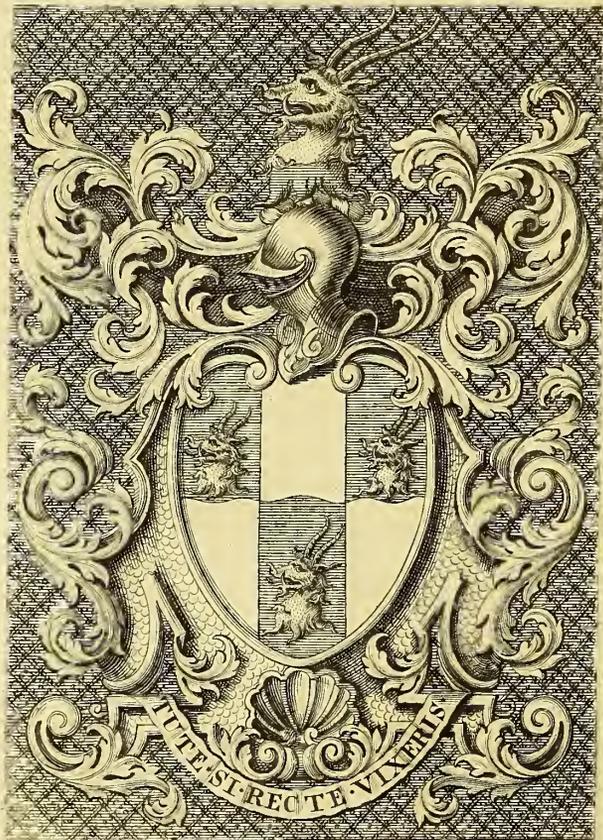


Natural History Museum Library



000163701





PHILOSOPHICAL
TRANSACTIONS.

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. XLI. Part I. For the Years 1739, 1740.

L O N D O N :

Printed for T. WOODWARD, and C. DAVIS, over-
against *Gray's-Inn-Gate* in *Holbourn*; PRINTERS
to the ROYAL SOCIETY. M.DCC.XLIV.

PHILOSOPHICAL

TRANSACTIONS

A CT Q V D



To the HONOURABLE

Sir HANS SLOANE, *Bar^t. M. D.*

Late

PRESIDENT *of the* ROYAL SOCIETY, *and*

ROYAL COLLEGE *of* Physicians, LONDON;

First Physician *to His* MAJESTY;

Member *of the* Imperial Academy *of* Sciences
at ST. PETERSBURGH,

The ROYAL ACADEMIES *of* SCIENCES
at PARIS, MADRID, *and* BERLIN,

And

Fellow *of the* ROYAL COLLEGE *of* Phy-
sicians *at* EDINBURGH, &c.

S I R,

I CANNOT sufficiently extol that eminent
Zeal you have shewn on all Occasions
for the Promotion of *Natural Knowledge*,
the present Perfection of which is greatly in-
debted to your Labours and Patronage. The

A

noble

D E D I C A T I O N.

noble and immense Collection you have made of the most rare and valuable Curiosities, and a Library the most complete in the Universe, in all the Branches of Natural History and Physic, have not only been a Treasure to yourself, but a common Benefit to Mankind; your generous Disposition having constantly led you to communicate their Use to whoever at home or abroad was desirous to improve himself, or to inform the rest of the World.

As your great Knowledge and Skill in your own Profession, brought you into the highest and most extensive Practice for a long and uninterrupted Course of Years, so your Merit and Reputation could not fail of setting you at the Head of those two learned Bodies, you had so much loved and served, the *Royal College of Physicians*, and the *Royal Society of LONDON*, over which you presided with so much Dignity and Honour.

YOUR FAVOURS to the *Royal Society* in particular, have been too many to be here mentioned;

DEDICATION.

tioned; and your Regard for that Body hath ever been conspicuous, by your constant and diligent Application to their Affairs, in the midst of the greatest and most necessary Avocations; yet such has been your Modesty, that you have lately, to our general Regret, resigned the Office of President, which you could not be induced to hold any longer, when you began to apprehend the Infirmities of a venerable Age might oblige you to be less constant in your Attendance on the Duties of it.

GIVE me Leave, Sir, in this Place, also to record my own Thankfulness to you, by whose Recommendation I at first obtained the Honour of being chosen one of the Secretaries of the *Royal Society*, which hath been so many Years continued to me; an Employ, I hope, I have discharged, at least, with Diligence and Fidelity, and shall always endeavour so to do: I beg of you to take under your Protection those Volumes of Papers sent to the *Royal Society*, that have been published under my Inspection, and particu-

DEDICATION.

larly to Patronize this *Forty-first* Volume of the *Philosophical Transactions*, which ends with the last Year of your Presidentship. I remain,

Honoured SIR,

Your much devoted

and obliged

Humble Servant,

Dartmouth-street,
WESTMINSTER,
Dec. 31. 1741.

CROMWELL MORTIMER, *M. D.*
Secretary to the ROYAL SOCIETY, and
Fellow of the ROYAL COLLEGE of
PHYSICIANS LONDON.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *January, February, and
March, 1739.*

The CONTENTS.

- I. *A Catalogue of FIFTY PLANTS from Chelsea Garden, presented to the ROYAL SOCIETY by the Company of Apothecaries, for the Year 1737. pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. & Soc. Reg. Præs. by Isaac Rand, Apothecary, F. R. S. Hort. Chel. Præsfect. ac Prælect. Botan.*
- II. *De Mensura & Motu Aquarum fluentium. Tentamen Primum. Quo agitur de aqua effluente ex vase semper pleno per foramen rotundum, & de resistentia ejusdem ex defectu lubricitatis oriunda. Auctore Jacobo Jurin, Soc. Reg. & Coll. Medic. Londinens. Sodale.*

III. *Dies*

The CONTENTS.

- III. *Dias Antrorum mirabilis Naturæ, glacialis alterius, alterius Halitus noxios eruētantis, ad Regiam Societatem Scientiarum Londinensem, a Matthia Belio Pannonio, R. S. S. missa.*
- IV. *An Account of a very extraordinary Tumour in the Knee of a Person, whose Leg was taken off by Mr. Jer. Peirce, Surgeon at Bath; communicated in a Letter to Dr. Mead, Physician to His MAJESTY, and Fellow of the College of Physicians, and of the Royal Society, London.*
- V. *An Experiment concerning the Spirit of Coals, being part of a Letter to the Hon. Rob. Boyle, Esq; from the late Rev. John Clayton, D. D. communicated by the Right Rev. Father in God Robert Lord Bishop of Corke to the Right Hon. John Earl of Egmont, F. R. S.*
- VI. *An Experiment concerning the nitrous Particles in the Air; by the same Hand.*
- VII. *Extract of a Letter from John Ruty, M. D. to Dr. Mortimer, Sec. R. S. concerning the Poison of Laurel-Water.*



Fig. 1.

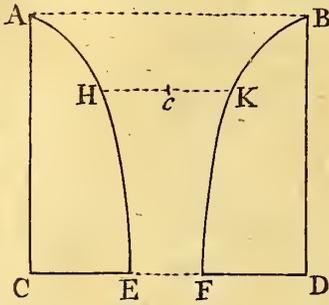


Fig. 2.

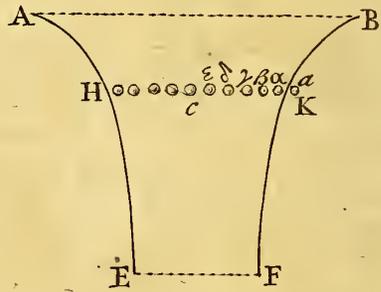


Fig. 3.

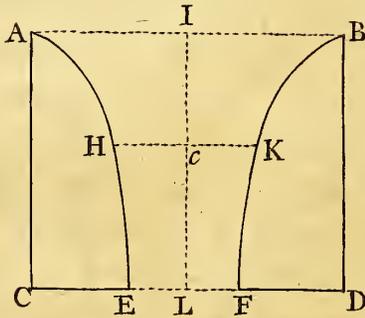




Fig. 1.

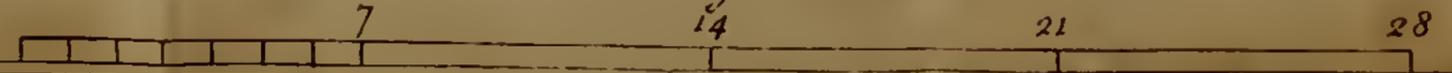


Fig. 2.



A Scale of Inches

T. Ross pinx.



J. Mynde deli et. Sculp.

1. *A Catalogue of the FIFTY PLANTS from Chelsea Garden, presented to the ROYAL SOCIETY by the Company of Apothecaries, for the Year 1737. pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. & Soc. Reg. Præs. by Isaac Rand, Apothecary, F. R. S. Hort. Chel. Præs. ac Prælec. Botan.*

751. **A** CETOSA Ægyptia; roseo seminis involucro; folio lacero; *Lippi. D. Shaw Phyt. Afr. Spec. No. 5.*

752. Adiantum nigrum; radice prælonga, arbores annosas perreptante. *Gris. Virid. Lusitan.*

Filicula Lusitanica; Polypodii radice. T. Inst.

541.

753. Admirabilis Peruviana; flore rubro. *Clus. rar. Plant. Hist. Lib. V. p. 89. Sloan. Cat. Jam. 91.*

754. Androsæmum maximum frutescens. *C. B. 280.*

755. Aralia Canadensis. *T. Inst. 300.*

Panaces κάρπιμον sive racemosa, Canadensis; Cornut. 74.

756. Astrantia major; corona floris candida. *T. Inst. 314.*

757. Astrantia major; corona floris purpurascente. *T. Inst. 314.*

758. Betonica Orientalis; folio angustissimo, & longissimo; spica florum crassiori. *T. Cor. 13.*

A

759. Bi-

759. *Bidens Americana* triphylla; foliis angustis, acutis.

Chrysanthemum Virginianum, folio acutiore lævi trifoliato; seu *Anagyridis folio*. H. Ox. III. p. 21.

760. *Campanula pentagonia*, perfoliata. *Morif.* H. Ox. II. 457.

761. *Cassine vera* perquam similis, *Arbuscula Phillyreæ* foliis antagonisticis. Ex Provincia Caroliniensi. *Pluk. Mantiss.* p. 40. *Phytogr. Tab.* 381. *Fig.* 3.

762. *Cepa fissilis*. *Matthiol.* 555.

763. *Cepa Ascalonia*. *Matthiol.* 556.

764. *Chenopodium Ambrosioides Mexicanum* fruticosum.

765. *Christophoriana Americana* procerior & longius spicata. *Dillen. H. Elt. Tab.* 67.

766. *Commelina angustifolia* procumbens.

767. *Dalea*. *Dni. Millar inventoris*.

768. *Dentaria bulbifera*. *Lob. Icon.* 687.

769. *Digitalis minima*, *Gratiola dicta*. H. Ox. *Part. II.* 479.

Gratiola Centauroides. C. B. 279.

770. *Elichysum*; angustissimo folio. *T. Inst.* 452.

Stæchas citrina tenuifolia altera, sive Italica. J. B. III. 155.

771. *Epimedium*. C. B. 323. *Dod.* 599.

772. *Eruca sylvestris*. *Ger.*

Eruca sylvestris tenuifolia perennis; flore luteo. J. B. II. 861.

773. *Eruca*; *Bellidis folio*. *Mor. H. Ox. P. II.* 231.

774. *Eupatorium cannabinum*. C. B. 320.

Eupatorium Avicennæ *Officin.*

775. *Eu-*

775. Eupatorium; folio oblongo rugoso; caule purpurascente. *T. Inst.* 456.

776. Fagopyrum erectum; femibus ad angulos dentatis, spicatum nascentibus. *Ex Russia.*

777. Ficus Orientalis; foliis in lacinias angustas profunde incis. *s.*

The large white Turkey Fig.

778. Fumaria officinarum & Dioscoridis. *C. B.* 143.

779. Fumaria bulbosa; radice cava, major. *C. B.* 143.

780. Fumaria lutea. *C. B.* 143.

781. Fumaria semper virens & florens; flore albo. *Boerb. Ind.* I. 130.

782. Galeopsis patula segetum; flore purpurascente. *T. Inst.* 185.

Sideritis arvensis rubra. Park.

783. Hernandia; amplo Hederæ folio, umbilicato. *Plum. Nov. Gen.* 6.

Nux vesicaria oleosa, foliis umbilicatis, ex Insula Barbadosi. Pluk. Phyt. 208. Fig. 1.

784. Hypericum Androsæmum dictum. *J. B. III.* 382.

Androsæmum alterum hirsutum. Colum. Ec. 75.

785. Lycopersicon Galeni. *Ang.* 217.

786. Petiveria, Solani foliis; loculis spinosis. *Plum. Nov. Gen.* 50.

Verbena aut Scorodonia affinis anomala, flore albedo; calyce aspero; Allii odore. Sloan. Cat. Jam.

64. Guinea Hen-weed. *Hist.* I. 172.

787. Pyrethrum Officinarum.

Pyrethrum flore Bellidis. *C. B.* 148.

*Chamæmelum specioso flore; radice longa fer-
vida.* D. Shaw. Phyt. Afric. Spec. No. 138.

788. *Pyrethrum frutescens Canariense.*

*Leucanthemum Canariense; foliis Chrysan-
themii; Pyrethri sapore.* T. Inst. 666.

789. *Rapuntium Americanum, flore dilute cœ-
ruleo.* Ac. Reg. Par.

790. *Rapuntium Virginianum; foliis oblongis;
floribus parvis cœruleis, spica longissima laxa.*

791. *Reseda alba.* J. B. III. 467.

792. *Reseda crispa Gallica.* Bocc. Rar. 76.

793. *Solanum fruticosum bacciferum.* C. B. 166.

794. *Thlaspidium fruticosum; Leucoii folio, sem-
per florens.* T. Inst. 214.

*Thlaspi fruticosum Persicum; foliis Keiri, flore
odorato.* Zanon. 196.

795. *Trichomanes sive Polytrichum Officinarum.*
C. B. 356.

796. *Turritis vulgaris ramosa.* T. Inst. 224.

Pilosella siliquata Thalii. Tab. vii.

797. *Turritis Leucoii folio.* T. Inst. 224.

798. *Turritis Leucoii folio; floribus amplioribus.*

799. *Ulmaria.* Clus. Hist. CXCVIII.

Barba Capræ floribus compactis. C. B. 164.

800. *Ulmaria; floribus in longas spicas congestis.*
Boerb. Ind. Alt. 295.

Barba Capræ floribus oblongis. C. B. 163.

II. De Mensura & Motu Aquarum fluentium.

TENTAMEN PRIMUM.

Quo agitur de aqua effluente ex vase semper pleno per foramen rotundum, & de resistentia ejusdem ex defectu lubricitatis oriunda. Auctore Jacobo Jurin, Soc. Reg. & Colleg. Medic. Londinens. Sodale.

A Quarum fluentium *Mensuram* veteres nullam habuerunt, nisi incertam illam & fallacem, quæ, nulla velocitatis habita ratione, sola rivi sectione perpendiculari nitebatur. Ad veram aditum primus aperuit, centum circiter abhinc annis, *Benedictus Castellus, Italus, Galileo* familiaris. Is quum comperisset copiam aquæ per datam rivi sectionem transfluentis, datam non esse, quod veteres crediderant, sed proportionalem celeritati qua fertur aqua per datam sectionem, nobili hoc invento novæ & utilissimæ fundamenta scientiæ jecit, *Hydraulicæ*. Hoc itaque auctore philosophi certatim in eam disciplinam excolendam incubuerunt, ut nemo pene fuerit a *Castelli* temporibus mathematicus paulo insignior; quin aliquid operæ ad ejus incrementum contulerit, sive experimentis instituendis, sive rationibus & argumentis a priori excogitandis.

At plerisque, utut magnis viris, propter summam operis difficultatem, parum feliciter res processit. Nam & theoriam excolentes ea tradiderunt theoremata, quibus factò periculo refragare deprehenditur experientia; & qui experimentis capiendis operam dede-

dederunt, cum animum non adverterent ad circumstantias quasdam minutiores, quod iis quid momenti inesset nondum erat compertum, inde factum est, ut tum singuli magnopere inter se diffideant, tum ab illa *Mensura*, quæ reperiri debuerat, pene omnes insigniter aberrârint.

Cujus rei non aliud luculentius dari potest exemplum, quam simplex illud omniumque facillimum, quod reliquis fere universis pro fundamento esse confuevit, quodque nos idcirco diligentius pertractandum suscepimus, ubi aqua ex vase constanter pleno, constanti velocitate, per foramen circulare in fundo factum decurrit. Hic enim ex omnibus unus *Poelenus* veram tradidit aquæ effluentis *Mensuram*, aut eam saltem, quæ ad veram proxime accedit: unus *Newtonus* verum posuit ejus *Mensuræ* indagandæ fundamentum; verum, at a plerisque repudiatum; a quibusdam, dissimulato auctoris nomine, pro suo venditatum.

His itaque duobus ducibus rem aggredimur, & primo quidem loco, phænomenon nomine proponemus ea, quæ aut ipsis experimentis comparent, aut ex iisdem, certissimis argumentis confirmantur: deinde ad eorum phænomenon solutionem accedemus.

Phænomena effluxus aquæ ex foramine in fundo vasis constanter pleni.

1. Data altitudine aquæ & tempore effluendi, *Mensura* aquæ effluentis est fere in ratione foraminis.

2. Data altitudine aquæ & foramine, *Mensura* aquæ effluentis est in ratione temporis effluendi.

3. Dato

3. Dato tempore effluendi & foramine, *Mensura* aquæ effluentis est fere in ratione subduplicata altitudinis aquæ.

4. *Mensura* aquæ effluentis est fere in ratione composita ex ratione foraminis, ratione temporis, & ratione subduplicata altitudinis aquæ.

5. *Mensura* aquæ dato tempore effluentis longe minor est ea, quæ ex Mathematicorum theorematis vulgo elicitur. Ea nempe vulgo habetur aquæ effluentis velocitas, quam acquirat in vacuo corpus grave cadendo ex integra altitudine aquæ supra foramen; & hoc posito, si area foraminis vocetur F , A altitudo aquæ supra foramen, V velocitas quam comparat corpus grave cadendo in vacuo ex ista altitudine, T tempus cadendi, & effluat aqua constanti hac velocitate V , per tempus T , erit $2 A$ longitudo columnæ aqueæ, quæ eo tempore effluit; eritque ejus *Mensura* $2 AF$. At si accuratissima * *Poleni* experimenta ad calculum revoces, copiam aquæ, quæ eo tempore effluit, non nisi $\frac{571}{1000}$ circiter hujus *Mensuræ* $2 AF$ conficere perspicies.

Hujus autem viri illustrissimi experimenta, cum propter summam ejus diligentiam, & accurationis studium, tum alio etiam nomine, reliquorum omnibus omnium præferenda censeo. Is siquidem deprehendit copiam aquæ effluentis ex vase per tubum cylindricum, eam quæ exiret per foramen circulare in tenui lamina factum, pari existente diametro tubi & foraminis, & pari altitudine aquæ ambobus incum-

* Polenus de Castellis, *Art.* 35, 38, 39, 42, 43.

bentis, longe superare. Idque ita se habere cognovit, cum tubus non fundo quidem, quod alii prius animadverterant, sed lateri vasis infereretur.

Est autem foramen vel in tenui lamina factum, pro brevi tubo cylindrico habendum. Unde patet majorem aquæ copiam ex foramine in lamina tenui facto profluere, quam quæ effluxura fuisset, si, quod aiunt, infinite parva fuisset laminæ crassities. Cujusmodi lamina cum neque haberi, nec etiam cogitatione concipi queat, relinquitur ut augeamus diametrum foraminis, quo laminæ crassities, quam fieri commode potest, minimam rationem obtineat ad foraminis diametrum.

Id vero magno cum judicio præstitit *Polenus*, cum uteretur diametro linearum 26, lamina autem non integram lineam crassa; cum ante eum vix quisquam adhibuerit diametrum 6 aut 7 lineas superantem; aut omnino animum adverterit ad laminæ vel fundi vasis crassitiem, nisi quod unus *Newtonus*, pro summa sua providentia, sese lamina pertenui usum fuisse scribat.

Nec foraminum solum, sed vasorum etiam amplitudini *Polenus* supra omnes prospexit, quo aqua liberrime & quam minimo cum impedimento versus foramen descenderet; ut nullus dubitandi locus sit, quin *Mensuræ* ab eo captæ propius longe quam ullæ a reliquis traditæ ad verum accedant.

6. Cum, ut modo vidimus, *Mensura* aquæ effluentis prædicto tempore T , sit $2 AF \times \frac{571}{1000}$, est longitudo columnæ aquæ, quæ eo tempore effluit, $2 A \times \frac{571}{1000}$. Itaque, si particulæ aquæ, quæ eodem temporis

poris puncto in foramine versantur, singulæ pari velocitate profiliant, liquet communem omnium velocitatem eam esse, qua percurratur tempore T spatium $2A \times \frac{571}{1000}$, sive velocitatem $V \times \frac{571}{1000}$. Hæc autem

ea est, quæcum aqua in vacuo profilire possit ad tertiam fere partem altitudinis aquæ supra foramen.

7. Atqui, cum sursum vertitur aquæ motus, ut in fontibus salientibus, profilire cernuntur fontes ad altitudinem aquæ in cisterna pene integram. Profilit ergo ex foramine aqua, aut aliqua saltem aquæ portio, cum velocitate V pene integra, certe velocitate multo majori quam $V \times \frac{571}{1000}$.

8. Hinc certissime liquet particulas aqueas, quæ eodem temporis puncto in foramine versantur, non omnes erumpere cum eadem velocitate, sive nullam esse velocitatem omnibus communem. Contrarium hæcenus pro indubitato habuerunt Mathematici.

9. Ad parvam a foramine distantiam, venæ aqueæ erumpentis diameter multo minor est diametro foraminis. Nempe, si foraminis diameter sit 1, erit venæ aqueæ diameter $\frac{21}{25}$, sive 0,84 mensurante *Newtono*, qui mirabile hoc phænomenon primus animadvertit; ex mensuris *Poleno* captis erit $\frac{20}{26}$, vel $\frac{20,5}{26}$; hoc est, si diametrum intermediam ceperis, 0,78 fere.

His expositis, progrediendum est deinceps ad solutionem horum phænomenon expediendam: id vero antequam fiat, ex usu erit lectorem pauca præmonere.

1. Aquam nos non aliter consideramus, quam ut corpus fluidum, continuum, cujus partes vi minimæ illatæ cedunt, & cedendo moventur inter se.

2. Per aquam effluentem intelligimus eam aquæ copiam, quæ actu ex foramine egreditur: Quod, etsi minus necessarium videri possit, monendum tamen idcirco duximus, quod in *Dissertatione* nostra de *Motu aquarum fluentium* ante annos circiter 24 *Actis Philosophicis* inserta, aquæ defluentis nomine designata fuerit tota illa aquæ copia, quæ intra vas in motu constituta est, & versus foramen descendit.

3. Vasis amplitudinem pro infinita habemus, aut tanta saltem, ut in eo decrementum altitudinis aquæ toto temporis spatio, quo aqua ex foramine effluit, sensu percipi nequeat.

4. Aquam consideramus ut effluentem constanti velocitate. Nimirum ipso motus initio per minimum temporis spatium effluit aqua minori velocitate, quam mox elapsura sit. Nos autem ipsum motus initium præterimus, & tum demum investigamus aquæ *Mensuram & Motum*, cum integram velocitatem, quanta fieri potest, comparaverit. Hæc autem constans sit, necesse est, dum constet aquæ superincumbentis altitudo.

5. Fundum vasis non aliter concipimus quam ut planum mathematicum, vel laminam saltem eatenus tenuem, ut ejus crassities quasi nulla sit respectu diametri foraminis.

6. Per *Mensuram aquæ effluentis* in sequentibus semper intelligimus eam aquæ copiam, quæ ex foramine erumpit illo temporis spatio, quo corpus grave in vacuo cadens percursurum sit altitudinem aquæ supra foramen.

7. Per

7. Per *Motum aquæ effluentis* intelligimus summam motuum omnium aquæ particularum, quæ supradicto temporis spatio ex foramine erumpunt. Motus vero cujusque particulæ est, ut factum ex ipsa particula & velocitate quacum ex foramine erumpit.

8. Quo facilius animo concipiantur sequentia, casus simpliciores primo proponemus, deinde ad magis compositos, sed propius ad verum rerum statum accedentes, progrediemur.

Nempe in problemate primo, quo simplicior evadat solutio, ponimus aquam ex foramine in vacuum effluere, aqueasque particulas, dum versus foramen descendunt, omni carere resistantia ex defectu lubricitatis oriunda.

In secundo & tertio problemate ponitur adhuc effluxus aquæ in vacuo institui; sed concipimus particulas aqueas, dum versus foramen descendunt, nonnullam ex defectu lubricitatis experiri resistantiam, tantulam tamen, ut decrementum *Motus* aquæ ex foramine effluentis, exinde ortum, pro nihilo haberi possit.

In quarto & quinto, vacui positionem adhuc retinemus; at sensibile ponitur decrementum *Motus* aquæ effluentis, ex defectu lubricitatis.

Tandem in problemate sexto & sequentibus rem consideramus prout revera se habet, cum in aëre res transigitur, adeo ut particulæ aqueæ resistantiam sensibilem patiantur, non modo a sese invicem per defectum lubricitatis, intra vas, sed etiam post exitum e vase, per attritum aëris ambientis.

P R O B L E M A I.

Definire Motum, Mensuram, & velocitatem aquæ in vacuum effluentis per foramen in fundo vasis, ubi particule aquæ nullam patiuntur resistantiam ex defectu lubricitatis.

Dum foramen obturaculo occluditur, sustinet obturaculum pondus columnæ aquæ ipsi ad perpendicularum incumbentis. Remoto obturaculo, columna aquæ foramini ad perpendicularum imminens, cum non amplius sustineatur, pressione sua efficiet, ut aqua per foramen defluat, & postquam eam ad debitam velocitatem compulerit, deinceps constanti sua pressione constantem aquæ effluentis velocitatem conservabit.

Concipiendum est quidem, *Motum* aquæ ex foramine effluentis non a pondere solius columnæ perpendicularis ortum ducere, sed partim ex hujus columnæ pressione, partim ex pressione aquæ circumpositæ derivari. Sed hoc pacto neque major neque minor fit *Motus* aquæ effluentis, quam si ex pressione solius columnæ perpendicularis oriretur: Non minor, quia pressio columnæ perpendicularis, si non impediatur, *Motum* sibi proportionalem generabit, impedi autem non potest nisi quatenus aqua circumposita urget aquam effluentem: non major, quia pressio aquæ circumpositæ non potest aliquid conferre ad *Motum* aquæ effluentis, nisi tantundem demat ex pressione columnæ perpendicularis.

Causa igitur adæquata *Motus* aquæ ex foramine effluentis, est pressio sive pondus columnæ aquæ, quæ foramini insistit. At vis data, quocunque modo

applicetur, dato tempore datam generat *Motus* quantitatem versus easdem partes, quo tendit vis. Parem itaque *Motus* quantitatem dato tempore generat columnæ incumbentis pondus in aqua effluente, atque generare posset eodem tempore in ipsa columna libere per vacuum cadente.

Jam quoniam, per hypothefin, particulæ aqueæ nullam experiuntur resistentiam ex defectu lubricitatis, & omnes illæ particulæ, quæ jamjam exituræ in ipso foramine versantur, æquali urgentur pressione aquæ superincumbentis, liquet harum omnium æqualem esse velocitatem.

Sit v communis ista velocitas; a altitudo unde cadendo in vacuo comparetur ea velocitas; A altitudo aquæ supra foramen; V velocitas quæ comparetur cadendo in vacuo ex altitudine A ; T tempus cadendi ex eadem altitudine; F area foraminis; & effluat aqua ex foramine per tempus T .

Jam quoniam tempore T velocitate V percurratur spatium $2A$, percurreretur eodem tempore velocitate v spatium $\frac{2Av}{V}$. Hæc itaque erit longitudo columnæ

aqueæ, quæ effluit ex foramine tempore T ; eritque magnitudo hujus columnæ, sive *Mensura* aquæ effluentis tempore T , $\frac{2AvF}{V}$, & *Motus* ejusdem erit

$$\frac{2AFv^2}{V}$$

Motus autem, qui eodem tempore T , in columna aquea foramini insistente generari possit, si suo ipsius pondere per vacuum feratur, sic habetur.

Erit

Erit ejus velocitas V , & cum magnitudo ejusdem sit AF , erit ejus *Motus* AFV .

Atqui *Motus* iste, ex suprapositis, æqualis est *Motui* columnæ aqueæ effluentis tempore T , sive $AFV = \frac{2AFv^2}{V}$.

$$\text{Hinc autem } V = \frac{2v^2}{V}, \text{ sive } v^2 = \frac{V^2}{2}, \text{ \& } v = \frac{V}{\sqrt{2}}.$$

Porro *Mensura* supraposita aquæ effluentis tempore T , sive $\frac{2AFv}{V} = \frac{2AF}{V} \times \frac{V}{\sqrt{2}} = \frac{2AF}{\sqrt{2}} = AF \times \sqrt{2}$. Q. E. I.

COROLL. I. Cum sit $a : A :: v^2 : V^2$, erit $a = \frac{Av^2}{V^2}$, hoc est, $a = \frac{A}{V^2} \times \frac{V^2}{2}$, sive $a = \frac{A}{2}$. Ita-

que altitudo a , quam effluens aqua motu sursum verso attingere queat, dimidia est altitudo aquæ in vase supra foramen. Quæ est ipsa altitudo *Newtono* definita *Prop. 36. Lib. II. Princip.* Editionis primæ.

COROLL. II. Si tribuatur aquæ effluentis ea velocitas, quæ comparatur cadendo ex integra altitudine aquæ supra foramen, hoc est, si ponatur velocitas $v = V$, erit *Motus* aquæ supra definitus $\frac{2AFv^2}{V} =$

$2AFV$, sive du plus ejus *Motus*, qui a columna foramini incumbente generari possit, & proinde non nisi a duplo hujus columnæ generandus; quod docuit *Newtonus Corollario secundo, Prop. 36. Lib. II. Princip. Edit. 2 & 3.*

S C H O L I U M.

