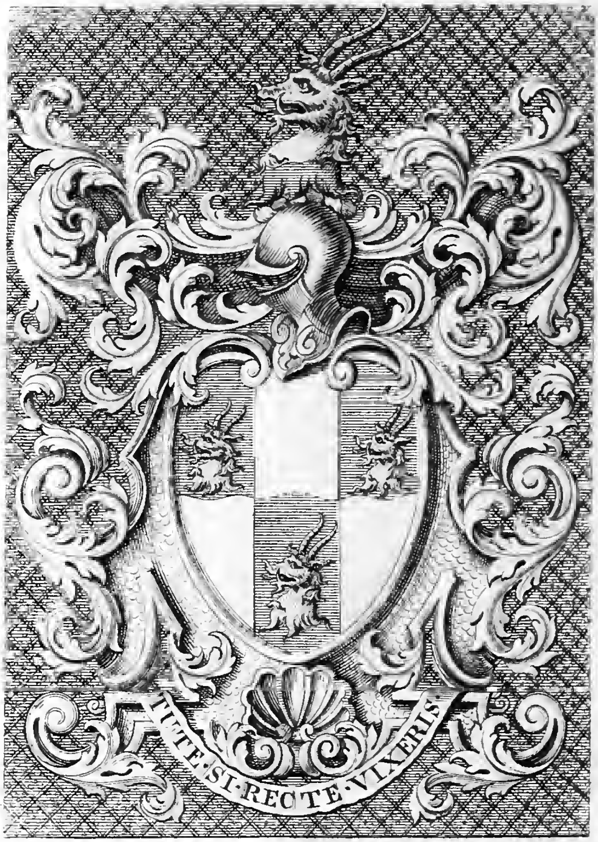
TUTE·SI·RECTE·VIXERIS

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. LXIV. PART I.

LONDON:

Printed for LOCKYER DAVIS, in *Holbourn*,
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M.DCC.LXXIV.

ADVERTISEMENT.

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

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

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

At a COUNCIL, January 28, 1773.

Reſolved, That after Volume LXII. the *Philoſophical Tranſactions* be publiſhed twice in a year; the firſt publication to be of the months of November and December of the preceding year, and January and February of the current year, as ſoon as may be after February, under the name of the "firſt part" of the volume: and the ſecond publication to be of the remaining months unto the receſs of the Society, as ſoon as may be after the receſs, under the name of the "ſecond part" of the volume.

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VOL. LXIV. PART I.

PHILO-

PHILOSOPHICAL TRANSACTIONS.

I. *Observations on the* Solar Spots. *By* Alexander Wilson, *M. D. Professor of Practical Astronomy in the University of* Glasgow. *Communicated by the Rev.* Nevil Maskelyne, *Astronomer Royal.*

Redde, Apr. 29, 1773.

WHEN the great COPERNICUS had revived the true system of the world, it was objected against it, that the planet Venus ought to be seen with different phases like the moon; to which he answered, that some time or another that very thing would perhaps come to pass.

paſs. Who then would have thought, that this prophecy was ſo near being fulfilled, and that, by means of combining a convex and a concave glaſs, the common uſes of which had been known for near 300 years before, the ſight of man was to be extended almoſt to the remoteſt parts of the univerſe? This inſtrument, which nothing but its being ſo common hinders us from regarding with the greateſt wonder, was invented, about 170 years ago, by one of thoſe happy accidents, to which we owe many of the fineſt diſcoveries.

At that time flouriſhed the famous GALILEO, who was the firſt that conſtructed a teleſcope from ſome knowledge of the refraction of the rays of light. Having finiſhed one in 1610, he directed it to the heavens, and thus entered upon the moſt pleaſing and moſt ſtriking field of obſervation, which any philoſopher ever enjoyed. The name of GALILEO will always be familiar to us, ſo long as there remains any remembrance of thoſe celebrated diſcoveries, which he then made by the help of his glaſſes.

One of the moſt remarkable of them was that of the SOLAR SPOTS. So ſtrange a diſcovery, relating to the ſun, commanded a great deal of attention. At that time the authority of Ariſtotle, by controlling the reaſon of men, governed their opinions in all parts of philoſophy; and one of the abſurd doctrines, then taught in the ſchools, was, that the matter of the heavens was ungenerated, incorruptible, and ſubject to no alteration. But the arguments, which GALILEO now produced againſt that notion, were founded upon the evidence of ſenſe, and it was impoſ-

impoſſible to oppoſe them. The ſpots upon the ſun, and the many ſtrange variations, to which they were ſubject, convinced all, who were able to lay aſide the prejudices of a vain philoſophy, that there was not ſo great a difference, betwixt celeſtial and ſublunary bodies, as had been imagined.

To ſuch as were ſo reconciled to it, the diſcovery appeared grand and ſtriking; and great hopes were entertained, that, by duly attending to the phænomena of the ſpots, ſomething curious and important might be determined concerning the nature and conſtitution of the ſun itſelf.

We accordingly find, that many aſtronomers, of the firſt note, were very early engaged in this inquiry. Of all thoſe who applied themſelves this way, SCHEINER and HEVELIUS deſervedly hold the firſt place, and nothing but the charms of ſo noble an inveſtigation could have induced them to proſecute their obſervations with ſo much aſſiduity. SCHEINER began his in the year 1624, 14 years after GALILEO had firſt made the diſcovery. In the year 1630, he at laſt publiſhed his Roſa Urſina, in which we have a detail of his labours during that long interval of time. HEVELIUS came after SCHEINER, and diligently watched the appearances of the ſpots for two years, the reſult of which application he has given us in his Selenographia and Cometographia.

But notwithſtanding theſe attempts, ſo worthy of men actuated by a true deſire of knowledge, it muſt be confeſſed, that nothing of moment hath been derived from them. If we except a few concluſions concerning the rotation of the ſun round its axis, and the inclination of its axis to the plane of the

 ecliptic,

ecliptic, every thing elſe, which hath been inferred from the phænomena of the ſpots, ſeems altogether to be matter of conjecture. HEVELIUS, from his great fondneſs of the ſubject, and from a deſire to avail himſelf of that long courſe of obſervation, to which he had ſo patiently ſubmitted, has been led into many ſpeculations concerning the ſpots and the nature of the ſun's body. The following quotations furniſh us with a remarkable inſtance of this, and will ſerve to give us a view of the ideas, which he came to entertain upon theſe ſubjects. In his Cometographia, p 360, ſpeaking of the ſolar ſpots he expreſſes himſelf thus:

"Hæc materia nunc ea ipſa eſt evaporatio et ex-
"halatio (quia aliunde minime oriri poteſt) quæ ex
"ipſo corpore ſolis, ut ſupra oſtenſum eſt, expiratur
"et exhalatur: quæ penitus ex diverſis partibus
"heterogeneis, certos gradus opacitatis et denſitatis
"habentibus, conſtat, modo ex tenuioribus, modo
"opacioribus, modo ſiccioribus, modo magis viſ-
"coſis, glutinoſis (ut noſtro loquar more uſitato)
"ſive bituminoſis. Unde etiam diverſa procreantur
"phænomena; ex tenuioribus, purioribus, et ſicciori-
"bus, umbræ videlicet faculæ et luculæ; ex impuri-
"oribus vero craſſioribuſque illis viſcoſis, maculæ
"eorundemque nuclei progignuntur. Cum ergo
"ſolis exhalationes ejuſmodi partes ſint hetero-
"geneæ, quidni etiam illud ipſum corpus, quod eas
"ejaculatur, prout in præcedentibus fuſe aſſertum
"eſt, ex diverſis partibus heterogeneis conſtabit?
"Ad hæc, quia tales admirabiles et manifeſtiſſimæ
"generationes, mutationes, alterationes, condenſa-
"tiones, rarefactiones, coitiones, diductiones, imo

"motus

"motus locales corporum, ac rerum materiarumque
"ex ipſo ſole ejectarum evaporatarumque, pene
"continenter, ad inſtar nubium ac meteororum ſub-
"lunarium ex terrâ promanantium, peragi videmus;
"nonne clare ex eo evincitur, ſolem habere ſuam
"atmoſphæram, in eâque dari, pro virtutis ſuæ
"indole, generationes et corruptiones rebus ſublu-
"naribus haud diſſimiles varioreſque? imo, non-
"nunquam longe frequentiores, et inſigniores ac-
"cidere (ſi præſertim obſervationes macularum a
"Scheinero nobiſque habitas exacte examinaveris)
"quam prope hanc terram unquam poſſint?" And again, when ſpeaking of the ſun's body: "Atque
"ita hic liquor igneus (ut ſic loqui liceat) eſt quaſi
"vaſtiſſimum luminum pelagus et mare igneum,
"quod ſuos habet abyſſus, occultos meatus, vora-
"gines atque vortices; quod, ad inſtar maris noſtri,
"perpetuis fluctuum quibuſdam voluminibus agi-
"tatur, et ſuo modo evaporationes et exhalationes
"jugiter evomit atque exhalat: eoque magis aut
"minus, quo magis vel minus materia iſta in viſ-
"ceribus ejus, atque intimis receſſibus, igneum
"illud pelagus, illiuſque fluctus commoventur ac
"concitantur."

But all that we here find, however plauſible and ingenious, can be regarded only as conjecture. It does not appear, that any who have followed HEVELIUS have met with more ſucceſs. Their obſervations ſeem not to differ from his in any remarkable circumſtance; nor do we find, that their inferences from them, although ſometimes different, have any better pretenſions to the truth. The many ſtrange and variable circumſtances of the ſpots, which were diſco discoverable from a minute observation, still remained unaccountable; and we often find them at a loss, in framing any hypothesis, which could fully satisfy the mind concerning them. In process of time, astronomers began to withdraw their attention from a subject, which remained so dark and perplexing, and, for many years, all researches of this sort have been, in a great measure, laid aside.

Chance, or a happy concurrence of circumstances, hath sometimes effected more, than could have been expected from the most promising measures: a remark which, it is hoped, will, in some degree, be found justified in the sequel of this paper. The observations upon the solar spots, which I now proceed to relate, appear to be totally different from any hitherto to be found, and such as seem to open a new and curious field of speculation into the whole of this subject.

Astronomers will remember, that a spot of an extraordinary size appeared upon the sun, in the month of November 1769. The first notice, I had of it, was by a letter from a friend at London, but the weather continued dark and cloudy for some days after; at length, on the 22d day, I had a view of the sun through an excellent Gregorian telescope, of 26 inch focus, which magnified 112 times. I then beheld the spot, which at that time was not far from the sun's western limb, and below his equatorial diameter. The atmosphere being now very clear, and free from all tremor and undulation, it was pleasant to see the nucleus of the spot, and the shady zone or umbra which surrounded it, so very distinct. It being afternoon when I first got sight of it, I had not an opportunity of observing it long that day. Next day being the 23d, I had a curiosity to see it again, and so repaired to my telescope, in order to examine, if any alterations, in the size and figure, had taken place since last observation. The air was still favourable, and I again saw the spot, it having its nucleus and umbra very sharply defined. I now found however a remarkable change; for the umbra, which before was equally broad all round the nucleus, appeared much contracted *on that part which lay towards the center of the disc*, whilst the other parts of it remained nearly of their former dimensions.

This change of the umbra seemed somewhat extraordinary, as it was the very reverse of what I expected from the motion of the spot towards the limb. But next day, at 10 o'clock, I had another observation, and discovered changes, which were still more unexpected. The distance of the spot from the limb was now about 24″. By this time, the contracted side of the umbra above mentioned had entirely vanished; and the figure of the nucleus was now remarkably changed, from what it had been the preceding day. This alteration of the figure appeared evidently to have taken place upon that side which had now lost the umbra, the breadth of the nucleus being thereby more suddenly impaired than it ought to have been, by the motion of the spot across the disc. Fig. 1, 2, [TAB. I.] represent the appearance which the spot had on the 23d and 24th days.

Regarding theſe circumſtances as new, I began to conſider, what might be the cauſe of them. One of two things ſeemed neceſſarily to be the caſe; either, that they were owing to ſome phyſical alteration or waſting of the ſpot, and of that part of it where the deficiency of the umbra was obſerved; or elſe, that they were owing to the nearer approach of the ſpot to the limb, by the ſun's rotation on his axis.

The laſt of theſe two ideas had no ſooner ſtruck me, than I began to ſuſpect, that the central part, or nucleus of this ſpot, was beneath the level of the ſun's ſpherical ſurface; and that the ſhady zone or umbra, which ſurrounded it, might be nothing elſe but the ſhelving ſides of the luminous matter of the ſun, reaching from his ſurface, in every direction, down to the nucleus: for, upon this ſuppoſition, I perceived, that a juſt account could be given of the changes, of the umbra and of the figure of the nucleus, above deſcribed.

The opinion therefore, which I ventured to form from what I had ſeen this day, was, that this ſpot might, probably, be a vaſt EXCAVATION in the luminous matter of the ſun; the nucleus, commonly ſo called, being the bottom, and the umbra the ſhelving ſides of the excavation: and, moreover, that the umbra, next the center of the diſc, although out of my view, did ſtill however exiſt, and was rendered inviſible by its preſent poſition only; and further, that the ſudden alterations, now diſcernible in the figure of the nucleus, were occaſioned by ſome part of it alſo being hid, by the interpoſition

Suns Eastern Limb.

Basire sc.

N.° 1. Great Spot Nov. 23. 1769.
2 .... 24.
3 .... Dec.r 11. for the second time.
4 12
5 17

pofition of the edge of the excavation, between the nucleus and the eye.

Thefe views, which now prefented themfelves, I remember to have communicated, that afternoon, to my fon; when I then told him, that, if they were well founded, there would be room to verify them, if the fpot fhould again return upon the oppofite fide of the difc. I was however uncertain, if it would laft fo long upon the fun's body, as to be again vifible after the time of half a revolution; a circumftance which I wifhed to take place, as I was aware, that my prefent obfervations might juftly be deemed infufficient, for eftablifhing fo fingular an opinion concerning the nature of this fpot; and that, notwithftanding all which I had feen, we might ftill imagine, that thefe changes were produced by certain phyfical alterations of the fpot itfelf.

Thefe confiderations made me attentively wait its return. At laft, on December 11th, I again difcovered it, on the oppofite fide of the difc, it having by that time advanced a little way from the eaftern limb, being diftant from it 1′ 30″. And now I could only perceive three fides of the umbra, namely, the upper and under fides, and that towards the limb, which was the fide that formerly had vanifhed. The fide towards the center of the difc was not as yet vifible; but I concluded, upon the fame grounds as formerly, that it was hid from my fight, by its averted pofition only, and that, after the fpot had advanced a little further, it would make its appearance. Accordingly, the next day, being December 12th, at ten o' clock, it came into

 view,

view, and I ſaw it diſtinctly, though narrower than the other ſides. After this, my obſervations were interrupted, by unfavourable weather, till the 17th, when the ſpot had paſſed the center of the diſc, the umbra now appearing to ſurround the nucleus equally. Fig. 3, 4, and 5, repreſent the ſpot, as it appeared on December 11th, 12th, and 17th, when it came upon the diſc for the ſecond time.

All the foregoing appearances, when taken together, and when duly conſidered, ſeem to prove in the moſt convincing manner, that the nucleus of this ſpot was conſiderably beneath the level of the ſun's ſpherical ſurface.

The next thing, which I took into conſideration, was to think of ſome means, whereby I could form an eſtimate of its depth. At the time of the obſervation I had on December 12th, I had remarked, that the breadth of the ſide of the umbra, next the limb, was about 14″; but, for determining the point in queſtion, it was alſo requiſite, to know the inclination of the ſhelving ſide of the umbra to the ſun's ſpherical ſurface. And here it occurred, that, in the caſe of a large ſpot, this would, in ſome meaſure, be deduced from obſervation. For, at the time when the ſide of the umbra is juſt hid, or begins firſt to come in view, it is evident, that a line joining the eye and its obſerved edge, or uppermoſt limit, coincides with the plane of its declivity. By meaſuring therefore the diſtance of the edge from the limb, when this change takes place, and by repreſenting it by a projection, the inclination or declivity in ſome meaſure may be aſcertained. For in fig. 5. [Tab. II.] let I L D K be a portion of the ſun's limb, and A B C D a ſection of the ſpot, S L the ſun's ſemidiameter, L G the obſerved diſtance from the limb, when the ſide of the umbra changes, then will the plane of the umbra, C D, coincide with the line, E D G, drawn perpendicular to S L, at the point G. Let F H be a tangent to the limb, at the point D, and join S D.

Since G L, the verſed ſine of the angle L S D, is given by obſervation, that angle is given; which, by the figure, is equal to F D E, or G D H; which angle is therefore given, and is the angle of inclination of the plane of the umbra to the ſun's ſpherical ſurface. In the ſmall triangle therefore C M D, which may be conſidered as rectangular, the angle M D C is given, and the ſide D C, equal to A B, is given nearly, by obſervation; therefore the ſide M C is given, which may be regarded as the depth of the nucleus, without any material error.

I had not an opportunity, in the courſe of the foregoing obſervations, to meaſure the diſtance G L, not having ſeen the ſpot, at the time when either of the ſides of the umbra changed. It is however certain, that, when the ſpot came upon the diſc for the ſecond time, this change happened ſometime in the night between the 11th and 12th of December; and I judge that the diſtance of the plane of the umbra, when in a line with the eye, muſt have been about 1′ 35.″ from the ſun's eaſtern limb; from which we may ſafely conclude, that the nucleus of the ſpot was, at that time, not leſs than a ſemidiameter of the earth, below the level of the ſun's ſpherical ſurface, and made the bottom of an amazing cavity, from the furface downwards, whofe other dimenfions were of much greater extent.

Being thus perfwaded of the depreffion of this great fpot below the furface, I immediately fet about examining fmaller ones, in order to difcover if they were of the fame kind. With this view, I began a courfe of obfervations, that from them I might either make the inference univerfal, or limit it, as the phœnomena fhould point out. I was not long engaged in this purfuit, before I perceived in them the fame changes of their umbræ, which have been defcribed above at fo much length. This was manifeft in fpots of any confiderable fize, when the air was favourable, and the telefcope well adjufted for diftinct vifion. The firft, which I faw undergo this change upon its near approach to the limb, was on January 17th, 1770, as reprefented in Fig. 1. [TAB. II.] The figures 2, 3, 4, in this plate, and 1, 2, 3, 4, [TAB. III.] are fo many other inftances taken from the regifter of the obfervations at that time. But, as this appearance, of the umbra changing, is evident from a bare infpection of the figures, we have omitted any unneceffary defcription of them. Thefe eight cafes are taken out from above 40 others of the fame kind; and in them all, the nuclei were in the middle of the umbræ, before their approach to the limb. It may be remarked, that in fig. 2. [TAB. II.] the fame fpot *c* traverfed the difc and had its umbra changed, both in the coming on and going off. In general, we have found that the umbra thus changes, when a fpot is about a minute diftant from the limb, at a medium.

From

Fig. 1.

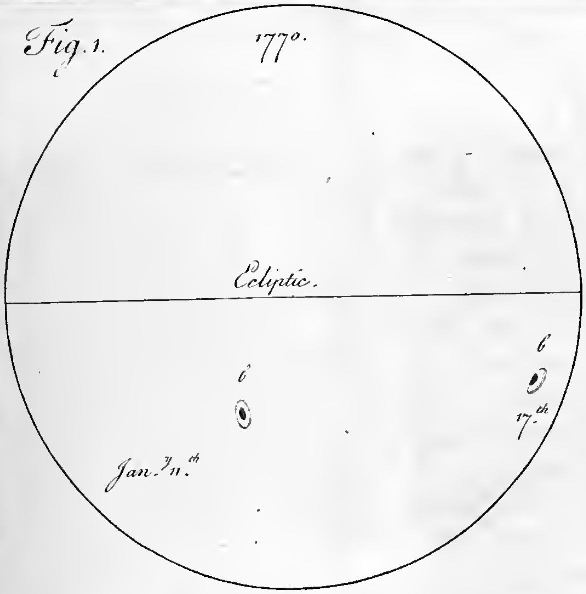


Fig. 2.

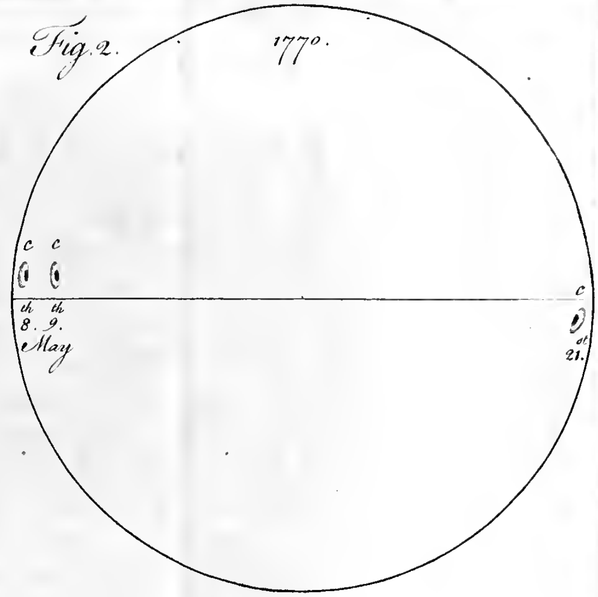


Fig. 3.

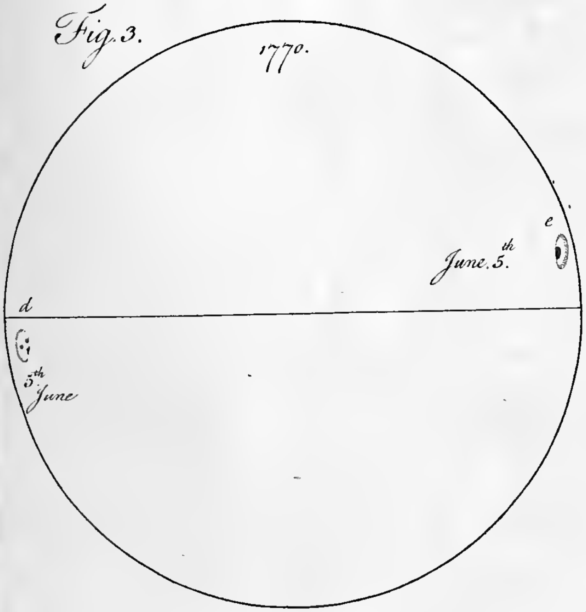
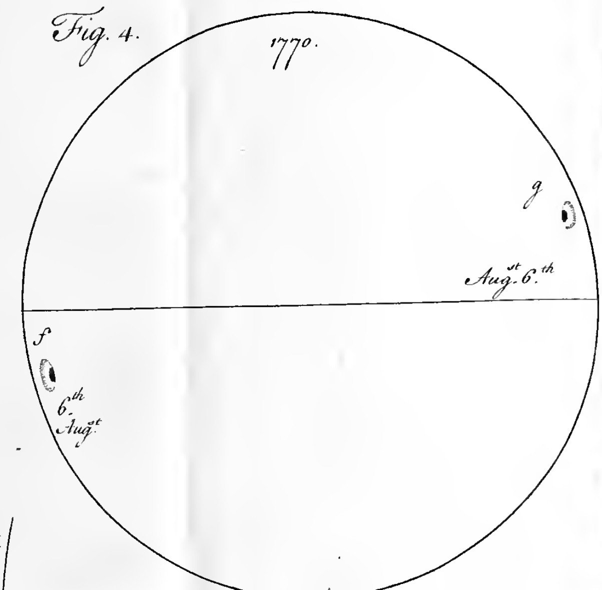


Fig. 4.



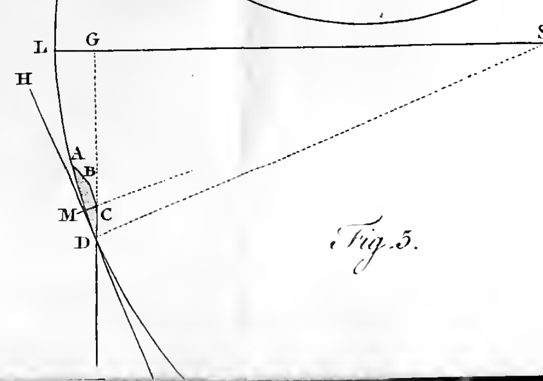


Fig. 5.

From all these observations, may we not safely conclude, that every spot consisting of a nucleus and surrounding umbra, as defined by SCHEINER and HEVELIUS, is of the same kind with those above described? But as, in researches of this sort, we can never be too cautious in making a general conclusion, so I would humbly recommend these observations to every lover of the subject, to those especially who are provided with large and good telescopes; for, without this advantage, I have found by repeated trials, that I could not discern the minute changes of the small spots.

In the course of the foregoing observations, I had occasion to remark, five different times, another extraordinary circumstance of the spots, which I have not seen mentioned, by any one who has written upon the subject. It consists of changes, which seem to arise from a disturbing force, when one spot breaks out in the neighbourhood of another. The first case of this sort which I met with, was on Nov. 9th, 1770, when the umbra of the spot *m*, fig. 1. [TAB. IV.] although a great way from the limb, was deficient towards the right hand, at which side, and very near it, there lay another spot much smaller. In like manner, the two spots *n* and *o*, fig. 2. [TAB. IV.] which lay very near one another, had each of them that side of its umbra, which faced the other, taken away. But it was remarkable, that, three days after, the spot *o* had nearly vanished, when the side of the umbra of the spot *n*, which faced it, began now to dilate. In fig. 3, the spot *p*, had its umbra flattened upon opposite sides, by three small spots on one hand, and one upon the

 other.

other. Again, in fig. 4, the two ſpots *q*, *r*, had their umbræ deficient, by the intervention of ſome ſmall ſpots, that lay between them.

Now it muſt here be particularly remarked, that though a ſpot, when undiſturbed, will, when near the ſun's limb, exhibit the change of the umbra formerly mentioned, yet it is plain, that a caſe may now and then occur, when this change will be counteracted, by means of the phænomenon which we have juſt now deſcribed. For if we ſhould ſuppoſe, for example, the ſpot *m*, fig. 1. TAB. IV. to have been on November 9th, near the weſtern limb, it is evident, that we ſhould have formed a different judgment concerning the change of the umbra. And accordingly, in the courſe of the obſervations formerly mentioned, I in reality met with three caſes, when this change did not take place.

I am ſenſible, that it may be thought ſtrange, that none of the obſervers, who had looked at the ſolar ſpots with ſo much attention, ſhould ever have taken notice of the gradual changes above deſcribed. This partly may be accounted for from the following conſiderations. We have already ſeen, that conjectures, concerning the nature of the ſun, were early indulged in the courſe of this inquiry. His body was thought to be an immenſe globe of fire, which was for ever raging with the moſt fervent heat. Hence the firſt obſervers, reflecting upon the perpetual generation, changes, and decay of the ſpots, and that through ſo wide an extent of his ſurface, very naturally imagined, that they could conſiſt of nothing but ſmoke and

groſſer

groſſer exhalations, or ſuch tranſient and periſhable materials. This hypotheſis had at leaſt the air of being ſupported by a very plauſible analogy. The minds of men being carried away by ſuch prepoſſeſſions, it would leſs readily occur, that ſucceſsful obſervations were only to be made, by an accurate and critical attention to thoſe minute changes, which the ſpots ſometimes undergo. But what would ſtill more conduce to this overſight, was the method, which moſt of them followed, in making their obſervations. This was by the camera obſcura, which both SCHEINER and HEVELIUS often uſed, and which we find greatly extolled by them, and deſcribed at great length in their writings. But ſpots, when ſeen in this way, have nothing of that diſtinctneſs, which is ſo remarkable, and ſo pleaſing, when they are viewed directly through a good teleſcope armed with an telioſcope, or glaſs properly ſmoaked.

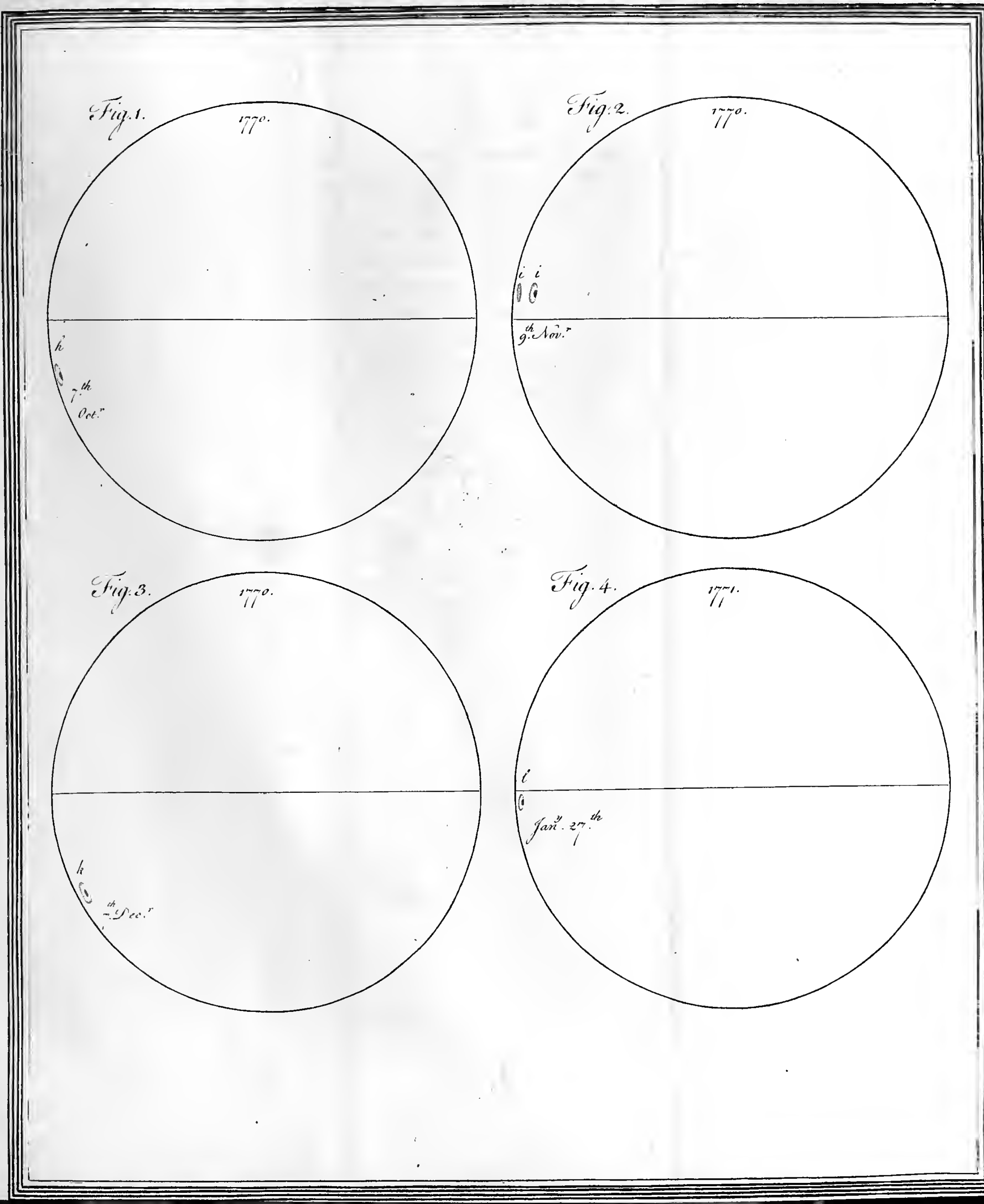
PART

PART II.

IT appears then that the ſolar ſpots are immenſe excavations in the body of the ſun; and that what hitherto hath been called the nucleus is the bottom, and what hath been called the umbra the ſloping ſides of the excavation. It alſo appears, that the ſolar matter, at the depth of the nucleus, does not emit light, or emits ſo little, as to appear dark compared to that reſplendent ſubſtance at the ſurface; that this beauteous ſubſtance is at the ſurface, moſt fulgid; and when any of it is, ſeen below the general level, forming the ſides of an excavation, that then its luſtre is ſomehow impaired, ſo as to give the appearance of a ſurrounding umbra. Here our induction ends. To proceed further would be to carry it beyond its true limits, and to intermix with concluſions, which are certain and manifeſt, the ſuggeſtion of hypotheſes, which at beſt are precarious and liable to error.

But from what we have now ſeen, many curious ſpeculations do naturally preſent themſelves. By what myſterious proceſs is it, that thoſe aſtoniſhing excavations are at firſt produced? What is the nature of that ſhining ſubſtance, which is thereby perpetually diſturbed? To what are we to aſcribe the darkneſs of the nucleus, and the diminiſhed luſtre of the umbra? And what conceptions are we to form of the many ſtrange changes, and at length of the final decay of all theſe appearances, whereby thoſe regions of the ſun, that were ſo hurt and diſfigured, again undergo a renovation?

We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We often find SCHEINER and HEVELIUS mentioning many things concerning the ſpots, which appeared to them very inexplicable. HEVELIUS, when ſpeaking of the vaſt number of ſpots which break out upon the ſun, and of the prodigious ſize of ſome of them, admires how from his ſingle body ſo much matter, exhalations, &c. could be generated, as in any degree to be adequate to ſo many and ſo vaſt phænomena. "Nuclei autem, " (ſays he, Cometograph. p. 401) macularum " ſcilicet partes denſiores, ſæpius unam partem cen- " teſimam, imo quinquageſimam, de ſolari dia- " metro occupant; ſic, ut paucis dicam, immania " et admiranda ſunt corpora. Adhæc, intellectum " humanum fere ſuperat, quomodo unquam, ex " unico iſto corpore ſolis, tantum materiæ, totque " exhalationes vaporeſque erumpere ac produci " queant, quo talia vaſtiſſima phænomena procreari " poſſint." Again, SCHEINER, when ſpeaking of that property of the ſpots, where a large nucleus is often divided into two or more nuclei, ſeems greatly puzzled to account for it. Sch. Roſa Urſina, p. 498, ſays: " Ex uno ſæpe magno nucleo " fiunt duo, tres, pluresve, non locali partium ip- " ſius diſtractione, ſed intervallorum neſcio quâ " exinanitione. Manent enim illorum centra in- " ter ſe æqualiter diſſita." Many inſtances of this ſort could be brought; and indeed, conſidering the contrariety betwixt any hypotheſis they had formed, and what would now ſeem to be the real condition of the phænomena, it is no wonder that ſuch difficulties ſhould occur. Every theory, how ingenious ſoever, which is founded upon a miſapprehenſion

of

of things, is apt to be preſſed with many difficulties; and whenever palpable contradictions appear, they may be regarded certainly as proofs of our having fallen into error. Upon this principle, I think, we might freely have rejected any theories, hitherto entertained, concerning the ſolar ſpots, tho' their falſity had not become manifeſt from more enlarged experience and obſervation. It muſt indeed be acknowledged, that it is very diſadvantageous to ſcience, to indulge much in hypotheſes, the truth being rarely hit upon in this way, and very often miſſed. Sometimes, however, it may not be improper, to throw out hints and conjectures, when we can attain to nothing better, provided we are at due pains to diſtinguiſh betwixt ſuch and that real knowledge, which we derive, by ſtrict induction, from inconteſtable principles. The beſt way therefore, of preſerving ſo proper and neceſſary a diſtinction, will be to propoſe what further remains to be ſaid, upon this ſubject, in the form of queries; becauſe, however plauſible they may appear, they are at beſt but matter of conjecture. Hints, when propounded in this way, are freed from the danger of making us reſt in any error, whilſt, ſooner or later, they may become helps in leading us to a right underſtanding of the ſubject.

The queries which we ſhall proceed to make, are chiefly founded upon the following phænomena of the ſpots, as deſcribed by Scheiner and Hevelius.

1. Every ſpot which hath a nucleus, hath alſo an umbra ſurrounding it. Vid. Scheiner, p. 496. Hev. p. 409. 349.

2. The boundary betwixt the nucleus and umbra is always diſtinct and well defined. Vid. Scheiner, p. 497.

3. The encreaſe of a ſpot is gradual, the breadth of the nucleus and umbra dilating at the ſame time. Vid. Scheiner, p. 491, &c.

4. In like manner the decreaſe of a ſpot is gradual, the breadth of the nucleus and umbra contracting at the ſame time. Sch. p. 491. 498.

5. The exterior boundary of the umbra never conſiſts of ſharp angles, but is always curvilinear, how irregular ſoever the out-line of the nucleus may be. Sch. 511.

6. The nucleus of a ſpot, whilſt on the decreaſe, in many caſes changes its figure, by the umbra encroaching irregularly upon it; inſomuch that, in a ſmall ſpace of time, new encroachments are diſcernible, whereby the boundary, betwixt the nucleus and umbra, is perpetually varying. Sch. 514. Hev. 412.

7. It often happens, by theſe encroachments, that the nucleus of a ſpot is divided into two or more nuclei. Sch. 498.

8. The nuclei of ſpots vaniſh ſooner than the umbræ. Many inſtances of this ſort are to be ſeen in Hevelius' plates, and the ſame is affirmed by Mr. Derham in The Philoſophical Tranſactions.

9. Small umbræ are frequently ſeen without nuclei. Sch. p. 497.

10. An umbra of any conſiderable ſize is ſeldom ſeen without a nucleus in the middle of it. Ibid.

11. When a ſpot, which conſiſted of a nucleus and umbra, is about to diſappear, if it is not ſuc-

ceeded by a facula, or more fulgid appearance, the place, which it occupied, is ſoon after not diſtinguiſhable from any other part of the ſun's ſurface.

This is certain from the accounts of all obſervers.

QUERIES *and* CONJECTURES, *tending to explain the above properties of the ſpots.*

When we conſider, that the ſolar ſpots, ſome of whoſe properties have juſt now been enumerated, are ſo many vaſt excavations in the luminous ſubſtance of the ſun, and that, wherever ſuch excavations are found, we always diſcern dark and obſcure parts ſituated below; is it not reaſonable to think, that the great and ſtupendous body of the ſun is made up of two kinds of matter, very different in their qualities; that by far the greater part is ſolid and dark; and that this immenſe and dark globe is encompaſſed with a thin covering of that reſplendent ſubſtance, from which the ſun would ſeem to derive the whole of his vivifying heat and energy? And will not this hypotheſis help to account for many phænomena of the ſpots in a ſatisfactory manner? For if a portion of this luminous covering were by any means diſplaced, ſo as to expoſe to our view a part of the internal dark globe, would not this give the appearance of a ſpot? In this caſe, would not that part of the dark globe, which is now laid bare, correſpond to the nucleus, and the ſloping ſides of the luminous matter to the umbra? And is not this conſonant to that property of a ſpot mentioned in the firſt

article;

article; for would it not hence follow, that every ſpot, having a nucleus, ſhould alſo have an umbra ſurrounding that nucleus, a natural account being at the ſame time ſuggeſted, for the boundary betwixt the nucleus and umbra being always diſtinctly defined, as mentioned in the ſecond article.

Although we may never have a competent notion of the nature and qualities of this ſhining and reſplendent ſubſtance, or of the means by which the excavations in it are formed; we however diſcover, in their production, the agency of ſome mighty, though unknown, cauſe, which is there often exerting itſelf. Although we manifeſtly behold its effects, yet the mode of its operations may perhaps remain unſearchable. But if we were here to venture a conjecture, might we not ſuppoſe, that the luminous matter is ſo diſturbed, and the excavations in it occaſioned, by the working of ſome ſort of elaſtic vapour, which is generated within the dark globe? And might not this elaſtic principle, by its expanſion, ſwell into ſuch a volume, as to reach up to the ſurface of the luminous matter, which would thereby be ſeparated and laid aſide in all directions? And foraſmuch as there is no regularity in the time of a ſpot's enlarging, compared to the time of its decreaſing, ſome enlarging quickly, and decreaſing ſlowly, and *vice verſa*, may we not imagine, that this is owing to the duration and quantity of the elaſtic principle now mentioned? and in general, may we not from hence form ſome idea of the production and ſubſequent enlargement of a ſpot, as mentioned in the 3d article?

But

But to proceed. As we know, from experience, that the ſpots are of a tranſient nature, not laſting upon the ſun for a long ſpace of time, does it not ſeem reaſonable to think, that their gradual decreaſe, as mentioned in article 4th, is occaſioned by the luminous matter encroaching again upon that part of the dark globe, which had been uncovered? And from this may we not infer, that the luminous matter gravitates, and is in ſome degree fluid; for thereby would it not have a tendency to flow down, in all directions, and encroach, ſo as at laſt to cover the nucleus? And do not theſe things appear further probable, when we reflect upon that uniform inclination, which the ſides of the umbra, or excavation, have to the external ſurface of the ſun's body? For does not this indicate a fluid ſort of matter gradually yielding to the force of gravity? And again, is not this notion further ſupported, when we conſider the property mentioned in the 5th article, namely, that the exterior boundary of the umbra never conſiſts of ſharp angles or turnings, but is always curvilinear, and, moſt frequently, of a round form: for we know, that this boundary is nothing elſe but the lip of the excavation, which, on ſuppoſition that the luminous matter poſſeſſes ſome degree of fluidity, will not be diſpoſed, either in enlarging or contracting, to become irregular by ſudden or ſharp turnings?

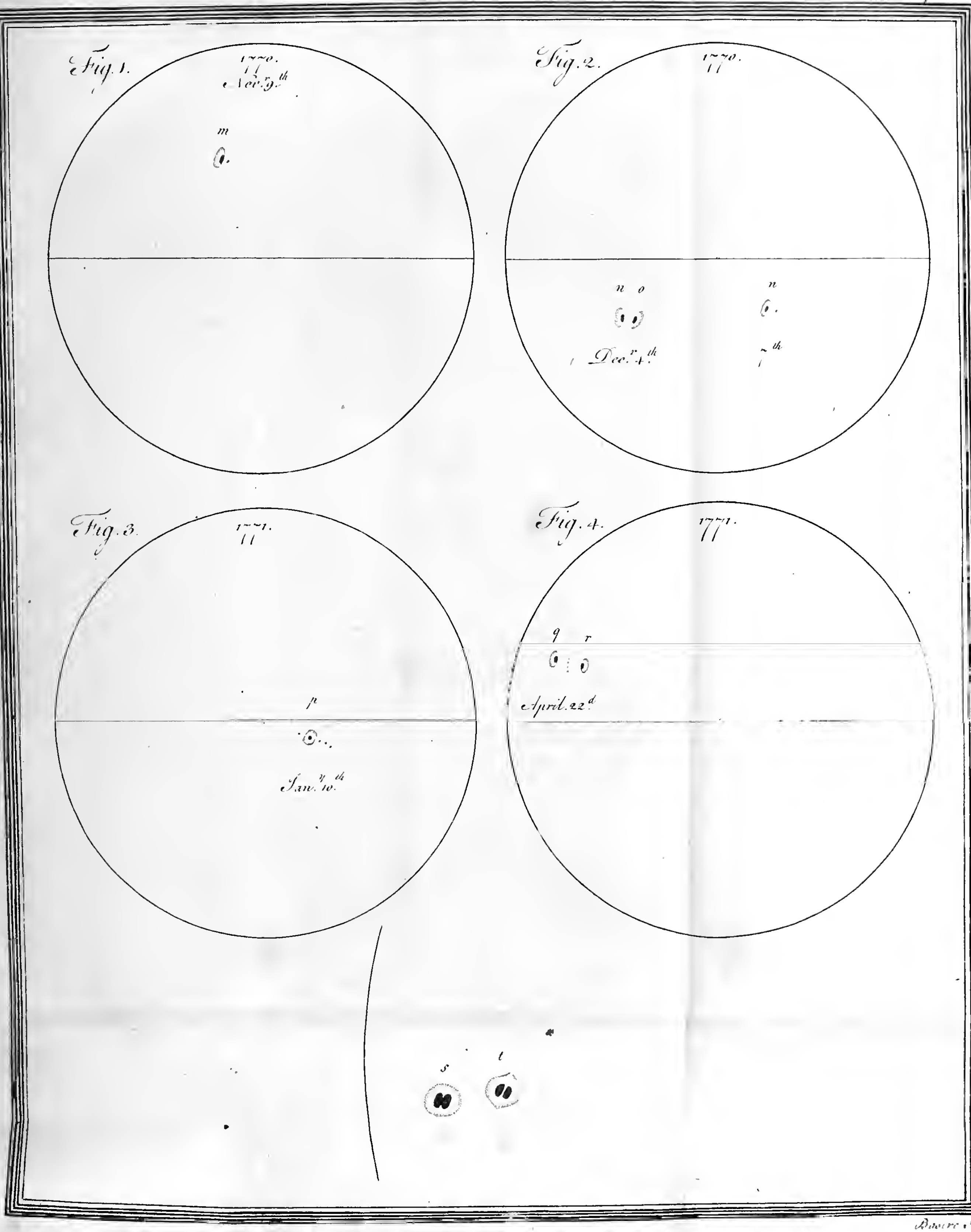
Upon ſuppoſition that the ſurface of the dark globe of the ſun is ſmooth and level, it may be urged, that the nucleus of a ſpot, whilſt upon the decreaſe, ſhould, according to the preſent view of things, always acquire a figure, at leaſt nearly, circular,

cular, and that the luminous matter, continuing to flow down on all ſides by an equal gravity, ſhould ſo encroach upon the nucleus, as to make it retain that figure, till at laſt it be entirely overflowed. But this not being the caſe, and becauſe it moſt frequently happens, that the encroachments of the umbra upon the nucleus are extremely variable, as mentioned in the 6th article, may we not from this infer, that the ſurface of the internal dark globe of the ſun, is by no means ſmooth and level, but on the contrary very irregular, for, upon this ſuppoſition, if for example the area of the nucleus of a great ſpot were ſo diverſified by mountains and vallies, would not the encroachments of the luminous matter be conſequently irregular; and, according as it was more or leſs retarded or accelerated, at different places, by being contiguous to prominencies or hollows, would not all the alterations in the figure of the decreaſing nucleus, how variable ſoever, be thus plainly accounted for? and becauſe it often happens, that the nucleus of a ſpot, whilſt on the decreaſe, is gradually cut in pieces by a luminous zone or zones, which wander acroſs it, as mentioned in the 7th article, does not this look like the gradual flowing in of the luminous matter, as it were, into deep channels, which would thus appear to abound in the ſurface of the ſun's dark body? If we reflect upon the irregularities, which are upon the ſurface of this earth, and upon the enormous mountains and cavities, which are in the moon, may we not, from ſuch analogy, imagine, that there may be the like, or much greater, irregularities in the ſurface of the

ſun?

ſun? I may here take notice of a curious inſtance of a nucleus being divided into two parts, which fell under my own obſervation. On October 8th, 1770, the ſpot *s*, TAB. IV. had a ſmall jutting in of the luminous matter, upon the oppoſite ſides of the nucleus, which, by the 10th day, had advanced contrary ways, ſo as to meet, by which means the nucleus was now divided into two parts, as repreſented by fig. *t*. It was here very remarkable, that the parts of the umbra oppoſite to this cut in the nucleus, were dilated as in fig. *t*. May not this dilatation have been occaſioned by the rapid flowing in of the luminous matter into the deep channel below?

Is not the property mentioned in the 8th article, namely, that the nucleus of a ſpot vaniſhes ſooner than the umbra, alſo agreeable to the preſent views? from this ſtate of the phænomenon, we ſuppoſe that that part of the ſun's dark body, which had been uncovered and expoſed to our view, when the ſpot firſt broke out, is now again juſt overflowed by the gradual inundation of the luminous matter. But, after the nucleus thus diſappears, may there not however, in many caſes, be ſtill left a cavity in the luminous matter, large enough to be perceived? and will not this cavity, ſo long as it continues, give the appearance of a ſmall undivided umbra? and will not this umbra ſtill be perceivable, till the luminous matter, by continuing to flow in, has filled up the cavity? after which, will not the place of the umbra acquire the ſame luſtre with the reſt of the ſun's ſurface, and thus will not all traces of the ſpot vaniſh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vanifh from his body? And do not the particulars mentioned in the 9th 10th and 11th articles feem agreeable to what is now faid?

Both SCHEINER and HEVELIUS feem to think, that fpots fometimes alter their place upon the difc, not only by the fun's rotation round his axis, but alfo by a motion, which they impute to the fpots themfelves. This I could never obferve. It is very true, that when a number of fmall fpots lye near one another, there may be from time to time a change of their relative fituation, but it is plain, that this may proceed entirely from fome of them encreafing and others diminifhing irregularly. But what would further contribute towards forming a judgment of this kind is, the apparent alteration of the relative place, which muft arife from the motion acrofs the difc on a fpherical furface; a circumftance which I am uncertain if it has been fufficiently attended to.

What has been advanced, in the courfe of the foregoing queries, may perhaps be rendered ftill more probable, by confidering the obfervations related in the firft part of this paper, concerning the changes which are made upon the figure of a fpot, when another breaks out in its neighbourhood; and which feem to arife from a difturbing force. For, from the cafes there laid down, would it not appear, that when a fpot is breaking out, the luminous matter is then forced, in all directions, from the nucleus, and is affected much in the fame manner, as it would be, were it a fluid matter encompaffing the fun's dark body?

 As

As to the particular nature and qualities of this luminous matter, we have been ſometimes apt to imagine, that it cannot well be any very ponderous fluid, but that it rather muſt reſemble as to its conſiſtence a very denſe and thick fog, which broods on the ſurface of the ſun's dark body. How far will this idea tend to facilitate our conceptions of the various phænomena of the ſpots above deſcribed?

It has been gathered from many obſervations, that the time which the ſpots take to traverſe the whole diſc, is nearly equal to the time that they are hid by being on the oppoſite ſurface. It is plain, that the time of their appearing upon the diſc muſt be ſome ſmall matter ſhorter than that of their being hid behind it, on account of our not ſeeing a complete hemiſphere of the ſun. But further, it muſt now be conſidered, that when a ſpot juſt enters the diſc, the part, which is firſt viſible, is the fartheſt umbra, by which time the ſpot has really advanced a whole diameter of itſelf upon the diſc. And again, when the ſame ſpot goes off the diſc, it is evident, that the part, which is laſt viſible, is then the fartheſt umbra, on which account the continuance of the ſpot upon the diſc will be ſhortened by an interval of time, which correſponds nearly to the whole breadth of it. This, as well as the other appearances, deſcribed in the firſt part of this paper, concerning the change of the umbra and figure of the nucleus, when ſpots approach the limb, are all well illuſtrated, by making, in a ſphere, an excavation ſimilar to what we have deſcribed, the bottom of which may be painted

black

black to repreſent the nucleus, and the ſloping ſides ſhaded, if the ſphere be of a light colour.

According to the view of things given in the foregoing queries, there would ſeem to be ſomething very extraordinary in the dark and unignited ſtate of the great internal globe of the ſun. Does not this ſeem to indicate, that the luminous matter, which encompaſſes it, derives not its ſplendor from any intenſity of heat? For, if this were the caſe, would not the parts underneath, which would be perpetually in contact with that glowing matter, be heated to ſuch a degree, as to become luminous and bright? At the ſame time it muſt be confeſſed, that although the internal globe was in reality much ignited, yet when any part of it, forming the nucleus of a ſpot, is expoſed to our view, and is ſeen in competition with a ſubſtance of ſuch amazing ſplendor, it is no wonder, that an inferior degree of light ſhould, in theſe circumſtances, be unperceivable.

In order to obtain ſome knowledge of this point, I think an experiment might be tried, if we had an opportunity of a very large ſpot, by making a contrivance in the eye-piece of a teleſcope, whereby an obſerver could look at the nucleus alone with the naked eye, without being in danger of light coming from any other part of the ſun. In this caſe, if the obſerver found no greater ſplendor, than what might be expected from a planet very near the ſun, and illumined by as much of his ſurface as correſponds to the ſpots umbra, we might reaſonably conclude, that the ſolar matter, at the depth of the nucleus, is in reality not ignited. But, from the nature of the thing, doth there ſeem any neceſſity for thinking, that there prevails there any ſuch raging and fervent heat, as many have imagined? It is proper here, to attend to the diſtinction betwixt this ſhining matter of the ſun, and the rays of light which proceed from it. It may perhaps be thought, that the re-action of the rays upon the matter, at their emiſſion, may be productive of a violent degree of heat. But whoever would urge this argument, in favour of the ſun being intenſely heated, as ariſing from the nature of the thing, ought to conſider, that all poliſhed bodies are leſs and leſs diſpoſed to be heated, by the action of the rays of light, in proportion as their ſurfaces are more poliſhed, and as their powers of reflection are brought to a greater degree of perfection. And is there not a ſtrong analogy betwixt the re-action of light upon matter, in caſes where it is reflected, and in caſes where it is emitted?

It may perhaps be expected, that, in this paper, mention ſhould be made of the other appearances, that are diſcernible upon the ſurface of the ſun, beſides the ſpots properly ſo called; I mean the faculæ, luculi, &c. as deſcribed by Scheiner and Hevelius. But all theſe phænomena ſeem to be ſo different from any thing we have conſidered, and ſo unconnected with the preſent diſcovery, that little aſſiſtance can be brought from that quarter towards a right conception of them. As to the faculæ, or brighter parts of the ſun, we are at a loſs for their origin. It may in general be remarked, that although we have obtained an expe-

rimental

rimental proof, that the luminous matter acquires ſome degree of ſhade, when forming the ſides of an excavation, yet it is uncertain, if this be merely the effect of poſition, and much more ſo, if any different modification of poſition could ever diſpoſe it to put on a brighter or more fulgid appearance. Yet, after all, may not theſe faculæ, &c. depend upon ſome irregularities in the bright ſurface of the ſun? For may not the luminous matter, by being agitated by the ſame cauſe to which the ſpots owe their origin, though in a leſs degree, have its ſurface perpetually diſturbed, and made irregular, and thus give occaſion to a variety of light and ſhade, ſufficient perhaps to produce the phænomena under conſideration? And does not this conjecture receive further confirmation, when we conſider, that theſe faculæ, &c. are found only in that zodiac, within which the ſpots appear, and that they always abound moſt in the neighbourhood of the ſpots themſelves, or where ſpots recently have been? For in thoſe undiſturbed regions of the ſun which lye towards his poles, and where no ſpots ever appear, and which SCHEINER calls the *plagæ æquabiles*, we never diſcover any diverſity of appearance.

Thus we have endeavoured to give a general idea of the production, changes, and decay of the ſolar ſpots, conſidered as excavations in the body of the ſun; a thing which ſeems to be eſtabliſhed from the obſervations deſcribed in the firſt part of this paper. But concerning the nature of that mighty agency, which occaſions thoſe amazing commotions in the luminous matter, or concerning

ing the denſity, viſcidity, and other qualities of this matter, or the manner in which it is diſturbed in the middle zone only, and not at the polar regions, and many ſuch other queſtions, I freely confeſs, that they far ſurpaſs my knowledge.

To conclude; as what hath now been ſaid may open a new field of inquiry into this ſubject, ſo a diſcuſſion of theſe curious points may, ſome time or other, fall to the ſhare of abler men, whoſe love of philoſophy may induce them to purſue ſo noble an inveſtigation.

II. *Astronomical Observations by the Missionaries at* Pekin. *Transmitted to the* Supra-cargoes *at* Canton, *by the Rev. Father* Louis Cipolla, *of the Tribunal of Mathematics, and communicated to the Royal Society by the Court of Directors of the* East-India Company *.

PREFACE, BY THE ASTRONOMER ROYAL.

Redde, Nov. 4, 1773.

MOST of the following Observations appear to have been made with a telescope of eight feet, to which a micrometer, for measuring differences of right ascension and declination was occasionally adapted. There was besides another telescope of eight feet, consisting of two object-glasses, in the manner of RÖEMER, which might be brought nearer together or separated, in order that the moon's diameter might completely fill a fine reticule, in the focus, divided into twelve equal parts, for measuring the digits eclipsed in lunar eclipses. It is mentioned in the account of the lunar eclipse of Nov. 12, 1761, that

* The Society was favoured with another copy of these Observations by John Blake, Esq. of Parliament-street, to whom they were transmitted by his son John Bradby Blake, Esq. Supra-cargo at Canton.

the clock was regulated by a tranſit inſtrument. It is therefore probable, it was regulated in the ſame manner in all the ſucceeding obſervations, which conſiſt of the following particulars.

1. Obſervations of the laſt tranſit of Venus over the Sun, viz. of differences of right aſcenſion and declination between Venus and the Sun, and the internal and external contact of the limbs at the egreſs. It is however remarked, that the clock was counted by a perſon, who was ſometimes found to make miſtakes.

2. The eclipſe of the Sun, May 25, 1770. The beginning and end were obſerved, and the lucid parts meaſured, during the eclipſe, with the micrometer.

3. The beginning and end of the eclipſe of the Moon, Oct. 23, 1771.

4. Emerſion of Jupiter from occultation by the Moon, July 5, 1770.

5. An Occultation of Spica Virginis by the Moon; the immerſion and emerſion both obſerved; Jan. 25, 1772.

6. The occultation of a ſtar in Scorpio by the Moon; the immerſion and emerſion both obſerved.

7. The obſervation of Venus in the Sun's parallel, Jan. 5, 1772, by taking the difference of right aſcenſion and declination of Venus and the Sun.

8. The total eclipſe of the Moon, Nov. 12, 1761. The beginning, total immerſion, emerſion, and end, were obſerved by three different obſervers, with teleſcopes of 5, 7, and 8 feet, in the domeſtic obſer-

vatory of the College of the Jesuits; where also all the former observations were made. The same eclipse was also observed at the Royal Observatory at Pekin, 14″ west of the Jesuit's College, with a telescope of 8 feet, composed of two object-glasses, with a reticule at the focus, divided into 12 equal intervals for measuring the digits eclipses, in the manner of RÖEMER. It is remarked, that the leaf, in which this observation was recorded, had been lost, and was found again, Oct. 12, 1772: on which account this observation was never transmitted to Europe before.

NEVIL MASKELYNE.

Tranſitus ♀ infra diſcum ⊙, die 4 Junii, anni 1769. Pekini, in reſidentiâ S. Joſephi, tubo 8 pedum, obſervatus.

Temp. ver. mat. h	′	″		′	″
4	42	5	limbus occ. ⊙ ad horarium		
4	44	21	limb. ort. ⊙ ad hor.		
	2	16	mora diametri ⊙ - - - -	31	38
		4	mora diam. ♀ - - - - - -		56
4	54	39	limb. occ. ⊙ ad hor.		
4	55	55	centr. ♀ ad hor.		
	1	16	♀ à limb. bor. ⊙ - - - -	5	5
5	19	36	limb. occ. ⊙ ad hor.		
5	20	45	cent. ♀ ad hor.		
	1	9	♀ à limb. bor. ⊙ - - - -	5	30
5	24	44	limb. occ. ⊙ ad hor.		
5	25	52	cent. ♀ ad hor.		
	1	8	♀ à limb. bor. ⊙ - - - -	5	35
5	53	30	limb. occ. ⊙ ad hor.		
5	54	30	cent. ♀ ad hor.		
	1	0	♀ à limb. bor. ⊙ - - - -	6	18
5	57	50	limb. occ. ⊙ ad hor.		
5	58	49	cent. ♀ ad hor.		
		59	♀ à limb. bor. ⊙ - - - -	6	36
6	2	56	limb. occ. ⊙ ad hor.		
6	3	53	cent. ♀ ad hor.		
		57	♀ à limb. bor. ⊙ - - - -	6	44
6	42	16	limb. occ. ⊙ ad hor.		
6	43	3	cent. ♀ ad hor.		
		47	♀ à limb. bor. ⊙ - - - -	7	25

Temp.

Temp. ver. mat.

h	′	″		′	″
6	51	42½	limbus occ. ⊙ ad horarium		
6	52	27	centr. ♀ ad hor.		
		44	♀ à limb. bor. ⊙ - - - - -	7	28
7	0	2	limb. occ. ⊙ ad hor.		
7	0	44	cent. ♀ ad hor.		
		42	♀ à limb. bor. ⊙ - - - - -	7	18
7	4	7½	limb. occ. ⊙ ad hor.		
7	4	48	cent. ♀ ad hor.		
		40½	♀ à limb. bor. ⊙ - - - - -	7	24
7	7	13	limb. occ. ⊙ ad hor.		
7	7	53	cent. ♀ ad hor.		
		40	♀ à limb. bor. ⊙ - - - - -	7	25
7	15	53	limb. occ. ⊙ ad hor.		
7	*18	31	cent. ♀ ad hor.		
		38	♀ à limb. bor. ⊙ - - - - -	7	33
7	52	36	limb. occ. ⊙ ad hor.		
7	53	4	cent. ♀ ad hor.		
		28	♀ à limb. bor. ⊙ - - - - -	8	10
7	58	5½	limb. occ. ⊙ ad hor.		
7	58	32	cent. ♀ ad hor.		
		26½	♀ à limb. bor. ⊙ - - - - -	8	14
8	3	2	limb. occ. ⊙ ad hor.		
8	3	28	cent. ♀ ad hor.		
		†25½	♀ à limb. bor. ⊙ - - - - -	8	24

* The ſame in both the manuſcripts. But there muſt be an error either in this number, or that next above. S. HORSLEY.

† The manuſcripts ſtill agree. The error probably lies upon the number next but one preceding, viz. the time of the appulſe of the ⊙'s weſt. limb to the hor. wire; which may have been 8 3 2½. S. HORSLEY.

Temp. ver. mat.

h	′	″		
8	6	4½	limbus occ. ☉ ad horarium	
8	6	29	cent. ♀ ad hor.	
		24½	♀ à limb. bor. ☉ - - - -	8 38
8	24	28½	limb. occ. ☉ ad hor.	
8	24	48	cent. ♀ ad hor.	
		‡29½	♀ à limb. bor. ☉ - - - -	8 50
8	26	2	limb. occ. ☉ ad hor.	
8	26	21	cent. ♀ ad hor.	
		19	♀ à limb. bor. ☉ - - - -	8 58
9	6	50	Videtur ♀ prolongari, ac velut radios emittere offuscantes limbum facie occidentali Solis.	
9	7	32	Contactus interior circularis.	
9	22	8	limb. ort. ♀ ad horarium.	
9	24	19	limb. ort. ☉ ad hor.	
	2	11	♀ à limb. bor. ☉ - - - -	9 54
9	26	32	Egref. tot. ♀ à ☉.	

Amice rogatus obfervationem hanc communem facio, quamvis non nimis confidam, ejus præfertim caufâ, qui horologio aderat, et quem in notando tempore minus fidelem aliquando deprehendi. Eclipfin ☉ hâc ipfâ die fpectandam nubes eripuere.

‡ The manufcripts agree: but the error is manifeft. Read 19½. S. HORSLEY.

Eclipfis

Eclipsis ⊙, die 25 Maii, anni 1770. Pekini, in residentiâ Sancti Josephi, tubo 8 pedum, observatâ.

Temp. ver. h	′	″		′	″	‴
			Diam. ⊙ capta plur. = $33\frac{35}{40}$ Rev. =	31	35	44
7	31	7	Cœperat Eclipsis.			
7	31	0	Init arbitratum.			
			Pars ⊙ illuminata.			
7	38	11	- - - - - - - - -	29	25	35
7	39	34	- - - - - - - - -	27	54	21
7	40	40	- - - - - - - - -	27	27	44
7	42	23	- - - - - - - - -	26	47	6
7	43	45	- - - - - - - - -	26	10	40
7	45	14	- - - - - - - - -	25	56	40
7	46	21	- - - - - - - - -	25	32	31
7	47	49	- - - - - - - - -	25	7	37
7	49	25	- - - - - - - - -	24	42	24
7	50	30	- - - - - - - - -	24	11	34
7	51	59	- - - - - - - - -	23	35	8
7	54	32	- - - - - - - - -	22	50	19
7	55	47	- - - - - - - - -	22	36	18
7	57	25	- - - - - - - - -	22	19	29
7	58	26	- - - - - - - - -	21	51	28
7	59	29	- - - - - - - - -	21	37	27
8	0	36	- - - - - - - - -	21	16	26
8	1	52	- - - - - - - - -	21	1	1
8	2	57	- - - - - - - - -	20	44	13
8	4	18	- - - - - - - - -	20	30	11
8	5	25	- - - - - - - - -	20	23	11
8	6	42	- - - - - - - - -	20	9	10

Temp. ver. h ′ ″	Pars ☉ illuminata. ′ ″ ‴
8 7 49	19 42 33
8 9 35	19 32 45
8 10 49	19 1 56
8 13 1	18 58 44
8 13 56	18 54 56
8 15 30	18 39 31
8 16 55	18 32 30
8 18 20	18 31 6
8 20 30	18 29 42
8 21 33	18 24 6
8 23 2	18 28 18
8 25 30	18 38 6
8 27 12	18 40 55
8 28 30	18 47 55
8 35 34	19 21 33
8 37 58	19 36 57
8 39 24	19 41 9
8 41 16	19 48 9
8 44 14	19 55 10
8 46 47	21 2 25
8 48 2	21 22 2
8 49 16	21 31 51
8 50 26	21 48 40
8 52 37	22 2 40
8 56 42	23 51 57
8 58 19	24 42 24
9 0 17	25 7 37
9 1 41	25 17 26
9 2 58	25 24 27
9 5 52	26 44 18

Temp.

Temp. ver.			Pars ☉ illuminata.			
h	′	″		′	″	‴
9	7	42	- - - - - - - - -	27	29	8
9	9	27	- - - - - - - - -	27	55	45
9	10	49	- - - - - - - - -	28	13	59
9	12	6	- - - - - - - - -	28	33	36
9	13	44	- - - - - - - - -	29	7	13
9	14	52	- - - - - - - - -	29	17	2
9	17	34	- - - - - - - - -	30	48	6
9	19	52	Finis Eclipſeos.			

Eclipſis ☾, die 23 Octob. anni 1771. Pekini, in reſidentiâ S. Joſephi, per me ægrotum obſervata.

h	′	″	
11	22	6	Init. Eclipſeos.
13	37	13	Finis.

Maximam Eclipſin non notavi.

♃ à ☾ tectus, die 5 Julii 1770. Pekini, in residentiâ Sancti Josephi, tubo 8 pedum, observatus.

Ingressum non observavi, tarde accedens ad observandum.

Temp. ver. h	′	″		′	″
7	57	29	egredi cœpit.		
7	59	59	egressus totalis.		
8	10	6	limb. occ. ☾ ad horarium.		
8	11	20	centr. Tychonis ad hor.		
	1	14	Tych. à limb. bor. ☾ - - -	24	59
8	18	20	limb. occ. ☾ ad hor.		
8	19	34	centr. Tych. ad hor.		
	1	14	Tych. à limb. aust. ☾ - - - -	4	51
			Igitur diam. ☾ - - - - - -	29	50
			Capta alias - - - - - - -	29	52
8	25	56	♃ cent. ad hor.		
8	27	26	cent. Tych. ad hor.		
	1	30	- - - - - - - - - - -	25	57
8	36	3	♃		
8	36	46	T		
	1	43	* - - - - - - - - - - -	24	18
8	45	20	♃		
8	47	9	T		
	1	49	- - - - - - - - - - -	25	5
8	53	27	♃		
8	55	29	T		
	2	2	- - - - - - - - - - -	25	13

* The manuscripts agree. It is uncertain, whether the error be upon the number next preceding, or the next but one. S. Horsley.

Temp.

Temp. ver. h	′	″			′	″
8	59	3	♃			
9	1	15	T			
	2	12		- - - - - - - - - -	25	22
9	12	24	♃			
9	14	40	T			
	2	16		- - - - - - - - - -	25	37
9	19	11	♃			
9	21	46	T			
	2	36		- - - - - - - - - -	25	48
9	28	29	♃			
9	31	22	T			
	2	53		- - - - - - - - - -	26	13
9	34	19	♃			
9	37	20	T			
	3	1		- - - - - - - - - -	26	22
9	14	56	♃			
9	45	9	T			
	3	13		- - - - - - - - - -	26	29
9	52	49	♃			
9	56	*7	T			
	3	28		- - - - - - - - - -	26	39
10	5	6	♃			
10	8	53	T			
	3	47		- - - - - - - - - -	26	43
10	14	37	♃			
10	18	37	T			
	4	0		- - - - - - - - - -	26	44

* It is so in both the manuscripts, but if the numbers next above and next below be right, this ought to be 17. S. Horsley.

Adde sequentes observationes eodem tubo per me factas.

In residentiâ Sancti Josephi.

	′	″
✳ Spic. Virg. a ☾ tecta, d. 25°. Jan. 1772.		
Ingr. $5^h\ 27'\ 41''$ t. v. ✳ a limb. bor. ☾	29	2
Egr. 6 34 07 ✳ a limb. bor. ☾	19	30
Capta ☾ diameter.	32	29

In residentiâ Sancti Josephi.

✳ Scorp. a ☾ tecta, d. 29°. Jan. 1772:		
Ingr. $4^h\ 16'\ 35''$ t. v. ✳ a limb. aust. ☾	13	0
Egr. 5 33 31 ✳ a limb. aust. ☾	20	9
Capta ☾ diameter.	31	53

In collegio Pekin.

♀ in parallelo ☉is d. 5°. Jan. 1772.						
			diam. ☉			
6^h	$35'$	$47''$	limb. ort. ☉ ad hor.		31	44
9	55	05	♀ ad hor. a parallel. limb. bor.		22	11
3	16	15	diff. asc.	diff. decl.	9	33

Eclipsis

Eclipſis Lunæ totalis, anno 1761, Novembris 12°, veſpere obſervata, Pekini Sinarum.

In Obſervatorio domeſtico Collegii Soc. Jeſu.

	P. Florian Bahr. Tubo 5 pedum			P. Ignatius Franciſco. Tubo 7 pedum.			P. Joſephus Bernardo Tubo 8 pedum.		
	h	′	″	h	′	″	h	′	″
Initium Eclipſeos	5	29	12	5	28	4	5	29	3
Immerſ. tot. Lunæ	6	28	59	6	24	33	6	27	18
Init. emerſ. Lunæ	8	2	37	8	7	2	8	4	23
Finis eclipſeos	9	4	32	9	4	42	9	3	52
Medium. ex init. et fine - -	7	16	52	7	16	23	7	16	27
Med. ex immer. et emerſ. -	7	15	48	7	15	47	7	15	50
Et horum med.	7	16	20	7	16	5	7	16	8
Ex his conſit med. eclipſeos				7	16	11			

Et obſervata ſunt hæc ad pendulum, quod quotidie ad ſolem et ſtellas exigitur, teleſcopio meridiano, quod, vocant " l'inſtrument des paſſages."

In Obſervatorio Regio, tubo 8 pedum cum duobus objectivis et reticulo.

	h	′	″		h	′	″
Initium eclipſeos	5	27	30	Initium Emerſ.	8	5	30
Digit. umbræ I.		32	30	Digit. lucis I.		11	0
II.		37	0	II.		16	0
III.		42	0	III.		21	30
IV.		47	0	IV.		26	30
V.		52	0	V.		31	0
VI.		57	0	VI.		35	30
VII.	6	2	0	VII.		41	0
VIII.		7	0	VIII.		46	0
IX.		11	0	IX.		50	30
X.		16	0	X.		55	0
XI.		22	0	XI.	9	0	0
Immerſ. Lunæ tot.		27	0	Finis eclipſ.		5	0

Ex his conficitur medium eclipſ. 7 16 20

Atqui conſtat obſervatorium regium orientalius eſſe collegio 14″ pend.

Obſervata ſunt hæc ad horologium manuale bonæ notæ, quod ante cum eodem pendulo compoſitum, et poſt cum illo collatum ſibi probe conſtitit, uti alias ſolet.

Tempora

Tempora ſunt vera omnia. Numeri ab obſervatione intacti. Cœlum toto tempore ſerenum fuit, et quietum a vento; umbra tamen terræ admodum non terminata, præſertim initio: Luna in umbrâ ſemper clarè viſa tota, margine bene terminato; quin tamen maculæ diſtinguerentur. Color Lunæ inſtar ferri candentis; poſt immerſionem obſcurior, et ante emerſionem obſcurior, quà axem umbræ, in medio umbræ obſcurior, quà centrum terræ ſpectabat. Paullo ante initium emerſiónis viſa ſtellula ſic ſatis lucida, verticaliter Lunæ imminens, nempe in tubo ſitu inverſo, diſtans a limbò Lunæ quaſi $\frac{2}{3}$ diametri; et ſpes erat videndi hujus occultationem, antequam eclipſis finiret: ſed vix digitus unus Lunæ emerſit, cum lumine Lunæ extincta videri deſiit. Porro,

		h	′	″	
Medium Eclipſeos dabant tab.	D. Halleii	7	20	56	Ad merid. Collegii hujus.
	D. Caſſini	7	23	20	
	P. Gramatis	7	22	26	
Inſtitit. Aſtron.	D. Monnier	7	15	39	
Ephemerides Pariſienſes, neſcio ex quibus tabulis		7	33	33	

Folium, in quo præſens obſervatio erat notata, perditum fuerat, et, die 19 Octobris anni 1772, caſu inventum; adeoque hæc obſervatio eclipſeos Lunæ nunquam in Europam fuit miſſa.

III. *The Lunar Eclipse*, Oct. 11, 1772, *observed at* Canton. *Communicated by* John Blake, *Esq. of* Parliament-Street.

Redde, Nov. 4, 1772.

To JOHN BLAKE, *Esq. of* Parliament-Street, Westminster.

MY DEAR FATHER,

WITHIN an hour after I closed my letter to you, by this ship, enclosing Astronomical Observations from Pekin, I received the enclosed observation, of the late Eclipse, made by a Jesuit here. An accident prevented their sealing the packet for a few hours; and Mr. Hume is now waiting only for this letter of mine to finally dispatch.

Canton, Dec. 12, 1772.
per ship Osterley.

J. B. BLAKE.

I know nothing of the accuracy of the observation, as he is lately arrived here *.

* And the time being taken only by a watch regulated by the sun the day before, the observation is not much to be depended on. NEVIL MASKELYNE.

Obſervatio Lunaris Eclipſeos habita Cantone, die 11 Octobris, 1772.

Obſervationem inſtitui teleſcopio 6 palmarum: tempus adnotavi oſtenſum ab horologio parvo ſecundorum, ſpirâ, a mag°. Arnold Londinenſi elaborato, quod in meridie vero antecedente correxi; illius tamen fidelitatem examinare non potui in meridie ſequenti. Cœlum erat omni ex parte limpidiſſimum, et intentus expectabam penumbræ denſioris adventum, quæ tamen valdè tenuis apparuit, ideoque initium non notavi.

	h	′	″
Jam cœperat Eclipſis ——	10	54	26
Forſan 32″ citius fuiſſet.			
Initium verum ——	10	53	54
Immerſio ——	12	3	29
Medium ——	12	58	59
Emerſio ——	13	54	29
Finis ——	15	4	4

IV. *Experiments on Dying Black, by Mr.* James Clegg, *of* Redivales, *near* Bury. *In a Letter to Dr.* Percival.

DEAR SIR,

Redde, Nov. 11, 1773.

LIME having been proved to increase the solvent power of water, upon astringent vegetables, for medical purposes, I was desirous of knowing if it would be equally useful in the art of dying black; to this end I made the following experiments.

EXPERIMENT I.

Four penny-weights of each of the following astringents; videlicet, Galls, Sumach, Oak Bark, Bistort Root, and Logwood, were boiled during ten minutes, in half a pint of pure river water; upon mixing the decoctions with a saturated solution of martial vitriol, in the proportion of $\frac{1}{3}$ of the solution to $\frac{2}{3}$ of the decocton, they struck colours differently inclining to blackness, in the following order; videlicet, Oak Bark, Bistort Root, Sumach, Galls. I then boiled the same weight of all the astringents, in the same quantity of lime water; and upon mixing them as as above, the colours they produced were inferior to thoſe with plain water, the aſtringency of the logwood, or whatever gives it the property of ſtriking black with green vitriol, was entirely deſtroyed; it produced not the leaſt blackneſs with any quantity of vitriol.

EXPERIMENT II.

Four penny-weights, of each of the aſtringents above-mentioned, were tritured in plain water, and four others in lime-water; the meaſures of water uſed were equal to thoſe left, after boiling, in the laſt experiment; and, upon being mixed with martial vitriol as in the laſt experiment, the colours produced, by this means, were ſuperior to thoſe produced by boiling. Thoſe tritured in lime-water were judged to be the deepeſt, which agrees with Mr. HENRY's experiments; but we muſt again except the Logwood, which gave no colour by trituration, more than by boiling in lime-water.

EXPERIMENT III.

All the above mixtures, having been written with as inks, and expoſed ſix months to the air; thoſe boiled in lime-water had failed much; thoſe tritured in lime-water, and in plain water, had faded a little; thoſe boiled in plain water evidently preſerved their colour beſt.

Upon ſlightly rubbing the faded writings, with a freſh aſtringent liquor, they recovered their ori-

 ginal

ginal blacknefs; by which it appears, that it was the aftringent parts of thofe Inks which had failed.

Does it not appear, by thefe experiments, that, though lime-water tends to deepen the colour produced by fome aftringents and martial vitriol, it by no means adds to the duration of thofe colours; and as lime-water, either by trituration or coction, entirely deftroys the property, in logwood, of ftriking black with martial vitriol, it can by no means be of fervice, in the black dye, where logwood is a material ingredient.

Does it not alfo appear, that a flight boiling is preferable to trituration, for the purpofes of dying, when a durable colour is wanted?

Having obferved a folution of iron, in a *vegetable* acid, ftruck a deeper black, upon mixture with an aftringent, and produced its effect much more expeditioufly, than a ftrong folution of martial vitriol; it occurred to me, that the iron, being more flightly combined with the vegetable acid, than with the vitriolic, made it more eafy, for the aftringent matter to decompound the former, and produce an ink; if this was the cafe, I fufpected, that lime-water deepened the colour of aftringent and chalybeate mixtures, not fo much by its action upon the aftringent, as upon the chalybeate, the lime uniting with the fuperabundant acid, and leaving the iron, with fo much of the acid, as is neceffary for the formation of an ink, to be more eafily attached by the aftringent matter of the vegetable.

But if this theory was well founded, it followed, from analogy, that any ſubſtance, which had a greater affinity with the vitriolic acid than iron had, would produce the ſame effect, in ſome degree, as lime. To determine this:

EXPERIMENT IV.

I took two veſſels, containing equal meaſures of a ſtrong aſtringent liquor, compoſed of galls and logwood; into one veſſel I put a ſmall quantity of pearl aſhes; the other remained as a ſtandard. Pieces of linen and cotton cloth, after maceration in theſe liquors, were thrown together into a ſtrong ſolution of copperas; they were ſoon after taken out, and waſhed in cold water; when dry, the pieces prepared in aſhes were, all of them, much deeper than the others.

I made uſe of different kinds of pearl and pot-aſhes, as well as of many kinds of aſtringents; the aſhes had the ſame effect, whatever aſtringent was made uſe of, and the ſtrongeſt alkali always produced the deepeſt colour; and though aſhes, uſed with an aſtringent, always gave a deeper black, than the ſame aſtringent without aſhes, yet logwood, which without aſhes gave not ſo deep a colour as galls with them, gave a much deeper black than galls with the ſame addition.

There was a remarkable difference, in this caſe, betwixt lime and aſhes, in their effect upon logwood; with lime it gave no blackneſs, but with aſhes it produced a deeper black, than any other aſtringent I made uſe of.

Being desirous of trying the duration of colours, produced by astringents, in which different quantities of pearl ashes had been dissolved;

EXPERIMENT V.

In two pints of river water, I boiled one ounce of logwood, during ten minutes; I then added half an ounce of Aleppo galls, and boiled them together ten minutes longer; the liquor having stood to cool, was decanted off, and divided into six equal quantities. N°. 1 remained as a standard, into N°. 2 I put six grains of fine pearl ashes, N°. 3 twelve grains, N°. 4 eighteen grains, N°. 5 twenty-four grains, N°. 6 thirty grains; to six drops of each of these liquors, I added two drops of a saturated solution of copperas; N°. 2 and 3 struck a deep black, N°. 1 and 4 black, but inferior to 2 and 3, N°. 5 a brown black, N°. 6 brown.

From this experiment it appears, that N°. 5 and 6 were spoiled by an over proportion of ashes. Before I had the pleasure of seeing your experiments, wherein you demonstrate, that a quantity of acid enters into the composition of ink, I imagined the alkali decompounded the copperas too suddenly, and disengaged the iron faster, than the astringent matter could unite with it.

But, most probably, the alkali neutralized too great a portion of the acid.

All these writings having been now exposed six months to the air, in N°. 5 and 6 the blackness is quite

quite deſtroyed, N°. 4 is ſomething faded, N°. 1, 2, 3, remain nearly as they were, N°. 2 and 3 being ſtill ſuperior to the ſtandard.

I am,

Your moſt obliged,

humble ſervant,

Redivales, May 18,
1773.

JAMES CLEGG.

V. *Observations on the State of Population in* Manchester, *and other adjacent Places. By Dr.* Percival. *Communicated by the Rev. Dr.* Price.

Redde, Nov. 25, 1773. FROM an account taken in 1717, the number of inhabitants in MANCHESTER, for I am uncertain whether SALFORD was included [a], appears to have been 8000.

By a survey, made in 1757, of Manchester and Salford, the number of inhabitants was found to be 19839. And from 1754 to 1761 inclusive, the number of deaths amounted to 5769. The annual deaths therefore, at the period of the survey, must have been 721, exclusive of dissenters. It is probable, as will appear afterwards, that these would have increased the number to 771. At this time therefore, 1 in 25.7, of the inhabitants of Manchester, died every year.

[a] Manchester and Salford, though distinguished by different names, like London, Westminster, and the Borough of Southwark, may be considered as one and the same town, being divided only by a small river, over which two bridges are erected.

A new

A new ſurvey of Mancheſter has been executed this ſummer, 1773, with great care and accuracy, of which the following is a particular account.

Mancheſter.				Salford.
3402	-	Houſes	-	866
5317	-	Families	-	1099
10548	-	Males	-	2248
11933	-	Females	-	2517
7724	-	Married	-	1775
432	-	Widowers	-	89
1064	-	Widows	-	149
7782	-	Under 15	-	1793
3252	-	Above 50	-	640
342	-	Male Lodgers	-	18
150	-	Female Do.	-	13
44	-	Empty Houſes	-	26

From hence it appears, that the number of tenanted houſes, in Mancheſter and Salford, amounts to 4268; the families to 6416; and the inhabitants to 27,246. The proportion of perſons to a houſe, therefore, is very nearly $6\frac{2}{5}$; and, of individuals to a family, about $4\frac{1}{4}$. The females exceed the males, by 1654; the widows are more than double the number of widowers; and about a ſeventh part of the inhabitants have attained the age of fifty.

The

The following table is formed from the regifter of burials and baptifms at the collegiate or parifh church in Manchefter, and gives the annual number of each, on an average.

			Burials.	Baptifms.
From 1580	to 1587	inclufive,	184	
1680	1687		286	
1720	1727		359	
1754	1760		736	769
1761	1765		731	843
1766	1770		870	970

But it fhould be remarked, that this account does not include the deaths, or births, amongft the Diffenters. Thefe, by a late improvement in our bills of mortality, are now admitted into the parifh regifter, and laft year, 1772, the former amounted to 50, the latter to 181. Admitting thefe to be the average of unregiftered baptifms and burials in Manchefter, the annual medium of deaths, from 1768 to 1772 inclufive, will be 958. And the annual births, during the fame period, with the like allowance, will be 1098. Hence the prefent proportion, of annual deaths to the inhabitants, is nearly as 1 to 28 .4; and, of births to the inhabitants, almoft as 1 to 25. The births alfo, it appears, exceed the burials 140 every year, at a medium.

The rapid growth of Manchefter is fufficiently evident from the preceding facts. Yet LEVERPOOL, during the fame fpace of time, has increafed in a much greater proportion. This appears from the following Table, which I have extracted from a very curious and entertaining work, lately published by my ingenious friend Mr. ENFIELD, Lecturer on the Belles Lettres in the Academy at Warrington.

[b] Year.	Number of Inhabitants.	Annual Addition.
1700	5714	
1710	8168	245
1720	10446	227
1730	12074	162
1740	18086	601
1750	22099	401
1760	25787	368
1770	34004	822

According to this Table, Leverpool has, at present, upwards of six times the number of inhabitants, which it contained, at the beginning of this century.

But the progress of trade and opulence, in Manchester, has been more than adequate to its advancement in population. For a considerable part, of the manufactury of this flourishing town, is carried on in the adjacent country, which is thereby crouded with houses and inhabitants. So populous are the environs of Manchester, that every house in the township has been found, by a late survey, to contain, at an average, six persons. The township is indeed but of small extent; and the greatest

[b] History of Leverpool, p. 28, second edition, corrected.

part of it will probably, in a ſhort time, be included in Mancheſter. It contains 311 houſes; 361 families; 947 males; 958 females; 656 married perſons; 21 widowers; 42 widows; 763 under 15 years of age; and 222 above 50.

It is pleaſing to obſerve, that, notwithſtanding the enlargement of Mancheſter, there has been a ſenſible improvement, in the healthineſs and longevity of its inhabitants; for the proportion of deaths is now conſiderably leſs, than in 1757. But this is chiefly to be aſcribed, as Dr. PRICE has juſtly obſerved [c], to the large acceſſion of new ſettlers from the country. For as theſe uſually come in the prime of life, they muſt raiſe the proportion of inhabitants to the deaths, and alſo of births and weddings to the burials, higher than they would otherwiſe be. However, excluſive of this conſideration, there is good reaſon to believe, that Mancheſter is more healthy now than formerly. The new ſtreets are wide and ſpacious, the poor have larger and more commodious dwellings, and the increaſe of trade affords them better cloathing and diet, than they before enjoyed. I may add too, that the late improvements in medicine have been highly favourable to the preſervation of life. The cool regimen in fevers, and in the ſmall pox, the free admiſſion of air, attention to cleanlineſs, and the general uſe of antiſeptic remedies and diet, have certainly mitigated the violence, and leſſened the mortality of ſome of the moſt dangerous and malignant diſtempers, to which mankind are inci-

[c] See a moſt valuable Treatiſe on Reverſionary Payments, p. 188, third edition.

dent.

dent. The ulcerous ſore throat, which prevailed here in the year 1770, is the only epidemic, which has appeared in Mancheſter, with any fatal degree of violence, for many years. Miliary fevers, which were formerly frequent in this town and neighbourhood, now rarely occur; and, if I may judge from my own experience, the natural ſmall pox, for inoculation is not much practiſed here, carries off a much ſmaller proportion, of thoſe who are attacked by it, than is commonly ſuppoſed. Puerperal diſeaſes alſo decreaſe every year amongſt us, by the judicious method of treating women in child-bed: and, as nature is now more conſulted in the management of infants, it is reaſonable to ſuppoſe, that this muſt be favourable to their health and preſervation.

