtandard gold by a dingy bell-metal caſt. It worked well, was forged into a thin plate without cracking, and drawn into moderately fine wire.

5. Twenty-two carats and a half of gold, and one and a half of platina (= 15 : 1), melted into an uniform maſs, which, after the uſual nealing and boiling, proved ſomewhat tougher than the preceding, and of a better colour.

6. Twenty-three carats of gold were melted with one of platina; which is nearly half the proportion, that ſtandard gold contains of alloy. The compound worked extremely well, but was diſtinguiſhable from gold by a manifeſt dingineſs, which it retained after repeated forgings, fuſions, nealings, and boilings.

7. Twenty-three carats and one-fourth of gold, and three-fourths of a carat of platina (= 31 : 1), formed an equal mixture, very malleable, ductile like the three foregoing whilſt hot as well as cold, but not intirely free from their peculiar dingy colour.

8. A mixture of twenty-three carats and a half of gold, with half a carat of platina (= 47 : 1), was very ſoft and flexible, of a good colour, without any thing of the diſagreeable caſt, by which all the foregoing compoſitions were readily diſtinguiſhable, in the maſs as well as on the touchſtone, from fine or ſtandard gold.

9. A mixture of twenty-three carats and three-fourths of gold, with one-fourth of a carat of platina (= 95 : 1), could not be diſtinguiſhed by the eye or hammer from the fine gold itſelf.

In

In all these processes, even where the proportion of platina was small, the fusion was performed by a vehement fire, that the mineral might be the more intimately dissolved, and equally diffused thro' the gold. The necessity of this precaution appeared from an experiment formerly related; in which one of platina having been melted with four of gold, the button appeared not much paler than standard gold with silver alloy. On a second fusion it lost its yellow colour, which had at first been only external, from an imperfect mixture, great part of the platina being concealed in the internal part of the mass, and covered as it were by a golden coat.

The crucibles were rubbed on the inside with chalk, to prevent any particles of the metal from lodging in their cavities. A little borax was employed in each as a flux; with the addition of nitre, by which the colour of gold is somewhat heightened. On remelting some of the mixtures with sundry other additions, powdered charcoal seemed to improve the colour most.

Experiment II.

The preceding compositions, after being gently hammered and boiled, were weighed hydrostatically with great care, by a very tender balance, in distilled water, wherein the gravity of standard gold turned out 17.788.

All the mixtures proved heavier than standard gold. Their gravities were nearer to the medium of the gravities of the ingredients, than those of the compositions of platina with any of the other metals formerly

formerly given an account of; none falling confiderably ſhort of the mean gravity, and ſome rather exceeding it.

	Gravity. By Experiment.	Gravity. By Calculation.	Difference.	
Platina	17.000			
Platina 1 Gold 1	18.140	18.142	0. 02	Diminution.
Platina 1 Gold 3	18.613	18.714	0.101	Diminution.
Platina 1 Gold 5	18.812	18.904	0.092	Diminution.
Platina 1 Gold 11	18.835	19.094	0.259	Diminution.
Platina 1 Gold 15	18.918	19.142	0.224	Diminution.
Platina 1 Gold 23	19.089	19.189	0.100	Diminution.
Platina 1 Gold 31	19.128	19.213	0.085	Diminution.
Platina 1 Gold 47	19.262	19.237	0.025	Increaſe.
Platina 1 Gold 95	19.273	19.261	0.012	Increaſe.
Gold	19.285			

Experiment III.

As a mixture of platina with an equal quantity of gold has been reported to be ſpecifically heavier than gold itſelf, but turned out otherwiſe in the above experiments; ſome further trials were made on that head

1. Inſtead of the crude mineral, whoſe gravity is but 17, we took platina, that had been cupelled with lead, one of the neateſt of the buttons formerly mentioned, which, tho' retaining a portion of the lead, was nearly as ponderous as fine gold, *viz.* 19.240. This was melted with equal its weight of the gold, in a ſtrong fire, and continued in fuſion for about an hour: the maſs proved ſpongy, and very light. We

remelted it ſeveral times with vehement degrees of fire, ſuffering it to cool leiſurely in the crucible; and, in order to ſeparate as much as poſſible of the lead, to which its ſponginefs ſeemed owing, boiled it in aqua-fortis, and repeatedly injected corroſive ſublimate upon it during fuſion: the maſs, nevertheleſs, ſtill turned out cavernulous and brittle, and ſpecifically lighter than either the gold or platina by themſelves.

2. I likewiſe endeavoured to combine platina with ſmall proportions of gold. By vehemence of fire, it was made to unite, tho' not perfectly, with half its weight and leſs: but the mixtures were extremely ſpongy and brittle; in ſpecific gravity one ſcarce 16, another leſs than 15.

3. As a caſt metalline body from the Spaniſh Weſt Indies, of which ſome account will be given hereafter, appears to have been confounded with the mineral platina, this alſo was melted with an equal quantity of gold. They united with great eaſe, by a moderate fire, into an uniform compound, tolerably compact, but whoſe ſpecific gravity was only $16\frac{1}{2}$; which is nearly the mean gravity of the two ingredients.

Experiment IV.

As a ſmall portion of copper ſomewhat heightens the colour of pale gold, platina was melted with eight times its weight of ſtandard gold made with copper alloy. The fuſion was performed, as in the preceding experiments, in a cloſe crucible, with a ſtrong fire, but without any flux, and continued for about an hour. The metal appeared covered with a black

black ſcurf, and had loſt about $\frac{1}{100}$. It was much duller coloured, harder to the hammer, and cracked ſooner about the edges, than mixtures of fine gold with a larger quantity of platina. By repeated fuſion, and frequent nealing, it became a little ſofter and tougher, ſo as to be drawn into pretty fine wire; but the colour was ſtill exceeding dull, more reſembling that of bad copper than of gold.

The ſpecific gravity of this compound was 17.915; a little leſs than the medium of the three ingredients unmixed, and a little greater than the mean gravity reſulting from the platina by itſelf, and the copper and gold mixed; for copper, in the ſtandard proportion, appears to diminiſh the gravity of gold more than it ought to do according to calculation.

From the foregoing experiments it appears, that platina is miſcible with gold, in certain proportions, without injuring either its colour or ductility, or occaſioning any conſiderable alteration in the gravity: experiments related in former papers have ſhewn, that it ſtands aqua-fortis, and the other trials by which the purity of gold is eſtimated. It is to be hoped, that the abuſes manifeſtly practicable by this mineral have hitherto been but rarely made uſe of. To guard againſt them is the object of this paper; to detect them, of the next.

XX. *Experimental Examination of* Platina. *By* William Lewis, *M. B. F. R. S.*

PAPER VI.

Experiments of diſtinguiſhing and purifying Gold mixed with Platina.

1. *By Amalgamation with Mercury.*

Read Mar. 31, 1757.

IN an experiment related in the fourth paper, an amalgam of one part of platina and two of gold with a ſuitable quantity of mercury, having been triturated with water for a conſiderable time, and occaſionally waſhed over, the platina was gradually thrown out, and the gold retained by the quickſilver.

Repetitions of this experiment have ſhewn, that tho' the ſeparation ſucceeds in ſome caſes, it does not perfectly in all: that if there is any particle of the platina imperfectly diſſolved in the gold (which will generally be the caſe, unleſs the quantity of gold is three or four times greater than that of the platina), this part will be retained, after long trituration, undiſſolved by the mercury, uncomminuted by the peſtle, and too ponderous to be waſhed off in its groſs form. A variety of mixtures of platina and gold were treated in the manner above deſcribed; and the gold, recovered from the amalgams, ſubmitted to further examinations. Where the proportion of platina was large, the microſcope almoſt always diſcovered ſtill ſome granules of it on the fracture of the

the ingot: where the proportion was ſmall, the recovered gold was frequently, but not conſtantly, found to be pure.

From theſe experiments it appears, that mercury has a greater affinity with gold than platina, and that platina is capable of being totally ſeparated by elutriation; but that the proceſs is too vague and undetermined to be applicable in the way of aſſay, as we have no mark of the preciſe time for diſcontinuing it, and as we can never be certain, without making another aſſay, whether the whole of the platina is ſeparated or not. As a preparatory examination, where the quantities of platina and gold to be ſeparated are large, it is nevertheleſs of good uſe, as greateſt part of the platina may by this means be waſhed over with little trouble, and the gold brought into a leſs compaſs, ſo as to be commodiouſly ſubmitted to a perfect purification by the means hereafter pointed out. This proceſs has a ſimilar effect on platina and gold to that of ſtamping and waſhing on metallic ores; which could not be reduced into pure metal in the furnace to advantage, without the previous ſeparation of great part of the earthy and ſtony matter by water.

2. *By Precipitation with Alkalies.*

Gold is precipitated totally by fixed alkaline ſalts, but platina only in part. When ſolutions of the two metals are mixed together, ſo much of the platina remains ſuſpended, after ſaturation with the alkali, as to be readily diſtinguiſhable by the yellow colour, which it communicates to the liquor. It has been objected,

objected, that tho' the platina was diſcoverable, when thus mingled ſuperficially with the gold, it may nevertheleſs, when combined more intimately by fuſion, elude this method of trial.

1. Mixtures of gold with ſmall proportions of platina were therefore kept in fuſion, by a very ſtrong fire, for ſeveral hours, and afterwards diſſolved in aqua-regis. The ſolutions being diluted with water, and a pure fixed alkaline ſalt gradually added, ſo long as any effervescence or precipitation enſued, the liquors remained manifeſtly coloured, tho' apparently paler than when the two metals had been diſſolved by themſelves.

2. A more convincing proof, that part of the platina remains ſuſpended, after the precipitation of the gold, was obtained, by putting into the filtered liquors ſome plates of pure tin, which preſently contracted an olive hue, and threw down a large quantity of a brownish precipitate, as from the common ſolutions of the crude mineral. It was obſervable, that the tin plates were often ſenſibly acted on, even whilſt the liquor was overcharged with alkali.

3. It has been further ſuggeſted, and with great appearance of probability, that as a part of platina is precipitated as well as gold by alkaline ſalts, if only this part be mixed with gold, it will be thrown down by them again upon diſſolving the compound. To determine this point, a precipitate of platina made by fixed alkali was melted with thrice its weight of fine gold, and kept in ſtrong fuſion for above an hour: they united more eaſily than gold does with ſo large a proportion of the crude mineral, and formed a ſmooth neat bead, which hammered well

into

into a pretty thin plate before it cracked, and appeared internally uniform and equal. This compound being diſſolved in aqua-regia, and a fixed alkaline ſalt added by degrees till the acid was more than ſaturated, the liquor became indeed pale; but tin plates put into it quickly diſcovered, that it held a very conſiderable quantity of platina. It appears therefore a conſtant property of this mineral to remain partially diſſolved in the neutraliſed liquor; and that minute proportions of it, mixed with gold, are by this means diſtinguiſhable.

4. Many other experiments were made of the precipitations and precipitates of gold and platina, by alkalies both of the fixed and volatile kind. The moſt remarkable effects were, that volatile alkalies, added to both ſolutions in quantity juſt ſufficient to ſaturate the acid, precipitated gold intirely, but platina only in part, ſo much of it remaining ſuſpended as to give the ſame colour to the liquor as when fixed alkalies were made uſe of: that, on adding a larger quantity of the ſpirit after the precipitation of the gold, the liquor became yellow, a part of the metal being taken up again; and that the platina was more copiouſly rediſſolved, the liquor becoming of a deep browniſh red: that the waſhed precipitates of both metals, whether made by volatile or fixed alkalies, proved ſoluble, by moderate digeſtion, in ſpirit of ſalt; thoſe of platina much more difficultly and ſparingly than thoſe of gold.

3. *By inflammable Liquors.*

1. Inflammable ſpirits, which revive gold from its ſolutions in form of yellow films, have no ſuch effect

on ſolutions of platina. This experiment affords not only a criterion for diſtinguiſhing with certainty whether gold has been debaſed by platina, but likewiſe an infallible means of recovering it perfectly pure from any admixture of that mineral. If the compound be diſſolved in aqua-regis, the ſolution mingled with twice its quantity or more of the ſpirit, and the mixture ſuffered to ſtand for ſome days in a glaſs ſlightly covered; the whole of the gold ariſes to the ſurface, leaving the whole of the platina diſſolved. The golden pellicles may be collected, by pouring the matter into a filter juſt large enough to contain it. The diſſolved platina paſſes thro', leaving the gold upon the paper, which is to be waſhed with freſh parcels of water till the liquor runs colourleſs. The paper is then to be carefully ſqueezed together, and burnt in a crucible previouſly lined with vitrefied borax: when fully ſunk down, a little freſh borax is to be injected, and the fire raiſed to melt the gold. The uſe of lining the crucible with borax is to prevent any moleculæ of the gold from lodging in its cavities.—This proceſs is attended with one inconvenience, the ſlowneſs of the ſeparation of the gold from the ſolution: this may be in ſome meaſure expedited by employing a ſpirit, which has been diſtilled from vegetables, that give over an eſſential oil.

2. As eſſential oils take up gold from aqua-regis, and keep it diſſolved for a time upon the ſurface of the acid; a pure colourleſs oil, that of roſemary, was poured into about half its quantity of a ſolution of platina, the mixture well ſhaken, and ſuffered to reſt: the oil quickly aroſe, without taking up any thing from the platina, or receiving any colour: the acid

liquor

liquor underneath remained coloured as at firſt. Compoſitions of platina and gold being diſſolved in aqua-regis, and treated in the ſame manner, the whole of the gold was imbibed by the oil, and the whole of the platina remained diſſolved in the acid. The oil, loaded with the gold, appeared of a fine yellow colour, and, on ſtanding for a few hours, threw off great part of its contents, in bright yellow films, to the ſides of the glaſs. Sundry other diſtilled oils were made trial of, with the ſame event. The gold is eaſily recovered, by ſetting the oil on fire; and, when thoroughly burnt out, melting the reſiduum with borax, as in the preceding experiment. After the ſeparation of the oil employed at firſt, it may be proper, for the greater ſecurity, to add a little more; which, if any part of the gold ſhould happen to have been left in the liquor, will effectually take it up.

3. The experiment was repeated likewiſe with the ſubtile fluid, prepared from vinous ſpirits with the vitriolic acid, called by the chemiſts æther. The ſeparation ſucceeded in the ſame manner as before; the æther receiving nothing from pure platina, but inſtantly taking up the gold from a mixture of the two. It is obſervable, that the gold imbibed by this fluid is kept permanently diſſolved by it; without ſeparating or reviving, as it does from the common eſſential oils and vinous ſpirits.

4. The liquors remaining in theſe experiments, after the extraction of the gold, appear on all trials the ſame with the common ſolutions of platina; and readily betray their being impregnated with that mineral by their colour, by the precipitation with

tin, by their yielding a ſparkling red precipitate with volatile ſpirits, *&c.* A far more minute proportion of platina, mixed with gold, is more diſtinguiſhable by theſe proceſſes, than by thoſe with alkaline ſalts above-mentioned; theſe exhibiting the whole of the platina diſſolved by itſelf, thoſe only a part of it.

4. *By metallic Solutions.*

All the metals, which precipitate gold from aqua-regia, have been already ſhewn to precipitate platina alſo. As gold is thrown down by ſome metallic ſolutions, as well as by the metals in ſubſtance, particularly thoſe of mercury and iron, it remains to apply theſe liquors as precipitants for platina.

1. A ſaturated ſolution of mercury in aqua-fortis, which readily and totally threw down gold in its metallic form, being added to a ſolution of platina, the liquor became immediately turbid, and, on ſtanding for a little time, nearly the whole of the platina fell to the bottom. A ſolution of mercury in the marine acid, or of corroſive ſublimate, likewiſe precipitated platina, but leſs perfectly, and with this difference, that the former precipitate was of a greyiſh brown colour, the latter of a ſparkling red.

2. Solutions of iron in the vitriolic acid, or of common green vitriol in water, which totally threw down gold, happily made no change in ſolutions of platina. Compoſitions of platina and gold being diſſolved in aqua-regis, the ſolutions diluted with about twice their quantity of water, and a filtered ſolution of the vitriol gradually added; the mixtures inſtantly grew turbid, and, on ſtanding, depoſited the gold in form

form of a purpliſh grey calx, the whole of the platina remaining diſſolved. It appeared, on numerous repetitions of this experiment, that no part of the platina was precipitated along with the gold, nor any of the gold kept ſuſpended with the platina. Where the quantity of the mixt to be aſſayed was very ſmall, the precipitation was uſually performed in a filter, that the gold, which ſeparates in very minute moleculæ, ſome of which might poſſibly remain unobſerved in the bottom of a glaſs, might be detained on the paper. The colourleſs ſorts of filtering-paper are preferable for this uſe to the coloured; as theſe laſt may be impregnated with aſtringent matter, which would extricate ſome of the ferrugineous part of the vitriol. The vitriol was diſſolved in about ſix times its quantity of water, and a few drops of oil of vitriol added, to prevent the ſeparation of any of its iron in the filter. This ſolution was put into the filter firſt, the ſolution of gold and platina immediately poured into it, the whole ſtirred together with a clean glaſs rod, and ſuch part of the liquor, as had run thro' before they had been duly mixed, poured back to the reſt. The gold remaining in the filter was waſhed with freſh parcels of water, the paper cautiouſly rolled up, and burnt in a crucible, as mentioned in a former experiment.

3. Solutions of the vitriol, recommended by Kunckel and others for precipitating gold of an uncommonly high colour, made no change in the ſolutions either of gold or platina. The bluiſh green did indeed precipitate the gold; not as blue vitriols, but by virtue of the ferrugineous matter, of which theſe kinds largely participate. White vitriol was likewiſe

made trial of, but without producing any ſenſible effect in either ſolution.

4. The experiments with green vitriol were repeated on the ſolutions of platina and gold made in ſpirit of ſalt. The event was the ſame as with thoſe made in aqua-regis; the gold being conſtantly precipitated, and the platina remaining diſſolved.

Remarks.

It may be proper to obſerve, that by the proceſſes here pointed out, the gold is purified from other metallic admixtures at the ſame time that it is ſeparated from platina; the inflammable ſpirits reviving, eſſential oils and æther imbibing, and green vitriol precipitating, gold alone. Care ſhould be had, that the piece of the mixt, taken for examination, be totally diſſolved before any trials are made with the ſolution; the menſtruum not acting with equal facility on the two metals, but diſſolving the gold more readily than the platina. Where the acid has been dilute, and only a gentle heat applied, great part of the gold has appeared to be taken up before the platina was conſiderably acted on. Where the filter, with the gold in it, is burnt in the crucible, borax is the moſt commodious flux: but as this ſalt gives a ſenſible paleneſs to gold, a little nitre may be injected, after the metal has come into fuſion, to reſtore its colour. If the nitre was added at firſt, whilſt the gold continues ſubtilely divided, ſome particles of the metal would be diſſipated during the deflagration, which that ſalt produces with the coaly remains of the paper.

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As the foregoing experiments exhibit platina and gold diſſolved in a mineral fluid, which by ſimple mechanic agitation rejects the one and retains the other, and which diſcovers this different appetite of union ſo much the more remarkably, as the two metals have been the more intimately combined: — as they further exhibit platina diſſolved in liquors incapable of holding gold ſuſpended, — gold diſſolved in liquors incapable of holding platina ſuſpended,--- gold totally precipitated by ſubſtances, which precipitate no particle of platina,--- and gold, when mixed *per minima* with platina, perfectly recovered from it by theſe means, without increaſe as well as without diminution: --- it follows, that platina is not, as ſome believe, gold naturally debaſed by the admixture of ſome other metallic body, but a metal of a peculiar kind, eſſentially different from all the others. Before the diſcriminating characters of platina were diſcovered, ſuch a notion was highly plauſible, and direct experiment ſeemed to confirm it: a portion of the platina might be ſeparated in the proceſs; a quantity of gold mixed with the remainder, in order to collect the gold ſuppoſed to be contained in it; the mixture ſubmitted to operations, which gold alone was ſuppoſed capable of withſtanding; and the augmentation, which the noble metal ſtill retained, held to be true gold gained from the platina.

The methods of trial above related will, it is preſumed, be ſufficient to undeceive thoſe, who may have been impoſed upon by ſuch appearances, and betrayed into the practice of unintended frauds: to convince them, that all they have gained from platina, after the moſt laborious attempts to diveſt it of

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its imaginary coat, is no other than platina ſtill: and, which is of more extenſive utility, to diſtinguiſh all the abuſes, that may be made with this metal, and reſtore the gold, ſo debaſed, to its original purity and value.

XXI. *An Account of the Temple of* Serapis *at* Pozzuoli *in the Kingdom of* Naples: *In a Letter to* John Ward, *LL. D. and R. S. Vice-Præſ. by the Rev.* John Nixon, *M. A. F. R. S.*

SIR,

Read Mar. 17. 1757.

BEFORE we enter upon a more particular conſideration of this noble piece of antiquity, it may not be improper to premiſe the general account (and indeed the only one I have met with yet publiſhed), which is given of it by Meſſ. Cochin and Bellicard, in a little (1) treatiſe printed at Paris in 1755. Theſe gentlemen acquaint us, that in 1749 there were only three pillars of this building viſible, and that they were buried half way within the ground: but that ſoon after, workmen being employed by order of the King of the Two Sicilies to dig at the place, they came to the pedeſtals of thoſe pillars; and at length diſcovered the building to have been a temple, which

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(1) Obſervations ſur les Antiquités d'Herculaneum, &c. p. 82.

(as

(as it was judged by the principal (2) idol found there, and ſome other circumſtances) was dedicated to Serapis. They tell us further, that many ſtatues and vaſes of excellent workmanſhip had been taken out of the ruins; and that the whole temple was extremely magnificent, being built, or caſed throughout, with marble, even to the parts appropriated to the meaneſt offices.

This account, tho' ſhort, is yet ſufficient to excite in the curious a deſire to be more fully informed, both as to the ancient and modern ſtate of this place. To gratify in ſome meaſure that deſire is the purpoſe of the preſent letter.

In order to form any conjecture concerning the antiquity of the building before us, we muſt know, that the worſhip of Serapis, to whom it is ſuppoſed to have been conſecrated, was not introduced at Rome till towards the end of the republic; and then tolerated in the ſuburbs only (3). However, at length he was allowed to have temples erected to him within the precincts of the city; chiefly by the authority of Veſpaſian, who was thought to have reſtored a blind man (4) to his ſight at Alexandria by the aid and direction of this deity. And upon this account he continued to be held in high veneration by Titus and Domitian, the ſons of that Emperor, as appears by their (5) ſtamping his

(2) For a more particular account of this ſtatue, now in the palace at Portici, I beg leave to refer you to a paper of mine read before the Royal Society on Feb. 24. laſt.

(3) Dio. L. xl.

(4) Suet. Veſp. c. 7.

(5) For authorities, ſee Middleton's Germana Antiq. Mon. p. 152.

image

image on the reverſe of their coins. Now as it is reaſonable to ſuppoſe, that the other cities of Italy followed the example of the metropolis in this inſtance, as well as, we find, they did in others of a ſimilar nature; we may with ſome probability place the foundation of this temple at Pozzuoli ſomewhere within the period aſſigned above.

As for the particular ſtate of this building, it is ſituated on the weſt ſide of the town, near to, and upon a level with, the beach (*See* TAB. II.). Its grand entrance is towards the ſouth, and ſeems to have been a veſtibule ſupported by four columns. This introduces you into a ſpacious portico, or corridor (6), which was deſigned to defend ſuch as aſſembled here to worſhip from the injuries of the weather; as alſo to afford a commodious paſſage into a range of rooms of different dimenſions, diſpoſed on all the four ſides of the court.

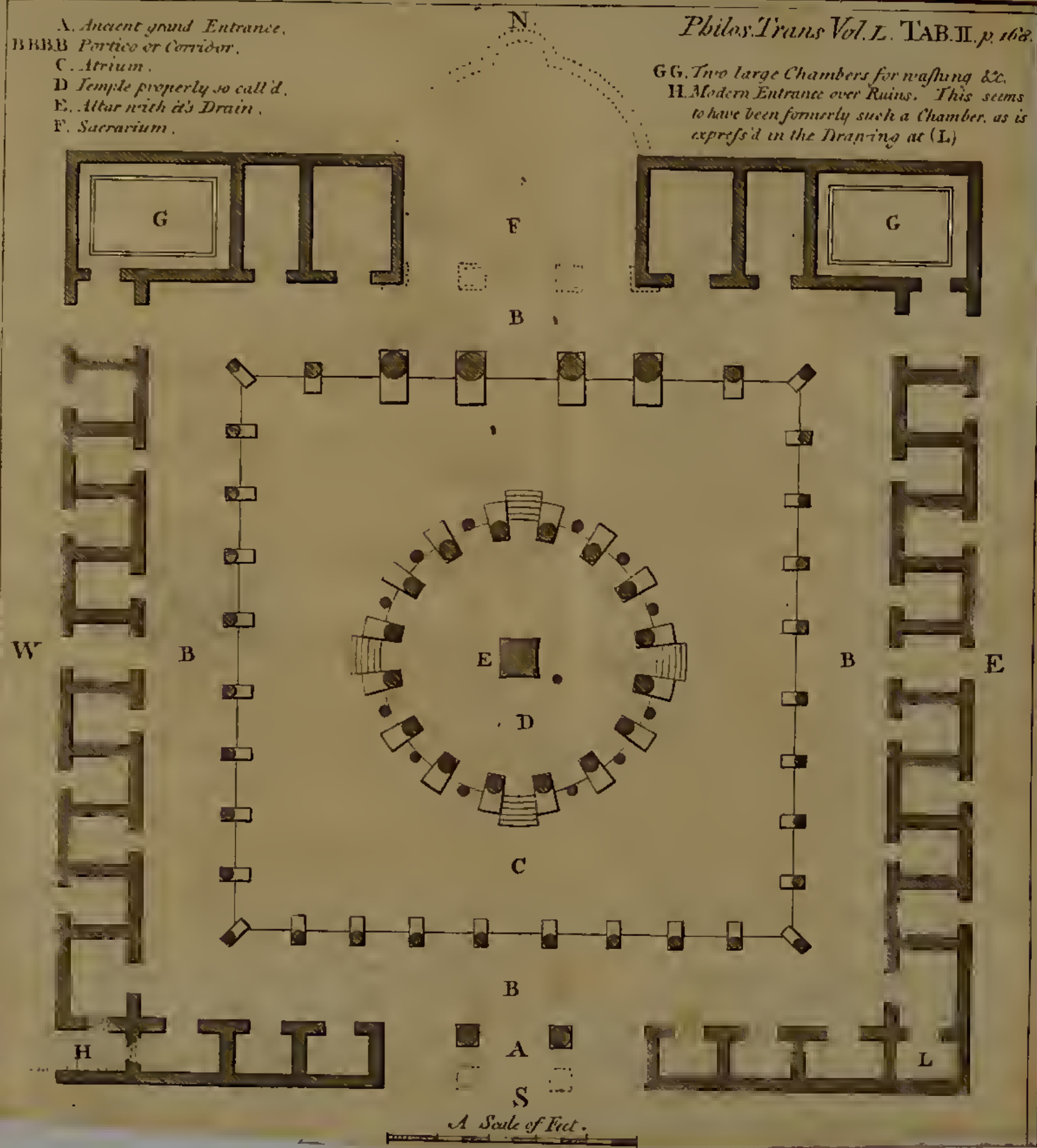
Theſe chambers ſeem deſigned for preparing the ſacrifices, lodging the prieſts, and keeping their veſtments; as alſo the fuel, ſtores, and other things requiſite for the ſervice of the temple: not to omit the convenience of purifying both the prieſts and the

(6) Pliny (L. ix. ep. 39.) acquainting his architect with his purpoſe to repair a temple of Ceres, which was upon his eſtate, ſays, *Nullum in proximo ſuffugium aut imbris, aut ſolis. Videor ergo munifice ſimul religioſeque factûrum, ſi ædi, quam pulcherrimam exſtruxero, addidero porticus: illam ad uſum deæ, has ad hominum.* That theſe portico's commonly incloſed the whole ſite of the ancient temples, as in this at Pozzuoli, ſeems implied in what follows: *Quantum ad porticus, nihil interim occurrit, quod videatur eſſe iſtinc repetendum: niſi tamen, ut formam ſecundum rationem loci ſcribas; neque enim poſſunt circumdari templo: nam ſolum templi hinc flumine — hinc viâ cingitur.*

worſhippers

A. Ancient ground Entrance.
BBBB Portico or Corridor.
C. Atrium.
D Temple properly so call'd.
E. Altar with it's Drain.
F. Sacrarium.

GG. Two large Chambers for washing &c.
H. Modern Entrance over Ruins. This seems to have been formerly such a Chamber, as is express'd in the Drawing at (L)



worſhippers by bathing or waſhing. This laſt deſtination is countenanced, with regard to the chamber on the north-weſt and that on the north-eaſt corner, by the row of ſtone ſeats, which ſtill remains on each of the ſides of the former. Theſe ſeats have a gutter, or channel running along at the foot of them on the floor; and are likewiſe perforated with holes of a proper ſize, with funnels paſſing from them below. On theſe benches probably the perſons to be purified placed themſelves, that the water might be let out upon them from pipes; or adminiſtred in vaſes or ewers by the attendants, and afterwards be carried off by the paſſages mentioned above (7).

Nor can a proviſion for waſhing or bathing in this temple ſeem ſtrange to any one, who reflects, how high a rank this mode of purification held among the religious ceremonies of almoſt all nations of the world. As for the Romans, with whom we are principally concerned in the preſent inquiry, the ſubſiſtence of this uſage among them might be abundantly ſhewn by the teſtimonies of their writers; and alſo by the accommodations provided for it in other buildings of the ſame character with that before us. Some of theſe ſtill remain within the neighbourhood of Pozzuolo, *viz.* the magnificent temple near the lake of Avernus aſcribed to Apollo, which has an apartment adjoining to it indiſputably intended for the purpoſe

(7) Meſſ. Cochin and Bellicard ſeem to think this room was intended for another purpoſe, by their calling the funnels under the holes in the ſeats of it, *conduits des foſſés d'aiſance.* Which of the two hypotheſis's is to be preferred, I ſubmit to the judgment of the learned; or rather, whether both of them may not be admitted, as in no-wiſe incompatible the one with the other.

intimated above. For it is furniſhed with ſeveral ſtone ciſterns, whoſe inward dimenſions are proportioned to the ordinary ſize of an human body; and near them is a ſpring, out of which the water was taken up, and poured into a baſon hollowed out in the ſide of the wall: from thence it ran along in a groove or channel cut in the ends of the ciſterns, to be let out upon the perſons bathing in them, according to their pleaſure, or as occaſion ſhould require. So likewiſe in the temple of Venus (as it is commonly called) near Baiæ, there is a large chamber containing ſeveral ſtone ſeats for waſhing, with little cells contiguous to it for undreſſing before, and anointing the body and dreſſing afterwards.

I have but one more particular to add concerning the apartment in the north-weſt angle of the temple, *viz.* that, when it was cleared of its rubbiſh, there was found in a niche in one of its ſides a male and female figure naked, and in the moſt flagrant act of natural lewdneſs. It is now (as we were informed) in a private room in the palace at Portici, nor can be ſeen without the King's ſpecial permiſſion. In the ſame place, probably, may ſtand the ſtatue of a ſatyr in an unnatural action with a goat, which was found at Herculaneum, and is, they ſay, of exquiſite ſculpture, but concealed in the palace above-mentioned with the ſame ſtrict care as the former.

Having thus viewed the ſeveral chambers in the exterior parts of the building, it is requiſite (in order to a regular proſecution of our deſign) to return to the grand entrance. And here, paſſing thro' the corridor above deſcribed, we come to a ſquare court or *atrium* paved with large ſlabs of white marble

ſtreaked

ſtreaked with blue or greyiſh veins. At the diſtance of 25 feet further, in the center of the ſaid court, ſtood the temple properly ſo called, containing a circular area of 54 feet diameter, and elevated above the level of the pavement, ſo as to admit an aſcent to it of five ſteps (8), in four different parts anſwering to the four ſides of the corridor. This area is ſurrounded with ſixteen pedeſtals, on which formerly were columns to ſupport a rotundo or dome. Againſt each of theſe columns, on the outſide, there ſeems to have been placed a ſtatue, and, in the intermediate ſpaces, vaſes for incenſe, or lavers for waſhing, upon low ſtands on the floor. In the middle of the temple was erected the grand altar, the traces of which ſtill remain, with a ſink or drain near it to receive and carry off the blood of the victims, *&c.*

Northward of the temple, and at the diſtance of 25 feet, being the ſame ſpace, that was between it and the corridor at the entrance, was once another ſtately veſtibule or pavilion, ſupported by four columns four feet and an half in diameter, and of the Corinthian order, as appears by three of them, which ſtill ſubſiſt ſtanding in a line with the outer face of the corridor. This pavilion (if we may judge by analogy from what we find in other temples) led to an inner receſs or *ſacrarium* terminating, probably, in the ſegment of a circle: but of this we had no certain proof, as the rubbiſh was not yet removed from this part of the building.

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(8) Vitruvius, Lib. iii. cap. 3. *Gradus in fronte ita conſtituendi ſunt, uti ſint ſemper impares: namque cum dextro pede primus gradus aſcendatur, item in templo primus erit ponendus.*

I beg leave further to mention a remarkable appearance in ſome of the columns of this temple, *viz.* that that part of them, which was lowermoſt, as well as that, which was neareſt the capitals, is well preſerved and pretty entire; while part of the intermediate ſpace for two or three feet together is diſcoloured, as if it had ſuffered by burning; and alſo excavated in ſuch a manner (9), as to contain multitudes of little ſhell-fiſh, which appear, like the pholades in ſome ſtones, almoſt totally incloſed within their cells, ſo as not to be got out without breaking. I know no way to account for this ſo probable, as by ſuppoſing, that the lower parts of theſe columns were ſecured by the maſs of rubbiſh, that incloſed them, as the uppermoſt were by their elevated ſituation, from being perforated either by the corroſive quality of the ſea, which (according to tradition) formerly covered the ſite of this temple; or by the animalcula, which are bred in that element; while the middle parts ſtanding in the water were (perhaps for ages) expoſed to the injuries mentioned above. I had no opportunity of taking the height of the uppermoſt line, where the above-mentioned

(9) The learned Abbate de Venuti, F.R.S. and Antiquary to the Pope at Rome, has lately by letter favoured me with an ingenious account of this phænomenon; tho' he mentions the granite columns only as affected by it. *Cùm columnæ, quæ circumibant templum, excavarentur e terrâ, quâ erant partim obrutæ —— minutiſſimæ conchæ, quæ ex teſtaceorum genere ſunt, atque in ſaxorum rimulis prope mare reperiuntur, ideoque a vulgo* Trutti di Mare *appellatæ, columnas haſce (i. e. Thebaicas) quam ſæpiſſimè perforaverant, ſeſe componentes, veluti apes in alveari, cùm eſſent ſejunctæ integumentis ex ipſo lapide ſubtiliſſimis.* ——

alteration

alteration in the columns ended, from the level of the ſea in the bay; which would have ſhewn how high the water muſt have riſen formerly above its preſent mark, to produce the effect aſcribed to it on this hypotheſis. But, however that may be, the nature of the ſituation of this place (10) being conſidered, the innovation ſuppoſed to have happened in it will not, I preſume, be thought improbable; eſpecially in a country ſo plentifully ſtored with combuſtible matter in its interior parts, and conſequently ſo liable to changes in its outward form, as this is, and has been for many ages. For an extraordinary inſtance of this we need go but a little way from this place, *viz.* to Il Monte Nuovo, a hill about four miles in compaſs, which in 1538 was caſt up in one night's time by an eruption, whereby the greateſt part of the Lucrine lake was filled up, and the town of Tripergola, with a church, convent, hoſpital, and other buildings, intirely buried.

I ſhall conclude with acquainting you, that at the corner of the court of this temple, near the preſent entrance, there lie ſome large baſes of marble, which (as we were informed) were taken out of the ſea at about a mile's diſtance from Pozzuolo, and are inſcribed DVSARI SACRUM. What was the original ſituation of theſe monuments, it may not be poſſible now to aſcertain. As for DVSARES mentioned in the inſcription upon them, G. Voſſius (11) (upon

(10) Vid. p. 168.

(11) Nec alius a Διονύσω ſive ſole eſt Δυσάρης, ſive Δεσάρης, vel Δευσάρης. Quæ vox (ut ſuſpicor) conflata ex דוץ *Dutz* et ארץ *Aretz.*

(upon the authority of Tertullian, and Stephanus Byzantius) makes him to have been an Arabian deity, the ſame as Bacchus or Sol according to the Roman theology. That learned man is likewiſe inclined to think, that the name DVSARES is compounded of two Hebrew words; one of which ſignifies *joy*, and the other, *the earth*, i. e. mortals, who inhabit it. This etymology properly expreſſes the genial effects of the ſun, which makes glad the heart of man by ripening the fruits of the earth, eſpecially the grape: Whence Virgil calls wine,

Munera lætitiamque Dei. Æn. i. *v.* 640.

and ſtiles Bacchus the fabled inventer of it,

—— *Bacchus lætitiæ dator.* Ib. *v.* 738.

I am,

S I R,

With the greateſt reſpect,

Your moſt obedient humble Servant,

London,
March 14. 1757.

J. Nixon.

Aretz. Quorum prius *(gaudium)* alterum notat *(terram)* ut notet *lætitiam terræ*, ſive mortalium. Nam Liber ſive ſol lætitiâ implet mortales, maturando fruges, et uvas, unde de vino ſic Maro,

Munera lætitiamque Dei. Æn. i.

Duſarem verò eſſe Arabum numen indicat locus ille Tertulliani in Apolog. c. 24. " Unicuique etiam provinciæ et civitati ſuus deus " eſt. Ut Syriæ Aſtartes; *ut Arabiæ Diſares.*"

Etiam apud Stephanum ſive Hermolaum Byzantium — Δυσάρη —σκόπελος καὶ κορυφὴ ὑψηλοτάτη Αραβίας. Ἔιρηται δὲ ἀπὸ τοῦ Δυσάρου. Θεὸς δὲ οὗτος παρὰ Ἄραψι καὶ Δαχαρηνοῖς τιμώμενος —— Voſſius de Idololat. L. ii. c. 8.

XXII.

XXII. *Some Remarks on a* Parthian *Coin with a* Greek *and* Parthian *Legend, never before publiſhed: In a Letter from the Rev.* John Swinton, *M. A. of* Chriſt-Church, Oxon. *F. R. S. to the Rev.* Thomas Birch, *D. D. Secret. R. S.*

Reverend Sir,

Read Mar. 24, 1757.

AS we know little of the reigns of the latter Parthian kings, and leſs ſtill of their coins; an attempt to ſtrike out even the leaſt new light on either of thoſe heads will not, I perſuade myſelf, prove unacceptable to the Royal Society. In this perſuaſion, I do myſelf the honour to ſend you a few conjectures upon another braſs Parthian medal, in my poſſeſſion, as remarkable, on account of the double legend it contains, as the former, which I endeavoured a little to illuſtrate about a year ago. This medal, which certainly merits the attention of the curious, is in very good conſervation, and approaches near the ſize of thoſe of the middle Roman braſs. It exhibits the head, or effigies, of a Parthian king, with a beard, diadem, and hair formed into ſuch curls as I never yet obſerved upon any antient coins. Under the effigies, the Greek letters ΒΑCΙΛЄωΝ ΜЄΓΑC ΜΟ⁝⁝⁝⁝⁝, which demonſtrate the piece to be Parthian, appear; and, on the reverſe, a *Victory*, done ſomething after the Roman manner, tho' the workmanſhip is pretty rude, preſents itſelf to our view, together with a legend in a language and

and character at this time unknown. The legend consists of ten complete elements, placed behind the *Victory* above-mentioned; besides which there is one in the field of the medal, being probably the initial letter of the name of the city where the coin was struck. The metal, tho' termed by me brass, discovers something of a composition similar to that of his Grace the Duke of Devonshire's medal of Vologeses III. as described by (1) Sig. Haym. This coin, of which I herewith transmit you a draught most accurately taken (*See* TAB. IV. *Fig.* 1.), having not yet, as I apprehend, been published; you will permit me now to offer a few cursory remarks upon it, drawn up in the shortest and most concise manner possible.

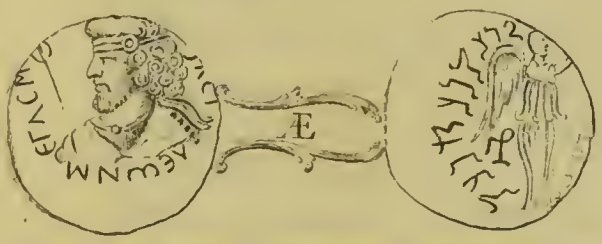
1. The Greek legend, had all of it appeared, would undoubtedly have been either ΒΑϹΙΛΕΩΝ ΜΕΓΑϹ ΜΟΝΝΗϹΗϹ, or ΒΑϹΙΛΕΥϹ ΒΑϹΙΛΕΩΝ ΜΕΓΑϹ ΜΟΝΝΗϹΗϹ; of which I should chuse the former, notwithstanding it is such Parthian Greek, as the round of the medal seems not to have been capable of containing the latter. It will be, at first sight, observed, that this legend is different from those of the Parthian coins hitherto described; which are exhibited in the (2) genitive, not the nominative, case. The last two letters MO leave no room to doubt, tho' the O is not so extremely well preserved, that the piece was struck when Monneses sat upon the

(1) Nicol. Haym Roman. *Del. Tesor. Britan.* Vol. ii. p. 36, 37. In Londra, 1720.

(2) J. Foy Vaill. in *Arsacid. Imper.* Parisiis, 1728. *Numism. Antiqu. Collect. a* Thom. Pembroch. et Montis Gomer. Com. P. 2. T. 76. Nicol. Haym Roman. ubi sup. p. 30—38.

Parthian

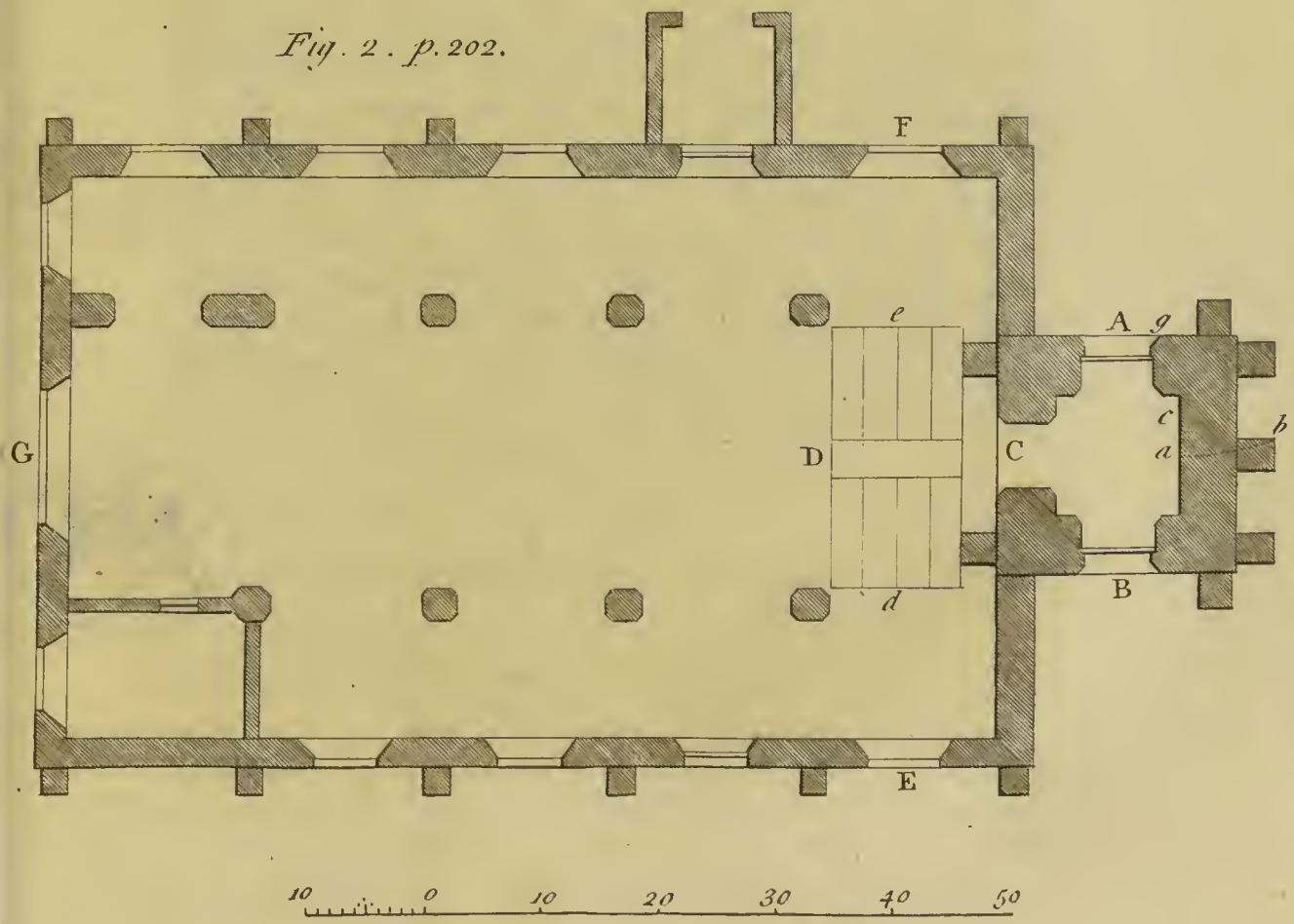
Fig. 1.



Num. Parth. apud Jemnem Swinton, A.M. Oxonienf. R.S.S.

		Palm.	Heb
Daleth			
He			
Mem			
Nun			
Schin			
Pe			
Aleph			

Fig. 2. p. 202.



Scale of Feet, 20 — 1 Inch

J. Mynde sc.

Parthian throne. It is true, indeed, the firſt element of the prince's name is not ſo apparently a *Mu*; tho' it reſembles that letter, even as preſerved on this very medal, much more than any other of the Greek alphabet. It is however totally unlike the *Alpha* near it, as well as all the other forms of that element to be met with in Montfaucon (3), and bears not the leaſt reſemblance to *Beta* (4), particularly the *Beta* which ought to have been impreſſed on this piece; as moſt evidently appears from a particle of that letter, viſible in its proper place. From whence we may infer, that the character I am conſidering muſt be *Mu*; as *Alpha*, *Beta*, and *Mu*, were the only initial letters, according to Dr. Vaillant (5), of the names of the Parthian kings reigning after the introduction of the *Omega* of the minuſcular form, as it here occurs, upon the Parthian coins. But Monneſes was the only one of thoſe princes whoſe name began with *My*, and conſequently the two laſt Greek elements on this medal are part of his name. The metal and ſize of this piece, as none belonging to the other Parthian monarchs ſeem to have been yet met with of the ſame form, which exactly agree with thoſe of Monneſes's (6) coin publiſhed by Dr. Vaillant, may likewiſe be urged, as an additional argument of no ſmall weight, in ſupport of my opinion.

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(3) Montfauc. *Palæograph. Græc.* p. 123, 124, 125. Pariſiis, 1708.

(4) Idem ibid.

(5) J. Foy Vaill. ubi ſup.

(6) J. Foy Vaill. ubi ſup. p. 335, 341.

2. That the Greek and unknown legends onthis medal are either of the ſame or a ſimilar import, will be acknowleged by all verſed in this kind of literature extremely probable. The Greek and Phœnician legends on the ſame (7) coins of Tyre and Sidon, as I have, upon examination, found, and ſhall hereafter more fully evince, clearly correſpond. The Latin and Punic legends on Juba's medals, as has been by me formerly proved (8), very well agree. The ſenſe (9) I have aſſigned the legend in unknown characters, exhibited by the reverſe of my former Parthian coin, with, I flatter myſelf, ſome appearance of truth, ſufficiently anſwers to the Greek one preſerved by other medals of the ſame prince. We may therefore be permitted to ſuppoſe, that both the legends handed down to us by the coin before me related to Monneſes, and conveyed the ſame, or at leaſt extremely ſimilar, ideas to the Parthians and the Greeks. Nothing can be more conſonant to reaſon, tho' we muſt not directly aſſume this as a poſtulate, than ſuch a ſuppoſition.

3. This notion will likewiſe receive a farther acceſſion of ſtrength from the characters of which the unknown legend is compoſed. The firſt of them ſo nearly approaches one of the forms of the Pal-

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(7) Montfauc. ubi ſup. p. 118, 119. Hadrian. Reland. *Palæſtin. Illuſtrat.* Tom. ii. p. 1014, 1015, 1055. Trajecti Batavorum, 1714. I have a Latin diſſertation in the preſs here, almoſt printed off, containing an explication of a conſiderable number of coins of Tyre and Sidon, with Phœnician legends upon them.

(8) *De Num. quibuſd. Sam. et Phœn. &c. Diſſert.* p. 53—56. Oxon. 1750.

(9) *Philoſoph. Tranſact.* Vol. xlix. p. 593---607.

myrene

myrene *Pe*, as it appears in (10) Mr. Dawkins's alphabet, that we may without ſcruple aſcribe to it the power of that letter. The ſecond is ſo like the Palmyrene and the Chaldee *Daleth* (11), that it ought indubitably to paſs for that element. The third differs ſomething, tho' not greatly (12), from one of the forms of the Palmyrene *He*. The fifth, which likewiſe occupies the eighth place, is by no means remote from the figures of the (13) Palmyrene and Chaldee *Nun*. The ſixth occurred in the third place before. With regard to the ſeventh, it ſeems to me pretty ſtrongly to reſemble ſome forms of the Palmyrene *Mem* (14), and even exactly to anſwer to that of the ſame letter in (15) one of the Palmyrene inſcriptions preſerved amongſt thoſe celebrated remains of antiquity commonly, tho' perhaps improperly, ſtiled *The Ruins of* PERSEPOLIS. The ninth is the *He* touched upon before. The tenth, which alſo ſucceeded the third, if the powers of the other elements have been rightly determined, muſt be *Schin*. Nor does this character, if we view it in a certain poſition, appear very remote from a rude form of that letter. This legend then, according to what is here advanced, as it now remains, conſiſts of the words -----PADESHANE MONESH, PADESHAN

(10) *Philoſoph. Tranſact.* Vol. xlviii. p. 693.
(11) *Philoſoph. Tranſact.* Vol. xlviii. p. 693, 740.
(12) *Philoſoph. Tranſact.* ubi ſup.
(13) *Philoſoph. Tranſact.* ibid.
(14) *Philoſoph. Tranſact.* ubi ſup. p. 693, 740.
(15) Chard. *Voyages en Perſe*, &c. Tom. iii. p. 119. A Amſterdam, 1711. *Philoſoph. Tranſact.* Vol. xlix. p. *597, *598.

EMONESH, or (16) PADESHAN AMONESH, that is, MONESH, or AMONESH,-----OF KINGS; the word PADISHAH, or rather PADESHAH, as it ſeems to have been written and pronounced by the (17) antient Perſians, in the Pehlawian, Pehelawian, or Baſtanian, that is, the old Perſic, tongue, denoting (18) KING. That NI, or NE, was ſometimes a maſculine plural termination in the antient Perſic, ſeems to appear from the word, or rather words, بیر زیوشنی, BIR. ZEIVESHNI, LONGÆVI, which occurs in Dr. Hyde (19). And that the vowels A and E were ſometimes prefixed to the Perſian proper names, in the remoter periods of time,

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(16) That the plural termination of PADESHAH, or SHAH, which, according to Khojah Afdhalo'ddîn, denoted originally the ſame thing, was AN, or perhaps ANE, in the days of Ammianus Marcellinus, there is good reaſon to believe; the word SAANSAA, KING OF KINGS, having been then uſed by the Perſians, and handed down to us by that author. The term ΣΑΑ, SAA, equivalent to the Perſic SHAH, KING, likewiſe occurs in Agathias, a writer of the ſixth century. Should my explication of the Parthian legend of the coin before me meet with the approbation of the learned, it will perhaps be granted me, that the plural of PADESHAH, or PADESHA, amongſt the Parthians was PADESHAN, if not PADESHANE, in the ſecond century after CHRIST. Hyd. *Hiſt. Rel. Vet. Perſ.* p. 416. Khojah Afdhalo'ddîn, D'Herbel. *Biblioth. Orient.* p. 767. Hadr. Reland. *Diſſert.* viii. *de Vet. Ling. Perſ.* p. 221, 222. Ammian. Marcellin. Lib. xix. cap. 2. Agath. Lib. iv. p. 135, 136. Pariſiis, 1660. Ezech. Spanhem. *De Præſtant. et Uſ. Numiſm. Antiquor.* Tom. i. p. 463---466. Lond. 1706.

(17) Hyd. *Hiſt. Rel. Vet. Perſ.* p. 79. Oxon. 1700.

(18) D'Herbel. *Biblioth. Orient.* p. 699, 767. Hyd. ubi ſup. Hadr. Reland. *Diſſert.* viii. *de Vet. Ling. Perſ.* p. 147. Trajecti ad Rhenum, 1707.

(19) Hyd. ubi ſup. p. 326.

is

is abundantly manifeſt, from the words SFITAMAN, ESFINTAMAN, or ESPINTAMAN, the (20) name of either the father or one of the anceſtors of Zerâtuſht; XERXES, (21) AXERXES, or AXERSES, and others of the ſame kind, that might, with equal facility, be produced. Whether therefore we read this legend - - - - PADESHANE MONESH, PADESHAN EMONESH, or PADESHAN AMONESH, we cannot greatly err. As the coin was not ſo perfectly ſtruck, part of the Greek legend is thereby apparently loſt; from whence, and the evident want of a word there, we may conclude, that the Parthian legend, for Parthian by the explication here laid down it appears to be, on the correſpondent part of the reverſe, muſt have met with the ſame fate. What that Parthian term was, I cannot take upon me to ſay; but the whole legend was probably ſomething like THE GREAT KING MONNESES, MONNESES THE KING OF KINGS, or MONNESES (22) THE GREAT KING OF KINGS; all which titles are intirely conſonant to thoſe aſſumed by the Parthian kings (23), and tranſmitted down to us on their other coins. The Perſian, or Parthian, termination of the proper name MONNESES, and others that occur, was ESH. This may be inferred from the legend now before me, in conjunction with Scripture (24), and has

(20) Hyd. ubi ſup. p. 18, 312.

(21) Matth. Hiller. *Onomaſt. Sacr.* p. 619. Hadr. Reland. ubi ſup. p. 259—262.

(22) Nicol. Haym Roman. ubi ſup. p. 33.

(23) J. Foy Vaill. & Nicol. Haym Roman. ubi ſup. & alib.

(24) Eſth. c. x. *v.* 1.

been

been remarked by ſome good authors (25). That the Greeks alſo expreſſed the *Schin* by their *Sigma*, and ſuperadded another termination to ſuch words, is ſufficiently manifeſt from the Perſian, or Parthian, ARSHAK (26), which was converted into ARSACES by the Greek writers. I muſt farther obſerve, that, according to Herodotus (27), the antient letters of the Perſians were thoſe uſed by the Aſſyrians; which I take to have been the ſame with the elements that prevailed amongſt the Syrians, and formed the alphabet of the Palmyrenes. Nay, we may collect from (28) Epiphanius, that a conſiderable part of the Perſians uſed the Palmyrene characters as late as the decline of the fourth century after CHRIST. All which being maturely weighed, I cannot forbear thinking the interpretation of the Parthian legend here laid down in a good degree probable. I am far however from inſiſting upon the truth of it. I ſhall only beg to be indulged the liberty of propoſing it to the conſideration of our moſt illuſtrious Society, and leave the fate of it intirely to the deciſion of ſo very eminent a part of the learned world.

4. But however my explication of the Parthian legend may be received, I believe it will ſcarce be denied, that the coin was ſtruck in the reign of Monneſes, one of the Parthian kings; this point

(25) Scalig. *Prolegom. in Lib. de Emend. Temp.* p. 41. Col. Allobrog. 1629. & *Can. Iſag.* f. 317. Matth. Hiller. ubi ſup. p. 619, 620.

(26) Moſes Chorenenſ. *Hiſt. Armen.* Lib. ii. iii. Theophil. Sigefr. Bayer. *Hiſt. Oſrhoen. &c.* p. 97. Petropoli, 1734.

(27) Herodot. Lib. iv. c. 87.

(28) Epiphan. *Adv. Hær.* Lib. ii. Tom. ii. p. 629. Pariſ. 1622.

having been ſo clearly evinced, by the reaſons above alledged. It may not be improper here to remark, that the republic of letters has been obliged with the publication of two of this prince's medals; the firſt of which was (29) coined in the year of the Parthian æra 422, and the other in 425 (30). Neither of them however exhibits the *Victory* impreſſed on the reverſe of mine. That ſymbol, adopted by the Parthians in imitation of the Roman manner, muſt undoubtedly have alluded to ſome victory, or at leaſt ſome remarkable advantage, gained by the Parthian forces over the Romans, a little before the piece was ſtruck. What that advantage was, when and where obtained, and whether hiſtory conſpires with the medal, in order to ſettle this point, I am next to inquire.

5. Monneſes, if Dr. Vaillant (31) may be credited, aſcended the Parthian throne in the year of the Arſacidæ 422, nearly coincident with the 166th of CHRIST, when M. Aurelius and L. Verus preſided over the Roman world. But ſo far were the Parthians at that time from gaining any victories over the Romans, that they were every where worſted by them, and put to the rout. A Roman army, under the command of Avidius Caſſius, had penetrated into Meſopotamia and Aſſyria, ravaged thoſe provinces, laid Seleucia in aſhes, taken Cteſiphon, and levelled the royal palace there with the ground,

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(29) J. Foy Vaill. ubi ſup. p. 339.

(30) Maffeius, in *Gall. Antiqu. Quæd. Select.* Epiſt. 22. p. 106. Pariſiis, 1733.

(31) J. Foy Vaill. ubi ſup. p. 334, 335, 336, 339.

according

according to Dio (32), the preceding year. Nay, it appears both from (33) Capitolinus and the Roman (34) coins, that M. Aurelius and L. Verus triumphed over the Parthians, the very year after Monneſes, as (35) Dr. Vaillant will have it, came to the crown. All which that celebrated antiquary acknowleges to be true; and adds, that Monneſes concluded an infamous peace with the Romans, ceding to them the whole province of Meſopotamia, for which he was ſoon after depoſed by his ſubjects. Here then we can diſcover not the fainteſt traces of a reaſon for the appearance of a *Victory* upon the medals of this prince. Nor does any thing like a reaſon for ſuch an appearance, in antient hiſtory, occur, before the 950th year of Rome, correſponding with the 197th of CHRIST; when the Parthians, animated by the civil diſſentions, which reigned amongſt the Romans, Albinus and Severus then fiercely contending for the empire, entered Meſopotamia with a powerful army (36), and reduced to their obedience moſt of the cities of that

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(32) Dio, Lib. lxxi. p. 802. *Imperator. Romanor. Numiſm. &c. Stud. & Cur.* Franciſc. Mediobarb. Birag. p. 218. J. Foy Vaill. *Can. Chronologic. Reg. Parthor.* p. 41.

(33) Jul. Capitolin. in *M. Antonin. Philoſ.* et in *Ver.*

(34) *Imperator. Romanor. Numiſm. &c. Stud. & Cur.* Franciſc. Mediobarb. Birag. S. R. I. Com. &c. p. 220. Mediolani, 1683.

(35) J. Foy Vaill. ubi ſup. & in *Arſacid. Imper.* p. 338.

(36) Dio, Lib. lxxv. p. 853. J. Foy Vaill. in *Arſacid. Imper.* p. 356. & in *Can. Chronologic.* p. 42. Ludovic. Du Four de Longuerüe, Ab. S. Joan. de Jardo ad Melod. et Sept. Font. in Theraſc. *Annal. Arſacidar.* p. 51. Argentorati, 1732. Eraſm. Froelich, S. J. S. in *Dub. de Minniſar. Aliorumque Armen. Reg. Num. et Arſacidar. Epoch. nuper vulgat.* p. 66. Viennæ Auſtriæ, 1754.

province.

province. Now if, with F. Corſini (37), we admit the commencement of the Parthian æra to have happened in the 525th year of Rome; the 425th or 426th year of the former and the 950th of the latter of thoſe æra's, wherein the Parthians undertook the Meſopotamian expedition, will nearly coincide. But it may be proved from a medal of Monneſes, deſcribed by the (38) Marquis Scipio Maffei, in oppoſition to (39) Dr. Vaillant, that this prince was not dethroned in the year of the Arſacidæ 423, but had two years afterwards the management and direction of the Parthian affairs: wherefore, notwithſtanding what has been advanced to the contrary by that antiquary, he might ſtill have ſat one or two years longer upon the Parthian throne. This may likewiſe be inferred from Dio, who mentions Vologeſes as preſiding over the Parthians, not in 198, but in 199, about two years after their irruption into Meſopotamia. All which being with proper attention conſidered, it will appear extremely probable, that the medal I have been endeavouring to explain was coined either in the 425th or 426th year of the Parthian æra, that is, the 197th of CHRIST; that the authority of (40) Arrian, (41) Juſtin, and (42) Athenæus, on which

(37) Edv. Corſin. Cl. Reg. Scholar. Piar. in Acad. Piſan. Humanior. Litterar. Profeſſ. *De Minniſar. Aliorumq; Armen. Reg. Num. et Arſacid. Epoch. Diſſertat.* p. 13—29. Liburni, 1754.

(38) Maff. in *Gall. Antiqu. Epiſt.* 22. p. 106.

(39) J. Foy Vaill. ubi ſup. p. 338.

(40) Arrian. in *Parthic.* apud Photium, *Cod.* 58. & apud Syncell. in *Chronograph.* p. 226.

(41) Juſt. Lib. xli. c. 4.

(42) Athen. *Deipnoſoph.* Lib. iv. c. 13.

F. Corſini principally founds his notion, may be intirely depended upon; and, in fine, that the arguments he makes uſe of on this occaſion, to (43) evince the truth of his ſcheme, however they may be oppoſed by F. Frœlich, and the medal before me mutually ſtrengthen and ſupport one another.

6. I muſt not forget to remark, that F. Corſini thinks, without a proper foundation, that the piece of Monneſes publiſhed by Dr. Vaillant may be conſidered either as a Parthian or (44) an Armenian coin. This, I ſay, he takes, without a proper foundation, to be the caſe. For the medals of the Armenian kings, ſuch as that he has obliged the learned world with a draught of, and a moſt excellent diſſertation upon, diſcover a taſte far different from that which is exhibited by Dr. Vaillant's coin. The air of the face, the curls into which the hair is formed, and in fine every thing elſe viſible upon the former, except the ſymbols on the reverſes, bear little reſemblance to what is preſented to our view by the latter. As for the titles, impreſſed on theſe medals, they are far from being of the ſame kind; the Armenian princes in this particular approaching nearer the ſucceſſors of Seleucus (45), and contenting themſelves with more plain and ſimple titles than that lofty one affected by Monneſes, according to Dr. Vaillant, in common with the other Parthian kings. To which I may add, that my coin ſets this point beyond diſpute, by the Parthian characters it has ſo apparently preſerved; all the Armenian medals I have hitherto met with,

(43) Corſin. ubi ſup.
(44) Corſin. ubi ſup. p. 2.
(45) Eraſm. Froel. ubi ſup. p. 72.

about

about three or four in number, as well as that of Baron Stoſch (46), which F. Corſini has ſo learnedly explained, having only Greek legends upon them. What therefore he has advanced, on this head, muſt be conſidered as not altogether ſo conſonant to truth; eſpecially, as he ſeems to have offered nothing of any great weight in ſupport of his opinion.

7. Before I diſmiſs the preſent ſubject, I muſt beg leave to take notice of the Parthian character on the field of my medal, which is not very unlike the Chaldee (47) or Palmyrene *Aleph*, tho' of ſomething a more complex form. Should this be allowed, that character may be conſidered as the initial letter of ARSACIA, the name of a city ſubject to the Parthians, placed by (48) Ptolemy in Media; where, according to (49) Dr. Vaillant, many of the Parthian medals were coined. That city is however believed by ſome to have been ſeated on the ſpot occupied at this time by Caſbin, or rather Kazwîn (50), one of the principal towns of the Belâd Al Jebâl, or mountainous part of the Perſian Irâk; for a particular account of which place, the curious may have recourſe to Golius, and the eaſtern geographers. As I have already far exceeded the limits propoſed to myſelf, when I began this letter, I ſhall not now touch upon any other obſervations relative to the

(46) Corſin. in *Ded. Nob. Vir.* Phil. Venut. p. 5.
(47) *Philoſoph. Tranſact.* Vol. xlviii. p. 693, 740.
(48) Ptol. *Tab. Urb. Inſign.* p. 39. Ed. Hudſ. Oxon. 1712.
(49) J. Foy Vaill. in *Arſacid. Imper.* paſſ.
(50) Hamdalla Iſm. Abu'lfed. Mohammed Al Firauzabad. Naſsîr Al Tûſi, Ulugh Beik, &c. Golii not. ad Alfragan. p. 200 ---204.

medal before me, which is the only one I have hitherto met with carrying a Greek and a Parthian legend upon it; but only assure you that I am, with the most perfect regard,

SIR,

Your most obliged,

Christ-Church, Oxon. Nov. 29th, 1756.

and most obedient Servant,

John Swinton.

XXIII. *An Account of a Red Coral from the* East-Indies, *of a very singular Kind: In a Letter from Mr.* John Ellis, *F. R. S. to Mr.* Peter Collinson, *F. R. S.*

Dear Sir,

Read Mar. 24, 1757.

I Promised you, in my letter of the 7th of February 1754, published in our Transactions, Vol. xlviii. p. 507. that I would, when I had an opportunity, endeavour to illustrate the tubular structure of the common red coral of the Mediterranean sea, and of some of the keratophyta; which two kinds, tho' evidently of as different natures as stone and horn, yet are, from late observation, found to be fashioned, or raised up into those beautiful forms, by animals of the same class.