Mensura hic determinata, $\frac{2 AF}{\sqrt{2}}$, sive $2 AF \times$

0,707, ut longe deficit ab ea, quæ vulgo Mathematicis statuitur, nempe $2 AF$, ita longe superat illam *Mensuram*, quam exhibent *Poleni* experimenta, sive $2 AF \times 0,571$. Nec mirum: quod enim ponitur in hoc problemate, carere omni resistantia particulas aqueas inter defluendum, hypothesis est a vero rerum statu aliena.

P R O B L E M A II.

Definire Motum, Mensuram & velocitatem aquæ in vacuum effluentis per foramen circulare in medio fundo vasis cylindrici, ubi particule aquæ resistantiam patiuntur ex defectu lubricitatis, sed tam parvam, ut decrementum Motus aquæ effluentis exinde ortum pro nihilo haberi possit.

Sit vas cylindricum immensum $ABCD$, Fig. 1. EF foramen circulare in medio fundo factum, & aqua in hoc vase quiescente prorsus & immota, detrahatur obturaculum a foramine, ut pateat exitus aquæ per foramen.

Tum quoniam aqua hætenus immota fuerit, & jam per foramen effluere incipit, & effluentem sequitur aqua supraposita, & motus naturalis aquæ nulla de super affusione perturbatur, & foramen obtinet ipsum fundi medium, induet sese necessario illa aquæ portio, quæ in motu versatur, & versus foramen descendit,
in

in figuram aliquam regularem $AHEFKB$, cujus basis inferior sit ipsum foramen, basis autem superior sit superficies aquæ suprema AB , & sectiones omnes horizontales sint circulares. Hanc vocamus *Cataractam*; qualis autem sit *Cataractæ* figura, nondum disputamus: in præfenti sufficit nostro instituto, ut animadvertamus regularem esse, & per singulas ejus sectiones horizontales eandem aquæ copiam dato tempore transire.

Jam quoniam omnis illa aqua, quæ deorsum fertur, *Cataracta* continetur, patet reliquam aquam $AHEC, BKFD$, quæ extra *Cataractam* sita est, omni motu carere, & penitus quiescere. Itaque in sectione quavis horizontali *Cataractæ* HcK , cujus centrum c , puncta H, K repræsentabunt limites inter aquam descendente[m] versus foramen, & aquam circumpositam quiescentem.

Porro, cum punctum K sit limes motus & quietis, & particule aqueæ, dum moventur inter se, resistantiam patiantur ex defectu lubricitatis, particula aquæ α , Fig. 2. intra *Cataractam* sita, & adjacens puncto K , non poterit nisi quam minima velocitate deorsum ferri. Alioqui, necessario secum abriperet particulam proximam a extra *Cataractam* positam, contra hypothese[m]. Particula autem β , quæ particule α introrsum contigua est, nonnisi quam minima velocitate relativa descendet respectu particule α ; cum alioqui particulam α accelerando eam secum abriperet, & hæc particula α , jam celerius mota, abriperet secum particulam a . Pariter particula γ magis introrsum posita, & particule β contigua, descendet quam minima velocitate relativa respectu particule β ; & reliquæ particule $\delta, \epsilon, \&c.$ aliæ aliis magis introrsum sitæ, descendunt

dent velocitate quam minima relativa respectu particularum singulis extrorsum adjacentium. Hac autem ratione velocitas absoluta particularum crescat necesse est gradatim a limite versus centrum c , ut velocitas aquæ sit maxima in ipso centro, minima in limite utroque K & H .

Necesse vero est, ut resistentia, quam experitur particula quæque celerior ex affricu adjacentis particulæ tardioris extrorsum positæ, perpetuo sibi æqualis sit per totam sectionem *Cataractæ*. Alioqui, particula illa, quæ majorem patitur resistentiam, accelerabit particulam tardiozem adjacentem, donec minuatur hoc pacto resistentia, & fiat æqualis illi resistentiæ, quam patiuntur cæteræ particulæ. At si resistentia sit ubique sibi æqualis per totam *Cataractæ* sectionem, erit & velocitas relativa particularum ubique æqualis, cum altera alteram necessario consequatur.

Ergo velocitas absoluta cujuslibet particulæ, quæ est summa velocitatum omnium relativarum ab ambitu sectionis ad eam usque particulam simul sumptarum, est in ratione distantia ejusdem particulæ ab ambitu *Cataractæ*.

His expositis, sit modo r radius foraminis, m ad r in ratione peripheriæ ad diametrum, mr^2 area foraminis, v velocitas quacum aqua descendit in centro foraminis, a altitudo unde cadendo in vacuo comparetur velocitas v , A altitudo aquæ supra foramen, V velocitas quæ comparetur cadendo in vacuo ex altitudine A , T tempus cadendi ex eadem, z distantia cujuslibet particulæ a centro foraminis, & effluat aqua tempore T .

Jam *Mensura* aquæ, quæ tempore T ex foramine egreditur, ad hunc modum invenietur.

C

Erit

Erit z radius circuli cujuslibet intra foramen, $2mz$ circumferentia ejusdem, $2mzz$ annulus nascens ei circumferentiæ adjacens, $v \times \frac{r-z}{r}$ velocitas aquæ in annulo nascente.

Cumque sit $V : v \times \frac{r-z}{r} :: 2A : \frac{2Av \times r-z}{Vr}$,
erit $\frac{2Av \times r-z}{Vr}$ spatium, quod conficit aqua per
annulum nascentem fluens tempore T , & *Mensura*
ejusdem aquæ erit $2mzz \times \frac{2Av \times r-z}{Vr} =$
 $\frac{4mAv \times rzz - z^2z}{Vr}$.

At *Mensura* aquæ per annulum nascentem trans-
euntis est fluxio *Mensuræ* aquæ transeuntis per cir-
culum, cui radius z . Est itaque *Mensura* aquæ, quæ
tempore T transit per hunc circulum, quantitas fluens

fluxionis modo expositæ $\frac{4mAv}{Vr} \times rzz - z^2z$, i. e.
 $\frac{4mAv}{Vr} \times \frac{3rzz - 2z^3}{6} = \frac{2mAv}{3Vr} \times 3rzz - 2z^3$.

Et ponendo $z = r$, habebitur *Mensura* aquæ per totum
foramen transeuntis tempore T , nempe $\frac{2mAvr^2}{3V}$.

Motus vero aquæ ejusdem sic habebitur.

Mensura

Mensura aquæ tempore T effluentis per annulum nascentem est, ut modo perspeximus, $\frac{4 m A v}{V r} \times$

$r z \dot{z} - z^2 \dot{z}$, & cum velocitas ejusdem sit $v \times \frac{r-z}{r}$,

erit ejus *Motus* $\frac{4 m A v}{V r} \times \overline{r z \dot{z} - 2 z^2 \dot{z}} \times \frac{v}{r} \times \overline{r - z}$

$= \frac{4 m A v^2}{V r^2} \times \overline{r^2 z \dot{z} - 2 r z^2 \dot{z} \times z^3 \dot{z}}$, cujus quantitas

fluens est $\frac{4 m A v^2}{V r^2} \times \frac{r^2 z^2}{2} - \frac{2 r z^3}{3} + \frac{z^4}{4} = \frac{m A v^2}{3 V r^2} \times$

$6 r^2 z^2 - 8 r z^3 + 3 z^4$, qui est *Motus* aquæ transcuntis per circulum cui radius z . Et posita $z = r$, habetur *Motus* aquæ effluentis tempore T per totum foramen, $\frac{m A v^2 r^2}{3 V}$.

Hic autem *Motus*, per solutionem *Problematis* primi, & hypothesin hujus, æqualis est *Motui*, quem columna foramini insistens comparare possit eodem tempore T , suo ipsius pondere per vacuum cadendo, hoc est, *Motui* AFV , sive $AV \times m r^2$. Itaque $\frac{m A v^2 r^2}{3 V} = m A V r^2$.

Hinc autem $v^2 = 3 V^2$, & $v = V \times \sqrt{3}$.

Porro *Mensura* supraposita aquæ effluentis per foramen tempore T , nempe $\frac{2 m A v r^2}{3 V} = \frac{2 m A r^2}{3 V}$

$\times V \times \sqrt{3} = \frac{2 A m r^2}{\sqrt{3}}$. Q. E. I.

COROLL. I. Cum sit $V^2 : v^2 :: A : a$, erit $a = \frac{Av^2}{V^2} = \frac{A}{V^2} \times 3V^2 = 3A$. Itaque altitudo, ad quam aqua in vacuo profilire possit ea velocitate, quacum effluit in centro foraminis, tripla est altitudinis aquæ supra foramen.

COROLL. II. *Cataractæ* figura ad hunc modum definietur:

Sit HK , Fig. 3. quælibet sectio *Cataractæ*, cujus centrum c , sitque ejus radius $cK = y$, altitudo aquæ supra istam sectionem, sive $Ic = x$, t tempus cadendi in vacuo ex altitudine x , sitque, ut prius, $LF = r$, & $IL = A$.

Jam transit aqua per hanc sectionem HK eadem copia atque effluit ex foramine EF .

Quod si vas eo usque decurtetur, ut ejus altitudo redigatur ex IL ad Ic , adeoque sectio ista HK jam fiat ipsum foramen in fundo vasis, transibit aqua dato tempore, per hanc sectionem, copia neque majori, neque minori, atque prius transierat per eandem, vase nondum decurtato: non majori, quia non urgetur ista sectio nisi eodem columnæ superincumbentis pondere, quo prius urgebatur; non minori, quia aqua inferior $HKFE$ non obstat motui aquæ per sectionem HK transituræ.

Vase autem decurtato, *Mensura* aquæ effluentis ex foramine HK tempore t , per solutionem præcedentem, est $\frac{2xy^2}{\sqrt{3}}$, & *Mensura* aquæ effluentis tempore

$$T \text{ est } \frac{2xy^2}{\sqrt{3}} \times \frac{T}{t} = \frac{2xy^2}{\sqrt{3}} \times \frac{\sqrt{A}}{\sqrt{x}}. \text{ Nam } T : t :: \sqrt{A} : \sqrt{x}.$$

Sed,

Sed, ex supradictis, *Mensura* aquæ tempore dato T effluentis ex foramine HK vase decurtato, æqualis est *Mensuræ* aquæ eodem tempore transeuntis per sectionem HK vase integro, sive *Mensuræ* aquæ eodem tempore effluentis ex foramine EF . Itaque $\frac{2xy^2}{\sqrt{3}} \times \frac{\sqrt{A}}{\sqrt{x}} = \frac{2Amr^2}{\sqrt{3}}$, sive $y^2\sqrt{x} = r^2\sqrt{A}$, vel $y^4x = r^4A$, quæ est ipsa æquatio curvæ hyperbolicæ, cujus rotatione figuram *Cataractæ* gigni olim ostendimus in *Actis Philosophicis* Numero 355.

SCHOLIUM I.

Mensura aquæ supra inventa $\frac{2Amr^2}{\sqrt{3}}$, sive $2Amr^2 \times 0,577350$ tantillo major est *Mensura* $2Amr^2 \times 0,571$, quæ ex Cl. *Poleni* experimentis elicitur. Hoc autem differentiæ, aliqua saltèm ex parte, inde provenit, quod in hoc problemate decrementum *Motus* aquæ ex resistentia ortum pro nihilo habuimus.

SCHOLIUM II.

Recte se habet *Mensura* aquæ effluentis hac solutione definita, si altitudinem vasis pro infinite magna habeamus respectu diametri foraminis. Cum vero hæc altitudo finitam rationem obtinet ad diametrum foraminis, paulo minor erit *Mensura*, ita tamen, ut cum altitudo quinquies major sit diametro, non nisi parte $\frac{1}{32000}$, & cum dupla sit diametri, non nisi parte $\frac{1}{5120}$ circiter, a vero aberret, quæ differentiæ minores

nores sunt quam ut ullo experimento deprehendi queant.

Tantillum autem hoc discrimen exinde proficiscitur, quod velocitas supradicta relativa, & proinde ipsa velocitas absoluta particularum aquæ, quas consideravimus ut in directione ad horizontem perpendiculari, revera obtinent directionem paululum obliquam, cum propius ad axem *Cataractæ* accedat quæque particula inter descendendum.

Quod si aliquis desiderio teneatur solutionem veram & accuratam consequendi, cum altitudo aquæ quamcunque rationem obtinet ad diametrum foraminis, eam hunc in modum consequi poterit.

Ex curvæ *Cataracticæ* proprietate corollario secundo hujus problematis exposita, qua $y^4 x = r^4 A$, subtangens hujus curvæ ad ambitum foraminis invenietur $4A$, & ad ambitum cujuslibet sectionis subtangens erit $4x$, æqualis scilicet altitudini aquæ supra illam sectionem quater sumptæ.

Curvam vero ejusmodi *Cataracticam* describit non modo aqua exterior, quæ foraminis ambitum præterfluit, sed etiam illa pars aquæ, quæ per quemlibet foraminis annulum effluit; i. e. unaquæque particula aquea curvam ejusmodi describit.

Sit modo z distantia cujusvis particulæ in foramine positi, a centro foraminis, & descendat hæc particula per spatium quam minimum in tangente ad curvam *Cataracticam*. Hinc erit ejus velocitas in directione hujus tangentis, sive velocitas $v \times \frac{r-z}{r}$,

quæ in hoc problemate exposita est, ad velocitatem ejusdem in directione ad horizontem perpendiculari, ut $\sqrt{16A^2 + z^2} : 4A$.

Est

Est itaque velocitas in directione ad horizontem

perpendiculari, $v \times r - z \times \frac{4A}{\sqrt{16A^2 + z^2}}$.

Hinc autem, insistendo vestigiis solutionis superioris, habebis pro *Mensura* aquæ per annulum nascentem transeuntis, $\frac{16m A^2 v}{rV} \times \frac{r z z - z^2 z}{\sqrt{16A^2 + z^2}}$.

Hujus vero fluxionis quantitas fluens, per *Mensuras* rationum *Cotesianas*, Form. V. & VI. inveniatur $\frac{16m A^2 v}{rV} \times \frac{2r - z}{2} \sqrt{16A^2 + z^2} + 8A^2 \left| \frac{z + \sqrt{16A^2 + z^2}}{4A} \right|$;

& ponendo primum $z = 0$, deinde $z = r$, habebis $\frac{16m A^2 v}{rV} \times \frac{r}{2} \sqrt{16A^2 + r^2} - 4Ar + 8A^2 \left| \frac{r + \sqrt{16A^2 + r^2}}{4A} \right|$ pro *Mensura* aquæ per totum foramen transeuntis tempore T .

Porro, similem in modum procedendo, habebis pro *Motu* aquæ per annulum nascentem transeuntis,

$\frac{64m A^3 v^2}{r^2 V} \times \frac{r^2 z z - 2r z^2 z + z^3 z}{16A^2 + z^2}$, cujus fluxionis quantitas fluens, per Formam I & II. *Cotesianam*, reperiatur $\frac{64m A^3 v^2}{r^2 V}$ in $\frac{z^2 - 4r z}{2} + \frac{r^2}{2} \left| \frac{16A^2 + z^2}{16A^2} - \frac{16A^2}{2} \left| \frac{16A^2 + z^2}{16A^2} + 2r\sqrt{-16A^2} \right| \frac{z + \sqrt{-16A^2}}{\sqrt{16A^2 + z^2}} \right|$.

& ponendo $z = r$, habebis $\frac{64m A^3 v^2}{r^2 V}$ in $\frac{r^2 - 16A^2}{2} \left| \frac{16A^2 + r^2}{16A^2} + 2r\sqrt{-16A^2} \right| \frac{r + \sqrt{-16A^2}}{\sqrt{16A^2 + r^2}} - \frac{3r^2}{2}$,

qui

qui est *Motus* aquæ transeuntis per foramen tempore *T*.

$$\text{Sit jam } M = \frac{r}{2} \sqrt{16 A^2 + r^2},$$

$$N = 8 A^2 \left| \frac{r + \sqrt{16 A^2 + r^2}}{4 A} \right., \text{ vel}$$

$$N = 4 A^2 \left| \frac{16 A^2 + 2 r^2 + 2 r \sqrt{16 A^2 + r^2}}{16 A^2} \right.,$$

$$K = \frac{r^2 - 16 A^2}{2} \left| \frac{16 A^2 + r^2}{16 A^2} \right., \&$$

$$L = 2 r \sqrt{-16 A^2} \left| \frac{r + \sqrt{-16 A^2}}{\sqrt{16 A^2 + r^2}} \right., \text{ vel}$$

$L = 2 r \times 4 A$ (Rad : Tang : Sec :: $4 A : r : \sqrt{16 A^2 + r^2}$), & *Mensura* aquæ per foramen transeuntis tempore *T*, erit $\frac{16 m A^2 v}{r V} \times \frac{M + N - 4 A r}{r}$;

Motus vero ejusdem aquæ erit $\frac{64 m A^3 v^2}{r^2 V} \times \frac{L + K - 3 r^2}{2}$.

Sed $\frac{64 m A^3 v^2}{r^2 V} \times \frac{L + K - 3 r^2}{2} = m r^2 A V$, unde

$$v^2 = \frac{r^4 V^2}{64 A^2 \times \frac{L + K - 3 r^2}{2}} \quad \& \quad \text{Mensura aquæ per}$$

foramen effluentis tempore *T*, est $2 m A r \times \frac{M + N - 4 A r}{\sqrt{L + K - 3 r^2}}$.

Sin autem pro Mensuris rationum & angulorum adhibere malis series infinitas, erit supraposita *Mensura* aquæ per anulum nascentem effluentis, $\frac{16 m A^2 v}{r V}$

$\times \frac{r z \dot{z} - z^2 \dot{z}}{\sqrt{16 A^2 + z^2}}$, ad hanc formam reducenda, $\frac{m v}{r V}$

$\times r z \dot{z} - z^2 \dot{z} \times \frac{16 A^2}{\sqrt{16 A^2 + z^2}}$; & reducendo

$\frac{16 A^2}{\sqrt{16 A^2 + z^2}}$ ad seriem infinitam, habebis $\frac{m v}{r V}$

$\times r z \dot{z} - z^2 \dot{z}$ in $4 A - \frac{z^2}{8 A} + \frac{3 z^4}{8^3 A^3} - \frac{5 z^6}{4 \times 8^4 A^5} +$
 $\frac{35 z^8}{8^7 A^7},$ &c. pro *Mensura* aquæ per anulum nascentem

effluentis; & per hujus fluxionis quantitatem fluentem, sive per $\frac{m v}{V}$ in $\frac{2 A r^2}{3} - \frac{r^4}{20 \times 8 A} + \frac{r^6}{14 \times 8^3 A^3}$

$- \frac{5 r^8}{36 \times 8^5 A^5} + \frac{7 r^{10}}{22 \times 8^7 A^7},$ &c. exponetur *Mensura* aquæ effluentis per foramen integrum.

Porro *Motus* suprapositus aquæ per anulum nascentem transeuntis, $\frac{64 m A^3 v^2}{r^2 V} \times \frac{r^2 z \dot{z} - 2 r z^2 \dot{z} + z^3 \dot{z}}{16 A^2 + z^2}$

$= \frac{4 m A v^2}{r^2 V} \times r^2 z \dot{z} - 2 r z^2 \dot{z} + z^3 \dot{z} \times \frac{16 A^2}{16 A^2 + z^2}$

$= \frac{4 m A v^2}{r^2 V} \times r^2 z \dot{z} - 2 r z^2 \dot{z} + z^3 \dot{z}$ in $1 - \frac{z^2}{16 A^2}$

$+ \frac{z^4}{16^2 A^4} - \frac{z^6}{16^3 A^6} + \frac{z^8}{16^4 A^8} - \frac{z^{10}}{16^5 A^{10}},$ &c.

D

Et

Et per fluxionis hujus quantitatem fluentem, five per

$$\frac{4mAv^2}{V} \text{ in } \frac{r^2}{12} - \frac{r^4}{60 \times 16 A^2} + \frac{r^6}{168 \times 16^2 A^4} - \frac{r^8}{360 \times 16^3 A^6} + \frac{r^{10}}{660 \times 16^4 A^8} - \&c. \text{ exponetur Motus}$$

aquæ per foramen integrum effluentis.

$$\text{Ergo } Amr^2V = \frac{4mAv^2}{V} \text{ in } \frac{r^2}{12} - \frac{r^4}{60 \times 16 A^2} + \&c.$$

$$\text{five } V^2 = v^2 \text{ in } \frac{1}{3} - \frac{r^2}{15 \times 16 A^2} + \&c. \text{ vel}$$

$$v^2 = \frac{V^2}{\frac{1}{3} - \frac{r^2}{15 \times 16 A^2} + \&c.}$$

$$\& v = \sqrt{\frac{1}{3} - \frac{r^2}{15 \times 16 A^2} + \&c.}$$

Unde *Mensura* aquæ effluentis per foramen, five

$$\frac{mv}{V} \text{ in } \frac{2Ar^2}{3} - \frac{r^4}{20 \times 8 A} + \frac{r^6}{14 \times 8^3 A^3} - \frac{5r^8}{36 \times 8^5 A^5} + \&c$$

$$= \frac{m}{V} \text{ in } \frac{2Ar^2}{3} - \frac{r^4}{20 \times 8 A} + \frac{r^6}{14 \times 8^3 A^3} - \frac{5r^8}{36 \times 8^5 A^5} + \&c$$

$$\times \frac{1}{\sqrt{\frac{1}{3} - \frac{r^2}{15 \times 16 A^2} + \&c.}}$$

$$= m \text{ in } \frac{2Ar^2}{3} - \frac{r^4}{20 \times 8 A} + \&c.$$

$$\sqrt{\frac{1}{3} - \frac{r^2}{15 \times 16 A^2} + \&c.}$$

Unde tandem *Mensura* aquæ effluentis per foramen habetur

$$\frac{2Amr^2}{\sqrt{3}} \text{ in } 1 - \frac{r^2}{20 \times 16 A^2} + \frac{r^4}{56 \times 16^2 A^4} - \&c$$

Hinc

Hinc ponendo A infinitam respectu diametri foraminis, evadit $Mensura = \frac{2 Amr^2}{\sqrt{3}}$, ut in *Problemate* hoc determinavimus.

Cum $A = 10r$, $Mensura = \frac{2 Amr^2}{\sqrt{3}} \times 1 - \frac{1}{32000}$
circiter.

Cum $A = 4r$, $Mensura = \frac{2 Amr^2}{\sqrt{3}} \times 1 - \frac{1}{5120}$ cir-
citer.

Potest itaque loco veræ *Mensuræ* adhiberi $Mensura \frac{2 Amr^2}{\sqrt{3}}$, sine periculo sensibilis erroris, etiam in tantula altitudine, multo magis in altitudine multis vicibus majori, qualis fere in experimentis adhiberi consuevit; & hoc pacto computus ex operoso admodum & intricato facillimus evadit.

PROBLEMA III.

Isdem positis, & negligendo accelerationem aquæ extra foramen, determinare diametrum venæ aqueæ ad parvam distantiam extra foramen, ubi vena maxime contrahitur, & velocitatem aquæ in vena sic contracta.

In problematis superioris solutione ostensum fuit, particulas aqueas ex foramine erumpentes non una omnibus communi velocitate proflire, sed eo velocius ferri, quo propius absunt a centro foraminis; & velocitatem relativam particularum interiorum, respectu particularum singulas extrorsum contingentium, con-

stantes sibi æqualem fieri per totum foramen; & relativam hanc velocitatem proficisci ex resistantia, quam ab aqua circumposita patitur aqua versus foramen descendens.

At postquam aqua ex foramine egressa est, ejusque superficies exterior nullam jam patitur resistantiam ab aqua circumposita, nec etiam ab aëre ambiente, quippe quæ ex hypothesi per vacuum feratur, fieri nequit ut amplius persistet illa velocitas relativa, aut velocitatis absolutæ inæqualitas. Jam enim particulæ celeriores accelerent necesse est particulas tardiores contiguas, & ipsæ vicissim a tardioribus retardentur, donec universæ unicam velocitatem sortitæ fuerint particulis omnibus communem; quod intra parvum spatium fiet, postquam ex foramine fuerint egressæ.

Dum vero communem hanc velocitatem consequuntur omnes particulæ, contrahitur necessàrio venæ diameter. Similiter nempe hic res accidit, atque cum flumen rapidius cum tardiori, *Rhodanus* puta cum *Arare*, conjungitur. In alveo communi par est velocitas aquæ ex utroque flumine advectæ, & pari copia transmittitur aqua per sectionem hujus alvei, atque prius transmissa fuerat per sectiones fluminum amborum: Sed longe minor est *Rhodani* sectio post *Aram* receptum, quam summa sectionum *Rhodani* & *Araris*, priusquam confluant.

Sit igitur venæ aqueæ contractæ, ubi omnes particulæ in eadem venæ sectione sitæ æqualem velocitatem adeptæ fuerint, radius ρ , & communis ista velocitas vocetur v .

Jam *Mensura* aquæ per venæ contractæ sectionem transfluentis tempore T sic habebitur.

Est $V : v :: 2A : \frac{2Av}{V}$, quæ est longitudo venæ aqueæ per hanc sectionem transeuntis tempore T . Estque $\frac{2Av}{V} \times m \rho^2$ Mensura aquæ per hanc sectionem transeuntis eodem tempore.

Et *Motus* aquæ per sectionem venæ transeuntis tempore T , est $\frac{2Av}{V} \times m \rho^2 \times v$, sive $\frac{2Am\rho^2 v^2}{V}$.

Atqui *Mensura* aquæ per venæ sectionem transeuntis æqualis est *Mensuræ* aquæ per foramen eodem tempore effluentis, hoc est, $\frac{2Am\rho^2 v}{V} = \frac{2Amr^2}{\sqrt{3}}$, sive $2\rho^2 v = \frac{2r^2 V}{\sqrt{3}}$.

Porro *Motus* aquæ ex foramine erumpentis, cum non mutetur ex actione particularum inter se, æqualis erit *Motui* aquæ per venæ sectionem transluentis, hoc est $AVmr^2 = \frac{2Am\rho^2 v^2}{V}$, sive $2\rho^2 v^2 = r^2 V^2$.

Est autem $v = \frac{2\rho^2 v^2}{2\rho^2 v} = r^2 V^2 \times \frac{\sqrt{3}}{2r^2 V}$, hoc est $v = \frac{V\sqrt{3}}{2}$, & $v^2 = \frac{3V^2}{4}$.

Et $\rho^2 = \frac{r^2 V^2}{2v^2} = \frac{r^2 V^2}{2} \times \frac{4}{3V^2}$, sive $\rho^2 = \frac{2r^2}{3}$,

& $\rho = \frac{r\sqrt{2}}{\sqrt{3}}$. Q. E. I.

COROLL. Cum fit $v^2 = \frac{3V^2}{4}$, altitudines autem sint in ratione duplicata velocitatum inde cadendo geni-

genitarum, patet eam esse velocitatem aquæ in vena contracta, qua sursum proflire queat in vacuo ad tres quartas partes altitudinis aquæ supra foramen.

S C H O L I U M I.

Mirabilem hanc venæ aqueæ contractionem primus omnium, ante annos fere 30, animadvertit *Newtonus*, cum occasione difficultatum quarundam ab altero illo *Britanniæ* lumine, & amico nostro nullis unquam lacrymis fatis deflendo, *Rogero Cotesio*, propositarum, qui tunc temporis secundam *Principiorum* editionem adornabat, attentius in motum aquæ effluentis inspiceret: eandem postea pluribus experimentis confirmavit *Polenus*. Exinde philosophorum ingenia fatis superque exercuit hoc phænomenon: sed omnes hactenus latuit vera causa hujus contractionis.

Radius autem venæ hoc problemate definitus, nempe $\frac{r\sqrt{2}}{\sqrt{3}}$, sive $r \times 0,8165$, paulo minor est radio $r \times 0,84$, quanta a *Newtone* traditur; paulo major radio $r \times 0,78$, qualis fere *Poleno* mensuranti contigit, estque pene inter utramque intermedia.

At velocitas supra determinata $\frac{V\sqrt{3}}{2}$, qua proflire sursum possit aqua ad tres quartas partes altitudinis vasis supra foramen, longe abest ab experimentis, quibus reperiuntur fontes salientes ad integram fere cisternæ altitudinem adsurgere. Provenit autem istud velocitatis discrimen ex aëris ambientis resistentia, quæ tantum abest ut minuat altitudinem salientium, quod vulgo creditur, eandem non parum auget, id quod ex *Problematis* septimi solutione patebit.

S C H O-

SCHOLIUM II.

Ex iis, quæ supra exposuimus in *Scholio 2. Problematis II.* patet valores hosce ipsarum ρ & v , pro accuratis haberi non posse, nisi altitudo aquæ pro infinita habeatur respectu diametri foraminis, proxime tamen ad veros valores accedere, si altitudo aquæ sit diametri foraminis dupla, aut duplo major. Quod si eosdem valores accurate velis determinare, adhibere poteris *Mensuram* eodem *Scholio* definitam, sive $2mAr \times \frac{M+N-4Ar}{\sqrt{L+K-\frac{3}{2}r^2}}$, unde habebis $v = \frac{rV \sqrt{L+K-\frac{3}{2}r^2}}{2M+N-4Ar}$

& $\rho = \sqrt{2} \times \frac{M+N-4Ar}{\sqrt{L+K-\frac{3}{2}r^2}}$. Poteris etiam adhibere series infinitas eodem *Scholio* expositas.

PROBLEMA IV.

Aqua in vacuum effluente ex foramine circulari in medio fundo vasis cylindrici, ubi particule aquæ inter defluendum intra vas tantam patiuntur resistentiam ex defectu lubricitatis, ut inde notabiliter imminuatur Motus aquæ, & data Mensura aquæ effluentis, definire Motum ejusdem, & velocitatem qua per medium foramen egreditur.

Sit data *Mensura* aquæ tempore T effluentis, $2mr^2Aq$. Huic ergo æqualis erit *Mensura* per analysin designata in solutione *Problematis* secundi, nempe

nempe $\frac{2mr^2 Av}{3V}$, hoc est $2mr^2 Aq = \frac{2mr^2 Av}{3V}$,
 five $v = 3Vq$.

