of the electrified rod. If this experiment is made with a tube, instead of a sphere, as it cannot be so uniformly excited as the sphere, the light will issue from the rod in flashes, as the tube is more or less excited.

Several very ingenious gentlemen, and in particular the Abbé *Nollet*, have imagined, that the light seen at the point of the non-electric was produced by means of *effluvia* issuing from it in diverging rays towards the electrified rod, and which current of *effluvia* is therefore supposed to be the cause of the attractive, as a like current issuing at the same time from the electrified rod is supposed to be the cause of the repulsive property of Electricity.

This conjecture being directly contrary to the account I have given of this *phænomenon*, I shall offer some considerations in support of what I have advanced, and which I think will make it appear highly improbable, that any such current of *effluvia* issues out of the non-electric.

A continuation of the foregoing Essay. Ibid. p. 213. *Read* May 19. 1748.

32. In the preceding paper, I endeavoured, from the principles therein laid down, to account for some of the most remarkable *phænomena* of Electricity; and in particular for that appearance of a light issuing from the end of an iron rod, when pointed, and made electrical; why this light was visible only at the point, and in no other part of the rod: why the light was visible to a greater length when the point was approached by a non-electric: and why a light will be seen as issuing from the non-electric when it is pointed, but not when it is flat.

I shall now endeavour, from the same principles, to account for those *phænomena*, which will be produced on a nearer approach of the non-electric to the electrified rod.

Exp. VIII. If the non-electric body, whether flat or pointed, is brought nearer to the end of the rod, than in the last experiment, there will be a small stream of light produced, reaching quite from the electric to the non-electric body; and if brought still nearer, there will issue a spark attended with a small snapping noise, which will be succeeded by others at equal intervals; and if the non-electric is held at some distance from the side of the rod, the point of it will frequently appear luminous, but no part of the electrified rod will be so. If it is brought nearer, there will likewise be sparks produced at nearly equal intervals from each other, which will sometimes appear as issuing from the side of the electrified rod, at others, as coming from the non-electric.

If a finger is used as the non-electric, it will receive a smart stroke; and if spirit of wine, heated so as to emit an inflammable vapour, is made use of, it will be kindled by the spark.

These *phænomena* may, on the afore-mentioned principles, be thus accounted for.

If the non-electric rod is pointed, and brought so near, as, by it's attraction, to prevent the rays issuing from the point of the electrified rod from diverging, they will be drawn off parallel to each other, and consequently be equally luminous throughout the whole distance between the two rods.

If

If the non-electric be brought ſtill nearer, the attractive force will be ſo much increaſed, as not only to affect the *effluvia*, when they are driven off from the point of the electrified rod, but to be capable of drawing them off from a conſiderable part of the rod beyond the point; and that with a velocity, and in a quantity, ſufficient to occaſion both the ſpark and blow, as well as the noiſe that is heard.

The ſame is the caſe, when the non-electric rod, or a finger, is held againſt the ſide of that which is made electrical: at a greater diſtance a light will appear as iſſuing from the non-electric, the particles attracted from a large ſurface of the rod (and therefore not viſible as coming from it) being made to converge to a point, are thereby rendered luminous, and, if brought nearer, there will iſſue ſparks in the ſame manner as when held to the end: and that this is owing to the increaſe of the attractive force, ſeems plain; for it was obſerved in the laſt experiment, the attraction was capable of changing the direction of the rays at the diſtance of ſeveral inches; whereas a ſnap or ſpark is ſeldom produced, when the non-electric is held more than an inch and half diſtant. If therefore the attraction decreaſes, as the ſquares of the diſtances increaſe, as it probably does, the attractive force will be many times greater in one caſe than in the other, and if where the attractive power was weaker, as in the former experiment, there were ſo many rays of the electric matter collected, as to be ſufficient to produce a light, it cannot be thought extraordinary, when the attraction is ſo greatly increaſed on the nearer approach of the non-electric, that both the denſity and velocity of the particles ſhould be thereby increaſed, ſo as to produce heat ſufficient to fire the vapour ariſing from ſpirit of wine, or any other inflammable vapour.

And that the quantity of the electric particles is greatly increaſed, as well as their velocity, is evident from that large ſurface of the rod, which, by the approach of a finger, is in one ſpark diveſted of them; and which requiring ſome time before it can be again ſufficiently recruited, I apprehend is the reaſon of that interval between the ſparks. And here it muſt be obſerved, that the diſtance the point of the non-electric is held at from the rod, in order to produce the greateſt ſpark, muſt be varied, in proportion as the rod is electrified in a greater or leſs degree; the more ſtrongly the rod is impregnated the greater will be the diſtance; and if then the non-electric is brought nearer, the ſparks will be ſmaller, but ſucceed each other quicker; ſo that when it is brought almoſt to touch the rod, they will appear like a ſmall ſtream. The reaſon of which I take to be, that as the electric atmoſphere ſurrounding the rod is denſer nearer it than farther off, when the non-electric is brought into ſo very denſe a part of the atmoſphere, it will from thence become nearly as electrical as the rod itſelf; and therefore loſe great part of it's attractive force, and conſequently will only be able to draw off thoſe particles from the rod which are neareſt to it.

I would farther take notice, that the ſparks are always produced in the ſpace between the non-electric and the rod, and often appear as iſſuing from the non-electric. This appearance is probably owing to thoſe particles, which, by their elaſticity, are reflected back again from the non-electric towards the rod, and which, by ſtriking againſt thoſe coming from it, produce both the ſparks and noiſe that is heard; and as I have already ſhewn, that the particles often appear in luminous rays at the point of the non-electric, it thence happens, that the ſpark is frequently kindled ſo near to the non-electric, as to appear as iſſuing from it.

I obſerved, in my former paper, that ſeveral ingenious gentlemen, from this appearance of a light at the point of the non-electric, have imagined there was a current of electrical *effluvia* continually iſſuing out of it, and which, ſetting in towards the electrified rod, was the cauſe of the attraction of the Electricity: and this conjecture of theirs will ſeem to be greatly favoured by the following experiment.

If ſome of the fibres of a down-feather be faſtened to the end of a ſmall ſkewer or wire, and made electrical, they will ſtrongly repel each other, and will expand themſelves on all ſides to the greateſt diſtance poſſible from each other; but if a non-electric perſon bring the point of a pair of compaſſes, or any other ſmall-pointed body near them, they will be repelled from it, and driven up together as with a blaſt of wind, and, in the dark, a light will be ſeen as iſſuing from the point; from whence it might be concluded, that the fibres are repelled by *effluvia* iſſuing out of the point of the non-electric.

As the Abbé *Nollet* endeavours to account for the attraction of Electricity on this principle, I ſhall offer ſome conſiderations, which, notwithſtanding theſe appearances, have induced me to be of a different opinion; and they are founded on the following obſervations.

1. That however replete any bodies may be with the electric matter, none of theſe *phænomena* are ever produced, unleſs the *effluvia* are firſt excited in ſome particular body, and put in motion, either by rubbing, or ſome ſuch-like operation.
2. That the *effluvia* are not to be equally excited in all bodies, but much ſtronger in ſome than in others; and that, in particular, they are not capable of being at all excited in metals by friction.
3. The attractive and repulſive property will be ſtronger or weaker in any body, in proportion to the quantity of excited *effluvia* wherewith it is impregnated.
4. That thoſe bodies which are moſt eaſily excited by friction, will receive the leaſt quantity of the electrical *effluvia* from any other excited body; and, on the contrary, metals, or thoſe bodies in which they cannot be excited by friction, will receive the moſt.

From theſe obſervations I think it may be ſhewn, that this appearance of light is ſo far from proving that the *effluvia* come out of the non-electric,

electric, at whoſe point they are viſible; that from thence it cannot be concluded the body has any of the electrical matter reſiding in it, but is rather a proof to the contrary. For I have already ſhewn, that the ſame appearance would be produced from the ſetting in of the *effluvia* into the non-electric; and this might be confirmed, if neceſſary, by a variety of experiments. And as thoſe bodies, at whoſe point this light appears the ſtrongeſt, afford us no ſigns of their having any of the electrical *effluvia* reſiding in them, either by their attracting or repelling other bodies, or by their being capable of being excited in them by friction, as in glaſs, *&c.* nor in ſhort any ſort of evidence whatſoever, but what ariſes from this appearance; may we not expect ſome better proof of their being poſſeſſed of theſe *effluvia*, before we admit of their iſſuing out of them?

Again, it appears very extraordinary, that thoſe bodies, in which the *effluvia* cannot be excited by any other method, ſhould ſend forth ſuch ſtreams of them, only on their being brought within a few inches of the electrified rod, and that theſe ſtreams ſhould increaſe as the rod is more ſtrongly electrified; and yet that few or none of theſe ſtreams ſhould iſſue from thoſe bodies in which the *effluvia* can be excited: and if the firſt-mentioned bodies are themſelves ſtrongly impregnated, the ſtreams will diſappear, and they will be ſo far from parting with any of their *effluvia*, that, on the contrary, they will be ſtrongly repelled by the rod.

I farther apprehend, on this ſuppoſition, it will be extremely difficult, if not impoſſible, to account for the ceaſing of the ſtream from the point of the non-electric on ſtopping the machine; as likewiſe that the rod ſhould ſo ſoon be diveſted of it's *effluvia*, on ſuch a non-electric's being held near it, which it would otherwiſe retain for ſeveral hours, and which I think is a ſtrong proof of the *effluvia*'s paſſing from the rod into the non-electric. And that it certainly does ſo, may be confirmed by the perſon who holds the non-electric ſtepping upon a cake of wax, when he will ſoon become electrical, from the *effluvia* he will receive (thro' the point of the non-electric) from the rod; but ſo long as he continues to be ſo, there will not be ſeen any light to iſſue from the point; which I apprehend cannot be accounted for on any other principle, but that of the ſetting in of the *effluvia* at the point of the non-electric. And as I have already ſhewn, that all the *phænomena* are naturally to be accounted for on this principle, without being liable to any of the above-mentioned objections, I muſt remain of the opinion (till I can ſee theſe objections anſwered) that this appearance of light is no proof that the *effluvia* iſſue out of the non-electric, but of the direct contrary.

The above-mentioned objections might be brought, with equal force, againſt the fibres of the feather being repelled by *effluvia* iſſuing out of the point of the non-electric that is held near it, and in particular, that this effect would ceaſe to be produced, either when the machine was ſtopped, or the perſon who held the point became electrical. And to theſe I would

would add, that if this was really the case, the fibres would continue to be repelled, notwithstanding any alteration in the shape of the non-electric; whereas, on the contrary, if the joint of a pair of compasses was held towards them, instead of the point, they would be strongly attracted to it: and the same will always happen, whenever an obtuse body is brought near them instead of a pointed one.

The true cause of this remarkable *phænomenon* I apprehend to be the different density of the *effluvia* at the extremities of the two bodies; for I have already shewn, the *effluvia* will be much denser at the extremity of a pointed body than at an obtuse one: and as the force by which the particles endeavour to expand themselves, increases in proportion to their density, it follows, that the particles will be reflected back with greater violence from the pointed body than the other; and this force exceeding the attractive power of that particular part of the feather, to which it is directed, the fibres will be repelled by it; whereas the force, with which the particles endeavour to expand themselves from the obtuse body, being less than the attractive power, it follows, that the fibres of the feather will continue to be attracted by it.

Exp. IX. Take two plates of metal, very clean and dry, whose surfaces are nearly equal; hang one of them horizontally to the electrified rod, and bring under it upon the other any thin light body, as leaf-silver, *&c.* when the upper plate is made electrical, the silver will be attracted by it; and if the under plate is held at a proper distance, will be perfectly suspended at right angles to the plates, without touching either of them; but if they are either brought nearer together, or carried farther asunder, the leaf-silver will cease to be suspended, and will jump up and down between them. The same effect will be produced, if you reverse the experiment, by electrifying the bottom plate, and suspending the other over it.

If the upper plate is electrified when the leaf-silver is brought near, it will be attracted upwards by it, and thereby become electrical; and so long as it continues to be electrical, it will likewise be attracted downwards by the non-electrical plate. Whenever therefore this last attraction added to the gravity of the silver, which acts in the same direction, is equal to the contrary attraction upwards, the leaf-silver will, by means of these two opposite forces, be kept suspended between the plates, and will continue to be so, as long as the equality of these forces is preserved.

I have already shewn, that the attraction between any two bodies will always be in proportion to the different quantity of electric *effluvia* they are possessed of; the greater that difference is, the greater will be the attraction. In order therefore to obtain this equal attraction at first, the leaf-silver must be imbued with a greater or lesser quantity, in proportion as the plate is more strongly or weakly electrified; but always with a much less quantity than the plate; and likewise the lower plate will require to be placed at different distances, in proportion to the quantity of electric matter the upper plate is possessed of. As therefore the suspension of the

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the ſilver depends upon the exact proportion of attraction (ariſing from the different quantities of electric matter) in the two plates and leaf-ſilver, it follows, that whatever alters the quantity contained in any one of them would prevent the ſuſpenſion.

It is well known, that, by the attraction between any two bodies, the electric *effluvia* are continually drawn off from that which has the greateſt quantity of them, till the other being ſufficiently impregnated, the attraction ceaſes. In order therefore to preſerve theſe proportions, it is neceſſary, that, as faſt as the non-electric plate draws off any of the *effluvia* from the leaf-ſilver, it ſhould part with it again; and ſo, by continuing to be a non-electric, an equal degree of attraction be preſerved; and again, that the leaf-ſilver ſhould receive a freſh ſupply from the electrical plate, equal to what it conſtantly parts with; and the electrical plate muſt likewiſe receive an equal ſupply from the globe; and that there is ſuch a conſtant current of the electrical *effluvia*, is evident, from thoſe ſmall ſtreams of light, viſible at the two corners of the ſilver next the plates. If therefore the globe ſhould be ſtopped, or the under plate by any means become electrical, theſe proportions would be thereby deſtroyed, and the leaf-ſilver would ceaſe to be ſuſpended.

That the leaf-ſilver is always nearer to the non-electrical than to the electrified plate, is owing to it's receiving it's ſupply of *effluvia* from the atmoſphere ſurrounding the electrified plate: for as the plate is more ſtrongly electrified than the ſilver, it's atmoſphere of *effluvia* will be denſer to a greater diſtance than that ſurrounding the leaf-ſilver, and therefore can ſupply an equal quantity at a greater diſtance than what the lower plate can receive from the ſilver, whoſe atmoſphere is rarer; and therefore, as the ſilver will always be ſuſpended in that part where the two currents are equal, without which I have already ſhewn the proportion would be deſtroyed, it will conſequently be always nearer to the non-electrical than to the electrified plate. If the experiment is reverſed, by electrifying the under plate, and making the upper one the non-electric, the only difference will be, that the gravity of the ſilver muſt then be added to the attraction of the electrified plate, and will therefore cauſe the ſilver either to be nearer the non-electrical one, or the plates to be moved a little farther aſunder, or perhaps both.

A new diſcovery of the uſefulneſs of Electricity *in Medicine, by* John Hen. Winkler, *Prof. at* Leipſick, *F. R. S.* N°. 486. p. 262. Feb. *and* Mar. 1748. *Dated* Mar. 12. 1748. *Read* Mar. 31. 1748.

33. Electricity has a power of dividing ſubtilly. It carries off with it the parts of thoſe bodies which it diſſolves, and transfers them to thoſe places where the electrical ſparks appear. If odorous ſubſtances are ever ſo cloſely confined in glaſs veſſels, it ſo divides them, that their exhalations penetrate the glaſs as eaſily as magnetical powers, and flow like a river thro' the atmoſphere of cylinders and chains, to which the Electricity is communicated. The electrical matter, which comes out of the other extremity of the cylinder, gives an aromatic odour to the hand that touches it. But the odour communicated does not ſtop in that part of the body on which the electrical river has flowed, but with a continued aſpiration pervades the whole human body. Not only the ſkin and garments are ſcented, but even the air breathed by the lungs, the

the ſpittle, and the ſweat of the perſon affected, ſmell of the aromaticks, which are agitated by Electricity in the cloſed veſſel.

This unexpected virtue is made probable by ſeveral obſervations and experiments which were made with care and attention. In 1747 I filled a glaſs veſſel with water, and diſſolved nitre therein. This veſſel ſtood unmoved for ſeveral weeks. The water therefore became very clear, after the heavier parts of the nitre had ſubſided. At the latter end of the year I put a wire into this clear water, and joined it to a metalline tube ſuſpended on ſilken threads. I put under this tube at different times ſometimes metals, ſometimes metalline veſſels full of water, in which were glaſs ſpheres filled with metalline particles. When theſe were prepared I excited the Electricity. The electrical fire touched the bodies placed underneath. I repeated the agitation of Electricity for ſeveral days. And now, beyond my expectation, I found a great quantity of nitrous parts of various textures in the metals and veſſels, which had been touched by the electrical fire under the metalline tube. More veſſels were placed in the room where I made the experiments, and were not touched by the electrical matter from the metalline tube. In theſe there was no trace of nitre. Hence it is eaſy to conjecture, that the parts of nitre are taken out of water by Electricity, and carried into places which are touched by the electrical fire.

About the beginning of the preſent year 1748, I received a letter from *Venice*, which greatly confirms this conjecture. The author, *Joannes Daniel Gaiſel*, related an affair, which ſurprized all the learned in *Venice*, *Bologna*, and other cities of *Italy*. It was accompanied with a printed epiſtle in *Italian* *, written by an eminent perſon at *Venice*, Sig. *Jo. Franc. Pivati*. In this epiſtle, the ſubject of which is Medical Electricity, he relates a ſtory of wonderful effects to Sig. *Fr. Maria Zanotti*, Secretary of the Academy of *Bologna* : and the art, by which theſe things were performed, was the invention of *Pivati*. A manifeſt example of the virtue of Electricity was ſhewn in the balſam of *Peru* ; which was ſo concealed in a glaſs cylinder, that before the application of Electricity, there could not be the leaſt ſmell of it by any means diſcovered. A man, who having a pain in his ſide, had applied hyſſop to it by the advice of a Phyſician, approached to the cylinder. The man was electrified by it, went home, fell aſleep, ſweated, and diſperſed the power of the balſam. His cloaths, bed, chamber, all ſmelt of it. When he had refreſhed himſelf by this ſleep, he combed his head ; and found the balſam to have penetrated his hair, ſo that the very comb was perfumed. The next day S. *Pivati* electrified a man in health after the ſame manner, who knew nothing of what had been done before. On his going into company about ½ an hour afterwards, he found a gradual warmth diffuſing itſelf thro' his whole body. He grew lively and more chearful

* *Lettere ſopra L'Elettricitá principalmente per quanto ſpetta alla Medicina. In* Venezia *appreſſo* Simone Occhi, *con Licenza de ſuperiori* 1747.

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than ufual. His companions were furprized at an odour, and could not imagine whence it proceeded : but he himfelf perceived, that the perfume arofe from his own body, at which he was much furprized ; not having the leaft fufpicion that it was owing to the operation, that had been performed upon him by S. *Pivati*.

Being ftruck with a relation fo extraordinary, I was defirous to try the power of Electricity on certain fubftances, and found the event to confirm what had been related. I put fome beaten fulphur into a glafs fphere, fo well covered and ftopped, that on turning it over the fire, there was not the leaft fmell of fulphur perceived. When the fphere was cooled, I electrified it. Immediately fulphureous vapours iffued from it, and on continuing the Electricity, filled the air, fo as to be fmelt at the diftance of more than 10 feet. I called a friend well verfed in Electricity, Prof. *Haubold*, and feveral others, as witneffes and judges of this fact : but they were prefently driven away by the ftench of the fulphur. I ftaid a little longer my felf in this fulphureous atmofphere, and was fo impregnated thereby, that my body, cloaths, and breath, retained the odour even the next day. On repeating the experiment in the prefence of one who was converfant in the effects of fulphur, the figns of an inflamed blood were vifible in the mouth on the third day. After this I tried the effect of a more agreeable fmell, and filled the fphere with cinnamon. When I had treated it as before, the fmell of cinnamon was foon perceived by the company, and the whole room was in a fhort time fo perfumed by it, that it immediately faluted the nofes of all that came in ; and the odour remained on the next day. I tried the balfam of *Peru* with like fuccefs. My above-mentioned friend, whofe teftimony I did not care to be without, after he had received the power of the balfam, fmelt fo ftrong of it, that going abroad to fupper, he was often afked by the company, what perfume he had about him. The next day, when I drank tea, I found an unufually fweet tafte, owing to the fumes of the balfam, that ftill remained in my mouth. In a few days, when the fphere had loft all the fcent of the balfam, we let a chain out of the chamber window, and extended it thro' the open air into another room detached from the former. Here we fufpended the chain on filken lines, and gave it into the hand of a man who ftood on an extended filken line, and knew nothing of our purpofe. When the Electricity had been excited for fome time, the man was afked whether he fmelt any thing ; and on fnuffing up his nofe he faid he did. Being afked again what fmell it was, he faid he did not know. When the electrical commotions had been continued a quarter of an hour, that room fmelt fo ftrong of it, that the man, who knew nothing of our balfam, faid his nofe was filled with a fweet fmell, like that of fome fort of balfam. After fleeping in a houfe at a confiderable diftance from the room where the experiment was tried, he rofe very chearful in the morning, and found a more pleafant tafte than ordinary in his tea.

When I consider these things, I cannot think it improbable, that Electricity may be of service in the cure of some diseases. There are 2 great benefits to be expected from medicine; for either noxious particles being mixt with the blood or other jucies, are to be separated and expelled; or such as are beneficial to health are to be introduced. In both these cases Electricity may be of service. For as soon as it touches a human body, it immediately pervades it in such a manner, that no place is left free from it; nor is there any thing in the body that can be rendered volatile, that is not dissolved, dissipated, and carried off by it. We cannot doubt therefore, that blood, with which Electricity is communicated, is divided into more minute parts; that several of them are separated from the mass of blood, and in a short time are dispersed in the air. The tenacity of the blood does not blunt the electrical power; nor does the firmness of the veins hinder the avulsion; nor the fat repress it. The coherence of glass, tho' it is much firmer than the contexture of the veins, flesh, and skin, cannot however hinder spirits and aromaticks from being dissolved into particles, that fly off thro' the pores of the glass. We seem therefore to have reason enough to think, that Electricity may cause certain substances to fly off from the blood, and other parts of the body.

That the blood and humours of the body are greatly agitated, resolved, and attenuated by Electricity is manifest: for I knew a woman, whose *menses* flow immediately on her being electrified. Dr *Thebesius* wrote to me a few days ago from *Hirschberg* in *Silesia*, that his being electrified was constantly attended by a bleeding at the nose.

But Electricity has not only a power of separating and expelling, but is also very efficacious in filling the blood with powers, which are contained in plants, and minerals; which is manifest from what I have already shewn with regard to sulphur, cinnamon, and balsam of *Peru*. The electrical power of nourishing the blood differs from the usual method of healing in this, that it supplies the blood with aliment without the help of the stomach, and that it enriches the vital juice with those exhalations, which pass thro' the glass, and excel in subtilty and purity. Medicines received by the mouth must be carried into the stomach, before they can be mixed with the blood, and wander thro' many and long paths, and therein be changed. But the spirits raised by benign Electricity, flow into the blood without these windings. Sometimes a part of the body is disordered by it, because the passages, thro' which the blood or other liquor ought to flow, are so obstructed, that the remedies applied have no power at all of opening them, or at least require a long time for it. But the part that is touched by electrical *effluvia* is strongly opened and penetrated by them.

By the conjunction therefore of medicine and the electrical art, I am of opinion, that new and happy cures of diseases may be performed, remarkable examples of which have been published by the learned S. *Pivati*, who made use of the advice of a learned and experienced Physician.

ſician. He reſtored the obſtructed courſe of the blood in a woman, by treating the uſual medicines in ſuch a manner, that their powers reached the body of the patient by means of Electricity, from the glaſs cylinders in which they were incloſed. S. *Pivati*'s aſſiſtance was implored by a young gentleman, who was ſo miſerably affected by an abundance of collected and corrupted humour in his foot, that it eluded all the attempts of the Phyſicians. S. *Pivati* filled a glaſs cylinder with proper materials, and having electrified it, applied to the part affected, which he cauſed to emit electrical ſparks, and continued the operation for ſome minutes. When the patient went to bed, he had a good night, and a mitigation of his pain. When he awaked in the morning, he found a ſmall red tubercle on his foot, which only itched, as if a cold humour had flowed thro' the inner part of his foot. He ſweated every night for 8 days together, and at the end of this time was perfectly well. After this S. *Donadoni*, Biſhop of *Sebenico*, came to S. *Pivati*, attended by his Phyſician and ſome friends. His Lordſhip was at that time 75 years old, and had been afflicted with pains in his hands and feet for ſeveral years. The gout had ſo affected his fingers, that he was not able to move them, and his legs, ſo that he could not bend his knees. He was ſo miſerable, that when night came, his ſervants were obliged to bring him in a chair to the bedſide, and lift him gently into it. The poor old Biſhop intreated S. *Pivati* to try the effects of Electricity on his body. He proceeded after the following manner; he filled a glaſs cylinder with diſcutient medicines, and managed it ſo that the electrical virtue might enter into the patient. He preſently felt ſome unuſual commotions in his fingers. The action of Electricity was continued for 2 minutes. His Lordſhip in leſs time than could be imagined, opened and ſhut both his hands, gave a hearty ſqueeze with his hand to one of his attendants, got up, walked, ſmote his hands together, helped himſelf to a chair and ſate down, wondering at his own ſtrength, and hardly knowing whether it was not a dream. He walked out of the chamber down ſtairs, without any aſſiſtance, and with all the alacrity of a young man. Soon after, S. *Pivati* relieved a Lady of 60 years in like manner from the gout, with which ſhe had been 6 months tormented. Her fingers were much ſwollen, and continually trembling, and one of her arms was convulſed. But after receiving the powers of Electricity for 2 minutes, the trembling of her fingers ceaſed: and the next day the ſwelling was ſo far abated, that ſhe could draw on her gloves, and make uſe of her fingers.

Theſe things are ſo manifeſt, that there ſeems to be no room to doubt of the aſſiſtance that may be given to Medicine by Electricity. In which opinion I am greatly confirmed by the concurrence of the judicious and ſkilful Phyſician S. *Morgagni*, Profeſſor of Anatomy at *Padua*, who highly approved of what S. *Pivati* had done, and encouraged him to proceed in his attemps to improve Medicine, in a manner ſo beneficial to human kind *.

* *See Art.* 38, *and* 39.

A letter from Mr Henry Baker *F. R. S. to the Preſident, concerning ſeveral* Medical Experiments *of* Electricity. Ibid. p. 270. *Read* March 13. 1748.

34. Though perhaps as many curious and well-contrived experiments have been made in *England* as in all the other parts of *Europe*, to diſcover the general laws and properties of Electricity; we have not hitherto attended to the effects that may be thereby produced in the bodies of living animals, any further than to aſſure ourſelves they may be killed thereby; a ſuppoſition that diſeaſes may be cured by means of this power, having met with ſo little countenance amongſt us, that very few trials have been made, to aſcertain what, in diſtempered caſes, it can or cannot perform. *Foreigners*, on the contrary, ſeem fond of believing that the ſubtile electric fluid (be it fire, æther, or whatever elſe) which can pervade all bodies, and (being accumulated) even kill an animal, in certain circumſtances, and by certain methods of application, may, poſſibly, in other circumſtances, and applied in different degrees, and by different methods, ſo operate on the fluids or ſolids, and perhaps on both, that very beneficial and ſalutary effects may reſult therefrom.

With this view the Abbé *Nollet* made ſeveral experiments on living birds, kittens, and human bodies; and if we may give credit to the accounts thereof communicated to us, he found, in every trial, that *perſpiration* was ſo conſiderably promoted thereby, as to cauſe a very ſenſible difference between the weight of ſuch animals as had been electrified, and others of the ſame kind that were treated exactly alike in every reſpect beſides: whence he naturally concludes, that, in caſes where it is neceſſary to quicken the circulation of the fluids, and throw off a greater quantity of the perſpirable matter, Electricity muſt be greatly uſeful.

The Philoſophers in *Italy* and *Germany* have applied their induſtry to diſcover by experiment, how far Electricity may, ſimply and in itſelf, be of ſervice in ſeveral diſeaſes, and likewiſe how far it may conduce towards conveying the more ſubtile and active *effluvia* of uſeful Medicines, either into the whole body, or into ſome diſtempered part. Mr *Watſon* read, laſt *Thurſday*, before the *Royal Society*, an abſtract of the preceding paper.

My ingenious friend Dr *Joſeph Bruni*, one of the principal Phyſicians at *Turin*, and F. R. S. has likewiſe ſent to me an account, lately received by him, of experiments made at *Rome*, and at *Bologna*; which I now lay before you, in order to ſhew what attempts to the ſame purpoſe have been made in different countries, and by different people. The Doctor informs me, that at *Turin* they have repeated, with great ſucceſs, the electrical experiments made in *England*, whereof I had ſent him printed accounts; that people all over *Italy* are buſily at work making electrical experiments; and that, at *Bologna*, the electrical power has been applied to the cure of diſeaſes. He then gives me a tranſcript of an account ſent him from thence in *French*, which, tranſlated, is as follows.

A man, who had been for a whole twelvemonth deaf of one ear, with a continual noiſe in it like the running of water, attended with moſt

violent

violent pain whenever he lay with that ear uppermoſt, coming to Dr *Verati* for advice, the Doctor electrified him, bringing out abundance of fiery ſparks around the diſtempered ear; which, in about five minutes that the electrification was continued, became as red as if a bliſtering plaiſter had been applied to it. But the redneſs diſappeared in a few minutes after, the patient paſſed the night with leſs pain and noiſe, and was perfectly cured of his diſorder.

A footman belonging to the ſaid Doctor, being taken ſuddenly ill of a violent pain in the head, which continued many hours, he was thereupon electrified, the Doctor cauſing the ſparks of fire to iſſue from the temple wherein the pain was felt. The part appeared red, the pain abated; in 3 hours it was entirely gone, and has never returned ſince.

A woman that nurſed one of the Doctor's children, having had a moſt grievous diſorder in her eyes for ſome months, with a continual running of water from one of them, and a conſtant pain over the eye-lid, came to the Doctor for advice; who immediately electrified her, bringing out the fiery ſparks about the eye and eye-lid, whereby the eye appeared very much blood-ſhot; but that went off in 7 or 8 minutes. The woman felt leſs pain the following night, and opened her eye in the morning more eaſily, and without being obliged to wipe it, as ſhe did before: the watry humour and pain were much diminiſhed; and the Doctor hoped, that, by repeating the operation twice more, he ſhould be able to cure her quite.

Dr *Bruni* gives me next his information from *Rome*; which is, that a gentleman there covered the internal ſurface of a cylinder of glaſs (which ſome uſe inſtead of a globe) with a purgative Medicine; and that a man, electrified therewith, found on the ſpot the ſame effects as if he had ſwallowed the Medicine. He then recommends to us in *England* to try how far the electric power may be of ſervice in diſtempers.

Theſe caſes, Sir, and particularly the laſt, as it may to ſome appear extravagant and whimſical, I ſhould have been cautious of bringing before the *Royal Society*, had you not judged it proper they ſhould be added to thoſe ſimilar accounts from other places which were read to us laſt meeting. I think neither myſelf nor Dr *Bruni* anſwerable for the truth of theſe facts, as we relate no more than what we have received. In truth, all the *phænomena* in Electricity are ſo wonderful, that it is ſcarcely prudent to deny the poſſibility of any accounts concerning it, till we have made experiments carefully ourſelves. We are *very ſure* it is poſſible to render a living body replete with electrical *effluvia*, or to tranſmit and ſend ſuch *effluvia* through a living body, in a ſtream, as long as we think proper: we are *not ſure* that it is impoſſible for theſe *effluvia* to convey with them into that living body the moſt ſubtile and active *effluvia* of other ſubſtances; and if they can do ſo, the effects ſuggeſted are not wholly improbable; for ſeveral experiments have proved, that a very minute quantity of Medicine, transfuſed directly into the blood, and circulating fluids, will have the ſame effect as a large doſe

thereof

thereof taken into the ſtomach. Therefore even this laſt caſe, romantic as it may ſeem, ſhould not be abſolutely condemned without a fair trial; ſince we all, I believe, remember the time, when thoſe *phænomena* in Electricity, which are now the moſt common and familiar to us, would have been thought deſerving as little credit, as the caſe under conſideration may ſeem to do, had accounts of them been ſent us from *Rome*, *Venice*, or *Bologna*, and had we never experienced them ourſelves.

A letter from Mr Robert Roche, *to the* Pres. *of a* Fuſtian Frock *being ſet on fire by* Electricity N°. 487. p. 323. April, &c. 1748. *Dated London*, May 17. 1749. *Read* May 19. 1748.

35. I have a ſon about 16 years old, that has been for 6 or 7 years paſt troubled with ſudden fits that intirely take away his ſenſes. I got him all the helps I could, but to no purpoſe; at laſt I ſent him to St *Bartholomew*'s Hoſpital, as an out-patient; and there he was turned out as incurable. So finding his caſe deſperate, I conſidered the power of Electricity, and made a large machine for electrifying; and afterwards ſhocking him commonly twice a day, he has received ſome benefit: and laſt *Sunday*, being *May* 15, he being on the pedeſtal, and very high electrified, and having on a coarſe fuſtian working frock, the condenſing phial being on the conductor, and I, touching him to procure ſnaps as uſual, touched his right ſhoulder blade; and, to my great ſurprize, the furzy flax of the frock caught fire, with a great blaze, and burnt the whole breadth and length of the ſhoulder, the flame riſing 6 inches above the collar, and I believe would have ſet the frock on fire, had I not put it out with my hands. There was no fire in the room that day: this was about noon; neither was there any thing that could have any inflammable vapour there.

My ſurprize was the greater, becauſe all I read on that ſubject ſays nothing will burn but what ſends forth ſuch vapours.

At 9 the ſame evening I made him put on the ſame frock, and touched the left arm, where the flax had not been burnt before; and it had the ſame effect as above.

Extract of a letter from the Rev. Dr Steph. Hales, *F. R. S. to the Rev. Mr* Weſtly Hall, *concerning ſome* Electrical *Experiments.* N°. 488. p. 409. June 1748. *Dated Teddington,* Feb. 23, 1746-7, *Read* June 30. 1748.

36. I ſaw laſt week in *London* ſome electrical experiments; in which new field of reſearches there are daily new diſcoveries made: the active electric fluid ſeems to be a great agent, in conjuction with the air, in the production of fire.

A warm thick piece of iron being ſuſpended by two ſilk lines, had a warm very thick piece of braſs laid on it, on which was placed a common hen's egg: when electrified, the flaſhes from the iron were of a bright ſilver light colour; from the braſs (eſpecially near it) the flaſhes were green; and from the egg of a yellowiſh flame colour; which ſeems to argue, that ſome particles of thoſe different bodies were carried off in the flaſhes, whence theſe different colours were exhibited.

It is ſuſpected that great degrees of electrifying have occaſioned ſome women to miſcarry; and no wonder that ſuch ſudden ſhocks ſhould do it. I wrote to Mr *King* the experimenter to electrify a frog, while the circulation of it's blood was viewed with a microſcope, to ſee if it accelerated it's motion, which he has not yet done.

He obſerves, that a piece of linnen that has never been waſhed, will ſoon give a good degree of Electricity to a large warm glaſs tube; *viz.* on account of the mealy paſte, which weavers dreſs the linnen with; and therefore any piece of linnen thus dreſſed will do.

37. I laid before the *Royal Society* the beginning of laſt winter an account of what had been done by ſome gentlemen, in order to aſcertain the reſpective velocities of Electricity and Sound; from which it appeared, that through a ſpace meaſuring 6732 feet, the Electricity was perceptible in a quantity of time leſs than $\frac{837}{1000}$ of a ſecond. But the gentlemen concerned were deſirous, if poſſible, of aſcertaining the abſolute velocity of Electricity at a certain diſtance; and a method had been thought of, by which this might be determined with great exactneſs.

An account of the Experiments made by ſome Gentlemen of the R. Soc. *in order to meaſure the abſolute velocity of* Electricity; *by Mr* W. Watſon, *F. R. S.* N°. 489 p. 491. Oct. &c. 1748. *Read* Oct. 27. 1748.

Accordingly, *Aug.* 5. 1748. there met at *Shooter's-Hill* for this purpoſe, the *Preſ.* of the *R. Soc.* Rev. Mr *Birch*, Rev. Dr *Bradley*, Aſtron. Royal, *James Burrow*, Eſq; Mr *Ellicot*, Mr *G Graham*, *R. Graham*, Eſq; Rev. Mr. *Lawrie*, *Cha. Stanhope*, Eſq; and myſelf, who were of the *R. Soc.* Dr *Bevis*, and Mr *Griſchow*, jun. a Member of the *R. Acad.* of *Sc.* at *Berlin.*

It was agreed to make the electrical circuit of 2 miles; in the middle of which an obſerver was to take in each hand one of the extremities of a wire, which was a mile in length. Theſe wires were to be ſo diſpoſed, that this obſerver being placed upon the floor of the room near the electrical machine, the other obſervers might be able in the ſame view to ſee the exploſion of the charged phial, and the obſerver holding the wires; and might take notice of the time lapſed between the diſcharging the phial and the convulſive motions of the arms of the obſerver in conſequence thereof; inaſmuch as this time would ſhew the velocity of Electricity, through a ſpace equal to the length of the wire between the coated phial and this obſerver.

The electrifying machine was placed in the ſame houſe as it was laſt year. We then found ourſelves greatly embaraſſed by the wire's being conducted by the ſide of the road, which we were compelled to, on account of the ſpace neceſſary for the meaſuring of Sound: but ſo great a diſtance from the machine was not now wanted, though the circuit through the wire was intended to be at leaſt 2 miles. We had diſcovered, by our former experiments, that the only caution now neceſſary was, that the wires conducted upon dry ſticks ſhould not touch the ground, each other, or any non-electric, in a conſiderable degree, in any part of their length: if they did not touch each other, the returns of the wire, be they ever ſo frequent, imported little, as the wire had been found to conduct Electricity ſo much better than the ſticks. It was therefore thought proper to place theſe ſticks in a field 50 yards diſtant from the machine. The length of this field being eleven chains, or 726 feet, eight returns of the wire from the top to the bottom of the field, made ſomewhat more than a mile, and 16 returns more than 2 miles,

miles, the quantity of wire intended for the Electricity to paſs through to make the experiment.

We had found laſt year, that, upon diſcharging the electrified phials, if 2 obſervers made their bodies part of the circuit, one of which graſped the leaden coating of the phial in one hand, and held in his other one extremity of the conducting wire; and if the other obſerver held the other extremity of the conducting wire in one hand, and took in his other the ſhort iron rod with which the exploſion was made; upon this exploſion, I ſay, they were both ſhocked in the ſame inſtant, which was that of the exploſion of the phial. If therefore an obſerver, making his body part of the circuit, was ſhocked in the inſtant of the exploſion of the charged phial in the middle of the wire, no doubt would remain of the velocity of Electricity being inſtantaneous through the length of that whole wire. But if, on the contrary, the time between making the exploſion, and ſeeing the convulſions in the arms of the obſerver holding the conducting wires, was great enough to be meaſured, we then ſhould be able to aſcertain it's velocity to the diſtance equal to ½ the quantity of wire employed only, let the manner of the Electricity's diſcharging itſelf be what it would.

It has been a queſtion with ſome, who have conſidered this ſubject, whether the Electricity, in compleating the circuit from the matter contained in the glaſs, paſſed either by the wire in the mouth to the coating of the glaſs, the contrary way by the coating to the wire in the mouth, or otherwiſe directed itſelf both ways at once? that the Electricity muſt paſs off one of theſe three ways, was certain, as the exploſion would not be complete, unleſs in the inſtant thereof ſome matter very non-electric communicated between the wire in the mouth, and the coating of the glaſs. Unleſs therefore the obſerver was placed in the centre of the conducting wires, it might be objected, that the experiment was not made with the exactneſs neceſſary; becauſe any perſon, who was of opinion that the Electricity directed itſelf from the mouth of the glaſs to the coating, might object, if the wire from the ſhort iron rod to the obſerver was only ½ the length of that between the obſerver and the coating of the glaſs, that the Electricity, in the time found, paſſed only through the ſhort wire, and *vice verſa*. But if, as it was here thought proper, the obſerver was placed in the centre of the conducting wire, let the direction of the Electricity be what it would, no difference could happen in the reſult of the experiments, if made with the neceſſary caution; becauſe, if the effects in the middle and both ends of the wires were inſtantaneous, the concluſion therefrom would be very obvious. To make the experiment, the ſame phial filled with filings of iron, and coated with ſheet-lead, which was uſed laſt year, was placed in the window of the room near the machine, and was connected to the prime conductor by a piece of wire. To the coating of this phial a wire was faſtened; which, being conducted upon dry ſticks to the before-mentioned field, was carried in like manner to the bottom; and being conducted thus from the bottom of the field to the top,

top, and from the top to the bottom 7 other times, returned again into the room and was held in one hand of an obſerver near the machine. From the other hand of this obſerver, another wire, of the ſame length with the former, was conducted in the ſame manner, and returned into the room, and was faſtened to the iron rod with which the exploſion was made. The whole length of the wires, allowing 10 yards for their turns round the ſticks, amounted to 2 miles $\frac{1}{4}$ and 6 chains, or 12276 feet.

As the night preceding theſe experiments had been very rainy, care was taken, by ſilk lines properly diſpoſed, that the wires in their paſſage from the window of the houſe might not touch the wood thereof; leſt, from the moiſture of this wood, the electrical circuit might be ſhortened.

When all parts of the apparatus were properly diſpoſed, ſeveral exploſions of the charged phial were made; and it was invariably ſeen, that the obſerver holding in each hand one of the extremities of theſe wires was convulſed in both his arms in the inſtant of making the exploſions.

Inſtead of one, 4 men were then placed holding each other by the hand near the machine, the firſt of which held in his right hand one extremity of the wire, and the laſt man the other in his left. They were all ſeen convulſed in the inſtant of the exploſion. Every one who felt it, complained of the ſeverity of the ſhock.

It was then deſired, by one of the gentlemen concerned, that an exploſion ſhould be made with the obſerver holding only one of the wires. This was done accordingly; but the obſerver felt nothing, the phial diſcharging itſelf in a different manner to what it did before, on account of the circuit's not being compleated.

It was then tried, whether an obſerver would be ſhocked upon the diſcharge of the phial, if the 2 wires at their extremities ſlightly touched each other, whilſt an obſerver at the ſame time held one of theſe about a foot from their ends in each of his hands. Upon trial he felt nothing, though the phial exploded very quick, becauſe the iron wire conducted the Electricity better than the body of the obſerver.

It was then tried, whether or no, as the ground was wet, if the exploſion was made with the obſerver holding the extremity of each wire ſtanding upon the ground near the window of the houſe, any difference would ariſe in the ſucceſs of the experiment. No difference was found, the obſerver being ſhocked in the inſtant of the exploſion, as before, in both his arms, and acroſs his breaſt.

Upon theſe conſiderations we were fully ſatisfied, that through the whole length of this wire, being, as I mentioned before, 12276 feet, the velocity of Electricity was inſtantaneous.

As it was found laſt year, we obſerved again, that although the electrical commotions were very ſevere to thoſe who held the wires, the report of the explosion at the prime conductor was little, in comparison of that which is heard when the circuit is short. From whence it was conjectured, that the very loud report, in the experiment of *Leyden* is confined to a very short circuit.

A letter from Mr William Watson, *F. R. S. to the* R. Soc. *declaring that he as well as many others, have not been able to make* Odours *pass thro' glass by means of* Electricity; *and giving a particular account of Prof.* Bose *at* Wittemberg*'s experiment of* Beatification, *or causing a* Glory *to appear round a man's head by* Electricity. N°. 494. p. 348. Jan. &c. 1750. *Read* March 1. 1749-50.

38. The inquiry into the nature and properties of Electricity has been, within these few years, the pursuit of many excellent and ingenious persons; and most of it's extraordinary phænomena, which have been made to appear in one place, have, with proper attention to the requisite circumstances, appeared in others: but there have happened 2 very remarkable exceptions to this rule. The first is, that the odours of odoriferous substances do not only pervade, from friction, the glasses which contain them, but that these odours were carried along with the current of Electricity into such non-electric bodies as were destined to receive them, and manifested themselves in those bodies by communicating to them their smell, and other properties. These, and other things yet more extraordinary, were said to have been performed by Mr *Pivati* at *Venice*, and to have been repeated by Mr *Winckler* at *Leipsick*; but though no care or expence has been spared, either by Abbé *Nollet* at *Paris*, Mr *Jallabert* at *Geneva*, Mr *Bose* at *Wittemberg*, *Pere Garo* at *Turin*, and by myself at *London*, to bring about the same effects, they have hitherto been unsuccessful. For which reason the truth of these relations has been greatly questioned by many; as Mr *Buccamare*, in a * treatise since published, says, that Mr *Pivati* confessed to those, who addressed themselves to him to see the experiments, that more especially made with balsam of *Peru*, that it never succeeded but once, and that he could never repeat it. I likewise received yesterday a letter from Abbé *Nollet*, who is just returned to *Paris* from *Turin* and *Italy*. He says, that his first care was to inquire into the truth of those wonders in Electricity, of which we have heard so much for almost 3 years, and which have not succeeded either with himself or me: and he imagines the *R. Soc.* would be glad to know what they really were: for which reason he has just now sent a Memoir to the D. of *Richmond*, in which will be seen the most circumstantial account he has been able to procure of them at *Turin*, at *Venice*, and at *Bologna*. For his own part, he thinks that there has been a great deal of prejudice, credulity, and exaggeration; to which may be added, very little care and caution in making these experiments. He is now sorry he has lost so much time in attempting to make them; and thinks Mr *Winckler* has been too hasty in asserting, that he had repeated these *Italian* experiments: but why should he call them *Italian*, when the nation he says will not allow the appellation, and except 3 persons, he finds there no defender of what has been said to be done; and adds, that there is not a Philosopher of repute there, who believes them any more than himself?

This experiment then seems not to arrive at what we have been told; but, for further information, we must wait till the reception of Abbé *Nollet*'s Memoir.

* *Tentamen de vi Electric.* &c. p. 183.

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The other is, an experiment called by Prof. *Bose* at *Wittemberg*, the *Apotheosis* or Beatification. The making this experiment, in the manner mentioned by this gentleman in his writings, has been attained to by none. He says, if in electrising you employ large globes, and place a man upon a large cake of pitch, by little and little a lambent flame arises from the pitch, and spreads itself around his feet; from hence by degrees it is propagated to his knees, his body, and at last to his head: that then by continuing the electrisation the man's head is surrounded by a glory, such a one in some measure, as is represented by Painters in their ornamenting the heads of Saints: that in this state if the electrised man is touched by one that is not, the pain felt by both is very severe, reaches from the finger to the shoulder, and remains a long time. Prof. *Bose*, in another part of his * writings, says, that the beatification indeed does not always succeed with him; that sometimes, when other circumstances have been very favourable, a man will be beatified by one sphere in 2 minutes; at other times, 2 or 3 globes will not do it under 6 or 8 minutes; and even at some times after 20 minutes, when 5 or 6 globes were made use of, no light has been visible: that under the same circumstances, when one person was capable of being beatified, another was not. This is a short account of Prof. *Bose*'s beatification, given in his writings, in which, nevertheless, nothing of what he says essential to the operation is omitted.

This experiment, which was not only a desirable thing to be seen, but as it seemed to communicate to non-electric bodies a greater quantity of Electricity than any other did, that of *Leyden* excepted, I was very desirous of repeating: But though I omitted no trouble, and varied not the least circumstance, that could any ways conduce thereto, I was disappointed. I tried the combined force of many globes, of different machines, in the best weather, and with different persons, but no radiation in the manner before-mentioned. When I underwent this operation myself, supported by solid electrics *per se* of more than 3 feet high, and as much distant from the sides of the room as possible to prevent the escaping of the electric matter, I found in myself, as several others did, a tingling upon the skin in my head, and in many parts of my body such a sensation as would be felt from a vast number of insects crawling upon our bodies at the same time; but I constantly observed this sensation to be greatest in those parts of my body which were nearest any non-electric; but still no light upon the head, though to make the eye more ready to observe it, this experiment was made in the dark for some continuance. The sensation of the snaps in this state were very acute. If the hand of a by-stander was brought near the back of the hand of the person electrised, the hairs thereupon sent forth a great number of luminous points; and if a bunch of fine lace wire was placed upon his head, you saw a great deal more of the same appearance; but this was

* De Electricit. Comment. novus, pag. xvi.

always moſt brilliant in thoſe parts neareſt the non-electric, and ſtill more, when the non-electric was brought to a proper diſtance. But this was vaſtly ſhort of that mentioned by Mr *Boſe*, not only in it's luſtre, but as it never was general, hardly ever ſhewing itſelf in 2 parts of the body at the ſame time. This want of ſucceſs after many trials, as I by no means doubted Mr *Boſe*'s veracity, induced me to conclude, that either ſome very eſſential part of the apparatus had been ſuppreſſed by the author, or that the air of *Germany*, being upon the continent, was more dry, and more fit, than that of our iſland. It was difficult indeed to allow this laſt, as the experiment had failed here, after the long continuance of a very dry ſeaſon. This want of ſucceſs occaſioned many perſons here, well verſed in theſe matters, to conclude, that the experiments in Electricity had been carried further in *Germany* than in *England*.

However, ſome time after, I found that this experiment, in the manner before-mentioned, had been made no-where upon the Continent, *Wittemberg* excepted; and Mr *Jallabert* at *Geneva*, in his excellent * treatiſe upon Electricity, ſays, that he had likewiſe attempted it; but inſtead of beatification, he ſaw from the hair of the head of the perſon electriſed, eſpecially from the back part thereof, a great number of luminous points. Theſe, he ſays, were likewiſe obſervable upon his cloaths, which were made of a mixture of thread and cotton, more eſpecially upon their borders. When the perſon electriſed changed his ſituation upon the pitch, upon which he ſtood, the place he left appeared luminous. What this gentleman mentions beſides is very near alike to what I myſelf experienced, and what I have juſt now related. He ſays likewiſe, that he believes Mr *Boſe* had been the only perſon, who had made the beatification ſucceed.

A perſon here however, that we ſhould not even ſeem to be outdone by our neighbours, exhibited to the public the famous experiment of beatification, found out, as he ſays, by a *German* Profeſſor. Whether he knew how this experiment was ſaid to be done, or whether it was with him as with many of the diſcoverers of the Longitude, and of the quadrature of the circle, I do not determine; but thus it is, that his experiment has been exhibited as Mr *Boſe*'s for 2 or 3 years.

I am unwilling to be thought to detract from the merit of this experiment, which I think a very beautiful one; but I take upon me to ſay, that it differs as eſſentially from every part of that, ſaid to have been made by Prof. *Boſe*, as any two electrical experiments ſoever.

In a letter, I wrote the beginning of laſt year, to my correſpondent Mr *Boſe*, among other things, I acquainted him of my not being able to make the beatifying experiment ſucceed; and that, as far as I had yet heard, nobody any-where had been able to do it, ſo that the power of ſeeing this extraordinary *phænomenon* was yet with himſelf alone. I

* *Experiences ſur l'Electricité.* p. 50.

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desired of him further, that if any material part of the process had been omitted in his writings, he would communicate it; for that some people here were not quite satisfied of it's having ever been made. To this he was so obliging as to send an answer nearly in the following words, "As to my beatification, I am highly obliged to you for writing to me "so freely and candidly about it; and I will discover to you my whole "artifice without any retention, though I concealed the same from all "my friends and correspondents: but, Sir, it is true, that I have em-"bellished a little my beatification by my stile and expressions; but it "is also true, that the basis of the *phenomenon* is constant. I found in "our armoury at *Leipzig*, a whole suit of armour, which was decked "with many bullions of steel; some pointed like a nail; others in form "like a wedge; others pyramidal. In the dark you well know, that "not all, but very many, of the said bullions will sparkle and glister "with tails like comets: and it is clear, that when the Electricity is "very vigorous, the helmet upon the head of the person electrised will "dart forth rays like those round the head of a canonised Saint; and this "is my beatification. You are the first, Sir, with whom I trust my my-"stery, which if you communicate to the *Royal Society*, I hope you will "take care of it's being inserted in the *Philosophical Transactions*, that "the beatification did not succeed until I communicated my method. "Many people have imagined this experiment of mine to be extrava-"gant and false. If the armour is not ornamented with steel bullions, "I believe it will not succeed. If the armour is well enriched with bul-"lions, and well polished, the comets appear twice, once in the air, "and once by reflexion from the armour. A stomacher, or a doublet, "set with nails or needles, will exhibit a small degree of beatification."

Thus far Mr *Bose*, to whom I am very much obliged, for the discovery of his process; I cannot but be sorry for his having, as he says, embellished his relation by his stile and expressions. The language of Philosophers should not be tainted with the licence of the Poets; their aim in the communicating their discoveries to the world, should be simple truth without desiring to exaggerate; as we constantly see enough to raise our admiration every step we take in investigating the operations of nature.

The electrising a man in polished armour, with several globes, must exhibit a very beautiful *phænomenon*, by the Electricity running off from several of the points; but I cannot but say, it must fall greatly short of the general radiation promised and expected from the preceding accounts.

Extract of a letter from the Abbé Nollet, *F. R. S.* &c. *to* Charles *Duke of* Richmond, *F. R. S. accompanying an examination of certain*

39. If your Grace shall have done me the honour to have perused the treatise I sent, intituled, *Recherches sur les Causes particulieres des Phenomenes electriques*, you will have seen my doubts touching the reality of certain facts published in *Italy*, and which have not succeeded any-where else. I will not dissemble, that the desire of knowing how far those things were true, has been one of the principal motives of my journey; and if you have been desirous to have learned the fruits of my inquiries in this respect, you need only look over the Memoir sent herewith, which

Phænomena in Electricity, published in Italy, by the same, and translated from the French, by Mr Watson, F.R.S. Ibid p. 368. Dated Paris March 5. 1750. Read March 29 1750.

which I beg your Grace afterwards to present to the *R. Soc.* I well know how much that learned body interests itself in relation to the subject of this Memoir; and, as one of it's Members, I think it my duty to communicate the result of my labours. As I correspond with Mr *Watson*, who is well versed in these matters, it may not be disagreeable to him to put these papers in a condition to be laid before the *R. Soc.* I have made the whole tour of *Italy*, which has enabled me to make many observations relating to Natural Philosophy. I have made some experiments at the *Grotto del Cani*, near *Naples*, which take off a good deal, in my opinion, of the marvellous of that famous *phænomenon.* I propose to myself the honour of transmitting them upon some future occasion, as my letter is already too long. The eructations from *Vesuvius* were very great when I was there, and were the prelude to 3 earthquakes, which happened just after my departure, and which I was fortunate enough not to be witness of. The *Lagunes* of *Venice*, and the waters of the *Mediterranean* Sea, appear luminous every-where in summer, in dark nights: I have discovered, that this light proceeds from a very small insect, which multiplies prodigiously. I have heard all my life, that the water of the ocean appears sometimes luminous: It may possibly proceed from the same cause, and I should be very glad of a particular inquiry into this fact.

An Examination of certain Phænomena in Electricity, published in Italy.

Electricity, after having excited every-where the emulation of the ingenious, after having filled us with wonder by an infinite number of *phænomena* more singular and more admirable one than another, seems, within these few years, to have shewn itself equally surprising, but more useful, in *Italy*, than it had done in *England*, *France*, *Germany*, &c. where, for these 20 or 25 years, so great a progress had been made. We have heard of nothing less than the cure, or the almost sudden relief, of distempers of every kind, and of purging all sorts of persons in a manner of all others the most proper to avoid the repugnance and disgust we naturally have to medical potions. Even that disease which we are most desirous of concealing, was not by these means without it's remedy; the mercury being volatilized, and carried, by the electric matter, into the body of the patient, tinged his skin of a leaden colour, and procured him a certain cure by a copious salivation.

The manner in which this was done was not less to be wondered at than the thing itself; persons afflicted with inveterate gouts, rheumatisms, fluxions, tumours, *&c.* were relieved therefrom by being electrized for a few hours, and often a less time was sufficient. Sometimes the rubbing a glass tube only, or at other times a glass tube lined with some medicine appropriated to the disease of the patient, was employed. These medicines, to exert their operation upon the patient, passed thro' the glass; and this they were very certain of, as they saw them sensibly diminish in their quantity, although the glass containing them was stopped as close as though sealed hermetically. To promote stools, it is only necessary that a person should be electrized for 6 or 8 minutes, holding

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in his hand a piece of ſcammony or gamboge; the effects were as certain, as though theſe drugs were taken internally. Beſides, if a perſon was deſirous of being perfumed from head to foot, nothing more was neceſſary than being electrized with a glaſs veſſel lined with balſam of *Peru*, *Benjamin*, or ſome ſuch drug; and from this electrization the odours were perceptible for 2 or 3 days, even ſo much as to incommode thoſe to whom theſe ſmells were diſagreeable.

Effects no leſs wonderful than theſe were publiſhed every day by writings printed and printed again *, or by particular Letters and Memoirs in manuſcript addreſſed to the ingenious all over *Europe*. They were alſo confirmed by reſpectable witneſſes, and by ſuch as were capable of impoſing them upon perſons the moſt guarded againſt the exaggerations, which never fail accompanying the relations of intereſting novelties.

The importance of the facts themſelves, and the appearance of authenticity which attended them, demanded that they ſhould be conſidered; and indeed they rouſed every-where the attention of thoſe Philoſophers, who had for any time turned their thoughts to theſe enquiries. Every one of them was deſirous of repeating what Mr *Pivati* ſaid had been done at *Venice*, Mr *Verati* at *Bologna*, and Mr *Bianchi* at *Turin*; and to begin them, as the experiment ſeemed more ſimple, they attempted at firſt the tranſmiſſion of odoriferous ſubſtances through the pores of the glaſs, the firſt foundation of *Intonacatores* †, ſo called by Mr *Pivati*; and which we ſhall, in the progreſs of this paper, call medicated glaſſes; and they endeavoured to purge perſons of all ages, and of both ſexes, by making them hold in their hand, while they were electrized, Scammony, Gamboge, Aloes, and ſuch-like. But it was very extraordinary, that of all the perſons who were engaged in theſe experiments, no one could ſucceed; and, from a ſort of ſhame, each of them expected, that ſome one would complain of his want of ſucceſs: but this was retarded, as yet, by the haſte with which Mr *Winckler*

* The firſt of theſe works is a letter of Mr *Pivati*, an Advocate at *Venice*. It was firſt printed at *Lucca* in 1747. and ſome time after reprinted at *Venice*, always with the ſame title, *Della Elettricita, Letira Al chiariſſimo Signor Franciſco Pivati*, &c This letter was tranſlated into *French*, and printed at *Paris*. In 1748, there appeared another treatiſe, printed at *Bologna*, intituled, *Obſervationi fiſico-mediche intorno alla Electricita, da Gio. Giuſeppe Verati publ. Profeſſ. nella Univerſita, e nella Academia delle Scienze del inſtituto academico Benedettino*. In the ſame year, 1748, there was printed at *Verona* a little treatiſe concerning medical Electricity, intituled, *Lettra del Signiore Canonico Brigoli ſopra la Machina Elettrica*. Afterwards, in 1749, there was publiſhed at *Venice* a new treatiſe, conſiderably larger than the firſt, in which we find not only the author's own experiments, but alſo thoſe of Mr *Bianchi* of *Turin*, and of ſome other perſons who had taken pains with this view. This laſt work is intituled, *Reſleſſioni fiſice ſopra la Medicina Elettrica*. It is principally in this volume that the facts of which we are now treating are mentioned.

† Mr *Pivati* has given this name to the manner in which he prepares hollow cylinders of glaſs in filling them, or lining them, with ſome drug, the virtue of which, he pretends, will tranſude with the electric matter.

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ſent to the R. Soc. and to ſome ingenious men in *France*, the reſult of his own experiments, which well agreed with thoſe of *Italy*, and upon the credit of which he had made them.

For my own part, I will ſpeak without any reſtraint: when I found my attempts were fruitleſs, I without any difficulty communicated it to all the Philoſophers with whom I correſponded: I deſired them to let me know if they had been more ſucceſsful than myſelf, and to acquaint me how they had proceeded, that I might conform myſelf thereby. I was much more willing to confeſs my inability, and to learn from others the method which muſt of neceſſity be obſerved, than to be deprived longer from ſeeing thoſe *phænomena* which ought to reſult therefrom. Inſtead of inſtructions, which might conduct me to the ſucceſs I wanted, I received nothing but ſuch confeſſions as mine: from theſe I ſaw, that all methods had been tried; and that nothing remained to be done, but either to believe every thing upon the faith of others, or to doubt, without hopes of being better informed. The firſt of theſe two caſes was directly oppoſite to the law I had determined to abide by, when I firſt engaged in the ſtudy of Experimental Philoſophy; and the other was putting a great violence upon myſelf. But from this moment I formed my project of travelling; and, among the different motives which made me undertake the journey to *Italy*, I muſt confeſs one of the moſt preſſing was, the deſire of ſeeing ſucceed, in the hands of thoſe who had ſaid they had, thoſe *phænomena* in Electricity, towards the verification of which I had made ſo many fruitleſs efforts. I formed to myſelf a great pleaſure in ſeeing balſam of *Peru*, *Benjamin*, *Camphire*, *Cinnamon*, *&c.* pervade an electrized glaſs, which I had taken care to ſtop myſelf; to ſee people purged by the palm of their hands; to ſee an old gouty Man, as the Biſhop of *Sebenico* *, clap his hands together, ſtrike the ground with his feet, and walk freely, after an Electrization of 2 minutes: but what ſtill more piqued my curioſity was, to learn, if poſſible, why the *Italian* Electricity ſhould enjoy theſe prerogatives, to the excluſion of that of every other country. If this ſingularity was as real as it appeared to be, it was a new wonder more difficult to be explained than any other; and of which I propoſed to ſtudy attentively the circumſtances, to endeavour to find out the cauſe.

If I have had the trouble of paſſing the *Alps*, to ſearch out the truth, it is neither to conceal it, nor yet leſs to diſfigure it with falſhood; and I will relate, with a liberty truly philoſophical, all that I have heard, and all that I have ſeen: but if, in doing this, I ſhall find myſelf obliged to contradict ſome of the facts publiſhed by ſome perſons known in the republic of letters, I proteſt that it is without prejudice, on my part, to the advantageous idea I may have of their candour or abilities; and I ſincerely wiſh, that the reader may conſider them in the ſame manner. If he is judicious, he will willingly concur with me; becauſe, in an enquiry ſo

* See Mr *Pivati*'s letter, printed at *Lucca*, p. 37.

obſcure as this of which we are now treating, an ingenious man, with a very juſt intention, may take what is falſe for that which is true.

I arrived at *Turin* about the beginning of *May* 1749. and one of my firſt cares was, to viſit Mr *Bianchi*, a celebrated Anatomiſt, and the firſt author of purging by Electricity. I related to him all that he had written to me upon this ſubject; and I begged of him, that all the experiments, which had neither ſucceeded with me, nor a great many others, might be repeated between us, and under his direction. His complaiſance eaſily granted what I deſired: we ſet about it; and Pere *Garo*, a *Minim*, and Profeſſor of Philoſophy in the Univerſity, cauſed to be carried to the place where we determined to make our experiments his electrifying machine; which is exactly like that which I have deſcribed in my Eſſay, *Page* 19. *Fig.* 2.

The Experiments of the first day.

May, 21. about 4 in the afternoon, the weather cool, but uncertain, Mr *Bianchi* having procured a lump of ſcammony, and another of gamboge, each of which was about the ſize of an hen's egg; I took the former in my right-hand, and having applied my left near the ſurface of the glaſs globe, and ſtanding upon a cake of reſin, I was electrized 15 minutes without interruption. This day the Electricity was indifferently ſtrong.

After me, a young man, aged 22, and of a pale complexion, was electrized; whom, a few days before, I had taken into my ſervice.

They then electrized a young woman of about 16 or 17, of a weakly conſtitution; but who, at that time, was tolerably well.

After that M. *Beccari*, Prof. Philoſ. in the Univerſity, aged about 35, of a dry habit, was electrized.

They then electrized a ſervant belonging to the houſe where they made the experiments, aged about 24, who did not appear to be indiſpoſed.

They alſo made the ſame experiment upon another ſervant, a ſtrong man of 40, or thereabouts; and each of theſe perſons was electrized the ſame time as I had been; that is, fifteen minutes ſucceſſively.

I did not perceive in myſelf any effect, which I could attribute to the Electricity; no extraordinary motion or pain in my bowels; and it was the ſame with M. *Beccari*, with the ſervant aged 40, and with the young woman.

But the young man of 22, being interrogated after the others, ſaid, that he had had in the night 2 ſtools, and ſome complaints of the colic. The ſervant of the houſe, who was aſked the ſame queſtions, declared, that he had had a very large ſtool, as though he had taken a purge.

Theſe two laſt depoſitions were, as the others, taken upon the ſpot; and I began to conſider them as important, when I learned, from the confeſſion of the laſt, that he had taken, for ſome days, a decoction of wild ſuccory, for an indiſpoſition which he had not ſpoke of till then. The young man who ſaid he had had 2 ſtools, rendered his teſtimony more than suspicious, by certain singularities * which he was desirous of adding some hours after; and since that time he has conducted himself in such a manner, as to prevent my having any confidence upon what he said.

What I have just now mentioned to have found in these two servants, one of which kept me ignorant some time of his having taken broth with succory; and the other having testified such a love for the marvellous, that one ought in prudence to suspect every thing he said; this, I say, made me very delicate in the choice of the persons who I was desirous should be admitted to our experiments. I declared that I was not willing to receive thereto either children, servants, or people of the lower class; but only that reasonable people should be admitted, and of an age sufficient to leave nothing to be feared of the truth of what they might depose.

The second Experiment. The day after we had made our first set of experiments I was again electrized 15 minutes successively, as I had been the day before, holding in my hand a large piece of scammony; and after me there went successively through the same trial Dr *Scherra* a Physician, Mr *Verne* Demonstrator of Anatomy, the Marquis of *Sirié*, the Abbé *Porta* a Professor in the University, the Preceptor to the children of the Marquis *D'Ormea*, and the Preceptor to the young Messieurs *D'Osa*. This day the Electricity was indifferently strong.

Of all these persons who were electrized, not one felt any pains in his belly, no one had any evacuation which could be attributted to the electrical power; but to say scrupulously all that came to my knowledge, after several questions, the Preceptor to young Messieurs *D'Ormea* declared, that he had parted with more wind than he had usually done, and he believed also with more urine. Thus of 7 persons there was but one who suspected the operation of Electricity to have had any sensible effect upon him, and this suspicion, as we see, was a very slight one.

The third Experiment. *May*, 23 the Electricity being more strong than the preceding days, we chose a piece of new † scammony, very strong in it's flavour, and which weighed ℥iv. the Marquis *D'Ormea*, Dr *Allion*, a Physician, the 2 above-mentioned Preceptors, Pere *Garo*, Count *Ferrero*, and myself, held, one after the other, this piece of scammony, and each was electrized 15 minutes, as had been done in the former experiments.

Two days passed, and absolutely none of these persons perceived any thing, that could be attributed to the Electricity.

* This young man made himself very happy in relating to every body, that he had been electrized; and that he had been purged thereby, as though he had taken physic: and added, that, an hour after his electrization, having had the curiosity of visiting his wife, to see what would be the consequence, he had communicated this Electricity to her, and that she had been purged as well as himself.

† M. *Bianchi* suspected, that the drugs we had made use of in our first experiments had lost their most subtile parts, only capable, as he said, of being introduced with the electric matter.

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The ſame day we endeavoured to repeat an experiment, which M. *Bianchi* had writ me word of ſome months before, and which had not ſucceeded with me at *Paris*. This experiment was the tranſmiſſion of odours along a chain, or an iron bar electrized. One of us prepared and applied a little piece of linen, covered with balſam of *Peru*, upon the iron bar, which received the Electricity from the globe: we faſtened to this rod the end of an iron chain, which was electrized by communication; and we expected, that the odour of the balſam would be tranſmitted to the other end of the chain, to which was hung a ball of metal. But this was expected in vain; nobody could perceive the ſlighteſt ſign of this tranſmiſſion. *The fourth Experiment.*

M. *Bianchi*, ſeeing, as I did, that the reſult of all theſe experiments did not agree with thoſe, which he had believed to have taken place before, told me, that this difference might ariſe from our having employed an Electricity too ſtrong: becauſe that which he had experienced with ſucceſs had always appeared more weak. I ſubmitted to this reaſon, having no other to give him more plauſible; and to bring the whole operation, as near as might be, to it's firſt circumſtances, we met together, to the number of 14, at M. *Bianchi*'s, where we were expected; and we were electrized, one after the other, by him, as long a time as he judged proper, ſometimes with ſcammony, and ſometimes with gamboge, which he himſelf had choſen.

The machine uſed this day was the ſame, with which M. *Bianchi* had always made his own experiments. It conſiſted of an hollow glaſs cylinder, 3 inches in diameter, and ſomething more than ½ a foot in length *, mounted between two ſupporters upon a board, which was faſtened to a table with ſcrews. This cylindrical veſſel was turned round, without any other intermediate apparatus, by an handle, which was at leaſt 4 inches in it's radius; ſo that the hand, by which this machine was turned, revolved with greater velocity than the ſurface of the glaſs cylinder, which was thereby put in motion.

This machine had this convenience, that one perſon only might turn the handle with one hand, and rub the ſurface of the glaſs veſſel with his other: but there is no difficulty in comprehending, that the Electricity could not but be always very weak with ſuch a cylinder, and from ſuch friction; ſo that, in the experiments of this day, we were ſcarce able to perceive any ſnaps, in touching the iron chain, by which the Electricity was communicated, or from the perſon electrized; but this was preciſely what was deſired.

Theſe experiments were made on *Thurſday*, *May* 29, between 4 and 6 in the afternoon, in a very hot and ſerene day: on *Sunday* evening, all the perſons who had been electrized, being interrogated, anſwered without heſitation, and in a manner abſolute in all reſpects, that they had perceived nothing which could be attributed to theſe experiments:

* The author here means the *French* foot.

these perſons were the Marquis *de Siria*, Count *Ferrero*, the Marquis *D'Ormea*, M. *de Tignola*, an officer of artillery, Pere *Beccari*, Pere *Garo*, Dr *Allion*, M. *Verne*, Dr *Scherra*, the Abbé *Porta*, the 2 Preceptors, the young woman, whom I mentioned before, and myſelf.

The night following, that is to ſay, that between *Sunday* and *Monday*, I was troubled with an indigeſtion, and felt pains of the colic; but I attributed them much leſs to the being electrized the preceding *Thurſday*, than to ſome roots I had eaten the day before at dinner, and to a very large glaſs of iced lemonade, which I had drank ſome time after, and contrary to my uſual cuſtom. Nevertheleſs, as ſome perſons were deſirous of ſaying, that the electrizing had purged me, and that I had not the candour to ſpeak of it, I thought it my duty to add here for my juſtification, that during my whole life I have had a weak ſtomach; that I could never take ice, nor liquors very cold, without a good deal of circumſpection, and always at the hazard of being incommoded therefrom; and that theſe roots, which are called *Ravanelle* in *Piedmont*, notwithſtanding my attention to eat ſparingly of them, had oftentimes diſturbed my digeſtion, during my ſtay there, and at times when I had no concern in electrical experiments. Beſides the not being incommoded for 3 days, and more, was ſufficient to prevent my attributing what happened to me to the electrical power.

The extreme circumſpection, with which I was deſirous of chooſing the perſons for all our experiments; the difficulty of procuring and moving ſuch ſick people, who were in a condition and diſpoſition to leave nothing to be feared on their parts from their prejudice, and their heated imagination; that of reconciling my time with that, which a Phyſician of great practice could grant me; theſe obſtacles, I ſay, prevented my attempting with M. *Bianchi* ſuch cures, as he believed to have been brought about by means of the electric virtue, either by it's own action, or by joining thereto Medicines appropriated to the condition of the ſick, and contained in glaſs veſſels electrized by friction. But I teſtified a great deſire of ſeeing thoſe perſons who had been cured, or conſiderably relieved, by this method before this time. I aſked, for this purpoſe, the gentlemen of the Profeſſion, who had been witneſſes of the experiments, and who were yet in a condition of ſeeing every day ſome of the perſons, cited in a manuſcript which I had of M. *Bianchi*'s; and of whom the exact hiſtory is mentioned in the 9th Chapter of a Treatiſe of M. *Pivati* *: I went myſelf to the Shoemaker, in whoſe ſhop the young man of 21 years of age worked, mentioned in the 110th page of the above treatiſe, and in page 419 of my *Recherches* †. The obligation of ſaying the truth, to which Philoſophers ought to ſacrifice every human

* *Refleſſioni fiſiche ſopra la Medicina elettrica*, p. 149.

† They have tranſlated *Hatter*, inſtead of *Shoemaker*, in taking the *Italian* word *Calcetario* for that of *Calzolaio*, which was ſufficiently legible in the manuſcript.

regard,

regard, will not permit me to diſſemble that my inquiries made with all poſſible diligence, and without any other intereſt than that of knowing the truth, have made me ſee ſufficiently clear, that theſe facts have been greatly exaggerated. I am willing to believe, that it is the fault of the ſick, who, being prejudiced perhaps by too great hope, and poſſeſſed by a kind of enthuſiaſm, have ſaid themſelves, and made others believe, more than really was the caſe. One might have examples enough to cite of ſuch illuſions; but be that as it will, I cannot help believing, that a great part of the electrical cures of *Turin* have been no other than temporary ſhadows, which have been taken with a little too much precipitation, or complaiſance, for realities.

I carried with me to *Venice* the ſame curioſity, and the ſame deſire of being inſtructed, upon the ſubject of the tranſmiſſion of odours from medicated tubes, and of the cures, or of being relieved from diſorders almoſt ſuddenly, by the electrical power. One of my firſt cares was, to find out ſome friends or acquaintance of M. *Pivati* to acquaint him of my arrival, and to obtain of him the favour of being admitted into his laboratory; and that he would have the complaiſance to ſatisfy my great deſire of ſeeing him cauſe odours to pervade the ſides of a well ſtopped glaſs, or by electrifying to diminiſh ſenſibly any ſubſtance therein contained. M. *Angelo Quirini*, a *Venetian* gentleman, a great friend to the Sciences, and one always ready to aſſiſt thoſe who apply themſelves thereto, did me this ſervice among many others, for which I am indebted to his friendſhip and politeneſs. He accordingly acquainted M. *Pivati*; and on *Auguſt* 1, 1749. we waited upon him, and found there a large company, among which were ſeveral perſons of diſtinction: among others were M. *Antony Moſſinigo*, heretofore Embaſſador in *France*, Abbé *Horter*, &c. At the ſight of this great aſſembly I believed (and I had ſome reaſons for believing it) that my curioſity had been * ſuſpected of diſbelief, and of an obſtinacy to doubt; this company therefore was called together to be an evidence of my conviction. I would have been willing to have bought at this price the pleaſure of ſeeing a *phænomenon*, for the verifying of which I had taken ſo much fruitleſs trouble. The manner of making it ſucceed had been without doubt ſome novelty to me, as curious itſelf as the effect which ſhould have reſulted therefrom. But how great were my ſurprize, and my regret, when M. *Pivati* declared frankly to me, in the preſence of this whole company, that he would not attempt to ſhew me the tranſmiſſion of odours; that that *phænomenon* had not ſucceeded but once or twice, as he had ſaid in his firſt letter printed at *Lucca*, although ſince that he had made many attempts to repeat that experiment, with the ſame as well as with other

* I had been acquainted, that my arrival at *Venice* had been notified by letters from *Turin*, which had deſcribed me as a man ſo prejudiced againſt facts, that the ſtrongeſt could not make me believe. In this they did me great injuſtice; unleſs they took for incredulity on my part the precaution I took, againſt illuſion, and falſe appearances.

glaſſes;

glasses; that this cylinder had been since broken; and that he had not so much as kept the fragments of it!

But at least, I told him, I might see him use one of his medicated tubes, and weigh it before and after electrifying, to see, with him, the included matter diminish sensibly. This fact, he told me, had succeeded with him a great many times; but that now there was too much company; that it was too hot, and, in consequence, that the Electricity would be too weak for it. He might perhaps be in the right: but why did he call together so numerous a company?

I then asked him concerning the cures related in his works, and especially concerning that of the Bishop of *Sebenico* *. He avowed to me (and in part I knew it already), that the Prelate was not cured; and that, since the electrification, he had been as he was before.

I took my leave of M. *Pivati*, and acquainted him, that I proposed to continue about a week in *Venice*; and I very earnestly begged of him to collect together his best vessels, to renew the substances therein, and to let me know, that, if they succeded, I might wait upon him, that I might be able to publish them as an eye-witness; and I spoke to him with a good deal of sincerity. M. *Pivati* promised me he would; but, as I heard nothing from him afterwards, I presume that he had nothing to shew me.

Dr *Sommis*, of the faculty of Physic at *Turin*, being at *Venice* a little while after me, had also the curiosity of visiting M. *Pivati* in *August* last, and to see, under his management, the effects attributed to the medicated tubes. The following is the letter † he wrote me upon this subject, *Nov.* 15. 1749.

"Here is, kind Sir, in a few words, the account of what I observed
" in *Venice*, at Signor *Pivati*'s, during the month of *August* last. The
" 25th day, after dinner, he electrified me, making use of a tube of
" the length of about 5 inches, and a little more than 2 in diameter,
" causing me to hold in my hand an ounce of scammony. There were
" present at this experiment his Excellency the Abbate *Barbarigo*, the
" Fathers *Bertinelli* and *Magrini*, Jesuits, Dr *Grampini*, and several
" other persons. I found not any change in myself either that evening
" or the following day. The 29th of the same month I returned again
" to Signor *Pivati*, where I found a gentleman of the House of *Soranzo*,
" 2 *Spanish* officers, 2 other *Venetian* gentlemen, a Physician, and some
" others; and he caused a tube to be lined [or plastered within] for the
" experiment, which was represented by him in a dangerous light; but
" which was not such however as to hinder my telling him, that I desired
" that the experiment might be made upon myself. He began then to
" electrize me at 35 minutes after 5 in the afternoon, and made an end,
" because the line of the wheel tangled, at 57 minutes after 5. Having

* See page 374, *suprà*.
† This letter is translated from the *Italian*.

" then

“ then new-fitted the line, he began again at 5 minutes after 6, and
“ continued till 14 minutes after, making again this time the ſparks to
“ iſſue continually from my forehead. This tube was nearly of the ſame
“ length and ſize as the former. The experiment being over, I then
“ prayed him to tell with what materials the tube had been lined; and
“ ſo much the rather, as he had let fall in diſcourſe with the *Spaniſh*
“ gentlemen, that they might have ſeen me ſleep; and he anſwered me,
“ that the tube was lined with ℥ij ʒvi of flowers of *Benjamin*, and two
“ drams of *Opium*. Having heard him mention the *Opium*, I prayed
“ him to take the trouble of making another experiment, his Excellency
“ S. Abbate *Pietro Barbarigo*, and myſelf alſo, having with us ℥ſs of
“ *Opium*; and he complied with my deſire. He therefore electrified
“ his Excellency, making him hold the *Opium*, that is to ſay, the
“ quantity of ℥ſs of it, in his hand, and the ſparks iſſuing from his
“ hand for half an hour together, beginning at 18 minutes, and finiſh-
“ ing at 48 minutes, after ſix. In this ſecond experiment he made uſe
“ of the ſame tube which he had uſed the *Monday* before, the 25th of
“ the ſame month: but neither his Excellency nor I ſlept more than
“ ordinary. Theſe are the experiments which I made at *Venice* with
“ Signor *Pivati*. In my return home, paſſing thro' *Placentia*, I here
“ ſpoke with Dr *Cornelius*, who aſſured me, in preſence of Dr *Riviera*,
“ that he alſo had tried a great many times to purge others by electri-
“ fying them; but that it had never ſucceeded with him but once, which
“ was upon a maid-ſervant, to whom he had given ſome rhubarb to hold
“ in her hand. Notwithſtanding which, finding it never to have pro-
“ duced the ſame effect in any other perſon, it rather ſeemed to him,
“ that ſome other cauſe might have occaſioned what happened to his
“ maid.”

We ſee then from this letter, and from the account I have before given of my viſit to M. *Pivati*, that I have not been able to verify at *Venice* any of thoſe facts, in which my curioſity was intereſted. I might add alſo (and I ought, without doubt, ſince I have engaged to mention exactly every thing I have been able to find out from my enquiries upon this ſubject), that of all the perſons of the country, who have been with M. *Pivati*, to be certified of the truth of his experiment from ocular demonſtration, and whom I was able to interrogate, I found but one who atteſted them, as having ſeen them: this was a Phyſician, a friend of M. *Pivati*, whom I found at his houſe, and who had, as he ſaid, almoſt always aſſiſted him in his experiments.

From *Venice* I went to *Bologna*, where I became acquainted with Dr *Verati*, a Member of the Academy *De l'Inſtitut*. From the frequent converſations I had with him, I was convinced that he was a learned, wiſe, and candid man, as I had heard before. I laid before him, without ſcruple, the doubts I had, touching the tranſmiſſion of odours, the effects of lined tubes, purging by electrizing, as well as the almoſt ſudden cures.

Dr

Dr *Verati* anſwered me, firſt, "That he had made many experi-
"ments, from the reſult of which it ſeemed to him, that the odour of "balſam of *Peru* pervaded from within to the outſide of a glaſs cy-"linder which he ſhewed me." This tube however, at this time, would not convince us of it's having been done, although we rubbed it with our hands very ſtrongly. But upon my repreſenting to him, that as the glaſs was cloſed only with wooden ſtoppers, which could be taken off at pleaſure, to put in or take out the odoriferous ſubſtances, it might happen, that the odours, agitated by the heat might have paſſed through the pores of the wood; he anſwered me, "That this was poſſible; and "although appearances had inclined him to believe the tranſmiſſion of "theſe odours through the pores of the glaſs, he had nevertheleſs ſuſ-"pended his judgment upon this effect, as well as upon that from lined "tubes, until new proofs, made with more precaution, ſhould have en-"tirely diſſipated his doubts. Secondly, with regard to the purging by "Electricity, he had in his houſe a man and maid-ſervant, who had "been purged in this manner: that at leaſt theſe two perſons had felt "the ſame effects as though they had taken Phyſic; after having been "electrized in M. *Bianchi*'s manner: that this effect having no other "apparent cauſe than the preceding electrization, *the great number of* "*facts of this kind, which had manifeſted themſelves at* Turin, had de-"termined him to believe, that what happened to his two ſervants was "the natural conſequence of this electrization: that, with regard to the "reſt, he propoſed to try the experiment again upon a ſufficient num-"ber of perſons of another ſort; and if this method of purging was not "conſtant, according to the idea he had had thereof he would correct, "with great freedom, what he had publiſhed thereupon in his works, "printed in 1748."

Thirdly, Dr *Verati* aſſured me, "That the ten cures, related in his "work juſt mentioned, were exactly made in the manner they are de-"ſcribed:" and they are related with a good deal of prudence, and with a ſimplicity which characterizes the truth. The fifth of them was told and certified to me by the perſon himſelf, one day when I viſited Father *Trombelli*, Abbot of the houſe in which he lives. Theſe cures are not ſuch as give me difficulty to believe them: we ſee, at leaſt, that they are made with ſpeed: we ſee that the diſorder, if I may be allowed the expreſſion, defends itſelf againſt the remedy, and does not give place but by little and little; and that nature makes no ſudden tranſition from one ſtate to the other abſolutely different, by the means of an Electricity ſcarce ſenſible. Theſe cures, I ſay, give me no trouble to believe them; becauſe it appears to me natural enough, and I have ſaid it a great while ago *, that a fluid, active as the electric matter, and which paſſes into our bodies with ſo much eaſe, may produce therein, in time, alterations either ſalutary or pernicious.

* In a diſcourſe read to the *Royal Academy of Sciences* juſt after *Eaſter*, 1746.

I learned

I learned nothing in the other cities of *Italy*, which did not ſtrengthen my doubts in relation to thoſe electrical *phænomena*, which I had a deſire to verify in the courſe of my travels. Pere *La Torre*, Profeſſor of Philoſophy at *Naples*; M. *De la Garde*, Director of the Coinage at *Florence*, one who has been much engaged in theſe inquiries; M. *Guadagni*, Profeſſor of experimental Philoſophy at *Piſa*; the Marquis *Maffei*, at *Verona*; Dr *Cornelio*, at *Placentia*; Pere *Garo*, at *Turin*; all theſe, I ſay, with very excellent and well-contrived machines, and with a great deſire of ſucceeding, have attempted many times to tranſmit the odours, as well as the powers of drugs cloſed (carefully) in tubes or ſpheres of glaſs, by electrizing them: all theſe have attempted to purge a number of perſons; and, according to the accounts they gave me, have never gained their point; or the little ſucceſs they had, appeared too equivocal to draw therefrom conſequences conformable to thoſe M. *Pivati* had believed to have ſeen in his experiments.

I am now then, as it were, certain of what I began to believe laſt year, when I printed my treatiſe, intituled, *Recherches ſur les Cauſes particulieres des Phænomenes Electriques:* I am, I ſay, as it were certain, that M. *Pivati* has been deceived by ſome circumſtance to which he had not given ſufficient attention; and what makes me believe it more than ever is, that he aſſured me himſelf, that this transfuſion of odours, and of drugs, through electrized glaſs veſſels, had never manifeſted itſelf to him but once or twice directly; I mean by a ſenſible diminution of bulk, and by ſuch emanations as the ſmell was capable of perceiving.

Since I have underſtood *Italian*, I have been ſurpriſed not only to find this confeſſion in a letter printed at *Lucca* *, but alſo to ſee, that it had not had all the effect it ought to have had upon the minds of thoſe, who have been in a ſituation to be inſtructed: for my own part, had I known it earlier, I might have ſaved myſelf a great part of the trouble I have taken in verifying the fact; and I am aſtoniſhed, that they ſhould be deſirous of building upon ſuch very ſlender foundations.

It is however upon this pretended tranſmiſſion, and with a glaſs tube, *which was cracked from one end to the other*, as M. *Pivati* tells you himſelf †; it is, I ſay, upon this fact, than which, in my opinion, nothing can be leſs certain, that they have eſtabliſhed the uſe and effects of *lined tubes*, of which they are willing to abate nothing. But how can we reconcile theſe two things, the almoſt never failing operation of *lined tubes*, upon ſo many diſtempers which are ſaid to have been cured, or conſiderably relieved, on one part, and on the other the tranſmiſſion ſo very ſeldom to be perceived of the odours of the drugs incloſed in thoſe glaſſes,

* Page 28. *Un tale dileguamento ſuccedutomi in un cylindro, non mi è poi veramente ſucceduto in altri, di quali mi ſon ſervito per varie guarigioni*

† *Si conſumo la materia interna a ſegno, che ſi riduſſe non oſtante l'eſſere quaſi ermeticamente ſerato alla ſottiliezza di un delicato foglio di carta, e come un capo morto, che ne tenea più odore ni ſapore, e fino il vetro medeſimo quaſi conſunto ſi apri da ſe ſteſſo in più ſiſſure per longo.*

with which you electrize? If it is truly the *Peruvian* balſam, the benjamin, camphire, *&c.* which, being animated by the electric matter, have brought about ſo many cures, as M *Pivati* has given us in his writings, why do not theſe ſtrongly-ſcented ſubſtances ſend forth their *effluvia* copiouſly, and always, in thoſe places where the experiments are made? And why do they not communicate themſelves by their odour to all perſons, who are penetrated thereby by means of electrification? Will they ſay, that the Electricity, ſpecifically operating upon their medical virtue, ſeparates it from their odoriferous quality? Miſerable ſubterfuge! Which does not merit to be oppoſed ſeriouſly; and the more ſo, as it is by the transfuſion of their odours, that they pretend to be aſſured of the efficacy of their *lined tubes*.

I am diſpoſed to believe, that the Electricity may have cured or relieved diſtempered perſons; but I do not find the proofs of M. *Pivati* ſufficiently ſtrong, or ſufficiently certain, to make me conceive, that the *lined glaſſes* have contributed to theſe good effects. I think, and M. *Verati* himſelf appeared to me pretty much of the ſame opinion, that if any one has been ſo happy as to cure diſtempers by electrifying with glaſſes containing drugs, all that can be ſaid in favour of theſe ſubſtances is, that they have not hindered the operation of Electricity.

M. *Pivati* appears by his converſation an honeſt and diſintereſted perſon, and one capable of inducing me to be of his opinion: but among the facts which he collects in his writings to fortify his proofs, I find ſome that do not do much honour to his delicacy in chooſing; and which may make him ſuſpected of too great credulity. Would one believe with him, for example, that the electric virtue was capable of ſetting a watch a going, which was ſtopped; and, by it's means, of regulating it's motion, when ſo diſordered as to be impracticable to be done by the hands of the workmen *? Would one believe with him upon the faith of a letter void of authority, and without having tried it, "that an "ounce of mercury had been entirely evaporated through the pores of a "glaſs veſſel, with which a man was electrized, which had made his "ſkin of a leaden colour, and which had been followed by a copious † "ſalivation?" This fact, which was ſaid to have been done at *Naples*, intereſting as it is, had there made ſo little noiſe, that I was not able to find any traces of it during my ſtay in that city, after the printing and publiſhing of the book, in which it is cited.

If any one ſhould think fit to ſay, that it is from humour, or from ſome perſonal intereſt, that I am ſo obſtinate in diſbelieving the facts publiſhed in *Italy*, which are the ſubject of this Memoir, I flatter myſelf, that ſo unjuſt an imputation will make no impreſſion upon reaſonable people, by whom I have the honnour of being known, either perſonally,

* *Refleſſioni fiſiche ſopra la medicina ellettrica*, p. 103. *La ſubita efficacia (della ellettricita) in dar giuſto movimento alle moſtre, di orologio, o ferme, o reſtie, o ritardanti ſanza rimedio.*

† Ibid. p. 153.

or

or by my writings. Have not I received and published in *France* all the wonders in Electricity, which have manifested themselves in *England*, in *Germany*, and in *Holland*, as soon as I have been able to be assured thereof by repetition of them? Have not I spoken and written concerning the cure of the paralytic of *Geneva*, as a man truly persuaded of the truth of the fact, since it had been so justly authenticated *? By what caprice then am I made more difficult of believing what passed in *Italy* than in other countries, if the *phænomena*, which are pretended to have been seen there, could have been repeated; or if the testimonies, which they offered me, were not considerably weakened, or entirely abolished, when, being in the places themselves, I was in a condition of knowing their just value?

Had I only consulted my personal interest, to whom would it have been more convenient than to me, to have adopted these novelties? If they were real, they would have been so many evident proofs of a principle †, by which I have endeavoured to account for the electrical *phænomena*: a principle, which as yet has sufficicently well served me, and which, having offered itself to Mr *Watson* as well as to myself, has enabled him likewise to give some inferences exceedingly probable concerning them: would not odours, would not medical substances, carried through the pores of glass, prove, without doubt, that the *effluent* electric matter served them for a vehicle? If purging substances were forced to pass into the hand, and into the body, of an electrized person, could one doubt of their being introduced there by the *effluent* matter, which came to the electrized body? If the Electricity restores health to a sick person, in delivering him from some vitious humour, might not I say with great probability, that this effect is brought about by the effluence of the electric matter? Especially as I have demonstrated by experiments, made with great care, that this same matter, in going from the body electrized, accelerates, and considerably augments, the insensible transpiration of animals, and, in general, all organized bodies, replete with fluids.

I have then set apart my own interest to follow the truth; and if prejudice has tended to lead me astray, it would be in inclining me to receive rather than call in doubt the facts, which are the subject of this paper. It is only because I cannot consider them as true, that I refuse to believe them; and this even with regret, as they favour my system: this indeed is of no great importance; but what makes me more desire their reality, is, the great good which would result to society. Could any good subject, possessed of the art of healing by Electricity, as M. *Pivati* pretends to be, spend his whole time better than in devoting it to the relief of a great number of human creatures, afflicted with great variety of maladies? I am induced to believe, that the greatness of this

* See my Essay *sur l'Electricité des Corps*, printed at *Paris*, 1746, and my *Recherches sur les Causes particulieres des Phænomenes electriques*, 1749.

† *Essai sur l'Ectricité des Corps*, p. 148 et suiv.

idea has imposed upon those, who have published, without doubt, with a little too much precipitation, this new Medicine: the great desire of being useful has made them hope; and the goodness of their hearts making them dispense too easily with the severity of a necessary examination, it may be imagined, that they have considered as real success, what was in truth only a phantom.

It remains to say, that in these researches I have coveted truth, only for her own sake; and have no interest in convincing those who may think proper still obstinately to believe, what has been published concerning lined tubes, electrical purgations, instantaneous cures, *&c.* I do not pretend to make any of my opinion, but those, who, having read without prejudice what I have here related, may find themselves touched by my reasons: but if after this there can be any one, upon whom the love of the marvellous can make a victorious impression, I shall not think ill of them, if they embrace opinions opposite to mine; *Qui vult decipi, decipiatur.*

An Observation on the Barometer, *by* Sam. Christian Hollman, Philos. Prof. Pub. Ord. Gotting. N°. 475. p. 239. Jan. *&c.* 1745. *Dated* Oct. 15. 1744. *Read* Jan. 10. 1744-5.

II. 1. The common Barometer, which I use, is sunk so deep at the time of my writing this, that there is only one line remaining of the common scale. The S. wind blows with great violence. But at 12 at night the quick-silver fell even below the last line of it's scale.

The Agreement of Barometers *with the changes of the* weather; by the same. N°. 492. p. 101. April, *&c.* 1749. *Read* April 23. 1749.

2. It has hitherto seemed to be a matter of great difficulty, to explain the true cause of the alteration of the height of the quicksilver in the Barometer, and the manner of it's agreement with the subsequent changes of the weather. That this is not unattended with some difficulty is *evident* from the hypotheses hitherto framed by the greatest men. That invented and published by *Leibnitz*, which has given rise to controversies between *Schellhamer* and *Ramazzini*, has been preferred before all others. But it has been sufficiently proved, by Dr *Desaguliers*, that this hypothesis of *Leibnitz* is contrary to the laws of Hydrostaticks: whence it is surprising, that so many have since endeavoured to defend it; especially as it is evident that it by no means agrees with the *phænomena* of Nature. Now if the cause of descent and ascent of the quicksilver in Barometers was that which *Leibnitz* has alledged, and if the quicksilver could not descend much sooner than the drops of rain began to descend thro' the incumbent atmosphere: and again if it could not ascend before they ceased to descend; both which are contrary to experience, as is well known by all who have attended the changes of the Barometer, even for a month or two. For the quicksilver will often fall for 3 or 4 days together before the least drop falls from the ambient air; and rise again, tho' it continues to rain for several days. Nay the quicksilver often falls, without the succession of any rain. Nor do the drops of rain, that fall at the end of it's descent, press the succumbent air in like manner as a weight, in the experiment of *Leibnitz*, descending thro' water

water preſſes the bottom of the veſſel in which it deſcended; nor can they reſtore to the air it's loſt equilibrium, as the weight does to the ſuſpended veſſel. Thus all things in the Leibnitian experiment are in nature difform and diſſonant, ſo that I am in doubt whether I ſhould moſt admire the genius of *Leibnitz*, a man in other reſpects of great merit, or rather the blind aſſent of his followers. To this we may add what is called a *fallacy* of *Leibnitz*, when he ſuggeſted that the cauſe of the equilibrium being altered, was that ſolids deſcending in a fluid do not gravitate during their deſcent into the ambient fluid. For the experiment ſucceeds beſt of all, when the diameter of the body deſcending thro' water, as of a leaden ball for inſtance, is very little leſs than that of the tube, and therefore almoſt fills the cavity of the tube, which is full of water. For becauſe the ball cannot deſcend, without expelling a volume of water ſimilar to itſelf, from it's place, it muſt impreſs upon it a motion receding from the bottom of the veſſel, and therefore in the whole time of that deſcent, the volume of water, which anſwers to the magnitude of the falling ball, muſt thereby receive a *niſus* and *impetus* by the natural force of bodies contrary to the centripetal force; and the tube, being wholly ſuſpended on the leg of the ballance, muſt *loſe* it's equilibrium, during the deſcent of the ball, till the ball reaches the bottom of the veſſel, and ſo a part of the water muſt ceaſe to be driven upwards. Thus the famous experiment of *Leibnitz* muſt be faulty and erroneous, and is moſt prepoſterouſly applied to the explanation of the mutations of the Barometer. I paſs over the *vitious circle*, which *Leibnitz* has been guilty of thro' his whole reaſoning. For he is to aſſign the *cauſe*, why the air is lighter before rain, and therefore makes a leſs preſſure on the quickſilver ſtanding in the veſſel of the Barometer, and yet he ſuppoſes it to be rendred lighter: for as long as the gravity and elaſtic force of the air, by which the exhalations might hitherto have been ſuſtained, are ſtill the ſame, they will never be reduced into drops, or deſcend thro' it. But it would be tireſome to add more in a thing ſo evident. I ſhall however ſay a few words with regard to the thing itſelf.

In the firſt place; it ſeems a great difficulty to moſt, what is the cauſe, why when the air is turbid and made more heavy by various exhalations, the quickſilver falls in the Barometers, and riſes again when it is clear, and is therefore rendered lighter? When every thing ought rather to happen quite contrary. But when the queſtion is thus formed, as we ſee it is done by moſt, is not ſomething tacitly aſſumed and ſuppoſed, which has never yet been proved, nor can eaſily be proved, namely, that the air is rendered heavier, when it is turbid and replete with various exhalations; and on the contrary lighter when it is clear? Nay is it not at the ſame time tacitly ſuppoſed, that thoſe vapours and exhalations *come into* our air, and diminiſh it's elaſtc force, at the very time that we ſee them: and on the contrary, that they *are gone*, and that the air is freed from them, and purified, as ſoon as they are withdrawn from our ſight? Therefore if we deny both theſe ſuppoſitions, the greateſt part of the difficulties

difficulties hitherto ſtarted immediately vaniſhes. But that they may, and ought to be denied may be proved by very ſubſtantial reaſons. For, who that is verſed in theſe matters, will affirm, that the motes in our circumambient air are not preſent before they are diſcovered to our ſight by the ſun beams ſhining into a dark room, or that they retire as ſoon as an open and free light is admitted? But not to mention a great many experiments that have been made with acid and alkaline ſalt, which are ſufficiently known, let us make uſe of an example, which is more to our preſent purpoſe. Who does not know, that in the glaſs bell put over the wet orb of the pnuematic engine, as ſoon as the pump begins to work, ſome light clouds begin to ariſe, which, as ſoon as the air is admitted again into the ſame bell, immediately diſappear? Now it would not be eaſy to find any one ſo unſkilful in theſe things, as to think, that the watery exhalations, from which theſe miſts ariſe, enter the glaſs bell at the time of their becoming conſpicuous to us, by the diminution of the elaſtic power of the air; or that they no longer exiſt there, after they are withdrawn from our ſight, and are again hidden in the pores of the air, which before ſuſtained them, after it is become again more heavy and elaſtic. We know therefore, that thoſe exhalations are preſent, before they approach nearer to each other, and become viſible to us; and we know that they are not annihilated, or no longer exiſting in the air, when being more diſſipated they eſcape our ſight, on account of their very great ſubtilty. But they begin to approach nearer to each other, when the elaſtic form of the air is ſo far diminiſhed, that they cannot eaſily be ſuſtained thereby as they were before; and recede again, and ceaſe to affect our ſight, when the air has it's former gravity and elaſtic force reſtored. Therefore *both* theſe things happen *at the ſame time*, that the elaſtic force of the air is diminiſhed, and at the ſame time the exhalations, which hung together in it, are again gradually looſe by it, and begin to appear to our ſight: and again, that the air recovers it's elaſticity, and at the ſame time, that the vapours hanging in it are diſſipated and diſappear; but *the one* cannot therefore be ſaid to be the *cauſe* of the other: nor can the air therefore be ſaid to be at one time more, at another leſs heavy, on account of the ſame exhalations.

Now if we ſuppoſe the ſame thing to happen in the air that ſurrounds our earth; the greateſt part of the difficulties is removed. For let us ſuppoſe it's elaſtic force to be diminiſhed, by any means whatſoever, of which we ſhall ſay more hereafter; the exhalations hanging in it muſt neceſſarily ſubſide and become viſible: and when it is by any means reſtored, the vapours and clouds therein muſt again be diſſipated and vaniſh. But at that very time when thoſe ſubſiding vapours come into our view; the mercury in the Barometer begins to fall, nay often ſome time before for the ſame reaſon; but yet thoſe vapours do not in like manner contribute any thing to it's deſcent, when the vapours and clouds begin to diſappear in our circumambient air, or even a little before the mercury begins to aſcend; and yet that ſerenity of the air is no more the cauſe of the

the aſcent of the mercury in the Barometer, than the aſcent of the mercury can be ſaid to be the cauſe of that ſerenity. It therefore again deſerves to be called a *ſallacy* of the *cauſe*, when one of thoſe *phænomena*, which happen about the ſame time, is referred to the other, and ſo one has hitherto been taken for the cauſe of the other by moſt perſons.

But to make all this apparent to the eye, take a glaſs cylinder about 3 or 4 inches in diameter, open at both ends, and long enough to receive a portable Barometer. Let this cylinder be put for a little while before the experiment on the orb of an air pump, covered with water and wet leather, that ſome watery vapours may in the mean time enter into the air therein contained. Afterwards let a Barometer be introduced, and let the cylinder be exactly cloſed at the top, that the air may be exhauſted. Now if all things are rightly prepared, and the included air begins to be pumped out; both a ſort of *miſt* will begin to *riſe* immediately in the cylinder, and the mercury of the Barometer at the ſame time to ſubſide: both indeed, as is apparent enough, from one and the ſame cauſe, but neither of them from the other. But if the ſame air, which was juſt now taken away, is immediately admitted again into the glaſs cylinder; the natural *ſerenity* of the included air will return directly, and the Mercury of the Barometer will aſcend at the ſame time or a little ſooner: and yet it is plain enough that one of them does not depend on the other. Nay at the ſame time it is manifeſt enough, that it is not rendered *heavier* in one caſe, nor lighter in the other. Thus art in ſome meaſure imitates nature: tho' becauſe of the wonderful complications of natural cauſes, it can never be ſufficiently exact.

But tho' the chief difficulty is now removed, yet the whole affair is not brought to a concluſion. For it ſtill remains to be enquired, by what cauſes the elaſtic power of the air can be ſo diminiſhed or increaſed, as to produce theſe alterations. But yet the anſwer does not ſeem very difficult. For the cauſes which ſhew how eaſily the air is expanded and rarified by any approaching heat, and particularly how great is the force of the *ſun-beams* falling perpendicularly, will ſhew, or perhaps, this alone will ſeem ſufficient, how the *equilibrium* of the air is taken away, if there were no others, which are however various. We will paſs over the *diurnal revolution* of the earth, and our air with it, about it's axis; and alſo the *annual motion* of them about the ſun; we will not mention the many *burning mountains* on the ſurface of our earth, nor the many thunders and lightnings in the air; nor the many *earthquakes* and *ſubterraneous fires*, that ſo terribly ſhake the ſurface of the earth and ſea, tho' each of them may have a wonderful effect in increaſing or diminiſhing the elaſtic force of the air: and ſhall at preſent only conſider one thing, which ſeems more worthy to be mentioned than the reſt.

It is allowed by all, that the elaſtic force of the air which immediately touches the ſurface of our earth, depends chiefly on the weight of the incumbent air. It is alſo no leſs known, that the lower air is more or leſs elaſtic according to the greater or leſs height of the incumbent column

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of air, by the different height of the barometrical mercury, on mountains of greater or less height, and in lower places of the earth. Nor is it less generally allowed, that the moon is the primary cause of the flux and reflux of the sea. But by what means soever this wonderful *phænomenon* is effected by the body of the moon on our earth, of which we need not here particularly treat; this certainly is past all doubt, that the moon cannot act on the seas of our globe, without acting at the same time on the *air which lies* between the moon and our earth. Now if the mobility of fluids is in proportion to their density, and their density in proportion to their specific gravities; the air nearest the earth, which is about 860 times lighter than fresh water, will be about 900 times more moveable than sea water: and therefore the very same cause, which gives so constant and regular a motion to our seas, acting by the same force, may much more easily increase or diminish the height of our air. By a sort of *flux* therefore arising in a determinate region of the air, the columns of air cannot fail of becoming *higher* in the same place, and therefore, *caeteris paribus*, the elastic force of the lower air must be increased: but upon a *reflux*, the height of the columns of air must necessarily be *diminished*, and so again, *caeteris paribus*, the elasticity of the lower air must decrease. And perhaps this difference of heights is the greater, as the specific gravity of the air, which is most rare in the utmost limits of our atmosphere, is overcome by the specific gravity of our sea waters, and of the lower air. But that this flux and reflux of the air does not observe it's alternate motions so regularly and constantly as the reciprocal tide of the seas; beside it's very great fluidity, thro' which it may be agitated by very slight causes, the causes above enumerated without doubt effect, by which it is manifest enough, that there are wonderful agitations and perturbations of the air, almost continually, in different parts of our terraqueous globe. From all these causes therefore taken together, the changes of the heights of columns of air in different times and places, and also the changes in the lower air depending on these, are to be sought in the barometrical *phænomena*.

There remains one thing to be just spoken of. We find many are sollicitous to *foretel the weather* from the rising or falling of the mercury in the Barometer, and endeavour to form certain rules for that purpose: so that this seems to be the only thing, which persons otherwise not very skilful in nature may expect from this wonderful machine. And I could wish, that we knew any thing certain about this alone; because it would be of great use in human affairs. But these things, which we would have so conjoined, do not seem to be necessarily connected. For our baroscopes cannot properly, and of themselves, shew any thing except the increase or decrease of the elastic force of the air; but the weather depends on various exhalations, existing at the same time, or together in the air, or not existing or at least not present in the same quantity. It may therefore be, that to diminish the elastic force of the air, and so to make the mercury fall in the Barometer, there must

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be a ſufficient quantity of thicker exhalations in the air, and it muſt thereby become turbid, and the vapours begin to ſubſide, and ſo coaleſce into greater or ſmaller drops of rain, and other *phænomena* follow. But it may as eaſily be, that, tho' the elaſticity of the air is diminiſhed, yet becauſe of a defect of a ſufficient quantity of exhalations therein, no ſenſible change of the weather may happen. The ſame may happen on the contrary, by too great a quantity of exhalations in the air; tho' the riſing of the mercury may moſt evidently ſhew, that the elaſtic force of the air is increaſed. Becauſe therefore theſe things *coincide* as it were by ſome *accident*; no certain prediction can be made of a change of weather, either by the riſing or falling of the Barometer. But we may ſafely enough make a *negative* concluſion from it. For a conſtant conſent of obſervations ſhews, that, if foul weather has followed a deſcent of the mercury, fair will not ſucceed, till the mercury begins to riſe again: or if the aſcent of the mercury has been joined with fair weather, there will not be clouds or rain, till the mercury has begun to fall. And this is often of as great uſe in common life, as if we could always *poſitively* predict, what weather ſhould exactly follow the aſcent or deſcent of the mercury. The fall however of the mercury does *more frequently* predict foul than fair weather; and the riſe more often fair than foul, as is confirmed by manifold experience: and ſo it may be of great uſe, to be able to judge *ſafely* in ſuch occurrences. I have alſo obſerved, that in theſe regions, when the wind blows from any point between the N. and W. rain and foul weather begin, or are continued more often than fair, notwithſtanding the riſing of the mercury in the Barometers.

A letter from the Rev. H. Miles, *D. D. to Mr* Henry Baker, *F. R. S. concerning the difference of the degrees of* Cold *marked by a* Thermometer *kept within doors, or without in the open air.* N°. 484. p. 613 Oct. &c. 1747. *Read* Dec. 10. 1747.

III. 1. I ſend you herewith an extract from my regiſter of the weather, ſhewing the ſtate of my Barometer and Thermometers, for ſome days of laſt week: in which you will obſerve a ſudden change of the temperature of the air, particularly on *Thurſday* morning the 3d inſtant, and by the ſame, you may ſee the little uſe a Thermometer is of, when kept within-doors, to determine the ſtate of the air abroad, as to heat or cold.

I have two Thermometers filled with mercury, and of the ſame conſtruction, made by the late Mr *Siſſon*, in the *Strand*. The one is placed without my chamber-window, in a north-eaſt ſituation, under covert, contrived to admit a free paſſage of the air, but to keep off ſun and rain; the other hangs within the window, about three feet from the former, where the ſun never falls on it: The room is conſtantly occupied, as a bed-chamber, but has had no fire in it this ſeaſon.

It appears by the adjoining table, that on *Tueſday* the 1ſt inſtant, at 8 in the morning, the Thermometer without ſtood at at 17° above 0. or freezing point; that within at 14. At 9 at night, that without was at 0. and that within at 12 above 0. So that in the ſpace of 13^h the former had fallen 17°, the latter but 2. For the other particulars, relating to the Barometer, wind, and weather, I refer to the table.

As the Barometer had been for a good while paſt ſubject to ſudden conſiderable variations, I ſuſpected the ſevere cold on *Wedneſday* night and *Thurſday* morning would not continue long: accordingly, upon my obſerving the Thermometer without at 4 in the morning, I found it at $\frac{0}{9}$ 9° below the freezing point, that within at $\frac{5}{0}$ 5° above the freezing point. But at 8 the ſame morning, I found the Thermometer without at $\frac{3\frac{1}{2}}{0}$ 3°½ above freezing, and that within at 4° above; ſo that in 4 hours time, that without had riſen 13°½, and that within had fallen 1°. This naturally led me to examine what ſigns there might be of a thaw begun, but could find none, in the ſnow (which was 5 inches deep) or in the poſt, on the windows, but within an hour it was viſible enough, and before 10 the houſes dropt. I would obſerve to you, that the wind at 8 in the morning had varied very little, if any, from what it was the night before, *viz.* from the E. but ſoon after it bore to S. E. and S.

May not this ſudden change of the temper of the air be attributed to a ſubterranean heat? And may not the ſhifting of the wind be cauſed, in a great meaſure, by the ſame?

Days	Morning Barom.	Morning Ther.	Evening Barom.	Evening Ther.	December 1747.
1	28 6 3	$\frac{17}{0}$ $\frac{14}{0}$	29 3 0	0 $\frac{12}{0}$	At 8 Morn. Wind high at S. W. much Rain preceding Night. Showery afterward in the Morning, and Wind exceeding high. Sleet at 1½ *p. m.* calmer and clearer ſoon after, Wind N. W. and N began to freeze in the Evening, clear at 9½ *p. m.* when the Evening Account was ſet down.
2	29 5 5	$\frac{0}{3}$ $\frac{2}{0}$	29 4 0	$\frac{0}{2}$ $\frac{5}{0}$	At 8 Morn. cloudy thick Air, hard Froſt; at 4 ſame Morn. very clear, and Glaſſes were at 29 5 0 and $\frac{0}{3}$ and $\frac{4}{0}$ Wind Eaſt, cloudy all Day, at 4½ *p m.* Snow fell, and was deep before 8. Evening Account taken at 9½ *p. m.*
3	29 5 6	$\frac{3\frac{1}{2}}{0}$ $\frac{0}{8}$	29 4 9	$\frac{13\frac{1}{2}}{0}$ $\frac{0}{8}$	At 8 Morn. cloudy, Wind at near E.; blows briſk at 4 the ſame Morn. Glaſſes were 29 6 1 $\frac{0}{9}$ and $\frac{5}{0}$ Rain before 11. Evening Account at 9½ *p. m.*
					Explication. 1 Day Barom. Morn. 28 Inches $\frac{6}{10}$ $\frac{3}{100}$. Ditto Therm. Morn. $\frac{17}{0}$ is 17 Degrees above freezing Point the upper Number is for the Therm. without Doors, the lower for that in my Room, and ſo for the reſt.

2. Chemiſtry

2. Chemistry being the most extensive branch of Experimental Philosophy, hath furnished mankind with the greatest number of curious and useful discoveries; for not only the art of separating metals from their ores, of which metals are formed such variety of useful Instruments, but likewise Cookery, which is so much concerned about the food of mankind during health, and also Pharmacy, which furnishes medicines for the restoring health when lost, the art of dyeing, and many other useful manufactures, all owe their improvements to this science; many of which have been light on unexpectedly by the operator, while he had something else in view: but in many cases the Chemists complain, that having once accidentally light on a curious experiment, upon endeavouring to repeat it, they have never been able to make their process succeed exactly, as it did the first time, notwithstanding that they made use of the same materials, in the same quantity, and conducted the process thro' exactly the same operations. Where then must the cause of the miscarriage lie? Surely in the degree of heat made use of in the two experiments: for, in many common operations, how usual is it for a preparation to be spoiled either by too little, or, most commonly, by too much fire, too long or too short a time applied. In order therefore to prevent these many miscarriages, I would advise the Chemist, in his operations, to observe his clock with as much exactness as the Astronomer doth in his observations; and in order to know to a certainty the very degrees of heat he ever made use of in any process, that so he may be able to repeat and continue the same again in any repetitions of the same experiment, let him have his laboratory furnished with various sorts of Thermometers, proportioned to the degree of heat he intends to make use of. He will find these instruments as useful to him in his processes, as they have proved to the curious Gardener in his stoves, who by them is taught to keep his plants in the same degrees of heat, as are natural to them in their respective climates; which hath been set forth in tables, after a very ingenious manner, by Mr *Sheldrake* of *Norwich*. And besides the enabling him to perform his operations with more exactness, these instruments would save him a deal of fuel; for as liquors, while boiling, are not capable of receiving a greater degree of heat, all fuel which is used more than to keep them in that state is useless; and the like happens in many other cases.

A discourse concerning the usefulness of Thermometers *in Chemical Experiments; and concerning the principles on which the* Thermometers *now in use have been constructed; together with the description and uses of a* Metalline Thermometer, *newly invented by* C. Mortimer, *M. D.* Sec. R. S. *&c.* Ibid. p. 672. *Read* May 8. 1735. *here printed with some alterations.*

These instruments would also be of great service to Maltsters, Brewers, Distillers, and Vinegar-makers; for by Thermometers placed in different parts of the heap of wetted malt, the proper heat for it's sprouting might be determined, and then regulated: the same for the heat of the kiln when the malt is spread on it. By Thermometers the Brewer may ascertain the heat of the water when he pours it upon the malt, the heat of the wort when he sets it to work, and the heat while working: and in the like manner the Distiller and Vinegar-maker, in a word, every artificer, who employs heat in his business, may by these instruments be certain of every degree necessary in each part of his work.

Many experiments ſhew, that all known bodies, whether fluid or ſolid, increaſe their bulk or rarefy by an addition of heat; and, on the contrary, contract or become more denſe by the diminution of heat, which is the preſence of cold: and theſe alterations are always more or leſs ſenſible in proportion to the natural rarity or denſity of the bodies.

The air we live in, as it is the moſt rare and light fluid, ſo are it's alterations the moſt ſenſible; and indeed I know of no experiments which determine how far it is capable of being expanded by heat, or condenſed by cold; only we find that it will make it's way thro' any fluid in which it lay dormant, when it's elaſtic property is rouzed by the approach of ſuch an heat as will make the fluid boil. On the other hand, when compreſſed by a fluid ſo contracted by cold, as to freeze, or become ſolid, it's elaſticity will only bear a certain degree of compreſſion, till the force wherewith it endeavours to reſtore itſelf, exceeds the force by which the parts of the ſolid, that confines it, adhere to each other, and ſo burſts it's priſon; as we often ſee during hard froſts in ice, glaſs, and other hard bodies, whoſe parts cannot ſtretch.

Next to air is *alcohol*, or the higheſt rectified ſpirit of wine: this, water, and all other liquids, are capable of receiving no greater degree of heat than what makes them boil, as was firſt demonſtrated by M. *Amontons*, a Member of the *R. Acad. des Sci.* at *Paris*; but that ingenious inventor of the quickſilver Thermometer Mr *Fahrenheit* hath diſcovered, that when the Barometer marks a greater preſſure of the atmoſphere, the ſame liquor will receive 8 or 9° more of heat than when the Barometer is at the loweſt. From hence the great *Boerhaave* gives the hint, that, from nice experiments being made of the different degrees of heat marked by a Thermometer in boiling water compared with the different heights of the Barometer, and tables formed upon them, a Thermometer applied to boiling water might, at ſea, where the motion of the ſhip hinders obſervations with the Barometer, ſerve to determine the difference of the gravity of the atmoſphere *.

Theſe, and all other liquids, by a certain determinate degree of cold peculiar to each ſort, loſe their fluidity, and freeze, or become ſolid, but not in the ſame order as by heat they boil; for by cold, oil or water is ſooner frozen than ſpirit of wine, tho' ſpirit of wine will boil ſooner than oil or water. All ſolid bodies likewiſe, as minerals, metals, and even ſtones, will become fluid, or melt, at a certain degree of heat peculiar to each ſpecies; and, when thoroughly melted, it is probable they are capable of receiving no higher degree of heat; and, on the abſence of that heat to a certain degree, they all return to their natural ſolid ſtate. Hence we may reaſonably conclude, that ſolidity is the natural ſtate of all bodies; and that ſome are only accidentally fluid, becauſe their conſtitution is ſuch as to melt by thoſe degrees of heat which our atmoſphere is moſt commonly ſubject to. All ſolid bodies are obſerved to contract themſelves into ſmaller dimenſions by cold, and gradually to expand themſelves at the approach of heat, till at laſt, being by heat

* See his *Chemiſtry*, Tom. I. p. 171.

heat forced to the greateſt degree of expanſion, the particles of which they are compoſed loſing their coheſion, they become fluid; but no experiments have yet been made, which determine whether ſolids, expoſed to cold beyond certain degrees, will ceaſe to contract any more.

Dr *Muſſchenbroek*, Prof. Aſtron. at *Utrecht*, hath lately invented a very ingenious inſtrument, which he calls a *Pyrometer*, and which Dr *Deſaguliers* hath made ſome improvements to *; a full deſcription of which he hath given in his *Courſe of Experimental Philoſophy*. †

By this inſtrument the elongation of rods of ſeveral ſorts of metals by the approach of a certain number of flames of a ſpirit lamp, and likewiſe their as ſudden contraction, on the extinguiſhing one or more of thoſe flames, is rendered ſenſible to the eye: which ſufficiently evidences the matter of fact, and puts it beyond all doubt.

From the above-mentioned property of bodies contracting and expanding in cold and heat, have all Thermometers been conſtructed, that have ever been made uſe of in order to obſerve and compare the different degrees of heat, either in our atmoſphere, or in other bodies. The moſt ſimple and moſt ſenſible of any is that aëreal Thermometer deſcribed by Mr *Boyle*, in his New Experiments and Obſervations touching cold, *Lond.* 1683. 4*to*. *p*. 39. It conſiſts of a glaſs bubble, with a very ſlender ſtem not bigger than a raven's quill. The bubble is left full of air, and a few drops of water being conveyed into the ſtem in an erect poſition, will there remain ſuſpended to a certain height; but, by the leaſt addition of heat, the air in the bubble expanding will puſh the water up higher; or, by the approach of cold, the air contracting, the water will fall lower in the ſtem. This inſtrument may be of uſe in ſmall degrees of heat, and in cold, till the water begins to freeze, when it becomes uſeleſs.

The next in order of ſenſibility is that firſt invented by *Cornelius Drebbelius* of *Alcmar*, and improved by *Boerhaave* ‖. It conſiſts of an hollow glaſs *lens* joined to a ſtem of a larger ſize than in the preceding, and a baſon into which the end of the ſtem is inverted. The air in the *lens* muſt be ſo much rarefied, that the ſtem being inverted into a tinged liquor in the baſon, the liquor will riſe up ſome way in the ſtem; then, by the application of heat to the *lens*, the liquor in the ſtem will be puſhed down, and by cold the liquor will riſe up. This inſtrument will give notice of the ſmalleſt changes in the air; but it cannot be immerſed into any liquid for chemical experiments, unleſs the ſtem were made much longer, and bent downward in form of a ſyphon: but even then it would be very unhandy, and, like the preceding, it would never ſerve for any degree below what would freeze the liquor made uſe of, nor for any above what would force out the confined air through the liquor in the baſon. Beſides, both theſe inſtruments, being ſubject to the preſſure of the atmo-

* This inſtrument hath ſince been greatly improved by Mr *John Ellicot, F. R. S.*

† *Vol.* I. *p*. 421. *&c.*

‖ See his *Chemiſtry*, Tom. I. p. 152, 153.

ſphere,

ſphere, are not proper, without comparing the Barometer at the ſame time, to determine the degrees of heat at a great diſtance of time between each experiment.

The moſt uſual ſort of Thermometers is that deſcribed in the account of the experiments by the *Academy* of *Cemento*; which being the common ones, made of ſpirit of wine tinged, it is needleſs to deſcribe. The bounds of the degrees of heat which theſe will meaſure, and which is commonly called the range of the inſtrument, are from the degree which freezes ſpirit of wine, up to that which makes it boil. The ſpirit Thermometers, commonly made here in *London*, are ſo graduated, that when the ſpirit is rarefied to the degree that the moſt ſultry ſunſhine commonly known in our climate of $51^{\circ}\frac{1}{2}$ N. Lat. can raiſe it, there is placed the mark o. or degree of no cold. Some few are marked 10 or 20 above this, if they are deſigned to be uſed in hotter climates; but all are graduated downwards from this: ſo that the 45°. is the point of temperate, and 65°. is the point of freezing, and 100°. is placed juſt above the ball. But the moſt accurate ſpirit Thermometers are thoſe lately made by M. *Reaumur*, he hath taken a great deal of pains, and uſed great exactneſs, in fixing the certain points of freezing of water, of temperate air, and boiling water. He determines the freezing point, by leaving his Thermometer a conſiderable time in water, into which is put a good deal of ice, at a time when the water would not freeze of itſelf; and this he marks o. or the degree of no heat; and his ſcale is marked with numbers running downwards from o. meaſuring the degrees of cold, and upwards meaſuring the degrees of heat: at $10\frac{1}{4}$ upwards he marks the point of temperate, which he determines by placing his inſtrument in a ſubterranean cavern, which is neither affected by froſt nor ſunſhine, but is obſerved to keep an equable temperature all the year round; ſuch as deep cellars and wine vaults commonly do. In boiling water he finds that his Thermometer riſes to his 80th diviſion, * or 80°, which are formed by dividing the ſpirit when condenſed to the freezing point, into 1000 equal parts; ſo that, with the heat which makes water boil, the ſpirit is expanded only $\frac{80}{1000}$, more than with the cold which freezes water.

Theſe ſpirit Thermometers are of uſe in experiments where ſomewhat greater cold than the freezing of water is required; but they can never be of uſe in any degrees of heat beyond the boiling of the ſpirit itſelf; becauſe it then becomes volatile, or riſes up in ſteam, and not only expands no more, but likewiſe the quantity is diminiſhed by the particles which fly up from the ſurface of the liquor, and are ſuſpended in the top of the tube.

* But, with ſubmiſſion to ſo great a man, I cannot apprehend that his Thermometers, when the ſpirits are raiſed up to 80 do mark any greater degree of heat than their own ſpecific boiling heat, which, if they are *alcohol*, or the moſt rectified ſpirits, anſwer to 174. of *Fahrenheit*'s ſcale; if of the ſtrength of common brandy to 190.

Many

Many have filled their Thermometers with various ſorts of oils *: theſe indeed will meaſure many degrees above the boiling of water, till they boil themſelves, and then they have the ſame defect as the ſpirit ones juſt mentioned, which is the liquor loſing of it's bulk by evaporation; and they congeal much ſooner than water, and ſo are uſeleſs in meaſuring any degrees of cold.

The moſt uſeful inſtruments, as they comprehend the largeſt range, are the mercurial Thermometers, which were brought into uſe by that ingenious artificer *Fahrenheit*. But, to do juſtice to a moſt worthy Member of the *R. Soc.* Dr *Halley*, he firſt gave the hint, and even propoſed the making Thermometers of quickſilver long before *Fahrenheit*'s time. However, *Fahrenheit* deſerves thanks from the world for having brought theſe inſtruments into uſe, becauſe they will meaſure the greateſt degrees of cold yet known; for no cold hitherto obſerved hath been able to freeze or render *mercury* ſolid: and in meaſuring heat, they go far beyond boiling water, even beyond the melting of tin or lead. *Fahrenheit* begins his ſcale from 0. the point to which the mercury hath been obſerved to fall by the greateſt cold in *Yſland*; and computes, that the mercury then † occupies 11124 parts. This is his point of no heat. Then reckoning upwards from this, he finds that when the mercury is rarefied only 32 parts or degrees more, common water juſt begins to freeze: in a temperate air it will riſe to about 60. The moſt ſultry ſunſhine ſeldom raiſes it to 90; the heat of an animal body to 96; the boiling of *alcohol* to 174; the boiling of water to 212; and before the mercury itſelf boils, it will riſe to 600.

I cannot here forbear giving an abſtract of a very curious and ſurpriſing experiment of *Fahrenheit*'s, concerning the artificial production of cold, as it is related by *Boerhaave* in his *Chemiſtry*. *Fahrenheit* had a mercurial Thermometer made with ſo long a ſtem, that he could carry down the ſcale 76 parts or degrees below 0. With this inſtrument he found, that cold might be produced by gradually pouring ſpirit of nitre upon powdered ice, till the *mercury* would ſubſide to 40° below 0. that is 72° ‖ lower than the cold which freezes common water. *Boerhave*, in his *Chemiſtry*, *** mentions a very pretty way of determining the freezing point: he adviſes to hang the Thermometer free in the open air, not againſt any wall or building; and near it you muſt hang a piece of very fine linen or muſlin juſt dipped in clean water: when this begins to grow ſtiff you will find the *mercury* ſtand at about the 33d degree; and it will alſo ſtand at the ſame height when an hoar froſt appears upon the ground; which he looks upon as a certain ſign of the beginning of freezing.

* See Dr *Martin*'s Eſſays *Med. & Philoſ.* p. 225. Sir *I. Newton*'s Thermometer is made of linſeed Oil. See his Scale of Heat, Vol. IV. P. ii. §. 1.

† See *Boerh. Chem.* Tom. I. *p.* 174.

‖ But what is this to the marvellous natural cold of *Siberia*, 120°. below 0? See the preface to *Gmelin*'s Flora Siberica, *Petrop.* 1747. 4*to*.

*** Tom. I. p. 161.

Having

Having thus given an account of the feveral forts of Thermometers hitherto ufed, and what degrees of heat they are proper to meafure, we find none of them capable of meafuring the greater degrees of heat, which are the moft commonly made ufe of by the Chemifts in many of their operations. Befides, all the above inftruments, being made of glafs, are eafily broken by accidents, and as liable to crack of themfelves, by being taken out of a great heat, and too fuddenly expofed to cold. I therefore confidered whether the above-mentioned property of folids, and efpecially of metals contracting with cold, and exapnding with heat, might not be applied to the conftruction of an inftrument capable of meafuring all degrees even of the greateft cold, as well as the greateft heat, to the melting copper or iron, which require more heat than any other metals to melt them. Altho' the alterations in metals are but fmall, in refpect of thofe in fpirits, or even *mercury*, yet it being found, that iron, *e. g.* becomes [illegible] longer * when red-hot, than when of its natural temperature; and Dr *Derham*, in his laft paper read before the *Royal Society* concerning the vibration of *pendulums*, fays, that a rod 39.$\frac{126}{1000}$ inches long, becomes $\frac{1}{10}$ inch longer than its natural dimenfions in temperate air, by being expofed to heat equal to that of an human body; $\frac{2}{100}$ inch longer in hot funfhine; that it was $\frac{2}{10}$ or $\frac{1}{5}$ inch longer than its natural ftate, by being heated in a flaming heat; that it became $\frac{7}{100}$ fhorter than its natural length by being quenched in cold water; and ftill $\frac{3}{100}$ fhorter, by being put into a mixture of falt and fnow. From which experiments one may conclude, that from *Fahrenheit*'s cold of 40 below 0. to the greateft heat iron can bear without melting, a rod of 3 feet long will have about $\frac{1}{4}$ inch increafe; which increafe of length will be range enough to make all the intermediate degrees obfervable upon an inftrument.

Fig. 18. Plate IV

Suppofe in *Fig.* 18 *AB* a rod of iron at its natural length by the heat of the atmofphere, placed upright upon one end; upon the point of that refts a bar *CD* moveable on an axis at *a*; and that, by making a fire about the end *B* of the rod, till it is juft ready to melt, the rod will increafe in length *Ab*, and confequently pufh the bar into the fituation *cd*. Now it is obvious to any one who underftands ever fo little of Mechanics, that tho' the elongation of the rod *Ab* be even fcarce perceptible to the eye, yet if upon the bar *CD* the diftance *aA* from the axis to the place where the rod *BA* pufhes againft it be very fmall, and the other part of the bar *aD* very long, the arch *Dd* may be increafed at pleafure, fo as to bear to be divided into any number of divifions that fhall be found neceffary: for the arch *Dd* will always be to the arch *Cc* in the fame proportion as the diftance *Da* is to *aC*; and likewife the chords of thefe arches *Dd* and *Ab* will be in the fame proportion; γ, δ, is the fitutation of the lever on the level; and if it

* Vide *Sturm. Coll.*

be

be found inconvenient to make the arm *a D* fo long, as to make very minute alterations in the length of the rod *AB* eafily obfervable, this inconveniency may be readily removed by having a fecond bar *EF*, turning on the axis *g*, whofe arm *g E* bearing up againft the extremity *D* of the firft bar or lever, will rife with it, or be preffed down by it; and the other arm *g F* being lengthened at pleafure, the arch *Ff* will be as large as you find convenient; or even a third and fourth lever may be added.

When I firft defigned to have an inftrument conftructed anfwering to the foregoing principles of *Fig.* 18. I drew a figure of it, wherein I propofed the lever *AD* to have terminated in two arches of circles made out of one piece of brafs; the fmaller arch formed on the radius *a A* to be loaded with a quantity of metal fufficient to overcome all the friction of the feveral parts, fo as to prefs down with a confiderable weight, and always to reft upon the point *A* of the upright rod *AB*; at *a* the axis, on which they were to turn; and the larger arch formed on the radius *a D*, was propofed to be a fextant, the outward edge of which was to be toothed, which teeth were to play into the teeth of a fmall brafs wheel carrying a fteel *index* like the minute-hand of a clock, which fmall wheel with the hand was to make one revolution nearly by the utmoft rife and fall of the fextant at *D*; or, inftead of teeth, I propofed a piece of a watch-chain to be faftened to the upper limb of the fextant, and fo to be brought downwards, and paffing nearly round the fmall wheel in one groove, to be faftened to it: in another groove in this fmall wheel was another piece of watch-chain to be faftened, which, being paffed contrarywife round the faid wheel, was to have a weight hung to it that would be a counterpoife to the fextant; but, upon confulting my two ingenious friends Mr *G. Graham* and Mr *John Ellicot*, they each of them perfuaded me to lay afide that more complexed conftruction, and to have the inftrument made in the plain and fimple manner in which Mr *Jackfon* executed it for me in the year 1736, as is reprefented in *Fig.* 19, and 20.

The defcription of the inftrument. Fig. 19.

Fig. 19. *A B* a round rod of fteel or brafs $\frac{3}{4}$ of an inch thick, and 3 feet 1 inch long: when the rod is of brafs 3 feet long, the point *A* muft be of fteel 1 inch long, to prevent its wearing away, or lofing its point; which conical point is made to fcrew on and off.

I had the firft rods made 1 $\frac{1}{4}$ inch thick at *B*, and of the fame thicknefs 6 inches up; but I found inconveniences from that form, and that a rod all of a fize was better.

CD, *c d*, are two iron fupporters, joined by a flat crofs bar at bottom *D d* two inches long, in the middle of which is a point $\frac{1}{8}$ inch high under *B*, which goes into an hole at the bottom of the rod *B*, and ferves to keep the rod in its place at bottom, as the crofs bar ** having an hole in it, thro' which the rod paffes, does in the middle or about $\frac{2}{3}$ up the fupporters, and the point *A* goes into a fmall hole in the under fide ſide of the lever; all which keep the upright rod firm and ſteady in its place. The iron ſupporters are flat, or parallel to the front of the machine from *C* to *X* and *c* to *x*, where they are twiſted half-round, ſo that the lower parts *XD*, *x d* ſtand at right angles with the upper parts. This contrivance gives the freer acceſs to the rod for the ſand or fluid into which the machine is ſet to meaſure the heat of it, the ſupporters ſtanding 2 inches aſunder at *Dd*; and that the degrees of heat may be compared uniformly in different experiments, the bottom of the rod muſt always be immerſed to the ſame height in the matter to be examined; and therefore I make a mark, a ſmall furrow † quite round the rod, 1 $\frac{1}{4}$ inch from the bottom *B*. For the deeper the rod is immerſed into any matter, it will be lengthened the more by the ſame degree of heat.

E F, the lever, which turns upon an axis *G*. At *F* is faſtened a ſtring, which, paſſing twice round the ſmall pulley *H*, has a weight *I*, hanging to the other end of it, of about half a pound, being enough to keep the ſtring always ſtretched. At the other end *E* of the lever is hung another weight *L*, which muſt be heavy enough not only to counter-balance the longer arm *G F*, but preſs down upon the point *A* with a weight ſufficient to keep it ſteady.

MNO, is the back part of the plate, like the dial-plate of a clock made of braſs. See the front of it at *Fig.* 20,

The pulley *H* turns upon an axis *C* in *Fig.* 3. which goes thro' the plate, and on the other ſide or front of the dial-plate carries a hand or index *A B* in *Fig.* 20.

N. B. *G* being the *Fulcrum* of the lever, the diſtance *G A* being very ſmall, and the diſtance *G F* being very great, the ſmalleſt motion at *A* will produce a very great one at *F*, and therefore the index will turn very ſenſibly upon the plate.

The proportions of the rod and lever are diſcretionary; my rods both of ſteel and braſs are 3 Feet long in one ſolid piece, but they have each a point or cone of ſteel 1 inch high, that ſcrews upon the top at *A*. The lever has 4 inches from *E* to *A*, 1 $\frac{1}{5}$ inch from *A* to *G*, and 12 inches from *G* to *F*; the diſtance of *G* above *c* is 1 $\frac{1}{2}$ inch, the braſs pulley *H* is $\frac{1}{4}$ inch diameter; all the other parts of the machine are of oak. The main ſupport or pillar *PQ* is 1 inch ſquare, 2 $\frac{1}{2}$ feet high, and at bottom is let thro' a groove at *Q* made in a great heavy block or pedeſtal of wood *R S*. In this groove the pillar may be raiſed higher or lower, in order to adjuſt the height of the pillar to the ſituation, which the bottom of the rod *A B* may require in different experiments; and it is to be fixed in that place by a ſcrew at *T*, which goes thro' the front of the block, and preſſes againſt the bottom of the pillar.

Fig. 20. repreſents the *dial-plate*, or front of the plate marked *M NO*: in *Fig.* 19. it is a plate of braſs, with ſtrong paper glued upon it and may be of what ſize you pleaſe; mine is 11 inches over.

AB

AB is the hand or *index*, which flips on very ftiff upon the axis *C*, that carries the pulley *H* in *Fig.* 19. The outer circle is to be left wide enough to contain the chemical characters or marks which are to be made upon it, the arch *DE* contains the divifions of *Fahrenheit's* mercurial Thermometer; the arch *FG* thofe of *Reaumur*, or the fpirit of wine Thermometer.

In order to adjuft this inftrument for ufe I place the bottom of the rod *B* in *Fig.* 19. immerfed up to the mark † in cold river or rain-water, in a veffel proper to be fet over the fire; and when it has boiled for ¼ of an hour, I turn the index *AB* in *Fig.* 20. till it ftands in the horizontal pofition, as at *B*, being the point of boiling-hot water, and which anfwers to divifion 212 on *Fahrenheit's* arch. I then take it out of the water, and dry it, by holding it a little over the fire: and now great care muft be taken, that nothing alters the fituation of the index upon the axis; even a nut to fcrew on upon the axis at *C* may be the beft to keep it fixed. If the inftrument be left to cool in the air, the index will fall below *B*, fhewing the degrees of cold, or lefs heat than boiling water; and if put into melting tin, lead, *&c.* it will fhew the degrees of heat above boiling water. A brafs rod will ferve for an inftrument to meafure the greateft degrees of cold, and all the degrees of heat, to the melting of filver or gold; but if you have a mind to make one to meafure greater degrees of heat, the rod muft be of fteel, or the fineft iron. A rod of brafs, according to Dr *Muffchenbroeck's* experiments, *l. c.* was found to lengthen 377, when one of iron lengthened only 230 parts. An iron rod, being regulated by boiling water, as above directed, will meafure not only the heat of melted tin and lead, but of filver, gold, and copper, and will even fhew the degree when iron itfelf begins to melt, which will be the greateft degree of elongation of the rod juft before the bottom of it runs; and I imagine, that an inftrument may be conftructed with fupporters, and a rod made of tobacco-pipe-clay, which, being regulated by boiling mercury (for it muft never touch water,) may be adapted to meafure ftill greater degrees of heat, till the materials themfelves melt into glafs.

I fhould advife, that not only the fcale of this fort of Thermometer, but likewife of all others, be determined by experiments, without regarding any equality as to meafure between the divifions, and that in every individual that fhall be made; for a difference in the length and thicknefs of the rods in this fort will make a difference in the fcale, as much or more than the inequality in the cavity of the ftem, or glafs tube of other Thermometers, which can never be juft, if applied to a fcale whofe divifions are made equal; unlefs the cavity of the ftem, be perfectly equal; which it is impoffible for any workman to undertake to do, and which is very feldom, if ever, hit on by chance. Therefore in thefe inftruments, let the point *B* in *Fig.* 20. or the horizontal pofition of the index, be the fituation of the index when the rod has ftood ¼ of an hour in boiling water; there mark ▽ *boiling* on the outer circle; on

Fahrenheit's arch mark 212. then ſet your machine up to the mark ♃ into melting tin, which is the metal that melts eaſieſt. When the rod is arrived to its greateſt elongation in that metal, inſcribe the character ♃ on the outer circle; do the like with lead, and ſet the character ♄ at it. At the boiling of mercury put the mark ☿, and on *Fahrenheit*'s arch mark 600. the utmoſt extent his mercurial inſtruments can meaſure: Then proceed to the melting of ſilver, and ſet the mark ☽; at the melting of gold place the mark ☉; at the melting of copper place the mark ♀; at the melting of iron place the mark ♂, the moſt difficult to melt of all metals.

As the diviſions pointed out by the index will be different with rods of different metals or ſubſtances, you may make different circles upon the plate for the range of the different rods, and mark them; the iron rod, the braſs rod, the clay rod; and ſet the ſeveral marks above ſpecified upon each circle apart; or you may, to avoid confuſion, have a different inſtrument for each kind of rod.

Being obliged to take down my *athanor* and wind-furnace, upon removing twice to different houſes, and not having rebuilt them where I now live, I have not had opportunities yet of fixing the ſcale of my own inſtrument, which was one reaſon why I did not publiſh an account of my invention ſooner; for I hope hereafter to be able to compare the degrees of heat neceſſary for the melting of each metal, and to determine the queſtion whether metals in the higheſt degree of fuſion, are ſuſceptible of greater degrees of heat by increaſing the fire, as water thoroughly boiling can never be made hotter; nor did I intend to have publiſhed any deſcription of this inſtrument till I had compleated tables of the degrees of cold and heat, from *Fahrenheit*'s experiment of cold produced by art 40 degrees below 0 to the heat of melting iron.

According to *Fahrenheit*'s ſcale, the heat of the ſtrongeſt ſunſhine is at about 80. Spirit of wine boils at 176. Water at 212. the *lixivium* of ſalt of tartar at 240. Spirit of nitre at 242. Oil of vitriol at 546. Quickſilver at 600 *.

As all chemical digeſtions, where an equable heat is to be continued for ſome time together, will come in between hot ſunſhine and the boiling of quickſilver, a Thermoſcope of that range will be ſufficient for common uſes; and therefore one fitted with a braſs rod will anſwer theſe purpoſes.

In large furnaces for running down ores, or melting great quantities of metal together, it is not poſſible to place ſuch an inſtrument; but then in lead and tin there may be ſmall outlets contrived, into which ſome of the melted metal may be permitted to flow, and remain in contact with the ſame body of metal within, where the inſtrument may be placed; and for placing a Thermoſcope in iron, copper, or glaſs furnaces,

* *See* Auguſtin. Griſchow *Thermometria comparata accuratius, & harmonica.* Berolini 1740. 4to. p. 10.

naces, there may be a place contrived, which ſhall not open into the furnace, but have the thickneſs of a ſtone or brick left between, upon which the inſtrument may be placed; and tho' in ſuch a ſituation it will not meaſure the actual heat within the furnace, it will always give the relative or comparative heat in the like circumſtances at different times, and ſo ſhew us how to regulate the heat within.

Although a Chemiſt ſhall have one of theſe inſtruments to meaſure the heat, he ſhall have uſed in any experiment, and have noted down the ſeveral degrees made uſe of, and the time each laſted, he ſtill labours under another difficulty, which is the not being able to command any required heat, and that it ſhall laſt a certain required time, unleſs it be below that of boiling water, which may be procured and continued by various contrivances of lamps, either of ſpirits, or of oil; but how to continue a fire for 12 or 24 hours together, without attendance, which ſhall continually keep quickſilver boiling, lead in fuſion, or may be let down ſo low as not to exceed the heat of ſunſhine, and then be raiſed again, and that without letting out the fire, or moving the veſſels, may ſeem almoſt impracticable; but by an improvement of the furnace the antient Chemiſts called their *athanor*, I hope to ſucceed in it, which may be the ſubject of another paper.

The Rev. *Stephen Hales*, D. D. upon hearing the minutes of my paper delivered in to the *R. Soc.* on *May* 8. 1735. read upon the *Thurſday* following, deſired me to lend him the original for ſome days, telling me he had ſome thoughts of making a Thermoſcope with a rod of lead. After a few days he returned me my paper, with the following obliging letter, and kind remarks.

SIR,

I HAVE read over your *Thermometrical Tract* with ſatisfaction, and believe it will be of good uſe. The want of aſcertaining the degrees of heat and cold is a great and important *deſideratum* in Experimental Philoſophy.

What I intended to do was only this, *viz.* to get a leaden wire, of ſuch a ſize and ſtrength as to bear it's own weight, to have it as long as the longeſt gun-barrel I could procure, and to have it ſuſtain a lever as you have done; then to pour boiling water into the barrel, for a long time, till the lever riſes no more; the water to have vent at the bottom, yet ſo as to have the gun-barrel always full of water; the breech-pin to be out, and the leaden rod to reſt on a piece of wood ſet upright, according to the courſe of it's fibres, not ſideways.

To give at the ſame time to a mercurial Thermometer the heat of boiling water. Then to take the freezing point of the leaden and mercurial Thermometers; and afterwards to graduate all the intermediate degrees, from the mercurial Thermometer upon the leaden Thermometer, as they occur. Thus a ſtandard Thermometer may be made to graduate

graduate others by; but I will not now set about it, since you have undertaken the subject.

His *Remarks* on the foregoing Paper.

1. *Thermometers* must be of excellent use in garden-stoves; but foreign plants must not be kept in an equal degree of heat in stoves, to that of their native country; *viz.* because they cannot bear as great a heat in a confined close air, as in an open free air. I have been told of coffee-trees being killed here in *England* by this mistake: such plants must doubtless be kept warm *, but not so warm as in their native country.

2. [*All solid bodies are observed to contract with cold*] I have found that wood does not contract or dilate lengthways with heat or cold. I am told that Mr *G. Graham* [is about making] this experiment, as I am also, in order to regulate *pendulums.*

3. I fear that *Boerhaave*'s wet linen, which is so thin, may begin to freeze before all the *mercury* or spirit of wine in the ball of the Thermometer has the same degree of cold: though hanging there long before and after freezing will bring it pretty near.

4. [*A rod* of iron 3 *feet long will have about* $\frac{1}{4}$ *inch increase*] or $\frac{1}{144}$ part.

A Letter from Maurice Johnson, *Esq;* Pres. *of the* Gentlemens Society *at* Spalding, *to* James Jurin, *M. D. F. R. Coll. Phys.* London, *and F. R. S. concerning a* Metalline Thermometer, *in the* Museum *of that* Society. No. 485. p. 128. Jan. 1747-8, *dated* Spalding, Jan. 16. 1747-8. *Read* Jan. 28. 1747.

3. As I know it must give you pleasure, and, being by you (as I desire it may be at their next meeting) communicated to the *R. Soc.* may be of some credit to the memory of the inventor, the late Mr *Sam. Frotheringham*, a Grazier at *Holbeach* in *Elloe Holland*, *Lincolnshire*, and of some profit to the maker, give me leave to acquaint you, that he (Mr *John Ingram*, of this place, Watch-maker and White-Smith, whose father, originally a Black-Smith at *Cowbitt*, and inventor of the Machine for cutting watch-wheels, was also a most accurate artificer) having made, and, at my instance some time since, fixed up in our *Musæum*, a metal Thermometer, which we, on experience and observation, found to answer and go truly, I proposed to the company, at our last meeting in *Dec.* that our *Society* should purchase it of him, I send you his description thereof, as entered from his mouth in our minutes, which I trust may be agreeable to you, and the worthy members of that illustrious body, for which we here have the highest honour: and though Mr *Beridge* (some time a Watch-maker at *Boston*) under the same inventor's direction, made and carried up to town a machine somewhat of this sort, which several of your members may have seen, yet I trust this account may not be unacceptable.