But it muſt be acknowledged, that large towns are injurious to population; and the advantages I have enumerated, which, in hamlets or country villages, would have operated, with full force, to the benefit of mankind, have only ſerved to check the deſtructive tendency of the accumulation of inhabitants in Mancheſter. In the Pais de Vaud, a diſtrict of the province of Bern in Switzerland, and in a country pariſh in Brandenburgh, 1 in 45 of the inhabitants dies annually; and at Stoke Damarell in Devonſhire, 1 in 54 [*d*]; whereas, in this town, the yearly mortality appears to be 1 in 28; in Leverpool 1 in 27; and in London 1 in 21. Half the children, who are born in Mancheſter, die under five years old; and the proportion,

[*d*] See the Treatiſe, before referred to, on Reverſionary Payments, by my learned friend Dr. Price.

which the births bear to the number of inhabitants who attain the age of 80, is as 30 to 1. Difeafes are moft frequent and fatal here, in the months of January, February, and March: and leaft fo, in July, Auguft, and September. The mortality, of thefe two feafons, is as 11 to 8; and, of the firft fix months of the year compared with the laft fix months, as 7 to 6.

In April, 1773, feveral Gentlemen, from motives of curiofity, undertook an enumeration of the people of BOLTON, a manufacturing town about twelve miles diftant from Manchefter. The houfes were found to be 946; the males 2159; the females 2392; and perfons aged feventy years and upwards 74. To thefe numbers 17 muft be added, which, by a miftake, were not claffed under either denomination. The inhabitants of Bolton therefore amount to 4568; the number of individuals to a houfe is 4 .8; and about a fixtieth part of the people have attained the age of feventy.

LITTLE BOLTON, a fuburb of Bolton, including the manor, and extending into the country as far as the inhabitants are fubject to fuit and fervice, contains 232 houfes; 771 individuals; 361 males; 410 females; and 15 perfons aged feventy years and upwards. From this account it appears that the inhabitants are 3 .3 to a houfe; and that 1 in 51 has reached the age of feventy. The difference, in thefe proportions, between a fmall town, and a country manor contiguous to it, is worthy of obfervation.

MR. FLETCHER has favoured me with an enumeration of the people of BURY, which he has juft executed

executed with great care. The town contains 463 houſes; 464 families; and 2090 inhabitants. Each houſe and family therefore conſiſts of $4\frac{1}{2}$ individuals. Bury is ſituated nine miles from Mancheſter, and is enriched by a branch of the woollen manufactury.

At ALTRINGHAM, a market town in Cheſhire, which has no manufactury, the number of houſes, according to an exact ſurvey, made in July 1772, was 248; of inhabitants 1029; or $4\frac{1}{7}$ to a houſe. An enumeration, of the people of this town, was made about twenty years ago, at which time they amounted very nearly to 1000.

The following is a comparative view of the ſtate of population, the duration of life, and the mortality of the ſeveral ſeaſons of the year, &c. in EASTHAM and ROYTON, two country places widely different from each other, in climate, ſituation, and in the occupation of their inhabitants.

The pariſh of EASTHAM lies in Wirral, one of the hundreds into which Cheſhire is divided, and is extended along the banks of the river Merſey, a few miles diſtant from the Iriſh ſea. The people are moſt of them farmers; though ſome are fiſhermen, and others are employed in the ferry to Leverpool.

ROYTON is a chapelry, ſituated ten miles eaſtward of Mancheſter, under the great chain of mountains, which divides Lancaſhire and Yorkſhire. The inhabitants are employed chiefly in the cotton and linen manufactury; a few of them are farmers; and ſome, I believe, work in the coal pits, with which this country abounds.

I am

I am indebted to my learned friend the Rev. Mr. TRAVIS, Vicar of Eaftham, for the furvey of his own parifh; which he undertook at my defire, and executed himfelf; and alfo for that of Royton, which was made by his uncle, the worthy and refpectable clergyman of that chapelry.

January 1ft, 1772, the number of inhabitants, in the chapelry of Royton, were found to be 1105.
The number of inhabitants in the parifh of Eaftham, 912.

The number of perfons in a *houfe*, in the chapelry of Royton, is fomewhat more than — — $5\frac{1}{7}$
The number of ditto, in the parifh of Eaftham — — exactly — 5
The number of perfons in a *family*, in the former on an average — about — $4\frac{3}{4}$
The number of ditto, in the latter — — — more than — $4\frac{1}{2}$
The proportion of males to females, in Royton — — nearly as — 53 to 56
The proportion of ditto, in Eaftham — — — nearly as — $54\frac{1}{2}$ to 56
The widows to the widowers, in Royton — — — as 10 to 3
The widows ditto, in Eaftham — — — — as 10 to $8\frac{1}{3}$
The number of births in Royton (on an average of 3 years) 42 } Proportion between males and females as 13 to 11
The number of do. in Eaftham — Do. 34 } Proportion between ditto, — as 13 to $11\frac{5}{9}$
N. B. Thefe proportions for 7 years.

The number of births in Royton to the number of married inhabitants, as (very nearly) 1 child to 5 married couples.
The number of do. in Eaftham to ditto, — as (fomewhat more than) 1 child to 4 married couples.
The number of births in Royton to the whole number of inhabitants, — as 1 to $26\frac{1}{3}$
The number of do. in Eaftham to ditto, — — — as 1 to $26\frac{4}{5}$
The number of married perfons in Royton to the number of unmarried perfons above 15, as 8 to 5
The number of ditto, in Eaftham to the number of ditto, — ditto, nearly as 6 to 5
The number of burials in Royton (on an average of 3 years) 21 } Proportion between males and females as 13 to 10
The number of ditto in Eaftham ditto — 26 } Proportion between ditto as 13 to $11\frac{1}{7}$
The number of burials in Royton to the number of all the inhabitants, — as 1 to 52
The number of ditto in Eaftham to — ditto — — as 1 to 35
The number of children dying under 3 yrs old to the number of childr. born in Royton (on an average of 3 yrs) as 1 to 7
The number of children ditto, to ditto in Eaftham — — as 1 to 17

Perfons

Perfons alive in Royton under 3 years old Jan. 1. 1772, 129; dead under 3 years old, average of 3 years, 6; or 1 of $21\frac{1}{2}$
Ditto in Eaftham — — — 100; — — dead 2; or 1 of 50
Perfons alive in Royton under 15 years old Jan. 1. 1772, 450; dead under 15 years old, average of 3 years, 11; or 1 of 41
Ditto in Eaftham — — 329; — — dead 4; or 1 of 82
Perfons alive in Royton between 15 and 30 years old Jan. 1. 1772, 333; dead before 1773 of thefe 5; or 1 of $66\frac{1}{2}$
Ditto in Eaftham — — 199; — dead 5; or 1 of 40
Perfons alive in Royton from 30 to 40 years old ditto, 96; dead before 1773 of thefe on an average, 2; or 1 of 48
Ditto in Eaftham — — 124; — — dead 4; or 1 of 31
Perfons alive in Royton from 40 to 50 years old ditto, 98; dead before 1773 of thefe on an average, 2; or 1 of 49
Ditto in Eaftham — — 83; dead before 1773 of thefe — 3; or 1 of 28
Perfons alive in Royton from 50 to 60 years old ditto, 61; dead before 1773 of thefe — $1\frac{1}{3}$; or 1 of 45
Ditto in Eaftham — — 64; dead before 1773 of thefe — 2; or 1 of 32
Perfons alive in Royton from 60 to 70 years old ditto, 49; dead before 1773 of thefe — $1\frac{1}{3}$; or 1 of 36
Ditto in Eaftham — — 54; dead before 1733 of thefe — $1\frac{1}{3}$; or 1 of 40
Perfons alive in Royton from 70 to 80, Jan. 1. 1772, 10; 80 to 90, ditto, 8 } above 70 years, 18 { dead before 1773, on an average of 3 years, } 1 of 18.
Perfons alive in Eaftham from 70 to 80, ditto, 34; 80 to 90, ditto, 5 } above 70 years, 39 { dead before 1773, on an average of 3 years, } 1 of 21 [e]

The mortality of the feafons at Royton and Eaftham, for the laft feven years, has been as follows:

			Royton.	Eaftham.
January,	February,	March,	39	56
April,	May,	June,	31	34
July,	Auguft,	September,	31	45
October,	November,	December,	18	53
			119	188

[e] The averages, here adopted, may, in fome inftances, feem to be too fmall; but Mr. Travis affures me, that, through a feries of fifteen fucceffive years, the Marriages, Births, and Deaths, at Eaftham, do not vary, in any degree worth remarking, from the foregoing Table,

Of

Of all the months in the year, singly taken, October is the most, and April the least fatal to the inhabitants of Eastham. Whereas the three last months of the year appear to be the most healthful at Royton; although a very large quantity of rain usually falls there, during this season. For the wind, at this time, being generally westerly, the clouds are intercepted by the high mountains, and discharge themselves in frequent and heavy showers. At TOWNLEY, which is situated under the same chain of hills, and is not very far distant from Royton, 42 inches of rain fall, at a medium, every year. The quantity of rain, at Manchester, which is farther removed from the mountains, is about 33 inches *communibus annis*. It has been observed, by a very useful writer, that the moist seasons, in Great Britain, and Ireland are more remarkably free, from epidemic diseases, than the dry ones; and that storms, the usual concomitants of rain, are attended with more health and less sickness, than calm weather; probably because they dissipate the vapours, which, by stagnation, might prove an occasion of various distempers [*f*]. I shall not presume to determine, that these observations account for the superior healthiness of the last months of the year at Royton; but they certainly should remove the prejudice, which is too generally entertained, against the wetness of the climate in Lancashire, and other western counties of England. For the bounties of Providence are dispensed, with an equal as well as with

[*f*] Rutty's Chronological History of Weather.

a liberal hand. And if we, in this part of the iſland, enjoy leſs ſunſhine, than our neighbours, we have milder winters, and ſummers tempered with more refreſhing ſhowers, to balance the inconvenience.

The Rev. Mr. BOLTON, a very worthy diſſenting clergyman at MONTON, a few miles from Mancheſter, has, at my requeſt, made an enumeration of his people, with a retroſpective view of the births and deaths amongſt them during the laſt ten years. By this ſurvey, his congregation, including ſervants, conſiſts of 196 males; 190 females; 97 families; 60 married perſons; 14 widowers; 13 widows; 142 under 15 years of age; and 64 above 50. The deaths, during ten years, have been 57, and the births 138. Hence it appears, that, of this ſociety, 1 in 6 has attained the age of 50; that the births are more than double the burials; and that only 1 in 68, at a medium, dies every year. The laſt circumſtance is ſomewhat extraordinary; but to remove all doubts, concerning the accuracy of his enumeration, Mr. BOLTON, with the moſt obliging aſſiduity, has repeated it twice. And he has derived his information, not only from the regiſter of his chapel, but alſo from the private records, or deliberate recollection, of every family in his congregation. The ſituation of Monton appears to rather unfavourable to health, from the vicinity of a large moſs; but the people are moſt of them farmers, and are remarkable for their diligence and ſobriety. The long life, which they enjoy, affords a ſtriking and pleaſing proof of the great

 advan-

advantages of temperance; and confirms a curious obſervation of M. MURET, who examined the regiſter of mortality in one town, to mark thoſe, whoſe deaths might be imputed to exceſs. The number of theſe he found ſo great, as led him to believe, that drunkenneſs is more deſtructive to mankind, than pleuriſies, fevers, or the moſt malignant diſtempers.

King's Street,
Aug. 16th, 1773.

VI. Ob-

VI. *Obſervations on the Bill of Mortality, in* Cheſter, *for the Year* 1772. *By Doctor* Haygarth.

Redde, Nov. 18, 1772.

A Faithful and minute regiſter of mortality, and of the various diſeaſes moſt fatal to mankind, at different ages, muſt evidently be of the moſt important conſequence, to the politician, the philoſopher, and the phyſician, in their ſeveral endeavours to relieve the miſeries, and promote the happineſs of human nature.

A writer, of diſtinguiſhed abilities in political arithmetic, has offered many arguments, which give too much cauſe to apprehend, that England, in about 70 years, has loſt near a *quarter* of her people. Accurate regiſters of mortality, with other collateral inquiries, can, with moſt certainty, confirm or confute this opinion, and determine a queſtion, of the moſt ſtriking importance to our very exiſtence as a nation.

The doctrine of annuities for widows, and other perſons in old age, the value of reverſionary payments, and of aſſurances on lives, and other important queſtions in civil ſociety, can only be determined by faithful regiſters, ſhewing the duration of human life, in various ſituations of town and country. The ſlighteſt ſurvey, of the following Tables, will manifeſty ſhew, how erroneous and unjuſt every calculation, relating to this ſubject, muſt be, drawn from the London bills, or perhaps thoſe of moſt other conſiderable towns, and applied to the inhabitants of this city.

Cheſter is healthy to an uncommon degree, when compared with towns of the ſame ſize. Various circumſtances, which contribute to render this place ſo remarkably ſalubrious, might be pointed out; but it can here be only obſerved, in general, that this ſalutary effect may, with great probability, be chiefly attributed to the dry ſituation, clear air, pure water, and general temperance of the people.

In Auguſt 1772, the inhabitants of St. Michael's, one of the nine pariſhes into which Cheſter is divided, and ſituated in the very center of the city, were numbered with great accuracy: in this pariſh were 151 families, 127 houſes, 618 inhabitants, 246 males, 372 females, 166 married, 41 widows, 21 widowers, and 137 children under 15 years old. Hence the number of perſons, never married above 15, is 253. From this account alſo it appears, that near $4\frac{5}{6}$ perſons dwell in each houſe; that the proportion, of females to males, is as 62 to 41, or nearly as 3 to 2; that the widows are to the widowers nearly as 2 to 1; that the number married is little more than one quarter of the inhabitants: the common proportion of married people is about one third of the

the whole. The number of chriſtenings, at St. Michael's, for the laſt ten years, are 147, or 14,7 yearly; the burials, during the ſame period, are 127, or 12,7 yearly. Hence the proportion of annual births to inhabitants is nearly as 1 to 42; and burials nearly as 1 to $48\frac{2}{5}$. During 1772, only nine perſons died in this pariſh; hence the proportion of deaths to the living, this year, is leſs than 1 in 68. Theſe facts muſt appear moſt aſtoniſhing to any one, who reflects, that in the largeſt towns, ſuch as London, 1 in $20\frac{3}{4}$ dies annually; and, that in towns of a moderate ſize, as Leeds, 1 in $21\frac{3}{5}$; that in Northampton and Shrewſbury, either of them leſs than Cheſter, 1 in $26\frac{2}{5}$ dies yearly. Theſe facts, relating to this pariſh, are true, beyond a poſſibility of doubt; and yet they are ſo very extraordinary, that one cannot, without farther enquiries, apply to the whole town, by analogy, the obſervations which were made upon only a ſmall proportion of the inhabitants. However no peculiarity of air, water, or any other obvious circumſtance, can be ſuppoſed, to render this pariſh more healthy than the reſt of the town. How far theſe facts have been accidental, the following, and other collateral enquiries, will diſcover.

For the laſt eight years, preceding 1772, there have been 385 births, and 375 deaths annually in Cheſter. The number of deaths this year, excluding thoſe who were killed by the dreadful exploſion of gunpowder, is 379; ſo that, probably, the concluſions drawn from the following Tables,

Tables, which were executed with great care and fidelity, will not be liable to any conſiderable errors; and ſuch errors, by continuing this account for a period of years, will moſt effectually be corrected.

The following obſervations are offered as a ſmall ſpecimen of the concluſions, that may then, with more certainty, be deduced from ſuch a regiſter of mortality.

From the following bills, which diſtinguiſh the ages at which the inhabitants die, it appears, as far as one year's obſervation may be truſted, that, taking the whole town, 1 in 31,1 dies annually. This proportion, of deaths to the living, is probably too high, becauſe the births, upon an average, exceed the burials; a fact, which affords another proof, that the place is uncommonly healthy. Other facts amply confirm this obſervation.

Half the inhabitants, born in London, die under 2 years and three-quarters old; in Vienna, under 2; in Mancheſter, under 5; in Norwich, under 5; in Northampton, under 10; in Cheſter, this year, above half who died were 20 years old.

Of all the children born in this city, 1 in $5\frac{1}{3}$ lives to above 70, and 1 in $15\frac{3}{4}$ attains 80 years of age; whereas in Northampton, only 1 in $21\frac{2}{3}$; in Norwich, 1 in 27; and in London, 1 in 40 lives till 80.

In the Hotel-Dieu, a large hoſpital in Paris, above 1 in 5 dies, of all that are admitted; in St. Thomas's and St. Bartholomew's, in London, 1 in

13; in the Chester infirmary, since its first institution in 1755, 'till 1772 inclusive, only 1 in $25\frac{1}{6}$.

But Table IV, will, at one view, shew, in the most satisfactory manner, the comparative state of health, between this and some other towns of different magnitudes. It is curious to compare, by this table, in the early part of life, the probability that the inhabitants in Chester have, to live longer than in Northampton, Norwich, and especially much longer than in London. But when they have arrived to 70 years old, the chance of living, at all the places, is nearly equal.

It is a matter of curiosity, to observe how much longer women live than men. This fact is well established, by former observations on this subject, and is confirmed by the following register (Table I.). During the last year 12 widowers have died, and 53 widows; that is above four times the number. Between 80 and 90 years old, 2 men and 18 women have died; that is nine times as many. Above 90 years old, 4 have died, and all women.

To know at what period of life each disease is most fatal to mankind, is manifestly a sort of intelligence the most important, both to the patient and the physician; and though an enquiry, of this nature, may be attended with considerable difficulties, yet the advantage of such information is so obvious to all, it seems wonderful, that no attempt has been made, to execute a plan so generally beneficial to mankind.

The Table of diseases (N°. II.) has been compiled with all possible accuracy. At the beginning of of the year was printed a catalogue, with a clear and concise description of the diseases most fatal in this place. The faculty, in general, have sent written certificates, or given verbal information, to the clerks of each parish, of what diseases the patients died. The most considerable errors, in this table, will be among the diseases of children, who are not able to describe their complaints; and concerning whose disorders medical advice is more rarely desired. The article of *Convulsions* is particularly to be suspected; for when an infant is seized with a fever, or almost any other disorder, if there be any distortion of features, expressing pain, or any irregular motion of the eyes, nurses always denominate the disease, inward convulsions.

Consumption is another term, which the English are inclined to apply to many disorders, besides the disease of the lungs, to which medical people give that peculiar name. In this Table, particular care has been taken, to distinguish the *weakness* of Infancy, and *decay* of old age, which are generally confounded with pulmonary consumptions. And to prevent every error of this kind, with all possible accuracy, strict injunctions were given, that no disorder, unless attended with a *cough*, should be called consumption.

Notwithstanding this care to separate, as far as prejudice would allow, every other disease from consumptions, it is matter of astonishment to observe, that, between the ages of 10 and 50, more people die of consumptions, than of all other diseases: this is the period of life, when every faculty,

and

and every enjoyment, both of body and mind, are in their moſt vigorous perfection. It becomes therefore an enquiry, of the moſt ſtriking conſequence to ſociety, to diſcover the cauſe, in order to prevent the fatality, of a diſeaſe, which makes ſuch dreadful havock among mankind. The *Scrophula* has been ſuſpected as the moſt general cauſe of conſumptions; but this diſeaſe is leſs common in Cheſter than in moſt other places. Would the limits of this paper admit of ſuch a diſcuſſion, very ſtrong evidence could be produced, that the moſt frequent and moſt fatal cauſe of conſumptions, in this town and neighbourhood, is catching cold, which occaſions a cough, that is entirely neglected for many weeks or months, and is never ſuppoſed to be dangerous, 'till it becomes deſperate.

There are no marſhes ſo near this city, as to infect the air: hence we have no dyſenteries, and very few intermitting or remitting fevers, unleſs a few faint irregular paroxyſms may be ſuppoſed to require that denomination. This fact may be brought in evidence, to ſupport the obſervation, that marſhy effluvia are the cauſe of thoſe diſeaſes; an opinion lately controverted by a very ingenious writer.

For more than 30 years paſt, the miliary fever has been ſuppoſed endemic in this city and neighbourhood; but very probable evidence has been produced, by the moſt judicious phyſicians, that this eruption is very rarely, or never, critical and ſalutary, but that it is generally, or always, fabricated by cloſe warm rooms, too many bed-cloaths, hot medicines and diet. If this be univerſally

 true,

true, it is much to be wiſhed, that the miliary fever may be entirely baniſhed from the catalogue of diſeaſes. It muſt give ſingular pleaſure, to every benevolent heart, and well-wiſher to this place, to find only one fatal miliary fever in the regiſter of this year. May not this circumſtance, with great probability, be attributed to the method, lately adopted, of treating fevers, by admitting freſh air into ſick chambers, and adminiſtering ſuch regimen and remedies, as are cooling, and check putrefaction?

It is obſervable, (Table III.) that the healthieſt months of this year, were July and Auguſt; and that the moſt ſickly was November; and that Autumn and Winter, compared with the Spring and Summer quarters, were more fatal, nearly in the proportion of three to two. That Summer is leſs fatal than Winter, is a fact, confirmed by various obſervations, made in different places both of town and country. A moſt ingenious philoſopher, by a train of experiments, planned with amazing ſagacity, among a great variety of curious and uſeful diſcoveries, has proved, in a very ſatisfactory manner, that air, rendered noxious by the breath of animals, or by putrefaction, is reſtored, by vegetables growing in it, to a ſtate fit for reſpiration, and the ſupport of animal life. Hence is it not probable, that vegetation, among other cauſes, contributes to render Spring and Summer ſo remarkably more healthy, than the reſt of the year.

The only diſeaſe, that was generally epidemic this year, was the chincough, which began about

Auguſt, and, during the remainder of 1772, infected a very large proportion of the children of the whole town; about the end of the year, the diſorder almoſt entirely ceaſed. In Liverpool, the chincough became epidemic in May, and declined in November. From the Table of diſeaſes it appears, that moſt of the children, who died of this diſorder, were under two years of age. Vomits of emetic tartar were of ſignal ſervice, during this epidemic, in mitigating both the cough and fever. When we reflect, that the weakeſt infants puke up their food without much difficulty or fatigue, that nature takes this method ſo frequently of relieving them, and that emetic tartar, when diſſolved in water, imparts to it no ſenſible taſte, ſo that there could be no difficulty in giving it, there is a high degree of probability, that this remedy might have ſaved many more lives, if it had been more generally adminiſtered to children, in early infancy.

In the Table of diſeaſes (No. II.) the technical are added to the vulgar names, and the arrangement of a juſtly celebrated profeſſor is adopted, in order to convey more diſtinct ideas to the faculty, and to place diſorders of a ſimilar nature neareſt each other, for their mutual illuſtration.

TABLE I. DEATHS.

Ages	Males.	Females.	Ages.	Batchelors	Husbands	Widowers	Maids.	Wives.	Widows.	Total.
Under 1 Month.	8	6	20-25.	2	1		6	2		11
Between 1-2 Months	3	2	25-30.	1			5	5	1	12
2-3	6	5	30-35.	2	2		2	3	1	10
3-6	5	13	35-40.		1		2	2	2	7
6-9	5	8	40-45.	1	2		2	3		8
9 Months and 1 Year	7	13	45-50.	2	4	1	1	4	1	13
1-2 Years old	22	29	50-60.		13	2	4	3	4	26
2-3	6	13	60-70.	1	7	2	3	11	8	32
3-4	2	3	70-80.	3	11	7	4	4	18	47
4-5	6	3	80-90.		2		4		14	20
5-10	5	3	90-100						4	4
10-15	4	2								
15-20	2	8								
Total of the above Ages	81	108	Total of each Condition.	12	43	12	33	37	53	190
										108
										81
									Total	379

TABLE II. DISEASES.

[To be placed facing p. 76.]

DISEASES.	Under 1 Year	Betw. 1 & 2.	2-3.	3-5.	5-10.	10 15.	15-20.	20-30.	30-40.	40-50.	50-60.	60-70.	70-80.	80-90.	90 100.	Total.
I. FEBRILE DISEASES.																
Fever (G. 5, 6.) —	3		1	1	2		2	1	3	3	1	4				21
Jail Fever (5.) —								1	1							2
Mortification (7.) —											1		1			2
Pleurify (12.) —								1								1
Inflam. of the Bowels (16.)										1						1
Gravel and Stone (19.) —													1			1
Rheumatic Fever (22) —							1									1
Teething (Sauv. 198.) —	2	1														3
Gout (23.) —												1				1
St. Anthony's Fire (24.) —	1															1
Small Pox (26.) —	4	2	4	4	2											16
Meafles (28.) —		2														2
Miliary Fever (29.) —									1							1
Confumption (35.) —	2	3	2		2	4	6	12	10	6	9	3	3			62
Hæmorrhage (37.) —								1		1						2
II. NERVOUS DISEASES.																
Apoplexy (40.) —								1		1	1	1	3	1		8
Palfy (41.) —										1	1	4	3			9
Swoon (42.) —								1	1							2
Indigeftion (43.) —									1	1	1					3
Convulfions (48, 50.)	50	16	5	3		1					1					76
Afthma (52.) —										1	3	10	10	2		26
Chincough (53.) —	12	15	3	2	1											33
Colic (55.) —													1			1
Purging and Vomiting (56.)								1								1
Infanity (63.) —								1								1
III. DISEASES of the HABIT.																
Weaknefs of Infancy (65.)	3	9	3	2												17
Decay of Age (66.) —											2	5	22	17	4	50
Dropfy (71, 75.) —										3	4	2	1			10
Dropfy of the brain (72.)				1				1								2
Rickets (79.) —	3	2	1	1												7
King's Evil (80.) —						1		1								2
Jaundice (87.) —													1			1
IV. LOCAL DISEASES.																
Cancer (114.) —										1		1	1			3
Ulcer of the bladder (134.)												1				1
Unknown difeafes —	1	1			1		1			1	1					6
Cafualties — —								1		1	1					3
Total —	81	51	19	14	8	6	10	23	17	21	26	32	47	20	4	379

MORBORUM GENERA.

CLASSIS I. PYREXIÆ.
G. 5, 6. Typhus. Synochus.
G. 5. Typhus carcerum.
G. 7. Sphacelus.
G. 12. Pluritis.
G. 16. Enteritis.
G. 19. Nephritis.
G. 22. Rheumatifmus.
(Sauvag. G. 198. Odontalgia dentitionis.)
G. 23. Arthritis.
G. 24. Eryfipelas.
G. 26. Variola.
G. 28. Rubeola.
G. 29. Miliaria.
G. 35. Phthifis.
G. 37. Menorrhagia lochialis.

CLASSIS II. NEUROSES.
G. 40. Apoplexia.
G. 41. Paralyfis.
G. 42. Synchope (puerperalis.)
G. 43. Dyfpepfia.
G. 48, 50. Convulfio. Epilepfia.
G. 52. Afthma.
G. 53. Pertuffis.
G. 55. Colica.
G. 56. Cholera.
G. 63. Mania.

CLASSIS III. CACHEXIÆ.
G. 65. Tabes, atrophia infantilis.
G. 66. Atrophia, fenilis.
G. 71, 75. Afcites. Anafarca.
G. 72. Hydrocephalus.
G. 79. Rachitis.
G. 80. Schrophula.
G. 87. Ictcrus.

CLASSIS IV. LOCALES.
G. 114. Cancer.
G. 134. Ulcus (Sauv. G. 266. Pyuria.)

TABLE III.

Deaths in	Males.	Females.	Total.	Total of Deaths in each Seafon.
January, — —	22	19	41	
February, ——	15	21	36	105 Winter.
March, — —	11	17	28	
April, — —	13	12	25	
May, — —	11	19	30	83 Spring.
June, — —	10	18	28	
July, — —	6	18	24	
Auguft, — —	13	10	23	72 Summer.
September, ——	7	18	25	
October, — —	12	26	38	
November, — —	20	26	46	119 Autumn.
December, — —	13	23	35	
	153	226	379	

	M.	F.	Tot.
Explofion by Gunpowder, on the 5th of Nov.	11	12	23
Locked-Jaw, in Confequence of the Explofion,	0	3	3
			26

Marriages, 154.

Chriftenings, Males, 192, Fem. 229. Total, 421.

TABLE

TABLE IV.

The Year to which the ſeveral Ages below have an equal Chance to live.

Ages.	Cheſter.	Northam.	Norwich.	London.
0	21¼	9¼	5	2¾
3	55⅔	43½	43¼	34½
5	58½	46½	47	40
10	60	50	52¼	44
20	63	53¼	55¾	47½
40	69	62½	63⅓	58
50	71½	67½	67	65
60	73⅔	72¾	71⅓	70½
70	77	78	77	77

VII. *Elec-*

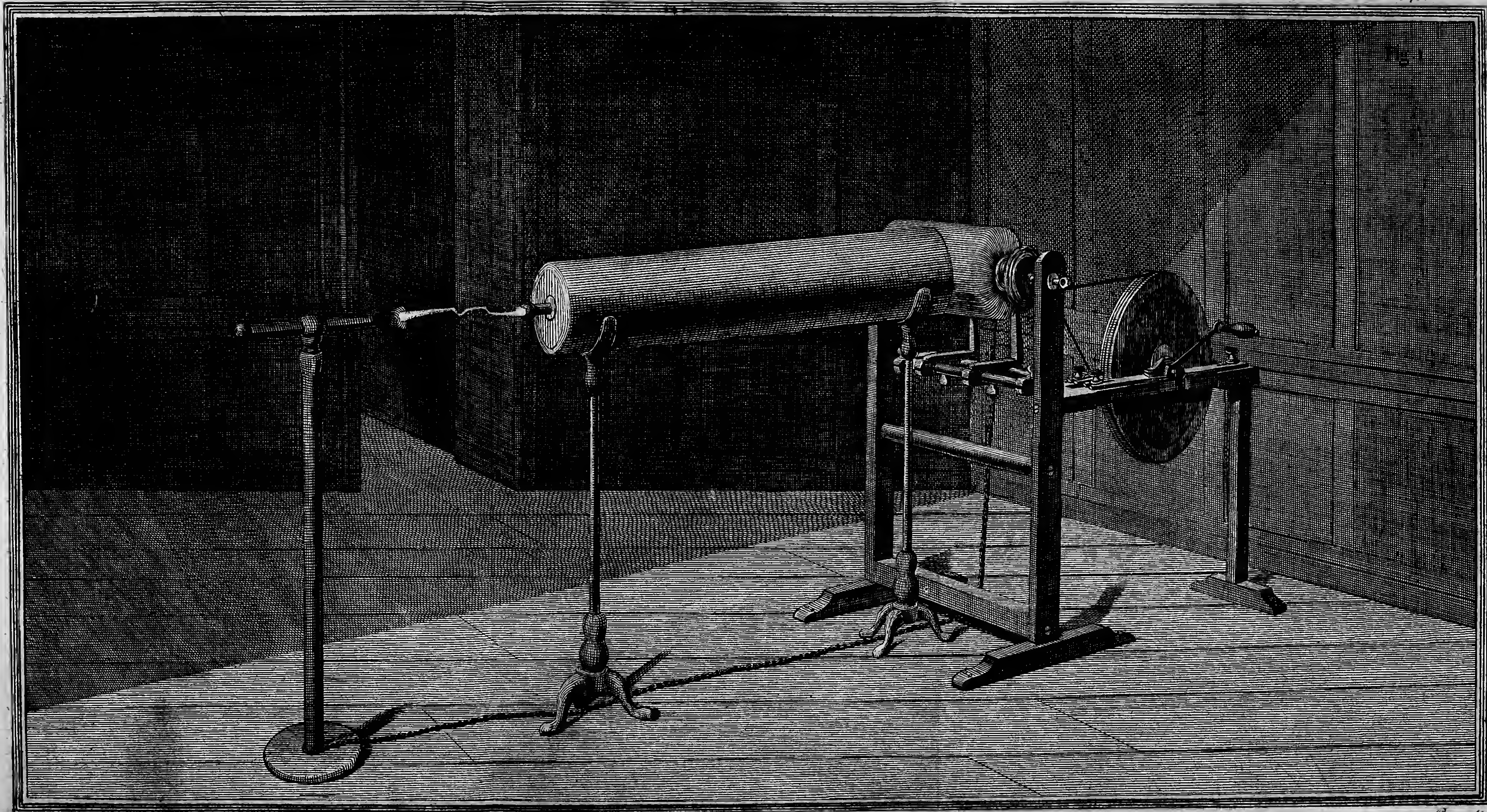
VII. *Electrical Experiments by Mr.* Edward Nairne, *of* London, *Mathematical Inſtrument-maker, made with a Machine of his own Workmanſhip, a Deſcription of which is prefixed.*

Redde, Dec. 9, 1773.

FIG. Iſt. [TAB V. and VI.] is a repreſentation of the machine, with its apparatus, as was uſed when electrical ſparks were drawn from the conductor, in a poſitive ſtate. The glaſs cylinder, of this machine, is 12 inches diameter, and the cylindrical part 19 inches long, excluſive of the necks; the cuſhion or rubber is 14 inches long, and 5 inches broad, ſupported by two wooden ſprings; which ſprings are fixed on two glaſs rods, which lie horizontal under the cylinder, and ſerve to inſulate the cuſhion. The conductor to this machine is 5 feet long, and 12 inches diameter; at the end of it is a ſhort braſs rod, with a ball; it is ſupported on two ſtands, with ſolid glaſs rods or pillars. The ball, which is repreſented as receiving the electrical ſpark from the conductor, is of braſs, and fixed to the end of a braſs tube, which tube is moveable, in a hole in the top of the receiving ſtand; from the bottom of this ſtand a chain paſſes along the floor, till it is in contact with the chain hanging from the back of the cuſhion, as is repreſented in the figure. With this machine I have frequently drawn electrical ſparks, at the diſtance of 12, 13, or $13\frac{1}{2}$ inches from the prime conductor. Theſe were indeed the diſtances, to which the electrical fire would commonly ſtrike. It would ſometimes reach the diſtance of 14 inches; but this was but ſeldom.

Fig. II. repreſents the ſame machine, with a ſmall braſs conductor, inſtead of the large one, for charging the batteries, which batteries are compoſed of four boxes, each containing 16 jars of 12 inches high and 4 inches diameter, coated 8 inches high; ſo that, in the 64 jars, there are very nearly 50 ſquare feet of coated ſurface. The electrometer * is raiſed, ſo as to be 4 feet from the bottom, which reſts on the jars, to the ball at top. I at firſt ſet it on the battery, ſo that the ball, at the end of the Index, was about one inch higher than the balls or wires of the battery, which is the general method of uſing it; but I found, on placing it ſo low on theſe batteries, that the index would move but a very little way, hardly to 15°, when the battery was full charged; at laſt, after trying a great variety of experiments, I found, that, placing it at the height of 4 feet, the index would riſe to 60, with the ſame quantity of charge as, in the other caſe, raiſed it only to 15: diſcharging this battery, through a piece of *iron* wire (not ſteel) of $\frac{1}{151}$ of an inch diameter, and three feet nine inches long, it flew about the room in innumerable red hot balls; on examining theſe balls, they were in

* Vide Phil. Tranſ. Vol. LXII. p. 359.



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general hollow, and ſeemed to be nothing but ſcoria. The drawing is a repreſentation of diſcharging the battery through the wire. I have made a piece of the ſame wire, of 3 feet 11 inches long, red hot, from end to end, ſo that it ſeparated into ſeveral pieces. After this, I took a piece of the fine iron wire beforementioned, of ſix inches in length, and, to the end of it, connected a piece of iron wire $\frac{1}{25}$th of an inch in diameter, and 48 feet long. Then, on diſcharging the battery, the electrical fire from the inſide paſſed immediately along the diſcharging rod to the fine wire, and afterwards had 48 feet to paſs, to get to the outſide coating of the battery: I then laid another piece, ſo that the electrical fire paſſed 48 feet, from the inſide of the battery, before it came to the ſmall wire; and again another, ſo that the electrical fire paſſed from the inſide of the battery 24 feet, before it came to the fine wire, and had 24 feet afterwards to paſs, before it could get to the outſide coating of the battery; in each caſe, the 6 inches of the ſmall wire was melted into red hot balls; and I could not perceive, that there was the leaſt difference, in the melting of the wire, on its being placed in different parts of the circuit. Next, I connected to a piece of the ſame fine iron wire, of 6 inches in length, a piece of the iron wire $\frac{1}{15}$th of an inch in diameter, and in one continued piece of 274 feet in length. In this arrangement, when the battery was diſcharged, the electrical fire paſſed immediately from the diſcharging rod to the fine wire, and had 274 feet to paſs afterwards, to get to the outſide coating; then the fine wire was laid next the outſide coating of the battery, ſo that the

 electrical

electrical fire paſſed 274 feet, before it reached it. This experiment was repeated ſeveral times, with this difference, that before every diſcharge of the battery I ſhortened the fine wire, till, at laſt, there was but half an inch of it connected with the 274 feet of wire; but even that ſhort piece was not made red hot by the diſcharge of the 64 jars. The electrical fire, in paſſing that 274 feet of wire, though it was one entire piece without any joinings, ſeemed to meet with great reſiſtance, for the exploſion from the battery was not ſo loud, as when a very ſmall electrical bottle is diſcharged. Next, I took ſome ſilver thread, and made a circuit, of 40 feet, from the inſide of the battery to the outſide; and, at the diſtance of about 12 feet from the battery, I held the ſilver thread between my finger and thumb, ſo that the electrical fire, paſſing along the thread, paſſed between them; on diſcharging the battery, I received a ſmart ſhock, particularly in both my ancles, though the thread was held three feet and a half from the dry floor, on which I ſtood: by the electrometer, the battery did not appear to be half diſcharged. Then I made a circuit, of 40 feet, with iron wire $\frac{1}{25}$th of an inch in diameter, and this was held in the ſame manner as the ſilver thread; on diſcharging the battery through the iron wire, there was not the leaſt ſhock felt, though the whole of the battery was diſcharged, the iron wire of that length conducted it ſo perfectly.

Then I tried the effect of the battery on ſome *platina*, which came from Doctor Lewis, of Kingſton; ſeveral of the grains, or *laminæ*, were laid on on a piece of white wax, fo as to make a length of half an inch; this method being recommended by my ingenious friend Mr. HENLEY, F. R. S. who had before ufed it with fuccefs. On difcharging the battery through the platina, I found, that not only the furface of fome of the *laminæ*, or grains, had been in fufion, but that part of it was melted in beautiful white fpherules, vifible to the naked eye.

Another experiment that I tried, was on a duck; a chain was faftened to its legs, and, holding it by the wings, the head was brought up to one of the rods of the battery, fo that the battery was difcharged through it, from the head to the feet; the confequence was, the duck was thrown into violent convulfions, and expired in two or three minutes. Then I took a turkey, and faftened a wire round its neck, and another on its rump, in fuch manner, that the neareft diftance between the wires was along the back bone, thinking the charge of the batteries might pafs down the fpine, and that the turkey would be made paralytic; but, on difcharging the battery, the turkey opened its bill, and died inftantly. Then I took a cock, and faftened a wire on his rump, and placed one of the balls, of the difcharging rod, on the middle of his back, fo that the charge might not pafs near his vital parts; the battery being difcharged, the body of the cock was violently agitated, for about half a minute, and the head was turned, fo that the bill came againft its breaft; the head and neck however foon recovered, fo that it moved its neck, to all appearance, as well as it did before it

 was

was ſtruck; but the body was quite motionleſs, for about twenty minutes; after that it recovered very faſt, and, in about ten minutes more, was able to ſtand, and walk a little. After this, I put a wire round its neck, in the ſame manner as on the turkey; the effect was exactly the ſame; for, on diſcharging the battery through it, it died inſtantly. The wire, that conducted the electrical ſtroke which killed the turkey and cock, was $\frac{1}{25}$th of an inch in diameter.

The next experiment I tried was on ſome plants. I diſcharged the battery through a branch of a balſam, and examined it very attentively immediately after it was ſtruck, but could not perceive, that there was the leaſt alteration in the branch, till about ten or fifteen minutes after; and then the upper part of the branch began to droop its head, and continued drooping it, till it hung quite ſtrait down, and in two or three days entirely withered, though the other part of the plant was very vigorous, and did not appear to be in the leaſt affected; this experiment I repeated, ſeveral times, on ſeveral balſams, and always found the ſame appearances. I next tried the effect of the battery on a privet tree: what led me to try it, was the appearance, that I had often obſerved in trees, more particularly this ſummer, that ſome of their branches were deprived of all their leaves, while the reſt of the tree was in a very flouriſhing ſtate, and that many trees had loſt all their leaves before the uſual time; it ſtruck me, that this might poſſibly be cauſed by lightning; I therefore reſolved to try, whether I could produce a ſimilar appearance; accordingly I diſcharged the battery through a branch of the privet tree, without the leaſt apparent injury to the branch, at that time; but after three weeks, or thereabouts, the leaves of that branch, as far as the electrical fire had paſſed, began to wither and drop off, ſo that, about a month after it was ſtruck, it had juſt the appearance of the branches of the trees beforementioned; and the reſt of the tree was in a flouriſhing ſtate, and did not appear to be the leaſt hurt. The index of the electrometer, in all the beforementioned experiments, was raiſed to 60°.

September 14, 1773, the following experiments were made, in the preſence of Mr. BANKS, and ſeveral other members of the ROYAL SOCIETY, upon the undermentioned plants, which Mr. BANKS procured to be brought in pots, in a healthy, and moſt of them in a flowering ſtate; theſe were a female balſam, a marvel of Peru, a cardinal flower, an African geranium, a laurel, and a myrtle. Theſe plants were ſo diſpoſed, that a part only, of each, lay within the electric circuit; which part was carefully diſtinguiſhed, by a piece of thread tied juſt below it. The electrometer being raiſed to 60°, the battery was diſcharged, and the effect was, that, in proportion as theſe vegetables were herbaceous and ſucculent, the ſooner the parts of them, through which the ſhock paſſed, were obſerved to decay. Thus the main ſtem of the balſam, though, before the ſtroke, perfectly erect, drooped in a few minutes, and was quite dead the next morning; notwithſtanding that the other ſtems of the ſame plant, which were not within the circuit, continued in flower;

flower; and as a proof that the injury was local and partial, the plant, is yet alive, (November 15.)

The marvel of Peru and Geranium, ſuch parts of them as were ſtruck, were dead the next day, though the other parts ſtill continue alive.

The cardinal flower, much leſs ſucculent than any of the former, ſeemed for ſome days to have ſuffered in its flowers only, which withered the next day after the ſtroke, though the flowers of the other ſtems, and which were not ſtruck, went through their uſual courſe: in a few days however the leaves changed their colour, as well as the ſtem, and died; after which, the ſtem became perfectly dry; the other ſtems did not ſuffer in the leaſt.

It was a fortnight after the ſtroke, before the branch of the laurel, through which the electricity was directed, gave any indication of its being injured; ſoon after, however, the leaves changed their colour, dropped off, and the branch died; the reſt of this ſhrub is in perfect health.

For near a month the whole myrtle continued without any ſeeming injury, ſince which time ſeveral of the ſprigs near its top have died; and though it was ſo long before the injury received declared itſelf, yet the miſchief to this myrtle was more extenſive, than it was to any other of the vegetables expoſed to theſe trials.

Dr. WATSON, who has very long been converſant in electrical enquiries, was preſent at theſe experiments; he took the plants abovementioned under his care, and uſed his beſt endeavours for their preſervation, in order to obſerve, with the greater exactneſs, the progreſs of the effects of theſe electrical ſtrokes.

From

From theſe experiments we find, that electricity, accumulated to a certain degree, puts an end to vegetable as well as animal life.

After having recited theſe experiments, I would beg leave to mention a caution, which may be of ſervice to future electricians who may uſe large batteries.

It is, never to diſcharge their batteries, if it is through a ready conductor, without the charge paſſes at leaſt five feet from the inſide of the battery to the outſide; by making uſe of this precaution, which I learnt from experience, I have diſcharged the beforementioned battery near one hundred times, and have never, ſince I have uſed that precaution, broke a ſingle jar by the electrical diſcharge, before which I was continually breaking them, by diſcharging the battery in the common method. There is another experiment, which I would wiſh to mention, as it probably may give ſome light in reſpect to balls, or points, for conductors, for buildings or ſhips: the apparatus and manner of trying the experiment, is as follows; in fig. 3. A repreſents the end of the large conductor of the electrical machine; B a braſs ball ſcrewed into the end of it, of one inch and nine tenths diameter; C a ſmall conductor, which was 5 feet 11 inches long, and one inch and four tenths diameter; it was made of wood, covered with tin-foil, and was inſulated, by being ſupported on a ſtand, the part D of which was of ſolid glaſs. The ball E, at the end of this conductor, was three inches diameter, and the ball F one inch and nine tenths diameter; under this ball, F, was a ſtand, G, made of wood covered with tin-foil, which had a moveable

moveable part, H, which might be raiſed higher or lower. On the top of this moveable part was ſcrewed either a pointed wire, or a wire with a ball $\frac{1}{4}$th of an inch diameter, and from the bottom of this ſtand a chain paſſed along the floor, till it was connected with the chain, which hung from the cuſhion; then I placed the conductor C, ſo that the ball E was four inches diſtance from the ball B, and having ſcrewed into the top of the moveable part, H, of the ſtand, G, a pointed wire, I moved it till the point was directly under the ball F, at the diſtance of 3 or 4 inches; and, on exciting the electrical machine, the fire paſſed from the ball B, to the ball E, and, almoſt at the ſame inſtant, ſtruck on the point from the ball F. I increaſed the diſtance ſlowly between the point and the ball, F, till I found the utmoſt diſtance, to which it would ſtrike to the point, which was ſix inches; I continued to move the point to nine inches diſtance or more; it then was luminous, and the fire continued to ſtrike, from the ball B, to the ball E, which ſhewed that the point carried off all the electrical fire from the conductor, C, otherwiſe it would not continue to ſtrike from B to E. Then I removed the point, every thing elſe remaining as before, and, in its ſtead, placed, a wire with a ball of $\frac{1}{4}$th of an inch diameter, at the top of it, at the diſtance of 3 or 4 inches, directly under the ball F, in the ſame manner as the point; then, on encreaſing this diſtance ſlowly, the electrical fire was found to ſtrike to the ball at nine inches, which is half as far again as to the point, and with this remarkable difference, that the quantity of fire was much greater, and the explosion

plosion much stronger and louder, at its striking the ball, than at its striking the point.

It may here be observed, that a point cannot possibly be placed in circumstances more unfavourable, than these, to its operation *as a point*: the body of electric fluid falling upon it almost instantaneously, with the stroke from B to E, so that it had scarce any measurable time, wherein to act, as a point, in diminishing the quantity, before the whole fell upon it as upon a conductor. In the use of points to receive and conduct lightning, they generally act on the electrical atmosphere of a cloud, while the cloud is yet at a distance, diminishing gradually that atmosphere, before the cloud approaches near enough to give the stroke, and thus diminishing the stroke, if not quite preventing it. If the small conductor, C, is placed so as to be in contact with the large conductor, A, instead of being four inches distant, as before, the electrical fire will not strike to the point at any distance whatever; but the point will carry off silently all the electrical fire from the conductors, as fast as the cylinder supplies them, even if the point is placed at the distance of ten inches or more.

I am afraid I shall have tired you with this minute detail; but I have been thus particular, that any Gentlemen may satisfy themselves of the truth of these experiments.

To this machine there was another large conductor, 12 inches diameter, and five feet long, which being applied with its points to the back of the cushion, the machine was either negative or positive, only by hanging a chain on either conductor.

VIII. *On the noxious Quality of the Effluvia of putrid Marshes. A Letter from the Rev. Dr.* Prieſtley *to Sir* John Pringle.

To Sir JOHN PRINGLE, Baronet.

DEAR SIR,

Redde, Dec. 16, 1773.

HAVING purſued my experiments on different kinds of air conſiderably farther, in ſeveral reſpects, than I had done, when I preſented the laſt account of them to the Royal Society; and being encouraged by the favourable notice which the Society has been pleaſed to take of them, I ſhall continue my communications on this ſubject; but, without waiting for the reſult of a variety of proceſſes, which I have now going on, or of other experiments, which I propoſe to make; I ſhall, from time to time, communicate ſuch detached articles, as I ſhall have given the moſt attention to, and with reſpect to which, I ſhall have been the moſt ſucceſsful in my inquiries.

Since the publication of my papers, I have read two treatiſes, written by Dr. ALEXANDER, of Edinburgh, and am exceedingly pleaſed with the ſpirit

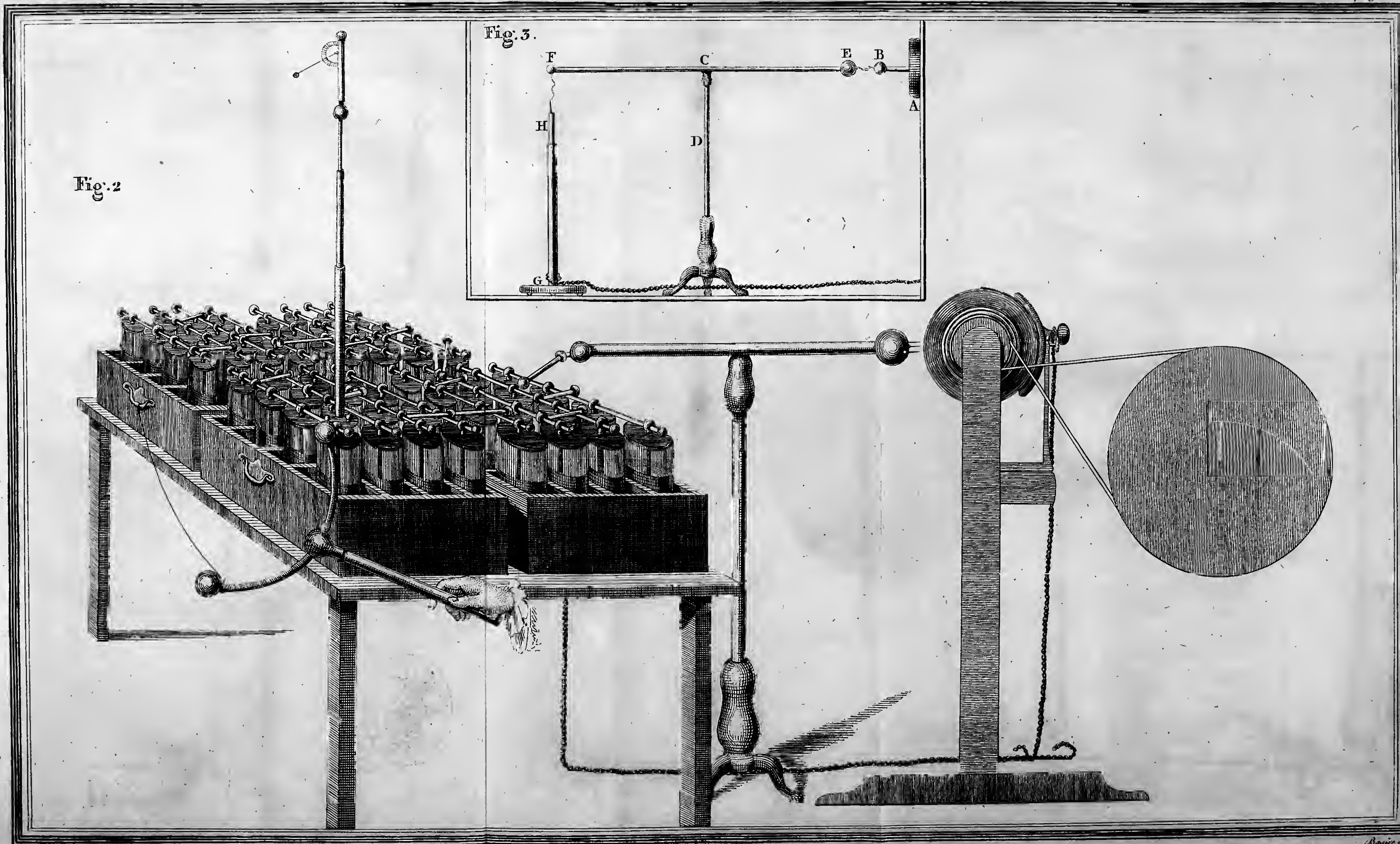


Fig. 2
Fig. 3.
F
C
E
B
A
H
D
G

of philosophical inquiry, which they discover. They appear to me to contain many new, curious, and valuable observations; but one of the conclusions, which he draws from his experiments, I am satisfied, from my own observations, is ill founded, and from the nature of it, must be dangerous. I mean his maintaining, that there is nothing to be apprehended from the neighbourhood of putrid marshes.

I was particularly surprized, to meet with such an opinion as this, in a book inscribed to yourself, who have so clearly explained the great mischief of such a situation, in your excellent treatise on the diseases of the army. On this account, I have thought it not improper, to address to you the following observations and experiments, which I think clearly demonstrate the fallacy of Dr. ALEXANDER's reasoning, indisputably establish your doctrine, and indeed justify the apprehensions of all mankind in this case.

I think it probable enough, that putrid matter, as Dr. ALEXANDER has endeavoured to prove, will preserve other substances from putrefaction; because, being already saturated with the putrid effluvium, they cannot readily take any more; but Dr. ALEXANDER was not aware, that air thus loaded with putrid effluvium is exceedingly noxious when taken into the lungs. I have lately, however, had an opportunity of fully ascertaining how very noxious such air is.

Happening to use at Calne, a much larger trough of water, for the purpose of my experiments, than I had done at Leeds, and not having fresh water ſo near at hand as I had there, I neglected to change it, till it turned black, and became offenſive, but by no means to ſuch a degree, as to deter me from making uſe of it. In this ſtate of the water, I obſerved bubbles of air to riſe from it, and eſpecially in one place, to which ſome ſhelves, that I had in it, directed them; and having ſet an inverted glaſs veſſel to catch them, in a few days I collected a conſiderable quantity of this air, which iſſued ſpontaneouſly from the putrid water; and, putting nitrous air to it, I found that no change of colour or diminution enſued, ſo that it muſt have been, in the higheſt degree, noxious. I repeated the ſame experiment ſeveral times afterwards, and always with the ſame reſult.

After this, I had the curioſity to try how wholeſome air would be affected by agitation in this water; when, to my real ſurpriſe, I found, that after one minute only, a candle would not burn in it; and, after three or four minutes, it was in the ſame ſtate with the air, which had iſſued ſpontaneouſly from the ſame water.

I alſo found, that common air, confined in a glaſs veſſel, in contact only with this water, and without any agitation, would not admit a candle to burn in it after two days.

Theſe facts certainly demonſtrate, that air which either ariſes from ſtagnant and putrid water, or which has been for ſome time in contact with it, muſt be very unfit for reſpiration; and yet Dr. ALEXANDER's opinion is rendered ſo plauſible by his experiments, that it is very poſſible that many perſons may be rendered ſecure, and thoughtleſs of danger, in a ſituation in which they muſt neceſſarily breathe it. On this account, I have thought it right to make this communication as early as I conveniently could; and as Dr. ALEXANDER appears to be an ingenuous and benevolent man, I doubt not but he will thank me for it.

That air iſſuing from water, or rather from the ſoft earth, or mud, at the bottom of pits containing water, is not always unwholeſome, I have alſo had an opportunity of aſcertaining. Taking a walk, about two years ago, in the neighbourhood of Wakefield, in Yorkſhire, I obſerved bubbles of air to ariſe, in remarkably great plenty, from a ſmall pool of water, which, upon inquiry, I was informed had been the place, where ſome perſons had been boring the ground, in order to find coal. Theſe bubbles of air having excited my curioſity, I preſently returned, with a baſon, and other veſſels proper for my purpoſe, and having ſtirred the mud with a long ſtick, I ſoon got about a pint of this air; and, examining it, found it to be good common air; at leaſt a candle burned in it very well. I had not then diſcovered the method of aſcertaining the goodneſs of common air, by a mixture of nitrous air. Previous to the trial, I had ſuſpected that this air would have been found to be inflammable.

I ſhall conclude this letter with obſerving, that I have found a remarkable difference in different kinds of water, with reſpect to their effect on common air agitated in them, and which I am not yet able to account for. If I agitate common air in the water of a deep well, near my houſe in

Calne,

Calne, which is hard, but clear and ſweet, a candle will not burn in it after three minutes. The ſame is the caſe with the rain water, which I get from the roof of my houſe. But in diſtilled water, or the water of a ſpring-well near the houſe, I muſt agitate the air about 20 minutes, before it will be ſo much injured. It may be worth while, to make farther experiments, with reſpect to this property of water.

In conſequence of uſing the rain water, and the well water above-mentioned, I was very near concluding, contrary to what I have aſſerted in my printed papers, that common air ſuffers a decompoſition by great rarefaction. For when I had collected a conſiderable quantity of air, which had been rarefied about four hundred times, by an excellent pump made for me by Mr. SMEATON, I always found, that when I filled my receivers with the water above-mentioned, though I did it ſo gradually as to occaſion as little agitation as poſſible, a candle would not burn in the air that remained in them. But when I uſed diſtilled water, or freſh ſpring water, I undeceived myſelf.

I think myſelf honoured by the attention, which, from the firſt, you have given to my experiments, and am, with the greateſt reſpect,

Dear Sir,

Your moſt obliged

humble Servant,

London, 7 Dec. 1773.

J. PRIESTLEY.

POST-

POSTSCRIPT.

I cannot help expreſſing my ſurprize, that ſo clear and intelligible an account, of Mr. SMEATON's air-pump, ſhould have been before the public ſo long, as ever ſince the publication of the forty-ſeventh volume of the Philoſophical Tranſactions, and yet that none of our philoſophical inſtrument makers ſhould attempt the conſtruction. The ſuperiority of this pump, to any that are made upon the common plan, is, indeed, prodigious. Few of them will rarefy more than 100 times, and, in a general way, not more than 60 or 70 times; whereas this inſtrument muſt be in a poor ſtate indeed, if it do not rarefy 200 or 300 times; and when it is in good order, it will go as far as 1000 times, and ſometimes even much farther than that; beſides, this inſtrument is worked with much more eaſe, than a common air-pump, and either exhauſts or condenſes at pleaſure. In ſhort, to a perſon engaged in philoſophical purſuits, this inſtrument is an invaluable acquiſition. I ſhall have occaſion to recite ſome experiments, which I could not have made, and which, indeed, I ſhould hardly have dared to attempt, if I had not been poſſeſſed of ſuch an air-pump as this. It is much to be wiſhed, that ſome perſon of ſpirit in the trade would attempt the conſtruction of an inſtrument, which would do great credit to himſelf, as well as be of eminent ſervice to philoſophy.

IX. *Farther Proofs of the Inſalubrity of marſhy Situations. In a Letter from the Rev. Dr.* Price *to the Rev. Dr.* Horſley.

DEAR SIR,

Redde, Jan. 13, 1774.

DR. PRIESTLEY's paper, on the noxious effects of ſtagnant waters, read laſt Thurſday to the Royal Society, brought to my remembrance a table, exhibiting the rate of mortality in a pariſh ſituated among marſhes, which I had ſeen in Mr. MURET's Obſervations, publiſhed in the Memoirs of the Oeconomical Society at Bern, for 1766. I have ſince reviewed this table, and found that it affords a full confirmation of Dr. PRIESTLEY's aſſertions. This pariſh is a part of the diſtrict of Vaud, belonging to the canton of Bern, in Switzerland; and contains 169 families, and 696 inhabitants. Mr. MURET's table, of the rate of mortality in it, is formed from a regiſter of the ages, at which all died in it for 15 years. With this table he has alſo given tables, from like regiſters, of the rates of mortality in ſeven ſmall towns; in 36 country pariſhes and villages; in 16 pariſhes ſituated in the Alps; in 12 corn pariſhes, and in 18 vintage pariſhes.— From comparing theſe tables, it appears, that the

the probabilities of life are higheſt in the moſt hilly parts of the province, and loweſt in the marſhy pariſh juſt mentioned. The difference is indeed remarkable, as will appear from the following particulars. One half, of all born in the mountains, live to the age of 47. In the marſhy pariſh, one half live only to the age of 25. In the hills one in 20, of all that are born, live to 80. In the marſhy pariſh, only one in 52 reaches this age. In the hills, a perſon aged 40 has a chance, of 80 to 1, for living a year. In the marſhy pariſh, his chance for living a year is not 30 to 1.—In the hills, perſons aged 20, 30, and 40, have an even chance for living 41, 33, and 25 years reſpectively. In the fenny pariſh, perſons, at theſe ages, have an even chance of living only 30, 23, and 15 years.—In ſhort, it appears, that, though the probabilities of life, in all this country except this one pariſh, are much higher than in London; yet here, after 30, they are much lower. Before the age of 30, they are indeed higher in this pariſh; the reaſon of which muſt be, that the London air and cuſtoms are particularly noxious to children *.

I am ſenſible, that obſervations, for only 15 years, in one ſmall pariſh, do not afford as deciſive and ample an authority, in the preſent caſe, as there is reaſon to wiſh for; and that, therefore, the perfect exactneſs, of the particulars I have recited, cannot be depended on.—They are, however, ſufficiently

* In London, one half of all that are born, die under 3 years of age. But this is not peculiar to London. In Berlin the ſame proportion dies under three; and at Vienna under two.

ficiently near the truth to demonſtrate, in general, the unhealthfulneſs of a marſhy ſituation, and as the regiſter from whence they are derived is the only one, in ſuch a ſituation, which I have ever met with, and Dr. ALEXANDER's experiments may lead ſome to very wrong concluſions on this ſubject; I could not help thinking, that there would be no impropriety, in ſending you the account, I have now given. If you think it of any importance, I ſhall be obliged to you for reading it to the Royal Society.

I cannot help taking this opportunity to add my wiſhes, that ſuch regiſters of mortality, as thoſe publiſhed by Mr. MURET, were eſtabliſhed in every part of this kingdom. We might then determine immediately every ſuch queſtion, as that which has occaſioned this letter; and know certainly, what influence different airs and different ſituations have on the duration of life. Two ingenious phyſicians, Dr. PERCIVAL at Mancheſter, and Dr. HAYGARTH at Cheſter, have lately, with much zeal, promoted inſtitutions of this kind; and a great deal of uſeful information may be expected, from the accurate and comprehenſive regiſters of mortality, which, under their direction, have been eſtabliſhed in theſe towns. But the inſtruction ariſing from theſe eſtabliſhments cannot be complete, till they become univerſal.

I am, Sir,

Your moſt obedient

and humble Servant,

Newington Green,
Dec. 21, 1773.

RICHARD PRICE.

X. Of

X. *Of the Culture and Uſes of the* Son *or* Sun-*plant of* Hindoſtan, *with an Account of the manner of manufacturing the* Hindoſtan *Paper. By Lieutenant Colonel* Ironſide. *Communicated by Dr.* Heberden *.

Redde, Dec. 23, 1773. THIS uſeful plant, I believe, is cultivated all over Hindoſtan. The ſeeds are ſown in July, before the rains begin; they ſhould be ſown near to one another, to make the ſtem riſe higher, more erect, with fewer branches, and to encreaſe the produce. It flowers in October, and is taken up in December.

The black ladies uſe the ſeeds, reduced to powder and mixed with oil, for their hair, upon a ſuppoſition, that this compoſition will make their hair grow to a great length, which they are very fond off.

From the bark are made all kinds of rope, packing cloths, nets, &c. and from theſe, when old, moſt of the paper, in this country, is prepared; for

* This Plant is deſcribed by Linnæus, under the name of Crotalaria juncea, vid. Spec. Plant. 1004. A figure of it is given by Ehret in Trews Plant. Select. t. 47. and another in the Hort. Malab. 9. p. 47. t. 26. Both theſe figures are good.

 theſe

theſe purpoſes, the freſh plant is ſteeped four days in water, afterwards dried, and treated as the cannabis for hemp, to which it is ſo ſimilar when prepared, that Europeans generally ſuppoſe it to be the produce of the ſame plant.

As the ſubſtances, producing cloths, ropes, and paper, are few in preſent uſe, this plant may perhaps be cultivated with advantage, in ſome of the Britiſh Weſt India ſettlements, and in other countries deſtitute of hemp and flax. It is not improbable, that it may be raiſed in the warmer climates of Europe, as it ripens here in winter. I cannot ſay, what ſoils it may refuſe; where I have ſeen it, in the greateſt plenty and perfection, has generally been upon an earth compoſed of clay, calcareous grit, and ſand.

There are other vegetable ſubſtances uſed here for the purpoſe of rope making; one of them is a ſpecies of the hibiſcus, a deſcription of which I propoſe for the ſubject of another paper: I can ſcarce doubt, but that it is only for want of experiments, we have not a greater number of vegetables rendered uſeful in this manner. The claſs *Monadelphia*, of LINNÆUS, promiſes fair for trials of this kind.

The Hindoſtan method of manufacturing Paper.

The manufacturer purchaſes old ropes, cloths, and nets, made from the ſun plant, and cuts them into ſmall pieces, macerates them in water, for a few days, generally five, waſhes them in the river in a baſket, and throws them into a jar of water

lodged

lodged in the ground; the water is ſtrongly impregnated with a lixivium of ſedgi mutti * ſix parts and quick lime ſeven parts. After remaining in this ſtate eight or ten days, they are again waſhed, and while wet, broken into fibres, by the ſtamping lever, Fig. 1. [TAB. VII.] and then expoſed to the ſun, upon a clean terraſs, built for this purpoſe; after which, they are again ſteeped, in a freſh lixivium, as before. When they have undergone three operations of this kind, they are fit for making coarſe brown paper; after ſeven or eight operations, they are prepared for making paper, of a tolerable whiteneſs.

The rags, thus prepared, are mixed with water in the ciſtern, Fig. 2. at the edge of which the operator ſits, and removing the ſtick, he extends the ſcreen, Fig. 3. upon the frame, Fig. 4. with which he agitates the water in the ciſtern, until it appears of a milky whiteneſs, by the floating particles of the rags; he then dips the frame and ſcreen, in a perpendicular manner, and raiſes them gently, in a horizontal poſition, to the ſurface of the water, where he gives the frame a gentle motion, from ſide to ſide, or from end to end, to make the particles of the rags fall in an equal layer upon the ſcreen, and then he lifts them out of the water, and reſts them for the ſpace of a minute upon the ſtick 3, in Fig. 3. After repeating the dip once more in the ſame manner, the new ſheet

* Sedgi Mutti is an earth, containing a large portion of foſſil alkali. The νατρον of the antients. It is found in great plenty in this country, and univerſally uſed in waſhing, bleaching, ſoap-making, and for various other purpoſes.

of

of paper is formed; then, taking off the extenſors, BB, of the ſcreen, he rolls inwards, for about an inch, the upper part of the ſcreen and ſheet, by which means, ſo much of the ſheet will be ſeparated from the ſcreen; the ſcreen is then inverted, and the already ſeparated end of the paper is laid upon the mat, in Fig. 5, and the ſcreen is gently raiſed from the paper.—In this manner he forms ſheet after ſheet, until he has made 250, his day's taſk, laying them all upon the firſt ſheet, in a regular manner; then he covers the whole with a coarſe cloth of the ſun-plant, equal in ſize with the paper; above this he lays a thick plank, ſomewhat larger than the paper. This, by its weight, preſſes out the water from the wet ſheets; to aſſiſt which, the operator ſits upon it for ſome time. Then the heap is ſet to one ſide until morning, when the ſheets are taken up, one by one, and ſpread with the bruſh, Fig. 6, upon the clean plaſtered walls of the houſe; as they dry, they readily peel off, and are ſpread upon a clean mat or cloth, and with a piece of blanket, dipped in thin rice paſte water, rubbed all over, and immediately hung up, to dry, upon ſtrings run acroſs the houſe for this purpoſe. When ſufficiently dried, they are cut into a quadrangular form, according to a ſtandard ſheet, which ſerves as a guide to the knife, Fig. 7. From this operation, they are carried to another perſon, who rubs every ſheet ſmooth with a globular piece of moorſtone granite, which he holds in both hands. Then he folds the ſheets for ſale. The finer paper is poliſhed a ſecond time. All the cuttings, and damaged ſheets

ſheets, are trampled to pieces in water, and renovated as above.

Inſtruments uſed in making the paper. TAB. VII.

Fig. 1ſt. *a.* A ſtamping lever, ten feet long, and ſeven inches ſquared timber.

b. Two pieces of wood, fixed in the floor, to ſupport the axis of the lever.

c. This end of the lever is preſſed down by the feet of two men.

d. Is a ſtick, ſuſpended from the roof of the houſe, to which are faſtened four ropes which ſupport the arms of the workmen.

e. The head of the ſtamper four feet long, and four inches ſquared timber, bound and ſhod at the point with iron.

ff. A perpendicular ſection of a terraſſed ciſtern, dug in the ground-floor about 4 or 5 feet ſquare.

gg. A ſquare ſtone, in the bottom of the ciſtern, excavated in the middle, to receive the head of the ſtamper, by which the rags are beat to pieces. A perſon is ſtationed in the ciſtern, to ſupply the ſtamper with rags.

Fig. 2d. 1. 1. A terraſſed ciſtern, dug in the floor, 4 or 5 feet ſquare, having two little eminences,

2. 2. at the edge, to ſupport the ſtick

3. occaſionally.

4. A jar, lodged in the floor, to hold in readineſs the prepared rags,

Fig.

Fig. 3d. Is made in the manner of the Chinefe bamboo window-fcreens. The tranfverfe lines are fine rufh, or a grafs, neatly bound with horfe hairs, which makes the longitudinal lines.

A A. Two fticks, to which the fcreen is faftened, and extended by the two fticks,

B B. occafionally.

Fig. 4th. A form of wood, with feven bars, to fupport the fcreen, Fig. 3d. The bars are fo fixed, as that their acute edges only touch the fcreen, that there may be no obftruction to the paffage of water through the fcreen.

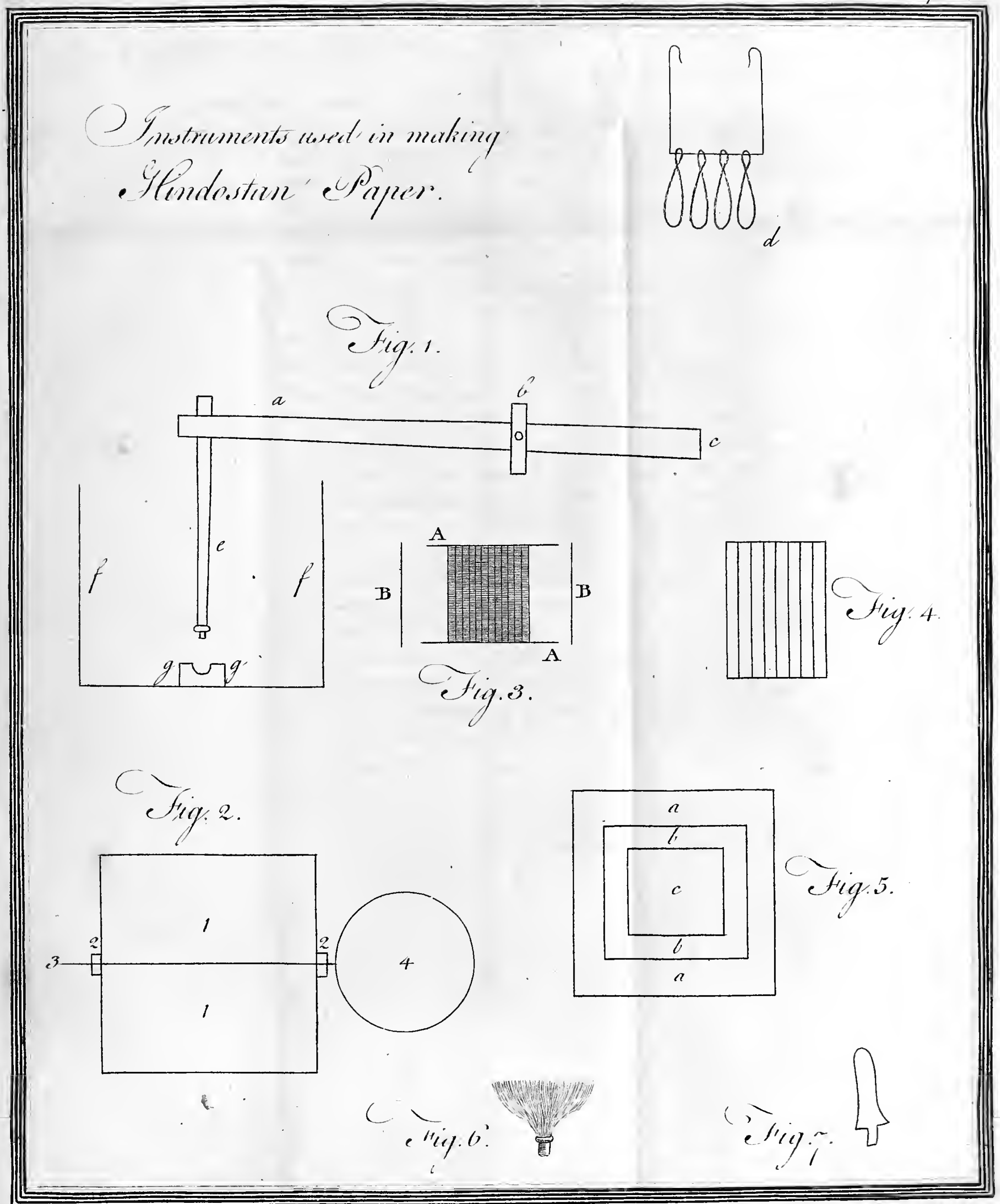
Fig. 5th. *a a.* Is a terrafs, 4 or 5 feet fquare, inclined a few inches, that water may readily run from it.

b b. A mat or board laid upon the terrafs.

c. The new formed fheet of paper laid upon the mat.

Fig. 6th. A flat hair brufh for fpreading the wet paper upon the walls of the houfe.

Fig. 7th. The double edged knife with which the paper is cut into a proper form.


Instruments used in making Hindostan Paper.
d
Fig. 1.
a
b
c
e
f
f
g
g
A
B
B
A
Fig. 3.
Fig. 4.
Fig. 2.
1
1
2
2
3
4
a
b
c
b
a
Fig. 5.
Fig. 6.
Fig. 7.

XI. *An Improvement propoſed in the croſs Wires of Teleſcopes, by Dr.* Wilſon, *of* Glaſgow. *In a Letter to the Aſtronomer Royal.*

College, Glaſgow, Oct. 25, 1773.

S I R,

Redde, Dec. 23, 1773.

I Have lately put in practice a method of improving the croſs wires, that are made uſe of in aſtronomical teleſcopes, which has pleaſed me ſo much, that I propoſe, in this letter, to give you an account of it, knowing that you are always warmly intereſted in the ſucceſs of any thing of this nature.

It has been hitherto a deſideratum to draw ſilver wire fine enough for aſtronomical uſes. I need not mention the inconveniences, attending our being limited in this particular, as they will, of their own accord, occur to you. I therefore proceed to deſcribe the means, I have fallen upon, of obviating the difficulty. This, in practice, is extremely ſimple, and conſiſts in nothing but in flattening, the fineſt wires, which are now drawn. I have made the experiment, upon ſilver wire, which is marked 500 to the inch. Having prepared a ſmall block of ſteel, the face of which was made very flat and ſmooth, a number of the wires were ſtretched acroſs it, at conſiderable intervals, by having their ends faſtened, by pitch, at each ſide of the block. This done, I took another block of ſteel, of the ſame ſize, the face of which had been made likewiſe flat, and the top of it rounded, the better to determine the ſtroke of the hammer; upon applying this, over the wires lying upon the firſt block, which was firmly fixed in a vice, and giving a ſmart ſtroke with a hammer of about five pound weight, I found all of them flattened in a very even manner.

That I might have no difficulty of fitting theſe wires, ſo flattened, into the teleſcope, I purpoſely made the face of the ſteel blocks a ſmall matter narrower, than the width of the braſs ring, in our tranſit inſtrument, upon which the croſs wires are fixed. By this means the wires retained their roundneſs at both ends, and ſo were eaſily fixed acroſs the ring, by the ſcrew-pins, when their fine edges regarded the eye. By means alſo of a ſimple contrivance, which will readily occur in practice, I made the horizontal wire to go acroſs the others, ſo as juſt to touch them. This horizontal wire was a round one, of 500 to the inch, which I purpoſely uſed along with the others, that I might form ſome judgment of the effects of flattened ones, when viewed along with it in the field. I accordingly found a very ſtriking diminution of the viſible ſubtenſe of theſe wires, when compared with the round one; and this ſo conſiderable confiderable, as I am perfuaded could not be obtained with round wires, unlefs they could be drawn to two or three thoufand to the inch.

From what I perceived, in the experiment of flattening the wires, I do not fee but that this diminution, if it were requifite, might be carried a great deal further.

I am, &c.

ALEXANDER WILSON.

XII. *The Case of a Patient voiding Stones through a fistulous Sore in the Loins, without any concomitant Discharge of Urine by the same Passage: In a Letter to Dr.* Maty, *from Mr.* S. F. Simmons.

Redde, Dec. 23, 1774.

I Have taken the liberty of addressing this letter to you, because, from your situation as Secretary to the Royal Society, and the reputation, you have acquired, in the literary world, I have no doubt of your being very ready to receive it favourably. If you think it sufficiently interesting, you will be pleased to communicate it to the Royal Society; but if you think, that it has neither enough of novelty, or utility, to merit a place in their Transactions, I beg the favour of you, to let it rest with you.

ELEANOR PILCHER, the subject of it, is about 52 years of age, and lives at Littlebourn in Kent. About twenty-five years ago she first began to complain of pain in her back, of a difficulty in making water, and of other nephritic symptoms, which gradually increased. Soon after this she began to void gravel with her urine, and to pass several very small stones; and these symptoms continued to return very frequently, and with much

much ſeverity. About ten years after the firſt appearance of theſe complaints, a ſwelling came on in the left lumbar region, which, after having been very painful, for a conſiderable time, ſuppurated. This wound, which very ſoon became fiſtulous, has continued open ever ſince, and has conſtantly afforded an ichorous diſcharge. It was not till December 1772, fifteen years from the appearance of the tumour, that this diſcharge began to abate, and that the wound, from being perfectly eaſy, became painful and inflamed. During all this time the nephritic ſymptoms had continued to return, without any variation, the urine had conſtantly afforded a gravelly ſediment, and ſeveral ſmall ſtones had paſſed through the *meatus urinarius*; but theſe concretions were now about to take a different courſe. The pain in the back, which had commonly affected the left ſide, became much more intenſe than uſual, but was not attended by any of the other ſymptoms, which had been the uſual forerunners of a fit of the gravel. The diſcharge, from the wound, was ſuddenly diminiſhed, and the pain and inflammation exceedingly increaſed, though the urine continued to paſs in a healthy quantity, and without difficulty. Theſe complaints continued, during eight days, and then a round and ſmooth *calculus*, weighing about twelve grains, was extracted, with ſome difficulty, from the wound. Since that time no gravel has been voided with the urine, though no urine ever paſſes through the wound; and ſix other paroxyſms, like that I have deſcribed, have taken place, in which the ſame ſymptoms have occurred, and

and which have terminated in a ſimilar manner, ſo that ſeven *calculi* have paſſed through the wound, only two of which have been preſerved, and the leaſt of them weighs ſix grains. During the intervals of theſe paroxyſms, the patient enjoys a ſtate of eaſe and health; and the orifice, of the wound, ſoon after the excluſion of a *calculus*, returns to its uſual ſize, admitting with difficulty a common probe. This caſe, of which I have endeavoured to give you an accurate hiſtory, appears to be a great proof of the powers of nature. The right kidney does not ſeem to be affected, and as no urine ever paſſes through the wound, it ſhould ſeem, as if the ſecretion, by the left kidney, is deſtroyed; for, as no gravel is now voided with the urine, the left *ureter* is probably cloſed. The caſe however, though a very intereſting one, is not perfectly ſingular, for DELECHAMPIUS relates, that he ſaw a man who paſſed ſeveral ſtones, through an abſceſs of the loins, that had become fiſtulous. And TULPIUS, in the fourth book of his *Obſervationes Medicæ*, gives the hiſtory of a patient, who after undergoing much pain, from a nephritic complaint which he inherited from his father, at length paſſed a ſtone, from the kidneys, externally through the loins, which occaſioned a callous ulcer, through which *pus* and urine were perpetually flowing. Neither time, or any of the remedies employed, afforded him any relief, but, the paſſage through the loins cloſing, and the matter taking a different courſe, an acute fever was at length brought on, of which the patient died. And the late Mr. CHESELDEN obſerves, that he had three patients, from

from whom he had extracted ſmall ſtones, which had made their way from the kidnies to the integuments, and there occaſioned an impoſthumation. But caſes like theſe, though not perfectly new, ſeem to deſerve to be recorded, as very rare ones, eſpecially when they afford more intereſting circumſtances, than ſeem hitherto to have occurred.

You will do me much honour, by acknowledging the receipt of this letter. I ſhall be very ready, to give any neceſſary information, you may wiſh to receive, on the ſubject. And am very reſpectfully,

SIR,

Your moſt obedient humble Servant,

Wingham in Kent,
Nov. 7, 1773.

SAMUEL FOART SIMMONS.

XIII. *The*

XIII. *The Disparition of* Saturn's *Ring, observed by* Joseph Varelaz, *Lieutenant of the Royal Navy of the King of* Spain, *and Professor of Mathematics, in the Academy of* Guard-Marine *at* Cadiz. *In a Letter to Dr.* Morton.

Translated from the Spanish.

Cadiz, October 12, 1773.

S I R,

Redde, Dec. 23, 1773.

IT has been my luck to observe the celebrated phænomenon of *the round phase of Saturn*, which was so much recommended to Astronomers, in the Gazette of France of July 23. From the 24th of September to the 4th of October, I saw, clearly and distinctly the two *ansæ* of the ring; but with this particular circumstance, that the occidental *ansa* appeared more strongly illu-

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Cadiz, 12 de Octubre de 1773.

Muy Senor mio,

ACABO de observar el celebre fenomeno de la phase redonda de Saturno, que tanto se recomendó á los Astronomos en la Gazeta de Francia de 23 de Julio de este ano. Desde el dia 24 de Septiembre hasta el 4 de Octubre he visto clara y distintamente las dos asas del anillo, aunque con la particularidad de que el asa occidental parecia mas bien iluminada que illuminated than the oriental. The atmoſphere was thicker on the 5th, and I could only ſee the occidental. The 6th, I thought I could diſcern ſome faint remains of the ring; but that might be a deception of my ſight, becauſe the atmoſphere remained very thick, and the planet could not be ſeen well terminated. On the 7th, the atmoſphere being more tranſparent, and the heavens clearer than I have ever ſeen them, I obſerved the total diſparition of the ring; and, having repeated the ſame obſervation the following day, I was convinced that this famous phænomenon took place the 6th day of the month, in which determination I have all the exactneſs, which can be expected in obſervations of this kind. The moſt ſtriking circumſtances of this phænomenon were the following; 1. The occidental *anſa* conſtantly appeared more bright than the oriental. 2. On the diſc of the planet, one could clearly diſtinguiſh the line of the ſhadow projected from the

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que la oriental. El dia 5 eſtaba la atmosfera algo denſa, y ſolo pude diſtinguir el aſa occidental. El dia 6 me pareció haver percivido algun viſlumbre del anillo, pero tal vez ſeria una iluſion de mi viſta, porque eſtaba la atmosfera ſumamente denſa, y no ſe podia ver el planeta bien terminado. El dia 7, con la atmosfera mui limpia y el cielo mas claro que he viſto jamas, obſervé la total deſaparicion del anillo, y haviendo repetido la miſma obſervacion al dia ſiguiente, me aſeguré en que eſta celebre phaſe ſe havia verificado el dia 6 de eſte mes, en cuya determinacion hay toda aqueſta exactitud de que ſon ſuſceptibles eſtas obſervaciones. Las particularidades mas notables de eſte fenomeno han ſido las ſiguientes. 1. El aſa occidental parecia ſiempre mas bien iluminada que la oriental. 2. En el diſco del planeta ſe diſtinguia claramente la linea de la ſombra que proyecta the thicknefs of the ring. 3. On the extremities of this, fome luminous points were perceived, which reflected the light more ftrongly than the others. 4. I did not obferve a fenfible variation in the apparent diameter of the ring.

I made ufe, for this obfervation, of a five-feet reflector of Mr. Short, of another of four of Mr. Nairne, and of an excellent acromatic telefcope of Mr. Dollond, to which I applied one of the ftrongeft and brighteft magnifiers.

I affure you, Sir, that I have given to this obfervation all the attention, which the importance of the matter required; and this joined to three others, form a fet of obfervations on the planets, the occultations of fixed ftars by the moon, and the Satellites of Jupiter. I fhall fend you a copy of the whole, if you think proper to prefent them to the Royal Society, and will accept of my correfpondence.

Don

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yecta el efpefor del anillo. 3. En los extremos de efte fe percibian unos puntos luminofos que reflectaban la luz con mas viveza que los otros. 4. En el diametro aparente del anillo no hubo una variacion fenfible. Me fervì para efta obfervacion de un telefcopio de 5 pies de Mr. Short, de otro de 4 de Mr. Nairne, y de un excelente anteojo acromatico de Mr. Dollond, al qual apliquè uno delos tubos mas fuertes y claros.

Afeguro a vm. haver hecho efta obfervacion con toda la prolixidad, que pide la importancia de la materia, y con la mifma tre formado un Quaderno de obfervaciones fobre los planetas, ocultaciones de eftrellas fixas per la Luna, y Satelites de Jupiter. De todo embiarè à vm. una copia, fi guftare prefentarla à la Sociedad Real, y admitir mi correfpondencia.

Dn.

Don Vincent Tofino Captain of a frigate in the King's fleet, and Director of the Academy of the Guarde-marines, has had a great ſhare in theſe obſervations. He is no leſs deſirous, than myſelf, that they ſhould be communicated to the Royal Society: for which purpoſe, I requeſt an anſwer from you, which you will pleaſe to addreſs to,

SIR,

Your moſt humble obedient ſervant,

JOSEPH VARELAZ,

Lieutenant in the Royal Navy, and Profeſſor of Mathematicks in the Academy of Guarde Marines.

Dn. Vincente Tofinò, Capitan de Fragata de la Rl. Armada, y Director de la Academia de Guardias-Marinas, ha tenido mucha parte en eſtas obſervaciones. Deſea como yo comunicarlas à la Sociedad Real, à cuyo fin ſuplico à vm. que me reſponda, y que me mande como à

Su mas rendido y obediente ſervidor,

JOSEF VARELAZ,

Teniente de Navio de la Real Armada y Profeſor de Mathemathicas en la Academia de Guardias Marinas.

XIV. *Of the Gillaroo Trout. A Letter from the Hon.* Daines Barrington, *to the Rev. Dr.* Horſley.

DEAR SIR,

Redde, Dec. 23, 1773.

YOU will find upon the table a Gillaroo trout, as it is termed in Ireland, the peculiarity of which is, that the ſtomach very much reſembles the gizzard of a bird.

The firſt time I ever happened to hear of theſe ſingular fiſh, was from an Iriſh Judge, who being on the Connaught circuit at Ballynrobe, in the county of Mayo, expreſſed his incredulity with regard to their exiſtence, but was obliged to pay the common Iriſh wager, of a rump of beef and a dozen of claret, on three or four being produced the next day from a neighbouring lake.