Motus vero ejusdem aquæ per analysin designatus in eodem *Problemate*, est $\frac{mr^2 Av^2}{3V}$; & loco v^2 substituendo ejus valorem modo inventum, fit is
Motus $\frac{mr^2 A}{3V} \times 9V^2 q^2 = 3q^2 mr^2 AV$. Q. E. I.

COROLL. Si ex *Motu*, qui tempore T generari possit a columna aquea foramini insistente, five ex $mr^2 AV$, detrahatur *Motus* aquæ eodem tempore effluentis, $3q^2 mr^2 AV$, relinquitur *Motus* tempore T ex resistentia deperditus $mr^2 AV \times 1 - 3q^2$.

SCHOLIUM.

Si accuratam solutionem desideres, recurrendum est ad Scholium secundum Probl. II. hunc in modum; $2mr^2 Aq = \frac{16mA^2v}{rV} \times \frac{M+N-4Ar}{M+N-4Ar}$, unde

de $v = Vq \times \frac{r^3}{8A \times M+N-4Ar}$. Et *Motus*

aquæ effluentis tempore T , erit $mr^2 AV \times q^2 r^2 \times \frac{L+K-\frac{3}{2}r^2}{M+N-4Ar}^2$: unde *Motus* ex resistentia deperditus

tempore T , erit $mr^2 AV \times 1 - \frac{q^2 r^2 \times L+K-\frac{3}{2}r^2}{M+N-4Ar}^2$.

P R O B L E M A V.

Iisdem positis datisque, & negligendo accelerationem aquæ extra foramen, determinare diametrum venæ aquæ ad parvam distantiam extra foramen, ubi vena maxime contrahitur, & velocitatem aquæ in vena sic contracta.

Per tertium *Problema*, *Mensura* aquæ per sectionem venæ transeuntis tempore T est $\frac{2m\rho^2 Av}{V}$: hæc autem æqualis est *Mensuræ* datæ $2mr^2 Aq$; unde $\rho^2 v = r^2 Vq$.

Porro, per idem *Problema* tertium, *Motus* aquæ per sectionem venæ transeuntis tempore T , est $\frac{2m\rho^2 Av^2}{V}$, cui æqualis est *Motus* superiore *problemate* definitus, $3q^2 mr^2 AV$, unde $2\rho^2 v^2 = 3q^2 r^2 V^2$.

$$\text{Est autem } v = \frac{2\rho^2 v^2}{2\rho^2 v} = \frac{3q^2 r^2 V^2}{2q r^2 V} = \frac{3qV}{2}.$$

$$\text{Et } \rho^2 = \frac{r^2 Vq}{v} = r^2 Vq \times \frac{2}{3qV} = \frac{2r^2}{3}; \text{ unde } \rho =$$

$$\frac{r\sqrt{2}}{\sqrt{3}}. \text{ Q.E.I.}$$

COROLL. I. Eadem perstat ratio inter radium foraminis & radium venæ contractæ, sive minuatur utcunque per resistantiam *Motus* aquæ effluentis, ut in hoc *Problemate*, sive non minuatur, ut in *Problemate* III. cum sit utrobique $\rho = \frac{r\sqrt{2}}{\sqrt{3}}$.

E

COROLL.

COROLL. 2. Cum minuitur per resistantiam *Motus* aquæ effluentis, minuitur simul velocitas in vena contracta. Cum enim in *Problemate* tertio fuerat $v = \frac{V\sqrt{3}}{2}$, fit modo $v = \frac{3qV}{2}$, hoc est, minuitur v ex $V \times 0,866$ ad $V \times 0,856$ fumendo $q = 0,571$ ex *Poleni* experimentis.

SCHOLIUM.

Accurate erit $v = V \times r^2 q \times \frac{L + K - \frac{3}{2}r^2}{M + N - 4Ar}^2$,
 eritque $q = \sqrt{2} \times \frac{M + N - 4Ar}{\sqrt{L + K - \frac{3}{2}r^2}}$, pariter atque inventum est in *Scholio* secundo *Problematis* tertii.

PROBLEMA VI.

Aqua in aërem effluente per foramen circulare in medio fundo vasis cylindrici, ubi particulae aquæ inter defluendum intra vas tantam patiuntur resistantiam ex defectu lubricitatis, ut inde notabiliter minuatur Motus aquæ, & data Mensura aquæ effluentis, definire Motum ejusdem, & velocitatem qua per medium foramen egreditur.

Sit data *Mensura* aquæ tempore T effluentis $2mr^2Aq$, ut in *Problemate* IV. & ope ejusdem *Problematis* habebitur *Motus* ejusdem $3q^2mr^2AV$, & velocitas quacum egreditur per centrum foraminis, sive $v = 3qV$. Q. E. I.

COROLL.

COROLL. Cum detur q , est v ut V , hoc est, ut \sqrt{A} .

S C H O L I U M.

Hæc eadem accurate definita reperies in *Scholio Problematis IV.*

P R O B L E M A VII.

Aqua in aërem effluente, negligendo accelerationem aquæ extra foramen ex gravitate ortam, si dentur duæ qualibet ex tribus sequentibus, nempe Mensura aquæ effluentis, velocitate in axe venæ contractæ, & diametro ejusdem venæ, reliquam determinare.

Cum aqua ex foramine erumpens per vacuum fertur, ostensum est in solutione *Problematis III.* æqualem fieri velocitatem particularum aquæ per totam sectionem venæ contractæ: Nunc autem, cum vena per aërem fertur, tollitur necessario æqualitas ista velocitatis. Partes enim venæ exteriores aërem circumjacentem in motum concitant, atque ab eodem ipsæ retardantur, adeo ut parem cum reliquis velocitatem adipisci nequeant. Partes autem extimæ, cum ab aere retardentur, partes contiguas interiores retardant, hæque proximas; atque eo pacto fit, ut particula quæque interior celerius feratur particula contigua exteriore, adeo ut velocitas maxima sit in axe venæ, in ambitu minima. Et cum partes exteriores tardius ferantur per aërem, quam, sublato aëre, per vacuum ferrentur, inde fit ut partes mediæ velocius

ferantur, aëre venam ambiente, quam ferrentur aëre sublato. Qua de causa mediæ partes aquæ in fontibus salientibus multo altius adsurgunt in aëre aperto, quam in vacuo essent adscensuræ, prout monuimus sub finem *Schol. I. Probl. III.*

Porro, eæ partes aeris, quæ venæ aqueæ sunt contiguæ, cum ab aqua in motum concitentur, ipsæ alias sibi extrorsum adjacentes in motum concitant, hæque proximas exteriores, & illæ reliquas successive ad certam aliquam distantiam ab ambitu venæ.

Velocitas autem particularum aquæ ab axe venæ ad ambitum ejusdem ita decrescat, necesse est, ut particulæ cujusque ubicunque sitæ una eademque sit velocitas relativa respectu particulæ extrorsum adjacentis, iisdem ex causis quas exposuimus in solutione *Problematis* secundi. Nam si quævis particula velocitatem relativam majorem habeat quam reliquæ, ea majorem experietur resistentiam ex attritu particulæ extrorsum adjacentis, & eo pacto ad æqualem cum ceteris velocitatem relativam perducetur. Pari modo particula quæque aeris circumpositi, qui in motum concitatur, unam eandemque habebit velocitatem relativam respectu particulæ aëreæ extrorsum adjacentis.

At longe discrepat velocitas relativa particularum aquearum inter se, a velocitate relativa particularum aeris, quod hoc modo concipi potest.

Particula quævis aquæ in extima vena constituta, a particula aquæ introrsum proxima sollicitatur ad motum accelerandum; eadem a particula proxima aeris retardatur: & cum particula ista extima justam velocitatem adepta sit, pares sint, necesse est, hæ duæ vires contrariæ, quarum altera retardat particulam, altera acce-

accelerat. Id vero fieri non potest, nisi factum ex velocitate relativa & densitate particulæ aqueæ accelerantis, æquale sit facto ex velocitate relativa & densitate particulæ aeræ retardantis. Est autem densitas aeris ad densitatem aquæ, ut 1 ad 900 circiter. Itaque velocitas relativa inter extimam particulam aqueam & proximam aeream, est ad velocitatem relativam inter duas proximas particulas aqueas, ut 900 ad 1 circiter.

Porro, particula ista intima aerea ad motum accelerandum sollicitatur a proxima contigua particula aquea, retardatur a particula aerea extrorsum proxima. Cumque hic etiam vires duæ contrariæ sibi invicem æquales sint, erit factum ex densitate & velocitate relativa particulæ aqueæ accelerantis, æquale facto ex densitate & velocitate relativa particulæ aeræ retardantis. Unde erit velocitas relativa, quæ est inter duas istas particulas aereas, ad velocitatem relativam, quæ est inter particulam intimam aeream & proximam aqueam, ut 900 ad 1 circiter; eritque eadem ad velocitatem relativam, quæ est inter duas proximas particulas aqueas, ut 900×900 ad 1 fere: & hæc tanta velocitas relativa perpetuo sibi constabit per totam crassitiem annuli acrei, qui ab aqua profluente in motum concitatur.

Designentur jam literis r, m, v, a, V, A, T , eadem atque in secundo Problemate literis iisdem significantur. Esto etiam v velocitas aquæ in axe venæ aqueæ contractæ, ρ radius ejusdem venæ, R radius venæ imaginariæ, per quem velocitas v , decrescendo gradatim, pari modo atque decrescit in vena vera, tandem ad nihilum redigatur.

Sit etiam *Mensura* aquæ tempore T effluentis per foramen, $2 q m r^2 A$.

Jam *Mensura* aquæ eodem tempore fluentis per venam contractam, methodo in *Problemate* II. exposita, invenietur $\frac{2 m A v \rho^2}{3 R V} \times \overline{3 R - 2 \rho}$.

Hæ autem *Mensuræ* æquales sunt; hoc est,
 $2 q m r^2 A = \frac{2 m A v \rho^2}{3 R V} \times \overline{3 R - 2 \rho}$, five, $3 q r^2 R V = v \rho^2 \times \overline{3 R - 2 \rho}$.

Porro, cum *Mensura* aquæ effluentis per foramen tempore T sit $2 q m r^2 A$, *Motus* ejusdem, per *Problema* VI. est $3 q^2 m r^2 A V$.

Et *Motus* aquæ per venam fluentis eodem tempore, per methodum *Problemate* secundo usurpatam, invenitur $\frac{m A v^2 \times 6 R^2 \rho^2 - 8 R \rho^3 + 3 \rho^4}{3 V R^2}$.

Hi autem æquales sunt, hoc est, $3 q^2 m r^2 A V = \frac{m A v^2 \times 6 R^2 \rho^2 - 8 R \rho^3 + 3 \rho^4}{3 V R^2}$, five, $9 q^2 r^2 R^2 V^2 = v^2 \times \overline{6 R^2 \rho^2 - 8 R \rho^3 + 3 \rho^4}$.

Duabus his æquationibus rite reductis ad expungendam R , pervenitur ad æquationem sequentem,
 $\rho^4 v^2 = 2 q v V r^2 \rho^2 + 12 q^2 V^2 r^2 \rho^2 - 9 q^2 V^2 r^4$,
 unde $\rho^2 = \frac{q V r^2}{v^2} \times \overline{v + 6 q V - 2 \sqrt{3 q v V + 9 q^2 V^2 - 2 v^2}}$,
 & hinc obtinetur ipse ρ , five radius venæ contractæ, cum dantur q & v .

Porro,

Porro, ex eadem æquatione elicitur, $v = \frac{q\sqrt{r}}{e^2} \times$
 $\frac{r + 2\sqrt{3e^2 - 2r^2}}{r + 2\sqrt{3e^2 - 2r^2}}$.

Denique, $q = \frac{e^2 v}{r\sqrt{r + 2\sqrt{3e^2 - 2r^2}}}$. Q. E. I.

SCHOLIUM I.

Supra posuimus *Motum* aquæ per venam contractam fluentis æqualem *Motui* effluentis per foramen. Id autem, si rigorem Mathematicum spectes, non est verum. *Motus* enim aquæ per foramen effluentis aqualis est *Motui* aquæ per venam contractam fluentis, & *Motui* annuli aerei venam ambientis, qui aer ab aqua per venam fluente in motum concitatur, simul sumptis. Sed annuli aerei *Motum*, cum ejus annuli crassities non sit major quam $\frac{R - e}{900 \times 900}$, ejusque den-

sitas non sit major parte $\frac{1}{900}$ densitatis aquæ, pro nihilo habemus; idque faciendo æquationes longè simpliciores reddimus quam alioqui essent futuræ.

SCHOLIUM II.

Per *Corollarium* I. *Problematis* V. cum aqua in vacuum effluit, eadem semper perstat ratio inter radium foraminis & radium venæ contractæ, sive minuatur utcunque per resistantiam *Motus* aquæ effluentis, sive non minuatur. Unde, ut in re physica, veri simillimum censemus, datam haberi rationem inter

inter hos radios, etiam cum aqua per aerem profluit, utcunque minuatur *Motus* aquæ effluentis per resistantiam, aut saltem eam rationem non nisi quam minimum mutari. Idque cum reperiatur consentaneum experimentis hactenus factis, quod infra clarius apparebit, nos pro vero habebimus, donec experimenta in posterum accuratius instituenda aliquid certius docuerint.

Porro, si datur ratio inter r & ρ , datur etiam ratio inter r & R , sive ratio inter radium foraminis, & radium imaginarium, per quem velocitas v gradatim decrescendo ad nihilum redigitur.

Nam, eliminando v ex æquationibus duabus supra positis, $9q^2 r^2 R^2 V^2 = \rho^2 v^2 \times \overline{6R^2 - 8R\rho + 3\rho^2}$, & $3qr^2 RV = \rho^2 v \times \overline{3R - 2\rho}$, pervenitur ad æquationem, $\rho^2 \times \overline{9R^2 - 12R\rho + 4\rho^2} = r^2 \times \overline{6R^2 - 9R\rho + 3\rho^2}$, unde $R = \frac{\rho \times 2}{3} + \frac{r}{\sqrt{3\rho^2 - 2r^2}}$.

Præterea, ex altera harum æquationum, $3qr^2 RV = \rho^2 v \times \overline{3R - 2\rho}$, fit $3r^2 R : \rho^2 \times \overline{3R - 2\rho} :: v : qV$, & cum data sit ratio prior, datur etiam ratio posterior,

hoc est, datur quantitas $\frac{v}{qV}$.

Quantæ autem sint tres hæ rationes datæ, postea demonstrabimus.

Reliqua proximo Transactionum Numero communicabimus.

III. *Dias Antrorum mirabilis Naturæ, glacialis alterius, alterius Halitus noxios eructantis, ad Regiam Societatem Scientiarum Londinensem, a Matthia Belio Pannonio, R. S. S. missa.*

C A P U T I.

De Antro Glaciali Szelicensi.

§. I. **E**ST, in *Carpathi* radicibus, qua se in meridiem, jugis dorsisque sensim mitescentibus, sinuant, *Comitatus Thornensis*, ab arce *Thorna*, sic adpellatus. Angusto is ambitu, ac fere montibus impedito, definitur: quos tamen, pro *Carpathi* ingenio, passim naturæ prodigia, insignes faciunt. In his montem suo merito recensueris, qui inter *Szelicze*, & *Borsua*, vicos adtollitur: non quod mole reliquos antecellat; sed quod speciem habeat portentosam, cujus nunc historia exhibenda est.

§. II. *Antrum Szelicense* dicimus, a vico *Szelicze* nuncupatum. Situs pago, inter sylvas ac nemora, collinus ideoque effætus, cœlum asperum, aër obtigit frigidus, quem validi ac fere perpetui Aquilones, ex nivoso celsoque illo *Carpatho*, a Septentrione dejecti, extra modum immitem reddunt, ut muscis atque culicibus tunc quoque inhospitalis sit, cum tota regione maxime calet. In hujus vici agro, antrum, de quo fermo est, in montem nunc memoratum grandi & obverso in Aquilonem hiatu dehiscit: quippe qui, orgyas XVIII. altus, IX. latus est, atque ideo accipiendis ac penitus hauriendis Aquilonibus sic satis ido-

neus laxusque. Profunda antri, & subterranei petrosique specus, inexpertis meatibus, in Meridiem abeunt.

§. III. Natura antri id habet prodigii, quod cum extus brumâ intensissime riget, tepido sit intus aere; frigidus contra, immo glaciali, cum sunt fervidissimi soles. Nimirum, simul diffugientibus nivibus, ver inire cœpit, interior antri concameratio, qua ea meridiano soli dorso objicit, aquam limpidam & passim distillantem exsudat: quæ interni frigoris vi, in pelucidam glaciem concrevens, stirias efficit, ad ingentium doliorem molem crassas ac pendulas, inque ramos abeuntes, miris illusos speciebus. Quod aquæ, e stiriiis, humi arenosæ decedit, id quoque opinione celerius congelat. Ergo, non fornices modo, natura in solidam petram cavati, sed fundum quoque antri, multa & nitida glacie conveltutum est. Crederes, totam cavernam, e crystallo, substructam incrustatamque esse, ita circumquaque glaciës resplendescit.

§. IV. Inusitatus hic obtutus eo est ingredientibus speciosior, quo antri intercapedo & latius abit, & deprimitur profundius. Qua adiri potest antrum, quinquaginta orgyiarum profundum, sex & viginti laxum est; concamerationum altitudine, ob tuberosos rupium arcus, inæquali. Quod ultra demergitur, propter hiatus præcipitium, inexpertos habet recessus. Neque enim erat adhuc, qui in adundis antri penetrabilibus facere voluisset periculum, ob lubricos gressus, & fallentem vestigia glaciem. Certe enim, incisis, multo labore, gradibus opus habent, qui vel exploratum profundum illud adire volunt. Erant, qui immissa bolide, specus interiora ibant exploratum: verum, quod antrum, non ad putei modum recto tramite,

tramite, seu perpendiculariter, deprimitur, sed varie se, & in anfractus sinuat, inutile fuit experimentum. Plus ii profecerunt, qui sclopos, solide largeque fartos, intra specum disploserunt. Adversum est enim, absorptos intime boatus, per plura temporis minuta, ad modum validissimi tonitru, illiso huc & illuc sonitu, horrendum quid reboasse: quod argumento fuit, profunde antrum propagari, habereque diffusos quaquavorsum meatus.

§. V. Glaciale istud specus ingenium, totum æstivum est: quod ideo admirationis habet plurimum; augetur enim cum increfcente solis ardore. Primo, nimirum, vere, hibernus ille tepor cessare, mox, ubi id adolevit, intendi frigus occipit, tantis accessionibus, ut quo magis aër incalescit, eo antrum frigeat exquisitius. At ubi æstas iniiit, jamque fervet Canicula, in glaciale brumam intus abeunt omnia. Tunc scilicet, ubertim destillant ex specus fornicibus aquæ, in glaciem solidantur, tantis incrementis, ut, qua hodie tenues adparebant stiria, eæ perendie dolia referant, & quasi in præceps ruentia petrarum fragmenta. Passim, qua guttatim per antri latera, demanat latex, mirificæ visuntur incrustationes, & ad tapetum formam, quasi ex artis præscripto, fabrefactæ. Reliquum glaciei ita mutuo hærescit, prouti æstus sub dio sunt vicissitudines. Nam, si continenter & vehementius caleat, plus glaciei & stirii adcrefcit, & parietibus fundoque antri; sin calor, quod fit, aut Aquilonibus aut delapsis imbribus temperetur, lentius concrefcunt aquæ, quin & glacies ipsa largius tunc sudat, & exiles quasi rivulos quosdam fundere incipit: dum, incalescente iterum cœlo, ad ingenium redeat, glaciale specus temperamentum. Fuere, qui observârunt, præ-

fagire ejus naturam aëris viciffitudines, ficuti folent barometra. Nam, fi aër extus immineat fervidior, pluribus ante horis, quam fervor ille adefl, tenaciore gelu aquæ adflringuntur in glaciem: quod contra fe habet, fi aer fub dio ingruat frigidior; tunc enim, calidiffimo adhuc cœlo, dimanare fenfim glacies incipit.

§. VI. Fit, ex hac antri natura, ut tantum ei fufficiat, de pellucidiffima glacie, quantum ne fexcentæ quidem quadrigæ, in fingulas hebdomadas onerata, poffint exinanire. Atque habent hoc omnino in more pofitum accolæ, ut cum operis campeflribus diftinentur, & aut foeno parando, aut cogendis frugibus, in antri hac vicinia, fuo ritu, vacant, glaciem hinc promant, eaque aut refrigerent tepentes fontanas, aut ad folis æflum glaciem ipfam in potum liquefaciant: infigni falubritatis opinione; quando putant aquæ iftud genus & ftomachum minus onerare, & pronius feu per fudores excerni, feu per veficam: quod proclive efl omnino ad credendum, in ea aquarum percolatione: qua de re jamjam arguendi erit locus.

§. VII. Sunt vero frigori, intenfori illi, & remiffiori, fuæ per antrum regiones. Ex aprico, jucundus ad fpecum efl aditus: quippe auram adflans, Ethefiarum fimillimam. Mox, ad paffus aliquot poffreffis, plus inhorrefcere, ac fi porro pedem proferant, fubeantque antri propylæon, artus intremifcere occipiunt, ut neceffum habeant laxatas forte fub dio veftes curatius adflringere. - Inde, in profundum abeuntes frigus excipit brumali fuppar, ut hi quidem calidiorè halitu manus confovere, illi valida artuum commotione, moleflum, ac fere intolerabile frigus, mitigare debeant.

Quod

Quod eo subinde fit intensus, quo se profundius immittunt curiosi scrutatores. Sed, gradum referentibus, contraria eveniunt omnia: nam, quo ex imo, pedem efferunt altius, eo frigus est remissius; dum remittat atque intepescat penitus. Postea, ubi Canicula deseruit, jamque in autumnum æstas desit externi aëris habitum specus sūapte natura sequitur. Primis nimirum mensibus, & dum noctes ineunt gelidiores, dimanare sensim glacies, tunc frigescente magis ac magis aëre, & ubi jam glacie extus rivi adstringuntur, quasi admotis ignibus, liquefcere incipit; donec incumbente bruma, nullo post se relicto vestigio, prorsus inarescat. Tunc vero placidus, toti specui, calor inducitur, fitque glaciale illud conditorium perfugio insectis, & bestiolis aliis hiemem ægre ferentibus. Nam præter muscarum & culicum agmina, vespertilionum item globos, atque noctuarum contubernia, lepores quoque & vulpes hospitatum huc concedunt: dum adolescente vere iterum redeat specui glaciale illud ingenium. Antri superficies, profunda humo obruta, atque soli opposita meridiano, ubere ac pingui gramine herbescit, estque ideo pabulantibus armentis, quin & fœniscoio, si pecus arceatur, opportuna.

§. VIII. Hæc ita se habent. Nunc, quemadmodum insolens antri natura explicanda sit, paucis videamus. Universim, eæ sunt caloris atque frigoris vicissitudines, in subterraneis, ut certare quasi mutuo videntur. Nam, cum æstuat foris aer, frigent cavernarum istiusmodi recessus; atque tepent iterum, cum extus aëre frigus est: id quod, cellæ vinariæ, profundius paulo depressæ, quotidiano & vulgari fatis experimento, condocent. Nempe, ea est caloris efficacia, ut, quum telluri, elemento ex se crasso, humido,

ac frigido, incumbit, nativum ejus frigus interius propulset, atque in factas arte aut natura specus, coactum, notabiliter condenset. Quod aliter se habet, quando frigus terræ superficiem incubat; prolicit enim, imis visceribus, conceptum, a quocunque principio, calorem, & qua se per cellas aut cavernas diffundere potest, aërem efficit tepidum, passim etiam eximie calentem. Succurrit, hæc scribenti, civium meorum, per Hungariam, campestem illam, & æstivis solibus, extra modum torridam, vini frigefaciendi, vetus consuetudo. Nempe, cum per vastas solitudines iter faciunt, aut illic, pro re nata, moras coguntur necere, neque aut glacies, aut fontana aqua, refrigerando potui sufficit, scrobem, ad duorum fere pedum altitudinem, humi defodiunt: huc demum vinarias lagenas immittunt, atque refoffa humo, diligenter ac solide contegunt: tunc ignem subitaneum & luculentum, super eadem illa scrobe, in qua vinum est conditum, ex stramine, foeno aut arundine, componunt; qui ubi suapte restinctus, aut disjectus est consulto, regeffa calente extus humo, vinum eximunt, non secus refrigeratum, quam si aquæ glaciali immersum habuissent. Unde vero, vino illi inductum frigus illud? Nimirum, flammæ ille fervor, qui superficiem scrobis subito concalescit, nativum terræ frigus, undiquaque condensatum, lagenæ circumfudit, quod demum vinum pervasit, fecitque gustui idoneum.

§. IX. Pronum hinc est conjicere, quemadmodum antrum istud *Szelizense*, nunc glaciali sit natura, nunc iterum tepescere. Caloris extus, in antri superficiem incumbentis, vis & efficacia, condensato, terræ atque petrarum, quæ subtus in fornices abeunt, nativo frigore, aquas gelidas primum, mox & congelantes,

lantes, efficit: dum refrigerato iterum aëre, prolucatur, imis terræ visceribus conceptus calor. Hæc, ad eum modum, vulgo in sensus incurrunt. Quæ cum adfero, non ideo accedo *Morini* sententiæ, qua apud *du Hamelium*, terram, uti aërem, in tres dispertitur regiones; quarum prima, alternis friget & calet; hieme calida, æstate frigidior, usque ad cccc hexapedas seu orgyias patet. Hanc secunda regio, ejusdem illa opinione, excipit, quam semper fervere, ipse expertum se in fodinis Hungaricis ait. Tertiam, jugi frigore tanquam centro terræ, proximam algere conjectat; quemadmodum media aëris regio perpetuo frigida est, ac suprema, & cœlo finitima, semper calet. Hæc, inquam, *Morino* relinquimus, tanquam inexplorata incertaque. Sane, quæ de fodinis Hungaricis, ad regionis secundæ perpetuum calorem adferendum, memorat, non sunt undiquaque vero consentanea. Nam fodinarum quidem, quas optime cognitæ habeo, nulla ad quadringentarum orgyiarum profundum, vix ulla ad ducentas hexapedas, deprimitur, ob erumpentium subtus aquarum impedimenta; qui ergo poterat ferventem perpetuo regionem terræ secundam, demum infra quadringentas hexapedas, ineuntem, adire? Sunt fodinis suæ omnino regiones, tepidæ hic, ferventes alibi, etiam iis locis, quorum profundum ne octoginta quidem orgyias conficit. Sed, ad antri glaciale naturam redeamus.

§. X. Multum, ad eam confert, præter vulgarem, quam adtulimus, causam illam, ab incumbentis caloris vi petitam, ipsa antri positio, & concamerationis ingenium. Obversum est nempe ore hiantæ, frigidissimis Aquilonibus, qui a *Scepusiaco Carpatho*, in has ejus radices immissi, & frequentes sunt, & vernis

potissimum atque æstivis mensibus, ultra modum sæviunt, immo furunt; quodque ex nivosis montibus illis deruunt, vehunt secum gelidissimas particulas, quæ demum in os antri illapsæ, & in ejus concavo, quasi conglomeratæ urgente extus calore, ex facili, destillantem aquam pervadunt, inque glaciem adstringunt. Accedit, habitus concamerationum, quæ totæ e petris, solidissime invicem compactis, & heic submotis altius, alibi in ruinam quasi impendentibus, factæ sunt. Petrarum autem *Carpathi*, plerumque, falsa, nitrosa, aluminosa atque vitriolica est natura. Ergo quid esse potest proclivius, quam glaciem in antro hoc *Szelicensi*, ex glacialium & dictarum particularum commixtione, ubertim progenerari? Adfentientur nobis, qui artificiosam liquorum congelationem, repetitis sæpius experimentis, condidicerunt. Nix enim, vel glaciès, cum, sale communi, nitro, aut alumine, aut vitriolo, æqua portione, permixta, & vasi circumposita, aquam vase contentam, in glaciem congelat, etiam media æstate, vel prope ad ignem: ut documenta alia, quæ sunt in promptu, fileamus. Hucusque de antro *Szelicensi*.

C A P U T II.

De Antro Ribariensi, Halitus noxios eruclante.

§. I. **L** Etales specus, passim, per orbem terrarum observârunt veteres, juxta & nuperi scriptores. Non est instituti mei, eorum, vel nomina tantum confectari: fuscè, id genus scrobes seu antra, & horum halitus, *Georgius Agricola* prosequitur, Libro IV. de Natura eorum, quæ effluunt e terra.

Do-

Domesticum Hungariæ antrum, quod est in *Comitatu Zoliensi*, ad vicum *Ribar*, describendum mihi proposui: partim, ut rem omnem ex vero expromerem, partim ut scriptorum detegerem aberrationes. Meminet, antri hujus, laudatus nunc *Agricola*, eodem, quem citavi, loco: *Nec pars Ungariæ, inquit, quondam Dacia dicta, libera est a pestilentibus spiraculis: quæ a Neusohla, oppido, aris metallis clara, absunt circiter sex millia passuum, hæc interdum aves, quæ advolârint, & reliquas animantes, quæ accesserint, solent necare. Male, Neosolium, Dacia concludi, suo loco a monui; de pestilentibus spiraculis, quæ accusat Agricola, infra dispiciam. Ad eum fere modum, Wernherus, ejusdem Ribarensis antri meminit^b, quem jam non est opus exscribere: quippe qui, propositum habeam, meas ipsius ἀπόψεις observationes, paucis recensere.*

§. II. *Ribar* vicus est *Comitatus Zoliensis*, sesquimillari, secundo amne *Grano*, Neosolio, in meridiem repressus: quem loco^c nunc citato, memoravimus. In hujus agro, collina in orientem positione, *Thermæ* scatent, exquisitæ quidem salubritatis; sed ingenii admirabilis: quas jam non vacat expromere. A thermarum istis scatebris, sexcentis circiter passibus, ad solem meridianum, specus hæc, obnoxios, quos eructat, halitus, jam pridem infamis, in lætæ valliculæ, exiguo quodam, sed graminoso prato, se adaperit, habetque vicinum fontem acidum, ad potandum, sic

^a Notitiæ Hung. Novæ Tomo II. in Historia Comitatus Zoliensis, Parte Gen. Memb. I. §. XII. p. 396.