This I shall attempt to do, by comparing them with bodies of a similar kind, but of a less compact texture: for which reason I formerly referred you, in the above-mentioned letter, to a figure, which I have given of the herring-bone-coralline, with its animals

A. A piece of pipy knobbed root Coral from the Spice Islands in the East Indies which appears to be formed by animals of the Polype kind, centering in the central opinion of those bodies being vegetables; the vermicular tubes of which it is composed being no more than the mere coverings of the bodies of those insects.

B. The radiated holes on the yellow mealy surface, through which those insects extend their arms or claws as in the engraving at second.

C. Small branches almost like of Lycopodium, which division of tubes, that have risen all round and increased the bulk of the stem, continuing themselves along it to extend the ramifications; by this means they involve the former side branches into their own stem, dropping entirely from the laws of vegetation.

D. A piece of fleshy pipe magnified to show their cellular texture like that of spongy ramage of tubes.

d. The same in its natural size.

E. A piece of Floral red tube, which is hardened into a solid porous substance with some appearance of articulation, taken from a piece both in a fine state.

e. The same in its natural size.

F. Small holes in the tubes formed by a branches of the polypus.

G. A mass of the stem not so highly magnified showing porous of the tubes passing from strong to spongy.

g. The same in its natural size.

H. The transverse section of the tube showing the holes in the ends of the tubes.

I. One of the radiated cells magnified this is generally by 8 spiraled valves, which close inward on how this polypus shrinks as his head to which is equal at K. and opens outward when it extends it.

animals alive in it, *Phil. Tranſ.* Vol. xlviii. TAB. XVII. *Fig.* E, F, G, to ſhew you the nature of the tubular ſtructure of the keratophyta.

I now lay before you a piece of red coral (*See* TAB. III. *Fig.* A.) from the Eaſt Indies of a very ſingular kind, which I received from your friend Abraham Hume, Eſq;. The ſtem and branches of this appear evidently to the naked eye to conſiſt of a combination of vermicular tubes cloſely connected together: and, if we trace theſe little tubes to their ſtarry openings on the ſurface, *Fig.* B. we ſhall plainly diſcover them to be the red teſtaceous coverings of certain marine polypes, which have raiſed themſelves thus upright, and diſpoſed themſelves into this remarkable vegetable form.

In order to form ſome idea, how theſe maſſes are increaſed and extended to the ſizes we often meet with them, and where the ſame regularity of ſhape is preſerved in the large, that we find in the ſmall; we think it more than probable to ſuppoſe, that the ſpecies of polypes, that compoſe this coral, breed as we find all other polypes do: and this appears more evident to me, from what I have already diſcovered in many kinds of corallines (*See Plate* 38. *of my Eſſay on Corallines*), where the young polypes in ſome ſpecies are produced in the egg ſtate, while others fall in great numbers from their matrices, completely formed, down to the roots of their parent corallines, either to begin a new race of the ſame ſpecies near them, or to increaſe the trunk, and extend the ramifications, of the plant-like figure which they juſt deſcended from.

From obſerving this method in nature, we ſhall the eaſier account for the progreſs of thoſe generations

tions of young teſtaceous polypes of this coral; which appear to us ſucceeding each other, and raiſing themſelves up from the root or baſe, paſſing along the ſtem and branches, and covering the whole anew with their ſhelly caſes: and in this their paſſage upwards we may obſerve, in the ſpecimen before us, how they have involved and incruſted the ſmall lateral branches of the former generation, ſo as almoſt to hide their appearance. From hence we may trace them extending themſelves to the extremities of the upper branches, and there forming a new ſeries of ſlender twigs, proportionable to thoſe which they had juſt covered, ſtill keeping order and exact ſymmetry in the whole ſtructure.

The diſtinguiſhing character of this red coral, after we have conſidered its fiſtulous texture, is the knotty joints, of which it is compoſed: theſe appear more diſtinct, and are placed at a greater diſtance, in the ſmaller branches than the large; and, as we deſcend to the trunk, the traces of theſe inequalities but juſt appear.

From theſe protuberances, or knots, the lateral branches take their riſe; and as theſe and the leading branches grow up together, they frequently inoſculate at theſe joints, forming a kind of network, like what we obſerve in many of thoſe ſpecies of keratophyta, which are called ſea-fans.

The ſurface of this coral, when recent, is covered with a mealy friable matter, of a yellow colour, not unlike that of the true red coral, but much fuller of little raiſed ſtarry cells. The figure of theſe cells is owing to the radiated poſition of the claws of the polypes.

Upon removing this friable matter, we obſerve, that

that the polypes of these cells have had a communication with a small hole or opening into one of the tubes, that lie immediately under it.

This frequent intervention of the openings of the small tubes, or their ramifications, between the sides of the larger ones, makes the latter appear more irregular, and not so parallel, as in the true red coral; where we find fewer stars; and, where those occur, we may observe it always alters the direction of the tubes.

I must further remark to you, that many of the tubes of this coral appear, thro' a magnifying glass, full of small holes, like those I have described in the keratophyton *(Plate 26. Fig. G. p. 62. of my Essay on Corallines)*; and these holes will appear more distinctly to you, when you examine the half tubes, or broken irregular ones, on the stem and great branches of this coral.

Further, if you compare the transverse section, at the base of this coral, with a section of a common Rattan cane, they will both appear full of holes in the same regular order, and of nearly the same diameter: whereas the tubes, on the surface of the stem of this coral, look as irregular as so many holes pierced or eaten out by worms.

I hope by this time our ingenious botanical friends, whom we could not persuade to part with these beautiful sea-productions from the vegetable kingdom, are thoroughly convinced, that this mealy, friable, or calcarious covering, full of starry cells, which we are sure to find covering all the recent red corals and keratophyta, is not a mere blight of insects, common to the sea vegetables as well as land ones, which they have formerly insisted on; but that they will consider

conſider this covering, for the future, as proper and neceſſary for the well-being of theſe little animals, as they do at preſent hair and wool for beaſts, feathers and down for birds, and ſcales and ſlime for fiſhes.

This red coral is mentioned by Rumphius, in his *Herbarium Amboinenſe*, Vol. vi. Tab. 85. p. 264. but, as the figure he has given, is not ſufficient to demonſtrate its tubular texture and animal ſtructure, I have had it more accurately drawn; and thoſe parts in particular magnified, which may tend to illuſtrate the foregoing deſcription. He mentions, that it is in great eſteem with them, on account of its beautiful figure; but would be much more ſo, if it was not for the great difficulty they find in preſerving the ſmalleſt twigs from falling off; which is the reaſon, I ſuppoſe, that this ſpecimen is not more complete.

Laſtly, he tells us, that it is uſed by the inhabitants of the Spice-iſlands as a principal ingredient in their medicines to expel poiſon: as alſo, that they have it in great eſteem on account of its excellent diuretic quality.

Upon examining this coral in the microſcope, I obſerved, that the outſide tubes of the ſtem were chiefly ſtony, but that the inward parts were compoſed of as many diviſions of ſpongy tubes, as there were of ſtony ones.

This I find ariſes from the ſmaller ramifications, which being ſpongy at the knobs, and ſtony in the ſpaces between them, are incloſed and united together into one common maſs during the growth of this coral; ſo that both the ſoft and hard parts together make up the inſide of its trunk or ſtem. When we examine minutely the two parts, that compoſe

compoſe the branches, we find, that the knobs conſiſt of little ſponge-like tubes interwoven together, as they appear magnified at *Fig.* D; and the ſhank or part between the knobs is compoſed of ſtony tubes, that are more erect (*See the piece magnified at* E): theſe tubes appear to be branched from the lateral holes at FF. The Fig. E likewiſe ſhews the appearance of the tubes on the ſurface of the main ſtem.

The radiated openings in the little wart-like figures on the ſurface of the branches are guarded by eight pointed valves, as magnified at *Fig.* I.: theſe incloſe the heads of the polype, one of which is figured at K.

The ſtem of this ſpecimen is ſo intirely diveſted of its yellow mealy covering, that we may eaſily trace the manner in which the animals, that compoſe it, have carried up their ſtony tubular cells, which lie ſide by ſide along the ſurface. Theſe tubes have ſtill ſome marks of ſponginefs at particular diſtances, which, as they come to join together, form thoſe irregular croſs-lines, that ſurround the ſtem in ſeveral places. *See Fig.* A.

In other ſpecimens I have obſerved the principal ſtems covered over with calcarious tubes, ſuch as I have deſcribed in the Eſſay on Corallines, *&c.* in that ſpecies of keratophyton called Venus's Fan. *Plate* 26.

The ſponginefs of the knobby joints occaſions that exceſſive brittleneſs in the leſſer branches; which accounts for the difficulty, which Rumphius mentions, of getting good ſpecimens of this beautiful coral.

I have lately ſeen a white pipy and ſtony coral

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with ſpongy knobs, which is only a different ſpecies of this genus, in the very curious cabinets of our friends Dr. John Fothergill, M. D. and Mr. Iſaac Romilly, F. R. S. ſpecimens of which they have both lately received from the Eaſt Indies. The examining of theſe has given me an opportunity to be more particular in the deſcription of this coral; which ſeems to point out to us the great affinity there is between corals, keratophyta, and ſponges.

I am,

Dear Sir,

Your moſt affectionate humble Servant,

Laurence-Lane,
Mar. 24. 1757.

John Ellis.

XXIV. *An Account of the Effects of a Storm at* Wigton *in* Cumberland. *Communicated by Mr.* Philip Miller, *F. R. S.*

To the Rev. Tho. Birch, *D. D. Secr. R. S.*

SIR,

Read Mar. 31, 1757.

I Received the incloſed letter by the poſt, giving an account of the ſtorm, which happened lately in the north. If the Royal Society have not already been informed of the effects of it, and you think the contents of it worthy their notice; I beg you will be ſo good as to communicate it to them. The facts therein mentioned have been confirmed to me by a perſon of ſkill and integrity.

tegrity. Mr. Thomlinſon's conjecture of the cauſe of the leaves of trees appearing ſcorched after the ſtorm, I believe to be true; having two or three times myſelf obſerved the ſame in Suſſex, at a conſiderable diſtance from the ſea; when all the hedges, trees, and woods, on the ſide toward the ſea, have had their leaves ſcorched, as if fire had paſſed over them; and their oppoſite ſides from the ſea have continued in full verdure; which frequently happens in ſtorms from the ſouth-weſt: and, upon taſting their leaves, I have found them as ſalt, as if they had been ſteeped in brine. I am,

SIR,

Your moſt obedient humble Servant,

Chelſea, Nov. 23. 1756. Philip Miller.

To Mr. Philip Miller.

SIR, Wigton, Nov. 15. 1756.

ON the 6th of laſt month, at night, happened a moſt violent hurricane, ſuch an one, perhaps, as has not been known in theſe parts in any one's memory. It laſted four hours at leaſt, from about eleven till three. The damage it has done is very deplorable. The corn has ſufferd prodigiouſly. Stacks of hay and corn have been intirely ſwept away: houſes unroofed, and in ſeveral places driven down by its fury: trees without number torn up by the roots; others ſnapt off by the middles, and their fragments ſcattered over the adjoining fields. Some were twiſted almoſt round, or ſplit down to the very

ground; and, in ſhort, left in ſuch a ſhattered, mangled condition, as ſcarce any deſcription can give you an adequate idea of.

The change in the face of the country was very ſurpriſing in one ſingle night: for, to complete the diſmally-deſolate ſcene, the ſeveral tribes of vegetables (in all their verdure the day before), as if blaſted with æthereal fire, hung down their drooping heads. Every herb, every plant, every flower, had its leaves withered, ſhrivelled up, and turned black. The leaves upon the trees, eſpecially on the weather ſide, fared in the ſame manner. The evergreens alone ſeem to have eſcaped. The graſs alſo, in a few days time, recovered itſelf in a great meaſure.

I agreed at firſt with the generality of people in their opinion, that lightning had done all this miſchief: but upon recollecting, that there had not been much ſeen any where, in many places none at all, but that the effect was general (1), as far as ever the wind had reached; I began to think, that ſome other cauſe might probably be aſſigned. Accordingly, I ſet myſelf immediately to examining the dew or rain, which had fallen on the graſs, windows, *&c.* in hopes of being enabled, by its taſte, to form ſome better judgment of the ſulphureous or nitrous particles (or of whatever other quality they were), with which the air was ſo ſtrongly impregnated that night, as to produce ſuch ſtrange effects. Nor was I deceived in my expectations: for, upon taſting it,

(1) Upon inquiry, I find, that no ſuch thing was taken notice of in Northumberland; ſo it probably has not extended any further to the eaſtward, than the ſkirts of our own county.

I found

I found it as brackiſh as any ſea-water. The ſeveral vegetables alſo, which I taſted, were all ſalt, more or leſs, and continued ſo for five or ſix days after; the ſaline particles not being then waſhed off; from the corn and windows in particular; the latter of which, when the moiſture on the outſide was exhaled next day, ſparkled and appeared exceeding brilliant in the ſunſhine. This ſaltneſs, I conceive, has done the principal damage: for common ſalt diſſolved in water, I find, upon experiment on ſome freſh vegetables (when ſprinkled two or three times upon them) has the very ſame effect, except that it does not turn them quite ſo black: but particles of a ſulphureous, or (2) other quality, may have been mixed with it. That this ſalt water had been brought from the ſea (3), every body, I think, will allow; but the manner how (4), is not ſo eaſy to conceive.

This freedom, Sir, perhaps may want an apology: but, as a gentleman (5) of the Society you have the honour to be a member of, did not think ſomething of the like nature either unworthy of his own notice, or that of the world; and as the hurricane principally affected theſe parts of nature, in the knowlege of which you have ſo eminently diſtinguiſhed your-

(2) In an adjoining bleach-yard, a piece of cloth, which had been left out all night, was turned yellow; and was not without ſome difficulty waſhed out again. Some alſo, which was ſpread out the next day, contracted the ſame colour.

(3) The wind was weſterly, and conſequently would ſweep the Iriſh ſea.

(4) No rain, or however very little, during the hurricane.

(5) Mr. Derham, in his Phyſico Theology.

ſelf;

ſelf; I flattered myſelf you would excuſe the trrouble I ſhould give you in a peruſal of an account of this very ſtrange, tho' hitherto unnoticed, phænomenon.

I am, Sir, with the greateſt reſpect and eſteem,

Your moſt humble Servant,

Thomas Thomlinſon.

XXV. *An Account of the Effects of Lightning upon the Steeple and Church of* Leſtwithiel, Cornwall; *in a Letter to the Right Honourable the Earl of* Macclesfield, *Preſident of the R.S. By Mr.* John Smeaton, *F. R. S.*

Read April 21, 1757.

JAnuary 25. 1757. about five o' clock in the evening, returning home from the Edyſtone works near Plymouth, I obſerved four flaſhes of lightning, within the ſpace of ſix or ſeven minutes, towards the weſt; but heard no noiſe of thunder *. A few days after, I was informed, that the ſame evening the lightning had ſhattered the church of Leſtwithiel in a very ſurpriſing manner.

The 1ſt of March I was at Leſtwithiel: they had then begun to repair the damages; but had not made ſuch a ſuch progreſs, but that the principal effects were equally obſervable as at firſt. I obſerved, and was informed, as follows:

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* Diſtance about thirty miles.

At

At the time before-mentioned, the inhabitants were alarmed by a violent flaſh of lightning, accompanied with thunder ſo ſudden, loud, and dreadful, that every one thought the houſe he was in was falling upon him; almoſt every one being within doors, on account of a violent ſhower of rain, which preceded the lightning: ſo that no body ſaw or heard any thing of the miſchief done to the church, till it was obſerved accidentally after the ſhower.

The ſteeple is carried up, plain and ſquare, to about 49 feet, with a kind of ſlate-ſtone, rough-caſted on the outſide; upon which is formed a very elegant octogon Gothic lanthorn about 9 feet high, and thereon a ſtone ſpire about 52 feet height, with a ſpindle and vane riſing about 3 feet above the ſtone: ſo that the whole together was about 113 feet. Each face of the lanthorn finiſhes above with a ſort of a Gothic pediment, with a little pinacle upon each, ſeparated from the body of the ſpire.

I will not affirm, that the lightning entered in at the ſpindle or vane at top; but will ſuppoſe it, for the ſake of methodizing the facts. The vane was of plate copper, which being turned round, and rivetted, made a ſocket to turn upon. The ſpindle did not reach thro' the ſocket, but the weight of the vane reſted upon the top of the ſpindle, the top of the ſocket being cloſed. About the vane were many acute angles, and ſome almoſt ſharp; but I did not obſerve any pointing directly upward. The vane was much bruiſed, which might be occaſioned by the fall; but the ſocket was rent open, as if it had been burſt by gunpowder; and in ſuch a manner, as I cannot conceive could be occaſioned by the fall.

Under

Under the ſpindle, that carried the vane, was a bar of much the ſame ſize and length †, that paſſed thro' the center of ſeveral of the uppermoſt ſtones ſucceſſively, in order to unite them the more firmly together, and was run in with lead: all which ſurrounding ſtones were broke off, except one, which, together with the bar, fell down within the tower.

The ſhell of the ſpire, as far down as 35 feet from the top, was no more than 7 inches thick, and the courſes about the ſame height: ſo that ſcarce any one ſtone in the ſpire could weigh more than 30 or 40 pounds; but they were joined together at the ends with mortoiſe and tenon, in a curious manner. Above 20 feet of the upper part was intirely thrown down, and diſperſed in all directions; and, as I was informed, ſome pieces were found at the diſtance of 200 yards. A great many ſtones fell upon the roof of the church; and ſeveral made their way thro' both roof and cieling down into the church, breaking the pews, and whatever they fell upon. Six feet ſtill lower the ſpire was ſeparated; the weſtermoſt half being thrown down; the eaſtern half was left ſtanding, but disjointed, and in ſo critical a poſture, that it ſeemed ready to fall every moment: ſo that this was ordered to be taken down immediately; and likewiſe to 6 feet below, the work being found remarkably ſhattered. In this condition it was when I ſaw it. The whole of the ſpire I found much cracked and damaged, but the remainder of the 7 inch ſhell ſo greatly, that there ſeemed ſcarcely a whole joint.

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† *Viz.* About four feet long, and one inch ſquare.

The pediments over every face of the lanthorn were damaged more or leſs; but the whole aſhlering of that to the N.W. was torn off from the inner wall, to which it was connected. At firſt ſight this might ſeem to be done by the falling of the ſtones from above; but I was convinced to the contrary, by obſerving, that ſeveral of the pediments were damaged, and even ſtones ſtruck out, where the little pinacles above them were left ſtanding.

About the top of the lanthorn is a bell for the clock to ſtrike on: it is hung upon a croſs-bar, with gudgeons at each end; the whole being ſuſpended to a beam laid acroſs the tower. The croſs-bar was ſo bent, that the clock-hammer would not touch the bell by above 2 inches. This could not be done by the falling of ſtones, becauſe the beam would defend the bell from receiving any ſtroke in the direction to which the croſs-bar was bent. As to the wire, that drew the hammer, as I was informed, not one bit of it could be found.

The bells (four in number) for ringing hung in the ſquare part of the tower, below the lanthorn, two above and two below: the wheels of every one were broke to pieces, and one of the iron ſtraps, by which they are faſtened to the yoke, unhooked; and, as appeared to me, could not be replaced without great force, or unlooſing. Whether theſe accidents were occaſioned by the lightning, or the falling ſtones, I leave undetermined.

In the floor under the bells was placed the clock, caſed up with ſlight boards. The verge, that carries the pallets, was bent downwards, as if a ten pound weight had fallen ten feet high right upon it. The

 crutch,

crutch, that lays hold of the pendulum, looked as if it had been cut off by a blunt tool, and heated by the blow, till it was coloured blue, at the place where it was cut. It turned at a right angle, and might be about $\frac{4}{10}$ of an inch broad by $\frac{2}{10}$ thick. As to the pendulum, which hung pretty near the wall, the upper part of the rod was ſtruck with ſuch violence againſt the wall, that a ſmart impreſſion thereof was made in the plaiſter: and near the upper part of the impreſſion appeared a circular ſhady ring, of a blackiſh colour, ſomething like as if a piſtol had been diſcharged of powder, and the muzzle held near the wall. The caſing of boards round the clock remained unhurt.

In this ſtory, on the north and ſouth ſide, are two narrow windows or air-loops; againſt the upper part of which, on the outſide, were fixed the timber dials belonging to the clock, both which were blown off, and broke to pieces, poſſibly by the fall: and not only that, but part of the ſtone jambs were broke out alſo, near to where the rod paſſed, that carried the hands. In this ſtory alſo was a ſort of window or air-loop on the eaſt ſide, that had communicated with the church, but was ſtopped up with lath and plaiſter: alſo ſeveral putlock-holes for the ſcaffolding, which had gone thro' the wall into the church, but were ſtopped up with ſtone, and plaiſtered over: all theſe were forced out into the church, and the plaiſter torn from the wall.

The ground-ſtory of the tower or bellfrey is expreſſed in the plan (*See* Tab. IV. *Fig.* 2.). The ſouth entrance A and north B were ſhut with wooden doors. The upper part of the eaſtern C, that

that communicated with the church, was made up with lath and plaiſter; and before it, in the church, are the ſeats D, raiſed one higher than another; ſo that the floor of the ſeats next the wall was half up the door-way; conſequently the vacuity under the ſeats lay open to the bellfrey.

About the middle of the weſtermoſt ſide, at *a*, one of the paving-ſtones, about 1 foot ſquare, and 1 ½ inch thick, was thrown up, and a hole pierced into the wall, rather below the level of the pavement, into which one might put three fingers. On the oppoſite ſide, the ſouth-weſt angle of the middle buttreſs at *b* had a ſtone taken out even with the ground, and a hole continued in to the buttreſs; ſo that there is great appearance of its reaching thro' both wall and buttreſs, which together is 8 feet; but the hole was too rugged and crooked to put any thing thro'. Beſides this hole, this wall was pierced in ſeveral places, and the plaiſter thrown off both within and without. One place within, about 4 feet above the floor, right over *c*, was a hole of about 14 inches ſquare pierced 6 inches in the wall; and ſo near ſquare, that I inquired, whether it had not been made by art; but was aſſured of the contrary.

The north and ſouth doors of the tower were both blown out, and broke in many pieces. Many of the arch-ſtones over both doors were disjointed and diſplaced: two of the ſtones making the jamb of the ſouth door at *g* were forced quite out, and one of them broke.

The vaulting of the eaſt door-way C was plaiſtered underneath: the plaiſter was ſprung from the ſtone in 30 or 40 places, like as if a ſmall bar of iron had

 been

been drove from above thro' the joints of the ſtone, and thereby forced off the plaiſter with its end. The lath and plaiſter partition, which ſtopped up the upper part of this door-way, was forced into the church, and the wainſcotting making the back of the laſt ſeat was torn from the wall from end to end. Some part of the vapour ſeems to have made its way thro' the cavity under the ſeats; for moſt of the boards compoſing the riſe of the ſteps from ſeat to ſeat were blown out forwards; and ſeveral panels of wainſcot at each end of the ſeats, at *d* and *e*, were forced out, and broke. Hence the vapour ſeems to have divided itſelf into three branches; one moving directly forward to the eaſt window G, being 13 feet wide, and about 20 feet high, conſiſting of five principal lights divided by ſtone mullions: two of the lights were in a manner wholly deſtroyed, and ſeveral large holes in thoſe remaining; the glaſs and lead being carried outward, like as if an harlequin had leaped thro' the window. The north window E, fronting the broken panels at *d*, was very much ſhattered: but the ſouth window F had ſcarce a whole pane left.

It is farther to be noted, that almoſt all the lights in the church, tho' not broke, were bagged outward; but thoſe parts remaining intire in the window D moſt remarkably ſo.

N. B. It was ſaid in the London papers, that the organ was intirely ſpoilt: it is certain there is not, nor ever was, any organ in this church.

XXVI.

XXVI. *An Account of the Case of the late Right Honourable* Horace *Lord* Walpole; *being a Sequel to his own Account published in the* Philosophical Transactions, *Vol.* xlvii. *p.* 43. *and* 472.

I.

Copy of a Letter from John Pringle, *M. D. F. R. S. to Dr.* Robert Whytt, *Professor of Medicine in the University of* Edinburgh, *and F. R. S. relating to the Case of the Right Honourable Lord* Walpole; *with Dr.* Whytt's *Answer. Communicated by Dr.* Pringle.

SIR, London, 22 Feb. 1757.

Read April 21, 1757.

I Imagined, that upon hearing of Lord Walpole's death, you would be desirous to know the state of his Lordship's health from the time he published his own case; whether he continued the use of his medicines to the last; what distemper he died of; and, if his body was opened, what was the condition of his bladder and kidneys. I informed myself, as well as I could, of all these particulars; and I hope I shall be able to give you some satisfactory account of most of them.

Last year, in the month of March, about ten months before his Lordship's death, I happened to meet him at a friend's house, where he dined, and never saw any man of his age with a more healthful appearance. He was then in his 78th year. He eat with

with an appetite, and of a variety of dishes; drank some Madeira, and was very chearful the whole time. His Lordship then told me, that he had enjoyed perfect health since he sent his case to the Royal Society; that he thought it probable there was still a stone in his bladder, but so diminished, or smoothed, as to give him no uneasiness; that he did not think it safe to go about the streets of London in a coach, but that he went every where in a chair; and that, in the country, he could travel 40 miles a day in his post-chaise, without fatigue, or feeling any of his old pains upon the motion. That he continued to drink, for a constancy, three pints of oyster-shell lime-water daily; and to take, as often, from half an ounce to a whole ounce of soap, by way of lenitive. All these circumstances I am sure of, because I noted them down when I came home.

From this time to the beginning of winter, Lord Walpole (as Mr. Graham, his apothecary, informed me) continued in the same state of health; but some time after coming to town, his Lordship was seized with a lingering feverish disorder, very much affecting his spirits, but intirely unconnected with the stone. Dr. Shaw, who attended his Lordship for about a fortnight before his death, told me, that there had never been any stoppage of water, or passing of bloody urine, or any pain about his bladder or kidneys, during his last illness; but that he now and then felt some irritation in making water, a symptom too inconsiderable to require any other medicine than the continuation of his lime-water; which, in a smaller quantity, he drank till within two or three days of his end.

Mr.

Mr. Ranby and Mr. Hawkins, ſurgeons, with Mr. Graham, were preſent at the opening of the body; and from the two laſt I received the account of the diſſection.

The coats of the bladder appeared to be a little thicker than natural, but were otherwiſe ſound. The *glandula proſtata* was of a large ſize, but not diſtempered. They found three *calculi*, two lying looſe in the bladder, and the other, a very ſmall one, ſticking in the paſſage, at that part, which is ſurrounded by the proſtate gland. Mr. Graham favoured me with a ſight of them all. The two firſt were very much alike, being of the ſhape and ſize of the kernel of a Spaniſh nut; only the ſides were irregularly flattened, but without forming any ſharp angle. The ſurface of each was every where ſmooth, except where there had been a ſeparation of ſome ſmall ſcales, not ſo thick as one's nail; and the largeſt exfoliation from one of theſe ſtones appeared to have been nearly about the breadth of the nail of my little finger. The poliſh otherwiſe, as well as the colour of both, might be compared to a boy's marble. One of theſe *calculi* weighed 21 grains, the other 22 grains: they were heavy for their bulk, and ſeemingly of a hard ſubſtance. The ſmalleſt ſtone having been put up with ſome others of the ſame ſize, taken out of the gall-bladder, Mr. Graham could not be poſitive which of them it was; and therefore I can only ſay, that what he thought moſt likely to be ſo, was about the ſize and ſhape of the ſeed of an apple, with the point broken off and the edge ragged. This, as I obſerved, was found in the paſſage, ſeemed to be coming away, and probably had occaſioned that irritation

tation the patient had now and then felt during his laſt illneſs. It weighed only about a grain.

No parts could have a ſounder appearance than both the ureters and kidneys. The firſt were not dilated; nor did the laſt contain any ſtone, mucus, or gravel: the pelvis in each was of a natural ſize.

The reſt of the abdominal *viſcera* were in the ſame healthful ſtate, except the gall-bladder, which was full of ſtones. The largeſt was about the ſize of a ſmall cheſnut, but rounder. The ſurface was ſmooth, particularly at one part, where it ſeemed to have rubbed upon a leſſer *calculus*, of the ſhape of one of the *vertebræ* of a ſmall animal, without the proceſſes. This laſt had a hollow on each ſide correſponding to the convexity of the large ſtone; and theſe cavities being finely poliſhed, it ſeemed as if ſometimes one ſide, ſometimes the other, of the ſmall ſtone had been turned to the great one, and had been ſhaped in that manner by the attrition. The largeſt *calculus* weighed one drachm two ſcruples and two grains; the ſmall one but nine grains: they both ſunk in water; and felt ſpecifically heavier than any ſtones I have ever ſeen taken out of the gall-bladder. Beſides theſe two, there were ſeveral very ſmall *calculi* of irregular ſhapes, and of rough ſurfaces, which all together did not weigh above five grains. Mr. Graham, who had attended his Lordſhip for about 40 years, aſſured me, that he never had any ſymptom that indicated a ſtoppage of the bile, or the paſſage of a ſtone from the gall-bladder into the inteſtines.

Neither the head nor breaſt were opened.

Theſe are all the materials, I can furniſh you with, relating to this caſe. If you deſire to be more particularly

cularly informed of any of theſe circumſtances, let me know, and I will endeavour to procure you all the lights I can. In the mean while, I ſhould be glad to have your remarks upon what I have now ſent you; and ſince you have been ſo long in the train of thinking, with more than uſual attention, on this ſubject, I preſume it would be very agreeable to the gentlemen of the Royal Society to have a paper from you on this occaſion; and the rather, as his Lordſhip began his courſe of ſoap and lime-water, upon hearing of your ſucceſs by that method of cure. I am,

SIR, *&c.*

John Pringle.

II.

Some Obſervations on the Caſe of the late Right Honourable Lord Walpole, *of* Woolterton: *In a Letter to Dr.* John Pringle, *F. R. S. By* Robert Whytt, *M. D. F. R. S.*

SIR, Edinburgh, March 16. 1757.

Read April 21, 1757.

PHYSICIANS have not, perhaps, differed more widely in any thing, than in their opinions of the medicines lately propoſed for the cure of the ſtone. While ſome imagined, that Mrs. Stephens's medicines, or ſoap and lime-water, were in moſt caſes to accompliſh a diſſolution of the ſtone; others have been poſitive, that nothing of this kind was to be expected from them: nay, they have condemned theſe medicines, when uſed in large quantities, and long perſiſted in, as hurtful to the

ſtomach, guts, and urinary paſſages; and have aſcribed the remarkable eaſe, which they almoſt always give to calculous patients, to their depoſiting a calcarious powder upon the ſurface of the ſtone, by which it is rendered leſs hurtful to the bladder. And this opinion ſeems to have been not a little ſtrengthened, by the great quantity of white ſediment obſerved in the urine of thoſe patients, who have uſed ſoap and lime-water in conſiderable quantities. Now, as I am of opinion, that moſt of theſe objections and doubts, concerning the effects of ſoap and lime-water in the cure of the ſtone, may be cleared by a candid conſideration of Lord Walpole's caſe, I ſhall trouble you with a few remarks, which have occurred to me, in comparing it with the appearances found in his Lordſhip's body after death, of which you were ſo obliging as to ſend me a particular account.

1. Whatever doubts may have been entertained concerning the cauſe of Lord Walpole's complaints, yet it now appears evidently beyond diſpute, that they muſt have been owing, not to a ſcorbutic corroſive humour in his bladder, as was imagined by ſome (1), but to ſtones lodged in it. Theſe ſtones may poſſibly have lain there ſince 1734; for from that time to ſpring 1747, his Lordſhip was free of any graveliſh complaints, only paſſing ſome red ſand at times. But at what time ſoever they may have firſt arrived in the bladder, in 1747 and 1748 they

(1) Philoſoph. Tranſact. Vol. xlvii. p. 48. and Eſſay on the Virtues of Lime-water, &c. edit. 2d. p. 197.

ſeem

ſeem to have acquired ſuch a bulk, or were become ſo rough or pointed in their ſurface, as to occaſion great pain, frequent provocations to urine, and ſometimes bloody urine; eſpecially after any conſiderable motion. Theſe complaints, however, were ſoon relieved, by ſwallowing daily an ounce of Alicant ſoap, and three Engliſh pints of lime-water made with calcined oyſter-ſhells: and from 1748 to 1757 his Lordſhip was kept almoſt intirely free from any return of them, except for ſome months of 1750 and 1751, during which he took only one-third part of the quantity of ſoap and lime-water above-mentioned (2).

2. It is highly probable, nay, I think, altogether certain, that the ſoap and lime-water not only relieved Lord Walpole of the painful ſymptoms occaſioned by the ſtones in his bladder, but alſo prevented their increaſe.

If theſe ſtones came into the bladder in 1734, they muſt, in ſo many years as his Lordſhip lived after this, have acquired a very great bulk: nay, if we ſuppoſe them not to have been lodged in the bladder above a year before they began to occaſion frequent inclination to make urine, with pain, and ſometimes ſudden ſtoppages of urine; yet, from 1746 to 1757, they ought to have grown to a much larger ſize than that of the kernel of a Spaniſh nut (3). 'Tis

(2) Philoſoph. Tranſact. Vol. xlvii. p. 48 and 473. and Eſſay on Lime-water, p. 157 and 200.

(3) The two ſtones found in Lord Walpole's bladder were of this ſize, and weighed one of them 22 and the other 21 grains.

true, the ſtone may increaſe faſter in ſome patients, and ſlower in others; but ſtones, after remaining a dozen or more years in the bladder, generally weigh ſeveral ounces. Some years ſince I ſaw a ſtone, weighing near ſix ounces, taken from a boy of no more than 14 years of age.

3. Lord Walpole's caſe not only ſhews the power of ſoap and lime-water to relieve the painful ſymptoms, and prevent the increaſe, of the ſtone in the bladder, but alſo makes it probable, that theſe medicines do communicate to the urine a power of diſſolving the ſtone.

In the beginning of 1749 his Lordſhip voided with his urine a calculous ſubſtance of a flat ſhape, about the bigneſs of a ſilver penny, and covered with a ſoft white *mucus* (4); and upon the ſurfaces of the ſtones found in his bladder there were ſome inequalities, which ſeemed to have been made by the ſeparation of thin *lamellæ* or ſcales. Further, the ſmall ſtone found in the beginning of the *urethra* muſt have been in a diſſolving ſtate, and conſiderably leſſened in the bulk: for, if it had lain long in the bladder, and never been larger, it ought to have been voided thro' the *urethra* with the urine; and it could not have arrived lately in the bladder, ſince Lord Walpole had not had, for ſeveral years before his death, any nephritic pains, or ſymptoms of ſtones paſſing from the kidnies; and ſince it is not likely, that a ſtone of the ſize and ſhape of the ſeed of an

(4) Philoſoph. Tranſact. Vol. xlvii. p. 47.

apple

apple (5) would paſs thro' the ureters without being felt. Now if this ſmall ſtone, found in the *urethra*, was partly diſſolved by the virtue of the ſoap and lime-water; it will appear at leaſt probable, that the two larger ſtones in the bladder were ſo likewiſe. But altho' Lord Walpole's calculous concretions had remained undimininiſhed, and without any ſymptoms of diſſolution; it would not therefore follow, that ſoap and lime-water cannot diſſolve the ſtone in other patients, where the concretion may be of a leſs firm texture.

The Revd. Dr. Richard Newcome, now Lord Biſhop of Llandaff, while drinking two Engliſh quarts of lime-water daily, for the cure of the ſtone in his bladder, poured his urine every morning and evening upon a piece of human *calculus* weighing 31 grains; by which, in the ſpace of four months, it was reduced to three pieces, weighing in all only ſix grains. Upon one of theſe pieces, weighing 2.31 grains, he cauſed to be daily poured, for two months, the freſh urine of a perſon, who drank no lime-water; at the end of which time the piece of *calculus* was found to weigh 2.56 grains, having increaſed in weight a quarter of a grain. This ſame piece being afterwards ſteeped in the biſhop's urine (who continued to drink lime-water as above), from June 24th to July 9th, was in theſe few days quite crumbled into powder. Since this experiment ſhews, beyond diſpute, that lime-water, unaſſiſted by ſoap, can communicate to the urine a power of diſſolving the ſtone out of the

(5) The ſtone found in the beginning of the paſſage from the bladder was of this ſize, and weighed about a grain.

body,

body, it can ſcarcely be doubted, that it muſt have the like effect on it, when lodged in the bladder. And that the diſſolution of the ſtone in the bladder has been completed by ſoap alone, appeared evidently in the caſe of the Rev. Mr. Matthew Simſon, Miniſter of Pancaitland near Edinburgh; an account of which will ſoon be made public (6) by Dr. Auſtin, who opened his body after death. Mr. Simſon had, from 1730, been afflicted in a leſs or greater degree with the ſymptoms of a ſtone in the bladder; and in November 1735 was ſounded by Dr. Drummond of Perth, and Mr. Balderſton, ſurgeon in this city, by whom a ſtone was not only plainly felt, but alſo by the patient himſelf. In February 1737 he began to take ſoap; and after 1743 never had any graveliſh ſymptoms. He died in May 1756; and, when his bladder was looked into, there was neither ſtone nor gravel found in it.

4. It appears from Lord Walpole's caſe, that ſoap and lime-water, even when taken in large quantities, proceed very ſlowly in diſſolving the ſtone.

From July 1748, to the beginning of 1757, his Lordſhip drank three Engliſh pints of lime-water, and ſwallow'd for the moſt part an ounce of ſoap, daily; except from April 1750 to June 1751, during which time he took only one pint of lime-water, and one-third part of an ounce of ſoap, daily. However ſpeedily ſoap and lime-water may diſſolve the greateſt part of urinary ſtones out of the body, yet

(6) It is printed in this volume of the Philoſoph. Tranſactions, under the 28th of April, p. 221, & ſeqq.

being

being mixed with the aliment and humours of the ſtomach and guts, and afterwards with the whole maſs of blood, it is impoſſible but their force muſt be greatly impaired before they arrive with the urine at the bladder. When, therefore, urinary ſtones are of an uncommon hard texture, we are perhaps ſcarcely to expect any ſenſible diſſolution of them by the uſe of ſoap and lime-water: but when they are of a ſofter kind, there is no reaſon to doubt, that theſe medicines will in time diſſolve them; and this will happen ſooner or later, in proportion to the hardneſs of the ſtone, to the quantity of the medicines ſwallowed by the patient, and the exact regimen he obſerves, as to diet (7).

But however ſlowly ſoap and lime-water may proceed in diſſolving the ſtone, yet they generally give ſpeedy relief to the patient. Lord Walpole did not take theſe medicines in the full quantity till the end of July 1748; and, in a few months after, he was not only greatly relieved of all his complaints, but in December was able to ride an hundred miles in his coach, without finding any uneaſineſs, altho' the two laſt days of the journey the horſes went at a full trot (8). In winter 1750, and ſpring 1751, when his Lordſhip ſwallowed only one third part of the ſoap and lime-water, which he had been in uſe to take, his pains and frequent inclination to make urine returned in a good degree (9); but, after taking the medicines in the full quantity, he ſoon became as eaſy as before (10).

(7) Eſſay on the Virtues of Lime-water, 2d edit. p. 140.
(8) Phil. Tranſact. Vol. xlvii. p. 46.
(9) Phil. Tranſact. Vol. xlvii. p. 472, 473.
(10) Eſſay on Lime-water, &c. p. 200, 201.

It

It would ſeem, while Lord Walpole uſed only one pint of lime-water and one third of an ounce of ſoap daily, that the petrifying quality of his urine was not intirely deſtroyed, and that the ſtony particles newly formed on the ſurface of the *calculi* occaſioned, by their roughneſs, the return of his painful ſymptoms. However, when he had recourſe to the medicines in a larger quantity, the petreſcent quality of his urine was not only deſtroyed, but this fluid ſeems to have acquired a power of diſſolving the rough ſtony particles depoſited on the ſurface of the *calculi*; and in this way ſoon removed the pain, bloody urine, and frequent deſire to make water, upon uſing any conſiderable exerciſe.

Soap and lime-water not only relieve the painful ſymptoms occaſioned by the ſtone, by wearing off its ſharp points, and rougher parts, which uſed to irritate the tender membrane, which lines the bladder; but, when this membrane has been wounded or lacerated by the ſtone, there is nothing, that will heal it more ſpeedily than lime-water; which the ingenious Dr. Langriſh has found to be remarkable alſo for its effects in curing the bladders of dogs, after being fretted with ſoap-lees (11).

The power of ſoap and lime-water to alleviate the painful ſymptoms attending the ſtone is ſo great, that, as far as I remember, I have only met with one patient, who did not find himſelf conſiderably relieved by them. But it is to be obſerved, that this patient neither took them in full quantity, nor perſiſted in their uſe for a long enough time: and, when he was afterwards cut, the ſtone taken out of his

(11) Phyſical Experiments, p. 19.

bladder

bladder was almoſt as thick ſet with ſharp prickles, as the back of an hedgehog: ſo that, in this caſe, no remarkable eaſe could be procured to the patient by the medicines, until they had quite diſſolved theſe ſharp points, and rendered the ſurface of the ſtone ſmooth and equal; which was not to be done but after a very long time, eſpecially as the ſtone was of a pretty hard texture.

It may be proper to take notice, that when, along with the ſtone, there is any ulceration in the bladder, ſoap does miſchief, and lime-water often fails of giving any conſiderable relief. However, even in this caſe it is perhaps one of the beſt remedies we know.

5. Soap and lime-water, taken in large quantities, and perſiſted in for many years together, appear to be innocent, and no way injurious to health.

Lord Walpole, who uſed theſe medicines for upwards of eight years, was not only relieved of the painful ſymptoms of the ſtone, but had his health improved by them in other reſpects (12). His appetite, healthful look, and a degree of ſpirits uncommon at his age, continued till the end of 1756, when his laſt illneſs began firſt to attack him. And as his health did not appear to be any way injured by theſe medicines; ſo, when his body was opened after death, his kidneys and ureters were obſerved to be quite ſound and natural, as was likewiſe his bladder; only its coats appeared a little thicker than uſual, owing probably to the long-continued friction of the ſtones upon it. Neither the kidneys, ureters, nor

(12) Eſſay on Lime-water, &c. 2d edit. p. 171, 201.

bladder, were loaded or crusted with any calcarious matter; an effect most unjustly ascribed to soap and lime-water, since in the urinary passages, to which the air has no access, they cannot deposite their calcarious part (13); and since the white stuff observable in the urine of such patients, as take these medicines in large quantities, is only the usual sediment of the urine changed in its nature and colour, with, perhaps, some of the dissolved particles of the stone (14).

As the urinary passages were no-way injured, so neither were the stomach, guts, and other *viscera* of the lower belly. These had all a healthful appearance, except the gall-bladder, which was almost full of biliary concretions: nor is it surprising, that soap and lime-water, which prevent the growth of urinary *calculi*, should have no effect on biliary stones, since, altho' these medicines dissolve the former out of the body, yet they do not make the smallest impression on the latter.

I presume it will be needless to take notice, that the lingering nervous fever, of which Lord Walpole died, cannot, with any colour of reason, be ascribed to the large use of soap and lime-water; since, if they could have produced such an effect, they must have done it in much less time than eight years and an ha f

It may not be amiss to observe, that altho' soap and lime-water, taken in large quantities, are no-way injurious to health, yet in some cases they may

(13) Essay on Lime-water, &c. 2d edit. p. 170.
(14) Ibid. p. 24, 25. 30 & 31.

become

become improper, on account of the particular ſtate of the patient. Thus, in a ſcorbutic or putrid diſpoſition of the humours, ſoap at leaſt ought to be totally omitted; and ſuch patients, who are much troubled with the hæmorrhoids, ought to be ſparing in its uſe, as the alkaline ſalt, with which it abounds, will ſcarcely fail to exaſperate their pain. Where the patient is naturally very coſtive, leſs lime-water and more ſoap ought to be uſed; and, on the contrary, where the body is too looſe, little or no ſoap is to be taken, but the cure is to be truſted to lime-water alone; which, in this caſe, ought to be drank to the quantity of two Engliſh quarts a day.

As the foregoing obſervations will, I am afraid, appear more tedious than important, I ſhall only add, that I am, with great eſteem,

SIR,

Your moſt obedient humble Servant,

Robert Whytt.

III.

Dr. Pringle's *Paper read after Dr.* Whytt's *Letter.*

Read April 21, 1757.

DR. Pringle begs leave to inform the Society, that having read the copy of his letter, within theſe few days, to Dr. Shaw, Mr. Hawkins, and Mr. Graham, thoſe gentlemen found his account agreeable to their ſeveral obſervations; only Mr. Graham took notice, that, of late years, Lord Walpole, in his journies to Norfolk, had

twice voided ſome blood with his urine, but with little uneaſineſs; and that at other times he had paſſed ſome ſand and ſtony particles (tho' never larger than the head of a ſmall pin), attended with frettings of the parts, ſcarce painful. But Mr. Graham was not ſure, whether theſe accidents were prior or ſubſequent to the ſequel of the caſe, communicated to the Society by his Lordſhip.

Dr. Pringle thinks it may be likewiſe proper to acquaint the Society with another circumſtance in Lord Walpole's caſe, which he had both from Dr. Shaw and Mr. Graham, *viz.* that after uſing the ſoap and lime-water for ſome time, his Lordſhip was freed from a very obſtinate dry and ſcurfy eruption, which had reſiſted ſeveral other medicines. But as there were no marks of a putrid ſcurvy (that ſpecies expreſſly alluded to towards the end of Dr. Whytt's letter) the Society will eaſily underſtand, how the lithontriptic medicines may be prejudicial to one troubled with the true putrid ſcurvy (ſuch as is moſt incident to ſailors) and yet not be improper for thoſe, that are ſubject to the ſcurfy eruptions, which are commonly, tho' erroneouſly, called ſcorbutic.

Pall-Mall, 20 April,
1757.

XXVII.

XXVII. *An Account of the Virtues of Soap in diſſolving the Stone, in the Caſe of the Rev. Mr.* Matthew Simſon. *Communicated by* John Pringle, *M. D. F. R. S.*

To the Rev. Tho. Birch, *D. D. F. R. S.*

SIR,

Read April 28, 1757.

A Few days ago I received from Dr. Auſtin, phyſician at Edinburgh, the caſe of the Rev. Mr. Simſon, drawn up by himſelf, in the form of a letter to Dr. Auſtin; and which you may remember was alluded to by Dr. Whytt, in the paper read at the laſt meeting of the Society.

As I am at liberty to communicate this account to others, I thought it would not be unacceptable to the gentlemen of the Society, to have another well-atteſted inſtance laid before them of the virtues of ſoap in diſſolving the ſtone, or, at leaſt, in removing all thoſe painful ſymptoms, which uſually accompany that diſtemper.

To the patient's own narration I have ſubjoined an extract from Dr. Auſtin's letter to me, containing the ſequel of the caſe from the date of Mr. Simſon's letter to his death; with an account of the ſtate of his bladder, as it appeared to Dr. Auſtin upon diſſection. I am,

SIR,

Your moſt obedient humble Servant,

Pall-Mall, 27 April, 1757.

John Pringle.

A Let-

A Letter from the Rev. Mr. Simſon, *Miniſter at* Pencaitland, *to Dr.* Adam Auſtin, *Phyſician in* Edinburgh.

Dear Sir,

Read April 28. 1757.

ACcording to your deſire, I ſend you the hiſtory of my caſe; which is as follows:

I was of an healthy conſtitution till the year 1730, when I was ſeized with a frequent inclination to make water, without any previous pain in the kidneys or ureters. This ſymptom continued till the year 1733, without giving me much uneaſineſs.

In June 1733, as I was riding from Edinburgh to my own houſe at Pencaitland, I was ſeized with a great difficulty and pain in making water, which went off when I got home.

In the month of July, having again got on horſeback, I was ſeized with the ſame complaint, but more violent; for then ſome drops of blood came away. From this time, if I rode eight or ten miles, I paſſed ſome blood, but without pain.

In September I made a journey of 60 miles on horſeback; but every two miles was obliged to diſmount, and made ſome bloody water.

I continued much in the ſame way all the year 1734, as the preceding; only had one additional complaint, of a pain in the glans after making water, and likewiſe in the neck of the bladder. The only thing I did for it was, to drink plentifully of warm milk and water; and gave over riding, on account of the bloody urine.

In

In the month of Auguſt I was ſounded by my nephew, Dr. Simſon, profeſſor of medicine in the univerſity of St. Andrew's; but he found no ſtone, which he attributed to a wrong poſture I was in, when he ſounded me.

During the winter, if I walked more than uſual, I was ſure to have a return of the bloody urine and ſtrangury.

In November 1735, I was ſounded by Mr. Balderſtone, ſurgeon, in Edinburgh, a gentleman very expert in that operation, and likewiſe by Dr. Drummond of Perthſhire. They both diſtinctly felt a ſtone: and I myſelf took hold of the catheter, when it was in my bladder, and felt the ſtone as diſtinctly, as if it had been in my hand.

About Chriſtmas I was ſeized with a pain along the left ureter, and violent vomitings; but, upon uſing a turpentine clyſter and opiates, it went off.

During the year 1736, I continued much the ſame as the preceding year, always drinking great plenty of milk and water; which gave me great relief, as to the bloody urine.

I was adviſed by my nephew, Dr. Simſon, to go to London, and be cut by Mr. Cheſelden; the reſt of my friends adviſing me to be cut by Mr. Smith, a lithotomiſt at Perth. However, I deferred the operation, and continued much the ſame all the year 1737, having ſevere fits now and then.

In the year 1738 Sir Alexander Gibſon, of Addiſton, informed me, that he had been in my condition, had paſſed ſeveral ſmall ſtones, and had found incredible ſervice from the uſe of ſoap pills: for, from not being able to get out of bed, in the ſpace

of

of two months after uſing the ſoap he was able to go a hunting. However, for ſome time I was afraid to try the ſoap, not knowing what effects it might have on a confirmed ſtone; Sir Alexander Gibſon's caſe being only that of ſmall ſtones. But the Rev. Mr. Lundie, of Salton, by experiments convinced me of the efficacy of ſoap in diſſolving a confirmed ſtone out of the bladder; for the ſtone gradually grew ſmoother and ſmoother, and at laſt was quite diſſolved.

On the 12th of February 1739, I firſt began the uſe of the ſoap, and in the beginning took only a drachm in the 24 hours. The firſt week it made me a little qualmiſh: however, I gradually increaſed the doſe; ſo that in ſix weeks I took ſix drachms a day, without its diſagreeing in the leaſt with me. I made it up into pills, and waſhed them down with a draught of warm milk and water.

From the time I began to uſe the ſoap, my graveliſh ſymptoms gradually abated; but, upon walking two or three miles, I made bloody urine. However, that ſymptom gradually abated; and in the year 1743 all the ſymptoms of a ſtone quite vaniſhed, inſomuch that I could walk, ride, or go in a machine, as well as ever.

From February 1739, to July 1743, I took every day five or ſix drachms of ſoap: but after that time I diminiſhed the doſe to half an ounce; and never after had any return of a graveliſh ſymptom, tho' I ſtill imagine the ſtone is not intirely diſſolved; for, after ſitting ſome time, I find as it were ſomething come to the neck of the bladder; but which gives me no uneaſineſs.

This,

This, Sir, according to the beſt of my memory, is my caſe: and if it can be of any benefit to you, in the cure of this painful diſeaſe, it will give great pleaſure to,

Dear Sir,

Your moſt obedient Servant,

Nov. 20th, 1749. Matthew Simſon.

The Extract from Dr. Auſtin's *Letter to Dr.* Pringle.

Read April 28. 1757. THE Rev. Mr. Simſon's letter to me was written in the year 1749; about which time he told me, that he had ridden 40 miles in a day, without any bad ſymptom enſuing.

In the year 1752 he broke his thigh-bone at the neck, by a fall from his horſe, and continued for ſix weeks in great pain; but after that time he grew eaſier, and was able to put his foot to the ground. One day, as his ſervant was helping him to walk acroſs the room, he let him fall; upon which Mr. Simſon felt a ſevere pain: the broken leg became then evidently ſhorter than the other; and by that misfortune he was confined to his bed for near two years. However, about ſix months before he died, he was ſo well recovered, as to be able to go to church, and to perform divine ſervice.

About the beginning of May 1756, Mr. Simſon was ſeized with a diarrhæa, which reſiſted all medicine, and carried him off in the 83d year of his age. From the date of his letter to his death he had never diſcontinued the uſe of the ſoap (except during the time

of his laſt illneſs), tho' he had not been troubled with any painful ſymptom of a ſtone ſince the year 1743.

I obtained leave of his friends to open the body, but found no ſtone or gravel in the bladder; that part appearing to be, in every reſpect, in a natural ſtate, except at the neck, where the coats ſeemed to be ſchirrous, and were about a quarter of an inch thick.

It is probable, that the ſtone had been of a ſofter texture, and more eaſily diſſolved, than ordinary; otherwiſe five or ſix drachms of ſoap taken daily, even for ſo long a time, could not have diſſolved it intirely; for many have uſed that medicine in much larger doſes, and at the ſame time have drank lime-water plentifully, without obtaining ſuch effects; tho' all their painful ſymptoms were removed by that courſe, as Dr. Whytt has ſhewn in his treatiſe on this ſubject.

I ſhall only add, that Mr. Simſon's ſon, who is now miniſter at Fala, was preſent at the opening of the body, and can atteſt, that there was no ſtone found in the bladder.

Edinburgh,
15 April 1757.

Adam Auſtin.

A Letter from Dr. Adam Drummond *to Dr.* Adam Auſtin, *relating to the Rev. Mr.* Matthew Simſon's *Caſe. Communicated by* J. Pringle, *M.D. F.R.S.*

Read June 23, 1757.

I Have yours; and was preſent when Mr. Balderſtone ſounded Mr. Simſon; and both of us perceived, very diſtinctiy, a large ſtone: and Mr. Simſon himſelf felt it; which we were

were the more ſollicitous he ſhould do, as he was ſounded before by Dr. Simſon, who had declared there was no ſtone. But the particular magnitude of it we could not well determine at the end of a long catheter; tho' I remember Mr. Balderſtone, who was well verſed in that buſineſs, conjectured it to be pretty large. He was ſounded only once by us, as the urethra was a little hurt by turning the catheter. There is only one circumſtance in the caſe, which Mr. Simſon ſeems to have omitted; that, from the firſt ſymptoms of the ſtone, he paſſed a great deal of *mucus* mixed with *pus*, as well as blood; and great quantities of gritty red ſand, all in ſingle grains, never any concreted into ſmall ſtones. I take the more notice of this, as I do not remember, that, after he uſed the ſoap, he ever paſſed any ſand, but a good deal of *mucus*, in which the ſoap was diſcoverable by its frothing. Could the gritty particles of ſand be again ſuſpended in the urine, ſo as to become inviſible? or were they wrapt up in the ſoapy liquid, ſo as to eſcape obſervation? I have ſeen ſeveral ſtones of a ſoft conſiſtence diſſolved into mucilage by ſoap: but the ſand paſſed by Mr. Simſon, before he uſed the ſoap, ſeems to indicate his ſtone of a harder nature, tho' indeed it felt obtuſe at the end of the catheter.

I ſhall rejoice, if many inſtances of this kind are found afterwards: but this ſeems to be the only one yet, of a ſtone in the bladder being diſſolved by ſoap alone. I am,

Dear Doctor,

Bandeeran, June 5. 1757.

Your moſt humble Servant,

Adam Drummond.

XXVIII. *An Account of the Impreſſions of Plants on the Slates of Coals: In a Letter to the Right Honourable* George *Earl of* Macclesfield, *Preſident of the R. S. from Mr.* Emanuel Mendes da Coſta, *F. R. S.*

My Lord,

Read April 28, 1757.

I Have the honour to addreſs this letter to your Lordſhip, in order to be communicated to the Royal Society, if your Lordſhip deems it worthy the attention of that learned and illuſtrious aſſembly.

The impreſſions of various kinds of plants are frequently, I might ſay always, found in ſome of the ſtrata lying over coal; but more particularly in a ſtratum of earthy ſlat; which, in my Hiſtory of Foſſils, page 168. Species IV. I have ſynonymed *Schiſtus terreſtris niger carbonarius*, and which always lies immediately upon the coal-ſtratum, not only in the coal-pits of this kingdom, but of many other parts of Europe, *e. g.* France, Saxony, Bohemia, Sileſia, *&c.*

Moſt of theſe impreſſions, my Lord, are of the *herbæ capillares et affines*, the gramineous, and the reed tribes: but, however, among them many rare and beautiful impreſſions undoubtedly of vegetable origin, and impreſſed by plants hitherto unknown to botaniſts, are not unfrequently met with.

Beſides theſe, my Lord, found over coal-pits, there are likewiſe found in ſome parts of this kingdom, as at Robinhood's-bay in Yorkſhire, Coalbrookdale in

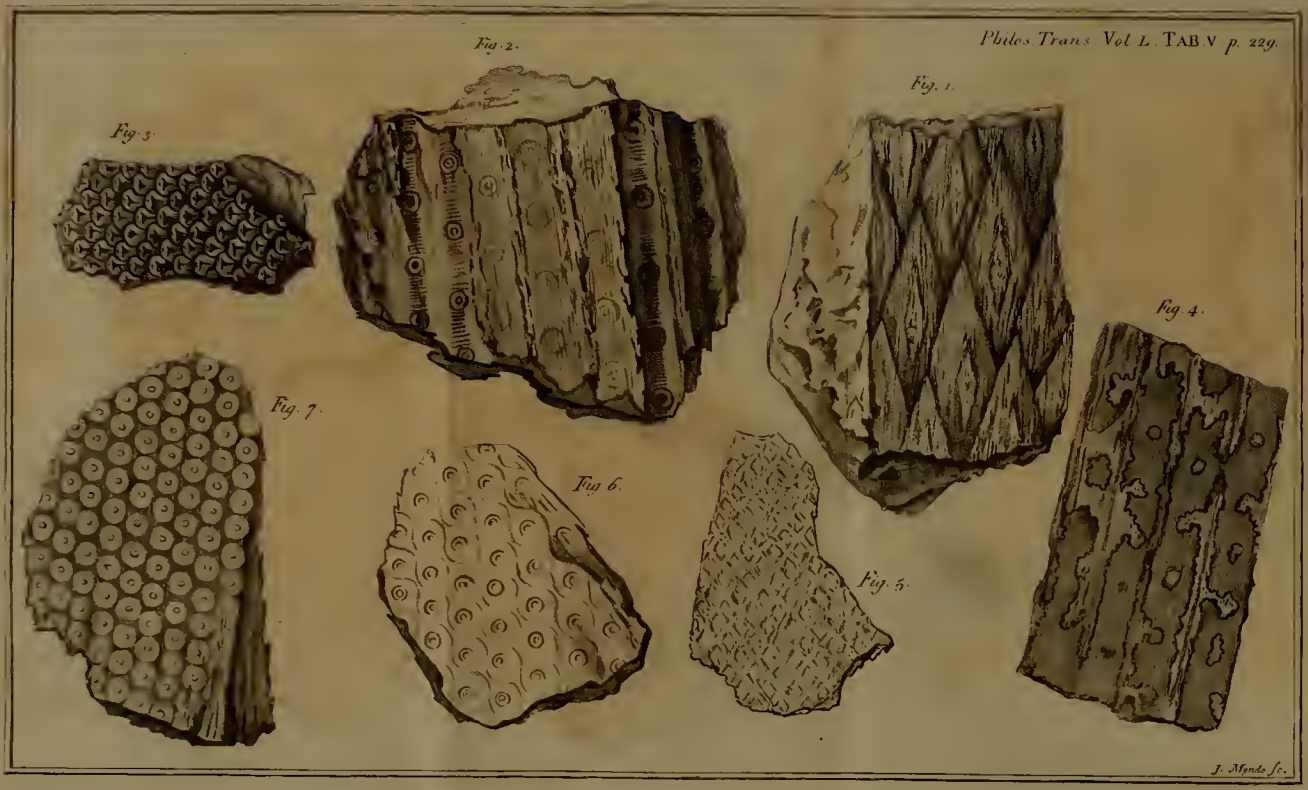


Fig. 2.
Fig. 1.
Fig. 3.
Fig. 4.
Fig. 7.
Fig. 6.
Fig. 5.
J. Mynde sc.

in Shropſhire, *&c.* many curious impreſſions of the fern tribe in regular nodules of iron-ſtone; and, in the latter place, not only impreſſions of plants, but even the cones or iuli of ſome kinds of trees are met with, very perfect and fair, and curiouſly imbedded in maſſes of iron-ſtone.

The moſt part of the impreſſions of ferns, graſſes, *&c.* are eaſily recognizable, they ſo minutely tally to the plants they repreſent. Others indeed, tho' they do not exactly anſwer any known ſpecies, yet have characters ſo diſtinctly expreſſed, that they are eaſily arranged under their reſpective genera (1). Therefore I ſhall not trouble your Lordſhip with any further remarks on all ſuch, but ſhall only touch on thoſe elegant and extraordinary impreſſions, probably of unknown vegetables, above-mentioned: for that they are the parts and impreſſions of vegetables, I think clearly evinced, if we attentively and with a philoſophical mind conſider them, and reflect on the various circumſtances, which attend them in the places, where they now lie buried.

I have therefore the honour, my Lord, to exhibit the drawings of ſeven ſuch extraordinary impreſſions, and the foſſiles themſelves, for your Lordſhip's and this learned body's inſpection (*See* TAB. V.). The impreſſion figured N°. 1. is from Mr. Mytton's collieries at Drilt, near Oſweſtry, in Shropſhire; as are alſo thoſe figured N°. 2, 4, and 7: they are found

(1) The celebrated Dr. Scheuchzer has arranged the foſſile plants botanically, by Tournefort's ſyſtem, in his folio work, intituled, *Herbarium Diluvianum*; and Dr. Woodward's foſſile plants, Catalogue B, he informs us, were botanically conſidered and arranged by thoſe famous botaniſts Dr. Plukenet, and Meſſ. Doody, Buddle, and Stoneſtreet.

 ſome-

ſometimes two feet in length, and are generally covered with a thin cruſt of coal. The ſpecimina Dr. Woodward exhibits, Catalogue B, pages 106, 107. ſpecimina *q*. 22. and *q*. 32. are analogous to this, tho' not exactly the ſame. The Doctor's foſſiles were from Haigh in Lancaſhire; and he imagines the impreſſions to be made by vegetables of the fir kind. Volckman alſo, in his *Sileſia ſubterranea*, tab. 22. fig. 2. figures a branch with a rhomboidal work on it, and with three long narrow leaves, which ſeems akin to this impreſſion.

N°. 2. ſeems of the reed tribe: the knobs placed in rows, which are like the veſicles on the *quercus marina*, and ſome other *algæ*, are very remarkable. Woodward, Catalogue B. page 9. ſpecimen *a*. 1. exhibits an impreſſion akin to this, which he imagines to be of the fern kind.

N°. 3. from a coal-pit in Yorkſhire. I cannot but think this impreſſion is owing to ſomewhat of the fir kind. Dr. Woodward, who exhibits ſuch a like impreſſion, Catalogue B. p. 16. ſpecimen *a*. 108. imagines the ſame: his words are, "The impreſſion is much like what might be made by the branches of the common fir, after the leaves are fallen or ſtript off."

N°. 4. ſeems to be of the ſame kind as N°. 2.

N°. 5. This extraordinary impreſſion is from Moſtyn-colliery in Flintſhire. It is a little obſcured; but, when attentively viewed, exhibits a reticular impreſſion, the meſhes whereof are rhomboidal hollows, and the ſides of the rhombs, or the net-work, are raiſed, or in relief.

N°. 6. is from Newcaſtle. Volckman, ibid. part 3. tab. 4. fig. 9. ſeems to be of this kind.

N°. 7.

N°. 7. The ſame author, Volckman, figures a ſomewhat-like impreſſion, ibid. fig. 5.

Only theſe ſeven extraordinary impreſſions I have preſumed, my Lord, to treat of at this preſent time; but I have many more in my cabinet equally curious, ſome few of which I here exhibit to the Society, without taking any further notice of them: only I ſhall add, that many extraordinary impreſſions occur in Woodward's and other collections, and many are iconed in authors, worthy the attention of the curious.

Theſe impreſſions, my Lord, are not only met with in ſmall pieces; but large evident branches, ſome feet in length, have been found. I have, in the collieries of Derbyſhire, frequently traced branches with (what ſeemed to me) long narrow leaves proceeding from them, and parts of other vegetables, above a foot's length: but the hardneſs of the ſubſtance they are immerſed in renders it impoſſible to get them out without breaking them to pieces (2).