Since that time I have endeavoured to procure a ſpecimen, with the entrails adhering, and have at laſt ſucceeded, the ſtomach upon the table having been extracted by Mr. HUNTER, F. R. S. in the preſence of ſeveral.

I do not find that any ichthyologiſt takes notice of ſuch a part belonging to fiſh, except GOUAN *, who ſays, that the ventricle, of ſome ſorts, reſembles the gizzard of fowls, by being partly fleſhy and partly membranous: GOUAN, however, does

* Philoſophia Ichthyologica, p. 80.

not

not ſpecify the ſpecies of fiſh, which hath ſuch a ſtomach.

No mention is made by any naturaliſt of this *variety* in the trout; nor is it noticed in the collection of tracts for illuſtrating the natural hiſtory of Ireland, which was publiſhed in 1727, though their lakes and rivers are particularly deſcribed.

If the ſpecimen, now ſhewn to the Society, was a ſingle one, it might be conſidered as an accident; but there can be no doubt, of trouts being conſtantly caught, with this extraordinary ſtomach, in ſome of the Iriſh lakes, though the greater part indeed do not differ from the common ſorts: I have therefore been informed by Lord Louth, that he hath ſeen a ſmall diſh, conſiſting merely of ſuch gizzards at an Iriſh table in Galway, and I could corroborate this fact, was it neceſſary, by the teſtimony of an Iriſh Archbiſhop.

There are no exterior marks by which the ſpecimen on the table can be diſtinguiſhed from the common trout; but I have ſhewn the ſtomach to Everet of Clare-Market, a very intelligent fiſhmonger, who declares, that though he hath cut up thouſands of trouts and ſalmons, he never obſerved any thing ſimilar in the inſide.

Mr. HUNTER opened a charr, which is ſcarcely diſtinguiſhable from a trout in it's outward appearance, and found the *poke*, as our fiſhmongers call it, very different; you will find however the ſtomach of a common trout upon the table, which may be compared with that of the Gillaroo, though it is admitted to be a bad ſpecimen.

I have been informed that the term *Gillaroo* ſignifies a *gizzard* in the Iriſh language; but as I cannot find any ſuch word in Lhuyd's Iriſh Dictionary, poſſibly it may be only a corruption of Killaloe *, near which town ſuch trouts are caught.

The *poke* of the Gillaroo ſeems to perform the office of a gizzard, becauſe ſeveral ſmall ſnails were found within the preſent ſpecimen, and I conclude, that this ſpecies of food abounds in the lake, which this *variety* of trout frequents.

By the beſt information I can procure, they are more common in LOUGH CORRYB, and the lakes of GALWAY, than the other waters of Ireland: they are alſo caught in LOUGH DERN, through which the SHANNON runs: I incloſe however ſome memoranda relative to the Gillaroo trouts, which Mr. WALSH, F. R. S. was ſo good as to make for me, whilſt he was, laſt ſummer, in Ireland, ſome of which he received from an inn-keeper at KILLALOE, and the others from fiſhermen of LOUGH DERN.

If the particulars, I have ſtated, happen to prove intereſting to the Society, I hope they will do me the honour of giving the ſpecimen a place in their Muſeum. I am,

DEAR SIR,

Your moſt faithful humble Servant,

DAINES BARRINGTON.

* Though I do not pretend to underſtand either the Welſh or Iriſh languages, yet I know that there is a great affinity betwixt them, in the names for common things.

Now *cylla* ſignifies the *ſtomach* in Welſh (ſee Davis's Dictionary Artic. *Ventriculus*) and *ruadh* is rendered *ſtrong* by Lhuyd, in his Iriſh Vocabulary: Gillaroo therefore is the *ſtomach ſtrong*, as the adjective follows the ſubſtantive, according to the Welſh idiom, and probably therefore according to the Iriſh.

POST-

POSTSCRIPT.

SINCE I wrote the above, I have this morning procured the pokes of two ſalmon, one of which weighed 34 lb, and the other 24 pounds. You will obſerve in theſe ſomething ſimilar; but though both theſe fiſh exceed the Gillaroo trout ſo much in ſize, yet the gizzard part is ſmaller, and is at the ſame time leſs muſcular. The Gillaroo trout did not probably weigh more than four or five pounds.

MEMORANDA OF MR. WALSH, F. R. S.

Killaloe. October 1, 1773.

Innkeeper's Account.

The Gillaroo or Gizzard trout.

The gizzard of the ſize of a large chicken: it is white, and excellent eating; vaſtly broader than a trout of the ſame length; ſome of them three feet long; ſome from twelve to eighteen; the trout itſelf is bad eating.

They catch many trout, and but few Gillaroos. They have the fineſt trout in the world.

Lough

Lough Derg. October 2, 1773.

Fiſhermen's Account.

From May to Auguſt, trout of all kind is the moſt plenty.

The largeſt Gillaroo is 12 ½ pound; the ſmalleſt, 2 pound. There is a red Gillaroo and a white; the laſt is the ſmalleſt, and the better eating. It is white, with black ſpots on it; the red Gillaroo is red, with black ſpots on it.

The large Gillaroo have gizzards bigger than a turkey gizzard; they have gravel and little ſhells in them. Never ſaw a roe, which is here called a pea, in the Gillaroo, nor a milt. They ſeldom take a Gillaroo at the ſpawning ſeaſon. They are quite different from the other trout; their ſpots are larger, and fewer.

A very large ſpecies of trout, of 20 and 30 pounds weight, called Budhurs, that is, *big*, reſemble the red Gillaroo outwardly, but have no gizzards, nor are they broad. Are ſeldom killed till the latter end of the ſeaſon; that is, from this time to Chriſtmas.

XV. *Account of the Stomach of the Gillaroo Trout. In a Letter from Mr.* Henry Watſon *to Sir* John Pringle.

SIR,

Redde, Jan. 27, 1774.

THE requeſt, you was pleaſed to make to me, in the name of the Royal Society, that, at my leiſure, I would examine into the ſtructure of the ſtomach of the Gillaroo trout, I ſhall now endeavour to comply with, in the beſt manner I am able.

The ſtate, in which I received this ſtomach, did not admit of the moſt accurate examination; and the ſtomach of the common trout, ſent to me at the ſame time, was ſo hardened, and ſhrivelled up, I could by no means unfold it, or draw any compariſon between them.

The ſtomach of the Gillaroo trout may be divided, at leaſt, into three coats;—an internal, a middle, and an external one.

The external is a kind of peritonæal covering, common to the ſtomach, inteſtines and other *viſcera*.

The middle coat appears to be of a fibrous muſcular texture, pretty thick in fleſh, ſtronger than in the ſalmon; and of a yellowiſh colour.

 The

The internal coat, has a rough, but not rugous ſurface. It is ſpongy and perhaps glandular, with a kind of honey-comb texture, and ſtrong *villi*, a little ſimilar to the internal appearance of the gall-bladder in the human ſubject; and no doubt would make an elegant figure, was it well injected. This, SIR, is the moſt, that I am at preſent able to obſerve upon the ſtructure of this *viſcus*.

But, as the ſtomach of the Gillaroo trout is ſuppoſed to perform the office of a gizzard, it may be neceſſary to examine a little, how far they agree.

The gizzard, in birds of the gallinaceous kind, is compoſed of large maſſes of fleſh and tendon; between which lies the ſtomach, a ſtrong, denſe, cartilaginous, or horny bag, furniſhed with eminent *rugæ*, and deep furrows; but we have none of theſe appearances in the ſtomach of the Gillaroo trout; it does not ſhew the leaſt reſemblance, nor can I think it will bear any compariſon, with the gizzard in fowls; nor is it at all concluſive, that the ſtomach of this trout performs the office of a gizzard, becauſe ſeveral ſmall ſnails were found within it; for might we not as well conclude, that the *cæca* annexed to the *duodenum*, and which are, in this fiſh, very numerous, and loaded with the ſame little ſhells, do the offices of ſo many gizzards? But we know that theſe *cæca* are glandular pouches, and do the office of a *pancreas*.

If the ſtomach of the Gillaroo trout does not appear, to correſpond with the ſtructure of the gizzard in fowls, neither does it agree, in every reſpect, with

with the appearances commonly obſerved in fiſhes. It is indeed membranous and muſcular, but its internal ſurface is not ſo ſmooth, white, and poliſhed; nor is it made up into large folds, or plaits, as we commonly find it in moſt fiſhes; in ſome indeed, more reſembling a gut, than a ſtomach: but the internal ſurface, in the ſtomach of the Gillaroo trout, is rough, and over-ſpread with *villi*, which are ſo ſtrong, they may be eaſily ſeen with the naked eye, and which I have never obſerved in the ſtomach of any other fiſh.

Upon the whole, we may venture to conclude, that there are ſome ſingularities, in the ſtructure of the ſtomach, which may perhaps be peculiar to the Gillaroo trout.

If, in the beſt manner I have been able to prepare this ſtomach, you ſhould think it worth preſerving, I hope it may poſſeſs a corner in the Muſeum of the Royal Society. I am,

SIR,

with great reſpect,

Your moſt obedient,

humble ſervant,

Rathbone-place,
16 Jan. 1774.

HENRY WATSON.

XVI. *A Description of a petrified Stratum, formed from the Waters of* Matlock, *in* Derbyshire. *By* Mathew Dobson, *M. D. Communicated by Dr.* Fothergill.

Redde, Jan. 13, 1774.

DURING a short stay at MATLOCK, this summer, I made some observations on the petrifying quality of the waters, and examined a very singular *Stratum*, which has been formed in their course.

This *Stratum* I found about 500 yards in length; in several places, near 100 yards in breadth; and, where thickest, from 3 to 4 yards in depth. The manner, in which this body of stone has been produced, is easily ascertained.

Within the memory of some persons now alive, the waters of Matlock were not appropriated to the purposes either of bathing or drinking. They issued from near the bottom of the hill, which lies to the west, immediately behind the present houses, and ran, at random, down a declivity of about 100 yards, to the river DERWENT. In their course, they formed large petrified masses, intermingled with great quantities of petrified moss, nuts, leaves, acorns, pieces of wood, and even trunks of trees.

The waters were thus conftantly raifing obftacles to their own progrefs, and were frequently therefore forced into new channels; fo as, by degrees, to be extended over a furface of at leaft 500 yards in length. And, by being repeatedly returned into the fame channels, a *Stratum*, of confiderable thicknefs, has been formed.

On examining this *Stratum*, fome parts are difcovered to be extremely hard, and others fo foft, as eafily to be cut. The foft parts, however, on expofure to the air, become as hard as flint; and on being ftruck, found like metal. The reafon, of this difference in the hardnefs of different parts, appears to be this: as the waters frequently changed their channels, and repeatedly likewife returned again to the fame channels, if, in the intervals, there were any parts confiderably raifed, and confequently longer before they were covered with frefh incruftations, thefe, from a longer expofure to the air, would acquire a greater degree of hardnefs.

Whole houfes, in the neighbourhood, are built of this ftone, which they find more durable, than any other they meet with; and as it has the excellent property of growing harder, from being expofed, and has likewife many little cavities and interftices, good mortar fo infinuates itfelf into thefe, as to form a wall as firm as one continued ftone.

This *Stratum* affords very curious and beautifully varied petrifactions. Mofs exhibits great varieties; for it is evident, that the mofs has continued to vegetate, after the roots and lower parts

had

had been penetrated by the ſtony particles; and thus, ſtretching itſelf to a conſiderable extent, it has, in ſome places, been mixed and interwoven with other ſubſtances. In ſome parts, ſnails have been arreſted in their ſluggiſh walks, and locked up in the ſtony concrete. In others, the petrifying matter has ſhot, in different directions, and formed an intricate kind of net-work. And in others again, there are large maſſes, which, on being broken aſunder, are found hollow; and their cavities ornamented with branches of petrifaction, ſomewhat reſembling coral, but of a darkiſh-white colour, and generally of a rough and granulated ſurface.

Under the *Stratum* there is, from a foot to a foot and a half, of good ſoil; and immediately under this lies the limeſtone rock. The ſoil is of the ſame nature with that of the adjoining fields, which form the ſlope of the hill, and is evidently a continuation of that ſoil.

Any further additions, to this petrified *Stratum* are now inconſiderable, and in many places none at all; for the two principal ſprings are confined to their channels, covered from the day, through the greateſt part of their courſe, and are rapid in their motion.

Had proper obſervations been made on the progreſs of this *Stratum*, a tolerably exact eſtimate might have been formed, with reſpect to the time, when theſe waters were firſt impregnated with their mineral ingredients. From theſe two conſiderations, however, that the *Stratum* is not very thick, and that the foil, immediately under it, is a

continu-

continuation of that, which lies on the ſlope of the neighbouring hills, it is probable, that many centuries have not been requiſite to its production; and, conſequently, that theſe mineral waters are not of very antient date.

And, if we may rely upon an obſervation, which I had from a plain, inquiſitive, and intelligent man, on the ſpot, the ſource, whence theſe waters derive their impregnation, is in ſome degree exhauſted. This perſon aſſured me, from his own experience, that pieces of moſs, and other ſubſtances, put in the courſe of the waters, and in the ſame circumſtances as formerly, require more than double the time, for their petrifaction, that they did thirty years ago.

The *Stratum*, therefore, from which the Matlock waters are impregnated, muſt either be conſiderably exhauſted; or the waters have deviated from their former courſe, and are now only partially diſtributed over this *Stratum*.

Leverpool, Oct. 15, 1773.

XVII. *Remarks on the Aurora Borealis. By Mr.* Winn. *In a Letter to Dr.* Franklin.

Spithead, Auguſt 12, 1772.

SIR,

Redde, Jan. 20, 1774.

I Have often wiſhed, that ſomebody would carefully collate a ſufficient number of meteorological journals, with intent to obſerve and claſs the ſeveral appearances in the atmoſphere, before great changes in the weather, particularly before great ſtorms. I am perſuaded, from my own obſervation, that, in general, ſufficient indications, of impending tempeſts preceed them a conſiderable time, did we but carefully note them. The phænomenon, which I am going to mention, is one of thoſe indications which not only portend an approaching tempeſt, but aſcertain from what quarter it will come; a circumſtance, that may render it of eſſential ſervice to ſeamen. I believe the obſervation is new, that the *Aurora Borealis* is conſtantly ſucceeded by hard ſoutherly, or ſouth-weſt winds, attended with hazy weather, and ſmall rain. I think, I am warranted from experience, to ſay conſtantly; for in twenty-three inſtances, that have occurred, ſince I firſt made the obſer-

obſervation, it has invariably obtained. However, I beg leave to requeſt, you will recommend it to the notice of the Royal Society, as a matter, which, when confirmed by further obſervations, and generally known, may be of more conſequence than at firſt appears. To ſhew that it may, give me leave to recite the circumſtance, which firſt occaſioned my taking notice of it. Sailing down the Engliſh channel in 1769, a few days before the autumnal equinox, we had a remarkably bright and vivid *Aurora* the whole night. In ſhore, the wind was fluctuating, between N.N.W. and N.W. and farther out, W.N.W. Deſirous of benefiting by the land wind, and alſo of taking advantage of an earlier ebb-tide, I diſpenſed with the good old marine adage, *never to approach too near a weather-ſhore, leſt it ſhould prove a lee-ſhore*, and, by ſhort tacks, clung cloſe along the Engliſh coaſt. Next day, the wind veered to the S.W. and ſoon after to S.S.W. and ſometimes S. We were then in that dangerous bay between Portland and the Start Point, and carried a preſſing ſail, with hopes of reaching Torbay before dark; but night fell upon us, with thick haze, and ſmall rain, in ſo much, that we could not have ſeen the land at the diſtance of a ſhip's length. The gale was now increaſed to a ſtorm; in this dilemma, nothing remained but to endeavour to keep off the ſhore, till the wind ſhould change. Luckily our ſhip was a ſtout one, and well rigged.

Reflecting ſome time after, on the circumſtances of this ſtorm, and the phænomena that preceded it, I determined to have particular attention to future *Auroræ*, and the weather, that ſhould ſucceed them; and, as I have obſerved above, in twenty-three inſtances, have found them uniform, except in degree: the gale generally commencing between twenty-four and thirty hours after the firſt appearance of the *Aurora*. More time and obſervation will probably diſcover, whether the ſtrength, of the ſucceeding gale, is proportionate to the ſplendor and vivacity of the *Aurora*, and the diſtance of time between them. I only ſuſpect, that the more brilliant and active the firſt is, the ſooner will the later occur, be more violent, but of ſhorter duration, than when the light is languid and dull. Perhaps too, the colour of the *Aurora* may be ſome guide, in forming a judgement of the coming gale. That which preceeded the ſtorm I have mentioned, was exceedingly ſplendid. The tempeſt ſucceeded it in leſs than twenty-four hours, was violent, but of ſhort (about eight hours) continuance. In June laſt, a little without ſoundings, we had for two nights following, faint inactive *Auroræ*; the conſequent gale was not hard, but laſted near three days: the firſt day attended with haze, and ſmall rain; the ſecond with haze only, and the laſt day clear.

The benefit which this obſervation, on the *Aurora Borealis*, when further confirmed and known, may be of to ſeamen, is obvious, in navigating near coaſts, which tend eaſt and weſt, particularly in the Britiſh channel. They may, when warned by the *Aurora Borealis*, get into port, and evade the impending ſtorm; or, by ſtretching over to the ſouthward, facilitate their paſſage, by that very ſtorm, ftorm, which might have deftroyed them; for no winds are fo dangerous, in the channel, as foutherly and fouth-weft. In a word, fince I have made this obfervation, I have got out of the channel, when other men, as alert, and in fafter failing fhips, but unapprized of this circumftance, have not only been driven back, but with difficulty have efcaped fhipwreck.

Perhaps, the obfervation, that foutherly gales conftantly fucceed thefe phænomena, may help to account for the nature of the *Aurora Borealis*; my own thoughts on that fubject, I fhall fome time beg leave to lay before you.

I am, with great refpect,

SIR,

Your obliged,

humble fervant,

J. S. WINN.

A further Note from the ſame Gentleman.

In Auguſt laſt, Mr. WINN ſent Dr. FRANKLIN ſome obſervations on the *Aurora Borealis*, to which he would add, that, on Saturday evening, the 16th inſtant, as Mr. Winn came to town, the *Aurora* was ſo bright, that he found a croud of people in the Minories, gazing at it, which they took to be the effect of a great fire about Biſhopſgate-ſtreet; the next day we had a hard gale at S. S. W. with rain.

Friday Morn. 22 Jan.

Conjecture on the foregoing.

The *Auroræ Boreales*, though viſible almoſt every night of clear weather, in the more northern regions, and very high in the atmoſphere, can ſcarce be viſible in England, but when the atmoſphere is pretty clear of clouds, for the whole ſpace between us and thoſe regions, and therefore are ſeldom viſible here. This extenſive clearneſs may have been produced by a long continuance of northerly winds. When the winds have long continued in one quarter, the return is often violent. Allowing the fact ſo repeatedly obſerved by Mr. Winn, perhaps this may account for the violence of the ſoutherly winds, that ſoon follow the appearance of the *Aurora* on our coaſts.

B. F.

XVIII. *Ex-*

XVIII. *Experiments concerning the different Efficacy of pointed and blunted Rods, in ſecuring Buildings againſt the Stroke of Lightning.* *By* William Henley, *F. R. S.*

Redde, Dec. 16, 1773.

FROM an accident which lately happened to the chapel in Tottenham-court road, where a poor man was killed, an account of which I had the honour of preſenting to the Royal Society, the Gentlemen, who have the care of that building, were deſirous of erecting a proper conductor, to prevent ſuch accidents in future; which was done accordingly under my direction, except three points at the top, to which I rather incline to prefer a ſingle one. On this occaſion, I was willing to obtain the beſt information I could, upon the queſtion, *whether the preference be due to points or knobs, for the termination of conductors*; for which purpoſe, I made the following experiments.

EXPERIMENT I.

I placed two of Mr. CANTON's electrometers, A. and B. [TAB. VIII. Fig. 1ſt.] inſulated, upon ſtands

 of

of ſealing-wax, about ſeven inches aſunder, and as many from the end of a prime conductor, which was eighteen inches long, and one inch and an half diameter; and had a ball, at each end, two inches and a half diameter: the diameter of the electrical globe being nine inches. On the top of the box A, I placed a wire, projecting three inches from the end of it, and terminated by a ball three quarters of an inch in diameter. On the top of the box B, I placed a ſharp pointed wire, projecting alſo three inches from its end. The knob and point were now exactly at the ſame diſtance, namely, ſeven inches, from the end of the conductor. Then, giving the winch five or ſix turns, the light cork balls, hanging from the box A, were repelled to the diſtance of one inch from each other; but thoſe hanging from the box B, ſeparated full two inches. Then touching the prime conductor with a finger, the balls at A cloſed, while thoſe at B remained a full inch aſunder. From this experiment, I think it ſeems evident, how much better adapted a ſharp point is, to draw off lightning, than a knob of three quarters of an inch in diameter; and, conſequently, how much more likely to cauſe it to paſs in that conductor, to which it is affixed, rather than in any other part of the building, where it might occaſion much damage, as well as endanger the lives of thoſe, who might happen to be in it. The following experiments ſeem to make ſtill more ſtrongly in favour of the ſame concluſion.

EXPERIMENT II.

I affixed, to the top of a glafs-ftand, a wire, three eighths of an inch in diameter, terminated, at one end, by a ball, three quarters of an inch in diameter; and, at the other end, by a very fharp point. fee Fig. 2. Round the middle of this wire, I hung a chain twelve inches long. I then charged a bottle containing one hundred fquare inches of coated furface, and, connecting the chain with the coating of the bottle, I brought the knob of it, very gently, towards the ball on the infulated wire, that I might obferve precifely, at what diftance it would be difcharged upon it; which I found to happen conftantly, at the diftance of half an inch, with a loud and full explofion. Then, re-charging the bottle, I brought the knob, in the fame gradual manner, towards the point of the infulated wire, to try alfo, at what diftance that would be ftruck; but this, in many trials, never happened at all. The point, being approached in this gradual manner, always drew off the charge imperceptibly, leaving fcarce a fpark in the bottle.

EXPERIMENT III.

I had now recourfe to the apparatus known, to electricians, by the name of the *thunder-houfe* which I thought a nearer refemblance of the operations of nature, on thefe occafions. Having connected a jar, containing five hundred and nine fquare inches of coated furface, with my prime

con-

conductor, see Fig. 3, I observed, that, if it was so charged, as to raise the index of my electrometer to sixty degrees, by bringing the ball, on the wire of the thunder-house, to half an inch distance from that connected with the prime conductor, the jar would be discharged, and the piece in the thunder-house thrown out to a considerable distance. Using a pointed wire, for a conductor to the thunder-house, instead of the knob, as in the former experiment, the charge being the same, the jar was discharged silently, though suddenly: and the piece was not thrown out of the thunder-house.

EXPERIMENT IV.

Having made a double circuit to the thunder-house, the first by the knob, the second by a sharp pointed wire, at one inch and a quarter distance from each other, but of exactly the same height, see Fig. 4. the charge being the same; although the knob was brought first under that connected with the prime conductor, which was raised half an inch above it, and followed by the point, at one and a quarter inch distance, yet no explosion could fall upon the knob; the point drew off all the charge silently; and the piece in the thunder-house remained unmoved.

EXPERIMENT V.

Having insulated the jar, and connected, by chains, with the external coating, on one side, a knob, and, on the other side, a sharp-pointed wire,

wire, both being insulated, and standing five inches from each other, see Fig. 5, I placed a large copper ball, C, eight inches in diameter (insulated also) so as to stand exactly at half an inch distance both from the knob and the point. The jar being fully charged, I delivered it upon the copper-ball by my discharging rod, whence it leaped to the knob A, which was three quarters of an inch in diameter, and the jar was discharged by a loud and full explosion, and the chain was very luminous.

These experiments were made in the presence of Mr. BELL, a very ingenious electrician, whose province it was to be particularly attentive to the phænomena, and he could perceive no light upon the chain, which connected the pointed wire B, with the coating of the jar.

EXPERIMENT VI.

I insulated my three largest jars, containing together about sixteen square feet of coated surface. From the bottom of these jars projected a wire, terminated by a ball, three quarters of an inch in diameter; and, at the distance of one inch and an half from it, I placed the insulated ball C. Fig. 6, on which I brought down the charge of the three jars, by my discharging rod; which leaped from thence to the ball in contact with the jars, and discharged them, by a loud and full explosion; but the same thing did not happen, if I removed the insulated ball only one eighth of an inch further from the other. I then removed the wire, which was terminated by the ball, from the bottom of the jars; and placed another in its ſtead, of the ſame length and diameter, but very nicely tapered to a point, as all mine are. Then, placing the inſulated ball, C, one inch from the point, I brought down the charge of the three jars, as before, which flew upon the point, and melted it a little. The jars were diſcharged with a loud and full exploſion. But having removed the ball C, to the diſtance of one inch and an eighth from the point, the charge could not ſtrike it; though much of it was preſently drawn off ſilently, by the point, as appeared by the falling of the index of the electrometer.

From this experiment, I think it ſeems ſomewhat more than probable, that a conductor terminated by a ball, of three quarters of an inch in diameter, would be in danger of a ſtroke, from an highly electrified cloud, at a much greater diſtance, than another with a ſharp termination. Indeed I cannot help remarking, how very improbable it appears to me, that a ſharp pointed conductor ſhould, at any time, invite, or ſolicit, a ſtroke of lightning. Imagine, if you pleaſe, that a large cloud is, by the force of the wind, driven violently towards ſuch a point, and actually ſtrikes upon it; yet, as the point would act *as ſuch*, at ſomewhat more than the ſtriking diſtance, it ſeems probable, that part of the electricity of the cloud would be drawn off ſilently, before the actual ſtroke could be made; and the ſtroke itſelf might thereby, perhaps, be a little leſſened.

I ſhall

I ſhall here inſert what ſeems to afford a ſufficient proof of the truth of this reaſoning.

Extract of a letter from Captain RICHARD NAIRNE, of the Generous Friends, dated Montreal, June 24, 1773, to Mr. THOMAS MARSHAM, in the Borough.

" I ſhall make every obſervation I can, for the " good of electricity, and the ſatisfaction of my " friend Mr. HENLEY. I put up a longer top- " gallant-maſt, the day I arrived at Quebeck. The " conductor, by this means, became too ſhort; " and my mate ſtill let it hang, without making " any addition to it. They had a ſevere thunder " ſtorm that night; but think how pleaſed I was " to find, that, from the wetneſs of the ſhip's " ſides, the electricity paſſed into the water, " without the leaſt injury to the ſhip; but the " ſpark on the point of the conductor, which was " very ſharp, was ſo lucid, that my people were " very much frightned."

Since I received this account of Mr. NAIRNE's obſervation, I have been favoured with the following remark, by my ingenious and worthy friend, Lieutenant Fairlamb, of the Artillery; who informs me, that the church of St. Michael, in CHARLES TOWN, South Carolina, uſed to be ſtruck and damaged by lightning, in every two or three years from its firſt erection; but in fourteen years, that it hath been furniſhed with a pointed conductor, it hath never been ſtruck at all. It appears alſo, that when a ſtroke of lightning fell upon a ſtable belonging to WILLIAM LYTTELTON Eſquire, Governor of South Carolina, and ſplit and threw down two of the raſtors; yet the dwelling houſe, at twenty yards diſtance, being provided with a conductor, terminated by a ſharp point, eſcaped unhurt. I would here alſo juſt remark, that nothing can be more ſharply pointed, than the weather-fane which terminates the conductor, * on one of the pinnacles on the tower of St. MICHAEL's church in CORNHILL, which conſiſts of two darts, with a ſtar, having many pointed radii between them; yet in the late thunder ſtorm, the lightning does not appear to have ſtruck this building; but fell upon the key, at the top of the ſpire of St. PETER's church, which is conſiderably lower than the fane of St. MICHAEL'S; and the diſtance of the two churches is not more than two hundred feet; as I have been informed by Mr. EDWARD NAIRNE who meaſured it. This key is terminated by a thick blunted end: the ſpire is covered with lead, from the key to the brick tower; and ſo far the lightning was conducted with ſafety to the building; nor could I obſerve, that there had been the leaſt fuſion upon the metal; ſome parts of it, however, I could not get at to examine; but having quitted the lead work, and entered the brick tower, it there did conſiderable damage, till it reached the leaded roof on the body of the church; whence it ſeems to have been conducted, by the pipes, which carry down the rain water, and which reach to the bottom of the building, without further damage.

* Erected by my ingenious friend MR. EDWARD NAIRNE.

Almoſt

Almoſt at the ſame inſtant that this ſpire was ſtruck, the lightning fell alſo upon a Dutch ſhip, in the river Thames, lying off the Tower, which had an iron ſpindle, terminated by a thick blunted end, at her maſt-head, and did her much damage. A very exact account of which hath been taken by Mr. BELL, and, at another opportunity, will be laid before the members of this Society.

The lightning ſtruck alſo upon the pillar, commonly called the OBELISK, in the croſs road in St. George's-fields, Southwark; a very curious obſervation of which was made, ſoon after the ſtroke, by Mr. Coventry, and Mr. Thomas Green, well known to many in the Royal Society, for his ſingular and very curious method of preſerving the ſubjects of natural hiſtory. It likewiſe ſtruck the chimney of the new Bridewell there, which it threw down to the ridge of that building, which was covered with lead; and then diſperſed itſelf with little damage. The lightning fell likewiſe upon another chimney, at LAMBETH; and upon a houſe at the Phyſick-garden, near VAUXHALL; and, as before obſerved, it appears by the beſt information, nearly at the ſame time; and in many other places, conſiderably diſtant from each other.

I have obſerved, on another occaſion, that if a round ball of metal, two inches in diameter, was preſented towards the large prime conductor to a good cylinder, at the diſtance of two inches, it would continue to receive ſuch ſtrong ſparks, as would give the perſon who held it, a ſenſible ſhock in both his legs; but that if the point of a lancet, or a wire ſix inches long, nicely tapered to a point, and tipped with ſteel, were, at the ſame time,

time, held towards the conductor, at the distance of two feet, the point would draw off all the electricity of it silently, and not suffer a spark to pass from thence to the ball; and from this experiment I inferred, that a sharp point might probably, in some measure, produce the same effect on a cloud highly charged with electricity, * and thus perhaps contribute to lessen a little, if not actually prevent, a stroke. I also observed, that if the point of the wire, or lancet, was brought nearly into contact with the prime conductor; yet no sensation would be felt in the hand of the operator; and this, I imagined, was a kind of demonstration, that there could be no danger of inviting a stroke of lightning from a cloud, by a sharp pointed conductor; as it could make no difference in the experiment, whether the point moved towards the large prime conductor, or the conductor moved towards the point. It having, however, been objected to this experiment, that it was not analogous to the effect of nature operating by a cloud; forasmuch as the cloud being a loose and floating body, it might accede to, and strike upon the point with its contents; which the conductor, being a fixed body, was incapable of doing, I made the following experiment.

EXPERIMENT VII.

I procured, by means of my ingenious friend Mr. Coventry, a bullock's bladder, of the largest size; which bladder Mr. Coventry gilded for me with leaf copper, and suspended it, by a silken

* Or rather on the electric atmosphere, surrounding the cloud.

ſtring, at one end of an arm of wood, placed horizontally, and turning freely upon the point of a needle; the needle being ſtuck upright in another piece of wood, inſerted in a firm baſe, and ſtanding in a perpendicular direction to the floor. The bladder was balanced by a leaden weight, at the other end of the wooden arm, ſee Fig. 7. The apparatus being thus adjuſted, I gave the bladder a ſtrong ſpark, from the knob of a charged bottle; when, preſenting towards it a braſs rod, terminated by a ball, two inches in diameter, I obſerved, that the bladder would come towards it, at the diſtance of three inches; it would even come back to it, when ſwinging in a contrary direction; and when it had got within one inch of it, it would throw off its electricity in a full and ſtrong ſpark: the bladder gave the ſpark nearly, if not quite, as large as it received it. I then gave it another ſtrong ſpark, as before, when, preſenting towards it the pointed wire above-mentioned, I could never perceive that it acceded to that; and when it was brought nearly into contact with the bladder, there was no ſpark at all, ſcarce any ſenſible quantity of electricity remaining in it. I repeated the experiments many times, with the utmoſt care and accuracy I was able; and always with the very ſame reſult.

To the obſervations I have now made, upon the different effects of ſharp pointed wires, or thoſe terminated by blunted ends, or round balls, in electrical experiments, I ſhall add another, with which I have lately been favoured by THOMAS RONAYNE Eſquire, whoſe permiſſion I have to inſert it in this paper.

Having

Having cut off a few inches of ſmall harpſichord-wire, he connected one end of it with the the loop, at the bottom of his electrical battery, conſiſting of nine bottles; the other end of the wire he faſtened to his diſcharging rod, which was terminated by a large round ball. Upon this ball he took the charge of his battery, when the whole of the ſmall wire was inſtantly melted. He then cut off another piece, of equal length with the former; and, unſcrewing the ball, he fixed the ſmall wire in its ſtead, upon the upper end of his diſcharging rod. The lower end of the rod was now in contact with the bottom of the battery. The apparatus being thus diſpoſed, he re-charged the battery, and took off the charge upon the end of the ſmall wire, which was ſharply pointed; but, in this caſe, only a very little of the wire was melted; proving, as he obſerves, the preference due to points rather than knobs, as terminations to the conductors for the lightning on buildings, ſhips, &c. The ſame gentleman hath lately favoured me with the following experiment, and the inference reſulting from it.

"Having charged a battery, and unſcrewed one "of the knobs of the diſcharging rod, I cemented "a very ſmall portion of leaf gold on that end, "in ſuch a manner as to act like a point; then ap-"plying the other end, or knob, ſo as to commu-"nicate with the coatings of the jars, I ſuddenly "brought the gold-leaf, as near the diſcharging "part of the battery, as could be done without "danger of a ſtroke; ſtill advancing nearer, in "proportion to the deſcent of the index of your

"elec-

"electrometer. In this manner, I exhaufted the "battery, without any fenfible diminution, or de- "ftruction, of the gold leaf. Whereas a greater "quantity of leaf gold may be diffipated, by a "ftroke from a fingle fmall jar, if put under one "of the knobs of the difcharging rod, and the "ftroke be invited by the other; for in fuch cafe, "the leaf gold cannot, in any manner, act like a "point.

"Now as bodies act at a greater diftance, by "how much they are more acute, and thereby "diminifh any known electrical force; and, as in "any particular cafe, the fmallnefs of the pencil, "or ftroke, depends on the acutenefs of the point "prefented, I cannot avoid giving my fuffrage "for points, in preference to obtufe bodies.

It may not, I think, be improper to introduce, in this place, an experiment lately made by Mr. EDWARD NAIRNE, in Cornhill; which, though it doth not immediately relate to the particular fubject of this paper, is a very proper one to demonftrate the utility of metallic conductors in general.

MR. NAIRNE'S EXPERIMENT.

He affixes, in a little apparatus refembling the hulk of a fhip, a glafs tube, about eight inches long, and half an inch in diameter, which is to reprefent the main-maft. The ends of the tube, which is filled with water, are properly fecured by corks; and, through each cork, a wire is introduced, of fuch a length as to reach nearly

 to

to the middle of the tube, and leave a diſtance of about half an inch, between the ends of the two: as in a curious experiment of Mr. LANE's, made with ſmall phials. A ſlight ſhock, diſcharged through this apparatus, inſtantly breaks the tube in pieces, at that part, where the electric matter quits the upper wire, and expands itſelf in the water, before it reaches the lower one; as the natural electricity hath been obſerved to do, in bodies, wherein it hath met with ſuch an interrupted and broken communication of metal; but Mr. NAIRNE having fixed, at the top of ſuch a glaſs tube, and united with the wire of it, a piece of very ſmall harpſichord wire, which was continued to the bottom of it, and there faſtened to a regular communication of metal, in contact with the coating of the jars; he diſcharged through it his four batteries united, conſiſting of ſixty-four jars, containing fifty ſquare feet of coated ſurface fully charged, when the whole of the ſmall wire was inſtantly exploded and loſt; but the tube remained unhurt. An effect analogous to that of the natural electricity, where, though it hath ſometimes happened, that the conductor, being too ſmall, hath been in part deſtroyed, or much injured by a ſtroke; yet the building, to which ſuch a conductor hath been affixed, hath eſcaped, without receiving the leaſt damage.

Among ſome very intereſting remarks on the effects of lightning, by the ingenious Profeſſor WINTHROP of NEW CAMBRIDGE, which have lately been communicated to me by my learned friend Dr. FRANKLIN, I find one, on the influence of of ſharp pointed conductors, ſo immediately relating to the queſtion under conſideration, that no apology will be neceſſary for introducing it in this place. Dr. WINTHROP, having given a very curious and exact account of a violent flaſh of lightning, which fell upon and greatly damaged HOLLIS-HALL, in NEW CAMBRIDGE, obſerves, that HARVARD-HALL, being furniſhed with pointed wires, which wires were at the diſtance of one hundred and ſixty feet from the chimney of Hollis-hall, on which the lightning fell, eſcaped unhurt, though the wires were ſeen by many to tranſmit a large quantity of it, which left viſible marks upon the bricks, where the wire hooked together. This gentleman alſo obſerves, that a tree, ſtanding at the diſtance of fifty-two feet from a pointed wire, erected upon the ſteeple of a meeting-houſe, as a conductor for the lightning, had been ſtruck and ſhivered; but that the meeting-houſe remained uninjured; and this, he ſays, is the leaſt diſtance from ſuch a conductor, ſo far as he knew, at which any thing had been ſtruck by lightning. It appears, therefore, I think, very clearly, from theſe inſtances, that ſharp pointed wires, inſtead of inviting, and drawing down ſtrokes of lightning, ſerve rather to prevent them, and that they extend their protecting influence to ſome diſtance around them, and ought therefore ever to be uſed, as the termination of the rods erected upon houſes, ſteeples, magazines, maſts of ſhips, &c. in ſhort, on all occaſions, where conductors for the lightning may be thought neceſſary.

I cannot avoid taking notice, in this place, of ſome appearances, upon the iron conductors on St. PAUL's cathedral, ſuppoſed to have been the effects of lightning; of which an account hath been given to the Royal Society, by a very ingenious and worthy member, of whoſe candor I would willingly think too highly, to ſuppoſe he can be offended, if my opinion concerning theſe appearances ſhould be different from his. In the firſt place, had thoſe bars been heated to a red heat ſo inſtantaneouſly, it ſeems probable, that the moiſture in the ſtone, which almoſt ſurrounds a conſiderable length of them, at each end, where they are inſerted in the pavement, would have been turned to ſteam; and, acting like gun-powder, would have exploded, and driven out the bars with great violence. Alſo the leaden pipes, particularly the ends of them which are in contact with the iron bars, muſt have been much melted; but theſe I carefully examined, and could not perceive, that they had been at all affected. Secondly, the end of the iron bar, ſuppoſed to be the moſt affected, was not in contact with the lead-work below it, by near two inches; yet it had no appearance of fuſion, as it certainly would have had, if ſo large a quantity of electricity had paſſed through it; the ruſt likewiſe, I think, would have been cleared away, which was not the caſe, and the end of it have been left quite bright. As to the hole through the dirt, mentioned by Mr. Gould, which lay adjoining to it; the dirt lying in a perpendicular direction, I am inclined to think, that the rain-water had ſoaked through it, and not waſhed it intirely away. I ſhould apprehend likewiſe, that the ruſt obſerved to lie upon the pavement, had been beaten off from the bars, by the hail, or waſhed off, by the rain, which could not happen to the end of that before mentioned; it being covered by a ſtone, which compleatly ſheltered it from the weather. Had it been exploded from the bars, by ſo violent a ſtroke of lightning, as hath been ſuppoſed to paſs through them, I have ſome doubt, whether much of it, if any, would have been diſcovered. Thirdly, in ſo great a ſtroke, the pointed ornaments of copper, upon the croſs, would probably have been affected; perhaps melted down; but theſe do not appear to have received the leaſt injury. I muſt further remark, that as the conductors, on which theſe appearances were obſerved, were neither of them in contact with the lead-work below them; and there were two other conductors of equal ſize, forming a regular communication of metal throughout; it ſeems probable, that the electricity would have paſſed in the two laſt mentioned; and that thoſe ſuppoſed to be ſo much affected, would have conducted very little, if indeed any part at all of the ſhock. The improbability, I believe I might venture to ſay the impoſſibility, of thoſe bars, which are four inches broad and half an inch thick, being heated to a red heat, by lightning, appears to me ſtill more plainly, when I conſider, that in a ſtroke, which fell upon the weather fane in the ſpire of St. BRIDE's church, in Fleet-ſtreet, the iron ſpindle, which ſupported it, being twenty feet long, and two inches in diameter, and the lower ten feet of it being ſurrounded by ſtone work, and fixed firmly therein by melted lead, was not in the leaſt affected; but conducted the ſtroke, as far as it went, with ſafety to itſelf as well as the building; and no doubt would have done the ſame to the bottom, had it been continued thither. In this bar, the lightning had an opportunity of accumulating, and it appears, that it did ſo, to a moſt aſtoniſhing degree; yet the gilding only, on the fane, was a little diſcoloured, owing perhaps to the gold ſize, which connected it with the metal; and this, it ſeems, was all the damage it ſuſtained. Similar to this, I would obſerve, that the paint, on ſeveral parts of the iron-work, now in my poſſeſſion, of the chapel, through which the lightning hath paſſed, is not at all affected. An iron bar alſo, upon the OBELISK before-mentioned, of about half an inch diameter, which ſupports the lamp iron at the N. W. corner, upon which the lightning concentrated; and two of the iron rails, on which it leaped from thence, are not injured in the painting; though the points of the rails, which were the fourth on the N. and the fifth on the W. ſide, and the two corners of the iron bar, from whence it flew upon them, were melted a little. It is worthy notice, that theſe two rails were the neareſt to, and ſtood at nearly an equal diſtance, about twenty-five inches, from the lower end of the bar before-mentioned. In ſhort, the more I reflect on this remarkable account, and conſider the effects of lightning in former and ſimilar inſtances, the more firmly I find myſelf fixed in my opinion, that the appearances, obſerved upon

upon the conductors on St. PAUL's cathedral, were not the effects of lightning, but proceeded from very different causes.

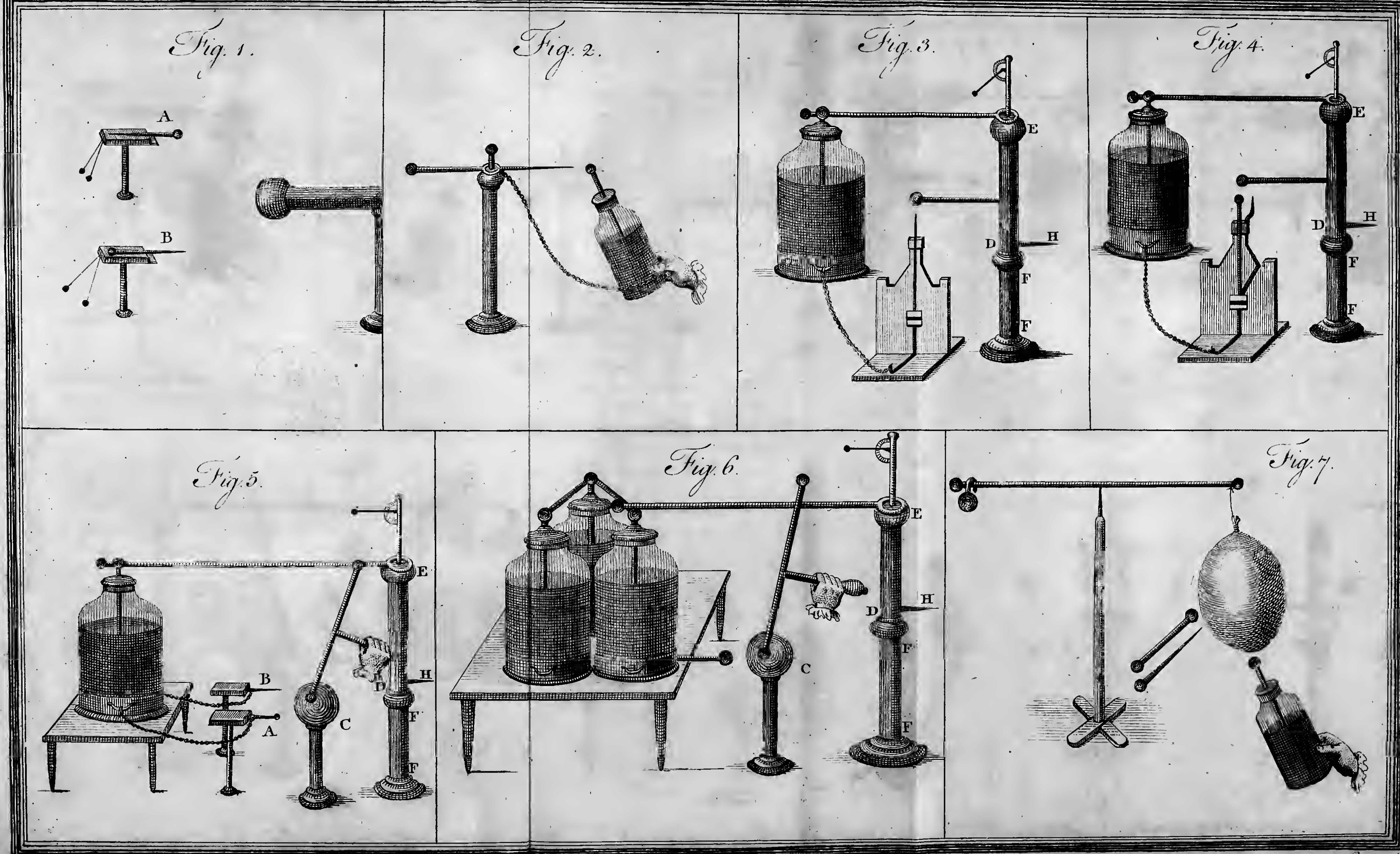
I have made many other experiments on the different effect of knobs or points, as opposed to insulated electrified bodies; but as they all concur, in establishing and confirming the opinions before advanced, it seems unnecessary to mention them; and the more so, as I believe those already recited will be deemed sufficiently decisive without them.

Having now finished what I had to offer, upon the subject of pointed conductors, as being the most proper for the security of buildings, &c. I shall add, by way of appendix, a very curious observation, relating to *personal* security, which I find likewise among those of the learned Professor WINTHROP, communicated to me by Dr. FRANKLIN. This gentleman, having remarked, that people standing in an open plain, are by no means secure from a stroke of lightning, advises those, who may be overtaken by a storm, in such a situation, to retire within some small distance, as from thirty or forty, to ten or fifteen feet, of an high tree (perhaps about fifteen or twenty feet from the outermost branches, may be as proper a distance as any) or rather two such, if at hand, and there wait the event, but by no means to go under them. This advice will, I believe, be acknowledged to be most judicious, and, if properly attended to, may be of great ſervice to travellers, and be the means of ſaving the lives of numbers; and as ſuch, cannot be made too public.

P. S. Since the reading of the preceeding paper, I have taken an opportunity to repeat the moſt intereſting of the experiments therein recited; and find no reaſon to alter a tittle in my account of them.

The 6th experiment I have alſo varied a little, as follows; I placed the large copper ball, C, at ſuch a diſtance from that in contact with the jars, that on bringing down the charge by my diſcharging rod, it ſeemed to remain almoſt undiminiſhed, though the rod was kept in contact with the prime conductor a full minute. Then, repeating the experiment, with a point inſtead of a knob, the charge was, in a great meaſure, preſently drawn off ſilently. Upon the whole, it appears that though a ſharp point will draw off a charge of electricity ſilently, at a much greater diſtance than a knob, yet a knob will be ſtruck with a full exploſion, or ſhock, the charge being the ſame in both caſes, at a greater diſtance than a ſharp point; and this, I imagine, completely decides the queſtion, which I propoſed to examine.

XIX. *Re-*



D.F. Fig. 3. 4. 5. 6. is the prime conductor which is supported on a Glass Rod F.F. The point H. draws off the Electricity from the excited Globe, which is not shewn in these figures.

Basire sc.

XIX. *Remarks upon a Paſſage in* Caſtillione's *Life of Sir* ISAAC NEWTON. *By* John Winthrop, *LL. D.* Hollißian *Profeſſor of Mathematics, at* Cambridge, *in* New England.

Redde, Jan. 20, 1774.

THERE is a paſſage in CASTILLIONE'S life of Sir ISAAC NEWTON, prefixed to his edition of the OPUSCULA, in three volumes 4to. publiſhed at Lauſanne and Geneva in 1744, which appears to me a palpable miſtake; and tends to place Sir ISAAC NEWTON in an inferior light to DES CARTES, in the eyes of foreigners. It is this, p. xxxii: "Sæpiùs ſe reprehendebat [NEUTONUS] "quòd res merè geometricas algebraïcis rationibus "tractaviſſet, & quòd libro ſuo de Algebrâ *Arith-* "*meticæ Univerſalis* titulum poſuiſſet, meliùs aſ- "ſerens CARTESIUM ſuum de re eâdem volumen "dixiſſe *Geometriam*, ut ſic oſtenderet has com- "putationes ſubſidia tantùm eſſe Geometris ad "inveniendum." The authority he quotes for this is Dr. PEMBERTON, in the preface to his *View of Sir* ISAAC NEWTON'S *Philoſophy*; but I will venture to ſay, he has miſinterpreted his Author. He repreſents Dr. PEMBERTON as ſaying; 1ſt, That Sir ISAAC NEWTON often cenſured *himſelf* for handling geometrical ſubjects by algebraic calculations: 2dly, That another thing he often cenſured *himſelf* for, was, his having called his book of Algebra by the name of *Univerſal Arithmetic*: 3dly, That he commended DES CARTES, as having done better, in giving the title of *Geometry* to his treatiſe on the ſame ſubject.—The two laſt particulars, certainly, and I think the firſt alſo, have no foundation in the account Dr. PEMBERTON has given of this matter. His words are: "I have often heard him [Sir ISAAC] "cenſure the handling geometrical ſubjects by al-"gebraic calculations; and his book of Algebra "he called by the name of Univerſal Arithmetic, "in oppoſition to the injudicious title of Geome-"try, which DES CARTES had given to the trea-"tiſe, wherein he ſhews, how the geometer may "aſſiſt his invention by ſuch kind of computa-"tions."—Dr. PEMBERTON's expreſſion does not at all imply, that Sir ISAAC NEWTON cenſured *himſelf* for handling geometrical ſubjects by algebraic calculations: the only idea it ſuggeſts, is, that he cenſured that way in general, and thoſe who practiſed it, and that he had his eye particularly upon DES CARTES;—and, far from intimating, that he had inconſiderately called his book of Algebra by the name of *Univerſal Arithmetic*, and afterwards cenſured himſelf for doing ſo, and wiſhed that he had rather called it *Geometry*, as DES CARTES did his; it directly affirms, on the contrary, that by expreſs deſign and choice, he called it *Arithmetic*, in *oppoſition* to DES CARTES's *injudicious* title of *Geometry*.

It

It is true, indeed, that in a following paſſage, Dr. PEMBERTON ſays, "of their [the antients] taſte "and form of demonſtration, Sir ISAAC always "profeſſed himſelf a great admirer: *I have heard "him even cenſure himſelf for not following them "yet more cloſely than he did*; and ſpeak with "regret of his miſtake, at the beginning of "his mathematical ſtudies, in applying himſelf "to the works of DES CARTES, and other alge-"braic writers, before he had conſidered the ele-"ments of EUCLID with that attention, which "ſo excellent a writer deſerves."—But the mode of expreſſion here uſed, is ſo different from the foregoing, that there can be no doubt, but that it was intended to convey a different meaning. And if, in the cenſure firſt mentioned, viz. "*for "handling geometrical ſubjects by algebraic cal-"culations*," Dr. PEMBERTON had underſtood that Sir ISAAC meant to include *himſelf*, this laſt paſſage would have been a mere tauto-logy. But this laſt ſtrongly implies, on the con-trary, that Sir ISAAC had, in general, endeavoured to *follow cloſely the antient geometrical* form of de-monſtration, in preference to that by *algebraic* cal-culation; which is of modern invention.

There is a remarkable inſtance of the attention he paid to the diſtinction between theſe methods, and of the preference he gave to the former, in his great work of the *Principia*. Having in Lemma XIX. and its Corollaries, given a conciſe and ele-gant ſolution of a noted geometric Problem, he ſubjoins: "Atque ita problematis veterum de qua-

"tuor lineis ab EUCLIDE incepti, & ab APOLLO-"NIO continuati, *non calculus, ſed compoſitio geo-"metrica*, qualem veteres quærebant, in hoc co-"rollario exhibetur." That the words, "*non cal-"culus ſed compoſitio geometrica*," refer to DES CARTES's prolix, algebraic ſolution of this Problem, in his *Geometry*, p. 25—34, will, I believe, be readily granted by every one, that is acquainted with Sir ISAAC NEWTON's writings.

Upon the whole, I humbly conceive, that Dr. PEMBERTON's meaning, in the former paſſage, might have been better expreſſed in Latin, as follows: "Sæpiùs eos reprehendebat, qui res merè "geometricas algebraïcis rationibus tractaviſſent; "et libro ſuo de Algebrâ *Arithmeticæ Univerſalis* "titulum ponebat, aſſerens CARTESIUM ſuum de "re eâdem volumen inſcitè dixiſſe *Geometriam*, "in quo oſtendit, quomodo hæ computationes ſub-"ſidia eſſe poſſunt geometris ad inveniendum."

Which of theſe tranſlations does moſt juſtly expreſs the ſenſe of the original, may, I ſuppoſe, be ſafely left to the judgement of every perſon that underſtands both the languages.

I would only add, that this miſtake of CASTILLIONE, muſt have been owing, either to inadvertence, or to his not being perfectly acquainted with the Engliſh language; as he elſewhere appears to have had the higheſt veneration for Sir ISAAC NEWTON.

This miſtake may, to ſome, appear trivial; but, in my apprehenſion, every circumſtance, relative to ſo illuſtrious a character as that of Sir ISAAC

NEWTON,

NEWTON, derives importance from it; and ought to be marked with great exactneſs.

The foregoing remarks are, with all deference, ſubmitted to the judgement of the ROYAL SOCIETY, by

Their moſt obedient,

humble Servant,

Cambridge, New England,
March 4, 1773.

JOHN WINTHROP.

XX. *M.* De Luc's *Rule for meaſuring Heights by the Barometer, reduced to the* Engliſh *Meaſure of Length, and adapted to* Fahrenheit's *Thermometer, and other Scales of Heat, and reduced to a more convenient Expreſſion. By the Aſtronomer Royal.*

Redde, Jan. 13, 1774.

M. DE LUC, F. R. S. in a large and valuable Treatiſe upon the Barometer and Thermometer, lately publiſhed at Geneva, the reſult of many years labour and ſtudy, has given a rule for the meaſurement of heights by the barometer, deduced from his experiments, and far more accurate, than any publiſhed before; ſince it appears that he could determine heights by it generally to 10 or 15 feet, and that the error ſeldom, if ever, amounted to double that quantity. This valuable degree of exactneſs he has obtained principally by detecting the faults of the common barometer, and, in conſequence, improving the conſtruction of it; and by introducing the uſe of the mercurial thermometer, to accompany that of the barometer. The principal faults, which he found in the common barometers, aroſe from the repulſion of the quickſilver by the glaſs tube, from air and moiſture admitted into the tube, and from the variations of the denſity of quickſilver by heat and cold; another very conſiderable error aroſe, in calculating heights from the barometer, by not allowing for the changes of the denſity of the air, whoſe gravity affords us this meaſure of heights, owing to heat and cold. The firſt cauſe of error, that of the repulſion of the tubes, he remedied, by ſubſtituting a ſyphon-barometer inſtead of the ſimple upright tube, the repulſion of the two legs of the ſyphon, counteracting itſelf; the error ariſing from air and moiſture in the tube he cured by boiling the quickſilver, after it was put into the tube, and other precautions; the errors, in the eſtimation of the heights, ariſing from the changes of the denſity of the quickſilver, and denſity of the air by heat and cold, he ſhews how to correct by allowances depending on two thermometers, one attached to the frame of the barometer itſelf, and the other made to be expoſed to the open air, to ſhew its degree of heat; which thermometers are to be noted both at the top and bottom of the hill. Laſtly, by a great number of experiments made with accurate barometers and thermometers of his own conſtruction, he has deduced a rule for calculating heights of places; the exactneſs of which he has ſufficiently proved by a large table of experiments. But this rule is expreſſed in French meaſure, and is adapted either to a thermometer, whoſe freezing point is o, and that of boiling water 80, or to thermometers of particular ſcales. It

may

may be therefore uſeful, to reduce M. DE LUC's rule to Engliſh meaſure, and to adapt it to the thermometer of Fahrenheit's ſcale, which is generally uſed in this country.

M. DE LUC, in the winter ſeaſon, heated the air of his room to as great degree as he could, and noted the riſe of the barometer, owing to the diminution of its denſity, or ſpecific gravity, by heat; he alſo noted the height of the thermometer, both before and after the room was heated. Hence he deduced a rule, that when the barometer is at 27 French inches, which was the caſe in this experiment, an increaſe of heat, from freezing to that of boiling water, will raiſe the barometer 6 lines, or $\frac{1}{54}$th part of the whole. It is eaſy to ſee, that when the barometer is higher than 27 French inches, this variation will increaſe in the ſame proportion; or will be always $\frac{1}{54}$th of the heighth of the barometer; therefore, if the height of the barometer be called B, the riſe of the barometer, for an increaſe of heat from freezing to boiling water, will be $\frac{B}{54}$; and, as it will be leſs for a leſs difference of heat, therefore, if the number of degrees, marked on the thermometer, between freezing and boiling water, be called K, and the riſe of the thermometer from any given point be called H, the correſpondent riſe of the barometer will be $\frac{B}{54} \times \frac{H}{K}$, by the increaſe of heat from the given point by the number of degrees H. If the heat, inſtead of increaſing, was to decreaſe, then H would

would signify so many degrees decrease of heat, and the barometer would sink by $\frac{B}{54} \times \frac{H}{K}$. The fixt temperature of heat, to which M. DE LUC thought best to reduce his observations of the barometer, is $\frac{1}{8}$th of the interval from freezing to boiling water above the former point: and if the thermometer was higher than this degree, he substracted $\frac{B}{54} \times \frac{H}{K}$; if it was lower, he added it to the observed height of the barometer; and thus he obtained the exact height of the barometer, such as it would have been, if the density of its quicksilver had been the same as answers to the fixt degree of temperature. He thus corrected the height of both his barometers (that at the bottom, and that at the top of the hill) for the particular degree of heat, indicated by a thermometer attached to the barometer, at each station; for it might and would commonly happen, that the degree of heat would be different at the two stations. The heights of the barometers, thus corrected, were what he made use of in his subsequent calculations. Calling these two altitudes of the barometer B and *b*, putting log. B and log. *b*, for the logarithms of B, and *b*, taking only the four first places of figures, after the characteristic, or considering the remaining figures as decimals, and putting C for the mean height of a thermometer, exposed to the air at top and bottom of the hill, the freezing point being o, and the point of boiling water at 80, he finds, by his experiments, that the height of the hill will be given in French toises, when C is 16¾, by

 simply

ſimply taking the difference of the logarithms of the heights of the barometer, or will be equal to log. B — log. *b*; and in any other degree of heat, will be greater or leſs, in proportion as the rarity of the air is greater or leſs, than in the fixt temperature; or greater or leſs by $\frac{1}{215}$th part of the whole, for every degree of the thermometer reckoned from the fixt temperature $16\frac{3}{4}$; and conſequently the height of the place will be expreſſed generally in French toiſes, by this *formula*

$$\log. B - \log. b + \overline{\log. B - \log. b} \times \frac{C - 16\frac{3}{4}}{215}$$

$$= \overline{\log. B - \log. b} \times \overline{1 + \frac{C - 16\frac{3}{4}}{215}}.$$ To reduce this *formula* to Engliſh meaſure, and to the ſcale of Fahrenheit's thermometer, we ſhould firſt premiſe ſome particulars. The French foot is to the Engliſh foot, as 1,06575 to 1, as was found by a very accurate experiment: ſee Phil. Tranſ. Vol. LVIII. for 1768, p. 326; and it is well known, that the point of freezing, on Fahrenheit's thermometer, is at 32, and that of boiling water at 212, or the interval between them 180 degrees. But M. DE LUC's point of boiling water 80, was marked when the barometer was at 27 French inches; and it is the cuſtom of our principal Engliſh workmen to mark the point of boiling water, 212, on Fahrenheit's thermometer, when the barometer ſtands at 30 inches, which is equal to 28 inches, 1,8 lines French meaſure; or 13,8 lines higher than M. DE LUC's barometer, when he ſet off the point of boiling water on his thermometers; and it is well known, that the heat of boiling water varies with the weight of the atmoſphere: M. DE LUC finds, by his experi-

ments,

ments, this rule, that an increafe of 1 line in the height of the barometer, raifes the quickfilver of the thermometer, placed in boiling water, by $\frac{1}{1134}$th part of the interval between the freezing point and that of boiling water: he afterwards indeed found, that this rule would not anfwer for fuch large variations of the barometer, as take place in afcending to very great heights above the earth's furface; *vid. Effai fur les variations du chal: de l'eau bouill:* but it is accurate enough for any fmall variation of the barometer, on one fide or other of its mean height in thefe loweft regions of the atmofphere. The change therefore of the boiling point on Fahrenheit's fcale, for a change of 1 line in the barometer, will be $\frac{180}{1134} = 0{,}16$; therefore 13,8 lines will caufe $0{,}16 \times 13{,}8 = 2{,}2$ degrees of Fahrenheit's fcale; and a thermometer, whofe point of boiling water was marked 212, when the barometer ftood at 30 Englifh inches=28 inches, 1,8 lines French meafure, will, when the barometer defcends to 27 French inches, fink 2,2 degrees in boiling water, or to 209,8, or in round numbers to 210 degrees, which is diftant only 178 from 32, the point of freezing. Hence an extent of 80° of M. DE LUC's thermometer anfwers to an extent of 178 of our Fahrenheit's thermometer; and putting F for the degrees of this thermometer, correfponding to C of M. DE LUC's, we fhall have $C : F - 32 :: 80 : 178$, and $C = \overline{F - 32} \times \frac{80}{178}$ which fubftituted in M. DE LUC's *formula*, gives

$$\overline{\log. B - \log. b} \times \overline{1 + \frac{C - 16\frac{3}{4}}{215}} =$$

$$= \overline{\log. B - \log. b} \times \overline{1 + \frac{\overline{F - 32} \times \frac{80}{178} - 16\frac{3}{4}}{215}} =$$

 =

$$=\overline{\log. B - \log. b} \times \overline{1 + \frac{80}{178 \times 215} F - 32 - \frac{178}{80} 16,75}$$

$$= \overline{\log. B - \log. b} \times \overline{1 + \frac{F - 32 - 37,27}{478,38}} =$$

$= \overline{\log. B - \log. b} \times \overline{1 + \frac{F - 69.27}{478,38}}$. Where the answer will still come out in French toises, though adapted to Fahrenheit's thermometer. To bring it out in English fathom (or measure of 6 feet) multiply the above expression by 1,06575, and we shall have $\overline{\log. B - \log. b} \times \overline{1 + \frac{F - 69\,27}{478,38}} \times 1,06575$

$$= \overline{\log. B - \log. b} \times \overline{409,11 + F} \times \frac{1,06575}{478,38} =$$

$= \overline{\log. B - \log. b} \times \frac{409,11 + F}{448,87}$, or in round numbers

$$= \overline{\log. B - \log. b} \times \frac{\overline{409 + F}}{449} =$$

$= \overline{\log. B - \log. b} \times \overline{1 + \frac{F - 40}{449}}$; which will express the height between the two stations in English fathom.

In the foregoing expressions, B and b, as has been mentioned before, signify heights of the barometer, at the lower and higher stations, both corrected according to Mr. DE LUC's directions, for the difference of heat between a fixed temperature, (namely $\frac{1}{8}$th of the interval between freezing and boiling water), and the present heat, indicated by the thermometer attached to the barometer at each station; but it is not necessary, to correct *both* barometers for the effect of heat, but only one for the difference of heat of the two; which will be more convenient also on another account, because the difference of heat, at the two stations, will

will be generally ſmall, and the correction to reduce one barometer to the heat of the other will conſequently be ſmall alſo; whereas the difference of the preſent heat, and the fixt temperature, and conſequently the correction of both barometers, may be frequently very conſiderable: this is evident: becauſe if the heat of the barometers, at both ſtations, was the ſame, however different from the fixt temperature choſen by M. DE LUC, no correction would be neceſſary; the mercury in the barometer in both ſtations, being expanded in the ſame proportion, and conſequently the difference of the logarithms of its height, at both ſtations, being the ſame, as if the heat of both barometers had agreed with that of the fixt temperature. I ſhall now therefore ſuppoſe the upper barometer is to be corrected, to reduce it to the temperature of the lower one, and that b ſignifies the height of this barometer, as obſerved, and not yet corrected; the correction, from what has been ſaid above, calling D the difference of height of the thermometer attached to the barometer at the two ſtations, will be $\pm \frac{D b}{54 K}$, according as the thermometer ſtands higheſt at the lower or upper ſtation; and the upper barometer corrected, inſtead of b, will be $b \pm \frac{D b}{54 K}$, which ſubſtituted in the *formula*, gives $\overline{\text{log. B} - \text{log.}\left(b \pm \frac{D b}{54 K}\right)} \times \overline{1 + \frac{F - 40}{449}}$. But the correction, on account of the difference of heat of the barometer at the two ſtations, may be reduced to a ſtill eaſier expreſſion, in which the variable quantity b, the height of the upper barometer, ſhall

ſhall not appear. The fluxion of a logarithm is to the fluxion of its natural number, as the modulus of the ſyſtem to the natural number; and 4343 is the modulus of the common logarithms, when the four places, next following the characteriſtic, are taken as whole numbers, inſtead of decimals, which is meant to be done in the uſe of the foregoing *formula*. Therefore $\frac{Db}{54K}$ being very ſmall with reſpect to b, we ſhall have, variation of log. b : variation of b $(=\frac{Db}{54K})$ :: 4343 : b very nearly, and thence variation of log. $b = \pm \frac{Db}{54K} \times \frac{4343}{b} = \pm \frac{4343 D}{54 K}$. Which (putting $K = 178$) $= \pm 0{,}452\,D$. Hence $\log.\ (b \pm \frac{Db}{54K}) = \log.\ b \pm 0{,}452\,D$; which being ſubſtituted in the *formula* above, will give the difference of height, of the two ſtations, in Engliſh fathom, in a more convenient expreſſion, namely $\overline{\log.\ B - \log.\ b \mp 0{,}452\,D} \times \overline{1 + \frac{F-40}{449}}$; where the upper ſign, —, is to be uſed, when the thermometer of the barometer is higheſt at the lower ſtation, and the lower ſign, +, is to be uſed, when the ſaid thermometer is loweſt at the lower ſtation. The firſt caſe will be moſt common; eſpecially where the difference of height of the two ſtations is conſiderable. It ſhould alſo be obſerved, that when F, the height of Fahrenheit's thermometer, is leſs than 40°, $+\frac{F-40}{449}$ becoming negative or ſubſtractive, muſt be applied in the calculation accordingly.

It

It may perhaps be convenient to repeat here the meaning of the algebraic terms, uſed in the foregoing *formula*, that any perſon may make uſe of it, without having occaſion to recur to the foregoing inveſtigation. B ſignifies the obſerved altitude of the barometer at the lower ſtation, and b that at the upper ſtation; Log. B and Log. b, ſignify their logarithms taken out of the common tables, by aſſuming the four firſt figures next following the characteriſtic as whole numbers, and conſidering the three remaining figures, to the right hand, as decimals; D ſignifies the difference of height of Fahrenheit's thermometer, attached to the barometer at the top and bottom of the hill; and F ſignifies the mean of the two heights of Fahrenheit's thermometer, expoſed freely for a few minutes to the open air in the ſhade, at the top and bottom of the hill.

The *formula*, for the meaſure of heights, may alſo be changed, and adapted to thermometers of particular ſcales, for the convenience of calculation, as M. DE LUC has done; but theſe ſcales will be different from his. The thermometer, attached to the barometer, had better be divided with the interval between freezing and boiling water, conſiſting of 81,4 degrees ($=180 \times ,452$) the freezing point may be marked 0, and the point of boiling water will be 81,4; for then, if the difference of height of this thermometer, at the two ſtations, be called d, we ſhall have $d =$, $0,452 \times D$. for $d : D :: 81,4 : 180 :: 0,452 : 1$, and the number of degrees expreſt by d, will ſhew immediately the correction for the difference of heat of the two barometers. If the thermometer, deſigned to ſhew the temperature of the air, be divided,

divided with the interval between freezing and boiling water $= 200$, and the freezing point be marked $- 9$, and the boiling point $+ 191$, and the heights of this thermometer, at the two ſtations, be called G and I, we ſhall have $\frac{F - 40}{449} = \frac{G + I}{2 \times 500} = \frac{G + I}{1000}$. For $F - 40 = F - 32 - 8$, is the height of Fahrenheit's thermometer, reckoned from 8 degrees above freezing, and $449 : 500 :: 180 : 200 :: 8 : 9$, and the fraction $\frac{F - 32 - 8}{449}$, if both the numerator and denominator be increaſed in the ratio of 449 to 500, will become $= \frac{\overline{F - 32 - 8} \times \frac{500}{449}}{500} = \frac{\overline{F - 32} \frac{500}{449} - 9}{500} = \frac{G + I}{2 \times 500} = \frac{G + I}{1000}$, becauſe $\frac{G + I}{2} + 9 = \overline{F - 32} \times \frac{500}{449}$. Therefore, if the thermometer of the barometer has the freezing pointmarked 0, and the point of boiling water 81,4, and the difference of its height, at the two ſtations, be called d; and the thermometer for meaſuring the temperature of the air, be divided with the interval of 200 between the freezing point and that of boiling water, and the firſt be marked $- 9$, and the latter $+ 191$, and the degrees, ſhewn by this, at the two ſtations, be called G and I; the *formula*, that will give the height of the upper ſtation above the lower one, in Engliſh fathoms, will be $\overline{\log. B - \log. \overline{b \mp d}} \times \overline{1 + \frac{G + I}{1000}}$; which conſequently multiplied by 6, will give the height in Engliſh feet. It is to be obſerved, as before, that $- d$ or $+ d$ is to be uſed, according as the thermometer, attached to the barometer, is higheſt at the lower or upper ſtation; and if G and I ſhould happen to fall below o of the ſcale, or to be ſubſtractive, they muſt be applied accordingly in the calculation.

I ſhall now add nothing more, but to give the rule for finding heights by the barometer, according to the *formulæ* delivered above, in common language; firſt, as adapted to Fahrenheit's thermometer, and next, as adapted to the two thermometers of particular ſcales. Take the difference of the tabular logarithms of the obſerved heights of the barometer, at the two ſtations, conſidering the 4 firſt figures, excluſive of the index, as whole numbers, and the three remaining figures to the right as decimals, and ſubſtract or add $\frac{452}{1000}$th of the difference of the altitude of the Fahrenheit's thermometer, attached to the barometer at the two ſtations, according as it was higheſt at the lower or upper ſtation; thus you will have the height of the upper ſtation above the lower, in Engliſh fathom, nearly; to be corrected, as follows: make this proportion; as 449 is to the difference of the mean altitude of Fahrenheit's thermometer, expoſed to the air at the two ſtations, from 40°, ſo is the height of the upper ſtation found nearly, to the correction of the ſame; which added or ſubſtracted, according as the mean altitude of Fahrenheit's thermometer was higher or lower than 40°, will give the true height of the upper ſtation above the lower, in Engliſh fathoms; and multiplied by 6, will give it in Engliſh feet.

The ſame rule, adapted to the thermometers of particular ſcales, is this:

 Take

Take the difference of the tabular logarithms of the obſerved heights of the barometer, at the two ſtations, conſidering the 4 firſt figures (excluſive of the index) as whole numbers, and the three remaining figures to the right as decimals; and ſubſtract or add the difference of the thermometer, of a particular ſcale, attached to the barometer, at the two ſtations, according as it was higheſt at the lower or upper ſtation, and you will have the height of the upper ſtation above the lower one, in Engliſh fathom, nearly; to be corrected as follows: make this proportion; as 1000 is to the ſum of the altitudes of the thermometer of a particular ſcale, expoſed to the air at both ſtations, ſo is the height of the upper ſtation above the lower, found nearly, to the correction of the ſame; which added or ſubſtracted, according as the ſum of the altitudes of the thermometers, expoſed to the air, is poſitive or negative, will give the true height of the upper ſtation above the lower in Engliſh fathoms; and multiplied by 6, will give it in Engliſh feet.

NEVIL MASKELYNE.

XXI. *A Letter to the Aſtronomer Royal, from* Samuel Holland, *Eſq. Surveyor General of Lands for the Northern Diſtrict of* America, *containing ſome Eclipſes of* Jupiter's *Satellites, obſerved near* Quebec.

Kittery, Piſcataqua River, Oct. 18, 1770.

SIR,

Redde, Jan. 20, 1774.

I Would have taken the liberty, of troubling you with a letter before I left Quebec; but being in hopes of making ſome obſervations at my arrival here, to join with thoſe I now incloſe, made at Quebec ſince I had the honor of tranſmitting to you my laſt, I deferred it till now, and am ſorry to inform you, I have had no ſucceſs with Jupiter's ſatellites, and muſt wait till after the conjunction of that planet with the Sun; when I ſhall uſe all my endeavours, to ſupply my loſſes that way, this ſeaſon, through the badneſs of the weather, my travels, and the ſurvey.

Agreeable to your deſire, I have here incloſed, Mr. Sproule's obſervations, made at GASPEE, with the ſteps he took relative thereto (which, I hope, will enable you to determine the longitude of his place of obſervation) with thoſe of mine made at St. JOHN's iſland, and QUEBEC, which will be of great ſervice to the buſineſs I have the honour to be entruſted with.

 I have

I have now materials for forming a general map of our ſurveys of the gulph and river St. LAWRENCE, and with the aſſiſtance of Mr. Deſbarre's ſurveys, employed by the Admiralty, I ſhall be able to bring my map as far as CAPE SABLE, the Southweſt point of the peninſula of NOVA SCOTIA.

Mr. Wright is ſurveying the N.E. part of that province, and will be obliged to winter in thoſe parts. Mr. Sproule is now employed in ſurveying CASCO BAY, and from thence continues his operations towards the bay of FUNDY; while I am buſy in defining this harbor, river, and the ſea coaſt, from CAPE ELIZABETH to CAPE ANNE, where Mr. Wright will begin next ſpring, on his return from the Gulph of St. Lawrence; ſo that I hope to complete the ſurvey of the Northern diſtrict in a few years more; when I ſhall reduce our work to general maps and charts, with the exact latitudes, and, with your aſſiſtance, the longitudes alſo, determined from our obſervations, compared with yours.

Our ſurvey of places and harbors of note, is by a ſcale of 2000 feet to an inch, and the other parts by 4000; after which it is reduced to a ſcale of two ſtatute miles to an inch, or better than 8 $\frac{65}{100}$ inches to a degree of latitude. Theſe maps I purpoſe making on a globular projection, conſidering the earth as a ſphere.

As, by the time I hope to receive your determination of the longitudes of the above places, I ſhall be ready for compoſing my firſt map, I ſhall be greatly obliged to you for your opinion of the moſt uſeful and exact globular projection for a ſpace contained between the 37th and 51ſt parallels of latitude,

tude, and between the 53d and 78th degrees of longitude; which projection may equally ſerve for the ſeparate maps, I may find it convenient to make of different parts of my diſtrict, as well as of the whole of it, which is comprehended in this ſpace: but thoſe charts intended for the uſe of the navy I ſhall compoſe on Wright's or Mercator's projection.

Mr. Wright has had an opportunity, laſt ſpring, of rectifying the latitude of the iſland of COUDRE, where he obſerved the tranſit of Venus, of which he has acquainted you: I had likewiſe an opportunity, on my way to this place, to rectify the latitude of St. JOHN's iſland, at the entrance of PORT JOY, where I made my obſervations in 1765, as I had, at that time, no other inſtrument, but an aſtronomical quadrant, of the old conſtruction; which I found to be very erroneous, when I compared it with BIRD's, on its arrival at Louiſburg, ſome time afterwards: Mr. Wright and I, from very careful obſervations with this laſt inſtrument, have now determined the latitude of the place, where my obſervations were made in 1765, by reducing it from Fort Amherſt, the place we now obſerved at, to be 46° 11′ 0″ North.

It gave me great ſatisfaction, to have an opportunity of examining BIRD's aſtronomical quadrant, laſt year, in New-York province, in determining the latitude of 41°, for ſettling the boundary line, between that colony and New Jerſey, with the ſame inſtrument Meſſ. Maſon and Dixon uſed for determining the boundary line between Penſylvania and Maryland: on this occaſion, Mr. RITTENHOUSE, an efteemed aftronomer and ingenious mechanic of Penfylvania, made ufe of it, and I, of BIRD's; when we never found them to differ more than 17″, which furprized that gentleman much, to find an inftrument, of fuch fmall dimenfions, executed with that accuracy, as to equal fo nearly his large zenith inftrument, which alfo is of BIRD's workmanfhip.

'I have found the latitude of the entrance of this harbor to be 43° 5′ 0″, though it is in general laid down in 43° 20′, and, in fome maps, 26′: and for the charts of thefe coafts, they have but little refemblance to what they fhould exprefs.

How Meff. Wright and Sproule came to neglect the equation of correfpondent altitudes, I cannot otherwife account for, but, that they muft have wanted tables to work by; as before they went on thefe obfervations, I had made ufe of M. DE LA LANDE's tables, and before I was mafter of his, I ufed thofe calculated by M. MARALDI, which, though not fo exact as the former, are very near.

Mr. Sproule's reafon for bringing his obfervations to equal or mean time, was for convenience, as the timepiece he had, conftructed by MUDGE and DUTTON, was eafily brought to it, and is made on the fame principles with that time piece, I always ufe, conftructed by the late Mr. GRAHAM, which keeps going for a month, and,when winding up, continues going by a fpring.

M. ST. GERMAIN, a prieft of the Roman church, and who is ftiled, in the feminary, *Le profeffeur en philofophie,* had been fome time with me, practifing himfelf in the ufe of the telefcope, at Jupiter's fatellites; and, fome days before the tranfit, he was frequently

 trying

trying the poſition he ought to be in, when that phænomenon ſhould by your calculation come: I think he perceived the contact before me, but was fearful to call out too ſoon; for he could not have taken the ſignal from me, as he was ſome diſtance off; and I did not call out, but when I obſerved it, gave a ſignal, by a ſqueeze, to a gentleman ſtanding near, by which time he called. In regard to the luminous point, which, I think, I ſaw juſt before the contact, I am at a loſs to account for it; unleſs ſomething may be urged, from my not having that part of the Sun's diſc exactly in the middle of the field of my teleſcope.

Your favor, directed to me at Portſmouth in New Hampſhire, I ſhall receive with the greateſt regard. I am,

SIR,

Your moſt obedient humble ſervant,

SAMUEL HOLLAND.

ECLIPSES

ECLIPSES OF JUPITER'S SATELLITES,

obſerved by me, at my houſe, bearing South 36° Weſt from QUEBEC, diſtance from the caſtle of ST. LEWIS 2½ miles, with DOLLOND's long refracting teleſcope.

1770.		Mean time. h.	′	″		
Apr.	19	14	5	21	immerſion of the 2d	ſatellite.
May	1	12	42	32	—— —— 1ſt	
	14	12	5	30	—— —— 2d not exact to 4″ thro' the thickneſs of the atmoſphere.	
	21	13	41	30	—— —— 2d	
	24	12	52	20	—— —— 1ſt	

XXII. *Ob-*

XXII. *Obſervations of the Immerſions and Emerſions of the Satellites of* Jupiter, *taken in the Year* 1768, *by Enſign* George Sproule, *of his Majeſty's* 59*th Regiment, on the South Point of the Entrance of* Gaſpee Baſon, *which bears from* Cape Ferrilong, *or the Cape forming the Bay to the Northward, N.* 68¼ *W. by the true Meridian, diſtant* 12¾ *Marine Miles. Communicated by the Aſtronomer Royal.*

Redde, Jan. 20, 1774.

THE obſervations were made with Mr. SHORT's reflecting teleſcope, and the times ſhewn by a pendulum time-piece, made by Meſſieurs MUDGE and DUTTON, firſt adjuſted to equal or mean time, by obſerving the paſſage of Sirius through a gun barrel fixed in the plane of the meridian, with a contraction made in the bore of the barrel; and then proved, in its going, by correſponding and ſingle altitudes, taken with HADLEY's quadrant, as often as the weather would permit, by reflection from clear oil, placed in a room with two windows; one to the S.E. and the other nearly S.W. adjacent to where the clock ſtood. The windows being high, and the Sun having no great altitude, during the time the obſervations were made, the oil was therefore placed at ſuch a diſtance, from the windows, that no wind could get at it to diſturb it. There was alſo every precaution uſed to render it as ſtill as poſſible, by fixing a ſhed to windward without the windows.

The method I uſed, in taking all my correſponding altitudes, was firſt to ſcrew the index to a certain altitude; then, for the morning obſervations, I firſt noted, when the upper limb, by reflection, touched the lower limb in the oil; ſecondly, when the centers coincided, by obſerving an equal coincidence in both images; and laſtly, when the Sun's lower limb, by reflection, touched the upper in the oil. In the afternoon, I obſerved the Sun's falling, noting each contact correſponding to that in the forenoon. I always made it a rule to take a large number, that I might reject thoſe, where the oil ſuffered the leaſt agitation,

The clock was fixed to an upright ſteady poſt, in a warm room, kept as temperate as poſſible, by encreaſing or diminiſhing a large wood fire oppoſite the clock.

Notwithſtanding every precaution, the intenſeneſs of the cold was ſo great, that it frequently ſtopped the clock, by which I loſt many obſervations during the months of January and February.

The following obſervations were made, when it was proved to go, at an equal rate, ſome days before and after.

 The

The teleſcope and quadrant were the ſame Mr. Wright uſed on the iſland of Anticoſti.

The ephemeris uſed was that of Mr. DE LA CAILLE, calculated for ten years to the meridian of Paris.

The teleſcope I had fixed on a ſteady ſtand without doors, near the room where the clock ſtood, and the time counted by a very careful perſon, with another to overlook him: great care was alſo taken of the quadrant, that it ſhould not alter its adjuſtment between the morning and evening altitudes.

The latitude of the place of obſervation, at GASPEE, I determined, as accurately as poſſible, by ſeveral meridian altitudes of the Sun, taken in an artificial horizon, with the aforementioned quadrant; the error of adjuſtment being moſt accurately obtained by different methods. The mean reſult of the whole (15 of which agreed one with the other to 6 or 7 ſeconds) I found to be 48° 47′ 32″. The variation of the needle, by repeated trials different ways, I found 16° 30 Weſt; one of the meridian altitudes I inſert, to ſhew the method I uſed.

	°	′	″
15th May, 1768, double angle of meridian altitude of the Sun's upper limb.	121	10	0
Error of adjuſtment of quadrant to the left,	0	0	35
	121	9	25
Apparent altitude of Sun's upper limb,	60	34	42½
Semi-diameter ſubt.		15	51
App. alt. of center,	60	18	51½
Refraction,			31
	60	18	20
— 90 = Sun's zenith diſtance =	29	41	40
Sun's decl. reduced to the meridian of obſervation *.	19	5	50
Lat. of the S. Point of Gaſpee Baſon,	48	47	30

GEORGE SPROULE.

* i. e. The ☉'s declination, at the time of his paſſing the obſerver's meridian, computed from tables adapted to another meridian, according to the known or ſuppoſed longitude of one meridian from the other. S. HORSLEY.

Mr.

Mr. Sproule, *in reducing the time of his clock, having neglected the equation of equal altitudes, it was necessary to re-compute the corrections; and here follow the immersions and emersions of Jupiter's satellites, reduced to apparent time, from the original observations, due allowance being made for the equation of equal altitudes, by the Astronomer Royal.*

1768			Apparent Time. h	′	″	
Jan.	29.	Im. 1.	13	57	47	
Mar.	15.	Im. 1.	14	21	14	
	16.	Im. 2.	11	59	7	
		Im. 3.	13	30	10	
April	9.	Em. 1.	11	18	26	N. B. In these two emersions, the satellites seemed to emerge slowly out of the shadow.
	10.	Em. 2.	11	38	8	
	25.	Em. 1.	9	39	40	This is the best observation, the satellite starting out instantaneously.
May	9.	Em. 1.	13	30	54	
	12.	Em. 2.	11	15	43	

XXIII. *Astro-*

XXIII. *Aſtronomical Obſervations made by* Samuel Holland *Eſquire, His Majeſty's Surveyor General of Lands for the Northern Diſtrict of North America, for aſcertaining the Longitude of ſeveral Places in the ſaid Diſtrict. Communicated by the Aſtronomer Royal.*

Redde, Jan. 20, 1774.

KITTERY POINT, in the province of MAIN, in PISCATAQUA harbour.

Latitude { by reſult of repeated obſervations, of ☉ and fixed ✱'s, made with BIRD's aſtronomical quadrant, } 43° 4′ 27″ N.

Obſerved, with DOLLOND's 12 feet refracting teleſcope,

Immerſion and Emerſions of ♃'s ſatellites as follow,

		Apparent time. h	′	″
1771				
April 11th, an immerſion of the 1ſt,	at	15	43	30
27th, ſame	ſame	14	1	43
May 4th, ſame	ſame,	15	55	54

The VARIATION of the COMPASS at this place, is 7° 46′ Weſt.

PORTSMOUTH,

PORTSMOUTH, province of NEW HAMPSHIRE.

Latitude { by result of repeated obſervations, of ⊙ and fixed ∗'s, made with BIRD's aſtronomical quadrant, } 43° 4′ 15″ N.

Obſerved, with DOLLOND's 12 feet refracting teleſcope,

Immerſions and Emerſions of ♃'s ſatellites as follow,

1772,				h	′	″
Sept.	6th,	an emerſion of the	2d, at	11	9	20
	18th,	ſame	1ſt,	9	42	35
Oct.	11th,	ſame	ſame,	10	5	4
Nov.	3d,	ſame	ſame,	10	23	54
	9th,	ſame	2d,	10	51	39
	12th,	ſame	1ſt,	6	48	1
	* 19th,	ſame	ſame,	8	42	44
	23d,	immerged entirely,	3d	6	8	6
	ſame,	began to emerge,	ſame,	9	28	14
Dec.	4th,	an emerſion,	2d,	7	50	0
	5th,	ſame,	1ſt,	6	57	44

(Apparent time.)

The VARIATION of the COMPASS at this place is 7° 48′ Weſt.

All theſe obſervations were made by time kept by GRAHAM's time-piece, with a gridiron pendulum, rectified occaſionally by equal altitudes of the Sun, corrected agreeably to the tables of M. DE LA LANDE, for the alteration of the declination in the intervals of correſponding obſervations.

