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XI. Part of a letter from Mr Will^m Anderson, J. L. S. to Mr Henry Baker, J. L. S.
concerning the formation of pebbles + Aff. N^o. 51.

Dear Sir,

In my late Searches after Sand, 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 lye together or very near each other: but as Bodies of such Hardness could not be broken without some comprehensible Force or Violence, their Situation implies, that they suffered such Force or Violence as broke their Parts asunder, in or near the very Place where they at present lye.

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 distant Place from where they now lye, 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 tumbled backwards and forwards
against

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 Cortex, which surround each other, somewhat like the different years growth in Trees, that they must once have been complete and entire: 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 sheweth 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 ten or a dozen 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 twenty 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 mixt, the more beautiful the Pebbles themselves are, however different their Colors 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 compound^{ed}, or made up of Sands, Clays,

Mosses, 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 ^{into 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 ~~all~~ other Countries may not be very different.

Believe me, Sir,

Yours, &c.

Wm. Addison.

Norwich. Mar. 3. 1745/6.

Apr. 2. 1747.

For Gr. n^o 483. XI.

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XV. Abstract of a Letter from Mr Wm. Bideron, J. R. S. to Mr Henry Baker, J. R. S. containing some Observations made on the Banstiele, or Pricklebag, alias Prickleback, and also on Lish in general.

Dear Sir,

Many of my Leisure Hours last Summer were employed in attending and making Observations on several kinds of Lish; some whereof I with great Care have preserved alive in Glass Jars for many Months together.

I sent you some Time ago a brief Account of what I had observed remarkable in the Dace and Ruff, and am now going to lay before you what I have thought worth Notice in that little common Lish called the Prickle-back, ~~a Picture whereof, the size of one fully grown, you will find herewith inclosed.~~

About the Beginning of last April I took a Banstiele out of our River, full of Spawn, and put it into one of my Glass Jars, at the Bottom of which I had placed a small Quantity of Sand, as I ^{ways} do in every Vessel wherein my Lish are kept; and about the 20th of May it buried its Spawn in the said Sand. I was in hopes this Spawn would have produced a young Brood, but was unluckily disappointed; which I impute to its being frequently disturbed by the pouring in of fresh Water.

For some Days after I had caught this Banstiele, it refused to eat any thing I could offer it, as is common with all Lish I have yet kept: but frequently giving it fresh Water, and coming often to it, it became so familiar as to eat small Worms I now and then threw into the Jar, and from that Time grew so tame as to take them out of my Hand: nay, it became so bold at last, that when its Belly was full, or it did not like what I offered, it would set up its Prickles, and with its utmost Strength make a Stroke at my Fingers, if

I put them into the Water to it.

This Fish was of so unsociable a Disposition, that it would suffer no other Fish to live in the Jar with it, and so audacious as to attack whatever I put in, though Ten Times its own Size.

One Day, for the sake of Diversion, a Friend being then with me, I put a small Kuff into the Jar to it, which the Banstick immediately assaulted and put to flight, having in the Conflict torn off a good Part of its Tail; and would, I dare say, have killed it, had I not separated them very soon.

Infinite Numbers of these Brickle-backs are to be found in almost all fresh waters, where-ever tis possible for Fish to live; and whatever other Kinds the Water is replenished with, this certainly is one, as far as I have yet had Opportunity to make any Enquiry.

The Endeavours they use, and the Ability they have to get from Place to Place, is also extraordinary: for though the largest of them scarce measures above two Inches in Length, I have seen some of them leap out of the Water a foot high perpendicularly, and even much farther in an oblique Direction, when they wanted to get over Boards, or Stones, or some other Obstacle to their Passage.

It is scarce to be conceived what Damage these little Fish do, and how greatly detrimental they are to the Increase of all the Fish in general amongst whom they inhabit. For it is with the utmost Industry, Sagacity, and Greediness that they seek out and destroy the Spawn of all Sorts of Fish; and moreover, all the young Fry that come in their way, are pursued by them with the utmost Eagerness, and swallowed down without Distinction provided they are not too large. And in proof of what I here assert, I must assure you, that the Banstick beforementioned, in my Glass Jar, did, on the 4th of May last, devour, in five Hours Time, 74 young Dace, which were about a quarter of an Inch long, and the thickness of an Horse Hair.

Two Days after it swallowed 62, and woud I am perswaded have eat as many every Day, could I have procured them for it.

Courte Gentlemen who take Pleasure in high ponds intirely prevent these destroyers from getting into them, I am convinced their Produce woud be much greater than it commonly is: and though it may not be possible to keep them out intirely, tis most certainly adviseable to be very Diligent in the destroying of them: and whenever by Netting or other means any of them are got out of the water, never throw them in again, on a Supposition of their being harmless.

Nature has furnished this little fish with a Kind of Breast Plate or Armour, to be its Defence against any outward Injury: She has likewise bestowed upon it several offensive Weapons or Spines, placed upon its Sides and Back, which it immediately erects upon the least Appearance of Danger, or when it attacks some other fish. The sharpness of these Prickles guards it well enough from larger Animals, that might otherwise prey upon it: but neither these, nor all the Endeavours it can use, are able to free it from an Enemy that torments it even to Death; what I mean, is a Kind of Louse, of an oval Figure, having eight Legs, and a very transparent Body; which is able either to swim or crawl, and sticks on it so fast, sucking and plaguing it all the while, that it makes it almost mad.

One remarkable Particular in this louse is, that its little fibrillous Sins are always in Motion, whether the Creature be swimming about or fixed upon the fish.

~~A Drawing of this Louse, greatly magnified, is hereto annexed.~~

All fish regulate their Times of eating and Abstinence, by the Temperature of the Air, and the Quarter from whence the Wind blows; and woud those Persons who are Lovers of angling take the Pains to keep a few small fish in Glasses, they might at any time easily foretell, from their

their taking or refusing food, what Sport is to be expected, and often save themselves many a weary Step taken to no purpose.

I have always observed amongst the Fish I keep in Jars, that such as have lived a while together, contract ^{great} an Affection for each other, that if they are separated, they become melancholy and Sullen, and are a long Time before they forget the loss.

About Christmas last I put two Ruffs into a Jar of Water, where they lived together until April: when, at the Desire of a Friend, I gave one of them away. After this Separation, the Ruff that remained with me was so affected, that for three Weeks it would eat nothing I could give it: and therefore fearing it would pine to Death, I sent it to the Gentleman on whom I had bestowed its Companion; and what is very extraordinary, upon being put together again, it eat immediately, recovered its former Briskness, and both of them are still alive.

I have made abundance of other Observations on Fish, but shall only add at present, that when they remain supine and inactive, they every now and then gape and yawn, as most Land Animals do, when weary of the Situation they are in.

I remain, Sir, Yours etc
J^m Roderon.

Norwich. July 9th 1746.

Ms. B. 1. 482. 11.

X. Abstract of a letter from Dr D^m Anderson, J. R. S. to Dr Henry Baker, J. R. S.
concerning the perpendicular Ascent of Eels. Norwich. July 9th 1746.

Sir,

When I read, some years agoe, what Dr Plot in his History of Staffordshire relates, concerning the Passage of Eels across Meadows, in the Night Time, from Pond to Pond, I could hardly forbear thinking, that the Gentleman there mentioned, must, by some means or other have been deceived: but what I have lately seen with my own Eyes, gives me great Reason to believe his Account to be strictly true.

On the 12th Day of last June, whilst I was viewing the Flood-gates belonging to the Water-works in this City of Norwich, I beheld a great Number of Eels sliding up them and the Posts adjacent, notwithstanding they all stood perpendicular to the Horizon, and 5 or 6 feet above the surface of the Pool below the Water-works. They ascended these Posts and Gates, until they came into the Dam above; and what makes the Matter appear still more strange, they slid up with the utmost Facility and Readiness, though many of the Boards and Posts were quite dry, and as smooth as a common-Plain had left them. I observed, that at first, they thrust their Heads and about half their Bodies out of the Water, and held them up against the Wood-work for some Time: I imagine, until they found the glutinous Matter, which is constantly about their Bodies, become sufficiently thick or viscid, by being exposed to the Air, to sustain their Weight: then would they begin to ascend directly upwards, with as much Ease, seemingly, as if they had been sliding along the level Ground; and thus they continued to do, until they had got into the Dam above.

Jan. 29. 1746-7.

Mr. Gr. 482. X.

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- T. A. H. -

XVI. A Supposition how the white Matter is produced, which floats about in the Air in Autumn; in a Letter from Mr Addison to Mr Baker. Norwich Aug^r 28th 1746

Sir,

Having lately a large Spider in my Hand, by chance I let it fall; and it hung by its Thread, as they very commonly do. On holding my Hand very still, it readily ascended up it again: And thus by giving it a Shake, and then holding my Hand still, the Spider ascended and descended a great many Times. — I thought at first, it had spun a new Thread at every descent, and was desirous to have measured how long an One I could cause it thus to spin; but upon a stricter Examination, I very plainly perceived, that when ever it ascended, it wound its Thread with its Feet into a sort of Coil, and when it descended, only unravelled it out again.

The Manner how they perform this is diverting enough; but as Spiders may be had almost in every Place, and the Experiment is so easily tryed, I shall forbear describing it; and only add, that as these Coils of Thread are ~~not~~ exactly like those floating in the Air towards the End of Summer, I think tis not improbable those are made in the same Manner, when Spiders have a Mind to direct their Course in the same Direction their Threads lye.

Feb. 26. 1746-7.

pr. Gr. 482. XVI.

Dear Sir,

Of all the several kinds of Fish which for some years past I have been keeping in Glass Jars, (in hopes of becoming acquainted with the Nature and Properties of these Animals by having them daily under my Inspection,) none seems more impatient of Imprisonment, if I may so call it, than the Roach. Nor, if they are well looked after, and supplied often enough with fresh Water, have I observed any, except the Roach, to become distempered. But most commonly, after this Fish has been a little while confined, the finny Part of its Tail begins to drop off Piece by Piece, and when the finny Part is all gone a Sort of Mortification seizes upon the Tail itself, and gradually creeps along until it reaches the Intestines, at which Time the Fish immediately dies.

The last Roach I had under this Disorder was about the Beginning of January: when in the Space of a Month it had lost the greatest Part of the Fin, which induced me to clip off the rest, hoping thereby to stop the Progress of the Mortification. But this was of no manner of Service that I could perceive: the Distemper still gained Ground, and as it increased, a fine fibrinous Substance grew out from it, and appeared like what the Picture shews, at Fig. I.

These Fibrils when examined by the Microscope shew themselves to be a Number of minute Tubes filled with a brownish Liquor, and this liquor upon ~~pressing~~ pressing them becomes immediately discharged.

A small Piece of this Fish with the Fibrils growing out of it, as seen by the fourth Magnifier of Mr Cuff's Double Microscope, is shewn at Fig. II.

When first I perceived this fibrous Substance enveloping the Fish's Tail, I supposed it to be nothing but a Mortification, of that kind which frequently is seen upon decayed Flesh and Fish; but upon Trial, I found it to be of a much stronger Texture and Consistence

12.99 1848 +
Consistence than such Montinefs is ever known to have; for notwithstanding I have several Times let a full Stream of Water run upon it from a Lock, I could never wash it off.

This poor Fish lived with me till the latter End of March, and then died, having for many Days before its Death lain at the Bottom of the Jar, without being able to rise.

As the Mortification advanced, and came nearer to its Intestines, the Quickness of its taking Water in at the Mouth increased, till at last it took it in three Times faster than a lively strong Fish did.

On my cutting off Part of the Fish's Tail in Hopes of stopping the Mortification, the Equilibrium of the Body was so far lost, that it hung in the Water most commonly with the Head downwards, and could never afterwards continue in any other Posture without great Strugglings, or sinking down to the Bottom of the Vessel. Which may serve to shew how nicely and wonderfully the Bodies of Fishes are balanced, for the keeping them in an horizontal Position, since in this Case the losing a few grains of the Tail could so sensibly destroy the Equilibrium as to render the rest of its ~~Life~~ almost useless.

I dare not however assert it will happen thus to all Sorts of Fish on cutting off the Tail, nor does ^{it} to the Roach immediately; for as it is a Posture very unnatural and troublesome to ~~fishes~~ ^{fishes}, they exert all their Strength to prevent their Heads from sinking downwards, until being wearied out, they at last are forced to Submit.

I remain, Dear Sir,

Your most obed^t servant

Will^m Asterson.

Gorwich. / April 20th 1748.

May 12. 1748.

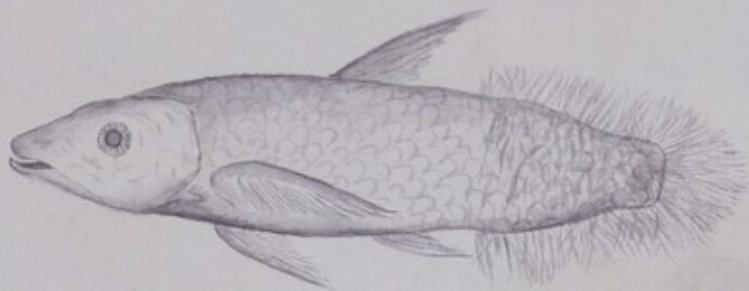
ps. Ser. 487. VIII.

A Distemped Roach.

Fig. II.



Fig. I.



May 12. 1748.

78977

Some Account of an ancient Seal; in a Letter from Mr Wm Anderson to
Henry Baker, F.R.S.

+ MS. N^o 44.

Sir,

Wesenhams

At ~~Wesenhams~~ All Saints, an ancient Country Town, 11 Miles from Lynn, and
21 from Norwich, as some Labourers were fitting up a Well, belonging to a Manor
House formerly standing there, and called (if I am rightly informed) North-Hall Manor:
they found an Original Seal, belonging I believe to the Bishop of Candida Casa, or
Whithorn*, in Scotland. The Inscription round it, if I read right, contains the
following Words.

Sillium cohunne dei gracia Nicolai
Episcopi de Cancafa.

But whether I am mistaken or not in my Reading, the Drawing of the Seal,
which you receive herewith, is a very exact Picture both of its Inscription and Figure;
and if I am wrong may be a means of rectifying my mistake.

At what Time this Bishop resided at ^{Wesenhams} ~~Wesenhams~~, whether he resided there at all,
or by what Accident this Seal should be brought to the Place where it was found, are
Enquiries naturally to be made on this Occasion: but all the Pains I could take to ob-
tain some Information, have been entirely fruitless.

* belonging to the Earl of Leicester.

Q Whitehorn in Gallway in Scotland.

n
march 19. 1746-7.

N^o 13

To Martin Folkes Esq. President of the Royal Society.

+ MS. 78977

Sir,

As the Inspection and Study of Nature is the particular Province of the Royal Society, and every Attempt to improve our Knowledge is certain of your Favour, I take the liberty to lay before you the Substance of a Letter from my industrious Correspondent and Friend Mr William Bosseron, S. L. S. containing the Description of a large Vault or Cavern, extended under several Hills near the City of Norwich, with some Observations and Experiments made by Him there.

About a Quarter of a Mile from the City of Norwich, on the East Side thereof, and near the Entrance of Moushold Heath, is a large subterraneous Cavern, which has been formed in a long ~~time~~ Series of Time, by the digging out of Chalk for the making of Lime. There's but one Entrance into it, whose Breadth is about two Yards, & its Height nearly the same, ^{however} the Height gradually rises, till at last it measures in some Places from twelve to fourteen Yards. ~~in Height~~. But notwithstanding the Entrance is so small, the whole Area within, is of such a large Extent, that twenty Thousand ~~Men~~ Men might with great Ease be plac'd therein, as I believe will scarcely be doubted, when I assure you, that from the Entrance to the farthest Part ~~the~~ of these darksome Cells, measures full four Hundred Yards, & that these Passages are frequently ten or twelve Yards wide, with Branchings out on the Sides, into various Lanes & Labyrinth-Kind of Windings, that every now and then open into one another; which renders it no easy Task to find the Way out, when a Person has been a little bewilder'd in these subterraneous Mazes. Most of these Vaults are arched at Top, whereby the immense Weight which every Moment presses on them, is well supported; a Weight no less than that of Hills, whose perpendicular Altitude above

Here.

the Tops of these Arches is twenty or thirty Yards, if not much more. I have frequently, says my Correspondent, gone into these Caverns out of Curiosity, but could never perceive the least Appearance of those Damps which are so common in Mines & other subterraneous Places, where the Air is stagnant for Want of a due Current, which ^{should} seem to be the very case here, as there is but one Entrance into it. The Passage indeed lies Horizontal, & open to the West Wind, but the included Air's being free from Putrefaction may possibly be owing to the large quantity of Salt which the Chalk contains.

How deep or thick these Rocks of Chalk are, no one, so far as I can find, can tell, for in sinking the lowest Wells, they have never, that I know of, been dug thro', & consequently must be exceeding deep. The Chalk at the further End of this Cavern is so very soft, that it may be moulded with the Hand like Paste, which I take to be it's original Consistence, & what it always retains till it becomes expos'd to the Air; In the very lowest Parts of these Vaults I have pick'd up several Kinds of Fossils, figur'd by Marine Bodies, such as Echini, Pectunculi, common or fluted Cockle, Belemnites, &c. & 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 twenty Yards from where it was plac'd.

I visited this Place on the 1st Day of Nov^r last, in order to try the Temperature therein, as to Heat & Cold, & carried with me a Thermometer regulated by one of M^r Hawksbee's, which I set down at the further End of these Caverns, & 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 $\frac{1}{2}$ a Degree of a Medium betwixt the greatest Heat & the sharpest Cold we have known in this City for ten Years past, & it's 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 & Cold in this Climate.

The greatest Degree of Heat was July. 18 th . 1746	Hawksbee's Therm. 15°
The greatest Degree of Cold was January 9 th . 1740	88
Which added together make	103
The Medium of which is	51 $\frac{1}{2}$

This

This Method of computing the Temperate or Medium State of the Air with Respect to Heat and Cold, may possibly be found as reasonable as any one yet made Use of. The common Way of placing the two Extremes at freezing & boiling, & taking the Middle between them for the temperate Point, may, I think, be subject to Objection, there being many Degrees of boiling Heat, as well as freezing Cold, which I think must prevent any real Certainty by that Way of Computation.

I find by inspecting M^r George Martin's Collection & 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, & 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 expos'd to the open Air, it stood at 57° .

Permit me, Sir, to subscribe myself, with the utmost Truth & Respect,

Your most obedient humble Serv^t.

London. Mar: 15th 1747/8

J. Baker

XV. an account of large Subterraneous Caverns in the chalk
Hills near Norwich by M^r W^m Aiden F. R. S. comprised in
a Letter from M^r Henry Baker F. R. S. to The President.

March 24. 1797-8.

Pr. Fr. 486. XVI.

In Obedience to the Commands of this illustrious Society, I have carefully examined with the Microscope those Pieces of Ash Tree Wood presented lately by Mr Benjamin Martin, with Design to shew the Structure of the said Wood and the Arrangement of its Vessels, and find them nothing different from that curious and elegant Figure which the laborious and accurate Dr Grew has given of this same Wood, in his Anatomy of Plants, Table 29.

I have likewise well considered Mr Martin's Observations on the Structure of Wood, in his Letter to the President, which was lately read before this Society, and find nothing to object thereto, unless where he asserts, that there are no lateral Vessels in a Plant by which the Air can pass: whereas Dr Grew has shewn, that the Air enters in at the Trunk, as well as at the Root and Leaves, and that the Pores are so very large in the Trunks of some Plants, particularly in the better Sort of Walking Canes, that they are visible to a good Eye without a Glass, but with a Glass the Cane seems as if stuck full of Holes with great Pins. Anatomy of Plants pag. 127. And of these he gives a Drawing, Tab. 20. In short, this Subject seems to be new to Mr Martin, and the Pleasure it gave himself might probably induce him to believe we should be no less pleased with what he thought a considerable Discovery, We

are therefore obliged to him for his good Intention and for his Trouble.

I am, and shall ever be, ^{Yours} the Society's

Most faithful & obed^t ^{to} the Servant

London. March. 21st 1747/8.

J. B. Aker.

no. 4.

March 24. 1747-8-

To Martin Folkes Esq. President of the Royal Society. N^o. 18.

Sir,

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 farther 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 can not perform. Foreigners, on the contrary, seem fond of believing, that the subtle electric Fluid, (be it Fire, Ether, 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 Abbe 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 thereby promoted, 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 subtle and active Effluvia of useful Medicines either into the whole Body or into some distempered Part. — ^{an abstract of the preceding paper} Mr Walton read last Thursday before the Royal Society, an Account sent ^{to} from Leipzig, by Professor Winckler, of several Experiments to this purpose, made at Venice, by Dr. Pirati and repeated afterwards by himself at Leipzig with the same Success. He gives Instances of saturating the Body, by Electrification, with the Effluvia of Balsam of Peru, and of Sulphur, so as to produce very remarkable Effects; and of taking a Bit of the Gout away intirely, by conveying into the Part afflicted the sanative Effluvia of warm and discutient Drugs.

My ingenious Friend Dr Joseph Brunni, one of the principal Physicians at Turin, and Fellow of our Royal Society, has likewise sent to me, an Account lately received by him, of Experiments made at Rome and at Bologna; which I now, Sir, 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; and 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 the French ^{Language}, 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 Pain whenever he lay with that Ear uppermost, coming to Dr Veratti for Advice, the Dr Electrified him, bringing out abundance of fiery Sparks around the distempered Ear, which in about five

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 three Hours it was entirely gone, and has never returned since.

A Woman that nursed one of the Doctors 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~~ ^{constant} Pain over the Eye-lid, came to the Doctor for Advice, who immediately electrified her, bring^{ing} out the fiery Sparks about the Eye and Eye-Lid, whereby the Eye appeared very much Bloodshot, 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 ~~Watering~~ watery Humour and Pain were much diminished, and the Doctor hopes ^{that} by repeating the Operation twice more he should be able to cure her quite.

Doctor 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 Distemper.

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 his 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 Phenomena 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 subtle and active Effluvia of other Substances; and if they can do so, the Effects suggested are ^{wholly} not impossible; for several Experiments have proved, that a very minute Quantity of Medicine, transfused directly into the Blood and circulating Shins, will have the same Effect as a large Dose 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 Phenomena 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 at present may seem to do, had Accounts of them been sent us from Rome, Venice or Bologna, and had we never experienced them Ourselves.

I am proud to seize every Occasion to assure you, with what great Respect I

am,

Sir,

Your most faithful and Obedient
little Servant

Strand. March 28th 1748.

W. A. B. 1748

[1698-1794]

XVIII. a letter from Mr. Henry Baker F. R. S. to the President
concerning several medical experiments of Electricity

The first experiment was made on the 1st of January 1794. It consisted in passing the electric fluid from a glass tube charged with electricity, through a series of small holes, into a series of small vessels, each containing a different fluid. The result was, that the electric fluid, when it passed through the holes, it produced a series of small sparks, which were observed to be of a different color, according to the nature of the fluid in the vessel. The second experiment was made on the 2nd of January 1794. It consisted in passing the electric fluid from a glass tube charged with electricity, through a series of small holes, into a series of small vessels, each containing a different fluid. The result was, that the electric fluid, when it passed through the holes, it produced a series of small sparks, which were observed to be of a different color, according to the nature of the fluid in the vessel.

no. 24

March 31. 1748.

Mr. Dr. 486. XIX.

A Tophus or Hair Ball formed in the Stomach of a Sheep.
Shewn by Mr Baker.

This, like all such Kind of Productions, is composed of Wool, licked in by the Sheep, pressed together by the Action of the Stomach, & ~~concocted~~ by the Juices thereof.

Masses of Hair, Wool, &c. are often found in the Stomachs of Animals; but what makes this Ball uncommonly remarkable, is the exact Regularity of its Figure, and the Similitude of its Surface to fine Woollen Cloth; And a still more extraordinary Particular is, that seven Balls of the like Shape, Size, and Consistence, were taken out of the Stomach of the same Sheep. Three of ~~them~~ which I saw, were as much alike as possible; and the other four, which had been distributed by the Person who brought them all from the Cape of Good Hope, where the Sheep died, not of Distemper but by the Butcher's Knife, were, I was assured, Nothing different.

The other Ball was taken out of the Stomach of a Calf, is compos'd of Hairs which the Calf had licked up, & coated by the Mucus of the Stomach.

may. 19. 1748.

V. Observations of an Occultation of Cor Leonis by the Moon on Thursday March 12th, 1747 in Surry Street in the Strand London, with a Reflecting Telescope made by M^r Short ^{S.R.} 10th magnify'd about 100 Times. communicated to Apparent Times the Royal Society by J. Bevis M. D.

1747. Mar. 12. 8^h. 24^m. 19^s The Star immerg'd into the dark Limb.
 9 . 27 . 4 It emerg'd from the enlighten'd Limb, a small matter to the West of the Moon's Zenith.
 44 . 4 $\frac{1}{2}$ The Moon's preceding Limb pass'd of Meridian, in the Transitory.
 44 . 21 The Star pass'd the Meridian.

M^r Short, another Gentleman, and myself, agreed to a single Second in the Immersion, with different Telescopes; but I saw, and pronounced the Emersion 2 or 3 Seconds before them. There had been an exact observation of the Sun's Transit at Noon, and the Clock gain'd about half a Second a Day.

We reckon Surry Street 27 Seconds in Time West of the Royal Observatory at Greenwich.

M^r John Catlin had a few days before deliver'd me a Computation of this Eclipse, corrected from two places of the Moon observed the 28th of Feb. and the 2^d of March, 1729. corresponding pretty nearly with her present Situation; as likewise from the Stars Position, as I had rectify'd it from several late Observations: And this gave the Immersion at - - - - - 8^h. 26 $\frac{1}{2}$
 Emersion - - - - - 9 . 30

5403/11

$\frac{1}{2}$

J. Bevis

Mar 19 - 1747.

March. 19. 1746-7.

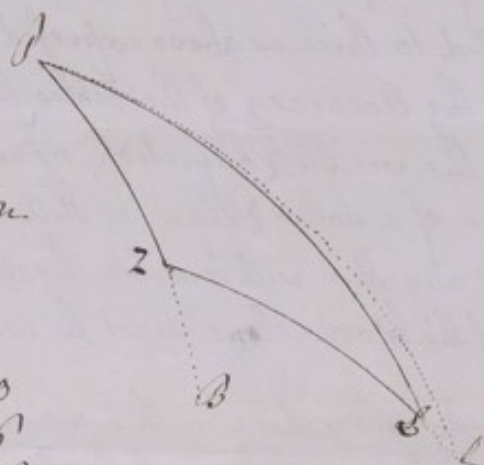
per. Gr. 483. V.

An Occultation of Cor Leonis by the Moon the 12th of March 1767 in the Evening when the Place of the Star is $\text{Dec } 26^{\circ} 19' 00''$ with the Latitude North $52^{\circ} 38'$ according to the British Catalogue, but $26^{\circ} 18' 30''$ Lat. N $27^{\circ} 35''$ according to D^r Bevis.

	Apparent Time	Apparent Time	Apparent Time
	^h ^m ^s	^h ^m ^s	^h ^m ^s
At	8 23 15	8 53 15	9 23 15
True Place of the Sun	γ 2 50 26	γ 2 51 31	γ 2 52 48
His Right Ascension	2 36 15	2 37 23	2 38 30
Time since Noon in Degrees	125 18 45	133 18 45	140 18 45
Right Ascension of the Merid. Soli	128 25 0	135 50 8	143 27 15
The Moons True Place in Ecliptic	Ω 25 41 10	Ω 25 58 00	Ω 26 11 17
Latitude North D ^r	50 51	49 20	47 48
Declination North	13 46 44	13 39 35	13 32 27
Right Ascension	148 14 50	148 30 43	148 46 26
Horiz: Parallel	57 43	57 43	57 43
Horiz: Semi Diam	15 40	15 46	15 46
Angle at the Pole between the Moons Meridian & that of the Tables suppos'd Greenwich.	19 49 50	12 34 35	5 19 11

Having thus got the above Heads of Calculation the Remaining are obtain'd as follows & for Example let it be for the first Point of Time above mention'd i.e. 8 23 15 P.M.

Let P represent the Pole
Z the Zenith and
PZ Part of the Meridian of the Tables
L the true & L the visible Place of the Moon in the same Vertical Circle ZL



In the Triangle ZPL are given
PZ the Distance of Zenith & Pole 38 31 30
PL Complement of the Moons Dec $70^{\circ} 13' 16''$
& the Angle at the Pole ZPL 19 49 50

To find
ZL Dist: of the Moon from the Zenith 40 56 28

and

And the Angle $\angle ZP$ or its Complement $\angle ZL$ the
Azimuth from the North or South $30^{\circ} 11' 18''$

By the given Horizontal Parallax of the Moon ~~given~~
~~above~~ & her Distance from the Zenith just found
her Parallax in Altitude, or the little Arc $\angle L$ is
found. $37^{\circ} 19'$

Consequently the visible Dist: from the Zenith ZL $1^{\text{h}} 3^{\text{h}} 17'$

There is now a new Triangle form-
ed in which are given.

The Side PL common to both $38^{\circ} 31' 30''$
 ZL just found $1^{\text{h}} 3^{\text{h}} 17'$

& the Angle $\angle ZP$ or its Comp: $\angle ZL$ also common
to both $30^{\circ} 11' 18''$

To find.

The Side PL the Comp: of the visible Declination $70^{\circ} 19' 5''$

& The Angle $\angle ZPL$ the new Angle at the Pole $20^{\circ} 2' 28''$

Which added to, or subtracted from the Right
Ascension of the Medium Celi as the Moon
shall be East or West of the Meridian gives
her visible Right Ascension.

$11^{\text{h}} 8^{\text{h}} 27' 28''$

From this & her visible Declination just found her visible Longitude
& Latitude are easily computed, & are express'd hereafter.

By this Method the visible Place of the Moon being obtain'd to as many
Points of time, the more & the nearer to each other the better, (in this Case I have only
computed to three as above express'd) her true visible Path will be had proportiona-
ble to the Accuracy of the Tables from whence her Motions are computed, from
which the visible Conjunction, nearest Approach of the Centres, & all other Circum-
stances of a Solar Eclipse, or Stellar Occultation are easily found, only when the
Sun or any Star with sensible Parallax is concern'd the Difference of Parallax in
stead of the Moon's alone must be used.

By proceeding in this manner at $8^{\text{h}} 53' 15''$ & $9^{\text{h}} 23' 15''$ the respective & cor-
responding Circumstances of the Triangles ZPL & ZPL are obtain'd, & which with those
at $8^{\text{h}} 23' 15''$ are as follow.

	At	^h 8 ['] 23 ["] 15	^h 8 ['] 53 ["] 15	^h 9 ['] 23 ["] 15
The Moon's Altitude	49	3 32	50 50 52	51 49 23
Parallax in Alt		37 49	36 26	35 40
Azimuth from South	30	11 18	19 31 11	8 23 6
vis: Altitude	18	25 43	50 14 26	51 13 43
vis: Declina: Nor	13	10 55	13 1 0	12 56 55
vis: R. Ascension	118	27 28	118 38 14	118 49 50
vis: Longitude	26	1 17	26 17 28	26 30 2
vis: Lat N D		21 17	18 31	15 33

At 8 ['] 53 ["] 15 P.M	Flamsteed	Devis
Wanting to the visible &	1 32	1 2
visible Hourly Motion of D	25 15	25 15
Interval in Time add	3 39	2 28
Therefore the visible & at	8 56 54	8 55 43
Moon's visible Lat: at that time	18 10	18 17
Centre from the *	8 28	9 18
Angle of her Visible Way add	12 17 10	12 17 10
Dist: at the nearest Approach	8 15	9 1
Interval in time between that		
and the vis: & subtract	1 26	1 52
Therefore nearest Appr: or Middle	8 52 29	8 50 51
Moon's Sim: Diam: Altitude 50	16 12	16 12
Half Chord of the D under the		
Path of the Star	13 57	31 53
Therefore half Duration	33 9	31 53
Hence		

Immersion or Beginning 8 19 19

Emersion or End 9 25 37

From two Places of the Moon observ'd the 25th of February & 2^d of March

1729 corresponding pretty nearly with her present Situation in her Eye when her Error in Motion deduc'd from the Tables was found about 3 Minutes in Motion. If therefore we put down the Times of this Occultation as below it is imagin'd they can't be far from the true ones.

	Flamsteed	Devis
Immersion	8 27	8 26½
Eversion	9 : 32	9 : 30

no 4.
March 12. 1746/7.

Having now obtain'd the visible Place of the Moon for 3 Points of Time at half an Hour Distance from each other which shew her visible Path during the time of this Occultation sufficiently exact, & the more especially so, as they happen to be near the most remarkable Circumstances of it the Middle Immersion & Edd. For obtaining the Moments of which it is desir'd to be observ'd, that the remaining Part of this Calculus for that Purpose, is propos'd upon the Principles of computing the Times & Duration of a Lunar Eclipse, which supposes her Motion equable & in a Right Line, neither of which it is well known where her visible Place is concern'd as in this Case, is true, but here the Difference from both is so inconsiderable, that it is imagin'd the Moments of Immersion & Eversion thus determin'd cannot ~~create~~ create any notable Error from the true Result of the Tables obtain'd upon the strictest Rules of Computation.

no. 3.

Apr. 2. 1747.

+ ~~MS~~ N^o 52

78977

41 Dr. Bevis of a Glica.

To

Marloni Tothkes Esquire
President of the Royal Society

5403/12

Sir

Perhaps you may think it worth
while to inform the Society of what I
have seen this Day, and esteem a very
great Curiosity, in this Country at least.
It is a true Plica of more than
ten feet in length, issuing from that
part of the Skin of the Head w^{ch} is
over the middle of the left Branch
of the Lani-Occipital Suture. The
Person is a clean healthy fresh
complexioned Woman of about fifty.
born at Thistleworth; her name Anne
Norton. now at the White Swan
a Publick house near the little
Turn-File Hobborn. The Plica has
been in wrapping for thirty years. Many
of our best Physicians, and all the
best Surgeons, have seen her that have
been in England, who have assured that it is of
rare Curiosity.

Yours with much respect.

Yours most obedient Serv^t.

Apr. 2. 1747

W. B. 1747

When brought to the Society
deemed a cheat

Dear Sir

N^o 7

+ ~~MS~~ N^o 24

78977

Just after your Servant was with me this morning I had the mischance to break my Spheroids, and so disappointed two young Ladies my Relations, of the Entertainment they expected. It w^old be so good to show them a few Experiments any time tomorrow, be please to let me know your Hour of Leisure, and I shall be infinitely oblig'd to you.

Your very obedient Servant

Boyer

S. S. In case no more particular account should come to the R. Society, about the present Comet, you may inform them, that this day I saw a great luminous who came last night from Greenwich; where M^r. John Bradley had seen it about eleven o'clock yesterday night but could not get an Observation. It was in Hevelius' Lacerta, between Capricorn and Pegasus, as you had inform'd me. M^r. Bradley believes it passes the Sub-polar Meridian too much in twilight to be seen there. I have watched these last two nights for it and shall this night attend it again.

5403/13

Honoured Sir

+ AMM N^o. 24

67734

5403/14

1/5

The subjects of natural history are often strange & uncommon, but the Authors who have treated on them, have not fail'd on their parts to support & raise the wonder & once conceiv'd astonishment, by ascribing properties which never existed in Nature, thus indulging the humor of finding a marvellous, in all things, Truths have been greatly obscured & errors propagated without number. 'Tis to this cause originally, (if I mistake not) we are to ascribe the prodigious multiplicity of Poisons & that equally numerous tribe of antidotes treated of by the Antients, in their *Materia Medica*, & I should be very glad to have found modern Authors always just to truth in the qualities by them ascribed to particular drugs. Upon hearing lately part of M^r. Joan Antonio De Lea's Letter to you^s. I was suspicious M^r. Le Condomine had taken some facts there, upon the Authority of others, or else had been himself a little ^{too much} addicted to that general bias of Mankind, The Love of Prodigy & wonder.