As theſe remains of vegetables are very extraordinary, I would recommend to the curious in botany to take notice of them, as an *Appendix Plantarum adhuc incognitarum*. For my part, I am ſo very little ſkilled in botany, that I hardly preſume to offer my opinion; which is, that they are impreſſions and parts of ſpecies of the firs and pines, of the tithymals, the cereus's, and other arboreſcent plants, and of large reeds; for ſome of the ſaid

(2) Woodward, Catalogue B. p. 104. ſpecimen *q*. 1. was of 6 1-half feet in length; and Catalogue D. p. 60. ſpecimen *h*. 38. was a yard long; *et alibi paſſim*. In the collieries at Swanvich in Derbyſhire, in 1752. a plant of the cane kind was found 14 feet long: it ended in a point at one end, and at the root in a large knob, and in the middle meaſured nine inches about.

kinds

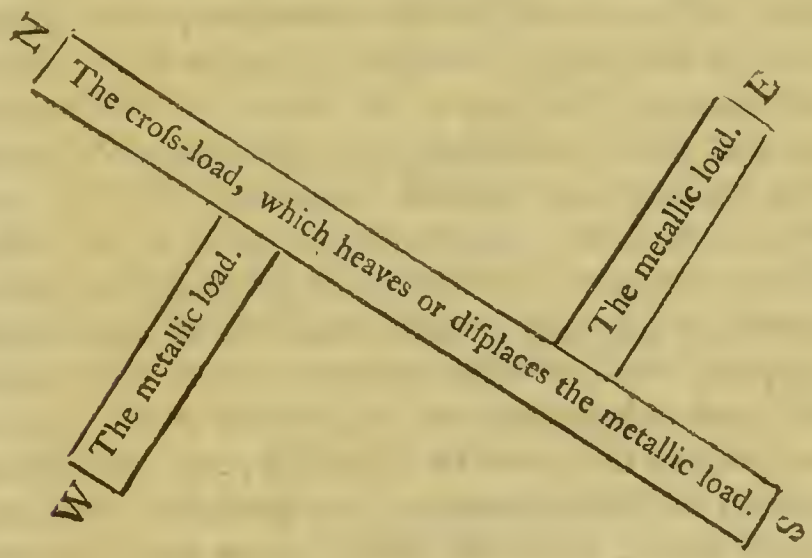
kind are embellifhed with ribbed, ftudded, and reticulated works; *e. g.* the Hercules' club, or *rubi facie fenticofa planta Lobelii*, defcribed by Dr. Grew, *Mufeum Reg. Soc.* p. 221. the *cerei*, *&c.*

I further exhibit to the Society fome few fpecimina of iron-ftones with cones or iuli imbedded in them. Thefe, my Lord, are from veins of ball iron-ftone, in the lands of Lord Gower, at Okenyate, a village on the Roman road of Watling-ftreet; and from the iron-works at Coalbrookdale in Shropfhire. The cones are frequently met with in fragments, but rarely fo intire, and are never found but in the ftrata of iron-ftone. I have added to thefe a figured foffile body, much like a cone, found fometimes in our chalk-pits in England, but chiefly in the pits at Cherry-Hinton in Cambridgefhire. Dr. Woodward, Catalogue B. p. 22. fpecimen *b.* 72. calls them cones feeming to be of the larix; and imagines they were not come to ripenefs or maturity. They certainly have fome refemblance to cones, tho' I much doubt them to be fo; but they moft exactly refemble the roots of the *cyperus rotundus vulgaris* of botanifts.

I fhall finifh this paper, my Lord, by acquainting your Lordfhip and the Society, that I firmly believe thefe bodies to be of the vegetable origin, buried in the ftrata of the earth at the time of the univerfal deluge recorded by Mofes. It is, I muft confefs, with regret, that I find there are fome, who reject the burial of thefe bodies at that fatal cataftrophe, but fubftitute partial deluges to account for it. Did thofe gentlemen confider, or maturely weigh, the many remarkable and ftrong evidences of an univerfal deluge, every-where obvious in the bowels of the earth, they certainly would abandon their imaginary

fyftem:

ſyſtem: for, my Lord, it is not only the immenſe quantities of marine remains, diſperſed in all terreſtrial ſtrata, which are to be conſidered (that circumſtance alone might give ſome reaſoning to their ſyſtem of partial deluges), but the following more weighty circumſtances are likewiſe to be added and flung into the ſcale. 1°. The heavings, diſplacings, trappings, and breaks of the metallic veins, and the loads of rubble, met with at vaſt depths, and where no marine remains were ever found; and ſuch heavings, *&c.* are not rare in metallic or mineral works: of which, to give your Lordſhip an idea, I have preſumed to ſketch the following plan of ſuch a phænomenon.



Theſe croſs-loads are not unfrequent in the mines on North Downs, near Redruth, in Cornwall. Wheal-Widden copper-work there, in 1750, was about 60 fathom deep. The load was 20 feet over; and has many croſs-loads two or three feet over, which ſometimes heave the metallic load from one to five or ſix fathom. Theſe croſs-loads are generally filled with fragments of ſtone, minerals, and other rubble.

2°. If theſe effects proceed from local deluges, recedings of the ſea, gulphs atterrated, *&c.* we ſhould then indeed find marine remains: but how will that account for the vaſt quantity of remains of terreſtrial vegetables and animals, equally met with, and in like manner as the marine remains, in the bowels of the earth? And, 3°. Were local or partial deluges the cauſe, we ſhould then find only the animals and plants of the climates or places, where ſuch deluges have happened; whereas in theſe foſſile remains it is quite the contrary: the remains of thoſe plants and animals, we know, are of animals and plants, the inhabitants of the moſt remote climes from thoſe, where they now lie buried; *e.g.* the rhinoceros-bones, in the cave called Baumans-hole, in the Hartz Foreſt in Germany; the ſtrange bones in the Antra Draconum in Hungary; the horns of the mooſe-deer, and other prodigious horns, and elephants bones, found in England, Ireland, Germany, Sibiria, and even America, *&c.* of vegetables, parts of the arbor triſtis in France; bamboo's, or great Indian reeds, frequent in England; with numbers of other ſuch examples. And of thoſe remains even of the marine ſhells, yet unknown to us, all appear exotic to the climes where they now lie depoſited; *e.g.* the cliffs at Harwich in Eſſex abound with a ſpecies of *buccinum heteroſtrophum*, and other ſhells, never yet diſcovered in the adjacent waters. The *ammonitæ* of ſo many ſpecies, and the innumerable variety of *conchæ anomiæ*, with which this iſland abounds, are yet unknown to be inhabitants of our ſeas, and appear exotic to this climate. Therefore, my Lord, I reaſonably conclude partial or local deluges could never have

produced

produced ſuch effects. However, unprejudiced to any opinion, if the learned, who favour the ſyſtem of partial deluges, will either confute theſe my aſſertions, or give ſolid reaſons for the facts alleged to be producible by local deluges, atterrations, *&c.* I will joyfully embrace the truth: but till then, my Lord, I would recommend to thoſe ſyſtematical gentlemen, not to pervert that excellent maxim of the great Lord Bacon, and, inſtead of *Non fingendum neque excogitandum, ſed inveniendum quid natura faciat, aut ferat*, not to corrupt it into *fingendum atque excogitandum, non inveniendum quid natura faciat, aut ferat.*

I am, with great ſubmiſſion and reſpect,

My Lord,

Your Lordſhip's

Moſt devoted,

and moſt obliged,

humble Servant,

Emanuel Mendes da Coſta.

London, 27 April, 1757.

XXIX. *A Catalogue of the* Fifty Plants *from* Chelſea Garden, *preſented to the* Royal Society *by the worſhipful Company of Apothecaries, for the Year* 1756, *purſuant to the Direction of Sir* Hans Sloane, *Baronet, Med. Reg. & Soc. Reg. nuper Præſes, by* John Wilmer, *M. D. clariſſ. Societatis Pharmaceut.* Lond. *Socius, Hort.* Chelſ. *Præfect. & Prælector Botan.*

Read April 28, 1757.

1701 ABrotanum campeſtre incanum Carlinæ odore. C. B. P.

1702 Abrotanum humile corymbis majoribus aureis. H. Reg. Par.

1703 Acer platanoides. Muntingii hiſtor.

1704 Amelanchier. Lobel.

1705 Anchuſa lutea minor. J. B. 3. 583.
Bugloſſum luteum annuum minimum. Tourn. 134.

1706 Arctotis ramis decumbentibus foliis lineari-lanceolatis rigidis ſubtus argenteis flore magno aureo pediculo longiſſimo. Miller's Icons.

1707 Aſcyrum magno flore, C. B. 280.

1708 Aſphodelus Allobrogicus magno flore Lilii. H. L. 65.

1709 Aſter caule ramoſo ſcabro perenni foliis ovatis ſeſſilibus pedunculis nudis unifloris. Miller's Icons.

1710 Aſtragalus repens minor flore cæruleo, ſiliqua Epiglottidi ſimili. Index Plant. Roerh.

1711

1711 Barleria inermis foliis ovatis denticulatis petiolatis. Lin. Sp. Plant. 637.
Barleria ſolani folio flore coccineo. Plum. nov gen. 31.

1712 Blitum Kaly minus album dictum. Kaly minus Ger. Emac. 535.

1713 Campanula maxima foliis latiſſimis. C. B. 94.

1714 Caryophillus montanus umbellatus floribus variis, luteis ferrugineis Italicus. Barrel. obſ. 648.

1715 Cataria Hiſpanica Betonicæ folio anguſtiore flore cæruleo. Tourn.

1716 Celaſtrus ſpinis nudis, ramis teretibus, foliis acutis. Hort. Cliff. 72.
Lycium. Boerhaav. Ind. alt. 2. 237.

1717 Ceraſus racemoſa ſylveſtris fructu non eduli rubro. H. R. Par.
Ceraſia racemoſa rubra. 2. Tabernamont. Icon, 987.

1718 Chamædrys Hiſpanica tenuifolia multiflora. H. R. Par. Tourn. 205.

1719 Cherophyllum paluſtre latifolium flore albo. Boerh. 70.
Myrrhis paluſtre latifolia rubra. Tourn. 315.

1720 Chenopodium Stramonii folio. Vaill.

1721 Cirſium foliis non hirſutis floribus compactis. C. B. P. 377.

1722 Cirſium maximum Aſphodeli radice. C. B. 377.

1723 Colutea foliolis ovatis integerrimis caule fruticoſo. Phil. Miller's Icons.

1724 Convallaria foliis ſeſſilibus racemo terminali compoſito. Lin. Sp. Pl. 315.

1725 Conyza mas Theophrasti major Dioscoridis. C. B. P. 265.
Major Monspeliensis odorata. J. B. 2. 1053.
1726 Coriandrum majus. C. B. 158. Officinar. 145.
1727 Cornus Orientalis sylvestris fructu teretiformi. T. Cor.
1728 Crithmum, sive Fæniculum maritimum, minus. C. B. 288. Offic. 152.
1729 Crocus sativus. C. B. 65. Officinar. 152.
1730 Cyclamen vernum minus orbiculato folio, inferne rubente, flore minore ruberrimo. Mor. Hist. 3. 551.
1731 Elichrysum graveolens acutifolium alato caule. Hort. Eltham.
1732 Gramen spica aristata. Lin. Sp Pl. 83.
Gramen loliaceum spica longiore lolium Dioscoridis. C. B. P.
1733 Leucanthemum Tanaceti folio, flore majore. Boerh. 107.
Matricaria Tanaceti folio, flore majore, semine umbilicato. Tourn. 493.
1734 Meadia. Catesby Hist. Car. 3. p. 1. Dodecatheon. Lin. Sp. Pl. 144.
1735 Medica magno fructu, aculeis sursum et deorsum rendentibus. Tourn. 411.
1736 Moscatellina foliis fumariæ bulbosæ, de qua Cordus. J. B. 2. 206.
Radix cava minima viridi flore. Ger. 933.
1737 Narcissus Illyricus Liliaceus. C. B. P. 55.
Pancratii Monspeliaci Hemerocallidis Valentinæ facie. Lilio-narcissus, vel Narcissus tertius Matthioli. J. B. 2. 613.

1738 Oſmunda vulgaris et paluſtris. Tourn. 547.
Filix ramoſa non dentata florida. C. B. P. 357.
1739 Papaver laciniato folio capitulo hiſpido rotundiore. Tourn.
Argemone capitulo rotundiore. Park. 369.
1740 Papaver Orientale hirſutiſſimum magno flore. Tourn. Cor. 17.
1741 Periclymenum perfoliatum Virginianum ſempervirens et florens. H. L. B.
1742 Phillyrea, Oleæ Epheſiacæ folio. Pluk. Phyt. Tab. 310. fig. 5.
1743 Phlomis Lychnitis. Cluſ. Hiſt. 27.
Verbaſcum ſylveſtre Monſpelienſe flore luteo hiante. J. B. 3. 307.
1744 Polygonum foliis cordatis caule volubili, floribus carinatis. Lin. Sp. Plant. 364.
Fagopyrum ſcandens Americanum maximum. Tourn. Inſt.
1745 Saxifraga ſedi folio, flore albo, multiflora. T. 252.
Sedum Pyrenaicum pyramidale longifolium elegantiſſimum. Schol. Botan. Par.
Sanicula Pyrenaica longifolia multiflora elegantiſſima. H. L.
1746 Serratula Noveboracenſis maxima foliis longis ſerratis. Hort. Eltham. 355.
1747 Thalictrum minus. C. B. P. 337.
Thalictrum minus ſeu Rutæ pratenſis genus minus ſemine ſtriato. J. B. 3. 487.
1748 Verbaſcum fœmina flore luteo magno. C. B. 239.
1749 Vicia leguminibus ſeſſilibus reflexis piloſis ſolitariis pentaſpermis corollæ vexilis glabris. Lin. Sp. Pl. 736.

Vicia

Vicia leguminibus ſolitariis deorſum flexis hirſutis. Sauv. Monſpelienſ. 235.

1750 Ulmus folio latiſſimo ſcabro. Ger. Emac. 1481. Latiore folio. Park. 1404.

XXX. *Remarks on the Opinion of* Henry Eeles, *Eſq; concerning the Aſcent of Vapour, publiſhed in the* Philoſoph. Tranſact. *Vol.* xlix. *Part* i. *p.* 124. *By* Eraſmus Darwin, *M. D. Communicated by Mr.* William Watſon, *F. R. S.*

To Mr. William Watſon, *F. R. S.*

SIR,

THE incloſed papers were deſigned for the peruſal of the Royal Soeiety; being an endeavour to confute the opinion of Mr. Eeles about the aſcent of vapours, publiſhed in the laſt volume of their Tranſactions. But the author, having no electrical friend, whoſe ſagacity he could confide in, has at length prevailed upon himſelf to be ſo free to ſend them to Mr. Watſon; to whom the world is ſo much indebted for the advancement of their knowlege in electricity.

Whence, Sir, if you ſhould think, that theſe papers have truth, the great Diana of real philoſophers, to patronize them, you will confer a favour upon me, by laying them before that learned Body. If, on the contrary, you ſhould deem this confutation trifling or

or futil, I hope you will be humane enough to ſuppreſs them, and give me your objections; and by that means lay a ſtill greater obligation on one, who has not the pleaſure to be perſonally acquainted with you. From,

SIR,

Your very humble Servant,

March 23. 1757. Eraſmus Darwin,

Phyſician at Litchfield, Staffordſhire.

LETTER I.

To the very honourable and learned the PRESIDENT *and* MEMBERS *of the* Royal Society.

Gentlemen,

Read May 5. 1757. THERE is ever ſuch a charm attendant upon novelty, that be it in philoſophy, medicine, or religion, the gazing world are too often led to adore, what they ought only to admire: whilſt this vehemence of enthuſiaſm has generally ſoon rendered that object contemptible, that would otherwiſe have long laid claim to a more ſober eſteem. This was once the fate of chemiſtry: the vain and pompous boaſts of her adepts brought the whole art into diſreſpect; and I ſhould be ſorry, if her ſiſter electricity ſhould ſhare the ſame misfortunes. It is hence the ingenious Mr. Eeles will excuſe me, for endeavouring to lay before you my opinion on the aſcent of vapours, tho' it by no means coincides with that he is ſo ſtrenuous to eſtabliſh, and plucks a plume from his idol goddeſs electricity.

The probability, ſupporting the hypotheſis of Mr. Eeles, according to his own expreſſions, reſts on this: "That every particle of vapour is endued with a "portion of electric fire; and that there is no other "ſufficient cauſe aſſigned for their aſcending." *(Phil. Tranſ. vol.* xlix. *part.* i. *p.* 134.). My deſign is therefore firſt to attempt to ſhew, that another theory, founded on principles better known, will ſufficiently explain the aſcent of vapours: and then, that ſome kinds of vapours are not endued with a more or leſs than their natural ſhare of electric æther.

The immenſe rarefaction of exploſive bodies by heat, depends either on the eſcape of air before condenſed in them, or on the expanſion of the conſtituent parts of thoſe bodies. This diſtinction has not been ſufficiently conſidered by any one to my knowlege; nor ſhall I at preſent amuſe the Society upon this head; it being enough for my preſent purpoſe to obſerve, that they may be thus diſtinguiſhed: where air is emitted, it cannot be condenſed again into the ſame bulk by cold; but the expanſion of heated parts of bodies, as ſoon as that heat is withdrawn, ceaſes to exiſt.

Nitre comes under the firſt of theſe claſſes: in detonation it emits great quantities of air, not afterwards condenſible to the like ſpace. This may be ſeen by firing a few grains of gunpowder in an unblown bladder, or in a veſſel nearly full of water with its mouth inverted. The ſame is true of all the ſolid parts of animals and vegetables, when ſubjected to fire; as appears from the experiments of that learned philanthropiſt, Dr. Hales.

But of water the contrary is evident. In the ſteam-

engine,

engine, a jet of cold water, we find, inſtantly condenſes that immenſe rarefaction; which I apprehend could not be, if it was conſtituted of eſcaped elaſtic air. And altho' this ſteam muſt be acknowledged to put on ſome properties of air; ſuch as ventilating a fire; or that a taper blown out by it, is capable of being again lighted immediately, and that without a crackling noiſe, which occurs when touched with water; this does not in the leaſt invalidate our opinion, tho' it has certainly conduced very much to propagate the former one: ſince from this way of reaſoning, the whole muſt be air, and we ſhould have no water at all in vapour.

From conſidering this power of expanſion, which the conſtituent parts of ſome bodies acquire by heat; and withal, that ſome bodies have a greater affinity to heat, that is, acquire it ſooner and retain it longer than others; which affinity appears from experiments, and which, I apprehend, is in ſome ratio of their ſpecific gravities and their powers of refraction, reflexion, or abſorption of light; or at leaſt in ſome ratio much greater than that of their ſpecific gravities alone. From conſidering theſe, I ſay, many things, before utterly inexplicable, became eaſily underſtood by me. Such as, Why when biſmuth and zinc are fuſed together, and ſet to cool, the zinc, which is ſpecifically heavier, is found above the biſmuth? Why the buff covering of inflammatory blood, the ſkum of heated milk, the ſedative ſalt of borax, which are all ſpecifically heavier than the liquids in which they are formed, are ſtill formed at the ſurface of them? How benzoin, ſulphur, and even the ponderous body mercury, may be raiſed into vapour, again to be condenſed unaltered?

And, laſtly, how water, whoſe parts appear from the æolipile to be capable of immeaſurable expanſion, ſhould by heat alone become ſpecifically lighter than the common atmoſphere, without having recourſe to a ſhell incloſing air, or other aſſiſtant machinery? and when raiſed, I am perſuaded we ſhall find, that to ſupport them floating, perhaps many days, in the atmoſphere, is not a knot ſo intricate, as to oblige us to conjure up a new divinity to unravel it.

But before we proceed to this ſecond part of our taſk, it will be neceſſary previouſly to conſider, firſt, how ſmall a degree of heat is required to detach or raiſe the vapour of water from its parent-fluid. In the coldeſt day, I might ſay the coldeſt night, of winter, when the weather is not froſty or very damp, wet linen or paper will become dry in the courſe of a few hours. A greater degree of heat muſt indeed cauſe a quicker evaporation. But I am perſuaded, that was it not for the preſſure of the ſuperincumbent fluid, greatly leſs than that of boiling water would inſtantly diſperſe the whole ſo heated into vapour.

Secondly, That in the opinion of Sir Iſaac Newton, well illuſtrated by the late lamented Mr. Melvil, the ſun-beams appear only to communicate heat to bodies by which they are refracted, reflected, or obſtructed; whence, by their impulſe, a reaction or vibration is cauſed in the parts of ſuch impacted bodies.

This is ſupported by the experiment of approaching ſome light body, or blowing ſmoke near the focus of the largeſt glaſſes; and from obſerving, that theſe do not aſcend, it is evident the air is not ſo much as warmed by the paſſage of thoſe beams thro' it, yet would inſtantly calcine or vitrify every

opake

opake body in nature. And from this we may collect, that tranſparent bodies are only heated at their ſurfaces, and that perhaps in proportion to their quantity of refraction: which will further give and receive illuſtration from thoſe very curious experiments, of producing cold by the evaporation of liquors, publiſhed by the learned Dr. Cullen, in the late volume of Eſſays Phyſical and Literary, at Edinburgh. In theſe experiments a ſpirit-thermometer was immerſed in ſpirit of wine, and being ſuddenly retracted, was again expoſed to the air; and as the ſpirit of wine adhering to the glaſs evaporated, the ſpirit contained within the thermometer was obſerved to ſubſide. Now as the difference of the refraction of ſpirit of wine and glaſs is exceedingly minute, compared with the difference of refraction of ſpirit of wine and air; we may conſider, in the above experiment, the heat to be communicated to the thermometer only at its ſurface: but here the adherent fluid eſcapes as ſoon as heated; by which means the glaſs, and its contents, are deprived of that conſtant addition of heat, which other bodies perpetually enjoy either from the ſun-beams immediately, or from the emanations of other contiguous warmer bodies; and muſt thence, in a few minutes, become colder than before.

The ingenious Mr. Eeles, I dare ſay, has already foreſeen the uſe I am going to make of this principle; *viz.* "That the little ſpherules of vapour will thus, "by refracting the ſolar rays, acquire a conſtant "heat, tho' the ſurrounding atmoſphere remain "cold." And as from the minuteneſs of their diameters, if they are allowed to be globules, they muſt

do

do this to a very great degree, I apprehend none of thoſe objections will take place againſt us, with which Mr. Eeles has ſo ſenſibly confuted the former received theories on this ſubject.

If we are aſked, how clouds come to be ſupported in the abſence of the ſun? it muſt be remembered, that large maſſes of vapour muſt for a conſiderable time retain much of the heat they have acquired in the day; at the ſame time reflecting, how ſmall a quantity of heat was neceſſary to raiſe them; and that doubtleſs even a leſs will be ſufficient to ſupport them, as from the diminiſhed preſſure of the atmoſphere at a given height, a leſs power may be able to continue them in their preſent ſtate of rarefaction; and, laſtly, that clouds of particular ſhapes will be ſuſtained or elevated by the motion they acquire from winds.

I ſhould here have concluded this paper, perhaps already too long; but upon reviſing it, I find, where the affinity of ſome bodies with heat is mentioned, that the deductions made from thence are not ſufficiently explained to be intelligible. Firſt then, If the power of expanſion of any two bodies, by heat, be in a greater proportion than their ſpecific gravities, then will there be a certain degree of heat, in which their ſpecific gravities will be equal; and another, in which the gravity of that, which was lighter when cold, will exceed the gravity of that, which was heavier when cold. Hence zinc and biſmuth alter their ſpecific gravities in fuſion; ſome urine, and many ſolutions of ſolids, grow turbid as they cool; others alter their colours. Secondly, If (the power of expanſion by heat being equal) the power of retaining

taining heat be in a greater ratio than the ſpecific gravities; then, during the time of cooling after being ſufficiently heated, there will be an inſtant, when the heavier body will become the lighter, and ſwim upon the other. This ſeems the caſe in the buff covering of inflamed blood, the ſkum of heated milk, and the criſtallization of ſome ſalts: for if theſe effects were from the evaporation of the thinner parts at the ſurface, they ſhould happen during the greateſt evaporation, or when boiling; but, on the contrary, they are all done in the greateſt degree when the liquor has for ſome time began to cool. Laſtly, If the quickneſs of acquiring heat be in a greater proportion than their ſpecific gravities (the power of expanſion being equal), then, during the time of their acquiring heat, there will be an inſtant, when the body, that was heavier when cold, will now become the lighter. From one or more of which principles, I apprehend, the volatility or fixity of all minerals, and many other bodies, takes its origin.

It is no part of my deſign to account to you, gentlemen, in what manner ſuch an expanſion of the parts of bodies can be brought about by the action of fire. Tho' perhaps a rotatory motion only of each particle on its own center might be ſufficient to produce ſuch a rarefaction; and the more ſo, if ſuch parts were any other figures than ſpheres, as by the percuſſion of their angles they muſt reſult further from each other. Nor is the exiſtence of ſuch a rotatory motion without ſome probability, when we obſerve the verticillary motion given to charcoal-duſt thrown on nitre in fuſion, or the wonderful agitation of the parts of burning phoſphorus, or even of a common

common red letter-wafer touched by the flame of a candle. But as in this paper I have laboured (and I hope not without ſucceſs) to ſhew you, that ſome properties of ſolar heat are ſufficient to account for the elevation and ſupport of vapours; ſo in another letter I propoſe nearly to demonſtrate to you, that the electric æther is far from having any ſhare in the production of this important phænomenon.

From,

GENTLEMEN,

Litchfield,
Mar. 20. 1757.

Your very humble Servant,

Eraſmus Darwin.

LETTER II.

To the very honourable and learned the PRESIDENT *and* MEMBERS *of the* Royal Society.

Gentlemen,

Read May 5, 1757.

EVERY theoretical inquiry, whoſe baſis does not reſt upon experiments, is at once exploded in this well-thinking age; where truth, under your patronage, has at length broke thro' thoſe clouds, with which ſuperſtition, policy, or parade, had overwhelmed her. But experiments themſelves, gentlemen, are not exempted from fallacy. A ſtrong inventive faculty, a fine mechanic hand, a clear unbiaſſed judgment, are at once required for the contrivance, conduct, and application, of experiments; and even where theſe are joined (ſuch

(ſuch is the condition of humanity!) error too frequently intrudes herſelf, and ſpoils the work.

My very reſpectable antagoniſt, Mr. Eeles, to whoſe ear, I am convinced, the voice of truth is more agreeable than that of applauſe, will forgive me the following critique on his performance; as by that means, I am perſuaded, the probability of his notions will be intirely deſtroyed, and the foregoing theory receive additional ſupports.

For this purpoſe our firſt endeavour will be to ſhew the uncertainty of ſome of the moſt material principles, that ſupport his arguments; and afterwards, the fallacy of the experiments he has given us.

Firſt then, in page 130. Mr. Eeles has aſſerted, that the greateſt poſſible rarefaction of water is when it boils. I think it might be ſaid, with equal propriety, that the greateſt rarefaction of ſolids was when they began to melt: and this may indeed be verbally true, if we chuſe to alter the names of bodies, when they undergo any alteration by fire: ſo ſolids take the name of fluids, when they are in fuſion; and water the name of vapour, when it is greatly rarefied in the ſteam-engine. Whence we find this aſſertion ſeems to be founded on a confuſion in terms, and the fact far from being exiſtent in nature.

In page 133. the ſphere of electrical activity is ſaid to be increaſed by heat. If by electrical activity is here meant an increaſe of its repulſive power (the thing, which ſeems to be wanted in Mr. Eeles's hypotheſis), I know no experiment to ſhow it. If it be meant, that it is capable of being attracted to a

greater diſtance; I conjecture it may, as the heat will rarefy the ambient air, and we know the electric æther is attracted at very great diſtances in *vacuo*; but this cannot properly be called an increaſed activity of electric fire.

We are afterwards told (page *ib.*) " that electric " fire will not mix with air:" whence, in the ſucceeding ſection, it is argued, " That as each particle " of vapour, with its ſurrounding electric fluid, will " occupy a greater ſpace than the ſame weight of " air, they will aſcend." In anſwer to this, it muſt be obſerved, that there are ſome bodies, whoſe parts are fine enough to penetrate the pores of other bodies, without increaſing their bulk; or to paſs thro' them, without apparently moving or diſturbing them. A certain proportion of alcohol of wine mixed with water, and of copper and tin in fuſion, are inſtances of the firſt of theſe; the exiſtence and paſſage of light thro' air, and, I am perſuaded, of electric fire, are inſtances of the ſecond.

To illuſtrate this, the following experiment was inſtituted. A glaſs tube, open at one end, and with a bulb at the other, had its bulb, and half way from thence to the aperture of the tube, coated on the inſide with gilt paper. The tube was then inverted in a glaſs of oil of turpentine, which was placed on a cake of wax, and the tube kept in that perpendicular ſituation by a ſilk line from the cieling of the room. The bulb was then warmed, ſo that, when it became cold, the turpentine roſe about half-way up the tube. A bent wire then being introduced thro' the oil into the air above, high electricity was given. The oil did not appear at all to ſubſide:

whence I conclude, the electric atmoſphere flowing round the wire and coating of the tube above the oil, did not diſplace the air, but exiſted in its pores.

This experiment I formerly tried various ways, as I had conceived, if the electric matter would diſplace air, it might have been applied to anſwer the end of ſteam in the ſteam-engine, and many other great mechanical purpoſes. But as from the above it appears, that the contrary is true, it is evident, that electric matter ſurrounding particles of vapour muſt, in fact, increaſe their ſpecific gravity, and cannot any-ways be imagined to facilitate their aſcent.

I may add further, that if this be true, that it pervades the pores of air, its ſpecific levity cannot, by any means I know, be compared with that of air. Its particular attraction to ſome bodies, at leaſt to much the greater part of the terraqueous globe, is abundantly greater than that of air to thoſe bodies: and hence its gravitation to the whole globe would appear, at firſt view, to exceed that of air. But the more I conſider this, the more perplexing and amazing it appears to me: and thence muſt leave it to the inveſtigation of my very ingenious antagoniſt, or ſome other able philoſopher.

I come now to the experiments, that are given us to ſhow all vapour to be electriſed. In theſe Mr. Eeles ſeems to have been led into error, by not having obſerved, that many bodies electriſed will retain that electricity for ſome time, altho' in contact with conductors. The Leyden phial may be touched three or four times by a quick finger before the whole is diſcharged. Almoſt all light dry animal or vegetable ſubſtances, ſuch as feathers and cork, do this in a

much greater degree: and in general I have obſerved, the more ſlow any bodies are to acquire electricity, the more avaritious they are to keep it.

Part of the plume of a feather, hanging to a green line of ſilk about a foot long, which was ſuſpended from the midſt of an horizontal line of the ſame, about four yards in length, was electriſed with a dry wine-glaſs, according to the method of Mr. Eeles; and, after being touched nine times with my finger, at the intervals of two ſeconds of time, ſtill manifeſted ſigns of electricity, by being attracted at the tenth approach of it.

A cork ball, on the ſame line and circumſtances, after being electriſed, was touched at the intervals of ten ſeconds repeatedly, for ſeven times, before it was exhauſted. The fumes of boiling water were conveyed upon this ball after being electriſed; and, after a fumigation for thirty ſeconds, it ſhewed ſigns of electricity, by being attracted to the approaching finger; and, after thirty ſeconds more, without any fumigation, it again obeyed the finger; and again, after thirty more, but at leſs and leſs diſtances. The ſame appearances occurred to me from the fumes of reſin. From whence I apprehend, that Mr. Eeles, having dipped the electriſed down of the *juncus bombycinus* in vapour for perhaps half a minute (for no time is mentioned), and finding it ſtill retained its electric attraction, was not aware, that this ſame had happened, if he had by intervals touched it with his finger, or any other known conductor of electricity.

As Mr. Eeles had here objected, that there was no real oppoſition in the electric æther of glaſs, and that from wax; the common experiment to ſhew this

was

was many times repeated with conſtant ſucceſs; *viz.* the cork ball, ſuſpended as above, after being electriſed by the wine-glaſs, and repelled from it, was ſtrongly attracted by a rubbed ſtick of ſealing-wax; and *vice verſâ.* In the ſame manner I obſerved the electric æther from a black ſilk ſtocking (which was held horizontally extended by the top and foot, and, being rubbed in the midſt with an iron poker, was applied to the cork ball), to be ſimilar to that of glaſs, and oppoſite to that of wax. But the following experiment appears to me to put this matter out of all doubt, and to demonſtrate, that this difference is only a *plus* and *minus* of the ſame ſpecific æther, and not different qualities of it, as Mr. Eeles would ſuppoſe.

A ſtick of dry ſealing-wax was rubbed on the ſide of a dry wine-glaſs, and a cork ball, ſuſpended as in the former experiments, played for ſome time between them: but glaſs rubbed with glaſs, or wax with wax, did not manifeſt any electric appearance. Whence it would appear, that in rubbing glaſs and wax together, the glaſs accumulated on its ſurface the identical æther that the wax loſt. Nor is this a digreſſion from my deſign: for if this oppoſition of the electricity of glaſs and wax be eſtabliſhed, it ſtill contributes to demonſtrate the fallacy of Mr. Eeles's experiments.

But what alone would intirely deſtroy this electric hypotheſis, is, that from the experiments of Mr. Franklin and others, the clouds are ſometimes found to be electriſed *plus*, ſometimes *minus*, and ſometimes manifeſt no ſigns of electricity at all. Whence to ſay an accumulation of electric æther ſupports theſe clouds,

clouds, ſeems an aſſertion built upon a very unſtable foundation, whoſe whole ſuperſtructure may well enough be termed an air-built caſtle, the baſeleſs fabric of a viſion.

Add to this, that Mr. Eeles, in page 140. tells us, that himſelf has paſſed thro' clouds reſting on the ſides of mountains. Ought not thoſe clouds to have immediately diſcharged their electricity, and fallen? And common experience may remind us, that any cold bodies will condenſe vapour, whatever be their electric properties. So mirrors, or the glaſs of windows, in damp rooms, are moſt frequently found covered with dew; which, of all other bodies, ought moſt to be exempted from collecting vapours ſupported by electricity, as they are the leaſt capable to attract or draw off that æther.

From all which, well examined, I am perſuaded, gentlemen, you will be induced to conclude, that tho' clouds may ſometimes poſſeſs an accumulation of electricity, yet that this is only an accidental circumſtance, and not a conſtant one; and thence can have no poſſible influence either in the elevation or ſupport of them. I am,

GENTLEMEN,

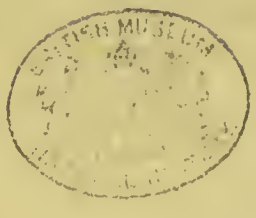
Your very humble Servant,

Litchfield,
March 23. 1757.

Eraſmus Darwin.

XXXI.

The grey Coot-footed Tringa *shot near Halifax in Yorkshire (January 1757) and Presented to me by Mr. Thomas Bolton Florist of Worley-clough in Yorkshire. Drawn from nature of the bignes of Life by Geo. Edwards in Feb.y 1757.*



XXXI. *An Account of a new-discovered Species of the Snipe or Tringa: In a Letter to the Rev.* Tho. Birch, *D. D. F. R. S. from Mr.* George Edwards, *Librarian of the College of Physicians.*

SIR,

Read May 5, 1757.

I TAKE the liberty to lay before you the figure and description of a new-discovered species of the snipe or tringa kind, which was lately shot at Sowerby-bridge in Yorkshire, and sent to me by Mr. Florist of Worley-clough, near Hallifax in the same county. If the account, that follows, shall be thought by you deserving to be communicated to the Royal Society, the real bird, which I have preserved dry, shall be produced at the same time.

This bird is like in shape to most others of the tringa or snipe kind. Its size is better shewn by the figure lying before you (*See* TAB. VI.), than by the dried bird, which is much shrunk since the drawing of it was made. I chuse, by way of distinction, to name it the coot-footed tringa, as it differs from other birds of that genus no otherwise, than in having its toes webbed in the same particular manner as the fulica, or our bald-coot. One of its feet is shewn in the plate, magnified a little, to make it the better understood, in what manner the webs or membranes spreading on both sides of the toes are scalloped or indented at each of the toe-joints. These scallopings are finely pectinated on their outer edges, as the enlarged figure expresses. The hinder toe is small, and finely

finely pectinated on the under ſide. The bill is black, and channelled on both ſides of the upper mandible; in which channels the noſtrils are placed near the forehead: it is compreſſed ſomewhat like a duck's bill, and ridged along its upper part, as a figure of the head in the corner of the plate may ſhew. The lower head, figured with it, is intended to ſhew the bill (which is very narrow) of another ſpecies of coot-footed tringa, brought from North America, and deſcribed and figured in my Natural Hiſtory of Birds, *&c.* plate 46. The eyes are placed farther backward from the bill than in many other ſorts of birds; in which the wiſdom of Providence is remarkable: for birds of this genus commonly feeding in ſoft muddy ground on the banks of rivers or the ſea, have occaſion to thruſt their bills deep into the ſhores, to extract worms and inſects; and their eyes would be in danger, were they placed more forward. The fore part of the head, the neck, breaſt, belly, thighs, covert-feathers withinſide the wings and under the tail, are white: the top and hinder part of the head is black. The lower part of the neck behind, and the back, are of a blueiſh aſh or ſlate-colour, with a mixture of blackiſh or duſky: the upper ſides of the wings and tail are of a blackiſh or duſky colour: the tips of the covert of the wings are white; the tips of the middlemoſt or ſhorteſt of the quills are alſo white, and form white tranſverſe bars acroſs the wings. Two or three of the middle quills are wholly white, and all of them have their inner webs white toward their bottoms. It hath twelve feathers in the tail; the outermoſt of which, on each ſide, is edged with white. The covert-feathers on the rump, or upper ſide of the tail, are

duſky

duſky and white. The legs are bare of feathers above the knees (as they are in moſt birds, who wade in ſhallow waters), and of an aſh-colour.

I believe no diſcovery of this bird has been made till now: and it is very probable there are many more ſpecies of birds in this iſland, that have hitherto eſcaped the notice of curious inquirers. Mr. Ray, in a book by him publiſhed, London, 1674. called, A Collection of Engliſh Words, *&c.* with a Catalogue of Engliſh Birds and Fiſhes, *&c.* after naming the coot in his catalogue of birds, p. 92. ſays, "Mr. Johnſon of Brigna, near Grota-bridge in York-"ſhire, ſhewed me a bird of the coot kind, ſcallop-"toed, not much bigger than a black-bird." As ſo little is ſaid by Mr. Ray, one can hardly determine any thing concerning the bird he mentions: and 'tis plain he thought this note ſcarce worth notice, as he hath not preſerved it in the Ornithology ſince by him publiſhed. Nor can I believe it was the bird now before us; for he ſays it was not much bigger than a black-bird; which implies, that it was ſomething bigger. And, on reading his deſcription of the black-bird, I find he makes it to weigh four ounces; conſequently it is four times the weight of the bird above deſcribed by me: for my obliging friend, Mr. Floriſt, who ſent me this bird, ſays in his letter, that, when newly killed, it weighed one ounce. Therefore I am inclined to think, that the bird Mr. Ray has ſo ſlightly mentioned, is a bird not as yet fully diſcovered. I am, Reverend Sir,

Your moſt humble Servant,

College of Phyſicians, Lond.
May the 3d, 1757.

Geo. Edwards.

XXXII. *Observationes de Corallinis, iisque insidentibus Polypis, aliisque Animalculis Marinis: Quas Regiæ Societati Londinensi offert* Job Baster, *Med. Doct. Acad. Cæsar: Reg. Societ. Lond. & Scient. Holland. Socius.*

Read May 19. 1757.

DOMICILIUM meum mari propinquum † occasionem præbet, in nondum satis cognitam quorundam animalium, in mare degentium, generationem et œconomiam inquirendi. Quæ observavi Regiæ Societati temporis successu offerre animus est; sed in hac prima dissertatione tantum observationes quasdam de corallinis, iisque insidentibus polypis, et aliis animalculis marinis, exhibere in animum induxi.

Paucis abhinc annis, inter doctos viros dissensio fuit, utrum corallia, corallinæ, et kerato-phyta veræ essent plantæ, quæ crescunt, et vegetant, in quibus insecta marina nidificant; an vero horum ipsorum essent opus et fabrica. Multi et sagacissimi historiæ naturalis scrutatores ultimam complexi sunt sententiam; sed nemini contradicere studens, simpliciter tantum et fideliter illa referam, quæ variis temporibus in corallinis observavi, et quæ lectorem benevolum, ut spero, convincent, corallinas non magis a polypis fabrefieri, quam diversa fungorum genera ab illis fabri-

† Zirizææ, quæ, in insula Scaldiæ, secundum Zelandiæ oppidum est.

cantur

cantur animalculis, quibus, æſtivo tempore, quaſi repleta inveniuntur.

Dura corallia, quæ recenter ex mari extracta, et in rotundum animalculis obſita reperiebantur, primam anſam dedere ſuſpicandi, hæc ab illis eſſe fabrefacta.

Cum omnia juniora conchylia tenerrimæ et viſcoſæ ſubſtantiæ reperiantur, ex analogia concluſum fuit, parva et tenera animalcula, quæ coralliis inſident, æque hæc fabricare potuiſſe, ac illa duriſſimas ſuas conchas et buccina. Sed in hiſtoria naturali non tuto ex analogia licet concludere.

Verum quidem eſt, quod recens natum conchylium tam tenerum eſt, quam parvus ille corallio inſidens polypus; ſed tunc concha ejus vel buccinum etiam erit tenerrimum, et quo magis in eo contentum creſcit animal, eo major, durior et firmior fit concha: et interior conchæ ſuperficies ſemper eſt læviſſima et glaberrima, ne tenerrimum animalculi corpus aliquo modo lædi poſſet, ut in oſtreis, mytulis, ſolenibus, et quibuſcunque conchis et buccinis, hoc videre eſt.

Sed an hoc in coralliis invenitur? Nunquam polypi in ipſa coralliorum ſubſtantiâ habitant, ſed ſemper intra hanc et circumdatam corticem. Cavitates, quas in coralliis invenies, non glabræ aut læves ſunt, ſed aſperæ et acutæ. Parvus corallii ramulus nec tener eſt nec mollis, ſed æque durus habita magnitudinis proportione ac maximus: nec minores illi inſident polypi quam ramis majoribus.

Cum vero hic, in Zelandia, necdum ulla dura corallia recenter ex mare extracta explorare licuit, me tantum ad corallinas determinare debui, ubique fere

prope littora reperiundas, et quibus ſimillimi, qui coralliis, inſident polypi.

Animadvertam tantum mihi videri, animalcula, quæ ſummas coralliorum extremitates inhabitant, et Nobiliſſimo Marſiglio flores viſa ſunt, ad genus etiam polyporum referri debere, et in his extremitatibus non nata eſſe, ſed irrepſiſſe, dum illas vacuas et domicilio aptas invenerint: eodem certe modo, quo cancelli, quos *Bernard l'hermite* vocant Galli, vacua irreptant buccina. Et hi cancelli, ut obſervant piſcatores, non caſu aut temerarie id faciunt, ſed quaſi ex conſulto. Si ſex vel ſeptem cancelli vivi prudenter ex ſuis buccinis extrahantur, et hæc inter ſe permixta iis iterum exhibeantur, quiſque cancellus in proprium ſuum, nunquam in alterius buccinum irrepit, et hoc ablatum undique quæritat, quod jucundum viſu eſt.

Neceſſe mihi fuit quaſdam ſed paucas corallinarum et polyporum figuras addere: ſi vero quis plura deſideret, elaboratum opus ſagaciſſimi Domini Ellis adeat, in quo quamplurimas et accuratiſſimas corallinarum et polyporum delineationes inveniet †.

De Plantis Marinis generatim.

Plantæ marinæ a terreſtribus in plurimis differunt, nam hæ in raro aëre excreſcentes, nutrimentum ſuum radicum ope, in terra proſerpentium, hauriunt: du m illæ plerumque nec radices nec folia habent,

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† Dominus Ellis, anno 1755. Lond. in 8° edidit *Eſſay on the Natural Hiſtory of Corals*, &c. quem librum Gallice verſum in 4° recudit P. de Hond Hagæ Comitum, ſub titulo *Eſſay ſur l'Hiſtoire Naturelle des Corallines, &c. par J. Ellis*, 1756.

ſed

ſed ex trunco et ramis conſiſtunt. Illa plantæ marinæ pars, qua ſubſtantiæ, cui increſcit, adhæret, radicis nomen non meretur, niſi quatenus plantam uni ſemper loco tenet adfixam: ſed quod primarium radicis munus eſt, nutrimentum ei non adfert: ipſa planta per truncum et ramos ex medio, cui ſemper immerſa eſt, incrementum ſuum acquirit. Neque maris fundus radicibus recipiendis aptus eſſet, nam plerumque inconſtans et volubile ſabulum eſt, quod continuo fluctuum motu de loco in locum dimovetur, ita ut uno temporis momento radices nudæ, altero ſub arena forent ſepultæ.

Sed quamdiu vera vegetatio marinarum erit ignota, non bene explicari poterit, quare corallia et keratophyta, licet ab imo ad ſummum undique animalculorum cellulis obſeſſa, læte tamen creſcant, ut hoc in plurimis, non tamen in omnibus, obſervare eſt. Nam Nobiliſſimus Marſigli kerato-phyta invenit, quæ nulla cortice, aut quæ uno loco cortice erant obducta, altero non: et corticem hanc non niſi polyporum cellulas fuiſſe clare ex ejus verbis patet *.

At rogare mihi liceat, an vera vegetatio plantarum terreſtrium, quæ ſemper nobis ante oculos ſunt, bene cognita et perſpecta ſit? an quidem novimus, quæ vera ſit radicum functio, et quomodo hanc exerceant? Nonne plurimæ inveniuntur plantæ, quæ pauciſſimis inſtructæ radicibus in altum creſcunt, maxime ergo foliorum ope, quæ ſuccos nutritios ex vaporibus in

* Il y a une ſorte de Lithophyte, qui veritablement eſt curieuſe, et bien extraordinaire: elle n'a point d'ecorce continuée, mais bien quelques fragmens, par ci par la interrompus d'un glu, qui fleurit dans l'eau. Hiſt. de la Mer. pag. 89. fig. 101. 179, 1.

aëre

aëre natantibus hauriunt, plantam alunt, et ſic radicum defectum ſupplent. Sed liceat mihi, accuratiſſimi Bonneti verba adferre; " Plantæ, dicit hic ſagaciſſi-
" mus naturæ ſcrutator, ſemper ſunt ſugentes, et in
" ſtatu ſuctionis, interdiu radicum ope nutriuntur,
" noctu foliorum *. Sed optandum eſt †, quod arte
" quadam exacte poſſet determinari, et tunc inter ſe
" comparari hanc nutrimenti copiam, quam plantæ
" radicum ope acquirunt, cum illâ, quæ folia adfe-
" runt. Examen hoc forſan nos doceret, quod *Aër*
" non minus quam *Terra* ad plantarum nutritionem
" et incrementum contribuat."

Si ergo medium tam rarum et tenue, ut aër (ut ex Celⁱ. Halæi et Boneti experimentis certum eſt) tantum ad plantarum nutritionem adferat, mirum non eſt, quod nunquam quieſcens et quam maxime heterogenea aqua maris plantas marinas, licet expanſis radicibus deſtitutas, ad tantam magnitudinem, altitudinem et duritiem faciat excreſcere, Sed nonne ſimiliter creſcunt plurima fungorum genera? Quas radices habent quercubus aliiſque lignis increſcentes agarici? Quas phallus, elvela, et plura, quæ in *Methodo ſuo fungorum* deſcribit *J. G. Gledifch.*

Hæc de plantis marinis præmittere volui, ut evincam corallinas, licet radicibus careant, creſcere, vegetare, et plantas eſſe poſſe, ut aliæ terreſtres et fungi, quæ ſimiliter aut minimas habent radices, aut plane iis deſtituuntur,

Cætera, quæ de vegetatione, floribus et ſeminibus plantarum marinarum obſervavi, alio tempore indicabo.

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* Bonnet ſur l'uſage des Feuilles, pag. xviii. & 286.

† Id. ibid. pag. 66.

De

De Corallinis.

Corallinæ omnes habent proprietates, quas in genere de plantis marinis indicavimus: Sed præterea, quod præcipuum eſt hujus diſſertationis propoſitum, omni fere corallinæ, ſi rami ejus ad juſtam magnitudinem et firmitatem creverint, tam hyeme quam ætate, animalcula inſident, quæ a multitudine brachiorum, et ſimilitudine, quam cum polypis, in aqua foſſarum dulci, reperiundis, habent, etiam polypi vocantur.

Si quis corallinæ plantam, eique inſidentes polypos, rite examinare ſtudet, non incipiat parvam tenuis ramuli partem in vitro concavo jacentem microſcopio inſpicere; ſed totam corallinæ plantam recenter ex mare extractam in vitrum pellucidum, aqua marina repletum, prudenter inſerat: aut talem plantam orbi porcellano concavo in fundo coloris profunde cærulei imponat, et addita ſufficiente aquæ marinæ pellucidæ quantitate, ejus ramos prudenter expandat; tunc poſt quadrantis horæ quietem, illos lente amplificante intueatur, et ſic diſtinctiſſime in hac unica planta polypos diverſi generis, et plerumque mira alia videbit inſecta; quæ microſcopio dein ad libitum ulterius examinari poſſunt.

Corallinæ, quæ [a] capillares et filamentoſos habent ramulos, aut quæ [b] juniores et tenuiores adhuc ſunt, ut

Quæ navibus poſt longum iter;

Aut quæ doliis iſtis coniformibus, quæ ad littora vel fluminum majorum exitum in mare ad nautarum ſecuritatem ponuntur;

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[a] Tab. VII. fig. I, II. [b] Ibid. fig. III.

Aut:

Aut januis emiſſariorum aquæ marinæ in his regionibus,

accreverint, raro vel nunquam habent polypos.

Sed ſi eadem corallinæ ſpecies jam ad ſufficientem magnitudinem et firmitatem, et præſertim ſupra oſtrea, ſaxa, ſilices, aliaque in fundo maris quiete jacentia corpora, increverit, polypis ſcatet. Perſpicaci ſuo judicio decidat lector B.

An hoc efficitur, quia polyporum ſemen, ova, vel nata progenies gravitate ſua fundum petat?

Vel quia animalculis his perpetuus navium aut doliorum motus obſtet, ut hæc non ſatis tuta credant, et ideo in fundo maris ſemper immota eligant corpora, quibus ovula ſua confidant?

Vel quia pix et colophonia, quibus naves, dolia, et emiſſariorum januæ illiminuntur, corallinis, quæ illis increſcunt, noxiam vel polypis ingratam qualitatem communicent? Hoc ſaltem ſemper obvenit, me numquam tot polypos inveniſſe in corallinis, quas a navibus, doliis aut emiſſariorum januis abraſeram, quam in illis, quæ oſtreis, mytulis et ſilicibus in fundo maris erant innatæ.

Hoc *primum* mihi *argumentum* eſt, corallinas a polypis non formatas eſſe; nam tunc plantæ juniores et minores æque ſuos haberent polypos, ac maximæ.

Secundum argumentum, quod polypi vel caſu vel inſtinctu quodam ſeſe corallinis affigant, ſed veram plantæ partem non conſtituant, eſt, quod polypi non omnes majoris plantæ ramos æqualiter obſident: hic ramus vel hujus rami tantum pars polypis obſeſſa erit quam plurimis, altera nullis. Corallinam habeo, [c]

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[c] Tab. VIII. fig. V.

cujus

cujus truncum plurimi inhabitant polypi, dum nullos in ramulis poteram detegere, licet armato oculo. Et ſic algæ * vel quercui ſic dictæ marinæ ſæpius corallinæ increſcunt, in quibus nunquam polypos inveni. Hoc fieri non deberet, ſi corallinæ polyporum eſſent opus. Omnes corallinæ ſemper ſuos deberent habere, et ramis ſuis æqualiter et proportionaliter inſidentes polypos; et nunquam ſine his eſſent reperiundæ, ut tamen ſæpius fit. Cel. Juſſieu † quaſi mirabundus dicit, ſe ſemel alcyonium et ſpongiam ramoſam ſine polypis inveniſſe, licet recenter a rupe eſſent abſtractæ.

Tertium argumentum erit, quod fere ſemper una eademque [d] corallinæ planta diverſi generis alat polypis: in una eademque [e] corallinæ tubulariæ planta quinque diverſas polyporum ſpecies inveni ‡.

Liceat jam mihi rogare, quibuſnam horum quinque hæc corallina ortum ſuum debeat? Certe non primæ aut ſecundæ, ut videtur, magnitudinis, nam hi ſummis tantum inſident corallinæ extremitatibus, et microſcopium clare oſtendit locum, ubi corpus polypi minus pellucidum et ſuperficiei rudioris glabræ huic corallinæ adhæret. Nec fabricavit corallinam tertia ſpecies, quæ extrema corporis parte, quaſi caudâ, corallinæ eſt affixa: et minus adhuc quarta, nam clare videre eſt, horum cellulas corallinæ cir-

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[d] Tab. VIII. fig. II, IV. [e] Tab. VIII. fig. IV.

* Vide talem delineatam in Mem. de l'Acad. p. 394. anni 1711.

† Mem. de l'Academie, 1742.

‡ Sic D^s. *Ellis* in una eademque planta diverſos polypos delineat, tab. IV. fig. C. tab. V. fig. A. tab. XIV. fig. A. B. tab. XXXVIII. F. N. E.

cumdatas, ut examen apum arboris ramum circumcludit: et ſi hæ polyporum cellulæ non nimis denſæ ſunt, ipſius corallinæ color tranſlucet.

Si hæ quatuor polyporum ſpecies hanc corallinam non fabricaverint, non fecit certe quinta. [f] Mirabiliſſima et minima hæc animalcula ad genus polyporum certe pertinent, et omnibus fere corallinis, algis, aliiſque plantis marinis incredibili ſæpe inſident copia. Jucundiſſimum eſt videre, quomodo ſe expandunt, et mox mira agilitate, capta prædâ, ſe ſubito contrahunt, quod bis vel ter in minuto horæ repetunt.

Æque jam, ut in una eademque corallinæ planta diverſos invenis polypos, ſic in diverſis corallinæ ſpeciebus videbis ſimiles et eoſdem polypos: * quod etiam illi obſtat ſententiæ corallinas polyporum eſſe opus aut fabricam. Polypi dum operantur, ut cætera animalia, inſtinctu innato operantur: ergo, una eademque polyporum ſpecies ſemper eaſdem fabricaret corallinas: ſic ſemper ſimiles et uniformes apes faciunt favos, veſpæ nidos, araneæ tela. Hic vero contrarium eveniret; iidem polypi uno tempore hanc, alio tempore illam fabricarent corallinam: quod rationi contrarium eſt, et mihi *quartum argumentum*. Sed hoc verum eſt, quod eadem polyporum ſpecies non diverſas corallinas, ſed in diverſis corallinis eaſdem et uniformes ſemper ſibi conſtruant cellulas.

Quinto, ſi corallinæ a polypis eſſent fabrefactæ, nunquam polypi et eorum cellulæ etiam vivis animalibus, aliiſque corporibus, eſſent adfixæ. Polypos inveni in

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[f] Tab. IX. fig. A, B, C.

* Sic Clar. *Ellis* eoſdem etiam polypos invenit in corallina aſtaci cornicularum æmula N°. 14. et in corallina ſetacea inſtar arundinis geniculata N°. 16.

oſtreorum

oſtreorum g conchis, cancri h arachnoideæ pedibus, animali i, quod emiſſariorum januis et navibus quietis ſæpius adhæret et anus *(aars-gat)* vocatur, et aliis plurimis, ſine minimo corallinæ veſtigio. Et animalia illa, quæ piſcatores noſtri k *klap-konten* vocant, et majorum polyporum ſpecies eſſe videntur, nunquam teneris inſident corallinis, ſed ſemper hæc ſupra oſtreorum conchas et lapides inveni, et quamvis illorum progreſſum ipſe non viderim, tamen de loco in locum ſeſe tranſmoviſſe, expertus ſum.

Sexto: Hæ corallinæ non ſolum polypis, ſed cochleis, buccinis l aliiſque plurimis inſectis marinis conveniunt, ut illis ova vel progeniem confidant. Hoc præcipue menſibus Februario et Martio videre eſt: accepi tunc diverſas corallinas diverſis cochleis et buccinis plenas, et ſæpe inter hæc quoſdam cancellos ova ſua jamjam excludentes. Mater cochlea ovula ſua ſupra vel juxta corallinas depoſuit; pulli excluſi contra has aſcenderunt, ne illis innixi fluctuum motu eluerentur, vel ut adverſus hoſtes ſuos tuto ſe abſconderent.

Quantum omnibus animalibus divina proſpexerit prudentia, animus nunquam ſatis aſſequi quacunque induſtria poteſt. *Deus*, ut jam animadvertit Rex † Pſalmiſta, *plantavit arbores cedros Libani, ubi aviculæ nidificent, et abietes domicilia ciconiæ: Montes excelſiſſimos rupicaprarum, petras murium montanorum perfugium.* Sic corallinæ domicilia et perfu-

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g Tab. VII. fig. VI. h Ibid. fig. VII. i Ibid. fig. VIII.
k Tab. IX. fig. IV, V, VI. l Tab. VIII. fig. VI.

† Pſalm. civ. verſ. 16, 17, 18.

gium ſunt polypis aliiſque minoribus inſectis marinis. Et præſertim hoc cenſeo, quia tempore hyemali, menſibus Decembri et Januario, corallinarum ramos plurimis veſiculis, operculo vel valvula tectis, obſeſſos inveni: quare has veſiculas habeo pro ovis ab aliis inſectis his corallinis impoſitis.

Omnes, qui nunquam ſatis laudanda incomparabilis REAUMURII ſcripta legerunt, ſciunt, quam miris et differentibus modis inſecta quædam ſua ova opponunt, vel in * gyrum, vel in † lineam ſpiralem, vel ‡ ſingula ova a filis libere in aere pendentia, vel, ut hic fere caſus eſt, per || paria lignoſæ ſubſtantiæ ramorum impoſita. Quam mira quam varia eſt etiam ipſorum :‡: ovorum figura! Culicum ⸸ autem ova, quoad externam figuram, quam maxime his corallinæ veſiculis ſimilia ſunt.

Omnia vero inſectorum ova inter ſe et cum his veſiculis in eo conveniunt, quod :||: operculum vel valvulum habent eo in loco, ubi eruca vel vermis exitum invenire debet; permittente quidem hac valvulâ exitum extrorſum, omnem vero introitum aëris vel aquæ prorſus negante. Sed præter hanc cum aliorum inſectorum ovis convenientiam, ipſæ veſiculæ mihi vera ova videntur, quia recenter ex mare extractæ erant perſpicuæ, in ſpiritu frumenti vero ſtatim fie-

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* Memoir. des Inſect. tom. ii. tab. iv. fig. 6, 8.

† Id. ibid. tab. iii. fig. xv.

‡ Id. tom. iii. mem. xi. tab. xxxii. fig. 1.

|| Id. tom. v. mem. iii. tab. xv. fig. 1, 2, 3.

:‡: Id. tom. ii. mem. ii. tab. iii. tom. iv. tab. xxxvii. fig. 11, 12, 19, 20.

⸸ Id. tom. iv. tab. xliv. fig. 8.

:||: Id. tom. ii. mem. ii. pag. 163. tab. iv. fig. 11, 12, 13. Ellis Eſſay. pag. 100. tab. xxxiii. *a* A.

bant

bant opacæ, albæ, cum puncto flavo [m]. Secundo, quia hyeme plures veſiculæ in corallinis reperiuntur, perpaucæ vero æſtate; nam tum illorum pulli excluſi ſunt, et corallinæ cochleis parvis, forſan ex his ovis progenitis, magis ſunt obſeſſæ.

Nam hæ veſiculæ mihi ipſorum polyporum ova eſſe non videntur, licet ſæpius * polypos in illis invenire contigit: et ideo crederem hos polypos in veſiculis repertos, quoniam licet adulti tamen veſiculis multo minores ſint, poſt veri pulli excluſionem in vacua illa ova irrepſiſſe; cum jam antea obſervavimus cancellos in vacua irrepere buccina, et ipſos polypos vacuis corallinæ tubulariæ ſummitatibus ſeſe adfigere. Sed quam maxime has veſiculas non polyporum, ſed aliorum animalculorum ova eſſe cenſeo, quia perſpicaciſſimus *Reaumurius* initio hyemis in oris Galliæ provinciæ *Pictavienſis (Poitou)* ova cochlearum marinarum invenerit, quæ quamvis his veſiculis paulum majora, erant tamen ſimillima †. Licet corallinæ ſpecioſam habeant veriſimilitudinem, ut quaſi animalium opus appareant, aliæ tamen ſunt plantæ marinæ, præſertim *alcyonium digitatem molle*, quas adhuc magis a polypis fabrefactas diceres; ſed ſi accuratione indagine obſervantur, pro veris plantis erunt agnoſcendæ, ut in poſterum, quando profuſius agam de plantis marinis, demonſtrare ſpero. Alia vero alcyonia, ut *veſicaria marina Bauhini*, &c. vera animalium opera, namque eorum ſunt ovaria. Plantæ marinæ unicæ non ſunt, quæ pro inſectorum opere

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[m] Tab. VIII. fig. VI. *a* A.

* Ellis paſſim in figuris, præſertim tab. v. fig. A.

† Mem. de l'Acad. viii. pag. 253. tab. vi. fig. 1, 2, 3, 4.

ſunt

ſunt habitæ.. Liceat mihi locum adferre ex notis, quas Nobiliſſimus *Lyonnet Theologiæ inſectorum Doctiſſ. et Rev. Leſſers* ſubjunxit: " Credendum non eſt, ut " quidam faciunt, ſi ſtagnantes aquas viridi et fibroſâ " membrana tectas videas, hanc inſectorum textu- " ram eſſe. Eſt algæ ſpecies, quæ in his ſtagnis " creſcit, et inſectis eſt pro alimento." *

De Polypis Corallinis inſidentibus.

Completam hiſtoriæ et oeconomiæ polyporum in corallinis repertorum deſcriptionem dare nondum ſuſcipio, ſed paucis enarrabo, quæ de illis obſervare mihi licuit.

Auctores, qui aquæ dulcis polypos examinarunt, quatuor eorum ſpecies enumerant: ſed in aqua marina major differentium ſpecierum numerus eſt; plus quam viginti diverſos vidi polypos, quos ut plurimum in laudatis figuris etiam delineavit accuratiſſimus *Ellis*, et profecto adhuc plures ſunt. Sed hic monendum, quod cum aquæ dulcis polypi nudo oculo facile queant conſpici, plerique marini non niſi lente aut microſcopio poſſint examinari: et aqua marina, in qua polypi vivunt, bis, vel ad minimum ſemel, nucthemeri ſpatio, debet renovari, aut polypi moriuntur.

Ad exemplum Di. *Schaffer* in duas ſpecies primo polypos diſtinguam; in polypos, qui cellulas ſibi conſtruant, et in polypos, qui corporis extremitate corallinis aliiſque corporibus extus ſeſe affigant, vel in cavitates naturales alcyoniorum, coralliorum, &c.

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* Leſſer, Theologie des Inſect. tom. ii. p. 112.

(de

(de quibus in posterum) irrepant, ut cancelli in vacua buccina. Polypi, qui in cellulis habitant, cæteris plerumque sunt minores: horum cellulæ corallinæ [n] truncum vel ramos circumdant, a quibus prudenti manu sine corallinæ læsione satis facile possunt abradi: et si hæ cellulæ non nimia sint copia, et corallina contra solis splendorem inspiciatur, præsertim si color vivide ruber, aureus aut subniger est, facile per ipsas cellulas transluceat.

Polyporum, qui sine cellulis in corporum quorundam cavitatibus habitant, aut extrinsecus illis affixi sunt, plurimæ sunt species. Apertis [o] corallinæ tubulariæ ramis (nam in tali planta semper adsunt rami vel tubi naturaliter [p] clausi, id est, integri, in quibus nunquam polypus insidet) majorum polyporum species sæpius supra sedet; quos a colore rubente polypos vocabo *coccineos*, et præ cæteris observavi. In detritis et apertis, ut modo dixi, corallinæ tubulariæ ramulis, inserta sua cauda vel corporis parte posteriore, polypus habitat, et ex hoc ramo facile et sæpe levi quassatione delabitur, præsertim si ille jam per aliquot dies servatus debilis, æger, aut mortuus sit †.

Si vegetum talem polypum, corallinæ insidentem, vehementius amplificante microscopio intueris, facile differentiam vides inter scabram et quasi granulatam polypi cutem, et læviſſimam corallinæ corticem.

Hic polypus coccineus duplici brachiorum genere est munitus, quod in aliis minoribus non vidi; et hæc

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[n] Tab. VIII. fig. II. IV. [o] Tab. VIII. fig. III. [p] Tab. VII. fig. III.

† Idem observat Cel. Jussiæus, licet corallinas a polypis tamen fabrifatas autumat. Vide Mem. de l'Acad. 1742. et figuram inspice, quomodo corporis extremitate corallinæ insident.

coloris

coloris ſunt ſubalbidi. [q] Inferiora et longiora octodecim vel viginti ſunt, nam numerus ſæpe variat, et expanſa patinam formant, in cujus medio [r] ipſum polypi corpus coccineum eſt. Hoc corpus in duas partes dividi poteſt. Inferiori placentam referenti perpendiculariter alia pyriformis eſt infixa, quæ duodecim [s] vel quatuordecim habet brachia, prioribus tenuiora et breviora.

Hanc partem pyriformem polypus valde [t] extendere poteſt, præſertim ſi prædam captans expanſa claudit [u] brachia; et rurſus ita contrahere, ut quaſi [x] globulus inferiori et depreſſæ parti adhæreat. In extremo hujus partis [y] os polypi eſſe videtur: ſed ob partium exilitatem non omnia ſatis diſtincta poſſunt videri, ut in majoribus aquæ dulcis polypis; ſed ex ſimilitudine partium hæc tuto licet concludere.