SAMUEL HOLLAND.

* This ſatellite in emerging was in conjunction with another, which occaſioned ſuch a deception as to render this obſervation uncertain about 20 ſeconds ſooner than what is marked here.

XXIV. *Obſer-*

XXIV. *Observations of Eclipses of* Jupiter's *first Satellite made at the Royal Observatory at* Greenwich, *compared with Observations of the same, made by* Samuel Holland *Esquire, Surveyor General of Lands for the Northern District of* America, *and others of his Party, in several Parts of* North America, *and the Longitudes of the places thence deduced, by the Astronomer Royal.*

At the ISLAND OF ST. JOHN.					At GREENWICH.				
			Mean time.	Length of telescope.		Apparent time.	Length of tel.		Diff. of from wich,
			h ′ ″	Feet		h ′ ″	Ft.		h
1765	Jan. 27.	Em. 1.	9 36 31	10	Feb. 19.	13 46 42	6	Very good observation.	4
At LOUISBOURG.									
1766	Mar. 10.	Em. 1.	11 21 27	2 & 10	March 5.	7 43 55	6		4
	Apr. 25.	Em. 1.	11 46 50	2 & 10	April 11.	11 56 30	6		4
1767	Feb. 27.	Im. 1.	8 10 17	10	Feb. 27.	11 57 7	6		3
	Mar. 15.	Im. 1.	8 41 19	10	March 22.	14 28 48	6		3
	April 7.	Em. 1.	8 53 11	10	April 9.	7 20 1	6		3
	April 14.	Em. 1.	10 47 44	10	April 14.	14 47 52	6		4

On the SOUTH POINT at the entrance of the Bay of GASPEE. — At GREENWICH.

			Apparent time.	Length of tel.		Apparent time.	Length of tel.		Diff. of merid. from Greenwich, West.
			h ' "	Ft.		h ' "	Ft.		h ' "
1768	Jan. 29.	Im. 1.	13 57 47	2	Jan. 22.	16 24 13½	6	Air very clear.	4 17 56
	April 25.	Em. 1.	9 39 40	2	April 25.	13 57 19	6		4 17 59
	May 9.	Em. 1.	13 30 54	2	May 11.	12 16 46	6		4 17 31

At Capt. HOLLAND's HOUSE, bearing S. 56 W. from QUEBEC, distant from castle of ST. LEWIS 2½ miles.

			Mean time.						
1769	Mar. 11.	Im. 1.	15 0 45	10					4 44 57
	April 3.	Im. 1.	15 10 22	10	Mar. 29.	12 25 7	2		4 44 28
	April 19.	Im. 1.	13 26 27	10	April 12.	16 16 8	2		4 44 41
	Juue 6.	Em. 1.	10 26 22	10	June 8.	9 40 56	6		4 44 42
1770	May 1.	Im. 1.	12 42 32	10	Mar. 16.	17 2 47	6		4 45 13

At KITTERY POINT, in the province of MAIN, in PISCATAQUA Harbour.

			Apparent time.			
1771	April 11.	Im. 1.	15 43 30	12	I suppose the error of the times computed in the Nautical Almanac in 1771, when the weather allowed me to observe no immersions of this satellite, to be a mean between the errors in 1770 and 1772.	
	April 27.	Im. 1.	14 1 43	12		
	May 4.	Im. 1.	15 55 54	12		4 42 58

At PORTSMOUTH, in the Province of NEW HAMPSHIRE.

1772	Sept. 18.	Em. 1.	9 42 35	12	Sept. 27.	10 52 43½	3½	Air very clear.	4 43 2
	Oct. 11.	Em. 1.	10 5 4	12	Oct. 13.	9 17 4	3½	Air clear.	4 42 40
	Nov. 3.	Em. 1.	10 23 54	12	Oct. 20.	11 14 18	3½	Air clear.	4 42 51
	Nov. 12.	Em. 1.	6 48 1	12	Nov. 14.	5 59 28	6	Air clear.	4 42 59

The obſervations here referred to, made by Capt. HOLLAND and others of his party, are to be found in Philoſophical Tranſactions, Vol. LVIII. for the year 1768, and Vol. LIX. for 1769, and other papers, ſent to me from Capt. HOLLAND, which I have the honour of preſenting to the Royal Society, together with this paper.

I have re-computed the times of Enſign SPROULE's obſervations made at GASPEE BAY, applying the equation of correſponding altitudes, which he had neglected; and have annexed them to his account of his obſervations, tranſmitted to me by Capt. HOLLAND.

It appears, that the obſervations of Capt. HOLLAND, and his party, were ſometimes made with a two feet reflector of SHORT's conſtruction, and ſometimes with an achromatic teleſcope of DOLLOND of 10, and at other times, one of 12 feet. The obſervations at the ROYAL OBSERVATORY were alſo made with different teleſcopes, at different times; ſometimes with a two feet reflector of SHORT's conſtruction; ſometimes with a treble object glaſs teleſcope of DOLLOND, of $3\frac{1}{2}$ feet; but oftener with a 6 feet reflector, of SHORT's conſtruction. Theſe circumſtances are always noted againſt the obſervations: I have only made uſe of the obſervations of the firſt ſatellite, as being much more exact than thoſe of the others.

I reckon, that the 6-feet reflector ſhews an immerſion of the firſt ſatellite of Jupiter later, and an emerſion ſooner, by 20″, than a 2-feet reflector; or by 13″, than a $3\frac{1}{2}$-feet treble object glaſs refractor, or a 10 or 12-feet double object glaſs refractor of DOLLOND.

LOND. These allowances were accordingly made in deducing all the differences of meridians expressed in the last column of the foregoing table.

The observations made at GREENWICH, opposed to those made in NORTH AMERICA, are either corresponding ones, which however is very seldom the case, or else the nearest to them that were observed. The error of the computed time of the eclipse in the Nautical Almanac was found by the nearest observation at Greenwich, and the time of the Nautical Almanac, thus corrected, compared with the time observed in North America, gave the difference of meridians between the place and Greenwich; farther reduction being first made for the difference of telescopes, if necessary, in the manner already explained. It is however to be understood, that, in this way of deducing the difference of meridians, by comparing an eclipse observed at Greenwich with *another* eclipse observed elsewhere, the interval of time between the two eclipses ought to be very short; otherwise it cannot be supposed, that the errors of the tables should continue the same. Hence it has happened, that many of the North American observations have been lost for this use, for want of observations near enough to them made at Greenwich. In some other cases, indeed, I have been obliged to compare together observations made with a greater interval between them than I should have chosen, but for fear of losing some useful comparisons. As the preceding table affords several determinations of the longitude of most of the places, it may be proper to point out, how the true difference of meridians may be beſt deduced. To take a mean of the ſeveral reſults will not be the proper method, except the ſeveral obſervations had been either *all* immerſions, or *all* emerſions; but when both immerſions and emerſions have been obſerved, then the beſt method will be, to take a mean of all the reſults given by the immerſions, and a mean of all the reſults given by the emerſions, ſeparately; the mean of theſe two means will be the true difference of the meridians, and will be much more to be depended upon, than if it was deduced from immerſions or emerſions only; becauſe this method of compariſon removes the conſtant errors ariſing from the differences of the teleſcopes, air, and eyes of the diſtant obſervers; theſe cauſes of error affecting the ſeparate reſults of immerſions and emerſions, in contrary ways. I ſhall now only add the difference of meridians deduced from the obſervations contained in the preceding table, by taking a proper mean between the ſeveral reſults.

Meridian of

	h ′ ″	h ′ ″	
Place of obſervation on ST. JOHN's iſland,	4 11 49 by ſingle obſervation.		WEST of ROYAL OBSERVATORY at GREENWICH.
LOUISBOURG.	4 0 13 by mean of 4 E. 3 59 41 by mean of 2 I.	3 59 57 by mean of means.	
SOUTH POINT, entrance of GASPEE.	4 17 56 by 1 I. 4 17 45 by mean of 2 E.	4 17 50 by mean of 1 I. and mean of E.	
Capt. HOLLAND's houſe near QUEBEC.	4 44 50 by mean of 4 I. 4 44 42 by 1 E.	4 44 46 by mean of 1 E. and mean of I.	
KITTERY POINT, province of Main, in Piſcataqua harbour,	4 42 58 by mean of 3 I.		
PORTSMOUTH, NEW HAMPSHIRE.	4 42 53 by mean of 4 E.		

XV. *Immer-*

XXV. *Immersions and Emersions of* Jupiter's *first Satellite, observed at* Jupiter's *Inlet, on the Island of* Anticosti, North America, *by Mr.* Thomas Wright, *Deputy Surveyor General of Lands for the Northern District of* America; *and the Longitude of the Place, deduced from Comparison with Observations made at the Royal Observatory at* Greenwich, *by the Astronomer Royal.*

Redde, Jan. 20, 1774.

THESE observations were communicated to me some years ago by Mr. WRIGHT. They were made, as he informed me, at JUPITER'S INLET, two leagues to the westward of the south-west point of the island of ANTICOSTI, situated at the entrance of the river ST. LAURENCE, in latitude 49° 26″ North, with a two-feet reflecting telescope of the late Mr. SHORT's construction. The pendulum clock, made use of, vibrated half-seconds, and was regulated by equal altitudes of the sun, taken by reflection, from the surface of a fluid, with a HADLEY's sextant of 18 inches radius, having a brass arch and index, and ivory vernier, made by Messieurs HEATH and WING.

In

In computing the going of his clock from the corresponding altitudes of the sun, Mr. WRIGHT had neglected to apply the equation of the middle time, owing to the change of the Sun's declination between the morning and afternoon observations, commonly called *the equation of corresponding altitudes*. I have therefore made the proper allowance on this account, in computing the apparent time, on supposition, that the interval of time between the morning and afternoon observations was always four hours; Mr. WRIGHT having informed me, it was always from three to five hours; whence the equation may be allowed certainly within a second.

Mr. WRIGHT's observations of the eclipses of Jupiter's first satellite, thus corrected, are as follows:

1767		Apparent time. h. ′ ″	Time at Greenwich, per Naut. Almanac. h. ′ ″	Difference of meridians. h. ′ ″
Jan. 17.	Im.	14 50 27	19 5 29	4 15 2
Feb. 2.	Im.	13 2 21	17 17 53	4 15 32
18.	Im.	11 19 0	15 34 14	4 15 14
25.	Im.	13 13 46	17 29 16	4 15 30
Mar. 29.	Em.	12 10 59	16 25 27	4 14 28
April 5.	Em.	14 7 23	18 22 11	4 14 48
7.	Em.	8 36 19	12 51 16	4 14 57
14.	Em.	10 32 56	14 47 48	4 14 52
30.	Em.	8 54 17	13 8 59	4 14 42

The mean difference of meridians by the four immersions is $4^h\ 15'\ 19\frac{1}{2}''$, and by the five immersions is $4^h\ 14\ 45\frac{1}{2}''$; both which ought to be corrected, by the help of the nearest observations made at the Royal Obſervatory at Greenwich. The immerſions and emerſions obſerved there, proper to compare with the preceding obſervations, are theſe; all obſerved with a ſix-feet reflector, which, I reckon, ſhews an immerſion of the firſt ſatellite 20″ later, and an emerſion of the ſame as much ſooner, than a two-feet reflecting teleſcope.

1767		Obſerved at Greenwich. Apparent time.				App. time per Naut. Almanac.			Correction of Naut. Almanac.		
			h	′	″	h	′	″		′	″
Jan.	12.	Im.	11	41	41	11	42	8	—	0	27
Feb.	27.	Im.	11	57	7	11	58	6	—	0	59
Mar.	22.	Em.	14	28	48	14	28	50	—	0	2
April	9.	Em.	7	20	1	7	20	25	—	0	24
	14.	Em.	14	47	52	14	47	48	+	0	4
	16.	Em.	9	16	13	9	16	55	—	0	42
	30.	Em.	13	9	10	13	8	59	+	0	11
May	9.	Em.	9	32	26	9	33	12	—	0	46

The correction of the Nautical Almanac for a ſix feet reflector, by the mean of the two immerſions, is — 43″, which applied to $4^h\ 15'\ 19\frac{1}{2}''$, the longitude of Jupiter's inlet found from immerſions, by the help of the Nautical Almanac, gives $4^h\ 14'\ 36\frac{1}{2}''$, the difference of longitude deduced from the immerſions. The correction of the Nautical Almanac, by the mean of the ſix emerſions, is — $16\frac{1}{2}''$, which applied to $4^h\ 14'\ 45\frac{1}{2}''$, the longitude of Jupiter's inlet found by the emerſions, by the help of the Nautical Almanac, gives 4^h 14 29″, the longitude deduced from the emerſions. The mean of theſe two reſults, found from the immerſions and emerſions ſeparately, is $4^h\ 14'\ 33''$, the proper difference of longitude of JUPITER'S INLET

WEST

WEST of GREENWICH. I have here made no allowance for the difference of power of the two-feet reflector, used at Jupiter's inlet, and the six-feet reflector, used at Greenwich; because the mean is taken between the results from the immersions and emersions; which method includes that correction; that is to say, gives the same result whether that correction be made or not. From the foregoing comparisons it should seem that the air is much clearer at the island of ANTICOSTI than at GREENWICH, which Mr. WRIGHT confirmed to me, since the immersions give the longitude only $7\frac{1}{2}''$ greater than the emersions; which shews that Mr. WRIGHT observed an immersion only $4''$ sooner, and an emersion as much later, with a two feet reflector, than was done at Greenwich in a six feet reflector; although, in an equally good air, this latter telescope would have had the advantage of the former by $20''$ instead of $4''$.

XXVI. *Extract of a Letter from Mr.* Humphry Marſhall, *of* Weſt Bradford, *in* Cheſter County, Pennſylvania, *to Dr.* Franklin, *ſent with Sketches of the Solar Spots, dated* May 3, 1773.

Redde, Feb. 3, 1774.

HAVING for ſome time declined making any more obſervations, on the dark ſpots that appear on the Sun's diſk, I now ſend a copy of the figures, I drew of them; which I deſire may be preſented to the Royal Society. Perhaps ſome one or more of the members may be pleaſed with them, in which caſe, I ſhall not think my labour loſt. They were viewed with a reflecting teleſcope of inches, and their appearances, I think, pretty truly delineated, both as to magnitude and ſituation. Upon the whole, I am of opinion, that the ſpots are near the Sun's ſurface, if not cloſely adhering thereto, for theſe reaſons; 1. That their velocities are apparently greateſt near the center, and gradually ſlower towards each limb. 2. That the ſhape of the ſpots varies, according to their poſition on the ſeveral parts of the Sun's diſk; thoſe that appear broad, and nearly round, when on the middle, ſeeming, at their firſt appearance on the eaſtern limb, but as lines; and, as they advance towards

wards the center, grow oval, then round, and, in their progreſs to the weſtern limb, appear again as ovals and lines. My other remarks were, that the ſpots were twelve days and an half, and about two or three hours, in paſſing; that, though ſome continued viſible from one limb to the other, a few would diſappear, after having been viſible ſeveral days; and others divided into parts; that ſcarce any ſpots ever appeared beyond what may be called the polar circles of the ſun; and that the ſame ſpot never appeared, a ſecond time, on the eaſtern limb, at leaſt not in the ſame form and poſition.

The figures of the ſolar ſpots, mentioned in this letter, are ſketches with black lead pencil, upon a very ſmall ſcale. They are accompanied with ſhort notes of the ſtate of the weather at the time of each obſervation, and ſometimes the height of the thermometer is mentioned. Among theſe meteorological remarks, the following ſeems the moſt extraordinary.

February 21ſt, 1773, Thermometer at 3 degrees below 0 at Sun-riſe. This morning, had there been a ſnow on the ground, I believe it would have been as cold as it was January 2d, 1767, when the thermometer was 22 degrees below 0, there being a large ſnow on the ground at that time, and none now.

XXVII. *Account of the Houſe-martin, or Martlet. In a Letter from the Rev.* Gilbert White *to the Hon.* Daines Barrington.

DEAR SIR,

Redde, Feb. 10, 1774.

IN obedience to your injunctions, I ſit down to give you ſome account of the HOUSE-MARTIN, or MARTLET: and if my monography, of this little domeſtic and familiar bird, ſhould happen to meet with your approbation, I may probably ſoon extend my enquiries to the reſt of the Britiſh *hirundines*, the ſwallow, the ſwift, and the bank-martin.

A few houſe-martins begin to appear about the 16th of April; uſually ſome few days later than the ſwallow. For ſome time after they appear, the *hirundines* in general pay no attention to the buſineſs of nidification; but play and ſport about, either to recruit from the fatigue of their journey, if they do migrate at all; or elſe, that their blood may recover its true tone and texture, after it has been ſo long benumbed by the ſeverities of winter. About the middle of May, if the weather be fine, the martin begins to think, in earneſt, of providing a manſion for its family. The cruſt, or ſhell of this neſt, ſeems to be formed of ſuch dirt, as comes moſt readily to hand, and is tempered and wrought together

together with little bits of broken ſtraws, to render it tough and tenacious. As this bird often builds againſt a perpendicular wall, without any projecting ledge under, it requires its utmoſt efforts, to get the firſt foundation firmly fixed, ſo that it may ſafely carry the ſuperſtructure. On this occaſion, the bird not only clings with its claws, but partly ſupports itſelf by ſtrongly inclining its tail againſt the wall, making that a *fulcrum*; and thus ſteadied, it works and plaiſters the materials into the face of the brick or ſtone. But then, that this work may not, while it is ſoft and green, pull itſelf down by its own weight, the provident artichitect has prudence and forbearance enough, not to advance its works too faſt; but by building only in the morning, and by dedicating the reſt of the day to food and amuſement, gives it ſufficient time to dry and harden. About half an inch ſeems to be a ſufficient layer for a day. Thus careful workmen, when they build mud-walls, informed at firſt perhaps by this little bird, raiſe but a moderate layer at a time, and then deſiſt; leaſt the work ſhould become top-heavy, and ſo be ruined by its own weight. By this method, in about ten or twelve days, is formed an hemiſpheric neſt, with a ſmall aperture towards the top; ſtrong, compact, and warm; and perfectly fitted for all the purpoſes, for which it was intended: but then nothing is more common than for the houſe-ſparrow, as ſoon as the ſhell is finiſhed, to ſeize on it, as its own, to eject the owner, and line it after its own manner.

Though ſo much labour is beſtowed in erecting a manſion, yet as Nature ſeldom works in vain, martins

tins will breed on, for ſeveral years together, in the ſame neſt, where it happens to be well ſheltered, and ſecure from the injuries of weather. The ſhell, or cruſt, is a ſort of ruſtic work, full of knobs and protuberances on the outſide: nor is the inſide, of thoſe that I have examined, ſmoothed with any exactneſs at all; but is rendered ſoft and warm and fit for incubation, by a lining of ſmall ſtraws, graſſes, and feathers, and, ſometimes, by a bed of moſs, interwoven with wool. In this neſt they tread or engender frequently, during the time of building; and the hen lays from three to five eggs. At firſt when the young are hatched, and are in a naked and helpleſs condition, the parent birds, with tender aſſiduity, carry out what comes from their young. Was it not for this affectionate cleanlineſs, the neſtlings would ſoon be burnt up, and deſtroyed, in their own cauſtic excrement. In the quadruped creation the ſame neat precaution is made uſe of; particularly among dogs and cats, where the dams lick away what proceeds from their young. But, in birds, there ſeems to be a particular proviſion, as the dung of neſtlings is enveloped in a tough kind of jelly, and therefore is the eaſier conveyed off without ſoiling or daubing; yet, as Nature is cleanly in all her ways, the young perform this office for themſelves in a little time, by thruſting their tails out at the aperture of the neſt. As the young of ſmall birds preſently arrive at their ἡλικια, or full growth, they ſoon become impatient of confinement, and ſit all day with their heads out at the orifice, where the dams, by clinging to the neſt, ſupply them with food from morning to night.

For a time the young are fed, on the wing, by their parents; but the feat is done by so quick, and almost imperceptible, a flight, that a person must have attended very exactly to their motions, before he would be able to perceive it. As soon as the young are able to shift for themselves, the dams immediately turn their thoughts to the business of a second brood; while the first flight, shaken off and rejected by their nurses, congregate in great flocks, and are the birds that are seen clustering and hovering, on sunny mornings and evenings, round towers and steeples, and on the roofs of churches and houses. These congregatings usually take place first, about the first week in August; and therefore we may conclude, that, by that time, the first flight is pretty well over.

It has been observed, that martins usually build to a N. E. or N. W. aspect, that the heat of the Sun may not crack and destroy their nests; but instances are also remembered, where they bred for many years, in vast abundance, in a hot stifled inn-yard, against a wall facing to the S.W. and W. Birds in general are wise in their choice of situation; but, in this neighbourhood, every summer, is seen a strong proof to the contrary, at an house without eaves, in an exposed spot; where some martins build, year by year, in the corners of the windows; but, as the corners of these windows, which face to the S.S.E. and S.W. are too shallow, the nests are washed down every hard rain; and yet these birds drudge on to no purpose, from summer to summer, without changing their aspect or house. It is piteous to see them labouring and bringing dirt, when half their

nest

neſt is waſhed away—"*generis lapſi ſarcire ruinas.*" Thus is inſtinct a moſt wonderful, but unequal faculty; in ſome inſtances ſo much above reaſon, in other reſpects ſo far below it!

Martins love to frequent towns, eſpecially if there are great lakes and rivers at hand: nay, they even affect the cloſe air of London. And I have not only ſeen them neſting in the Borough, but even in the Strand and Fleet-ſtreet; but then it was obvious, from the dingineſs of their aſpect, that their feathers partook of the filth of that ſooty atmoſphere.

Martins are by far the leaſt agile of the four ſpecies. Their wings and tails are ſhort; and therefore they are not capable of ſuch ſurprizing ſhort turns, and quick and glancing evolutions, as the ſwallow. Accordingly they make uſe of a placid eaſy motion, ſeldom mounting to any great height, and never ſweeping long together over the ſurface of the ground or water. They do not wander far for food, but affect ſheltered diſtricts over ſome lake, or under ſome hanging wood, or in ſome hollow vale, eſpecially in windy weather. They breed the lateſt of all the ſwallow *genus*: in 1772 they had neſtlings on to October 21ſt, and are never without unfledged young, as late as Michaelmas. As the ſummer declines, the congregating flocks increaſe in number daily, by the conſtant acceſſion of the ſecond broods; till at laſt they ſwarm, in myriads upon myriads, round the villages on the Thames, darkening the face of the ſky, whilſt they frequent the eyetts of that river, where they rooſt. They retire, for the bulk of them, in vaſt flocks together about the

beginning

beginning of October; but have appeared of late years, in a confiderable flight, in this neighbourhood, for one day or two, as late as November the 3d and 6th; after they were fuppofed to have been gone for more than a fortnight. They therefore withdraw with us the lateft of any fpecies. Unlefs thefe birds are very fhort-lived indeed, or unlefs they do not return to the diftrict where they are bred, they muft undergo vaft devaftations fome how, and fome where; for the birds, that return yearly, bear no manner of proportion to the birds that retire.

Houfe-martins are diftinguifhed from their *congeners*, by having their legs covered, with foft downy feathers, down to their toes. They are no fongfters; but twitter in a pretty inward foft manner in their nefts: are greatly molefted in their nefts by fleas; and annoyed by a large dipterous infect, with narrow fubulated wings, which crawls about under their feathers, and is known by the name of *hippobofca hirundinis:* a fpecies of which is familiar to horfemen under the name of foreft-fly; and to fome under the name of fide-fly, as it runs fideways like a crab, creeping under the tails and about the groins of horfes.

Thus does all the creation prey upon one another; and thefe birds, though infect-eaters themfelves, are diftreffed and tormented by infects.

I am, DEAR SIR, with the greateft refpect,

Your obliged and moft obedient fervant,

Selborne,
Nov. 20, 1773.

GIL. WHITE.

XXVIII. *Ex-*

XXVIII. *Extract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon *in* Rutland, 1773, *by* T. Barker, *Esq; Communicated by Sir* John Pringle, *Bart. P. R. S.*

Redde, February 17, 1774.

		Barometer.			Thermometer.						Rain.
					In the house.			Abroad.			
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean	
Jan.	Morn.	29.96	28.30	29.40	45½	33	40	45	22	35	1.131
	Aftern.				46	35	41	50	29	40	
Feb.	Morn.	30.24	28.30	29.48	44	31½	38	46	19½	32	1.458
	Aftern.				45	33	39	50	29	39	
Mar.	Morn.	30.08	29.28	29.75	52	39½	44	46	25½	35½	0.561
	Aftern.				53	40	45	62	37	47	
April	Morn.	30.10	28.63	29.48	53	39½	47	49	28	40	0.603
	Aftern.				54	40½	48½	62	38½	51	
May	Morn.	30.04	29.14	29.47	60½	45½	51	56½	31	44	6.843
	Aftern.				63	47	52	70	41	54½	
June	Morn.	29.86	29.03	29.53	64	55	58½	61	46	53	2.389
	Aftern.				65	55½	59½	75	52½	63½	
July	Morn.	29.93	29.36	29.66	65	54	59½	63	46	54½	1.077
	Aftern.				69	56	61½	73½	58	66½	
Aug.	Morn.	29.87	29.13	29.63	68½	57	62	61	48	55	3.379
	Aftern.				73	59	65	81	59	69½	
Sept.	Morn.	29.73	28.77	29.39	63½	51	56	56½	36½	49	2.812
	Aftern.				64	52	57½	70	53	60	
Oct.	Morn.	29.92	28.71	29.40	57	49	52	56	32	44	2.621
	Aftern.				57½	49½	53	60½	43	53	
Nov.	Morn.	29.89	28.00	29.18	50	37	44	52	23	37	3.605
	Aftern.				51	37½	45	56½	31	42	
Dec.	Morn.	29.80	28.62	29.23	45½	36½	41½	45	27	36½	2.897
	Aftern.				46	37	42	49	31	39½	
											29.376

The

The firſt part of 1773 was very favourable; the winter was mild; there were froſts, but no ſevere ones. The latter part of February was ſtormy, and wet; but, from thence to the beginning of May, there was a great deal of fine weather, dry, and a very good ſeed time; and the grain came up very friendly: yet the dryneſs of the ſeaſon, and ſeveral froſty mornings, even till near the middle of May, made the graſs, and other things, very backward.

The beginning of May many were apprehenſive of a drought, and the firſt rains were very acceptable; but ſo exceedingly wet a fortnight followed, with great floods, and the fens drowned, that all were as much tired of rain, as before they deſired it, and the crops of grain never recovered it. In the reſt of the year there were many fits of great wet, but intermixed with fits of fair and fine. The wet of May was beyond example at that time of year. Juſt the end of May, and almoſt half June, were pretty fine; but then came wet again, till the beginning of July. The longeſt fine ſeaſon was the chief part of July and Auguſt, and in that time the great crop of hay was well got, as alſo the firſt part of harveſt; and ſome rains, at times, kept things in a growing ſtate. The latter part of harveſt, in September, was wet, but not ſo bad as laſt year, and the harveſt was more hindered than hurt by it. The wheat ſeed-time, in the middle of October, was good; but moſt part of the autumn was wet, eſpecially the latter part of October, and firſt half of November. Toward the latter part of November, there was a ſmart, but ſhort, froſt; and another longer, but more broken, the beginning of December;

ber; and, by the middle of that month, the ground was got conſiderably drier than it had been; but it grew very wet again, and miſty, till three ſharp froſty days concluded the year.

This year was much too wet for corn; the barley was almoſt every where bad; the wheat and beans looked better in the field; but moſt ſorts of grain, when they came to be threſhed, this year yielded leſs corn, than was expected; and it continues to be dear, and is too likely to be ſo, while the ſeaſons are ſo wet as they have of late years been.

XXIX. *An Account of certain Receptacles of Air, in Birds, which communicate with the Lungs, and are lodged both among the fleſhy Parts and in the hollow Bones of thoſe Animals. By* John Hunter, *F. R. S.*

Redde, Feb. 27, 1774.

THE ſingular communications which are found to ſubſiſt, in birds, between the cavities of the lungs, and certain other cavities in the fleſhy parts and in the bones, being peculiar to that tribe of animals, and having never yet been ſufficiently explained, nor perhaps attended to, either by anatomiſts or natural hiſtorians; I imagine, that an account of them will not be unacceptable to this ſociety. It is not my preſent deſign, to enter into minute deſcriptions of all the particular communications of this ſort, to be found in the diſſection of theſe animals; but only to mention ſuch general facts, as will be ſufficient to introduce the ſubject into natural hiſtory, and ſerve to open the enquiry into the final cauſe.

To make this matter more intelligible, I muſt previouſly give an idea of the difference, between the particular cells in queſtion, and thoſe other cells of the cellular membrane, common to all animals; and

and alſo ſhew wherein thoſe bones which receive air, differ from ſuch as do not.

The air-cells, which are found in the ſoft parts of birds, have no communication with the cavity of the common cellular membrane of the body: ſome of them communicate immediately with one another; and all of them may be ſaid to have a communication together, by means of the lungs, as a common centre. Some of theſe are bags placed in larger cavities, ſuch as the *abdomen*; others are ſo lodged in the interſtices of parts, that they would, at firſt, appear to be the common connecting membrane, as about the breaſt, *axilla*, &c.

They are of very different ſizes, juſt as beſt ſuits the circumſtances of the particular parts, where they are placed.

The bones which receive air are of two kinds; ſome, as the *ſternum*, ribs, and *vertebræ*, have their internal ſubſtance divided into innumerable cells; whilſt others, as the *os humeri* and the *os femoris*, are hollowed out into one large canal, ſometimes with a few bony columns running acroſs, at the extremities. Bones of this kind may be diſtinguiſhed from thoſe that do not receive air, by ſeveral marks; 1ſt, by their leſs ſpecific gravity; 2dly, by being leſs vaſcular than the others, and therefore whiter; 3dly, by their containing little or no oil, and conſequently being more eaſily cleaned; and appearing much whiter, when cleaned, than common bones; 4thly, by having no marrow, nor a bloody, pulpy ſubſtance, even in their cells; 5thly, by their not being, in general, ſo hard and firm as other bones; thoſe of ſome birds are ſo ſoft, that they can be

ſqueezed

ſqueezed together with the finger and thumb: however, the bones of the extremities have very ſolid ſides. 6thly, the paſſage by which the air gets into the bones can be eaſily perceived, even in cleaned bones. Generally there are ſeveral holes, placed together, near the end of the bone which is next the trunk of the bird; and diſtinguiſhable, by having their external edges rounded off; which is not the caſe with thoſe holes, through which either nerves or blood veſſels paſs, into the ſubſtance of the bone.

I muſt next give an idea of the mechaniſm of the lungs in birds, which renders them fit for communicating air to the above deſcribed parts. This conſiſts principally in certain connexions.

It has been aſſerted, that birds have no diaphragm; but this opinion muſt have ariſen either from a want of obſervation, or from too confined an idea of a diaphragm; for there is a pretty ſtrong, but thin and tranſparent, membrane, which covers the lower ſurface of the lungs, and adheres to them; this membrane gives inſertion to ſeveral thin muſcles, which ariſe from the inner ſurfaces of the ribs.

The uſe of this part is to leſſen the concavity of the lungs towards the *abdomen*, at the time of inſpiration; and thereby to aſſiſt in dilating the air-cells; for which reaſon, it is to be conſidered as anſwering one main purpoſe of a diaphragm.

Beſides this attachment of the lungs to the diaphragm, they are alſo connected to the ribs and ſides of the *vertebræ*.

Theſe adheſions are peculiar to this tribe of animals; and are of ſingular uſe, or rather of abſo-

lute

lute neceſſity, in ſuch lungs as thoſe of birds, out of which the air can find a paſſage into other cavities; for if the lungs were looſe in the cavity of the *thorax*, as is the caſe in all other animals, their cells could not be expanded, either by the depreſſion of the diaphragm, or the elevation of the ribs; ſince the air ruſhing in, to fill up the *vacuum* in the cavity of the cheſt, occaſioned by theſe actions, would take the ſtraight road from the *trachea* through thoſe paſſages; and, of conſequence, expand no part of the lungs which lay out of that line, whereby reſpiration would be totally prevented; it would be exactly the ſame caſe, as when the lungs are ſo much wounded, in other animals, as to allow of a free paſſage for the air into the cavity of the *thorax*, &c.

Of the internal openings of the lungs.

The openings in the lungs, by which they communicate air to the other parts, are as follow:

The membrane, or diaphragm above mentioned, is perforated in ſeveral places, with pretty large holes, which admit of a free paſſage, between the cells of the lungs and the *abdomen*. A communication which has frequently been noticed.

To each of theſe perforations is joined a diſtinct membranous bag, which is extremely thin and tranſparent; it receives the air, and being afterwards continued through the *abdomen*, gets attachments to many of thoſe parts with which it comes in contact.

There is no occaſion to deſcribe here all theſe bags, or their attachments; it being ſufficient to ſay, that they extend over the whole *abdomen*.

The lungs open at their anterior part, that is, towards the *ſternum*, into certain membranous cells which lie upon the ſides of the *pericardium*, and communicate with the cells of the *ſternum*.

The ſuperior part of the lungs opens into the large cells of a looſe net-work, through which the *trachea*, *oeſophagus*, and large veſſels, going from, and coming to the heart, paſs.

When theſe cells are diſtended with air, it encreaſes the ſize of that part, where they lie, very conſiderably; which, in general, is a mark of paſſion; as appears evidently in the turkey-cock, the pouting-pidgeon, &c. and is extremely viſible, in the breaſt of a gooſe, when ſhe cackles.

Theſe cells communicate with others in the *axilla*, under the large pectoral muſcle, &c: and thoſe again with the cavity of the *os humeri*, by means of ſmall openings, in the hollow ſurface, near the head of that bone.

The poſterior edges of the lungs, which lie on the ſides of the ſpine, and project backwards between the ribs, open into the cells of the bodies of the *vertebræ*, thoſe of the ribs, the canal of the *medulla ſpinalis*, the cells of the *ſacrum*, and other bones of the *pelvis*; from which parts the air finds a paſſage to the cavity of the thigh-bone.

This account agrees with what we find in moſt birds; though ſome have more, and ſome fewer of theſe communications.

In the OSTRICH, no air gets into the *os humeri*; but it enters every other part, as defcribed above, in very large quantities. In the common FOWL, no air appears to enter any bone but the *os humeri*. The WOOD-COCK has none either in the firft bone of the wing, or in the thigh-bones. On the other hand, in the PELICAN, the air paffes on to the *ulna* and *radius*, and into thofe bones which anfwer to the *carpus* and *metacarpus* of *quadrupeds*.

Thus then, the *abdomen*, the cells furrounding the *pericardium*, the lower and fore part of the neck, the *axilla*, the cellular membrane under the pectoral mufcles, &c. all communicate with the lungs, and are capable of being filled with air; and again, from thofe cells the *fternum*, ribs, *vertebræ* of the back and loins, bones of the *pelvis*, *offa humerorum*, and *offa femorum*, can, in many birds, be furnifhed with air.

This fupply, of the bones with air, is not wholly by means of the lungs; for the cells of the bones of the head, in fome birds, are filled with it, of which the OWL is a remarkable inftance. In this bird, the *diploe* between the two plates of the fcull is cellular, and admits a confiderable quantity of air, which is furnifhed by the Euftachian tube.

Some authors confidered the *diploe*, in the *cranium* of a bird, as a continuation of the mamillary procefs *; and have looked upon it as a circumftance peculiar to finging birds; but this is not the cafe.

* The only thing fimilar to this communication, in birds, of the cells of bones with the external air, is that of the internal ear of *quadrupeds*, by the Euftachian tube.

The lower jaw of the PELICAN is alſo furniſhed with air; but by what means I do not know.

Having formerly obſerved theſe facts, I made ſeveral experiments in the year 1758, upon the breathing of birds, to prove the free communication between the lungs and the abovementioned parts.

The firſt was upon a COCK. I made an opening into the belly of this animal, and introduced a ſilver *cannula*; then tied up the *trachea*, and found that he breathed by this opening, and lived; but an inflammation came on, in the bowels, which produced adheſions, and cut off the communication.

I cut the wing through the *os humeri*, in another fowl, and tied up the *trachea* as in the cock; and found that the air paſſed to and from the lungs by the canal in this bone; the ſame experiment was made with the *os femoris* of a young HAWK, which was attended with nearly the like ſucceſs; but the paſſage of air, through both theſe parts, eſpecially the laſt, was attended with more difficulty than in the firſt experiment; indeed ſo much, as to render it impoſſible for the animal to live longer, than to prove evidently, that he did breathe through the cut bone.

The exceeding ſingularity of theſe communications, in birds, put me upon thinking, what could be the final cauſe. At firſt I ſuſpected, that it might be intended for the benefit of flying, that being the circumſtance which appears the moſt peculiar to birds; and it might be of ſervice in this reſpect, I thought, by encreaſing the volume and ſtrength, with the ſame quantity of matter, and therefore without adding to the weight of the whole; which will rather indeed be diminiſhed by the difference of ſpecific gravity, between the external and internal air. This opinion was ſtrengthened, by obſerving, that the feathers of birds alſo contain a conſiderable quantity of air, and in the very part which requires the greateſt ſtrength; as likewiſe by the analogy of fiſh, which have air contained in their bodies, to leſſen their ſpecific gravity; though the uſe of this, in fiſh, which are to move in a much heavier element, is more obvious than in birds.

But when I found the OSTRICH, which is not intended to fly, endowed with nearly the ſame conſtruction; and that the common FOWL, and many others of that claſs, which do fly; and alſo the WOOD-COCK, which flies, and is ſuppoſed to be a bird of paſſage, are not ſo well provided with air, as the OSTRICH; and that the BATT, which flies, differs not, in that way, from animals that do not fly; ſo many contradictions to the theory, obliged me to think of ſome other uſe, for this ſingular mechaniſm.

The next conjecture, that offered itſelf, was, that theſe parts were to be conſidered as an appendage to the lungs. The analogy of *amphibious* animals leads to this; for, in many of them, the ſnake, viper, &c. the lungs are continued down through the whole belly, in form of two bags, the upper part only of which can do the office of reſpiration; and therefore, the remainder is to be conſidered as a reſervoir of air. Now there is a great ſimilarity between birds and that claſs of animals,

mals, called *amphibious*; and although a bird and a ſnake are not the ſame, in the conſtruction of the reſpiratory organs, yet the circumſtance of the air paſſing beyond the lungs, into the cavity of the *abdomen*, in both, naturally leads us to ſuppoſe, that ſo ſimilar a ſtructure is deſigned for the ſame purpoſe in both; and this analogy is further confirmed by the texture of the lungs in both, which conſiſt of large cells. Now in amphibious animals, the uſe of this conformation of the lungs is evident; for it is in conſequence of this, that they can breathe leſs frequently than others. Conſidering the matter in this light, it may ſtill, in birds, have ſome connexion with flying; for that motion may eaſily be imagined, to render frequency of reſpiration difficult; and may, therefore, make a reſervoir of air ſingularly uſeful.

It may, perhaps, occur to ſome, that the whole of theſe communicating cells are to be conſidered as extended lungs; but I can hardly think, that any air, which gets beyond the veſiculated lungs themſelves, is capable of affecting the blood of the animal; as the other cavities, into which it comes, as well thoſe of the ſoft parts, as of the bones, are very little vaſcular.

How far this conſtruction, of the reſpiratory organs, may aſſiſt birds in ſinging, is worthy of conſideration, as the vaſt continuance of ſong, between the breathings, in a CANARY-BIRD, would appear to be owing to this cauſe.

At preſent, I ſhall purſue this ſubject no farther; but leave it to a ſubſequent paper, as requiring a great many comparative facts, to eſtabliſh the final cauſe.

XXX. *M.* DE LUC's *Rules, for the measurement of Heights by the Barometer, compared with Theory, and reduced to* English *Measures of Length, and adapted to* Fahrenheit's *Scale of the Thermometer: with Tables and Precepts, for expediting the practical Application of them. By* Samuel Horsley, *LL.D. addressed to Sir* John Pringle, *Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR,

Redde, Feb. 17, 1774.

IN the papers which I have the honor to present to you, nothing more was at first intended, than to reduce the *formulæ* given by Mr. DE LUC, in his elaborate work upon *the modifications of the atmosphere*, for expressing differences of elevation, as indicated by the barometer and thermometer, in Paris toises, to others, which, from the like *data*, should express such differences in English fathom; and at the same time be adapted to that scale of the thermometer, which is in general use in this country. Had I confined myself to this, which was my original

ginal deſign, I ſhould certainly have ſuppreſſed whatever I might have executed, as I find my learned friend the ASTRONOMER ROYAL hath devoted ſome of his leiſure hours to theſe calculations; and the reductions, I propoſed, are actually performed, in a ſhort and elegant paper of his upon the ſubject, which is already, I believe, in Dr. MATY's hands. But I had made but a ſmall progreſs in my intended work, when it occurred to me, that *tables of the equations* for the effects of heat and cold, would be very uſeful. Such I have taken the pains to conſtruct, and have carried them to as great a length, as can be ever wanted. They are annexed to the enſuing piece, and will render the application of Mr. DE LUC's rules more eaſy and expeditious, than any peculiar diviſions of the thermometrical ſcale. When my tables were finiſhed, I thought it might be ſtill further uſeful, to give a ſuccinct explanation of Mr. DE LUC's orignal *formulæ*; that ſuch as have not leiſure to peruſe his excellent work, might be furniſhed with a competent idea of the reſult of his reſearches. This, I found, I could not do, in any way ſo ſatisfactory to myſelf, as by opening, as I went along, the principles of theory, in which the concluſions, he hath arrived at, appear to originate. And thus I was inſenſibly led into minute diſquiſitions, concerning the agreement of Mr. DE LUC's concluſions, from a long train of accurate experiments, with the geometrical theory of the atmoſphere, founded on the general laws of gravitation. In this manner, SIR, the papers before you have taken, as it were ſpontaneouſly, the form, in which they now appear; and a ſubject, remotely connected

with

with my firſt deſign, takes up the far greater part of them. They can hardly be free from the imperfections naturally incident to productions, in which the plan hath been gradually changed, during the actual progreſs of the work. I flatter myſelf, however, that they are not deficient in two eſſential points, the preciſion and the perſpicuity of the mathematical reaſoning; and that, however unfiniſhed in ſome particulars, they are ſuch upon the whole, as the dignity of the ſubject may, in ſome degree, commend to your protection. There is perhaps no branch of phyſical enquiry, intrinſically more ſublime and intereſting, nor likely to be more important in its uſes, than that which immediately regards the conſtitution of that elaſtic fluid, which ſurrounds our globe, and appears to be a principal agent, in many of the moſt ſtriking operations of nature, and a neceſſary inſtrument, at leaſt, in carrying on the wonderful buſineſs of vegetation and of animal life. Upon a ſubject of ſo much importance, it muſt afford ſatisfaction, to find ſo exact an agreement, as is evinced, if I am not much deceived, in the enſuing pages, between a multiplicity of experiments, not ſuggeſted by any previous ſpeculations of theory, with a theory, whoſe concluſions in this branch had never before been duly ſubmitted to the teſt of experiment.

The whole of the following paper is divided into ſix ſections. The buſineſs of the firſt, is merely preparation for the principal work. It contains a brief account of the ſum of Mr. DE LUC's reſearches concerning the *variation of the heat of* *boiling water*, ſo far only as they reſpect the conſtruction and compariſon of mercurial thermometers; and it exhibits an actual compariſon of the ſcale of M. DE LUC's thermometer, with that which is likely to continue in general uſe here. In the ſecond, I ſtate the general principles of meaſuring heights by the barometer; or in other words, of the modification of the air, with reſpect to its condenſations, at different heights, by the preſſure of the ſuperior parts, excluſive of every other cauſe. Theſe principles are recapitulated for the ſake of perſpicuity and order; but, as they are generally well known, I refer to former writers for the demonſtration of them. This ſection concludes with the expoſition of M. DE LUC's fundamental rule. The third ſection treats of the equation depending on the difference of temperature of the quickſilver in the barometers, at the different places of ſynchronous obſervation. It explains, whence the neceſſity of this correction ariſes, and conſiders a caſe, in which the application of it requires ſome particular attentions; namely, the levelling a tract of flat country by the barometer. The fourth ſection explains the equation depending on the temperature of the air. This is likewiſe traced to its origin in theory; and upon this occaſion, I enquire into the condenſations of a fluid, unequally elaſtic in its different parts, under different degrees of compreſſive force. The fifth ſection contains the reduction of M. DE LUC's *formulæ*. I have found, with pleaſure, by the peruſal of Mr. MASKELYNE's paper, that we agree in our final concluſions. To this ſection, I have ſubjoined two problems; the one for eſtimating the variations which the denſity of the air, at any place, undergoes; and the other for determining the ſpecific gravity of the air of any temperature, at any elevation. The ſixth and laſt ſection points out certain conſequences, which ſeem to follow from the diſcoveries already made in conjunction with the theory eſtabliſhed. Theſe are only propounded to awaken curioſity, and promote enquiry. I am well aware, how little theory is to be truſted, in its *remote* concluſions, on account of the neceſſary deficiencies of the phyſical *data*, upon which its reaſonings are founded. The true uſes of it are, either to explain the mutual connexions and dependencies of things already known, or to ſuggeſt *conjectures* concerning what is unknown, to be tried by future experiment. And he who applies it, with due circumſpection, to theſe purpoſes, will always find it an uſeful engine. I flatter myſelf, that I have aſſigned the true cauſe of ſome very ſingular phænomena, remarked by M. DE LUC. I have endeavoured, to treat every part of my ſubject, in the plaineſt manner, that the nature of it would admit; and ſo diffuſely, as to be, I hope, intelligible to all, who are moderately well founded in the mathematical ſciences; for I have obſerved with pleaſure, that M. DE LUC's book hath raiſed a general curioſity upon the points it treats of; and for that reaſon, it ſeemed the more neceſſary, to depart in this inſtance, from a practice of late become too general, with mathematicians, *to write only for one another.* To which, if I am not miſtaken, it is in great meaſure owing, that theſe noble and uſeful ſtudies are leſs generally attended to, than they were in former times; when men of eminence,

eminence, in this, as well as in all other branches, placed their principal glory, no leſs in communicating knowledge, than in acquiring, or ſeeming to poſſeſs it; and were content to dedicate large portions of their time, to the removing of thoſe difficulties for others, which they had once ſurmounted for themſelves. It is true, that in the variety of mathematical diſcuſſions, there are ſome of ſuch a nature, that it would be a deſperate attempt to render them intelligible to any but mathematicians; to thoſe, who to the natural faculty of combining, have added the mechanical and acquired habits of analytic calculation. Such ſubjects are beſt treated with ſcientific brevity. But this is no reaſon for wrapping up others in a ſimilar ſtile; and I muſt obſerve, that *concluſions*, at leaſt, are always ſuſceptible of ſimple perſpicuous expoſition, however abſtruſe the mode of inveſtigation, in ſome caſes, may neceſſarily be, by which they are brought out.

Such, SIR, is the general ſcope and plan of the following piece, which is preſented to you, in teſtimony of the author's reſpect and gratitude.

I have the honour to be, SIR,

your much obliged,

and moſt obedient ſervant,

Temple,
Nov. 18, 1773.

SAMUEL HORSLEY.

SECTION FIRST.

Of the VARIATION of the point of BOILING WATER, and the COMPARISON of THERMOMETERS.

THE degree of heat wherewith any fluid boils, is invariably the ſame, under a given preſſure; but if the preſſure be diminiſhed or increaſed, the boiling heat is diminiſhed or increaſed.

Water, placed under the exhauſted receiver, would be converted into ſteam, with a degree of heat, far inferior to that, which is neceſſary to its boiling in the open air; and under the preſſure of its own vapour, confined in PAPIN's digeſter, it is ſaid to ſuſtain a degree of heat, without boiling, far exceeding that, which, in the open air, would convert it into ſteam.

Hence it follows, that in climates, where the preſſure of the atmoſphere is liable to conſiderable change, the heat of boiling water, in the open air, will be different, at different times. Conſequently, thermometers, made in different ſtates of the barometer, will diſagree; unleſs allowance hath been made, for the effect of the variation of the barometer, upon accurate principles.

If care were taken to adjuſt the boiling point, to the mean height of the barometer, in every country, the inſtruments of the ſame country would always be conſiſtent; but thoſe of different countries would ſtill diſagree; that is, they would expreſs the ſame temperature differently, though their fundamental intervals ſhould be ſimilarly divided; for, in every ſcale, the number of degrees above or below melting ice, by which any given temperature is expreſſed, will be as the value of each degree inverſely; that is, if each be a given part of the fundamental interval, as the value of the fundamental interval inverſely; but, if the degrees of different ſcales be different parts of the fundamental intervals, as the value of the fundamental interval inverſely, and the number of degrees contained in it directly.

It is neceſſary here to explain ſome of my expreſſions. By the *fundamental interval* is to be underſtood the whole extent of the ſcale between melting ice and boiling water. This is not a particular *length*; for its length is not the ſame, even in thermometers made in the ſame ſtate of the barometer, unleſs their figures be equal and ſimilar; but it is a portion of the whole *ſolid content* of the thermometer, including ſo much of the tube, as reaches up to the point of melting ice; and it is the ſame portion of the whole ſolid content in all made in the ſame ſtate of the barometer; but in ſuch as have been made in different ſtates, a different portion in each. The degrees of the ſcale are ſubdiviſions of this portion of ſolid content, into leſſer aliquot parts. With reſpect to the number of theſe ſubdiviſions, the practice of different countries is different. By the *value* of the fundamental interval, or of the degrees of a thermometer, I mean the fractions, which expreſs what parts they are reſpectively of the whole ſolid content, terminated as above; that is, by the point of melting ice: and by the proportion of the fundamental intervals, or of the degrees, of different inſtruments, compared together, I underſtand the proportion of theſe fractions,

tions. Or every thing may be reduced to a comparifon of lengths, if in comparing the inftruments of different countries, the comparifon be imagined to be made between two of equal dimenfions, and fimilar figure, fetting afide that part of each which is above the point of melting ice.

To compare the thermometers, therefore, of different countries, the proportions of their fundamental intervals to each other muft be afcertained; or, we muft have fome means of finding, upon one fcale, the place of the boiling point of another. For this purpofe, a general folution is requifite of the following problem: "*The fundamental interval* "*being given for a given height of the barometer,* "*to find the fundamental interval, for any other given* "*height of the barometer.*" The folution is furnifhed by M. DE LUC's laborious refearches.

M. DE LUC fixes the boiling point of his thermometer, when the barometer is at 27 inches French [a], that being its mean height at GENEVA. He divides the fundamental interval, after the French manner, into 80 equal parts; and, by a great number of experiments, on the heat of boiling water, at different heights above the level of the fea, the detail of which is to be found in his *Effai fur la Variation,* &c: he hath found, that the height of his thermometer, plunged in boiling water, may be expreffed, in all ftates of the barometer, by the following *formula,* viz. $\frac{99}{20000}$ log. y — a = T. In which, y denotes the height of the barometer, in fixteenths of a Paris line: T the height of a thermometer, plunged in boiling water, above melt-

(a) Recherches, fur les Modificat. de l'Atmofphere, §. 451. a.

ing

ing ice, in hundredths of a degree of M. DE LUC's ſcale; and a the conſtant number 10387 (b).

It is proper in this place to inform the reader, that M. DE LUC, by logarithms, always means the tabular or Briggian logarithms, and conſiders the 7 figures given by the tables, beſides the index, as integral figures; that is, he conſiders the eighth figure of the logarithm as ſtanding in the place of units. Throughout this paper, I have conformed myſelf to this manner of conceiving the tabular logarithms, that, upon the ſame ſubject, we may ſpeak the ſame language. But it is more uſual, with mathematicians, and, in general, it is more convenient, to conſider all the figures, after the index, as decimals. Thus the number, which M. DE LUC expreſſes by $\frac{99}{2000000}$ log. y, would, in the common mathematical ſtile, be $\frac{99 \times 100}{2}$ log. y; or, 99×50 log. y.

It is but ſeldom that the barometer in this country ſtands ſo low as 27 French inches. 30 inches Engliſh are little more than its mean height upon the

(b) Ibid. §. 961. and §. 1143. note a. It may ſeem doubtful whether there is not a ſmall error in the conſtant number 10387. The experiments for aſcertaining the variation of the boiling point were made with a thermometer, of a peculiar ſcale; and the *formula* deduced from them was this, $\frac{7175}{300000000}$ log. y — 5015 = the height of the thermometer, plunged in boiling water, above melting ice, in parts of that peculiar ſcale. Recherch. ſur les Modificat. de l'Atmoſphere, §. 960. Now a degree of this ſcale was afterwards found to be to a degree of M. DE LUC's common ſcale as 80 to $3869\frac{1}{4}$. And this proportion between the degrees of the two ſcales, gives 10368,9 or 10369 very nearly, inſtead of 10387, for the value of the conſtant number a, in the *formula* for hundredths of a degree of the common ſcale.

plane

plane country in the neighbourhood of LONDON. It may, therefore, be proper for the London workmen to fix their boiling point, when the barometer is at 30 inches. FAHRENHEIT's division of the ſcale, which makes 180 degrees between melting ice and boiling water, and places the point of 0 at the 32d degree below melting ice, may be retained; becauſe long uſe hath rendered it familiar to us. A thermometer thus conſtructed, I ſhall call BIRD's FAHRENHEIT: that eminent mechanic, our countryman, Mr. JOHN BIRD, having been, as I believe, the firſt workman, who took the pains to attend to the ſtate of the barometer, in making thermometers, and having always made it his practice, to fix the boiling point, when his barometer hath ſtood at 30 inches. Taking it for granted, that this ſcale will continue in general uſe here, I ſhall give an accurate compariſon of it with the ſcale of M. DE LUC.

T being put for the height of a thermometer plunged in boiling water, above melting ice, in hundredths of a degree of M. DE LUC's ſcale, in any given ſtate of the barometer; let Θ denote the ſame height, in hundredths of a degree of BIRD's Fahrenheit.

Put y for the height of the barometer	in 16ths of a Paris line.
v for its height	in Paris lines.
x	in 10ths of a Paris inch.
z	in 10ths of an Engliſh inch.

And for 10387 put a.
for 16 put b.
for 10 put c.
for 12 put d.

Alſo put E and F for numbers expreſſing the proportion of the Engliſh foot to the French foot.

Now

Now M. DE LUC hath found, that whatever be the value of y;

$$\frac{99}{200000}\log. y - a = T.$$

$$\text{But } \log. y = \log. v + \log. b.$$

$$\text{and } \log. v = \log. x + \log. d - \log. c.$$

$$\text{and } \log. x = \log. z + \log. E - \log. F.$$

$$\text{Therefore } \log. y = \log. z + \log. E + \log. d + \log. b. - \log. F - \log. c.$$

$$\text{and } \frac{99}{200000}\log. z + \frac{99}{200000}\overline{\log. E. + \log. d + \log. b - \log. F - \log. c} - a = T.$$

$$\text{But } \frac{99}{200000}\overline{\log. E + \log. d + \log. b. - \log. F - \log. c - a} = -4171,55.$$

the French foot being to the English as 2,1315 to 2; *vide* Phil. Tranf. vol. LVIII.

$$\text{Therefore } \frac{99}{200000}\log. z - 4171,55 = T.$$

And $\frac{99}{20000000}\log. z - 41,7155 = \frac{T}{100} =$ the height of the thermometer, plunged in boiling water, above melting ice, in degrees of M. DE LUC's fcale, when the height of the barometer, in tenths of an Englifh inch, is z.

Now for z write 300. Then $\frac{T}{100} = 80,902$. which therefore is the height of the thermometer, in boiling water, above melting ice, in degrees of DE LUC's fcale, when the barometer is at 30 inches Englifh. And in the fame ftate of the barometer, the height of the thermometer plunged in boiling water, above melting ice, in degrees of BIRD's Fahrenheit, or, $\frac{\Theta}{100}$, is 180. Hence the numbers T and Θ are in the conftant proportion of 809 and 1800, whatever be the value of z. For the change produced in the heat of boiling water, by any change of z, being always the fame for both thermometers, the temperature expreffed by T in parts of one fcale, is always the

 fame,

ſame, as Θ expreſſes in parts of the other; and, therefore, putting $\frac{1}{L}$, and $\frac{1}{B}$, for the values of the hundredth part of a degree, of the ſcales of M. DE LUC and BIRD reſpectively, the fractions $\frac{T}{L}$, $\frac{\Theta}{B}$ are always equal, and T, Θ, are in the conſtant proportion of the invariable numbers L, B: conſequently, when the proportion of T and Θ is determined for any particular value of z, it is found generally for all. Therefore, as was affirmed,

$$T : \Theta = 809 : 1800.$$

And $T = \frac{809}{1800}\Theta = \frac{899}{2000}\Theta$ very nearly (c) in all values of z; and ſubſtituting this value, for T, in the equation exhibiting the relation between z and T, we ſhall have, for the relation between z and Θ,

$$\frac{99}{20000000}\log. z - 41,7155 = \frac{899}{2000 \times 100}\Theta.$$

Or, $\frac{99}{10000 \times 899}\log. z - 92,804 = \frac{\Theta}{100}$ = the height of the thermometer in boiling water, above melting ice, in degrees of BIRD's Fahrenheit, when the height of the barometer, in 10ths of an Engliſh inch, is z. And thus M. DE LUC's *formula*, for the variation of the boiling point, is adapted to Engliſh inſtruments, and reduced to Engliſh meaſures of length.

(c) It might be ſufficiently accurate for moſt purpoſes, to ſubſtitute $\frac{9}{20}\Theta$ $(= \frac{81}{180}\Theta)$ for $\frac{809}{1800}\Theta$. The error of this ſubſtitution would be about $\frac{1}{809}\Theta$; and conſequently would amount to about $\frac{2}{9}$ of 1° of BIRD's Fahrenheit, when z is 300. But the error in the ſubſtitution I have uſed is much leſs, not amounting to $\frac{1}{8000}\Theta$, which makes leſs than $\frac{1}{44}$ of a degree of BIRD's ſcale, in the ſame caſe.

For

For z write 287,7525 (the length of 27 French inches in tenths of an Englifh inch) and $\frac{\Theta}{100}$, the height of DE LUC's boiling point, above melting ice, in degrees of BIRD's Fahrenheit, comes out 177,989. Hence M. DE LUC's boiling point falls upon 209,989 of BIRD's fcale; that is, upon 210 very nearly, or infenfibly more than two degrees below BIRD's point of boiling; and the reduction of either fcale to the other, in all inferior temperatures, will be as the table of comparifon fhews.

By M. DE LUC's *formula*, thus reduced, the height of the thermometer, plunged in boiling water, above melting ice, in degrees of BIRD's Fahrenheit, in any given ftate of the barometer, may be computed. But 899 being a troublefome divifor, to render the computation more eafy and expeditious, take the following method. For $\frac{11}{1000000}$ log. z. write s.

Then $s + \frac{1}{900} s - 92,804 = \frac{\Theta}{100}$ very nearly. (d)

Upon thefe principles I have computed a little table, for finding the heights, to which a good BIRD's

(*d*) If, according to note (*b*), we take 10369 inftead of 10387, for the value of the conftant number a in M. DE LUC's *formula*, we fhall find, $\frac{99}{200000000}$ log. $z - 41,5355 = \frac{T}{100}$. Whence we fhould obtain $s - \frac{1}{900} s - 92,198 = \frac{\Theta}{100}$ very nearly. But I abide by the *formula* given in the text: being perfwaded, that M. DE LUC hath purpofely adopted the number 10387, as agreeing better, upon the whole, with his experiments than the other; though I do not recollect that he hath, in any part of his work, exprefsly faid fo.

 Fahrenheit

Fahrenheit will riſe, when plunged in boiling water, in all ſtates of the barometer, from 27 to 31 inches Engliſh. Among other uſes of this table, it will ſerve for a direction to inſtrument-makers, to make a true allowance, for the effect of the variation of the barometer, if they are at any time obliged, to finiſh a thermometer, when the barometer is above or below 30 inches; but in general it ſhould be their rule, to watch an opportunity of fixing the boiling point, when the barometer is actually at the height preſcribed.

I muſt, upon this occaſion, declare, how heartily I concur with M. DE LUC, in wiſhing, that ſome *common* ſcale, the ſame in the number of its diviſions, its point of 0, and its boiling point, might be received, with unanimous conſent, by philoſophers of all parts of the world; that, for the future, we might have one general language, for ſo very general an object of enquiry and diſcourſe, as the different degrees of heat and cold. To mathematicians the compariſon of different ſcales, is a taſk of little labour. But among thoſe who have a taſte for phyſical reſearches, and are capable of purſuing them, in certain branches at leaſt, with ſome degree of advantage to ſcience, as well as to themſelves, there are many, to whom every little calculation is a toil. Not having acquired the habits of it, in the early part of their lives, they never can acquire them, in ſuch a degree, as to be able to put a confidence in themſelves, and rely upon the accuracy of their own concluſions. The eaſe of ſuch perſons ſhould, in my opinion, be conſulted. But theſe are not the only perſons intereſted. The inconveniencies to be apprehended from a diverſity of

ſcales

ſcales are general. It is not one of the leaſt, that inſtruments eſſentially different, as made at different elevations in the atmoſphere, will continue to paſs under the ſame name. The error and confuſion, which this may create, is remarkably inſtanced, in what hath actually happened to the thermometer of the celebrated REAUMUR. The inſtrument, which at this day paſſes, all over Europe, under the name of REAUMUR's thermometer, is eſſentially different from his; yet it was always ſuppoſed to be the ſame, even among his own countrymen, till M. DE LUC detected the miſtake. At what time, upon what occaſion, or by whom, the change was introduced, is ſtill unknown.

SECTION SECOND.

Of the GENERAL PRINCIPLES of meaſuring HEIGHTS by the BAROMETER.

AS it is my deſign to compare the practical rules, which M. DE LUC hath deduced from experiment, with theory, it is neceſſary, for the clearer arrangement of the argument, previouſly to ſtate the general principles, upon which the meaſurement of heights by the barometer depends. In doing this, I ſhall rather aim at perſpicuity than at brevity; referring, however, for the demonſtration of whatever hath been advanced before, to writers of approved authority.

In any column of the atmoſphere, reſting perpendicularly upon any ſmall portion of the earth's ſurface, the denſities of the air diminiſh, as we aſcend to greater heights; and if the accelerative force of gravity were the ſame at all heights, the denſities would decreaſe geometrically as the height increaſed arithmetically. This is an obvious conſequence from a known property of air, that it expands itſelf through a greater or a leſs ſpace, in proportion as the force, by which it is compreſſed, is leſs or greater; that is, that the denſity of air, is always as the compreſſing force (*e*). And from hence it would follow, that the difference of the elevation of any two places, would be as the logarithm of the *ratio* of the denſities of the air, at each: and the denſity being every where as the compreſſing force, and the compreſſing force as the length of the column of quickſilver ſuſtained by it in the barometer, the difference of elevation would be, as the logarithm of the *ratio* of the altitudes of the quickſilver in the barometer, at the ſame time, at the different ſtations; that is, as the difference of the tabular logarithms of the numbers, by which thoſe altitudes would be expreſſed in any given meaſure (*f*). But the accelerative force of gravity diminiſhes, in the ſame proportion, as the ſquare of the diſtance, from the earth's center, is increaſed. It is not the ſame

(*e*) Cotes's Hydroſtat. Lectures, lect. ix.

(*f*) See Philoſoph. Tranſact. n. 181. Phil. Nat. Princip. Math. lib. ii. prop. 22. Scholium. Cotes's Hydroſtat. Lectures, lect. ix. Harmon. Menſ. p. 17.

Of all theſe demonſtrations, that given by Mr. COTES, in the hydroſtatical lectures, will be the moſt perſpicuous to the generality of readers. It is very diffuſe, and he hath been at great pains to reduce it to the moſt ſimple principles. Mathematicians will find the ſubſtance of his argument well ſummed up, by M. DE LA LANDE, in the Connoiſſance for the year 1765, p. 211, 212. who, I am perſwaded, would not have produced it as a new demonſtration, had he known it had been given before.

therefore

therefore at all heights above the ſurface; and the denſities of the air muſt decreaſe, by a different law, from that which would obtain, if the force of gravity were uniform. This other law, however, is ſuch, that, to a much greater height than is acceſſible to man, the gradual variation of the compreſſing force and denſities of the air, will be ſo little different, from what it would be upon the former hypotheſis, that the error of that hypotheſis, in the meaſurement of heights, will be abſolutely inſenſible.

Let the point C [tab. IX. fig. 1.] repreſent the center of the earth. CA the earth's ſemi-diameter. AB any height above the ſurface. At A, place a right line, AD, of any finite length, at right angles with AC. In the right line AC, towards C, take Aβ, ſuch that CA may bear to Aβ the proportion of CB to BA. In a right line drawn through β, at right angles with AC, take βE, of ſuch length, as to bear to AD the proportion of the denſity of the air at B to the denſity at A, or at the earth's ſurface. The curve, which the point E always touches, is a logarithmic, of which AC is the aſymptote.(g).

As I ſhall have frequent occaſion to conſider the curve, which thus exhibits the relation between denſity and elevation, I ſhall call it the ATMOSPHERICAL LOGARITHMIC.

Imagine this curve deſcribed, and take another height A*b*, and take $A\mathit{6} = \frac{CA \times Ab}{Cb}$, and draw $\mathit{6}e$

(g) Cotes's Hydroſtat. Lectures, p. 161—167. Harmon. Menſ. p. 18. Phil. Nat. Princip. Math. lib. ii. prop. 22. Brooke Taylor. Method. Increment, Prop. 26.

parallel

parallel to βE, meeting the curve in *e*. Then βϐ is the logarithm of the *ratio* of βE to ϐ*e*, or of the denſity at B to the denſity at *b*. But if the greater of the two heights, AB and A*b*, bear but a very ſmall proportion to the ſemi-diameter of the earth, their difference B*b* will be very nearly equal to βϐ.

For, becauſe CB : BA = CA : Aβ (by conſtruction.)
Therefore, by converſion, CB : CA = CA : Cβ
In like manner, and by inverſion, CA : C*b* = Cϐ : CA.
by equi-diſtance perturbate, CB : C*b* = Cϐ : Cβ.
and converting, CB : B*b* = Cϐ : βϐ.
by permutation, B*b* : βϐ = CB : Cϐ.

But when AB is infinitely diminiſhed, CB = CA ultimately. Alſo A*b* being infinitely diminiſhed, Cϐ = CA ultimately. Therefore CB = Cϐ ultimately, and B*b* = βϐ ultimately. Q. E. D.

Now AB and A*b* will always be ſo ſmall, with reſpect to CA, if B and *b* be ſuppoſed to repreſen any acceſſible places, that CB, Cϐ, and B*b*, βϐ, may always, in this caſe, be conſidered, as in their ultimate proportion of equality.

It is ſtill therefore to be admitted as a principle, in practice, that the difference of elevation of any two places, is as the difference of the tabular logarithms of the heights of the quickſilver in the barometer at the ſame time, at both places; that is, it is the logarithm of the *ratio* of thoſe heights in *ſome* ſyſtem of logarithms. And the heights of the quick-ſilver being given, by obſervation, the difference of elevation will be known, if that particular ſyſtem can be determined; that is, if the *modulus* of the ſyſtem, or the length of the ſubtangent of the curve

curve DE*e* of that ſyſtem, can be aſcertained, in ſome known meaſure, as Engliſh fathoms, or Paris toiſes.

The eaſieſt method of doing this, that theory ſuggeſts, is to compare barometers at two ſtations, ſuppoſe B and *b*, each of a known elevation AB and A*b*, above the level of the ſea. For the logarithms of any given *ratio*, in different ſyſtems, are proportional to the ſubtangents; and the difference of elevation, B*b*, diminiſhed in the proportion of CB, (the diſtance of the higher ſtation from the earth's center) to Cϐ, (a third proportional to C*b*, the diſtance of the lower ſtation from the earth's center, and CA, the earth's ſemi-diameter) is the logarithm of the *ratio* of the denſity at B, to the denſity at *b* (that is, of the columns of quickſilver ſuſtained in the barometer at B and *b*) in the atmoſpherical ſyſtem. Therefore, as the difference of the tabular logarithms, of theſe columns, to the ſubtangent of the tabular ſyſtem, ſo ſhould B*b*, diminiſhed as hath been ſaid, (that is, ſo ſhould βϐ) be to the ſubtangent of the atmoſpherical logarithmic. The utmoſt height, to which we can aſcend, above the level of the ſea, is ſo ſmall, that the reduction of B*b* may, even in this inveſtigation, always be neglected. For, if AB were four Engliſh miles, which exceeds the greateſt acceſſible heights, even of the Peruvian mountains, and Aϐ three, βϐ would be ſcarce one part in 500 leſs than B*b*. So that, by comparing barometers at different elevations, within a mile above the level of the ſea, the ſubtangent of the atmoſpherical curve might be determined, as it ſhould ſeem, without ſenſible error, by taking ſimply the difference of elevation, without reduction,

tion, for the logarithm of the *ratio* of the obſerved heights of the quickſilver in the atmoſpherical ſyſtem. Thoſe, however, who have attempted to determine the ſyſtem, by this method, have hitherto agreed but ill in their concluſions. The fact is, that the length of this ſubtangent is very different at different times. The cauſes and quantity of its variation will be conſidered in another place. It appears, from M. DE LUC's experiments, that, though otherwiſe ſubject to change, it is conſtant in a given temperature. And that when the temperature of the air is $+ 16\frac{3}{4}$ of his ſcale, the difference of the tabular logarithms of the heights of the quickſilver in the barometer, gives the difference of elevation in 1000ths of a Paris toiſe [(b)]. This is the rule, which he hath derived from a great number of experiments made at very different elevations: and the truth of it being admitted, it is a neceſſary conſequence, that the number, which is the *modulus* of Briggs's ſyſtem, expreſſes the length of the ſubtangent of the atmoſpherical curve, ſuch as it is in that temperature, in 1000ths of a Paris toiſe.

(*b*) Recherch. ſur les Modif. de l'Atmoſph. §. 588.

SECTION

SECTION THIRD.

Of the EQUATION for the DIFFERENCE of TEMPERATURE of the QUICKSILVER in the BAROMETERS, at the DIFFERENT STATIONS.

THE preceeding rule, however, muſt not be expected to give an accurate reſult, even in that particular temperature of the atmoſphere, to which it is adapted, unleſs the ſpecific gravity of the quickſilver, in the barometer, hath been the ſame at both ſtations, at the time of obſervation. If the ſpecific gravity hath been different, at the different ſtations, the heights of the quickſilver, in the barometer, will *not* have been proportional to the denſities of the air; that is, to the forces by which they have been ſuſtained: and the calculation, built upon the ſuppoſition that they were ſo, becomes erroneous. The ſpecific gravity of every material ſubſtance varies with its temperature. If the temperature of the quickſilver, therefore, hath been different, at the different ſtations, the difference of elevation, found by the foregoing rule, will require correction, though the mean temperature of the air may have been ſuch as it preſcribes (i). No particular temperature of the quickſilver is neceſſary to the accuracy of the reſult of the preceeding rule, or to render the

(i) Obſerve, that the temperature of the quickſilver, in the portable barometer, will not be neceſſarily the ſame with that of the circumambient air, at the place and time of obſervation; there will generally be a conſiderable difference.

correction, I am now considering, o; but only, that its temperature, whatever it be, be the same at both stations. If the *temperature* hath been the same, the *specific gravity* hath been the same; and if the *specific gravity* hath been the same, the *length* of the columns of quicksilver have been as the forces by which they were sustained, whatever the common specific gravity may have been. Columns of water, sustained in evacuated tubes, of sufficient length, would be proportional to columns of quicksilver in the barometer, at the same times and places, provided the temperature of the two water columns were the same, and that of the two mercurial columns the same; and, consequently, the difference of the logarithms of the water columns would be precisely the same as of the mercurial columns. For the water columns, and the mercurial columns, are only expressions of the same absolute magnitudes, the forces by which they are both sustained, in parts of different scales. But if the temperature of the quicksilver, or of the water, be different at the same time, at different stations; then, though we compare the water columns with each other, and the mercurial columns with each other, still we compare different things, though we call them by the same name. We compare fluids of different specific gravities; that is, we measure the pressure of the air at one place, in parts of one scale, and at the other place, in parts of another. The error is of the same kind, as if one should attempt to determine the proportion between two parts of a building, by measuring one with a Paris foot-rule, and the other with an English foot-rule, without attending

attending to the difference between Paris feet and London feet; and the ſame method muſt be taken to get rid of the error in both caſes: we muſt aſcertain the difference of the ſcales we have applied, and make allowance for it: we muſt know the length of the one in parts of the other; that is, in the particular caſe in queſtion, we muſt be able to determine, from the obſerved height of the barometer, when the quickſilver is of any given temperature, what its height would have been, at the ſame place, and at the ſame time, if its temperature had been any other, that may be aſſigned. I have thought it neceſſary to be thus minute, in explaining the principles, upon which the correction in queſtion depends; becauſe it is a point, which is likely to be miſunderſtood, though in itſelf of no great difficulty. M. DE LUC himſelf hath fallen into a miſtake, not with reſpect to the quantity of the error, but the manner of allowing for it; which, however, is of no other bad conſequence, than that of lengthening the calculation unneceſſarily. He imagines that a particular temperature of the quickſilver is neceſſary, that the error ſhould be nothing; and to this he always reduces the obſerved heights of the quickſilver, in the barometer, at both ſtations [k]. But the reſult of the computation would, in all caſes, have been the ſame, if they had been reduced to any other given temperature; and therefore, it is always ſufficient, to reduce the one to the temperature of the other. This little overſight I ſhould have touched upon with more

(k) Recherch. ſur les Modif. de l'Atm. §. 369—374.

reluctance,

reluctance, were it not of too little importance to derogate, in the least degree, from the general merit of M. DE LUC's elaborate and invaluable work.

The quantity of the correction in question, is thus determined. At times, when the barometer, in a temperate air, stood at 27 Paris inches, M. DE LUC tried what change was effected in its height, by changes of temperature, induced by art in the quicksilver, without any alteration in the state of the atmosphere; and, by repeated experiments of this kind, it was found, that the difference between the length of the column of quicksilver, successively heated to the temperature of boiling water, and cooled to that of melting ice, amounted to half a Paris inch exactly; that is, to $\frac{1}{54}$th of the mean height (*l*). But the whole extent of the thermometer's scale, from the temperature of melting ice to M. DE LUC's boiling point, being 178° of BIRD's Fahrenheit, the change of the height of the barometer, due to 1° of BIRD's Fahrenheit is always $\frac{1}{54 \times 178} = \frac{1}{9612}$. And if n denote the number of degrees of BIRD's Fahrenheit, in the difference of the temperatures of the quicksilver, in the barometers, at different stations, the reduction will be $\frac{n}{9612}$; that is to say, the height of the warmer column must be shortened by this part of its own length, or that of the cooler augmented by the like part, to reduce either, to the height it would have stood at, at the time and place of observation, with the temperature of the other.

(*l*) Recherch. sur les Modif. de l'Atm. §. 362—364.

These

These determinations of the effect of heat upon the column of quickfilver, in the Torricellian tube, fhew the proportional alterations of the denfity of that fluid, by given increments, or diminutions, of heat. For the perpendicular height of a column, of any inelaftic fluid, fuftained in the Torricellian tube, by a given compreffive force, muft, by the known laws of hydroftatics, be as the denfities of the fluid inverfely. And as this proportion muft obtain, whatever be the fize, or figure, of the tube, there feems to be no method, by which the change of denfity, or the proportional expanfion of quickfilver by heat, can be meafured with more precifion. Thefe conclufions, therefore, may be of ufe in many phyfical enquiries; and there are many cafes, in which it may be neceffary, to reduce the obferved height of the barometer, in one temperature, to another. Thus when that height is to be made the meafure of the variable preffure, or of the denfity of the air, in fome particular place, it will be neceffary to choofe fome *ftandard* temperature, to which the obferved length of the column may always be reduced. And it was this confideration, as I have gathered from many converfations with him, which gave occafion to M. DE LUC's miftake. He had fettled it with himfelf, at his very firft entrance upon thefe refearches, that the point he was to keep conftantly in view, as the ultimate object of his whole purfuit, was to find, in the variable length of the mercurial column, a meafure of the preffure and denfity of the air. This, he faw, was only to be looked for in quickfilver of fome one conſtant temperature; and thus he became poſſeſſed with a general notion, of the neceſſity of reducing the obſervations of the barometer, to ſome ſtandard temperature, upon all occaſions.

But, for the particular purpoſe of computing differences of elevation, inſtead of attending to the correction of the obſerved heights of the quickſilver at all, it will be a readier way, to make the correſponding correction immediately, upon the difference of the tabular logarithms. If any quantity be diminiſhed or increaſed, in a given proportion, the logarithm of its proportion to any other given quantity (that is, the difference of the tabular logarithms) is diminiſhed or increaſed by a given magnitude; namely, by the logarithm of the given *ratio*, in which the variable quantity is altered. The logarithm of the *ratio* of x to $x + \frac{1}{9612}x$, or of 9612, to 9613, is 452 (m). Therefore, if n, as before, expreſs the difference of temperature in degrees of BIRD's Fahrenheit, $\overline{n \times 452}$ is the correction to be applied to the difference of the logarithms of the obſerved heights of the quickſilver in the barometer. This correction is to be ſubſtracted, if the temperature, at the upper ſtation, hath been the cooler of the two, and to be added, in the contrary caſe. I am indebted to the ASTRONOMER ROYAL for the firſt hint of this elegant method of applying the correction in queſtion.

(*m*) The reader will here recollect, that I ſpeak of the tabular logarithms in the language of M. DE LUC. The logarithm of the *ratio* of 9612 to 9613 is, in the more common ſtile, 0.0000452.

The

The third of the annexed tables, ſhews the quantity of this correction correſponding to every value of n from 1° to 70°. The third column, to the right, exhibits the value of it in Engliſh fathom, in that particular temperature of the air, in which the difference of the tabular logarithms, of the heights of the quickſilver in the barometer, gives the difference of elevation in 1000ths of an Engliſh fathom. What that temperature is, will be determined in the ſequel. It is to be obſerved, that the value of theſe corrections, in the fathom of any other country, will be the ſame, in that particular temperature, in which the tabular ſyſtem meaſures the difference of elevations in 1000ths of the fathom of that country.

The rule, which I have given for applying this correction, ſuppoſes that it is previouſly known, which of the two ſtations is the higheſt; otherwiſe it will be doubtful, whether it ſhould be added or ſubtracted. This doubtful caſe may actually happen in levelling a conſiderable tract of country, that is pretty even. The table, however, will ſtill give the quantity of the correction. Add that quantity to the logarithm of the height of the cooler barometer; and the difference between this logarithm, ſo augmented, and the logarithm of the height of the other, is the difference of the logarithms of the obſerved heights, duly corrected: and the ſtation of the cooler barometer, was the lower of the two, if its augmented logarithm exceeds that of the other: in the contrary caſe, the higher.

It hath been already obſerved, that the change of the denſity of quickſilver, by an alteration of its heat, muſt always be proportional to the increaſe

 or

or contraction of the length of the column in the barometer; or, in other words, that the alteration, in the perpendicular height of the Torricellian column, is proportional to the change of the *ſolid content* of ſpace, occupied by a given quantity of the fluid. Thoſe who recollect the experiments of the celebrated BOERHAAVE, for meaſuring this change of volume, cannot but be ſtruck with the great and ſingular diſagreement between his concluſions and thoſe of M. DE LUC. BOERHAAVE makes the whole expanſion, produced by a change of temperature from the 0 of Fahrenheit, (or the forced congelation of *ſal ammoniac*) to the heat of boiling water, $\frac{1}{52\frac{25}{33}}$ of the whole volume, which is very little more than M. DE LUC found to be produced by a change from 32° of Fahrenheit (or the diſſolution of natural froſt) to the heat of boiling water (n).

(n) Vid. Elementa Chemiæ, vol. I. p. 174. In this place, the author expreſsly treats of the expanſion of quickſilver by heat; and the proportion, which he here aſſigns, hath been adopted by the writers of elementary phyſics. But, p. 165, having deſcribed an artificial cold, in which the thermometer had been ſunk to −40 of Fahrenheit, he ſays, "*Novimus hoc* "*argenti vivi corpus ab illo gradu* 40 *infra* 0, *ad gradum* 600 *quo* "*incipit ebullire contractum fuiſſe per partes* 640 *totius molis* "10782." And, in the explanation of one of his copper plates, he deſcribes a thermometer in which the quickſilver, in the greateſt natural cold, juſt filled the bulb, which contained, he ſays, 11520 ſuch parts as the tube contained 96. It ſeems probable, that the number 11124, which is given, p. 174, as the whole volume of the quickſilver in the temperature 0, (a degree of Fahrenheit being the unit) hath been deduced from the mean of a variety of experiments upon different inſtruments, for it differs not greatly from the mean of the two numbers 10782 and 11520.

Upon this it is to be obſerved, that BOERHAAVE's method gave him the expanſion of his quickſilver, diminiſhed by the whole expanſion nearly of the veſſel which contained it. DE LUC's gave him the expanſion of his quickſilver, diminiſhed by the longitudinal expanſion of the ſcale applied to the barometer, by which the height of the column was meaſured. The veſſel which contained BOERHAAVE's quickſilver was *glaſs*. The ſcale of DE LUC's barometer was *deal*. Glaſs is conſiderably altered in its dimenſions by heat; deal but very little, in the direction of its longitudinal fibres: therefore, though M. DE LUC neglected the expanſion of his ſcale, the expanſion which he obſerved of the quickſilver would be nearly the whole; but BOERHAAVE muſt have reckoned it much too ſmall, neglecting, as he ſays he did, the expanſion of his glaſs [o]; and the difference is no more than what the expanſion of the glaſs will very nearly reconcile. BOERHAAVE's expanſion is too ſmall, to be in proportion to M. DE LUC's, by about $\frac{1}{286}$ of the whole volume, or ſomewhat more than $\frac{1}{300}$. The expanſion of glaſs, in length, by 180°, is, as I am informed, about $\frac{1}{100}$ of an inch upon a foot, or $\frac{1}{1200}$ part; therefore, its expanſion in ſolid content, or volume, will be very nearly $\frac{1}{400}$, by 180°; and, by 212°, about $\frac{1}{331}$, or leſs than $\frac{1}{300}$.

(o) *Sepoſitâ ergo vitri interea dilatatione*, &c. Theſe words may ſeem ambiguous; but the meaning ſeems ſufficiently determined by what is ſaid, p. 173. l. 2—5.

SECTION FOURTH.

Of the EQUATION for the TEMPERATURE of the AIR.

IF the temperature of the atmoſphere hath been any other than is expreſſed by + 16¾ of M. DE LUC's ſcale, the reſult of the calculation, formed upon the preceeding rules, requires a further correction. This correction ariſes from *a variation of the length of the ſubtangent of the atmoſpherical logarithmic*; which, as hath been already remarked, is found to be not conſtantly the ſame. That this matter may be the better underſtood, it will be proper to ſtate, in this place, what this ſubtangent is, in the nature of things, and upon what phyſical circumſtances its length depends.

Imagine then, that inſtead of the atmoſphere, in its natural ſtate, the earth were ſurrounded with an inelaſtic fluid, of an uniform denſity throughout, equal to that of the natural atmoſphere, in its loweſt parts, which are contiguous to the ſurface of the earth; imagine alſo, that every atom of this homogenous fluid were urged towards the earth's center, with an accelerative force, equal to that of gravity, at the ſurface of the earth. Now the preſſure of the atmoſphere upon the earth's ſurface, or on any given part thereof, being, at all times, a finite, though not a conſtant force, of which the phænomena of the Torricellian tube are a ſufficient proof, it is evident that it might, at any time, be equalled by the preſſure of a finite quantity of this imaginary fluid; and that, to render the preſſure of this fictitious atmoſphere, upon the whole, or any part of the earth's ſurface, equal to that of the real atmoſphere upon the like part, it would be requiſite to aſſign to it ſome finite thickneſs or depth. *Now that*

that particular thickneſs or depth, of the imaginary fluid, which is, at any time, neceſſary to render an entire column of it a counterpoiſe for an entire column of the natural air, is, at that time, the length of the ſubtangent of the atmoſpherical logarithmic. In ſhort, the ſubtangent of the atmoſpherical logiſtic, is the length of a column of ſuch a fluid as I have ſuppoſed, which would be ſuſtained in the Torricellian tube, by the preſſure of the air, at the level of the ſea, if we could ſuppoſe a tube of a ſufficient length.

This is demonſtrated by Mr. COTES, Harmon. Menſ. p. 18. and by no one elſe, that I know of, with equal ſimplicity.

It is a manifeſt conſequence from this, that the ſubtangent muſt always be as the preſſure of the whole cylindrical column (upon a given part of the earth's ſurface) directly, and the denſity of the air, at the ſurface, inverſely; and it may therefore appear to be repugnant to the theory already eſtabliſhed, to ſuppoſe it ſubject to variation. For if the preſſure be always as the denſity, upon which hypotheſis the whole theory is founded, that which is always as the preſſure directly, and the denſity inverſely, can be no other than a conſtant quantity. But M. DE LUC's experiments prove, beyond a doubt, that the atmoſpherical ſubtangent is variable; therefore the denſity of the atmoſphere, at the ſurface of the earth, at different times, is not proportional to the whole preſſure, at ſuch times, reſpectively. And in this there is nothing inconſiſtent with the foregoing theory, rightly underſtood. When the denſity of the air is ſaid to be as the compreſſing force, this is to be underſtood of air in the *ſame ſtate of elaſticity*; that is, in which the abſolute force of elaſticity, under all the different degrees of preſſure, is conſtantly the ſame. (The abſolute force of elaſticity is meaſured by the force exerted between two particles of given magnitudes and figures, at a given diſtance.) But there is no reaſon, from any experiments, to conclude, that the denſity of the air will be ſimply as the compreſſing force, in different abſolute forces of elaſticity. On the contrary, ſince the proportion of the compreſſing forces is found to obtain, between the denſities, when the abſolute elaſticity is conſtant; this alone is a proof, that the like proportion will not obtain, if the abſolute elaſticity be changed as well as the compreſſive force.