* I should think it best to lessen the heat in stoves towards the night, and so to keep the plants exposed to less degrees of heat a-nights than a-days, nay to vary the heat daily, or to endeavour by art to procure different degrees of heat, agreeable to the natural vicissitudes of the climate the plants come from, having regard both to the seasons of the year, and the state of flowering or fructification of the plants; so that the best way of ranging plants in green-houses or stoves is according to the climates they come from; for which Mr *Sheldrake*'s Tables above-mentioned must be of excellent use. *C. M.*

It

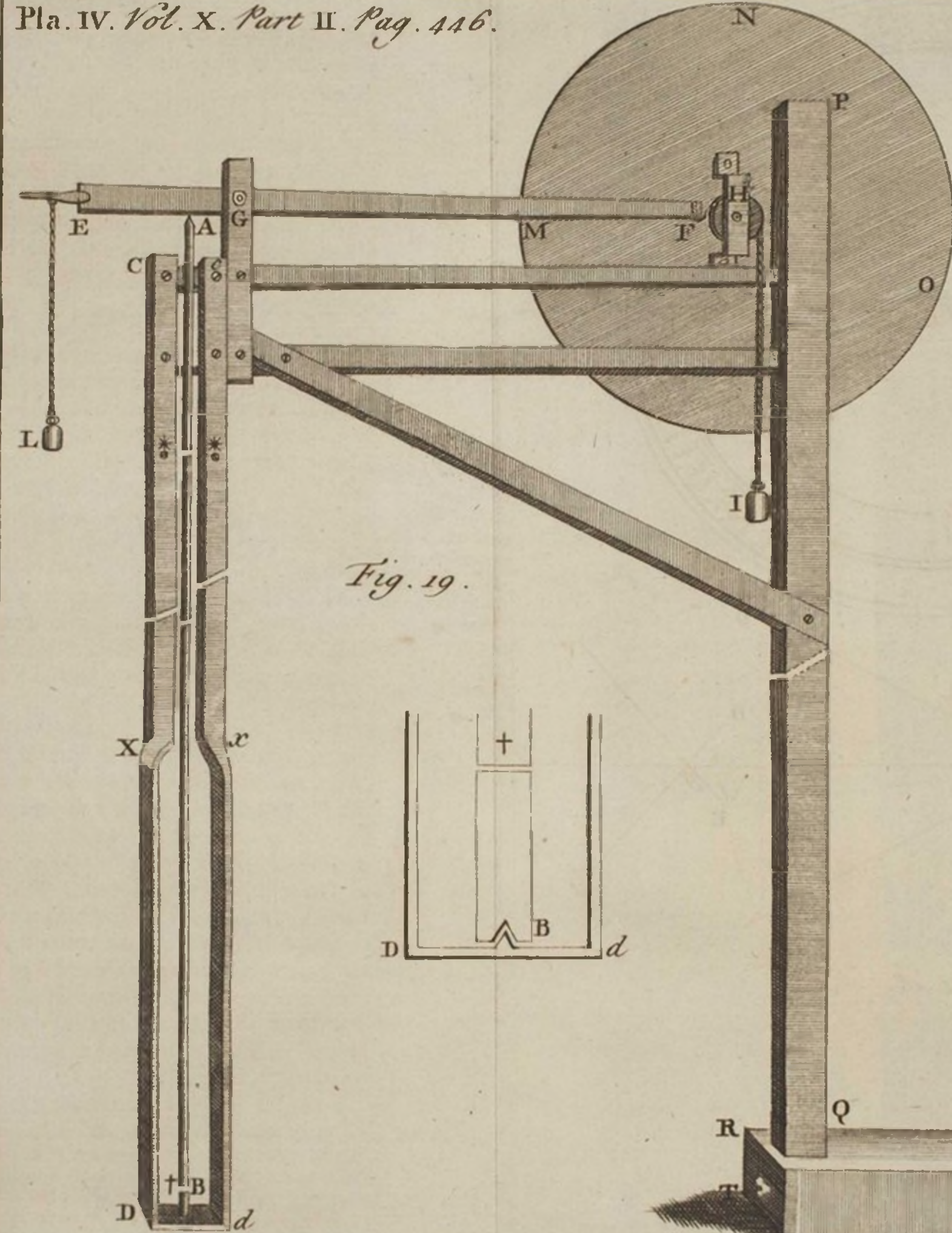


Fig. 19.

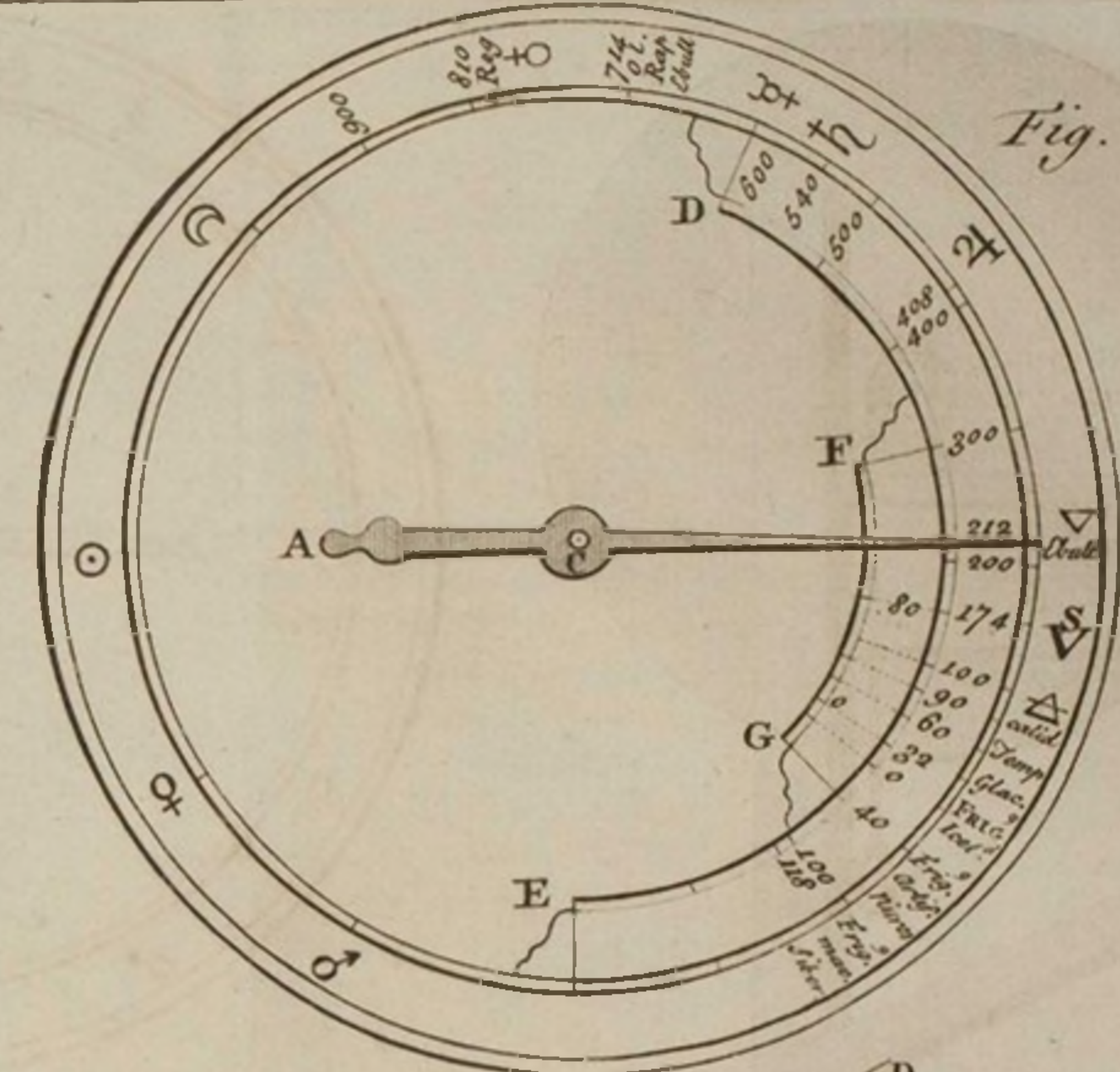


Fig. 20.

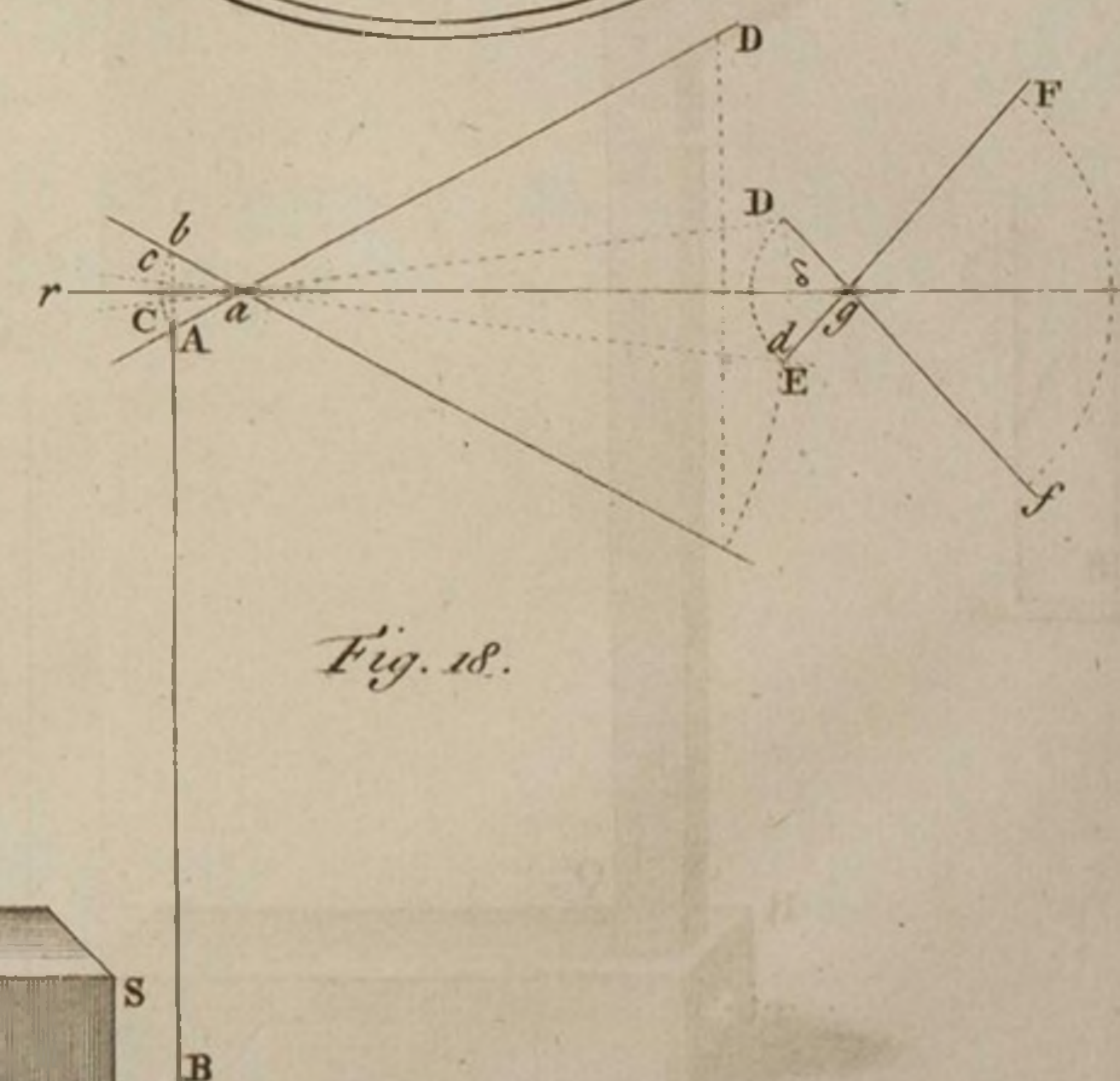


Fig. 18.

It is composed of an upright staff or bar *a* of the best iron, 4 feet long, and 1 inch broad, having a polished brass bar of the same length and width screw'd to it before it, with four steel screws, and being also capped *b* with steel, and thereon a lever *c* moving upon a stud of steel, which communicates with another less lever *d* (also upon a stud) having a chain *e* at the end of it, which laps round an *axis f*, whereto the index is fixed, which shews the degrees marked on a semicircular arch *g*: Under the steel screw-heads there are small slits in the brass bar (except the lowermost which is fixed) which admit of its expanding, whereby it protrudes and operates on the first-mentioned lever, which being raised moves the less lever, and thereby draws the chain which turns the axis affixed to the index, which shews the degree of warmth of the weather marked on the semicircular arch. At *h* is a screw thro' two studs, to draw the great lever backwards and forwards, as occasion may be; *i* is a counter-balance to the small lever to draw the hand back when the brass bar shrinks *.

A description of the Metal Thermometer *in the* Museum *of the* Gentlemens Society *at* Spalding *in* Lincolnshire. Fig. 21.

4. It has been often complained of, that the theories we have of the air and weather, are so imperfect, and that an unfinished one, of the hon. Mr *Boyle*, published since his death, should be the best we yet have; perhaps there is equal reason for complaint, that the Thermometer first introduced into use in *England* by the same excellent philosopher, should be so little improved for more than half a century, and be made to serve a not much better purpose than that of amusement.

A Letter from the Rev. Henry Miles, *D.D. & F. R. S. to* M. Folkes, *Esq; Pr. R. S. concerning* Thermometers, *and some observations of the* Weather. N° 491. p. 1. Jan. &c. 1749. *Read* Jan. 12. 1748.

For some years past, several eminent philosophers at home and abroad, have applied themselves to bring this instrument to greater perfection, and to render it more useful; and among them the great Sir *I. Newton* did not think it unworthy his attention. It seems now to be pretty generally agreed, that Thermometers made with quicksilver are preferable to all others; that *extravagant fluid*, as Mr *Boyle* calls it, being most easily susceptible both of heat and cold, and, when well purified, not liable to be obstructed in its motion.

I had, by some years experience, found both the excellence of them, and the necessity of keeping them in the open shaded air, before I met with the learned and curious essays medical and philosophical of Dr *George Martine*, in which he so much recommends their use; and it

* In the beginning of the year 1735. I invented, and caused to be constructed, a Thermometer on the same principles as this: I found that a rod of brass 3 feet long was sensibly affected by the changes of heat of the weather, having one exposed in my garden during the hard frost of the winter 1739 and 40. And my instrument was very sensible with either a brass rod or an iron rod, when the bottom of it was placed in a sand-heat for chemical uses; but I shall refer the reader to the preceding paper, wherein I have given a full description of my invention, and the reasons why I did not publish it before; tho' I have shewn the instrument to scores of people ever since *May* 1735. and sent a description and draught of it to M. *Buffon*, superintendant of the *Royal Physick Garden* at *Paris* in the year 1744. in order for his laying it before the *Royal Academy* of *Sciences* at *Paris*.

C. M.

was

was no ſmall ſatisfaction to me, to find that Gentleman had proved, by experiments, that quickſilver both heats and cools faſter than any liquor we know; faſter, I am ſure (ſays he), than water, oil, or even ſpirit of wine, and never freezes, by any degree of cold hitherto obſerved.

Might I be indulged the liberty, I would embrace this opportunity of inviting ſuch gentlemen, as attend to this branch of Natural Philoſophy, to conſider what Dr *Martine* has ſaid to recommend the uſe of Thermometers made with quickſilver, and to place them in the open air, guarded from the ſun's rays; which, ſome obſervations I made *, may ſerve to ſhew the neceſſity of; eſpecially a more remarkable one, lately made, which I ſhall ſubjoin hereto.

There is another particular of great importance, which I fear we may rather wiſh than hope to ſee made a general practice, recommended by the ſame gentleman; that is, the conſtructing all Thermometers with one ſcale. But if this may not be expected, certainly no Thermometer ſhould be made without adjuſting two determinate and ſufficiently diſtant points of heat and cold; ſuch, for inſtance, as thoſe of boiling water, and of water juſt beginning to freeze, and the intervening ſpace divided into a convenient number of equal degrees. By this means we ſhould be able to know what is meant by any ſpecified degrees of heat or cold, and a compariſon might be eaſily made of the ſtate of the air in diſtant places, provided the inſtruments were accurately made.

Dr *Martine* ſeems to think, that the degree of cold which cauſeth water to begin to freeze, is nearly equal in all places, whatever little variation there may be found in that degree of heat which cauſeth water to boil, at different times, from the different weight of the atmoſphere: So that we may look upon theſe two points as ſufficiently determinate.

An account of an obſervation I made of the ſudden change of the temperature of the air on Tueſday *the 22d of* Nov. *laſt, with the ſtate of the Barometer, and other circumſtances.*

On *Monday* the 21ſt in the evening the ſky very clear, the wind N. and a ſmart froſt, the Barometer was 30 inches $\frac{2}{10}\,\frac{0}{100}$. At near 9^h the Thermometer without my window at 7 *gr.* below 0, or freezing point. The Thermometer within, of the ſame conſtruction with it, and not a yard from it, (the room having had no fire in it this ſeaſon) at 5 *gr.* nearly above 0.

On *Tueſday* morning, at 4^h 20′, when I got up, I found the Barometer at 30. $\frac{2}{10}\,\frac{6}{100}$ inch; the Thermometer without at 14 *gr.* $\frac{1}{2}$ below 0; that within at 2 *gr.* $\frac{1}{2}$ above 0. I was much ſurprized hereat, and before I had finiſhed my entry, I returned to renew my obſervation, fearing I might have made a miſtake, but found I had not: At 7^h 40′ the ſame morning, upon opening my ſtudy-window, I obſerved the ſky to look red and lowering; this induced me to go up to examine my glaſſes, ſuſpecting there might be a change, and found the Barometer fallen to 30. $\frac{0}{10}\,\frac{2}{100}$, the Thermometer without riſen to 5 *gr.* below freezing point, but that within fallen to 1 *gr.* above; the wind getting

* See Art. 3. of this Section.

about

Pla.V. Vol.X. Part II. Pag. 448.

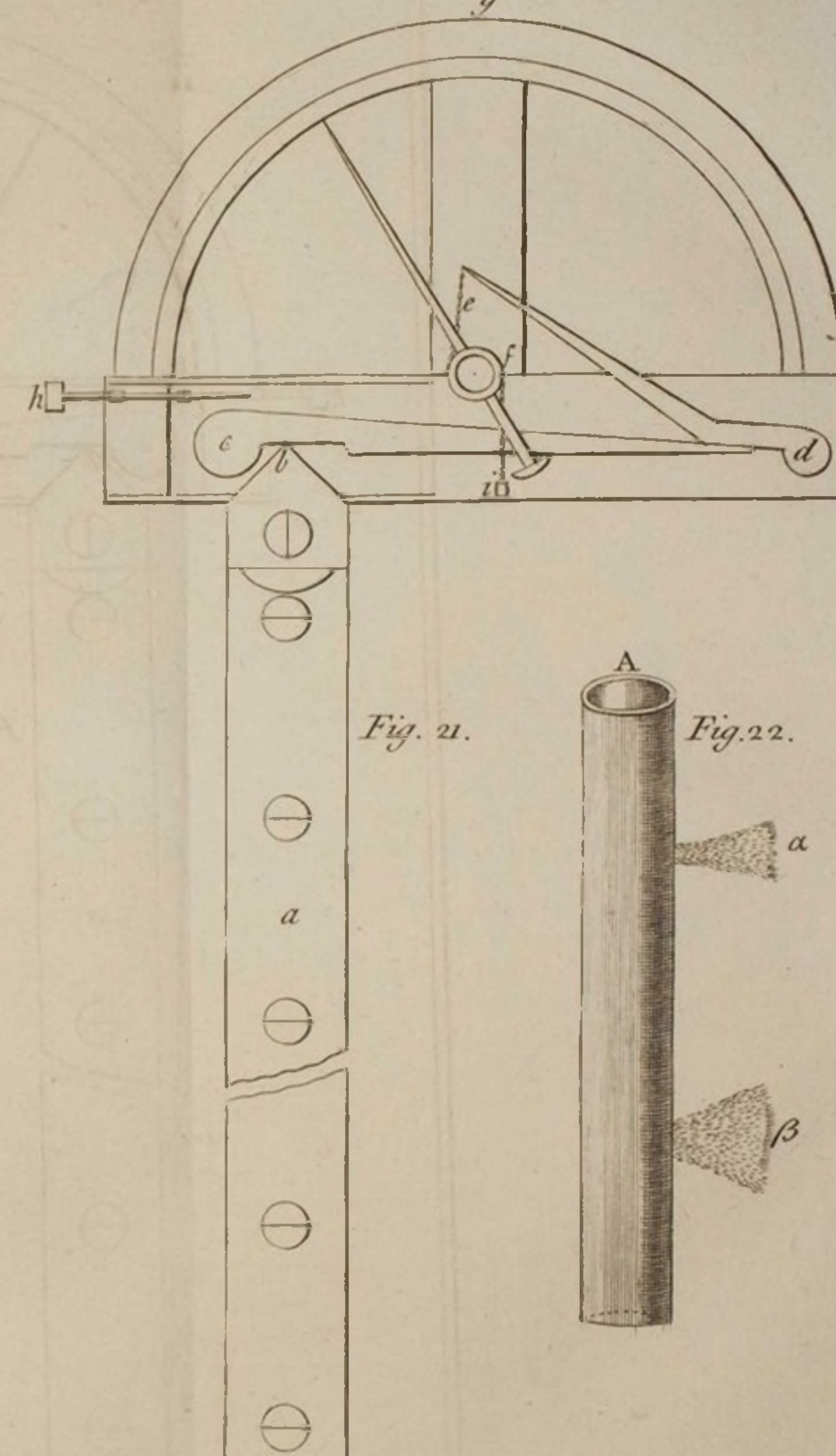


Fig. 21.

Fig. 22.

to W. and S. W. and before 10 in the Morning we had ſome rain, and this ſevere froſt went off. At this laſt-mentioned hour the Thermometer without had riſen to *5 gr.* above 0; that within continuing at 1 *gr.* as before. At $8^h\ \frac{1}{2}$ that evening the Thermometer without was at no leſs than 12 *gr.* above 0, that within at *3 gr.* above 0: ſo that from that time I made my obſervation at $4^h\ 20'$ in the morning to $8^h\ \frac{1}{2}$ at night, there was a change in the temperature of the air abroad of 26 *gr.* $\frac{1}{2}$; while the change within doors did not amount to more than $\frac{1}{2}$ *gr.* warmer.

It ſeems probable from hence, that we may have frequently had greater extremes of heat and cold by far, than have fallen under obſervation.

The heat *of* boiling water *varies according to the* weight *of the* air; *by* M. Secondat de Monteſquieu, *of the* Acad. *of* Sc. *at* Bordeaux. *In a letter to* M. Folkes, *Eſq*; P. R. S. N°. 472. p. 32. Jan. *&c.* 1744. *Read* Mar. 8. $174\frac{3}{4}$.

IV. It is well known, that the greateſt degree of heat in common water is that which it acquires by boiling; that is to ſay, if water is put upon the fire, it grows by degrees hotter and hotter, till it quite boils; but, after that, though there be never ſo much fire added, and it ſtand never ſo long upon it, it will never grow hotter than it was on the firſt inſtant, when it began to boil. Hence the degree of heat of boiling water is looked upon as fixed and invariable.

Fahrenheit, that ingenious maſter in Mechanics, ſo well known by his mercurial Thermometers, is the firſt who has remarked the contrary. He obſerved, that the heat of boiling water was greater when the air was heavy (that is to ſay, when the mercury ſtood higher in the Barometer); and, on the contrary, the heat was leſs when the air was lighter.

Mr *Le Monnier* the younger, who has obliged us with a tranſlation of Mr *Cote*'s Lectures in Nat. Philoſ. with excellent notes upon the ſaid work, has put *Fahrenheit*'s diſcovery paſt all doubt, and has very much improved it.

Oct. 6. 1739, being provided with a Barometer, and a mercurial Thermometer of M. *De Liſle*, he climbed up to the higheſt top of the *Canigou*, a mountain in *Rouſſillon*, which paſſes for the higheſt among the *Pyrenees*: There he found his Barometer to ſtand at 20 inches $2\frac{1}{2}$ lines; whilſt at *Perpignan* it ſtood at 28 inches 2 lines. The difference between the heat of the water which he boiled there, and that which he boiled at *Perpignan*, was 15 degrees of his Thermometer.

The ſame Thermometer being ſurrounded with ſnow, the mercury fell down to the ſame degree as pounded ice had made it do at *Paris*. Hence he concludes, that the heavineſs of the air has a ſenſible influence on boiling water; but that it in no way alters the term of congelation. All theſe particulars may be ſeen, *p.* 408. of *Cote*'s Experimental Lectures; and in the Mem. of the Acad. of Sc. of *Paris*, 1740.

This is the ſame experiment which I have repeated on the top of the *Pic du Midy*; thinking that ſo ſingular a fact ought to be obſerved more than once.

I carried two Barometers, the tubes of which the Rev. Father *Francis* had been so good as to fill for me with great care. I had likewise with me 2 mercurial Thermometers, upon which I set the degrees at *Bagneres*: I took the fixed terms of the graduation; that is to say, that of congelation, and that of boiling water, afterwards putting nought to the term of congelation. I marked 180° difference between this term and that of boiling water.

Being come to the highest top of the *Pic du Midy* on the 9th of last *July*, the mercury rose in one of my Barometers to 20 Inches 2 lines; and in the other, to 20 inches 1¼ line. I surrounded my Thermometer with snow, and the mercury fell exactly to the same degree as the snow had made it fall to at *Bagneres*. Afterwards I plunged it into boiling water; whereupon the mercury rose to 165° of my graduation; so that the difference between the heat of boiling water on *Pic du Midy*, and that at *Bagneres*, consisted of 15°.

At my return to *Bordeaux*, I observed, that I had marked the term of boiling water at *Bagneres* less high by 3½ than at the term of boiling water at *Bordeaux*, taken at the time when the Barometer was at 28 inches 2 or 3 lines: therefore having anew graduated my Thermometer the 165th, the degree of the former graduation fell now upon the 162d; so that the complete difference between the term of boiling water on the top of the *Pic du Midy*, and that of the same at *Bordeaux* the Barometer being at 20 inches 3 lines, amounts to 18 degrees on the Thermometer of *Fahrenheit*.

Now the conformity between the observation made by M. *Le Monnier*, and this repetition of the same observation, can hardly be greater; seeing the heights of the Barometers are almost the same; and the 15 degrees of difference, found by M. *Le Monnier* on *De Lisle*'s Theremometer, amount precisely to 18 degrees on the Thermometer of *Fahrenheit* which I made use of.

Of sudden freezing; by Sam. Christian Hollman, Prof. Pub. Ord. Philos. Gotting. No. 475. p. 239. Jan. &c. 1745. *Read* Jan. 10. 1745.

V. The *phaenomenon* of Mr *Triewald* related in the Philos. Trans. seemed to me so extraordinary at first reading, that if I had not had some farther proof, that all congelations are performed almost in a moment of time, it would have seemed to surpass all belief. But I happened to meet with something not very unlike it about the end of 1742, which being at first discovered by chance, I endeavoured to reduce to a sure and certain experiment, both in that winter, and the following. About the end of 1742 Dec. 24. N. S. On the coming of a sudden very great cold, there appeared in 2 conical glasses what they call Cartesian devils, of various form and colour, some specifically lighter than the water, and others by a greater quantity of water admitted into their cavities, rendered heavier; so that being put into the air-pump when the air was exhausted, they could ascend in the surrounding water as it were of their own accord. I put both the glasses in a cold room unstopped, and covered only with a glass phial;

phial; from which seeing the same on the morning of Dec. 24, I removed them immediatly into a stove, and set them on a walnut-tree chest, being still full of a very fluid and transparent water. But as I had just recollected that glasses, and other such like cold bodies, on being removed into a warm place, used to have a remarkable quantity of dew running down from the surface in streams; being desirous to to save my chest from being damaged, or at least stained, I examined the paper which I use to lay under my glasses, and as I lifted up one of the glasses, I found it already covered with a great quantity of dew; but when I took up the other, in which the devils being heavier than water, sunk to the bottom, I found it dry but quite full of ice. This unusual and unexpected sight greatly surprized me, and being sollicitous about my little glass images, I took up a brazen tube which happened to be at hand, in order to save them, if possible, from the ice. But when I found the cakes of ice to be surprisingly interwoven with each other, and to be very thin and soft, and that the whole icy cone did not cohere with the glass, but only in a small part, and that what was at the bottom of the glass, to the thickness of an inch, swam in the water and began sensibly to dissolve into water at the sides, I began to enjoy this pleasing spectacle with more security and delight. All the thin plates of ice, which constituted that whole truncated cone, were very thin, and as transparent as glass, and by their various inclinations, when the whole glass was turned about, they shone surprisingly like tables of glass; and this delightful spectacle was not a little increased by those which stuck about the head of the black Cartesian devil.

I was willing to make use of this opportunity to try, whether on melting the ice again, and putting the glass again in it's former place, the same *phaenomenon* could be again procured. But when I first read the experiment of Mr *Triewald*, I was of opinion, that by the pressure of the bladder tied about his glass, the degree of pressure arising from cold was perhaps increased, and that thereby that sudden effect of congelation was produced, and so I hoped that by the same way, it would also succeed with me in water sufficiently cooled.

It would be tedious to relate all the experiments that I made on Dec. 24, 26, and 27, and repeated often with much trouble. I confess the experiment did sometimes succeed; but then I was more often frustrated of my hope, tho' all the circumstances were the same. The *whole* glass [*a*] was sometimes filled with ice, as I was looking upon it; sometimes there was a sort of *icy bladders* [*b*], when I was pressing the bladder of the glass, that rose to the sides of the glass, which presently turned into an icy crust, that incompassed the whole inner surface of the glass; whilst the remaining inner mass of water closely surrounded the *axis* of the glass, and remained fluid; sometimes *very transparent single* thin plates [*c*], almost resembling the form of *snow*, appeared in the middle of the clear water, as I was holding the glass in my hand, in viewing it in the full light of day, and these were so very thin, that on any little shaking of my hand they would fall into the water and tremble. But there would be no end, if I should relate all. I will therefore, as briefly as I can, relate only the principal experiments which were often and diligently repeated.

1. When the water had conceived a sufficient degree of cold, whether the glass was removed into a warm place, or was only taken in a warm hand, almost the whole water would in a moment be turned into ice. But this happened more often and more easily in the former case than in the latter.

2. It did not signify whether the glass was covered with a bladder or not; and in the former case, whether the bladder was pressed with the fingers or not. Nor,

3. Did I find it to signify, whether any Cartesian devil, or any other like image was in the glass or not; and if there was one, whether it kept at the bottom of the glass, or swam on the surface of the water.

4. When there was any little image in the water, as often as I was able to observe the beginning of freezing, it always began from some part of it, and thence diffused itself on all sides.

5. The experiment succeeded according to my mind, only when the glass being full of water was so exposed to the cold air, that the whole mass of water could be equably penetrated thereby. But if,

6. The glass was placed before an open window, which admitted the cold air, or on a plastered floor, which is usual here, there was ice indeed generated, but such as I described above at [*b*]. But if,

7. The glass was placed before an open window, where the wind did not blow, on a boarded floor, or on a wooden plank laid over the plaster, the experiment generally succeeded: unless perhaps I came a little too late, and the glass was already wholly filled with ice. But,

8. That I might more easily observe the degree of cold, to which the water ought to come, if the momentaneous freezing of it was to happen in a warm place, I filled a broader earthen vessel with water, and set it on the same base with the glass; and observed carefully, when the water contained therein began to skin over. And tho' these congelations did not happen exactly at the same time, yet this observation often contributed to the better success of my experiment. For it is a very troublesome and tedious experiment, without it. But perhaps,

9. The conical figure of the glass, on account of the narrow orifice and broad base, does not a little contribute to the force of the cold and heat, which is to be equably diffused thro' the whole mass of water. At least I am persuaded, that the experiment will succeed with more difficulty in any cylindrical vessel, or account of its orifice being too broad; tho' I have not yet tried it.

The weather did not afterwards give me any opportunity of repeating

ing the same experiments that winter. But I repeated them in *Jan.* 1744, with the same success, and with the same *phaenomena* as above.

I will not presume to assign the cause, why the water cooled almost to the point of freezing, as it were in one moment, turns into innumerable icy plates, crossing each other in a wonderful manner, and forming one continued body, if the glass in which it is contained is surrounded by a *sudden heat*. The *Aristotelians* perhaps would here please themselves and others with their word *Antiperistasis*. But I am not desirous to increase the number of empty words; and shall content myself with having related the *phaenomena*, and submitting to the judgment of the *Royal Society*.

VI. Mr *Boyle* has taken much pains to bring the Hygrometer to perfection; and Mr *Pickering* has lately made an improvement to it: But, as the instrument I use differs from them both, I shall beg leave to describe it to you.

Description of an improved Hygroscope. *From Mr* Will. Arderon, *F. R. S. to Mr* H. Baker, *F. R. S.* N°479. p. 95. Mar. & Apr. 1746. *Read* Feb. 27. 1745-6.

Some years ago I applied my thoughts to consider the nature of Hygroscopes, and compared many different sorts together, in order to determine which I might employ with the greatest certainty; when none appeared to me to come nearer the truth than that recommended by Mr *Boyle*, of weighing a piece of sponge in a pair of gold scales. But the difficulty and time, which I found, upon trial, were requisite to adjust the weights, and discover the true state of the air, set me upon contriving another method, whereby at all seasons I might perceive, by inspection only, the most minute alterations with respect to moisture or dryness; and the following drawing will, I believe, sufficiently describe what I found most effectual for that purpose.

A represents a thin piece of sponge, so cut as to contain as large a superficies as possible. This hangs by a fine thread of silk, upon the beam *B*, and is exactly balanced by another thread of silk at *D*, strung with the smallest lead-shot, at equal distances, and so adjusted as to cause the index *E* to point at *G*, in the middle of the graduated arch *F*, *G*, *H*, when the air is in a middle state between the greatest moisture and the greatest dryness. *I* shews a little table or shelf, for that part of the silk and shot which is not suspended, to rest upon. Fig. 23.

VII. The Weather-cord is an Hygrometer of a very ancient invention, and, if properly constructed, may be made use of with very good success, to shew the various alterations of the atmosphere, in respect to moisture and dryness; but, as commonly made, it never rises or falls sufficiently to point out such minute changes as the curious would be desirous to know. A sense of this defect set me upon endeavouring to find out some method of removing it; and how far I have succeeded, will best appear upon casting your eye upon the drawings, *Fig.* 24. and 25.

Improvement of the Weather-cord. *In a letter from Mr* Arderon, *F. R. S. to Mr* Baker, *F. R. S.* N°. 479. p. 169. Mar. &c. 1746. *dated* Dec. 21. 1745. *Read* Apr. 24. 1746.

In

Fig. 24. In the firſt of theſe Hygrometers which I made, as in *Fig.* 24. I only fixed the end of the index AB faſt to the ſilk CE at A, leaving it lying looſe upon the point D; and in this manner the other end of the index would nearly deſcribe the arch FGH: But then I ſoon perceived, that the centre of motion, whereon the index turned, was changed whenever it moved ever ſo little; and, conſequently, that the arch ſtruck by the end B muſt be irregular.

Fig. 25. On conſidering this, I toothed two pieces of braſs, as 1, 2, and 3, 4, (*See Fig.* 25.) to fit each other ſo exactly, that, upon the leaſt motion of the one, the other would move; then, fixing the index upon the centre C, it's motions were rendered much more regular.

I placed likewiſe a little collar of braſs at B, upon the cord SR, and to that collar tied the ſilk, which gave motion to the index, that the cord SR might twiſt and untwiſt without any impediment.

If there is no weight placed at bottom, as in *Fig.* 24. X the piece of braſs 1, 2 muſt be ſo heavy as to keep the cord SBR at a convenient tightneſs, and alſo to counterbalance the end of the index CE, provided it be heavier than the other.

The length of the cord SBR, it's thickneſs, and the manner of preparing it, are already deſcribed in ſo many books which treat of Hygroſcopes, that to mention them would ſeem unneceſſary.

An Hygrometer made of a Deal Rod; *by the ſame.* N°. 480. *p.* 184. May *&c.* 1746. *Read* May 8. 1746.

VIII. In *Oct.* laſt I contrived and made an Hygrometer; the firſt hint whereof I received from obſervations on the ſwelling of deal doors againſt rain. I perceived this wood expanded itſelf very conſiderably, laterally, or acroſs its grain: and this I imagined, if properly made uſe of, might ſhew, not badly, the different degrees of moiſture or dryneſs in the air. Theſe thoughts ſet me upon ſearching the *Philoſ. Tranſ.* to ſee if any ingenious perſon had recorded his opinion upon this ſubject: and I found * that an anonymous author had made ſeveral attempts to conſtruct Hygrometers of deal boards †; and again ‖, that Mr *J. Coniers* had added ſome improvements thereto; but, as the method taken by theſe two gentlemen ſeemed liable to ſome objections, I determined to make a trial on a plan and form intirely different from theirs; and have been ſo fortunate to find it ſucceed greatly beyond my expectations.

My way was thus: I procured a piece of coarſe deal board; moſt of it, if not all, ſap. From this I ſawed 7 pieces croſs the bate or grain, 10 inches long and an inch broad; and as the board was juſt an inch in thickneſs, I thereby conſequently obtained 7 parallelopipeds of an inch ſquare each.

* *See Vol.* II. *Chap.* I. § xvi. 1.

† This author ſays, Poplar would do much better; but of that I have had no trial.

‖ *See Vol.* II. *Chap.* I. § xvi. 2.

Theſe

Theſe 7 pieces of deal I joined together, lengthways, with ſtrong glue; which made a ſquare rod of 70 inches long. I found it neceſſary to place theſe ſmall pieces in ſuch a manner, when I glued them together, in reſpect to their grain, as is repreſented in the two figures annexed, to prevent their forming themſelves into a ſort of curve; which they naturally do, if they are placed all the ſame way; and I found myſelf obliged to fix the rod in ſuch a number of brackets as appear in the drawings, in order to keep it ſtrait.

I placed this rod, at firſt, perpendicular to the horizon, betwixt two pieces of wood of the ſame thickneſs, and nailed againſt the cieling of my room; but then I had one ſide only expoſed to the air: however it acted tolerably well, which encouraged me to try to make it more perfect; as you will find delineated, *Fig*. 26. and 27. Fig. 26, 27.

Both theſe deal rods were placed againſt the cieling of my room with brackets, and were buttoned down into ſquare mortiſes in each bracket with ſmall pieces of deal, that fitted their tops exactly. Hereby all their 4 ſides became expoſed to the air; and the only difference between them is, the increaſing the effect of their variation by two different methods.

To the rod at *Fig*. 26. I added 2 levers: The firſt of which *A B D* had it's ſhorter end *A B* but 3 inches in length, and it's longer *B D* 12; conſequently the end *D* moved through 4 times the ſpace that the end *A* did.

The ſecond lever *E F G*, I fixed to act with the other before mentioned. The ſhorter end *E F* of this lever was 3 inches, and the longer end *FG*, 45 inches; whereby the effect of the other lever was increaſed 15 times, and that of the deal rod 60 times. So that if the rod lengthens but one tenth of an inch, the point of the lever *G* moves 6 inches; and if the rod lengthens but one inch, the point *G* moves 60.

The longer end of the ſecond lever in *Fig*. 26. muſt be made ſo much heavier, that it may move down freely by its own gravity whenever the bar ſhortens.

To this Hygrometer I fixed a ſmall index, ſuch as is common in Mr *Haukſbee*'s Barometers, to ſlip up and down on a wire, as is repreſented at *K*.

Fig. 27. repreſents another method I employed to increaſe the power of the deal rod. This may be fixed in a much ſmaller compaſs, and yet is no leſs capable of ſhewing the minute differences in the moiſture or dryneſs of the air than the other before deſcribed.

The deal rod in this was managed and faſtened in the ſame manner as was ſhewn before. I likewiſe applied a lever *A B D* to the top thereof, exactly of the ſame dimenſions as in the other; but, inſtead of a ſecond lever, I placed a graduated circle, with an index thereto like that of the minute-hand of a clock. This I fixed to a ſmall *axis*, which was moved one way by a ſilken thread wrapped twice or thrice round it, whereof one end was tied to the longer end of the lever at *D*, and the

the other way by the gravity of the weight *W*. And here, if the length of the index *R S* be 15 times as long as the ſemidiameter of the *axis* which the ſilk turns upon, it is evident that our ſenſibility of the rod's alteration will be increaſed 60 times, *&c.* The deal rod is ſtrongly nailed down at *N*, both in the firſt and ſecond draught; but, in all other parts, they have free liberty of contraction or dilatation.

Unleſs I am too much prejudiced in favour of this Hygrometer, it far exceeds all that I ever ſaw; and I may take upon me thus far to aſſert, that, by acting 10 or 12 hours before the apparent change of weather happens, as this has done ſince I got it finiſhed; it may very much aſſiſt to form a true judgment on the impending changes, when the wind is in or near the E. or W. point, when the Barometer is of little or no ſervice.

I obſerve, that heat and cold have a conſiderable power of lengthening and ſhortening the deal rod, as well as the moiſture and dryneſs of the air; and this, at firſt ſight, would induce one to imagine, that it would thereby be rendered almoſt uſeleſs; but it is really far otherwiſe; for, by placing it near a Thermometer, it is eaſily rectified with reſpect to its expanſion or contraction, by heat or cold, at the ſame time that it truly ſhews the various degrees of moiſture or dryneſs in the air. In ſhort, it is an inſtrument made very eaſily, of materials to be got almoſt every where, and of little coſt. It is capable of being ſerviceable either by ſea or land, and may be placed in any direction.

I have lately finiſhed one of theſe Hygrometers, the bar whereof is but a foot in length, yet, by making the two levers of metal, I eaſily make the end of the ſecond lever riſe and fall 8 inches. At this ſize it becomes portable, and anſwers the end or purpoſe as well as the other.

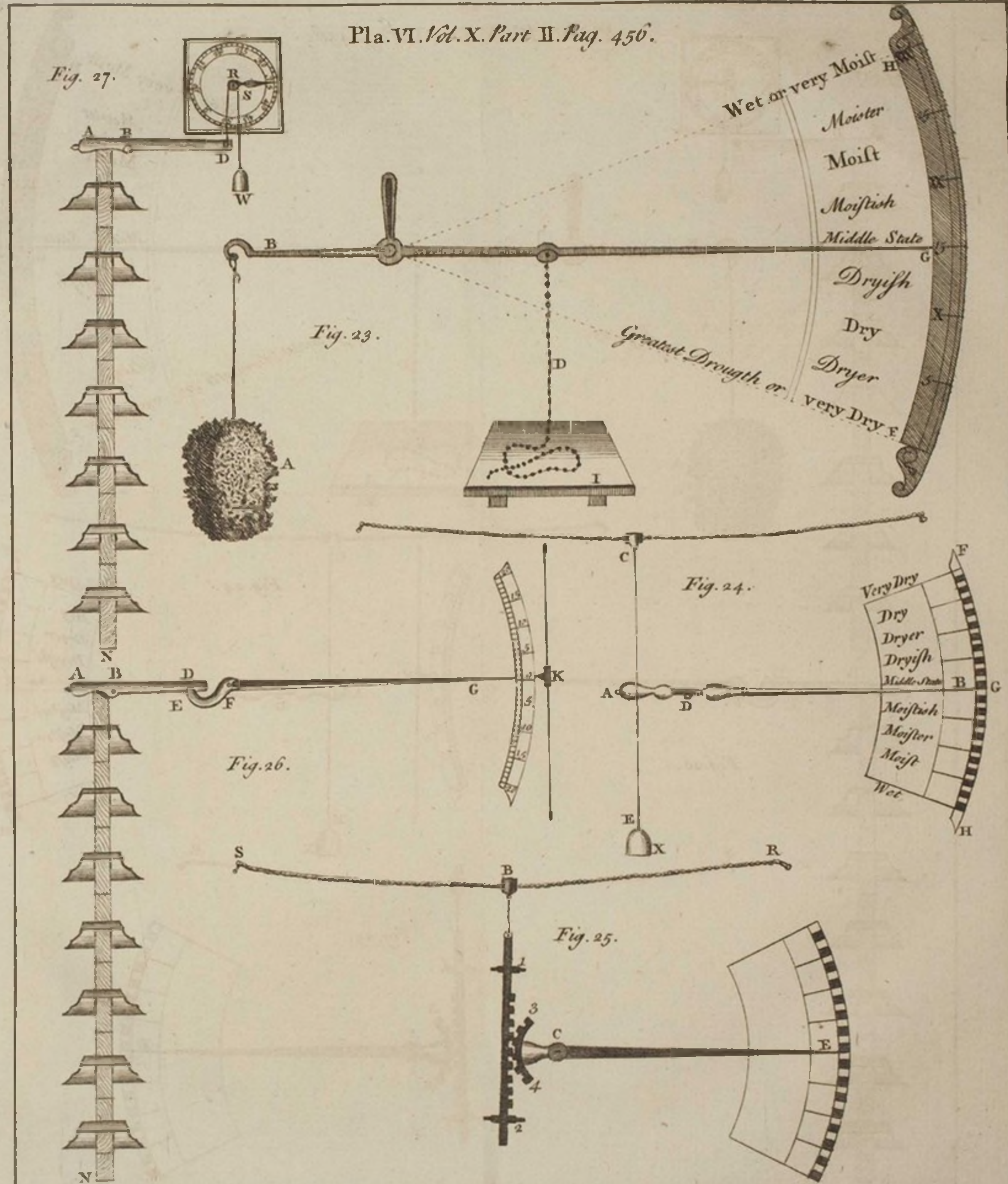
P. S. Since my writing this, I have been turning over a little treatiſe of Mr *Boyle* upon the Hygroſcope, wherein I find he had made ſeveral trials with different ſorts of wood; but then they were turned into the ſhape of bells; which, he ſays, anſwered very well, by comparing their weights: That their different weight increaſed or diminiſhed, according to the different degrees of the air's moiſture; and that he had taken notice of the expanſion of the wood in doors and door-caſes: but he proceeds no farther than to recommend the different kinds of wood to be examined by their weight.

Scheme *of a* Diary *of the* Weather; *together with draughts and deſcriptions of* Machines *ſubſervient thereunto; by Mr* Roger Pickering, *F. R. S.* No. 473. *p.* 1. May *&c.* 1744. *Read* May 3. 1744.

IX. A ſenſe of the importance of obſerving the weather induced *Hippocrates*, in his remarks upon the Epidemic Diſeaſes in *Thaſos*, to premiſe a general hiſtory of the weather preceding them; and with the ſame view did our great Mr *Boyle* turn his thoughts ſo cloſely upon the ſame ſubject: whoſe example, being followed by ſeveral judicious inquirers into nature, both abroad and at home, has brought the *Natural Hiſtory* of the *Air* to a ſurpriſing degree of perfection, beyond what the Antients ever could pretend to, or even thought of. Had but each county in *England* gentlemen of ſuch ſentiments, who would charge

themſelves

Pla. VI. Vol. X. Part II. Pag. 456.



themſelves with the annual trouble of ſending a regular account of the *weather* to this *learned body*, by it to be compared and digeſted, to what degrees of accuracy may we not ſuppoſe a knowledge of the nature and affections of the *atmoſphere* may be brought; and how well may we not hope to be guarded againſt the diſorders, which, as *iſlanders*, we are expoſed to, by ſuch a cloſe inquiry into the nature of that neceſſary fluid in which we breathe! Not to mention the advantages which ſeveral important branches of trade may receive from ſuch meaſures: and were the digeſted obſervations of the *R. Soc.* compared with thoſe of foreign ſocieties, formed upon the ſame plan, how ſhort a time would bring this part of Philoſophy to the greateſt degree of demonſtrable certainty!

The trouble of making and keeping ſuch *meteorological regiſters*, which, in all probability, prevents ſeveral gentlemen from performing this piece of ſervice to the public, might be rendered very inconſiderable, by the propoſal of an eaſy, as well as comprehenſive, method for a *diary*, and a ſet of ſimple and convenient *machines* for making the neceſſary obſervations.

The plan of the *Ephemerides Ultra-jectinæ*, though comprehenſive, is, with ſubmiſſion, very perplexed; and the ſeveral others, mentioned in the *Philoſ. Tranſ.* perhaps, do not include all the particulars of which ſuch a *Diary* ſhould conſiſt. The Society of *Edinburgh* has prefaced to their *Medical Eſſays* a ſcheme (which I had not the pleaſure of ſeeing till a great while after I had fallen into the following method) the moſt calculated for uſefulneſs; but their *machines* are neither ſo ſimple nor accurate, as ſuch a work requires; not to mention their being entirely without one for obſerving the *force* of the *wind*.

On a page of a folio paper-book, opening broad-ways, are drawn, at proper diſtances, 9 *horizontal*, and 7 *perpendicular* lines; in the void ſquare ſpaces of which the particulars of the *diary* are written down. The firſt of the horizontal lines is for the days of the *month* and *week*, on which the examination is made: The ſecond for the *hour* of the *day*: The third for the *weight* of the *air*: The fourth for its *heat*: The fifth for its *moiſture*, or *dryneſs*: The ſixth for the *quarter* of the *wind*: The ſeventh for its *force*: The eighth for the *weather*; as whether it be *rainy*, or *cloudy*, or *clear*: The ninth for the *quantity* of *rain*; and the ſpace between the laſt line and the end of the paper, for the *bill of mortality*. Sect. 1. *An account of the* Diary *in general.*

The 7 *perpendicular* lines are for the 7 *days of the week*; which, in our *Diary*, begins with *Sunday*. If you therefore carry your eye along the paper from left to right, you may, at one view, ſee the *weight* of the *air*, and the degrees of *heat* and *moiſture*, &c. for the whole *week*. If you carry your eye from top to bottom down the column, for any one day, you ſee regularly the whole of the obſervations in one line for that day. Four pages, or weeks, we allow to each month, and then leave a void page for the *obſervations* made in that month; and the overplus *calendary* days are carried on to the page allotted for the next month;

month; only taking care to deſcribe in every ſuch page, where the ending and beginning of two different months are to be found, the names of both the months, directly over their final and initial day.

The abſtract of the *weekly bill of mortality* is apparently a part of obſervation peculiar to this plan, under which article all *acute caſes*, depending on the *ſtate* of the *air*, are ſet down. Perhaps the ignorance of the *ſearchers*, appointed to inſpect dead bodies, as to the preciſe diſeaſes people die of, may lay this method open to objection: To which it may be ſufficient to anſwer, That this being obviouſly a requiſite article for a *Diary*, we muſt be content to take our advices on this point from ſuch hands, rather than none; eſpecially, as all *political arithmetick* has always been allowed upon no more certain a foundation.

Sect. 2. A view of the machines in general.

The *machines* neceſſary to the making obſervations for a *diary of the weather*, are theſe five:

1. *The Barometer.*

I have found thoſe with *open ciſterns* more ſenſible than the *portable* ones. That with which I make my obſervations, is with an *open ciſtern*, furniſhed with a *Micrometer*, that divides an *inch* into 400 parts; by which I am capable of perceiving the moſt minute alteration of the *gravity* of the *air*: It was made by Mr *Bird* of the *Strand*; whoſe accuracy in graduation deſerves, I think, notice and encouragement.

2. *The Thermometer.*

Mine is one made by *Fahrenheit*'s ſcale on one ſide, with it's correſpondence to the graduation of the *alcohol* Thermometer on the other.

Of the three next *machines*, two are *new*, and the other conſiderably altered, and, I hope, improved, from one offered to the Society a great while ago.

Note, All the *machines*, except the *Barometer*, are expoſed to the open air. The *Thermometer* and *Hygrometer* are placed in a little *ſhed*, made for their reception, againſt my *ſtudy-window*, where I can ſee the graduation through the glaſs; and, by lifting up the ſaſh, can take them in, as occaſion requires.

Sect. 3. Of the Hygrometer.

I had, for ſome time, made uſe of Dr *Hooke*'s *Hygrometer*, made of the *beard of a wild oat*, ſet in a *ſmall box*, with a *dial-plate* and an *index*; but I ſoon found an inconvenience, without the remedying of which no dependence could be had on this *machine*, *viz.* its making more than one *revolution* in a night. I endeavoured to remedy this by the following method, deſcribed in *Fig.* 33.

At the vertical point, from which *moiſture* and *dryneſs* are graduated, I cauſed a ſmall *circle* to be deſcribed; the lower *arch* of which ſhould juſt interſect with that *arch*, round which the *index* of the oat deſcribed its circuit. In the centre of this ſmall circle I placed a *pin*, eaſily turning in the central cavity, and furniſhed with a flat piece of thin *ivory* on its head. This piece of *ivory*, interſecting with the index of the oat, by it was turned either to the moiſt or dry ſide of its graduation, as the *index* made a double *revolution*. I flattered myſelf with ſucceſs; but

ſoon

foon found, in the great fogs we had laſt winter, that the *wild oat* is not a ſafe material to make an accurate *Hygrometer* of: For,

1. In the great fogs it grew limber; ſo as that the weight of the *index* brought it down upon the plate, where its friction prevented it's further motion.

2. It ſoon loſes its *ſenſibility*, grows harſh, and is abſolutely unfit for uſe. So I immediately turned my thoughts upon ſome other for my *diary*, and reſerved this for my ſtudy; where, or in any incloſed place, it does well enough, and may be very uſeful in the following reſpects; as,

1. To examine, in caſes of ſickneſs, the *dampneſs* of *rooms*.

2. To examine damps in *ſubterraneous cavities*, being let down with a *weight*, where a light would ſometimes ſet the place on fire.

3. To obſerve the proper ſtate of dryneſs in *ware-houſes*, *wine-vaults*, *ſtudies*, where damps would be detrimental and pernicious.

4. To examine the ſtrength of *ſudden fogs*, and the *comparative* dampneſs of particular ſituations.

As a *ſuccedaneum* to this, I thought upon a *ſtatical* one; it recurring to my mind, that the weight and moiſture of the air being but two properties of one and the ſame body, a *ſtatical Hygrometer* (*cæteris paribus*) promiſed the beſt aſſiſtance towards a more complete knowledge of the *Barometer*, which acts upon ſtatical principles; and that theſe two machines muſt have a reciprocal correſpondence with each other. I then remembered, that Mr *Boyle* had mentioned ſomething of this nature; after conſulting whom, I made the the following *machine*, acting upon his principles, but formed in a manner differing from his.

I cauſed a *balance* to be made to turn with ¼ *a grain*, ordering that the *axis* of the *balance* ſhould, on one ſide, be drawn out to the length of one inch, and its end to be furniſhed with a *male ſcrew*, to which a light *index* with a *female ſcrew* might be fixed. I had this *balance* faſtened in a *wainſcot box*, 12 inches in length, 9 in diameter, and 4 in the depth at top, but gradually widening towards the bottom, with a back to ſlide up and down in a groove. The *axis*, already mentioned, of an inch length, came through a *hole* in the front of the box, and then had the *index* faſtened on, which deſcribed the *ſegment* of a *circle* upon a braſs plate, ſilvered and graduated into 180 gr. as if it had conſiſted of a perfect *ſemicircle*, or two *quadrants*. The reaſon why the graduation did not begin exactly with the diametrical line was, to prevent the friction of the *brachia* of the balance, with the little drop placed at the bottom of the *axis* already mentioned.

My next concern was to *charge* this *balance*. The beam turned, as has been ſaid, with ¼ *a grain*; and every ſuch turn, after repeated trials, moved the index ſomewhat more than one *degree* of the 180 deſcribed upon the *plate*; ſo I immediately pitched upon a 4 *penny-weight* all but 6 *grains*, which contained as many ¼ *grains* as there were *degrees*. This weight I fixed with a thread to one *brachium* of the *balance*, without

out any *ſcale*, the ſeveral threads or ſilk ſtrings of which, as they would imbibe more moiſture, would make the *machine* leſs accurate; and the other *brachium* I charged with a *ſponge*, ſuſpended likewiſe by a thread, of ſuch a weight, when reduced to abſolute dryneſs, as made an *equilibrium*; and then ſcrewing on the *index* to the firſt degree of the 180, and expoſing the *machine*, thus ordered, to the open *air*, in one night's time the *index* had got to the 70th degree; which, as the *ſponge* had been abſolutely dry, muſt have been the true *ſtate* of the *air*, as to *moiſture*, at that time.

I find this *machine* extremely ſenſible and accurate; it will alter 10 degrees in a night, and as many in a day; and has, I think, the following advantages:

1. It is more *portable* than any, except that of the *wild oat*; and, upon any accident, more eaſily and ſpeedily rectified than it, or any other whatever.

2. Being graduated from abſolute *dryneſs*, it is beſt calculated for the diſcovery of the true ſtate of the *air*, as to *moiſture*.

3. The near correſpondence between the degrees on the graduated *plate*, and the weight of the *moiſture* neceſſary to be imbibed or exhaled, to make either *brachium* of the balance preponderate every ſuch degree, gives it the preference to any other.

For a more perfect idea of this machine, ſee *Fig.* 28. where it is viewed on the inſide, the back being ſlid up. At *Fig.* 29. is repreſented the *Plate* with its graduations and index, as it ſhould appear on the front of the caſe.

Sect. 4. *Of the Anemoſcope.*

The *Anemoſcope* is a machine $4\frac{1}{2}$ feet high, conſiſting of a broad and weighty *pedeſtal*, a *pillar* faſtened into it, and an iron *axis*, of about $\frac{1}{4}$ an inch diameter, faſtened into the *pillar*. Upon this *axis* turns a wooden *tube*, at the top of which is placed a vane, of the ſame materials, 21 inches long, conſiſting of a *quadrant*, graduated and ſhod with an iron *rim*, notched to each degree; and a *counterpoiſe*, of wood as in the figure, on the other. Through the centre of the quadrant runs an *iron pin*, upon which are faſtened two ſmall round pieces of wood, which ſerve as moveable *radii* to deſcribe the degrees upon the quadrant, and as handles to a *velum* or ſail, whoſe plane is one foot ſquare, made of canvas ſtretched upon four battens, and painted. On the upper batten, next to the ſhod *rim* of the quadrant, is a ſmall *ſpring*, which catches at every notch correſponding to each degree, as the wind ſhall, by preſſing againſt the *ſail*, raiſe it up; and prevents the falling back of the *ſail*, upon the leſſening of the force of the wind. At the bottom of the *wooden tube* is an *iron index*, which moves round a circular piece of wood faſtened to the top of the *pillar* on the pedeſtal, on which are deſcribed the 32 points of the *compaſs*. The figure of this machine may be ſeen *Fig.* 30. Its uſes are the following

1. Having a circular motion round the *iron axis*, and being furniſhed with a *vane* at top, and *index* at the bottom, when once you have fixed the

the artificial *cardinal points*, deſcribed on the round piece of wood on the *pillar*, to the ſame *quarters* of the heavens, it gives a faithful account of that *quarter* from which the wind blows.

2. By having a *velum* or ſail elevated by the wind along the *arch* of the quadrant, to an height proportionable to the power of the column of wind preſſing againſt it, the *relative force* of the wind, and its *comparative power*, at any two times of examination, may accurately be taken.

3. By having a ſpring fitted to the notches of the *iron* with which the quadrant is ſhod, the *velum* is prevented from returning back upon the fall of the wind; and the machine gives the force of the higheſt blaſt, ſince the laſt time of examination, without the trouble of watching it.

I have carefully examined the dependence that may be had upon this machine, during the late ſtorms in *February* 174$\frac{2}{3}$, by comparing the height to which the wind then forced the *velum*, with the *Deal* letter. The 19th of *Feb. Sabbath*, 8 *a. m.* the Anemoſcope was at 75: The *Deal* letter for that day called it a ſtorm. The *Saturday* following, being the 25th, at 8 *p. m.* the machine was at 79: The *Deal* letter called that a violent ſtorm. The *Wedneſday* following, the laſt of *Feb.* it was at 84: The *Deal* letter called that a violent ſtorm. So that it appears, that, in ſuch as the ſailors allow to be *violent ſtorms*, the machine has hitherto anſwered well, and has had ſix degrees to ſpare for a more violent guſt, before it comes to an horizontal poſition.

It is certainly to be depended upon in ordinary weather, the *velum* being hung ſo tender, as to feel the gentleſt breeze. But, after all, I muſt freely own, that I fear the expoſing this machine to all winds, for a continuance, muſt ſoon diſorder it; and that irregular blaſts and ſqualls cannot fail in a ſhort time to impair it. It may not therefore be amiſs, to prevent this, for gentlemen to take the machine in in violent weather; and, by taking the *tube* off the iron *axis*, to make their obſervations with the *tube*, *vane*, and *velum*, in their hands; which, as it is very light, and far from cumberſome, is eaſy to do, as I have often experienced.

Sect. 5. *Of the Ombrometer.*

This machine conſiſts of a tin *funnel*, whoſe ſurface is an inch ſquare, a flat *board*, and a glaſs *tube* let into the middle of it in a groove (the length and breadth of both board and tube being *ad libitum*), and an *index*. My board is about 3 feet long, to anſwer the height of the rails that go round the top of my houſe, to one of which it is hung, clear of any obſtacle to prevent the free fall of the rain, with 4 little ſtaples that ſlide over as many tenter-hooks. The *bore* of my tube is about $\frac{1}{4}$ an inch; which, at a medium, is the beſt ſize, a larger bore obliging you to make your graduation the more contracted, and, conſequently, the leſs plain and accurate; and a leſſer not permitting you to return the water out of the tube when full, without the adheſion of a great deal to its ſides; which, when you have placed the tube in its perpendicular ſituation, ſubſides, and ſometimes fills up $\frac{2}{11}$ of an inch; which, with-out

out care, must necessarily make great mistakes in the *diary*. The method of *graduating* the board is this :

I had a vessel of tin made, whose contents were exactly a *cubic inch*. With this vessel, filled with water exactly to it's surface, I frequently gauged the tube, till, by repeated trials, I had found the height to which a *cubic inch* of water would rise in it. The space answering to this on the board I had graduated into 32 equal parts, and took the same method with the rest of the tube, till in the same manner I had graduated 4 such inches. Now the surface of the funnel being, as has been said, exactly a square inch, no rain can by it get into the tube, but such as falls within the square of one inch ; which, as the shower is more or less, has its exact quantity shewn upon the board, on which a moveable *index* is placed.

This machine has highly answered my expectation ; its form being very simple, and easily repaired, if any accident happen. For, should the tube be broke, 'tis only rubbing out the graduation, which is marked with a black-lead pencil upon the board painted white, and gauging your new tube with the cubic measure for a new graduation, and your machine is again complete. I had one tube broke, and about 3 hours pains set all to-rights. In winter it will be necessary to let no depth of water remain in the tube ; for, should there be a frost, the expansion of the ice will certainly break it. The machine will equally serve for dissolved hail and snow. See *Fig.* 32.

Sect. VI. *Of the monthly observations.*

The vacant page at the end of every 4 weeks, reserved for observations occurring in the preceding month, and giving a summary account of the greatest difference of the weather in it, is a method peculiar to this *Diary* ; and one which, I flatter myself, will be allowed exceeding pertinent and useful. The great end of this, and all *Diaries*, is to furnish materials for a set of sound observations upon which to build a thorough knowledge of the *atmosphere*, and its effects upon mankind : and it is easy to see what great advantage to this part of *natural knowledge* must arise from a variety of observations, made by different men of application and judgment, upon one and the same subject. Besides, in this portion of our design may be included, what could not well without perplexity be thrown into the columns of the Diary, all the meteorological appearances of the *Aurora Borealis*, lightning, thunder, *&c.* together with abstracts of the most authentic accounts of such *phaenomena*, as at any time in the preceding month have been seen in different parts of our own country, or abroad. But this article must be left to every gentleman's judgment ; it opening a fair field for the most happy advancements of many parts of *natural knowledge*.

An explanation *of the* characters *in the* Diary.

This —— line implies the machine's being at the same degree as it was in the preceding observation. This O character in the spaces for the force of the wind implies a *calm*.

Note, 1st, None, but the cardinal and subcardinal *points* of the *compass* are commonly marked, unless in case of a *storm*.

2dly,

2dly, In the abstract of the *bill of mortality*, which comes out on a *Tuesday*, the account in each week is to be compared from the *Tuesday* of the week before, to the *Tuesday* in that week where the abstract is placed.

APRIL 1744.

Days of the Month and Week.	1 Sabbath.		2 Monday.		3 Tuesday.		4 Wednesday.		5 Thursday.		6 Friday.		7 Saturday.	
Hours of the Day.	8 *a. m.*	8 *p. m.*	8 *a. m.*	8 *p. m.*	8 *a. m.*	8 *p. m.*	8 *a. m.*	8 *p. m.*	8 *a. m.*	11 *p. m.*	8 *a. m.*	8 *p. m.*	8 *a. m.*	8 *p. m*
Barometer.	$29\frac{196}{400}$	$29\frac{192}{400}$	$29\frac{126}{400}$	$29\frac{45}{400}$	$29\frac{144}{400}$	$29\frac{246}{400}$	$29\frac{297}{400}$	$29\frac{305}{400}$	$29\frac{146}{400}$	$29\frac{116}{400}$	$29\frac{132}{400}$	$29\frac{335}{400}$	$29\frac{392}{400}$	$29\frac{378}{400}$
Thermometer.	37	—	36	—	37	35	38	35	40	45	40	34	55	45
Hygrometer.	70	77	—	79	81	80	—	74	81	74	77	74	—	69
Anemoscope: Quarter.	W.		N. W.		—		—		S. E.		N. W.		W.	
Anemoscope: Force.	28		30		74		16		20		16		O	
Weather.	Sleet.	Rains.	Snow.	Sleet.	—	Cloudy	—	Starlight.	Rain.	Cloudy.	Overcast.	Starlt.	Fine.	Overcast.
Ombrometer.		13	3	13	22	3			2	1	13			
Bill of Mortality.	Buried.		Males - - 176 Females - 217 Total - - 393 Decreased 70		Died of		Apoplexy — 1 Asthma — 8 Colic — 1		Fever — 52 Gripes — 4 Lunatic — 2		Small Pox 22 Suddenly 2			

OBSER-

OBSERVATIONS in APRIL 1744.

Days of the Month.	Days of the Week.	Hours of the Day.	
19	Monday.	*M.* 12	LAST Night, as 8 ½ Hour, carefully watched, whether the lunar Eclipse had any Effect upon the Hygrometer; but could not, after several Examinations, perceive that it had any.
20	Friday.	*P. M.* 8	This is the first Day of our being favoured with warm Spring Weather. The Thermometer at 8 this Morning was at 65.
24	Tuesday.	*P. M.* 10½	It now lightens towards the S. E. This is the first we have had this Year.

A SUMMARY of the greatest Difference of the WEATHER in APRIL 1744.

Days of the Month.	Days of the Week.	Hours of the Day.			
21	Saturday.	*A. M.* 8	Mercury	Highest being then - -	$30\frac{167}{400}$
5	Thursday.	*P. M.* 11		Lowest - - - - - - -	$29\frac{116}{400}$
21	Saturday.	*P. M.* 8	Thermometer	Hottest - - - - - - -	75
6	Friday.	*P. M.* 8		Coldest - - - - - - -	34
3	Tuesday.	*A. M.* 8	Hygrometer	Moistest - - - - - - -	81
21	Saturday.	*P. M.* 8		Driest - - - - - - -	65
			Anemoscope	Quarterly most from S. E.	
				Force greatest from N. W.	74
3	Tuesday.	*A. M.* 8	Weather very unconstant.		
			Ombrometer	Most Rain on 7th in the Night	$\frac{34}{32}$
				Total Rain 5 Inch and	$\frac{6}{36}$
			Mortality	Greatest in the 3d Week	432
				Least 1st Week - - -	393
				Total - - - - - -	1702

Description *of the* Figures.
Fig. 28.

aaaa. The *Hygrometer* seen in the inside. *bb.* The balance. *c.* A small piece of wood, by which the balance is fastened to the box. *d.* The sponge. *e.* The weight. *ff.* Two little rings, by which the Hygrometer is hung up.

Fig. 29.

The graduated plate on the front of the machine, with it's *Index* and *Divisions.*

a. The

a. The pedeſtal. *b.* The pillar, in which the iron axis is fitted. *c.* The circle of wood, on which are deſcribed the 32 points of the compaſs. *d.* The index. *e.* The wooden tube upon its axis. *f.* The *velum.* *g.* The graduated quadrant. *h.* The counterpoiſe of the vane. Fig. 30 The *Anemoſcope.*

The plane of the *velum.* *b.* The ſpring. *cc.* The wooden *radii.* *dd.* The holes, thro' which the pin, in the centre of the quadrant, goes. Fig 31. *The Velum taken off.*

aa. The board. *bb.* The tube. *c.* The graduation. *d.* The funnel fixed in the tube. *e.* The funnel one *Inch* ſquare. Fig. 32. The *Ombrometer.*

a The box and plate. *b.* The wild oat, with the index upon it. *c.* The pin, with a ſmall piece of ivory on its head. Fig. 33. The wild-oat *Hygrometer.*

A Letter from Dr John Lining, *to* C. Mortimer, *M. D. Sec. R. S. concerning the weather in* South-Carolina; *with abſtracts of the tables of his* Meteorological obſervations *in* Charles-Town N°. 487. p. 336. Apr. &c. 1748. *Read* May 6. 1748.

X. The viciſſitudes of the weather, with reſpect to heat and cold, are perhaps no where greater than in *Carolina*; and our ſummer's heat is probably not inferior to that under moſt places of the Equator; nor is our winter's cold much leſs at ſome times than that in *Britain.*

From near 8 years obſervation, the greateſt increaſe of the heat of the air which I have diſcovered in 24 or 30 hours, in ſpring, ſummer, autumn, and winter, was 19, 24, 13, and 16° of *Fahrenheit*'s Thermometer; and the greateſt decreaſes of heat, in the ſame ſpaces of time, in thoſe ſeaſons, were 35, 32, 27, and 27° reſpectively. It frequently happens, that one day is 10° or more warmer than the preceding day; but the decreaſes of heat are always greater and more ſudden than its increaſes. *Jan.* 10. 1745. at 2 *p. m.* the mercury in the Thermometer was at 70; next morning it had ſunk to 26, and on the 12th in the morning it was at 15, which was the greateſt and moſt ſudden change I have ſeen.

In ſummer, the heat of the ſhaded air, about 2 or 3 in the afternoon, is frequently between 90 and 95°; and on the 14th, 15th, and 16th of *June* 1738, at 3 *p. m.* it was 98; a heat equal to the greateſt heat of the human body in health. In winter I never but once ſaw the Thermometer ſo low as 15: therefore the difference between the moſt intenſe heat and cold of the ſhaded air, in this province, is 83°; which is a much greater range than could well have been expected in this latitude; and taking the mean between thoſe extremes, 56 ſhould be the temperate degree of heat in this province: but the ſum of the thermometrical altitudes, divided by the number of obſervations which I made for ſome years together, gives 66, which may therefore more juſtly be reckoned the temperate heat in *Carolina*, which exceeds 48, the temperate heat in *England*, more than that exceeds the freezing point.

The mean heat of the ſhaded air, in ſpring, ſummer, autumn, and winter, taken from the mean nocturnal heat, and from the mean heat at 2 or 3 *p. m.* is 61, 78, 71, and 52°.

The mean heat of the ſhaded air at 2 or 3 *p. m.* in ſpring, ſummer, autumn, and winter, is 65, 82, 75, 55°, and the mean nocturnal heat in theſe ſeaſons is 57, 74, 68, and 49°. Therefore our winter's nocturnal heat, at a medium, coincides nearly with the temperate heat in *England*.

The Thermometer, when ſuſpended 5 feet from the ground, and expoſed to the direct rays of the ſun, and to thoſe reflected from our ſandy ſtreets, has frequently riſen in a few minutes, from 15 to 26°, above what was at that time the heat of the ſhaded air (but I have never yet made that experiment when the heat of the ſhaded air was above 88): when we are therefore expoſed in the ſtreets to the ſun in ſummer, we inſpire air from 4 to 28 degrees warmer than the heat of the human body.

The Thermometer, when buried in the ſands of the ſtreets, when the heat of the ſhaded air was 88, roſe in 5′ to 108, tho' there was at the ſame time a moderate wind.

In *June* 1738, when the heat of the ſhaded air was 98, the Thermometer ſunk one degree in my arm-pits; but continued at 98 in my hand and mouth: from which we ſee what little concern the air has in cooling the blood in the lungs. Two men who where then in the the ſtreets (when the heat was probably 124 or 126 degrees, as the ſhaded air's heat was then 98) dropped ſuddenly dead; and ſeveral ſlaves in the country, at work in the rice-fields, ſhared the ſame fate. I ſaw one of the men immediately after he died; his face, neck, breaſt, and hands, were livid,

From the barometrical table it appears, that the barometer's mean altitude, taken from it's greateſt and leaſt height, is 30.09 inches; and that its range is only 1.22 inch. Wherefore our atmoſphere varies only $\frac{1}{25}$ part in its weight. In the warm months, the mean barometrical ſtation, taken from it's greateſt and leaſt altitudes in theſe months, is 30.09 inches; and I have never yet ſeen it's range in theſe months exceed $\frac{58}{100}$ parts of an inch: therefore the changes of our atmoſphere's weight, in the warm months, will have but little effect upon human conſtitutions, as the difference between it's greateſt and leaſt preſſure is but $\frac{1}{5}$ part of that in cold climates, where the range of the Barometer is 3 inches. May not the great height of the Barometer in the warm months in this climate, proceed from the vaſt quantity of water, which is at that time ſupported in our atmoſphere, as the exhalation is then very great; or may it not proceed from the rarefaction of the mercury? for the weight of the mercurial column, at equal altitudes, will be different under different degrees of heat; and the mercury may therefore be ſupported at equal heights by columns of air of unequal weights.

It appears, from the barometrical table, that our eaſterly or northerly winds elevate the mercury, and that our ſoutherly or weſterly winds depreſs it; and I have as yet never obſerved the contrary.

A TABLE

A TABLE *of the highest and lowest Stations of* Fahrenheit's *Mercurial Thermometer in the shaded Air, with the mean meridian and nocturnal Heat, taken after Dr* Jurin's *Method.*

	1738 Highest	1738 Lowest	1739 Highest	1739 Lowest	1740 Highest	1740 Lowest	1742 Highest	1742 Lowest	1738 Mean Altit. in the Heat of the Day	1738 Mean Altit. about 10 p. m.	1739 Mean Altit. in the Heat of the Day	1739 Mean Altit. about 10 p. m.
January	71	36	69	19	67	30	68	34	60	53	53	45
February	72	34	75	38	75	27	68	25	57	51	63	55
March	74	32	79	40	80	34	77	32	62	56	65	59
April	86	50	84	50	83	51	88	59	75	65	69	65
May	91	52	86	62	87	56	88	63	79	70	77	73
June	98	59	87	69	90	66	90	65	87	77	81	74
July	91	70	89	68	91	70	95	73	85	77	[illegible]2	75
August	89	64	87	60	90	67	93	69	82	75	82	75
September	83	62	88	59	84	56	86	43	76	71	71	68
October	74	41	79	43	73	35	78	42	65	59	68	60
November	70	37	69	30	67	32	66	32	57	51	57	51
December	67	28	70	30	69	21	69	34	53	50	59	53
The Means									70	63	69	66

	1740 Mean Altit. in the Heat of the Day	1740 Mean Altit. about 10 p. m.	1742 Mean Altit. in the Heat of the Day	1742 Mean Altit. about 10 p. m.	Mean Altitude in the Heat of the Day	Mean Altitude about 10 p. m.	Meridian Heat warmer than the Nights	Mean Heat taken from the Mean Meridian and nocturnal Heat	R. Society's Thermometer 1739 Higheſt	R. Society's Thermometer 1739 Loweſt	R. Society's Thermometer 1740 Higheſt	R. Society's Thermometer 1740 Loweſt
January	49	43	55	49	54	48	6	51	40	74	44	67
February	59	49	52	45	58	50	8	54	36	60	40	69
March	63	54	60	53	62	56	6	59	35	63	34	65
April	74	65	77	67	74	66	8	70	32	55	33	55
May	78	70	79	70	78	71	7	75	31	46	31	50
June	83	74	83	72	84	74	10	79	31	42	28	44
July	86	76	86	79	85	77	8	81	29	42	27½	39
Auguſt	81	74	87	75	83	75	8	79	31	48	29	42
September	78	72	76	69	75	70	5	73	31	49	32	50
Oċtober	63	56	66	58	66	58	8	62	35	58	38	62½
November	56	50	53	46	56	50	6	53	42	67	42½	64
December	48	41	57	48	54	48	6	51	37	66	41	72
The Means	68	60	69	61	69	62	7	65½				

A TABLE

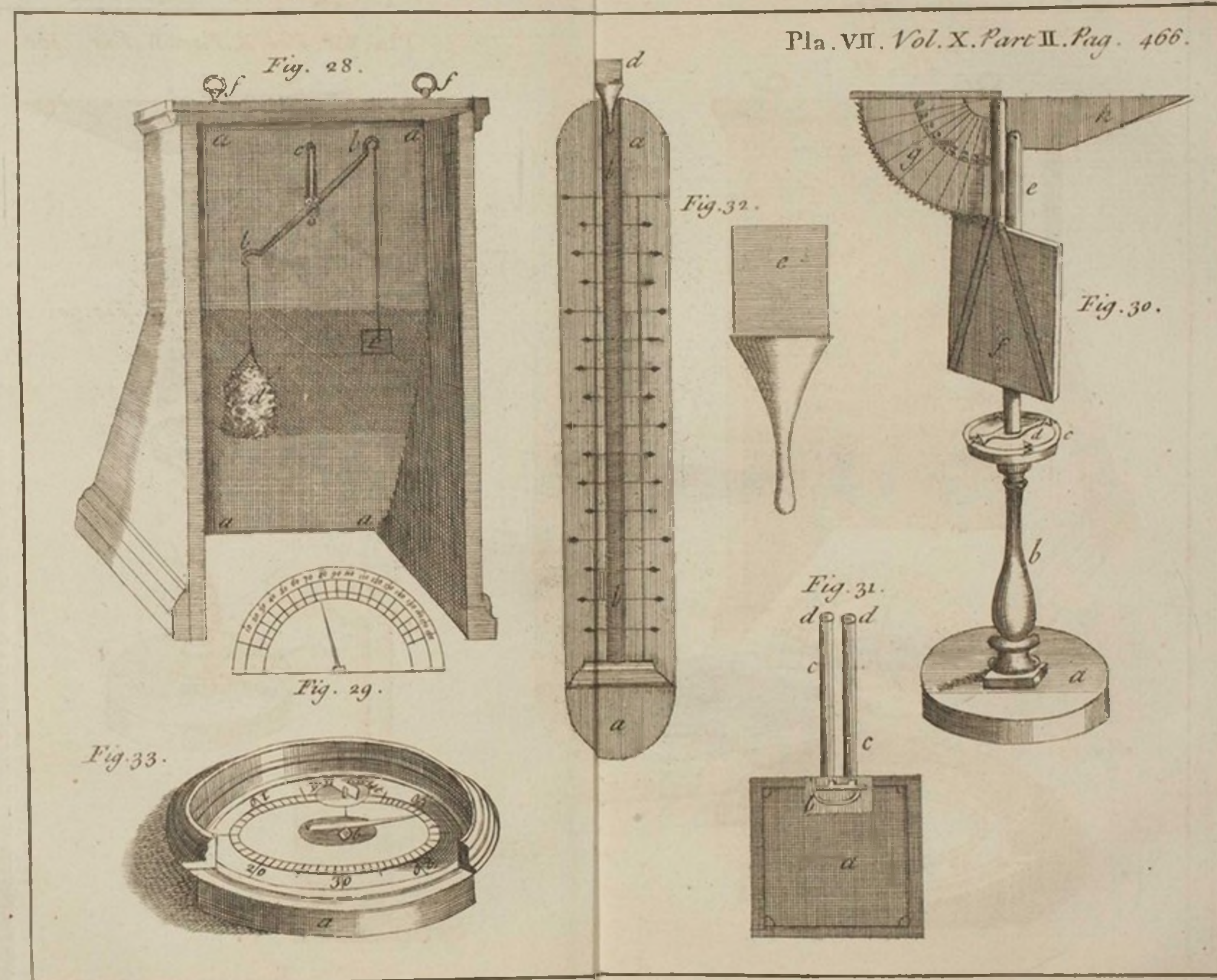


Fig. 28.
Fig. 29.
Fig. 30.
Fig. 31.
Fig. 32.
Fig. 33.

A TABLE *of the highest and lowest Barometrical Stations; with the Directions which the Wind then had.*

× A Northerly or Easterly Wind } preceded or succeeded
♪ A Southerly or Westerly Wind }

Jan.	—	—	—	—	—	30.48	N	29.88	SW	.60
Feb.	—	—	—	—	—	30.38	NE	29.68	S	.70
March	—	—	—	—	—	30.26	SE	29.58	S	.68
April	30.42	E	29.48	W	.94	30.38	W×	29.78	WSW	.60
May	30.23	NE	29.85	S	.38	30.35	E	29.80	W	.55
June	30.20	NE	29.85	W	.35	30.30	E	29.98	SW	.32
July	30.13	SSW×	29.83	SW	.30	30.38	E	30.00	SW	.38
Aug.	30.18	E	29.88	SW	.30	30.38	NE	29.98	SW	.40
Sept.	30.33	NNE	29.85	SE	.48	30.38	E	29.88	NW	.50
Oct.	30.33	E	29.83	WNW	.50	30.45	E	29.68	W	.77
Nov.	30.58	N	29.72	S	.86	30.35	NE	29.58	W	.77
Dec.	30.60	N	29.93	W	.67	30.58	N	29.75	NNW♪	.83

Jan.	30.70	N	29.50	NW♪	1.20	30.46	NNE	29.76	W	.70
Feb.	30.55	N	29.85	W	.70	30.54	NNE	29.72	WSW	.82
March	30.50	SE	29.65	W	.85	30.40	ENE	29.60	W	.80
April	30.32	E	29.75	N♪	.57	30.48	E	29.58	W	.90
May	30.28	E	29.85	S	.43	30.30	S×	29.90	SSW	.40
June	30.18	S×	29.86	S	.32	30.28	ESE	29.90	NE♪	.38
July	30.08	SSE×	29.85	SSW	.23	30.22	W	29.98	SW	.24
Aug.	30.26	E	29.85	W	.41	30.25	NE	29.95	N	.30
Sept.	30.28	NE	29.85	NE♪	.43	30.36	NE	29.86	S	.50
Oct.	30.32	NNE	29.72	SW	.60	30.50	N	29.95	W	.55
Nov.	30.51	N	29.72	S	.79	30.55	NNW	29.73	SW	.82
Dec.	30.60	ENE	29.86	SW	.74	30.58	NNE	29.65	WNW	93

A TABLE

A TABLE *of the Depth of Rain, in Inches and millesimal Parts, which fell in* Charles-Town.

	1738	1739	1740	1741	1742
January	1 097	2 310	4 873	4 492	2 189
February	4 416	2 875	3 084	4 615	1 650
March	4 532	5 609	1 141	5 713	5 203
April	1 082	0 195	1 092	1 308	0 918
May	3 127	5 120	5 612	4 841	5 898
June	1 567	15 839	4 648	5 538	3 250
July	10 660	5 452	3 013	3 399	1 252
August	4 104	12 211	7 301	7 144	7 647
September	10 792	4 834	3 200	6 734	2 895
October	1 358	6 593	1 258	3 399	0 759
November	2 656	1 235	1 848	2 964	3 388
December	3 877	3 689	2 736	1 919	0 957
Total Depth	49.268	65.962	39.806	52.066	36.006

	1743	1744	1745	The Means	1746
January	3 172	1 994	0 863	2 624	1 144
February	2 435	3 063	7 739	3 735	2 701
March	0 621	0 582	3 229	3 329	1 628
April	5 292	2 866	3 842	2 074	1 128
May	2 535	2 871	1 832	3 979	3 988
June	1 903	5 814	9 510	6 009	4 109
July	7 738	8 437	6 771	5 840	9 895
August	3 767	4 202	9 339	6 964	6 114
September	4 686	5 657	0 754	4 944	0 932
October	1 672	1 595	2 962	2 450	
November	3 220	1 562	0 682	2 194	
December	2 706	9 680	2 623	3 523	
Total Depth	39.747	48.323	50.146	47.666	

A Letter from the Rev. Hen. Miles, *D. D. F. R. S. to Mr* Hen. Baker, *F. R. S. concerning a very* cold day, *and another a very* hot day, *in* June *and* July 1749. *and of the near* agreement *of* Thermometers *in* London *and at* Tooting. N°. 493. p. 208. Oct. &c. 1749. *Read* Nov. 16. 1749.

X I. *June* 10. ſuſpecting a froſt that night, I ſat a *China* ſaucer full of water upon the graſs-plot, in the garden; and the next morning, a little before ſun-riſing, I found the water frozen over, of ſuch a conſiſtence, as that I forced a hole through the centre of it with my finger, without breaking it elſewhere, and carried the cake of ice into the houſe, where it remained a good while not diſſolved. Wind N. W. On ſome following days there were ſeveral conſiderable froſts, the wind continuing the ſame way; the fatal effects of which are ſufficiently known throughout the kingdom.

July 2, at 12^h 20′ my Thermometer of *Farenheit*'s ſcale, in the ſhaded air, ſtood at 88, and at 2^h *p. m.* at 87. At which laſt number two others of the ſame ſort ſtood exactly, at that hour, in *London.*

Having agreed with Mr *John Canton* of *Spital-Square*, to make obſervations of the temperature of the air here, and in *London* at a ſtated hour: we procured Thermometers, made exactly alike, by that accurate workman Mr *Bird*; and having found, by hanging them firſt together a ſufficient time, that they perfectly agreed, we began our obſervations in *April*, and have continued them ever ſince.

The Thermometers are of the ſmaller ſize, the bulbs being but about $\frac{47}{100}$ of an inch diameter, and are immediately affected with any mutations of the air; ſo that I have frequently been entertained with obſerving, in ſome circumſtances of the weather, that the mercury has not been ſtationary, but has ſucceſſively riſen and fallen for a good while; and Mr *Canton* has informed me, that he has ſeveral times obſerved the ſame.

I have annexed a paper, containing an extract from my journal of the weather, in which I have ſet down the extremes of the Barometer and Thermometer, obſerved at 2^h *p. m.* for ſix months; and Mr *Canton* has been ſo kind as to communicate a like extract from his journal; which is likewiſe put into your hands.

It appears by a more general compariſon which we have made, as well as by this particular one, that the difference in the temperature of the air, as to heat and cold, is very little between this place and *Spital-Square.* Sometimes my Thermometer has been higher than his; more times upon an equality, but moſt times lower. And I have reaſon to think the difference, many times, may have been owing to accidental cauſes: for Mr *Canton* has informed me, that he has found 2 Thermometers, when removed but a few yards from each other, have differed 2 or 3 deg. for which no apparent cauſe could be aſſigned. So that upon the whole, it may reaſonably enough be concluded, that the difference between the temperature of the air in the two places, is imperceptible to ſenſe.

P. S. Upon my having obſerved that the days, in which my Thermometer and Mr *Canton*'s ſtood at the extremes, in ſome months did not coincide, I was deſirous of knowing, how much the Thermometers differed, when the extremes did not happen on the ſame day: but,

but, upon a compariſon, the difference was never conſiderable, except on *Aug.* 4. at 2 *p. m.* when my Thermometer was about 6° higher than Mr *Canton*'s. This being ſomewhat remarkable, he, upon hearing it, had recourſe to his regiſter, and found, that at the time of obſervation a heavy ſhower of rain fell; whereas we had none here: but about 6 *p. m.* came on a thunder ſhower attended with rain; ſo that it ſhould ſeem the falling rain had ſo great an effect upon the air as to render it cooler, by the degrees mentioned: and perhaps the difference between the two inſtruments, at other times, may have been owing to the ſame, or a ſimilar cauſe, rather than to a *ſtated* different temperature of the air, in the two places.

EXTRACT *from a Journal of the Weather, made at* Tooting, *in* Surry, *in which the Extremes of the Barometer and Thermometer are noted at* 2 p. m. *equal Time, for* May, June, July, Aug. Sept. *and* Oct. *this preſent Year* 1749.

MAY. 1749.			
Day	Bar.	Day	Ther.
14	higheſt 30,04	17	higheſt 76
Day	Bar.	Day	Ther.
24	loweſt 29,15	3	loweſt 50

AUGUST. 1749.			
Day	Bar.	Day	Ther.
15	higheſt 30,02	22	higheſt †
Day	Bar.	Day	Ther.
2	loweſt 29,20	31	loweſt 62½

JUNE. 1749.			
Day	Bar.	Day	Ther.
8	higheſt 30,10	28	higheſt 79
Day	Bar.	Day	Ther.
17	loweſt 29,25	3	loweſt 46

SEPTEMBER. 1749.			
Day	Bar.	Day	Ther.
26	higheſt 30,37	5	higheſt 71
Day	Bar.	Day	Ther.
17	loweſt 29,3	12	loweſt 53

JULY. 1749.			
Day	Bar.	Day	Ther.
8	higheſt 29,95	2	higheſt 87 *
Day	Bar.	Day	Ther.
20 22	loweſt 29.44	30	loweſt 62½

OCTOBER. 1749.			
Day	Bar.	Day	Ther.
10	higheſt 30,44	4 7	higheſt 61½
Day	Bar.	Day	Ther.
28	loweſt 29,51	27	loweſt 43

† Being

† Being abſent *Aug.* 22d, at 2 *p. m.* I could not obſerve the ſtate of the Thermometer; but am well ſatisfied that day was the hotteſt in the month, from the obſervation I made at other times of the day, and particularly from the account I had from Mr *Canton*, of the ſtate of his, which ſtood at 80½ at 2 *p. m.*

It may be proper to obſerve, that the Barometer made uſe of ſtands $\frac{2}{10}$, or more, lower than others of the ſame conſtruction (which is the common upright make) during the warmer ſeaſon of the year, and uſually as much higher than they do in the colder; but is made uſe of (as it has been for more than 10 years) becauſe I have always found it to riſe and fall ſooner than any other I have compared it with, and in particular than a very good one, made by the late Mr *Siſſon*, which has always hung by it, and is conſtantly compared therewith.

EXTRACT *from a Journal of Obſervations on the* Barometer *and* Thermometer, *made in* Spital-Square, London, *in which the Extremes of each are noted, at* 2ʰ p. m. *equal Time, for* May, June, July, Aug. Sept. *and* Oct. *this preſent Year* 1749.

MAY. 1749.			
Day	Bar.	Day	Ther.
13	higheſt 33,23	13	higheſt 76½
Day	Bar.	Day	Ther.
25	loweſt 29,33	4	loweſt 59

JUNE. 1749.			
Day	Bar.	Day	Ther.
27	higheſt 30,32	28	higheſt 80
Day	Bar.	Day	Ther.
17	loweſt 29,30	3	loweſt 48¼

JULY. 1749.			
Day	Bar.	Day	Ther.
8	higheſt 30,35	2	higheſt 87
Day	Bar.	Day	Ther.
24	loweſt 22.66	18.30 31	loweſt 64½

AUGUST. 1749.			
Day	Bar.	Day	Ther.
15	higheſt 30,25	22	higheſt 80½
Day	Bar.	Day	Ther.
2	loweſt 29,41	4	loweſt 59½

SEPTEMBER. 1749.			
Day	Bar.	Day	Ther.
26	higheſt 30,44	5	higheſt 70½
Day	Bar.	Day	Ther.
17	loweſt 29,37	2	loweſt 54½

OCTOBER. 1749.			
Day	Bar.	Day	Ther.
10	higheſt 30,49	7	higheſt 61
Day	Bar.	Day	Ther.
28	loweſt 29,43	27	loweſt 43

Two Letters from the Rev. Hen. Miles, *D. D. F. R. S. to Mr* Hen. Baker, *F. R. S. concerning the* Heat *of the* Weather *in* July *and* Sept. *laſt.* N° 496. p. 571. Nov. &c. 1750. *Read* Nov. 22. 1750.

Firſt Letter.

XII. *July* 11. The morning at 4 had nothing remarkable: at 2 *p. m.* the heavens moſtly clear, and no indications of a ſtorm; the Barometer having fallen but $\frac{6}{100}$ inch ſince 4 *a. m.* it then ſtood at 30,20, the Thermometer at $87\frac{1}{2}$, and before 3 *p. m.* at $88\frac{1}{2}$, which is the hotteſt temperature of the air I ever knew.

At 4 *p. m.* we had very diſtant thunder; ſoon after it came a little nearer, and was one continued murmur, without any perceivable intermiſſion for great part of an hour: the lightning accompanying it, not much. The wind was nearly S. W. and dark clouds paſſed by on each ſide of us till they united in the N. forming one of the blackeſt clouds I ever ſaw, over the city, as near as I could gueſs. We had not one drop of rain, nor did there fall either rain or hail for near 3 miles to the N. of us towards *London:* a few hail-ſtones, I am informed, fell in ſome parts of *Clapham*; what the extent of the ſtorm might be on other ſides of the city, I have not heard.

By the obſervation I made, there did not appear any conſiderable change in the ſtate of the air, as to the weight or heat of it. The Barometer fell little, and the Thermometer no more than uſual at that time of the evening. Mr *Canton* writes me, that his Thermometer in *Spital-Square* (of the ſame conſtruction with mine, and kept too in the open air) fell no leſs than 17 degrees.

Second Letter.

At 4 *a. m.* the wind being eaſterly, and blowing ſtrong, accompanied with ſeveral ſhort ſhowers of rain, the Barometer being at 29,97, I obſerved my Thermometer abroad to ſtand at 61: a degree of heat exceeding any I have taken notice of during the whole ſummer at that time of the morning.

Tooting, Sept. 2, 1750. *H. Miles.*

Extract of a Letter from Mr William Arderon, *F. RS. to Mr* H. Baker, *F. R. S. concerning the* hot Weather *in* July *laſt.* Ibid. p. 573. *Read* Nov. 22. 1750.

XIII. The beginning of this heat was on the 8th of *July*; on which (though the whole day was cloudy) the ground was ſo uncommonly hot, that I could not bear to walk on it long together without much uneaſineſs; and many were ſenſible of the ſame inconvenience as well as myſelf.

On *Wedneſday* the 11th, which was the hotteſt day of all, my Thermometer in the ſun's rays ſtood 11° above the heat of human blood; and in the ſhade, in my houſe, only 8° below it. The diſtance between freezing and the heat of human blood being divided into 100 parts.

An inch of tallow, $\frac{6}{10}$ of an inch in diameter, liquefied in the ſun in leſs than 30′. A piece of reſin, $\frac{5}{10}$ of an inch in diameter, became ſo ſoft as to be liable to take any impreſſion in the ſame time.

But, that you may form a better judgment of the heat at *Norwich*, on the ſaid 11th of *July*, and for 3 days before and after, you will ſee below how *Haukſbee*'s Thermometer ſtood at different times in each of thoſe days.

July

July 8	Morning	7	33
	Evening	10	25
July 9	Morning	7	26
	Evening	2	15
	Evening	10	18
July 10	Morning	7	24
	Evening	2	11
	Evening	10	16
July 11	Morning	7	19 *Reaum. Fahr.*
	Evening	2	$8\frac{1}{2} = 1026 = 83$
	Evening	10	16
July 12	Morning	7	18
	Evening	2	12
	Evening	10	16
July 13	Morning	7	18
	Evening	2	12
	Evening	10	16
July 14	Morning	7	32
	Evening	2	13
	Evening	10	29

I obſerve 3 in the afternoon, when the ſky is clear, is the hotteſt part of the day; but clouds moſtly came on about that time on theſe days.

Many people here, who judged by their outward ſenſes only, without paying any regard to Thermometers, have thought the 11th of *June* 1748 was hotter: but I imagine the reaſon to be, that the heat this year came on gradually from day to day; whereas in the year 1748 it was much more ſudden; the Thermometer then riſing 22° more in one day than the preceding; which, conſequently, would make the difference between one day and another appear the more extraordinary. But, by my obſervations on the 11th of *June* 1748, *Haukſbee*'s Thermometer ſtood at $14\frac{1}{2}$; full 6° cooler than on the 11th of this preſent *July*.

P. S. Several horſes have dropped down dead under their maſters, overcome by this violent heat.

XIV. The preceding day had been remarkably hot, and in the afterternoon very cloudy, with the uſual indications of an approaching ſtorm, in the evening. At 9 at night, the wind ſoutherly, my Barometer ſtood thus; one, which is the moſt ſenſible, at 29 inches $\frac{7}{10}\,\frac{1}{100}$; the

A Letter from the Rev. H. Miles, *D. D. F. R. S. to the* Preſ. *concerning the* Storm *of* Thunder, *which happened* June 12. 1748. *at* Streatham *in* Surry, N°. 488. p. 383. June 1748. *Read* June 23. 1748.

other at 30. $\frac{2}{10}$ 0. The Thermometers (of *Sisson*'s construction) one without-doors, at 43°, another within, at 49°, above 0. or the freezing point.

At 1 next morning, a person apprehensive of the thunder, upon looking out at window, was surprized to find an unusual clear sky, every-where equal to what is observed in frosty weather, or after a high wind, except that in a few places some thunder-clouds shewed themselves just above the horizon. At 2 we heard thunder at a distance: at 3^{h} $30'$, when I got up, I perceived the storm approaching apace from the S. where the wind then was, but the darker clouds seemed to bear off chiefly to the E. and W. of us, so that I did not think we should hear of any mischief near us. At 4 we had a smart shower of rain, and about 5 two loud claps of thunder over our heads, but pretty high; the lightning was very pale, and the flashes large, descending in a spiral form, almost perpendicular to the horizon to the eastward of us, which is the situation of *Stretham*, and at about 2 miles distant from us. At a little before my Barometers stood thus, 29. $\frac{7}{10}$ $\frac{3}{100}$, and 30. $\frac{2}{10}$ $\frac{2}{100}$; and continued successively rising and falling during the storm, but very inconsiderably.

Upon hearing 2 houses were damaged, situate at the foot of the hill on which the mineral wells are, fronting the east, by the wood-side, I went next day to view them. The house to the S. which is a publick house kept by Mr *Howard*, seemed to have received the greatest shock. Some of the family being up, the front door stood partly open, when the storm began: the upper half was of glass, framed like a sash-window, having two sliding shutters, one on each side, which had not been taken down. The glass between them was shattered to pieces, but the shutters no-ways touched, except that a nail in one of them was forced in a little way. To the door-post, on the left hand, hung by an iron pin an iron bar, which served to fasten the door at night: this pin was driven out of the post, and the bar considerably bent, and in divers places melted in small spots, as were the hinges of the door, chiefly upon the edges in both, and the door-post split. A sheet of lead on the pediment, or shelter over the said door, was raised, and partly rolled up at one corner; the cornice underneath being torn off without being split, a good part of the tiling near the eaves and over the pediment was loosened, and some tiles beat off, and the lathing and some of the moldings of the windows had taken fire.

In a bed-chamber fronting the road, on the second floor where Mr *Howard* lay, 3 boards of the lining of the room, on the E. side, were driven inwards 5 or 6 inches at one end; but at the other the nails were a little loosened only. In a garret over this bed-chamber, the upper part of a bed-post was shivered; and nearly over where this bed stood, a large hole was broke in the roof, on the W. side, just by where one of the chimneys goes up; the chimneys having all additional funnels of brick-work on the top, of a roundish form, and plastered: these were

struck

ſtruck, and inclined to the N. eſpecially that which was on the ſouth end of the houſe, the plaſter being beat off, and ſome of the bricks broke down. There were about 13 perſons in this houſe, none of which received any hurt; though a lad, who was in the kitchen, into which the door opened, before-mentioned, and the window of which (near where he was ſtanding) had ſeveral panes of glaſs broke, muſt certainly be much expoſed. He informed me, among other things, that the fire flew about him in ſparks, like thoſe which fly out of burning charcoal, but larger, and ſnapping as they do. Some pieces of glaſs were ſhewed me, which I found to have been melted, one of which I take the liberty of laying before you.

The adjoining houſe, inhabited by Mr *Figgins*, had the plaſtering beat off in the front in patches, and one of the chimneys cracked for a great length In the kitchen window frame, one of the croſs pieces, near the middle of the window, had a chip ſtruck off from it about 5 inches in length, and at one end about $\frac{1}{5}$ of an inch thick, but thin at the other, and near the width of the frame, but none of the glaſs broke, nor the lead bent, though in a manner contiguous with the ſplinter beat off. The ſame thing happened to a parlour-window, on the other end of the houſe; both the ſhivers were found directly oppoſite to the windows, at ten or twelve yards diſtant in the road.

In a ſmall garret (which is next to Mr *Howard*'s houſe) where two maid-ſervants lay, the plaſter was broken, to appearance, inwards, on oppoſite ſides of the room, and near the feet of the bed, which ſtood on each ſide about $\frac{3}{4}$ of a yard from the wall. The breach on the E. ſide, near a window (ſome panes of the glaſs of which were broken) was oppoſite to the vailings of the bed, which were ſinged, and a hole burnt through them big enough to receive the end of one's fore finger. On the oppoſite ſide, juſt by the chimney, another breach was made, of the ſame height, in the wall, which was continued downwards for about a yard, but the curtains not at all ſinged. Directly againſt this breach, one of the maids (who had got up) ſat on the bed's ſide, who was inſtantly ſtruck down, but received no hurt: Upon enquiring of her, whether ſhe ſeemed to receive a blow on any particular part of her body? ſhe replied, ſhe was ſtruck all over alike.

But the moſt remarkable, though the leaſt terrible effect, appeared on the frame of a pannel of wainſcot, about 5 feet long, and about $1\frac{1}{2}$ wide, in the parlour fronting the E: on this pannel a landſcape is painted, and the moulding belonging to it had been gilt, but on the laſt painting the room, the gilding was covered with the ſame paint: that which covered the gilt moulding was ſtripped off in irregular ragged ſtreaks throughout, ſo that the gilding appeared as freſh as it may be thought to have looked when it was painted at firſt: and as the gilding does not ſeem to have been affected, ſo neither does the paint appear to have been cracked any-where, but where the gilding lay under.

If

If it be ſuppoſed, that the lead in the paint was melted by the lightning, it will be difficult to account for it, that it ſhould *not at all affect* the paint contiguous with that which was upon the gilding; though we ſuppoſe a reſiſtance to have been made by the leaf-gold, and to have contributed to the producing the mentioned effects.

Of the burning of the ſteeple of Danbury *in* Eſſex, *by* Lightning; *by* Smart Lethieullier, *Eſq*; *F. R. S. to the* Preſident. N°. 497. p. 611. *Read* March 1. 1749-50.

XV. As I think there was ſome miſtake in the account of the late burning of *Danbury* ſteeple by lightning (read laſt *Thurſday* before the *Royal Society*) which miſtake ſeemed to give room to imagine it had ſome connexion with the late earthquake, I take the liberty of ſending you the particulars of the aforeſaid accident, as they were tranſmitted to me by two gentlemen of diſtinction in the pariſh.

Monday, the 5th of this inſtant *Feb.* about 3 in the afternoon, a very great and black cloud paſſed over the hill on which *Danbury* ſtands, and broke into a violent ſtorm of thunder, lightning, and rain. No immediate danger was apprehended that night; but, between 4 and 5 next morning, ſome perſons perceived the ball on the top of the ſpire (which was of wood, and on which the weather-cock ſtood) to be on fire, they immediately alarmed the neighbourhood, and, by the help of a large fire-engine, fetched from *Chelmsford*, they got the maſtery of the fire by about 11 at noon; tho' it broke out twice afterwards. It burnt downwards with great fury, and has entirely conſumed 11 feet of the ſpire, and damaged 8 feet more, as likewiſe the beams on which the ſpire was fixed.——Some of the burnt timbers and melted lead have hurt the roof of the chancel, but not very conſiderably.

The great height of the ſituation of this church probably expoſes it to accidents of this nature; for *Walſingham* relates, "that, on *Corpus* "*Chriſti* day anno 1402. the devil entered this church in the likeneſs "of a Friar-Minor, where he raged, to the great terror of the pariſhi- "oners, and at the ſame time, by the violence of thunder and a tem- "peſt, the whole body of the church was broken."

This devil, or friar, was, I conclude, no more than a flaſh of ſome fiery meteor, which the fruitful fancy of thoſe times could immediately cloath with the firſt habit that occurred to them.

The appearance of a fiery Meteor, *as ſeen by Mr* Cradock, *communicated to the* R. S. *by Mr* H. Baker, *F. R. S.* N°. 473. p. 78. May, &c. 1744. *Read* June 7. 1744. Fig. 34.

XVI. The head and body emitted an extremely lucid and white flame. The tail appeared of a tranſparent blue, like the flame of ſulphur.

This *phænomenon* was ſeen on *Sunday*, *May* 27. 1744. at 11^h 11′ *p. m.* It's direction from *S. E.* to *N. W.* or thereabouts; it's height ſeemingly not ¼ a mile.

It was ſeen, as here deſcribed, from the terrace in *Somerſet-Gardens*, by me,

Zach. Cradock,
Of *Somerſet-Houſe.*

XVII.

XVII. As I was coming from my Living, juſt before I reached a place called *Stanlake Broad*, and a little before 8 in the evening, I was on a ſudden ſurprized to ſee a long ſtream of fire, of a colour reſembling molten glaſs, *Fig.* 35. which ſhot down from *A* to *B*, in length, I gueſs, about 20°, and ſeemed immediately to run up again from *B* to *A*; where it turned to a ſort of ſmoke, or rather to a fine lambent flame like that of an *Aurora Borealis*; which continued for ſome time in a ſort of oblong ſhape, *Fig.* 36. but afterwards by degrees, changed into *Fig.* 37. and at laſt into *Fig.* 38. under which, parallel to the horizon, it grew fainter and fainter, till it intirely vaniſhed about 9.

Part of a Letter from the Rev. Mr Geo. Coſtard *to Mr* John Catlin, *concerning a fiery* Meteor *ſeen in the air on* July 14. 1745. N°. 477. p. 522. Aug. &c. 1745. *dated* Aug. 2. 1745. *Read* Nov. 7. 1745.

There was a fine gentle breeze all this time; but I could not obſerve that it affected the *phænomenon* ſo far as to make it change it's place, which was to the eaſtward of the N. Perhaps this change of figure might, in ſome meaſure, be owing to it. Being on horſeback, I ſaw it from the beginning to the end; but having no watch with me, I only gueſſed, by my riding, that it continued about an hour. I heard afterwards, from ſome who had their watches, that it laſted an hour and one minute.

Fig. 35. Fig. 36. Fig. 37. Fig 38.

When I came home, I put down what I had obſerved; it being, as I thought, a very ſingular appearance *.

XVIII. *Sept.* 15. 1749. a remarkable meteor was ſeen in *Rutland*, which I ſuſpect to have been of the ſame kind as ſpouts at ſea.

An extraordinary Meteor *ſeen in the County of* Rutland, *which reſembled a* Water-Spout, *communicated to the* Preſ. *by* Tho. Barker, *Eſq*; N°. 593. p. 248. Oct. &c. 1749. *Read* Dec. 14. 1749.

It was a calm, warm, and cloudy day, with ſome gleams and ſhowers; the Barometer low and falling, and the wind S. and ſmall. The ſpout came between 5 and 6 in the evening; at 8 came a thunder-ſhower, and ſtorm of wind, which did miſchief in ſome places; and then it cleared up with a briſk N. W. wind.

The earlieſt account I have was from *Seaton*. A great ſmoke roſe over or near *Gretton*, in *Northamptonſhire*, with the likeneſs of fire, either one ſingle flaſh, as the Miller ſaid, or ſeveral bright arrows darting

* As I was returning home from the *Royal Society* to *Weſtminſter*, on *Thurſday*, *Dec.* 16. 1742. 8h. 40′. *p. m.* being about the middle of the parade in *St James's Park*, I ſaw a light ariſe from behind the trees and houſes in the S. by W. point, which I took at firſt for a large ſky-rocket; but when it had riſen to the height about 20°. it took a motion nearly parallel to the horizon, but waved as in *Fig.* 39. and went on to the N. by E. point over the houſes. It ſeemed to be ſo very near, that I thought it paſſed over *Queen's-Square*, the iſland in the park, croſs the canal, and I loſt ſight of it over the *Hay-market*. It's motion was ſo very ſlow, that I had it above ½ a minute in view; and therefore had time enough to contemplate it's appearance fully. *A* ſeemed to be a light flame, turning backwards from the reſiſtance the air made to it. *B B* a bright fire like burning charcoal, incloſed as it were in an open caſe, of which the frame *C C C* was quite opaque, like bands of iron. At *D* iſſued forth a train or tail of light flame, more bright at *D*, and growing gradually fainter at *E*, ſo as to be tranſparent more than half it's length. The head ſeemed about ½° in diameter, the tail near 3° in length, and about ¼ of a degree in thickneſs.

Fig. 39. Fig. 40.

C. M.

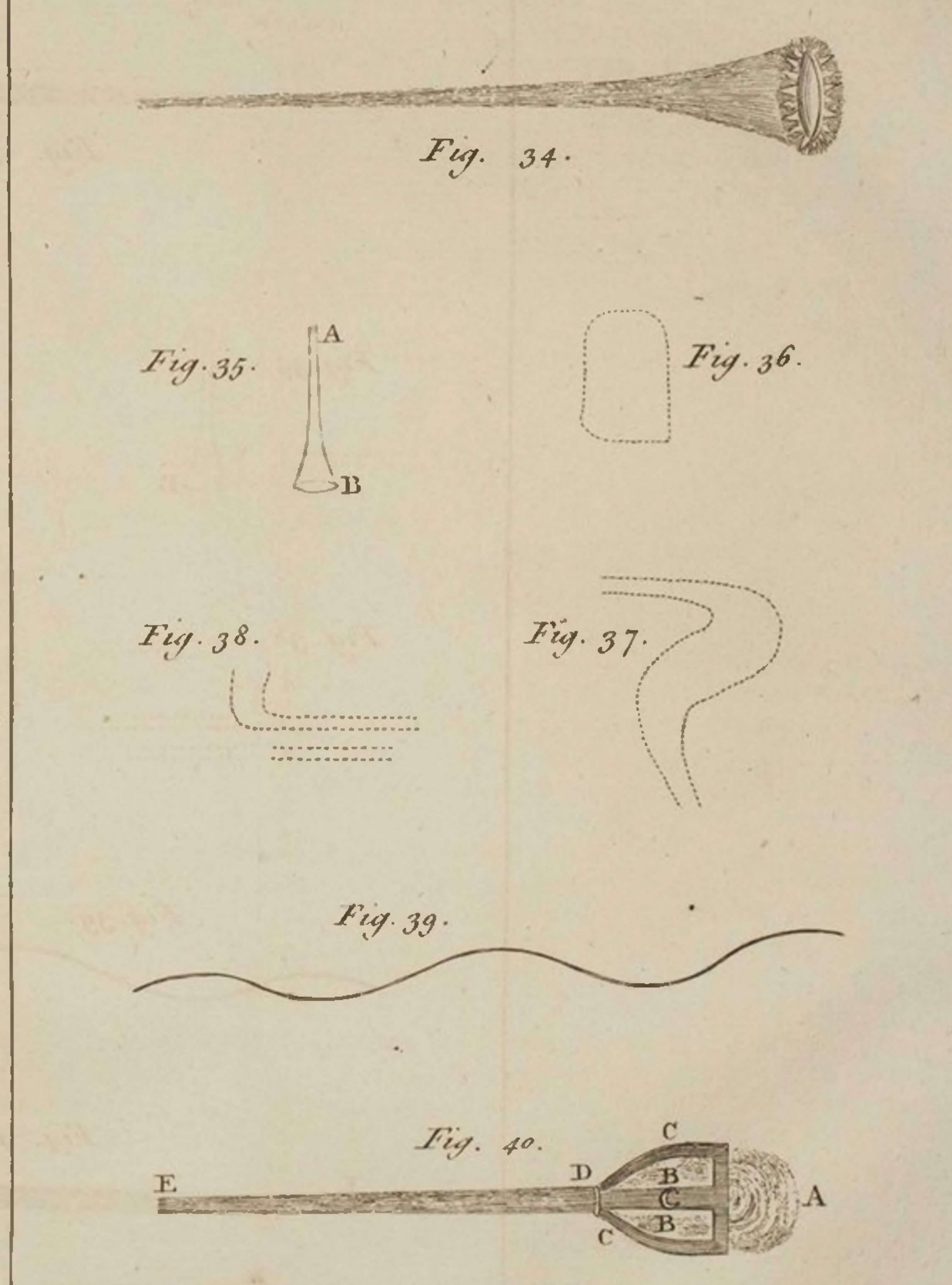
down

down to the ground, and repeated for some time, as others say. Yet some who saw it, did not think there was really any fire in it, but that the bright breaks in a black cloud looked like it. However, the whirling, breaks, roar, and smoke, frightened both man and beast. Coming down the hill, it took up water from the river *Welland*, and passing over *Seaton* field, carried away several shocks of stubble; and crossing *Glaiston*, and *Morcot* lordships, at *Pilton* town's end, tore off two branches, and carried one of them a good way. In a hedge-row in the meadow, at right angles to the spout's course, stood an oak and an ash 15 yards asunder; the oak a young sound one, 16 inches thick, it split two yards down, and one half fell to the ground, but was not quite parted from the other; the ash, about 8 inches thick, was torn off in the middle, and carried 10 or 12 yards. Between and on each side of these trees were other smaller ones, which were not hurt: I heard of no harm it did after, but breaking and scattering a few boughs. I saw it pass from *Pilton* over *Lyndon* lordship, like a black smoky cloud, with bright breaks; an odd whirling motion, and a roaring noise, like a distant wind, or a great flock of sheep galloping along on hard ground; it was divided into two parts all the way it went, and though there was no wind, moved apace from S. by W. to N. by E. As it went by a quarter of a mile E. from me, I saw some straws fall from it, and a part, like an inverted cone of rain, reached down to the ground. Some who were milking, said it came all round them like a thick mist, whirling and parting, and, when that was past, a strong wind for a very little while, though it was calm both before and after. It then passed off between *Edithweston* and *Hambleton*, but how much further I do not know.

An account of an extraordinary Fire-ball bursting at Sea, communicated by Mr Chalmers. N°. 494. p. 366. Jan. &c. 1750. *Read* March 22. 1749.

XIX. *Nov.* 4. 1749. in lat. 42° 48′, long. 09° 03′, the Lizard then bore, N. 41° 05′, about the distance of 569 miles. I was taking an observation on the quarter-deck, about 10′ before 12: one of the quarter-masters desired I would look to windward, which I did, and observed a large ball of blue fire rolling on the surface of the water, at about 3 miles distance from us: we immediately lowered our topsails, and had our fore and main clew garnets manned to haul up our courses; but it came down upon us so fast, that before we could raise the main tack, we observed the ball to rise almost perpendicular, and not above 40 or 50 yards from the main chains: it went off with an explosion as if hundreds of cannon had been fired at one time; and left so great a smell of brimstone, that the ship seemed to be nothing but sulphur. After the noise was over, which I believe did not last longer than half a second; we looked over head, and found our maintopmast shattered into above an hundred pieces, and the mainmast rent quite down to the heel. There were some of the spikes, that nail the fish of the mainmast, drawn with such force out of the mast, that they stuck in the main deck so fast, that the carpenter was obliged to take an iron crow to get them out:

Pla. VIII. Vol. X. Part II. Pag. 480.



out: there were five men knocked down, and one of them greatly burnt, by the explosion. We believe, that when the ball, which appeared to us to be of the bigness of a large millstone, rose, it took the middle of the main topmast, as the head of the mast above the hounds was not splintered: we had a very hard gale of wind, from the N. by W. to the N. N. E. for two days before the accident, with a great deal of rain and hail, and a large sea: from the northward we had no thunder nor lightning, before nor after the explosion. The ball came down from the N. E. and went to the S. W.

This account was given by Mr *Chalmers*, who was, when the abovementioned accident happened, on board his Majesty's ship the *Montague*, under the command of admiral *Chambers*.

XX. On the 8th of *March* 1746-7, near 8 in the morning, as I was riding within 3 miles of *Brentwood* in *Essex*, there appeared a singular *phænomenon* in the Heavens; the sketch may give some idea of it.

An observation of an uncommon Gleam *of* Light *proceeding from the* Sun, *by Mr* Peter Collinson, *F. R. S.* N°. 483. p. 456. Mar. &c. 1747. *Read* March 19. 1746-7. Fig. 41.

The morning was fine and clear, the sun shone bright, no cloud to be seen, but the air a little hazy: where the *phænomenon* appeared, which was a bright cloudy spot, seemed a very small portion of a rainbow, only the colours very faint. It was in a horizontal direction north of the sun, and from it projected a long luminous ray, which terminated in a point.—It continued very strong for more than half an hour after I saw it, and then vanished away by degrees.

XXI. *July* 11. 1749. The sun's rays shone through the clouds at the same time, as they frequently do when the sun is near the horizon. The drawing, which represents the whole, makes any farther description of it needless.

A Halo *or* mock Sun, *observed by* Mr W. Arderon, *F. R. S.* Fig. 42.

N°. 493. p. 203. Oct. &c. 1749. *Read* Nov. 2. 1749.

XXII. *Monday July* 18. 1748. about ¼ before 7 in the evening, the weather being temperate, and the wind about N. N. W. as I was walking in the fields, beyond *Islington*, I saw a distant rainbow which appeared to take in a large portion of the Heavens; but had nothing remarkable, and vanished by degrees.

A description of an extraordinary Rainbow, *observed* July 15. 1748. *by* Peter Daval, *Esq; Sec. R. S.* Ibid. p. 193. *Read* Oct. 26. 1749.

Continuing my walk, about 20′ after the disappearing of the first rainbow, a rainy cloud crossed me, moving gently with the wind, which exhibited to me a more perfect and distinct rainbow, than I had ever before seen; wherein I could plainly distinguish all the secondary orders of colours taken notice of by the late Dr *Langwith* in his letters to Dr *Jurin*; that is, to say, within the purple of the common rainbow, there were arches of the folowing colours.

1. Yellowish green, darker green, purple. 2. Green, purple. 3. Green purple.

This innermoſt arch Dr *Langwith* calls faint vaniſhing purple, and I likewiſe found, that it ſometimes appeared and diſappeared alternately; but during about 2′ it ſeemed to me to be as permanent as any of the other colours.

I ſtood ſtill, and looked attentively at this appearance, during the whole time of it's continuance, which was near 8′, and could for the greateſt part of that time diſcern all the above-mentioned colours, except the innermoſt purple in the upper parts of the bow; but could not diſtinguiſh any of them in thoſe parts of it which were near the horizon, tho' they were extremely vivid, as was likewiſe the outer bow, in which the colours appeared as bright, though not ſo well defined, as in moſt inner rainbows I had ſeen.

As I had read Dr *Langwith*'s letter a ſhort time before I ſaw this beautiful appearance, and as I compared his account with what I had ſeen, the ſame evening, and again the next morning, I can the better be aſſured of the exact agreement of our obſervations.

On my firſt ſeeing this phænomenon, I was ſurprized, that the diameter of the bow appeared to me very ſmall, compared with that I had ſeen a little before. The occaſion of this I think muſt have been, that the legs of the firſt-mentioned bow appeared to me to terminate at diſtant places: whereas in the latter appearance I could plainly ſee both ends of the inner and outer bows terminated in the neighbouring fields, at a very ſmall diſtance from each other: hence, and from my being involved in the ſhower which occaſioned this rainbow, I conclude it was very near me; which might be one cauſe of the great vividneſs of it's colours, and of my diſtinguiſhing the inner arches. But whether this was the only cauſe of thoſe appearances, or whether they might not be owing to ſome particular diſpoſition of the atmoſphere at that time, I much queſtion: as well becauſe I have often ſeen rainbows which have been very near me, and oppoſed to a bright ſun, wherein I could not diſcern theſe inner orders of colours, as that I have heard from ſome intelligent perſons, that ſome very bright rainbows were ſeen ſoon after the ſolar eclipſe which happened on the 14th of *July* 1748. particularly that an unuſually vivid and diſtinct rainbow was obſerved at *Twickenham* 3 or 4 days after that eclipſe, which agrees with the day on which I ſaw the above-mentioned appearance.

A luminous arch, by the Rev. W. Cowper *D.D. Dean of* Durham. N°. 497. p. 648. *Read* Feb. 16. 1749.

XXIII. A luminous arch appeared *Feb.* 16. 1749. about 9 at night. It had *Arcturus* in it's eaſtern limit, which was then low in the horizon, and extended to the bright ſtar in the right ſhoulder of *Orion,* then bearing S. W. It was exceedingly bright, regular, and well defined, and about the breadth of the rainbow; which it reſembled in every thing, but it's variety of colours. It continued thus for almoſt 20 minutes, declining gently ſouthward, and then gradually ſeparated and diſappeared.

XXIV.

XXIV. *Oct.* 20. at night, the ſky was darkened by a ſlight fog, thro' which the moon appeared of a fiery red colour, till 8^h. 40'. when the fog was thoroughly diſperſed, and the Heavens were overcaſt with a whitiſh ſtreaky cloud. At the ſame time there appeared round the moon a halo (*Fig.* 43. *A B C D*) accompanied with four other ſegments of circles, two of which *E A F* and *G H* of 10°, were concentric, ſo as to have their common center at the Zenith. The ſegment or arch *I P L* on the N. ſide, of 7°, was concentric with the great lunar circle, and conſequently had the moon for it's centre; and in fine the arch *M C N*, which faced the horizon, was of 12°.

An obſervation of an extraordinary lunar circle, and of two paraſelene's, made at Paris, Oct. 20. 1747. N. S. by Aug. Nath. Grefchow, Memb. of the R. Acad. of Sc. at Berlin, &c. N°. 489. p. 524. Oct. & Nov. 1748. *Tranſlated from the French by* T. S. *M. D.* *Read* Nov. 10. 1748.

Beſides theſe 4 ſegments, what was moſt remarkable was a mock-moon or *paraſelene B*, ſhaped like a mock-ſun or *parhelius*. The diameter of this mock-moon, tho' ill-determined, was of 35' at leaſt, with a tail *B P* oppoſite to the moon, as the tail of a comet is oppoſite to the ſun. This tail varied in it's degree of light from time to time, extending as far as the arch *I P L*, which, as well as the arch *G H*, was 4° diſtant from the lunar circle *A B C D*. The *paraſelene B* had the ſame colours with a common *parhelius*, excepting that they were not ſo lively, but they very much inclined to the tawny, eſpecially on the ſide, which faced the moon. This *paraſelene* was in the ſame altitude as the moon. It's tail was much more faint and tranſparent; inaſmuch as *Capella* appeared thro' this luminous tail. The lunar circle *A B C D* was much weaker to the ſouth, and there appeared no *paraſelene* on that ſide. This meteor did not ſeem to undergo any alteration till 9^h. 18'. when the atmoſphere was covered with thick clouds. Fig. 43.

The clouds being diminiſhed at 9^h. 32'. the meteor appeared again, but very different from what it was before; for, inſtead of ſeeing a lunar circle with 4 other arches of circles, I ſaw the lunar circle *D A B D* and on the ſouth ſide a faint arch *Q R* of 4°, having the moon for it's centre in common with the great lunar circle. There were likewiſe two *paraſelene's*, one of which *B* was to the N. and the other *D* to the S. as they are expreſſed in *Fig.* 44. Theſe two *paraſelene's* did not caſt ſo ſtrong a light as that which had appeared before, nor were they ſo diſtinctly formed. On the contrary, the lunar circle was very beautiful, and remarkably bright, until 9^h. 50'. when the whole *phænomenon* diſappeared, and the ſky grew clear by degrees. The moon's diameter was 30'. 30''. On the ſame night a very beautiful lunar circle was obſerved at *Berlin*, but without *paraſelene's*. Fig. 44.

XXV. 1. On *Friday* the 16th there was a bright *Aurora Borealis*, the northern part of the ſky being entirely filled with a pale light, in which frequent coruſcations were viſible. Beſides theſe lights, there was a perfect uniform arch, extending from E. to W. the colour of it was the ſame with that of the *Aurora*; with which however it did not ſeem to have any communication, being placed ſeveral degrees to the ſouthward. The ſhoulders of *Orion* were viſible thro' this luminous arch,

An Aurora Borealis *ſeen at* Chelſey, Feb. 16. 1749-50. *by* John Martyn, *F. R. S.* Prof. Bot. Cantab.

No. 494. p. 345. Jan. &c. 1750. *Read* Feb. 22. 1749-50.

in the weftern part of it, and *Cor Leonis* in the eaftern part. I did not happen to fee it till about a quarter before ten; and at ten it difappeared. The weather was then, and has been ever fince, exceedingly warm for the feafon. The walls are covered with bloffoms; and the hyacinths, daffodils, *&c.* are blown before the ufual time.

—— *at* Tooting, Jan. 23. 1750-1, *by the* Rev. Hen. Miles, *D. D. F. R. S.* ibid. p. 346. *Read* Feb. 22. 1749.

2. On *Tuefday Jan.* 23. laft, I was called out, about fix in the evening, to fee a ftrange appearance in the fky, in the weft. Sufpecting it to be an ordinary *Aurora*, I did not make great hafte—When I came out, I faw a cloud (not large) of an obfcure red colour, but much deeper than any I had ever feen before, which, I was informed, rofe from the S. W. it was then advancing apace to the N. E. and quickly reached the zenith, when, it being intercepted by the houfe, I haftened to the other front, which regards the N. E. by which time there appeared a luminous zone, about the breadth of the galaxy, it's edges regularly defined, compaffing the hemifphere, from the horizon in the N. E. to the zenith, in the fame direction, in which the above-mentioned cloud had paffed (as far as I faw it's courfe) from the S. W. The colour was much fainter, and more luminous, refembling the ufual colour of an *Aurora*, and the *laminæ* or ftreamers foon appeared—upon this, not being well fenced againft the wind, which blew brifk, I went in, to purfue my intention of viewing the 2 beautiful planets, *Jupiter* and *Venus*, with a reflecting telefcope, made by my ingenious friend Mr *Short* of *Surry-ftreet* (the greateft magnifying power of which is about 200 times); and after I had viewed them to my fatisfaction, and fhewed them to fome friends, when I was about to put up the inftrument, a cloud, of near the fize of the firft, but not fo deep a colour, appeared, rifing up from the S. W. which proceeded in a line with the planets, and, in a little time, furrounded both: *Venus* appearing ftill, to the naked eye, in her full luftre, I immediately viewed her with the telefcope, without altering the *focus*, and faw her much more diftinctly than ever I had done, on that evening, or on any other, and of the fame opinion were all my friends as to the fight they had of her, on that occafion: we all faw her fpots plain, refembling thofe in the moon; which I was never fo happy as to have a fight of before: and this, while the cloud feemed to furround it, as much as ever: but whether the vapour might be really rarer near the planet, than it was at fome diftance, no judgment could be made, becaufe of her too powerful light.

Many have obferved the fixed ftars to appear thro' the vapour with an undiminifhed light oftentimes: and our great Dr *Halley* tells us, in his account of that remarkable *Aurora*, which was feen in *March*, 1715-16. that he obferved " one of the *laminæ* pafs fucceffively over " all the ftars of the Little Bear, without effacing the fmaller ones, in " the tail, of the fifth magnitude; fuch was the extreme rarity and " perfpicuity of the matter whereof it confifted."

I this week received a letter from Dr *Short* of *Sheffield*, in which he fays, " The 23d paft, at fix at night, the fky being overclouded, all

" the

" the clouds over the hemiſphere, turned of a ſudden to a deep blood-
" red colour, for 15′. then ſucceeded red ſtreamers for half an hour."

3. *Feb.* 15. 1749-50. in the evening there was a very vivid northern light, which darted forth ſeveral beautiful, crimſon, and fiery-coloured rays; wind N W *b* N 1, Barometer 30.2; 50 minutes paſt 8 a ſurpriſingly bright and exceedingly white arch, about the breadth of a common rainbow, appeared in the Heavens, extending nearly from E. to W. it reached within 5 or 6° of the weſtern horizon, and ended about 8 or 10 above the eaſtern. It paſſed exactly between *Caſtor* and *Pollux*, and directly over *Aldebaran*, which appeared plainly through it. Near the top of the arch ſeveral very lucid, white, ſhort, vibrating columns were attached to it; none of them ſeemed above 6 or 7 degrees long, and did not appear to communicate in the leaſt with the *Aurora borealis*. *Fig.* 45. ſomewhat repreſents it. About 9^h 12′ the arch vaniſhed; but ſeveral white, bright, coruſcating *nubeculæ* remained here and there in the zodiac for 12 or 15 minutes longer. The *Aurora borealis* continued more or leſs till midnight: the next morning the wind was E. 1^r, Barom. 30.1.

—*at* Plymouth, Feb. 15. 1749-50. *by* John Huxham, *M. D. F. R. S.* N°. 495. p. 472. Apr. &c. 1752. *Read* June 21. 1750.

Fig 45.

4. *Feb.* 16. about 7 *p. m.* we had another *Aurora borealis*, though not quite ſo fiery and luminous as that of the night before: it continued till near 11. At 8^h 56′ *p. m.* exactly, ſuch another arch appeared, very nearly of the ſame extent and direction, but not altogether ſo broad or lucid. This at firſt alſo paſſed between the two bright ſtars of *Gemini*, but declined more and more to the ſouthward, till it was 2 or 3° to the S. of *Pollux*. It's weſtern limb, about 9, paſſed thro' the N. ſhoulder of *Orion*: it quite diſappeared about 10 or 12 minutes after. This had no columns attached to it, as the former; tho' it was ſomewhat jagged and unequal towards the N. near the *vertex*. The wind this evening was E. 2; the Barometer 29.9. The next morning the wind was SW*b*W 1, the Barometer 29 9. Neither any part of the arch, or the attached rays were coloured, but perfectly white, and exceeding bright.

—Feb. 16. *by the ſame.* Ibid. p. 473.

5. On *Monday Jan.* 23. laſt, ſome unuſual appearances were obſerved in the ſky, at *London*, and the towns about it, by thouſands of people during the whole evening, of which ſome accounts were laid before the *R. S.* and as appearances of the like kind were obſerved in the Heavens, the ſame evening, at great diſtances from *London*, I take the liberty to preſent you a deſcription of what was ſeen at the city of *Norwich*, by Mr *W. Arderon*, F. R. S.; and alſo of what was obſerved at *Wells* (a little ſea-port town in the ſame county of *Norfolk*, about 30 miles nearly due N. from *Norwich*) by Mr *Joſeph Sparſhal*, and ſent by him to Mr *Arderon*, who communicated it to me.

—*at* London, Jan. 23. 1749-50. *by* Mr Hen. Baker, *F. R. S.* ibid. p. 499. *Read* June 28. 1750.

6. The wonderful appearances of the *Aurora borealis* on *Jan.* 23, laſt, have been taken notice of in moſt parts of *England*, though in different forms. At *Norwich* I believe it was as extraordinary as at any place whatever: but the weather being very cold, and myſelf ſomewhat indiſpoſed,

—*at* Norwich, Jan. 23. 1749-50. *by* Mr W. Arderon, *F. R. S.* ibid p. 500.

3 A

disposed,

diſpoſed, I did not make all the remarks I could have wiſhed: the few I did make are as follow:

This wonderful *Aurora* began at 6 in the evening, with a blackiſh cloud in the N. E. out of which ſprung up a ſtreak of ſcarlet-coloured rays, of a ſurprizing beauty and vividneſs. This preſently extended to within a few degrees of the S. W. horizon, paſſing directly thro' the *zenith*, and ſo continuing near a quarter of an hour, when red and yellow columns began to riſe upwards from every quarter.

At 7 a black cloud roſe up in the S. E. and quickly put on a ſemicircular form, with light yellowiſh vapours aſcending out of it's upper edge, and repreſenting a glory of an uncommon brightneſs.

At 8 the black cloud was diſperſed, but the yellow glory remained; and round that ſprang up another circle of red, which made the whole appear very tremendous.

The reddiſh ſtreams, as well as this laſt-mentioned circle, were ſometimes ſo denſe, that even ſtars of the firſt magnitude could not be ſeen through them.

There was now-and-then ſome of the flaſhing *Aurora* in different parts of the firmament, though not ſo common as I have obſerved at other times.

The night was full as light as it is when the moon is about 8 days old; but I could compare it neither to the light of the ſun nor moon, ſome of the original colours ſeeming to be wanting: and the beſt deſcription I can give of it is, to liken it to that light produced in a dark room, when one of the ſeven original colours is ſeparated from the reſt, after they have paſſed through a priſm, and been collected together again by a convex lens.

This evening the Barometer was 30.1 falling. *Haukeſbee*'s Thermometer 63. Wind E. $\frac{2}{10}$ force. The morning miſty, and very cold, but all the day clear.

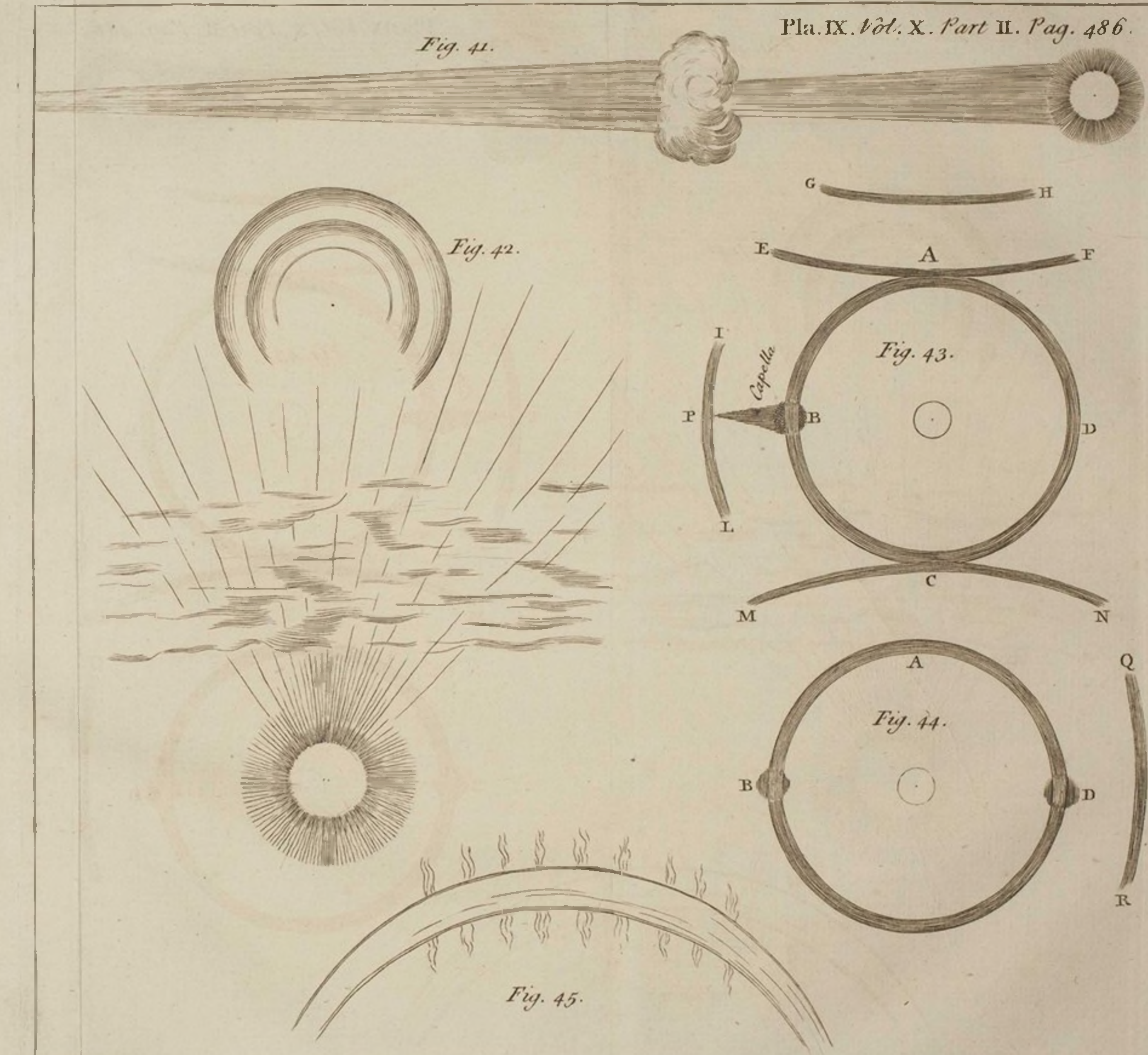
——*at* Wells *in* Norfolk, *by Mr* Joſ. Sparſhal, ibid. p. 502.

7. On *Tueſday Jan.* 23, the air at *Wells* was clear and ſerene during the greateſt part of the day, with a freſh breeze of wind at S. S. E. which terminated in an evening extremely remarkable for appearances in the Heavens of an uncommon *Aurora borealis*.

At 15′ paſt 5, I firſt took notice of the foot of an arch, which formed an angle of about 10° with the N. E. part of the horizon. This arch ſhot out pointed ſtreams like pyramids, of a fiery red colour, which generally aſcended within a few degrees of the *zenith*, then vaniſhed, and were immediately ſucceeded by others, from the N. E. where the principal magazine ſeemed to be. They continually ſhifted towards the E. and S. W. with ſudden flaſhings and dartings; but towards the weſt the appearances ſeldom altered.

At 30′ paſt 5, a luminous ſtream, of a bright flame-colour, ſhot up on the N. ſide of the fiery arch, which ſtill kept ſomewhat of that form, though frequently interrupted by ſhooting flaſhes from the N. E.

At


Pla. IX. Vol. X. Part II. Pag. 486.
Fig. 41.
Fig. 42.
G
H
E
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Capella
Fig. 43.
P
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Fig. 44.
B
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Fig. 45.

At 46′ paſt 5, there appeared ſuddenly in the N. E. an elliptical *corona*, of an amazing brightneſs, elevated about 9° above the horizon, and having it's longeſt diameter parallel thereto. There ſhot up perpendicularly from this, ſtreams reſembling columns of flame intermixed with others of bright red.

At 50′ paſt 5, part of the hemiſphere, included between the N. E. and S. E. was ſtrongly illuminated, with a vaſt number of pointed rays of crimſon and flame-colour, darting towards the *zenith*. Theſe vaniſhing in about 4′, were ſucceeded by many whitiſh ſtreaks, ſhifting from the N. to the S. E.

At 55′ paſt 5, there ſprang up in the N. 3 bright pyramids of flame-colour, aſcending to the height of about 70°, not perpendicular to the horizon, but inclining towards the E. and theſe were joined at the upper parts by an equal number of a blood-red colour from the S. E.

At 6, a pyramid of a crimſon colour roſe from the N. E. to the height of about 60°. This ſoon diſappeared; but a diffuſed redneſs remained; and from the N. E. to the S. W. appeared an arch, crowned at the *vertex* with ſomething reſembling a Glory, with a round body of light in it's centre.

At 20′ paſt 6, the redneſs was contracted into a narrower compaſs, but was much deeper in colour, forming an arch from S. E. to S. W. and appeared at the *zenith* like a fiery ſheet ſpread towards the S; the N. being at the ſame time illuminated with white ſtreams, like the common *Aurora borealis*.

At 30′ paſt 6, a whitiſh ſemicircular arch was formed to the ſouthward, encompaſſing the red lights in the *zenith*, and extending from the E. to the S. W. But this appearance continued only a few moments.

At 40′ paſt 6, the redneſs quite diſappeared in the N. E. and that to the ſouthward became much paler. The common *Aurora* appeared very plain in the N. E. till 7, and then totally diſappeared.

At 8 the common *Aurora* appeared again very bright from N. to E.

At 30′ paſt 8, appeared another large ſemicircular arch, extending from E. to W. pointed columns of a bright red ſhooting up from each ſide of it's baſe; with other ſmaller ones on the upper part of it, ſuch as the common *Aurora*. At the ſame time aroſe in the N. from within a very few degrees of the horizon, a bright pyramidal ſtream of light, of a ſurpriſing magnitude. This appearance continued near $\frac{1}{4}$ of an hour very regular, and exceeding beautiful.

At the beginning of theſe lights the mercury ſtood at 29.9, but quickly fell to 29.8. The wind at S. E. $\frac{2}{10}$ force.

During part of the time I obſerved an uncommon motion in the magnetic needle; but was too cloſely engaged in obſerving the Heavens to take much notice of it.

I ſaw

I saw this evening those meteors called falling stars; particularly one, which, on taking fire, left a long train of sparks behind them.

My situation being quite open to the sea on all sides, except the S. afforded me as favourable an opportunity of viewing the above particulars as I could wish; and I gave up my whole attention to them.

Aurora Australis, *seen* Jan. 23. 1749-50. *at* Chelsey; *by* John Martyn, *F. R. S.* Prof. Bot. Cantab. N°. 494. p. 319. Jan. &c. 1750. *Read* Jan. 25. 1749-50.

XXVI. *Jan.* 23. 17$\frac{49}{50}$. at about half an hour after 5 in the evening, casting my eye accidentally toward a window which looked to the S. S. W. I thought I saw a reddish light about the planet *Venus*, which then shone exceedingly bright. Being suspicious of some fire in the neighbourhood, I went immediately to a window on the stair-case, where I saw a reddish light, which shone with such exceeding brightness, that the lustre of the fine constellation of *Orion* was almost effaced. I then went to a window facing the N. N. E. where I presently saw a very broad band of crimson light, like that which I observed from the same window, *March* 18. 1738-9; * but in the former the red band was bounded on the N. by streams of a greenish blue; whereas the band now observed was entirely of a deep crimson colour, being of a much darker red than the former.

Thence I withdrew into my garden, where I plainly saw a band or arch, of a very deep crimson colour, in appearance about 15° broad, the southern edge of which passed just above *Canis minor*, and the shoulders of *Orion*. It was terminated to the westward, near *Venus*, then about 20° high: but it extended to the eastward as far as I could see, and the farther it went that way, the deeper was the colour, and the broader the band. About a quarter before eight, there was formed a crown about 30° to the southward of the zenith; for I could plainly perceive the *Pleiades*, which had then just passed the meridian, very near it, when the light was faint. From this crown a great many rays darted to the E. S. and W. but not toward the N. where only some whitish streaks were to be seen, but very faint. Presently after this, the part of the arch extending to the E. seemed to be suddenly kindled, as if some train had been fired; grew extremely bright and vivid; and as if all the red matter had been then consumed, put an end to the phænomenon before eight.

During the rest of the evening, a pale light covered the S. part of the Heavens, as if the moon had shone.

A Letter from the Rev. Mr John Forster, *to Mr* Henry Baker, *F. R. S. concerning an* Earthquake *at* Taunton, N°. 488. p. 398. June 1748. *Read* June 15. 1748

XXVII. In answer to your inquiries concerning the earthquake, which happened last year on the first day of *July*, when I was at *Taunton* in *Somersetshire*, after taking some pains to inform myself more particularly what other people observed in different places, you may depend on the truth of what follows.

Between ten and eleven o'clock at night, on the said first day of *July* 1747, being myself in some company at *Taunton*, we were suddenly

* See Vol. VIII. P. ii. chap. i. § liv. 1.

surprized

ſurprized with a rumbling noiſe like diſtant thunder, which was followed immediately by ſo conſiderable a motion of the earth, that the chair whereon I ſat rocked under me. The noiſe and ſhaking ſeemed to come from a diſtance, and approached gradually, in ſuch a manner as if a loaded waggon had paſſed along; and continued nearly the ſame time as ſuch a waggon would require to go about 100 yards. The motion went from S. E. to N. W. which being the direction of the ſtreet, on one ſide whereof the houſe ſtood, ſome of us imagined at firſt that a waggon had really gone along; but, upon inquiring, we found there had been no waggon: and indeed, as we were ſatisfied afterwards, no waggon could have been heard or felt in the back room where we ſat, on account of its too great diſtance from the ſtreet.

Notwithſtanding this happened between 10 and 11 at night, when moſt of the town were in bed, the ſhock was ſo ſenſible, that many people got up very much terrified; and they waking others, the conſternation ſoon became general; inſomuch that, although it was a rainy night, numbers of people ran out into their gardens, and ſpent the night there, being apprehenſive of other ſhocks. The account then newly brought us of a dreadful earthquake at *Lima*, being freſh in every body's mind, contributed to increaſe the ſurprize.

A worthy clergyman, who lives 5 miles from *Taunton*, informed me, that the china and glaſſes upon the cupboards in his houſe rattled and ſhook as if they would fall down, and the bells in his houſe rang. A perſon who was at that time coming on foot to *Taunton* likewiſe told me, that the noiſe ſeemed to him like the diſcharge of cannon at a diſtance, and came rumbling onwards, till the earth moved under him in ſuch a manner that he could hardly keep upon his legs: ſeveral others alſo that were abroad aſſured me they had much ado to ſave themſelves from falling.

The extent of this earthquake, as far as I can learn, was from ſea to ſea; that is, from the *S. Chanel* to the *Severn*. It moved from S. E. to N. W. and was felt in every pariſh through this whole courſe, which is in length about 40 miles: nor was its breadth much leſs; for it was felt at the ſame time both at *Exeter* and *Crookhorn*, which lie from one another about the ſame diſtance of 40 miles, in a line directly acroſs its before-mentioned courſe.

P. S. I have heard it reported that there were flaſhes of lightning at the time of the earthquake; but I neither ſaw any myſelf, nor have met with any body that could affirm he did.

XXVIII. 1. As I was walking along *Chancery-lane* to-day, towards *Holbourn*, about 40′ paſt 12, people came out of ſeveral houſes to their doors in great ſurprize, complaining of the ſhaking of their houſes, and imputing it to the fall of ſome great timber, or other heavy body, which they ſuppoſed at ſome little diſtance from them, and which they came out to inquire after.

—— *at London, Feb.* 8. 1749-50. *by Mr* Henry Baker, *F.R.S.* N°. 497. p. 601. *Read* Feb. 8. 1749-50.

When I was got into *Holbourn*, I found the people under the ſame conſternation, and expreſſing themſelves nearly in the ſame manner.

Going to *Gray's-Inn*, many people were got together in the great ſquare, talking about the ſhock they had felt; and in particular a lamp-lighter was giving an account, that, being on his ladder, pouring oil into a lamp, he was in danger of falling off, by ſomebody's ſhaking the ladder, as he at firſt imagined.

I then went to a gentleman's chambers under *Gray's-Inn* library, where I was told the ſhaking had been ſo much, that they thought a clock would have been thrown down; and imagined at the time, ſome large box or heap of books had been tumbled down over head.

The people in all the ſtreets, as I returned home, were talking of this matter; and ſome of the women complaining that the motion had made them ſick.

My own family, in *Catherine-ſtreet* in the *Strand*, had been no leſs ſurprized, and had ſent to the neighbours to inquire if ſome heavy thing had fallen down, to occaſion the ſhaking of the houſe, which Mrs *Baker* deſcribed as very great: ſhe ſat in the dining-room, which is to the ſtreet, and her belief at the inſtant was, that the ſervant was fallen all along in the backward room of the next ſtory higher, thereby ſhaking the houſe, and making a conſiderable noiſe.

My ſon felt the ſame ſhock at the *Tower*; where alſo a gentleman, who was ſitting at a table to write, in his houſe in the Mint, was thrown out of his chair with a conſiderable force towards the table; and where every body was much ſhocked with the apprehenſion of ſome exploſion of gun-powder.

What therefore ſo many people, in different ſtreets, at great diſtances from each other, have been ſurprized at, cannot be only fancy, but muſt be owing to ſome real cauſe; and if no powder-mill, or magazine of powder, has beeen blown up, it muſt have been an earthquake, or ſome tremor of the earth itſelf.