Si longiora brachia fortiori microſcopio attente contemplaris, cutem eorum valde aſperam vides, ut eſt piſcium (quos *Haijen* vocant) vel ut ſuperficies corii granulati *(chagrein-leer)* forſitan ut minora animalcula, quæ polyporum eſca ſunt, eo melius, ne elabantur, retinere poſſint. Sed inter hujus ſpeciei polypos vidi unum cæteris [z] majorem, ex cujus corpore, illo loco ubi pars ſuperior inferiori et depreſſæ inhæret, ſex vel octo enaſcebantur [a] ramuli, in quorum extremitatibus erant duo vel tres parvi globuli, punctum coccineum in medio habentes. Huic polypo bis in die novam dedi aquam, quam maxime de ejus vita ſollicitus, ea ſpe, ut hi globuli in juniores

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[q] Tab. IX. fig. II. *a* A. [r] Tab. IX. fig. II. A. *c*. [s] Ibid. *b. b*. [t] Ibid. A. *c*. [u] Ibid. B. [x] Ibid. C. *c*. [y] Ibid. fig. III. [z] Ibid. fig. III. [a] Ibid. *c. c*.

excreſcerent

excrescerent polypos, sed quamvis per mensem sic vivum servaverim, nihil mutatum vidi, nisi quod globuli paululum evaserint majores.

Quæ deinceps de polyporum generatione observavi, non ita mihi satisfaciunt, ut illa pro certis et comprobatis habeam: si vero hac æstate meliora adiscam, Regiæ Societati indicabo.

De aliis Insectis Marinis.

Si noctu aqua maris, quæ littora nostra alluit, lapide injecto vel baculo movetur, innumeras videre est scintillas igneas, quæ nihil aliud sunt, quam minima animalcula lucentia, non nisi fortiore microscopio visibilia.

Ut hæc animalcula idoneâ colligas copiâ, sufficientem aquæ quantitatem, in qua has scintillas observasti, per chartam emporeticam filtrare facias, donec semiuncia aut minus aquæ supra chartam restat: hujus guttula, vitro concavo, penicilli vel pennæ ope, imposita, fortiore microscopio examinetur, et celerrimo motu illa natare videbis. Tres diversas horum observavi species, quas ad vivum delineatas exhibet tabulæ X. fig. I.

Sed mare plura alit insecta, quibus hæc lucendi facultas inest, et quorum in corallinis repertorum quædam delineata sunt tab. X. fig. 1, 2, 4, 5. sed quoniam plures † auctores de his scripserunt, non commemorabo.

Si corallinæ recenter ex mare extractæ major planta, orbi porcellano concavo, et cujus fundus est

† *Vionelli* nuove luci coperte. *Linnæi* Amæn. Acad. tom. iii. de noctiluca marina.

profundi coloris cærulei, cum ſufficiente quantitate aquæ marinæ pellucidiſſimæ et filtratæ, ut ante jam monui, imponatur, et ejus ramuli prudenter penna expandentur, et lente oculari inſpiciantur, ſilvam ſæpius videre eſt, in qua plurima paſcuntur animalia, præter diverſi generis polypos ramis infixos et brachia extendentes: alia plura ſunt, præſertim in infima parte, ſi corallinæ ſupra oſtrea creverint, quæ huc et illuc curſitant, et forſitan ſæpius hianti oſtreo alimento inſerviunt. Sic vigeſimo tertio Octobris 1756 oſtreum accepi, cui magna [b] corallinæ planta erat innata, in qua, præter tres diverſas polyporum ſpecies, ſex differentia reperiebam inſecta. [c] Primum vermis erat, cujus caput ſex majoribus et duobus minoribus cornubus erat inſtructum. [d] Alterum valde parvum araneam longipedem (Gallis *le Faucheur*) referebat, admodum lente ſe movens. Tertium vermis erat, ſimilis figuræ 3, ſed in deſignando deperdebatur. [e] Quartum, quintum et ſextum non niſi fortiore microſcopio diſtincte erant viſibilia. Horum quod littera c notatur, mirandæ erat ſtructuræ.

Diverſa ſic oſtrea et corallinas explorans, plura talia admiranda inſecta vidi, quorum delineationem ſiſtunt tabulæ X. figuræ 2, 3, 4, 5, 7. Decimo ſexto ejuſdem menſis Octobris, plures mihi ſed valde parvæ apportabantur corallinæ, quæ a dolio coniformi erant abraſæ: in his licet ſæpe et attente exploratis nullos detegere poteram polypos, ſed duo alia mirabiliſſima inſecta.

Horum, quæ ſecunda figura tabulæ decimæ littera A

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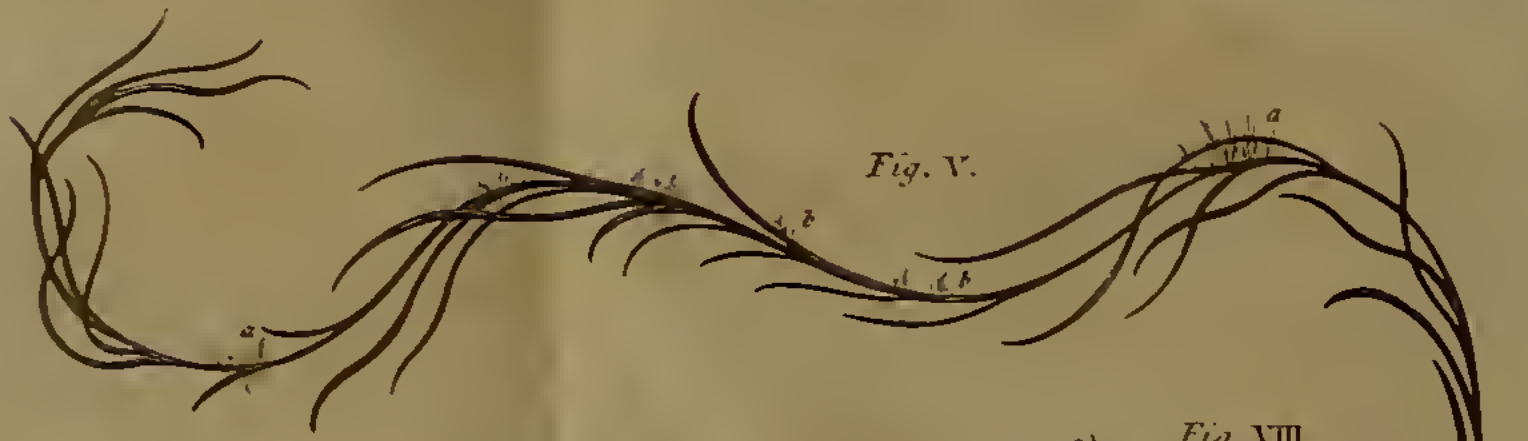
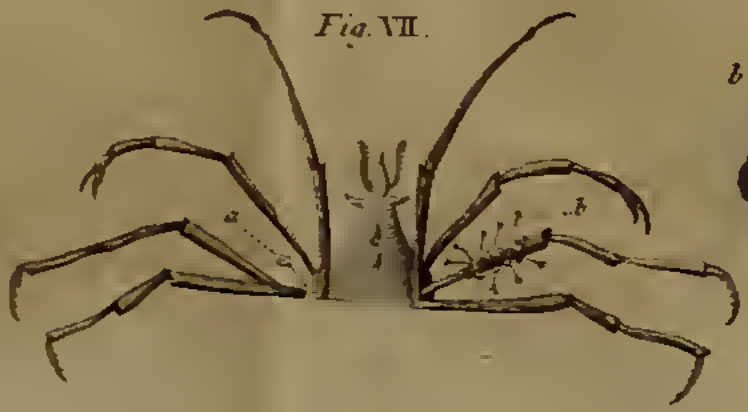
[b] Tab. VIII. fig. VII. [c] Tab. X. fig. I. [d] Ibid. fig. 6. [e] Ibid. fig. 8.





Kirchiul ad vivum pinxit.

J. Mynde fe.

exhibet, erant millia, celerrimo motu vel repentia vel natantia: ſex ſuis pedibus poſtremis ramulum arripiebant, ut erucarum, quas geometras appellant, mos eſt, et mirum in modum prone et ſupine ſe flectentes, de ramo in ramum ſaliebant quaſi agiliſſimi. Inter hæc erant pauca cæteris majora, quæ [f] naturali et [g] aucta magnitudine delineare curavi. [h] Alterum animal non minus mirum erat: ſed horum omnium cognitio multo melius delineationis inſpectu, quam ex valde prolixa deſcriptione peti poteſt.

Sed ſi omnia inſecta marina, quæ in diverſis corallinis reperi, delineare vellem, infinitum opus ſuſciperem; nam eorum numerus et diverſitas captum noſtrum ſuperant.

Hæc ergo, ut ſpero, ſufficient ad demonſtrandum corallinas non polyporum opus aut fabricam eſſe, ſed his et plurimis aliis inſectis marinis domicilio et perfugio aut alimento inſervire.

Dabam Zirizœæ in Zelandia,
17 Martii 1757.

TABULARUM EXPLICATIO.

Tabula Septima.

Fig. I. Exhibet corallinæ plantam, quæ corallina muſcoſa, ſive muſcus marinus tenui capillo ſpermophorus vocatur.

Fig. II. Corallina ramulis dichotomis teneris capillaribus rubentibus.

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[f] Tab. X. fig. II. B. [g] Ibid. C. [h] Ibid. fig. III.

Fig. III. Junior planta corallinæ tubulariæ laryngi similis.

Fig. IV. Duæ ſpecies *a*, *b*, fig. I. et II. et *c* eſchara papyracea utrinque cellulifera, uni baſi adnatæ, quod ſæpius in doliis marinis coniformibus contigit.

Fig. V. Corallinæ rubræ ramulus, quem per aliquot hebdomadas in aqua marina ſæpius renovata ſervavi, quo tempore parvi ramuli *a*, *a*, multum creverunt, et alii *b*, *b*, pullulaverunt.

Fig. VI. Pars conchæ oſtrei, in qua, præter filamenta quædam viridia, duo polypi *a a* conſpiciendi.

Fig. VII. Cancer arachnoideus, cui duæ polyporum ſpecies inſidebant. Singulus in *a*, et multi cellulas habitantes in *b*.

Fig. VIII. Animal, quod *aarſgat* vocatur, et januis emiſſariorum veterioribus et navibus accreſcit: huic parva corallinæ planta erat innata, in qua nullos detegere poteram polypos; plurimos vero *b. b.* ipſi animali inſidentes.

N. B. Caudas horum et præcedentis figuræ polyporum nimis longas delineavit pictor, ut eo melius in conſpectum venirent.

Tabula Octava.

Fig. I. Ramus corallinæ rubentis magnitudine naturali.

Fig. II. Idem microſcopio viſus, et tres polyporum ſpecies in eo conſpiciendæ.

a. b. Duæ diverſæ ſpecies caudâ vel corporis parte poſteriore corallinæ affixæ.

c. Tertia ſpecies in cellulis habitans.

d. Poly-

I. Rhodiad ad vivum pinxit

J. Mynde sc.

d. Polypus mortuus.

e. Polyporum cellulæ.

Fig. III. Planta corallinæ tubulariæ laryngi ſimilis magnitudine naturali.

Fig. IV. Hujus plantæ ramus maximus, microſcopio viſus, in quo quinque diverſos polypos inveni.

a. Prima et maxima ſpecies polypi, quem coccineum voco, et tab. IX. fig. II. fortiore adhuc microſcopio viſum exhibet.

b. Eadem ſed minor ſpecies.

c. Tertia, quæ eadem ut hujus tabulæ fig. II. litt. *b.*

d. Quarta, quæ eadem ut hujus tabulæ fig. II. litt. *c.*

e. Quinta et minima polyporum ſpecies, maxime aucta magnitudine adhuc delineata fig. 1. tab. IX.

f. Cellulæ, quas quarta ſpecies habitat.

Fig. V. Corallina erecta pennata denticulis alternis caule appreſſis: in hac nulli erant polypi niſi in cellulis circumcirca truncam affixis *a a.*

b. Cochleæ magnitudine auctæ in B.

c. Eſchara millepora minima cruſtacea cellulis tubiformibus, animalculis domicilio inſerviens, et magnitudinæ auctæ in C.

Fig. VI. Corallina abietis forma, quam menſe Decembri accepi: ejus rami veſiculis vel ovulis *a, a,* per paria ordine quadam poſitis, erant obſeſſi.

A. Talis veſicula vel ovum microſcopio viſum.

b. Cochleæ, & *c.* Eſchara minima, ut in præcedente figura magnitudine aucta in B et C.

d. d. Dua corpuſcula fuſca, quæ microſcopio viſa nidum vermium referunt in D.

Fig. VII. Corallina pennata et ſiliquata, ab oſtreo abſtracta: in hac præter tres polyporum ſpecies

a A, *b* B. (quæ cædem ac in fig. II.) *c* C, ſex alia inſecta reperire contigit, quæ delineata ſunt in tab. X. fig. 1, 6, 8.

Tabula Nona.

Fig. I. Minimorum polyporum marinorum genus, cum polypis ramoſis *(polypes à bouquet)* aquæ dulcis conveniens.

A. talis polypus conſervæ marinæ viridi inſidens vix oculo nudo conſpicuus.

B. idem lente oculari, et in C fortiore viſus microſcopio.

Fig. II. Polypus coccineus, quem tabulæ ſecundæ

Fig. III. & IV. naturali et aucta magnitudine exhibent, hic fortiſſimo microſcopio viſus.

A. hic polypus expanſis brachiis, prædam expectans.

B. idem brachia contrahendo, prædam arripiens.

a. Brachia majora inferiora numero 16, 18, vel 20.

b. Brachia ſuperiora breviora numero 12, 14, vel 16.

c. Corporis pars ſuperior pyriformis, inferiori infixa.

d. Corporis pars inferior compreſſa.

e. Locus, ubi polypus corallinæ inhæret.

C. idem polypus a parte anteriore viſus, cum corporis partem ſuperiorem pyriformem in globulum contraxerat, quod in majoribus polypis (vide infra fig. IV, V, VI.) magis viſibile.

Fig. III. Similis polypus coccineus, cæteris major, ex cujus corpore (ubi partes *c* et *d* conjunguntur) octo ramuli enaſcebantur, qui in ſummitatibus duos vel tres gerebant globulos, punctum rubrum in medio habentes, et quos in polypos juniores excreturos fore ſperaveram fruſtra.

a. Hujus

Fig. I.

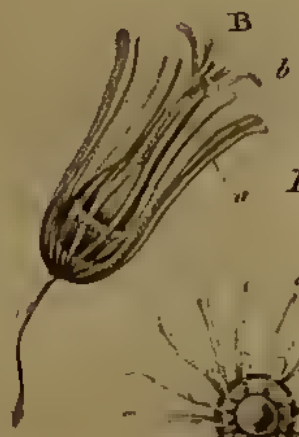


Fig. II.

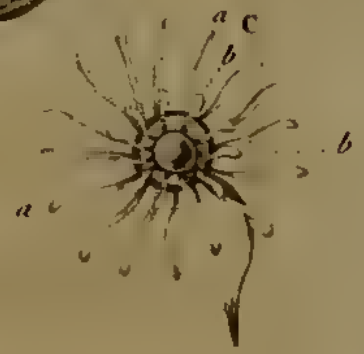
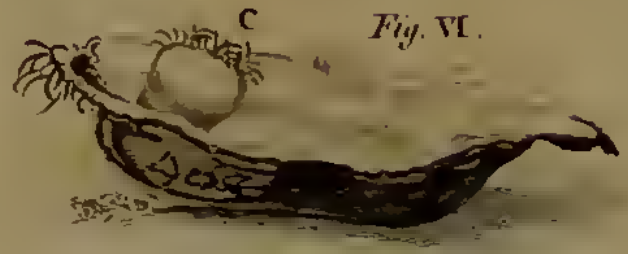


Fig. III.

Fig. IV.

Fig. V.

Fig. VI.



I. Rhodius ad vivum pinxit.

J. Mynde sc.

I. Rheinhold ad vivum pinxit.
J. Mynde sc.

a. Hujus polypi brachia longiora inferiora.
b. Brachia breviora ſuperiora.
e. In medio corporis pyriformis os polypi eſſe videtur.

Fig. IV. Majus, ut videtur, genus polyporum, quos *klapkonten* vocant, oſtreorum conchis inſidentium, quorum hic, rudius attractum, brachia penitus in corpus ſuum abſcondit.

Fig. V. Idem polypus corpore extenſo brachia expandens.

Fig. VI. Idem capta præda ſe contrahens.

Tabula Decima.

Fig. I. Tres ſpecies animalculorum lucentium in guttula aquæ marinæ fortiore microſcopio viſorum.

Fig. II. Mirum animalculum in corallinis a doliis marinis coniformibus abraſis repertum.
A. tales minores erant centeni.
B. decem vel duodecim erant hac magnitudine naturali.
C. idem animal microſcopio viſum.
a. Antennæ.
b. Primum par pedum vel brachiorum.
c. Secundum par.
d. Tertium et maximum par.
e, e, e, e. Quatuor corpuſcula oviformia, quæ animal ut movebant natando.
f, f, f, f, f, f. Sex pedes poſteriores, quibus ſimul corallinæ ramum arripiens, quaquaverſum ſe flectore poterat.
g. Cauda in cujus extrema parte anus.
h. Oculi.

Fig.

Fig. III. Aliud animal in iisdem corallinis repertum.
A. illud animal pronum.
B. supinum.
C. fortiore microscopio visum.

Fig. 1, 2, 3, 4, 5, 6, 7, 8. exhibent quasdam noctilucas, et alia animalcula in diversis corallinis reperta, ea magnitudine, qua tertia et quarta lens microscopii à Do. Cuff in Anglia fabrefacti illa ostendit. Animalculum *c*, fig. 8. mirabilissimæ erant structuræ, et plurima habebat membra.

XXXIII. *Remarks on Dr.* Job Baster's Observationes de Corallinis, &c. *printed above, p.* 258. *In a Letter to the Right Honourable* George *Earl of* Macclesfield, *President of the R. S. from Mr.* John Ellis, *F. R. S.*

My Lord,

Read June 9. 1757.

I HAVE read Dr. Job Baster's letter to the Royal Society; wherein he endeavours to prove, that corallines are not of an animal, but a vegetable nature; and has brought many arguments to support his system; which, to gentlemen not well acquainted with the subject, may appear plausible.

I could have wished the Doctor had read and examined thoroughly what has been lately written on the subject: I then should not have had occasion

to

to trouble your Lordſhip with the following remarks, which I find neceſſary to ſupport what I have already advanced on that head.

His firſt argument is, That becauſe he does not find as many polypes in the corallines adhering to ſhips, flood-gates, and buoys, as in deep water on oyſters, muſcles, and rocks, therefore he concludes, that corallines are not formed by polypes.

In anſwer to this, let us examine the pliable ſtructure of theſe bodies, and how wiſely nature has defended ſuch tender ſubſtances with a tough thin membranaceous covering, and we ſhall find, that the ſea is calm enough often near the ſurface to give them time to grow, even in the ſtrongeſt currents: but, without doubt, they are more liable to be deſtroyed in ſuch agitated ſituations, than in the calm depths of the ſea.

His ſecond argument is, That finding polypes are not equally diſperſed over the whole plant, how can they form it? and gives us an example, *Tab.* VIII. *fig.* 5. of a coralline, that is incruſted with many other corallines or polypes on the ſtem, but has none on the branches.

Here we plainly ſee the miſtake: the Doctor looks for the tender part of the polype on the ſurface of the coralline, conſidering it as a plant; and indeed, if this was the caſe, he ought ſo to do; but he never once takes notice of the internal hollow ſtructure of the ſtem, branches, and denticles of thoſe bodies, to inform us, whether he found an animal in thoſe parts or no. This material point he ſeems not to have thought on; which is really the true point in controverſy at preſent among gentlemen, who have not examined theſe bodies recent in ſea-water.

His third argument is, That almoſt always one and the ſame coralline plant cheriſhes polypes of different kinds; and refers us to Tab. VIII. fig. 2. and 4.

In fig. 2. he gives us an elegant painting of a geniculated red conferva for a coralline, ſurrounded, as is very common, by many ſpecies of ſmall corallines and eſcharas. And in fig. 4. he gives us a drawing of one of the tubular corallines, with the head of the animal at the top of it; the ſtem of this is incruſted with four different corallines and eſcharas, like the conferva fig. 2; and then he aſks, which of theſe five polypes made the tubular coralline?

To give him ſome proof of the animal nature of this coralline, let him conſult Ray's Synopſis, ed. 3. p. 34. n. 4. and there he will find one of this ſpecies, called *adianti aurei minimi facie planta marina*, taken notice of ſo long ago as the year 1713. by Dr. Lloyd, as a zoophyte, from its ſtem or tube's being full of a thick reddiſh liquor, rather reſembling blood than the juice of a plant; which, upon preſſing the ſtem, communicated with the little head at top.

His fourth argument is, That as upon one and the ſame coralline plant you ſhall find different kind of polypes; ſo, in different ſpecies of coralline, the ſame polypes: and, to confirm this, he quotes my Eſſay on Corallines; where I have remarked, that the polypes in the denticles of the ſetaceous or briſtly coralline, N°. 16. appear to be like thoſe, that are on the lobſter's-horn coralline, N°. 19. And to illuſtrate this, he obſerves, that bees and waſps always build their cells invariably the ſame; and that therefore theſe two corallines ſhould be the ſame.

But

But herein he takes this matter wrong: he has conſidered, in all his obſervations, the heads of thoſe parts of the polype, in which are the mouths, arms, or tentacula, which appear coming out of the cups, denticles, and at the ends of the tubes of the corallines, as ſo many whole and intire animals, without ever obſerving, that the body of the animal is contained in the tubular part of the root, ſtem, and branches; and that theſe differ from one another widely both in ſize and ſhape, as he may plainly ſee in the two corallines he has inſtanced: for the more exact drawings of which, I ſhall refer him, *viz.* for the ſetaceous or briſtly coralline, to my Plate, N°. 38. the natural ſize of which is at fig. 4. and the magnified one at fig. D: this he will obſerve to have a ſmall ſtem, and its branches diſpoſed in a pinnated form: and for the lobſter's-horn coralline, I ſhall refer him to Tab. xxii. of Vol. xlviii. of the Philoſophical Tranſactions; where, at N°. 3. the natural ſize is expreſſed, and at C the upper part of this coralline is drawn in proportion to the briſtly coralline from the ſame magnifying glaſs; which ſhews the ſtem to be much larger, and ſurrounded by its branches growing in whorles at equal diſtances, not unlike the equiſetum, or horſe-tail plant; and yet the heads of this animal nearly reſemble the other, only a little larger. Further, his compariſon to bees and waſps, and their cells, is not concluſive: for theſe ramified, hollow, and denticulated bodies, called corallines, which we ſo frequently find dead on our ſhores, are properly ſkins of certain marine polypes, and not neſts, as thoſe conſtructed by theſe little winged animals are. And yet we find as great a

regularity in the ſame ſpecies of theſe corallines, as when we compare two oak trees to one another, or two of Mr. Trembley's branched freſh-water polypes to one another.

He then proceeds to his fifth argument, That if corallines were formed by polypes, neither the polypes, nor even their cells, would ever fix on living animals, or any other bodies.

Here we may obſerve, that the conſequence he draws doth not follow: for corallines may be formed or produced by certain ſpecies of polypes, and yet polypes of another ſpecies may be found adhering to other bodies, and even to animal bodies.

By his ſixth argument he endeavours to prove, That the veſicles, which are found in regular rows on the ſea-fir coralline in winter, Tab. VIII. fig. 6. do not belong to it; and are no more than the eggs of ſome ſea inſect depoſited on it, of which there may be a great variety.

But to convince him of his miſtake, let him take off one of the veſicles, and apply a large magnifier to the place, and he will diſcover a hole, by which this veſicle or ovary has had a communication thro' the ſkin with the parent polype. For a further illuſtration of the manner in which theſe veſiculated polypes breed, let him conſult the 38th Plate of my Eſſay, where he will find ſeveral accurate figures (drawn by Mr. Ehret from the life) of theſe veſicles, with the ſpawn of the polypes coming out of them; ſome of which ſpawn we evidently diſcovered to be young polypes with their arms formed; and, as they fell from the veſicle, extending themſelves in the watch-glaſs of ſea-water.

In

In examining the drawings for his plates, I have obſerved, that Tab. VII. fig. 2. is evidently a red conferva, which he calls a coralline. We have no corallines, but many confervas, of this form and bright red colour on our coaſts; and theſe ſhores, I believe, are allowed to have ſimilar marine productions with thoſe of Holland.

Tab. VII. fig. 5. he calls a branch of red coralline, which he ſays he kept ſeveral weeks in ſea-water, and that often changed; during which time it ſprouted and grew very much. This experiment, I am perſuaded, is very true; becauſe it is plainly a vegetable, as appears from his own exact drawing of it; and ſeems to be the *fucus teres rubens minus in longum protenſus* of Ray's Synopſis, ed. 3. p. 51. N. 53. This is one of his principal arguments to prove the vegetation of corallines.

Tab. VIII. fig. 1. he calls a branch of red coralline; and at fig. 2. he has it magnified, where it appears to be a geniculated red conferva, drawn and painted with great exactneſs.

Theſe arguments, my Lord, and theſe figures of real vegetables, which the Doctor has given us for corallines, ſhew, how much he is willing to ſupport the old opinion of the botaniſts: but I am ſatisfied he will ſoon alter his opinion, when he obſerves the remarkable difference of the texture of vegetable and coralline bodies, when viewed in ſea-water thro' a good aquatic microſcope. And to convince him more fully, that corallines are an animal ſubſtance, let him burn them, and he will perceive the ſame pungent volatile alkaline ſmell, which he finds in burning

burning horn, hair, or oyſters; whereas burnt fucus's and confervas yield a ſmell not much unlike that of common land vegetables. Even the ſtony corallines, when their cretaceous covering has been diſſolved in vinegar, the membranous part, that remains of them, put into the fire, yields the ſame animal ſmell with other corallines.

Further, ſince I find the Doctor has promiſed the Royal Society to continue his reſearches at the ſea-ſide, the following hints may be of uſe to him. And, firſt, he will find, that thoſe he ſeems to think naked polypes, which he found adhering to corallines and other bodies, are really ſmall corallines and eſcharas, with their proper ſkins and cells; all which I have particularly deſcribed already. I would then recommend him to examine ſuch corallines as are taken out of the deepeſt water, which are found adhering to ſhells and fucus's. He will find Mr. Cuff's aquatic microſcope, or one of that form, the moſt commodious for obſerving theſe animals alive.

The moſt tranſparent ones he will find the beſt to diſcover their gelatinous inſide, which runs thro' the ſtem and ramifications, and ends in the heads, where the claws are. Some of the beſt kinds to obſerve are as follows: The ſea-oak coralline, the lily-flowering coralline, the great tooth coralline, the ſea-thread coralline, and the branched tubular coralline. Pieces of theſe ſhould be cut off while they are in the ſea water, and placed in watch-glaſſes full of the ſame: in theſe they ſhould remain a while, till they recover themſelves; and when they are placed on the ſtage of the microſcope, the motion of the internal part communicating with the heads will be eaſily diſcovered.

If

If the Doctor will immerſe ſome of theſe coral-lines, when they are extended, in two thirds of ſpirit of wine and one third of clear ſea-water, it will preſerve them many years, as I have experienced. He may then put the different ſorts into diſtinct phials, and view them at pleaſure with a lens of about one inch and half focus.

In fine, my Lord, opportunities ſo ſeldom offer at the ſea-ſide to make theſe experiments with accuracy; add likewiſe to this, the ſtrong lines of vegetation that theſe bodies carry in their appearance, and your Lordſhip will not be ſurpriſed, that there are ſo many gentlemen, even of the Royal Society, that totally diſbelieve them to be animals.

Many there are in the Society, that are wavering between both opinions. If then, my Lord, you think, that any ſpecimens which I have, or any demonſtrations tending to clear up this point, that lie in my power, will be acceptable to your Lordſhip and the Society, your Lordſhip may freely command them, whenever you think proper, from

Your LORDSHIP'S

Much obliged and moſt obedient Servant,

London,
June 9. 1757.

John Ellis.

XXXIV.

XXXIV. *An Account of an extraordinary Operation performed in the Dock-Yard at* Portſmouth: *Drawn up by Mr.* John Robertſon, *F. R. S.*

Read May 26. 1757.

THE Royal William, a firſt rate man of war, built about 40 years ago, having, upon examination, been judged in ſo good a ſtate, as to be worthy of repairing for ſea ſervice, was ordered into dock, and brought thither on the 29th of June 1756. On theſe occaſions it is uſual to lay acroſs the middle line of the bottom of the dock, at diſtances of about five feet from one another, thick pieces of oak timber of about four feet long; their upper ſurfaces lying in the ſame plane, or ſo poſited, that a line ſtretched from the two extreme blocks will touch all the intermediate ones; and on the middle of theſe blocks the keel of the ſhip is to reſt. On the ſaid day the tide did not riſe ſo high as was expected; and there was not quite depth enough of water to float the ſhip in, and ſet her on the blocks, notwithſtanding the aſſiſtance of an empty lighter, which, being fixed to the ſtern, lifted the ſhip at that end ſix inches: and as the officers knew they ſhould not have ſo much water again before the next ſpring-tides, they were determined to heave her in; which is a very common operation in moſt dock-yards. Now it ſo happened, thro' the great weight of the head and ſtern, that the ſhip cambered very much; that is, her keel, from being ſtrait, was become much curved, the

two

two extremities hanging lower than the middle part by many inches; and conſequently the foremoſt part of the keel, inſtead of ſliding over the blocks, forced all the foremoſt ones away, for above 60 feet; whereby that part of the keel reſted on the bottom or floor of the dock, and the aftermoſt part reſted on ſuch of the blocks, as had eſcaped the violence, which had diſplaced the others. In this ſituation the keel was very far from being ſtrait; and ſo it was reſolved to liſt by main force the head of the ſhip, until the keel ſhould be ſtrait; and in that poſition to ſupport it by the blocks, which had been forced away from their places.

For this purpoſe there were ſet up, under the wales and other parts of the ſhip, to the length of near 80 feet of the ſtem, as many ſhoars, as were judged neceſſary; and alſo nine pair of bed-ſcrews, three pair under each bow, and three pair under the knee of the head. At each ſhoar a workman was appointed, to drive wedges between the heels of the ſhoars and the parts of the dock whereon they reſted; whereby the ſhoars were raiſed end-wiſe, and conſequently the body of the ſhip lifted at the ſame time. While this was doing, the 18 ſcrews were alſo at work: and between theſe efforts, the fore part of the ſhip was raiſed upwards of 19 inches, ſo much being neceſſary to bring the fore part of the keel in a right line with the hinder part.

In this ſervice were employed about 270 men; whereof about 144 worked at the ſcrews, and the others worked at the ſhoars with their mawls and wedges; and the whole operation was performed in about ſeven hours.

My curiofity leading me to inquire what was the weight of the ſhip, in the condition ſhe was at the time of bringing her into the dock; for this purpoſe I procured draughts of the elevation and ſection, and of the plans at the line of floating, and at the parallel ſections of every foot diſtance down to the keel. Then, by finding the mean area between every two ſections, I was thereby enabled to come at the magnitude of a ſolid, that would nearly fill the trough the ſhip made in the water; and, by increaſing this magnitude by that of the keel, and ſo much of the ſtern-poſt and ſtem, as were under water, the cubic feet of the fluid diſplaced by the ſhip were obtained; being 54869; and conſequently her weight was 3532091 pounds, or 1576 tons, 16 *C.* 2 qrs. 3 ℔. Theſe numbers were not altogether ſo eaſily come at, as they would have been, had the ſhip ſwam on an even keel, her draught of water before being 13 feet 2 inches, and abaft 16 feet 6 inches. However, the computation may be eſteemed as correct as the nature of the ſubject would admit; becauſe I found pretty near the ſame ſolidity by another method.

I got a block or model made, by a ſcale of a quarter of an inch to a foot, of ſo much of the Royal William's body, as was immerged, when ſhe was brought into dock; and this block I immerſed in a trough of ſea-water, and found its weight in the following manner.

The length of the trough was 46 inches, breadth 14 inches, and depth 8 inches: at each corner was a graduated ſcale of inches, and pencil-lines drawn round the inſide of the trough at every inch. Sea-water was poured into the trough to the height of 5 inches;

inches; and the trough was exactly levelled, by means of the pencil-line, at 5 inches: then the block being forced under the water's surface, the fluid, when still, was risen to $6\frac{1}{3}$ inches; consequently the magnitude of the block was equal to a parallelopipedon of 46 inches long, 14 inches broad, and $1\frac{1}{3}$ inches deep, or to $858\frac{2}{3}$ cubic inches.

Now $858\frac{2}{3}$ cubic inches are equal to 0.4969 cubic feet.

And a cubic foot of sea-water weighs 64.3732 pounds avoirdupoize.

Then $64.3732 \times 0.4969 = 31.987$ pounds.

So that by a quarter inch scale, a model similar to the Royal William weighs near 32 ℔.

But a quarter inch scale is $\frac{1}{48}$ of a foot scale.

And the model is to the ship as $\overline{1}^{13}$ is to $\overline{48}^{13}$, or as 1 is to 110592.

Then 3537506 ℔ ($= 110592 \times 31.987$), or 1579 tons, 4 *C.* 3 qrs. 14 ℔. is the weight sought.

The difference by the two methods amounts to 5415 ℔. or to 2 tons, 8 *C.* 1 qr. 11 ℔.

Some of the persons present at this experiment read the height of the water at $6\frac{3}{8}$ inches: the difference between $6\frac{3}{8}$ and $6\frac{1}{3}$ inches is $\frac{1}{24}$ of an inch; a difference easily to be made by different persons in an experiment of this kind. But observing, that the computation made on $6\frac{3}{8}$ inches amounted to near 50 tons more than on $6\frac{1}{3}$ inches, I caused the trough to be diminished in its depth to $6\frac{1}{2}$ inches, had one of the ends cut off, and a board fixed on the open side, being desirous of making the experiment with the trough standing on one end: and indeed, in this situation, an error of $\frac{1}{10}$ of an inch in the

height of the water makes a difference of about 16 ½ tons in the weight of the ſhip. Into this upright trough water was poured to the height of 36 inches; and the block being immerged, the water was raiſed 9 ⅓ inches: ſo that the block was equal in magnitude to a parallelopipedon of 14 inches long, 6 ½ inches wide, and 9 ⅓ inches deep, or to 849 ⅓ cubic inches: from whence I find the weight of the ſhip to be 1562 tons, 1 *C.* 2 qrs. 16 ℔. And altho' I take this number to be neareſt the truth, yet it may be obſerved, that it is no eaſy matter to come at accuracy in this ſubject by any of the methods in common uſe.

My next inquiry was, to find how much of this weight was lifted, and how to proportion it among the ſcrews and mawl-men: but in this, leſs accuracy muſt be expected than in the preceding inquiry; for the exact number of men employed is not known; neither can it be told, how many worked at the ſcrews, and how many with the mawls; and only a gueſs can be made at the part lifted. However, ſomething may be gathered, which may, perhaps, be worth the knowing.

Let the weight raiſed be taken at half the weight of the ſhip; for 64 feet, the length of the keel raiſed, is not far from half the whole length: add to this the ſally of the head, the weight of the forecaſtle, the friction of the timber, and the reſiſtance of the parts bent by the cambering: beſide, the mawls worked at ſeveral ſhoars ſet up abaft the ſaid 64 feet.

Now the weight by the laſt experiment was 3499064 pounds: one half, or 1749532 ℔. I take to

to be the weight raiſed between the ſcrews and mawls.

The diſtance between two contiguous threads of each ſcrew was $1\frac{1}{3}$ inches; the length of the two oppoſite levers was 12 feet 8 inches, or 152 inches, and deſcribed a circumference of $477\frac{1}{2}$ inches: each ſcrew was worked by 8 men: their force, reckoned at 30 ℔. each, makes the power working on each ſcrew equal to 240 ℔.

Hence, from the known property, each ſcrew could raiſe 65485 ℔.

And the 18 ſcrews raiſed 1178730 ℔.

Then there remained 570802 ℔. to be raiſed among about 126 mawls:

Which gives 4530 ℔, or a little more than two tons, to be raiſed by each man with his mawl and wedges; which is conſiderably leſs than what I have ſeen raiſed by way of experiment.

XXXV. *Obſervations on an Evening, or rather Nocturnal, Solar* Iris. *By Mr.* George Edwards, *Librarian of the College of Phyſicians.*

To the Reverend Dr. Birch.

SIR,

Read June 16, 1757.

ON Sunday evening the 5th of June 1757, being walking in the fields near Iſlington, about half a mile north of the upper reſervoir or baſon of the New River, I obſerved the ſun

ſun to ſink beneath the viſible horizon to the north-weſt, it being very clear in that quarter, except ſome thin clouds a little above the horizon, which were painted of fine red and golden colours, as is uſual when the ſun ſets in a calm clear evening. But about 20 minutes after ſun-ſet, as near as I could judge, it then being darkiſh, I was greatly ſurpriſed to ſee an Iris in the duſky air, at a height greater than is ſeen at any time in the rainbow. It was in the contrary quarter of the heavens to the ſetting ſun, and fell on the ſmoke, miſts, and evening vapours ariſing from the city of London and its neighbourhood. The arch ſeemed to be a full half circle, tho' its lower parts fell ſome degrees ſhort of the horizon. It was very diſtinctly ſeen for about 15 minutes. Its colours the ſame as in the rainbow, but fainter. The lower ends of the bow aroſe gradually higher from the earth, as the ſun declined beneath the horizon, until the whole arch diſappeared. The center of the arch was above the horizon at its firſt appearance. What moſt perplexed me, was, to find the cauſe of this painted arch. I could not believe, that it proceeded from the ſun-beams falling on rain; for there had been none that afternoon; nor was there any ſort of ſigns of rain or rainy clouds to be ſeen; the wind being northerly, and the air cool, and ſomewhat hazy in the quarter where the bow appeared; which was not near ſo bright as the rainbow appears to be in the day-time; and I believe, that it would not have been viſible at all in the preſence of the ſun. I imagine it was formed on the groſs particles of the evening vapours, mixed with thoſe of the ſmoke ariſing from the town;

town; for had the ſun-beams ſhot from beneath the horizon on falling rain at a conſiderable height above the earth, I believe the darkneſs would have rendered the appearance of ſuch a bow far brighter than it appears to the ſight in the preſence of the ſun: but this night or evening arch being reflected, as I ſuppoſe, from particles ſo minute as thoſe of floating vapours, gave but little light and colour to the ſight, and what would not have been viſible, had the ſun been above the horizon. For the ſame reaſon, the moon and ſtars are viſible in the abſence of the ſun, and, on the contrary, are unſeen when the ſun is preſent: and if we light a candle, and ſet it in the ſun-beams, the flame is loſt to our ſight, tho' the ſame candle will give us a conſiderable ſhare of light in the night. As I have never before ſeen or heard of ſuch an arch, I thought this account of it (imperfect as it is) might not be diſagreeable to the Royal Society.

It could not be a lunar arch, the moon being then many degrees below the horizon, and the arch in a place, where it could not be affected by the moon's rays. The conſciouſneſs of my inability to give a proper account of ſuch an uncommon appearance could not deter me from the attempt.

I think I have ſaid all that is neceſſary on this ſubject; yet am ready to anſwer any queſtion for the farther illuſtrating of it. I am,

Reverend Sir,

Your moſt humble Servant,

College of Phyſicians, London, June 6th, 1757.

Geo. Edwards.

XXXVI.

XXXVI. *The Effects of the* Opuntia, *or Prickly Pear, and of the* Indigo *Plant, in colouring the Juices of living Animals. Communicated by* H. Baker, *F. R. S.*

June 23d, 1757.

Read June 23, 1757.

MR. Baker received a letter yesterday from Dr. Alexander Garden, of Charles Town in South Carolina, part of which he hopes he shall be excused for laying before the Royal Society.

The Doctor writes thus: —— " As you desired, I " tried the effects of the prickly pear in colouring " the urine. A few days after your letter, I went " down to one of the islands, and gathered some of " the fruit, and gave four of the pears to a child of " three years of age, and six pears to one of five. " The next morning I examined the urine of both, " and it appeared of a very lively red colour, as if " tent-wine had been mixed with clear water. The " urine of the eldest was deeper coloured, and of a " darker look: the youngest (who always naturally " made clear urine) was of a more lively and beau- " tiful red. Next day I gave six pears to a Negroe " wench, who gave suck, and strictly forbad her " suckling her child for six or eight hours; and " then taking some of her milk in a tea-cup, and " setting it by for some hours, the cream had a red- " dish lustre, tho' it was very faint.

" I was led to this last experiment by an observa- " tion, which I made on the milk of cows, who " had

" had fed in an indigo-field: the indigo had not " only tinged their urine blue, but the cream of " the milk was of a moſt beautiful blue colour, and " had a radiated appearance from the centre (Is it " not hence probable, that the dye is the oily part " of the plant?). The milk underneath was clear " and white as uſual."

Dr. Garden wrote, a year ago, that the prickly pear grows in great abundance about Carolina; and alſo that the cochineal inſects are found upon it; but hitherto no attempts have been made to cure them as the Spaniards do. In hope, that ſome rich dye may be produced from the plant itſelf, Mr. Baker propoſed ſome experiments to Dr. Garden, which he intends to proſecute this ſummer.

XXXVII. *An Account of an extraordinary Shower of black Duſt, that fell in the Iſland of* Zetland *20th* October 1755 *. *In a Letter from Sir* Andrew Mitchell, *of* Weſtſhore, *Bart. to* John Pringle, *M.D. F.R.S.*

SIR, Pall-Mall, June 9th, 1757.

Read June 23, 1757.

IN compliance with your deſire, I made particular inquiry, whether at or about the time the earthquake happened at Liſbon the 1ſt of November 1755. any uncommon phænomena

* See Phil. Tranſact. Vol. xlix. Part 2. p. 509.

were obſerved to appear in the iſlands of Orkney or Zetland, as ſuch had happened about that time in other parts of Scotland. From Orkney I was informed, that nothing particular had happened; only, that about the time mentioned the tides were obſerved to be much higher than ordinary. I received from Zetland a letter, dated 28th May 1756. from Mr. William Brown, Maſter of the grammar-ſchool at Scalloway in that country, a ſenſible and obſerving man; wherein he writes verbatim as follows. " Bleſſed be " God, notwithſtanding the great devaſtations, that " have been made in other parts of the world by " earthquakes, we have been intirely free from any " diſaſter of that nature: nor has any thing extra- " ordinary happened in this country ſince you left " it; only on Monday the 20th October laſt, be- " twixt the hours of three and four in the afternoon, " the ſky being very hazy, as it uſes to be before a " ſtorm of thunder and lightning, there fell a black " duſt over all the country, tho' in greater quantities " in ſome places than in others. It was very much " like lampblack; but ſmelled ſtrongly of ſulphur. " People in the fields had their faces, hands, and " linen, blackened by it. It was followed by rain. " —— Some people aſſign the cauſe of it to ſome ex- " traordinary eruption of Hecla. But I ſhall trou- " ble you no more about it, as no doubt ſome of " your friends have written to you of it ſome time " ago."——

In June 1756. I returned to Zetland; and, upon further inquiry, found what Mr. Brown had written me was atteſted by Mr. Mitchell, parſon of the pariſh of Tengwall, and by ſeveral Gentlemen of credit and reputation, who had ſeen and obſerved

the

the ſame phænomenon in different parts of the country at the time above-mentioned.

Mr. Brown having omitted to mention, how the wind did blow at the time the black duſt was obſerved, I made particular inquiry about that circumſtance, and found it was from the S. W. which does not ſeem to favour the opinion, that the duſt proceeded from an eruption of mount Hecla, which lies about N. W. from Zetland; unleſs it may be ſuppoſed, that a north wind happening juſt before had carried this duſt to the ſouthward, and the ſouthweſt wind immediately following had brought it back to the northward. But, in this caſe, would not this black duſt have been obſerved in Zetland at its firſt travelling to the ſouthward? Upon inquiry, I did not hear it was.

Thus far I have obeyed your commands, which I will always do with pleaſure; and if you think it worth while to lay this letter before the Royal Society, I leave you at full liberty to do ſo, or not, as you think proper: but what it contains may be relied on as truth. I am, with great regard,

Dear Sir,

Your moſt obedient humble Servant,

And. Mitchell.

P. S. I may add, that the diſtance from mount Hecla to Zetland is between 500 and 600 miles.

XXXVIII. *A Deſcription of ſome Thermometers for particular Uſes. By the Right Honourable the Lord* Charles Cavendiſh, *V. P. R. S.*

Read June 30, 1757.

THE thermometer (TAB. XI. *fig.* 1.) is deſigned for ſhewing the greateſt degree of heat, which happens in any place during the abſence of the obſerver. It conſiſts of a cylinder of glaſs joined to a tube, and differs from common thermometers only in having the top of the ſtem drawn out into a capillary tube, which enters into a glaſs ball C, joined on to the ſtem at the place where it begins to be contracted. The cylinder, and part of the tube, are filled with mercury; the top of which ſhews the common degrees of heat as uſual. The upper part of the tube above the mercury is filled with ſpirit of wine, and ſome of the ſame liquor is left in the ball C, ſo as to fill it almoſt up to the top of the capillary tube.

Now when the thermometer riſes, the ſpirit of wine will be driven out of the tube, and will fall into the ball C. When the thermometer ſinks again, as the ſpirit cannot return back from the ball, the top of the tube will remain empty, and the length of the empty part will be proportional to the fall of the thermometer. Therefore, by means of a proper ſcale, the top of the ſpirit of wine will ſhew how many degrees it has been higher than when obſerved; which being added to the preſent height, will give the greateſt degree of heat it has been at.

To

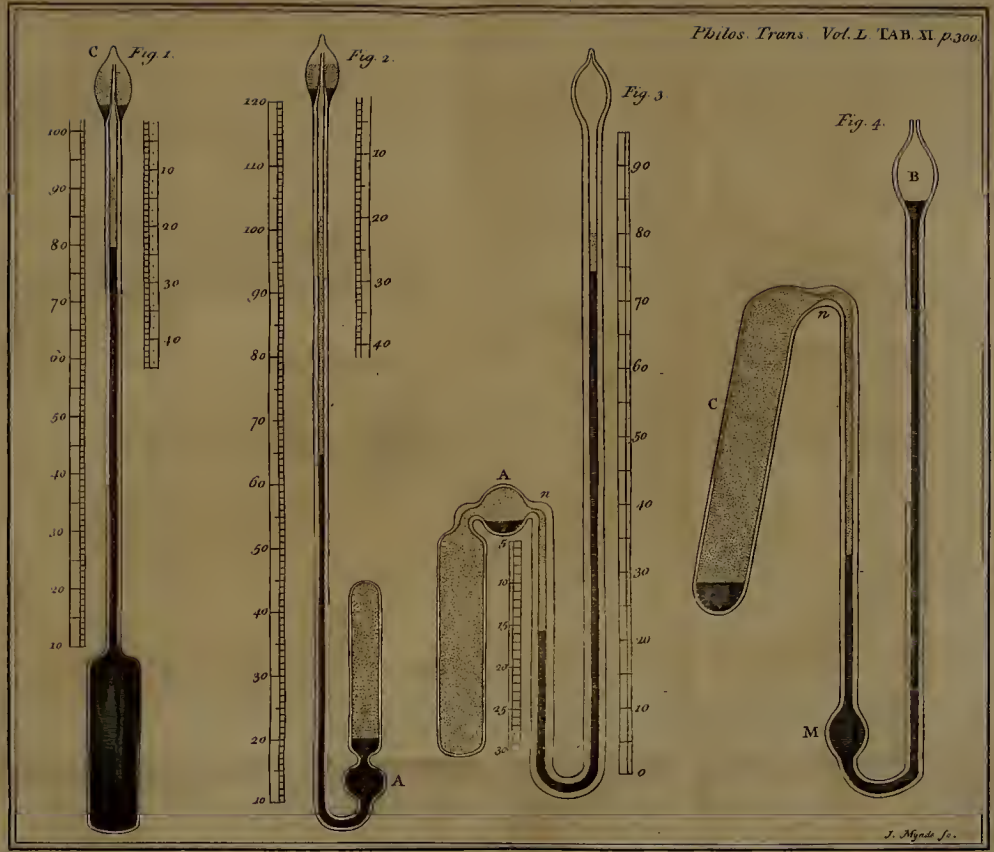


Fig. 1.
C
Fig. 2.
A
Fig. 3.
A
n
Fig. 4.
B
C
n
M
J. Mynde sc.

To fit this thermometer for a new obſervation, it is neceſſary to fill the upper part of the tube with ſpirits; which may be done, by inclining the inſtrument till the ſpirits in the ball C cover the end of the capillary tube. For if the cylinder is then heated, by applying the hand to it, or by the flame of a lamp held at ſome diſtance, till the ſpirits riſe to the top of the tube and run over into the ball C, and is then ſuffered to cool in the ſame poſition, the tube will remain full of ſpirits, and the thermometer will be fitted for a new experiment.

The top of the capillary tube is made to ſtand pretty near to one ſide of the ball, and alſo to the top of it, that a leſs inclination of the inſtrument may be ſufficient to make the ſpirit of wine in the ball cover the end of the tube.

The ball C is joined on as high as poſſible, ſo as to hide no part of the tube, except that, where the bore is contracted. By this means, the top of the ſpirit of wine begins to appear before the thermometer has ſunk one degree.

It will be convenient to leave ſome mercury in the ball C, which may be made to cover the end of the capillary tube, by inclining the thermometer more than what is neceſſary to make the ſpirit of wine cover it. By this means ſome mercury may be got back into the tube, in caſe any of it ſhould happen to be driven into the ball by the thermometer's being expoſed to too great a heat.

The ſcale of degrees at top, which ſhews the deſcent of the thermometer from the higheſt point it has arrived at, ought not, in ſtrictneſs, to be the ſame at all times of the year; for thoſe degrees exceed the

the common degrees of heat pointed out by the top of the mercury, as much as the column of ſpirit of wine expands, and therefore are greateſt when that column is ſo; that is to ſay, when the greateſt heat to which the inſtrument has been expoſed is leaſt. A difference of 30 degrees of Fahrenheit's ſcale, in the greateſt riſe of the thermometer, would require the ſcale to be altered one ſixtieth part: and the error ariſing from making uſe of the ſame ſcale will be about one ſixth of a degree, if the thermometer is obſerved when it has fallen ten degrees.

In the inſtrument here deſcribed, the bore of the tube is about 0.027 inches; and one inch of it contains two grains of mercury, and anſwers to about ten degrees, the cylinder containing about 2280 grains. If a much ſhorter tube was made uſe of, a conſiderable error might ariſe from too great a quantity of ſpirits adhering to the ſides of the tube, in that part, which is filled with mercury; eſpecially when the thermometer riſes faſt. This makes it neceſſary to employ a cylinder of a conſiderable bigneſs, if it is deſired to have the ſcale of degrees pretty large.

If the weight of the mercury is thought inconvenient, it may be avoided by the conſtruction deſcribed in fig. 2. where the bottom of the tube is bent ſo as to point upwards, and is joined to a ball A, which communicates with a cylider placed above it. In all other reſpects it is the ſame as the inſtrument before deſcribed.

It is filled with ſpirit of wine and mercury; the quantity of the latter being ſufficient to fill the whole tube and the ball A.

No

No part of the ſpirit, with which the cylinder is filled, can get into the tube, as long as the inſtrument is kept in an erect poſition, or even if it is carefully laid down flat on a table. For tho' in this laſt caſe ſome of the ſpirits may get into the ball A, it will riſe to that part of the ball, which is then uppermoſt, and will not touch the orifice of the tube *n*; which was the reaſon for adding this ball, which would be unneceſſary, if the inſtrument was kept conſtantly erect, or nearly ſo. If the ſpirit ſhould come to touch the orifice of the tube *n*, it would work up between the mercury and the glaſs; which would put the inſtrument out of order.

The thermometer fig. 3. is deſigned for ſhewing the greateſt cold, which happens in any place during the time the inſtrument is left in it. The tube is bent into the ſhape of a ſyphon of unequal legs ſtanding parallel to one another, the bend being at the bottom. The top of the ſhorter leg is bent to a right angle, and immediately opens into a ball A, which, by means of a ſhort bent tube on the oppoſite ſide, communicates with a cylinder ſtanding parallel to the legs of the ſyphon, and pointing downwards. This cylinder contains the greateſt part of the fluid; and is added only to make the thermometer more ſenſible than it would be, if the ball A was made of a ſufficient bigneſs to contain the proper quantity of fluid. This inſtrument is filled with ſpirit of wine, with the addition of as much mercury as is ſufficient to fill both legs of the ſyphon, and about a fourth or fifth part of the ball A.

The

The common degrees of heat are ſhown by the top of the mercury in the longeſt leg, or by the top of the ſpirit, in caſe any of it is left above the mercury.

When the mercury in the longeſt leg ſinks by cold, that in the ſhorter leg will riſe, and will run over into the ball A; from whence it cannot return back when the thermometer riſes again, as the ſurface of the mercury in the ball is below the orifice of the tube *n*. Therefore the upper part of the ſhorter leg will be filled with a column of ſpirits of a length proportional to the increaſe of heat; the bottom of which, by means of a proper ſcale, will ſhow how much the thermometer has been lower than it then is; which being ſubſtracted from the preſent height, will give the loweſt point that it has been at.

If no further contrivance was uſed, the mercury would fall into the ball A in large drops; which would make the inſtrument leſs accurate. For the thermometer's beginning to riſe immediately after a drop is fallen, or juſt as it is going to fall (in which caſe it will return back into the tube), will make a difference of ſuch part of a degree nearly as that drop anſwers to. To prevent this inconvenience, the top of the ſhorter leg, cloſe to the ball, is contracted, by being held in the flame of a lamp; and the paſſage is further ſtreightened by a ſolid thread of glaſs placed within the tube, and extending from the bottom of the ſhorter leg to the part near the ball A, where it is moſt contracted. By this means, as ſoon as any ſmall portion of mercury is got beyond the end of the thread of glaſs, it breaks off, and falls into the ball

ball in very ſmall drops. This thread of glaſs is faſtened by the heat given to the tube in making the bend next to the ball. In order to fill the ſhorter leg with mercury, to fit the inſtrument for a new experiment, it muſt be inclined till the mercury in the ball covers the orifice of the tube *n*. The cylinder being then heated, the mercury will be forced into the ſhorter leg, and will run down the thread of glaſs in drops, which will ſoon unite. By this means, ſuch a quantity of mercury muſt be got into the ſhorter leg, as, upon the cooling of the inſtrument, will be ſufficient to drive all the ſpirit of wine into the ball with a leſs degree of cold than what the thermometer is likely to be expoſed to.

The ball A muſt always have ſome mercury in it, but never enough to fill it up to the orifice of the tube *n*. It muſt therefore be made of ſuch a ſize, as to contain all the mercury, which can come into it from the tube without being too full. If it ſhould happen to be made too ſmall, ſo as to be too full in cold weather, any part of the mercury may eaſily be driven into the cylinder, and got back again into the ball when wanted in warmer weather.

It will be better to leave a little of the ſpirit above the mercury in the longeſt leg; in which caſe the top of the ſpirit will ſhew the common degrees of heat. For the filling the tube, ſo as to leave none, is attended with ſome trouble; and more of it will be apt to get up there, if the inſtrument ſhould happen to be held in an improper ſituation, or if it be kept in too warm a place without filling the ſhorter leg with mercury by the method above deſcribed. If too great a quantity ſhould get up, tho' it would

not affect the ſcale for the common degrees of heat, it would however cauſe ſome error in the degrees on the ſhorter leg; inaſmuch as the expanſion of that portion of ſpirits, which has got up into the longer leg, exceeds the expanſion of the mercury, which muſt ſupply its place. It may be got back at pleaſure, by expoſing the thermometer to ſuch a degree of cold as will make the ſpirit get beyond the bend of the ſyphon; for then it will run up along the thread of glaſs in the ſhorter leg till it gets above the mercury there. For this purpoſe the point of 0 degrees of Fahrenheit's ſcale ſhould be near the bend; by which means, any part of the ſpirit of wine may be got beyond it by an artificial cold; and there will be no danger of the whole getting beyond it by any natural cold; in which caſe the air would get up into the ball.

The ſcale of degrees on the ſhorter leg will, in different ſeaſons, be liable to an error of the ſame kind as that, which was explained in the firſt-mentioned thermometer; but in this it will be leſs conſiderable, as the ſpace between the two ſcales is filled with mercury, whoſe expanſion is about ſix times leſs than that of ſpirit of wine.

In the thermometer, which I have, the bore of the tube is about 0.054 inches; and one inch of it contains eight grains of mercury, and anſwers to ſeven degrees of Fahrenheit's ſcale. The drops of mercury, which fall into the ball A, anſwer to about one eighth of a degree.

If inſtruments of the nature of thoſe above deſcribed, were to be uſed for finding the temper of the

the ſea at great depths, ſome alteration would be neceſſary in the conſtruction of them, principally upon account of the great preſſure of the water; the ill effect of which can, I believe, be prevented no other way, than by leaving the tube open. For if the thermometer was made ſtrong enough to reſiſt the preſſure without breaking, yet it would be impoſſible to be ſure, that the figure of the glaſs would not thereby be altered, which ſhould make the experiment uncertain.

The inſtrument for finding the greateſt heat might be made juſt like that of fig. 1. only leaving the top open. It is to be filled with mercury only, as is alſo the lower part of the ball at top, but not near ſo high as the end of the capillary tube. The upper part of that ball, being left open, will in a great meaſure be filled with the ſea-water, which will be forced into it by the preſſure.

If this inſtrument (the tube being quite full of mercury) is plunged into any part of the ſea, where the heat is greater than that of the air above, part of the mercury will be driven out of the tube; and, upon bringing it into a colder place, the ſea-water or air in the ball will enter into the tube, and will fill the ſpace left by the mercury.

As this thermometer does not ſhow the common degrees of heat, it muſt be placed in a veſſel of water with another thermometer, and the ſcale of degrees at top will ſhew how much the heat it has been expoſed to is greater than that of the water in the veſſel.

The ſea-water getting into the glaſs will corrode the mercury, and thereby foul the glaſs; which

will make the experiment leſs exact: and therefore it would be much more convenient, if the ſea-water could intirely be kept out; which probably may be done by tying a bladder full of air to the neck of the ball C, which will contract by the preſſure of the water, without letting any of it get in.

If this can be done, the inſtrument may be filled with mercury and ſpirits, juſt like that at fig. 1. But it would be more convenient to fill it with mercury only: in which caſe it may be made with as ſmall a tube and ball as common mercurial thermometers: or it may be filled with ſpirits only. The inſtrument will thereby become much leſs bulky; which will compenſate the want of the common ſcale of heat.

It is better to put but little mercury into the ball at top, for fear of its getting into the capillary tube by the motion of the inſtrument.

The thermometer for finding the greateſt cold, if applied to this purpoſe, muſt alſo be left open at top. There is another inconvenience to be avoided; which is, that the mercury in the ball A, by the toſſing of the inſtrument, might ſometimes get into the ſhorter leg of the ſyphon; which would ſpoil the experiment. To prevent ſuch an accident, the moſt convenient conſtruction, which occurs to me, is that of fig. 4. which differs from fig. 3. in having the ball A omitted; ſo that the mercury running out of the ſhorter leg will fall to the bottom of the cylinder, and will not be ſo liable to get back into the tube by motion. The cylinder is made to ſtand not quite parallel to the legs of the ſyphon, that the mercury contained therein may more eaſily be brought to touch

touch the end of the tube, in order to fit the inſtrument for a new experiment.

If, by means of a bladder, the ſea-water can be kept out of the glaſs, this inſtrument may be made to ſhew the common degrees of heat; but even then, in order to render it leſs bulky, it may be better to ſupply the want of them as in the laſt inſtrument. The longer leg of the ſyphon may in that caſe be made as ſhort as you pleaſe; only making the ball B big enough to receive all the mercury, which may be driven into it by heat.

If thermometers of this kind were to be ſent up into the air by means of a kite, they might be made like thoſe propoſed for the ſea; but it would not be neceſſary to leave them open.

As it would be deſirable to make them as ſmall as poſſible, they ſhould be made ſo as not to ſhew the common degrees of heat: and it would alſo, on that account, be convenient to omit the thread of glaſs placed within the ſhorter leg of the ſyphon in fig. 3. and 4. This thread of glaſs is placed there in order to make the mercury fall into the ball A. fig. 3. or cylinder C. fig. 4. in ſmaller drops, and alſo to facilitate the filling the ſhorter leg with mercury. The latter purpoſe may be anſwered by having a ball blown towards the bottom of the ſhorter leg, as marked in fig. 4. at M: for as ſoon as the mercury driven out of the cylinder by heat gets to that ball, it will paſs by the ſpirit of wine. The other purpoſe may probably be anſwered by having the tube contracted as much as poſſible at *n*.

In the foregoing inſtruments the tubes made uſe of were of a large bore, as moſt of the errors in them

them would increaſe by making uſe of ſmaller ones. Poſſibly leſs ones might be uſed without much inconvenience. The chief advantage will be, the making theſe thermometers more ſenſible of the changes of heat, than when large cylinders are uſed. This will be of ſervice when the greateſt degree of heat or cold continues but a ſhort time.

It is better to uſe plain ſpirit of wine, than what is tinged, which ſeems more apt to cauſe a foulneſs in the tube, and thereby makes the ſurface of the mercury leſs well defined. I am induced to believe ſo, from obſerving, that the portion of ſpirits above the mercury in the tube fig. 3. which at firſt was ſtrongly tinged with cochineal, in ſome months became perfectly colourleſs, the tinging particles being depoſited in different parts of the tube, and cauſing a foulneſs there. The colour of the ſpirits in the cylinder does not appear to be altered.

The dark-ſhaded part in the ſeveral figures repreſents mercury, the dotted part ſpirit of wine.

XXXIX.

J. Mynde f.

XXXIX. *Obſervationes Anatomico-Medicæ, de Monſtro bicorporeo Virgineo A.* 1701. *die* 26 Oct. *in* Pannonia, *infra* Comaromium, *in Poſſeſſione* Szony, *quondam Quiritum* Bregetione, *in lucem edito, atque A.* 1723. *die* 23 Febr. Poſonii *in Cœnobio Monialium* S. Urſulæ *morte functo ibidemque ſepulto. Authore* Juſto Johanne Torkos, *M. D. Soc. Regalis Socio.*

[*See* TAB. XII.]

Read May 23. 1751.

I. PARTUS hic bicorporeus ſingulare exemplum exhibet admirandarum virium imaginationis maternæ in fœtum utero contentum. Mater enim hujus bicorporis, primis graviditatis ſuæ menſibus vel potius hebdomadis, attentius contemplabatur canes coëuntes, arctius cohærentes, et capitibus erga ſe invicem quodammodo converſos, eoſque ſibi crebrius præfigurabat.

II. In partu, primum prodiit umbilicotenus Helenæ corpus; poſt tres demum horas editi ſunt ejus pedes, cum adnexo corpore altero Judithæ. Helenæ corporis ſtatura erat altior et rectior, Judithæ brevior et obliquior; et quamvis infra lumbos, a tergo, in unum corpus concretæ fuiſſent, attamen vultu et corporibus, ſemilateraliter, erga ſe fuerant converſæ, ut commode ſedere, lentoque gradu procedere et recedere potuerint. Unus communis ipſis erat alvi exitus, intra duas nates, ſeu Helenæ dextrum et Judithæ ſiniſtrum femur, ſituatus. Unam quoque habebant vulvam, intra quatuor pedes reconditam, ut dum erectis

erectis ſtarent corporibus, ne veſtigium ejus conſpicuum eſſet. Quoad duos iſtos excretionum meatus, obſervatum eſt, quod, una excretionem alvi moliente, altera quoque niſum egerendi ſenſerit; in reddenda vero urina, quælibet, diverſo tempore, ſtimulos habuerit: quamobrem altera ad urinæ miſſionem ſolicitata, altera ſubinde receſſum negavit. Unde in juventute, utut alias ſemper ſemet tenerrime amarent et amplexarentur, ſæpius altercationes inter ipſas exortæ, et alterutra aliam vel dorſo injectam abripuit, vel colluctando eo, quo vellet, protraxit.

III. Anno ætatis ſexto, Judithæ paralyſis totius partis ſiniſtræ; obtigit ex qua affectione, utut convaluiſſet, per totam vitam ſuam debilior, tardior, et ſtupidior perſtitit; e contra Helena ſemper agilior, docilior, et formoſior fuit.

IV. Prout diverſa erant corpora, ita functionum vitalium, animalium, et naturalium, magna in utroque corpore, tam in ſano quam ægroto ſtatu, obſervata eſt differentia. Et quamvis variolas et morbillos uno eodemque tempore habuiſſent, reliqui tamen morbi eis non erant communes. Cum Judith ſæpius convelleretur, Helena nec alterata nec debilitata fuit. Helena erat pleuritica. Judith benigniore febre laboravit: alterâ tuſſi, catarrho, colicâ afflictâ, altera ſana exſtitit. Hinc etiam quælibet, pro ſuo diverſo ſtatu, diverſis medicamentis tractabatur: phlebotomia autem ſemper in ſaniore et vegetiore celebrabatur.

V. Anno ætatis decimo ſexto, menſtrua comparuerunt, quæ deinde per totam vitam, non tamen æquali tempore, modo, et quantitate ſucceſſere. Subinde alterutra majores hinc ſenſit moleſtias; Judith vero crebrius convellebatur, variiſque hyſtericis et pectoris affectionibus obnoxia fuit.

VI.

VI. Anno ætatis viceſimo ſecundo, ſeu A.C. 1723. die 8 Febr. Judith fortiter convulſa eſt, poſtea comatoſa, uſque ad mortem, quæ die 23 Febr. mane contigit, perſtitit. Intra hos dies Helena febricula laboravit, eique acceſſerunt crebriores lipothymiæ, quibus tandem ita debilitata eſt, ut integra quamvis mente et loquela, ſubito, tribuſque horæ minutis prius quam Judith, in agonem inciderit: poſtea vero ambæ, poſt brevem agonem, uno ferme momento expiraverint.