Imagine two parcels, ſimilarly ſhaped, A and B, of different fluids, the ſame in all their other properties, and ſimilarly, but unequally, elaſtic; that is, imagine the integrant particles of A to be equal in quantity of matter and bulk, and ſimilar in figure, to the integrant particles of B, ſeverally; and if each of the integrant particles of A, be itſelf an aggregate of leſſer integrants, each of the integrant particles of B is to be underſtood to be a like aggregate of equal and ſimilar leſſer integrants, ſimilarly compoſed; and whatever forces, except that of elaſticity, are exerted between the particles of A, imagine equal forces, of the ſame kinds, exerted according to the ſame laws, between the particles of B. Likewiſe, imagine the equal and ſimilar particles to be ſimilarly ſituated, in their reſpective maſſes. Such fluids are the ſame in all reſpects, elaſticity excepted. Further, imagine an elaſtic force to be exerted between every two integrant particles

particles of A, and to vary, with the diſtance, as any power or function of the diſtance whatever. Imagine an elaſtic force, varying according to the ſame power or function of the diſtance, exerted between every two particles of B. Thus the fluids are *ſimilarly* elaſtic; but they are to be ſuppoſed *unequally* ſo; that is, the force exerted between two particles of the one, at any given diſtance, is to be greater than the force between two particles of the other, at the ſame diſtance. Now, if the law by which the elaſtic force varies, at different diſtances, in one of theſe fluids, as A, be ſuch, that the denſities of A, under different compreſſions, are to each other, as the compreſſive forces; the denſities of B, compared with each other, under different compreſſions, will likewiſe be as the compreſſive forces. But the proportion of the compreſſive forces will not ſubſiſt between the denſities of the two *different* fluids. The denſities will not be the ſame, when the compreſſive forces upon A and B are equal; nor will they be *as* the compreſſive forces, when thoſe forces are unequal. The denſities will be as the compreſſive forces directly, and the abſolute elaſticities inverſely; that is, the compreſſive forces will be as the denſities and abſolute elaſticities jointly, which is eaſily demonſtrated.

The compreſſive force upon any fluid, is to be eſtimated by the quantity of the preſſure, acting perpendicularly upon a given ſurface, or plane section of it. Whatever degree of preſſure acts upon the maſs of an elaſtic fluid, its particles will approach or recede, till the diſtances of every two adjacent particles are ſuch, that the whole elaſtic force, exerted perpendicularly againſt every ſurface or plane

ſection.

ſection of it, is preciſely equal to the preſſure acting perpendicularly upon the ſame ſurface, in an oppoſite direction. And in the particular degree of denſity, determined by that diſtance of the adjacent particles, the fluid will remain, while the ſame degree of preſſure is continued. Imagine therefore the denſities of the two maſſes, A and B, to be the ſame; then, from the ſuppoſed ſimilarity of the fluids, it follows, that the number of particles, exerting their elaſticities upon any equal and ſimilar ſections of A and B, muſt be equal; and that the diſtances of the corresponding particles, from each other, in the two maſſes, muſt be the ſame: conſequently the whole elaſticities, exerted upon equal and ſimilar ſections of the two maſſes, will be as the abſolute elaſticities of which they are compoſed. Therefore the compreſſive forces are, in this caſe, as the abſolute elaſticities. Call the common denſity of the fluids D; the compreſſive force upon A, P; upon B, Π; the abſolute elaſticities *a*, *b*, reſpectively. Let *d* denote ſome other denſity of the maſs A, and *p* the compreſſive force correſponding to that denſity.

Now D and *d* are different denſities of the ſame fluid A, under different compreſſive forces P, *p*.

Therefore, $p : P = d : D$ (by hypoth.)

But $P : \Pi = a : b$. (as hath been proved.)

Therefore, $p : \Pi = d \times a : D \times b$; that is, the compreſſive forces upon the different fluids A and B, when the denſities are unequal, are as the denſities and abſolute elaſticities jointly. Q. E. D. This demonſtration is independent of any more particular hypotheſis, concerning the law of the elaſticity, than barely that it is the ſame in both maſſes; and ſuch, in both, as to make the denſities of either always proportional

to

to the forces by which it is compreſſed. But to this condition, a particular law of elaſticity is requiſite; namely, that the force exerted between every two adjacent particles diminiſh as the diſtance between them is increaſed; and, adopting this law, the compreſſive forces might be proved to be in the proportion aſſigned, that of the denſities and abſolute elaſticities jointly, by the ſame kind of reaſoning as is uſed to demonſtrate the 23d propoſition of the ſecond book of the *Principia*, which is only the moſt ſimple caſe of the more general theorem now propoſed.

Hence it follows, that if any cauſes act upon the atmoſphere, to change the degree of its abſolute elaſticity, provided they act in ſuch manner, as to change it equally at all heights; ſo that, though different at different times, it ſhall always, at any one time, be the ſame at all different heights; then, the denſities in all different parts of the cylindrical column, reſting perpendicularly upon a given ſmall part of the earth's ſurface, will, at any one time, be as the compreſſive forces upon each part reſpectively. But the denſities, in any one part of this column, at different times, when the abſolute elaſticities are different, will not be as the compreſſive forces upon that part, at ſuch *different* times reſpectively. Hence the relation between the decrement of denſity, and the increment of height, reduced according to the conſtruction of ſection ſecond, will, at every time, be repreſented, by the decrement of the ordinates, and the increment of the aſymptote of *ſome* logarithmic curve. But of *different* curves, at different times, or in different ſtates of the abſolute elaſticity; that is, the length of the ſubtangent of the curve, deſcribed in ſection ſecond, will vary: for the difference of the ſubtangents is the only thing that conſtitutes a real difference in two logarithmic curves. If ſuch curves have equal ſubtangents, though they have unequal ordinates, they are only *different parts* of the *ſame* curve.

The ſubtangent of the atmoſpherical logiſtic muſt always be as the preſſure of the whole column of the atmoſphere directly, and the denſity, at the earth's ſurface, inverſely; therefore it is directly as the abſolute elaſtic force. For call the ſubtangent S; the preſſure at earth's ſurface, P; the denſity, D; the abſolute elaſticity, A.

Now that S is as $\frac{P}{D}$ is obvious (from p. 245.); but $\frac{P}{D}$ is as A (by what hath now been proved). Therefore S is as A.

Now *heat* (p) is one cauſe, which is well known to influence the abſolute elaſtic force. An increaſe of heat increaſes elaſticity; and elaſticity is diminiſhed by a diminution of heat. Accordingly M. DE LUC's experiments ſhew, that when the temperature of the air is uniform (or the ſame at all heights) the ſubtangent of the atmoſpherical curve is increaſed, or diminiſhed, exactly in proportion to the increment or decrement of the uniform temperature, as indicated by the mercurial thermometer. His concluſion, from repeated experiments on the mountains near Geneva, is, *that, if* L *denote the difference of the tabular logarithms of the heights of the quickſilver, at two ſtations, corrected for the difference of the temperature of the quick-*

(p) BOERHAAVE, Elementa Chemiæ, vol. i. p. 456, &c.

ſilver,

ſilver, and n *denote the difference of the temperature of the air, in degrees of his own ſcale, above or below* + $16\frac{3}{4}$, *then* $L \pm \frac{n}{215} L$, *is the difference of elevation in* 1000*ths of a Paris toiſe. The correction* $\frac{n}{215} L$, *to be added or ſubtracted, according as the temperature of the air is above or below* $16\frac{3}{4}$; *or according as* n *is a difference in exceſs or defect*[(q)].

But in any given value of *n*, the proportion of $\frac{n}{215} L$ to L is given, being that of *n* to 215. Therefore, $L \pm \frac{n}{215} L$ is the logarithm of the *ratio* of the heights of the barometer, in a ſyſtem of logarithms, in which the ſubtangent of the curve is to ſo many 1000ths of a Paris toiſe, as are expreſſed by the ſubtangent of the Briggian ſyſtem, as $215 \pm n$ to 215. And this being the caſe, whatever be the value of L, in every given value of *n*, it is evident, that the variation of the *modulus* of the atmoſpherical ſyſtem, or of the ſubtangent of the atmoſpherical curve, is always as *n*, the variation of temperature.

But a uniform temperature is a condition of the atmoſphere, which rarely obtains, within ſmall diſtances, at leaſt, above the earth's ſurface; therefore the more uſual caſe of unequal temperatures muſt be conſidered.

When different temperatures obtain, at different heights, at the ſame time, they muſt render the abſolute elaſticities, at thoſe heights, unequal. Thus the hypotheſis of M. BOUGUER will take place;

(q) Recherch. ſur les Modif. de l'Atm. §. 607—611.

who, from a great number of experiments, made upon the CORDILEROS, and at various other heights above the level of the sea, concluded, that the absolute elasticity of the air, which he calls, "the intensity of the elastic force," must be different at different elevations. His conjectures indeed about the cause from whence the difference might arise, are not the most natural; and in one point he was evidently mistaken: namely, that he imagined the absolute elasticity to be constant at every *given* elevation; and accordingly he hath traced the curve, which, according to his experiments, exhibits the laws of its variations from one height to another. This, it must be confessed, was trusting his experiments too far, which were not made with the most accurate instruments; and this may have given occasion to his learned countryman M. DE LA LANDE, to suppose that this curve may rather exhibit the deviations of his experiments from the truth, than any thing which really obtains in nature [r]. His general hypothesis, however, of a difference of absolute elasticity, in different parts of the atmosphere, *must* obtain, whenever the temperatures of such different parts are unequal, unless the effect of the inequality of temperature were to be compensated by the synchronous unequal operations of some other cause. That it does in fact obtain, when the temperature is unequal, is proved, beyond a doubt, instead of being refuted, by M. DE LUC's experiments; and this is to be considered, as a further instance of their exact agreement with the genuine conclusions of accurate theory.

(r) Connoissance pour l'année 1765, p. 215.

When

When the abſolute elaſticities are different at different heights, the denſities will no longer be proportional to the preſſures; and the change of denſity, throughout the whole column of the atmoſphere, will no longer be repreſented by any *one* logarithmic. But to ſmall diſtances on one ſide or the other of different heights, it may ſtill be nearly repreſented by parts of *different* logarithmics. Now this is really the caſe, according to M. DE LUC: for, when the temperature, at two different heights, hath been different, he finds, *that the difference of thoſe heights will ſtill be expreſſed in* 1000*ths of a Paris toiſe, by the preceeding* formula, *viz.* $L \pm \frac{n}{215} L$, *if* n *be underſtood to denote the difference between the conſtant temperature* + $16\frac{3}{4}$, *and that which is the mean of the different temperatures of the two places of obſervation* (s); that is to ſay, though the temperature of that portion of the column of the atmoſphere, which is intercepted between the level of the two places of obſervation, hath not been the ſame, perhaps, in any two different parts; yet the variation of denſity and height will be exhibited, through the whole of this ſmall ſpace, without ſenſible error, by the curve, which would have repreſented them ſtrictly, if the entire column had been of the mean temperature of the extremes of this portion of it; namely, by the logarithmic, whoſe ſubtangent is expreſſed in 1000ths of a Paris toiſe, by $B \pm \frac{n}{215} B$, B denoting the ſubtangent of the Briggian ſyſtem. Imagine, therefore, that the barometer and thermometer have been obſerved at

(s) Recherch. ſur les Modif. de l'Atm. §. 663.

the ſame time, at three different heights. Put the difference of the tabular logarithms of the heights of the quickſilver, at the 1ſt and 2d elevation L; at the 2d and 3d, L″. Imagine the thermometer at the 1ſt, or loweſt height, to have been $+ 16\frac{3}{4}$; at the 2d, $16\frac{3}{4} + 2n$; at the third, $16\frac{3}{4} - 2m$.

Then the diff. between 1ſt and 2d elevation, $= L + \frac{n}{215} L$

between 2d and 3d, $= L'' + \frac{n-m}{215} L''$

that is, the variations of height and denſity between each two of theſe three places, are nearly exhibited by two different logarithmics, the ſubtangents of which are $B + \frac{n}{215} B$; $B + \frac{n-m}{215} B$; therefore, the variations of height and denſity, throughout the atmoſphere, are not repreſented by any *one* logiſtic, when the temperature is unequal (as they would be, if, notwithſtanding an inequality of temperature, the abſolute elaſticities were the ſame in all parts); but by parts of different logiſtics, at different heights; as they ſhould be when the abſolute elaſticities are different, in different parts, and can only be conſidered as uniform to a ſmall diſtance, above or below any height aſſigned.

The mention of M. BOUGUER occurred ſo naturally upon this occaſion, that I muſt have reproached myſelf with an impiety towards the aſhes of a man, whoſe memory will ever be dear to ſcience, had I not attempted to vindicate his conjectures upon the point under immediate conſideration, from an animadverſion thrown out, in very general terms, with little tenderneſs, and only juſt in part. At the

the ſame time, I have pointed out, in what his principal miſtake conſiſts; which was, as M. DE LUC well obſerves, that, inſtead of ſeeking a rule which ſhould be general for all heights, and vary at every height, in proportion to the temperature, he ſet himſelf to find one which ſhould be general for all temperatures, at a particular elevation. The conſequence likewiſe, which he would have deduced from the unequal elaſticity of the particles of the air, that the leaſt elaſtic would be driven to the bottom, was undoubtedly erroneous. The exceſs of temperature may fall ſometimes in the lower parts of the atmoſphere, and ſometimes at greater heights; and where the greater temperature is, there, *cæteris paribus*, the elaſticity will be greater. The argument, which M. DE LUC directs againſt the exiſtence of ſenſible inequalities of elaſtic force in aggregate maſſes of the atmoſphere, derived from the ſuppoſed effect of the winds, throughout all the regions expoſed to their agitations (t), militates only againſt the probability of *permanent* inequalities in *given* places, ariſing from ſuppoſed ſpecific differences in the original conſtitution of the particles of the air; not againſt ſuch temporary inequalities, as we aſcribe to the occaſional energy of extraneous cauſes. An inequality of temperature undoubtedly exiſts, in aggregate maſſes, more frequently than the oppoſite. And from an inequality of temperature, whether in the aggregate or the diſcrete, neceſſarily follows, for the time, an analogous inequality of abſolute elaſtic force.

(t) Recherch. ſur les Modif. de l'Atm. §. 328.

Having

Having now ſufficiently explained, what the correction is, for a variation of the temperature of the air, and whence it ariſes, I proceed to reduce M. DE LUC's *formula* to BIRD's Fahrenheit, and a ſcale of Engliſh fathom.

SECTION FIFTH.

M. DE LUC's RULES reduced to ENGLISH SCALES.

THE whole of this reduction I divide into three problems.

PROBLEM FIRST.

To find the length of the ſubtangent of the atmoſpherical curve, in thouſandths of a Paris toiſe, *the mean temperature of the air being given in degrees of* BIRD's *Fahrenheit.*

BY the compariſon of M. DE LUC's ſcale with BIRD's Fahrenheit, it appears, that $+ 16\frac{3}{4}$ of the former correſponds to $+ 69{,}25$ of the latter. Hence 69,25 is the temperature in BIRD's Fahrenheit, in which, the ſubtangent, of the atmoſpherical curve, is equal to ſo many 1000ths of a Paris toiſe, as are expreſſed by B, the ſubtangent of the Briggian ſyſtem. But the atmoſpherical ſubtangent is increaſed or diminiſhed by $\frac{1}{215}$ of this quantity for every degree of M. DE LUC's

ſcale

ſcale, above or below this given temperature; and a degree of M. DE LUC's ſcale is to a degree of BIRD's Fahrenheit as 178 to 80. Therefore, the ſubtangent varies by $\frac{8}{3827}$ of the ſame quantity, for every degree of BIRD's Fahrenheit, above or below the given temperature. Hence, if n denote the difference of the temperature of the air, or, in the caſe of unequal temperatures, the difference of the mean of the temperatures of the two ſtations, above or below 69,25 in degrees of BIRD's Fahrenheit; then, $B \pm \frac{n \times 8}{3827} B$ is the length of the ſubtangent in 1000ths of a Paris toiſe: that is, the ſubtangent of the atmoſpherical curve, in the temperature $69,25 \pm n$, is to ſo many 1000ths of a Paris toiſe, as are expreſſed by the *modulus* of the Briggian ſyſtem, as $3827 \pm \overline{n \times 8}$ to 3827.

PROBLEM SECOND.

To determine the temperature, in which the length of the ſubtangent of the atmoſpherical curve is expreſſed in thouſandths of an English fathom, *by the ſubtangent of the Briggian ſyſtem.*

FROM the number 69,25 ſubtract the 8th part of the number, to which 3827 bears the proportion of 10000 to 617; that is, from 69,25 ſubtract 29,51; the remainder 39,74 expreſſes the required temperature, in degrees of BIRD's Fahrenheit.

For, let S, Σ repreſent the ſubtangents of the atmoſpherical curves in the temperatures 69,25 and 69,25 — 29,51, reſpectively.

 Then

Then $S : \Sigma = 3827 : 3827 - \overline{29,51 \times 8}$ (by prob. I.)

But $3827 : 3827 - \overline{29,51 \times 8} = 10000 : 10000 - 617 = 10000 : 9383$.

Therefore, $S : \Sigma = 10000 : 9383$; that is, as one Paris toiſe to one Engliſh fathom. Therefore, whatever multiple S is of the Paris toiſe, or any part thereof, the ſame multiple is Σ of the Engliſh fathom, or its like part. And the length of S is expreſſed in 1000ths of a Paris toiſe, by the number which is the *modulus* of the Briggian ſyſtem (by prob. I.); therefore, the length of Σ is expreſſed by the ſame number, in 1000ths of an Engliſh fathom.

PROBLEM THIRD.

To find the equation for every degree of BIRD's *Fahrenheit in the mean temperature of the air, above or below* 39,74.

CALL the variation of the length of the ſubtangent, correſponding to an increment or decrement of one degree of BIRD's Fahrenheit, V; and let S, Σ, as before, repreſent the ſubtangents correſponding to the temperatures 69,25 and 39,74.

Now $V : S = 8 : 3827$ (by prob. I.)

And $S : \Sigma = (3827 : 3827 - \overline{29,51 \times 8} =) 3827 : 3591$ very nearly (by prob. I.)

Therefore, $V : \Sigma = 8 : 3591$.

that is, $V = \frac{8}{3591} \Sigma$. $(= \frac{1}{449} \Sigma$ nearly$)$.

Hence the length of the ſubtangent of the atmoſpherical curve, in any temperature, $39,74 \pm n$ is to its length in the temperature 39,74 as $3591 \pm \overline{8 \times n}$ to 3591; that is, putting B for the ſubtangent of the Briggian curve, $B \pm \frac{n \times 8}{3591} B$ is the length of the ſubtangent of the atmoſpherical curve, in 1000ths of an Engliſh fathom. And putting L for the difference of the tabular logarithms of the obſerved heights of the barometer, at two ſtations, corrected by the equation for the temperature of the quickſilver, and *n* for the mean difference of the temperatures of the air, in degrees of BIRD's Fahrenheit, above or below 39,74, the difference of the elevation of the two ſtations, is expreſſed in 1000ths of an Engliſh fathom by $L \pm \frac{n \times 8}{3591} L$.

Upon theſe principles I have made a table, by which the equation $\frac{n \times 8}{3591} L$ may be computed for any temperature not above + 80, nor below 0 of BIRD's Fahrenheit, and for any height leſs than 10000 fathom.

It hath already been remarked, that the temperature of the quickſilver, in the portable barometer, may happen to be very different from that of the air, at the place and time of obſervation. For this reaſon, M. DE LUC adviſes, that every portable barometer ſhould be furniſhed with two thermometers; one fixed to the frame of the barometer, to indicate the temperature of the quickſilver; another, to be expoſed, at the time of obſervation, to the open air. The uſe of the annexed tables might be anſwered by a particular diviſion of the ſcales of theſe thermometers. The thermometers which accompany the two barometers, made for the ſociety, by Mr. EDWARD NAIRNE, under my directions,

 are

are BIRD's Fahrenheits; but, to the fixed thermometer, I have had a particular ſcale applied, in which the fundamental interval, between melting ice and BIRD's boiling point, is divided into 81 equal parts. The equation for the temperature of the quickſilver is one fathom for every degree, of this ſcale, in the difference of temperatures. The thermometers, for the temperature of the air, have each a ſcale, in which the fundamental interval between melting ice and BIRD's boiling point, is divided into 120 equal parts. The point of 0 is placed at the 5th of theſe diviſions above melting ice. If n be the mean height of the thermometer, in degrees of this ſcale, at the two ſtations, the equation for the temperature of the air is $\frac{n}{300}$ L. + or —, according as n is poſitive or negative.

The place of the point of 0 upon the former ſcale is indifferent. It was put very low, that the temperatures of the quickſilver, at both ſtations, might always be above it. The computation, however, is rendered eaſier by the tables now given, than it can be by any ſuch peculiar diviſions of the ſcale.

It is to be particularly obſerved, that M. DE LUC always expoſed the thermometer, by which he meaſured the temperature of the air, *to the ſun*, if it happened to ſhine; but then the ball of his thermometer was always quite detached from the frame, which is a neceſſary precaution in this manner of uſing it. If the temperature of the air were meaſured by a thermometer, expoſed in the ſhade, I cannot but think the quantity of the equation would be different. His

His reaſon for not ſhading his thermometer from the ſun, was this: when one ſide of the thermometer is expoſed to the ſun's rays, the other is in its own ſhade; therefore, in this ſituation, he thought it the juſt meaſure of the *mean* temperature of the air, which is neither the temperature of that part on which the ſun's rays fall, nor of that from which they are intercepted, but leſs than the one, and greater than the other, or a mean between the two [u]. I confeſs, I ſhould have expected an irregularity, from heat excited by the rays of light, in their paſſage from the air into the glaſs, and from the glaſs into the quickſilver; and ſhould therefore have expoſed my thermometer in the ſhade; but the ſucceſs of M. DE LUC's experiments ſeems ſufficiently to juſtify the method he hath taken.

I ſhall cloſe this ſection with a brief ſolution of two phyſical problems, for which this ſeems the proper place.

PROBLEM FOURTH.

To compare the denſities of the air, at any given elevation above the ſurface of the earth, in different temperatures.

IF the barometer hath been obſerved, at any given elevation above the earth's ſurface, in different temperatures, reduce the obſerved heights of

(*u*) Recherch. ſur les Modif. de l'Atm. §. 533—536.

the

the quickſilver to a common temperature, if the temperatures of the quickſilver, as well as the air, have been different, at the different times of obſervation. Find the ſubtangent for each temperature of the air (by problem 3d); divide the corrected heights of the quickſilver by the ſubtangents correſponding to the obſerved temperatures of the air. The quotients are as the denſities in theſe temperatures reſpectively; that is, calling the heights of the mercury reduced, P, Π; the temperatures, T, Θ; the ſubtangents, S, Σ; and the denſities, D, Δ, $D : \Delta = \frac{P}{S} : \frac{\Pi}{\Sigma}$. For the denſities are as the compreſſive forces directly, and the abſolute elaſticities inverſely (by ſection 4th); and the abſolute elaſticities are as the ſubtangents (by ſection 4th); whence the truth of the rule is manifeſt.

The moſt convenient method, however, for practice will be, to make the denſity of the air, in ſome given ſtate of the barometer and thermometer, a ſtandard, with which to compare the denſities in all other ſtates of theſe inſtruments. Suppoſe, for inſtance, at the level of the ſea, 30 inches be taken for the ſtandard height of the barometer, and + 40 for that of the thermometer. Let D be the denſity of the air, at the level of the ſea, when the barometer is 30 inches, and the thermometer + 40. Put P = 30 inches, and S = the ſubtangent of the logiſtic correſponding to the temperature + 40. Now in another temperature $40 + n$, let the ſubtangent be $S + \frac{b}{m} S$. And let the height of the barometer P be changed into $P + \frac{a}{r} P$; and let Δ be the

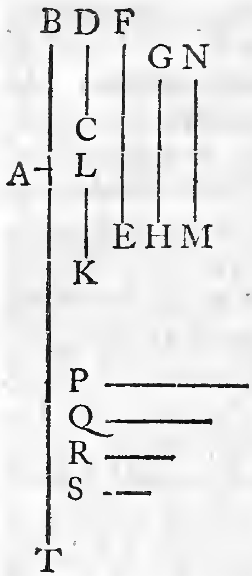
the denſity at the level of the ſea, in the temperature $40 + n$, and height of the barometer, $P + \frac{a}{r} P$. Then the difference between Δ and D, or $\Delta - D$, will be $D \times \frac{ma - br}{r \times m + b}$. Where obſerve, that b is poſitive or negative, according as n is poſitive or negative.

By this problem, the denſities of the air at a given height, in different temperatures, are to be compared with each other. Even good writers have hitherto been generally run away with by a notion, that the air's denſity, at a given elevation, at different times, would be always truly meaſured by the length of the mercurial column; than which nothing can be more erroneous, as hath been at large explained in the 4th ſection.

PROBLEM FIFTH.

The height of the quickſilver in the Torricellian tube, and the temperature of the air being given, at a given elevation, above the level of the ſea, to compare the denſity of the air with that of the quickſilver, at the time and place of obſervation.

LET TA repreſent the ſemi-diameter of the earth; T being the center, and A at the ſurface. Let B be a place at a given height, AB, above the ſurface. Let CD be the obſerved length of the quickſilver in the Torricellian tube, at the place B, in a given temperature of the air. It is required to find



find the proportion between the denſity of the air, and that of the quickſilver, the obſerved temperature being ſuppoſed to obtain uniformly throughout the whole atmoſphere; or at leaſt, throughout a great portion of it, on all ſides of B.

Suppoſe it done; and that the denſity of the air, at the time and place of obſervation, is to the denſity of the quickſilver in the barometer, as the right line CD to EF. Now the temperature of the air being given, the ſubtangent of the logiſtic correſponding to the given temperature is given (by prob. 3d). Let GH be equal to that ſubtangent; and let KL be the perpendicular height of the mercurial column ſupported in the barometer at A, the earth's ſurface, at the time of obſervation at B. Now ſince EF is to CD as the denſity of quickſilver, in the barometer, to the denſity of the air at B, and CD is the perpendicular height of the column of quickſilver ſuſtained in the Torricellian tube at the place B, at the time of obſervation, it is evident, that EF is the perpendicular height of a column of an inelaſtic fluid, of the ſame denſity with the air at B, which, if it were urged with an accelerative force through its whole length every where equal to that of gravity at B, would, by its compreſſive force, ſuſtain the mercurial column CD: for the accelerative forces

ſuppoſed

ſuppoſed to act upon the fluids in the columns EF, CD, being equal, and the heights of the columns being, reciprocally, as the denſities of the fluids, the compreſſive forces of the columns will be equal. But GH is the perpendicular height of a column of an inelaſtic fluid, of the ſame denſity with the air at A, the level of the ſea, which, if the whole were urged with an accelerative force equal to that of gravity at A, would ſuſtain the mercurial column KL (by ſect. 4). Suppoſe MN equal to the perpendicular height of a column of an inelaſtic fluid, of the ſame denſity with the air at B, which, if the whole were urged with an accelerative force, every where equal to that of gravity at A, would ſuſtain the mercurial column CD. It is evident that EF is to MN as the accelerative force of gravity at A to the accelerative force of gravity at B; for EF and MN are columns of fluids, of the ſame denſity, exerting equal compreſſive forces. Therefore, the heights of the columns muſt be reciprocally as the accelerative forces by which the compreſſive force is produced. But gravity at A is to gravity at B as the ſquare of TB to the ſquare of TA. Therefore, $EF : MN = TB^2 : TA^2$. Again, GH and MN are columns of fluids of different denſities, acted upon by equal accelerative forces; therefore, the compreſſive forces which they exert, that is, which they ſuſtain, will be as their heights and the denſities of the fluids jointly; that is, if P, Q be as the compreſſive forces of thoſe fluids, and R, S as their denſities, then $P : Q = GH \times R : MN \times S$. But the compreſſive forces ſuſtained by theſe columns are the preſſures of the air at A and B; alſo

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the densities of the fluids are the densities of the air at A and B. Therefore the compressive forces are as the densities; that is, $P:Q = R:S$. Therefore $GH \times R : MN \times S = R:S$. Therefore GH and MN are equal. Therefore $EF:GH = EF:MN$; but it hath been shewn, that $EF:MN = TB^2:TA^2$. Therefore $EF:GH = TB^2:TA^2$. But AB being given, TB is given, and the proportion of the square of TB to the square of TA is given. Therefore the proportion of EF to GH is given. And GH, is given as hath been shewn. Therefore EF is given, and the proportion of the given line EF to the given line CD, or of the density of the quicksilver to the density of the air, at B, is given. Q. E. I.

COMPOSITION.

Find the subtangent GH competent to the given temperature; find a line, EF, to which that subtangent shall bear the duplicate proportion of TA to TB; that is, of the earth's semi-diameter to the earth's semi-diameter increased by the given elevation of the place of observation, above the level of the sea. As that line EF to the observed height of the quicksilver, so is the density of the quicksilver, in the barometer actually employed, to that of the air, at the time and place of observation.

Thus the specific gravity of air may be found, comparing it with quicksilver, and by means of quicksilver with other fluids. Though it is only the most simple case of this problem that can ever come into practice, I chose to discuss it in its most general ex-

tent;

tent; as this is the only one method which gives entire ſatisfaction to the mind.

SECTION SIXTH.

I HAVE now gone through the expoſition and reduction of M. DE LUC's rules, for meaſuring heights by the barometer. The conſonance of them with theory appears, upon a ſtrict examination, to be ſuch, as ſtrongly confirms the principles, upon which the theory is founded. I ſhall conclude theſe diſquiſitions, with pointing out ſome further objects of enquiry, concerning the modifications of the atmoſphere, naturally ariſing out of M. DE LUC's diſcoveries, in conjunction with the theory already eſtabliſhed.

1. *It is probable, that the abſolute elaſticity of the air may be affected by various cauſes beſides heat.* The degree of humidity muſt occur to every one, as a circumſtance, which may reaſonably be ſuſpected to have ſome influence upon it; and, perhaps, the ſtate and quantity of the electricity of the air may have more.

2. *If M.* DE LUC's formulæ *are to be admitted as univerſally true, in all imaginable temperatures, there is a given temperature in which the elaſticity of the air would be deſtroyed, and, in any lower temperature, it would be negative; that is, the repulſion would be changed into attraction.* This given temperature is, —409,13 of BIRD's Fahrenheit; for if $B + \frac{n \times 8}{3591} B$, be the length of the

ſubtangent of the atmoſpherical curve, in 1000ths of an Engliſh fathom, whatever be the value of n, then, when $n = -448,875$, $B - \frac{8 \times n}{3591} B = 0$; that is, the ſubtangent vaniſhes; and the abſolute elaſtic force, which is always as the ſubtangent (by §. 4.) muſt vaniſh with it; but when $n = -448,875$ the temperature is $-409,13$.

Perhaps it may be thought more probable, that the variation of the ſubtangent, or of the elaſtic force, is not preciſely as the variation of temperature. If the ſubtangent changes in a *geometrical* proportion while the temperature, as ſhewn by the thermometer, changes arithmetically, the ſubtangent, or the abſolute elaſtic force, will not vaniſh with any aſſignable decrement of temperature; and in that temperature, in which it ſhould vaniſh, accords to M. DE LUC's *formulæ*, it will ſtill remain more than $\frac{3}{10}$ths of what it is in the temperature $+39,74$; and yet the equation, for an increment or decrement of temperature, amounting to 40°, will not differ from M. DE LUC's by more than four fathom in the height of 1000. I muſt repeat, that I am now only pointing out the concluſions of theory, as hints of further enquiry. I do not mean to ſubſtitute this hypotheſis as more accurate than M. DE LUC's in practice; I do not affirm, that it is more true in theory. I mean only to ſuggeſt, that if M. DE LUC's *formulæ* are admitted as mathematically true, a conſequence will follow, which may ſeem to ſome unlikely to obtain in nature. That however this conſequence, if otherwiſe improbable (which is not the opinion to which, for

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my own part, I incline) is not to be too haſtily adopted, upon the bare evidence of its ariſing out of M. DE LUC's *formulæ*; becauſe the general extent of theſe *formulæ* is more than experiment hath hitherto proved. And I ſhew, by a very ſimple inſtance, that it is eaſy to imagine other laws, between the temperature and the elaſticity, which, to all practical purpoſes, would be the ſame as M. DE LUC's, within the utmoſt limits of his experiments, and even beyond them, and yet differ from it in remote conſequences.

If M. DE LUC's *formulæ* be univerſally and ſtrictly true, the conſequence I have pointed out muſt be admitted; and it will follow, that the force of elaſticity owes its firſt production and origin to temperature: or, at leaſt, that the relation between heat and elaſticity, if not the moſt intimate one of cauſe and effect, is that which ſtands the next in the ſcale of natural affinities, namely, that they are inſeparable effects of ſome common cauſe. And theſe concluſions will hold, if the form of the general expreſſion be true, though the quantity of the equation $\frac{n \times 8}{3591}$ B ſhould require a correction: for a change of the fraction $\frac{8}{3591}$ will only alter the particular temperature, in which the ſubtangent and elaſticity vaniſh.

3. *The diminution of the air's denſity, as we aſcend from the ſurface, is ſubject to a limit.* This limit is different, in different heights of the barometer, at the level of the ſea, and even in a given height of the barometer, in various temperatures. But the denſity even at an infinite height is never 0, or infinitely ſmall. In fig. 1. through C draw

C draw CF, parallel to AD, meeting the atmoſphe-rical curve in F. CF is the denſity of the atmo-ſphere, at an infinite height (by p. 231). The pro-portion of CF to the denſity at the level of the ſea is given in any uniform given temperature of the air. For the uniform temperature being given, the ſubtan-gent of the curve DF is given; and conſequently, the proportion is given, of which the given line CA is the logarithm. The diminution of com-preſſive force is ſubject to a correſponding limit.

This curious circumſtance hath been remarked by no one, that I know of, but Dr. BROOKE TAYLOR, whoſe writings are too little attended to, even among mathematicians, from an opinion which prevails of their obſcurity. That conſummate geometer ſeems indeed to have thought it im-probable, that a finite denſity of the atmoſphere, at infinite diſtances, though the neceſſary conſequence of the theory, ſhould actually exiſt; and, for this reaſon, he would imagine ſuch a law of the elaſtic force, as ſhould render the denſity of the atmoſphere, beyond a certain height, much greater than in proportion to the compreſſive force; and circumſcribe the whole within narrow limits [(w)]. And if the theory inferred a *great* denſity at infinite or even at great heights, ſuch a den-ſity as would ſenſibly diſturb the motions of the pla-nets, it would be neceſſary to have recourſe to ſome ſuch hypotheſis; but, as this is not the caſe, I ſee no ſuch neceſſity. I know not, for what reaſon, mathe-maticians have been afraid to admit the infinitude of the atmoſphere of the earth; whether they thought it would bear hard upon the Newtonian doctrine

(*w*) Method. Increment. prop. 26. Scholium.

of a void; or, that it implied the infinitude of matter. But neither the one nor the other of these consequences is to be apprehended: for neither the phænomena of nature, nor the principles of the Newtonian philosophy, require, that there should be any where a great chasm in the universe; or that the whole material world should be actually circumscribed within any finite space. A large proportion of *pore*, or interspersed vacuity, is sufficient for all purposes. Nor doth an absolute infinity of matter by any means follow from the hypothesis of an infinite *number* of finite masses; and an infinite number of finite masses is all that is implied in the notion of a rare elastic fluid, diffused throughout infinite space. I agree, indeed, with Mr. COTES, that there are no *data* from which any great altitudes of the atmosphere can indubitably be concluded, in the way of experiment: but I do contend, that there are no *data*, from which the supposition of its infinite height can, in the same way, be disproved. And this may justly be held more probable than the contrary, as being the consequence of a theory which hath never yet, in any instance, proved fallacious; and this I venture to assert, with the less hesitation, as, besides the evident reason of the thing, the great authority of NEWTON is on my side. The infinite extent of the earth's atmosphere is manifestly supposed, in that subtle disquisition concerning the tails of comets, which occurs in the posthumous work *De Systemate Mundi*. Instead of framing hypotheses, therefore, to remove imaginary difficulties, I shall pursue the theory as far as it will lead; and my next step shall be, to exhibit the successive rarefactions of the atmosphere, as we ascend from the earth, in a table.

Heights

	Heights in miles	Volume.
0,000	00,0	1
0,01	40,0	3069
0,02	80,8	$\overline{3069}\rceil^{2}$
0,03	122,6	$\overline{3069}\rceil^{3}$
0,04	165,0	$\overline{3069}\rceil^{4}$
0,05	208,5	$\overline{3069}\rceil^{5}$
0,06	252,1	$\overline{3069}\rceil^{6}$
0,07	298,2	$\overline{3069}\rceil^{7}$
0,08	344,5	$\overline{3069}\rceil^{8}$
0,09	391,9	$\overline{3069}\rceil^{9}$
0,10	440,3	$\overline{3069}\rceil^{10}$
0,11	489,7	$\overline{3069}\rceil^{11}$
0,12	540,6	$\overline{3069}\rceil^{12}$
0,20	990,6	$\overline{3069}\rceil^{20}$
0,30	1698,1	$\overline{3069}\rceil^{30}$
0,40	2641,7	$\overline{3069}\rceil^{40}$
	Semidiameter.	
0,50	1,00	$\overline{3069}\rceil^{50}$
0,60	1,50	$\overline{3069}\rceil^{60}$
0,70	$2\frac{1}{3}$	$\overline{3069}\rceil^{70}$
0,80	4,00	$\overline{3069}\rceil^{80}$
0,90	9,00	$\overline{3069}\rceil^{90}$
0,98	49,00	$\overline{3069}\rceil^{98}$
0,9836 +	62,00	$\overline{3069}\rceil^{98,36+}$
1,00	Infinite.	$\overline{3069}\rceil^{100}$

This table ſhews the rarefactions of the atmoſphere at different heights, above the ſurface of the earth, upon a ſuppoſition, that the temperature, throughout the whole, is that which is indicated by + 40° of BIRD's Fahrenheit. The middle column ſhews the heights above the ſurface, correſponding (according to the conſtruction of the atmoſpherical logarithmic, §. 2) to the decimal parts of the earth's ſemi-diameter expreſſed by the numbers in the firſt column; and in the third the correſponding rarefactions, or the proportions of the ſpaces which a given quantity of air would occupy at the different heights, are expreſſed by the powers of the number 3069.

The principle and method of the conſtruction of this table is obvious. I compute the length of the mean ſemi-diameter of the earth in thouſandths of an Engliſh fathoms. I divide that number by 100. The quotient is the tabular logarithm of the 100th root of a number, which is to unity, as the denſity at the ſurface to the denſity at an infinite height, in the

the temperature ſuppoſed; or, which is the ſame thing, as the ſpace occupied by a given quantity of air, at an infinite height, to the ſpace occupied by the ſame quantity, at the ſurface. This root I call the *radical number*. Then, imagining the earth's ſemi-diameter, divided into 100 equal parts, and numbering thoſe parts 1, 2, 3, &c, downwards, towards the center, I compute the heights, above the ſurface, correſponding to thoſe parts ſucceſſively, according to the conſtruction of the atmoſpherical logarithmic, §. 2; and, writing the reſulting numbers in the ſecond column, in the third, I write the powers of the radical number, increaſing by unit, in regular ſucceſſion downwards.

The proportion in which the atmoſphere will be rarefied, at given heights above the ſurface, will be very different, in different temperatures. It may always be exhibited by a table of this form, but every different temperature will have its own radical number. The radical number, in the temperature + 40, is 3069; and, for any other given temperature, may be thus found. Call the tabular logarithm of 3069, y; and let n be the number of degrees of BIRD's Fahrenheit, in the difference between the given temperature, for which the radical number is to be found, and + 40. Then $y \pm \frac{n}{449 \mp n} y$ will be the tabular logarithm of the radical number, for the temperature aſſigned: obſerving, that n is to be poſitive or negative in the denominator of the coefficient $\frac{n}{449 \pm n}$, according as

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the given temperature is greater or leſs than + 40; and that the term $\frac{n}{449 \pm n} y$ is negative when n in the denominator of its coefficient is poſitive, and *vice verſa.* Thus the radical number, for the temperature 0, is 6731,2 +.

This table is not intended for any practical purpoſes; but merely for ſpeculative amuſement. They, who take delight in the contemplation of final cauſes, will remark, with admiration, how large a part of the whole rarefaction of the atmoſphere is performed on this ſide of the moon. Indeed there is comparatively but a very ſmall part performed beyond it: ſo that the moon revolves at a diſtance where the reſiſtance, from the earth's atmoſphere, is reduced almoſt to its *minimum.* For if we imagine a ſeries of quantities, conſiſting of 101 terms, decreaſing from the firſt continually in geometrical proportion; and ſuch, that the firſt and greateſt ſhall be to the laſt and leaſt, as the denſity of the atmoſphere, at the earth's ſurface, to the denſity at an infinite diſtance; the denſity, at the mean diſtance of the moon, will be leſs than the 99th of theſe, or the leaſt but two.

4. *An increaſe of temperature rarefies the lower regions of the atmoſphere, more in proportion than the upper, and brings the conſtitution of the whole nearer to that of an uniform denſity.* Though the effect of heat alone, upon every orb of the atmoſphere ſeparately, is to rarefy each, perhaps, proportionally, yet its operation upon the different orbs is ſo modified, by the different degrees of preſſure they

they ſuſtain, and by the communication between them, that the greater the temperature of the atmoſphere, though uniform throughout, the leſs will the proportion be of the denſity at any given height to the denſity at any greater given height. For the greater the temperature, the greater the ſubtangent; and the greater the ſubtangent, the leſs the proportion of AD to CF, of which the given line AC is the logarithm. (*Vid.* fig. 1.)

5. *If at any height above the ſurface of the earth a given alteration of temperature diminiſh the air's denſity in the ſame proportion, as it increaſes the abſolute elaſticity, or* vice verſâ, *the* preſſure *of the ſuperincumbent atmoſphere, at that height, will remain unchanged. At all lower heights, the preſſure will be leſs, than in a cooler condition of the atmoſphere, and greater at all greater heights. On the contrary, the preſſure at all lower heights will be greater than in a warmer condition, and at all greater heights leſs.* For let CA (fig. 2.) repreſent the ſemi-diameter of the earth, the curve DEF the atmoſpherical logarithmic for a certain temperature, and GHK the logarithmic for another greater temperature. Let the ordinates of the two curves AD, AG be as the denſities, at the earth's ſurface, in the different temperatures, to which the curves belong, reſpectively. Then it is evident, the ordinates βE, βH, drawn through any other point β in the aſymptote, will be as the denſities, at the height to which Aβ correſponds, in the different temperatures, reſpectively. Now, ſuppoſe that the denſity of the air, at the height B, in the greater temperature, is leſs than the denſity at the

the ſame height in the cooler temperature, in the ſame proportion, as the abſolute elaſticity in the greater temperature exceeds the abſolute elaſticity in the leſs. Then I ſay firſt, that the preſſure of the ſuperincumbent atmoſphere at B, is the ſame in both temperatures. For take $C\beta = \frac{CA^2}{CB}$, and draw the ordinate βE, cutting the curves in E and H, and through E and H draw tangents to the curves, EL, HM, meeting the common aſymptote, AC, in L and M. Now the ſubtangents βL, βM, are as the abſolute elaſticities in the different temperatures (by p. 250.). And βE, βH are as the denſities at B (by conſtruction). Therefore $\beta H : \beta E = \beta L : \beta M$. Therefore $\beta H \times \beta M = \beta E \times \beta L$. But the rectangle $\beta E \times \beta L$ is equal to the area intercepted by the ordinate βE, the curve EF, and the aſymtote βC, infinitely produced. And the rectangle $\beta M \times \beta H$ is equal to the area intercepted by the ordinate βH, the curve HK, and the aſymptote βC infinitely produced. Therefore theſe areas are equal. And theſe areas are as the preſſures of all the atmoſphere above B, in the temperatures to which the curves belong, reſpectively. Therefore the preſſures in theſe different temperatures are equal. *Q. E. D.* I ſay further, that the preſſure of the ſuperincumbent atmoſphere, at any height below B, is leſs in the greater temperature than in the cooler. Let AP be any height leſs than B, and take $Cp = \frac{CA^2}{CP}$, and draw the ordinate pN, cutting the curves in the points N and O. Now pN, βE being as the denſities at P and B, in

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the cooler condition of the air, and pO, βH as the densities at the same heights P and B, in the increased temperature, pO is less than to bear to βH, the proportion of pN to βE (by 4th of this): and, by permutation, pO is less than to bear to pN the proportion of βH to βE, or of βL to βM. Therefore $pO \times \beta M$ is less than $pN \times \beta L$; that is, the area intercepted between the ordinate pO, the curve OHK, and the asymptote pC infinitely produced, is less than the area intercepted by the ordinate pN, the curve NEF, and the asymptote pC infinitely produced. But again, these areas are as the pressures of all the atmosphere above P, in the temperatures to which the curves respectively belong; therefore, the pressures in the greater temperature, to which the curve OHK belongs, is less than the pressure in the cooler temperature, to which NEF belongs. *Q. E. D.* In like manner it may be shewn, that the pressures in the warmer temperature, at all heights above B, are greater than in the cooler.

And thus theory might have brought us to expect a phænomenon, which M. DE LUC hath actually observed, and was not a little surprised at. For if the temperature of the atmosphere be at any time gradually augmented, a barometer, placed below the height of stationary pressure, will sink, while another, placed higher up in the atmosphere, will rise. This is what M. DE LUC hath observed in two barometers at the foot and summit of a hill, in settled weather, while the natural heat of the day hath been upon the increase; from whence he, with

reason,

reafon, concludes, that between thefe two ftations, where the preffure was changing, at the fame time, contrary ways, there muft have been an intermediate one, which I call the height of ftationary preffure, where no change, in either fenfe, could take place.

I fhall hereafter fhew, at what height the place of unaltered preffure fhould fall, by theory, for every change of temperature. It feems a problem worthy of a naturalift, to enquire how far theory doth, in this circumftance, agree with the real operations of nature.

6. It may feem, perhaps, ftill more furprizing, but it is no lefs true, that *there will generally be a particular height in the atmofphere where the* denfity *will remain unchanged, by a given change of temperature.* To determine in what changes this will happen, and at what height the place of unaltered denfity, for given changes of temperature, fhould fall, requires only the folution of the following problem.

PROBLEM FIRST.

To find the interfection of two logarithmics, which have a right line given in pofition for their common afymptote, and their fubtangents given in magnitude; an ordinate in each curve, drawn at right angles with the common afymptote, through a given point in it, being alfo given in magnitude.

IMA-

IMAGINE two logarithmics, CDE, FDG (fig. 3, 4, 5, 6.), having the right line AB, given in poſition, their common aſymptote; and ſuppoſe the ſubtangent of each curve given in magnitude. Through a given point A, in the common aſymptote, imagine the right line AC drawn, an ordinate at right angles with the aſymptote, meeting the curves in C and F; and let AC, AF be ſeverally given in magnitude. It is required to find the point where theſe curves interſect. Suppoſe it done, and let D be the interſection. Draw DL perpendicular to AB. Through F, the point where AC meets one of the curves FDG, draw FM parallel to AB, meeting the other curve CDE in M. Draw MN perpendicular to AB, and take AH, AK equal to the given ſubtangents of the curves, CDE, FDG, reſpectively. Now AL is the logarithm of the *ratio* of AF to LD, in the ſyſtem of the curve FDG; and (becauſe NM = AF) NL is the logarithm of the ſame *ratio*, in the ſyſtem of the curve CDE. Therefore, AL : LN = AK : AH. Therefore the proportion of AL to LN is given; and conſequently, that of AN to AL is given. But AN is given in magnitude. For AC and AF are given in magnitude (by hypotheſis). Therefore, the proportion of AC to AF or NM is given; and AN is the logarithm of that given proportion in the ſyſtem of the given curve CDE. But AN being given in magnitude, and the proportion of AN to AL being given, AL is given in magnitude. And it is given in poſition, and the point A is given (by hypotheſis). Therefore the point L is given (by 27. dat.). Therefore,

LD,

LD, being perpendicular to AL, is given in poſition (by 30. dat.). But AL being given in magnitude, the proportion of AF to LD is given (by logarithms). And AF is given in magnitude. Therefore LD is given in magnitude. Therefore the point D is given. *Q. E. I.*

The conſtruction is obvious. It is evident, that the points L and N are on the ſame ſide of A, if F be at the greater curve, as in fig. 3 and 4; but on different ſides of A, if the curve to which F belongs be the leſs, as in fig. 5 and 6 [x].

The calculation of the lengths AL, LD, by means of the logarithmic canon, is very ſimple. Putting B for the ſubtangent of the Briggian ſyſtem, L for the tabular logarithm of AC, and D for the difference of the tabular logarithms of AC, AF, we ſhall have,

$$\text{Firſt, } AL = \frac{AH \times AK \times D}{B \times KH}.$$

$$\text{And again, } L \mp \frac{\overline{AK} \times \overline{D}}{KH} = \text{tab. log. of } LD.$$

In this ſecond expreſſion, the ſecond term is negative, if the greater of the given ordinates belong to the leſs curve, as in fig. 3 and 5; but poſitive if the greater ordinate belong to the greater curve, as in fig. 4. and 6.

Both theſe theorems are ſo eaſily derived from the preceeding analyſis of the problem, that it is needleſs to add the ſynthetic demonſtration; but they may be reduced to more commodious forms for practice by the following artifice.

(*x*) By the greater and the leſs curve I mean that which hath the greater or the leſs ſubtangent.

Firſt,

First, if AH be lefs than B, and AK greater,

put $AH = B - \frac{p}{q}B$, and $AK = B + \frac{s}{t}B$. Then $HK = \overline{\frac{s}{t} + \frac{p}{q}}\,B$.

And fubftituting thefe values for AH, AK, and HK, refpectively, we fhall have, by the theorem, for AL,

$$AL = \frac{qt + qs - tp - ps}{qs + tp}D.$$

And hence if $t = q$, $AL = \frac{q+s}{p+s}D \times \overline{1 - \frac{p}{q}}$.

2. If AH and AK both exceed B,

put $AH = B + \frac{p}{q}B$, and $AK = B + \frac{s}{t}B$. Then $HK = \overline{\frac{s}{t} - \frac{p}{q}}\,B$.

In this cafe $AL = \frac{qt + qs + tp + ps}{qs - tp}D$.

And if $t = q$, $AL = \frac{q+s}{s-p}D \times \overline{1 + \frac{p}{q}}$.

3. Again, if both AH and AK be lefs than B,

put $AH = B - \frac{p}{q}B$, and $AK = B - \frac{s}{t}B$. Then, $HK = \overline{\frac{p}{q} - \frac{t}{s}}\,B$.

And fubftituting thefe values, $AL = \frac{qt - qs - tp + ps}{tp - qs}D$; or if $t = q$,

$$AL = \frac{q-s}{p-s}D \times \overline{1 - \frac{p}{q}}.$$

And, by the theorem, for LD, we fhall have by due fubftitution, in the firft cafe,

$L \mp \frac{qs + qt}{qs + tp}D =$ tab. log. LD; or, if $t = q$,

$$L \mp \frac{s+q}{s+p}D = \text{tab. log. LD}.$$

 In

In the 2d case, $L \mp \frac{qt+qs}{qs-tp} D = \text{tab. log. } LD$; or, if $t=q$,

$$L \mp \frac{s+q}{s-p} D = \text{tab. log. } LD.$$

In the 3d case, $L \mp \frac{\overline{t-s} \times q}{tp-qs} D = \text{tab. log. } LD$; or, if $t=q$,

$$L \mp \frac{q-s}{p-s} D = \text{tab. log. } LD.$$

The *formulæ*, in which q and t are equal, will be of particular uſe in the application of this theory to the atmoſphere, of which examples will ſhortly be given.

In Caſe 1, if $\frac{p}{q} = 0$; that is, if $AH = B$,

$$AL = D \times \overline{1 + \tfrac{t}{s}},$$

$$\text{and } L \mp D \times \overline{1 + \tfrac{t}{s}} = \text{tab. log. } LD.$$

$$\text{But if } \tfrac{s}{t} = 0, \ AL = D \times \overline{\tfrac{q}{p} - 1};$$

$$\text{and } L \mp D \times \tfrac{q}{p} = \text{tab. log. } LD.$$

COR. *The interſection of two logarithmics being given, which have a right line given in poſition for their common aſymptote, and ſubtangents ſeverally given in magnitude; to find the point in the common aſymptote, through which the ordinate, drawn at right angles with the aſymptote, is cut by the curves in a given proportion, and to aſſign the magnitude of each ſegment,* is the converſe of the foregoing problem; and the ſame principles lead to its ſolution. For ſuppoſe A the point required. The proportion of AC to to AF being given (by hypothefis), the length of AL will be given, from the fame analyfis as before. But D being given, and the right line AB given in pofition, LD is given in pofition and magnitude; and, AL being given, the proportion of LD to AC, and alfo to AF, is given (by logarithms). Therefore AC and AF are each given.

The expreffion for the length of AL is the fame as before. And tab. log. $LD \pm \frac{AK \times D}{KH} = L$.
In this expreffion the fecond term is pofitive, if the ordinate from A to the leffer curve is to be the greater of the two; in the contrary cafe, negative.

Now imagine CDE, FDG to be logarithmics of the atmofphere, in different temperatures; AC being the denfity at the earth's furface in one temperature, and AF in the other; and let AB be the femi-diameter of the earth: let the two logarithmics meet in D, and draw DL perpendicular to AB. Now if the point L be any where in the line AB, between A, which is at the furface of the earth, and B, which is the center, the ordinate LD will reprefent the denfity of the air, in the fyftem of both curves, at the diftance $\frac{BA \times AL}{BA - AL}$ above the earth's furface; and therefore, at this height, the denfity is the fame in the one temperature as the other.

If L coincide with B, DL reprefents the denfity at an infinite height; but if L falls beyonds B, DL is not among the ordinates, of either curve, which reprefent denfities any where exifting. The exprefſion $\frac{BA \times AL}{BA - AL}$, which is infinite when $AL = AB$, now becomes more than infinite, the denominator being negative; and there is no height in the atmoſphere, at which the denſity is the ſame in both temperatures. Again, if L fall above the earth's ſurface (as in fig. 4. and 6.), LD is not among the ordinates of either curve which repreſent denſities; and AL, being negative, the expreſſion $\frac{BA \times AL}{BA - AL}$ becomes $-\frac{BA \times AL}{BA + AL}$; which expreſſes a diſtance below the earth's ſurface; but whether L fall above or below the ſurface depends upon the ſtate of the denſity at A, and the temperature jointly. If the denſity were to be greater, in the greater temperature, then the greater ordinate at A belongs to the greater curve (as in fig. 4. and 6.), and L is above the ſurface: but if the denſity be leſs, when the temperature is greater, then, of the two ordinates at A, the leſs belongs to the greater curve, and L is below the ſurface (as in fig. 3. and 5.); and in this caſe, the place of the point L depends upon the magnitude of AN, and the proportion of HK to AK. If $HK : AK = AN : AB$, then L and B (in fig. 3. and 5.) coincide, and the denſities are the ſame at an infinite height. If HK be leſs than to bear to AK, the proportion of AN to AB, AL will be greater than AB, and L will fall beyond B, and the denſities are no where the ſame. But if HK be greater than to bear to AK, the proportion of AN to AB, AL will be leſs than AB; and in this caſe thre will

will be a height above the earth's ſurface, namely, $\frac{BA \times AL}{BA - AL}$, at which the denſities, in both temperatures, will be the ſame. And it is evident, that this height is given, if AL be given. But AL is given, by the preceeding problem, if the ſubtangents AH, AK be given in magnitude, and the proportion of AC to AF be given. But if each temperature be given, each ſubtangent is given (by ſect. 5. prob. 3.) and the proportion of AC to AF will be given by the barometer. (ſect. 5. prob. 4.)

EXAMPLE.

1744, d	h	Brs obſerved.	Tr in.	Tr out.	Brs reduced to common temperature	Denſities, by §. 5. prob. 4.
March 26	$1\frac{1}{2}$	30,17	54	61	30,176	137904
April 2	6	29,37	56	48	29,37	138039

Hence D = 0.0004080 = 4,08 fathom.

But $p = 8$. $s = 21$. $q = 449$. $q + s = 470$. $s - p = 13$.

and $\frac{q+s}{s-p} = 36,1$.

Therefore by *formula* 2d, AL = 150 fathom; and at a height, inſenſibly greater, the denſity was the ſame in both conſtitutions of the atmoſphere.

7. *It is manifeſt, that the changes of denſity above and below the height where it remains unaltered, are contrary. If the lower denſities are diminiſhed, the higher ones are increaſed, and* vice verſâ. Notwithſtanding the mathematical evidence of theſe concluſions, I am perſuaded, it will appear to many a *phyſical* paradox,

paradox, that heat ſhould any where *condenſe.* I have already hinted at the ſolution. Heat doth not condenſe any finite maſs of matter, which is at liberty, on all ſides, to expand itſelf; but acting on a finite maſs, which hath not unlimited liberty of expanſion, it may condenſe one part by the rarefaction of another. Acting on an infinite maſs, it muſt do this; becauſe it can only tranſpoſe. It neither generates new matter, nor annihilates what already is. What is taken therefore from one part, muſt be added to another, and *vice verſâ*; otherwiſe the quantity of matter, in the whole, muſt be changed.

Imagine ABCD to be a ſmall portion of any orb of the atmoſphere (fig. 8.) AEFB, CMND, CHGA, DLBK, contiguous portions. Heat drives many of the particles, which occupy the ſpace ABCD, out of it; but it likewiſe drives out many of the particles which occupy the contiguous ſpaces. And of thoſe which are driven out of the contiguous ſpaces, many will enter the ſpace ABCD. If the particles which are driven *into* ABCD, be more in number than thoſe which are driven *out* of it, the air, in this ſpace, is condenſed, by that very cauſe, which would rarefy it, if the contiguous portions were annihilated. Thus condenſation in one part may, in an infinite fluid, muſt enſue from rarefaction in another, if the quantity of matter remains unaltered. Where then is the wonder, that the like effect ſhould follow from the cauſe of rarefaction combined with other cauſes?

8. *The caſes in which* L *falls below* B, *or above the ſurface, ſeem to be phyſically impoſſible, without*

an alteration of the quantity of matter in the atmosphere; for without this, it cannot be every where rarefied, or every where condensed at one and the same time.

9. *Having found the heights where the density is the same in any two different temperatures, the height where the pressures in different temperatures are equal, will easily be determined.* For this purpose, we have only to seek the solution of the following problem.

PROBLEM II.

Two logarithmics (DQF, GQK) *intersecting in a given point* (Q), (*Vid. fig.* 2.), *having a right line* (AC) *given in position for their common asymptote, and subtangents severally given in magnitude; to find the point* (β) *in the asymptote, where an ordinate* (βHE) *being drawn at right angles, to meet both curves, the areas intercepted between the two curves and the common asymptote infinitely extended, beyond the ordinate* βE, *are equal.*

LET βL, βM be equal to the subtangents of the curves DQF, GQK, respectively. Because the curvilinear areas are equal; therefore, the rectangles $\beta L \times \beta E$, $\beta H \times \beta M$, are equal. Therefore, $\beta E : \beta H = \beta M : \beta L$. But βM, βL, being given, the proportion of βM to βL is given. Therefore, the proportion of βE to βH is given. Therefore, the point β is given (by corollary of preceeding problem).

CON-

CONSTRUCTION.

Find the point β (by corollary of preceeding pre-problem) ſuch that the ordinate βHE being drawn, βE, βH, may be reciprocally as the ſubtangents; or that βE may be to βH as the ſubtangent of the curve GHK to that of DEF: and the thing propoſed is done.

Now imagine the curves GQK, DQF, to repreſent the logarithmics correſponding to different given temperatures of the air, A being at the ſurface of the earth, and C at the center. And having found the point β, take $AB = \frac{A\beta \times CA}{CA - A\beta}$; then B is the place where the barometer will ſtand at the ſame height, in both conditions of the atmoſphere.

LIMITS.

Draw the ordinates, AGD, CFK, and QR. If the point Q be any where below the ordinate through A, it is evident, the point β will lye above the point R. And if the ſubtangents βL, βM, be reciprocally as the denſities AD, AG, the points β, A, and conſequently B, A, coincide, and the preſſure, at the level of the ſea, remains unchanged.

If the greater ſubtangent βM be greater than to bear to the leſs βL, the proportion of the greater denſity AD to the leſs AG, β will fall above A, and B conſequently will be below it; for Aβ becoming

 negative,

negative, the expreſſion $\frac{CA \times A\beta}{CA - A\beta}$ becomes $-\frac{CA \times A\beta}{CA + A\beta}$. And there is no place in the atmoſphere where the preſſure is the ſame in one condition as the other. But it is, at all heights, greater in the warmer condition (by 5th of this).

If the point Q fall below the central ordinate CK, (fig. 7.) and the ſubtangents βL, βM, be reciprocally as CF, CK, the points β and C will coincide, and B will go off to an infinite height.

If the greater ſubtangent βM be leſs than to be βL as the greater denſity CF to the leſſer CK, β will be found below C, and the ordinate βE is not one of thoſe, by which the denſity of the air, at any height, in either condition, is repreſented. The expreſſion $\frac{CA \times A\beta}{CA - A\beta}$ is more than infinite, the denominator being negative; and there is no height at which the preſſure is the ſame, in both conditions of the atmoſphere; but it is at all heights leſs in the warmer condition (by 5th of ths). But whenever Q falls below the ſuperficial ordinate, whether it be above or below the central ordinate, if βM be *leſs* than to be to βL as AD to AG, and *greater* than to be to βL as CF to CK, β will fall between C and A, and an equality of preſſure, in both conditions of the atmoſphere, will take place at a finite height, determined as above.

If Q ever falls above the ſuperficial ordinate through A, β will be more above it, and B will be below the earth's ſurface, and there will be no height at which the preſſure will be the ſame; but

but it will every where be greater in the warmer temperature.

The calculation for determining $A\beta$ is obvious from the foregoing solution of the problem. For putting B for the subtangent of the Briggian system, D for the difference of the tabular logarithms of AG, AD, and Δ for the difference of those of βL, βM, we have,

$$AR = \frac{\beta L \times \beta M \times D}{B \times LM} \text{ (by prob. 1.)}$$

$$\text{and } R\beta = \frac{\beta L \times \beta M \times \Delta}{B \times LM} \text{ (by cor. 1.)}$$

$$\text{Therefore, } A\beta = \overline{D - \Delta} \times \frac{\beta L \times \beta M}{B \times LM}.$$

Therefore, putting $\beta L = B \pm \frac{p}{q} B$, and $\beta M = B \pm \frac{s}{t} B$. and substituting $D - \Delta$ for D in the *formulæ* for AL, deduced from problem 1, we shall change them into *formulæ* for $A\beta$.

EXAMPLES.

In the example of the preceeding problem $D = 4{,}08$, and $\Delta = 121{,}817$ fathom. Whence $D - \Delta$ is negative, and the point B falls above the surface, and the pressure was no where the same, but at all heights greater in the warmer condition.

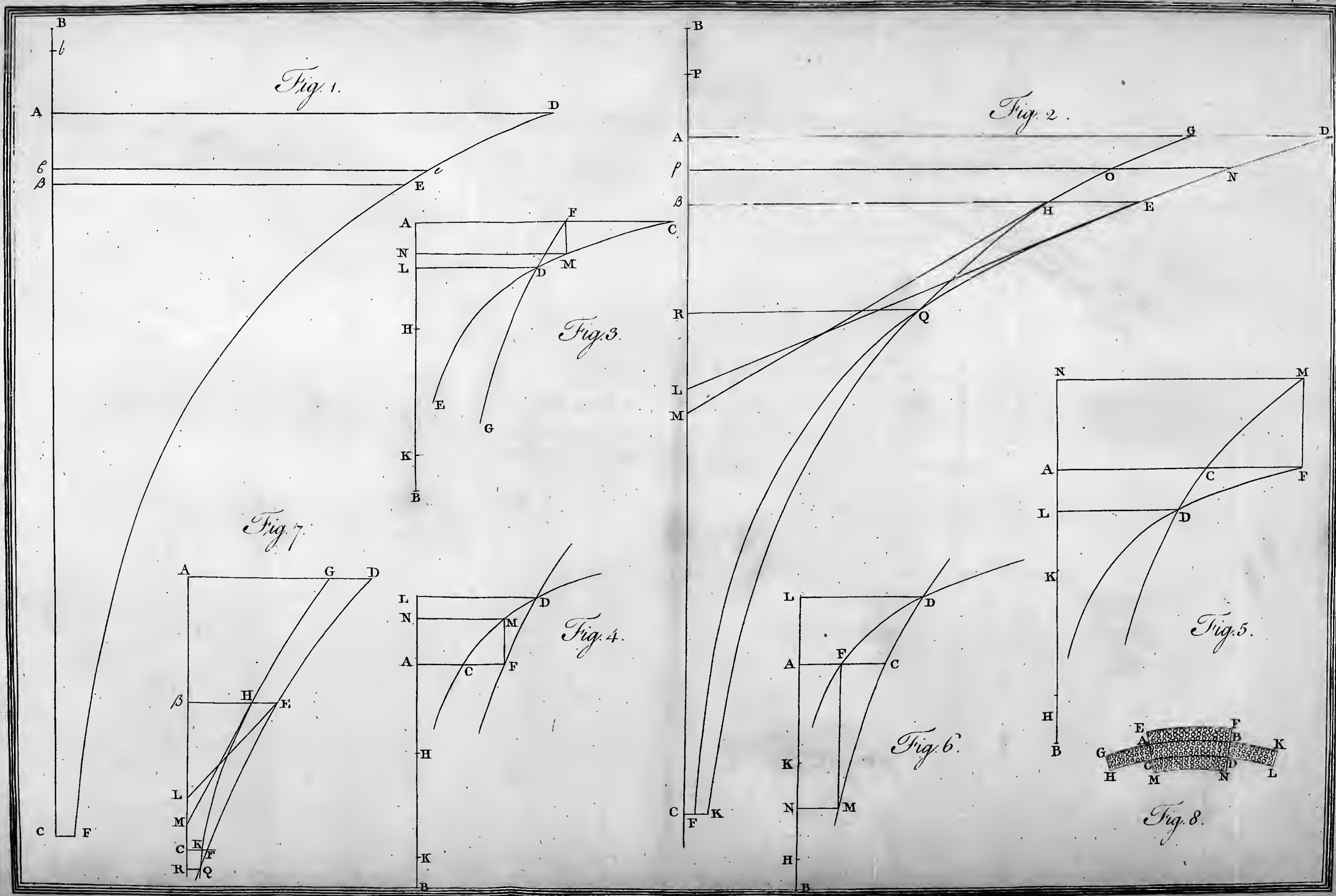


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.

EXAMPLE II.

	d	h	Br. observed.	Tr. in.	Tr. out.	Br. reduced to common temperature	Densities, by §. 5. prob. 4.
March	26	20	30,07	58	59	30,058	13796,6
	28	21	30,21	54	50	30,21	14138,2

Hence $p=10$. $s=19$. $q=449$. $q+s=468$. $s-p=9$. $\frac{q+s}{s-p}=52$.
$D=0.0106238$. $\Delta=0.0084332$. $D-\Delta=0.0021906=21,906$ fathom. Therefore, $A\beta=1164,42$ fathom $=1,32$ miles; and at a height insensibly greater, the pressure was the same in both conditions.

In this manner may the heights be determined, in which either the density or pressure of the air, in a given temperature, is the same as in any greater or lower temperature; provided the proportion be known between the densities, in the different temperatures, at the level of the sea, or at a given elevation above it; otherwise both the problems are indeterminate. And I know no method of ascertaining this proportion, but by actual observation of the barometer. The change of temperature determines the proportion in which the subtangent of the atmospherical logistic is changed; but in whatever proportion the subtangent of a logistic is changed, the ordinate, at a given point of the asymptote, may be of any imaginable length, or may bear any imaginable proportion to the subtangent. There is no given *geometrical* relation, in the nature of the curve, between the length of the one and the other; and experiment hath not yet brought to light any *physical* relation in the particular case in question. Diligent observation of the barometer and ther-

 mometer

mometer may perhaps, in future, give more ſatisfactory information upon the ſubject. Perhaps I have been too minute in detailing conſequences from an hypotheſis, of what probably never doth obtain; namely, that the atmoſphere may be conſidered as equally heated in all its different parts; and that the variation of the abſolute elaſticity of its component particles is proportional to the change in the degree of uniform temperature, as if the abſolute elaſticity were influenced by no other cauſe. But ſuch is the neceſſary order of enquiry. Theory muſt begin with the moſt ſimple caſes, conſidering what *would* be the effect of ſome known cauſe, acting ſingly, and in the moſt ſimple manner; and comparing the concluſions from what it is *ſuppoſed*, with the effects which *are*, the *difference* leads us to the diſcovery of other cauſes, and to an eſtimation of the ſeparate effects of each, and of the compound effects of all the known cauſes any how combined. I ſhall only add, that as the ſtate of an atmoſphere, unequally heated in its different parts, may be repreſented by parts of different logarithmics, it is poſſible, that inſtead of one point of unaltered denſity, and one point of unaltered preſſure, we might find ſeveral, if ſynchronous obſervations could be made at ſeveral ſucceſſive elevations ſufficiently different.

10. If the atmoſphere of the earth reaches to infinite heights with a finite denſity; for the ſame reaſons, that of Jupiter and every other planet, will reach alſo to infinite heights, above the ſurface of the planet with a finite denſity. The atmoſphere therefore of every planet will reach to the ſurface of every other planet, and to the ſurface of the Sun;

and

and the atmoſphere of the Sun to the ſurfaces of them all. All theſe atmoſpheres will mingle, and form a common atmoſphere of the whole ſyſtem. This common atmoſphere of the ſyſtem will be infinitely diffuſed, ſince the particular atmoſpheres that compoſe it are ſo. It will reach therefore to every fixed ſtar; and, for the ſame reaſon, that of every fixed ſtar will reach the central body of our ſyſtem, and of every other ſyſtem. The atmoſpheres of all the ſyſtems will mix. The univerſe will have one common atmoſphere, a ſubtle elaſtic fluid, which pervades infinite ſpace, and being condenſed near the ſurface of every larger maſs of matter, by the gravitation towards that maſs, forms its peculiar atmoſphere.

To certain diſtances from every one of theſe great bodies, the condenſations of this infinite fluid will follow the laws of the preceeding theory *nearly*; but to certain diſtances *only*. For that theory conſiders only the effects of the attraction of a ſingle ſphere, and aſſigns the law of the variation of denſity, ſuch as would obtain, if *one* ſpherical body exiſted in the midſt of an infinite fluid; and ſuch as cannot generally obtain, unleſs that hypotheſis were true: for many great ſpheres being immerſed in a common atmoſphere, the attraction of any one, at great diſtances from it, becomes but an inconſiderable part of the whole cauſe, by which the denſity is modified, the joint forces of them all. And in many other circumſtances, beſides the condenſation, the modifications of every particular atmoſphere may depend upon thoſe of others, innumerable and infinitely remote; as the *effluvia* and exhalations of each

each great maſs, mingling with its atmoſphere, may be diſtributed, in certain proportions, among all the reſt. So that there is probably no branch of phyſics, in which human diſcovery, in its utmoſt extent, muſt always bear ſo ſmall a proportion to what will ſtill remain unknown.

TABLE I.

EQUATION of the boiling point.

Barometer.	Equation.	Difference.
31,0	+ 1,57	
		0,78
30,5	+ 0,79	
		07,9
30,0	0,00	
		0,80
29,5	— 0,80	
		0,82
29,0	— 1,62	
		0,83
28,5	— 2,45	
		0,85
28,0	— 3,31	
		0,86
27,5	— 4,16	
		0,88
27,0	— 5,04	

The numbers in the firſt column to the left expreſs heights of the quickſilver in the Barometer, in Engliſh inches and decimal parts. The ſecond column gives the Equation to be applied, according to the ſign prefixed, to 212° of Bird's Fahrenheit, to find the true boiling point, for every ſuch ſtate of the barometer. The heights of the barometer decreaſe by $\frac{1}{2}$ inches from 31 to 27 inches. The boiling point, for all intermediate ſtates of the barometer, may be found, with a very ſufficient accuracy, by taking proportional parts. For which purpoſe, the differences of the equations are given in the third column.

TABLE II.

For the Comparifon of Thermometers.

	L reduced.	P reduced.
80	210,0 —	211,73 —
75	198,86 +	200,50 —
70	187,74 +	189,26 +
65	176,62 —	178,03 —
60	165,49 +	166,80 —
55	154,37 —	155,56 +
50	143,24 +	144,33 —
45	132,12 +	133,09 —
40	121,0 —	121,85 +
35	109,87 +	110,62 +
30	98,75 —	99,39 —
25	87,62 +	88,15 +
20	76,50 +	76,92 +
15	65,38 —	65,69 —
10	54,25 +	54,45 +
5	43,13 —	43,22 +
0	32.	32.
— 5	20,88 —	20,77 —
—10	9,75 +	9,53 +
—15	— 1,37 +	— 1,70 —
—20	— 12,50 —	— 12,93 +
—25	— 23,62	— 24,16 +
—30	— 34,74 +	— 35,40 —
—35	— 45,87 —	— 46,63 +
—40	— 57,00 —	— 57,86 +
—45	— 68,12 —	— 69,10 —
—50	— 79,24	— 80,33
—55	— 90,36 +	— 91,56 +
—60	—101,49 —	—102,80 —
—65	—112,61 +	114,03 —
—70	—123,75 —	125,25 +
—75	—134,87	136,48 +
—80	—146,0 —	147,72 —

	LF	PF
1	2,225	2,246
2	4,450	4,493
3	6,674	6,74
4	8,9	8,986
5	11,124	11,233

	FL	FP
1	0,449	0,445
2	0,899	0,890
3	1,348	1,335
4	1,798	1,780
5	2,247	2,225
6	2,696	2,671
7	3,146	3,116
8	3,595	3,561
9	4,045	4,006
10	4,494	4,451

EXPLA-

EXPLANATION.

The principal table conſiſts of three columns. The numbers in the firſt column ſignify degrees, either of DE LUC's ſcale, or the ſcale which hath been hitherto chiefly in uſe among the French, which is divided as M. DE LUC's is; but 28 French inches being generally mentioned, by the French mathematicians, as the mean height of the barometer at PARIS, it is to be ſuppoſed, that their boiling point will generally agree to that ſtate of the barometer. The numbers in the next column to the right, which is marked L *reduced*, give the degrees of BIRD's Fahrenheit correſponding to the degrees of M. DE LUC's ſcale, expreſſed by the numbers of the firſt column; and the numbers in the column marked P *reduced*, give the degrees of BIRD's Fahrenheit correſponding in like manner with the PARIS ſcale. The heights in the firſt column diminiſh by 5° ſucceſſively. But the reduction may be computed to every degree, by means of the little table on the left; which gives the value of ſingle degrees, either of DE LUC's, or of the PARIS ſcale, in degrees of BIRD's Fahrenheit. The titles LF, PF ſignifying DE LUC's degrees, or Paris degrees, reduced to Fahrenheit's, reſpectively.

If there ſhould be occaſion to reduce heights of BIRD's Fahrenheit, either to M. DE LUC's, or the PARIS ſcale, this may be done by the principal table, and the ſmall one on the right hand; which exhibits the value of ſingle degrees of BIRD's Fahrenheit in degrees, both of DE LUC's and the Paris ſcales: the titles FL, FP ſignifying degrees of BIRD's Fahrenheit reduced to M. DE LUC's degrees, or the PARIS degrees, reſpectively.

EXAMPLE.

To find the point upon a BIRD's Fahrenheit, which correſponds to + 58 of DE LUC's ſcale.

By principal table	+ 55 =	154,37
By little table on the left hand	3 =	6,67
Therefore	+ 58 =	161,04

EXAMPLE 2.

To find the point upon the Paris ſcale, which correſponds to + 55 of BIRD's Fahrenheit.

By principal table	+54,45	=10
55—54,45=0,55		
By little table on the right hand	0,5	= 0,22
By ditto - - -	0,05	= 0,02
Therefore	+55	=10,24

TABLE III.

EQUATION for the temperature of the QUICKSILVER.

Degrees of Bird's Fahrenh.	Correction of Diff. Logarith.	Fathom	
1	452	0,452	0,003
2	904	0,904	0,006
3	1355	1,355	0,009
4	1807	1,807	0,012
5	2259	2,259	0,016
6	2711	2,711	0,019
7	3163	3,163	0,022
8	3614	3,614	0,025
9	4066	4,066	0,028
10	4518	4,518	0,031
20	9036	9,036	0,062
30	13554	13,554	0,094
40	18072	18,072	0,125
50	22590	22,590	0,156
60	27108	27,108	0,188
70	31626	31,626	0,219

The numbers in the firſt column are to be underſtood to expreſs differences of temperature, in the quickſilver of the portable Barometers at the two ſtations, indicated by Thermometers fixed in the caſes of the Barometers, in degrees of Bird's Fahrenheit. The ſecond column gives the correſponding corrections of the difference of the tabular logarithms of the obſerved heights of the quickſilver in the barometer, to be added or ſubtracted, according as the higher barometer hath been the warmer or cooler of the two. The numbers in the third column are the $\frac{1}{1000}$th parts of thoſe in the ſecond. The manner of uſing them is explained in the general precepts. The fourth column gives the reduction of the obſerved height of the column of quickſilver, in decimal parts of an inch, for the differences of temperature expreſſed by the numbers of the firſt column, when the height of the quickſilver is 30 inches; and the reduction for any other height of the quickſilver may be thus found. Suppoſe the height of the barometer $30 \pm a$ inches, in a certain temperature b, and this is to be reduced to the temperature $b \pm n$. From the fourth column collect the number correſponding to n (by repeated entry, if need be). Call that number M. Then $M \pm \frac{a}{30} M$ is the reduction.

TABLE IV. EQUATION for the Temperature of the AIR.

	40+	50 + 30 —	60 + 20 —	70 + 10 —	80 + 0 —
1	0,0	0,022	0,044	0,067	0,089
2	0,0	0,044	0,089	0,134	0,178
3	0,0	0,067	0,134	0,200	0,267
4	0,0	0,089	0,178	0,267	0,356
5	0,0	0,111	0,223	0,334	0,445
6	0,0	0,134	0,267	0,401	0,535
7	0,0	0,156	0,312	0,468	0,624
8	0,0	0,178	0,356	0,535	0,713
9	0,0	0,200	0,401	0,601	0,802
10	0,0	0,223	0,445	0,668	0,891
20	0,0	0,445	0,891	1,337	1,782
30	0,0	0,668	1,337	2,005	2,673
40	0,0	0,891	1,782	2,673	3,564
50	0,0	1,114	2,228	3,342	4,455
60	0,0	1,337	2,673	4,010	5,347
70	0,0	1,559	3,119	4,678	6,238
80	0,0	1,782	3,564	5,347	7,129
90	0,0	2,005	4,010	6,015	8,020
100	0,0	2,228	4,455	6,683	8,911
200	0,0	4,455	8,911	13,367	17,822
300	0,0	6,683	13,367	20,050	26,733
400	0,0	8,911	17,822	26,733	35,645
500	0,0	11,139	22,278	33,417	44,556
600	0,0	13,367	26,733	40,100	53,467
700	0,0	15,594	31,189	46,783	62,378
800	0,0	17,822	35,645	53,467	71,289
900	0,0	20.050	40.100	60.150	80.200

	1	2	3	4	5	6	7	8	9
1	0,002	0,004	0,007	0,009	0,011	0,013	0,015	0,018	0,020
2		0,009	0,013	0,018	0,022	0,027	0,031	0,036	0,040
3			0,020	0,027	0,033	0,040	0,047	0,053	0,060
4				0,036	0,044	0,053	0,062	0,071	0,080
5					0,055	0,067	0,078	0,089	0,100
6						0,080	0,093	0,107	0,120
7							0,109	0,125	0,140
8								0,142	0,160
9									0,180

In the principal Table, the numbers in the firſt column to the left expreſs fathom. The numbers at the top of each vertical column expreſs degrees of Bird's Fahrenheit above the point 0. The numbers under theſe, in every column, ſhew the corrections correſponding to theſe temperatures, reſpectively, in fathoms and decimal parts, upon ſo many fathom, in an approximate determination of the height in queſtion, as are expreſſed by the numbers in the firſt column to the left. The little Table gives the correction for ſingle degrees of Bird's Fahrenheit. The numbers in the uppermoſt horizontal row, and the outermoſt vertical row, to the left, expreſſing, the one fathom, and the other degrees of the thermometer, indifferently.

GENERAL

GENERAL PRECEPTS, for the CALCULATION of HEIGHTS by these TABLES.

FROM the common tables of logarithms, namely, thoſe which exhibit the Briggian logarithms to eight places, write out the logarithms of the numbers, which expreſs the obſerved heights of the quickſilver, in the portable barometers, at the two ſtations.

2. Subtract the leſſer logarithm from the greater.

3. Divide the remainder by 1000. The quotient is a certain number of fathom.

4. Take the difference of the temperatures of the quickſilver, as indicated by the thermometers in the caſes of your portable barometers, and look for the correction correſponding thereto, in the third column of TABLE III. and add that correction to the number laſt found, if the barometer at the higher ſtation hath been the warmer of the two; otherwiſe ſubtract it. Call the ſum in the firſt caſe, the remainder in the latter, the *approximate height*.

5. Add together the temperatures expreſſed at the two ſtations by the thermometers in the open air, under their proper ſigns. Take half the aggregate, and call it the *temperature of the air*.

6. In the principal table of the equation for the temperature of the air (TABLE IV.), look a-top for the decade of degrees next leſs than the temperature of the air, found by the laſt rule; and from the column underneath it (by repeated entries if need be) collect the corrections for the units, decades, and centuries of

fathom in the approximate height; and if you have chiliads of fathom in your approximate height, find the correction for them, by taking the decuple of the correction for the corresponding centuries. Add these several parts of the correction together, and call the sum N.

7. In the little table look, either a-top or in the outer vertical row to the left, for the units of degrees in the temperature of the air, over and above decades, and collect the correction for these, for the units, decades, centuries, &c. of fathom in the approximate height, by taking the numbers found in the little table, and proper multiples thereof. Add these numbers together, and call the sum n.

8. If the sign of the correction found in the great table be +, add $N+n$ to the approximate height; but if the sign of the correction found in the great table be —, subtract $N-n$ from the approximate height. The sum in the former case, the remainder in the latter, is the correct height, according to M. DE LUC's rules.

EXAMPLE.

EXAMPLE.

Barometer.	Tr. in.	Diff.	Tr. out.	Temperature of air.
Lower 29 68	+57	14	+57	$+49\frac{1}{2}$
Upper 25,28	+43		+42	

Log. of lower Br.	14724639
of upper	14027771
$\frac{\text{Diff.}}{1000}$	696,868
Equation for ☿	— 6,327
Approximate height	690,541
Equation for air N + *n*	+ 14,5
Height correct	705,04

Diff. of Thermometers in = 14° = 10 + 4°.
10° gives 4,519
4° gives 1,808

Equation for ☿ 6,327

Temperature of air = $49\frac{1}{2}$ = 40° + $9°\frac{1}{2}$.
N = + 0

9° × 600 fathom gives	12	} by little table for air.	
9° × 90	1,8		
$\frac{1}{2}$ × 600	0,6		
$\frac{1}{2}$ × 90	0,1		

n = 14,5
N + *n* = 14,5

EXAMPLE II.

Barometer.	Tr. in.	Diff.	Tr. out.	Temperature of air.
Lower 29,45	+38	3	+31	33
Upper 26,82	+41		+35	

Log. of lower	1.4690853
of upper	1.4295908
$\frac{\text{Diff.}}{1000}$	394,945
Equation for ☿	+ 1,356
Approximate height	396,301
Equation for Air — N — *n*.	— 6,182
Height correct	390,119

Diff. of Trs. in = 3° gives equation for ☿ 1,356.
Temperature of air = 33° = 30° + 3°.

30° upon 300 fathom gives	6,683	} by great table for air. sign —
upon 90	2,005	
upon 6	0,134	

N = 8,822

3° × 300 fathom gives	2,000	} by little table for air.
× 90	0,600	
× 6	0,04	

n = 2,640

N — *n* = 6,182

XXXI. *A*

XXXI. *A Catalogue of the Fifty Plants, from* Chelsea *Garden, presented to the* Royal Society, *by the Worshipful Company of Apothecaries, for the Year* 1773, *pursuant to the Direction of Sir* Hans Sloane, *Bart. Med. Reg. et Soc. Reg. nuper Præses. By* William Curtis, *clariss. Soc. Pharmaceut.* Lond. *Soc. Hort.* Chelsean. *Præfect. et Prælector Botan.*

Presented February 17, 1774.

2501 ÆGOPODIUM (Podagraria) foliis caulinis summis ternatis. Lin. Spec. plant. ed. 3tia. 379.
Angelica sylvestris minor sive erratica. Bauh. pin. 155. 3.
Herba Gerardi. Gerard, 848. fig. 2.

2502 BALLOTA (Nigra) foliis cordatis, indivisis, serratis; calycibus acuminatis. Lin. Spec. plant. 814. 1.
Marrubium nigrum fœtidum. Bauh. pin. 230. 4.
Marrubium nigrum. Gerrard, 566.

2503 BUNIAS (Orientalis) ſiliculis ovatis, gibbis, verrucoſis. Lin. Spec. plant. 936. 3.
Crambe Orientalis dentis leonis folio, erucaginis facie. Tournef. cor. 41.

2504 CACALIA (Suaveolens) caule herbaceo, foliis haſtato-ſagitatis, denticulatis; petiolis ſuperne dilatis. Lin. Spec. plant. 1170. 10.

2505 CALCEOLARIA (Pinnata) foliis pinnatis. Lin. Mantiſ. alter. 171. deſcript. opt.
Calceolaria foliis ſcabioſæ vulgaris. Feuill. Peruv. 3. 12. t. 7.

2506 CHELONE (Glabra β) foliis lanceolatis ſerratis; ſummis oppoſitis. Lin. Spec. plant. 849. 1.
Chelone floribus ſpecioſis pulcherrimis, colore roſæ damaſcenæ. Gron. Virg. 71. Miller, Icon. 62. t. 93.

2507 CHENOPODIUM (Vulvaria) foliis integerrimis rhombeo ovatis; floribus conglomeratis. Lin. Spec. plant. 321.
Atriplex fœtida. Bauh. pin. 119. Ger. em. 327.

2508 CONIUM (Maculatum) ſeminibus ſtriatis. Lin. Spec. plant. 349.
Cicuta major. Bauh. pin. 160.

2509 CORONILLA (Varia) herbacea, leguminibus erectis teretibus, toroſis, numeroſis; foliolis plurimis glabris. Lin. Spec. plant. 1048.
Securidaca dumetorum major, flore vario, ſiliquis articulatis. Bauh. pin. 349.

2510 CROTALARIA (Chinenſis) foliis ſimplicibus, ovatis, ſubpetiolatis; ſtipulis minutiſſimis. Lin. Spec. plant. 1003.

2511 CY-

2511 Cytisus (Supinus) floribus umbellatis, terminalibus; ramis decumbentibus; foliolis ovatis. Lin. Spec plant. 1042.

Cytisus supinus foliis infra et siliquis molli lanugine pubescentibus. Bauh. pin. 390.

2512 Dianthus (Armeria) floribus aggregatis fasciculatis, squamis calycinis lanceolatis, villosis, tubum æquantibus. Lin. Spec. plant. 586. 3.

Caryophyllus barbatus sylvestris. Bauh. pin. 208.