^b De Admirandis Hung. Aquis; & hunc sequuti, innumeri alii.

^c Tomo II. p. 504. num. 22.

fatis idoneum. Olim, dum incuriosior istiusmodi portentorum ætas fuit, vix erat quisquam, qui animum advertisset, ad scrobis inusitatæ ingenium. Ergo, sibi quasi relicta, & dumis circumdata, raro, aut nunquam adibatur. Fuit vero tunc, ad fontis modum, aquas ubertim eructantis, comparata, quæ, altius salientes non secus, circumquaque dimanabant, quam id in filanis fieri advertas, ubi aqua ex fusore, in cadum saliens, iterum defluit, dum meatus arte factos, subeat. Aquæ indoles, lapidescens fuit: quippe longo adfluxu, tophum generans, in eam sensim molem, circa os fontis excrefcentem, ut collis postea speciem retulerit: quo quidem, tophacei lapidis incremento, ipsa demum scaturigo, adobruta est; quando nequibat latex ad summa fontis labra, sicuti siebat antea, bulliando prosilire. Accessit, rusticorum, fontis præfocandi studium, ne esset, quod jumentis suis metuerent: postea.

§. III. Hic vetus scrobis an fontis habitus fuit, cujus vestigium, adparet hodieque, in meridionale prati latus, ad silvosi collis pedem, reductum. Postea, cum subterraneæ atque uberes aquarum venæ, occultis meatibus, e nupero eo fonte, dimanarent, factum est, ut resolutam humum, proxime ad pristinam illam scaturiginem, subruerent, atque dehiscente prato, novum hiatum aperirent. Hic demum, noxios illos halitus eructare iterum, & cum avibus, tum alii animantium generi, exitialis esse cœpit. Advertere agrestes accolæ, non cavernam tantum, fatifcente humo, loco antehac continenti, recenter factum, sed perniciosas etiam ejus exspirationes; quando, nunc aves enectas, nunc prostratas feras, ultimo, pascencia isthic jumenta, subito exanimata, reperiebant.

Dici

Dici nequit, quantis exsecrationibus invisam specum, rustica plebs, infectata sit: tamquam, quæ ab irato numine, depressa, toti illi agro perniciem, sibi exitium, aut jamjam minaretur, aut portenderet in posterum. Ostenti fama late didita, Avernum hi, Orcum illi, alii, hiatum M. Curtii facinore, expiandum, conclamabant. Dehiscebat vero caverna, ad infundibuli speciem, cujus suprema circum labra, ad calicis modum, laxè & in longum diducta, (erant enim viginti quatuor passuum longa, lata duodecim) in arctum sensim coibant, dum in angustum foramen, imo fundo, desinerent, per quod noxius ille halitus erumperet. Aquarum certe subterlabentium streperum murmur exauditur adhuc, ut pronum sit conjectatu, undosum esse oportere rivum, qui per cœcos istos meatus fertur, hauriturque demum incertis gurgitibus. Neque enim usquam erumpit in apricum, tametsi, prona, in vallem declivitas, modico a scrobe intervallo, incat.

§. IV. Insolens hoc cavernæ ingenium, uti penitus indagarem, non unam, inde ab Anno MDCCVIII. viam inii. Et primo quidem, cum amico, naturæ curioso, pullum gallinaceum, bene adultum, hastili alligatum, supra os specus ita protendi, ut ferire eum debuerint erumpentes ex imo halitus. Vix vero, hostile, ad specum, cum pullo admoveram, cum miser ille, alas complodere cœpit, momentoque expiravit. Secuimus enectum ita pullum, ut observaremus quid virulentæ rei fuerit, quod eum tam subito exanimârit? Sed nihil quidquam deprehendimus, quam restagnantem in præcordiis sanguinem, ceteroquin ab omni labe alia intemeratum. Tunc, ipse ego, exsecris prius in humo gradibus, in scrobem me immisi, ut micantem intus aquam propius contemplerer; sed

ante pes erat referendus, quam penitius descendiſſem; eo quod, ex halituum adflatu, & pectus præfocari cœpit, & caput tentari vertigine. Illud mirabile viſum eſt, quod vapores cavernæ, & cum maxime noxii ſunt adverſi, nihil nebulofi aut craſſi, quodque notabiliter in oculos incurriſſet, referrent: purus ei ſemper & limpidus aër incubat: quod aliter ſe habuit in Plutoneo, cujus Strabo atque Plinius meminere. Ergo experiendum rebamur, quid hoc ſit exſpirationum, quod, cum oculis notari haud poſſit, citam adeo mortem animantibus acceleret. Succurrebat, vix re alia naturam earum explorari poſſe, quam longiori & capaci fiſtula, ſclopum vocant, pyrio infarta pulvere, intra ſpecus intima, diſploſa. Id ut factum eſt, intonuit caverna, & quaſi fremitu edito, conceptum interne fumum, pluribus poſtea horis, jucundo ſpectaculo, cructabat. Cepimus proinde, ex hoc phænomeno, argumentum, immiſſas, diſploſione illa, in antrum, flammas, ſulphuroſas intus exhalationes, incendiſſe, quæ huc & illuc rotatæ, diutius poſtea in auras exſpirabant. Atque fuerunt ſane foetoris eximie ſulphuroſi, prorsus ad eum modum, qualis tempeſtate ingruente, in thermis obſervatus a me eſt: qua de re infra.

§. V. Sulphureo iſto halitu obſervato, facile fuit ad exiſtimandum, quidquid pernicioſiſſimæ noxæ ineſt, exſpirationibus illis, ſulphuri id adtribui oportere, volatili ei, & ultra omnem modum ſubtili. Itaque, dedimus operam, ut hauſtas ex caverna letifera aquas, firmando experimento, poſſemus ſollicitius adhibere. Incendit eam cupiditatem, avicularum enectarum, & paſſim circa ſpecum jacentium frequentia: vix enim unquam, ſeu mane, ſeu a prandiis, ad orcum hunc
acce-

accedebamus, quin nova reperiremus funera. Inprimis, Erinaceus, nos adtentos fecit, qui eodem cacoëthe suffocatus, unius noctis intervallo, adeo intumuit, ut distenta cute, spinæ simul, firmissime alioquin hærescentes, radicitus elisæ conspicerentur: quod quidem suspicandi locum præbuit, annon forte bestiola, non halitus tantum cavernæ imbiberit, sed & aquas ipsas, quæ interne magno nisu ebulliunt, & eodem iterum hiatu, ceu gurgite quodam, absorbentur denuo. Pulmones certe erinacei, livido colore tincti, quin & viscera reliqua, ultra solitam molem distenta, indicio fuerunt, & laticem gustâsse, & halitus noxios penitus bibisse bestiam, jam tum, grave quid ac molestum, redolentem. Proinde, aquam, ex profundo eo cavernæ gurgite haurimus; haud, sine famuli, quem primum in caput, sed funibus religatum, immiseramus, periculo. Fuit ea limpitudine plusquam crystallina, levis adhæc, & quasi ætherea, odoris modice sulphurosi, saporis acidiusculi quidem & modice acris, sed haud tamen, seu linguam, seu palatum adrodentis. Nempe, ad ingenium acidularum, quas vicinas habet caverna, proxime sunt visæ accedere. Timidius primum, laticem suspectum, & extremis tantum labris gustabamus, dum, alter alterius exemplo factus audentior, modicos primum haustus, mox largiores etiam, nihil dubitarem facere. Neque cuiquam, tametsi plerique nostrum, delicatioris, & ex thermarum usu, etiam fastidiosi fuerint ventriculi, noxia fuit ea curiositas; quin erant, qui aqua ea, vinum Hungaricum, generosum illud, miscere posse optarent.

§. VI. Ista sic, & ex vero, plurium annorum curis adhibitis observavimus; ut ferri hinc possit iudicium, rectene, ab *Agricola*, & *Wernhero*, pestilentes specus hujatis

hujatis exspiraciones, adpellentur. Nuperus scriptor, virus omne, vel a terræ putredine, vel a stagnantis nescio cujus humoris, vitiositate, provenire, audaciter existimat: quas opinationes singulas, jam excutiamus. Et primo quidem, tametsi letales sint cavernæ hujus halitus, pestilentes certe non sunt: quippe qui, non ob congenitum, & quod secum veherent, virus enecant animantia; sed ex causis aliis, quas mox indicabimus. Enimvero, si pestilenciales essent exspiraciones illæ, aquæ bibi, multo minus, enectæ halitibus istis animalcula, uti sunt galli gallinacei, turdi, palumbes, ac lepusculi comedi, sine vitæ, saltem melioris valetudinis jactura, possent: quod faciunt tamen procul omni noxa, qui rei inusitatæ faciunt experimentum, dum in thermis hujatibus, aut animi aut salubritatis causâ, lavant. Sed terræ forte putredo, aut stagnantis cujuscunque humoris vitium, exitiales illos ructus exspirant. At horum profecto neutrum est. Nam, si hoc admittas; halitus cavernæ crassos, atque nebulosos; aquam contra turbidam & molestum quiddam sapientem esse oportebit: quam tamen limpida, saporis adhæc, non putridi, sed subacidi; vapores præterea, ita comparatos esse diximus, ut oculis cerni observarique haud possint. Ergo a subtilissimi copia sulphuris, & mineralium admixtis halitibus, qui per subterraneos eos meatus, una cum aquis, præcipiti ac strepero lapsu feruntur, exspiracionum earum letalis noxietas arcessi debet: non quod suapte natura sint exitiales; sed quod nimia illa subtilitas, completis subito pulmonum bronchiis, præclusoque necessario ad respirandum aëre, adeoque præfoeato illic sanguine, ictu oculi, & ante enecent animantia, quam id existimari potest. Qui aliter de re tota sentiunt, & ipsi errant,

errant, & in errorem inducunt alios; cavernæ autem infamiam conflant, nulla alia ratione expungendam, quam si hac nostra observatione, ad erroris confessionem, induciti, palinodiam cecinerint.

§. VII. Ceteroquin, de sulphuroso isthoc expirationum cavernæ nostræ ingenio, & inde capi potest indicium, quod thermæ adsitæ, ejusdem sint naturæ; tametsi crassiorum halituum, & qui, nunc sunt intensiores, nunc remissiores iterum: quemadmodum scilicet, cœli aërisque habitus est, quo in calidas illas, singularis prorsus esse consuevit influxus. Nempe, si fervente aëre, in tempestatem pronum sit cœlum, thermæ nunquam non sulphur redolentes, ultra omnem modum sulphurosum quid foetere incipiunt, fiuntque lavantibus non molestæ tantum, sed etiam intolerabiles, immo letales; tunc maxime, cum, aquis per emissarium ex lavacro subductis, recentes iterum ebulliunt. Vidi equidem, virum robustum ac militarem ætate adhuc firma, cum ingruente, horis postmeridianis, graviore tempestate, in thermas lavaturus se immisisset, sustinuissetque aliquantisper sulphurosos, qui tunc ex more invaluerunt, halitus, lymphato similem, ex lavacro profiliisse, conceptaque inde febri acuta, quadriduo post, mortem suam obiisse. Nescio autem, etiamne, tempestatum his vicissitudinibus caverna, de qua agimus, sit obnoxia: neque enim, quod dolendum, ita fui curiosus, ut & hoc momentum, reliquis experimentis, curatius, sicuti meruit, adjunxissem. Illud constat, cœlo etiam sudo, & cum nulla in aëre est tempestatis suspicio, variare halituum noxiam efficacitatem illam; & nunc celerius enecare admota animalcula, nunc iterum lentius: quandoque cessare pœnitus, & quasi ingenium suum deserere: in
cujus

cujus tamen rei causas, neque ipse ego indagavi, neque memini, amicos unquam indagavisse. Jam, ita sarmentis cavernam obruerunt accolæ, ut vix adiri possit amplius. Quin & avicularum funera rariora sunt hodie, quam fuerunt olim: quod equidem profundius, quam antea, manantibus aquis adtribuerim. Hæc de his, hæctenus.

IV. *An Account of a very extraordinary Tumour in the Knee of a Person, whose Leg was taken off by Mr. Jer. Peirce, Surgeon at Bath; communicated in a Letter to Dr. Mead, Physician to His MAJESTY, and Fellow of the College of Physicians, and of the Royal Society, London.*

S I R,

Bath, June 11. 1737.

AS no one has been more conversant with the surprising Disorders to which Men are liable, or takes a greater Pleasure in their Relief, than yourself, so I flatter myself the following Case cannot fail of being acceptable to you.

William Hedges of Stratton in Somersetshire, a Farmer's Son, of 25 Years of Age, of a muscular healthy Habit, having never known any kind of Disease; about eight Years since first observed a small Swelling on his Right Leg near the superior Epiphysis of the Tibia, which (to use his own Terms) he called a Splint, about the Bigness of a split Horse-bean. As he was not conscious of any Bruise on the Part, and

as

as it was wholly free from Pain, so the only Reason he had to regard it, was from its constant Increase, which during the two first Years was very slow; but afterwards it increas'd so fast (though without Pain) as to render him altogether incapable of Labour from the time of Hay-Harvest 1735.—

Upon taking off the Limb in *May* last, I found it weigh'd, with the Leg and Foot, Sixty-nine Pounds, which (to the best of my Remembrance) is Twenty-seven more than the Leg some Years since taken off at *St. Bartholomew's* Hospital by *Mr. Gay*, for the like Disorder. The Operation itself afforded nothing uncommon, except the Quantity of recurrent Blood, which, however greater than usual, seem'd proportional to the increased Bulk of the Part.

Upon examining this surprising Tumour, the adjacent Muscles were found destitute of their fibrous and fleshy Appearance (probably from the Pressure, and great Extension, which they had suffer'd, and the little Motion which for some Years they had employ'd upon the *Tarsus* and Toes); but the *Fascia* and common Membranes of the Muscles, being greatly thickened and callous, adhered to the subjacent Tumour; and upon removing this callous Integument, the Tumour appear'd cover'd with great Quantities of Blood-vessels, much distended, and of a Colour more intensely red than natural.—

The Tumour itself was Cartilaginous for the Space of half an Inch from its external Surface; from whence it form'd numberless bony Substances of various Forms, Colours, and Consistences, which (growing more and more numerous, as they lay deeper) at last form'd a continual Substance completely

pletely ossify'd: In the Centre of this bony Substance we found about a Quart of mucilaginous Liquor, no ways fetid, (though it was then ten Days from the Operation) whose Colour and Consistence nearly resembled that of *Linseed Oil*; in which we observ'd many little bony Substances loose and floating, similar to many others adhering to the internal Surface of the Cavity, all which had nearly the Appearance of those irregular Incrustations, which in hollow Rocks are sometimes made by the dropping of petrifying Waters. After the Operation, every Circumstance of the Cure proceeded as I could wish, and the Stump is now healed.

It seems well worth observing, that the Parts above the Tumour were very little alter'd from their natural State. The Cartilaginous Extremity of the *Femur* was perfectly smooth; nor had the *Rotula* suffer'd any other Injury except the Ossification of the Ligament by which it is fixed to the *Tibia*; but the superior Extremity of the *Fibula* was wholly lost in the Tumour.

May we not justly admire the Goodness of a Constitution, which could bear such enormous Extensions in the Integuments, the Tendinous *Fascia*, and even the Bone itself, without Pain and Inflammation? Or can we sufficiently wonder, that the Fluids should be so little disposed to putrify, as to bear so great a Diminution in their Motion, and for so long Time, without vitiating the Constitution, or tainting even the Parts affected? Herewith (Sir) I beg your Acceptance of two Portraits of this surprising Case (See T A B. II.): The first Figure represents the Limb immediately after the Operation; the 2^d Figure shews the

the Tumour as opened : In both the Artift has fo far employed his Care and Skill, as well in preferving the juft Dimensions as in the Colours and Appearances, that I am left only to wifh, that in the Defcription, which I have made, my Pen had not fallen fhort of his Pencil.—I am, with the greateft Refpect,

S I R,

*Your moft obliged
humble Servant,*

Jer. Peirce.

V. *An Experiment concerning the Spirit of Coals, being part of a Letter to the Hon. Rob. Boyle, Esq; from the late Rev. John Clayton, D. D. communicated by the Right Rev. Father in God Robert Lord Bishop of Corke to the Right Hon. John Earl of Egmont, F. R. S.*

— **H**AVING seen a Ditch within two Miles from *Wigan* in *Lancashire*, wherein the Water would feemingly burn like Brandy, the Flame of which was fo fierce, that feveral Strangers have boiled Eggs over it; the People thereabouts indeed affirm, that about 30 Years ago it would have boiled a Piece of Beef; and that whereas much Rain formerly made it burn much fiercer, now after Rain it would fcarce burn at all. It was after a long-con-

tinued Season of Rain that I came to see the Place, and make some Experiments, and found accordingly, that a lighted Paper, though it were waved all over the Ditch, the Water would not take Fire. I then hired a Person to make a Dam in the Ditch, and fling out the Water, in order to try whether the Steam which arose from the Ditch would then take Fire, but found it would not. I still, however, pursued my Experiment, and made him dig deeper; and when he had dug about the Depth of half a Yard, we found a shelly Coal, and the Candle being then put down into the Hole, the Air caught Fire, and continued burning.

I observed that there had formerly been Coal-pits in the same Close of Ground; and I then got some Coal from one of the Pits nearest thereunto, which I distilled in a Retort in an open Fire. At first there came over only *Phlegm*, afterwards a black *Oil*, and then likewise a *Spirit* arose, which I could noways condense, but it forced my Lute, or broke my Glasses. Once, when it had forced the Lute, coming close thereto, in order to try to repair it, I observed that the Spirit which issued out caught Fire at the Flame of the Candle, and continued burning with Violence as it issued out, in a Stream, which I blew out, and lighted again, alternately, for several times. I then had a Mind to try if I could save any of this Spirit, in order to which I took a turbinated Receiver, and putting a Candle to the Pipe of the Receiver whilst the Spirit arose, I observed that it caught Flame, and continued burning at the End of the Pipe, though you could not discern what fed the Flame: I then blew it out, and lighted it again several times; after which

which I fixed a Bladder, squeezed and void of Air, to the Pipe of the Receiver. The *Oil* and *Phlegm* descended into the Receiver, but the Spirit, still ascending, blew up the Bladder. I then filled a good many Bladders therewith, and might have filled an inconceivable Number more; for the Spirit continued to rise for several Hours, and filled the Bladders almost as fast as a Man could have blown them with his Mouth; and yet the Quantity of Coals I distilled were inconsiderable.

I kept this Spirit in the Bladders a considerable time, and endeavour'd several ways to condense it, but in vain. And when I had a Mind to divert Strangers or Friends, I have frequently taken one of these Bladders, and pricking a Hole therein with a Pin, and compressing gently the Bladder near the Flame of a Candle till it once took Fire, it would then continue flaming till all the Spirit was compressed out of the Bladder; which was the more surprising, because no one could discern any Difference in the Appearance between these Bladders and those which are filled with common Air.

But then I found, that this Spirit must be kept in good thick Bladders, as in those of an Ox, or the like; for if I filled Calves Bladders therewith, it would lose its Inflammability in 24 Hours, though the Bladder became not relax at all.

VI. *An Experiment concerning the nitrous Particles in the Air ; by the same Hand.*

— I Took a small Gally-pot, such as the Apothecaries in the North of *England* make use of, where I was when I made this Experiment, and ground the Top of it very smooth and true, and adapted thereto a Cover of blue Slate, which I had likewise ground with much Care. Into this Gally-pot I put equal Quantities of Nitre and Flour of Sulphur, about a Drachm of each. I then fixed on the Cover, putting it into a new Digester ; but the Height which I raised the Heat to, and how long I continued it, I do not exactly remember, but believe it was three or four Seconds. When I opened it the Day following, I perceived something had transpired betwixt the Top of the Gally-pot and the Cover ; the top Edges of the Gally-pot, where the Glazing was ground off, being discolour'd, though the Nitre and Sulphur were very little diminished as to their Weight ; only they were melted into one Lump, which I took out of the Gally-pot.

And having set the empty Gally-pot upon a Shelf, upon looking at it the next Day, I found long hoary Hairs, very bright and brittle, all around the ground Edges of the Pot, very specious to behold. After I had admired them a while, I gathered them, and, tasting them, found them to be pure Nitre. I then set the Pot upon the Shelf again, and in three or four Days, still finding there were fresh Shoots made, as large and specious as at the first, I gathered them a second and third time ; so that I suppose the
Pot

Pot would have continued to have shot fresh Nitre much longer, if I had not had urgent Use for it, to make other Experiments in. However, it is to be observed, that I had already gathered more Nitre than I put into the Pot at first; though, as I said before, for what I could perceive, I had taken all or near all the Nitre that I first put in together with the Sulphur, out of the Pot in a Lump. Hence we may have some Conceptions of the Nature of mineral Earths, and how they grow and increase, when once impregnated with the Seeds of a Mineral. This likewise is a Proof of the Quantity of nitrous Particles with which the Air abounds, since the large Quantity of Nitre which I collected out of the Pot, when left empty upon the Shelf, could be supplied by the Air only.

N. B. These three Experiments are all that I could save intire out of a great Number which were sent to the Hon. Mr. *Boyle*, in Answer to a Letter from him to Mr. *John Clayton*, containing 17 *Quare's*.

ROBERT CORKE.

VII. *Extract of a Letter from John Ratty, M. D. to Dr. Mortimer, Secr. R. S. concerning the Poison of Laurel-Water.*

Dear Doctor,

Dublin, May 17. 1732.

I Received thine of Feb. 15. with the *Transactions* N^o 418 and 420. giving an Account of the Experiments upon *Laurel-Water*. I wish your Experiments

ments with the Milk, had more fully determin'd and ascertain'd it to be an Antidote, than they have yet done. I am inform'd by Dr. ——— that some Apothecaries in *England*, being us'd to sophisticate *Black-Cherry-Water* with *Laurel-Leaves*, will not be persuaded, that this is a Poison on human Bodies, notwithstanding our few Instances. I can now confirm that it really is so by this Story, which thou mayst be assur'd of the Truth of.

At *Lisminy* in *Westmeath*, a Girl of 18 Years old, very well and healthy, took a Quantity, less than two Spoonfuls, of the first Runnings of the *Simple Water of Laurel-Leaves*; whereupon within half a Minute she fell down, was convulsed, foamed at the Mouth, and died in a short time, nor was there any Swelling on her Body.

Printed for T. WOODWARD, at the *Half-Moon*, between the *Two Temple-Gates* in *Fleetstreet*; and C. DAVIS, the Corner of *Pater noster-row*, next *Warwick-lane*; PRINTERS to the ROYAL SOCIETY. M.DCC.XLII.

N. B. The *Philosophical Transactions* being three Years behind-hand in regard of Time, we shall skip over 10 Numbers, and commence the *Transactions* for the current Year 1742. and the XLII^d Volume with N^o 462. and so continue to publish the Papers for the future in the precise Order of the Times of their being read before the *Royal Society*; in the mean time this XLIst Volume, or the *Transactions* from N^o 452. to 462. shall be filled up with all possible Speed.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *April, May and June, 1739.*

The CONTENTS.

- I. *Tentaminis de Mensura & Motu aquarum fluentium, præcedente Transactionum Numero communicati, pars reliqua; Auctore Jacobo Jurin, M. D. Soc. Reg. & Colleg. Medic. Londinens. Sodale.*
- II. *A Collection of the Observations of the Eclipse of the Sun, August 4th 1738. which were sent to the Royal Society.*
- III. *Some Electrical Experiments, chiefly regarding the Repulsive Force of Electrical Bodies; communicated in a Letter from Granvile Wheler, Esq; F. R. S. to Cromwell Mortimer, M. D. R. S. Secr.*
- III. *An Account of some of the Electrical Experiments made by Granvile Wheler, Esq; at the Royal Society's House, on May 11. 1737. drawn up by C. Mortimer, M. D. R. S. Secr.*
- IV. *A Letter from Granvile Wheler, Esq; to Dr. Mortimer, Secr. R. S. containing some Remarks on the late Stephen Gray, F. R. S. his Electrical Circular Experiment.*
- V. *An Account of the Influence which two Pendulum Clocks were observed to have upon each other, by Mr. John Ellicott, F. R. S.*

VI. *Further*

The CONTENTS.

- VI. *Further Observations and Experiments concerning the two Clocks above-mentioned, by the Same.*
- VII. *The Case of a Wound in the Cornea of the Eye being successfully cured by Mr. Thomas Baker, Surgeon to St. Thomas's Hospital, and by him communicated to the ROYAL SOCIETY, in a Letter to Dr. Mortimer, R. S. Secr.*
- VIII. *Extract of a Letter dated at Montpelier, Dec. 27. 1731. N. S. from Andrew Cantwell, M. D. Monspel. to T. S. M. D. and by him translated from the French, giving an Account of a monstrous Boy.*
- IX. *Three extraordinary Cases in Surgery, by Bezael Sherman, Surgeon, at Kelvedon in Essex, communicated in November 1738.*
- X. *A Letter from Thomas Stack, M. D. to Sir Hans Sloane, Bart. Pr. R. S. &c. containing an Account of a Woman sixty-eight Years of Age, who gave Suck to two of her Grand-children.*
-

Printed for T. WOODWARD, at the *Half-Moon*,
between the *Two Temple-Gates* in *Fleetstreet*;
and C. DAVIS, the Corner of *Pater-noster-row*,
next *Warwick-lane*; PRINTERS to the ROYAL
SOCIETY. M.DCC.XLII.

I. *Tentaminis de Mensura & Motu aquarum
fluentium, præcedente Transactionum Numero
communicati, pars reliqua; Auctore Jacobo
Jurin, M. D. Soc. Reg. & Colleg. Medic:
Londinens. Sodale.*

*De Resistencia partium aquæ inter se, ex defectu
lubricitatis oriunda.*

Priusquam ulterius progrediamur, expendenda est ea *Resistencia* fluidorum, quæ oritur ex motu partium eorundem inter se, quamque *Newtono* auctore vocamus *Resistentiam* ex defectu lubricitatis oriundam.

Hanc ille duplicem statuit, alteram quæ oritur ex tenacitate fluidi, alteram quæ fit attritu, seu affricu mutuo partium fluidi inter se.

Priorem, data superficie, uniformem esse censet, seu effectum edere tempori proportionalem; & favent experimenta: posteriorem opinatur augeri in ratione velocitatis, vel ratione paulo minori. Sed de hac nihil diserte statuit, cum desint idonea experimenta.

Diversam autem rationem inter se invicem obtinent hæ duæ *Resistentiæ*, non solum pro diversitate fluidi, quum oleo, ex. gr. aut sevo liquefacto major insit tenacitas quam aquæ, minor attritus; sed etiam in fluido eodem, pro diversa velocitate qua moventur partes fluidi inter se. In dato autem fluido datur necessario certa aliqua velocitas, ubi pares inter se invicem sint hæ *resistentiæ*; & si istam velocitatem experimento reperire liceret, posset in aliis quibuscunque velocitatibus earundem proportio determinari. Experimenta vero nulla habemus, quod sciam, nec facile est ulla excogitare, quorum ope cognosci queat ista velocitas, quæ cæteris pro fundamento inservire possit.

Susplicamur quidem, immo pro verisimili habemus non una de causa, quamminimam in aqua esse velocitatem istam fundamentalem, ubi resistentiæ ex tenacitate & ex affricu oriundæ æquales sunt inter se. Hoc autem concesso, cum crescente velocitate crescat pariter resistentia ex affricu, nullatenus vero crescat resistentia ex tenacitate, patet ultimam hanc resistentiam non nisi parvam admodum rationem obtinere ad priorem, ubi partes fluidi notabili aliqua velocitate moventur inter se; & proinde tuto negligi posse.

Cæterum, sive hac neglecta, alteram solam resistentiam, quæ ex affricu oritur, sive utramque comprehendi oporteat nomine *Resistentiæ* ex defectu lubricitatis oriundæ, leges certe, quibus crescat aut minuat hæc *Resistentia*, non nisi ab experientia sunt petendæ. Sequentes itaque crescendi leges cum ei tribuimus, etsi post diligentem experimentorum hætenus factorum considerationem, magnam veri similitudinem habere videantur, id tamen eo animo facimus, ut si quid certius docuerint experimenta in posterum instituenda, sententiam non inviti mutemus.

HYPOTHESIS.

Resistentia, quæ oritur ex defectu lubricitatis aquæ, est in ratione composita ex tribus sequentibus:

1. Ex ratione superficiæ partium quæ moventur. Hoc, puto, admittunt omnes Philosophi.

2. Ex ratione velocitatis relativæ, qua moventur partes aquæ inter se. Hoc a reliquis, ni fallor, admittitur, nec multum dissentit *Newtonus*.

3. Ex ratione subduplicata altitudinis aquæ. Id nos adsumimus, duce experientia, & aliquatenus etiam auctore

auctore *Newtono*, qui cenſet majori preſſione fieri attritum partium fortiorem, & ſeparationem ab invicem difficiliorem. *Princip. Lib. II. Prop. LII. Schol.*

P R O B L E M A VIII.

Exponere reſiſtentiam partium Cataractæ, quæ oritur ex defectu lubricitatis.

Sit r radius foraminis, A altitudo *Cataractæ*, y radius cujuſlibet ſectionis horizontalis, x altitudo *Cataractæ* ſupra iſtam ſectionem, z radius circuli cujuſvis in iſta ſectione, v velocitas aquæ in centro foraminis.

Erit modo $\frac{v x^{\frac{1}{2}}}{A^{\frac{1}{2}}}$ velocitas aquæ in centro ſectionis, cui radius y . Nam velocitas in centro ſectionis eadem eſt ac ſi ſectio iſta eſſet foramen in fundo vaſis decurtati, cui altitudo x ; adeoque eſt ut $x^{\frac{1}{2}}$ per Corollarium Probl. VI. Erit etiam $\frac{y-z}{y} \times \frac{v x^{\frac{1}{2}}}{A^{\frac{1}{2}}}$ velocitas aquæ in circumferentia circuli, cui radius z ; $\frac{z v x^{\frac{1}{2}}}{y A^{\frac{1}{2}}}$ velocitas relativa; $2 m z x$ ſuperficies cylindri naſcentis, cui radius z , altitudo x ; eritque per tres noſtras poſitiones, *Reſiſtentia* ſuperficiei hujus cylindri, ut $2 m z x \times \frac{z v x^{\frac{1}{2}}}{y A^{\frac{1}{2}}} \times x^{\frac{1}{2}} = \frac{2 m v x x z z}{y A^{\frac{1}{2}}}$.