VII. Corporibus poſt mortem diſſectis, reperta ſunt in quolibet corpore viſcera ſingula · In Helena omnia ſana; in Judithæ thorace vero cor nimis magnum, fortiſſimo pericardio velatum, et pulmonum dexter lobus putridus: Arteria aorta et vena cava ex utroque corde deſcendentes, antequam arteriæ et venæ iliacæ ex iiſdem emergerent, inflexæ coadunabantur, et unam arteriam aortam, unamque venam cavam, e corde uno ad aliud procedentes ſeu reflexas, præſentarunt. In abdomine utrinque viſcera omnia ſana et integra. Quodlibet corpus ſuum habuit hepar, ſplenem, pancreas, renes, veſicam, uterum cum ovariis, tubis Fallopianis, et portione vaginæ, quæ utrinque concurrentes unam communem vaginam efformarunt. Partes genitalium externorum, præter commune orificium vaginæ, cuilibet erant propriæ, velut clitoris, nymphæ, orificium urethræ; alæ ſeu labia utrinque ad perinæum concurrentia foſſulam navicularem denſiorem conſtituerant. Ventriculus cum inteſtinis, in utrâque, naturaliter erant ſituata; inteſtina recta autem utrinque ad os ſacrum reflexa et coalita, unum ſatis amplum et communem canalem conſtituerunt: os ſacrum ad ſecundam diviſionem

concretum erat, et unum corpus efformando, in uno utrique offi ſacro communi, oſſe coccygis, terminabatur.

Ex prærecenſitis, ſicut cauſa diverſitatis actionum et functionum patet, ita etiam ex arteriarum aortarum, et venarum cavarum, inteſtinorum quoque rectorum et vaginarum uteri, compagine, coadunatione et harmonia, apparet ratio conformitatis et diſparitatis morborum, ſynthanaſiæ, communis niſus egerendæ alvi, poſſibilis impræ̃gnationis alterutrius, vel fors utriuſque virginis, uno eodemque coitu.

Hæc omnia conquiſivi et retexui, partim e relationibus fide dignis autoptarum; partim ex ephemeridibus B. Caroli Raygeri, ſoceri mei, qui, dum viveret, medicum cœnobii dicti ordinarium agebat; partim ex libro cœnobiali, cui B. Vir formulas medicamentorum inſcripſerat. Dab. Poſonii die 3 Julii 1757.

Juſtus Joannes Torkos,
Eques Pannonius, Medic. Doct. et Liberæ Regiæ Civitatis Poſonienſis Phyſicus ordinarius.

The interval between the reading of this paper before the Royal Society, and the preſent publication, was occaſioned by the long indiſpoſition, and afterwards death, of their late Preſident Martin Folkes, Eſq; who having taken it to his houſe, with a view of collecting and adding to it ſome further particulars, it could not be found after his deceaſe. But Dr. Torkos, the writer, being again applied to, immediately tranſmitted the copy of it printed

printed above: and, in order to ſupply in ſome meaſure the want of what Mr. Folkes's extenſive reading and induſtry might have furniſhed the public with, in relation to ſo very remarkable a fact, the following accounts, printed and manuſcript, are ſubjoined as a ſupplement to the preceding article.

Extract of a Letter of William Burnet, *Eſq; F.R.S. eldeſt Son of Dr.* Gilbert Burnet, *Lord Biſhop of* Saliſbury, *to Dr. (afterwards Sir)* Hans Sloane, *dated at* Leyden, May 9. 1708. *N. S.* *

" S I R,

" I Send you incloſed the print of a wonderful " union of two twin ſiſters, who are at this " time to be ſeen at the Hague. I ſaw them, and " obſerved all, that I could think tended to explain " the appearance. They are Hungarians, as the " lines under the print will ſhew you. There is " there an exact enough deſcription of their condi- " tion; only I may add, that in fig. 1. the urinal " paſſage is between the two foremoſt thighs, as " they are in the print. The ſame is true of the " anus in the 2d figure, in ſuch manner, that the " ſituation of theſe parts is the ſame to outward ap- " pearance as naturally, with this difference, that " they are between two different bodies here, where- " as in the courſe of nature they are between the " two parts of the ſame body. It ſeems probable,

* Original Letters to Sir Hans Sloane, Bart. vol. A---B. in the Britiſh Muſeum.

" that their parts are diſtinct; but that the moſt
" remote labia of each are outwardly viſible, and
" the two contiguous ones are within. There ſeems
" to be no cheat in the thing; and the ſkin, where
" they are joined, is perfectly ſmooth, without any
" ſcar. They are now about ſix years old. They
" ſpeak French and High German. They are very
" full of action, and talk one more than the other.
" When one ſtoops to take up any thing, ſhe carries
" the other quite from the ground; and that one
" of them often does, being ſtronger as well as more
" lively than the other. They have not their feeling
" common any where but in the place of their con-
" junction. This is all I can ſay about it. If you
" think it worth while, you will do me an honour
" in giving the print, and the ſubſtance of this ac-
" count, to the Society; to which, tho' an unworthy
" member, I would be proud to be capable of any
" ſervice."

This letter was read to the Royal Society on the 12th of May 1708 †; and the print mentioned in it produced; which, being now become extremely difficult to be met with, is thought proper to be engraved again, and inſerted here. *See* TAB. XIII.

Soon after the date of Mr. Burnet's letter, the twin ſiſters were brought to England, and publicly ſhewn in London, as appears from the following MS. note in a copy of the print bound up by the writer with Fortunius Licetus *de Monſtris* ‡, edit. Am-

† Journal, vol. xi. p. 143.

‡ In this treatiſe, L. 2. p. 80. is the following paſſage: *In pago Rorbachio non procul Heydelbergâ, Paræi etiam relatu, gemini utriuſque ſexûs obverſis tergoribus annexis orti ſunt.*

ſtelod.

Corpora Binarum sic concrevere Sororum,
Non nisi Divinâ dissocianda manu.
SZÖNY Patria est, vicus COMORÆ conterminus Arci,
Quæ nunquam Lunæ paruit Imperio.
Amplexa est ulnis HELENAM Lucina priorem,
Horis deinde tribus nata JUDITHA fuit.

Exitus Urinæ patet unicus, unicus alvo,
Observant numerum cætera membra suum.
Misit ad Ignotos tenuis Fortuna Parentum,
Neu pereat tantæ Fama stupenda rei.
Inter Iora Latent, neqVeVnt abstrVsa VIDerI:
EXIgVo totVM CorpVs In ere patet.

J. Mynde f.

ſtelod. 1665. 4to. in the poſſeſſion of Thomas Wilbraham, M. D. F. R. S. " *Londini* 14 *Junii* 1708. " *has vidi gemellas (plus annis ſex natas) quarum* " *forma et vivacitas elegantior et vegetior quam* " *pictura et deſcriptio.*"

Another account of them by an eye-witneſs in London is in a manuſcript volume among thoſe of Sir Hans Sloane, Bart. in the Britiſh Muſeum, intituled, *A ſhort Hiſtory of human Prodigies and monſtrous Births, of Dwarfs, Sleepers, Giants, ſtrong Men, Hermaphrodites, numerous Births, and extreme old Age, &c.* The name of the writer was James Paris du Pleſſis. In p. 39. under the Title *Two Siſters conjoined*, he gives a drawing of them, and the following deſcription: " Theſe two monſtrous " girls were born at Szony in Hungary in the year " 1701. They were born conjoined together at the " ſmall of the back. I aſked the father and mother, " if they could not be ſeparated one from the other? " but they anſwered, No; becauſe the urinary and " fœcal veſſels and paſſages were ſo united, as to have " but one iſſue for the urine, and another for the ex- " crements, betwixt both. They were briſk, merry, " and well-bred: they could read, write, and ſing very " prettily: they could ſpeak three different languages, " as Hungarian or High Dutch, Low Dutch, and " French, and were learning Engliſh. They were " very handſome, very well ſhaped in all parts, and " beautiful faces. Helen was born three hours be- " fore her ſiſter Judith. When one ſtooped, ſhe " lifted the other from the ground, and carried the " other upon her back; neither could they walk

" ſide

" ſide by ſide. They loved one another very ten-
" derly. Their clothes were fine and neat. They
" had two bodies, four ſleeves; and one petticoat
" ſerved to the bodies, and their ſhifts the ſame.
" When one went forward, the other was forced
" to go backward."

A later and more particular account is contained in p. 41, *& ſeqq.* of a book very ſeldom met with in this country, being printed at Vienna in 1729. intituled, *Gerardi Cornelii Drieſchii Hiſtoria magnæ Legationis Cæſareæ, quam Caroli VI. auſpiciis ſuſcepit Damianus Hugo Virmondtius*, &c. The following extract, tho' long, will not probably be thought unentertaining.

" Sunt in comitatu Commaronienſi in terris il-
" luſtriſſimi Zichii (pagus Hungaris Szony dicitur)
" à parentibus colonis, quibus ſua vita conſtat, dum
" hæc ſcribo, anno 1701. vii calendas Novembreis
" procreatæ in lucem duæ filiæ gemellæ, poſticâ
" parte, quâ ſpina dorſi deſinit, concretæ, ſic ut al-
" tera alteram ſequi, quo ſe cunque vertat, cogatur:
" cætera haud deformes aſpectu, niſi concretio illa
" corporum prodigium efficeret. Binæ ſingulis ma-
" nus, totidem pedes, et capita, necnon corpora:
" ſuus membris omnibus uſus; rationis multò etiam,
" quod mirere, certior; ut, ſi ſedentes ſolum videris,
" neque ſciveris, hic monſtri notare nihil valeas.
" Majori natu, quæ lucem citius aſpexit tribus
" horis, Helenæ, minori Judithæ nomen eſt. Hæc
" ante annos circiter tres ſtupore apoplectico tacta
" linguæ modicum ex eo ac bonæ mentis officium
" impeditum habuit, ſimplicitatem quandam ingenii
" modo

" modo ut redoleat. Illa animo ſemper integro at-
" que ſpiritu prædita eodem, pudicâ facie, non in-
" concinnis motibus, intuentium in ſe oculos ad
" miſericordiam commovet, utpote quæ rationis
" planè compos, ſororis tenerrimè amans, nec ſtatus
" ignara ſui, duplicem miſeriam tolerat, ſuam et
" iſtius. Ductæ ſunt olim infantes per varias re-
" giones ac provincias, Germaniam, Angliam, Gal-
" liam, Italiam, Poloniam, Bataviam, Auſtriam, Mo-
" raviam, Hungariam, à medico Hungarico nomine
" Cſuſzio, qui eaſdem certâ pecuniâ ad tempus ſibi
" a parentibus concreditas et elocatas, bonâ eorun-
" dem veniâ, paterno à ſolo âbduxit; unde trium
" gentium linguis, Germanicâ, Gallicâ, Hungaricâ,
" hodiedum etiam loquuntur; alias deſuetudine
" uſuque interrupto, ætate præſertim nondum ſatis
" confirmatâ, omnino dedidicere.

" Dux Auguſtus Saxo Cizius inter purpuratos
" LXXII patres à conſtantia religionis, timore erga
" Deum et caritate in proximum notiſſimus, archi-
" epiſcopus Strigonienſis, veritus, ne frequentes hæ
" perignationes puellarum adhuc infantium inno-
" centiæ officerent, ac mores denique illarum, ut
" fieri non raro aſſolet, planè depravarent, pacto
" perſolutoque pretio à medico redemit, et revocatas
" domum ad ſuos virginibus à divâ Urſulâ nuncu-
" patis intra Poſonium deinceps educandas commiſit,
" neceſſariis ad hoc ſumptibus benignè ſubminiſtratis.
" Ingreſſæ non diu puerilem ætatem fuerant, nonuſ-
" que illis annus agi cœptus currebat, quando harum
" in diſciplinam virginum tradebantur. Hic legere
" primum ac ſcribere, ea, quæ ad fidem neceſſaria
" ſunt, mente atque animo comprehendere; operas

" manuum

" manuum exercere varias, acu præcipuè phrygionicâ
" pingere, denticulatas affabre fimbrias conficere, et
" cætera quæ ſunt ejuſdem generis, edoctæ fuerunt.
" Vidi ego ex illarum operibus aliqua, quæ magiſ-
" tras hâc in arte feciſſe non pudeat. Receptæ autem
" ſunt ſacrum hoc in collegium anno ſecul IX. die XII.
" kalendas Aprilis, ibi XI manſionis, vitæ XIX jam
" planè complerunt. Iſtuc divarum contubernium,
" quod adhuc conſtanter incolunt, nunquam poſtea
" deſeruere. Addita illis è prudentioribus virago,
" quæ indeſinentur adſit, quo velint, ducat, actioni-
" bus invigilet, de quibus reſpondere, ad alioſque
" referre, ſi neceſſum fuerit, aliquando poſſit. Ex
" hâc ſcire quæ cupiebam, remotis arbitris, nullo
" negotio percepi. Crediderat namque, quod res
" quoque erat, non curioſitatis gratiâ, ſed officii, ac
" boni publici cauſâ iſta à me rogari: quare alios
" omnes ſecedere juſſi, ſolus cum eadem remanens,
" ut quæ, junioribus præſertim aliquot præſentibus,
" accuratius explicare verecundia illam antea pro-
" hibuerat, majori mecum libertate communicaret.
" Partes, quas vel nominare pudor honeſtaſque ve-
" tuit, per quas potus ciborumque fæces et reliqua
" corporis excrementa (ſit verbo venia) ejicimus, non
" illis his, quibus nobis, conſtitutæ locis. Illis qui-
" dem, ubi nos eas habemus, occluſa ſunt omnia;
" at infernè, quâ parte concretio illa corporum in-
" cipit, eaſdem obtinent utrique communes. Neque
" tamen cum neceſſitas alterutram premit ad exone-
" randum, exempli gratiâ, ventrem, altera ſe quoque
" ſentit tam inutili pondere gravatam, ut ſatisfacien-
" dum neceſſario naturæ ſit: ſed jam huic, jam iſti
" iſtud imbecillitatis humanæ incommodum perfe-
" rendum

" rendum eſt, fitque etiam, ut cum alvum purgat
" altera, alteri meatus ſit urinarius aperiendus. Mu-
" liebria, quæ ſtatis fœminas vicibus incommodant,
" non uno ambabus tempore veniunt. Octidui
" quandoque intervallo ac longiori disjuncta ſunt.
" Dum dormit hæc, ſæpe vigilat illa, et in alterius
" labore altera nonnunquam quieſcit. Viſa una po-
" tare eſt, aut cibo corpus reficere, cum aliud alii
" agebatur. Sedent, ſtant, ambulant, jacent ſemper
" unà, nec ſine incommodo. Non permittit con-
" glutinatio iſta corporum, hæ uti actiones ſeparen-
" tur. Si colloquuntur, obtortis faciem collis ob-
" vertunt. Suavia dant ſibi, cum amant, et pugnis
" impetunt, cum furunt. Donec ſuæ utrique vires
" adhuc conſtabant, ſi forte exortæ inter illas ali-
" quando diſcordiæ eſſent, hæc, quæ ſe læſam magis
" credebat aut fortem, ſublatam in humeros aliam
" alio aſportabat. Veruntamen ingenio miti magis ac
" placido ſunt quam incenſo aut iracundo, et in com-
" munibus malis communem fidem, commune ro-
" bur adhibent, immiſſam ſibi à Deo miſeriam for-
" titèr ſuſtinentes. Ante triennium in gravi ſecundò
" genitæ morbo, de quo nonnulla ſuperiùs facta eſt
" mentio eſt, prior nata ſacris omnibus munita ad
" mortem quoque feliciter obeundam diſpoſita ab
" ſacerdote fuit, quia medicorum pars potior credit
" aliâ extinctâ aliam haud poſſe longùm amplius
" ſupereſſe. Id quod probare ex hoc etiam laborant,
" quod quoties male uni ſit, quamvis altera non
" eadem continuo ægrotatione teneatur, anguſtias
" tamen animi certas, hebetationem ſenſuum, et
" commotionem quandam viſcerum in ſeipſa ex-
" periatur. Equidem dubitandum minimè reor, quin

" monſtroſa hæc bina corpora duplici mente ac ſpi-
" ritu regantur. Nam ſive cor faciamus, ſive cere-
" brum ſtatuamus animi ſedem, ex utrolibet idem
" nullo negotio evincitur. Adde tot actiones multi-
" plices, cogitationes rerum diverſas, ſenſa animi varia,
" quæ, ut aliud nihil ſit, iſthuc pariter nos docent.
" Unum præcipuè hic admirandum venit, quod
" commemorare ſuperiùs memoriâ excidit; poſt
" prodigioſum videlicet hunc difficilemque partum
" natos eſſe matri alios liberos, ex eodem patre pro-
" creatos, ſanos et valentes, corpore, ſpecie ac forma
" integros, qui monſtri nihil admixtum habeant."

XL. *Obſervations on the Origin and Uſe of the Lymphatic Veſſels of Animals: being an Extract from the* Gulſtonian *Lectures, read in the Theatre of the College of Phyſicians of* London, *in* June 1755. *By* Mark Akenſide, *M.D. Fellow of the College of Phyſicians, and of the Royal Society.*

Read Nov. 10, 1757.

IT is proved, by a multitude of experiments, that the lymphatics communicate with the blood-veſſels. They may be diſtended by blowing air, or by injecting water or mercury, into an artery: and the lymph, which they carry, is frequently, in a morbid ſtate, found tinged with a mixture of the red globules or craſſamentum of the blood. Upon this foundation two

different

different theories have been raiſed, concerning the connection of the lymphatics with the arteries.

Of theſe, we ſhall firſt conſider that of the late famous profeſſor Boerhaave. He obſerved, that every artery of the body is greater, in its diameter, than any of its branches: and this obſervation being found true, as far as our eye and the microſcope can inform us, he inferred, by analogy, that it held good even thro' the moſt minute ſubdiviſions of the arterial ſyſtem. But, ſays he, proportionable to the diameter of the canal is the ſize of the particles moving thro' it: therefore, if an ultimate capillary artery, admitting only one red globule at once to paſs thro' it, ſend off lateral branches, theſe branches will be capable of receiving ſuch particles only as are ſmaller than a red globule. But the particles next in magnitude below the red globules are the yellow ſerous ones; and the lateral veſſel, thus receiving them, is a ſerous artery, and the trunk of a ſecond order of veſſels. In like manner, this trunk, being continued on thro' many leſſening branches, will at laſt grow ſo minute, as to admit only one ſerous globule: its lateral branches, therefore, will receive only ſuch particles as are ſmaller than the ſerous ones: but theſe are the particles of the lymph; and this lateral branch is a lymphatic artery, and the trunk of a third order of veſſels. Thus, in the red arteries are contained all the circulated fluids of the body; in the ſerous arteries, all except the red blood; in the lymphatics, all except the red blood and ſerum: and this ſubordination is, according to the ſame laws, continued down thro' fluids more ſubtile than the lymph, to the ſmalleſt veſſel, which is propagated from the aorta.

Such was Boerhaave's doctrine concerning the vascular ſyſtem of animal bodies; like many of his other notions, ingenious, plauſible, and recommending itſelf, at firſt ſight, by an appearance of geometrical and mechanical accuracy: but founded upon inſufficient data, and by no means to be reconciled to appearances.

For, in the firſt place, ſhould we admit his hypotheſis, it is certain, that the conical or converging form of the aorta, and the change of direction in its branches, muſt, in the diſtant blood-veſſels, occaſion a great reſiſtance to the moving blood, and a great diminution of its velocity. Suppoſe that this reſiſtance be, in any capillary red artery, to the reſiſtance in the trunk of the aorta, as any larger aſſignable number is to unit: the reſiſtance, then, in a capillary ſerous artery will, to that in the aorta, be as the ſquare of that number is to unit; in the capillary lymphatic, as the cube; and ſo in progreſſion: that is, the velocity of the fluids, in the remoter ſeries of veſſels, will be, phyſically, nothing. But we know, on the contrary, that ſome very remote ſeries of veſſels have their contents moved with a very conſiderable velocity; particularly the veſſels of the inſenſible perſpiration: and in anatomical injections, the liquor thrown into an artery ſcarce returns more eaſily or ſpeedily by the correſponding vein, than by the moſt ſubtile excretory ducts. Moreover, there are an infinite number of obſervations of morbid caſes, in which the red blood itſelf has been evacuated thro' ſome of the moſt remote ſeries of veſſels, merely from an occaſional temporary obſtruction in one part, or a præternatural laxity in another; and without

any

any lasting detriment to the structure and subordination of the vessels; which yet, upon this hypothesis, must have been utterly destroyed before such an irregularity could have happened.

The other theory concerning the origin of the lymphatics has been maintained by some very eminent physiologists later than Boerhaave; and supposes, that these vessels receive their lymph from the blood-vessels, or from the excretories of the larger glands, by the intermediation of only one small vessel, which these authors term a lymphatic artery, invisible in its natural state, nor yet rendered subject to the senses by experiments. But to this it may be answered, that the lymphatics are traced into many parts of the body, and lost there; and therefore most probably have their origin there, where no large gland nor blood-vessel is to be found in their neighbourhood: that it contradicts the whole analogy of nature, to suppose the motion of an animal fluid more discernible in the veins than in the arteries: and, finally, that it seems rather an instance of want of thought, and of being imposed upon by words, to call the lymphatic vessels veins, because they are furnished with valves; and then, because they are called veins, to take for granted, that of course they must be the continuation of arteries.

In attempting to investigate matters too subtile for the cognizance of our senses, the only method, in which we can reasonably proceed, is by inferring from what we know in subjects of the same nature: and our conclusion thus inferred, concerning the subject sought, will be firmer and more unquestionable, in proportion as it resembles the subject known. But

if

if the ſubjects be really of the ſame kind; if no difference can be ſhewn between them, in any reſpect material to the inquiry, in which we are engaged; in this caſe our inference from analogy becomes the very next thing to a phyſical certainty: and this I apprehend to be true in relation to the problem before us, concerning the origin of the lymphatic veſſels. Tho' in general we cannot, by experiments, arrive at the extremities of thoſe tubes, nor ſatisfy ourſelves, by inſpection, in what manner they receive their fluid; yet in a very conſiderable number of them we can do both. There is a certain part of the human body very abundantly provided with lymphatics; in which part we can actually force injections thro' thoſe veſſels into a cavity, where their extremities open: and from this cavity, on the other hand, we can at pleaſure introduce a coloured liquor into their extremities, and trace it from ſmaller into wider canals; from capillary tubes, without valves, into large lymphatic trunks copiouſly furniſhed with them. We know likewiſe, that into this cavity are continually exhaling an infinite number of watery and mucous veſſels, both arterial tubes and excretory ducts: that theſe keep it moiſt with a perpetual vapour, which the extremities of thoſe lymphatics are, in the mean time, perpetually imbibing. Does it not ſeem ſtrange, while theſe particulars are known and acknowleged by all the world, that the great authors of anatomy and phyſiology ſhould never have reaſoned from them; but ſhould run into complex and obſcure ſuppoſitions, in order to explain a proceſs, which they may at any time examine with their own eyes? But perhaps this inadvertency may be accounted

accounted for, if we recollect, that at the time when these veſſels, and the ſtructure of this part, were diſcovered, the lymph, and every thing belonging to it, was utterly unknown; and that the veſſels in queſtion were firſt ſeen and conſidered as performing another and more remarkable office: which circumſtance, it ſhould ſeem, has prevented ſucceeding authors from being duly attentive to them in the capacity of lymphatics. However this be, it is certain, that the lymphatics of the meſentery, commonly called the lacteals, differ from thoſe of the other parts in no one particular, ſave that occaſionally they carry chyle inſtead of lymph; or rather carry lymph mixed, at ſtated times (that is, for two or three hours after the creature has taken food) with an emulſion of vegetable and animal ſubſtances, and coloured white by that mixture. At other times, (that is, during ſixteen or eighteen hours out of the twenty-four) they contain nothing but lymph; and are, in every reſpect, mere lymphatic veſſels, not to be diſtinguiſhed from thoſe in any other part of the body. Their ſtructure is the ſame; the membrane of which they are formed, their valves, the lymph which they contain, the glands thro' which they paſs, their direction from ſmaller tubes to larger, and from theſe to the blood, differ in nothing from what we obſerve of the other lymphatics. Their lymph, in the mean time, is without doubt or controverſy ſupplied from the cavity of the inteſtines; being the watery moiſture continually exhaled there for the purpoſes of digeſtion, and for the preſervation of the alimentary canal, and as continually taken up by the roots or extremities of theſe veſſels, in

order

order to be carried back to the blood, after it has performed its office in the bowels. Let it alſo be remembered, that theſe veſſels, in other places of the body, are generally, when we trace them, loſt in muſcular, tendinous, or membranous parts: and then, I ſhould preſume, it may fairly, and with a good degree of evidence, be concluded, that the lymphatics of the body, in general, have their origin among the little cavities of the cellular ſubſtance of the muſcles, among the mucous folliculi of the tendons, or the membranous receptacles and ducts of the larger glands: that their extremities or roots do, from theſe cavities, imbibe the moiſture exhaled there from the ultimate arterial tubes, juſt as the lacteals (the lymphatics of the meſentery) do on the concave ſurface of the inteſtines: and that the minute imbibing veſſels, by gradually opening one into another, form at length a lymphatic trunk, furniſhed with valves to prevent the return of its fluid, and tending uniformly, from the extremities and from the viſcera, to reconvey to the blood that lymph, or that fine ſtream, with which they are kept in perpetual moiſture; a circumſtance indiſpenſibly neceſſary to life and motion: while, at the ſame time, the continual re-abſorption of that moiſture by the lymphatics is no leſs neceſſary, in order to preſerve the blood properly fluid, and to prevent the putrefaction, which would inevitably follow, if this animal vapour were ſuffered to ſtagnate in the cavities where it is diſcharged.

XLI.

XLI. *A Letter to the Right Honourable the Earl of* Macclesfield, *President, the* Council, *and* Fellows, *of the* Royal Society, *concerning the Variation of the Magnetic Needle; with a Sett of Tables annexed, which exhibit the Result of upwards of Fifty Thousand Observations, in Six periodic Reviews, from the Year* 1700 *to the Year* 1756, *both inclusive; and are adapted to every Five Degrees of Latitude and Longitude in the more frequented Oceans. By* William Mountaine *and* James Dodson, *Fellows of the Royal Society.*

Dated London, Nov. 9th. 1757.

SIRS,

Read Nov. 10. 1757.

ON the 20th of March 1755, we presented an address to this illustrious Body, intituled, "An Attempt to point out, "in a concise manner, the Advantages which would "accrue from a periodic Review of the Variation of "the Magnetic Needle, throughout the known World; "requesting contributions thereto, by communicat-"ing such observations concerning it, as had then "been lately made, or could be procured from cor-"respondents in foreign parts."

This address was read at the same time, and afterwards honoured with a place in the Transactions, vol. xlviii. part ii. for 1754: which favour we now acknowlege in the most grateful manner; and, pur-

ſuant to our engagements, beg leave to lay before you ſome account of the communications received, with a ſpecimen of the uſes and applications which we have been enabled to make of thoſe, and other aſſiſtances with which we have been indulged.

On application to the Honourable the Commiſſioners of the Navy, we were obliged with an order of free acceſs to all their maſters log-books and journals.

The Directors of the Honourable Eaſt India Company granted the like privilege.

The Honourable Committee of the Hudſon's Bay Company obliged us with ſundry obſervations, made, and tabulated, by their own Captains.

James Bradley, D. D. Regius Profeſſor of Aſtronomy, and F. R. S. favoured us with ſeveral obſervations made at the Royal Obſervatory at Greenwich.

John Hyde, Eſq; F. R. S. communicated a ſett of uſeful obſervations, extracted from two journals kept on board the Triton and Britannia Eaſt Indiamen.

A correct journal kept on board the Delawar Eaſt Indiaman was handed to us by a gentleman unknown.

Capt. George Snow furniſhed a conſiderable number of obſervations, made with care and accuracy by himſelf, in ſeveral ſucceſſive voyages to, and from Barbadoes and Virginia; together with ſeveral remarks upon the ſubject: *One*, which we apprehend to be material, we beg leave to inſert, as it meets with ſome confirmation by the tables annexed; *viz.* "At Barbadoes the variation ſeems at a ſtand very "near; for in the road, 1752, I obſerved 5 degrees "eaſt; and by Mr. Halley's draught, in the year "1701, 5 ½ degrees: in 1747, at Port Royal keys, "Jamaica, I obſerved the variation 7° 20′ E.; and "on the coaſt of Carthagena the ſame week, off

"the

" the high land of Sancta Martha, 7° 45′ E. nearly
" ſouth of Port Royal: Therefore theſe curves are
" not much altered; and the curve at Jamaica is
" nearly at a ſtand, as tho' tied; and the ſouth part
" of them, with the reſt, dropping to the weſtward."

Mr. Mungo Murray, author of a treatiſe on ſhip-building, preſented us with ſeveral obſervations taken on board the Prince Edward and Cheſterfield Eaſt Indiamen, and his Majeſty's ſhip the Neptune.

For all theſe favours we return our ſincere thanks.

No obſervations made upon land have been received, except Dr. Bradley's aforeſaid; which has fruſtrated our intentions of continuing the curves from ſea to ſea.

By collecting, comparing, and adjuſting, all theſe materials, we have been enabled to conſtruct variation-curves upon Dr. Halley's mercator-chart, adapted to the year 1756; which will ſoon be in readineſs to preſent to this Royal Society.

As a work of this kind requires much time, and a multitude of obſervations, both by ſea and land, to render it more perfect and general; we hope the ingenious in all nations will lend their aſſiſtance: By this means every periodic review will be productive of improvement.

From the firſt inſtant that we made this affair the object of our more particular conſideration, we have attended to the mode of increaſe and decreaſe in the variation: and as a conſiderable number of obſervations, made at periodic times, and duly regiſtred, ſeem to be the moſt eſſential toward determining the laws of its mutation, or proving its irregularity, we have therefore formed a ſett of tables, from actual obſervations collected for the years 1710, 1720, 1730, and 1744, the date of our laſt chart; which, together

with Dr. Halley's for the year 1700, and the preſent chart now publiſhing, compleat ſix reviews: Theſe are tabulated, and ſhew the quantity of the variation, at thoſe ſeveral periods, to every 5 degrees of latitude and longitude in the more frequented oceans; which we hope will prove acceptable, as nothing of the like kind has yet appeared, or can eaſily be obtained.

Our materials have been ſo deficient, that even in the limits to which our tables are confined, we have been obliged to leave blanks in ſome of the above periods, for want of that concurrent teſtimony, on which the numbers inſerted are founded: but, conſidering the difficulties unavoidably attending a work of this ſort, and the little aſſiſtance which we have met with from private hands, we hope that this Royal Society will not only excuſe thoſe vacancies, but alſo thoſe in the great tracts of ſea, as well as land, concerning which we are very unwillingly obliged to be intirely ſilent.

Agreeable to our former addreſs, we lay only what appear to be facts before you, without attempting to introduce any hypotheſis for the ſolution of theſe phænomena; ſome of which (being very extraordinary) we recommend peculiarly to the notice of thoſe gentlemen, who may endeavour the inveſtigation of their cauſes.

Under the equator, in longitude 40° E. from London, the higheſt variation during the whole 56 years appears to be 17° $\frac{1}{4}$ W. and the leaſt 16° $\frac{1}{2}$ W.: and in latitude 15° N. longitude 60° W. from London, the variation has been conſtantly 5° E. but in other places the caſe has been widely different; for in the latitude 10° S. longitude 60° E. from London, the variation has decreaſed from 17° W. to 7 $\frac{1}{4}$ W.; and in latitude 10° S. longitude 5° W. from London, it has

has increaſed from $2^\circ \frac{1}{4}$ W. to $12^\circ \frac{3}{4}$ W.; and in latitude 15° N. longitude 20° W. it has increaſed from 1° W. to 9° W.

But there is ſtill a more extraordinary appearance in the Indian ſeas: for inſtance, under the equator.

Longitude from *London*	Variation in 1700.	Variation in 1756.	Longitude from *London*	Variation in 1700.	Variation in 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
40 E	$16\frac{3}{4}$ W	$16\frac{3}{4}$ W	75 E	$9\frac{3}{4}$ W	1 W
45 E	$17\frac{3}{4}$ W	$14\frac{1}{2}$ W	80 E	$7\frac{3}{4}$ W	$0\frac{1}{4}$ E
50 E	$17\frac{1}{2}$ W	$11\frac{3}{4}$ W	85 E	$5\frac{1}{2}$ W	$1\frac{1}{4}$ E
55 E	$16\frac{1}{2}$ W	$8\frac{3}{4}$ W	90 E	$4\frac{1}{4}$ W	1 E
60 E	$15\frac{1}{4}$ W	6 W	95 E	$3\frac{1}{4}$ W	$0\frac{1}{2}$ W
65 E	$13\frac{1}{2}$ W	$4\frac{1}{2}$ W	100 E	$2\frac{1}{2}$ W	1 W
70 E	$11\frac{1}{2}$ W	$3\frac{1}{4}$ W			

Where the weſt variation in the longitude 40° E. is the ſame in both the above years; and in 1700 the weſt variation ſeemed to be regularly decreaſing from longitude 50° E. to the longitude 100° E.; but in 1756 we find the weſt variation decreaſing ſo faſt, that we have eaſt variation in the longitude 80°, 85°, and 90° E; and yet, in the longitude 95° and 100° E. we have weſt variation again.

Such are the irregularities, that experience hath ſhewn us, in the variation of the magnetic needle; which appear ſo conſiderable, that we cannot think it wholly under the direction of one general and uniform law; but rather conclude, with the learned and judicious Dr. Gowen Knight, Fellow of this Society, in the 87th prop. of his treatiſe upon attraction and repulſion, That it is influenced by various and different magnetic

magnetic attractions, in all probability occaſioned by the heterogeneous compoſitions in the great magnet, the *Earth*.

Notwithſtanding all which, ſhould the ſagacity of ſome eminent philoſopher be able to exhibit rules, whereby the quantity of the variation may be computed for future times, yet then ſuch a review, as we have now made, will be neceſſary at a proper interval, to prove the truth of them: and ſhould no ſuch rules appear, then will a continued ſucceſſion of ſuch reviews be neceſſary ſo long as commerce and navigation ſubſiſt among us.

What we have now done is intirely for the public ſervice, the ſale of the former chart never having made good its expence; and we propoſe to continue our endeavours for another review, at the proper time, if we ſhall then be alive, and capable of the taſk: but as the contrary may probably happen, we beg leave to conclude with recommending ſuch a continuation, in the ſtrongeſt manner, to ſuch of the members of this Royal Society, or others, who may, at the proper intervals, have leiſure and ability for ſuch a performance.

We are, with the greateſt deference,

Your Lordſhip's,

And the Royal Society's,

Moſt faithful and

moſt obedient Servants,

William Mountaine.
James Dodſon.

A

A TABLE, *exhibiting the different Variations of the* MAGNETIC-NEEDLE *in the more frequented Oceans, from the Year* 1700 *to the Year* 1756.

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*
0	0	4½ W			10¼ W	14¼ W	15¼ W
0	5 W	2¾ W			8¾ W	12 W	13¼ W
0	10 W	1½ W	3½ W	5½ W	7 W	10 W	11 W
0	15 W	0½ W	2 W	3½ W	5 W	7½ W	9 W
0	20 W	0¾ E	0½ W	1¾ W	3 W	5¼ W	6½ W
0	25 W	1½ E	0½ E	0½ W	1½ W	3 W	4 W
0	30 W	2½ E	2 E	1½ E	1 E	0¾ W	0½ W
0	35 W	3¼ E	3 E	2¾ E	2½ E	1½ E	1½ E
0	40 W	4¼ E	4¼ E	4 E	3¾ E	3½ E	3½ E
0	45 W	5½ E	5½ E	5¼ E	5 E	4¾ E	5 E
0	50 W	6¾ E			5¾ E	6 E	6½ E
0	5 E	6 W			12½ W	15¾ W	16½ W
0	10 E	7¾ W			14¼ W	17 W	17½ W
0	40 E	16¾ W	17 W	17¼ W	17 W	16½ W	16¾ W
0	45 E	17¾ W	17¼ W	16¾ W	16¼ W	15¾ W	14½ W
0	50 E	17½ W	16¾ W	16 W	15 W	14 W	11¾ W
0	55 E	16½ W	15¼ W	14 W	13 W	11½ W	8¾ W
0	60 E	15¼ W	13¾ W	12½ W	11 W	9 W	6 W
0	65 E	13½ W	11¾ W	10 W	8¼ W	6½ W	4½ W
0	70 E	11½ W	9¾ W	7¾ W	6 W	4 W	3¼ W
0	75 E	9¾ W	7¾ W	5¾ W	4 W	1¾ W	1 W
0	80 E	7¾ W	6 W	4¼ W	3 W	0¼ W	0¼ E
0	85 E	5½ W	4 W	2½ W	1½ W	0¼ E	1¼ E

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
0	90 E	4¼ W	3½ W	1½ W	0½ W	1¼ W	1 E
0	95 E	3¼ W	2¼ W			2 W	0½ W
0	100 E	2½ W	1 W			2½ W	1 W
5 N	0	4¾ W			10½ W	15 W	15¼ W
5 N	5 W	3¼ W			9 W	13 W	13 W
5 N	10 W	1¾ W			7½ W	10¾ W	11¼ W
5 N	15 W	0¾ W	2¼ W	3¾ W	5½ W	8½ W	9¼ W
5 N	20 W	0	1½ W	2½ W	3½ W	6 W	7¼ W
5 N	25 W	1 E	0	1 W	2 W	3¾ W	4½ W
5 N	30 W	1¾ E	1¼ E	0½ E	0¼ W	1½ W	1½ W
5 N	35 W	2½ E	2¼ E	1¾ E	1½ E	0½ E	0½ E
5 N	40 W	3½ E	3¼ E	3 E	2½ E	2¼ E	2¼ E
5 N	45 W	4½ E	4¼ E	4¼ E	4 E	3¾ E	4 E
5 N	50 W	5½ E	5½ E	5¼ E	5¼ E	5 E	5¼ E
5 N	55 W	6¾ E				6¼ E	6½ E
5 N	5 E	6¼ W			12¾ W	16¼ W	16½ W
5 N	10 E	7¾ W			14¼ W	17½ W	17½ W
5 N	45 E	16¾ W	16 W	15¼ W	14¾ W	14 W	13¾ W
5 N	50 E	16¾ W	15¾ W	14¾ W	13½ W	12¼ W	11 W
5 N	55 E	15½ W	14¼ W	13 W	12 W	10 W	8¼ W
5 N	60 E	14¼ W	12¾ W	11¼ W	10 W	8½ W	6 W
5 N	65 E	12¾ W	11 W	9½ W	8 W	6 W	4½ W
5 N	70 E	10¾ W	9 W	7¼ W	5½ W	3¾ W	2¾ W
5 N	75 E	8¾ W	7 W	5¼ W	3¾ W	1¾ W	0¾ W
5 N	80 E	6½ W	5 W	3½ W	2½ W	0¼ W	0¼ E
5 N	85 E	4¾ W	3¾ W	2¾ W	1½ W	0¼ W	1¼ E
5 N	90 E	3¾ W				1¼ W	0½ E
5 N	95 E	2¾ W				2¼ W	0½ W

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
10 N	15 W	1¼ W		4½ W	6 W	9¼ W	10 W
10 N	20 W	0½ W	2 W	3¼ W	4½ W	7 W	8 W
10 N	25 W	0¼ E	0¾ W	1¾ W	2¾ W	4½ W	5½ W
10 N	30 W	1 E	0½ E	0	1 W	2¼ W	3 W
10 N	35 W	1¾ E	1¼ E	0¾ E	0¼ E	0½ W	1 W
10 N	40 W	2½ E	2¼ E	2 E	1¾ E	1¼ E	1 E
10 N	45 W	3½ E	3½ E	3¼ E	3 E	2¾ E	2½ E
10 N	50 W	4½ E	4½ E	4¼ E	4¼ E	4 E	4 E
10 N	55 W	5½ E	5½ E	5½ E	5¼ E	5¼ E	5½ E
10 N	60 W	6½ E				6½ E	6¾ E
10 N	50 E	16 W	15 W	14 W	12¾ W	11¼ W	10½ W
10 N	55 E	15 W	13½ W	12 W	11 W	9¼ W	8 W
10 N	60 E	13½ W	12 W	10½ W	9¼ W	7¾ W	6 W
10 N	65 E	12 W	10¾ W	9 W	7½ W	6 W	4½ W
10 N	70 E	10 W	8¾ W	6½ W	5 W	3¾ W	3 W
10 N	75 E	8 W	6½ W	5 W	3½ W	1¾ W	1 E
10 N	80 E	5¾ W	4½ W	3¼ W	2 W	0½ W	0¼ E
10 N	85 E	4½ W	3½ W	2¼ W	1¼ W	0	1 E
10 N	90 E	3½ W				1½ W	0½ E
10 N	95 E	2½ W				2¼ W	0½ W
15 N	20 W	1 W	2½ W	4 W	5½ W	7 W	9 W
15 N	25 W	0½ W	1½ W	2½ W	3½ W	4¾ W	6½ W
15 N	30 W	0¼ E	0¼ W	1 W	1¾ W	2¾ W	4½ W
15 N	35 W	1 E	0½ E	0	0½ W	1¼ W	3½ W
15 N	40 W	1½ E	1¼ E	1 E	0½ E	0	0½ W
15 N	45 W	2¼ E	2¼ E	2 E	1¾ E	1½ E	1 E
15 N	50 W	3¼ E	3¼ E	3¼ E	3 E	2¾ E	2½ E
15 N	55 W	4 E	4 E	4 E	4 E	4 E	3¾ E

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*
15 N	60 W	5 E	5 E	5 E	5 E	5 E	5 E
15 N	65 W	6 E			5 E	6 E	6 E
15 N	70 W	7 E			5½ E	6¾ E	7 E
15 N	75 W	7¾ E				7 E	7¾ E
15 N	80 W	8¼ E				7 E	8 E
15 N	50 E	15½ W	14¼ W	13 W	11¾ W	10½ W	9¾ W
15 N	55 E	14¼ W	12¾ W	11½ W	10¾ W	8¾ W	7¾ W
15 N	60 E	13 W	11¾ W	10¼ W	9 W	7½ W	6 W
15 N	65 E	11½ W	10 W	8¾ W	7½ W	6 W	4½ W
15 N	70 E	9¾ W	8¼ W	6¾ W	5½ W	4 W	2¾ W
15 N	75 E	8 W	6½ W	5 W	3½ W	2¼ W	0¾ W
15 N	80 E	5¾ W	4¼ W	3½ W	2½ W	1¼ W	0
15 N	85 E	4¼ W	3¼ W	2¼ W	1 W	0¼ W	0¾ E
15 N	90 E	3¼ W	2½ W	1¼ W		0¾ W	0½ E
15 N	95 E	2½ W	1½ W			2¼ W	0½ W
20 N	20 W	1½ W	3 W	4¼ W	5¼ W	7 W	10 W
20 N	25 W	1 W	2 W	2¾ W	3½ W	4¾ W	8 W
20 N	30 W	0½ W	1 W	1½ W	2¼ W	3¼ W	5¾ W
20 N	35 W	0¼ E	0¼ W	0¾ W	1½ W	2¼ W	4 W
20 N	40 W	0¾ E	¼ E	0	0½ W	1 W	2½ W
20 N	45 W	1½ E	1¼ E	1 E	¾ E	0¼ E	0¾ W
20 N	50 W	2 E	2 E	1¾ E	1½ E	1¼ E	0¾ E
20 N	55 W	2¾ E	2¾ E	2¾ E	2½ E	2½ E	2 E
20 N	60 W	3¾ E	3¾ E	3¾ E	3½ E	3½ E	3¼ E
20 N	65 W	4¾ E				4¼ E	4 E
20 N	70 W	5½ E				5 E	5 E
20 N	75 W	6¼ E				5¼ E	5¼ E
20 N	80 W	7 E				5¼ E	6 E

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*
20 N	60 E	12¾ W	11¼ W	10 W	9 W	7½ W	6 W
20 N	65 E	11½ W	10 W	8½ W	7¼ W	6 W	4½ W
20 N	70 E	9¾ W	8½ W	7 W	5¾ W	4½ W	2¾ W
20 N	90 E	3½ W	2½ W	1½ W	1 W	0½ W	1 E
25 N	20 W	2 W	3¼ W	4 W	5¼ W	7 W	11 W
25 N	25 W	1¾ W	2¼ W	2¾ W	4 W	5¼ W	9½ W
25 N	30 W	1¼ W	1¾ W	2¼ W	3 W	4 W	7½ W
25 N	35 W	1 W	1½ W	2 W	2½ W	3 W	5½ W
25 N	40 W	0½ W	0¾ W	1 W	1½ W	2¼ W	3¾ W
25 N	45 W	0¼ E	0	0½ W	1 W	1½ W	2½ W
25 N	50 W	0¾ E	0½ E	0¼ E	0	0½ W	1 W
25 N	55 W	1¼ E	1 E	1 E	¾ E	0½ E	0
25 N	60 W	2 E	2 E	2 E	1½ E	1½ E	1¼ E
25 N	65 W	2¾ E				2¼ E	2¼ E
25 N	70 W	3½ E				3¼ E	2¾ E
25 N	75 W	4¼ E				3½ E	3 E
25 N	80 W	4¾ E				3½ E	3 E
25 N	60 E	12¾ W	11½ W	10¼ W	9 W	7½ W	6 W
25 N	65 E	11½ W	10¼ W	9 W	7½ W	6 W	4½ W
25 N	70 E	10 W	8¾ W	7½ W	6 W	4½ W	2¾ W
30 N	10 W	3½ W				11½ W	13¾ W
30 N	15 W	3¼ W				10¼ W	12¾ W
30 N	20 W	3 W	4¼ W	5½ W	6¾ W	8¼ W	12 W
30 N	25 W	2¾ W	3½ W	4¼ W	5½ W	6¾ W	10¾ W
30 N	30 W	2½ W	3 W	3½ W	4¼ W	5¼ W	9 W
30 N	35 W	2¼ W	2¾ W	3¼ W	3¾ W	4¼ W	7 W
30 N	40 W	1¾ W	2 W	2½ W	3 W	3½ W	5¼ W
30 N	45 W	1¼ W	1½ W	2 W	2¼ W	2¾ W	4 W

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*
30 N	50 W	0¾ W	1 W	1½ W	1¾ W	2¼ W	3 W
30 N	55 W	0¼ W	½ W	1 W	1¼ W	1½ W	2¼ W
30 N	60 W	0¼ E	0	¼ W	0½ W	1 W	1½ W
30 N	65 W	0¾ E	½ E	¼ E	0	¼ W	1 W
30 N	70 W	1½ E	1¼ E	1 E	¾ E	¼ E	½ W
30 N	75 W	2 E	1½ E	1¼ E	1 E	½ E	0
30 N	80 W	2¼ E				¾ E	0
35 N	10 W	4¼ W			9¾ W	12¼ W	14¼ W
35 N	15 W	4 W			9¼ W	11½ W	13¾ W
35 N	20 W	4 W			8¼ W	10¼ W	13 W
35 N	25 W	3¾ W	4¾ W	6 W	7½ W	9 W	12¼ W
35 N	30 W	3¾ W	4½ W	5½ W	6¾ W	8 W	10½ W
35 N	35 W	3¾ W	4¼ W	5¼ W	6 W	7 W	8¾ W
35 N	40 W	3¾ W	4 W	4½ W	5¼ W	6¼ W	7¼ W
35 N	45 W	3½ W	3¾ W	4¼ W	4¾ W	5½ W	6¼ W
35 N	50 W	3½ W	3¾ W	4 W	4¼ W	5 W	5½ W
35 N	55 W	3½ W	3¾ W	4 W	4¼ W	4¾ W	5 W
35 N	60 W	3¼ W	3¾ W	4 W	4¼ W	5 W	5¼ W
35 N	65 W	3 W				5¼ W	6 W
35 N	70 W	2½ W				5¾ W	6¾ W
35 N	75 W	2¼ W				6½ W	7 W
40 N	10 W	5 W			10¾ W	13¼ W	15 W
40 N	15 W	5¼ W			10½ W	12¾ W	14½ W
40 N	20 W	5¼ W			10 W	12¼ W	14¼ W
40 N	25 W	5½ W			9½ W	11¼ W	13½ W
40 N	30 W	5½ W	6¾ W	8 W	9 W	10¼ W	12¾ W
40 N	35 W	5¾ W	6½ W	7¼ W	8¼ W	9½ W	11¼ W
40 N	40 W	5¾ W	6¼ W	7¼ W	8 W	9 W	10 W

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
40 N	45 W	6 W			7½ W	8½ W	9½ W
40 N	50 W	6¼ W			7¼ W	8½ W	9½ W
40 N	55 W	6½ W			7¾ W	8¾ W	10 W
40 N	60 W	6¾ W			8 W	9¼ W	11 W
40 N	65 W	7 W			8½ W	10¼ W	12 W
40 N	70 W	7 W			9 W	11½ W	12¾ W
45 N	5 W	6 W			12½ W	15½ W	16½ W
45 N	10 W	6½ W			12¼ W	15 W	16½ W
45 N	15 W	6¾ W			11¾ W	14¾ W	16¼ W
45 N	20 W	7 W			11½ W	14½ W	16 W
45 N	25 W	7½ W			11½ W	14 W	16 W
45 N	30 W	8 W			11¾ W	13¾ W	15¾ W
45 N	35 W	8¾ W			11¾ W	13¼ W	15¼ W
45 N	40 W	9½ W			12 W	13 W	15 W
45 N	45 W	10½ W			12¾ W	13¼ W	15½ W
45 N	50 W	11½ W			13½ W	14 W	16 W
45 N	55 W	12½ W			14 W	15¼ W	17 W
45 N	60 W	13¾ W			14 W	16 W	18½ W
50 N	5 W	7½ W				17 W	19¼ W
50 N	10 W	7¾ W				17¼ W	19½ W
50 N	15 W	8½ W				17½ W	20 W
50 N	20 W	9 W				17½ W	20½ W
50 N	25 W	9¾ W				17¾ W	21 W
5 S	0	4¼ W			10 W	13¼ W	15 W
5 S	5 W	2½ W			8½ W	11¼ W	13 W
5 S	10 W	1 W	3 W	4¾ W	6½ W	9¼	10¾ W
5 S	15 W	0	1½ W	3 W	4½ W	6¾ W	8½ W
5 S	20 W	1¼ E	0¼ E	0¾ W	2¼ W	4 W	6 W

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
5 S	25 W	2¼ E	1¼ E	0¼ E	0¾ W	2 W	3 W
5 S	30 W	3¼ E	2¼ E	1¾ E	1 E	0¼ E	0
5 S	35 W	4¼ E	4 E	3½ E	3¼ E	2¾ E	2¾ E
5 S	5 E	6 W			12½ W	15 W	16 W
5 S	10 E	7½ W			14¼ W	16½ W	17 W
5 S	40 E	18 W	17¾ W	17¾ W	17½ W	17½ W	17 W
5 S	45 E	18½ W	18¼ W	18 W	17½ W	17 W	16 W
5 S	50 E	18½ W	17¾ W	17 W	16¼ W	15½ W	12¾ W
5 S	55 E	17½ W	16½ W	15½ W	14¾ W	13 W	9¼ W
5 S	60 E	16¼ W	14¾ W	13¼ W	12 W	10 W	6½ W
5 S	65 E	14¾ W	12¾ W	10¾ W	9 W	7 W	4½ W
5 S	70 E	13 W	11 W	9 W	6¾ W	4½ W	3 W
5 S	75 E	11 W	9 W	7 W	5 W	2¼ W	1 W
5 S	80 E	9 W	7 W	5 W	3 W	0¾ W	0
5 S	85 E	7 W	5½ W	3¾ W	2½ W	0½ W	0¾ E
5 S	90 E	5 W	4½ W	2¾ W	2 W	1¼ W	0¾ E
5 S	95 E	3¾ W	3½ W	1½ W	1½ W	2 W	0½ W
5 S	100 E	3 W	2½ W	1½ W	1 W	2¾ W	1½ W
10 S	0	3¾ W			9½ W	12½ W	14¼ W
10 S	5 W	2¼ W	4¼ W	6¼ W	8¼ W	10½ W	12¾ W
10 S	10 W	0¾ W	2¾ W	4¾ W	6½ W	8¼ W	10¼ W
10 S	15 W	½ E	1 W	2½ W	4 W	5¾ W	7¾ W
10 S	20 W	1¾ E	0½ E	0	1½ W	3 W	4¾ W
10 S	25 W	3 E	2½ E	1¾ E	0¾ E	0¾ W	2 W
10 S	30 W	4 E	3½ E	3 E	2½ E	1¾ E	1 E
10 S	35 W	5¼ E	5 E	4¾ E	4½ W	4 E	3½ E
10 S	5 E	5¾ W			12 W	14½ W	15¾ W
10 S	10 E	7½ W			14 W	16 W	16¾ W

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
10 S	15 E	9¼ W			15½ W	17¼ W	17¾ W
10 S	40 E	18¾ W	18¾ W	18¾ W	18¾ W	19 W	19¼ W
10 S	45 E	19½ W	19¼ W	19 W	18¾ W	18½ W	18 W
10 S	50 E	19½ W	19 W	18½ W	17¾ W	16¾ W	14¼ W
10 S	55 E	18½ W	17½ W	16½ W	15½ W	14½ W	10½ W
10 S	60 E	17 W	16 W	15 W	14 W	11¼ W	7¼ W
10 S	65 E	15¾ W	13¾ W	11¾ W	10 W	8 W	5 W
10 S	70 E	14½ W	12 W	10 W	8 W	5½ W	3½ W
10 S	75 E	12½ W	10¼ W	9 W	5¾ W	3½ W	2 W
10 S	80 E	10½ W	8 W	5½ W	4 W	1¾ W	1 W
10 S	85 E	8½ W	6¾ W	5 W	3¾ W	1½ W	0¼ W
10 S	90 E	6¾ W	5½ W	4¼ W	3 W	1¾ W	0¼ W
10 S	95 E	5 W	4½ W	4 W	3¼ W	2½ W	1 W
10 S	100 E	3¾ W	3½ W	3¼ W	3 W	2¾ W	2 W
10 S	105 E	2¾ W	2¾ W	2½ W	2½ W	3¼ W	2¾ W
10 S	110 E	2 W				3¾ W	3¼ W
15 S	0	3½ W	5½ W	7½ W	9½ W	11¾ W	14 W
15 S	5 W	1¾ W	3¾ W	5¾ W	7¾ W	9½ W	12 W
15 S	10 W	¼ W	2 W	3¾ W	5½ W	7½ W	9½ W
15 S	15 W	1¼ E	0¼ W	1¾ W	3¼ W	4¾ W	7 W
15 S	20 W	2½ E	1½ E	0¾ E	0½ W	1¾ W	4 W
15 S	25 W	3¾ E	3 E	2¼ E	1½ E	0½ E	1 W
15 S	30 W	5 E	4½ E	4 E	3½ E	3 E	2 E
15 S	35 W	6½ E	6¼ E	5¾ E	5½ E	5 E	4½ E
15 S	40 W	7¾ E				6¾ E	6½ E
15 S	5 E	5½ W			11½ W	13¾ W	15¼ W
15 S	10 E	7½ W			14 W	15½ W	16½ W
15 S	40 E	19¾ W	19¾ W	20 W	20 W	20 W	20 W

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
15 S	45 E	20½ W	20½ W	20¼ W	20¼ W	20 W	19½ W
15 S	50 E	20½ W	20 W	19½ W	18¾ W	18 W	16¼ W
15 S	55 E	19½ W	18½ W	17½ W	16½ W	15½ W	12½ W
15 S	60 E	18¼ W	17 W	16 W	14½ W	12½ W	9 W
15 S	65 E	17 W	15½ W	13½ W	12 W	9¾ W	6 W
15 S	70 E	15½ W	12¾ W	10¾ W	9 W	7¼ W	4¼ W
15 S	75 E	14 W	12 W	9¾ W	7½ W	5 W	3¼ W
15 S	80 E	12 W	10 W	8 W	6 W	3½ W	2½ W
15 S	85 E	10 W	8 W	6¼ W	4¾ W	2¾ W	2¼ W
15 S	90 E	8½ W	7¼ W	6 W	4½ W	3 W	2¼ W
15 S	95 E	6½ W	5¾ W	5 W	4¼ W	3¼ W	2¾ W
15 S	100 E	5 W	4¾ W	4½ W	4¼ W	3¾ W	3½ W
15 S	105 E	3½ W	3½ W	3¾ W	3¾ W	4¼ W	3¾ W
15 S	110 E	2½ W				4½ W	
20 S	0	3¼ W	5¼ W	7¼ W	9 W	11 W	13½ W
20 S	5 W	1½ W	3¼ W	5 W	6¾ W	8¾ W	11¼ W
20 S	10 W	0½ E	1¼ W	3 W	4¾ W	6½ W	8¾ W
20 S	15 W	1¾ E	0½ E	0¾ W	2 W	3½ W	5¾ W
20 S	20 W	3 E	2¾ E	1½ E	0½ W	0½ W	3 W
20 S	25 W	4¾ E	4¼ E	3½ E	2¾ E	2 E	0
20 S	30 W	6 E	5½ E	5¼ E	4¾ E	4¼ E	2½ E
20 S	35 W	7¾ E	7½ E	7¼ E	6¾ E	6½ E	5 E
20 S	40 W	9¼ E				8 E	7½ E
20 S	5 E	5½ W			11 W	13¼ W	15 W
20 S	10 E	7½ W			13½ W	15 W	16¼ W
20 S	15 E	9½ W			15½ W	16½ W	17¾ W
20 S	35 E	19 W	19¼ W	19¾ W	20¼ W	20¾ W	22 W
20 S	40 E	20½ W	20¾ W	21¼ W	21½ W	21¾ W	22 W

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*	*Degrees.*
20 S	45 E	21¼ W	21¼ W	21½ W	21½ W	21¾ W	21¼ W
20 S	50 E	21¼ W	21 W	20¾ W	20¼ W	19¾ W	18¾ W
20 S	55 E	20½ W	20 W	19¼ W	18½ W	17 W	15 W
20 S	60 E	19½ W	18¼ W	17 W	5¾ W	14½ W	11¼ W
20 S	65 E	18¼ W	17 W	15¾ W	14¼ W	12 W	8 W
20 S	70 E	16¾ W	15½ W	13½ W	12 W	10 W	6 W
20 S	75 E	15 W	13 W	10½ W	9 W	7¾ W	4¾ W
20 S	80 E	13½ W	11¾ W	9¾ W	8 W	6 W	4½ W
20 S	85 E	11½ W	10 W	8½ W	7 W	5 W	4¼ W
20 S	90 E	10 W	8¾ W	7½ W	6¼ W	4¾ W	4½ W
20 S	95 E	8 W	7¼ W	6½ W	5¾ W	4¾ W	4½ W
20 S	100 E	6½ W	6¼ W	6 W	5½ W	5 W	4¾ W
20 S	105 E	4¾ W	5 W	5 W	5 W	5 W	4¾ W
25 S	0	3 W	5 W	7 W	8¾ W	10½ W	12½ W
25 S	5 W	1 W	2¾ W	4½ W	6¼ W	8 W	10 W
25 S	10 W	1 E	0½ W	2¼ W	4 W	5½ W	7½ W
25 S	15 W	2½ E	1¼ E	0	1¼ W	2½ W	4½ W
25 S	20 W	4 E	3¼ E	2 E	1¼ E	0½ E	2¾ W
25 S	25 W	6 E	5¼ E	4½ E	3¾ E	3 E	1 E
25 S	30 W	7½ E	7 E	6½ E	6 E	5½ E	3½ E
25 S	35 W	9¼ E				7½ E	6 E
25 S	40 W	11 E				9 E	
25 S	5 E	5¼ W	7 W	8¾ W	10½ W	12½ W	14½ W
25 S	10 E	7½ W				14½ W	16 W
25 S	15 E	9½ W				16¼ W	17¾ W
25 S	35 E	19½ W	20¼ W	20¾ W	21½ W	22¼ W	23½ W
25 S	40 E	21 W	21½ W	22 W	22½ W	23¼ W	23¾ W
25 S	45 E	22¼ W	22½ W	22¾ W	23 W	23¼ W	23 W
25 S	50 E	22½ W	22½ W	22¼ W	22¼ W	22 W	21 W

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
25 S	55 E	22 W	21½ W	21 W	20½ W	19¾ W	18 W
25 S	60 E	20¾ W	19¾ W	19 W	18¼ W	17 W	14½ W
25 S	65 E	19¼ W	18¼ W	17¼ W	16¼ W	15 W	11¾ W
25 S	70 E	17¾ W	16¾ W	15¼ W	14½ W	13 W	9½ W
25 S	75 E	16¼ W	15 W	13¾ W	12½ W	11¼ W	8 W
25 S	80 E	14¾ W	13½ W	12¼ W	10¾ W	9¼ W	7½ W
25 S	85 E	13 W	11¾ W	10½ W	9¼ W	8 W	7¼ W
25 S	90 E	11¼ W	10¼ W	9¼ W	8¼ W	7¼ W	7 W
25 S	95 E	9½ W	9 W	8½ W	7¾ W	7 W	6½ W
25 S	100 E	7¾ W	7½ W	7¼ W	7 W	6¾ W	6 W
30 S	0	2¾ W	4¼ W	6 W	7¾ W	9½ W	11¾ W
30 S	5 W	0½ W	2 W	3½ W	5¼ W	7 W	9 W
30 S	10 W	1½ E	0	1½ W	3 W	4½ W	6¼ W
30 S	15 W	3¼ E	2¼ E	0¾ E	0½ W	1½ W	3½ W
30 S	20 W	5 E	4¾ E	3¾ E	2¾ E	1½ E	0½ W
30 S	25 W	7¼ E	6½ E	5¾ E	5 E	4 E	2 E
30 S	30 W	9 E				6½ E	4½ E
30 S	35 W	11 E				8½ E	7¼ E
30 S	5 E	5 W	7 W	8¾ W	10¼ W	11¾ W	13¾ W
30 S	10 E	7¼ W	9¼ W	11¼ W	12¾ W	14 W	15½ W
30 S	15 E	9½ W				16 W	18 W
30 S	30 E	17½ W	18½ W	19½ W	20½ W	21¾ W	23¼ W
30 S	35 E	20¼ W	21 W	21¾ W	22½ W	23½ W	24¾ W
30 S	40 E	21¾ W	22½ W	23¼ W	24 W	24¾ W	25¼ W
30 S	45 E	23 W	23½ W	24 W	24½ W	25¼ W	24½ W
30 S	50 E	23½ W	23¾ W	24 W	24¼ W	24½ W	23¼ W
30 S	55 E	23 W	23 W	23 W	22¾ W	22½ W	21 W
30 S	60 E	21¾ W	21½ W	21 W	20½ W	20 W	18 W
30 S	65 E	20¼ W	19½ W	19 W	18½ W	17¾ W	15 W

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
30 S	70 E	18¾ W	18 W	17¼ W	16½ W	15½ W	13 W
30 S	75 E	17¼ W	16½ W	15¾ W	14¾ W	13¾ W	11½ W
30 S	80 E	15¾ W	15 W	14 W	13 W	12 W	10¾ W
30 S	85 E	14 W	13¼ W	12½ W	11½ W	10½ W	10¼ W
30 S	90 E	12½ W	11¾ W	11 W	10¼ W	9½ W	9½ W
30 S	95 E	10½ W	10 W	9½ W	9¾ W	8¾ W	9 W
30 S	100 E	8¾ W				8¼ W	
35 S	0	2½ W	4 W	5½ W	7 W	8½ W	10½ W
35 S	5 W	0	1 W	2½ W	4 W	5¾ W	7¾ W
35 S	10 W	2¼ E	1½ E	0½ W	1¾ W	3¼ W	5½ W
35 S	15 W	4¼ E				0¼ W	2¼ W
35 S	20 W	6¾ E				2½ E	0½ E
35 S	25 W	8¾ E				5 E	3 E
35 S	30 W	10¾ E				7¼ E	5¾ E
35 S	35 W	12¾ E				9¼ E	8¼ E
35 S	5 E	5 W	6½ W	8 W	9½ W	11 W	13 W
35 S	10 E	7¼ W	8½ W	10¼ W	11¾ W	13½ W	15¼ W
35 S	15 E	9¾ W	11¼ W	12¾ W	14¼ W	15½ W	17½ W
35 S	20 E	12½ W	14 W	15½ W	17 W	18½ W	19¾ W
35 S	25 E	15¼ W	16½ W	18 W	19¼ W	20¾ W	22½ W
35 S	30 E	18¼ W	19½ W	20½ W	21½ W	22¾ W	24¼ W
35 S	35 E	21 W	22 W	22¾ W	23½ W	24½ W	26 W
35 S	40 E	22¾ W	23½ W	24¼ W	25 W	26 W	26¾ W
35 S	45 E	24¼ W	25 W	25 W	26 W	27 W	26½ W
35 S	50 E	24¾ W	25¼ W	25¾ W	26 W	26½ W	24¾ W
35 S	55 E	24¼ W	24½ W	24½ W	24¾ W	25 W	23 W
35 S	60 E	23 W	23 W	23 W	22¾ W	22¾ W	21 W
35 S	65 E	21½ W	21¼ W	21 W	20¾ W	20¼ W	18¾ W
35 S	70 E	19¾ W	19¼ W	18¾ W	18¼ W	17¾ W	16¾ W

Latitude.	Longitude, From London.	VARIATION. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
35 S	75 E	18¼ W	17¾ W	17¼ W	17½ W	16 W	15¼ W
35 S	80 E	16¾ W	16¼ W	15¾ W	15 W	14¼ W	14¼ W
35 S	85 E	15 W	14½ W	14 W	13½ W	13 W	13½ W
35 S	90 E	13½ W	13 W	12¾ W	12½ W	12 W	12¾ W
35 S	95 E	11½ W	11½ W	11¼ W	11 W	10¾ W	
40 S	0	2 W	3½ W	5 W	6½ W	7¾ W	9¼ W
40 S	5 W	0¾ E	0½ W	2 W	3½ W	5 W	6¾ W
40 S	10 W	3¼ E				2½ W	4¼ W
40 S	15 W	5½ E				0½ E	1½ W
40 S	20 W	8 E				3¼ E	1¼ E
40 S	25 W	10½ E				5½ E	4 E
40 S	30 W	12½ E				8 E	6½ E
40 S	5 E	4½ W	6 W	7½ W	9 W	10¼ W	12 W
40 S	10 E	7¼ W	8½ W	10 W	11½ W	12¾ W	14½ W
40 S	15 E	9¾ W	11¼ W	12¾ W	14 W	15½ W	17¼ W
40 S	20 E	12¾ W	14¼ W	15½ W	17 W	18½ W	20 W
40 S	25 E	16 W	17½ W	18½ W	19¾ W	21¼ W	22¾ W
40 S	30 E	19 W	20 W	21¼ W	22¼ W	23½ W	25¼ W
40 S	35 E	21¾ W	22¾ W	23½ W	24½ W	25½ W	27 W
40 S	40 E	23¾ W	24½ W	25 W	26 W	27 W	28½ W
40 S	45 E	25¼ W	26 W	26¾ W	27½ W	28¼ W	27¼ W
40 S	50 E	26 W	26½ W	27 W	27½ W	28¼ W	26 W
40 S	55 E	25¼ W	25½ W	25¾ W	26 W	26½ W	24½ W
40 S	60 E	24 W	24 W	24 W	24¼ W	24¼ W	22¾ W
40 S	65 E	22½ W	22½ W	22¼ W	22¼ W	22 W	20¾ W
40 S	70 E	20¾ W	20½ W	20¼ W	20 W	19½ W	19½ W
40 S	75 E	19¼ W	18¾ W	18¼ W	17¾ W	17¼ W	18¼ W
40 S	80 E	17½ W	17 W	16½ W	16 W	15½ W	17¼ W

VARIATION *of the* Magnetic-Needle, *from the Islands of* Orkney *to* Hudson's Straits, *for the Year* 1757.