I was pretty curious to inquire of people in different places, to judge the better from their ſeveral reports; and I found them agree, almoſt in general, in the firſt ſuppoſition of ſome weighty body falling; moſt ſaid with a noiſe, but ſome ſeemed uncertain as to that. I endeavoured likewiſe to learn its courſe, by comparing the accounts of people in different ſituations: it ſeems to have lain E. and W. and to have paſſed from the W. eaſtward. I felt nothing of it myſelf as I walked the ſtreet, nor do I find that many who were walking did.

—— *by* Gowin Knight, *M.B.* & *F.R.S.* Ibid. p. 603. *Read* Feb. 8. 1749-50.

2. To-day, betwixt 12 and 1, the houſe in which I live in *Lincoln's-Inn-Fields* was ſhaken violently for a moment. The room where I was ſhook very much, but nothing was thrown down. In another room the grate was ſeen to move, and the fire-ſhovel was thrown down. A maid-ſervant that was above-ſtairs was much frightened with the ſhock and noiſe: ſhe ſaid, ſhe heard a ſound like thunder, which ſeemed to come from below: that the window-curtains and bed ſhook very ſenſibly, and the

the latter was moved from it's place nearer to the wall. Three other perſons in the houſe both heard the noiſe and felt the ſhock; but I did not take notice of the noiſe myſelf, being intent upon ſomething elſe at that time.

Soon after this happened, a ſervant came from his Grace the Duke of *Newcaſtle*, to inquire if we had perceived what had happened; and ſaid, that his Grace's houſe had ſhaken all over.

I ſent to two or three houſes in the neighbourhood, and was informed, that they were equally ſenſible both of the ſhaking and the noiſe; and at one of the houſes it had thrown down a firkin of butter from a ſhelf. I was further informed, that ſeveral of the neighbouring inhabitants ran out of their houſes; and ſome gentlemen that were playing at tennis ran out of the tennis-court.

A woman reports, that ſhe felt a like ſhock the night before laſt about 7 o'clock, which made the candle jump off the table at which ſhe then ſat.

Whilſt I was writing, a man came in from *Greenwich*, who ſaid, he was at *Dockhead* when the ſhock was felt there, and the noiſe ſeemed to him like that of a cannon at a diſtance: that all the way he came, as far as *London-Bridge*, the people were alarmed at it.

3. As I paſſed thro' the *Mews* to-day, a little before 1, I felt a ſhock like an earthquake, and I thought I heard a hollow deep noiſe. Several women thereabouts came running out of their houſes much terrified.

—*by* Jo. Freeman, *Eſq*; *F. R. S.* Ibid. p. 605. *Read* Feb. 8. 1749-50.

At *Leiceſter-Houſe* they were apprehenſive the foundation was giving way, and were going to ſend immediately for the ſurveyor.

All the way in my return home, I ſaw many groups of people together, and diſcourſing upon this ſhock that had juſt happened; ſome imagining it was occaſioned by ſome houſes being blown up in *Gold-Lane*, where there was a great fire, and others from ſome powder-mills blowing up; the ſame thing having been obſerved about 9 years ago, from the like accident at the mills at *Hounſlow*. If neither of theſe cauſes appear, it can be no other than an earthquake.

4. On *Thurſday Feb.* 8. 1749-50. at about half an hour after 12, as I was ſitting reading with one elbow on the table, on the ground-floor, in my houſe at *Eltham* in *Kent*, I felt two ſhocks from E. to W. which I immediately thought was an earthquake, as I had felt ſomething like it once at *Naples*; and was confirmed in my opinion, by my wife's running down-ſtairs frighted, and declaring it was an earthquake, ſhe having felt one in the *Weſt Indies*. She was in the room over me, in which room there was china ſtanding on a cabinet, which, ſhe ſays, ſhook in ſuch a manner that ſhe expected it to fall. My children, who were in the room over her, ſeem to have felt it ſtronger; as they ſay, they apprehended a cheſt of drawers in their room was falling. The ſervants that were in the kitchen, which has no room under it, ſeem to have felt but little of it. One that was writing ſays he felt the dreſſer move, and the wall, but thought it was only the ſhutting of a door. Other

—*by* Will. Fauquier, *Eſq*; *F. R. S.* Ibid. *Read* Feb. 15. 1749-50.

ſervants in the ſame room felt nothing at all of it. My gardener, who was at work in the garden, felt nothing of it.

The wind was at S. W. and had been high in the night and morning, but was very much abated; and after this, for ſome time, it was quite calm; which I believe it is generally obſerved to be, in thoſe countries where earthquakes are more frequent. A flight of pigeons I have, ſeemed to be much frightened.

Eltham is about 8 miles S. S. E, from *London-Bridge*, and ſtands on a hill.

This account was written before I had heard any thing from *London.*

—*by the Rev.* Henry Miles, *D. D. F. R. S.* Ibid. p. 607. *Read* Feb. 15. 1749 50.

5. I find, upon inquiry, that the earthquake on *Thurſday* laſt was felt in a gentleman's houſe in this place, * pretty ſenſibly, in two chambers, and in another over one of them, by a tremor of the wainſcot and utenſils, and a ſmall ſhock ſucceeding; but was not perceived at the other end of the houſe, in a room on the ſame ſtory with the chambers. I ſhould have thought, when it was felt ſo near us, as about 150 yards, our houſe, which I look upon as very ſuſceptible of impreſſions, ſo as to be ſhaken by the winds, would have been affected: but if it had, 'tis hardly poſſible but ſome of the family muſt have perceived it, conſidering the parts they were in, and their being chiefly in a ſitting poſture. I conclude therefore it could not be very conſiderable here. I have endeavoured to learn whether it extended any further to the S. of us, but cannot yet find it did.

The wind has been chiefly S and S. W. for ſome months paſt, much longer than is uſual at any time of the year; and yet we have had but a ſmall quantity of rain hereabout for the ſeaſon.—I give you, on the other ſide, the ſtate of the Barometer and Thermometer on the 8th, and two days before and after that, as obſerved at *London* by Mr *Canton*, and by myſelf here, at 2h *p. m.* and at 8h *p. m.* each day. I ſhall only add, that yeſterday (the 13th) my Thermometer abroad at 1h *p. m.* was at 59 = to 27° above the freezing point: a degree of warmth exceeding what we had ſeveral days in laſt *June*, at 2h *p. m.*

At *London.*

	At 2 *p. m.*		At 8 *p. m.*	
Day.	Barom.	Thermom.	Barom.	Thermom.
6	29,14	48	29,27	43
7	29,90	48½	29,99	43
8	29,83	54	29,95	52
9	29,97	55½	29,96	52½
10	29,90	54	30,03	45½

* Tooting *in* Surry.

At

At *Tooting.*

Day.	At 2 *p. m.* Barom.	Thermom.	At 8 *p. m.* Barom.	Thermom.
6	29,18	49	30,32	42
7	29,94	49	30,03	$38\frac{1}{2}$
8	29,88	55	29,98	52
9	30,—	56	29,99	$52\frac{1}{2}$
10	29,88	$55\frac{1}{2}$	30,04	45

——*by* J. Martyn, *F. R. S.* Prof. Bot. Cantab. Ibid. p. 600. *dated* Chelſey Feb. 14. 1749-50 *Read* Feb. 15. 1749-50

6. It was felt here * at 40′ after noon. All the houſes were violently ſhaken, eſpecially thoſe which are neareſt to the river. I was ſitting in my ſtudy, which fronts the S. W. up one pair of ſtairs. I imagined that ſomething heavy had fallen down in the room below me. The ſervants, who happened to be diſperſed in ſeveral rooms, each of them thought one of the others had thrown down ſome heavy cheſt or cabinet. A maid-ſervant, who happened to be paſſing from one of the under offices to another, felt the ground ſhake under her. As the place on which her feet were, was full 6 feet below the ſurface, I immediately concluded, that ſuch a motion muſt be occaſioned only by an earthquake.

However, as moſt of the neighbours were confident that the ſhock was occaſioned by the blowing up of the powder-mills at *Hounſlow*, I diſpatched a meſſenger thither on horſeback, who brought me word, that the ſhock was not felt either there or at *Brentford*; and that he could not learn that it had been felt farther weſtward than *Kenſington*-turnpike.

I have alſo ſince been inform'd, that it was felt at *Fulham*; but a friend, who lives a little beyond *Richmond*, has ſent me word, that they did not feel it there.

Of thoſe who were in the ſtreet, or upon the river, ſome felt it, and others not.

I am credibly informed that letters from *Calais* and *Boulogne* mention it's having been felt on that coaſt.

It ſeems therefore to have extended itſelf far to the E.; but to have terminated in the W. about two miles beyond this place.

——*by Mr* Trembley, *F. R. S.* Ibid. p. 610. *Read* Feb. 15. 1749-50.

7. The earthquake was not felt at *Ingatſtone*, nor at *Colcheſter*, nor at this town †.

On *Thurſday* laſt the lightning fell on 2 ſteeples not far from *Colcheſter*. ‖

8. With

* Chelſey.

† Harwich.

‖ Mr *Trembley*, who was going the next day for *Holland*, was at the time of the earthquake with Mr *Folkes* in his ſtudy in *Queen's-Square*; where were alſo the Earl of *Macclesfield* and the Hon. *Charles Bentinck*; they all felt themſelves at the ſame inſtant ſtrongly lifted up, and preſently ſet down again: they alſo heard a noiſe over their heads as of ſome heavy piece of furniture being thrown down, whilſt thoſe who were in the room over them were frighted, and apprehended the like accident had happened below-ſtairs. The coachmen on the boxes of 2 coaches then ſtanding at the door, were extremely ſenſible of the ſhock, and apprehended the houſe was going to fall upon them.

—*by* Smart Lethieullier, Esq; *F. R. S.* Ibid. p. 613. *Read* Mar. 1. 1749-50.

8. With regard to the extent of the late earthquake this way (I mean due E. from *London*) I cannot learn that it passed the little river *Rodden* that runs by my gardens, and crosses the *Harwich* road at *Great Ilford*. My own house stands about ½ a mile N. from the 6 mile-stone. I was in my gardens with several workmen, and none of us were sensible of any thing; but my wife, who was in her dressing-room, felt the house tremble so much, that, upon my coming in, she told me she verily believed there had been an earthquake; the motion of the house under her being exactly like what she had often heard described when she was in *Italy*.

—*at* Plymouth, *about one in the morning, between* Feb. 8. *and* 9. 1749-50. *by the Rev. Mr* W. Barlow. Ibid. p. 692. *Read* May 24. 1750.

XXIX. It is proper to observe, that the following relations are not made by mean, ignorant, or fanciful people, but by persons of good sense, whose veracity is unquestionable, and whose judgment in this case is, I think, rational and just.

The Earthquake at *London* was on *Thursday*, *February* 8. about noon.

The declaration of the Hon. Philip Vanbrugh, *Esq; Commissioner of his Majesty's dock-yard near* Plymouth, Devon.

That, in the night, betwixt *Thursday* the 8th of *Feb.* 1749. and the day following, towards one as he was reading in bed, he was sensibly affected by a sudden shake: that, looking about, he observed betwixt the foot-curtains, which were partly open, some drawings which hung on the wall, also the side-curtains, in a trembling motion: that it immediately brought into his mind the earthquake in *Jamaica* in 1692. which several years ago had been, at *Jamaica*, particularly described to him, by a person who was in the island at the time of that earthquake.

The declaration of Mrs Vanbrugh, *Sister of the Commissioner, living in the same house.*

That, the night above-mentioned, at what hour she cannot say, she was greatly surprized by an unusual motion of her bed; which immediately brought to her thoughts the shock of an earthquake she felt several years before, to which in her mind she resembled the present shake.

The declaration of Mrs Slade, *Daughter of Mr* Slade, *Master-Shipwright of his Majesty's dock-yard,* Devon.

That, in the night above-mentioned, towards one she perceived a sensible motion of her bed, and (there being a light in the room) saw the curtains shake. This she thought to be occasioned by the wind (which she then heard blow strong), supposing the sash not to be close down: accordingly she rose, and went to the windows, but found the sashes intirely close.

N. B. Both the Commissioner and the Ladies believe there was only a single shake. Miss *Slade*'s house joins the Commissioner's.

On account of the dead time of the night in which it happened, not many people would be sensible of the motion: of those who were, most would naturally attribute it to the high wind, which then blew; and a month being passed before any inquiry has been made, it is not to be wondered at that, but few recollect any thing concerning it.

The Commissioner, by reason of frequent returns of the gout, does not sleep up-stairs, but in a very small room behind the house, joining to the

the houſe, and communicating by a door with one of the back parlours. This building is ſo low (being only a ground-floor, without any room over it), and is ſo ſkreened by higher buildings, that hardly any wind can affect it; eſpecially the wind which blew that night; as the body of the Commiſſioner's houſe was betwixt that and the ſtroke of the wind.

The dock is about 2 miles W. from *Plymouth*, and about 220 almoſt weſt from *London*.

XXX. 1. At very nearly half an hour after 5 this morning, being then in bed, but perfectly awake, I felt a very ſtrong ſhake, or rather 3 or 4 ſucceſſive ſhakes of an earthquake, as I immediately took them to be. I judge the whole *phænomenon* to have laſted about 3 or at moſt 4″; and the ſhocks appeared to differ from what I had felt on the 8th of laſt month, in this particular, that I did not now perceive that lifting upwards, and ſudden ſettling again, which I was then ſenſible of; nor did I hear that diſtinct noiſe, as of a great weight falling, which moſt people were then ſurprized with: but what now moſt affected me was the ſenſation of very quick ſhakes, or of a *Tremor*, as it appeared in a horizontal direction; and I heard, during all the time, a ſort of crackling of the wainſcot, window-frames, and floor, with ſuch a rattling in the chimney, as I have ſometimes heard upon a ſudden and ſtrong guſt of wind.

—March 8. 1749-50. *at* London *by the* Preſident. Ibid. p. 613. *Read* March 8. 1749-50.

I inſtantly jumped out of bed, to ſee if there was any damage done; and going to my chamber-door, I met my daughter running in a fright from her room, who ſaid ſhe was waked with ſuch a ſhock, that ſhe thought her room had been falling; two men-ſervants alſo, who lay in the garret, and whom I had called to, anſwered me whilſt I was talking to my daughter, that they were both awakened by the ſhock, and that they felt, as they both expreſſed it, ſuch a motion, as they had ſometimes known given to a child in a cradle.

Preſently after I had got on my clothes, I ſpoke to ſome of my neighbours, who all gave me very nearly the ſame account as this I have been juſt giving of what I had obſerved myſelf; only ſome added, particularly a gentleman who lives in an older houſe than mine, that he apprehended, from a crackling noiſe over his head, that a chimney had been thrown down, and was then breaking thro' the tiles and lathing of his houſe.

I ſent a ſervant about 7, and he met a countryman, who was bringing a load of hay from beyond *Highgate*, and who was on the other ſide of the town when the ſhock happened; he did not, he ſaid, feel it, as he was driving his waggon; but that the people he ſaw in the town of *Highgate* were all greatly ſurprized, ſaying they had their houſes very much ſhocked, and that the chairs in ſome were thrown about in their rooms.

The

The chamber I lie in is up two pair of ſtairs forwards, and my bed ſtands N. W. and S. E. I took particular notice, that there was neither cart nor coach going by, but that every thing was entirely quiet at the time.

—— *by the Rev. Mr* Tho. Birch, *F. R. S.* Ibid p. 615. *Read* March 8. 1749-50.

2. This morning, *Thurſday March* 8. 1749-50 about 18′ before 6, according to equal time, or half an hour after 5 by the ſun, I felt, in my bed-chamber, on the ſecond ſtory of my houſe in *Norfolk-ſtreet*, adjoining to the river, a ſhock of an earthquake, much ſtronger, and of longer duration, than that which I had felt on *Thurſday Feb.* 8. I was full awake, and had juſt begun to dreſs myſelf, when I was alarmed with the trembling of the room, attended with a noiſe ſomewhat different from that of the former ſhock this day month, which ſeemed to be occaſioned by ſome great weight falling upon the floor above me; whereas the noiſe of this latter appeared to me cauſed only by the tremulous motion of the whole room and the adjoining ones, the walls, wainſcot, furniture, *&c.*

When I came down to my ſtudy on the firſt floor, I found a book thrown down from an upper ſhelf.

My family were awakened by the ſhock. The air was at that time, and for ſome hours after, extremely calm, and the wind weſterly.

This account was drawn up immediately before I had ſeen any other perſon, except my own family.

I have been ſince told by a gentleman, who reſided many years in the *Weſt Indies*, that this laſt ſhock was more violent than any he had felt there, except one at *Carthagena*, in which a city, about 200 leagues diſtant from thence, was ſwallowed up at that inſtant.

Another gentleman deſcribed to me the ſenſation, upon being awakeened by the motion, to be like that of falling into a fit.

——*by Mr* Henry Baker, *F. R. S.* Ibid. p. 617. *Read* March 8. 1749-50.

3. About 40′ after 5 this morning I was waked out of a ſound ſleep by a great noiſe, hardly to be deſcribed, but what ſeemed then to me as if the roof of the houſe was tumbling in, or like the rumbling I have ſometimes heard thunder make before a very loud clap. It continued 2 or 3″ as nearly as I could gueſs, under the ſurprize of being awaked ſo ſuddenly; during which time the windows rattled, the tables, chairs, and other furniture in the room, ſhook greatly; and a braſs warming-pan, in particular, that ſtood upon a marble ſlab before the chimney, made a very remarkable ringing and jarring noiſe.

Two maids, in a room over mine, were alſo waked by it, and got up immediately, being frighted by the ſhaking of the bed on which they lay; but for my own part I felt little or no motion of my own bed. However, as moſt people who were in bed agree in deſcribing ſuch a motion, it was probably that which waked me, tho' the violence of it might be over, before I was ſufficiently awake to take due notice of it.

My ſon was likewiſe waked by the great rattling of the windows in his chamber, which he at firſt imagined to be ſhaken by a high wind,

they

they being apt to make some noise in windy weather, though never like what he then heard, which seemed like opening a casement-window and shutting it again very suddenly and violently. A cat that lay upon his bed started up in much surprize.

Several people in the neighbourhood went out into the streets, and others got upon the leads of their houses, to see if any damage was done. Some milk-women felt it very strongly as they were milking their cows near *Maribone*. It was violent in *Bond-street*, and all that quarter of the town; and I find, by people living in very different and distant parts of *London* and *Westminster*, that it was felt universally through the whole, and was not longer in continuance, but that it's motion was more violent and shocking than the earthquake, which happened on *Feb.* 8.

A waterman that was lying in his boat, then at anchor near *Kingston*, told a friend of mine, that he was frighted with something like a violent blow striking against the bottom of the boat; that the water was much agitated, and that the barges and other vessels upon the river were tossed about as in a tempest.

4. As I was walking this morning in my garden at *Kensington Gravel-Pits*, about 40′ after 5 by my watch, I felt an exceeding great shock, a succussion of the earth, attended with an explosion that, I fancy, resembled the blowing up of a mine, and with the same kind of noise. It was followed by a trembling, very brisk at first, which gradually abated; and in about 3″, as near as I can judge, totally ceased. The tremor was attended with the noise of a distant thunder, which, with the motion, gradually died away.

——*by* Martin Clare, *Esq*; *F. R. S.* Ibid. p. 620. *Read* March 8, 1749-50.

I am not certain the building near me moved; but I fansied it did. My feet I am sure felt great emotion; and a large watering-pot, of 9 inches base, that stood near me, was thrown all along, the moment the trembling ceased.

P. S. I have since discovered that my watering-pot was overset by a brick that was thrown off the house by the shock.

5. At 40′ past 5 this morning, as I lay awake in a two pair of stairs room, I heard a noise, first as if the top of the house was cracking, and then as if it fell in. Instantaneously the house shook with great violence, rocking to and fro from N. E. to S. W. so as to make all the furniture rattle, the windows and bells ring. It also waked all the family, but the servants in the garrets felt the greater shock.

——*by* Daniel Peter Layard, *M D. & F. R. S.* Ibid. p. 621. *Read* March 8. 1749-50.

It is reported, that two small shocks preceded this, one at 12 last night, the other at 2 this morning; but I cannot say I felt either.

The milk-people in the fields were very sensible of this earthquake, and say, that it began by a report like that of a cannon fired near at hand. Those who were milking in a barn thought it was coming down; but were not so sensible of the earth's motion as those milking in the fields, who could scarcely stand. The cows were also frightened, and ran away from the people.

This earthquake was univerſally felt at *Hampſtead*, and much ſtronger than that on *Feb.* 8.

— *by the Rev Mr* Roger Pickering, *F. R. S.* Ibid p. 622. *Read* March 8. 1749-50.

6. The earthquake this morning, which happened at ſo early an hour as to ſurprize moſt people in their ſleep, I had all the opportunities imaginable of obſerving to as much exactneſs as *phænomena* of this nature can be; and therefore preſent you with this early intelligence.

About ¼ after 5 I was lying in bed awake, and under the compoſure which one generally feels upon recovering from a regular and refreſhing ſleep. To this the ſerenity of the morning greatly contributed, as well as the gradual increaſe of light, which, the ſhutters being open, could eaſily be perceived through the linnen furniture of the bed and window-curtains. I mention this particular, becauſe, every object in the chamber being thus rendered ſufficiently diſtinct, I had the opportunity of examining the event by the evidence of my eyes, as well as that of my ears. On a ſudden I heard a ſound like that of a blaſt of wind; ſuch a blaſt I mean, as thoſe we perceive in the dry windy days, ſo frequent with us about the equinoxes; one that, at different intervals, riſes gradually to its full ſtrength, and gradually dies off. The attention this occaſioned led me very ſenſibly to perceive myſelf raiſed in my bed (which ſtood N. and S.) and to obſerve, that the motion, as I lay upon my back, began on my right ſide, and from head to foot inclined me towards the left.

This was nearly inſtantaneous with the commencement of the blaſt, and I immediately concluded it to be an attack of the ſame kind with that on this day was a month. The ſenſation I felt at this time was rather ſolemn than terrifying; ſo that I patiently lay to obſerve the following circumſtances.

The firſt ſhock being given, the motion that followed was that of a very quick vibration; and looking at the curtains of the bed, I perceived their motion was ſimilar. The hinges of the drawers of a dreſſing-table on my right hand clapped, not only diſtinctly, but conſiderably loud; and a ſound, ſuitable to its materials, came from every moveable body in the room. The whole ſhock, to my judgment, laſted about 10 or 12″; when, the ruſhing noiſe above mentioned having gradually died away, every thing was reſtored to that calmneſs which had preceded the event.

I then roſe, and found by my watch that it was half an hour paſt 5.

I am not able to judge of the comparative force of this ſhock with that on this day month; for I was then at my houſe on *Enfield-chace*, where it was not in the leaſt perceived, nor nearer to there (as I can learn) than *Edmonton*, which lies, in a S. E. direction, about 3 miles nearer to town. But one of my chimnies in town has ſuffered by this ſhock; and I am informed that two others have been thrown down in *Monkwell-ſtreet* and *Jewin-ſtreet*, which lie in a N. W. direction, from *Aldermanbury*, at about ½ a mile diſtance.

I think

I think I have ſomewhere found it obſerved by the ancient Naturaliſts, that earthquakes uſually, tho' not conſtantly, happen in the ſpring and autumn; and the principle they go upon gives ſome credit to their obſervation. However, this induced me to look over my minutes of ſuch earthquakes with us as have come to my knowledge ſince the middle of the laſt century. Theſe I found as follow; *viz.*

19 *Jan.* 1665-6.	at *Oxford* ——— ———	about 6 even.
17 *Sept.* 1683.	at *ditto* ——— ———	about 7 morn.
9 *Oct.*	*ditto* thro' the midland counties	about 11 night.
8 *Sept.* 1692.	In *London*, and parts adjacent, and in the *Engliſh* camp in *Flanders*	about 2 *p. m.*
28 *Dec.* 1703.	In the N. of *England* ———	about 5 even.
8 *February*, 1749-50.	In *London*, and about 7 miles round, and from private hands at *Calais*	about 12½ *p. m.*
8 *March*, *ditto*	In *London*, where elſe, at preſent unknown,	about 5½ morn.

Out of theſe 7 inſtances, you will perceive, that 3 have happened in the autumn, 2 in ſpring, and 2 in *Dec.* and *Jan.* ſo that the balance in favour of the ancient obſervations is as 5 to 2. I beg leave alſo to obſerve, that of theſe, the 2 in the ſpring are thoſe we have ſo lately felt, and thoſe, after the openeſt winter, and in the warmeſt ſpring, that our country, poſſibly, ever knew. How far this obſervation may be applicable to adjuſt the cauſe of the two late earthquakes, it would be impertinent in me to offer.

7. He was juſt come into the garden (at ¼ an hour after 5), and was ſcarce arrived at the great tree, near the little door under *Harcourt*'s buildings (which is about 10 or 12 yards perhaps diſtant from *Harcourt*'s buildings), when he heard a GREAT NOISE, LOUDER BY MUCH than the noiſe of the exploſions upon the proof of the *great cannon* at *Woolwich*, when full-charged (at which proofs he has been often preſent); nay *louder, he thought, than* ANY *noiſe he ever heard.* This noiſe ſeemed to come from behind the buildings; and his face was then oppoſite to them.

—— *as ſeen in the* Inner-Temple *garden, by* Robert Shaw (*a very ſenſible* Scotchman) *then at work there; communicated by* James Burrow, *Eſq*; *F. R. S.* Ibid p. 626. *Read* March 15. 1749-50.

At the ſame inſtant he ſaw the whole building *move upwards*, then *incline forwards* towards him (ſo that he thought it would fall upon him); then *recline backwards*, and then *ſettle*; at which time all the *windows rattled and clattered*, as upon an exploſion of a cannon; the ſound in the interim *rolling away* (ſeemingly from the water-ſide up towards *Temple-Bar*) juſt like the rolling ſound or echo which accompanies or ſucceeds the exploſion of thunder, or of ſhip-guns.

As to the continuance, he often expreſſed himſelf that it was *as a thought*; and, in point of time, he ſuppoſed it could not be 3''.

The ſound and the motion began both together; but the ſound was quite rolled off, rather *ſooner* than the buildings were ſettled.

 I aſked

I asked him how much he judged the declination of the building to have been *from the perpendicular?* he answered, "that it was impossible to be exact in relating that particular, both by reason of the surprize he was in, and also from the suddenness of the whole compass of the appearance, which was but as a thought." However, in order to form some notion of it, I made a comparative inclination of the garden key, which I held perpendicular to my hand, and moved backwards and forwards, till he should judge it to approach nearest to what he could recollect of the heeling of the buildings (which indeed was but an inaccurate method of coming at the truth of a thing professedly uncertain even to the relator): It seemed to me, by his description in this rough comparison (in which I might easily mistake him) to be a declination of several degrees from the perpendicular; which is also agreeable to his apprehension, that the building would fall upon him. Possibly, his surprize might magnify the appearance far beyond the reality.

I then inquired what perception he had of his own *personal motion*, and that of the particular spot of *ground whereon he then stood?* he said, his surprize was so great, on seeing so new and uncommon an appearance (for he was not in or near *London* when the last earthquake happened), that he either had *no perception* at all concerning himself and the ground under him; or at least was so intirely engaged in observing what he saw and heard, that he had no attention to what he felt.

N. B. I made this memorandum immediately after conversing with this *Robert Shaw*; and therefore I suppose it contains a tolerably exact account of what he related to me.

—*by the Rev* H Miles, *D. D. F. R. S.* Ibid. p. 628. *Read* March 15. 1749.

8. At $5^h\ 40'$ nearly, equal time, I was standing in my study, when I heard a noise, at the first moment, very like the explosion I heard several years ago, when some drying and storehouses of gunpowder were blown up about 6 miles from us: this noise issued in a murmur in the air, like distant thunder, and ended like a rushing wind: there was no perceivable intermission in the noise from first to last; it seemed to come from the W. and continued about $4''$ exclusive of the tremor, which lasted some time after the noise seemed to have ceased. I found no motion of the floor, which is over a cellar, upon a level with other adjoining ground-floors; but the doors of my book-presses, at the west end of the room, shook to and again, and the rings on the doors rattled exceedingly; but I was not sensible of any motion of the other doors on the sides of the room, near to some of which I then stood. Such of the family as were in bed felt their beds to be raised up, and then shaken from side to side.

A sensible young gentleman, who called upon me that morning as he came from *London*, told me, that, considering the situation his bed was placed in, and the motion he felt from one side to another, he concluded the shock proceeded from the W. The same observation, he said, others had made, with whom he had talked.

It

It was univerſally felt here *, and very much at the following places around us: at *Merton*, about a mile S. W. from us, and at *Mitcham*, about 2 miles S. at *Croydon*, 4 miles S ſome tiles fell from houſes; it greatly alarmed the inhabitants of *Streatham*, about a mile and ½ E. of us; at *Clapham*, two miles and a half N. a chimney, and other parts of a building fell down; at *Wandſworth*, two miles N. W. the ſhock was very great; eſpecially near the ſide of the *Thames*. It was felt likewiſe at *Epſom*, about nine miles S. W. from us; but how much farther, my information does not ſay.

Upon inquiry made of ſeveral perſons, who were abroad at the time, going to their work, I cannot hear of any one, who ſaw any lightning, perceived any noiſe, or felt any thing of an earthquake. †

The ſtates of the Barometer and Thermometer were not remarkably different from what they had been for ſeveral preceding days; however I have added them underneath.

7 day at 4^h *a m*. Bar. 30,07. Therm. 32 almoſt, Wind W. clear, and white froſt.

D° at 9^h *p. m*. Bar. 29,99. Therm. 40, clear evening.

8 day at 4^h *a. m*. Bar. 30,07. Therm. 40, cloudy morning. Wind W.

D° at 2^h *p. m*. Bar. 30,14. Therm. 58, cloudy evening. Wind W.

9. It was much ſtronger than that which happened in *Feb.* two great ſhocks being felt preſently after each other. Thoſe who were out of doors felt the ground ſhake under them very ſenſibly; whereas, in the former, few were ſenſible of the ſhock, except thoſe who were in houſes. I do not hear of any miſchief done by it in this neighbourhood; ‖ neither can I learn that it extended farther weſtward than the former. I am very well aſſured, that it was ſtrongly felt at *Fulham*, and at *Turnham-Green*. ——*by* John Martyn, *F. R. S.* Prof. Bot. Cantab. Ibid. p. 630. *Read* March 15. 1749.

Several were ſenſible of a ſmaller ſhock about 3 the ſame morning; and ſome talk of unuſual appearances in the air. But this laſt circumſtance ſeems not very well atteſted. For my own part, I looked out of my chamber-window to the S. W. at ¼ after 5, and only perceived it to be a grey morning, ſuch as uſually predicts a fine day.

10 As there happened ſome particular circumſtances in the laſt violent ſhock of an earthquake, that were not taken notice of at our laſt meeting, I preſume therefore to ſend you the obſervations I made thereon, as they appeared to me at my houſe in *Bloomſbury*. ——*by* Michael Ruſſel, *Eſq*; *F. R. S.* Ibid. p. 631. *Read* March 15. 1749.

On *Thurſday* morning laſt, about half an hour after 5 o'clock in the morning, I was awakened by a violent percuſſion of my bed; the ſhocks, I apprehend, laſted about 10 or 12″, becauſe my bed was rocked from one ſide to the other ſeveral times. The motion appeared to me to be in a horizontal direction. I heard no noiſe before the ſhake; but if any, it might perhaps occaſion my being ſo thoroughly awakened. My ſervants told me they heard a report like a great blaſt of wind with a

* Tooting *in* Surry. † See Art. 14. ‖ Chelſey.

clat-

clattering and ſnapping of the windows and doors of the houſe, as they expreſſed it.

In my dining-room there was an *India* cabinet, on which was placed ſome ornamental *China*, part of which was thrown down on the floor, and ſome broke.

But what was moſt remarkable, I had two *China* figures placed on the cabinet, with their faces fronting the W. which were, by the ſeveral ſhocks, turned about facing the N. E. which I took to be nearly ¼ of the circumference of a circle. In this ſituation I found them as ſoon as I aroſe in the morning; and I am aſſured nobody had been in the room before to diſplace them.

Since, I was told by ſome company, that were at my houſe, that a porter was going down *Chancery-Lane* to call a gentleman to go to the *Brentford* election, and, in his way, as he called it, was ſtruck with a blaſt, turned round on his heel, and fell down, and has not been well ſince. Alſo another perſon, that was ſet out on ſome buſineſs, was nearly turned round by the ſhock; which ſeems a little to confirm the moving the *China* figures in the direction before mentioned.

——*by* James Parſons, *M.D F.R S.* Ibid. p 633. *Read* March 15. 1749.

11. Between 5 and 6 in the morning I was awaked by a violent motion of the houſe, with a great noiſe reſembling the fall of ſome heavy body over-head: at waking I found my bed move very much; but in my ſurprize could not diſtinguiſh what kind of motion it might be, as to its direction; this being momentary, as well as my ſurprize, I ſaw and heard the *China* rattle upon a cabinet in my chamber, and perceived a trembling motion in the room for ſeveral ſeconds. My wife was alſo diſturbed; and aſking what was the matter, I ſaid I felt a ſhock, which I believed to be like what had happened that day month: ſhe anſwered, if this is one, I felt another about 2 this morning, while you ſlept. I immediately looked at my watch, and ſaw it was 36′ or 37′ after 5. I obſerved a light ſhine in at my windows more than was natural; and from the redneſs of the ſky, and clearneſs of the morning, concluded the ſun was riſing; but imagined it ſomething ſtrange it ſhould riſe ſo many minutes ſooner than was uſual upon the 8th of *March:* upon which I roſe, and went to my window, which looked eaſtward, and then ſaw it was a red light only, extended from about N. N. E. to S. E. which I take to be that mentioned by the Rev. Dr *Miles* laſt *Thurſday.*

Then I went into the ſquare, where ſeveral ſervants came out of their houſes much frighted. They all felt it the ſame way; and ſeveral labourers and market-people told me they were ſhaken very much as they walked, and that all *Newgate-Market* was in the greateſt confuſion imaginable, no one thinking himſelf ſafe. The greateſt part of thoſe I ſpoke to about it heard the ſame ſort of noiſe that occurred to me at the time.

Many complained all day of great ſickneſs at the ſtomach, and *headaches*; I had the latter 3 or 4^h after it; and others of my acquaintance felt pains in the back the greateſt part of that day.

At

At *Mary'-bon* it was univerſally felt; and with the greateſt violence imaginable. At one houſe where I was at 12 that day, a maid-ſervant ſaid, ſhe was thrown firſt to one ſide, and then back to the other; and many compared it to the rocking of a cradle. I inquired particularly of ſuch as were thus rocked, about the ſituation of their beds, and obſerved, that, though all deſcribed the motion to be from ſide to ſide, their beds were in all directions.

As to the ſhock which was ſaid to be felt at two the ſame morning, there can be no doubt of it; for I went to ſeveral families in my neighbourhood, who poſitively aſſerted it to be true; but all ſaid it was not ſo ſtrong as that which happened afterwards; nor could any one ſay he was awaked by it: on the contrary, all thoſe, who felt it, ſaid they were awake before it happened. As for myſelf, I did not perceive it, being aſleep; and therefore I ſhould be inclined to think the motion to be inſufficient to diſturb a ſleeping perſon; for I am eaſily diſturbed.

There is a ſtrong report of a trembling motion being felt at two on *Friday* morning following, which laſted not long, and another at four the ſame morning which laſted ſeveral minutes, without any noiſe. I met counſellor *William Swinburn*, of *Devonſhire-ſtreet*, who told me, that Mr *Love*, an eminent Apothecary at *Weſtminſter*, declared to him, that he had occaſion to be up that night, and was very ſenſible of thoſe trembling motions both times; and I have been ſince informed, that one Mrs *Marſhal* of *Queen's-ſquare* felt the motion about two that morning, but was not ſenſible of the other.

I alſo made ſome inquiries concerning the extent of the ſhock of *Thurſday* morning, and was informed by *John Wolffe*, Eſq; a great ſtudier of nature, that he wrote to his gardener at his houſe at *Haſely-Court* near *Tedford* in *Oxfordſhire* about it, with orders to go to *Henley* upon *Thames*, and inquire whether it was felt there, and he wrote back word, that the market-people who came there from 20 miles round knew nothing of it. I was alſo informed by Mr *Sherwood* of this *Society*, that it was not felt at *Horn-Church*, a gentleman having ſent his ſervant on purpoſe to know; nor any further that way than *Ilford*. It was alſo felt at *Stanmore*, but not at *Watford*, five miles further.

I was yeſterday in the city, and a gentleman ſaid, the fiſh in his ponds were remarkably diſturbed by the ſhock, and that many of them leaped quite out of the water upon the bank; and while he was relating this, another came in, who ſaid, the ſame happened in a pond of his, and in that of another gentleman in his neighbourhood at the ſame time.

12. Mr *Joſiah Boyfield*, of *Gravel-Lane*, near *St George's Fields*, *Southwark*, told me, that at the time of the earthquake, he was not far from his canal, and that it was a clear ſtill morning; the air (as he expreſſed it) as clear as a bell, with not a breath of wind ſtirring. As clear as a bell, except that ſmall hazineſs which there almoſt always is in the morning-air.

—*by* James Burrow, *Eſq; F. R. S. of what Mr* Boyfield *told him concerning the ſame* Ibid.

p. 637. *Read* March 22. 1749.

He

He heard, he said, a great noise like thunder; which he at first took to be the report of ship-guns; and immediately after the ground shook and trembled under him, and he heard his men say, *Look at the fish.* The sound came from the S. E and moved to the W. or N. W. over *Westminster:* It went off rattling like ten thousand cannon; and he thought that *Westminster-Abbey*, and all that part of the town was comeing down. The sound preceded the motion, and was ended before the motion ceased.

He went, he says, nearer to the canal, and saw the water much disturbed; but he did not himself actually see the fish leap out of the water: however, some of his men, whose account he can depend upon, saw several roach leap up, and saw the fish in general shoot away in all sorts of directions, and seem to shift for themselves, as if they were frighted and alarmed at what had happened.

—*by* C. Mortimer, *M. D. Secr. R. S.* Ibid. p. 638. *Read* March 22. 1750.

13. I awaked a little after 5; I opened my curtains, and observed the sky hazy: I drank a draught of water, and, looking on my watch, found it to be but ½ an hour past 5; and therefore was going to lie down again in my bed; when, leaning upon my right elbow, I first felt a shock, as if the whole house was violently pushed from the N. W. to the S. E. and then, with equal force, pushed back again from S. E. to N. W. between which shocks I heard a dull noise; as if something heavy, but soft, rolled to and fro' in the garret over my head. I saw the cornice and ceiling of the room sensibly move, and then perceived a third and fourth shock, so slow as to give me time to reflect what might be the event; and then a fifth and sixth, which I own began to frighten me; the strokes seeming to be exceeding strong; not at all like a quaking or tremulous motion, but like a forcible shoving backwards and forwards; much resembling the rocking of a coach, when crossing the rutts, or giving way to another carriage. My son, who lay in the next room called out in the midst of it to know what was the matter? saying, the wainscot partition between the two rooms seemed as if rending asunder. The whole seemed to have lasted about 3″. I find that a post, which supports the above-mentioned partition close to the door-case, has given way a little; the paint being fresh cracked; and a crack in a wall upon the stairs is grown something wider. The door between the two rooms, which shuts with a fall-latch, that was unlatched by the first earthquake, and flapped to and fro' 3 or four times, was not unlatched by this last; which I think is an argument, that by the first the house was lifted up, and that it was not so by the last, but only rocked from side to side.

I am credibly informed of a pannel of wainscot in the city being wrenched out of the groove, and not returning into it again.

—*by the Rev.* H. Miles, *D. D. F. R. S.* Ibid. p. 639. *Read* March 22. 1750.

14. It having been commonly reported, that there was much lightning on the morning, and about the time when the earthquake happened on the 8th[*] of this instant, I was induced to get what information I could of the truth of it, in this neighbourhood.

* See Art. 8.

A per-

A perſon who went into an open cart at 4 that morning at *Mitcham*, for *London*, ſaw no appearance of lightning at all. Several other perſons, who were abroad, going to work, ſome time before the earthquake happened, declare that they ſaw none; nor were they ſenſible of any noiſe, or motion in the earth.

I got inquiry to be made of the watchmen, who belong to the callico-printers, and watch all night in the grounds, and received this anſwer from one, that he neither ſaw any lightning, nor heard any noiſe, and had no perception of an earthquake; and from another, who watched in other grounds, a mile or more diſtant from the firſt-mentioned, that he ſaw no lightning all that night; but ſays, that he heard an unuſual noiſe about the time in which the earthquake is ſaid to have happened; but this it ſeems was not ſo remarkable as to have cauſed him to take notice of it to any one, had he not afterwards been told there was an earthquake.

I have, beſides this, made inquiry of various perſons, and cannot meet with one, who did ſee any lightning, or who heard others ſay that they had ſeen it.

P. S. I am credibly informed, that a gentleman's ſervant at *Wandſworth* was watering his maſter's horſe by the river-ſide, at the time when the earthquake happened, who found the water ſo much agitated on a ſudden, that the horſe ſtarted back, and would not drink.

15. Whilſt I was in the country, I made as much inquiry into the progreſs and extent of the earthquake northward as I was able; but could collect very little worth notice.

—*by the Rev.* Wm. Cooper, *D. D. Dean of* Durham. Ibid. p. 647. *Read* March 29. 1749.

It was felt very ſenſibly at *Northaw*, and at *Gubbins*, the ſeat of Sir *Jeremy Sambrooke* in that neighbourhood, whoſe houſe was ſhaken very much. At a farm $\frac{1}{4}$ of a mile N. E. of *Hatfield*, it was felt by the farmer and his family, but not perceived by the inhabitants of *Hatfield* itſelf.

In like manner at *Hertingfordbury*, a village a ſmall mile W. of *Hertford*, the ſhock was felt diſtinctly; but not obſerved by any of the inhabitants of *Hertford*.

At my own houſe at *Panſhanger*, 2 miles W. of the above-mentioned town, the noiſe was heard twice, at the interval of about $\frac{1}{2}$ a minute, reſembling the rumbling of a cart through the ſtreets; but no ſhock felt either within or without-doors.

This circumſtance of the noiſe being heard without any ſenſible tremor or heavings of the earth, makes me imagine, that the force of the vapour was ſpent before it reached that place; and thereabouts may be reckoned the northern limit of the earthquake; at leaſt, I have not heard of any places more to the N. that were affected by it. It is no wonder, in a ſhock ſo ſudden and alarming, that very few ſatisfactory obſervations are made, either as to the nature of the ſhock, the direction of the tremor, the time it was felt, or it's duration; of none of which can I give you any particulars.

—*The President's report of the account given him by* Tho. Burrat, *of* Kensington Ibid. p. 681. *Read* April 26. 1750.

16. On *Tuesday* the 24th of this present *April* 1750. Mr *Thomas Burrat*, of *Kensington*, a husbandman, and bailiff to the R. Hon. *Henry Fox*, Esq; of *Holland-House*, gave me the following account:

That, being early about his business in the grounds about ½ of a mile from the said house, on *Thursday* the 8th day of *March* last, as he was going to tell his sheep (which he does every morning, and which were then lying down on a dry sloping piece of ground), he heard, as he thought, about ½ after five a noise, much like thunder at a distance; which coming, to his apprehension, from the N. W. continued some small time, growing louder as it came nearer him, and gave a crack (so he expressed himself) over his head; and then went off in the same manner it came on towards the S. E.

He said, that the sky was, to his thinking, quite clear, and without any cloud; and that he saw neither lightning, nor any appearance of fire; but that, immediately after the crack, he found the ground to shake under him; and that he even saw it move where he was (though as dry and sound a spot as any he knows) like a quagmire or quicksand; insomuch that he could not help being apprehensive, that it would have opened, and taken him in.

He says, that the sheep he was beginning to tell all started up at once, as frighted, and presently began to run, as if pursued by somewhat they were apprehensive of. He said further, that he took notice, that several crows, which were at roost upon some trees not far off, all at the same instant flew away, making the same noise they constantly make when they are affrighted at the discovery of a bird of prey, or any other enemy; and that the trees themselves very sensibly trembled and shook.

The noise he heard began a sensible time before the shake of the earth; and he judges that the whole matter lasted better than a minute.

He first expressed himself about the direction of the noise he heard in the manner just above related; and, being asked again concerning that particular, he explained himself by saying, that, to his thinking, it came on from between *Hillington* and *Harrow on the Hill*, and went off over *Deptford*: which may be observed to be very agreeable to his other description.

Mr *Bird*, the eminent mathematical instrument-maker in the *Strand*, told me also the same day, that he heard, at his house, a noise, like the discharge of a cannon at some distance, just before the earthquake; and that his bed, in which he then was, was very sensibly rocked from right to left twice: and that he is well assured the feet of the bed were actually lifted up from the floor, during the motion; as he was very sensible, by the noise they made when they came to the floor again, 4 times in all, twice to his right hand, and as often to his left hand.

—*an account of part*

17. At a pot-house belonging to Mr *Oad* in *Gravel-Lane*, a large part of the roof, containing near two square, was intirely thrown down by the

the laſt earthquake, *March* 8. 1750. and ſeveral fiſhermen, then at work, imagined a porpoiſe, or ſome other large fiſh, had riſen under their boat.

of a Roof *of a* Pot-houſe *at* Lambeth *being flung down by the ſame: communicated by Mr* Wm. Jackſon, *Potter, to* C. Mortimer, *M. D. Sec. R. S.* Ibid. p. 700. *Read* June 21. 1750.

XXXI. 1. By a letter I had from Mr *Oakes* at *Portſmouth*, dated the 19th inſtant, he gives me an account of the inhabitants being alarmed with a ſevere ſhock of an earthquake on *Sunday* the 18th at 6 in the evening; and that it was felt ſtronger at the *Common*, which is about ¾ of a mile diſtant.

—at Portſmouth, March 18. 1749-50. *in an extract of a letter to Mr* J. Ellicot, *F. R. S* Ibid. 646. *Read* March 22. 1749.

2. Yeſterday, about ¼ before ſix in the evening, a ſlight ſhock of an earthquake was felt here. I don't find it was general, as many people felt nothing of it. It was felt ſenſibly at Mr *Carter*'s and Mr *Taylor*'s; ſo that it is ſomething more than fancy.

P. S. Since writing the above, I am told the ſhock was very violently felt in the *Iſle of Wight* yeſterday.

— in an extract of a letter; communicated by Daniel Wray, *Eſq; F. R. S.* Ibid. p. 647. *Read* March 22. 1749.

3. The firſt part of the preceding week was fine weather. *Wedneſday* night it became damp and cold, and continued ſo all *Thurſday*, with miſts. *Friday* was a fine day; *Saturday* damp and cloudy; *Sunday* morning fine. Juſt before 6 in the evening we had ſome large drops of rain, and a thunder-cloud paſſed to the S. E. juſt as the earthquake happened.

— in an extract of a letter from the Rev. Mr Taylor, *vicar of* Portſmouth, *to Mr* Roderick. Ibid. p. 649. *Read* March 29. 1750.

The firſt thing perceived was a ſhock, like the ſudden ſtop of a body in motion; a kind of jarring. This was ſucceeded immediately by a gentle motion, nearly in the direction between E. and W. which made 3 or 4 ſlow and deliberate vibrations. Thoſe who ſat facing the E. or W. were moved backward and forward; and thoſe who faced to the N. or S were moved ſideways. The whole was attended with a noiſe like that of thunder at a very great diſtance. It laſted, as I judge by different accounts, about 4 or 5''.

I do not hear of any exploſion. The ſaſhes and door in my chamber ſhook, as in a blaſt of wind a little ſtronger than ordinary. Several, who were on the battlements of the church, felt it more violent, and heard the bell-frames and floor ſhake and crack. Few on the ground, or in motion, were ſenſible of it.

It was felt at *Havant*, 7 or 8 miles to the E. and at *Fitchfield*, 7 miles to the W.

It paſſed to, or from, the *Iſle of Wight*, where it affected the ground-floor, as much as the chambers here. It ran along the coaſt between E. and W. but I have not heard that it was perceived at ſea, or went far inland.

I am informed it has been felt at *Guernſey* and *Jerſey*; ſo that if it moved horizontally, it muſt be a conſiderable depth under-ground; the ſoundings from hence to thoſe iſlands being, in ſome places, 45 or 50 fathoms: ſo that I flatter myſelf, that the ſmall reſiſtance which it can meet with at ſea is ſo diſproportionate to what it muſt encounter under ſo many more fathoms of earth, that a more violent return will rather open and diſcharge itſelf there, than do us any conſiderable miſchief at land.

——in an extract of a letter from Mr Benj. Cooke, *F. R. S. to Mr* Peter Collinſon, *F. R. S.* Ibid. p. 651. *Read* March 29. 1750.

4. Beſides the ſhock, which happened here * about 6 in the evening on the 18th inſtant, as has been mentioned in the public prints from many neighbouring places, there was another, which was felt by ſome betwixt 3 and 4 o'clock next morning: but whether this latter was as extenſive as the former, I cannot yet learn.

In the evening ſhock, a gentleman of my acquaintance was ſitting alone in his parlour by the fire with the doors ſhut; the ſpaniel-dog, which lay as it were aſleep before him, was ſo terrified at the unuſual motion, that he ran round the room in the greateſt fright and confuſion, as endeavouring to find a way of eſcape.

——in an extract of a letter from a gentleman at Southampton *to* Joſiah Colebrooke, *Apothecary, F. R. S.* Ibid p. 652. *Read* March 29. 1750.

5. We have had nothing of an earthquake in theſe parts till laſt *Sunday* evening, when they had it all over the *Iſle of Wight*. My ſon wrote me the following account of it, which is dated at *Newport*, on *Monday* the 19th:

"Laſt night, juſt at 6 o'clock, as my aunt and I were ſitting to-
"gether, we felt an earthquake, and a terrible one I think. At firſt
"we heard a ſmall noiſe, which we ſuppoſed was a chariot; and, as
"the noiſe grew louder, the houſe began to ſhake; till at laſt the noiſe
"grew ſo loud, and the houſe ſhook ſo much, that we expected it
"would fall down. I believe it continued near a minute; and it was a
"great mercy we were not all conſumed. I confeſs I was dreadfully
"frightened. We had nothing fell down from the ſhelves in our
"houſe; but both our neighbours had things thrown down from their
"ſhelves. I heard a man ſay there was another ſhock this morning be-
"tween 3 and 4, but we felt nothing of that. I believe it was felt all
"over the iſland; for here is a man in town who felt it at *St Helen's*."

Yeſterday I heard they had a little of it at *Portſmouth* and *Lymington*: and a ſervant-maid in this town ſays, ſhe felt her chair ſhake; and the windows ſhook, and the wainſcot cracked, juſt at the ſame time: but I neither felt it, nor can find any body beſides that did.

——in a Letter from Mr Peter Newcome, *F. R. S. to the* Preſ. *concerning the ſame ſhock being felt at* Hackney, *near* London. Ibid. p. 653. *Read* March 29. 1750.

6. I beg leave to give you ſome account of what was felt in our houſe at *Hackney*, on *Sunday* the 18th, a little after 6 in the evening; as we have received accounts of the ſhock of an earthquake being felt at *Bath*, *Portſmouth*, and ſome other places on that day.

My couſin *Peter Newcome* was ſitting in his chamber on the upper floor of the houſe, looking towards the fire, when he plainly perceived the hearth of his chimney to be moved; and immediately felt the chamber rock 3 or 4 times from W. to E. but heard no noiſe, as at the time of that ſhock felt on the 8th.

* Iſle of Wight.

Being

Being much ſurprized, he was running down-ſtairs, and there met with one of the maid-ſervants, who was running in a great fright out of another room on the ſame floor; and, before they could ſpeak, a young gentleman, of about 15 years old, came out of a cloſet juſt by, and they all at one moment aſked each other if what they had felt was not another earthquake.

In a chamber on the floor below, maſter *Hadley*, ſon to the late Mr *Hadley* of this *Society*, was in his bed, being ill of a cold; he felt the bed move upwards ſo ſenſibly, that he imagined ſomebody had got under it out of wantonneſs, and was lifting it up; and actually got out to look under the bed.

The ſame was felt by a ſervant, on the upper floor, at the other end of the houſe; and by another young gentleman underneath in that part alſo. The reſt of the family, being all together below-ſtairs, felt nothing of it.

7. On *Sunday* the 18th inſtant, at a very little after 6 in the afternoon, as miſs *Lethieullier* was ſitting in her dreſſing-room up two pair of ſtairs, fronting to the S. with a book before her, ſhe felt ſuch a ſhock of an earthquake, as ſhe apprehended, that ſhe immediately ran down-ſtairs, frighted; and finding Mr *Lethieullier* her father, and another perſon, ſitting together in the parlour, aſked them, "if they had not "felt another ſhock of an earthquake." But, finding that neither they, nor any one elſe, had perceived any thing like it, ſhe neither ſaid or thought any more of it; ſuſpecting it might be only a ſudden guſt of wind, or ſome other accidental cauſe.

—*in a letter from* James Burrow, *Eſq*; *F. R. S. to* Peter Daval, *Eſq*; *Sec. R. S. concerning the ſame* Earthquake *being felt at* Eaſt-Sheen, *near* Richmond-*Park in* Surry. Ibid. p. 655. *Read* April 5. 1750.

On reading the accounts in the publick papers of a real ſhock of an earthquake being felt at *Portſmouth*, at the *Iſle of Wight*, and at other places, exactly at the ſame time, her father, and the gentlewoman who was in the parlour with him, began to doubt whether the young lady's apprehenſion was not founded upon ſomewhat more than mere fancy or imagination; and Mr *Newcome*'s account ſeems to render it probable that ſhe felt a real motion.

Whether it was, or was not, I don't pretend to determine; and ſhould ſcarce have mentioned it, if it had not ſo exactly coincided with what Mr *Newcome* has communicated. I have no doubt of the fact above rehearſed; having been aſſured of it by all the 3 perſons before ſpoken of, who firſt mentioned it in an accidental converſation upon the ſubject, and afterwards (on being particularly interrogated) poſitively and expreſly atteſted it.

XXXII. I have here incloſed a letter from my neighbour Mr *Bowman*, at *Moleſey*, near *Hampton-Court*; whoſe veracity and abilities to make the proper obſervations, I can depend on. I well remember the extraordinary redneſs, &c. in the ſky the evening before, which he mentions. The ſhock which he felt in a chair, was, as I gueſs, in *Italy*; he having travelled much abroad.

—*at* Eaſt Moleſey *in* Surry, March 14. 1749-50. *in a Letter from the Rev.* Stephen Hales, *D.D.*

Without

& F. R. S. to the Preſ. *ſerving to incloſe a letter to him from* Walter Bowman, *Eſq;* Ibid. p. 684. *Read* May 3. 1750.

Without any regular connexion with the moon, it happened about 7 days after the ſecond ſhock, on the 14th of *March*, I believe, before 4 in the morning; when full awake, I felt my houſe, for a ſecond or two, ſhake, like a ſpaniel juſt come of water. My bell on the ſtair-caſe rung only one twitch. I roſe, looked out, and ſaw the moon ſhine bright, without one cloud, or one breath of wind; and finding none of my ſervants diſturbed, I returned to ſound and quiet ſleep.

It was exactly of the ſame nature with the ſecond ſhock, a ſhudder of the houſe from top to bottom; ſo that I neither miſtook the one nor the other for an exploſion of Mr *Norman*'s horſe-powder-mills, wherein, you know, he never works above 40 pounds at a time. Here I felt nothing like an exploſion, but a concuſſion, which any man may conceive, from his hand ſhaking a bed upon caſters, if we may compare great things to ſmall. Nor can I deſcribe the ſecond ſhock, felt alſo in bed, compared with this third, otherwiſe than by the ſhudder of a horſe after ſwimming, more ſtrong than that of a dog; while the ſame bell ſounded all in confuſion, as if it had been packed and toſſed in a hamper.

March 13. in the evening, about ſun-ſet, the ſky was dreadfully charged with a deep purple mixed with red, which, from the W. tinged the clouds by the S. quite to the E. and was ſucceeded by a clear effulgent crimſon or pink-colour, luminous, as deep, as ever eye pierced into the azure blue.

I neither have met with, nor heard of, any perſon, who felt this ſecond ſhake which I have deſcribed. But, if any memorials are to be preſerved of theſe ſeveral ſhocks, all which I have felt moſt diſtinctly, I think this ought not to be forgot; becauſe I do not apprehend it to have been ſtrong enough to have waked any perſon, nor to alarm even any one awake in bed. And as for thoſe who were up, and on foot, I do not think they could have perceived it, if I may judge by ſuch a one, which I once felt by a ſingle ſtart of my chair, without knowing what it was, till I compared notes with my more experienced neighbours.

——*at* Bridport, March 18. 1749-50. *in the poſtſcript of a letter from Mr* Nath. Downe, *to the Hon. Mr* Greenville. Ibid. p. 688. *Read* May 10. 1750.

XXXIII. In the morning the ſun ſhone very bright; which, between 11 and 12, was with dark clouds ſo obſcured, as rendered it darker than common. Soon after, a violent clap of thunder, and a heavy ſhower of hail, ſucceeded: after which it grew again ſerene; and in the evening about 6, a ſhock of an earthquake was felt in this town, and the neighbeuring villages, with (thro' mercy) no other damage than a great ſurprize to all who felt it.

——Apr. 2. 1750. *in an*

XXXIV. 1. On *Monday* night laſt, about 10, we felt in this city a ſhock of an earthquake. It was ſenſibly felt by all or moſt of the inhabitants.

bitants. A few bricks were shaken off a chimney in *Forest-street*: several house-bells were rung; the centinel at the castle was shaken off his seat in the centry-box; the houses all over the town were shaken, and the people terribly frightened and alarmed. It has been felt for some miles round the town; particularly at *Burn-Hill*, where the houses were greatly shaken. *extract of a letter from* Chester, *communicated by* Robert Paul, *Esqs F. R. S.* Ibid. p. 683. *Read* May 3. 1750.

2. We were greatly alarmed with a violent shock of an earthquake between 10 and 11. I, who was in bed, was frequently moved up and down; and the bed, having casters, was removed some small space from its proper situation. *——in* Flintshire, *in an abstract of a letter from Mr* Pennant *to* Rich. Holford, *Esq;* *Master in* Chancery. Ibid. p. 687. *Read* May 10. 1749.

During the shock, a great noise was heard in the air; and, some nights before, lights were seen in the sky; such as were previous to the earthquake in town.

Thanks to providence, no further mischief has happened, than the terror this unusual *phænomenon* occasioned in our family.

This place is about 2 miles from the sea.

3. The inclosed extract is the only written account of the late earthquake which I can obtain from any of the Naturalists in this country: and as I have conversed with several intelligent persons who perceived it, the enclosed extract corresponds very exactly with their sentiments and observations upon it. I have now in my custody the original letter from Mr *Seddon* to Mr *Philpot*; and shall not part from it without your direction. *——in* Cheshire, *in a letter from Mr* Ph Warburton *to the* Pres. *serving to inclose an extract of* Jo. Philpot, *a Letter from the Rev. Mr* John Seddon, *of* Warrington *in* Lancashire, *to Mr* in Chester. Ibid p. 695. *Read* June 14. 1750.

The late earthquake happened the 2d of *April*, at 10 at night, as nearly as can be determined; if any thing, rather after than before. I was at *Liverpool* at that time, where the shock was not so sensible as at some other places; and yet a person in company with us that had lived in *Jamaica* a great number of years, and well acquainted with motions of this kind, having felt 11 in one night, declared it to be the smartest he ever felt. The duration of the motion was every-where extremely short, not exceeding, as nearly as I can guess, 2 or 3″. *Extract of a letter from the Rev. Mr* J. Seddon, *of* Warrington *in* Lancashire, *to Mr* John Philpot *in* Chester, *dated* Warrington, May 10. 1750.

As to the nature of the motion, as far as I can judge of it myself, and from the observations of others, it seems to have been of the horizontal kind, proceeding in an undulating manner from N. W. to S. E. I was in a sitting posture, and the motion I felt was like that of a vessel falling from the top of a wave, and rising again upon the next. Mr *Breckell* of *Liverpool*, and others that I have conversed with upon the subject, represent it in the same way.

It is, I think, universally agreed, that an uncommon noise attended the shock, a noise that much resembled distant thunder, or a hollow rumbling wind: some persons also say, that they perceived a sultry sulphureous smell, much about the time of the shock; tho' that day and evening

evening were remarkably cold; and whether this was real, or only imaginary, I cannot determine.

The fhock was felt as far N. as *Lancafter*, and as far to the S. as *Wrexham*, and the adjacent parts; in all, about 70 miles N. and S. It was felt as far as *Stockport* and *Altringham* to the E. and quite into *Flintfhire* on the W. that is, about 30 or 40′ E and W.

The fhock was not fo great, or of fo long continuance, as to do any material damage. I think I heard of a large *China* jar falling from a chimney-piece in a gentleman's houfe, of a piece of marble reared againft a wall falling and breaking, and two or three trifling inftances of that kind. There is only one thing further that I would mention upon the occafion: as foon as I felt the fhock, I was immmediately apprehenfive what it was, and went out to fee whether there was any thing remarkable in the atmofphere. I then obferved a very uncommon appearance; *viz.* an infinite number of rays, proceeding from all parts of the Heavens, converged to one point; no luminous body appeared at all. The rays were at firft of a bright yellow; afterwards they became blood-red. This *phænomenon* was not far from our *zenith*. It continued about 20′, and then difappeared.

The enfuing night was very ftormy; a large quantity of hail fell about two in the morning; and the barometer was extremely low.

—— *at* Winbourn *in* Dorfetfhire, May 4 1749 *and at* Taunton *in* Somerfetfhire, July 1. 1747. *by Mr* Henry Baker, *F. R. S. to the* Pref. Ibid. p. 689 *Read* May 17. 1750.

XXXV. As the two remarkable fhocks of an earthquake, lately felt at *London*, may probably excite a curiofity of knowing what of the fame kind has happened in other parts of the kingdom within thefe few years, I wrote, about a week ago, to a fifter of my wife, who lives at *Winbourne* in *Dorfetfhire*, defiring her to fend me the beft account fhe could collect, of an earthquake, which, in one of her letters fome months ago, fhe had mentioned to have felt herfelf at that place, in *May* laft year; and the fubftance of her anfwer is as follows:

She fays, that, on the 4th of *May* 1749. about 10 in the morning, fhe was ftanding at one of the windows in her chamber, her fon (a boy about 9 years old) was fitting on a bed in the middle of the room, and her fifter was in another chamber two rooms from her, all on the fame floor; when they heard a fudden blow (fo fhe expreffes it) that feemed to be very near, which fhook the houfe fo much, that the windows rattled, and the floor fhook very much, and frighted her to fuch a degree, that fhe cried out, Lord, have mercy upon me, what is that? fuppofing it had been a burft of thunder. Then looking out, the fky was very clear, without any cloud near at hand; but there feemed to be a heavy cloud hovering at a diftance, whence fhe and her fifter imagined the fhock came; for they had then no thought of an earthquake.

There was, fhe fays, but one blow, with a noife very loud, like the difcharge of a cannon; which made her fend to inquire if there were any powder-mills in that part of the country, but was affured there were none. Her hufband (Mr *Bofton*) was then at a place called *Cafhmoor*, *Caſhmoor*, on the *London* road, 6 miles from *Blandford*, and about 8 from *Winbourne*, where he heard it in much the ſame manner. Their next neighbour was at the ſame time 12 miles diſtant, and heard it there; and every body ſaid it was an earthquake.

It was heard at *Shapeck*, about 4 miles from *Winbourne*; and at a place called *Eaſtbrook*, about half a mile from *Winbourne*, the people ſay it threw the pewter off the ſhelves. She ſays, I may depend upon it as a truth, that it was heard 20 miles round *Winbourne*; and adds, that people were very much frighted, but no harm was done.

I had the honour, two years ago, to lay before you an account of an earthquake felt at *Taunton* in *Somerſetſhire*, and for 40 miles in length as well as breadth, on the 1ſt of *July*, 1747. which was communicated to me by the Rev. Mr *John Forſter*, who happened at that time to be there by accident. It was likewiſe by mere accident I came to the knowledge of what I have juſt now been deſcribing: which induces me to imagine, that ſhocks of this kind may poſſibly happen more frequently than is commonly ſuppoſed, though we hear nothing of them: For, in country places, people are ſo little attentive to ſuch matters, that, unleſs ſome conſiderable miſchief be done, they mind them very little at the time, and, as ſoon as over, think no more about them.

XXXVI. Mr *Arderon* writes me word from *Norwich*, that, on *Thurſday* laſt, the 7th inſtant, as he and a friend were walking to take the air, a little to the W. of that city, they heard, about 7 in the evening, a kind of hollow noiſe, as loud as that of a large cannon. Which noiſe was once repeated nigh the ſame place as an echo, and then continued dying as it were away for about ½ a minute.

——*at* Norwich, June 7. 1750. *by Mr* H. Baker, *F. R. S. to the* Pref. *containing an extract of a letter from Mr* W. Arderon, *F. R. S.* Ibid. p. 698. *Read* June 15. 1750.

They ſaw no lightning, nor any clouds, except a few thin whitiſh ones in the weſtern horizon.

It was heard, he ſays, by great numbers of people in the city of *Norwich*, notwithſtanding the continual noiſe and hurry there: he likewiſe received accounts of it from *Swanthorpe*, 6 miles S. W. and from *Racka*, 4 miles N. E. of that city, agreeing with the above deſcription.

He has not heard that any perſon obſerved any tremor of the earth; and confeſſes his own ſurprize was ſo great, he does not know whether there was or not.

Mr *Wilſon*, a gentleman who was with him, thought the noiſe much reſembled the fall of a great building; and a gentleman at *Norwich* deſcribed it like a large weight falling down upon a chamber-floor over his head.

XXXVII. 1. The air mild and calm, no wind ſtirring, the ſun ſhining bright, at about 45′ paſt 6 in the morning, a ſhock of an earthquake was ſenſibly felt here, and hereabout, attended with a loud noiſe, and crack (as ſome call it): This was perceived both ſouthward, and north-

——Aug 23. 1750 *in an extract of a letter from*

Maurice Johnson, *Esq*; *to* Emanuel Mendez da Costa, *F. R. S. dated* Spalding *in* Lincolnshire, Aug. 25. 1750. Ibid. p. 725. *Read* Nov. 1. 1750.

northwardly, for some seconds. A gentleman from *Newark* in *Nottinghamshire*, N. E. of us about 30 miles, says it was also felt there.

——in a letter of Dr John Green, *to the same*. Ibid.

2. The earthquake was sensibly felt through the whole county of *Lincolnshire*, which is above 70 miles; but most strongly on the coast. The weather had been for some days before mild and calm: an *Aurora Borealis* appeared vertically, shooting rays of all colours around, which turned to a very deep red colour.

——at Newton *in* Northamptonshire, *on* Sunday, Sept. 30. 1750. *in a letter from* W. Folkes, *Esq*; *F. R. S. to his brother the* Pres. Ibid. p. 701. *Read* Oct. 25 1750.

XXXVIII. 1. On *Sunday* last, rather before one, whilst we were at church, we had an earthquake here. The noise, to the best of my judgment, continued near a minute; but was not so loud as either of those I heard at *London*. Several of the congregation perceived the ground to tremble; but I cannot say I did. It is said to have been more violent in several places in the neighbourhood than here; but this I much question. Also the day I went thro' *Stamford* to *Grantham*, in my way to *Yorkshire*, an earthquake was felt in both those places: so that I have been within the knowledge of no less than 4 of these shocks of nature in eight months time; but, thank God, none of them attended with any ill consequences, any farther than furnishing room for melancholy reflections upon such a disagreeable alteration in our climate, which had been generally thought before tolerably free from this calamity.

—near Bury St Edmund's *in* Suffolk, *and at* Narborough *in* Leicestershire; *in a letter from* James Burrow, *Esq*; *F. R. S. to the* Pres. Ibid. p. 702 *Read* Oct. 25. 1750.

2. This morning I have been making a visit at Lord *Cornwallis*'s at *Culford*, about 4 miles from *Bury* in *Suffolk*. Lady *Cornwallis* (whose judgment and accuracy are superior to all doubt or exception, and her veracity still more so) assured me, that on *Sunday* last, about one, as she was sitting and reading in her dressing-room at *Culford*, she suddenly felt and saw her chair and person move backwards and forwards; so that she searched and examined whether any dog had got under her feet and chair, or any one entered her chamber unperceived; but found herself absolutely alone in the room: whereupon she tried, whether, by laying her hand or elbow upon the table, she could repeat the same motion, or any thing like it; but could not. She added, that she felt herself a good deal surprized at this extraordinary sensation, at the instant of perceiving it: but neither then, nor afterwards, had the least imagination about an earthquake; till, upon coming down to dinner, she was asked by Miss *Charlotte Cornwallis*, her second daughter, a young lady grown up, "Whether she had not felt the earthquake?" Miss *Charlotte* agreed to the time; and was herself also sitting and reading in her own dressing-room, which was one pair of stairs higher than her ladyship's, yet on the same side of the house. However, it was also felt by Miss *Charlotte Cornwallis*'s maid-servant, whose chamber was in a different part of the house, and distant from either of the ladies apartments; and who was so alarmed at it, as to leave her room, and come into her young lady's, to see what was the matter. No one else in the house

houſe perceived it. But Lady *Cornwallis* ſays, that, as far as ſhe can learn, they were all upon their feet; none being ſitting, except the three already mentioned.

The houſe ſtands alone in the park: and Lady *Cornwallis* had declined making any inquiry amongſt the inhabitants of the adjacent village; partly, for fear of alarming them with apprehenſions of danger, of which they would be very ſuſceptible from the name of an earthquake; and partly from the little hopes ſhe could have of procuring any tolerably accurate account of the fact from ſuch reporters.

P. S. On our return hither to Mr *Wollaſton*'s, we found a letter from a worthy friend of Mr *Wollaſton*'s and mine, Mr *Metcalfe*, a clergyman of reputation, ſenſe, and fortune; who reſides at *Leiceſter*, and has two livings near that place; one at *Narborough*, the other at *Tilton*: out of which I will tranſcribe a paragraph, which will ſerve to confirm Lady *Cornwallis*'s relation.

"Yeſterday [It is dated *Leiceſter*, *Oct.* 1. 1750.] about noon, we were all "greatly alarmed with a very great ſhock of an earthquake. I was "in the pulpit at *Narborough*; where the whole church ſhook with "ſuch violence, that the congregation expected that the roof was "falling in, and run out of the church immediately, leaving the "poor parſon to ſhift for himſelf. I ſtood my ground; and, by "calling to them, and aſſuring them there was no harm, prevailed "on them to return, and make an end of the duty: but it was with "fear and trembling. It was felt pretty much at *Leiceſter*; but how "much further, I have not heard."

Since the receipt of the above letter, I have read, in the public newspapers, an account of it's having been alſo felt at *Northampton* about the ſame time.

So that no doubt can remain of the ſhock which Lady *Cornwallis* perceived at *Culford*, having been a real earthquake.

3. I beg leave to ſend you ſome memoirs relating to the earthquake, which happened in theſe parts on *Sunday* ſe'nnight, *viz. September* 30. the one is a letter from Sir *Thomas Cave*, Baronet, of *Stanford* near *Lutterworth* in *Leiceſterſhire*, a gentleman of good ſenſe, and unqueſtionable veracity. The other is a paragraph taken from the *Northampton Mercury* of this day. As for my own part, being engaged at church in a very ſolemn part of our worſhip, I only remember to have heard a loud exploſion, like that of thunder; but my neighbours aſſured me, they perceived the windows to ſhake and jar. I believe it was more violent in other parts of our county, and the counties adjacent; but I am cautious of tranſmitting any accounts, but ſuch as I think may be depended upon.

——in a letter from the Rev. Mr John Nixon, *F. R. S. to Mr* John Ward, *F.R.S. and* Rhet. Prof. Greſh. *ſerving to accompany two letters; one from Sir* Tho. Cave, *Bart. and another from the Miniſter of* Weſton with Sutton *in* Northamptonſhire. Ibid. p. 705. *Read* Oct. 25. 1750.

—by Sir Tho. Cave.

We were amazed at ¼ an hour after 12 on *Sunday* by a violent ſhock of an earthquake while we were at church; it laſted between 3 and 4′ and was attended with a prodigious rolling noiſe, louder than all the thunder I ever heard in my life, was it collected into one exploſion. Thank God, no damage accrued to any of us, beyond the confuſion it occaſioned.

Northampton, Oct. 8.

On *Sunday* the 30th of laſt month, about ¾ after 12, a ſhock of an earthquake was felt in this town, and in the country round us for many miles; but was not thought to be ſo violent as thoſe which happened at *London* at the beginning of the year. People who were ſitting in the churches, or in their houſes, were moſt ſenſible of it, but thoſe who were walking were not ſo much affected; and many conſidered it at firſt only as the noiſe of a ſudden guſt of wind, or the remote running of a coach or chair. We don't hear of any damage done thereby.

A letter from the Rev. Miniſter of Welton with Sutton, *in the county of* Northampton, *dated* Oct. 2. 1750.

On *Sunday Sept.* 30. at *Aſhley*, in this neighbourhood, about ¼ before one, whilſt they were ſinging after ſermon, the whole congregation was flung into the utmoſt conſternation, by a very terrible ſhock of an earthquake; the ſingers could ſcarce perſuade themſelves to finiſh their anthem. The reading-deſk ſtands juſt by the ſinging-pew; and I really thought that part of the church betwixt the chancel and the pillar next to it would have ſunk into the earth, with a loud and dreadful noiſe from a ſort of ſubterraneous exploſion, or whatever the learned and curious will term it. After that awful noiſe, and ſomething far exceeding a common tremor, it kept rolling on ſeemingly from N. to S. with an hollow rumbling, like thunder at a diſtance. This uncommon ſhock, I find, upon inquiry, was felt in all the neighbouring towns in *Leiceſterſhire*, as well as in this county; and very likely we ſhall hear that many parts of the iſland were affected by it.

—further particulars; in a letter from the Rev. Mr Nixon, *to the* Pr. R.S. Ibid. p. 710. *Read* Oct. 25. 1750.

4. As to the extent of this *phænomenon*, with reſpect to the S. and S.W. (of which alone I am at preſent capable of giving you any information) it ſeems not to have reached much further than *Towceſter*: for it was not perceived at *Stony-Stratford*, 8 miles ſouth of that place on the *London*-road; nor at *Newport-pagnel* in *Buckinghamſhire*, ſomewhat more to the E. of that town.

I ſpent the week before laſt at Mr *Blencow*'s, at *Marſton St Laurence* in the S. W. angle of our county; and found that it had not been perceived there, nor in the other towns on the borders of *Oxfordſhire*.

We went from thence to Mr *Holbeche*'s of *Farnborough*, 8 miles weſtward of *Marſton*, on the confines of *Warwickſhire*, where we could hear nothing of it; tho' it was felt at *Stockton* and *Leamington*, villages lying more to the N. in the ſame county, about 6 miles from *Warwick*.

It did not reach *Warwick*, but paſſed on to *Rugby*, and from thence entered *Leiceſterſhire*. I lately ſent Mr *Ward* a letter, which I received from Sir *Thomas Cave*, Baronet, at *Stanford*, on the borders of that county;

county; whereby it appears, that the ſhock and exploſion were felt in a very ſurprizing manner there.

Wm. Hanbury, Eſq; of *Kelmarſh*, in the road from *Northampton* to *Market-Harborough*, told me laſt week, that it was felt ſo violently there, that the miniſter and the congregation went out of the church; the roof of which ſeemed to be disjointed, and ready to fall: and his Lady, who was at home, leaning forwards to read, was ſhaken out of her chair upon the floor. I hear that ſeveral repeated vibrations of the ſhock were perceived at *Peterborough*; the particulars of which I expect ſoon from a gentleman who lives in that place.

In anſwer to your ſecond query, I find different accounts given by different perſons. Sir *Thomas Samwell*, Baronet, at *Braddon*, 3 miles W. of *Towceſter*, being in his garden with two of his ſervants, heard nothing of the exploſion. Some of my neighbours tell me they perceived it. Mr *Brookes*, our clerk of the peace, informs me, that he was walking abroad, at a conſiderable diſtance from any buildings, at a place called *Oakly*, 3 miles from *Kettering*, and there heard the noiſe as of a riſing ruſtling wind, during the time that he walked 20 or 30 yards.

The difference of theſe accounts may, in my opinion, be reconciled, by ſuppoſing, that the exploſion might have been heard abroad in ſuch places where it was more violent; and not in others where it was leſs ſo.

As for any thing (I preſume you meant lambent flame, vapour, *&c.*) being perceived on the ſurface of the ground, before or during the earthquake, nothing of this kind has as yet been mentioned to me from any quarter.

I find there has been a report of a meteor, like a ball of fire, appearing in the morning before the ſhock was felt; but it is, by the judicious part of the world, ranked among the other *mirabilia* uſually invented upon theſe occaſions to amuſe the vulgar.

5. The ſhock of an earthquake lately felt here, which has been ſo much talked of, and in ſome public papers magnified far beyond the truth, happened on *Sunday*, *Sept.* 30. about 20′ after 12. Our *Mercury* ſtrangely fixed it at $\frac{1}{4}$ before one; which is ſo palpable a miſtake, and contrary to the certain knowledge of ſo many hundreds of people, that I could not but be ſurprized to ſee it.

— *in a letter from the Rev.* P. Doddridge, *D. D. to Mr* Henry Baker, *F.R.S.* Ibid. p. 712. *Read* Oct. 25. 1750.

The effects here were by no means ſo conſiderable as were repreſented, eſpecially at *London*. I was at that time in company with a pretty large number of friends, juſt returned from divine worſhip, and hardly ſet down in the parlour; but no one of us felt any thing of it; and if we heard any noiſe, did not diſtinguiſh it from a coach: but ſome gentlemen, who were retired into their ſtudies up two pair of ſtairs, plainly felt it; yet they were not the twelfth part of the perſons then in my houſe, who all, whether on the ground, or firſt floor, were quite inſenſible of it. However, it is certain that a great number of perſons in different

different parts of the town, perceived themſelves lifted up by it, as they were in their houſes, though hardly any in the ſtreets took notice of it. Thoſe that accurately obſerved it, deſcribe it as ſomething horizontal rather than perpendicular. A Lady of my acquaintance ſtanding with her face to the S. W. plainly felt her heels lifted up, and was thrown ſo much on her toes, that ſhe was in danger of falling, : and it was obſerved, that ſome caſements were moved outward, as if an attempt had been made to force them open, and the clattering of ſaſhes was as when a ſtrong wind blows againſt them.

In the long ſtreet that runs from S. to N. it was obſerved, that the ſhock was felt more on the eaſtern than the weſtern ſide of the way; and I think the whole eaſtern part of the town was moſt affected. Dr *Stonehouſe*, who lives in that part of it, felt it with great violence, as if a loaded waggon had run ſtrongly againſt the gable end of his houſe: and tho' the walls are remarkably thick, he was greatly alarmed with an apprehenſion that they would have fallen.

What further confirms this remark of the horizontal, or at leaſt oblique direction of the impulſe is, that a cradle was rocked by it. In the houſe of Mr *Yeoman*, where our little philoſophical ſociety meets, it threw down a board from the teſter of a bed; yet Mr *Yeoman* himſelf did not feel it.

There was a report, that in *Abington-ſtreet* ſome chimnies were thrown down; and this brought numbers of people from different parts of the town, to ſurvey the ſuppoſed ruins; but it only ſerved to illuſtrate the uncertainty of rumour. However, it was true that a few bricks were thrown down from a chimney in *College-Lane.*

It is very certain, that all who felt the ſhock heard a hollow ruſhing noiſe; which, ſo far as I can learn, ſeemed to come in a direction from the S. W. to the N. E. In rooms where ſeveral perſons were together, ſome were ſtrongly ſenſible of it, while others felt nothing at all: and (*cæteris paribus*) I think it was felt more ſenſibly by thoſe above than thoſe below, and by ſuch as were ſitting, ſtanding, or leaning, rather than walking.

A lumbering kind of noiſe was heard by ſome in lower apartments, as if ſome one over their heads had fallen down on a ſudden, with a dead weight; and ſome thought they heard ſuch a noiſe in the floor beneath; ſome thought the quivering of the ground continued longer than others apprehended; but I have met with none who in this reſpect were ſo accurate in their obſervations, as my ingenious friend Mr *Shippley*, who aſſures me that he felt four diſtinct concuſſions (the ſecond and third of which were much more violent than the firſt and laſt) all within three or at moſt 4″.

As far as I can learn from the moſt diligent inquiry I can make, the tremulation of the ground extended itſelf at leaſt 60 miles in length from S. to N. and from W. to E. about 25, or at moſt 30.

It

It did not affect either *Newport-pagnel*, or *Towcester*, to the south; but was felt very near the latter, and at least 6 miles S. of this place, and all the way between that and *Nottingham*, and a little beyond it eastward; it scarce reached *Higham-Ferrers*, and was not felt at *Coventry*, and but very feeble within 5 or 6 miles to the E. of it: but its greatest violence seemed to have been spent on the villages of *Creaton*, *Cottesbrock*, *Kilmarsh*, *Maidwell*, *Evefton*, and some other small towns within 4 or 5 miles of *Market-Harborough*, mostly between us and that place.

At *Creaton*, a friend of mine was so moved, as he sat at dinner, that his elbow struck against the wall, tho' he sat at some distance from it; the roof of the house gave a great crack; and in a neighbouring house a brass kettle was thrown down, as in another a plate of pewter was.

At *Cottesbrook*, *Kilmarsh*, and *Weston*, the congregations, which were not yet come out of their respective churches, were all exceedingly terrified; some shrieked out, others quitted the place; and the worthy clergyman at *Weston with Sutton* near *Harborough*, says, in a letter published in our *Mercury*, dated the 2d instant, " that, as they were sing-" ing after sermon (he adds, a quarter before one) the whole congre-" gation were thrown into the utmost consternation, so that the singers " could hardly prevail on themselves to finish the anthem." He adds, " that he thought that part of the church betwixt the chancel and the " pillar next to it, would have sunk into the earth; and that it was " attended with a loud and dreadful noise, from a sort of subterraneous " explosion."

At *Maidwell*, Mr *Scawen*, leaning upon a large marble chimney-piece, was violently shaken; and in the neighbouring parish of *Kilmarsh*, Mrs *Hanbury*, who was then reading by her fire-side, her chair being tilted forward, was thrown down on her hands and knees; and the whole parish at church were so alarmed, that they broke up the assembly, and ran out into the church-yard; but the Minister persuaded some of them to return, and dismissed them (as I am told) with an *extempore* prayer, proper to the occasion.

Some strange stories have been told of much more violent effects produced elsewhere; particularly that a chasm was opened at a garden at *Daventry*; but I can find no real foundation for them.

No building, that I can learn, has any-where been thrown down; but I am very credibly informed, that a beam in the new toll-house near *Harborough* was split by the shock: and one tragical effect is certain; I mean, that Mrs *Allcock*, wife to the chief gentleman in *Loddington*, who had been delivered of her first child a few days before, and was in a very fine way, was so alarmed with the accident, that she expired within a few hours, to the great grief of all that knew her.

I was surprized to see how little the inhabitants of *Northampton* were impressed with this awful (though by no means supernatural) event: the sound of such a shock was, in a manner, grown familiar to their ears, by by what they had heard from *London*, and other places. Many did not themſelves perceive it; others found it very gentle; and in a very few hours it ſeemed to have affected them no more than a ſhower of rain.

I obſerve, that moſt accounts from the northern parts date the ſhock later than we felt it here; but that may perhaps be accounted for by the difference of clocks; but where they were moſt exactly adjuſted, all agree pretty well as to the time. I find alſo, that the degree of the ſhock was very different in nearly contiguous places. Thus at *Eaſton-Maudit* it was hardly felt at all; but at *Caſtle-Aſhby* was very violent. It was ſenſibly perceived at Lord *Pomfret*'s, and not at all at *Towceſter*, tho' within ¼ a mile of it; and, generally ſpeaking, the higher places were moſt affected, tho' in ſome it was quite otherwiſe.

It had been calm cloudy weather for ſeveral days before; and what little wind there was to be perceived was generally N. W. The height of the Barometer was as follows, on and about this remarkable day; *viz.*

Thurſday, *Sept.* 27.	29—95
Friday, *Sept.* 28.	29— 9
Saturday, *Sept.* 29.	29—88
Sunday, *Sept.* 30.	29—79
Monday, *Oct.* 1.	29—68
Tueſday, *Oct.* 2.	29—80
Wedneſday, *Oct.* 3.	29—83

The morning on which this *phænomenon* happened was remarkably calm; but quickly after the ſhock the wind roſe, and clouds which had covered the Heavens for ſeveral days, were pretty much diſperſed. On *Monday*, and on *Tueſday* and *Wedneſday*, the ſun ſhone clearly all day long. There was a report, that, on the morning of that *Sunday*, about 4 o'clock, a ball of fire was ſeen; but I could not trace it to any certainty. On *Monday* night the ſky in the eaſt was as red as blood; and, on *Tueſday* night, we had abſolutely the fineſt *Aurora Borealis* that I remember to have ſeen; of which I ſhall add a ſhort account, by way of poſtſcript, when I have added a miſcellaneous circumſtance or two to thoſe I have mentioned concerning the earthquake.

I am told, that, in ſome places, two ſhocks were felt, nearly at the ſame time, and within a few ſeconds of each other. This is ſaid to have been the caſe at *Noſely* in *Leiceſterſhire*, which ſtands very high; and at *Yelvertoft* in this county. But I have received the moſt certain account of this from *Hill-Morton*. Mr *Pool*, who keeps the turnpike there, and is remarkably curious, for a man in his ſphere of life, informs me, that he felt himſelf moved, as he ſat in his chair, in ſuch a manner, that he thought ſomebody had been at the door; or (if I underſtand him right) as if ſomething had fallen againſt it: and when he came to the door, about 2 or 3″ after, he felt a motion that he certainly knew to be an earthquake.

I do

I do not hear of any thing ſeen in or upon the ground; unleſs the caſe of a good woman at *Welden* may be accounted an exception; who ſays, that, while ſhe was ſhaken by it, ſhe ſaw the ground move around her.

Mr *Scawen* is very confident, that he heard that ruſhing noiſe (ſo generally ſpoken of by all who obſerved any thing extraordinary) not only before, but after the ſhock; and that he could by both trace the direction mentioned above.

I ſhall only add, that a very worthy lady of this town (niece, as I remember, to Sir *Hans Sloane*) obſerved, that, juſt before the ſhock, her birds drooped remarkably, and hid their heads under their wings: a circumſtance which is often obſerved in *Italy*, and other places where theſe *phænomena* are frequent.

Theſe are the moſt material circumſtances I have yet had an opportunity of collecting; and if any thing elſe, which ſeems at all worthy of notice, ſhould occur, I ſhall be very ready to impart it.

P. S. The principal circumſtances attending the *Aurora Borealis* mentioned above, were as follows:

On *Tueſday*, the ſecond of this month, as I was walking home from a neighbouring village, between 6 and 7 in the evening, the ſky being remarkably clear, and the moon then ſhining with delightful luſtre, I happened to turn, and obſerve in the N. E. a pretty large cloud, nearly in the form of a globe; which ſeemed firſt of a whitiſh, then of a very luminous appearance. It ſeemed connected with a cloud, which was dark, and ſomewhat bluiſh, and ſpread itſelf (almoſt like a vaſt beam of a building) from the N. E. to the N. W. At each end it was a little bent towards the horizon; but by far the greater part was in a horizontal poſition, and ſeemed to occupy the northern part of the Heaven, from the altitude of 10 to about 15°. The cloud I firſt mentioned ſoon appeared like a globe of lucid fire, much brighter than the moon; and ſhot coruſcations, ſometimes in a perpendicular direction, but much more towards the dark horizontal beam, if I may be allowed ſo to deſcribe it (thro' which, by the way, I could not then diſcern any ſtars). Thro' this the light darted from N. E. to N. W. ſo that at length it ſeemed all in a blaze; and from it there ſhot up ſeveral luminous pillars, perpendicular to the horizon, and directed towards the zenith. They were of very unequal lengths, and ſome of them appeared in a conical rather than a cylindrical form. As thoſe to the W. brightened, thoſe firſt raiſed diſappeared; till at length all the horizontal track of light vaniſhed, and ſome long truncated pillars, often varying their length, remained in the weſt; often riſing almoſt to the zenith, but generally ſeeming to hang between 30 and 70°, ſo far as I could conjecture. When this beautiful appearance ceaſed, the ſky appeared reddiſh in the eaſt; what before conſtituted the lucid globe, ſeemed reſolved into light clouds, of various

 forms

forms; and that part of the horizon looked much as it does in a ſummer's morning, when the ſun is within a few minutes of its riſing, and tinges the clouds of a light red. But all this ſeemed to diſperſe in a few minutes, about 7; and I neither ſaw nor heard of any thing remarkable.

—by Mr —ſteward to the earl of Cardigan. *Dated* Deene, Oct. 2. 1750. *Read* Oct. 25. 1750.

6. In the morning, ¼ before one, there was a gentle weſterly wind, ſomething cool; but for ſome time before the earthquake happened, it was quite calm and cloſe, and much warmer. The air was very dry, and filled with clouds that had no motion, but prevented the ſun's appearing (I think) all day. The noiſe that preceded the earthquake was, for a few ſeconds, like the rumbling of a coach upon a bridge, or thunder at a diſtance, when there were two conſiderable exploſions very near one another, which gave the great ſhock; and after that the noiſe continued as before for about half a minute, the earth trembling all the while; but I don't find any body perceived any ſulphureous ſmell.

The direction of the earthquake was from W. to E. as was very eaſily diſcerned by every body that was out of door, as I was, and took notice of the noiſe.

It is probable it began in *Derbyſhire*, or ſome of the counties to the W. of that (for I am informed it was felt as much at *Derby* as here, and at all places between); and paſſed off the iſland thro' *Lincolnſhire*, and part of *Cambridgeſhire*.

The breadth from N. to S. I imagine to be 40 or 50 miles; of which much the greateſt part lay N. of this place.

The force of the ſhock was chiefly, if not intirely, lateral; and ſo conſiderable, as that ſeveral people, who were ſitting in chairs, catched at the walls, tables, and ſuch things as ſtood next them, expecting they ſhould be thrown down: buildings of all kinds were ſhaken greatly; and the beds, chairs, and ſuch things as ſtood above-ſtairs were diſplaced, and rocked about very much: windows were ſhaken as if they would have been broken; and in ſeveral places pewter upon ſhelves in kitchens thrown upon the floor.

At ſeveral churches, where divine ſervice was not finiſhed, both in this county, *Rutland*, and *Leiceſterſhire*, the people were ſo alarmed, that they ran out, fearing the churches would fall on their heads; and ſome were ſo terrified, they ſwooned away.

At *Stonton*, ſome of the plaiſtering of the church was ſhaken down; which moſt terribly frightened the people that were in it, and obliged them to run out. ——— I have not heard of any damage being done by it more than ſome chimnies thrown down, but nobody hurt by them.

P. S. *Deene* ſtands cloſe to the road which leads from *Northampton* to *Stamford*; it is about 25 miles from the former, and 10 from the latter.

7. The

7. The firſt ſhock appeared to us at about half an hour paſt 12, at our houſe, as if a large ſtack of chimnies had fallen through the roof upon the chamber-floor over our heads (which at firſt we took to be the caſe, but in a very few ſeconds recollected what it was). This was ſucceeded by a prodigious rolling noiſe, as if the whole houſe was falling upon us, which we expected it would do, before we could get out of it. When we were out, we could not help looking behind us, to ſee if any thing was fallen; but found all ſafe. We could perceive the floor, *&c.* to ſhake very ſenſibly; and a walking-ſtick, that ſtood in one corner of the parlour, was thrown down: it alſo ſhook down ſeveral large ſtones from off an heap that lay in the yard.

— *in a letter from Mr* Hen. Green *to Mr* James Ayſcough, *Optician, in* Ludgate-Street. Ibid. p. 723. *Dated* Rolleſton *in* Leiceſterſhire, Oct. 22. 1750. *Read* Oct. 25. 1750.

A gentleman, who was walking from his own houſe to dine with us, happened to lay his hand upon a gate, to open it, juſt after the firſt noiſe, and found the gate-poſts, *&c.* to ſhake and totter about as if they were falling. In a great many places ſervice was not ended at church; and in ſome the whole congregation ran out, and happy they that could get out firſt. A gentleman of fortune near *Leiceſter*, narrowly eſcaped being killed by one of his own chimnies; which fell ſo near him, that ſome of the bricks grazed upon his ſhoulder.

By what I can hear, it reached 30 or 40 miles from us each way; and I fanſy we were not far from the center of it.

As near as I can judge, it laſted about 20'' at leaſt; but it's duration and appearance was different to people in different places.

8. The Rev. Mr *Daniel Goodrich*, at *Oundle* in *Northamptonſhire*, has ſent me an account of the earthquake felt in that and the neighbouring counties, *Sept*. 30. at 12 ½ at noon.

— *in a letter from the Rev.* Hen. Miles, *D. D. F. R. S. to Mr* H. Baker, *F. R. S.* Ibid. p. 726. *Read* Nov. 1. 1750.

He informs me, that he was then at *Uppingham* in *Rutlandſhire*, ſitting in a room intent on writing and thinking, when he was ſurprized with a ſound very ſtrong and awful; which, at the firſt moment, made him think of the rattling of a coach upon the pavement: but this apprehenſion was immediately corrected by ſomewhat very different in the ſound, and raiſed in him an idea of the craſhing of a falling houſe. He felt himſelf in a very ſhaking way: the table at which he ſat ſhivered, and the windows of the room jarred: but he did not think of an earthquake, till the people of the houſe came into the room, and told him what had happened. Some ſlates were ſhaken off the houſes, and in one houſe the hammer of a clock ſtruck the bell: ſome chimnies were thrown down; many talked of a ſenſible heaving of the ground: but, he ſays, he is credibly informed, that, in one houſe in that county, the mud floor was cracked not a little in 3 or 4 places. In one houſe, in the town of *Uppingham*, where two men and a woman were ſitting, upon the approach of the ſound (tho' they had no thought of an earthquake), the men could hardly draw their breath in the houſe; but were immediately obliged to go out for freſh air; but the woman felt no diſorder. My friend adds, that attentive obſervers apprehended the ſound to have moved from the N. to the S. or from N. W. to S. E. and

 that

that himſelf had the ſame apprehenſion; and that, according to his preſent intelligence, the whole ſhock was felt in the counties of *Northampton*, *Leiceſter*, *Nottingham*, *Rutland*, and *Lincoln*, affecting a track of the county of about 60 miles in length, and as much in breadth. I have given you the account in his own words, with very ſmall variation.

—a letter from the Rev. Mr John Nixon, *F.R.S. to the* Preſ. *ſerving to accompany a letter from Mr* William Smith *to Mr* Nixon, *giving a very particular account of the* Earthquake *felt on* Sept. 30. 1750. Ibid. p. 727. *Dated* Higham, November 8. *Read* Nov. 15. 1750.

9. This waits upon you with a letter I have juſt received from *Peterborough*. I hope the veracity of my correſpondent, who is Regiſter of the place above-mentioned, and a gentleman of good credit, will atone for ſome defects in the form of his account.

Since I had the honour to write to you laſt, I have met with two gentlemen in my neighbourhood, one of them a clergyman, who aſſured me, that, about 6 or 7 on the morning before the late earthquake, they both ſaw a ball of fire in the air, reſembling the meteor commonly called a falling ſtar; only with this difference, that this phænomenon, after running ſome ſpace, burſt into ſeveral ſtreaming rays, conſiſting of ſparks of fire, in the manner of a ſky-rocket.

Reverend Sir,

I Received your favour of the 16th current; and the following account of the late earthquake, as it was variouſly felt here, is an anſwer thereto; which (as I could gather it from others, and collect of myſelf) pleaſe to take as follows:

Some little time after morning ſervice at the cathedral church was over, as near as I can gueſs about 25 or 30 minutes paſt noon, as I was in my garden with two friends, it being exceedingly calm, the ſky ſomewhat covered with light mottled clouds, I took notice of a hollow odd noiſe, and at firſt judged it to be a coach under the monaſtery walls; but ſoon found it was otherwiſe; and all of us then judged it to be thunder at a diſtance, but pretty loud, and of a hollow report. I was then 40 yards at leaſt from my own houſe, and about half that diſtance from Mr Archdeacon *Browne*'s, and not any buildings to the S. of us. I heard the noiſe of the exploſion at leaſt 2′ after I took notice of it. It's courſe, as I could plainly diſtinguiſh, was from N. E. to S. W. or thereabouts. We felt no ſort of ſhake, nor did my wife or family in the houſe. My next neighbour to the S. of me, and his family, being at dinner, felt the ſhock after the noiſe had partly paſſed, and were ſenſible what it was; but not very much, and but for a ſhort ſpace of time.

A perſon of very good reputation, who lives in the *Minſter-Cloſe*, was then in a chamber, heard the noiſe, ſuſpected the cauſe (it being *Sunday*, and no carr about), immediately threw up the ſaſh, and obſerved, as the noiſe of the exploſion decreaſed, the ſhock came on: the tables, chairs, *&c.* in the chamber ſhook; the windows clattered: he very ſenſibly felt the ſhock, which he affirms laſted a minute at leaſt; only,

only, when it firſt ſhook, it was with the greater violence, and dwindled away by little and little, till 'twas gone, in the ſame manner as went the noiſe. The people below were all affected by it; but not ſo much, or ſo long. One other perſon in a narrow ſtreet in the town, ſitting in a low room, with a brick floor, heard the noiſe, but judged it to be an odd lumber above ſtairs, or ſome carriages coming on (as moſt people firſt thought who were within doors); and preſently the floor under his chair heaved, and continued in a ſurprizing agitation for 1″. He was very much alarmed, and ran into the ſtreet, where a great number of people inſtantly appeared; ſome to ſee if any coaches or carriages were coming, others to get away, expecting their houſes were tumbling; and others finding ſomewhat extraordinary had happened, but at that inſtant did not know what, and came to ſee, *&c.* The perſon who felt the ground heave was ſo frighted, that he became ſick thereupon. At *Longthorpe*, a mile W. of us, the clergyman who did duty there was juſt then ſat down to dinner with Sir *Francis St John*, Bart. and his daughter, when a rumbling noiſe was heard, particularly in the chimney. Sir *Francis* expected the ſame was on fire, got up to ſee; which ſuddenly ceaſed, and immediately was ſucceeded by a concuſſion of the ground. I had this from the clergyman, who tells me further, that he felt it ſo ſenſibly, that he was obliged to relinquiſh his chair, and, when upon his legs, to lay his hands on the table to ſupport himſelf. Miſs *St John* was in like manner affected. Sir *Francis* only heard the noiſe, which he compared (and, for what I know, very properly) to the exploſion of a cannon at a diſtance, not being in the leaſt ſenſible of any motion under foot, altho' the diſtance between the parties was very inconſiderable. The ſideboard, with the clattering of glaſſes, *&c.* they thought was tumbling: they were ſo ſurprized, that he doth not remember how long the trembling continued. A perſon in the ſame village affirms, that, being ſitting, he was ſuddenly lifted twice or thrice on the ground, as with a ſpring, and dropped again. Many people felt it there in various ſhapes. At *Caſtor*, a mile and half ſtill farther W. one Mr *Serjeant* ſays, that, looking out of a window a conſiderable height, he found the houſe reel more than once, and then come into it's place again with a jolt. Many very odd inſtances we have of it. Some heard the noiſe, and felt not the ſhock; others felt it, and did not hear the noiſe. I am informed it was felt at *Boſton*, which lies about 30′ near N. of us; and it was felt a few miles to the S. So that its extent here, from N. W. to S. E. or thereabouts, ſeems to be about 40 miles.——Upon the whole, I find, the higher one was, as farther from the centre, the more the ſhock was felt; that it was local; the ſound of the exploſion was heard as well abroad as in the houſes, though people differently ſituated judged differently what the ſound was; that not any ſmoke, vapour, or flame, appeared on the ſurface, as I have heard.

XXXIX.

Part of a letter from M. de Reaumur, *F. R. S. to the* Pres. *concerning an* Earthquake *felt in* France, Oct. 11. 1749. Ibid. p. 691. *Dated* Paris, April 23. 1750. *Read* May 17. 1750.

XXXIX. On *Saturday, Oct.* 11. 1749. about 7 in the evening, there was an earthquake in *France*, which, according to my own inquiries, reached one way above 60 leagues in extent, from our coast of *Poictou* beyond *Luçon*, as far as the neighbourhood of *Blois*. I was then at my own house at *Reaumur*, and sitting in a closet on the ground-floor, where I had only notice of it by a noise like to the rattling of a coach over a rough uneven pavement, which seemed to grow stronger, as at different fits, for about a minute and a half. I went out of my closet to inquire what others might have felt, as I had not been at all shocked myself, in other parts of the house; and I met with some ladies just come in a fright from the apartments up one pair of stairs; and a learned Father of the *Oratoire* that was with me, and was just then run down, upon it's shaking, from a turret, on the top of the house, the motion of which had very much surprized him.

In the village several people also felt the shake; but others, that happened to be employed, were not sensible of it. I was informed, that, in some other villages and country-towns, it was more considerable than where I was; but I did not hear that it did any-where any mischief worth speaking of.

Extract of a letter from Dr Mackenzie, *to Dr* Mead, *F. R. S. concerning* Earthquakes *at* Smyrna. Ibid. p. 700. *Dated* Constantinople, May 23. 1750. *Read* July 5. 1750.

XL. I observe, in Mr *Touchit*'s Journal, that a certain ingenious gentleman would not allow the last shock of an earthquake in *London* to be an earthquake, because it was not central; but rather calls it an airquake, because it was lateral. I have felt many shocks, since I have been in this country; particularly in *Smyrna*, 1739. when, after the great shock on *March* 24. there were some shocks every day for a month; so that few or none went into houses before the latter end of *April*: when I had time enough to observe their motion, which was regularly periodic, or about the same hour every day. But I must differ from this unknown gentleman's opinion so far as to say, that every one of those shocks was lateral, moving from the S. to the N. and, as near as I could judge, the motions formed acute angles, somewhat resembling the motion of lightning.

On the causes of Earthquakes, *by the Rev.* W. Stukely, *M. D. & F. R. S. to the* Pres. Ibid. p. 641. *Read* March 22. 1749.

XLI. 1. When so great and unusual a *phænomenon* as an *earthquake*, and *that* repeated, happens among us, it will naturally excite a serious reflection in every one that is capable of thinking; and we cannot help considering it in a philosophical as well as religious view. Any mind will take the alarm, when we perceive a motion that affects the earth, that bears the whole city of *London*, and some miles round; and at the same time, whilst it gives us so sensible a shake, so gently sets us down again, without damage to any buildings, and without a life lost.

In the works of Nature and Providence there are no degrees of *Great* and *Little*: comparisons are incompatible; nevertheless we ourselves are more affected with what seems great, in our own apprehensions; but an OMNIPOTENT POWER admits of no distinctions; and whilst pro-

prodigious effects are produced from cauſes imperceptible, it rightly claims our ſerious attention, as well as wonder; nor need we loſe ſight of the theological purpoſe of theſe amazing alarms, whilſt we endeavour to find out the philoſophy of them.

Permit me, then, to throw in my thoughts on the cauſe of earthquakes I did not enter into the common notion of ſtruggles between ſubterraneous winds, or fires, vapours, or waters, that heaved up the ground, like animal convulſions; but I always thought it was an electrical ſhock, exactly of the ſame nature as thoſe, now become very familiar, in electrical experiments.

When we reflect on the unuſual winter now paſt, beyond what occurs to any one's memory, that it has been dry and warm to an extraordinary degree, the wind generally S. and S. W. and *that* without rain, we may, with much reaſon imagine, that the earth has been in a ſtate of Electricity, ready for that particular vibration wherein Electricity conſiſts.

And that it has been ſo, we may further conclude from the extraordinary forwardneſs of vegetation, from the frequency of the northern lights, and eſpecially of that called *Aurora auſtralis*, which are with us infrequent, and twice repeated, juſt before the earthquakes (being of ſuch colours as we had never ſeen before), and removed ſouthward, quite contrary to thoſe common with us.

Add to this, that ſome foreigners among us, from *Italy*, and thoſe parts, where earthquakes are frequent, obſerving theſe lights, and the particular temper of the air, did actually foreſee the event of an earthquake. All theſe matters concur, in ſhewing, that the earth was in a ſtate of Electricity, beyond what has ever been in our memory.

Admitting this, there is nothing wanting, to produce the wonderful effect of an earthquake, but the touch of any non-electric body; and that muſt neceſſarily be had *ab extra*, from the region of the air, or atmoſphere.

We had lately a very pretty diſcourſe read here, from Mr *Franklyn* of *Philadelphia* *, concerning thundergufts, lights, and like meteors. He well ſolves them by the touch of clouds, raiſed from the ſea (which are non-electrics), and of clouds raiſed from exhalations of the land (which are electrified): that little ſnap, which we hear, in our electrical experiments, when produced by a thouſand miles compaſs of clouds, and *that* re-echoed from cloud to cloud, the extent of the firmament, makes that thunder, which affrightens us.

From the ſame principle I infer, that, if a non-electric cloud diſcharges its contents, upon any part of the earth, when in a high-electrified ſtate, an earthquake muſt neceſſarily enſue. As a ſhock of the electric tube in the human body, ſo the ſhock of many miles compaſs

* Read *Nov.* 16, 1749. publiſhed with other tracts on Electricity; by Mr *Peter Collinſon, F. R. S. London* 1750. *8vo.*

of

of ſolid earth, muſt needs be an earthquake; and that ſnap, from the contact, be the horrible uncouth noiſe thereof.

I have been informed, by thoſe who were up, and abroad, the preceding night, and early in the morning, that coruſcations in the air were extremely frequent (which confirms us in the notion of the earth's being then in an electrified ſtate); and that, a little before the earthquake, a large and black cloud ſuddenly covered the hemiſphere; which probably occaſioned the ſhock, by diſcharge of a ſhower.

It may be ſaid, that, if this were the caſe, earthquakes would happen much oftener than we find them. It may be anſwered, that they probably do, much oftener than obſerved: but *ſlight* ones; becauſe of the earth's being *ſlightly* electrified. And ſuch a winter as this has not been known before; to which we attribute the preſent earthquake.

The reaſon is obvious, why earthquakes are not ſo frequent with us, and the northern regions in general, as in *Italy*, and more ſouthern climes; and a due conſideration of it confirms our reaſoning. All Electricity requires great *dryneſs* and *warmth*; and I doubt not but earthquakes, of a ſmall degree, have and do frequently happen. And many people now recollect, that they have been ſhaken in their beds; though they took no notice of it then, having had no experience of an earthquake.

All that we have ſaid upon the ſubject receives great ſtrength from *this* particular, that water ſtrengthens and conveys the force of Electricity. From whence we may account for that obſervation, that the moſt dreadful effects of earthquakes are always felt in maritime towns; as *Port-Royal* in *Jamaica*, *Lima* in *Peru*, *Meſſina* in *Sicily*, *&c.* And *here*, we find plainly, that the ſhock went along the river, both upwards and downwards, farther than by land; like the bottle of water held in the hand, in electrical experiments.

We argue the ſame from the ſickneſſes, pains in the joints and back, rheumatic, hyſteric, nervous caſes, head-ach, colics, and the like; which many people, eſpecially of the weaker conſtitutions, felt, for more than one day after: juſt as after electrification.

But from hence it is highly worthy of remark, that the finger of PROVIDENCE is notoriouſly diſcernible herein;

——— *of* HIM,
Who guides the Thunder, *and directs the* Storm.

Tho' it operates by natural cauſes, yet it is *that* which gives them their deſtination. For, though the coaſts of the ſea are moſt liable to, and ſuſceptible of, this mighty ſhock, which we call an earthquake; yet the chaſtening rod is directed to *towns* and *cities*, where are inhabitants, the objects of it's monition; not to *bare cliffs*, and an *uninhabited beach*. And there cannot be a more direct proof, that earthquakes are divine judgments, than *this* obſervation: for, in all antient hiſtory, earthquakes are ever found in great cities. *A. D.* 17, no leſs than 12 flouriſhing cities

cities in *Aſia minor* were deſtroyed in one night. In *A. D.* 1456. at *Naples*, 40,000 people periſhed by an earthquake. In 1531. in the city of *Liſbon*, 1400 houſes were thrown down.

We ſee and admire the effects of Electricity, and its ſtupendous properties, every day; which ſeems as it were an animating ſoul to matter. The Antients had a notion that the earth was a great animal; probably from ſome obſervations of Electricity; but certainly, when in our days we feel theſe unuſual and extraordinary convulſions of nature, it is a leſſon to us, to do our duty toward that Great Being, who, by a drop of water can produce effects ſo prodigious.

2. Among all the appearances of nature, which are the ſubjects of the inquiries of the *Royal Society*, none would more ſenſibly affect the minds of contemplative perſons than that of an earthquake; eſpecially to us in this country, where they ſo ſeldom happen. —*by the ſame.* Ibid. p. 657. *Read* April 5. 1750.

When I laid the preceding paper before the *Society*, I found that ſome worthy members had not fully entered into my way of reaſoning; nor with that ſerіouſneſs ſo awful a ſubject required: therefore I judged it neceſſary to treat upon it in a more diffuſive manner; and with ſome further conſiderations relating to that argument.

Among the numerous accounts received here, and obſervations upon the manner of it, I judged it became the *Society* to inquire into the cauſe of ſo extraordinary a motion, of which we could not have formed a proper idea, had we not repeatedly both ſeen and felt it.

The notions of the Antients are ſufficiently known; nor have the moderns any-way improved upon them, any further than by ſome chymical mixtures. The vulgar opinion goes no deeper than ſome caverns, not far below the ſurface of the earth; wherein are ingendered vapours, exploſions, fermentations, and fires from inflammable minerals, that cauſe theſe convulſions of the ſurface.

I ſhall not pretend to deny, that there may be ſuch vapours, fermentations, rarefactions, and inflammable ſubſtances, and actual fires, in the bowels of the earth, and that there may be ſome caverns underground, as well as we find ſome few above-ground, *Pool's Hole*, *Okey Hole*, and the like, in mountainous countries. We know there are hot ſprings running continually, and *vulcano's* frequently belching out flames and ſmoke; and to theſe moſt probably, ſome ſmaller earthquakes are owing.

But theſe matters are very rare, much rarer than earthquakes, both as to time and place. *Veſuvius* in *Italy*, and in that part of it abounding with mines of ſulphur; *Ætna* in *Sicily*; ſome on the great *Andes* mountains in *America*, and the like. The ſcarcity of them, in my opinion is ſo far from being a proof of the general cavernous ſtate of the earth, that it ſtrongly proves the contrary.

How many thouſand acres of coal-mines, ſtone-pits, and the like, do they daily work in *England*, and have done for ages? I have been myſelf 100 yards deep in a ſalt-rock. I have walked half a mile length-wife, directly into the earth, and under the bed of the ocean: but we never hear, from the many hundreds of thoufands of workmen in this kind, of the cavernous ftate of the earth.

On the contrary, by their hard labour they confefs it far otherwife. Nor have we any reafon, in *England*, to believe there are great mines of fulphur, and inflammable minerals: nor, if there were, could they burn, and caufe convulfions of the earth, unlefs there were proper cavities, and conveyances of air; as in coal-pits, when fet on fire.

But even from thefe coal pits, when fired, do we ever find any thing like an earthquake produced? Nor did we obferve, in thefe two laft earthquakes, any fire, vapour, fmoke, or fmell; or any kind of eruption in the leaft, in fo great a ftruggle of the fuperfice, as affected a circle of 30 miles in diameter.

Indeed this confideration alone, of the extent of this furface, is fufficient to overthrow any fuppofition of earthquakes being *chiefly* owing to fubterraneous vapours. For it cannot poffibly be imagined, that fuch can have fo immenfe a force, as to act upon that compafs inftantaneoufly, all at once, and never break ground, fo as to be difcoverable to fight or fmell. Many accounts we have of a little fire-ball burfting in the air, at a great diftance, and inftantly propagating a fulphureous fmell all around it for miles.

If the motion of a fuperficies of 30 miles diameter was owing to fumes and vapours, we ought reafonably to find fome great difcharge of them, like a coal-pit fired: the operation of it ought to be hours and days in continuance, not inftantaneous: and the evaporation of fuch a quantity of inflammable matter requires a long time to evacuate itfelf.

There is another argument, which, in my opinion, utterly overthrows thefe fuppofitions; and that is, a due confideration of fprings. If we would form any tolerable idea of the nature of fprings and fountains perpetually flowing, and *that* (generally fpeaking) from the creation of the world, we muft needs conceive, that GOD ALMIGHTY has laid their pipes and canals in the earth, like as he has planted the veins, arteries, and glands, in an animal body; and that likewife they are more and more ramified, as they nearer approach the outward fhell of the earth.

The workmen in coal-mines, and the like, never fail to meet with the veins of fprings every-where: they that dig for wells feldom fail of finding water every-where. The colliers are obliged to drain, at very great and continual expence. A circumftance not very favourable to fubterraneous fires.

Now it is apparent enough, that the hypothefis of vapours, and fubterraneous fermentations, explofions, and eruptions, being the caufe of earthquakes, muft abfolutely ruin the whole fyftem of fprings and fountains, where-ever it has once been. But this is quite contrary to fact;

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even where an earthquake has been repeatedly; for inſtance, from home.

On *Wedneſday*, *April* 6. 1580. about 6 in the evening, juſt ſuch another earthquake was felt in *London*, and around it, as theſe two we have ſeen. Another, exactly ſimilar, in 1692. In all theſe 4 no houſes thrown down, nor any ſprings diſturbed thereby: no ſenſible eruptions or ſmells.

Theſe conſiderations I apply only to this little inconſiderable ſpace of a circle of 30 miles diameter. But what is that to the appearance of ſome earthquakes we read of in hiſtory? In the year of our Lord 17. no leſs than 13 great and noble cities of *Aſia minor* were deſtroyed in one night. The fact is ſo notorious, that ſome perſons here preſent have ſeen a vaſt block of white marble, now ſtanding near *Naples*, being the pedeſtal of a coloſs ſtatue of *Tiberius* the emperor, having carved on it, in *baſſo relievo*, the *genius's* of all thoſe cities, with their names; which were rebuilt by that Emperor.

Without going ſo far, we may ſee another evidence of it, a coin of that Emperor ſtruck upon it, with this inſcription,

CIVITATIBUS ASIAE RESTITVTIS.

I have one of them in large braſs, which was found at *Colcheſter*.

The compaſs of this earthquake may be reckoned to take up 300 miles in diameter. How can we poſſibly conceive the action of any ſubterraneous vapours to produce ſuch an effect, as inſtantaneouſly to demoliſh all theſe cities? And that ſuch an accident ſhould never happen after? How comes it to paſs, that the whole country of *Aſia minor* was not at the ſame time deſtroyed, it's mountains renverſed, it's fountains and ſprings broken up, and ruined for ever, and it's rivers diſannulled? Inſtead whereof, we find nothing ſuffered, but thoſe cities; no kind of alteration in the ſurface of the country, which remains the ſame to this day.

From theſe conſiderations therefore, I cannot perſuade myſelf eaſily to enter into the opinion of earthquakes *generally* ariſing from pent-up vapours and eruptions. I know there are many ſtrange relations of effects of ſubterraneous fires, told by authors that deal pretty much in the marvellous, and whoſe minds were prepoſſeſſed with thoſe vulgar notions. My conceptions of the matter are derived from the more general appearances, and what we have ſeen and felt ourſelves.

After we have treated this argument in a ſuperficial view, we muſt go a little deeper. If we would conſider things like Philoſophers, let us propoſe to ourſelves this problem;

Where is the power to be placed, that is required to move a ſurface of earth 30 miles in diameter?

To anſwer this, conſult the engineers, and thoſe that make mines in the ſieges of towns. They will acquaint us, that the effect of mines is produced in form of an inverted cone: and that a diameter of 30 miles in bafe, will require an axis of 15 or 20 miles to operate upon that bafe, fo as to fhake it, at leaft. So that the vapours, and whatever power we propofe to operate upon that bafe, according to the foregoing hypothefis, in order to form the appearance of an earthquake, muft be 15 or 20 miles deep in the earth.

But what mind can conceive, that any natural power is able to move an inverted cone of folid earth, whofe bafe is 30 miles in diameter, and axis 20? or, was it poffible, would not the whole texture of that body be quite difturbed and fhattered; efpecially in regard to it's fprings and fountains? but nothing like this is ever found to be the confequence of an earthquake, tho' fatal to cities.

Apply this reafoning to the earthquake of *Afia minor*; and this vigorous principle muft lie at leaft 200 miles deep in the ground. Enough to fhew the abfurdity of it. A cone of 300 miles diameter at bafe, and 200 miles axis; I dare be bold to fay, that all the gunpowder made fince it's invention, put together, would not be able to move it. How much lefs would pent-up vapours?

And, could it be admitted as a thing poffible, will any one be perfuaded, that fuch a fubterraneous tumult, of fo vaft an extent, will be no-ways injurious to the internal fyftem of fprings and fountains? We may as well imagine, that we may ftab a man 100 times, and never touch a vein or artery.

In an age when Electricity has been fo much our entertainment, and our amazement; when we are become fo well acquainted with it's ftupendous powers and properties, it's velocity, and inftantaneous operation, through any given diftances; when we fee, upon a touch, or an approach, between an Electric and a Non-electric, what a wonderful vibration is produced, what a fnap it gives, how a lambent flame breaks forth, how violent a fhock; is it to be wondered at, that hither we turn our thoughts, for a folution of the prodigious appearance of an earthquake?

It is every body's obfervation, that there never was a winter like the paft, for warmth and drinefs, thunder and lightning very uncommon then; for corufcations in the air, juftly thought to be electrical; efpecially for that called *Aurora auftralis*; the wind continually S. and S. W. and that without rain, which is unufual. This ftate of the atmofphere had continued 5 or 6 months, before the firft earthquake: is it not hence reafonable to conclude, that the earth muft, efpecially in our region, be brought into an unufual ftate of Electricity; and, confequently, wanted nought, but the approach of a non-electric body, to produce the fnap, and the fhock of Electricity?

That the earth was in that vibratory and electric ftate, we have further reafon to conclude, from the very extraordinary forwardnefs of all the vegetable world with us. Every one knows, that, at the end of *February*, all forts of garden-ftuff, fruits, flowers, trees, were as forward

as,

as, in other years, in the middle of *April*. Conformable to which, experiments abundantly ſhew us, that electrifying of plants quickens their growth ; for the ſame reaſon as in animals it quickens the pulſe.

Any ſolid matter is capable of being put into a ſtate of Electricity ; ſuch as iron guns ; and the more ſo, by reaſon of their ſolidity : and in proportion to it is the greatneſs of the ſnap, and of the ſhock ; and a kind of lambent flame iſſues out of the point of contact ; and likewiſe ſomewhat of the ſulphureous ſmell. So that if both flame and ſmell were diſcernible in an earthquake, it is to be found, without going to the bowels of the earth.

As to the immediate cauſe of this wonderful appearance of an earthquake, I hinted that it was owing to a non-electric body coming near or touching the earth, when in it's electrified ſtate ; which may be a ſhower of rain : and the learned Dr *Childrey* obſerves, that earthquakes always ſucceed rain : a ſudden tempeſt of rain, in the time of a great drought.

At the ſame time that the force of Electricity in ſolids is as the quantity of matter, we ſee moſt evidently, that water is equally forcible in ſtrengthening and conducting it, and that in proportion to it's quantity : which very much juſtifies my obſervation, that moſt frequent earthquakes have fallen upon maritime places. And I find the ſame obſervation is made before me by *Acoſta* and *Dolittle*, who wrote on that in 1692. and others. In the dreadful cataſtrophe of *Port-royal*, it is notorious, that it's violence was chiefly near the ſea : and even in thoſe ſo lately felt by us, they were ſenſibly more violent toward the river, than further from it. And in that earthquake in *England*, in 1692. (which was very much like that we are treating of) there were no houſes thrown down, nor perſons killed ; but it reached more particularly *Sandwich*, *Deal*, *Dover*, *Sheerneſs*, *Portſmouth*, and the maritime parts of *Holland*, *Flanders*, and *Normandy*.

In this that happened laſt *Sunday* at 6 in the evening at *Bath*, it was felt particularly at *Portſmouth*, the whole *Iſle of Wight*, and *Jerſey*.

If we look into antient hiſtory, we find, in the 197th year before Chriſt, an earthquake ſhook terribly the iſle of *Rhodes*, damaged many cities, and ſome were ſwallowed up.

17 years before Chriſt, many cities in the iſle of *Cyprus* were deſtroyed.

6 years before Chriſt, the iſle of *Coos* was moſt vehemently afflicted.

During the *Peloponneſian* war among the *Greeks*, the iſle of *Delos* was afflicted, and the moſt beautiful temple of *Apollo* thrown down.

Soon after, the city of *Lacedæmon* was totally deſtroyed.

A. D. 79. three cities in *Cyprus* were overthrown.

A. D. 182. the city of *Smyrna* was ruined.

Conſtantinople has often ſuffered ; particularly in 1509. 13000 people overwhelmed.

A. D.

A. D. 1456. in the city of *Naples* 40000 people were deſtroyed.

In 1531. at *Liſbon*, 1400 houſes were thrown down, and near as many ſhattered.

In the time of *Valens* the Emperor, a terrible earthquake happened in *Crete*, whereby 100 cities were deſtroyed.

But inſtances enough, to ſhew what I aimed at, that maritime places are moſt ſubject: which is no contemptible argument in favour of Electricity; when both the ſolid of the earth, and the quantity of the water, concur to make the ſhock exactly, as in common electrical experiments.

The gardener in the *Temple* garden obſerved the ſound to roll from the water-ſide toward *Temple-Bar*, before the ceaſing of the nodding of the houſes; juſt as the electrical ſnap precedes the ſhock. Others, that write upon earthquakes, commonly obſerve, that the noiſe precedes the ſhock. But it is obvious it muſt be quite the contrary, did the concuſſion depend on a ſubterraneous eruption.

We may well enough expect, that burſting vapours, and ſubterraneous exploſions, ſhould diſperſe every thing that happened in their way into the air: but, in my apprehenſion, it is not poſſible for us to imagine any thing to produce ſuch a vibration as we felt, but Electricity.

Several people felt pains in their joints, rheumatiſm, ſickneſs, headach, pain in their back, colic, hyſteric and nervous diſorders, for the whole day after, and longer (eſpecially weak conſtitutions), exactly as upon Electrification; and to ſome it has proved fatal.

Upon this principle alone can we account for the fiſhes leaping out of the ponds; or a ſort of thump felt at the bottom of a boat. Nay, we are told of earthquakes felt at ſea, far diſtant from land: which are eaſily ſolved by an electrical ſhock impreſſed upon the water: but we cannot eaſily ſee vapours and fires reſiding at the bottom of the ocean.

From electrical vibration alone can we account for that obſervation of ſprings and fountains being no-ways damaged after an earthquake. I doubt not but they run more plentifully at that time; juſt as the blood circulates quicker upon Electrification.

From Electrification only we can account for this particular. The walls of *Weſtminſter-Hall* are of no mean thickneſs; yet thoſe that ſat with their backs to it, during the ſhock, all relate that it ſeemed to puſh toward them with great force. So in that of 1692. at *Deal*, the wall of the caſtle, which is of an extraordinary thickneſs and ſtrength, ſhook ſo much, that the people living in it expected it would have fallen on their heads.

For thus the force of the electrical ſhock is proportionate to the quantity of the ſolid. And were fumes and lambent flames ſeen to iſſue out of the gaping ground on theſe diſaſters, as relations tell us, we juſtly may pronounce them to be purely the effect of Electricity.

It

It muſt be accounted no inconſiderable argument in favour of our hypotheſis, that the northern regions of the world are little ſubject to earthquakes, in compariſon of the ſouthern; where the warmth and drineſs of the air, ſo neceſſary in Electricity, is common. Notwithſtanding that we have a vulcano in *Iceland*, yet we hear not of earthquakes frequent in that latitude of the globe.

But whether our conjectures upon this important affair be well founded or no, it certainly becomes a Chriſtian Philoſopher, whilſt he is inveſtigating material cauſes, to look up to the moral uſe of them; for, in reality, every thing in the whole world was ultimately made for that purpoſe. And of all the great and public calamities which affect us mortals, earthquakes claim the firſt title to the name of warnings and judgments; none ſo proper to threaten, or to execute vengeance: nor has any other thoſe annexed terrors, ſo much of the unuſual, the unavoidable, and the horrible apprehenſion of being cruſhed to death, or buried alive.

I cannot but inſiſt upon my former obſervation to be juſt, that earthquakes proclaim themſelves to mankind in this light; becauſe peculiarly directed to great cities and maritime; abounding with wealth and luxury. It would be childiſh to make a long recital of particulars from hiſtory; for had we no other ſort of notices of earthquakes? look upon thoſe two ſhocks we have felt. We own, that *Hampſtead-Heath* and *Finchley-Common*, and *Kennington-Common*, may have been affected with it; yet it is notorious, that *London* was the centre; the place to which the finger of GOD was pointed.

3. As the late earthquakes in *London*, and ſome other parts of *England*, have rouſed the attention of mankind, to conſider the cauſes of them, both in a religious and natural view: and as in a religious view they have been conſidered by the Biſhop of *London*, in his excellent letter to the clergy and people of *London*, which has been received with general approbation: ſo I ſhall here give a ſhort account of what ſeems to me to be a probable natural cauſe of them.

——*by the Rev.* Stephen Hales, *D. D. & F. R. S.* Ibid. p. 669. *Read* April 5. 1750.

But I muſt firſt obviate an objection of ſome ſerious well-meaning people, who are apt to be offended at any attempts to give a natural account of earthquakes; which, but rarely happening in theſe more northern parts, are apt to be looked upon as the more miraculous. But it ought to be conſidered, that the ordinary courſe of nature is as much carried on by the divine agency, as the extraordinary and miraculous events. God ſometimes changes the order of nature, with deſign to chaſtiſe man for his diſobedience and follies; natural evils being graciouſly deſigned by him as moral goods: all events are under his direction, and fulfil his will.

On the other hand, there are ſome who make light of earthquakes, becauſe they are capable of being acounted for by natural cauſes. But the hand of God is not to be overlooked in theſe things, under whoſe government all natural agents act; eſpecially ſuch rare and unuſual

events.

events as earthquakes. God uſes all creatures to be the inſtruments of his will: natural and moral agents are all under his direction. When he inflicts a famine on a nation, it is not the leſs the hand of God, becauſe we know the natural cauſes of it, *viz.* great drought, and unkindly ſeaſons: *fire and hail, ſnow and vapour, and ſtormy wind, fulfil his word,* Pſal. cxlviii. 8. Infectious air, peſtilential diſeaſes, and earthquakes, however occaſioned by natural cauſes, are under the divine influence. He not only orders and directs the operations of nature, but alſo influences the actions of moral agents, turning, as he pleaſes, the hearts of the governors of the nations, ſo as frequently to chaſtize mankind by that ſevere ſcourge, and great diſgrace of human nature, war. Earthquakes are not therefore ſlightly to be regarded, becauſe we think we can give a probable natural account of them; neither ought we, on that account, to encourage ourſelves to go careleſly on in wicked courſes. If national judgments do not overtake us, yet it cannot be long before we ſhall come into the puniſhment of our future ſtate: *And tho' ſentence againſt an evil work is not ſpeedily executed tho' a ſinner do evil an hundred times, and his days be prolonged; yet ſurely I know it ſhall not be well with the wicked.* Eccleſ. viii. 11, 12, 13.

It may not be improper, on this occaſion, to mention another conſtant and uninterrupted plague, in which of late years, we have been, and are like to continue ſufferers, in common with many other nations. A plague, of all others the greateſt that ever befel unhappy man; it being by far the moſt deſtructive, not only of the lives, but alſo of the morals, of mankind; both a natural and a moral evil; I mean fermented diſtilled ſpirituous liquors of all denominations. Did God Almighty deſtroy as many by earthquakes as are yearly deſtroyed by diſtilled ſpirituous liquors, which is probably about a million of perſons in a year all over the world; how great a terror and conſternation would it cauſe every where! But, alas! with what unconcernedneſs, with what calmneſs, and even complacency, is this enormous both natural and moral evil received, and even foſtered, among us; inſomuch that it is now become, by a juſt judgment, the curſe and the puniſhment of the world, even the greateſt that ever befel unhappy man! notwithſtanding which this inchanting *Siren* ſo bewitches and infatuates the nations, that it ſpreads it's baleful influence far and wide, making yearly farther and farther devaſtations, both of the lives and morals of mankind, and even debaſing the breed of man.

As to the affair of earthquakes, particularly that which happened at *London March* 8 laſt, about 20′ before 6 in the morning; I being then awake in bed, on a ground-floor, near the church of *St Martin's in the Fields*, very ſenſibly felt the bed heave, and conſequently the earth muſt heave too. There was a hollow, obſcure, ruſhing noiſe in the houſe, which ended in a loud exploſion up in the air, like that of a ſmall cannon: the whole duration, from the beginning to the end of the earthquake, ſeemed to be about 4″. The ſoldiers who were upon

duty

duty in *St James's Park*, and others who were then up, ſaw a blackiſh cloud, with conſiderable lightning, juſt before the earthquake began; it was alſo very calm weather.

In the hiſtory of earthquakes it is obſerved, that they generally begin in calm weather, with a black cloud. And when the air is clear, juſt before an earthquake, yet there are then often ſigns of plenty of inflammable ſulphureous matter in the air; ſuch as *Ignes Fatui* or *Jack-a-Lanterns*, and the meteors called falling ſtars.

Now, I have ſhewn many years ſince, in the Appendix to my *Statical Eſſays*, Exp. 3. Page 280. the effect that the mixture of a pure and ſulphureous air have on each other; *viz.* by turning the mouth downwards, into a pan of water, of a glaſs veſſel of a capacity ſufficient to hold about two quarts, with a neck about twenty inches long, and two inches wide. Then, by putting under it, in a proper glaſs veſſel, with a long narrow neck, a mixture of *aqua fortis*, and powdered pyrites, *viz* the ſtone with which vitriol is made, there will be a briſk ferment, which will fill the glaſs with rediſh ſulphureous fumes; which, by generating more air than they deſtroy, will cauſe the water, with which the whole neck of the glaſs veſſel was filled, to ſubſide conſiderably. When the rediſh ſulphureous air in the upper part of the glaſs is clear, by ſtanding 2 or 3 hours, if then the mouth of the inverted glaſs is lifted out of the water, ſo as to let the water in the neck of the glaſs fall out; which, ſuppoſing it to be a pint, then an equal quantity of freſh air will ruſh in at the mouth of the neck of the veſſel, which muſt immediately be immerſed in the water: and upon the mixture of the freſh air with the then clear ſulphureous air, there will inſtantly ariſe a violent agitation between the two airs, and they will become, from tranſparent and clear, a rediſh turbid fume, of the colour of thoſe vapours which were ſeen ſeveral evenings before the late earthquakes: during which effervefcence, a quantity of air, nearly equal to what freſh air was let in, will be deſtroyed; which is evident by the riſing up of the water in the neck of the glaſs, almoſt as high as before. And if, after the effervefcence of the mixed airs is over, and become clear again, freſh air be admitted, as before, they will again grow rediſh and turbid, and deſtroy the new admitted air as before; and this after ſeveral repeated admiſſions of freſh air: but after every readmiſſion of freſh air the quantity deſtroyed will be leſs and leſs, till no more will be deſtroyed. And it is the ſame after ſtanding ſeveral weeks, provided, in the mean time, too much freſh air had not been admitted. Now, I found the ſum total of the freſh air thus deſtroyed to be nearly equal to the firſt quantity of ſulphureous air in the inverted glaſs.

Since we have in this experiment a full proof of the briſk agitation and effervefcence which ariſes from the mixture of freſh air with air that is impregnated with ſulphureous vapours, which ariſe from ſeveral mineral ſubſtances, eſpecially from the *pyrites*, which abounds in many parts of the earth; may we not with good reaſon conclude, that the irksome heat, which we feel in what is called a close sultry temperature of the air, is occasioned by the intestine motion between the air and the sulphureous vapours, which are exhaled from the earth? which effervescence ceases, as soon as the vapours are equably and uniformly mixed in the air; as happens also in the effervescences and ferments of other liquors. The common observation therefore, that lightning cools the air, seems to be founded on good reason; that being the utmost and last effort of this effervescence.

May we not hence also, with good probability, conclude, that the first kindling of lightning is effected by the sudden mixture of the pure serene air above the clouds, with the sulphureous vapours, which are sometimes raised in plenty, immediately below the clouds? The most dreadful thunders being usually when the air is very black with clouds; it rarely thundering without clouds: clouds serving, in this case, like the above-mentioned inverted glasses, as a partition between the pure and sulphureous airs: which must therefore, upon their sudden admixture through the interstices of the clouds, make (like the two airs in the glass) a more violent effervescence, than if those airs had, without the intervention of the clouds, more gradually intermixed, by the constant more gradual ascent of the warmer sulphureous vapours from the earth, and descent of the cold serene air from above. And tho' there was no luminous flash of light in the glass, yet, when such sudden effervescence arises, among a vast quantity of such vapours in the open expanse of air, it may, not improbably, acquire so rapid a velocity, as to kindle the sulphureous vapours, and thereby become luminous.

And since, from the effects that lightning is observed to have on the lungs of animals, which it often kills, by destroying the air's elasticity in them, as also from it's bursting windows outwards, by destroying the air's elasticity on the outside of those windows: Since, I say, it is hence probable, that the sulphureous fumes do destroy a great quantity of elastic air; it should therefore cause great commotions and concussions in the air, when the air rushes into those evacuated places; which it must necessarily do with great velocity.

Dr *Papin* has calculated the velocity with which air rushes into an exhausted receiver, when driven by the whole pressure of the atmosphere, to be at the rate of 1305 feet in a second of time; which is at the rate of 889 miles in an hour: which is near 18 times a greater velocity than that of the strongest storms; which is estimated to be at the rate of 50 miles in an hour *.

Hence, we see that an outrageous hurricane may be caused, by destroying a small proportion of the elasticity of the air of any place, in respect to the whole. No wonder then that such violent commotions of the air should produce hurricanes and thunder-showers; especially in the warmer climates; where both the sulphureous and watry vapours, being raised much higher, and in greater plenty, cause more violent effects.

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* See Vol. I. p. 586.

M *de Buffon*, in his Natural Hiſtory and Theory of the Earth, mentions black dark clouds in the air near the tempeſtuous *Cape of Good Hope*, and alſo in the ocean of *Guiney*, which are called by the ſailors the *Ox's Eye*; which are often the forerunners of terrible ſtorms and hurricanes. Whence it is to be ſuſpected, that they are large collections of ſulphureous vapours; which, by deſtroying ſuddenly a great quantity of the elaſtic air, cauſe the ambient air to ruſh with great violence into that vacuity, thereby producing tempeſts and hurricanes. And off the coaſt of *Guiney* they have ſometimes 3 or 4 of theſe hurricanes in a day; the forerunners of which are theſe black ſulphureous clouds, with a ſerene clear air, and calm ſea; which on a ſudden turns tempeſtuous, on the exploſion of theſe ſulphureous clouds. And in *Jamaica* they never have an earthquake when there is a wind to diſperſe the ſulphureous vapours.

In the like manner we find, in the late earthquakes at *London*, and in the accounts of many other earthquakes, that, before they happen, there is uſually a calm air, with a black ſulphureous cloud: which cloud would probably be diſperſed like a fog, were there a wind: which diſperſion would prevent the earthquake; which is probably cauſed by the exploſive lightning of this ſulphureous cloud; being both nearer the earth than common lightnings; and alſo at a time when ſulphureous vapours are riſing from the earth in greater quantity than uſual; which is often occaſioned by a long ſeries of hot and dry weather. In which combined circumſtances, the aſcending ſulphureous vapours in the earth may probably take fire, and thereby cauſe an earth-lightning; which is at firſt kindled at the ſurface, and not at great depths, as has been thought: and the exploſion of this lightning is the immediate cauſe of an earthquake.

It is in the like manner that thoſe meteors, which are called falling ſtars, are ſuppoſed to be kindled into a flame at the upper part of a ſulphureous train, which is kindled downwards into a flame, in the ſame manner as a freſh-blown out candle is inſtantly lighted from another candle held over it at a diſtance, in the ſulphureous inflammable ſmoke of it.

I am ſenſible that it may ſeem improbable, that the aſcending ſulphureous vapours in the earth ſhould thus be kindled; but, ſince they are continually aſcending through the pores of the earth, more or leſs, for many good and uſeful purpoſes, it is plain there is room for them to paſs. Beſides, as M. *de Buffon* remarks, Naturaliſts have obſerved perpendicular and oblique clefts, in all kinds of layers of earth, not only among rocks, but alſo among all kinds of earth, that have not been removed, as is obſervable wherever the earth is open to any depth. Now theſe clefts are cauſed by the drying of the ſeveral horizontal layers of the earth; and will alſo be conſiderably the wider in long dry hot ſeaſons, which are uſually the preparatory forerunners of earthquakes,

 and

and the explosion of the sulphureous vapours may probably widen them more.

It is very observable, in the opinion of *Borelli*, and other Naturalists, that *volcano's* begin first to kindle near the surface or top of the mountains, and not in the caverns in the lower parts of the mountains. M. *de Buffon* says, that earthquakes are most frequent where there are *volcano's*; sulphureous matter abounding most there: but that, tho' they continue burning long, yet they are not very extensive. But that the other sort of earthquakes, which are not caused by a *volcano*, extend often to a great distance. These are much longer E. and W. than broad N. and S; and shake a zone of earth with different degrees of force in different parts of their course; *viz.* in proportion to the different quantities of explosive sulphureous matter in different places. These kind of earthquakes are observed to be progressive, and to take time to extend to the great distances sometimes of some thousands of miles. They are an instantaneous explosion in every place, near the surface of the earth; and therefore do not produce mountains and islands, as *volcano's* sometimes do.

The earthquake in *London*, *March* 8. was thought to move from E. to W. M. *Buffon* mentions an earthquake at *Smyrna*, in 1688. which moved from W. to E. *viz.* because the first kindling probably began on the western side; and in the earthquake at *London* on the eastern side. And accordingly it was observed, that the redish bows in the air, which appeared several days before that earthquake, arose in the east, and proceeded westward. It was observed, after the earthquake at *Smyrna*, that the castle-walls, which run from E. to W. were thrown down; but those from N. to S. stood; and that the houses on rocks stood better than those on the earth.

M. *de Buffon* relates, that the vibrations of the earth, in earthquakes, have commonly been from N. to S.; as appears by the motion of the lamps in churches: which makes it probable, that, tho' the progress of the earthquake at *Smyrna* was from W. to E. yet the vibrations of the earth might be from N. to S; and thereby occasion the falling of the castle-walls, which run from E. to W. but not those which run from N. to S. A probable argument, that, as the freest passage, so the greatest explosions were made in the clefts of the earth which run E. and W. which would make the vibrations N. and S.

It was observed, that the waters turned foul the day before an earthquake at *Bologna* in *Italy:* and I was informed, that the water of some wells in *London* turned foul at the time of the earthquakes. Which was probably occasioned by the ascent of great plenty of sulphureous vapours through the earth.

As to the hollow rumbling noise, which is usually heard in earthquakes, it seems not improbable, that it may be occasioned by the great agitation that the electrical æthereal fluid is put into by so great a shock of a large mass of earth. For, if the like motion of a small revolving glass globe

globe can excite it to the velocity of lightning, and that with a force ſufficient to kill animals, how much greater agitation may it probably be excited to, by the exploſive force of an earthquake!

The exploſion of a cannon in *St James's Park* is obſerved to electrify the glaſs of the windows of the *Treaſury.* And what makes it ſtill more probable, is, the analogy that there is between them in other reſpects. For, as the electrical flaſh ruſhes, with the velocity of lightning, along the moſt ſolid bodies, as iron, *&c.* and as I have ſeen it run only on the irregular gilding of leather; ſo ſuch ſolid bodies are obſerved to be the conductors of aëreal lightning, which rends oaks in pieces, and has been known to run along and melt an iron bell-wire on two ſides of a room, *&c.* And accordingly it was obſerved, in the great earthquake in *Jamaica*, that the moſt tremendous roaring was in the rocky mountains. And in the late earthquake of *March* 8. in *London*, the loudeſt exploſions were thought to be heard near ſuch large ſtone buildings as churches, with lofty ſteeples and ſpires.

I, who lay in *Duke's Court*, near *St Martin*'s church, and was awake all the time of the earthquake, plainly heard a loud exploſion up in the air, like that of a ſmall cannon: which made me conjecture, that the noiſe was owing to the ruſhing off, and ſudden expanſion, of the electrical fluid, at the top of *St Martin*'s ſpire; where all the electrical *effluvia*, which aſcended up along the larger body of the tower, being by attraction ſtrongly condenſed, and accelerated at the point of the weather-cock, as they ruſhed off, made ſo much the louder expanſive exploſion.

The Philoſophy *of* Earthquakes; *by the Rev.* Will. Stukeley, *M. D. F. R. S. &c. in a letter to* M. Folkes, *Eſq; LL. D.* Preſ. R. Soc. *&c.* Ibid. p. 731. *Read* Dec. 6. 1750.

XLII. Since I had the honour to lay before the ſociety, in the ſpring, my thoughts upon earthquakes, we have had many opportunities of reflecting on that moſt awful, and hitherto unuſual, appearance. The year 1750. may rather be called the year of earthquakes, than of *Jubilee.* For, ſince they began with us at *London*, as far as I can learn, they have appeared in many parts of *Europe*, *Aſia*, *Africa*, and *America*, and have likewiſe reviſited many counties in our iſland: at length, on 30th of laſt *Sept.* taken their leave (as we hope) with much the moſt extenſive ſhock we have ſeen in our days.

It may well be expected, that theſe frequent viſits, in themſelves ſo very extraordinary, to us ſo rare, and that in one year, ſhould keep up our attention; and, as to my own part, induce one to reflect on what I before offered concerning them, and be a ſufficient apology for the preſent paper.

We have been acquainted, by thoſe that remember it, that in the earthquake of *November* 1703. which happened in *Lincolnſhire*, the weather was calm, cloſe, gloomy, warm, and dry, in a degree highly unuſual at that ſeaſon: and thus it has been with us all the year: and from the numerous accounts we have received at the *Royal Society*, in the beginning and end of the year, where any mention is made of the

weather,

weather, they agree in the like particular: which is conſentaneous to what I remarked as the conſtant forerunner of earthquakes, and what prepares the earth's ſurface to receive the electrical ſtroke.

In my laſt we had a paper read at the *Royal Society*, concerning the firſt earthquake felt by us at *London*, *Feb.* 8. A ſhepherd belonging to Mr Secretary *Fox* at *Kenſington*, the ſky being perfectly ſerene and clear, was much ſurprized with a very extraordinary noiſe in the air, rolling over his head, as of cannon cloſe by: he likewiſe thought that it came from the N. W. and went to the S. E. a motion quite contrary to what muſt have been the caſe, if it were really of cannon. This noiſe paſſed ruſhing by him; and inſtantly he ſaw the ground, a dry and ſolid ſpot, wave under him, like the face of the river. The tall trees of the avenue, where he was, nodded their tops very ſenſibly, and quavered. The flock of ſheep immediately took fright, and ran away all together, as if the dogs had purſued them. A great rockery in the place were equally alarmed; and, after an univerſal clangor, flew away, as if chaſed by hawks.

I was likewiſe informed, that, in the ſame earthquake, a great parcel of hens and chickens, kept at that time in *Gray's-Inn Lane*, upon the ſhock, ran to the rooſt affrighted: and the like was obſerved of pigeons. And in our account of the laſt earthquake from *Northampton*, it is remarked, that the birds in cages put their heads under their wings, as to hide themſelves.

June 21. at the *R. Soc.* Mr *Jackſon*, Potter at *Lambeth*, gave an account of ſome boats and loiters, in the river at that time; the people in them ſeemed to feel as if a porpoiſe, or ſome great fiſh, had heaved and thumped at the bottom of the loiters. This is ſometimes the caſe of ſhips at ſea; which ſeems evidently owing to an electrical impreſſion on the water.

In the *Evening-Poſt*, *June* 23. we had a paragraph from *Venice*, that a terrible earthquake had lately been felt in the iſle of *Cerigo*; a little rocky iſle. It threw down a great number of houſes, and above 2000 inhabitants were buried in the ruins.

Another earthquake about that time happened in *Switzerland*, which ſplit a vaſt rocky mountain, and an old caſtle-wall, of an immenſe thickneſs.

But, ſince then, theſe wonderful movements have ſtalked round the globe; and again been lately felt in our own iſland, to the terror only of many thouſand people; beſides thoſe that appeared in the weſtern parts, in the more early time of the year.

I received a letter from *Maurice Johnſon*, Eſq; the founder and ſecretary of the *Literary Society* of *Spalding*, which has now ſubſiſted theſe 40 years. He acquaints me, that, on *Thurſday*, *Aug.* 23. laſt, an earthquake was very ſenſibly felt there, about 7 in the morning, throughout the whole town and neighbourhood, and many miles round; but chiefly ſpread northward and ſouthward. He ſays, that, for a fortnight

night before, the weather had been ſerene, mild, and calm; and one evening there was a deep-red *Aurora auſtralis*, covering the cope of Heaven, very terrible to behold. This ſame ſhock was felt at *Grantham*, *Stamford*, and *Milton* by *Peterborough*; and generally at all the intermediate places.

Since then, I had a letter from Mr Alderman *Taylor*, of *Stamford*, giving an account of another earthquake, that happened there on *Sunday*, *Sept*. 30. at 36′ after noon. He deſcribes it thus: " They were " ſuddenly ſurprized with an uncommon noiſe in the air, like the roll- " ing of large carriages in the ſtreet, for about 20 ſeconds. At the " ſame inſtant they felt a great ſhake, or ſnap (as he calls it); inſo- " much that it ſenſibly ſhook a punch-bowl, which was in his parlour, " and made it ring. He ſays, it was perceived of moſt of the people " of *Stamford*, who generally ran out of their houſes. At *Okeham*, the " chief town in *Rutland*, the congregation ran out of the church. All " the towns round *Stamford* were ſenſible of it, and at *Peterborough*, " down to *Wiſbich*."