Conſiderentur jam x , x , & y ut quantitates conſtantes, dum fluit z uſque donec evadit æqualis ipſi y ;

& erit fluxionis $\frac{2m v \dot{x} x z \dot{z}}{y A^{\frac{1}{2}}}$, quantitas fluens
 $\frac{2m v \dot{x} x z^2}{2y A^{\frac{1}{2}}}$, sive $\frac{m v \dot{x} x z^2}{y A^{\frac{1}{2}}}$, sive, (ponendo $z=y$)
 $\frac{m v \dot{x} x y}{A^{\frac{1}{2}}}$, ut resistentia cylindri nascentis, cui radius
 y , altitudo x .

Sed per proprietatem curvæ *Cataracticæ*, $y^4 x = r^4 A$,
 & $y x^{\frac{1}{4}} = r A^{\frac{1}{4}}$: unde *Resistentia* cylindri hujus na-
 scentis erit ut $\frac{m v \dot{x} x r A^{\frac{1}{4}}}{A^{\frac{1}{2}} x^{\frac{1}{4}}}$, sive ut $\frac{m v r \dot{x} x^{\frac{3}{4}}}{A^{\frac{1}{4}}}$; &
Resistentia totius *Cataractæ* erit ut hujus fluxionis
 quantitas fluens, sive ut $\frac{m v r \dot{x} x^{\frac{7}{4}}}{A^{\frac{1}{4}}} \times \frac{4}{7}$, sive, ponendo
 $x=A$, ut $\frac{4}{7} m v r A^{\frac{3}{2}}$. Et cum per *Problema* IV.
 sit $v = 3 q V$, erit *Resistentia* in *Cataracta*, ut
 $\frac{12 q m V r A^{\frac{3}{2}}}{7}$, sive ut $q V r A^{\frac{3}{2}}$. Q.E.I.

COROLL. Cum sit V ut \sqrt{A} , erit *Resistentia* in
Cataracta, ut $q r A^2$.

SCHOLIUM.

In solutione modo exposita, pro superficie taleolæ
Cataracticæ, cui radius z , secundum quam particulæ
 aqueæ se mutuo præterfluunt velocitate relativa æqua-
 bili, adhibuimus superficiem cylindri nascentis, cui
 radius z , altitudo x , sive superficiem $2m z x$, cum
 revera ejus taleolæ superficies sit $2m z x \sqrt{x^2 + z^2}$.

Id autem si corrigatur, inveniatur *Resistentia* super-
 ficiei hujus taleolæ ut $2mz\sqrt{x^2+z^2} \times x^{\frac{1}{2}} \times \frac{zv x^{\frac{1}{2}}}{yA^{\frac{1}{2}}}$

$$= \frac{2mvxz\sqrt{x^2+z^2}}{yA^{\frac{1}{2}}}$$

Cumque, per *Scholium 2. Problematis II.* substan-
 gens curvæ *Cataracticæ* sit $4x$, & tangens ipsa
 $\sqrt{16x^2+z^2}$, erit $4x : \sqrt{16x^2+z^2} :: x : \sqrt{x^2+z^2}$

$$= \frac{x\sqrt{16x^2+z^2}}{4x}$$

Itaque *Resistentia* superficiæ taleolæ erit ut

$$\frac{2mvxz}{yA^{\frac{1}{2}}} \times \frac{x}{4x} \sqrt{16x^2+z^2} = \frac{mvx}{2yA^{\frac{1}{2}}} z\sqrt{16x^2+z^2}$$

$$= \frac{mvxz}{2yA^{\frac{1}{2}}} \text{ in } 4x + \frac{z^2}{2 \times 4x} - \frac{z^4}{8.4x^3} + \frac{z^6}{16 \times 4x^5}$$

$$- \frac{5z^8}{128 \times 4x^7} + \frac{7z^{10}}{256 \times 4x^9} \text{ \&c.} = \frac{mvx}{2yA^{\frac{1}{2}}} \text{ in } 4xz$$

$$+ \frac{z^3}{2 \times 4x} - \frac{z^5}{8 \times 4x^3} + \frac{z^7}{16 \times 4x^5} - \frac{5z^9}{128 \times 4x^7}$$

$$+ \frac{7z^{11}}{256 \times 4x^9} - \frac{21z^{13}}{1024 \times 4x^{11}} \text{ \&c.}$$

Habendo autem quantitates x, x , & y pro constanti-

bus, hujus fluxionis fluens erit, $\frac{mvx}{2yA^{\frac{1}{2}}} \text{ in } \frac{4xz^2}{2}$

$$+ \frac{z^4}{8 \times 4x} - \frac{z^6}{48 \times 4x^3} + \frac{z^8}{8 \times 16 \times 4x^5} - \frac{z^{10}}{256 \times 4x^7}$$

$$+ \frac{7z^{12}}{12 \times 256 \times 4x^9} - \text{\&c.}$$

Et

Et ponendo $z = y$, erit hæc fluens $\frac{m v x}{2 A^{\frac{1}{2}}}$ in $2 x y$

$$+ \frac{y^3}{8 \times 4 x} - \frac{y^5}{48 \times 4 x^3} + \frac{y^7}{8 \times 16 \times 4 x^5} - \frac{y^9}{256 \times 4 x^7}$$

$$+ \frac{7 y^{11}}{12 \times 256 \times 4 x^9} - \&c. \text{ quæ erit ut } \textit{Resistentia} \text{ in}$$

taleola Cataractica, cui radius y , altitudo x .

Hæc autem est ut fluxio *Resistentiæ* in tota *Cataracta*,

& ponendo $y = \frac{r A^{\frac{1}{4}}}{x^{\frac{1}{4}}}$, fit $\frac{m v x}{2 A^{\frac{1}{2}}}$ in $\frac{2 r x A^{\frac{1}{4}}}{x^{\frac{1}{4}}}$

$$+ \frac{r^3 A^{\frac{3}{4}}}{8 \times 4 \times x^{\frac{7}{4}}} - \frac{r^5 A^{\frac{5}{4}}}{48 \times 4^3 \times x^{\frac{17}{4}}} + \frac{r^7 A^{\frac{7}{4}}}{8 \times 16 \times 4^5 \times x^{\frac{27}{4}}}$$

$$- \frac{r^9 A^{\frac{9}{4}}}{256 \times 4^7 \times x^{\frac{37}{4}}} + \&c. = \frac{m v r}{2 A^{\frac{1}{4}}} \text{ in } 2 x x^{\frac{3}{4}} + \frac{r^2 A^{\frac{1}{2}} x^{-\frac{7}{4}}}{32}$$

$$- \frac{r^4 A x^{-\frac{17}{4}}}{48 \times 4^3} + \frac{r^6 A^{\frac{3}{2}} x^{-\frac{27}{4}}}{8 \times 16 \times 4^5} - \&c. \text{ Hujus autem fluxi-}$$

onis quantitas fluens est $\frac{m v r}{2 A^{\frac{1}{4}}}$ in $2 x^{\frac{7}{4}} x^{\frac{4}{7}} + \frac{r^2 A^{\frac{1}{2}} x^{\frac{3}{4}}}{32} x^{-\frac{4}{3}}$

$$- \frac{r^4 A x^{-\frac{13}{4}}}{48 \times 4^3} x^{-\frac{4}{13}} + \frac{r^6 A^{\frac{3}{2}} x^{-\frac{23}{4}}}{8 \times 16 \times 4^5} x^{-\frac{4}{23}} - \&c. \text{ Hæc au-}$$

tem, ponendo $x = A$, fit $\frac{m v r}{2}$ in $\frac{8 A^{\frac{2}{2}}}{7} - \frac{r^2}{3 \times 8 A^{\frac{1}{2}}}$

$$+ \frac{r^4}{12 \times 13 \times 4^3 A^{\frac{5}{2}}} - \frac{r^6}{32 \times 23 \times 4^5 A^{\frac{9}{2}}} + \&c. \text{ five,}$$

$$\frac{4 m v r A^{\frac{3}{2}} \text{ in } 1 - \frac{7 r^2}{3 \times 4^3 A^2} + \frac{7 r^4}{6 \times 13 \times 4^5 A^4} - \frac{7 r^6}{23 \times 4^9 A^6} +$$

&c. quæ est ut *Resistentia* per totam *Cataractam*.

Quod

Quod si altitudo pro infinita habeatur respectu diametri foraminis, erit *Resistentia* ut $\frac{4mvrA^{\frac{3}{2}}}{7}$, prorsus uti definitum est in solutione priori.

Si $A=10r$, *Resistentia* erit ut $\frac{4mvrA^{\frac{3}{2}}}{7} \times 1 = \frac{1}{2743}$ circiter.

Si $A=4r$, *Resistentia* erit ut $\frac{4mvrA^{\frac{3}{2}}}{7} \times 1 = \frac{1}{439}$ circiter.

Potest itaque usurpari $\frac{4mvrA^{\frac{3}{2}}}{7}$ pro mensura *Resistentiæ*, absque periculo sensibilis erroris, etiam ubi altitudo aquæ non superat duas diametros foraminis, & multo magis in altitudine longe majori.

PROBLEMA IX.

Data Mensura aquæ effluentis per datum foramen circulare in medio fundo vasis cylindrici datæ altitudinis, definire Mensuram aquæ effluentis ex alio vase cujuscunque altitudinis datæ per foramen circulare quodcunque datum.

Sit r radius foraminis dati, A altitudo data, $2qmr^2A$ data *Mensura* aquæ effluentis illo tempore, quo casurum in vacuo sit corpus grave per altitudinem A .

Hinc erit, per *Problema IV.* $3q^2mr^2AV$ *Motus* aquæ eodem tempore effluentis: eritque, per *Corollarium Problematis IV.* *Motus* eodem tempore per *Resistentiam* deperditus, $mr^2AV \times 1 = 3q^2$. Hunc itaque *Motum*

Motum vis æqualis *Resistentiæ* generare potest eodem tempore.

Sunt autem *Motus* eodem temporis spatio generati viribus eisdem generantibus proportionales.

Itaque *Motus* $mr^2 AV$, quem hoc tempore generare potest, per *Problema* I. pondus columnæ aquæ $mr^2 A$, cum abest omnis *Resistentia*, est ad *Motum* $mr^2 AV \times \sqrt{1 - 3q^2}$, quem eodem tempore generare potest *Resistentia*, ut pondus $mr^2 A$, ad ipsam *Resistentiam*. Unde *Resistentia* $= mr^2 A \times \frac{mr^2 AV \times \sqrt{1 - 3q^2}}{mr^2 AV}$
 $= mr^2 A \times \sqrt{1 - 3q^2}$.

Eodem modo, ponendo s & E pro radio foraminis, & altitudine novi vasis, & $2pm s^2 E$ pro *Mensura* aquæ effluentis eodem tempore, quo casurum sit in vacuo corpus grave per altitudinem E , habebis *Resistentiam* in novo vase $= ms^2 E \times \sqrt{1 - 3p^2}$.

Sed per *Corollarium Problematis* VIII. sunt ad invicem hæ duæ *Resistentiæ* ut qrA^2 ad psE^2 .

Itaque, $mr^2 A \times \sqrt{1 - 3q^2} : ms^2 E \times \sqrt{1 - 3p^2} :: qrA^2 : psE^2$, five $r \times \sqrt{1 - 3q^2} : s \times \sqrt{1 - 3p^2} :: qA : pE$, five $prE \times \sqrt{1 - 3q^2} = qsA \times \sqrt{1 - 3p^2}$, qua æquatione rite reducta pervenitur ad sequentem,

$$p = \sqrt{\frac{1}{3} + \frac{rE \times \sqrt{1 - 3q^2}}{6qsA}} - \frac{rE \times \sqrt{1 - 3q^2}}{6qsA},$$

vel ponendo $rE = nsA$,

$$p = \sqrt{\frac{1}{3} + \frac{n \times \sqrt{1 - 3q^2}}{6q}} - \frac{n \times \sqrt{1 - 3q^2}}{6q}.$$

Unde

Unde habetur $p \times 2 m s^2 E$, quæ est *Mensura* aquæ effluentis ex secundo vase, quo tempore cadit in vacuo corpus grave per altitudinem E . Q. E. I.

COROLL. 1. Si diametri foraminum fuerint in ratione altitudinum aquæ, eadem erit ratio *Mensurarum*, ac si aqua efflueret sine ulla *Resistentia*.

Nam, si $r : s :: A : E$, $r E = s A$, & $n = 1$, unde

$p = \sqrt{\frac{1}{3} + \frac{1 - 3q^2}{6q}} \min. \frac{1 - 3q^2}{6q}$, & per reductionem $p = q$; unde $2 q m r^2 A : 2 p m s^2 E :: 2 m r^2 A : 2 m s^2 E$, quæ est ratio *Mensurarum*, cum abest omnis *Resistentia*.

COROLL. 2. Si E pro nihilo habeatur respectu altitudinis A , habenda est etiam n pro nihilo, unde

fit $p = \frac{1}{\sqrt{3}}$. Itaque, quo minor capitur altitudo E ,

eo propius vergit p ad $\frac{1}{\sqrt{3}}$.

COROLL. 3. Si s pro infinite magno habeatur respectu radii r , fit $p = \frac{1}{\sqrt{3}}$. Itaque quo major capitur

radius s , eo magis vergit p ad $\frac{1}{\sqrt{3}}$.

PROBLEMA X.

Aqua in aërem effluente determinare rationem inter diametrum foraminis & diametrum venæ contractæ.

Hæc ratio sine experimentorum ope determinari nequit. Est equidem, per *Problemæ VII*,

K

$\xi^2 =$

$$\rho^2 = \frac{qVr^2}{v^2} \times v + 6qV - 2\sqrt{3qV + 9q^2V^2 - 2v^2},$$

unde cognitis q & v definitur ρ .

Sed nulla, quod sciam, habemus experimenta, quibus utramque harum quantitatum q & v metiamur.

Poleni siquidem experimenta *Mensuram* aquæ effluentis exhibent, unde cognoscitur q ; sed distantiam maximam, ad quam fertur aqua ex foramine horizontaliter profiliens, sive distantiam, ad quam pertingit media pars venæ, quæ velocitate v exilit, non designant.

Mariotti vero experimenta altitudinem maximam perpendicularem, ad quam profilit aqua motu fursum verso, sive altitudinem, quam attingit aqua ex media vena profiliens, metiuntur, unde cognoscitur v^2 ; sed non exhibent *Mensuram* aquæ effluentis.

Deficientibus itaque idoneis experimentis, vix licebit rationem eam, quam quærimus, nisi præterpropter determinare. Id autem fiet in modum sequentem.

In *Scholio 2. Problematis VII*, verisimile esse docuimus constantem esse rationem inter hos radios, aut saltem non nisi quamminimum mutari.

Constat autem ex *Mariotti* experimentis discrimen inter altitudinem, quam attingit aqua fursum exiliens, & altitudinem vasis, rationem obtinere duplicatam circiter ipsius altitudinis vasis.

Itaque, si a sit altitudo, ad quam motu fursum verso salire possit aqua fluens per axem venæ cum velocitate v ; erit ex *Mariotti* experimentis, $A-a$ ut A^2 ,

& erit $\frac{A^2}{A-a}$ data quantitas.

Sed in uno experimento, quod pro fundamentali habet *Mariottus*, fuit $A = 60$ digit. Parisiens. & inventa est $a = 59$ digit. Paris. diametro foraminis metiente digitum dimidium. Fuit itaque in hoc casu

$$\frac{A^2}{A-a} = 3600, \text{ cumque data sit hæc quantitas, erit}$$

$$\text{semper } 3600 a = 3600 A - A^2, \text{ vel } a = \frac{3600 A - A^2}{3600}$$

$$= A - \frac{A^2}{3600}.$$

Ergo, si fit $A = 1$ dig. sive dupla diametri foraminis, erit $a = 1 - \frac{1}{3600}$. Sed $v^2 : V^2 :: a : A :: 1 - \frac{1}{3600} : 1$.

Itaque, cum altitudo vasis dupla est diametri foraminis, haberi potest $v^2 = V^2$, vel $v = V$.

Porro, per *Coroll. 4. Probl. IX.* decreſcente E , vergit p ad $\frac{1}{\sqrt{3}}$.

Itaque, cum fit altitudo vasis valde parva, velut si non superet duas diametros foraminis, haberi potest

$$p \text{ vel } q = \frac{1}{\sqrt{3}}.$$

Sed, per *Problema VII,*

$$q^2 = \frac{qVr^2}{v^2} \times v + 6qV - 2\sqrt{3}qV + 9q^2V^2 - 2v^2,$$

& pro v & q substituendo valores eorundem modo

inventos, sive V & $\frac{1}{\sqrt{3}}$, fit

$$q^2 = \frac{r^2}{V\sqrt{3}} \times V + 2V\sqrt{3} - 2\sqrt{V^2\sqrt{3} + 3V^2} - 2V^2.$$

$$= \frac{r^2}{\sqrt{3}} \times \frac{1 + 2\sqrt{3} - 2\sqrt{1 + \sqrt{3}}}{\sqrt{3}}, \text{ five}$$

$$\rho^2 = r^2 \times 2 + \frac{1}{\sqrt{3}} - 2\sqrt{1 + \sqrt{3}} = r^2 \times 0,6687553907$$

unde $\rho = r \times 0,81777466$.

Hic itaque est valor ipsius ρ , cum altitudo aquæ dupla est diametri foraminis; & cum per *Scholium 2. Problematis VII.* ρ constantem obtineat rationem ad radium foraminis, obtinebit eundem valorem in quacunque altitudine aquæ. Q. E. I.

COROLL. I. Per *Problema VII.*

$$R = \frac{\rho}{3} \times \frac{\rho^2 + r}{\sqrt{3}\rho^2 - 2r^2}, \text{ \& ex modo invento va-}$$

lore ipsius ρ , habetur $R = r \times 3,98877150$, qui est valor ipsius R , cum altitudo aquæ dupla est diametri foraminis; cumque per *Scholium 2. ejusdem Problematis*, constans habeatur ratio inter r & R , obtinebit R hunc ipsum valorem, quæcunque fuerit altitudo aquæ.

COROLL. II. Quoniam v est fere æqualis ipsi V , & q est fere $= \frac{1}{\sqrt{3}}$, ubi altitudo aquæ dupla est diametri

foraminis; erit ad hanc altitudinem aquæ, $\frac{v}{qV} = \sqrt{3}$ quamproxime. Et cum, per *Scholium 2. Problematis VII.*, constans sit ratio inter v & qV , erit $\frac{v}{qV} = \sqrt{3}$, quæcunque fuerit aquæ altitudo.

PROBLEMA XI.

Aqua ex dato vase semper pleno per datum foramen in aërem effluente, & data una quavis ex tribus quantitativibus sequentibus, nempe Mensura aquæ effluentis, velocitate in axe venæ contractæ, aut altitudine, ad quam motu sursum verso salire possit media pars venæ, reliquas duas determinare.

Sit A altitudo vasis, r radius foraminis, $2 q m r^2 A$ Mensura aquæ effluentis, v velocitas in axe venæ contractæ, a altitudo, ad quam salire queat aqua effluens per axem venæ, & detur primo $2 q m r^2 A$, unde datur q .

Per Corollarium 2. Problematis X. $\frac{v}{qV} = \sqrt{3}$,
unde $v = qV\sqrt{3}$. Hinc $v^2 = 3q^2V^2$. Sed
 $V^2 : v^2 :: A : a = \frac{v^2 A}{V^2} = \frac{3q^2V^2 A}{V^2} = 3q^2 A$.

Si secundo detur v , erit $q = \frac{v}{V\sqrt{3}}$, &

$$2 q m r^2 A = \frac{2 m r^2 A v}{V\sqrt{3}}$$

$$\text{Porro } a = \frac{v^2 A}{V^2}.$$

Postremo, si detur a , cum sit $a = 3q^2 A$, erit $q^2 = \frac{a}{3A}$,

$$\& q = \sqrt{\frac{a}{3A}}$$

Item $v^2 = \frac{aV^2}{A}$, unde $v = V\sqrt{\frac{a}{A}}$. Q. E. I.

PRO-

PROBLEMA XII.

Data altitudine, ad quam motu sursum verso salit aqua per aërem erumpens ex vase altitudinis datæ per datum foramen circulare, definire altitudinem, ad quam aqua motu sursum verso ascensura sit, cum erumpit ex vase cujuscunque altitudinis datæ per foramen circulare quodcunque datum.

Significentur literis, r, s, A, E, q, p , res eadem atque in *Problemate IX*; sintque a & e altitudines, ad quas salire queat aqua erumpens ex vasis, quibus altitudines A & E respective.

Erit jam, per *Problema XI*, $a = 3q^2 A$, $e = 3p^2 E$,
unde $3q^2 = \frac{a}{A}$, $1 - 3q^2 = \frac{A-a}{A}$, $q = \sqrt{\frac{a}{3A}}$, $p = \sqrt{\frac{e}{3E}}$,

$$\& p^2 = \frac{e}{3E}$$

Cumque, per *Problema IX*, sit

$$p = \sqrt{\frac{1}{3} + \frac{rE \times 1 - 3q^2}{6qsA}}^2 - \frac{rE \times 1 - 3q^2}{6qsA}, \text{ vel}$$

$$\text{ponendo } rE = nsA, p = \sqrt{\frac{1}{3} + n \times \frac{1 - 3q^2}{6q}}^2 - n \times \frac{1 - 3q^2}{6q};$$

hinc substituendo $\frac{A-a}{A}$ pro $1 - 3q^2$, & $\sqrt{\frac{a}{3A}}$ pro q ,
ac pro $A-a$ scribendo α , evadet

$$p = \frac{\sqrt{4Aa + n^2\alpha^2} - n\alpha}{2\sqrt{3Aa}}, \&$$

$$p^2 = \frac{2Aa + n^2\alpha^2 - n\alpha\sqrt{4Aa + n^2\alpha^2}}{6Aa};$$

Sed

Sed $p^2 = \frac{e}{3E}$, unde $\frac{e}{E} = \frac{2Aa + n^2\alpha^2 - n\alpha\sqrt{4Aa + n^2\alpha^2}}{2Aa}$,

sive $e = E \times \frac{2Aa + n^2\alpha^2 - n\alpha\sqrt{4Aa + n^2\alpha^2}}{2Aa}$, unde

scribendo ε pro $E - e$, fit $\varepsilon = \frac{nE\alpha}{2Aa} \times \sqrt{4Aa + n^2\alpha^2} - n\alpha$.

Data autem ε , sive $E - e$, datur e , sive altitudo ad quam aqua fertur, cum ex novo vase erumpit. Q. E. I.

COROLL. 1. Si æqualia fuerint foramina in utroque vase, seu $s = r$, erit $E = nA$, vel $n = \frac{E}{A}$ unde $\varepsilon = \frac{n^2\alpha}{2a} \times \sqrt{4Aa + n^2\alpha^2} - n\alpha$.

COROLL. 2. Si æquales fuerint vasorum altitudines, seu $E = A$, erit $r = ns$, seu $n = \frac{r}{s}$, unde

$$\varepsilon = \frac{n\alpha}{2a} \times \sqrt{4Aa + n^2\alpha^2} - n\alpha.$$

COROLL. 3. Si diametri foraminum fuerint in ratione altitudinum, salient aquæ ad altitudines ipsis vasorum altitudinibus proportionales. Nam, si $r : s ::$

$A : E$, $rE = sA$, & $n = 1$, unde $\varepsilon = \frac{E\alpha}{A}$, seu $\varepsilon : \alpha :: E : A$, vel $E - e : A - a :: E : A$, sive $e : a :: E : A$.

COROLL. 4. Cum fit $p \times 2\sqrt{3Aa} = \sqrt{4Aa + n^2\alpha^2} - n\alpha$, erit $\varepsilon = \frac{nE\alpha}{2Aa} \times 2p\sqrt{3Aa} = \frac{pnE\alpha\sqrt{3}}{\sqrt{Aa}}$, unde pro \sqrt{a}

substituendo ejus valorem suprapositum, $q\sqrt{3A}$, & reductione debita, fit $\varepsilon = \frac{pnE\alpha}{qA}$, sive $\varepsilon = \frac{prE^2\alpha}{qsA^2}$.

COROLL.

COROLL. 5. Hinc autem ponendo $p=q$, $\varepsilon = \frac{rE^2\alpha}{sA^2}$,
 five $\varepsilon : \alpha :: rE^2 : sA^2$. Hoc est, defectus altitudinum aquarum salientium, five discrimina inter altitudines salientium, & altitudines vasorum, sunt in ratione composita ex ratione duplicata altitudinum vasorum directe, & ratione diametrorum foraminum reciprocé. Hæc autem regula accuratè vera est, ubi $sA=rE$, per *Coroll. 1. Probl. IX*; & proxime ad verum accedit, ubi E & s in eadem circiter ratione augentur, vel minuuntur; nec nisi paulum aberrat a vera aquæ salientis altitudine in quocunque casu, modo E non sit major pedibus 50, & eodem tempore s non sit minor lineis 3.

COROLL. 6. Ubi $s=r$, $\varepsilon = \frac{E^2\alpha}{A^2}$ circiter, hoc est, ubi paria sunt foramina, defectus altitudinum aquarum salientium sunt fere in ratione duplicata altitudinum vasorum, quæ est ipsa *Mariotti* regula.

COROLL. 7. Ubi $E=A$, $\varepsilon = \frac{r\alpha}{s}$ circiter, hoc est, ubi pares sunt altitudines vasorum, defectus aquarum salientium sunt fere ut diametri foraminum reciprocé.

SCHOLIUM GENERALE I.

Theoriæ supratraditæ fidem si quis experimentis instituendis explorare voluerit, ei auctor sim,

I. Vase uti amplissimo, saltem in parte superiori, eum in finem ut toto tempore, quo capitur experimentum, altitudo aquæ ad sensum non mutetur.

Quod

Quod si vas ita amplum non sit, quin durante effluxu ex foramine decrementum altitudinis aquæ notatu dignum reperiatur, habenda est pro constanti altitudine altitudo debita intermedia inter maximam & minimam aquæ altitudinem; quod fieri præstat, quam motum aquæ naturalem perturbare affundendo desuper aquam novam.

2. Vasis altitudo tanta sit, ut si aquam per foramen in latere factum emittere velis, velocitas aquæ per centrum foraminis exituræ tuto haberi possit pro velocitate quacum aqua per totum foramen exitura sit, cum abest omnis resistentia.

3. Lamina, in quo sit foramen, tam tenuis sit, aut saltem acie tam tenui in ambitu foraminis, ut ejus aciei crassities pro nihilo haberi possit respectu diametri foraminis. Debet autem recidi crassities laminæ facie externa, relicta plana facie interiore proxime aquam: & angulum hujus aciei tam acutum esse oportet, ut aqua per foramen effluens lateri exteriori laminæ non adhærescat.

His paratis sequentia institui poterunt experimenta, quibus quasi totidem criteriis de certitudine doctrinæ suprapositæ dijudicari queat.

EXPERIMENTUM I. Cum aqua per foramen in latere vasis emittitur, mensuretur diligentissime diameter venæ contractæ, notando utrum semper sibi constet mutata utcunque altitudine aquæ.

EXPER. 2. Observetur etiam utrum hæc diameter eandem semper obtineat rationem ad diametrum foraminis, cum foramina diversæ magnitudinis usurpantur.

EXPER. 3. Aqua effluente vel recta deorsum per fundum vasis, vel horizontaliter per latus ejusdem,

observetur accuratissime quantum effluat dato tempore, adhibendo diversas altitudines aquæ, sed unum idemque foramen.

EXPER. 4. Idem observetur, cum foramina diversæ magnitudinis usurpantur, sed eadem adhibetur altitudo aquæ.

EXPER. 5. Observandum quantum effluat dato tempore in casibus duobus diversis, quorum in utroque eadem sit ratio diametri foraminis ad altitudinem aquæ. Nam si *Mensuræ* reperientur in ratione composita ex ratione duplicata diametrorum, & ratione simplici altitudinum, ut in *Coroll. 3. Problematis IX*, magnam habebis Theoriæ nostræ confirmationem.

EXPER. 6. In iisdem duobus casibus, motu aquæ sursum verso ope tubi ampli lateri vasis adaptati, & superiori parte foramine pertusi, observetur ad quantas altitudines aqua saliat. Nam si reperientur hæ altitudines proportionales altitudinibus aquæ in vase, ut in *Corollario 3. Problematis XII*, habebis alteram hujus Theoriæ certissimam confirmationem.

EXPER. 7. Eodem manente foramine, sed mutata utcunque altitudine aquæ, observandum ad quantam altitudinem feratur aqua.

EXPER. 8. Idem observetur, cum eadem persistente altitudine aquæ mutatur foraminis magnitudo.

Cæterum, ex omnibus his experimentis præferenda sunt ea, quibus motu aquæ sursum verso notatur altitudo ad quam aqua salit. Hæc enim altitudo & facilius longe capi potest, quam *Mensura* aquæ effluentis, & error, si quis forte admittatur in capienda altitudine, longe minoris est momenti, quam qui admittitur in *Mensura* æstimanda. Cum enim, per
Pre-

Problema XI, altitudo aquæ salientis fit $3q^2 A$, patet, quod error minimus admissus in *Mensura*, sive in q , duplicabitur fere in q^2 , adeoque duplicabitur in altitudine aquæ salientis.

At minimus error admissus in altitudine aquæ salientis, sive in $3q^2 A$, redigitur fere ad dimidium in æstimandâ q , hoc est, in *Mensura* aquæ effluentis.

SCHOLIUM GENERALE II.

Interim, dum ab iis, quibus otium non minus quam veri cognoscendi studium suppetit, fiant aliquando ista experimenta, utendum, quantum fieri potest, iis experimentis quæ nobis suppeditavit antecessorum diligentia.