West Longitude From *London*.		Degrees of North Latitude.							
		56	57	58	59	60	61	62	63
Degrees.	Degrees of West Variation.								
4					18	18	19	19	
10				19	19	20	20	21	
27				24	24	25	25		
45		29	29	30	31				
55									
65						39	40	41	

VARIATION *in* Hudson's-Bay *and* Straits, *for the Year* 1757.

West Longitude From *London*.		Degrees of North Latitude.									
		52	55	56	57	58	59	60	61	62	63
Degrees.	Degrees of West Variation.										
65								39	40	41	
71									41	41	
79											43
81									38	39	40
83		18	20							39	40
86									35	37	
92					17	17					
94						17	18				
95							18				

We have been informed, that in Hudson's-Bay, there has been very little alteration in the variation of the compass during the twenty years last past.

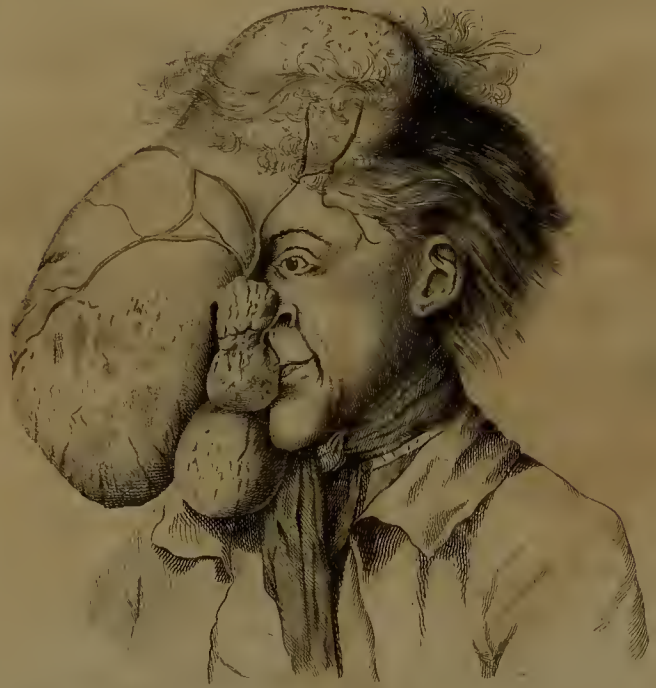
XLII.

XLII. *An Account of ſome extraordinary Tumors upon the Head of a labouring Man, now in* St. Bartholomew's *Hoſpital. By* James Parſons, *M. D. F. R. S.*

Read Nov. 10, 1757.

THIS poor man, whoſe name is John Tomlinſon, gives this account of himſelf: That he was born at or near Rotherham in Yorkſhire, and is now about 25 years of age: that when he was a boy of four or five years old, at play with other children, he received a blow from one of them upon the top of his head; and believes that hurt, he then received, was the beginning of the appearances, that are repreſented before you. *See* TAB. XIV. The tumor upon the top of his head, however, grew firſt, and, after having ſpread all over the vertex, extended gradually downwards over his right ſhoulder, and forwards over the *os frontis*, on the ſame ſide, till it ſtretched downwards into a lax flabby ſubſtance all over the right ſide of his face and ſhoulder: then the upper of the three anterior tumors aroſe from the large one; the middle one from the *ala naſi*, pulling it down by its weight, as you ſee it in Figure 1.; and the lower one was pendulous from the inſide of the great tumor by a narrow neck. Theſe are the appearances which preſent themſelves at firſt ſight; but thoſe under the great tumor are no leſs extraordinary; for, upon lifting up the great tumor, and looking up

* The two figures ſhew a fore and back view of this ſubject.



I. Parsons MD. *ad viv. del*

J. Mynde sc.

under it, his right eye comes in ſight, with which he ſees very well, and the eye is clear and ſound; but the under lid is pulled down, and ſtretched to ſix or ſeven inches long, to which a tumor hung alſo, as large as that anterior one at the chin, the loweſt of the three; beſides ſeveral flaps and *rugæ* of ſkin, and ſmaller tumors.

The hairy ſcalp is ſo ſtretched by the vertical tumor, that the hairs are driven aſunder; ſo that the tumor is in ſome places bald, and the whole is rugged and uneven. At its baſis, all round, till we come to the extended part that goes away to the right ſhoulder, a bony edge may be diſtinctly felt, as if the ſkull was depreſſed at the top: and yet I cannot but believe, that there is no depreſſion of the arch of the inner table, becauſe the man was from his childhood ever very healthy; being never troubled with thoſe ſymptoms, which uſually attend a depreſſion of the *cranium*. From this ſeeming edge the *os frontis* ſhoots out a great way over the *oſſa naſi*, perhaps to two or three inches beyond the frontal ſinus's; and is the baſis, from which the great pendulous tumor hangs downwards and forwards.

From the root of the noſe, under the upper of the three ſmaller tumors, ariſes a large trunk of a vein, which ramifies up to the vertical tumor, and to the right over the upper part of the great pendulous one: theſe are very conſpicuous, and ſerve to bring back the reſidual blood from the tumors: nor is it unlikely that the arteries bear a proportion with theſe veins in their ſize, in order to ſupply the tumors with the matter, which has given them their great increaſe; but theſe, lying concealed, cannot be ſpoken to with any certainty.

If

If we compare this growth of the frontal bone with that of other *exoſtoſes*, I believe there may this difference be rationally obſerved; that other *exoſtoſes* are generally attended with ulcerous tumors, which are for the moſt part cancerous; and theſe may commence at any age. I have now drawings, taken from the right hand of a man of 50, which repreſent riſings of the *radius* and *ulna*, with the fingers, to a moſt frightful degree; and theſe begun but ſix years before, and are attended with foul running ulcers; and now the bones of the arm and hand, on the left ſide, are beginning to have the ſame appearances: whereas the frontal bone of the preſent ſubject appears ſound, as far as we are able to judge by examination: nor does there appear the leaſt diſpoſition to ulceration in any part of it. When this is the caſe, the growth generally begins while the ſubjects are young; upon which we ſhall be more particular a little further on. His ſenſation upon every part of theſe tumors, is exactly like that of every other part of his ſkin, having not the leaſt uneaſineſs upon being handled. This poor man worked at day-labour in the fields till ſome months before he came to town.

Perhaps it may not be improper to lay down the dimenſions of theſe tumors, as the caſe is ſo extraordinary; for the ſize of them is almoſt incredible: but I made my drawing in the preſence of ſeveral of the gentlemen of that hoſpital, who allowed it to be very exact, and preciſe in the expreſſion of the parts, as well as in the dimenſions. The vertical tumor is about ſeven inches diameter at the baſis, where the bony edge is felt, mentioned before, and about four inches high from that edge. From that

edge,

edge, or the bafis of the vertical tumor, to the bottom of the great tumor, is ten inches; fo that the length of both, from the vertex to the end of the great one, is about 14 inches: and upon viewing it, when he turns his fide towards you, the whole mafs is eight or nine inches over all the way; hard at top, and flabby downwards, hanging in kinds of plaits. From the eye to the oppofite outline of the great tumor is fix inches; and lower down, from the left corner of his mouth to the oppofite outline of the fame tumor, eight inches. The upper fmall tumor, over the nofe, is one inch three quarters long by one inch and a half; the middle tumor is two inches long from the *ala nafi*, to which it hangs, and of the fame breadth; and the loweft tumor, fhaped like a goofe's egg, is four inches and a half long by near three inches over.

This man is under the care of Mr. Crane, an eminent furgeon of St. Bartholomew's Hofpital, who has juft now taken off the loweft of thefe three anterior tumors, and alfo the tumor mentioned, which hung underneath to the under lid of his right eye. He intends proceeding to take off that at the *ala nafi* next, and fo on till he takes away all the fmaller tumors firft: afterwards the larger will be confidered. The fubftance of thofe cut off was intirely fat; nor was there the leaft fpeck of blood in the loweft of the three fmaller tumors; but there was an hæmorrhage from a veffel divided in taking off that hanging to the right eye-lid; which foon yielded to the methods he made ufe of, and went on fuccefsfully till quite healed.

It is pity no one of the people of condition in the

 country,

country, where this poor man lived, took notice of him while he was a lad; becauſe, when the vertical tumor begun, or even after it had made ſome progreſs, if he had been ſent up to any of our hoſpitals, there would have been no difficulty in curing him. This leads me to ſome precautions, which, I hope, will render my account of the caſe of ſome uſe, conſidered in a phyſiological light; which was my intention in thus laying it before this learned Society.

There is a great deal of difference between injuries received in young ſubjects and in adults. In the latter, the conſequences are not apt to be of ſo dangerous a nature as in the former (except indeed where there happen violent fractures or wounds, which immediately diſpatch the perſon, young or old); becauſe, in ſuch as are ſo far advanced in years, as that the parts have done growing, or, in other words, are incapable of carrying the perſon to any larger ſize, preſerving the natural proportion, a tumor arifing from a blow on the head would be merely local, without extending to any neighbouring parts in ſo extraordinary a manner: but in children, as in the caſe before you, a tumor may increaſe every moment from a blow, and ſpread itſelf to the neighbouring parts, to the ruin of the child, unleſs timely care be taken to prevent it; becauſe in ſuch young ſubjects the parts are continually growing, the veſſels enlarging in their diameters, and carrying more and more nutrition to every point, in proportion to the nature of each individual organ, always preſerving ſuch an equilibrium, in the diſtribution of the nutritive juices, as is proper to ſecure the due proportion of every part as it increaſes: but when a tumor ariſes

from

from a blow in ſuch a growing ſubject, if no wound is made, nor ſuppuration brought on in the tumor, then the parts of the tumor being only weakened, the equilibrium is deſtroyed, a greater flux of juices than ordinary is carried to it, the due reſiſtance being impaired, and a luxuriancy of growth is produced in the place of the injury, which greatly exceeds that of the reſt of the body; and will moſt certainly continue in the ſame manner, during the growth of the ſubject, when once thus begun. In the preſent ſubject, this luxuriancy was communicated even to the veins, which are apparent and large, and which were before, in their natural ſtate, ſcarce viſible; and not only to theſe, but to the very bones of the forehead: and as to the integuments and membranes of the body, their great diſtenſibility is well known to every one. I have ſeen an *ovarium* ſo diſtended by water, and thickened as it grew, that it had ſubſtance enough to bear being dreſſed by a tanner, and contained nine gallons, which I ſaw poured into it after it was dreſſed. And does not every corpulent perſon ſhew the ſame power of diſtenſion in the membranes and integuments of the body, as well as wens of all kinds upon the ſurface?

I thought ſo extraordinary a caſe well worth the notice of the learned members of this Society in itſelf; and the more ſo, as theſe few hints fall naturally from it, to render its publication uſeful. We are taught by this, how neceſſary it is for all ſuch as have the management of youth under their care, to have an early regard to every accident that may befall children; for many times injuries of this kind have been thought very trivial, which, being overlooked

and neglected too long, have been followed by very direful circumſtances. I have known a young gentleman to have great hard ſwellings about his head, and become epileptic, loſing his ſenſes as he advanced in years, from a blow with the back of a book given him by a maſter. I am, with due reſpect,

The SOCIETY'S

Sept. 18. 1757. Moſt obedient Servant,

James Parſons.

XLIII. *An Extract of the Regiſter of the Pariſh of* Great Shefford, *near* Lamborne, *in* Berkſhire, *for Ten Years: With Obſervations on the ſame: In a Letter to* Tho. Birch, *D. D. Secret. R. S. from the Rev. Mr.* Richard Forſter, *Rector of* Great Shefford.

Great Shefford, near Lamborne,
Berks, July 8. 1757.

Rev. Sir,

Read Nov. 17, 1757.

WHEN I ſettled in the country, abundant leiſure enabled me to keep an exact pariſh-regiſter. I have now finiſhed ten years, I truſt, with ſufficient care, having examined every thing accurately myſelf. The ſight of three letters, lately publiſhed in the Tranſactions, upon the ſubject of political arithmetic, put me upon over-

overlooking and methodizing my own account; which I here ſend you, to make what uſe you think proper of it.

From Lady-day 1747. to D°. 1757.

Baptized	Males	73	148
	Females	75	
Buried	Males	44	83
	Females	39	
		Increaſe	65

Buried	Under 2 years of age	25
	Between 2 & 5	4
	5 — 10	3
	10 — 20	4
	20 — 30	5
	30 — 40	9
	40 — 50	4
	50 — 60	4
	60 — 70	9
	70 — 80	11
	80 — 87	5
		83

And but one alive above 87, who is 91.

The Number of People 425.
The Number of Houſes 90.
The Number of Acres 2245. whereof $\frac{1}{6}$ is waſte.

I do not offer ſuch trifling numbers as theſe, as a fit ſubject to build a canon of life upon; but only

as they may furniſh us with a few particulars, which may throw ſome ſmall light upon a ſubject hitherto very little cultivated: and as what has been advanced this way has been always taken from great cities, a little from the country perhaps may not be diſagreeable.

The firſt obſervable in my numbers is, that the two infancies of human life are exactly equal; i. e. as many die above 60 as under 2 years of age; and that theſe two periods of life are by much the moſt ſickly, five eighths of the whole, nearly, dying in theſe two ſtages, which renders the intermediate numbers very ſmall.

This will give us ſome reaſon to ſuſpect, that capital cities are very improper to eſtimate the probabilities of life from. The continual flux of people from the circumjacent country, to ſeek for employment, makes the decrements of life ſeem much larger than they really are. London is very remarkable upon this account; and Breſlaw muſt receive pretty large acceſſions, as a very conſiderable manufacture is carried on there.

The ſecond thing I would obſerve from my table is, that it confirms what Dr. Brackenridge obſerves of the Iſle of Wight; *viz.* that the births are to the burials as 2 to 1 almoſt; ours being as 15 to 8 nearly. Now if this is the caſe of all the country places in England, it will give us a ſtrong preſumption, that the increaſe of mankind is much quicker than Dr. Derham's proportion of 1 to 12; eſpecially if we conſider,

Thirdly, That of the living not 1 in 50 dies yearly; and this in a village not very healthy. We are ſituated upon the celebrated Lamborne ſtream, which

which dries up generally in Auguſt, and leaves a ſtagnated water, and ſtinking mud, at a critical ſeaſon of the year, which bring on a putrid fever, and make our place ſometimes very ſickly. In the year 1751 we buried 17, and in 1756. 11: and therefore we may preſume, that in the healthieſt parts of the nation, the proportion is ſtill greater, perhaps not one in 60. In order to clear up this, it were to be wiſhed, that the actual number of the people was known, where-ever the bills of mortality are exhibited. All reaſoning without this preliminary is really not much better than groping in the dark.

A fourth thing obſervable from my numbers is, that the quantity of people allotted to a houſe is too big in all former calculations: for if we divide 425, the number of people, by 90, the number of houſes, it gives but 4.72, which is not quite $4\frac{3}{4}$ to a houſe; and therefore 5 to a houſe, I believe, is as much as ought to be allowed, taking the nation all together. Now if the number of houſes, taken in Queen Anne's time, be any thing near the right, with one fourth more allowed for cottages, according to Dr. Brackenridge's computation, we ſhall make the people in England, allowing 5 to a houſe, to be only 4,556,550. which appears, at firſt ſight, to be too ſmall a number. However, of Shefford I would beg leave to obſerve, (and it is far from being the poorest of villages) that more than two thirds of all the houſes are downright cottages, and muſt be excluded, one as much as another, from any propoſed aſſeſſment. Upon this foundation we muſt grant, that at leaſt half the houſes in England, take towns and all together, muſt be cottages, and plead an exemption from taxation

all

all alike. And thus the number of houſes will be 1,458,096. which, multiplied by 5, will give us the number of people, 7,290,480. If to this we add the propoſed increaſe, 789,558. we ſhall have 8,080,038 for the number of people now alive in England.

The fifth and laſt thing I would obſerve from my numbers is, that we may hence gueſs at the number of people in the whole kingdom: for if 1871, the good acres in Shefford, demand 425 perſons for their cultivation, then will 25,300,000 good acres in England require 5,704,168 for the cultivation of the land only. Now ſuppoſing one third part of the people only to live in towns, above what is neceſſary for the cultivation of the land belonging to ſuch towns, then we muſt add 2,852,084 to the above ſum, which gives us 8,556,252 for the number of people in England. It may probably here be ſaid, that this is but little better than reckoning at random. Indeed I allow it is ſo. But then I muſt beg leave to obſerve, that it has full as good a foundation to ſtand upon, as any calculation, that I have ſeen hitherto advanced. It has one *datum*, viz. a certain number of perſons to a certain number of acres. It ought to be noted at the ſame time, that we are an inland place, have no ſort of manufacture carried on, and conſequently no acceſſion of ſtrangers.

If we examine the calculation ariſing from the conſumption of wheat, we ſhall ſee ſome reaſon to ſuſpect, that the number of inhabitants in England is not ſhort of eight millions. I am perſuaded I do not exaggerate, when I affirm, that three fourths of the people north of Trent, and in Wales, do not

eat

eat wheat: and as this is near a third part of England, it will follow, that one fourth of the whole is left out of the calculation, and that we muſt add near two millions to it to make it complete.

Again, I compute, that in my pariſh there are killed annually 160 fat hogs, *viz.* above one to three perſons; and that this humour of pig-killing prevails over half of England at leaſt, and is in ſome meaſure indulged in all parts. Now we will ſuppoſe, that there are but ſix millions of people in the nation, and that what is killed in the northern half makes up for what is deficient by reaſon of towns in the ſouthern half; we muſt from hence conclude, that a million of fat hogs are killed in England every year. Now one hog with another takes two quarters of corn, ſometimes barley, ſometimes peaſe: if we put half barley, we ſhall be under the truth. And here we ſhall have a million quarters of barley, not only to balance the exportation of wheat, but alſo to be equivalent to, as much bread-corn as will maintain a full million of people.

Farther, it is well known, that the greateſt part of the corn-trade is, of late years, got into the hands of millers: and it has been whiſpered about for a conſiderable time, and, I think, now the millers do not deny it, that *ſome* whiting is carried to all the great mills. The excuſe alleged for it is, that it makes the flour *wet*, and conſequently *bake*, the better. I am rather inclined to be of opinion, that it is to give a colour to ſomething that wants colour. And indeed, whoever taſtes the common bakers bread againſt a piece of genuine wheat-bread, will have ſome reaſon to ſuſpect, that all is not gold, that glitters. Every body

 knows,

knows, that the millers buy large quantities of barley and peaſe, they ſay, to fat hogs: but then they have pollard, middlings, *&c.* to fat them with; and ſo may poſſibly mix the barley and peaſe with wheat to grind. But as this is all ſurmiſe, I would have no more weight laid upon it than it deſerves.

The next article is of the ſame nature; I mean, ſomething of a myſtery in trade; and therefore to be touched very gently. What I would hint is, that it is the opinion of many very intelligentper ſons, that a good deal of malt is made, which does not pay the exciſe. I do not pretend to aſcertain the quantity: perhaps one eighth may not be an extravagant ſuppoſition. And if this be the caſe, we ſhall find as much barley, as will weigh againſt bread for half a million of people.

But here, in all probability, you will object, that if all theſe articles be admitted, we ſhall make the number of people near eleven millions; which is undoubtedly too much. I am ready to grant it. And here, if I might take the liberty to ſpeak my mind, I think, that the allowance of one quarter of wheat to three perſons is too ſcanty, and muſt quite ſtarve the poor, whoſe chief proviſion is bread: and therefore, two perſons to a quarter may be pretty near the truth. And then the numbers will ſtand thus:

Such as eat wheat, by ſuppoſition -	4,500,000
In the North, and in Wales - -	1,500,000
Againſt the fatting article - - -	1,000,000
Againſt the two laſt articles - -	1,000,000
	8,000,000

I can-

I cannot conclude this long ſcroll without recommending it ſtrongly to the members of the Royal Society, who have many of them ſeats in parliament, and moſt of them intereſt in thoſe that have, to get an Act paſſed for perfecting regiſters. The trouble is trifling; the expence nothing. It would be of great ſervice likewiſe to number the people: and this might be done with great eaſe. I was not three hours in finiſhing mine on foot; tho' it is, perhaps, as extenſive, for the number of people, as moſt in England, being near five miles in length. I am,

Reverend Sir,

Your affectionate Brother,

and very humble Servant,

Richard Forſter, *Rector*.

XLIV. *A remarkable Caſe of an Aneuriſm, or Diſeaſe of the principal Artery of the Thigh, occaſioned by a Fall. To which is prefixed a ſhort Account of the Uncertainty of the diſtinguiſhing Symptoms of this Diſeaſe. By* Joſ. Warner, *F. R. S. and Surgeon to* Guy's Hoſpital.

Read Nov. 17, 1757.

WHEN the coats of an artery become by any means præternaturally diſtended, when they become wounded, or when they become ruptured in ſuch a manner as to

 diſcharge,

diſcharge and depoſit their former contents under the neighbouring integuments, under the aponeuroſis, or tendinous expanſion of a neighbouring muſcle, or ſtill more deeply under the muſcles themſelves; the natural conſequence attending this accident will ſooner or later be a degree of elevation, or tumor: which ſpecies of tumor is known by the term *aneuriſm*.

If a true aneuriſm happens, that is, a ſwelling ariſing from a general weakneſs of the coats of an arterial veſſel, or from a wound or rupture of ſome of its coats, it may be often diſtinguiſhed from a tumor proceeding from any other cauſe by a degree of pulſation, ſuppoſing the ſituation of the injured veſſel be ſuperficial; as may be evinced in recent aneuriſms of the humeral artery, which ſometimes happen from bleeding near the bending of the elbow-joint; as well as in aneuriſms of the inferior part of the radical artery, of the ulnary artery, or of the anterior artery of the leg called *tibialis antica*; and as may be obſerved to be ſometimes the caſe too in thoſe arteries, whoſe ſituations are not ſuperficial; to wit, in aneuriſms of the *aorta aſcendens*, the curvature of the *aorta*, and of the *carotides*.

The ſymptom of pulſation in tumors, which take their riſe from a partial wound, or from a general weakneſs, and ſubſequent dilatation of the coats of an artery, is not confined to this ſpecies of aneuriſm, but is frequently attendant upon falſe aneuriſms (that is, ſuch tumors, as are occaſioned by extravaſated arterial blood), ſuppoſing the diſeaſe to be a recent one of either of the preceding veſſels, or of any other arterial veſſel not deeply ſituated: and this

this ſymptom of pulſation in falſe aneuriſms will ſometimes be accompanied with a diſcoloration, or variegated appearance, of the integuments dependent upon the inſinuation of the blood underneath them.

But if the extravaſation be confined under an aponeuroſis, or if the diſeaſe has been of ſo long ſtanding, as to admit of the thinner parts of the extravaſated blood being abſorbed, or by any other means diſperſed, and the fibrous parts, which are left behind, ſhould be accumulated in conſiderable quantities, and acquire ſo compact and ſolid an appearance, as to reſemble brown macerated leather in their colour and texture, which I have always obſerved to be the caſe in old diſeaſes of this kind; under theſe circumſtances, the original ſymptoms of pulſation on the ſwelling, and a diſcoloration of the integuments, for the moſt part become imperceptible: for which reaſons the true nature of the diſeaſe muſt be attended with a degree of uncertainty.

It muſt be acknowleged by all thoſe, whoſe experience has given them opportunities of examining into theſe diſeaſes, that the ſymptoms of a pulſation, and a diſcoloration of the teguments from extravaſated blood, are not only very often wanting in old aneuriſms, but in the moſt recent ones: which proves the non-exiſtence of theſe ſymptoms to be no certain characteriſtics of tumors not being aneuriſmal: and the reaſon why this often happens may be readily explained, and conceived of, from demonſtrating the very deep or low ſituation of many arteries, that are known to be liable to theſe injuries; ſuch as the femoral arteries, the *arteriæ tibiales poſticæ*, the *arteriæ peroneæ*, and ſome others.

Notwith-

Notwithſtanding I have treated of pulſation on tumors, and a diſcoloration of the integuments or coverings of the part, when they do exiſt, as being the trueſt marks of aneuriſms; yet it muſt not be inferred from what has hitherto been advanced, that the appearances of theſe ſymptoms are unexceptionable rules of tumors being aneuriſmal; ſeeing it does happen, that mere impoſthumations, or collections of matter, ariſing from external as well as from internal cauſes, are ſometimes ſo immediately ſituated upon the heart itſelf, and at other times upon ſome of its principal arteries, as to partake in the moſt regular manner of their contraction and dilatation (ſyſtole and diaſtole).

Some years ago I ſaw an inſtance of a boy, about 13 years of age, who had his breaſt-bone much broken by a fall. On this acccount he was admitted into Guy's Hoſpital; but not till a fortnight after the accident happened.

Upon examination, there appeared an evident ſeparation of the broken parts of the bone, which were removed at a conſiderable diſtance from each other: the intermediate ſpace was occupied by a tumor of a conſiderable ſize: the integuments were of their natural complexion: the tumor had as regular a contraction and dilatation as the heart itſelf, or the aorta could be ſuppoſed to have.

Upon preſſure, the tumor receded; upon a removal of the preſſure, the tumor immediately reſumed its former ſize and ſhape. All theſe are the diſtinguiſhing ſigns of a true recent aneuriſm. The ſituation and ſymptoms of this ſwelling were judged ſufficient reaſons for conſidering the nature of the

diſeaſe

diſcaſe as uncertain; on which account it was left to take its own courſe. The event was, the tumor burſt in three weeks after his admittance, diſcharged a conſiderable quantity of matter, and the patient did well.

From what has been above advanced it is plain, if theſe arguments can be ſupported by facts, that the laying down ſuch rules for infallibly diſtinguiſhing aneuriſmal tumors from tumors proceeding from very different cauſes, muſt be a matter of the greateſt difficulty: and, as a further proof of their uncertainty, I take the liberty of offering the following ſhort hiſtory of a remarkable caſe, which has lately occurred in my own experience.

In the month of December 1756. John Yates, aged 35 years, received an hurt upon and about his knee, by falling upon the ground from a man's back. The accident was immediately followed with a conſiderable degree of lameneſs and pain; which upon ſtanding or walking were greatly increaſed.

He continued in much the ſame ſtate for about ſix weeks after the accident. At the end of this time, the calf or the leg was attacked with an œdematous or doughy ſwelling; which, in a fortnight, became ſo painful, as to diſable him from walking. The tumor continued to increaſe for about eight weeks; and at length extended itſelf ſo far upwards, as to affect the greateſt part of the thigh, the whole of which was attended with exceſſive pain, but more particularly ſo about the knee.

N. B. So far I relate from the patient's own account.

On

On the 28th of April 1757. he was admitted into Guy's Hoſpital under my care.

Upon examination, the thigh appeared enlarged to a very great ſize. The tumor was uniform, and extended from the inſide of the knee to within a very ſmall ſpace of the groin. The integuments were in every part of their natural colour.

Upon preſſing the tumor on the inſide, it appeared ſoft, and there was a very evident fluctuation to be felt on its internal and lateral part; but there was not the leaſt appearance of pulſation.

The tumor, on its ſuperior and poſterior parts, was of a ſtony hardneſs.

The leg, which, according to the patient's account, had ſome time ago been much ſwelled, did not now appear to be at all ſo.

He was continually in great pain, and had been for ſome time incapable of getting any ſleep. His appetite was bad. He was a good deal emaciated. He had a conſtant ſlow fever, which aroſe about five weeks before his admiſſion into the hoſpital. He appeared pale and ſallow in his complexion.

From the time of his being placed under my care to the end of ten days, there was no alteration in the ſwelling, or in the ſymptoms attending it.

In expectation therefore of affording him that relief, which could by no other means be procured, I judged it adviſeable to make an opening into the tumor; which I did by inciſion into the moſt prominent and fluctuating part; upon which there immediately guſhed out a large ſtream of thin florid blood, and at this inſtant diſcovered to me the true ſtate of that diſeaſe; which, till now, could not be aſcertained

tained by any peculiar ſymptom diſtinguiſhable by by the touch, or perceptible to the eye.

Seeing this, I immediately filled up the wound with lint and tow; and then proceeded, in as expeditious a manner as poſſible, to apply a tight bandage upon the thigh, near to the groin; and, leſt this might accidentally break, I applied a ſecond ligature below the firſt, and proceeded to amputate the limb upon the ſpot.

During the operation the man fainted, but ſoon recovered from this deliquium; and, without any bad ſymptoms, gradually recovered his reſt, appetite, and ſtrength, and is now in perfect health.

Upon a diſſection of the thigh and leg, I diſcovered the following appearances:

A great part of the fleſhy portions of two of the extenſor muſcles of the leg, to wit, the *vaſtus internus*, and *crureus*, were deſtroyed, with the ſubjacent *perioſteum*.

Four of the muſcles, whoſe uſes are to bend the leg, and which compoſe the internal and external hamſtrings; to wit, *gracilis*, *ſemitendinoſus*, *ſemimembranoſus*, and *biceps tibiæ*, together with that adductor and flexor muſcle of the leg called *ſartorius*, were removed at a conſiderable diſtance from the thigh-bone on its inferior part, and from the *tibia* and *fibula* on their ſuperior parts; by which means a large bed or cavity was formed for containing the extravaſation, which conſiſted partly of a fluid, and partly of a coagulated blood; but by far the greateſt part of the coagulum had acquired ſo firm and fibrous a conſiſtence and appearance, as nearly to reſemble brown macerated leather in its colour and

texture. The neighbouring muſcles appeared livid and lacerated.

The *os femoris* was become carious on its inferior and poſterior parts; and, at about an inch diſtance above the condyle of that bone internally, there aroſe a conſiderable *exoſtoſis*.

The capſular ligament of the knee-joint was become much thickened, and contained about two ounces of a viſcid yellow *ſynovia*.

The femoral artery, on its inferior part, juſt above its diviſion into *tibialis antica* and *poſtica*, was diſeaſed; which diſeaſe extended four inches upwards.

The coats of the artery were conſiderably thickened, and lacerated longitudinally.

The ſmalleſt diameter of the diſeaſed part of the artery was two inches and one quarter: the largeſt diameter of the diſeaſed part of the artery was two inches and one half.

Hatton-Garden,
Nov. 17. 1757.

XLV. *Farther Experiments for increaſing the Quantity of Steam in a Fire-Engine. By* Keane Fitz-Gerald, *Eſq*; *F. R. S.*

Read Nov. 24, 1757.

I Gave a former account to the Royal Society of ſome experiments made for increaſing the quantity of ſteam in a fire-engine, by blowing air thro' boiling water *. The effects

* See above, No. X. p. 53.

then evidently produced left me, and I believe many others, who came to view the experiments, no room to doubt the ſeeming cauſe. In which error I ſhould probably have ſtill remained, had not farther experiments demonſtrated the miſtake.

Whatever apology I ought to make this learned Society, for having given in that account prematurely, I believe their great regard to truth, which has always been the baſis of their reſearches for the improvement of natural knowlege, will require none for this. I ſhall therefore, as briefly as I can, relate the further experiments, that were made, which evidently demonſtrate the error of the former; and from which ſome phænomena have occurred, perhaps hitherto unknown.

In order to try what difference the air paſſing thro' a thinner body of water might occaſion, I brought the horizontal pipe, which (as mentioned in the former account) was placed 12 inches under the ſurface of the water, to within ſix inches; and found, on ſetting the engine to work, that the leaden pipe, for the conveyance of air from the bellows into the boiler, became much hotter than I had perceived it before; which could not happen, if a conſtant cool air had paſſed thro': and on ſhutting the cock, which was fixed in the leaden pipe to hinder the ſteam from aſcending into the bellows before the engine ſhould be ſet to work, tho' no air could then poſſibly paſs thro', yet the bellows ſtill continued to move with the ſame regularity as before; which, on examination, was found defective on the inſide, where the middle board, that divides the two bodies, was warped and cracked in ſeveral places, thro' which

the air paſſed very regularly from one body to the other at each ſtroke, inſtead of paſſing thro' the pipe into the boiler, as imagined. By this, the cauſe of deception was evident; which I was ſtill in hopes of remedying, by having a new pair of bellows made, ſomewhat larger, and much ſtronger. When this was fixed, and the engine worked a few ſtrokes, I was ſurpriſed to find the bellows did not come down, but remained fully charged with air, tho' it had 400 lb. weight upon it; and that, on increaſing the weight gradually to 1400 lb. which was as much as the bellows could ſupport, the air was not forced thro'.

I alſo made ſeveral experiments, by lowering the horizontal pipe two feet under the ſurface of the water, and raiſing it at different times to within four inches of the ſurface, and could not at any depth force the air thro', whilſt the engine worked; but on opening the ſteam-pipe, which is a pipe for letting the ſteam paſs from the boiler whenever the engine ſtops, the bellows could then readily force the air thro', tho' the water boiled ever ſo ſtrong, and ſeemingly made a ſurpriſing increaſe of ſteam.

I had the leaden pipe to convey the air from the bellows, which was firſt put thro' the top into the boiler, carried on the outſide, and paſſed horizontally into it, about the height the water generally ſtands, that by opening a cock, fixed for the purpoſe cloſe to the boiler, I could readily diſcharge all the ſteam lodged in the pipe; and by ſhutting the cock, and making ſmall holes at three or four inches diſtance, I could almoſt find the point, where the air and ſteam met in oppoſition, cool air being ſtrongly expelled thro' one, and hot ſteam thro' the other.

It

It was alſo perceptible, that the air was impelled ſomewhat, tho' not conſiderably, more forward by the addition of each hundred weight on the bellows:

That the deeper the horizontal pipe was placed in the water, the leſs reſiſtance was made by the ſteam:

That in proportion as the heat of the ſteam was increaſed, by making the water boil more ſtrongly, the reſiſtance to the preſſure of the air by the weight on the bellows became greater.

It is a very doubtful matter, whether air forced thro' boiling water would have anſwered the purpoſe intended: but I believe it was never imagined, that air could not be readily forced thro', until proved by the foregoing experiments. The attempt, tho' it has failed demonſtrably in that point, has produced the ſame effect from another cauſe, as to ſaving coals, and throwing up more water. For, by the conſtant care, that was taken during the time of making theſe experiments, to meaſure the coals, to admit only a proper quantity of fuel to be laid on, and alſo to mark the time exactly it took in burning; the engine then did, and ſtill continues to require eight buſhels of coals leſs, in every 24 hours work, than it did before; and alſo, from the regularity of its ſtroke, to throw up more water; the ſame care being required from the engineer, who can have no pretence for conſuming more coals now, than appeared ſufficient during the time the experiments were making.

Tho' ſome of the properties of ſteam are well known; yet the degrees of expanſion it is capable of;

of; whether air be mixed with, or neceſſary to, its formation; as alſo how far its power of reſiſtance may reach; are probably not yet known, to a proper degree of exactneſs. Niewentit fixes the expanſion of a cubical inch of water, converted into ſteam, at 13365, Dr. Deſaguliers at 14000, and Mr. Payne at 4000 times. The great ſcope in this ſubject from a plenum to a vacuum, if I may be allowed the expreſſion, as alſo the very uſeful purpoſes, to which it has already been, and poſſibly may be ſtill further applied, will, I hope, be an inducement to thoſe, who are much better qualified, to proceed in ſo uſeful an inquiry.

XLVI. *Obſervatio Eclipſis Lunæ Die* 27 Martii, *Ann.* 1755. *habita Uliſſipone in Domo Patrum Congregationis Oratorii à* Joanne Chevalier *ejuſdem Congregationis Preſbytero, Regiæ* Londinenſis *Societatis Socio, Regiæque* Pariſienſis *Scientiarum Academiæ correſpondente.*

Tubo optico 8 pedum peracta eſt obſervatio cœlo ſereno, claroque.

	IMMERSIONES.	Hora poſtmeridiana temporis veri. h	′	″
Read Dec. 8, 1757.	INitium penumb rædubium	10	29	50
	Initium eclipſis dubium	10	33	35
Certe jam incæperat	—— ——	10	34	05
Umbra ad mare humorum	——	10	44	00

Umbra

	h	′	″
Umbra ad Grimaldum — —	10	44	53
Grimaldus totus in umbra —	10	47	58
Mare humorum totum in umbra —	10	51	14
Thico incipit mergi — —	10	53	29
Thico totus mergitur — —	10	55	14
Umbra ad Reinholdum —	11	08	04
Umbram ingreditur Copernicus —	11	19	22
Umbra ad mare nectaris —	11	24	52
Totum in umbra — —	11	33	50
Umbra ad mare tranquillitatis —	11	35	24
Promontorium acutum in umbra —	11	45	46

EMERSIONES.

	h	′	″
Incipit emergere ab umbra Copernicus	12	04	38
Totus Copernicus extra umbram --	12	07	40
Incipit egredi Grimaldus —	12	09	38
Totus Grimaldus extra umbram —	12	12	38
Incipit emergere mare fœcunditatis	12	31	37
Emergit mare humorum —	12	36	11
Incipit emergere Capuanus —	12	39	40
Egreditur Schicardus — —	12	48	30
Emergit Thico — —	12	51	40
Totum mare nectaris egreditur —	12	58	09
Finis eclipsis — —	13	13	02
Finis penumbræ dubius —	13	16	50

XLVII. *Eclipsis Lunæ Die* 4ª Februarii, *Ann.* 1757. *habita Ulissipone à* Joanne Chevalier *Presbytero Congregationis Oratorii, Regiæ* Londinensis *Societatis Socio, Regiæque Scientiarum* Parisiensis *Academiæ correspondente, et a* Theodoro de Almeida *ejusdem Congregationis Presbytero, ac Physicæ publico Professore.*

Read Dec. 8. 1757.

HANC observationem peregi tubo optico novem pedes longo, cujus lens ocularis focum habebat ad 4 pollices, et lineam unam: adhibui preterea vitrum planum cæruleum, quod oculum inter et ocularem lentem interponebam, ut ingressum macularum in umbram observarem juxta ea quæ in observatione eclipsis lunæ ann. 1755. invenit clarissimus vir Josephus Soares de Barros ex regia Berolinensi academia. Primum igitur vitro cæruleo adhibito observabam ingressum maculæ in umbram, et tempore notato iterum solo tubo optico ingressum ejusdem maculæ in umbram observabam, et differentiam utriusque ingressûs notabam.

Initio eclipsis cœlum serenum ac clarum fuit, postea nubilum, et post maximam obscurationem vapores horizontis et claritas incipientis diei observationem peragere impediere.

			Manè.		
			H.	M.	S.
Initium penumbræ	—	—	4	52	49
Initium dubium eclipsis	—	—	4	55	29
Certo jam incæperat	—	—	4	57	30

Umbra

	Manè. H.	M.	S.
Umbra ad Ariſtarchum obſervata vitro cæruleo plano —— ——	5	00	19
Solo tubo optico adhibito ———	5	00	50
Keplerus umbram ingreditur obſervatus tubo, et vitro cæruleo plano —	5	13	20
Obſervatus ſolo tubo optico ——	5	14	00
Plato umbram ingreditur obſervatus tubo, et vitro cæruleo ———	5	15	2
——— Solo tubo —— ——	5	15	40
Umbra ad Eudoxum —— ——	5	17	18
Mare ſerenitatis incipit mergi ——	5	30	10
Copernicus obſervatus tubo et vitro cæruleo umbram ingreditur ——	5	36	48
——— Solo tubo —— ——	5	37	22
Mare Criſium ingreditur umbram —	5	53	51
——— Medium in umbra ——	5	59	30
——— Totum mergitur ———	6	5	21
Mare fœcunditatis occultari incipit --	6	7	41
Umbra ad promontorium acutum —	6	8	33
Umbra tangit mare nectaris ——	6	22	51
Umbra ad Langrenum —— ——	6	23	33

Obſervationes Eclipſium Satellitum Jovis Uliſſipone habitæ a JOANNE CHEVALIER, *&c.*

ANNO 1757 teleſcopio Gregoriano 7 pedum nocte ſerenâ, nullâque lunari luce illuſtratâ, obſervavi immerſionem totalem primi ſatellitis die 21 Martii tempore vero poſtmeridiano — $11^h\ 13'\ 1''$

Die vero 22 Martii tempore vero, et antemeridiano, obſervavi immerſionem totalem tertii ſatellitis $0^h\ 13'\ 32''$

XLVIII. *Obſervationes Eclipſium Satellitum Jovis Uliſſipone habitæ à* Joanne Chevalier, *Preſbytero Congregationis Oratorii, Regiæque* Londinenſis *Societatis Socio, Anno* 1757.

Read Dec. 8. 1757.

TEleſcopio Gregoriano 7 pedum obſervavi emerſionem primi ſatellitis die 7 Junii cum cœlum ſerenum ac clarum eſſet; hora poſtmeridiana temporis veri —— 10^h 29′ 21″

Die 8 Junii eodem teleſcopio obſervavi emerſionem ſecundi ſatellitis hora poſtmeridiana 8^h 32′ 48″ cœlum aliquantum nubilum erat.

Eadem die obſervavi emerſionem tertii ſatellitis hora poſtmeridiana —— —— 9^h 36′ 25″ cœlo claro.

Die 15 Junii cœlo claro obſervavi emerſionem ſecundi ſatellitis hora poſtmeridiana —— 11^h 6′ 15″

Sequenti die 16 Junii obſervavi immerſionem tertii ſatellitis hora matutina temporis veri — 0^h 0′ 29″

XLIX.

XLIX. *A remarkable Case of the Efficacy of the Bark in a Mortification. In a Letter to* William Watson, *M. D. F. R. S. from Mr.* Richard Grindall, *Surgeon to the* London *Hospital.*

Austin-Friars, Dec. 7th, 1757.

SIR,

Read Dec. 8, 1757.

THE following case being very singular has induced me to lay it before the Royal Society, and beg the favour to do it through your means. Although numerous instances are related in the records of medicine, of the great danger in interrupting nature in her operations, there is not one (so far as I know), in which more violent and extraordinary effects have been produced, than in the following.

It may happen also, that this instance may be of service in ascertaining the virtue of the medicine in intermittents, when in the hands of men of judgment.

On the 28th of June 1757. Mary Alexander, aged 31 years, of the parish of Whitechapel, was brought into the London hospital, having a mortification in both hands, which reached about an inch and half above the wrists. All her toes, and about an inch of one foot beyond the last joint, were mortified; her nose was also intirely destroyed by a mortification; and all these happened at the same time. Upon inquiry into the cause of this misfortune, I found, that on Monday the 30th of May she was seized with a quotidian

quotidian ague, which uſually began about three of the clock in the afternoon, and laſted near two hours; which was ſucceeded by a hot fit, and then a violent ſweat. And in this manner ſhe was afflicted for ſeven days without any material alteration; when, being informed by a neighbour, of a perſon, who had an infallible remedy for the cure of an ague, ſhe applied to him. He brought her two phials, containing about an ounce and half each, of a pale yellowiſh liquor; one of which he directed her to take directly, promiſing, that ſhe ſhould have no return of the fit of conſequence; and that, if ſhe had any ſmall return, the ſecond bottle ſhould cure her effectually. In conſequence of which, ſhe took one doſe, which was at the time the cold fit had been on about a quarter of an hour: ſhe had no ſooner ſwallowed it, but, as ſhe ſays, her ſtomach was on fire, and felt as if ſhe had ſwallowed the ſtrongeſt dram poſſible. The cold fit left her inſtantly; but ſhe was immediately ſeized with ſo violent a fever, as to make her burn, and be extremely thirſty, all the following night; much more ſo than ever ſhe had been before, till the next morning, when a ſweat a little relieved her from the violent heat. When ſhe roſe in the morning, ſhe was much troubled with a great itching in the hands, feet, and noſe; and ſoon after all thoſe parts began to feel numbed, or, as ſhe deſcribes it, as if her hands and feet were aſleep; which ſhe took but little notice of, till the evening of that day, when ſhe found the nails of both hands and feet were turning black, and, at the ſame time feeling great pain in both, as alſo in her noſe, and that they appeared of a darkiſh red colour, like the ſkin in cold weather.

Upon

Upon which, at nine o'clock that night ſhe ſent for an apothecary, from whom, I have ſince been informed, the perſon before mentioned had bought the medicine, which he gave her. The apothecary was not at home; his journeyman went, and finding the woman had a difficulty of breathing, ordered her a mixture with ſperma ceti and ammoniacum to be taken occaſionally. The apothecary did not ſee her himſelf till the 16th of June, when finding her in a very bad condition, that her hands, and feet, and noſe, were intirely black, and had many veſicles or ſmall bladders upon them, filled with a blackiſh bloody water; he opened them, and let out the fluid, and dreſſed them with yellow baſilicon; and in this manner continued treating her till the 20th of the ſame month, when, finding no material alteration for the better, he ordered her a browniſh mixture, of which ſhe was to take four ſpoonfuls every four hours; which, he informed me, was a decoction of the bark; and ſays, on taking this, ſhe was better, as the mortification ſeemed inclined to ſtop. But as it was a bad caſe, he adviſed the woman to be carried to an hoſpital: and in this condition was ſhe brought in, when ſhe was immediately put into a courſe of the bark, taking a drachm of the powder every four hours; and in 48 hours taking it there was a perfect ſeparation of all the mortified parts. She was then ordered to take it only three times in 24 hours; and purſuing this method for eight days, there was a very good digeſtion from the parts above the mortification.

The mortified part became now ſo offenſive, that the poor woman preſſed me much to take off her hands, aſſuring me ſhe would go through the operations

tions with good courage, being very deſirous to live, though in this miſerable condition.

On the 12th of July I took off both her hands: I had very little more to do, than ſaw the bones, nature having ſtopped the bleeding, when ſhe ſtopped the mortification. In a day or two after, I took off all the toes from both feet, and now diſcontinued the bark, the parts appearing in a healthy and healing condition; which went on ſo for five weeks, when, on a ſudden, the parts began to look livid, her ſtomach failed her, and ſhe was feveriſh; but, upon taking an ounce of the bark, in 36 hours her ſores began again to look well. She was not ſuffered to leave off the bark ſo ſoon this time, but continued taking it twice a day for a month. She is now almoſt well: that part of her face, from whence the noſe mortified, was healed in ſeven weeks; the ſtumps of both arms are intirely healed; and both feet are well, only waiting for one piece of bone ſcaling off, which I believe will be in a very ſhort time; and ſhe is now in good health.

The perſon, who gave her this medicine, is a Barber and Peruke-maker at Bow. I applied to him ſeveral times, to inform me what it was he had given her. The affair was talked of ſo much in his neighbourhood, and the man threatned by the woman's huſband, that for a long time I could not get him to tell me, till I told him, I had been informed where he bought the medicines; and the time of the day, that he had them, correſponding with the time of his giving them to the woman, and that I knew it was tincture of myrrh, he at laſt told me, that he had frequently given the above quantity of an ounce and half

of

of it in an ague; that it had never done any harm; and hardly ever failed to cure. Upon which information, I carried ſome tincture of myrrh to the woman, who taſted it, and is well aſſured it is the ſame liquor the barber gave her in her ague-fit.

I am, with reſpect,

Your obliged and obedient Servant.

Richard Grindall.

L. *A Letter to the Rev.* Tho. Birch, *D. D. Secret. R. S. from* John Pringle, *M. D. F. R. S. incloſing Two Papers communicated to him by* Robert Whytt, *M. D. F. R. S.*

Pallmall-Court, St. James's, Dec. 10. 1757.

SIR,

Read Dec. 15. 1757.

ABOUT three weeks ago I put into your hands an extract of a letter, I had then received from Dr. Whytt, containing a poſtſcript to his *Obſervations on Lord* Walpole's *Caſe*; and ſlightly mentioning ſome doubts he had then about the juſtneſs of Dr. Springsfeld's experiments with lime-water, from ſome trials he himſelf had made, upon reading that gentleman's curious treatiſe on the extraordinary lithontriptic quality of the waters at Carlſbad in Bohemia. Within theſe few Days, Dr. Whytt having favoured me with a full account of thoſe experiments, I have herewith ſent you his paper, in order, if you pleaſe, to lay it before

fore the Society; which the author defires may be done, in cafe thefe obfervations fhould be judged ufeful.

The other paper inclofed was fent me by the fame hand, to be likewife prefented to the Society, as a well-attefted inftance of the electrical power in the cure of a palfy. To the other teftimonies I have fubjoined what Dr. Whytt fays in his letter to me, by way of ftrengthening the evidence. I fhall only add, that fince Mr. Brydone, the author of this account, has omitted telling how long the patient has continued in perfect health fince the operation, it appears fhe muft have been well for fome months before the date of his paper; becaufe, before the end of laft fummer, Dr. Whytt tranfmitted the fame cafe to me, which I then returned, in order to have it drawn up in a fuller manner, and with other vouchers befides the gentleman, who performed the cure. The Doctor has been fo good as to comply with my requeft, having procured a more ample account of the circumftances from Mr. Brydone, and the atteftation of two minifters, befides that of the patient herfelf.* My difficulties being thus removed,

* After this paper was read at the Society, Dr. Pringle having acquainted Dr. Whytt, that Mr. Patrick Brydone had omitted, in his account, the name of the parifh, where the woman lived, the time when fhe was cured, and alfo that he had not fully dated his paper; Dr. Whytt fome time after wrote to Dr. Pringle, that having defired Mr. Brydone to furnifh him with thefe particulars, he had received for anfwer, "That the woman, on whom the cure was "performed, had lived all her life in the parifh of Coldinghame, "and for the laft twelve years in that town: That her father had "died

moved, I believe I may now with freedom offer this very curious caſe to the attention of the Society.

I am,

SIR,

Your moſt obedient humble Servant,

John Pringle.

Poſtſcript to Dr. Whytt's *Obſervations on Lord* Walpole's *Caſe* *.

Read Dec. 8, 1757.

" I Do not know, if it be worth while " to obſerve, that lately, in making " ſome experiments with different *calculi*, there was " one almoſt as white as chalk, but of a leſs hard " ſubſtance than the others; and which was not " in the leaſt degree diſſolved or ſoftned by being " infuſed 20 days in oyſterſhell lime-water, but " yielded ſomewhat to a ſolution of Spaniſh ſoap " in common water.

" From this experiment one may conclude, that " it is better to preſcribe both ſoap and lime-water

---

" died of the palſy ſeven years ago, after having been ſubject to " that diſtemper for ſeveral years: That the cure was performed " in his father's houſe at Coldinghame, on the 4th, 5th, 6th, " and 11th of days of April 1757. a circumſtance he had noted " down: That as to the date of his paper, preſented to the Royal " Society, he only recollects it was written ſome day in the be- " ginning of November laſt; but as the woman ſtill continued " well, he hoped the preciſe day of the month was no material " omiſſion." This letter to Dr. Whytt is dated, Coldinghame, January 9th, 1758.

* See above, p. 209, & ſeqq.

" for the ſtone, than any one of them alone; and
" that if one of theſe remedies has failed of giving
" relief, the other ought to be tried: for as the
" above white *calculus*, which yielded a little to the
" ſolution of ſoap, reſiſted lime-water; ſo there may
" perhaps be others, that are readily diſſolved by
" lime-water, but little affected by ſoap.

" Dr. Springsfeld's experiments with lime-water
" are ſomehow not juſt; for in ſeveral *calculi* I
" have found the diſſolving power of oyſterſhell
" lime-water above eight times greater than he
" makes it."

Some Obſervations on the lithontriptic Virtue of the Carlſbad *Waters, Lime-water, and Soap: In a Letter to Dr.* John Pringle, *F. R. S. from Dr.* Robert Whytt, *F. R. S. and Profeſſor of Medicine in the Univerſity of* Edinburgh.

SIR,

Read Dec. 15. 1757.

FROM the experiments related in Dr. Springsfeld's *Commentatio de prærogativa thermarum Carolinarum, &c.* which you were ſo good as to ſend me ſome time ago, it appears, that theſe waters are not only poſſeſſed of a very extraordinary power of diſſolving the ſtone, but that in this reſpect they greatly exceed lime-water.

(A) Thus, Dr. Springsfeld having infuſed, for 14 Days, in a heat of 96 degrees of Fahrenheit's ſcale, three pieces of the ſame *calculus*, each weighing 30

grains, in eggſhell lime-water, the Carlſbad water, and in the urine of one who daily drank this laſt water, renewing theſe ſeveral menſtruums every day, he found, on the 15th day, that the *calculus* in the lime-water had loſt 1 grain, the *calculus* in the Carlſbad water 6 grains, and that in urine 5 grains.

(B) Again, having divided another *calculus* into four parts, each of which was reduced to 80 grains, he put the firſt in oyſterſhell lime-water, the ſecond in Carlſbad water, and the third in the urine of a perſon who drank this water. After 20 days, during which time the menſtruums were renewed every day, and kept in a heat of 96 degrees, the dried *calculi* had loſt of their weight as follows: the firſt 3 grains, the ſecond 18 grains, and the third 14 grains.

Altho' I make no doubt that Dr. Springsfeld, who appears to be a man of candour, as well as learning, has faithfully related the event of the experiments, which he made; yet either the lime-water he uſed muſt have been very weak, or ſome other miſtake muſt have happened in his experiments: for in all the numerous trials I made, about 15 years ago, of lime-water, as a ſolvent for the ſtone, I always found its diſſolving power much greater, than it appears in Dr. Springsfeld's experiments. And as in theſe trials different urinary ſtones were uſed, it can ſcarcely be imagined, that it was owing to the peculiar hardneſs of Dr. Springsfeld's *calculi*, that the lime-water made ſo little impreſſion on them. However, to be ſtill further ſatisfied of this matter, I made the following experiments.

1. I put a piece of a very hard *calculus*, which I ſhall call *x*, weighing 80 grains, in oyſterſhell lime-water, renewing the lime-water every day, and keeping it in a heat between 90 and 106 degrees of Fahrenheit's ſcale. After 20 days, I took out the *calculus*; and having ſet it by for ſome days, till it was become quite dry, I bruſhed away all the rotten part of it, which was reduced to a kind of chalky powder, and found that the undiſſolved part of it weighed 57 grains.

2. At the ſame time a piece of another *calculus*, *z*, weighing 15 grains, was, after a like infuſion of 20 days in oyſterſhell lime-water, reduced to 10 grains.

3. I put a piece of *z*, weighing 14 grains, in a ſolution of half an ounce of the internal part of Spaniſh ſoap in nine ounces of water, and every third day renewed the ſolution, which was kept in a heat of about 60 degrees. After 14 days, I found the undiſſolved part not to exceed 11 grains.

4. A piece of a white chalky *calculus*, *y*, weighing 30 grains, had near 4 grains of its ſubſtance diſſolved, by being 14 days infuſed as above in a ſolution of ſoap.

From N°. 1. above, compared with Dr. Springsfeld's Exper. (B), it appears, that the diſſolving power of oyſterſhell lime-water is to that of the Carlſbad water as 23 to 18, ſuppoſing the *calculi* uſed in theſe experiments to have been equally eaſy to diſſolve.

N°. 3. compared with Dr. Springsfeld's Exper. (A), ſhews, that the diſſolving power of a ſolution of the inner part of Spaniſh ſoap, in a heat of 60 degrees, is

is to that of the Carlſbad water, in a heat of 96 degrees, as 15 to 14.

From N°. 4. compared with (A), the diſſolving power of ſoap is to that of the Carlſbad water only as 4 to 6; but it is probable, that had the ſolution of ſoap been kept in a heat of 96 degrees, its diſſolving power would, even in this experiment, have nearly equalled that of the Carlſbad water. It may, perhaps, be worth while to obſerve, that a piece of the white chalky *calculus* of N° 4. was not in the ſmalleſt degree diſſolved by lying in lime-water 20 days.

5. In Exper. 19. of my Eſſay on the Virtue of Lime-water, a piece of a *calculus*, *b*, weighing 31 grains, loſt 7 grains by being infuſed 36 hours, in a a heat of above 100 degrees, in very ſtrong oiſter-ſhell lime-water. And in the ſame water, of a moderate ſtrength, another piece of *b* loſt, in the ſame time, 5 grains.

In this laſt experiment, the lithontriptic virtue of lime-water appears to be ſtronger than in N°. 1. and 2. above; and greatly exceeds that of the Carlſbad water in Dr. Springsfeld's Exper. (A) and (B).

But altho', from what has been ſaid, it appears not only that lime-water, but alſo a ſolution of ſoap, diſſolves the ſtone in cloſe veſſels as faſt, nay faſter, than the *thermæ Carolinæ*; yet theſe laſt waters, when the *calculi* were ſo placed in open veſſels, that the water from the fountain might conſtantly flow along them, effected a much quicker diſſolution than lime-water, or even ſoap-lye, or indeed any known menſtruum, except, perhaps, ſtrong ſpirit of nitre: for,

for, in the firſt experiment made by Dr. Springsfeld, a *calculus* of two ounces and a half was, in this manner, quite diſſolved in ſix days. From this experiment, compared with that of Dr. Springsfeld mentioned above (B), it will be found, upon calculation, that the diſſolving power of the Carlſbad water, when it is allowed to flow conſtantly from the fountain along the ſtone, is nearly 39 times greater than when it is only poured freſh on the *calculus* once a day *. What may have been the reaſon of this ſurpriſing difference of the lithontriptic power of the Carlſbad water in theſe different circumſtances, I will not pretend to ſay. I think it can ſcarcely be accounted for from the gentle motion of the water along the ſurface of the *calculus*. Was it then owing to ſome very volatile active part, which the water quickly loſes, after being taken from the fountain?

But how great ſoever the diſſolving power of the Carlſbad waters may be, when they iſſue from the bowels of the earth, yet that they do not communicate a much greater diſſolving power to the urine, than lime-water, will appear from comparing the two following experiments.

In Dr. Springsfeld's Exper. (A) above, the urine of a perſon, who drank the Carlſbad waters, reduced, in 14 days, a piece of *calculus*, weighing 30 grains, to 25 grains. And in an experiment made by Dr. Newcome, now Lord Biſhop of Llandaff, who drank four Engliſh pints of oyſterſhell lime-water

* Vid. Eſſay on the Virtue of Lime-water, 2d edit. p. 176, 177.

daily,

daily, his Lordſhip's urine reduced, in four months, a piece of *calculus*, weighing 31 grains, to three ſmall bits, weighing in all 6 grains †. Whence it follows, that the diſſolving power of his Lordſhip's urine muſt have been to the diſſolving power of the urine of the perſon who drank the Carlſbad waters nearly as 35 to 65 ‡. But if we conſider, that the *calculus* infuſed in the urine of the perſon who drank the Carlſbad waters was kept always in a heat of 96 degrees, while in Dr. Newcome's experiment, which was made during part of the autumn and winter, no artificial heat was uſed, it will appear probable, that the diſſolving power of his Lordſhip's urine was little inferior to that of the perſon who drank the Carlſbad waters; for lime-water, in a heat of 96 degrees, diſſolves the *calculus* at leaſt twice as faſt, as in the common heat of the air in winter. Further, if it be attended to, that the quantity of Carlſbad waters drank every day before dinner is from ſix to eight lib. while his Lordſhip only drank four lib. of lime-water in 24 hours, it will follow, that whatever the different diſſolving powers of the lime-water and Carlſbad waters may be out of the body, yet the former ſeems, in proportion to the quantity drank, to communicate at leaſt an equal diſſolving power to the urine.

But without preſuming to decide certainly, as to the comparative virtue of the Carlſbad waters and lime-water, I ſhall conclude with obſerving, that tho' the Carlſbad waters are leſs diſagreeable to the

† Eſſay on Lime-water, 2d edit. p. 208, &c.
‡ Ibid. p. 176 and 177.

taſte;

taſte, and may be drank in larger quantity, than lime-water, yet this laſt may be drank equally good in all places, and at all ſeaſons of the year; which is not the caſe with the Carlſbad waters.

November 30. 1757.

An Inſtance of the Electrical Virtue in the Cure of a Palſy. By Mr. Patrick Brydone.

Read Dec. 15. 1757.

ELizabeth Foſter, aged 33, in poor circumſtances, unmarried, about 15 years ago was ſeized with a violent nervous fever, accompanied with an aſthma, and was ſo ill, that her life was deſpaired of. She recovered however from the violence of her diſtemper, but the ſad effects of it remained. For, from this time, ſhe continued in a weakly uncertain ſtate of health till the month of July, 1755, when ſhe was again taken ill of the ſame kind of fever; and after it went off ſhe was troubled with worſe nervous ſymptoms than ever, ending at laſt in a paralytic diſorder, which ſometimes affected the arm, ſometimes the leg, of the left ſide; in ſuch a manner as that theſe parts, tho' deprived of all motion for the time, yet ſtill retained their ſenſibility. In this condition ſhe remained till the ſpring 1756, when unexpectedly ſhe grew much better; but not ſo far as to get quite rid of her paralytic complaints; which, in cold weather, ſeldom failed to manifeſt themſelves by a numbneſs, trembling, ſenſation of cold, and a loſs of motion in the left ſide.

This

This paralytic tendency made her apprehenſive of a more violent attack; which accordingly ſoon happened: for, about the end of Auguſt, in the ſame year, her ſymptoms gradually increaſed, and in a very ſhort time ſhe loſt all motion and ſenſation in her left ſide. In this ſtate ſhe continued throughout laſt winter with the addition of ſome new complaints; for now her head ſhook conſtantly; her tongue faltered ſo much, when ſhe attempted to ſpeak, that ſhe could not articulate a word; her left eye grew ſo dim, that ſhe could not diſtinguiſh colours with it; and ſhe was often ſeized with ſuch an univerſal coldneſs and inſenſibility, that thoſe who ſaw her at ſuch times ſcarce knew whether ſhe was dead or alive.

Whilst the woman was in this miſerable condition, obſerving that ſhe had ſome intermiſſions, during which ſhe could converſe and uſe her right leg and arm, in one of thoſe intervals I propoſed trying to relieve her by the power of electricity. With this view, I got her ſupported in ſuch a manner as to receive the ſhocks ſtanding, holding the phial in her right hand, whilſt the left was made to touch the gun-barrel. After receiving ſeveral very ſevere ſhocks, ſhe found herſelf in better ſpirits than uſual; ſaid ſhe felt a heat, and a prickling pain, in her left thigh and leg, which gradually ſpread over all that ſide; and after undergoing the operation for a few minutes longer, ſhe cried out, with great joy, that ſhe felt her foot on the ground.

The electrical machine producing ſuch extraordinary effects, the action was continued; and that day the woman patiently ſubmitted to receive above 200

ſhocks from it. The conſequence was, that the ſhaking of her head gradually decreaſed, till it intirely ceaſed; that ſhe was able at laſt to ſtand without any ſupport; and on leaving the room quite forgot one of her crutches, and walked to the kitchin with very little aſſiſtance from the other. That night ſhe continued to be well and ſlept better than ſhe had done for ſeveral months before, only about midnight ſhe was ſeized with a faintiſhneſs, and took notice of a ſtrong ſulphureous taſte in her mouth; but both faintneſs and that taſte went off, upon drinking a little water. Next day, being electriſed as before, her ſtrength ſenſibly increaſed during the operation, and when that was over ſhe walked eaſily with a ſtick, and could lift ſeveral pounds weight with her left hand, which had been ſo long paralytic before. The experiment was repeated on the third day; by which time ſhe had received in all upwards of 600 ſevere ſhocks. She then telling us that ſhe had as much power in the ſide that had been affected as in the other, we believed it unneceſſary to proceed farther as the electricity had already, to all appearance produced a compleat cure. And indeed the patient continued to be well till the Sunday following, *viz.* about three days after the laſt operation; but upon going that day to church, ſhe probably catched cold; for on Monday ſhe complained of a numbneſs in her left hand and foot; but, upon being again electriſed, every ſymptom vaniſhed, and ſhe has been perfectly well ever ſince.

Coldingham, Nov. 1757. Patrick Brydone.

That

That the above is a true and exact account of my case, and of the late wonderful cure wrought on me, is attested by

Elizabeth Foster.

I was eye witness to the electrical experiments made by my son on Elizabeth Foster, and saw with pleasure their happy effects. By the blessing of God accompanying them, from a weak, miserable, and at sometimes almost an insensible state, she was, in a very short time, restored to health and strength; of which the above is in every respect a true account.