Thus far the Alderman. But we have had many advices from all hands, at the firſt and ſecond meetings of the *R. S.* for the winter-ſeaſon; with further particulars relating to this great concuſſion: that it was felt at the ſame time at *Rugby* in *Warwickſhire*, and reached to *Warwick*; at *Lutterworth* in *Leiceſterſhire*; at *Leiceſter*, and round about. They deſcribe it, that the houſes tottered, and ſeemed to heave up and down, tho' it laſted but a few ſeconds. It was attended with a ruſhing noiſe, as if the houſes were falling; and people were univerſally ſo affrighted as to run out; imagining that their own, or neighbours houſes, were tumbling on their heads. In the villages around, the people, being at divine ſervice, were much alarmed, both with the noiſe, which exceeded all the thunder they had ever heard, beyond compare; and with the great ſhock accompanying, which was like ſomewhat that ruſhed againſt the church-walls and roof; ſome thinking the pillars cracked; many, that the beams of the roof were disjointed; and all, that the whole was falling; and happy were they that could get out firſt. A few ſlates, tiles, and parts of chimnies, fell from ſome houſes; pewter, glaſſes, and braſs, fell from ſhelves; a clock-bell ſometimes ſtruck; windows univerſally rattled; and the like circumſtances of tremor.

The ſame extended itſelf to *Coventry*, *Derby*, *Nottingham*, *Newark*; then came eaſtward to *Harborough*, *Towceſter*, *Northampton*, *Rowell*, *Kettering*, *Wellingborough*, *Oundle* in *Northamptonſhire*, *Uppingham*, *Okeham* in *Rutland*, *Stamford*, *Bourn*, *Grantham*, *Spalding*, *Boſton*, and to *Lincoln*, in *Lincolnſhire*; *Holbech*, and all *Holland*, in that county; *Peterborough*, *Wiſbich* in the iſle of *Ely*, together with all the intermediate and adjacent places. Then it paſſed over the whole breadth of *Ely-Fen*, and reached to *Bury* in *Suffolk*, and the country thereabouts; of which we had notice from Lady *Cornwallis*: an extent from *Warwick* to *Bury* of of about 100 miles in length; and, generally ſpeaking, 40 miles in breadth. And this vaſt ſpace was pervaded by this amazing motion, as far as we can get any ſatisfaction, in the ſame inſtant of time.

In regard to circumſtances, they were pretty ſimilar throughout. At *Northampton*, a Gentlewoman, ſitting in her chair, relates, that ſhe and her chair were twice ſenſibly lifted up, and ſet down again. A ſtack of chimnies were thrown down in *College-Lane*; a place retaining the memory of a ſort of univerſity once beginning at *Northampton*. The windows of houſes rattled throughout the whole town; but no miſchief done: in general, frightful, and innocuous.

They fanſied there the motion of it, as they expreſs it, to be eaſtward. In the ſtreets that run N. and S. the houſes on the E. ſide of the way were moſt affected: and Dr *Stonehouſe*'s dwelling, the ſtrongeſt in the town, was moſt ſenſibly ſhaken. So it was likewiſe obſerved, that churches were moſt ſubject to it's violence. They thought too that the motion ſeemed rather horizontal, or lateral, than upward. Some counted the pulſes diſtinctly, to the number of four: that the ſecond and third pulſe were ſtronger than the firſt and fourth.

From all theſe various accounts, there was no ſulphureous ſmell, or eruption; no fiſſures in the ground perceived: yet ſeveral people were ſick upon it; infinite numbers terribly affrighted; and as ſoon forgot the impreſſion of it, or talked of it in a merry ſtrain, as commonly with us at *London*. So little are the vulgar affected, without ſomething very ſenſible, and ſo ſoon is the ſenſe of it worn out!

It was more evidently perceived by people ſtanding; moſt, by thoſe that were ſitting; leaſt, by ſuch as were walking; and in upper ſtories of houſes more than in lower, or in cellars. Some, coming down ſtairs, were in danger of being thrown forwards: ſeveral ſitting in chairs, and hearing the hollow thundering noiſe, and thinking it was a coach paſſing by, when they attempted to get up, to ſee what it was, they were thrown back again into their chair. Some heard the wainſcot crackle. A lady, ſitting by the fire, with her chair leaning forwards, was thrown down on her hands and knees.

It was particularly remarked (as before obſerved), that birds in cages were ſenſibly affrighted, thruſting their heads under their wings. Mrs *Allicock*, of *Loddington*, a Lady in childbed, was ſo affected, that it cauſed her death. Some people felt ſuch a ſudden ſhortneſs of breath, that they were forced to go out into the open air, it ſo affected the pulmonary nerves. Many were taken with head-achs.

Theſe are, in general, the obſervations made at the time of theſe earthquakes; when we recollected ourſelves, after the ſuddenneſs and affright. Give me leave to make the following remarks.

1. As far as we can poſſibly learn, where no one can be prepared at different places, by time-keepers, this mighty concuſſion was felt preciſely at the ſame inſtant of time, being about half an hour after 12 at noon. This, I preſume, cannot be accounted for by any natural power, but

but that of an electrical vibration; which, we know, acts inſtantaneouſly.

2. Let us reflect on the vaſt extent of this trembling, 100 miles in length, 40 in breadth, which amounts to 4000 ſquare miles in ſurface. That this ſhould be put into ſuch an agitation in one moment, is ſuch a prodigy, as we ſhould never believe, or conceive, did we not know it to be fact, from our own ſenſes. But, if we ſeek for a ſolution of it, we cannot think any natural power is equal to it, but that of Electricity; which acknowledges no ſenſible tranſition of time, no bounds.

3. We obſerve, the vulgar ſolution of ſubterraneous eruptions receives no countenance from all that was ſeen or felt during theſe earthquakes: it would be very hard to imagine how any ſuch thing could ſo ſuddenly and inſtantaneouſly operate thro' this vaſt ſpace, and that in ſo ſimilar and tender a manner, over the whole, thro' ſo great a variety as well as extent of country, as to do no miſchief.

A philoſophical inquirer in *Northamptonſhire*, and who had his eye particularly on this point, takes notice there were not any fiſſures in the ground, any ſulphureous ſmells, or eruptions, any-where perceived, ſo as to favour internal convulſions of the earth; yet we learn, from a letter, at *Uppingham* in *Rutland*, that a plaſter floor became cracked thereby. Theſe kind of floors are frequent in this country; what we call *Stucco* in *London*; and it gives us a good notion of the undulatory vibration produced by an earthquake; which ſome have compared to that of a muſical ſtring; others, to that of a dog, or a horſe, ſhaking themſelves when they come out of the water.

4. The former earthquake, that happened at *Grantham*, *Spalding*, *Stamford* (which towns lie in a triangle) took up a ſpace which may in groſs be accounted a circle of 20 miles in diameter; the centre of which is that great moraſs called *Deeping-Fen*. This comprehends 14 miles of that 20 in diameter; and where, probably, the electrical impreſſion was firſt made. Much the major part of *Deeping-Fen* is under water in the winter; underneath is a perfect bog: now it is very obvious how little favourable ſuch ground is for ſubterraneous fires.

In the ſecond earthquake, not only this country was affected again, but likewiſe a much larger ſpace of the ſame ſort of fenny ground, rather worſe than the former: all *Donington-Fen*, *Deeping-Fen*, *Croyland-Fen*, *Thorney-Fen*, *Whitleſea Fen*, *Bedford-Level*, and the whole extent of *Ely-Fen*, under various denominations. This country, under the turf, abounds with ſubterraneous timber of all kinds; fir, oak, and bruſh-wood; ſtags horns: now-and-then they find a quantity of hazel-nuts, crouded together on a heap: I have ſome of them. This is a matter common to all boggy ground over the whole globe. They are the ruins of the antediluvian world, waſhed down from the high country, where they grew, here lodged, and by time overgrown with the preſent turf. They that ſeek for any other ſolution of this affair, than the univerſal *Noachian* deluge, want to account for a general effect by a par-a partial cauſe; and ſhut their eyes, both to the plain hiſtory of this matter, and to the infinite notorious demonſtrations of it from foſſil appearances.

5. All this country, tho' underneath it is a watry bog, yet, through this whole ſummer, and autumnal ſeaſon (as they can have no natural ſprings in ſuch a level) the drought has been ſo great on the ſuperficies, that the inhabitants were obliged every day to drive their cattle ſeveral miles, for watering. This ſhews how fit the dry ſurface was for an electrical vibration; and we learn from hence this important particular, that it reaches but very little below the earth's ſurface.

Mr *Johnſon*, in another letter which he wrote to me concerning the ſecond earthquake, obſerved at *Spalding*, ſays, upon this occaſion, he was obliged to ſcour his canal, and deepen it; that they came to a white quickſand, which afforded to all the neighbourhood excellent water in plenty.

In the gravelly ſoil of *London*, and where the two ſhocks were felt by us, in the beginning of the year, we know there is not an houſe in the whole extent of this vaſt city, and all around it, but a ſpring of water is ready, upon digging a well: whence we have much reaſon to believe, that the internal parts of the earth are like a ſponge ſoaked in water; ſo that the only dry part of it is the ſuperficies; which is the object, and the ſubject, of that electric vibration, wherein (according to my ſentiments) an earthquake conſiſts.

This ſhews the miſtake of the Antients; who, fancying that earthquakes proceeded from ſubterraneous eruptions, built their prodigious temple of *Diana* of *Epheſus* upon a boggy ground, to prevent ſuch a diſaſter.

6. Earthquakes are truly moſt violent in a rocky country; becauſe the ſhock is proportionate to the ſolidity of the matter electrified: ſo that rocks, old caſtle-walls, and ſtrong buildings, are moſt obnoxious to the concuſſion. The iſle of *Cerigo* was more liable, and more rudely handled by the late earthquake; both becauſe it was an iſle, and becauſe it was rocky. So we muſt ſay of the late earthquake in *Switzerland*, that ſplit the mountain and the old caſtle-wall. Whence Mr *Johnſon*, in his ſecond letter, ſays, it cracked a very ſtrong brick houſe in *Goſberton* by *Spalding*. Dr *Doddridge* obſerves, from *Northampton*, that Dr *Stonehouſe*'s dwelling, being a very ſtrong one, was moſt ſenſibly ſhaken. And, throughout the whole extent of this great earthquake, we find both the noiſe, the ſhock, and the terror, was greateſt at the churches, whoſe walls and bulk made more reſiſtance than houſes: and, generally ſpeaking, the churches throughout this whole extent have very fair and large towers, and very many remarkable ſpires of good ſtone.

This ſame vibration, impreſſed on the water, meeting with the ſolid of the bottom of ſhips and loiters, gives that thump felt thereon. Yet, of the millions of ordinary houſes, over which it paſſed, not one fell:

a con-a conſideration which ſufficiently points out to us what ſort of a motion this was not; what ſort of a motion it was; and whence derived: not a convulſion of the bowels of the earth, but an uniform vibration of it's ſurface, aptly thought like that of muſical ſtring; or what we put a drinking-glaſs into, by rubbing one's finger over the edge; which yet, brought to a certain pitch, breaks the glaſs; undoubtedly an electric repulſion of parts.

7. We find, from all accounts antient and modern, that the weather preceding theſe ſhocks was mild, warm, dry, ſerene, clear, froſty: what notoriouſly favours all our electrical experiments. We very well know, that, generally, all laſt winter, ſpring, ſummer, and autumn, have been remarkably of this kind of weather; more ſo than has been obſerved in our memory; and have had all thoſe requiſites, appearances, and preparations, that notoriouſly cauſe Electricity, that promote it, or that are the effects of it.

8. We find the blood-red *auſtralis aurora* preceding at *Spalding*, as with us at *London*. This year has been more remarkable than any for fire-balls, thunder, lightning, and coruſcations, almoſt throughout all *England*. Fire-balls more than one were ſeen in *Rutland* and *Lincolnſhire*, and particularly obſerved. All theſe kinds of meteors are rightly judged to proceed from a ſtate of Electricity in the earth and atmoſphere.

9. Mr *Johnſon*, in both his letters to me on the firſt and ſecond earthquakes at *Spalding*, remarks particularly of their effects being moſtly ſpread to the N. and S. and eſpecially felt on the ſea-coſt. We may obſerve that ſuch is the direction of *Spalding* river, which both conducts and ſtrengthens the electric vibration; conveying it along the ſea-ſhore, thence up *Boſton* chanel, and ſo up *Boſton* River to *Lincoln*; as we diſcern, by caſting our eye upon a map.

We obſerve further, that the main of this ſecond earthquake diſplayed it's effects along and between the two rivers *Welland* and *Avon*; and that from their very origins down to their fall into the ſea. It likewiſe reached the river *Witham*, which directed the electric ſtream that way too to *Lincoln*: for which reaſon, as there meeting the ſame coming from *Boſton*, the ſhock was moſt ſenſibly felt. It reached likewiſe to the *Trent* at *Nottingham*, which conveyed it to *Newark*.

The firſt electrical ſtroke ſeems to have been made on the high ground above *Daventry* in *Northamptonſhire*, where the *Roman* camps are, made by *P. Oſtorius* the Proprætor. From thence it deſcended chiefly eaſtward, and along the river *Welland*, from *Harborough* to *Stamford*, *Spalding*, and the ſea; and along the river *Avon*, or *Nen*, to *Northampton*, *Peterborough*, and *Wiſbich* to the ſea. It ſpread itſelf all over the vaſt level of the iſle of *Ely*, furthered by very many canals and rivers, natural and artificial, made for drainage. It was ſtill conducted eaſtward, up *Mildenhall* river in *Suffolk*, to *Bury*, and the parts adja-

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cent. All this affair, duly conſidered, is a confirmation of the doctrine I advanced on this ſubject.

10. I apprehend it was not the noiſe in the air, as of many cannon let off at once, preceding the earthquake, that ſo much affrighted people, or affected the ſheep, the rookery at *Kenſington*. the hen and chickens in *Grays Inn-Lane* and the pigeons: it could not be barely the ſuperficial movement of the earth that diſturbed them all at once: I judge it to be the effect of Electricity, ſomewhat like what cauſes ſea-ſickneſs; ſuch a ſort of motion as we are not accuſtomed to. So the earthquake affects all thoſe of weak nerves, or that have nervous complaints, obnoxious to hyſterics, colics, rheumatic pains in their joints. Several women were ſeized with violent head-achs, before both the ſhocks we felt in *London*. It was this that affected the people, with a ſhortneſs of breath. This made the dog run whining about the room, ſeeking to get out: this made the fiſhes leap up in the pond at *Southwark*; like as the experiment of electrifying the fiſhes; it makes them ſick: and this cauſes the birds in cages to hide their heads under their wings, becauſe they cannot fly away: which is commonly obſerved of them in *Italy*, and countries where earthquakes are more frequent.

11. I obſerve, the ſhepherd of *Kenſington* thought the motion of the earthquake, and the ſound, were from N. W. to S. E. On the contrary, Mr *Byfield*, the ſcarlet-dyer in *Southwark*, thought the noiſe came from the river below-bridge, and went toward *Weſtminſter*; where it rattled ſo, that he did not doubt but that the abbey-church was beaten down.

Dr *Parſons* took pains to find out the way of the motion of the earthquake, from the different poſition of the beds; but, from the contradictory anſwers given, he could obtain no ſatisfaction, as to that point. All this, and what was obſerved from *Northampton*, of the motion being thought by ſome to be upward and downward, by others, rather horizontal or lateral, the counting the pulſes, and the like, only points out to us the prodigious celerity, and the vibratory ſpecies of the motion of an earthquake; but far, very far, is this from being owing to the tumultuous ebullition, the irregular hurry of ſubterraneous exploſions.

12. How the Atmoſphere and earth are put into that electric and vibratory ſtate, which prepares them to give or receive the ſnap, and the ſhock, which we call an earthquake, what it is that immediately produces it, we cannot ſay; any more than we can define what is the cauſe of magnetiſm, or of gravitation, or how muſcular motion is performed, or a thouſand other ſecrets in nature.

We ſeem to know, that the AUTHOR of NATURE has diſſeminated ethereal fire thro' all matter; by which theſe great operations are brought about. This is the ſubtil fluid of Sir *I. Newton*, pervading all things; the occult fire diffuſed thro' the univerſe, according to *Marſilius Ficinus* the *Platonic* Philoſopher, in the *Timæus* of his maſter. And the *Platoniſts*

toniſts inſiſt on an occult fire paſſing thro' and agitating all ſubſtance by it's vigorous and expanſive motion.

Before them, *Hippocrates* writes in the ſame ſenſe, I. *de victus ratione*, that this fire moves all in all. This ethereal fire is one of the 4 elements of the Ancients: it lies latent, and diſperſed thro' all the other 3, and quieſcent; till collected in a quantity, that overbalances the circumjacent; like the air crouded into a tempeſt; or till it is excited by any proper motion.

This fire gives Elaſticity, and Elaſticity, or vibration, is the mother of Electricity. This fire is in water, and betrays itſelf to our ſenſes in ſalt water. Many a time, when I have paſſed the *Lincolnſhire* waſhes, in the night-time, the horſe has ſeem'd to tread in liquid flames. The ſame appearence oft at the keel of a ſhip.

The operation of the ethereal fire is various, nay infinite, according to it's quantity, and degree of incitement, progreſs, hindrance, or furtherance. One degree keeps water fluid, ſays the learned Biſhop of *Cloyne:* another turns it into elaſtic air: and air itſelf ſeems nothing elſe but vapours and exhalations rendered elaſtic, by this fire.

This ſame fire permeates and dwells in all bodies, even diamond, flint, and ſteel. It's particles attract with the greateſt force, when approximated. Again, when united, they fly aſunder with the greateſt celerity. All this according to the laws preſcribed by the ſovereign ARCHITECT. This is the life and ſoul of action, and reaction, in the Univerſe. Thus has the great AUTHOR provided againſt the native ſluggiſhneſs of matter! light, or fire, in animals, is what we call the animal ſpirits; and is the author of life and motion. But we know not the immediate mode of muſcular motion, any more than how, in inanimate matter, it cauſes the vibrations of an earthquake.

Of this fire *Manilius* thus writes, who lived in the time of *Auguſtus*, *Aſtronom.* I.

Sunt autem cunctis permiſti partibus ignes,
Qui gravidas habitant fabricantes fulmina nubes,
Et penetrant terras, Ætnamque imitantur Olympo,
Et calidas reddunt ipſis in fontibus undas,
Ac ſilice in duro, viridique in cortice, ſedem
Inveniunt; cum ſilva ſibi colliſa crematur.
Ignibus uſque adeo natura eſt omnis abundans!

Which may thus be engliſhed:

Fire, univerſal nature traverſes;
It makes the thunderbolt in tumid clouds,
In dire *Volcano's* penetrates the earth;
And ſends the boiling water from it's ſprings:
In hardeſt flint, and ſofteſt wood, it dwells;

Which,

Which, by collision, shews itself in flame.
With fire so pregnant is all nature found!

13. The great question then with us, is, how the surface of the earth is put into that vibratory and electric state by heat and driness? we must needs acquit the internal of the earth from the charge of these superficial concussions. How is the ethereal fire crouded together, or excited, so as to cause them; seeing, in our ordinary electrical experiments, we make use of friction?

But that friction alone does not excite Electricity, we know, from the obvious experiment of flint and steel; where the suddenness of the stroke, and hardness of the matter does it. Another method of exciting it, is the letting off a number of great guns; which so crouds the ethereal fire together, as to electrify glass windows: observed by Dr *Hales*. The *Aurora borealis*, *australis*, all kind of coruscation, meteors lightning, thunder, fireballs, are the effects, and may reciprocally be the cause, of Electricity; but how, in particular, we know not.

Come we to the animal world, we must needs assert, that all motion, voluntary and involuntary, generation, even life itself, all the operations of the vegetable kingdom, and an infinity more of nature's works, are owing to the activity of this electric fire; the very soul of the material world. And, in my opinion, it is this alone that solves the famous question, so much agitated with the writers in medicine, about the heat of the blood. How these, how earthquakes, are begun and propagated, we are yet to seek.

We may readily enough presume, that the contact between the electric and the non-electric, which gives the snap, and the shock, must come from without, from the atmosphere; perhaps by some meteor, that crouds the ethereal fire together, causes an accension in the air, in the point of contact, on the earth's surface; perhaps another time by a shower of rain. We may as readily conclude, that, tho' the original stroke comes from the atmosphere, yet the atmosphere has no further concern in it: no aëreal power, or change therein, can propagate itself so instantaneously over so vast a surface as 4000 miles square: therefore the impetuous rushing noise in the air, accompanying the shock, is the effect, not the cause.

But surely there is not a heart of flesh that is not affected with so stupendous a concussion. Let a man estimate his own power with that which causes an earthquake, and he will be persuaded that somewhat more than ordinary is intended by so a rare and wonderful a motion.

Hippocrates makes the whole of the animal oeconomy to be administred by what we call nature; and nature alone, says he, suffices for all things to animals: she *knows* herself, and what is necessary for them.

Can we deny then that he here means a conscious and intelligent nature that presides over, and directs all things; moves the ethereal spirit, or fire, that moves all things; a divine necessity, but a voluntrary agent, who gives the commanding nod to what we commonly call nature; the

chief

the chief inſtrument in the moſt important operations of the vaſt machine, as well as in the ordinary ones? And this leads us,

14. Laſtly, in regard to the ſpritual uſe we ought to make of theſe extraordinary *phænomena*, or of our inquiries about them; I ſhall firſt obſerve, that we find abroad, ſeveral of theſe earthquakes this year have been very fatal. In the laſt we read of at *Philippoli* in *Thrace*, the whole city was deſtroyed, and above 4000 inhabitants killed. At home, where above half a ſcore ſeparate concuſſions have been felt, there has not been one houſe thrown down, one life loſt. This ought to inſpire us with a very ſerious reflection about them. 2. We may obſerve, that if we did but read the works of *Hippocrates*, *Plato*, and his followers, of *Tully*, *Galen*, and the like ethic writers of antiquity, whilſt we ſtudy and try the affections of matter, we ſhould improve in Philoſophy, properly ſpeaking; we ſhould lift up our minds from theſe earthly wonders, and diſcern the celeſtial monitions they preſent to us.

The original meaning of the word Philoſophy was rightly applied to moral wiſdom: we, who have improved both, ſhould join them both together. By this means we gather the truth of the higheſt and moſt excellent Philoſophy, to be found in thoſe volumes of firſt antiquity, which we call ſacred; and we ſhould adore that divine light which they hold forth to us; eſpecially in a country where the principles of true religion are open and undiſguiſed; where the eſtabliſhed profeſſion of it is rational, noble, and lovely; worthy of the Moral Governor of the world.

Two letters from the Rev. Mr Granville Wheler, *F. R. S. to the* Preſ. *concerning a* rotatory *motion of* glaſs tubes *about* *their* axes, *when placed in a certain manner before the fire.* N°. 476. p. 341. April &c. 1745. *Read* March 28. *and* April 4. 1745.

XLIII. About 4 years ago, Mr *Charles Orme*, of *Aſhby de la Zouch* in *Leiceſterſhire*, acquainted me that, in drying his glaſs tubes for his diagonal Barometers (which for ſome years he has continued to make in much greater perfection than any other perſon that I know of in *England**), he had obſerved a rotatory motion about their *axes*, and at the ſame time a progreſſive one towards the fire. He was ſo obliging then as to promiſe at any time to ſhew me the experiment; but other buſineſs intervening, I ſtill deferred accepting his offer; having the leſs curioſity to ſee it, as I imagined the motions were occaſioned by the draught of air up the chimney, aſſiſted by the weight of the inclined tube. But a little above a year ago, making ſome ſtay at *Aſhby*, upon repeating his offer, I went to ſee the experiment, which anſwered fully to his deſcription: the tubes which were about 4 feet long, and ¼ an inch over, moving at 6 or 8 inches diſtance from the fire, not only progreſſively, and about their *axes* along the ſide-wall they leaned againſt, but along the front-wall of the chimney, which made an obtuſe angle with the other; ſo that they ſeemed to move up hill, and againſt their weight.

Surpriſed at this, I thought the caſe deſerved a little farther examination; and propoſed placing two tubes horizontally, parallel to each

* One of which Mr *Wheler* made a preſent of to the *R. S.*

other

other, and at right angles to the face of the fire, to be ſupporters to a third which was to be placed upon them parallel to the fire. We did ſo, and with pleaſure obſerved the ſupported tube turn about it's *axis*, and move on towards the fire in ſuch a manner, as made me ſtill leſs inclined to think either of the motions owing to the draught of the fire, and certainly not to the whole weight of the moving tube; a fine ſpirit-level informing us, that the ſupporting tubes leaned from the fire; ſo that the motio was a little up-hill.

This ſucceſs determined me, with Mr *Orme*'s leave, to go on farther; and, furniſhing myſelf, from him, with tubes of ſeveral lengths and thickneſſes, I made ſeveral trials; and found, that with a moderate fire the experiment ſucceeded beſt, when the ſupported tube was about 20 or 22 inches long, the diameter about $\frac{1}{10}$ of an inch, and had in each end a pretty ſtrong pin, fixed in cork, for an axe to roll with upon the ſupporting tubes; which, to leſſen the contact, had nearly the ſame diameter with the moving one. Under theſe circumſtances the tube would begin to move at 18 inches diſtance from the fire; and continue to do ſo, with little intervals, till it touched the bars, and moved much in the ſame manner, when a little ball of cork, an inch or more in diameter, was fixed in the middle of it. But what ſurprized me ſtill more, and ſeemed to take off the objection of the draught of the chimney, was, letting it once ſtay a little while againſt the bars, I found it ſtill continue it's motion about its *axis* in the ſame direction.

This put me upon making little rings of wire, to fix upon and move along the ſupporting tubes, ſo as to ſtop the moving tube at any diſtance from the fire I pleaſed. Stopped with theſe, the motion of the tube about its *axis* ſtill continued.

Deſirous to try what would be the effect in or near an upright poſture, I made the pin at one end of my tube reſt upon a *China* plate, that at the other turn in a ſilver ſocket (that carried my pencil) fixed in an horizontal arm of wood, but ſo as I could ſlip it up and down, to adapt it to the length of the tube. Here I found, that if the tube leaned to my right hand, (which was the caſe of Mr *Orme*'s tubes before his fire) the motion was from E. to W.; but if they leaned to my left, the motion was from W. to E.; and the nearer I could get to the perfectly upright poſture, the leſs the motion ſeemed to be either way.

I now proceeded to place my tube horizontally upon a glaſs plane (a large fragment of a coach-ſide window-glaſs). The tube, inſtead of moving towards the fire, moved from it, and about it's *axis*, in a contrary direction to what it had done before. Obſerving that this glaſs plane was broader at one end than the other, and that the rotation backwards was more ſenſible when the narrower end was towards the fire, I placed a triangular piece of the ſame glaſs with it's *vertex* towards the fire nearly horizontal, but rather riſing from the fire; ſo that it's baſe was a little higher than it's *vertex*; and upon it a tube of glaſs, about 22 inches long, and $\frac{1}{5}$ of an inch diameter, near the *vertex* and the fire.

This

This tube receded from the fire, moving about it's *axis* till it came to the diſtance of eight inches; which is four inches more than it receded the day before upon the ſame piece of coach glaſs, before it was broken into this triangular form.

I was naturally led now to make uſe of two ſupporting tubes, inſtead of the triangular glaſs plane. Theſe were about 18 inches long each, and $\frac{1}{70}$ of an inch in diameter, and placed parallel to one another at the diſtance of about two inches, ſo as to ſupport the moving tube near the middle of it. When very nearly horizontal by the level, the ſupported tube moved from the fire about it's *axis* to the diſtance of 13 inches: when the ſupporters were a little raiſed at their remote ends, ſo as manifeſtly by the level to deſcend towards the fire, it receded to the diſtance of 10 inches, moving as before about it's *axis*; but in this latter caſe the fire had declined a good deal, otherwiſe, probably, the tube would have receded farther, tho' up-hill.

The next day, the ſame tube, when the ſame ſupporting tubes were 8 $\frac{1}{2}$ inches diſtant from each other, receded nearly as before: when 12 $\frac{1}{2}$ inches from each other, it ſtood ſtill; and when removed to the diſtance of 16 $\frac{1}{2}$ inches, the ſupported tube very manifeſtly changed it's motion, and went towards the fire; as it did afterwards, when the inclination of the ſupporting tubes was altered, ſo as to aſcend towards the fire.

I made ſeveral other experiments, with regard to the ſituation of the tubes to the fire, with regard to the quantity of fire ſuffered to come at the tubes, and with regard to attraction and repulſion, which I will not trouble you with at preſent: only obſerve, that, when the tube had 4 others under it, all ſupporting, one near each extremity, and one on each ſide of it's centre, no motion at all was perceived; and when two of them on the ſame ſide of the centre were taken away, the ſupported tube moved into an oblique ſituation with regard to the fire, the unſupported half receding from the fire.

Upon the whole, it appears ſufficiently plain, that the ſtream of air up the chimney is not the cauſe of the rotation: another may be aſſigned, ſimple and eaſy; but as I have already ſaid too much, it will be better to make it the ſubject of another paper.

I ſuſpended two fragments of ſmall tubes, 8 inches long, and about $\frac{1}{70}$ of an inch in diameter, near the fire, from two pins, by blue ſilk lines, which had each a loop at one end, were tied at the other to the top of the tubes, and hindered from ſlipping off by a little ſealing-wax. The tubes came together at the upper end, and receded manifeſtly from each other at the lower, appearing to be in a ſtate of attraction above, and a ſtate of repulſion below: but, ſuſpecting this to be owing to the ſealing-wax, which ſoon began to melt, I ſcraped it off both, leaving only as little as was poſſible, to hinder the ſilks from ſlipping. The conſequence then was, they came together at the lower ends, and very near ſo at the upper; and, when ſuſpended from one pin, ſo that the *The ſecond Letter.*

loops of the silks touched each other, the tubes seemed equally close all the way down, without any appearance either of attraction or repulsion. But, imagining still that a repulsive power in the heated supporting tubes, when placed near together, might possibly be the occasion of the receding of the upper tube at contact with them. To put the matter out of all doubt, I wet the 3 tubes all over; yet the regressive and rotatory motion was still manifest, with very little, if any difference; not more than might be well accounted for, from the increase of resistance by wetting.

These two experiments fully convinced me, that neither attraction nor repulsion would be of any assistance in solving our rotation Upon considering therefore the matter farther, I found nothing was wanting, but that the moving tube should swell towards the fire; and indeed I thought I could perceive such a swelling in Mr *Orme*'s long tube of four feet and an half, which I saw first placed near a good fire in the manner described in my last. For, allowing such a swelling, gravity must pull the tube down, when supported near it's extremities horizontally; and a fresh part being exposed to the fire, and swelling out again, must fall down again, and so on successively; which is, in other words, a rotatory motion towards the fire.

When the supporting tubes are brought near to each other, as well as near to the centre of the supported tube, then the parts hanging over on each side, being larger than the part which lies between the supporters, will, by their weight, pull downwards, and consequently force the middle part, resting upon it's two *fulcra*, upwards; and being less advanced towards the fire, as being less heated, will, by their oblique situation, pull the middle part backward also from the fire: which effects, being successive, will exhibit a rotatory regressive motion, quite contrary to what the tube had when supported near it's extremities: and when a single tube lies inclining opposite to the fire, either to the right hand or the left, out of a plane perpendicular to the surface of the fire, gravity will not permit the curved part to rest, but pull it down till it coincides with a plane perpendicular to the horizon; and, consequently, as new curves are generated, new motions will be so too; that is, the tube will be made to move about it's *axis*; but with this difference, when the tube inclines to the right hand, the motion about the *axis* will be from E. to W. when to the left hand, from W. to E. The justness of this reasoning is made manifest with a very little trouble; only bending a wire, and supporting it first near it's extremities, then near it's centre on each side, afterwards inclining it to the right, and then to the left; the bending in every case representing the curved part of the tube next the fire. And that this solution is the true one, seems farther probable from hence, that when 4 supporters were made use of, one at each extremity, and 2 near the middle, there was no motion at all either backward or forward: nor is it of any service to object here, that the increase of contact hinders the motion; because, upon

upon the plane of glaſs, mentioned in my former paper, ſo large as to have a much greater contact with the tube, both a rotatory and regreſſive motion was manifeſt.

An account of a book intituled, De quamplurimis Phoſphoris nunc primum detectis Commentarius. *Auctore* Jac. Barthol. Beccario. *Printed in* 4to *at* Bologna, 1744. *Extracted and tranſlated from the* Latin *by* W. Watſon *F. R. S.* N° 478. p. 81. Jan. *and* Feb. 1746.

Read Feb. 27. 1745-6.

XLIV. The ingenious author does not treat expreſly of thoſe productions of the chemical art, which we uſually call *phoſphori*, but principally of ſuch ſubſtances, whether natural or artificial, which imbibe the rays of light in ſuch quantities, and in ſuch a manner, as to appear luminous for a time, even in abſolute darkneſs.

Before I enter upon the ſubject matter of this treatiſe, I muſt take notice of the *apparatus* made uſe of in theſe inquiries. He cauſed a wooden box to be made, large enough either to ſit in, or ſtand in upright: yet not ſo large but he might with eaſe be carried to any place the moſt convenient for his obſervations. In this box was a little window, in which a cylindrical tube was fitted ſo exactly, that no light could be admitted but through the tube, in which there was an *apparatus* ſo adapted, that the perſon within could place any object proper for obſervation in ſuch a manner, as to receive as much light as it could contain, and then to turn it inſtantly towards his eye, without admitting the leaſt ray of light, beſides that brought in by the object. The inſide of this box muſt be abſolutely dark, without which caution many of the attempts would be unſucceſsful; becauſe the light in many of the ſubjects was neither very lively nor very laſting. Therefore it was neceſſary alſo, that the eyes of the obſerver ſhould be as free as poſſible from the impreſſions of former viſion: ſo that it were proper to make theſe obſervations immediately after ſleep, or to keep the eye you intend to make uſe of ſhut at leaſt ½ an hour before you enter the box. A proof to know whether your eye is properly diſpoſed, will be expoſing a piece of white paper to the light in the tube, and then being able to perceive it's form and colour when turned towards you in the box.

He divides the *phoſphori* into ſeveral kinds; ſome of which ſhine of themſelves naturally, as the glow-worm and dates; or adventitiouſly, as the fleſh of animals, which moſt probably ariſes from a degree of putrefaction, ſometimes too ſlight to be obvious to our ſenſes. Other bodies become luminous by attrition, heat, the free acceſs of air, and, laſtly, by imbibing and retaining the rays of light. Thoſe bodies that are luminous by attrition, are amongſt others, ſome diamonds, and the hairs of animals; by heat, ſeveral ſort of gems, and mountain cryſtals; from the free acceſs of air, the *phoſphori* of *Kraft* and *Homberg*; from the aſpect of light, the *Bolognian* luminous ſtone, the preparation by *Chriſtian Adolphus Baldwin* of chalk diſſolved in ſpirit of nitre, as well as ſeveral others diſcovered by the late M. *Du Fay*, who found, that whatever ſubſtances would, by calcination, be converted into a *calx*, or whoſe concrete, from a ſolution in the acid of nitre, would bear fire enough to become red-hot, theſe bodies were adapted to imbibe and retain light.

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The greateft number of *phofphori* are of the laft-mentioned kind, and thefe are principally the fubjects of this treatife. Some of thefe are natural, others artificial; but of thefe laft the preparation is fo flight, as not to change the nature of their conftituent parts. The natural *phofphori* are either foffil, vegetable, or animal. The foffil are, though very different in degree, fome forts of earths, white fand, lime-ftones, *ftalactites*, and feveral other figured ftones, ifland cryftals, flints, fome fpecies of agates, white arfenic; but no fort of metals, metallic or fulphureous bodies, as jet, amber, except the before-mentioned arfenic. On the other hand, falts imbibe light, provided they are divefted of every metallic principle; otherwife not, though pellucid as poffible. For this reafon, none of the vitriols will imbibe light; but other falts will, though with a confiderable difference as to quantity; for *fal. gem.* and rock-falt imbibe very little; fea-falt, if dry, and in cryftals, much more; and, in like manner, *fal ammoniac.*; more yet, *fal. catharticum* and nitre; weak in the *natron* of the Ancients, and alum; but brighteft of all in *borax*.

In the vegetable kingdom we find very few *phofphori*; that of dry rotten wood is weak, and not lafting; it appears chiefly upon the edges and inequalities of the furface. But this is moft remarkable in the rotten wood of the fir-tree, and fome others, where in the dark you fee fhining fpots as big as tares; whereas, in full light, the whole furface appears alike. Some few barks are luminous, but not confiderably fo; but no fruits, feeds, or their meals. Cotton appears very bright, and the cryftals of tartar; but fine loaf fugar appears the moft luminous of all, both without and within. Gums and refins retain no light.

There is a vaft variety of *phofphori* in the animal kingdom, fuch as the bones and teeth; to thefe may be added the fhells of fifh, egg-fhells, the human *calculus*, bezoar, and in whatever parts of animals the terreftrial principle is very predominant. But where there is a confiderable quantity of oily matter, as in the hoofs, horns, and feathers, no light is manifeft.

Having gone through the natural *phofphori*, he propofes fome queries concerning them; of which the firft is, In what and how great a light the object ought to be placed? He tried different *phofphori* in different degrees of light, and found them imbibe moft light from the fun itfelf; next in quantity, when the fky was clear; and the leaft in foggy weather. Thefe experiments fhould be made in the open air, and not in a houfe with the glafs windows fhut; becaufe many bodies appear luminous, when the light has come directly to them, which will not have that appearance when the light has paffed through the glafs. He laftly tried what light they would imbibe from very bright flame, and found, that alabafter itfelf, which is faturated more than any fubftance by the fun's rays, imbibed exceedingly little. The next query is, How long thefe bodies fhould remain in the light to be fufficiently faturated? 4 or 5″ were found the utmoft length of time required for that purpofe.

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The other query is, How long the received light will continue in these *phosphori?* It does not last the same time in all; but continues more or less, from 2 to 8'', in proportion to the strength of the *phosphorus*, and the quantity of light received.

We pass now to those *phosphori*, which are produced by art; and, first, to them which are made by the maceration of plants alone, and without any fire; such as thread, linen cloth, but, above all, paper. The luminous appearance of this last is greatly increased by heat. This is confirmed by two experiments: the first is, by exposing the paper, spread upon an iron grate, to the naked fire, yet not near enough to scorch it, and then laying a warm brick thereon to retain the heat; by which means it was observed, that where the paper was not skreened by the iron grate, it was most luminous; so that, by the lights and shades, you might distinguish in the dark the image of the iron grate a considerable time. The other experiment is the application of the paper to a plate of warm brass; from which, when in the dark, you might very easily, by it's being less luminous, distinguish the margin of the paper, that had not been warmed by the brass.

He proceeds to take notice of those *phosphori* which become so by the assistance of fire. But the fire here spoken of is not great enough to dissolve their constituent parts, but only such as may affect the external parts of their texture, and that but gently; so that the process here mentioned is only drying or roasting. For it is not the watery or the saline part in bodies which is torrefied; but the oleaginous, wherewith many vegetables, and most animals, abound.

The white flesh of animals, such as that of chickens, becomes a *phosphorus* by roasting, as well as the tendons, and whatever parts of animals become glutinous by boiling, such as carpenter's glue, isinglass, to these may be added cheese. Bones, though they imbibe light without any preparation, have that property in a much greater degree when burnt, and their luminous appearance is much more lively. But roasting has not this effect upon feathers, hoofs, horns, or whites of eggs. The same operation, which produces several *phosphori* from the animal kingdom, gives also several from the vegetable. Thus, by gently toasting, gums, as myrrh, gum tragacanth, and others, appear luminous, tho' different in degrees; and this light is clear, in proportion to the gentle evaporation of their aqueous parts. By this treatment, nuts of every kind, pulse, corn, coffee-berries, meal, bread, and wafers, also become *phosphori*. Turpentine, amber, and some resins, require more fire before they imbibe light; so that you must divest them of their acid, and their light ethereal oil, to make them appear luminous. But here great care must be taken that they boil no longer than from being white they turn yellow; for if you proceed longer, your labour is lost.

It is necessary that you should be acquainted, that those *phosphori*, which are produced by torrefaction, soon lose their power, which, perhaps,

haps, neither time, nor a thorough diffolution of their parts, can deprive the natural ones of. In general, as long as the *phofphori*, gained by torrefaction, preferve their power, their light is more fharp and ftriking, but the natural, more weak. But thofe that are gained by calcination, and *Baldwin*'s *phofphorus*, feem to poffefs both the ftriking light of thofe gained by torrefaction, and the weaker light of the natural *phofphori*: the laft they preferve a long time, but the former is loft by degrees much fooner. The well calcined afhes of plants, or rather their terreftrial parts remaining after the folution of their fixed falts by wafhing, and neutral falts, continue *phofphori* after many years. So that, as far as we can judge, the luminating power which is gained by calcination, tho' not fo intenfe, continues perpetual; whereas that gained by torrefaction always decreafes, and in a very little while is no longer vifible. Some even, by this method, continue to imbibe light much longer than others. Gum *Arabic*, which continues longeft, lafts 6 days; bread, not one; and coffee, only a few minutes. However, at any time, by a frefh torrefaction, you may recover thefe languid *phofphori*; in which property they have great likenefs to the *Bolognian* ftone, and other *phofphori* prepared by art. The *phofphori* gained by torrefaction, as well as that of *Bolognia*, will not imbibe light, while they are warm; and this laft does not appear fo luminous when firft prepared, as when it has been fo fome time.

The natural *phofphori* do not differ only in the before-mentioned particulars, but alfo in the colour of the light itfelf. The light of the natural generally appears either perfectly bright, or fomewhat inclining to yellow: the artificial produces a red, and fometimes a brown light; but there are fome exceptions to both thefe rules. From thefe different appearances, the author conjectures, that there are two forts of fire arifing from different principles; *viz.* that in torrefied fubftances, from a fulphureous, and that of the natural, from a terreftrial principle.

In obferving a piece of *lapis tutiæ*, which was rough and unequal on it's convex fide, fmooth and fomewhat polifhed on the concave; he found, to his furprize, that the rough fide was luminous, and the fmooth one not. He was very defirous of inveftigating the caufe of this appearance. He remembered that fome polifhed marbles did not imbibe light, or very little, and that at their edges; but, having loft their polifh, they did admit and retain it. He therefore conjectures, that bodies, according to the difpofition of their furfaces for the reflection of the light, either fuffer or prevent it's entrance into them. If this pofition holds good in the reflection, why fhould it not with regard to the refraction? He produces 2 experiments, which he apprehends not foreign to the prefent purpofe; but is yet making others, for his further fatisfaction. He expofed a glafs bottle full of well-water to the light, and, as foon as poffible, obferved it in the dark. As he expected, it imbibed no light. Upon pouring into it fome oil of tartar, it became turbid and whitifh, from the well-water being ufually impregnated with calcarious matter. Upon obſerving it then in the dark, after having been expoſed as before, it retained enough of a pale light to diſtinguiſh the ſhape of the bottle. In a bottle of rain-water he diſſolved ſome talc; which ſtone, by rubbing, will diſſolve in water as ſalts do, without rendering it opaque; to this ſolution he added oil of tartar, and this mixture was luminous as the preceding. He therefore concludes, that ſo long as earthy corpuſcles are very ſmall, ſeparate, and agreeing in their ſurfaces with the water in which they float, they readily tranſmit the light they receive; for which reaſon it is impoſſible they ſhould retain light enough to appear luminous in the dark. But, by the affuſion of the ſaline principle, the earthy corpuſcles unite with the water and ſalt; and from the union of theſe principles the mixture grows thick, whereby the ready tranſmiſſion of light is prevented; ſo that, if this mixture is without colour, or any thing metallic, the light will be ſtopped long enough to be viſible in the dark. But if, inſtead of oil of tartar, you add ſugar of lead, the mixture will be turbid, but retain no light. In theſe two experiments the water becomes a *phoſphorus*.

Gems, cryſtals, and glaſs, whether whole, or powdered ever ſo fine, retain no light; ſo that neither their tranſparency nor whiteneſs contribute to their becoming luminous in the dark. Of ſeveral diamonds, in all appearance perfectly the ſame, ſome were very luminous, others not at all. Of many opaque ſubſtances, whether rough, poliſhed, or finely powdered, ſome were luminous, others not. So that it appears, that not only the external, but the internal texture of bodies alſo, may conduce ſometimes to their being luminous.

From the preceding experiments, the author is led to make ſome inquiries into the cauſe of this luminous appearance; and takes notice, that almoſt all bodies, by a proper treatment, have that power of ſhineing in the dark, which, at firſt, was ſuppoſed to be the property of one, and afterwards only of a few. How this is brought about, is not very eaſy to ſolve. If we ſuppoſe with ſome (from which our author, in ſeveral paſſages of this work, ſeems not averſe), that the light from a luminous body enters and abides in the *phoſphori*, we ſhall find ſomewhat new to admire in light itſelf. It is no new opinion, that this fluid conſiſts of very fine particles, which are continually darted forth from a luminous body, in all directions, with a very great velocity: but it has by nobody been laid down hitherto, that theſe particles are not diſſolved by the violence of their agitation, not diſperſed, nor immediately ceaſe to exiſt; but ſubſiſt ſtill, and adhere to what bodies come in their way, as heat does, and are the cauſes of odours. If therefore the particles of light are not diſſolved as ſoon as they are emitted from a radiant body, but continue ſome time, what elſe is required, but that we allow it's atmoſphere to every lucid appearance? If the *phoſphori* ſhine with a borrowed light, but not with their own, and that only when put in motion, and fired by the rays of a ſhining body, which ſome experiments ſeem to confirm, then other new doctrines will ariſe.

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There muſt be then a hidden, a ſecret principle in bodies, to be lighted up by this moſt ſubtle fire. There will be in the univerſe a certain perpetual fire from theſe *phoſphori*; the matter of which, tho' conſtantly diſſipated by burning, does not waſte enough to be obvious to our ſenſes.

I cannot conclude my extract of this author, without mentioning, that his work is the reſult of a great variety of very ingenious obſervations, and of experiments made with the utmoſt accuracy; to which I may add, that where-ever he makes any conjecture concerning their cauſes, he does it with all poſſible decency, and ſubmiſſion to the judgment of the learned.

The Lacrymæ Batavicæ, *or glaſs-drops, the* tempering *of* ſteel, *and* efferveſcence, *accounted for by the ſame principle. By* Claud. Nic. le Cat, *M. D. F. R. S.* &c. *Tranſlated from the* French, *by* T. S. *M. D.* N°. 492. p. 175. Apr. &c. 1749. *Read* June 15. 1749.

XLV. The glaſs-tear, or drop, commonly called *Lacryma Batavica*, or *Lacryma Boruſſica*, becauſe it was firſt made in theſe countries, is much celebrated among Natural Philoſophers, upon account of the ſingular phænomena which it exhibits, and which have for a long time exerciſed their ſagacity.

The make of this drop is as ſimple as its explanation is difficult. It is the work of the meaneſt workman in a glaſs-houſe. On the top of an iron-rod they take up a ſmall quantity of the matter of glaſs in fuſion: they let it drop into a pail of water: the drop makes that part of the water which it touches, to boil with a hiſſing noiſe, as a red-hot iron would do, which it reſembles in that inſtant; and when it does not break in this operation, as it moſt frequently does, it forms the little pyramidal maſs, which is known by the name of a glaſs-drop; the effects whereof I will firſt relate, and then endeavour to account for them.

Experiment 1. This drop is of ſuch hardneſs and reſiſtance, that it bears ſmart blows of a hammer, without breaking.

Exper. 2. Nevertheleſs, if you grind the ſurface of this drop which reſiſted the hammer, or if you only break the tip of the ſmall end or tail, the whole ſhatters into powder.

Exper. 3. This ſhattering of the drops is attended with a loud report; and the duſt or powder to which it is reduced, ſhoots out, and ſcatters all around.

Exper. 4. If the drop be ground with powder of emery, imbibed with oil, it often eſcapes breaking.

Exper. 5. If this experiment be made in the air-pump, the drop burſts with greater impetuoſity, ſo as ſometimes to break the receiver; and it's duſt is finer than when done in the open air; and if it be made in the dark, the drop in burſting produces a little light.

Exper. 6. If this drop be annealed in the fire, it loſes all theſe ſingularities; and being reduced to the ſtate of common glaſs, it eaſily breaks under the hammer; and does not burſt upon breaking the ſmall end.

Exper. 7. The drops that are made by letting them cool in the air, produce no other effects than thoſe which have been annealed.

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The firſt Natural Philoſophers who endeavoured to inveſtigate the cauſe of theſe phænomena, imagined that they found it in the air. Some of them ſuppoſed, that this air was ſhut up in the drop by the cruſt which the cold water forms on it's ſurface while it is yet red-hot; and attributed it's rupture to the violence with which this air iſſued through the too narrow paſſage made for it, in breaking the ſmall end of the drop. Others maintained on the contrary, that the drop, in this ſtate, contained no air at all, nor any thing but particles of fire, or ſubtile matter; or, in one word, a vacuum of air; and that the ſudden burſting of the drop was occaſioned by the impetuous entry of the air into this ſort of vacuum. In fine, the *Carteſians* have ſubſtituted their ſubtile matter in the room of this exterior air, and ſay, that the drop is burſted by the leſs ſubtile particles of this matter; which entering with force into the drop by the opening made therein, and finding large pores on the inſide, and ſmall ones on the outſide, burſt the ſides of the drop, by ruſhing from the centre to the circumference, wherewith it's paſſage is obſtructed.

Meſſ. *Mariotte* and *Homberg* came afterwards; being provided with an air-pump, they cauſed one of theſe drops to be broken *in vacuo*; and *Homberg* having obſerved, that it broke therein better and with a louder report than in the open air; they both inferred, that neither the impetuous entry of the outward air, nor that of a fluid ſomewhat leſs groſs, could be the cauſe of this ſhock; becauſe the receiver of the air-pump is void of theſe fluids; and even if a little ſhould remain therein, it is too much rarefied, and too thin to be capable of ſuch an effect.

Mr *Mariotte*, thro' ſome remains of attachment to an opinion, which he had held to that time, did not intirely exclude the exterior air from the cauſe of the phænomenon of the drop; but thought proper to add another to it; which he makes uſe of as a ſubſtitute in caſes like thoſe of the preceding experiment, where the inſufficiency of the air, or of a fluid nearly ſimilar to it, plainly appears.

Mr *Homberg* ſhews no indulgence to the exterior fluid; and aſcribes the whole to the new cauſe, which is, the quality of tempered glaſs, which the drop acquires, like ſteel, by being thrown red-hot into cold water. This tempering, according to theſe great Academicians, confers at the ſame time more ſpringineſs to the parts, and leſs connection with each other. When a ſteel ſword-blade is bent forcibly, it breaks more eaſily than one of iron; and the jarring which is occaſioned by it's ſpring, is capable of breaking the other parts of the blade: and thus we ſee, that it generally breaks into ſeveral pieces. This blade is the image of the *Lacryma Batavica*, or glaſs-drop.

This is the point to which I found things brought, when I began to ſtudy the phænomena of the glaſs-drop.

The air was partly baniſhed from the inſide of this maſs of glaſs: there is none in the liquid red-hot matter of a glaſs furnace. It was purely out of complaiſance for a generally received opinion, that Mr

Mariotte allowed the exterior fluid any ſhare in the phænomenon; and Mr *Homberg* put the finiſhing hand to it's excluſion. But the ſort of temper given to the drop by plunging it red-hot into cold water, and it's compariſon with tempered ſteel, is not ſo much a cauſe as a compariſon: and moreover, is this compariſon very juſt? Can there be any between a long, thin ſword-blade, which breaks into two or three pieces, and a thick inflexible maſs of glaſs, which flies into powder. The tail alone of the drop might ſeem to favour this parallel: but an experiment, which I made, entirely deſtroys this opinion, and proves, that it is not the ſpring, or the vibrations of the parts of the drop, that occaſion it's burſting.

I put about half the tail of a glaſs drop into a vice between two bits of deal-board of about a finger's breadth. I ſcrewed the vice, till I ſaw this ſmall cylinder or thread of glaſs make impreſſions in the wood on each ſide for its lodgment, in order to be ſure that it could not be ſuſceptible of vibrations. In this condition I broke the end of the tail, ſupporting it on my nail, to prevent forcing any part but the end which I intended to break; and in order to be the more certain of giving no ſhock to the part that was ſqueezed in the vice. My drop flew into powder as uſual; and the portion ſecured between the two bits of wood, perfectly retained it's figure in the impreſſions wherein it was lodged. But when I touched this little cylinder, it was reduced to powder, much in the ſame manner as is ſaid to have happened to ſome men who had been ſtruck with lightning. Now, it was not poſſible for this glaſs to receive, or convey to the body of the drop any vibrations; or if any, they muſt be infinitely ſmall; and yet the effect was preciſely the ſame as uſual. Therefore the ſyſtem of vibrations is not happier than thoſe invented before it.

It is among the glaſs-workers, and in their art, that the ſecret of the *Lacryma Batavica*, or glaſs-drop, is to be ſought; and there it is that I think I have diſcovered it.

All thoſe who have ſeen glaſs-houſes know, that when a piece fails in the hands of a workman, he throws it aſide; and this piece is not long expoſed to the air, before it breaks in pieces: and when the ſame workman has ſucceeded in making a piece, and is willing to preſerve it, he takes great care not to let it cool in the air; but carries it hot into another oven of a moderate heat, where he leaves it for a certain ſpace of time. And this laſt operation is called *annealing the glaſs*.

A Natural Philoſopher, who is witneſs to this management, ought to inquire into the reaſons and neceſſity of it.

How comes it that the glaſs, which cools in the air, breaks; and when it has been nealed, it does not break? This is the reaſon, if I am not miſtaken.

A bit of melted glaſs, red-hot and liquid at the ſame time, is in that ſtate, purely becauſe it's particles are divided by ſo great a quantity of particles of fire, or ſubtile matter ſo violently agitated, that theſe com-

ponent

ponent parts of the glaſs do hardly touch one another: they ſwim, if I may be allowed the expreſſion, in a flood of this matter of fire; and for this reaſon it is, that melted glaſs affects the colours of flame.

When this ſubſtance is expoſed to the air, the coolneſs of this fluid, which touches the ſurface of the glaſs, cools that ſurface firſt; that is, brings the particles nearer together, braces their pores, and thus impriſons the particles of fire, which ſtill fill the inſide of this ſubſtance. While theſe fiery particles find pores enough on the ſurface, to move freely, the glaſs continues whole; but when the glaſs grows colder, that is, when the pores of it's ſurface begin to confine theſe fiery particles; then their whole action is exerted againſt the parts of the glaſs, which they break into a thouſand pieces. In order to avoid this *fracas*, nothing more is requiſite than to keep the pores on the ſurface of the glaſs wide enough, that the fiery particles contained therein may paſs through, and fly off inſenſibly. Now, this is what is done, by putting the hot piece of glaſs into an oven, the moderate heat of which keeps theſe pores open to a certain pitch, and yet allows the glaſs to acquire it's due conſiſtence in this ſtate of middling poroſity: wherein conſiſts the annealing of glaſs and other fuſed ſubſtances.

Hence it appears, that all unnealed glaſs carries within itſelf it's principle of deſtruction, which is the matter of fire impriſoned. But the *Lachryma Batavica*, or glaſs-drop, is in this reſpect, in a worſe caſe ſtill than unnealed glaſs: for beſides that it has not been expoſed to this ſecondary heat, which keeps it's pores open, till the glaſs has acquired it's due conſiſtence, for fear that the coolneſs of the air alone ſhould not cloſe it's pores ſoon enough, and impriſon a ſufficient quantity of the igneous matter, it is ſuddenly thrown into cold water, which by it's coldneſs and weight is fitter than the air to produce ſuch an effect ſpeedily and effectually. Wherefore the only ſurprizing circumſtance in theſe glaſs-drops is, that any of them remain without breaking, by the great quantity of igneous matter ſuddenly ſhut up in them by the cold water. And indeed this accident befals more than one half of them; and thoſe that eſcape, doubtleſs owe their preſervation to the ſpherical or cylindrical figure of the compact ſhell, which the coldneſs of the water forms on their ſurface: for it is well known that this figure produces an equality of reſiſtance on all ſides, which conſiderably increaſes the reſiſting force: and this is the firſt reaſon why, as ſoon as this æquilibrium is broken, either by rubbing away one ſide of this ſurface, or by making a hole in it, or, in fine, by breaking the ſmall end of the drop; the reſiſtance is inſtantly overcome, and the igneous matter, impriſoned within the glaſs, and conſtantly upon the ſtrain againſt it, burſts it into powder.

This deſtroyed æquilibrium is but one diſpoſition that favours the effect of the impriſoned igneous matter: but the communication which is opened for it with the ſubtile exterior fluids, rouſes this matter which is in a ſtate of inactivity, develops it's ſpring, kindles it ſomewhat in the manner of the phoſphorus, which produces no effect while cloſe ſhut up, but takes fire, as ſoon as a free communication with the outward air is given it.

On the union of theſe cauſes depend the phænomena of the glaſs-drop. It is of a hardneſs that reſiſts the ſtrokes of a hammer, becauſe the violent condenſation, given to it's ſurface by the cold water, into which it was thrown when in a ſoft ſtate, rendered it's texture very cloſe, compact, and conſequently hard. It burſts with great noiſe; and in ſo doing it retains the character of all the effects produced by the exploſion of the igneous matter. It's duſt flies two or three feet all around, becauſe it is puſhed forward by the action of a fluid contained in it's centre; which would not happen, if it had been the effect of an exterior fluid. This ſame duſt of the glaſs-drop darts forward with greater force in the air-pump than in the air, becauſe the air is an obſtacle, of which it is freed in the receiver of the air-pump: wherefore it ſometimes breaks the receiver; and for the ſame reaſon it's duſt is finer, that is, more minutely broken, as being done by a ſtronger power, and leſs counter-balanced. This violent exploſion produces light, becauſe the property of ſhining lightning is always the effect of ſuch an exploſion of the matter of fire: wherefore this fact affords another proof, that this matter is the principle of the phænomenon of the drop.

If the ſurface of the drop be ground with fine powder of emery, imbibed with oil, it frequently happens, that it does not burſt; becauſe the ſort of oily maſtic that reſults from this mixture, ſtops the pores of the drop, and prevents the ſudden communication of the exterior fluids with the impriſoned igneous matter; and as glaſs cannot be ground with very fine emery and oil, but by long rubbing; ſuch rubbing heats the drop, and gradually opens the pores ſo as to grant an inſenſible paſſage to the igneous matter, whereby the drop becomes at laſt in the ſame caſe with nealed glaſs; and in the caſe in which itſelf is, when it is put into the oven to be nealed.

When a glaſs-drop is made, by ſuſpending it in the air only, it does not break ſooner than nealed glaſs: becauſe as this ſmall maſs of glaſs retains it's heat a long while in the air, the heat ſerves as a nealing-oven, and keeps it's pores dilated long enough for the igneous particles to find a free paſſage.

The principles, by which I have accounted for the effects of the glaſs-drop, are not confined to this phænomenon alone: they are more general than is commonly imagined. Some corollaries, which I ſhall deduce from them, will prove what I advance.

The tempering of ſteel.

Steel, like the glaſs-drop, acquires it's hardneſs by being plunged into water: and if Meſſ. *Mariotte* and *Homberg* had compared them together in this circumſtance alone, they had been in the right.

The moſt celebrated Natural Philoſophers, in order to account for the tempering of ſteel, have had recourſe to different arrangements of it's it's parts produced by the fire, and fixed, by the cold of the water, in the new state, in which the violent heat had put them.

The mechanism of the tempering of glass-drops, applied to that of steel, is the most simple of all the hypotheses, and answers all it's properties, which are these: 1. Tempered steel has a coarser grain. 2. It is increased in bulk. 3. It is more hard and brittle. 4. By annealing it becomes less brittle.

Steel made red-hot is filled and swollen, and it's pores dilated, by the igneous matter. In this state, the cold water, into which it is thrown, compresses and closes the parts of the surface, while the imprisoned igneous matter dilates the pores within: thus the texture of steel becomes more compact by these two causes, while it's pores are dilated. *Explanation.*

These large pores constitute the coarse grain of tempered steel. It's dilatation by the igneous matter, which could not be thoroughly condensed by the cold of the water, causes it's augmented bulk: the close texture of the substance that surrounds the pores, and the imprisoned igneous matter, occasion it's hardness and brittleness. It's recoction or annealing deprives it of this brittleness, and of a part of it's hardness: because it opens this texture, which it relaxes at the expence of the neighbouring pores, and drives the igneous matter out of it.

The fermentation of acids and alkali's seem, to me to be another corollary of the same principle. *Fermentation.*

1. It is pretty universally allowed, that the acid particles have the figure of small needles; and that alkali's are spheroidal or polyhedrous bodies with a vast number of pores proper to admit the acid needles.

2. Experience shews, that salts are alkalised by fire, and that our juices are alkalised by heat, *&c.* What can the repeated action of the fire produce on salts, in order to alkalise them? It calcines them, blunts their points, and hollows them with a vast number of pores; and we see with the naked eye, that calcination has this effect on all bodies. In a word, it converts an angular very solid body into a very porous and light spheroid; and this body is an alkali by the first supposition.

3. Calcination introduces, and generally leaves in the pores of the calcined body, after the operation, a great quantity of igneous matter. This matter is perceptible to the senses in the *Lapis Bononiensis*, which becomes a phosphorus by calcination; in lime-stone, which by calcination is furnished with so great a quantity of igneous matter, that in the effervescence, which is raised in it by throwing a little water on this stone, you may kindle sulphur or a match by it. The alkaline, or alcalized salts also, that is, those which are calcined, have their pores full of the igneous matter.

4. Such is the nature of the igneous matter, that it tears asunder whatever opposes it's passage, and makes it fly off with a report. This principle is universally allowed: the effects of gun-powder, of volcano's and earthquakes, prove it: and to come nearer our subject, unnealed

unnealed glaſs breaks in the air, and the *Lacryma Batavica* does as much upon breaking it's ſmall end.

Whereas an alkali is a ſpongious body filled with the igneous matter, and an acid are points proportioned to theſe pores; theſe ought to be regarded as ſo many pegs or pins, which enter into the holes on the ſurface of the alkali, and fill them up exactly: whereby the igneous matter is impriſoned; and by the preceding principle it burſts the alcaline globule with noiſe, and ſcatters around the acid pegs, in the ſame manner as it burſts the glaſs-drop.

A mixture of an alkaline and acid liquor being compoſed of an infinite number of ſuch particles that burſt and broke to pieces, the liquor muſt take up more room, or ſwell. The particles of air therein contained, being toſſed about by all thoſe little exploſions, together with the neutral liquors, which are a vehicle to the ſalts, form the ſcum or froth; and the igneous matter, which gets out of the alkali's, and is agitated by the ſhocks of all theſe exploſions, produces heat, drags with it the aqueous and other volatile particles, which form the ſteam.

Yet there are cold fermentations, becauſe then, either the motion of the particles of fire, and their *fracas*, is inconſiderable; or becauſe theſe particles fly off eaſily by a direct motion. Moreover, at this day that we have it in our power to be convinced, that the *bruſh* or ſtream of electric matter is very cold, nobody will be ſurprized, that a ſtream of the matter of fire may produce cold.

If all the alcalious corpuſcles burſted at once, the fermentation would laſt but an inſtant: but as the acid liquor requires a certain ſpace of time, to penetrate the whole alcaline liquor, and fill the pores of the alcalious corpuſcles, the fermentation is performed ſucceſſively in a certain number of corpuſcles at a time, until they are all broken: and this ſucceſſion conſtitutes the duration of the fermentation; which ceaſes when there are none of the alkali's left entire.

Theſe principles not only ſerve to explain the fermentation which reſults from the mixture of acids and alkali's, but alſo almoſt all the motions of this kind, which are occaſioned by the mixture or penetration of two or more ſubſtances.

For example; lime, which we have mentioned above as a body filled with the matter of fire, and which produces an effervеſcence capable of lighting ſulphur, if water be thrown on it; lime, I ſay, produces this effect, only becauſe the particles of water, which enter into it's pores, have a tendency to ſhut up the igneous particles more cloſely. It is by a mechaniſm entirely ſimilar, that *Homberg*'s phoſphorus kindles into flame, upon being expoſed to the air: 'tis upon this principle likewiſe, that a mixture of ſpirit of wine and water acquires a new degree of heat; and ſo of other phænomena of this nature.

CHAP.

CHAP. II.

HYDROLOGY.

I. THOUGH the ſpring called *La Fontaine du Salût* is at a good diſtance from the town of *Bagneres*, it is, nevertheleſs, as much frequented as any in that country; and, beſides it's admirable effects in curing a great number of diſtempers, it likewiſe offers, to the eyes of the lovers of Natural Hiſtory, a very remarkable ſingularity.

Of the Fontaine du Salût, *near* Bagneres *in* Gaſcony: *with other obſervations; communicated by M.* Secondat de Monteſquieu, *of the Acad. Sc. of* Bourdeaux; *in a letter to M.* Folkes, *Eſq; Pr. R. S.* N°. 472. p. 26. Jan. &c. 1744. *Read* Mar. 8. 1743-4.

In the firſt bath, through which the largeſt of the two branches of the ſpring flows, there are found, from time to time, ſmall ſtones, of the colour of iron-ruſt, and of a regular figure; being either parallelopipedes with oblique angles, of which the ſides are unequal; or ſmall ſolid bodies with 6 ſides, only differing from cubes or dice in this, that the ſurfaces are not perfectly perpendicular one to another, but a little inclined; as alſo commonly longer than they are broad, and broader than they are high.

The largeſt which I have ſeen were but 11 lines in length, $9\frac{1}{2}$ in breadth, and 6 in height: they are moſtly a great deal ſmaller. I have one which is very odd, being a parcel of 100 in one lump. There are ſome on which one may obſerve ſhining *ſtriæ*, that ſeem to be of a metallic ſubſtance.

I have heard, that a great many ſtones like theſe are alſo found on the ſides of a brook in *Spain*; from whence, without doubt, they got the name they are commonly called by, of *Ferreles d'Eſpagne*.

About two months ago, happening to take a walk in the road newly made between *Bagneres* and the *Fontaine de Salût*, I perceived, that, in digging the ditch on the ſide of the road, the workmen had laid open a rock of a ſort of imperfect ſlate, but ſofter, and of a lighter colour, than ſlate commonly is. The rock itſelf is compoſed of layers or beds lying almoſt parallel one over the other: the ſubſtance of the ſlate ſeems to be a compoſition of fibres or ſtrings, placed on the ſides of each other, and equally inclined to their beds or layers; whence it comes, that, upon breaking them with a hammer, the pieces, ſometimes, are pretty like the figure of a regular parallelopepid with oblique angles.

Upon a narrower examination of this ſort of ſlate, I found a great number of parallelopiped ſtones, like thoſe before ſpoken of, only ſmaller: I have ſeen them of all ſizes, from thoſe in which the largeſt ſide is but of 2 or 3 lines. I obſerved alſo, after having broken to pieces ſeveral little bits of ſlate, certain black ſpots; which, by the help of a microſcope, I found to be real figured ſtones.

Beſides this, I took notice, that every one of theſe ſtones, as long as it remains in the rock, is always found between two bundles or cluſters of of tranſparent fibres, of which, generally, one is placed on the one, and the other on the oppoſite ſide. Theſe bundles are larger in great ſtones: thoſe which ſeem, to the naked eye, to be but ſmall black ſpots, are, nevertheleſs, accompanied by their bundles.

I have ſome of theſe ſtones, where the tranſparent fibres, of which every bundle is compoſed, had left a vacancy in the middle of their *axes*: this ſort of conduit being coloured with a matter of a ruſty colour, one finds likewiſe, ſometimes, between the fibres a little of this ruſty-coloured matter; and now-and-then metallic and ſhining veins. One might ſay, that the uſe of theſe tranſparent fibres is the ſame as of ſtrainers; which (let the matter be of a metallic nature or not) ſuffer nothing to paſs thro' them to the little ſtones, but ſuch particles as are proper to advance their growth, and ſo to ſerve them as it were for roots. In bits of imperfect ſlate, though harder, and of a bluer colour, than the ſort I before mentioned, are found ſmall ſtones, of a like figure, but different in this, that they are of a fine and ſhining braſs colour. They are, as well as the others, accompanied by tranſparent lumps.

One meets likewiſe with large ſtones of the colour of iron-ruſt in ſeveral rocks thereabout. It is probable, that the *Fontaine de Salût* paſſes thro' one like that I have deſcribed; and, if it meets with any of theſe figured ſtones in the ſides of the conduits thro' which it paſſes, it eaſily looſens them, and carries them along with it. The bundles of tranſparent fibres ſtick pretty faſt to the ſlate or rock, but are ſlightly faſtened to the little ſtone, from which they are very eaſily ſeparated. Hence it comes, that all thoſe which fall into the bath, or *Fontaine de Salût*, are got thither without their bundles.

The formation of theſe ſtones deſerves to be examined: if it was carefully ſtudied, it might, perhaps, give light to the formation of other figured ſtones. It even ſeems, that, by beginning with this figure, which is the moſt ſimple that one can imagine, one may the rather hope for ſucceſs in the like inquiry.

This parallelopiped figure with oblique angles is common to many ſtones in the country of *Bagneres*, and the neighbouring mountains. Several cryſtallizations of the grotto of *Campan* break into fragments of this figure: thoſe which hang down from the top of the vault of that grotto, are, originally, ſmall hollow pipes, formed by the water which trickles down drop by drop; and whoſe outer ſurface, fixing themſelves by their ſmall baſes, forms, by degrees, a ſort of blunted pyramids, which, like ſo many rays from the axis, which is nothing but the hollow pipe, become ſolid at laſt. This axis ſeems compoſed of plates, almoſt cylindrical, laid one over another; but, if broken, the whole divides into fragments of a parallelopiped figure. The blunted pyramids, that are about the axis, divide themſelves at firſt into other blunted pyramids; but, afterwards, almoſt all theſe fragments divide of themſelves into other fragments of a parallelopiped figure.

The

The ſtone of the mountain of *Barege*, upon which the *aſbeſtos* grows, breaks alſo conſtantly into fragments of the ſame figure.

I have likewiſe preſerved a bit of rock half tranſparent, the fragments of which are like the others.

Having ſeen ſeveral productions of nature, in which one diſcovers, that the figure I have been ſpeaking of ſo remarkably prevails, I was nevertheleſs ſurprized, when I found the ſame figure in the ſediment of the water of the *Fontaine de Salût*.

I had let a conſiderable quantity of the water of the mineral ſpring evaporate; there remained a ſhining duſt, in which I could diſtinguiſh nothing. I then looked at it through a microſcope; and, among ſeveral cryſtals of a leſs regular figure, I found many which were quite regular and well-ſhaped, with ſix faces, and oblique angles. Several perſons, who have, at different times, been eye-witneſſes of this ſight, have been well ſatisfied with it.

The waters of this ſpring contain no iron, as it is commonly believed. When you put the tincture of galls in it, it grows neither black nor red: this mixture only turns it a little, and makes it look whitiſh, after having ſtood ſome hours.

When theſe waters are evaporated by a mild and equal heat, the ſmall cryſtals are found ſwimming on the ſurface; where they join, and form a film upon the water; ſome of which ſticks alſo to the ſides and the bottoms of the veſſel. Thoſe cryſtals which are formed firſt, are inſipid; but thoſe which are produced towards the end of the evaporation, are, indeed, of the ſame ſhape, but of a tart and ſaltiſh taſte. There remains yet a little of this matter, which cannot be reduced to very regular cryſtals: it is of a very ſharp and pungent ſavour, but has nothing of the prevailing character of acid or alkali; at leaſt, it makes no ſenſible impreſſion upon blue paper.

The waters of the ſpring *du Pied* have the ſame quality as thoſe of the *Salût*: they produce the ſame effect when mixed with galls; yield cryſtals exactly of the ſame figure, but in yet greater quantity.

I have not made the ſame experiments with the other ſprings at *Bagneres*; but, it is probable, they do not differ from the former, except as to more or leſs.

Of the hot ſprings at Carlſbad; *by* James Mounſey, *M. D. Phyſ. to the* Czarina's *army.* N°. 493. p. 219. Oct. &c. 1749. *Read* Nov. 23. 1749.

II. *Carlſbad* is a ſmall town not far from the frontiers of *Saxony*, ſituated in an hollow between two high mountains: a ſmall river called *Toeple* runs through it from S. E. to N. W. The principal fountain riſes on the N. E. ſide, about 20 paces from the river, and about 5 or 6 feet higher than the ſurface of the water. This ſpring riſes through a ſquare tube of wood, whoſe diameter is about 7 inches, with a conſiderable degree of violence: whence it is called the *Sproudle*, or *Furious Fountain*. It comes from the mountain on the other ſide, and paſſes underneath the river, where the petrifying quality of it's own water has formed for itſelf an aqueduct of *Tophus*, through which it is conducted to this place. Sometimes this aqueduct is ſo filled and choaked up with the *tophus*, that it burſts into the river, and puts the inhabitants to a conſiderable expence for repairing it. But to prevent this, they bore and clean it every year near the fountain. It forms rocks of *tophus* along the river-ſide, compoſed of *ſtrata* of ſeveral colours, according as the water has been impregnated with different matter, or perhaps from the difference of heat or cold, or the impreſſions of the air at the times of forming the *lamellæ*. This *tophus* is hard, and receives a good poliſh, and of it they make ſnuff-boxes, heads of canes, and other toys. Some years ago, in digging to lay the foundation of a church, 40 or 50 paces higher up the hill, they found vaſt quantities of this *tophus*, which was in many places ſo decayed and rotten (reſembling very ſoft clay), that they were obliged to dig ſeveral fathoms deep, before they could find a ſolid foundation. Here they threw out great quantities of the *piſolithus*, of the kinds I ſend you, which are compoſed of the ſame matter as the *tophus*, though of a very different conſtruction: the *tophus* being made up of plains joined together, whereas the *piſolithi* are globular, and compoſed of ſeveral ſpherical ſhells. Some globules are found above an inch in diameter, but more commonly about the bigneſs of white peas, and decreaſing gradually in ſize till they become as ſmall as fine ſand, and at laſt common *tophus*.

The ſeveral ſhells which compoſe theſe globules differ in colour as the *lamellæ* of the other *tophus* do: but theſe ſhells ſeparate more eaſily than the *lamellæ*, and ſhew that the colour often conſiſts in one very thin ſhell between two thicker ones.

Such kind of *tophus*, with *piſolithi*, is found at other places; but I have never yet met with any body who could give a ſatisfactory account of it's formation. Some think the *piſolithi* are drops of water petrified, as they are found commonly near falls of water which is impregnated with the like ſtony matter: and as the ſmalleſt ſcattered drops of water fly fartheſt from the centre, they ſo account for the gradual diminution in magnitude of the *piſolithi*. Others affirm they grow from the vapour of this petrific water, though they cannot tell how.

Amidſt this uncertainty, I determined, whilſt I was on the ſpot, to ſpare no pains to ſearch after (and diſcover, if poſſible) the manner how theſe regular globous bodies are produced. I have already told you, that the waters of theſe hot ſprings at *Carlſbad* are ſo replete with tophaceous matter, that where-ever they run, maſſes of *tophus* are formed; and when theſe waters are cold, a ſcum (like the little ſcales of the ſame matter) riſes on the top, ſome of which I ſend you, and I believe you will think it, on examination, little or nothing different from the ſubſtance of the component matter of the *piſolithi*, or from that which forms the common *tophus*, which I ſuppoſe to differ from the *piſolithi* only in appearance.

Firſt of all, I obſerved in the chinks and hollows of the ordinary rock-ſtone very ſmall *moleculæ* looſely adhering; I found alſo cluſters of of *piſolithi* in the like places, and on breaking up a piece of the rock by the ſide of the river, where it had been burſt by the water, I diſcovered maſſes of *piſolithi* lying in the chinks, and many looſe ones twirling round and played about in the bubbling water. My ſuppoſition therefore is, that the ſtream deſcending from the body of the oppoſite mountain, paſſing beneath the river, and afterwards burſting out on the ſide of the hill with a conſiderable force, could not fail to form caverns in the ſide of the mountain, and to change it's current as the paſſages became choaked up with the tophaceous matter: and as I found vaſt numbers of *moleculæ* like grains of ſand in the chinks where water paſſed, theſe being waſhed off might ſerve for *nuclei* to the *piſolithi*, and being kept in continual motion by the *vortex* or whirling of the water, would acquire a globular figure, and by receiving new ſhelly coats, from time to time, would increaſe in bulk, ſo long as they were ſuſtained, and whirled about in the water. And as in this caſe ſome would be precipitated ſooner, and others later, a difference in ſize muſt conſequently happen, and their arrangement muſt be according to their proportion of ſurface and gravity, till the place becoming full of ſuch matter, the water was obliged to ſeek out a new paſſage. At the formation of this kind of *tophus* in the caverns, ſome intervening accidents from the motion of the medium, the influence of the air, and other concurring cauſes, have ſometimes ſo far prevented a compact and firm conjunction of the component particles, that in ſeveral places it ſeems in a decaying ſtate; and is even ſoft as clay. In the air indeed it grows again ſomewhat harder, but then it is porous and light: and they call it *Sproudle Sand*. The inhabitants of a houſe near the church have a hollow, out of which they take this, and ſell it for the ſcouring and poliſhing of ſilver, *&c*. In this hollow it is very warm and ſuffocating, eſpecially in rainy weather, and then there riſes from it a ſtrong vapour.

On the other ſide of the river, at the foot of the mountain, are a good many houſes, and a broad ſtreet; croſs under which the ſtream runs, and in the winter no ſnow lies on the place where it paſſes. Some rooms in a houſe built here are always warm like a bagnio, and in one of the cellars may be heard the noiſe of the water running under ground. Along this ſide of the river are ſeveral hot ſprings, which differ in quality from one another, as well as from the water of the *ſproudle*. The principal of theſe is called the mill-fountain (from it's being near a mill) which is much uſed, and reckoned milder than the *ſproudle*. It is not near ſo ſaturated with the limy matter, and forms ſcarce any *tophus*.

Theſe ſprings either have different origins, or elſe the great ſtream divides in the body of the mountain into ſeveral branches; which, according to the nature of the paſſages they run through, or from the different thickneſs of their columns, and the velocity they move in, are impregnated with different matter, and when cold precipitate more or leſs calx; but their ſalts are the ſame, nor is there much difference in the quantity they yield. The *ſproudle* is ſo full of the ſtony matter, that any thing laid into it is covered over with a thick tophus in a few days. When the water is taken up, and let ſtand a little in the air, it incruſts the veſſels that contain it, and it's ſurface is covered with a ſcale, like lime-water, which is made uſe of as a dentifrice.

I don't propoſe to inform you of the medical virtues of theſe waters, nor to enter into phyſical accounts of their origin: I have only in view to ſatisfy you about the formation of the ſpecimens I ſend you.

Moſt of the rocks about *Carlſbad* are an aggregate of *ſpatum*, *mica*, *quartzum*, *rubrica*, *cum matrice lapidis calcarii*, and cleave into rhomboids. The ſoil on the ſide of the mountain is made by the diſſolution of ſuch rocks intermixed with ſome vegetable earth; and the whole ſurface is covered with the leaſt diſſolvable parts, often adhering together in maſſes by the intervention of a limy matter like incruſted ſpatum. And I found higher up the mountain ſome rocks moundering into ſuch ſoil.

The *Carlſbad* waters give a good deal of neutral ſalt by boiling and cryſtallizing. From 1080 ℔. of water xxii ℥ of pure ſalt. I ſend you ſome which I prepared myſelf, ſuſpecting the Apothecary might adulterate it to increaſe the quantity.

My thermometer being broke, I procured one of a friend: but not knowing of what conſtruction it was, I tried it in the following manner: in melting ice the mercury fell to 28 ¼ of it's equal parts, and by the heat of my body it roſe to 66 of thoſe parts. This thermometer held into the *ſproudle* fountain roſe by it's head to 96, and in the mill-fountain to 67.

About twenty miles from *Carlſbad* to the S. W. near the town of *Eyra*, is a cold ſpring of mineral waters, much in uſe in theſe countries. This gives alſo a ſalt much of the ſame kind. To the ſouth from *Carlſbad* about twenty five *Engliſh* miles are likewiſe ſeveral cold ſprings: one of which is much richer in this ſame kind of ſalt than the former. It belongs to the monaſtery of *Toeple*. In the winter, when they boil this water, from x ℔. of water they get ſometimes above ℥i of ſalt. They prepare here a neutral ſalt, by adding a mineral acid, or perhaps ſome other neutral ſalt (but the preparation they keep a ſecret) which makes it ſhoot into beautiful cryſtals. It is called *Sal Medium Toeplicenſe*, and is ſold in many places of *Germany*. I ſend you ſpecimens of all theſe. On expoſing theſe ſalts ſome time to the air, they fall into a *magneſia*, but diſſolving and cryſtallizing them again recovers them; though the oftener they are diſſolved, the cryſtals ſhoot the ſmaller.

About 7 miles S. W. from *Carlſbad*, at *Altſettle*, are mines of black *ſchiſtus*, and formerly they made a great deal of alum and vitriol from it; but it is now neglected, as they find in the ſame mines plenty of *gleba pyriticoſa*, from which they diſtil ſulphur. Six hundred weight of this *pyrites* give one of ſulphur: and the oven makes from one to two

two hundred weight per week. The reſiduum being thrown in great heaps in the open air, takes fire, and conſtantly ſmokes. This matter they throw into large reſervoirs of water, which afterwards they let run off into the boiling-houſe, and ſo make copperas.

About 9 *Engliſh* miles to the ſouth from *Carlſbad*, are the tin mines of *Schlachtenwald*. They reckon this mine has been wrought near 500 years. There are five entries, four whereof are provided with machines for hoiſting the barrels with the tin ſtone: the fifth is for drawing the water out of the mine. The number of miners who work below ground are 90: each man delivers 25 barrels of this ſtone per week, and receives ſomething leſs than half a crown wages. They have different inventions in the mine for ſplitting the rock, but the moſt effectual one is burſting it with gunpowder. The whole people employed in theſe mines are about 300. The main body of the mine is nearly 700 feet in diameter, and from this go ſeveral E. and W. for ſo the mineral runs. The broadeſt of theſe ways is about 2 feet, and the mineral in theſe veins is richer than what is found in the main body of the work, whoſe greateſt depth is 650 feet. The tin-ſtone is firſt burnt in kilns, which they ſay betters the tin conſiderably, and makes it much more eaſy to ſtamp. After this preparation it is brought to the ſtamp-mills, where by ſtamping it becomes like grey river ſand, which they waſh and ſeparate the tin from in the following manner. They throw it by ſhovels-full into baſons where there paſſes a current of water, and by keeping of it ſtirring it runs over by a broad conduit deſcending by ſteps, which are covered with coarſe linen cloth; and by this operation the ſand is waſhed away, and the tin remains on the cloth in form of a black ſcaly powder, and dried is fit for melting. One hundred weight of the ſtone gives only ℥iii of tin; and 150 ℔. of the clean-waſhed tin-mineral give 140 ℔. of tin. There are ten melting-ovens, each whereof can melt 9 or 10 hundred weight in 24 hours; the breadth of theſe ovens within-ſide is 8 or nine inches, and from 10 to 12 feet long, blown by two pair of bellows. The proportion of charcoal to the metal is near an equal weight. They are thrown into the oven by degrees, alternately: the reſiduum they melt three times over, which always yields new metal. They make here about 800 centers *per annum*, which is ſold from 53 to 56 imperial gouldens *per center*. They find ſometimes the black and ſometimes the white cryſtal mineral in neſts, or cluſters: the *ſtannum poledron nigrum* is a very pure and rich tin ore: they ſay the white is rich alſo, but 'tis ſo hard and difficult to melt, that the tin is burnt to an aſh before it can be brought to fuſion.

Near *Geffries*, in *Bareith*, they boil vitriol. The mineral from which they make it, is a black *ſchiſtus*, ſome of it too is brown. It has ſeveral ſmall veins of pyrites in it. When firſt taken out of the pits it has no taſte, but after it has been expoſed ſome time to the weather, and begins to moulder, it acquires a very ſharp taſte. It is laid in great heaps, under which there are ciſterns for receiving the water that runs from

from it after rain, or that they pump upon it when the weather is dry. This water is conveyed by conduits into the boiling-houſe, where there are two leaden kettles, in which it is boiled to a ſtrong lee, and then let off into receivers where it ſhoots. Theſe two kettles make from 8 to 9 hundred weight *per* week, which is all wrought by two ſervants: it not having been found neceſſary to add any new mineral to the heaps theſe 15 years paſt, as they aſſured me. But as the quantity of the mineral conſumed in that time is not known, it is impoſſible to determine how much of this ſalt has been ſupplied by the air. They only add to the quantity half an hundred weight of iron, which is conſumed in the kettles every week, and makes it ſhoot into copperas; but in place of this, if they add copper, it makes blue vitriol. Formerly they made alum here likewiſe from the ſame lee, only inſtead of iron or copper they added pot-aſh and urine: but the expence of the firſt, and the difficulty of getting the other in ſufficient quantity, has made them leave off making alum here for ſome years paſt.

An examination of the ſtrength of ſeveral of the principal purging waters, *eſpecially of that of* Jeſſop's Well; *by the Rev.* Stephen Hales, *D. D. & F. R. S. communicated in a letter to* C. Mortimer, *M. D. Secr. R. S. with a letter from* Swithin Adee, *M. D. F. R. S. to Dr* Hales, *on the virtues of the ſaid* well. N°. 495. p. 446. Apr. &c. 1750. *Read* May 24. 1750.

III. *An account of the ſeveral quantities of* ſediment *which were found in a pound averdupois of the following* purging waters, *evaporated away to dryneſs, in* Florence *flaſks, cut to a wide mouth*; viz.

Grains

1. *Marybon-fields* near *London* ——— —— —— 24
2. *Peterſtreet Brewhouſe, Weſtminſter* ——— —— 27
3. *Ebſham* —— —— ——— ——— 34
4. *Scarborough* ——— ——— —— 40

And it was found nearly the ſame by Dr *Shaw* and Dr *Short*: a little more or leſs, according to the wetneſs or dryneſs of the ſeaſons $\frac{1}{1.5}$ of this in calcarious matter; the reſt, moſtly what is called nitrous ſalts, on account of the oblong cryſtals which it ſhoots into.

5. *Dog* and *Duck, Lambeth* ——— ——— 40½
6. *Kilburn*, four miles from *London*, in the way to *Edgware* 43
7. *Acton* —— ——— — — 44
8. *Cheltenham, Glouceſterſhire* ——— —— 60

Dr *Short* found the following proportions in *Cheltenham* water; viz.

Sept. 1738. calcarious ſediment —— —— $\frac{1}{10.5}$ of 74
Dec. 1738. —— —— —— $\frac{1}{14}$ of 42
July 1739. ——— —— $\frac{1}{9.7}$ of 70

He ſays it is the beſt and ſtrongeſt nitro-calcarious water in *England*, very bitter, having only a little ſubtil impalpable earth mixed with it's ſalt.

9. *Cobham Well*, a mile ſouth of *Church Cobham, Surrey*, once 68 grains, another time 60 grains —— — —— 68
10. *Jeſſop's Well*, on *Stoke Common*, in Mr *Vincent*'s manor, about three

three miles S. of *Claremont*, *Surrey*, *Sept.* 11, 1749. after long dry weather, 82 grains in a pound of the ſurface-water 82

October 16, after a conſiderable quantity of rain, the ſurface-water yielded but 60 grains. *Nov.* 21, the ſurface-water yielded 65 grains.

This great inequality of the ſtrength of the ſurface-water put me upon trying whether the water at the bottom of the well, near the ſprings, were ſtronger than the ſurface-water. And in order to this, I procured, *Dec.* 11. a bottle of the water near the bottom, which was ten feet below the ſurface of the water; which was done by tying an empty bottle to the end of a long pole, with a line fixed to the cork, to pull it out when at the bottom, for the water to fill it: and I had at the ſame time another bottle full of the ſurface-water. The lower water yielded 82 grains; the ſurface water but 48 grains; and it was the ſame upon a ſecond evaporation of thoſe waters. Hence we ſee how much ſtronger the water near the bottom is, than at the ſurface; even when the preceding rains have been but moderate; for they had not as yet been ſufficient to raiſe the ſprings in this country much. Hence we ſee that the ſtronger lower water may eaſily be come at by means of a pump; as alſo, that the upper land-ſprings, ſoon after rains, make the water near the ſurface weaker: but, in long dry weather, when there are no land-ſprings, the ſurface-water, and that at the bottom, are nearly of an equal ſtrength: for it requires time for the ſaline mineral virtue to be equally diffuſed through a maſs of that depth of water, whoſe upper part is inceſſantly weakened by a land-ſpring of freſh water.

Hence we ſee how adviſeable it is, in order to keep out the land-ſprings, to dig a narrow trench ſome feet depth, round the well, to be filled with ſtiff clay well rammed.

The mineral virtue in this water ſeems to be much like that of *Cheltenham*, in it's ſhooting into very bitter, regular, oblong cryſtals, which are, on that account, called nitrous; though they are not a true nitre; for neither theſe, nor thoſe of *Cheltenham*, will deflagrate or flaſh in touch-paper, nor on burning charcoal, as true nitre will do; ſome of which ſtill retain their form and firmneſs for 17 months ſince they were cryſtallized; whereas the cryſtallized ſalts of ſeveral other purging waters have crumbled, and in a great meaſure waſted, away in much leſs time: a greater proportion of the ſalts of *Jeſſop*'s *Well*, ſhoot into oblong cryſtals than thoſe of *Cheltenham*; and it's water alſo gives a ſtronger green tincture, with violet-flowers. The purging quality reſides chiefly in theſe cryſtalline ſalts, and a ſmall proportion of common ſalt; ſome of which there is in all theſe mineral waters.

The proportion alſo of it's earthy calcarious matter, is but $\frac{1}{112}$ part of it; which, like that of *Cheltenham*, is but little, in compariſon of the much greater quantity of it in other purging waters: it is alſo ſoft

and

and impalpable, like that of *Cheltenham*, and not harſh and coarſe, as it is in ſome other purging waters.

And as the quantity of purging ſalt in this water is conſiderably greater than in any other, ſo it is found by experience, that, proportionably, a leſs quantity of it ſuffices, which makes it ſit the better on the ſtomach. It is alſo obſerved to exhilarate thoſe who take it.

It was obſervable of the ſediment of ſeveral of theſe waters, that, when dried, and while hot, there aſcended plenty of inviſible volatile ſalt fumes, ſo pungent that the noſe could not bear them. Hence we may reaſonably conclude, that the waters which abound moſt with purging ſalts, ſuch as thoſe of *Jeſſop*'s *Well*, ſhould be proportionably preferable to weaker waters, which are ſtrengthened by boiling half away; whereby not only the more ſubtil active parts are evaporated; and thoſe that are left are decompounded, and formed into new groſſer combinations; as are alſo the calcarious particles, which are ſo fine as to paſs the filter before evaporation, but not after it. This was the reaſon which induced me to examine, by various repeated trials, and to give an account of the ſuperior ſtrength of *Jeſſop*'s *Well* water, above all others that I have examined or heard of.

When *Jeſſop*'s *Well* was cleaned, *Oct.* 16. 1749. after a conſiderable quantity of rain, after about half a foot depth of black muddy filth was taken out, then the natural fat ſandy-colour'd clay-bottom appear'd; thro' ſeveral parts of which the water ouzed up at the rate of 160 gallons in 24 hours.

The water which then came freſh from the ſpring gave a weak bluſh with galls; but when put into bottles it did not do ſo next day; a ſign that there is ſome degree of ſteel in it.

It was very obſervable, that the man who ſtood about 3 hours barelegged in this well-water to clean it, was purged ſo ſeverely for a week, that he ſaid he would not venture, on any account, thus to clean the well again. And it was the ſame with another man, who cleaned the ſame well about 12 years ſince. And I am credibly informed by a merchant, that, being in a warehouſe in *Egypt* to ſee *ſenna* bailed up, it had the like purgative effect on him.

In order to get a ſatisfactory account of the efficacy of theſe waters, I deſired Dr *Adee* of *Guilford*, who has long preſcribed them to his patients, to give me his opinion of them; which he has done in the following letter; *viz.*

SIR, *Guilford, March* 14, 1749.

I HAVE found very advantageous and uncommon effects from the uſe of the waters of *Jeſſop*'s *Well.* Some of my patients who have drank them ſteadily and cautiouſly have been cured of obſtinate ſcurvies. As I had a long time ago reaſon to think there was a fine volatile ſpirit in them, I therefore obliged ſome to drink them for a courſe of time at the well as an alterative, with very happy conſequences. When I have ordered

ordered them as a purge, they have worked very ſmartly, but have not diſpirited. I am glad to have it in my power to confirm your ſentiments by my own obſervations; and am ſatisfied theſe waters, if continued a proper time, and taken in a proper manner, may be rendered very beneficial to mankind, according to the beſt opinion that I can form.

IV. No accounts of the flux and reflux of the ſea were ſatisfactory, till Sir *I. Newton*'s penetrating genius deduced their true cauſe from the laws of gravitation. His principles carried ſuch conviction along with them, and gave ſuch an eaſy ſolution of ſome of the moſt remarkable *phænomena*, that mankind ſeemed to imagine a thorough knowledge of the tides might be obtained from an attentive conſideration of the principles he had eſtabliſhed, without the trouble of further obſervations; but, as he, and all Philoſophers ſince his time, have conſidered only, or principally, the influence of the moon in elevating or depreſſing the tides; their ſeveral directions, velocities, and other affections, reſulting from the influence of land, ſhoals, and winds, remain ſtill as inexplicable, and as little known as ever.

The ſtate of the tides *in* Orkney; *by Mr* Murdoch Mackenzie. N°. 492. p. 149. Apr. &c. 1749. *Read* May 25. 1749.

As a diſtinct knowledge of theſe things is not only conducive to the advancement of ſcience, but would greatly contribute to a convenient and ſafe navigation, it may not be unacceptable to communicate ſuch remarks on the tides about the *Orkney* iſlands, as came under my obſervation, while I was employed in ſurveying and navigating that and other adjacent places; hoping it may incite others to explore the various motions of that element, on which ſuch a conſiderable part of the world are daily employed, in a more extenſive and accurate manner than has yet been done.

From ſome obſervations on the tides in *Orkney*, I incline to think the water begins to riſe and fall ſooner near the ſhore than at a diſtance from it.

When ſpring-tide is at it's greateſt altitude, or depreſſion, the water continues in a quieſcent ſtate near half an hour: neap-tides continue ſo about an hour and a half.

The motion of the water, both in aſcent, deſcent, and progreſſion, is accelerated from the firſt to the fourth hour, commonly; from the fourth to the laſt hour it's velocity diminiſhes. This, however, admits of ſome variation from the influence of winds.

The greateſt ſpring-tides, and leaſt neap-tides, are commonly on the third or fourth day, after the ſyzygies and quadratures; but in this alſo the winds have a conſiderable influence; W. and S. W. winds making the greateſt floods, and leaſt ebbs; N. and N. E. winds, on the contrary, making the greateſt ebbs and leaſt floods in *Orkney*, and on the North Coaſt of *Scotland*. When flood tide is raiſed higher than ordinary by winds, the ſubſequent ebb is not ſo low as it would have otherwiſe been. When a high flood is raiſed by the moon, the ſucceeding ebb is proportionally low.

 Ordinary

Ordinary ſpring-tides riſe 8 feet perpendicular, ordinary neap-tides 3; extraordinary high ſpring-tides riſe 14 feet; extraordinary low, only 5; extraordinary high neap-tides riſe above 6 feet; extraordinary ſmall neap-tides not above 2. Low-water neap-tide, at a mean, I judge is about 3 feet above low-water ſpring-tide, and high-water ſpring-tide about 3 feet above high-water neap-tide: yet the riſe and fall vary ſo much, that it would require a longer courſe of obſervations than I have had opportunity of making, to determine what is moſt frequent in this caſe.

When a ſtream of tide is interrupted by land, or rocks, or is confined within a chanel, or long arm of the ſea growing uniformly narrower, the water will riſe higher there than in neighbouring places, where it is not ſo affected. If the chanel, or arm of the ſea, has ſeveral windings, or reaches, as they are called in the *Thames*, the ſuperior elevation will not be ſo conſiderable.

The following obſervations of the riſing and falling of the water, were made in the day-time, in the bay of *Kirkwall*, *anno* 1748.

Auguſt 8. Wind. W. a breeze.
Laſt quarter 4th day.
Moon's apogee diſtant 24°.
Moon's declination 27° N.
Moon bearing at firſt W. by N.

		Feet	Inch
The water roſe	*1ſt* hour	0	½
	2d	0	2
	3d	0	4½
	4th	0	9½
	5th	0	5½
	6th and to the end	0	5½
	In all	2	5

Auguſt 15. Wind E. N. E. ſmall breeze.
New moon 3d day.
Moon's apparent diſtance 65°.
Moon's declination 7° S.
Moon bearing S. S. E.

		Feet	Inch
The water fell	*1ſt* hour	0	1½
	2d	0	4½
	3d	1	4
	4th	1	8
	5th	1	5
	6th	0	10
	In all	5	9

Auguſt

August 23. Wind W. almoſt calm.
Firſt quarter, 3d day.
Moon's perigee diſtant 13°.
Moon's declination 25° S.
Moon bearing E. by N.

		Feet	Inch
The water roſe	*1ſt* hour	0	6
	2d	1	0
	3d	1	8
	4th	1	0
	5th	0	8
	6th	0	7
	In all	5	9

Aug. 29. wind S. *b* W. breeze at firſt, afterwards calm.
Full moon 3d day.
Moon's perigee diſt. 68°.
Moon's declination 6° N.
Moon bearing N. N. W.

		Feet	Inch
The water fell	*1ſt* hour	1	0
	2d	0	10
	3d	1	4
	4th	1	2
	5th	2	10
	6th	1	0
	In all	8	5

Aug. 27. Wind W. S. W. a ſmall breeze, the day of full moon, moon's perigee diſtance 36°, decl. 4° S. bearing N. N. W. the water fell in all 8 feet 4 inches perpendicular.

Aug. 30. calm, 4th day after full moon, perigee diſtance 80°, decl. 13° N. moon bearing N. *b* W. the water roſe 8 feet 3 inches.

Sept. 3. Wind S. W. a ſmall breeze, firſt day of the laſt quarter, apogee diſt. 50°. decl. 27°. N. moon bearing W. the water roſe 6 feet 1 inch.

Sept. 6. Wind E. a ſmall breeze, 4th day of the laſt quarter, apogee diſt. 15°. decl. 21° N. moon bearing W. the water roſe 3 feet 9 inches.

Sept. 15. Wind S. a moderate breeze, 5th day after new moon, perigee diſt. 80°. decl. 24° S. moon bearing S. *by* E. the water fell 5 feet 9 inches.

To aſcertain all the varieties in the riſing and falling of the water, the obſervations ought to have been continued much longer, the night-tides as well as day-tides obſerved; alſo the exact times of the beginning and ending of each, the ſtrength of the wind and weight of the atmoſphere by a barometer.

The foregoing articles relate to the riſing and falling of the water; the following to the various motions of the ſtream, and their conſequences.

On the coaſt of *Orkney*, and *Fair* iſle of *Shetland*, the body of the flood comes from the north-weſt; on the eaſt and weſt coaſts of *Lewis*, one of the weſtern iſles of *Scotland*, it comes from the ſouth.

A league or two off the coaſt, the ſtrength of the ſtream is ſcarce ſenſible, except when it is confined by land, or near rocks or ſhoals.

When the tide begins to riſe or fall on the ſhore, about that ſame time the ſtream near the ſhore begins to turn or reverſe it's direction, a few irregularities excepted.

The ſtream of tide changes it's direction ſooner near land than at a diſtance from it; inſomuch that, in a place two or three miles from land, the turning of the tide is two hours, or more, later than on the adjacent ſhore: at intermediate diſtances the ſtream turns at intermediate times. Hence a veſſel may find a favourable tide near land, while it would be againſt her a mile or two from it; and the contrary.

During the continuance of flood, the ſtream varies it's direction gradually from the E. toward the S. and the ſtream of ebb from the W. towards the N: that is, if the ſtream, when it becomes firſt ſenſible, runs E. at the latter end of the tide it will run S. if the proximity of land or ſhoals does not hinder this change of direction.

The greateſt velocity of ſpring-tide in *Orkney*, in the chanels where it runs quickeſt, is about 9 miles an hour: the greateſt velocity of neap-tide is about one third or fourth of ſpring-tide. The tides are moſt rapid commonly between the third and fourth hour. Spring-tides acquire a conſiderable degree of ſtrength in leſs than one hour after their quieſcent ſtate begins; neap-tides are hardly ſenſible in two hours after.

In ſimilar ſtreights or chanels, lying in the ſame direction, and ſupplied from the ſame part of the ocean, the velocity of the ſtreams will be in the direct *ratio* of the breadth of the inlets, and the inverſe of the outlets.

If a ſound, or ſtreight between two iſlands, or continents, lies in the direction of the main body of the tide, the velocity of the ſtream in that ſtreight will be greater (all other things alike) than in any other adjacent one, not lying in that ſame direction.

If an iſland lies directly in the tide-way, the ſtream will divide, or ſplit, before it reaches the iſland, into two branches, one of which will run toward one ſide or end of the iſland, and the other toward the other end of it; and, in paſſing by, will be reflected a little from the land.

Hence

Hence a veſſel, in a calm, carried along with a ſtrong ſtream of tide, is in no danger of touching an iſland, or viſible rock, if the water is deep enough near them.

If the tide runs ſtronger, or more obliquely, by one end of an iſland than the other, from the ſtrongeſt ſtream, and from the moſt oblique, there will be a languid current toward the other; that is, the tide, along that ſide of the iſland, will ſet longer one way than the other.

If a ſtrong ſtream of tide runs acroſs the mouth of a bay that does not reach far into the land; within that bay there will be a ſlow ſtream ſetting the contrary way. Or, if a ſtrong ſtream ſets directly, or nearly ſo, along the extremity of a point, or promontory, that ſtretches ſtrait out from the coaſt, between this ſtream (before it reaches the point) and the coaſt, there will be a languid current with a contrary direction. By attending to this, one veſſel may keep her courſe, or gain a port, while another is carried away with the tide.

If a ſmall iſland lies thwart a tide-way, that part of the ſtream which runs along one end of it, will join what runs along the other, at ſome diſtance beyond the iſland, incloſing between them a curved ſpace, within which there will either be no ſenſible current, or a ſlow one, contrary to the other ſtreams. The counter current, in the middle of this almoſt ſtagnant ſpace, or eddy, when it gets near the iſland, ſplits in two; one branch of which runs towards one extremity of the iſland, the other towards it's other extremity; where meeting the ſtronger direct ſtreams that form the eddy, are by them again carried towards it's *vertex*.

Theſe eddies may be of great ſervice to ſhips or boats, by ſheltering them from a rapid ſtream, or even carrying them againſt it; or may enable them to croſs it with more advantage, according to the different places to which they are bound. The oppoſition of the contrary tides bounding the eddy, makes that part of the ſea rougher in blowing weather, and of a darker colour in calms, than the reſt, by which the limits and direction of theſe eddies are always diſtinguiſhed.

The colliſion of the oppoſite and oblique ſtreams, near the ends of the iſland, will excite a circular motion in the water, and, if the celerity of the tide is conſiderable, will occaſion whirlpools, or cavities in the ſea, in form of an inverted bell, wide at the mouth, or at the ſurface of the ſea, and growing gradually narrower toward the bottom: their width and depth are in proportion to the rapidity of the ſtreams that cauſe them, and are ſometimes ſo large as to be dangerous. Thoſe in *Petland Firth*, near the iſlands *Stroma* and *Swona*, may, with ſpring-tide, turn any veſſel quite round, but are never ſo large as to endanger one otherways: there have been inſtances, however, of ſmall boats dropping into, and being ſwallowed up by them. The *hiatus*, or cavity, is largeſt when it is firſt formed, and is carried along with the ſtream, diminiſhing gradually in dimenſions as it goes, until it quite diſappears. The ſuction, or ſpiral motion communicated to the water, does not ſeem to extend far beyond the *hiatus*. I paſſed, in a boat, within 20 yards

yards of one, without being ſenſible of any attraction; but indeed it was toward the latter end of the tide, when it's ſtrength was much abated: the diameter of the cavity, at that time, I judged to be between 2 and 3 feet. When fiſhermen are aware of their approach toward a whirlpool, or well, as it is called in *Orkney*, and have time to throw an oar, or any other bulky body into it before they are too near, the ſpiral motion is interrupted, and the continuity of the water broke; which, ruſhing in on all ſides, immediately fills up the cavity, and enables them to go over it ſafe. Hence in blowing weather, or when there is a breaking ſea, tho' there may be a circular motion in the water, there can be no cavity.

When there is a ſteep ſunk rock near the concourſe of ſuch ſtrong tides, and not very deep below the ſurface, a moſt amazing *phænomenon* will happen: for, the ſtream being interrupted in it's courſe, and falling ſuddenly over the rock, is reflected from the bottom upwards, ſwelling and bubbling on the ſurface like boiling water, and carrying ſand, ſhells, fiſhes, or other looſe bodies along with it; which, with boats, or whatever elſe is near, are driven with great force from the center all around toward the circumference, upon which, a gyration of the water enſuing, a whirlpool begins, which is carried along with the ſtream, as was ſaid above, leſſening gradually till it is quite extinguiſhed: in a little time a new eruption and ebullition, like the former, begins, which proceeds in the ſame manner, till the ſwiftneſs of the ſtream abates, or the tide riſes or falls too much above the rock.

Queries concerning the tides in a large ocean; which, if reſolved from obſervation, would render the theory more perfect.

1. Since the attraction of the moon raiſes the water directly below her, by diminiſhing it's gravity toward the earth's center, and, at that very ſame time, depreſſes it at a quadrant's diſtance, by augmenting the gravity there, ſo that the ſuperior altitude of one part of the ocean is immediately balanced by the ſuperior gravitation of another; do not, therefore, the tides in the ocean riſe and fall without any progreſſive motion, or ſenſible velocity? And do not all currents, or ſtreams of tide (not cauſed by winds) proceed from the interruption which land, or ſhoals, give to the undulatory motion which muſt accompany the perpendicular aſcent or deſcent of the fluids?

2. Is it agreeable to obſervation, that the power of the ſun and moon together, raiſes the tides within the tropics about 14 feet, as *Newton*, *Halley*, and *Maclaurin* ſuppoſe? And how high are the tides found to riſe in parts of the ocean of a greater latitude? If the water does not riſe and fall ſo much within the tropics, as in places more diſtant from the equator, what hinders the greater power to have a greater effect? For the moon muſt act with greateſt force on thoſe parts to which ſhe is vertical.

3. If the times of high and low water depend on the moon's appulſe to the meridian, is it not high or low water in all parts of the ocean, under the ſame meridian, about the ſame time? And is the difference of

of the times, in places under different meridians, in any certain proportion to their difference of longitude?

4. Since the power of the moon to raise the tide in any place is greatest when she is nearest the zenith, it is agreeable both to observation and theory, that the water rises and falls more when she is above, than when below the horizons of places on the same side of the equator with her; and the contrary: are not the tides also of longer duration in that case? Since a greater portion of the hemispheroid, into which the sea is formed by the moon's attraction, is then above the horizons of these places, than is below them. If this is found to be fact, it will also be found, that the duration in different places (other things alike) will be in some measure proportional to their latitudes, and the declination of the moon.

5. In an oblique sphere, all azimuth circles cut the equator and it's parallels obliquely; and therefore the moon must come sooner to, or from, a given azimuth, with one declination than with another. In some latitudes this difference will amount to several hours. Is it not then a false rule to judge of the times of high or low water by the moon's azimuth, or to signify one by the other, as is the custom of sailors?

An irregular tide in the river of Forth; *by Mr* Edward Wright. No. 495 p. 412. Apr. &c. 1750. *Read* May 3. 1750.

V. There are in this river, at ebbing and flowing, certain irregular motions, not to be found in any other river in *Scotland*, perhaps in *Great Britain*, or even in all *Europe*, called by the common people betwixt the villages of *Alloa* and *Culross*, Leakies, which name I shall likewise make use of, for want of a better. This leaky is this: when the river is flowing, before high water, it intermits and ebbs for a considerable time, after which it resumes it's former course, and flows till high water; and, *vice versa*, in the ebbing, before low water, the river flows again for some time, and then ebbs till low water. The leaky begins at a place called *Queen's Ferry*, seven miles above *Leith*, at neap tide, and low water, and goes to the house of *Maner*, which is about 25 miles above *Queen's Ferry*, which is to be understood by water; for in this river, from a little above *Alloa* to *Stirling*, there are such a prodigious number of turnings and windings, that though it be but 4 miles betwixt these two places by land, yet it is 24 by water. This I take notice of rather, as I take these windings to be the cause of the leakies. At neap tide and high water, as also at spring-tide and low water, the leaky reaches as far as the sea fills, which is to the groves of *Craigforth*, 19 miles above *Maner* house, and three above the town of *Stirling*. At *Queen's Ferry* there are no leakies at neaps and springs at high water, nor in the latter at low water; they begin betwixt *Burrowstowness*, a village about 7 miles above *Queen's Ferry*, and the mouth of a rivulet called *Carron*, five or six miles further up the river than *Burrowstowness*. What is very remarkable, in the very lowest neaps the leaky, after it has ebbed for some time, before high water, makes up again, and will be two feet higher than the main tide. In the beginning

ning of the spring-tides, it does not rise so high by a foot: at the dying of the stream, it is often two feet higher than the main tide, which is to be understood, before high water, when the leaky makes up again. At neap tide and low water it will ebb two hours, and fill as much, and at full water ebb an hour, and fill another.

It is likewise to be remarked, that at change of the moon, at low water, the leaky will continue two hours, the beginning of the tide for that time, which then stands, and does not ebb till flood (the beginning of the flowing), and at full water, will ebb and flow an hour or more.

It is observable, that at full moon, there are no leakies, either at high or low water, in the spring tides which are at that time, but in the neaps which follow them, these motions are observable, as before described; as also in the spring tides, which happen upon the change of the moon, called by the commonalty, the overloup, there are leakies both at high and low water. All this is to be understood, when the weather is seasonable; for, otherwise, these motions are not so discernible.

This account I have collected partly from my own observation, having past a great part of my life at a country-seat near *Alloa*, where the leakies are to be seen in great perfection, and partly from what I have learned from people living on different parts of the river, whose observations, as well as my own, I find exactly agree with those of a learned * relation of mine, who, near 70 years ago, diligently observed and inquired into the phænomena of this irregular tide.

A surprizing inundation in the valley of St John's *near* Keswick *in* Cumberland, Aug. 22. 1749. *in a letter from a young clergyman to his friend; communicated by* John Lock, *Esq; F. R. S.* N°. 494. p. 362. Jan. &c. 1750. *Read* March 15. 1749.

VI. In order to give you a distinct answer to your queries in relation to the inundation at *St John's*, I took a ride to the place to satisfy myself of the matter of fact, because the accounts which were given me were very different.

This remarkable fall of water happened at 9 in the evening, in the midst of the most terrible thunder, and incessant lightning, ever known in that part in the memory of the oldest man living, the preceding afternoon having been extreme hot and sultry. And what seems very uncommon, and difficult to account for, the inhabitants of the vale, of good credit, affirm to have heard a strange buzzing noise like that of a malt-mill, or the sound of wind in the tops of trees, for 2 hours together, before the clouds broke.

I am not so much a Philosopher as to find out what could occasion such a vast collection of clouds or vapours, particularly at that time and place; but am satisfied from the havock it has made in so short a time (for it was all over in less than 2 hours), that it must have far exceeded any thunder-shower that we have ever seen. Most probably it was a spout or large body of water, which, by the rarefaction of the air, occasioned by that incessant lightning, broke all at once upon the tops

* His observations were communicated to Sir *Robert Sibbald*, in order to assist him in compiling a *Scotish* Atlas.

of

of these mountains, and so came down in a sheet of water upon the valley below.

This little *Valley of St John's* lies E. and W. extending about 3 miles in length, and ½ a mile broad, closed in on the S. and N. sides, with prodigious high, steep, rocky mountains: those on the north side, called *Legburthet Fells*, had almost the whole of this cataract; for I do not find that any remarkable quantity of water was observed from those on the S. notwithstanding the distance from the tops on each side cannot be a mile. It appears likewise, that this vast spout did not extend above a mile in length; for it had effect only upon 4 small brooks, which come trickling down from the sides of the rocky mountains. But no person, that does not see it, can form any idea of the ruinous work occasioned by these rivulets at that time, and (what seems almost incredible) in the space of an hour and half. At the bottom of *Catcheety Gill*, which is the name of the greatest, stood a mill and a kiln, which were intirely swept away, in 5′ time, and the place where they formerly stood, now covered with huge rocks, and rubbish, 3 or 4 yards deep. One of the mill-stones cannot be found, being covered, as is supposed, in the bottom of this heap of rubbish.

In the violence of the storm, the mountain has tumbled so fast down as to choak up the old course of this brook; and, what is very surprizing, it has forced it's way through a shivery rock, where it now runs in a great chasm, 4 yards wide, and betwixt 8 and 9 deep.

In the course of each of these brooks, such monstrous stones, or rather rocks, and such vast quantities of gravel and sand, are thrown upon their little meadow-fields, as render the same absolutely useless, and never to be recovered.

It would surpass all credit to give the dimensions and weight of some rocks, which are not only tumbled down the steep parts of the mountains, but carried a considerable way into the fields, several thrown upon the banks larger than a team of ten horses could move. Near a place called *Lobwath*, I had the curiosity to measure one carried a great way, which was 676 inches, or near 19 yards about.

The damage done to the grounds, houses, walls, fences, highways, with the loss of the corn and hay then upon the ground, is computed variously, by some at 1000 *l.* by others at 1500 *l.*

One of these brooks, which is called *Mose* or *Mosedale Beck*, which rises near the source of the others, but runs N. from the other side of *Legburthet Fells*, continues still to be foul and muddy, having, as is supposed, worn it's chanel so deep in some part of it's course as to work upon some mineral substance, which gives it the colour of water hushed from lead-mines, and is so strong as to tinge the *River Derwent* (into which it empties itself) even at the *sea*, near 20 miles from their meeting.

Theſe are moſt of the particulars I could collect concerning this wonderful inundation. I ſhall only add *Mr.* N.'*s Philoſophical Account to his Philoſophical Friend.*

" *Tueſday*, *Aug.* 22. 1749. was the beſt hay-day we had here that " ſeaſon, but at 8 at night it began to thunder, firſt W. from *Cockermouth*, then in a few minutes E. from *Penrith.* Theſe thunder-" clouds, with equal force, and contrary direction, met together upon " the mountains above the valleys of *St John's* and *Threlkeld*, as at or " about the *Great Dod* and *Cova Pike*, and muſt of conſequence hover " on or about them, and thereon vent *water-ſpouts* (but not ſo on the " valleys, otherwiſe than by the violent courſe of the brooks and rivu-" lets, from the one down to the other); which would increaſe and " perpetuate the lightning, ſo ſwift in motion, and viſible to our eyes, " but retard and obſtruct the undulations of the air, which are far more " ſlow in motion, and later in coming to our ears.—For any two ſuch " bodies as thick clouds, driven by contrary winds, and meeting to-" gether with equal force, and contrary directions, cannot impel each " other backwards or forwards, but muſt remain at or about the place " where they met, and there exert their vigour: which, in this caſe, " muſt be the reaſon of ſuch water-ſpouts upon theſe mountains, and " not in the valleys; and alſo why the ſight of the the lightning was " more terrible to our eyes than the ſound of the thunder to our ears.— " Like to this is the caſe of *whirl-puffs.*"

A burning well; by the Rev. Mr Maſon, Woodw. Prof. Cantab. *& F. R. S.* N°. 482. p. 371. Jan. &c. 1746-7. *Read* Jan. 22. 1746-7.

VII. At *Broſeley* in *Shropſhire*, in 1711, was a well found, which burned with great violence, whereof ſome account is given in *Philoſ. Tranſ.* N°. 334; but it has been many years loſt. The poor man, in whoſe land it was, miſſing the profit he uſed to have by ſhewing it, applied his utmoſt endeavours to recover it; but all in vain, till *May* laſt; when, attending to a rumbling noiſe under the ground, like what the former well made, though in a lower ſituation, and about 30 yards nearer to the river, he happened to hit upon it again.

The well for 4 or 5 feet deep is 6 or 7 feet wide; within that is another leſs hole, of like depth, dug in the clay; in the bottom whereof is placed a cylindric earthen veſſel, of about 4 or 5 inches diameter at the mouth, having the bottom taken off, and the ſides well fixed in the clay rammed cloſe about it. Within the pot is a brown water, thick as puddle, continually forced up with a violent motion, beyond that of boiling water, and a rumbling hollow noiſe, riſing and falling by fits 5 or 6 inches; but there was no appearance of any vapour riſing; which perhaps might have been viſible, had not the ſun ſhone ſo bright.

Upon putting down a candle at the end of a ſtick, at about a quarter of a yard diſtance, it took fire, darting and flaſhing in a violent manner, for about half a yard high, much in the manner of ſpirits in a lamp, but with a greater agitation. The man ſaid, that a tea-kettle had been

been made to boil in 9′ time; and that he had left it burning 48 hours together, without any ſenſible diminution.

It was extinguiſhed by putting a wet mop upon it, which muſt be kept there a ſmall time; otherwiſe it would not go out. Upon the removal of the mop, there ſucceeded a ſulphureous ſmoke, laſting about a minute; and yet the water was very cold to the touch.

The well lies about 30 yards from the *Severn*; which, in that place, and for ſome miles above and below, runs in a vale full 100 yards perpendicular below the level of the country on either ſide, which inclines down to the country at an angle of 20 or 30° from the horizon; but ſomewhat more or leſs in different places, according as the place is more or leſs rocky.

The country conſiſts of rock, ſtone, earth, and clay, unequally mixed; and as the river, which is very rapid, waſhes away the ſoft and looſe parts, the next ſucceſſively ſlip into the chanel; ſo as, by degrees, and in time, to affect the whole ſlope of the land: and as the inferior *ſtrata* yield coal and iron-ore, their fermentation may produce this vapour, and force it to aſcend with violence through the chinks of the earth, and give the water the great motion it has. This might be obſtructed in one place by the forementioned ſubſiding of the ſloping bank, and might afterwards find a vent in another; in like manner as it happened at *Scarborough Spaw*, a few years ſince.

CHAP. III.

MINERALOGY.

I. IN the night-time, between the 24th and 25 of *June* laſt, a violent ſtorm of thunder and lightning happened at the city of *Norwich*, and the places adjacent; though at the city of *Norwich* it ſeemed extraordinary only for the loudneſs of it's claps, and the length of ſeveral of the flaſhes; ſome whereof continued near half a minute, and were ſo extremely bright, that they cauſed ſome thin deal ſhutters to the windows of my bed-room (which then happened to be unpainted) to appear almoſt quite tranſparent.

Some account of the ſinking down of a piece of ground, at Horſeford, *in* Norfolk; *communicated by Mr* Arderon, *of* Norwich, *to Mr* H. Baker, *F. R. S.* N°. 477. p. 527. Aug. &c. 1745. *Read* Nov. 14. 1745.

But at *Horſeford*, a ſmall country village, about four miles N. W. of this city, a remarkable *phænomenon* appeared the next day, the like whereof has not been obſerved in this county, ſince that communicated to the *R. S.* by Mr *P. Le Neve*.

A ſudden *Lapſus*, or ſinking down of the earth, happened at this village, in the night above-mentioned, and left a hole 12 feet deep, and 12 ½ in diameter, in form almoſt exactly round. It's ſides are nearly per-

pendicular;

pendicular; and what ſeems moſt ſtrange, no ruffles, cracks, or chaſms, are to be found nigh it, but the ground appears intirely firm and ſolid; and, for miles about is a fine champaign country, of a dry ſandy ſoil, but not hilly; neither is there any watercourſe above ground near it.

The firſt yard from the ſurface downwards is corn-mould earth; the other three are compoſed of brown and yellow ſand, diſpoſed in ſeveral different *ſtrata*.

I ſhall not pretend to account for this accident; but might it not poſſibly be occaſioned by ſome ſubterraneous current waſhing away the ſandy matter by little and little, until it had left only a cruſt, which the *tremor* of this terrible thunder had thrown down from the very ſurface; though on this conjecture, one would expect ſome overflow or appearance of water; whereas I could not perceive here the remains of a ſingle drop.

A letter from Mr J. Durant *to the Hon.* R. Boyle, *Eſq; F. R. S. concerning a* coal-mine *taking fire near* Newcaſtle *upon* Tyne; *of the* blue well; *and of a* ſubterraneous cavern *in* Weredale; *communicated by Dr* Miles. N°. 480. p. 221. May & June 1746. *dated* Newcaſtle, Feb. 9. 1673-4. *Read* June 5. 1746.

II. The fire at firſt was occaſioned by a candle, negligently placed by a pitman, as he was working in a pit about 30 years ago. So ſmall it was at firſt noticing, that half a crown was denied reward to one, who for that price would have engaged to have extinguiſhed it: now it has waſted land and mine, and grown ſo furious, as no hopes of it's ceaſing are conceived, before the failure of it's fuel.

The grounds where it began belong to a village called *Benwell*, about $\frac{1}{4}$ of a mile N. from the river *Tyne*; whence, by a ſlow progreſs, and frequent deviations E. and W. it marched N. ſometimes preying on the coals nearer the ſurface of the earth, and then ſubverting houſes and grounds lying over it; ſometimes on the deeper mines, and was conſpicuous only by it's ſmoak and fire in the night. Now it rages, and has already cauſed great devaſtation, in grounds belonging to a village called *Fenham*, near a mile N. from the place where it firſt was kindled.

It's eruptions at preſent are in many places, and various depths. I have, both laſt winter and this, in froſty nights (for then it burns moſt furiouſly) occaſionally riding by, in near 20 places, ſeen it's flames and pillars of ſmoak. That ever it has ejected ſtones, or the like, I cannot, by information or obſervation, affirm; the concreted ſalts we have from it being always found either candying the ſupercreſcent furze, or impacted in the ſurface of the earth, at it's eruptions.

If you ſhall command it, I may be able to give you ſome account alſo of a ſtream near this town, which, on it's banks, in the ſummer-time, as alſo, being evaporated over the fire, leaves behind it a blue powder. It's head is thence called by neighbouring inhabitants, *The Blue Well*; as alſo, of ſome ſubterraneal grottoes or caverns in *Weredale*, about 20 miles S. W. of this place; where, by a little hole creeping into the ſide of a vaſt mountain, is entered a ſpacious cavity, chambered with walls and pillars of decident lapideſcent waters; the hollowneſs in ſome places being pervious further than any yet has adventured to diſcover;

cover; the darkness of these caverns requiring the help of candles, which are often extinguished by the dropping water.

III. 1. *July* 19. 1746. I had the pleasure of seeing those stupendous and amazing precipices which bound our N. E. sea-coast; and rode some miles betwixt them and the sea. I can assure you, I was highly delighted with viewing them; and, as Sir *Richard Steele* says, by the description of *Dover Cliffs*, Whoever looks upon these precipices, and is not moved with terror, must either have a very *good* head, or a very *bad one.*

Extract of a letter from Mr W. Arderon, *F. R. S. to* Mr H. Baker, *F. R. S. containing observations on the* precipices *or* cliffs *on the N. E. sea-coast of the county of* Norfolk. N° 481. p. 275. Oct. &c. 1746. *Read* Nov. 20. 1746.

These dreadful heights are equally dangerous to come nigh, above or below; as they are so frequently tumbling down, and as often washed away by the raging billows: and although they are 20, 30, and in some places 40 yards and upwards in perpendicular altitude, yet I am credibly informed the sea has got of the land at least 110 yards in less than 20 years time for some miles on this coast.

The various *strata*, which make up this long chain of mountainous cliffs, must be greatly entertaining to every one, who takes a pleasure in looking into the many changes, which the earth undoubtedly has undergone since it's first creation.

Vegetable mould, oaz, sands of various kinds and colours, clays, loams, flints, marles, chalk, pebbles, *&c.* are here to be seen at one view beautifully interspersed; and frequently the same kind many times repeated; as if at one time dry land had been the surface; then the sea; after, morassy ground; then the sea, and so on, till these cliffs were raised to the height we now find them.

What makes this come up almost to demonstration, are the (*a*) roots and trunks of trees, which are to be seen at low water in several places on this coast near *Hasborough* and *Walket.*

With respect to the tooth I sent you some time ago, I could trace nothing more out than what I have before informed you; but, that bones of animals are often found here is indisputably true; and I have now by me another (*b*) tooth of an elephant found betwixt *Munsley* and *Harborough*, which (*c*) I shall forbear to describe to you, as I design it to wait upon you with some other fossils in a few days.

That the rest of the bones of these animals are not preserved so commonly as the teeth, I am informed is their prodigious bulk and weight; which are so great, that the country people thereabouts have never

(*a*) Dr *Hook*, in his Posthumous Works says, the like are to be seen on the coasts of *Cumberland* and *Pembrokeshire.*

(*b*) *Richard Verstegan* says, nigh *Bruxelles* in *Flanders* were found the bones of an elephant, the head of which he had seen. *Verst.* p. 115.

(*c*) "Only, that as this tooth is much more decayed than that I before sent you, and "hath several pieces broken off it, yet it weighs 10 ¼ pounds; and would have weighed "as much or more than that which Mr *Tentzelius* describes in the *Philos. Trans.* N° 234. "found near *Erfurt* in *Germany.* This, I think he says, was the biggest ever found in "*Europe.*"

thought

thought it worth their while to save them from the fury of the tempestuous waves.

In perusing Dr *Hook*'s Posthumous Works, *p.* 313. in his Discourse on Earthquakes, I there find, that Dr *Thomas Brown* of this city presented the *R. S.* with a petrified bone, found at *Winterton*, a little country town on this coast, in the year 1666; but am far from being of (*d*) Dr *Brown*'s opinion, to take them all for bones of sea animals, much less of the whale kind, which are found here.

Amongst the many *strata*, which I took notice of in these cliffs, there is one of a dark-grey colour, which sweats out a yellow sulphureous matter: I take it to be that sort of earth from which vitriol is made; but this is of such a caustic nature, that, if but a small piece of it be held to the tongue, in a moment it causeth as sharp and excruciating a pain, as if a red-hot iron had been holden to it.

An account of the strata *of* shells, *and other* fossils, *found at* Cantley White-House *in* Norfolk. Ibid. p. 279.

2. After giving you a short account of what I thought worth my notice on our N. E. sea-coast, I shall here lay before you what I have observed in some marl-pits at a place called *Cantley White-House*, about 3 miles from *Norwich*, almost S. E. and adjacent to the country seat of the honourable *Thomas Vere*, Esq; (*e*).

These marl or rather chalk-pits are made in the side of a long chain of hills, which runs along the side of the river *Yar*, and about a furlong or two now-and-then distant from it.

These hills I take to have been formerly the boundaries to an arm of the sea, which made *Norwich* a famous sea-port. This some of our (*f*) antient histories make mention of as an undoubted truth, tho' now looked upon as a mere fable, as no footsteps of it remain above-ground at this day.

In the above-mentioned marl-pits I have lately discovered a (*g*) *stratum* of shells, of about 2 feet thick, running nearly parallel to the horizon, and I believe nigh level with many parts of the ground in *Norwich*. This seems to put the matter out of all dispute, and fairly confirm our antient history (*h*).

(*d*) " Whoever will take the pleasure to read Mr *Blair*'s description of the skeleton of " the elephant, now at *Dundee* in *Scotland*, inserted in *Phil. Trans.* N°. 326. will find " these teeth before-mentioned to be the real teeth of elephants, whatever Dr *Brown*'s " may be: and though I have never seen that bone he presented to the *Society*, yet I " imagine it to be a bone from the same kind of animal."

(*e*) *Verstegan* says, that many places which were sea became dry land, at the breaking of the *German* ocean through the *Isthmus* which once joined *England* to *France*. *Verst.* p. 117.

(*f*) The Rev. Mr *Bloomfield*, in his history of *Norwich*, produceth several antient writings, which assert the truth of this. Page 2.

(*g*) The Rev. Mr *Thomas Lawrence*, in his *Mercurius Centralis*, is of opinion, these shells, and all others under-ground, are lodged here by subterraneous currents. Page 47.

(*h*) Dr C. *Leigh*, in his Natural History of *Lancashire*, boldly affirms all fossils to be the disports or *lusus naturæ*. *Book* III. *p.* 41. and many other places.

I examined

I examined carefully this *ſtratum*, where I found a great many (*i*) kinds of ſhells, but none which had withſtood time's all-devouring teeth, ſo as to bear the handling; excepting the common wilk, ſome of which were very perfect (*k*).

Amongſt the variety of things I took notice of in this *ſtratum* was a piece of coal; which I picked out from amongſt the ſhells. This muſt have lain here as long as they, and been brought from ſome other county, as nothing of it's kind is to be found here, but what is brought from diſtant parts.

This ſeems to prove, that the uſe of coals has been known to the antient *Britons*; and that they brought them hither from the more northern parts, when the ſea covered the greateſt part of *Norfolk*: though, in all probability, this muſt have been ſome thouſands of years ago.

Theſe ſhells lie 14 yards above the ſurface of the river, and nearly 6 beneath the top of the hill; and I believe 34 yards above the ſurface of the ſea at *Yarmouth*.

I have one thing ſtill to relate to you, which is really wonderful, and very much beyond my utmoſt endeavours to find out a ſufficient reaſon for: it is, that in theſe very marl-pits, and I dare be bold to ſay, 6 or 7 yards lower than the above-mentioned *ſtratum* of ſhells, are found an unaccountable quantity of ſtags horns lying in all directions: ſeveral I took out with my own hands; and the workmen, which are employed here. tell me, that they ſcarce work a day, but they find leſs or more of them.

But with my utmoſt diligence I have not yet been able to find one whole and entire; nor do the workmen ſay they ever did; which I take to be very ſtrange.

(*i*) Common Cockle. Black muſſel. Oyſter. *Pectunculus*, &c.

(*k*) The ever memorable Dr *Hook*, in his Poſthumous Works, ſays, that earthquakes ſeem to be the chief efficients which have tranſported theſe petrified bodies, ſhells, woods, &c. and left them in ſuch parts of the earth as are no otherwiſe likely to have been the places wherein ſuch ſubſtances ſhould be produced.

" That ſeveral mountains and vallies have taken their riſe from earthquakes muſt in-" evitably be allowed; but then they are to be found in hotter countries than this. If " the contrary is believed, why don't ſuch things happen now? And why is all hiſtory " ſilent upon this head? Beſides, the regularity of the *ſtrata* of ſhells, and their often " lying parallel to the horizon for many yards, I own, puzzle me very much, and prevent " me from acquieſcing with this ingenious man on this head."

" Dr *Woodward*'s hypotheſis, or manner of bringing theſe ſhells, and all other foſſils, " into the places where we now find them, by a total diſſolution of matter, is indeed very " pretty; but ſo many difficulties ariſe (however plain it might appear to him) I believe " few now-a-days are of his opinion.

" Above all, I think Mr *Petit*'s way of thinking is liable to the feweſt objections; *viz*. " by the variation of the parallelifm of the earth's *axis*; which, being allowed, muſt " certainly alter the centre of gravity: if ſo, then all the fluid parts will conform there-" to; and then it will follow, that one part will be covered, and overflowed by the ſea, " that was dry before, and another be diſcovered and laid dry, that was before " over-whelmed."

Theſe

These horns (*l*) have been very large ones; some of the spines measuring 12 inches and upwards in length. The horns themselves, many of them, are better than 2 ½ inches in diameter, and several of them above 12 inches from spine to spine.

The entire skeleton of a man was found in the same bed or *stratum* with the above-mentioned horns, as one of the workmen assured me: he said, he took the pains to lay it all together upon the grass, as regularly as he was able; but his curiosity being then satisfied, he left it to be ground to pieces by the carts and waggons that came thither for the marl; so careless were these poor ignorant people of so valuable a specimen of the human race! What instructive inferences might perhaps have been drawn from such a skeleton, with respect to the magnitude of men in the early ages of the world! I own, I cannot but regret the loss of it.

Helmet stones (*m*) and *belemnites* are here to be found in abundance, at all depths, and in every different *stratum*; which I think shews, that the fish, which produced these fossils have been very plentiful: and so they must have been all over the county, as the like are to be found in every place where-ever the earth is broken open, or a pit is digged.

An account of other fossils, *found near* Hartford-Bridge *in* Norfolk. Ibid. p. 283.

3. About a mile S. of a little country town called *Kisick*, and near two furlongs from (*n*) *Hartford Bridge*, is a pit, in which the country people dig a particular sort of clay to lay upon their sandy lands. Amongst this clay, lie a great many knots, lumps, or nodules, of a bluer sort of earth, not widely differing from that which is found in *Harwich* cliff: these, when digged up, are soft; but when they have been for some time exposed to the open air, they become almost as hard as flint.

In and upon these lumps are the impressions of the *cornu Ammonis*, or snake-stones in a beautiful manner, from one inch to 5 or 6 in diameter; and several I saw with part of the shells upon them of a yellowish white (*o*).

Many other shells are to be found in these lumps, as the *pectunculus*, helmet-stones, *belemnites*, common cockle, *turbo's*, *&c.* but these are most of them very small.

But still more curious than all the rest are certain lumps of petrified crystallized matter, of a very odd form, such as I have never seen or ever read of.

They appear to have been originally lumps of blue clay, cracked by some subterraneous heat, or other unknown cause, into which the water

(*l*) "The nature of these horns seems to the eye to be entirely changed into that of "chalk; only retaining their outward form, and the porosness of their inward parts."

(*m*) "Whatever *strata* these helmet stones are found in, I observe they are mostly filled "with matter of the same nature and consistency, provided they have lain there un-"disturbed."

(*n*) About 3 miles S. W. of *Norwich*.

(*o*) "When this clay was soft, I found it impossible to get one of them whole."

has

has infinuated, and the falts contained therein have cryftallized in the cracks.

When thefe lumps are taken up, and become dry, the clay part falls from out the exterior cells; and then they may be thought grofly to reprefent an honey-comb. At firft I took them to be bones from within-fide the fkull of fome great fifh, or other fea animal; but fome which I have feen lately, and of which I fhall fend you a fpecimen, have convinced me I was miftaken. Indeed fince I wrote the above, I have compared this foffil with the defcription Dr *Woodward* gives of the *Ludus Helmontii*, and I think it agrees fomewhat therewith.

An account of large fubterraneous caverns *in the chalk hills near* Norwich; *by Mr* W. Arderon, *F. R. S. in a letter from Mr* H. Baker, *F. R. S to the* Pref. N°. 486. p. 244. Feb. & March 1748. *Read* March 24. 1747-8.

IV. About a quarter of a mile from the city of *Norwich*, on the E. fide thereof, and near the entrance of *Moufhold-Heath*, is a large fubterraneous cavern, which has been formed in a long feries of time, by the digging out of chalk for the making of lime. There's but one entrance into it, whofe breadth is about two yards, and it's height nearly the fame; however the height gradually rifes, till at laft it meafures in fome places from 12 to 14 yards. But notwithftanding the entrance is fo fmall, the whole area within is of fuch a large extent, that 20,000 men might with great eafe be placed therein, as I believe will fcarcely be doubted, when I affure you, that, from the entrance to the furtheft part of thefe darkfome cells, meafures full 400 yards; and that thefe paffages are frequently 10 or 12 yards wide, with branchings out on the fides, into various lanes and labyrinth-kind of windings, that every now and then open into one another; which renders it no eafy tafk to find the way out, when a perfon has been a little bewilder'd in thefe fubterraneous mazes.

Moft of thefe vaults are arched at top, whereby the immenfe weight, which every moment preffes on them, is well fupported; a weight no lefs than that of hills, whofe perpendicular altitude above the tops of thefe arches is 20 or 30 yards, if not much more. I have frequently, fays my correfpondent, gone into thefe caverns out of curiofity; but could never perceive the leaft appearance of thofe damps * which are fo common in mines, and other fubterraneous places, where the air is ftagnant for want of a due current; which fhould feem to be the very cafe here, as there is but one entrance into it. The paffage indeed is horizontal, and open to the W. wind; but the included air's being free from putrefaction, may poffibly be owing to the large quantity of falt which the chalk contains.

How deep or thick thefe rocks of chalk are, no one, fo far as I can find, can tell; for, in finking the loweft wells, they have never, that I know of, been dug through; and confequently muft be exceeding deep. The chalk at the further end of this cavern is fo very foft, that it may

* Thofe are commonly caufed by fulphureous vapours, which never appear in chalk. *C. M.*

be moulded with the hand like paste; which I take to be it's original consistence, and what it always retains, till it becomes exposed to the air. In the very lowest parts of these vaults I have picked up several kinds of fossils, figured by marine bodies; such as *echini*, *pectunculi*, common or fluted cockle, *belemnitæ*, *&c.* and, by diligent search, other sorts might perhaps be found. Sounds made beneath these arched roofs are strongly reflected from side to side; so that the least whisper may be heard at a considerable distance. The beat of a pocket-watch was heard distinctly full 20 yards from where it was placed.

I visited this place *Nov.* 1. in order to try the temperature therein, as to heat and cold; and carried with me a thermometer regulated by one of Mr *Hauksbee*'s, which I set down at the further end of these caverns; and letting it remain there for some time, I found the mercury rested at 52°. which comparing with the register I had kept, was, I found, within half a degree of a medium betwixt the greatest heat and the sharpest cold we have known in this city for ten years past; and it is very probable, if the two extremes had been taken more exactly, the temperature in these caverns would be found to come yet nearer to the medium of heat and cold in this climate.

	Hauksbee's Therm.
The greatest degree of heat was *July* 18. 1746.	15
The greatest degree of cold was *Jan.* 9. 1740.	88
Which added together make —— ——	103
The medium of which is —— ——	$51\frac{1}{2}$

I find, by inspecting Mr *George Martin*'s collection, and comparison of the scales and degrees of heat with various Thermometers, that the temperature of heat in these caverns coincides with that in the cave at the observatory at *Paris*, within one degree; which I think comes very near, considering the observations were made with different instruments, and formed upon different principles.

At the foot of a high hill, adjacent to these vaults, issues out a curious spring, whose water I found exactly of the same temperature with that under-ground; though, when the Thermometer was exposed to the open air, it stood at 57°. *

An account of the Giants Causeway in Ireland; in a letter to the Pres. from the Rev. Rich. Pococke, *L.L.D. archdeacon of*

V. In my last passage over to this kindom, I saw that very remarkable curiosity, commonly called the Giants Causeway: † The sea-cliffs are very high thereabouts, and what is called the Causeway is a low head, extending from the foot of the cliffs into the sea like a mole. This head does not appear at first so grand as it is represented in the views

* A terrible thunder-storm, *June* 12. 1748. shook the earth to such a degree as to throw down those chalk-vaults.

† This Causeway is before taken notice of in Vol. II. Part iii. chap. 3. § lxxvi.

engraven

Dublin, *and F. R. S.* N°. 485. p. 124. Jan. 1747-8. *dated* Dublin, Jan. 5. 1747-8. *Read* Jan. 28. 1747.

engraven of it; but when one comes to walk upon it, and conſider it more attentively, it appears to be a ſtupendous production of nature. The head ends in two points: I meaſured the more weſtern to the diſtance of 360 feet from the cliff, and it appeared to me to extend about 60 feet further; but this part I could not meaſure, by reaſon that the ſea was then high; and I was told, that at low tides it might be ſeen about 60 feet yet further upon a deſcent loſing itſelf in the ſea. I alſo meaſured the more eaſtern point 540 feet from the cliff, and ſaw about as much more of it as of the other, when it winds about to the eaſtward, and is alſo loſt in the water. One may walk upon this head on the tops of the pillars to the edge of the water. Theſe pillars are all of angular ſhapes from 3 ſides to 8. The eaſtern point, towards that end where it joins the rocks, terminates itſelf for ſome way in a perpendicular cliff, formed by the upright ſides of the pillars, ſome of which I meaſured, and found to be 33 feet and 4 inches in height. They ſay there are in all 74 different ſorts of figures among them. Each pillar conſiſts of ſeveral joints or ſtones lying one upon another, from 6 inches to about a foot in thickneſs: ſome of theſe joints are in the middle ſo convex, as for thoſe prominences to be nearly quarters of ſpheres, round each of which is a ledge, upon which the ſtones above them have reſted, every ſtone being concave on the under ſide, and fitting in the exacteſt manner upon that which lies next below it. The pillars are from 1 to 2 feet in diameter, and conſiſt moſt commonly of about 40 joints, moſt of which ſeparate very eaſily, tho' ſome others, which are more ſtrongly indented into each other, cohere ſtrongly enough to bear the being taken away in pairs.

But the cauſeway is not I think the moſt ſingular part of this extraordinary curioſity; the appearance of the cliffs themſelves being yet to me more ſurpriſing; theſe and their ſeveral *ſtrata* I examined from the rocks on the other ſide of a little bay, about ½ a mile to the E. of the cauſeway. I thence obſerved, that there runs all the way a *ſtratum* from the bottom of black ſtone, to the height, as well as I could conjecture, of about 60 feet, divided perpendicularly at unequal diſtances by ſtripes of a reddiſh ſtone, looking like cement, and about 4 or 5 inches in thickneſs. Upon this there is another *ſtratum* of the ſame black ſtone divided from it by a *ſtratum* 5 inches thick of the red. Over this another *ſtratum* of ſtone 10 feet thick divided in the ſame manner; then a *ſtratum* of the red ſtone twenty feet deep; and above that a *ſtratum* of upright pillars. Above theſe pillars lies another *ſtratum* of black ſtone 20 feet high; and above this is again another *ſtratum* of upright pillars riſing in ſome places to the top of the cliffs, in others not ſo high, and in others again above it, where they are called the Chimneys.

This face of the cliffs reaches for 2 computed miles E. from the cauſeway, that is about 3 meaſured *Engliſh* miles, to the houſe of Mr *John Stewart* 2 miles W. of *Balintoy.* The upper pillars ſeem to end over the cauſeway, and, if I miſtake not, become ſhorter and ſhorter as one

 goes

goes from it, lying between two binds of ſtone like ſeams of coal, and like thoſe little pillars found in *Derbyſhire*.

Theſe binds probably meet together all round, and incloſe this extraordinary work of nature; and if ſo, the pillars muſt be very ſhort towards the extremities.

I was led to this conjecture by the following obſervations: the lower *ſtratum* of pillars is that which goes by a deſcent into the ſea, and which makes what is called the Giants Cauſeway; and where this deſcent approaches the ſea, it ſeems probable that the pillars become ſhorter and ſhorter, ſo as to end not much further off. Now the upper bind of this *ſtratum* may have been of ſo ſoft a nature, as by degrees, in proceſs of time, to have been waſhed away by the ſea. And in the cliff over the cauſeway I ſaw ſeveral pillars lying along in a rude manner almoſt horizontally, which ſeemed to me to be ſome of the pillars of the upper *ſtratum* fallen down by the giving way of the bind that was under them, and over the lower ones that compoſe the cauſeway. And here moſt probably the upper pillars ended, as they are ſeen no farther in the cliff. I ſaw the tops of pillars even with the ſhore, both on the eaſt and weſt ſides of the cauſeway, and ſome much lower than the cauſeway itſelf; and it is probable that theſe are much ſhorter than thoſe of the cauſeway, which I meaſured above thirty feet higher than the tops of them.

When I was upon the cauſeway, I ſaw in the cliff, to the ſouth-eaſt, what they call the Organs, about a quarter of a mile off, and a third part of the way up the cliff. They appeared ſmall, and ſomewhat like a black *ſtalactites*: they were not commonly known to be ſuch pillars as the others; but they are ſo, and belong to the lower *ſtratum*. When with great difficulty I climbed up the ſteep hill to them, I found they were hexagonal, and larger pillars than moſt of the others, being about 2 feet in diameter; and I meaſured 5 ſides of one of them, which were of 13, 15, 12, 21, and 16 inches reſpectively. The joints I could come at were about 9 inches thick, and each pillar, as well as I could count, conſiſted of between 40 or 50 of them: theſe joints are almoſt flat and plain, the convexities on their upper faces being ſo ſmall as to be ſcarce diſcernible. I inquired whether any of theſe pillars were found in the quarries within land, and the people there told me they were not; but ſince I left the place, I have been aſſured by others, that there are ſome found 2 or 3 miles from the ſhore.

A moving moſs in the neighbourhood of Church-Town *in* Lancaſhire; *by the Rev.* Mr Richmond; *communicated by*

VI. On *Saturday Jan.* 26. 1744-5. a part of *Pilling Moſs*, lying between *Heſcomb* houſes and an eſtate of Mr *Buttler*'s, called *Wild Bear*, was obſerved to riſe to a ſurprizing height: after a ſhort time it ſunk as much below the level, and moved ſlowly towards the ſouth ſide: in half an hour's time it covered 20 acres of land. The improved land adjoining that part of the *Moſs* which moves is a concave circle containing near 100 acres, which is well nigh filled up with moſs and water. In ſome parts it is thought to be five yards deep. A family is driven out

out of their dwelling-houſe, which is quite ſourrounded, and the fabric tumbling down. Mr *Buttler*, *Whitehead*, and *Stephen White*, are the firſt ſufferers by this uncommon accident. An intenſe froſt retards the regreſs of the *Moſs* to-day; but I fear it will yet ſpoil a great deal of land. The part of the *Moſs* which is ſunk like the bed of a river, runs N. and S. is above a mile in length, and near half a mile in breadth; ſo that I apprehend there will be a continual current to the S. A man was going over the *Moſs* when it began to move: as he was going eaſtward, he perceived, to his great aſtoniſhment, that the ground under his feet moved ſouthward. He turned back ſpeedily, and had the good fortune to eſcape being ſwallowed up.

Edward Milward, *M. D. F. R. S.* N°. 475. p. 282. Jan. &c. 1745. *Read* Feb. 28. 1744-5.

VII. We have few or no foſſils in this country; but a friend in *Staffordſhire* [Mr *Platt*] informs me, that that country abounds much in foſſils; ſuch as ſea-ſhells, rock-plants, and other marine bodies left at the deluge. Near *Bakewell* in *Derbyſhire* was lately found the ſkeleton of a man, with ſome ſtags horns, in digging a lead-mine.

A foſſil Skeleton *of a* Man; *by* Roger Gale, *Eſq*; *F. R. S. to Mr* Peter Collinſon, *F. R. S. dated* Scruton *in* Yorkſhire. Jan. 14. 1743-4. Ibid. p. 265. *Read* Jan. 24. 1744-5.

[In the ſecond letter, dated *Scruton*, *May* 19. 1744. Mr *Gale* gives the following account of this *Skeleton* in Mr *Platt*'s own words, from a letter written to himſelf by that gentleman.]