In order to be better satisfy'd, I dissolv'd in a certain quantity of fair water, as much of the Indian Poison, as could be suspended. & let it stand to clear 24 hours. And having made a superficial incision with a Lancet, into the nose of a young Cat, a few drops were sprinkled on the wound; the Creature at first discover'd no marks of Injury receiv'd, yet in half an hour she seem'd by meowing ~~longer~~ more than before to be sensible of some pain, thus she remain'd about 20 minutes when at length she shiver'd, was sleepy, soon became convuls'd & in about half an hour her limbs were flaccid & her belly swell'd, these symptoms continu'd till she in a short time expired. Some time pass'd. viz. I sat down to inquire what visible effects had been produc'd on the body. I then separated the head from its trunk, & carefully examin'd the brain, & particularly the origin of the Nerves, but when I had consider'd it thoroughly I could

I could not discover any preternatural appearance, in any of these parts, having spent near half an hour in this inquiry I opened the thorax, & with some surprise found the pulsation of the heart as regular as if the animal were in perfect health, this appearance continued above two hours after the cat's head was off, but afterwards languid & was much weaker. I then ~~opened~~ opened one ventricle of the heart in which the blood was somewhat coagulated. this may be thought to be partly owing to the medicine, for soon after it had produced convulsions in the creature, I had a mind to see what bleeding would do, & with that view cut off the tail, but contrary to my expectation the Arteries that supply it with blood, bled very little, & upon cutting off the head, the Carotids & both vertebrals did not pour out above half a common spoonful.

But as it might be questioned by some from the continuance of the heart's pulsation, whether the cat might not possibly, if let alone, have recovered. I poured of the same solution as before a few drops into a superficial wound of a young Dog, weighing 12 Pounds, the creature in less than an hour, shivered, became sleepy, was very cold, & so stupid, that he suffered himself to be often burnt by the hot ashes, beneath the grate where he lay for warmth. In this comatous way he continued near four hours, & then shook of his stupor & was much better, I left him all night & found him next morning quite well & as hungry as ever. Upon this I made an incision at that time into one of the crural veins, & poured a few drops of the solution into it; in less than ten minutes the dog gave signs of great pain, soon shivered, grew cold, was convulsed & in less than 20 minutes died. upon opening him nothing uncommon was found, nor was the blood in this creatures heart so thick as in the former. The Crural Vein did not bleed from a large orifice after the poison was infused though it was likely to do it before.

But as some Authors have said that Birds in particular are instantly deprived of life if the least particle of certain poisons are infused into the blood, I had a mind to try one experiment & infused a few drops of our solution, into a cuticular wound of a small bird, this occasioning hanging of the feathers & a stupor in less than ten minutes & killed him in somewhat more than fifteen. I gave about two drams of Sugar to another bird of the same kind & ~~then~~ shortly afterwards pour'd a little of the solution into its mouth, but two drops had scarce touch'd his tongue before the creature was convuls'd & I could with difficulty lay him down before all motion was taken away.

I gave these two birds to two cats & whether from eating them or not I don't pretend to say, the cats made so uncommon a noise the whole night, that disturb'd the family's rest.

From these experiments we find that the supposed specifick is of no manner of use even when the poison is only taken at the mouth; & from them it may appear probable that our poison is nearly upon the same footing with white Arsenick in the cure of the tooth Ach.

Thus S.^r having satisfied myself, I thought I could do no less ^{than} give you an Account of the result of my trials, if they contain any thing that could afford you any pleasure, ten times the trouble I have taken would be amply repaid, but as the subject itself is far from the most entertaining & I'm conscious that others may have carry'd on to much better purpose, so I have nothing to plead in excuse of this trouble farther than that I have the honor to be ^{to} great respect
S.^r

London Jan^y 14th 1746

Your most Oblig'd & Humble S^r
Richard Brocklesby

XII. a Letter from Rich^d. Brocklesby M. D. & F. R. S. to
the president concerning the Indian poison sent on
from M. Condamine. member of the R. Acad. of Sci
at Paris.

To

Martin Folkes Esq^r

at his house in, Lanes Square

Spec. 5. 174677.
per Ser. 482. XIII

Sir

Madrid 14 Sep. 1746.

----- I herewith send you a sample of the Marag-
nan poison, which the Indians of that province prepare,
 and with which they kill their wild-fowl: that you
 may have an opportunity of verifying your self the
 experiments Monsieur de La Condamine has mentioned,
 in the relation of his voyage down the great river
 of the Amazons.

I have my self just drawn blood
 of a chicken with the point of a dart dip't in the
 composition, and that chicken became senseless in a very
 instant and died presently after: but I have not yet
 experienced the effect of the sugar said to be an anti-
 dote to this poison.

That which I send you
 was brought over by a particular friend of mine, a
 Priest, who made the same route as Mons^r de La
 Condamine, a little while after him. I shall have
 set down the manner in ^{which} the Indians use the poison, but
 I shall reserve for another letter, the account I
 have received of the several particulars which enter the
 composition, and of the manner in which it is prepared
 by the natives.

They steep in water some leaf tobacco, and after squeezing
 out the juice of that tobacco, they put it with a small quantity

a proportionable quantity of the poison, so as to make a mixture of a moderate consistency, neither very thick nor yet too liquid.

Into this mixture they dip the points of their darts or arrows, and then let them dry: when they are ready to use them they again moisten their ends with their spittle, and they immediately perform their effect upon the animal that is ever so little wounded with them. I have nevertheless been constantly assured, that however violent the effect of the poison is upon any animal so hurt, there is no manner of danger in putting the points of the darts in the mouth, to moisten them, as the Indians do: provided there is no scratch or bleeding in the mouth or gums, in which case it would prove of the most fatal consequence.

When any animal thus killed, is opened, almost all the blood is found congealed and clotted in the heart and lungs, and scarce any is met with in the other parts. The Indians nevertheless, eat their very ^{parts} and the blood itself by choice, without receiving any hurt from it: and the Priest who has been a missionary in the country, told me that he himself had frequently eat of it with them. He told me besides that the Indians use this poison for the tooth ach, and that they look upon it as a specific for that complaint, putting a small quantity of it

into the hollows of their teeth that are decayed. But having your self the poison now by you, you may your self cause experiments to be made with it: for the Indians are very superstitious and ignorant, and frequently besides affirm things, that on a stricter examination are not found to be exactly true. - - - - - I shall stay in this city about eight months, where I should be proud you would honour me with your commands: in the mean time I beg leave to present my humblest respects to the young Lady your daughter, and to all those gentlemen your friends from whom I received so many favours in England, being with the greatest truth and gratitude, Sir
your most obliged humble servant.

Antonio de Ulloa.

As what Mons^r de la Condamine says in his late book, concerning the poison above mentioned, may possibly not be unacceptable to the company upon this occasion; I here add an abstract of what he has there delivered upon that subject.

Pag. 67. The Yameos are very dextrous both in the making and the using of a sort long hollow trunks, which are the most common hunting weapons of the Indians. To these trunks they fit little darts made of the wood of the palm-tree, to the upper ends of which they fasten instead of feathers, small bunches of cotton exactly filling the hollows of their trunks. Thus prepared they blow them to the distance of 30 or 40 paces, and scarcely ever miss their mark.

This simple weapon, amongst all these

nations, fully supplies the want of fire arms. They dip the points of these darts, as well as those of their arrows, into a poison of so active a nature, that when it is fresh, it constantly kills in less than a minute any animal, that has blood drawn by a dart dypt in it.

Tho we had guns with us, we scarcely ever eat upon the river, any sort of game killed otherwise: and we have frequently found the points of the darts under our teeth as we eat; this is not however at all dangerous as the venom no way acts unless mixed with the blood, in which case indeed it would be no less fatal to man than to other animals. Salt is an antidote to this poison, as is sugar also and that more certainly even than salt.

Aug. 20th. During my stay at Cayenne, my curiosity led me to try, whether the poison upon the darts, which I had now had by me above a year, still preserved its activity: and whether sugar was really so efficacious an antidote to it, as I had been assured. I made both experiments before the Commandant of the Colony, several Officers of the Garrison, and the King's Physician there. A hen slightly wounded with a dart blown at her through a trunk, and the point of which had been dipped into the poison 13 months before, lived half a quarter of an hour. Another pricked in the wing with one of the same darts, newly dipped into some of the poison diluted with water, and immediately drawn out from the wound, appeared to become stupified in about a minute: presently after which convulsions followed,

and the one was then forced to swallow sugar, she expired immediately after. A third pierced with the same dart again dipped into the poison, having been succored with the same remedy in the instant, showed no sign of having been hurt. I have again repeated the same experiments at Leyden, before Messrs. Muschenbroeck, Van Swieten, Albinus and others of that University the 23^d of January 1744. And then the poison which must have lost of its violence, both by the time and the cold, did not perform its effect till 5 or 6 minutes after the wound given, but the sugar was given without any success: The hen only to which it was given seemed to live a little longer than the others: and the experiment was repeated no more. This poison is extracted by fire, from the juices of several plants, and particularly from several Lianes. It is affirmed that the juices of above 30 sorts of herbs ^{or} of roots, go to the composition of the poison made among the Picunas, which is that, that I made use of, and that is the most esteemed, amongst all the several sorts that are known along the River of the Amazons. The Indians always make it in the same manner, and follow exactly the process that has been handed down from their Ancestors, with the same scrupulosity as our Apothecaries proceed to the composition of the Treacle of Andromachus, without ever omitting the least ingredient prescribed: altho in all probability, this great multiplicity

of ingredients is no more necessary in the American
poison, than it is in the European antidote.

One will without doubt be surprised, that among
nations pronest of to cure and to sudden an instrument to
satisfie their jealousies and gratify their revenge; a poison
of so subtle and so pernicious a nature, should only prove
fatal to the monkeys and other game of the woods; and
it may yet seem more strange, that a Missionary always
feared and often hated by his new converts; for whom, he
discharge of his ministry, does not allow him always to have
the complaisance they would desire; never to leave them amongst
them without either danger or mistrust. And yet here
poor harmless people, are men entirely wild, no way
at all civilized, and for the most part without any idea
of religion.

5403/14 5/5

Dec: 18-1746

5403/15

XIII. Upon the sounds and Hearing of Fishes, by Isaac Theodor: Klein.

Or

Some account of a treatise entitled an Inquiry into y^e reasons why the Author of an Epistle concerning the hearing of fishes endeavors to prove they are all mute & deaf.

by Richard Brocklesby M.D. F.R.S.

Dear Author in the first place, clasps them into two orders, the first hath lungs, the other is furnished with organs analogous to lungs, w^{ch} we call fish-ears, or gills, all the whale kind the Dolphin, Porpoise, & such like have lungs. There ^{are} two families of the second class, to one of them belongs all that tribe, which have one, two, 5, or 9, air holes at the back, or sides of the head, or in their thorax, in w^{ch} are found ^{concealed} gills, the other family comprehends all kinds of fishes, whose gills are usually placed off each side the back of the head. ~~All these~~ Our Author's Antagonist alleges ^{that all fishes of both orders are equally deaf.} ~~In answer to our~~ ^{But that all Nature}

~~lists except M^r Raumer are of a contrary opinion, that fishes hear distinctly.~~ Our Author begins with an air of ridicule & shews how far the letter writer is ignorant of the various opinions of modern as well Antient. Our Learned Countryman M^r Ray thinks to reconcile these by allowing some to hear while others are deaf, but the greatest part allow fishes actually hear & ^{most} except Scheuchz^{er} seem agreed about the auditory passages, but the letter writer denies they have any organs of voice ^{inversely} upon the proverbial authority, mute as a fish, hence he concludes, they are likewise deaf; But in answer, is replied, the spouting whale hath all its internal organs, precisely similar to organs of voice in other creatures, & therefore they may answer the same purposes, nay actually serve this end for in the green land fishery the whales when struck ^{they} frequently roar so loud, as to be heard at two french Miles distance. but ~~some~~ of the first family of our second class as the Skate Lamprey & others, our Author hath heard, utter some kind of noise & ^{his} opinion that most sorts of cartilaginous ^{sort} from analogy he argues that as no beast from the Lion to the meanest animal, nor from the Eagle to the humming bird, but can utter a ^{note} so he thinks the same generall use is observed in the Economy of fishes, but the same time our Author here seems to lay too much weight, upon what he supposes final causes & metaphysical arguments; which have in all ages ruined natural philosophy.

But the letter writer queries, whether fishes may not be mute in ~~our~~ air &

yet capable of some voice in their own element; ~~which~~ ^{Author} ~~over~~ takes the noise which carp & such fish make in hot weather, on the surface of the water, to be a voice, & this is most remarkable when the male impregnates the female ^{before} ~~yet~~ this is often heard when the fish is six or seven inches under water. Our Author farther enumerates many others of the ind foreign fishes & particularly our Smelt ^{is} put alive into vinegar hives very audibly. The letter writer had objected against ~~fishes~~ that they have no occasion ^{of hearing} because they never copulate as other animals do, but our Author describes the following manner of whales, ~~when~~ which is performed as ^{that of} other animals ~~by means of a horizontal~~ ^{by means of a horizontal} & observes, they bring forth their young alive, that follow the female, & suck milk, from the tits ^{which} are placed in them, near the organs of generation, & ~~in violent~~ storms the dam takes her offspring into ~~her~~ ^{her} mouth & protects them from danger. This ^{last} is common to several of the skate kind. The letter writer alleges that fish never sleep, but our Author assures us all such as have lungs ~~sleep~~ ^{do} in the night time, thrusting up their nostrils into the open air, for others he can not be positive as their history is little known.

The letter writer premises two questions, first whether fishes have any ears or if the gills ~~serve~~ ^{do} the same purpose & answers positively in the negative, & therefore concludes they can't hear. But our author asserts that ~~snakes~~ ^{to both} frogs, chameleons & others of the lizard kind actually hear without any of the usual ^{external} apparatus of hearing, for though they want the auricles & ears yet have they auditory papae by ^{which they hear} ~~which they hear~~ & even internal organs to which the breath audibly reaches, but our author farther asserts that all the whale kind & in general such fishes as have lungs have likewise ^{an internal auditory} the internal organs of hearing & appeals to a public dissection of a Porpoise & another fish of the whale kind ^{made by himself in which the} ~~where~~ or petrosaurus with the other parts of these organs ~~to~~ ^{had} separately shew'd, & calls in the concurrent testimony of D. F. in his anatomy of a Porpoise. Thus having satisfied us about such fish as have lungs he goes on to consider the cartilaginous species such as the Skate Ray & kind of Lamprey ^{which} have organs of generation & copulate like brutes, yet ~~are~~ ^{are} excluded while yet in the egg state & this from analogy of that ~~these~~ & in general other fish as they have organs ^{which} serve them for lungs so they may have what answers in others the apparatus of hearing. In proof of this he asserts that all kinds of fish but these ^{which} have lungs are always found to have ~~two~~ stones in their heads naturally form'd & invariably plac'd in the same situation, being join'd to the contiguous parts ^{with} ligaments.

& Nerves ^{ch} take their rise from the substance of the brain, & having examin'd the head of a Pike minutely with a microscope he discovered the Auditory pores in the stones & persuades himself that three pair of stones are to be referred to this use, Therefore concludes as there is some analogy in the Organs, that all fishes in some measure hear.

The letter writer farther objects, that water is not the medium of sounds, & though air is actually contain'd in all water, yet it cannot be put into undulations any more, than the circumambient water, but that would require a much greater vibration than the ^{external} air can give. Thus says he if a person immerge his head a foot under water, he will hear nothing but a boiling din, & however great noise is made in the open air the event will be the same, & if the water itself be put into the most violent agitation the person will discover no odds in that sensation of his ears from what he perceived in the stillest water, hence he concludes water incapable of transmitting sounds. Our Author replies that as fishes are unanimously agreed to be capable of smelling so ~~that~~ by analogy tis probable they have hearing, for ~~odours~~ ^{odours} are convey'd by the air, as well as sound. But he thinks the unnatural position of a mans head immergd a foot under water may be some cause for that confused noise. And opposes the experimental Testimony of Abbè Nollet himself who went different depths under water to satisfy himself how far sounds could be convey'd ~~in~~ that he ^{drov} down At four Inches under water he heard the sound of a Gun ^{discharg'd} a Rock striking & of a hunters horn, these ~~were~~ repeated at different depths were heard first at 4 then at 8 afterwards at 10 & lastly at two foot, A Man's voice was also heard in the same manner.

At different altitudes of water none of them exceeding two feet he could perfectly distinguish mixt sounds when two bells were struck or two Pipes sounded together

He could distinguish ^{under water} very distinctly words utter'd & prov'd this assertion by declaring when he came above water what was said while he was under it.

All sounds were heard more faintly & attenuated yet the difference of the sound at 4 & 10 Inches depth was not answerable to the difference of the Altitude of water

He observed ^{at first} that momentary sounds were not so well conveyed as continued, yet he afterwards ^{determined} at the same depth one tap of ~~the~~ a drum head as plainly as a continued round, this he thinks ~~was~~ the same in a man's voice & the sound of a Pipe, but ingenuously owns he was not fully satisfied in this experiment, & therefore does not lay as great stress on its certainty as on the former.

Lastly he held his head under the surface of the water, ^{to} ~~cover~~ ^{to} him, ~~but~~ could not hear the flock strike, ^{which} was audible in the open air at 45 feet distance ^{especially} ~~often~~ on a plain.

The Abbe therefore concludes if fishes do not actually ^{hear} for want of proper organs & not because the medium cannot convey sounds.

Our Author mentions the common notion of carp & other fish coming out of their holes at the sound of a bell, ^{to be false, adds} ~~the~~ a story 10th Mr. Boyle some where relates, that near Geneva a Man had a fish pond whose banks were so high from the plane on ^{which} it was, that ~~one~~ ^{one} could not look over them into the pond, & therefore 'twas impossible the fish could see the person, yet the fish of that pond were at any time conven'd at ~~the~~ ^{in order to be fed} certain sounds by the gardener, ~~as~~ a creditable person asserted.

The latter writer having made a high partition in a pond & watch'd while an accomplice behind it, made a very great noise & discharged a gun in order to frighten the fish (if possible) that were playing on the surface of the water, but they did not give any attention ~~but~~ as soon as ever they came in sight, the fish immediately made off.

Our Author thinks this objection of little weight, ~~because he thinks that objects of a new sense when once familiar to any animal never make a fish apprehend~~ ~~the~~ ~~question is~~ ^{can be frightened by sounds} ~~not whether fishes when they see nothing, hear~~ ^{only.}

Upon the whole our Author shows himself an experienced & diligent naturalist, & will (if I mistake not) be allowed to have fully prov'd the falsity of any assertion that all fish are entirely mute & deaf.

Pr. Gr. 486: XIV.

16. March 1745

Dr. Brocklesby's
Act. of Klein de
Sonne & Audite Primum

h. 2.

The following account is the best I have received of the
 poysonous effects of a noxious root, lately found in a parcel of
 gentian, & exhibited for use to several Persons instead of it, and
 as it is attended with such dangerous consequences, I thought even an imper-
 fect Relation of facts had better be given immediately, than to expect more circum-
 stances & wait so long for them, till greater mischiefs might happen,
 by the inattention of such, ~~who~~ ^{as} are constantly administering medi-
 cine. The following account was sent by a Gentleman of Hambleton
 Parish Buckinghamshire & ~~has been~~ ^{has been} agree^d in general with some other fatal
 instances that have happened since in London.

Mary Burgess aged 60 years, about 5 o'clock in the morning
 drank of an Infusion of only one penny worth (without other
 ingredients) of supposed gentian root, in half a Pint of white Wine,
 it is uncertain what precise quantity she took, but in two hours
 afterwards she faltered in her speech, had twitchings & convulsions
 of her hands so far, that the ignorant bystanders alledged the
 poor woman was drunk & so left her alone till 12 o'clock to sleep
 it out, on their return however she appeared much worse, was
 speechless, & remains so 3 whole days, & did not know any body
 all that time, in her illness a purging came on & at last carry^d
 her off.

Katharine Woodward aged 44 Years took about a teaspoonful
 of the same Wine & soon after fell down speechless & her limbs
 were paralytic near 36 hours, after that she recovered her speech
 but continued ill above a fortnight & part of that time her
 under Jaw was convulsed, & she bled both at mouth & nose in
 the beginning.

Mary Diggins aged 33 years tasted a much less quantity
 of the same wine that the former had done, & though terrified
 at her neighbors bad Symptoms she drank warm water with
 oil in order to vomit, yet she soon staggered & grew delirious
 could not swallow any solids, & lost the perfect use of her
 eyesight a fortnight.

The vague reports of these & M^r Potts's Cases induc'd me to obtain the
 favor of two or 3 Druggists to look over some gentian root, one parcel
 of w^{ch} had no less than a 20th part of a root w^{ch} at first sight was

discovered to be no Gentian.

This root for which we have yet no name

is of a greyish brown color externally, but it is browner & more resinous internally, most ^{wh} I have seen is about the thickness of a finger, tho' some is much larger & whiter, which is a reason with several for thinking there are two sorts of it. & indeed some pieces emit a stronger & more nauseous smell, but this I apprehend may be occasion'd only by a larger quantity of resin in them, All of them are of ^{an} acrid purgent taste & leave a dryness on the tongue.

I judg'd it therefore necessary to try what effects this root might have on Dogs, that I might thereby the better conjecture concerning them ~~if given~~ ^{on} to the human Species, & though no man has any right wantonly to torture, or destroy in a cruel manner the least animal, yet when good purposes are answer'd in the whole of things by inferior Natures yielding to superior ones, a man may without just imputation to his moral character sacrifice the Interest of a baser order to the ~~Interest~~ ^{Happiness} of one superior.

With this intention I decocted $\frac{1}{2}$ an Ounce of this unknown root, powder'd grossly in ten ounces of fair Water, till two evens evaporated ~~by boiling~~, 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 & vomited part of the dose, yet in less than $\frac{1}{2}$ an hour rec'd like one drunk, had twitchings of his limbs & after some time the motion of his heart was irregular, & intermittent, though strong he was sleepy about an hour, but came gradually to himself in half an hour more, & eat victuals which before he refus'd.

Two days after the same Dog took 4 Ounces of Decoction of Gentian made as strong as the former, but I discover'd not any bad Symptom from it. I used this quantity as Gentian root ^{if sometimes} may be given to that quantity in the practice of Physick, It is above ten days since he took the first Decoction & hitherto continues well.

Another Dog took above a Dram weight of the unknown root finely powdered & mixed with butter, it instantly made him foam from the mouth & caused ^{sudden} vomiting, & in $\frac{1}{2}$ an hour weakness of his limbs & staggering which lasted $\frac{1}{2}$ an hour & then he recovers.

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 th a smaller quantity had before produced.

One of the Dogs had some loose Stools after taking it, another urined plenty.

Like Experiments have been ^{made} by M^r. Pearce at St. Thomas's Hospital, th 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, & to reject what they find not genuine, since one of the Women before mentioned & a man ~~that~~ I have heard of are both dead & since Gentian is of general use in medical Compositions as well as the primary ingredient in the Cordial Bitters Ladies make for their own use.

XIV. An account of the poisonous Root lately ~~found~~
found mixt among the Gentian; by the same.

Dr. Brocklesby.

Recd 13/11/73 42

Per Dr. 486. XV.

of Army Work of York and Root only

1646

VII. Part of a Letter from Mr John Drowning, of Bristol, to Mr Henry Baker. I. L. S. Dated Dec. 11. 1746. concerning the Effect of Electricity on ~~these~~ Vegetables. 78977

Having an Operator at Bristol with a good electrifying Machine, I was desirous to electrise a Tree, and therefore sent him the following for that purpose, *Scorbuticus*, *Leucogium majus*, *flore pleno ferrugineo*, and *Stoechas cibina erecta*. These were not chosen with any Design: their being the least Plants I had, and in the least Sort was the only Reason.

I promised Myself the Pleasure of seeing their Leaves erecter when electrified, but was disappointed: (whether it's being the dormant Season of the Year for all Plants might not be some hindrance I can not determine) neither did the Leaves flag on their being touched. However I was agreeably recompensed by a Stream of fine purple-blue coloured Light, much resembling an Anæsthes, that issued from the Extremity of each leaf upwards, of an Inch in length, when the Finger, or any other non-electric approached near it. This Colour I attribute to the watry Particles in the Earth, having often observed the very same Colour issuing from the long leg of a Syphon. On putting my Finger on the Gun Barrel to stop the Electricity, the Leaves of each Tree had a trembling Motion, which remained for some little time, and immediately ceased on withdrawing my Finger from the Barrel and admitting the Electricity. This constantly happened as I put my Finger on or off the Barrel.

The *Stoechas* Plant has a very long hoary leaf, and bears its Blossom on a very small, slender, and almost naked Stem, rising near a foot above the Body of the Plant. This Stem had a Motion given it, when any non-electric was brought within about two Inches of its Summit, much like the Vibration of the Pendulum of a Clock: which vibrating Motion was parallel with the Breech of the Gun, quite contrary to the same kind of Motion I had before observed in a Needle, hanging perpendicularly by a Thread at the End of the Gun; the Needle always vibrating in the Direction of the Gun. The Motion of the Plant and Needle at ways continued as long as the Glass Globe was excited.

I was also desirous to be satisfied, whether Electricity could be propagated without mutual Contact, by suspending another Gun in Silk Cord about two Inches from contact, and the Electricity was near as strong in the second Gun as in the first. At the Distance of between 3 and 4 Inches it was much abated, and so it gradually diminished as the Distance increased to near 6 Inches, where it would scarce attract a Thread of Lint.

I prevailed on a Man to be let Blood, and then placed him on a Cake of Pitch; but could not be sensible of any Increase of Velocity in his Blood, by being electrified, as has been asserted.

I had almost forgot to mention, that the Strokes I received from the Electrified garden Pots, were more violent and painful to my fingers, than from any other Body I ever experienced.

Mr Baker since his receiving the above Account, has had an Opportunity of electrifying a Myrtle Tree, of between 2 and 3 Feet in Height, growing in a Pot, at the Seat of the Duke of Montague & Ditton; in Presence of his Grace, the President of this Royal Society, and several other curious Gentlemen: who found, that whenever the Hand or other non-electric Body was brought near the leaves, Streams of fine purple Linn issued plentifully therefrom, together with a very considerably cold Air; and that the leaves would be attracted at some Distance, and move vigorously towards a non-electric Body.

no 3.

Jan. 22. 1746-7.

Pr. Soc. 1742. VII.

An Account of the Naptha Alba,

5403/18

By M^r Peter Collinson.

The Specimen I present to the Society was procured from the Pitts which afford it, in the Neighbourhood of Baku, a City on the border of the Caspian Sea And was sent by my ingenious Friend D^r Sanchez Physician at Petersburg Procured for him by D^r Cook at Astracan.

Wee are obliged to D^r Kempter for his observations on the Naptha Alba, In his *Umanitates Exotica* p. 274. where he gives a particular Account of this Liquor And Describes the Curiosities of the tract of Ground where it is Collected which are both Numerous & very Singular.

Tho the Book is well known, yett as it is not in every ones hand, I shall presume to say before you an Abstract of what our Author presents us with on this Subject. Not far from Baku a Peninsula call'd Okesra Stretches out into the Caspian sea In which are the pitts from whence the Naptha is taken, on the North Side, it is Rocky & unequal, the rest is more plain & barren, the Soil being Cheisty Clay or Gravel or else small Lakes of Salt Water.

Two kinds of Naptha are produced in this Place called by the Natives White & Black Naptha What wee have now before us is the White Sort And to this I shall principally Confine myself.

The pitt where it is found, is of an Irregular Figure about 100 paces Long And 50 broad & about two Fathom deep; the Bottom is a Layer of Soil & in the midst Stands a little Salt water, about a hand breadth deep.

Towards one end ~~and~~ of this pitt are two narrow Square wells, which the Inhabitants said were 40 fathom deep, but sunk without any walls or timber to support the Sides from falling in, the Earth being sufficiently firm for this purpose; into these wells the Naptha gradually Cuzes out of its Subterraneous Passages, & Distills as our Author expresses it, *Parce et quasi guttation*, and is Collected in the Bottom of the well.

If the Naptha ceases flowing into a Well the Natives sink another pretty near it, they showed us 7 or 8 just close by one another that had formerly ^{yielded} great

great Quantities but now scarcely afforded the Smell of it; they Draw it out in Buckets & putt it ~~it~~ up in Skins for use.

The Naptha fresh Drawn up takes fire in a Moment if a Lighted Candle or Lamp is brought near it & it burns with such force that it is scarce to be Extinguished; but it loses this property by age, however closely it is Secured. But yett it will burn at any age, but this Rapacity of Flame seems to Cease as its finer Spirit Evaporates.

The principal use which is there Made of it is to mix with Persian Varnish, this Consists of an ordinary sort of gum Sandarach boiled with ~~seed~~ Oil to the Consistence of Ointment, this is called Varnish Butter & is used by them for use and a little of it being mixed with white Naptha and laid on their work with a pencil & afterwards Exposed to the Sun the Naptha evaporates & with it some lighter parts of the Oil, leave the Surface finely polished, *Shining & hard.*

Our Author tells us that the Black Naptha is much more plentiful: it is produced & collected in the same Manner in another part of the same peninsula. It serves for Lamps & instead of other fuel in various parts of Persia & the Neighbouring Countries.

The Campus Ardens in the Neighbourhood of this place which our Author describes, seems to owe its singular Phenomena to the Earth's being Impregnated with Naptha and other Sulphureous substances sett on Fire in the Bowels of the Earth.

In some parts of it, an invisible Vapour only arises which smells of Sulphur & if the Flame of a Lamp approaches one of these Chinks a sudden fire fills the Beholder's with Astonishment but it soon Disappears.

In other places the Flame is continual & so strong as to burn Lime & send the Neighbouring Inhabitants instead of any other Fire to Dress their Victuals.

an Extract of
Stump for.

122

Jan. 15. 1746-7.

the white Naptha

IV. Some Observations on the Belluga Stone of Hollown

+ ~~the~~ 41. 78977

These Stones of the Belluga ^{were} ~~was~~ collected by Do^r Cook at Astracan & sent to Do^r Panches at Petersburg, by whose favour they came to me. I have applied myself to these Gentlemen to satisfy my inquiries about them; ^{and the} ~~accounts~~ ^{accounts} they have communicated with my own Observations are as follows.

The Calculus of the Belluga is found of various, Shapes and Sizes it is mostly of a flattened oval figure, sometimes roundish, Globular with unequal depressions & of a yellowish white colour externally; and a smooth polished Surface.

It differs in magnitude, as it does in figure; from the bulk of a pigeons Egg up to four or five times that Size.

They are mostly compact, ponderous, & Solid, not very friable, but requiring a pretty smart blow of a hammer to break them, They yield easily to the Saw but this defaces their Internal ^{Texture} ~~Structure~~, which is very remarkably elegant, & regular. The Stones consist of concentric coats firmly adhering to each other formed about a Nucleus which appeared to be quite an heterogeneous substance, both from its colour, hardness & Texture.

~~These coats or layers are of different thicknesses with respect to each other, and to the different parts of each Coat, so that if the Stone appears irregularly protuberant on any side, it does not always seem owing to an internal Layer, but to the increase of their thickness in the prominent part perhaps from its situation, and the easier access & stronger adhesion of the lapidescient matter to one side than the other. Careful dissection discovers this to be fact, and is common to this and many other kinds of animal Calculi.~~

But another Obvious circumstance in its Structure renders the Belluga Stone different from most others: which is its radiated appearance. It seems composed of an Infinite number of shining Rays, regularly diverging from the Central Nucleus to the circumference representing both in Colour & form the flakes of a pure white Terra foliata Tartari, or (excepting the Colour ^{wh} is yellowish) the striated Spicula of antimony.

This Stone is found in the Fish called the Belluga, a Species of Sturgeon: the Acipenser tuberculis carens — Artesij post. iii pag: 92: — it is commonly called Lapis Belluga, by the Russians Kamen Belluga, w^h signifies the same thing. Of ^{min} Fresh Fish several authors have given us the following account — In shape it is not much unlike a Sturgeon — only its snout is proportionably shorter & thicker, the skin on the back is light gray, but under the belly is white, without scales: its flesh is whiter than Veal, whence the name Belluga, or the white Fish & affords a much more delicious dish, than Sturgeon. Of its Ro^u or Spawn is made the celebrated Caviar, and some are found so large as to yield from 150 to 200 weight of it. They are found in greatest plenty & especially those of the largest size in the River Volga, about the city Astracan. ~~Bar~~ Strahlenberg says he saw one caught in this River 56 feet long & 18 feet thick, and takes them to be the largest River Fish in the World — They are likewise found in other Rivers as the Don & those that flow into the Baltic & Caspian Seas.

I am not certainly informed, neither do authors agree, in what part of the Fish this Stone is found — Strahlenberg says in the Head & Stomach, some say in the Air bladder, others in a particular bag near the Anus or Inferior Gutt, others again in still different parts — It is found in both Sexes, but oftenest in the Male & of all ages, but is very Rare & scarce, for in a Thousand Fish, it ~~is commonly~~ ^{often} happens not to meet with a Stone.

From hence it would appear that these Stones are preternatural to the Fish, & perhaps morbid productions, just as the Stone in the humane bladder notwithstanding its curious and regular Form, probably the food of the Fish the situation of the parts in which it is generated & many other circumstances, may contribute to this uniformity of appearance —

A little of this Stone Scraped and laid upon an hot Iron
gave a faint Urinous Smell, and calcined into a light,
greyish, insipid Earth.

If it been a real animal substance or a constituent
part of the animal its smell would in all probability have
at once discovered it.

The Natives about the Volga ^{very much} esteem
this Stone for its Vertues, being ^{Reputable} great to promote
Delivery the common people take from 10 grains to 30:40
or even 60 Scraped fine in a little Water 2.3. or 4 Times
in 24 hours when the Case is Dangerous.

It is also highly commended as a Diuretick
and Lithonriptick and this not only amongst the common
people, but amongst such as are more Capable of informing
themselves of its Effects.

Mr. Collection of the
Academy of the Stone.

March. 12. 1796/7.

for 483. IV.

p. 454.



Fig. 6



Fig. 7



TAB. II.

483. 78977

5403/19
2/2

John C. ...
John C. ...

On the Eighthth of March 1747
 Near Eight a Clock in the Morning, As I was
 Riding within Three Miles of Greatwood on Essex
 There appeared a singular Phenomenon in
 the Heavens the Sketch on the other Side may
 give some Idea of It.

The Morning was fine & clear the Sun
 shone bright no Cloud to be seen, but the
 air a little Hazy where the Phenomenon
 appeared which was a bright Cloudy Spot
 seem'd 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, & then Vanish'd away by Degrees

~~If I may compare Distances above with those
 Below — the Phenomenon seem'd 3 or 4 feet long
 and 2 feet broad — and about 200 yards
 distant from the Sun~~

Hollington

VI. an observation of an Uncommon Glean
of Light proceeding from the Sun, by ~~Mr~~ ^{Mr} ~~W. H. W.~~ ^{W. H. W.}
Robinson F. R. S.

March 19. 1746-7.

Pr. No. 483. VI.

North

Fig. 2



Fig. 4

may be contracted



South

no. 2.

March 19. 1746-7.

Apr. 10
1746-6
Pr. 57. 179.
VII.

5403/21

XIII. a Letter ^{the Rev.^d} from ^{m^r} G. Costard + Aff. N^o 55

To

Tho.

The Reverend ~~D^d~~ Shaw. D.D.

F.R.S. & Principal of St Edmund Hall
concerning the Chinese chronology & astronomy.

3.

Apr. 30. 1747.

Pr. 5 r. 483. XIII.

put this at the end

P. S. The Title of the work I have here cited is at length
Observations Mathématiques, Astronomiques, Géogra-
phiques, Chronologiques, & Physiques, tirées des Ancien-
s Livres Chinois, ou faites nouvellement aux Indes, à la Chine
& ailleurs, par les Pères de la Compagnie de Jesus. It con-
sists of three volumes printed at Paris 1725. 1732.

Reverend Sir

The Subject of our late Conversation turned up the Expectation of some Nations in carrying up their Histories to so immoderate a Height, as plainly to shew those Accounts to be fictitious, & without Foundation. This it was agreed was y^e Case of y^e Babylonian & Egyptian Accounts, & you seem'd to think ^{it} w^d be found to be the same wth any other People that sh^d make y^e like pretensions.