2513 Draba (Alpina) scapo nudo simplici, foliis lanceolatis integerrimis. Lin. Spec. plant. 896. Oeder. fl. Dan. tab. 56.

Sedum alpinum luteum hirsutum. Bauh. pin. 284.

2514 Elymus (Caninus) spica nutante arcta, spiculis rectis involucro destitutis, infimis geminis. Lin. Spec. plant. 124. Buxb. Cent. 4. p. 29. t. 50.

2515 Elymus (Sibiricus) spica pendula arcta, spiculis binatis, calyce longioribus. Lin. Spec. plant. 123. Gmelin. Sibir. 1. p. 123. t. 28.

2516 Erica (Multiflora) antheris simplicibus, bifidis, exsertis; corollis cylindricis; foliis quinis patentibus. Lin. Spec. plant. 503.

Erica foliis corios multiflora. Bauh. Hist. 1. 356. Lobel. Hist. 620.

2517 Fumaria (Capnoides) siliquis linearibus, tetragonis; caulibus diffusis, acutangulis. Lin. Spec. plant. 984. Pluk. alm. 262. t. 90. Miller, Icons.

2518 Inula (Squarrosa) foliis ovalibus, lævibus, reticulato-venosis, subcrenatis; calyculis squarrosis. Lin. Spec. plant. 1240.
After luteus latifolius glaber, foliis rigidis et minutissime crenatis. Pluk. alm. 37. t. 16. f. 1.

2519 Lobelia (Triquetra) caule erecto; foliis lanceolatis, dentatis; racemo terminali aphyllo. Lin. Mantiss. 120. 27.

2520 Lupinus (Angustifolius) calycibus alternis appendiculatis; labio superiore bipartito, inferiore integro. Lin. Spec. plant. 1015.
Lupinus angustifolius cœruleus elatior. Ray Hist. 908.

2521 Lupinus (Luteus) calycibus verticillatis, appendiculatis; labio superiore bipartito, inferiore tridentato. Lin. Sp. pl. 1015. 6.
Lupinus sylvestris flore luteo. Bauh. pin. 348.

2522 Lantana (Involucrata) foliis oppositis; caule inermi; floribus capitato-umbellatis, involucrato-foliosis. Lin. Spec. plant. 874.
Camara arborescens salviæ folio. Plum. gen. 32. Ic. 71. fig. 2.

2523 Mirabilis (Longiflora) floribus congestis, terminalibus, longissimis, nutantibus; foliis subvillosis. Lin. Spec. plant. 252.
Mirabilis Mexicana. Hern. Mex. 170. f. 2.

2524 Monarda (Punctata) floribus verticillatis; corollis punctatis. Lin. Spec. plant. 33. 4.
Clinopodium Virginianum angustifolium quovis verticillo duodecim foliolis rubentibus cincto. Pluk. alm. 111. t. 24. fig. 1.

2525 Nepeta (Cataria) floribus ſpicatis, verticillis ſubpedicellatis; foliis petiolatis cordatis dentato-ſerratis. Lin. Spec. plant. 796.

Mentha Cataria vulgaris et major. Bauh. pin. 228.

2526 Oenothera (Pumila) foliis lanceolatis, obtuſis, glabris, ſubpetiolatis; caulibus proſtratis; capſulis acutangulis. Lin. Spec. plant. 493.

Lyſimachia ſiliquoſa glabra minor mariana, anguſtioribus foliis. Pluk. mant. 123. Miller, Icon. 188.

2527 Oenothera (Fruticoſa) foliis lanceolatis, ſubdentatis; capſulis pedicellatis, acutangulis; racemo pedunculato. Lin. Spec. plant. 492.

2528 Ononis (Rotundifolia) fruticoſa; pedunculis trifloris; calycibus triphyllo-bracteatis; foliis ternatis ſubrotundis. Lin. Spec. plant. 1010.

Cicer ſylveſtre latifolium triphyllum. Bauh. pin. 347.

2529 Orobus (Vernus) foliis pinnatis ovatis; ſtipulis ſemiſagittatis integerrimis; caule ſimplici. Lin. Spec. plant. 1028.

Orobus ſylvaticus purpureus vernus. Bauh. pin. 351.

2530 Panicum (Filiforme) ſpicis ſubdigitatis, approximatis, erectis, filiformibus; rachi flexuoſa; dentibus bifloris, altero ſeſſili. Lin. Spec. plant. 85.

2531 Panicum (Capillare) panicula capillari erecta, patenti; caule hirto. Lin. Spec. plant. 85.

2532 Panicum (Polyſtachion) ſpicis teretibus; involucellis unifloris, faſciculato-ſetoſis, culmis erectis ſuperne ramoſis. Lin. Spec. plant. 82.

2533 Phlomis (Herba Venti) involucris ſetaceis, hiſpidis; foliis ovato-oblongis, ſcabris; caule herbaceo. Lin. Spec. plant. 819.
Marrubium nigrum longifolium. Bauh. pin. 230.

2534 Phlomis (Tuberoſa) involucris hiſpidis, ſubulatis; foliis cordatis, ſcabris; caule herbaceo. Lin. Spec. plant. 820.
Galeopſis maxima foliis hormini. Buxb. cent. 1. p. 4. t. 6.

2535 Phlox (Glaberrima) foliis lineari-lanceolatis, glabris; caule erecto; corymbo terminali. Lin. Spec. plant. 217.
Lychnidea folio malampyri. Dill. Hort. Elth. 203. t. 166. f. 202.

2536 Polygonum (Amphibium) floribus pentandris, ſemidigynis; ſtaminibus corolla longioribus. Lin. Spec. plant. 517.
Potamogeton ſalicis folio. Bauh. pin. 193.

2537 Psoralea (Pinnata) foliis pinnatis, linearibus; floribus axillaribus. Lin. Spec. plant. 1074.
Geniſtæ affinis arbor Africana monoſpermos, flore cœruleo, foliis pinnatis. Herm. Lugdb. 272. t. 273.

2538 Pteris (Cheuſanica). Polypodium caule ſimplici, foliis ſimplicibus variis longis ſerratis. Burm. Theſ. Zeyl. p. 196. tab. 87.

Filicula cheuſanica. Pluk. Amalth. p. 94. Phyt. t. 407. fig. 2.

2539 RANUNCULUS (Arvenſis) ſeminibus aculeatis; foliis ſuperioribus decompoſitis linearibus. Lin. Spec. plant. 780.

Ranunculus arvenſis echinatus. Bauh. pin. 179.

2540 ROSMARINUS (Officinalis). Lin. Sp. pl. 33. Bauh. pin. 217.

2541 SAXIFRAGA (Stolonifera) foliis radicalibus ſubrotundis, crenatis, hirſutis; ſtolonibus repentibus.

2542 SIEGESBECHIA (Orientalis) petiolis ſeſſilibus; calycibus exterioribus linearibus majoribus patentibus. Lin. Spec. plant. 1269.

Bidenti ſimilis foliis latiſſimis ſerratis. Buxb. cent. 3. p. 29. t. 52.

2543 SILENE (Acaulis). Lin. Spec. plant. Oed. dan. t. 21.

Muſcus alpinus lychnidis flore. Bauh. hiſt. 3. p. 767.

2544 TEUCRIUM (Virginicum) foliis ovatis, inæqualiter ſerratis; racemis terminalibus. Lin. Spec. plant. Gron. Virg. 64.

2545 TRAGOPOGON (Dalechampii) calycibus monophyllis, corolla brevioribus, inermibus; foliis runcinatis. Lin. Spec. plant. 1110.

Hieracium aſperum flore magno dentis leonis. Bauh. pin. 127.

2546 TRIFOLIUM (Rubens) ſpicis villoſis longis; corollis monopetalis, caule erecto, foliis ſerrulatis. Lin. Spec. plant. 1081.

Trifolium

Trifolium montanum ſpica longiſſima rubente. Bauh. pin. 328.

2547 Verbena (Nodiflora) tetrandra, ſpicis capitato-conicis; foliis ſerratis; caule repente. Lin. Spec. plant. 28.

Verbena nodiflora. Bauh. pin. 269.

2548 Vicia (Faba minor β) caule erecto, petiolis abſque cirrhis. Lin. Spec. plant. 1039.

Faba minor ſive equina. Bauh. pin. 338.

2549 Ziziphora (Capitata) capitulis terminalibus, foliis ovatis. Lin. Spec. plant. 31.

Thymus humilis latifolius. Buxb. cent. 3. p. 28. t. 51. f. 1.

2550 Ziziphora (Tenuior) floribus lateralibus; foliis lanceolatis. Lin. Spec. plant. 31.

Acinos Syriaca folio mucronato capſulis hirſutis. Moris. hiſt. 3. p. 404. ſ. 11. t. 19. f. 3, 4.

XXXII. *Obſervations on the Gillaroe Trout, commonly called in* Ireland *the Gizzard Trout, by* John Hunter, *F. R. S.*

Redde, March 17, 1774.

ONE of the digeſtive organs of this trout being ſo very remarkable as to have given name to the fiſh, and to be looked upon as its diſtinguiſhing characteriſtic; it will be neceſſary, to take a general view of the varieties in the digeſtive organs of animals, to determine what place the ſtomach of this particular trout holds among them, and alſo to throw ſome light upon the queſtion, whether its reſemblance to a gizzard be ſuch, as to render the name of *gizzard trout* a proper appellation.

To begin with ſome general facts: the food of animals may be divided into two kinds, ſuch as does, and ſuch as does not, require maſtication, to facilitate the digeſtion. All animal food is of this latter kind. But grain, and many other ſubſtances which ſerve for aliment, require a previous grinding or trituration; and therefore thoſe animals which live on ſuch food, are furniſhed with organs for that purpoſe. Granivorous quadrupeds have the two powers, viz. maſtication and digeſtion, ſeparate or diſtinct from one another; the firſt being exerted by a ſet of teeth of a particular form,

form, which ſerve as ſo many grindſtones for reducing their food to a powder, before it be conveyed into the ſtomach for digeſtion; and when ſo prepared, it is, with regard to the digeſtive power, become ſimilar to animal food: therefore in many ſuch animals the ſtomach is ſimilar to that of the carnivorous; and whenever the ſtomach in granivorous quadrupeds differs from this general rule, there is a ſingularity in the operations of digeſtion. Such birds as live upon this kind of food, for the digeſtion of which trituration is indiſpenſably neceſſary, have the powers of maſtication and digeſtion united in one part; which is of a peculiar ſtructure for that purpoſe; this is the *gizzard*. In granivorous birds therefore one ſingle organ anſwers both to the teeth and ſtomach of granivorous quadrupeds, and conſequently the gizzard alone of birds will point out the food of the ſpecies as clearly, as the teeth and ſtomach together do in other animals, in which the two offices of maſtication and digeſtion are not joined together in the ſame part.

As it appears then to be the difference of the ſtomachs only, that fits birds for their different kinds of food, it is evident that every gradation of ſtomach muſt be found among them, from the true *gizzard* which is one extreme, to the more *membranous ſtomach* which is the other; ſince the food of different ſpecies is of every different kind, from the hardeſt grain, to the ſofteſt animal matter. In conſequence of this, it muſt be as difficult to determine the exact limits of the two different conſtructions, to which the names of *gizzard* and *ſtomach*

ſpeci-

ſpecifically belong, as, in any other caſe, to diſtinguiſh proximate ſteps in the flow and imperceptible gradations of nature.

The two extremes of true gizzard, and membranous ſtomach, are eaſily defined; but they run ſo into each other, that the end of one and the beginning of the other are quite imperceptible. Similar gradations are obſerved in the food; the kinds ſuited to the two extremes mixing together in different proportions, adapted to the intermediate ſtates of ſtomach.

A true *gizzard* is compoſed of two ſtrong muſcles placed oppoſite, and acting upon each other, as two broad grindſtones. Theſe muſcles are joined together at their ſides by a middle tendon, into which the muſcular fibres are inſerted, and which forms the narrow anterior and poſterior ſides of the flat quadrangular cavity, in which the grinding is performed. The upper end of this cavity is filled up by the termination of the *œſophagus*, and the beginning of the inteſtine. The lower end conſiſts of a thin muſcular bag connecting the edges of the two muſcles together.

By a ſoft flexible ſubſtance being thus interpoſed between the two ſtrong grinding muſcles, a double advantage is gained; for whilſt it gives an eaſy paſſage to the *œſophagus* and gut above, it allows of that free motion of the grinding ſurfaces on each other, which is neceſſary for the comminution of the food. The half-muſcular half-membranous bag, which is fitted on to the lower end of the cavity, at the ſame time that it ſerves for a reſervoir, is perhaps the only part which has the power

power of digeſtion, and therefore that is to be called the true *ſtomach*. In this caſe then we have ſome diſtinction between the two grand offices of maſtication and digeſtion.

The two flat lateral ſides of the grinding cavity are lined with a thick horney ſubſtance ſimilar to a hard and thick cuticle: the narrow anterior and poſterior tendinous parts are alſo lined with a cuticle, but not ſo ſtrong as the former: this horney ſubſtance is gradually loſt at one end in a very thin cuticle, which lines the paſſages of the *oeſophagus* and inteſtine for a little way; and at the other end is alſo ſimilarly loſt in the membranous bag, or true ſtomach.

The two large muſcles may be conſidered as a pair of jaws, whoſe teeth are taken in occaſionally, being ſmall rough ſtones or pebbles which the animal ſwallows: and from the feeling on the tongue, it can diſtinguiſh ſuch of theſe as are proper, from thoſe which are ſmooth or otherways unfit for the purpoſe, which laſt it inſtantly drops out of its mouth.

Some birds, with gizzards, have a *craw* or *crop* alſo, which ſerves as a reſervoir, and for ſoftening the grain; but as all of them have not this organ, it is not to our preſent purpoſe.

There are other animals beſides that claſs of birds, which maſticate their food in their ſtomach, but their teeth are placed there by nature: CRABS and LOBSTERS are of this kind.

The gradation from *gizzard* to *ſtomach* is made by the muſcular ſides becoming weaker and weaker, and the true ſtomachic or gaſtric part proportiona-

 bly

bly larger and larger, in the ſucceſſive order of birds, till at length it conſtitutes the whole of the organ; and the food keeps pace with this change, varying gradually from vegetable to animal. In one point of view, therefore, food may be conſidered as a firſt principle, with reſpect to which the digeſtive powers, with their appendages, are as ſecondary parts, being adapted to and determined by the food, as the primary object.

We find then that in granivorous animals of all ſorts, there is an apparatus for the maſtication of the food, although of different kinds and differently placed. But in true carnivorous animals of whatever tribe, maſtication is not neceſſary, and therefore they have no apparatus for that purpoſe. The teeth of ſuch quadrupeds, as are carnivorous, ſerve chiefly to procure food and prepare it for deglutition. The ſame thing holds in the true carnivorous bird, the office of whoſe beak and talons is to procure the aliment, and fit it for deglutition, correſponding in this reſpect to the teeth of the others. Applying this to fiſh, it ſeems, at firſt ſight, that there is no occaſion in them for that variety of ſtructure in the digeſtive organs, which is found in the beforementioned quadrupeds and birds; the food of fiſh being principally of one ſort, namely, animal, which however with regard to the digeſtive powers, is to be diſtinguiſhed into two kinds, viz. common ſoft fiſh and ſhell-fiſh. Such fiſh as live on the firſt kind, have like the carnivorous quadrupeds and birds, no apparatus for maſtication; their teeth being intended merely for catching the food and fitting it to be

ſwallowed.

ſwallowed. But the ſhells of the ſecond kind of food render ſome degree of maſticating power neceſſary, and accordingly we find in certain fiſh a ſtructure ſuited to this purpoſe.

Thus the mouth of the WOLF-FISH is almoſt paved with teeth, by means of which he can break any ſhells to pieces, and ſo effectually diſengage the food for digeſtion, that though he lives upon ſuch hard food, his ſtomach does not differ from that of other fiſh: the organs of maſtication and digeſtion therefore in this animal exactly correſpond to thoſe of many granivorous quadrupeds.

Other fiſh, on the contrary, approach nearer to the ſtructure of birds, in having their ſtomach furniſhed with ſome degree of maſticating power; but it is very imperfect, compared with that of the gizzard of fowls, though perhaps the difference is ſuch only as the difference of food will properly allow: for in fiſh who have this power, the food being ſtill animal, and but imperfectly covered with the ſhell, it perhaps wants only to be broken; whereas that of granivorous birds requires to be ground into a kind of meal.

Of all the fiſh I have ſeen, the MULLET is the cleareſt inſtance of this ſtructure; its ſtrong muſcular ſtomach being evidently adapted, like the gizzard of birds, to the two offices of maſtication and digeſtion. The ſtomach of the fiſh now before us holds the ſecond place.

But ſtill neither of thoſe ſtomachs can be juſtly ranked as *gizzards*, ſince they want ſome of the moſt eſſential characters, *viz.* a power and motion fitted for grinding, and the horney cuticle. The ſtomach of the GILLAROO TROUT is however more circumscribed than that of moſt fiſh, better adapted for ſmall food, and endued with ſufficient ſtrength to break the ſhells of ſmall ſhell fiſh; which will moſt probably be beſt done by having more than one in the ſtomach at a time, and alſo by taking pretty large and ſmooth ſtones into the ſtomach, which will anſwer the purpoſe of breaking; but not ſo well that of grinding; nor will they hurt the ſtomach as they are ſmooth, when ſwallowed: but this ſtomach can ſcarcely poſſeſs any power of grinding, as the whole cavity is lined with a fine villous coat, the internal ſurface of which appears every where to be digeſtive, and by no means fitted for maſtication.

The ſtomach of the ENGLISH TROUT is exactly of the ſame ſpecies with the GILLAROO, but its coat is not ſo thick by $\frac{2}{3}$ (*a*). How far this difference in thickneſs of ſtomach is ſufficient to make a diſtinct ſpecies, or barely a variety of the ſame, is only to be determined by experiment (*b*).

The *oeſophagus* in the trout is conſiderably longer and ſmaller than in many other claſſes of fiſh.

The inteſtines are ſimilar to thoſe of the ſalmon, herring, ſprat, &c.

(*a*) The Engliſh trout ſwallows ſhell-fiſh, and alſo pretty large ſmooth ſtones, which ſerve as a kind of ſhell-breaker.

(*b*) *Viz.* Take ſome Gillaroo trout, male and female, and tranſplant them into ſome other water where there are no trout, to ſee if they continue the ſame.

The

The *pancreas* is appendiculated (*c*).

The teeth ſhew them to be fiſh of prey.

So far as we are led to determine by analogy, we muſt not conſider the ſtomach of this fiſh as a *gizzard*, but as a true *ſtomach*.

(*c*) I chuſe to give this name to the part from its appearance.

XXXIII. *Explication of a most remarkable Monogram on the Reverse of a very antient Quinarius, never before published or explained. In a Letter to* M. Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Custos Archivorum of the University of* Oxford, *Member of the Academy* degli Apatisti *at* Florence, *and of the* Etruscan *Academy of* Cortona *in* Tuscany.

DEAR SIR,

I.

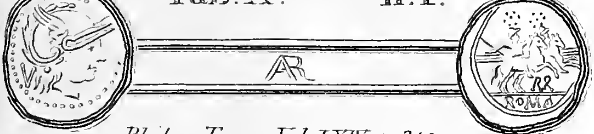
Redde, Nov. 11, 1773.

HAVING given a draught and short account of a very antient QUINARIUS, with a most remarkable and uncommon monogram on the reverse, which the Royal Society did me the honour to publish in a former volume of the *Philosophical Transactions* (1), I shall now beg leave to resume the subject; and, in order the more fully to evince what was there advanced, submit a farther

(1) *Philosoph. Transact.* Vol. LXII. Tab. II. n. 3. p. 61, and 64, note *.

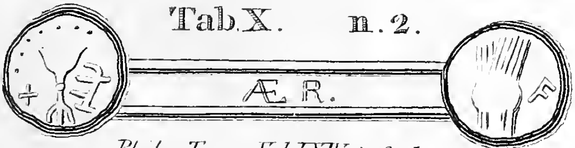
and

Tab. X. n. 1.



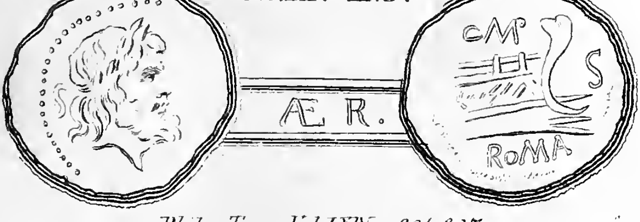
Philos. Trans. Vol. LXIV. p. 318.

Tab. X. n. 2.



Philos. Trans. Vol. LXIV. p. 325.

Tab. X. n. 3.



Philos. Trans. Vol. LXIV. p. 326, 327.

Basire sc.

and more particular account of this curious piece to the consideration of that most respectable body. And this I am the more inclined to do, as an explication of the monogram here mentioned may not improbably enable us to explain a legend on the reverses of other antient (2) Roman coins, which has hitherto been deemed by some learned men almost, if not altogether, inexplicable.

II.

The piece before me is a very antient, or rather an original, QUINARIUS, extremely well preserved. It has on one side a female head in an helmet, with the letter V behind, standing for FIVE, the number of asses it contains; and on the reverse *Castor* and *Pollux*, or, according to Sig. OLIVIERI, two *Castors*, on horseback, with seven stars over each of their helmets, or caps. In the exergue we discover the word ROMA, formed of very antient characters; and under the belly of one of the horses the monogram, which distinguishes this quinarius from all the other similar pieces that ever fell under my view or observation. Nor have I ever met with it in any author I had occasion to consult, or peruse. To me therefore it cannot but appear in the light of an inedited coin.

III.

The Romans first coined silver money, according to (3) Pliny, with whom (4) Livy, in this point,

(2) Joan. Baptist. Biancon. *de antiq. litt. Hebræor. et Græcor. Libel.* p. 74, 75. Bononiæ, 1748.
(3) Plin. *Nat. Hist.* Lib. xxxiii. cap. 3.
(4) Liv. *Epit.* Lib. xv.

agrees,

agrees, in the 485th year of the city. Some of the earlieſt pieces, of which ſeveral ſtill remain in the cabinets of the curious and the great, exhibited a female galeated head on one ſide, as does the quinarius I am conſidering; and on the reverſe (5) *Caſtor* and *Pollux*, or, as Sig. OLIVIERI calls them, * two *Caſtors*, as both theſe figures are horſemen, ſuch as clearly and diſtinctly appear upon my coin. Wherefore, as the letters forming the word ROMA, in the exergue, are antique enough, at leaſt, for the time when ſilver was firſt coined at Rome, or five years before the (6) commencement of the firſt Punic war, we may fairly ſuppoſe my QUINARIUS to be either coeval with, or, as I rather imagine, a little anterior to the commencement of that war.

IV.

The monogram on the reverſe of this quinarius, ſo extremely remarkable for the number of letters it contains, we ſhall find, upon a cloſe and attentive examination, to exhibit the word ROMANORO, the maſculine genitive caſe plural of ROMANVS, in the days of C. DUILIUS and L. SCIPIO, the ſon of BARBATUS, towards the cloſe of the fifth century of Rome; ſome time after the completion of which, the Romans converted the (7) laſt ſyllable RO into

(5) P. Joubert, *Science des Medaill.* c. 5. Annib. degli Abati Oliver. in *Sag. di Diſſertaz. Academic.* &c. *di Corton.* Tom. IV. p. 134. In Roma, 1743.

* Caſtor and Pollux, or the Dioſcuri, Διόσκουροι, are ſometimes both denominated Caſtor, as we learn from Pliny and Arnobius, not to mention other antient authors of good repute. Plin. *Nat. Hiſt.* Lib. X. c. 43. Arnob. *Contra Gent.* Lib. IV.

(6) Plin. ubi ſup.

(7) Joan. Bapt. Biancon. ubi ſup.

RVM. But to analyſe this extraordinary complex character a little more particularly, the firſt part of it perfectly anſwers to the word ROMA, as repreſented by * a monogram on ſeveral coins of the (8) CALPURNIAN family; and the latter part of it is evidently formed of the letters NORO, the laſt of which is apparently included in the head or top of the R. As the maſculine plural termination of the genitive caſe was RO, inſtead of RVM, in the year of Rome (9) 494, when the inſcription mentioning L. SCIPIO's conqueſt of CORSICA, and reduction of ALERIA, ſeems to have firſt appeared; it is highly probable, that the piece in queſtion was either coeval with, or a little anterior to, that year. The inſcription is as follows:

HONC. OINO. PLOIRVME. CONSENTIONT. R.
DVONORO. OPTVMO. FVISE. VIRO.
LVCIOM. SCIPIONE. FILIOS. BARBATI.
CONSOL. CENSOR. AIDILIS. HIC. FVET.
HIC. CEPIT. CORSICA. ALERIAQVE. VRBE.
DEDET. TEMPESTATEBVS. AIDE. MERETO.

* M. HAVERCAMP calls this curious character the monogram of Rome, meaning the word ROMA, which it very plainly exhibits; and we may, with as much reaſon, denominate the extraordinary ligature on the reverſe of my quinarius the monogram of the Romans, or the complex character that contains all the letters forming the word ROMANORO in it. Sig. Havercamp. ubi ſupra, p. 68.

(8) Morel. Num. Fam. Calpurn. Tab. IV. num. 16. et Tab. V. Sig. Havercamp. *Comment.* p. 68. Amſtelodami, 1734.

(9) Joan. Nic. Func. *De Puerit. Ling. Lat.* cap. v. p. 133, 134. Marburgi Cattorum, 1720.

Hunc unum plurimi consentiunt Romæ,
Bonorum optimum fuisse virum,
Lucium Scipionem. Filius Barbati,
Consul, Censor, Ædilis, hic fuit.
Hic cepit Corsicam, Aleriamque Urbem.
Dedit Tempestatibus ædem merito.

For a farther account of which very curious inscription, recourse may be had to the authors here referred to, as well as others who have expatiated very largely upon it (10).

V.

From what has been here laid down it seems highly probable, that my quinarius first appeared about the year of Rome 494, or rather that its first appearance was a little anterior to that year. Which if we admit, it will follow, that the Romans borrowed the monogrammatic way of writing rather from the ETRUSCANS than the Greeks, as I asserted in (11) one of my former papers; with the first of which nations they were perfectly well acquainted,

(10) Though the events mentioned by this inscription are a year later than those glorious exploits recorded by the Duilian, yet the rudeness of the language announces the former to be much superior to the latter in point of antiquity. This, I doubt not, will be allowed by every sober critic, who examines the language of both with proper attention. Jacob. Sirmund. Hieronym. Aleand. apud Joh. Nic. Func. ubi sup. ut et ipse Joh. Nic. Func. ibid. Vid. etiam Joh. Georg. Græv. Jacob. Facciolat. Patavin. Joh. Georg. Walch. aliosque philolog. pass.

(11) *Philosoph. Transact.* Vol. LXII. Tab. II n. 3. p. 61. 64. not. *.

even from the very beginning of their ſtate; whereas they ſeem to have had little or no intercourſe with the other, when the piece in queſtion was coined. It remains, therefore, that what I advanced, (12) in the paper here referred to, is clearly and indubitably true.

VI.

From the explication of the monogram, on the reverſe of the coin in queſtion, we may perhaps diſcover the interpretatiou of the word ROMANO, on certain antient coins, which has hitherto not a little embaraſſed the learned. For as it appears from my quinarius, that the Romans impreſſed the word ROMANORO, for ROMANORVM, as well as ROMA, on ſome of their moſt antient (13) coins; we may either, with Sig. BIANCONI (14), take NO in ROMANO for an earlier termination of the maſculine genitive caſe plural, or make it to ſtand for ROMANORO, or ROMANORVM, and conſequently look upon it as a mutilated gentile name. The latter of theſe opinions,

(12) *Philoſoph. Tranſact.* ubi ſup.

(13) The antient coins of cities not ſeldom exhibit the gentile names, or thoſe of the citizens, inſtead of the names of the places themſelves; ſuch names being frequently found in the genitive caſe plural, as here, on the reverſes of thoſe coins. Of this ΣΜΥΡΝΑΙΩΝ, ΘΕΣΣΑΛΟΝΙΚΕΩΝ, SMYRNAEORVM, THESSALONICENSIVM, PAISTANO, for PAISTANORVM, SVESANO, for SVESANORVM, as here, are ſufficient proofs, to omit many other ſimilar inſtances that occur, and might, with equal facility, be produced. Vid. Joan. Vaillant. *Num. Imperat. Auguſtar. Cæſar. &c.* Amſtelodami, 1700. Joan. Bapt. Biancon. ubi ſup. alioſque quam plurim. ſcriptor. paſſ.

(14) Joan. Bapt. Biancon. ubi ſup.

however ſtrongly oppoſed by the learned author above-mentioned, ſeems to me to be the moſt eligible of the two.

VII.

It has already been remarked, in a former paper, that the old ETRUSCANS uſed monograms in their writing, on certain (15) occaſions. To what was there obſerved I would beg leave here to add, in corroboration of that remark, that this has been likewiſe aſſerted by the famous Sig. GIOVANNI BATTISTA PASSERI (16), one of the greateſt Etruſcan antiquaries of the preſent age; and it is certain, that ſuch ligatures, or complex characters, were actually made uſe of by the earlier Etruſcans, in ſome of their inſcriptions, that have even to this day eſcaped the ravages of time. This has been irrefragably proved by the authors of the *Univerſal Hiſtory* (17), who have favoured the republic of letters with three or four of thoſe complex characters, deduced from Etruſcan monuments of undoubted integrity and authenticity, dug up in Tuſcany, or the antient Proper Etruria. To theſe we have coreſpondent types here in OXFORD, which may perhaps be of ſome ſervice hereafter to the learned world.

(15) *Philoſoph. Tranſact.* ubi ſup.
(16) Joan. Bapt. Paſſer. apud Anton. Franciſc. Gor. in *Muſ. Etruſc.* Vol. III. p. 142. Florentiæ, 1743.
(17) *Univ. Hiſt.* Vol. XVI. p. 45. Lond. 1748.

VIII.

VIII.

With regard to monograms in general, it may not be improper to remark, that they were known and uſed in ſeveral parts of the eaſt, from pretty remote antiquity. They occur on ſome of the Hebrew (18), or Samaritan, and Phœnician coins, as well as on the Greek and Roman. I have an exceeding curious HEBREW, or SAMARITAN, coin, coeval with Simon the Juſt, prince and high prieſt of the Jews, with a monogram upon it; of which, as it has never yet been communicated to the learned world, and is anterior to all thoſe ſtruck by that prince hitherto publiſhed, I may perhaps ſend you a more diſtinct and particular account, in ſome future paper. That the PHOENICIANS were not unacquainted with monograms, has been admitted by the learned and ingenious M. PELLERIN (19), and is evinced by one or two of the Phœnician inſcriptions on the ſtones found in the (20) ruins of CITIUM. That the ARABS likewiſe antiently uſed them, on certain occaſions, we learn from (21) the ligatures of the KUFIC letters, and the inſcriptions ſtill remaining on ſeveral of the earlier ARABIC coins. Nay, they are not diſuſed amongſt the modern Arabs, in their common writing, even at

(18) Peller. Let. I. *Sur diverſes Medailles*, p. 51—54. A Francfort, 1770.

(19) Idem ibid.

(20) Poc. *Deſcript. of the Eaſt*, Vol. II. Par. I. Tab. XXXIII. n. 12, 18, 31. p. 213. Lond. 1745.

(21) Vid. *Monarch. Aſiatico-Saracen. Stat. &c. illuſtrat.* à M. Georg. Jacob. Kehr, Sleuſinga-Franc. Oriental. Tab. I. et II. p. 1. et alib. Lipſiæ, 1724.

this

this very day. As for the GREEKS, nothing is more common than ligatures, or monograms, on their coins; ſome of which F. FROELICH has (22) illuſtrated, and explained. I ſay, "*ſome of which* F. Froelich has "illuſtrated, and explained," as many of them have been intirely unnoticed by him. That the PALMYRENES alſo had ſeveral ſuch ligatures, or complex characters, I have many years ſince inconteſtably (23) proved. But for farther ſatisfaction on this head, recourſe may be had to the paper here referred to.

IX.

With reſpect to the ROMANS, nothing is more certain than that combinations of two, three, and even four elements, formed into one character, not ſeldom occur on their coins. More extenſive or complex ligatures than the monograms of four letters on their antient medals very rarely appear. I have, however, an inedited *ſemiſſis* of the POMPEIAN family, with the head of SATURN, and behind it the letter S, the mark of the ſemiſſis, on one ſide; and the prow of a ſhip, over which a monogram compoſed of the five letters, Q, P, O, M, P, preſents itſelf to our view. Dr. VAILLANT (24) attributes two ſimilar ſemiſſes, with a monogram, or concatenation of the five letters L, P, O, M, P, upon the re-

(22) Eraſm. Frœl. *Annal. Compend. Reg. et Rer. Syr.* Tab. XX. p. 158. Viennæ, 1754.

(23) *Philoſoph. Tranſact.* Vol. XLVIII. Par. II. p. 693. Lond. 1755.

(24) Jo. Vaill. in num. *Fam. Pompei.* p. 275, 276. Amſtelodami, 1703.

verſe, to the POMPONIAN family, and aſſerts them to have been ſtruck in the 533d, or 534th year of Rome. M. HAVERCAMP (25) ſeems inclined to adopt the ſame ſentiments. But neither the ſize, weight, fabric, nor forms of the letters of mine will permit us to aſcribe it to the Pomponian family, and aſſign it ſo early a date; but determine ſtrongly in favour of the Pompeian, and, in conjunction with what has been advanced by HAVERCAMP (26), evince it to have been ſtruck by QUINTUS POMPEIUS, the grandſon of Q. POMPEIUS RUFUS and SULLA, a little after the (27) middle of the ſeventh century of Rome. I could expatiate more largely upon this very valuable piece, which is tolerably well preſerved, did not the intended brevity of this paper reſtrain me, and oblige me now to conclude; which I ſhall therefore beg leave to do, with aſſuring you that I remain,

SIR,

Your moſt humble,

and moſt obedient ſervant,

Chriſt-Church, Oxon.
Oct. 18, 1773.

JOHN SWINTON.

(25) Sig. Haverc. *Comment.* p. 340. Amſtelodami, 1734.
(26) Idem ibid. p. 342.
(27) Sig. Havercamp. ubi ſup. p. 342.

THE END OF VOLUME LXIV. PART I.

PHILOSOPHICAL

TRANSACTIONS.

VOL. LXIV. PART II.

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. LXIV. For the Year 1774.

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M.DCC.LXXIV.

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TO

VOL. LXIV. PART II.

was

PHILO-

PHILOSOPHICAL TRANSACTIONS.

XXXIV. *Aſtronomical Obſervations made at* Chiſlehurſt, *in* Kent, *in the courſe of the Year* 1773. *By the Rev.* Francis Wollaſton, *LL.B. F. R. S.*

Redde, Mar. 10, 1774.

MY having theſe two laſt winters communicated to this ſociety, what aſtronomical obſervations I had occaſionally made in the courſe of each year, ſeems to be a call upon me to continue the ſame now. And I am the rather inclined to do ſo; becauſe I could wiſh we were favoured with the correſpondent obſervations of all our worthy brethren, and therefore ought not to be backward to throw in my mite towards a general ſtock.

My inſtruments and ſituation are the ſame as before deſcribed; and the following tables are in the ſame form as the laſt year. My clock has been kept going on, without any alteration of any kind: it is only by long and uninterrupted trials, that any judgement can be formed concerning the cause of errors.

The three first months of the following tables are, perhaps, less accurate than they might have been. I was absent from home great part of that time; and could only take such observations as occurred when I was occasionally in the country: hence the thermometer and barometer might be either higher or lower, in any of those months, than I have here set them down. I could truly give none but what I observed.

In the course of the summer I received from Mr. Nairne a Smeaton's *hygrometer*, which I had ordered the year before. I did not get it adjusted to my mind till the beginning of August; but from that time, have added its highest and lowest state in each month, to those of the thermometer and barometer. Its situation is the same as that of the clock itself; being fastened against the same wall, and close by its side. I do not apprehend the rod of the pendulum to be affected by sudden or small changes in the degree of humidity of the air; though it seems to be so by a long continuance of damp or dry weather. The hygrometer may perhaps shew that. The general dryness of summer, and the thickening of the oil in winter, (as far as I have had opportunity for trial) I take to be the principal causes of the change of rate in such a clock as mine. It now throws out rather less than it did: perhaps owing to its being less clean, or to the drying of the oil.

1772.		Clock + too fast — too slow for mean sol. time.			Gain + or Loss —	Number of days.	Rate per day.	Throwing out South side.	Throwing out North side.
			′	″	″		″	° ′	° ′
Nov.	2	Clock +	15	13,0				1 45	1 48
	14	+	15	38,9	+ 25,9	12	+ 2,15		
Dec.	19	+	16	3,5	+ 24,6	35	+ 0,70	1 46	1 48
1773.									
Jan.	10	+	16	20,4	+ 16,9	22	+ 0,77	1 43	1 45
	30	+	16	18,5	— 1,9	20	— 0,095		
Feb.	3	+	16	19,3	+ 0,8	4	+ 0,20	1 38	1 42
Mar.	2	+	16	23,3	+ 4,0	27	+ 0,15	1 39	1 42
	13	+	16	28,6	+ 5,3	11	+ 0,48		
	29	+	16	45,5	+ 16,9	16	+ 1,06		
Apr.	10	+	17	21,0	+ 35,5	12	+ 2,96	1 37	1 40
	24	+	18	1,2	+ 40,2	14	+ 2,87		
May	1	+	18	22,4	+ 21,2	7	+ 3,03	1 42	1 45
	10	+	19	0,6	+ 38,2	9	+ 4,24		
	29	+	20	21,4	+ 80,8	19	+ 4,25		
June	8	+	21	0,7	+ 39,3	10	+ 3,93	1 44	1 46
	19	+	21	46,2	+ 45,5	11	+ 4,14		
July	1	+	22	39,5	+ 53,3	12	+ 4,44	1 46	1 49
	17	+	23	54,1	+ 74,6	16	+ 4,66		
Aug.	6	+	25	37,4	+ 103,3	20	+ 5,16	1 45	1 48
	17	+	26	36,8	+ 59,4	11	+ 5,40		
	27	+	27	38,5	+ 61,7	10	+ 6,17		
Sept.	6	+	28	36,6	+ 58,1	10	+ 5,81	1 44	1 47
	18	+	29	33,8	+ 57,2	12	+ 4,77		
Oct.	1	+	30	17,9	+ 44,1	13	+ 3,39	1 39	1 42
	16	+	30	55,2	+ 37,3	15	+ 2,55		
Nov.	2	+	31	22,9	+ 27,7	17	+ 1,63	1 35	1 38
	13	+	31	40,7	+ 17,8	11	+ 1,62		
Dec.	6	+	32	1,1	+ 20,4	23	+ 0,89	1 32	1 35
	27	+	32	19,7	+ 18,6	21	+ 0,88		

1772.		Thermometer without doors, exposed to the north.			Therm. near the clock.	Barom. on the ground floor.	Hygrom. near the clock.
		Hor. 8. Mat.	Hor. 2. P. M.	H. 11. P. M.	Hor. 9. Mat.		
Nov.	Higheft	46	56	56	52	30,02	
	Loweft	39,5	42	39	44	29,31	
Dec.	Higheft	49	51	48	50	30,20	
	Loweft	30	33	30,5	34	29,01	
1773.							
Jan.	Higheft	50	52	48	58	30,15	
	Loweft	30	34	21	34	28,56	
Feb.	Higheft	48	50	48	48	30,42	
	Loweft	23	31	22	29	28,44	
Mar.	Higheft	53	65	48	55	30,26	
	Loweft	33	39	31	40	29,58	
April	Higheft	53	63	48	53	30,27	
	Loweft	40,5	44	35	48	28,82	
May	Higheft	61	70	57	57	30,19	
	Loweft	42	41	33	45	29,19	
June	Higheft	70	74	61	64	29,99	
	Loweft	48	54	43	52	29,18	
July	Higheft	71	77	65	65	30,18	
	Loweft	52	57	48	55	29,58	
Aug.	Higheft	71	82	64	69	30,06	23
	Loweft	55	61	50	54	29,11	9,5
Sept.	Higheft	64	66	59	60	29,97	39
	Loweft	48	52	47	49	29,045	15
Oct.	Higheft	59	62	58	55	30,16	39
	Loweft	42	49	40	47	28,95	20
Nov.	Higheft	49	54	52	50	30,09	63
	Loweft	30	33	30	38	28,42	20
Dec.	Higheft	48	50	47	46	29,97	67
	Loweft	28	36	27	36	28,86	30

Occultations

Occultations of Stars by the Moon. Obſerved with a $3\frac{1}{2}$-feet achromatic teleſcope, magnifying 150 times.

1773.		App. time.	
		° ′ ″	
♄ Feb. 6.	☽ 2 ad. α ♋	6 37 20	Im. Perhaps 1″ ſooner. ☽ nearly full, and a protuberance juſt at that part of her ragged edge which ſeemed to miſlead me.
		7 38 48	Em.
♀ Feb. 26.	☽ ✱ (N° 53 of La Caille, I believe.)	9 4 59	Im. good. Dark limb. ✱ began to loſe ſome of its light 9″ before it diſappeared.
		9 56 50	Em. doubtful. ☽ low, and great undulation. Em. perhaps 20″ ſooner.
♂ Sept. 7.	☽ Aldebaran	20 48 39,5	Im. very good. Alt. about 35°. Em. rainy.
☽ Nov. 1.	☽ Aldebaran	9 13 44	Im. very good. Light limb. Alt. 25° 30′.
		10 13 3	Em. It might perhaps be 1″ ſooner, but not more. Alt. 35° 15′.

I have obſerved many other occultations of ſmall ſtars; but, as their emerſions were ſcarcely viſible, and probably there have been no correſponding obſervations, I ſuppoſe it can be of no uſe to record them.

ECLIPSES OF JUPITER'S SATELLITES: obſerved with the ſame teleſcope, magnifying 100 times.

In theſe eclipſes, I have endeavoured to make uſe of the method recommended by M. BAILLY, in a paper communicated to this ſociety the laſt year, and printed in our Tranſactions, Vol. LXIII. p. 185. The diameter of the aperture of my teleſcope is 3,6 inches; and the diaphragms I have made, have their apertures differing from each other in diameter, as near as may be, $\frac{1}{10}$th of an inch. When the air is ſteady for any continuance, and uninterrupted with clouds, even though it be not perfectly clear, I ſhould apprehend this method may be of conſiderable uſe in reducing the obſervations of different perſons to ſome ſtandard: but when there are flying clouds, or any changes in the atmoſphere, during the obſervation, it cannot be ſatisfactory; and at ſuch times may be ſcarce worth attempting; unleſs for the ſake of rendering ſuch practitioners as myſelf more expert, when the air is more favourable to their endeavours.

I have, in ſome of theſe eclipſes, as well as the preceeding occultations, ſet down the altitude of the object; as that may ſometimes be of uſe, in conſidering the ſtate of the obſervation. But it ſhould be remembered, that theſe altitudes are not taken with any great preciſion. In theſe eclipſes, as in the occultations, I have ſuppreſſed thoſe obſervations which appeared doubtful; they tend only to miſlead.

1772.

1772.		App. time.	
		h ′ ″	
♄ Nov. 14.	First sat.	5 59 44	Em. good.
1773.			inches.
☉ Aug. 8.	Third sat.	11 15 ±	Invisible 0,4 aperture.
		11 24 23	Im. 0,6
		11 31 10	Im. 3,6, or whole aperture. Observation good. Night clear. ♃ Alt. 21°.

This being the first trial of the diaphragms, and the evening favourable, I tried the effect of them upon the other satellites.

		inch.		inch.
Second sat. near ♃	invisible	0,8	visible	0,9
Fourth sat.	scintillation	0,4	visible	0,5

♂ Aug. 31.	First sat.	9 40 ±	Invisible 0,7 aperture.
		9 49 46	Im. 1,0; but *quere*, I have some suspicion I used 1,2 by mistake; for I found that afterwards in the cap which contains the diaph.
		9 51 44	Im. 3,6, or whole aperture. Alt. 20° 20′. Air calm, but not very clear. ☽ distant from ♃ 32°.
	Second sat.	12 35 ±	Invisible 0,7
		12 43 14	Im. 1,0
		12 44 51	Im. 3,6 Alt. 39°. Air as before.
	First sat.	12 47 8	Em. from behind ♃'s disc.
☽ Sept. 20	Third sat.	11 40 ±	Invisible 0,6; but there were flying clouds at that time, though clear afterward.
		11 46 19	Im. 0,8
		11 51 6	Scintillation.
		11 51 23	Im. 3,6 good. Alt. 39°.

1772

1772.				h ′ ″		
♄ Oct.	9.	First sat.	10 45 35	Em.	3,6	good. Alt. 38° 30′.
			10 47 34	Em.	1,5	
			10 48 49	Em.	1,0	
			10 55 ±	Invisible	0,7	a little haziness made the trial of the diaph. uncertain.
♂	19.	Third sat.	6 33 29	Em.	3,6	good. Alt. 19 +
			6 36 28	Em.	1,0	
			6 40 ±	Invisible	0,5	night clear and still.
☿	20,	Second sat.	9 58 19	Em.	3,6	good obs. Alt. 38°.
			9 59 25	Em.	1,0	
			10 0 45	Em.	0,6	
			10 5 ±	Invisible	0,5	

Other Observations, made with the same telescope, magnifying 150 times.

		App.time. h ′ ″	
♃ Sept.	2.	8 55 0	I looked at ♃ to observe the shadow of the 2d sat. then on his disc; and thought I saw the sat. itself very visible, though central, and on a bright zone, and much larger than I could expect it to appear; reaching from the middle dark belt, which it indented, across the bright one, and quite into the next dark one. *Vide* fig. I.
		11 0 0	I looked again to observe its exit; when I perceived the sat. in a very different part of the disc: and now perceived that what I had seen before, must be a spot on ♃ himself; which, though now gone, had been very visible with the power of 100, notwithstanding ☽ was distant from ♃ but 6°.
♄ Sept.	4.	9 50 0	Spot seen again, advanced about $\frac{1}{4}$ or more. I was now convinced that the spot was on the disc itself.
		12 0 0	Spot not visible. The southern (or upper) belt appears only on each side; but is discontinued in the middle.

 ☽ Sept.

	App. time.			
	h	′	″	
☽ Sept. 6.	8	45	0	No ſpot viſible; nor the belt contiguous to it, that I could perceive. The ſouthern belt now compleat. The 3d ſat. juſt emerged from behind ♃.
	11	15	0	Spot appears; and the northern belt ſeems to appear about the ſame time. This belt was continued all round the planet the year before. The lower or northern edge of the middle zone, this year much darker than the ſouthern.
♄ Sept. 11.	10	44	0	Spot $\frac{1}{3}$ advanced. The 4th ſat. in inferior conjunction; ſeemingly in contact, but not on the diſc. *Vide* fig. II.
☿ Oct. 13.	7	30	0	Spot central, as near as I could judge.
♀ Oct. 15.	9	0	0	Spot almoſt central; that is, its preceding edge central. The planet very clear. The middle belt appeared undulated, juſt as in fig. III.
♂ Nov. 9.	5	10	0	Spot ſtill viſible, about $\frac{2}{3}$ advanced. I have not been able to attend to it ſince that time.

Since the reading of a Paper, communicated laſt year to this ſociety by Dr. WILSON, Profeſſor at Glaſgow, on the ſpots of the ſun; who mentions ſome appearances when they approach the limb, which I thought I had now and then obſerved; I have frequently turned my glaſs that way, as occaſion offered, in order to ſee whether thoſe appearances were conſtant, or what might be diſcovered to confirm the hypotheſis laid down in the latter part of that paper.

Dr. WILSON, I hope, will excuſe me when I ſay, that the appearance he mentions when the ſpots approach the ſun's limb, as if they were in a cavity on his ſurface, is not conſtant. They generally have appeared ſo to me, I confeſs. But as they ſometimes have not, and as I have very frequently ſeen them

them almoſt in contact with the limb; that is, not $\frac{1}{2}$ of a ſecond of time diſtant in paſſing a wire, for I have no micrometer; I think they can ſcarcely be in ſuch a hollow, below his ſurface, as the doctor deſcribes. To me, indeed, by the brighter light often adjoining to them when near the limb, they have rather put on the appearance as if they were in the crater of a volcano on the top of an eminence, which then turned its ſide towards us; and if ſo, the ſpot would appear ſomewhat nearer to the limb than it actually was. I have, indeed, never ſeen any protuberance on either limb of the ſun, as I have on the moon; but I have many times obſerved, near the eaſtern limb, a bright *facula* juſt come on, which has the next day ſhewn itſelf as a ſpot; though I do not recollect to have ſeen ſuch a *facula* near the weſtern one, after a ſpot's diſappearance. Yet, I believe, both theſe circumſtances have been obſerved by others; and perhaps not only near the limbs.

As to the *nebulæ*; they are certainly not always, though they are uſually, quite round each ſpot, or each cluſter of ſpots; neither are they always externally convex. The ſpot, fig. IV. which I ſaw Nov. 13, nearly in the centre of the ſun, is a remarkable inſtance of the contrary. Nothing therefore can be concluded from that circumſtance. Beſides, ſpots are ſometimes quite without any *nebulæ* at all; or none that I could perceive with any power of my glaſs.

What the ſpots, or their *nebulæ*, are, I pretend not to gueſs. To me they appear as if they were adjoining to the ſurface; though that is doubted by better

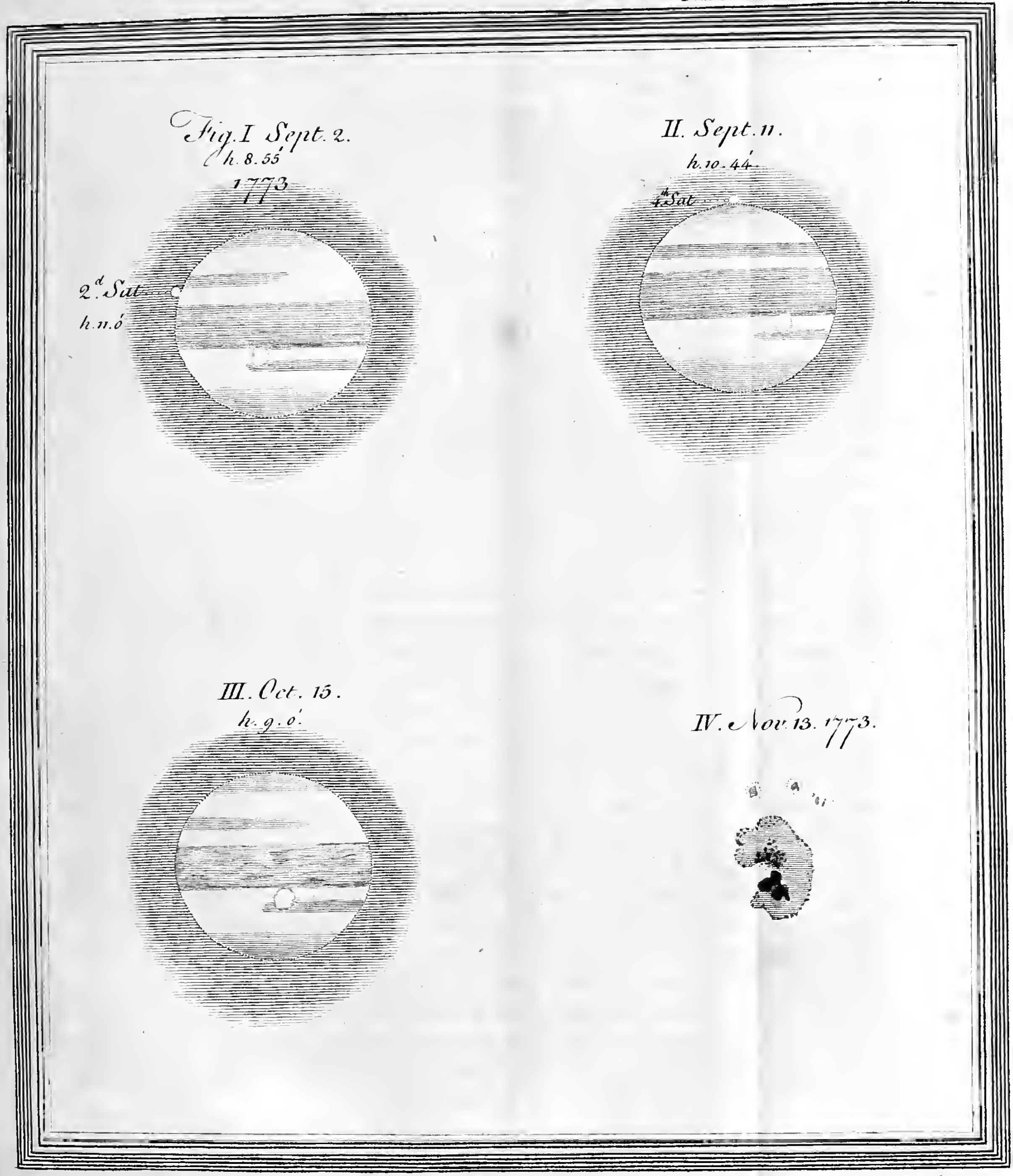
Fig. I Sept. 2.
h. 8.55′
1773
2d. Sat.
h. 11. 0′
II. Sept. 11.
h. 10. 44′.
4th Sat.
III. Oct. 15.
h. 9. 0′.
IV. Nov. 13. 1773.

Basire sc.

better aſtronomers, who have calculated their motions. The circumſtance of the *faculæ* being ſometimes converted into ſpots, I think I may be ſure of. That there is generally (perhaps always) a mottled appearance over the face of the ſun, when carefully attended to, I think I may be as certain. It is moſt viſible towards the limbs; but I have undoubtedly ſeen it in the centre: yet I do not recollect to have obſerved this appearance, or indeed any ſpots, towards his poles. Once I ſaw, with a 12-inch reflector, a ſpot burſt to pieces while I was looking at it. I could not expect ſuch an event; and therefore cannot be certain of the exact particulars: but the appearance, as it ſtruck me at the time, was like that of a piece of ice when daſhed on a frozen pond, which breaks to pieces and ſlides on the ſurface in various directions. I was then a very young aſtronomer; but think I may be ſure of the fact. Perhaps I may be thought a young aſtronomer ſtill, for throwing out theſe rough obſervations and crude thoughts: but whatever they be, if my errors ſhall lead others into enquiries which may be productive of certainty, their end will be anſwered.

Chiſlehurſt,
Jan. 3, 1774.

FRANCIS WOLLASTON.

 XXXV. *An*

XXXIV. *An Account of a Woman accidentally burnt to death at* Coventry. *By* B. Wilmer, *Surgeon, at* Coventry. *In a Letter to Mr.* William Sharpe.

SIR,

Redde, Mar. 20, 1774.

THE following cafe, which has lately engaged the attention of every one in this part of the world, appears to me fo very extraordinary, that I was determined to give you a minute account of its circumftances; which will be the more agreeable to you, as you may depend upon the truth of every thing that I fhall relate to you, concerning it.

MARY CLUES, of Gosford ftreet, in this city, aged 52 years, was of an indifferent character, and much addicted to drinking. Since the death of her hufband, which happened about a year and a half ago, her propenfity to this vice increafed to fuch a degree, that, as I have been informed by feveral of her neighbours, fhe has drank the quantity of four half pints of rum, undiluted with any other liquor, in a day. This practice was fo familiar to her, that fcarce a day has paffed this laft twelve-month, but fhe has fwallowed from half a pint to a quart of rum or anifeed-water. Her health gradually declined; and,

and, from being a jolly, well-looking woman, ſhe grew thinner, her complexion altered, and her ſkin became dry. About the beginnning of February laſt, ſhe was attacked with the jaundice, and took to her bed. Though ſhe was now ſo helpleſs, as hardly to be able to do any thing for herſelf, ſhe continued her old cuſtom of dram-drinking, and generally ſmoaked a pipe every night. No one lived with her in the houſe. Her neighbours uſed, in the day, frequently to come in, to ſee after her; and in the night, commonly, though not always, a perſon ſat up with her; to whom ſhe has often cried out, that ſhe ſaw the devil in ſome part of the room, who was come to take her away.

Her bed-room was next the ſtreet, on the ground-floor, the walls of which were plaiſtered, and the floor made of bricks. The chimney is ſmall, and there was a grate in it, which, from its ſize, could contain but a very ſmall quantity of fire. Her bedſtead ſtood parallel to, and at the diſtance of about three feet from, the chimney. The bed's head was cloſe to the wall. On the other ſide the bed, oppoſite the chimney, was a window opening to the ſtreet. One curtain only belonged to the bed, which was hung on the ſide next the window, to prevent the light being troubleſome. She was accuſtomed to lye upon her ſide, cloſe to the edge of the bedſtead, next the fire; and on Sunday morning, March the 1ſt, tumbled upon the floor, where her helpleſs ſtate obliged her to lye ſome time, till Mary Hollyer, her next neighbour, came accidentally to ſee her. With ſome difficulty ſhe got her into bed. The ſame night, though ſhe was adviſed to it, ſhe refuſed

refused to have any one to sit up with her; and, at half past eleven, one Brooks, who was an occasional attendant, left her as well as usual, locked up her door, and went home. He had placed two bits of coal quite backward upon the fire in the grate, and put a small rush-light in a candlestick, which was set in a chair, near the head of the bed; but not on the side where the curtain was. At half after five the next morning, a smoak was observed to come out of the window in the street; and, upon breaking open the door, some flames were perceived in the room, which, with five or six buckets of water, were easily extinguished. Betwixt the bed and fireplace lay the remains of Mrs. CLUES. The legs and one thigh were untouched. Except these parts, there were not the least remains of any skin, muscles, or *viscera*. The bones of the skull, *thorax*, spine, and the upper extremities, were compleatly calcined, and covered with a whitish efflorescence. The skull lay near the head of the bed, the legs toward the bottom, and the spine in a curved direction, so that she appeared to have been burnt on her right side, with her back next the grate. The right *femur* was separated from the *acetabulum* of the *ischium*; the left was also separated, and broken off about three inches below the great *trochanter*. The connection of the *sacrum* with the *ossa innominata*, and the inferior *vertebræ* of the loins were destroyed. The intervening ligaments kept the *vertebræ* of the loins, back, and neck together, and the skull was still resting upon the *atlas*. When the flames were extinguished, it appeared that very little damage had been done to the furniture of the room; and that the side of the bed next the fire

had

had ſuffered moſt. The bedſtead was ſuperficially burnt; but the feather-bed, ſheets, blankets, &c. were not deſtroyed. The curtain on the other ſide the bed was untouched, and a deal door, near the bed, not in the leaſt injured. I was in the room about two hours after the miſchief was diſcovered. I obſerved, that the walls and every thing in the room were coloured black: there was a very diſagreeable vapor; but I did not obſerve, that any thing was much burnt, except Mrs. CLUES; whoſe remains I ſaw in the ſtate I have juſt deſcribed. I took away one of the bones (the remains of the *ſacrum*) which you have incloſed with this letter. The only way that I can account for it is, by ſuppoſing that ſhe again tumbled out of bed on Monday morning, and that her ſhift was ſet fire to, either by the candle from the chair, or a coal falling from the grate. That her ſolids and fluids were rendered inflammable, by the immenſe quantity of ſpirituous liquors ſhe had drank; and that when ſhe was ſet fire to, ſhe was probably ſoon reduced to aſhes, for the room ſuffered very little.

Coventry,
April 9, 1772.

B. WILMER.

Received

XXXVI. *Experiments on Animal Fluids in the exhausted Receiver.* *By* D. Darwin, *M. D. of* Litchfield. *Communicated by Dr.* Franklin.

Redde, March 24, 1774.

THE antient opinion, that air exists in some of the blood-vessels, was exploded by the discovery of the circulation. But many of our modern theorists seem to have conceived, that an elastic vapour of some kind exists in the blood-vessels, as they have ascribed the lunar and equinoctial diseases to the variations of atmospheric pressure.

This opinion seems to have arisen from observing, that the skin rises, and that the vessels are distended, even to bursting, under a cupping-glass; when the pressure of the atmosphere is taken off from one part, and continues to act on all the remaining surface of the body: and would indeed, at first sight, appear to be demonstrated by the following experiments.

About four ounces of blood were taken from the arm of one of the attendants, and immediately put under the

the receiver of an air-pump; and, as the air was exhausting, the blood began to swell, and to rise in bubbles, till it occupied above ten times its original space.

As false reasoning is, in no science, of more dangerous consequence, than in that of medicine, I persuaded myself the removal of this error might be thought worthy the attention of the Royal Society.

In April 1772, Mr. YOUNG, an ingenious surgeon at Shiffnal in Shropshire, and Mr. WALTIRE, who gives very accurate lectures in natural philosophy, made, at my request, the following experiments.

1. A part of the jugular vein of a sheep, with the blood in it, was included between two strict ligatures, during the animal's being alive, and being cut out with the ligatures, was immediately put into a glass of warm water, and placed in the receiver of an air-pump: it sunk to the bottom of the water, and would not rise when the air was diligently exhausted. It was then wiped dry, and laid on the brass floor of the receiver, and the air again exhausted, but there was not the least visible expansion of the vein, or its contents.

2. A ligature was put round the neck of the gall-bladder of the same animal, as soon as it was slaughtered; the gall-bladder, with the bile in it, was first put into water, in which it sunk, and was placed in the exhausted receiver of the air-pump; and was afterwards wiped dry, and laid on the brass plate at its bottom, as in the former experiment; but in neither case, on the greatest degree of exhaustion, did it shew the least alteration of its bulk.

 3. The

3. The neck of the urinary bladder of the ſame animal was well ſecured with a ligature, and contained about two or three ounces of fluid. The bladder ſunk immediately on being put into warm water; but, upon exhauſting the receiver, many ſilver-like globules appeared upon the ſurface of it; and it ſoon ſhewed manifeſt ſigns of expanſion, and roſe to the top of the veſſel. The ſame experiment was tried with it wiped dry, and laid on the floor of the receiver, and the reſult was, that its expanſion and contraction were very perceptible to the eye.

In January 1773, by the aſſiſtance of Mr. WEBSTER, an ingenious ſurgeon from Montroſe, the above experiments were repeated in the manner following.

A part of the *vena cava inferior* of a large ſwine, which was killed by ſome ſtrokes on his head with an axe, was intercepted, when full of blood, between two ligatures. The part was about an inch and an half long, and held, by conjecture, near an ounce of blood; this was immerſed in warm water, as ſoon as it was cut out of the warm body, and immediately put into the receiver of an air-pump. The air was well exhauſted, and again let into the receiver repeatedly, without any appearance of enlargement of the vein; which muſt have been eaſily perceivable by its aſcending in the warm water.

The ſame experiment was tried on the urinary bladder, with the ſame ſucceſs, the *urethra* being tied with a ligature, whilſt it was ſtill in the body.

The gall bladder roſe in the warm water, though the bile-duct was tied before it was taken out of the body, and had air bubbles appearing on its ſides, like

like globules of quickſilver, as happened to the urinary bladder in the experiments at Shiffnal; which, in both caſes, we aſcribed to ſome portion of cellular membrane adhering to the bladders, into the cells of which, at the time of cutting them out, ſome air had inſinuated itſelf.

In theſe experiments the water, in which the animal parts were immerſed, was warmed to about an hundred degrees of Fahrenheit's ſcale, leſt a greater degree of heat in the water might have raiſed an elaſtic vapour from theſe fluids, which did not naturally exiſt in the living animal; and all the parts were well cleared from the cellular membrane and fat; as it was imagined the atmoſpheric air might intrude itſelf into the cellular membrane, as is ſeen in tearing off the ſkins of animals recently killed, and which did indeed diſappoint two of the above experiments, as was manifeſt from the ſilvery globules, which appeared upon the ſurfaces of the bladders.

From the facts eſtabliſhed by theſe experiments, we may draw the following concluſions.

1. That ſo great a change is produced in the blood, by its receiving, in its paſſage from the arm of the patient to the baſon, a great admixture of atmoſpheric air, that the experiments afterwards made on its ſenſible or chemical properties are rendered very uncertain and erroneous; ſince the fluid colour of the blood, its property of coagulation, and perhaps of putrefaction, may depend on this aſcititious admixture of atmoſpheric air: and, at the ſame time, we ſee why ſo much leſs froth is produced in the operation of cupping, than from blood placed in the exhauſted receiver of an air-pump; though

perhaps as great a degree of *vacuum* is made in one cafe as in the other.

2. It is probable, from thefe facts, that animal bodies can bear much greater variations of the preffure of the atmofphere, than the natural ones, without any degree of inconvenience. Some who have afcended high mountains are faid to have been feized with a fpitting of blood: but as this never happens to animals, that are put into the exhaufted receiver of an air-pump, where the diminution of preffure is many times greater than on the fummit of the higheft mountains, it is probable it was an accidental difeafe, or was owing to fome violent exertions in afcending. And in the curious account Dr. HALLEY gives of his defcending in a diving-bell fo low, as to have the weight of many atmofpheres over him, no other complaint is recorded, but a difagreeable fenfation, as he was defcending, like fomething burfting in his ears, and which recurred at about the fame depth of water in his afcent.

From the above obfervations of Dr. HALLEY on the fenfation in his ears, when he defcended and afcended in the diving-bell, I was led to imagine, that the air contained behind the *tympanum* in the *veftibulum*, *cochlea*, and femicircular canals of the ear, had found or made itfelf a way into the *Euftachian* tubes, or into the external ear, by fome undifcovered paffage; and concluded, that a fimilar operation might be of fervice to fome deaf people, where the immediate caufe of their deafnefs might be owing to the excefs or defect of this internal air. For this purpofe, a cupping-glafs, which had a fyringe to exhauft it, was put over the ears of three different

different people, who were very hard of hearing. The inequality of the mammoid procefs of the temporal bone, made it neceffary to put two or three circles of wafh-leather dipt in oil around the *helix* of the ear. On working the air-fyringe, the external ear fwelled, and became red; and at length the patients complained of pain in the internal ear, and the air was re-admitted. One of thefe three patients heard confiderably better immediately after the operation, and received permanent advantage; the others received neither benefit nor differvice.

If this fmall degree of fuccefs from the ufe of the cupping-glafs, as fo little pain or trouble attends the operation, fhould encourage other deaf perfons to make ufe of it, it may be a means to give fome light into the intricate difeafes of this organ, the ftructure of the parts of which, and their ufes are yet fo little underftood.

XXXVII. *An*

XXXVII. *An Account of a Storm of Lightning observed on the 1ſt of March,* 1774, *near* Wakefield, *in* Yorkſhire, *by Mr.* Nicholſon, *Teacher of Mathematics in* Wakefield. *Communicated by Dr.* Prieſtley.

TO THE REV. DR. HORSLEY, SEC. R. S.

DEAR SIR,

Redde, April 14, 1774.

THE following account of a ſtorm of lightning was communicated to me by the obſerver Mr. NICHOLSON teacher of mathematics in Wakefield, who is a very ingenious man, and a good electrician. I have no doubt of his having given proper attention to the phænomena which he has mentioned, or of his exactneſs and fidelity in deſcribing them. Some of the circumſtances appear to me to be very extraordinary; and, as far as I know, quite new, not having been noticed by any writer that I have met with; I therefore think it very proper that the account of them be communicated to the Royal Society.

I am, DEAR SIR,

Your very humble ſervant,

JOSEPH PRIESTLEY.

ON

ON the 1ſt of March, about half an hour paſt ſix in the evening, as I was returning from CROFTON, a village near WAKEFIELD, I ſaw, in the north-weſt, a ſtorm approaching; the wind, which had been ſtrong all the day, ſetting from the ſame quarter; and, as in the afternoon of the ſame day, there had been ſome violent ſhowers of hail, I made the beſt of my way to the turnpike at Agbridge. The air was ſo much darkened, before the ſtorm began, that it was with difficulty I found my way.

When I was about three hundred yards from the turnpike, the ſtorm began; when I was agreeably ſurprized with obſerving a flame of light, dancing on each ear of the horſe that I rode, and ſeveral others much brighter on the end of my ſtick, which was armed with a ferule of braſs, but notched with uſing. Theſe appearances continued till I reached the turnpike-houſe, where I took ſhelter.

Preſently after, there came up five or ſix graziers, whom I had paſſed on the road. They had all ſeen the appearance, and were much aſtoniſhed. One of them, in particular, called for a candle, to examine his horſe's head, ſaying, "It had been all on fire, "and muſt certainly be ſinged."

After having continued about twenty minutes, the ſtorm abated, and the clouds divided, leaving the northern region very clear; except that, about ten degrees high, there was a thick cloud, which ſeemed to throw out large and exceedingly beautiful ſtreams of light, reſembling an *Aurora Borealis*, towards another cloud that was paſſing over it; and, every now and then, there appeared to fall to it ſuch

meteors

meteors as are called *falling ſtars.* Theſe appearances continued till I came to Wakefield; but no thunder was heard.

About nine o'clock a large ball of fire paſſed under the zenith, towards the ſouth-eaſt part of the horizon. I have been informed, that a light was obſerved on the weather-cock of Wakefield ſpire, which is about 240 feet high, all the time that the ſtorm continued.

XXXVIII. *Ac-*

XXXVIII. *Account of a Woman enjoying the Use of her right Arm after the Head of the* Os Humeri *was cut away. By* James Bent, *Surgeon, at* Newcastle. *Communicated by Dr.* Hunter.

Redde, April 28, 1774.

MR. WHITE, of Manchester, in an history of an operation performed upon the *humerus*, published in his treatise, entitled, *Surgical Cases, with Remarks*, and read before the Royal Society, Feb. 9, 1769; asserts, that he sawed off the upper head of that bone; and that his patient enjoyed the entire use of the joint. As the supposition of the head of the bone, with its ligaments, &c. being regenerated, must appear a little marvellous, and may prevent some from paying that attention to the operation, that it certainly merits, I flatter myself the following case will not be unacceptable to the Royal Society, as it proves, that the operation is not only practicable, but adviseable; and, at the same time, points out the nature of Mr. WHITE's mistake. In plate VI. fig. 1. he has given a drawing of the bone he cut off; the bare inspection of which is sufficient to convince any one, that it could be only the body of the *humerus* that

that was carious, and ſeparated from its *epyphyſis*; as the round head, with its cartilage, is wanting; and, I believe, there are few inſtances where the whole head of any bone is ſo entirely deſtroyed, in two or three weeks (*a*) by a *caries*, as that drawing repreſents. Hence it appears, that the joint, with its capſular ligament, remained in a ſound ſtate. We ſhall be farther confirmed in this opinion, if we attend to the deſcription he has given of his mode of performing the operation, (*vide* p. 58.) where he ſays, " that he began his inciſions at the orifice " which was ſituated juſt below the *proceſſus acro-* " *mion*." Now as the *proceſſus acromion* reaches a little over the joint, his beginning his inciſion below that muſt, of courſe, be below the inſertion of the *capſular* ligament.

MARY TURNER, a farmer's daughter, of IPSTONES, in this county, applied to me in October 1771, on account of an abſceſs in the joint of her right ſhoulder, with which ſhe had been afflicted near three years. Upon examining it, I found three apertures; two near the middle and lower edge of the clavicle; and the third, near the inſertion of the pectoral muſcle into the *humerus*. By introducing two probes, from the upper and lower orifices, they eaſily met in the joint, the opening into which, through the ligament, ſeemed to be very ſmall, and I could perceive the head of the *humerus* carious. As in this caſe, there ſeemed nothing to be propoſed for her relief, but either to amputate the arm; or, by

(*a*) *Vide* p. 57. of his Treatiſe.

an opening, to cut away the head of the bone. I determined upon the latter; and accordingly began my incision from the upper orifice, near the clavicle, and continued it over the joint to the insertions of the pectoral muscle: but finding a single incision too small, to allow me to get at the head of the bone readily, I separated a part of the deltoid muscle from its insertion into the clavicle; and likewise a little of its insertion into the *humerus*, which gave me liberty to come at the joint, the capsular ligament of which, from frequent inflammation, was so thickened, and kept the head of the bone so close to its socket, that it was with difficulty I could introduce a *spatula* between them. This likewise, after opening the ligament, prevented the head of the bone from rising out of its socket, upon pressing the elbow backwards, as is common in performing the operation upon a dead body, when the joint is in a sound state; so that I was obliged to separate it quite round, before I was able to come at the bone with the saw. I then moved the elbow backwards, and brought the head of the bone over the pectoral muscle, as I found it impossible to saw it directly a-cross, as Mr. WHITE directs, without leaving a considerable portion behind, that had been laid bare with the knife, and which, in all probability, must have exfoliated. By placing a card betwixt the edge of the deltoid muscle and the bone, and the saw within the incision, with its point into the joint, I cut off all that had been deprived of the *periosteum*, and had no exfoliation; nor had I occasion to take up one artery. As the tendon of the *biceps* muscle was cut through, I kept the fore-arm suspended.

My patient walked from my houſe to her own lodgings; her pain not very conſiderable, and ſhe recovered, by the common treatment, without any bad ſymptom. She left this town in ſix weeks after the operation.

By uſing her arm too freely when ſhe got home, the *cicatrix* was tore open about an inch and a half, which retarded its healing for three weeks longer; but from that time ſhe has remained well. She has the perfect uſe of the fore arm; can raiſe her elbow about five or ſix inches from her ſide, put her arm back, lace her ſtays, put on her cap, ſew, and do any buſineſs, as well as ever, that does not require the elbow to be more raiſed. The upper end of the *humerus* plays about an inch below the point of the *ſcapula*; and the *proceſſus acromion* and *coracoides* appear on each ſide of the *cicatrix*, at nearly equal diſtance. I mention this only to point out more exactly the courſe of my inciſion.

Newcaſtle, Jan. 18, 1774.

XXXIX. *Continuation of an experimental Inquiry concerning the Nature of the mineral elaſtic Spirit, or Air, contained in the* Pouhon *Water, and other Acidulæ. By* W. Brownrigg, *M. D. F. R. S. Addreſſed to Sir* John Pringle, *Bart. F. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR,

Redde, April 28, 1774.

YOU have often, with others of my friends, been pleaſed to inquire after the continuation of my experiments on the mineral water of SPA, which I promiſed to communicate to the Royal Society, with a view to explain "the mode of union that exiſts between the "air of thoſe waters, and the other principles of "which they are compoſed, together with the "relation which that elaſtic fluid bears to common "air, and to various other bodies (*a*)." Thoſe experiments

(*a*) See Concluſion to an experimental Inquiry concerning the mineral elaſtic Spirit, or Air, contained in Spa Water, &c. Philoſophical Tranſactions, Vol. LV.

riments I, indeed, intended to have preſented to the Royal Society, ſoon after my experiments on the ſame ſubject had been honoured with its approbation; but was, at that time, prevented from putting them in due form, by other more preſſing engagements; and, afterwards, thought the publication of them the leſs neceſſary, as a ſimilar mode of union between various abſorbent earths and water, by the medium of mephitic air, had been ſo fully explained by the Hon. HENRY CAVENDISH, in his excellent treatiſe on the RATHBONE-PLACE waters (*b*); and the ſolubility of iron in water, by means of the ſame air, had alſo been demonſtrated by Mr. LANE, in his valuable experiments on that ſubject (*c*). However, as my obſervations and experiments are different from thoſe of the above-named gentlemen, and the great efficacy of this aëreal ſolvent is by them ſhewn, in a variety of inſtances; and, more eſpecially, as a coagulating power in other kinds of air is alſo there detected; by which, and the diſſolving power of mephitic air, many great changes in bodies are daily produced, I therefore now lay theſe papers before you, requeſting, that if, after ſo long an interval, I may again be allowed to reſume this ſubject, you will do me the honour to communicate them to that moſt reſpectable body, over which you ſo worthily preſide. I am,

SIR,

Your moſt obliged,

and moſt obedient ſervant,

WM. BROWNRIGG.

(*b*) Philoſophical Tranſactions, Vol. LVII. p. 92.
(*c*) Phil. Tranſ. Vol. LIX.

PROPO-

PROPOSITION THE FIRST.

The ferruginous and absorbent earths contained in the Pouhon *water are kept dissolved therein, by means of the mephitic air, to which those earths are united.*

IN an *Inquiry concerning the nature of the mineral and elastic spirit, or air, contained in this water*, published in the Transactions of the Royal Society (*a*), it hath been shewn (*b*), that when the POUHON water is excluded from all contact with the common air, in such manner that the mephitic air which it contains has free liberty to fly from it into an empty bladder, this air does not separate from the water by any spontaneous motion, as it would from its rare texture and elastic force, was it at liberty to exert these its qualities: but, on the contrary, in this situation, it remains united to the other ingredients of of the water, when exposed to the most intense heat that we usually observe, in the open air, in this our climate.

It hath been further shewn (*c*), that this elastic fluid, when excluded from common air in the manner before related, is but slowly expelled from the POUHON water by a heat of 110 degrees of *Fahrenheit's* thermometer, although such heat is sufficient to raise water (a much heavier body) in distillation:

(*a*) Vol. LV. p. 233.
(*b*) Experiment the first of the said Inquiry.
(*c*) Experiment the second.

and

and ſo cloſely is air united to the other ingredients of the water, that it is not wholly expelled from them by a ſcalding heat of 160 or 170 degrees of the ſcale, when expoſed thereto for two hours.

Which experiments therefore prove, that this air is not detained in the POUHON water, by the preſſure of the atmoſphere, or by any other external force, as is the air with which beer, or other fermentiug liquors, are often ſurcharged, while they are confined in bottles: but that this elaſtic fluid is equally mixed with the watery element, and with the other ingredients of which this mineral water is compoſed, and exiſts with them in a ſtate of ſolution, or in a fixed ſtate, being attached to the water, and to the other ingredients diſſolved therein, by a force ſufficient to keep them all united together in one uniform compound, while this force is not removed by ſome external cauſe.

It further appears, from the ſame experiments (*d*), that ſo long as this air continues united to the other ingredients of the POUHON water, its martial and abſorbent earths do alſo remain ſuſpended therein; but, ſo ſoon as any part of this air is expelled by heat, thoſe earths begin to ſeparate from the water, which then grows white and turbid; and when, by continuance of the heat, more of this air is expelled, more of the earthly particles alſo ſeparate from the water, in the ſame proportion as its air is ſeparated from it; and while only a ſmall portion of the air remains, ſome portion of the martial earth alſo remains diſſolved in the water, as appears from its

(*d*) See Experiment the ſecond.

giving

giving a ſlight tinge of the purple, when mixed with galls: but none of thoſe earths are any longer detained in the water, than while it continues impregnated with ſome mephitic air; when this air is entirely ſeparated from the water, it is wholly decompounded, having loſt its diſtinguiſhing briſk and pungent taſte, and its power of ſtriking a purple colour with galls; its more volatile and elaſtic principles being exhaled, its metalline and abſorbent earths then ſubſide in a white flocculent ſediment, and no other ſubſtance remains diſſolved in the water, ſave only the ſmall portion of alkaline and neutral ſalts, which enter its compoſition.

From this ſhort recapitulation of the above-mentioned experiments, it therefore appears, that the POUHON water undergoes a decompoſition, when its air is expelled from it by means of heat. The oppoſite extreme of cold is alſo found to produce the ſame effect of decompounding the POUHON water, when this its aëreal principle is expelled from it by means of congelation.

For having poured ſome of this water into open tin veſſels, that were placed in the common freezing mixture of ſea-ſalt and ſnow, ſo ſoon as the water began to ſhoot into ice at the bottom and ſides of the veſſels, very minute bubbles of air inceſſantly aroſe therein, and were diſcharged from its ſurface with ſuch force, as to carry with them ſmall particles of the water to a conſiderable height; and continued thus to fly off, till all the water was congealed. The ice was very white, from the minute bubbles of air, which were every where interſperſed though it, and by which the frozen water considerably increased in bulk, so as to rise at its surface into a very convex form. The water, when thawed, was white and turbid, and soon let fall its metallic and absorbent earths in a white sediment: it then had almost lost its taste; and, being mixed with tincture of galls, only gave therewith a slight purple tinge. By a second congelation, it seemed almost entirely deprived of its air, and, with it, of the remaining part of its white earths; and, when decanted from its sediment, no longer struck a colour with galls. From these experiments it therefore appears, that so soon as this water is deprived of its air, whether it be by heat or by cold, it is no longer capable of keeping those earths dissolved, which, while it is impregnated with this air, continue suspended therein.

In these decompositions of the POUHON water, by heat and by cold, no volatile spirit, either acid or sulphureous, nor any other subtile matter, has been found to fly from it, save only its mephitic air: while this air is present in the water, its martial and absorbent earths remain dissolved therein; so soon as this air is separated from the water, in whole or in part, those earths, either in the whole or in part, do also separate from it, and are no longer suspended therein, than while they are united to a due proportion of this aërial solvent. From whence it appears, that this mephitic air is the medium by which the metalline and absorbent earths contained in the POUHON water are therein held in solution; and, contrary-wise, that those earths are the medium, by which this air is more firmly united to the watery element in this compound, in which it enters as a principal ingredient, and, by its ſolution in the water, and its union with theſe earthy ſubſtances, from a a very rare volatile and elaſtic body, is reduced to a fixed ſtate.

This diſſolving power of mephitic air may farther be proved from the recompoſition of the POUHON water, by adding thereto the air expelled from it by coction. But as Mr. CAVENDISH has already ſhewn, that the abſorbent earths of RATHBONE-PLACE water may be rediſſolved by the mephitic, or fixed air, which had been extracted from that water; and as Mr. LANE has alſo demonſtrated, that iron is rendered ſoluble in water, by the medium of mephitic air, I therefore ſhall not, at this time, detain the Society with my experiments on the ſame ſubject; but as thoſe experiments contain ſome phænomena, that have not yet been noticed, I may, perhaps, offer them to the public on ſome future occaſion.

SCHOLIA.

From the foregoing experiments, it appears that the mephitic air and martial earth, contained in the POUHON waters, ſtrongly attract each other, and, uniting together, form a concrete ſoluble in water, and readily diſtinguiſhed therein, by the peculiarly briſk acidulous taſte, which it receives from this aëreal principle, joined to a rough ſubaſtringent taſte, which proceeds from the iron. This concrete, like other vitriols of iron, ſtrikes a black colour with galls, and may well be eſteemed a ſaline body of the neutral kind, of which the mephitic air conſtitutes the ſpirituous ſolvent, and the martial earth its baſe. It further appears, that the mephitic air is poſſeſſed of all the properties, by which ſome of the chemiſts have diſtinguiſhed thoſe pure and ſimple bodies, or ſpirits, which by them are eſteemed, in *their own nature*, and *of themſelves*, ſaline, and which, in union with other bodies, form ſalts that are more compound. For this aëreal ſolvent, in like manner with the pure acid ſpirits, is ſoluble in water, and imparts thereto its peculiar ſharp and acidulous ſavour: moreover, in combination with various metalline and abſorbent earths, this volatile elaſtic ſpirit, like thoſe acids, forms various ſaline concretes of the neutral kind; inaſmuch as thoſe metalline and abſorbent earths, when united to this elaſtic ſpirit, are thereby rendered ſoluble in water; and, in union therewith, acquire peculiar ſavours, reſulting, in part, from this their ſpirituous principle, and, in part alſo, from the particular kind of earth with which it is combined. This air, therefore, conſidered in the relation which it bears to ſeveral earthy ſubſtances, and to water; conſidered alſo as it impreſſes the organs of taſte, with its peculiar briſk and acidulous ſavour, may juſtly be ſtiled a *mineral elaſtic ſpirit of a ſaline nature*, and is ſufficiently diſtinguiſhed from all other ſaline ſpirits, by its great rarity, and by its aëreal nature. How far, and under what laws, this relation between mephitic air and various ſaline earths, and other bodies, may be extended, hath not yet been fully diſcovered: ſuffice it in this place to remark, that a claſs of ſaline bodies of a neutral nature are here detected, compoſed of various earthy baſes, united to a volatile aëreal ſpirit, all of which

which agree in one common ſolvent, the mephitic air, but differ from each other, according to the nature of the baſe to which this air is united.

The agreement of theſe ſaline concretes with neutral ſalts in theſe eſſential properties, by which theſe laſt are diſtinguiſhed from other more ſimple ſaline bodies, will further appear from their decompoſition; which is effected by thoſe various ways, and under the ſame laws, by which all other neutral ſalts are decompounded; namely, by all thoſe different ways, by which the acid ſpirits, and the terrene or alcaline baſes of neutral ſalts, can be ſeparated from each other.

For, *firſt*, the aëreal ſpirit of theſe ſaline concretes is forced, *by fire*, from its union with the earthy baſe, which it holds diſſolved in water, in like manner as the acid ſpirit of other neutral ſalts are expelled by fire from the more fixed principles, which enter the compoſition of thoſe ſalts. The degree of heat required to ſeparate the acid ſpirit of neutral ſalts, from their more fixed alcaline or earthy baſe, varies in the decompoſition of almoſt every different kind of ſalts; and the extreme volatility and expanſive force of this aëreo-ſaline principle renders it more eaſily ſeparable, by heat, from the fixed principles to which it unites, than any other kind of ſaline ſpirit.

Secondly. The ſaline concretes, formed with this aëreal ſolvent (in like manner as other neutral ſalts), are decompounded by the *addition of ſtronger acids*, which more powerfully attract the terrene or metallic baſe of theſe concretes, than it is attracted by their light and ſubtle aëreal ſpirit, and detaches from them the aëreal ſolvent to which thoſe earths were before united. All

acids,

acids, found in a liquid form, have this effect, from the light vinous acids to the moſt ponderous acid of vitriol; ſo that the affinity between theſe metalline and abſorbent earths, and this their aëreal ſolvent, is leſs than that which exiſts between the ſame earths and all the known acid ſpirits. In all additions of theſe acids to the ſpirituous or acidulous waters, an effervescence has been obſerved, not readily accounted for, by thoſe who ſuppoſe an acid to predominate in thoſe waters. The conflict and diſcharge of air here ariſes from the expulſion of the aëreal principle from its terrene baſe; in like manner as the acids of ſea ſalt and nitre are expelled, with effervescence, from their alcaline baſes, by the more powerful acid of vitriol. And here, by the way, it may be proper to remark, that the vitriolic acid, when mixed with the *acidulæ* and other chalybeate waters, doth not preſerve thoſe waters from decay, as the excellent HALES, and others, after him, have ſuppoſed; but, on the contrary, deſtroys their texture, or decompounds them, by expelling their elaſtic ſpirit, and entering into new combinations with their earthy principles; thereby forming a new compound, leſs periſhable indeed than the former, but alſo leſs efficacious in the cure of many diſeaſes. When Rheniſh wine is added to the *acidulæ*, the large quantity of air that flies off may, in part, proceed from the wine; but when I mixed the vitriolic acid with POUHON water, a conſiderable quantity of air was indeed diſcharged; but not the whole which that water holds in ſolution. I therefore conjectured, that ſome part of the air, contained in that water, might be imbibed by the ſuperabundant acid, which

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I uſed in the experiment, and that more mephitic air might, perhaps, have been expelled from the water, had I only mixed with it the exact quantity of this acid, that was required to diſſolve the earthy ſubſtances contained therein.