The ſkeleton I formerly mentioned to you was found at *Lathill-dale*, near *Yolgrave* and *Bakewell* in *Derbyſhire*, as the workmen were driving a ſough, or drain to a lead-mine, about 9 yards deep from the ſurface of the earth, and about 40 fathom from the beginning of the ſough. There were found with the ſkeleton ſtags horns; two pieces of which I have now in my cuſtody; *viz.* the brow-antler, which is 9 inches long, and ſeems to have about 2 inches broke off the tip-end; the other is a piece of the large horn near the head, and is 3 inches diameter. Both the horns of the ſtag, and the rib-bones of the ſkeleton, are much decayed; and as ſoon as the head of the latter was expoſed to the air it crumbled all away, except a piece of the lower jaw; now alſo ſo imperfect as not eaſily to be diſtinguiſhed what it has been. Several of the larger teeth were taken out, which were covered with their natural enamel, and perfectly ſound. The place where theſe things were found, is on every ſide ſurrounded with a rocky petrified ſubſtance, or *terra lapidea*, by the miners called *Tuſt*, ſo hard (as they ſay) as to ſtrike fire againſt their tools. This ſubſtance lay above the bones and horns a yard and half thick or more, and on either ſide; and beneath them to a breadth and depth uncertain: ſo that it appears, that the ſkeleton and horns lay in a cavity, which was not however contiguous to them, there being a ſort of ſoft coarſe clay or marl interſperſed thick with little petrified balls, or pellets of the ſame kind of ſubſtance as the tuft, for near a quarter of a yard round them; but none of the bones ſeemed in any degree to be petrified. The work-

men

men conjectured there was more of the skeleton to be found; but they dug no further than was necessary to complete their sough.

The interment of this man and stag seem to me to have been accidental, by their falling into a chasm or wide cleft of the rock in very early times; which has since closed up, and grown over them, by the accretion of the marly substance, which environs the skeleton, *&c.* and in time, perhaps, will grow as hard as the tuft, and rest of the rock.

An account of some human bones incrusted with stone, now in the Villa Ludovisia *at* Rome; *communicated to the* R. S. *by the* Pres. N°. 477. p. 557. Aug &c. 1745. *Read* Dec 12. 1745. Fig. 46.

VIII. Something like the body of a petrified man being mentioned by several authors, as preserved in the *Villa Ludovisia* at *Rome*, I thought, that a drawing of that curiosity, which I procured at *Rome* some years since, might, possibly deserve the notice of the gentlemen here present: especially, as it will appear thereby, that the several accounts hitherto given of it are not very accurate, or, at the best, convey but a very imperfect idea of the truth.

The following passage occurs in the journal-book of the *Society*, for *April* 17. 1689: " Mr *Henshaw* related, that he had seen, in the *Villa* " *Ludovisia* at *Rome*, the body of a man incrusted with a sort of a white " marble or alabaster case, supposed to have been a man frozen in the " *Alps*, and after, in long process of time, this incrustation to have " grown upon him; and that one of his arms was broken off, purposely to shew, that it was no imposition."

Mr *Richard Lassels*, in his travels to *Italy*, printed at *Paris* 1670. *p.* 180. tells us, that in the lesser *Casina*, belonging to the *Ludovisian Villa*, he saw, " in a great square box lined with velvet, the body of a " petrified man, that is, a man turned into stone; one piece of the leg " (broken off to assure an ambassador doubting of the verity of the " thing) shewed plainly both the bone and the stone crusted over it. " The head and the other parts lie jumbled up together in the box."

F. *Athanasius Kircher* says, in his *Mundus subterraneus*, l. viii. chap. 2. " *Spectatur et hic Romæ in horti Ludovisiani palatio, corpus humanum* " *totum in saxum conversum, ossibus adhuc integris, at lapideo cortice obductis.*" And in the following page he gives an imperfect sketch of the same thing, under the title of " *Sceleton humani corporis in saxum* " *conversum, ex palatio Pinciano principis Ludovisii.*" This sketch, however imperfect, gives a truer idea than either his or Mr *Lassels*'s words seem to convey, as there is indeed nothing like the body of a man, but only a cluster of disjointed bones cemented together by the same matter that incrusts them over. Mr *Misson* in his travels has more truly described them, when he says, that " in the same room they shew " a small heap of bones, said to be the sceleton of a petrified man; " which is a mistake, for the bones themselves are not petrified, but " there has gathered about them a sort of candied crust, or stony in- " crustation, which has made them pass for being of real stone." Mr *Wright* also, in his late observations made in travelling through *Italy*,

&c.

&c. has taken notice, that in the *Villa Ludovisia* "they shewed some "bones of a human body all crusted over with a petrified substance."

When I was at *Rome* in the year 1734, I myself saw this curiosity, which is still preserved in the same *Casina* of the *Ludovisian* gardens; and in the very square box lined with velvet, that is mentioned by Mr *Lassels*, and represented by Father *Kircher*: and as I had before heard it much spoken of, and had conceived an idea of it very different from the truth, I was willing both to preserve a true notion of it myself, and to be able to give such a one to others. I therefore employed an ingenious young painter to make as exact a drawing of it as he could; and I afterwards very carefully compared his drawing with the original, which is the same I have here to produce, hoping that the sight of it will not be unacceptable to the company. The stony substance that joins the bones together is of a whitish colour, and the same as that which incrusts the bones themselves: small fractures in several places discover the natural bones; and the size of the whole mass may be judged of, by considering the scull, which is of the common dimensions, as a scale to the other parts.

A letter from Mr H. Baker, F. R. S. to the Pres. concerning an extraordinary large fossil Tooth *of an Elephant.* N°. 475. p 331. Jan. &c. 1745. *Read* March 28. 1745. Fig 47.

IX. It was sent me lately, from *Norwich*, by Mr *W. Arderon*. It seems to be a grinder belonging to the left under-jaw of a very large elephant, as it's own size and weight may shew: for the circumference, measured by a string drawn round the edge, is 3 feet, wanting 1 inch; in length it measures 15 inches; in breadth, where widest, 7 inches, in thicknefs above 3; and it's weight is upwards of 11 pounds.

On one side it is convex, and on the other concave, with 16 ridges and furrows running on each side transversly, and corresponding with the same number of eminencies on the grinding edge, which appears furrowed like a mill-stone. On the bottom of the part that lay within the gum are several cavities for the insertion of the nerves. The whole tooth is almost intire, and seems very little, if at all, petrefied; but, since it's being exposed to the air, several little cracks appear. Other monstrous bones were found with it, as I am informed; and particularly thigh-bones, 6 feet long, and as thick as the thigh of a man; all which belonged probably to the same animal, and may be considered as farther proofs of the creature's enormous size.

The place where, and the manner how, these bones were discovered, are particulars so well deserving consideration, that I shall make no apology for relating them.

A little town, called *Munsley*, is situated close to the sea-shore, on the N. E. coast of the county of *Norfolk*, where the sea is bounded by exceeding high rocky cliffs: some whereof being gradually undermined by the continual dashing of the waves when the tide comes in, great pieces frequently tumble down upon the shore: and by the tumbling down of one of these the above mentioned bones and grinder were discovered.

Here

Here therefore can be little reason for imagining (as I know some have done, when such-like bones have been found in more inland counties), that the *Romans* brought elephants over hither; which, when dead, they buried deep in the earth, to prevent their becoming offensive: for they could never think of burying such a carcase in a rocky cliff, close to, or perhaps over-hanging, the sea. But, on the contrary, this discovery seems a convincing demonstration, that the earth has undergone some very extraordinary alterations: for the remains of animals, of quite different climates and regions, and of kinds, which, in the present situation of the world, could never possibly come over hither, must either imply their having been placed here by Providence, originally, or, that this island must, heretofore, have been contiguous to the Continent: but, since we find these creatures in very hot countries only, it is highly probable they were never placed here by Providence; unless we can suppose the temperature of our climate, as to heat and cold, to have been greatly altered: and, without such a supposition, it would be no less unreasonable to imagine they would wander hither from warmer regions, though even all the quarters of the globe should have been contiguous.

What changes have happened to our earth, and how they have been produced, no human wisdom can possibly find out with any certainty: but suppose only the polar points, or *axis* thereof, to have been shifted at any time but a few degrees, and it's centre of gravity to have been altered (which some great men have imagined not improbable), what convulsions in nature, what an universal change in the face of things, must thereby have been occasioned! What inundations, or deluges of water, bearing every thing before them! What breaches in the earth, what hurricanes and tempests, must have attended such an event! For the waters must have been rolled along, till, by them, an equipoise was produced. In short, all parts of the world would thereby acquire different degrees of heat and cold from what they had before. Seas would be formed where continents had been: continents would be torn in sunder, or perhaps split into islands. The antient bed of the sea would be changed into dry land, and appear covered at first with shells, and other marine bodies; of which the action and nitrous salts of the air would, in a few years, moulder away and turn to dust those upon the surface; but such as were buried deep would be preserved and remain for many ages.

Such would probably have been the fate of inanimate things: and as to living creatures, they must have been almost universally destroyed and buried in the ruins of the world, as perhaps this elephant may have been. Some few, however, would in all likelihood escape, either by swimming to, or being left on, rising lands; where, if they met with proper food, and an agreeable climate, they would continue and increase, or otherwise would wander till they found such a country, unless prevented by interposing seas, or impassable rivers.

All

All this indeed is barely conjecture: but the bones and teeth of fishes, the multitudes of sea-shells (some whereof are petrefied, and others not), and the many sea-productions found buried in the earth in almost every country, at vast distances from the sea, and even in the midland parts, are demonstrations of the surprizing alterations that must have happened as to the disposition of sea and land. The horns of the great mouse-deer, dug frequently out of the bogs in *Ireland*, and sometimes in *England*, the bones and teeth of elephants found there, and this present discovery, together with some others of the like kind that have been made in *England*, seem to prove, that such animals formerly inhabited these countries, notwithstanding the mouse-deer is known at present only in *America*, and elephants are not found except in *Africa* and *Asia*.

Part of the horn and palm of a deer, found in a chalk-pit, at a village called *Baber*, 4 miles E. of *Norwich*, at the depth of 16 feet, and almost converted into a chalky substance, being of a kind of which, I am told, we have none in this island, I also lay before you, as another proof to the same purpose.

X. The head and horns, represented in *Fig.* 48. were found in a sand-bed, in the river *Rye*, which runs into the *Derwent*, in the east-riding, belonging to *Ralph Crathorn*, Esq; They were discovered as he was fishing for salmon; the net happening to hang on one or two of the antlers. He ordered to pull away; by which some of the antlers were broke off, and discovered it to be part of a deer's horn. At length, with some difficulty, it was dug out pretty intire. Mr *Crathorn* supposes, that these wild moors were once inhabited by this kind of deer, not any such now being known to be in this kingdom; and supposes it is, at least, 7 or 800 years since it's death; and that by age or poverty destroyed, and by time buried in those sands. It is about 3 years since it was found (as the abovesaid worthy gentleman told me) where he lives; which is at *Ness* near *Malton* in *Yorkshire*.

An account of two extraordinary Deers Horns, *found under-ground in different parts of* Yorkshire; *in a letter from Mr* Tho. Knowlton, *to Mr* Mark Catesby, *F. R. S.* N°. 479. p. 124. Mar. & Apr. 1746. *Read* March 13. 1745-6. Fig. 48.

a is 12 inches long. *b* the same. *c* the same. *d* is 4 inches from the main horn, and the 2 cross branches are 8. *e* is 6 inches. *f* is 7 inches. *g* is 6 inches; and 2 feet 10 inches from the root of the horn to the tip.

All those places with marks were broke, and put together again.

	St.	lb.
This skull and horns weigh ———	4	12

It was found in a peat-moss, at *Cowthorp* near *North Dreighton* in *Yorkshire*, in the year 1744.

	Feet	Inch.
The length of the skull, from the nose-end marked *A*, to the back-part of the head *B*,	1	10
The breadth of the forehead, from *C* to *C*,	0	11
Length of each horn, from the skull *D* to the tip,	5	1

	Feet	Inch.
The extent of the horns, from *E* to *E*	6	1
The breadth of the web or palm, from *FG* to *F*,	2	1

F G and *G*, two places where the horns are broke.

1 The nostrils.

2 The eye.

3 The teeth, which are very large and sound.

N. B. It is evident the horns are not at their full growth, being yet covered with what is called the Velvet.

The figure above is the representation and extraordinary dimensions of the skull and horns of a deer, dug from the depth of 6 feet out of a peat-moss, as above mentioned.

But what I think more extraordinary is, that the late Earl of *Carlisle*'s steward, Mr *Joice*, in digging the foundation of an house and cellars, found, at the depth of 6 feet, a part of a jaw-bone with teeth, and a horn of a buck or stag, of most exceeding large dimensions, which lay buried under 2 feet common soil; then one foot of scalping or sand-bed; then 18 inches of stone; then another vein of sand, 6 inches; then another head of stone; under which lay those before-mentioned jaw-bone, and piece of horn; which, in all appearance, to every one that viewed these *stratums*, had never been removed.

Dimensions of the Deers Horns in the Musæum *of the* Royal Society.

	Feet	Inch.
Length of the skull — —	1	4
Breadth of the forehead — —	0	9
Length of each horn — — —	5	0
Distance of the extreme tips of the horns —	6	0

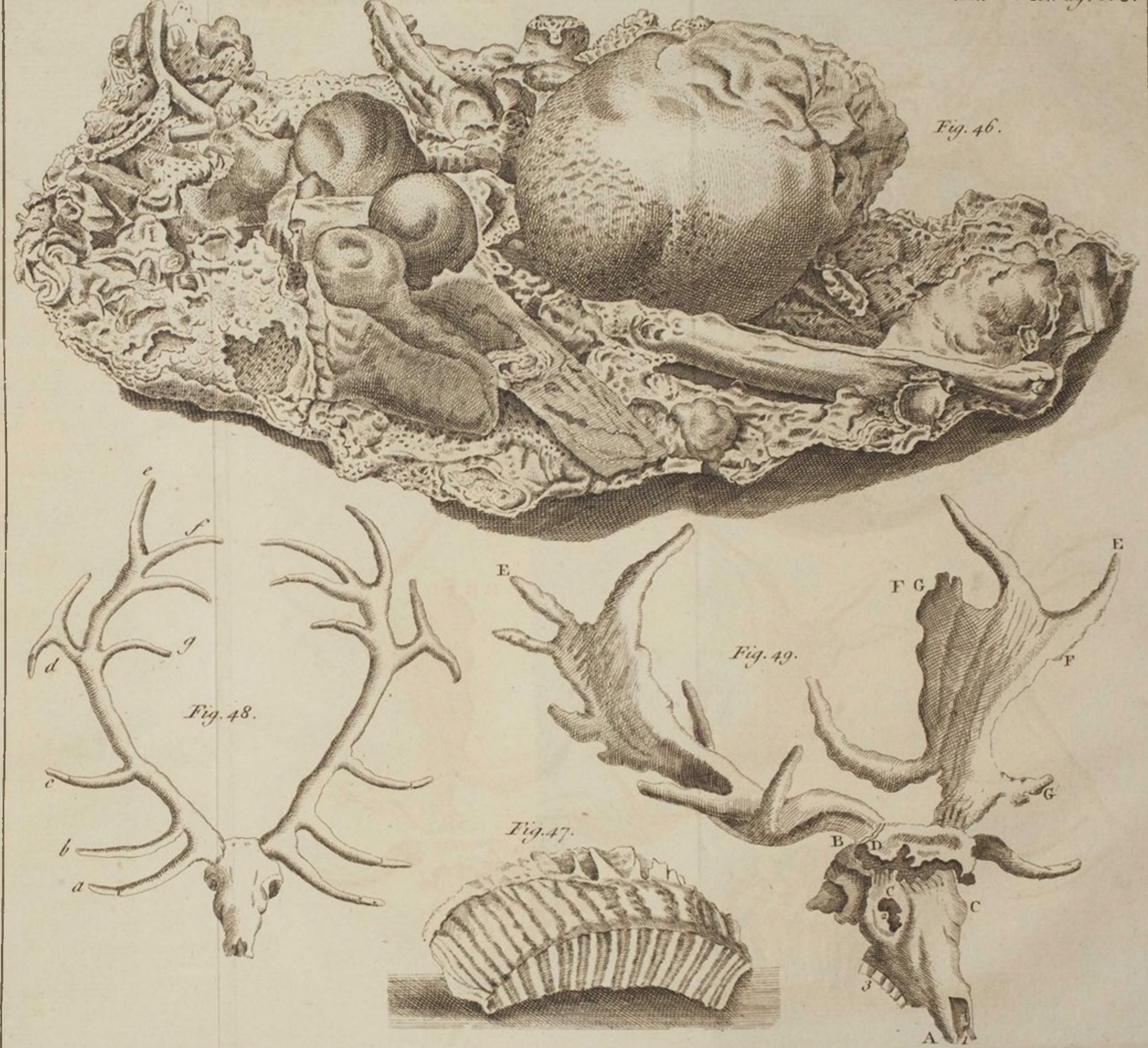
N. B. These horns (*Fig.* 49.) are evidently of the same sort as those often found in *Ireland*, of which descriptions are given in *Transact. n.* 227. *n.* 394. and *n.* 444. *p.* 389. But I do not remember to have met with any before of this species found in *England*, or any-where else besides *Ireland*. *C. M.*

An inquiry into the Lapis Osteocolla; *by* Ambrose Beurer *of* Nuremberg; *communicated by Mr* Peter Collinson, *F. R. S.* N°. 476. p. 373. Apr. &c.

XI. The *lapis osteocolla* is distinguished by several names; it is however generally called *Ostiocolla*, or *Osteocolla*, a name compounded of 2 Greek words, ὀςέον, *a bone*, and κόλλα, *glue:* it is otherwise called *Lapis ostites*, *ollosteos*, *ossina*, *ossisana*, *ossifraga*, *lapis Asiaticus*, *lapis Morochius*, *flores arènae*, *fossile arborescens*, *lapis sabilis*, and *lapis arenosus*. It is found in several parts of *Germany:* but our ancestors had no just knowledge of it; for some took it to be petrified bones, and others believed it to be a sort of *gypsum*.

The soil wherein it is produced, is always sandy and barren; and seldom produces any sort of plant, except the poplar: but none of them, as

Pla. X. Vol. X. Part II. Pag. 602.



as some have pretended, are ever found in vallies, or springs, or in clay. 1745. *Read* May 9. 1745.

Kræuterman has mentioned one that had the figure of a sort of chest: but it seems rather to have been a *tophus*, than *osteocolla*. *Mercatus* also had no just knowledge of this substance, when he called some petrefactions, and *tophi calcarii* by that name, which in the opinion of *Herman* are rather *bolaria* or *cisti*.

As to it's origin, it is produced in the sandy soil mentioned above, at the depth of some feet, and has the form of a root. The largest can hardly be compassed by both hands, but the others are gradually less. As to it's consistence, the *osteocolla*, whilst under ground, is never hard, but always soft and muddy, so that it seems like suet or fat to the touch, but when dry it is white, like any calcarious substance. As it is found under ground, it is partly grey, partly yellow or white, and the sand adheres copiously to it's outside. It's soft consistence will not permit it to be taken out whole, so as to shew the true form of a root, unless the work is carried on with great art, and some weeks, or even months are bestowed upon it: for if it is taken up hastily out of the sand, it will break: wherefore the following rules are to be observed:

1. It must be sought for with labour and patience. 2. The sand must be gently wiped off. 3. It must be taken far from the root. 4. An examination must be made, whether there are any small secondary roots shooting forth, of which great care must be taken, that they are not lost. 5. Several must not be taken at the same time; but liberty should be given them to grow dry and hard. 6. The pit should be covered with boards, to keep off any rain that may happen to fall. 7. The digging must not be repeated, till those which are already exposed, are grown sufficiently hard. 8. In warm, dry, and clear weather, the planks should be removed, that they may be the sooner dry. 9. The digging should be begun at a good distance, as water is generally found underneath.

Many authors have observed, that *osteocolla* is hollow within; but they do not agree, whether it is to be referred to the vegetable or mineral kingdom. Many of the Ancients thought they were bones transformed; but many denied it, because there are not found any perfect fragments of bones, nor is any trace of animal parts discovered by Chemistry. *Erasmus* wrote very probably concerning it: those who would not acknowledge it to be bones, have supposed it to be a mineral naturally generated from the sand. Prof. *Teichmeyer* calls it a marle. The skilful Metallist *Henckelius* refers it to minerals; but is silent as to it's generation. Prof. *Juncker* will have it to be generated in the sand, but does not add, whether it proceeds from the trunks or roots of trees. For my part I think it is a root, to which the sand adheres, and that it is thus gradually generated. And tho' there is a great quantity of *osteocolla* found, yet there is never any wood, at least such as is green, found standing above ground; and tho' I made a very exact inquiry into the

 origin

origin of this *osteocolla*, and of it's tree, the root of which had degenerated into *osteocolla*; yet it was a long time before I discovered a dry bough, and green twig adhering to a tree, which still remained woody in the upper part, and was at the same time changed into mere *osteocolla* in the lower part, and upon a more scrupulous examination appeared to be a sort of poplar. It's origin therefore is to be sought in the black poplar, on the roots of which, when the tree is cut down, the *osteocolla* is formed.

In all the parts of *osteocolla*, something ligneous is found, tho' rotten, and the decay of the wood leaves them hollow; so that they have the resemblance of bones. But I never found any *osteocolla* on the trees near them, and growing in the same soil: whereas if the cause of it's production was in the soil, it would be found on pines, birches, and other trees.

Hence we may learn, 1. That *osteocolla* is hardly ever found in *saline* places. 2. That it is probable, that there have always been poplars, where *osteocolla* is found. 3. That all the diggers up of *osteocolla* may see that it has been a root. 4. That another *criterion* may be made, that when any *osteocolla* is found, some calcarious bones are prominent, which seem to express a flower: whence many have fancied, that it grows and flowers.

The *osteocolla* therefore is always under the sand, but never in any place but where it's roots had been, in adhering to which it has gradually grown hard, and so far as it has appeared above ground become white; and if any difference is found, it must be imputed to accident. When the root is once found, if you dig a span deeper, you will certainly find *osteocolla*. So long therefore as it remains under ground, it is soft, or about the consistence of macerated lime mixt with sand; but as the moisture is evaporated, it becomes gradually harder.

To the generation of it therefore is required, 1. The root of a poplar. 2. If the root of a poplar cannot be seen, yet by distillation it's vegetable substance may be demonstrated in the empyreumatic oil 3. The acid of salt closely united with sand conduces much to it's generation. 4. that, as we know by Chemistry, that there always remains something of an acid in sand, and consequently something lapidescent; so by the force of these, the acid of salt with much moisture of the sand constitutes a substance, only here the free access of the air is still wanting, whereas otherwise it must have been transformed into a stone in the earth.

It appears from this, because 1. The mass hardens as soon as it is exposed to the air and dried. 2 Distillation discovers something of an empyreumatic *petroleum* composed of vitriolic and bituminous parts. 3. If oil of vitriol is poured on *osteocolla*, an acid of common salt separates from it. 4. I infer that *osteocolla*, is not a *calx*, because it can by no means be extracted from it.

I have tried the *osteocolla* with various menstruums what weight of each would be dissolved in each of them: wherefore I always took ℥ ß of of *oſteocolla*, and ℥ ſs of each *menſtruum*; and obſerved, 1. That oil of vitriol diſſolved gr. iv. of it, that the ſolution was yellow, and that the reſidue was of a yellowiſh white. 2. That ſpirit of vitriol reduced all into a ſaline form. 3. That Sp. of Nitre diſſolved ℈i. gr. iv. And 4. The acid of common ſalt ℈i. gr. vi. 5. That *Aqua Regia* diſſolved ℈i. gr. iv. and that both ſolutions were yellow, and the reſidue of a yellowiſh white. 6. That diſtilled vinegar diſſolved ℈iſs, and that the ſolution of it was yellowiſh; but that the reſidue, like the others, remained free from the *menſtruum* as to any change of colour.

Oſteocolla therefore, as it is uſed in the ſhops, is to be reputed a mineral from which a vegetable has been ſeparated: it may be called a calcarious earth; but it does not change the ſyrup of violets.

By diſtillation with an open fire, it affords an urinous ſpirit, it ferments on the pouring on of a fixt alkali, and diſcovers at the ſame time an urinous ſpirit: the reſidue being mixt with water, and reduced to a lixivium, did not produce any thing ſaline; I endeavoured at the ſame time to reduce one part into a *calx*, but could not obtain any. But if oil of vitriol is poured on *oſteocolla*, an acid of common ſalt ſeparates from it. *Oſteocolla* being calcined with an alkali ſeems to conſtitute an opake glaſs, but it may be reſolved again into water, and therefore it cannot be looked upon as a true glaſs. *Oſteocolla* being put into a tubulated retort, and ſet over a ſlow fire, and having oil of vitriol poured thro' the tube, gets free from the ſpirit of ſalt, which may alſo be obtained by diſtillation. I have alſo ſaturated the Sp. of ſalt thus obtained with a fixt alkali, and produced a regenerated common ſalt: I have put it again into the retort after it has been dried, and poured oil of vitriol upon it, and at laſt have obtained by diſtillation an acid ſpirit of common ſalt.

The chief *baſis* of *oſteocolla* is ſand; ſome will have *lac lunae*, *medulla ſaxonum*, and *lapis oſteccollae* to be one and the ſame thing, but they are miſtaken. If any redneſs is found in the *oſteocolla*, it is owing to ſomething of iron. It's uſe in medicine is abſorbent; whence ſome preſcribe it for the *Fluor albus*.

A letter from Mr John Hill, *Apothecary, to the* Preſ. *concerning* Windſor Loam. N°. 483. p. 458. Mar. &c. 1747. *dated* May 28. 1746. *Read* Mar. 19. 1746-7.

XII. An accident calling me yeſterday to *Hedgerley*, the place where there is dug an earth commonly called *Windſor* loam, and famous not only in *England*, but many other parts of the world, I took an opportunity of going to the pits, and informing myſelf of the preſent condition of them: and as there appears too much probability that this earth will be exhauſted, and loſt intirely to the world, in a few years, I preſume it may not be unacceptable to you to have an account of the pits of it, and whatever elſe relates to it, taken on the ſpot; which I here do myſelf the honour of communicating to you, and ſhall take the liberty of adding to it what has ſince occurred to my thoughts in regard to the ſupplying it's place when loſt, in the many different occaſions on which it is now uſed.

This

This earth itself is a coarse harsh loam, composed of a very large shining sand, of extreme hardness, and a fine soft tenacious clay: it's value is it's remarkable quality of standing the force of the most violent fires without running to a glass; which makes it extremely useful to all who have occasion for such fires, and is the reason of it's being sent not only into all parts of *England*, but to *Holland*, *Germany*, and many other parts of the world. It is used for making the bricks employed in building the wind-furnaces for melting iron, for coating over the insides of assay-furnaces, used by the workers on metals, and on many occasions of like kind at the glass-houses, both in *England* and other nations.

The place where it is dug is *Hedgerley* before-mentioned, a small village about 22 miles from *London*, surrounded with hills, under one of which this loam lies. The pits are about ¼ of a mile S. W. from the town, and 5 miles N. of *Windsor*: they extend over 4 acres of ground, situated on the descent of a hill; and were intended to have been carried over much more ground by the person who now works them; but, on trials, the loam is found not to extend as was imagined.

They dig, before they come at this, a very good common brick-clay, a tile-clay, and a potter's earth, a kind of clay of a firmer texture, and deeper colour, than either of those; but the *strata* of these are seldom pure or regular, and at the boundaries of the *stratum* of loam a pure hard sand, evidently the same with that in the composition of the loam, but left loose, from there not having been clay in the way to bring it into the condition of the perfect mass. They have already worked the *stratum* so far as to find it bounded E. and W. by beds of this sand, and N. by chalk, and are therefore afraid it will be soon exhausted; at least, whatever they get hereafter, must be procured with more labour and expence, as they have no where to search for it but higher up in the hill; from whence it must be fetched at greater depths, and much more expence: and this increasing difficulty of procuring it has been the reason of it's rising in it's price to that it is now sold at, which is 5 shillings a bushel in *London*; but which is not to be wondered at, since on the spot the quantity that makes 1000 bricks, which used to cost 1 *s.* and 8 *d.* now costs 10 *s.* the digging, and will every year cost more and more, unless a new *stratum* of it should be discovered somewhere thereabouts, which their many unsuccessful trials make them at present despair of.

It is to be observed, that this valuable earth forms but a single *stratum*, and that does not rise and dip with the elevation and descent of the hill, as the *strata* of the earth, stone, *&c.* in hills usually do, but seems to be even and flat at it's bottom; for the higher up the hill they open their pits, the deeper in proportion they find the *stratum* of loam lie.

It is worthy observation, that this hill appears from this not to have been formed as the hills and mountains on the earth in general have been

been by a diſruption and elevation of the *ſtrata* by violence from within the earth; for, in that caſe, this *ſtratum* of loam muſt have been elevated with them, and would have been as near the ſurface, or nearly ſo, in one part of the hill as in another, and need have been dug for no deeper from the top than from any other part; whereas, on the contrary, it appears to lie flat and level underneath the whole maſs of earth, which makes the hill, and was, in all probability, the ſurface, on the firſt ſettling of the terreſtrial and other matter from among the waters of the Deluge.

The earth, which makes the hill, ſeems to have been a prodigious maſs of matter, rolled along by the irreſiſtible force of that immenſe body of water, and afterwards lodged upon it.

That this might be the caſe, the immenſe force of that vaſt quantity of water, and the eaſe with which heavy bodies are moved in water, may ſerve to make probable; and what the more favours the conjecture is, that the earth which makes the hill is not diſpoſed in ſuch regular pure *ſtrata* as the earths ſettled regularly from the waters always are, but ſeems evidently a mixed maſs, made by the jumbling together of various kinds of clay, *&c.* which are, in ſome parts of it found pure, tho' not in whole *ſtrata*; and in others irregularly blended in different proportions one with another; which, as the principal matters that compoſe it are of very different colours, *viz.* a red and a white clay, is the more apparent. And this is further confirmed, by there being none of thoſe common extraneous nodules found lodged in it, which are ſo frequent in the *ſtrata* of clay formed by ſubſidence; ſuch as the *Ludus Helmontii*, *pyritæ*, *&c.* Theſe have ſettled with, and lodged themſelves almoſt every-where among thoſe *ſtrata*; but it is no wonder there are none of them here, if this hill has been formed, as I imagine; ſince, in the rolling it along, they muſt naturally have been left behind: and I promiſe myſelf, that the frequency of theſe bodies in almoſt all our little clay-pits, and the intire abſence of them in the vaſt quantities of clay that have been dug here, will be eſteemed, by all who have looked deeply into theſe ſtudies, one great argument of the truth of this ſyſtem; which may alſo extend perhaps to many other hills as well as this.

As the workmen are now obliged to dig this loam at 26 feet deep, inſtead of about 14, at which depth they long found it, and muſt hereafter, as they are obliged to aſcend the hill, dig it at 38 or 40 feet, the price of it will, I am afraid, rob us of it, before the vein is exhauſted. I think it would be a matter worthy conſideration, whether, from examining the parts it is compoſed of, a *ſuccedaneum* might not be found for it, by an artificial mixture of ſimilar ſubſtances. In order to attempt this, I have, by means of water, diſunited it's parts, and procured them ſeparate; and, on comparing them with the various earths and ſands from different parts of *England*, which I have at times procured, I think that I can exactly match the ſand with one from *Hampſtead-Heath*, and

and the clay with one from a pit near the lower end of *Highgate*: the proportions may be easily learned, by accurate observation of the quantities of each, where disunited; and a *succedaneum* on these principles easily made.

It is evident to me, that the only reason why it endures the fire so much better than other clays, is the extreme hardness and great quantity of the sand it contains: and as I imagine it easy to throw a sand of equal hardness, and in equal quantity, into an artificial loam, I see no reason to doubt of making it equally useful.

The formation of Pebbles; *in a letter from Mr.* W. Arderon, *F. R. S. to Mr* H. Baker, *F. R. S.* Ibid. p. 467. *Read* April 2. 1747.

XIII. In my late searches after sands, pebbles, and other fossils, in our county of *Norfolk* (some whereof I had the pleasure to send you not long ago) I made such occasional observations on the situation and condition of the several bodies I met with, as reason must, I think, suggest to every man that considers them. I shall trouble you with no hypothesis, nor form any random guesses, to account for such their situation, and the condition wherein they are found; but, if a relation of true facts, and conclusions naturally deducible therefrom, may prove acceptable, they are intirely at your service.

In all *strata* of pebbles, that I have yet examined, there are some which are broken, and whose pieces lie together, or very near each other; but, as bodies of such hardness could not be broken without some considerable force or violence, their situation implies, that they suffered such force or violence as broke their parts asunder, in or near the place where they at present lie.

Others again have had pieces broken from them, though not the least fragment of those pieces can now be found: from whence we must conclude, that whatever might be the cause of their fracture, they must either have been broken at some place distant from where they now lie, or the pieces broken from them must at some time or other have been removed to some distant place.

Several of these pieces of broken pebbles have their edges and corners so very sharp, that it seems as if they had never been removed from the place where they received the damage. Others have their sides and corners so blunted, rounded, and worn away, that one cannot help imagining they must have been very roughly tossed backwards and forwards against other hard bodies, and that too with great violence, or for a very long continuance; since, without a great deal of friction, such hard bodies could scarcely have been reduced to the forms they are now found in.

It may possibly be objected, that these pieces of stones grew in the figure wherein they now appear; but I am fully satisfied, that any man who will take the pains to examine these bodies carefully, will soon be convinced, from their veins, or grain, or coats, which surround each other, somewhat like the different years growth in trees, that they must once

once have been complete and intire: and this will be more fully evident, if they are compared with a ſtone broken by art.

Among theſe *ſtrata* of pebbles are ſeveral fragments of various kinds of marble, various kinds of ſand-ſtone, and various kinds of *gypſum* (though this part of the kingdom affordeth no ſuch thing); moſt of which have attained the hardneſs of the very hardeſt of our pebbles, as it ſhould ſeem, by lying amongſt them.

Such pebbles as are found here in *ſtrata* near the ſurface of the earth, are much more brittle, and break eaſier without compariſon, than thoſe which lie in deeper *ſtrata:* for, if the firſt of theſe fall, but with their own weight, upon any other ſtone, from the height of 3 or 4 feet, they will break very frequently into 10 or 12 pieces; whereas ſuch as are found deep in the earth will endure being thrown againſt one another with all the force one can give, and that too 20 times perhaps, before the leaſt ſplinter of them can be broken off.

I have conſtantly found, that the more clean and tranſparent the ſands are with which our pebbles are mixed, the more beautiful the pebbles themſelves are, however different their colours be.

It is wonderful to obſerve and conſider with what amazing ſkill the Creator of all things hath diſpoſed the different *ſtrata* of the earth, to ſerve the purpoſes of His Wiſdom.

The vegetable mould or ſurface of the earth is compounded or made up of ſands, clays, marls, loams, rotten ſtalks, and leaves of herbs, *&c.* ſerving as a proper bed and covering, as well as a receptacle and conductor of moiſture, to the roots of trees and plants in general.

Sands and pebbles may be conſidered as drains for carrying off the redundant moiſture, to where it may be ready to ſupply the place of what is continually riſing in exhalations; but, leſt the *ſtrata* of ſand ſhould be too thick, ſmall ones of clay are often placed between, and ſeem intended to prevent this moiſture from departing too far from where it may prove of general uſe. And, leſt theſe curious but thin partitions of clay ſhould give way, by their ſoftneſs, for the particles of ſand to inſinuate into them, and thereby let the moiſture paſs through, thin cruſts of a ferrugineous ſubſtance are placed above and beneath each of theſe clayey *ſtrata*, and ſerve effectually to keep the clay and ſand aſunder.

The obſervations you have now read muſt be underſtood to relate to the county of *Norfolk* only; for I have never had any opportunity of ſearching the bowels of the earth in other places; but the general uniformity of nature makes me ſuppoſe the ſituation and circumſtances of pebbles, ſands, *&c.* in other countries may not be very different.

XIV. Gems or precious ſtones, of all ſpecies, are ſometimes found of regular ſhapes, and with a natural poliſh; and ſometimes of irregular ſhapes, and with a rough coat. The firſt ſort may be conſidered as of the pebble kind; and they are ſaid to be found near the beds of rivers,

Some obſervations upon Gems *or* Precious Stones; *more particu-*

larly such as the Ancients used to engrave upon; by Robert Dingley, *Esq;* Ibid. p. 502. *Read* May 7. 1747.

rivers, after great rains: the others are found in mines, and in the clefts of rocks.

The gems of the first sort were what the Ancients most usually engraved upon: these are commonly called *intaglio's*; and they are mostly of a long oval figure, inclining to a point at each end, convex as well on the engraved face, as on the others, with a ridge running from end to end on the under side, which is hereby, as it were, divided into two faces; both which are also, though not so distinctly, parted from the upper face, by another ridge running quite round the oval.

The stone most commonly found engraved is the *beryl*; that most frequently found next is the *plasm*, or prime *emerald*; and then the *hyacinth* or *jacinth*. The *chrysolite* is sometimes, but rarely, found engraved; as are also, but that very seldom, the *crystal*, or *oriental pebble*, the *garnet*, and the *amethyst*.

Of the *beryl* there are three species; the red, inclining to orange-colour, transparent and lively; the yellow, of an ochre-colour; and the white, commonly called the *chalcedon*, of the colour of sheer milk. These two last have less life than the first.

The *plasm* or prime *emerald* is green, nearly of the colour of stagnated water; sometimes tolerably clear, but, for the most part, full of black and white specks, and rather opaque.

The *jacinth* is of a deep tawny red, like very old *port* wine, but lively and transparent.

The *chrysolite* is of a light-green grass-colour, and is supposed to have been the beryl of the Ancients, transparent, but not lively.

The *crystal* or *oriental pebble* is harder and more lively than the common rock *crystal*; is of a silverish hue, and but very little inferior to the white *sapphire*.

The *garnet* is of the same colour as the *jacinth*, but more inclining to the purple, and not so lively.

The *amethyst* is of a deep purple, transparent and lively.

There were some other species of stones engraved upon by the *Romans*; but rarely before the latter times of the empire, when the art itself was greatly upon the decline.

All the before-mentioned sorts of stones are said to have been of the produce of *Egypt*, or of the *East-Indies*; and to have been brought from the borders of the *Nile*, or of the *Ganges*.

Here follows a general table of what are usually called Precious Stones.

The *beryl*, is red, yellow, or white.
The *plasm*, is green.
The *jacinth*, of a deep tawny red.
The *chrysolite*, of a light grass green.
The *crystal*, or *oriental pebble*, of a silverish white.
The *garnet*, of a deep red claret-colour.
The *amethyst*, purple.

The

The *diamond*, white.
The *ruby*, red or crimſon-colour'd.
The *emerald*, of a deep green.
The *aqua marina*, of a bluiſh ſea-green, like ſea-water.
The *topaz*, of a ripe citron yellow.
The *ſapphire*, of a deep ſky-blue, or of a ſilver white.
The *cornelian*, red or white.
The *opal*, white and changeable.
The *vermilion-ſtone*, is more tawny than the *jacinth*.

All theſe ſtones are more or leſs tranſparent: the following are all opaque:

The *cat's-eye*, brown.
The red *jaſper*, called alſo thick *cornelian*, is of the colour of red ochre.
The *jet*, black.
Agates, are of various ſorts.
The *blood-ſtone*, is green, veined or ſpotted with red and white.
The *onyx*, conſiſts of different parallel *ſtrata*, moſtly white and black.
The *ſardonyx*, of ſeveral ſhades of brown and white.
The *agat-onyx*, of two or more *ſtrata* of white, either opaque or tranſparent.
Alabaſter, different *ſtrata* of white and yellow, like the *agate-onyx*, but all opaque.
The *toad's-eye*, black.
The *turquoiſe*, of a yellowiſh blue inclining to green.
Lapis-lazuli, is of a fine deep blue.

Of moſt of the ſpecies before-mentioned there are ſome of an inferior claſs and beauty. Theſe are commonly called by Jewellers Occidental Stones: they are moſtly the produce of *Europe*, and found in mines or ſtone-quarries; and are ſo named, in oppoſition to thoſe of a higher claſs, which are always accounted oriental, and ſuppoſed to be only produced in the more eaſtern parts of our continent.

The *onyx*, *ſardonyx*, *agate-onyx*, *alabaſter* of two colours or *ſtrata*, as alſo certain ſhells of different coats, were frequently engraved by the Ancients in *relief*; and theſe ſorts of engravings are commonly called *Cameo's*. They alſo ſometimes ingrafted a head, or ſome other figure in *relief* of gold, upon a *blood-ſtone*.

Beſides which there are ſome antiques, moſtly *cornelians*, that are covered with a *ſtratum* of white. This *ſtratum* has by ſome been looked upon as natural; but it was really a ſort of coat of enamel that was laid on. This was uſed only in the times of the lower empire.

The ſtones eſteemed the beſt for engraving upon were the *onyx* and *ſardonyx*; and next to them the *beryl* and the *jacinth*.

The Ancients engraved moſt of their ſtones, except the *onyx* and *ſardonyx*, juſt as they were found; their natural poliſh excelling all that can be done by art; but the beauty of the ſeveral ſpecies of *onyx's* could only be diſcovered by cutting.

The merit both of *intaglio's* and *cameo's* depends on their erudition, on the goodneſs of the workmanſhip, and on the beauty of their poliſh.

The antique gems of *Greek* work are the moſt eſteemed; and next to them the *Roman* ones, in the times of the higher empire.

An account of certain perfect minute cryſtal ſtones; by J. Parſons, *M. D. &* *F. R. S.* N°. 476. p. 468. Apr. &c 1745. *Read* June 27. 1745. Fig. 50.

XV. The drawing here annexed, repreſents a ſmall cryſtal magnified; it is one of a great number brought by a very curious gentleman from *Gibraltar*, who has cauſed many of them to be ſet in buckles of different kinds, for the wear of his lady and himſelf: and although they are formed and poliſhed by nature; yet they look very bright, and produce a very good effect in the buckles.

They were found accidentally. This gentleman ſaw a man cleaving a rock near that town, and obſerved a great quantity of fine black powder fall from it's crevices; and, being very curious, he examined the powder, and found theſe little ſtones in cluſters, conſiſting of no more than 12 or 14 each; and each cluſter lying at conſiderable diſtances from one another. They are all of the ſame form, ſome leſs perfect than others, and are in general hexagonals.

The ſpecific Gravity *of* Diamonds; *in a letter from Mr* John Ellicot, *F. R. S. to the* Preſident. Ibid. *Read* July 4. 1745.

XVI. As, from ſome experiments I have lately had the opportunity of making, it appears highly probable, that what has formerly been publiſhed concerning the ſpecific gravity of diamonds, is not to be depended upon; I hope a ſhort account of theſe experiments will not be unacceptable to you, eſpecially as I do not find the leaſt notice taken of the ſpecific gravity of diamonds in any of the tables publiſhed in the *Philoſophical Tranſactions*.

In the account the hon. Mr *Boyle* has given of diamonds *, he relates it "as the opinion of a famous and experienced cutter of diamonds, "that ſome rough diamonds were conſiderably heavier than others of "the ſame bigneſs, eſpecially if they were cloudy or foul; and Mr "*Boyle* mentions one that weighed 8 grains and $\frac{1}{16}$; which, being care- "fully weighed in water, according to the rules of hydroſtatics, pro- "ved to an equal bulk of that liquor, as $2\frac{12}{23}$ to 1; ſo that, as far as "could be judged by that experiment, a diamond weighs not thrice "ſo much as water." And yet, in this table of ſpecific gravities, that of a diamond is ſaid to be to water as 3400 to 1000, or as 3, 4. to 1; and therefore, according to theſe two accounts, there ſhould be ſome diamonds, whoſe ſpecific gravity ſhall differ nearly the $\frac{1}{8}$ from others; which I am perſuaded, is a much greater difference than could be ex-

* Pag. 83. Vol. V. new edition of Mr *Boyle*'s works in *folio*.

pected

pected in any bodies of the fame kind, or that which, on a more nice examination, will be found to be in diamonds.

The firft diamonds I had the opportunity of feeing weighed, were two very large ones from the *Brafils*, which were furnifhed by Mr *Chace*, a merchant in *Auftin-friers*: the fpecific gravities of thefe were found to be much greater than the heavieft of Mr *Boyle*'s, the one being to an equal bulk of water as 3518, and the other as 3521 to 1000, and the difference between them lefs than the $\frac{1}{1000}$ part. There were two fmaller *Brafil* diamonds weighed at the fame time, which indeed were not quite fo heavy as the former, the lighteft being but as 3501, the other as 3511; but, as thefe were of the fame kind, and comparatively fmall, I judged this difference could not be much depended on. Having therefore an opportunity fome time fince of a large parcel of *Eaft-India* diamonds, I chofe out 10, which, both in fhape and colour, and every other refpect, were as different from each other as poffible. Thefe being weighed in the fame fcales and water as the former, the lighteft proved to be as 3512, and the heavieft as 3525; the very near agreement of thefe laft with each other, and with the former, tho' weighed at about eight months diftance, makes it highly probable, that fo great a difference as appears from the place above-cited, and Mr *Boyle*'s table, is not to be found in any diamonds whatfoever, much lefs fo great a difference as appears between the lighteft of his and the heavieft of mine, being above $\frac{1}{7}$ of the whole.

I had never made any experiments myfelf, by which I could form a judgment, how much of the difference between thefe and former trials might arife from the different tempers and qualities of the waters ufed; warm water being lighter than cold, and pump-water generally heavier than river-water. But, taking it for granted, that all perfons who make fuch experiments ufe common and not mineral waters, and waters of the natural temper, and not heated defignedly, I am affured by a friend, who has made many careful trials for this particular purpofe, that the fpecific gravity of any body will not differ above $\frac{1}{200}$ at the moft, on account of the quality of the water and temper taken together; whereas the heavieft of Mr *Boyle*'s diamonds, as in his tables, differs from the lighteft of mine by above $\frac{1}{35}$ part, which is about fix times as much as $\frac{1}{200}$: and yet I can think of no other way of accounting for the reft of this difference; unlefs it fhould arife from the fmallnefs of the diamonds, or any defect in the inftruments with which his experiments were made.

The fcales in which thefe diamonds were weighed turned very fenfibly with the $\frac{1}{200}$ part of a grain; and as one of the diamonds weighed above 92 grains, it was capable of being weighed to lefs than the 18000th part: feveral of them were weighed twice over both in water and air, and the weights found to agree to the greateft exactnefs; and if to this is added the very near agreement of the weights of the feveral diamonds, though weighed at different times, and at a confiderable

diftance

diſtance from each other, I think it highly improbable, that there could be any conſiderable miſtake in theſe trials; and therefore their ſpecific gravities, as in the following table, may fully be depended on.

I have ſet down the weights of the ſeveral diamonds both in air and water, that if any miſtake ſhould have happened, it may be the more eaſily rectified.

N°.		In Air	In Water	Specif. Grav.
	Water	. .	. .	1000
		Grains	Grains	
1	A *Brazil* diamond, fine water, rough coat	92,425	66,16	3518
2	A *Brazil* diamond, fine water, rough coat	88,21	63,16	3521
3	Ditto. fine bright coat, ...	10,025	7,170	3511
4	Ditto. fine bright coat, ...	9,560	6,830	3501
5	An *Eaſt-India* diamond, pale blue, .	26.485	18,945	3512
6	Ditto. bright yellow, ...	23,33	16,71	3524
7	Ditto. very fine water, bright coat, .	20,66	14,8	3525
8	Ditto. very bad water, honeycomb coat,	20,38	14,59	3519
9	Ditto. very hard blueiſh caſt . . .	22,5	16,1	3515
10	Ditto. very ſoft, good water, . . .	22,615	16,2	3525
11	Ditto. a large red foul in it, . . .	25,48	18,23	3514
12	Ditto. ſoft bad water, ...	29,525	21,140	3521
13	Ditto. ſoft brown coat, ...	26,535	18,99	3516
14	Ditto. very deep green coat, . . .	25,25	18,08	3521

The mean ſpecific gravity of the *Brazil* diamonds appears to be 3513.
The mean of the *Eaſt-India* diamonds . . . . 3519.
The mean of both to be . . . . 3517.

An extract, by Phil. Hen. Zollman, *Eſq; F. R. S. of a* Philoſ. Account *of a new opinion concerning the origin of* Petrifactions *found in the* Earth, *which has been hitherto aſcribed to the univerſal Deluge; as contained in*

XVII. The *Italian* author has adopted a new ſyſtem concerning marine petrifaction, the cauſe of which he refers to fire, inſtead of water, according to the opinion commonly received.

The place of his abode has furniſhed him with particular opportunities of comparing marine petrifactions found in the mountains, with the true marine bodies produced by the ſea. The ſaid place is called *San Vito di Tagliamento*, 6 hours journey from *Venice*, under the Biſhop of *Concordia*, belonging to the patriarch of *Aquileia*.

The author is a clergyman; but never entered into any eccleſiaſtical community, nor into any univerſity as profeſſor; to be out of the way of envy: however he keeps a boarding-ſchool for young men. He has publiſhed the book at his own expence; which has brought him into ſome trouble, and rendered the book at firſt very ſcarce. He ſhews a great conformity to the principles of Sir *Iſaac Newton*, and other modern

dern Philosophers, not very common in *Italy*, grounding himself upon experience, and mathematical proofs.

an Italian *book, intitled,* De Crostacei ed altri marini Corpi che se trovano su' Monti, di *Anton. Lazzaro Moro,* Venice 1740. *communicated together with several remarks: by Dr* Belthasar Ehrhart, *physician in ordinary at* Memmingen, *and member of the Acad* Nat. Curios. *in* High Dutch at Memmingen, 1745. *4to.* N°. 479. p. 163. Mar. & Apr. 1746. *Read* April 24. 1746.

Having in the first part formed the state of the question, he examines the systems of *Burnet* and *Woodward*, almost generally received by the learned, though the former does not make any express mention of petrifaction. He refutes their opinions about the Deluge, and of it's being the cause of petrifactions. He lays down for a fundamental maxim, that the Deluge ought to be believed, according to the Scripture, as a miracle, and not to be proved by natural rules; from which he proceeds to another; *viz.* That whoever lays down, for a foundation, a principle which does not fit the several *phænomena*, builds upon an erroneous principle.

After having refuted at large Dr *Woodward*'s opinions, he proceeds to the establishing his own system, grounded upon subterraneous fire, with various arguments of his own, and with the refutation of those of others.

He first lays down some general principles, according to Sir *Isaac Newton*, *&c.* and then applies to them several instances for supporting his system.

The first is the new island risen out of the sea in the year 1707, near the island *Santorini* in the *Archipelago*.

The second is a mountain, which rose out of the earth in 1538. near *Pozzuolo* in the kingdom of *Naples*, overwhelmed the little town *Tripergula*, and dried up a navigable lake named *Lucrano*; being now called the *New Mountain*, equal in height to a neighbouring old one, called *Monte Barbaro*. From the circumstances attending those events, he endeavours to prove his new hypothesis. He calls to help the several eruptions of the mountains *Vesuvius* and *Ætna*; and then forms his thesis; *viz.* "That marine animals and productions (for instance, "shells, *&c.*) which are now found in high mountains, were first generated in the sea: but when those mountains were raised, by subterraneous fire, above the surface of the sea, were petrified so as they "now appear."

This thesis *Moro* endeavours to support, by giving the detail of the 12 several *strata* found in the territories of *Modena*, when they are digging for wells, mentioned by *Woodward*, *Camerarius*, *Vallisnieri*, and *Ramazzini*; whose remarks, as well as the newer ones of *Whiston* and *Bourguet*, he will not allow to be satisfactory; the greatest difficulty being this, that, from the nature of some of those *strata*, it seems that the sea has twice covered the plain of *Modena*, now above some hundreds of feet above the level of the sea; and that from another *stratum* it may be inferred, that, in the intervals between those overflowings of the sea, the land has been inhabited and cultivated. His thesis he endeavours to support, by a remarkable passage from *Pliny*, *book* II. *chap.* 87. *Ingens terrarum portentum L. Marcio, Sex. Julio Coss. in agro Mutinensi!*

Mutinensi! Namque montes duo inter se concurrerunt, crepitu maximo assultantes, recedentesque; inter eos flamma fumoque in cœlum exeunte, &c.

Dr *Ehrhart* compares with this the several *strata* found in digging in the neighbourhood of *Memmingen* last year.

Moro touches next upon the hypothesis of some, that the sea increases about one foot in height in about two centuries; and of some others, that it decreases 5 feet in one century; as also, how the saltness of the sea may be deduced from his hypothesis.

Dr *Ehrhart* hopes that *Moro*'s system may one time prevail against prejudices, as well as those of *Vergilius*, *Galilæus*, *Harvey*, &c.

A letter from Mr James Simon, *of* Dublin, *to* M. Folkes, *Esq*; *P. R. S. concerning the* Petrifactions *of* Lough-Neagh *in* Ireland: *to which is annexed a letter from the R. Rev. Dr* Geo. Berkeley, *L. Bishop of* Cloyne *to* Tho. Prior, *Esq*; N°. 481. p. 305. Oct. &c. 1746. *Read* Dec. 18. 1746.

XVIII. Most of the antient writers, that have treated of *Ireland*, have made mention of the peculiar qualities of *Lough-Neagh* of turning wood into stone; some of them (*a*) have gone so far as to say, that it would turn that part of the wood which was in the mud into iron; the part in the water into stone, whilst the part above water remained wood.

Some later writers, particularly Messieurs *William Molyneux*, *Francis Nevill*, and *Edward Smyth*, and from them the late learned Dr *Woodward*, (*b*) the author of the notes on *Varenius*'s Geography, and others (*c*), seem rather to think, that this petrifying quality doth not lie so much in the lake itself, as in the ground near or about it.

Mr *Edw Smyth* (*d*), who inlarges the most on this subject, and seems to have led the others, and drawn them into his opinion, tells you, "That no experiment or observation yet made, that he could hear of, "could prove that this lough has really the quality of petrifying wood, "or that the water doth any way help or promote the petrification." He there gives you an example of a gentleman of worth and credit, "who had fixed two stakes of holly in two different places of the lough, "near that place where the *Upper Bann* enters into it, and that the "parts of the stakes which had been washed by the water for about 19 "years, yet remained there without any alteration, or the least advance "to petrification."

Another reason for his doubting of this quality is, "That tho' it is "reported that the water hath this virtue, especially where the *black-water* discharges itself into the lake, yet that, as it seems evident, "from the nature of liquid bodies, that any virtue received in one part "must necessarily be diffused thro' the whole, at least in some degree; "therefore (saith he) there is good reason to believe, that the water is "wholly destitute of this petrifying quality:" But a few lines lower he tells you (*e*), "That he had sufficient ground to conjecture, that

(*a*) *Boëtius* Hist. Gem & Lap.
(*b*) Catal. of *English* Fossils, Part II. p. 19.
(*c*) Sir *James Ware*'s Antiq. by *Walt Harris*, p. 227. Edit. 1745. folio.
(*d*) Afterwards Bishop of *Down*. *See* Phil. Trans. N°. 174.
(*e*) *Ibid. ut supra.*

"other

" other wood as well as holly had been petrified about this Lough; be-
" cause some fishermen, being tenants to a gentleman from whom he
" had this relation, told him, that they had found buried, in the *mud* of
" this *Lough*, great trees, with all their branches and roots petrified;
" and some of that bigness, that they believed they could scarcely be
" drawn by a team of oxen; that they had broke off several branches
" as big as a man's leg, and many bigger, but could not move the great
" trunk."

I suppose Mr *Smyth* (or the gentleman his friend) saw these branches, and was thereby convinced of their real petrification, as he was by the bulk of those trees of their being oak, and not holly; "because, says " he, no other tree in that country, these excepted, grows to that " prodigious bigness; at least it is certain, that holly never grows to " that bigness."

But how Mr *Smyth* came to be convinced, that these trees were oak, and not holly, and yet was not convinced of the petrific quality in some parts of the Lough, tho' these trees were found petrified in it's mud, is amazing to me: for, if a team of oxen could scarcely draw them from thence, it was as hard, in my opinion, to draw them from any adjacent ground (where they must have grown, lain, and been petrified) into the mud of the lake, where they were afterwards found: for it must be supposed, that either these trees grew on the banks of the lake, and, thro' age, or any other accident, fell into the water or mud, and were there petrified; or that, with great labour and expence, they were brought into it from some adjacent ground, after their actual petrification, which is hardly to be supposed.

Mr *Smyth* (*a*) tells you farther, that "Two gentlemen of the north
" (of *Ireland* where this Lough lies) had told him, that they had seen the
" same body, partly wood, and partly stone; but the only reason for
" thinking so, being the diversity of colours, which might well enough
" proceed from several degrees of petrification, we may properly think
" them deceived; for they made no experiment on that part which
" they reputed wood. The bark is never found petrified, as I am in-
" formed by a diligent inquirer; but often something rotten about the
" stone, answerable to the bark."

Mr *Smyth* I think contradicts himself no less in his last supposition than he did in the first. His friends assured him, that they had seen one or more of the *Lough-Neagh* stones partly wood and partly stone; but they were deceived, he says: the diversity of colours, by which they judged one part of the stone by it's colour to be wood, and the other part likewise, by it's colour different from the other, to be stone, were no more than different degrees of petrification. What are we to understand by these different degrees of petrification? by this something rotten about the stone often found? if not, that some part of the wood

(*a*) *Ibid. ut supra.*

was actually turned into ſtone, ſome other part in a degree leſs petrified, and ſome other part not petrified at all, as theſe gentlemen aſſured him: the diverſity of colours, ſeeing and feeling, was enough to convince them, and to determine the point.

As to his aſſertion, That, becauſe the water of this lake has not every-where, and in every place, that petreſcent virtue, it muſt therefore be a good reaſon to doubt of it's having that peculiar quality in ſome particular places, I think it may be denied for theſe reaſons; *1ſt.* Becauſe a ſpring, tho' ever ſo much impregnated with petrific, mineral, or metalline particles, iſſuing out in ſome particular place of the lake, can no more communicate it's petrifying virtue to the waters of the whole lake, than the river *Thames* it's ſweetneſs to the ſea, and make all it's water freſh.

Secondly, Becauſe that if this lapideſcent quality was equally diffuſed thro' the water of the whole lake in a degree ſufficient to turn a whole tree, or any of it's larger branches into ſtone, in all parts of the lake without exception, that petreſcent virtue muſt act equally on all the plants or vegetables whatſoever that grow in the lake, and upon all other bodies, gravel, ſand, mud, and clay, that are in, or are daily brought into it; and, at laſt, by a general aggregation, agglutination, and attraction of theſe different bodies together, the whole bottom of the lake, nay the whole lake itſelf, by the different degrees of coalition of particles, muſt become a ſolid body; unleſs you would ſuppoſe, that that this petrific quality has no power on any other matter but wood, which is contrary to experience, ruſhes, or other plants, having been found petrified on the ſhores of this lake, as alſo ſhells, clay, and ſand petrified in different ſhapes, of all which I have ſpecimens.

" The earth, ſays the great *Robert Boyle* (*a*) harbours different kinds " of petreſcent liquors, and many of them impregnated with one ſort " of mineral or other." There are no ſprings, no waters, but are more or leſs impregnated with ſuch mineral and ſaline particles; which appears from the moſt limpid; which, after evaporation, ſtill in the *reſiduum*, gives ſome particles of ſalt together with ſome ſtony and mineral ones.

I have found by experience, that petrifying ſprings are generally impregnated, ſome with calcarious and particles of other ſtones, and others with ferrugineous and vitriolic particles. Thoſe of the ſtony or calcarious kind, I have obſerved, when they drop on wood, or other vegetables, act on them for the moſt part by incruſtation, having different degrees and periods for their reſpective incruſtations and coalitions, which yet ſtick cloſe to one another: they ſeldom turn the wood into ſtone; but, ſticking to the wood, plants, *&c.* coagulate on it, and by degrees cover it with a cruſt of a whitiſh ſubſtance of different thickneſs, whereby the wood is immerged or wrapped in a ſtony coat, which,

(*a*) *R. Boyle*, of the origin and virtues of Gems.

if

if it be broken before the wood be rotten, you will find it in the heart of the ſtone or incruſtation, as is ſeen in thoſe petrifications at *Maudlin Meadows* in *Glouceſterſhire*, at *Hermitage* near *Dublin*, and many other places: or, if the wood be rotten, you will find a cavity in the ſtone, which very often is filled by a ſubſequent incruſtation or petrification; the ſtony particles then taking the place of the rotten wood.

Sometimes indeed, theſe waters, permeating the pores of the wood either longitudinally or tranſverſely, inſinuate themſelves therein, fill them up with their ſtony particles, ſwell, and, by their burning or corroding quality proceeding from the lime-ſtone, deſtroy the wood, and aſſume the ſhape of the plant, the place whereof they have taken.

Theſe kind of petrifications generally ferment with acids and ſpirit of vitriol, and, by calcination, may be reduced to lime.

Ferrugineous or metallic petrifying waters moſtly act by inſinuating their fineſt particles through the pores and veſſels of the wood, or other vegetables, without increaſing their bulk, or altering their texture, tho' they greatly increaſe their ſpecific gravity: and ſuch is the petrified wood found in or on the ſhores of *Lough-Neagh*; for it doth not ſhew any outward addition or coalition of forcing matter ſticking to or covering it (except in ſome places, where a thin ſlimy ſubſtance, taken notice of hereafter, is ſometimes obſerved), but preſerve the grain and *veſtigia* of wood; all the alteration is is in the weight and cloſeneſs, by the mineral particles pervading and filling the pores of the wood: theſe ſtones, or rather wood-ſtones, do not make the leaſt effervescence with ſpirit or oil of vitriol, nor *aqua fortis*; which ſhews, that they are impregnated with metalline particles, or ſtony ones, different from the calcarious kind, and may be the reaſon why the petrified wood, mentioned by *N. Grew* (*a*), made no ebullition, at which it ſeems he was ſurprized (*b*). Theſe ſtones I could not reduce into lime by the moſt intenſe fire, nor, with proper ingredients, procure a vitrification or fuſion (*c*).

Altho' mines have not perhaps been diſcovered near the *Lough*, I have reaſon to believe that there are ſuch in its's neighbourhood, from the great quantity of iron-ſtones found on it's ſhores, and places adjacent to it, and from the yellowiſh ochre and clay to be met with in many places near it. Of theſe iron-ſtones, which are very ponderous, outwardly of an ocheriſh yellow colour, and inwardly of a reddiſh brown, I have calcined many, and do find the powder of all to yield ſtrongly to the magnet.

(*a*) Reg. Soc. Muſ. p. 270.

(*b*) This contradicts an obſervation of Mr *John Beaumont* (*Phil. Tranſ.* N°. 129. p. 731.) That moſtly mineral ſtones will ſtir with acids; whereas all thoſe that I have tried, whether *Engliſh* or *Iriſh*, did not at all ſtir with acids.

(*c*) Stones of the calcarious kind turn to lime by calcination, and ferment with acids; but other kinds, ſuch as ſlate, fire-ſtone, free-ſtone, rag, grill, &c. will do neither, as experience has hitherto teſtified.

Gerald Boate (*a*) mentions an iron mine, in the county of *Tirone*, not far from the *Lough*, and ſuch others at the foot of *Slew-Gallen* mountains.

That mines are generated and found in the bowels of hills and mountains, is obvious to any that have the leaſt knowledge of Metallurgy; and that ſprings alſo proceed from mountains, is no leſs obvious; therefore ſhould a ſpring happen in the bowels of any of theſe mountains to run thro' a vein of mineral of any kind ſoever, it will waſh and dilute ſome parts of ſuch mineral, impregnate itſelf with the unctuous, ſaline, and metallic particles of ſuch mines, and convey them along with it's water; and if in it's way, whether under-ground, or at it's iſſuing out of the cliffs of a mountain, of the ſides of a river, or of the lake in queſtion; or whether it riſes under water, in the middle of ſuch a river or lake in any particular place, and in it's courſe meets with wood, vegetables, or any other lax bodies (lodged in the mud or gravel), whoſe pores, by the natural heat of the mineral ſteams, or any other accident, being open and duly prepared, theſe metallic *moleculæ* and ſaline particles will penetrate through, inſinuate and lodge themſelves in the pores and veſſels of ſuch wood, *&c.* fill them up, and, by degrees, turn them into ſtone; (*b*) "There being ſome of theſe *lapideſcent juices* of "ſo fine a ſubſtance, yet of ſo petrifying a virtue, that they will pene-"trate and petrify bodies of very different kinds, and yet ſcarce, if at "all, viſibly increaſe their bulk, or change their ſhape and colour."

That ſuch ſprings there are, hidden under the water or mud of this lake, I hope will appear probable, from what has been ſaid, and perhaps evident, from the account I have ſince received, that, in the great froſt of 1740, the lake was frozen over ſo as to bear men on horſeback, yet ſeveral circular ſpaces continued unfrozen. But how the ſeveral attempts, made, as mentioned, by Meſſieurs *Molineux*, *Nevil*, and *Smyth*, to procure wood half petrified (by fixing ſtakes of holly in the lake, which received no alteration) proved unſucceſsful, the reaſon I think is plain, becauſe they were not fixed in the proper place, *viz.* the courſe or vein of the ſpring, where nothing but chance could have directed them. This petrified wood is often found in different places on the ſhores of the *Lough*, but generally in greater plenty when the water has been diſturbed by great ſtorms; which makes it impoſſible to fix on the particular place where the petrifying juice moſt prevails; except a tree, or any large piece, ſhould be found ſo fixed as to reſiſt the force of the waves.

Mr *Smyth* (*c*) makes this further obſervation: "This virtue is cer-"tainly, if *not only*, in the ground or ſoil, he judges (ſays he) for theſe "reaſons; That there are many ſtones turned up daily, eſpecially at

(*a*) *Nat. Hiſt.* of *Ireland*, *Dub.* 1726.
(*b*) *Rob. Boyle*, of Gems, p. 124. 8*vo*.
(*c*) Philoſ. Tranſ. *ibid. ut ſupra.*

"their

" their breaking up new ground, which we cannot in any probability " think were brought thither; they are often found at two miles dif- " tance from the *Lough*, feldom farther, in great numbers, and very " deep in the ground; and a gentleman (on whofe credit I received " the information) faw a ftump of a tree digged out of the ground at a " fmall diftance from the *Lough*, which, by handling of it, he found to " be petrified. He affured me, the roots and all were ftone, and alto- " gether like thofe ftones that are ordinarily found, and go by the " name of *Lough-Neagh* ftones. This gentleman was of opinion, thefe " were *lapides fui generis*, till this obfervation convinced him: and that " thefe ftones were once wood, is, I think, very certain; for they fhew " the plain *veftigia* of wood; they likewife burn, and cleave: filings " of this ftone thrown into the fire emit a fragrant fmell; and they cut " kindly with a knife, tho' not fo eafily as other wood (*a*).

That this petrific quality is in fome peculiar parts of the lake, I have endeavoured to prove; that it is or may be in fome peculiar places of the adjacent ground, I grant; tho' as yet, I could not procure any of thofe ftones found in the ground, with wood continuous. Such as I have, or have feen, are of the white whetftone-kind, and feem to be holly or afh, petrified by fome ftrong nitrous and ftony particles; for, in a fo- lution of it in *aqua fortis* and oil of vitriol, it leaves no tincture, but the liquor growing muddy, like pipe-water after great rains, therefore fhews, that they are not fo ftrongly impregnated with metalline parti- cles, as thofe ftones found in or on the fhores of the lake.

I need not add any more, to fhew how mineral fprings may petrify wood, or any other vegetables under-ground; but as to whole trees found petrified and buried within a fmall diftance from the lake, I fhould think that the *Lough* might have been formerly broader than it is at prefent, or perhaps hath loft on one fide what it has gained on the other; by which means, what is now dry ground was formerly under water, and the other fide *vice verfa*: if fo, fuch trees as are found un- der-ground might have been petrified in that part which was over-flow- ed, and is now dry land.

Mineral fteams or exhalations, being highly faturated with ftony and mineral particles, are often found to have a petrifying virtue, as is feen at the bath called *Green Pillars* (*b*) in the city of *Buda* in *Hungary*. If fuch fteams fhould, in certain places, find or force their way thro' the fand or pores of the earth, they may operate on wood, *&c.* buried in the ground, permeate it's veffels, and, by degrees, turn it into ftone; and fuch, I apprehend, is the moft probable, if not the only reafon, that can be affigned for thofe petrifications of wood found in fand, as mentioned by *Boyle* and *Plot*.

(*a*) An anfwer to this, fee in the defcription of the city of *Down*, p. 162. The argu- ment is confuted by the defired proof.

(*b*) *Philof. Tranf.* N°. 59. p. 1049.

It may be obferved, that the finer the lapidific particles are, the more beautiful and natural the petrification will appear; fuch is a petrified root of the flag or *iris fylveftris* in my poffeffion, which is folid ftone at the bottom, the pith being turned into a white or fparry fubftance, and the growing knots of the root, tho' petrified, preferving their fkin brown, and fomewhat flexible. This *phænomenon* indeed has been lately folved in the defcription of the county of *Down*, *p.* 162. The *lufus naturæ*, or fportings of nature, is a general folution, too often brought in, and comes in very *à propos* to anfwer queries concerning petrifications, fuch as wood, fhells, worms, *&c.* If the fhells, or other like petrified bodies (found in marble or lime-ftone) which preferve the moft exact refemblance of the fifh or body they reprefent, were not formerly a real fifh, fhell, worm, *&c.* how comes it that fuch fhells and other bodies are found unpetrified in marble, lime-ftone, rock, marl, or any other ftone? The R. Rev. Dr *Robert Clayton*, Bifhop of *Clogher*, hath fhewn me in his collection a piece of *Italian* marble, where petrified fhells are feen, and others no way petrified, but that may be crumbled to duft with the fingers. I have a ftone (which I found lately in the river *Liffy* at *Chapel-Izod)* of the *lapis vermicularis* kind, the furface whereof, on one fide, is covered with a number of fmall petrified worms or plants; part of which, fticking and continuous to the ftony ones, are ftill foft and flexible.

To return to the *Lough-Neagh* petrifications; I received laft fummer, 1745. from the Rev. Mr *Richard Barton*, about 30 of thefe ftones, found on the fhores of the lake, fome in the water, fome in the mud, fome in the fand, and others in a yellowifh clay. That they were petrified in the lake is probable, but whether in the water, mud, fand, or clay, is no matter; for certain it is (to ufe Mr *Smyth*'s own words), that they were not brought hither from any diftance, fuch as 2, 4, 6, 8 miles, after being dug out of the ground, and then thrown and difperfed on the fhores of the lake: and befides, the difference in the colour of thefe ftones, thofe found in the lake, and thofe found in the ground fomewhat diftant from it, is fuch that they cannot well be miftaken one for the other. Thofe found in the ground are white, and of a loofer texture; thofe found in or on the fhores of the lake are black, clofer, and heavier. That thefe laft were petrified by a mineral fpring, appears from the few following obfervations. They do not ferment with acids, fpirit and oil of vitriol. The folution of this ftone in *aqua fortis* gives a beautiful red tincture, and in oil of vitriol leaves a tincture of a brown dark red. The woody part of thefe ftones in *aqua fortis* alfo gives a red tincture, tho' fomewhat paler; and, when taken out of the liquor, fhews red fpots in it's pores, which I take to be particles of iron and fulphur: thefe fpots, when the wood began to dry, became black; and the wood, when dry, turned of the colour of a deep red *Jefuit's-bark*.

In

In ſome of theſe ſtones, ſeveral curious veins, of a red and blueiſh colour, are very remarkable, being intermixed with black and white *ſtriæ*.

Having broken ſome of theſe ſtones, I found in the inſide a kind of white, and ſeveral cluſters of ſmall white and black angular cryſtals, which thro' the miſcroſcope appear tranſparent, and of different ſhapes, but moſtly hexagonal. I diſcovered ſuch cryſtals in ſome of the woody part of theſe ſtones.

One piece of a white ſtone I calcined in a crucible for 24 hours, but could neither reduce it to coal or lime. The powder yielded faintly to the magnet. This ſtone was found in the ground at ſome diſtance from the lake.

One piece of a black ſtone, found in the lake, I likewiſe calcined for 24 hours, and could not reduce it to coal or lime: the powder yielded briſkly to the magnet.

I calcined one piece of another ſtone, about one inch thick, for about 4 hours, in an intenſe fire, until it grew as red as it could be, when I took it out of the crucible. I obſerved ſeveral veins (not diſcernible before) of a ferrugineous matter, about $\frac{1}{16}$ of an inch thick, and when reduced to powder, it applied ſtrongly to the magnet.

In other ſtones I found ſome veins of wood, about one and two inches thick, no way petrified, though the ſtones were every way ſo outwardly.

Some of that woody part I alſo burnt in a crucible; it emitted a bluiſh flame, as if impregnated with ſulphur, and had the ſtrong ſmell of burning charcoal. When burnt to a coal, and reduced into powder, it faintly yielded to the magnet.

How wood happens to be found in theſe petrifications, ſound and untouched, is ſomewhat ſurprizing, and to account for it not very eaſy. It may be attributed to this, that the texture of the wood is not everywhere equal; eſpecially where knots happen, that part is much harder and cloſer than any other; and if the petreſcent particles ſhould be once ſtopped, they will fix there, coagulate, and go no further; by which means that part of the wood will remain free from petrification, while the reſt will be turned into ſtone; or the pores of the wood may happen in ſome places, and in the very heart of it, to be ſo full of a reſinous matter, that it will keep out the petrific juices, and hinder their further penetrating into them equally: to this may be attributed the ſtrong ſmell of this wood when burning; and the more ſo, as I ſuſpect that moſt of this petrified wood was fir, there being a good deal of that kind found daily in turf-pits near the lake; ſome not above 20 yards diſtance from it; and the laſt piece of wood and ſtone continuous that I have received, appearing by the grain to be of that kind of wood.

Laſtly, the petrific juices may happen to be ſo ſtrongly impregnated with ſalts and metal, or any other mineral particles, that they will immediately ſwell and fill the minuteſt pores of the wood, and, by a ſudden

den coalition, hinder their further penetrating into it; which ſeems to appear clearly from ſome cavities in one of theſe ſtones, which I ſuppoſe to have been worm-holes, and which were no way filled by the petreſcent liquor which was ſtopped round it; all the ſides of this hole being overlaid with ſmall brown cryſtals, occaſioned by the evaporation of the aqueous parts, and their being ſtopped and ſoaked by the neighbouring ſtone or wood.

The woody part of theſe ſtones, as I have obſerved, will burn to a coal, and emit a flame: that part intermediate betwixt the ſtone and wood, and which is but partly petrified, being harder than wood, and ſofter than ſtone, will grow red in the fire, emit a kind of flame, or rather ſparks of fire, but doth not conſume, and is properly what Dr *Grew* * calls incombuſtible wood. The ſtony part doth not burn, though it grows as red as coal.

I calcined another of theſe ſtones, weighing 1 *oz.* 13 *penywts.* $12\frac{1}{4}$ *gr.* after burning 4 hours it weighed but 1 *oz.* 10 *penywts.* $8\frac{1}{4}$ *gr.* and loſt 3 *penywts.* 4 *gr.*; which proceeds, I ſuppoſe, from unpetrified veins of wood in the heart of the ſtone, which were deſtroyed by the fire, as in the crucible it emitted now-and-then a blueiſh flame, as brandy doth when burning. This ſtone, when taken out of the crucible, and cooled, had the colour of iron, when heated in, and cooled from the forge.

Part of another ſtone, which, by viſible veins of ore, appears to contain a good deal of iron, I likewiſe calcined for 4 hours; the powder yielded moſt ſurprizingly to the magnet; ſo that it appears, that the opinion of *Nennius*, *Boëtius*, and other ancient writers, was not abſolutely deſtitute of foundation.

The white wood-ſtones are generally found in the ground at 2, 4, 6, and 8 miles diſtance from the lake, and ſometimes very deep in the earth.

The black ones are always found in the water, or on the ſhores of the Lough; ſometimes at the mouths of rivers or rivulets that empty themſelves into it; but thoſe with wood continuous have not yet been found above 20 yards diſtance from the water of the lake; that is, where the water reaches in the winter, or at other times.

Some of theſe ſtones are outwardly covered with a thin white ſubſtance, which hath run thro' the pores of that part of the ſtone that was expoſed to the air, and not covered by the water, mud, or clay; and on ſome others it is rather an incruſtation of that white ſubſtance, which I take to be the ſlimy, unctuous, ſaline parts of the petreſcent juices that filled the outward pores of the ſtone, or coagulated on it. This white part ſcraped, and put into a crucible in a violent fire, could not be reduced to lime, tho' it grew red as coal. This powder calcined appeared thro' the microſcope quadrangular, like grains of ſalt; which makes me ſuſpect, that theſe petrifications contain, beſides metalline, a great

* *Muſ. R. S.* p. 269.

deal

deal of saline particles, whose sides being strongly attracted to each other, and closely joined, hinders the fire from expanding the pores of these stones, and their being reduced to lime.

This black stone, when broken, appears thro' the microscope very beautiful, and like cloth of silver, the pores and vessels of the wood being filled with white minute crystals.

Of these stones I have some with wood outwardly continuous; others with wood inwardly; one, the least part whereof is stone, the rest wood; another *vice versa*; another intirely wood, except a thin coat of stone on one side, which appears to be the very bark; one stone which at one end distinctly shews the annual ringlets of the wood; one that shews the wood, before it was petrified, had been bent, and partly broken, the fissure being filled with a sparry matter, and appears plainly from the present appearance and position of the fibres of the stone. Some of these stones strike fire with a steel, and others, by a strong collision, emit a train of sparks.

Some of these stones shew the grain of holly, ash, and fir. I have but one piece of oak petrified, easily distinguished by it's grain; it shews the very knots of the wood where young twigs were cut; and has a hole made thro' it before it was petrified.

As for these stones being fit for sharpening or setting of razors, &c. the black ones are rather too hard, and the white ones too soft. The whet-stones or hones, vulgarly so called, which are sold for *Lough-Neagh* stones, are none of these, but of a soft gritty kind, and found near *Drogheda*.

When these stones with wood continuous are taken out of the water, mud, or clay, the woody part dries, cracks, and falls away; which is the reason why few can be well preserved; and besides, every body, unwilling to trust their eyes, will touch and scrape the wood, and by these means, destroy the most curious part of the stone.

The curious gentleman above-mentioned, who hath already begun, and intends, at his leisure, to take an accurate survey of the lake, will, I hope, be able to give a more just and satisfactory account of it's petrifying virtue than I possibly can; my design in the present attempt being only to pave the way, and induce others to make further experiments in search of truth, and for improving natural knowledge.

P. S. I had lent the above papers to the Bishop of *Cloyne*, from whom I received them yesterday, with the original letter to *Thomas Prior*, Esq; of which follows the copy.

Cloyne, May 20. 1746.

I Here send you back the curious Dissertation of Mr *Simon*, which I have perused with pleasure; and though variety of avocations gives me little time for remarks on a subject so much out of my way, I shall never-

A letter from the Right Rev. Dr George Berkeley, *L. Bishop of*

Cloyne, *to* Tho Prior, *Esq; in* Dublin.

nevertheless venture to give my thoughts briefly upon it, especially since the author hath been pleased to invite me to it by a letter.

The author seems to put it out of doubt, that there is a petrifying quality both in the lake and adjacent earth. What he remarks on the unfrozen spots in the lake is curious, and furnisheth a sufficient answer to those, who would deny any petrifying virtue to be in the water, from experiments not succeeding in some parts of it; since nothing but chance could have directed to the proper places, which, probably, were those unfrozen parts.

Stones have been thought by some to be organized vegetables, and to be produced from seed. To me it seems, that stones are vegetables unorganized. Other vegetables are nourished and grow by a solution of salt attracted into their tubes or vessels. And stones grow by the accretion of salts, which often shoot into angular and regular figures. This appears in the formation of crystals on the *Alps:* and that stones are formed by the simple attraction and accretion of salts, appears in the tartar on the inside of a claret-vessel, and especially in the formation of a stone in the human body.

The air is in many places impregnated with such salts. I have seen at *Agrigentum* in *Sicily* the pillars of stone in an ancient temple corroded and consumed by the air, while the shells which entered the composition of the stone remained intire and untouched.

I have elsewhere observed marble to be consumed in the same manner; and it is common to see softer kinds of stone moulder and dissolve merely by the air acting as a *menstruum*. Therefore the air may be presumed to contain many such salts, or stony particles.

Air, acting as a *menstruum* in the cavities of the earth, may become saturated (in like manner as above ground) with such salts, as, ascending in vapours or exhalations, may petrify wood, whether lying in the ground adjacent, or in the bottom of the lake. This is confirmed by the author's own remark on the bath called the *Green Pillars* in *Hungary*. The insinuating of such salts into the wood seems also confirmed by the author's having observed minute hexagonal crystals in the woody part of the petrifactions of *Lough-Neagh*.

A petrifying quality or virtue shews itself in all parts of this terraqueous globe, in water, earth, and sand; in *Tartary* for instace, and *Afric*, in the bodies of most sorts of animals, it is even known that a child hath been petrified in the mother's womb. *Osteocolla* grows in the land, and coral in the sea. Grottoes, springs, lakes, and rivers, are in many parts remarkable for this same quality. No man therefore can question the possibility of such a thing as petrified wood; tho' perhaps the petrifying quality might not be originally in the earth or water, but in the vapour or steam impregnated with saline or stony particles.

Perhaps the petrification of wood may receive some light from considering amber, which is dug up in the king of *Prussia*'s dominions.

I have

I have written these hasty lines in no small hurry; and send them to you, not from an opinion, that they contain any thing worth imparting, but merely in compliance with your and Mr *Simon*'s request.

And yet, before I have done, I must needs add another remark, which may be useful for the better understanding of the nature of stone. In the vulgar definition, it is said to be a fossil incapable of fusion. I have nevertheless known stone to be melted, and when cold to become stone again. Such is that stuff, by the natives called *Sciara*, which runs down in liquid burning torrents from the *craters* of mount *Ætna*, and which, when cold and hard, I have seen hewed and employed at *Catania*, and other places adjacent. It probably contains mineral and metallic particles; being a ponderous, hard, grey stone, used for the most part in the basements and coinage of buildings. *Added from a letter to Dr* J. Fothergill, Dublin, Aug. 8. 1746.

Hence it should seem not impossible for stone to be cast or run into the shape of columns *, vases, statues, or *relievo's*; which experiment may perhaps, some time or other, be attempted by the curious; who, following where nature has shewn the way, may (possibly by the aid of certain salts and minerals) arrive at a method for melting and running stone, both to their own profit, and that of the public.

XIX. It was found in a cavern, that was discovered amidst the vast marble rocks at *Cat-down* near *Plymouth*. It hung perpendicularly from the top of the rocky cavern, and was a cylindrical tube of 20 inches long at least; but was unluckily broken into several pieces in bringing to me. This I have sent was by much the longest of them; but Mr *Long* (the master of the quarries) assured me the whole was above 20 inches long, and quite cylindrical, and quite hollow. I went to the cave the next day, and found 5 or 6 of such kind of tubes, but none above 2 inches long. They all sprang from a broad, hollow, protuberating basis, in some sort as a nipple arises from the breast. These also were cylindrical and hollow. There were in the same cavern many other petrifications, which had formed a kind of hollow pilasters against it's sides; and also several large solid masses, which arose from the continual dropping of the petrefying water through the crevices of the superior rock. These all afford very good alabaster. *An account of a beautiful* Stalactites *now in the* Museum *of the* R. S. *by* John Huxham, *M. D. F. R. S. in a letter to Dr* Mortimer. N°. 474. p. 207. June &c. 1744. *Read* Dec. 13. 1744.

XX. The *belemnites* is a fossil of different magnitudes and colours, ever regular in shape, which is either cylindric, conic, or thereunto approaching. Numbers of them have, on one side only, a chap or seam running their whole length; others have it in part; and in others it is not at all to be observed: it consists of a talcy matter, with an intermixture of spar or crystal, disposed in *striæ* from or near it's centre to it's *A dissertation on those fossil figured stones called* Belemnites; *in a letter from Mr* Emanuel Mendez da Costa *to* Martin Folkes, *Esq; Pr. R. S.* N°. 482. p.

* To confirm what the Bishop says, I remember when I was in the college in *France*, that I went to see a relation of mine, a frier, at *Fontevraud*, where he shewed me in their church two pillars of stone, about 60 feet high, all of one solid piece, which he said had been run. *J. S.*

397. Jan. & Feb. 1747. *Read* Jan. 29. 1746-7.

it's circumference, and is made up of crusts inclosing each other, the innermost whereof is as regular as the outermost. Sometimes, though seldom, in comparison to the numbers of the *belemnites*, in the centre is a cavity ever conic, whatever the external shape of the *belemnites* be. This conic cavity is at different times empty, or else filled, either with a solid body of mineral matter, crystal, stone, *pyrites*, *&c.* or with a regular-jointed conic body, called by Lithologists the *Alveolus* of the *Belemnites*; which, though constantly regular and jointed, is nevertheless found composed of various mineral or metallic substances.

The *alveolus* above-mentioned, tho' not fully proved such, yet seems, by the assent of most of the present Naturalists, to be a body of marine origin; a shell the nighest related to the *nautilus* kind: it is concamerated, and even in some is discovered another great characteristic of the *nautilus* kind, I mean the gut or *siphunculus*. Therefore, taking this body for granted to be of marine origin (for what reasons, or of what kind, is not my present intended subject to prove) it remains to discuss, Whether this body became accidentally lodged in the *belemnites?* or, Whether the *belemnites* itself is also of marine origin, and a part dependent on it's *alveolus?*

Various have been the opinions of Lithologists concerning the origin of the *belemnites*; some have even asserted them of the vegetable kingdom; others, that they are teeth or horns of fish, appendages of shells, bodies cast in shells of the *tubuli* kind, or the very shells themselves, spines of *echini*, or a kind of strait *nautilus*. The three last opinions are what I shall strive to confute, as they seem somewhat probable, and are now the most prevailing; and prove the *belemnites* to be a natural fossil or *lapis sui generis*. I desire no recourse to the subterfuges used by others, but hope you will agree with me in the axiom, that all *belemnites* are of one and the same origin.

That the *belemnites* are not teeth or horns of fish, I shall refer you to the letter Dr *J. Woodward* wrote on that subject to Mr *Bourguet*, of *Switzerland*, wherein he fully proves the erroneousness of those opinions. But a further argument against their being teeth, which that learned Naturalist has not touched upon, is, that no *belemnites* have that natural varnish or polish, which always covers the teeth of all animals; whereas the greatest part of those fossil bodies, which we know to be such, as the *bufonitæ*, *glossopetræ*, *&c.* are found with that same varnish or polish. As for their owing their form to being moulded in shells, it will appear contradictory to reason, when we consider, 1. Their constitution to be ever as regular as their figure; and, 2. That their inner layer or *nucleus* is as equally regular as the outer crust or whole body; which particular could never have happened, had they been moulded in shells; as is evident, by the *turbinitæ*, *conchitæ*, and other bodies, which owe their figures to that cause. That the *belemnites* are not spines of *echini*, let us first consider, that no kinds hitherto discovered have been ever found to have spines analogous to these bodies; nor indeed has any

marine

marine ſhell whatever ſuch a texture. The immediate ſubterfuge for an anſwer to this objection is, that the kinds of ſhells unknown to mankind are far more in number than thoſe yet diſcovered. I allow it; but think that cannot be an argument in the preſent caſe, ſince no one ſingle ſpecies is yet diſcovered with ſuch, nor even any *genus*, which have ſpines analogous to the *belemnites*. Nature bears an analogy through all her works; and though all the ſpecies of any one *genus* is not known to any man, yet that analogy nevertheleſs capacitates us to judge of thoſe undiſcovered by thoſe we know. Thus we find of the *echinus* kind, all the ſpecies now known are ever found near the ſhores; conſequently, are not ſubject to be eternally hidden from us; as is undoubtedly the caſe of the *cornua ammonis*, and *conchæ anomiæ*. They are no pelagian ſhells, as thoſe are; bays and harbours are the places where they are fiſhed; their ſtructure even evinces the reaſons for it. We may therefore with probability conclude, that all the *echinus* kind are of the ſame nature, and have the ſame way of living; that they only inhabit ſuch places, and that none are pelagian ſhells; conſequently might have been diſcovered.

I am ſenſible there are ſome ſpecies of foſſil *echini*; as, the moſt common *conoid* or *pileated echini*, the common *echini galeati*, the *echini clypeati*, and ſome kinds of the *echini ovarii*, &c. which though we are certain that they have been marine ſhells, yet thoſe particular ſpecies are not known in the ſea: but then ſeveral other ſpecies of that ſame *genus* are. The caſe of this is quite different, ſince not one ſingle ſpecies of ſuch a *genus* has ever been found.

The exceſſive bigneſs and thickneſs of numbers of *belemnites* deſcribed by authors, *viz.* of near 2 feet in length, and above 2 inches in diameter in the thickeſt part, others of 3 feet long, and others as thick and long as a man's arm; not to enumerate thoſe only under a foot length, and of proportionable thickneſſes, concludes *echini* of a vaſt bigneſs, to have a number of ſuch ſpines to move.

The varieties of the *belemnites*, how can they quadrate to the ſpines of one *genus* of *echini* only? ſolid *belemnites*, *belemnites* with a ſingle cruſt, or like a tube, with a conic cavity only; that empty, or otherwiſe filled with a ſolid maſs, or with a regular-jointed body, as the *alveolus*, *belemnites* of various magnitudes and thickneſſes, *&c.* can all theſe varieties be imagined to belong to one *genus* of ſhells, which we ſuppoſe to exiſt to maintain a favourite ſyſtem?

The number of ſpecies of *echini* diſcovered is great; and the ſpines of all thoſe agree in having a hollow *axis*, which runs proportionably from their *baſis* to their *apex*, quite different to the *belemnites*: and for their conſtitution, a foreign Naturaliſt, a Member of the *R. S.* Mr *Klein* of *Dantzick*, who has profeſſedly written on this ſubject, could only find of 2 kinds, *viz.* thoſe of a porous conſtitution, which he obſerved only to belong to one *genus*; and thoſe of a ſolid ſhattery ſubſtance, like

like a talcy ſpar not ſtriated; which is the moſt general, and is exactly the ſame conſtitution as all the foſſil ſpines, or *lapides Judaici* are.

Further, the *Lapides Judaici* have, at ſome times, been found adhering to their *papillæ* or tubercles, and with fragments of their ſhells; whereas no Naturaliſt has ever known to be found foſſil either the ſhells, or the fragments of ſuch a *genus* of *echinus*; not even any remains proportionable to ſuch large ſpines. In whatever manner the greater part of ſuch ſhells may have periſhed (which is unlikely, if we conſider their texture and ſtrength), ſome muſt have eſcaped, when the ſpines are found in ſuch exceſſive numbers every-where, and always perfect and regular; whereas the foſſil ſpines, or *Lapides Judaici*, as they are called, as likewiſe the *echini* or ſhells, and all the foſſil bodies of marine origin, are found broken and ſhattered in all kinds of manners.

As for their being ſhells of the *tubuli* kind, my reaſons againſt it are; Were the *belemnites* ſuch, they muſt be all tubular more or leſs; or otherwiſe muſt have ſuffered ſome degree of petrifaction to fill up their cavities. The unreaſonableneſs of that argument is demonſtrated by all *belemnitæ* being of one and the ſame texture and conſtitution; though numbers are ſolid, and numbers are tubular, in different degrees. Now one kind of petrifaction, or any other change in the earth, which they might have undergone, could never have given ſo regular a texture and ſubſtance, and cauſe ſuch different effects as ſolidity and tubularity. And if, on the other hand, we allow it to be inconſiſtent, as it is, to form the idea of a ſhell of the *tubulus* kind, by a ſolid body, without that body having ſuffered ſome change in the earth, while buried in it, we muſt either deny all ſolid *belemnites* to be ſuch *tubuli*, and run to ſubterfuges, by owning them to be natural foſſils; or elſe allow a great inconſiſtency, to uphold a wrong ſyſtem.

That the *belemnites* are not a tubular caſe, which is part of, and covers a ſhell of the *nautilus* kind, as is it's *alveolus*. The variety of circumſtances already alledged of the *belemnites* ſerve to demonſtrate the improbability alſo of this opinion, as it has done of the other two. The numbers of *belemnites* of all kinds, ſo plentiful every-where, and the conſideration of how few are furniſhed with *alveoli*.

Numbers, I am ſenſible, have conic cavities; but that thoſe cavities never did contain *alveoli*, is evident; that the ſides of the ſaid cavities are even, and without any circular or other impreſſions, which a *belemnites* that has ever contained an *alveolus* muſt have; that body being in cloſe contact to all parts of the inveſtient *belemnites*, muſt conſequently impreſs it with it's concamerations; which impreſſions muſt be therefore found on the ſides of the cavities of all *belemnites* which ever contained them.

As for aſſerting, that all the *alveoli*, which are now found looſe, were originally lodged in *belemnites*, it cannot be; without inferring alſo, that all *belemnites* which are now devoid of *alveoli*, contained ſuch formerly;

merly; which, by some external or other agent, have been forced out and loosened from them.

To consider such an agent, we must also conclude it's force to have been exceeding great, to loosen out the *nucleus* of a body in close contact with all it's investient parts; and strengthened further to it by ridges and grooves, such a force must have compressed, shattered, and otherwise broken and destroyed the *belemnites* that contained them; which is contrary to observation. Further, forcing out the *alveolus* might perhaps easily have happened to the conic *belemnites*; which hath a basis of a larger diameter than the middle, where the *alveolus* is lodged; but we cannot conceive the same by the cylindric, fusiform, and other *belemnites*, of which the two ends or extremes terminate pointed; while the middle, where the *alveolus* is lodged, is thick and swollen.

To force an *alveolus* out of such shaped *belemnites*, it is evident, that the narrow ends of the said *belemnites* must be quite forced open, broken, and shattered, before a broader and more capacious body could be forced through, especially to such a brittle shattery fossil as the *belemnites* is. The evident facts to the contrary of this are also too common to insist on, since all these *belemnites* are ever found regular, perfect, and intire.

Further, let us consider the *alveoli* which are now found in *belemnites*, they are very seldom if ever found as mere shells, but ever differently changed or petrified. They are moulded of stone, *pyrites*, crystal, *&c.* Now it can never be argued, that the contained bodies can ever be so differently changed or petrified in their covers or shells, and those covers or shells which admitted such different petrifying particles to undergo no change or petrification whatsoever.

Another proof against this opinion, is the diverse forms of *alveoli* now discovered by Naturalists, as conic, cylindric, curved, spiral at the *apex*, &c. whereas all *belemnites* which have cavities have none but conic ones.

These cylindric, *&c. alveoli* are now found in *Pomerellia* in *Poland*, in the marble of the island of *Oeland* in the *Baltick Sea* belonging to *Sweden*, and in the marble of *Sweden*; in *Gothland* in masses of building-stone; in *Ingria*, in several parts of *Prussia*, *&c.* and are commonly of an immense bigness, to several feet in length, and proportionably thick, yet not perfect. For such *alveoli*, which are only *nuclei*, we must suppose immense large *belemnites*; and such we have never heard of, so with probability we may conclude none such to exist.

I do not doubt the growth of this error, of the *belemnites* being a part of it's *alveolus*, to have been caused by too rash conclusions, and too little an insight into the mineral kingdom; which has propagated that assertion of the *alveoli* being found only in the *belemnites*; which experience daily contradicts, since we find them loose, as well as imbedded in many other fossil substances, as in marble, stone, *&c.* as has been above observed.

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Theſe are the arguments which I alledge for the improbability of the ſaid opinions. I could advance a number of other proofs; but as I have already extended my letter beyond a due length, I beg leave, before I conclude, only to offer ſome few reaſons for their being a natural foſſil, or *lapis ſui generis*.

The very view of a *belemnites* ſufficiently evinces it's mineral origin, and ſhews it evidently compoſed of two foſſil ſubſtances, a talc, and a ſpar, or baſtard cryſtal; whereof the former is the baſis, and from which principle I do not heſitate to attribute it's ſtriated texture. Moſt of the talcy bodies are of a fibrous nature, and ſeveral are compoſed of cruſts incloſing each other, in the ſame manner as the *ſepta* of the *ludus Helmontii*, ſome of the *aſbeſtos* kind, the *Hæmatites* cruſts, &c. Of the *ſtalactites* tribe there are ſeveral, which ſo intirely approach the texture and conſtitution of the *belemnites*, that were their ſhapes a little more regular, the moſt experienced Lithologiſt might eaſily be deceived: and I remember, when abroad, to have ſeen ſuch, of a prodigious bigneſs, which, though I was then ſomewhat converſant in the foſſil ſtudy, I could not help taking for *belemnites*. I do not therefore wonder, that *Petrus Aſſaltus, in notis ad Metallothecam Mercati*, p. 282. and *Langius, Hiſt. Lap. figurat. Helvetiæ*, p. 133. ſhould judge them a native figured foſſil, formed in the earth, of the *ſtalactites* kind, if that term for the *belemnites* might with propriety be uſed.

The cavities of *ſtalactites* in ſome meaſure illuſtrate, and are adequate to the cavities of *belemnites*; they are placed in as various poſitions, and are only different from them by not being exactly conic. As for the regular figure of the *belemnites* being excepted againſt, I believe few Foſſiliſts will argument that, when we ſee as perfect regular figures in the mineral kingdom as in any other parts of the creation; as witneſs the ſalts and cryſtals of all kinds; the rhomboid, hexagonal, columnar, and other *ſelenites*; the cubic, octangular, dodecaedral, and other *pyrites*; the quadrangular pyramids of tin, the rhombs of iron, cubes of lead, and infinite other native foſſils, which would take up time to enumerate, and which are far more perfect figures than the *belemnites* are. Chemical and other trials and teſts (which I hope to lay before you in ſome future letter) demonſtrate a greater certainty of its mineral origin. As for that marine body the *alveolus*, I cannot think otherwiſe than that it is of the *nautilus* kind; and which, at the concretion or formation of the *belemnites*, became accidentally lodged in it's cavity, in the ſame manner as all other marine bodies became lodged in the various foſſil ſubſtances we now find them in.

Some remarks on the precious ſtone called the Turquoiſe;

XXI. This ſtone has received it's modern name of *turcheſia*, and *turquoiſe*, from it's being moſt commonly brought from *Turky* into various parts of *Europe*. *De Boodt* * ſays, the colour of this gem is a variegation of green, white, and blue; and that there are two ſorts of

* Gemmar. & Lap. Hiſt.

it,

it, the oriental, from the *Eaſt Indies* and *Perſia*, and the occidental, from *Spain*, *Germany*, *Bohemia*, *Sileſia*, *&c.* that in *Perſia*, where it is found in greateſt plenty, adheres to black ſtones, as if it were an excrement or a tranſudation from them. A ſtone of this ſort is ſeldom found to exceed a walnut in ſize; and he mentions one in the Great Duke's *Muſeum*, on which the head of *Julius Cæſar* is engraven as a very extraordinary ſample: he adds, that he never ſaw one bigger than an hazel-nut; that ſome of the oriental ones have the faculty of preſerving their colour perpetually, which are called Stones of the old Rock; and that others loſe their colour gradually, and are called of the new Rock. He then gives an inſtance of a turquoiſe which had loſt it's colour upon being laid by ſome time after it's owner's death, which recovered it's beautiful colour upon our author's wearing it upon his finger in a ring.

by Cromwell Mortimer, *Sec. R. S. &c.* Ibid. p. 429. *Read* Feb. 26. 1746-7.

Cæſius, in his Treatiſe *de Mineralibus*, p. 601. ſays, This ſtone is called *Turcoïs* by *Mylius*, in his *Baſilica Chemica*; by *Albertus Magnus*, in his Treatiſe of Minerals; and by *Rueius*, in his Treatiſe of Gems: but *Turca*, by *Cauſſinus de Lapillis Symbolicis*. *De Boodt*, and Dr *Woodward*, * with other modern writers, take it for the *Callaïs* of *Pliny*. *Salmaſius*, in his *Plinian. Exercit.* p. 142. ſays, Many have miſtaken the modern *turquoiſe* for the *cyanus*, but that the *cyanus* was tranſparent like the ſapphire; whereas the *turquoiſe* is a ſort of jaſper.

Dr *Woodward*, in his Letter to Sir *Jo. Hoſkyns*, † ſays, That the *turcoïs*, or *callaïs* of *Pliny*, is nothing elſe but foſſil ivory tinged with copper. I do not deny, that ſome ſtones ſold for *turquoiſe*, and poſſibly all that the Doctor ſaw were certainly ſuch; but I imagine thoſe which the authors call of the old Rock, and in which the colour is permanent, are real mineral ſtones: this ſample now before us ſeems to ſhew this, from both the form and ſize: it's ſhape ſhews it not to be part of any animal bone; but it's botryoïd form is to me a demonſtration that it is the product of fire, which had once melted this ſubſtance; and that when it cooled, it's ſurface was formed into bubbles and bliſters, in the ſame manner as the *hæmatitis botryoïdes* or blood-ſtone, whoſe ſurface conſiſts of knobs, reſembling a bunch of grapes.

That the *elephas* ὀρυκτὸς, or *ebur foſſile* of *Theophraſtus* ‖, ſaid to be of various colours, I do not in the leaſt deny to be tinctured with copper, and to be what Dr *Woodward* calls the *turquoiſe*: indeed I ſuſpect it to be what *De Boodt* calls of the new Rock; and ſays is liable to loſe it's colour, which it recovers again from the *effluvia* of the perſon who wears it. I therefore, for diſtinction ſake, think all theſe ſtones of the ivory origin ſhould be called *Pſeudo-Turcheſiæ*, or baſtard *Turquoiſe*;

* Method of *Foſſils*. Letters, *p.* 17. † Ibid. p. 16.

‖ See *Theophraſtus*'s Hiſt. of *Stones*, tranſlated, *&c.* *John Hill*, *Lond.* 1746. 8vo. p. 94.

 and

and the other ſort, of which this before us is one, the true or real *turquoiſe*; for, by examination in the chemical way, I find it to be a very rich copper ore; ſome of it pounded and diſſolved in ſpirit of hartſhorn gives a deep blue; in *aqua fortis* a fine green; and an iron wire put into it was in 1 hour's time incruſted with copper: ſome of it calcined, without any flux in a crucible, run to a ſlag, or half vitrified ſubſtance; whereas the ſame heat, had it been ivory or bone, would have reduced it to a white aſh like bone-aſhes; for I expoſed it to ſuch a fire as vitrified the tile that covered it. It's hardneſs and conſiſtence to an engraver's tool ſeems to be the ſame as common white marble: it's colour is not mended by heat, but it grows brittle when red hot.

This ſpecimen, now ſhewn to the *Society*, was about 12 inches long, 5 inches broad, and in ſome places near 2 inches thick; rough on the under ſide, as though broken off from the rock it had been affixed to; and the upper ſide was compoſed of ſmooth poliſhed knobs, in form like to the botryoïd iron ore.

Sir *Hans Sloane*, in his noble *Muſeum*, has ſeveral ſpecimens of theſe oriental *turquoiſes*, all botryoïd; eſpecially a maſs from *China*, about 3 inches long, 2 broad, and near an inch thick: all which ſeem to be copper ores: and he has likewiſe ſamples of *turquoiſes* from *Spain*, and the South of *France*; which are all ſmall, and ſeem really to be pieces of ivory tinged with copper.

A deſcription of a curious Echinites; *by Mr* Henry Baker, *F. R. S.* Ibid. p. 432. *Read* Feb. 26. 1746-7.

XXII. 1. Mr *Baker* takes the liberty of ſhewing the *Society* a very extraordinary *echinites*, the like to which he has never ſeen in any *Muſeum*, or found deſcribed by any author. For the *echenitæ* uſually met with, are made up either of chalk or flint, or ſome ſtony, chalky, or ſparry matter, formed within the ſhell of the *echinus*, and taking their figure thence as in a mould: which ſhell is oftentimes broken off and gone, but remains at other times impregnated with talcy or ſparry particles: whereas the ſubject now laid before us is compoſed of a tranſparent cryſtalline ſubſtance, which has received it's general figure by having been circumſcribed within the ſhell of ſome *echinus*, and ſhews linear ridges and diviſions correſpondent to the lines and plates found in this kind of *echinus*.

Fig. 51. Was this all, it would be a very uncommon production, as theſe bodies have been very rarely known to be formed of cryſtal *; but it is rendered much more curious and extraordinary, by having exact rows and ſeries of little cells, all of the ſame regular figure, though leſſening gradually in ſize, as they aſcend from the baſe upwards.

* Sir *Hans Sloane* has a maſs, which was formed within an *echinus*, the ſhell being broken off; it is one half or ſide cryſtal, the other ſide of a ſubſtance like chalk, but much harder.

This

This body having been formed within the ſhell of an *echinus*, one would expect (as is the caſe in all other *echinitæ* uſually known), that it's figure ſhould be exactly anſwerable to the mould wherein it was formed; but Mr *Baker* begs leave to take notice, that the *echinus*' ſhell is perfectly ſmooth internally, having no riſing parts correſpondent to theſe cells or cavities; and therefore, as it could not receive it's configuration from thence, it muſt be owing to the natural ſhooting of the cryſtalline matter (tho' unlike every thing of that kind yet deſcribed), or to ſome other cauſe, which he don't pretend to know *.

The configuration ſeems nevertheleſs in ſome meaſure to correſpond with the nature of the ſhell wherein it was formed: as to the number of the rows of cells, they being ranged by fives, as the *papillæ*, *indentings*, *lines*, or other *marks* on the recent ſhells of *echini* conſtantly are; theſe rows are twenty in number; *viz.* five double ranks of large and extremely regular cells, as at *a a*, &c. between which lie five other double rows of ſmaller and leſs diſtinct *cellulæ*, ſhewn at *b b*, &c. Theſe cells, which are hexagonal, and whereof thoſe in every row lie alternately to thoſe of the next (by which means they fill up the whole ſpace), decreaſe in their ſize gradually, as they approach nearer to the top; all the rows at laſt almoſt concentring at the *apex*, leaving only a ſmall ſpace or vacuity, where in the ſhells themſelves of this kind of *echinus* there is an aperture. The ſmooth part at *A* is formed of a pebbly ſtone, bearing the ſame marks as are uſually found in the impreſſion of theſe *echinitæ* dug up in gravel-pits; which proves, that this muſt have received it's general figure from one of thoſe ſhells, whatever has been the cauſe of this remarkable configuration of the cryſtalline part.

This curious *echinite* was found in a marl-pit at *Baborough*, about 3 miles W. of the city of *Norwich*, and preſented to Mr *Baker* by Mr *Wm. Arderon*, F. R. S.

A letter from Mr Emanuel da Coſta, *F. R. S. to the* Preſ. *concerning two beautiful* Echinites N° 492. p. 143. April &c. 1749. *Read* May 11. 1749.

2. I have the honour of laying before you (in order to communicate to the *R. S.* if you think it worthy) the deſcription of two *echinites*, or ſtones moulded in foſſil *echini* ſhells, hitherto undeſcribed, as far as I know.

Theſe *echinites* are undoubtedly moulded in ſhells, of a *genus* of which we at preſent find ſome ſome ſpecies now living in the ſeas; moſtly in the *Weſt Indies*. The *echinometra* of *Ariſtotle*, *Aldrovand*, and of Dr *Grew* (*a*), is of this *genus*. Dr *Breynius* (*b*) calls the whole *genus Echinanthus*; and Mr *Klein* (*c*) *Scutum*. *Woodward* (*d*) in his diſtribution of Foſſil *Echini*, calls them the *Pentaphylloides*, from the rays on the upper part forming a beautiful cinquefoil figure; but wrongly fixes their characteriſtics in having only one aperture, and that

* Perhaps to ſome cells or membranes belonging to the body of the *echinus*. *C. M.*

(*a*) *Muſeum Reg. Soc.* p. 139. (*b*) *Schediaſma de echinis*, p 60.

(*c*) *Nat. Diſp. Echinodermatum*, p. 20. Tab. 17. *A*, & Tab. 18. *B*.

(*d*) *Cat.* of foreign extraneous Foſſils, *p*. 16.

at the baſis; in which he not only contradicts nature, but alſo the very ſpecimens he quotes in his own Collection, which have all two *foramens* or apertures, and are elegantly figured ſo by *Agoſtino Scilla* (*e*), who was the perſon that ſent them to the Doctor; and Sir *Hans Sloane* (*f*) has alſo figured and deſcribed two ſpecies of this *genus*, whereof one ſpecies is an inhabitant of our *Engliſh* ſeas.

I obſerved above, that, to my knowledge, no author has ever deſcribed *echinites* or ſtones moulded in the foſſil *echini* of this *genus*; nor even have the foſſil *echini* or ſhells themſelves been ever exhibited by any Lithologiſt, except by the above-quoted *A. Scilla*, who ſent them to Dr *Woodward*, and found them in *Malta*; to which the Doctor in his Catalogue recounts two other ſpecimens, which were dug up in *Maryland*; ſo rare are the inſtances of the foſſils of this whole *genus!*

The two *echinites* here deſcribed (as alſo ſome few other ſpecimens of this ſort, which I hear are in ſome cabinets in this metropolis) were all found in the midſt of ſome rocks, which were blown up at *Port Mahon* ſome years ago, and from whence they were all brought.

The firſt or largeſt is in the poſſeſſion of the Right Rev. Dr *Lavington*, L. Biſhop of *Exeter*; it is compoſed of a hard or ſtony arenaceous greyiſh ſubſtance, and is of an eſcutcheon or heart-like ſhape: it meaſures about $14\frac{1}{2}$ inches in circumference, or quite round the limb or edge, about two inches high from the flat or baſis to the tip of the *apex*, five inches in length at the baſis, and $4\frac{1}{2}$ in breadth. On the upper part it riſes nearly gradually from the edge quite to the *apex*. A central point, with a ſlight declining ſpace, tops the ſaid *apex*; from which ſpace the body regularly divides into five parts figured like leaves to the edge. Theſe leaves are narrow at the *apex*, greatly widen toward the bottom, and narrow a little again at their end. Each diviſion or leaf is bounded on each ſide by a row of parallel ridges, which are accompanied alſo on each ſide of every ſaid row, with two other ranges of points or knobs; all which rows do not meet or cloſe together at the lower end of the diviſion, but leave a void unwrought ſpace: a row of larger irregular knobs runs through the midſt of each leaf. From the diviſions between each leaf runs a rugged knobbed pillar, which is joined to the edge: the other parts between the leaves and the edge are hollows, or void ſpaces. The edge or *limbus* is of a thick cylindric make, runs quite round the whole body, and only has ſome ſigns of being disjoined at the one extreme of the length, or where the aperture was; the ſtone anſwering which is here extended a little cylindrically outward like an appendage, and was ſo formed by the ſtony matter being too much in quantity for the ſhell, and ſo was protruded through the ſaid *foramen*. On the outer edge of the *limbus* are ſome few irregular ſtony concretions. The baſis is flat, and is likewiſe divided into five

(*e*) *La vana Speculazione diſingannata dal ſenſo*, TAB. 9, 10, and 11.
(*f*) *Nat. Hiſt.* of *Jamaica*, Vol. II. TAB. 242. *Fig.* 3. & *ſeq.*

parts

parts from the center, which is one of the *foramens*; the other *foramen* (as has been above deſcribed) being placed at one of the extremes of the length. This *foramen* or center is about the ſize of a ſhilling. The 5 diviſions extend to the utmoſt edge of the body, or quite over the *limbus*, contrary to the diviſions on the upper part, which extend only to it. Each diviſion is formed by a ſtony line edged on each ſide with ſtony cylindrical bodies of the thickneſs of a pin, but of different lengths, ſo as to appear like the teeth of a comb, or the gills of a fiſh; the interſtices between all which is a rugged ſtony work, and hollows pervading quite through the body to the upper part.

I cannot but think theſe five pectinated diviſions on the baſis owe their figure to ſome parts of the included fiſh; which I am more confirmed in, as I have ſeen ſome ſpecimens of the common pileated and galeated *echinites*, which have been hollowed at their *apex*, and marked ſtar-wiſe; that concavity, and the ſtellar mark proceeding from the interpoſition of the fiſh between the ſtony matter then filling the ſhell, and the top of the ſhell itſelf.

The other *echinite* I have the honour of producing before the *Society*, belongs to Mr *Edward Jacobs* of *Feverſham*. It is of a different ſpecies, though of the ſame *genus*, of a heart-like ſhape, and about one third the ſize of the above-deſcribed. This is greatly copped, the *apex* lying very high, and the five diviſions running near perpendicularly down to the edge. The upper part of this is elegantly perfect; the work is near the ſame as on the other; only that, by the perfection this is preſerved in, we obſerve that the rows of parallel ridges, which adorn each ſide of each leaf or diviſion, riſe into a kind of arched work or bridge, made up of arched cylindrical bodies, through which the middle row runs, joined or connected in a long ſtrait cylindrical ſtem, in a moſt curious and elegant manner. The baſis or under part of this ſpecimen is very imperfect, and only ſeems to differ in the center being greatly excavated or concave, anſwering to the great copping or height of the *apex* or upper part. This foſſil alſo conſiſts of a hard ſtony arenaceous ſubſtance like the other.

From the inſpection of the ſeveral hollows of theſe *echinites*, it is evident they were not immediately moulded in the ſhells, but were formed in cavities which thoſe ſhells formerly filled in the rocks they were lodged in. The rocks were apparently of a looſened arenaceous texture, and the water, *&c.* continually pervading them, rotted and deſtroyed the incloſed ſhells, and bore away their whole ſubſtance. In the ſame manner, and by the ſame means, were the ſtony particles replaced into thoſe very cavities which the ſhells formerly filled; conſequently theſe bodies were moulded exactly to the ſaid cavitities.

This remark carries a concluſion with it, if obſervation be made, that the hollows and ſolid parts of theſe ſtones exactly anſwer to the hollows and ſolid parts of the very ſhells themſelves; which, had they been moulded in the very ſhells, muſt have happened directly contrary; the ſolid

solid parts of the shells forming hollows in the stone, and *vice versa*. In all sandy or lax earthy matter fossil shells are very seldom found, but only the moulded stones; the loose texture of those substances giving free access to water, vapours, and mineral exhalations, *&c.* which intirely corrode and destroy the shells buried in it.

I have taken the liberty to produce before the *Society* a recent *echinus* of this *genus* from the *West Indies*, to elucidate my subject; as also two drawings done by Mr *Mynde*; *viz.* of the basis of the large *echinite*, and the upper part of the small *echinite*.

Fig. 52, 53, 54.

Fig. 52. A view of the upper part of a curious *echinite*, in the possession of Mr *Edward Jacobs*, of *Feversham* in *Kent*.

Fig. 53. A view of the under side of the same *echinite*.

Fig. 54. A view of the under side of a curious large *echinite*, in the possession of the Right Rev. Dr *George Lavington*, Bishop of *Exeter*.

N. B. The upper part of this echinite having nothing remarkably particular or different, it was not judged necessary to give a figure of it.

A letter from Mr Joseph Platt *to Mr* Peter Collinson, *F. R. S. concerning a flat* Spheroidal Stone *having lines regularly crossing it.* N°. 496. p. 535. Nov &c. 1750. *Dated* Manchester, Dec 9. 1749. *Read* Nov. 8. 1750.

Fig 55.

3. A little while since a man brought me a stone, which he found at *Ardwick*, 7 feet deep, near this town, in driving a slough through some gret-stone. It is what I call a nodule, of a close, compact, smooth matter; was incrustated with coarser earth, or soft stone; is 3 inches and a half diameter; formed not unlike one of the *echini marini*; except the *papillæ* or small protuberances, which it wants. Upon examining it, I find four white seams, about the bigness of a horse-hair, which quarter the stone very correctly. The angles are exactly the same, and correspond so well, that it would require the nicest mathematical head and hand to draw the like.

The diameter *A B* is 3.7 inches; the strait line *C D* at the bottom, or greater base, is .42 of an inch; that at the top of the stone is .21 of an inch, which make the angle *C D* equal at top and bottom, tho' of different diameters. The seams are like talc or spar. It weighs about 3 pounds. I have several nodules, but none like this. There is nothing curious in this stone but the lines, which I have described in the best manner I can. I am confident chance had no hand in forming it; and I am as certain, that no artist was ever concerned or able to do the like; therefore I conclude it has been something formed before the Flood, and is of marine production *.

Fig. 56.

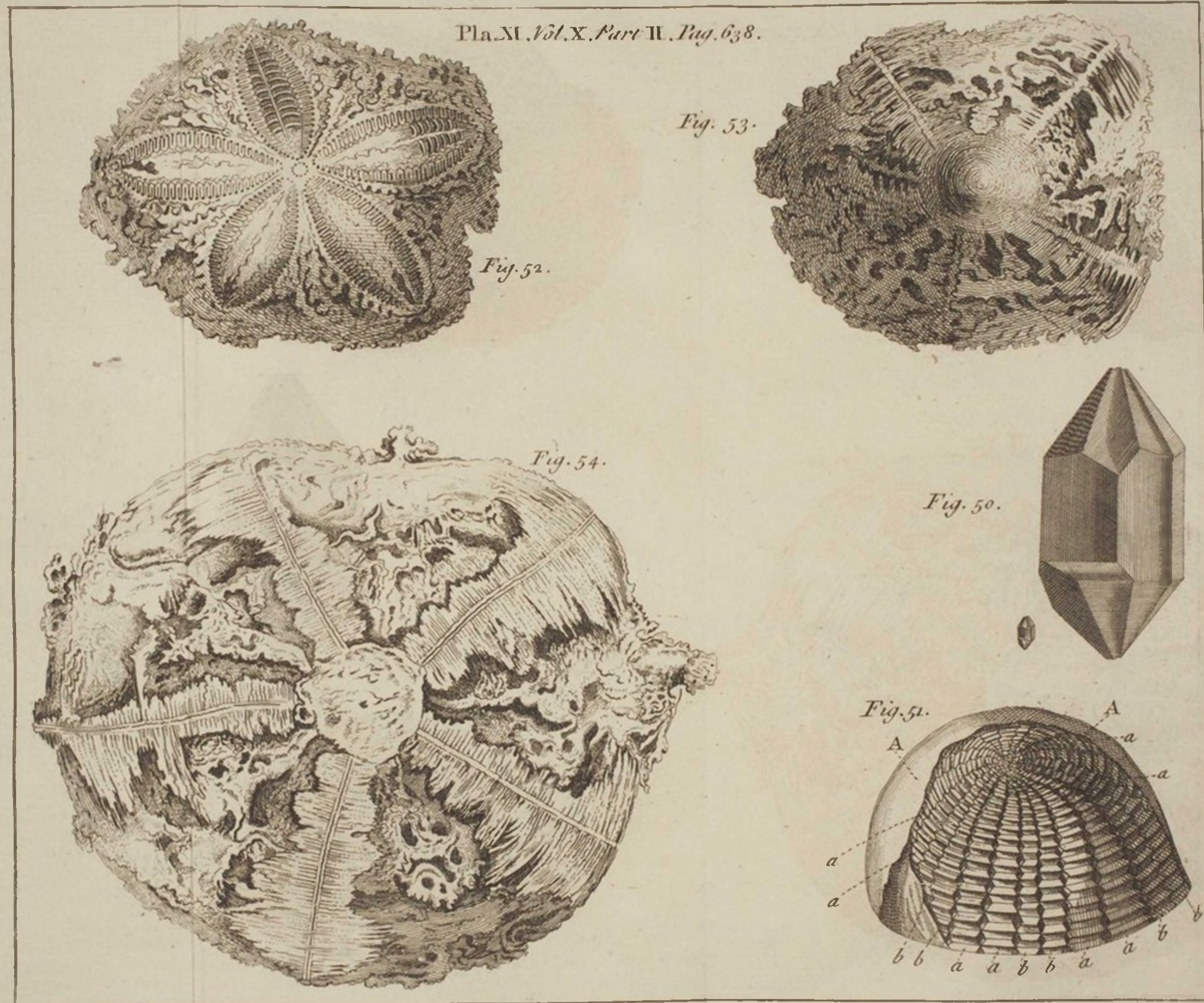
The description and figures of a small flat spheroidal Stone, *having lines formed upon it; by* C. Mortimer, *M. D. & Sec. R. S.*

4. I lately received, by a friend, from the isle of *Shepey* in *Kent*, a small stone, with similar lines upon it. Mine is only ⅞ of an inch in diameter, of a brown colour, and of the consistence of marble. As a

* The stone having been since cut in two, it was found that those regular lines, composed of a sparry matter, penetrated the whole substance of the stone quite thro', and that they grew wider as they were nearer to the centre. *See* Fig. 56. *C. M.*

descrip-

Pla. XI. Vol. X. Part II. Pag. 638.



description in words does not convey so clear an idea as an exact drawing, I have endeavoured to give representations of this stone in different views. Ibid. p. 602.

Fig. 57. represents the top of the stone, on which the lines are most regular, being depressed into the stone, and of the same colour with it. *a*, *c*, *d*, *e*, are the four principal lines, answering to those on Mr *Collinson*'s stone, and are connected, as in his, by the transverse line *g b* The line *b* is an irregularity in this stone, and so is *f*, which are not in the other; these irregular, or supernumerary lines being continued to the other hemisphere, Fig. 57.

Fig. 58. or bottom of the stone, make the directions of the other lines very irregular, as may be seen in the figure; only the lines *c*, *d*, and *e*, being connected by the transverse line *g b*, which here stands at right angles with that in *Fig.* 57. Fig. 58.

The following figures represent the section of the stone through it's equator, as nearly as possible; only the mill cut away the substance to about the thickness of a shilling. In these sections the ramifications appear quite white.

Fig. 59. shews the section of the upper hemisphere, as Fig 59.

Fig. 60. does that of the lower hemisphere; in both of which the letters of reference answer to those in the other figures, shewing where the outside lines abutt upon these sections. Fig. 60.

XXIII. This curious fossil seems to be composed of a stony matter like marble. which has penetrated the cells of the *nautilus* while in it's natural state. The diaphragms or partitions remain still distinct and visible. The different colour of the stony matter in some cells of a dark-brown or hair-colour, in others of a light-brown or ash-colour, with the natural polish of the outside, gives it a beautiful appearance; as it is represented in the figure, where it is drawn of it's natural size in three different views.

A beautiful Nautilites, *shewn to the* R. S. *by the Rev.* Charles Lyttleton. *L. L. D. F. R. S. and Dean of* Exeter. N°. 487. p. 320. Apr. &c. 1748. *Shewn* May 5. 1748.

A shews the side view of it. *B* the fore part. *C* the back part.

It was found in *Pool*'s *Hole* in *Derbyshire.* It's sutures or diaphragms resemble those of some of the larger *cornua ammonis*; but it's shape bespeaks it to be a species of *nautilus*; and it is thought to be a non-descript, both in it's natural and fossil state. Fig. 61.

XXIV. Various have been the opinions of authors concerning the origin of the *belemnitæ*, and as various the systems and hypotheses advanced by them in support of their opinions; some having imagined them vegetable productions; others have taken them for the different parts of animals, as teeth, horns, bones, *&c.* in which even these again have differed, as to the referring them to land or marine animals; and they have been by others supposed of mineral origin, or *lapides sui generis.* What they really are, will, I doubt, be still very difficult to determine; but, as one principal objection to their being originally marine

Considerations on two extraordinary Belemnitæ; *in a letter from Mr* David Erskin Baker, *to* M. Folkes, *Esq*; *P. R. S.* N°. 490. p. 598. Dec. 1743. *Read* Nov. 24. 1748.

marine bodies (which ſuppoſition ſeems to carry the greateſt colour of probability) has been, that no marine bodies have been found adhering to them, that objection will be obviated by no leſs than two ſpecimens, from the ſame place, of *belemnitæ*, whereto undoubted marine ſubſtances are found firmly affixed; by which inſtances, as ſome further light may be thrown on this ſubject, that conſideration will, I hope, ſtand as an excuſe for my troubling you with this paper.

Theſe curious foſſils were found in a chalk-pit in *Norfolk*, from whence they were ſent not long ſince to my Father Mr *Hen. Baker*, F. R. S.

Fig. 62. *Fig.* 62. Is a *belemnites*, whoſe *apex* is perfect; the conic cavity, and the longitudinal ſeam, evidently diſtinguiſhable; which, as well as the contexture of the ſubſtance whereof it is compoſed, ſhew it to be a true *belemnites*; but on it's ſurface are placed, in their natural condition, by which I mean not at all ſeemingly petrified, or otherwiſe altered, two of thoſe *vermiculi* that are ſo frequently found ſticking to oyſters, ſcallops, and many other kinds of ſhells, when taken out of the ſea.

Fig. 63. *Fig.* 63. A *fruſtum* of another *belemnites*, the *apex* whereof is broken, but the conic cavity is ſtill remaining, and ſhewn at *a*. To this *belemnites* adheres a ſhell of the oyſter-kind, which is faſtened thereto ſo ſtrongly, that they are not to be ſeparated without breaking: which ſhell, as well as the before-mentioned *vermiculi*, ſeems not altered in it's ſubſtance, but appears like a recent one, of which many are to be met with in the cabinets of the curious.

Fig. 64. *Fig.* 64. Shews the other ſide of the ſaid ſhell, wherein the *cardo* or hinge at *b* is plainly diſcernible; at *c* appears the broken end of the *belemnites*, where the radiated contexture (well known to belong to their bodies) is repreſented, as alſo the longitudinal ſeam at *d*.

As theſe ſpecimens are undeniable proofs of marine bodies adhering to *belemnitæ*, ſeveral of the curious who have ſeen them, are of opinion, that they tend likewiſe to prove the *belemnitæ* to be marine productions. It may probably be objected, that theſe ſhells might have been brought and depoſited near the *belemnitæ* whereto they are affixed, by whatever mighty change it came to paſs that productions of the ſea are diſcovered in moſt countries at great depths in the earth, and in the bowels of mountains at great diſtances from the ſea (even ſuppoſing the *belemnitæ* to be *lapides ſui generis*, and produced in the earth) and that theſe ſhells might be cemented to them afterwards by ſome mineral, ſtony, or other matter. But the following obſervations will render this improbable; for,

1. The *vermiculi* of *Fig.* 62. are not any ſpecies of the *tubuli marini*, found ſometimes recent, and ſometimes foſſil, detached intirely from every other body; but are of that ſort, which is perhaps never ſeen

ſeparate,

Fig. 55.

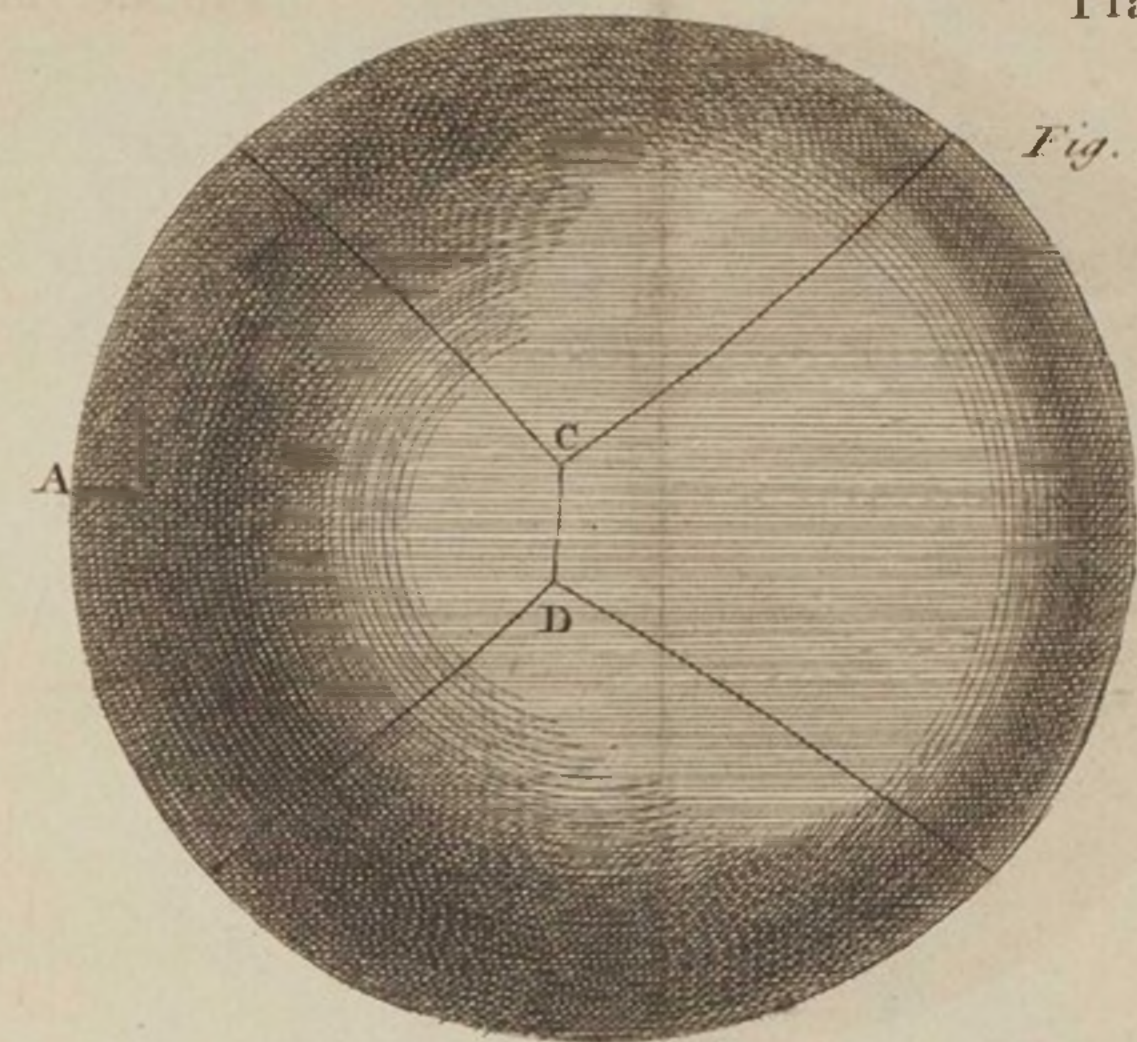


Fig. 57.

Fig. 58.

Fig. 56.

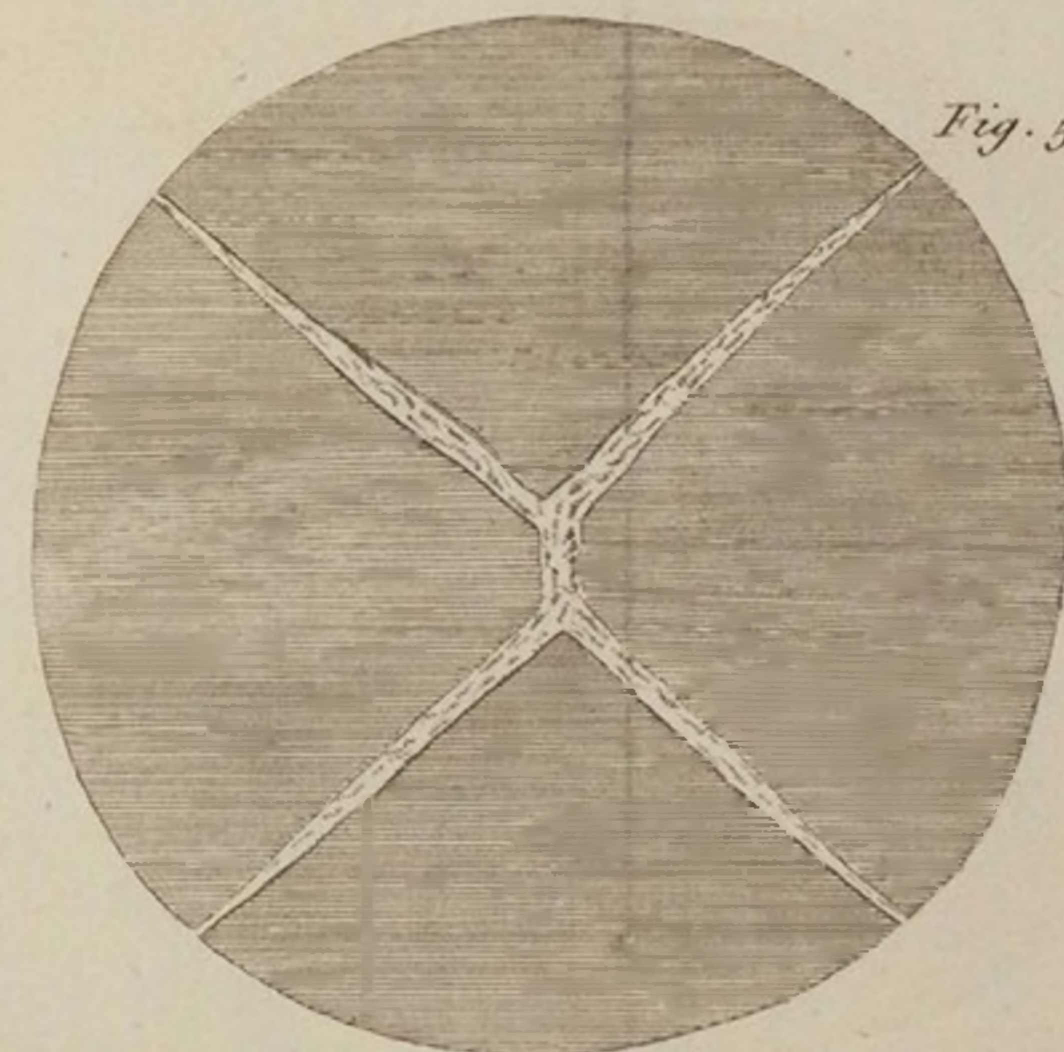
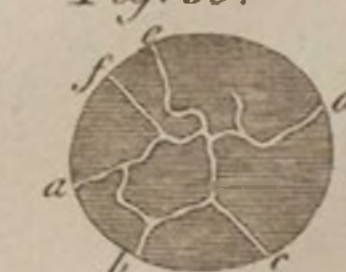


Fig. 59.

Fig. 60.



ſeparate, or in any other manner, when recent, that attached and faſtened to other ſhells or ſtones; and they are placed on this *belemnites* exactly in the ſame manner as they are commonly found on other marine bodies; *viz.* lying on their broadeſt ſide, with their ridge upwards, and glued as it were thereto by a ſhelly ſubſtance.

2. In *Fig.* 64. at *e*, is plainly to be diſtinguiſhed, that the ſhell has been faſhioned thus by the convex ſurface of the *belemnites*, in the ſame manner as theſe ſhells commonly receive a form from whatever ſubſtance they adhere to; which plainly implies, that this ſhell was faſtened to the *belemnites* when itſelf was very ſmall, and in a growing ſtate; and that the ſhell in it's growth was formed according to the figure of the body on which it was affixed: but ſuch growth could not poſſibly have proceeded any-where but in the ſea; and therefore theſe two bodies muſt neceſſarily have been in the ſea at one and the ſame time. Fig 64.

There is now but one way more, whereby theſe ſhells (ſuppoſing the *belemnitæ* to be ſtones *ſui generis*) could poſſibly become affixed to them; which is, that the *belemnitæ* might have been by ſome accident thrown on the ſea-ſhore; and that there the ſhells might faſten themſelves to them, as well as to any other ſtone. But as this muſt imply ſome former convulſion in nature, whereby they were caſt out of their natural beds upon the ſea-ſhore; and again a ſecond convulſion to carry them to the chalk-pit where they were found; ſo far-fetched an objection will, I believe, carry but little weight.

To conclude, I ſubmit to your opinion, whether the ſides of the conic cavity, whereto the oyſter-ſhell is affixed, has moſt the appearance of a ſtone or of a ſhell.

Some Vertebræ *of* Ammonitæ, *or* Cornua Ammonis; *in a letter from Mr* H. Baker, *F. R. S. to the* Preſ. N°.491. p. 37. Jan &c. 1748. *Read* Feb. 9. 1748-9.

XXV. I lay before you a curious and moſt extraordinary foſſil, which was lately ſent to me for that purpoſe by my worthy friend Dr *Miles*, of *Tooting*, *F. R. S.* It conſiſts of 26 joints, which he calls *vertebræ*, and I believe ſuppoſes to have been the joints of the back-bone or tail of ſome animal; but, upon conſidering them with attention, they will perhaps rather be judged to be the ſeveral articulated diviſions that compoſe the body of ſome kind of *nautilus*, or of ſome one or other of the various ſpecies of the *ammonitæ*: which opinion is I think ſupported not only by the ſpiral figure, which they form when put together, but likewiſe by the traces or markings of ſuch-like articulations, found on ſome particular kinds of foſſil *nautili* and *ammonitæ*; one whereof I alſo lay before you, as a proof of this conjecture.

You will obſerve all the parts of this uncommon foſſil are converted into a ſort of ſparry ſubſtance, and that they are articulated with one another in an exact and beautiful order. I have faſtened them together in two diviſions, that they may be examined more eaſily than they could be, if they were all ſeparate, and in confuſion: and indeed I am not quite ſatisfied that theſe two bundles belonged both to the ſame individual animal; if they did, ſome joints muſt be wanting that came between them, and united them together, as the two ends do not at present match: and what makes me suspect they did not, is a different articulation to be observed on one side of that division made up of the largest joints: besides, the whole number appears rather too much, and the smaller joints seem to make up a body whose figure is nearly perfect.

Fig. 65. *Fig.* 65. The larger joints.

Fig. 66. *Fig.* 66. The smaller joints.

Fig. 67, 68. *Fig.* 67. and 68. The fore and back side of a single joint.

P. S. Dr *Miles* says they belong to Dr *Clark* of *St Alban*'s; that they were found in *Oxfordshire*, and were formerly in the possession of the late Mr. *William Becket*, surgeon, *F. R. S.*

An enquiry into the original state and properties of Spar, *and* Sparry Productions; *particularly, the* Spars, *or* Crystals, *found in the* Cornish *mines, called* Cornish Diamonds; *in a letter to* Emanuel Mendez da Costa, *Esq; F. R S. from the Rev Mr* Wm. Borlace. N°. 493. p 250. Oct. &c. 1749. *Read* Dec. 14. 1749. May 3. 1750. Sect. 1.

XXVI. I shall consider spar here as the genus, at the head not only of all the species of common spar, and incrustations of what colour soever, but of crystals and gems, which are here understood only as finer and purer substances of the spar kind (1).

There are several sorts of these sparry productions, which are carefully to be distinguished from each other; but they may all be considered, first, with regard to their original state, or what they have been; and, in the next place, as to what they now are, that is, as to form, size, colour, hardness, texture of parts, and direction of their shoots in the mines or quarries. These particulars, separately discussed, may possibly lead us to several probable conjectures concerning the original and most distinguishing properties of these bodies, such as may afford some light to this perplexed and intricate subject.

That all spar has been, at one time or other, in a state of fluidity, may be maintained, I think, with great reason, as well as supported by the authority of some of the most eminent Naturalists (2). In some spars are found straws, and other light bodies; and we may therefore as justly conclude them to have been once fluid, as the amber that incloses the bee. In some stones, whereon were sparry concretions, Dr *Woodward* found fragments of shells, and pellicles of the ova of fishes; a

(1) The properties of crystal assigned by a late treatise (*Mr* Hill's *Nat. Hist. of Fossils*), such as keeping itself unaffected by acid menstrua, remaining unaltered in a moderate fire, and giving forth sparks of fire by collision (whereby that author distinguishes it from spar), are here reckoned, rather to be accidental than classical differences, owing to a purer stony juice, less friable and terrene than that of the common spar, than to any essential and radical difference in the *Principia* of these bodies. ["There is in all spar "more or less of crystal." *Woodward's Nat. Hist. Fossils*, 158.] For many spars there are, which are opaque, and yet in the same hexagonal form as crystals; whence it appears, that spar and crystal do not differ in substance and nature, but in transparency, colour, and different degrees of purity. "Spars much the same with crystals, says Dr "*Plott, Oxf. p.* 98. §. 52." And *Boëtius* doubts not, but they (*viz.* Spars) are of the same matter with gems, *ib.* §. 53.

(2) *Woodward*'s Cat. Foss. Vol. I. p. 151, and 157. N°. 78. *alibique passim.*

certain

certain evidence, that this ſparry production was not anterior to the Deluge; for the ſtone muſt have coaleſced and hardened upon the ſhell, before the ſparry concretion could have fixed upon the ſurface of the ſtone; and as I apprehend, the Learned are now very well ſatisfied, that ſuch extraneous foſſils as are mentioned above, are not the *luſus naturæ*; but the exuviæ of animals brought where we find them by the waters of the Deluge. Wherever any number of the ſhotten ſpars occur, there may be ſeen ſucceſſive incruſtations and cryſtals fixing on other cryſtals, ſome incruſtations broken off, and ſhewing their concave baſe, ſhaped by the cuſpis or apex of the diamonds on which they were once faſtened; which ſhews, that there has been a ſucceſſion of ſeparate and diſtinct indurations. In ſeveral places we find wavy proceſſes form- Fig. 69.
ed in thin plates, on the perpendicular ſides of the rocks, by the ſpars flowing down in the ſame manner, as one wave ſucceeds another on the ſea-ſhore; to which we may add, the frequent formation of ſparry efflorеſcencies, accretions on walls, and ſtalactites hanging down from the vaults and caverns of deſerted mines, evidences ſufficient of the modern date of ſuch productions. That we may the eaſier apprehend this truth of ſpars having been once a fluid (upon which much depends), it may be here obſerved, that ſomething very like this proceſs, (I mean liquors hardening into ſtone) is commonly ſeen in the effects of petrifying waters; where as ſoon as the ſtony juice meets a proper nidus of wood, reed, graſs, or the like, it will forſake it's ſtate of fluidity, and become a ſolid ſtone: why then ſhould it ſeem more unaccountable or difficult, to conceive that the ſame alteration ſhould happen in the bowels of the earth, and in larger maſſes of matter? For as the ſame cauſe will in like and equal circumſtances produce the ſame effect, ſo to produce a greater effect (*viz.* an alteration of form or motion in a greater quantity of materials), there is need only of a proportionably greater force in the cauſe; it being as eaſy for a powerful effort to produce a rock, or a mountain, as for a ſmaller force to congeal a pebble, or form the ſmalleſt gem. If it were poſſible, therefore, for us to be as attentive witneſſes of the changes which happen under the ſurface, as we are of thoſe which appear on the banks of every petrifying ſpring, we ſhould diſcover many new ſtones produced every now-and-then, which by their firmneſs appear now to have been as old as the world (3). It is indeed a vulgar miſtake to imagine, that time has added, or ſhall add, to the firmneſs of a ſpar; or becauſe it is ſo hard and compact a body, that it cannot therefore but be as old as the firſt formation of things; for ſpar becomes as hard at the firſt time of it's conſolidating, as it will be ever after, as we find by the exact ſhape, and the ſmooth ſides which *Corniſh Diamonds* make in incruſtations, and all after and ſecondary

(3) Since the writing of the following Treatiſe, Mr *Hill* (*Nat. Hiſt. of Foſſils*, *p.* 157.) by a curious chemical inveſtigation of the lapideous contents of water, ſays, "That ſtones "and minerals, formed of cryſtal and ſpar, need not be ſuppoſed all of them as old as "the Creation or Deluge; but may be, and unqueſtionably are, formed to this day."

concretions. This ſparry liquor is ſtiff and ſluggiſh, and apt to harden; but it is a liquor however, before it becomes a ſtone. Nor is this opinion ſingular, but adopted by many of the Moderns as well as Antients. *Pliny* (4), from the reſemblance that cryſtals have to water, carried this hypotheſis much too far, and thought them to be nothing more than water congealed by exceſſive cold; and *Diodorus* eſteems them no better than a concretion of pure water, aſſigning however a different cauſe, concluding them hardened by a divine heat. *Agricola* makes the *ſuccus lapideſcens* the original matter of which ſtones are formed, ſome by the heat, others by the cold (5) they meet with, during the ſtate of fluidity (6). Mr *Geoffroy*'s hypotheſis ſuppoſes cryſtal to be formed of thin equable plates, that water is the vehicle of cryſtalline parts; and when thoſe parts meet together in any quantities, the water eaſily evaporating leaves the cryſtals to form themſelves into hard, pellucid bodies. Mr *Boyle*'s opinion was, that theſe bodies were originally in a fluid ſtate (7).

Sect. 2. *Whence this fluidity.*

'Tis water that firſt occaſions, and afterwards maintains, this fluidity; and the reaſon why we find none of this ſparry maſs in it's fluid ſtate, nor ever ſee this lapideous juice, is, becauſe whilſt it remains incorporated with the water, it is not to be diſtinguiſhed from the liquor in which it ſwims; and as ſoon as ever it is deſerted by the water that circulated it in the bowels of the earth, and other neceſſary circumſtances concur to produce that change, it becomes ſtone: by water it is that the ſparry atoms are waſhed forth out of their repoſitories (8), collected into a thick, tranſparent, or opake juice (the ſtony particles attracting each other as much as the intermediate water will give leave); and as ſoon as the redundant water is drained off, or evaporated, the lapideous parts (now more at liberty) accede to a cloſer union, and are aſſiſted greatly therein, as well by the condenſing nature of cold, which compreſſes the parts, and forces them nearer one to the other, as by ſudden evaporating heats; and thus the ſtone forms itſelf, ſo much water reſting in the pores and interſtices of the parts (in proportion to the number and magnitude of thoſe pores), as is neceſſary to fix it into a conſiſtency; for, as I apprehend, there is no compound body, but by means of the Chemiſt's fire will yield ſome water; but as ſoon as all the water is thrown off, the body loſes it's hardneſs and continuity, and turns to a calx and powder.

(4) *Lib.* 37. *c.* 2.

(5) " *Utroque enim modo effici poteſt Lapis.*" *Ag. de Ort. Subt. Lib.* 4. *Baſil. Edit. p.* 57.

(6) *Ib. p.* 56.

(7) See *Boerhaave*'s Theory of Chemiſtry, by *Shaw*, *Not.* 120.

(8) *Woodward*'s Nat. Hiſt. of the Earth, 2d Edit. p. 189.

" Water is the only agent that educes the matter, of which they (*viz.* ſpar and " cryſtal) conſiſt, out of the ſtrata, and compiles and forms it in the perpendicular " fiſſures." *Woodward's N. Hiſt. Foſſ.* Vol. I. p. 150.

Here

Here I beg leave to propofe a few queries.

Whether fpar is not the univerfal glue of ftones, diftinguifhed from each other by the various mixtures of earthy, mineral, or metallic particles, but all united by the fparry liquor? for it feems to me, that there is fcarce any fand, nodule, ftone, or ore, which either by the naked eye, or glaffes, may not be difcerned to have a certain portion of fpar, clear, or opake, in it's compofition. *Query* 1.

Whether it is not reafonable to believe, that ftones in all ages have been, and are ftill forming in the earth, in fome fuch manner as is here mentioned, whenever the neceffary materials and caufe concur with proper incidents? *Query* 2.