Hæc autem sunt triplicia. Nam metiuntur vel,

1. Diametrum venæ contractæ; vel,
2. *Mensuram* aquæ effluentis; vel,
3. Altitudinem ad quam aqua salit.

1. Venæ contractæ radius mensurante *Newtono* est $r \times 0,84$, cum diameter foraminis est $\frac{1}{8}$ digiti *Londinensis*.

Idem *Poleno* dimetiente est $r \times 0,78$ circiter, cum diameter foraminis est digitorum *Parisiensium* $2\frac{1}{8}$.

Per calculum nostrum est $r \times 0,818$ fere, quæcunque fuerit diameter foraminis. Quæ magnitudo est intermedia circiter inter mensuram *Newtonianam* & *Polenianam*.

2. Perincommode accidit, ut *Mensura* aquæ effluentis ab omnibus captæ, præter unum *Polenum*, ad propositum nostrum penitus sint inutiles. Nam docente viro illo eximio, hæc *Mensura*, cum per tubum exit aqua, longe major est quam cum exit ex nudo

foramine. Et cum foramina in laminis facta pro tubis brevibus habenda sint, saltem nisi laminæ craftities quamminima sit respectu diametri foraminis, inde factum est, ut omnes *Mensuræ* aquæ effluentis ante illum captæ majores veris invenirentur.

Utendum ergo solis *Mensuris* a *Poleno* captis. Hæ^a autem, quæ quidem magno illo foramine 26 linearum captæ fuerunt, sunt numero decem, nempe ponendo corpus grave cadere in vacuo per pedes Parisienses 15, digitum 1, lineas 2, tempore minuti secundi, evadit *Mensura*

1 ^a = 2 m r ² A × 0,5772	Quarum omnium inter-
2 ^a 0,5772	media est 2 m r ² A × 0,571
3 ^a 0,5731	fere. Hanc itaque habemus
4 ^a 0,5710	pro <i>Mensura Poleniana</i> aquæ effluentis, cum
5 ^a 0,5690	vasis altitudo est digitorum
6 ^a 0,5675	33 <i>Parisiensium</i> , quæ est
7 ^a 0,5689	altitudo intermedia inter
8 ^a 0,5703	eas quæ <i>Poleno</i> fuerunt
9 ^a 0,5732	usurpatæ.
10 ^a 0,5613	
	5,7087

Mensura autem, quæ ad hanc altitudinem calculo nostro elicitur ex fundamentali *Mariotti* experimento, quod mox proponemus, est 2 m r² A × 0,5768; quæ parte circiter nonagesima octava superat *Mensuram Polenianam*. Tantulum vero discrimen oriri potuit vel ex errore centesimæ partis digiti in æstimanda dia-

^a Polenus de Castellis, art. 35, 38, 39, 42, 43. & Epistol. ad Mari-
nonium.

metro foraminis; vel ex eo, quod vas excipiens aquam effluentem, centesima circiter parte majus esset quam pro computo *Poleni*; vel partim ex utroque. Adde, quod duplo minus est hoc discrimen, quam quantum reperitur inter ipsa experimenta *Poleniana*.

3. Supra docuimus inutilia reddidisse *Polenum* omnia antecessorum experimenta de *Mensura* aquæ effluentis, quod in iis instituendis nulla habita fuisset ratio crassitie laminæ, per cujus foramen aqua efflueret. Undè possit aliquis non absurde suspicari, laborare pari vitio etiam illa experimenta, quibus exploratum fuerat ad quantam altitudinem aquæ salirent. Sed dubitationem istam altera egregia observatione sustulit *Polenus*. Is siquidem deprehendit *Mensuram* quidem aquæ longe majorem ex tubo, quam ex nudo foramine effluere; at, quod mireris, quodque nos forsitan, si modo Deus vitam & otium concesserit, aliquando explicabimus, aquam effluentem per tubos 7^a vel 13 lineas Parisienses longos, non nisi ad eandem, aut etiam tantillo minorem profilire distantiam horizontalem, quam attingit aqua ex nudo foramine exiliens. Tantillo itaque minor est velocitas maxima aquæ post exitum e tubo, quam post exitum e foramine, cum tubus non admodum brevis est: sed cum tubus est brevissimus, qualis est foramen etiam in lamina non admodum tenui, eadem haberi potest velocitas maxima aquæ post exitum ex hoc tubo, atque post exitum ex foramine in lamina tenuissima.

Itaque, ad explorandam Theoriæ nostræ certitudinem, licet æque nobis uti experimentis *Mariotti* de

^a Epistol. ad Marinonium.

altitudine fontium salientium, atque si foramina, quibus is usus est, in laminis tenuissimis facta fuissent.

Adsumamus ergo ex ejus experimentis unum aliquod, quod pro fundamento habeatur, ad altitudinem in reliquis experimentis per *Problema* nostrum XII indagandam.

Is quidem pro experimento fundamentali proponit istud, ubi altitudo aquæ in vase est præcise pedum 5 *Parisiensium*. At cum tantillus error, puta duarum linearum, in hoc experimento, errorem satis grandem, nempe plusquam 8 digitorum, gignat in altitudine septies majori, quali postea utitur *Mariottus*; nos illud experimentum pro fundamentali habere malumus, in quo maxima illa adhibetur altitudo septies major priori.

Sit itaque nobis pro fundamento examinis instituendi experimentum id *Mariotti*, ubi diameter foraminis est linearum *Parisiensium* 6, & altitudo aquæ in vase est pedum *Parisiensium* 34, digitorum $11\frac{1}{2}$, sive digitorum $419\frac{1}{2}$.

Hanc ille cum altitudinem adhiberet, reperit aquam ex foramine exilientem adsurgere ad altitudinem pedum 31 digitorum 8 vel 9, hoc est, ad altitudinem digitorum $380\frac{1}{2}$.

Est itaque $A=419,5$ dig. $a=380,5$ dig. & $\alpha=39$ dig.

In altero experimento, ubi *E*, seu altitudo aquæ in vase est pedum 26 digiti 1, salit aqua per idem foramen teste *Mariotto* ad altitudinem pedum 24 digitorum $2\frac{1}{2}$. Prodit vero *e*, seu altitudo aquæ salientis, per *Corollarium* 1. *Problematis* XII, pedum 24, digitorum 3.

Cæterum, quo melius conferantur altitudines, quas attingere aquam salientem deprehendit *Mariottus*; cum altitudinibus iis, ad quas salire debuerit ex calculo nostro, utraque coniecimus in Tabellam I; ubi vides ita convenire calculo cum observatis, ut vix quicquam possit supra. Cumque capta sint hæc experimenta eodem foramine diametro sex linearum, altitudine sola mutata, vix potest dubitari, quin tertia nostra positio, qua *Resistentia, cæteris paribus*, est in ratione subduplicata altitudinis, recte se habeat.

TABELLA I.

Diameter foraminis 6 linearum.

Altitudo aquæ in vase	Altitudo salientis aquæ	
	Ex Mariotto	Ex calculo
ped. dig.	ped. dig.	ped. dig.
34. 11,5	31. 8,5	31. 8,5
26. 1	24. 2,5	24. 3
24. 5	22. 10	22. 10
12. 4	12. 0	11. 11
5. 6	5. 4,75	5. 5
5.	4. 11	4. 11. 2 lin.
35. 5	32. 0	32. 1

TABELLA II.

Diameter foraminis 4 linearum.

ped. dig.	ped. dig.	ped. dig.
34. 11,5	30. 0	30. 0
24. 5	22. 8,5	21. 11
5. 6	5. 4,7	5. 4,4

TABELLA III.

Diameter foraminis 3 linearum.

ped. dig.	ped. dig.	ped. dig.
34. II, 5	28. 0	28. 0
26. I	22.	22. I
24. 5	22. 2	20. II
5. 6	5. 4,7	5. 3,7

Cum loco foraminis linearum sex uteretur *Mariottus* foramine linearum quatuor, reperit aquam profilientem ex vase altitudinis supra demonstratæ, pedum 34 digit. $II \frac{1}{2}$, attingere altitudinem pedum 30. Salire debuit per *Corollarium 2. Problematis XII* ad pedes 30. digitos $2 \frac{1}{2}$ fere.

Postea cum uteretur foramine linearum trium, aqua profiliens ex eodem vase attingit altitudinem pedum 28. Profilire debuit per idem *Corollarium* ad pedes 28, digitos 9 circiter.

Sed hæc discrimina inter altitudines ex calculo prodeuntes & eas quas observavit *Mariottus*, ex parvo errore in capiendis foraminum tantulorum diametris oriri potuerunt.

Nam si radius maximi foraminis, quem lineis tribus æqualem statuit *Mariottus*, tres lineas superaverit parte $\frac{1}{100}$ digiti *Parisiensis*; vel si radius secundi foraminis, quem lineis duabus æqualem facit, *Mariottus*, parte $\frac{1}{156}$ digiti *Parisiensis* a duabus lineis defecerit; in alterutro casu saliet aqua per calculum ad altitudinem 30 pedum, prorsus uti observavit *Mariottus*.

Item, si radius minimi foraminis, parte $\frac{1}{100}$ digiti *Parisiensis* minor fuerit linea $1 \frac{1}{2}$; & simul radius maximi foraminis parte $\frac{1}{100}$ digiti superet tres lineas; dabit

dabit calculus altitudinem aquæ salientis pedum 28, quantam deprehendit *Mariottus*.

Calculo autem ad hunc modum correcto, exhibent Tabellæ 2^a & 3^a altitudines *Mariotti* cum calculo nostro collatas.

Hic autem notandum est, in Tabella II. altitudinem salientis ex vase alto pedes 24. digitos 5. *Mariotto* observatam, nempe altitudinem pedum 22. dig. 8 $\frac{1}{2}$, item in Tab. III. altitudinem salientis ex eodem vase, nempe altitudinem ped. 22. digit. 2. altitudines, quas exhibet calculus noster, magno intervallo superare.

Sed corruptos esse *Mariotti* numeros satis constat. Nam,

1. Regula *Mariottiana* supratradita, cui satis bene convenire cum observatis ipse testatur, numeros multo minores, & satis ad calculum nostrum accedentes exhibet.

2. Fieri omnino nequit, ut aqua saliens ex foramine 4 linearum attingat altitudinem ped. 22. dig. 8 $\frac{1}{2}$; neque ut aqua saliens ex foramine trium linearum attingat altitudinem pedum 22. dig. 2; si quidem aqua saliens ex foramine 6 linearum non attingat nisi altitudinem ped. 22. dig. 10. quod ex analogia observationum *Mariotti* facile patebit.

3. Si vera sit altitudo ped. 22. dig. 2. in Tab. III. salit aqua erumpens ex vase alto ped. 24. dig. 5. ad majorem altitudinem, quam ubi erumpit ex vase alto ped. 26. dig. 1. quod manifeste absurdum est.

His causis adducor ut credam *Mariottum*, ubi de priori ex his experimentis verba faceret, in adversariis suis scriptum reliquisse, *Le jet de quatre lignes n' a été plus bas que d' onze pouces ou onze pouces &*

demi, que celui dont l'ajutage étoit de six lignes; undè transcripserit *De la Hirius, plus bas que d'un pouce ou un pouce & demi.* Facta autem hac correctione erit altitudo *Mariotto* observata 21 pedum, & digitorum 11. vel $10\frac{1}{2}$, quæ cum calculo nostro adamussim convenit.

In secundo experimento, cum erumpit aqua ex foramine tres lineas amplo, patet ex analogia salire aquam debere ad altitudinem duobus circiter pedibus minorem, quam ubi erumpit ex foramine sex linearum. Forte, loco verborum *celui de trois lignes a été plus bas que celui de six lignes* de pres de 8 pouces, scriptum fuerat *Mariotto, plus bas que celui de six lignes d'un pied & 8 pouces*, quod non longè distat a calculo nostro.

Id vero mirum non videbitur, ejusmodi errata contingere potuisse, si animadverteris ipsum Cl. *De la Hirium*, qui, post obitum *Mariotti*, ejus chartas imprimendas curaverit, in præfatione huic operi præfixa hæc habere. *La moitié de cet ouvrage étoit assez au net pour être imprimé: mais le reste m'a donné beaucoup de peine à rassembler sur les Memoires qui m'en ont été mis entre les mains après sa mort.*

Cæterum, omnibus perpensis, adeo bene convenit calculo nostro cum experimentis clarissimi hujus & diligentissimi observatoris, ut etiam cum *Mensura Poleniana* aquæ effluentis, cumque mensuris diametri venæ contractæ *Newtono* & *Poleno* captis, ut vix dubitandum sit quin aut vera, aut vero quamproxima sit supra exposita theoria.

Hæc autem facile extenditur ad aquam effluentem per foramen quadratum, aut rectangulare quodvis,
vel

vel etiam ad foramen annulare, quale ambit circellum Newtonianum Corollariis ultimis *Prop.* XXXVI. *Libr.* II. *Princip.* adhibitum, unde in Resistentia fluidorum continuorum ex hujus circelli contemplatione deducta plura videntur mutanda; quod in antecessum eruditos monere visum est, quo eos ad accuratius præcedentium examen excitarem.

II. *A Collection of the Observations of the Eclipse of the Sun, August 4th 1738. which were sent to the Royal Society.*

I. *An Eclipse of the Sun, observed August the 4th 1738. by Mr. George Graham and Mr. Short, FF. R. S. at Mr. Graham's House in Fleetstreet, London, by a Refracting Telescope of 12 Feet Focus, armed with a Micrometer, and by a reflecting Telescope of nine Inches focal Length.*

	h.	'	"	
Beginning of the Eclipse at	9.	59.	29	A. M.
End at	11.	59.	36	
Quantity of Obscuration by } the Micrometer . . . }	dig.	min.		
	3.	28.		
	h.	'	"	
Duration	2.	0.	16	

N. B. The Person, who was observing the Transit of the Sun over the Meridian, observed the End to be at the same Instant with the above Observation.

2. *Eclipsis Solis, Tubo 7 ped. Micrometro D. Gra-
hami instructo, d. 4 Aug. paulo post meridiem 1738.
Upsalix observata à D^{no}. Andrea Celzio, R. S.
Lond. S. & R. S. Suec. Secr.*

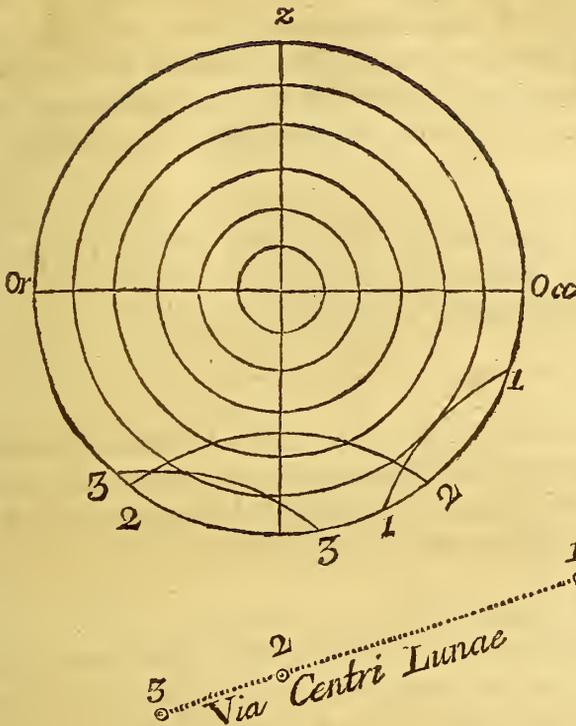
Temp. ver.					
h.	'	"			
12.	18.	52	.	.	Initium eclipseos.
12.	35.	57	.	.	Dig. eccl. o. $5\frac{2}{3}$
12.	37.	47	.	.	O. $3\frac{1}{2}$
12.	42.	22	.	.	Finis.
0.	23.	30	.	.	Duratio.

Propter nubes per vices solem obtegentes ma-
ximam obscurationem & ceteras eclipseos phases ob-
servare non licuit; maximum tamen solis defectum
dig. o. 8' h. 12. 30' 37" accidisse ex hisce observatis
deducitur.

3. *Tres Phases Eclipseos Solis partialis Vitem-
bergæ die xv. Augusti St. N. iv. St. Vet. anno
C10.1000.XXXVIII. à Jo. Frid. Weidlero, R. S.
Lond. Sodal. observatæ.*

ET si propter nubes, quibus tum cœlum involve-
batur, nec initium, nec finis deliqui spectari
potuerit, apparuerunt tamen, distractis subinde venti
impetu nubibus, Phases sequentes;

Hor.



Hor. Min.

11. 30. Notata est phasis prima deliquii crescentis, 1 digit.
 12. 19. *p. m.* visa est phasis altera 2 digit. 30 Minutorum.
 12. 37. Apparuit phasis decrefcentis eclipsis tertia.

Spectatæ

Spectatæ etiam sunt eodem tempore maculæ in disco Solis decem.

Ceterum Lunæ discus sub Sole, peripheriam accurate terminatam, absque ulla inæqualitate, necnon faciem nigerrimam ostendit. Nullum quoque Atmosphæræ orbi Lunæ insidentis vestigium potuit apprehendi.

Aberravit calculus, ex Tabulis *Ludovicianis* deductus, quoad magnitudinẽ & tempus summæ Eclipses. Magnitudo enim prædicebatur 2 digitorum, 20 minutorum; Medium h. 12. m. 5.

4. Defectus Solis, *observatus e specula Bononiensis Scientiarum Instituti die 15 Augusti M.DCC.XXXVIII. mane, referente Eustachio Manfredio ejusdem instituti Astronomo, & R. S. Lond. S.*

CUM Solis discus per hosce dies maculis pluribus scateret, ipso mane instantis Eclipses circiter horam 21. 30 post meridiem, *Eustachius Zanottus, Phil. Doct. Math. Professor publicus, Collega meus,* ope micrometri aptati tubo pedum 8, præcipuarum positum investigavit, quæ præsertim Australem Solis partem, (qua parte Luna subitura erat) obsidebant; omnes enim describi neque ad rem attinebat, neque per spectatorum turbas licuit. Eas maculas, quarum loca definire potuit, subjectum schema exhibet.

Initium deliquii non persenssi ante horam 22. 52. 25 post meridiem, licet & ego tubo pedum undecim, & alii tubis aliis Solis margines diu perlustrassent. Opinor tamen ipsum Luminarium contactum minuto saltem maturius accidisse, quam animadverterim; quod ipsum a succedentibus phasibus confirmari videtur.

Digiti Ecliptici per circulos in tabella de more exaratos, digitorum vero partes æstimatione definitæ sunt. Telescopium erat pedum 6. Imago unciarum 2, aut circiter. Phases emerfionis phasibus immerfionis certiores sunt multis de causis.

Immerfionis phases.			Emerfionis phases.			
Temp. ver.			Temp. ver.			
h	'	"	h	'	"	
23.	o.	10	Defectus unius digiti	o.	4. 14	adhuc dig. $4\frac{4}{5}$
	11.	20	dig. 2	18.	5	dig. $4\frac{1}{2}$
	23.	56	dig. 3	22.	43	dig. $4\frac{1}{3}$
	35.	14	dig. 4 dub.	31.	50	dig. 4
	45.	14	dig. $4\frac{1}{3}$	39.	13	dig. $3\frac{1}{2}$
	47.	6	dig. $4\frac{1}{3}$	46.	50	dig. 3
	51.	14	dig. $4\frac{2}{3}$	52.	55	dig. $2\frac{1}{2}$
	55.	14	dig. $4\frac{2}{3}$	57.	31	dig. 2
	58.	14	dig. $4\frac{1}{4}$	I.	3. 26	dig. $1\frac{1}{2}$
o.	I.	46	dig. $4\frac{4}{5}$	7.	52	dig. I
				I.	13.	4 semidigitus
Finis Eclipsæo Tubo pedum } hor. I. 18. I						
undecim } hor. I. 18. 2						
Tubo pedum octo			hor. I. 18. 2			

Interea maculæ Solis a Luna obteſtæ ac reſeſtæ notabantur in hunc modum.

Temp. ver.

h ' "

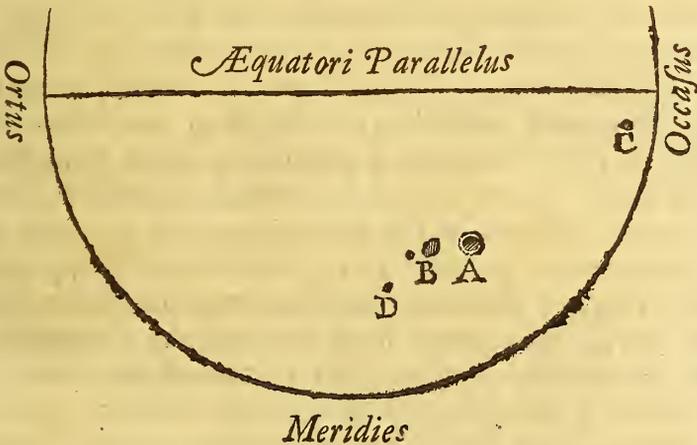
23. 3. 50 Macula *C* Lunam ſubit, tubo pedum 8.
 21. 3 Macula *A* deliteſcere incipit, tubo pedum undecim.
 21. 49 Maculæ *A* centrum occultatur.
 22. 41 Tota ſub Luna immergitur.
 23. 54 Duarum Macularum ad *B* prior immergi incipit.
 25. 10 Ejuſdem Maculæ centrum latet.
 25. 45 Tota abſconditur.
 26. 24 Duarum ad *B* poſterior centro ſuo Lunæ marginem ſubit. Haſtenus eodem teleſcopio pedum undecim.
 27. 2 Macula *D* abſcondi incipit tubo pedum 8.
 23. 31. 2 Tota deliteſcit eodem tubo.
 o. 31. 45 Macula *A* apparere incipit in ſpecie Solis per tabellam excepta.
 32. 30 Eadem macula tota cum areola ſua emerſerat tubo pedum undecim.
 33. 25. Emerſio centri prioris duarum ad *B* eodem tubo.
 34. 59 Totalis emerſio ejuſdem Maculæ eodem tubo.
 35. 51. Poſterior duarum ad *B* tota emerſerat, tubo eodem.

Obſervationes tam macularum, quam digitorum eclipticorum habuerunt (præter *Zanottum*) *Franciſcus Vandellius Mutinensis Inſtit. Scient. Profeſſor,*
Thomas

*Thomas Perellus Florentinus, M. D. Joseph. Rover-
sius, Petronius Matheucius, Jo. Andreas Boldrinus
Placentinus, Salvator Oliva Mediolanensis, atque
alii. Omnes ex eodem horologio tempora notârunt,
quæ postmodum à meridianis observationibus correctâ
consignavimus.*

Vigente Eclipsi observavi transitum Lunæ ac Solis
per planum semicirculi muralis juxta meridianum
suspensi.

Ad definiendum Lunæ transitum, tempus notavi,
quo segmentum perexiguum e disco Lunæ in Sole
conspicuo filo horizontali telescopii subtensum, a ver-
ticali filo bissectum apparuit : tunc enim oportet ipsum
Lunæ centrum in verticali extitisse. Transitit autem
centrum Lunæ ante centrum Solis secundis horariis
34. hoc est hora 23. 59. 26 post meridiem diei 14.
Altitudo Meridiana limbi borei Lunæ grad. 59. 36.
15 ; limbi borei Solis 59. 53. 0.



III. *Some Electrical Experiments, chiefly regarding the Repulsive Force of Electrical Bodies; communicated in a Letter from Granville Wheler, Esq; F. R. S. to Cromwell Mortimer, M. D. R. S. Secr.*

S I R,

THE following Experiments I made in the Autumn of the Year 1732. and repeated them to Mr. *Grey* the following Summer, when he came into the Country. I had then Thoughts of communicating them to you through his Hands, to whom they owe their Being, and drew them up with a Letter to him prefixed: But, unwilling to be an Author, I deferred the Communication from time to time, till the second Summer came, when I was informed, that Mr. *Dufay* had written a Letter to his Grace the Duke of *Richmond*, dated *Dec. 27. 1733.** wherein he takes Notice of the same Solution of the Repulsive Force, a Copy of which Letter I received the *September* following, inclosed in one from Mr. *Grey*. All Thoughts of publishing them were then laid aside, but meeting some time since with a fuller Account of them in the *Memoirs of the Academy of Sciences* for the Year 1735. by which it appeared Mr. *Dufay's* Experiments were not the same with my own, and having since received the Commands of our worthy President to communicate them, I take the Liberty at last of sending them to you, as I intended they should have passed through Mr. *Grey's*

* *Philosophical Transactions*, N^o 431. p. 258.

Hands,

Hands, if Mr. *Dufay* had not appeared upon the same Subject, in three Propositions, and a few Corollaries. I am, Sir, with a very great Regard,

Ottesden-Place,
Jan. 17. 1737-8.

Your much obliged,
humble Servant,

Granv. Wheler.

P R O P O S I T I O N I.

Bodies made Electrical, by communicating with an Electrical Body excited by Friction, are in a State of Repulsion with regard to such excited Bodies.

E X P E R I M E N T I.

I hung a fine white Thread by a Loop, to an horizontal blue Silk Line, about four Feet long, tied at each End, and at about a Foot distance from it, placed a Glass Tube two Feet and a half long nearly, and one Inch and quarter Diameter, fixed in the Centre of a circular Piece of Wood supported upon three brass Skrewws, so that the Tube and pendulous Thread were parallel to each other. The Tube being rubbed, the Thread was attracted and repelled Seven or Eight times (in very good Weather, I have observed it move to and from the Tube Twelve times, at above one Foot Distance). I then tied a Piece of new smooth Packthread to the Top of the Tube, and to the Loop of the Thread hanging down as before, and again excited the Tube: The Thread, without coming once towards the Tube, went into and con-

tinued in a State of Repulsion; but if I only touched the communicating Packthread with my Finger, the white Thread immediately hastened to the Tube: And upon hanging another long Piece of Packthread, which reached the Ground, to the communicating Packthread, and again rubbing the Tube, the pendulous white Thread was so far from going into a State of Repulsion, that it became attracted to the Tube, and continued so, without shewing the least Tendency to a State of Repulsion, as long as the Virtue of the Tube lasted.

EXPERIMENT II.

I tied a Piece of small Cane about sixteen Inches long, and one fourth of an Inch Diameter at one End, and a little more at the other, at right Angles to the Top of my Tube, fixed in the same Pedestal as before, and making unequal Arms with it; and at the End of the larger Arm, a Piece of Stick traversly, about six Inches long, so as it might slide backwards and forwards to and from the Tube: This moveable short Stick at one End supported a very fine white Thread, at the other a very fine blue Silk, by which means we had now a Silk and a Thread at the same time hanging parallel to the Tube. The Thread, after the Tube was rubbed, first was attracted, but then immediately repelled, and continued a considerable time in a State of Repulsion; but upon tying to the End of the shorter Arm of Cane, a Piece of long Packthread, which reached down upon the Table, and rubbing the Tube again, the Thread continued in a State of Attraction, without being once repelled during the whole Virtue of the Tube,

as

as in the preceding Experiment. Yet the Silk, whether the long Packthread was added or not to the shorter Arm of the Cane, continued constantly attracted towards the Tube; but upon putting a short Silk only six Inches long, in the same Circumstances, it would, after some time rubbing the Tube, turn into a State of Repulsion, the upper Part first bending from the Tube, and the lower Part towards it, the upper Bending still increasing till the Whole was repelled; and, which is remarkable, the upper Part or Bending, upon the Approach of the Finger, or any Body not impregnated with Electrical *Effluvia*, flying towards it, and the under Part or Bending rather seeming to fly from it, till the Whole was saturated, and in a State of Repulsion with regard to the Tube, and then any Part of it would come to the Finger, or any other Body, not made Electrical. It is proper to add here one more Difference remarkable between the Thread and Silk: The Thread in a State of Repulsion touched with the Finger, would immediately fly towards the Tube; but the Silk in the same State, after touching several times, still continued in a State of Repulsion, and would not be attracted till squeezed from Top to Bottom between the Finger and Thumb, once, and sometimes two or three times. And farther, the Thread would immediately turn again into a State of Repulsion, whereas the Silk, after the Violence committed by the Thumb and Finger, being attracted to the Tube, would not without a good deal of rubbing the Tube, be repelled again.

N. B. The Silk was untwisted, and about one fourth part of the Thickness made use of.

COROLLARY I.

From the different State of the pendulous Silk and Threads at the same time under the same Circumstances, the former being attracted while the latter is repelled, it follows, that a mere Vibration of the Parts of the Tube is not sufficient to account for the Electrical *Phænomena*; which appears farther from the two contrary States continuing some time, and from the same Piece of Silk being at once part in a State of Repulsion, part in a State of Attraction.

COROLLARY II.

That some Bodies immediately receive and immediately part with the Electric *Effluvia*, but that others are some time before they receive it, or receive enough of it; and when they have received enough of it, part with it more unwillingly.

COROLLARY III.

That any light Body, as a Feather, after touching or nearly approaching the Tube, must fly from it: Upon Contact or a near Approach, it saturates itself with the Electric *Effluvia*, and by this means becomes itself Electrical (as is plain from its coming to all other Bodies too large to come to it); and consequently, from the foregoing Experiments, is in a State of Repulsion with regard to the Tube. As soon as it touches any other Body, it loses its acquired Electricity, and therefore may be attracted as at first.

PROPOSITION II.

Two or more Bodies made electrical by communicating with an Electrical Body, excited by Friction, are in a State of Repulsion with regard to one another; or Bodies made electrical by Communication, repel one another.

EXPERIMENT I.

I suspended two Pieces of white Thread, each about one Foot long, by Loops, upon an horizontal blue silk Line four Feet long, about half an Inch asunder from each other; and upon holding the excited Tube over them at a little Distance, the two Threads immediately receded from each other considerably at the Bottom. I then removed one of the Threads, and held the Tube over the other, in the same manner as before. The single Thread was not observed to move to either Side; consequently the moving of the Threads side-ways was occasioned neither by the Attraction of the cross Line, nor that of the Tube, nor by the Frame of Wood, to which the cross Line was tied at each End, but only by their Action upon each other.