The only people in later times that have been thought to contradict this Opinion are y^e Chinese, of whose History the world hath been taught to entertain very extraordinary Conceptions. But that even they will be no exception to your Surmise, but on y^e contrary a strong Confirmation of it, will I persuade myself appear from what I am now going to offer.

I need not inform you, y^e the Eastern Writers in general are much addicted to Fable & Romance. This is a fact too well known to need any Proof, & therefore great Judgment is many times required to distinguish what is real, from what is purely imagi-

nary, improbable, & absurd. I say this, not so much
wth regard to their acts of foreign Nations, with
whose Affairs they may be presumed to be less ac-
quainted, as of y^e own ancient State & Condition, &
that in Ages not exceeding remote. But if this Ob-
servation holds but too true, with respect to those,
whose History we are in some manner acquaint-
ed wth how much ought it to put us upon our guard
as to those, we are in great measure absolute Stran-
gers to? The best Accounts we have rec^d of China
are owing to the Jesuites. But those acc^{ts} of selves
are, I am afraid, to be frequently received wth great
caution. These Fathers have been sometimes perhaps
not sufficiently versed in European, or Chinese Lear-
ning, or both, to give us proper Information. At
other times, it may be, they have been too much
prejudiced in favour of their Converts, or had Ends
to serve, of wth the world hath not been properly
enough apprised. To have propagated their Reli-
gion only in a barbarous & uncultivated Nation,
w^d not have been so much for the Credit of the
Mission, as to have been able to introduce it among
a people civiliz^d & polish^d by Arts & Literature.

Suspicious as these & if like Circumstances are,
is it not surprising to hear Authors, upon their
words only, & upon little or no foundation besides,
(as I question not will appear, asserting wth so much
positiveness that the Chinese History reaches up
indisputably to the Times of Noah, a Thing so far

ibid. pag. 25.

from being indisputably true, that no Article w^t
ever perhaps will admit of greater Debate. 'Tis
true indeed, if Chinese give us a long List of Kings
that reigned among them from a Time of Fo-Hi,
& a series of Dates, that if allowed, may carry up
His Age 2352 or 2847 Years before of Fan Aia.
But how easy is it to feign Dates & ^{Successions of} Kings ~~King~~?
Let it be made appear what Foundation this
Chronology depends on, what ancient Monuments
the Chinese have, & in what manner preserv'd. Mar-
bles I suppose they have none & their Paper, such
at least as is brought into Europe, ~~seem~~ appears to
be of too fine a Consistence for the preservation of Records.
~~any one very improper Materials for the purpose~~

You will be told, Ist perhaps, that a gr^t part
at least of their Chronology is verified by Eclipses.
A very pompous Argument! but when narrowly exa-
mined into will be found to prove just nothing at
all. We are told indeed, that the ancient Chinese
Observations consist of 26 Eclipses of \odot Sun, and
21 Conjunctions of \odot with \odot fixt Stars. The oldest
Eclipse of \odot Sun is placed in \odot first Year of the
Reign of Tching-Kam, supposed to coincide wth the
year before χ^t 2155. But \odot oldest Conjunction of
 \odot reaches no higher than the year after χ^t 73, &
how inaccurate \odot Observation was, appears from hence,
that if Chinese only mark \odot Day when \odot happens.

But if Question naturally arising here is,
it comes to pass that
now, if Chinese Accounts afford no example of any
planetary Conjunction before this, when they produce
an Eclipse of \odot 2228 Years earlier. By what good For

Observat. Math.
Astron. Geogr.
Chronol. Tom. 1.
Pref. p. 13. 14.

ibid. pag. 18.

ibid. p. 19.

Not much before
of Oldest Babylon
an Eclipse if is pre-
served. See Letter
to M. Folkes Esq
pag. 21.

time came of Eclipse to be preserved, & all appulses
of Planets to fixt Stars for so many Years be lost.
Let us suppose that these were ~~things~~ below if No
lice of Chinese Astronomers, or if they did not
know w^{ht} use to make of them. But in w^t manner
must we account for this, if we hear nothing of any
other Eclipse till 17 years before 776. That all of
Eclipses obser'd during so long an interval as
1379 Years sh^d have perished, & this ^{have} one escaped, re-
quires a pretty strong Faith to believe, ~~or great~~
~~Proof to make it a plausible Hypothesis.~~

But farther, we are told that they obser'd of
winter Solstice in 17 years before 776. There is
nothing tis true impossible in it, for tis not s^how
accurate of Observation was. The Difficulty is on-
ly to ascertain if Fact, & convince reasonable peo-
ple if it was made at all.

Observat. at
sup. Tom. 2. pag.
117.

Tis well known, & allow'd by of Missionaries
themselves, if the Reception they have met with
in China, hath been more owing to of Character
as Philosophers than Apostles. When therefore
they brought wth them into of Country Accounts of
European Discoveries, & particularly, Astronomy,
~~if Observations had been made in it~~, might not the
Chinese, agreeable to their vain-glorious Character,
tell of ^{that they had} their own much older than any thing
they c^d pretend to? It may be so indeed if this
is no more than Supposition, & w^h consequently
argues but little. But then if Supposition is so
easy & natural, if it requires at least of Contrary
to be made out by some very good Proof.
One Reason why this may be insisted on the

more is, if of Chinese, according to of Fathers themselves, have not always been faithful in their relating Observations. Y-hang, abt of Year after x^t 721. had the Reputation among them of an able Astronomer. But being mistaken, it seems, in His Calculation of an Eclipse, rather than own His Ignorance, He pretended of the Heavenly Bodies did not always observe of same Laws. In support of w^h extraordinary Hypothesis He urged, if in

Observat. ut sup. of time of Tsin, if Star Sirius was eclipsed by if Planet Venus, tho' if Latitude of Sirius is $35^{\circ} 32' 8''$ & that of Venus never exceeds 4° . The same sort of Observation wth this, I suppose, is if other of if Conjunction of π , γ , δ , ϵ , in if Constellation ϕ , when if ϕ & δ likewise were in Conjunction in 15° of α in if time of Tchouen-yu.

Tom. 2. p. 86.
Flamsteed's British Catalog.
Greg. Astron. p. 5.

Observat. ut sup. Tom. 2. pag 149.

Observ. ut sup. Tom. 2. p. 33.

And to put it out of all doubt, ~~in consequence~~ if of Chinese are capable of obtruding upon if word fictitious Observations, we need no other Authority, than if of if Learned Fathers y^selves. In if Year 1725 the Missionaries sent into Europe an Act of an Approximation of if four Planets γ , δ , ϵ , & π . Such planetary Conjunctions, it seems, in China are looked on as happy Omens of good Fortune to if Prince upon if Throne. The Chinese therefore, as if bred up at if Court of Versailles, wth a true French Politeness, in Compliment to their Sovereign, marked in their Registers a Conjunction of all if. This false Act of an imaginary Conjunction, as if Learned Jesuite himself observes, may in future times be if Occasion of very great Errors. To if Chinese, I suppose He means; for in Europe if Danger will be but small, where there are better Tables, exacter Act,

& more accurate Observers, than if most languine Le
suite will pretend to be among if Chinese. But if they
do venture at recording such a spurious Observation,
at a time when they were sure of being detected, w^t
may we not suspect if to have been guilty of, when
they had none to confront them, & how little may we
presume they know of if uses to be made of Celestial
Observations.

Observ. at sup.
Tom. 2. pag. 158.

We are told wth great Pomp & Assurance that there
always was in China an Office of Mathematicks &
another of History. That it is if Business of if former
to calculate Eclipses, & of if latter to register them, &
w^{ry} other Occurrences, if happens in the State.

It w^d have been well if if Learned Fathers had
told us with a little more exactness, what we are to
understand by if Term always, & whether if Chinese
are acquainted enough with if uses for wth Eclipses
serve, to make it probable if they sh^d have had such
an Office any considerable Number of Years & much
less always. May we not in if mean time suspect up
on hearing such Language as if, if Fathers mis
take Pekin for Paris, & having if Heads full of the
Academy of Sciences, cannot help figuring to them
selves if like, in if remotest Corners of Asia.

Be if as it will, they tell us, if the Mathematici
ans have often had if Credit, to take out of if Regis
ters their false Calculations, & substitute in if Room
others agreeing wth Observations. But where things
are kept wth so little exactness, what can be expected
but Confusion? what less indeed can be expected, than
what if Fathers assure us themselves hath frequently
been if Case, if by this means Eclipses have been pre

3 R 483

at sup. pag. 159.

ibid.

served, if Calculations made by European Tables demonstrate to be false? Can we wonder likewise, if Learned Fathers sh^d doubt many times, whether such or such a particular Eclipse, be an Observation, ^{made at the time,} or if the result only of a Calculation & perhaps a false one?

Observat. ut sup.
Tom. 2. p. 32.

For after all it hath been P of Chinese Eclipses & Calculations of y^m, 'tis agreed if before the Time of Lieou-hong or A. D. 206 they had no fixed Principles upon w^{ch} to proceed in that Business. This Observation, I am afraid, will extend to much later Times, or 'tis scarce to be imagined they sh^d look upon total Eclipses of y^e Sun as ill Omens. In consequence of this Superstitious Belief, we are told, if the Chinese Astronomers have carry'd their Compliment to y^e Family or y^e Throne so far, as to affirm, no such Eclipse do happen during their Time. On y^e other hand sh^d an Eclipse of y^e sort happen without being foretold, they immediately pretend it to be a warning from Heaven of some Misfortune likely soon to befall y^e Govern^t. But if it sh^d be foretold, & not come to pass, they w^d then make y^e ^{many} Virtues of y^e Sovereign of preventing cause, & what is better still, a Shelter for their own Ignorance. Such Notions as these however, I think, plainly demonstrate y^m to be very bungling Astronomers, & if they ^{can} hardly look upon these Phenomena as depending on established invariable Laws of Nature, y^e consequence of w^{ch} is, that they can no more attempt bringing them to a Calculus, than Winds Thunder & Lightning & y^e like. It was observed before, if y^e Mathematicians had many times y^e Art or y^e Credit to take out of y^e Registers their false Calculations. But we are told in y^e same place if before they were deposited there, they were presented to y^e Emperor for His

ibid. p. 33.

Inspection. Lett any one if knows of least of of Despotick Governments of if fast reflect on if probability of this, & whether the Attempter w^d not run gr^t Danger of paying for it wth his Head.

What hath been already s^d, will, ^{I suppose} be more than sufficient, ~~to make~~ to shake if Credit of Chinese Observations. But what must we think of those very ancient Ones, when we are farther told if from if time of Tschun Hieou or 480 years before ⁺ the Chinese themselves allow Astronomy was almost intirely neglected among them, & that Tsin-chi. hoang, whose Reign began in if year before ⁺ 246, order'd all Books of History & Astronomy to be burnt. But every one will easily imagine what Destruction of Observations must have been made, during a neglect of these Studies for 234 Years, & how little w^d remain to be burnt by this Anti-Astronomical Prince.

It was owing to if Devastation it seem, if the Chinese are s^d to have lost if Method taught by if ancients, & particularly if Emperor Yao, of calculating if Places of the 7 Planets & if fixt Stars. It may with good Reason be questioned, whether they really had any methods of calculating their Places at all. For to what purpose ^c such Calculations serve when their Catalogues of Stars, many Centuries later are acknowledged to have contain'd nothing more than bare Names, without Longitudes Latitudes Right Ascensions or Declinations. Such their Catalogues were, if they deserve that Name, if were made under if Name of Emperors call'd Souy, or between A. D. 551 & 620, & will be difficult to prove they were any thing else before if Jesuites introdu

Observ. at sup.
Tom. 2. p. 1.

ibid. p. 2.

Observat. de.
Tom. 2. p. 3.

Observat. de.
Tom. 2. p. 65.

Observ. at sup.
Tom. 2. p. 8.

ced there Tycho-Brahé's, or other European ones. As to Places of the Planets how little they were able to compute them, will appear from hence, of the ut most of Licou-hin & Lo-hia-hong in 17 year before 1662 pretended to was to calculate a plain rect-angled Triangle. In what manner they did this is not said, but it wd be well if Learned Fathers wd make it appear, of Chinese had in much later Times, any thing like a Table of natural Sines & Tangents. A small Skill in Mathematicks is requisite to apprehend ^{from hence,} how bungling their Astronomy must have been, & if so, much more that of 4 Ages preceding them. And will be ^{yet farther} hard to imagine that they knew how to find Places of Planets, when we are assur'd, that Tchang-tse-tsin about A. D. 550 was 1st person of introduced Equations into their Computations of Planets Motions; that Co-chiou-king abt A. D. 1280 was the first Chinese that knew any thing of Spherical Trigonometry, & that before 1st arrival of 1st Jesuits, they were intire Strangers to 1st Inclinations of 1st Planes of the ^{Planetary} Orbits.

Observat. &c.
Tom. 2. p. 58.
59.

Ald. p. 114.

Ald. p. 84.

Observ. at sup.
Tom. 2. p. 3.

After what hath been s^d, I think, we need but let the more to convince us of 1st small Acquaintance of 1st Chinese wth Astronomy. They tell us however themselves of from 1st time of Tsin-chi-hoang above mentioned, they had no expert Astronomer, no Books of Astronomy, nor known method of computing. All that remained were some confus'd Traditions, Catalogues of Stars & Constellations & fragments of Books. A mighty Encouragement all this, to expect reforming the European Astronomy, or Chronology, by 1st Chinese! What sort of Catalogues these were, hath been already observed, & we may without offence I presume beg to be excus'd from paying over-much 1st Deference to Chinese.

Tradition, at least till of Fathers have better determined wth Degree of Assent it deserves.

Observ. ut sup.
Tom. 2. p. 119.

A.D. 164. several Jew Families & other Subjects of the Western Empire came into China. At that time, as tis allowed by of Jesuites themselves, Ptolemy's Astronomy was in great Vogue all over of East, & they seem to suspect, if by this means of Chinese might get some faint Knowledge of it. Tis certainly from of time we meet wth Things unknown to of former Accounts of Astronomy. At this time tis P^r Tchang-heng made a Catalogue of 2500 Stars. Such a Catalogue as those already mentioned were, we may suppose it to be, if it was at all; for Tchang-heng's Book is lost, & wth Accounts we have of Him or His Works, depend on of Authority of others.

Observ. ut sup.
Tom. 2. p. 29.

Observ. ut sup.
Tom. 2. p. 44.

A.D. 284 Liou Kiang-ki, of first Chinese of is P^r to have known any thing of of Motion of of fixt Stars. This we see was 120 Years after of above mentioned arrival of of Jews in China. But either they must have been unskillfull relaters, or of Chinese Disciples, since Kiang-ki, it seems, made of motion to be at of rate of 1° in 50 years, whereas Ptolemy his well known made it 1° in a 100 years. It may be P^r indeed, that of Difference shews it c^d not be borrowed from Ptolemy, but then it shews at of same time, if it c^d not be of result of any Series of Observations, & that is as much, I think, as we need be concerned at. And this will appear yet farther by remarking of A.D. 460 it was made by Tson-tchong 1° in 45 solar years & 9 Lunar Months. At other times it was made yet different still, but never I think from Observations of of Stars of selves. The method it seems was by comparing the places of of Solstices in their own time, wth

Ibid.

Ibid. p. 52.

Observ. ut sup.
Tom. 2. p. 148.

2^d places in Reign of 3^d Emperor Yao, whom they suppose to have lived at a time coinciding wth 2300 years before x^t. Let us suppose 2^d to have been right in this, a thing we are by no means obliged to allow, yet as they never appear capable of taking of Solstices wth any tolerable Degree of Accuracy, we cannot wonder at any mistakes we may meet wth.

Observ. ut sup.
Tom. 2. p. 6.

ibid. p. 24.

ibid. p. 58. 55.

critical learning

You have seen I^t all along constant mention made of Chinese Calculations; ~~except~~ ^{very} of word used by Fathers of ~~tho~~ I am sensible ^{that Term} it will be apt to lead of unwary into great Errors. For if most of can be made of of Calculations is nothing more, than finding of Places of of Luminaries by Numbers, expressing their Periods & parts of Periods, or in other words by their mean Motions. For as to of Sun we are assured they made His Motion one Chinese Degree in a day, without knowing any thing of an Equation necessary to correct it. It was not till of 3^d Century that they had any Equation for of Moon & Tchang-tse-tsun abt A. D. 550, as we have seen, was of first of use any for the Planets. 'Tis observable however wth regard to this Author, if there are no writings of His extant, & therefore what is here s^d of Him, may be nothing more of Report. Among an idle, vain, ^{people unacquainted with} ~~unreliable~~ of People, & round Apertions pass for Proof, & in of manner, be good Question, of Chinese have frequently impos'd up on credulous unskillful Europeans.

ibid. p. 71. 72. 56

A. D. 618 began of Reign of 4th Emperors of of Race of Tang, & abt this time other Western Strangers came into China. From them therefore of Chinese might learn what farther improvements we may ^{besides those} ~~chance~~ chance to meet with in of Astronomy, & for w^{ch} they are ^{ascribed} ~~are~~ indebted to the Jesuites.

It was observed but now, of in comput

ibid. p. 16.

ing of Places of the heavenly Bodies, of Chinese at best knew nothing but their mean Motions. But in such Computations, 'tis necessary to begin from some Radix or other. European Tables generally begin wth the Commencement of the Christian Era. The Chinese appear never to have known any. One Epoch indeed they have, but intirely imaginary, called Chang-Quen, & w^{ch} began some time or other at Midnight, at the moment of the Winter Solstice, when the Sun, Moon & 5 Planets π , γ , δ , ϵ , ζ , were all in Conjunction, & the Moon without any Latitude.

ibid.

ibid. p. 18.

This extraordinary Epoch began according to these able Astronomers 1,43,127 Years before the Winter Solstice, in the year before J^{r} 104. What time the Epoch came first into use is not known, but the Fathers think, & it must be acknowledged wth great Probabilitty, that it is not older than the burning of Books under Tsin-chi-koeng, or as we have already seen, the year before J^{r} 246. Sh^d we place it how ever many Ages later, or say that it never served any real use at all, we may perhaps come much nearer the Truth.

ibid. p. 17.

For the Chinese Astronomers as the Fathers observe, have spent an infinite deal of Time and Pains, in searching out the Chang-Quen, & w^{ch} has been carried up they say, by some, two by others three Millions of Years beyond the Time it was above fixt at. But this shews to a Demonstration that it is an Epoch purely fictitious, & if it was real it could be only of an Astronomical Nature, & if they must be stupidly stupid, & sh^d from thence collect, that the Chinese had any Historical Memoires of so ancient a Date. For the Fathers them

ibid. p. 17.

selves allow, if of Opinion w^h ascribes to of world a Duration of some Millions of Years, is neither of general Opinion of of Chinese, nor of any ancient standing among them.

From what hath been here offered, I think it is pretty evident, if how ingenious soever of Chinese may be in works of Art, their Talents do not lie towards Mathematicks & Astronomy. For was not this of Case, must it not be surprising, that having, as they say, so long a Series of Observations in of one Science, & of Professors in of other, they sh^d never have been able to get beyond of first Elements of either?

It is not my Design to enter into any Controversy with of Learned Fathers of of Society of Jesus. The world hath been fregetly indebted to them for their Philosophical Labours, & will be so again, when they shall have considered of Chinese History wth proper Accuracy, & told us in what manner they have been able to preserve Acc^t & Observations of so ancient a Date. Publick Librarys tis allowed they have none, nor doth it appear they ever had. Where then E things so usefull, as of generallity must have thought Astronomical Observations, be reposit^d? When intrusted to private hands, they must have run great Risque of being destroyed by wars, by Fires, & in popular Com^motions, w^{ch} must frequently have happened in so long a Course of Years.

Let us suppose things of of Sort are of more value to of Chinese Commonalty, or if you please their Nobility, than they are to of European &

Observ. ut sup.
Tom. 2. Pref.

Observ. ut sup.
Tom. 2. Pref.

that they w^d lay hands on every thing they could meet with, & when once in their Possession w^d preserve it wth a religious exactness. But whence then comes it to pass, that it is so difficult a matter in China to meet wth Books upon these Subjects, to understand them when found, or to get any Assistance from y^e Natives towards understanding them?

Observ. ut sup.
Tom. 2. Pref.

But besides, are not writings thus kept in private Persons Custody, unless carefully laid by, apt to be scribbled on by Suiotists, so y^t it may be hard to distinguish many times y^e Text from what may be called y^e Comment? Is not this in fact according to y^e Learned Fathers own Account, very frequently y^e Case? May not by this means a Callulation, if it must be so called, be mistook for an Observation, if it must be so called, be mistook for an Observation made many hundred Years before? 'Tis confessed that Martini was imposed upon in this very manner, & 'tis much to be suspected, that He hath not been y^e only one.

Observ. ut sup.
Tom. 2. p. 103.

You see so that I have produced the Jesuites own Authority for every thing here offered. I designed to have cited their words at length, but that I found w^d have swelled these papers above y^e size of a Letter. I am not conscious to my self of having misrepresented them, I am sure I have not done it willfully for I have nothing in my view but Truth.

I am so

Your most Obedient
Humble Serv^t
G Costard

March 12. 1747.

To P Collinson From M Bealoke att ⁷⁸⁹⁷⁷ N^o 46.
Newport in the Isle of Wight

Jan^y 13th 1746

^{46/47} Read
Since I have ^{the} Transaction N^o 476
with Respect to the Sparkling Lady
who could communicate a kind of
Electrical Fire to Her Garments.

I can give you an Instance
nearly like It, of a Lady who was
surprised att such an appearance from
a flannel Pettycoat, which she happen'd
to shake in the Dark. but at last, we
found, that new Flannel, after some time,
wearing, would acquire this Property.
But ^{that it} Lost It, by being Washed.

25

VII. Extract of a Letter from Mr. Benj. Cooke
to Mr. Peter Collinson F. R. S. concerning the prop
of New Lannel Sparkling in the dark.

New Lannel Sparkling in
the dark.

March 19. 1746-7.

Pr. No. 483. VII.

9:30
9:27.4
2.56

9:26.30
8:24.19
2.21

75977

No. 6

XII. An Account of a very learned Divine who was
born with two tongues, communicated to the Royal
Society by C. Mortimer. M.D. Secr. R.S. + M.H.

In a m.s. account of the Life of the Rev. Mr. Henry
Wharton, chaplain to Archbp. Sancroft, written by
himself I have met with the following passage

"mihi quidem ex utero materno exeunti duplex erat
Lingua, utraque ejusdem figura ^{ac} magnitudinis;
inferiorem effundendam esse clamaverunt mulieres
obstetricis. Verum id noluit Mater puerpera. Pietati
ejus obfecundavit fortuna. Lingua enim inferior pau-
latim emareuit, & in exiguum pifque hand majorem
lingulam, quæ hodiernum manet, contracta est. Lingua
interim superior ad justam crevit magnitudinem, quam plu-
rimis longis profundisque fuleis distincta, an vulneribus
claniata, dicam! quæ parallelis fitu posita una cum
lingua creverunt, neque unquam coitura esse videntur.

Nat. Nov. 14. 1664. Ob. 1694-5. Mart. 5. æt. 31.

It appears by this journal of himself that he was always
infirm & sickly.

Rev. Mr. Weston with
2 tongues.

10th March 1744

Pr. Br. A 86. XIII.

Dear Brother

+ Mth

78977

I have now the Satisfaction to ac^k you that by Gods blessing
upon the means his providence directed me to the use of, I am so far
recovered of my Distemper (no other than the Stone) that I have not the
least doubt of being quite free from it in a little time: I am continually
voiding Stones, all broken, white of the out side, without much pain: I
can now walk twice as fast as I could 3 weeks since ^{without} uneasiness, nay
I rid the other day at once 24 nears miles, trotting most of it
without pain or change of urine, in one word I can be as certain
of the Dissolving power of my medicine, as I can be without seeing
the Dissolution. Dr. Morgan advised me to drink a pint of Lime water
every day: Col. Morgan & Lady coming on a visit, advised me to take 4
pills of alicant Soap morning & evening, upon wh^{ch} I resolved to add ~~the~~
8 Soap pills to the use of the Lime water, only instead of the Quantity
proposed, I took ^{between} 20 & 30 a day, amounting to near an Ounce
wh^{ch} I thought I might safely do, well knowing that M^r. Stephens dosage
amounted to almost 3 ounces of Soap beside other ingredients; afterwards
I found in an Extract published in the Magazine, taken from Dr. Whitt's
Treatise about Dissolving the Stones in humane bodies, the prescription
of the very medicine, I used, only a quart or 3 pints of Lime water
instead of a pint, upon wh^{ch} I doubled my Quantity, I have since
borrowed the treatise my self, and woud earnestly recommend the reading

of it to every person troubled with that distemper, the Experi-
ence the Dr. has had of the real Effects of ^{this prescription} in this Disorder,
joined with the many experiments I have found of the Dissolving
power of Lime water and Soap gave me great Satisfaction. I have
used with great success Stone limes, newly calcined, but by those Experi-
ments I had seen that the dissolving power of lime water made of oyster
Shells is almost double to that of lime Stone, there are two good
Qualities attending these remedies, the first is that they are
Cheap, easily come at and prepared by ones self 2^d That they
may be safely used for a long time without danger to health
I can vouch by my own experience for a quart of lime water
and an ounce of Soap, has never given me the least nauseating
towards of Sp. or abatement of appetite & I think I was never better
in health than I am now. My Motive for being so particular in
this Affair is ^{easy to} Desiring to be instructed by your means of
giving others in so unhappy a condition, being firmly persuaded
that what has already so far relieved me, will if prescribed Dissolve
Stones of greater magnitude than I suppose mine to be

From Your Affectionate Brother

(signed) Robt. Lucas

Abercromby

Dec. 1746.?

IX. a Letter from M^r. Rob^t. Lucas concerning
the Relief he found in the Stone from the use of
allicant Soap & Lime water, to his Brother the
Rev^d. M^r. Rich^d. Lucas F. R. S.

Ms. Br. 493. IX.

March 26. 1747.

a copy.

Mr Rob^t Lucas to
the Rev^d M^r Lucas F. R. S.
of his curing himself
of the Stone.

47
+ MS. 78977
To Martin Folkes Esquire President of the Royal Society

Sir

I now put into your hands the other collection of letters, for the use of the Royal Society, which I promised sometime since.

Several of these letters were written by Mr Hartlib D^r J^{no} Beale and other learned and ingenious persons who were very instrumental in forming & afterwards useful members of that illustrious Society, which now subsists; and may it flourish to the end of time.

They are originals, except a very few transcribed by some friends of the authors about the time when they were written, and none of them, so far as it appears to me, have been printed. The substance, indeed, of Morhoffs letter to Mr Oldenburg may be supposed to be contained in his treatise, entitled Stentor $\Upsilon\Lambda\Lambda\text{ΟΚΛΑΣΤΗΣ}$. and if the two curious letters of Dr Wallis to Mr Rob^t Wood afterwards L^d. and Ma^r. of the mathematical school at Chr^s Hospital, should be any ^{one example of approximation} where printed, yet being in the authors own hand, I imagined the originals, would be judg'd worth preserving.

The principal motive which first led me to collect these and the former papers together, was the hope I had that they might be useful in affording some assistance in compiling an History of the Royal Society, should such a design be set on foot. For this reason I have placed them in the order of the time of their dates, and numbered them: general heads of their contents are drawn up and prefixed in the same order; and those without dates, with some few papers, which may be subservient to the same design, are subjoined.

I am not without hopes of meeting with some more papers, which may be proper to accompany these, and have accordingly prepar'd room for them in the same Chart-book - When they shall come to hand, they will with very great pleasure be added, by

Yoursth
Tooting April 30 1747.

Sir

Your (and the Royal Society's)
most obedient &

faithful humble Serv^t
Henry Meley

apr: 30. 1747.

Further observations on the Distemper now raging among the Cow-kind by the Publisher of these Transactions.

(Nov. 21.)

67726

Since my former paper on this subject, I have had opportunities of being present when 3 cows have been fleeced & ^{the} lungs in all were inflamed & blistered & the guts in some places inflamed, in others livid, the Gall bladders exceeding large: a collar-makers man who has been assisting in fleecing above 100. dead Cows assures me these are the general appearances in them all; except that in one he met with a large bag full of corruption ~~in~~ ^{between the} ~~the~~ ^{bag enclosing} the heart & the back bone; in another he found the Gall bladder quite contracted & shrivelled up, having little or no Gall in it, & in several he found scirrhous knots in the Livers.

Nov. 26. I desired Mr. Hill an ingenious Apothecary in Westminster to accompany me to see a Cow Dissected & to help me examine every thing very carefully, having got her drawn into a Shed to defend us from the weather.

When the skin was taken off she appeared very fat, the muscles lookt of a darker colour than usual: on opening the abdomen the caul appeared very fat, the paunch was greatly distended, on making a puncture much wind gushing out; it had in it a great deal of food; the inside lookt well & did not peel; the 2^d & 3^d Stomach or the Omasum, ^{as also the 4th stomach or abomasum} were almost empty, but lookt well the liver was firm, well coloured, & sound, except a few Scirrhous knots about the size of Nutmegs: the Gall bladder was exceeding large & full of very fluid Gall: the guts were inflamed in many places, the colon & cæcum livid: I had the curiosity to have them measured, from the anus to the infertion of the cæcum there were 12 yards (the cæcum was an ell long) & from the cæcum to the pylorus there were 52 yards. The midriff was much swelled & inflamed. The Lungs were swell'd inflamed, adhered in some places to the pleura, & almost wholly cover'd with bladders of water: there was no appearance of any Inflammation on the pleura, or in either the internal or external intercostal muscles: the Aspera Arteria or Windpipe was inflamed greatly thro'out its whole course especially its inside; but the Gullet, which lay so near it was not in the least inflamed. The heart was of its natural size, the pericardium full of very fluid blood, probably from the bursting of some branch of the coronary artery ~~from the~~ caused by the extraordinary accumulation of blood in the ~~the~~ right ventricle, for the Venæ cava, & right ventricle of the heart & the lungs were turgid

& full of black coagulated blood, tho' this cow ^{had} been dead 12 or 14 hours; the lungs were likewise turgid with blood, but little or none was found in the left ventricle or aorta; the obstruction seemed to have been so great in the lungs, that very little blood could pass thro' them from the right to the left ventricle of the heart, & therefore evidently evinces the existence of a confirmed peripneumony. All the membranes lining the nostrils & the spongy bones thereof were quite turgid with blood & in the highest state of inflammation. The greater & lesser Brain looked fair & well ~~being~~ seeming no way distempered.

I have not seen in any cows I have examined any cutaneous sores or ulcerations, nothing like the Boils, Carbuncles &c. described by authors as the constant concomitants of the plague in men: nor does there seem to be any attempt of nature to fling off the Distemper by any internal impostumation or Discharge unless by the running at the nose & by the bilious stools; the few ^{or bilious urine} which have recovered, have been such as have been kept within Doors very warm, have been blooded once, twice or oftener, have had warm mashes of Malt & Brann given them & warm Drenches of warm herbs such as Rose-mary, Wormwood & ground Ivy with honey or Treacle, & have neither purged at all or but little; & when they have not purged at all their Urine has been observed to be as high colour'd as porter's beer.

I am informed by the Farness & Cowlecher of an horse or a Cow will bear to have 2 gallons of blood taken away without fainting. One cow, I have seen, within about a month or 6 weeks of her calving time, was taken with the running at the nose & shortness of breath, the owner of her immediately took away out of the neck 5 quarts of blood by measure, & gave her a warm mash of Malt once in 6 or 8 hours; next day he cut her tail & let her bleed 2 hours; the day after he took away 2 quarts from under the Tongue & so continued bleeding her at 14 or 15 hours distance for 7 times; she did not purge at all; ~~but~~ her Urine was as high colour'd as coffee at first, but grew paler & paler every time of bleeding; she soon recover'd, now eats heartily, looks brisk & ^{has} not sunk her calf.

The concern the Cowkeepers are under for the loss of their substance, the various methods offer'd to them & their want of judgement either to chuse the most rational, or their want of accuracy in making experiments & following directions, is quite discouraging & is the reason why none of them have pursued any

Regimen so steadily as to give one any opportunity of making any conclusions from it: indeed several own to me they are quite bewildered, not knowing wh way to turn themselves or whose advice to follow, what one says being quite contrary to the Directions given by another; Some to whom I have given my Directions have bled once, have given the purge once, but have not given the Oily Branch, or have given this once & have not repeated it, others have given the Chalky Branch once & not repeated it & have not followed the other parts of my Instructions; so that I am sorry to find that I can have no satisfactory Experiments made; yet as the State of the Disease seems so evidently to be a Peripneumony or Inflammation of the Lungs Wind pipe & Nostrils, attended with an abundance of Galls I can't forbear urging to the publick the following method

while well

Give to all Cows in general, ~~before they fall ill~~ $\frac{1}{2}$ oz. or 1. oz. (according to the size of ~~cow~~ of a cow) of Crocus metallorum. as soon as a Cow falls off her meat give her another Dose of Crocus metallorum; ~~and~~ & give her warm mashes of Malt, Bran &c: when she runs at the Nose, lay a bag of malt meal ~~dressed~~ ^{moistened} with boiling water upon her forehead & nose, tying it to her ^{morning & evening} horns; pour warm vinegar & salt into the nostrils; if a short cough or Difficulty of breathing comes on bleed her 1 quart twice a day ^{for 3 or 4 days} & every 6 hours give the Oily Branch. ~~for 3 or 4 days~~ If a purging comes on, give another Dose of the Crocus metallorum, if it continue give the Chalky Branch every 6 hours, & if it does not abate, inject the same in 24. hours, inject the same Mixture by way of Glyster; & if the Husky cough continues with the purging give the Oily Branch one 3 hours & the Chalky Branch the next 3 hours.

Most of the Cows which have recovered from this Distemper recover their Milk again as their appetites mend; but they are observed to have scabby eruptions come out in their Groins & axillae, which itch much, for a cow will stand still, hold out her leg & shew signs of great pleasure when a man scratches these pustules or scabs for her.

I am informed that some Cow-leeches have given Coloquintida & salt of Tartar, each 1. oz. in a quart of warm ale, but I imagine it must be too griping a purge & improper where the Guts are inflamed indeed I have not heard of any cows recovering which took it.

As for the cause of this Distemper I am still at a loss; I think it can't be owing to the food, because the Cows which had it first in Essex eat only Grass, turnips & hay or straw; the cows about London

eat

eat some, Grass, all Grains & Hay, some little or no Grass, but live chiefly on Grains, Turnips, offfalls from the garden ground, & hay. I am in doubt as to the air, the Spring & Summer were very wet & the ground very damp, the autumn was very dry & cold, the beginning of winter very damp & cold; the cows in Essex had the Distemper in Summer; it first began about London in autumn: ~~it spread itself~~ it has spread itself equally among cows wch have lain in the fields a nights & those wch stood in stables or sheds. it spread itself in Essex ^{as first} into such farms where they bought in strange calves or lean cows at market wch they did not know where they came from, but most probably from the hundreds where the Disease first broke out; but how it got thither, whether by importing any cattle from Flanders I know not: for surely there is too wide a tract of sea for any infectious miasmata to wafted over to that part of the country by the winds? this is certain the viscera concerned in Respiration are the parts chiefly affected. Its spreading here in England has been progressive, & therefore one may reasonably think it is not constitutional in the air, for then it ought to be universal every where, but that it is contagious & propagated by sick ~~cows~~ infected cows being mixt with well cows: therefore the ^{best} buying in calves or strange beasts, but every farmer keeping his herd by itself must be a great means of preventing the propagation of it: & housing the Cows a nights may be a proper preservative against it.

Further obs. on
Distemper among
cows.

Dec. 12. 1745.

Pr. No. 2. 477. XIII.

2-844-6-1745

1. II. a Third Acc^t. of the Distemper among
the cows. ~~Emostimus~~ M.D. fellow of the Royal College
of Physicians & Sec^y. of the Royal Society London.