Thirdly. Theſe ſaline concretes, contained in the POUHON water and other *acidulæ*, are ſubject to decompoſition, not only from acids, as before related, but alſo from *alcalies*, whether fixed or volatile: all which more powerfully attract this ſubtile aëreal principle than it is attracted by the martial and abſorbent earths, to which it is united in thoſe waters. And here again appears an exact agreement between theſe aëreo-ſaline concretes and various neutral ſalts, in the mode of their decompoſition. For the ammoniacal ſalts (which are all compoſed of the volatile *alcali*, united to an acid ſpirit, either muriatic, nitrous, or of ſome other kind) ſo ſoon as one of the fixed alcalies, or quicklime, is added to any of them, the acid ſpirit which it contains, quitting its union with the weaker volatile *alcali*, this laſt is let looſe; aud the ſtronger *alcali*, or quick-lime, takes its place; between which and the acid ſpirit a new combination is formed. The ſame happens when any *alcali*, either fixed or volatile, is added to the *acidulæ*; their elaſtic ſpirit then quits the ferruginous and abſorbent earths, to which it was joined, and forms a new combination with the *alcali*, by which it is more powerfully attracted than by theſe earthy ſubſtances. Theſe earths, therefore, being no longer ſuſpended in the water by the aëreal ſolvent, render it turbid and milky, until they have gradually ſubſided therein, in the form of a white

ſediment:

ſediment: for ſuch is the native appearance of the martial earth, as well as of all the other earths contained in theſe waters, as will be ſhewn hereafter. In theſe decompoſitions of acidulous waters, by means of alcalies, no effervеſcence, or diſcharge of air-bubbles, takes place; for here the air is all abſorbed by the *alcali* added thereto, and not expelled from the water, as it is in the decompoſition of the ſame waters, by means of ſtronger acids.

When the *acidulæ* are mixed with common ſoap, a two-fold decompoſition takes place. The fixed *alcali*, quitting the unctuous ſubſtances, to which it was joined in the ſoap, unites itſelf to the aëreal ſpirit, or mephitic air, of thoſe waters, while this air, at the ſame time, deſerts the earthy ſubſtances with which it was before combined. The ſame new combinations ſeem to take place, when ſoap is mixed with any of thoſe waters which are uſually called hard; many of which waters have been found to contain an earthy ſubſtance, diſſolved by means of this ſubtile aëreal principle.

The above obſervations and experiments ſhew an exact agreement, in the ſeveral ways, by which the various neutral ſalts, and thoſe ſaline concretes formed of mephitic air united to an earthy baſe, are decompounded. It ought, however, here to be remarked, that the ſaline concretes, which exiſt in the POUHON water, in a diſſolved ſtate, though evidently of the neutral kind, have not hitherto been obtained in a ſolid form; owing, perhaps, in ſome meaſure, to the great volatility of their ſpirituous principle; but chiefly to their being ſubject to decompoſition, from the precipitation of their earthy baſe,

base, by means of common air, during the evaporation of the water in which they are dissolved, as will be shewn hereafter.

The mephitic air of the *acidulæ*, although it is soluble in water, and imparts thereto its brisk and pungent taste, which has been usually stiled subacid; and although it produces effects exactly similar to those of acid spirits (by readily uniting to various earthy substances, which of themselves are not soluble in water, but, by their union with this aëreal fluid, are rendered soluble therein, and communicate to the water peculiar savours, and form therein saline concretes of the neutral kind; which concretes, so formed, are again separable into their component ingredients, by all those ways by which the acid and alcaline principles of other neutral salts are separable from each other) yet it differs from all acid spirits, found in a liquid form, in its rare texture and its elastic quality, and in not striking a red colour with syrup of violets, and other blue tinctures of vegetables; which change in the blue colour of those tinctures, is usually esteemed a test of the presence of an acid. Besides the trials which other gentlemen and myself have made, by mixing syrup of violets with pure water, impregnated with various kinds of mephitic air, in which no change in the colour of the syrup was observed, I have for several days suspended pieces of linen, that had been dyed blue with fresh juice of violets, in the mephitic air of SPA water, and also in that of chalk; and, when the linen was taken out of the said air, did not perceive its blue colour in any wise changed, al-

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though the ſame pieces of dyed linen were inſtantly turned of a green colour, when expoſed to the fumes of ſpirit of hartſhorn. Whether therefore, and under what relations, this aëreo-ſaline ſpirit may merit the title of an *acid*, I leave to the determination of others. Such, however, it has appeared to be to many philoſophers, ſince this mephitic air is doubtleſs the ſame with the *acidum vagum fodinarum* of BOERHAAVE and others; and with the *acidum centrale perpetuum inexhauribile* of BECHER; with the *ſpiritus ſulphureus aëreo-æthereo-elaſticus* of HOFFMAN; and the *ſal embrionatus* and *ſal eſurinus* of the ſagacious HELMONT, which, he ſays, corrodes the ore of iron, and with it forms a volatile vitriol in the POUHON water. All theſe, and many other philoſophers, had acquired ſome knowledge of this ſubtile aëreo-ſaline principle from contemplating its effects; but, not having obtained it in a palpable form, were unacquainted with ſeveral of its principal properties.

From conſidering the great ſubtility of this aëreo-ſaline principle, its power of diſſolving many earthy ſubſtances, together with its property of uniting readily to water, and with it, of pervading the very minute veſſels of the animal frame, without injuring them, as ſtronger acids do by their corroſive quality, we may from thence form ſome judgement of the great efficacy of this air, as a *de-obſtruent* and *ſolvent*, in many diſeaſes of the human body ariſing from preternatural concretions and obſtructions thence enſuing. If to theſe we add the great antiſeptic powers of this kind of air, which it poſſeſſes in commonmon with acids (and which were firſt detected by SIR JOHN PRINGLE, and have ſince been more fully explained by Mr. MACBRIDE and Dr. PRIESTLY); we then, in ſome meaſure, may account for thoſe extraordinary effects, which this kind of air is found to produce in the cure of many obſtinate diſeaſes, with which mankind are afflicted.

XL. *Particulars of the Country of* Labradore, *extracted from the Papers of Lieutenant* Roger Curtis, *of His Majesty's Sloop the* Otter, *with a Plane-Chart of the Coast. Communicated by the Honourable* Daines Barrington.

Redde, Feb. 24, 1774.

THERE is no part of the Britiſh dominions ſo little known as the immenſe territory of LABRADORE. So few have viſited the northern parts of this vaſt country, that almoſt from the ſtreights of BELLEISLE, until you come to the entrance of HUDSON'S BAY, for more than ten degrees of latitude, no chart, which gave any tolerable idea of the coaſt, had hitherto been formed. The barrenneſs of the country explains why it has been ſo ſeldom frequented. Here avarice has but little to feed on.

Perhaps, without an immoderate ſhare of vanity, I may venture to preſume, that, as far as I have been, which is to the latitude of 59° 10′, the draught, which I have been able to form, is by much the beſt that has hitherto been made.

Others have gone before me, bleſt with abilities ſuperior to mine, and to whom I hope to be thought equal

equal only in affiduity. But I had advantages of which they were deftitute; with a fmall veffel, and having an Indian with me, who knew of every rock and fhoal upon the coaft, I was enabled to be accurate in my obfervations; and thefe are the reafons, why I deem my own fketch preferable to all others.

Of the COAST.

As this country is one of the moft barren in the known world, fo its fea-coaft is the moft remarkable. Bordered by innumerable iflands, and many of them being a confiderable diftance from the main land, a fhip of burthen would fail a great way along the coaft, without being able to form any notion of its true fituation.

Hence it is that all charts of it have been fo extremely erroneous; and hence arofe thofe opinions, that fome of the inlets extended a vaft diftance into the country, if not quite into the fea of HUDSON'S BAY.

DAVIS'S INLET, which has been fo much talked of, is not twenty leagues from the entrance of it to its extremity.

The navigation here is extremely hazardous. Towards the land, the fea is covered with large bodies, and broken pieces, of ice; and the farther you go northward, the greater is the quantity you meet with.

Some of thofe maffes, which the feamen call iflands of ice, are of a prodigious magnitude, and they are generally fuppofed to fwim two thirds under water. You will frequently fee them more than a hundred feet above the furface, and to fhips in a ftorm, ſtorm, or in thick weather, nothing can be more terrible.

Thoſe prodigious pieces of ice come from the north, and are ſuppoſed to be formed by the freezing of cataracts upon the lands about EAST GREENLAND and the Pole. As ſoon as the ſeverity of the winter begins to abate, their immenſe weight breaks them from the ſhore, and they are driven to the ſouthward. To the miſerable inhabitants of LABRADORE, their appearance upon the coaſt ſerve as a token of the approach of ſummer.

Of the CLIMATE, SOIL, and NATURAL PRODUCTIONS of the country.

This vaſt tract of land is extremely barren, and altogether incapable of cultivation. The ſurface is everywhere uneven, and covered with large ſtones, ſome of which are of amazing dimenſions. There are few ſprings; yet, throughout the country, there are prodigious chains of lakes, or ponds, which are produced by the rains, and the melting of the ſnow. Theſe ponds abound in trout, but they are very ſmall.

There is no ſuch thing as level land. It is a country formed of frightful mountains, and unfruitful vallies. The mountains are almoſt devoid of every ſort of herbage. A blighted ſhrub, and a little moſs, is ſometimes to be ſeen upon them; but, in general, the bare rock is all you behold. The vallies are full of crooked low trees, ſuch as the different pines, ſpruce, birch, and a ſpecies of the cedar. Up ſome of the deep bays, and not far from the water, it is ſaid, however, there are a few ſticks of no inconſiderable confiderable fize. In a word, the whole country is nothing more than a prodigious heap of barren rocks.

The climate is extremely rigorous. There is but little appearance of fummer before the middle of July; and, in September, the approach of winter is very evident: It has been remarked, that the winters, within thefe few years, have been lefs fevere than they were known heretofore. The caufe of fuch an alteration it would be difficult to difcover.

All along the coaft there are many rivers, which empty themfelves into the fea; yet there are but few of any confideration, and you muft not imagine that the largeft are any thing like what is generally underftood by a river. Cuftom has taught us to give them this appellation, but the moft of them are nothing more than broad brooks, or rivulets. As they are only drains from the ponds, in dry weather they are everywhere fordable; for running upon a folid rock, they become broad, without having a bed any depth below the furface of the banks.

The fuperficial appearance of this country is exceedingly unfavourable. What may be hidden in its bowels, we cannot pretend to fuggeft; probably it may produce fome copper; the rocks, in many places, are impregnated with an ore of that refemblance. Something of a horny fubftance, which is extremely tranfparent, and which will fcale out into a multitude of fmall fheets, is often found amidft the ftones. There are both black and white of this fort, but the black is the moft rare. It has been tried in fire, but feemed to be no ways affected by heat.

The ſpecies of wood here are not very various: excepting a few ſhrubs, which have as yet received no name from the Europeans, the principal produce of the country is the different ſorts of SPRUCE and PINE. Of thoſe, even in the more ſouthern parts, there is not abundance; as you advance northwards, they gradually diminiſh, and by the time you arrive at the ſixtieth degree of latitude, the eye is not delighted with any ſort of herbage. Here the wretched reſidents build their miſerable habitations with the bones of whales. If ever they cheer their aching limbs with fire, they gather a few ſticks from the ſea-ſhore, which probably have been wafted from NORWAY, or from LAPLAND. Here a vaſt quantity of ſnow remains upon the land throughout the year.

Although the winter here is ſo exceſſively rigid, in ſummer the heat is ſometimes diſagreeable, and in that ſeaſon the weather is very moderate, and remarkably ſerene. It is but ſeldom foggy, ſpeaking comparatively between this and NEWFOUNDLAND; nor are you ſo frequently liable to thoſe deſtructive gales of wind, which viſit many other parts of the globe.

It is, in general, high land, and ſometimes you meet with mountains of an aſtoniſhing height; you are alſo frequently preſented with proſpects that are really awful, and extremely romantic.

There is no great variety of animals in this rocky country, nor are they at all numerous. Here are the REIN-DEER; the females have horns, which nature has given them to procure food, for with theſe they beat away the ſnow in winter, and, by that means, come at the tops of trees, which, during the inclemency of that ſeaſon, is their only ſuſtenance.

There are BEARS black and white, WOLVES, the CARKASHEW, FOXES, PORCUPINES a great many, the MOUNTAIN-CAT, MARTINS, BEAVERS, OTTERS, HARES, and a few ERMINE.

The female BEAR'S, eſpecially the white, in defence of their young, will attack any thing; but at other times, unleſs you wound them, it is ſaid they are not very dangerous. Many people affirm, and mention inſtances, that, being purſued by a bear, if you fall on your face, and remain immoveable, it will retire, without doing you any miſchief. A ſingle WOLF will never approach a man, nor need he be afraid of ſeveral attacking him together, unleſs in winter, when they are impelled to it by hunger.

A venomous reptile, or inſect, is not to be found here, except TOADS, and they are extremely rare. The whole country is filled with very ſmall flies, which are exceedingly tormenting.

Here are EAGLES, HAWKS, the HORN-OWL, and the RED-GAME, with a ſmaller ſort which reſemble them, called the SPRUCE-PARTRIDGE: theſe we may call the conſtant inhabitants of the feathered kind.

Of ſea-birds, there are great variety.

In the ſummer, the woods are viſited with many ſorts of little birds, and ſome of them are of beautiful plumage. They breed here, but, towards winter, they ſeek a happier climate.

In the autumn, there come a prodigious quantity of birds, which are called CURLEWS. They are about the ſize of a wood-cock, ſhaped like them, and nearly of the ſame colour; extremely fat, and moſt delicious eating. They continue here but a very little while, nor is it known from whence they come, or whither they go.

It is a very remarkable phænomenon, that ſeveral beaſts, and ſome of the birds, change their colour with the ſeaſons. In the winter, your eye ſcarcely beholds any thing but what is white. In this miſerable climate, providence has armed moſt animals with a defence againſt the rigour of winter. The quadrupedes are cloathed with a longer thicker hair, or fur; to the birds are given ſoft down, and feathers of a cloſer contexture, than thoſe of milder countries.

The principal fiſh are WHALES, the COD-FISH, and SALMON. Of SHELL-FISH, there are but few ſorts, and theſe in no great plenty. LOBSTERS, there are none at all; which is very remarkable; for, at a particular part in the Streights of Belliſle, not more than five or ſix leagues from Newfoundland, there are great abundance.

Obſerving that the ſeal-darts of every Indian were headed with the teeth of the SEA-COW, I was led to inquire, how they came by them; and particularly, as upon theſe inſtruments they ſeemed to fix but little value. I was informed, that they purchaſed them from the Indians of Nuckvank, about the latitude 60°; and that thoſe Indians were viſited by multitudes of the ſea-cows, in the winter, and that they killed a vaſt number of them.

My Indian, of whom I obtained this knowledge, could not tell me where the ſea-cows went to in the ſummer, becauſe he had never been beyond Nuckvank; but he told me, that he had often heard the northern Indians ſay, that, a good way

farther

farther to the north, they went aſhore upon iſlands, which was thought a very extraordinary thing.

If the ſituation of theſe iſlands was known, it is very probable, an extremely valuable ſea-cow fiſhery might be carried on there.

Of the INHABITANTS.

It is not ſurprizing, that ſuch a country as has been deſcribed ſhould be thinly inhabited. The human ſpecies upon this extenſive territory are but few; and ſuch as we know of are extremely ſavage. The populouſneſs of mankind generally bears an affinity to the ſoil they live on. Upon barren rocks, covered with ſnow for more than half the year, and where the winters are ſo rigorous, and of ſuch long continuance, we cannot expect to find the inhabitants ſo very numerous.

The people of this country form various nations or tribes; and are at perpetual war with each other. Formerly the ESQUIMAUX, who may be called a maritime nation, were ſettled at different places upon the ſea coaſt quite down to the river ST. JOHN's; but, for many years paſt, whether it has been owing to their quarrels with the Mountaineers, or the incroachments of the Europeans, they have taken up their reſidence far to the north.

A good way up the country live a people diſtinguiſhed by the appellation of MOUNTAINEERS, between whom and the Eſquimaux there ſubſiſts an unconquerable averſion. Next to the Mountaineers, and ſtill farther weſtward, you come to a nation called

called the ESCOPICS. We know not much of this people: and beyond them, are the Hudſon Bay Indians, with whom the world is but little better acquainted. There are, doubtleſs, in ſuch a vaſt tract of land, a great number of other nations; but of whom we have not the leaſt information.

We are ignorant as yet, why theſe poor people bear each other ſuch implacable hatred; but it ſeems a melancholy reflection, that, in ſo large a country, and withal ſo badly inhabited, the few there are ſhould be eternally ſolicitous to extirpate one another: though, perhaps, multiplying the ſpecies would augment the natural ſcarcity of proviſions, and only ſerve to render them all more miſerable.

The Mountaineers are eſteemed an induſtrious tribe; and, for many years, had been known to the French traders. Their chief employment is to catch fur, and procure the neceſſaries of life. They are extremely illiterate, but generally good-natured; and are reckoned to be leſs ferocious than any other of the Indians. This ſoftneſs of their manners is owing to their long intercourſe with Europeans; and the other nations will doubtleſs loſe their ſavage diſpoſition, in proportion as they imbibe our cuſtoms.

They come every year to trade with the Canadian merchants, who have ſeal-fiſheries on the ſouthern part of the coaſt, and have the character of juſt dealers. They are immoderately fond of ſpirits; for which, blanketing, fire-arms, (in the uſe of which they are remarkably dexterous), and ammunition, they truck the greateſt part of their furs.

Their canoes are covered with the rind of birch; and, though ſo light as to be eaſily carried, yet ſufficiently large to contain a whole family and their traffic. By means of the multitude of amazing ponds throughout this country, they convey themſelves a vaſt diſtance in a very little time. Whenever they find a pond in their way, they embark on it, and travel by water; when its courſe alters, and by following it they would lengthen their diſtance any thing conſiderable, they land, place their canoe on their head, and carry their baggage on their ſhoulders, until other water gives them an opportunity of re-embarking. They are moſt excellent travellers. They bear inconceivable fatigue with aſtoniſhing patience, and will travel two days ſucceſſively without taking any ſort of nouriſhment.

Theſe Indians are of a deeper colour than the Eſquimaux. They are low of ſtature. Though of a robuſt conſtitution, their limbs are ſmall, and extremely well adapted to the rocky country they are continually traverſing. They have no hair, except on the head. For many years they have dreſſed their food, which they boil to a jelly; whereas the other Indians eat every thing raw. Their manner of feeding is certainly conducive to that hoſpitable diſpoſition, which they are ſaid to poſſeſs, and was doubtleſs originally a great cauſe of their civilization. Indeed the Eſquimaux begin to imitate us; but it is no more than a year or two, that the buſineſs of cookery has been known among them.

It is their cuſtom to deſtroy the aged and decrepid, when they become uſeleſs to the ſociety, and burthenſome to themſelves. They have been queſtioned of this ſeeming inhumanity; and perhaps their reaſons are not totally devoid of ſound philoſophy.

They

They tell you, that as it is with difficulty they procure the neceſſaries of life, they can admit of none, who do not contribute towards acquiring it; that having no fixed reſidence, and it being impoſſible to carry the helpleſs with them, as they are obliged to be continually traverſing the country; they aſk you, if it is not better to put an end to miſerable beings, than ſuffer them to periſh with cold and hunger? The ſon generally does this kind office for the father; and, it having ever been a practice among them, they wonder at our conſidering it as an act of inhumanity.

Of the ESQUIMAUX.

The ESQUIMAUX Indians, inhabiting the ſea-coaſt of the northern part of LABRADORE, are indiſputably from GREENLAND. They are a very deep tawney, or rather of a pale copper-coloured complexion. Conſidered altogether, they are inferior in ſize to the generality of Europeans; and but a few among them are of good ſtature. They bear a very near reſemblance to the LAPLANDERS, both in their perſons and cuſtoms. It is not inſinuated that they are a Lapland colony; but it is very probable, they came originally from Greenland. They have beards, ſo have the Greenlanders, and indeed ſo have the inhabitants of Lapland: whereas the Iroquois, the Hurons, the Eſcopics, and the Mountaineers their neighbours, have hair no where except on the head. It is true this is no proof. The Samojedes are no more hairy than the nations we have juſt mentioned; but

but who will believe that any part of the new world was peopled from Samojeda? All we know is, that the great Author of Nature has been pleaſed to diverſify the human ſpecies upon every continent.

Theſe Indians, in general, are not very diſagreeably featured, though there are ſome among them who are extremely ugly. They are flat-viſaged, and have ſhort noſes. Their hair is black and extremely coarſe. Their hands and feet are remarkably ſmall. The women load their heads with large ſtrings of beads, which they faſten to the hair above the ears; and they are fond of a hoop of bright braſs, which they wear as a coronet. Their dreſs is intirely of ſkins, except thoſe who have trafficked for a little blanketing. It conſiſts of a ſort of hooded cloſe ſhirt, breeches, ſtockings, and boots. They wear the hairy ſide towards them, according to the ſeaſons; and between the dreſs of the different ſexes there is no variety, except that the women wear monſtrous large boots, and their upper garment is ornamented with a tail. In the boots they occaſionally place their children; but the youngeſt is always carried at their back, in the hood of their jacket.

They have no ſort of bread; but live chiefly on the fleſh of ſeal, deer, fiſh, and of birds. Till very lately they ate every thing raw, and putrefaction was deemed no objection.

In the winter they live in houſes, or rather caverns, for they are ſunk in the earth. In the ſummer they dwell in tents, which are made circular with poles, and covered with ſkins ſewed together. The houſe conſiſts of one room, and though not very large, yet it contains ſeveral brothers or other relations, with their wives and children. Their tents are ſtill more crouded; becauſe, as the whole ſummer they are generally rambling up and down the coaſt, they endeavour to diminiſh their baggage as much as poſſible.

In the ſummer they find no difficulty in procuring food; but it is not ſo with them in winter, againſt which ſeaſon they dry fiſh in the ſun, and preſerve the fat or oil of ſeals in ſkin veſſels.

They have no ſort of beverage among them, except water. They are not as yet fond of ſpirituous liquors, and there are but few that will taſte of any. It is certain they are able to ſubſiſt a long while without eating; but when they have plenty, they devour a prodigious quantity. When they are preſſed with hunger, and have nothing to ſatisfy it, they make their noſes bleed, and ſuck the blood to ſupport themſelves.

They appear to be abſolutely without any ſort of religion; nor have they ſo much as an object of adoration among them. They live happy in their ignorance, and enjoy the bleſſing of being ſtrangers to perſecution and torture.

They are without any government; and no man is ſuperior to another, but as he excels in ſtrength or in courage, and in having the greateſt number of wives and children. Being entirely without laws, general cenſure is the only puniſhment for the moſt deteſtable crimes.

They have no marriage ceremony. A wife is conſidered as property, and a huſband lends one of his wives to to a friend. The wives are given very


A
CHART
of part of the Country of
LABRADOR.
Taken by order of Commodore Shuldham,
in a Tour up the Coast in the Year 1773.
By
Lieutenant Roger Curtis.
N.B. the pricked line denotes Mr. Curtis's track up the Coast, but the
same passage must not be attempted by a Ship of burden.
NAIN
OGBUCKTOKE
CANYKETOKE
Inlet 20 Leagues
Inlet 15 Leagues
Kyucktabuck
Cannotocktut

Cannotocktuit
IVUCKTOKE
Inlet 30 Leagues
C. Webuck
Byrons Bay
Ockpaseeguock
Okehowtet
Low Land towards the Sea
Inmuckchucklick
Netshucktoke
C. Con
Noebootaleweet
Low Land towards the Sea
Ikenaluke
C. Bluff
C. Columbus
Webatelue
Etterlick
Kyomuckoomick
EAST ISLAND
54
55
56
57
58

early in marriage, frequently ſeveral years before conſummation; and the reaſon of this is, becauſe the girl's father, by that means, has one leſs in family to provide for.

The Eſquimeaux men are extremely indolent; and the women are the greateſt drudges upon the face of the earth. They do every thing except procure food, and even in that they are frequently aſſiſtants; ſo that they are at continual labour. They ſew with the ſinews of deer, and their needle-work is amazingly neat.

Their language is the ſame as the Greenlanders. It is not altogether devoid of harmony, and the women have very delicate voices.

Theſe Indians are ſtrangers to jealouſy; they do not appear to be at all quarrelſome, and they very ſeldom ſteal from one another. They do not ſeem very paſſionate; but woe be to the woman that offends her huſband.

If polygamy was not allowed among them, their numbers would be very few. Some of the women bear many children; but, in general, they are by no means fruitful. The wives live happily together; and, if deſerving, ſhare equally in their huſband's favours.

They have but few diſeaſes among them, and conſequently are without phyſicians; they believe, that tying to their neck or wriſts the particular part of ſome fiſh or animal, according to the complaint, will produce a cure. The moſt dreadful malady upon earth, has not as yet reached them: nor have they ever been viſited by the ſmall pox.

 Theſe

Theſe Indians cannot reckon numerically beyond ſix; and their compound numbers reach no farther than twenty-one. Every thing beyond is a multitude.

They live always upon the ſea-ſhores, from their dread of the Mountaineers. Their canoes contain only one perſon; they are extremely long in proportion to their breadth, being upwards of twenty feet by two; they are covered with ſkins, and are extremely light, ſo that they are overſet with the leaſt inclination to one ſide or the other. It is really a very extraordinary circumſtance, that though theſe people are almoſt ever in their canoes, which are ſo exceſſively tickliſh, there is not one among them that can ſwim.

They navigate their ſhallops without a compaſs in the thickeſt fogs, and are very good coaſters. They have always a vaſt number of dogs in their camp, which are of ſeveral uſes. Theſe animals ſerve as a guard; they are food; their ſkins are valuable for cloathing; and they draw their ſledges in winter. They have not the power of barking, but their howl is hideous; they are large, and have a head like a fox, whereas the dogs of the Mountaineers are extremely ſmall. The Samojedes and the Laplanders train the rein-deer to their ſledges. The country of Labradore produces theſe animals; but they are only ſerviceable to the Eſquimeaux for food and raiment.

The weapons of theſe Indians are, the dart and the bow and arrow. They are not very expert in the uſe of either; although it is with theſe they defend ſend themſelves, and procure the neceſſaries of life.

Of their NUMBERS.

This is a calculation not eaſily formed. I have been at ſome pains to obtain information upon this head; and by the means which I ſhall purſue, of their populouſneſs one may be able to make a tolerable eſtimation.

Leaving the ſtraights of Belleiſle, and proceeding northwards, the firſt tribe, or ſettlement, you come to, is that of Ogbuctoke. Here they have the moſt boats, by reaſon of their being neareſt to the Europeans; and allowing fifteen perſons to each boat, including men, women, and children, which is rather an under-rating, the boats being eighteen, the number of this tribe will be, 270

The next tribe is at Nonynoke, where the Moravians are ſettled. Theſe have only five boats; but then they are more crouded, ſo admitting twenty to a boat their number is, 100

Keewedloke is the ſeat of the next tribe. Here they have no more than ſix boats; yet notwithſtanding, they are the largeſt tribe upon the coaſt. My Indian imagined them to be one third more numerous than the Ogbucktoke tribe, ſo that they amount to about 360

Nepawktoot, 70

800

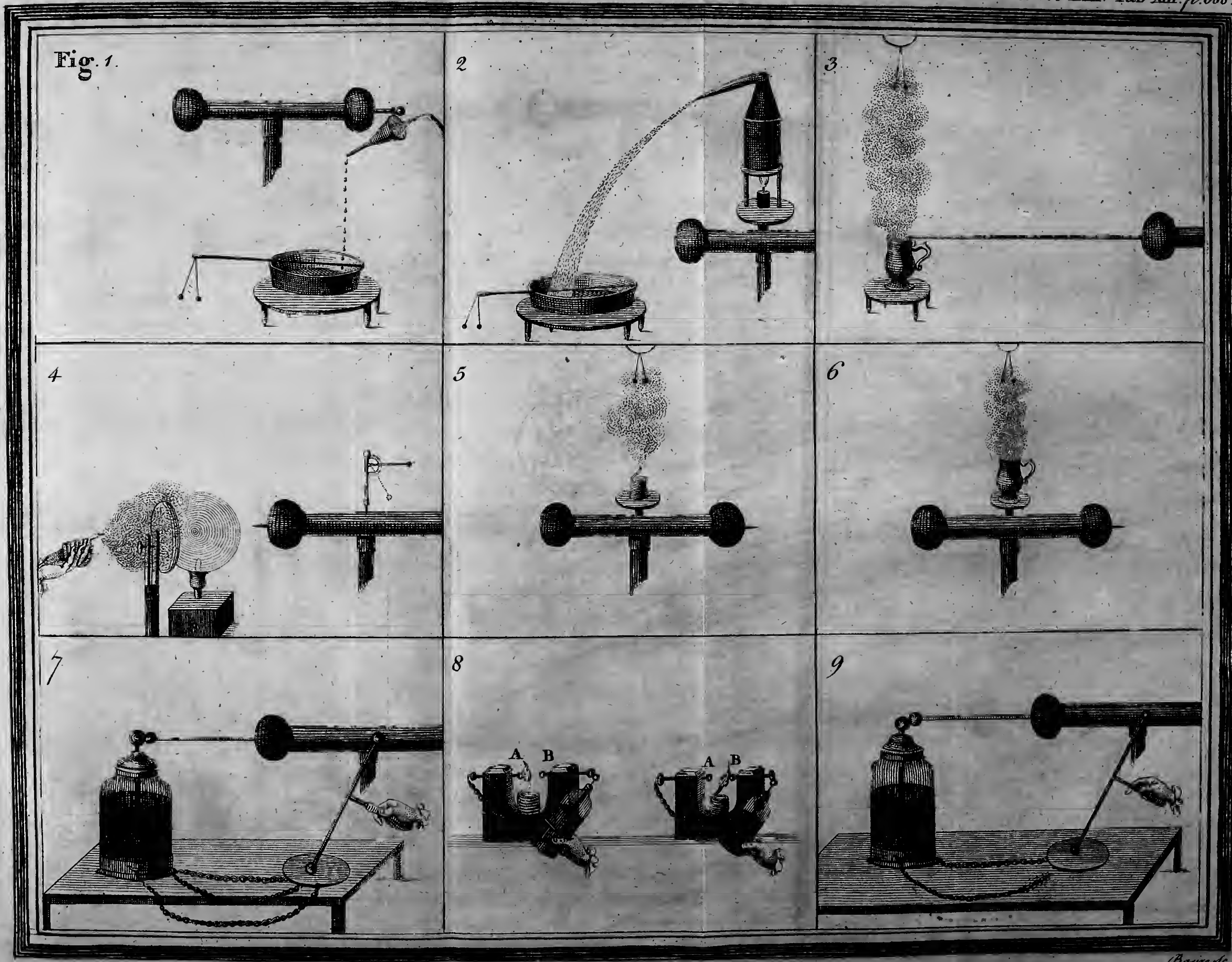
Brought over,	800
Cannuklookthuock, nearly equal to Keewedloke, ſuppoſe,	345
Chuckluck, about	140
Chuckbelweet,	40
Noolatucktoke,	30
Nuckvauk,	60

Hitherto, as I was myſelf no farther than Keewedloke, I have been guided in my computation by the Indian that accompanied me; but he having never been beyond Nuckvauk, imagines, by what he has heard related, that at the following places, which are all the ſettlements he has ever heard of, there may be at each, upon an average, about thirty:

Cummucktobick,	30
Kidlenock,	30
Toogeat,	30
Congerbaw,	30
Ungabaw,	30
Ivevucktoke,	30
Igloo-ockſhook,	30
	1623

If this calculation comes any thing near the truth, the ESQUIMEAUX inhabitants of LABRADORE are far from being numerous; and thoſe ſavages who inhabit the inland parts are ſtill leſs populous.

XLI. *An*



Basire Sc.

XLI. *An Account of ſome new Experiments in Electricity, containing,* 1. *An Enquiry whether Vapour be a Conductor of Electricity.* 2. *Some Experiments, to aſcertain the Direction of the Electric Matter, in the Diſcharge of the* Leyden *Bottle: with a new Analyſis of the* Leyden *Bottle.* 3. *Experiments on the lateral Exploſion, in the Diſcharge of the* Leyden *Bottle.* 4. *The Deſcription, and Uſe, of a new Prime-conductor.* 5. *Miſcellaneous Experiments, made principally in the Years* 1771 *and* 1772. 6. *Experiments and Obſervations on the Electricity of Fogs, &c. in Purſuance of thoſe made by* Thomas Ronayne, *Eſq; with a Plan of an Electrical Journal, &c. By* William Henly, *F. R. S.*

SECTION FIRST.

An Enquiry whether VAPOUR be a CONDUCTOR of ELECTRICITY.

EXPERIMENT I.

Redde, May 5, 1774. I Inſulated a glaſs funnel [TAB. XIII. fig. 1.] into which the ſtreams, from a capillary tube, were directed by the electricity.

tricity. From this funnel, the electrified drops were received into a large insulated earthen dish; across which lay a long wire; and from its end hung a pair of light cork-balls. On working the machine (after about ninety or an hundred turns of the winch, and when fifty or sixty drops had fallen into the dish) *the balls separated*, and presently diverged, to the distance of half an inch. Then taking off the electricity, from all the bodies concerned, I blew the column of water out of the capillary tube, replaced it in the bucket, pointing towards the funnel as before, and worked the machine again, to try whether the electricity, issuing from the syphon, and passing through the air, might not electrify all the bodies, so as to separate the balls, without the jet of water; but no such event happened. I then replaced it, with the jet falling into the funnel as before; when it succeeded. I then tried it a second time, without the jet of water; and it failed. I thus repeated the experiment alternately, with, and without the jet, taking off the electricity of the apparatus carefully between the trials; till I was perfectly satisfied, that the jet of water, received into the funnel, and falling from thence into the insulated dish below, was the *medium* by which the balls, hanging from the end of the wire placed therein, became electrified. Hence I inferred, that vapour from boiling water, &c. must also be a conductor of electricity, though probably in a less degree, as being more dissipated. Having since repeated this experiment by receiving the electrified jet *immediately*

diately into a large inſulated diſh, I obſerved the effect to be much greater.

EXPERIMENT II.

Having procured a tin veſſel, ſomewhat reſembling an eolipile, or a chymical retort; I placed it over a ſmall lamp, upon my prime-conductor [TAB. XIII. fig. 2.], and filled it about half-full of boiling water. The noſe of it was ſo ſituated, as to throw the electrified drops into an inſulated diſh, furniſhed with balls, as in the former experiments. After the water had been ſome time poured in, and I imagined enough had evaporated to have produced ſome drops in the neck; I examined the lip, to ſee whether any deſcended, but ſaw none. However, on giving the machine a turn or two, I was very agreeably ſurprized to ſee the electric ſtreams iſſue exactly as from a capillary tube; and a few drops having fallen into the diſh, the balls became electrical, and were attracted by my finger, at the diſtance of an half or three quarters of an inch. In a few turns more of the globe, they ſeparated half an inch. I then threw out the water; and, clearing the veſſel of its vapour, I remounted it upon its ſtand (pointing towards the diſh as before), to try whether the ſharp edge on the lip of the veſſel would not electrify the air, ſufficiently to ſeparate the balls, as the evaporated water had done. I turned the winch a long time for this purpoſe; but the balls never diverged at all. I then poured in the boiling water a ſecond time; and,

and, when the drops began to fall, the fourth turn ſeparated the balls; and the tenth cauſed them to diverge to the diſtance of half an inch; and in this ſtate of repulſion they continued a conſiderable time, after I had ceaſed to work the machine. I then took off the electricity with my finger, and again cleared the veſſel of its water, &c. and, having replaced it with the point as before, I worked the machine again as uſual. The air was now grown in ſome meaſure electrical; for, at the ſeventh or eighth turn, the balls began to ſeparate, and in forty turns they were about three eighths of an inch diſtant from each other. I then ceaſed to turn the winch any longer; but had no ſooner ſtopped, than the balls began to cloſe, and in a very few ſeconds they were in contact; whereas, in the former experiment (when the electrified drops were in the diſh), on my ceaſing to turn the globe, they ſhewed no ſign at all of converging; and, I imagine, would have remained ſeparate a long time, if I had not taken off their electricity with my finger. I apprehend, therefore, from this experiment, *that the vapour of hot water is a conductor of electricity.*

EXPERIMENT III.

I hung on a ſtring, as near to the cieling of the room as I could, a pair of pith-balls, which, on working the machine a conſiderable time, diverged three quarters of an inch, but no wider. Then ſticking into the conductor a *ſmoking deal-match*, and working the machine again, they preſently

presently separated to the distance of two inches. The match, when placed in the same situation, and not smoking, had no such effect.

EXPERIMENT IV.

Having placed an earthen half-pint mug, upon a stand properly insulated; I fixed to a large ball of brass, which I had placed in the bottom of it; the end of a wire, six, or eight feet in length. The other end of the wire I connected with the prime conductor of a small electrical machine [TAB. XIII. fig. 3.]. Over this mug, and as near to ceiling of the room as I could, I suspended a pair of light cork balls. Then filling up the vessel with boiling water, I began to work the machine; and in fifty, or sixty turns of the winch, observed the balls to separate three eighths, or half an inch, from each other. I then took off the electricity of the bodies; emptied the vessel, and cleared it of the vapour; and having placed the apparatus in the same manner, I again worked the machine, for a longer time; but without effect. On replacing the boiling water, I succeeded as at first. At other times, when I have been able to separate the balls by the air alone, to a small distance; yet by pouring in the hot water, the vapour has presently increased their divergence from one eighth, or three sixteenths, to half an inch distance; or in that proportion, according to the state of the atmosphere with respect to dryness or moisture. In short I have repeated these kinds of experiments so often; and many times with so much success; that I can have no doubt of vapour being a conductor of electricity.

EXPE-

EXPERIMENT V.

I insulated the rubber of my machine, and hung a pair of Mr. CANTON's balls upon the prime-conductor. I then worked the machine, and having taken off a spark, or two, to draw off the electricity naturally inherent in the rubber, &c. I observed the divergence of the balls; which was very great; in so much that the strings were bent: and on approaching the back of the rubber with a *smoking green wax taper*, just blown out, (the smoak of which was instantly attracted to it,) they diverged no wider [(a)]. I then took off the balls, and placed my own electrometer in its stand, upon the prime-conductor [TAB. XIII. fig. 4]; and having taken off a spark, or two, as before; I again worked the machine, to observe the repellency of the index from the stem; and found it constantly to vibrate between five, and ten degrees, of the quadrant, which was divided into fifteen. I then brought the smoking taper, within four, or five inches, of the back of the rubber, as before; and observed, that on the attraction of the smoak to it, the index presently began to rise; and in a very short time, got up to right angles. I repeated the experiment, several times, with the same success. I then tried the experiment by bringing my finger to the same distance from the rubber, and pointing towards it; but this, in many trials, had not the least effect. The taper likewise, when held at the same distance, *and not smoking*, had no effect at all. I am

(*a*) For this experiment, the back of the insulated rubber should be perfectly smooth: mine, is of wood; with the leather pasted down closely to it; so as to leave no points.

convinced

convinced therefore, that *the ſmoak* was the *medium*, which conveyed the electricity, from my hand, to the inſulated rubber.

EXPERIMENT VI.

I placed upon a ſtand, on my prime-conductor, a piece of ſmoking wax taper [TAB. XIII. fig 5.], when immediately, on working the machine, the ſmoak, from a large, and diffuſed volume, was much contracted; and its motion upwards, greatly accelerated. I then took off the electricity of the conductor, and held a pair of cork balls a quarter of an inch diameter, hung on threads two and a quarter inches long, (being the neareſt at hand,) perpendicularly, over the riſing ſmoak; and as high as I could poſſibly reach, ſtanding on a chair; this might raiſe the balls about five feet and an half above the prime-conductor; when (working the machine) in a few ſeconds the balls ſeparated to half an inch diſtance. I then removed the taper, but could not perceive that the balls were at all affected without it; but on replacing it, they ſeparated as before. I repeated the experiment ſeveral times, with, and without the taper, and the different effect, was conſtantly as above recited. I then ſet a tin ſaucer upon the ſtand, and placed upon the ſaucer, an half pint mug of boiling water [TAB. XIII. fig. 6.); and over this water, I preſented the balls, in the riſing vapour; as I had before done in the ſmoak. On working the machine a few ſeconds, the balls diverged to the diſtance of one twelfth part of an inch. On removing the water, and preſenting the balls as before, they never ſeparated at all; though I worked the machine for a longer time; but on replacing the water, in a few ſeconds the balls diverged, as at firſt. Theſe experiments I repeated ſeveral times, and always with the ſame ſucceſs. The ſmoak, therefore, in the firſt experiment; and the vapour of the hot water, in this laſt; was certainly the *medium*, which conveyed the electricity, from the prime-conductor, to the balls: and I think I may now very ſafely pronounce, that SMOAK, *and the* VAPOUR *of hot water, are abſolutely conductors of electricity*; though ſmoak is a far better one than the vapour of hot water, and both of them are exceedingly bad ones.

Upon the queſtion, whether vapour be a conductor of electricity; I would obſerve, that Dr. FRANKLIN's curious experiment, of making a viſible atmoſphere, round an inſulated, electrified body; cannot be made, but in an exceedingly dry ſtate of the air. The reaſon is obvious; but in a very dry day, I have often ſucceeded in the experiment; and have ſhewed it to ſeveral of my friends, particularly Mr. MARSHAM, and Mr. NAIRNE.

My method of doing it, is as follows: I place *the braſs caſe* of a ſteel-yard weight (about two and an half inches diameter,) upon a clean, dry, ſtand of ſealing wax: then having in readineſs, *a green wax taper, with a long ſnuff*, I give the inſulated body, a ſpark, from the knob of a poſitively charged bottle; or take one from it, by the knob of a negatively charged one (the appearance is the ſame in both caſes); then bringing the taper, juſt blown out, very gently towards the inſulated body,

the

the ſmoak is inſtantly, and with a kind of violence, attracted to it; till it becomes compleatly covered with it. It remains in this ſituation for ſome ſeconds, when it begins to diſappear, at the bottom; and proceeds gradually, till it comes to the top; where it hangs quivering, like the departing flame of a lamp. It goes off in a long thin column, which rarefies, and diſperſes at the top, till it occupies a great ſpace. Obſerve that, in giving the ſpark, and bringing the taper towards the inſulated body, particular care muſt be taken, that the air be diſturbed as little as poſſible.

The *green wax taper*, on account of the verdegreaſe it contains, anſwers excellently, in this, and ſeveral other experiments, where ſmoak is required *without heat*: and I firſt uſed it, from a hint Dr. FRANKLIN was ſo obliging as to give me.

SECTION SECOND.

Of THE DIRECTION of the electric matter, in the diſcharge of the LEYDEN BOTTLE.

EXPERIMENT I.

LIGHT a ſmall wax taper, and place it, with the flame exactly between two braſs balls A and B, about two inches aſunder; properly introduced into the circuit [TAB. XIII. fig. 8.]. Then, having given a ſmall phial two or three turns of the globe, charging it poſitively, connect *the coating of it*, by a chain, with the wire of the ball A; and upon applying the knob of the phial, to the wire of the ball B, you will obſerve the flame to be plainly

driven

driven from it; being often blown upon the ball A, ſo as to blacken it with the ſmoak. Then charge the phial negatively, and (the apparatus remaining as before) apply the knob of the phial as at firſt; and you will then perceive the flame to be blown quite in the contrary direction, viz. from A *towards*, and *often upon* B, as upon Dr. FRANKLIN's principles of the Leyden bottle, it ought to be. Obſerve that, in this experiment it is neceſſary to uſe the leaſt charge that can be given, juſt ſufficient to leap the interruption in the circuit; which experience will preſently determine; for if the charge be too great, the flame will be attracted as well as repelled, in the diſcharge of the phial; and then, nothing can be infered from the experiment.

EXPERIMENT II.

Charge a large jar *poſitively*, and inſulate it; then take a long curved wire, *pointed, at both ends*, and hold it by a glaſs handle, ſo as to bring one end of the wire, half an inch, from the knob, and the other end of it, to the ſame diſtance, from the coating of the jar. You will then obſerve a ſmall luminous ſpark, upon the point appoſed to the knob of the jar, and a fine pencil, diverging from the lower point, ſpreading upon the coating of the jar, which will preſently diſcharge it ſilently. Then charge your jar *negatively*; inſulate it, and apply the wire as before; and the appearances, at the points of the wire, will be directly reverſed; plainly demonſtrating the direction of the electricity in the diſcharge of the bottle.

Another

Another very convenient and eaſy method, of exhibiting the phænomena of the poſitive and negative electricity of the inſide, and outſide ſurfaces, of a charged Leyden bottle, is by ſlipping a cap of metal, furniſhed with a ball and wire, upon the outſide coating; and mounting it upon an electric ſtand, in an horizontal poſition; as TAB. XIV. fig. 3.; or if the bottom of the glaſs be turned much upward into the body of it, a piece of wood may be worked to its ſhape, and cemented thereto; and through the middle of this wood, a ſhort tube of metal may be inſerted, ſo as to admit the wire which is connected with the ball to paſs through it; and be brought into contact with the coating of the jar, at pleaſure [(b)]. By this means, experiments may be made, at either end of the bottle with great facility; and other charged or exhauſted, bottles; excited ribbons; or other electrics: the curved pointed wire, &c. &c. may be readily applied; and give, or receive a ſpark; be attracted, or repelled; according to the kind of electricity in the two bodies, ſo applied towards each other. By hanging a chain round either of the wires, and connecting it with one end of the diſcharging rod; and bringing the other end of the rod, ſo as to leave a proper ſpace between *that* and the ball on the wire, at the oppoſite end of the bottle; the flame of a taper, &c. may be interpoſed; and ſhew the direction of the electricity in the diſcharge: or a cork-ball, *hung by ſilk*, may play between them, in the man-

(*b*) For many experiments, it needs only to be connected with the curved metal, or wood, in which the charged bottle is placed.

ner deſcribed by Dr. FRANKLIN. If the balls are taken off from the wires of the bottle; the wires being pointed, and one of them placed before the globe; or a prime-conductor, electrified *poſitively*: the phænomena of charging the Leyden bottle will be diſcovered by the different appearances, at the end of the wires; as at TAB. XIV. fig. 4. If the bottle be thus placed before a conductor, electrified *negatively*; or *the inſulated rubber* to a machine; the appearances, at the ends of the wires, will be reverſed: as upon Dr. FRANKLIN's principles they ought to be; and thus explain his theory of the Leyden phial.

But a more ſimple, and beautiful analyſis of the Leyden phial, hath not, I think, yet been exhibited; than the following. Let a bottle that will hold near a pint; having a long neck (about an inch in diameter,) be furniſhed with a ſmall plate at the top; with a valve properly ſecured, after the bottle is exhauſted: from which plate, a wire about one eighth of an inch in diameter, is to project a little below the neck; and terminate with a blunt end. The top is to be covered with a round braſs cap, firmly fixed thereon; and made air-tight. The bottom of the bottle ſhould be coated with tin-foil, which ſhould be continued *three inches up the ſide*. This bottle will charge and diſcharge ſeveral times in a minute; and the tin-foil coating, will prevent the ſhock from affecting the hand of the operator [c]. The phænomena of charging the Leyden bottle, is elegantly explained by this contrivance; and is made viſible, by the end of the wire; on which, the appearances vary, according as the bottle is charged, viz. poſitively,

(*c*) The bottle being held below the edge of the coating.

or

or negatively; or as the conductor, from which it charged, is electrified. Tab. xiv. fig. 5. letter A. shews such a bottle, charging *negatively*, at a conductor, loaded with *positive electricity*. Letter B. shews the same bottle, charging *positively*, at the same conductor.

Tab. xiv. fig. 6. letter C. shews the bottle charging *positively* at a conductor, *electrified negatively*; or at the insulated rubber. Letter D. shews the same bottle, charging *negatively* at the same conductor.

SECTION THIRD.

Of the LATERAL EXPLOSION in the discharge of the LEYDEN BOTTLE.

EXPERIMENT I.

HAVING made a double circuit, the first by an iron bar, one inch and an half in diameter, and half an inch thick; the second, by four feet and an half of small chain; on discharging a jar, containing five hundred square inches of coated surface, the electricity passed in both circuits, sparks being visible on the small chain in many places. On making the discharge of three jars, containing together sixteen square feet of coated surface, through three different chains *at the same time*, TAB. XIII. fig. 7. bright sparks were visible in them all; and I have not the least doubt, but it would have been visible in as many more. The chains were of iron and brass, of very different lengths; the shortest ten, or twelve inches, the longeſt, many feet, in length. When thoſe jars were diſcharged through the iron bar before-mentioned, together with a ſmall chain, three quarters of a yard in length; the whole chain was illumined, and covered throughout with beautiful rays, like briſtles, or golden hair. Having placed a large jar in contact with my prime conductor, I affixed to the coating of it an iron chain, which I alſo connected with a plate of metal, on which I intended to make the diſcharge by my diſcharging-rod, TAB. XIII. fig. 9. This done, I hooked another chain, much longer, and of braſs, to the oppoſite ſide of the jar, and brought the end of it within eight inches and an half of the metal plate. In contact with this end, I laid a ſmall oak-ſtick, eight inches long, which I covered with ſaw-duſt of fir-wood. On making the diſcharge upon the plate, *both the chains* were luminous through their whole lengths; as was alſo the ſaw-duſt, which was covered by a ſtreak of light, making a very pleaſing appearance. I repeated the experiment ſeveral times. Perhaps, if I had uſed a bar of iron, inſtead of the chain firſt-ſpoken of, there might have been no light upon the ſecond chain, or upon the ſaw-duſt, eſpecially as the electricity had half an inch of air to paſs through, before it reached the end of the ſtick. But from this experiment may, I think, be inferred, the neceſſity of making the conductors, erected as a ſecurity to buildings, &c. from the damage of lightning, both of the beſt materials, and of a very ſufficient ſubſtance; and, for this purpoſe, perhaps nothing will be found ſo proper as

lead,

lead, which will remain in the earth many centuries without any conſiderable decay; and the tops of chimneys being covered with it [(d)], and furniſhed with a long, ſharp-pointed rod of copper, or iron pointed with copper, which I think ſhould extend at leaſt five, or ſix feet, above the top of the chimney, or higheſt part of the building; a communication ſhould be made from it by plates of lead, eight, or ten inches broad, with the lead, on the ridges, and gutters, and with the pipes which carry down the rain-water; which pipes ſhould be continued to the bottom of the building, and there made to communicate, by means of other leaden pipe, or a plate of it, as before-mentioned, with the water in a well, the moiſt earth, or the main pipe which ſerves the houſe with water.

SECTION FOURTH.

Deſcription and Uſe of a new PRIME-CONDUCTOR. Contrived by Mr. HENLY, and executed by Mr. EDWARD NAIRNE.

A. TAB. XIV. fig. 7. A glaſs-tube, eighteen inches long, and near two inches in diameter.

B. C. Balls of Braſs, with a ferule, two inches long, to each of them; which ferules are to be cemented to the ends of the tube, and made air-tight.

(*d*) I mention covering the tops of chimneys with lead, as a protection to the upper courſes of bricks, from the effects of wind; and not as being of any eſſential ſervice to the conductor, any farther than as it may aſſiſt in fixing the pointed rod, which is to be elevated above it, more ſecurely.

 One

One of the braſs plates, which are ſoldered to the ferules, hath a ſmall hole drilled through it, by which the air is to be exhauſted. It is covered by a ſtrong valve, properly ſecured, and concealed by the braſs ball B, or C.

D. E. Balls of braſs, about five eighths of an inch in diameter, fixed upon wires, which project two inches and an half from the braſs plates, at each end of the glaſs-tube.

F. A fine-pointed wire, to collect the electricity from the excited glaſs-globe, &c.

G. Supporters, of ſealing-wax; upon which the luminous conductor is to be mounted.

N. B. The dots in the tube are intended to repreſent the appearance of the electricity in it, in the experiments deſcribed in TAB. XIV. But, when a bottle, or a large jar, is diſcharged through the glaſs-conductor, it is uniformly filled with light.

The USE of the GLASS CONDUCTOR.

The glaſs-tube, thus furniſhed, and mounted, *being properly exhauſted, and perfectly dry*, will act in all reſpects like one of metal; and the electrometer, being placed upon the braſs ball B. will anſwer to the charge of a jar, or battery, exactly. But the principal uſe of this inſtrument, is *to aſcertain the direction of the electric matter, as it paſſes through it*. And this end, it completely anſwers in the manner following, viz. ſet it with the collecting-point F. before the globe, and place the knob

knob of an uncharged bottle nearly in contact with the braſs ball B. or hang a chain, &c. from thence to the table; and, on working the machine, the ball D. in the tube becomes entirely enveloped, in a denſe, white atmoſphere of electricity. If the point F. be brought nearly into contact with an inſulated rubber, and a communication be made from the ball B. to the table; the atmoſphere will be upon the ball E. in the tube. If a bottle, poſitively charged, be preſented as in the drawing TAB. XIV. fig. 9. the appearances in the tube will be as therein delineated. But, if a bottle, charged negatively, be thus applied, the atmoſphere will ſurround the ball E. in the tube, as in TAB. XIV. fig. 10.

CONJECTURES on theſe Phænomena.

It is ſuppoſed, that the impelling power of the globe, or the knob of a *poſitively charged bottle*, drives the particles of electricity *through the ſubſtance of the balls*, wire, &c. with which they are in contact, with great velocity, and in a kind of ſtraight line; but, the electricity having entered the *vacuum*, the repulſion of its particles immediately takes place, and the tube is inſtantly filled with light. The denſe white atmoſphere upon the oppoſite ball is ſuppoſed to proceed from the reſiſtance of the air in the tube; a ſmall portion of which, will, in this method of exhauſtion, inevitably remain in it. And, as every particle of electricity, is ſuppoſed to be in a ſtate of repulſion with reſpect to its next neighbour, the *vacuum* giving them free liberty of expanding themſelves,

or

or ſtanding at the greateſt diſtance from each other; it is imagined, that they actually do ſo, and will not enter the ball, and wire, at the oppoſite end of the conductor, *in a point, or ſmall ſpace*, (as they do in the open air); but that they entirely ſurround them, and enter *at all parts at the ſame time*, in order to their conveyance into thoſe bodies placed at the end of the braſs work to receive them.

If, inſtead of the braſs *balls* in the tube, *points* are uſed; or if *a point* be fixed at one end of the tube, and *a ball* at the other; the effect will be preciſely the ſame.——Note alſo, That the glaſs-conductor, for the purpoſe of making Dr. FRANKLIN's curious experiments, with a pointed and blunted wire, is far ſuperior to one of metal, the electric atmoſphere being ſo much better retained by it. By this eaſy and ſimple proceſs, may an ocular demonſtration, at all times, be given, in a dark room, and dry air, of the truth and propriety of Dr. FRANKLIN's hypotheſis of the Leyden bottle.

SECTION FIFTH.

Miſcellaneous Experiments, made principally in the Years 1771 and 1772.

EXPERIMENT I.

IF a black ſilk ribband, or a piece of black ſilk, be laid on a quire of paper, &c. on a table, and excited by drawing over its ſurface ſealing-wax, ſulphur,

ſulphur, amber, or a tube of glaſs with *the poliſh taken off by emery*; its electricity will be *poſitive*: whereas, if it be excited, ſingly, or together with a white ribband, by drawing them briſkly between the fingers, it is always *negative*.——Laying it on the paper, and drawing over its ſurface a rod, or tube of ſmooth glaſs, its electricity will alſo be negative.

EXPERIMENT II.

If a plate of glaſs, ten, or twelve inches in diameter, be excited, and placed upon the top of a box, from which a pair of light pith or cork-balls are ſuſpended, being mounted on a ſtand of ſealing-wax; the balls will ſeparate, and ſtand repelled from each other, being electrified poſitively, (in a dry air), upwards of four hours. When they come into contact, on removing the glaſs, they diverge again, and are negatively electrified; but, on replacing it, they cloſe. On removing it again, they ſeparate; and thus alternately as long as any electricity remains in it [(e)].

If the plate of glaſs be placed in a frame of wood, and a light pith or cork-ball be laid on its ſurface; on preſenting towards it the end of a finger, or the point of a pin, &c. the ball will recede from them, with a very briſk motion, and may thus be driven about upon the ſurface of the glaſs, like a feather in the air, by an excited tube, or the wire of a charged bottle. The cork-ball,

(*e*) For an explanation of theſe phænomena, ſee Mr. CANTON's experiments, Philoſophical Tranſactions, Vol. LVIII. Part I. N° 53.

being

being deprived of its electricity by the pin, &c. inſtantly flies to that part of the glaſs to which it is attracted the moſt forcibly.

EXPERIMENT III.

I hung on my prime-conductor a ſmall phial, two inches in diameter, coated three inches and a quarter from the bottom. From the coating of this phial, I ſuſpended two chains; the firſt, in contact with a heavy weight, placed upon a card, acroſs which, I had ruled lines, at equal diſtances, TAB. XIV. fig. I. the ſecond chain formed a circuit, with leaden pipe, ſmall braſs wire, ſmall chain, &c. of one hundred and twenty feet in length. From the ball of my diſcharging rod, which reſted on another weight (ſee the figure), I alſo hung a chain, in contact with, and compleating, the circuit of one hundred and twenty feet before-mentioned; and obſerved, that, if my bottle was charged quite full, the electricity would, in the diſcharge, *paſs through the long circuit*, rather than over the ſurface of the card, when the weights were placed at nine ſixteenths of an inch aſunder: but, if I charged the bottle only about half-full, the electricity would, in the diſcharge, paſs through the long circuit, rather than over the ſurface of the card, though the weights were placed at the diſtance of only *three ſixteenths* of an inch.— Query, Can there be a greater proof of the ſmall reſiſtance made by *metal* to the paſſing of the electric matter, compared with card, wood, &c. and conſequently of the utility

lity of metallic conductors to buildings, ſhips, &c.? The ſame obſervation hath been repeatedly made, upon the effects of the natural electricity. And a remarkable inſtance hath lately happened, at the church of St. John, Weſtminſter; a very exact account of which hath been taken, by Dr. WATSON, F.R.S. and J. BANKS, ESQ. F.R S. who, I hope, will at a convenient opportunity, favour the curious in theſe matters with their ingenious and intereſting remarks on it. The tower of this church, I am informed, hath no *pointed* metallic termination.

EXPERIMENT IV.

Having prepared a phial, in the manner directed by Mr. LANE, for making his curious experiment; by paſſing a wire through the bottom, and another through the cork, ſo as to bring the ends of the two, within half an inch of each other, about the middle of the bottle (which was filled with water) I found, as that gentleman obſerved, that a ſlight ſhock of electricity diſcharged through it, would break the bottle. But having put a very ſmall wire from the top, to the bottom of it, *through the water*; I diſcharged through it, three large jars, containing ſixteen ſquare feet of coated ſurface, when the whole of the ſmall wire was exploded; but the bottle remained unhurt. If therefore a metallic conductor (being too ſmall) ſhould happen to be deſtroyed by a ſtroke of lightning, yet the building, &c. to which it is affixed, will probably eſcape uninjured.

EXPERIMENT V.

When I ſtrongly electrify a large prime-conductor, three feet long, and twelve inches in diameter; if a perſon hold in his hand a braſs rod terminated by a ball, two inches in diameter, at the

 diſtance

diſtance of two inches, from the ſide of the conductor [TAB. XIV. fig. 2.], he will continue to draw ſuch ſtrong ſparks as will give him a ſenſible ſhock in both his legs: but if another perſon at the ſame time preſent the point of a lancet, or a wire five or ſix inches long, nicely tapered to a point, tipped with ſteel, towards the conductor; though at the diſtance of two feet, or ſomewhat more, this will draw off all its electricity *ſilently*; and not ſuffer a ſpark to paſs from thence to the braſs ball: it is alſo obſervable, that if the point of the wire, or lancet, be brought nearly into contact with the prime-conductor, yet no ſenſation is felt in the arm, &c. of the operator: hence, I think, appears clearly the preference due to *points*, rather than round balls, or blunted ends, for the termination of the conductors erected as a ſecurity to buildings, &c. from damage by lightning: for to me, it ſeems probable, that the ſharp point of the conductor will act upon the electric atmoſphere of the cloud, and perhaps gradually and ſilently continue to diminiſh the contents, before the cloud can approach near enough to ſtrike; and thus contribute to leſſen, if not actually prevent, a ſtroke (f). But ſhould the point be ſtruck, the conſequence I ſuppoſe will not be great, and a curious inſtance I have now before me, which I ſhall beg leave to quote as follows. " About nine o'clock we had a " dreadful ſtorm of thunder, lightning, and rain, " during which the main-maſt of one of the Dutch " Eaſt Indiamen was ſplit, and carried away by " the deck (g); the maintop-maſt and top gallant-

(*f*) Captain WINN's conductor, though two of the links were broken, effectually anſwered this intention.

(*g*) The ſtays, ſhrouds, &c. being all cut aſunder (Dr. SOLANDER informed me) as with a knife.

maſt,

" maſt, were ſhivered all to-pieces; ſhe had an
" iron ſpindle at the main top-gallant-maſt-head,
" which probably directed the ſtroke. This ſhip
" lay not more than the diſtance of two cables
" length from ours, and in all probability we ſhould
" have ſhared the ſame fate, but for the electrical
" chain which we had but juſt got up, and which
" conducted the lightning over the ſide of the ſhip;
" but though we eſcaped the lightning, the exploſion ſhook us like an earthquake, the chain at
" the ſame time appearing like a line of fire (b): a
" centinel was in the action of charging his piece,
" and the ſhock forced the muſket out of his hand,
" and broke the rammer rod. Upon this occaſion
" I cannot but earneſtly recommend chains of the
" ſame kind to every ſhip, whatever be her deſtination; and I hope that the fate of the Dutchman will be a warning to all who ſhall read
" this narrative, againſt having an iron ſpindle at the maſt-head."—— See Capt. Cook's voyage. This conductor was of copper wire, three ſixteenths of an inch in diameter; which I am inclined to think is rather too ſmall for the purpoſe; I am of opinion it ought to be a quarter of an inch at leaſt: and I have been informed by Dr. SOLANDER, that the point originally belonging to the conductor, had been ſtolen; and that *this*, on which the lightning fell, was of inferior workmanſhip, and not ſo ſharp; which was another great diſadvantage: perhaps if the wire of the chain had been larger, and

(b) It does not however appear, that the ſmall *hempen cord*, with which the links of the chain are cloſely connected, or even the ſtrings which connected them, had received the leaſt injury from the lightning.

 the

the point more acute, the ſtroke would have been much leſſened; if not abſolutely prevented. If, inſtead of thoſe chains, *plates* of copper, three ſixteenths of an inch thick, and two inches in diameter, with the edges neatly rounded off, were inſerted in a groove, and continued down the main-top-gallant-maſt, the main-top-maſt, and part of the main-maſt, into the well-hole; a communication from the maſt, to the under-ſide of one of the decks, might be made with a plate, or rod of metal, flattened at each end; and from that rod, the conductor might be continued by plates of lead, or copper, on the under-ſide of the deck, and down both the outer-ſides of the ſhip, as low as the keel, if it be thought neceſſary: and this method (the conductor being always in readineſs, and kept perfectly in order) I ſhould apprehend would be preferable to the chains, which are now in uſe. Particular care ſhould be taken, to have all the plates, which form the conductor, as nearly as poſſible in contact with each other, and to fix a ſharp-pointed, ſlender rod of copper at its ſummit. And for the purpoſe of connecting the plates, inſerted in the main-top-gallant-maſt, the main-top-maſt, and the main-maſt; if an hoop of copper were fixed in a groove of its own thickneſs, at the top of the main-maſt; and another ſuch hoop at the upper end of the main-top-paſt; perhaps they might anſwer this end very conveniently (i). The learned and ingenious Dr. WATSON, F. R. S. hath, with

(i) If an objection ſhould be made to cutting grooves in the maſts, the plates of metal, which form the conductor, might readily be faſtened upon the ſurface, and very ſecurely.

great

great judgement and accuracy, collected from ancient hiſtory, the accounts therein recited, of electrical appearances, upon pointed bodies; as the ſpears of ſoldiers, &c. &c. [k] which have been very judiciouſly introduced by Dr. PRIESTLEY into his Hiſtory of Electricity [l]: and I cannot but think, thoſe accounts, furniſh a very ſtrong argument, in favour of *pointed conductors*: for had the bodies here ſpoken of been terminated by blunted ends, or round knobs, it is probable that many of them inſtead of drawing off the lightning *ſilently*, would have been *ſtruck* with it; and this, being deemed a common occurrence, would have paſſed unnoticed, and conſequently never have been recorded in hiſtory.

If pointed bodies had really the property of drawing down ſtrokes of lightning upon themſelves, I think the pillar upon Fiſh-ſtreet Hill, commonly called the MONUMENT, could not long have eſcaped. This pillar is terminated by a baſin of metal, four feet and ſix inches in diameter. The baſin is ſurrounded by a great number of bended plates of metal, *ſharply pointed*, to repreſent flames of fire. From the baſin, to the floor of the gallery, are fixed perpendicularly in a circular order four thick bars of iron; and in thoſe bars are inſerted twenty-eight ſtrong hoops, and four ſegments of circles, of the ſame metal; which ſerve as ſteps from the gallery to the baſin. One of theſe bars (being one inch thick, and five inches broad) is connected with the iron rails of the

(*k*) See Philoſophical Tranſactions, vol. 48, part I. p. 210.

(*l*) See Hiſtory and Preſent State of Electricity, ſecond edition, p. 371.

ſtair-caſe,

ſtair-caſe, which reaches to the bottom of the building, and forms a ſubſtantial, regular conductor of metal the whole length. The monument was erected by Sir CHRISTOPHER WREN in remembrance of the fire of London, which happened in the year 1666. It was compleated by that great architect, in the year 1677; is, including the blazing urn at its ſummit, about two hundred and two feet in height, from the pavement; and hath never (ſo far as I have been able to learn) been ſtruck by lightning. The *antennæ* and legs of the graſshopper on the Royal Exchange in Cornhill; and the tongue and tail of the dragon on the ſpire of Bow church in Cheapſide, London, are alſo remarkable inſtances [m]: indeed I have often thought it rather a favourable circumſtance, that moſt of the lofty public buildings in this metropolis which have metallic terminations, have generally been furniſhed with weather-fanes, which fanes commonly end in ſharp points: for had they been terminated with large round balls of metal, perhaps many more of them might long ſince have been demoliſhed. Here therefore I cannot but expreſs my earneſt wiſhes, that, on all future occaſions, where lofty public edifices are to be erected; a good pointed conductor for the lightning, may be conſidered by every architect, or ſurveyor, as an eſſential part of the edifice itſelf.

EXPERIMENT VI.

I attempted to aſcertain the conducting power, of different metals, in the manner following. I

(*m*) A great variety might be produced, but Dr. FRANKLIN hath himſelf rendered this unneceſſary.

took

took a thick piece of paſte-board, acroſs which I ruled lines, exactly an inch aſunder. Upon theſe lines, croſs-wiſe, I placed the wires; which I confined by heavy weights: the edges of which weights juſt touched the ruled lines; leaving exactly an inch of wire between them [ſee TAB. XIV. fig. 1.]. The kinds I tried were, *pure gold*, *ſilver*, *braſs*, *copper ſilvered*, and *iron*. They were all drawn through the ſame hole, except the iron, which was ſomewhat larger than the others. I proved them with two jars, containing eleven ſquare feet of coated ſurface; and adjuſted the charges, by an electrometer graduated in diviſions of one tenth of an inch each, the diameter of the ſcale being two inches. The reſult was as follows:

Pure Gold	was melted at	4	Diviſions.
Braſs		6	
Copper ſilvered		8	
Pure Silver		10	
Iron		10	

If I gave either of the wires a diviſion leſs than the number above ſpecified, it was not melted: if I gave either of them a diviſion more, it was *exploded*; the greater part vaniſhing in ſmoak: whereas theſe charges juſt burſt them into balls.

Should any gentleman chooſe to repeat this experiment, I would recommend it to him to be very particular in ſizing the wires; *to uſe a greater length*, to do it when they are all freſh drawn, to make the experiment in a very dry day, and in a room where there is no fire. With theſe precautions, probably there may be ſome difference in the reſult; and this method will perhaps give a true account, of the conducting power, of the different kinds of metal.

Having lately been presented, in the most polite manner, by the celebrated Dr. LEWIS, F. R. S. &c. with six specimens of his *platina*; in as many different states: I selected the largest grains, from one of the parcels which Dr. LEWIS informed me had been repeatedly exposed to long-continued vehement fires; the most intense which he had been able to excite, or any vessels he could procure would support: and after a few small globules (consisting doubtless in great part of heterogeneous metal) had melted out, repetitions of the operation produced no further change. It was afterward boiled successively in oil of vitriol, aqua-fortis, and spirit of salt, in order to its further purification; and which indeed reduced it to a state the most pure of any that excellent chemist had been able to produce. Having ruled a line with a blunt-ended wire, over the surface of a plate of white wax;

EXPERIMENT VII.

I pressed in the grains of *platina* lightly, and in contact with each other; so as to form a regular line, half an inch long. At each end of the line of *platina*, and in contact with it, I placed a thick wire, with its ends nicely rounded off, and made perfectly smooth. I covered the *platina* with a piece of thick plate-glass; and then discharged through it, three jars containing sixteen square feet of coated surface: when I obtained many beautiful spherules of the *platina*. Several of them stuck to the wax, and glass; and others *imperfectly formed*, upon the edges, &c. of the grains: which proved

proved that the fusion had been compleat. This experiment I made in the presence of Mr. Ferguson, F. R. S. Mr. William Canton, Mr. Bell, and Mr. Marsham, who all acknowledged it perfectly satisfactory. Having mentioned the result of this experiment, and the method of making it, to Mr. NAIRNE; he hath since repeated it with equal success.

Being informed, by Mr. William Canton, that his brother, Mr. Thomas Canton, had, in preparing *a dried cork* for an experiment in electricity, observed some appearances which induced him to believe, that the cork had been made electrical, by only cutting it with a penknife, and that on examination he found it really was so; I made the following experiment.

EXPERIMENT VIII.

I made a long cork perfectly dry, and held one end of it very near the fire, till it began to burn. At the same time, I held a small, fine-toothed file, in the clear part of the fire, till that also, had become very dry, and rather hot. Then, having filed off the end of the cork, I applied it to a pair of neat, light pith-balls; when it attracted them both, and raised them perpendicularly, as high as the strings would permit. Having electrified the balls by excited amber, the cork would increase their divergence from one, to near two inches; or it would repel them at an inch distance, so as to drive them one inch and an half out of the perpendicular. Electrifying the balls by excited glass, these appearances were directly reversed. The cork therefore had parted with its electricity

 to

to the file, and plainly acted as a negative electric.

EXPERIMENT IX.

Having neatly rounded off the corners of a piece of thin talc, about three inches square; I coated both the sides of it, within three quarters of an inch of the edges, with tin-foil, which I also rounded off at the corners. The talc, thus prepared, I observed would readily charge, without wiping, or drying the uncoated part; and the force of the shock, in the discharge, was really astonishing.

Having been shewn, by my late truly ingenious friend Mr. CANTON, an electric spark, of a very beautiful *crimson colour*, which always appeared as it was drawn *over*, or *through*, a piece of smooth wood, *at the top of the conductor-stand*, and which was supposed by some gentlemen, to be the light of electricity, *very thinly spread* upon the surface of the wood; I was exceedingly desirous to know from what cause this phænomenon really proceeded; and for that purpose made the following experiment.

EXPERIMENT X.

I fixed between two balls, introduced into the circuit of an electric discharge, a piece of smooth wainscot, about two inches in diameter, and a quarter of an inch thick; when, upon making the discharge of a pretty large jar, I observed the wainscot to be nearly covered with the electric light, the outer parts, or edges of the light, were exceedingly thin, but the colour *very white*; as it was also in several

3 other

other experiments, made with the ſame intent. I then procured a circular piece of coloured box, which was glued to the top of the ſtand to my prime-conductor; when, drawing ſtrong ſparks *through this wood* (of whatever colour it was), I became clearly of opinion, that *the colour of the ſpark varied according to its depth in the wood*; viz. if it paſſed upon the ſurface, it was white; a little below it, yellow, or orange; ſtill lower, ſcarlet; and, deeper in the wood, crimſon.

It having been mentioned, by ſome gentlemen, as their opinion, that the matter of light, and the electric matter, were the ſame thing(n); I made the following experiment, in order to determine whether there was any foundation for ſuch an opinion or not.

EXPERIMENT XI.

I inſulated the rubber of my machine, and placed it in ſuch a ſituation, that the rays of the ſun, paſſing through the open window of my room, might fall immediately upon it; but this I obſerevd produced no electricity. I then collected the rays into a *focus*, by means of a good convex glaſs, and threw them upon the back of the rubber, till it was burned quite black; but this method was attended with no better ſucceſs. I then mounted one of Mr. CANTON's electrometers, furniſhed with very light balls, upon a ſtand of ſealing-wax; and, having electrified them negatively, by excited amber, ſo as to diverge a full inch, I again

(n) Query, If this were really the caſe, ſhould not electrical experiments ſucceed, in the moſt perfect manner, *in the cleareſt light of the ſun?* and would not the evening, or night, be an exceedingly improper time to attempt making them?

collected the rays of the ſun by the convex glaſs, and held it at ſuch a diſtance as to bring the *focus* exactly upon the end of the box, which was burnt very black, and the glue in the joints melted; but the balls were not in the leaſt affected.

Extract from Mr, BOYLE's Continuation of HAWKESBY's Phyſico-mechanical Experiments.

" I took a large piece of good amber; and, " having in a ſummer-morning (while the air was " yet freſh) tried that it would not, without being " excited, attract a light body I had expoſed to it, " I removed it into the ſun's beams, till they had " made it moderately hot; and then I found, as I " expected, that it had acquired an attractive " virtue, and that not only in one particular " place, as it is uſually obſerved when it is excited by rubbing, but in divers and diſtant " places at once; at any of which it would draw " to it the light body placed within a convenient " diſtance from it; ſo that, in this climate of " ours, a ſolid body may quickly acquire an at-" moſphere by the preſence of the ſun, and that " long before the warmeſt part of the day."

ANOTHER.

" I took a little, but thick, veſſel of glaſs, and " held it near the fire till it had got a *convenient* " *degree of heat*, which was not very great, " though it exceeded that of the amber. I found, " as I imagined, that the heat of fire had made

" even

"even this body attractive, as that of the sun "had made the amber."

REMARK.

If Mr. BOYLE, when the amber was heated, pressed it ever so lightly against his hand, in order to try its warmth, (though without the least friction), *he excited it*; and, *without this*, it was not electrical, *neither would it become so in cooling*. If the amber was *too hot*, the heated air about it would *conduct*. Therefore he was obliged to find *a convenient degree of heat*. This assertion may be proved in the following manner.

EXPERIMENT XII.

Hold a piece of amber near the flame of a candle, till it becomes hot; then apply it to a suspended thread, and *it will not attract it*, neither will it become electrical *in cooling*; but press it ever so lightly on your hand, in order to try its heat, though without the least friction, and *(if it be not too hot)* it will be electrical, and attract it violently. Heat it again at the candle, and its electricity shall be taken quite away. Press it again gently on your finger, or hand, and the power will be restored. Apply it again to the candle, it is lost. And thus alternately. Other electrics may probably act in the same manner; as *the flame of a candle*, or *hot air*, will conduct away the electricity of glass, almost instantaneously.

EXPERIMENT XIII.

Shewing Mr. NAIRNE the above-mentioned experiments; when the amber had been well-heated, and being preſented to a ſuſpended thread, having ſhewn no ſign at all of electricity; I held it, between my thumb and fore-finger, very near the table, but not ſo as to touch it, that we might entirely avoid friction. He then blew againſt it thirty blaſts, with a pair of kitchen-bellows; when preſenting it to the thread, it attracted it, at the diſtance of one-eighth of an inch. He then blew againſt it, thirty blaſts more, as above deſcribed; when applying it again to the thread, we ſaw it attracted, at half an inch diſtance; and, on drawing back the amber, it drew the thread after it, ſix, or eight inches. We repeated the experiment three times, with the like ſucceſs; and are ſatisfied, that the amber was made electrical *by the friction of the particles of air againſt its ſurface*; *and not in the leaſt by heating only.* We afterwards excited the amber, when it muſt have been perfectly cold, but dry, by only blowing againſt it as before.

The ſame proceſs ſucceeds with glaſs.

SECTION SIXTH.

Experiments and Obſervations on the Electricity of FOGS, &c. in purſuance of thoſe made by THOMAS RONAYNE, Eſq;

1771, Nov. 14. Half paſt eight, A. M. I find a fog, not very thick, pretty ſtrongly electrified. The balls ſeparate full half an inch. They keep ſtationary, there being little or no wind.

Nov. 19.

Nov. 19. The air is pretty ſtrongly electrified; but the wind is ſo very troubleſome, that I cannot determine the kind with certainty.

Dec. 2. Half paſt eight, A. M. a fog, moderately thick, is ſtrongly electrified. The balls diverge half an inch; but, if they are brought near the building, they cloſe, and open again on removing them. The mercury in the thermometer, is fifteen degrees, above the freezing point.

Dec. 18. Half paſt four, P. M. a moderately thick fog is ſtrongly electrified, ſoon after its appearance. The balls diverge, full half an inch, and regularly cloſe, at the approach of excited wax. The wind is troubleſome; but the balls keep their diſtance, and at intervals very well admit trying the experiment.

1772, Jan. 5. A fog is ſtrongly electrified poſitively. The balls diverge full half an inch. The air is ſharp, and froſty.

Jan. 13. Nine o'clock, A. M. a fog, not very thick, is ſtrongly electrified poſitively. The mercury in the thermometer, is ſeven degrees and an half above the freezing point. There is little or no wind.

Jan. 18. Ten o'clock, A. M. The air is pretty ſtrongly electrified by a fall of ſnow.

Jan. 21. Nine o'clock, A. M. I find the air ſtrongly electrified, during a fall of thin ſleet, a mixture, of ſnow, and rain, very gentle. The balls ſeparate three quarters of an inch, and remain ſtationary, there being little or no wind. N. B. The electricity in the air is poſitive.

Jan. 29.

Jan. 29. Nine o'clock, A. M. a very thick fog, and ſharp froſt. The air is ſo ſtrongly electrified *poſitively*, that the balls ſeparate *full an inch and a quarter*. There is little or no wind, and they remain ſtationary; ſo that the experiment may be made without the leaſt danger of a miſtake.

Twelve o'clock, the balls diverge as wide as at nine.

Three o'clock, P. M. the balls are exceedingly diſturbed by the wind; but, blow as it will, they ſtill keep at a great diſtance from each other. It freezes very hard. A quarter paſt four: the ſame as at three o'clock.

Half paſt five. The balls are ſtationary, *at three quarters of an inch diſtance, from each other*. The fog increaſes; and the rods are perfectly wet, from end to end. It is now too dark for further accurate obſervation.

Jan. 30. Nine o'clock, A. M. I find the air ſtrongly electrified *poſitively*, in a ſlight froſt, and thick fog. The balls ſeparate full half an inch: they are diſturbed by the wind; but it does not cloſe them; and the experiment is tried eaſily. There hath been a ſmall ſhower of ſnow, which lies thinly ſpread upon the houſes; and I have often ſuſpected (as I do now), that *this forms points*, and conducts the electricity faſter. The electricity continued the whole day.

Feb. 4. Nine o'clock, A. M. A ſharp froſt, and thick fog. The air is ſtrongly electrified *poſitively*: the balls diverge full three quarters of an inch. Eleven o'clock, A. M. The balls are ſtationary,

ſtationary, *at upwards of an inch diſtance.* They cloſe at the approach of excited wax.

Half paſt two, P. M. The ſame as at eleven o'clock. Three o'clock. Over-caſt and cloudy: the balls are very ſtill, but ſhew ſcarce any ſign of electricity.

Feb. 11. Eight o'clock, A. M. A thick fog is ſenſibly electrified *poſitively.* The balls diverge a quarter, or three eighths, of an inch. Wind S. W. and troubleſome. Thermometer 38. Barometer, 29,94.

Feb. 15. Half paſt ten, A. M. I find a thick fog, ſenſibly electrical. The balls diverge five-eighths of an inch, full. Preſently, after I had fixed my rod, there fell ſome ſmall drops of rain. *Upon the moment of its falling*, the balls increaſed their divergence near, or quite, a quarter of an inch. I never ſaw a fog more ſtrongly electrified when the weather was ſo warm, the mercury in my thermometer, in the open air, being ſeven and an half degrees above the freezing point. I ſuppoſe *higher in the atmoſphere* it is now uncommonly cold.

Fifty minutes paſt two, P. M. It ſnows very faſt. The air is now ſtrongly electrified *poſitively.* The balls ſeparate full three quarters of an inch. Wind S. W.

From the ſmall number of experiments I have been able to make, on the electricity of the atmoſphere, I cannot help being of opinion, that fogs are much more ſtrongly electrified *in*, or *immediately after, a froſt*, than at other times; and that the electricity in the fogs is often the ſtrongeſt,

 ſoon

ſoon after their appearance. I alſo now hold it for a certain rule, that, whenever there appears a thick fog, *and the air is at the ſame time ſharp and froſty*, that fog, is ſtrongly electrified *poſitively*. Though rain may not be *an immediate*, yet I am inclined to think it is by no means a very remote conſequence of electricity in the atmoſphere; and, from the trifling obſervations I have had an opportunity to make on that ſubject, I have not failed to find, that, in two or three days, after I have diſcovered the air to be ſtrongly electrified, (eſpecially if that electricity continued for as long a time), we have had rain, or other falling weather, and I incline to believe, more plentifully, in proportion to the ſtrength, and continuance, of the electricity; if not rain, ſnow, &c. according to the ſtate of the atmoſphere, with reſpect to heat and cold. If electricity be not a cauſe, I think it at leaſt *a prognoſtic*, of falling weather. But, for further ſatisfaction in this particular, I would recommend it to any gentleman curious in theſe enquiries, and having leiſure, to keep an electrical journal, upon a plan of the following kind.

Let a large book be provided, and ruled in the manner of a bill-book, uſed by tradeſmen. The columns ſo ruled may contain a collection of obſervations in the following order: Date and day; hour, latitude and longitude, or place; divergence of the balls; kind of electricity; variation of the needle; dip of the needle; barometer, thermometer, hygrometer, wind, weather, occaſional obſervations; to which may be added, the rain-gage, wind-gage, &c. Theſe things being carefully noted,

and

and kites being frequently raiſed, to the greateſt heights poſſible, *together with a thermometer* (o), in different ſtates of the weather, would probably ſoon throw new light upon this ſubject, and perhaps produce diſcoveries, of which we now have not the leaſt idea. From my experiments, and obſervations, on the electricity of fogs; I once imagined, that, whenever I ſaw a very thick one, and the air was at the ſame time ſharp, and froſty; that fog, was ſtrongly electrified poſitively: but I have met with ſeveral exceptions to this rule, for on Dec. 24, 25, 26, 27, 28, 29, 1772, in all which days there were thick fogs, thermometer from 36 to 33, wind conſtantly N. E. I could perceive no ſenſible electricity. Hence I conjectured, that though I could diſcover none *in my ſituation*, yet higher in the atmoſphere it might probably be found in plenty, and this conjecture was preſently afterward verified by Mr. NAIRNE, who obſerved the air to be electrical, when he ſtood in the golden gallery of St. Paul's cathedral, which is about two hundred and eighty feet in height, though he could diſcover none in the ſtone gallery, which is conſiderably lower: and the ſame obſervation hath ſince been made by others; and by myſelf, when I found the divergence of the balls to be increaſed, if I projected the rod, (from which they were ſuſpended) through one of the lights, in the lanthorn; which is ſtill higher. And it is with ſome pleaſure that I have ſince obſerved that Dr. LIND, and Mr. BRYDONE, have made the

(o) If Lord CHARLES CAVENDISH's can be raiſed properly, perhaps that may be preferable to any other.

ſame remark in raiſing their electrical kites (p). I have made many obſervations on the electricity of the atmoſphere, of which I kept a journal more than a year: but I have no deſire to publiſh it; as I hope the curious in theſe matters will be favoured with a much more accurate one, by gentlemen better qualified to undertake it.

It may not perhaps be improper to obſerve, that in the courſe of my experiments upon the electricity of fogs, I have frequently obſerved the balls to diverge *full two inches*: but this never happened except in a thick one; when the wind was S. W., and the mercury in the thermometer, under 40. In the inſtances I met with, where I could diſcover no electricity in ſuch a fog, though the mercury ſtood at 35 or 36, the wind was always N. E. The apparatus I uſed, conſiſted of a light rod about ſeven feet long, furniſhed with a box containing a pair of light cork balls, hung by linen threads, ſeven inches long. This rod was placed in a piece of wood, (over the top of one of the higheſt windows in the houſe, moſt remote from other buildings) properly fitted to receive it. The end of the rod, from which the balls were ſuſpended, was elevated to an angle of about forty-five degrees. Another rod, of equal length, was provided with a tin ſocket, into which went a long, ſubſtantial, ſtick of hard ſealing wax, or ſhell lac; which being excited, and projected out at the window, was brought near the balls; and thus readily, determined the kind of electricity in the atmoſphere.

(*p*) See Dr. PRIESTLEY's Hiſtory of Electricity, ſecond edition, p. 333. Experiments by Mr. DE ROMAS.

But

But in *an open situation*, such an apparatus is unnecessary, as one of Mr. CANTON's electrometers *having light balls*, succeeds very well, when held at the distance of about two feet from the body: the back of the observer being turned towards the wind. This method also serves to determine the kind of it, when there is a much larger quantity of electricity in the atmosphere; and which would electrify the balls so strongly, if held at the end of a long rod, high in the air; that excited wax, &c. being brought under them, would not produce the least alteration in their divergence.

Notwithstanding the balls hanging from the end of my rod, *in the open air*, have in some fogs diverged full two inches; yet I have never been able to make a pair of very light ones, *hanging from an insulated conductor, in my room*, diverge in the least, by means of the electricity collected from such a fog, by a long fishing-rod, round which a fine pointed wire was twisted, and made to communicate with the conductor. On mentioning this circumstance to my worthy friend Dr. FRANKLIN; he desired me to try whether having electrified the air *in one room*, I could by introducing the end of such an insulated rod into that air, make the balls diverge, when hanging at the opposite end of it, *in another room*. I have since tried the experiment in two rooms, *separated by a passage, nine feet long*, in the following manner: I drew off the charge of a large jar, without success; but having recharged it, and drawn it off a second time; the balls hanging from the end of the rod, (upwards of twenty feet long) in the other room, diverged a full

full inch; and I ſuppoſe the effect would have been greater, but there being a large fire in the room where the experiment was made, on opening the door, in order to introduce the pointed wire, which was twiſted round the inſulated rod, I apprehend, much of the electrified air was diſplaced, by the outer air preſſing in upon it; and driving it to the chimney, &c.