I then added a third String, at the same Distance from the second, that the second was from the first, and upon holding the excited Tube over the middle one, at the same Distance from the cross Silk I did before, if the Strings continued in the same Plane, the middle one stood still, and the String on each side of it receded considerably at the bottom Part, which in this Case must necessarily happen upon a
Sup-

Supposition, that they repel one another equally; for the two contrary Forces of the outer Threads destroy each other, and consequently the middle one must remain quiet; but there was nothing to hinder the middle one from repelling the two outer on each hand sideways. If, as it often happened, the three pendulous Threads did not remain in the same Plane, they then all receded from one another equally, and formed nearly a triangular Prism; the three Threads being the three Edges, or rather a triangular Pyramid with the Top cut off.

Upon suspending four Threads at the same Distance as before from one another, if they continued in the same Plane, they all parted, but the two outermost more from their Neighbours, than the two in the middle from each other.

If they moved out of the Plane they were first in, they formed two Prisms, each extreme with the two in the middle forming one, or rather a Paralleloepid, less at Top than at Bottom.

When five Strings were suspended, either the middlemost continued Stationary when the Plane was not altered, or if it was, they formed three Prisms.

EXPERIMENT II.

I afterwards placed two cross blue Silks, of the same Length as before, about half an Inch asunder from one another horizontally, and tied at each End; and upon each of these, at different times, hung two, three, four, and five Threads, at the same Distances as before, when every thing succeeded, as it ought to have done, upon a Supposition of their mutually repelling one another.

E X P E R I M E N T III.

To each of the Ends of two Threads, suspended as at first, a Feather being tied, the two Feathers manifestly receded from each other: And when three Threads had each a Feather at their Extremities, the middlemost became Stationary, and the two outer went off on each hand.

E X P E R I M E N T IV.

I suspended afterwards two, three, four and five blue silk Strings by Loops, upon one cross blue Silk, and found the several Experiments succeed in the same manner as in Threads; except that they remained a longer time before they appeared in a State of Repulsion, receded from one another more slowly, and continued much longer in the repulsive State, after the Tube was removed.

E X P E R I M E N T V.

This done, I made several Experiments, by mixing Silks of different Colours, and Silks and Threads of different Colours, and suspended them by Turns upon Silks of different Colours, whence arose several different *Phænomena*, which I shall not take Notice of here; but I must not omit mentioning, that upon suspending two black Silks at the before-mentioned Distances from each other, upon a scarlet cross Silk, they did not only open and recede from each other at the Bottom considerably, but when the Tube was held under, ran or jump'd away from each other, to the very Ends of the cross red Silk that supported them, taking two, three, or more Jumps from each other.

I observed the same of two white Silks suspended upon red Silk, but think they did not move away so briskly as the Black.

EXPERIMENT VI.

I tried whether Threads hanging parallel, as above, from a cross blue silk Line, and joined with one or more transverse Threads, so that the perpendicular Threads remained nearly parallel, would mutually repel when the Tube was held over them; they seemed to repel each other full as strongly as before. When they were joined by only one cross Thread towards the Top, the lower Parts separated considerably; when joined by two cross Threads, one towards the Top, and one towards the Bottom, they separated both in the middle Parts between the two cross Threads, and at their lower Ends under the second or lowest cross Thread. When several were tied together at the Top and Bottom, and about a Foot long, not by transverse Threads, but in a Knot at each End, they all bellied out from one another, describing a Figure generated by an Ellipsis, revolved about its greater Axis; approaching nearer to a Sphere, the stronger the repulsive Force was. And, though it was only a necessary Consequence, I could not without some Pleasure observe the Knot at the Bottom, as the Strings swelled out, sensibly rising up. I could scarce forbear imagining my Bundle of Silks, a Bundle of muscular Fibres.

EXPERIMENT VII.

I suspended two brass and afterwards two iron Wires upon a cross blue Silk, in the same manner
as

as the Threads and Silks before-mentioned, and found the Experiments succeed as in Threads of the same Number, except that they did not recede so far from one another, which must necessarily follow from their greater Weight.

N. B. These Experiments were made sometimes with the Tube held over, sometimes held under the cross Line; but they generally succeeded best when the Tube was held under the Extremities of the pendulous Wires, which in this Case separated much farther, and kept their repulsive Force much longer.

EXPERIMENT VIII.

I must not omit here, that I hung up two Fragments of Barometer Tubes, each about a Foot long, by blue silk Lines going through each, so that they hung parallel, horizontal, at equal Heights, and about one quarter of an Inch asunder; upon holding the excited Tube above and under them, they manifestly receded from each other.

I suspended the same Fragments of Tubes by blue silk Lines of equal Length, from a cross blue Silk in a perpendicular Posture, each having a little red Sealing-wax at the upper End, to hinder the Strings from slipping off: The excited Tube being brought near them, they receded manifestly, especially at the lower Ends; the Distance from one another, when at Rest, being about a quarter of an Inch.

COROLLARY I.

From the repulsive State of the pendulous Threads tied transversely with two or more Threads, and bend-

ing out from each other, where at Liberty, it follows that all the Threads of a Table-cloth, or other large Piece of Linen, when made Electrical, (as has been often done all over) have a Desire to fly from each other; and consequently, was the repulsive Force strong enough, the Whole would be dissolved, or torn in Pieces. A short Thread of black Silk, by repeated Applications of the Tube, I have separated into its smallest Fibres: Whence is suggested more plainly, than from any other known Experiment, a Reason for the Dissolution of Bodies in their respective Menstruums, *viz.* That the Particles of the Solvend having imbibed the Particles of the Menstruum, so as to be saturated with them, the saturated Particles become repulsive of each other, separate, and fly to Pieces.

And hence, perhaps, arises a Reason, why Particles of Bodies specifically heavier than the Menstruums in which they are dissolved, are, after the Dissolution and Dispersion, suspended all over the Menstruum, *viz.* That they repel each other. Attraction is insufficient; for Parts attracted equally in all Directions, are, in Effect, not attracted at all, and the Imperfection of the Fluid will not do; for if this occasioned the Suspension, striking or joggling the Vessel would make them subside.

C O R O L L A R Y II.

Hence we plainly see how Heat may divide the Particles of Water with greater or less Force, in Proportion to the Degree of Saturation, and throw them into the Air; where they may continue to ascend, if at the same time they are divided, they are expanded into little Shells or Bubbles, of a Diameter large
enough

enough to be specifically lighter than the lower Air, as the great *Halley* has sagaciously conjectured. Or if the upper Parts of the Air, as being less saturated than the lower Parts, may be able to draw them upwards, till the Excess of Weight, which is constantly increasing, is equal to the Excess of Attraction.

P R O P O S I T I O N III.

Bodies, made Electrical by rubbing, do themselves repel one another, or the electrical excited Bodies themselves repel one another.

E X P E R I M E N T I.

The two Fragments of Tubes before-mentioned *, being suspended horizontally, and in a Posture parallel to each other, I held in one Hand, and with the other rubbed some time; then gently letting them go so as to be at Rest, I could plainly perceive them recede from each other towards that End which had not been taken hold of.

But as upon repeated Trials I found it difficult to make this Experiment succeed unexceptionably, the Tubes generally having some reciprocating Motion of their own, after quitting the Hand, I made use of the following Method.

I suspended a single little Tube about a Foot long, by a long blue silk Line, perpendicularly, and upon a Table placed my great Tube fixed in a Stand as before, excited each alternately, two or three times;

* Prop. II. Exper. VIII.

then

then gently moved the Tube with the Stand it was fixed in, near the suspended little one: The little Tube manifestly receded so much, that a cross blue silk Line stretched horizontally at about an Inch Distance on the opposite Side, would sometimes, upon the first Approach of the great Tube, be touched by it.

E X P E R I M E N T I I.

Three scarlet Silks, pendulous each by Loops from a cross silk Line, and close together, being rubbed downwards two or three times, between the Finger and Thumb, shewed a considerable repulsive Force with regard to each other, forming themselves immediately into a triangular Pyramid, and continuing in this State of Separation some time, and which shews their Attraction at the same time, with regard to other Bodies not excited, coming to them when brought near them.

I observed the same repulsive Force in three yellow and three green Silks, under the same Circumstances, and excited in the same manner, but not in so great a Degree as in Scarlet. In Blue the repulsive Force was scarce discernible after several times rubbing.

S C H O L I U M.

The Rev^d Dr. *Stephen Hales*, in his 12th Article of his 13th Experiment, in his second Volume of *Statistical Essays*, observes, “ That if a Piece of one
 “ of the *Bronchia* or Gills of the *MuscleShell-Fish*, be
 “ cut off, and put into a small concave Glass, with
 “ three or four Drops of its Liquor, and be then
 “ placed under a double Microscope, the Blood may
 “ be

“ be seen greatly agitated in the fine Vessels; and at
 “ the cut Edge of the Piece of Gill, may with great
 “ Pleasure be seen many Blood-globules, repelled
 “ from the cut Orifices of the Blood-vessels, and
 “ attracted by other adjoining Vessels; also other
 “ Globules rolling round their Centre, and repelling
 “ each other; whence (as he says) it is plain, that
 “ Bodies, by brisk rubbing and twirling about, may
 “ acquire, in a watry Fluid, both attractive and re-
 “ pulsive Virtue or Electricity.”

From our last Experiments we are led to think, that the Globules of the Blood, if by Friction they acquire an electrical attractive Virtue, must of necessity repel one another; and that Electricity is not so properly called an attractive and repulsive Virtue, as a Virtue attractive of those Bodies that are not attractive themselves, and repulsive of those that are; and that this repulsive Force of the electrical Blood-globules, excited by Friction, as they flow in their Channels (and particularly in the small ones, and perhaps more so in those of the Lungs, where the refrigerating Power of the Air may assist, as Dr. *Hales* has observed); this repulsive Force of the Blood-globules, I say, may be the great Cause that hinders the Blood from coagulating as it circulates; may be the great Cause of the constant Perspiration in an healthy State, and of the Increase of it, *ceteris paribus*, in Proportion as the Velocity and Friction of the Blood increases.

If these things are so, the Necessity of Exercise appears more plainly than ever, in order to keep the Body in an healthy State, as we may observe here the very Steps that Nature makes use of to free herself from her Suppressions.

Granv. Wheeler.

III. *An Account of some of the Electrical Experiments made by Granville Wheler, Esq; at the Royal Society's House, on May 11. 1737. drawn up by C. Mortimer, M. D. R. S. Secr.*

E X P E R I M E N T I.

A Large Octavo Book was placed horizontally upon silk Lines, and the upper Surface strewed with several Pieces of leaf Brass, all or the greatest Part of which flew upwards, from one another, and off the Book; upon holding an excited Tube at a little Distance underneath the Book.

E X P E R I M E N T II.

Two Lines were extended horizontally the whole Length of the Library being between 30 and 40 Feet, distant from one another about two Feet at one End, and meeting together in a Knot at their other Ends, the whole Lines being Packthread, except five Feet of silk Line tied at each of the separated Extremities, as well as at the Knot where the other Ends united, in order to stop the Current of the *Effluvia*. Upon the united Extremities was placed horizontally a Piece of Card about two Inches square, on which were strewed Pieces of leaf Brass: The excited Tube being held at a little Distance under the separated Extremities of the Packthread, the leaf Brass on the Card at the other End flew upwards, and off the Card.

E x-

E X P E R I M E N T III.

Five glass Receivers placed one within another upon an electrical Cement of Bees-wax and *Venice* Turpentine, were all exhausted: In the innermost a fine white Thread about five Inches long, was suspended from the Crown of it, by the Assistance of a little Cement made of Bees-wax and Oil. Upon moving the excited Tube up and down near the Side of, and horizontally to and from the outward Receiver, the suspended Thread manifestly made many Vibrations corresponding to the Motions of the Tube.

E X P E R I M E N T IV.

An electrical circular Cake of Bees-wax and Rosin, ten Inches in Diameter, was placed horizontally upon a tall glass Receiver near three Feet high, such as is made use of for the dropping the Feather and Guinea. This Cake being, the preceding Evening about Eight o'Clock, warmed with an hot Iron held over it, and then struck perpendicularly all over its Surface with the Hands in parallel Directions, and so left cover'd with a thin Pasteboard, was about Twelve o'Clock next Day at Noon gently uncover'd, and an ivory Ball about one Inch and half Diameter placed in the Centre, a fine white Thread about ten Inches long, with a small Piece of Cork, the Size of a Pin's Head, at the End of it, being held between the Finger and Thumb, was gently let down upon the Vertex of the Ball; it first flew off at some Distance, and then made several pretty regular Revolutions from West to East about it, in the Form of a Circle.

E X P E R I M E N T V.

The Ball was removed, and the Cake again warmed and excited as before; after which the Ball was replaced at a little Distance from the Centre, nearer to Mr. *Wheler*; the Consequence of which was, that the pendulous little Body moved with a direct Motion as before, but in an Orbit that resembled an Ellipse, having the Ball in one of its *Foci*.

E X P E R I M E N T VI.

Two Bullets fixed on little Stands of Cork about one quarter of an Inch high, were placed upon the Cake, each about an Inch distant from the Centre of it, and in a Line with the Centre and Mr. *Wheler*; the pendulous Body described an Orbit resembling an Ellipse, having the two Bullets for its *Foci*, and the Motion was direct from West to East.

E X P E R I M E N T VII.

Instead of the Cork, another pendulous Body of a cylindrical Form was made use of, tied to a fine white Thread about twenty Inches long; the Cylinder consisted of two circular Bases of Paper half an Inch Diameter, but all cut away except a Ring and a small Bar cross the Middle, through which Basis six equal fine Threads passed at equal Distances from one another, knotted at the lower Base separately, and joined together in one Knot at about half an Inch Distance from the upper Base, from which Knot proceeded the long Thread. This Body moved from West to East about the central Ball, and at the same time discover'd

a Motion about its own Axis in the same Direction; but after two or three Turns generally stop, and turn'd the contrary Way, which seem'd to arise from the untwisting of the Thread.

EXPERIMENT VIII.

A Thread about a Foot long, was suspended from an horizontal Line of Packthread, parallel to it an excited Tube placed erect in a Stand, the Thread approached the Tube, and continued in a State of Attraction: A Thread of the same Length, suspended from a silk Line, vibrated backward and forwards two or three times, being first attracted, and then repelled, and continuing some time repelled; but upon joining the Top of the Tube, by a Packthread going round it, to the Loop of the Thread, the Thread continued constantly in a State of Repulsion, shewing no Tendency to Attraction.

EXPERIMENT IX.

Two black Silks, about the same Length with the Thread in the preceding Experiment, were suspended by Loops from an horizontal red silk Line, at the Distance of about half an Inch from each other; upon holding the excited Tube under them, the Silks swelled out from one another, and then jump'd away on each Hand to the Distance of two Feet.

EXPERIMENT X.

A circular Board of nearly the same Diameter with the electric Cake, was suspended horizontally by six silk Lines, tied to one silk Line which was brought

over a Pulley at the Top of a Frame of Wood, so as to be moved up and down. From the Board hung six fine white Threads about eighteen Inches long, fixed by a little Cement at equal Distances from each other. The Board being let down till the Ends of the Threads were about an Inch distant from the electric Cake, which was directly under, and had the ivory Ball on its Centre; the Threads all approached towards the Centre of the Cake, both when the Ball was in the Centre, and when taken away, keeping an equal Distance from the Centre, and from one another, as long as a Packthread joined the Circle of Board and the Frame to keep it steady; and upon removing the Ball out of the Centre towards the Circumference, the Figure lengthen'd, the Threads next the Ball advancing nearer the Circumference; when the Ball was placed at about an Inch Distance from the Circumference, the Thread that was before nearest the Circumference, whipp'd between the Ball and the Centre, so as to be almost in the same Plane with its two neighbouring Threads, the Figure form'd by the Extremities resembling an Ellipse with one End cut off: But when instead of the Packthread that join'd the Board to the Frame, a blue silk Line was tied in the same manner in all respects, the Threads, instead of coming towards the Centre, all flew away at a great Distance from the Cake, and from one another.

It ought to be observed in the Experiments of the circular Motion of the pendulous Body, that Mr. *Wheler's* Hand seem'd as steady as possible, except in the first Experiment, when a little Trembling appear'd; Mr. *George Graham* taking a very good Method to observe

observe it, by keeping his Eye fix'd upon a Point at a considerable Distance, in the same Line with the End of Mr. *Wheler's* Finger, and his own Eye.

Yet when Mr. *Wheler* had finish'd the Experiments to the Satisfaction of all present, Mr. *Hawskbee*, Mr. *George Graham*, and Dr. *Mortimer*, held the Thread with the pendulous Body over the Cake with the Ball on its Centre, after the Cake had been excited by Mr. *Wheler*; but they had no regular Revolutions at all, though several very manifest Motions were made with the Hand, to try if a projectile Motion might by that means be given to the pendulous Body. Mr. *Wheler* had tried the same thing with his Servant; from whence it is reasonable to conclude, that it is necessary, that the same Person who excited the Cake should likewise hold the Thread; as if there were some Analogy between the *Effluvia* excited by the clapping of the Hand on the Cake, and the *Effluvia* which may be communicated along from the Hand which holds the Thread to the Piece of Cork at the End of it. And this seems to be the Reason of what the late Mr. *Grey* told me, *viz.* That there was something in the human Hand essential to the Experiment, which he had not yet found in any other Supporter of the Thread.

IV. *A Letter from Granvile Wheler, Esq; to Dr. Mortimer, Secr. R. S. containing some Remarks on the late Stephen Gray, F. R. S. his Electrical Circular Experiment.* *

S I R,

SOME odd Circumstances led me to make Mr. Gray's circular Experiment in the following Manner. While I excited a Cake of Rosin and Beeswax ten Inches Diameter, by clapping with my Hand, I let my ivory Ball continue in a Basin of Water; then shaking off the Drops, placed it in the Centre, and with my right Hand held a fine Thread, about eight or nine Inches long, having one End rolled up into a little Ball, and the other, for about an Inch, reduced to its greatest Fineness, to only one Fibre, myself and Hand being supported on the Back of a Chair. The Success was, I had a great many Revolutions, to the Number of Fifty, from West to East; but at first not so regular as towards the last, at first describing only about one third Part of the Circumference at a time, and after standing still a little, describing another third Part. I might probably have had a great many more Revolutions, but being tired, I was forced to rest myself, which I did for ten Minutes, then took up the Thread again. The Thread stood repelled at some Distance, without making any Revolutions, and at last only made half an one the contrary way to what it did before; but upon wetting it, by drawing it two or three times

* See *Philosophical Transactions*, n. 441, p. 220. and n. 444, p. 400. of the Revolutions of *pendulous Bodies* by Electricity.

over the Surface of the Water, it made again twenty more Revolutions from West to East, only at a smaller Distance from the Ball, (for the Water must make it heavier) but full as regular as before, and rather quicker: The Virtue of the Cake must now have lasted about three quarters of an Hour. After resting about six Minutes, I tried again with the String fresh wetted, the Ball and Cake continuing as before; and had, to my great Surprize, one hundred Revolutions in the Space of about twelve Minutes, the Revolutions being still quicker, and more regular, and nearer the Ball; and at the sixth Revolution of this last Hundred, the Thread was attracted to the Surface of the Ball, and, being wet, did not disengage itself, till pulled away; yet after this, described the remaining Ninety-four Revolutions of the Hundred, and seemed inclined to describe a great many more, but that I was forced to rest my Arm again, which I did for about eight Minutes, then tried again, the Thread being fresh wetted, and had seventy Revolutions at nearly the same Distance from the Ball in less than nine Minutes, all very regular, and without any Attraction of the Thread to the Ball. I rested again sixteen Minutes, wet the Thread again, and held it as usual; it was repelled at about half an Inch Distance from the Ball, but seemed to have no Tendency to a circular Motion; yet after continuing stationary about a Minute, I perceived a Motion about its Axis, about which it took several Turns; but still had little or no progressive Motion, till about a Minute longer, when it began to move forward, and continued doing so from West to East, for about thirty-three Revolutions, very regular, but slower than in the two last Cases, the String having been held about ten Minutes.

nutes, and the Revolutions performed in about seven or eight of them. I observed each of these three last times, it was rather longer before the progressive Motion began than usual; and in all the Trials of this Experiment, I frequently perceived a Motion about the Axis, which was generally from West to East, though now-and-then the contrary Way. The Virtue of the Cake must now have lasted near two Hours; about three quarters of an Hour after, I tried again, and had sixty Revolutions from West to East, in about ten Minutes, the Distance from the Ball being still less than before, hardly one quarter of an Inch, scarce any Revolution about the Axis appeared, and at the Beginning the Thread was twice attracted to the Ball. About an Hour and half after, the Virtue of the Ball was not quite gone, the wet Thread being repelled, and making three or four Revolutions from West to East, as well as moving a little about its Axis the same Way. But as it was reasonable to suppose the Ball itself in the Centre of the Cake was now dry, with a Feather dipp'd in Water I wet its Surface; yet found no Increase of Virtue, rather a Diminution of it, the pendulous Body seeming scarce at all repelled; but it is to be observed, that the Ball, as it was wetting, twice tumbled over, and rolled upon the Surface of the Cake; by which means the Virtue of the Cake might be much diminished.

It is not improper too to take Notice here, that during the Revolutions of the wet String, I have frequently observed a kind of Oscillatory Motion, as if there was an alternate Intention and Remission of the repulsive Force. As also that I have often took Notice of little Plucks, and convulsive Motions,

in

in the pendulous Body, and sometimes thought I have felt something like it in my Arm that held it, though at no other time have I ever been sensible of any such thing.

I have several times since repeated this Experiment with the Thread and Ball both wet, and found it succeed much better than when they were both dry; and once I find by my Notes I had two hundred and twenty Revolutions before I rested my Arm. I have tried too with the Ball dry, and the String only made wet; but the Virtue did not continue so long, as when both were wet.

I now flattered myself with Hopes of Success, if the Thread was suspended from an undoubted fixed Point, which therefore I proceeded again to try with the greatest Care and Caution, but in vain; the Revolutions were uncertain.

This Difference naturally led me to reflect upon the Cause of it. The Tremor of the Hand would not account for it; for this being both ways backward as well as forward, must as often hinder as promote a continual Motion one way: And though in two opposite Parts of a Circle, the Motion is really in contrary Directions, and therefore the contrary Impulses of a Tremor may promote a Revolution applied at opposite Places of the Orbit; yet as these Tremors are irregular, and succeed much quicker than the Revolutions are perform'd, they seem insufficient to account for the Motions of the pendulous Body, performed with any Degree of Regularity.

A Stream of Air in my Room might impel along the Tangent the pendulous Body, kept at a Distance from the Ball by its repulsive Force; and then Gravity,
 Q taking

taking place, might with the first Motion compound a Curve; but still the Resistance of the Air would soon destroy the original Impulse, could a few Revolutions be performed; and besides, one Revolution could not be performed, because the same Stream of Air that began the Motion, must be contrary to it in its Return.

A Finger held on the right Hand near the pendulous Body, when suspended from a fixed Point, will make it revolve from West to East; but then it must be applied and removed alternately: The repulsive Force therefore which the Arm may acquire, by being held in the Sphere of the *Effluvia*, is insufficient; for, as it is in one Place, it must impel only one way, and constantly the same way; and therefore, like a Stream of Air in the Room, though it might create the Beginning, it must hinder the Completion of a Revolution.

Sometimes I have doubted, whether the Pulse of the Arm might not be assisting in giving a projectile Motion. When one Leg is laid over the Knee of the other, a Motion and Heaving of the Leg that lies over, answering to every Stroke of the Pulse, is very apparent at a Distance: The Arm therefore in some Postures, in which its great Artery meets with a proportionable Pressure or Resistance, may have a constant Motion, though less perceivable.

What seemed the most probable Solution, was this: When the Arm is extended, the Posture being uneasy, there must be a Reaction of the Muscles, or a continual pulling of the Arm towards the Body. When therefore the right Arm is made use of, the pulling will be from Right to Left; and consequently the

the Motion produced in the Body held by it in the same Direction, or from West to East. When the left Arm is made use of, the Reaction of the Muscles will be from Left to Right, and therefore the Motion of the pendulous Body from East to West. And, agreeably to this, I have observed, (as I formerly took Notice, though this Reason did not then occur to me) when I used my left Hand, all other Circumstances continuing the same, the Motion of the pendulous Body was from Left to Right, or from East to West, contrary to what was observed when held by the right Hand.

Yet still neither of these Solutions would account for the Variety of Oddnesses I have met with under various Circumstances.

I proceeded therefore to try with Rests for my Arm of different Heights, having an Arm of Wood, about two Feet long, fixed to a Rest for my Telescopes, which I could raise to any Height I wanted; and I found the Experiment succeed only well, when the Rest was lower than the electric *Area*, and the Arm was supported upon its Elbow, which was the Posture constantly made use of, when rested upon a Chair, the Chair being lower than the electric *Area*, that it might less affect the *Effluvia*, as was then thought.

I began now to think with myself, whether it was not possible, that an Inclination to a Motion one way in the Person that holds the Body, might not have such an Influence upon the Arm, and consequently the String and pendulous Body, as to determine them the same way by some Pressure or Byass put upon it, though no Motion sensible even to him-

Q 2

self,

self, was produced in the Hand. If so, I might, by a contrary Inclination, produce a Motion the contrary way. Having therefore a fine Day, and my circular Cake being well excited, I tried if I could not produce a regular Motion from East to West, about the Ball in the Centre, having my Hand supported, as usual, upon the Back of a Chair. I found I could produce a very regular one from East to West for many Revolutions, and change from one Motion to another, without being sensible I moved my Hand at all.

I then wet the Ball and String, as in the Experiment before-mentioned, and found I could tire myself with a Motion either from East to West, or from West to East, as I pleased, without giving any Motion, that I could perceive, to my Hand or Fingers. Hence many odd Experiments that please, may, when repeated, succeed.

Since therefore the Motion of the pendulous Body from a Point undoubtedly fixed, is irregular, as I have found by many different Experiments, repeated with the greatest Care and Caution; and since I am convinced from these last-mentioned Trials, the Motion from West to East, and from East to West, must generally have been determined by myself; I am inclined to think, that a Desire of producing a Motion from West to East, was the secret Cause that determined the pendulous Body to that Direction, by some Impression from Mr. *Gray's* Hand, as well as my own, though I am persuaded at the same time, he was not sensible of giving any Motion to his Hand himself: And I the rather think this was the Case, from the Instance Mr. *Gray* gives, by way of Explanation,

nation, of a Man resting his Elbows upon his Knees, this implying that he rested his Arm upon his Elbow, as I did myself.

But though upon the Whole it does at last appear, that this Motion from West to East in a pendulous Body, applied to another in the Centre of an electric *Area*, is to be ascribed to the Hand that holds it, and not solely to the Nature of the electric *Effluvia*, or the Figure of the central Body; yet still, perhaps, it may not be improper for Astronomers to consider, whether or no a Medium with this Property, that all Bodies immersed in it, are repulsive of one another, ought not to be joined with Gravity to explain the heavenly *Phænomena*; especially since the *Phænomena* of Fire, and our electric *Effluvia*, have a great Affinity to each other; and since many of the heavenly *Phænomena* are to be accounted for, upon this Supposition, with great Simplicity; and some of them, that have not yet perhaps been fully accounted for, seem necessarily to follow.

I am, Sir, with a very great Regard,

Otterden-Place,
Feb. 20. 1737-8.

Your much obliged,
humble Servant,

Granv. Wheler.

P S. This was intended to have been sent eleven Days ago.

Mar. 3. 1737-8.

V. An

V. *An Account of the Influence which two Pendulum Clocks were observed to have upon each other, by Mr. John Ellicott, F. R. S.*

THE two Clocks upon which the following Observations were made, being designed for Regulators, particular Care was taken to have every Part made with all possible Exactness: The two Pendulums were hung in a manner different from what is usual; and so disposed, that the Wheels might act upon them with more Advantage. Upon Trial they were found not only to move with greater Freedom than common, but an heavier Pendulum was kept in Motion by a smaller Weight. They were in every respect made as near alike as possible. The Ball of each of the Pendulums weighed above 23 ℥; and required to be moved about 1° 5' from the Perpendicular, before the Teeth of the swing Wheel would scape free of the Pallets; that is, before the Clocks would be set a going. The Weight to each was 3 ℥, which would cause either of the Pendulums in their Vibrations to describe an Arch of 3°. The two Clocks were each in Cases, which shut very close, and placed Sideways to one another, so near that when the Pendulums were at Rest, they were little more than about two Feet asunder. The odd *Phænomena* observed in them were these: In less than two Hours after they were set a going, one of them (which I call N^o 1.) was found to stop; and when set a going again, (as it was several times) would

would never continue going two Hours together. As it had always kept going with great Freedom, before the other Clock (which I call N^o 2.) was placed near it, this led me to conceive its stopping must be owing to some Influence the Motion of one of the Pendulums had upon the other; and upon watching them more narrowly, I found the Motion of N^o 2. to increase as N^o 1. diminished; and at the time N^o 1. stopped, N^o 2. described an Arch of 5°, that is nearly two Degrees more than it would have done, if the other had not been near it, and more than it did move in a short time after the other Pendulum came to be at Rest: This made me imagine that they had a mutual Influence upon each other. Upon this I stopped the Pendulum of N^o 2. leaving it quite at Rest, and set N^o 1. a going, the Pendulum describing as large an Arch as the Case would permit, *viz.* about 5°. In about 20 Minutes after, I went to observe whether there was any Motion communicated to the Pendulum N^o 2. when, to my Surprize, I found the Clock going, and the Pendulum to describe an Arch of 3°, whereas at the same time N^o 1. did not move 4°. In about half an Hour after, N^o 1. stopped, and the Motion of N^o 2. was increased to very near 5°. I then stopped N^o 2. a second time, and set N^o 1. a going, as before; and standing to observe them, I presently found the Pendulum of N^o 2. to begin to move, and the Motion to increase gradually, till in 17' 40" it described an Arch of 2° 10', at which time the Wheel discharging itself of the Pallets, the Clock went. The Arches of the Vibrations continued to increase, till (as in the former Experiment) the Pendulum moved 5°; the Motion

Motion of the Pendulum N^o 1. gradually decreasing all the while, as the other increased; and in three Quarters of an Hour after, it stopped. I then left the Pendulum of N^o 1. at Rest, and set N^o 2. a going, making it describe an Arch of 5^o; it continued to vibrate less and less, till it described but about 3^o; in which Arch it continued to move all the time I observed it, which was several Hours. The Pendulum of N^o 1. seemed but little affected by the Motion of N^o 2. I tried these Experiments several times over, without finding any remarkable Difference. The freer the Room was from any Motion (as Peoples walking about in it, &c.) I found the Experiments to succeed the better; and once I found N^o 2. set a going in 16' 20'', and N^o 1. at that time stopped in 36' 40''.