During the Christmas Holidays, we sent
for some milk as usual from the Vineyard in St James
Park, none of the cows belonging to that house
having as yet caught the Distemper, tho' 3 had al-
ready died in the park, we used part of the milk
for chocolate & set part by for cream for the
next morning; the milk had a rank sourish smell
& tast like rank butter. the cream next morning
was more so, we boiled the milk, ^{the cream} which did not
curdle: so we used ^{the cream} it with tea, tho' the tast was not
very agreeable; ^{the milk boiled & curdled in the tea my family a friend} we none of us, nor ^{Dr} Batesby who
drank of it found any inconvenience from it; upon
sending the ^{next} morning following for more milk the
people refused selling any saying one cow was taken

* See ill & another was near dy^{ing}; this was the cow whose milk we
on ^{nothing} had had & she died in 48 hours; next day another fell ill
fide & was knockt on the head by the publick officer in abt.
a note 48 hours after ~~her~~ being seized: I had the curiosity
to see this cow opened, which was done the next day
but one: The inflammation in general in this creature
was greater than what I had before seen in any of
those, which die of the Distemper, this cow had been
blooded about 3 weeks before she was taken & once
as soon as taken; the cawl was greatly inflamed, the
paunch inflamed & the inner coat peeled off, espec-
ially that of the faidle, the gutts were all inflamed
the liver was much inflamed in some parts, in others
was turned livid the gale bladder was very large
& the gall ~~very~~ liquor: the Lungs adhered in
many places to the Pleura, were greatly in-
flamed & turgid with blood & were in many
places quite black; I did not find any of Watery
bladders on the surface of these as I did on all
the others I had seen opened.

Here's an instance of the most surprizingly quick progress
of this Distemper & to such a violent degree that I
do not think it in the power of medicines to have
prevented Death; but I think this case is still a further
confirmation of necessity of plentiful bleeding as soon

as a beast falls sick, especially if a shortness of breath ensues; this cow was not come to the stage of purging. From the Distemper getting into the Park, I think there is Reason to conclude it cannot arise solely from any fault in the food because the pasture is allways good there & from the great number of horses always kept low & the cows have plenty of Hay in Winter. ~~How it got into the park is very strange. I have been up fresh cows brought in there since walls fair in August: this is further very observable that the Distemper seems to be very infectious among the cow kind yet I do not hear any of the Deer have fallen ill which is much more likely to happen than to the Horses, because they chew the cud & these do not.~~

I humbly therefore suggest whether it would not be the most likely ^{means} to put a stop to the spreading of the Distemper to forbid any cows or calves being brought to market to be sold alive, or that any farmers should ~~not~~ buy in any fresh cattle for 6 months or till it is found yt the Distemper is entirely ceased; & yt all fat cattle should be kept carefully separated from the cows & calves & yt under severe penalties.

X I sent for some of the milk of the sick cow, after she had been about 12 hours ill, they could not get above 2.02. which was as thick as cream & yellow like cheese; in about 3 days keeping it turned of itself into a substance like cream cheese, without separating any serum it curdled being put into Bohea tea next morning in 8 days it dried away to an hardy cheesy substance, & in 14 days became quite dry like the rinde of Gloucester cheese it smelt like rank butter at first, but never corrupted or stunk.

5.
The 3^d. acct. of w^h
Distemper among
the cows.
Jan. 9. 1745/6.
Pr. Br. 478. II.

67461

Dr

67756
+ M^{rs}. N^o 54

I am extremely obliged to you for the Trouble —
you have taken in Corresponding with Prof^r —
Euter, upon the Russian Discoveries Eastward from Kamt-
schatka, and Communicating to me the accounts he had
of Behrings last voyage and of his Discovery of the —
Lands North East of Japan; which the Prof^r could only
have inaccurately, not having seen any Journal to fix the
Latitude and Longitude of the Countries he then discovered; —
But since Prof^r Euter, swayed by the opinion of Captⁿ —
Behring, seems still to Believe that the last Land he —
discovered is joined to California, which Country is now
known to be part of the Continent of America and not —
an Island; in which fact, of its being continuous to California,
I differ still in opinion from him; for if that were a —
Fact to be depended upon, I would candidly own, that there
could be no Passage from the Northwest of Hudsons Bay
to the western Ocean of America, without sailing near
70. of Longitude; the Distance of the North East Cape
of Asia from the Northwest of Hudsons Bay; in a parallel
almost as far North as the Polar Circle, before the —
Passage could be made to the Pacific Ocean, which might
therefore be very reasonably called an Impracticable. —
Passage, as it could not possibly be made in one Summer,
if at all; and since Prof^r Euter has been so kind as to
give me Captⁿ Behrings Reasons for Supporting his opinion,
which are principally from the small Distance he supposed
it was, from the Coast he discovered, to the western American
Coast at California; which he imagined was much nearer
his North East Cape of Asia, than it is in Fact; I must there-
fore in Return to the Prof^rs goodness, in communicating to
me all he has known in that Discovery, beg leave to give

you this further Trouble of Communicating to the Professor
my Reason for still differing from Behnings Opinion, that the
Land he discovered last, was part of the Continent of America,
or continuous with California; and if he find the Reasons
for supporting my opinion make it more probable, that there
still may be a large opening betwixt these New discovered
Countries and California, I am sensible it will give the
Ingenious & Learned Professor great Pleasure, to think we
may yet hope for a Passage by Hudsons Bay to the western
American Ocean, without being obstructed with Ice after getting
Hudsons Strait.~

The Professor Imagines, I might have been led astray by not
considering that the North East Cape of Asia is much more
easterly, than has been laid down in any former Charts,
which is now known accurately by the Eclipte of the Moon
observed by Capt. Behning at Kamtschatka.~

I have an Abstract of his Journal by me upon his first
Discovery in 1728, ^{& 1729} when he observed that Eclipte, and his
Calculation of the Long: from it, and stand by the Longitude
he has fix'd; and allow that his North East Cape to be in
the other Hemisphere, reckoning eastward, either from Paris
as first Meridian, or from London, which last I shall
follow; Behning fixes his North East Cape 126.7 East
Long: from Tobolsk, and Tobolsk is 86. East from Paris,
so the Cape is 212.7 East of Paris, or about 194 East
from London; By Capt. Middletons Observation of Jupiters
Satellite at Churchill River, in Hudsons Bay; that River is
95 West from London; which added to 194, makes 289. Con=
sequently the North East Cape of Asia, is 71 - distant from
Churchill, to amount 360. - which in the Lat. of 69. Com=
puting 8 Leagues to a Degree of Long: of which 20 make a
Degree of Latitude; the Distance betwixt that Cape and
Hudsons Bay would be 568 such Leagues.~

From the known Longitude of the North Cape of Japan
in 40° Latitude, which is fully & exactly known, from the ob-
servations made by the Jesuits at Peking, and is about 150°
East from London, and from the best computed Longitude of
California in 40° North Latitude, it lies in 130° Longitude
West from London, making together 280° . Leagues 80° —
for the Distance of California, from Japan; allowing
17 Leagues to a Degree of Longitude in 40° North Latitude, the
Distance would be about 1360 Leagues; by the same
Calculation California must be at least 700 ^{Leagues} 800° Leagues
from the North East Cape of Asia; so that in so great a
Space there may be very great Countries or Islands, without
supposing the New discovered Country, continuous to California,
and might well allow of an open Channel or Sea, from 50° to
100 Leagues wide, between the discovered Coast, and California.

By the acc^t given to Prof^r Enter, Behring sailed
Southwardly to the Isles of Japan, and from thence sailed
Eastwardly 50 German Miles, about 250° English Miles, which
makes about 80 Leagues, of 20° to a Degree. At that Distance
from Japan, he discovered Land, which he coasted Northwardly,
still approaching towards the North East Cape, without going
ashore, until he came to the Entrance of a great River, where
sinking his Boats and men ashore, they never returned, being either
lost, killed or detained by the natives, which made his Discovery
incomplete; his ^{body} being preserved, and ^{he} afterwards dyed in an uninhabited
Island. — as no Latitudes nor Longitudes are fixed, by this account
I must believe he sailed from Kamtschatka South East, & hence more
Southwardly than to 50° Latitude, and therefore found Land North East from
Japan; otherwise by coasting it Northwardly, he could never approach
the North East Cape, which is at least 40° Longitude East of Japan,
and if he made Land, 80 Leagues East of Japan, he must have sailed
North East, to make the North East Cape; I have therefore Reason to believe
this Coast was Part of that he saw in his full voyage, where he lost
his anchor, and is the Coast Gama discovered, and the Gulch afterwards

324775
Call the ~~unexplored~~ Land, east of the Straits of Uziex, which is at
least 700 Leagues west of any known Land of America, and
above 1000, near the latitude of Japan; So that, if I should allow 700
Leagues for Countries on Islands, east of his new discovered Coast,
there might still be a Passage of 100 Leagues, for the Southern
on Pacific Ocean to Communicate with Hudsons Bay, and to pass
such great Rides & Currents, as are found on the north west of
Hudsons Bay, as also a free Passage for the Whales, which are
seen in all the openings north west of that Bay, and are caught
there in numbers by the Eskimaux Savages; for as these can
go in by Hudsons Strait, from our Atlantic Ocean; it can't be
presumed that they should go up by Japan, towards the north east
Coast; and from thence go 70, or above 500 Leagues, to Hudsons
Bay, and be there in the month of June, and after playing until
September, return again the same way to the Southern Ocean
to pass the winter. - Now as Behring only coasted at a distance
he could not possibly know whether it was a Continuation great
Islands, the East of which seems the most probable; However
a few months now, if our Ships return safe, will give us a
certainty on one side or the other; altho' I am sanguine enough
to believe, they have by this time sailed through, and discovered
this so much wished for Passage. -

These be the Reasons I have still to expect success
in the attempt I have promoted; and if you think it may
give any satisfaction to Profess^r Guter, to know the Reason
that support my Belief of a practicable safe Passage, be-
pleased to communicate it to him, with my Compliments, for
the Trouble I have given him by you, and accept of my
best Acknowledgments for your Favours. - I am with the
greatest regard & esteem Sir
Castle Dobler Feb: 10th 1746/7

Yr Most Obedt^t Humble Serv^t

Arthur Dobbs

To Mr Wellstein Secretary
to his Royal Highness the Prince
of Wales. -

Mr. G. 1747.
Apr. 9. 1747.
Mr. G. 483. XII.

XIII. a Letter from Mr. Rich^d. Dunthorpe of the Rev^d.
Mr. mason Woodward. prof. of Nat. Hist. at Cambridge
concerning the Moon's motion.
+ ~~III~~ N^o. 25

Feb. 5. 1746-7.

Pr. Ser. 482. XIII.

To

The Rev^d. Mr. Mason

Fellow of Trinity College

these pres.

In the Preface to my Lunar Tables, I hinted that one Use of publishing those Tables, would be the assisting of Persons desirous farther to rectify the Lunar Astronomy, by enabling ^{them} more readily to compare the Newtonian Theory with Observations.

Since the Publishing those Tables, I have spent some Time my self in that Comparison; and here send you the result, that you may communicate it to the Royal Society (if you think it deserves to be made publick)

As the Motion of every Secondary Planet must partake of the Errors in the Theory of its Primary, I thought proper, before I undertook the Examination of the Lunar Numbers, to compare those of the Sun with Observations. I compared several Sets of M^r. Flamsteeds Observations after the Method he himself teaches, in *Doctegom. Hist. Celest.* p. 133. & seqq. which, for many Reasons, I think the best Method hitherto used; and with the Concurrence of a Gentleman well skilled in these Matters determined, The mean Motion of the Sun, the last day of December at Noon, Anno 1700. O. S. $45^{\circ} 20' 43''.40$, of its Apogee $9^{\circ} 7' 36''.0$, and the greatest Equation of the Sun's Centre $1^{\circ} 55' 40''$; which I am fully perswaded are very near the truth.

The Theory of the Sun being thus settled, I proceeded to examine the Elements of the Lunar Astronomy: I began with Observations of Lunar Eclipses about the Equinoxes when the Apogee of the Moon was in the Sun's Quadratures; because at those Times I could conceive the Moon's Motion affected with no inequality, but the Annual one called by Newton the first Equation, and the Elliptic one called Prosthaphærisis: from a Comparison of such Observations I obtained the Moon's mean Longitude, which came out, at least, greater than in the Tables, and very nearly as Newton has it in the last Edition of his Principles.

I went on to examine the Place and Motion of the Apogee, and Theory of the increase and decrease of the Eccentricity, as well as the greatest and least Eccentricities themselves from the best Observations, and best situated, that I could procure) all which agreed so well with the Tables, about the Sun's mean Distances, that I dare venture to make no alteration therein: indeed I think the 6th Equation does not so well account for the Variation of the Motion of the Apogee, and change of the Eccentricity, according to the greater or lesser distance of the Sun from the Earth; and therefore I set my self to compute what change this difference of the Sun's Action upon the Lunar Orbit would introduce in the Moon's place in every Situation of the Sun and Lunar Orbit; and found, after many tedious Computations, that the Sun being in Apogee this change where greatest would amount to about $4'$, and to $4'.16$ when the Sun is in Perigee. In other Distances of the Sun from the Earth, this greatest change is proportional to the Difference of the Cubes of the mean and present Distances, and in every Situation of the Moon, and of her Orbit, the present is to the greatest Equation nearly as the Sine of the Epoch of the Moon's mean Anomaly above twice the Annual Argument to Apogee: It increases the Moon's Longitude when the Sun is in his ^{Apogee} ~~Perigee~~ Semicircle, and that Excess ^{is} greater than 180° ; and diminishes it when otherwise.

In fine, I compared the Theory of the Moon, as to her Longitude, with several Observations, as well in the Octants and Semi Octants, as in the Syzygies & Quadratures, and found such an Agreement, when the above Corrections were made, as seemed rather to be wished than hoped for; considering the many inequalities wherewith the Sun's Action disturbs the Motion of the Moon, and the Defects to which the best Observations I have hitherto met withall are liable.

I have compared 100 observed Longitudes of the Moon with the Tables, viz. 25 Eclipses of the Moon, all except the first, taken from Flamsteeds *Historia Celestis*, the Philosophical Transactions, and the Memoirs of the Royal Academy of Sciences, the two great Eclipses of the Sun in 1706 and 1715, 25 select places of the Moon from Flamsteeds *Historia Celestis*, and 48 of those Longitudes of the Moon computed from Flamsteeds observations by D^r. Halley (as I suppose) printed in the first Edition of the *Historia Celestis*. They are as follows,

- + If this Equation be increased and diminished in a direct Ratio of the Moon's horizontal Parallax, it will become more exact. And, I think, if it were always diminished by a 4th or perhaps a 3^d part, it would agree better with Observations.

25 Eclipses of the Moon and 2 of the Sun compared with the Tables, corrected as above.

Place of observation	Anno Dom.	App ^t Time at Greenwich	True place obs ^d	M. line ①	M. line ②	Ann. long ^t	③ place computed	diff. from observat.
Dantzick	1652	Septem. 7. 6. 21. 35	11. 25. 25. 51	2. 20	6. 2	0. 3	11. 25. 26. 10	+ 0. 19
a Greenwich	1670	Septem. 18. 14. 36. 48	0. 6. 13. 29	3. 1	6. 0	0. 0	0. 6. 16. 37	+ 3. 8
	1678	Octob. 19. 8. 21. 54	1. 6. 47. 9	4. 1	7. 16	2. 2	1. 6. 46. 38	- 0. 31
	1682	Febr ^y 11. 10. 58. 52	5. 3. 47. 3	7. 25	6. 28	1. 14	5. 3. 46. 14	- 0. 49
Paris	1684	Decem. 11. 10. 47. 6	3. 1. 11. 20	5. 24	1. 8	7. 16	3. 1. 13. 7	+ 1. 47
Dantzick	1685	Novem. 30. 10. 35. 30	2. 19. 40. 0	5. 13	11. 26	5. 25	2. 19. 39. 46	- 0. 14
Dublin	1686	Novem. 19. 11. 13. 11	2. 8. 11. 57	5. 2	10. 12	4. 4	2. 8. 11. 28	- 0. 29
Greenwich	1689	Septem. 18. 14. 30. 37	0. 6. 32. 8	3. 1	4. 24	10. 8	0. 6. 34. 16	+ 2. 8
	1690	March 14. 9. 36. 30	6. 4. 37. 11	8. 25	9. 18	3. 15	6. 4. 37. 28	+ 0. 17
	1696	May 6. 12. 2. 0	7. 26. 53. 14	10. 18	3. 1	8. 28	7. 26. 53. 40	+ 0. 26
Paris	1699	March 5. 7. 13. 44	5. 25. 32. 27	8. 16	8. 27	3. 1	5. 25. 32. 14	- 0. 13
New England	1703	Decem. 11. 18. 29. 8	3. 0. 49. 36	5. 23	11. 24	5. 23	3. 0. 48. 51	- 1. 5
Marshall's	1706	May 0. 21. 13. 57	1. 20. 46. 44	10. 12	6. 22	7. 6	1. 20. 46. 32	- 0. 12
Greenwich		Octob. 10. 7. 10. 10	0. 28. 0. 20	3. 22	5. 27	11. 25	0. 27. 58. 47	- 1. 33
	1707	April 5. 13. 39. 0	6. 26. 18. 33	9. 17	11. 9	5. 3	6. 26. 19. 9	+ 0. 36
	1708	Septem. 18. 9. 10. 25	0. 6. 39. 23	3. 1	2. 12	8. 7	0. 6. 39. 19	- 0. 4
Paris	1712	Jan ^y 12. 7. 34. 0	4. 2. 57. 47	6. 24	1. 17	7. 25	4. 2. 58. 37	+ 0. 50
	1713	Novem. 20. 15. 27. 30	2. 9. 58. 21	5. 3	9. 21	3. 17	2. 9. 56. 56	- 1. 25
London	1715	April 21. 21. 11. 5	1. 12. 0. 22	10. 3	6. 12	6. 22	1. 11. 59. 36	- 0. 46
Paris	1717	March 15. 15. 7. 4	6. 6. 23. 50	8. 26	8. 22	2. 27	6. 6. 23. 31	- 0. 19
	1719	Aug ^t 18. 8. 22. 46	11. 5. 42. 8	1. 29	10. 28	4. 20	11. 5. 42. 39	+ 0. 31
	1722	June 17. 13. 46. 10	9. 6. 47. 43	11. 29	5. 8	10. 25	9. 6. 47. 33	- 0. 10
	1724	Octob. 20. 15. 40. 40	1. 9. 0. 0	4. 2	5. 25	11. 22	1. 8. 58. 50	- 1. 10
	1729	Feb ^y 2. 8. 42. 55	4. 25. 13. 39	7. 16	3. 25	9. 14	4. 25. 14. 40	+ 1. 1
		July 28. 13. 0. 0	10. 16. 15. 28	1. 9	8. 3	2. 15	10. 16. 16. 26	+ 0. 58
	1731	June 8. 13. 47. 51	8. 28. 9. 58	11. 19	4. 27	10. 11	8. 28. 8. 45	- 1. 13
	1732	Novem. 20. 9. 49. 25	2. 10. 3. 54	5. 3	7. 8	1. 24	2. 10. 0. 56	- 2. 58

25 Places of the Moon computed by my self from Flamsteed's observations, compared with the Tables.

Anno Dom.	App ^t Time at Greenwich	True place obs ^d	M. line ①	M. line ②	Ann. long ^t	③ place computed	diff. from observat.
1684	March 13. 8. 9. 8	2. 28. 48. 40	8. 25	2. 25	11. 19	2. 28. 50. 44	+ 2. 4
1693	March 6. 7. 22. 48	3. 16. 43. 12	8. 17	3. 11	11. 7	3. 16. 44. 0	+ 0. 48
1694	Octob. 11. 18. 12. 34	3. 28. 34. 2	3. 24	2. 27	5. 15	3. 28. 38. 21	+ 4. 19
1694	Feb ^y 27. 10. 29. 16	4. 27. 27. 31	8. 10	3. 12	9. 20	4. 27. 26. 48	- 0. 43
	Aug ^t 23. 11. 13. 54	11. 0. 19. 11	2. 5	8. 2	2. 21	11. 0. 21. 41	+ 2. 30
	Septem. 21. 10. 50. 31	11. 22. 47. 41	3. 3	9. 3	3. 17	11. 22. 49. 16	+ 1. 35
	Decem. 13. 6. 2. 53	0. 6. 28. 43	5. 25	8. 26	6. 0	0. 6. 29. 58	+ 1. 15
1695	Febr ^y 8. 3. 55. 22	1. 5. 14. 3	7. 21	9. 7	7. 22	1. 5. 12. 14	- 1. 49
	July 9. 5. 56. 14	7. 2. 2. 42	0. 20	3. 14	0. 2	7. 2. 3. 4	+ 0. 22
	Septem. 8. 8. 30. 26	10. 3. 0. 12	2. 20	5. 21	1. 24	10. 2. 59. 50	- 0. 22
1696	Jan ^y 16. 17. 29. 2	7. 5. 4. 20	6. 29	2. 29	5. 20	7. 5. 6. 11	+ 1. 51
	March 4. 9. 8. 13	4. 12. 2. 24	8. 16	11. 8	7. 3	4. 12. 0. 59	- 1. 25
1697	Feb ^y 18. 6. 29. 47	2. 20. 52. 29	8. 2	8. 19	5. 10	2. 20. 51. 7	- 1. 22
	19. 7. 22. 57	3. 4. 18. 16	8. 3	9. 2	5. 11	3. 4. 17. 54	- 0. 22
	Septem. 15. 7. 54. 40	10. 1. 7. 8	2. 28	3. 21	11. 9	10. 1. 5. 38	- 1. 30
1698	Septem. 8. 11. 2. 35	11. 12. 15. 6	2. 21	3. 24	9. 23	11. 12. 15. 27	+ 0. 21
	Novem. 27. 3. 49. 54	10. 11. 33. 39	5. 9	2. 3	0. 3	10. 11. 31. 13	- 2. 26
1699	March 5. 12. 8. 23	5. 28. 15. 59	8. 16	8. 29	3. 1	5. 28. 16. 11	+ 0. 12
1701	Septem. 28. 6. 55. 23	9. 27. 13. 17	3. 10	9. 9	6. 8	9. 27. 12. 49	- 0. 28
1702	Octob. 16. 6. 16. 49	10. 3. 25. 41	3. 28	8. 11	5. 13	10. 3. 26. 27	+ 0. 46
1703	Septem. 13. 11. 58. 50	11. 28. 50. 30	2. 25	8. 28	3. 3	11. 28. 53. 30	+ 3. 0
	Octob. 6. 6. 28. 39	9. 27. 1. 44	3. 18	7. 4	3. 23	9. 27. 0. 38	- 1. 6
1706	Octob. 10. 12. 11. 10	1. 1. 11. 15	3. 22	6. 0	11. 25	1. 1. 10. 17	- 0. 58
1714	Septem. 6. 6. 34. 7	9. 3. 44. 13	2. 18	3. 17	0. 0	9. 3. 45. 24	+ 1. 11
* 1694	Septem. 15. 5. 34. 13	8. 27. 1. 34	2. 27	6. 10	3. 11	8. 27. 1. 41	+ 0. 7

* N.B. 1694 Septem. 15. Should have been inserted between 1694 Aug 23 & Sept 21.

48. Places of the Moon computed by D^r Halley from Flamsteeds observations, compared with the Tables. 69293

Anno Dom.	App ^t Time at Greenwich	True place observ ^d	M. an ^o 0	M. an ^o 3	Ann. long ^t	S. place computed	diff. from observ ^d
1689	Novem. 16. 11. 59. 0	2. 6. 12. 43	4. 29	6. 1	0. 0	2. 6. 14. 40	+1. 57
	Decem. 9. 6. 1. 0	11. 28. 48. 54	5. 21	3. 21	0. 20	11. 28. 48. 40	-0. 14
	10. 6. 46. 35	0. 12. 46. 49	5. 22	4. 4	0. 21	0. 12. 47. 14	+0. 25
	12. 8. 26. 33	1. 12. 13. 8	5. 24	5. 0	0. 23	1. 12. 10. 39	-2. 29
	13. 9. 24. 30	1. 27. 38. 36	5. 25	5. 14	0. 24	1. 27. 36. 16	-2. 20
1690	Jan ^y 16. 12. 42. 0	3. 15. 14. 54	5. 28	6. 24	0. 27	3. 15. 15. 48	+0. 54
	4. 3. 3. 46	11. 11. 12. 59	6. 17	2. 24	1. 14	11. 11. 13. 8	+0. 9
	6. 4. 30. 15	0. 8. 13. 31	6. 19	3. 20	1. 16	0. 8. 14. 36	+1. 5
	10. 7. 59. 22	2. 6. 11. 20	6. 23	5. 14	1. 19	2. 6. 10. 37	-0. 43
	12. 10. 8. 49	3. 7. 5. 21	6. 25	6. 12	1. 21	3. 7. 3. 39	-1. 22
b	Feb ^y 13. 11. 14. 0	3. 22. 36. 28	6. 26	6. 25	1. 22	3. 22. 35. 19	-1. 9
	2. 2. 25. 39	0. 3. 57. 24	7. 15	3. 5	2. 10	0. 3. 56. 9	-1. 15
	5. 4. 51. 10	1. 16. 31. 33	7. 18	4. 26	2. 12	1. 16. 34. 15	+2. 42
	7. 6. 48. 17	2. 15. 58. 14	7. 20	5. 24	2. 14	2. 15. 59. 13	+0. 59
	8. 7. 51. 54	3. 0. 56. 20	7. 21	6. 8	2. 15	3. 0. 56. 31	+0. 11
	10. 9. 56. 26	4. 0. 55. 24	7. 24	7. 6	2. 17	4. 0. 55. 1	-0. 23
	11. 10. 52. 31	4. 15. 42. 16	7. 25	7. 20	2. 18	4. 15. 41. 49	-0. 27
	14. 13. 19. 31	5. 28. 12. 40	7. 28	9. 2	2. 21	5. 28. 11. 44	-0. 56
	19. 17. 3. 55	8. 2. 23. 26	8. 3	11. 12	2. 25	8. 2. 21. 17	-2. 9
	21. 18. 45. 37	8. 27. 1. 25	8. 5	0. 10	2. 27	8. 26. 59. 6	-2. 19
	March 22. 19. 37. 40	9. 9. 21. 32	8. 6	0. 24	2. 28	9. 9. 23. 26	+1. 54
	7. 5. 50. 38	2. 26. 22. 50	8. 18	6. 12	3. 9	2. 26. 25. 20	+2. 30
	11. 9. 43. 23	4. 24. 31. 45	8. 22	8. 8	3. 13	4. 24. 33. 15	+1. 30
	12. 10. 33. 9	5. 8. 31. 54	8. 23	8. 22	3. 14	5. 8. 33. 11	+1. 17
	13. 11. 18. 30	5. 22. 16. 37	8. 24	9. 5	3. 15	5. 22. 17. 26	+0. 49
b	April 14. 12. 2. 18	6. 5. 44. 39	8. 25	9. 19	3. 15	6. 5. 45. 18	+0. 39
	25. 20. 54. 54	10. 24. 54. 20	9. 6	2. 21	3. 25	10. 24. 53. 20	-1. 0
	7. 7. 48. 46	4. 20. 8. 0	9. 19	8. 5	4. 6	4. 20. 9. 18	+1. 18
	8. 8. 36. 30	5. 3. 59. 40	9. 20	8. 19	4. 7	5. 4. 0. 34	+0. 54
	9. 9. 22. 0	5. 17. 34. 42	9. 21	9. 2	4. 8	5. 17. 35. 40	+0. 58
	10. 10. 5. 19	6. 0. 54. 50	9. 22	9. 16	4. 9	6. 0. 55. 35	+0. 45
	11. 10. 47. 56	6. 14. 0. 8	9. 23	9. 29	4. 10	6. 14. 2. 13	+2. 5
	12. 11. 30. 56	6. 26. 55. 56	9. 24	10. 12	4. 11	6. 26. 56. 55	+0. 59
	13. 12. 17. 31	7. 9. 41. 19	9. 25	10. 26	4. 12	7. 9. 42. 20	+1. 1
	14. 13. 4. 0	7. 22. 16. 0	9. 26	11. 9	4. 12	7. 22. 17. 34	+1. 34
	15. 13. 52. 44	8. 4. 45. 10	9. 27	11. 23	4. 13	8. 4. 44. 47	-0. 23
	18. 16. 26. 47	9. 11. 43. 52	10. 0	1. 3	4. 16	9. 11. 44. 53	+1. 1
	20. 18. 5. 47	10. 6. 30. 55	10. 2	2. 0	4. 18	10. 6. 33. 2	+2. 7
	22. 19. 37. 30	11. 2. 8. 6	10. 4	2. 27	4. 20	11. 2. 5. 45	+0. 39
	24. 21. 6. 38	11. 29. 14. 33	10. 6	3. 23	4. 21	11. 29. 14. 30	-0. 3
b	May 14. 13. 30. 30	8. 25. 6. 53	10. 25	0. 7	5. 8	8. 25. 8. 48	+1. 55
	22. 19. 41. 40	0. 6. 50. 26	11. 3	3. 23	5. 15	0. 6. 49. 43	-0. 43
	June 3. 6. 2. 13	5. 22. 31. 45	11. 15	8. 19	5. 25	5. 22. 32. 6	+0. 21
	Septem. 1. 6. 48. 53	9. 2. 27. 42	2. 13	11. 15	8. 12	9. 2. 31. 20	+3. 38
	8. 12. 16. 42	0. 1. 44. 51	2. 21	2. 22	8. 18	0. 1. 44. 18	-0. 33
1691	Feb ^y 23. 3. 55. 10	1. 19. 39. 56	8. 6	3. 16	1. 18	1. 19. 40. 45	+0. 49
	March 11. 18. 27. 51	9. 7. 30. 5	8. 22	10. 23	2. 2	9. 7. 30. 23	+0. 18
	Septem. 22. 8. 51. 3	10. 19. 26. 24	3. 4	11. 16	7. 19	10. 19. 28. 32	+2. 8

a The Time of the Middle of this Eclipse here set down is from the Beginning and End, but Hevelius says he could not observe the beginning exactly; several intermediate Phases compared together shew the Middle to have been about 4^h sooner: to which the Moon's place computed is 0. 6. 14. 3^d, and diff. +34^s.

b, b, b. The Moon's Places observed on Feb. 2, April 7, and May 22 are computed by my self from the Observations; there being manifestly Errors, either of the Computation or Press, in those printed in the Hist. Calist.

Several observed Latitudes of the Moon which I have compared with the Tables, shew them to be very near the Truth, both in the Motion of the Nodes, and also in the Quantity and Variation of the Inclination.

Sam. J^r Your humb^l Serv^t

Rich^d. Dunthorne

Extract of a Letter from ~~Mr. Miller~~ ^{5403/26}
Dr Balthazar Erhart to Dr Mortimer
N^o. 37.

Memmingen in Suabia
Nov. 5. 1745.

I send you some of the products of my late labours;# one copy of weh I desire you will in my name present to Dr Hans Sloane. If the curious in England should be desirous to buy any copies of Moro's Italian work [on petrified & fossil Bodies#] I shall send some over.

I desire you will deliver a copy of the catalogue of my Dried Plants to Mr. Philip Miller for the use of the Garden at Chelsea, & inform him I shall be glad of his correspondence in Botanical Affairs.

Physikalische Nachricht von einer gegründeten neuen Meinung, welche den Ursprung dieser aus der Erden kommenden versteinerten Sachen. &c. Memmingen 1745.⁴⁰

De' Crostacei, e degli altri marini corpi, che si trovano su' monti, libri due di Anton. Lazzaro Moro Venez. 1740.

apr. 10. 1946

67769

Sir

+ Mth N^o. 7.

78977

5403/27

45/4

According to your Directions have sent
an Account of the Stone I left with you and
which is now fourteen years since I took it out of
the Bladder of a very large Mastiff about five
years old Belonging to the Porter of his Maj^{ty}
Dock Yard at Portsmouth The Dog died in
about three Days after receiving a Kick from
some one endeavouring to part him from another
Mastiff he was fighting with Being then an
Apprentice to the Surgeon of the Yard & hearing
the Dog was dead sent the Labourer who attended
the Surgery to get him for me in order to Dissect
When I had open'd the Abdomen I found it fill'd
with Bloody Urine & before having heard that
his Death was suppos'd to be occasion'd by the
Kick I immediately thought the Bladder must
be the part hurt which when I had cleans'd the
Abdomen I examin'd & found this large Stone
with the Bladder contracted close to it on every
Side & rent at the Bottom about three quarters
of an Inch so that what Urine came to the
Bladder was discharg'd into the Abdomen which
was plainly the cause of his Death.
When I first took it out it weigh'd Ten oz^z 3½

It is not more than Two Months since I cut it asunder when
finding it form'd upon (as I imagine it is) the piece of
Dog-grass thought it would not be an unacceptable present
to the Curious Therefore having some Affairs which call'd
me to Town brought it with me for that purpose
What is to be further remark'd is that I did not find any
the least particles of Gravel or Sand either in the Kidneys
or Ureters And that all the Bones (except the Ribs & Cranium)
are more or less affected as the Bones you have with of Stone
If when you shew it to the Royal Society it should be
thought deserving a place among their Curiosities It will
give me the greatest pleasure in having this opportunity
of presenting something worth the Notice of so Learned
and Ingenious a Body of Gentlemen

I am Sir with all Due Respect

Your Most Obed^t. humb^l. Serv^t.

London

29th Decem^r. 1746

W. J. J.

1. a Letter from Mr. Wm Fidge Surgeon at Portsmouth to Cromwell Mortimer M. D. Secret. R. concerning a Stone taken out of the bladder of Dog, which being cut asunder had a peice of Dog in its center.

To
Sir Cromwell Mortimer
Fellow of the College of
Physicians & Secretary
to the Royal Society

Yours
Wm Fidge

Ms. A. 82. I.

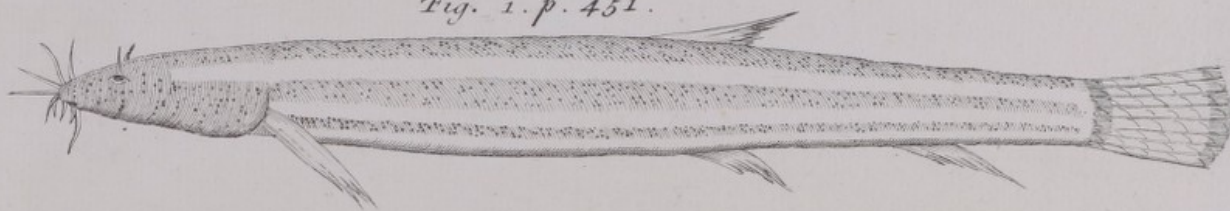
Jan. 5. 1745-7.



78917

N^o. 41

Fig. 1. p. 451.



III The figure of the *mustela fossilis*
communicated from Dr. Gronovius at
Leyden to Mr. Peter Collinson F. R. S.

Mustela fossilis, sive *Cobitis co-*
rulescens, lineis quinque nigris longitu-
dinalibus. Arted. Ichthyol. Gen. XI. 3. *
vide TAB. I. fig.

This fish was kept alive in a jar of water
a year wanting 9 days, without changing of water
& without any other food, than what it water af-
forded. They dig them out of the Sands near Wesel
in Holland. * Willoughby. hist. pic. p. 124. Tab. G. 34.

5403/28

~~Watson on Monday~~

~~No. 4. Jan. 29. 1746-7~~

~~May 12. 1740~~

Pr. Gr. 483. III.

March 12.
1746-7.

Letter from M^r. Jonas Hanway
to the Rev^d. M^r. Forster.

Dear Sir,

Having been very particular in my Enquiries concerning the Burning Air or Earth and also the Napht on the Western Coast of the Caspian & finding the Testimonys of her honest Men agree, I can venture to acquaint you, that what the Indians call the Ever-lasting Fire lies about 30 English Miles N. E. by E. from the City of Baku, on Dry rocky land. There are several Ancient Temples built with Stone, supposed to have been ^{all} dedicated to Fire, most of them are low arched Vaults not above 10 to 15. high; amongst the rest there is a Temple in which the Indians now Worship; near the Altar about 3 feet high, there is a large hollow Cane, from the end of which Issues a flame in Colour & gentleness not unlike a Lampe that burns with Spirits. The Indians affirm this Flame has continued ever since the Flood, & they believe it will last to the end of the World, that if it was resisted or suppressed in that place it would rise in some other.