Whether this hypothefis is not better adapted to account for teftaceous, and other extraneous bodies, found inclofed fo often in maffes of ftone, than Dr *Woodward*'s fuppofition, that all ftones were reduced into a fluid mafs by the waters of the Deluge; which waters being thofe of the ocean, we cannot allow to have any fuch diffolving power inherent in them, and therefore they produce no fuch effect? *Query* 3.

Whether there are not quarries of ftone, which when left idle, or unwrought for fome time, yield a frefh fupply of ftone in the chanels and hollows of the faid quarries, which had been before thoroughly cleared by the workmen (9); and whether this will not confirm the fuppofition, that ftones formed fince the Deluge, in places where fhells, teeth, and the like bodies, were depofited by the waters, inclofed them in their fubftance? *Query* 4.

Our *Cornifh* fpars are either plain, fimple, and unfigured, or figured into various and rectilineal fhapes. Sect. 3. *Different appearances of Spar.*

All fparry liquor is in itfelf ftiff and fluggifh, and covets no fhape; but, being intimately mixed with water, which is the reftlefs agent, to difperfe, collect, and renew all fubterraneous nature, it moves as a fluid by the rules of gravitation, that is, from a higher to a lower pofition, till meeting with a retentive bed, the water no fooner retires, and leaves it expofed to a drier or colder air, than it dries, and hardens into ftone, in fhape and fize, as the attraction and quantity of it's own parts determine, or the circumambient bodies will give it leave to fix and extend itfelf.

Sometimes we find the fparry liquor fpread into thin plates on the horizontal or oblique planes of rocks; fometimes we trace it in fheets down the fides of fiffures; and where it meets with impediments of gravel, or ftone, it will refemble branched limbs, clay, boughs, and ftumps of fhrubs; fometimes it drops from vaults, and roofs of caves, whence it has the name of *Stalactites* (10). In all thefe cafes it is plain, that the juice had no other motion, whilft a juice, nor appears in any *Plain Spars.*

(9) See *Addifon*'s Travels into *Italy*, and Bp. *Burnet*.

(10) It alfo veins or granulates, or both, every kind of ftone; and is oftentimes found to compofe whole loads or veins, without any metallic or mineral mixture, or any particular fhape, more than the fiffure in which it refted compreffed it into.

other

other ſhape now a ſtone, than what it's own weight or gravitation, during it's ſtate of fluidity, inclined it to. In theſe uninformed rude productions, it is very plain, I think, that the juice wanted thoſe active principles (whatever they be), which enable it at other times to ſhoot into regular forms.

Fig 70. *Fig.* 70. is a ſpar pebble, it's ſurface about the roughneſs of the peach-ſkin, incloſed in part of it's ſocket, which is alſo of ſpar, angular, and puculated (which latter property is rarely met with): the coat or ſocket is mixed with ſolid white mundic, and cockle; which laſt (or the ſame principle, which throws cockle (11) into this oval figure) ſeems to have determined this ſpar to it's ſingular, *viz.* orbicular ſhape; for it is obſerved, that where cockle is plenty, ſpar-nodules round as muſket balls, and black, in ſockets of the ſame colour and ſubſtance, are frequently found. But as this pebble was not black, as cockle always is; it may therefore be queſtioned, whether the ſhape of it may not be owing to ſome metallic (*viz.* iron or copper) principle, rather than to cockle, and whether cockle itſelf be not more probably indebted to other powers for it's orbicular nodules, in ſuch ſheaths, than derive them from any inherent activity of it's own. The exterior of the ſhell or ſocket has a thin incruſtation of gritty cinereous mundic. It came out of *Wheal Royal* mine, in the pariſh of *Cambron*, *Cornwall.*

Of incruſtations. The next appearance of ſpar bodies, which I ſhall here take notice of, is that of incruſtations; theſe ſometimes make one continued ſheath, lump, or maſs, and incloſed in them we find *Corniſh Diamonds*, grains of tin, and other adventitious bodies, plainly of a different texture and colour from the cruſts which ſurround them; ſo that incruſtations muſt be cautiouſly diſtinguiſhed from the entire ſheaths, or laminæ, which compoſe the column of hexagonal cryſtals, and which are really formed at the ſame time with that column, whereas incruſtations are additional, and after concretions made on the before ſettled original grains and columns, (12) ſometimes theſe incruſtations, are but ſprinklings of the cryſtal drops, without any continuity, or mutual contact; and in this caſe, when the pearly drops are themſelves bright and tranſparent, and the ſtone they fix on, of an agate colour, or any lively oppoſition, the incruſtation is exceedingly beautiful. On one ſhotten bliſtered ſpar,

Fig 71. I find the incruſtation white, not pellucid, flowing in parallel threads by each other in ſeveral places paſſing from one tubercle to another,

(11) Cockle is a black, ſhining, light ſtone, free of all metal, different from mock-lead, common in the tin-mines of *Cornwall.*

(12) Incruſtations are ſo many evident proofs of ſtones not being formed all at the ſame time; for many *Corniſh Diamonds*, and columnar ſhoots of tin, cubes of mundic, and grains of lead, are often broke off from theſe their incloſures; but the angular cavities, with their ſtrait edges and ſmooth ſides, ſtill appear in the incruſtation; which plainly ſhews, that the diamonds and tin-ſhoots, *&c.* were firſt formed and hardened, and then ſurrounded and united into one lump, by a ſucceſſive induration of theſe cryſtal or ſpar cruſts.

without

without touching the interſperſed hollows; by which I conclude, that this ſpar was fixed on the perpendicular ſide of a fiſſure; that the juice of this incruſtation was of the *ſtalactites* kind, and, proceeding from the ſame cauſe, deſcended in a ſimilar direction.

Fig. 72. is a bunch of ſemi-pellucid ſpar, ſhot into reclined cones, making an angle of 30°, with the ſurface of the ſtone; the ſides of theſe cones are a very curious fretwork of little ſpires or briſtles, many of them ſharp as the ſmalleſt needle, and pointing nearly in the ſame direction, as the cone on which they riſe. The ſurface of theſe ſhoots is of a ferrugineous tint, but their inner ſubſtance pellucid, very little ſhort of that ſpar, which for it's clearneſs is called Cryſtal, and more tranſparent than many hexagonal ſhoots; 'tis the only one I have ſeen of it's kind. Fig 72.

Fig. 75. is an aſteriſk of the cleareſt ſpar; it's ſhoots or rays are hexagonal, ſwelling, or gibbous, in the middle; their ſides not plain, or of one level ſurface, as our *Corniſh Diamonds* generally are, but ridged near the edges, and ſomewhat hollow, but not uniformly, in the middle; the points or terminations were entire and ſharp, but not to that degree aculeated as the former conic ſpar; but it is very plain, that theſe ſpires never had any hexagonal apices: the undermoſt ſhoots ſpread horizontally; but the other riſe gradually, making a greater angle, till the middle ones make nearly a right angle with the baſe, which has a ferrugineous circular ſpot in the middle, from whence the rays regularly proceed on every ſide. Fig 75.

Fig. 76.

Theſe are ſome of the moſt ſingular ſpars which have reached my obſervation; but the general ſhape of our figured ſpars is hexagonal; and theſe hexagons either conſiſt of a ſhaft or column, and a point with the ſame number of ſides correſpondent to the column, or are only points, that is, pyramidal hexagons ſtuck on at their baſe, upon the ſurface of their ſtony beds. Fig. 77. Fig. 78.

In a thin cake or lump in my poſſeſſion, one half of the ſpar ſhoots from one ſide, the other half from the other; and ſo incruſts the planes of both ſides with hexagonal *apices*. Fig. 79.

Here the ſpar (as appears by the tendency of it's fibres, when the interior texture is examined) ſtruggled to form ſtems or ſtalks to theſe *cuſpides*; but either the effort was not vigorous enough, or, through the impurities of the juice, the ſparry or cryſtalline principles had no room to extend or protrude themſelves into the ſhape they ſeem inclinable unto; ſo they lie blended, and their lineaments ſcarcely to be diſtinguiſhed from the general maſs.

Some ſpars riſe out of the general ſurface, into large orbicular bliſters, thick ſet with hexagonal points diverging, as rays from a center. Fig 80.

Theſe bliſters or protuberances are in other ſpars ſubdivided into numbers of other ſmall, orbicular excreſcencies, and the *cuſpides* very ſmall, but, like the pointed ſhoots of moſt *Corniſh* ſpars, hexagonal. Fig. 81.

At

At the root, or where they join to the rock, theſe globular maſſes ſhew in what direction the juice exerted itſelf, ſpringing commonly, as from one general center, and extending itſelf equably on every ſide. When the juice is ſimple, and of one ſort only, the rays are continued Fig. 82. from the center to the extremities. But when the juice is of two or more different mixtures and impregnations (which will generally appear from the different colours and degrees of tranſparency), then the effort is various and ſucceſſive, protruding the juice according, and in proportion to the different activity of the *ſtamina*, of which it conſiſts; and in both theſe caſes I find the coarſeſt and moſt terrene part of the ſparry lump next the center, and the moſt tranſparent and pureſt ſhot forth, to form the pyramidal *cuſpides* of the circumference. Though the effort is various and multiple, the ſeveral juices preſerve a parallelíſm to each other, and to the extremity, each juice proceeding no farther than it's own impregnation would carry it; and therefore ſettling in parallel liſts or lines behind each other, and their angles leſs and leſs perfect, that is, becoming more obtuſe, till you come to the rock, or lifeleſs lump of ſpar, which the effort had no power to move, and thro' which the impregnated and purer juices eſcaped, according to their degree of mobility: the baſe of one of theſe orbicular lumps, which has ſeven diſtinct liſts or fillets one within the other, beſides leſſer liſts, Fig. 83. marked (*b*), will explain what is here ſuggeſted †.

Theſe hexagonal points do not always fit cloſe to the body of the rock, but are as often found mounted upon columnar ſhoots of the ſame number of ſides; and theſe are what are commonly called *Corniſh Diamonds:* they are generally found larger ſomewhat at the baſe, where they faſten on the rock, than at the top, where they ſupport the *cuſpis*.

Fig. 84. Some of theſe ſhoots have alſo hexagonal points at each end, and are Fig. 85. ſometimes found ſingle, that is, detached, and without a root, as the Naturaliſts ſay; but I have them alſo in lumps fixed ſide by ſide, but Fig. 86. in no parallel direction (13).

This is the general and moſt common appearance of our *Corniſh* figured ſpars and cryſtals, *viz.* either hexagonal points on the rock, or common ſpar, or fixed on ſhoots or columns of the ſame figure; but we muſt not imagine, that ſpar aſſumes no other ſhape, but what has been here mentioned. There are alſo trigonal and cubical ſpars; but of theſe ſorts I have not yet ſeen any in *Cornwall*; however, as our

† *a*. The cleareſt cryſtal.
b. Sup-pellucid, inclining to purple.
c. Flock-white, not pellucid.
d. Large fillet of purpled ſpar.
e. Flock-white.
f. Small fillet of purpled ſpar.
g. Flock-white.
h. Liſts of ſpar leſs diſtinct.
i. That ſide on which the effort was faint, and the ſhoots ſcarce perceivable.

(13) Theſe ſhoots are not always ſtrait, but are found ſometimes bent or crooked; but as this deformity is owing to ſome accidental interpoſing force or obſtruction, during the time of forcing, it will not, I apprehend, make the body of a different ſpecies, as long as all the other common properties are continued.


Fig. 61.
Fig. 64.
e
b
d
c
C
A
B
Fig. 63.
a
Fig. 65.
Fig. 67.
Fig. 62.
Fig. 68.
Fig. 66.

obſervations in this inquiſitive age are daily growing more extenſive, it is very probable, that new and undeſcribed ſhapes of ſpars may often fall under our notice: what I have here mentioned ſeem moſt worth notice; but to pretend to number them all, would be very extravagant; for they are varying every day upon our hands, and new mines throw forth new forms, according to the different combinations of their ſolids, and the impregnation of their waters.

It has been obſerved before, (*Sect.* 3.) that water and the ſparry juice, conſidered ſimply, without any other aid, will produce only the irregular, ſhapeleſs maſſes of ſpar, incapable of any activity, or ſtruggle towards ſhape and figure, and determined only by the common principle of gravitation, to that poſition in which we find it: we muſt call in therefore the aſſiſtance of ſome other principle, to account for the rectilineal regular phænomena of theſe ſparry productions. Sect. 4 *Whence the hexagonal ſhape of Spars.*

As the general appearance of our *Corniſh* ſpars is hexagonal and uniform, there muſt be ſome one general principle to which this great uniformity is to be referred. If theſe ſpars owed their form to any metallic principle, that metal would be found, when the fluor was thrown off by fire: but the contrary is apparent, the cleareſt ſpars yielding no metal. And, indeed, it may here be obſerved, that when the ſtony juice meets and coaleſces with any metallic particles, that juice ſhoots not into it's natural form, but by tin is forced into priſms, and various *ſpeculums*; by lead, into cubes; and by other metals into other forms: ſo that it never retains it's hexagonal ſhape, but when free of metals. As therefore there is no metal in our regularly-figured ſpars, we muſt have recourſe to another origination; and ſalt, as I take it, is moſt likely to be that active principle, by whoſe force the fluid in which it is mixed, be it pure water, or lapideous juice, is made to ſhoot forth into regular rectilineal maſſes, agreeable to the original ſhape and figuration in which theſe ſalts were firſt created. 'Tis by the force of ſalts that liquid bodies are thrown into all the geometrical planes, angles, and more compounded ſhapes, the variety of which is no leſs ſurprizing, than the conſtancy and uniformity of each particular ſpecies; the ſame ſalt ſhooting ſtill into the ſame figure (as is plain from all artificial cryſtallizations), when not ſtreightened in room, or otherwiſe determined by heterogeneous mixtures. To produce ſalt from any liquid body, two things are requiſite: firſt, that the redundant liquor, in which the ſaline particles are kept too much diſperſed, and too remote to attract each other, be diſcharged (which is uſually performed by evaporation), and that the remainder be expoſed to a colder air. This ſimple plain proceſs will produce all the varieties of cryſtallization; the ſalts contained will ſhoot into their peculiar forms, pointing forth their darts, regular planes, or ſpires, into ſuch figures as are proper either to their native or compounded ſalts. From this eaſy and inconteſtable procedure of liquids into figured and ſolid bodies (to which nothing more is required than heat and cold), may it not appear probable, that ſomething like this has happened, and does ſtill happen, among our ſpar-loads in the mine? For inſtance: when the juice of ſpar, impregnated ſtrongly with ſalts, which have been from time to time imbibed, is ſufficiently drained from the water (which not only collected the ſparry maſs, but kept it in a fluid ſtate), either by natural heat, ſo common in mines, or by the water's running off into crevices, where the ſtiffer ſtone-juice cannot follow it; in other words, when the water deſerts the ſpar; the ſpar, as ſoon as a colder air ſucceeds (14), ſhoots, and is protruded into figures by the ſalt which it contains (15); and thus it happens that we have ſuch figured bodies from the ſpar, which, without thoſe ſalts, would ſhew us no ſuch ſhoots as we call *Corniſh* Diamonds, but fix quietly into ſheets, and even plates, or drop down wherever it's own weight would carry it.

What ſort of ſalt it is, which inclines ſpar to this hexagonal form, is the next thing to be inquired into; and moſt probably will appear to be that of nitre, if we conſider that the reſemblance betwixt the figure of what we call *Corniſh* Diamonds, and that of the pure unmixed nitre, is ſo great, that no two things can be more exactly alike. "The known "figure of nitre, ſays *Grew* (*Coſmol. p.* 15.) is a ſexangular priſm." "Particulas nitri *Liſterus* deprehendit ſexangulas, tenues, longas, late-"ribus parallelogrammis, & ex altera parte in pyramidale acumen "deſinentes." *Phyſ. Cler.* 8*vo*, *tert. Edit. p.* 150. This exact reſemblance is ſufficient to make us conjecture, that theſe ſparry productions may owe their general figure to a nitrous ſalt, which exerted itſelf at the time when the juice of ſpar became ſtone; and I ſhall endeavour to ſupport this conjecture only by one authority, which is that of the curious, and, in the Studies of Natural Hiſtory, indefatigable *Linnæus*, which he favoured me with in anſwer to ſome queries, jointly with the opinion of the preſent Dr *John Fred. Gronovius* of *Leyden*. "The "origine of thoſe cryſtals (16) is a moſt intricate thing; but you may "conclude—*quod omnis cryſtallizatio a ſale, quod cryſtalli gaudent figura* "*nitri, quodque omnes generentur in cavo: hi (viz. cryſtalli) quo magis* "*ſimplices, eo magis puri & pellucidi: hinc nitro originem debent, quem-*"*admodum gemmæ iſtæ, quæ priſmaticam nitri figuram exhibent.*"

As nitre may be reaſonably conjectured to give the ordinary and general hexagonal figure to cryſtals, it may be as juſtly inferred, that when they depart from this uniformity, it is owing to ſome mineral,

(14) "*Cryſtallus eſt ſuccus, quem frigus intra terram conglutinavit.*" Agric. p. 282.

(15) Mr *Boyle*'s opinion is, that ſuch ſtones, (*viz.* Spars and Cryſtals) were originally in a fluid ſtate; that the figure of them is determinate and geometrical, like the cryſtals produced by alum, nitre, vitriol, in water; and their texture like the congelations of ſalt produced in cryſtallization by cold.

Grew (*Coſmol. p.* 14.) after talking of the regularity of forms, and the ſalts of bodies, proceeds thus: "Arguing (ſays he) that the atoms of the lapidific, as well as of the ſaline "principle, being regular, do therefore concur in producing regular ſtones."

(16) *Corniſh* Diamonds, ſent to Dr *Gronovius* from *Cornwall*.

earthy,

earthy, or metallic mixture, ſome heterogeneous ſalt, which impedes the nitre in it's ſhooting, and turns it into trigonal, cubical, conic, or other unuſual figures.

The next thing to be conſidered, is the cauſe to which the different ſize in which theſe bodies do appear may be owing. Some cryſtals are ſaid to be a cubit high: *Livia Auguſta* dedicated one in the *Capitol* of fifty pounds weight (17); and Dr *Iſaac Lawſon*, late phyſician to the army in *Flanders*, informed me, that he ſaw a cryſtal in a foreign mine, with it's edges well preſerved, which he believed might weigh about 200 pounds. Dr *Woodward* (18) reckons, among his *Corniſh* diamonds, a ſingle column or ſhoot very large, if it be three inches in length, and 1½ inch in diameter near the baſe. The largeſt I have yet ſeen in *Cornwall* weighs ſomewhat more than 3 pounds, is about 10 inches in girt at the largeſt end, and more than ſeven high; from which ſize there are of all degrees, down to the bigneſs of a ſmall pin. As the ſize of this laſt mentioned is very unuſual, I have given a drawing of it.

Sect. 5. *Of their ſize.*

Fig. 87.

The largeſt proceed out of a large courſe or load of ſpar; but the ſmalleſt of all from ſmall bits or lumps of ſpar; and the ſmall pyramidal *apices* are generally ſtuck on upon the ſide of the large ones, ſometimes in diſtant ſpangles, other times in thin and broken incruſtations. Now the cauſe of theſe different ſizes ſeems to be this: wherever great maſſes of the ſparry juice have happened at the ſame time to be in a ſtate of fluidity, the exuberant water drained off ſuddenly, and conſequently left cavity ſufficient for the ſalts to extend themſelves, there the great quantities of nitrous ſalts mixed with the lapideous juice incline it to ſhoot vigorouſly, and form large cryſtallizations; and from proportionably leſſer maſſes, whilſt they are indurating, proceed leſſer diamonds. If the whole maſs be impregnated with nitrous ſalts, the whole ſurface of the rock ſhall riſe into points or ſpires (19), according as the maſs is more free or more ſtubborn to comply with the agitation. If the nitrous ſalts are not intimately mixed, but ſwim in clouds and bunches, thoſe lumps only, where the nitre is, ſhall be ſhot, and the reſt be plain. This is the caſe when the ſparry juice gets into it's proper *nidus*, or reſts in it's fiſſure. But where ſmall ſcattered quantities of this ſtony juice circulate in the ſubterraneous waters, ſome trickle down the ſides of ſtones and fiſſures, and already-formed diamonds, and, ſticking in little globules, form a cruſt by juxta-poſition, whoſe points are ſexangular, their columns ſhort, ſometimes crooked and unfiniſhed. At other times water, charged with theſe ſparry juices, falling from higher into lower parts of the mine, cannot but be daſhed and diſperſed about the cavity in all directions; and thus it is, perhaps,

(17) *Pliny, lib.* 37. *c.* 2. Sir *Hans Sloane* has one block of cryſtal which weighs between 40 and 50 pounds, and another about 20, quite clear and regular. *C. M.*

(18) *Catal.* 158. *f.* 98.

(19) See *Fig.* 72, 75, 79, 82.

that the distant spangles, like the dew or mist that rises from a cascade sprinkled on the surfaces of all bodies in their reach, are there congealed, and shot by their salts.

Sect. 6. *Of their colour.* Spars are of different colours, and different degrees of transparency; some yellow, some reddish, brown, green, purple, black, some of a cloudy fleecy white, some freckled with little specks of various colours and magnitudes, and others of a water not inferior to the purest crystals. The yellow is supposed to be indebted for it's tinge to sulphur and iron, or lead, or both; the red to iron, and perhaps *gossan*, that general companion of copper; green, to the solutions or rust of copper; copper will also probably impart it's purple (for of that colour we find some of our most beautiful copper ores) to the juices near it: black may possibly be indebted to copper also of like colour, to tin, or the particles of coal; but the most transparent owe that advantage to the purity and simplicity of the juices of which they are formed. What this purity is owing to, cannot be so easily determined. Some think to percolation, or straining through the pores of other bodies, the lapideous juice depositing the sediment and impurities, which it may have contracted in it's passage. Now, it is not impossible but that the water, and that liquid spar, of which these bodies are principally formed, passing by their own weight through a soft, porous, sandy stone in the *Oxford*, *Bath*, and other-like quarries, may undergo a change for the better, and acquire a greater degree of transparency; but it cannot be so with our spar, on which we find the crystals above-mentioned: for, Fig. 79. besides that these crystals are found on both sides the stone, (which, in the procedure of percolation, could never happen), and in very large shoots, our spar will no more transpire or exude than glass, it is of such consistency and hardness: so that whatever filtration has happened to these crystals, must have befallen them during a former percolation, before they rested in their present beds, not from any sweating through that bed in which we find them, as Dr *Plott* imagines (20).

Crystals therefore, it is certain, owe their transparency and purity to the simple state of the juices that form them; but to what that state and condition is owing is uncertain. Whether it may be to some purifying menstruum or spirit, that precipitates every kind of sediment, I do not presume to say: I shall only observe, that in *Cornwall* the clearest diamonds are for the most part found in a dry, lax, sandy soil, where no dirty or dark-coloured loam, mineral, or opake stones prevail: so we may conclude in general, that if, during the fluidity of these bodies, no metallic or mineral fume, no dust, clay, or sand, was imbibed, the water and lapideous juice make up a clear pellucid mass. If the case was otherwise, whatever impurities the waters contracted, and had not at the time of forming discharged, are still to be seen in the stone.

(20) *Oxfordsh.* p. 98. and *Ramundus in Alonzo Barba*, p. 36.

Sect. 7. Of their hardness.

As to the hardness of our *Cornish* crystals, all I have to observe is, that they cut well into seals, when they have no flaws: their natural points also will cut glass; but not freely or deep; in which particular they fall much short of the true diamond.

Conjectures relating to the properties of the true Diamond.

Of this I shall not pretend to assign any other reason, than that the true diamond seems to have more lapideous juice included, and more intimately and congenially united under an equal surface, than any other body in the world. It has also very little salt in it, as Dr *Grew* observes (*Cosmol. p.* 14.); and his opinion is confirmed by it's being found in such small masses; and by it's great weight it can have little water; both which observations are supported by it's great resistance, and almost immutability in fire: so that the true diamond has little salt, and little water, consisting almost entirely of stony juice concreted; to which properties it's great hardness may therefore be attributed: whereas in our diamonds there is much salt, and much water (comparatively speaking); which two ingredients, mixed with the lapideous juice, may incline those bodies to be more friable and tender, and deprive them of that hardness, which a less-reduced lapidific juice would certainly have had. This seems to me the real cause of the true diamond's hardness, and of our false ones falling as much short of it in this particular, as in lustre: but I must acknowledge, that, for want of sufficient experiments relating to both, I cannot decide peremptorily. However, by weighing the *Cornish* diamonds in water, I find they are generally to the weight of our common water, at a medium, as $10\frac{1}{2}$ is to 4; and I apprehend, that if they had more lapideous particles, they would weigh more, as they find the true diamond really does *. I find also the clearest and brightest *Cornish* diamonds weigh much heavier than the other which are more shady and opake. That they have much salt also, may be concluded from their being projected sometimes into such large, regular, hexagonal columns.

Sect. 8. Of the texture of Cornish *Diamonds.*

There are some little varieties in the texture of our *Cornish* diamonds, which are sometimes to be observed in their broken sides and edges; but always, and more distinctly, in their base. Some are uniform, of one colour and transparency throughout; some have hexagonal sheaths described one within another, as in *Fig.* 88. In the first case, the shotten juice was of one and the same nature and consistency, equally impregnated, and the production of one effort; in the latter case, where the sheaths are one within another, it is also the product of one effort or shoot, the concretion of one and the same time, as the tremulous undulations upon the surface of water, on throwing in a stone, are all the offspring of one force, tho' the first be strongest, and the rest gradually fainter and less distinct: but the juices being differently mixed, gave way to the effort in proportion to their sensibility of the impreg- Fig. 88.

* The weight of *Crystals* to water is as $2\frac{1}{2}$ to 1. of *Diamonds* as $3\frac{1}{2}$ to 1. See these Trans. N°. 488. p. 451. *C. M.*

nation,

nation, the most agile flying off to the greatest distance from the center, and the most mixed, coarsest, and most opake, remaining nearest the center (21). That this is the true process, and that those different sheaths are the produce of one effort or birth, and not formed at different times, and in succession, as *Steno* and *Aldrovandus*, and some others, say (22), is plain, I think, from the two following observations. If the juice which forms these sheaths was impregnated and shot at different times, it would not form in sheaths round what appears to be the central or master-shoot, but would project itself into it's natural hexaedral figure. If it were not impregnated, but mere spar only, without nitre, or any other active principle, it would form itself, as the stalactites, in a pendulous undulating direction, drop or wave upon wave; and in this case these sheaths could never surround, in that neat and exact manner, the central shoot; but would be found only on the under part of that shoot, where it's own gravity would inevitably carry it: for it would be observed immediately, that these diamonds in the mine point forth in all directions; which must consequently prevent every unimpregnated juice from hardening into a regular uniform sheath.

It is also to be noted, that these sheaths are often found broken and interrupted: but this cannot be owing to their being *gemmæ inchoatæ & non perfectæ* (23), for the above-mentioned reasons, but to the insufficiency and want of juice, or to some accidental impediments of stone, earth, or sand, which make those breaks, and prevent it from forming a complete sheath.

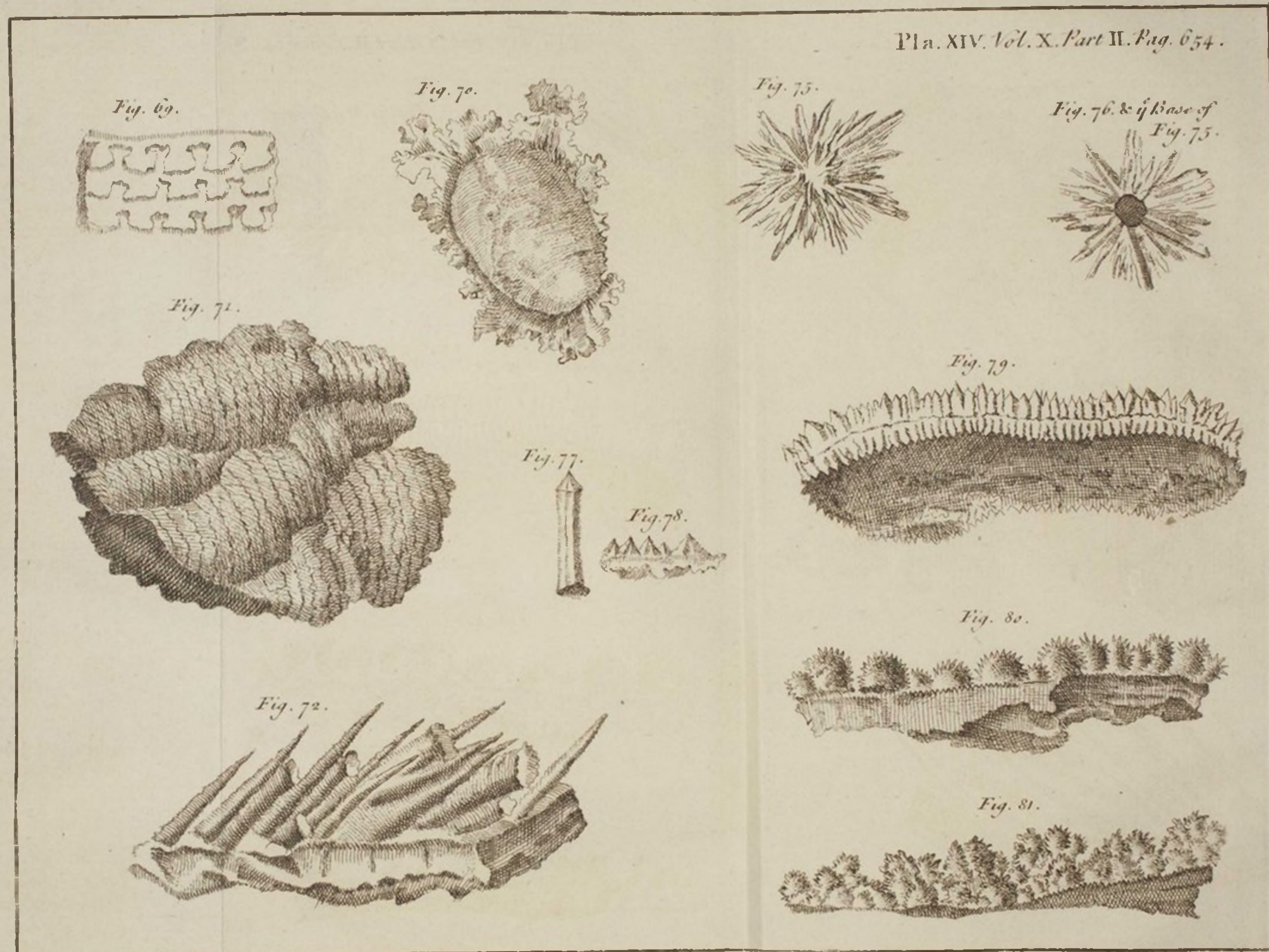
Sect. 9 *Of their direction in the* Mine.

Not long since I went with a friend or two into a mine called *Pillion Erth*, in the parish of *St Just*, on purpose to survey the *Cornish* crystals in their natural situation. The cave, to which we were introduced, was not much larger than a common baker's oven, and much of that figure. We had two candles with us, by means of which we saw the roof, which might, in the middle, be about five feet high from the floor; in the other parts not so much. The roof was the most surprizing piece of fretwork imaginable, and consisted intirely of spar shot into *Cornish* diamonds, of which the large one (*Fig.* 87) was a part. I could not discern any coveting a position exactly perpendicular to the horizon; but in every indifferent direction they pointed forth very plentifully of several sizes; sometimes in groupes and clusters, sometimes single, now crossing each other, and now standing by each other with parallel sides: some were smooth, shining and clear, others rough and opake; some veined with red, like porphyry; others speckled thick with the smallest spots of deep purple, and a bluish cast: but the finest of all were those which had innumerable little diamonds or sparks (of

Fig. 87.

(21) See *Fig.* 83. and it's description, *p.* 648.
(22) See *Plott's Oxf.* p. 98.
(23) As *Plott*, *ibid. ut supra.*

the


Pla. XIV. Vol. X. Part II. Pag. 654.
Fig. 69.
Fig. 70.
Fig. 75.
Fig. 76. & ye Base of Fig. 75.
Fig. 71.
Fig. 79.
Fig. 77.
Fig. 78.
Fig. 80.
Fig. 72.
Fig. 81.

the cleareſt water) ſtuck upon their ſides, and, by means of the candle, had a luſtre not to be conceived. We gazed here in this incommodious, but beautiful little cave, till we could no longer hold up our heads; and then crept out as we came in, upon our knees.

Now, among the great numbers of ſparry productions which I ſaw in this mine, I could not obſerve, but that they pointed indifferently in all manner of directions; which I ſuppoſe owing to the great unevenneſs of the ſurface on which the baſes of theſe little columns were fixed; and for the ſame reaſon I doubt not, but in all concave beds the points converge, as in all convex they turn the contrary way, and diverge, as from a center: ſo that the natural cauſe of theſe different directions is probably no other than the accidental form of that general maſs from whence theſe ſhoots proceed. I would ſay, that as the figure and regularity of the lapidific impregnated matter happens to be when drained of all ſuperfluous moiſture, and by the cold, heat, or dryth, diſpoſed to ſhoot, ſo will the tendency of the ſhoots, both column and point, be. If the concave part (for inſtance) of the *voluta* of the *cornu-ammonis* be ſet with cryſtals, their points muſt tend inwards to the center, where the tail ends, being thereto compelled by the regular contour of the ſhell; and indeed they are ſo in fact, as ſee *Fig*. 89. But if the cryſtal juice chance to fix on, or proceed from a convex bed (to which it's *nidus*, and other cauſes, may contribute), or be itſelf an orbicular lump, and equally impregnated on all ſides; then will the fibres ſpring as from one common center, and at their extremities point forth their *cuſpides* in a circular figure, as in *Fig*. 90.*

Fig. 89.

Fig. 90.

If there be a thin plate of ſpar equally expoſed to cold on each ſide, and having equal room and force to ſhoot, it will throw forth it's points on each ſide; and the line from whence the fibres began to ſpring ſhall be exactly in the middle of ſuch plate of ſpar, and the figures ſhall cut the ſaid line at right angles, as in *Fig*. 79. whence it ſeems reaſonable to conclude, that the direction in which theſe ſparry productions ſhoot forth is generally rectangular, or very near it, to the beds or maſſes from which they proceed; and that all the ſeeming confuſion in pointings of the *Corniſh* diamonds in *Pillion Erth*, was owing to the great variety of planes and ſurfaces, into which that large body of ſpar was hardening, when theſe diamonds ſhot from it's extremities.

A letter from the Rev. Char. Lyttelton, *L. L. D. and F. R. S. Dean of Exeter to*

XXVII. 1. The curious foſſil I now exhibit to the *Society*, is as rare as it's figure is elegant; having been mentioned by none of our own writers who treat on foſſils, and but very imperfectly deſcribed by foreign Lithographiſts †.

I diſcovered

* See *Fig*. 76.

† I ſuppoſe the *Dean* means Dr *Bruckmann*, and the late Mr *Linck*, an eminent Apothecary at *Leipſic*: for Dr *Bruckmann*, in his *Centuria Epiſt. Itinerar. Wolffenbuttl.* 1742. 4to, Epiſt. XXIII. has given ſeveral figures of petrifactions, very much reſembling theſe

Dudley

the Pref concerning a non-defcript petrified infect. N°. 496. p. 598. Nov. &c 1750. *Read* Dec. 20. 1750. Fig. 91, 92, 93, 94.

I difcovered a fingle fpecimen of it (*Fig.* 91, 92, and 93.) laft yea in the limeftone pits at *Dudley* in *Worcefterfhire*; and very lately a larg mafs of limeftone (*Fig.* 94.) full of them in the fame place; both which are now fubmitted to the infpection of this Learned Body, who are beft able to determine to what clafs of the animal kingdom it properly belongs.

Extract of a letter from the fame: to C. Mortimer, *Secret. R. S.* Ibid. p. 600.

2. The Rev. Dr *Shaw*, of *Oxford*, has procured a fpecimen of the extended *eruca*. As the Foffilifts differed in their opinion of this *Dudley* foffil, fome pronouncing it an *eruca*, others a *bivalve*, I thought it beft to leave the reader to judge for himfelf from the engravings; but, as we are now able to add a fpecimen of this foffil in an extended pofture, there is a better pretence to call it an *eruca*.

Some further account of the before mentioned Dudley Foffil; *by the* Editor *of thefe* Tranfactions. Ibid.

3. The Rev. Dr *Pocock*, F. R. S. was fo obliging as to fend feveral fpecimens of this foffil to the *Pref.* who put them into my hands, and defired me to draw up an account of them to be annexed to the preceding paper.

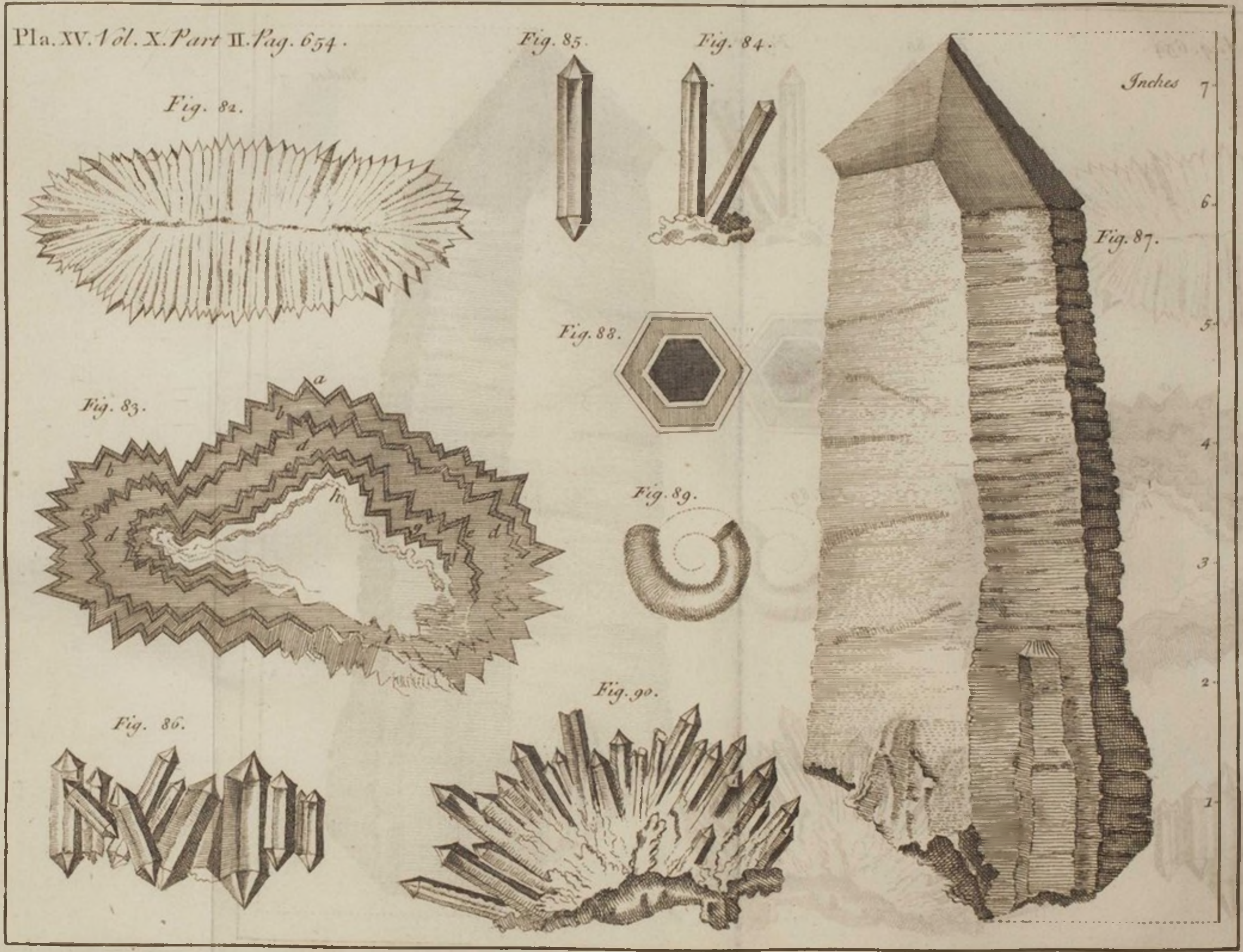
The firft fpecimen is a mafs of ftone containing the face and eyes, with fome rudiments of legs on the fides; but the back is intirely broken away. Another fpecimen contains the head only: a third, the head, and part of the back, but greatly diftorted. But the moft beautiful and complete are the two which I caufed to be drawn and engraven in *Fig.* 98, 99, 100, 101.

Fig. 98, 99, 100, 101.

At *Fig.* 98. is one of thefe infects completely extended at it's whole length; wherein it appears, that the head is covered with a fhell or cruft confifting of 3 parts; the middle part is broad and round, *a.* which I fhall therefore call the nofe: the two fide pieces are of a triangular form, *b. b.* in each of which is fituated a large protuberant eye, *c. c.* The anterior part of the whole is encompaffed by a round border, *d. d. d.* which looks like an upper lip; tho' I do not take it to be fo; but that the mouth is fituated lower down, as in the crab-kind, and does not appear in any of the fpecimens I have yet feen. On each fide the crown of the head, towards the back part of it, are two fmall knobs, *e. e.* At *f. f.* in *Fig.* 99. appear fome traces of feet, which feem to lie under the belly: but, as the belly, or under fide, was not diftinct, not being cleared from it's ftony and earthy matter, I could not difcern any other legs.

Dudley Foffils; the firft was found at *Steme*, a village in the neighbourhood of *Paderborn*, given him by Dr *Kœnig*, which he took for a fort of *polypus marinus*; he fays it is an animal unknown to him, but he gives thofe figures of it, in hopes that fome curious perfons, who live near the fea, may light upon fome animal refembling this. The body of this ftone, he fays, has, on each fide three ftriated lobes, and three pointed *appendices* beneath; it's inner fubftance is white, being *felenites*, or white fpar; it's colour on the outfide is every-where brown. His friend *Linck* had fent him fpecimens of thefe ftones 6 years before, fome modelled in wax, others engraven upon copper. *C. M.*

It


Pla. XV. Vol. X. Part II. Pag. 654.
Fig. 82.
Fig. 83.
Fig. 85.
Fig. 84.
Fig. 88.
Fig. 89.
Fig. 86.
Fig. 90.
Fig. 87.
Inches
7
6
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4
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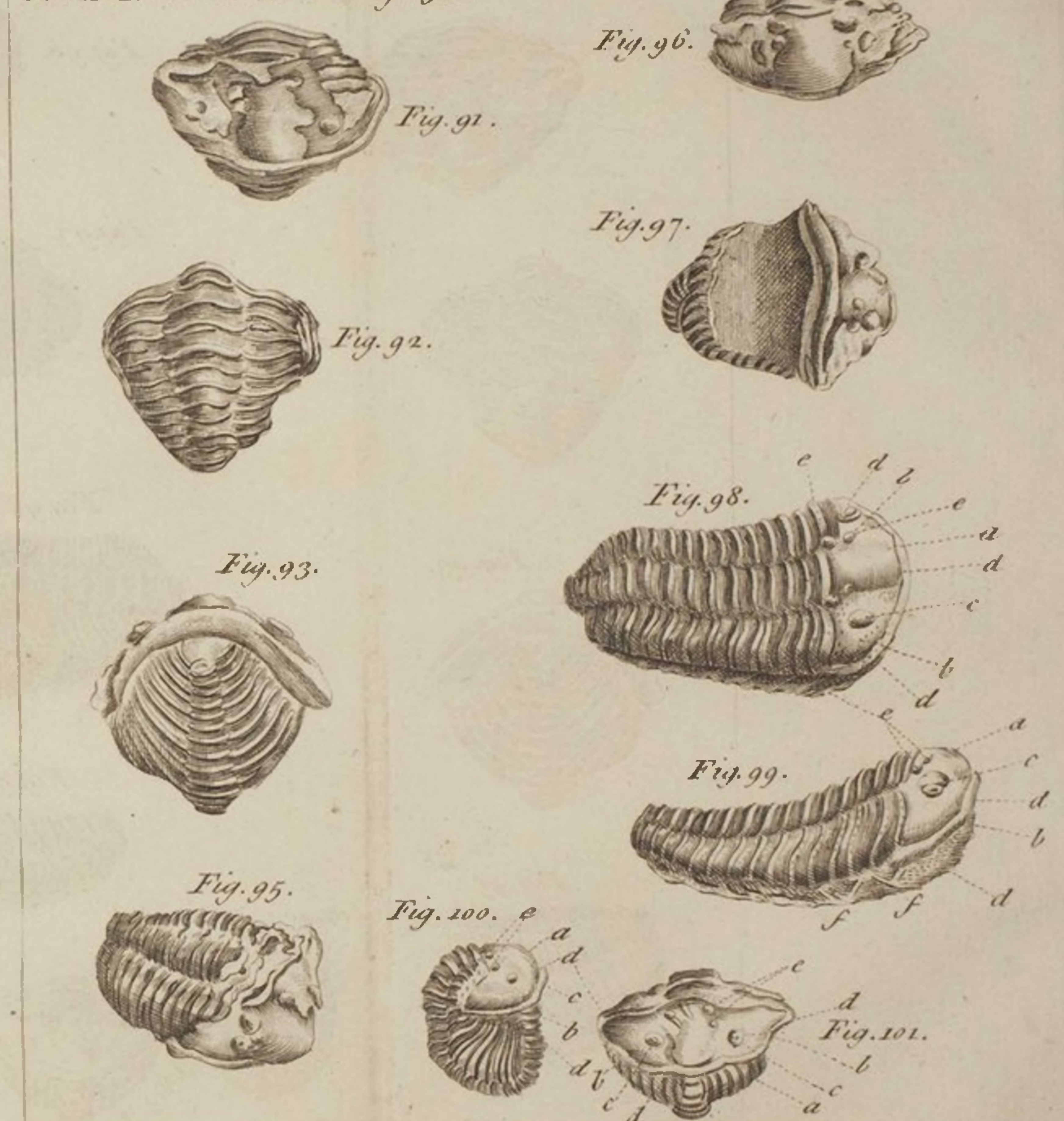
1

Pla. XVI. Vol. X. Part II. Pag. 656.



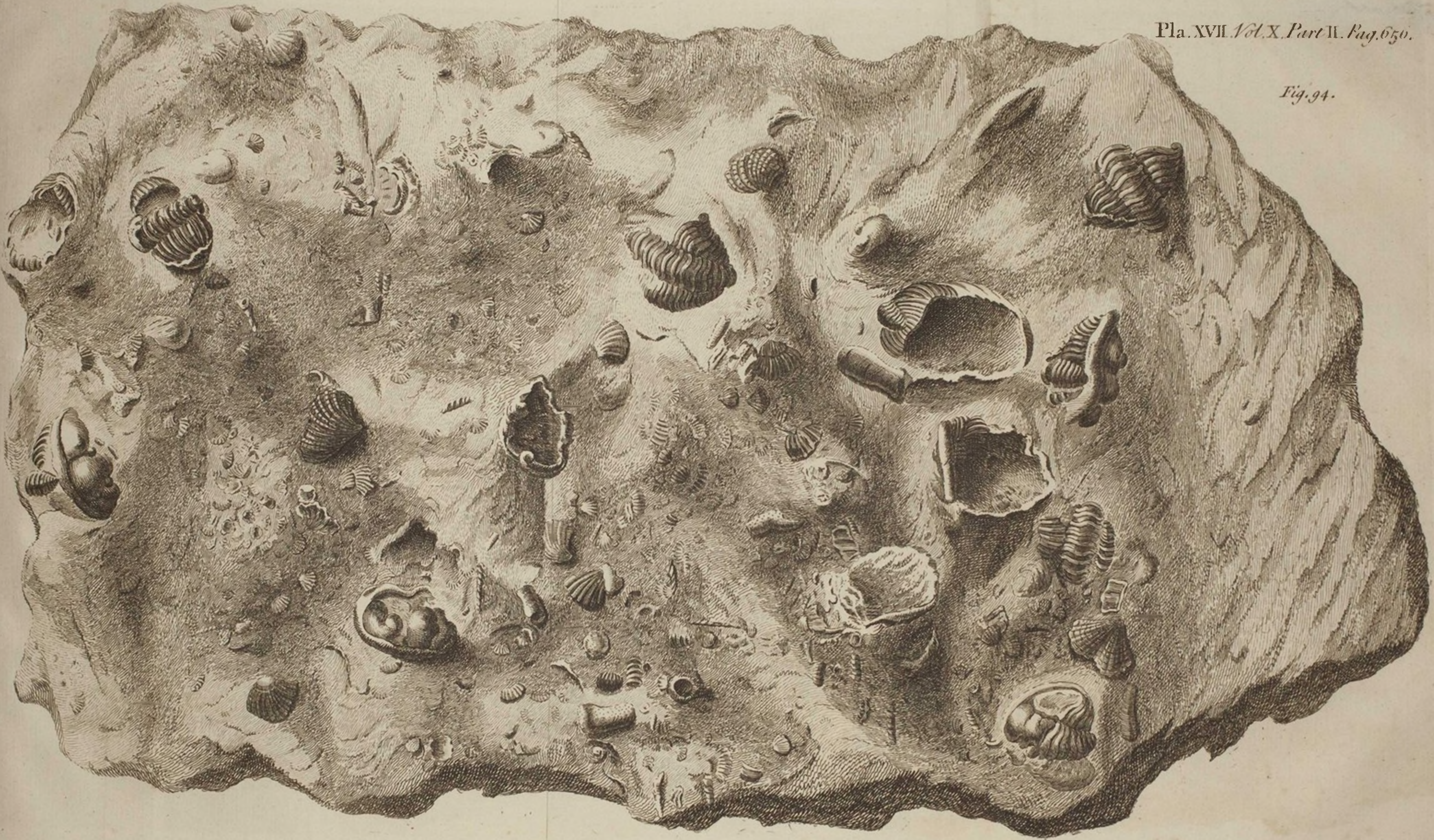


Fig. 94.

It is most likely the whole back of this creature was, when alive, covered with a case, or undivided *elytrum*, as is the *Scolopendra aquatica scutata*, described by M. *Klein*, of *Dantzick* *; and afterwards by the Rev. Mr *Littleton Brown*. M. *Klein* says, the case was whole; and that he was forced to slit it open to shew the back underneath; when it appears, that the body was trilobated, as in *Fig.* 78. The case, being very thin and tender, may probably have been broken off at the death of the animal, before it's being petrified.

Mr *Brown* does not mention in his insect the property of rolling itself up, which this certainly had; as appears by several of the figures, as *Fig.* 91, 92, 93, 96, 100, and 101, which are intirely rolled up; and as is more particularly represented by *Fig.* 100 and 101, in which it appears, that the tail is turned up under the belly quite to the mouth; and at *Fig.* 97. the creature seems but half rolled up.

I have consulted all the books I could meet with, which give figures of insects and crustaceous animals in their natural and petrified states; and find none resemble this *Dudley* fossil so near as M. *Klein*'s insect; therefore I shall, till we get more information, call it, *Scolopendræ aquaticæ scutatæ affine animal petrifactum.*

An account of a treatise by Wm. Brownrigg, *M. D. F. R. S intituled,* "The "Art of making common Salt, "as now "practised in "most parts "of the "world; "with several Im"provements "proposed in "that Art, "for the Use "of the *British* Domi"nions;" *abstracted by* W. Watson, *F. R. S.* N°. 487. p. 351. April &c. 1748. *Read* June 15. 1748.

XXVIII. This work consists of 295 pages in 8vo, exclusive of the preface, and of 6 copper-plates, exhibiting different views of salt-houses, instruments, *&c.* necessary to the preparation of salt. It is enriched likewise with notes of great importance to the work, not only of the author, but also from the *Philos. Trans. Medical Essays*, *Mem. of the R. Acad. of Sc. at Paris*, *Pliny*, *Agricola*, *Alonso Barba*, *Ramusio*, *Boyle*, *Hoffman*, *Lister*, *Herrera*, *Dampier*, *Baccius*, *Pomet*, *Marsilli*, *Plott*, *Scheuchzer*, *Hales*, *Rastel*, *Leigh*, *Boerhaave*, *Shaw*, and others.

Amongst the vulgar arts, that of preparing sea-salt for the uses of mankind hath been thought worthy the notice of many great and learned men, as well antient as modern. Thus many things relating to this art are recorded by *Cato* and *Pliny*, *Agricola* and *Hoffman*, to whom our author is much indebted for those memoirs that have been transmitted to us, relating to it's history. Had those great men been as diligent in improving this art, as they were in recording the improvements made therein by others, there would not now have been occasion to remark, that, after the practice of so many ages, an art so simple, and withal so necessary, hath not yet been brought to any degree of perfection.

That this art was capable of great improvements, especially as practised in *Great Britain*, was the sentiment of this *Society* soon after it's institution; at which time the members thereof were very intent upon bringing it to a greater perfection; as may be gathered from the inquiries and suggestions of Dr *Beal*, and the histories of several methods of making salt, which then were published by the *Society*. And although the *English* have, since that time, considerably improved their method

* See Vol. IX. Part iii. § iv.

 of

of boiling ſalt; yet this art is ſtill practiſed with greater ſkill and ſucceſs by the *Dutch*, as the ſuperior goodneſs of the fiſh, cured with their ſalt, doth ſufficiently prove.

The Commons of *Great Britain*, having taken into conſideration the great importance of this art, judged ſome improvements propoſed therein worthy their regard and encouragement; well knowing, that, could this be brought to the ſame perfection in *Britain* as in ſome neighbouring countries, large ſums of money might be ſaved in the nation, which are now paid to the *French* and others; it's fiſheries improved, and it's navies and commerce, and many of it's richeſt colonies, would no longer depend upon it's enemies for one of thoſe neceſſaries, without which they cannot be ſupported.

Theſe conſiderations have induced our author to give a brief account of the various methods of making ſalt, which are now uſed in *Great Britain*, and in other countries, where this art is practiſed with more ſucceſs; and alſo to attempt ſeveral further improvements for the uſe of the *Britiſh* dominions. How far he has ſucceeded in theſe attempts, will beſt appear, if the public ſhall think the following propoſals ſo far worthy their attention, as to merit a fair and impartial trial. The principal concluſions, deduced from a variety of obſervations and experiments, are as follows: 1. That, by the methods here propoſed, an excellent bay-ſalt may be made in *Britain* in very large quantities, ſo as to be afforded cheaper than at the prices paid for foreign ſalt; and that the *Britiſh* colonies in *America* may very commodiouſly be ſupplied with bay-ſalt of their own manufacture, without having recourſe for it to the *French*, *Spaniards*, and *Portugueſe*. 2. That, by the methods here propoſed, an excellent kind of refined white ſalt may be made in *Britain*, as well from ſea-water and rock-ſalt, as from natural brine, in any quantity wanted, ſo as to be afforded cheaper than foreign bay-ſalt; and which will alſo be better for curing fiſh, fleſh, and other proviſions.

In forming theſe concluſions, an impartial regard has been had to truth, without attending to the private advantage of any particular ſet of men. The ſenſe of this, together with a deſire of promoting the publick advantage, has induced our author to communicate the following ſheets at this time, although by deferring the publication ſome time longer he might have made them poſſibly more accurate; becauſe, beſides other conſiderations of no ſmall import, an opinion has prevailed, that the eſtabliſhing of fiſheries in the north of *Scotland* would be the beſt means of affording an uſeful employment to more uncivilized inhabitants of that part of the kingdom, for carrying on of which they are moſt commodiouſly ſituated.

What Mr *Lowndes* * hath lately done towards the improvement of brine-ſalt, may, perhaps by ſome, be thought to ſuperſede the neceſſity

* Mr *Lowndes*'s proceſs is inſerted in this Work. See *p.* 104, *&* *ſeq.*

of

of further attempts for improving and extending our ſalt manufacture. Dr *Brownrigg* is very far from depreciating the endeavours of that gentleman, which have met with parliamentary encouragement; and had his diſcovery appeared to the doctor ſufficiently complete and extenſive, he would not have given the public and himſelf this trouble. He makes no doubt but that the ſpecimen of ſalt, which Mr *Lowndes* exhibited before the College of Phyſicians, was a ſtrong and pure ſalt, ſince ſuch it appeared to that moſt learned Body. Whether the alum mixed with it (agreeable to the antient practice of the *Cheſhire* ſalt-boilers) contributed any thing to it's goodneſs, is more properly conſidered hereafter. It is only neceſſary here to obſerve, in juſtification of the preſent undertaking, that Mr *Lowndes*'s method of making ſalt for curing proviſions, doth not appear to be the beſt that may be put in practice; ſince our author hopes to ſhew, that, by other methods, a purer and a ſtronger ſalt may be made, and at a leſs expence. Neither is his method ſo general and extenſive as ſeems to be required for the public good; ſince Mr *Lowndes* confines it almoſt intirely to boiled brine-ſalt; and hath given no directions concerning the preparation of bay-ſalt. He indeed propoſes to meliorate the *Britiſh* ſea-ſalt, but ſeems to deſpair of preparing a ſalt either from ſea-water, or *Engliſh* rock-ſalt, fit for the uſes of the navy or fiſheries; although the *Dutch* ſalt, which is the ſtrongeſt and pureſt boiled ſalt now made, is entirely a marine ſalt, and even the brine, of which Mr *Lowndes* makes his ſalt, is only a ſolution of the *Engliſh* rock-ſalt, often in very impure water, as is well known to the Naturaliſts.

Our author, treating of ſalt in general, takes notice of the excellence and uſefulneſs thereof; and that it hath pleaſed the *Author* of Nature to provide mankind therewith in ſuch abundance, that there are few countries which do not afford vaſt quantities of rock or foſſil ſalt. Mines of it have been long diſcovered and wrought in *England*, *Spain*, *Italy*, *Germany*, *Hungary*, *Poland*, and other countries in *Europe*. Moreover the ſea affords ſuch vaſt plenty thereof, that all mankind might thence be ſupplied with quantities ſufficient for their occaſions. There are alſo innumerable ſprings, ponds, lakes, and rivers impregnated with common ſalt, from which the inhabitants of many countries are plentifully ſupplied herewith.

In ſome countries, which are remote from the ſea, and have little commerce, and which are not bleſſed with mines of ſalt, or ſalt waters, the neceſſities of the inhabitants have forced them to invent a method of extracting their common ſalt from the aſhes of vegetables.

In ſhort, this ſalt is diſperſed all over nature; it is treaſured up in the bowels of the earth; it impregnates the ocean; it deſcends in * rains; it fertilizes the ſoil; it ariſes in vegetables; and from them is conveyed into animals; ſo that it may well be eſteemed the univerſal condiment of nature.

* See *Boyle* on the ſaltneſs of the ſea.

Naturaliſts, obſerving the great variety of forms under which this ſalt appears, have thought fit to rank the ſeveral kinds of it under certain general claſſes, diſtinguiſhing it moſt uſually into rock or foſſil ſalt, ſea-ſalt, and brine or fountain-ſalt: to which may be added others of thoſe muriatic ſalts, which are found in vegetable or animal ſubſtances. Theſe ſeveral kinds of common ſalt often differ from each other in their outward form and appearance, or in ſuch accidental properties as they derive from the heterogeneous ſubſtances with which they are mixed; but, when perfectly pure, they have all the ſame qualities; ſo that Chemiſts, by the exacteſt inquiries, have not been able to diſcover any eſſential difference between them. In this our author agrees with the celebrated * *Hoffman*. Leaving therefore theſe diviſions to thoſe whom they may concern, it may for the preſent purpoſe be more proper to diſtinguiſh common ſalt after a different manner into the three following kinds; *viz.* into rock or native ſalt, bay-ſalt, and white ſalt.

By rock-ſalt †, or native ſalt, is underſtood all ſalt dug out of the earth, which hath not undergone any artificial preparation.

Under the title of bay-ſalt may be ranked all kinds of common ſalt extracted from the water, wherein it is diſſolved by means of the ſun's heat, and the operation of the air; whether the water, from which it is extracted, be ſea-water, or natural brine drawn from wells and ſprings, or ſalt water ſtagnating in ponds and lakes.

Under the title of white ſalt, or boiled ſalt, may be included all kinds of common ſalt extracted by coction from the water wherein it was diſſolved; whether this water be ſea-water, or the ſalt water of wells, fountains, lakes, or rivers; or water of any ſort impregnated with rock-ſalt, or other kinds of common ſalt.

The firſt of theſe kinds of ſalt is in ſeveral countries found ſo pure, that it ſerves for moſt domeſtic uſes, without any previous preparation, triture excepted. But the *Engliſh* foſſil ſalt is unfit for the uſes of the kitchen, until by ſolution and coction it is freed from ſeveral impurities, and reduced to white ſalt. The *Britiſh* white ſalt alſo is not ſo proper as ſeveral kinds of bay-ſalt for curing fiſh, and ſuch fleſh-meats as are intended for ſea proviſions, or for exportation into hot countries. So that, for theſe purpoſes, we are obliged, either wholly or in part, to

* *Hoffman* de ſalinibus *Hallenſ.* cap. viii.

Ut igitur noſtra hac de re innoteſcat ſententia, hanc interponimus; ſicuti in tota univerſi hujus orbis compage, una tantum eſt aqua, unus per fermentationem paratus ſpiritus ardens, unus Mercurius, unum volatile ſal, unum acidum nitroſum ac vitriolicum ſal; ita, pari ratione unum idemque ſal commune eſt. Sed quum plures alienæ, terreæ, lapidoſæ, ſulphureæ, calcariæ minerales ac pingues particulæ cum hiſce corporibus connubium ineant, diverſa exinde emergit eorum indoles; & ſal commune idem ſemper obtineret ingenium, ſiquis pingues terreas, calcareaſque partes ab illo artificioſe ſegregaret.

† By *Rock-Salt*, or *Sal Rupium*, the antient *Chemiſts* mean ſalt adhering to the rocks above the high water mark, being there lodged by the ſpray of the ſea, evaporated by the heat of the ſun; which is the pureſt ſalt of all for chemical uſes, and is to be had off the rocks of *Sicily*, and ſeveral iſlands in the *Weſt Indies*. C. *Mortimer*.

uſe

ufe bay-falt, which we purchafe in *France*, *Spain*, and other foreign countries. To remedy thefe inconveniencies this treatife was written, in order to fhew how the fubjects of *Great Britain* may be fupplied with falt of their own manufacture, fit and fufficient for all their occafions.

In order that the methods here propofed might be better underftood, and that the reafonablenefs of them might more fully appear, the author thought it neceffary to premife a brief account of the feveral ways of preparing bay-falt, as well as white falt, as far as they came to his knowledge. From this hiftory may be formed a judgment, how far the methods now in ufe are proper, in what deficient, where erroneous, and how they may be improved.

Bay-falt in general may be divided into two kinds. Firft, bay-falt, drawn from fea-water, as is practifed in *France*, *Spain*, *Portugal*, and many other countries. Secondly, bay-falt extracted from falt fprings, ponds, and lakes; as at *Cape de Verd* iflands, *Tortuga*, and other places. Of thefe the firft is imported in large quantities into *Great Britain* and *Ireland*: our *American* colonies, in times of peace, are chiefly fupplied with the latter; but in time of war they have large quantities of bay-falt from *Lifbon*, and other parts of *Portugal*.

Bay-falt is prepared in a manner the moft fimple and eafy, when the water of ponds and lakes impregnated with falt is totally exhaled by the force of the fun and air, and the falt is left concreted into a hard cruft at the bottom of the lake or pond. Of falt thus prepared we have inftances in many parts of the world, as in the *Podolian* defert near the river *Boryfthenes*, on the *Ruffian* frontiers towards *Crim Tartary*, in the kingdom of *Algiers*, and in other parts of the world.

Bay-falt is alfo drawn from the brine of ponds and lakes, and our author gives us an account of the preparing it it in this manner in the *Cape de Verd* Iflands. This account was collected chiefly from the relations of feveral perfons of credit, who themfelves affifted in making falt in thefe iflands. He alfo takes notice of the bay-falt made at *Tortugas*, and other places in *America*. He defcribes likewife the manner of making marine bay-falt in *France*, and other parts of *Europe*. For the particulars of thefe operations I muft refer you to the work itfelf; and only take notice, that every kind of bay-falt is prepared without artificial heat, and by only expofing the brine under a large furface to the action of the fun and air, by which, in proportion to the ftrength of the brine, and to the different temperature of climate and feafon, the falt cryftallizes into what we call bay-falt, and comes under different appearances to us from different places, which arife principally from the cleanlinefs and care of the artift.

Our author, when treating of white falt in general, acquaints us, that although falt is made, in warm climates, with the greateft eafe, and at the leaft expence, by the heat of the fun, after the methods already defcribed; yet, in feveral countries, where bay-falt might be conveniently made, they prepare all their falt by culinary fires. Thus in *Auftria*,

Bavaria,

Bavaria, and many other parts of *Germany*, and alſo in *Hungary*, and even in ſome parts of *Italy*, they conſtantly boil the water of their ſalt ſprings into white ſalt. But in other parts of *Europe*, as in *Britain*, and in the northern parts of *France* and *Germany*, an erroneous opinion long prevailed, that the heat of the ſun was not there ſufficiently intenſe, even in the ſummer ſeaſon, to reduce ſea-water, or brine, into bay-ſalt. And all arguments would probably have been inſufficient to remove this prejudice for the *Engliſh*, had not the contrary been fully proved by experiments, which were firſt accidentally made in *Hampſhire*. However, the method of making ſalt by coction will probably ſtill continue to be practiſed in *Britain*; as the ſalt ſo prepared is for ſeveral uſes preferable to bay-ſalt; and when prepared after a particular manner, is preferable to common bay-ſalt, even for curing proviſions, as the practice of the *Hollanders* ſufficiently teſtifies: ſo that the due and right preparation of white ſalt ſeems very deſerving of the notice and regard of the public.

White ſalt, as it is prepared from various ſaline liquors, may therefore be diſtinguiſhed into the following kinds:

1. Marine boiled ſalt, which is extracted from ſea-water by coction. 2. Brine or fountain-ſalt, prepared by coction from natural brine, whether of ponds or fountains. 3. That prepared from ſea-water, or any other kind of ſalt-water, firſt heightened into a ſtrong brine by the heat of the ſun, and the operation of the air. 4. That prepared from a ſtrong brine or *lixivium* drawn from earths, ſands, or ſtones impregnated with common ſalt. 5. Refined rock-ſalt, which is boiled from a ſolution of foſſil ſalt in ſea-water, or any other kind of ſalt-water, or pure water. 6. Laſtly, ſalt upon ſalt, which is bay-ſalt diſſolved in ſea-water, or any other ſalt water, and with it boiled into white ſalt; and under theſe heads may be ranked the ſeveral kinds of boiled ſalt now in uſe. Our author has given us an exact hiſtory of the manner of preparing theſe different kinds of ſalt, as practiſed in different places, with miſcellaneous obſervations and cautions relating to their reſpective proceſſes, for which in the general I muſt refer you to the work itſelf: but the making ſalt upon ſalt deſerves more particular attention; as the author, being under no tie of ſecrecy, has revealed to us the method of making in *Holland* and *Zealand* that ſtrong and pure kind of ſalt, with which they cure herrings, and all other proviſions for long keeping; which gives the *Dutch* a great advantage over all other nations in the herring-fiſhery; ſince fiſh preſerved with this ſalt look much cleaner and fairer than thoſe that are cured with bay-ſalt, and keep much better than thoſe preſerved with any other kind of white ſalt.

From the proceſs whereby white ſalt is made from ſea-water by coction, it appears, that ſea-water, beſides common ſalt, contains ſeveral other ingredients; ſome of which are ſeparated before the common ſalt falls, and others remain in the bittern, after all the ſalt is extracted. Our author has given a full and circumſtantial account of theſe in an expreſs

expreſs chapter, under the appellation of memoirs for an analyſis of ſea-water.

The ſalt-boilers. and particularly thoſe who prepare brine-ſalt, have long been accuſtomed to make uſe of various ſubſtances, which they call additions or ſeaſonings, and mix them with the brine while it is boiling, either when they firſt obſerve the ſalt begin to form, or elſe afterwards during the time of granulation. Theſe additions they uſe for various purpoſes. Firſt, to make the ſalt grain better, or more quickly form into cryſtals. Secondly, to make it of a ſmall fine grain. Thirdly, to make it of a large firm and hard grain, and leſs apt to imbibe the moiſture of the air. Fourthly, to render it more pure. And laſtly, to make it ſtronger, and fitter for preſerving proviſions.

Theſe additions, moſt commonly uſed to anſwer the above-mentioned purpoſes, are wheat-flour, reſin, butter, tallow, new ale, ſtale beer, bottoms or lees of ale and beer, wine-lees and alum. Wheat-flour and reſin are uſed for the property they poſſeſs of making the ſalt a ſmall grain. Butter, tallow, and other unctuous bodies are commonly applied, as they are ſaid to make the brine cryſtallize more readily; for which end ſome ſalt boilers more particularly prefer the fat of dogs: but others have little to plead for their uſing theſe ſubſtances, but immemorial cuſtom: how far they have the effects aſcribed to them can only be determined by experiments, as ſeveral boilers, who formerly uſed them, now find they can make as good ſalt without them. Wine-lees, new ale, ſtale ale, the lees of ale and beer are now generally rejected by the marine ſalt boilers; except in the Weſt of *England*, where the briners, who uſe them, affirm that they raiſe a large grain, and make their ſalt more hard and firm, and ſome ſay that they make it cryſtallize more readily. *Hoffman* prefers the ſtrongeſt ale; and *Plott* aſſures us, that it makes the ſalt of a larger or ſmaller grain, according to the degree of it's ſtaleneſs. The only good effects that fermented liquors can have as an addition, are probably owing to their acid ſpirit, which may correct the alcaline ſalts of the brine, and ſo render the common ſalt more dry and hard, and leſs apt to diſſolve in moiſt air. If therefore it ſhould be thought neceſſary to uſe any of theſe additions, in order to correct the alcaline quality of the brine, ſtale ale, or *Rheniſh* wine *, ought to be choſen, as new ale contains but little acid.

Alum is an addition long known and uſed in *Cheſhire*, together with butter, to make the ſalt precipitate from ſome ſorts of brine, as we are aſſured by Dr *Leigh* in his Natural Hiſtory of *Lancaſhire*, *Cheſhire*, &c. who firſt taught the *Cheſhire* ſalt-boilers the art of refining rock ſalt. As the bad properties of their ſalt proceeded from hard boiling, they found every method ineffectual, until they had recourſe to a more mild and gentle heat. And as alum hath been long diſuſed amongſt them, it is not likely, that they found any extraordinary benefit from it; other-

* Why not Malt-Vinegar? *C. M.*

wiſe

wise they would scarce have neglected it, and continued the use of butter. However Mr *Lowndes* hath lately endeavoured to revive it's use; asserting, that brine-salt hath evermore two main defects, flakyness and softness; and to remedy these imperfections, he tried alum, which fully answered every thing he proposed; for it restored the salt to it's natural cubical shoot, and gave it a proper hardness; nor had it any bad effect whatever. But our author is of opinion, that whoever considers the nature of alum, will scarce expect such extraordinary effects from it. Neither does it here seem wanted; for the grains of common salt will always be sufficiently hard, and of their natural figure, large size, and no ways disposed to run by the moisture of the air, if formed by a gentle heat, and perfectly free from heterogeneous mixtures: so that the goodness of Mr *Lowndes*'s salt does not seem owing to the alum, with which it is mixed, but chiefly to the gentle heat used in it's preparation.

The *Dutch*, who have long shewn the greatest skill and dexterity in the art of boiling salt, make use of another addition, which they esteem the greatest secret of their art. This is whey, kept several years 'till it is extreamly acid; now first revealed by our author to the *British* salt-boilers, but long held in great esteem by the *Dutch*, for the good effects it hath upon their salt; which it renders stronger, more durable, and fitter to preserve herrings, and other provisions.

Bay-salt, as well as white salt, is of different kinds, and possessed of different qualities: with the different kinds of these provisions must be cured, according to the uses for which they are designed. The *Dutch* indeed use no salt for curing provisions, besides their own refined salt. With it they can preserve flesh and fish of all kinds as well as with the strongest bay-salt; and chuse to be at the expence of refining bay-salt, rather than to defile their provisions with the dirt and other impurities, with which it commonly abounds.

Salt, esteemed the best for curing provisions, and for preserving them the longest time, is that which is the strongest and the purest. This may be known by the following characteristics; *viz.* it is usually concreted into large grains or crystals, which are firm and hard, and in respect to those of other kinds of common salt, the most solid and ponderous; it is not disposed to grow moist in a moderately dry air, to which it has been exposed a considerable time; it's colour is white, and somewhat diaphanous; it hath no smell; it's taste is truly muriatic, and more sharp and pungent than that of other kinds of common salt. It has, besides these, several other distinguishing properties mentioned by our author. The salts, which approach nearest to this degree of perfection, are the best kinds of bay-salt, and the strong *Dutch* refined salt; but most of the salt now made for sale is very far from answering to these characteristics.

Having related the various methods of preparing salts that now are in use, as far as they are come to our author's knowledge, it appears, that

that this art is not brought to ſuch perfection in the *Britiſh* dominions as in ſeveral other countries, the ſalt there prepared being unfit for preſerving many kinds of proviſions. It remains now to ſhew, that this want of a ſtrong ſalt of *Britiſh* manufacture proceeds not from any defect in nature, but of art; and that, if proper ſkill and induſtry be uſed in the *Britiſh* dominions, and due encouragement there given by the legiſlature, ſuch improvements may be made in this art, that not only *Great Britain*, but *Ireland* alſo, and the *Britiſh* colonies in *America*, may be ſupplied with ſalt of their own manufacture, proper for curing all kinds of proviſions, in quantity ſufficient for all their occaſions, in quality equal, if not ſuperior, to any foreign ſalt now made, and at a moderate price. Theſe are truths, which the author hopes will appear evident from the facts and reaſonings contained under the following poſitions:

The quantity of water which annually falls in rain, ſnow, and hail, is very different in different parts of *Great Britain*; there commonly falling almoſt double the quantity on the weſtern coaſts, that falls on the eaſtern coaſts of that iſland. *Lemma* I.

The quantity of rain which falls in *Lancaſhire*, during the four hotteſt months of the year, *viz.* *May*, *June*, *July*, and *Auguſt*, doth not at a medium amount to more than [illegible] of the quantity of water, which falls in rain, ſnows, and hail, during the whole year. *Lemma* II.

The water which aſcends in vapours from the ſea very greatly exceeds that which deſcends thereon in rain and other aqueous meteors: but the quantity of water, which uſually exhales from a given part of the ocean in a given time, cannot with any exactneſs be determined. *Lemma* III.

The quantity of water which commonly exhales in *Great Britain* from ſhallow ponds during the 4 hotteſt months of the year, greatly exceeds the quantity of rain which commonly falls on the ſurface of thoſe ponds during the ſaid months. *Lemma* IV.

From theſe *lemmata*, which the author has ſupported by the obſervations, not only of himſelf, but of other learned men, are deduced the following propoſitions:

In ſeveral parts of *England* large quantities of bay-ſalt may be extracted from ſea-water during the hotteſt months of the year, by receiving the ſalt-water into ponds, and ſuffering it's aqueous parts thence to exhale by the heat of the ſun, and the operation of the air and winds. *Prop.* I.

In ſeveral parts of *England* large quantities of bay-ſalt may very commodiouſly be extracted from ſea-water, after the ſame manner that is practiſed in *France*, and in other parts of *Europe*. *Prop.* II.

Bay-ſalt may be extracted in *England* from ſea-water in larger quantities, and with more certainty, than by the foregoing method, if care be taken to preſerve the brine contained in the ſalt-pits from being dilu- *Prop.* III.

ted with rains, and to promote the evaporation of the water by several artificial means, which may easily be put in practice.

Prop. IV. In several parts of *England* large quantities of excellent bay-salt may with great ease be made from the natural brine of salt springs, and also from rock-salt dissolved in weak brine or sea-water.

Prop. V. Bay-salt may be prepared in *England* by the foregoing methods at a very moderate expence, equal in goodness to the best foreign bay-salt, and in quantity sufficient for the consumption of all the *British* dominions.

Prop. VI. In several of the *British* colonies in *America*, bay-salt might, with little expence and trouble, be prepared from sea-water, in quantities sufficient to supply the *American* fisheries, and all other occasions of those colonies, so as to become a considerable branch of their trade.

The author has supported all these propositions with great ingenuity; but I cannot pass over in silence the artificial means to promote the evaporation of sea-water, mentioned in *Prop.* III. as well as to preserve the brine contained in the salt-pits from being diluted with rains. I therefore shall lay before you a short account of these.

It will be proper, says he, to make all the salt-pits of the marsh in one long row extended from E. to W. and for each pit to make covers of thin boards, or rather of coarse canvas, or sail-cloth, stretched on frames of wood and painted white. These covers must all be fixed with hinges to strong posts and beams on the N. side of the pits; so that they may be let down and drawn up with cords and pulleys, or by some other contrivance, somewhat like draw-bridges. These covers thus fixed may be let down over the pits like a shed or penthouse in rainy weather; and in dry weather may be erected almost to a perpendicular, but inclining a little towards the S. so as to form a wall with a S. aspect. Thus these may serve a double purpose, as coverings for the pits in wet weather, and as reflectors of the sun's heat upon them in dry weather, and thus greatly promote the evaporation of the aqueous parts of the brine. The hinges on which the reflectors turn may be fixed about 8 or 10 inches from the ground; by which means, when the reflectors stand upright, there will be an opening left beneath them, through which the air will continually flow in a brisk current, and greatly increase the evaporation of the water.

After having gone through that part of Dr *Brownrigg*'s work, which relates to bay-salt, we proceed to the methods that gentleman proposes for preparing and improving white salt, which, if brought into use, may probably be of advantage not only to private undertakers, but also to the public. For it appears, that two very different kinds of white salt are required; the one for the use of the table, and the other as a condiment for provisions. It's whiteness, dryness, and the smallness of it's grain, are the properties which chiefly recommend the first kind; and it's great strength and purity the latter. It is this strong and pure kind

kind of white falt, which is wanted in the *Britifh* dominions; and it is therefore our author's principal defign here to confider how this defect may be fupplied; although at the fame time inftructions are given how to prepare table falt, not only better in quality, but alfo at a lefs expence than it is now prepared by the common methods.

In the common proceffes for making white falt, the falt is deprived of a confiderable part of it's acid fpirit, by the violent boiling ufed in it's preparation. *Lemma* I.

Moft kinds of white falt are rendered impure by the mixture of various heterogeneous fubftances. *Lemma* II.

White falt, by the violent coction commonly ufed in it's preparation, is rendered lefs fit for preferving fifh, flefh, and other provifions, than it would be if prepared with a more gentle heat. *Lemma* III.

The heterogeneous fubftances which are commonly mixed with white falt, render it lefs proper for preferving provifions, than it would be if feparated from them. *Lemma* IV.

After having fully confidered the foregoing, our author gives a method of preparing a kind of white falt proper for curing fifh, flefh, and other provifions; likewife a method of refining falt; but for thefe I muft refer you to the work itfelf, as well as for the tables, wherein the feveral expences attending thefe operations are minutely confidered.

Moft of the facts referred to in thefe difquifitions are fuch, as the conftant practice of thofe who make falt fufficiently warrants us to rely upon for true and certain; or elfe, they are the obfervations of judicious falt-officers, daily converfant in thefe matters, or of curious and inquifitive navigators, merchants, travellers, and Naturalifts; or, laftly, the experiments of many learned Phyficians, Chemifts, and Philofophers: the truth of which feveral facts, though many of them have long been publifhed, hath never been called in queftion. So that thefe obfervations and experiments may probably be more relied on by the public, than if they had only been made by our author; fince they have the teftimony of many fkilful and unprejudiced perfons, who could have no notion of the ufes to which they have been here applied. If therefore the arguments founded upon thofe facts fhould be efteemed any-ways reafonable and fatisfactory, the author prefumes to remark, that it might not be unworthy the wifdom of the *Britifh* Legiflature to direct a more full inquiry to be made into a matter of this importance, and to order proper works to be erected for making bay-falt, and for making and refining white falt, and to put thofe works under the management of able and judicious perfons, to make exact and accurate trials, in order to difcover the beft and cheapeft methods of doing them. And the methods, which fhould be moft approved of, might for the general good be made public, and eftablifhed by law as a common ftandard, to which all thofe who make falt in the *Britifh* dominions fhould be obliged to conform.

Of the Salt-mines near Cracau; *by* James Mounsey, *M. D. Phys to the* Czarina's *army*, N°. 493. p. 219. Oct &c 1749. *Read* Nov. 23. 1749.

XXIX. Near *Cracau* in *Poland* are famous salt-mines, of which I shall give you a short account, as well as of the most remarkable things I found there. The town is situated near the foot of a vast chain of mountains, and from it, passing by hills and rising grounds about two *German* miles southward, I came to the mines of *Vilitzca*. These are in a hill flat and irregular above, surrounded with hollows and vallies, and to the south there is a neighbouring hill much higher. The mine has ten entries, which are provided with horse-engines, whereof 7 are for hoisting up the salt, and the rest for drawing water from the works, and for the descent and ascent of the people. I entered the mine by winding stairs of 484 steps, which brought me to the first story. The descent into the second is by strait stairs of 133 steps. Into the lowermost story there are no stairs, but 18 ladders from different floors, which make together 300 feet; and the computed depth of the whole is about 900 feet perpendicular.

The several strata of the earth are as follow.—On the surface is a common clayey ground, next is pure clay, and then a bed of soft, moist, black, slimy earth; and below this are hills of a kind of earth without any mixture of grit or sand. Here are first found particles and veins of salt; and, descending a good way through this and some salt rocks, we enter into the first story, where there are a great many alleys and cross-ways (which are run out to considerable distances), and many large caverns, out of which salt has been cut. Here the floor, walls, and ciellings, are solid salt rock. As the religion of the country is *Roman* Catholic, there are several large chapels, with altars adorned with columns, crucifixes, statues of saints, and other ornaments in that way, hewed out of the salt rock, and well wrought in different orders of architecture. Some of these, which are of the purer salt, and not much smoked with the torches that the workmen use in the mine, have a very beautiful effect. In some places the sides of the alleys, and some of the great vaults, are lined and doubled with timbers, where they thought the pillars of earth or salt left for supporting the superior weight might prove too weak. I observed in one place, that a sinking of the earth some years ago had crushed some of the baulks almost flat, and made a rent in the salt-rock on the other side, about 9 inches wide.

Notwithstanding there is no remembrance or tradition of any remarkable accident by the falling-in of these mines, yet they have lately discovered a wooden house, which must have been swallowed in very long ago. These mines were on fire in the year 1644. but this accident must have happened long before that time; for they have a plan of these works, taken about 200 years ago, with remarks of every thing that was curious in all three contignations; but no mention is made of this house, nor is there any-thing in the registers of these works that shews it to have sunk in since. The wall of this house is seen at the side of one of the cross-ways: they have found plates, spoons, and some other things

things of metal; but they make no farther ſearch, as the pains would exceed the profit: ſo it is left as a curioſity.

Notwithſtanding the ſalt rocks are on all ſides, and the earth that is among them is full of veins and particles of ſalt, there is a ſpring of very good freſh water, which is the drink of the thirſty workmen, and of the horſes employed below-ground. This ſource comes from above; but directly over that place, on the ſurface, there is no well, nor ſpringy ground, only it is hollow.

They find in theſe mines alabaſter, *Glacies Mariæ*, *gypſum*, and ſometimes *pectines*, or ſmall ſea-ſhells: but the moſt remarkable thing of all is, in the middle of a vaſt ſalt rock, a large tree is found, with all it's branches incaſed in it, lying horizontally. I ſend you a piece of it, which I hewed out of the rock myſelf. It ſeems to be a beech-tree, of which there grow plenty in theſe countries at preſent.

From the upper ſtory the rocks grow broader like cones, and the deeper they go, the ſalt is always finer, and leſs mixed with earth: but it is not yet known how deep they run. They do not however find it turns ſo much to account to work the lowermoſt ſtory, though it is all pure rock, the hoiſting being more expenſive than the running out croſs-ways, and working the upper ſtories. The rocks have roots or veins, which ſhoot into the earth on all ſides, ſome in ſtrait lines, others in zigzag, even to the diſtance of 70 feet; whereby the miners are often directed to the body of the rock. Theſe veins are very white and clear, yet they make no uſe of what is found in them, being impure, and mixed with other ſalts: it likewiſe diſſolves much eaſier than the true ſalt. *Sal gemmæ* is found in veins and neſts in ſeveral places of the mines, but 'tis often very troubleſome to hew, and get it out of the other ſalt rock. Here are no wells of *naphtha*, but there are ſome cavities where the air is ſo inflammable, that ſome, by going raſhly into ſuch places with a light, have been damaged by the fire, and even run the riſque of their lives. This only happens in places where the air has no free admiſſion; for in all the main ſtreets and croſs-ways there is a conſiderable draught of air through the ten entries: and, in the winter, while it is a ſtrong froſt, and quite calm weather, there reigns a very ſtrong wind in the mines: but ſtormy weather makes no alteration; and in the ſummer there never happen any ſuch changes.

From the ſeveral ways are entries into the chambers or vaults where they work. They hew the ſides of the wall into large ſquare columns, the height of the room, and about 2 or 3 feet thick. By driving their wedges in behind theſe pillars, they make them rend from the rock, and their fall on the floor makes a very great noiſe. The workmen are ſo accuſtomed, that by the ſound of their blows they know the inſtant it is going to fall, and get out of the way accordingly. Sometimes they hew the like pieces from the floors. Theſe pillars are again hewed into blocks, from 3 to 6 feet long, according to their thickneſs. They are drawn up, and tranſported in ſuch pieces, and the ſmall in barrels.

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The quantity of salt dug here yearly, comes to about 120,000 centeners of *Vienna*: and the whole expences for officers, workmen, materials, *&c.* amount to about 100,000 dollars. The number of workmen of all sorts make about 600 persons: they are very healthy and long-lived, not subject to the scurvy, or any particular distempers. The officers on the contrary are very subject to diseases of the breast, and consumptions, which is probably owing to the frequent changes of air they meet with, their business obliging them to stir about much, both above and below ground, where the air is very different.

Of the Fossils of Bohemia, *by the same.* Ibid. p. 218.

XXX. The kingdom of *Bohemia* is a fine fertile country, rich in metals and minerals of all sorts. The frontiers all round are very high mountains: the inward parts of the country are hilly, with plains and rising grounds intermixed, that have the appearance of being the remaining bases and ruins of former mountains, the soil being a composition of decayed rocks mixed with some vegetable earth. The rocks on the highest mountains are an aggregate stone of *lapides calcarii*, *spati*, *quartzi*, *micæ*, *&c.* The plains are covered with the least dissolvable parts of such rocks. Their finest crystals, and precious stones, are gathered behind the plough; many still retaining the same figures they had received at their formation in the veins and hollows of the rocks. I found on the tops of mountains decaying rocks, which, when mixed with a little vegetable earth, made exactly the same soil with that in the rising grounds and plains below.

There are several places in this kingdom where the mountains are wholly of *lapis scissilis*, which breaks into rhomboids; and I observed for many miles the shelves of this stone running through different mountains in the same direction, facing the S. E. with an inclination of the shelves of about 35°. The soil here in the plains is clayey.

Of some Fossils found in Ireland; *by Mr* James Simon. N°. 477. p. 531. Aug &c 1745. *Read* Nov. 14. 1745.

XXXI. In my little excursions in quest of fossils in this country, I found, *Sept.* 13. 1745. what Naturalists call *lac lunæ*; but think Dr *Plott* is mistaken, when he gives it as a criterion or sign of good limestone; for the two quarries where I found it were building stone, but will not burn into lime. This matter or earth makes a strong ebullition with vinegar and spirit of vitriol. Some of it was as soft as cream-cheese, when I took it out of the fissure of the rock; the other was hard, some in thin crusts, and some in pretty thick lumps. It never was taken notice of in this kingdom before. I also found, about six weeks ago, white native vitriol, which I take to be the *capillaris* sort; but as we have no Naturalist here, nor collection of fossils, or any other natural curiosities (though in great plenty in this kingdom), it is hard for me to give names to such as I have (about 800 articles) or do discover daily.

XXXII.

XXXII. What *ſpelter* is I don't well know, nor what uſes are already made of it; but I believe it was never yet applied to ſo large a work as the cylinder of a fire-engine, till Mr *Ford*, of *Colebrook-Dale* in *Shropſhire*, did it with ſucceſs: it run eaſier, and caſt as true as braſs, and bored full as well, or better, when it had been warmed a little: while cold, it is as brittle as glaſs, but the warmth of my hand ſoon made it ſo pliant, that I could wrap a ſhaving of it round my finger like a bit of paper. This metal never ruſts, and therefore works better than iron; the ruſt of which, upon the leaſt intermiſſion of working, reſiſts the motion of the *piſton*.

A letter from the Rev. Mr Maſon, Woodwardian *Prof. at* Cambridge, *and* F. R. S. *to the* Pr. *concerning* Spelter, *and melting* Iron *with Pit-coal.* N°. 482. p. 370. Jan. &c. 1746-7. *Read* Jan. 22. 1746-7.

Several attempts have been made to run *iron ore* with pit-coal; I imagine it hath not ſucceeded any-where, becauſe we have had no account of it's being practiſed; but I find that Mr *Ford*, from iron ore and coal, both got in the ſame dale, makes iron brittle or tough, as he pleaſes; there being cannon thus caſt ſo ſoft as to bear turning like wrought iron.

XXXIII. 1. I take the freedom to incloſe to you an account of a ſemi-metal called *Platina di Pinto*; which, ſo far as I know, hath not been taken notice of by any writer on minerals. Mr *Hill*, who is one of the moſt modern, makes no mention of it. Preſuming therefore that the ſubject is new, I requeſt the favour of you to lay this account before the *R. S.* to be by them read and publiſhed, if they think it deſerving thoſe honours. I ſhould ſooner have publiſhed this account, but waited, in hopes of finding leiſure to make further experiments on this body with ſulphureous and other cements; alſo with Mercury, and ſeveral corroſive *menſtrua*. But theſe experiments I ſhall now defer, until I learn how the above is received. The experiments which I have related were ſeveral of them made by a friend, whoſe exactneſs in performing them, and veracity in relating them, I can rely on: however, for greater certainty, I ſhall myſelf repeat them.

Several Papers *concerning a new* Semi Metal, *called* Platina; *communicated to the* Royal Society *by Mr* Wm Watſon, *F. R. S.* N°. 496. p. 584. Nov. &c. 1750. *Read* Dec. 13. 1750.

Extract of a letter from Will Brownrigg, *M. D. F. R. S. to* Wm. Watſon, *F. R. S. Dated* Whitehaven, Dec. 5, 1750.

2. Although the hiſtory of minerals, and other foſſil ſubſtances, hath been diligently cultivated, eſpecially by the Moderns; yet it muſt be acknowledged, that, among the vaſt variety of bodies which are the objects of that ſcience, there ſtill remains room for new inquiries.

Memoirs of a Semi-metal called Platina di Pinto, *found in the* Spaniſh Weſt Indies, Ibid. p. 585.

No wonder that, among the great, and almoſt inexhauſtible varieties of ſalts, ores, and other concretes, new appearances, and mixtures before unknown, ſhould daily be diſcovered: but that, among bodies of a more ſimple nature, and particularly among the metalline tribe, ſeveral diſtinct ſpecies ſhould ſtill remain almoſt wholly unknown to Naturaliſts, will doubtleſs appear more ſtrange and extraordinary.

Gold is uſually eſteemed the moſt ponderous of bodies; and yet I have ſeen, in the poſſeſſion of the late Profeſſor *s'Graveſande*, a metalline ſubſtance, brought from the *Eaſt-Indies*, that was ſpecifically heavier

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vier than gold, by at leaſt [illegible] part. Mercury, next to gold, is commonly ſaid to be the heavieſt body; yet Mercury is greatly exceeded in ſpecific gravity by a ſemi-metal brought from the *Weſt-Indies*, whereof I have now the honour to preſent ſpecimens to the *Royal Society*. And this ſemi-metal ſeems more particularly to deſerve our attention, as it is endued with ſome very ſingular qualities, which plainly demonſtrate that certain general theorems, tho' long eſtabliſhed, and univerſally received by the Metallurgiſts, yet do not hold true in all caſes, and ought not to be admitted into their arts, without proper limitations and reſtrictions. For inſtance, *That gold and ſilver may be purified from all heterogeneous ſubſtances by coppellation*, is a propoſition that all aſſayers and refiners have long thought true and undeniable; yet this propoſition ought not to be received by thoſe artificers, without an exception to the ſemi-metal here treated of; ſince, like thoſe nobler metals, it reſiſts the power of fire, and the deſtructive force of lead in that operation.

This ſemi-metal was firſt preſented to me about nine years ago, by Mr *Charles Wood*, a ſkilful and inquiſitive Metallurgiſt, who met with it in *Jamaica*, whither it had been brought from *Carthagena* in *New Spain*. And the ſame gentleman hath ſince gratified my curioſity, by making further inquiries concerning this body. It is found in conſiderable quantities in the *Spaniſh Weſt Indies* (in what part I could not learn) and is there known by the name of *platina di pinto*. The *Spaniards* probably call it *platina*, from the reſemblance in colour that it bears to ſilver. It is bright and ſhining, and of an uniform texture; it takes a fine poliſh, and is not ſubject to tarniſh or ruſt; it is extremely hard and compact; but, like Bath-metal, or caſt iron, brittle, and cannot be extended under the hammer.

The *Spaniards* do not dig it in the form of ore, but find it in duſt, or ſmall grains, as herewith preſented to the *Royal Society*. Whether they gather it in a pretty pure ſtate, as brought to us, or waſh it, like gold-duſt, from among ſand, and other lighter ſubſtances, is to me unknown: however, it is ſeldom collected perfectly pure; ſince, among ſeveral parcels of it that I have ſeen, I conſtantly obſerved a large mixture of a ſhining black ſand, ſuch as is found on the ſhores of *Virginia* and *Jamaica*, which is a rich iron ore, and anſwers to the magnet. It hath alſo uſually mixed with it ſome few ſhining particles of a golden colour, which ſeem to be a ſubſtance of a different nature.

It is very probable that there is great plenty of this ſemi-metal in the *Spaniſh Weſt Indies*; ſince trinkets made of it are there very common. A gentleman of *Jamaica* bought five pounds of it at *Carthagena* for leſs than it's weight of ſilver; and it was formerly ſold for a much lower price.

When expoſed by itſelf to the fire, either in grains, or in larger pieces, it is of extreme difficult fuſion; and hath been kept for two hours in an air-furnace, in a heat that would run down caſt iron in 15 minutes: which great heat it endured without being melted or waſted; neither could

could it be brought to fuſe in this heat, by adding to it borax, and other ſaline fluxes. But the *Spaniards* have a way of melting it down, either alone, or by means of ſome flux; and caſt it into ſword-hilts, buckles, ſnuff-boxes, and other utenſils.

When expoſed to a proper degree of fire, with lead, ſilver, gold, copper, or tin, it readily melts and incorporates with theſe metals; rendering the mixture, like itſelf, extremely hard and brittle.

Having been melted in an aſſay-furnace, on a teſt with lead, and therewith expoſed to a great fire for three hours, till all the lead was wrought off, the *platina* was afterwards found remaining at the bottom of the teſt, without having ſuffered any alteration or diminution by this operation,

A piece of *platina* was put into ſtrong and pure *aqua fortis*, and therewith placed in a ſand-heat for 12 hours, the *platina*, when taken out of the *aqua fortis*, was found of the ſame weight as when put into it; being in no-wiſe diſſolved or corroded by that *menſtruum*.

It had been reported, that this ſemi-metal was ſpecifically heavier than gold; but having weighed ſeveral pieces of it hydroſtatically in a nice aſſay-balance, I found one of theſe pieces to weigh in air *gr.* $\frac{345}{8}$, and in water *gr.* $\frac{322}{8}$: ſo that it's ſpecific gravity was to that of water exactly as 15 : 1. Another piece, that ſeemed to be caſt very open and porous, I found in gravity to water only as 13.91 to 1. Although this laſt mentioned piece, could it have endured the hammer as well as gold, might probably have been reduced to a conſiderably greater degree of ſolidity than that of the firſt-mentioned ſpecimen. For the pureſt gold is ſeldom found, after fuſion, to come up to it's true ſpecific weight, until it hath been brought up to it's greateſt degree of ſolidity under the hammer.

I alſo weighed an equal mixture of gold and *platina*, which I found nearly as ponderous as gold itſelf; the ſpecific weight of this mixture being to that of water as 19 to 1.

It hath been reported, that the *Spaniards* have ſometimes been tempted to adulterate gold with *platina*, as the mixture could not be diſtinguiſhed from true gold by all the ordinary trials: but the gold thus adulterated was, upon a nicer examination, found hard and brittle, and could not be ſeparated from the *platina*, and rendered ductile and pure, either by cementation, or by the more ordinary operations with lead and antimony. In order therefore to prevent this fraud, the king of *Spain* commanded that the mines of *platina* ſhould be ſtopped up; ſo that this ſemi-metal is now much ſcarcer than formerly.

From the foregoing account it appears, that no known body approaches nearer to the nature of gold, in it's moſt eſſential properties of fixedednefs and ſolidity, than the ſemi-metal here treated of; and that it alſo bears a great reſemblance to gold in other particulars. Some Alchemiſts have thought that gold differed from other metals in nothing ſo much as in it's ſpecific gravity; and that, if they could obtain a body that had the ſpecific weight of gold, they could eaſily give it all the other qualities of that metal. Let them try their art on this body; which, if it can be made as ductile as gold, will not eaſily be diſtinguiſhed from gold itſelf.

Upon the whole, this ſemi-metal ſeems a very ſingular body, that merits an exacter inquiry into it's nature than hath hitherto been made; ſince it is not altogether improbable, that, like the magnet, iron, antimony, mercury, and other metallic ſubſtances, it may be endowed with ſome peculiar qualities, that may render it of ſingular uſe and importance to mankind.

Specimens of Platina *preſented to the* Royal Society.

N°. 1. *Platina*, in duſt, or minute maſſes, mixed with black ſand, and other impurities, as brought from the *Spaniſh Weſt Indies.*
2. Native *platina*, ſeparated from the above-mentioned impurities.
3. *Platina* that has been fuſed.
4. Another piece of *platina*, that was part of the pummel of a ſword.

—To the Royal Society, *by* Mr Watſon. *dated* London, Dec. 13. 1750.

3. I beg leave to ſubjoin a few lines to Dr *Brownrigg*'s paper concerning the *Platina di Pinto*, or what is likewiſe called in *America Juan Blanco.* This ſubſtance is mentioned in no author I have met with, except by Don *Antonio d'Ulloa*, who, in the Hiſtory of his Voyage to *South America*, *Vol.* II. *Book* 6. *Chap.* 10. which I have here extracted, and tranſlated from the *Spaniſh*, when giving an account of the gold and ſilver mines in the province of *Quito*, and of the various methods of ſeparating theſe metals from other ſubſtances, with which they are combined, ſays, that, " in the territory of *Choco* . . . . there are gold " mines, in which that metal is ſo diſguiſed and enveloped with other " mineral ſubſtances, juices, and ſtones, that, for their ſeparation from " the gold, they are obliged to uſe quickſilver. Sometimes they find " mineral ſubſtances, which, from their being mixed with *platina*, " they chuſe to neglect. This *platina* is a ſtone (*piedra*) of ſuch reſi" ſtance, that it is not eaſily broken by a blow upon an anvil. It is not " ſubdued by calcination; and it is very difficult to extract the metal it " contains even with much labour and expence."

In the before-mentioned work, *Chap.* 11. the ſame author, when ſpeaking of the remaining works of the *Indians* of old, ſays, " the *ſpe*" *cula* wrought out of ſtones, which are found in the places of worſhip " of the *Indians*, are of two kinds, in relation to the matter of which " they are made: one of theſe is called *Piedra de Inga*, the other *Pie*" *dra de Gallinazo.* The firſt of theſe is ſmooth, of a leaden colour, " and not tranſparent; they are uſually found wrought of a circular " figure: one of the ſurfaces is plain, and as ſmooth as though it were " made of a kind of cryſtal; the other ſurface is oval, or rather ſome" what ſpherical, and not ſo much burniſhed as the plain one. Altho' " they vary in their ſize, they are commonly from 3 to 4 inches in " diameter; but he has ſeen one that was 1½ foot in diameter. It's " principal ſurface was concave, and much augmented the ſize of ob-

" jects,

" jects, as it's polish was in as great perfection as though it had been " worked by a dextrous artist in these times.

" This stone has certain veins, or hair-like appearances, on it's sur- " face; whereby it is rendered less fit for a *speculum*, and is apt to " break in these veins in receiving any blow. Many are persuaded, or " at least suspect, that the matter of these is a cast composition; and " although there are some appearances of this being so, they are not " sufficiently convincing. In this country there are gullies (*quebradas*) " where the mineral of them is found rough, and from whence some " are always taken; but these are not now wrought for those purposes " for which heretofore they were employed by the *Indians*: but this is " no reason but that some of them may have been cast, as with the " same material taken out of the mine, they may have been made ar- " tificially, and thereby have received a greater degree of perfection, as " well in their quality as in their figure." He says further, " that, " although at present, these, as well as several other things found there " are but of small value, nevertheless they are extremely curious, and " worthy to be esteemed, as well for their great antiquity, as for their " being the performances of those barbarous people."

Some of these *piedras de inga* I now take the liberty of laying before the *Society*, both in their rough and in their polished state. They were brought hither with several other curiosities from *America*, by Don *Pedro Maldonado*, and were presented by him to the *President*, who was pleased to put them into my hands. They are doubtless of a metalline substance, and have, in my opinion, evident marks of having been fused and cast. They very much resemble, as you will see by comparing them, the *platina* before-mentioned: and though they are called (*piedras*) stones by Don *Antonio d'Ulloa*, he likewise gives the same appellation to the *platina*. I cannot therefore help recommending to some curious Metallurgist of the *Society* to make the experiment, whether or no, when the *piedras de inga* are, by a proper process, divested of their stony and other heterogeneous parts, the metalline *residuum* will not resemble, as well in specific gravity (for which it is so remarkable) as in other properties, the purified *platina* now before us?

4. In *January* 1742-3. there were brought from *Jamaica*, in a man of war, several bars (as thought) of gold, consigned from different merchants of that island, to their different correspondents here, as bars of gold. These bars had the same specific gravity, or rather more than gold, and were exactly like that metal in colour, grain, *&c.* A piece of one of these counterfeit bars was sent to the mint to be tested, and it was found to be 21 carats 3 grains worse than standard.

—*by Mr* Emanuel Mendez da Costa. *Read* Dec. 20. 1750.

5. The gentleman, whose experiments on *platina* I mentioned to the *Royal Society*, was Mr *Charles Wood*, who permitted me to make what use of them I pleased; and I did not pretend to have made any new discovery, nor to know so much of that body, as hath long been known to

Extract of a letter from Wm. Brownrigg. *M. D. and F. R. S. to* Wm. Watson, *F. R. S. containing some further experiments upon the* Platina. Ibid. p. 594. *dated* Whitehaven, Feb. 13. 1750. *Read* Feb. 28. 1750-51.

to the *Spaniards*. I might indeed have made use of his authority; but he was not ambitious of appearing in print.

The chief thing about which I had any difficulty, was what had been asserted of the *platina*'s resisting the force of lead in coppellation. This experiment I have tried therefore, by adding to *gr. xxvi.* of *platina*, 16 times it's weight of pure lead, that I had myself reduced from litharge. To the lead put into a coppel, and placed in a proper furnace; as soon as it was melted I added the *platina*, which in a short time was dissolved in the lead. After the lead was all wrought off, there remained at the bottom of the coppel a pellet of *platina*, which I found to weigh only *gr. xxi.*; so that, in this operation, the *platina* had lost near ⅕ of it's weight.

According therefore to this experiment, the *platina* does not wholly resist the force of lead in coppellation; but, by repeated operations of that kind with larger quantities of lead, may probably all be destroyed: and by such repeated coppellations, gold and silver may very likely be refined from it; although what was before asserted may hold pretty true, with regard to the common coppellations of the assayers and refiners.

Mr *Wood* said, that, in his experiment, he thought the *platina* rather gained than lost in weight by coppellation. This might happen from some small mixture of lead, or other metal continuing with it after it remained no longer fused.

From this single experiment I will not be quite positive that lead thus consumes some small quantity of *platina*, since it is possible the *platina* used might not be pure. Besides, in order to keep it longer in fusion, I urged on the experiment with an uncommon degree of heat, especially towards the end of the operation; although I think no great error could thence arise; as ℥ss of silver, which I coppelled at the same time, had lost only *gr. ij.* in the operation.

I am told that one Mr *Ord*, formerly a factor to the *S. S. Company*, took in payment from some *Spaniards* gold, to the value of 500 *l.* sterling, which being mixed with *platina*, was so brittle, that he could not dispose of it, neither could he get it refined in *London*, so that it was quite useless to him: although, if no error hath been committed in the above-mentioned experiments, it might probably have been rendered pure by a much larger dose of lead than is usually applied for that purpose.

To my memoir I might have added, that, attempting to cleanse a parçel of the native *platina* from the black sand, wherewith it was mixed, I found that a great many of it's grains were attracted by the magnet I made use of for that purpose. This circumstance I took notice of in a letter to Lord *Lonsdale* two years ago.

Extract of a letter from

XXXIV. As the Natural History of *Persia* is but little known, and the authors of the *Universal History* have given no true account of the

ever-

everlasting sacred fire which the *Gauers* worship, I shall now send you a description thereof, which you may depend upon, as there was a *Russian* army for some years in the kingdom of *Dagestan*, where that fire is; and I took down what I am going to relate from the mouths and journals of many officers that were there, and more particularly from what was communicated to me by *Archiater Fischer*, who received an account thereof from Dr *Lerch*, Physician of that army.

Dr James Mounsey, *Phys. of the* Czarina's *army, to* H. Baker, *F. R. S. concerning the* Everlasting Fire *in* Persia. N°. 487. p. 296. Apr. &c. 1748. *dated* Riga, Feb. 24. 1748. *Read* April 27. 1748.

This perpetual fire rises out of the ground in the peninsula of *Abscheron*, about 20 miles from *Baku*, and 3 miles from the *Caspian* shore. The ground is very rocky, but has a shallow covering of earth over it. If a little of the surface be scraped off, and fire be applied to the hollow, it catches immediately, and burns without intermission, and almost without consumption; nor is ever extinguished, unless some cold earth be thrown over it, by which it is easily put out.

There is a spot of ground, about two *English* miles large, which has this very wonderful property; and here is a *caravansary*, round which are many places where the earth continually burns; but the most remarkable is a hole about 4 feet deep, and 14 feet in diameter. In this *caravansary* live 12 *Indian priests*, and other *devotees*, who worship the fire, which, according to their traditions, has burnt many thousand years. It is a very old vaulted building, and in it's walls are a great many chinks, whereto if a candle be applied, the fire catches instantaneously, and runs instantly wherever the chinks communicate; but it may be easily extinguished: they have hollow places in the house fitted to their pots, which they boil without any other fuel; and instead of candles, they stick reeds into the ground; from the tops whereof, upon applying fire thereto, a white flame immediately comes forth, and continues to burn without consuming the reeds, until they think proper to extinguish it, by putting little covers over them for that purpose.

They burn lime of the stones dug hereabouts, first making an hollow in the ground, and then heaping the stones on one another. This done, on applying fire to the hollow, a flame bursts out, and is dispersed at once with a very great crack through the whole heap of stones; and after it has continued burning for 3 days, the *lime* is ready: but stones placed in this fire for setting their pots on never turn to lime; which cannot be made but by heaping them on one another. The earth and stone are no farther warm than where the fire reaches: and what seems very well worth observation, this flame of fire gives neither smoke nor smell, however great it be.

About an *English* mile and half from this place there are wells of *white naphtha*; which is exceedingly inflammable; and though the flame of *naphtha* affords both smoke and smell, it is highly probable the perpetual fire I have been describing is owing to *naphtha*, but so purified, in filtering through the stone, that it becomes divested of all such particles as produce smoke or smell. The stone and earth are grey in colour, and saltish to the taste; and indeed much salt is found on this peninsula

peninſula of *Abſcheron.* There is alſo a ſalt lake, near the ſide of which the *white naphtha* flows by 5 different ſprings. This *naphtha* is made uſe of only in the medicinal way. It is yellowiſh from the ſpring, but when diſtilled reſembles ſpirit of wine. They give it internally, for gonorrhœa's, diſorders of the breaſt, and for the ſtone; and they apply it externally in gouty caſes, contractions of the ſinews, and cramps.

Black naphtha is produced 8 or 9 miles from the perpetual fire; it is thick, and being diſtilled grows not clear but yellow. About *Baku* there is ſome of it ſo thick, that they employ it for greaſing wheels: but the beſt and greateſt plenty, is at *Balachame*, where there are above 50 ſprings, the greateſt whereof produces every day 500 *batman*, each *batman* containing ten *Ruſs* pounds, which are ſomewhat leſs than *Engliſh* weight. You hear it make a conſiderable noiſe in riſing out of the ground, though the ſpring be 20 fathom deep.

In *Baku* they have little or no other fewel to burn beſides *naphtha*, but it muſt be mixed with earth or aſhes to make it fit for uſe. The fire it makes is only good to boil with; and this inconveniency attends it, that all their food ſo boiled ſmells and taſtes of *naphtha*. For baking and roaſting they make uſe of *abrotanum*, *abſynthium*, and ſuch-like; but in general *naphtha* is their fire.

CHAP. IV.

MAGNETICKS.

Magnetical Experiments, *ſhewn before the* R. S. *by Mr* Gowin Knight, *on* Thurſday, Nov. 15. 1744. *Read the ſame day.* N°. 474. p. 161. June, &c. 1744.

I. MR *Knight*, of *Magd. Coll.* in *Oxford*, being introduced to a meeting of the *R. S.* produced, before the gentlemen there preſent, ſeveral curious artificial magnets contrived by himſelf; ſome of which conſiſted of plain bars of ſteel naked, and other of bars or blocks of the ſame ſubſtance, armed with iron after the common manner of natural loadſtones: but, as he was apprehenſive the trials he had before made of the weights theſe magnets were reſpectively capable of lifting, could hardly be repeated with ſufficient exactneſs and advantage before ſo large a company, he deſired to refer himſelf, for thoſe particulars, to what the *Preſ.* had ſeen at his lodgings on the 7th, and 13th of the ſame month.

Whereupon the Preſident acquainted the company, that he had lately been ſeveral times at Mr *Knight*'s lodgings, where he had ſeen many experiments made with his artificial magnets; and that, particularly on the days above-mentioned, he had been preſent, and had taken minutes of the following trials then made by that gentleman; by which it appeared, that,

A ſmall

A ſmall eight-cornered bar of ſteel, of the length of 3 inches, and almoſt $\frac{7}{10}$, and of the weight of about $\frac{1}{2}$ an ounce *Troy*, lifted by one of it's ends about 11 of the ſame ounces.

That another plain bar of ſteel of a parallelopiped form, of the length of 5 inches $\frac{9}{10}$, the breadth of $\frac{4}{10}$, and the thickneſs of $\frac{2}{10}$ of an inch, weighing 2 ounces 8$\frac{1}{2}$ pennyweight, lifted, in like manner, by one of it's ends 20 *Troy* ounces.

That a ſteel bar, almoſt of the ſame form as the laſt, but only 4 inches in length, capped or armed with iron at each end, cramped with ſilver, and weighing all together one ounce 14 pennyweight, lifted by the feet of the armour full 4 pounds *Troy*.

That a ſingle block of ſteel of a parallelopiped form, almoſt 4 inches long, 1 inch $\frac{2}{10}$ in height, and $\frac{4}{10}$ of an inch in thickneſs, armed with iron, cramped with braſs, ſuſpended by a ring of the ſame, and weighing all together 14 ounces 1 pennyweight, lifted by the feet of the armour 14 pounds 2$\frac{1}{2}$ ounces *Troy* weight.

That a compound artificial magnet was alſo tried, conſiſting of 12 bars of ſteel armed; and that it was found to lift by the feet of the armour as the laſt, 23 *Troy* pounds, 2$\frac{1}{2}$ ounces.

The 12 bars, compoſing this laſt magnet, were each a little more than 4 inches long, $\frac{\text{[illegible]}}{10}$ of an inch in breadth, and $\frac{16}{100}$ of the ſame in depth, weighing one with another about 25 pennyweight each. They were all placed one upon another, ſo as to make together one parallelopiped body, of the common length and breadth of the ſeveral bars, but of the height of near 2 inches, being the ſum of the reſpective thickneſſes of all the bars taken together: and this parallelopiped body, being cramped with braſs, and fitted with an handle of the ſame metal, was armed at the 2 ends that were made up of the common extremities of all the bars, with 2 ſubſtantial pieces of iron, after the common manner of arming natural loadſtones, the whole frame weighing together about 20 *Troy* ounces.

Beſides theſe, the Preſident made alſo the following report of ſome trials he had ſeen made at the ſame time of the effects of an art Mr *Knight* is maſter of, by which he can improve or increaſe the lifting powers of natural loadſtones.

He carried with him, on *Wedn. Nov.* 7. a ſmall armed loadſtone belonging to an acquaintance, which weighed, with it's armour, 7 pennyweight 14 grains; but being reputed but of an ungenerous nature, took up, and with ſome difficulty, barely 2 ounces. Mr *Knight* took it into his ſtudy, and, returning it in about a minute, it then took up better than 4 ounces with eaſe: but, upon his ſaying, it would ſtill gain ſome more ſtrength, by remaining with him ſome time, it was left till the 13th, when it took up diſtinctly, with the ſame *apparatus* as before, 6 ounces 18 penny weights and 3 grains; ſince which time it has alſo ſeveral times been found to lift nearly the ſame quantity.

Mr *Knight* further, at the fame time, fhewed the Prefident the following inftances of his ability to invert or change the direction of the poles in natural loadftones.

Such a ftone belonging to Mr *Fr. Haukfbee*, weighing about 5 ounces and 14 pennyweights, of an irregular cylindrical form, with 2 of the fides fomewhat flatted, upon which armour had formerly been applied, had the direction of it's polarity from one of thefe flatted fides to the other, notwithftanding the ftone had a diftinct grain running at right angles to that direction. It was tried and obferved, that one of thefe flatted fides ftrongly attracted the N. end, and repelled the S. and that the other attracted the S. and repelled the N. end of the magnetic needle. The end of the ftone, attracting the S. end of the needle, was then marked, by the rubbing of a piece of filver upon it, as upon a touchftone: after which Mr *Knight* carried the ftone into his ftudy; and, reproducing it in about a minute, fhewed, that the poles were then directly inverted; and that the fame end, which before attracted the S. end of the needle, now attracted the N. and repelled the S. and *vice verfâ*.

After this, Mr *Knight*, again taking the ftone, brought it back in as fhort a time as before, with the direction of it's polarity turned at right angles to it's former direction, and into the direction of the natural grain of the ftone, the poles now lying in the flat ends of the cylinder; one of which, being the fmoother end, attracted the S. end of the needle, whilft the other, which was of a rougher texture, attracted the N. end, and repelled the S. end of the fame: when it was alfo obferved, that the polarity appeared ftronger in this cafe, than either of the former.

Laftly, Mr *Knight*, in about the fame time, inverted this laft direction of the poles, keeping it ftill parallel to the axis of the cylinder, but caufing the fmooth end of the ftone to attract the N. end of the magnetic needle, and the rough end to attract the S. and repel the N. end of the fame needle.

After this report, Mr *Knight* proceeded to fhew, at the meeting, fome of the fame artificial magnets therein mentioned; and it was found, that the compound magnet, confifting of 12 fteel-bars, which had, in the experiment made before the Prefident, lifted 23 pounds $2\frac{1}{2}$ ounces *Troy* weight, did here, under all the inconveniencies and difadvantages of a crouded room, ftill lift a weight amounting to 21 pounds and 11 of the fame ounces.

It was alfo found, that the fingle armed block of fteel, which had before lifted 14 pounds and 2 ounces, did here, under the fame difadvantages as the former, lift 13 pounds and 7 ounces of like *Troy* weight.

And, laftly, Mr *Knight* produced to the company the above-mentioned natural loadftone belonging to Mr *Haukfbee*, but with the direction of it's polarity again altered from what it was when it was laft feen by the Prefident.

P. S.

P. S. Since the artificial magnets mentioned in the foregoing paper, Mr *Knight* has caused some others to be made of a lesser size, but of a very great lifting power: and one of these, weighing without it's armour just an ounce, and with the armour, cramps, and rings, 1 ounce 17 pennyweights, lifted, before the President of the *Society*, on *Friday July* 27, 1745. 6 pounds and 10 ounces *Troy* weight.

This magnet consisted of 3 plates of steel, each 2 inches long, $\frac{7}{10}$ of an inch in breadth, and not above $\frac{6}{100}$ of an inch in thickness: they were laid flat upon each other, and screwed together by 2 small brass screws going through the 3 plates. After which, the little parallelopiped block so made up, was armed with iron at the two ends, cramped together with silver, and fitted with a double ring of the same metal, for the convenient holding of it.

II. 1. Being on *Wed. Feb.* 11. at the house of Mr *Knight*, I did there in company with *Will. Jones*, Esq; see the following experiments; which Mr *Knight* was desirous I should, as on this day, report to the *Society*: before whom he is also now prepared to exhibit the same, as well as the circumstances of the place and the number of the company will allow.

A collection of the magnet Exp. *communicated to the* R S. *by* Gowin Knight, *M.B. F. R. S. in* 1746, 1747. N° 484. p. 656. Oct. &c. 1747. *Read* Feb. 19. 1746-7. *An account of some* magnet. Exp *exhibited before the* R.S. *on* Thurs. Feb. 19. 1746. *of which* the President, *who had before seen the same performed with more deliberation on the* 11*th*, *was pleased to make this report.*

He first produced two almost equal bars of hardened steel, to which he had communicated a strong magnetic virtue. These bars were nearly square, each being of the length of about 15 inches $\frac{2}{10}$, and of the breadth and thickness of a little more than $\frac{1}{2}$ an inch: one of these bars weighed 2 pounds and 6 pennyweight *Troy*, the other 4 pennyweight less than 2 pounds; and either of them readily lifted with one of it's ends better than 3 $\frac{1}{2}$ pounds.

These bars were then laid down on a table, so as to be nearly in one and the same strait line, the N. pole of the one being next to the S. pole of the other, and at the distance of about an inch from it: that is to say, that the N. poles of both bars were pointed the same way, but without any regard to the position of the natural meridian.

Mr *Knight* then produced a piece of natural magnet, which was one of the same he had formerly made use of, in some experiments he had before shewed to the *Royal Society*. This piece was in length an inch and $\frac{1}{10}$, in breadth $\frac{7}{10}$, and in thickness about $\frac{3}{10}$ of an inch at a medium, being considerably thicker at the one end than at the other.

This piece of magnet was then applied, so as to lie between the 2 first mentioned bars, with it's thin end close to the N. pole of one of them, and it's thick end close to the S. pole of the other. After it had lain in this position a few moments, it was taken out, and upon presenting it to the magnetic needle of a small compass-box, it was obserserved that it's thinner end, the same which had just been contiguous to the N. pole of one of the bars, attracted the N. end of the needle; and that the thicker end, the same which had been contiguous to the S. pole of the other bar, attracted the S. end of the same needle.

This ſame piece of ſtone was then again put in between the bars, but in a contrary poſition; the thicker end now lying next to the N. pole of one of the bars, and the thinner end next to the S. pole of the other. After a few moments it was again taken out, and preſented as before to the compaſs box: when it was found that the thin end now attracted the S. end of the magnetic needle, and that the thicker end attracted the N. end of the ſame.

The piece of ſtone was then again placed between the bars as at the firſt, and being again taken out and preſented to the compaſs-box: the thin end was again found as at the firſt to draw the N. end, and the thick end to draw the S. end of the needle.

This ſame piece of magnet was then again placed between the bars, but in a poſition at right angles to both the former, one of it's ſides being now contiguous to the N. pole of one of the bars, and it's other ſide to the S. pole of the other. After which being again in a few moments taken out, and preſented to the compaſs-box as before; it was found that the ſide which had been in contact with the N. pole of one of the bars, did attract the N. end of the needle, and that the other ſide which had been in contact with the S. pole of the other bar, did attract the S. end of the ſame needle: whilſt the two ends of the ſtone in which the polarity was before obſerved, were now found to be indifferent to either end of the needle; ſo that the line of direction of the poles in the ſtone now lay at right angles to the poſition in which it was ſituated in the former experiments.

Mr *Knight* then produced 2 ſteel needles, of the ſame ſort as thoſe which are uſually fixed to the cards of ſea-compaſſes. Theſe needles were of the length of 5 inches and $\frac{1}{10}$, and weighed ſeverally with their caps 7 pennyweight 8, and 7 pennyweight 9 grains; one of theſe was tempered and of a blue colour, and the other was quite hard. He alſo produced two iron weights, ſeverally weighing 14 pennyweight 22 grains, and 15 pennyweight 7 grains, both nearly of a cylindrical form, but with one of the ends rounded off.

The 2 large bars were then placed in a line, as in the former experiments, but with their ends ſo near together, as only to admit of the cap of one of the needles between them.

The tempered needle was then placed flat upon the bars, ſo that nearly one half of it reſted upon one bar, and the other half upon the other, the cap lying between the two. The needle was preſſed cloſe to the bars in this poſition, after which the bars were drawn away, both at the ſame time contrarywiſe, till they were clear of the needle; and this operation was repeated 3 or 4 times: after which that end of the needle which had reſted upon the N. part of one of the bars, was found ſtrongly to attract the N. end of the needle in the compaſs-box; and the other end which had reſted upon the S. part of the other bar, was found to attract in like manner the S. end of the ſame needle in the box. The power of attraction alſo acquired by this needle appeared to be be very confiderable, it lifting eafily with either of it's ends, the two iron weights abovementioned, when cemented the one to the other with wax, and weighing together 1 ounce 10 pennyweights 5 grains.

The hard needle was then applied to the bars like the other and with the very fame fuccefs, it lifted alfo, as the other had done, both the weights together.

The two needles were then themfelves applied to each other, firft the N. half of the one, in a contrary direction, to the N. half of the other; and then the S. half of the firft, in a like contrary direction to the S half of the laft; and from thefe feveral pofitions, they were feverally drawn till they were clear of each other, and this feveral times fucceffively: after which operation it was found that the tempered needle had loft fo far it's virtue, that it's N. end had hardly any effect upon the needle in the box; that it's S. end even began to attract the contrary end of the needle from what it did before, and that it was no longer able to lift at either of it's ends any fenfible weight.

But as to the hard needle, that ftill retained a confiderable fhare of it's former virtue; it's ends ftill ftrongly drawing the fame ends of the needle in the compafs box as they drew before, and either of them lifting with eafe the heavier of the two above-mentioned weights.

Mr *Knight* then produced one of it's common fmall magnetic bars; the which being applied to the forementioned large bars, in the fame manner as the needles had been applied to the fame, but in a pofition contrary to that of it's prefent polarity, it had it's poles thereby counterchanged or inverted, and was found to lift at that which was now become it's northern end, the weight of 6 ounces 8 pennyweight and 5 grains.

He laftly produced one of his large artificial armed magnets, compofed of feveral thin plates of fteel cramped together, with which he acquainted us he had fome time before lifted 36 pounds, and with which he did now actually lift before us 31 pounds 9 ounces and three fourths.

The tempered needle fpoken of above, and which had nearly loft all it's virtue, had the fame again reftored in great meafure, upon being touched in the common way, on the armed poles of this artificial magnet; after which it difcovered a ftrong verticity, and was able to lift at one of it's ends, the heavier of the 2 abovementioned weights, that is to fay fomewhat more than three quarters of an ounce.

The hard needle which ftill retained, as has been obferved, a confiderable part of the virtue it had acquired by the touch of the large fteel bars, was laftly touched alfo in a contrary fenfe, upon the armed poles of this artificial magnet; whereby it not only loft the polarity yet remaining, but acquired a new one the other way, it would not however after this laft touch lift more than 9 pennyweight.

This is the true fubftance of the minutes I took, when thefe experiments were made, and which I prefume will now be verified by thofe Mr *Knight* is here prepared to fhew.

After the reading of this report, Mr *Knight* did accordingly produce before the *Society* the two large bars and all the other particulars therein mentioned, with which he publickly repeated all the ſame experiments; which notwithſtanding the diſadvantagious circumſtances of the place, ſucceeded perfectly in every particular, and to the entire ſatisfaction of all the company.

It was then further propoſed, that the tempered needle, having it's virtue again deſtroyed, ſhould be touched upon the fine armed *terella* belonging to the *Society*, which was the noble preſent of their late worthy member the R. Hon. *James* E. of *Abercorn*, and is eſteemed one of the beſt in *England*, and is ſaid to have lifted in his Lordſhip's hands upwards of 40 pounds: the ſame was immediately brought, and the needle being touched therewith, was found to have acquired a ſtrong polarity, and to lift about the ſame weight, as when it was before touched upon by Mr *Knight*'s large armed artificial magnet; that is to ſay, about 15 pennyweight.

An account of ſome new Exp. *lately made with* Artificial Magnets; *by the ſame.* Ibid. p. 662. *Read* July 2. 1747.

2. The *apparatus* for touching of needles, which I ſometime ſince had the honour to ſhew before the *R S.* was as perfect as I could have wiſhed, as far as relates to the intended uſe of it: but the manner in which the two bars were diſpoſed in their caſes made the length of them ſomething incommodious, eſpecially in thoſe of the largeſt ſize. This made me deſirous of trying if ſome method could not be found out of placing the bars parallel to each other without danger of weakening their force, by which means the caſes would be reduced to half their length. I remembered that ſome years ago, I had tried ſome experiments to this purpoſe, by placing ſome bars parallel and in contact, but ſo that their poles were turned different ways: in which poſition I found the virtue of ſome of them remained pretty entire, but that others were weakened thereby. I imagined the reaſon of their loſing their force was this; that the magnetic virtue was by degrees habituated to paſs out of the ſide of one bar into that of the other in contact with it, and thereby was hindered from arriving at the ends in it's full vigour. The reaſon why ſome ſuffered more than others was doubtleſs to be aſcribed to their difference in temper. I repeated the experiment about two months ago, with a little alteration. I placed the bars parallel with their poles in an alternate poſition, as before, but not in contact, having kept them at the diſtance of about $\frac{1}{4}$ of an inch. Then I applied to their ends two pieces of ſoft iron. Each piece was laid acroſs from the N. end of one bar to the S. of the other, in the ſame manner as the lifter is applied to the feet of an armed loadſtone. The intent of this was to draw the magnetic virtue thereby down to the ends of the bars, and to convey it through the pieces of iron from one to the other. In this condition I let them lie for about a month, and then tried if they would lift the ſame weight as before, which I found they did, and I thought with more vigour. After this I repeated the experiment with other bars

bars of various ſizes, and with the ſame ſucceſs: I have therefore now ventured to fit them up in caſes in the manner juſt deſcribed.

The ſucceſs of this experiment had led me to another improvement: I provided a caſe of braſs that would juſt contain two bars, ſuch as are ſold for ½ a guinea. At one end of the caſe were fixed two feet of ſoft iron, like thoſe of an armed loadſtone, the upper ſurface of which was within the caſe in contact with the ends of the two bars: which being parallel to each other, and their poles in an alternate poſition, the N. end of one bar will be in contact with one of the feet, and the S. end of the other bar will be in like manner applied to the ſurface of the other foot. Upon fitting a lifter to this new kind of armour, I found I was able to ſupport a weight of about 6 pounds: the bars are kept aſunder at the diſtance of about ½ of an inch, by a ſlip of wood, which ſlides in betwixt them.

An inſtrument thus conſtructed ſeems capable of anſwering all the purpoſes for which loadſtones are uſed; for when the bars are taken out of the caſe, they are fit for touching needles, or other magnetical uſes, which may require ſingle bars; when in the caſe, the whole together becomes an armed magnet, able to lift a conſiderable weight. And if we want to ſeparate iron filings from thoſe of other metals, the feet and all the lower part of the caſe will take them up in great plenty, and by drawing the bars a little way out of the caſe the filings will fall off.

3. The cauſe of the ſurprizing *phænomena* of the load-ſtone has hitherto eſcaped our knowledge, though diligently inquired after by men of abilities. Such a diſcovery is not to be made without long experience, and a great variety of facts: and the nature of the ſubject is ſuch, that the more facts we are acquainted with, the more we find ourſelves perplexed. The concluſions we draw from ſome experiments are ſeemingly contradicted by others: and yet theſe ſeeming contradictions are oft-times very reconcileable upon further experience. If what I am about to lay before the *Society* will in any-wiſe contribute to remove theſe difficulties, I am in hopes it will not be unacceptable, though I ſhould not ſo properly explain the nature of the cauſe, as the manner in which it acts. Many of theſe experiments are not altogether new, but have not been ſo much attended to as they ſeem to deſerve.

Some further Exp. *relating to the general* phænomena *of* Magnetiſm; *by the ſame.* *ibid.* p. 665. *Read* Dec. 17. 1747.

The magnetic matter of a load-ſtone moves in a ſtream from one pole to the other internally, and is then carried back in curve lines externally, till it arrives again at the pole where it firſt entred, to be again admitted. *Prop.* I.

If we lay a magnetical body under a piece of paper or glaſs that is ſtrewed over with ſteel filings or magnetical ſand, and by ſtriking the table put the filings in motion, they will readily diſpoſe themſelves in ſuch a manner as to repreſent, with great exactneſs, the courſe of the magnetic matter. Steel rendered magnetical is beſt for this purpoſe, *Exp.* I.

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becaufe it is of a more uniform texture, than load-ftones, and will on that account exhibit a more regular appearance. By this exp. the curve lines in which the magnetical matter returns back to the pole where it firft entered, are accurately expreffed by the arrangement of the filings. The largeft curves are fuch as take their rife from one polar furface, and are extended to the other; being larger in proportion as they arife nearer the axis or centre of the polar furface. Thofe curves which arife from the fides of a magnetical body, are always interior to thofe which arife from the polar furface; and are lefs and lefs in proportion to their diftance from the ends. If any one fhould doubt, whether the magnetical matter, which thus difpofes the filings is really moving back in a direction contrary to that with which it paffes through the magnetical body; let him try it in different parts with a fmall compafs needle, and the fact will appear beyond difpute.

Exp. II. The larger the diftance is from pole to pole in different magnets, the larger will thefe curves be. This appears from examining magnets of different lengths. And this is the reafon why in the fame magnet the curves are lefs in proportion to their greater diftance from the ends of the bars. For the poles from whence thefe curves arife are proportionably nearer each other.

Exp. III. If the S. pole of one magnet be oppofed to the N. of another, moft of the magnetic matter is carried directly out of one into the other: and does not return back in curve lines till after having paffed through both magnets. It appears from the arrangement of the filings that the magnetic matter proceeding from the polar furface, does not now diverge from the axis as before, but runs more in ftreight lines till it arrives at the polar furface of the other magnet. The curves arifing from the fides, which before were bent towards the oppofite end of the fame magnet, are many of them now bent the contrary way towards the correfponding fides of the other magnet. Thofe which are not bent the contrary way, are fuch as are too remote from the oppofed pole of the other magnet to be influenced thereby; and therefore continue their natural courfe.

Exp. IV. Whilft the bars are in the pofition of the laft experiment, if a fmall load-ftone be placed in the ftream running from one to the other in any pofition whatfoever, the ftream will pafs through the ftone: which being again removed, will be found to have a polarity exactly in the direction of that ftream.

Exp. V. If the N. or S. poles of two magnets be oppofed to each other, the filings will exhibit the appearance of two ftreams meeting; and the curves of each will all be turned towards the oppofite pole of the fame magnet. The appearance is altogether the fame, whether two N. or two S. poles be oppofed to each other. So that it is not to be determined from any of thefe experiments at which of the poles the magnetic ftream enters. As we have fome reafon to think it enters at the N. pole, we may fuppofe that the cafe, without danger of error; provided we build

build nothing upon the ſuppoſition, but what would hold good (*mutatis mutandis*) if the contrary ſhould be true. This being ſuppoſed, when the S. poles are oppoſite, the two ſtreams coming out at them are directly contrary, whereby the magnetic matter is accumulated, and therefore diverges ſo much the faſter to return back to the N. poles. When the N. poles are oppoſed to each other, the ſtreams of magnetic matter returning from the S. poles are directly contrary; and by crouding at once towards each polar ſurface are accumulated betwixt them, and converge towards them ſo much the faſter.

Theſe 5 experiments ſeem ſufficient to eſtabliſh the truth of the propoſition; many more might be produced to the ſame purpoſe.

The immediate cauſe, why two or more magnetical bodies attract each other, is the flux of one and the ſame ſtream of magnetical matter through them. *Prop.* 2.

It appeared in the third experiment, that when the S. pole of one magnet was oppoſed to the N. of another, a ſtream of magnetic matter was carried from one to the other, and did not return back to the pole where it firſt entered, till after having paſſed through both bars; and it is needleſs to obſerve, that two bars in this poſition are in a ſtate of attraction. The fifth experiment ſhewed, that when the two S. or N. poles were oppoſed, there was no ſtream common to both. Now it is well known, that magnetical bodies in this ſituation are ſo far from attracting, that they ſtrongly repel each other. If the third experiment be repeated, with the magnets placed at different diſtances from each other, we ſhall find that more of the magnetical matter will paſs from one polar ſurface to the other, in proportion as the diſtance betwixt is leſs. The attraction is therefore greater as the diſtances diminiſh. And at diſtances where none of the magnetic ſtream paſſes from one magnet to the other, there is no ſign of attraction. So that this cauſe is not only co-exiſtent with the effect, but alſo proportionable thereto. *Exp.* VI.

If a piece of ſoft iron which has no fixed magnetiſm is any where placed in the magnetical ſtream, it will be in a ſtate of attraction whilſt it remains in that ſtream, and no longer. *Exp.* VII.

A ball of ſoft iron in contact with the pole of a magnet will attract a ſecond ball, and that a third, and ſo on, till the ſtream becomes too weak to produce an attraction ſufficient to ſupport a greater weight. *Exp.* VIII.

Having hung a number of balls to each other, by applying the firſt to the N. pole of a magnet, upon preſenting the S. of another magnet to one of the middle balls; all thoſe below it will thereby be deprived of the magnetic ſtream, and inſtantly loſing their power of attraction fall aſunder: the ball, to which the magnet was applied, will be attracted by it, and all the others will ſtill remain ſuſpended. But if the N. end of a magnet be preſented, then the ball to which it is applied will alſo drop. *Exp.* IX.

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Exp. X. In a magnet unarmed the magnetic ſtream is carried back on all ſides in curve lines to the contrary pole, as was ſeen in *Exp.* I. but when armour is applied to each pole, the magnetic matter is thereby conducted to the feet of the armour; and a lifter being thus applyed to the feet, the whole ſtream coming out at one pole is carried back through it to the other: by which means the lifter is made to adhere to the feet of the armour with very great force. When the lifter is thus in contact, the magnet ſeems externally to have loſt the greateſt part of it's force; though in reality it never acted with more. If inſtead of the lifter we ſuſpend a number of iron balls in contact, they will adhere together, and hang like a bracelet betwixt the two feet; the returning ſtream paſſing now through them, as before through the lifter. Preſent the pole of a magnet, and they inſtantly fall aſunder.

Prop. III. *The immediate cauſe of magnetic repulſion is the conflux and accumulation of the magnetic matter.*

It appeared in *Exp.* V. that the ſame poles of two different magnets being oppoſed to each other, there was a conflux and accumulation of the magnetic matter; and we find by experience, that all magnetical bodies in a like ſituation are in a ſtate of repulſion.

Exp. XI. Two ſmall bars, the one hard, the other of a ſpring temper, being both magnetical matter, were oppoſed to each other, S. to S. the filings produced the ſame appearance of repulſion, as deſcribed in *Exp.* V. then the bars being brought ſo near as to touch each other at the ſame poles, the repulſion was inſtantly changed into attraction.

A letter from the ſame to the Preſ. *concerning the* poles *of* magnets *being variouſly placed.* N°. 476. p. 361. Apr. *&c.* 1745. dated *London,* *April* 3. 1745. *Read* April 4. 1745.

III. The favourable reception which thoſe magnetical experiments met with, which you lately did me the honour to communicate to your learned *Society*, incourages me to hope, that the following facts are remarkable enough to merit their attention.

1. I cut a piece of natural loadſtone into the ſhape of a parallelopiped, 1 inch $\frac{8}{10}$ in length, in breadth $\frac{4}{10}$ of an inch, and $\frac{2}{10}$ in thickneſs; it's weight was 3 drams and 10 grains. In this ſtone I placed the magnetical virtue, in ſuch a manner that the two oppoſite ends became, both of them, S. poles; and the middle was, quite round, a N. pole.

2. Another ſtone was in length 1 inch $\frac{1}{10}$, in breadth $\frac{7}{10}$ and in thickneſs about $\frac{2}{10}$ at a *medium*, it being thicker at one end than at the other: its weight 1 dram 57 grains. The 2 oppoſite ends of this ſtone I made both N. poles, and the 2 oppoſite ſides S. poles.

3. An irregular ſtone, that weighed about 5 ounces and a half, had 2 broad flat ſurfaces oppoſite to each other, at the diſtance of 1 inch and $\frac{3}{10}$. I made half of each of theſe ſurfaces a N. pole, and the other half a S. pole; ſo that the N. pole of one ſurface was oppoſite to the S. pole of the other ſurface, and *vice verſâ.*

4. I took a ſtone of a pretty good kind, that had a grain very apparent, running the lengthways of it: it was 1 inch $\frac{4}{10}$ in length, 1 inch $\frac{3}{10}$

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in breadth, and it's thickness at the sides was $\frac{6}{10}$ of an inch; but in the middle $\frac{7}{10}$ it being tapered away from the middle to the sides; it's weight was 3 ounces wanting 4 grains. At one end of it I placed a N. pole surrounded by a S. and at the other end a S. surrounded by a N. pole; so that the edges of each surface had a pole of a different denomination from that which occupied the middle.

A great many varieties of this kind might be easily devised, but the examples seem sufficient to shew how manageable the magnetic virtue is in respect to it's direction; and how defective most of the hypotheses are, which have been raised to account for the *phænomena* of the loadstone.

A description of a Mariner's Compass *contrived by* Gowin Knight, *M. B. F. R. S.* N°. 495. p. 505. Apr. *&c.* 1750. *Read* July 5. 1750.

IV. The discovery of the mariners compass has probably been of more general and important use to human society, than the invention of any one instrument whatsoever: and yet so far have they been from studying the improvement of it, that there would be no absurdity in supposing that the first which was made might be as much superior to those in common use now, as the most improved instrument we have is superior to it's first contrivance.

The compass which appeared before this *Society* last year on account of it's being rendered useless by lightning *, was what afforded me the first idea of their imperfections, some of which I then enumerated; but others have since occurred to me, arising from the structure of the needle, which I had not sufficiently considered at that time. It was then observed, that almost all the compasses on board our merchant-ships had their needles formed of two pieces of steel wire; each of which was bent in the middle, so as to make an obtuse angle; and their ends, being applied together, make an acute one; so that the whole represents the form of a lozenge; in the centre of which, and of the card, is placed the brass cap. I procured 20 cards, with needles of this kind fixed to them; and after touching them with a pair of large bars, I tried each of them, with the same cup and pin, by drawing them aside 90° from the true point, and then seeing where they would rest. I found them all to vary more or less, either to the E. or W. and some of them as far as 8°. Few of them came to the same degree twice together; and when they did, that was never the true point. In short, they not only varied from the true direction, but from one another, and from themselves. I then tried, by drawing them gently aside, how far I could make them stand from the true point, without returning; and found they might frequently be made to do it at the distance of a whole point on either side. One of them, which generally varied 6 or 7° to the E. being drawn the same way, would stand at 16°.

All these irregularities are owing to the structure of the needle: for the wires, of which it is composed, are only hardened at the ends; and

* See Art. VI. of this Chap.

that is done by making the ends red hot, and quenching them in water: if all thefe ends are not equally hard, or if one end be hardened higher up than the other, when they come to be put together, in fixing them to the card, that end which is hardeft, will deftroy much of the virtue of the other; by which means the hardeft end will have moft power in directing the card, and muft confequently make it vary towards it's own direction. If you retouch thefe wires when fixed to the card, the error will ftill remain; for that wire which is beft hardened will always become the ftrongeft. Confidering how uncertain this method of hardening the ends of the wires muft be, it is a great chance if they fhould once in an hundred times be equally and uniformly hard: and unlefs they are, the card to which they are fixed muft neceffarily vary.

The wires being difpofed in the form of a lozenge, is the reafon why thefe cards had fo little force, that they might be made to ftand at the diftance of feveral degrees, on either fide the point from whence they were drawn. For all magnetical bodies receive an additional ftrength, by being placed in the direction of the earth's magnetifm, and act proportionably lefs vigoroufly when turned out of it. Wherefore, when thefe kind of needles are drawn afide from their true point, two of the parallel fides of the lozenge will confpire more directly than before with the earth's magnetifm; and the other two will be lefs in that direction: by which means the two firft fides will very much impede it's return; and the two latter will have that impediment to overcome, as well as the friction, by their own force alone.

The needles that are ufed on board the men of war, and fome of the larger trading fhips, are made of one piece of fteel, of a fpring temper, and are broad towards the ends, but tapering towards the middle, where a hole is made to receive the cap. At the ends they terminate in an angle greater or lefs, according to the fkill or fancy of the workman. Now, though the worft of thefe are infinitely preferable to thofe of wire, yet the beft of them are far from being perfect. Every needle of this form has 6 poles inftead of two. There is one at each end, two where it becomes tapering, and two at the hole in the middle. This is owing to their fhape; for the middle part being very flender, it has not fubftance enough to conduct the magnetic ftream quite through from one end to the other. All thefe poles appear very diftinctly, when examined with a glafs that is fprinkled over with magnetic fand. Neverthelefs this circumftance does not hinder the needle from pointing true; but as it has lefs force to move the card, than when the magnetic ftream moves in large curves from one end to the other, it is certainly an imperfection.

I examined a hard needle of this fort, whofe ends were very broad, and terminated in an acute angle; and obferved, that, tho' it's motion was very free and vigorous, yet I could make it ftand one degree on either fide the true point; and being at a lofs to account for it, I tried what appearance it would make under a glafs with magnetic fand, and difcovered difcover'd that the magnetic ftream came out of the fides, which formed the acute angle at the ends, in lines that were almoft perpendicular to thofe fides, and then was bent round to go to the other pole: from whence I concluded, that when the needle was drawn a little from the true point, the ftream, which came out of one of thefe fides, would be more in the direction of the earth's magnetifm than before; on which account it would act ftronger in retaining the needle in that fituation, than the ftream of the other fide in reftoring it; efpecially as that ftream would be now weaker, on account of it's being turned out of the magnetical line, and would have the friction betwixt the cap and pin to overcome at the fame time.

I tried two other needles, whofe ends were formed into angles very obtufe, and could not find that they were liable to the fame objection.

Two needles, that were quite ftrait, and fquare at the ends, were found to have only two poles; but about the hole in the middle the curves were a little confufed. Thefe always came exactly to the fame point, after vibrating a long time; and if drawn never fo little on one fide, would return to it again without any fenfible difference. We may therefore conclude, that a regular parallelopiped is the beft fhape for a needle, as well as the fimpleft; with the holes for the caps as fmall as can well be contrived; or if it can be made to anfwer the purpofe without any hole at all, it will be ftill more perfect.

Yet the common fhape has one advantage which this has not: for, being made broad at the ends, and flender in the middle, it's weight is removed as far as poffible from the centre: on which account, if it once points true, the friction at the centre cannot fo eafily put it in motion; and it's vibrations, when in motion, will be flower; fo that their limits may be more nicely obferved, and the middle point betwixt them is that where it would ftand, if at reft. Being unwilling to part with thefe advantages, I contrived a light circle of brafs, of the fame diameter with the card, which will fupply a weight acting at the greateft diftance from the centre of motion, and alfo ferve to fupport the card; which may now be made of thin paper, without any thing to ftiffen it. So that the extraordinary weight of the brafs ring is compenfated in a great meafure by the lightnefs of the card. This ring is of fervice in another refpect; for, being fixed below the card, and the needle above it, the centre of gravity is placed low enough to admit of the cap being put under the needle; whereby the hole in the needle becomes unneceffary; and the latter being placed above the card, renders it eafier to be touched with a pair of bars.

Having thus completed the needle and card to my fatisfaction, what chiefly remains, is, to contrive fuch a cap and point as will have the leaft friction, and be moft likely to continue in a ftate of perfection. The caps in ufe are either of brafs, a mixed metal, like that of a reflecting telefcope, cryftal, or agate. The two firft will only admit of brafs points, and the latter are rather too expenfive for common ufe. Wherefore I bethought myfelf of trying glafs caps: I had three of them made by a glafs-blower, two of which I got polifhed: they were all fet in brafs, fo as to fcrew into the fame needle, which had alfo one of agate fitted to it. I compared them with that of agate, by trying with each of them how many vibrations the fame card and needle would make, when drawn afide 90°, on the fame point; which was a very fmall fewing needle.

The number of vibrations with the agate cap, on the firft trial, were 39, then 37, then 39 again; with one of the glafs caps it made 23, and then 20. This difference from the agate cap was fo great, that I concluded the point muft be damaged, and therefore chofe a finer; on which the fame glafs cap made 41 vibrations; then 43; and another glafs cap made 47, and the next time 43. But the agate cap with this point made 51, 57, and 58 vibrations. The unpolifhed glafs cap performed much the fame with the others. I had two of them polifhed again by Mr *Smeaton*; and in company with him repeated the fame experiments; but with no better fuccefs. The agate cap made always many more vibrations than the glafs one; and generally with the latter the number diminifhed by repeated trials; whereas with the agate cap it ufually increafed.

Thefe experiments made me lay afide the thoughts of glafs caps, and put me upon thinking how agate ones might be made with as little expence as poffible.

With this view I got a cap turned of ivory, in fuch a manner as to receive a fmall bit of agate at the top. This being ground concave, and polifhed on that fide, where it formed the *apex* of the hollow cone in the cap, was capable of anfwering the purpofe as well as if the whole had been agate, and was much lighter. Thefe caps may be made cheap enough for common ufe; and, if good at firft, cannot eafily be impaired.