A pair of balls hanging from my hand, near the end of the rod, *in the electrified air*, diverged one inch and an half: but being held near the other end of the rod, *in the unelectrified air*; they diverged only half an inch. I then inſulated the rubber of my machine, and ſtuck a long ſharp-pointed needle in the back of it. Then hanging a chain from my prime-conductor to the table, I began to turn the winch; when the air of the room, the end of the rod, &c. were preſently affected: and the balls at the oppoſite end of it, in the other room, ſoon ſeparated conſiderably more than an inch.

I cannot prevail on myſelf to quit this pleaſing, yet difficult ſubject, without expreſſing a wiſh, that an electrical machine may be conſtructed, to work from ten, to fifty large cylinders, furniſhed with a prime-conductor, batteries, and other apparatus, proportionably large; the whole to be incloſed by brick walls, having flews quite round it, (like an hot-houſe, for botanic purpoſes) to keep the air in the room in a due temperature for experiment. With ſuch a machine as this, properly managed; new, unexpected, and wonderful diſcoveries might be be made: and, to uſe the words of my learned and ingenious friend Dr. PRIESTLEY, they are not philoſophers who think no advantages could be gained by it. Electrical bells ſhould alſo be ſet up, to give notice of electricity, in the atmoſphere; and by a pair of light pith or cork balls, hanging by linen threads from the apparatus, the kind, continuance, and changes of the electricity ſhould be carefully noted. It would alſo be proper to put out occaſionally, a long ſtick, or the ſtrongeſt parts of a fiſhing-rod, having a box, with a pair of light cork balls hanging from thence, at the end of it, which would diſcover much ſmaller degrees of electricity in the air, than is ſufficient to ring bells; and by theſe, the kind of it may be readily aſcertained. Such a courſe of experiments as I have recommended in this paper, would ſoon throw new light upon the ſubject of electricity; hitherto, I believe, but little underſtood; though ſo intereſting to mankind, and ſo highly deſerving the niceſt inveſtigation, of the moſt curious enquirers into nature. But theſe purſuits can be properly attended to, by thoſe only, who are gentlemen of fortune, and leiſure: and could ſuch be prevailed on to undertake them, I have not the leaſt doubt but the exquiſite knowledge of *this ſecret part* of the operations of nature, which they would ſoon attain in the practice, would prove an ample reward of their labours; an honour to their country, and perhaps a benefit to the whole human race.

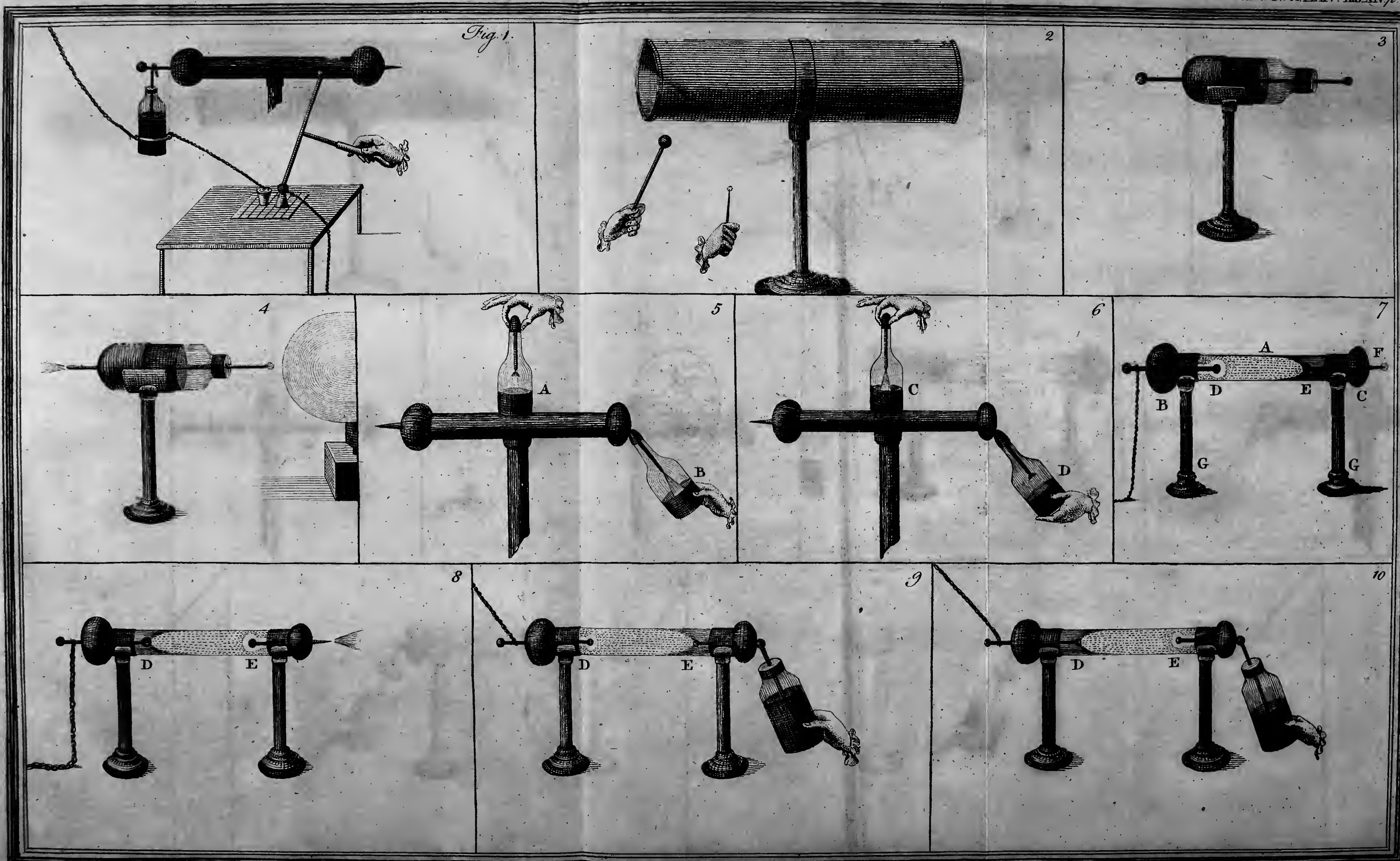
XLII. *A Letter from* David Macbride, *M. D. to* John Walſh, *Eſq; F. R. S. accompanying two Letters from Mr.* Simon *to Dr.* Macbride, *concerning the Reviviſcence of ſome Snails preſerved many Years in Mr.* Simon's *Cabinet.*

TO JOHN WALSH, Eſq;

DEAR SIR, Dublin, 22 Jan. 1774.

Redde, May 5, 1774. I Incloſe to you two letters, which I received from Mr. STUCKEY SIMON, concerning that extraordinary fact in Natural Hiſtory, which you ſeemed to regret had not been ſufficiently authenticated to be communicated to the public, in the Philoſophical Tranſactions of laſt year.—The Royal Society are undoubtedly in the right to be extremely cautious of allowing any thing, ſo very much out of the hitherto-obſerved courſe of nature, as this is, to appear in their publications, without the fulleſt evidence.

In



Basire sc.

In Mr. SIMON's letter of the 26th of November, you will pleafe to obferve, that he mentions a particular fhell, whofe fnail had come out repeatedly four different times, in the prefence of different people; each of whom have affured me that they faw it. That gentleman having done me the favour to dine with me, a day or two after the date of that letter, he brought the identical fhell (as he declared), in order that we might try if the fnail would again make its appearance.

The company were not difappointed; for, after the fhell had lain about ten minutes in a glafs of water that had the cold barely taken off, the fnail began to appear; and in five minutes more we perceived half the body fairly pufhed out from the cavity of the fhell. We then removed it into a bafin, that the fnail might have more fcope than it had in the glafs: and here, in a very fhort time, we faw it get above the furface of the water, and crawl up towards the edge of the bafin. While it was thus moving about, with its horns erect, a fly chanced to be hovering near, and, perceiving the fnail, darted down upon it. The little animal inftantly withdrew itfelf within the fhell, but as quickly came forth again, when it found the enemy had gone off. We allowed it to wander about the bafin for upwards of an hour; when we returned it into a wide-mouthed phial, wherein Mr. SIMON had lately been ufed to keep it. He was fo obliging, as to prefent me with this remarkable fhell; and I obferved, at twelve o'clock, as I was going to bed, that the fnail was ftill in motion: but, next morning, I found it in a torpid ftate, fticking to the fide of the glafs.

In a few weeks after the time above-mentioned, I took an opportunity of ſending this ſhell to Sir JOHN PRINGLE, who ſhewed it at a meeting of the Society; but, as he has been pleaſed to inform me, ſome of the members could not bring themſelves to believe, but that Mr. SIMON muſt have ſuffered himſelf to be impoſed on by his ſon, who, as they imagined, ſubſtituted freſh ſhells, for thoſe which he had got out of the cabinet.

When Sir JOHN PRINGLE acquainted me with this difficulty, I wrote to Mr. SIMON, and that produced his letter of the 4th of February. I afterwards alſo examined the boy myſelf; and could find no reaſon to believe, that he either did, or could impoſe on his father.

Mr. SIMON is a merchant of this place, of a very reputable character, and undoubted veracity. He lives in the heart of the city, a circumſtance which rendered it almoſt impoſſible for the ſon (if he had been ſo diſpoſed) to collect freſh ſhells. The father of Mr. STUCKEY SIMON was Mr. JAMES SIMON, a Fellow of the Royal Society; who, being a lover of Natural Hiſtory, as well as an Antiquarian, made a little collection of foſſils, which is ſtill in the ſon's poſſeſſion, and contains ſome articles that are rather uncommon.

Should Mr. SIMON's letters be inſerted in the Tranſactions, they will no doubt be the means of exciting Naturaliſts to enquire into the extent of vitality in the lower orders of animals.

I am, dear Sir, your moſt obedient,
and very humble ſervant,
DAVID MACBRIDE.

Mr.

Mr. STUCKEY SIMON to Dr. MACBRIDE.

SIR, Dublin, 26 Nov. 1772.

AN accident having brought to light what ſome Naturaliſts have not had an opportunity to examine into, and which has been a ſubject of ſome converſation amongſt gentlemen to whom I have mentioned it, has made me commit to writing the ſimple facts, in order to put others on making further experiments on the ſubject.—About three months ſince, I was ſettling ſome ſhells in a drawer; amongſt which were ſome ſnail-ſhells. I took them out, and gave them to my ſon (a child about ten years old), who was then in the room with me. The Saturday following, the child diverted himſelf with the ſhells, put them into a flower-pot, which he filled with water, and next morning put them into a baſin. Having occaſion to uſe it, I obſerved the ſnails had come out of the ſhells. I examined the child. He aſſured me they were the ſame I gave him ſome days before; and ſaid he had a few more, which he brought me. I put one of them in water; and, in half an hour after, I obſerved him put out his horns and body, which he moved with a ſlow motion, I ſuppoſe from weakneſs. I then informed Major Vallancy and Dr. Span of this ſurpriſing diſcovery. They did me the favour to come to my houſe the Saturday following, to examine the ſnails; and, on putting them in water, found that only one had life, which was that I put in water, for he came out of his ſhell, and carried it on his back about the baſin. The reſt, I ſuppoſe, died by being kept too long in water; for, on the firſt diſcovery, I let them remain in the water until the Monday following, when I poured off the water, the ſnails being ſtill out of their ſhells, and ſeemingly dead. They lay in that ſtate until Tueſday night, when I found they had all withdrawn into their ſhells; and, though I ſeveral times ſince put them into water, they ſhewed no ſigns of life. Dr. Quin and Dr. Rutty did me the favour, at different times, to examine the ſnail that is living; and were greatly pleaſed to ſee him come out of his ſolitary habitation in which he has been confined upwards of fifteen years, for ſo long I can with truth declare he has been in my poſſeſſion; as my father died in January 1758, in whoſe collection of foſſils thoſe ſnails were, and for what I know they might have been many years in his poſſeſſion before they came into my hands. The ſhells are ſmall, and of one kind; white, ſtriped with brown. —— Since this diſcovery, I have kept this ſnail in a ſmall phial, with a cover with holes, to let in air; and he ſeems at preſent very ſtrong, and in health. I ſhall be extremely glad, if this plain account I have given you would induce gentlemen to make ſome further experiments on this ſubject. I am, Sir,

Your moſt obedient,

humble ſervant,

STUCKEY SIMON.

Mr.

Mr. STUCKEY SIMON to Dr. MACBRIDE.

DEAR SIR, Strand-ſtreet, 4 Feb. 1773.

I RECEIVED your letter; and ſee that Sir JOHN PRINGLE received the ſnail ſafe. You ſay, that ſome gentlemen are inclined to think, my ſon has impoſed on me freſh ſhells, in the ſtead of thoſe I gave him. He had no opportunity to get any other ſhells, being, at the time and for ſeveral days after, confined to the houſe with a cold. I am poſitive they are the ſame I gave him, having more of the ſame ſort of ſhells in my cabinet, and nearly the ſame ſize.

The nine ſhells, which produced the ſnails, are of the ſame kind as the one you ſent to Sir JOHN PRINGLE; and I now ſend you one of them, with the ſnail in it, which I take to be dead. Having put it in water ſeveral times, it became ſoft; and a part of it puſhed out of the ſhell, but ſhewed no other ſign of life. I would have ſent you a few more of the ſhells, but that the Biſhop of Derry, and ſome other friends, have begged of me to give them a ſhare.

I am, dear Sir,

Your moſt obedient,

humble ſervant,

STUCKEY SIMON.

XLIII. *The Bill of Mortality of the Town of* Warrington, *for the year* 1773. *By the Rev.* J. Aikin. *Communicated by Dr.* Percival.

Redde, May 19, 1774.

THE town of WARRINGTON, by the beſt account yet procured, contains between 1600 and 1700 houſes. At five perſons to a houſe, which is ſuppoſed a ſufficient allowance, as but few are occupied by more than one family, this will give ſomewhat above 8000 for the number of inhabitants.

The average of yearly marriages, chriſtenings, and burials, regiſtered in the pariſh church, from 1750 to 1769 incluſive, is,

	Marriages.	Chriſtenings.	Burials.
- - - - - -	73	237	199
That for the years 1770, 1771, 1772, is, - -	95	331	258

This will ſerve to ſhow the increaſe of the place, and its comparative healthineſs; eſpecially if we conſider that the deaths are much more exactly regiſtered than the births. In the preſent bill, the number of children, who died after receiving only private

private baptiſm, in conſequence of which their deaths were regiſtered, but not their births, amounts to 17; which might therefore be added to the average of chriſtenings for the three laſt years, and will form an extraordinary inſtance of healthineſs and increaſe. The preſent bill alſo takes in the ſeparate regiſters kept by different ſocieties, in which the births much exceed the burials, as many of the latter are entered at the pariſh church.

The melancholy overbalance of burials, which now appears, plainly ariſes from the dreadful ravages, of a ſingle diſeaſe, the ſmall-pox; which perhaps has ſeldom raged with greater malignity than in its late viſitation of this town. Its victims were chiefly young children; whom it attacked with ſuch inſtant fury, that the beſt-directed means for relief were of little avail. The ſtate of the air went through all poſſible variations in the courſe of it, but with no perceptible difference in the ſtate of the diſeaſe. In general, the ſick were kept ſufficiently cool, and were properly ſupplied with diluting and acidulous drinks; yet where they recovered it ſeemed rather owing to a leſs degree of malignity in the diſeaſe, or greater ſtrength to ſtruggle with it, than any peculiar management. Where it ended fatally, it was uſually before the puſtules came to maturation; and indeed in many they ſhowed no diſpoſition to advance after the complete eruption, but remained quite flat and pale. The gradual progreſs and decline of the diſeaſe will appear from the Table of Months. Its proportional virulence began to abate conſiderably before it ceaſed. I cannot with certainty lay down the general proportion of deaths; but in one neigh-

bourhood

bourhood I found that out of 29 who had the diſeaſe, 12 died, or about 2 in 5; in others the mortality was ſtill greater, and I have reaſon to believe it was not leſs on the whole. It may perhaps be worthy of obſervation that the proportion of females who died, to males, was nearly as three to two. While we lament the ſeverity of the ſcourge with which we have been afflicted, we cannot but highly regret, that a practice, which experience has eſtabliſhed as ſo effectual a ſecurity againſt it, was ſo little followed. Not ten, I believe, were inoculated in the whole town and neighbourhood: theſe all did well, yet their example was not ſufficient to overcome ſome accidental prejudices taken againſt it. Indeed, the poor, who were the chief ſufferers in this calamity, beſides theſe prejudices, might be deterred by the idea of expence attending this branch of medical aſſiſtance. But if the opulent and charitable would reflect how exceedingly uſeful their benefactions directed to this point would be, that, by a proper encouragement of this practice the lives of 200 of the riſing generation might in all human probability have been ſaved to the public in the courſe of one year, the regret of having loſt ſuch an opportunity of doing good, would, I hope, be ſucceeded by ſuitable reſolutions for ſome future occaſion. It would be eaſy to ſuggeſt a plan for promoting the practice of inoculation at a very moderate expence; and, I am perſuaded, the taſk of engaging the aſſiſtance of the faculty, would be the leaſt difficult part.

With reſpect to the general Table of Diſeaſes, the obvious uncertainty and inaccuracy of an enquiry which, in moſt caſes, could only be made by the clerk

clerk in the church-yard, made me despair of rendering it in any great degree subservient to the purposes of science. It has not, therefore, been attempted to give it a scientific form; but the articles have for the most part been inserted just as they were given in. Indeed the alarming article of Consumption, which includes all those returned under the common terms of Weakness, Surfeit, and Decay, has been arranged under three different periods of age, to enable the medical reader the better to judge of the different diseases contained under it.

The Table of Ages and Conditions has been drawn up with great exactness; and will certainly, in a proper series, form very useful grounds for the calculations in political arithmetic. In this light it is hoped that the utility of accurate bills of mortality will be generally apparent; and that, since the benefit of them is not confined to one profession, all will concur in encouraging them.

JOHN AIKIN.

WARRINGTON, 1773,

BILL OF MORTALITY.

TABLE I. GENERAL BILL.

MARRIAGES 93.

BIRTHS { Males 175, Females 181 } 356.

DEATHS { Males 223, Females 250 } 473.

TABLE II. MONTHS.

	Total Deaths.	Small Pox.
January,	39	4
February,	34	4
March,	48	13
April,	52	23
May,	83	63
June,	71	49
July,	49	33
Auguſt,	27	11
September,	20	7
October,	14	3
November,	6	—
December,	30	1
	473	211

TABLE

TABLE III. DISEASES.

Aged - - - - - - -		32
Aſthma - - - - - - -		3
Caſualties - - - - -		5
Childbed - - - - -		5
Chincough - - - - -		16
Conſumption under 14 - - -	24	96
- - - - - - - - - 14 to 45 - - -	36	
- - - - - - - - - above 45 - - -	36	
Convulſions - - - - -		34
Dropſy - - - - - -		13
Fever - - - - - -		15
Inflammation of the Bowels - -		2
Meaſles - - - - -		1
Mortification - - - - -		1
Palſy - - - - - -		7
Quinſey - - - - -		1
Scrophulous Swelling - - -		1
Small Pox - - - - -		211
Soon after Birth - - - -		6
Suddenly - - - - -		6
Teething - - - - -		7
Worm Fever - - - - -		10
Unknown - - - - -		1
		473

TABLE IV.

AGES AND CONDITIONS.

	Males	Females	Total
To 1 Month	8	10	18
From 1 to 2	6	2	8
2—3	4	1	5
3—6	6	7	13
6—9	11	13	24
9-12	19	11	30
1-2 Years	49	59	108
2—3	12	26	38
3—4	12	14	26
4—5	9	12	21
5—6	2	6	8
6—7	4	3	7
7—8	1	1	2
8—9	1	7	8
9—10	0	2	2
10—14	2	2	4
Total	146	176	322

	Never married		Husbands	Wives	Widowers	Widows	Total
	Males	Females					
14—17	2	1					3
17—20	3	1					4
20—25	6	4	1	3			14
25—30	6		2	4	1		13
30—35	1	1	2	3	1		8
35—40	2	1		2			5
40—45	1		2	3			6
45—50		1	6	3	1	1	12
50—60	1	1	6	6	2	3	19
60—70	1	1	8	7	3	9	29
70—80		2	5	3	12	6	28
80—90			1	1	1	4	7
90-100				1	1	1	3
Total	23	13	33	36	22	24	151

XLIV. *Of*

XLIV. *Of the ſtilling of Waves by means of Oil. Extracted from ſundry Letters between* Benjamin Franklin, *LL. D. F. R. S.* William Brownrigg, *M. D. F. R. S. and the Reverend Mr.* Fariſh.

Extract of a Letter from Doctor BROWNRIGG to Dr. FRANKLIN, dated Ormathwait, January 27, 1773.

Redde, June 2, 1774.

BY the encloſed from an old friend, a worthy clergyman at Carliſle, whoſe great learning and extenſive knowledge in moſt ſciences would have more diſtinguiſhed him, had he been placed in a more conſpicuous point of view, you will find that he had heard of your experiment on Derwent Lake, and has thrown together what he could collect on that ſubject; to which I have ſubjoined one experiment from the relation of another Gentleman.

Extract of a Letter from the Reverend Mr. FARISH, to Dr. BROWNRIGG.

I ſome time ago met with Mr. Dun, who ſurpriſed me with an account of an experiment you had tried upon the Derwent water, in company with Sir

Sir JOHN PRINGLE and Dr. FRANKLIN. According to his reprefentation, the water, which had been in great agitation before, was inftantly calmed, upon pouring in only a very fmall quantity of oil, and that to fo great a diftance round the boat as feems a little incredible. I have fince had the fame accounts from others, but I fufpect all of a little exaggeration. PLINY mentions this property of oil as known particularly to the divers, who made ufe of it in his days, in order to have a more fteady light at the bottom(*a*). The failors, I have been told, have obferved fomething of the fame kind in our days, that the water is always remarkably fmoother in the wake of a fhip that hath been newly tallowed, than it is in one that is foul. — Mr. PENNANT alfo mentions an obfervation of the like nature made by the feal catchers in Scotland. *Brit. Zool.* Vol. IV. *Article* SEAL. When thefe animals are devouring a very oily fifh, which they always do under water, the waves above are obferved to be remarkably fmooth, and by this mark the fifhermen know where to look for them. — Old PLINY does not ufually meet with all the credit I am inclined to think he deferves. I fhall be glad to have an authentic account of the Kefwick experiment, and if

Note by Dr. BROWNRIGG.

(*a*) Sir GILFRED LAWSON, who ferved long in the army at Gibraltar, affures me that the fifhermen in that place are accuftomed to pour a little oil on the fea, in order to ftill its motion, that they may be enabled to fee the oyfters lying at its bottom; which are there very large, and which they take up with a proper inftrument. This Sir GILFRED had often feen there performed, and faid the fame was practifed on other parts of the Spanifh coaft.

it

it comes up to the reprefentations that have been made of it, I fhall not much hefitate to believe the old Gentleman in another more wonderful phænomenon, he relates, of ftilling a tempeft only by throwing up a little vinegar into the air.

Extract of a Letter to Doctor BROWNRIGG from Doctor FRANKLIN.

London, Nov. 7, 1773.

DEAR SIR,

I thank you for the remarks of your learned friend at Carlifle. — I had, when a youth, read and fmiled at PLINY's account of a practice among the feamen of his time, to ftill the waves in a ftorm by pouring oil into the fea; which he mentions, as well as the ufe made of oil by the divers; but the ftilling a tempeft by throwing vinegar into the air had efcaped me. I think with your friend, that it has been of late too much the mode to flight the learning of the antients. The learned, too, are apt to flight too much the knowledge of the vulgar. The cooling by evaporation was long an inftance of the latter. This art of fmoothing the waves with oil, is an inftance of both.

Perhaps you may not diflike to have an account of all I have heard, and learnt, and done in this way. Take it if you pleafe as follows.

In 1757, being at fea in a fleet of 96 fail bound againft Louifbourg, I obferved the wakes of two of the fhips to be remarkably fmooth, while all the others were ruffled by the wind, which blew frefh. Being puzzled with the differing appearance, I at laft pointed

pointed it out to our captain, and aſked him the meaning of it? "The cooks, ſays he, have, I ſuppoſe, been juſt emptying their greaſy water through the ſcuppers, which has greaſed the ſides of thoſe ſhips a little;" and this anſwer he gave me with an air of ſome little contempt, as to a perſon ignorant of what every body elſe knew. In my own mind I at firſt ſlighted his ſolution, tho' I was not able to think of another. But recollecting what I had formerly read in PLINY, I reſolved to make ſome experiment of the effect of oil on water, when I ſhould have opportunity.

Afterwards being again at ſea in 1762, I firſt obſerved the wonderful quietneſs of oil on agitated water, in the ſwinging glaſs lamp I made to hang up in the cabin, as deſcribed in my printed papers, page 438 of the fourth edition. — This I was continually looking at and conſidering, as an appearance to me inexplicable. An old ſea captain, then a paſſenger with me, thought little of it, ſuppoſing it an effect of the ſame kind with that of oil put on water to ſmooth it, which he ſaid was a practice of the BERMUDIANS when they would ſtrike fiſh, which they could not ſee, if the ſurface of the water was ruffled by the wind. This practice I had never before heard of, and was obliged to him for the information; tho' I thought him miſtaken as to the ſameneſs of the experiment, the operations being different; as well as the effects. In one caſe, the water is ſmooth till the oil is put on, and then becomes agitated. In the other it is agitated before the oil is applied, and then becomes ſmooth. — The ſame gentleman told me, he had heard it was a practice with the fiſhermen

men of LISBON when about to return into the river, (if they ſaw before them too great a ſurf upon the bar, which they apprehended might fill their boats in paſſing) to empty a bottle or two of oil into the ſea, which would ſuppreſs the breakers, and allow them to paſs ſafely: a confirmation of this I have not ſince had an opportunity of obtaining. But diſcourſing of it with another perſon, who had often been in the Mediterranean, I was informed that the divers there, who, when under water in their buſineſs, need light, which the curling of the ſurface interrupts by the refractions of ſo many little waves, let a ſmall quantity of oil now and then out of their mouths, which riſing to the ſurface ſmooths it, and permits the light to come down to them. — All theſe informations I at times revolved in my mind, and wondered to find no mention of them in our books of experimental philoſophy.

At length being at CLAPHAM where there is, on the common, a large pond, which I obſerved to be one day very rough with the wind, I fetched out a cruet of oil, and dropt a little of it on the water. I ſaw it ſpread itſelf with ſurprizing ſwiftneſs upon the ſurface; but the effect of ſmoothing the waves was not produced; for I had applied it firſt on the leeward ſide of the pond, where the waves were largeſt, and the wind drove my oil back upon the ſhore. I then went to the windward ſide, where they began to form; and there the oil, though not more than a tea ſpoonful, produced an inſtant calm over a ſpace ſeveral yards ſquare, which ſpread amazingly, and extended itſelf gradually till it reached the lee ſide, making all that quarter of the pond, perhaps half an acre, as ſmooth as a looking-glaſs.

After this, I contrived to take with me, whenever I went into the country, a little oil in the upper hollow joint of my bamboo cane, with which I might repeat the experiment as opportunity ſhould offer; and I found it conſtantly to ſucceed.

In theſe experiments, one circumſtance ſtruck me with particular ſurprize. This was the ſudden, wide, and forcible ſpreading of a drop of oil on the face of the water, which I do not know that any body has hitherto conſidered. If a drop of oil is put on a poliſhed marble table, or on a looking-glaſs that lies horizontally; the drop remains in its place, ſpreading very little. But when put on water it ſpreads inſtantly many feet round, becoming ſo thin as to produce the priſmatic colours, for a conſiderable ſpace, and beyond them ſo much thinner as to be inviſible, except in its effect of ſmoothing the waves at a much greater diſtance. It ſeems as if a mutual repulſion between its particles took place as ſoon as it touched the water, and a repulſion ſo ſtrong as to act on other bodies ſwimming on the ſurface, as ſtraws, leaves, chips, &c. forcing them to recede every way from the drop, as from a center, leaving a large clear ſpace. The quantity of this force, and the diſtance to which it will operate, I have not yet aſcertained; but I think it a curious enquiry, and I wiſh to underſtand whence it ariſes.

In our journey to the north, when we had the pleaſure of ſeeing you at Ormathwaite, we viſited the celebrated Mr. SMEATON near Leeds. Being about to ſhew him the ſmoothing experiment on a little pond near his houſe, an ingenious pupil of his, Mr. Jeſſop, then preſent, told us of an odd appearance on that pond,

 which

which had lately occurred to him. He was about to clean a little cup in which he kept oil, and he threw upon the water ſome flies that had been drowned in the oil. Theſe flies preſently began to move, and turned round on the water very rapidly, as if they were vigorouſly alive, though on examination he found they were not ſo. I immediately concluded that the motion was occaſioned by the power of the repulſion abovementioned, and that the oil iſſuing gradually from the ſpungy body of the fly continued the motion. He found ſome more flies drowned in oil, with which the experiment was repeated before us. To ſhew that it was not any effect of life recovered by the flies, I imitated it by little bits of oiled chips and paper cut in the form of a comma, of the ſize of a common fly; when the ſtream of repelling particles iſſuing from the point, made the comma turn round the contrary way. This is not a chamber experiment; for it cannot well be repeated in a bowl or diſh of water on a table. A conſiderable ſurface of water is neceſſary to give room for the expanſion of a ſmall quantity of oil. In a diſh of water, if the ſmalleſt drop of oil be let fall in the middle, the whole ſurface is preſently covered with a thin greaſy film proceeding from the drop; but as ſoon as that film has reached the ſides of the diſh, no more will iſſue from the drop, but it remains in the form of oil, the ſides of the diſh putting a ſtop to its diſſipation by prohibiting the farther expanſion of the film.

Our friend Sir JOHN PRINGLE being ſoon after in Scotland, learnt there, that thoſe employed in the herring fiſhery, could at a diſtance ſee where the ſhoals of herrings were, by the ſmoothneſs of the water over them, which might poſſibly be occaſioned, he thought, by ſome oilineſs proceeding from their bodies.

A gentleman from Rhode-iſland told me, it had been remarked that the harbour of Newport was ever ſmooth while any whaling veſſels were in it; which probably aroſe from hence, that the blubber which they ſometimes bring looſe in the hold, or the leakage of their barrels, might afford ſome oil, to mix with that water, which from time to time they pump out to keep the veſſel free, and that ſame oil might ſpread over the ſurface of the water in the harbour, and prevent the forming of any waves.

This prevention I would thus endeavour to explain.

There ſeems to be no natural repulſion between water and air, ſuch as to keep them from coming into contact with each other. Hence we find a quantity of air in water; and if we extract it by means of the air-pump, the ſame water again expoſed to the air, will ſoon imbibe an equal quantity.

Therefore air in motion, which is wind, in paſſing over the ſmooth ſurface of water, may rub, as it were, upon that ſurface, and raiſe it into wrinkles, which, if the wind continues, are the elements of future waves.

The ſmalleſt wave once raiſed does not immediately ſubſide, and leave the neighbouring water quiet: but in ſubſiding raiſes nearly as much of the water next to it, the friction of the parts making little difference. Thus a ſtone dropt in a pool raiſes firſt a ſingle wave round itſelf; and leaves it, by ſinking to the bottom; but that firſt wave ſubſiding raiſes a ſecond, the ſecond a third, and ſo on in circles to a great extent.

A ſmall power continually operating will produce a great action. A finger applied to a weighty ſuſpended bell, can at firſt move it but little; if repeatedly applied, though with no greater ſtrength, the motion increaſes till the bell ſwings to its utmoſt height, and with a force that cannot be reſiſted by the whole ſtrength of the arm and body. Thus the ſmall firſt-raiſed waves, being continually acted upon by the wind, are, though the wind does not increaſe in ſtrength, continually increaſed in magnitude, riſing higher and extending their baſes, ſo as to include a vaſt maſs of water in each wave, which in its motion acts with great violence.

But if there be a mutual repulſion between the particles of oil, and no attraction between oil and water, oil dropt on water will not be held together by adheſion to the ſpot whereon it falls; it will not be imbibed by the water; it will be at liberty to expand itſelf; and it will ſpread on a ſurface that, beſides being ſmooth to the moſt perfect degree of poliſh, prevents, perhaps by repelling the oil, all immediate contact, keeping it at a minute diſtance from itſelf; and the expanſion will continue, till the mutual repulſion between the particles of the oil is weakened and reduced to nothing by their diſtance.

Now I imagine that the wind blowing over water thus covered with a film of oil, cannot eaſily *catch* upon it, ſo as to raiſe the firſt wrinkles, but ſlides over it, and leaves it ſmooth as it finds it. It moves a little the oil indeed, which, being between it and the water, ſerves it to ſlide with, and prevents friction, as oil does between thoſe parts of a machine, that would otherwiſe rub hard together. Hence the

oil

oil dropt on the windward ſide of a pond proceeds gradually to leeward, as may be ſeen by the ſmoothneſs it carries with it, quite to the oppoſite ſide. For the wind being thus prevented from raiſing the firſt wrinkles that I call the elements of waves, cannot produce waves, which are to be made by continually acting upon and enlarging thoſe elements, and thus the whole pond is calmed.

Totally therefore we might ſuppreſs the waves in any required place, if we could come at the windward place where they take their riſe. This in the ocean can ſeldom if ever be done. But perhaps ſomething may be done on particular occaſions, to moderate the violence of the waves, when we are in the midſt of them, and prevent their breaking, where that would be inconvenient.

For when the wind blows freſh, there are continually riſing on the back of every great wave, a number of ſmall ones, which roughen its ſurface, and give the wind hold, as it were, to puſh it with greater force. This hold is diminiſhed by preventing the generation of thoſe ſmall ones. And poſſibly too, when a wave's ſurface is oiled, the wind, in paſſing over it, may rather in ſome degree preſs it down, and contribute to prevent its riſing again, inſtead of promoting it.

This as mere conjecture would have little weight, if the apparent effects of pouring oil into the midſt of waves were not conſiderable, and as yet not otherwiſe accounted for.

When the wind blows ſo freſh, as that the waves are not ſufficiently quick in obeying its impulſe, their tops being thinner and lighter are puſhed forward, broken,

broken, and turned over in a white foam. Common waves lift a veffel, without entering it; but thefe when large fometimes break above and pour over it, doing great damage.

That this effect might in any degree be prevented, or the height and violence of waves in the fea moderated, we had no certain account; PLINY's authority for the practice of feamen in his time being flighted. But difcourfing lately on this fubject with his excellency Count BENTINCK of Holland, his fon the honourable Captain BENTINCK, and the learned profeffor ALLEMAND, (to all whom I fhewed the experiment of fmoothing in a windy day the large piece of water at the head of the Green Park;) a letter was mentioned which had been received by the Count from Batavia, relative to the faving of a Dutch fhip in a ftorm, by pouring oil into the fea. I much defired to fee that letter, and a copy of it was promifed me, which I afterward received (b).

(b) Extrait d'une Lettre de Mr. TENGNAGEL à Mr. le Comte de BENTINCK, écrite de Batavia le 15 Janvier, 1770.

Près des ifles Paulus & Amfterdam nous effuiames un orage, qui n'eut rien d'affez particulier pour vous être marqué, fi non que notre capitaine fe trouva obligé en tournant fous le vent, de verfer de l'huile contre la haute mer, pour empecher les vagues de fe brifer contre le navire, ce qui réuffit à nous conferver, & a été d'un très bon effet: comme il n'en verfa qu'une petite quantité à la fois, la compagnie doit peut-être fon vaiffeau à fix demi aumes d'huile d'olive: j'ai été préfent quand cela s'eft fait, & je ne vous aurois pas entretenu de cette circonftance, fi ce n'étoit que nous avons trouvé les gens ici fi prévenus contre l'expérience, que les officiers du bord ni moi n'avons fait aucune difficulté de donner un certificat de la vérité fur ce chapitre.

" Extract of a Letter from Mr. TENGNAGEL to " Count BENTINCK, dated at Batavia the 5th " of January 1770.

" Near the iſlands Paul and Amſterdam, we met " with a ſtorm, which had nothing particular in it " worthy of being communicated to you, except " that the captain found himſelf obliged, for great- " er ſafety in wearing the ſhip, to pour oil into the " ſea, to prevent the waves breaking over her, which " had an excellent effect, and ſucceeded in preſerv- " ing us. — As he poured out but a little at a time, " the Eaſt India company owes perhaps its ſhip to " only ſix demi-aumes of oil-olive. I was preſent " upon deck when this was done; and I ſhould not " have mentioned this circumſtance to you, but that " we have found people here ſo prejudiced againſt " the experiment, as to make it neceſſary for the " officers on board and myſelf to give a certificate of " the truth on this head, of which we made no " difficulty."

On this occaſion, I mentioned to Captain BENTINCK, a thought which had occurred to me in reading the voyages of our late circumnavigators, particularly where accounts are given of pleaſant and fertile iſlands which they much deſired to land upon, when ſickneſs made it more neceſſary, but could not effect a landing through a violent ſurf breaking on the ſhore, which rendered it impracticable. My idea was, that poſſibly by ſailing to and fro at ſome diſtance from ſuch lee ſhore, continually pouring oil into

into the ſea, the waves might be ſo much depreſſed and leſſened before they reached the ſhore as to abate the height and violence of the ſurff, and permit a landing; which, in ſuch circumſtances, was a point of ſufficient importance to juſtify the expence of the oil that might be requiſite for the purpoſe. That gentleman, who is ever ready to promote what may be of public utility, though his own ingenious inventions have not always met with the countenance they merited, was ſo obliging as to invite me to Portſmouth, where an opportunity would probably offer, in the courſe of a few days, of making the experiment on ſome of the ſhores about Spithead, in which he kindly propoſed to accompany me, and to give aſſiſtance with ſuch boats as might be neceſſary. Accordingly, about the middle of October laſt, I went with ſome friends to PORTSMOUTH; and a day of wind happening, which made a lee-ſhore between HASLAR HOSPITAL and the Point near JILLKECKER, we went from the Centaur with the long-boat and barge towards that ſhore. Our diſpoſition was this: the long-boat was anchored about a quarter of a mile from the ſhore; part of the company were landed behind the Point (a place more ſheltered from the ſea) who came round and placed themſelves oppoſite to the long-boat, where they might obſerve the ſurff, and note if any change occurred in it, upon uſing the oil. Another party, in the barge, plyed to windward of the long-boat, as far from her as ſhe was from the ſhore, making trips of about half a mile each, pouring oil continually out of a large ſtone-bottle, through a hole in the cork, ſomewhat bigger than a gooſe-quill.

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The experiment had not, in the main point, the ſucceſs we wiſhed, for no material difference was obſerved in the height or force of the ſurff upon the ſhore; but thoſe who were in the long-boat could obſerve a tract of ſmoothed water, the whole length of the diſtance in which the barge poured the oil, and gradually ſpreading in breadth towards the long-boat. I call it ſmoothed, not that it was laid level; but becauſe, though the ſwell continued, its ſurface was not roughened by the wrinkles, or ſmaller waves, before-mentioned; and none, or very few white-caps (or waves whoſe tops turn over in foam) appeared in that whole ſpace, though to windward and leeward of it there were plenty; and a wherry, that came round the point under ſail, in her way to Portſmouth, ſeemed to turn into that tract of choice, and to uſe it from end to end, as a piece of turnpike-road.

It may be of uſe to relate the circumſtances even of an experiment that does not ſucceed, ſince they may give hints of amendment in future trials: it is therefore I have been thus particular. I ſhall only add what I apprehend may have been the reaſon of our diſappointment.

I conceive, that the operation of oil on water is, firſt, to prevent the raiſing of new waves by the wind; and, ſecondly, to prevent its puſhing thoſe before raiſed with ſuch force, and conſequently their continuance of the ſame repeated height, as they would have done, if their ſurface were not oiled. But oil will not prevent waves being raiſed by another power, by a ſtone, for inſtance, falling into a ſtill pool; for they then riſe by the mechanical impulſe of of the ſtone, which the greaſineſs on the ſurrounding water cannot leſſen or prevent, as it can prevent the winds catching the ſurface and raiſing it into waves. Now waves once raiſed, whether by the wind or any other power, have the ſame mechanical operation, by which they continue to riſe and fall, as a *pendulum* will continue to ſwing, a long time after the force ceaſes to act by which the motion was firſt produced: that motion will, however, ceaſe in time; but time is neceſſary. Therefore, though oil ſpread on an agitated ſea, may weaken the puſh of the wind on thoſe waves whoſe ſurfaces are covered by it, and ſo, by receiving leſs freſh impulſe, they may gradually ſubſide; yet a conſiderable time, or a diſtance through which they will take time to move, may be neceſſary to make the effect ſenſible on any ſhore in a diminution of the ſurff: for we know, that when wind ceaſes ſuddenly, the waves it has raiſed do not as ſuddenly ſubſide, but ſettle gradually, and are not quite down till long after the wind has ceaſed. So though we ſhould, by oiling them, take off the effect of wind on waves already raiſed, it is not to be expected that thoſe waves ſhould be inſtantly levelled. The motion they have received will, for ſome time, continue; and, if the ſhore is not far diſtant, they arrive there ſo ſoon, that their effect upon it will not be viſibly diminiſhed. Poſſibly, therefore, if we had begun our operations at a greater diſtance, the effect might have been more ſenſible. And perhaps we did not pour oil in ſufficient quantity. Future experiments may determine this.

 I was,

I was, however, greatly obliged to Captain BENTINCK, for the chearful and ready aids he gave me: and I ought not to omit mentioning Mr. BANKS, Dr. SOLANDER, General CARNAC, and Dr. BLAGDEN, who all affifted at the experiment, during that bluftring unpleafant day, with a patience and activity that could only be infpired by a zeal for the improvement of knowledge, fuch efpecially as might poffibly be of ufe to men in fituations of diftrefs.

I would wifh you to communicate this to your ingenious friend, Mr. FARISH, with my refpects; and believe me to be, with fincere efteem,

DEAR SIR,

Your moft obedient humble fervant,

B. FRANKLIN.

XLV. *Tranſlation of a Letter from M.* de Stehlin, *Counſellor of State to her Imperial Majeſty of* Ruſſia, *to Dr.* Maty, *with a Specimen of native Iron.*

SIR,

Redde, June 9, 1774.

AS a teſtimony of my attachment to the Royal Society, and as the firſt tribute I owe her, I have the honour to tranſmit herewith two real novelties, which I think worthy of her notice.

The firſt is, a *new map, and my preliminary deſcription, of a new Archipelago in the North,* diſcovered a few years ago by the Ruſſians, in the N.E. beyond Kamtſhatka.

The ſecond is *a piece of raw and native iron*; of which Mr. PALLAS, one of our academicians, who has theſe five years been employed in making reſearches in natural hiſtory, in the provinces of the Ruſſian empire, has diſcovered laſt year a hillock or maſs, weighing fifty puds (the pud conſiſting of forty Ruſſian pounds) in Siberia, in the mountains called NEMIR, between the rivulets Ubec and Siſim, which fall into the river JENISEI, ſcarce one hundred fathoms from a rich mine of loadſtone or iron [(a)].

(a) See the article in the Peterſburg Gazette.

You know, SIR, that the exiſtence of raw or native iron has hitherto been doubted; but I ſhould almoſt think, that this diſcovery determines the queſtion; eſpecially if it is conſidered, that in the whole diſtrict where this maſs has been found, there is not the leaſt trace extant of any ancient forge, nor any place that might leave room to ſuſpect that there had been, in former times, any works of iron ore, which had been melted, and afterwards abandoned to that maſs.

Should any doubt remain concerning the exiſtence of the native iron, and the authenticity of this diſcovery, I ſhould rather ſuppoſe that, many ages ago, there might have been a *Volcano*, which by melting the iron ore had formed the above maſs, to which might afterwards have been joined the little hyacinthine ſpars and other ſtones which are now mixed with it.

Tranſlation of an Article in the Peterſburg Gazette of Sept. 6, 1773.

"The academy expects from Siberia a black maſs weighing about forty puds [(b)], of raw or native, ſoft and flexible iron, which the academician PALLAS has diſcovered during his reſidence in the neighbourhood of the river JENISEI. This very remarkable and huge lump is of a ſpungy texture, of the moſt perfect and malleable iron, whoſe cavities are cloſely filled with ſmall poliſhed pieces of hyacinthine ſpar, ſome round, ſome with flat ſurfaces, and all of the colour of tranſparent amber.

(*b*) The maſs, in its preſent ſtate, weighs 152 Ruſſian pounds.

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The maſs (of which ſome conſiderable pieces have already been received) is ruſty only on the ſurface; but the interior has been preſerved by a kind of black varniſh that is ſpread all over the iron, which is of an irregular form blunted at the corners.

This iron may be bent and hammered when cold, and, when moderately heated, may be ſhaped into nails and other tools; but, in a violent heat, and eſpecially if in order to ſeparate it from the ſparry particles it is thrown into ſmelting ovens, it becomes brittle, granulated, and will not join again in the forge.

This maſs was found lying on the ſurface, at the top of a high woody eminence, not far from the mountains called, by the Tartars, NEMIR, between the two rivulets Ubei and Siſim, which fall from the right into the JENISEI, a little below Abakanſkoi Oſtrog, and ſcarce 100 fathom from a rich mine of hard ore of loadſtone.

The appearance and nature of this maſs, and the qualities of the iron, of which it chiefly conſiſts, are ſo deciſive, that it cannot be doubted but that it has been thus produced by Nature; and if ſo, the exiſtence of native iron, which has hitherto been queſtioned, is eſtabliſhed beyond all contradiction; eſpecially if it be conſidered, that no trace of any old iron work, of which there are many in the Siberian mountains, is to be met with in the deſart where the maſs was found; and that the mine abovementioned was not opened before the year 1752, when the miners, who were there employed, firſt diſcovered this maſs of iron: ſince which time no further notice had been taken of it.

XLVI. *Of*

XLVI. *Of Torpedos found on the Coast of* England. *In a Letter from* John Walsh, *Esq; F. R. S. to* Thomas Pennant, *Esq; F. R. S.*

Chesterfield-street, June 23, 1774.

DEAR SIR,

Redde, June 23, 1774. TO the Author of the British Zoology it will, I am persuaded, be no unwelcome information, that the Torpedo, or Electric Ray, frequents the shores of this island, contrary to a received opinion among Naturalists, who have in general considered it as an inhabitant only of warmer climates.

In consequence of inquiries I had set on foot in some of our southern fishing ports, two Torpedos, taken in Torbay, one in the beginning of August, and the other in the beginning of November, last year, have been actually sent up to this metropolis. The first, procured for me by the good offices of Mr. Amyatt, Apothecary in Berkeley Square, arrived during my absence; but it was examined, and the electrical organs were successfully injected, by Mr. John Hunter. The second, forwarded to me by Mr. Grant, a principal Fishmonger in the Land-carriage branch, then at Brixham, came up very fresh and perfect, in one of his fish-machines. This

I weighed

I weighed and meaſured before it was touched by the diſſecting knife, and found it to weigh fifty-three pounds avoirdupois, and to meaſure four feet in length, two feet and a half in its extreme breadth, and four inches and a half in its extreme thickneſs.

The largeſt Torpedo I met with in the neighbourhood of la Rochelle, where upwards of ſeventy paſſed through my hands, weighed little more than ten pounds, and meaſured not quite two feet in length, nor quite ſixteen inches in breadth: and the largeſt I have read of is that mentioned by Rhedi to Lorenzini [(a)], weighing twenty-four pounds, without doubt of Leghorn, which make about eighteen avoirdupois. Though this Mediterranean Torpedo has been ever conſidered as of an extraordinary ſize, it is exceeded in weight nearly three to one by our enormous Britiſh Torpedo.

The back of it was of a dark aſh-colour, with ſomewhat of a purple caſt, but not at all motled like thoſe of the Atlantic Coaſt of France, nor regularly marked with eyes, as they have been called, like ſome found in the Mediterranean. Its under part was white, ſkirted however with the ſame aſh-colour, which towards its tail became almoſt univerſal. The ſide fins being a little contracted and curled up, prevented the preciſe meaſurement of its breadth, but it appeared to hold the general proportion obſerved in thoſe of la Rochelle; that is, the breadth was two-thirds of the length [(b)]. Its electric

(a) Lorenz. Oſſ. intor. alle Torp. p. 4.

(b) The breadth of the Ray-Torpedo of Brazil is, according to Marcgrave, juſt one-third of the length, falling in with the proportion of the *Squatino-Raia*. See Marcg. Lib. iv. Cap. 6.

 organs

organs likewiſe were proportionate with theirs, each organ meaſuring fifteen inches in extreme length, and eight in extreme breadth. In ſhort, the Torpedo of Torbay no way differed from thoſe I ſaw in the Bay of Biſcay but in ſize and colour; and perhaps this difference may be thought rather caſual than denoting a ſpecific diſtinction.

It was a female, without any ſigns of pregnancy. The inteſtines contained, with ſome black ſlime, two vertebres of a fiſh, ſeemingly of the cod kind. The electrical organs of this Torpedo were likewiſe injected by Mr. Hunter, though not with his firſt ſucceſs, from the burſting of the artery in the operation; he determined, however, the number of columns in one organ to amount to 1182, and fully confirmed the obſervation he formerly made, that their numerous horizontal partitions were very vaſcular.

Shall I take notice that the reſt of this fiſh was dreſſed and brought to table, and that ſome of my friends ſuffered a little for their curioſity in taſting it? Rondelet ſpeaks unfavourably of the Torpedo as food; and tells us, that at Venice the Prefect of Health forbids it to be ſold in the market. But to deem it wholeſome diet, we have the ſanction of Hippocrates as well as Galen; and if forbid at Venice when Rondelet was there, in the markets of France I know it to be ſold. The truth is, that the electrical organs, which make one half of the animal, are, though wholeſome to be eaten, an inſipid mucilage; but its muſcular part is, at leaſt, as palatable as the fleſh of the other Rays: among theſe the old and overgrown are ever in little requeſt. Our Torpedo was

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was doubtleſs ſuch; and we muſt beſide confeſs that, as a rarity, it had been kept too long.

The firſt Torpedo was not weighed; but Mr. Hunter, who examined both, aſſured me, that they appeared to him equal in ſize, and alike in every other reſpect; indeed their electrical organs, preſerved to this hour, ſhew them to have been of the ſame magnitude. The firſt was a female likewiſe, and not pregnant.

The following accounts were received from Brixham, concerning Torpedos found in that quarter.

From Mr. Philip Lyde, Surgeon at Brixham, who forwarded the firſt, on the 4th of Auguſt, 1773.

"I ſend a Torpedo, or Numb-fiſh, which had, "when living, the greateſt effect, as I am informed "by the men who caught it. There have been "three taken this week. The one ſent ſeems to be "of the largeſt ſize."

In an earlier account, dated the 23d June, it is mentioned that one or two are caught there every week.

From Mr. Grant, fiſhmonger, who ſent the ſecond Torpedo, on the 3d November, 1773.

"The Torpedo, or Numb-fiſh, is by no means "plenty in theſe parts, as it rarely happens that "more than one is taken at a time; nor can the "proper ſeaſon for catching them be aſcertained, as "they have been ſeen at all times of the year. The "uſual depth of water in which they are caught is "from thirty-ſix to forty fathom; and being of the "Ray kind, they are commonly taken with them. "As to the time when their young are to be ſeen, no "ſatisfactory information can be obtained; but it is "imagined that the ſeaſon for them and the Rays is "the ſame. Their numbing quality is pretty ſtrong "through the net, though much weaker than when "they are taken out. The general name by which "they are known here, is the Numb or Cramp-fiſh. "Few or no ſmall Torpedos are to be met with in "theſe parts, thoſe hitherto caught being from ten "to ſixty, ſeventy, and eighty pounds weight; "which may probably proceed from the young "being thrown away promiſcuouſly with other offal "fiſh."

At la Rochelle, during the intire month of July, moſt of the larger female Torpedos, being thoſe from fifteen inches to two feet in length, were found with an uncertain number of eggs in their matrices, the largeſt females appearing to have the greateſt number. The eggs of the ſame female differed little from each other as to forwardneſs; nor did the eggs of one female differ much from thoſe of another: the embryo in all was but little advanced. A letter from M. Saunier of la Rochelle, written about ſix weeks after I had left that place, informed me, that on the 10th September, he opened a very large Torpedo of about two feet and a quarter by one and a half, and diſcovered floating in the left matrice nine fetuſes quite formed, near two inches long, and diſtinct from them nine eggs in no ſtate of forwardneſs; and that in the right matrice he met with four ſuch fetuſes and nine ſuch eggs. The obſervation, therefore, of Ariſtotle, that the Torpedo brings forth at the autumnal equinox, is well founded, notwithſtanding Lorenzini has queſtioned the fact.

From the remarkable ſuperfetation in this inſtance, we may beſides infer, that the Torpedo, agreeably

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to Ariſtotle's information, concerning the cartilaginous fiſh in general, goes with young near ſix months; as the eggs here found ſeem, from their being ſo little advanced in ſo late a time of the year, to be deſtined for the ſpring-brood. Lorenzini's diſſections of female Torpedos big with eggs in February and March, and of others without any in April, ſhew that they produce about the vernal as well as autumnal equinox. This was confirmed by the many young Torpedos we met with in the month of July, from four and a half to ſix inches long, which were evidently the offſpring of that year. Among theſe, however, was one, taken on the 4th July, weighing one ounce, and meaſuring four inches and a half by three, which had ſtill its native yolk unconſumed in its abdomen. By this circumſtance it appears, that there may be a production likewiſe at Midſummer; but our general obſervations led us to conclude, that it principally takes place at the two equinoxes. I have dwelt the longer on the ſeaſon in which foreign Torpedos bring forth, as we are yet entirely ignorant on that point with regard to thoſe of our colder climate, and as this knowledge may, in ſome degree, aſſiſt us in knowing when and where to look for the animal. Moſt fiſh, we are told, approach the ſhore in the ſummer ſeaſon; and the Torpedo will doubtleſs then, in queſt of food, and of a warmer element, both for itſelf and for its young, haunt the ſhoal water. In the cold and tempeſtuous part of the year, we ſhall probably find it in a deeper ſea, which then affords it the more mild and ſtill retreat.

My learned friend Dr. George Baker has procured for me a drawing and ſome account of a Torpedo,

taken

taken about five years ſince in Mount's Bay in Cornwall. The particulars, as they were lately collected by Mr. Scobell of Penzance, from the fiſherman who took it, are theſe: "that he judged it on memory to have weighed about forty pounds, and to "have been about three feet long and two and a half "broad; that its ſkin was ſmooth like an eel, dark-"brown on the back, and white underneath; and "that it had been caught with a large hook, in the "month of March, on ſandy ground."

For the ſake of Oppian's fine deſcription of the capture of the Torpedo, which Claudian has elegantly paraphraſed, and which you, ſtruck with the paſſage, have rendered in Engliſh metre, I am glad the Torpedo is found to take the hook. That deſcription, though thought to have been fabulous, proves to be no leſs juſt in each circumſtance, than, as it was always confeſſed to be, poetically beautiful in the whole.

But to catch with the greateſt ſucceſs the Torpedo, as well as other flat fiſh, which keep near the ground, the trawl, or drag-net, muſt be uſed. This kind of fiſhing is much practiſed in Torbay; but in Mount's Bay, as I am informed, not yet adopted; which may be the chief reaſon why the Torpedo has but ſeldom been taken at the one, and ſo very frequently at the other place.

I had an opportunity laſt autumn of making a ſhort viſit to Dungarvan in Ireland, where Smith in his Hiſtory of Waterford, as you have remarked, mentions a Torpedo of ſix or eight pounds to have been found about thirty years ſince. The fiſhery there, which is very conſiderable, is wholly carried on with hook and line, and the fiſhermen were entire ſtrangers to the Torpedo. But at Ring, a fiſhing village in the neighbourhood, furniſhing about fourteen or fifteen ſmall veſſels, with which they practiſe pole-trawling, the fiſhermen were acquainted with it. They told me, that they ſometimes caught one or two in a year, and ſometimes none for two or three years; that they had taken two that year, and one the year preceding; that theſe were about eighteen inches long, and fourteen broad, and were caught a league off ſhore; that the fiſh benumbs thoſe who touch it, and that they had been benumbed by it; and that its name in Iriſh is *Aunghelláw*. From this name it might ſeem that they confounded it with the Angel-fiſh: but by their deſcription, both of the animal and its effects, it was plain they knew and meant the Torpedo.

To prove the electrical effect of the animal, the curious ſhould be informed, not only where to find, but how beſt to preſerve it alive. The Electric Ray is ſo far amphibious as to exiſt in air twenty-four hours: in freſh water it ſurvives but little longer. Well-boats kept in ſalt water, and not put into much motion, may beſt ſuit it: In reſervoirs on ſhore, which on ſea-coaſts are not unfrequent, it will be ſubject to be annoyed, notwithſtanding its electrical armour, by the Sea-leach and the common Sea-crab. A commodious well-boat we experienced might ſoon be made from a ſmall flat-bottomed boat, termed by the French *Pouſſe-pied*; which, partitioned with laths into three or four chambers, and ſecured in the ſame manner at the top, kept floating, from its leaky ſtate, juſt even with the water. In this pen we were able to preſerve them ſeveral days, and always without food; for though in the ſtomach of

theſe

these animals, when taken, a Plaice or Surmullet may, as we have seen, be found, yet in confinement they neglect all kinds of prey: it will be easy however, as they are of a quiet nature, to force them to swallow food, if necessary.

But the frequent, and perhaps favourite situation of the Torpedo is to lie in concealment under sand. If he be placed by design, as he is sometimes left by accident, in any hollow of a sandy beach, from whence the tide has just retired, he swims to that brink where the water is still draining away, and on finding himself unable after repeated attempts to push himself over the shallow, and follow the course of the tide, he begins with admirable address to bury himself in the sand, and by a gentle but quick flapping of his extremities all round soon sinks himself a bed, and in the action throws the sand in a light shower over his back. Neither the animal nor the spot he is in can now be distinguished; save only that, on a nice search, his two small inspiratory foramina, and their membranes at play, may be perceived. It is in this situation that the Torpedo gives his most forcible shock, which throws down the astonished passenger who inadvertently steps on him.

I have thus shewn that Great Britain too claims the Torpedo, or Electric Ray; that ours is the *broad marine* sort, which Socrates, as Meno thought, resembled; and that it is the *black Torpedo*, whose influence subdues obstinate Head-achs, and the Gout itself [(c)]. In announcing to our Naturalists and

(c) Scribonius Largus, Cap. 1. & 41. See also several of the early physicians, Roman and Arabian, for different cures attributed by them to the effect of the Torpedo.

Electricians

Electricians the presence of this wonderful guest, I should certainly felicitate our Invalids on their acquisition, but that *the Leyden phial contains all his magic power.*

I remain, with true esteem,

DEAR SIR,

Your affectionate and obedient servant,

JOHN WALSH.

XLVII. *Description of a double* Uterus *and* Vagina. *By* John Purcell, *M. D. Professor of Anatomy in the College of* Dublin. *Communicated by Dr.* Morton.

Dublin, April 7, 1774.

Redde, June 23, 1774.

LAST summer the body of a woman, who had died in labour in the ninth month of her pregnancy, was dissected at the Anatomical Theatre of Trinity College. Upon opening the *abdomen*, an *uterus* appeared of such a size and form as are generally observed at that period. It contained a full grown *foetus*; but was furnished with only one *ovarium* and one *fallopian* tube, which were situated on the right side. On the left was placed a second *uterus* unimpregnated and of the usual size, to which the other *ovarium* and tube were annexed. But these two *uteri* were totally distinct and separated from each other, except at the lower extremity of their necks, where their union extended a quarter of an inch, and an acute angle was formed between them. There was nothing extraordinary in the formation of the external parts of generation; but from each side of the *meatus urinarius* a membrane ran downwards; and

and the two, having comprehended this orifice between them, were joined together a little below it, ſo as to form, by their union, a *ſeptum* or *mediaſtinum*, which taking the remainder of its origin from all that prominent ridge called the ſuperior *columna*, and deſcending perpendicularly, was inſerted into the inferior *columna*, ſo as to extend from the entrance of the *vagina* as far backward as its poſterior extremity, and thus to divide it into two tubes of nearly equal dimenſions. But each of theſe did not lead ſolely to the womb of its own ſide; for the right *vagina* became gradually wider as it ran backward, and at laſt was ſo far dilated as to comprehend, within its circumference, the orifices of both *uteri*; while that on the left ſide, having taken an oblique direction, ended in a *cul de ſac*, or *cæcum*. Such a conformation might have rendered it totally uſeleſs: to prevent which, Nature, fertile in expedients, ſeems to have had recourſe to a very extraordinary contrivance. This was a *fiſſure* in the *ſeptum*, an inch in length, and about an inch diſtant from the womb of that ſide. Although its circumference was perfectly ſmooth, we muſt acknowledge that it might have ariſen from an accidental rupture of the *ſeptum*; the lips of the wound not uniting, and, in proceſs of time, becoming callous; and yet, I imagine, that the parts were originally formed in this manner, in order to preſerve a communication between the two *vaginæ*.

Thus it appears, that both *uteri* might be impregnated through either *vagina*, as that on the right ſide led directly to both; and as, by means of the *fiſſure* in the *ſeptum*, the *ſemen* could eaſily be

 thrown

thrown from the left *vagina* into the right, where the apertures of the two wombs were placed. Through the latter paſſage both *uteri* would ſeem to have an equal chance for impregnation; for, notwithſtanding that which contained the *foetus* was placed almoſt directly in a line with the axis of the right *vagina:* yet this probably was not its original poſition; but by degrees its bulk increaſed ſo much as neceſſarily to occupy the middle ſpace, and puſh the unimpregnated one aſide. But, however ſurprizing it may ſeem at firſt view, yet there is reaſon to imagine, that the right womb, though at a greater diſtance, would be much more apt to conceive than the other, if the left *vagina* only had been made uſe of. For when this was diſtended, it appeared that the poſterior part of the *ſeptum*, by its protuberance, cloſed up and covered the left *os tineæ*; and, as ſuch would probably be the caſe in copulation, the *ſemen* not finding a ready admiſſion into it, would paſs over to the right orifice, where its entrance could not be ſo much obſtructed. So that, if we may hazard a conjecture, I think it more likely, ſince the right *uterus* alone conceived, that the left *vagina* had generally been employed.

It was a prevailing opinion among the ancients, that male children were conceived in the right ſide of the womb, and females in the left. Having ſo few opportunities of diſſecting human ſubjects, they depended too much on the analogy of the ſtructure of brutes, which has been the principal ſource of the many erroneous deſcriptions which we meet with in their works. It is well known that the *uterus* of many quadrupeds is divided into two *cornua*, in which the *foetus*' are lodged;

lodged; and it was not very abſurd to conclude, that Nature might have formed them for the diſtinct repoſitories of the two ſexes. Accordingly this was ſuppoſed to take place in the human *uterus*, which has been deſcribed and delineated as if diſtinguiſhed into two chambers. Hence aroſe the opinion, which is received in ſome places to this day, that a very ſure prognoſtic, with regard to the ſex of the child, may be drawn from the ſide of the belly on which the tumour is more ſenſibly felt. Diſſections being now more frequent have proved, that the human womb generally has only one undivided cavity; ſo that the *foetus*, let it come from which tube it may, will, when arrived to a certain ſize, occupy it entirely. This obſervation, however, is not ſufficient to refute the ſuppoſition that each ſex might have its peculiar *ovarium*; and ſome authors pretend, that they are able to determine how many males or females any animal has brought forth, by examining the number of *cicatrices* on its *ovaria*. For, when females only had been produced, the right *ovarium* was found ſtill full of *veſicles*, but the left quite exhauſted. That this is not always the caſe in brutes, appears from the obſervation of Dr. HARVEY, who frequently found male *foetus'* in the left *cornu*, and females in the right. In the human ſubject, opportunities of aſcertaining this matter muſt occur very ſeldom. We have an inſtance, recorded by CYPRIAN, where both a boy and a girl were conceived, although the right tube was wanting. But the preſent caſe affords another example, which is deciſive; for here the impregnated *uterus* had not the ſmalleſt communication with the left *ovarium* or tube, and yet it contained a female *foetus*.

The

The *ſeptum* was not merely membranous, but fleſhy, and of a conſiderable thickneſs; and, like moſt other *mediaſtina* in the human body, conſiſted of two *laminæ* combined. Of theſe each *vagina* furniſhed one; for each had its own *conſtrictor*, and, being completely ſurrounded by muſcular fibres, had a power of contraction independent on the other; which could not be effected if both *vaginæ* were comprehended within the ſame muſcular rings, and ſeparated by a membrane incapable of action.

It has been the opinion of many modern authors of the firſt reputation, that the *fundus* is that part of the womb, whoſe extent increaſes, in the greateſt proportion, during pregnancy; and upon this ſuppoſition, they have founded various theories. One of the principal arguments which they propoſe, in ſupport of their opinion, is, that the inſertion of the fallopian tubes is removed from the angles of the *uterus*, and gradually deſcends towards its neck, ſo that a ſhort time before delivery they are at a very great diſtance from their former poſition. HALLER does not attempt to deny theſe facts; but mentions three inſtances where the tubes did not change their place. But PETIT, in his *Memoire* on the cauſe and mechaniſm of child-birth, is clearly of opinion, that the whole doctrine is deſtitute of foundation. He aſſerts, that the *fundus* increaſes leſs than any other part, and that the ſurpriſing growth of the womb is effected by freſh ſupplies of fibres, ſucceſſively furniſhed by the neck and parts adjoining. As a deciſive proof, he inſiſts, that the inſertion of the tubes continues nearly in the ſame place, and accounts for the error of the abovementioned

tioned authors by obſerving, that as the *fundus* is puſhed upwards by the growth of the other parts, a greater portion of the tubes will adhere to the ſurface of the womb, and thus the apparent place of inſertion be very far diſtant from the real one. This remark is verified in the preſent inſtance; for the tube at firſt ſight appears to penetrate into the middle of the *uterus*; but, upon a cloſer inſpection, and by introducing a briſtle, it is found to run for a conſiderable ſpace between it and the coat, which it receives from the *peritonæum*, and at length to enter into its cavity not very far from the ſpot which it may be ſuppoſed to have occupied before impregnation.

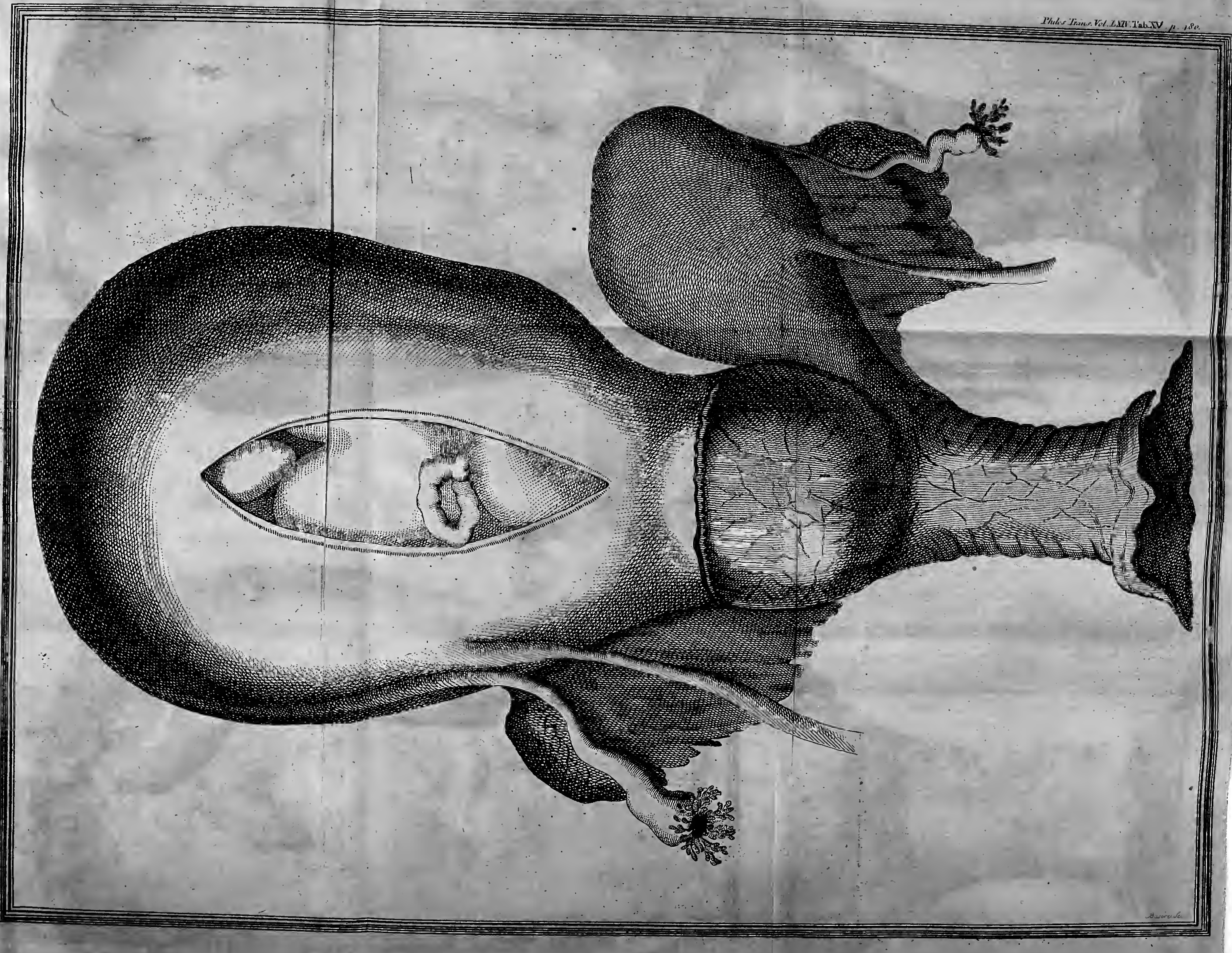
With regard to ſuperfœtation, it is evident how eaſily it might have been effected in the preſent ſubject; and the ſuppoſition of a double *uterus* can readily account for it on many other occaſions. But this is a matter on which it would be needleſs to dwell any longer, as it has been very fully treated in GRAVEL's Diſſertation, publiſhed in HALLER's Collection; where we meet with a ſimilar inſtance of two *uteri* and a *vagina*, the anterior part of which was divided by a *ſeptum*; but whoſe poſterior portion was ſingle, where the *ſeptum* was diſcontinued. HALLER, in his *Opuſcula Pathologica*, gives the hiſtory of a young lady of quality who had two wombs, each of an oval ſhape, and furniſhed with its own peculiar *vagina*. One of theſe *vaginæ* was anterior, and communicated with the right womb; the other was poſterior, and led to the left. And it is worth obſerving, that in theſe two caſes, and in moſt others of the ſame kind, which have been hitherto obſerved, each *uterus* had only one *ovarium* and one tube.

A double

A double *uterus* is deſcribed by O. ACREL, in a treatiſe printed at Stockholm, in 1762; and in the ſeventh volume of HALLER's *Elementa Phyſiologiæ*, various authors are referred to, who deſerve to be conſulted upon this ſubject. In ſome of theſe we find examples of two wombs, or one *uterus*, divided into two *cornua*. In other inſtances the *uterus* retained its proper external appearance, although it was really double, its cavity being divided by a *ſeptum*.

Since, therefore, it is certain that, in the ſtructure of the parts of generation, Nature frequently deviates from her ordinary courſe, practitioners in midwifery ought to conſider how many difficulties they may, perhaps, be expoſed to by not attending to the poſſibility of ſometimes meeting with thoſe organs formed in the ſame manner as in the ſubject of this eſſay. An attention of this kind would probably have been of the utmoſt conſequence in the preſent caſe; for the orifice of the unimpregnated *uterus* was ſo far dilated, as eaſily to admit two fingers, which might have ariſen from the attempts of the midwife to bring on delivery: nor can we conceive any thing more vexatious than ſuch a caſe would prove, were it to fall into the hands of an inexperienced perſon; as the orifices of the different wombs preſenting themſelves alternately to his touch, he might entertain doubts of the pregnancy of his patient, even when her labour was approaching; and, by endeavouring to dilate the left *vagina*, all his efforts to promote delivery would only ſerve to render it more difficult, or perhaps impracticable.

XLVIII. *A Letter*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XLVIII. *A Letter from* William Brownrigg, *M. D. F. R. S. to Sir* John Pringle, *Bart. P. R. S. relating to ſome Specimens of native Salts, collected by Dr.* Brownrigg, *and ſhewn at a Meeting of the Royal Society, June* 23, 1774 *.

SIR,

Redde, June 23, 1774.

I BEG leave, through your hands, to convey to the Royal Society, ſpecimens of various ſalts, which I have found native in the coal-mines near Whitehaven; and which, I flatter myſelf, may be of ſome uſe in aſſiſting us to form a right judgement concerning the generation of theſe ſalts, and the various ingredients that enter their compoſition.

The firſt of thoſe native ſalts which I offer to the Royal Society, is the *ſal catharticus amarus*, or the *bitter ſalt*; which, though it was only firſt diſcovered by Dr. GREW in the Epſom waters ſo lately as towards the cloſe of the laſt century, is found in more abundance, and more univerſally diffuſed over this globe than any other ſalt, the common ſalt

* Theſe ſpecimens were afterwards depoſited in the Britiſh Muſeum. S. HORSLEY.

 excepted.

excepted. The waters of the ocean are known to be ſtrongly impregnated with this ſalt, and from it to acquire their bitter taſte. It has alſo been diſcovered in many ſprings, in almoſt all known parts of the world; many of which are thereby rendered unfit for domeſtic uſes, though highly ſerviceable in medicine. Many lakes contain large quantities of this ſalt; as the lake Aſphaltus, and ſome of the lakes in Siberia [(a)]: and laſtly, it will appear from the ſpecimens of this ſalt here produced, that it continually germinates from ſtones, and other ſubſtances lodged in the bowels of the earth; from which ſubterraneous ſtores, not only the ſprings and lakes, but alſo the ocean itſelf ſeems, in part at leaſt, to be ſupplied with it.

In Partition N° 1. are various ſpecimens of this bitter ſalt, which I found germinating, in great abundance, from a whitiſh-grey free ſtone, in many parts of the colliery of Howgill near Whitehaven. It there grows, or ſhoots out from this ſtone, in very fine and tender filaments, which are white and ſhine like poliſhed ſilver. Theſe filaments are of various lengths: ſome I have ſeen three inches long; and they are often ſet ſo cloſe to each other as to adhere together into one maſs. They are often very pure, but ſometimes are intermixed with minute effloreſcences of green vitriol; which ſalt alſo germinates in great abundance in the ſame colliery. The green vitriol, which is extracted from the martial pyrites found among the coal in the ſame mine, has alſo ſometimes mixed with it a ſmall portion of this bitter ſalt, as I learned from experience, having

(a) Gmelin.

frequently

frequently taken large quadrilateral cryſtals of this laſt mentioned ſalt out of the ciſterns in which a ſtrong *lixivium* of green vitriol had been ſet to cryſtallize at the copperas works near Whitehaven.

And here it may be proper to remark, that the large beds of freeſtone found in the coal-grounds near Whitehaven, and in moſt other places where there are veins of coal, though ſuch ſtone is often very hard when dug out, and does not ſeem liable to decay while lodged deep in the earth, yet will ſeldom endure the weather; but, when expoſed to the open air, gradually moulders into duſt, and is therefore unfit for buildings. Which decay of theſe ſtones ſeems owing chiefly to the germination of this bitter ſalt, and ſometimes alſo to that of vitriol; but ſeldom to the formation of nitrous ſalts, as has been moſt commonly ſuppoſed.

Partition II. N° 2. Bitter ſalt, from the ſame colliery, in a concreted form, found lying in ſmall vacuities towards the top of ſome large pillars of coal, there left about forty years ago, to ſupport the roof of the mine. The ſalt has been generated from the whitiſh grey freeſtone, of which this roof is formed, and ſeems compoſed of many ſmall filaments (the ſame as N° 1.), that have gradually ſhot out from their ſtony *matrix* ſo cloſe to each other, as to unite into one ſolid maſs.

N. B. Various other kinds of ſalts, formed by germination, aſſume this fibrous texture, in like manner with this bitter ſalt, as will appear from the ſpecimens of green vitriol and alum here exhibited, which I collected from the ſame colliery of Howgill.

Partition III. N° 3. The ſame bitter ſalt concreted into ſmall ſolid and tranſparent maſſes, of an irregular form, in which ſtate it is found in great quantities, in many parts of the collieries of Howgill and Whingill, near Whitehaven, in old works which have been deſerted fourſcore or a hundred years. Specimens of the native bitter ſalt, in this form, I ſent to Sir Hans Sloane thirty years ago.

Partition III. N° 4. (*a*) The native bitter ſalt (N° 3.) depurated by ſolution in water, and decantation of the clear *lixivium* from the coal and other heterogeneous ſubſtances that had ſubſided therein; which clear *lixivium*, after having been brought to a due ſtrength by coction, and then ſet in proper veſſels, gave cryſtals, of which theſe here exhibited are a ſpecimen.

Partition III. N° 4. (*b*), Specimen of the ſame depurated ſalt, formed into ſomewhat larger cryſtals, in repeating the foregoing proceſs.

The figure of theſe cryſtals is that of a quadrilateral column terminating, at the ſummit, in a quadrilateral pyramid. At the baſe they uſually appear broken off from the veſſel or other body to which they adhered during their formation. The four plain ſides of the columns meet always at right angles, and in the ſlender cryſtals that firſt ſhoot, are of equal dimenſions; but in the larger cryſtals (which ſeem compounded of ſmaller ones) two of the ſides, that are oppoſite to each other, are often greater than the two other ſides. The four triangles, that ariſe from the four ſides of the column, moſt commonly differ from each other in form and in magnitude, and therefore compoſe irregular pyramids: and ſometimes theſe cryſtals, inſtead of ending at a

 point,

point, terminate in a ridge formed by two inclined planes, which arife from the oppofite and parallel fides of the column.

Thefe cryftals are perfectly clear, and of a fine deep water, and may be preferved in this ftate many years, by wrapping them up loofely in paper fo as to keep them from being fullied by duft and other impurities. In this manner the cryftals now prefented to the Royal Society have been kept twenty years; and in all that time have not parted with any of the water of their cryftallization fo as to turn white and powdery; neither have they deliquefced by attracting the moifture of the air. Indeed none of the falts, formed by germination, that I have feen are fubject to diffolve in a moift air; on the contrary, the falts, fo produced, fhoot moft vigoroufly in a clofe and moift air; a certain portion of moifture being required in their formation. Some part however of this moifture feveral of thofe falts readily loofe when expofed for a confiderable time to the open air. The *Aphronitrum* or *fal murarius* (which is a fixed *alcali)* afforded me an inftance of this kind. For, having depurated a confiderable quantity of this falt, and reduced it into *rhomboidal cryftals* of a very regular form; on examining thefe cryftals, after I had kept them ten or twelve years in a phial that was corked, though not with great exactnefs, I then found them dry, and in part reduced to powder fo as to have loft their tranfparency, and in a good meafure their proper figure. The green vitriol is alfo apt to grow rufty, and to lofe fome of its water in the open air, though its acid, when pure, attracts moifture more greedily than, perhaps, any other faline body.

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The native bitter ſalt of the coal-mines, purified as before related, I have found exactly to agree in the form of its cryſtals, as alſo in its cool bitter taſte, its purgative quality, and other properties, with that ſalt which is commonly ſold in the ſhops, under the name of Epſom ſalt, when this laſt named ſalt had been ſufficiently purified and reduced into larger cryſtals. This native bitter ſalt alſo agrees, in all its qualities, with a ſalt (which I extracted from the thick and ponderous liquor known by the name of bittern) that remains in the ſalt-pans after the common ſalt is ſeparated from ſea water by coction. It is alſo the ſame with the ſalt of the Scarborough waters, as deſcribed by Dr. SHAW, which is ſold at a great price under the name of Scarborough ſalt. Moſt of the other purging ſalts, ſold under the names of the ſeveral waters from which they are extracted, ſeem to be the ſame with this bitter ſalt; but differ as they are formed into greater or ſmaller cryſtals, or according to their purity; there being many purging waters which, beſides this bitter ſalt, hold common ſalt, the muriatic calcarious ſalt, natron, and other kinds of ſalt.

Of the ſeveral kinds of bitter ſalt abovementioned I have ſelected a few ſpecimens from thoſe which I have in my poſſeſſion.

Partition IV. N° 5. The common Epſom ſalts of the ſhops, purified in the following manner. The Epſom ſalt was diſſolved in water, and the *lixivium*, after ſtanding ſome time, was depurated from a large black ſcum which aroſe to its top, and from a dirty ſediment; it was then, by gentle coction, freed

from

from a large quantity of common ſalt, part of which aroſe to the top of the *lixivium* in ſmall grains, and was thence ſkimmed off, and part of it ſubſided therein in larger grains. The *lixivium*, when thus purified, and reduced to a proper ſtrength by gentle boiling, was ſuffered to ſhoot into ſmall cryſtals in proper veſſels, and the liquor that remained (which was a ſolution of the ſame ſalt, mixed with a conſiderable quantity of the muriatic calcarious and ſome common ſalt) was poured from it.

N° 6. Large cryſtals of depurated Epſom ſalt, which ſhot out from a fully ſaturated ſolution of this ſalt (N° 5.) made by pouring boiling water thereon, and ſuffering it to ſtand till cold.

N° 7. More regular cryſtals of the ſame ſalt.

N° 8. Bitter ſalt well purified, which I extracted from the marine bittern of the Salterns near Whitehaven, in which common ſalt is ſeparated from ſea water by coction.

The bitter ſalt, purified in this manner, is an excellent remedy in many diſeaſes, and may be prepared at the Britiſh Salterns, for foreign as well as for home conſumption, ſo as to be afforded at a very moderate price, I therefore heartily recommend it to be kept in the ſhops in this purified ſtate, inſtead of the common Epſom ſalt now every where ſold; which laſt, on account of its great impurities, is deſervedly grown into diſrepute.

N° 9. A ſalt which I extracted from the ſame marine bittern (N° 8.). Its cryſtals are very ſingular, being of a rhomboid and cuneiform ſhape, and are very conſiſtent in common air. Their taſte ſeems more bitter than that of the common bitter ſalt.

N° 10.

N° 10. Smaller cryſtals of the ſame ſalt with N° 9. Theſe are, moſt of them, of a rhomboidal figure.

N° 11. Salt of the Scarborough water, which I puchaſed, about twenty years ago, from an apothecary of that place, who there prepared conſiderable quantities of it for ſale. It does not differ from the purified Epſom ſalt (N° 7.), and that bitter ſalt which I extracted from the marine bittern (N° 8.)

N° 12. Several ſpecimens of native green vitriol, from the coal-mines near Whitehaven. I found this vitriol in the colliery of Howgill, lying in great plenty in the joints or openings of the pillars of coal that had been left to ſupport the roof of the mine, and in a part of it from whence the coal had been dug away about forty years ago. The vitriol is found in places to which the air ſeems to have had free acceſs, and the coal near it commonly appears in a looſe and crumbling ſtate.

N°. 13. A curious ſpecimen of the ſame green vitriol with the foregoing (N° 12.) which may ſerve to explain how it happens that the various ſpecimens of this native ſalt, and of ſeveral other ſalts here exhibited, aſſume a fibrous appearance. The ſaline germinations in this ſpecimen ſhoot out, or grow from the pyrites, pretty cloſe to each other, and in ſeveral places are united together into *faſciculi* or cluſters; but in other places there are many openings, or vacuities, which, had it remained in its native ſituation, would, moſt probably, have been filled up by other filaments ariſing from its *matrix*, and the

whole

whole ſaline concrete would have been reduced into one compact body, of a fibrous texture.

N° 14. Several ſpecimens of the ſame green vitriol, which are more cloſe and compact than the foregoing ſpecimens (N° 13.); and in which the fibrous texture of the ſalt, thus generated by germination, is more apparent than in the ſpecimens (N° 12.)

N° 15. Green vitriol germinating from martial pyrites.

N° 16. Sundry ſpecimens of pyrites, with green vitriol adhering thereto, or lodged in its crevices, where the ſalts ſwelling, or continually increaſing in bulk, act as wedges, and moulder the pyrites into powder. The pyrites, in this decayed ſtate, appears black, from its bituminous and earthy parts remainingafter its ſaline principles have been ſeparated from it. Though in the ſpecimens here exhibited, ſome foſſil coal may have been intermixed with the pyrites. The miners have called this decaying of the ſulphurous ores of copper, iron, and other metals and ſemi-metals, the *weathering* of theſe ores, having obſerved that this change or decompoſition of theſe ores is brought about by the operations of common air, and of the watery moiſture to which they have been expoſed. For the *pyritæ* and other ſulphureous ores remain without change in the bowels of the earth, while lodged in places, where neither the air nor water can act upon them.

N° 17. Native alum from the coal-mines near Whitehaven. In theſe ſpecimens the alum is found adhering to the ſtone, from which it ſhoots out, in very ſmall white and ſhining filaments, which have a flocculent or downy appearance, and is therefore the ſame ſalt which was heretofore known under the name of *alumen plumoſum*. Some kinds of *aſbeſtos*, from their reſemblance to this ſalt, have alſo, though very improperly, obtained the name of *alumen plumoſum*. This ſalt has the diſtinguiſhing taſte of alum, though ſomewhat more harſh and rough, owing either to ſome ſmall mixture of vitriol of iron, which ſeems to ſhoot out along with the alum; or elſe, becauſe moſt of the alum prepared for ſale has urine, kelp, and other mixtures, added to the *lixivium* during its coction, which may render the ſalt ſo prepared leſs harſh and ſtyptic than the native alum. The ſtone on which the native alum, here exhibited, germinates is black and ſhining, and has ſo much *bitumen* mixed with it, that it burns ſlowly, and leaves a white aſh, when ſet on fire in large heaps. N. B. From the top of theſe heaps, I have collected conſiderable quantities of brimſtone that was ſublimed from this bituminous ſtone, while burning in this ſlow manner.

N° 18. Large pieces of the ſame native alum, very pure; on the outſide it has got a yellowiſh caſt by being expoſed two years to the open air ſince it was taken from the mine; when broken, it appears on the inſide more ſhining, and has a blueiſh caſt.

N° 19. An earth found in conſiderable quantities near the above ſpecimens of alum. From its extremely harſh, rough, and ſtyptic taſte, like that of burnt alum, it ſeems to be an aluminous earth. It

3 may

may alſo contain ſome ſmall minture of ochery earth, which may give it its yellow caſt.

N° 20. A ſhining kind of ſtony clay, called by the miners *ſill*, lying in large beds in coal grounds, it ſtrongly reſiſts the fire, and ſome of it, by germination, yields alum.

W. BROWNRIGG.

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