By the Number of Temples it is probable here were formerly a great Number of Worshippers of Fire, as well Indians as Persians; they are called Gouers as is well known; at present there are only about 20 persons who live there constantly & go almost naked. In the Summer 'tis very hot, & in the Winter they live within doots, and can keep what Fire they please at a very cheap rate, as you will find presently. They live upon roots & Herbs for the most part, & are supposed to attend as Mediators for the Sins of many who are absent, and by their Application to this Fire, in which the Deity is supposed to be present & Visible, atone for the Sins of others.

Besides these there are some who Travel from India; In my Journey to Hamaden I overtook one who was returning home from thence, he was of that Auster Order who put some part of their bodies into certain unalterable Attitudes from

will also boil Water in a Pot; & they dress their Victuals in this manner; indeed they have no Fuel but a little Cow Dung of which they make Coals or Fowels. The flame may be extinguished the same as that of a Lamp burning with Spirits by blowing it. The Ground is dry & Stony & the more Stony the Ground the Stronger & clearer is the flame; It smells a little Sulphurous or rather in some degree like Naphth, but very little offensive. They also burn Lime to great perfection by means of this Fire, it immediately communicating it self to any Distance where the Earth is uncovered to receive it. As a proof of this, not long since 8 Horses were destroyed by an Accident of the Earths taking Fire under a Roof. Near this place they Dig Brimstone; & Naphth Springs are also near it. But the Chief place for the Naphth is Sweeti Island, a small Tract of Land on the Western Coast of the Caspian, & uninhabited except at such times as they load Naphth from thence; The Persians load it in their wretched Embarcations without Barrels or any Vessel, so that sometimes you see the Seas covered with it for leagues together. When the Weather is thick and hazy the Springs boil up the higher & the Naphth sometimes takes fire on the Surface of the Earth & runs lighted or burning into the Sea in great quantities, and to a great distance almost incredible. In clear Weather they do not boil up above 2 or 3 feet. The Springs form the Earth like a Hillock; They make Cisterns near the Springs & receive it by trows, & from thence take off the Naphth from the Surface, it having a Mixture of Water or heavier part with it. The greatest part is of a very dark grey, tis very unpleasant to the Smell, but generally used amongst the poorer sort of the Persians and other Neighbouring people as we use Oil in Lamps; & every Man has a Supply of it which they keep in Earthen Vessels under Ground to prevent any accidents from fire, of which it is extremely Susceptible. There is also a white Naphth but in very small quantities. The Russians drink this last as a Cordial or Medicine, but it does not intoxicate. and I am told that

Externally

a Religious motive: this Man had fixed his right Arm upon his head, from whence I believe at that time, he could not remove it, however to secure it when he Slept, he tyed it down.

With regard to these Worshippers of Fire I observed in Gilan some few Temples dedicated to that Element yet remain; In Mazondren a great number, as far as I could discover they had been all Givers, till Shah Abbas drove them out & planted Mahometism. The present Shah not troubling himself much upon the Subject of Religion (as they told me) there are yet some of these Idolaters in & about Isfahan, but I fancy their Number is very inconsiderable.

To return to the Fire near Baku a little way from the Temple I have mention'd, is a low Clift of a Rock, in which there is a Gap horizontal 2 f. from the Ground between 5 & 6' long and about 3 feet broad, out of which Issues a Constant flame much of the Colour I have mention'd already being a light blue. It rises sometimes 8 f. high but much lower in Still Weather. They do not perceive the Rock wastes in the least; this also the Indians Worship & say it cannot be put out. About 20 Yards on the back of this Clift is a Well in a Rock, 12 or 14 fathoms Deep, with exceeding good Water.

For several Miles round this place by taking up 2 or 3 Inches of the Surface of the Earth, & applying a live Coal, the part which is so uncover'd immediately takes fire almost before the Coal touches the Earth; this flame makes the Earth hot but does not consume it; the flame does not go out but by being smother'd by loose Earth being thrown on it, or by the like means. Any quantity of this Earth carr. to another place does not produce this Effect. Or take a Reed or tube even of Paper and set it about 2 Inches in the Ground, confine it close with Earth below, touch the top of it with a live Coal & blow & immediately a flame Issues without hurting either the Reed or Paper, provided the Edges be covered with Clay; & this method they use as Candles in their Houses, & 3 or four of these Canes will

Externally applyed 'tis of great use in Aches and Pains but
it must be put to the part affected only. They say 'tis carried
even into India as a great Rarity, & serves for a Tapan the
most beautyfull & lasting of any which has been yet found.

This Sir is what I am pretty well assured is fact, from Journals,
from the word of Men of Reputation, & from my own Observation
when I was in those parts: tho' my own Travels have been on the
Southern Coast of the Caspian to the E. frontier of Persia, & also some
weeks journey ^{inland} from Gilan Southward. If this Relation can in any
degree gratify your Curiosity or those of your friends, I shall
be extreamly pleased, being very Sincerely

Sir

Your most ob.^t Servant

Jonas Hanway

To the Rev. M^r Forster

S^t Petersburg, 12 April 1746.

Nov. 13. 1746.
Mr Jonas Hanway to the
Rev. Mr Forster of
the Prophets & fine
near Buxton.
& chaplain to L^d Marford
Embarked at Petersburg.

Sir

+ Mth N^o 45 78977

Place where ^{the} dug. An Accident calling me yesterday to Redgory, the
England, but many other parts of the world indeed, that and opportunity of going
to the pit, and informing me of the present condition of them, and of
there appears too much probability that this stratum will be exhausted, and lost
entirely to the world, in a few years; I presume it may not be unacceptable
to you to have an account of the pit and stratum also relates to it, taken
on the spot, which I here do only with the honour of communicating to you.
And shall take the liberty of adding to it, what ^{has been} ~~is~~ to my thoughts important
to the supplying its place, where lost, on the many different occasions on which
it is now used.

The Earth itself is a coarse harsh loam, composed of a very large
spinning sand, of extreme hardness; and a fine soft, tenacious clay. ^{It is} ~~the~~ ^{value} ~~of~~ ^{without}
its remarkable quality of standing the fire of the most violent fires ^{without}
which makes it extremely useful to all who have occasion for such fire,
and is the ^{reason} ~~cause~~ of its 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 inside of a clay furnace, and by the workers
on ironfalls; and on many occasions of like kind at the glasshouses &c.
in England and other nations.

The place where it is dug is Redgory before mentioned a small
village about 22 miles from London, surrounded with hills, under one
of which this loam lies. The pit ^{is} ~~the~~ ^{are} about a quarter of a mile south
west from the town, and five miles north of Windsor. They extend
over four 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 finer 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 ^{made} ~~made~~ in the
way to bring it into the condition of the perfect ~~stratum~~. They have
already worked the stratum so far as to find it bounded east and west by
beds of this sand, and northward ^{by} chalk, and are therefore afraid
it will be soon exhausted: at least whatever they get hereafter must
be procured with more labour and expense, as they have no where
to search for it but ~~from~~ higher in the hill, from whence it must be
fetched at greater depths, and much more expense, and this increasing
difficulty of procuring it has been the reason of its rising in price to
that it is now sold at which is five shillings a bushel in London, but which
is not to be wondered at, since on the spot the quantity that makes
a thousand bricks which used to cost 20 pence, now costs ten shillings
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 above & in it.

Providence
May 28. 1744

from very good
humidities
from this

usually do: but seems to be even, and flat at its bottom, to the edge
up the hill they open their pits, the super in proportion they find the
stratum of sand lies.
It is worthy observation that this hill appears to me like that to have
been formed at the hills and mountains on the earth in general, as they
by a disruption and elevation of the strata by violence from within the
earth, rather than that this stratum of sand must have been elevated
with them, and would have been as near the surface, or nearly so, as
one part of the hill as in another, and need have been any more
deeper from the top than from any other part, whereas on the
contrary it appears to lie flat and level with the white
strata of earth which makes the hill, and was in all probability the
surface on the first settling of the terrestrial and other strata.
Among the waters of the Deluge the earth which makes the hill
seems to have been a prodigious mass of water, and afterwards
irreducible stones of that immense body of water, and afterwards
lodged upon it. That this might be the case the immense force of that
body of water and the law which would heavy bodies descend
in the water, may serve to make probable, and what has been formerly
the conjecture is that the earth which makes the hill is not disposed
in a regular pure strata as the earth settled regularly from the
waters always are, but seems evidently a mixed mass made by the
jumping to gather various kinds of layers which are in some parts
of the sand, whereas the rest is white strata and in others irregularly
kindred in different proportions one with another, which as the
strata of sand is the more apparent, and that these are
some of the common commonest strata found in the hills
are so common in the strata of sand, that they are by the distance, such
as the sand of Delmon, or the sand of the hills, and long hills,
almost every where among these strata, but towards the top of the hills
that there are none of these, if this hill has been formed as I
imagine, since in the settling it along they must naturally have
been left behind, and I promise myself that the frequency of the
strata in almost all our strata, and the entire absence of them in
this vast quantity of sand, will have been my rose, wide and broad 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 ordered to dig this loam at 26 feet deep,
instead of about 14 at which depth they long found it, and must, as they
are ordered to ascend this hill dig it at 38 or 40 feet. The force of
this observation told us of it even before the Deane's discovery. I
think it would be a matter worthy consideration whether from reason-
ing the facts it is composed of decided ancient might not be found
in it, by an artificial mixture of similar substances. In order to
attempt this I have by means of water dissolved its parts and
poured them separately; 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 imitate the sand with one
from Hamstead Heath and the clay with one from a pit near
the lower end of the hill, the proportions may be easily learnt
by accurate observation of the quantity of each which disunder

VIII. a Letter from ^{Mr} J^o Hill Apothecary to the Pres^t.
concerning Windfor Loan.

For
Martin Dockes

Dr

Per Cr. 483. VIII.

March 19. 1746/7.

Mr. Hill on Windfor
Loan.

+ Mth N^o. 2.

LONDON, December 4, 1746.

PROPOSALS⁷⁹⁷⁷
For PRINTING by SUBSCRIPTION,
A GENERAL
NATURAL HISTORY:

O R,
NEW and ACCURATE
DESCRIPTIONS
OF THE
Animals, Vegetables, and Minerals,
Of the DIFFERENT
PARTS of the WORLD.

WITH
Their VIRTUES and USES, as far as hitherto certainly known,
in MEDICINE and MECHANICS.

ILLUSTRATED
By a GENERAL REVIEW of the Knowledge of the Ancients, and the Discoveries
and Improvements of later Ages in these Studies.

INCLUDING
The HISTORY of the MATERIA MEDICA, PICTORIA, and TINCTORIA,
of the Present and Earlier Ages.

AS ALSO
OBSERVATIONS on the neglected Properties of many valuable Substances known at present, and Attempts
to discover the lost Medicines, Paints, &c. of former Ages.

In a SERIES of
CRITICAL ENQUIRIES into the MATERIA MEDICA
of the Ancient GREEKS.

By JOHN HILL,
AUTHOR of the Late Translation and Commentaries on Theophrastus's History of Stones

SUBSCRIPTIONS taken in by
The AUTHOR, at his House in the Broad Way, Westminster;
J. and P. KNAPTON, in Ludgate-street;
S. BIRT, in Ave-Mary Lane;
T. LONGMAN and T. SHEWEL, and
C. HITCH, in Pater-noster Row;
C. DAVIS, opposite Gray's Inn, Holbourn;
A. MILLAR, opposite Katharine Street, in the Strand;
R. DODSLEY, at Tully's Head in Pallmall;
J. and J. RIVINGTON, in St. Paul's Church Yard;
Mess. THURLBOURN and CROWNFIELD, at Cambridge.
Mess. FLETCHER and BARRET, at Oxford.
And by the Bookfellers in GREAT BRITAIN and IRELAND.

CONDITIONS.

31

5403/31

C O N D I T I O N S.

I. **T**HAT the whole shall be comprized in Three Volumes in Folio, to be elegantly printed on a good Paper.

II. THAT the First Volume shall contain the History of the FOSSILS, the second that of the VEGETABLES, and the third that of the ANIMALS, each being complete in itself, and a GENERAL NATURAL HISTORY of the Subject it treats of.

III. THAT the three Volumes shall be deliver'd singly, within the Space of three Years, at One Guinea each Volume, in Sheets.

IV. THAT the first Volume shall be deliver'd to the Subscribers in the Month of *December 1747.*

V. THAT every Subscriber pay half a Guinea at the Time of subscribing, and the remaining half Guinea on the Delivery of the Volume; three Months after which, such Gentlemen as are willing to continue their Encouragement of the Work, will be desired to pay in their first Subscription of half a Guinea for the Second Volume.

VI. THAT there will be a few printed on Royal Paper for the Curious, Price one Guinea and a half a Volume, but of these no more will be printed than are subscrib'd for.

THE Subscribers, or any other persons curious in these studies, will be always welcome to the author, to see the forwardness of the work, and the collections of natural bodies, which are in a great Measure to be the Basis of it. And any Naturalist, who has discover'd any hitherto undescrib'd fossile, vegetable, or animal substances; or any property or use of those already describ'd, not commonly known; is humbly requested to communicate his discoveries, and permit them to be insert'd (with due honour to himself) in their proper places in the work.

Any communication of this kind that regards the fossile world is desired to be sent before Lady-Day next, as the first Volume is already in great forwardness, and will certainly be put to the press by that time at farthest.

Received of
the Sum of _____ *as the first Payment for* _____ *first Volume of*
A General Natural History, which I promise to deliver in December 1747, on Payment of
more. _____

Dec. 11/46

Dear Sir!

+ ~~MS~~ N^o. 36.

5403/32

The following Case was lately communicated to me by Mr Alexander Wills, an experienced & ingenious Surgeon, & Man-midwife, of Kingsbridge - It seems to have something particular & remarkable - If you think so (in Perusal) you may be so good as to lay it before y^e royal Society.

Be it as it will, I take this opportunity of wishing you many happy & prosperous years, & of assuring you I am with very great Respect & Affection,

Sir,

your much obliged,

&

most obedient

humble Servant,

Plymouth, Dec. 12th 1746.

~~MS~~ ~~Wills~~
1746

cc John Perrine's wife of Charlton Parish, in this County, a brisk active young woman (tho very infirm & consumptive) was delivered of a Daughter at full Time July 11th 1746 - The Child was perfect as to all its limbs, Head, Body, &c - ~~but~~ But from y^e Region over y^e Os sacrum, Glutei Muscles, & between y^e Thighs quite home to y^e Pudendum, was growing a very large Substance, w^{ch} y^e Midwife & others call'd a Wen, in Shape very like y^e Ventricle of a Sheep, & seem'd as to its Colour & outward Appearance a Continuation of y^e same Skin with y^e rest of y^e Body, but very full of Blood & Vessels. It hang down behind below y^e Heels, & was bigger than y^e whole Body of y^e Child itself - It felt very soft, and

cc & seemed to have Matter fluctuate in it - But in y^e middle of y^e whole was evidently felt a hard substance.

The Pudendum, as well as Anus were in all respects as natural; & both Urine & Stool were regularly Discharged; But y^e Anus was plac'd much more forward, & immediately under y^e Pudendum; so y^t y^e Excreta were Discharged in y^e same Direction with y^t of y^e Urine.

I made a Puncture in y^e depending Part of y^e Tumour & drew off near two Quarts of a palish red water without any Smell - The orifice being left open there was a continual Issue of y^e same kind of water for several Days, but by Degrees it became more & more glutinous, & at length whitish like Pus, & very foetid - As y^e Discharge was great y^e Child grew weaker & weaker, & at y^e end of 15 Days dy'd.

The next Day I open'd y^e Tumour, & found, near y^e Os Coccygis, an Abscess within a Cyst, in w^{ch} were 3 or 4 l^{bs} of white Pus prodigiously stinking - And on further Examination found several Cartilaginous Joints, as it were, somewhat resembling y^e Tail of a Sheep, continued from y^e Point of y^e Os Coccygis: These were about 2 Inches long & inclosed with a kind of fleshy substance cover'd with a sort of Fat: These when cut thro', appear'd exactly like y^e inner Part of Lamb-Stone - From those depending a Substance like y^e Head & neck of an Embryo, as big as a large Egg, w^{ch} on opening contain'd somewhat resembling Brain, & a kind of Cerebellum in y^e back Part - It had a Mouth & Tongue on one Side of y^e Face (if it might be so call'd) but no Appearance of Eyes, or Nose; however there was an Ear pretty evident.

cc In y^e large Tumour there hung a kind of loose membrane w^{ch} perhaps might be Part of a Secundine.

X1. a letter from John Huxham M.D. F.R.S.
to C. Anstimer Secr. R.S. concerning a child
born with an extraordinary Tumour near the anus
containing some rudiments of an embryo in it.

J. Huxham to
of a monstrous child.

May 19. 1748.

Pr. G. no. 487. X.

78977

Extrait d'un memoire sur la communication de l'électricité, lu a l'assemblée publique
de l'Académie Royale des Sciences le 12 nov. 1746.

L'auteur de ce memoire se propose l'examen de ces trois Questions: sçavoir, Que faut-il
pour communiquer de la vertu électrique aux corps qui n'en ont point, & qui ne sont pas
capables d'en acquies par le simple frottement? comment se fait la propagation de la
matière électrique? enfin dans quelle proportion la quantité de cette matière se distribue
telle?

Quand a la premiere l'auteur observe qu'on ne sauroit établir d'autre condition pour la
communication de la vertu électrique que l'approche d'un corps qui possède actuellement cette
vertu: quela règle posée par M^r Dufay sçavoir quels corps ne reçoivent jamais d'électricité par
communication moins qu'ils ne soient supportés par des corps électriques de leur nature, n'a pas
toujours lieu, & quelle souffre de grandes exceptions. car 1^o dans l'expérience de l'eyde, la
bouteille pleine d'eau s'électrise fortement par communication lors même qu'elle est portée
dans la main qui n'est pas un corps électrique de sa nature.

2^o tous les corps qu'on électrise par le moyen d'une bouteille pleine d'eau garnie d'un fil de fer
à qui l'on a donné beaucoup de vertu par communication, tous les corps dissés qui sont placés dans une
ligne courbe quelconque, qui joint le fil de fer extérieur & la partie de la bouteille qui est au-
dessus de la surface de l'eau, acquiesent de l'électricité sans qu'ils soient posés sur de la main
de la soie du verre, &c. ainsi on cause une commotion violente dans les deux bras de 200
personnes qui se tenant par la main forment la courbe en question lorsque le premier tenant
la bouteille le dernier de la bande touche a son fil d'orchaal avec le bout du doigt: & soit
que ces personnes se tiennent par la main ou avec des chaînes de fer qui tremper dans de l'eau
ou traignent a terre: soit qu'elles soient toutes montées sur des poutres de maine soit qu'elles
ayent les pieds sur le plancher, l'expérience a tout jour le même succès. on a fait passer
de l'électricité a travers d'un fil de fer long de deux mille toises (c. a. d. d'environ une lieue) & on a
vu qu'il trainât dans de l'herbe mouillée, sur des palissades de charnille & dans un champ
nouvellement labouré.

3^o on a électrisé par communication l'eau du bassin des Thuilleries dont la surface est
d'un espent, en cette manière: on a tendu autour du bassin une chaîne de fer, qui étoit toute
entière hors de l'eau; les deux extrémités de cette chaîne se répondoient a celles d'une des diagonales
de l'octogone: un observateur placé a une des extrémités tenoit la chaîne de la main gauche &
plongeait la main droite dans l'eau: un autre observateur placé de l'autre côté du bassin tenoit
5403/33

L'autre bout de la chaîne dans la main droite, et une bouteille bien électrisée dans la gauche: il approche le fil de fer de la bouteille d'une broche fixée au siège qui flotte sur le bord du bassin: Dans l'instant les deux observations se suivent un coup violente dans les deux bras. on s'essaye par une expérience faite dans deux bassins à la fois que la matière de l'électricité a bien réellement passé le long de la surface de l'eau.

4° on s'essaye par des comparaisons répétées, qu'une barre de fer placée dans la cuvette que l'on vient de désigner, n'acquière pas plus d'électricité soit qu'elle soit posée sur la table soit qu'on la tienne à pleines mains: en sorte qu'il parait que dans ce cas les corps non électriques conti-^{qu'on}gés ne partagent ni n'absorbent aucunement l'électricité communiquée.

C'est une exception si marquée à la règle de M^r Dufay l'auteur en joint une encore plus forte, car elle est directement contraire à la règle; c'est que cette même bouteille pleine d'eau et garnie d'un fil de fer, ne reçoit pas d'électricité (d'ailleurs bien sensible) tant qu'elle est placée sur un quenou de verre bien sec, ou suspendue à la soie, tandis que son fil de fer traîne sur le globe: il est nécessaire pour que l'électricité lui soit communiquée, que la partie qui est au dessous de ~~la~~ la surface de l'eau, communique avec quel que corps non-électrique: comme il en résulte. Lorsque l'on approche le doigt de cette bouteille placée sur le quenou de verre, car aussi bien elle devient électrique: la même chose arrive lorsqu'on lui présente un morceau de métal mais non pas quand on lui touche avec un tube de verre aussi bien sec.

Les supports électriques produisent sur cette bouteille un effet si contraire à la règle de M^r Dufay, qu'on met par exemple une bouteille parfaitement bien électrisée, et qui fait l'aigrette, sur un quenou de verre bien sec ou sur un fil de soie, salumière s'éteint aussitôt et l'électricité est comme absorbée: on peut alors approcher impunément le doigt de son fil de fer il n'y a aucune étincelle: l'auteur a même tiré, pour faire hors de la bouteille le fil de fer et le bouchon et l'a gardé une demi-heure dans sa poche sans perdre l'électricité: mais il ne faut toucher qu'au fil de fer et non pas à la bouteille car si on touchait aux deux en même temps on ferait l'expérience de Leyde: lorsqu'on ne touche qu'à la bouteille l'électricité se réveille dans son fil de fer et l'aigrette repass, si on n'a pas tardé trop longtemps: mais si on touche seulement au fil de fer, le corps de la bouteille devient très électrique et attire de fort loin les corps légers. ces derniers cas a servi à faire une expérience qui paraît magique. On a suspendu un petit Gnelot au fil d'argent très menu de 8 à 9 pieds d'hauteur, et on a mis sur un quenou de verre bien sec, une bouteille nouvelle ment électrisée: le centre du Gnelot exclu de la bouteille étoient
à peu près

après dans la même ligne horizontale, mais le globe est éloigné de 6 à 7 pouces de la surface de la bouteille: tout étant dans cet état le globe restait immobile, si le qu'on était bien sec. mais dès qu'on approchait le doigt, ou quelque autre corps non-électrique du fil de la bouteille, le globe s'élançait aussitôt sur elle: & on pouvait recommencer, l'exposition de suite sans qu'il fut besoin d'électrifier de nouveau la bouteille.

À l'égard de la propagation de l'électricité, la vitesse de la matière électrique a été trouvée si grande par qu'on ait pu la déterminer précisément: l'auteur en a fait l'expérience sur un fil de fer de 950 ^{pieds} toises de longueur, & il n'a pas seulement pu appercevoir un quart de seconde entre l'instant de la communication & celui où il a senti le coup dans les deux bras ce qui donne une vitesse déjà 30 fois plus grande que celle du son. en cherchant quelle pouvait être la ^{force} qui s'élançait avec tant de rapidité la matière électrique le long de son fil de fer, il crut d'abord l'appercevoir dans l'explosion de l'étincelle qu'on appercevait en appliquant la bouteille au fil de fer conducteur: mais l'expérience suivante l'en a déabulé. il a disposé horizontalement un fil de fer plié en deux sur des cordons de soie la longueur entière était de 1319 pieds; & les deux branches parallèles, étaient éloignées d'environ dix pieds l'une de l'autre. On lui a communiqué de l'électricité par le moyen d'une bouteille, & elle s'est conservée dans ce fil pendant plusieurs minutes, à cause des fils de soie sur lesquels il était porté: on a approché le doigt d'une des extrémités de ce fil de fer pour en ôter l'électricité & dans l'instant l'électricité a cessé aussi à l'autre bout du fil de fer: en sorte que cette matière ne venait vers le doigt c.-à-d. sur les pas avec la même vitesse, quelle avait été élançée: la matière se portait donc ici vers l'étincelle explosive; car cette étincelle paraissait au doigt lors qu'on l'approchait pour ôter l'électricité: donc ce n'est pas cette étincelle qui chasse la matière avec tant de vivacité.

La troisième partie de ce mémoire regarde la proportion suivant laquelle la matière électrique se communique aux corps de même nature. l'auteur établit premièrement que cette matière ne se communique pas en proportion des masses dans les corps homogènes mais plutôt en raison de leur surface: mais les corps qui ont des surfaces égales ne reçoivent pas d'égales quantités d'électricité ceux-la en reçoivent d'avantage dont les surfaces ont le plus d'étendue en longueur — ainsi une lame de plomb quadrée reçoit beaucoup moins d'électricité qu'une lanière du même métal d'une surface égale à celle du quadré: en sorte ^{que le} moyen d'augmenter dans un corps la faculté de recevoir l'électricité c'est de le faire passer continuellement par la filière.

Dec. 11. 1746

Dec. 6/7. 481. VI.

from Mr Ch. Lucas of Dublin.

Fig. I.



no trace on
of ground

Fig. II.
E



Fig. III.



Fig. IX.



Fig. X.

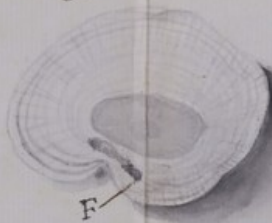


Fig. IV.



Fig. V.



Fig. VI.



Fig. VII.



Fig. VIII.



78977

5403/34
2/2

192.X

March. 26. 1747.

X. The Figures of some very extraordinary calculous
concretions formed in the Kidney of a Woman. com-
municated by M^r. Charles Lucas at Dublin.

N^o. 48

See TAB.

presented
March 26.
1747.

5403/34

$\frac{1}{2}$

Part of A Calculous Concretion formed in the ^{left} Kidney of Mary Anne Mac Mahon otherwise England. taken out after her Death, in the 30th year of her Age.

Fig. 1. A View of the anterior part of the Calculus in its proper Situation, wanting, ^{to compleat its form} ^{Some other} ^{Small Pieces} ^{which were joined or} ^{adhered to it at A.} ^{Fig. III.}

Fig. 11. ~~The~~ A view of the Posterior part, completely the Reverse of Fig. 1.

Fig. III. A View of an other Portion ^{smaller} which by the Intermediation of some other River ^{was} joined to the at B to
Lies: 1. at A.

Fig. IV. The Reverse of Fig. III.

Fig. V. A Portion w^h second broke off Fig. 1. at A. ^{first} fitted it exactly at C.

Fig. VI. VII. VIII. Different Fragments, whose places could not be ~~ascertain~~ ^{ascertain} ~~certainly~~ ^{certainly} determined.

Fig. IX. A Nucleus of an Olive Color, & oval Figure, of the Common Texture & Consistence of ordinary cuti, discovered by cutting Fig. V. transversely at *ed*.

Fig. X. A transverse section of Fig. II. at E. very solid except at F. where a brown vein of the color of
the surface of the Nucleus Fig. IX. at G. ^{white & spongy} ~~nucleus~~ ^{pith} runs through it.

$\frac{1}{2}$ Lintie = 18 Liliqua and each
Liliqua is a 2mlata for diamonds

$\frac{1}{3}$ 5/4

$$\begin{array}{r} 430 \\ 859 \overline{) 2} \\ \hline 20. \end{array}$$

Mr. Dr. 183. X

78977

N^o 38

What I have busied my self in ma-
Vegetables. Having prepared some Wood to shew

internal Structure of a Tree, I herewith send you a small Specimen or
two of it Prepared Wood of Ash, by viewing w^{ch} in a Microscope, I think it
sufficiently appears (1) That the Wood of a Tree consists of longitudinal Vessels or Pipes, &
those of various Sizes. (2) Of lateral Ligaments w^{ch} trace & consolidate the Pipes.
Vessels (3) The ~~Form~~ of these Vessels are nearly cylindrical but not quite,
their transverse Sections being more or less Elliptical. (4) Each of these Vessels have
Valves or transparent Diaphragms, very thick set thro^u their whole Length as appears
in their Section both ways (5) The Transverse Ligam^{ts} consist of fine lignous Fibres
crossing each other, Stratum super Stratum. (6) These Fibres seem to be solid & fila-
ments, & not hollow. (7) Next you will plain there ^{are} no lateral Vessels in a Plant
by w^{ch} Air can pass, as it usually maintains the truth of this may also be
shewn by a Mi- Pump. (8) The Texture of y^e longitudinal Vessels is plainly re-
ticular; for this Rete mirabile appears, & is covered of its Parenchyma in several of
y^e Pieces, as you will easily perceive (9) The Wood is more or less Solid in pro-
portion to y^e greater or lesser Number of these perpend^r Vessels. (10) The Annual Growth
of a lignous Ringlet is only a Number of these Vessels detach^d each Year from y^e Bark,
this is evident in y^e Wood it self, as also in y^e Bark; for I have separated 12 or
more of these Ringlets in y^e Bark. You will excuse y^e Trouble I here give You,
tho' you may not want y^e Light I have prepared for others less acquainted
with y^e Secret & obscure Scenes of Nature. I am, Sir, with y^e most
profound Respect, Your most obedient humble Serv^t

P. Martin

5403/36

What I have busied my self in ma-
Vegetables. Having prepared some Wood to shew
internal Structure of a Tree, I herewith send you a small Specimen or
two of it Prepared Wood of Ash, by viewing w^{ch} in a Microscope, I think it
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profound Respect, Your most obedient humble Serv^t

78848

A.S.

March 10. 1747-8.

11. 8.

1784
Extract of a Letter^{2d} from Dr Miles, to M^r Baker
N^o. 36 78917

I scarce shot of acct I sent last week of my kindling
Sp. of wine with Ice the 3^d of this Instant worth your no-
tice, but as I had not heard of any one at home
who had done it, I incline to relate it to you. Since
I made another trial & succeeded with all the ease
imaginable, of Sp. kindling the very moment of my ap-
proaching them with a lump of Ice which was $1\frac{1}{4}$ inch
thick. after this I took a clamp of Iron, such as is used
for heating Box Irons for smoothing Linen clothes,
& having heated it of same red hot applied it to the Sp. as I stood on
a cake of Wax electrified holding the same in a tongs. I did not
I confess expect much from this trial, & if event was not I
could not kindle the Sp. during the time the redness continued in the
clamp, but as soon as it disappeared & it began to look black-
ish, the Sp. were kindled as usual. I shall not draw any conclusion
from a single Trial, perhaps some reasons might be assigned
why the red hot Iron did not kindle the Sp. provided one were
sure this wd always be the case & if it experient^l were repeated
with the same consequence a good many times, one would
venture to say that the heat of Iron contributed no power
of inflammation to the Effluvia.

my tube I have used of late is not made of the fine
flint glass, but such as common wine glasses are made
of.

I have got me a Tube made of common green glass
this is exceeding light in comparison with others & may
be excited with double the time & pain required for
the others, but yet not without warming it at the fire
tho' this seems powerfull eno' to attract the bunch of
threads, yet I am not able to kindle any Sp. with it.

I have made these trials & might be able to determine
which kind of glass afforded the greatest quantity of Ef-
fluvia, or at least the strongest or near as might be
which may not be altogether unusefull to be known.

your humble serv^t.

H. Miles.

X. Extracts of 2 Letters from the Rev. Henry Miles D.D. & F.R.S.
to Mr. Henry Baker F.R.S. containing several Electrical Experiments.

no. 2.

Feb. 13. 1745-6

Mr. J. 478. X.

67721
IV. The Extract of a Letter from Dr. James Mounsey, Physician of the Carina's
Army, to Henry Baker, A. R. S. concerning the Everlasting Fire in Persia.
N^o. 25.

Sir,

As you inform me any thing relating to the Natural History of Persia will prove agreeable, I have some time ago wrote to a couple of gentlemen, a Physician and a Surgeon, both Men of Learning and Veracity, and my very intimate Friends, who are now with the Ambassador from this Court to Persia, and they both have promised to communicate to me whatever they shall meet with remarkable in that Country, and you may depend on receiving from me all the Accounts they shall please to send.

In the mean while, as the Natural History of Persia is but little known, and the Authors of the Universal History have given no true Account of the everlasting Sacred Fire which the Zaners 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 Dagistan 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 Archibuter Tischer, who received an Account thereof from Doctor Leich Physician of that Army.

This perpetual Fire rises out of the ground in the Peninsula of Absheron 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. ~~Here is a Caravansary~~

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

the Earth continually burns: but the most remarkable is a Hole about 4 feet deep, & 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 its 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 Jewel, 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 three 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 Naptha, which is exceedingly inflammable; and though the Flame of Naptha affords both Smoke and Smell, it is highly probable the perpetual Fire I have been describing is owing to Naptha, 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 of Abcheron. There is also

a

a Salt Lake, near the side of which the white Naptha flows by five different Springs. This Naptha 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 Gonorrhoeas, Disorders of the Breast, and for the Stone; and they apply it externally in Gouty Cases, Contractions of the Sinews, and Cramps.

Black Naptha 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 Balachane, where there are above 50 Springs, the greatest whereof produces every Day 500 Batman, each Batman containing ten Rufs 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 Jewel to burn besides Naptha, but it must be mixed with Earth or Ashes to make it fit for Use. The Use it makes is only good to boil with, and this Inconvenience attends it that all their Food so boiled smells and tastes of Naptha. For baking and roasting they make use of Abrotanum, Asynthium, and such like, but in general Naptha is their Fire.

You may depend on the Truth of this Account, and I hope it will be acceptable; the Hurry I am in, being Physician to the Army now on its march to the Assistance of the Allies, and to set out from this Place to Morrow, with the Commander in Chief, who has been some Time here, indisposed, and under my Care, prevents me from adding any more at present, but you shall be sure to hear from me when we are advanced into Germany. In the mean while, believe me to be sincerely,

Dear Sir, Your most ~~aff~~ obed^t Serv^t

Riga. Feb: 24th. 1748.

James Mounsey.

Apr. 27. 1748.

Pr. 8m. 487. 1v.

+ ~~Am~~

1747
Extract of a letter from Mr. Geo. Fred.
Muller F.R.S. & Prof. Hist. at Petersburg
to Mortimer, R.S. Sec.

47 Several years are pass'd since I returned from
Siberia, & I might have informed you T^r of
my affairs, but there have been reasons why I
could not impart the fruits of that great Jour-
ney to my friends. I ought especially to have had
regard to your illustrious Royal Society, who has
a right to expect from me an account of my
Inquiries; but living in a country so subject to pre-
cavtion, I am not in a situation to acquit myself
of my Duty, unless they think proper to publish
them here first in print. I now send you a small
Sample printed in the 10th volume of our com-
mentaries, of which peice I had a few copies taken
off to make presents of to my friends. I desire your
acceptance of one & beg you will present the
other in my name to R.S. If they shall print
any thing more of mine, I shall not fail to
impart it to you. Being with wth highest Esteem

Y^r Mst &c.

G. F. Muller.

St. Petersburg
July 15. 1747.

recd. Jan. 2. 1747/8.

no. 3

profr Muller to Oth
with his book
de Scripturis Tanguticis

May 12. 1748.

Dr Sir

Amf.

N^o 9.