Robert Brydone,
Minister of Coldingham.

Extract of a Letter from Dr. Whytt *to Dr.* Pringle, *relating to this Account: Dated* Edinburgh, 1 *Dec.* 1757.

SOME days ago I had transmitted to me Mr. Brydone's account (inclosed) of the success of the electrical shocks in a paralytic patient, attested by the patient herself, and by Mr. Brydone's father, who is minister at Coldingham, in the shire of Berwick. At the same time I had a letter from the Reverend Mr. Allan, Minister of Eymouth (in the neighbourhood), informing me, that he had examined the patient particularly, and found Mr. Brydone's account to be perfectly true. He further informs me, that he never observed the electrical shock so strong from any machine, as from Mr. Brydone's. It seems, that gentleman has not only applied himself to the study of natural philosophy, but also of medicine.

Robert Whytt.

LI. *An Account of ſome foſſile Fruits, and other Bodies, found in the Iſland of* Shepey. *By* James Parſons, *M. D. F. R. S.*

To the Right Honourable the EARL *of* MACCLESFIELD, Preſident *of the* Royal Society.

My Lord, Sept. 25, 1757.

Read Dec. 15. 1757. BEING ever deſirous to promote the buſineſs of this learned Society, I could not loſe the opportunity that preſented, of laying before you an account, and drawings (*See* TAB. XV. & XVI.), of a moſt curious parcel of foſſil fruits, and ſome other bodies, ſent me from Shepey-Iſland, by my ingenious friend Mr. Jacob, of Faverſham, Surgeon, and Fellow of the Antiquary Society.

I do not remember, that foſſil ſeeds, or fruits, are recorded in our Tranſactions, tho' many of other kinds have places in them; nor indeed that the memoirs of other academies have made mention of any ſuch fruits; and therefore, as theſe are chiefly pyritical, and conſequently liable to fall to pieces, I thought it neceſſary to make drawings of them while in a ſound ſtate, in order for engraving, if the Society ſhall think fit; leſt their being ſo ſubject to moulder away might put it out of my power to preſerve their forms. However, I have great hopes I ſhall be able to preſerve the greater part of them intire till they are ſhewed to the Society.

In

In deſcribing theſe bodies, we ſhall be obliged to make the beſt conjectures we can of ſome of them only; for ſeveral are ſufficiently obvious to every naturaliſt, and eaſily known by comparing them to ſuch recent fruits, as are frequent enough among us. Some of them are abſolutely exotics; and indeed they are all rare and curious, and, in my humble opinion, well worth the notice of the Royal Society.

Doctor Woodward's catalogue *, which is ſo ample and full of all kinds of foſſil bodies, has only a very few fruits; and theſe are only ſome hazle nuts found in different places, a few pine-cones, and laryxes; and one fruit, which was taken for an unripe nutmeg. In this collection before us they are all very different, and ſuch as have not been ſeen before.

It will not be amiſs, in this place, to give a ſhort detail of ſuch bodies as are capable of either being petrified themſelves, or of leaving their impreſſions in ſtony matter. By being petrified, is meant being impregnated with ſtony, pyritical, or any other metalline or ſparry matter; for there are inumerable ſpecimens, wherein all theſe are apparent.

Testaceous *and* Crustaceous Animals.

The ſhelly matter of theſe is of ſo compact and dry a nature, that they will endure for ages: and if in a ſoil or bed where moiſture has acceſs, they will receive ſtony matter into their pores, and become ponderous in proportion to the quantity imbibed. If in a dry

* Since my writing this diſcourſe, Dr. Maſon informs me, that theſe are found no other than recent nuts and laryxes.

place

place, they will remain fair and ſharp, ſuffering very little change by any length of time; whilſt the fleſh of theſe, being ſubject to putrifaction, is ſoon deſtroyed; and yet, according to circumſtances that happen, ſome of theſe may be replaced in due form by ſtony particles. I have a gryphites, with the form of the fiſh in its place, as is the caſe in ſeveral of the oyſter kinds. This may be occaſioned by the ſhells being cloſe, or nearly ſo, and ſtony matter gradually inſinuating into their cavity, ſo as to fill up the whole.

Wood.

The kinds of wood found foſſil are very different: ſome are of a firmer texture than others: and this too is according to the places wherein they are depoſited. Some I have ſeen ſo highly impregnated with a fine ſtony and pyritical matter, as to bear a poliſh like a pebble; ſome, tho' quite reduced to ſtone, yet preſerving the fibrous appearance of the original ſtate; and ſome which is found in boggy bottoms, being not at all changed, except in color: this is called bog oak, or bog deal, well known to country people in many places of theſe three kingdoms, who light themſelves about their buſineſs with ſlips of this wood, cut on purpoſe inſtead of candles, as it burns with a clear and durable flame. It is remarkable, that altho' oak or fir ſhall lie ages immerſed in water under ground, it ſhall not putrify; but acquire ſuch ſulphureous particles by lying in ſteep, in the bog-water, as to qualify it for this uſe. Other wood, depoſited in marly ground, is found incruſted over, trunk and branches, with a white cruſt; the wood remaining intire within.

At

At other times, wood thus incrusted shall be eroded by the matter which covers it, having something acrimonious in its substance. We may add to these, clusters of the twigs of shrubs, and small wood, which we find flakes of, incrusted with sparry or calcarious matter, in many places; parts of which are totally changed into that matter, whilst others are only inveloped with it.

Bones *of* Animals.

We see, by every day's experience, that the human skeleton moulders to dust in a very few years, when buried in mould: so it does even in vaults, where the coffins are kept dry. In the first case, the moisture and salts of the earth divide and dissolve the texture of the bones; in the latter, those of the air, which gradually insinuate themselves into them, and at length destroy them. How long a skeleton whose bones are well dried and prepared, being totally deprived of its medullary substance, will last, as we now order them for anatomical purposes, we cannot say: but it may be reasonably conjectured, that they will undergo the fate of the softer kinds of wood, such as beech, which grows rotten in no great number of years; because their internal substance is spungy and cellular, and their crust is very thin, except about the middle of the bones of the arm and thigh, I mean the humerus and fœmur. The same destruction would happen, if bodies were deposited in a sandy soil; because water finds its way either by dripping downwards, or by springs underneath. But human skeletons have been found intire within a rock, where neither moisture

ture nor air could get at them. Mr. Minors, an eminent Surgeon and Anatomiſt of the Middleſex-hoſpital, when he was in the Army, at Gibraltar, ſaw an intire ſkeleton, ſtanding upright, in a dry rock, part of which had been blown up with gunpowder, in carrying on ſome works in the fortifications, which left the ſkeleton quite expoſed. Indeed, the bones of Elephants have been found in Shepey-Iſland, but much deſtroyed, ſeveral of which I have in my Collection; an account of which we have in the laſt volume but one * of our Tranſactions; their ſize and ſubſtance being ſo conſiderable, as to reſiſt for a long time that decay which thoſe of the human could not withſtand. To theſe we may add the horns of large animals, as the elk, and others, which have been found in bogs, preſerved as the bog-oak, *&c.* mentioned.

TEETH *and* PALATES *of* FISHES *and other Animals.*

Theſe are of ſo hard and firm a texture, as to ſuffer no great change, wherefoever found; for we ſee, that no eroſion appears in them, their enamel and its poliſh being intirely preſerved; yet ſometimes their roots will be found changed, eſpecially in the yellow ones, having no enamel to guard them in their roots.

Parts of VEGETABLES.

The leaves of plants, whoſe fibres are firm and dry, will endure for a long time; but thoſe of a ſucculent nature never can, as they putrify very ſoon. We ſee the leaves of ferns of ſeveral kinds, polypodium,

* Vol. xlviii.

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tricomanes, and other capillary plants, with nodules of ſtone formed about them; flags, reeds, ruſhes, equiſetum, and many ſuch, of a firm texture, are found in ſlate and ſtone; and even the iuli of trees are ſaid to have been found foſſil as well as their leaves.

Seeds *and* Fruits.

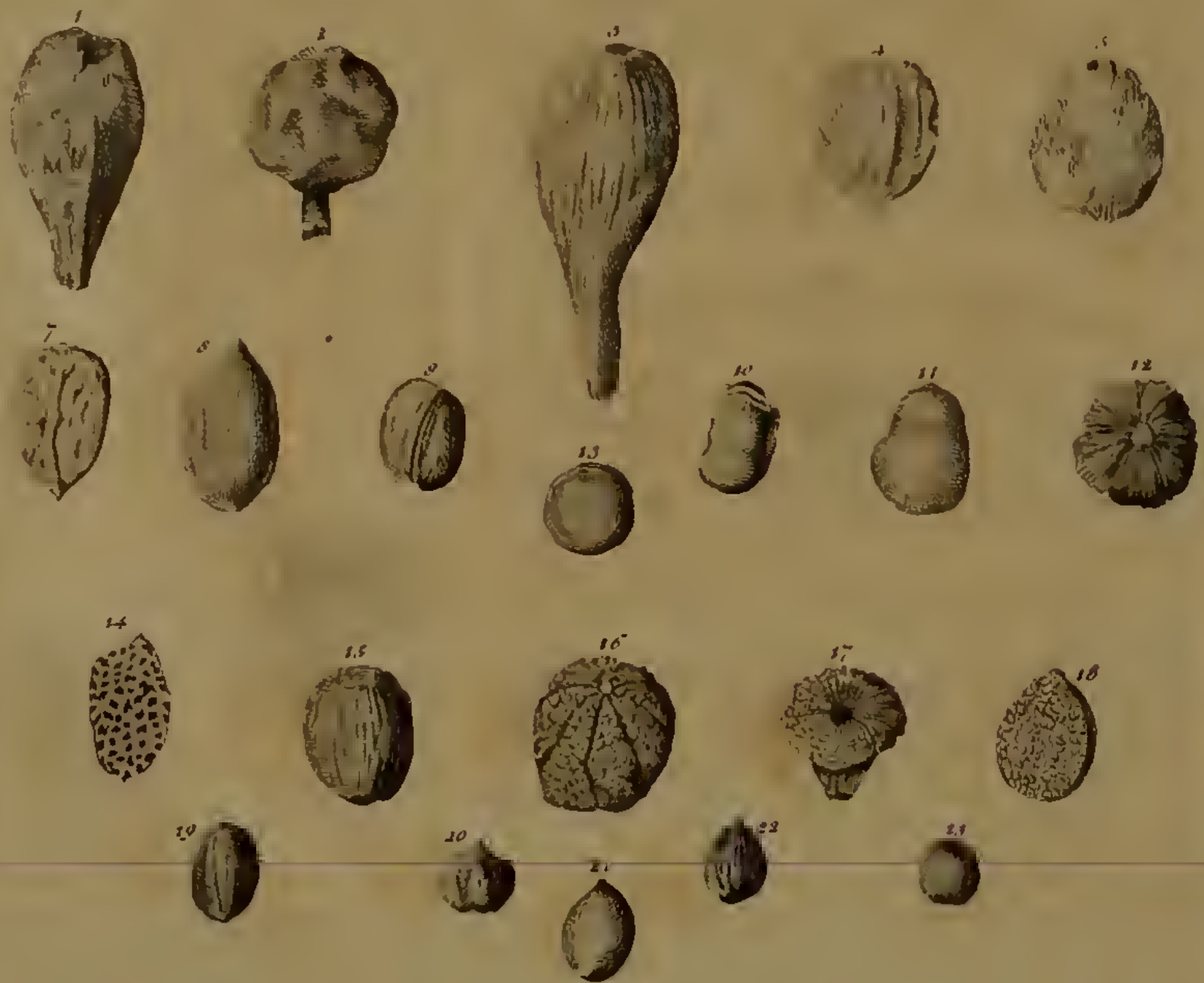
All ſeeds and the ſtones of fruits, having a firm texture, are alſo capable of being ſtrongly impregnated with ſtony and pyritical matter; and I make no doubt but that the ſmaller ſeeds, if careſully looked for, might be found foſſil, as well as theſe before you; ſuch, I mean, as have a firmneſs in the covering; but being ſmall, and mixt with the dirt, ſand, and the like, probably is the reaſon of their being overlooked. Fruits of various kinds are found petrified; but this is only in their green ſtate, when they are hard enough to endure till they are impregnated with ſtony or mineral particles. The rudiments of fruits, when once well formed, and a little advanced, are firm and acid: and the more remote they are from maturity, the more ſecure from putrifaction; and their acid juice is no ſmall help to their preſervation from growing ſoon rotten. But indeed, when the fruit advances in growth, the texture grows gradually more lax; the acid juices are now beginning to be replaced by ſaccharine or others more ſoft; the fibres are driven farther aſunder, and they now arrive at their moſt ripe ſtate: and the utmoſt maturity of fruits is the next ſtep to putrifaction. Hence they are deſtroyed before ſtony or other particles can have time enough to impregnate them: and this is exactly the caſe with the fleſh of animals of every kind. The

huſks and hard calyces of fruits, as well as their ſtones, are alſo ſuſceptible of petrifaction.

If theſe fruits, which I have the honour to lay before you, are antediluvian, one would be apt to imagine they, in ſome meaſure, point out, with Dr. Woodward, the time of year in which the deluge began; which he thinks was in May: and yet this very opinion is liable to ſome objections; becauſe altho' fruits capable of being petrified, from their green ſtate, may be pretty well formed in May here, as well as in the ſame latitude elſewhere, in favour of this opinion; yet there are the ſtones of fruits, found foſſil, ſo perfect, as to make one imagine they were very ripe, when depoſited in the places where they are diſcovered; which would induce one to think the deluge happened nearer Autumn, unleſs we could think them the productions of more ſouthern latitudes, where perhaps their fruits are brought to perfection before ours are well formed.

What follows is a catalogue of theſe foſſil fruits &c. before you: and I ſhould be glad, if any of the gentlemen would take the trouble of examining them, in order to aſſiſt in our conjectures about ſuch of them, as appear doubtful: but firſt beg leave to inſert the following remark:

I cannot omit an obſervation of Doctor Maſon, Woodwardian profeſſor, in this place; which is well worth notice, and indeed which I never attended to. It regards the impreſſions of fiſhes upon ſlate. Now there are ſeveral kinds of ſlate, which have ſuch impreſſions upon them: in ſome there remains only the bare impreſſion, without any part of the fiſh; in others the ſcales only, but retaining the intire form of the animal;



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animal; and in others no part adheres to the ſlate, but the ſkeleton, or part of it, moſt commonly the ſpine. He ſays that he always obſerved, that the bones are never ſeen but upon the grey or blue ſlate, or their impreſſions; and that the ſcales or ſkin are to be found only upon the black ſtone or ſlate; which makes him conjecture, that ſomething eroſive in the grey ſlate deſtroys every part but the bony ſyſtem; but that the black, being of a more ſoft and unctuous nature, preſerves the ſcales, and often the very ſkin. This, however, muſt bereferred to further obſervation.

TAB. XV.

Fig. 1. 3. Theſe two bodies ſeem to be figs, petrified when hard and green; being, as I have juſt obſerved, then capable of receiving the pyritical particles, with which they are manifeſtly impregnated. One is more perfect in its form than the other; and they are now ſhooting their ſalts, and will ſoon fall to pieces.

Fig. 2. appears to be a Myrobalan, diſtinguiſhed from the other ſpecies of that name by its round figure; and is called the belleric Myrobalan. It is nearly deſtroyed by the pyritical matter, and will not long remain whole.

Fig. 4. ſeems to be a ſpecies of Phaſeolus, one of thoſe eſpecially diſtinguiſhed by the fruits. *Fructibus ſplendentibus nigris.*

Fig. 5. Another Phaſeolus.

Fig. 7. Another. See *Fig.* 4.

Fig. 8. Semen Cucurbitæ, a large ſpecies of American gourd.

Fig. 9. Coffee-berries.

Fig. 10, 11. Two ſpecies of Beans, very apparent.

Fig. 12. Unknown. This, however, appears to be a fruit, with the calyx running up, and embracing it, in its hard green ſtate; being ſomewhat compreſſed on the upper part, as it lay confined in the earth.

Fig. 13. *An Staphilodendri ſpecies?* The learned and reverend Dr. Hales gave me, ſome years ago, a handful of the recent fruits, one or two of which are ſent with this foſſil one, for your conſideration. He had them from Bengal, and called them, in the Indian name, Neermelis; and ſaid the natives uſed them to fine down liquors.

Fig. 14. A compreſſed pod of the Arachidna, or Underground-Pea. The full-grown pods are much larger, but of various ſizes, as are other kinds. This, however, ſeems to have been, when depoſited where it was found, not ſo far advanced. It has the reticulated ſurface, the apex on one ſide, and every other character of that fruit or ſeed-pod, but ſomewhat compreſſed.

Fig. 15. is evidently an Acorn. We have of this ſpecies here, and in America alſo.

Fig. 16. An exotic fruit, like a ſmall melon; but uncertain. It is ſomewhat deformed by compreſſion.

Fig. 17. This I took at firſt for a fruit; but now I rather believe it a Fungoides of a very pretty kind.

Fig. 18.

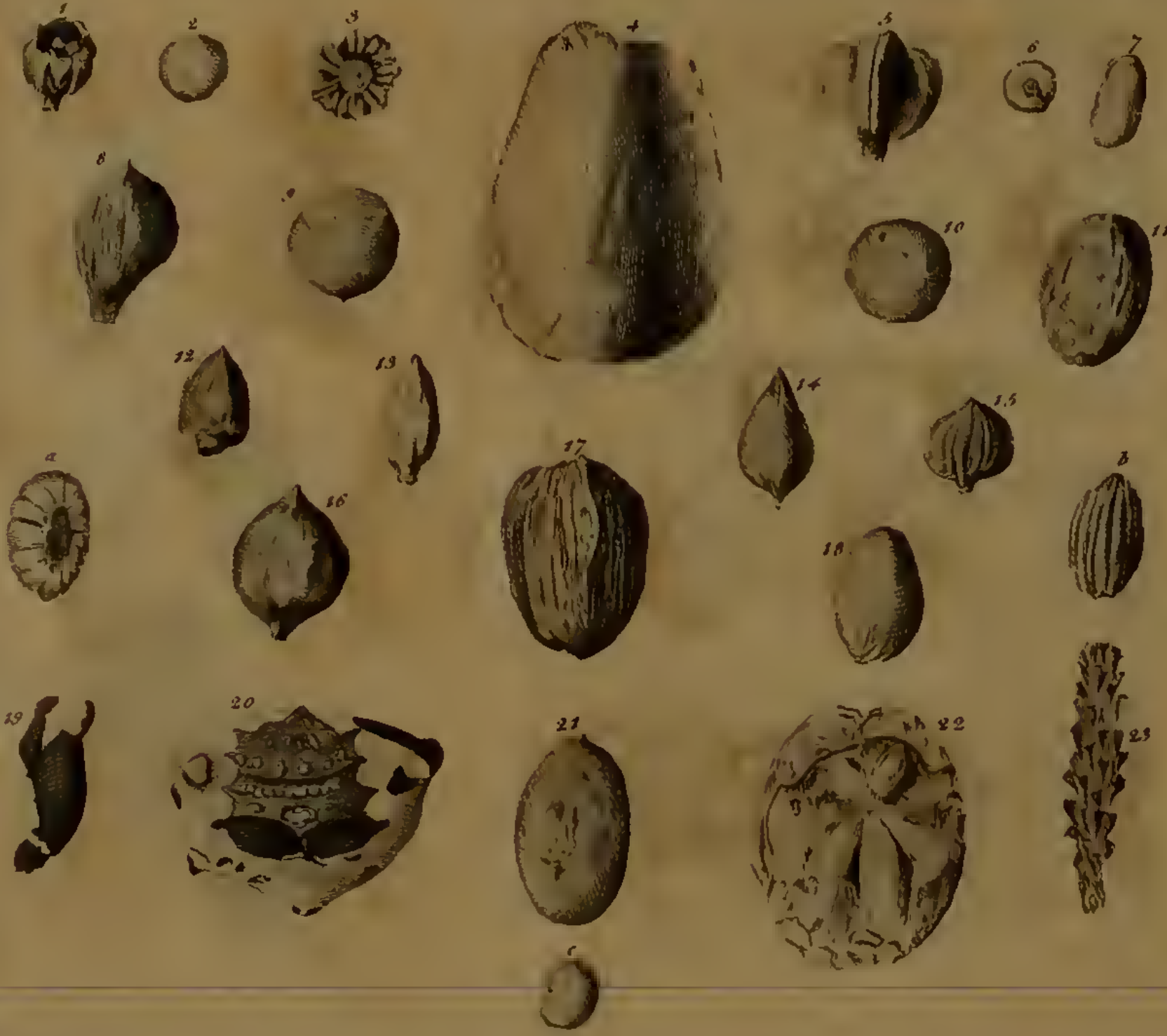

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Fig. 18. *An Anguria?* I take it for a ſeed of a ſpecies of water-melon.

Fig. 19. ſeems a ſmall plumb-ſtone.

Fig. 20. Unknown. The calyx ſeems to run up and embrace this fruit towards the apex.

Fig. 21. Unknown. This reſembles an American ſeed, which I have in my collection, but do not know its name. Its apex is inclining to one ſide; and it appears to have had a ſtrong pedicle.

Fig. 22. *An Lachryma Jobi?*

Fig. 23. A Cherry-ſtone.

TAB. XVI.

Fig. 1. *An Euonymi ſpecies?* If this be an Euonymus, it is not ſo far advanced as to form the ſeeds; and is therefore to be conſidered only in its progreſs from the flower towards ſeeding: which is the caſe in ſeveral of theſe, whoſe calyces appear ſtill upon them, and hinder us from abſolutely determining what they are.

Fig. 2. A berry of the Sapindus, or Soap-tree, of America, being not at all deformed, only having a little lump of pyrites upon it: but there is another quite free.

Fig. 3. *Huræ Germen.* This is undoubtedly the young Sand-box, or fruit of the Hura, ſo well known for its beautiful form to the curious, who collect ſpecimens of natural hiſtory; and ſeems to ſhew the time of the deluge.

Fig. 4. This, I think, is certainly the ſtone of an eaſtern Mango; ſuch as comes over to us pickled,

pickled, and, the ſtone being opened on one ſide, is generally ſtuffed with ſpices.

Fig. 5. *Euonymi latifolii ſpecies.* This is a large ſpecies of Euonymus, perhaps of Cluſius.

Fig. 6. This body ſeems to be a Milleped, or Wood-louſe. It is turned round, the two extremities meeting; which is the attitude aſſumed by theſe animals, upon being in any-wiſe obſtructed in their paſſage, or handled.

Fig. 7. A ſmall long Bean, like our horſe-bean; but longer than any we have in England.

Fig. 8. Unknown to me.

Fig. 9. A ſpecies of Horſe-cheſnut from America.

Fig. 10. The external huſk of the fruit of the Sapindus, or Soap-tree.

Fig. 11. I cannot determine whether this be an Olive, or the yellow Myrobalan; but believe it the Myrobalan.

Fig. 12. *An Palmæ ſpecies?* It ſeems a ſmall Palma-coco.

Fig. 13, 14. unknown, as well as *fig.* 15.

Fig. 16. Unknown. The reaſon of the four laſt being not to be diſtinguiſhed is, that they ſeem to be the buds of their ſeveral ſpecies, before they were perfectly formed. So that while ſome of the antediluvian productions are mature, others appear to be premature; and conſequently one would be inclined to think them the inhabitants of places of different latitudes.

Fig. 17. A ſpecies of foreign Walnut, injured and compreſſed.

Fig. 18. A Plumb-ſtone.

Fig. 19. The claw of an American Crab; which,

being on the oppoſite ſide of the maſs containing the body, could not come in view with it at the ſame time.

Fig. 20. The body of the crab, with other parts, appearing thro' the ſtony matter that invelopes it, which appears to be an induration of yellow clay.

Fig. 21. ſeems a long American Phaſeolus. Part of the petrified huſk is upon it.

Fig. 22. An American Echinite of the flat kind, much reſembling that ſpecies which Rumphius calls *Echinus ſulcatus primus.*

Fig. 23. *Ariſta cujuſdam Graminis.* This body has all the characteriſtics of an ear of corn, or ſome ſpecies of graſs, of which there are many.

This has been taken for a ſpine of an Echinus: but, as we are to conſider its neareſt reſemblance to whatſoever body, we muſt conclude it as we have ſaid. I never ſaw any ſpine in the leaſt like it; but an ear of corn, ripe and dry, is as ſuſceptible of being petrified, as a cruſtaceous animal, in every reſpect. Indeed the ſpiculæ of the ear, each ariſing from the grain, being very ſlender, are of courſe deſtroyed during the petrifaction; but the form of the ear is actually preſerved, as much as the nature and circumſtances of the thing will allow.

Fig. *a.* A manifeſt ſpecies of Pediculus Marinus crumped up.

b. A Seed-veſſel, given me by Mr. Da Coſta, found in a clay-pit in Staffordſhire.

c. Cocculus Indicus.

LII.

LII. *Obſervations upon the Comet that appeared in the Months of* September *and* October 1757, *made at the Royal Obſervatory by* Ja. Bradley, *D. D. Aſtronomer Royal, F. R. S. and Member of the Royal Academy of Sciences at* Paris.

Read Dec. 22, 1757.

I Deferred to give an account of my obſervations upon the Comet that hath lately appeared, till I could ſettle the places of the ſtars with which it had been compared; ſeveral of them not being inſerted in the Britiſh catalogue, and thoſe which are, requiring ſome ſmall corrections, which I have ſince made from my own obſervations.

When I firſt diſcovered this Comet, it appeared to the naked eye like a dull ſtar of the 5th or 6th magnitude; but viewing it thro' a ſeven-foot Teleſcope, I could perceive a ſmall Nucleus (ſurrounded, as uſual, with a nebulous atmoſphere), and a ſhort tail extended in a direction oppoſite to the ſun.

Some ſmall ſtars then appearing in the field of the teleſcope with the Comet, I meaſured its diſtance from them with a Micrometer; and on September 12d at 16h 2′ mean time, I found it to be 1° 13′ 5″ diſtant from a ſmall ſtar, whoſe right aſcenſion was afterwards found to be 89° 49′ 40″ and declination 36° 11′ 30″ north: and near the ſame time the Comet was obſerved to be 43′ 10″ from another ſtar, whoſe right aſcenſion was 90° 20′ 0″ and declination 35° 12′ 0″ north.

Hence

Hence I collected, that the Comet's right aſcenſion was 89° 29′ 10″ and its declination 35° 0′ 20″ north.

September 13d 12h 37′ mean time (which is likewiſe made uſe of in the following obſervations), the Comet had the ſame right aſcenſion with a ſmall ſtar, whoſe right aſcenſion was 93° 5′ 30″ and declination 34° 36′ 40″ north; and it was about two minutes more northerly than the ſtar. Hence the Comet's right aſcenſion was 93° 5′ 30″ and its declination 34° 38′ 40″ north.

September 14d 14h 0′ the Comet preceded θ Geminorum 1° 31′ 35″ in right aſcenſion, and was 11′ 35″ more ſoutherly. The apparent right aſcenſion of θ Geminorum was then 99° 11′ 40″ and its declination 34° 13′ 25″ north. Hence the right aſcenſion of the Comet was 97° 40′ 5″ and its declination 34° 1′ 50″ north.

Sept. 17d 13h 0′ a ſmall ſtar (whoſe right aſcenſion was 109° 55′ 20″ and declination 31° 27′ 40″) preceded the Comet 47′ 10″ in right aſcenſion, and was 12′ 30″ more northerly. Hence the Comet's right aſcenſion was 110° 42′ 40″ and its declination 31° 15′ 10″ north.

Sept. 19d 15h 17′ a ſtar (whoſe right aſcenſion was 118° 29′ 40″ and declination 28° 9′ 45″) preceded the Comet 1° 14′ 0″ in right aſcenſion, and was more ſoutherly 15′ 45″. Hence the Comet's right aſcenſion was 119° 43′ 40″ and declination 28° 25′ 30″ north.

Sept. 23d 15h 57′ a ſtar (whoſe right aſcenſion was 134° 55′ 45″ and declination 22° 15′ 55″ north) preceded the Comet 12′ 30″ in right aſcenſion, and

 was

was 29′ 0″ more northerly. Hence the Comet's right ascension was 135° 8′ 15″ and its declination 21° 46′ 55″ north.

Sept. 24d 15h 21′ the Comet had the same declination with a small star that preceded it 10′ 15″ in right ascension. This star's right ascension was afterwards found to be 138° 13′ 45″ and its declination 20° 5′ 20″. Hence the Comet's right ascension was 138° 24′ 0″ and its declination 20° 5′ 20″ north.

Sept. 28d 16h 22′ the Comet followed Regulus 1° 7′ 12″ in right ascension, and was 14′ 45″ more northerly. The right ascension of Regulus being then 148° 51′ 13″ and its declination 13° 8′ 35″ north; the Comet's right ascension was 149° 58′ 25″ and its declination 13° 23′ 20″ north.

Sept. 30d 16h 24′ ρ Leonis (whose right ascension was 155° 0′ 10″ and declination 10° 32′ 53″ north) followed the Comet 18′ 45″ in right ascension, and was 7′ 53″ more northerly. Hence the Comet's right ascension was 154° 41′ 25″ and its declination 10° 25′ 0″ north.

October 2d 16h 48′ the 37th star Sextantis. Hevel. in the British Catalogue (whose right ascension was 158° 21′ 25″ and declination 7° 38′ 40″ north) preceded the Comet 32′ 50″ in right ascension, and was 3′ 20″ more southerly. Hence the Comet's right ascension was 158° 54′ 15″ and its declination 7° 42′ 0″ north.

October 3d 16h 45′ c Leonis (whose right ascension was 162° 2′ 15′ and declination 7° 24′ 0″ north) followed the Comet 1° 12′ 55″ in right ascension, and was 56′ 40″ more northerly. Hence the Comet's right ascension was 160° 49′ 20″ and its declination 6° 27′ 20″ north.

Octo-

October 4d 17h 0′ *d* Leonis (whose right ascension was 162° 0′ 15″ and declination 4° 54′ 57″ north) preceded the Comet 40′ 15″ in right ascension, and was more southerly 20′ 53″. Hence the Comet's right ascension was 162° 40′ 30″ and its declination 5° 15′ 50″ north.

October 7d 16h 54′ the 79th Leonis in the British Catalogue (whose right ascension was 167° 53′ 37″ and declination 2° 44′ 15″ north) followed the Comet 13′ 0″ in right ascension, and was more northerly 38′ 35″. Hence the Comet's right ascension was 167° 40′ 37″ and its declination 2° 5′ 40″ north.

October 8d 16h 53′ the Comet preceded *v* Leonis 1° 53′ 30″ in right ascension, and was 37′ 20″ more northerly. The right ascension of this star was 171° 7′ 45″ and its declination 0° 30′ 55″ north; therefore the Comet's right ascension was 169° 14′ 15″ and its declination 1° 8′ 15″ north.

October 11d 16h 52′ the Comet followed *v* Leonis 2° 33′ 30″ in right ascension, and appeared 1° 55′ 5″ more southerly; but it being near the horizon, the difference of right ascension must have been contracted by refraction about 1′ 5″, and the difference of declination about 1′ 30″: so that the corrected right ascension of the Comet was 173° 42′ 20″ and its declination 1° 25′ 40″ south.

Immediately after this observation a fog arose, which prevented me from repeating it; and several mornings following proving hazy or cloudy, I could not see the Comet again till October 18th, about an hour and a quarter before sun-rising; when the twilight being strong, and the Comet low, it appeared very faint. However, I was unwilling to omit the

opportunity of determining its place, as near as I could, by a ſingle obſervation, in the following manner.

At 6^h $59'$ $54''$ ½ ſidereal time, I obſerved the paſſage of the Comet over the perpendicular wire of my equatorial Sector; then leaving the inſtrument in the ſame poſition till the next evening, I obſerved, that at 22^h $8'$ $15''$ ſidereal time, the 17th ſtar of Eridanus in the Britiſh Catalogue paſſed over the ſame wire (or horary circle) $9'$ $30''$ more ſoutherly than the Comet. And at 23^h $45'$ $36''$ ſidereal time the ſtar marked *h* in Eridanus paſſed, $19'$ $55''$ more northerly than the Comet.

I found that the ſituation of my inſtrument was not ſenſibly altered between the 18th and 19th of October; for the tranſits and the difference of declination of the ſame ſtars being obſerved with it again on the 19th of October, they agreed very well with thoſe that were taken the preceding night. It may therefore be ſuppoſed, that the poſition of the inſtrument continued the ſame likewiſe during the time of the foregoing obſervations.

The right aſcenſion of the 17th ſtar of Eridanus being $49°$ $39'$ $10''$ and its declination $5°$ $55'$ $25''$ ſouth; and the right aſcenſion of *h* of Eridanus being $73°$ $59'$ $25''$ and its declination $5°$ $25'$ $10''$ ſouth; I collected, that when the Comet paſſed the wire (or horary circle) which was October 17^d 17^h $12'$ mean time, its right aſcenſion was $182°$ $34'$ $0''$ and its declination $5°$ $45'$ $35''$ ſouth.

The laſt time that I ſaw the Comet was on the 19th of October in the morning; but it then appeared ſo faint, that I could not obſerve its place. Its elongation from the ſun was then but about 20 degrees;

degrees; and from that day to the preſent it hath always been leſs; which is the principal reaſon why it was inviſible to us at the time when it was in its perihelion, and hath remained ſo ever ſince. The elongation will indeed ſoon become greater, and yet it is probable that we ſhall not be able to ſee the Comet again; becauſe its real diſtance from the ſun will be greater than it was when I firſt ſaw it, and it will be alſo four times further from us than it was at that time.

The Comet kept nearly at the ſame diſtance from the earth for ten or twelve days together after I firſt ſaw it; but its brightneſs gradually increaſed then, becauſe it was going nearer to the ſun. Afterwards, when its diſtance from the earth increaſed, altho' it continued to approach the ſun, yet its luſtre never much exceeded that of ſtars of the ſecond magnitude, and the tail was ſcarce to be diſcerned by the naked eye.

All the forementioned obſervations were made with a Micrometer in a ſeven-foot Tube, excepting thoſe of the 3d, 11th, and 17th days of October, which were taken with a curious Sector conſtructed for ſuch purpoſes by the late ingenious Mr. George Graham; of which Dr. Smith has given a very exact deſcription in his third book of Optics.

Suppoſing the Trajectory of this Comet to be parabolic, I collected from the foregoing obſervations, that its motion round the ſun is *direct*, and that it was in its *perihelion* October the 21ſt, at $7^h\ 55'$ mean (or equated) time at Greenwich. That the inclination of the plane of its Trajectory to the ecliptic is $12°\ 50'\ 20''$; the place of the deſcending Node ☋ $4°$

12′

12′ 50″; the place of the Perihelion ☊ 2° 58′ 0″; the diſtance of the Perihelion from the deſcending Node 88° 45′ 10″; the Logarithm of the Perihelion diſtance 9.528328; the Logarithm of the diurnal motion 0.667636.

From theſe Elements (which are adapted to Dr. Halley's general Table for the Motion of Comets in parabolic Orbits), I computed the places of this Comet for the reſpective times of the foregoing obſervations, as in the following table; which contains likewiſe the longitudes and latitudes deduced from the obſerved right aſcenſions and declinations, and alſo the differences between the computed and obſerved places. Theſe differences (no-where exceeding 40″) ſhew, that the elements here ſet down will be ſufficient to enable future aſtronomers to diſtinguiſh this Comet upon another return; but as they do not correſpond with the elements of the orbit of any other Comet hitherto taken notice of, we cannot determine at preſent the period thereof.

Greenwich,

Greenwich, 1757. Mean Time.				Comet. Long. Obſerv.				Latit. Obſerv.				Long. Comp.				Latit. Comput.				Diff. Long.		Diff. Latit.	
	d.	h.	′	S.	°	′	″	°	′	″		S.	°	′	″	°	′	″			″		″
Sept.	12	16	2	♊	29	34	13	11	32	16	No.	♊	29	34	11	11	32	20	No.	—	2	+	4
	13	12	37	♋	2	35	34	11	12	13		♋	2	35	47	11	12	11		+	13	—	2
	14	14	0		6	27	45	10	44	3			6	27	42	10	43	43		—	3	—	20
	17	13	0		17	49	40	9	3	31			17	50	16	9	3	11		+	36	—	20
	19	15	17		26	6	8	7	36	49			26	5	50	7	36	30		—	18	—	19
	23	15	57	♌	11	19	18	4	33	38		♌	11	19	4	4	33	32		—	14	—	6
	24	15	21		14	44	19	3	49	37			14	44	3	3	49	39		—	16	+	2
	28	16	22		27	23	43	1	3	44	No.		27	23	32	1	3	52	No.	—	11	+	8
	30	16	24	♍	2	45	43	0	5	30	So.	♍	2	45	39	0	5	17	So.	—	4	—	13
Octob.	2	16	48		7	37	43	1	5	50			7	37	42	1	5	32		—	1	—	18
	3	16	45		9	51	36	1	32	22			9	51	29	1	31	55		—	7	—	27
	4	17	0		12	1	4	1	56	42			12	0	25	1	56	23		—	39	—	19
	7	16	54		17	51	3	2	56	48			17	51	6	2	56	24		+	3	—	24
	8	16	53		19	39	45	3	13	7			19	39	33	3	12	28		—	12	—	39
	11	16	52		24	47	22	3	48	49			24	47	47	3	49	29		+	25	+	40
	17	17	12	♎	4	38	58	4	15	42	So.	♎	4	38	36	4	15	2	So.	—	22	—	40

LIII. *The Resolution of a General Proposition for Determining the* horary *Alteration of the Position of the Terrestrial Equator, from the Attraction of the Sun and Moon: With some Remarks on the Solutions given by other Authors to that difficult and important Problem. By Mr.* Tho. Simpson, *F. R. S.*

Read Dec. 22, 1757.

SINCE the time, that that excellent Astronomer, my much honoured friend Dr. Bradley, published his observations and discoveries concerning the inequalities of the precession of the equinox, and of the obliquity of the ecliptic, depending on the position of the lunar nodes, mathematicians, in different parts of Europe, have set themselves diligently to compute, from physical principles, the effects produced by the sun and moon, in the position of the terrestrial equator; and to examine whether these effects do really correspond with the observations

Two papers on this subject have already appeared in the Philosophical Transactions; in which the authors have shewn evident marks of skill and penetration. There is, nevertheless, one part of the subject, that seems to have been passed over without a due degree of attention, as well by both those gentlemen, as by Sir Isaac Newton himself.

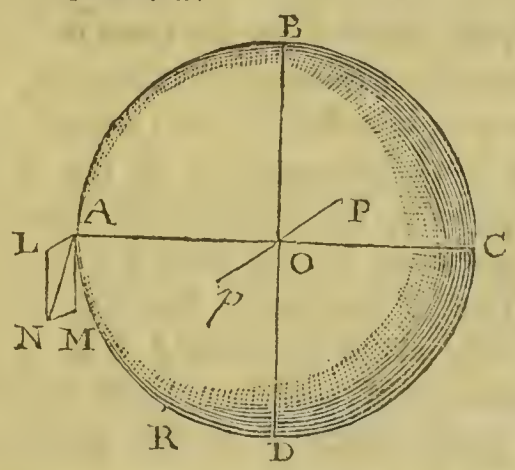
This part, which, upon account of physical difficulties, is indeed somewhat slippery and perplexing, I shall make the principal subject of this essay.

GENE-

General Proposition.

Supposing an homogeneous sphere OABCD (Fig. 1.) *revolving uniformly about its centre, to be acted on at the extremity* A *of the radius* OA, *in a direction* AL *perpendicular to the plane of the equator* ABCD, *and parallel to the axis of rotation* Pp, *by a given force, tending to generate a new motion of rotation at right angles to the former; It is proposed to determine the change, that will arise in the direction of the rotation in consequence of the said force.*

Fig. 1.



Let F denote the given force, whereby the motion about the axis Pp is disturbed, supposing f to represent the centrifugal force of a small particle of matter in the circumference of the equator, arising from the sphere's rotation; and let the whole number of such particles, or the content of the sphere, be denoted by c: let also the momentum of rotation of the whole sphere, or of all the particles, be supposed, in proportion to the momentum of an equal number

of particles, revolving at the diſtance O A of the remoteſt point A, as n is to *unity*.

It is well known, that the centripetal force, whereby any body is made to revolve in the circumference of a circle, is ſuch, as is ſufficient to generate all the motion in the body, in a time equal to *that*, wherein the body deſcribes an arch of the circumference, equal in length to the radius. Therefore, if we here take the arch A R = O A, and aſſume m to expreſs the time, in which that arch would be uniformly deſcribed by the point A, the *motion* of a particle of matter at A (whoſe central force is repreſented by f) will be equal to *that*, which might be uniformly generated by the force f, in the time m; and the motion of as many particles (revolving, all, at the ſame diſtance) as are expreſſed by cn (which, by hypotheſis, is equal to the momentum of the whole body), will, conſequently, be equal to the momentum, that might be generated by the force $f \times cn$, in the ſame time m. Whence it appears, that the momentum of the whole body about its axe Pp is in proportion to the momentum generated in a given particle of time m', by the given force F in the direction A L, as $ncf \times m$ is to $F \times m'$, or, as *unity* to $\frac{F}{ncf} \times \frac{m'}{m}$ (becauſe the quantities of motion produced by unequal forces, in unequal times, are in the ratio of the forces and of the times, conjunctly). Let, therefore, A L be taken in proportion to A M, as $\frac{F}{ncf} \times \frac{m'}{m}$ is to *unity* (ſuppoſing A M to be a tangent to the circle A B C D in A), and let the parallelogram A M N L be compleated; drawing alſo the diagonal A N; then, by the

the compoſition of forces, the angle NAM (whoſe tangent, to the radius OA, is expreſſed by $OA \times \frac{F}{ncf} \times \frac{m'}{m}$) will be the change of the direction of the rotation, at the end of the aforeſaid time (m'). But, this angle being exceeding ſmall, the tangent may be taken to repreſent the meaſure of the angle itſelf; and, if Z be aſſumed to repreſent the arch deſcribed by A, in the ſame time (m') about the center O, we ſhall alſo have $\frac{m'}{m} = \frac{Z}{AR} = \frac{Z}{AO}$, and conſequently $OA \times \frac{F}{ncf} \times \frac{m'}{m} = Z \times \frac{F}{ncf}$. From whence it appears, that the angle expreſſing the change of the direction of the rotation, during any ſmall particle of time, will be in proportion to the angle deſcribed about the axe of rotation in the ſame time, as $\frac{F}{ncf}$ is to *unity*. *Q. E. I.*

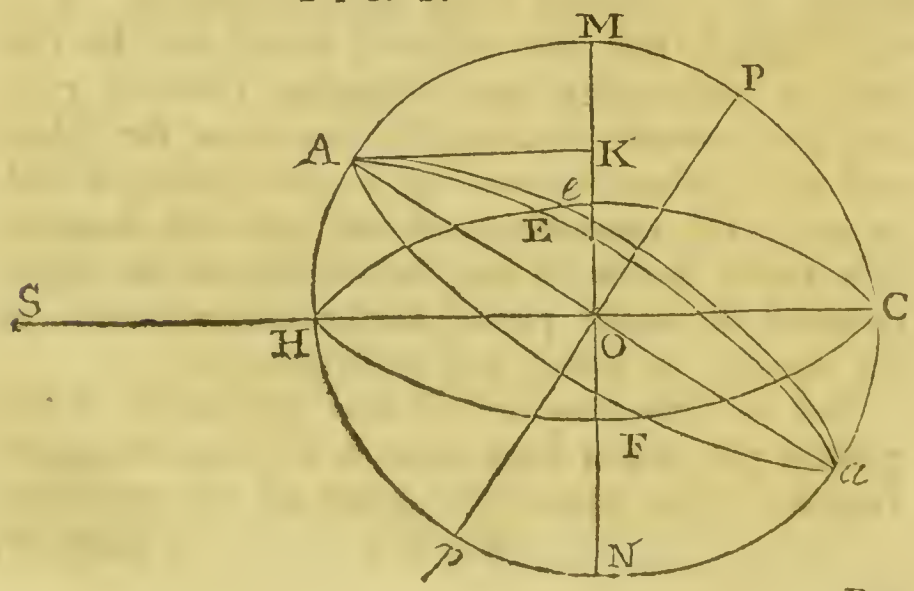
Altho', in the preceding propoſition, the body is ſuppoſed to be a perfect ſphere, the ſolution, nevertheleſs, holds equally true in every other ſpecies of figures, as is manifeſt from the inveſtigation. It is true, indeed, that the value of n will not be the ſame in theſe caſes, even ſuppoſing thoſe of c, f and F to remain unchanged; except in the ſpheroid only, where, as well as in the ſphere, n will be $= \frac{2}{5}$; the momentum of any ſpheroid about its axis being 2-5ths of the momentum of an equal quantity of matter placed in the circumference of the equator, as is very eaſy to demonſtrate.

But to ſhew now the uſe and application of the general proportion here derived, in determining the regreſs of the equinoctial points of the terreſtrial

 ſpheroid,

ſpheroid, let A E *a* F (*Fig.* 2.) be the equator, and P *p* the axis of the ſpheroid: alſo let H E C F repreſent the plane of the ecliptic, S the place of the ſun, and H A P N H the plane of the ſun's declination, making right-angles with the plane of the equator A E *a* F: then, if AK be ſuppoſed parallel, and O K M perpendicular, to O S, and there be aſſumed T and t to expreſs the reſpective times of the annual and diurnal revolutions of the earth, it will appear (from the *Principia*, B. III. prop. xxv.) that the force, with which a particle of matter at A tends to recede from the line O M in conſequence of the ſun's attraction, will be expreſſed by $\frac{3tt}{TT} \times \frac{AK}{OA} \times f$; f denoting the centrifugal force of the ſame particle, ariſing from the diurnal rotation. Hence, by the reſolution of forces, $\frac{3tt}{TT} \times \frac{AK}{OA} \times \frac{OK}{OA} \times f$ will be the effect of that particle, in a direction perpendicular to O A, to turn the earth about its center O.

FIG. 2.



But

But it is demonſtrated by Sir Iſaac Newton, and by other authors, that the force of all the particles, or of all the matter in the whole ſpheroid A P *a p*, to turn *it* about its center, is equal to $\frac{1}{5}$th of the force of a quantity of matter, placed at A, equal to the exceſs of the matter in the whole ſpheroid above *that* in the inſcribed ſphere, whoſe axis is P *p*. Now this exceſs (aſſuming the ratio of π to 1, to expreſs *that* of the area of a circle to the ſquare of the radius) will be truly repreſented by $\frac{4\pi}{3} \times OP \times \overline{OA^2 - OP^2}$; and, conſequently, the force of all the matter in the whole earth, by $\frac{3tt}{TT} \times \frac{AK}{OA} \times \frac{OK}{OA} \times \frac{4\pi}{15} \times OP \times \overline{OA^2 - OP^2}$. Let, therefore, this quantity be now ſubſtituted for *F*, in the general formula $\frac{F}{ncf}$, writing, at the ſame time, $\frac{4\pi}{3} \times OA^2 \times OP$, and $\frac{2}{5}$, in the place of their equals *c* and *n*; by which means we have (here) $\frac{F}{ncf} = \frac{3tt}{2TT} \times \frac{OA^2 - OP^2}{OA^2} \times \frac{AK \times OK}{OA^2}$. Put the given quantity $\frac{3tt}{2TT} \times \frac{OA^2 - OP^2}{OA^2} = k$; and let the angle E A *e* repreſent the horary alteration of the poſition of the terreſtrial equator, ariſing from the force *F* (here determined), and let the arch E *e* be the regreſs of the equinoctial point E, correſponding thereto: then, in the triangle E A *e* (conſidered as ſpherical) it will be ſin. *e* : ſin. A E (: : ſin. E A *e* : ſin. E *e*) : : E A *e* : E *e* $\left(= \frac{\text{ſin. AE} \times \text{EA}e}{\text{ſin. E}}\right) = k \times \frac{\text{ſin. AE}}{\text{ſin. E}} \times \frac{AK \times OK}{OA^2} = k \times \frac{\text{ſin. AE} \times \text{coſ. AH} \times \text{ſin. AH}}{\text{ſin. E}}$. But in the triangle E H A, right-angled at A (where H A is ſuppoſed

ſuppoſed to repreſent the ſun's declination, AE his right aſcenſion, and HE his diſtance from the equinoctial point E *) we have *(per ſpherics)*

$$\text{ſin. AE} : 1 \text{ (rad.)} :: \text{co-t. E} : \text{co-t. AH},$$

$$\overline{\text{ſin. AH}}^2 : \overline{\text{ſin. EH}}^2 :: \overline{\text{ſin. E}}^2 : 1^2 (\overline{\text{rad.}}^2)$$

From whence we get, ſin. AE × co-t. AH × $\overline{\text{ſin. AH}}^2$ = $\overline{\text{ſin. EH}}^2$ × co-t. E × $\overline{\text{ſin. E}}^2$. But co-t. AH × ſin. AH = co-ſ. AH × 1 (rad.), and co-t. E × ſin. E = co-ſ. E × 1 (rad.): therefore ſin. AE × co-ſ. AH × ſin. AH = $\overline{\text{ſin. EH}}^2$ × co-ſ. E × ſin. E; and, conſequently, $k \times \frac{\text{ſin. AE} \times \text{co-ſ. AH} \times \text{ſin. AH}}{\text{ſin. E}} = k \times \text{co-ſ. E} \times \overline{\text{ſin. EH}}^2$ (= E*e*).

Let, now, the ſun's longitude EH be denoted by Z (conſidered as a flowing quantity); then, $\overline{\text{ſin. Z}}^2$ being $= \frac{1}{2} - \frac{1}{2}$ co-ſ. 2 Z, we ſhall have k × co-ſ. E × $\overline{\text{ſin. EH}}^2 = \frac{1}{2} k \times \text{co-ſ. E} \times \overline{1 - \text{co-ſ. 2 Z}}$. But the angle deſcribed about the axe of rotation P*p*, in the time that the ſun's longitude is augmented by the particle $\dot{Z}$, will be $= \frac{T}{t} \times \dot{Z}$. Therefore (by the general propoſition) we have, as $1 : \frac{1}{2} k \times \text{co-ſ. E} \times \overline{1 - \text{co-ſ. 2 Z}} :: \frac{T}{t} \times \dot{Z} : \frac{1}{2} k \times \frac{T}{t} \times \text{co-ſ. E} \times \dot{Z} - \dot{Z}$ co-ſ. 2 Z, the true regreſs of the equinoctial point E, during

* No error ariſes from conſidering the triangles EA*e* and AEH, as being formed on the ſurface of a ſphere, tho' the earth itſelf is not accurately ſuch. The angle (EA*a*) repreſenting the effect of the ſolar force, is properly referred to the ſurface of a ſphere; therefore (after the meaſure thereof is truly determined) the figure AP*ap* is itſelf taken as a ſphere, in order to avoid the trouble of introducing a new ſcheme.

that

that time: whoſe fluent, $\frac{1}{2} k \times \frac{T}{t} \times$ co-ſ. E $\times \overline{Z - \frac{1}{2}}$ ſin. 2 Z, will conſequently be the total regreſs of the point E, in the time that the ſun, by his apparent motion, deſcribes the arch HE or Z; which, on the ſun's arrival at the ſolſtice, becomes barely $= \frac{1}{2} k \times \frac{T}{t} \times$ co-ſ. E $\times$ an arch of 90°: the quadruple whereof, or $\frac{1}{2} k \times \frac{T}{t} \times$ co-ſ. E $\times 360° \left(= \frac{3t}{4T} \times \frac{OA^2 - OP^2}{OA^2} \times \text{co-ſ. E} \times 360°\right)$ is therefore the whole annual preceſſion of the equinox cauſed by the ſun. This, in numbers (taking $\frac{OP}{OA} = \frac{229}{230}$) comes out $\frac{3}{4 \times 366\frac{1}{4}} \times \frac{2}{230\frac{1}{2}} \times 0.917176 \times 360° = 21'' \; 6'''$.

The very ingenious M. Silvabelle, in his eſſay on this ſubject, inſerted in the 48th volume of the Philoſophical Tranſactions, makes the quantity of the annual preceſſion of the equinox, cauſed by the ſun, to be the half, only, of what is here determined. But this gentleman appears to have fallen into a twofold miſtake. Firſt, in finding the *momenta of rotation* of the terreſtrial ſpheroid, and of a very ſlender ring, at the equator thereof; which *momenta* he refers to an axis perpendicular to the plane of the ſun's declination, inſtead of the proper axe of rotation, ſtanding at right angles to the plane of the equator. The difference, indeed, ariſing from thence, with reſpect to the ſpheroid (by reaſon of its near approach to a ſphere) will be inconſiderable; but, in the ring, the caſe will be quite otherwiſe; the equinoctial points thereof being made to recede juſt twice as faſt

as they ought to do. This may ſeem the more ſtrange, if regard be had to the concluſions, relating to the nodes of a ſatellite, derived from this very aſſumption. But, that theſe concluſions are true, is owing to a ſecond, or ſubſequent miſtake, at Art. 27; where the meaſure of the ſun's force is taken the half, only, of the true value; by means whereof the motion of the equinoctial points of the ring is reduced to its proper quantity, and the motion of the equinoctial points of the terreſtrial ſpheroid, to the half of what it ought to be.

That expert geometrician M. Cha. Walmſley, in his Eſſay on the Preceſſion of the Equinox, printed in the laſt volume of the Philoſophical Tranſactions, has judiciouſly avoided all miſtakes of this laſt kind, reſpecting the ſun's force, by purſuing the method, pointed out by Sir Iſaac Newton; but, in determining the effect of that force, has fallen into others, not leſs conſiderable than thoſe above adverted to.

In his third Lemma, the momentum of the whole Earth, about its diameter, is computed on a ſuppoſition, that the momentum or force of each particle is proportional to its diſtance from the axis of motion, or barely as the quantity of motion in ſuch particle, conſidered abſtractedly. No regard is, therefore, had to the lengths of the unequal levers, whereby the particles are ſuppoſed to receive and communicate their motion: which, without doubt, ought to have been included in the conſideration.

In his firſt propoſition, he determines, in a very ingenious and conciſe manner, the true annual motion of the nodes of a ring (or of a ſingle ſatellite) at the earth's equator, revolving with the earth itſelf, about

its

its center, in the time of one ſiderial day. This motion he finds to be $= \frac{3 \text{ co-f. } 23^\circ 29'}{4 \text{ rad.}} \times \frac{1}{366\frac{1}{4}} \times 360^\circ$. Then, in order to infer from thence, the motion of the equinoctial points of the earth itſelf, he, firſt, diminiſhes that quantity, in the ratio of 2 to 5: Becauſe (as is demonſtrated by Sir Iſaac Newton in his 2d Lemma) the whole force of all the particles ſituated without the ſurface of a ſphere, inſcribed in the ſpheroid, to turn the body about its center, will be only 2-5ths of the force of an equal number of particles uniformly diſpoſed round the whole circumference of the equator, in the faſhion of a ring. The quantity $\left(\frac{3 \text{ co-f. } 23^\circ 29'}{4 \text{ rad.}} \times \frac{2}{5} \times \frac{1}{366\frac{1}{4}} \times 360^\circ\right)$ thus ariſing, will, therefore, expreſs the true motion of the equinoctial points of a ring, equal in quantity of matter to the exceſs of the whole earth above the inſcribed ſphere, when the force whereby the ring tends to turn about its diameter is ſuppoſed equal to the force whereby the earth itſelf tends to turn about the ſame diameter, in conſequence of the ſun's attraction. Thus far our author agrees with Sir Iſaac Newton; but, in deriving from hence the motion of the equinoctial points of the earth itſelf, he differs from him; and, in the corollary to his third Lemma, aſſigns the reaſons, why he thinks Sir Iſaac Newton, in this particular, has *wandered a little from the truth.* Inſtead of diminiſhing the quantity above exhibited (as Sir Iſaac has done) in the ratio of all the motion in the ring to the motion in the whole earth, he diminiſhes it in the ratio of the motion of all the matter above the ſurface of the inſcribed ſphere to the motion of the whole earth: which matter, tho' equal

to that of the ring, has nevertheleſs a different momentum, ariſing from the different ſituation of the particles in reſpect to the axis of motion.

But ſince the aforeſaid quantity, from whence the motion of the earth's equinox is derived, as well by this gentleman, as by Sir Iſaac Newton, expreſſes truly the annual regreſs of the equinoctial points of the ring (and not of the hollow figure formed by the ſaid matter, which is greater, in the ratio of 5 to 4) it ſeems, at leaſt, as reaſonable to ſuppoſe, that the ſaid quantity, to obtain from thence the true regreſs of the equinoctial points of the earth, ought to be diminiſhed in the former of the two ratios above ſpecified, as that it ſhould be diminiſhed in the latter. But, indeed, both theſe ways are defective, even ſuppoſing the momenta to have been truly computed; the ratio, that ought to be uſed here, being that of the momenta of the ring and earth about the proper axe of rotation of the two figures, ſtanding at right-angles to the plane of the ring and of the equator. Now this ratio, by a very eaſy computation, is found to be as $\overline{230}^2 - \overline{229}^2$ to $\frac{2}{5}$ of $\overline{230}^2$; whence the quantity ſought comes out $= \frac{3 \text{ co-f. } 23^\circ\, 29'}{4 \text{ rad.}} \times \frac{1}{366\frac{1}{4}} \times \frac{\overline{230}^2 - \overline{229}^2}{230^2} \times 360^\circ = 21'' \, 6'''$: which is the ſame that we before found it to be, and the double of what this author makes it.

What has been ſaid hitherto, relates to that part of the motion only, ariſing from the force of the ſun. It will be but juſtice to obſerve here, that the effect of the moon, and the inequalities depending on the poſition of her nodes, are truly aſſigned by both the gentlemen

gentlemen above-named; the ratio of the diameters of the earth, and the denſity of the moon being ſo aſſumed, as to give the maxima of thoſe inequalities, ſuch as the obſervations require: in conſequence whereof, and from the law of the increaſe and decreaſe (which is rightly determined by theory, tho' the abſolute quantity is not) a true ſolution, in every other circumſtance, is obtained.

The freedom, with which I have expreſſed myſelf, and the liberty I have here taken, to animadvert on the works of men, who, in many places, have given inconteſtible proofs of ſkill and genius, may, I fear, ſtand in need of ſome apology. 'Tis poſſible I may be thought too peremptory. Indeed, I might have delivered my ſentiments with more caution and addreſs: but, had not I imagined myſelf quite clear in what has been advanced, from a multitude of concurrent reaſons, I ſhould have thought it too great a preſumption to have ſaid any thing at all here, on this ſubject. The great regard I have for this Society, of which I have the honour to be a member, will, I hope, be conſidered as the motive for my having attempted to rectify ſome overſights, that have occurred in the works of this learned body.

LIV. *Remarks upon the Heat of the Air in* July 1757. *in an Extract of a Letter from* John Huxham, *M.D. F.R.S. to* William Watson, *M.D. F.R.S. dated at* Plymouth 19*th of that Month. With additional Remarks by Dr.* Watson.

Read Dec. 22, 1757.

"FROM the beginning of June last "we have had a very dry season, "generally very warm, and sometimes excessively "hot. From the 7th to the 14th of this month "the heat was violent; greater, indeed, than has "been known here in the memory of man. I "have talked with several persons, who have lived "a considerable time in Jamaica, Gibraltar, and "Minorca; and they severally assert, that they ne-"ver felt such intense heat in any of those places. "Upon the 11th, 12th, and 13th of this month, "Fahrenheit's thermometer, in the shade, about "three o' clock in the afternoon, was at 87; nay, "upon the 12th it was even above 88.

"Abundance of people have suffered very severely "from these excessive heats: putrid, bilious, pe-"techial, nervous fevers, are exceedingly common "every-where. Dysenteries, hæmorrhages, most "profuse sweats, affect not only those in fevers, but "a vast many others. The days and nights were "so intolerably hot, that little or no sleep was to "be gotten day or night. The wind we had, like "the Campsin, actually blew hot, tho' strong.

"Upon

" Upon the 15th, about ſeven at night, at Fal-
" mouth, Penryn, Truro, and thereabouts, a pretty
" ſmart ſhock of an earthquake was felt, attended
" with a hollow rumbling noiſe, throwing down
" pewter, china-ware, and ſuch-like. The tinners
" felt it eighty fathom under ground. No great
" damage however was done. The day before we
" had, about eleven o' clock before noon, a moſt
" violent hurricane, which laſted five or ſix minutes,
" attended with a heavy ſhower."

Thus far Dr. Huxham.

The heat of the air at London, during the period above-mentioned, was much greater than has been uſually obſerved in theſe high latitudes; tho' it was never quite ſo ſevere here as at Plymouth. The following table exhibits the degrees of the heat, taken here upon the reſpective days, about four o' clock in the afternoon, by a Fahrenheit's thermometer. The inſtrument was placed in the ſhade; and the accuracy of the obſerver, who favoured me with his minutes, is not to be queſtioned.

1757. July	5	—	—	75
	6	—	—	78
	7	—	—	75½
	8	—	—	78
	10	—	—	80¼
	11	—	—	83¼
	12	—	—	80¼
	13	—	—	80
	14	—	—	85
	15	—	—	81
	16	—	—	73

From

From hence it appears, that the air at London was, upon ſeveral days, hotter than it had been obſerved at Madeira for ten years together: for, by Dr. Thomas Heberden's obſervations, mentioned in the Philoſophical Tranſactions, the heat of the air at Madeira, during that period, was never but once at 80.

William Watſon.

LV. *Remarks upon the Letter of Mr.* John Ellis, *F. R. S. to* Philip Carteret Webb, *Eſq*; *F. R. S. printed in the* Philoſophical Tranſactions, *Vol.* xlix. *Part* ii. *p.* 8ϕ6. *By Mr.* Philip Miller, *F. R. S.*

Read Dec. 15. 1757.

THE paper of mine, which was read before the Royal Society on the 8th of May 1755, and afterward printed in the xlixth volume of the Philoſophical Tranſactions *, was written at the requeſt of Mr. Watſon; who informed me, that a letter from the Abbé Mazeas to the reverend Dr. Hales had been communicated to the Royal Society, in which it was mentioned, that the Abbé Sauvages had made a diſcovery of the juice of the Carolina Toxicodendron ſtaining linen of a permanent black. But Mr. Watſon ſaid, that the letter, he thought, required a careful peruſal before it was printed; and he wiſhed I would confirm it. I told

* Part I. p. 161.

him,

him, if the letter was put into my hands, I would look it over, and deliver my opinion of it.

Accordingly Dr. Birch delivered the letter to me; and, upon reading it, I found, that tho' this might be a diſcovery to thoſe two gentlemen; yet, as it had been mentioned in ſeveral printed books long before, I thought it might not be for the reputation of the Royal Society to have it printed as ſuch in their Tranſactions.

This was my motive for writing that paper: in which I have not endeavoured to depreciate the diſcovery of the Abbé Sauvages, but have only mentioned what had occurred to me in thoſe books of botany, where that ſhrub is taken notice of. And as the knowlege of it, and the method of collecting the varniſh, might be of ſervice to the inhabitants of the Britiſh colonies in America, I took the liberty of adding the account given of it by Dr. Kœmpfer.

Mr. Ellis, in his letter to Mr. Webb, aſſerts, that the American *Toxicodendron* is not the ſame with Kœmpfer's *Arbor vernicifera legitima*. This aſſertion of his makes it neceſſary to lay before the Society the authorities, upon which I have grounded my belief, that they are the ſame. But it may not be amiſs firſt to take notice, that the ſhrub mentioned by the Abbé Sauvages is the ſame with that, which the gardeners about London call the Poiſon-aſh. The title of it, mentioned by the Abbé Sauvages, was given by myſelf to that ſhrub, in a catalogue of trees and ſhrubs, which was printed in the year 1730; before which it had no generical title applied to it. And about the ſame time I ſent ſeveral of the plants to Paris and Holland with that title, which

which I had raiſed a few years before from ſeeds, which were ſent by Mr. Cateſby from Carolina.

And altho' this ſhrub had not been reduced to any genus before, yet it had been ſome years growing in the gardens of the Biſhop of London at Fulham, at Mr. Reynardſon's at Hillenden, Mr. Darby's at Hoxton, and in the Chelſea garden, which were raiſed from ſeeds ſent by Mr. Baniſter from Virginia; two of which were growing at Chelſea in the year 1722, when the care of that Garden was intruſted to me.

The firſt intimation I had of the American ſhrub being the ſame with Dr. Kœmpfer's true varniſh-tree, was from the late Dr. William Sherard, in the year 1726, when that gentleman deſired me to bring him a ſpecimen of the American Toxicodendron from the Chelſea garden; which I accordingly did: and then the Doctor, and Dr. Dillenius, compared it with a dried ſpecimen in the collection of the former, which was gathered in Japan, and which, if I remember right, he told me he received from Dr. Kœmpfer ſome years before. It appeared to thoſe two gentlemen, that they were the ſame; and their ſkill in the ſcience of botany was never doubted.

About a year after this, I carried a ſpecimen of the American Toxicodendron to an annual meeting of ſome botaniſts at Sir Hans Sloane's in Bloomſbury; where there were preſent Mr. Dale of Braintree, Mr. Joſeph Miller, Mr. Rand, and ſome others; which was then compared with Dr. Kœmpfer's ſpecimen, whoſe collection Sir Hans Sloane had purchaſed: and it was the opinion of every one preſent, that they were the ſame. Nor has any one doubted of their being ſo, who has compared the American ſhrub with

with Kœmpfer's figure and deſcription of his true varniſh-tree, but Mr. Ellis.

And now give me leave to examine his reaſons for differing in opinion from every late botaniſt, who has mentioned this ſhrub.