For a point, I chofe a common fewing needle, and contrived to fix it in fuch a manner as to be taken out with the greateft eafe, and replaced by another, if neceffary; by which means an excellent point may be always had with little trouble or expence. Common needles, when well tempered, have all the qualifications that can be defired for the purpofe intended. The fmalleft are ftrong enough to bear the weight of a card; and are neither fo foft as to be liable to bend, nor fo hard and brittle as to break; and they are generally better pointed than any that a common workman could pretend to make *extempore*.

Thus we have gone through all the parts that are effential to a mariner's compafs; and endeavoured to put them upon fuch a footing, as to leave as little room as poffible for error in their firft conftruction, or failure in the long continued ufe of them.

This, which I have now the honour to exhibit to the *Society*, was made by Mr *Smeaton*, a gentleman whofe uncommon fkill in the theory and practice of mechanics has enabled him to execute whatever I propofed poſed in ſuch a manner as always to exceed my expectations: and not only ſo, but he has added a conſiderable improvement of his own. By a very ſimple contrivance he has made the ſame inſtrument capable of ſerving the purpoſes of an azimuth and amplitude compaſs; and that in a manner much preferable to any thing hitherto contrived; the deſcription and uſe of which he has drawn up himſelf, for the peruſal of the *Society*.

V. The cover of the wooden box being taken off, the compaſs is in a condition to be made uſe of in the binacle, when the weather is moderate: but if the ſea runs high, as the inner box is hung very free upon it's centres (the better to anſwer it's other purpoſes) it will be neceſſary to ſlacken the milled nut, placed upon one of the axes that ſupports the ring, and to tighten the nut on the outſide that correſponds to it. By this means the inner box and ring will be lifted up from the edges, upon which they reſt, when free; and the friction will be increaſed, and that to any degree neceſſary to prevent the too great vibrations; which otherwiſe would be occaſioned by the motion of the ſhip.

An account of ſome improvements of the Mariners Compaſs, *in order to render the* Card *and* Needle *propoſed by Dr* Knight, *of general uſe; by* John Smeaton, *Philoſophical Inſtrument-maker.* Ibid. p 513. *Preſented* July 5, 1750. *here printed with alterations.*

To make the compaſs uſeful in taking the magnetic azimuth, or amplitude of the ſun and ſtars, as alſo the bearings of head-lands, ſhips, and other objects at a diſtance, the braſs edge, deſigned at firſt to ſupport the card, and throw the weight thereof as near the circumference as poſſible, is itſelf divided into degrees and halves; which may be eaſily eſtimated into ſmaller parts, if neceſſary. The diviſions are determined by means of a cat-gut line ſtretched perpendicularly within the box as near the braſs edge as may be, that the parallax ariſing from a different poſition of the obſerver may be as little as poſſible.

Underneath the card are two ſmall weights, ſliding on two wires, placed at right angles to each other; which, being moved nearer to, or farther from the center, counterbalance the dipping of the card in different latitudes, or reſtore the *equilibrium* of it, where it happens by any other means to be got too much out of level.

There is alſo added an *index* at the top of the inner box, which may be put on and taken off at pleaſure, and ſerves for all altitudes of the object. It conſiſts of a bar, equal in length to the diameter of the inner box; each end being furniſhed with a perpendicular ſtile, with a ſlit parallel to the ſides thereof. One of the ſlits is narrow, to which the eye is applied, and the other is wider, with a ſmall cat-gut ſtretched up the middle of it, and from thence continued horizontally from the top of one ſtile to the top of the other: there is alſo a line drawn along the upper ſurface of the bar. Theſe four, *viz.* the narrow ſlit, the horizontal cat-gut thread, the perpendicular one, and the line on the bar, are in the ſame plane, which diſpoſes itſelf perpendicular to the horizon, when the inner box is at reſt, and hangs free. This *index* does not move round, but is always placed on ſo as to anſwer the ſame ſide of the box.

When

When the ſun's azimuth is deſired, and his rays are ſtrong enough to caſt a ſhadow, turn about the wooden box, till the ſhadow of the horizontal thread; or (if the ſun be too low) till that of the perpendicular thread in one ſtile, or the light through the ſlit in the other, falls upon the line on the *index* bar, or vibrates to an equal diſtance on each ſide of it, gently touching the box, if it vibrate too far: obſerve at the ſame time the degree marked upon the braſs edge by the cat-gut line. In counting the degree for the azimuth, or any other angle that is reckoned from the meridian, make uſe of the outward circle of figures upon the braſs edge, and the ſituation of the *index* bar, with regard to the card and needle, will always direct upon what quarter of the compaſs the object is placed.

But if the ſun does not ſhine out ſufficiently ſtrong, place the eye behind the narrow ſlit in one of the ſtiles, and turn the wooden box about, till ſome part of the horizontal or perpendicular thread appears to interſect the centre of the ſun, or vibrate to an equal diſtance on each ſide of it, uſing ſmoked glaſs next the eye, if the ſun's light is too ſtrong. In this method another obſerver will be generally neceſſary to note the degree cut by the cat-gut line, at the ſame time the firſt gives notice that the thread appears to ſplit the object.

From what has been ſaid, the other obſervations will be eaſily performed; only in caſe of the ſun's amplitude, take care to number the degree by the help of the inner circle of figures on the card, which are the complements of the outer to 90, and conſequently ſhew the diſtance from E. or W.

The azimuth of the ſtars may alſo be obſerved by night; a proper light ſerving equally for one obſerver to ſee the thread, and the other the degree upon the card.

It may not be amiſs to remark farther, that, in caſe the inner box ſhould loſe it's *equilibrium*, and conſequently the *index* be out of the plane of a vertical circle, an accurate obſervation may ſtill be made, provided the ſun's ſhadow is diſtinct: for, by obſerving firſt with one end of the *index* towards the ſun, and then the other, a mean of the two obſervations will be the truth.

Explanation of the figures. Fig 102.

Fig. 102. is a perſpective view of the compaſs, when in order for obſervation. The point of view being the centre of the card, and the diſtance of the eye two feet. *AB*, is the wooden box. *C* and *D* are two milled nuts; by means whereof the *axes* of the inner box and ring are taken from their edges, on which they move, and the friction increaſed, when neceſſary. *EF* is the ring that ſupports the inner box. *GH* is the inner box; and *I* is one of it's *axes*, by which it is ſuſpended on the ring *EF*. *KL* is the magnet or needle; and *M* a ſmall brace of ivory, that confines the cap to it's place. See *Fig.* 103. The card is a ſingle varniſhed paper, reaching as far as the outer circle of figures, which is a circle of thin braſs, the edge whereof is turned down

down at right angles to the plane of the card to make it more ſtiff. *O* is a cat-gut line drawn down the inſide of the box; for determining the degree upon the braſs edge. *P Q R S* is the *index* bar, with it's two ſtiles and cat-gut threads; which being taken off from the top of the box, is placed in two pieces, *T* and *V*, notched properly to receive it. *W* is a place cut out in the wood, ſerving as an handle.

Fig. 103. is the card *in plano* with the needle fixed upon it; being one third of the diameter of the real card. Fig. 103.

Fig. 104. is a perſpective view of the backſide of the card, where *A B* repreſents the turning down of the braſs edge. *C* is the under part of the ivory cap. *D* and *E* are the two ſliding weights to balance the card; and *F* and *G*, two ſcrews that fix the braſs edge, *&c.* to the needle. Fig. 104.

Fig. 105. is the pedeſtal that ſupports the card, containing a ſewing needle, fixed in two ſmall grooves to receive it, by means of the collet *C*, in the manner of a port-creyon. At *D* the ſtem is filed into an octogon, that it may be the more eaſily unſcrewed. Fig. 105.

A letter from Capt. John Waddell *to Mr* Naphthali Franks, *Merchant, concerning the effects of* Lightning *in deſtroying the* Polarity *of a* Mariners Compaſs; *to which are ſubjoined ſome* Remarks *thereon, by* Gow. Knight, *M. B. F. R. S.* N°. 492. p. 111. Apr. &c. 1749. *Read* April 13. 1749.

VI. *Jan.* 9. 1748-9. the new ſhip *Dover*, bound from *New York* to *London*, being then in Lat. 47° 30′ north, and long. 22° 15′ weſt, from *London*, met with a very hard ſtorm of wind, attended with thunder and lightning, as uſual, moſt part of the evening, and ſundry very large comazants (as we call them) over-head, ſome of which ſettled on the ſpintles at the topmaſt heads, which burnt like very large torches; and at 9 *p. m.* a ſingle loud clap of thunder with lightning ſtruck the ſhip in a violent manner, which diſabled myſelf, and great part of the ſhip's company, in the eyes and limbs; it ſtruck the mainmaſt about $\frac{2}{7}$ up almoſt half-through, and ſtove the upper deck one carling, and quick-work; part of which lightning got in between decks, ſtarted off the bulk-head, drove down all the cabbins on one ſide of the ſteerage, ſtove the lower deck, and one of the lower deck main lodging-knees.

Another part of it went through the ſtarboard ſide, without any hurt to the cieling (or inſide plank); and ſtarted off from the timbers four out-ſide planks being the whale upwards; one of which planks, being the ſecond from the whale, was broke quite aſunder, and let in, in about 10 or 15′, 9 feet water in the ſhip.

It alſo drew the virtue of the loadſtone from all the compaſſes, being 4 in number, all in good order before, one in a braſs and three in wooden boxes. The hanging compaſs in the cabbin was not quite ſo much diſabled as the reſt; they were at firſt very near reverſed, the N. to the S.; and after a little while rambled about ſo as to be of no ſervice. The ſtorm laſted 5 days, we loſt our mainmaſt and mizenmaſt, and almoſt all our ſails; arrived at *Cowes Jan.* 21. in a very ſhattered condition.

When

An account of the Mariners Compaſs, *that was ſtruck with* Lightning, *and ſhewn at the laſt meeting of the* Royal Society; *with ſome further particulars relating to that accident; communicated by* Gowin Knight, *M. B. F. R. S.*

When I came to examine the compaſs ſtruck with lightning, I obſerved that the outward caſe was joined together with pieces of iron wire, 16 of which were found in the ſides of the box, and 10 in the bottom. I applied a ſmall needle to each of theſe wires, and immediately perceived that the lightning had made them ſtrongly magnetical; particularly thoſe that joined the ſides. All the heads of the wires on one ſide of the box attracted the N. point of the needle, and repelled the S. whilſt all the heads on the the other ſide attracted the S. and repelled the N. The wires at the bottom attracted the S. and repelled the N. but it is not certain, whether this polarity was any-ways owing to the lightning; ſince it might be acquired by their continuing long in an erect poſture.

In examining the card, I found the needle was vigorous enough in performing it's vibrations, but that it's polarity was inverted; the N. point turning conſtantly to the S. I then tried to take out the card, to examine the ſtate and ſtructure of the needle: but the junctures were every-where well ſecured with putty, and that grown ſo hard, that I was obliged to uſe ſome violence, and at laſt broke the glaſs. The needle (if I may ſo call it) conſiſted of two pieces of ſteel wire, each of which was bent in the middle, ſo as to make an obtuſe angle; and the ends of theſe wires applied together, forming an acute one, the whole appeared in the ſhape of a lozenge; in the centre of which was placed the braſs cap whereon the card turned. And ſo far was it from being made with any tolerable degree of exactneſs, that there was not the leaſt care taken either to bend the wires in the middle, or to fix the cap exactly in the centre of the lozenge: for, upon trying it with a pair of compaſſes, I found it's greateſt eccentricity to be full $\frac{2}{10}$ of an inch. The pin, upon which it turned was made of a ſlip of plate-braſs ſharpened to a point.

Beſides the particulars already communicated to the Society, the Captain informed me, that he was obliged to ſail above 300 leagues, after this accident happened, without a compaſs, till he arrived at *Cowes* in the *Iſle of Wight*; where being provided with one, he placed it in the binacle, but was much ſurprized to find that it varied from the direction it ſtood at when out of the binacle nearly 2 points. He removed the binacle to different parts of the deck, but found that it always made the needle to vary after the ſame manner when placed in it. He repeated the ſame experiment lately in the river, with the like ſucceſs; only that he obſerved, that the variation of the needle, when placed in the binacle, was rather leſs than at firſt. It was natural to enquire if there was any iron about the binacle; but I was ſurprized when the Captain informed me, he had given ſtrict charge to the maker not to put ſo much as a ſingle nail in it; and that he firmly believed that there was not the leaſt bit of iron about it

Being willing to be ſatisfied of the truth of a circumſtance ſo very extraordinary, the Captain was deſired to ſend the binacle to a houſe in the

the City; where, in company with the Captain, Mr *Ellicot*, and another Gentleman, I tried it with a large compaſs touched by my bars; but finding no ſenſible variation, we at that time deſiſted, thinking the fact quite improbable: but having diſcovered the effect which the lightning had produced upon the wires which faſtened the ſides of the compaſs-box, I was induced to examine the binacle a ſecond time; which I did with a ſmall compaſs, and with great care, in every part; and at laſt, about the middle of the binacle, I found it to vary very ſenſibly, but could not diſcover any nails or iron any-where thereabouts; till, turning it up to examine the bottom, I there found 3 or 4 large nails or rather ſpikes, driven through it to faſten the upright partitions in the middle of the binacle.

It would not be difficult to explain why any needles, under the like circumſtances with thoſe above related, ſhould be rendered uſeleſs by lightning, though the needles themſelves had remained unhurt. So many iron wires made ſtrongly magnetical would doubtleſs have effected it; and 3 or 4 large nails in the binacle, if made magnetical, would alone have been ſufficient to have done it. But I have already taken notice that the polarity of the needle was inverted by this accident; and I would further obſerve, that all needles conſtructed after this manner are liable to be rendered uſeleſs not only by the lightning's deſtroying their virtue, but alſo by it's placing it in a particular direction; *e. g.* if the lightning ſtruck the needle in the direction of either of the two parallel ſides of the lozenge, it muſt ſtrike the other two ſides very obliquely; whereby the firſt two ſides may have their polarity deſtroyed, and a very ſtrong one given them in the contrary direction; whilſt that of the other ſides, if it be inverted, will be very weak; but it is probable that the virtue would be placed obliquely in the direction of the ſtroke; in either caſe, theſe two ſides can contribute but very little (if any thing) in directing the card; and if the two firſt ſides only are capable of acting upon it, it will point in the direction of thoſe ſides, which will produce a variation of about 4 points.

It may further be obſerved, that a needle would not continue long in this ſtate, but would every day grow more and more regular; becauſe if the virtue be placed obliquely, it generally turns itſelf in the direction of any piece of ſteel that is long and ſlender; and that may be the reaſon why this card is now become regular, except that it is inverted.

The wires that join the box ſeem weaker than when I firſt examined them; which makes it very probable, that they might be vaſtly ſtronger when firſt ſtruck with the lightning: and the ſame may be likewiſe true in regard to the nails in the binacle; which may account for the experiments not anſwering exactly the ſame as at firſt.

From what has been ſaid it appears, that this form of needles is very improper, and ought to be changed for that of one ſtrait piece of ſteel; and then if a needle ſhould be inverted it might ſtill be uſed. It alſo ſhews the abſurdity of permitting iron of any kind about the compaſs-

 box

box, or the binacle. Whoever confiders the whole defcription here given of this compafs, I am perfuaded, he will efteem it a moft defpicable inftrument: how then muft any one be fhocked to hear, that almoft all the compaffes, made ufe of by our trading veffels, are of the fame fort! the boxes all joined with iron wire, and the fame degree of accuracy obferved throughout the whole!

This I am credibly informed, is the cafe; and that for no other reafon, but that one of this fort may be purchafed for 5 *s.* and it will coft about 2 *s.* 6 *d.* more to buy a tolerable good one. So that the lives and fortunes of thoufands are every day hazarded for fuch a trifling confideration.

Obfervations, made during the laft 3 years, of the quantity of the variation *of the* magnetic *horizontal* needle *to the W. by Mr* Geo. Graham, *F. R. S. at his houfe in* Fleet-ftreet, London. N°. 487. p. 279. Apr. &c. 1748. *Read* Apr. 21. 1748.

VII.

			° ′
1745	*March*	26	17 = 0
		29	17 = 0
	March	18	17 = 10
		21	17 = 10
	April	22	17 = 15
	May	4	17 = 18
		14	17 = 20
		16	17 = 15
	Dec.	18	17 = 25 +
	Febr.	24	17 = 30
1747	*Dec.*	19	17 = 40
	Jan.	4	17 = 40 —

The inclination of the dipping needle has been during the fame time about 73½ degrees.

N. B. As the variation of the Needle at London *has not been regularly publifhed from time to time in the* Philof. Tranf. *it may not be improper to take notice here, that according to the beft obfervations extant, and which were made by perfons of great fkill and exactnefs, the Needle at* London *declined to the* E. 11° 15′ *in the year* 1580. *In* 1657 *there was no variation, the needle then pointing due N. In* 1672 *the variation was obferved by the late Dr* Halley 2° 30′ *towards the* W. *and in* 1692 6° 0′. *And towards the beginning of the year* 1723, *it was found by* Mr Graham, *from the medium of a vaft number of obfervations, to be then* 14° 17′ *the fame way. So that, during the courfe of* 167 *years elapfed fince the year* 1580 *to the end of the laft year* 1747, *the magnetic needle at* London *has moved to the* weftward 28° 55′. See before N°. 148, and N°. 383, of the *Philof. Tranf.*

CHAP.

Fig. 102.

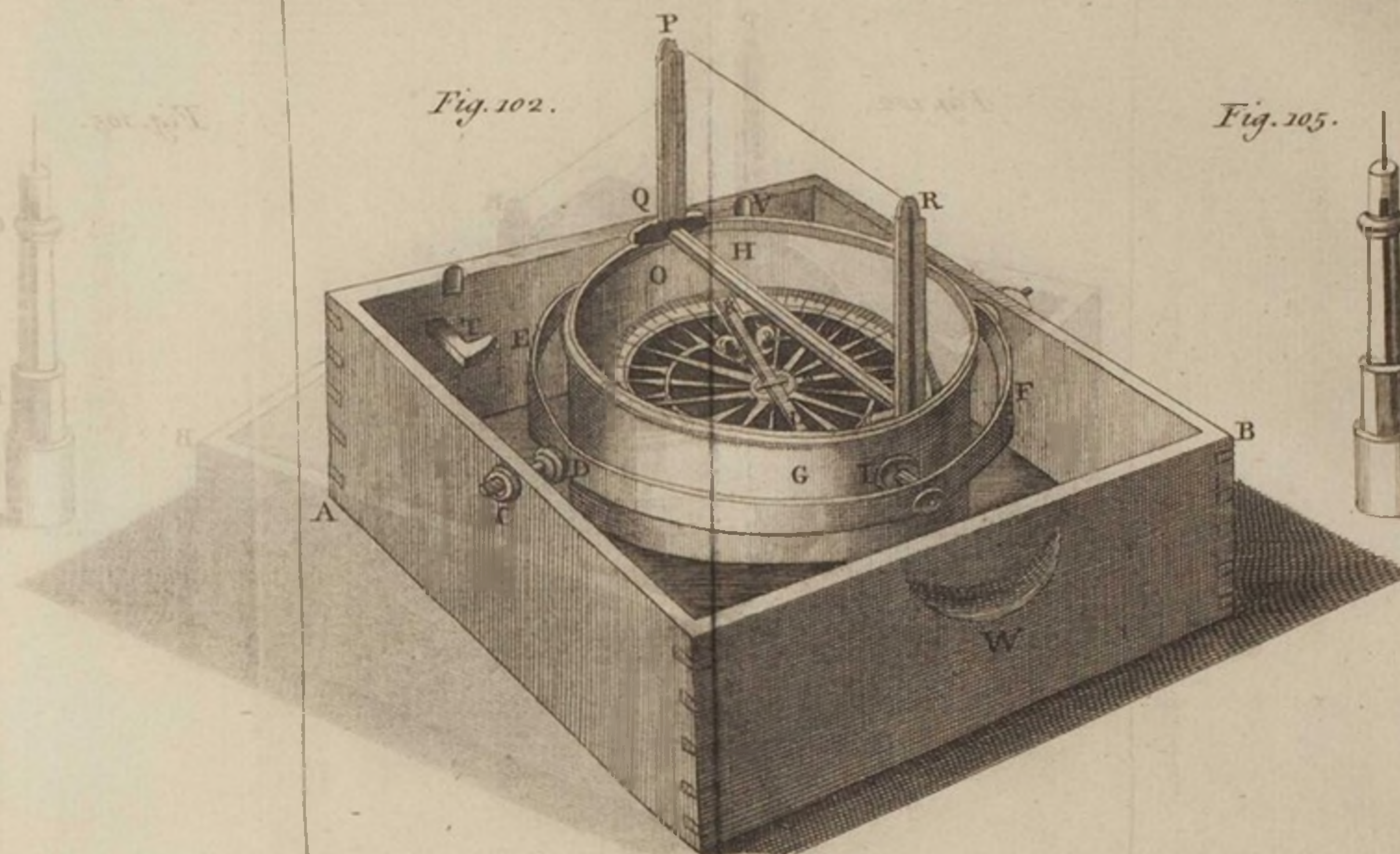


Fig. 105.

Fig. 103.

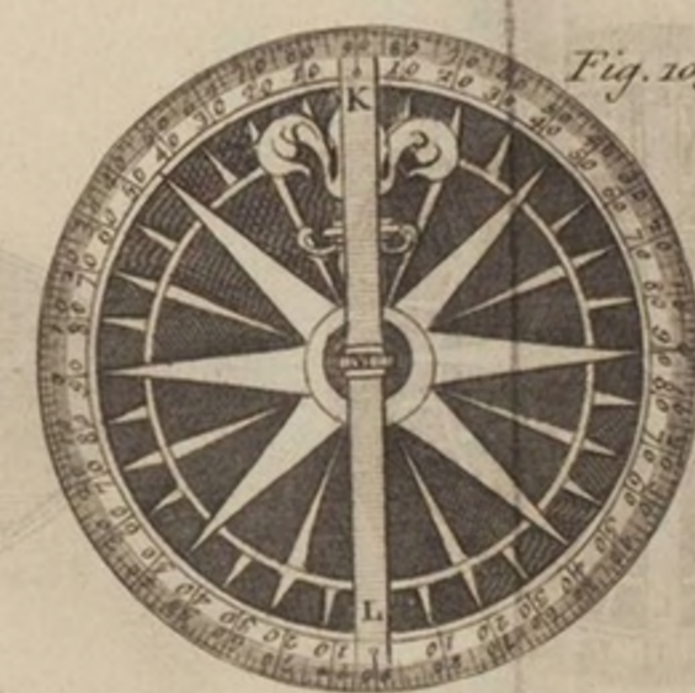
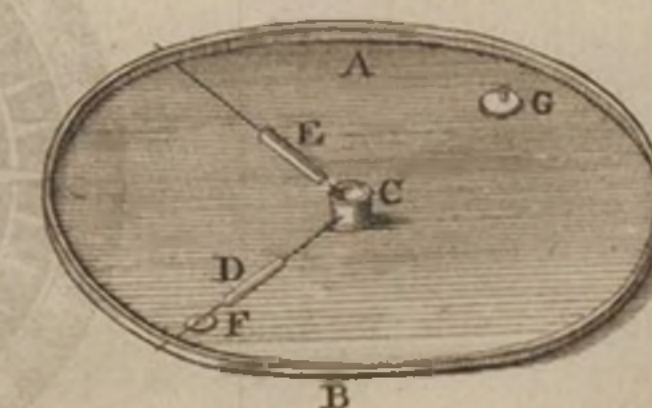


Fig. 104.



CHAP. V.

BOTANY, AGRICULTURE.

I. THIS plant is woody. It grows ſometimes into a tree, ſometimes into a ſhrub, and ſometimes into a buſh; ſpreading very tufted branches on all ſides down to the ground. It's native countries are the parts adjacent to the *Perſic Gulph*, the N. of *Arabia*, and the S. of *Perſia*. I cannot find that any author has known, or made the leaſt mention of it.

The eſtabliſhment of a new Genus *of* Plants, *called* Salvadora, *with it's deſcription; by* Laurence Garcin, *M. D. F. R. S. of* Neufchatel *in* Switzerland; *communicated in a letter to* Dr Mortimer, *Secr. R. S.* N°. 491. p. 47. Jan. &c. 1749. *Tranſlated from the* French, *by* T. Stack, *M. D. Read* Feb. 23. 1748-9.

First I will give it's characters, and then it's deſcription from accurate obſervations, which I have made on the ſpot.

Characters.

Calix. This is a monophyllous cup, divided into 4 lobes, which, as ſoon as they ſpread open, turn outward, and roll backward on themſelves; then wither, grow whitiſh, and dry up.

Corolla. It's flower is void of petals.

Stamina. Theſe are 4 in number, anſwering to the 4 lobes of the *calix*, and being likewiſe of the ſame length. They ſpring from the baſis of the *piſtillum*, and, as they ſhoot up, tend outward. Their ſummits are round, with a furrow turning in on one ſide; which gives each of them the form of a purſe.

Piſtillum. It is round, it's ſtyle ſingle and ſhort, and the *ſtigma* is blunt, and ſhaped like a navel.

Pericarpium. Is a round berry, of a middle ſize, with one cell or lodgment in it.

Semen. It is ſingle, ſpherical, incloſed in a callous firm ſkin, beſet with ſpots, forming a ſort of huſk like that of hemp.

I know but one ſpecies of this *genus*, which I deſcribe thus.

Deſcription.

It is a plant which varies conſiderably in ſize; that of a larger ſort of ſhrub, is what it moſt frequently grows to. It produces a number of boughs without order, and very tufted branches, which moſt commonly hang down to the ground. It's bark is moderately thick, ſometimes ſmooth, ſometimes full of cracks, of an aſh-colour, both in the trunk and branches, but green on the tender ſhoots. It's wood is every where brittle, and nearly of a ſtraw-colour.

The leaves are borne on young ſprigs, which ſhoot out along the boughs. Theſe ſprigs are ſtrait, generally ſhort, but ſometimes pretty long, like little wands. The leaves are thick-ſet, and tufted on the former, but thin on the latter. They grow ſometimes oppoſite to one

 another

another by pairs, croſſing alternately; and ſometimes by three and three diſpoſed like rays; but this more rarely. Their length, which varies on the ſame ſtalk, is generally from 1 $\frac{1}{2}$ inch to 2 $\frac{1}{2}$ inches, and their width is from 9 lines to an inch a little below the middle in each, which is the wideſt part. They are thick, pointed at their extremity, and rounded at their baſe, very even on their edges, ſomewhat ſucculent, but firm: their colour is a pale green, but ſomewhat yellowiſh, in thoſe that are ſhooting out. The pedicles which ſupport them are very ſhort, each being but $\frac{1}{2}$ a line in length, and $\frac{1}{4}$ in thickneſs. Every one of theſe pedicles, which is round, furniſhes a little nerve, which runs thro' the middle of the leaf; it is a little hollowed on the upper ſide, and ſomewhat raiſed on the back; and terminates at the end of it's reſpective leaf. This nerve gives 2 or 3 pair of almoſt imperceptible threads, which ſpread and divide into other ſmall irregular threads, through the body of the leaf. In fine, theſe leaves in ſhape nearly reſemble thoſe of the ſea-purſlain, and ſometimes thoſe of the miſletoe of the apple-tree. There are ſome generally on each plant, which have one, two, or more black ſpots, as in the *Perſicaria*, but almoſt round, and ſmaller.

The flowers, which are ſtamineous, that is, without petals, are ſmall, and diſpoſed in cluſters on the tops of the ſhoots. Theſe bunches of flowers intirely reſemble thoſe of the vine-bloſſoms. The empalement is ſmall, green on the under ſide, having four ſegments almoſt pointed, which roll outward, and then dry up. It's diameter in this rolled ſtate of it's lobes, is but of one line. The *ſtamina* are of a ſtraw-colour. The hollow furrow in each of their ſummits is not eaſily diſcovered without a glaſs.

The piſtil or *embryo* of the fruit, which is little, and yet occupies the whole inſide of the *calix*, is of the ſame colour with the bottom of this, that is, green. Afterwards it ſwells in all dimenſions, and grows into a berry, of the ſhape and ſize of a gooſeberry, of three or four lines in diameter. At firſt it is of a pale green, then a bright purple, and in it's maturity of a dark red. Each berry is ſupported on a ſtrong thick pedicle, attached to a ſmall bunch. It's ſubſtance is a white tranſparent fleſh, full of juice, much reſembling gelly, which ſurrounds a ſingle round grain, marbled with black or brown ſpots, as in the tortoiſe-ſhell, when ripe. This grain is as large as a grain of hemp-ſeed, that is, about two lines in diameter, but ſometimes leſs. It is properly a kernel, or a ſhell that has a cavity, which incloſes a ſort of little round almond, of a ſtraw-colour, yellowiſh on it's outward ſurface, and pale in it's inward ſubſtance, which is pretty firm.

Qualities. All the parts of our plant have an acid pungent taſte and ſmell, vaſtly like our garden-creſſes, but more biting. The fruit is the moſt pungent part of the whole. The ſmell of the plant is perceptible at 7 or 8 paces diſtance, when a perſon is to leeward.

The natives of the country uſe it againſt the bite of the ſcorpion, by rubbing the wounded part with it's bruiſed leaves. They employ alſo it's it's warm infusion to wash the bodies of their children, in order to keep them healthy. And they feed camels with it, who love it naturally.

This shrub, which is sometimes large, sometimes small, is most commonly found along high roads, and in dry low places of it's climate. As it's branches, which are slender and brittle, spontaneously bend downward, and form a thick tuft, this makes it generally resemble a great bush, which takes up a good deal of ground in most places, where it grows naturally. It delights in the hottest and driest places, such as those adjacent to the *Persic Gulph*, and perhaps more so than palm-trees: wherefore I doubt of there being any growing in the countries that lie to the east of the *Gulph*. And accordingly I have met with none, either in the neighbourhood of *Surat*, or in the kindom of *Bengal*, where there are regular rainy seasons every year. *Remarks.*

I should rather believe, it is more likely to be found in the deserts of *Africa*, on this side of our tropic; those being proper places for it, and where it rains seldomer than in any other part of the globe.

It's leaves have frequently excrescences of different sizes and shape, round, oval, and sometimes very large. They are the work of those flying insects, which commonly abound in these parts.

The inhabitants of the *Gulph* call this shrub by the name of *Tchuch*. Perhaps it's nature would not allow it to grow in lands far distant from the sea, no more than the *sea-plants*, to which this surname is given for that reason.

It's parts are all brittle, and even the leaves crack, if bent in the middle.

In fine, I have chosen a name, which I imagined I ought to give it, after the example of Mr *Linnæus*, who has called several plants by the names of Botanists of reputation. This laudable proceeding is a way to perpetuate the memory of all those who have contributed to the progress of Botany; and that much better than medals do with regard to Princes or Emperors. A proceeding, which, if duly pursued, will encourage those who come after us, to make useful discoveries in this science for the good of mankind, and in much greater number than have been published on the subject of plants up to our times. For it is easy to comprehend, that what remains to be discovered on this subject for our use, must infinitely surpass all that man has hitherto found out.

The name of *Salvadora*, which I have chosen for our shrub, is that of the late Mr *Salvador* of *Barcelona*, a very skilful Botanist, of whom M. *de Tournefort* makes mention in his Introduction to his *Inst. rei herbariæ*, where he stiles him *the Phœnix of his Nation*; because he was really the richest Naturalist, and the most expert in botanical matters that *Spain* ever produced. Before the last siege of *Barcelona*, in the years 1713 and 1714, they herborized together in *Catalonia*, and on the *Pyreneans*, while M. *de Tournefort* was on his travels there. They were intimate friends, and carried on a correspondence some years: and as I was personally acquainted with him for 3 or 4 years, and have likewise

likewise herborized with him before the said siege, and have since been honoured with his friendship and correspondence, I thought it incumbent on me to do honour to his memory, by giving his name to this plant. And I have done it with the greater justice, because it is certain, that, had he lived, he would have given a history of the plants of *Spain*, which, by it's accuracy, would have afforded much pleasure to the Botanists of *Europe*.

To conclude; from the characters of our *Salvadora* it is manifest, that it's place in *Tournefort*'s system ought to be in the first section of the 18th class. In the system of Mr *Linnæus* it ought to be placed after the *Rivina* in the fourth class, which receives plants with 4 *stamina* (*tetrandria* and *monogynia*).

An aquatic Plant *found at* Bagneres *in* Gascony; *by* *M.* Secondat de Montesquieu; *in a letter to* M. Folkes, *Esq*; *Pr. R. S.* N°. 472. p. 31. Jan &c. 1744. *Read Mar.* 8. 1743-4.

II. I have found, at *Bagneres*, a particular aquatic plant, which I had seen, for the first time, in the great bason of the boiling spring at *Dax*: it bears neither fruit nor flower, as far as appears; it's substance is entirely composed of small bladders full of air; the surface of it is like net-work or canvas; it grows only in the hottest mineral springs; it may be found at the spring, called, *de la Reine*, at the *Bath des Pauvres*, and at the *New Spring*; but most plentifully at that place where part of the *Spring de la Reine* issues out of a rock near the *Capuchins*. Nobody, as far as I know, has ever spoken of this plant, before I gave an account of it two years ago, at the public resumption of our academical meetings. The vegetation, and particular qualities of it, may, perhaps, deserve to be more narrowly examined; and I believe it may be properly called, *Fucus thermalis vesicularis, superficie reticulari*.

A description of the Cyanus foliis radicalibus partim integris, partim pinnatis, bractea calycis ovali, flore sulphureo; *by* Albert Haller, *Prof. Anat. & Bot.* Gotting. *R. S.* Aug. &, Suec. Soc. N°. 472. p. 94. Jan. &c. 1744.

III. The root is perennial: the leaves at the root are various, they are all green and firm, and have a small quantity of short down. Some are simple, with entire edges, and others are serrated. Others are half divided into unequal lobes. Others are variously semipennated and jagged. Others are quite pennated, with broad lobes, and the extreme one large and almost rhomboide. There are other leaves also of this sort accompanying the stalk, with a long, firm, hollow rib, on which are placed a great many pairs of lobes, 12 or more.

The stalk is a cubit high, winged, and having leaves rising under the origine of the branches.

The flowers are like those of the *jacea vulgaris laciniata*, but of a full yellow colour, like gamboge, and without smell. The head is of the same size as in the figure.

Fig. 106.

The *calyx* is globose, contracted at the upper part. The green part of the scales is oval; the dry part ovato-rhomboide, yellowish, and fringed. In the upper scales it has some dry, thin, serrated additions. The crown is composed of barren, reflext, bilabiated, quadrifid petals. The seed of the fertile ones is crowned with down; the floret is long, crooked, quinquefid, having one segment deeper cut. The tube of chives

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Fig. 106.

chives appears out of the floret, and out of that a club-headed tube. The ripe seed is a flat oval, and crowned with black hairs.

It was sent me by M. *Gerber* under the name of *jacea laciniata, flore luteo magno, squamis calycum ciliaribus, splendentibus.*

It's native place is in *Russia*, or at least on the banks of the *Wolga*.

By the empalement and flower, it is a species of *jacea*, according to *Vaillant*, according to me a *cyanus*, and according to *Linnæus*, a *centaurea*, under which name, he comprehends too great a number of plants, to have convenient names assigned them.

IV. *Geaster volvæ radiis & operculo elevatis.* *Geaster* is a *genus* of plants constituted by *Micheli*; of which the author discovered 5 *species*, and figured them in his *Nova Plantarum Genera.* It is so called from γῆ *terra*, and ἀστὴρ *stella*, on account of some acute angles radiated from the centre in all the *species* of this plant, like the coruscations of stars represented in pictures.

Concerning a plant but little known, and hitherto undescribed; by W. Watson, *F.R.S.* N°. 474. p. 234. June, &c. 1744. *Read* Dec. 20. 1744. *here printed with alterations.*

This *genus* is very nearly related to the *Lycoperdon* (*a*) of *Tournefort* and (*b*) *Linnæus*, or *Bovista* (*c*) of *Dillenius*, to the (*d*) *Lycoperdoïdes*, *Lycoperdastrum*, and (*e*) *Carpobolus* of *Micheli*, (*f*) *Linnæus*, and his follower (*g*) *van Royen* have comprised all these plants under the generical name of *Lycoperdon.* But with all the respect due to those eminent masters of Botany, to whose rules in constituting the *genera* of plants from the fructification I strictly adhere; yet in many of the *Cryptogamia* of *Linnæus*, as well as in the *Cryptantheræ* of *van Royen* it seems to be absolutely necessary to have recoursed to the habit of the plant, in constituting *genera*; especially in the *Fungi*, *Fuci*, *Algæ*, and perhaps in the Capillaries.

In order to form a description of this wonderful plant, I shall consider it in it's 3 parts, the *volva*, the *operculum*, and the *fructification.*

The *volva*, to speak not only with *Pliny*, but with *Clusius* also, and other moderns, is concave, representing the form of a cup, 1 $\frac{1}{2}$ inch in diameter. This cup has a broad base, from the centre of which, whilst the plant is still growing, it sends forth small fibrous roots, but few like the rest of it's kind that supply it with nourishment. But as moisture is by no means agreeable to this whole family of plants, the little roots, together with the centre of the *volva*, into which they were inserted, wither before the plant comes to maturity; which causes a hole to appear in the bottom of the base. But when the *volva* is risen to $\frac{1}{5}$ of an inch, it becomes quadrifid, having jaggs obtusely laciniated, a little reflexed at the point but entire at the edges. This *volva* is elastic; on the outside it is of an ash colour with an uneven surface, but it's inside is smooth and whitish.

(*a*) Inst. R. Herb. p. 563. (*b*) Linn. Gent. Plant. p. 510. (*c*) Cat. Giss. p. 196. (*d*) Michel. Nova Plant. Gen. p. 221. (*e*) Ibid. (*f*) Linn. Gen. p. 510. H. Cliffort. p. 479. (*g*) Flor. Ledeus. Prod. p. 518.

From

From earth of the tips of the reflexed ſegments of the *volva* ariſes, that part of the plant, which I call the *operculum*. By the gradual extenſion of this part, from it's joining with the *volva* are formed 2 arches, the heighth of which from the ſegments of the *volva* does not exceed 1 ½ inch. The ſubſtance and thickneſs of the *operculum* equal thoſe of cinnamon; it turns up a little at the edge, it is whitiſh within, and of a reddiſh yellow without. We may add, that a ſort of brown membrane, divided into 4 unequal parts, adheres to the top of each arch.

From the centre of the *operculum* juſt deſcribed, on a pedicle ſcarce ½ of an inch in heighth, appears a *pericarpium*, of an oblate ſpheroïdal figure, brown, ½ of an inch broad, reſembling the head of a poppy. At the top is a circular hole, in which are ſome *lamellæ*, adhering to the inſide of the *pericarpium*, through it's whole length, filled with a woolly ſubſtance; and to theſe *lamellæ* a great number of ſeeds, like a very fine duſt, adhere, as to ſo many *placentæ*.

It may ſeem perhaps very difficult to many Botaniſts to comprehend, by what means a proper nouriſhment is ſupplied to the *operculum* and *pericarpium*, whilſt the plant is in a flouriſhing ſtate. They muſt therefore be informed, that in the more tender ſtate of the plant, the *volva* and *operculum* lie ſpread on the ground, not unlike ſtar-fiſhes, and are joined together with a ſort of glutinous ſubſtance, by means of which they are nouriſhed together with the *pericarpium* and ſeed. In this ſituation all the ſpecies of *Geaſter* are ſhewn by the celebrated *Micheli*; as is alſo the *Fungus crepitus lupi dictus coronatus & inferne ſtellatus* firſt mentioned by our great Mr *Ray* (*a*), of which there is a figure (*b*) in the third edition of his *Syn. Stirp. Brit.* But when the ſeed is ripe, the glutinous matter, which lies between the *volva* and the *operculum*, dries up; which makes them rigid and elaſtic; whence they appear divided, except at the tips of the ſegments. Things being thus conſtituted, the *radii* of the *volva*, the *operculum*, and the fruit, gradually ariſe, and the whole plant nearly repreſents an arched tower. This manner of reaſoning, for no one has hitherto had the opportunity of examining it as it grows, ſcarce admits of any doubt, ſince the wrinkled exterior coat of the *volva* at this very time contains not only ſand, but alſo a ſmall ſtone. We learn alſo on the authority of *Micheli* (*c*) that the *Carpobolus*, which, as I ſaid before, is very nearly related to the *Geaſter*, cannot only raiſe it's *operculum* from concave to convex, but even do it violently in an inſtant, ſo that it's ſmall globoſe fruit is thrown up on high.

I have never ſeen more than 2 ſpecimens of this plant, which were communicated to me by Mr *Robert Nicholls*, Apothecary of London. The larger was gathered not far from *Reading* by Dr *Merrick*: the ſmaller was found near *Wickham* in *Kent*; both of them about the end of *March*.

A. the

(*a*) Raii Synopſ. Ed. 2. p. 16. (*b*) Tab. I. (*c*) Nov. Plant. Gen. Tab. 101.

A. The *Pericarpium.* *B.* the *Operculum.* *C.* the *Volva.* Fig. 107.
The *Pericarpium* ſeen in front; of which *A* is the circular *foramen.* Fig. 108.

An account of a new ſpecies of Fungus, *by* John Martyn, *F. R. S. Prof. Bot.* Cantab. No. 475. p. 263. Jan &c. 1745. *Read* Jan. 24. 17$\frac{44}{45}$.

V. In the latter part of the ſummer of 1744, Mr *Ebret* the Painter brought me a *Fungus* of a very extraordinary ſhape and ſize, which had been found growing on a piece of the trunk of an elm, in a damp cellar in the Hay-Market.

The whole plant was about 2 feet in height; and, at firſt ſight, ſeemed not very unlike the horns of ſome deer. being variouſly branched, and covered with a thick down. It was of a ſpongeous ſubſtance, and of a duſky-red colour inclining to black. The tips of the ſmaller branches were of a cream-colour. The larger branches or rather the tops of the whole plant, were expanded in form of a funnel, ſmooth on the concave, and full of pores on the convex ſide. The inner and lower part of the funnel was of the ſame colour with the ſtalk; the reſt of it was of a cream-colour

I have not been able to find, that this plant has been mentioned by any author: and am perſuaded, that it is a new ſpecies; and, perhaps, the remarkable branching of the ſtalks may induce ſome to think it a new *Genus.* As the funnel may be eſteemed a cap, and as this cap is not lamellated, it will be a *Boletus*, according to the method obſerved in the 3 Edit. of *Ray's Synopſis.* According to *Micheli* it ſeems to belong to the *genus* of *polyporus.* The method, which I have long uſed in the diſtribution of this claſs, is expreſſed in the following ſynoptical table, which, I think, comprehends all the ſpecies hitherto known.

F U N G I ſunt,

- lamellati,
  - cauliferi; AMANITA.
  - ſeſſiles; AGARICOIDES.
- poroſi,
  - cauliferi; BOLETUS.
  - ſeſſiles; BOLETOIDES.
- cancellati, aut ſcrobiculis excavati;
  - ex pila erumpentes; PHALLUS.
  - ex pila non erumpentes; MERULIUS.
- echinati; ERINACEUS.
- in pulverem abeuntes; LYCOPERDON.
- ſolidi,
  - cauliferi; CHANTERELLA.
  - ſeſſiles,
    - calyciformes; PEZICA.
    - non calyciformes,
      - in longitudinem producti; DIGITELLUS.
      - horizontaliter prodeuntes; AGARICUS.
      - ſubterranei; TUBER.

According to this method of mine, as well as that of the Editor of *Ray*'s *Synopſis*, the plant in queſtion will be a *boletus*: and, as I do not think it neceſſary to conſtitute a new *genus*, I have taken the liberty to call it

BOLETUS caule ramoſo; *ſummitatibus concavis expanſis*; *ramis* Fig. 109. *minoribus in acutum mucronem deſinentibus.* See Fig. 109.

A deſcription of a curious Sea-plant; *by Sir* Hans Sloane, *Bart. M D. late* Preſ *R. S. and* Col Med. Lond.*&c* N°. 478 p. 51. Jan & Feb 1746. *Read* Feb. 6. 1745-6.

VI. King *Charles* the ſecond had in his cloſet at *Whitehall*, this coralline (as I call it)*; which, I ſuppoſe, had been preſented to him by ſome of his ſea-officers, appointed to cruiſe in the ſoundings, lying off the W. of *England*, towards the *Atlantic* Ocean. I have had it from thence entire, and in perfection, from ſome of the late commanders on that ſtation (of which I here give an entire figure when young) who, by their ſounding lines, brought it up from the rocks at the bottom of the ſea; and which being a very curious coralline, I wonder it has been ſo little taken notice of.

Fig. 110. It riſes to 4 feet high, from a woody baſis, near an inch diameter, giving it a firm foundation on the rocks in the bottom of the ſea, ſpreading out it's branches like a fan, the ſubſtance or inner part of which is woody, of a light brown, or blackiſh colour (as at *a*, *b*,) covered all over with a thin tuberculated cruſt, of an aſh-colour, or ſometimes yellowiſh, ſeldom joined together, as the *rete marinum*, but looſe, and diſtorted; and not ſtrait, as moſt of this kind.

I have had it from *Tangier*, *Antigua*, and *Newfoundland*; from which laſt place, one with the *ſtella arboreſcens* Rondeletii, *p.* 121. (mentioned by Mr *Winthorp*, in theſe *Tranſ.* N°. 57. *p.* 1152.) having it's branches faſtened ſeveral times round thoſe of this coralline; a branch of which
Fig 111. is here figured, with the animal ſticking to it, at *Fig.* 111. in which *a*
Fig 112. is the mouth, and *Fig.* 112. repreſents the back part of it, having a crack in it by ſome accident. The fineſt of this kind was given me by the late Ducheſs of *Beaufort*; who told me, ſhe had it preſented to her by the late Colonel *Codrington*, Proprietor of the iſland of *Barbuda*; from whence in all likelihood he had it.

I do not pretend to give a new name to this coralline, to make confuſion; but only mention ſuch authors, as have already taken notice of it; of whom *John Baubin* is the firſt that deſcribes it plainly, both by words, and an imperfect figure of a ſmall piece or branch, which he had communicated to him by a perſon whom he does not name, by reaſon (as I ſuppoſe) he had by ſtealth broke it off a large branch, kept in what he calls *Theatrum Naturalium Sereniſſimæ Reginæ Angliæ*; whom I ſuppoſe to be Queen *Elizabeth*; and which, to my knowledge, is too much practiſed ſince by unworthy perſons.

* Frutex *marinus* Flabelliformis *cortice verrucoſo obductus.* Doodii *Raii* Hiſt. *Tom.* III. *p.* 7. *&* *Syn Ed.* 3. *p.* 32. Coralloides *granuloſa alba.* *J. B. Tom.* III. *p.* 809. Erica *marina alba fruteſcens.* *Muſ.* Pet. 50. Keratophyton *Flabelliforme, cortice verrucoſo obductum.* Raii *Syn. Ed.* 3. *p.* 32.

It

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Pla. XXI. *Vol.* X. *Part* II. *Pag.* 706.

It is likely, that many of the coralline substances mentioned by authors, may be this, or parts of it, the crust being rubbed off more or less, and it's colour changed, and thereby described for different corallines.

VII.

A catalogue of plants presented to the R. S. by the Comp. of Apothecaries of London, *pursuant to the direction of Sir* Hans Sloane, *Bart. by Mr* Joseph Miller, *Apothecary*, Hort. Chelf Præf. & Prælect. Botan.

N°. 474. p. 189. Ann. 1742. 1001. *Abrotanum Lini folio acriori & odorato* Tourn. *Draco herba* Park. Tarragon.

N°. 472. p. 75. Ann. 1741. 951. *Absinthium Ponticum Galeni* Ger. Roman or Cypress Wormwood.

952. *Absinthium Tanaceti folio odoratissimum* Amm. *p.* 142. The most odorous Wormwood with Tansey leaves.

N°. 474. p. 189. Ann. 1742. 1002. *Abutilon* Americanum *Ribesii folio, flore carneo, fructu pentagono aspero* Houst. American Abutilon with currant leaves, a flesh-coloured flower, and a rough five cornered fruit.

N°. 494. p. 359. Ann. 1748. 1301. *Abutilon Lavateræ flore, fructu cristato Hort. Elt.* Abutilon with a flower like Lavatera, and a crested fruit.

N°. 480. p. 213. Ann. 1744. 1101. *Acacia* Americana, *flore albo, pinnis latiusculis glabris, siliquis latis* Houston. American *Acacia*, with a white flower, broad smooth leaves, and broad pods.

N°. 480. p. 213. Ann. 1744. 1102. *Acetosa arborescens, ex* Insulis Fortunatis *Pluknet*. Shrubby Sorrel with a round leaf, from the *Fortunate Islands*.

N°. 494. p. 359. Ann. 1748. 1302. *Acetosa rotundifolia repens Eboracensis, folio in medio deliquium patiente* *. Creeping round-leaved Sorrel of the North.

N°. 474. p. 189. Ann. 1742. 1003.
N°. 475. p. 403. Ann. 1749. 1351. *Aconitum cæruleum, sive Napellus C. B.* 183. Blue Helmet-flower or Monks-hood.

N°. 476. p. 421. Ann. 1743. 1051. *Aconitum hyemale Ger. Park.* Winter Wolfs-bane.

* Mor. *Hist.* ii. 583.

N°. 484. p. 597. Ann. 1745. 1152. *Aconitum Lycoctonum cæruleum calcari oblongo J. B.* Blue Wolfs-bane with a large ſpur.

N°. 474. p. 189. Ann. 1742. 1004. *Aconitum Lycoctonum luteum C. B.* The yellow poiſonous Wolfs-bane.

N°. 495. p. 403. Ann. 1749. 1352. *Aconitum Pyrenaicum, ampliore folio tenuius laciniato T.* 424. Pyrenean Wolfs-bane, with a larger leaf, divided into finer ſegments.

N°. 484. p. 597. Ann. 1745. 1151. *Acorus verus, ſive Calamus aromaticus.* Off. & *C. B.* The ſweet-ſmelling Flag or Calamus.

No. 491. p. 43. Ann. 1746. 1203. *Acriviola maxima, odorata, flore pleno Boerh.* The great double *Naſturtium*, or Indian Creſs.

N°. 495. p. 403. Ann. 1749. 1351. *Adhatoda minor Canarienſis Pluk.* The ſmaller Canary Adhatoda.

N°. 491. p. 43. Ann. 1746. 1201. *Adiantum Americanum Cornut.* Black Maiden-hair of America.

1202. *Adiantum nigrum Offic.* Common black Maiden-hair, *or* Oak-fern.

N°. 484. p. 597. Ann. 1745. 1153. *Adonis alias Eranthemum, J. B.* Adonis flower, or red Maithes.

1154. *Agrimonia Orientalis, ſpica brevi craſſa, &c. Tourn.* † Dwarf Eaſtern Agrimony, with thick creeping roots, and the fruit growing in ſhort thick ſpikes.

N°. 494. p. 359. Ann. 1748. 1303. *Agrimonoides* Columb. *Ec.* 1. 145. Baſtard Agrimony.

N°. 491. p. 43. Ann. 1746. 1204. *Alaternoides Africana Telephii Imperati folio* Hort. *Amſt.* Baſtard Alaternus of *Africa*, with leaves like the *Telephium Imperati.*

N°. 472. p. 75. Ann. 1741. 953. *Alcea tenuifolia criſpa J. B.* Narrow curled leaved Vervain-Mallow.

† *Agrimonia Orientalis, humilis, radice craſſiſſima repente, fructu in ſpicam brevem & denſam congeſto* Tourn. *Cor.* 21.

N°. 494.

N°. 494. p. 331. Ann. 1747. 1251. *Alchimilla Alpina Quinquefolii folio, subtus argenteo J. R. H.* 218. The Alpine five-leaved Ladies Mantle with the under part of the leaves white.

N°. 491. p. 43. Ann. 1746. 1205. *Alnus nigra Offic.* The black Alder-tree.

N°. 494. p. 331. Ann. 1747. 1252. *Alyssoides incanum, foliis sinuatis* Inst. *R. H.* 218. Hoary *Alyssoides* with sinuated leaves.

1253. *Alysson Creticum, foliis angulatis, flore violaceo* T. *Cor.* 15. Candy Madwort, with angular leaves, and violet coloured flowers.

1254. *Alysson Creticum, Saxatile, foliis undulatis, incanis* T. *Cor.* 15. The *Alysson* of *Candia*, with hoary undulated leaves.

——— p. 359. Ann. 1748. 1304. *Alysson fruticosum incanum Tourn.* hoary, shrub Madwort.

N°. 472. p. 75. Ann. 1741. 955. *Amaranthoides Lychnidis folio capitulis argenteis Tourn.* The white or silver coloured Globe Amaranthus, or Eternal flower.

956. *Idem capitulis purpureis.* The purple Globe-Amaranthus, or Eternal-flower.

954. *Amaranthus maximus* Offic. *purpureus major Park.* The tree Amaranth.

N°. 484. p. 597. Ann. 1745. 1155. *Amaranthus* Siculus *Spicatus* Boccone. *Boccone*'s perennial spiked Flower-gentle of Sicily.

N°. 474. p. 189. Ann. 1742. 1005. *Ammi majus* Off. *C. B.* 159. Common broad-leaved Bishops Weed.

N°. 494. p. 359. Ann. 1748. 1301. *Ananthocyclus Coronopi folio Vaill.* Ananthocyclus with a Buckshorn leaf.

N°. 474. p. 181. Ann. 1742. 1006. *Anchusa purpurea Park.* Purple Alkanet.

N°. 472. p. 75. Ann. 1741. 957. *Anisum* Offic. *herbariis C. B.* Anise.

N°. 474. p. 189. Ann. 1742. 1007. *Anonis non spinosa, viscida, hirsuta, odore Theriacæ Hort. Cathol.* Hairy, viscous Rest-harrow

harrow, without ſpines, and ſmelling like Venice Treacle.

N°. 494. p. 359. Ann. 1748. 1306. *Anonis purpurea, ſpicata, alopecuroides major Boerh.* The greater fox-tail, purple, ſpiked Reſt-harrow.

N°. 495. p. 403. Ann. 1749. 1354. *Apocynum majus Syriacum rectum Cornut.* 91. The greater upright Syrian Dogs-bane.

N°. 491. p. 43. Ann. 1746. 1306. *Aquilegia hortenſis multiplex, flore * pleno C. B.* Double Columbines.

N°. 476. p. 421. Ann. 1743. 1052. *Arbutus folio ſerrato C. B.* Strawberry-tree.

N°. 484. p. 597. Ann. 1745. 1156. *Ariſtolochia, Clematitis recta Off. & C. B.* Creeping Birthwort.

N°. 495. p. 403. Ann. 1749. 1354. *Ariſtolochia, Piſtolochia Cretica.* The ever-green Birthwort from *Crete.*

N°. 474. p. 189. Ann. 1742. 1008. *Arum Africanum flore albo Parad. Bat.* The African *Arum* with white flowers.

N°. 491. p. 43. Ann. 1746. 1207. *Arum maximum, quod* Colocaſia *vulgo caulibus nigricantibus* Hort. *Lugd.* The greateſt Egyptian *Arum* with blackiſh ſtalks.

N°. 476. p. 421. Ann. 1743. 1053. *Arum venis albis, lituris nigris maculatum* Hort. *R. Par.* The white-veined *Arum*, ſpotted with black.

N°. 494. p. 360. Ann. 1748. 1307. *Aſarina caule erecto ramoſo, foliis oblongis acutis ſeſſilibus, floribus erectis.* Upright *Aſarina* with long ſharp-pointed leaves.

p. 331. Ann. 1747. 1255. *Aſarum* Dod. *Pempt.* 358. *Offic.* 54. Aſarabacca.

N°. 472. p. 75. Ann. 1741. 859. *Aſparagus aculeatus ſpinis horridus C. B.* Prickly Sparagus or Sperage.

858. *Aſparagus ſylveſtris tenuiſſimo folio C. B.* Wild Sparagus or Sperage with narrow leaves.

N°. 491. p. 43. Ann. 1746. 1208. *Aſperula odorata, flore albo Offic. & Dodon.* Wood-roof.

* *Magno.*

N°. 484.

N°. 484. p. 597. Ann. 1745. 1157. *Aster* Atticus *cæruleus vulgaris C. B.* Purple *Italian* Starwort.

N°. 491. p. 43. Ann. 1746. 1209. *Asteriscus perennis maritimus patulus Tournef.* The maritime *perennial dwarf yellow Starwort.*

N°. 476. p. 421. Ann. 1743. 1054. *Asteroides* Alpina, *salicis folio Tourn.* Bastard Starwort of the Alps, with a Willow leaf.

N°. 494. p. 360. Ann. 1748. 1308. *Astragalus annuus, angustis floribus, pediculis longis Tourn.* Annual Milk-Vetch, with narrow flowers, and long foot-stalks.

——— p. 331. Ann. 1747. 1256. *Astragalus luteus annuus Monspeliac. procumbens* Mor. *Hist.* Yellow annual trailing Milk-Vetch of *Montpelier.*

N°. 494. p. 360. Ann. 1748. 1310. *Astragalus luteus perennis procumbens vulgaris sive sylvestris* *. Wild-Liquorice *or* Liquorice-Vetch.

N°. 480. p. 213. Ann. 1744. 1103. *Astragalus luteus perennis siliqua gemella rotunda Tourn.* Yellow perennial Milk-Vetch with a round pod.

N°. 494. p. 360. Ann. 1748. 1309. *Astragalus Orientalis altiss. Galegæ foliis* T. Tall Oriental Milk-Vetch with Goats-rue leaves.

N°. 480. p. 213. Ann. 1744. 1104. *Astragalus pumilus siliqua epiglottidis forma Tourn.* Dwarf Milk-Vetch with a pod shaped like the *epiglottis.*

1105. *Atriplicis marinæ species* Valerandi *J. Baub.*

N°. 495. p. 403. Ann. 1740. 1356. *Baccharis Africana, Coronopi folio* Vaill. *Acad.* African Plowman's Spikenard with a Buckshorn leaf.

N°. 484. p. 597. Ann. 1745. 1158. *Balsamina fœmina C. B.* The female Balsam Apple.

N°. 474. p. 189. Ann. 1742. 1009. *Balsamita major* Dod. *Costus Hortorum Off.* The greater Costmar.

N°. 495. p. 403. Ann. 1749. 1357. *Barba Jovis Africana, foliis viridibus pinnatis; flore cæruleo Boerh.* The African Jupiter's Beard

* Mor. *Hist.*

Beard with deep green leaves and blue flowers.

N°. 480. p. 213. Ann. 1744. 1106. *Barba Jovis Hiſpanica incana flore luteo Tourn.* Spaniſh Jupiter's Beard.

N°. 476. p. 421. Ann. 1743. 1055. *Barbarea* J. B. *flore ſimplici Park.* Winter Creſſes.

N°. 484. p. 597. Ann. 1745. 1159. *Bellis radice repente, latioribus ſerratis foliis Moriſon* *. Creeping-rooted Ox-eye Daiſie with broad ſerrated leaves.

N°. 472. p. 75. Ann. 1741. 961. *Bellis ſpinoſa foliis Agerati* C. B. *Santolina ſpinoſa Agerati foliis Tourn.* Prickly-leaved naked yellow Daiſie.

960. *Betonica Alpina incana purpurea* Barellier *Icon.* Hoary Alpine Betony with purple flowers.

N°. 474. p. 189. Ann. 1742. 1010. *Bidens folio tripartito, diviſo Tourn.* Water Hemp-Agrimony, with a yellow flower.

N°. 484. p. 597. Ann. 1745. 1160. *Bidens foliis non diſſectis Tourn.* Water Hemp-Agrimony with an undivided leaf.

No. 480. p. 213. Ann. 1744. 1107. *Biſtorta major, radice minus intorta C. B.* The greater Biſtort or Snake-weed.

N°. 495. p. 403. Ann. 1749. 1358. *Blattaria alba C. B.* 241. The white Moth-Mullein.

N°. 476. p. 421. Ann. 1743. 1056. *Blitum perenne :* Bonus Henricus *J. B. G.* Engliſh Mercury *or* All-good.

N°. 494. p. 331. Ann. 1747. 1257. *Borrago Conſtantinopolitana, flore reflexo cæruleo, calyce veſic.* T. *Cor.* Borrage of Conſtantinople, with a blue reflexed flower, and a ſwelling empalement.

—— p. 360. Ann. 1748. 1312. *Brunella folio laciniato C. B.* 261. Laciniated Self-heal with a ſmall white flower.

1311. *Brunella major, folio non diſſecto* C. B. 260. *Off.* 386. Common Self-heal.

* *Bellis major radice repente foliis latioribus ſerratis D. Moriſ. Præelud. Bot.* Hiſt. Ox. iii. 29.

N°. 494.

N°. 494. p. 360. Ann. 1748. 1313. *Bugloſſum Creticum majus, flore cæruleo purpurante H. R. Par.* Greater Bugloſs of Candy with a blue flower inclining to a purple colour.

——— p. 331. Ann. 1747. 1358. *Bugloſſum Creticum verrucoſum perlatum quibuſdam H. R. Par.* Warted Bugloſs from Crete.

N°. 474. p. 189. Ann. 1742. 1011. *Bugloſſum latifolium ſempervirens C. B.* Ever-green Borrage.

1012. *Bugloſſum Orientale, flore luteo Tourn.* The Eaſtern Bugloſs with yellow flowers.

N°. 472. p. 75. Ann. 1741. 962. *Bugula Orientalis flore inverſo cæruleo Tourn.* * Hairy Eaſtern Bugle, with an inverted blue flower, ſpotted with white.

N°. 491. p. 43. Ann. 1746. 1210. *Buphthalmum Orientale, Tanaceti minoris folio, flore luteo amplo* † *Tourn.*

N°. 495. p. 403. Ann. 1749. 1359. *Bupleurum foliis linearibus acutis ſeſſilibus Fl. Leyd.* Hares-ear, with narrow ſharp-pointed ſeſſile leaves.

N°. 480. p. 213. Ann. 1744. 1108. *Burſa-Paſtoris major, folio ſinuato eleganti, inſtar Coronopi repentis C. B.* Greater Shepherds-purſe, with an elegant ſinuated leaf.

N°. 491. p. 43. Ann. 1746. 1211. *Calendula minor Hiſpanica* Hort. Lugd. *Bat.* Small Spaniſh Marygold.

N°. 480. p. 213. Ann. 1744. 1109. *Calendula* ‖ *polyanthos maxima C. B.* The largeſt double Marygold.

1111. *Calthoides, foliis oblongis, cæſiis, craſſis* Shaw *Specimen.* Calthoides with thick, bluiſh-green, oblong leaves.

N°. 474. p. 189. Ann. 1742. 1013. *Campanula hortenſis, folio & flore oblongo C. B.* 94. Coventry Bells.

* *Bugula Orientalis, villoſa, flore inverſo cæruleo, alba macula notato* Tourn. *Cor.* 14.
† *Ampliſſimo.* ‖ Caltha.

N°. 494. p. 331. Ann. 1747. 1259.	*Campanula minor annua foliis incisis* Mor. *Hist.* 2. 458. Small annual cut-leaved Bell-flower, *or* Venus's Looking-glass.
——— p. 360. Ann. 1748. 1314.	*Campanula minor, foliis incisis H. L. B.* Small cut-leaved Bell-flower.
N°. 474. p. 189. Ann. 1742. 1014.	*Campanula Persicæ folio, flore albo pleno Lob.* The double Peach-leaved Bell-flower.
N°. 476. p. 421. Ann. 1743. 1057.	*Canella alba* Off. *Cort.* Winteranus *vulgo* Winter's Bark.
1058.	*Cardiaca flore canescente Amman.* Mother-wort with a hoary flower.
N°. 474. p. 190. Ann. 1742. 1015.	*Carduus acaulis minor, flore purpureo C. B.* 380. * Dwarf Carline Thistle.
1016.	*Carduus aculeatus, Ptarmicae Austriacae folio Triumfetti.* Prickly Thistle with leaves like the Austrian Sneezewort.
N°. 484. p. 507. Ann. 1745. 1161.	*Carthamus Off. flore croceo Tourn.* Bastard Saffron *or Saf*-flower.
N°. 472. p. 75. Ann. 1741. 964.	*Carthamus Africanus folio Ilicis, flore aureo Boerh.* Shrubby African Bastard Saffron with an ever-green Oak leaf, and a golden flower.
N°. 474. p. 190. Ann. 1742. 1018.	*Carum Off.* Carawaies.
N°. 494. p. 331. Ann. 1747. 1260.	*Caryophyllata montana, flore luteo, nutante H. R. Par.* Mountain Avens, with yellow nodding flowers.
N°. 480. p. 213. Ann. 1744. 1112.	*Caryophyllus barbatus sylvestris C. B.* Deptford Pink.
N°. 484. p. 597. Ann. 1745. 1163.	*Caryophyllus Sinensis flore vario* Rand. *Hort. Chels.* Chinese Pink with a variable flower.
1162.	*Catanance flore luteo latifolia* † *Tourn.* Broad-leaved Candy Lion's-foot with a yellow flower.

* There is no such name in Bauhin's *Pinax*: I suppose the *Carlina acaulos minore purpureo flore* of that author is the plant intended.
† *latiore folio.*

No. 494. p. 360. Ann. 1748. 1315. *Cataria minor Alpina* T. *Inst.* 202. The smaller mountain Catmint.

——p. 332. Ann. 1747. 1261. *Cataria, quæ Nepeta minor foliis Melissæ Turcicæ* Hort. *Cath.* Lesser Catmint with leaves like Turkey Baum.

N°. 495. p. 403. Ann. 1749. 1360. *Centaurea calicibus setaceo-spinosis, foliis lanceolatis petiolatis dentatis* Hort. *Cliff.* Great Centory with spear-pointed leaves.

N°. 480. p. 213. Ann. 1744. 1113. *Centaurium majus Alpinum luteum C. B.* Greater yellow Centory of the Alps.

1114. *Centaurium minus luteum perfoliatum J. B.* Yellow Centory.

1115. *Cerinthe major, flore versicolore C. B.* * Great Mountain Honey-wort.

N°. 494. p. 360. Ann. 1748. 1316. *Cerinthe minor, flavo flore C. B.* 258. Yellow flowered Honey-wort.

N°. 484. p. 597. Ann. 1745. 1164. *Chamæcerasus Alpina fructu gemino rubro C. B.* Mountain upright Honey-suckle with red berries.

N°. 491. p. 43. Ann. 1746. 1212. *Chamæpitys lutea vulgaris, folio trifido C. B. & Offic.* Common Ground-pine.

N°. 494. p. 360. Ann. 1748. 1317. *Chenopodio-morus minor* Boer. *Ind.* 91: Smaller Bloody Spinach.

N°. 494. p. 332. Ann. 1747. 1262. *Chondrilla Sonchi foliis* †, *flore purpurascente major Tourn.* 475. Blue flowered Mountain Lettuce of the woods.

N°. 480. p. 213. Ann. 1744. 1110. *Christophoriana vulgaris Park.* Herb *Christopher* or Bane-berries.

N°. 484. p. 597. Ann. 1745. 1166. *Chrysanthemum Bermudense, Leucoii folio crasso Pluk.* Corn Marygold of Bermudas.

* *Cerinthe quorundam major versicolore flore* J. B. † *folio.*

N°. 480. p. 274. Ann. 1744. 1116. *Chrysanthemum Creticum Ger. Park.* Candy Corn Marygold

1117. *Chrysanthemum flore pleno Tourn.* Corn Marygold with double flowers.

1118. *Chrysanthemum mixtum, flore pleno* Hort. *Eyst.* Mixt Corn Marygold with a double flower.

N°. 484. p. 598. Ann. 1745. 1166. *Cirsium Anglicum Ger.* The English soft or gentle Thistle.

1167. *Cistus fœmina Salviæ folio C. B.* Female Rock-rose with Sage leaves.

N°. 494. p. 360. Ann. 1748. 1318. *Cistus ladanifera Hispanica, flore albo macula punicante insignito.** Spanish gum-bearing *Cistus*, or Rock-rose, with Willow leaves, and white flowers spotted with purple.

N°. 484. p. 598. Ann. 1745. 1169. *Cistus ladanifera Hispanica Salicis folio C. B.* † Spanish gum-bearing *Cistus* or Rock-rose, with Willow leaves and white flowers.

1168. *Cistus mas Lusitanicus folio amplissimo incano Tourn.* Male Portugal Rock-rose, with an ample hoary leaf.

N°. 472. p. 75. Ann. 1741. 963. *Citrullus sive Anguria vulgo Park.* Citruls.

N°. 491. p. 43. Ann. 1746. 1214. *Clinopodium Americanum rotundifolium, Pulegii odore Houst.* Round-leaved American Field-Basil, with a smell of Penny-royal.

1313. *Clinopodium minus Ocimi facie C. B.* § Wild Basil.

N°. 494. p. 332. Ann. 1747. 1263. *Clutia foliis petiolatis* Linn. *Hort. Cliff.* Clutia with foot-stalks to the leaves.

* *Cistus Ladanifera, Hispanica, Salicis folio, flore albo, macula punicante insignito Tourn.*

† *Cistus Ladanifera Hispanica incana* C. B.

§ *Clinopodium arvense Ocimi facie* C. B.

N°. 474. p. 190. Ann. 1742. 1017. *Cnicus Atractylis lutea dictus Off. Hort. Lugd. Bat.* The Diftaff Thiftle.

N°. 480. p. 214. Ann. 1744. 1119. *Colutea Æthiopica flore phœniceo, Barbæ Jovis folio Breyn.* Æthiopian Bladder-Senna with fcarlet flowers and leaves like the Silver Bufh.

N°. 480. p. 214. Ann. 1744. 1120. *Coma aurea Africana fruticans, foliis Linariæ anguftis* Hort. Amft. African fhrubby Goldilocks.

N°. 495. p. 403. Ann. 1749. 1361. *Commelina foliis ovato-lanceolatis; petalis tribus majoribus æqualibus Linn.* Commelina with oval lanceolated leaves, and the three greater petals equal.

N°. 494. p. 360. Ann. 1748. 1319. *Commelina radice Anacampferotis* Hort. Elt. *Tab.* 79. Commelina with a root like Orpine.

N°. 495. p. 403. Ann. 1749. 1362. *Convolvulus argenteus elegantiffimus; foliis tenuiter incifis* Inft. *R. H.* The moft elegant Silver Bindweed with fine cut leaves.

N°. 491. p. 44. Ann. 1746. 1215. *Convolvulus cæruleus major, folio fubrotundo Ger. Park.* The greater Bindweed with roundifh leaves.

N°. 476. p. 421. Ann. 1743. 1059. *Convolvulus cæruleus minor Hifpanicus Park.* Spanifh, blue, leffer Bindweed, with an oblong leaf.

N°. 495. p. 404. Ann. 1749. 1363. *Convolvulus peregrinus pulcher folio Betonicæ J. B.* Bindweed with Marfh-mallow leaves.

1364. *Convolvulus Siculus minor; flore parvo auriculato* Bocc. *Rar.* Small blue-flowered Sicilian Bindweed.

N°. 472. p. 76. Ann. 1741. 965. *Conyza humilior, Linariæ folio, floribus luteis umbellatis Amm.* 141. Low Flea-bane with Toad-flax leaves, and yellow umbellated flowers.

N°. 491.

N°. 491. p. 44. Ann. 1746. 1216. *Conyza minima Ger. emac.* Small Fleabane.

N°. 494. p. 360. Ann. 1748. 1310. *Cornus foliis lanceolatis acutis subtus incanis, umbellis minoribus.* Cornel with ſharp ſpear-pointed leaves, hoary underneath, and ſmaller umbells.

N°. 491. p. 44. Ann. 1746. 1217. *Corona Solis annua, flore pleno Tourn.* The double annual Sun-flower.

1218. *Coronilla herbacea, flore vario Tourn.* Herbaceous jointed-podded *Colutea*, with a variable flower.

N°. 494. p. 360. Ann. 1748. 1311. *Coronilla leguminibus teretibus articulatis erectis Fl. Leyd.* 387. * *Candia* herbaceous jointed-podded *Colutea* with a ſmall purpliſh flower.

N°. 484. p. 598. Ann. 1745. 1170. *Cotyledon Africana fruteſcens flore coccineo umbellato, Commelin.* Shrubby African Navel-wort, with ſcarlet umbellated flowers.

N°. 480. p. 214. Ann. 1744. 1121. *Cuminoides Tourn.* Baſtard *or* Wild Cummin.

1122. *Cyanus minor, flore purpureo C. B.* Small Blue-bottle with a purple flower.

N°. 472. p. 76. Ann. 1741. 966. *Cyanus Turcicus, ſeu orientalis odoratus major Park.* The Sultan flower.

N°. 494. p. 360. Ann. 1748. 1322. *Cynogloſſum Creticum argenteo folio C. B.* † *Candia* Houndſtongue with narrow ſilver coloured leaves.

N°. 476. p. 421. Ann. 1743. 1060. *Cyperus odoratus radice longa C. B.* The ordinary ſweet *Cyperus*, *or* Engliſh Galingale.

1061. *Cyperus rotundus eſculentus anguſtifolius Ibid.* Sweet *Cyperus*, *or* Ruſhnut.

N°. 484. p. 598. Ann. 1745. 1171. *Cytiſus* Alpinus *flore luteo racemoſo pendulo.* Bean-trefoil, with a yellow flower, hanging in bunches.

* *Coronilla legum teret. artic. erect.* caule herbaceo *Fl. Leyd.*

† *Cynogloſſum Creticum argenteo* anguſto *folio C. B.*

N°. 476. p. 421. Ann. 1743. 1063.	*Cytisus glaber foliis subrotundis pediculis brevissimis C. B.* Round-leaved smooth Base Shrub-Trefoil with short foot-stalks.
1062.	*Cytisus incanus, siliquis falcatis Ibid.* Shrubby three leaved hoary Moon-trefoil.
N°. 472. p. 76. Ann. 1741. 967.	*Dictamnus montis Sipyli. Sir Geo. Wheeler.* Dittany of Mount Sipylus.
N°. 480. p. 214. Ann. 1744. 1123.	*Digitalis lutea magno flore C. B.* Fox-glove with a large yellow flower.
N°. 495. p. 404. Ann. 1749. 1365.	*Digitalis lutea major; parvo flore* Mor. *Hist.* Great yellow Fox-glove with a small flower.
N°. 474. p. 190. Ann. 1742. 1019.	*Doronicum Americanum Park.* American Leopard's-bane.
N°. 480. p. 214. Ann. 1744. 1124.	*Dorycnium Monspeliensium* Lob. *Icon.* Shrub-trefoil of Montpelier.
N°. 476. p. 421. Ann. 1743. 1064.	*Draba siliquis donata C. B.* *
N°. 495. p. 404. Ann. 1749. 1366.	*Dracocephalon Canariense triphyllon; Cedronella* H. *Amstel.* Trifoliated Dragon's-head of the Canary Islands.
1367.	*Echium Creticum angustifolium rubrum C. B.* Narrow-leaved *Candia* Viper's Buglofs, with a red flower.
N°. 472. p. 76. Ann. 1741. 968.	*Elaterium* Tourn. *Cucumis asininus Ger.* Wild Cucumber.
N°. 474. p. 190. Ann. 1742. 1021.	*Elichrysum latifolium Americanum Tourn.* Broad-leaved American Eternal-flower.
1020.	*Elichrysum, seu Stæchas citrina angustifolia C. B.* Goldilocks or Cassidony.
N°. 484. p. 598. Ann. 1745. 1174.	*Elichrysum Spicatum Tourn.* Spiked Eternal-flower.
N°. 495. p. 404. Ann. 1749. 1368.	*Emerus Americanus, siliqua incurva* Inst. *R. H.* † American Scorpion-Senna with a crooked pod.

* I do not find this name in the author here quoted.

† This name is not in Tournefort's *Institutiones R. H.*

N°. 484.

N°. 484. p. 598. Ann. 1748. 1172. *Emerus* Tourn. * *Colutea Scorpioides Park.* The lesser Scorpion Senna.

1173. *Ephedra major maritima Tourn.* Greater Sea Horse-tail.

N°. 494. p. 332. Ann. 1747. 1266. *Erysimum Orientale, foliis Sonchi, flore sulphureo, siliquis longissimis Boerh.* Oriental Hedge-mustard, with Sow-thistle leaves, a pale yellow flower, and very long pods.

N°. 474. p. 190. Ann. 1742. 1022. *Erysimum polyceratium vel corniculatum C. B.* 101. Hedge-mustard with many crooked pods.

N°. 472. p. 76. Ann. 1741. 969. *Euonymo adfinis Æthiopica fructu globoso, Salicis folio Plukn.* An Ethiopic plant approaching to the Spindle-tree, with a globose fruit, and Willow leaves.

N°. 494. p. 332. Ann. 1747. 1165. *Euonymus latifolius C. B.* 428. Broad-leaved Spindle-tree.

1164. *Euonymus vulgaris, granis rubentibus C. B.* 428. Spindle-tree or Prickwood.

N°. 491. p. 44. Ann. 1746. 1219. *Euphrasia pratensis rubra C. B.* Red meadow Eye-bright, *or* Eye-bright Cow-wheat.

N°. 474 p. 190. Ann. 1742. 1023. *Fabago* Belgarum, *sive Peplus* Parisiensium *Lugdunens.* Bean-Caper.

N°. 476. p. 421. Ann. 1743. 1665. *Fagopyrum vulgare erectum Tourn.* Common upright Buck-wheat.

N°. 480. p. 214. Ann. 1744. 1125. *Filipendula omni parte major Boerhaave* †. Larger Dropwort with a narrower leaf.

N°. 474. p. 190. Ann. 1742. 1324. *Filix mas aculeata, pinnulis auriculatis angustioribus Raii* ||. Prickly Male-Fern with narrower leaves.

N°. 484. p. 598. Ann. 1745. 1176. *Fritillaria alba præcox C. B.* The early white Fritillary.

* *Emerus minor* Tourn.

† *Filipendula omni parte major, folio angustiori* Boer.

|| *Filix aculeata major, pinnulis auriculatis crebrioribus foliis integris angustioribus* Raii Syn. Ed. 3. 121.

N°. 494. p. 332. Ann. 1747. 1267. *Fritillaria lutea, foliis Polygonati, fructu breviore* Boerh. 2. 139. Yellow Fritillary, with Solomon's-seal leaves, and a shorter fruit.

N°. 484. p. 598. Ann. 1745. 1175. *Fritillaria præcox purpurea variegata C. B.* Common Fritillary or chequer'd Lily.

N°. 491. p. 44. Ann. 1746. 1220. *Galega Africana, floribus majoribus, & siliquis crassioribus Tourn.* African Goat's-rue with large flowers and thick pods.

N°. 484. p. 598. Ann. 1745. 1177. *Galega vulgaris floribus cæruleis C. B.* Common Goat's-rue with blue flowers.

N°. 495. p. 404. Ann. 1749. 1369. *Gallium saxatile minimum supinum & pumilum* Inst. *R. H.* 115. The smallest supine dwarf Ladies Bedstraw.

N°. 474. p. 190. Ann. 1742. 1025. *Genista juncea* J. B. *Hispanica Ger.* The Yellow Spanish Broom.

1026. *Gentiana Alpina magno flore J. B.* Large flowered Gentian of the Alps, commonly called *Gentianella.*

N°. 484. p. 598. Ann. 1745. 1177. *Gentiana* Offic. *major lutea C. B.* The most common great Gentian *or* Felwort.

N°. 491. p. 44. Ann. 1746. 1221. *Geranium Africanum Malvæ folio, petalis florum inferioribus vix conspicuis* Index. Hort. *Chelf.* African Mallow-leaved Cranes-bill, with the lower petals scarce discernible.

N°. 494. p. 332. Ann. 1747. 1268. *Geranium latifolium longissima acu C. B.* 319. Candy Cranes-bill.

N°. 480. p. 214. Ann. 1744. 1126. *Geum folio* * *rotundo majori, pistillo floris rubro Tourn.* London Pride *or* None-so-pretty.

1127. *Geum folio subrotundo minori* †. Sanicle with a lesser roundish leaf, and a red pointal.

* *Subrotundo.* † *Geum fol. subrot. min.* pistillo floris *rubro* Tourn.

N°. 474. p. 190. Ann. 1742. 1027. *Glaucium flore luteo Tourn.* Yellow horned Poppy.

N°. 476. p. 422. Ann. 1743. 1066. *Glaucium flore purpureo Tourn.* Purple horned Poppy.

N°. 495. p. 404. Ann. 1749. 1370. *Glaucium hirſutum flore phœniceo Tourn.* Hairy horned Poppy with a deep ſcarlet flower.

1371. *Glaucium Orientale flore magno aureo* * T. *Cor.* Eaſtern Horned Poppy with a large red flower.

N°. 484. p. 598. Ann. 1745. 1179. *Glychirrhiza ſylveſtris flore luteo pallido C. B.* † Wild Liquorice *or* Liquorice-Vetch.

N°. 474. p. 190. Ann. 1742. 1028. *Gramen Dactylon eſculentum C. B. Manna vulg.* H. L. *Bat.* The Manna Graſs.

N°. 472. p. 76. Ann. 1741. 970. *Guaicana Virginiana* Piſhamin *dicta Park.* The Piſhamin Plum.

N°. 491. p. 44. Ann. 1746. 1222. *Guidonia Ulmi folio flore roſeo Plum.* Guidonia with Elm-leaves and a roſe-coloured flower.

N°. 476. p. 422. Ann. 1743. 1067. *Hedyſarum annuum ſiliquis aſperis pendulis intortis Tourn.* ‖ Annual French Honeyſuckle with a rough waved and wreathed pod.

1068. *Helenium* Off. *Enula campana Park.* Elecampane.

N°. 494. p. 360. Ann. 1748. 1323. *Helianthemum foliis Sampſuchi, capitulis valde hirſutis J. B.* Dwarf Ciſtus with a Marjoram leaf and very hairy heads.

——— p. 332. Ann. 1743. 1269. *Helianthemum Salicis folio T.* 249. Dwarf Ciſtus with a Willow leaf.

N°. 491. p. 44. Ann. 1746. 1223. *Helianthemum vulgare, flore luteo J. B.* Dwarf Ciſtus *or* little Sun-flower.

N°. 495. p. 404. Ann. 1749. 1372. *Helleborus Fumariæ foliis* Amman. *Ruth.* 74. Hellebore with Fumitory leaves.

* *rubro.* † *Glychirrhiza Sylv.* floribus luteo-palleſcentibus *C. B.*
‖ *Hedyſarum annuum, ſiliqua aſpera, undulata, intorta* Tourn.

N°. 476.

N°. 476. p 422. Ann. 1743. 1069. *Helleborus niger, flore roseo C. B. Off.* True black Hellebore *or* Chriſtmas flower.

N°. 474. p. 190. Ann. 1742. 1029. *Helleborus niger fœtidus C. B.* 126. Stinking black Hellebore, Bears-foot *or* Setterwort.

N°. 484. p. 598. Ann. 1745. 1180. *Helleborus niger, folio Ranunculi flore globoſo Tourn.* The Globe-flower *or* Locker-gowlons.

N°. 474. p. 190. Ann. 1742. 1030. *Hermannia, folio Lavendulæ obtuſo, flore parvo aureo Boerhaave.* * Shrubby Hermannia with a broad blunt Lavender leaf, and a ſmall golden flower.

N°. 494. p. 332. Ann. 1747. 1270. *Hermannia fruteſcens, folio oblongo ſerrato T.* 656. Shrubby Hermannia with an oblong ſerrated leaf.

1274. *Heſperis caule ramoſiſſimo; foliis lanceolatis ſæpius dentatis* Linn. *Hort. Cliff.* Dames-violet with a very branched ſtalk, and ſpear pointed, thickly indented leaves.

1272. *Heſperis exigua lutea; folio dentato anguſto Boerh.* 2. 20. Small yellow Dames-violet with a narrow indented leaf.

1275. *Heſperis flore albo minimo; ſiliqua longa; flore † profunde dentato* Boerh. *Ind.* Dames-violet with a very ſmall white flower, a long pod, and a deeply indented leaf.

1273. *Heſperis maritima, anguſtifolia, incana T.* 223. Dwarf annual ſtock.

1271. *Heſperis ſylveſtris inodora C. B.* 202. Unſavoury wild Dames-violet.

N°. 494. p. 360. Ann. 1748. 1324. *Hieracium amygdalas amaras olens flore ſuaverubente C. B.* 127. ||

* *Hermannia fruteſcens; folio Lavendulæ latiori & obtuſo; flore parvo aureo H.R.D.* Boer. Ind. I. 273. † *folio.*

|| I do not find this name in *C. B.* perhaps it ſhould be *Hieracium amygdalas amaras olens, ſeu odore apuli ſuaverubentis* H. R. Par.

N°. 495. p. 404. Ann. 1749. 1373. *Hieracium calyce barbato* Col. *Ec.* 2. 27. Hawkweed with a bearded empalement.

N°. 474. p. 190. Ann. 1742. 1031. *Hieracium montanum tomentoſum* Moriſon. *Hort. Reg. Bleſ.* Downy Mountain Hawkweed.

N°. 491. p. 44. Ann. 1746. 1224. *Horminum coma purpuro-violacea J. B.* Clary with a purple violet top.

N°. 476. p. 422. Ann. 1743. 1070. *Horminum luteum, glutinoſum C. B.* Yellow Clary *or* Jupiter's Diſtaff.

N°. 474. p. 190. Ann. 1742. 1032. *Horminum pratenſe, flore minimo* Schol. *Botan.* Meadow Clary with a very ſmall flower.

1033. *Horminum ſylveſtre, Lavendulæ flore C. B.* 219. Common Engliſh wild Clary.

N°. 476. p. 422. Ann. 1743. 1021. *Hydrophyllum* Morini *Tourn.* * Water-leaf of *Morinus.*

N°. 494. p. 360. Ann. 1748. 1325. *Hypecoum latiore folio T.* 230. Broad-leaved *Hypecoon.*

N°. 480. p. 214. Ann. 1744. 1128. *Hypericum orientale, Roriſmarini folio Amman.* Oriental Saint John's-wort with a Roſemary leaf.

N°. 494. p. 360. Ann. 1748. 1326. *Jacea cinerea laciniata, flore purpureo Triumfet.* Jagged aſh-coloured Knapweed with a purple flower.

N°. 472. p. 76. Ann. 1741. 971. *Jacea non ramoſa tuberoſa radice latifolia* Baniſter. *Pluk.* Broad-leaved unbranched Knapweed with a tuberous root.

972. *Eadem anguſtifolia &c.* The ſame with a narrow leaf.

N°. 491. p. 44. Ann. 1746. 1225. *Jacea ſpinoſa alata cinerea caule ſpinis luteis longiſſimis.* Aſh-coloured winged Knapweed with very long yellow prickles.

N°. 494. p. 360. Ann. 1748. 1327. *Jacea ſpinoſa Cretica Zanon.* Prickly Knapweed of Candy.

* *Hydrophyllon Morini* Hort. Joncq.

N°. 472. p. 76. Ann. 1741. 973. *Jacobæa Ætnica Chenopodii folio* Hort. *Cathol.* Ragwort of Ætna, with a Goose-foot leaf.

N°. 495. p. 404. Ann. 1749. 1374. *Jasminum Africanum; ilicis folio; flore albo* Com. *Rar.* African Jasmine with a Holm-oak leaf, and a white flower.

N°. 476. p. 422. Ann. 1743. 1072. *Jasminum luteum Indicum odoratissimum Ferrar.* Yellow Indian Jasmine.

N°. 484. p. 598. Ann. 1745. 1181. *Jasminum sive* Sumbach *Arabum* Alpini *J. B.* Single Arabian Jasmine.

N°. 474. p. 190. Ann. 1742. 1034. *Ilex oblongo serrato folio C. B.* 234. Narrow-leaved Ever-green Oak with serrated leaves.

N°. 480. p. 214. Ann. 1744. 1129. *Imperatoria major C. B.* Common Master-wort.

N°. 474. p. 190. Ann. 1742. 1035. *Ketmia Syrorum flore albo* Boerhaave. *Althæa frutex* with white flowers.

N°. 484. p. 598. Ann. 1745. 1182. *Lamium rubrum minus foliis profunde incisis* Raii *Syn.* Lesser red Dead-Nettle with leaves deeply cut.

N°. 494. p. 361. Ann. 1748. 1328. *Lathyrus tuberosus arvensis repens C. B.* Pease-Earthnut.

N°. 491. p. 44. Ann. 1746. 1226. *Lavatera folio & facie Althææ* Act. *Reg. Sc.* Lavatera with the leaf and face of Marsh-mallow.

1227. *Lavatera flore albo.* Lavatera with a white flower

N°. 476. p. 422. Ann. 1743. 1073. *Laurus Alexandrina Off.* Alexandrian Laurel.

N°. 474. p. 191. Ann. 1742. 1036. *Lentiscus vulgaris C. B.* 399. Common Mastich-tree.

N°. 472. p. 76. Ann. 1741. 974. *Leonurus Africanus, Sideritidis folio, floribus phœniceis Boer.* * Perennial African Lion's-tail with an Ironwort leaf, and a large scarlet flower.

N°. 484. p. 598. Ann. 1745. 1183. *Lepidium latifolium* Off. & *C. B.* Dittander *or* Pepper-wort.

* *Leonurus perennis Africanus, Sideritidis folio; flore phœniceo majore* Breyn. Prod.

N°. 494.

N°. 494. p. 332. Ann. 1747. 1278. *Leucoium Hesperidis folio T.* 221. Great Tower-Mustard.

——— p. 361. Ann. 1748. 1329. *Lilium convallium, flore pleno variegato* Didac *T.* 77. Broad-leaved Lily of the valley, with a double striped flower.

N°. 472. p. 76. Ann. 1741. 976. *Limonium lignosum Bellidis folio C. B.* *

975. *Limonium maritimum majus C. B.* Sea Lavender.

N°. 476. p. 422. Ann. 1743. 1074. *Linaria latifolia Dalmatica C. B.* † Broad-leaved Dalmatian Toad-flax, with a large flower.

N°. 495. p. 404. Ann. 1749. 1375. *Linaria pumila foliis carnosis, flosculis minimis flavis C. B.* 213. Dwarf Toad-flax with fleshy leaves and very small yellow flowers.

N°. 494. p. 361. Ann. 1748. 1330. *Linaria triphylla minor lutea, floris vexillo & calcari purpureo Boer.* Small three-leaved yellow Toad-flax, with the standard and heel of a purple colour.

N°. 474. p. 191. Ann. 1742. 1037. *Lingua cervina multifida C. B.* 354. Harts tongue with divided leaves.

N°. 491. p. 44. Ann. 1746. 1228. *Lotus angustifolia flore luteo-purpureo ex insula Sancti Jacobi* Hort. *Amst.* Narrow-leaved Birds-foot Trefoil from the island of St James, with a purple yellow flower.

N°. 474. p. 191. Ann. 1742. 1038. *Lotus hæmorrhoidalis major Park.* Upright hoary Birds-foot Trefoil.

N°. 484. p. 598. Ann. 1745. 1184. *Lotus rubra siliqua angulosa C. B.* Square-codded Vetch.

N°. 480. p. 214. Ann. 1744. 1130. *Lotus siliquis Ornithopodii J. B.* Birds-foot-Trefoil with pods like a Birds-foot.

N°. 472. p. 76. Ann. 1741. 977. *Lupinaster floribus purpureis siliquis minoribus Amm. p.* 147. Lupinaster with purple flowers and smaller pods.

* This name is not in *C. B.*
† This is Parkinson's name: *C. B.* adds *magno flore.*

N°. 476.

N°. 476. p. 422. Ann. 1743. 1075. *Lupinus angustifoliis sylvestris cæruleus elatior* Hort. *Eyst.* Narrow-leaved tall blue wild Lupine.

N°. 474. p. 191. Ann. 1742. 1041. *Lupinus peregrinus major villosus cæruleus C. B.** Great blue Lupine.

1039. *Lupinus sativus flore albo Ibid.* White Lupines.

1040 *Lupinus sylvestris flore luteo Ibid.* The yellow Lupine.

1042. *Lychnis Chalcedonica flore miniato Park.* Nonesuch *or* flower of Constantinople.

N°. 476. p. 422. Ann. 1743. 1076. *Lychnis coronaria vulgaris J. B.* Garden Campions *or* Rose Campion.

N°. 494. p. 332. Ann. 1747. 1276. *Lychnis Cretica parvo flore; calyce striato purpurascente* T. *Cor.* 24. Lychnis of Candy, with a small flower, and a purplish striated empalement.

N°. 480. p. 214. Ann. 1744. 1131. *Lychnis hirsuta, flore eleganter variegato* Raii *Hist.* Hairy Campion with a flower beautifully variegated.

N°. 494. p. 332. Ann. 1747. 1277. *Lychnis supina Sicula, calyce amplissimo striato T.* 337. Low Sicilian Campion, with a large striated empalement.

N°. 491. p. 44. Ann. 1746. 1229. *Lychnis sylvestris flore albo minimo* Raii Hist. Small Corn Campion with a very small white flower.

1230. *Lychnis sylv. quæ Saponaria Tourn.* Common Sopewort.

N°. 476. p. 422. Ann. 1743. 1077. *Lychnis viscosa rubra angustifolia C. B.* Red German Catchfly.

N°. 484. p. 59‖. Ann. 1745. 1185. *Lysimachia lutea major quæ* Dioscoridis *Off.* & *C. B.* Yellow Willow-herb *or* Loose-strife.

N°. 494. p. 361. Ann. 1748. 1331. *Malva Alexandrina Alchimillæ folio Sherard.* Alexandrian Mallow with a Ladies-Mantle leaf.

* *Lupinus peregrinus major vel villosus cæruleus major C. B.*

N°. 480. p. 214. Ann. 1744. 1132. *Malva sylv. minor, folio rotundo C. B.* Small wild Mallow or Dwarf Mallow.

N°. 494. p. 361. Ann. 1748. 1332. *Malva-viscus arborescens, flore miniato clauso* Hort. *Elth.* Tree Malva-viscus, with a shut scarlet flower.

1333. *Marrubiastrum limbo atro-purpureo &c.* * Bastard Horehound with a yellow flower, edged with a dark purple.

N°. 472. p. 76. Ann. 1741. 978. *Marum vulgare* Park. *vulgo Masticben redolens C. B.* Herb-Mastick or Mastick Thyme.

N°. 478. p. 422. Ann. 1743. 1078. *Matricaria foliis florum fistulosis Hort. R. Par.* Feverfew with fistulous flowers.

N°. 484. p. 578. Ann. 1745. 1186. *Medica orbiculata J. B.* Orbiculated Snail Trefoil.

N°. 495. p. 404. Ann. 1749. 1376. *Melilotus Italica folliculis rotundis C. B. P.* 331. Italian Melilot.

N°. 474. p. 191. Ann. 1742. 1043. *Melilotus major candida Tragi.* White flowered Melilot.

1044. *Melilotus odorata violacea* Hist. Oxon. *Lotus urbana Off.* Sweet Trefoil.

N°. 484. p. 598. Ann. 1745. 1187. *Melilotus Offic.* &. *C. B.* Common Melilot.

N°. 476. p. 422. Ann. 1743. 1079. *Melissa Moldavia flore albo Park.* Turkey Baulm with a white flower.

N°. 495. p. 404. Ann. 1749. 1377. *Melissa Romana molliter hirsuta & graveolens.* H. R. *Par.* Stinking Roman Baulm, with softer hairy leaves.

N°. 472. p. 76. Ann. 1741. 979. *Mentha verticillata Ocimi odore, venis luteis* Ind. Hort. Chelf. Whirled Mint, with a Basil smell, and yellow veins.

N°. 491. p. 44. Ann. 1746. 1231. *Mespilus aculeata, Pyri denticulato folio, splendens Virginiana Pluk.* The Virginian Azarol with red fruit.

* *Marrubiastrum Sideritidis folio, caliculis aculeatis, flore flavo cum limbo atropurpureo, coma flavescente T. Cor.* 12.

N°. 494.

N°. 494. p. 332. Ann. 1747. 1279.	*Mespilus Canadensis, Sorbi torminalis facie T.* 642. Medlar of Canada, with the appearance of the common Service-tree.
——— p. 332. Ann. 1748. 1334.	*Mespilus Virginiana, Apii folio, vulgari similis, major.* Medlar of Virginia, resembling the common Haw-thorn, but larger.
N°. 472. p. 76. Ann. 1741. 980.	*Meum foliis Anethi C. B. — vulgatius Park.* Common Spignel or Meu.
N°. 495. p. 404. Ann. 1749. 1378.	*Milleria annua erecta, foliis conjugatis, floribus luteis spicatis Houst.* Greater upright annual Milleria with opposite leaves, and yellow spiked flowers.
N°. 472. p. 76. Ann. 1741. 921.	*Mimosa folio lato Sennæ spinosa Boerh.* The prickly sensitive plant, with a broad Senna leaf.
N°. 484. p. 598. Ann. 1745. 1188.	*Mirabilis Peruana flore variegato* Park. *Parad.* Marvel of Peru with a variegated flower.
N°. 474. p. 191. Ann. 1742. 1045.	*Moldavica Americana trifolia, odore gravi Tourn.* Trifoliated American Moldavica with a strong smell.
N°. 480. p. 214. Ann. 1744. 1133.	*Moldavica orientalis, Salicis folio, flore parvo cæruleo Tourn.* Oriental Moldavica with a Willow leaf, and a small blue flower.
N°. 494. p. 361. Ann. 1748. 1335.	*Mollugo foliis verticillatis, cuneiformibus acutis* Hort. *Upsal.* Bastard-Madder with sharp wedge shaped whorled leaves.
N°. 495. p. 404. Ann. 1749. 1379.	*Monarda floribus capitatis, caule obtuso* Hort. *Cliff.* The Ozweega Tea.
N°. 491. p. 44. Ann. 1746. 1232.	*Myagrum siliculis longis C. B.* * Treacle Wormseed.
N°. 494. p. 361. Ann. 1748. 1336.	*Myagrum siliculis obverse ovatis, lateribus depressis* Fl. *Lugd.* † Gold of pleasure.

* *Myagrum siliqua longa* C. B. † Linn. H. Cliff. 328.

N°. 495. p. 404. Ann. 1749. 1380.	*Myagrum filiculis fulcatis, rugosis foliis obtusis dentatis* H. *Upsal.* Gold of pleasure with wrinkled pods, and obtuse indented leaves.
N°. 494. p. 361. Ann. 1748. 1337.	*Myrrbis lutea ducoides* Mor. *H. R. Blef.* Yellow, carrot-like Cicely.
——— p. 332. Ann. 1747. 1280.	*Myrrbis major, vel Cicutaria odorata C. B.* 160. *Officin.* 321. Sweet Cicely *or* great sweet Chervil, *by some* Sweet-Fern.
N°. 476. p. 422. Ann. 1743. 1080.	*Myrto-cistus* Pennei *Clus.* Myrtle Rock-rose.
N°. 491. p. 44. Ann. 1746. 1234.	*Myrtus Buxi folio* Schyl. *Hort.* Box-leaved Myrtle.
N°. 472. p. 76. Ann. 1741. 984.	*Myrtus flore pleno Cornuti.* The double-flowered Myrtle.
982.	*Myrtus foliis odore Nucis moschatæ* Schyl. *Cat.* The Nutmeg Myrtle.
983.	*Myrtus latifolia Bætica, foliis confertim nascentibus C. B.* The Orange-leaved Myrtle.
N°. 491. p. 44. Ann. 1746. 1233.	*Myrtus latifolia Romana C. B.* Common broad-leaved Myrtle.
N°. 476. p. 422. Ann. 1743. 1081.	*Napus dulcis sativus Off.* Navew gentle.
N°. 495. p. 404. Ann. 1749. 1381.	*Nardus Americana procerior; foliis cæsiis* Pluk. *Alm.* Tall American Nard with blueish leaves.
N°. 472. p. 76. Ann. 1741. 985.	*Nigella Cretica semine aromatico C. B.* Candy Fennel-flower with an aromatic seed.
N°. 480. p. 214. Ann. 1744. 1134.	*Nissolia Tourn.* Crimson Grass-Vetch.
N°. 495. p. 405. Ann. 1749. 1383.	*Obeliscotheca Hydrophylli foliis, lobis angustioribus Vaill.* Dwarf Sun-flower with Water-leaf leaves, and narrower lobes.
1382.	*Obeliscotheca Hydrophilli foliis, lobis latioribus Vaill.* Dwarf Sun-flower, with Water-leaf leaves, and broader lobes.

N°. 480. p. 214. Ann. 1744. 1135. *Obeliscotheca integrifolia, radio aureo, umbone atro-rubente* Hort. *Eltham.* Dwarf Sun-flower, with entire leaves, a golden ray, and a dark red disk.

N°. 491. p. 44. Ann. 1746. 1235. *Obeliscotheca minor integro folio Dillen.* Smaller Dwarf Sun-flower, with an entire leaf.

N°. 474. p. 191. Ann. 1742. 1047. *Ochrus folio integro capreolas emittente* C. B. 243. *Ochrus* or winged Pea, with an entire leaf, sending forth tendrils.

N°. 484. p. 599. Ann. 1745. 1190. *Oenanthe Apii folio C. B.* Water Dropwort with Smallage leaves.

1189. *Oenanthe Staphilini folio aliquatenus accedens J. B.* Dropwort with Carrot leaves.

N°. 476. p. 422. Ann. 1743. 1084. *Oenanthe Stellata* Cretica P. Alpini *Park.* Starry Dropwort of Candy.

N°. 472. p. 77. Ann. 1741. 987. *Olea maxima Hispanica C. B.* * The Spanish Olive.

988. *Olea minor Lucensis, fructus odorato Ibid.* † The Luca Olive.

—— p. 76. ———— 986. *Olea vulgaris sativa C. B.* The manured Olive.

N°. 494. p. 333. Ann. 1747. 1281. *Omphalodes Lusitanica Lini folio T.* 140. Venus Navel-wort.

N°. 472. p. 77. Ann. 1741. 987. *Onagra frutescens argentea angustifolia* Ind. *Hort. Chelf.* Narrow-leaved silver shrubby Tree-Primrose.

N°. 476. p. 422. Ann. 1743. 1082. *Onagra latifolia Tourn.* Broad-leaved Tree-Primrose.

N°. 491. p. 44. Ann. 1746. 1236. *Ophioglossum, Lingua Serpentina Park.* Adder's-tongue.

1237. *Ophris bifolia C. B.* Common Tway-blade.

N°. 476. p. 422. Ann. 1743. 1083. *Origanum Heracleoticum, Cunila gallinacea* Plinii *C. B.* Winter Sweet Marjoram.

N°. 474. p. 191. Ann. 1742. 1046. *Origanum* Off. *Origanum Anglicum Ger.* Wild Marjoram.

* C. B. makes a query what this is; *sed quid sunt Olivæ maximæ Hispanicæ?* Tournefort calls it *Olea fructu maximo, Olive d'Espagne.*

† This is not the name of *C. B.* but of Tournefort.

N°. 494. p. 361. Ann. 1748. 1338. *Ornithopodium portulacæ folio T.* 400. Birds-foot with a purſlane leaf.

N°. 491. p. 44. Ann. 1746. 1238. *Orobus ſylvaticus Viciæ foliis C. B.* Wood *Orobus* with Vetch leaves.

N°. 474. p. 191. Ann. 1742. 1048. *Orobus vulgaris herbariorum Ger. Park.* The common bitter Vetch.

N°. 495. p. 405. Ann. 1749. 1384. *Oxys lutea J. B.* 388. Wood-Sorrel with a yellow flower.

N°. 494. p. 333. Ann. 1747. 1282. *Padus Theophraſti Dalechampio Lugd.* 312. The wild Cluſter-Cherry, *or* Birds Cherry.

N°. 491. p. 45. Ann. 1746. 1239. *Pæonia mas* Officin. & *C. B.* The Male Peiony.

N°. 474. p. 191. Ann. 1742. 1049. *Panicum Indicum ſpica longiſſima C. B* 343. Indian Panick with a very long ſpike.

N°. 484. p. 599. Ann. 1745. 1191. *Papaver hortenſe ſemine nigro ſylveſt.* Dioſcoridis *C. B.* Garden Poppy with black ſeeds.

N°. 495. p. 405. Ann. 1749. 1385. *Partheniaſtrum Helenii folio* Hort. Elth. *Partheniaſtrum* with an Elecampane leaf.

N°. 494. p. 333. Ann. 1747. 1283. *Pavia* Boerh. Ind. Alt. 2. 260. Scarlet-flowering Horſe-Cheſtnut.

N°. 476. p. 422. Ann. 1743. 1085. *Periploca foliis oblongis Tourn.* Periploca with oblong leaves.

N°. 480. p. 214. Ann. 1744. 1136. *Phalaris major ſemine albo* C. B. Canary-graſs.

1137. *Phalaris major ſpica longiori Ibid.* Canary-graſs with a longer ſpike.

N°. 491. p. 45. Ann. 1746. 1240. *Phaſeolus flore coccineo Cornut.* The ſcarlet Kidney-bean.

N°. 472. p. 77. Ann. 1741. 990. *Piſum cortice eduli Tourn.* Pea with an eſculent huſk.

N°. 491. p. 45. Ann. 1746. 1241. *Piſum humile caule firmo Tourn.* Dwarf Pea.

N°. 476. p. 422. Ann. 1743. 1086. *Plantago major incana Park.* Hoary Plantain *or* Lamb's tongue.

N°. 491. p. 45. Ann. 1746. 1242. *Polemonium vulgare Tourn.* Greek Valerian, Ladder to Heaven, *or* Jacob's Ladder.

N°. 494.

N°. 494. p. 361. Ann. 1748. 1339. *Polygonatum caule purpurascente* Raii *Syn.* 2. 148. Solomon's Seal with a purplish stalk.

N°. 476. p. 422. Ann. 1743. 1087. *Polygonatum floribus ex singulis pediculis J. B.* Broad-leaved Solomon's Seal, with a large sweet flower.

N°. 494. p. 333. Ann. 1747. 1284. *Polygonatum latifolium, Hellebori albi foliis C. B.* Broad-leaved Solomon's Seal with leaves like the white Hellebore.

N°. 494. p. 361. Ann. 1748. 1340. *Primula veris flore rubro Ger.* Red Primrose.

1349. *Pseudo-acacia T.* 649. Bastard Acacia.

N°. 472. p. 77. Ann. 1741. 991. *Pseudo-dictamnus acetabulis Moluccæ C. B.* Bastard Dittany with Molucca Baulm leaves.

992. *Ptarmica flore pleno.* Double Sneezewort.

N°. 494. p. 333. Ann. 1747. 1285. *Pulmonaria maxima, foliis quasi saccharo incrustatis Pluknet.* Greatest Lungwort with leaves very much spotted.

1286. *Pulsatilla folio crassiore, & majore flore C. B.* 177. The greater *or* Danish Pasque-flower.

N°. 474. p. 191. Ann. 1742. 1050. *Punica malus* Off. *Malus Granata Ger.* The Pomegranate-tree.

N°. 480. p. 215. Ann. 1744. 1138. *Ranunculus aconiti folio, flore albo multiplici C. B.* Crowfoot with a Monks-hood leaf, and a double white flower, *commonly called* the fair maid of France.

N°. 476. p. 422. Ann. 1743. 1090. *Ranunculus echinatus Creticus C. B.* * Starry Hedge-hog Candy Crowfoot.

N°. 494. p. 333. Ann. 1747. 1287. *Ranunculus montanus, foliis Plantaginis C. B.* 180. Mountain Crowfoot with a Plantain leaf.

N°. 480. p. 215. Ann. 1744. 1139. *Ranunculus pratensis, erectus, acris, flore pleno C. B.* Upright meadow Crowfoot with a double flower.

* *Ranunculus stellatus echinatus Creticus* C. B.

N°. 494.

Nº. 494. p. 361. Ann. 1748. 1342. *Rapiſtrum * maximum Cornuti* 147. The greateſt Charlock.

Nº. 476. p. 422. Ann. 1743. 1088. *Rapunculus montanus corniculatus Ger.* Horned Rampion with a round ſpike.

Nº. 491. p. 45. Ann. 1746. 1243. *Rapuntium maximum coccineo ſpicato flore* Col. *in Rech.* Greater Rampion, with a crimſon ſpiked flower, *commonly called* the ſcarlet Cardinal's flower.

Nº. 484. p. 599. Ann. 1745. 1292. *Rawolfia tetraphylla latifolia Plumier.* Four-leaved Rawolfia with broad leaves.

1293. *Reſeda calcitrapæ folio Moriſon.* † Great white baſtard Rocket.

Nº. 494. p. 361. Ann. 1748. 1343. *Reſeda foliis inferioribus integris, ſuperioribus laciniatis.* Baſtard Rocket with the lower leaves entire, and the upper jagged.

Nº. 491. p. 45. Ann. 1746. 1244. *Rhamnus catharticus* Off. *& C. B.* Buck-thorn *or* common Purging-Thorn.

Nº. 480. p. 215. Ann. 1744. 1140. *Rhus Virginiana, ſparſa panicula, ramis patulis glabris* Hort. *Elth.* Virginian Sumach, with a ſparſed panicle, and ſmooth ſpreading branches.

Nº. 472. p. 77. Ann. 1741. 993. *Ricinus humilis, folio ſubrotundo, flore fructuque conglomerato Houſt.* ‖ Dwarf Oil-ſeed with roundiſh ſerrated leaves, ſilvered underneath, and the flower and fruit growing in bunches.

Nº. 484. p. 599. Ann. 1745. 1194. *Roſa ſylveſtris pomifera noſtras* Raii *Syn.* § The greater Engliſh Apple-Roſe.

Nº. 472. p. 77. Ann. 1741. 994. *Rubia procumbens hexaphylla purpurea H. L. Bat.* Procumbent ſix-leaved purple Madder.

Nº. 476. p. 422. Ann. 1743. 1089. *Rubia tinctorum ſativa Off.* Madder.

* *Rapiſtrum maximum, rotundifolium, monoſpermon* Cornuti.

† *Reſeda folio Calcitrapæ flore albo* Mor. H. R. Bleſ.

‖ *Ricinus humilis, foliis ſubrotundis, ſerratis & ſubtus argenteis, flore fructuque conglomeratis* Houſt. *Miller. Dict.*

§ *Roſa ſylveſtris pomifera major noſtras* Raii Syn. 11. 297.

N°. 484. p. 599. Ann. 1745. 1195. *Salvia major, an Sphacelus* Dioscoridis * *C. B.* The greater *or* common Sage.

1196. *Salvia minor aurita & non aurita C. B.* Sage of Virtue.

N°. 494. p. 361. Ann. 1748. 1344. *Sambucus folio laciniato C. B.*456. The cut *or* Parsley-leaved Elder.

N°. 494. p. 361. Ann. 1748. 1345. *Sambucus humilis, sive Ebulus* C.B. *Off.* 180. Dwarf-Elder, Wallwort *or* Danewort.

N°. 491. p. 45. Ann. 1746. 1245. *Satureia hortensis æstiva C. B.* Summer Savoury.

1247. *Saurucus humilis, folio carnoso rotundo Plum.* Dwarf Lizard's-tail with a round fleshy leaf.

N°. 480. p. 215. Ann. 1744. 1141. *Saxifraga pratensis flore pleno.* Meadow Saxifrage with a double flower.

N°. 484. p. 599. Ann. 1745. 1197. *Scabiosa arborea Cretica Ponæ.* Shrubby Scabious of Candy.

N°. 480. p. 215. Ann. 1744. 1143. *Scabiosa peregrina, capite oblongo nigricante, Zibethi odore C. B.*†

N°. 476. p. 422. Ann. 1743. 1091. *Sclarea Off.* Common Garden Clary.

N°. 491. p. 45. Ann. 1746. 1246. *Sclarea Indica flore variegato Boerh.* Indian Clary with a variegated leaf.

N°. 495. p. 405. Ann. 1749. 1386. *Scrophularia foliis Filicis modo laciniatis, vel Ruta canina latifolia* ‖. Broad-leaved Dog's-Rue.

N°. 494. p. 333. Ann. 1747. 1288. *Scrophularia Hispanica Sambuci folio glabro T.* 166. Spanish Figwort with a smooth Elder leaf.

1289. *Scrophularia maxima Lusitanica, Sambuci folio lanuginoso T.* 166. Greatest Portugal Figwort with a woolly Elder leaf.

1290. *Scrophularia peregrina* Cam. Hort. *Tab.* 43. Camerarius's foreign Figwort.

* Theophrasti. † This name is not in *C. B.* ‖ C. B.

N°. 495. p. 405. Ann. 1749. 1386. *Scrophularia Ruta canina dicta, vulgaris C. B.* 236. Dog's-rue.

N°. 494. p. 361. Ann. 1748. 1346. *Scutellaria foliis cordato-lanceolatis serratis, pedunculis multifloris.* * Scull-cap with heart-shaped serrated leaves, and many flowers upon each footstalk.

N°. 472. p. 77. Ann. 1741. 995. *Serpyllum odoratissimum glabrum, longiore folio Amm. p.* 52. Smooth sweet-scented wild Thyme with a longer leaf.

N°. 494. p. 361. Ann. 1748. 1347. *Sherardia* Dillenii *Cat. Giss. p.* 96. Little Field Madder.

N°. 495. p. 405. Ann. 1749. 1388. *Sideritis orientalis, Phlomidis folio T. Cor.* Eastern Ironwort with a *Phlomis* leaf.

N°. 480. p. 215. Ann. 1744. 1144. *Siliqua dulcis* C. B. † *& Off.* The Carob-tree.

N°. 495. p. 405. Ann. 1749. 1389. *Sinapistrum Lusitanicum, triphyllum, flore rubro, siliquis corniculatis.* || Three-leaved Portugal *Sinapistrum* with a horned pod.

N°. 494. p. 333. Ann. 1747. 1291. *Sysymbrium Orientale, facie Barbareæ, foliis Plantaginis T. Cor.* 16. Oriental Water-cress with Plantain leaves.

N°. 480. p. 215. Ann. 1744. 1142. *Smilax humillima unifolia Tourn.* The lowest Bindweed *or* One-Blade.

N°. 494. p. 333. Ann. 1747. 1292. *Smyrnium* Marth. 773. *Officin.* 457. Alexanders.

1293. *Smyrnium peregrinum folio oblongo C. B.* 154. Foreign Alexanders with an oblong leaf.

——— p. 361. Ann. 1748. 1348. *Sonchus maritimus angustifolius C. B. P.* Narrow-leaved Sea-Sowthistle.

——— p. 333. Ann. 1747. 1294. *Sorbus sativa* C. B. 415. *Officin.* 464. The true Service-tree *or* Sorb.

N°. 491. p. 45. Ann. 1746. 1248. *Spiræa Hispanica, Hyperici crenato folio Tourn.* Spanish Spiræa with a notched leaf.

* Flor. Leyd. † *Siliqua edulis* C. B. || H. L.

N°. 480.

N°. 480. p. 215.	Ann. 1744.	1145.	*Spiræa Hyperici folio* Tourn. *Hypericum frutex.*
			1146.	*Spiræa Opuli folio Tourn.* Virginian Gelder-Rose with a Currant leaf.
			1147.	*Spiræa salicis folio* Tourn. *Spiræa frutex.*
N°. 495. p. 405.	Ann. 1749.	1390.	*Stachys alba Michel.* White Base Horehound.
N°. 476. p. 423.	Ann. 1743.	1092.	*Stachys Canariensis frutescens, Verbasci folio Tourn.* Canary Shrubby Base Horehound, with a Mullein leaf.
N°. 495. p. 405.	Ann. 1749.	1391.	*Staphylodendron Virginianum triphyllum T.* 616. Three-leaved Virginian Bladder Nut.
N°. 484. p. 599.	Ann. 1745.	1198.	*Statice foliis angustioribus, flore rubro Tourn.* * Narrow-leaved Thrift with red flowers.
N°. 495. p. 405.	Ann. 1749.	1392.	*Statice Lusitanica Scorzoneræ folio* Inst. *R. H.* 341. Portugal Thrift with a Scorzonera leaf.
N°. 472. p. 77.	Ann. 1741.	996.	*Stœchas citrina angustifolia C. B.* Narrow-leaved Goldylocks *or* Cassidony.
N°. 484. p. 599.	Ann. 1745.	1199.	*Stœchas purpurea* Off. *& C. B.* Cassidony *or* French Lavender, *by some* Sticadore.
N°. 494. p. 333.	Ann. 1747.	1295.	*Symphytum majus, tuberoso radice C. B.* 476. Greater Comfrey with a tuberous root.
N°. 476. p. 423.	Ann. 1743.	1095.	*Tacamahac foliis serratis* Pluk. *Phyt.* 228. *Fig.* 2. Tacamahaca with serrated leaves.
N°. 480. p. 215.	Ann. 1744.	1148.	*Tagetes minor, flore simplici striato* Tourn. †
N°. 476. p. 423.	Ann. 1743.	1093.	*Tamariscus latiore folio* Park. *Germanicus Tourn.* ‖ The German Tamarisk.
		1094.	*Tamariscus tenuiore folio* Park. *Narbonensis Tourn.* § The French Tamarisk.

* Boerh. † This name is not in Tournefort. ‖ *Tamariscus Germanica* Lob.
§ *Tamariscus Narbonensis* Lob.

N°. 476. p. 423. Ann. 1743. 1096. *Tanacetum vulgare luteum C. B.* Common Tansy.

1097. *Tanacetum foliis crispis C. B.* Curled *or* double Tansy.

N°. 484. p. 599. Ann. 1745. 1200. *Thlaspi amarum arvense umbellatum J. B.* The bitter Field umbellated Mithridate-mustard.

1098. *Thlaspi Creticum purpureum Park.* Candy Tufts.

N°. 495. p. 405. Ann. 1749. 1393. *Thlaspi Virginianum Iberidis foliis amplioribus & serratis Tourn.* Virginia Mithridate-mustard with leaves like Sciatica Cress; but larger and serrated.

N°. 491. p. 45. Ann. 1746. 1249. *Thuya Theophrasti C. B.* The Tree of Life.

N°. 472. p. 77. Ann. 1741. 997. *Tithymalus Juniperi folio Boccon.* Maritime Spurge with a Juniper leaf.

N°. 494. p. 333. Ann. 1747. 1296. *Tordylium minus, limbo granulato, Syriacum* Mor. *Umb.* 37. Small Hartwort of *Syria*, with a granulated border.

N°. 476. p. 423. Ann. 1743. 1099. *Trachelium umbelliferum Ponæ.* * Blue umbelliferous Throatwort.

N°. 495. p. 405. Ann. 1749. 1393. *Tribulus terrestris, foliis Ciceris, fructu aculeato C. B. P.* Land Caltrop with a Chich leaf and a prickly fruit.

N°. 494. p. 333. Ann. 1747. 1297. *Trifolium clypeatum argenteum* Alp. *Exotic.* 307. Silver clypeated Trefoil.

129‖. *Trifolium montanum spica longissima rubente C. B.* 328. Mountain Trefoil with a very long reddish spike.

N°. 494. p. 361. Ann. 1748. 1349. *Trigonella leguminibus pedunculatis congestis &c.* Hort. *Upsal.* Wild Fenugreek.

—— p. 333. Ann. 1747. 1299. *Turritis annua verna, flore purpurascente T.* 224. Spring annual Tower-mustard with a purple flower.

* *Trachelium azureum umbelliferum* Ponæ.

N°. 491. p. 45. Ann. 1746. 1150. *Turritis muralis hirſuta minor Tourn.* * Small Tower-muſtard.

N°. 476. p. 423. Ann. 1743. 1100. *Valeriana major hortenſis* Moriſon. *Phu Off.* Great Garden Valerian *or* Setwall.

N°. 494. p. 333. Ann. 1747. 1300. *Vella* Lin. *Gen.* 654. Wild Spaniſh Creſs *or* wild Spaniſh Muſtard.

N°. 472. p. 77. Ann. 1741. 998. *Verbena anguſtifolia C. B.* †

N°. 480. p. 215. Ann. 1744. 1150. *Verbena tenuifolia C. B.* Narrow leaved Vervain.

N°. 495. p. 405. Ann. 1749. 1395. *Veronica cærulea, trifido & quinquefido folio* Fl. Bat. Blue Speedwell with a trifid or quinquefid leaf.

1396. *Veronica petræa ſempervirens* Pon. *Bald.* Evergreen Rock Speedwell.

N°. 472. p. 77. Ann. 1741. 1000. *Veronica ſpicata anguſtifolia Ger. Park.* ‖ The leſſer ſpiked Fluellin or Speedwell.

999. *Veronica ſpicata latifolia major Park.* Great broad-leaved ſpiked Speedwell *or* Fluellin.

N°. 495. p. 405. Ann. 1749. 1397. *Veronica Virginiana altiſſima ſpica multiplici floribus candidis* §. Tall Virginian Speedwell with many ſpikes and white flowers.

N°. 494. p. 361. Ann. 1748. 1350. *Viola Martia arboreſcens purpurea C. B.* 199. Purple arboreſcent Spring Violet.

N°. 495. p. 405. Ann. 1749. 139‖. *Urtica foliis profunde laciniatis, ſemine lini Amman.* Siberian Nettle with deeply cut leaves, and a Flax ſeed.

N°. 480. p. 215. Ann. 1744. 1150. *Xeranthemum flore albo pleno* Hort. *Lugd. Bat.* Eternal flower *or* Ptarmica with a double white flower.

N°. 495. p. 405. Ann. 1749. 1399. *Xylon Americanum præſtantiſſimum ſemine vireſcente* Lin. The moſt excellent

* This name is not in Tournefort. Perhaps the *Turritis muralis minor Pet.* is intended. † This name is not in *C. B.* ‖ *Veronica ſpicata anguſtifolia* C. B. *aſſurgens ſive ſpicata* Ger. *erecta anguſtifolia* Park. § Flor. Bat.

excellent American Cotton with a greeniſh ſeed.

Nº. 495. p. 405. Ann. 1749. 1400. *Zacintha ſive Cichorium verrucarium Matth.* 505. Wart Succory.

N. B Part of this Catalogue, containing the plants numbered from 1251 to 1400, was drawn up after the death of Mr *Miller*, by *John Wilmer*, M. D. *Hort. Chel. Præf. & Prælect. Botan.*

Some account of the remains of John Tradeſcant's *garden at* Lambeth; *by Mr* W. Watſon, *F. R. S.* Nº. 492. p. 160. Apr.&c.1749. *Read* May 25. 1749.

VIII. Upon a viſit made to Mr *John Tradeſcant*'s garden at *South Lambeth*, *May* 21, 1749. by Dr *Mitchell* and myſelf, were obſerved the under-mentioned exotic plants.

This garden was planted by the above-mentioned gentleman about 120 years ſince, and was, except that of Mr *John Gerard*, the author of the Herbal, probably the firſt botanical garden in *England*. The founder, after many years ſpent in the ſervice of the Lord Treaſurer *Saliſbury*, Lord *Wotton*, &c. travelled ſeveral years, and procured a great variety of plants and ſeeds before not known in *England*; to ſeveral of which at this time the Gardeners give his name, as a mark of diſtinction; as *Tradeſcant*'s Spiderwort, *Tradeſcant*'s After, *Tradeſcant*'s Daffodil. He firſt planted here the *Cupreſſus Americanus Acaciæ foliis deciduis*, which has been ſince ſo much eſteemed, and is now one of the great ornaments of the Duke of *Argyll*'s garden at *Witten*.

Mr *Tradeſcant*'s garden has now been many years totally neglected, and the houſe belonging to it empty and ruined; and though the garden is quite covered with weeds, there remain among them manifeſt footſteps of it's founder. We found there the *Borrago latifolia ſempervirens C. B. Polygonatum vulgare latifolium C. B. Ariſtolochia clematitis recta C. B.* and *Dracontium Dod.* There are yet remaining two trees of the *Arbutus*, the largeſt I have ſeen; which, from their being ſo long uſed to our winters, did not ſuffer by the ſevere colds of 1729 and 1740, when moſt of their kind were killed throughout *England*. In the orchard there is a tree of the *rhamnus catharticus*, about 20 feet high, and near a foot in diameter, by much the greateſt I ever ſaw.

It is not unlikely but there may be ſeveral other plants yet remaining in the garden, but flouriſhing at a different time of the year.

A letter from Dr Laurence Garcin, *of* Neufchatel, *F. R. S. to Sir* H. Sloane, *Bart. late P. R. S. concerning the*

IX. The plant in queſtion is a ſhrub, which varies conſiderably in it's ſize and figure, according to the nature and ſoil of the country where it naturally grows, as well in *Aſia* as in *Africa*, where this plant is much uſed, both as a medicine, and for it's agreeable odour.

Our author has given us the true characters of the fructification of the *Cyprus*, after the method of *Linneus*.

1. It's *calyx* is an expanded monophyllous cup, cut into 4 lobes, pointed at their extremities, and continuing attached to the fruit.

2. It's

2. It's *corolla* consists of 4 oval petals, somewhat pointed and sinuous. They grow distant one from the other, and are placed between the lobes of the *calyx*.

Cyprus *of the Ancients; done from the French by* W. Watson, *F. R. S.* N°. 489. p. 564. Oct & Nov. 1748. *Read* Nov. 24. 1748.

3. It has 8 erect *stamina*, ranged two by two almost horizontally, and parallel to the sides of the petals, and surpass them in length about $\frac{1}{3}$ a line. They grow from the base of the embryo at a little distance one from the other, and arise diminishing in their bulk to their extremities. Their *antheræ* or summits form each of them a little kind of purse.

4. It's *pistillum* is round, and occupies the middle of the *calyx*. It's style is erect, and terminated with a pointed *stigma*. It's length somewhat exceeds that of the *stamina*.

5. It's *pericarpium* is a round dry capsule, slightly four-corned; each of which corners has a small prickle. It is divided into 4 compartments by an extremely delicate membrane, arising from a *placenta* which occupies the centre of the capsule.

6. It's seeds are small and numerous: each of them is pyramidal, and somewhat quadrangular, of which the point is sometimes streight and sometimes crooked. Every seed is fastened by it's point to the *placenta*, as to a common centre, and their bases are sustained by the sides of the capsule, all the cavity of which is filled by them.

There is but one species of this shrub generally known through all the East; and this is subject to vary according to the climate, the season, and the soil.

It's names are,

1. According to different nations.

Greek, Κύπρος
Latin, *Cyprus*
Hebrew, *Copher*
Arabian and Persic, *Henna*
Egyptian, *Elhanne*
Italian, *Alchanna*
Spanish, *Alkenna*
The Portuguese in the Indies, *Foula*, *Aybana*
Apothecaries, *Alcanna*
The people of Malabar, *Mail-Anschi*
The Brachmans, *Mety*
Malayans, *Daun Lacca*
Javans, *Batschiar*
Chinese, *Tsingka Hou*
Indians, *Inne*
At Bengal and Surat, *Mendi*

2. According to Authors,

Ligustrum Dioscoridis. Matth. 117.
Ligustrum Ægyptiacum latifolium; *item angustifolium.* C. B. Pin. 476.
Ligustrum Ægyptium. J. B. I. 532.
Ligustrum orientale. Park. 1447. Raii Hist. 1603.
Rhamnus Malabaricus, fructu racemoso caliculato. Raii Hist. 1573.

The

It's deſcription.

The *Cyprus* grows generally as a ſhrub of 10 or 15 feet in height, and has very much the appearance of privet.

It's trunk grows ſometimes as thick as a man's thigh, is ſometimes ſtreight and ſometimes crooked, and produces a great number of branches irregularly. It's outward bark is aſh-coloured, and much furrowed, and detaches itſelf from the trunk of the tree in long ſcales or pieces, by the heat and dryneſs of the climate, as in the *Perſian* gulf. It's inward bark is reddiſh without, and whitiſh within. That of the branches is ſmooth and red, like that of the hazel-tree, and green within. It's young branches are ſtreight, flexible, and moderately long. The wood of the trunk is hard and whitiſh.

It's leaves are diſpoſed in different orders upon the ſame twig. Sometimes they are placed oppoſite in pairs along the ſmall branches, and this moſt generally croſs-wiſe; ſometimes by three and three; but then the leaves are leſs, and this diſpoſition generally takes place in the larger branches; ſometimes they are alternate, but rarely, and then the leaves are largeſt. The leaſt branches are moſt charged with leaves, the larger ones leaſt. All theſe leaves are pointed at each end; the largeſt are 2 inches long, and about an inch broad in their middle; the ſmalleſt bear ½ the dimenſions of the largeſt: their edges are even: they are ſmooth, ſhining, and of a beautiful green colour: their middle rib, which ſerves to each leaf as a ſhort pedicle, is terminated in their point, but ſends out, in it's paſſage through the leaf, alternately 4 or 5 nervous filaments on each ſide. Theſe leaves are much like thoſe of privet.

The flowers grow in bunches at the extremities of the young branches, and are endowed with a very agreeable and ſingular odour. They are of a ſtraw-colour; but as they grow old and wither, they become of the colour of a citron. The *calyx* is more pale than the *corolla* of the flowers. It's petals are turned up as much if not more than thoſe ſmall petals are which adorn the centre of a double roſe. The *ſtamina*, which are white, teanſparent, and which grow from the baſe of the embryon of the fruit, form as it were a double croſs, by their almoſt parallel ſituation and extenſion between the petals. The lobes of the *calyx*, being of the ſame length and form with the petals, ſeem to give to the entire flower an octogonal figure. The ſummits or *antheræ* are ſmall, and of the ſame colour as the petals, each having a deep furrow in it's bottom; the more theſe decay, the more yellow they grow, in the ſame manner as the petals. The furrow in the *anthera*, which at firſt is of a paliſh black, grows of a deeper hue, as the flower fades. The *piſtillum*, after the flower is gone, grows larger in the *calyx*, and becomes, when perfectly ripe, a dry, membranous, round fruit, of about 3 lines in diameter. But before it arrives to this ſtate, it reſembles very much a fleſhy berty, green on one ſide, purpliſh, and ſometimes black on the other, with very little juice. This falſe berry is the growing capſule, the ſide of which is ſoft, ſucculent, and very thick; which, in proportion as it increaſes, becomes thin, membranous, dry, and brittle: in becoming

becoming thus capacious and thin it gives room to a large number of pyramidal ſeeds, very cloſe one to another, and faſtened all by their points to a common center, a kind of *placenta*. When this capſule is in it's perfection, its outſide is ſhining, and not unlike the ſeed of coriander in colour. The *pericarpium* is as it were divided into four *loculi*, by membranes ſo delicate, that they muſt be regarded with great attention, to be ſatisfied of their reality. The exterior form of this fruit ſufficiently ſhews this diviſion, by it's roundneſs being interrupted by 4 ſlight ribs, like thoſe of a melon, which ſhews as many cells. The membranes, which divide theſe cells, ariſe from the *placenta*, and are inſerted into the ſides of the capſule.

The ſeeds, which fill all the capſule, amount to about 4 or 5 dozen, according as they are more or leſs nouriſhed; becauſe the larger ones receiving more nouriſhment, make the ſmaller ones abortive. They are always ſo preſſed in their apartments, that their pyramidal figure is owing only to this preſſure, which ariſes from their reciprocal increaſe. The pyramidal points of theſe ſeeds are crooked in ſome, and bent in others, according to the direction given them in their growing. Their colour is red or brown, and always ſomewhat ſhining.

We find, in the ancient writers of plants, ſuch as *Theophraſtus*, *Dioſcorides*, and *Pliny*, who have all in their manner treated of vegetables, of how much eſteem the *Cyprus* was among the Ancients. The Hiſtorian *Joſephus*, and St. *Jerome* have mentioned it as a rare and precious plant, placing it in the ſame rank with the moſt valued ſpices. The fine ſmell, which it's flowers ſend forth in the countries where they grow naturally, as in *Egypt*, *Syria*, *Arabia*, *Perſia*, *&c.* has occaſioned it's uſe in the earlieſt time; and the ſame uſe continues in thoſe countries. It's being twice mentioned in * *Solomon's Song*, is a very great proof of it's being much valued in the moſt ancient time. We there ſee it was accuſtomed to be cultivated even in their vineyards. The perfumers in old times made thereof an oil or precious ointment for various uſes; but principally to give their anointings a grateful odour, and to make ſupple the limbs of the body. *Remarks.*

Modern authors have given themſelves great trouble to be thoroughly ſatisfied of the Hiſtory of this plant. There have been great controverſies among them concerning it in endeavouring to ſettle it's deſcription; but it muſt be confeſſed they have made a very ſmall progreſs in diſcovering to us it's true characters. How many miſtakes have the Botaniſts of the two laſt centuries made, owing to the bad deſcriptions of this plant, which the Ancients have left us.

Dioſcorides, who, by deſcribing the plants he treats of too briefly, always leaves their characters imperfect, ſays (perhaps after ſome other author more ancient than himſelf) *that the leaves of the plant in queſtion*

* *Solomon's Song*, chap. i. 14. chap. iv. 13. In both theſe places the *Engliſh* tranſlation of the bible has it *Camphire*, inſtead of *Cyprus*.

are

are like those of the olive-tree; that it's flowers are in bunches, and that it's fruit is black, like that of elder. This was enough to make the *Latins* conjecture, that the κύπρος of this author was the *ligustrum* or privet; and the more so, as the *Cyprus* was entirely unknown to them, since it only grew in *Egypt* and in *Syria*, where it was always called *henna*, or *alhenna*, and, by corruption, *alkanna*.

There is some appearance, that, as the *Greeks* received a good quantity of this drug from the isle of *Cyprus*, as a species of merchandize, they would chuse to call it *Cyprus*, rather than give it any other denomination, on account of the quantity furnished to them from the isle of that name. *Pliny* took it first for a kind of privet or *ligustrum*, which grew particularly in *Egypt*, and afterwards he thought it to be the common *ligustrum* of *Europe*: this shews how uncertain he was as to the plant in question. He judged ill in comparing the fruit of the *Cyprus* with that of the jujube-tree; but was more happy in likening the fruit (capsule) to that of the coriander, as they agree in colour, tho' that of the *Cyprus* was more large. *Matthiolus*, who thought himself greatly above his contemporaries in the theory of plants, asserts boldly, that our plant was the common privet: and in this he thinks himself justified, not only from the description of *Dioscorides*, but from the virtues attributed to the *Cyprus* by *Pliny*. He even ridicules those who think that the *ligustrum* and *Cyprus* are different plants. *Fuchsius*, who wrote before *Mathiolus*, had nevertheless reason to believe them of a different *genus*, by the account given of the *Egyptian* plant by *Pliny*; but he was wrong in confounding it with the *phillyrea* of *Dioscorides*; and in this mistake he has been followed by *Dodonæus*.

Bellonius, who has seen this plant in it's place of growth, well knew that it was not the *ligustrum* or privet: he saw also how the Commentators of the *Arabian* authors were deceived in taking it for such.

Rauwolf and *Prosper Alpinus*, who met with it in their travels, after having observed it in the places of it's growth, believed, as *Pliny* had done, that it was a kind of *ligustrum*, which approached very near to that of *Europe*. They have each of them given a different figure; which made *Caspar Bauhin* believe that there must be two new species of *ligustrum*; but herein he was not followed by Mr *Ray*. In fact, we ought to acknowledge, by the characters here set down, that our *Cyprus* is of a *genus* truly different, and the only one of it's kind.

The *Hortus Malabar.* has given a figure of this plant under the name of *Mail-Anschi*, which represents the end of a large branch ill-chosen, and somewhat withered, without doubt by the fault of the designer, who has drawn it in it's natural size; which is greater in *Malabar* than elsewhere, because of the rains which fall there in abundance half the year. This shrub is less in all it's parts in *Arabia*, and to the south of *Persia*, because in those countries it rains seldom; but, in recompence, it's flowers have much more smell than in *Malabar*. It must be remarked

marked here upon this occasion, that the description just now given, and which contains the size of the parts, was made in a garden in the *Persian* gulf belonging to the *Dutch* factory, and situated about a league from the town of *Gameroon*, otherwise called *Bender-Abassi*, where there was one of these trees carefully preserved, which was the first I saw in the *Indies*; as it was complete in all it's parts, having flowers and fruit; and as it appeared to me agreeable and curious, especially on account of the fine smell of the flowers, and as it was a new *genus* to be established in Botany, I examined it with great exactness, and noted it's characters, figures, and dimensions. I did not conceive it to be the *Cyprus*, not then knowing what it was. I asked the people of the country the name of this beautiful shrub: they only called it *Henna*, and I could learn no other name: they assured me it had no other name, either in *Persia*, or in *Arabia*. It was on the first of *Dec.* 1721. that I observed it, and described it under the name of *Frutex Persicus, foliis ligustri, flore & fructu racemoso, Henna vulgo dictus*. I thus characterized it, in expectation of finding it, if it had already been described among authors, after my return to *Europe*. When I returned in 1730. I had the satisfaction to find it in Mr *Ray*'s History, by the description which he has given of it, extracted from various authors, in the chapter of *ligustrum* under the *synonyma* of *Parkinson*, and to see it in the other authors I have mentioned, especially the figure given by *Rauwolf*, which is not a bad one, and is copied by *Clusius*, *Dodonæus*, *Parkinson*, and *Dalechamp*.

The figure in the *Hort. Mal.* under the name of *Mail-anschi*, does not so happily represent our *Cyprus*, as that excellent work generally does the plants it treats of. The leaves of this plant there are half withered, and not in their natural disposition. *Rauwolf*'s figure is much nearer the truth. The flowers are not much better represented than the leaves, in the *Hort. Mal.*; as, besides other things of less moment, the authors of that work have neglected to make the petals appear between the lobes of the *calyx*, as always happens in a natural state; by which disposition the flower appears of an octogonal figure. *Rumphius*, who has written an history of the plants of *Molucca*, has given a description of this shrub, not different from mine.

By what is here laid down of the characters of this plant, we plainly see that it differs widely from the *oxycantha* and *rhamnus*; of one of which the authors of the Notes to the *Hortus Malabaricus* suspected the *Cyprus* to be a species. This occasioned Mr *Ray* to range it under the last, supposing it's fruit to be a berry, which nevertheless it is not. This learned author moreover could not think that the *mail-anschi* was the *Cyprus*, because of the difference in the descriptions among authors, and of the imperfection of those of *Rauwolf* and *Alpinus*. *Rumphius*, just now quoted, has ill compared the colour of the leaves of *Cyprus* to those of the olive-tree.

This shrub, so cherished among the eastern nations, is cultivated in *Africa*, *Asia*, and all the *Indies*; that is to say, from near the equinoctial

tial even to 35° of N. lat. where it is much uſed, as we ſhall find by the great commerce cauſed thereby in the *Levant*, according to the relations of travellers of credit.

This plant does not love ſhade, even under the torrid zone, becauſe of the violent rains there at the time of the weſtern *Monſoon*, no more than it does in cold countries, our author means thoſe of the fifth climate; but towards the tropick, and even in *Arabia*, it grows beſt when a little ſheltered from the ſun. In hot and dry countries, as in the *Perſian* gulf, where I firſt ſaw it, it produced a great number of boughs and branches very ſhort, which gave it the appearance of white-thorn. On the contrary, towards the Equator, it's branches are further from each other, and longer, occaſioned by the moiſture from the rain. The bark ſplits into ſcales, and detaches itſelf in pieces from the trunk, in thoſe countries where it rains ſeldom; but in *Malabar*, in the iſles of *Ceylon* and *Sunda*; the bark continues entire and united almoſt all the year, becauſe of the moiſture of thoſe places.

Rauwolf remarks, that the *Turks* and *Moors* cultivate this plant with care, and even keep it in pots, on account of the ſmell of the flowers, which ſomewhat reſemble muſk. They keep theſe pots in winter in chambers or caves to preſerve the plants from cold.

Our author forgot to remark one circumſtance, mentioned by *Bellonius* in the firſt book of his Obſ. (*chap.* 44. *apud Cluſium*), where it is ſaid, that the *Henna*, or *Alchanna*, which is our *Cyprus*, differs from privet, becauſe the leaves of privet fall, and thoſe of *Cyprus* continue all the year. But this obſervation is of no weight, becauſe this difference is only apparent; and it is certain, that if our privet was cultivated in *Egypt*, it's leaves would not fall off in winter, becauſe it is not there ſufficiently cold.

The uſes of Cyprus.

Bellonius, who was the firſt of the Moderns who treated of this ſhrub under the name of *Alcanna*, and ſpoke of it's culture in *Egypt*, tells us, that the powder of it's leaves is ſo great an article of commerce among the *Turks*, that they load ſeveral veſſels from *Alexandria* for *Conſtantinople*, where the ſale of it is ſo great, that the grand Signior's revenue therefrom amounts yearly to 18000 ducats. According to him, the great conſumption of this powder ariſes from it's being uſed in beautifying the ſkin and nails, in making them red with a decoction made therewith. The women, he ſays, generally uſe it all over *Turkey*, to dye the ſkin of thoſe parts which are from the navel downwards, as well as their hands and their hair. Their children are ſerved in the ſame manner. They conſider this as a great ornament; and that the colour may hold longer, and penetrate deeper, they apply it uſually when they go out of the baths. This practice of dyeing, to beautify the body, is extended even to their horſes, of which they tinge the mane, the tail, and the hoofs. They often add alum to heighten the colour. This powder is ſent from *Conſtantinople* to *Ruſſia*. Let us now conſider the other properties of *Cyprus*.

It

It is not neceſſary here to take notice of what *Dioſcorides* and *Pliny* attribute to this plant; they may be conſulted, if, at the ſame time, they are regarded as being very little ſkilled in it's true qualities. Our author contents himſelf with ſaying, that the *Perſians* and *Arabians*, who appear to have been anciently the firſt that uſed this plant, frequently uſe at preſent not only it's flowers to perfume their linen, cloaths, and tables, but make a greater uſe of it's leaves in a decoction, for the cure of all diſtempers of the ſkin, as the itch, ſcabs, and ring-worm, which the air of their country cauſes from it's heat, and from the drought which often reigns there to a great degree. Theſe diſorders, if they are neglected to be cured as ſoon as poſſible in dry climates, eaſily degenerate into the leproſy; and it is on account of theſe diſorders of the ſkin, that the eating of pork is forbidden to people of every religion in theſe countries; becauſe that food there is known to occaſion theſe diſtempers.

All the nations of the *Eaſt Indies* make uſe of it in medicine, for the ſame, as well as for ſeveral other diſorders; but they particularly uſe the leaves to dye their nails; which our author thinks they had originally from the *Arabians*. In dyeing their nails, the *Indians* make uſe of the freſh leaves, which always grow in great plenty in their gardens, and apply them beaten upon their nails, mixing with them ſometimes a little lime and juice of citron. This colour laſts a great while upon the ſkin, on account of ſweating. A ſtrong decoction of the leaves in water is ſometimes uſed to tinge their nails, but more generally their ſkin and hair.

There is reaſon to believe, that this pretended beautifying of the ſkin, the hair, and nails, which long cuſtom has eſtabliſhed among the eaſtern nations, owes it's origin to a quite different principle than that of beautifying. The Ancients had no other view in the beginning, than the prevention of pruriginous and leprous diſorders in the ſkin, to which their climate ſubjected them, as well as to preſerve them from vermin, as the leaves of *Cyprus* have that property. But as in uſing baths with theſe leaves therein, they dyed their ſkin either red or yellow, according to the preparation, they accuſtomed themſelves to this colour by degrees, and afterwards regarded it as a ſalutary embelliſhment.

Theſe baths, which there are conſtantly employed for the cleanlineſs and health of the ſkin, and which the neceſſity of uſing has eſtabliſhed as a point of religion, and a duty, for the better prevention of theſe maladies, is certainly a true method to preſerve as well the body as the ſkin in a good ſtate. Theſe good effects are extended further by uſing the *Alcanna*; becauſe it's colour, paſſing in the opinion of theſe people for a neceſſary ornament, and a mark of cleanlineſs, makes the practice of bathing better obſerved.

It ſeems to our author, that theſe remarks ſhould be communicated, as well as the characters and deſcription of the plant in queſtion, to render it's hiſtory more complete, and by theſe means to make it known;

 to

to the end that the curious may form ſome opinion of the great praiſes which the Ancients have beſtowed upon this plant.

A letter from the Rev. H. Miles, *D. D. F. R. S. to Mr* H. Baker, *F. R. S. concerning the green* Mould *on fire-wood; with ſome obſervations of Mr* Baker's *upon the minuteneſs of the* Seeds *of ſome plants.* N°. 494. p. 334. Jan. &c 1750. *Read* Feb. 15. 1749.

X. Some days ago, happening to take notice of a quantity of what we commonly call Mould *, on the bark of ſome fire-wood, I had the curioſity to view it with a lens, of about an inch focus, when I found it to conſiſt of numbers of minute *fungus's*, whoſe regular appearance invited me to examine them in the microſcope, with a good magnifier; upon which their ſpherical heads ſeemed as if they had been nothing elſe but globules of ſeeds; at the ſame time, I obſerved ſeveral ſeeds adhering to the tranſparent footſtalks, which ſupported the heads, and many ſcattered on the glaſs-plate, whereon the ſubſtance was placed, in order to be viewed. And here I had an opportunity of ſeeing many diſtinct ſeeds, which appeared, nearly, of an oval form, but ſeveral times larger than the ſeeds of common muſhrooms, even when ſeen with the ſecond magnifier, and the latter with the firſt.

I pretend not to any ſkill in Botany; ſometimes, and, indeed, but ſeldom, I look into an author on the ſubject, as an amuſement and relief to my mind; therefore it would ill become me to attempt the referring this plant to the proper claſs. *Micheli*, in his *Nova plantarum genera*, has given us the draught of ſome, which well repreſent the figure of them, as they appear, when much magnified, Tab. LXXXII. *Fig.* 1. and in page 200. of his excellent Work, deſcribeth them, under this title, *Fungoidaſtri ſemine in ſuperna parte donati:* but then his figures are ſuch as the plants appear to have, to the naked eye (as we may preſume), ſince he does not ſay any thing to the contrary; not to mention that there are other different characteriſtics in his Deſcription. The ſame celebrated writer deſcribes another ſpecies, p. 215. under the following title, *Mucores pediculo donati*, which in reſpect of ſize, the ſubſtance, and ſome other characters, correſpond with theſe I am ſpeaking of, well enough: but as he refers to Dr *Hooke*'s Micrographia, Tab. XII. for an elegant figure of them (beſides what he has himſelf given us Tab. XCV.), both Dr *Hooke*'s and his own figures repreſent the heads, as quite ſmooth, on the ſurface; and the Doctor, in his deſcription of them, p. 126. expreſsly ſays they are of *a ſmooth ſurface.* Whence I conclude this muſt be a different ſpecies. However what the ingenious author of the *Micrographia reſtaurata* ſays of the ſeeds of theſe diminutive bodies, p. 19. is put out of all doubt.

Permit me to add, that having often viewed the heads of a ſmall kind of *fungus*, which are about $\frac{1}{8}$ inch diameter, of a coriaceous ſubſtance, I have ever found the ſeeds which are produced on the gills, much larger than thoſe of any muſhrooms I ever examined, though rather leſs than thoſe produced by this unregarded plant.