68412

I promised in my last to ~~you~~ write to you upon any subject, that deserved notice, but did not engage to write well, or treat it in a manner satisfactory to you, and my friends: had I gone so far in my promises, as great a regard as I have for you, and as much desirous as I may be, of obliging you with all that is curious, I must have fallen short of my engagements in a subject, wherein you must expect nothing at least from me, but a simple narrative of an inexplicable phenomenon. as it is, I compleatly answer my obligation by acquainting you with all I know at present, this 11th day of December, or think of electricity, Mr. Folkes was so good as to acquaint me, that all the experiments they know and practise here at Paris succeeded with you at London, except that of killing a bird; that it failed, it is no great wonder for it succeeded with Abbé Nolet but once; I have since however learnt, from Monsieur le Monnier, a method of increasing the electricity of the gunbarrel, or any other electrified body, at pleasure, so far, as to enable those, who desire it, to kill a bird, even without the vial, by the electrical spark only, that breaks forth upon contact. it is done by adding a small iron chain, or wire of 400 feet long, or upwards; one end whereof is twisted round the gunbarrel, and the rest of it, as it were, coiled, or disposed of in several directions about the chamber, so as to hang freely without touching any thing else, in sixteen threads at all convenient points of suspension. This experiment, or method of augmenting the electrical ^{Blow} ~~force~~ is extracted out of a little essay ^{wrote} in the German tongue upon electricity by one Father Gordon a Scotch Benedictine monk. in one word electricity is always augmented proportionably to the length, and surface of the electrified body, as nearly as we can discover here, ~~as well~~ by this, and many other similar experiments; and the whole force of the communicated electricity in a manner spent upon any body, that it is in full contact with it. I intimated to Mr. Folkes, that iron bars of several lengths might serve at the most distant points to discover

whether the motion of the electrical effluvia was ^{an} amplified ^{me,} or not; I believe, that even one iron chain alone, or wire of a very great length might suffice at several distant points from the globe, to confirm the same, in case it be so, provided for instance, if the design be to try it ^{at the distance of 10, 20, or any determinate} number of feet, a circle of ^{any one of the melted resins,} ~~any one of the melted resins,~~ electric, ~~per se~~ be drawn round the chain or wire at that distance, to prevent the electrical effluvia beyond that point from returning upon contact, or any other method of trial. upon the credit of ~~R.~~ Gordon's experiment I find no difficulty of believing, that they have carried matters ~~to matter~~ so high in Germany, as to have killed an ox. Monsieur l'Abbé Nollet, as I hear, has some letters from thence containing several new experiments, which he has not thought fit to communicate as yet in a publick manner; and of which he seems to make a sort of a secret, perhaps because not hitherto reconcileable with his system:

in any plausible way of evading; for you know it to be the method ever of Descartes, and his disciples, not so much to form ^{an} ~~an~~ hypothesis upon experiments, as to bring experiments to the torture of an hypothesis. but this case now; Monsieur l'Abbé Nollet however thinks it has been said by some of his friends, that among other particulars from thence, he has an account of the Philosophers of those parts having increased the ^{electrical} force so prodigiously, as to kill an ox. but more of this, when I know the whole truth. - my letter has a little connexion in its periods, as the several experiments relating to the subject, it treats upon; or the many systems, or parts of systems already advanced upon this head; so I proceed to something else. nothing will draw a smarter electrical spark from the body held by the ring; but if it be suspended by the string in case the string be pure silk without any silver twill worked into it, it produces no effect; this seems to prove the existence of an affluent matter from animal bodies necessary in many electrical phenomena; perhaps the very nervous fluid, ether, or aura productive of muscular motion, I remember you was intimating to me as a plausible hypothesis, soon after you had read some of your lectures upon that subject before the R. Society. in consequence of this last experiment, I with you would try at London, whether spirits of wine are inflammable by the effluvia, if the spoon be

suspended horizontally in a tilken thread, and so held in hand. I do
suppose not; but experience must prove every thing, for I profess I know
less of it than ever. Monsieur Le Monnier observed very lately, as he
was shewing ~~some~~ of his new experiments to some persons of quality, that
they succeeded perfectly well, almost beyond what he had ever seen, tho' the
weather was rainy, and the atmosphere surcharged with vapours. I myself
happened to be present at another place the same day, and nearly the same
hour, where the usual experiments were very far from being so vigorous,
as we could have desired; he does not know, what to attribute this
difference of place to, unless it be to his having distilled some materia
aetherea that day in his apartments, with the effluvia of which the air
of the chamber was highly impregnated. I must not forget another
particular I have from Monsieur Le Monnier: I had observed from him
some time ago in my first letter to Mr. Folkes, that many globes acting
together upon the same body will not increase its electricity; whether
this be owing wholly to the body not being susceptible of it beyond
a certain degree of intensity, is as yet a question; for he has since
discovered, that if many globes of unequal dimensions, and it is difficult
to find them otherwise, act together near each other, the largest
to the rest of their virtue, without increasing their own — it
you have any thing new upon this, or any other subject, may
send it me, and direct for me at Pleppis college Rue St Jacques
where you may give to Mr. Knowles in great Ormond street. I beg my
best respects to Mr. Folkes, and let him know, Monsieur Le Monnier
received lately a paper from Monsieur de Condamine to be sent
by my means to London for him, but he has locked it up, it seems
so carefully, that he cannot find it at present, otherwise I had
sent it inclosed in this. it will however unquestionably appear
soon, and shall be dispatched by the first subsequent post. I can't
well say what it is, unless it relates to his own papers taken at
sea. My complements, if you please, to Mrs Parson, Mrs Baker,
Messieurs Baker, Sherwoods, Hill, & Acotta &c. I am about
purchasing an electrical apparatus to be in the fashion, till
then this is all

from Dr Sir, your most obedient
Humble servant Thos. Needham.

Paris Dec. 11. N. S. 1746.

For Doctor Parsons F.R.S.

at his house in

Red Lyon Square
London.

Co

Jan. 8. 1746-7.

NEEDHAM (John Tomberville) [1713-1781]

Mr Mason of Trin Coll. Camb. & P.R.S. to the President 1797

Sir:

+ Mff
N^o. 13

Having met with severall things in a Ramble
last Summer that were new to me, & imagining they
might be so to you likewise, & being of some consequence,
~~make me~~ I presume to trouble you with a short account
of some of them

What Spelter is I dont well know, nor what uses
are already made of it; but I believe it was never yet
apply'd to so large a work as the Cylinder of a Fire-Engine
till Mr Ford of Colebrook-Dale in Shropshire ^{& bored} it
with success; it run easier & cast as true as brass ~~cast~~
full as well or better, when it had been warmed a little;
while cold it is 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 & therefore ^{works} ~~is~~ better than Iron, the rust of
which upon the least intermision, of working, resists the mo-
tion of the Piston.

Severall attempts have been made to run Iron Ore
with Pit coal, I imagine it hath not succeedd any where.
Because we have had no account of its being practis'd;
but I find that Mr Ford. from Iron Ore & Coal, both got
in the same Dale, does make Iron brittle or tough as he plea-
ses; There being Cannon thus cast so soft as to bare Turning
like wrought Iron.

at Brown Ley about a mile from the fore mentioned
place in the year 1711. was a well found; which burnt

with great violence, whereof some account is given
~~out~~. Phil^l. 9^o. 324 but ^{it} has been many years lost:
~~but~~ The Poor man in whose land it was, missing the profit
he used to have by showing it, apply'd his utmost Endeav-
ours to recover it, but in vain till May last, when by at-
tending to a rumbling noise under ^{the} ground, like what the
former well made, tho' in a lower situation & about 30
yards nearer ^{to} the River, he happened to hit upon it again.

That you may have some notion of what it is, I will lay
before you such an account of it, as the Curious view I had
of it will permit.

The well for 4 or 5 foot deep is 6 or 7 foot wide,
within that is another less hole of like depth dug in ^{the} Clay;
in the bottom whereof is plac'd a cylindrick Earthen vessel
of about 4 or 5 Inches Diameter at the mouth, having the
bottom taken off: & the sides well fix'd in the Clay round 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, & a rumbling hollow noise: rising & falling
by fits 6 or 8 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 $\frac{1}{4}$ of a yards distance it took
fire, darting & flashing in a violent manner, for about $\frac{1}{2}$ 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
made to boil in 9 minutes time; & that he had left it burning
48 hours together without any sensible diminution.

It was extinguish'd 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 ful-
phureous smock, lasting about a minute; & yet the water
was very cold to the touch. The well lies about 30
yards from the Severn, which in that place & for some miles
above & below runs in a vale full 100 yards ^{perpendicular} below
the level of the Country on either side, which inclines
down to the River at ^{an angle of} 20 or 30 degrees 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 & Clay unequally mixt; & as the River
which is very rapid, washes away the soft & loose parts,
the ~~rocks~~ successively slip into the Channell: so as by
degrees & in time to affect the whole slope of the Land;
and as the Inferior ^{strata} ~~that~~ yield Coal & Iron-ore, their
fermentation may produce this vapour, & force it to
ascend with violence through the chinks of the Earth
& give the water the great motion ^{it has}: ^{which} might
be obstructed ^{in one place} by the formentioned ^{subsiding of} ~~subsidence~~ 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. If these hints should be any amusement to you,
or be the means of letting any more able Person upon
farther Enquiries, & giving a better account of them, I
have all that is intended by your

humble servant

Jan: 18. 1746.

Cha: Mason

P. 5.

To Martin Bouquet Esq

President of the
Royal Society.

Mem. Book

Per. Br. 452. VI.

Jan. 22. 1746-7.

VI. a letter from the Rev. Dr. Johnson to
Martin Bouquet, at Cambridge, of Dr. A. S. to
with Pitt was a burning word at Bouquet

M^r. Henry Baker

4th 1788

me

Dear Sir,

68412

I may not allow my self to defer my Answer to your last kind Letter any longer, tho I am much disappointed in my late trial, and have nothing worth communicating to you as I hop'd to have had. My disappointment has put me on inventing ano^r. Method to excite the comon tube to a greater degree than by friction with the hand, but w^t the effect will be I am not able to say, for the faithless workman instead of finishing the Machine which he could do in a $\frac{1}{4}$ of an hour, and giving me the Satisfaction of a trial, has tho't fit to put me on a trial of ano^r. kind, by withdrawing himself, leaving me to discover whether I have the patience of a Philosopher or no, but that I may not exercise yours while I show my want of it, I will drop this disagreeable Subject

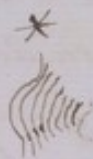
Two or three days since, as I was rubbing my tube with no other design, than to try the temperature of the air, I plainly heard the snapping of the effluvia, sometime after I had withdrawn my hand, tho no kind of body was near the tube, for I stood about the middle of the room: hereupon I repeated the trial several times with design, and cou'd sometimes number 8, 9, 10 and once 18 distinct Snaps, the space of time from the beginning to the ceasing of the Snapping being $\frac{1}{4}$ or $\frac{1}{5}$ or more, for I have not yet accurately noted that. At first I imagin'd this might be owing to the motes or fine dust which came out of my clothes, which as the sun shone into the room at the time I saw in great plenty, but I found I was mistaken, and ^{rather} think the noise arises from the resistance of the Air, for I tried in the open air, and found the Snapping equally strong, and loud where there cou'd not be so much dust as in a room, and after I purposely shook my clothes, and made a great dust in the room, but found no greater snapping in the room than abroad. I have several

* I mean
as soon as I
have done
rubbing.

times tried in a dark room & have been entertained with the flashes of fire exactly resembling the smaller flashes of very distant lightning in a summer evening. If you incline to try your tube, it will be necessary that you stand as still as possible when you have excited it, & draw down your rubbing hand to the other, which that I may the better do, I hold the lower end of the tube close to my body,* & take care to avoid inclining my head, for fear of affecting the effluvia thereby. the noise of the street may perhaps prevent your hearing the snapping, but the flashing will be visible enough.

I remember not that any one has yet taken notice of this circumstance, & that if you knew it not, you would not be displeased to hear it.

I am very sorry the figure of the Salts broke, which it did purely by its own weight, at least without being touched, it made a most beautiful appearance, as you may partly judge by the remainder, the best of all was when I turned aside the reflecting concave, and instead of casting the light thro' it view'd it only by the parallel common rays as you do an opaque object. I am inclin'd to think by Dr Mead's cut of the poison he could have had but a very small quantity of the Salts possibly because the viper was not made to bite on the glass itself, as this did: for surely his magnifier could not be so shallow, as one would conclude from the lines in the cut, provided the Salts were as substantial as even the the worst of those I have ever seen. I had one small drop more which was not worth bringing you, in which the Salts were color less, the lines were parallel and were no where united, as they commonly use, the drop was oval & the lines ran thus* from the circumference to the centre.



I am sorry I did not procure more but I was preparing to come to town, besides I had some fear of the old man, who brought 'em lest he should get a bite, for he had been,

as I learnt distracted. I imagined I might procure some of
your Apothecary, at this time ~~in~~ially, and have the opportunity
you desire of seeing the ~~effects~~ root, which I will venture to
say you will not repent of. The post is come & I am oblig'd to
conclude with my best wishes, and with assuring you that I am
sincerely &

affectionately yours
A Miles

Tooting May 7th 1746

~~I cant read over~~

n^o. 3.

May 8. 1746.

71 miles to Mr. Baker.

Dr 57/8

+ Mff. No. 9.

MS 5403/47
78977

Having read in the Philosophical Transactions for the Year 1746, of the surprising Effects of Musk in putrid Pesteilial Fevers, and having Reason to imagine from the Accounts given by those who are concerned in attending upon the horned cattle, that the Distemper now prevailing among them is of that kind, being attended with kind pesteilial Spits & Injections, ^{upon the Tongue,} especially in the latter Stage of the Disease, I concluded from Analogy of Reason that the Medicine being applied in larger doses might be serviceable to them. Accordingly a considerable Farmer in the Neighbourhood having the Distemper amongst his cattle, I advised Him to make the Experiment. The Symptoms of the Disorder at its first breaking out, as is usually observed in other Places, were very favourable, but as the Infection increased in the Herd, the Symptoms became more violent. The four first cows that were seized, recovered. The two next had the Disease more violent & died. After this it was observed that the Symptoms at the ~~very~~ first seizure grew worse. He having ^{then} two taken ill at the same time and nearly in the same Manner, in order to give the Experiment a fair Trial, I advised Him to give only one of them a Dose of Musk of 24 Grains: the Result was, that next Morning she was easier, eat her Hay, which is unusual with them, whilst they labour under the Distemper; the other two continued as before. Upon a Repetition of the Dose, the former recovered, the latter continually grew worse and died. Soon after He had two others seized in the same Manner: About three or four Days after their seizure, I gave each of them a Dose of Musk of 40 Grains; the consequence was, that the next Morning they were easier, eat Hay, and the Symptoms from that Time became more favourable, and they recovered. As the same Person had another taken ill soon after I advised

Him to apply the Medicine sooner than He had done to the others, in Order to see the Effect of it in an earlier Stage of the Distemper. Accordingly He gave her 50 Grains, and the result was, that in the Night of the 22^d of May 25th was given her, and never became so bad as the 11th. Another Cow was seized soon after this in a more violent Manner; He gave her ^{large} 75 Grains to the Quantity of 100 Grains, and found her the next Morning better, & eating Hay. I did not stay long enough in the country to see the Result of this last Experiment. The Musk was given to all of them in a Glass of Rum, which is observed to resist Putrefaction, mixed with about a Pint of Water-Gruel to make a Drench.

There seems to be good Reason to conclude that this Medicine may be of Service from the Manner of its operation, if the Information which I have rec^d be true, that every Cow which can be made to sweat plentifully, certainly recovers. For Musk is observed to act upon Human Bodies, by promoting a free and plentiful Perspiration.

If this Application should cure only three in four, I think it will be a great Relief both to Farmers & Gentlemen under this dreadful Calamity. But however, the apparent Success of it in these few Instances seems to give sufficient Encouragement to make a further Trial of it. In what Manner the further Trial of it should be promoted I leave Ist to your Judgment & Discretion. Great Care should be taken in procuring good Musk; because I am satisfied, that a great Deal of bad is sold by country Apothecaries, which may sometimes occasion Discredit to the Medicine. However, I would not have this made publick, unless upon further private Trial, it is found to be attended with Success.

I am V^r thankful to You for the Honour You did me last Year in recommending me to the Society, and to the Society for their favour in electing ^{me}; and I shall be particularly gl^d if I can by any means be the least instrumental in promoting or answering the End of their Institution. I am V^r

London
March 10. 1747.8.

With all Respect & Gratitude
Yours most obliged
& obed^t. Humble Serv^t
J. H.

Martin Bouker Esq^r
The very worthy President
of the Royal Society

Recd 12th 1745
The Rev^d Dr. W. Parker
to v^r Qu^r. of v^r Society
& curing the same with
much.

f²

When the means for preventing the Infection among the Cattle were under Consideration, Burying them was thought the most effectual method to hinder its progress; and by way of improvement to this project, the Addition of Lime, was imagined necessary for the more speedy destruction of the distempred Carcases. But some doubts arising, whether the Lime might not exalt the putrid particles and help to spread the Infection, it was the opinion of several of the Learned, that it was most safe on that account to bury them without it.

This difference will probably be decided by the inclosed account of Cattle buried both with and without Lime, written by John Milner Esq^r one of the Justices appointed to inspect into the affair, and one who has the good of Mankind at heart as much as any person whatsoever. This Gentleman related the Case to several others who were met at a Coffee-house, where I was present, and as I thought it concerned the public very much, I waited on him next morning to request he would permit me to lay it

before the Royal Society, which he readily com-
-ply'd with, and gave me the inclosed paper for
that purpose; I hope it will serve to prevent
the practice of Burying them with Lime for the
future, as this accidental Fact makes it more
than probable that malignant particles may be —
sent up, and spread through the air.

I am fr^d y^{rs} and the Society's
most Obed^t Serv^t.

N.B.

The cattle were buried

10. feet deep with Lime

8. feet deep without lime

J. J. Lartons

n^o. 1.

June 12. 1746

Pr. Gr. 480. XI. 1.

Martin Folkes Esq. Secy. of the
Royal Society

XL. a letter from James Garfons on D. of R. S.
to the President serving to introduce a Remark
from John Michu Esq; concerning the bury-
ing of the Count dead of the present reigning
Emperour in Rome or not.

The
10.

May the 31th 1746.

67462
MS 5403/48, 2/2

Mr Stalwood of Harmer at Flackney informed
the Justice, to whom the Care of the Indisposed
Cattle was committed, that hee had buried thirteen
Cows very deep, with the Quantity of Lime appointed
by the Justice, and observing his Dogs to scratch
and beare up the ground with their feet, to get
at the Cows flesh (the Lime fermenting, and causing
a stench, as hee called it, or strong scent being of
meat to arise, which made the Dogs soe eager to
come at it) hee beat them off severall times, but
the Dogs always returning as soon as hee was gone,
hee for some time hired Boys to keep them off.
But that hee had buried severall other Cows in
another place with their Hides cutt and flayed
without any Lime being ordered by the Justice to be
doe) and the Dogs were attempted to scratch or beare
up the ground there, though it lay open to them &
equally with the other ground, and they often run over
it.
For supply of Lime to each Cow,
was the allowance

John Milner

n^o. 1. X.

June 12. 1746

pr. Gr. 480. X1.2.

67462

Gentlemen

The Book which you did me the Hon^r. to com-
mit to my consideration, intitled: Alberti Haller
de Respiratione Experimenta Anatomica &c.
is a controversial Answer to Hamberger who has
opposed some opinions of our Author in his Commenta-
ries on Boerhaave's works; who says he cannot avoid
sustaining the opinions of his great Master, without
lessening his own Credit, and injuring the Truth.

The Controversy is twofold: as it regards Respirati-
on; Hamberger appears to be of opinion that there is
a Portion of Atmospherical air always between the
Lungs and Pleura, and Secondly that the Internal-
Intercostal Muscles, serve to draw down the Ribs
in Expiration, as the External ones draw them
upwards in Inspiration.

Haller has notions contrary to both these; and
as to the first, he asserts that of a great number
of Bodies, which were opened by himself and Others,
when the Integuments and Intercostals were removed,
and the Pleura laid Bare, the Lungs lay close to that
Membrane, and their natural Colours were as conspi-
cuous through it, as a picture thro' a glass: and that
therefore there could be no vestige of air, every space
in the Thorax being filled with lungs even in
Children in whom they are most likely to be free.
But that if a wound be made in the Breast so that
the external air can pass in, it never fails to drive
the Lungs inward from their Contact with the Pleura,
and deprives the Spectator of the sight of the white

white vessels upon their bluish surface.

He speaks here only of Dead Bodies, in which the internal air has no greater degree of heat than that of the Atmosphere, and says that if that intermediate air did exist, tho' a wound were made and the external air let in, there could be no change brought about, but an Equilibrium must be maintained between the External air, that contained in the Lungs, and the Thoracic air in question; and therefore demands why the Lungs should so sensibly recede from the Pleura upon making a wound thro' the Thorax; leaving so remarkable a distance between them where there was none before?

Thus he endeavours to confirm his assertion that the Lungs fill the cavities of the Thorax & Express their Figure as exactly as Wax fills a Mould into which it is cast; and to this our Author ascribes every Character that relates to their form, as their posteriour convex surface, the lateral Inclinations of their Planes downwards and forwards; their inner cavity for the Reception of the Heart; and in fine, the Hollow answering to the convexity of the Septum transversum which in some Measure gives them the Shape of an Oxes Hoofe; and hence he concludes that there could be no such Similarity of Form, between the Lungs and Thoracic cavities, if their Surfaces were not in close contact.

Our Author mentions an objection of his learned Antagonist Hamberger which is; that he had seen in a Dog whose Thorax was open'd on one side — only, the Mediastinum inflated like a Bladder — pass out at the Dissected Side, in the act of Expiration, and return upon Inspiration; and in order to try the Experiment himself, he dissected ten Dogs, four Cats, and four young Goats alive; and asserts that in Expiration the Lungs indeed were forced out, and were resorbed in Inspiration constantly, but not the Mediastinum; and that upon opening the Thorax by dividing the Sternum from the Ribs, there appeared a Bag like an inflated omentum situated to the left of the inferior trunk of the Vena Cava and sometimes continued between the Pericardium and the Sternum; which contains a small Lobe of the Lungs, that is never either expanded or collapsed; growing turgid in expiration, descended when the Animal Inspired, and inclining backwards was changed, as it were, into a rectilineal partition; and that — therefore it can by no means favour the opinion of Hamberger.

In order to confirm this, our Author proceeds to his Observations upon the young Goats, which, he says ~~he~~, being milder Creatures, and less liable to strong convulsive motions upon being dissected; he the more clearly discerned the Bag to be only part of the right cavity of the Thorax situated, as is mentioned before, & the small lobe it contains, an appendix

to the Lungs on that side; it is therefore no wonder, says he, when the right side of the Thorax be opened, if the air rushes in and distends that Bag which the Lobe fill'd in the living animal, mistaken by Hamberger for part of the Mediastinum; and which he further proves can never swell outward, nor can any air be contain'd in the unopen'd side of the Thorax, & which is also very demonstrable in the Goats; for in them the Mediastinum is driven to y^e opposite side, against the Pleura thro' the opening made on the Right side; because, says he, the external cold air, rushing into the opening, easily overcomes the warmer column in the Lungs of the unopened side, drives it out by the Aspera Arteria obliterates the Cavity, and confirms the last act of Respiration.

But if, says our Author, the Mediastinum is cut thro' and the air driven in, then in the left cavity, will swell towards the Right side, and if instead of the Mediastinum, the Lungs, on the left side, are inflated by the Aspera Arteria, they will fill their Cavity and force the mediastinum to swell to the other side.

He also repeated these Experiments by opening the Contrary side, and leaving the Right Cavity intire, and affirms that the same success attended, and concludes from this series of Experiments that the Lungs, Mediastinum & Pleura are in contact with each other, & that no space for air can exist between them, either in a living or dead Animal.

Thus far our Author has endeavoured to convince Hamberger of his ^{being} Mistaken as far as Relates to the first Question; he next proceeds to the second, which concerns the office of the internal Intercostal Muscles mentioned before; the sum of which is this; "that as the fibres of those Muscles arise at a greater distance from the vertebra in the upper Rib, than ^{their insertion} in the Rib next under it, they must of necessity draw down the Ribs," now in order to prove this he caused a Machine to be made representing the vertebra, Sternum and two Ribs, and fixed a string to them to imitate the direction of these Muscles, by which he drew down the Ribs and Sternum.

Haller made such a Machine also, and found it answer'd according to Hamberger's Assertion; but admires at the same time he could not perceive the ^{use} of the Machine; for the two ribs were made equally moveable upwards or downwards, which our Author endeavours to prove are not analogous to natural Ribs.

In order to demonstrate this, he prepared the Bones of a human Thorax leaving the Ligaments and Cartilages intire, and kept them moist & supple with wet cloths; to all the true Ribs of which he fixed pulleys and passed the Cord, which moved upon all these; over a larger pulley fix'd upon the vertebra, and by drawing this he imitated the natural motions of Respiration; all the Ribs were raised outwards and upwards, the Diameter of

of the Thorax was greatly increased, and the Spaces between the Ribs much diminished; for, says our Author, since the contraction of all the Intercoastal muscles is the cause of the Elevation of the Ribs, they must necessarily draw them together by becoming shorter, and the Sternum recede from the Vertebrae more in the Lower part of the Thorax than the upper.

Upon letting the Cord loose all the Ribs descended, their lower edges turning inwards, the Sternum approached the Vertebrae & straitened the Thorax, & the Ribs became more distant from each other.

Then he apply'd a Cord to two of the Ribs, and drew them as Hamberger had pull'd them in his Machine, avoiding every occasion of favouring him self, and even then the Lower Rib always approached the upper very evidently.

But because our Author would shew, how much the stability or firmness of every upper rib exceeded that of the next under it, he hung weights to them respectively; and found the upper Rib was scarce moved downwards by a weight of four ounces; the second was moved somewhat more by a weight of six drams; the third by four drams and half; and the fourth by four drams. Wherefore he concludes that since the upper Rib is the most firm in so great a Ratio, the second must move upwards to it by the contraction of the Intercoastals, that the first cannot descend by any means, and that therefore the first is the fixed point or center in the Thorax, and the twelfth Rib the most moveable.

In

In order to be yet more accurate he took the pains to measure the muscular fibres of the Intercostals, and the distance of the Ribs from each other, whereby he calculates the Power of every upper Rib as a Lever to that beneath it, and considers at the same time the different diminutions of the degrees of Firmness in the Articulations from the first to the last; and then Concludes that, as all the Internal Intercostal muscles undoubtedly act together, and the second Rib is more moveable than the first; the Contraction of the first Intercostal muscle which draws up the second Rib, will increase the Power of the second Rib to resist the ^{pulling down} ~~detraction~~ of the muscle beneath it, and so on to the last, whereby the facility of pulling up the Ribs is always increased, and the power of drawing them downwards diminished; and that therefore, from all the Excesses of the Resistance whereby every upper rib exceeds that next under it, the firmness of the first is to that of the last in a Compound Ratio.

Here our Author ends his Controversy, and hopes his Antagonist Hamberger upon reading his thoughts on the matter will be of his opinion; or if that be too much to expect, that he will, at least, not be angry if he sustains his former Sentiments, and the Honour of his Master the great Boerhaave.

no. 1.
Feb. 26. 1746-7.

An Account of M^r Klein's Letter to M^r Peter Collinson, concerning the Bobak, of Poland, by D^r Parsons.

N^o. 1. To Martin Folkes Esq^r. L.L.D. Pres^t. of The Royal Society

I have Read the Learned M^r Klein's Letter with some pleasure, which was occasion'd by a Passage in Cardinal Polignac's ^{Book} ~~entitled~~ *Anti Lucretius*, giving some hints ^{account of} concerning the great Sagacity of ~~that~~ ^{the} Animal; ^{called the Bobak} in waging War, encamping in Armies or droves, taking their adversaries Prisoners, & making Slaves of them to do the several Offices mention'd hereafter, of which Rzachzensky, in his natural History of Poland has also given much the same account & added that they make Slaves of the stragling Spies they Catch, and destine them to the same punishments.

These are Acts which require a reasoning faculty, wh^{ch} if skillfully conducted, would be an honour to the best Politician or the wisest General; and require much more ^{Sagacity} than the limited Knowledge allow'd to the brute Creation for their preservation. But these things have been receiv'd by several Natural ^{antiquary} Historians as: Agricola, & Spon^{ge}, which latter seems to have given the story to the Cardinal, at the following words plainly twice: "Rats des Alpes, faisant provisions l'été pour l'hiver, du foin et autres herbes, qui leurs sont nécessaires, pour s'en acquiter plus promptement, il y en a un, qui sert de Charette, se mettant sur le dos, les pattes en l'air, et emportant le foin; et un autre qui sert de Charretier, la tire par le queue jusqu'à leur Tannière, ce qui est cause, qu'on leur trouve ordinairement le dos tout pelé."

our

Our Ingenious Author (after Gesner) being a lover of Truth, doubts these facts, and cannot help thinking the ^{Emigrant} Prelate was either deceived, or too fond of embracing the opportunity of gracing his Book with a narrative, of so extraordinary a Nature, and so well adapted to his Poetical Genius; and accordingly ^{he} has taken some pains, to divest the History of this Animal of what he takes to be the marvelous and fabulous parts of it. He appears by the Number of Quotations in his Letter to have read ^{much} upon and considered the case maturely, and begins by assuring the world, he does not deny that several Animals have surprising sagacity, proportioned to their manner of Life, for their welfare, and enumerates several Examples of Creatures whose actions & contrivances have given hints to mankind which have lead not only to Mechanical, but Liberal Arts.

We are, says he, however, much deceived in many reports of the manners & customs of Animals, which we are but too apt to add to those which nature allows them, and ought to reflect on our selves for giving too much credit to Tales handed down by Tradition or raised by fancy, which always turn out meer Fables; and then he reflects on several vulgar errors concerning, Lions, Bears, Serpents, &c which are too trivial to trouble the society with, ^{there} & enters upon an impartial History of the Boback as follows:

It is call'd Mus Alpinus by Plin; Marmot in Savoy; Murmeltier in Germany; In Poland and other northern countreys Boback; and by

by the French Rats des Alpes.

The Bobak is somewhat thicker than a common Cat; is reddish while young, but of a darker colour when older; with stiff hairs, short feet, contracted Head, the Nose as if divided, long hairs ^{about} the mouth like those of a Cat, & teeth like those of a Squirrel.

Whilst this animal is wild, it eats grass, roots, herbage, and Insects of several kinds: but ^{kept} tamed, feeds upon bread & milk, meat & fruits, using its fore feet to put any thing to its mouth, like the Squirrel, & growling while it eats & drinks.

The females bring forth three or four young at a time; and from the beginning of Autumn to the Spring following, these Animals live in families together, upon a little straw in a profound sleep, in their caverns, which run under ground in a direction like the letter, Y, and are well & closely stop'd up. At the approach of the warm Sun they awake, open their dens and come forth to do the business of life; to void their Excrements, feed & copulate. They play nimbly together like mice - running, leaping, & climbing the trunks of trees, & sometimes walking upright; & their voice is shrill like that of a young whelp.

The circulation of the Blood and all the secretions are exceeding slow in this animal, & the blood is almost entirely destitute of serum. the Omentum & intestines are very fat; They cannot be thought to Ruminates, having but a simple membranous Stomach, altho' they feed upon herbs.

Towards the Gut cocum there are several annular valves stretched as it were into Branches,

as if also the entrance of the Heon ~~at its entrance~~
between the two coats, so that the passage of the ex-
crements is much retarded, & they are collected towards
the cocum, there to remain during the whole
winter.

It is wonderful that Agricola & Spon should
report that when these Creatures have gathered their
Hay, one of them lies on his back, & the others
load him with it as a waggon, and laying hold
on the tail, their Mouths, drag him along to
their dens, which, says Spon, is the reason
some of them are found with their Backs all
bare; but fatness & other accidents in the
economy of many animals, at certain times
of the year, cause their Hair to fall off.

But we have much more Reason to wonder
at the late Relation of the Great Cardinal —
Polignac, who says, that these Marmots wage
war with, & destroy one another; that the Conquerors
lead their prisoners into Captivity, and impose on
them all their Domestic Slavery, as carrying
their provisions & such like Employments, which
were never observed before, and that it is not
very likely that ^{such} Animals should war among
themselves, since, as the proverb has it,

Cornix Cornici nunquam perdidit Ocellum.

But however, it can scarce be conceiv'd why
Prisoners should be wanting, for we certainly
know these Creature sleep eight Months together,
and

and consequently have no need of provisions, during that time; and it would seem further ridiculous to imagine they should draw their wagons by the wrong end, against the grain and direction of the Skin & Hairs, & even of Nature; ^{itself} as if the Animal was condemned by Law, & distinguished as a Malefactor to such slavery, & to suffer many horrible Pains in the way besides.

But if they had need of hay or forage, they might furnish themselves amply by successively carrying it in their mouths & sometimes assisted by their fore feet, for they can walk erect as was observed before.

This is an ill digested Natural History sunk into a fable, for prejudice & Credulity ever banish truth, as it were to shun a great evil.

You have, my Dear Collinson, the genuine account of this Animal, which you may communicate to the worthy Doctor Mead & assure him I will take all possible care to procure the 2^d vol: of Arackinski's nat: Hist. of Poland, & transmit it with all speed.

Dantzick. Jan. 22. 1748

no. 1.

3: March 1847

Feb 6. 25. 1847

Dear Sir (Gentle),
I have the honor to acknowledge the receipt of your letter of the 25th inst. in relation to the matter of the 25th inst. and in reply to inform you that the same has been forwarded to the proper authorities for their consideration. I am, Sir, very respectfully,
Yours obedient servant,
J. J. [Signature]

N^o. 28

48 I have made diligent search amongst the writers upon fossils as well as of natural History in general, and cannot find that y^e Curious Specimen is any where described; the Singularity of its Sutures, and their being so well preserved renders it very valuable, for I believe no recent Shell of this Genus, nor any fossil one extant in the world have such features as this has.

I had some expectations of finding it described by Michael Mercatus but found his figures were quite different tho' nearly as small; and therefore I think we may conclude it an undescript & undoubtedly a Species of Nautilus.

The Fossil I had from Dorsetshire taken out of the Quarry near Sherborn, is also another undescript, and a different Species of the same Genus impregnated with the stony matter of that Quarry in which it was found. ~~It is~~

They are both very scarce specimens and deserve
a place in the Transactions. I send mine along
with y^r to m^r Hawksby, which y^r will please
to leave with him for me, after the society
have seen them, and am

Rev & fa
Y^r most Obed. Ser^t

J^d
Parsons

Red Lion Square
May 6th 1748



C

Fig. 2.



A

p. 320.



B

J. Myndes sc.

M.D.C.

May 5. 1748.

See Tr. 487. VII.

To

found in pool's hole
in Derbyshire

The Reverend

Dout Littleton

These

DIFFERENS MOYENS

D'empêcher de se corrompre les Oiseaux morts qu'on veut envoyer dans des Pays éloignez, & de les y faire arriver bien conditionnez. Quelques-uns de ces mêmes moyens peuvent être aussi employez pour conserver des Quadrupèdes, des Reptiles, des Poissons & des Insectes.

CEUX qui s'intéressent au progrès de l'Histoire Naturelle, & qui voudroient en faciliter l'étude, ne sçavoient manquer de desirer de voir les Collections des différentes sortes de productions qu'elle a pour objet, se multiplier & devenir plus amples, & d'être disposés à y contribuer de tout leur pouvoir : elles offrent dans un même lieu plus de différentes sortes de corps du règne minéral, du règne végétal & du règne animal à examiner & à comparer à l'aise les uns avec les autres, qu'on ne pourroit se promettre d'en trouver successivement dans les plus longs & les plus pénibles voyages. Pour que ces Collections devinssent assez complètes, il faudroit qu'il y eût dans tous les pays du monde des hommes zélés pour leur accroissement, qui se fissent un plaisir de faire passer les productions particulières à celui qu'ils habitent, dans les Recueils qu'ils sçavent être déjà considérables, & qu'on travaille à rendre utiles au public. La partie de l'Histoire qui a une plus grande suite d'objets agréables à nous offrir, & qui en offre en très-grand nombre qu'on ne recherche pas pour le seul plaisir de les voir, celle qui traite des Oiseaux est restée encore très-imparfaite, elle ne nous les a pas encore fait assez connoître, parce qu'on n'étoit pas parvenu jusqu'ici à en faire des collections considérables : ceux qui en ont commencé, ont été bien-tôt dégoûtés de les continuer, ayant eu le déplaisir de les voir détruire journellement par des Insectes voraces, malgré les soins employez pour les défendre contre leurs dents. M. de Reaumur après avoir trouvé des moyens simples de préparer les Oiseaux qu'on veut faire entrer dans ces collections, qui les mettent hors de risque de se corrompre, & qui leur conservent un air de vie, a trouvé ce qui étoit encore plus à desirer, des moyens de les mettre hors des atteintes des Insectes qui en sont avides. Il se propose d'apprendre bientôt au public comment on réussit à rendre ces sortes de collections durables. Il est parvenu à en faire une qui est déjà très-nombreuse, & il a lieu d'espérer qu'elle se deviendra bien davantage : les Oiseaux dont il est redevable à plusieurs Sçavans amateurs de l'Histoire Naturelle, l'assurent qu'il leur en devra d'autres, à mesure que des occasions de les lui procurer, se présenteront à eux. Il sçait d'ailleurs combien il doit compter sur leur disposition à l'instruire, & en est pénétré de reconnaissance. Avec beaucoup d'envie de faire parvenir des Oiseaux du pays où l'on se trouve, dans un autre pays où on n'en voit point de pareils, on peut être arrêté, parce qu'on ignore comment on peut leur faire faire un très-long voyage sans être défigurés ou mis en pièces par la pourriture pendant la route. On va expliquer ici les différens moyens auxquels on peut avoir recours pour les défendre contre la corruption, & pour les faire arriver bien conditionnez.