He ſays, that the midrib, which ſupports the lobe leaves, is quite ſmooth in the poiſon-aſh, as is alſo the under ſide of the leaves; whereas Dr. Kœmpfer, in his deſcription of the midrib of the true varniſh-tree, calls it *læviter lanuginoſo*; and in his deſcription of the lobes or *pinnæ* he ſays, they are *baſi inequaliter rotunda*; whereas thoſe of the poiſon-aſh come to a point at their footſtalks nearly equal to that at the top. Theſe characters, Mr. Ellis thinks, are ſufficient to prove, that they are different plants: and he blames Dr. Dillenius for having omitted theſe neceſſary characters in his deſcription of it; and ſuppoſes this muſt have miſled the accurate Linnæus, who quotes his ſynonyma.

But as Dr. Linnæus is poſſeſſed of Kœmpfer's book, he would little have deſerved the appellation of accurate in this particular, had he not conſulted the original, but truſted to a copy. But this I know he has done, and is as well aſſured, that the plants in queſtion are the ſame, as Mr. Ellis can be of the contrary.

But here I muſt obſerve, that the branch, from which Dr. Kœmpfer's figure is taken, is produced from the lower part of a ſtem, which ſeems to have been cut down, and not from a flowering branch; and it is not improbable, that his deſcription may have been taken from the ſame branch: and if this be the caſe, it is eaſy to account for the minute dif-

ferences mentioned by Mr. Ellis; for it would not be difficult to produce inſtances of hundreds of different trees and ſhrubs, whoſe lower and upper branches differ much more in the particulars mentioned by Mr. Ellis, than the figure and deſcription given by Kœmpfer do from the American Toxicodendron. I will only mention two of the moſt obvious: the firſt is the white poplar, whoſe ſhoots from the lower part of the ſtem, and the ſuckers from the root, are garniſhed with leaves very different in form and ſize from thoſe on the upper branches, and are covered on both ſides in the ſpring with a woolly down. The next is the willow with ſmooth leaves, which, if a ſtandard, and the head lopped off, as is uſual, the young ſhoots are garniſhed with leaves much broader, and of different forms from thoſe on the older branches; and theſe have frequently a hairy down on their under ſurface, which does not appear on thoſe of the older. So that a perſon unacquainted with theſe differences in the ſame tree would ſuppoſe they were different. And the American Toxicodendron has varied in theſe particulars much more, in different ſeaſons, than what Mr. Ellis has mentioned.

Mr. Ellis next ſays, that the Toxicodendron mentioned by Mr. Cateſby, in his Natural Hiſtory of Carolina, is not the ſame with that, which is now called by the gardeners poiſon-aſh: but I am very poſitive of the contrary; for moſt of the plants in the nurſery-gardens about London were firſt raiſed from the ſeeds, which were ſent by Mr. Cateſby from Carolina; part of which were ſent to the late Dr. Sherard, as is mentioned by him in the Philoſophical Tranſactions, N°. 367; and another part came to my hands,

hands, from which I raiſed a great many of the plants, which were diſtributed, and ſome of them are now growing in the Chelſea garden.

And that this ſhrub grows naturally in Carolina, I can have no doubt, having received the ſeeds of it two or three times from the late Dr. Dale, who gathered them in the woods of that country.

In my paper above-mentioned I likewiſe obſerved, that the ſeeds, which were ſent to the Royal Society by Father D'Incarville, for thoſe of the true varniſh-tree, did not prove to be ſo; but the plants, which were raiſed from them, were taken to be referred to the ſpurious varniſh-tree of Kœmpfer; which I believed to be the ſame, and own, that it is yet my opinion, notwithſtanding what Mr. Ellis has ſaid to the contrary: for the number of lobes or *pinnæ* on each leaf, with their manner of arrangement on the midrib, are the ſame. And here we muſt obſerve, that the figure of this given by Kœmpfer is from a flowering branch; and every gardener or botaniſt muſt know, that the leaves, which are ſituated immediately below the flowers of moſt winged-leaved plants, have fewer lobes or *pinnæ*, than thoſe on the lower branches: therefore I muſt ſuppoſe it to be the caſe in this plant; and from thence, with ſome other obſervations which I made on the ſeeds, I have aſſerted it to be the wild or ſpurious varniſh-tree of Kœmpfer. But Mr. Ellis is of a contrary opinion, becauſe the baſe of the lobes of thoſe plants, which were raiſed from Father D'Incarville's ſeeds, are rounded and indented like two ears. In Dr. Kœmpfer's figure and deſcription of the *faſi-no-ki*, the leaves are intire, and come to a point at their baſe.

Here I think Mr. Ellis is a little too hasty in giving his opinion, as he has not seen this plant in the state, that the branch was, from which Kœmpfer's figure was taken. For as there are often such apparent differences between the leaves on the lower branches of trees, and those which are at their extremities, as that in the descriptive titles of the species Dr. Linnæus frequently uses them to distinguish one from another; so in making the same allowance for the plant in question, I cannot help thinking that I am in the right, and must abide by my opinion, till the plants, which have been raised from Father D'Incarville's seeds, have flowered, to convince me of the contrary.

However, I cannot help observing, that Mr. Ellis has given a title to this shrub before he had seen any of the characters, which are necessary to determine the genus. And I have pretty good reason to believe it should not be joined to the Rhus; for the three seeds, which I received from the Royal Society, were shaped like a wedge, being thicker on one edge than the other, and not unlike those of the beech-tree, as I noted in my catalogue when I sowed them; and, by their structure, seemed as if the three seeds had been inclosed in the same capsule.

If it proves so, this will by no means agree with the characters of Rhus; especially if the male flowers should grow upon different plants from the fruit, which is what I suspect. Nor can I agree with Dr. Linnæus in this particular of joining all the species of Toxicodendron to the genus of Rhus, many of which have their male flowers growing upon different plants from the fruit; and therefore would

more.

more properly come into his twenty-ſecond claſs of *Dioecia*, than his fifth of *Pentandria*, into which he ranges the Rhus. At the bottom of the characters of that genus he has added a note, to ſhew the varniſh-tree is ſo:

But as there are ſeveral other ſpecies, which agree in this eſſential character of diſtinction; ſo, according to the Linnæan ſyſtem, they ſhould be ſeparated from the Rhus, with another generical title.

Mr. Ellis obſerves, upon the poetical deſcription, which he ſays Kœmpfer has given of the leaves of the wild varniſh-tree turning red in the autumn, that he had not found it to be the caſe of the tree growing in the ſtove at Buſbridge. How it appeared in that ſituation, I know not; but the leaves of all thoſe, which are growing in the Chelſea garden, and ſtand in the open air, do conſtantly change to a purple colour in the autumn, before they fall off from the ſhrub: but thoſe of the true varniſh-tree are much more remarkable for the deepneſs of their colour.

Mr. Ellis ſays, he had received a letter from Dr. Sibthorp, profeſſor of botany at Oxford, in which the Doctor informs him, that there is no ſpecimen of the true varniſh-tree in the Sherardian collection at Oxford; but that there is one of *faſi-no-ki*, or ſpurious varniſh-tree of Kœmpfer. How the Doctor could write ſo, I cannot conceive; for I am very ſure there was no ſpecimen of the latter in that collection while it remained in London, having myſelf often viewed that part of it: and ſure I am, Dr. Dillenius never added that ſynonym to the former: and I do believe the latter was no other way known in Europe, than by Kœmpfer's figure and deſcription of it, ex-

4 cepting

cepting that ſpecimen of Kœmpfer's now in the Britiſh Muſeum.

But, to confirm what I have before ſaid, of Dr. Sherard's having a ſpecimen of the true varniſh-tree, I beg leave to quote what Dr. Dillenius has written in the *Hortus Elthamenſis*; where, after having deſcribed the American Toxicodendron, he ſays, *Ceterum hiſtoriam verniciferæ arboris Japoniæ, diligenter et accurate more ſuo exſequutus eſt laudatus Kæmpferius, cujus et deſcriptio et figura, quin et planta ſicca, quæ in Japonia lecta ſervatur in phytophylacio Sherardino, noſtræ huic ſpeciei examuſſim quadrat: id tantum, ſexus nempe differentia, præterviſa fuit auctori: quoniam autem ille liber non in omnium his in locis, multo minus in America, manibus verſatur, non alienum videtur, ſi qui, quorum intereſt, hæc legerint, ut norint, quæ ille de collectione & preparatione vernicis illius habet, hoc loco tranſcribere.* Then he goes on tranſcribing from Kœmpfer the manner, in which it is collected.

After this, I find Mr. Ellis is inclinable to think, that the poiſon-aſh, as it is called by the gardeners, is the ſame with the *faſi-no-ki*, or ſpurious varniſh-tree of Kœmpfer. The difference between theſe ſhrubs does not conſiſt in ſmall and minute particulars, but the moſt obvious ſtriking marks of diſtinction appear at firſt ſight; for the poiſon-aſh has rarely more than three or four pair of lobes to each leaf, terminated by an odd one: in which particular it agrees with the true varniſh-tree of Kœmpfer; whereas in the figure, which Kœmpfer has given of the ſpurious varniſh-tree, the leaves have ſeven or eight pair of lobes terminated by an odd one: and this

this figure, as I before obſerved, is drawn from a flowering branch. Every one, who is the leaſt acquainted with theſe things, knows, that the leaves immediately below the flowers are conſiderably leſs than thoſe on the lower part of the branches: therefore this is a more eſſential note of diſtinction than thoſe mentioned by Mr. Ellis.

I muſt alſo obſerve, that Mr. Ellis would ſuggeſt, that I ſuppoſed theſe two ſhrubs were only varieties of each other produced by culture: whereas it muſt appear to every one, who reads my paper, that my intention in mentioning the ſpurious varniſh-tree was to ſhew it was different from Kœmpfer's true varniſh-tree, altho' Kœmpfer ſuppoſes otherwiſe.

For the ſatisfaction of the curious, I have added a leaf of each ſhrub, which are now growing in the Chelſea garden, that if any perſon has the curioſity, they may compare them with Kœmpfer's.

In my paper I took notice, that one of the beſt kinds of varniſh was collected from the Anacardium in Japan; and recommended it to the inhabitants of the Britiſh iſlands in America, to make trial of the occidental Anacardium, or Caſhew-nut tree, which abounds in thoſe iſlands. This has occaſioned Mr. Ellis to take great pains to ſhew, that the eaſtern and weſtern Anacardium were different trees: a fact, which was well known to every botaniſt before; and of which I could not be ignorant, having been poſſeſſed of both ſorts near thirty years. But as I was aſſured, from many repeated experiments, that the milky juice, with which every part of the Caſhew-tree abounds, would ſtain linen with as permanent a black as that of the oriental Anacardium; ſo I juſt hinted,

hinted, that it was worth the trial. Nor was my hint grounded on thoſe experiments only, but on the informations I had received from perſons of the beſt credit, who had reſided long in the American iſlands, that people are very careful to keep their linen at a diſtance from thoſe trees, well knowing, that if a drop of the juice fell upon it, they could never waſh out the ſtain.

But Mr. Ellis, in order to prove that this tree has no ſuch quality of ſtaining, ſays, he has made ſome experiments on the cauſtic oil, with which the ſhell or cover of the Caſhew-nut abounds; and that he found it was not endued with any ſtaining quality. But ſurely thoſe experiments cannot be mentioned to prove, that the milky juice of the tree has not this property: and Sir Hans Sloane, in his Hiſtory of Jamaica, ſays, that the inhabitants of Jamaica ſtain their cottons with the bark of the Caſhew-tree.

I ſhall not intrude farther on the patience of the gentlemen, who may be preſent when this paper is read; but humbly crave their pardon for detaining them ſo long: nor ſhould I have given them this trouble, had not I thought my reputation concerned on the occaſion.

LVI.

LVI. *An Answer to the preceding Remarks. By Mr.* John Ellis, *F. R. S.*

Read Jan. 19, 1758.

MY letter to Mr. Webb, which is printed in the second part of the xlixth volume of the Philosophical Transactions *, was intended to shew this Honourable Society, that Mr. Miller, in his reply to the Abbé Mazeas's letter, had brought no proofs to lessen the discovery, which he tells us the Abbé Sauvages had made, in attempting to improve the art of painting or staining linens and cottons of a fine durable black colour, by making use of the juice of the Carolina pennated Toxicodendron, instead of the common method of staining black with gauls and a preparation of iron; which, he says, always turns to a rusty colour when washed.

Mr. Miller, instead of producing the proper proofs, to shew that this method of staining cottons and linens of a black colour was known before, or quoteing the authors in which he says it is mentioned, contents himself with telling the Society, that this American Toxicodendron is the same plant with the true varnish-tree of Japan; and that callicuts are painted with the juice of this shrub.

In my letter to Mr. Webb, I have endeavoured to shew, that notwithstanding the authority of Dr. Dillenius, and the authors that have followed him, it does not appear, from Dr. Kœmpfer's description of this Japan plant, that it can be the same with our American one.

* Page 806.

The design, then, of this paper, is to lay before this Society some further reasons, why these plants cannot be the same; and that even if they were the same, Mr. Miller has produced no authority to shew, that this juice was ever made use of for this purpose abroad; with some remarks on his reply to my letter, in which he obliges me to be more particular than I intended, in explaining some errors, which I find he has run into.

In my letter to Mr. Webb, I have pointed out the exact description, which Kœmpfer has given us of the leaves of this plant, shewing how much they differ from our American one: but now I shall mention some observations that escaped me before, and which, I think, will give us a clearer proof of this matter.

Kœmpfer, then, informs us, that this Japan varnish-tree, or *Sitz-dsju*, is a tree, not a shrub: and this author (it is well known) is remarkably exact in the description of his Japan plants, making the necessary distinctions between a shrub, an arborescent shrub, and a tree. He then goes on to explain the manner of its growth; and tells us, that it grows with long sappy shoots, very luxuriantly, to the height of a sallow or willow-tree, which we may reasonably allow to be from 20 to 30 feet: whereas this Carolina pennated Toxicodendron, as Mr. Miller tells us in his Dictionary, 6th edit. in folio, is a shrub, and seldom rises above five feet high with us: and many people, who have been in North America, agree, that it is but a slow grower there, and is one of the shrubby underwoods of that country: so that, allowing it to grow even double the height it does here,

here, it is ſtill but a ſhrub, in compariſon with the other.

Further, while Dr. Dillenius was warm with this ſuppoſed diſcovery, of our having got the true Japan varniſh-tree in America, attempts were made there, by intelligent perſons under his direction, to procure this varniſh after the manner of Kœmpfer; but without ſucceſs, as I am aſſured by perſons of that country now here, with whom the Doctor correſponded.

Let us now conſult the growth of the Carolina and Virginia Sumachs, or Rhus's, in our nurſery-gardens, and compare them with this little ſhrubby Toxicodendron, and we ſhall find, that even in this cold climate nature keeps her regular proportionable pace in the growth of vegetables of the ſame country.

Let us obſerve the growth of ſome of theſe Rhus's, and we ſhall find that great luxuriancy of the ſhoots, which Kœmpfer ſo juſtly deſcribes in his varniſh-tree. One of theſe American ones even ſeems to promiſe the ſame height as the Japan Rhus; whereas this little ſhrubby Toxicodendron ſtill preſerves the ſame dwarfiſh ſlow-growing habit, that it has in its native country.

This leads me, in the next place, to ſhew, that theſe two plants muſt be of different genus's; the one a Rhus, and the other a Toxicodendron: and if ſo, according to Mr. Miller, they ought to be properly diſtinguiſhed, and not ranked together, as Dr. Linnæus has done.

In order to prove this, let us then examine Kœmpfer's deſcription of the parts of the flower, and ſee

whether it does not anſwer exactly to the genus of Rhus; and whether the flowers are not male and female in themſelves, that is, hermaphrodites, on the ſame tree. The original of Kœmpfer is as follows, p. 791 of his *Amœnitates:* "Floſculos conti-
" nent pumilos, et citra coriandri ſeminis magni-
" tudinem radiantes, in luteum herbaceos, pentape-
" talos, petalis carnoſis nonnihil oblongis et repan-
" dis, ſtaminibus ad petalorum interſtitia ſingulis,
" apicatis, breviſſimis, ſtylo perbrevi tricipite, floris
" turbini inſidente; fructus floſculum excipit gib-
" boſus utcunque in rhomboides figuram com-
" preſſus." Whereas Dr. Dillenius, and the authors that have copied after him, ſay, that his Toxicodendron has the male bloſſoms on one plant, and the female on the other; from whence it muſt evidently be another genus.

It appears, however, that Dr. Dillenius was not altogether ignorant of this difference of genus in theſe two plants; but, rather than his Toxicodendron, which he had made agree exactly in the leaves, ſhould not agree in the fructification, he makes the accurate Kœmpfer guilty of an unpardonable overſight, in not taking notice of the difference of the ſexes of this varniſh-tree in different plants: whereas we have juſt now ſhewn, that nothing can be more minutely and judiciouſly deſcribed, than he has done both the male and female parts of the bloſſom, which change into the fruit on the ſame plant.

The original of Dr. Dillenius's remarks on Dr. Kœmpfer's ſpecimen runs thus: "Planta ſicca, quæ
" in Japonia lecta, ſervatur in phytophylacio Sherar-
" dino, noſtræ huic ſpecie examuſſim quadrat, id
" tantum

" tantum ſexus nempe differentia præterviſa fuit auc-
" tori." Hence we find how this error came to ſpread, and this falſe ſynonym to be adopted by the botanic writers, who copied after Dillenius.

This ſhews us what little dependance we can have upon the reſult of that meeting, which Mr. Miller mentions he had with his botanic friends; where, from the ſimilitude of leaves only, without the parts of fructification, they determined theſe two plants, ſo different in their growth, to be one and the ſame plant.

Mr. Miller remarks very juſtly, that the leaves of the ſame tree often vary much in ſhape, ſuch as thoſe of the poplar, ſallow, *&c.*

But in anſwer to this, we may reaſonably ſuppoſe, that Dr. Kœmpfer, who was on the ſpot, would not chooſe for his ſpecimens leaves of the moſt uncommon ſorts that were on the tree, and neglect the moſt common. This would be carrying the ſuppoſition farther than can be allowed, unleſs we ſuppoſe this author had not the underſtanding even of a common gardener; for otherwiſe, I am perſuaded, Sir Hans Sloane would not have thought his ſpecimens worth purchaſing.

For another ſynonym to the true Japan varniſh-tree, as alſo to Dillenius's pennated Toxicodendron with rhomboidal fruit, Mr. Miller brings in (in his anſwer to the Abbé Mazeas's letter) the Bahama Toxicodendron *foliis alatis fructu purpureo pyriformi ſparſo* of Cateſby's Nat. Hiſt. vol. i. p. 40. ſo that he would have all theſe three different plants one and the ſame: and, in his reply to my letter, he ſtill inſiſts on it, that theſe two Toxicodendrons are the ſame. But

here I muſt beg the favour of this Honourable Society, when they come more attentively to conſider this matter, to compare his anſwer to the Abbé Mazeas's letter, and his reply to me, in this particular part.

I ſhall only at preſent take notice, that Cateſby ſays, this Toxicodendron, with the pear-ſhaped fruit, grows uſually on rocks in Providence, Ilathera, and other of the Bahama iſlands; and does not mention, that he ever ſaw it in Carolina. I cannot find it deſcribed by any author as growing in Carolina, or in any other part of the continent of North America: nor do I believe that there is a plant of it now growing in England, or that it is even the ſame genus with Dillenius's rhomboidal-fruited one, from the different ſtructure both of its leaves as well as fruit.

In looking over Dr. Linnæus's *Hortus Cliffortianus*, I find he gives this Bahama Toxicodendron of Cateſby as a ſynonym to his *Elemifera foliis pinnatis*, p. 486.

I now come to that part of Mr. Miller's reply, relating to the China varniſh-tree, that was raiſed from ſeeds ſent to the Royal Society by Father D'Incarville; where he ſtill inſiſts on it, that this is the ſame with the ſpurious varniſh-tree of Kœmpfer. His reaſons are, that notwithſtanding the indentation and roundneſs of the bottom of the lobe-leaves of the China varniſh-tree, and tho' the lobe-leaves of the ſpurious Japan varniſh-tree come to a point at the baſe, and are no-way indented, but quite even on the edges; yet he ſays, becauſe they have an equal number of *pinnæ*, or lobe-leaves, on the whole leaf of each tree, they muſt be the ſame.

In

Rhus Sinense foliis alatis foliolis oblongis Acuminatis ad basin subrotundis et dentatis.

Ex Horto Botanico Philippi Carter. Webb. Armig.

Pub. Mar 11 81.

In anſwer to this, I ſay their lobe-leaves are not equal; for I have examined both the ſpecimens and drawings of Dr. Kœmpfer's ſpurious varniſh-tree, and I don't find that the number of the *pinnæ* exceed ſeven on a ſide: whereas I have a ſmall ſpecimen of a leaf by me, that was taken from the top of one of D'Incarville's China varniſh-trees, which is above eight feet high, and ſtands in an open expoſure; and this leaf, tho' but a foot long, has 12 lobe-leaves on a ſide, and each lobe indented at the baſe *. At the ſame time I obſerved, that the leaves of the young ſhoots of another tree were a yard long, as they were this ſummer at the garden of the Britiſh Muſeum. Another thing is remarkable in the leaves of this China varniſh-tree; and that is, the lobes of the leaves, as they approach to the end, grow ſmaller and ſmaller; whereas in the ſpurious Japan varniſh-tree they are rather, if there is any difference, larger towards the end.

I ſhall make this further remark, that tho' theſe indentations on the lobe-leaves may vary in number in this China varniſh-tree; yet, as I obſerved before, ſince they are continued on even in the ſmaller leaves at the top of the branches of a tree eight feet high in the open ground, it appears to me, that this ſpecific character, beſides the form and inſertion of the lobe-leaves, will ever diſtinguiſh it as a different ſpecies from the *Faſi-no-ki*, or ſpurious varniſh-tree of Kœmpfer.

Mr. Miller now goes on to tell us, he is confirmed in his belief of their being the ſame, by making ſome obſervations on the ſeeds of this China varniſh-

* See TAB. XVII. where this ſpecimen is exactly delineated.

tree;

tree; and therefore afferts, that they are the fame. It is natural to fuppofe he compared them with the accurate drawings of the feeds of Kœmpfer's *Fafi-no-ki*, p. 794. that being the only place where the feeds of it are defcribed.

In the very next paragraph Mr. Miller feems to forget, that from his own obfervations on the feeds of the China varnifh-tree, he has afferted it to be the *Fafi-no-ki* of Kœmpfer; but now he finds, in his memorandums, that thofe feeds were wedge-fhaped, and like the feeds of the beech-tree; and that all the three feeds he received feemed to be inclofed in one capfule: fo that now he is at a lofs what to call it; and at the fame time fays I have been too hafty in calling it a Rhus.

Mr. Miller goes on, and allows this China varnifh-tree changes to a purple in the autumn; but not fo deep as the true varnifh-tree. I fuppofe he means, by this true varnifh-tree, the Carolina pennated Toxicodendron; for Kœmpfer has not told us what colour the true varnifh-tree of Japan changes to in autumn.

But this is no certain proof on either fide of the queftion, only a corroborating circumftance of the fpecies of a tree: nor fhould I have mentioned it, but for the manner in which Kœmpfer, with an imagination truly poetical, defcribes the autumnal beauty of his *Fafi-no-ki*, or fpurious varnifh-tree. "Rubore fuo autumnati quâ viridantes fylvas fua-"viter interpolat, intuentium oculos e longinquo in "fe convertit." Even this defcription would make one fufpect it is not the fame with the China varnifh-tree, which, I am informed, did not turn purplifh in the garden of the Britifh Mufeum till the firft froft

came

came on: whereas it is well known, that ſome of the Rhus's and Toxicodendrons, particularly the Carolina pennated one, change to a fine ſcarlet colour in the beginning of a dry autumn, even before any froſt appears.

Mr. Miller ſeems ſurpriſed, how I ſhould think, that the Carolina pennated Toxicodendron, or poiſonaſh is like the *Faſi-no-ki* of Kœmpfer. I muſt here acknowlege, at this time, not having ſeen Doctor Kœmpfer's ſpecimen, I imagined, from the ſhape of the lobe-leaves (as he has deſcribed them) and from the remarkable ſcarlet colour of both theſe trees in autumn, that Mr. Miller might be right in what he has advanced; for it was from his authority I took it, depending on the information he gives us in his Dictionary, fol. edit. 6. under the article *Toxicodendron*, where he takes ſome pains to aſſure us, that they are the very ſame plants.

In the next paragraph I find Mr. Miller has intirely miſtaken the meaning of one part of my letter to Mr. Webb; which I muſt recommend to him to read again, and he will find it exactly agees with his own ſentiments. There he will find my opinion is, that notwithſtanding the change of ſoil and ſituation, this *Sitz-dsju*, or true varniſh-tree, and the *Faſi-no-ki*, or ſpurious varniſh-tree of Kœmpfer, are diſtinct ſpecies of Rhus or Toxicodendron, and will ever remain ſo.

Mr. Miller now deſires me, ſince I have ſeen Dr. Kœmpfer's ſpecimens in the Britiſh Muſeum, to declare, whether I think I am miſtaken.

In anſwer to this, and to ſatisfy Mr. Miller as well as myſelf, I have been very lately at the Mu-

ſeum, and have looked very carefully over Dr. Kœmpfer's ſpecimens, and do ſincerely think, as did other judges at the ſame time, that the *Sitz-dsju* is not the ſame with the Carolina pennated Toxicodendron, nor the *Faſi-no-ki* the ſame with Father D'Incarville's China varniſh-tree.

Mr. Miller informs us, that one of the beſt kinds of varniſhes is collected from the Anacardium in Japan.

In anſwer to this, I muſt beg leave to ſhew the Society, that Dr. Kœmpfer does not ſo much as mention, that this Anacardium grows in Japan; but that the varniſh, which is collected from it, is brought to them from Siam: and I believe it will appear plainly, from what follows, that there is not a plant of this kind in the kingdom of Japan; for Siam and Cambodia, eſpecially the parts of thoſe kingdoms, where Kœmpfer informs us this * Anacardium grows, lie in the latitudes of from 10 to 15 degrees north, which muſt be full as hot as our Weſt Indies: ſo that it is not probable, that it would bear the cold of the winters in Japan; for Japan lies from the latitudes of 33 to above 40 degrees north, which is about the ſame parallel with our North American colonies.

I ſhall now beg leave to lay before the Society that paſſage of Dr. Kœmpfer, which relates to this diſpute, together with my tranſlation of it, that it may be compared with Mr. Miller's tranſlation, which he gives us in his reply to the Abbé Mazeas's letter, Philoſoph. Tranſ. vol. xlix. p. 164. 2d paragraph.

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* This is likewiſe called the Malacca Bean, from its growing in great plenty on that coaſt, near the equinoctial line.

Dr.

Dr. Kœmpfer, in his *Amœnitates*, p. 793. ſpeaking of the true varniſh-tree, ſays, " Colitur frequens " in provinciis Tſi-kocko et Figo, in quibus inſerti " agris ſcapi radices agunt et caudices edunt poſt " triennium vernicem ſuppeditantes. Optima regi- " onis, quin totius mundi, vernix perhibetur circa " urbem Jaſſino colligi. Vernicem ceres Japonica " largitur oppido nobilem et pretioſiſſimam, ſed ad- " modum parcam; nec pro operibus, quæ regio " conſtruit, ſufficeret, niſi prius cum, *Nam Rak*, i. e. " vernice ignobiliore ex Siamo invecta, pro baſi illi- " nerentur. Siamenſis vernix promitur in provincia " Corſama, et regno Cambodiæ ex arbore Anacardo, " incolis *Tong Rak*, i. e. *Arbor Rak* dicta, cujus " fructus officinis noſtris Anacardium dictus *Luk* " *Rak*, liquor *Nam Rak* appellatur. Perforatus " truncus immiſſo tubulo, tantâ copiâ fundit liquo- " rem ut Sinæ, Tunquino et Japoniæ pro deliniendis " utenſilibus ſufficiat, quin jam Bataviam et alia In- " diæ loca vaſis ligneis incluſa appellit."

Which, tranſlated into Engliſh, appears to me to be thus:

' This varniſh-tree is often cultivated in the pro- ' vinces of Tſi-kocko and Figo: there they plant ' the cuttings or truncheons in the fields, which take ' root, and ſend forth vigorous ſhoots, which in ' three years time yield this varniſh.

' The beſt varniſh of the kingdom, nay, of the ' whole world, is ſaid to be collected about the city ' of Jaſſino. The produce in Japan of this moſt ' noble and very precious varniſh, is ſo very little, ' that there would not be ſufficient for the wares ' made in the kingdom, if they did not firſt lay on

'a ground with an ordinary kind of varnish, which 'they call *Nam Rak*, and is brought to them from 'Siam.

'This Siam varnish is collected in the province of 'Corsama, and in the kingdom of Cambodia, from 'the tree Anacardus, called by the inhabitants *Tong* 'or *Tree-Rak*; the fruit of which is called in our 'shops Anacardium, or *Luk Rak*, and the liquor is 'called *Nam Rak*.

'To collect this liquor, they bore a hole in the 'trunk, and put in a tube. By this method they 'get as much of it as is sufficient not only to varnish 'all the utensils of China, Tonquin, and Japan, but 'it is even exported in close wooden vessels to Bata-'via, and other parts of India.'

The original of Kœmpfer, p. 794. speaking of the true Japan varnish, is as follows: "Prostat non sin-"cera modo, sed et colorata, vel cinnabari nativa "Sinensi, vel terra rubra (quam Batavi antea, nunc "Sinenses advehunt) vel atramenti popularis ma-"teriâ."

Which I apprehend may be read thus in English:

'This varnish is not only sold quite pure, but 'likewise coloured, and that with Chinese native 'cinnabar, and a kind of red earth, which the 'Dutch formerly, but now the Chinese, bring them; 'and also with the materials that they make their 'common (or Japan) ink of.'

Mr. Miller translates it thus (*See p.* 164. *vol.* xlix. *Phil. Transact.*): 'This varnish is used without mix-'ture to stain black: but the Chinese mix native 'cinnabar, or a red kind of earth, with it, to make 'a different colour.'

Here

Here we may obſerve, that Mr. Miller uſes the words ſtaining black; which is not the ſenſe of the author, who, by mentioning the materials of Japan ink, ſhews, that even in varniſhing black it was neceſſary to uſe this black mixture.

Further, Mr. Miller ſays, that the Chineſe mix theſe colouring ingredients with this varniſh: but the original plainly ſays, that the Chineſe import them, and the Japaneſe mix them with varniſh for ſale.

And in a former part of this letter, p. 162. vol. xlix. Phil. Tranſ. he ſays, ſpeaking of this true varniſh-tree, that callicuts are painted with the juice of this ſhrub. But this bare aſſertion of his, without producing a proper authority, I am perſuaded this Honourable Society will never admit as a matter of proof to invalidate the diſcovery of the Abbé Sauvages.

In looking over one of the numbers of Mr. Miller's Dictionary, under the title of Anacardium, I find he quotes a paſſage from Dr. Grew, which Sir Hans Sloane has placed among his obſervations on the Caſhew-tree, *Hiſt. Jam. vol.* ii. *p.* 127. which is, that cottons are ſtained with lime, and the oil, or mellaginous ſuccus, called Mel Anacardium (but for the account of this Mel Anacardium I ſhall refer to Parkinſon's Theat. p. 1568); and Mr. Miller ſeems to think it difficult to know which of the Anacardiums is here meant.

One would be apt think, from this paſſage, and another that follows a little after in the ſame page of the Hiſt. of Jamaica, relating to the black dye of the mellagoof this nut, that Sir Hans, at the time his hiſtory

was publiſhed, thought them, as Caſpar Bauhin did, of the ſame genus, but different ſpecies; and therefore he has mixt the obſervations on both together.

For, immediately after mentioning the ſtaining of cottons with this mellaginous ſuccus, Sir Hans ſays, that the gum is, in faculties and colour, like gum-arabic; and that it is given internally in female obſtructions; and that the juice ſtains linen, which will not waſh out ſuddenly: but he ſays it is falſe, that they remain till they flower next year, as Du Tertre aſſerts.

Sir Hans further quotes, from an anonymous Braſilian author, that the apples ſtain linen; and that the gum is good to paint and write; and the bark dyes yarn and veſſels ſerving for pots.

And in another place he quotes De Laet, who compiled a general hiſtory of America, and who likewiſe takes his quotation from an old Braſilian author, treating of the trees of Braſil, That the gum of the Acajou is uſed by painters; the bark is uſed to dye cotton-yarn and earthen ware. Here I muſt remark, tho' foreign to our preſent purpoſe, that in the original of Laet, what relates to the earthen ware runs thus: "Et a faire de vaiſſeaux de terre." So that I believe it will appear more probable, that the bark of theſe trees was uſed rather to burn earthen ware veſſels, than to dye them, as we find theſe earthen veſſels were uſed to boil their victuals in.

Theſe two quotations from Sir Hans Sloane confirm the former, with regard to the uſe of the gum; that is, its being fit, like gum-arabic, to be uſed for water-colours, and to make ink; and that it is the juice of the apple that ſtains, but this we find is not durable.

Mr.

Mr. Miller has now only the bark of the Caſhew-tree left to ſupport his argument. This the above-mentioned Braſilian writers ſay, that the native Indians of Braſil uſed to dye their cotton-yarn with; but of what colour no mention is made. And whether this bark is uſed to give ſtrength to this yarn, as we dye and tan our fiſhing-nets with oak-bark, or for ornament, is uncertain; for a great deal of this yarn was uſed in the making their net-hammocks, as well as their coarſe garments.

Mr. Miller then introduces Sir Hans Sloane, in oppoſition to Dr. Browne, whoſe Hiſtory of Jamaica I had quoted, to prove that the juice of the Acajou was of the ſame nature and properties with that of the gum-arabic, and conſequently not fit for varniſh: whereas it plainly appears from the foregoing quotations, taken from Sir Hans Sloane, that Dr. Browne is right, and agrees exactly in opinion with him.

He then makes Sir Hans ſay, that the inhabitants of Jamaica ſtain their cottons with the bark of the Caſhew-nut tree. By this, one would naturally conclude, that Mr. Miller has been endeavouring to prove, in oppoſition to the Abbé Mazeas's letter, that the art of painting or ſtaining cottons of a fine deep black colour, equal to that diſcovered by the Abbé Sauvages, as deſcribed in his experiments on the Carolina Toxicodendron, was practiſed by the Engliſh forty or fifty years ago in Jamaica.

If this was the caſe, it is ſomething ſurpriſing, that, notwithſtanding our great intercourſe with that iſland, the callico-printers of England never got intelligence of this valuable ſecret.

Further, if Mr. Miller will conſult Piſo and Margrave,

grave, writers of the beſt authority on the Braſilian plants, he will find their accounts of the Acajou exactly correſpond with that delivered by Dr. Browne, in his Hiſtory of Jamaica, as well as Sir Hans Sloane's: for they ſay, that the juice of this tree is equal in virtue, and mechanical uſes, to the beſt gum-arabic. And if he ſtill doubts, I ſhall laſtly recommend him to go to the Britiſh Muſeum, and there he may ſee a moſt elegant ſpecimen of the Caſhew-gum, which will put this matter quite out of all doubt.

I ſhall now leave the deciſion of this controverſy, which Mr. Miller has obliged me ſo fully to explain in my own vindication, to the candour and impartiality of this Honourable Society.

P. S. Since the foregoing paper was read, Profeſſor Sibthorp was ſo kind to deliver me an exact drawing of the *Faſi-no-ki* in the Sherardian collection at Oxford, taken by the Rev. Mr. William Borlaſe, F. R. S. the title and ſynonym of which are both in the Handwriting of Dr. Dillenius, as the Profeſſor aſſures me. See TAB. XVIII.

LVII.

Toxicodendron foliis alatis fructu Rhomboide Sl. Oth. from Japan.

W. Horsfall delin.
J. M. Sc.

Ex Horto meo Sherardi Oxon.

LVII. *A Letter to the Rev.* Thomas Birch, *D. D. Secr. R. S. concerning the Number of the People of* England; *by the Rev. Mr.* Richard Forſter, *Rector of* Great Shefford *in* Berkſhire.

Revd Sir, Shefford, Nov. 9. 1757.

Read Dec. 22. 1757.

SINCE I did myſelf the honour of writing to you in July †, my bookſeller has ſent me part ii. of vol. xlix. of the Tranſactions; in which * I find another medium advanced to determine the amount of the people in England: and this is the number of houſes, which pay the window-tax, and which "amount to about 690,000, "beſides cottages, that pay nothing." To this is added, that "tho' the number of cottages be not ac-"curately known, it appears from the accounts given "in, that they cannot amount to above 200,000."

Here I cannot but expreſs my concern, that this very ingenious gentleman has not been a little more explicit, by informing us, what theſe accounts are, upon which he builds ſo poſitive a concluſion. The law requires no ſuch accounts to be delivered in; and pariſh-officers cannot be accuſed of works of ſupererogation: beſides (which is more to the purpoſe) I am very certain no ſuch accounts have been given in from this part of the world. On the other hand, in all parts of England, which I have ſeen (and that is, I think, almoſt the whole) the number of cottages greatly exceeds that of all other houſes, except in the middle of towns, and ſome villages about Lon-

† See above, p. 356. * Page 887.

don. This is agreeable to the general interpretation of that ſentence paſſed upon our original parent, that *he ſhould eat bread by the ſweat of his brows*; which is, that the majority of his deſcendents ſhould be poor labouring people. This I do not mention with deſign to defend the interpretation, but only to ſhew the general ſenſe of mankind.

As my notion of the matter differs ſo widely from that of this worthy gentleman, I did every thing in my power to check any miſtake, which might ariſe from a fondneſs of one's own opinion; and which, I hope, will vindicate me in the eye of every candid inquirer. In a word, I ſet myſelf to count all the houſes in ſeveral contiguous pariſhes; and then examined how many of them paid the window-tax, or duty upon houſes. And here I muſt obſerve, that if there be any ſmall miſtake, it can hardly be ſuppoſed to be in favour of my own ſcheme; becauſe I had the whole number of houſes, by counting as I rode along; and ſome might poſſibly be miſſed, tho' of this I took the utmoſt care: whereas the number of thoſe, that pay the window-tax, I had from the collectors rolls.

The following table is the fruit of my labours:

Great Shefford	—	90	—	17
Little Shefford	—	12	—	3
Welford —	—	162	—	62
Chaddleworth	—	62	—	20
Bright-Walton	—	72	—	21
Catmore —	—	10	—	1
Farmborough	—	34	—	5
Fawley —	—	47	—	7
Eaſt Garſton	—	99	—	41
		588	—	177

Here

Here we ſee, that out of 588 houſes only 177 pay the window-tax. Now if we ſay with the philopher *ex pede Herculem*, and ſuppoſe, that 200,000 taxable houſes ſtand in the country, we ſhall have the following proportion, 177 : 588 : : 200,000 : 664406, for the whole number of houſes that ſtand in the country, commonly ſo called.

Again, Lamborn pariſh, in which is a market-town, contains 445 houſes, of which 229 pay the window-tax. Now if we ſuppoſe, in like manner, 200,000 taxable houſes to ſtand in country towns (I mean of the middling and inferior claſſes), we muſt then ſay 229 : 445 : : 200,000 : 388646, the whole number of houſes, that ſtand in country towns.

The remaining 290,000 houſes muſt be placed in cities and flouriſhing towns; and muſt have Dr. Brakenridge's proportion aſſigned them; for without all doubt he had ſome reaſon for pitching upon ſuch numbers; and as they could not be taken from country towns or villages, muſt be aſſumed from the preſent ſtate of ſome flouriſhing place. Upon this ſuppoſition, we muſt ſay 690,000 : 200,000 :: 290,000 : 84,058. for the number of cottages in great towns; which, if added to the houſes that pay, makes the whole number in large towns to be 374,058. Theſe three ſums added together make the total amount of houſes in the nation to be

664,406
388,646
374,058
1,427,110

 The

The two former of theſe numbers ſhould be multiplied by 5, and the latter by 6. The reaſon of this difference is the great quantity of ſervants kept in large towns.

$$1{,}053{,}052 \times 5 = 5{,}265{,}260$$
$$374{,}058 \times 6 = 2{,}244{,}348$$
$$7{,}509{,}608$$

By this way of proceeding it appears, that the whole number of people now alive in England is ſomewhat more than ſeven millions and an half. I would not be underſtood, as if I meant to recommend this as exact; tho' I am in hopes, that, upon trial, it will be found nearer the truth, than any thing hitherto advanced. Neither will I lay any ſtreſs upon its approaching ſo near to the numbers advanced in my former letter; being ſenſible, that all the methods I have hitherto tried are liable to very great objections. Where certainty may be arrived at by a little induſtry, all hypotheſis ſhould be deſpiſed and rejected.

The militia act levies 32,000 men upon the whole kingdom; and in the weſt riding of Yorkſhire 1 in 45, if my intelligence is right, completed their quota. Now if this proportion be applied to the whole nation, 32,000 × 45 will give 1,440,000 for the number of ballotters; and this multiplied by 5 (which, conſidering the number of perſons excepted, muſt be under the truth) will amount to 7,200,000 for the total of our people. But I dare not build any thing upon this computation, as many parts of the nation may have heavier quota's laid upon them than the weſt riding.

Whether the kingdom is really in a declining or increaſing ſtate, is, in like manner, a problem not to be ſolved, I think, by mere calculation. If there happens but a ſmall miſtake in the principles, what is built thereupon will be extremely wide of the truth. If one might take the liberty to gueſs by appearances, I ſhould think we are greatly increaſed within theſe forty years, or ſince the acceſſion of the preſent Royal Family. This conjecture I found upon the great facility, with which the government raiſes men, compared to the violent methods made uſe of in King William's and Queen Anne's time. Indeed I am ſenſible, that when the great eaſe, with which the government raiſes money, and the low intereſt it pays, have been urged in the Houſe of Commons, as evident proofs of a flouriſhing trade, and plenty of caſh, it has conſtantly been anſwered by a gentleman, who underſtands theſe matters better than any body elſe, that they are rather proofs of a want of trade, and that people do not know what to do with their money. In the ſame manner it may be anſwered, that the great facility, with which the government raiſes ſoldiers, is not owing ſo much to the great plenty of men, as to the want of employment: which it is poſſible may really be the caſe.

But where certainty may be had, it is trifling to talk of appearances and conjectures. For a century now paſt, the Engliſh way of philoſophiſing (and all the reſt of the world is come into it) is not to ſit down in one's ſtudy, and form an hypotheſis, and then ſtrive to wreſt all nature to it; but to look abroad into the world, and ſee how nature works; and then to build upon certain matter of fact. In compliance with this noble method, I have done all

in

in my power: I have examined the regiſters of ſeveral neighbouring pariſhes, and ſend you the ſubſtance of three of the moſt perfect ones. Indeed, I could have added ſeveral others; but as they ſeem to have been now and then neglected, I did not care to truſt to them. However, this I can ſafely deduce from them; *viz.* that what I have here ſent will be a proper ſtandard for theſe parts: and if other gentlemen would take the like pains (and it is next to nothing) in four or five pariſhes in each county, and in every great town, we might perceive, by one caſt of the eye, whether our people are in an ebbing or flowing ſtate. I have not ſet down the burials, as that would but have embarraſſed the table; and the increaſe will appear very well without them. However, upon an average of all the pariſhes I have examined, the proportion of the burials to the baptiſms is as 83 to 149,4.

	Lamborn.	Welford.	Shefford.	Total.
From 1614 to 1623 incluſ.	327	67	69	463
1624 to 1633 ——	401	62	64	527
1634 to 1643 ——	391	119	86	596
1662 to 1671 ——	441	146	93	680
1672 to 1681 ——	380	132	108	620
1682 to 1691 ——	451	201	112	764
1692 to 1701 ——	366	134	88	588
1702 to 1711 ——	387	137	84	608
1712 to 1721 ——	422	171	97	690
1722 to 1731 ——	483	156	106	745
1732 to 1741 ——	578	205	128	911
1742 to 1751 ——	566	253	137	956
1752 to 1756 ——	349	120	64	533

This

This table ſtands in need of no remarks: it ſpeaks loud enough of itſelf, that our people increaſe in a very rapid manner. All I ſhall take the liberty of obſerving from it is, that all the regiſters I have looked over ſeem to reſent the wretched policy of King Charles II. who ſubmitted himſelf and kingdom too much to a powerful neighbour: and that our civil war had no effect upon our numbers, in compariſon to our foreign wars.

I truſt, that the very ingenious author of the *politico-arithmetical* letters, I have all along had my eye upon, will take no offence, if I recommend an article or two advanced by him to be reconſidered; which, if purſued, might perhaps induce ſome ſmall errors in government.

The firſt is, That all ways to increaſe our people would be for the public welfare, even the naturalizeing of foreigners: whereas, if I remember right, all political writers lay it down as a maxim, that numbers of people without employment are a burden and diſeaſe to the body politic; and where there is full employment, there the people multiply of courſe. So that we ſhould not meaſure the happineſs of the nation by the number of mouths, but by the number of hands. Nay, if we were to import a quantity of foreigners, we muſt immediately re-export them, as we actually did in the caſe of the Palatines and Saltzburghers. Indeed, I cannot deny, but that if the new-comers were to bring new trades with them, they would be welcome: tho' I apprehend it is not an eaſy matter to find out many new manufactures. I can at preſent think of nothing but the cambrick

buſineſs;

buſineſs; and that, with a little encouragement, might be eſtabliſhed in either Scotland or Ireland, without the importation of ſtrangers.

The next thing I propoſe to be ruminated is the aſſertion, That our commerce at ſea is one cauſe of the decay of our fencible men: which ſounds in my ear like ſaying, that if we had leſs trade, we ſhould have more people. And if this is the purport of it, I am afraid it is a paradox, literally ſo called.

That emigrations to our colonies do leſſen our numbers in appearance, is beyond diſpute: but then it is only in appearance: for if employment begets people, the filling our plantations muſt increaſe us beyond imagination, it having been made out, if I miſremember not, that every man rightly occupied in America finds employment for three perſons in Old England. But then care ſhould be taken, that the planters were generally employed in raiſing rough materials; and that every thing imported there were manufactured by ourſelves; becauſe, if we ſettle colonies, and then ſupply them with Eaſt-India ſtuffs and foreign linens, it is neither better nor worſe than being at a vaſt expence to maintain other people's poor.

I cannot conclude without begging leave to obſerve, that this gentleman's doctrine is, from beginning to end, to ſay the beſt of it, ill timed. We are contending with our hereditary enemy, the moſt powerful prince in the world, not for ſuperiority, but for independence, *pro aris et focis*. And, at ſuch a time as this, to be told, that we are but little better than half peopled, and the few we have

dwindling

dwindling away every day, is indeed very discouraging: whereas, on the contrary, I do not balance one moment to declare it, as my fixt persuasion, that we can spare 100,000 brisk young fellows, and still be the most populous flourishing nation in Europe.

I am,

Reverend Sir,

Your affectionate Brother,

and very humble Servant,

Richard Forster.

LVIII. *A Letter to the Right Honourable the Earl of* Macclesfield, *President of the* Royal Society, *from the Rev.* William Brakenridge, *D.D. F.R.S. containing an Answer to the Account of the Numbers and Increase of the People of England, by the Rev. Mr.* Forster.

My Lord,

Read Mar. 16, 1758.

AS I endeavoured, at a former meeting of the Society, to answer extempore some objections offered by a Gentleman in the country, to what I have wrote concerning the number of people in England; I now presume to send you what I said then in writing, with some farther reflections. And this subject I never intended to

have meddled with any more; but as I ſeem to be called upon, to defend what I have formerly wrote, I hope I ſhall be excuſed if I briefly attempt it. Your Lordſhip, I know, and our illuſtrious Body only deſire a fair repreſentation of facts, which is the ground of all philoſophical inquiries; and therefore I ſhall endeavour to do this, as far as I can, without regarding any hypotheſis.

My deſign, when I firſt entered on this ſubject, was to diſcover whether our people were in an increaſing or decreaſing ſtate, with regard to their numbers; which I thought of great importance to be known, becauſe of its influence on the affairs of Government, in determining our ſtrength, in ſettling of taxes, and directing us in the œconomy and imployment of our people. Now, in order to proceed in this inquiry, it was evident to me, that if the number of houſes were exactly known, the number of people would be nearly aſcertained. And therefore I attended to this, to find out the number of houſes, as the only thing that could with any certainty help us to judge of this matter. And accordingly, being reſolved to depend only upon the moſt ſure, and general obſervations, I applied to a public office, where I thought I might poſſibly get at their number. And I there found, that from the laſt ſurvey that was made, ſince the year 1750, there were 690,700 houſes in England and Wales that paid the window-tax, and the two-ſhilling duty on houſes; beſides cottages that paid nothing. By cottages are underſtood thoſe who neither pay to church or poor, and are, by act of parliament in 1747, in conſideration of the poverty of the people, declared to be exempted both from the

tax

tax and the two-ſhillings duty; and they only remain not accurately known, to aſcertain the whole number of houſes. However, they are ſo far known, that from all the accounts that are hitherto given in, they do not appear to be ſo many as 300,000; and from what I myſelf have ſeen, in the books of that office, I ſhould think they were not much above 200,000; for in ſome places, that I was perfectly acquainted with, I found many of the day labourers rated to the two-ſhillings duty, and there did not appear to be one houſe in ten omitted. And therefore, if there are not 300,000 cottages, as ſeems plain to me, there cannot be a million of houſes in the whole in England and Wales; and the rated houſes are to the cottages more than two to one; of both which, according to the returns made, there is now about one in ſeventeen or 58,800 empty throughout the kingdom. But if we were to allow, that there are a million of houſes in the whole; which is more than the Gentlemen in the above mentioned office believe, and then deduct thoſe that are empty, there could not be above 941,200 inhabited houſes; and conſequently ſuppoſing ſix to a houſe, about 5,647,200 people, or near about five millions and an half; which at the utmoſt, is what I inſiſt on to be the real number.

But now the Gentleman, who objects to my calculations, thinks, that I have made the number of houſes too few, and that in the whole there are above 1,400,000 houſes, of which he imagines there are more than 700,000 cottages; for he ſuppoſes them to be more than the rated houſes; and from thence he infers, that there are about ſeven millions and an half of people, in England and Wales; which I wiſh, with all

my heart, was the true number: But I am ſo far from thinking that I have under-rated them, that I ſuſpect I have rather made them more than they are. However, this controverſy will ſoon be determined, there being now orders given, as I am informed, to all the Officers concerned in the window-tax, to make an exact return of all the cottages, as well as the rated houſes, in each of their ſeveral diſtricts. In the mean time, the Gentleman and I differ in this, that he ſuppoſes above 400,000 cottages more than I can poſſibly imagine.

Let us now ſee upon what grounds, and by what method of reaſoning he determines his numbers. He makes a diviſion of the 690,000 taxed houſes into three claſſes, placing 200,000 of them in the open country and villages, and 200,000 in the market and inferior towns, and the next, *viz.* 290,000, in the the cities and great towns; for which diviſion he has nothing to direct him; no proof, nor even probability. And as it is a mere arbitrary ſuppoſition, all reaſoning and calculations founded upon it are nothing to the purpoſe, and the number of houſes or people comptued from thence muſt be falſe or uncertain. But yet, upon this ſuppoſition, as if it was abſolutely certain, he goes on to compute the houſes and people in each diviſion.

As to the firſt, he ſays he has counted all the houſes in nine contiguous pariſhes in Berkſhire, in which he has found the whole number to be 588, and thoſe charged to the duty to be only 177; and therefore the cottages are to the rated houſes as 411 to 177, or above two to one. And from this he aſſumes, that the whole number of houſes thro' the villages and open country in England will

be

be to the cottages nearly in the ſame proportion. But here I am ſurpriſed, that he ſhould reaſon in ſo looſe and an inaccurate a manner. For, as there may be 7000 pariſhes in the villages and open country, to infer from the numbers in nine of them that are contiguous, and that all of them together do not make a very large pariſh, many being much larger as to the number of houſes, and where there may be particular circumſtances; I ſay, to infer from them what the proportion will be in all pariſhes, in the villages and open country, is the ſame way of reaſoning as to ſay, becauſe the poor in one pariſh are in ſuch a proportion, therefore they are ſo in 1000 pariſhes, or thro' four or five counties: whereas it is plain, that the proportion differs almoſt in every pariſh, and in every county; and the ſum of all muſt be added together, before we can know what the real proportion is. And nothing can be inferred from the circumſtances of a few pariſhes, or even of a County, what the proportion will be in the whole. And yet, from ſuch precarious and vague reaſoning he preſumes to compute, that there are above 460,000 cottages in the villages and open country; having aſſumed, without any heſitation, that there are 200,000 rated houſes in that extent. Such reaſoning is unuſual in philoſophcal inquiries.

In like manner the Gentleman reaſons very inaccurately about his ſecond diviſion, containing the leſſer market and country towns, having ſuppoſed in them 200,000 taxed houſes: For from one inſtance of the market town of Langborne, having found the whole number of houſes to be to the cottages as 445 to 229, or the rated houſes to the cottages as 216 to 229, he ſuppoſes the like proportion in all the market:

ket towns. That is, tho' there be perhaps above 300 market towns in England, he ſuppoſes each of them has the ſame proportion of the poor in it as the ſingle town of Langborne; which is unreaſonable to imagine. For every one of them may have a different proportion, according to the various circumſtances of their trade and ſituation. But yet from this ſtrange and uncertain way of reaſoning, without any induction, and from one inſtance among 300 caſes at leaſt, he concludes by proportion, that there are 388,646 houſes in the country market towns, of which there are 188,646 cottages, beſides thoſe in the cities and great towns.

In the next place, as to his third claſs, the cities and great towns, he allows, that my proportion may be among them, *viz.* that the rated houſes are to the cottages as 690,000 to 200,000, or 69 to 20: For he thinks, that it cannot be any-where but in the moſt flouriſhing places. And therefore, as he has arbitrarily placed 260,000 taxed houſes in them, he computes that they muſt contain 84,058 cottages. But he has given no proof, that my proportion is only in the moſt flouriſhing places, beſides theſe few inſtances that he has produced; which are nothing to form any general concluſion upon. For if we were to be directed by a few caſes, we might think that there were much fewer cottages than I have allowed. There are ſome pariſhes, in which there are none at all. In the great pariſhes of St. James's and St George's Weſtminſter, in which there are about 7000 houſes, there are none: in the country pariſh of Chiſelherſt in Kent, where there are above 100 houſes, there are but three: and in many pariſhes there is not one in 20. So that from particular inſtances, there is nothing

to be concluded. But in all Middlesex, London, Westminster, and Southwark included, in which the poor are as numerous as in most places in the kingdom, because of the numbers of labouring people that flock hither for imployment, there is nearly the same proportion that I have assigned. For from a late survey in that district, as I am informed, there are 87,614 houses in the whole, and of these 19,324 cottages, and 4810 empty. Which indeed shews, that we are not so populous, in and near the metropolis, as is commonly supposed, and much less than I had calculated in my first letter: For from this account, if it be true, there are not above 530,000 people in that compass; of which, within the bills of mortality, there die about 25,000 yearly; that is, not less than one in 20.

As to what the Gentleman mentions concerning the militia, he seems to be much mistaken. For if the proportion be as he says, that one in 45 is levied, this directly proves the number of people in England and Wales to be about five millions and an half, according to my calculation; because the electors or balloters are the fencible men, or those able to carry arms. And if the whole levy be 32,000, then 45 multiplied by 32,000 will give 1,440,000 for all the fencible men in England. But Dr. Halley has clearly shewn, that the fencible men are one quarter of the whole people, children included; and therefore, four times 1,440,000, or 5,760,000, will be the whole number of the people; which is nearly what I have made them.

And thus, having seen how he has established his numbers in opposition to me, let us now, in the next place,

place, conſider what he has ſaid with regard to the increaſe of our people. He ſays, whether the kingdom is really in a declining or increaſing ſtate, is a problem not to be ſolved by calculation: And yet he himſelf can gueſs by appearances, that it has greatly increaſed within theſe 40 years. But, by his good leave I muſt tell him, that it is a problem in political arithmetic to be ſolved from ſome *data*, as well as others. If the number of people be nearly found, and the general proportion of births to burials, at an average, thro' the kingdom be known, with the annual loſſes of our fencible men, at a moderate computation; from theſe *data*, I ſay, any one, who underſtands numbers, will eaſily determine whether we are increaſing or decreaſing. And accordingly, I have ſhewn, that the annual increment of our fencible men is not much above 8000, which number is conſumed by our annual loſſes; and therefore we are not in an increaſing ſtate. For the whole number of people muſt always be in proportion to the fencible men; ſo that, if there is no increaſe of them, there can be none upon the whole.

It is true, I am the firſt who ventured upon a ſolution of this queſtion; but when I conſider what I have done, I cannot ſee but that the principles upon which I reaſoned are right. The *data* are, I think, exact enough to diſcover our ſtate. And Dr. Halley's rule to compute the fencible men, where our loſſes are to be reckoned, is undoubtedly true. So that if there is any difficulty, it is in fixing the general proportion between births and burials, thro' the kingdom, *viz.* 112 to 100; which I have taken from Dr. Derham, who had collected many obſervations; being a greater

a greater proportion than Sir William Petty allowed. And which if it is thought too ſmall, it is to be conſidered, that within the bills of mortality the births are much under the burials as 4 to 5; and in ſome of the great towns there are fewer births than burials, and in others they are nearly equal; ſo that theſe reduce the proportion that ariſes from the villages and open country.

But if we were to make a calculation from the births and burials, only in the villages and open country; which Dr. Derham has found to be at an average as 117 to 100, or nearly as 7 to 6; and ſuppoſe this to obtain all over Britain and Ireland, in the towns as well as the country, which is ſurely more than the truth; we ſhall then find, that the annual increment cannot be more than 9000 fencible men; which corroborates my former eſtimate. For, to compute it by the principles I have formerly endeavoured to eſtabliſh; let the number of our people in Britain and Ireland be eight millions and an half, that is, five and an half in England and three millions in Scotland and Ireland; becauſe ſome Iriſh Gentlemen have aſſured me, from ſome facts, that there is half a million more in their country than I formerly allowed; for I did not pretend to calculate them; and then the annual number of the dead, in Britain and Ireland, being one in 40, will be about 212,500; which will be to the births as, 100 to 117: And therefore the births muſt be 248,625, and the increaſe 36,125; of which the fourth part is about 9000 for the fencible men, which I am perſuaded is more than the real number.

Now let any one compute our loſſes in the moderate way that I have done, and he will eaſily ſee, that

 they

they cannot be leſs than this number; and conſequently we are far from increaſing. And indeed it is evident from the number of empty houſes thro' the kingdom, mentioned above, *viz.* one in ſeventeen, or 58,000, and one in twelve of thoſe that are taxed within the bills of mortality. For it is impoſſible, if we were increaſing, that there could be ſo many empty; And therefore the appearance of ſo much building is only the effect of our luxury, requiring larger, more convenient, and more elegant houſes, and not cauſed by our increaſe.

However, the Gentleman objects to all this, and ſays, that he has examined the Regiſters of ſome neighbouring pariſhes, and particularly of three that are perfect; and he finds, that the burials are to the baptiſms as 83 to 149; which may poſſibly be the caſe, as I myſelf have known it in one pariſh in the Iſle of Wight, where the place is healthy, and people generally marry. But does he imagine that this proportion is general all over England? If ſo, we ſhould increaſe in a rapid manner indeed! for then we ſhould double our people in 35 years, if it were not for our loſſes; which no reaſonable man will venture to ſay. He does not reflect, that in many country places, from their bad ſituation, there is very little increaſe, and in ſome towns none at all, and in others a decreaſe, continually ſupplied from the neighbouring country. Within the bills of mortality there are annually 5000 burials more than the births; and conſequently, to maintain our numbers here, there muſt be a yearly ſupply of 5000; which deſtroys the whole increaſe of ſix or ſeven counties. And Dr. Derham found, from the accounts he had of country pariſhes, that

that in general among them the proportion of births to burials was not greater than 117 to 100, as we mentioned above; ſo that nothing can be concluded from particular healthy places. The queſtion is, what is the reſult upon the whole thro' the kingdom? what is the general proportion of the births to burials, from which the increaſe is to be eſtimated? and which Sir William Petty ſays is 111 to 100, and Dr. Derham as 112 to 100. See if he can diſprove theſe numbers by putting together all the different accounts from every corner, among the towns as well as the country; and if he cannot, to argue only from a few inſtances is nothing to the purpoſe; for where there there is a multitude of different caſes, they muſt all be conſidered, to arrive at the general truth. But even in the particulars he mentions, he has not completed his argument; for, to make it concluſive, he ſhould have ſhewn, that, within theſe laſt forty years, the time, he thinks, of our great increaſe, in thoſe pariſhes the number of houſes or people were increaſed, in proportion almoſt as the births were above the burials, as 149 to 83: and if that cannot be made to appear, it is plain, that, for all he has ſaid, the annual increaſe may be conſtantly conſumed by our loſſes.

And now the worthy Gentleman having endeavoured to ſhew, from the caſe of a few pariſhes in the country, that we are in an increaſing ſtate, he proceeds to give me his ſerious advice in two particulars:

Firſt, That I would reconſider a propoſition advanced by me, That all reaſonable ways of increaſing our people, even to the naturalizing of foreigners,

would be for the public welfare. In anſwer to which kind admonition I muſt ſay, that I have often conſiderd the thing, as far as I can; and I think this may be eaſily ſhewn againſt any political writer, That it is the intereſt of a government, when they have powerful and dangerous neighbours, to increaſe their people by all reaſonable means, even to the inviting of foreigners, ſo far as the natural produce of the country can ſuſtain them; and that it is the fault or weakneſs of an adminiſtration not to be able to employ them. And in Britain, where they can have the aſſiſtance of the produce of ſo many large and fruitful countries of their own in America, I will venture to ſay, that it is an error in their policy, not to endeavour to increaſe their people; by which they might be more formidable, and perhaps ſtronger than their grand Enemy. The preſent King of Pruſſia has ſhewn the utility of this within his dominions; by which he has been enabled to make ſuch a figure in Europe.

The *ſecond* thing he admoniſhes me to reconſider is, That I have ſuppoſed our commerce to be one cauſe of the loſs of our fencible men. And who in the world doubts of it, but himſelf! Do ſhipwrecks, the diſaſters and inclemency of the ſea, the ſcurvy, *&c.* beget people? But he will ſay, without theſe we could not have trade, which employs great numbers of our people; and therefore, what we loſe, we may gain another way. And juſt ſo he may ſay of our wars, that occaſion the deſtruction of ſo many of our people, that they are no loſs to us; for we gain by them in their conſequences, in ſecuring of our liberties and property, and by which

our

our trade is preſerved and promoted. But notwithſtanding this, can it be ſaid, that war does not diminiſh our fencible men! The truth is, trade increaſes riches, and gives more of the conveniencies of life, and brings luxury along with it; but it does not neceſſarily breed people: For we ſee in thoſe countries where they have little trade, the people increaſe much faſter than they do with us, as appears from the Bills of mortality in Pruſſia; where the general proportion of the births to the burials is greater than it is here, *viz.* 4 to 3; and by which the people might double in 84 years, if it were not for their loſſes. (*Vid. Phil. Tranſ. vol.* xxxvi.) Which great increaſe, by the way, eaſily accounts for thoſe vaſt ſwarms of people that came from thence and the adjacent countries in former ages, and over run all Europe. And therefore it is not ſo terrible a paradox, as he imagines, that poſſibly where there is much leſs trade the people may increaſe faſter; for luxury and other vices, that come with trade, do not promote an increaſe.

And now, as he has been ſo good as to give me his advice, I will return the favour, and deſire him to reconſider the method of reaſoning by induction; which may poſſibly help him to eſcape ſome paralogiſms, in arguing upon theſe ſubjects. And I would likewiſe recommend it to him to inquire diligently, whether the number of our houſes in England be increaſed theſe laſt ſixty years; which, according to his reaſoning, ought at leaſt to be doubled: For if there is no increaſe of the houſes, there can be none of the people.

To conclude: He adds, that my doctrine, from beginning

beginning to end, to ſay the beſt of it, is ill-timed, when we are contending with our hereditary enemy, *pro aris & focis.* But here his zeal hurries him on, that he does not look to the dates of my Letters. For the firſt three were read before the Society, and ordered to be printed, long before the war was proclaimed; and as for the laſt, it is only a ſupplement to the reſt; in which I have ſhewn, that France, by the bad œconomy of her people, is not in an increaſing ſtate; which, I think, is a comfortable hearing. But ſuppoſing they had been all printed during the war: What then? Is a fact to be concealed that, if diſcovered, may be uſeful to prevent errors in government, and rectify our notions of the œconomy of our people? What advantage can our enemies make of ſuch a diſcovery? Will it encourage them to imagine that we ſhall be eaſier ſubdued, when they know, by the moſt moderate computation, we have at leaſt two millions of fencible men in our Britiſh iſlands. Enough, ſurely, to reſiſt them in all their attempts! But I doubt we are not ſo deficient in our numbers as in public virtue, without which the greateſt multitude may be eaſily overcome.

And thus, my Lord, I have endeavoured to anſwer what this Gentleman has wrote in his ſecond Letter; for I paſs over the firſt, as it does not ſeem to contain any more in oppoſition to me, than what I have here conſidered. And upon the whole I cannot ſee, that he has ſaid any thing to invalidate what I have formerly advanced. If I could diſcover it, I ſhould be very ready to acknowlege my error.

I am ſenſible I have made this reply too long; but I truſt your uſual benevolence to all our worthy Members will excuſe me, who ſhall always eſteem it an honour to be,

My Lord,

Your Lordſhip's

Sion-College,
March 16. 1758.

Moſt obedient

and faithful Servant,

Wm. Brakenridge.

END of PART I. VOL. L.