Now, that a body whoſe form is not to be diſtinguiſhed by the unaſſiſted eye, ſhould produce ſeeds ſeveral times larger than another of

* Of a bright verdegriſe colour.

the

the ſame *genus* does, which exceedeth it many millions of times in bulk, muſt ſuggeſt thoſe thoughts to one's mind, which, I know, I need not point out to you.

Some obſervations on the above-mentioned plants and ſeeds; by H. Baker, *F. R. S.*

I have carefully examined the plants and ſeeds ſent me by Dr *Miles*, in order to determine their real bigneſs; and, taking the fungous heads of the middle ſize (ſome being larger and others ſmaller), I find, according to my micrometer, that 3 of them take up the ſide of a ſquare, 70 of which ſquares make an inch in length, and conſequently, that 3 times 70, or 210 of theſe *fungi* are required to make a line whoſe length is one inch; or, in other words, that the diameter of theſe fungous bodies is, at a medium, the 210th part of an inch.

The ſeeds are oval; and I find, by the ſame micrometer, that 10 of them laid by one another the ſhorteſt way of their diameter, or 8 of them the longeſt way, fill up the ſide of a ſquare, 270 of which ſquares make an inch in length. Taking therefore 9 at the medium, 270 times 9, or 2430 of theſe ſeeds will be required to make a line of an inch in length; or, in other words, each ſeed is the 2430th part of an inch in diameter.

And according to theſe calculations 44,100 of the fungous heads, or 5,904,900 of the ſeeds may lie by one another in the ſurface of an inch ſquare.

Yet minute as the ſeeds of this little fungus are, Dr *Miles* obſerves, very juſtly, that they are larger than the ſeeds of ſome muſhrooms, which exceed it many millions of times in bigneſs. As to which, I beg leave to take notice, that the proportion, in ſize of the fruits or ſeeds of trees or plants, to the ſize of the trees or plants that bear them, comes under no regulations that correſpond with our conceptions. For the vaſt bulk of ſome ſorts of timber-trees (the beech and aſh, for inſtance) is produced from a ſeed ſmaller than that of the common garden bean. The towering and mighty oak produces for it's fruit only a little acorn, whereas the pumpkin (ſome whereof weigh above 100 pounds) is the production of a feeble creeping plant, unable to ſupport itſelf, and much leſs it's enormous fruit. The *vanilla* (a plant that riſes to the height of ſeveral feet, by claſping about whatever it finds near it) produces, in long pods, ſeeds ſo ſmall, that their diameter is not more than the 100th part of an inch. Suppoſing therefore the cavity of the pod to be equal to a cylindrical tube of $\frac{1}{10}$ of an inch diameter, and the length of the pod to be ſix inches (which dimenſions are taken with great moderation) the number of ſeeds contained in one ſingle pod will be more than 47000. Moſt kinds of fern, of which ſome are pretty large plants, bear ſeeds ſo extremely minute, that they appear to the naked eye only like a fine duſt; while ſeeds of a conſiderable bigneſs are produced by plants of a great deal ſmaller ſize.

XI. The

Obſervations relating to vegetable Seeds; *by* J. Parſons, *M. D. F. R. S.* N°. 474. p. 184. June &c. 1744. *Read* Nov. 22. 1744.

XI. The ſeed of the *muſk ſcabious* reſembles an octogonal *vaſe* with a ſcalloped brim: the whole is bell-ſhaped, having ribs or diviſions, which run down from the mouth of the *vaſe*, and, becoming narrower, form the bottom: between theſe ribs, down to the beginning of the narrow part, it is clear, tho' not quite tranſparent; and, from thence to the bottom, the ribs are hairy. This *vaſe* contains a ſeed, which is like a *peſtle* ſtanding in a *mortar*: the *peſtle* is looſe in an octogonal caſe; but the narrowneſs of the mouth of this caſe hinders the peſtle's being drawn out, becauſe it's extremity, within, is round and bulky. From it's upper end ariſe 5 ſpiculated *ariſtæ*, whoſe little thorns are directed upwards, and are thereby prepared to cauſe the ſeed to recede from any thing that might injure it upon being touched; and the baſin, from which the *ariſtæ* riſe, is of a fine green colour; they are of a ſhining brown.

The *angelica* is one of the moſt fragrant and agreeable ſeeds, for it's ſmell, in the world. When the huſk is pulled off, the *nucleus* appears of a browniſh colour. and it's ſhape is elliptical. By the help of the *microſcope*, we know what produces that charming ſmell, being a fine amber-coloured *gum*, which appears in ridges diſpoſed alternately, with others of a browniſh colour, in a longitudinal direction all over the *nucleus*. What appears white, on the flat ſide, is a *theca*, which receives a very minute *ſtilus* from the pedicle that ſupports it.

The ſeed, which is vulgarly called *Grains of Paradiſe*, although promiſing from it's aſpect but very little that is curious, being only a brown irregular ſeed with flats and angles, and having an *apex* like the mouth of a purſe drawn up with a ſtring; yet, when diſſected, produces a moſt beautiful appearance. In a longitudinal ſection, you ſee, firſt, the edge of the brown *cortex*; next to that, a black pitchy ſubſtance; and, within that, an exceeding white radiated matter, which looks like a fine white ſalt, and is, probably, a mixture of a volatile pungent ſalt with a farinaceous ſubſtance: the radiation ſeems to confirm this opinion; for, if it were only a *farina*, it could have no ſuch appearance, and ſo does it's exceeding ſharp taſte. But the moſt remarkable and curious part of this ſeed, is a little piece of *camphire*, exactly ſhaped like a common vinegar-crewet, having a round bottom, and a long taper neck. This is the conſtant form in hundreds of theſe ſeeds that I have cut. Theſe curious appearances, I believe, were never obſerved before.

I ſhall, at preſent, only mention one more ſeed, which is that of the great *maple-tree*. It conſiſts of a *pod* and it's *wing*: two of theſe grow upon a foot-ſtalk with the pods together, which makes them reſemble the body of an inſect with a pair of expanded wings. The wings are finely vaſculated, and the pod is lined with fine ſilky down, which contains a round compact *pellet* covered with a brown membrane, that ſticks very cloſe to it. When this is peeled off, inſtead of diſcovering a kernel, as in other ſeeds, an entire *green plant* appears to be folded up in a moſt ſurprizing manner, whoſe *pedicle* is about $\frac{2}{3}$ of an inch long, and it's it's ſeminal leaves about $\frac{4}{8}$ each; between which the *germina* of the next pair of leaves are barely viſible to the naked eye, but plain with a *microſcope*. This diſcovery gave me great pleaſure, as believing myſelf the only one who had obſerved it; but, ſome time after, looking into *Derham*'s *Phyſico-Theology*, upon another account, I found it mentioned, as if Dr *Highmore* had ſeen and communicated it to Mr *Ray*. I believe, however, as none of this learned *Society* have ſeen it, except thoſe I have ſhewn it to, the ſight of it will not be diſagreeable.

Numbers of ſuch amazing *phænomena* appear every day in my obſervations (ſome of which ſhall hereafter be laid before you, if theſe prove acceptable), which excited me to a reſolution, of examining and deſcribing all the *genera* of ſeeds. A work which is now publiſhing under the title of *The Microſcopical Theater of Seeds*, &c. in a manner, which, I hope, will render Botany more eaſily underſtood, will hand down to poſterity the true figures of every ſeed and it's ſections; and, by the new diſcoveries, which often occur thro' the courſe of my obſervations, lay a foundation for future obſervers to build ſomething uſeful upon; and ſettle ſome points relating to the different ſubſtances contained in vegetables, which yet remain doubtful.

Extract of a letter from Mr Benj. Cook, *F.R.S. to Mr* Peter Collinſon, *F. R. S. concerning the effect which the* Farina *of the bloſſoms of different ſorts of* Apple-trees *had on the fruit of a neighbouring tree.* N°. 477. p. 525. Aug. &c. 1745. *The* Apples *ſhewn* Nov. 14. 1745.

XII. 1. I have ſent you ſome *ruſſetings* changed by the *farina* of a next-door neighbour, whoſe name I wanted ſkill to know; but can only ſay, that the *ruſſeting* has exactly acquired his face and complexion.

[Mr *Collinſon* then produced ſeveral ſamples of the apples; an unteinted ruſſeting; a ruſſeting changed in complexion, which grew among a great cluſter of unaltered brethren; and ſome apples of the other tree, which had cauſed the change in the ruſſetings, and whoſe fruit had in return received a rough coat from the ruſſetings.]

Theophraſtus takes notice of this Παραλλαγη, as he calls it; and tells us the old Divines were wont to make a great pother about it, and foretel great events by it: *Pliny* informs us, there was one who wrote a whole book about ſuch changes. But the uſe I ſhould make of it, is chiefly this, that it may be of importance to the curious in fruits, to take care how their trees are ſorted, and what company they keep. For tho' this change be not ſo conſpicuous in apples which have a ſmooth green coat, as in the ruſſet-breed, yet one may ſuppoſe impreſſions of this ſort often made on them; and perhaps their juices altered for the better or worſe. *

2. I ſent

* Sir *Joſ. Ayloffe*, Bart. F. R. S. communicated, on *July* 1. 1731. from the Rev. Mr *Henchman*, Prebendary of *Saliſbury*, ſome obſervations of peaſe of different colours infecting one another in the ſame manner as the apples above-mentioned.

Mr *Henchman*, in the ſpring 1729. ſowed a piece of ground in his garden with white peaſe, and two double rows of blue peaſe, with an alley four feet wide between; in autumn,

——*by the same.* N°.490. p. 602. Dec. 1748. *Read* Dec. 22. 1748. 2. I ſent you laſt year a ſpecimen of the effect of the *farina* of a rough coat apple ſtriking on the flower of a ſmooth-coat; I have now ſent an example of the *farina* of the latter changing the former into it's own dreſs and likeneſs.

The ſituation of the ruſſeting was ſuch, that he was ſurrounded by winter pippins, pearmains, and ſuch-like; and we put the maſter-fruit together with ſeveral of the changelings, as they grew on the ſame branches mixed together.

This inſtance will ſhew what alterations may be expected in cognate ſpecies; and I ſhould have given an example of a kind of antipathy betwixt the pear and the apple in like circumſtances, but was diſappointed.

——*by the ſame.* N°. 493. p. 205. Oct. &c. 1749. *Read* Nov. 2. 1749. 3. When the farina of one apple impregnates another's bloſſom of differing ſpecies, we ſee the change in the fruit; but whether any laſting impreſſion is left on the bough which bore it (as ſeems to be in tulips and ſome other flowers), is not ſo eaſy to determine, experiments of this ſort being not to be made at all, but catched at diſtant opportunities; and till this point is ſettled, the diſtemper of my good friend's tree muſt reſt unexplained.

Artificial helps of ſight have added to former diſcoveries the exploſive manner of the *farina*'s action; but what may be the effect of the inconceivably fine ſubtile matter emitted from it's globules, and continually wafted about in great plenty and variety in the ſummer air, not only on vegetable productions (where on different ſubjects it may not improbably have oppoſite effects) but other matters not yet ſuſpected to be ſo much under it's influence, remains a field of inquiry for future ages. However, to what Mr *Loggan* hath very juſtly obſerved on the manner of impregnation of the ſeeds in mayze, I can add this, that if the ſeed and whole ſpecies of mayze be planted about two yards diſtance from each other, there will be a mixture of red and white grains in the ears of each plant, and you may with pleaſure obſerve the filament in the white plant, which hath been ſtruck with the red farina, diſcovering it's alien commerce by a conſcious bluſh, and by counting the threads they ſtained, foretell how many correſponding ſeeds will appear red, at the opening of the ear, when ripe.

autumn, upon gathering ſome for ſeed, he opened one of the pods, and was ſurprized to ſee one blue pea at the end next the ſtalk, with ſix white peaſe: but after having examined ſeveral other ſhells very carefully, he found a great variety of intermixtures of the white and blue peaſe in the ſame ſhells; ſometimes one white (or blue) only at one end, ſometimes at each end; ſometimes two white (or blue) with one of the other colour interchangeably; and thus the whole parcel that was rubbed out for ſeed was intermixt white and blue. The next year, he ſays, not having plotts of white and blue peaſe ſtanding near one another, he did not find any ſuch mixture in the ſeveral parcels then ſaved for ſeed. But it is pity he did not pick out a ſufficient number of the blue peaſe from among the white, and ſow them by themſelves, in order to ſee what coloured peaſe this mixt breed would have produced. *C M.*

XIII. 1. The

XIII. 1. The firſt experiment I made, was gathering the bud of a hollyhock ſo young, that the *petala* were not yet formed; and ſtripping off the *calix*, nothing appeared, but the *apices* cloſe to the *ſtylus* (for the *ſtamina* were not yet perceptible): theſe *apices* appeared to me to be a kind of bag; and I could plainly perceive a ſeam (if I may ſo call it) run down the middle of it. This occaſioned me to take a fine needle, and carefully open them; which I did, and found each full of *farina*, which ſeemed to lie very regular. This determined me to take notice of the courſe of the *farina* in each flower, and I obſerved the following particulars:

A letter from R. Badcock, *Eſq; to Mr* H. Baker, *F. R. S. containing ſome microſcopical obſervations on the* Farina Fœcundans *of the* Holyoak *and the* Paſſion-Flower. N°. 470. p. 150. Mar. &c. 1746. *dated* Kenſington Nov. 6, 1745. *Read* April 13. 1746.

Aug. 24. I took notice of a flower juſt going to blow, and the *petala* appeared; the *farina* was then juſt burſt from it's *apices*. The time of theſe burſting is as ſoon as ever the *petala* blow out enough to be affected by the ſun.

25. The flower opened more, and the *farina* appeared ſo thick on the outſide of the *apices*, that they ſeemed quite covered from ſight, without a very narrow inſpection.

26. The *farina* began to decreaſe viſibly, and continued to do till the

27. When I perceived ſome red curled *ſtamina*, without any *apices*, puſhing themſelves out at the top through the others. Theſe were, within their bend, thick ſet with a kind of hairs *, and in their paſſage took a good quantity of *farina* with them, which remained a day longer than that which was contained in the *apices*. I could not obſerve the *farina* to fall on any particular part of the flower, but ſeemed rather to be diſperſed. When theſe red *ſtamina* appear, the *farina* is going, and the *apices*, which contained it, dead.

The flower was kept till it withered, and the *ſtylus*, &c. cut off; but in neither experiment was there found any difference, after a month's keeping the *farina*, except in the colour, which was deeper.

Cutting off the *ſtylus*, &c. may have a conſiderable effect upon the ſeed, but ſeems to have but little on the flower: for, tho' it was cut off as ſoon as poſſible, yet the flower blew out the ſame as if nothing had happened, till about the time that the *farina* might be ſuppoſed to act; then the *petala* began to look black next the *ſtylus*, and dropped off a day ſooner than the regular blowing flower.

Not having an opportunity of purſuing this further, for want of flowers and warm weather, I applied myſelf to the experiments of Mr *Needham* †. I bruſhed off ſome dry *farina*, and, putting *Thames*-water to it, found it would not burſt, under the ſpace of 7 or 8 minutes, and not till they are ſoaked in the liquid: for, at the time of acting, they

* I cannot obſerve any thing in this flower, unleſs it be theſe, that deſerve the name of *Papillæ*: tho' the firſt magnifier could ſhew nothing ſatisfactory as to this point.

† New *Microſcopical* Diſcoveries, &c. *Lond.* 1745. 8vo. p. 74.

 ſeldom

ſeldom or ever lie one upon another, but float off, till they are clear of all incumbrances. But I obſerved one particular, which ſeems entirely to have eſcaped Mr *Needham*, which was, That, on the application of water, they inſtantly emit a pellucid kind of matter (much thinner than that at burſting) thro' their capillary prickles, with which they are thick ſet. Upon the application of *Briſtol* water they are found to burſt much ſooner, and with leſs emiſſion. In vinegar they ſcarce ever burſt; at leaſt, if they attempt it, are inſtantly ſtopped by the ſharpneſs of the liquor. I dont find vinegar to have any other effect than this.

In making my experiments on a freſh-blown holly-hock, I obſerved a *luſus naturæ* of two globules quite ſmooth and ſhining (contrary to their nature, which is rough): one of theſe acted very ſoon, the other not at all. The whole *farina* ſeem to me to have a ſtrong ſuction; for I was obliged, in the ſpace of 10 minutes, to apply water 3 times, in order for them to have enough to act in; and I obſerve, that they burſt with a greater force, and throw out a much larger pulp, when thrown into a depth of water.

Tho' I have been often obliged to ſupply them with water, yet I find the greateſt number always act with the firſt water. I have often ſeen a globule, tho' it has been burſt on one ſide before, yet has burſt a ſecond time on the oppoſite. Which ſeems to me as if the firſt aperture was inſtantly cloſed, ſo as not to emit again: for I have made it an obſervation, that tho' the pulp is never thrown out at the ſame place a ſecond time, yet the globule, before it has done acting, ſhall have had ſo many burſts, as to look like a picture of a bomb-ſhell, with it's various diſcharges before the ſeparation of it's parts.

The paſſion flower I look upon to be the fitteſt flower for experiments on the *farina*, of any. Firſt, as it is large, and long in bloom: ſecondly, as the flower by it's nature preſerves itſelf and it's *farina* from injury: for, no ſooner is the ſun off of the flower, but it gradually cloſes up as the ſun declines, till the *petala* are ſo cloſe, as not to admit any but very violent ſhowers. This, with the diſpoſition of the *farina*, which is on the inſide of it's *apex*, when the flower is cloſed, likewiſe preſerves it from wind. Add to this the infinite quantity of *farina*, which may be taken off (from the largeneſs of it's *apex*) without any force, damage to the flower, or itſelf. To this likewiſe we may add, that, after a night's keeping gathered, the *farina* has the ſame effect and action in the morning, as it had when freſh-gathered: which quality no other *farina* has. *See* Needham, *page* 77.

The *farina* of the paſſion-flower appears (by Mr *Cuff*'s double reflecting microſcope) Mag^r. 6, 5, 4, to be a ſmooth round globule, of a
Fig 113. pretty full yellow, like the appearance *Fig.* 113. which we ſuppoſe the
Fig 114. *area* of the microſcope. Theſe globules, on being more magnified, are found to have ſome 3 circles (as *Fig.* 114.) others two, others none. Among theſe I have found a conſiderable number quite white, as attempted to be ſhewn in *Fig.* 113.; but I never obſerved theſe act. When the globules,

bules, *Fig.* 114. come to be magnified with the firſt or ſecond magnifier, they appear indented, exactly like *Fig.* 115. All the coloured Fig. 115.
ones, tho' differently marked, yet all act alike. I obſerve that theſe act in a much leſs ſpace of time than thoſe of the hollyhock, which are ten minutes, though freſh; whereas theſe act inſtantly, tho' kept for 24 hours *: neither have theſe any ſuction or convulſive motion; acting entirely ſtill, and in the firſt water. Attempting to apply them to the opake microſcope after their action, they ſtuck round the point like wet ſkins: but one thing I obſerve, that they burſt but once, throwing out all their pellucid matter, which is yellow, at the firſt diſcharge. They act no otherwiſe in oil, but by emitting a matter much thinner than that at burſting: but, having lain in oil for a minute, and put from thence into water, they act inſtantly, and with a ſeeming additional force. Being put into malt-ſpirits, they exhibit a very agreeable appeararance: all thoſe which emit, as in oil, lie dead and ſtill; but thoſe which neither burſt nor emit, are thrown into ſo violent an agitation, that they appear like animalcules; ſometimes joining ten or a dozen together; on a ſudden, an imperceptible force ſhall throw a globule, ſometimes (two or three) three parts over the *area* of the microſcope; often two globules ſhall be whirled round with incredible ſwiftneſs, for the ſpace of near a minute, then ſeparated by the ſame imperceptible ſwiftneſs, fly each a different way. They will act thus, till the liquor may be ſuppoſed to dry up, when ſupplying them with liquor, will regain their motion; and though you put liquor often to them, yet every time will give them that ſwiftneſs. Upon applying the magnifier, N°. 2. I find it is the white unacting globules that do thus, and imagine that they riſe with that ſpirit which evaporates; and their not being volatile occaſions them to ſtop at top, and continue this motion as long as the liquid has any evaporation; for I obſerve, after a certain time, they lie like the others which have acted. In this liquid they burſt, in ſuch a manner, as that the places from whence they burſt are perceptible (*See Fig.* 117.) and the pieces broke off very plain. The way I obtained a ſight of this, was to let the globules dry after their action, on the glaſs. Some burſt ſo fierce as to break off a piece, as *Fig.* 117.; others can ſcarce be ſeen to have any alteration: yet no Fig. 117.
magnifier will go ſo far as to ſhew the matter thrown from them, any otherwiſe than as a yellowiſh water.

Upon applying *aqua fortis* to this *farina*, the ſhape and marks are inſtantly changed to thoſe marked in *Fig.* 116.; whereas, on the holly- Fig. 116.
hock, it has no other effect than burning up their capillary prickles.

The *lilium flore reflexo* of Mr *Needham* I have never ſeen; thoſe kind of flowers having been a long time out of bloom; but, as to this of the paſſion-flower, what is placed on the top of the piſtil, the deepeſt mag-

* I have ſince had a flower lay in a window from *Friday* morning till *Monday*, and the *farina* has acted very briſkly.

nifier will not ſhew perfect: and though I have endeavoured all poſſible means, could never obtain any thing ſatisfactory; ſo that I muſt freely own either my misfortune or my ignorance. On the piſtil of the ſtock gilliflower there are very plain to be ſeen ſome kind of capillary tubes on it's top; but then they appear to ſtand thus , without any aperture, as deſcribed by Mr *Needham*. Whether theſe may have their pores, or not, I am not able to ſay. I ſhall continue making obſervations on this part of the flowers which may fall under my inſpection, and hope ſoon to be ſatisfied in this point: but, as to his opinion of the action of the *farina*, I cannot, at preſent, grant it any other active power than ſuction; for had there been any inward mechaniſm, the various experiments I have tried muſt have ſhewn it in ſome ſhape or other; whereas thoſe who have the greateſt motion before action, I can only obſerve to ſwell, and look larger, on the almoſt immediate application of water.

Further obſervations and experiments on the Paſſion-Flower, *and it's* Farina; *by the ſame.* Ibid. p. 166. *Read* April 24. 1746.

2. In my laſt account I concluded with obſerving, that what was in the top of the piſtil, was ſo far from making me believe it *papillæ*, or any other paſſage for the action of the *farina*, that I deſcribed it to ſtand thus ; and queried if there might not be pores on it's top, as granting the action of the *farina* to have it's effect and conſequence, as deſcribed by Mr *Needham*, *p.* 80 as there was no poſſibility of it's ever being in the nature of the *lilium flore reflexo*, deſcribed by him, by it's *papillæ* being of uſe in the manner and figure deſcribed *Plate* 5. *Fig.* 2. of his book. I have ſince taken all poſſible methods to ſatisfy myſelf, and ſhall communicate the following experiments, being the moſt material, without any apology: as, upon a conviction of an overſight or miſtake, I am very ready to acknowledge my error.

After the *calix*, *petala*, &c. are ſtripped off, the firſt thing the flower preſents to view are a double row of purple threads: theſe threads appear thus; *Fig.* 118. on which we may plainly perceive a ſort of capillary tubes (or whatever you will call them) ſtanding as I before obſerved. Here we may be at a loſs for a paſſage for the acting matter of the *farina*; we muſt therefore look further. Upon cutting theſe threads longitudinally, they appear in many places as this before us, and are often pretty full. The occaſion of theſe appearances, (*Fig.* 119.) I own I am not Botaniſt enough to ſolve, nor will the firſt magnifier give me ſatisfaction. At the bottom of theſe, ſet round the ſtem, is a ſingle row of ſmall threads, not exceeding $\frac{1}{4}$ an inch: theſe appear to have much broader heads than the long purple threads around them; and being ſo well ſecured and fortified from injury, I imagine to be of great uſe and conſequence to the flower; yet they appear ſet in the ſame manner, though the tubes do not riſe ſo high. I am inclined to believe theſe may be deſigned in this flower as *papillæ*; 1ſt, As they are ſo well fortified from all injury from without; 2dly, As the *farina*, when the flower blows, and cloſes at night, is turned inwards; which time, I am likewiſe

Fig 118.

Fig. 119.

likewise inclined to believe, is the time of action *, at least in this flower; for, after a hot day, why may not the evening dews penetrate, and the *farina*, having at that time a strong suction, from the drought, occasion it to act? But I have not made the observation at which time the *farina* acts most, morning or evening; which I fancy would satisfy. 3dly, From this being the only part of the flower which appears with a hollow or indenting on it's top; by which the action of the *farina* (thus lying inwards) may fall down, and settle in this hollow, as a drop of water would do: for I observe there are no obstructions to such a supposition, in the structure of the flower.

We will go now to the top of the flower, where are 3 *stamina* placed on the *uterus*: these are set in a manner described before with tubes; but, on making a longitudinal section, I cannot find them carried on in any shape.

We come next to the *uterus*; here I cannot observe any tubes at all; nor is there any appearance to me remarkable, till we come to the bottom of the *stylus*; and then, by degrees, from a smaller to a greater it rises, till the appearance becomes thus; (*See Fig.* 120.) Fig. 120.

On examination, I find the 5 appearances to answer the 5 *stamina* on which the *apices* are set; and from this appearance, growing nearer and nearer to each other by degrees, they join at last all in one in the stalk of the flower.

Concerning the Farina Fœcundans *of the* Yew-Tree; *by the same.* N°. 480. p. 189. *Read* May 15. 1746.

3. *March* 3. I observed a yew-tree in blossom, but having at that time only the bud (*Fig.* 121.) which, taken out of it's case, may be seen with it's *umbilicus a*, in *Fig.* 122. That every flower has it's *farina*, has been justly advanced; and as it was my design to observe that, I took a branch into the house; but, making but a slight observation at that time, I laid it on half a sheet of brown paper in a warm drawer; and, to my great surprize, coming to examine it in a few hours after, I found the whole number of the buds blown out into full flower, and such a quantity of *farina* on the paper, that it seemed more like a paper of brimstone than any thing else. I then no longer neglected a thorough examination, which I began and completed in the following manner: Fig. 121. Fig. 122.

Fig. 123. is a separate view of it's partitions, before it is near blowing. Every five of these go to a flower, and divide properly for blowing, some small space before they burst: there are sometimes six to a flower, but it is seldom: they open at the bottom *a a*, and immediately, letting drop the *farina*, turn themselves up; so the top, which now appears the head, will, when blown, be the centre of the flower. (See an explanation of this in *Fig.* 124. where the whole division makes the flower and the strokes the division of the *petala*.) *a* is the stem of the Fig. 123. Fig. 124.

* The two following experiments have given me grounds for this; 1st, That the *farina* I observe, is always damp in the morning; 2dly, On examining it after a frosty night, scarce one burst. I at this time made an observation, which, I believe, has as yet escaped every one, That the intense cold has such an effect on the globules as to throw many into the same shape as *aqua fortis* will.

flower:

flower: now the bottom opening difcovers this ftem; and the eafy tranfition of turning very near infide-out, makes a complete flower,
Fig. 125. *Fig.* 125. the fhape of which is feldom exact in any two; tho' there are
Fig. 126. near 12 flowers formed by the bud. (See the back part of one, *Fig.* 126. in which thofe ridges fhew it's divifion on the back.) We come now to
Fig. 127. the *farina*, *Fig.* 127. which matches the reft of the flower, as to irregularity; there fcarce being two alike; and, when viewed opakely, has a great refemblance to the fmall pieces we obferve in a paper of gum *arabick*; 'tis opakely of a clear white; but, when laid on paper in a quantity, appears like flower of brimftone, only paler. It's action is as va-
Fig. 128. rious as it's fhape. (See feveral forms, *Fig.* 128.) It feems to be only fixed in one particular, which is, that tho' there are ever fo many different fhapes, when dry, water once put to them makes them all round, before any action begins: a proof to me, that there is a fuction. In fhort, the whole procefs of this is fo various and entertaining, that I never met with any thing hitherto to be compared to it.

My hafte had made me almoft mifs the moft material point of all; which is, that this flower has neither *apices*, *ftamina*, nor *ftylus*; which is the reafon why fo much *farina* is fhed. I have not yet examined it as to impregnation, but fhall go on that the firft opportunity.

A letter from Mr John Hill, *Apothecary, to the* Pref. *concerning the manner of the feeding of* Moffes; *and in particular of the* Hypnum terreftre, trichoides, luteo-virens, vulgare, majus, capitulis erectis. Raii Syn. *Ed.* 3. *p.* 84. N°. 478. p. 60. Jan. & Feb. 1746. *Read* Feb. 13. 1745-6. *with alterations.*

XIV. The many late difcoveries of the feeds of vegetables (formerly fuppofed to produce none) have opened a way to an extenfive fcene of knowledge; and led to a feries of obfervations, in which there will be found an almoft inexhauftible ftore of delight and admiration. The moffes, in particular, one of the moft beautiful parts of the vegetable creation, will afford the curious obferver more matter of entertainment than perhaps any other clafs of it. A fpecimen of which I do myfelf the honour of communicating to you in this letter.

The particular obfervations it contains, though new and furprizing, you will allow unqueftionable in fact and certainty, when I add, that they are what I have not only often repeated myfelf, but have alfo fhewn to Mr *Baker*, Dr *Parfons*, and Mr *Needham*, Gentlemen of too nice eyes, and too converfant in obfervations of this kind, not to have difcovered miftakes, if there had been any; and who all agree, that no difcovery by the microfcope was ever clearer.

The manner of feeding of the moffes in general is a thing perhaps as little underftood, as any part of the vegetable fyftem: what I have to offer here is the explaining and defcribing it clearly, in one fpecies at leaft, from a number of obfervations made on it in it's different ftates, and at different feafons of the year.

The thorough knowledge of the operations of nature in the feeding of one of thefe little plants, may, I hope, be a fair ftep to the difcovering it in them all. And the almoft infinite variety of not only *fpecies*, but *genus's*, in this clafs of vegetables, feems to promife the inquirer a vaft variety of new difcoveries.

I fhall

I ſhall not trouble you with an account of the erroneous opinions of others on this ſubject; you will find, by this account, that thoſe who have been uſed to judge well in other parts of Botany, have been altogether miſtaken here: and even the accurate Mr *Hook*, who of all others has come neareſt a diſcovery of the truth, and who actually ſaw ſome part of the organization of theſe little bodies, was ſo far from making the leaſt gueſs at the nature and uſes of what he ſaw, that he even miſtook the ſtructure of it.

The particular ſpecies of moſs, whoſe head I here ſend you a deſcription of, is the *Hypnum terreſtre, trichoïdes, luteo-virens, vulgare, majus, capitulis erectis*. Raii Syn. *ed.* 3. *p.* 84. *Hypnum vulgare, ſericeum, recurvum, capſulis erectis cuſpidatis*, Dill. Hiſt. Muſc. 323.

The head of this moſs appears to the naked eye (as at *Fig.* 129.) of a pale-brown colour, and ſmooth ſurface, and is in part covered with a membranaceous *calyptra*, reſembling in ſhape an extinguiſher, or a funnel inverted. When this *calyptra* is taken off, and the head placed before the microſcope, the ſurface of it is ſeen to be ridged with longitudinal *ſtriæ*, the baſis of the head is of a dark orange-colour, and more opake than the reſt; and the top is bounded by an orange-coloured ring, ſwelling out ſomething beyond the ſurface of the contiguous parts of the head. A cloſe obſervation and good glaſſes have informed me, that in this little head there are not wanting the parts eſſential to the fructification of what are commonly called the more perfect plants. This ring is truly a monophyllous undulated *calyx*; and within it ariſe 16 pyramidal fimbriated *ſtamina:* Theſe are of a pale-greeniſh colour, and are loaded with a white oval *farina:* the *ſtamina* all bend toward each other from their baſes, and almoſt meet in a point at their tops. This is their appearance when the head is nearly ripe, and is what is expreſſed at *Fig.* 130. And immediately under the arch formed by theſe *ſtamina* is placed a ſlender, cylindric, hollow *piſtillum*, through which the *farina* makes it's way, and is diſperſed among the ſeeds in the head. The external membrane of the head is a continuation of the outer covering of the ſtalk, and is ſtrengthened at it's baſis by four or five ribs, which ſoon loſe themſelves in the *ſtriæ*. A longitudinal ſection of the head ſhews, that the membrane before-mentioned incloſes a ſeed-veſſel ſo large as to fill it every way: in moſt places they touch; but wherever they do not, a number of very ſlender, white, and tranſparent fibres ſhew themſelves, which join them together. This ſeed-veſſel is filled with perfect and very beautiful ſeeds; they are round, tranſparent while unripe, but afterwards opake, and of a very beautiful green; which colour they retain even when dried.

Fig. 129.

Fig. 130.

The number of ſeeds in one of theſe heads is aſtoniſhingly great: I have many times attempted to count them, in ſuch as were full, and out of which few or none had been diſlodged by the cutting; and as the accounts, at different times, and in various heads, have not much differed, I ſhall venture to inſert a gueſs from them. It will eaſily be conceived,

conceived, that in feeds fo minute as well as numerous, this muft be a very difficult tafk; and indeed to count every feparate feed, I believe would be not much lefs than impoffible: the method therefore by which I make the calculation is this. I count in $\frac{1}{8}$ part of the circumference of $\frac{1}{2}$ the head 9 feeds, $9 \times 8 = 72$; there are therefore 72 feeds in a line, which reaches round the circumference of $\frac{1}{2}$ of the head. I judge the length of this half to be to it's circumference as 3 to 2, or thereabout: therefore, in one longitudinal line in it, there muft be 96 feeds; the whole quantity in $\frac{1}{2}$ the head therefore is $72 \times 96 = 6912$; and, doubling that for the equal number of feeds of the other half, there appear to be in one head $2 \times 6912 = 13824$ feeds.

Fig. 131. *Fig.* 131. fhews a longitudinal fection of the head with the feeds, the *ftamina*, and the joining of the capfule with the external membrane of the head.

The *ftamina*, examined alone, afford a moft pleafing fight; they are compofed of a white tranfparent fubftance, of a pyramidal figure, every-where covered with a pale-greenifh cruft; which is the receptacle of a vaft quantity of an oval *farina*, fo extremely minute, as to be vifible only with the moft powerful magnifiers in the double microfcope.

The outer membrane of the head becomes feparable from the capfule when perfectly ripe and dry; and then, viewed in the double microfcope, fhews a reticular texture, not vifible in it before.

When this head is firft produced from the plant, the *ftamina* are very flender, and ftand erect; the head is fcarce any thicker than the ftalk, and the *calyptra* covers the whole, to fhield the tender fubftance of the *farina* from external injuries. As the *farina* afterwards fwells in the *ftamina*, the feeds alfo in the head increafe in bulk, and become vifible, and are then tranfparent; but when it is perfectly ripe, the *calyptra* falls off, and the wind diflodging the *farina* at times, as it ripens fome fooner, fome later, it makes it's way through the *piftillum* into the head, and the feeds then become much larger and opake; to favour the falling of the *farina* into the *piftillum*, the *ftamina*, as they ripen, are, by the increafe of thicknefs in the head, thrown farther and farther from each other at their bafes, but bend inward at the points, fo as to form a kind of arch over the opening of it.

The annual product of thefe moft minute feeds is aftonifhing: an ingenious gentleman has given an account of the wonderful increafe of the mallow; one of which he found to yield in one year 200,000. But this is much inferior to thofe of the little plant before us; for, allowing to a root of this 8 branches, and to each branch 6 heads (which any one, who will obferve it in a thriving fituation, will find a very moderate computation), the produce of this is $6 \times 13824 = 82944$; and $8 \times 82944 = 663552$ feeds, the annual produce of one feed; 13824 of which are contained in a head, whofe length is but $\frac{1}{9}$ of an inch, and it's diameter but $\frac{1}{23}$ of an inch, and whofe weight is but the thirteenth part of a grain.

Fig. 129.

Fig. 129. ſhews the head of this moſs in it's natural dimenſions, with and without the *calyptra*. *Explanation of the figures.*

Fig. 130. The ſame viewed through a powerful magnifier, without it's *calyptra*.

Fig. 131. A longitudinal ſection of the ſame.

Fig. 132. *Stamina* taken off from the head, and viewed by a more powerful magnifier.

Fig. 133. A piece of the outer membrane of the head, ſhewing it's reticular texture.

XV. About this time twelve-month, I found, accidentally, a paper of melon ſeeds that I had laid by, with the date of the year 1710 upon it. I ſowed ſome of them, not with any great hopes of their coming up; but, to my great ſurprize, I had a fine number of plants from them, which all proſpered very well, till they had put out 4 leaves, when they were all loſt by an accident. This I have mentioned to you, becauſe Mr *Triewald* has given an account of ſome old melon-ſeeds that produced fruit, though they exceeded mine 10 years in age: however mine may be a confirmation of their long retention of their vegetative quality; which I ſuppoſe may be aſcribed to the oilyneſs of the ſeed, and the hardneſs of it's outward coat.

Concerning the vegetation of Melon *Seeds 33 years old; by the late* Roger Gale, *Eſq;* *F. R. S. to Mr* P. Collinſon, *F. R. S.* No. 475. p. 265. Jan. &c. 1745. *dated* Scruton *in* Yorkſhire, Jan. 14. 1743-4. *Read* Jan. 24. 1744-5.

XVI. I was poſſeſſed of ſeveral good microſcopes, both ſimple and compound. The compound microſcope was that portable reflecting one without a micrometer, of Mr *Benj. Martin*'s invention, which I brought with me from *England*. Firſt of all I expoſed to the luminous *focus* of this inſtrument a very ſmall, pure, thin bit of the bark of this root ſcraped off very carefully. It's external ſurface appeared almoſt opake, very uneven, of the colour of dry earth, as it appears to the naked eye; but the inner ſurface viewed in the ſame manner diſcovered a confuſed heap of very ſhort and thin little maſſes, every where interrupted, and moſtly acute, and angular, in the ſmalleſt viſible atoms, chiefly purple, almoſt tranſparent, like ſome reſin, but connected by interſperſed threads of a like figure, variegated, but chiefly whitiſh; but at the edge there appeared real prickles; and the whole contexture ſeemed like that which in the animal œconomy Anatomiſts deſcribe in the fat, when they ſay this oil is lodged in cells, which cohere with that lanuginous ſoft cellular ſubſtance. Thus alſo this purple ſubſtance in form of reſin ſeemed to be mixt with the whitiſh threads. And thus, in many repeated obſervations, both by ſimple and compound microſcopes, this bark always appeared to me, with this only difference, that the reflecting one repreſented every particular more diſtinctly than the reſt.

Microſcopical obſervations on the root of Ipecacuanha; *by* P. Fred. Gmelin, *Med. Lic.* Wurtenberg. No. 476. p. 382. Apr.&c. 1745. *Preſented* May 9. 1745.

But I had not yet examined a nerve of this root, which was very brittle and deprived of it's bark, and made the greateſt bulk of the root, tho'

tho' it ſeemed to yield much to the bark in weight, as being very dry, by the compound microſcope: but to the naked eye it ſeemed to be ſprinkled with dark purple ſpots. I was deſirous to know what appearance theſe would have under the *focus* of a microſcope; and therefore viewed that little bit, which had the ſpots. The nerve itſelf appeared equally convex, and cylindrical, conſiſting of uninterrupted longitudinal fibres, cloſely applied to each other, very thin, and very white: but the ſpots, as they appeared to the naked eye, when ſubmitted to the microſcope, were found to be tranſparent dark-red little maſſes raiſed above the convexity of the cylinder: but I was in doubt whether I ſhould believe that theſe little maſſes did not riſe above the nerve, or determine that they inhered in the nerve itſelf, and belonged to it's ſubſtance. However on the application of ſome ſimple microſcopes, I was convinced that they were raiſed above the nerve, and belonged not ſo much to the nerve as to the bark.

From theſe ſeveral microſcopical obſervations I conclude *à priori*, that the bark muſt be more efficacious than the whole root, eſpecially if we call in that indubitable and ſo often confirmed practical experience, that the purging principle of vegetables reſides chiefly in their reſinous part. Beſides, from my obſervation, that the appearance of the whole bark by the microſcope, which ſhews it to be compoſed of many ſmall, ſharp, ſhort *ſpicula*, that *hypotheſis* will alſo be explained, which mechanically explains the force of purging and vomiting medicines, by ſuppoſing ſuch *ſpicula*, like ſo many little wedges (or perhaps prickles, ſuch as appear even to the naked eye in nettles, which ſenſibly irritate a living human body) which by the periſtaltic motion of the ſtomach, tho' mediately applied to it's muſcular coat, and being frequently toſſed about by this perpetual motion, ſtimulate the moving fibres, and excite them to a ſtronger contraction than ordinary: though at the ſame time I would not yet aſſert this *hypotheſis*, how probable ſoever it may appear from theſe microſcopical obſervations.

The Bark prevents catching cold; in a letter from the Rev. Dr Sam. Salter *to Mr* Arderon. N°. 478. p. 3. Jan. & Feb. 1746. *Read* Jan. 9. 1745-6.

XVII. Dr *Salter*, one of the prebends of *Norwich* cathedral, writes, in a letter to Mr *Arderon*, that he thought it might be of ſervice to take notice of the following effect of the *Jeſuit's Bark:* the doctor uſed to be ſubject eaſily to take cold, and, in conſequence thereof, to be ſubject to have a ſore throat to a very great degree; but the laſt time, above 15 years ago, after his recovery, he was adviſed, by Sir *Benj. Wrench*, to take ℥ii of the *Bark* (after due preparation by bleeding, or purging, or both, when he was altogether without complaint) every ſpring and fall. This, he ſaid, would more effectually guard him againſt taking cold; which he has found ſo far to anſwer, that he is now able to go 500 miles with leſs hazard of cold, than he could go 20 before; and he has never had what he can ſtrictly call a ſore throat ſince.

XVIII.

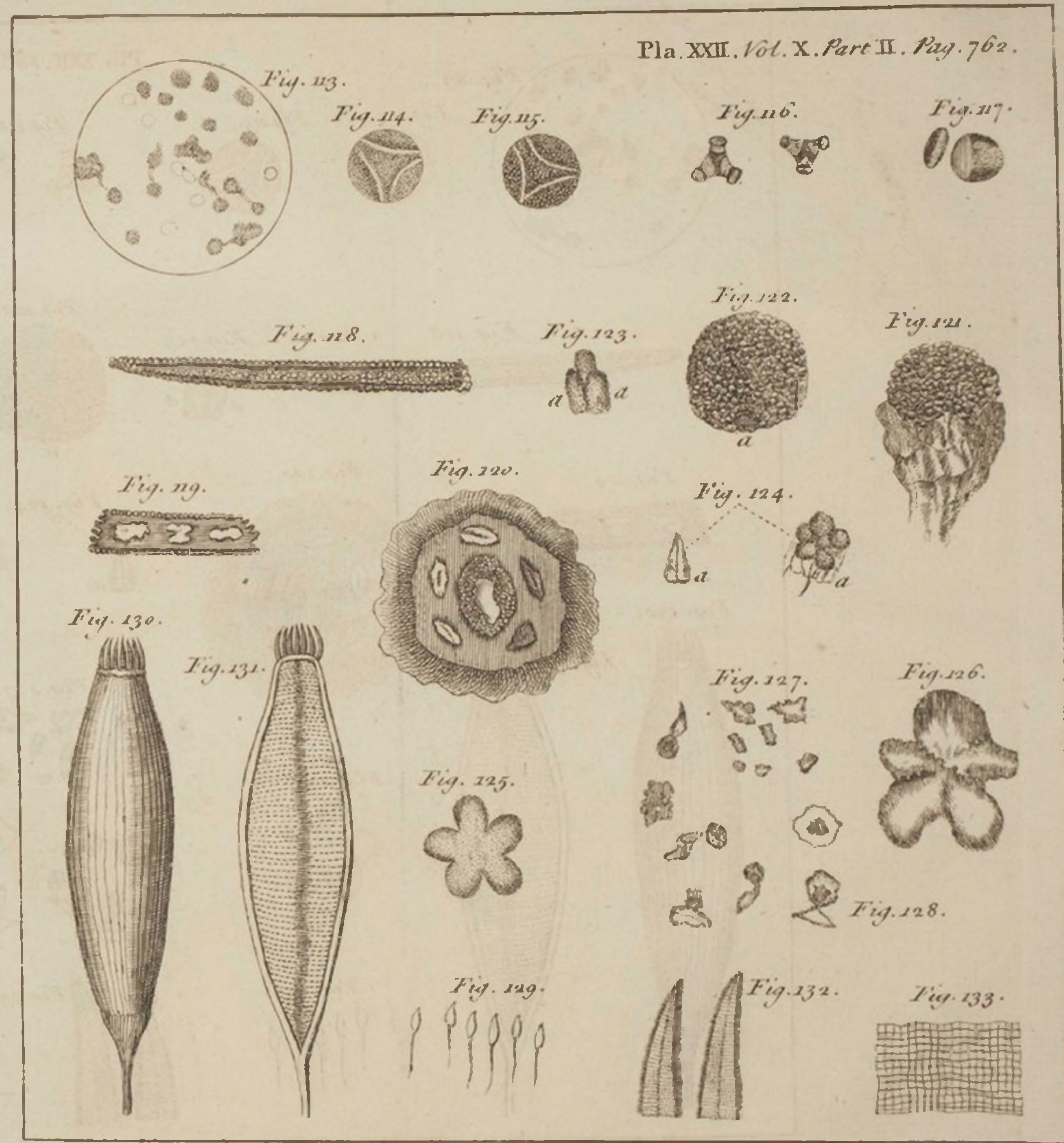
Pla. XXII. Vol. X. Part II. Pag. 762.
Fig. 113.
Fig. 114.
Fig. 115.
Fig. 116.
Fig. 117.
Fig. 118.
Fig. 123.
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Fig. 122.
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Fig. 121.
Fig. 119.
Fig. 120.
Fig. 124.
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Fig. 130.
Fig. 131.
Fig. 127.
Fig. 126.
Fig. 125.
Fig. 128.
Fig. 129.
Fig. 132.
Fig. 133.

XVIII. Notwithstanding the number of instances, which occur among writers, concerning the poisonous quality of our common hemlock, or *cicuta major C. B*; such as, that of *Cardanus* mentioning a man killed by a cake, wherein this plant was an ingredient; that of *Brassavola*, who assures us, that it is mortal not to men only, but to geese and swine; as well as those of *Matthiolus*, *Scaliger*, *Kircher*, *Boccone*, and others; yet the fatality of it's poison, when growing in this kingdom, has been doubted by many; inasmuch as that faithful collector the late Mr *Ray* mentions, *Syn. Ed.* 2. *p.* 326. that not only his friend Mr *Petiver* eat half an ounce of the root of this plant, but that Mr *Henly*, a friend of Mr *Petiver*'s, in his presence, eat, without any inconvenience, 3 or 4 ounces of the same root. From hence it has been thought, either that the root has effects different from the stalks and leaves, or, that difference of climate varies the degree of the violence of the poison

Concerning some persons being poisoned by eating boiled Hemlock; by Mr William Watson, Apothecary, F. R. S. N°. 473. p. 18. May &c. 1744. *Read* May 10. 1744.

An observation indeed of the same kind occurs in the *German Ephemerides*. *Linnæus*, *Hort. Cliff.* makes also some doubt concerning the malignity of this plant; and, in naming it, has kept to the old appellation of *Theophrastus* and *Dioscorides*, *Conium*; and has transferred that of *Cicuta*, to the *Cicuta aquatica* of *Gesner*, and of *Wepfer*. Besides, many of the accidents, said to have proceeded from *cicuta* or hemlock, have been occasioned by different plants; some of the accidents, probably, from the common one, but many more from the *cicuta aquatica* before-mentioned, and from the *Oenanthe succo viroso*, *cicutæ facie*, *Lob*. This confusion appears manifestly in several authors, and some of them of the greatest credit. Which of these plants, or whether any of them, was the *Athenian* poison, nobody has determined.

Although the eating of the roots, as abovementioned, was attended with no bad consequences, a late melancholy accident has been sufficiently convincing of the poisonous quality of the leaves of the *cicuta major*.

On *Sunday*, *May* 6. 1744. two of the *Dutch* soldiers lately arrived, who were quartered at *Waltham-Abbey* in *Essex*, collected, in the fields adjoining, a quantity of herbs, sufficient for themselves and two others for dinner, when boiled with bacon. These herbs were accordingly dressed, and the poor men first eat of the broth with bread, and afterwards eat the herbs with the bacon. In a short time after, they were all seized with violent *vertigo's*; they soon after were *comatose*; and two of them grew convulsed, and died in about 3 hours.

The people of the town being exceedingly alarmed at this accident, Dr *Barrowby*, jun. being there, immediately went, and ordered the other two, at that time almost dead, large quantities of oil; by which means they threw up most of what they had eaten, and afterwards grew better. In all of them the effects were the same as those from a large dose of *opium*.

 The

The next day, being at the place, I ſaw one of theſe men much recovered, and only complaining of a heavineſs in his head; but the other was ſo well, as to be gone to perform exerciſe with the other ſoldiers. There was a fifth ſoldier, whom I ſaw, who told me, he eat ſome of the bread out of the broth, but felt ſcarce any inconvenience therefrom. It ſo happened, that the two men, who gathered the herbs, were both killed.

As I went down to the place to ſatisfy myſelf in this matter, a *Dutch* officer went with me very courteouſly to an inn, where there were two other ſoldiers, who had ſeen and knew the herbs which had been eaten: he was ſo kind alſo as to attend me with theſe ſoldiers into the fields, to ſhew me the plants growing. They firſt gathered me the *cicutaria vulgaris J. B.* or cow-weed; then, the *myrrhis ſylveſtris ſeminibus aſperis C. B.* or ſmall hemlock-chervil. They then gave me ſome *cicuta major*, and, ſmelling it, immediately ſaid, That this was the herb that killed their comrades; which I then had no reaſon to doubt of; as of the two former plants, the firſt grows almoſt under every hedge, and is eaten by the cows, and the other is frequently given to tame rabbets for food; whereas cattle conſtantly refuſe to eat hemlock.

Before I was thus ſatisfied, I imagined this accident to have proceeded rather from *Lobel*'s *Oenanthe*; thinking, that as that plant grows near the ſides of rivers, theſe ſoldiers might have gathered it by the river *Lee*, which runs by the town, and eaten it for ſmallage, to which it has ſome reſemblance.

It is now known, that the *cicuta major*, the *cicuta aquatica*, and the *oenanthe* of *Lobel*, are certain poiſons; but there are two others of the ſame claſs, growing common in *England*, and not much unlike theſe in ſmell and other circumſtances, vehemently to be ſuſpected: the one is the *cicutaria tenuifolia* of Mr *Ray*, which grows frequently in waſte places, and in gardens among pot-herbs, of which *De la Champ* gives ſome account of it's malignancy; the other is the *cicutaria paluſtris* of *Lobel* and *Tabernæmontanus*, or *phellandrium* of *Dodonæus*, which grows in muddy ditches and ponds.

I don't remember any hiſtory of the pernicious effects of the *cicuta major* in this kingdom; but as the detecting poiſonous plants is of very great conſequence, I preſume to lay this paper before you.

Critical obſervations concerning the Oenanthe aquat. ſucco viroſo crocante Lob. *by the ſame; occaſioned by an extract of a letter from*

XIX. "Eleven *French* priſoners had the liberty of walking in and "about the town of *Pembroke*: 3 of them, being in the fields a little "before noon, ſound and dug up a large quantity of a plant with it's "roots (which they took to be wild *celeri*) to eat with their bread and "butter for dinner. After waſhing it, while yet in the fields, they all "3 eat, or rather taſted, of the roots.

"As they were entering the town, without any previous notice of "ſickneſs at the ſtomach or diſorder in the head, one of them was "ſeized with convulſions. The other two ran home, and ſent a ſur-

"geon

" geon to him. The surgeon endeavoured first to bleed, and then " vomit him; but those endeavours were fruitless, and he died pre- " sently.

Mr George Howel, *surgeon, at* Haverford west, *to the author, giving an account of the poisonous effects of this plant to some* French Prisoners *at* Pembroke. N°. 480. p. 227. May & June 1746. *Read* June 12. 1746.

" Ignorant yet of the cause of their comrade's death, and of their " own danger, they gave of these roots to the other eight prisoners, " who all eat some of them with their dinner. I cannot learn exactly " how much they eat, they being sent away a little time before your " letter arrived.

" A few minutes after, the remaining two, who gathered the plants, " were seized in the same manner as the first; of which one died: the " other was bled, and a vomit with great difficulty forced down, on " account of his jaws being, as it were, locked together. This ope- " rated, and he recovered; but was some time much affected with a " dizziness in his head, though not sick, or in the least disordered in " his stomach. The other 8, being bled and vomited immediately, " were soon well.

" There were in these men none of those comatose symptoms you " mentioned * to have happened to the *Dutch* soldiers, who were poi- " soned by eating the *cicuta major*.

" As I was not present myself, I send you the best information I was " able to procure. After I had done examining, I ordered some of the " herb and root to be brought me. As you suggested in your letter, " I found it to be the *oenanthe aquatica cicutæ facie* of *Lobel*, which " grows in great plenty all over this country, is called by the inhabi- " tants *five-fingered root*, and is much used by them in cataplasms for " the fellon, or worst kind of whitflow. The *Frenchmen* eat only the " root, and none of the leaves or stalk. — I must beg your pardon for " sending you this imperfect account: had this accident happened at " *Haverford*, you should have had one more exact."

So far Mr *Howell*'s letter.

The poisonous effects of this plant, in the instance beforementioned, exactly square with those mentioned of the same plant, in N°. 238. of the *Phil. Transf.* where 8 young lads, near *Clonmel* in *Ireland* (where this plant is called *tahow*) mistook it's roots for those of *sium aquaticum*, or water-parsnep, and eat plentifully of them. About 4 or 5 hours after, going home, the eldest, almost of man's stature, without the least previous disorder or complaint, fell down backwards, and died convulsed. Four more died in the same manner before morning; not one of them having spoken a word from the moment the venomous particles had attacked the *genus nervosum*. Of the other 3, one ran stark-mad, but came to himself next morning. The hair and nails of another fell off. One of them only escaped without any harm, who ran home above 2 miles, and drank warm milk, which caused a *diaphoresis*. A *Dutchman* likewise was poisoned with the leaves of this plant, boiled in his

* See the preceding article.

pottage;

pottage; which he took for ſmallage, and to which it's leaves have great reſemblance.

Dr *Allen*, in his *Synopſis Medicinæ*, mentions an inſtance of 4 children, who eat of theſe roots. They indeed were in great agonies, before they fell into convulſions In their fits they vomited, which was encouraged by large draughts of oil and warm water; and by other proper care they all did well. He takes notice likewiſe of a pig's dying in convulſions, from eating ſome of theſe roots, which it had grubbed up.

Stalpart van der Wiel, in his obſervations, takes notice of the deadly effects to two perſons, who had eaten theſe roots, miſtaking them for *Macedonian* parſley. Theſe men (like thoſe quoted from Dr *Allen*), ſoon after eating theſe roots, were troubled with violent heats in the throat and ſtomach, attended with a *vertigo*, ſickneſs at the ſtomach, and purging. One of them bled at the noſe; the other was violently convulſed. Both of them died; one in 2 hours, the other in 3. This author has given us 3 figures of the *oenanthe*: the 2 tables of the roots and the leaves are tolerably well executed; but that expreſſing the whole plant is very deficient. It were much to be wiſhed, that all botanical authors had induſtry and ingenuity enough to delineate their own tables, as *Columna* and *Dillenius* have done; which will always heighten the value of their otherwiſe excellent works.

It is very remarkable, that neither the *French* priſoners, who were killed at *Pembroke*, nor thoſe before cited in the *Phil. Tranſ.* felt any heat or diſorder in their ſtomach, before the attack of the convulſive paroxyſms: whereas theſe mentioned by Dr *Allen*, and *Stalpart van der Wiel*, were in great agonies, from the violent heat in their ſtomach and throat, before they were attacked by convulſions.

The ſame variety of ſymptoms we meet with in *Wepfer*, with regard to thoſe people who were poiſoned by the *cicuta aquatica*; where ſome of them, who had eaten the roots of this plant at the ſame time, ſtood and aſſiſted their friends, till they died of convulſions, without feeling themſelves any wiſe diſordered; and afterwards, in their turns, died in the ſame manner. Others were violently affected by it, as ſoon almoſt as they had eaten it. Confer *Wepfer*'s Hiſtory with the * *German Ephemerides*. *Linnæus* mentions, in the † *Flora Lapponica*, the great ſlaughter, and miſerable manner, in which the horned cattle died, from eating this plant at *Tornæa*. This author alſo, in his *Flora Suecica*, acquaints us (notwithſtanding *Rivinus* and *Mappus* have aſſerted, that the horned cattle not only eat this plant without detriment, but are very fond of it) that three oxen were killed by eating the roots thereof. He was fully convinced that they were the roots of the *cicuta aquatica*; becauſe, ſoon after this accident, the country people brought him ſome of them, deſiring to know to what plant they belonged. He thereupon

* *Ephemerid.* Natur. Curioſor. Dec. 2. Ann. 6. Obſ. 116.
† See *Flor. Lappon.* p. 72.

planted

planted them in the academical garden, and was fully ſatisfied what they were.

Wepfer has confounded his *cicuta aquatica*, in the Hiſtory thereof *, with the poiſonous *oenanthe* of *Lobel*; where he ſays, that *Lobel* has deſcribed the *cicuta aquatica* under the name of *Oenanthe cicutæ facie, ſucco viroſo crocante*; and mentions, that it is not very frequent, but in the northern parts of *England* by the ſides of rivers, and in watery places: he adds, that *Lobel* has not been exact in his deſcription. To which I anſwer, that *Lobel*'s deſcription of the *oenanthe* is very exact, for the time he lived; and it is very evident, that *Wepfer* never ſaw this *oenanthe*; which plant, I believe, is not found in *Germany*. *Wepfer* likewiſe, in the *Ephem. Nat. Curioſ.* † is under the ſame miſtake; and tells you, that *Stalpart van der Wiel* differs from him; and calls the plant, mentioned in his Obſervations, *Oenanthe*, as *Lobel* does: and though *Stalpart* has given figures of the plant accurate enough for a common obſerver to diſtinguiſh the plants by, and tho' nine years lapſed between the publication of his book de *Cicuta* and his Obſervations in the *Ephemerides*, he was ſtill in the ſame error; and believed the *oenanthe* of *Lobel*, and his *cicuta aquatica*, as well as that of *Geſner*, to be the ſame poiſonous plant. The accurate *Hoffman* ‖ alſo, when treating of vegetable poiſons, makes no mention of this difference.

Neither the roots of the *oenanthe* of *Lobel*, nor thoſe of the *cicuta* of *Wepfer*, have any flavour in them diſagreeable enough to deter thoſe, who taſte them, from eating. They both occaſion violent convulſions, and death, if not timely prevented. The intention of cure ſeems in both to be the ſame; *viz.* firſt, by emptying the ſtomach and inteſtines as ſoon as poſſible, and then by cauſing the patient to ſwallow large quantities of oleaginous fluids. But it is to be obſerved, that the cauſing the patient to ſwallow any quantity is attended with great difficulty, after he is attacked by the poiſon; becauſe of the jaws being, as it were, locked together by the violence of the ſpaſm. After the ſtomach is freed from this pernicious vegetable, the ſymptoms have generally diminiſhed by degrees, and the patient recovered.

* *Cicutæ aquat.* hiſtoria & noxæ, p. 15.

† Dec. II. Ann. VI. Obſ. 116.

‖ *Fred. Hoffman.* Medicin. Rational. Syſtematic. *Tom.* II. p. 174. *Edit. in 4to.* "*Ex* "*vegetabilium regno inter præſentiſſima venena referri debeat cicuta vera, napellus ſive* "*aconitum cæruleum, ſolanum furioſum, hyoſcyamus, ac datura.*" If here the epithet *vera* to *cicuta* is underſtood only to point out the poiſonous ſort of hemlock, there are no leſs than three ſpecies of this claſs, which, from their being known certainly to be poiſonous, may lay claim thereto; *viz. Cicuta major C. B. Cicuta aquatica Wepf.* and *Oenanthe cicutæ facie Lob.* But, it is very probable, the two laſt were unknown to the Ancients. The deſcription of *Dioſcorides*, lib. iv. cap. 79, which is the only one to be met with among the *Greek* writers, and that but obſcure, relates, in my opinion to the firſt of theſe. Κώνειον καυλὸν ἀνίησι γονατώδη ὡς μαράθου μέγαν. Φύλλα δὲ νάρθηκι ἐμφερῆ, στενώτερα δὲ καὶ βαρύοσμα· ἐπ' ἄκρων δὲ ἀποφύσεις, καὶ σκιάδια, ἄνθος ὑπόλευκον· σπέρμα ἐμφερὲς ἀνίσῳ, λευκότερον. ῥίζα κοίλη καὶ οὐ βαθεῖα. *Pliny*'s Deſcription, lib. xxv. cap. *ult.* is taken from this of *Dioſcorides*.

Threlkeld,

Threlkeld, in his *Synopsis Plantarum*, mentions, that he has seen great plenty of this *oenanthe* in *Cumberland*, where the country people call it *Dead Tongue*, and use it, when boiled like a pultice, to the galled backs of their horses.

Neither the *German* Botanists *, nor *Haller* in his *Enumeratio Stirpium Helvetiæ*, mention this plant as growing amonst them. I believe, therefore, it is seldom met with but in *Holland*, *England*, and in some parts of *France*; for *Morison* mentions it growing in *Bretagne* near the mouth of the river *Loire*. This plant was communicated to *Matthiolus* by a Professor of Physic at *Padua*. (See *Matth. p.* 628.) *Linnæus*, in the *Flora Suecica*, says, that he received it from a correspondent, who gathered it in *Scania*.

Lobel, and after him *John Bauhin* and others, take notice of this plant's growing in the northern parts of *England*. It grows also in the western and southern parts, by the sides of rivers, large waters, and sometimes by ponds. It grows near *Bath*. Dr *Allen* mentions it growing within 3 miles of *Bridgewater*. It's being produced in *Wales*, is the occasion of this paper. I have seen it very frequently by the sides of the river *Thames*, both above and below *London*. I have found it likewise by the side of a large pond near the road, in the town of *Dulwich*, not far north of the college; likewise by the sides of a large water near the mills, half a mile S. E. of *Dartford* in *Kent*.

Lobel is the first, who has given a small figure and a tolerable description of this *oenanthe*, in his *Adversaria Plantarum* †. He has likewise represented it in the 730th of his *Icones*. This seems likewise to be the plant described by *Valerius Cordus* ‖, under the denomination of *olsenichium*; and, by *Dodonæus*, under that of *apium sylvestre, sive thysselium* §; where the description, place of growth, and form of the roots, agree exactly with the plant under consideration; tho' his figure is execrably bad. This bad figure is copied, and the description translated, by *Gerard* ** in his Herbal, without making any mention of *Dodonæus*. This figure is likewise copied in *Parkinson*'s Theatre of Plants. *John Bauhin*, *Matthiolus*, *Gerard*, *Parkinson*, and *Morison*, have given us figures of this *oenanthe*; but these representations give us scarcely any other idea of the plant, than that it is an umbelliferous one, with roots divided like those of *Asphodel*. Of these, however, *Morison*'s †† is the best; and his description, in his Book *de Umbelliferis*, is very exact and copious. Mr *Ray*'s description is taken from *Lobel*. I have at the bottom of the ‖‖ page recited the various *synonyma*, under which this plant is mentioned amongst authors.

As

* Unless the *olsenichium* of *Valerius Cordus*, and *thysselium* of *Dodonæus*, hereafter mentioned, are other names of the plant in question.

† Adversaria Plant. nov. 326. ‖ *Valer. Cord.* p. 149. § *Dodon.* Pempt. 687.

** *Gerard.* Emac. 1020. †† *Morison* Umbel.

‖‖ OENANTHES, *de qua hic agitur, synomyma.*

Oenanthe tertia *Matthioli*, p. 629.

Oenanthe,

As it appears, from what I have laid down, that the *oenanthe* of *Lobel*, and *cicuta aquatica* of *Wepfer*, have not been ſufficiently diſtinguiſhed by medical writers hitherto, I hope I ſhall ſtand excuſed for making a few obſervations upon this laſt. This, though a plant frequently met with upon the Continent, and very well deſcribed by botanical writers, we ſeldom find near *London*; but it grows in many parts of *England* by the ſides of large ſtanding pools, and near the banks of fens. I am informed by *Robert More*, Eſq; an excellent Botaniſt, and a very worthy Member of this *Society*, that it grows plentifully in many parts of *Shropſhire*. I have lately received it from Dr *Wilmer*, who gathered it by the ſides of the river *Colne*, not far from *Uxbridge*. It is mentioned by Mr *Ray* to grow near *Brereton*-Mere in *Cheſhire*, and in ſeveral other places. You find it mentioned by *Geſner* *; and *Wepfer*, in his Hiſtory thereof, has given us four tables of different parts ſufficiently accurate. It is figured and deſcribed by *John Bauhin* †. *Lobel*'s *Icon*. 208. relates to this plant. *Dodonæus*'s figure, which is not a bad one for the time, is copied both by *Gerard* and *Parkinſon*. *Moriſon* has given us two figures thereof, one in his general Hiſtory, the other in his Book *de Umbelliferis*, though under different names. But the moſt elegant and deſcriptive figures are thoſe of the *Hortus Eyſtettenſis* and *Rivinus*. As the *ſynonyma* of this plant are very many, and very different, I have inſerted them at the bottom of the page ‖.

Though the medical writers have not ſufficiently diſtinguiſhed theſe plants, the Botaniſts have. Theſe indeed, in their turns, have been as

Oenanthe, ſucco viroſo, Cicutæ facie *Lobelii*. *J. B.* III. p. 193.
Oenanthe, Chærophylli foliis. *C. B. P.* 162.
Filipendula, Cicutæ facie. *Ger. Emac.* 1059.
Oenanthe, Cicutæ facie *Lobelii*. *Park.* 894.
Oenanthe maxima, ſucco viroſo, Cicutæ facie. *Moriſ.* Hiſt Sect. 9. Tab. 9.
Oenanthe, foliis omnibus multifidis obtuſis, fere æqualibus. Hort. *Cliff.* 99. *Royen.* 107.
Engliſh wild Parſley. *Ger.* 1020. and Hemlock Dropwort, p. 1059.

* *Geſner* Hort. 254. † *J. B.* III. 175.

‖ CICUTÆ aquaticæ *ſynonyma*.
Cicuta aquatica. *Geſn. Hort* 254. *Wepfer*. *Linnæi* Flor. Lap. 103.
Cicuta maxima quorundam. *Hort. Eyſtet.*
Cicuta. *Linn. Hort. Cliff.* 100.
Cicutaria. *Riv.* Tab. 76.
Sium alterum. *Dod. Pempt.* 579.
Sium alterum Oluſatri facie. *Lob. Ic.* 208. *Ger. Em.* 256. *Raii Hiſt.* 450.
Sium Erucæ folio. *C. B. P.* 154.
Sium majus anguſtifolium. *Park.* 1241.
Sium foliis rugoſis trifidis, ſeu multifidis dentatis. *Mor.* Umb. 63. Tab. 5.
Sium, pinnis laciniatis, pinnulis trifidis, nervo non folioſo. *Haller. Helv.* 436.

It is called in Engliſh *Long-leaved Water-Creſſes* by *Gerard*, p. 256. very injudiciouſly; and *Water Parſneppe* by *Parkinſon*, p. 1241. but much better named by Mr *Ray*, Long-leaved Water Hemlock.

 negligent,

negligent, when writing concerning their uſes (1). So that, notwithſtanding *Lobel* long ago (2) informed the world, that the *oenanthe cicutæ facie*, in it's effects, was very like hemlock; and that thoſe, who had eaten it in ſallads, were almoſt killed by it; this plant occaſioning *vertigo's*, and other violent ſymptoms; yet *Moriſon*, in his Treatiſe of umbelliferous plants, though very exact in the deſcription of the ſpecies of which we are now treating, recommends indiſcriminately the whole *genus*, as being temperately warm and dry; that they are uſeful in cleanſing the urinary paſſages, and in opening obſtructions: he quotes the authority of *Dioſcorides* for giving the powdered roots in wine to cure the dyſury, and to help aſthmatic complaints. What the *oenanthe* of *Dioſcorides* was (3), nobody has determined. He deſcribes it, as having leaves like parſneps, white flowers, a thick ſtalk about a ſpan high, ſeeds like thoſe of arrach, a large root divided into ſeveral round heads, and that it grows in rocky places. A ſhort account of the *oenanthe*, together with it's uſes in medicine, is taken from *Dioſcorides* by *Pliny* (4) the Naturaliſt. What the plant was that *Dioſcorides* here recommends, is uncertain: none of the ſpecies we are acquainted with come near this deſcription; all thoſe, that we know, much exceed his meaſure; none of them have leaves like parſneps, and all grow in watery places. *Ruellius* (5), *Fuckſius* (6), *Tragus* (7), *Dodonæus* (8), and *Matthiolus* (9), have given us the *filipendula* or *dropwort*, for the *oenanthe* of *Dioſcorides*: but this cannot be that plant, becauſe it's ſeeds are not like thoſe of *arrach*; neither has it a large root divided into many heads. *Parkinſon* (10), no great favourer of *Lobel*, ſays, that "*Lobel* only brandeth his *oenanthe cicutæ facie* to be virulent and venomous, from the relations of the north country people, where he ſays it chiefly grows." Mr *Ray*, in his Hiſtory, though he has tranſcribed *Lobel*'s Deſcription, in which it's venomous qualities are taken notice of, leaves this matter to further examination, other Botaniſts being of a different opinion.

The inſtances mentioned in theſe papers are but too ſufficient teſtimonies of the malignant properties of this plant; but Mr *Miller*, a worthy Member of this *Society*, informed me further, that, not many years ſince, a whole family were poiſoned therewith at *Batterſea*. As this plant is frequent ſo near us, and as it's appearance and ſmell are ſo like *ſmallage* and *celeri*, we are greatly intereſted that the knowledge of it be extended as much as poſſible. As I find no good repreſentation thereof among authors, and as a good figure conveys a ſtronger idea to the generality of readers than the moſt accurate deſcription, I have procured

(1) *Matthiolus*, ſpeaking of the *oenanthe*, ſays, p. 628. *Putamus tamen a cæteris filipendulis non multum differre.* (2) *Lobel*'s *Adverſaria* were publiſhed in 1572.

(3) Οἰνάνθη τὰ μὲν φύλλα ἔχει ὥσπερ σταφυλῖνος, ἄνθη δὲ λευκά. Καὶ καυλὸν παχὺν σπιθαμιαῖον· καρπὸν δὲ ὥσπερ ἀτραφάξιος· ῥίζαν μεγάλην κεφαλὰς ἔχουσαν πλείονας στρογγύλας· φύεται ἐν πέτραις· *Dioſcorid.* lib. iii. cap. 135. (4) *Plinii* Nat. Hiſt. lib. xxi. cap. 24. (5) Pag. 265. (6) *Fuchſii* Hiſt. 563. (7) *Trag.* p. 883. (8) *Dodon.* Pempt. 56. (9) Pag. 627. (10) *Park.* Theat. 895.

that

that admirable artist Mr *Ebret* to draw not only this plant, but also the *cicuta aquatica* of *Wepfer*; that they may be the more easily known from all plants, and distinguished from each other by their being both seen at one view. From these drawings the ingenious Mr *Mynde* has very accurately engraved the figures hereunto annexed.

P. S. I am informed by Mr *Ebret*, that, in drawing the *oenanthe*, which he has executed with his usual elegance and accuracy, he was obliged to have a quantity of it placed before him upon a table; when, the room being small, the *effluvia* thereof caused in him an universal uneasiness, with a *vertigo*; so that he was constrained to have it removed, and never after place before him but a small piece at a time.

There is something in the formation of the root of the *cicuta aquatica* before-mentioned, deserving particular notice. This plant generally grows either near the sides of large stagnant waters, or in shallow rivers, whose streams are slow. Towards the end of autumn, or the beginning of winter, the root for the succeeding summer is formed out of the lower part of the stalk. Out of the crown of this root are then seen the rudiments of the leaves of next year (*see Fig.* 136. *a.*) Fig. 136. and from the sides of this grow the crowns of several smaller roots. This root, in it's whole length, is divided transversly into a number of large unequal cells (*see Fig.* 137.) corresponding with the par- Fig. 137. titions, which divide these cells, the surface of the root is marked circularly with little round depressions. So great a part of this root is occupied by the cells, that it becomes specifically lighter than water; so that, in winter, upon the increase of water in the rivers and pools, this root, as well that part intended for the succeeding summer, as that which furnished the plant the preceding, is buoyed up. The old root then rots, and floats upon the surface of the water with the new one all the winter (*see Fig.* 136. *b.*); and in rivers these are frequently carried to very great distances from the places of their growth. In the spring the old root is washed away; and the new one, upon it's coming near the soil, sends out from the circles before-mentioned, particularly from those nearest the bottom, a great number of long slender white fibres, by which this root becomes again fixed to the soil, propagates it's species, and remains thus, until, by the rotting of these fibres, it is again weighed up. The old root decaying, and being washed from the new, is the cause of that truncated appearance we observe in the root of the figures of *Dodonæus*, *Parkinson*, and *Morison*, who have exhibited this plant in a flowering state. This provision of cells in the root seems to be given to this plant by nature, that, as great part of it's root is apt to perish in winter, vegetation might not be prevented, nor the root destroyed, unless the whole number of cells are spoiled, which very rarely happens.

 a, It's

Explanation of Fig. 134. *exhibiting the* Oenanthe Cicutæ facie.

a, It's tuberose roots surrounding the stalk. *b, b, b, b*, A leaf taken from near the bottom of the stalk. *c*, A branch with the umbels of flowers in different states. *d*, An anterior view of the flower of it's natural size. *e*, A posterior view of the same. *f*, The anterior appearance of the flower through a miscroscope. *g*, The posterior view of the same. *h*, A view of the rudiments of the fruit after the decay of the flower. *i*, The same magnified.

Explanation of Fig. 135. *representing the* Cicuta aquatica *of* Wepfer.

a, A branch of this plant with it's umbels of flowers in different states. *b*, The appearance of the bottom of the stem, growing from the crown of the old root. *c*, An anterior view of the flower of it's natural size. *d*, An anterior view of the same magnified. *e*, A posterior view of the flower magnified. *f*, The *Vasculum seminale*, and seed. *g*, The same magnified.

Explanation of Fig. 136, and 137. *representing the root of the* Cicuta aquatica *in Winter.*

Fig. 136. *a*, The rudiments of the leaves. *b*, The old rotten root not yet separated from the new one of the preceding summer.

Fig. 137. A longitudinal section of the root exhibiting the cells.

An account of the poisonous root *lately found mixed among the* Gentian; *by* Rich. Brocklesby, *M. D. F. R. S.* N°. 486. p. 240. Feb. & Mar 1748. *Read* March 17. 1747.

XX. The following account is the best I have received of the poisonous effects of a noxious root, lately found in a parcel of gentian, and exhibited for use to several persons instead of it. And as it is attended with such dangerous consequences, I thought even an imperfect relation of facts had better be given immediately, than to expect more circumstances, and wait so long for them, till greater mischiefs might happen, by the inattention of such as are constantly administring medicines. The following account was sent by a gentleman of *Hambleden* parish, *Buckinghamshire*; and is found to agree in general with some other fatal instances that have happened since in *London*.

Mary Burgess, aged 60 years, about 5 in the morning, drank of an infusion of only one pennyworth (without other ingredients) of supposed gentian-root, in ¼ a pint of white wine: it is uncertain what precise quantity she took; but in 2 hours afterwards she faltered in her speech, had twitchings and convulsions of her hands so far, that the ignorant by-standers alledged the poor woman was drunk; and so left her in bed till 12 to sleep it out. On their return however she appeared much worse, was speechless, and remained so 3 whole days, and did not know any body all that time. In her illness a purging came on, and at last carried her off.

Katharine Woodward, aged 44, took about a tea-spoonful of the same wine, and soon after fell down speechless, and her limbs were paralytic near 36 hours: after that she recovered her speech, but continued ill above a fortnight, and part of that time her under jaw was convulsed, and she bled both at mouth and nose, in the beginning.

Mary Diggins, aged 33 years, tasted a much less quantity of the same wine than the former had done; and though terrified at her neighbour's bad symptoms, she drank warm water with oil, in order to vomit; yet she

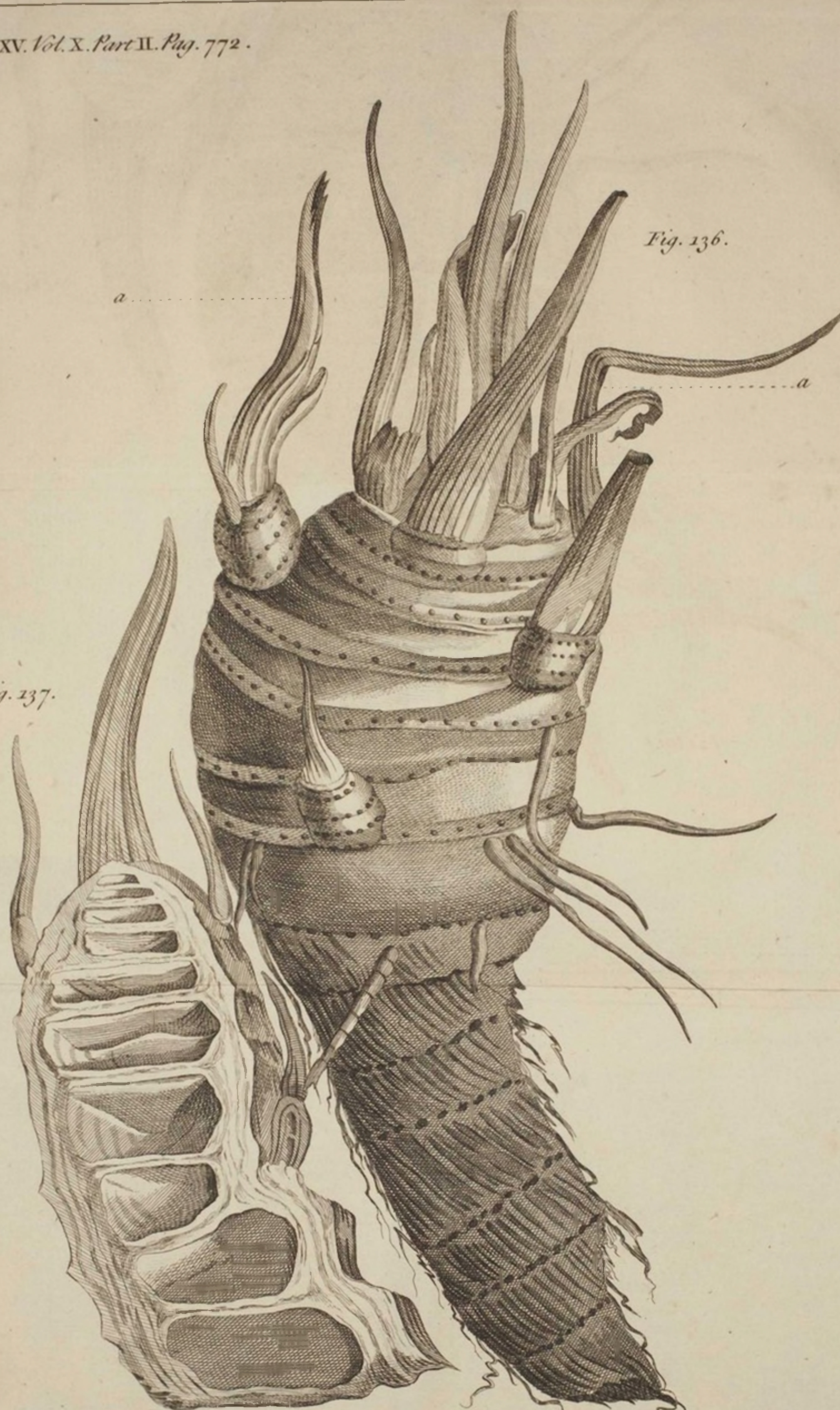
Fig. 134. Pla. XXIII. *Vol. X. Part II. Pag. 772.*

OENANTHE, *Cicutæ facie, succo viroso crocante.* Lob. adv. 326.

Pla. XXIV. Vol. X. Part II. Pag. 772.

Fig. 135.

CICUTA Aquatica Wepf. Sium alterum Olusatri facie Lob. Icon 208.



The Root of the Cicuta Aquatica in Winter.

ſhe ſoon ſtaggered, and grew delirious, could not ſwallow any ſolids, and loſt the perfect uſe of her eye-ſight a fortnight.

The vague reports of theſe, and Mr *Pots*'s caſes induced me to obtain the favour of 2 or 3 Druggiſts to look over ſome gentian-root, one parcel of which had no leſs than $\frac{1}{10}$ of a root, which at firſt ſight was diſcovered to be no gentian.

This root, for which we have yet no name, is of a greyiſh brown colour externally, but it is browner, and more reſinous internally: moſt of that which I have ſeen, is about the thickneſs of a finger; tho' ſome is much larger and whiter; which is a reaſon with ſeveral for thinking there are 2 ſorts of it; and indeed ſome pieces emit a ſtronger and more nauſeous ſmell: but this I apprehend may be occaſioned only by a larger quantity of reſin in them. All of them are of an acrid pungent taſte, and leave a dryneſs on the tongue.

I judged it therefore neceſſary to try what effects this root might have on dogs, that I might thereby the better conjecture concerning them on the human ſpecies. With this intention I decocted ℥ſs of this unknown root, powdered groſsly in ℥x of fair water, till ij were evaporated; then let the decoction ſtand 6 hours. After this I gave half of it, ſtirring up the powder, to a young dog. This made him inſtantly foam at the mouth; he grew ſick, and vomited part of the doſe; yet in leſs than $\frac{1}{2}$ an hour reeled like one drunk, had twitchings of his limbs, and after ſome time the motion of his heart was irregular, and intermittent, tho' ſtrong: he was ſleepy about an hour, but came gradually to himſelf in $\frac{1}{2}$ an hour more, and eat victuals, which before he refuſed.

Two days after, the ſame dog took ℥iv of decoction of gentian made as ſtrong as the former; but I diſcovered not any bad ſymptom from it. I uſed this quantity, as gentian-root is ſometimes given to that quantity in the practice of phyſic. It is above 10 days ſince he took the firſt decoction; and hitherto continues well.

Another dog took above ʒi of the unknown root, finely powdered, and mixed with butter: it inſtantly made him foam from the mouth, and cauſed ſudden vomiting, and, in $\frac{1}{2}$ an hour, weakneſs of his limbs, and ſtaggering, which laſted $\frac{1}{2}$ an hour, and then he recovered.

I tried to give a larger quantity to another dog; but it being too much like other irritating medicines, cauſed ſo great a vomiting, as deſtroyed the effects which a ſmaller quantity had before produced. One of the dogs had ſome looſe ſtools after taking it; another urined plentifully. Like experiments have been made by Mr *Pearce* at St *Thomas*'s hoſpital, which had nearly the ſame event.

Though none of the dogs were killed by this drug, but remain to appearance well, yet all Apothecaries have ſufficient reaſon to examine very ſtrictly their gentian, and to reject what they find not genuine, ſince one of the women before-mentioned, and a man that I have heard of, are both dead; and ſince gentian is of general uſe in medical compoſitions,

positions, as well as the primary ingredient in the cordial bitters ladies make for their own use.

Extract of an essay upon the origin of Amber; *by* John Fothergill, *M. D.* Lic. Coll. Reg. Med. Lond. N°. 472. p. 21. Jan &c. 1744. *Read* March 1. 1743-4.

XXI. After all that has been written upon the subject of amber, it's origin is yet, in a great measure, unknown. Several ingenious men have searched into this affair upon the spot where the amber is principally gathered: they have related their observations with great candour; they have given us the conclusions they drew from the facts they discovered; yet without satisfying us intirely about many particulars.

But, as a knowledge of the nature of things can only be acquired from the things themselves, I have carefully collected every material fact I could meet with, from those who were best acquainted with the natural history of this subject, and whose industry and accurateness in observing, and good faith in relating their observations, have been generally esteemed unexceptionable. Of these I shall only mention *Wigandus*, *Hartman*, and *Sændelius*; the last who has written, as far as I know, professedly upon this subject.

The evidence which these gentlemen afford us, I have endeavoured to throw together, in the most natural order I could, without respect to any hypothesis: but as this enumeration of facts admits of no abridgment, my papers would take up too much room: therefore I can only refer to the essay itself. Upon this foundation of facts is built a discussion of the following problems:

1. Whether amber is not strictly a marine production; or is reduced by some quality of the sea-water into the condition we find it in? Or,
2. Whether it is not to be considered only as a bituminous body, generated in the bowels of the earth? Or, lastly,
3. Whether it is not, in it's origin, a vegetable production, a resin; but changed into it's present form by a mineral acid?

It will only be necessary, in this place, to mention, that, after having shewn the difficulty of maintaining the 2 first, I have undertaken to support the last of these opinions.

I endeavour to make it appear, that Amber was, in it's origin, a vegetable resin; the product, perhaps, of the fir or pine kind; by considering the appearance of the substance itself: and that though it has some distinguishing properties, yet it has many others, which are common to an indurated resin. It's aspect, it's texture, it's form, are arguments for this. The bodies which it is known to inclose, are urged as proofs, that this inclusion could not happen in the sea, nor in the earth, but upon it's surface; as the included objects are mostly animals, mostly volatiles too; very few reptiles, except such as are often found aloft in trees, as ants, spiders, *&c.* and scarcely ever any aquatics, are found in amber. And, I believe, I may challenge all the cabinets of the curious to produce one instance of a marine body having been found naturally

naturally inclosed in amber. That there are several fictitious ones, is granted.

That this resin with the trees which afforded it were buried in the earth by the Deluge, or by some such violent renversement, and there constitute the proper veins of amber, I likewise endeavour to make appear, from the same evidence of facts. The substance of which these veins consist, hath several genuine characteristics of wood still remaining. The texture of this substance is often an undoubted proof of what it hath been; being fibrous, and, when dried, swims in water, and burns like other wood. The amber is not disposed in these veins in one continued *stratum*; but lumps of it are irregularly disseminated through the whole of what I call the woody mass.

A difficulty, which naturally offers itself in this place, is attempted to be removed: what proof have we, that this, which is called wood, is not mere fossil wood, the product of creating power, exerted in the place where it is now found? It is answered, That as there are undoubted proofs, that many substances now occur, where they were not originally framed, we are under no greater difficulty in accounting for the change of place in one than the other. It is known, that the *exuviæ* of fishes are sometimes found on the tops of the highest mountains. The bones of large animals are met with at prodigious depths, where nature never formed, nor art conveyed them. Whole woods are found underground. The cause that effected these, was capable of the other.

Yet, allowing these allegations to be just, by what causes is this change produced? It is urged, That time is one of the causes; and that the rest is completed by the acid of the earth, a vitriolic mineral acid. It is proved, from the facts above-mentioned, that such an acid is present wherever amber occurs in it's *proper matrix*: that it is sometimes found in the amber itself, in it's genuine appearance; that the acid of the salt of amber appears, from experiments, to be vitriolic; that common turpentine (a known vegetable resin) affords, by proper management with a vitriolic acid, a considerable portion of the same chemical principles that amber does; that those pieces of amber, which have been found soft and imperfect, are nearly related to a vegetable resin: in short, it is endeavoured to be proved, that we have the ingredients of amber in our power, and that nothing is wanting but a successful application of them to each other; at least to procure the medicinal preparations of amber at an easy expence. Time and repeated trials may, perhaps, ripen this beginning, in somebody's hands, into an happy useful imitation of this valuable substance.

This account is concluded with an inquiry into the medical virtues of amber, and some of it's principal preparations. It is observed, that a substance of so firm a texture, as scarce to yield to any common *menstruum*, is not likely to produce any considerable effects upon the human body; and that, indeed, there are very few genuine instances recorded of any: that busy imagination might, probably, at first, introduce

duce it, prejudice ſupport it, and engage men of parts and authority to recommend it to their inattentive ſucceſſors.

I ſhall finiſh this abſtract with remarking, that, were ſome of the leiſure moments of men of great abilities and experience devoted to inform the world of the inefficacy of ſuch methods and medicines as they have proved to be ſo, Phyſic would be reduced into narrower bounds; they would merit the thanks of every one in the profeſſion; and poſterity, at leaſt, would commend their endeavours.

Concerning the method of gathering Manna *near* Naples, *in a letter from* Robert More, *Esq; to Mr* W. Watſon, *F. R. S.* N°. 495. p. 470. May &c. 1750. *Read* June 21. 1750.

XXII. At *Arienzo*, a town between *Naples* and *Benevento*, I found an aſh-coppice, of 8 or 10 years growth, from which they collected *manna*. It ſeemed to have been tapped 2 years for that purpoſe; the branches had been barked each year about an inch broad, and 2 feet high; but they told me this was done by an inch at a time.

They place a cup at the bottom of the wound, which they empty every 5 days. This liquor becomes *manna*. They formerly let it dry upon the tree; but the preſent way keeps it cleaner. The manna begins to run (they ſay in the Scripture ſtyle to *rain*) the beginning of *Auguſt*; and if the ſeaſon proves dry, they gather it 5 or 6 weeks. The King of *Naples* has ſo large a revenue from it, that he is extremely jealous of it, during the ſeaſon guards the woods by *Sbirri*, who even fire upon people that come into them, and he makes the ſtealing of the liquor death. The ſeaſon in which I was at *Arienzo* prevented my ſeeing the ſpecies of aſh. I believe it to be what our Gardeners call the flowering aſh; the complexion of the bark and bud agrees with one of them I have in my garden at *Lindley*. The man who ſhewed me the wood, told me, it bore a pretty flower in the ſpring. At *Piſa* in the phyſic-garden they ſhewed me that tree in bloom as the manna-aſh. The tree is indeed common enough in that neighbourhood: I wonder Mr *Ray* does not mention it among the plants found there by him. The *Italians* call it *Orno*. A Botaniſt at *Rome* told me it was the *ornus officinarum*. A Phyſician at *Benevento* to the ſame purpoſe, that it was the *ornus* uſed in medicine. A perſon is gone from *Rome* to *Naples*, who has promiſed to be very particular in getting you information of their manner of curing it. He was bred a Chemiſt, and told me many ways of counterfeiting the ſeveral appearances of it. The moſt common is with *Glauber*'s ſalts and ſugar, with a ſmall mixture of manna. The price of manna at *Naples*, they told me, was 4 *carlins* ($4\frac{1}{2}$ *d.* ſterling each) the *rotolo* (32 ounces).

An account of the preparation and uſes of the various kinds of Pot-aſh; *by* John Mit-

XXIII. Altho' pot-aſh is a thing daily uſed, and well known even to the vulgar; yet, as the making it is a mechanic art, practiſed only by the vulgar, and neglected and overlooked by the learned, ſo we have had no ſatisfactory account of it; and they, who underſtand it, generally keep it a ſecret, leſt others ſhould learn ſo beneficial an art. But as it is a commodity that no nation hardly can well be without, either for

for making soap, glass, dying, or bleaching, so the way of making it is generally understood in most countries, except our own. For in *France*, and other countries where they make wine, they make a kind of pot-ash in an easy manner from the lees of their wine. In those and other more southern climates, they have many kinds of herbs hereafter mentioned, either spontaneous, or cultivated on purpose, which they as easily convert into pot-ash. In *Germany*, and other more northern countries, they make great quantities of pot-ash by extracting the salts of their wood-ashes, in a manner that is well known. But it is only in *Russia*, *Sweden*, and other northern nations, where the art of converting their wood-ashes into pot-ash, without the tedious process of elixiviation, is either well known to the learned, or practised by the vulgar.

chell, *M. D. F. R. S.* N°. 489. p. 541. Oct. & Nov. 1748. *Read* Nov. 17 *and* 24. 1748.

By this means most nations are supplied with this necessary commodity of their own, except the *English*, who might be supplied with any quantities of it, from the great plenty of otherwise useless wood they have in their colonies, if not at home, if they knew how to make it. But it seems this art is so little understood among us, that many attempts I have known to make pot-ash have all proved unsuccessful merely upon that account, so as to be entirely laid aside. This has put me for some time upon inquiring into the ways of making this commodity, of which several have been suggested to me, from the several trials and informations hereafter mentioned.

It is well known, that the ashes of all kinds of vegetables whatever, afford pot-ash in some measure or other; altho' some are much more fit for that purpose than others, which may be determined from the experiments of *Redi*, *Phil. Trans.* N°. 243, p. 281. *Boerhaave*, *Merret*, and others; so that we need not insist upon them here.

As for the trees and herbs of our colonies in *N. America*, most of those that are common in their woods are known to be fit for this purpose, as the ashes of them all, burnt promiscuously in their houses, make a very strong lye fit for soap. Of these, the fittest for that purpose is their *hiccory*, the most common tree in their woods, which makes the purest and whitest ashes, of the sharpest taste, and strongest lye, of any wood I have seen. Their *stickweed* is said to do the same, which is as common a weed. For this reason the ashes of both these plants were used by our *Indians* there, instead of salt, before they learnt the use of common salt from the *Europeans*. The ashes of *tobacco* likewise, when damnified, or not fit for a market, or it's stalks, stems, and suckers, of which great quantities are thrown away, and rot and perish, are very fit for pot-ash, as they contain a great deal of salts, and are well known to make a strong lye.

On the other hand, *pines*, *firs*, *sassafras*, *liquid amber*, or *sweet gum*, or all odoriferous woods, and those that abound with a resin or gum, are unfit for pot-ash, as their ashes are well known, even to our planters, to make a very weak lye, unfit for soap.

Beſides theſe that contain little or no ſalt, there are ſome other vegetables that afford a large quantity of it, but make a bad kind of pot-aſh, at leaſt for many purpoſes, on account of a neutral ſalt with which they abound. This ſeems to have been the caſe of the pot-aſh made in *Africa*, in a manufacture of that commodity ſet up there by the *African* Company, which Mr *Houſton* (who was chiefly concerned about it) tells us, in his *Travels*, proved ſo bad, on account of a neutral ſalt it contained, that the manufacture was left off on that account; or, perhaps, from their not knowing how to make it aright. What thoſe vegetables are, that afford this kind of aſh, is not well known, if it be not fern, and ſome ſea-plants.

Whatever vegetables we make our pot-aſh of ſhould be freſh or green, and no ways rotten, dried, or decayed. They ſhould likewiſe be burnt to aſhes by a ſlow fire, or in a cloſe place; otherwiſe, when they are burnt in the open air by a ſtrong fire, great quantity of the aſhes is conſumed in ſmoke, by the ſaline and terreſtrial parts being carried up in fumes, before they are ſeparated from theſe exhalable parts by the action of the fire. For the difference between burning wood in a cloſe place, or the open air, is ſo great, that the quantity of aſhes obtained from one is more than double the other. This we learn from the experiments of *Lundmarck* hereafter mentioned, who tells us, he burnt a quantity of birch in a cloſe ſtove, from which he obtained 5 pounds of aſhes; whereas the ſame quantity of the ſame wood burnt in the open air, yielded only 2 pounds.

It is for this reaſon, that moſt people who make pot-aſh, burn their wood in kilns, or pits dug in the ground; altho' the *Swedes* burn it in the open air, as the author above-mentioned informs us. This firſt ſtep, or the burning the wood to aſhes, ſeems to be taken by many for the whole proceſs of making pot-aſh; for they who pretend to have learned this art in *Ruſſia*, as well as *Lemery* and ſome other authors, hardly give us any other account of it.

But, in order to convert the aſhes, prepared in this or any other manner, to what is called pot-aſh, there are many different ways practiſed in different countries, which make as many different kinds of pot-aſh, that are all to be found in our markets, and have all their reſpective uſes.

1. The firſt of theſe is commonly called pearl-aſhes by our people, who import great quantities of it from *Germany*. This is no other than the lixivial ſalt of wood-aſhes, extracted by making a ſtrong lye of them, and by evaporating it to dryneſs, in a manner that is well known, and ſufficiently explained by *Kunkelius* in his art of making glaſs, *Boerhaave*, and many others; ſo that we need not inſiſt upon it here; we ſhall take a more fit opportunity to explain it, for the uſe of our people in *America*.

2. But the art of converting theſe wood-aſhes into pot-aſh, without this tedious proceſs of elixiviation, is only practiſed in *Ruſſia*, *Sweden*, and

and other northern countries, where it has been lately disclosed by one *Lundmarck*, who tells us he had often made it himself, in the manner he now describes. This account is contained in an academical dissertation upon this subject at *Aboe* in *Sweden*, and was communicated to me by Dr *Linneus*, Professor of Botany at *Upsal*, as a genuine account of this art; which I think has hitherto been generally unknown.

This author tells us, "They have many large woods of beech in "*Smoland*, and other parts of *Sweden*, in want of which they take alder: "of these they are allowed to use only the old and decaying trees for "this purpose, which they cut to pieces, and pile in a heap, to burn "them to ashes, upon the ground, by a slow fire. They carefully "separate these ashes from the dirt or coals in them, which they call "raking them; after which they collect them in baskets of bark, to "carry them to a hut built in the woods for this purpose. This they "continue to do till they have a sufficient quantity of these ashes. Then "their whole art follows; for which they choose a convenient place, "and make a paste of these ashes with water, by a little at a time, in "the same manner, and with the same instruments, as morter is com-"monly made of clay or lime. When this is done, they lay a row of "green pine or fir-logs on the ground, which they plaster over with "this paste of ashes: over this they lay another layer of the same strait "logs of wood, transversely or across the others, which they plaster "over with the ashes in the same manner: thus they continue to erect "a pile of these logs of wood, by layer upon layer, and plastering "each with their paste of ashes, till they are all expended; when their "pile is often as high as a house. This pile they set on fire with dry "wood, and burn it as vehemently as they can; increasing the fire from "time to time, till the ashes begin to be red-hot, and run in the fire. "Then they overset their pile with poles, as quickly as they can; and "while the ashes are still hot and melting, they beat and clap them, "with large round flexible sticks made on purpose, so as to incrust the "logs of wood with the ashes; by which the ashes concrete into a solid "mass as hard as stone, providing the operation has been rightly per-"formed. This operation they call *Walla*, i. e. *Dressing*. At last they "scrape off the salt thus prepared, with iron instruments, and sell it "for pot-ash; which is of a bluish dark colour, not unlike the *scoriæ* "of iron, with a pure greenish white salt appearing here and there "in it."

All the pot-ash we have from *Russia*, *Sweden*, and *Dantzick*, is exactly like what our author here describes, and seems to be made in this manner. It is, however, generally observed, that the *Russian* is the best of these, on account of the greater quantity of salt in it. Now if, in the preceding process, we make our paste of the ashes with lye, instead of water, it is plain the pot-ash will be impregnated with more salt, and make all the difference there is between these sorts of pot-ash. This then is likely to be the practice in *Russia*; where their wood may like-

 wise

wise be better for this purpose, and afford more salt. This is well known to be the case of different kinds of wood: so our author above-mentioned tells us, he obtained 2 ½ lb of salt out of eight cubic ells of poplar, which was very sharp and caustic; but the same quantity of *birch* afforded only one pound of salt, and that not so strong; and *fir* hardly yielded any at all.

The way of making pot-ash above described may be the more easily understood by our people in *America*, for whom this is chiefly intended, as it is the same with their way of making lime of shells, the only lime they use in most places. These shells they burn to lime between the layers of a pile of wood (instead of a kiln) till it is reduced to ashes, in the same manner as is here directed to be done with ashes, to make pot-ash. The lime, thus made, is reckoned very good; but, as it is impregnated with the ashes of the wood, and the marine salt that is often in the shells, it is apt to make the houses that are built with it very damp in moist weather; so that the water often runs down their walls in streams; which cannot but be very unwholsome in an air that is naturally close and damp: the only way to prevent which would be, to wash and dry their shells frequently, and burn them in dry pine, that afford little or no lixivial salt. But to return to our purpose:

3. There is another way of making pot-ash, practised chiefly in *England*, where they make it in the following manner, as I am informed by several, who have seen it done:

With their ashes of fern, or wood of any kind, they make a lye, which they reduce to what they call pot-ash, by burning it with straw. To do this, they place a tub full of this lye nigh a clean hearth of a chimney, in which they dip a handful of loose straw, so as to take up a quantity of lye with it. The straw thus impregnated with lye they carry as quick as they can, to hold it over a blazing fire on their hearth, which consumes their straw to ashes, and at the same time evaporates the water from the salts of the lye. Over the blaze of the first parcel of straw they burn another dipt in lye in the same manner. This they continue to do till their lye is all expended. By this means the coals and ashes of the straw, and salts of the lye, are left on the hearth, and concrete together into a hard solid cake of a greyish black colour, which they scrape off, and sell for pot-ash.

This is an easy way of making pot-ash, in want of proper vessels to extract the salt of the lye by evaporation, or in want of wood to reduce the ashes to pot-ash in the way above mentioned, for which it seems to be contrived, and for which it is only to be commended. For the pot-ash made in this manner is full of the coal of the straw, and it's salt is not so strong, as our workmen say, or so sharp and corrosive as the salt of the foreign pot-ash, that is calcined in an open fire; besides other differences hereafter mentioned; which makes this pot-ash unfit for some purposes, and not above half the value of the foreign.

4. They have a very different way in the north of *England* of reducing their *kelp* to pot-ash, which they use for making alum. This is made of

of the different kinds of *fuci*, or ſea-weeds thrown upon the ſhore, or gathered on the rocks; which they dry a little in the ſun, and afterwards burn them in a kiln, built of the ſtones they find on the ſhore, in a cylindrical form, and about 2 foot or leſs in diameter. In this they firſt burn a ſmall parcel of the herb, and before it is reduced to aſhes they throw on more, till the kiln is full, or their materials are expended. This is ſaid to reduce the aſhes to a hard and ſolid cake, by the heat of the kiln, and quantity of ſalt in the herb, which makes what is commonly called *Kelp-Aſhes*.

There are ſome other ways of making pot-aſh, ſuggeſted by ſeveral, both authors and others, which appear to be more eaſy and ready than any of the above mentioned; for which reaſon they are apt to be tried by thoſe who make attempts of this kind. Theſe are deduced from what they reckon the nature and properties of this production: and there is no doubt, but if that was well underſtood, it might afford ſome inſight in the way of making it. For this reaſon we made the following experiments with the beſt *Ruſſia* pot-aſh, in order to diſcover it's nature and properties, and how they are moſt probably communicated to it; that we might ſee what we are to make; in order to imitate the beſt, or to make what is accounted good pot-aſh.

1. *Ruſſia* pot-aſh, as it is brought to us, is in large lumps, as hard as a ſtone, and black as a coal, incruſted over with a white ſalt, that appears in ſeparate ſpots here and there in it.

2. It has a ſtrong fetid ſulphureous ſmell and taſte, as well as a bitter and lixivial taſte, which is rather more pungent than other common lixivial ſalts.

3. A *lixivium* of it is of a dark-green colour, with a very fetid ſulphureous ſmell, and bitter ſulphureous taſte, ſomewhat like gun-powder, as well as ſharp and pungent like a ſimple *lixivium*.

4. Altho' it is as hard as a ſtone, when kept in a cloſe place, or in large quantities together in a hogſhead; yet, when laid in the open air, it turns ſoft, and ſome pieces of it run *per deliquium*; whilſt moſt other kinds of pot-aſh only turn friable, and crumble in the open air.

5. It readily diſſolves in warm water, but leaves a large ſediment of a blackiſh grey colour like aſhes, which is in a fine ſoft powder, without any dirt or coals in it, that are to be obſerved in moſt other kinds of pot-aſh.

6. As it is diſſolving in water, I have ſcummed off from ſome lumps of it a dark-purple bituminous ſubſtance, like *petroleum* or tar, which readily diſſolved in the *lixivium*.

7. This, or any other true pot-aſh, or a *lixivium* made of them, will preſently tinge ſilver of a dark purple colour, difficult to rub off; whilſt a mere lixivial ſalt has no ſuch effect.

8. Pieces of this pot-aſh boiling in water make a conſtant exploſion like gun-powder; which was ſo ſtrong as not only to throw the water to ſome height, but to lift up and almoſt overſet a ſtone cup in which I boiled

I boiled them. These explosions were owing not so much to the included air, which some perhaps may imagine, as to the sulphureous parts of the composition expanding and flying off: for this boiled *lixivium* had neither the green colour, nor fetid sulphureous smell and taste; at least in any degree like what it has when made of the same pot-ash by a simple infusion in warm water.

9. I evaporated some of the green *lixivium*, made only by infusion, and filtred thro' a double rag: as soon as it began to boil, a green powder, to which it's colour is owing, fell to the bottom, and the lye became pale. After it was evaporated to a pellicle, and set in a cool place, a salt separated from it on the sides of the cup, in angular crystals like tartar. These crystals were soon formed, and in pretty large quantities, but were difficult to separate from the alkaline lye and salt, in which and the open air they were apt to dissolve: but from the pellicle I obtained some pieces of the same salt that would not dissolve in the open air.

10. Oil of vitriol makes a strong effervescence with this green precipitate, with a white fume, and a very strong sulphureous smell. It does the same with these white crystals, altho' the sulphureous smell is not so strong. But with the pure fixed alkali there was no such sulphureous smell to be discerned.

From these experiments we may determine something about the nature and contents of pot ash. This we are the better enabled to do, from the accurate experiments and reasonings of the learned Mr *Geoffroy*, on a like substance made of charcoal and an alkali salt calcined together, in which he observed all the properties and contents of pot-ash abovementioned, particularly related in the *Mem. R. Acad.* 1717. This was made of the same materials, and had all the properties above related of our pot-ash; particularly a green *lixivium*, a strong sulphureous smell and taste, a sulphureous green precipitate, crystallized salts, and sulphureous fumes with oil of vitriol. From hence this learned author concludes, that this substance contained the active sulphureous parts of the wood, blended with more active igneous particles. These, united with the alkaline salts, make a kind of soap, or sulphureous saponaceous salt, resembling soap of tartar, or *hepar sulphuris*. The crystallized salts he attributes to the acid of the wood, mixing with the alkaline salts. All these parts of the wood then are contained in our pot-ash; and he observed the same in the common *soda*, or *cineres clavellati*; altho' they are in a less degree in that than in the *Russian* pot-ash.

Besides these, he shews that pot-ash contains a metallic substance, which affords the *Prussian* blue. We may add further, that the combination of these principles makes many properties in pot-ash, more than what result from them in a state of separation. The most remarkable of these seems to be it's explosive quality; which we take to proceed from the crystallized salts approaching to the nature of nitre, and uniting with the sulphur and charcoal; by which they form, from all these

theſe ingredients of gun-powder, a kind of that exploſive ſubſtance, whoſe parts are highly rarefied in an intenſe and confined heat, by which they readily explode in boiling lye.

By this we may perceive, that the difficulty in making pot-aſh aright, is, firſt, to reduce the materials to cinders and aſhes, and at the ſame time to preſerve their volatile, ſulphureous, and exhalable acid parts, that are totally deſtroyed in ſuch a degree of heat; and, ſecondly, to calcine theſe aſhes ſtill further, ſo as to flux their ſalts, and vitrify their terreſtrial parts, and at the ſame time to keep them ſeparate from each other, or prevent their running into an indiſſolvable glaſs. To give pot aſh ſome of theſe properties, ſeems plainly to require a degree of heat that will totally deprive it of others.

The moſt likely way by which it comes to receive all theſe properties, is from the way of making it in *Sweden* above deſcribed. In that proceſs, the green fir, in which the aſhes are burnt, impregnates them with the acid ſaline parts of the wood or tar, which is well known to be in pretty large quantities, and is abſorbed and fixed by the alkaline ſalts, and porous terreſtrial parts of the aſhes in this proceſs; ſo that, beſides the fixed alkaline ſalts of the aſhes, the pot-aſh, thus made, muſt likewiſe contain the more volatile ſalts of the pine, which are exhaled in ſmoke by burning the pine alone in the open air. Beſides theſe, it likewiſe contains the reſinous parts and ſulphureous fumes of the pine, that are hindered from exhaling by the heap of the maſs.

At the ſame time the alkaline ſalts are fluxed in the open fire, and in a manner vitrified with the terreſtrial Parts of the aſhes, which gives them their hard and ſolid conſiſtence; whilſt the ſulphureous and acid parts of the green wood hinder them from turning to a perfect glaſs, or inert *calx*. All theſe parts united together in the fire, make that ſaponaceous ſubſtance we find in the pot-aſh thus made, which further hinders the vitrification of the maſs, and endows it with many of it's moſt peculiar and active properties.

From hence we may ſee how difficult it is to make a ſubſtance endowed with all theſe properties in any other manner. This is the reaſon why we could never before make pot-aſh equal to that of *Ruſſia*, and the other northern countries, although we have much greater plenty of materials, and perhaps better: for this way of making it has never before been thought of by the Learned, or practiſed any where elſe, as far as I can learn.

Somewhat of the ſame qualities are communicated to the *Engliſh* pot-aſh, by the way of making it above deſcribed; but in a degree as much inferior, as dry ſtraw, uſed for that purpoſe, is to green wood: accordingly our workmen find that pot-aſh as much inferior to the foreign, for many purpoſes.

From this account of the contents and qualities of pot-aſh, and the way of making it, we may form ſome judgment of the other ways of making it, propoſed by authors, and ſuggeſted by many. Thus *Lemery*

mery and others tell us, pot-aſh is made in *Ruſſia*, and all the northern countries, only by calcining the aſhes in pits bricked within, and ſprinkling them well with lye, till they become hard and ſolid. But ſuch a calcination of aſhes with a lixivial ſalt, muſt render them whiter, inſtead of black, and muſt further deſtroy the active ſulphureous parts of the wood, which we find in pot-aſh rightly made. So that this only leaves the aſhes in the ſtate they were at firſt, or turns them into a kind of indiſſolvable glaſs, as we have found upon trial.

This, and the like miſtakes about the way of making pot-aſh, ſeem to proceed from a general error concerning the nature of it; for it is commonly ſuppoſed to be only a kind of inert *calx*, impregnated with nothing but a lixivial ſalt. Some ſuch miſtake ſeems to have fruſtrated all the attempts hitherto made of making pot-aſh in *America*; for, upon trial, what they have made there was found to be no better than common aſhes.

But the moſt general miſtake about the way of making pot-aſh, ſeems to proceed from the accounts we have of making it, from glaſſwort, and ſome marine plants, which are ſaid to be eaſily converted to this kind of ſubſtance, in the manner above-mentioned. But we apprehend, the way of making it from wood muſt be very different: for theſe herbs are eaſily reduced to aſhes by a ſmall fire that does not intirely conſume their ſulphureous parts, which wood is not. Theſe aſhes abound with a great quantity of alkaline and ſome neutral ſalts, that readily convert them to a hard and ſolid conſiſtence, which wood does not. They have likewiſe few or no terreſtrial parts, to run them into an indiſſolvable glaſs, when fluxed in the fire, as happens in wood-aſhes. Beſides, theſe herbs have few or no ſulphureous or acid parts, like moſt woods; and the pot-aſh made of them has few of theſe principles in it, like what is made of wood.

It is however generally ſaid, if we burn our wood in a cloſe place, as a kiln in which we burn lime, or make charcoal, or a pit dug in the ground, we may impregnate the aſhes with the ſulphureous fumes and acid parts of the wood, only by the cloſeneſs of the place, or by ſmothering the fire in it. If at the ſame time we impregnate them with a greater quantity of lixivial ſalt, it will flux the whole maſs, and make it run into a ſolid hard conſiſtence like pot-aſh. This is commonly directed to be done, by throwing freſh or green wood or herbs upon the others, as they are burning, before they are quite reduced to aſhes; or by ſmothering the fire, as in making charcoal; and at the ſame time to ſprinkle the aſhes, thus burnt, with a ſtrong lye from time to time, in the manner commonly practiſed with glaſſwort.

This would be a more ready way of making pot-aſh than any of the above-mentioned; but as thoſe who give their advice about it, have neither tried it, nor ſeen it done; and thoſe who have tried this or any other way, find more difficulty in it, than they at firſt imagined, we ſhall ſuſpend our judgment about it, till we ſee it fairly tried, leſt we

ſhould

ſhould deter ſome from making uſeful experiments of it, or lead others into fruitleſs and expenſive attempts.

By the various ways of making pot-aſh above-mentioned, and the different materials it is made of, there appear to be many different kinds of it, that have as different qualities. It would lead us too far beyond our preſent deſign, to give a particular account of each of theſe; but as they are uſed in many of our manufactures, it ſeems worthy inquiry, to know what ſorts are generally uſed, and what are the fitteſt to be uſed in them.

The workmen in *England* make two general kinds of it, which they diſtinguiſh by the names of pearl-aſh and pot-aſh. The firſt is a mere lixivial ſalt, which is ſuppoſed to be the only ingredient of any efficacy in pot-aſh; but, upon trial, there is found to be a great difference between them, eſpecially in making ſoap. The ſalt is ſo weak in the pearl-aſh, that it does not intirely diſſolve and unite with the fat. The reaſon ſeems to be, that theſe ſalts are diſſolved in water, in order to extract them, by which they loſe many of their cauſtic igneous parts; whereas in pot-aſh, the ſalts are calcined and fluxed in an open fire, with the ignited terreſtrial parts of the aſhes, which makes them more ſharp and corroſive: they are likewiſe incorporated with the coal, and fuliginous parts of the vegetables they are made of, or with the reſinous parts of fir, which gives them the ſulphureous quality above-mentioned, and makes a kind of *ſoap* of *tartar*, or *hepar ſulphuris*, in all pot-aſh; which makes theſe ſalts ſo ready to diſſolve, and incorporate with oil, or other pinguious ſubſtances.

This is perhaps the reaſon, why the *Cineres Ruſſici* are ordered for this purpoſe, inſtead of a mere lixivial ſalt, by the *College* of *Phyſicians* in their late Diſpenſatory. The ſoap made of them muſt be impregnated with their heating ſulphureous quality, which will make it more aperient and detergent, but not ſo mild and ſoft as ſome others; by which it may be more fit for obſtinate and indurated obſtructions, but will be more offenſive to the ſtomach; which is much complained of by ſome people, who take large quantities of the ſharper kinds of ſoap.

But, to conſider pot-aſh as a commodity in trade and manufactures, which is it's chief uſe; it appears, that the people in *England* not only have it at a dear rate, but the worſt ſorts of it, at leaſt for moſt purpoſes; which cannot but have a proportional influence on their manufactures: for it is generally of as great, and ſome ſorts of a greater value in their markets, than a pure lixivial ſalt; notwithſtanding the ſmall quantity of ſuch ſalt in aſhes, and the trouble and expence of extracting it; which ſeems to be occaſioned by their not knowing how to convert aſhes into this commodity; for in *Sweden*, where this art is known, *Lundmarck* tells us, pot-aſh is ſold for little more than a farthing a pound, which coſts our workmen nigh ſix-pence.

But this is not the only inconvenience we labour under for want of this commodity; the sorts we are chiefly supplied with are perhaps the worst of any, and unfit for many purposes for which pot-ash is used. The only pot-ash almost to be met with here, comes from *Russia*, *Sweden*, and *Dantzick*, or is made in *England*. These are all made either of wood or fern-ashes, whose salts are never so pure and white at the best, as some others: but, by the way of making them, and the experiments on them above-mentioned, they appear to be impregnated with coal, smoak, and soot, which renders them still more foul and impure, makes them of a black, brown, or green colour, and of a peculiar sulphureous quality. On this account they are entirely unfit for making white glass: they make a very coarse and strong kind of soap; they are too foul, sharp, and corrosive for bleaching, and are as unfit for dyeing, at least many colours.

It is perhaps for this reason, that the workmen here, as they shewed me themselves, make all their white glass with salt-petre; which must not only be more costly, but *Neri*, *Merrett*, and others, tell us it is not so good, at least for the better sorts of glass, as a sharper lixivial salt. What they use for dyeing I am not so well apprised of: it is said, they use the volatile alkali of urine; but the *French* pot-ash, made of the lees of wine, is generally allowed to be the best for that purpose. So likewise the *Alicant* pot-ash is reckoned much the best for bleaching, and making of soap; as the *Syrian* and *Egyptian* is for making glass.

These purer kinds of pot-ash are all made of herbs, that grow only in the more southern climates, whose salts are finer and whiter, and less acrid and corrosive than the salts of wood, or most other vegetables; and by the way of extracting them by calcination in a more open fire, they are more free of coal, smoak, and soot, or any other heterogeneous mixture. On this account they are much better for the purposes above-mentioned, than the coarse and foul kinds of pot-ash that our people are supplied with.

All we have of these kinds of pot-ash, it seems, comes only from *Spain*; for which reason our people were obliged to petition to allow the importation of pot-ash from thence, during the late war; as appears by an order of the king and council of the 24th of *June* 1742. since they could not do without it in many manufactures: so that it may be worth our inquiry, to know what it is that produces so necessary a commodity.

This kind of pot-ash is commonly called *Barrilha*, from an herb of the same name in *Spain* that produces it. The first account we have of this *Barrilha* is from *Amatus Lusitanus*, who leaves us much in the dark about it. It is generally said in *England* to be a plant pretty well known to the Botanists by the name of *Ficoides Neapolitana, flore candido. Hort. Lugd. Bat.* but for what reason I cannot say. We have as little reason to believe with *John Bauhine* that it is what he calls *Kali vulgare:* For Mr *de Jussieu* has shewn us, that the true *barrilha* is a different plant from

from any of these, from his own observations of it in *Spain*, where it was cultivated; of which he has given us a particular account, by the name of *Kali Hispanicum, supinum, annuum, sedi foliis brevibus. Mem. Acad.* 1717. *p.* 93. or *Alicant glass-wort*.

The pot-ash made of this plant, he tells us, makes the best soap, the finest glass, and is the best for bleaching of any other; for which reason it is much sought after in all countries, where they value themselves for these manufactures. But I question very much, whether our workmen have it either pure and genuine, or in sufficient quantities for these purposes. All the use I find made of it among them, is to make hard soap; although they say what they have of it spoils their soft soap, by making it curdle. This is well known to be the effects of sea-salt; and Mr *de Jussieu* and others tell us, that the true *barrilha* is often adulterated with sea-weeds, which contain such a marine salt; so that it is probably only this adulterated sort that they have. Accordingly, all the *barrilha* I have found here, was of a dark brown colour, and very foul and ponderous; whereas the true sort is said, by all who know it, to be more porous, pure, and of a bluish colour. It is for this reason in all probability, that, notwithstanding all the *barrilha* our workmen have at so dear a rate from *Spain*, yet they can never make so good soap, as what comes from thence, and some other places.

The only way then, by which we are likely to have this commodity either pure and genuine, or in sufficient quantities at a reasonable rate, is from the herb itself that produces it. Whether or not it would grow in *England* is not known, as I believe it has never been tried: but there is no doubt but it would grow very well in our colonies in *America*, as I am certainly informed it does in the *Spanish* colonies there, where they have great plenty of it; and a sort that is indigenous, particularly in *Peru*, which might probably be found in our colonies, if sought for by those who knew it. But wherever it will grow in any of the *English* dominions, there is no doubt but it would be a considerable improvement, where pot-ash of all kinds is so valuable a commodity, and so much wanted; for it grows on the same ground with corn of any kind, which it does no harm to, as it is a small annual herb, that does not spread till the corn is ripe, or off the ground.

There are some other plants that are known to make a kind of pot-ash, commonly called *rochetta*, which is said to be even preferable to the *barrilha*, especially for making glass. These are the first and second kinds of *kali*, described by *Prosper Alpinus*, in his account of the plants of *Egypt*. The first of which is the above-mentioned *ficoides* that grows in *Italy*, and all over the *Levant*, but the other is peculiar to *Egypt*. These would be fit improvements for our colonies in *America*, where we seem to want nothing more than some proper production for the vast tracts of land we are possessed of there. But these plants alone afford a commodity, which *Pr. Alpinus* and *Rauwolfius* tell us they saw many large ſhips yearly loaded with iu *Egypt*, and which gives the excellency to the glaſs and ſoap that are made at *Venice*.

It would be worth while then at leaſt to make a trial of a production, that is likely to improve both our trade abroad, and our manufactures at home. It was this that put me upon the preſent inquiry, as an improvement fit for our colonies, which if I find acceptable, I ſhall hereafter conſider ſome others.

Concerning the Propagation *and* Culture *of* Muſhrooms; *by the Rev. Mr* Roger Pickering, *V. D. M.* N°. 472. p. 96. Jan. &c. 1744. *Read* April 26. 1744.

XXIV. 1. The late rains having thrown up upon my muſhroom beds a great quantity of thoſe plants, I take the opportunity to ſend ſome additional obſervations to thoſe printed in theſe *Tranſactions*. *

After having repeated the experiments, then made, upon plants and ſeeds of this year, I find no reaſon to alter any thing there mentioned, either as to the *lamellæ* or chives on the concave ſide of the *umbella*, being the *ſiliquæ* or ſeed-veſſels; or the ſeeds falling from thence to a lodgement wiſely prepared for it on the middle of the *caulis*, and from thence eaſily ſliding to the earth contiguous to the mother-plant; or as to it's propagation by fibrous runners, or *ſtolones*, like potatoes; all which, I am perſuaded, theſe following new obſervations ſufficiently confirm.

1. Upon examination of ſeveral *lamellæ*, I not only diſtinctly obſerved ſeeds, of ſize and colour proportionable to the maturity of the plant, lodged therein, but alſo a ſiliquaceous aperture, with a row of ſeeds ready to fall through it; which is a very evident proof, that each diſtinct chive is a *ſiliqua* or ſeed-veſſel.

2. Upon obſervation of the filament ſituated on the middle of the *caulis*, upon which, as I before obſerved, I at firſt diſcovered the ſeed, I found both it's contexture and ſituation evidently demonſtrating the end for which the wiſe Creator placed it there; *viz.* to intercept the ſeeds in their fall to the ground; whereby the power which the wind would otherwiſe have upon ſuch minute bodies is leſſened, and the ſeed, with little or no diſſipation, ſecurely directed near the ſtem of it's mother-plant. For this filament is indented and pappous, to catch and lodge the ſeed as it falls from the *ſiliqua*; and is, at firſt, rigid, and ſtanding horizontal to the *umbella* or head, and at right angles with the *caulis*; whereby few or no ſeeds can fall without being intercepted: but, as the plant comes nearer to it's decay, this filament relents, falls down cloſe to the ſides of the *caulis*; and it's ſeveral indentures then making parallel lines with the fibres of the ſtalk, the ſeeds are, through them, conveyed, as through little ducts or channels, to the ground.

'Tis further to be obſerved, that this filament is not of ſo ſucculent a contexture as the *ſiliqua* or ſeed-veſſel; ſo that the ſeeds, which would otherwiſe rot in the *ſiliqua*, are here retained in full health, till the period of their falling to the ground. I have now by me the filament of a plant, laid by for obſervation ever ſince *October* the 28th laſt paſt,

* See Vol. VIII. Part ii. Chap. v. § xi. 1.

which

which is near half a year ago; from which, two days ago, I took feeds fair and perfect.

3. Upon examination of the *caulis* in ſeveral ſections, I find the muſhroom a plant more perfect than has been thought. It has a perfect *radix*; a *caulis* conſiſting of fibres, the interſtices of which are filled up by a parenchymous ſubſtance, leading from the *radix* to the *umbella* or head: it has, as has been obſerved, it's *ſemen* and *ſiliquæ*, and more regular periods of vegetation than is ſuppoſed. The common opinion of a muſhroom's ſpringing up in a night, and periſhing in a day, has no foundation in fact. I have now by me ſome in all ſtates of maturity; ſome of which, to my knowledge, are near a fortnight old, and yet but juſt arrived to a fitneſs for the table.

4. Upon examination of ſeveral muſhrooms, expoſed to the open air, but kept from the injuries of the ſun and rain, I find no *animalcula* bred therein, nor, as yet, a tendency to putrefaction; though they have been expoſed thus for a week. On the other hand, upon examining a muſhroom, very far from being full grown, putrefied by the rain, and moiſture of the dung in the bed, I found *animalcula*, diſcoverable only by the third magnifier, floating in the liquor, ſqueezed out from it: from which I think it evident, that the dangerous conſequences which hiſtory has informed us to have attended the eating of muſhrooms, have not ariſen from any poiſonous quality eſſential to them, but from the accidental *ova* or *animalcula*, which the richneſs of their nutriment has allured to them, and which their contiguity to the ground, and the places they are produced in, render them obnoxious to. Theſe *animalcula* I have lately had an accurate view of; but as they demand a fuller account, than this paper, already too long, will permit, I ſhall reſerve the obſervations upon them for another opportunity of being honoured with the attention of the Society.

However, it may not be amiſs to ſubjoin a ſhort account of the culture in the kitchen-garden of a plant which contributes ſo much to the delicacy of polite tables, which may be depended upon, from perſonal trial and ſucceſs; as thoſe few writers upon the ſubject, not being acquainted with the true muſhrooms, are not entirely to be depended upon.

In the melonry, or place allotted in the garden for hot-beds, the muſhrooms muſt be thus ordered: having marked out a portion of ground one yard and a half broad, and of any length, as the ground will permit; faſten two ſticks at each end of the diametrical diſtance already marked out, which ſhall, by inclining to each other on the top, form an *Iſoſceles* triangle. To the breadth and height of theſe ſticks muſt the bed be made, of old, rich, dry dung, cloſely trod together: neither new nor moiſt dung is proper; for the muſhroom being naturally of a ſucculent and ſpongy contexture, too much heat, and too much moiſture, muſt neceſſarily injure it. Having raiſed your bed to the height and breadth propoſed, cover it with fine ſcreened mould, to the

the thickneſs of three inches, into which, at proper diſtances, put either that white fibrous ſubſtance, which may be collected from the place where muſhrooms have formerly grown; or elſe water it with water in which the chives and parings of muſhrooms have been ſteeped; or you may put in the chives in groſs. If you take the firſt ſtep, the muſhroom is propagated by tranſplantation; that white fibrous ſubſtance, already mentioned, being no other than the *ſtolones* of old muſhrooms, from which others are propagated, like potatoes: if you take the ſecond, that is, by watering; the ſeeds lodged in the parings being, by the water, ſeparated from the *ſiliquæ*, and with it poured upon the mould, are that which gives fertility to the beds thus managed. If you put the chives in groſs into the mould, it is no more than ſowing the ſeeds in the pods, as in other plants it is ſometimes neceſſary to do. Over the bed, thus prepared, muſt conſtantly be kept a covering of long new litter, to the thickneſs of one foot, to preſerve the plant from the froſt, the ſun, and the wind. During the middle of ſummer, and the extremity of winter, it is beſt to make theſe beds under ſhelter; but at other times they are beſt expoſed, the warm rains not a little contributing to their fertility; which, by the ſloping faſhion of the beds, are ſuffered to moiſten them no more than neceſſary.

I ſhall only add, that when I ſpeak of the muſhrooms, as I have all along done, I mean the *fungus* * *poroſus*, *craſſus*, *magnus*, called, by way of eminence, in *England*, the *Muſhroom*.

Remarks *on the preceding paper, with* Obſervations *upon the* poiſonous faculty *of ſome ſorts of* fungi; *by Mr* W. Watſon, *F. R. S.* N°. 473. p. 51. May &c. 1744. *Read* May 31. 1744.

2. I hope I ſhall have the Rev. Mr *Pickering*'s excuſe, if I lay before you a few further obſervations upon his papers concerning *muſhrooms*.

With regard to the ſeeds of *muſhrooms*, although they were never ſhewn to the R. S. before, the fact was known to many Members thereof: for the induſtrious *Micheli* did not only raiſe muſhrooms from their ſeeds, but has, in his tables, ſhewn the daily progreſs from their firſt point of vegetation, even to their perfect ſtate.

The *Fungus poroſus craſſus magnus* is not the muſhroom uſually raiſed in *England* for the table, as this gentleman did imagine; that name being given by *John Bauhin* †, to a ſpecies which is to be diſtinguiſhed from all other *fungus's*, by the inferior ſubſtance not being divided into *lamellæ*, or (what we call in *England*) gills; but has, in lieu thereof, a great many *papillæ*; and being of a greeniſh yellow colour. But what is raiſed in *England* (of which this learned gentleman brought ſeveral ſamples to the Society) is the *Fungus campeſtris albus supernè, infernè rubens*, of *John Bauhin*, which differs *toto cœlo* from the former, and which Dr *Dillenius* enumerates among the ſpecies of *boletus*; whereas the latter is a ſpecies of *amanita*.

I muſt beg leave to differ from this gentleman likewiſe, in regard to the uſe of the ring, which ſurrounds the ſtalk of this *muſhroom*. He imagines it placed there, by the wiſe Author of Nature, to break the

* See the following article. † Hiſt. III. 833.

fall

fall of the feeds when ripe; whereby thofe light bodies may be preferved from the fury of the winds, in order to the abundant propagation of their fpecies. I have reafon to believe, that thofe feeds, which fall upon this ring, fall there by accident; and adhere there only from the vifcofity, whereby they are intangled. But, before I examine this matter, give me leave to make a few obfervations upon the oeconomy of this plant. The *fungi*, then, are of that clafs of vegetables, which are ranged, by that moft fkilful Botanift *Linnæus*, under the appellation of *Cryptogamia*, or thofe which perform their fructification in fecret. Under this head we find the fig-tree, all the fpecies of fern, moffes, mufhrooms, and a few others, whofe flowering and feeding are obferved with more difficulty, than in thofe we ufually call the more perfect plants. In fome of this clafs, the fructification, notwithftanding the great affiftance furnifhed to the modern Botanifts by microfcopes, which the ancient were wholly deftitute of, remains yet undifcovered. This plant then being of this clafs, almoft all thofe whofe ftems are thick and flefhy, as well as their umbels, have a ring upon their ftem; from which, when the plant is young, and until it arrives at a flowering ftate, there arifes a membrane, which connects the rim of the umbel to the ftem, and preferves the under part of the plant in this ftate: but, when this is over, the umbel, which before was almoft of an hemifpherical figure, growing larger, and the membrane not giving way, is loofened from the rim of the umbel, and adheres only to the ftem. Soon after this ftate, the feeds ripen, and the umbel, lofing it's former figure, commences almoft a plane; and the plant in this ftate is fold in our markets, by the name of *flaps*. Now, when the umbel is of this figure, the feeds, being perfectly ripe, muft fall naturally upon the whole fpace the umbel covers (which *Micheli* obferved, by placing leaves of trees under them); and, upon the ring, as well as any other part; though I have reafon to believe not more. As for thofe fpecies of *fungi* whofe ftems are thin, and whofe umbels are foft, and more ductile, they need not, nor have they, this ring or membrane; becaufe, in their tender ftate, the rims of their umbels clap themfelves quite clofe to the ftalk, in the form of a contracted *umbrella*; and expand as the others do, when their feeds are ripe: neverthelefs the fpecies of this tribe are as numerous as the former.

I now come to confider how far the poifon of *mufhrooms* can poffibly proceed from animalcules: but, firft, give me leave to doubt, whether or no any perfon was ever injured from eating the common *mufhroom*, or *amanita*; unlefs fuch accident may have proceeded either from eating too many at once, and thereby overloading the ftomach; or from fome particular diflike in the conftitution; as we fometimes fee, even with regard to honey, cheefe, and fome of the moft innocent parts of our diet; but which, notwithftanding this, are by no means to be ranked among poifons. If there were many inftances of their being pernicious, fuch muft frequently occur to the practitioners in Phyfic, on account

of

of the vaſt quantity annually conſumed in *London*; but I don't remember to have even heard of any ſuch accident; but many inſtances occur of the noxious quality of many of the other ſpecies of this tribe: nor is it at all wonderful, that the different ſubjects of this claſs of vegetables ſhould differ in their effects more than thoſe of the more perfect kind. The roots of carrot, parſnep, and many others of the umbelliferous claſs, are daily uſed as food; but the water-hemlock, and *Lobel*'s *oenanthe*, though of the ſame claſs, are moſt certain poiſons.

Here I muſt obſerve what pains have been taken by Naturaliſts, to diſtinguiſh the uſeful from the pernicious kinds. Among the *Romans*, the *boletus* mentioned by *Juvenal*, on account of the death of the Emperor *Claudius*, is ſufficiently deſcribed by *Pliny*; but, among the later writers, *Carolus Cluſius* was of the firſt of thoſe, who, about the middle of the ſixteenth century, being tired with the Critics and Commentators of the time he lived in, preſumed to believe, that the whole of knowledge was not confined to the writings of the *Greek*, *Roman*, and *Arabian* Phyſicians; becauſe, from the revival of letters in the weſtern world to his time, nothing was regarded, as of any importance, but what was dignified with the authority of antiquity: and hence it came to paſs, that when the clouds of ignorance began to diſperſe, the *epocha* of Commentators took place; but many of the deſcriptions of the plants of *Theophraſtus*, *Dioſcorides*, and *Pliny*, were ſo very deficient, that little light could be acquired therefrom; eſpecially from this laſt author, who is to be conſidered as the only *Roman* Naturaliſt that we have handed down to us; and it is no wonder, if, among the vaſt variety of ſubjects that this moſt admirable Hiſtorian treats of, he is, in many inſtances, rather to be conſidered as an Enumerator, than as a Deſcriber: I ſhall only mention the imperfect ſketches he has left us of *Silaus*, *Geum*, *Molon*, among the many others.

There aroſe, I ſay, ſuch heats and diſputations among the critics upon thoſe authors, very often about trifles, that they rather increaſed than diminiſhed the ignorance of thoſe times. This excellent *Cluſius*, finding that a thorough knowledge of nature was neceſſary, not only to underſtand rightly the Ancients, but to lay the foundation of future knowledge, was deſirous to join careful obſervations of his own to thoſe which were to be acquired from books. How much he travelled, and what progreſs he made in this undertaking, his many valuable works are the beſt teſtimony. Among them, his hiſtory of *fungus's* bears not the *leaſt* character; he therein enumerates a great variety, not only of the eſculent, but noxious kinds; but, as the different appellations of every ſpecies was not, at that time, much conſidered, he gives no other *ſynonyms* to either claſs, than that of, *viz.* *Eſculentorum primum genus*, *noxiorum decimum genus*, and ſuch-like. But this want of ſpecific names has been ſufficiently ſupplied by *John* and *Caſpar Bauhin*, *Ray*, *Moriſon*, *Tournefort*, *Vaillant*; but, above all, by *Dillenius*, in his *Catalogus Giſſenſis*, and by *Micheli*, in his *Nova Plantarum Genera*. In moſt of

theſe

theſe authors we find inſtances of miſchievous effects from the pernicious kinds; which property ſome of them have equal to *opium*, aconite, or henbane; but how far this property proceeds from animalcules, the following inſtance will ſufficiently demonſtrate. We have a ſort growing in *England*, called, by *Caſpar Bauhin*, *Fungus albus acris*; which Monſieur *Tournefort* has rightly obſerved ſtimulates the tongue, and is almoſt as ſharp as though it were ſteeped in ſpirit of nitre; and, being rubbed upon paper dyed blue with turnſole, turns it as red as any violent acid ſpirit will. This cauſtic quality remains even after the *fungus* is dry. We need make no further inquiry for the cauſe of the poiſon in this plant; the above-mentioned is a ſufficient criterion. *John Bauhin* likewiſe tells you, that after having handled this *fungus*, he rubbed his eyes by accident, and brought on a violent irritation upon his eye-lids. *Caſpar Bauhin* mentions a ſort which kills the very flies. *Micheli* deſcribes a ſpecies, which, upon eating them, almoſt killed the Painter he uſually employed, and an old woman, the painter's mother. This man, being ſent by the author to delineate ſome of theſe *fungus's*, and being taken with their appearance, ordered ſome of them to be fried, and he and his mother eat thereof; but were, in about two hours, ſeized with violent pains in their bowels, from which they were with great difficulty relieved. I might produce many other inſtances of this ſort; but the above, I believe, are ſufficient.

Of covering trees with ivy; by R. Gale. *Eſq;*

XXV. By covering up my trees with ivy, in *February* I have vaſt quantities of apricots and peaches, while my neighbours have hardly any.

F. R. S. N°. 475. p. 267. Jan. &c. 1745. *Read* Jan. 24. 1744-5.

Extract of a letter from the Rev. H. Miles, *D. D. and F. R. S. to the* Pref. *relating to ſome improvements which may be made in* Cyder *and* Perry. N°. 477. p. 516. Aug. &c. 1745. *The letter read* Nov. 1745.

XXVI. The deſign of communicating the following paper to the *R. S.* is, to invite Gentlemen, after the example of a practice that has long obtained in *Herefordſhire*, to attempt an improvement of their waſte lands, by planting ſuch kind of fruit-trees, as are mentioned, in hedges and barren places; which, for aught appears, would thrive as well in other counties, perhaps in ſome parts of moſt counties in *England*, as in that of *Hereford*.

Extract from a manuſcript, written Anno 1657-8, *by Mr afterwards, Dr* John Beale, *and F. R. S. in the way of an epiſtolary addreſs to* S. Hartlib, *Eſq*; *for his uſe, and that of Mr* Pell, *the then* Britiſh *Reſident at* Zurich; *and which appears to have been intended as a ſequel to that ſcarce and valuable piece intituled* Herefordſhire Orchards, *inſerted in the later editions of Mr* Bradley's *New Improvement of Planting*, &c.

Concerning an excellent liquor made of

The author undertakes to evince, " That crabs and wild pears, ſuch " as grow in the wildeſt and barren clifts, and on hills, do make the " richeſt, *a mixture of rough* Pears *and* Crabs.

" richeſt, ſtrongeſt, the moſt pleaſant and laſting wines that *England* " yet yields, or is ever like to yield. — I have ſo well proved it al- " ready (ſays he) by ſo many hundred experiments in *Herefordſhire*, " that wiſe men tell me, that theſe parts of *England* are ſome hundred " thouſand pounds ſterling the better for the knowledge of it."

He mentions, of theſe kinds of auſtere fruit, the *Bareland* pear and the *Bromſbury* crab, of which notice is taken page 4th of the Tract intituled *Herefordſhire Orchards*; and intimates, " That tho' the diſco- " very of them was but then lately made, yet they had gotten a great " reputation."—He adds, " The croft crab and white or red horſe-pear " do excel them, and all others, known or ſpoken of in other coun- " tries." Of the red horſe-pear of *Felton* or *Longland* he obſerves, " That it has a pleaſant maſculine vigour, eſpecially in dry grounds, " and hath a peculiar quality to overcome all blaſts."—Of the quality of the fruits he ſays, " That ſuch is the effect which the auſterity has " upon the mouth on taſting the liquor, that the ruſtics declare 'tis " *as if the roof were filed away*;" and that " neither man, nor beaſt, " care to touch one of theſe pears, tho' never ſo ripe." Of the pear called *inny-winter*, which grows about *Roſſe* (in that county) he obſerves, " That it is of no uſe but for cyder; that if a thief ſteal it, he " would incur a ſpeedy vengeance; it being a furious purger; but, " being joined with well choſen crabs, and reſerved to a due maturity, " becomes richer than a good *French* wine; but, if drank before the " time, it ſtupifies the roof of the mouth, aſſaults the brain, and pur- " geth more violently than a *Galeniſt*." This quality, he apprehends, will ſufficiently ſecure the fruit from being ſtolen, tho' the trees ſhould be planted in the moſt remote grounds.

Of the quality of the liquor he ſays, " That, according as it is mana- " ged, it proves ſtrong *Rheniſh*, *Backrac*, yea pleaſant *Canary*, ſugar- " ed of itſelf, or as rough as the fierceſt *Greek* wine, opening or bind- " ing, holding one, two, three, or more years—that no mortal can yet " ſay at what age it is paſt the beſt. This (adds he) we can ſay, that " we have kept it till it burn as quickly as ſack, draws the flame like " *Naphtha*, and fires the ſtomach like *aqua vitæ*." He ſaith, " That " he made trial at his own houſe with wine *d'Hay*, by a Merchant of " *Briſtol* highly extolled, which, compared with a liquor made of crabs " and wild pears, was ſo much inferior, in the judgment of all, that " the compariſon was ridiculous." And he further relates, " That a " Gentleman (Sir *H. Lingen)* a great Planter, and expert in many ex- " periments, had then by him many tuns of a liquor made with this " mixture of fruit, which he, by a deſigned equivocation, called pear- " maine cyder, that carried the applauſe from all palates—that all his " common hedges yielded him ſtore of the ſaid fruit."

To recommend this eaſieſt, cheapeſt, and moſt profitable kind of agriculture, (as he calls it) he ſays, " That the beſt of theſe pears grow " upon very bare and ſandy hills, or vales; crabs on any mound or

" bank

" bank that may be raifed on an heath; that one pear-tree ordinarily " bears yearly 40, 50, 60, 70 gallons of ftatute-meafure, and fome 5, " 6, or 7 times as much. Since I undertook this argument (adds he) " within 10 miles of this place we made in one year 50,000 hogfheads, " as I examined, not by fancy, but by rule and inquiry; and this fhews " the hardinefs of the fruit. Let our noble patriots weigh, that this is " not a thing in the air, but a moft certain and apparent truth, import- " ing no lefs than the art of raifing ftore of rich wines on our common " arable, on our hills, and wafte grounds; the charge a trifle, the pains " very fmall, the profit incredible. Hence my defign is to urge the " incredible benefit that would redound to thefe nations, if leading per- " fons would make themfelves, their tenants and cottagers, all happy " by following our example. I leave the reader to caft up how many " millions of hogfheads of wine, in a few years, would be raifed in the " land. And truly I conceive it the chief caufe, that, in all thefe times " of late wars, none of our pooreft cottages did fee want; in all houfes " they had the fame number of meals, and the fame conftant fare: our " arable feems not a jot the lefs, nor our pafture the lefs; and for fome " ufes the fhadow of the orchard brings on the grafs a fortnight the " fooner, as commonly for ewes and lambs."

The author concludes his tract with thefe words, " If this Difcourfe " be duly valued, we need not raife wars to deftroy one another, or eat " up one another, as we do; in a fhort time we may be provided of " fruit enough for another world as big as this, and to make this a true " Paradife."

XXVII. Mr *Bonnet* was inclined to try whether plants were capable of vegetation, when they were only fet in mofs, inftead of being planted in the earth. With this defign, he filled with mofs feveral garden pots, and he compreffed the mofs more or lefs, as he judged, the feveral plants he intended to place in them, might refpectively require a clofer or a loofer foil.

The fubftance of fome experiments of planting feeds in mofs, lately made by Mr Charles Bonnet, *of* Geneva, *F. R. S.* N°. 486 p. 156. Feb. & Mar 1748. *Read* Feb, 18. 1747-8.

He then fowed in mofs, wheat, barley, oats, and peafe. And he found, firft, that all the grains fowed in that manner came to maturity later than thofe of the fame forts which were fowed at the fame time in mould.

2dly. That the ftems from the feveral grains fowed in the mofs were generally taller than thofe which fprung from the ground.

3dly. There came from the grains fowed in the mofs a greater number of blades than from the grains fowed in the earth.

4thly. The grains fowed in mofs produced more plentifully than the others.

5thly. Thofe grains that were gathered, from the produce of thofe which vegetated in the mofs, having been again fowed fome in mofs, and fome in earth, fucceeded well in both.

Mr *Bonnet* has also planted in moss, pinks, gillyflowers, daisies, tuberoses, tulips, hyacinths, jonquils, and narcissus's; and all these plants succeeded as well as others of the same sorts, which he at the same time planted in mould.

He also placed in moss cuttings and layers of vines, and these cuttings and layers became vines; and these vines in a short time grew larger than others, that came from cuttings and layers planted at the same time in the ground.

A letter from the Rev Mr R. Pickering, *F. R. S. to the* Pres. *concerning the manuring of land with* fossil shells. N°. 474. p. 191. June &c. 1744. *Read* Dec 6. 1744.

XXVIII. I take the liberty of offering, by your hands, to the *Society*, a specimen of fossil shells, lately sent me, which are pretty perfect; and, on account of the place from whence they were taken, remarkable. At *Woodbridge* in *Suffolk*, in a Farmer's ground, there are some pits, in depth equal to the usual height of houses, consisting of several *strata* of shells from the bottom to within about 9 feet of the surface, where the natural soil of gravel and sand begins. The mass of shells here collected is prodigious; the sorts various; but that kind which I have taken the liberty to produce, and which, I apprehend, is the *buccinum vulgare*, or whilk, prevails the most. The shells before you were taken up from the bottom of the pit, where the depth to which these shells reach is not yet dug down to. *Woodbridge* is seated 7 miles N. E. from *Ipswich*; and is about the same distance from *Orford* on the sea-coast, which bears from it due E. How, therefore, such a mass of shells should get there at such a distance from the sea, when history has informed us of no remarkable inundation in those parts, or that such a tract of land was ever recovered from the sea, appears to me difficult to determine, by any other than the *Mosaic* hypothesis of an universal Deluge. 'Tis true, indeed the river *Deben*, which rises at *Debenham* some miles off, runs by *Woodbridge*, within ¼ a mile of these pits, in it's course to the *German* ocean, where it empties itself: but such a collection of shells can hardly be supposed to have been thrown up by it, and a surface of earth, to the depth of 9 feet, settled over it, without allowing a space of time for such a circumstance, almost equal to the interval between us and the Deluge. But, however these things be, the Farmer, in whose ground these shells are, has, as I am informed, laid the foundation of an ample fortune from them. The man contented himself in the old beaten track of the Farmers (a behaviour which does infinite prejudice to the improvement of Natural Knowledge in Agriculture), till an happy accident forced him upon a bold improvement. He used to mend his cartways, when broken up by harvest-work, with these shells; in which business his cart one day broke down, and threw the shells out of the cart-track into the cultivated part of the field. This spot produced so remarkable a crop next year, that he put some loads upon a particular piece, kept the secret to himself, and waited for the event. This trial answering expectation, he directly took a lease of a large quantity of poor land, at about five shillings the acre; and having manured

manured it heartily with these shells, in about 3 years it turned to so good an account, that he had 15 shillings the acre profered to take the lease out of his hands. I know that manuring land with shells, those of oysters in particular, is no novelty: I mention this with regret, as an instance of what poor hands, both as to landlords as well as tenants, agriculture, an extensive branch of Natural Knowledge, is generally thrown into; which both requires and deserves the close attention of a philosophical mind.

A PAPER omitted.

A summary of some late observations upon the *Generation*, *Composi-* N°. 470. p.
tion, and *Decomposition* of *Animal* and *Vegetable Substances*; in a letter 615.
to *M. Folkes*, Esq; P. R. S. by Mr *Turbeville Needham*, F. R. S.

The End of the Second Part.

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