Première Manière.

La méthode pratiquée jusqu'ici pour faire connoître les Oiseaux d'un pays aux Naturalistes de pays fort éloignez, est de les envoyer empaillés, c'est-à-dire, de leur enlever la peau

chargée de toutes ses plumes, de dessus le corps & les cuisses, à laquelle on laisse attachées les pattes, les ailes, & pour le mieux, le col entier avec le bec. En remplissant ensuite la peau enlevée de quelque matière molle, soit de paille, soit de soie, soit de bourre, soit de filasse, &c. ou même en l'étendant sur un moule solide qui a la figure du corps de l'Oiseau, on fait reprendre, autant qu'il est possible, à cette peau la forme qu'elle avoit lorsqu'elle recouvroit des chairs & des os : c'est à quoi on réussit quelquefois assez bien, au moyen d'attentions & de petits procédés qu'on ne s'est pas proposé de détailler ici.

Seconde Manière.

La manière précédente de conserver la forme des Oiseaux, demande des mains exercées, qui même ne parviennent à imiter assez la Nature qu'avec de la peine & du temps. Il est assurément plus commode de n'avoir qu'à envoyer l'Oiseau tel qu'on l'a reçu. Il n'est besoin d'aucune adresse acquise pour en mettre un ou plusieurs dans un vase plein d'esprit de vin, ou d'une très-forte eau de vie. On est en usage depuis long-temps d'employer avec succès ces liqueurs pour conserver les chairs des animaux morts ; pourquoi donc s'en est-on très-peu servi jusqu'ici pour empêcher des Oiseaux entiers de se corrompre ! c'est apparemment parce que leurs plumes n'offrent pas les couleurs variées & éclatantes qui leur sont naturelles, pendant qu'elles sont plongées dans une liqueur, & qu'on ne retrouve pas ces couleurs aux plumes de l'Oiseau qui vient d'en être tiré. D'ailleurs les barbes des plumes sont alors mal arrangées & trop collées les unes contre les autres. Sur ces premières apparences on a jugé trop vite que les liqueurs spiritueuses altéroient la couleur des plumes, & empêchoient qu'on ne pût faire reprendre à celles-ci l'arrangement & le jeu qu'elles avoient sur l'animal sec & vivant. Des expériences répétées ont cependant appris à M. de Reaumur que la teinture des plumes est à l'épreuve de l'eau de vie la plus forte, & même de l'esprit de vin, & qu'après qu'on a fait sécher l'Oiseau qui avoit été mouillé, on parvient aisément à remettre ses plumes dans leur état naturel, & qu'on peut le faire reparaître tel qu'il étoit pendant sa vie.

1^o Pour conserver les Oiseaux qu'on veut envoyer loin, il n'y a donc qu'à les tenir dans l'eau de vie ; plus elle sera forte & meilleure elle sera pour produire l'effet auquel elle est destinée. L'esprit de vin est même préférable. Il est d'ailleurs indifférent que l'eau de vie soit de vin, de grains ou de sucre.

2^o Quoiqu'on puisse mettre les Oiseaux dans la liqueur tels qu'on les a reçus, il y a pourtant quelques petites attentions à avoir, & quelques précautions à prendre avant que de les y plonger, qui contribuent à les conserver dans un état plus parfait. Si quelques-unes des plumes de l'Oiseau sont ensanglantées, on les lavera à diverses reprises avec un linge mouillé, jusqu'à ce qu'elles cessent de donner de la teinture à ce linge ou à l'eau dont il est imbibé. Il est sur-tout important d'empêcher les plumes de prendre une mauvaise direction & de se chiffonner ;

il est aisé de les disposer dans le sens où elles doivent être, en les lissant avec un doigt, que l'on fait mouvoir de la tête vers la queue en le pressant contre elles. Les plumes sont ainsi aidées à prendre la position qui leur est la plus naturelle, & on les retient dans cette position en enveloppant l'Oiseau d'un mauvais linge qu'on assujétit autour du col & du corps par un gros fil auquel on fait faire plusieurs tours. Les plumes du col sont sur-tout celles qu'il faut empêcher de se jeter de côté, ou de rebrousser.

3° La précaution de tirer du corps les intestins & les autres parties qui y sont contenues, n'est pas absolument nécessaire, le mieux néanmoins est de la prendre : si ensuite on les remplace, si on remplit la cavité du ventre de toute la quantité qu'on y pourra faire entrer de bourre, de filasse, de coton, ou de quelque autre matière molle : si on remplit le col, mais sans le distendre, de la même matière molle, on conservera plus sûrement la forme & les dimensions de l'Oiseau. Il devient moins gros dans la liqueur spiritueuse, non pas précisément parce que les chairs se raccornissent & se dessèchent, mais parce qu'alors les parties qui forment les cavités, tendent à les rétrécir, & les rétrécissent effectivement si ces cavités ne contiennent pas une matière qui s'y oppose.

4° Après ces préparations simples & faciles, il n'y a qu'à mettre les Oiseaux dans le vase qui contient la liqueur qui doit les conserver. Ce vase peut être un bocal de verre, s'il n'est destiné qu'à recevoir de petits Oiseaux ; un seul bocal en peut contenir un grand nombre qu'on y mettra à différens jours, c'est-à-dire, à mesure qu'on les aura, & jusqu'à ce qu'il en soit entièrement rempli. Les barrils de bois sont pourtant préférables aux bocaux, parce qu'ils ne sont point exposés à se casser dans une longue route : on en peut avoir de très-petits destinés aux petits Oiseaux, & d'assez grands pour recevoir ceux de la plus haute taille. Le barril aura un trou assez grand pour laisser passer les Oiseaux qu'on y veut faire entrer ; ce trou peut n'être que celui du bondon agrandi ; il sera encore mieux placé à l'un des fonds. Il n'est pas nécessaire d'avertir qu'on le tiendra fermé par un bouchon d'un diamètre proportionné au sien, excepté pendant le temps court où il doit être ouvert pour donner passage à l'Oiseau.

5° On peut envoyer les Oiseaux dans les bocaux mêmes & les barrils où nous venons de les voir mettre ; mais s'ils doivent être en route pendant plusieurs mois, ou pendant des années, avant que de les faire partir, on renouvellera la liqueur : celle qu'on y a versée d'abord peut avoir été affaiblie par l'évaporation & par les sucs aqueux qui ont été extraits des chairs.

6° Si ces Oiseaux ne doivent pas arriver par mer à leur dernier terme, s'ils doivent être voiturés par terre pendant une partie de leur route, il faut faire en sorte qu'ils ne soient pas exposés à être trop ballotés par les cahots, & ils le seront d'autant moins que le vase en sera plus rempli, ils s'assujétiront mutuellement. Dans le cas où ils flotteroient trop dans la liqueur, on n'hésitera pas de les presser par du foin, ou par quelque autre matière qu'on introduira dans le vase.

7° Il est encore plus aisé d'empêcher les ballotemens, & les Oiseaux ne seront que mieux conservés, si avant que de les faire partir on les retire de la liqueur dans laquelle ils ont séjourné pendant un temps suffisant : elle les a mis en état de se sécher, sans être en danger de se corrompre. De petits Oiseaux, ceux de la grosseur des Moineaux, & même de celle des Merles, après être restés huit à dix jours couverts d'une forte eau de vie, peuvent en être retirés sans qu'il y ait à craindre qu'ils se corrompent. Les grands Oiseaux, & sur-tout ceux qui sont extrêmement charnus, demandent à être tenus dans la liqueur plus long-temps ; mais il n'en est point, ou il n'en est guère, à qui il ne suffise d'y avoir séjourné pendant un mois ou cinq à six semaines. A mesure qu'on retirera les Oiseaux de la liqueur,

on les arrangera les uns à côté des autres & les uns sur les autres dans une boîte, en remplissant les vides qu'ils laisseront entre eux, de la matière molle qu'on aura plus commodément, comme de balles d'avoine ou d'orge, ou de celles d'autres grains, c'est-à-dire, de ces petites coques qui forment l'enveloppe du grain pendant qu'il tenoit à l'épi. Ces balles sont la meilleure de toutes les matières pour cet usage. On peut aussi y employer du petit foin, de la mousse, de la filasse, du coton, &c. Loin qu'il soit nécessaire de faire sécher les Oiseaux avant que de les arranger dans la boîte, le mieux est de les y placer tout dégoutans de liqueur. Après avoir bien rempli la boîte, il ne reste qu'à la fermer.

8° Une boîte, quelle que soit sa forme, est convenable pour des Oiseaux qui ne doivent rester que quelques semaines, ou peu de mois en route ; ceux qui y resteront des années, demandent qu'on redouble de précautions ; quoiqu'ils ne soient pas exposés à se corrompre, ils peuvent être mis en pièces avant leur arrivée, si des insectes qui en sont avides, parviennent à pénétrer jusqu'à eux, & se multiplient dans leur logement. On peut avec de l'attention rendre des boîtes si closes, qu'il ne soit pas possible à ces insectes redoutables de s'introduire dans leur intérieur : du papier collé sur toutes les jointures y contribue. Mais les barrils sont préférables aux boîtes pour les Oiseaux qui doivent rester renfermés pendant une année, ou plus long-temps ; les plus petits insectes ne trouvent pas de passage pour s'introduire dans un barril qui ne permet pas aux plus petites gouttes de liqueur de s'échapper. Les Oiseaux qui ont été mis mouillés dans le barril, l'empêchent de se dessécher trop, & contribuent à le conserver clos. Heureusement que les insectes carnaciers ne sont pas de ceux qui savent percer le bois. En faisant l'usage de l'esprit de vin, ou d'une eau de vie forte, que nous venons d'expliquer, on réussira donc à faire arriver des Oiseaux en fort bon état aux termes les plus éloignés. Voici encore une autre manière de le faire qui pourra paroître plus commode, sur-tout pour les Oiseaux d'une grande taille.

Troisième Manière.

La troisième manière est de conserver les Oiseaux par une forte d'embaumement, & même par un véritable embaumement dans les pays où les aromates sont à bon marché. 1° On commencera par vider le corps de l'Oiseau, on le remplira ensuite de quelques-unes des poudres que nous allons indiquer ; on remplira son col de la même poudre qu'on fera passer par le bec. Si l'Oiseau est extrêmement charnu, on pourra faire une entaille dans la chair du gros de chaque cuisse, & une dans la chair de chaque aile, c'est-à-dire, deux sur la poitrine, & une plus proche du premier & gros os de chaque aile, dans lesquelles on introduira de la poudre : les chairs étant ensuite rapprochées, les plumes rajustées, ces entailles seront cachées de manière que l'Oiseau n'en fera aucunement défiguré. Mais il y en a très-peu à qui il soit besoin de faire de ces sortes d'entailles, on peut même en faire d'intérieures équivalentes ; après avoir introduit les doigts dans le ventre, on peut déchirer les tégumens vis-à-vis le gros de la cuisse & dans d'autres endroits, & creuser des cavités qui seront dans la suite remplies par la poudre. 2° Plusieurs poudres sont propres à produire le principal effet qu'on se propose ici, qui est que l'Oiseau se dessèche avant que de s'être assez corrompu pour qu'il soit permis aux plumes de tomber : tous les aromates y peuvent être employés avec succès : s'il y en a quelques-uns à très-grand marché dans le pays, on s'en servira. On peut de même employer une poudre composée d'autant de sortes d'aromates qu'on le voudra ; il en résultera au moins que l'Oiseau après être desséché, en aura une meilleure odeur, qu'il sera une cassiolette. Mais au lieu d'employer des gommes résineuses, comme l'aloès, la myrrhe,

Feneens & d'autres productions des plantes, comme la cannelle, le gérofle, le poivre, le gingembre, &c. qui sont des matières chères, on peut s'en tenir à un sel qui est à bon marché dans la plupart des pays; il suffit de remplir la cavité du corps & le col d'alun réduit en poudre. Une matière encore plus aisée à avoir en tous lieux, qui y est à vil prix, & qui opère très-efficacement, c'est la chaux. Si on peut en avoir de très-vive, on la prendra par préférence, & on n'hésitera pas à s'en servir quoiqu'elle soit vieille, & qu'elle ait été un peu éteinte par l'humidité de l'air. 3° Après que le corps & le col de l'Oiseau auront été remplis, soit de chaux pulvérisée, soit d'alun, soit de quelque autre poudre, on le placera dans la boîte ou dans le barril qui doit servir à le transporter. On aura soin, en l'y plaçant, de faire prendre au col une position naturelle, & de même de ne donner aux jambes que l'inflexion qu'elles ont lorsque l'Oiseau vivant est posé dessus. Le fond de la boîte ou du tonneau aura une couche épaisse d'un pouce ou environ (le plus ne sauroit nuire) de la même poudre qui occupe la cavité du corps, ou d'une de celles qui sont propres à y être mises. On enterrera l'Oiseau dans cette poudre, on en mettra assez autour & au dessus de lui pour qu'il soit caché sous une couche épaisse d'un pouce ou plus. La poudre extérieure avancera le dessèchement, & pourra arrêter les insectes voraces qui ne tenteront pas volontiers de passer au travers pour arriver jusqu'à la chair qu'ils aiment. Dans les premiers jours, & même dans les premières semaines, l'Oiseau pourra répandre une mauvaise odeur, qu'on n'en soit point inquiet, elle diminuera à mesure que le dessèchement avancera: celui-ci se fera sans qu'il arrive à l'Oiseau de perdre aucune de ses plumes, & quand il est une fois desséché, elles sont fermement assujéties sur lui pour toujours. Cette manière de conserver des Oiseaux, qui est très-simple, en a procuré à M. de Reaumur de ceux de pays fort éloignés, qui sont arrivés tels qu'il les souhaitoit.

Quatrième Manière.

La quatrième manière de mettre des Oiseaux en état de faire de longues routes sans se corrompre, en est une de les dessécher plus promptement que celle qui vient d'être expliquée, c'est de les dessécher au moyen de la chaleur du four. On profite de celle qui lui reste après que le pain en a été tiré; souvent elle est encore trop grande alors; mais il y a un moyen simple de s'assurer que le degré de chaleur n'est pas trop fort, c'est de mettre des plumes dans le four, & de les en retirer au bout de 5 à 6 minutes; si l'on voit qu'elles ne sont ni grillées ni roussies, il n'y a rien à craindre pour celles de l'Oiseau qu'on veut faire entrer dans le four. Les petits n'ont besoin que d'y rester une heure ou deux pour être assez desséchés; ceux de grandeur moyenne demandent à y être tenus plus long-temps; & ceux qui sont gros & très-charnus, veulent y être mis à plusieurs reprises. Lorsqu'ils sont refroidis on connoît s'ils sont assez desséchés, en pressant avec le doigt les chairs de la cuisse & celles de la poitrine; si elles ne cèdent pas, ou si elles cèdent peu sous le doigt, l'Oiseau n'a plus besoin d'être remis au four. L'inconvénient qu'il y a à l'y tenir au delà du temps nécessaire, est qu'on rend certaines parties, comme le col & le croupion, trop cassantes. On empêchera que le volume de l'Oiseau ne diminue sensiblement dans le four, si, avant que de l'y faire entrer, on remplit la cavité de son corps & celle de son col de quelque matière molle pareille à une de celles dont on a dit qu'on devoit faire usage pour remplir les cavités de ceux qu'on veut conserver par le moyen de l'esprit de vin, c'est-à-dire, de bourre, filasse, coton, &c. Ce que la façon de dessécher au four a de plus difficile, n'est pas de saisir le degré de chaleur convenable, & de connoître la durée du temps pendant lequel on doit faire

rester l'Oiseau au four; il le paroît davantage d'avoir, comme l'exige cette manière de dessécher, à assujétir l'Oiseau dans une attitude naturelle, avant que de le faire entrer dans le four: le dessèchement le fixera pour toujours dans celle qu'on lui aura donnée. Il y a plusieurs moyens simples en eux-mêmes, de mettre & de retenir l'Oiseau dans une attitude naturelle, qui cependant seroient très-long à expliquer en détail: le peu que nous en dirons, suffira aux personnes industrieuses qui en voudront faire usage. On peut assujétir l'Oiseau au moyen d'un petit métier fait à peu près comme le travail d'un maréchal; il est composé d'une petite planche qui en est la base, & dont la longueur n'a pas besoin d'être plus grande que celle de l'Oiseau. Près de chaque coin de cette planche s'élève un montant de bois; les quatre montans sont entretenus par des traverses qui y sont attachées par de petits clous. L'usage des montans & des traverses est de servir à arrêter de petits rubans & des fils qui maintiennent le corps, les ailes & le col de l'Oiseau dans les positions qu'on leur a fait prendre. Un fil qu'on a fait passer au moyen d'une aiguille au travers de la tête de l'Oiseau, rend maître de la placer aussi haut ou aussi bas qu'on veut. Il y a divers moyens de fixer les pattes sur la planche, ayant leurs doigts écartés; on le peut faire avec de petites pointes de clous. Avec un seul fil de fer & une petite planche, on peut faire tout ce qu'on fait avec le métier: on passe ce fil de fer tout du long du corps & du col de l'Oiseau en l'introduisant par l'anus; mais avant que de l'introduire, on lui fait une espèce de gros nœud en le contournant; ce nœud doit toucher l'anus, il sert dans la suite à empêcher l'Oiseau de glisser: tout près du nœud on recourbe perpendiculairement la portion du fil qui est hors du corps, elle doit avoir au moins une longueur égale à la hauteur qu'auront les jambes: on rend ensuite son bout pointu en le limant, si on ne l'a pas déjà fait, & on pique ce bout dans la planche. La partie du fil de fer qui est en dehors du corps, fait alors la fonction d'un montant qui porte l'Oiseau, parce qu'elle est continue avec le reste du fil qui passe par le corps & par le col: le fil de fer qui enfle ce dernier, le retient dans la courbure & la direction qu'on lui fait prendre.

Les Oiseaux desséchés doivent être envoyés dans des boîtes ou dans des barrils assez clos pour que les insectes ne puissent pas s'y introduire pendant la route; & on aura soin de remplir tous les vuides qu'ils laissent dans le barril avec quelque une des matières molles que nous avons déjà indiquées pour un semblable usage. Il peut se passer bien des semaines, & même bien des mois entre le temps où on a fait dessécher les premiers Oiseaux dont on se propose de composer un envoi, & celui où on peut les faire partir. C'est un intervalle dangereux. Certains vers & certains scarabés sont plus triands de ceux qui ont été desséchés au four, que de ceux qui l'ont été de toute autre manière; si les accès leur sont libres, ils profitent quelquefois des premiers momens pour s'aller établir sous leurs plumes, ou dans leur corps, où ils se multiplient. On mettra les Oiseaux à l'abri des dents redoutables de ces insectes, si, dès qu'ils ont été tirés du four, on les enterre dans le sable contenu dans une grande boîte ou un tonneau. Il faut prendre garde en les couvrant de sable, de ne leur point faire prendre de mauvaises attitudes, & de ne pas chiffonner leurs plumes. De la chaux éteinte & en poudre, de la craie & toute poudre terreuse, fine & sèche peut être employée avec succès pour la même fin. On pressera avec la main la surface de la poudre, pour en rendre la couche supérieure compacte, elle a seule besoin de l'être. Enfin si la chute des plumes apprenoit que des insectes ont été rendre inutiles les précautions qu'on a prises contre eux, il y a encore du remède; on arrêteroit les progrès du mal en remettant l'Oiseau au four qui peut n'être pas assez chaud pour griller les plumes, & l'être assez pour faire mourir les insectes en moins d'une demi-heure.

*REMARQUES communes aux quatre manières
de préparer les Oiseaux.*

1° Ce ne sera pas trop d'envoyer deux ou trois Oiseaux de chaque espèce, & on sera en forte, autant qu'on le pourra, que l'un soit mâle, & l'autre femelle. 2° On ne peut manquer d'être curieux de savoir le nom que porte chaque Oiseau dans le pays où il a été pris: on l'écrira avec de l'encre ordinaire sur une bande de parchemin qu'on attachera avec un fil à une de ses pattes. L'écriture se conservera lors même que l'Oiseau sera dans l'eau de vie. 3° Lorsqu'on saura d'un Oiseau quelque chose de plus que son nom, on fera un petit mémoire, qui apprendra dans quels lieux il habite, de quoi il se nourrit; s'il se tient ou ne se tient pas toute l'année dans le même pays, comment & où il fait son nid, combien il pond d'œufs, les ruses & les adresses qui lui sont particulières, s'il est bon à manger: en un mot tout ce qu'on saura de son histoire. 4° Une collection de nids est un assortiment convenable à celle d'Oiseaux, elle fait voir des ouvrages que les hommes auroient peine à imiter, admirables par leur forme, par leur travail & par les matériaux qui y entrent; M. de Reaumur en a déjà fait une de ce genre. Quand on pourra avoir des nids qui ne seront pas d'un volume qui les rende trop difficiles à transporter, on doit être certain qu'on les verra avec grand plaisir joints aux Oiseaux qui les ont construits. 5° Les couleurs & les figures des œufs entrent aussi dans l'histoire des Oiseaux; les collections qu'on en forme, ont de quoi satisfaire des esprits curieux: ceux qu'on enverra courroient risque d'être cassés en route par les matières mêmes qu'ils contiennent, lorsqu'elles viendroient à fermenter. Avant que de les faire partir, il faut donc les vider: pour cela on leur fait un très-petit trou à chaque bout, on les secoue ensuite, & si le secouement ne suffit pas, on souffle dans un des trous pour forcer de sortir par l'autre ce qui reste de liquide dans l'œuf.

QUADRUPÈDES.

Les Quadrupèdes qui ne sont pas d'une trop grande taille, & particuliers à certains cantons, pourront être mis en état d'être

4. envoyez dans les pays les plus éloignés, par un des quatre moyens employez à conserver les Oiseaux. On en peut faire comme de ceux-ci des collections durables. M. de Reaumur en a commencé une qui fait souhaiter à ceux qui la voyent, qu'il y en ait de plus complètes du même genre.

POISSONS & REPTILES.

Les Poissons & les Reptiles qui sont, comme les Quadrupèdes & les Oiseaux, des sujets intéressans pour les Naturalistes, sont plus aises à envoyer: il suffit de les mettre dans des barrils remplis d'une forte eau de vie. Ils peuvent aussi être desséchés, soit par des matières dont on remplira la capacité de leur corps, soit par une chaleur douce & bien ménagée.

INSECTES.

Les Insectes qui ont tant de variétés admirables à nous offrir, méritent qu'on songe à les ramasser, à en faire des récoltes qui ne scauroient manquer d'être précieuses à ceux qui ont tant fait que d'étudier ces petits animaux. Tous ceux qui sont mols, comme les Vers & les Chenilles, peuvent être conservés dans l'eau de vie. Leurs couleurs tendres seront moins en risque d'y être altérées, si on donne à cette eau de vie la quantité de sucre qu'elle pourra dissoudre. Les Scarabés peuvent aussi être mis dans la même liqueur. Mais les Papillons & plusieurs Mouches s'y gâtent: après les avoir tués, il faut les arranger par lits dans des boîtes, & séparer ces lits les uns des autres par des couches de coton. Quoiqu'on doive rassembler dans chaque pays, par préférence ceux qui frappent le plus, soit par la variété & l'éclat de leurs belles couleurs, soit par leur grandeur, soit par leur forme particulière ou bizarre, soit par les usages qu'on en sait faire, on ne négligera pas de ramasser & d'envoyer ceux qui n'ont pas de ces singularités remarquables à offrir, ceux qui sont les plus communs. Entre ces derniers, il y en a qui ont de quoi satisfaire un observateur qui les regarde avec d'autres yeux que ceux avec lesquels ils avoient été regardés, & dans d'autres vûes.

no. 5.
March 10. 1748.
Apr. 27. 1748.

While so many Gentlemen are labouring to find out the uses of Electricity, it has been my fortune to discover one at least of the inconveniencies attending that Property in Glass. And as it is such whereby vast numbers, very likely have been, and may hereafter be greatly prejudiced, I desire you will mention what follows to the Royal Society, to the end that it may be published, if they think proper, for the benefit of others, and particularly of those who use the Sea.

Having lately occasion to compare together two Compasses of a different make, the one having a bare Needle as usual, and the other a Chart in the manner that Mariners Compasses are commonly made, I happened to wipe off with my finger some dust which lay upon the Glass of the former, and thereby put the needle, which was before at rest, into a violent disorderly motion, partly horizontal and partly vertical or dipping. After several repetitions of the same thing, I found that the glass, by so slight a touch, was at that time excited to electricity, so far as to disturb the needle extremely.

The same glass being rub'd a very little more with a finger, a bit of Muslin or of Paper, would attract either end of the needle, so as to hold it to the glass, for several minutes far out of the due direction, according to what part of the glass was most excited.

And when the needle had for some time adhered to the glass, and afterwards drop'd loose and made Vibrations, those Vibrations would not be bisected, as usual, by that point where the Needle should rest, but either be made all on one side, or be very unequally divided, by means of some remains of Electrical virtue in that part of the glass which had attracted the Needle; Until at length, after 15... minutes or more, all the Electricity being evaporated, the
Magnetical

Magnetical Power took place.

The Cure for this inconvenience is to moisten the surface of the glass. Even a wet finger will do it immediately and effectually.

I need not suggest that the same quantity of friction will not at all times have the same effect upon these glasses, any more than it will upon the Electrical Tubes; but take the liberty to hint that I have reason to believe that glass does at some times become in some degree attractive without any friction at all; and may possibly be excited by great concussions in the Air, such as Thunder or the discharge of great Ordnance &c; and if so may thereby disturb the Compass.

I must however observe that the Mariners Compass is much less dangerously moved by wiping or exciting the glass than the other; by reason that the excited part of the glass attracts that part of the Chart which lies nearest, just underneath, without giving it so much verticity, as it does the other sort of Compass with a bare Needle. And farther that the deeper, or ^{the} farther distant the Needle hangs below the glass, the less disturbance it is likely to receive, by wiping, rubbing or otherwise exciting the Cover.

I shall make no farther Reflections upon these facts than to observe; first that all the minute irregular reciprocating Variations which have been observed in the Directions of Dipping and Horizontal Needles, as mentioned in some of the Transactions, may probably have been caused by the glasses which covered the Instruments made use of: and secondly, that the flat pieces of glass, often placed under the scales of an Essay balance, are likewise very capable of attracting, and making even the lighter scale preponderate, where the whole matter weighed is so very small. I have not tried this last, but do remember that ~~Mr. Ellicot~~ ^{Mr. Ellicot} a member of your Society, did some years ago suspect, if not find it certain, that such pieces of glass did

did disturb his Balance, and had given him a vast deal
of trouble, upon a supposition that the Beam itself was...
defective.

Your Most Obedt Servant.

XIII. Copy of a Letter to
 Mr. ^{Robinson} showing that
 the Electricity of Glass dis-
 turbs the Magnetic Com-
passe, and also vice Ver-
sance.

June 12. 1746

Pr. No. 430. XIII.

London May 17. 1748

Hon. J. D.

If your Goodness will excuse the Liberty a stranger has taken in giving you this trouble hoping the foll. account will atone for his boldness I shall think my self Greatly favoured. I have a Son about 16 years old that has bin for 6 or 7 years past troubled with sudden fits that entirely takes away his senses I got him all the help I could but to no purpose att Last I put him into St Bartho. Hospital as an out patient & there he was turn'd out as incurable. So finding his case Deperate I considered the power of Electricity & made a Large Machine so Electri. & afterwards shocking him Commonly twice a Day he has Recd some Benifit & Last Sunday being May 15 he being on the Pedestal & very high Electrified & having on a Coarse Tustian Working Trock the condencing Viol being on the Conductor & I tuckling him to procure Shaps as usual touch his Right Shoulder Blade & to my Great surprize the fussey Flase of the Trock caught a fire with a Great blaze & Burnt the whole Breadth & Lenth of the Shoulder the Flame Rising 6. above the Coller & I believe would have sett the Trock 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 than my surprize was the Greater becase all I Read on that Subject Says nathing will Burn but wot Sends forth Inell Vapour at 9 the Same Evening I made him put on the Same trock & touch'd the Left arme were the flase had not bin burnt before & it had the Same effect as above

I. if any farther account of the above will be acceptable to you or your Hon. Society if you please to Command I will wait on you there is Alterations in My Machine I think for the better & some new Experiments to Long to wright fearing Least it would be to trouble you from your Honours

Humble & obedient Servant Rob. Roche

113 I live at the fine of the Golden Horse
at Mount-Rose. Close near the Church

I just now in the same manner
fixed 2 other parts of the Trock

IX. a letter from Mr Robert Roche to the President
of a Justian Grock being set on fire by Electricity.

+ ~~MS~~N^o. 3

Of the Preparation, Sophistication, Uses and Virtue of China Inks.
Communicated to D^r. Le Syre by D^r. Jⁿ. Simon Saguet Physician to the King
of Portugal Hospital in Lyons.

Provides a competent number of Wicks from the pithy part of water Rushes the Barks being first strip'd off: so that each Pith may constitute one single Wick. Immerse two or three of these in ~~fresh~~ Hogs Lard melted in a proper Vessel, in such sort that one end of the Wicks may rise above the Surface of the Lard. Then light these Ends, and place over them a hollow brass Cone, whose base must not quite touch the Surface of the Liquid, yet descend so near it, as that all the sooty Smoke may arise within the Cone, and adhere to its internal Surface. When a proper Quantity is collected, sweep it off; and incorporate it with enough Mastick to give it a Solidity, which may then be fashioned into Sticks of various Shapes and Sizes. The Chinese blend therewith, especially in that which they prepare for People of Distinction, a certain aromatic Substance which they call Mofee, by way of perfumes.

The Sticks, as I said, are of various Shapes, all mark'd with Chinese Characters, some gilt, some green and others black. And This is the Principal of all.

Sophistications

They adulterate in three several ways. 1^o For Dispatch sake they will clap one or two Cotton Wicks among those of Rushes; yet this is still reckon'd a Good Ink. 2^o Employing Rushes, as before, they make use of earthen Cones instead of copper ones. In this case when they come to sweep off the Soot, part of the Substance of the Cones is sweep'd along with it, and so increases the weight of the Commodity. This call'd Bad Ink. 3^o They will make all their Wicks of Cotton, and use earthen Cones besides: Thus they catch a large Quantity of ponderous Soot. This is the Worst Ink of all.

An 2^d Way of distinguishing these several Sorts

An Elefiantick procur'd once a Sample of the best sort of all, which he receiv'd from the Hands of the King of Portugal, to whom the Emperor of China sent

sent twelve Touneis as a Present. Two or three of the Frugments are about an Inch long, their Bases being Parallelograms. Their Colour is not black, but or brown, but lightish; the Surface smooth and polished, being variegated with fine streaks. When broken under the inner Surface is smooth and glossy, and this is the main Characteristick of the Best and the Good Ink. It has a gentle full Scent, and if you hold a Bit in your Mouth, it is insipid, soft, and gummy. Upon dissolving it in water, it emits a subtle Fume, and a small portion thereof will tinge a great deal of water. They sell it at thirty thousand Touneij the Pound, that is above twelve Pounds Sterling.

The Good Ink I had from a Priest who brought it from the Indies. It is in the Shape of a Brick, with a smooth Surface, though I have seen Pieces of a cylindrical Form. Some are scented, others not; they are likewise glossy, when broken, but less so than the best sort, but the outward Surface is browner; The Chinese sell it at twelve thousand Touneij the Pound.

The Bad has commonly the form of a Parallelogram, with a Groove running along the Middle, and is never glossy or scented when broken; The external Surface is smooth, and without streaks, being commonly adorned with some little Imagery. It emits but little of a subtle fume upon Solution, and sometimes lets fall a thick heavy Sediment. It sells in China for six thousand Touneij the Pound.

The Worst resembles somewhat of a Parallelogram, but is very uneven and repare to the touch; it also breaks coarse and brown, and smells like common bot. In a word it seems to have a good store of Coal dust in its composition. But in general it should be noted that the Figure is no essential Mark of the Quality.

This is the very Ink with which the Chinese express their Writings and Characters, and also delineate their Pictures, the Price of which depends chiefly on the goodness of the Ink.

Its Virtues

Its only ~~main~~ medicinal use in India, is for inflammatory Disorders of the Eyes; and we use it here in the same cases. The Solution should be strong and used by way of Fomentation, to be repeated and continued according to the Exigency of the Complaint.

I have been satisfyd of its efficacy both in my own Practice, and from the
the

the report of other Physicians and Persons worthy of credit; This owing to its power of putting a stop to Rusefactions of the Blood and Juices, and in some measure restoring the Tone of the relaxed Solids.

It is probably on this score that we have frequently experienced it to be an excellent Remedy in Spitting of Blood and other Hemorrhagies; as also in ^{and bilious} ferous Diarrhoeas, arising from the like causes.

It is also very serviceable in Dysenteries, if the Tincture of it be given with a few Drops of Balsam of Peru or Capivi by way of Draught. which is a favourite Method of a certain Physician I much esteem.

I have experienced its goodness in cases where blood is drawn from the Larynx, Palate, or Gums.

It is a good Refrigeratory in Inflammations from heating with the Sun, shine, or over violent Exercise.

The Way here is to make a strong solution of the Juice in warm water or ~~the~~ other proper Vehicle, till it is no longer transparent, and to give $\mathfrak{z}\text{iv}$ for a Dose.

Vol. 3
3: March 1943

March 3. 1747-8.

+ MS

78977

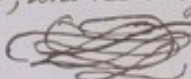
An account of an extraordinary Bird, as related in a letter from Doctor João Mendes Saquet Physician of the City of Elvas in Portugal, to Doctor De Castro wherein he desires him, to present the skin of the said Bird to the Royal Society. =

I have just now received from Brazil, by the last Rio de Janeiro Fleet, the skin of a remarkable Bird. I could have wished it might, if possible, have been brought alive, and in that condition, have been presented to the Royal Society, in yours or my name. But I find by my friend's account, that it could not be made to subsist long out of the water. For which reason, I can only send you the skin, with the Description my said Friend gives me of the Bird it self.

This Bird is a perpetual Inhabitant of the Water, either swimming with its Fins, which seem substituted for wings on its back, or crawling with its feet at the bottom, where it was taken with a net among Fish, which is its Food.

When brought alive into a House, it could neither walk, nor stand on its legs, but only as it were rowl along, nor even when provoked, could move his Body, either to resist, or get away; but would snapp with its Beak any thing in its reach, as a stick or the like, and that only once, if it felt hard; tho it gaggled sadly if further vexed therewith.

As there was no preserving its life, it was ordered to be strangled with a strong line, and afterwards to be skinned; but tho the ligature was kept tight about his neck a good while, he seemed to feel no inconveniency from the want of respiration, yet manifested symptoms of pain; so he was killed by cutting open his belly.

The Entrails were of an extraordinary size, the Heart as big as an Apricocke, the Intestines manifold, much intangled, and held together with a very thin membrane, in the following manner = 

n^o. 1.
June 9. 1748.

46/7 Abstract from a letter of Mr James Simon to the President

Dublin 20 Jan. 1746⁷

—— in my letter of the 11th of the last month I mentioned to you, that I thought Electricity if properly applied, might produce some good effects in Nervous complaints: and since I have been able to go abroad again I have been witness to the following Experiment.

One Henry M^c Cormock of about 60 years of age, was seized in the year 1739 with a palsy throughout the right side of his body. from head to foot, he lost his speech, his hearing on that side, with the use both of his leg and arm, which last he could no ways extend but kept hanging by his side, from which he could only move it like a Pendulum about 14 inches in a curve line. He has been ever since under the care of Physicians and Surgeons, but

to no purpose, and was at last placed in the Hospital for incurables of this City, as being past all hopes of recovery.

About 3 weeks since we had him brought to Mr Dooth, a young Gentleman who reads lectures here in Experimental Philosophy:

where he was strongly Electrified, in the presence of several Physicians, Surgeons, Apothecaries and other Gentlemen.

After he had been electrified 3 or 4 times on

different days, he began to move his arm, and can now walk with a stick, lift his hand to his head and

take off his hat and move his fingers, he speaks freely and hopes he shall be perfectly cured. He has now

been electrified about 20 different times, and says he

always feels he shocks very violently in all his

joints. A Glas vial of Water is used in the process of

the experiment. His leg is first electrified by bring-

ing round it a small chain of iron wire, which is

afterwards put about his arm or round his wrist.

As his Experiment has both surprized me, and given me pleasure, I have hoped his short account of

it would ~~not~~ also be disagreeable to you: as you may

fully depend on the truth of the relation.

m. 3.
Jan. 29. 1746-7.

ms 5403 / 59
Turin the 25th February N. S. 1747.

47
Sir

+ MS. N^o. 49 78917

The inclosed Italian Oration which -
was pronounced some Weeks ago in the -
University of this Metropolis, having met
with some Applause and been favourably -
received by the publick, I have been desired
by the Author of it to submit his Performance
to your Superior Judgement and that of the
Royal Society, whose approbation he is -
particularly ambitious to be honoured with:
If it is found upon an attentive perusal of this
Deduction that he has made some New Discovery
or given greater light in the Subject he has
treated therein, by which Mankind may in -
the least be benefitted, I dare say the justice
and commendation that are due to the
Author will be impartially bestowed on
this performance in your Journals; And

Martin Folkes Esq

Therefore

Therefore all I have to beg of you, Sir, is that
you would acknowledge the receipt of this
Letter with the Paper inclosed, and enable
me to acquaint Signor Loteri with the
judgement the Royal Society has pronounced
on his oration; In which you will particularly
oblige,

Sir

Your most obedient
Most Humble Servant,
Arthur Villotté.

n^o. 1.

Apr. 2. 1747.

67769

no. 4.
may. 26. 1748. + Aff

MS 5403/58

You'll be so good to make our Compliments to Societies:
 Since we last had occasion of Communicating, the principal matters
 worthy notice, which have occurred to us here, have been —
 8th February a large Disertation upon Bezoars by D^r Green,
 26th — the same continued, with some Accounts of other similar
 Productions in various parts of Human Creatures & other Animals.
 whereof we have several Specimens. N^y D^r presented several dried Plants to ^{our} Hortus Siccus.
 3 March a Discourse of Antiquity, Authority, & absolute necessity
 of the Court of Commissioners of Sewers here, on occasion of that
 Commission being now to be renewed: with the Power & Duty
 of the Commissioners, & of the several Officers of that Court, the
 Sheriff, Surveyors, & Dykes reeves — with the Rules of Practice
 therein — a MS — Compend for the Instruction & Use of Gentlemen,
 shortly to be inserted & set therein for the preservation and Im-
 provement of their Country, by a Lawyer who has been many
 yearses Chairman of the Sessions of those Commissioners:
 was communicated and read to the Company.
 a Lett^r from the Rev^d Mr Smyth of Woodston abt^r the Visit of Sheriffs — & drawing of an antient Tombstone
 — The Doctrine of Embanking & dreynning, and the Methods here used, ^{there}
 being well worth the knowing by the resident Members of this Society.

10. March From M^r. Vertue we had sent us by the Treasurer a copious list
of 4 English historical portrait pictures, after which he is engraving
and of several other curious paintings in his custody or collection
a long list of a Treatise of the Prices of Corn, Grain, & other Commodities from
before the Conquest. read **FAUSTINA & AVGVSTA.** Cere medio presentat
17. Some Accounts from the Register of several uncommon and very
Curious Flies, & other Insects, he had then lately seen at
M^r. Hughes's in Gracechurch Street & M^r. Wilkes's in Fleet Street
London - 2 Drawings by a young Lady after Italian Chariot & Poulterer
an ant. by D^r. Jurin to an ant. sent him of our Metal Thermometres. read
24. a Discourse of the great Improvements in Physick & Surgery made in
England by the Royal College of Physicians in London - & of the
Dieting, establishing, & endeavouring that Coll^y: and the Surgeons
Company, and of Holbein's Capital Picture in their Hall on
that Occasion, was read & Baren's Print thereafter & the
Complimentary inscription thereon to K^y. H. 8. explained & read.
6 Drawings in Indian Ink by Jonas Pratt were produced, to shew
31. the same Discourse as at last his continuing to improve under direction of some of the members, Soc. by desire of the Company, was
read over again. - The Treasurer brought in to the Museum the residue
of the works of the Antiq^y Soc. Londⁿ. completing their 8th Volume & were
reposed with all of preceding the kind present to us by those learned
Gentlemen by their Standing Order, to our Honour & Encouragement.
which we are proud of, and ever gratefully Acknowledge -
Spalding Apr. 2. 1748.

XI. A brief account of the Roman *tesera*.
by Mr. John Ward F. R. S. & Prof. Rhetor. Gresh.

The brass plate, which accompanies this paper, and has been the occasion of it, was dug up some time since at Marketstreet in Bedfordshire; which lies in the Roman road called *Watling-street*, about five miles on this side of *Dunstable*. The inscription ingraven on the two sides is,

TES · DEL · MAR
SEDIARVM

See
TAB.

Which words may, as I apprehend, be read at length in the following manner:

Tesera Dei Martis Sedarum.

The first abbreviated word TES. I take to stand for *tesera*, a dye or cube,¹⁾ so called from the Greek word *τέτρας* or *τέτρας*, four; respect being had to its number of sides, distinct from the two horizontal planes, above and below. And under this consideration it was distinguished from the *talus*, which being round at each end contained only four planes or faces, whereon it could stand, and therefore when thrown had no more than two side faces in view. Hence *ludere talis et teseris* are spoken of by Roman writers as two different games.²⁾

¹⁾ *Macrobius. In
romm. Scip.
Lib. II. cap. 2.*

²⁾ *Cic. De senect.
cap. 16.*

But if this was the first and original
notion

notion of the word *tesiera*, it was applied afterwards to many other things; and that not so much from a similitude in the figure, as from the relation they bore to some other thing, of which they were the sign or token, as the points on the upper plane of the die denoted the good or ill success of the cast. To recite the several uses of this word would both be tedious and unnecessary; and therefore I shall mention some few only, from which the design of this plate may the more easily appear.

And I shall begin with the *tesiera hospitalis*, which was either public or private. As to the former, we find among the inscriptions published by Gruter instances of two municipal towns, who put themselves under the patronage of a Roman governor. And the reciprocal ingagement between them, which was engraved on two copper plates, in the form of an oblong square, with a pediment at the top, is called in both *TESSERA HOSPITALIS*.¹⁾ The design of the latter was to cultivate and maintain a lasting friendship between private persons, and their families; and gave a mutual claim to the contracting parties, and their descendants, of a reception and kind treatment at each others houses, as occasion offered. For which end it was requisite, — that those *teserae* should be so contrived, as might best preserve the memory of that —
transaction

¹⁾ Pag. CCC LXII,
CCC LXIII.

transaction to posterity." And one method of ^{"Plant. Poenul.}
doing this was by dividing one of them length-
wise into two equal parts, upon each of which
one of the parties wrote his name, and inter-
changed it with the other. A draught of
one made of bone, and so divided, may be
seen in *Thomarius*, with the name of
the person on each part. Upon one of them
is,

POLYNICES
ASCANIO.F.

And upon the other,

CLAPHYR
ANDRAE.M.I.F.

The names are written on the inside, and when
the two parts were put together, they made a
cylinder.²⁾ From this custom came the prover-
bial expression, *teseram hospitalem confringe-*
re; which was applied to those persons, who
violated their engagements.³⁾

²⁾ *De tesser. hos-
pital. cap. 15.*

³⁾ *Plaut. Cistell.*
ll. 4. 29.

The *teserae frumentariae* are often mentioned
by Roman writers, which were small tallies given
by the emperors to the populace at Rome, intitling
them to the reception of a certain quantity of corn
from the public at stated seasons. And those,
who were possessed of them, when they did not
want the corn, sometimes sold them to others;
as we learn from the satyrists, when he says:

Summula ne pereat, qua vilis tessera venit
*Frumenti.*⁴⁾

⁴⁾ *Juvenal Sat.*
VII. 174.

The

The person, who had the inspection of these *teserae*, and distributed the corn to those, who produced them, seems to have been called *teserarius*; as *Pignorius* observes from a funeral monument, inscribed SYMPHORO TESSERARIO SER. CAESARIS."

¹⁾ *De servis*, pag. 318, ed. 1674, oct.

These *teserae* were first made of wood, as appears from the words of *Pliny*, where treating upon the nature and properties of trees he says, *Ligustra tesseris utilissima*.²⁾ But *Jabretti* has published the draughts of two of them made of stone, in the form of cylinders, and of the same size with the originals. The length of them is some what short of three inches, the diameter three quarters of an inch, and the following inscriptions cut upon them contain the names of the persons, to whom they belonged:

²⁾ *H. N. Lib. XVI.* cap. 8. §. 31.

TORQVATVS
CREOP. D. I.

LVPVS D. I.
PELORI

Where D. I. the two last letters in each inscription stand, as he supposes, for *die prima mensis*, the time appointed for receiving the corn.³⁾

³⁾ *Inscript. antiq.* pag. 530.

There was also another sort of *tesera*, not much unlike these, which intitled persons to a right of the public games and other diversions; but they are generally made in the form of an oblong square. *Pignorius* has given us the draught of one in his own possession, which consisted of ivory. Upon one of the sides is the name ^HPILOMVSVS PERELL, on the next SPECTAVIT, on

on the third a trident, and upon the fourth a palm branch"; the two last of which plainly shew, that it was given for admission to the combats of the gladiators. Others of them had on different sides the name of the person, with the day, on which the show was exhibited, and the names of the consuls at that time. Instances of these may be seen in Thomafinus, one of which, as he says, was made of yellow glass.²⁾

¹⁾ ubi supra
pag. ~~37~~ 38.

²⁾ De test. hospital.
tal. cap. 15.

But the testera militaris most frequently occurs in the Roman historians, which was the signal given by the general, or chief commander of an army, as a direction to the soldiers for putting in execution any duty or service required of them. This upon urgent occasions was only vocal; as for a sudden march, or an attack upon the enemy. But in ordinary cases, as for setting the watch, taking their dinner, or the like, it was written on a tablet. Tho in either way it was first given by the general to the officers next in rank, and from them to the subalterns, till it came to the person, whose province it was to communicate it to the soldiers in each company. This tablet was commonly made of wood, as appears from Polybius, who³⁾ calls it ξυδνίσιον, a small piece of wood.³⁾ The signal inscribed upon it was very short, and usually comprised in one or two words; as victoria, palma, virtus, Deus nobiscum, triumphus imperatoris, mentioned by Vegetius;⁴⁾ with many others of the like nature, which may

³⁾ Lib. vi. pag.
479. ed. Parif.

⁴⁾ Lib. iii. cap. 5.

¹⁾ Lib. II. cap. 7.

²⁾ Pag. DCVI. 10.
DCVIII. 7. DCIX.
10.

may be seen in antient writers. The person, whose office it was to impart the signal immediately to the soldiers, is by Vegetius called teserarius." Hence in Gruter's inscriptions we meet with AVRE. IANVARIVS. TESSERARIVS. LEG. XIII, and C. GALERIO. C. LIB. AGATHON. TESSERARIO. COH. XII. PRAET. MILITVM, as also L. POMPEIO. L. F. POMP. REBVRRO. TESSERARIO. IN. CENTVRIA ²⁾ By which different forms of expression compared together one would be lead to conclude, that every century had its teserarius, from whom the soldiers immediately received the signal; and that when the legion or cohort only is mentioned, the meaning is not, that the person named in the inscription performed that office to the whole legion or cohort, but only to some particular century in each of them.

But besides these civil and military teserae there were others, which more especially related to religious affairs, and may therefore be called sacred; to which the inscription on this brass plate seems to agree. For the two next words ingraven upon it, namely DEI. MAR. must, I think, stand for Dei Martis. And if the last word SEDIARVM be taken for the name of a town, called Sediae, this tesera may respect the God Mars, as the tutelar deity of that place. The religious worship among the Romans consisted chiefly in sacrifices and other public ceremonies, the expense of which in particular

particular places was supported either by the contributions of the inhabitants, or by private gifts. We have an instance of the latter in an inscription first published by Reinesius, where it is said, that L. Veratius Felicissimus, patron of Tolentium, (or Tollentium a municipal town in Italy") gave to the inhabitants their annual sacrifices, which were offered on the eleventh of May for a plentiful harvest. This inscription is cut on a brass plate in the form of an oblong square, with a female bust in a pediment at the top, designed very probably to represent the deity, to whom they addressed. As this inscription is peculiar in its kind, I shall here give the whole of it, as it stands in Reinesius.

TESSERAM . PAGANICAM
L. VERATIVS . FELICISSI
MVS . PATRONVS . PAGANIS
PAGI & TOLENTINES
HOSTIAS . LVSTR. ET. TESSER.
AER. EX. VOTO. L. D D
V. 1D. MAIAS. FELICIT.

²⁾ Append.
num. 8.

This is called tesse^{ra} paganica, as I imagine, from its intitling the pagani, or inhabitants of that town, to the annual claim of the sacrifices therein mentioned. And so far it agreed with the nature of a public tesse^{ra}, which being lodged in the hands of the proper

proper officer authorized him to collect the several contributions assigned for such religious purposes. And of this latter sort I take the plate to have been, which makes the subject of our present inquiry; both the form and size of it suiting very well with such a design, as it was portable, and ready to be produced, if occasion required. And agreeably to this notion of the word tesiera the antient Glossaries interpret tesierarius by yearuatrus, a scribe or clerk.

As to the following word SEDIARVM, tho it no where else occurs, that I know of; yet this, I presume, can be no just objection against its being taken here for the name of a town, called Sediae: when it is considered, how many instances of the like nature are to be found in the inscriptions collected by Gruter and others, which give us the names of many antient places in the Roman provinces not mentioned by any other writers. And besides, the form of this word appears analogous to the names of several other Roman towns here in Britain; as, Durobrovae Rochester, Ratae Leicester, Rutupiae Richborough, Spinae Spene, and some others. It is not improbable, that this plate was found not far from the place, whose name it bears, and which might be situated among the Cateuch-
lani,

lani, as their territories are described by Camden." But as I have never before seen, nor heard of any thing similar to it, I would submit what is here offered to the judgement of the curious in these inquiries.

"Britann.
pag. 275, ed.
1607.

G. C. Feb. 25
1747.

John Ward.

n^o 2.

March. 3. 1747 - 8

pr. Tr. 496. XII.

TES DEI MAR SEDIARVM

n^o 2

1917
M. 8403 160
27.

A continuation of the former letter, read Feb: 6 1745: ms 5403/61

+ All N^o 35

68412

As water is a non electric & of consequence a conductor of Electricity, I had reason to believe that Ice was endow'd with the same properties. Upon making the experiment, I found my conjectures not without foundation, for upon electrifying a piece of Ice wherever the Ice was touch'd by a non electric, it flash'd & snapped. A piece of Ice also held in the hand of an electrified man as the before mentioned proceeds, fired warm spirit, chemical vegetable oils, Lanthorn, & Gunpowder prepared as before. But here great care must be taken that by the warmth of the hand or of the air in the room the Ice does not melt; if so, every drop of water therefrom considerably diminishes the received electricity. In order to obviate this, I caus'd my assistant, while he was electrifying to the buttons of his coat, & thus being electrified as well as the Ice, prevented any loss of the force of the Electricity. The experiment will succeed likewise, if instead of the Ice, you electrify the spirit &c & bring the Ice not electrified near them. I must observe, that Ice is not so ready a conductor of Electricity as water, so that I very frequently have been disappointed in endeavouring with it to fire inflammable substances, when it ^{has} been easily done by a sword or the finger of a man. It appears from experiments, that besides the several properties, that Electricity is possest of peculiar to itself, it has ~~the~~ some in common with Magnetism & Light.

impregnate the whole, non electric mass in contact with it or nearly so, however different in itself, & which must of necessity be terminated by an originally electric before the Electricity exerts the least attraction, & then this power is observed first at that part of the non electric the most remote from the originally electric. Thus for example, by an excited tube held over it, leaf gold will be attracted through glass, cloth &c, held horizontally in the hand of a man standing upon the floor & this attraction is exerted to a considerable distance. On the contrary, the rubbed tube will not attract leaf gold or other light bodies however near through silver, tin, the thinnest board, paper, or any other non electric, held in the manner before mentioned. But if you rub the paper over with wax melted & by that means introduce the originally electric therein, you observe the electricity acts in right lines & attracts powerfully. And here I must beg leave to remind you, not only of the former corollary, but of some of the former experiments also, by which it appears, that although to make a non electric exert any power we must excite the whole mass thereof, yet we can excite what part & what only of an originally electric we please. Thus you observe, that leaf gold & the seed of Cotton-wool which grows upon bogs & is a very proper subject for these inquiries, are attracted under a glass jar turned bottom upwards, upon which are placed books & several other non electric's, & that the motions of the light

Silver, thread & such like, but also upon originally electric, as Silk, dry feathers, little pieces of glass & resin; it attracts all bodies, that are not of the same standard of Electricity (if I may be allowed the expression) as the excited body from which it proceeds. I have found no body however dense, whose pores are not pervious to Electricity by a proper management, not even Gold itself.

Propos. 2.

In common with Light, Electricity pervades glass, but suffers no refraction therefrom; I having from the most exact observations found its direction to be in right lines, & that through glasses of different forms included one within the other & large spaces left between each glass.

Corollary.

This rectilinear direction is observable only as far as the Electricity can penetrate through unexcited originally electric & that perfectly; nor is it ^{at} all material, whether these substances are transparent, as glass; semidiaphanous, as Porcelain or thin cakes of white wax, or quite opaque, as thick woollen cloth as well as woven silk of various colours; it is only necessary that they are originally electric. But the case is widely different with regard to non electric; where ^{upon} the direction, given to the Electricity by the excited originally electric, is altered as soon as it touches the surface of a non electric, & is propagated with a degree of swiftness scarcely to be measured in all possible directions to

impregnate the whole non electric mass in contact with it or nearly so, however different in itself, & which must of necessity be terminated by an originally electric before the Electricity exerts the least attraction, & then this power is observed first at that part of the non electric the most remote from the originally electric. Thus for example, by an excited tube held over it, leaf gold will be attracted through glass, cloth &c, held horizontally in the hand of a man standing upon the floor & this attraction is exerted to a considerable distance. On the contrary, the rubbed tube will not attract leaf gold or other light bodies however near through silver, tin, the thinnest board, paper, or any other non electric, held in the manner before mentioned. But if you rub the paper over with wax melted & by that means introduce the originally electric therein, you observe the electricity acts in right lines & attracts powerfully. And here I must beg leave to remind you, not only of the former corollary, but of some of the former experiments also, by which it appears, that although to make a non electric exert any power we must excite the whole mass thereof, yet we can excite what part & what only of an originally electric we please. Thus you observe, that leaf gold & the seed of Cotton-grass which grows upon boggs & is a very proper subject for these inquiries, are attracted under a glass jar turned bottom upwards, upon which are placed books & several other non electric's, & that the motions of the light

bodies underneath correspond with the motions of the glass tube held over them, the Electricity seeming instantaneously to pass through the books & the glass. But this does not happen, till the Electricity has fully impregnated the non electric which lie upon the glass, which received electricity is stopped by the glass & then the non electric dart their power directly through the upper part of the glass after the manner of originally electrics. But if the thinnest non electric, even the finest paper, as I before mentioned, is held in the hand of a man at the smallest distance over the leaf Gold & the Electricity is not stopped, not the least power will be exerted & the Gold will lie still. I must here remark likewise, that this law of Electricity is so constant & regular, that I have not found one deviation from it; so that even the quicksilver spread thin as it usually is at the back of a plate of looking glass, will prevent the passing through of the electrical attraction, unless stopped by an originally electric. This penetration of the electrical power through originally electrics is much greater than has hitherto been imagined & has caused the want of success to great numbers of experiments. I have been at no small pains to determine how far this power can penetrate through a dry originally electric, & have found by repeated trials, that either in a cake of wax alone or of wax & resin mixed, when the Electricity is very powerfull, it has passed, I say,

in straight lines through these cakes of the thickness of 2 inches $\frac{8}{10}$; but I never could make it act through one of 2 inches $\frac{8}{10}$, for in this it was perfectly stopped. So that the cakes commonly made use of to stop the Electricity by being too thin, suffer a considerable quantity of the electrical power to pervade them, & be lost in the floor. I make no doubt, if the Electrical power could be more increased, it would penetrate much further through these originally electric bodies.

Proposition 3^d.

Electricity in common with Light likewise, when its forces are collected & a proper direction given thereto upon a proper object, produces fire & flame.

Corollary.

The fire of Electricity (as I have before observed) is extremely delicate & sets on fire, as far as I have yet experienced, only inflammable vapours. Nor is this flame at all heightened by being superinduced upon an Iron rod, red hot with coarser culinary fire as in a preceding experiment, nor diminished by being directed upon cold water. However I was desirous of knowing, if this flame would be affected by a still greater degree of cold, &

in order to determine this, I made an artificial cold, by which the Mercury, in a very nice Thermometer adjusted to Fahrenheit's Scale, was depressed in about 4 minutes from 15 degrees above the freezing ^{point} to 30 degrees below it, that is, the Mercury fell 45 degrees. From this cold mixture when electrified, the flashes were as powerfull & the stroke as smart as from the red hot Iron. I could have made the cold more intense, but the above was sufficient for my purpose. This experiment seems to indicate, that the fire of Electricity is affected neither by the presence or absence of other fire. For as red hot Iron, by Sir Isaac Newton's scale of heat is fixed at 192 degrees, & as the ratio between 192 degrees & Fahrenheit's is as 34 to 180, it necessarily follows, that the difference of heat between the hot Iron & the cold mixture is 1040 degrees, & nevertheless this vast difference makes no alteration in the appearance of the electrical flame.

I may perhaps ~~be~~ be thought too minute in some of the before mentioned particulars; but in inquiries as these are, where we have so little a priori to direct us, the greatest attention must be had to every circumstance, if we are truly desirous of investigating the Causes of this surprizing power. For as has been said upon another occasion by my ever honoured Friend Martin Folkes Esq^r our most worthy President, "that Electricity

" seems to furnish an inexhaustible fund for inquiry,
" & Sure Phenomena so various & so ~~inexplicable~~ wonderful
" can only arise from causes very general & extensive,
" & such as must ^{have} been designed by the Almighty Author
" of Nature for the production of very great effects &
" such as are of great moment to the system of the
" Universe.

no. 1.

Feb. 6. 1745-6

Pr. No. 478. VIII.

Gentlemen.

To the Royal Society

N^o. 29.

I lately received your commands in relation to the examining the electrical experiments made by Professor Winckler of Leipzig; the purport of which were, that if odoriferous substances were included in glass globes closely stopped, & if these globes were electrified, the smell of the odoriferous substances would ~~come~~ pass through the glass, & be conveyed with the electrical ~~effluvia~~ ^{bodies} to considerable distances upon ~~substances~~ readily conducting electricity: that after the globes containing these matters had been rubbed a few minutes the flavour of their contents would be strongly perceptible upon entering the chamber wherein they were rubbed: & that the substances he had already put to the trial were Sulphur, Cinnamon, & Balsam of Peru; to which a letter from John Daniel Gaiffet, a friend of Mr Winckler, adds Camphire & Quinta essentia vegetabilis or our rectified vinous spirit. Mr Winckler mentions, that when he electrified with Sulphur the smell thereof was not perceptible in the room before the globe was rubbed, but that then the vapours would strike you at ten feet distance; ~~there~~ these so infected the air, that his cloaths, his body, & his breath ~~was~~ ^{were} scented therewith; & that upon repeating this experiment for three days there came large pimples in his face. That Cinnamon under the same circumstances sent forth its flavour in great abundance, & this continued in the chamber where the globe was rubbed untill the next day. That the smell of Balsam of Peru in like manner so impregnated the air of the room, that the cloaths & the breath of the persons therein smelled thereof after having passed through several streets; & even that Mr Winckler's tea tasted thereof next morning. As to what relates to the several remarkable cures said to have been wrought by electricity, they are not of his own knowledge; he mentions to have received them from a pamphlet of Johannes Franciscus Livati of Venice. His

cures are so loosely told & are of so extraordinary a nature, that at present to me they seem highly improbable.

In order conveniently to inclose the substances before mentioned, I order'd two globes of nine inches diameter to be mounted in such a manner upon wooden centers, that upon unscrewing one of these centers, any substances might be easily conveyed into the globe through an opening, into which was fitted a glass stopper. To observe if the thickness of the glass any ways contributed or hindered the success of the experiment, one of these was of the usual thickness, the other lighter. As these globes were disposed in my electrifying machine with their poles parallel to the horizon, the substances inclosed from the centrifugal force formed a kind of Zone round their equators & aptly presented themselves to the cushions in their rotation: this would not happen, if the poles of the globes were in a vertical direction.

I several times at intervals inclosed in the thickest of the globes, whose sides were about $\frac{1}{12}$ of an inch, the following substances always with the same success, though in different quantities. But I purpose now more particularly to lay before you the result of some experiments made at my house on Friday April 29 in the presence of those two learned members of this society, the right Honourable the Lord Charles Cavendish & Francis Wollaston Esq.

I put four ounces of powder'd camphire into the thickest globe. This was done in a room different from that in which the electricity was to be excited, that there might before the electrifying be no smell of the camphire. When it was well stopped, the globe was rubbed. At first, the electricity, upon touching the prime conductor or a person standing upon electrics &c, was very vigorous; but the longer the friction continued the electricity decreased; nor was it to be made more vigorous, as happens when the globe is

empty, by rubbing the cushions with fresh chalk. After more than a quarter of an hour, when the electricity was very weak, I discontinued the friction. All this while no smell of the camphire could be perceived, neither in any substance electrified, nor upon the outside of the glass itself.

I then ordered the camphire to be wiped very clean out of the globe; upon which, though just before when the camphire was therein the excited electricity was very languid, it electrified again with its usual vigour.

Eight ounces of bruised cinnamon were put into the same globe stopped close, which was rubbed about the same time as when the camphire was therein; but we were not able during this operation to distinguish the smell of the cinnamon either upon the prime conductor or at the equator of the globe. Neither was I able by any means while the cinnamon was in the globe to make an electrical snap by bringing my finger near the prime conductor. Some Swan's down connected by a wire to the prime conductor was scarcely disturbed though this substance is so sensible of the electrical effluvia.

Into a globe blown very thin for this purpose I put six ounces of powder'd native Sulphur, & stopped it close. This was rubbed the same time as the former. At first the electricity excited thereby was very vigorous, but, as it happened when the camphire was in the globe, grew less by continuing the friction. Nobody present could perceive the sulphureous smell either in the person electrified, or upon the prime conductor.

After this globe was well cleared of the Sulphur, I poured therein six ounces of Peruvian Balsam. This was rubbed as before without our being able to excite any electricity, or perceive the smell of the Balsam. But upon

unstopping the globe, which was warmed by the friction, & consequently the included air much rarefied, its odour diffused itself all over the room.

The next morning, I poured four ounces of rectified spirit into the thin globe, which was rubbed as before, but without being able to excite thereby ~~either~~^{the} electricity, or smell the spirit.

Of the fine substances, with which these experiments were made, two were originally-electrics; viz, camphire & sulphur. These, when included in the globes, though they lessen their power of exciting electricity, did not take that power absolutely away. The cinnamon & rectified spirit, both non-electrics, divested the globes of their usual power of exciting electricity. The Peruvian Balsam is a resinous substance, which, I am of opinion were it evaporated to a solid consistence, would like others of its class be an electric offspr; but here, from the quantity of water entangled with it, produces the effects of non-electrics.

It was the observation even of those, who first engaged in electrical inquiries, that their globes & tubes should be as free as possible from moist vapours dust & such like, for by these they found the effects of their globes considerably lessened.

I have here, Gentlemen, laid before you a detail of these experiments with the result of each, which, as often as I have made them, have been attended with the same effects. Why they differ so widely from those of Mr Winckler, I cannot determine? I am unwilling to call in question that gentleman's veracity. I would recommend therefore these trials to others, & am desirous that Dr Mortimer should send Mr Winckler so much of this account as he shall think convenient, that if that gentleman has any particular management by which he is able to exhibit different phenomena, he may communicate it to the royal Society.

London May 4th 1748.

N^o. 2.
May. 5. 1748.

MS 343/63 62412
Professor Winckler
Letter to M^r. Schrader at Londⁿ
Leipsick April y^e. 22th 1746
May y^e. 3rd

Since M^r. Muschenbroek has made an Experiment, that has caused astonishment, I would likewise tell You something ^{of what I have been doing.} ~~about my Doings.~~

When I heard of M^r. Muschenbroek's Experiment, I ~~did~~ try'd the same, but I found great Convulsions by it in my Body. It ~~did~~ put my Blood into great Agitation, so that I was afraid of a ^{an ardent} ~~hot~~ Fever, and was obliged to use ^{refrigerating} ~~precipitating~~ Medicines. I felt a heaviness in my Head, as if I had a Stone lying upon it. It gave me twice a Bleeding ^{at} ~~from~~ my Nose, to which I am not inclined. My Wife who had ~~used~~ ^{received} only ~~twice~~ the Electrical ^{Flash but} Sparks, found herself so weak after it, that she could hardly walk. A Week after ~~it~~, she ^{received} ~~tried~~ only once the Electrical ^{Flash} Sparks: a few Minutes after it, she bled ^{at} ~~from~~ the Nose.

I read in the News-Papers from Berlin, that they had tried these Electrical ^{Flashes} ~~Sparks~~ upon a Bird, and had made it suffer great pain ^{thereby} ~~by it~~. I did not ^{repeat this Experiment} ~~do this~~, for I think it wrong to give such pain to living Creatures.

Brutes ^{piece of}
I therefore take instead of Men or Beasts a Metal,
and I put it upon a stand under the Electrical
Pipe, which Pipe propagates the Electricity. To this
Metal is fasten'd an Iron Chain, which goes about
the Bottle with Water, in which the Brass Wire is put
which Wire is fastened to the Electrical Pipe.

When then the Electrification is made, the
Sparks that fly from the Pipe upon the Metal, are
so large and so strong, that they can be seen (even in
the day-time) and heard at the distance of fifty Yds.
They represent a Beam like Lightning of a clear and compact
Line of Fire, ^{and} they give a sound, that frightens the People
that hear it.

John Henry Winkler
Professor Ordinarius of the Greek
at Latin Tongue at Leipsick.

May 24. 1746

no. 2.
Prof. Winkler
of Electrical fire.
communicated to the Rev. A. S.
by Mr. Schrader.

May 29, 1746

Gr. No. 480. VI.

To
Dr. Mortimer
Secretary to the Royal Society
at his House in Dartmouth Street

Westminster.

Letter from Mr. John Henry Winkler
to the Rev. A. S. dated 1746. The letter
concerning the effect of electricity upon the
human body. - for friend in London
his wife

Extract out of Dr. Winckler's Abstract
of his Oration, made on the 22^d Febr.

1748. in praise of Electricity, which Abstract
he sent to Dr. Mortimer, on the 12th of
March.

By which is said, it is easy to judge, that Electricity
has a great influence, upon the Blood and Juices of
the Body, and puts them in motion, resolving and
thinning the Fluids. I know a Gentlewoman,
who presently gets the menfes, when she is Electrified.
a Physician of the Town of Hirschberg in Silesia,
whose name is Thebesius, writ to me some days
ago, that of late, when he is Electrified, he always
bleeds from the nose.

Mr. Winckler tells Mr. Schrader, that in the Abstract
sent to Dr. Mortimer, he forgot to mention these two
Experiments.

of Electricity

March 31. 1748.

date in post.

n^o. 186. p. 267.

Experiment.

Let us see what will happen if we take the glass and

rub it with the silk.

It will attract the light.

And if we take the glass and

rub it with the silk.

It will attract the light.

And if we take the glass and

rub it with the silk.

It will attract the light.

And if we take the glass and

The Caterpillars described in this Book are such as produce Moths and as there is a great variety of them they are ranged under the following heads or Chapters

Chap: 1.

The Caterpillars belonging to this Chapter have eight feet, two holders and six claws or hooks their bodies are composed of a series of circular joints or rings which they are capable of contracting or expanding like a spiral spring and by this means perform their progressive motions on their bellies

The Caterpillars under this head may likewise be Divided into three Classes, 1. Naked or without hair, 2. having very little hair, 3. having much hair

The first Class or the naked and without hair may be subdivided in the following manner

- Class 1
- § A Such as have their bodies perfectly smooth without any risings or Protuberances
 - § B having hooks or horns on their tails
 - § C having one Protuberance on their Rumps and indented Markings
 - § D having two or more Protuberances
 - § E having their Shoulders rising into an Angular Figure

Class 2

- § a having very little Hair
- § b with one Protuberance
- § c with one or more Protuberances

Class 3.

having much hair

- § 1 A having no Protuberances
- § 2 A Ditto having rings of 2 colours Surrounding them
- § 3 A Ditto having Indented Markings
- § B having hairy Protuberances on the head back or tail
- § C having many tufts of hair and therefore called Tussock Moths

Chap: 2

These Caterpillars like those in the former Chapter have 8 feet 2 holders and 6 Claws or hooks but perform their progressive motions in a different manner by drawing up the tail towards the head and thereby bending their bodies into the form of an half loop and from thence are called half loopers

Class 1

§ a having Protuberances

Class 2

These Caterpillars have 2 holders 2 feet and six hooks or Claws and are of the species of the half loopers

- § a having two feet and hairy

Chap: III These Caterpillars have four holders with 6 hooks or claws having no feet in the middle part of their bodies like those already described they perform their progressive motions by fixing the head and drawing up the tail to it so that each step forming a loop they are called loopers they are furnished with strong and sharp Claws in their holders by which they can fasten themselves in such manner as to support the rest of their bodies in the air either perpendicularly or Horizontally or in any other posture they please without any regard to the Center of Gravity in their respective bodies they are likewise able out of their Viscera or Bowels to furnish themselves with lines by which means they can let themselves down from the branches of Trees or other heights at pleasure.

Class: § 2 Smooth bodies without Protuberances

§ a with Protuberances

Class 2 § 1 b hairy without Protuberances

§ 2 b hairy with Protuberances

Chap: III Caterpillars having several feet being shaped like Woodlice

Chap: V Of Moths whose Generation is yet unknown

Book the 2:

Chap: 1 Caterpillars which produce Flies partly resembling the Moth and partly resembling the Butterfly

Book the 3:

Of Caterpillars which produce Butterflies

Chap: 1 Octopedes

Class 1 Naked

§ A Without any Protuberances fastening its Chrysalis in a Horizontal posture by a thread tied across the breast and producing the Swallow tail Butterfly.

Class 2. having a little hair and fastning their Chrysalis as the last sort

§ a producing round wing'd Flies

Class 3 Armed with Spikes and hanging their Chrysalis perpendicularly by the tail

§ a

Chap: 2 Resembling Grubs or Woodlice

Class 1

Chap: 3. Of Butterflies whose Generation is yet unknown

Gen. 29. 1746-7.