I shall not offer my Opinion to this Honourable Society, concerning the Cause of these *Phænomena*, or at least not till I have farther examined it by the Help of some more Observations and Experiments.

VI. *Further Observations and Experiments concerning the two Clocks above-mentioned, by the Same.*

IN the preceding Paper I had the Pleasure to communicate to this Honourable Society, an Account of the extraordinary Influence I observed two Clocks to have upon each other, and which was attended with such Circumstances, as I believe had never before been taken Notice of. I shall now beg
Leave

Leave to lay before you some farther Observations I have since made, which will, I hope, in great measure, account for the Facts then related.

In my former Account I took Notice, that the two Clocks were in separate Cases, and that the Backs of them rested against the same Rail; that the Pendulums, when at Rest, were about two Feet asunder, and weighed about 23 lb each, and were made to move with such Freedom, that a Weight of 3 lb. would cause either of the Pendulums to describe an Arch of three Degrees. The most remarkable Particulars then observed in them were these: If the Pendulum of one of the Clocks, which (for Distinction sake) I called N^o 2. was left at Rest, and that of the other, which I called N^o 1. was set a going, this would, in about 16 Minutes, communicate so great a Quantity of Motion to N^o 2. as would make its Pendulum describe an Arch of above two Degrees, and would set the Work a going: That the Motion of the Pendulum of N^o 1. constantly decreased as that of N^o 2. increased, and after about 30 Minutes it did not describe an Arch sufficient to free the Teeth of the Wheel from the Pallets, so that the Clock stopped. At the same time the Pendulum of N^o 2. described an Arch of five Degrees, which was two Degrees more than it would have done, had it not been affected by the Motion of N^o 1. Upon leaving the Pendulum of N^o 1. at Rest, and setting N^o 2. a going, the Pendulum of N^o 1. was found to be but little affected, and never moved sufficiently to set the Work a going. These seemingly different Effects, which the two Clocks had upon each other, I shall now endeavour to account for.

The Manner in which the Motion is communicated to the Pendulum at Rest, I conceive to be thus: As the Pendulums are very heavy, when either of them is set a going, it occasions by its Vibrations a very small Motion, not only in the Case the Clock is fixed in, but, in a greater or lesser Degree, in every thing it touches; and this Motion is communicated to the other Clock, by means of the Rail, against which both the Cases bear. The Motion thus communicated, which is too small to be discovered but by means of some such-like Experiments as these, will, I doubt not, be judged by many, insufficient to make so heavy a Pendulum describe an Arch of two Degrees, or large enough to set the Work a going; and indeed it would be so, but for the very great Freedom with which the Pendulum is made to move, arising from the Manner in which it is hung. This appears from the very small Weight required to keep it going, which, when the Clock was first put together, was little more than one lb. And if the Weight was taken off, and the Pendulum made to swing two Degrees, it would make 1200 Vibrations before it decreased half a Degree, so that it would not lose the $\frac{1}{3000}$ part of an Inch in each Vibration. Indeed if the Weight was hung on, the Friction would be increased, and the Pendulum would not move quite so freely; but even in that Case it was found to lose but little more than the $\frac{1}{2000}$ part of an Inch, or about three Seconds of a Degree, in one Vibration; and therefore if the Motion communicated to it from the other, will make it describe an Arch exceeding three Seconds, the Vibrations must continually increase till the Work is set a going.

And

And that the Motion is communicated in the manner above supposed, is confirmed by the following Experiments:

A Prop was set against the Back of the Case of N^o 2. to prevent its bearing against the Rail; and N^o 1. was set a going; then observing them for several Hours, I could not perceive the least Motion communicated to N^o 2. I then set both the Clocks a going, and they continued going several Days; but I could not find they had any Influence upon each other. Instead of the Prop against the Back of the Case, I put Wedges under the Bottoms of both the Cases, to prevent their bearing against the Rail; and stuck a Piece of Wood between them, just tight enough to support its own Weight. Then setting N^o 1. a going, I found the Influence so much increased, that N^o 2. was set a going in less than six Minutes, and N^o 1. stopped in about six Minutes after. In order to try what Difference would arise, if the Clocks were fixed on a more solid Floor, I placed them (exactly in the same manner as in the last Experiment) upon the Stone Pavement under the Piazza's of the *Royal Exchange*, and stuck the Piece of Wood between them, as before; and setting N^o 1. a going, the only Difference I could perceive, was, that it was 15 Minutes before N^o 2. was set a going, and N^o 1. continued going near half an Hour before it stopped. From these Experiments I think it plainly appears, that the Pendulum which is put in Motion, as it moves towards either side of the Case, makes the Pressure upon the Feet of the Case to be unequal, and, by its Weight, occasions a small Bearing or Motion in the Case on that Side towards which the

Pendulum is moving; and which, by the Interposition of any solid Body, will be communicated to the other Clock, whose Pendulum was left at Rest. The only Objection to this, I conceive, is the different Effects which the two Pendulums seemed to have upon each other. But this I hope to explain to Satisfaction.

For, notwithstanding these different Effects, I soon found, by several Experiments, that the two Clocks mutually affected each other, and in the same Manner, though not with equal Force; and that the Varieties observed in their Actions upon each other, arose from the unequal Lengths of their Pendulums only.

For, upon moving one of the Clocks to another Part of the Room, and setting them both a going, I found that N^o 2. gained of N^o 1. about one Minute 36 Seconds in 24 Hours. Then fixing both against the Rail, as at first, I set them a going, and made the Pendulums to vibrate about four Degrees; but I soon observed that of N^o 1. to increase, and that of N^o 2. to decrease; and in a short time it did not describe an Arch large enough to keep the Wheels in Motion. In a little time after it began to increase again, and in a few Minutes it described an Arch of two Degrees, and the Clock went. Its Vibrations continued to increase for a considerable time, but it never vibrated four Degrees, as when first set a going. Whilst the Vibrations of N^o 2. increased, those of N^o 1. decreased, till the Clock stopped, and the Pendulum did not describe an Arch of more than one Degree 30 Minutes. It then began to increase again, and N^o 2. decreased, and stopped a second time, but
was

was set a going again, as before. After this N^o 1. stopped a second time, and the Vibrations continued to decrease till the Pendulum was almost at Rest. It afterwards increased a small matter, but not sufficiently to set the Work a going. But N^o 2. continued going, its Pendulum describing an Arch of about three Degrees.

Finding them to act thus *mutually* and *alternately* upon each other, I set them both a going a second time, and made the Pendulums describe as large Arches as the Cases would permit. During this Experiment, as in the former, I sometimes found the one, and at other times the contrary Pendulum to make the largest Vibrations. But as they had so large a Quantity of Motion given them at first, neither of them lost so much during the Period it was acted upon by the other, as to have its Work stopped, but both continued going for several Days without varying one Second from each other; though when at a Distance, as was before observed, they varied one Minute 36 Seconds in 24 Hours. Whilst they continued thus going together, I compared them with a third Clock, and found that N^o 1. went one Minute 17 Seconds faster, and N^o 2. 19 Seconds slower, than they did when placed at a Distance, so as to have no Influence upon each other.

Upon altering the Lengths of the Pendulums, I found the Period in which their Motions increased and decreased, by their mutual Action upon each other, was changed; and would be prolonged as the Pendulums came nearer to an Equality, which from the Nature of the Action it was reasonable to expect it would. This discovers the Reason why the Pendulum

dulum of N° 2. when left at Rest, would be set a going by the Motion of N° 1. whereas if N° 1. was left at Rest, it would not be set a going again by the Motion of N° 2.

For I found by several Experiments, that the same Pendulum, when kept in Motion by a Weight, would go faster, than when it only moved by its own Gravity. On this Principle, which may easily be accounted for, it follows, that during the Time in which the shortest Pendulum, N° 2. was only acted upon by N° 1. it would move slower, and the Times of its Vibrations approach nearer to an Equality with those of N° 1. than after it came to be kept in Motion by the Weight; and by this means the Time which N° 1. would continue to act upon it, would be prolonged, and be more than was required to make the Pendulum describe an Arch sufficient to set the Work a going. But on the contrary, while the Pendulum of N° 1. which was the longest, was only acted upon by N° 2. as it would move slower, the Difference of the Times of the Vibrations would be increased; and consequently the Time which N° 2. would continue to act upon it, would for this Cause be shortened, so that before the Pendulum of N° 1. would describe an Arch sufficient to set the Work a going, the Period of its being acted upon would be ended, and it would begin to act upon N° 2. at which time its Vibrations would immediately decrease, and continue to do so till it came to be almost at Rest. And thus it would continue sometimes to move more, and at other times less, but never sufficiently to set the Clock a going.

This

This Account might be confirmed by many more Experiments I have made relating to this Subject; but as I hope these already mentioned will be thought sufficient to confirm the Truth of what I have advanced, I shall forbear to trespass any longer on your Time, and subscribe myself

*Your most obedient
humble Servant,*

John Ellicott:

VII. *The Case of a Wound in the Cornea of the Eye being successfully cured by Mr. Tho. Baker, Surgeon to St. Thomas's Hospital, and by him communicated to the ROYAL SOCIETY, in a Letter to Dr. Mortimer, R. S. Secr.*

A Young Woman, about the Age of 15 Years, on the 6th Day of *November 1733*, received a Wound just in the Pupil of her right Eye, by the Spear of a common Fork. An Inflammation followed, with great Pain. The whole Eye appeared dark and turbid; and the Humours seemed confused, and blended together. I opened a Vein in the Arm, and drew away 10 Ounces of Blood: I then washed the Eye with a Collyrium of *Trochisci Albi Rhafis*, and common Water, made Blood-warm; and dressed it with a Cataplasm of white Bread and Milk, with a little Saffron in it. The next Day there appeared
on

on the wounded Part of the *Cornea*, a large thick Slough: I dressed her in the same Manner, and so continued till the 18th Day of the same Month, when the Slough cast off. I purged her during this Time with *Decoct. Sennæ* ℥ij, *Mann. Solut.* ℥ss, *Aq. Pæon. comp.* ℥ij; *m. f. Pot.* at the Distance of about three Days, just as I found her Strength would permit. The Inflammation and Pain abated daily. During the whole Time, the Eye was quite blind, till the Slough cast off, when she complained she saw double. In a very little time her Sight returned, but not so perfect as before; her Eye having somewhat of a Cloud before it.

I made her six Visits at the Distance of two or three Days after the 18th: When I left her, she saw perfectly well, that Cloud which she before complained of, being removed; her Eye appeared fair and clear; and, as she told me, was equally strong and useful to her as her other. A little Speck (which was the Cicatrix of the Wound) remaining on the *Cornea*, I made her a Fontanel in the Arm, and ordered her to keep it open, and not to touch the Speck on her Eye. It is now more than two Years since, the Speck has gradually decreased, and is now so small, that it is scarce visible; and her Sight is as perfect and strong as before this Accident.

Thomas Baker, Surgeon.

VIII. *Extract of a Letter dated at Montpellier, Dec. 27. 1731. N. S. from Andrew Cantwell, M. D. Monspel. to T. S. M. D. and by him translated from the French, giving an Account of a monstrous Boy.*

THERE is actually in this Town, a Lad of thirteen Years of Age, born at *Cremona*, who bears the lower Parts of another Boy, which seem to issue from his Epigastric Region, between the *Cartilago ensiformis* and the Navel. The Fore-part of the one faces that of the other. The Head and Trunk seem buried in the Lad's *Abdomen*, down to the Hips, where the Connection is plainly to be seen. This Portion of the prominent Body has a well-form'd *Anus* and *Penis*. The *Scrotum* has a fine Down on it, but is void of Testicles, and seems to be filled with the Intestines. Nothing passes through these two Outlets. I have perfectly well distinguished the two *Ossa Ilium* in their natural State, but could not feel the *Os Sacrum*. The Articulation of the *Femur* is somewhat discernible on each Side: and I have perceived the Pulsation of the anterior crural Arteries. The Lad is very sensible when these additional Feet, Legs, or Buttocks, are pinched, or over-much pressed. He has lately had the Small-pox, and these have suffered by it equally with him. At his Navel I found a considerable Rupture, which is covered by this Portion of a Body. This Rupture grows monstrously big in wet Weather, and diminishes again in
S
dry,

dry. It has a circular Hole in it, which runs through the *Peritonæum*. The Lad is of a thin Habit of Body, but otherwise enjoys good Health. His Father, *Michael Martinetti*, a Tinker, told me, that this is the seventh Child his Wife *Nunciada* bore him. She was thirty Years of Age at his Birth, and bore him two more since. All the rest were of the natural Shape.

IX. *Three extraordinary Cases in Surgery, by Bezaleel Sherman, Surgeon, at Kelvedon in Essex, communicated in November 1738.*

S*A*MUEL Bush, of the Parish of *Wickham-Bishops*, in *September 1704*. being on the Top of a very high Timber Tree, in order to shake down the Acorns, he let go his Hold; and by falling from one Bough of the Tree upon another, he broke his Thigh-bone; and one End of it, by the Force of the Fall, stuck fast in the Ground, which fractured the Bone in another Place, about two Inches and an half above the former. This intire Piece of the *Os Femoris* was taken out; notwithstanding which, so large a *Callus* united the two Ends of the Bone, that his Thigh (when cured) was very little more than a quarter of an Inch shorter than the other Thigh. The Surgeon who had the Care of him, used his greatest Endeavours, during the Cure, to preserve the Extension; but he imputed the Largeness of the *Callus* to a very great Quantity of *Lap. Osteocolla*, which he made him take for six Weeks or two Months, in Powder with Milk,
in

in an Electuary, in his Bread, and in his Pudding; in short, in almost all the Food he took.—

— *Fitch*, of the Parish of *Keldon*, had a foul Ulcer in his Mouth, with a *Caries* in the lower Jaw-bone, one Part of which, from the Suture at the Chin to the End of it under the Ear, in Process of Time intirely came out, with three Teeth in it. This was also owing to a great Quantity of *Osteocolla* internally given, which was thought not only to expedite this large Exfoliation, but at the same time to generate so large and firm a *Callus*, that he can chew an hard Crust, or any other Food, on that Side as well as on the other.—

John Spilman, Bricklayer, of *Maldon*, came to me the 3d of *October* 1734. having a sinuous Ulcer in his *Rectum*, about two Inches from the *Anus*. This had remained a Twelvemonth, and was taken for the Piles, and treated as such, both internally and externally. I soon perceived a Tumour in his Buttock two or three Inches from the *Anus*, which coming to Suppuration, I opened it by Incision; and after dressing it several Weeks with little Prospect of Success, I discovered at the Bottom of the Ulcer something that looked like a Bone, which when extracted, proved to be the lower Jaw of a Fish, as a *Whiting*, or young *Cod*, &c. And unquestionably this was swallowed at least a Year before it came away, because the pricking Pain he felt when the sharp End of the Bone stuck in the *Rectum*, was the Symptom mistaken for the Piles; and when this had made its Way thro' the *Rectum*, and got into the fleshy Part, the Aposthume followed in course; and the Bone being extracted,

tracted, the Ulcer was soon cicatrized by the common Methods of Cure in such Cases.

N. B. All these Persons are yet living, and will attest the Truth of the Case.

X. *A Letter from Tho. Stack, M. D. to Sir Hans Sloane, Bart. Pr. R. S. &c. containing an Account of a Woman sixty-eight Years of Age, who gave Suck to two of her Grand-children.*

Honoured S I R,

A Gentleman of Credit having lately inform'd me of a Woman near seventy Years old, who actually suckles one of her Grand-children, and courteously offering to accompany me to her, excited my Curiosity to see so uncommon a Sight; and the more, in order to try if I could not discover some Fallacy in the Affair. Wherefore I went Yesterday, in Company with the aforesaid Gentleman, to a House in *Tottenham-Court-Road*, where the Woman we inquired for appear'd in an Instant. Her Breasts were full, fair, and void of Wrinkles; though her Face is very much wither'd, her Cheeks and Mouth vastly sunk in, her Eyes red, and running with a clammy Humour; and though she has, in short, all the other external Marks that one may reasonably expect to find in a Woman, who has spent the last Half of her past Life in Labour, Troubles, and other Concomitants of Poverty, and through them has reach'd near to her

70th Year. Upon pressing her right Breast, she fairly squeez'd out Milk, which gather'd in small Drops at three of the Lacteal Ducts terminating in the Nipple. This Experiment I made her repeat a second time, having myself carefully dry'd the End of the Nipple with my Handkerchief, as I had done before her first Trial. Convinced of the Truth of the Fact, I ask'd her several Questions, in order to procure Materials for a History thereof. The Substance of her Answers was as follows :

Her Name by Marriage is *Elizabeth Brian*. She is in the sixty-eighth Year of her Age since last *October*, and has not borne a Child these twenty Years and upwards. About four Years since, her Daughter being obliged to leave an Infant she then gave Suck to, in the Care of this her own Mother, and likely to be a considerable time absent; the old Woman, finding the Child froward for want of the Breast, apply'd it to her own, barely in order to quiet the Infant, without the least Thoughts of Milk. And this having reiterated several times, a Son of hers, by that time grown a Man, perceiv'd that the Child seem'd to swallow somewhat from the Nipple; whereupon he begg'd Leave of his Mother to try if she had not Milk. The Experiment succeeded: The Youth drew Milk from that same Breast from which he had been wean'd above twenty Years, and which had been unaccustom'd to any for seventeen or eighteen Years before: The good Woman then continued to suckle her Grand-child in earnest: And in some time her Daughter, *viz.* the Infant's Mother, seeing she was provided with such an extraordinary and tender Nurse, was embolden'd to bid fair for an Increase of Issue, which

which till then she knew not how to nourish or provide for. Accordingly, at the End of two Years, she brought forth another Child; whereupon the Grandmother wean'd the first, and suckled the latter; which she has done these last two Years, and still continues to do. And this Infant, in my Presence, took the Nipple with as much Eagerness, and seeming Delight, as I ever perceived in a Child of two Years old; and at it plainly perform'd the Actions of Suction and Deglutition. The two Children, both Girls, are, as to Constitution, such as I could wish to the dearest Friend; plump and firm in Flesh; in Complexion cleanly, fair and healthy, and in Temper brisk and sprightly; considering the Lowness of their Condition and Education, and the mean Diet of the Nurse.

When this good Woman came to Town, which was near two Years since, her Milk abounded to that Degree in both Breasts, that, to convince the Unbelieving, she would frequently spout it above a Yard from her: A Particular which, among others, the good Man and Woman of the House, and others of the Neighbourhood, likewise assured me of. Now her left Breast is run dry, and she has no great Quantity in the Right: But what there is, is as good Milk as one may desire in a Nurse.

The poor Woman seems perfectly honest and artless, and even inclines strongly to Dotage. She very religiously throws the Whole upon a Miracle.

I beg Leave to subscribe with Gratitude and Respect,
both in a supreme Degree, *Honoured SIR,*

*London, Jan. 8.
1733. St. Jul.*

*Your most obliged
and obedient Servant,*

Thomas Stack.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *July, August, September,*
 and *October, 1739.*

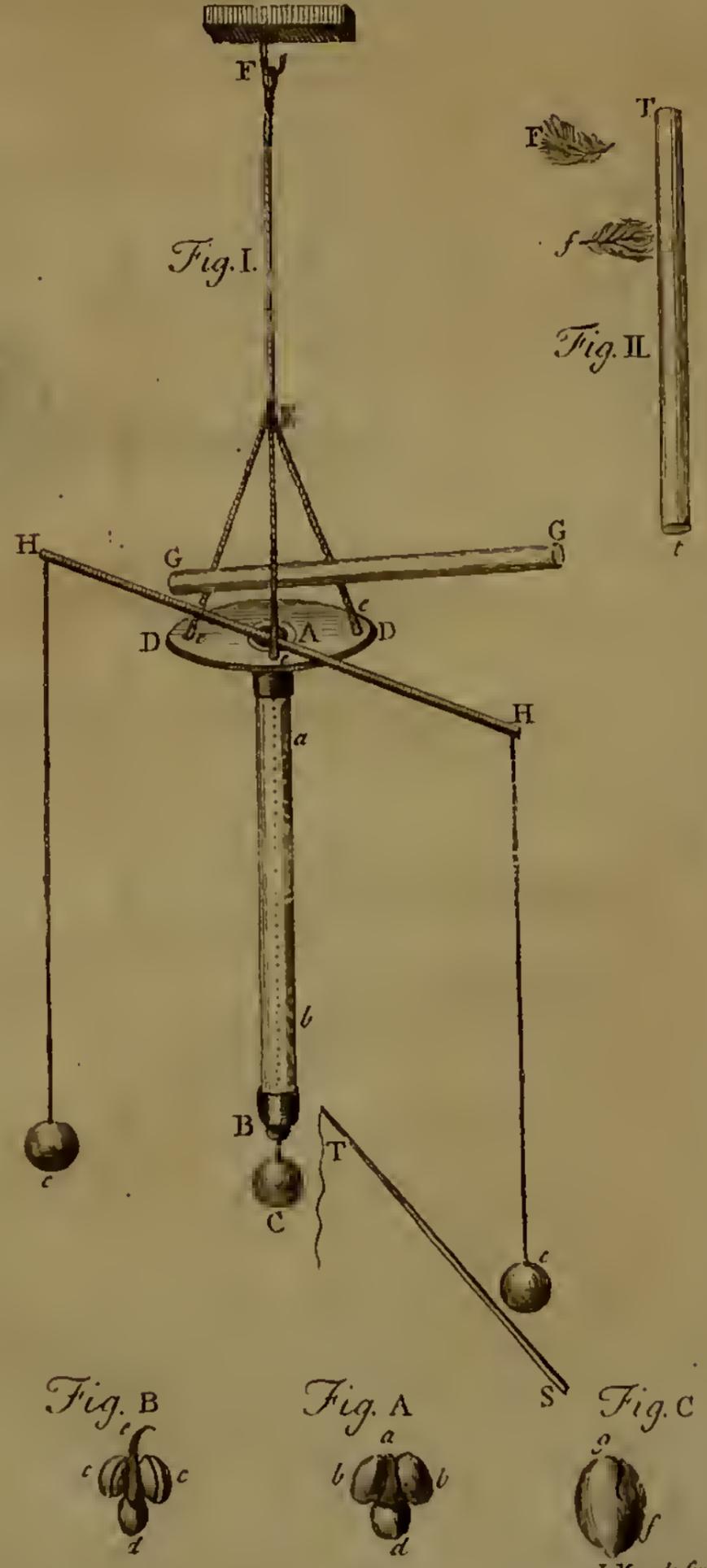
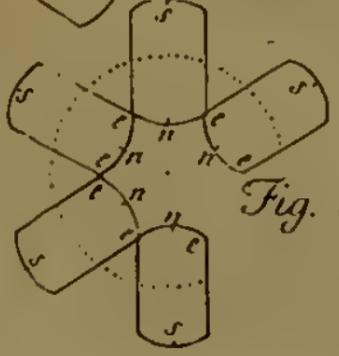
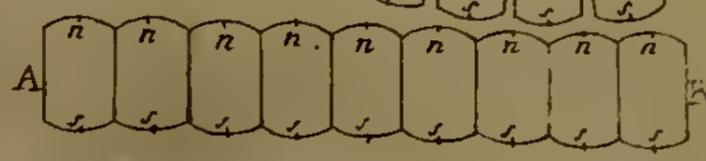
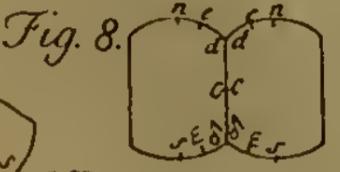
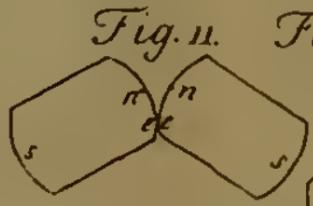
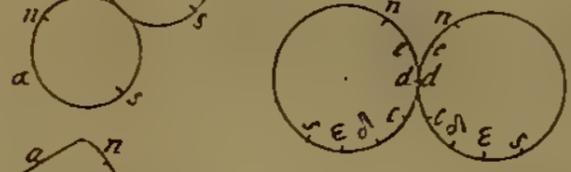
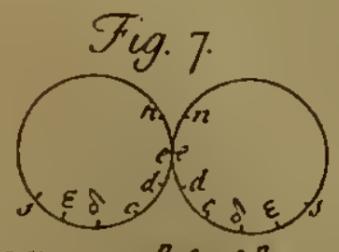
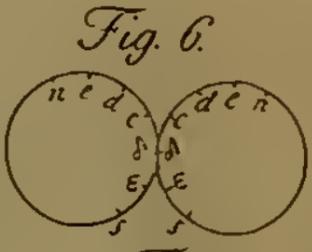
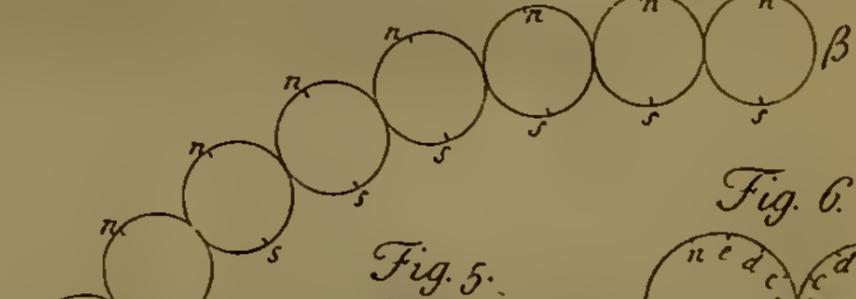
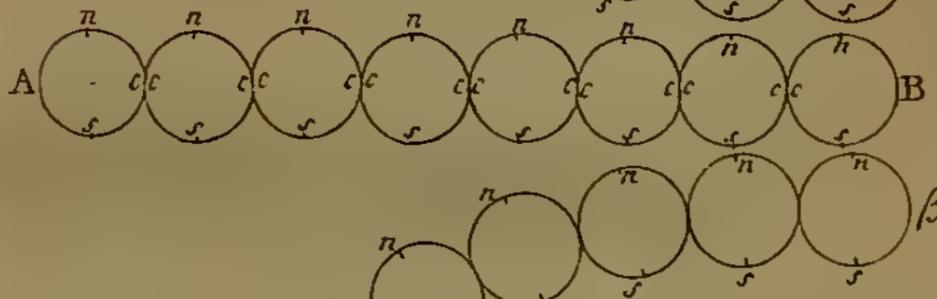
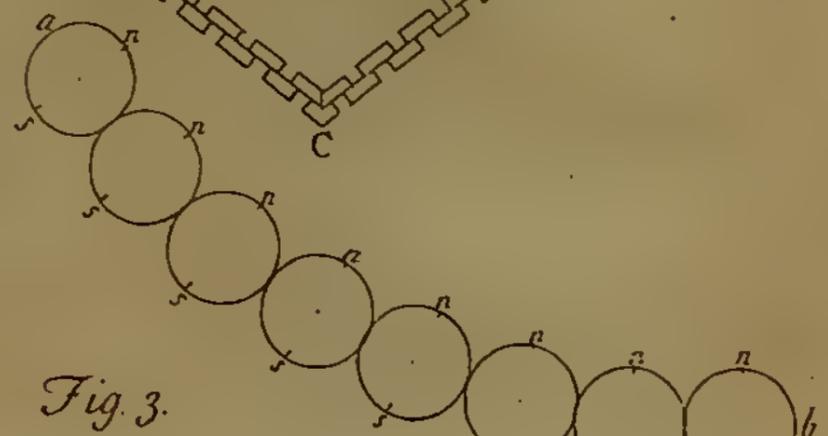
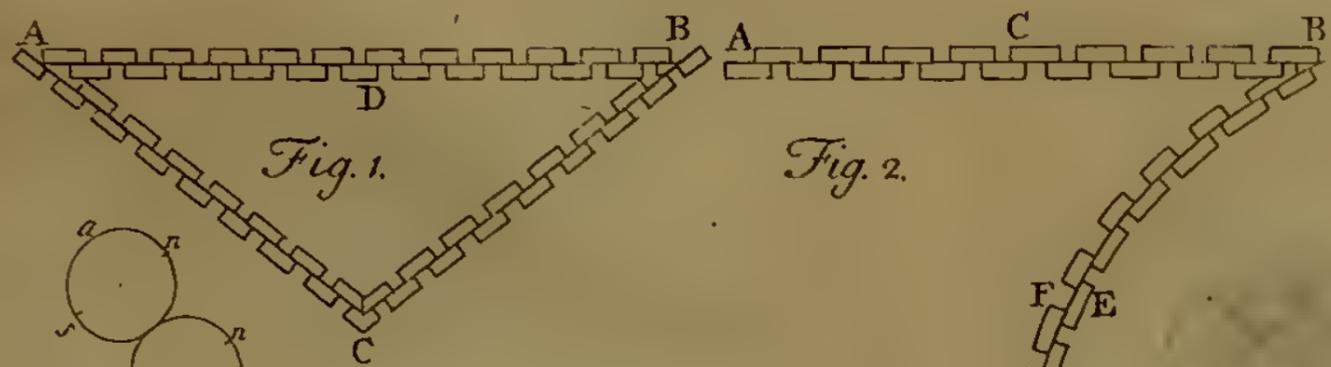
The CONTENTS.

- I. *A Letter from the Rev. Mr. John Clayton (afterwards Dean of Kildare in Ireland) to Dr. Grew, in Answer to several Queries relating to Virginia, sent to him by that learned Gentleman, A. D. 1687. communicated by the Right Reverend Father in God Robert Lord Bishop of Corke, to John Earl of Egmont, F. R. S.*
- II. *An Experiment to prove, that Water, when agitated by Fire, is infinitely more elastic than Air in the same Circumstances; by the late Rev. John Clayton, Dean of Kildare in Ireland. Communicated by the same Hand as the preceding.*
- III. *Part of a Letter from John Green, M. D. Secretary of the Gentlemens Society at Spalding in Lincolnshire, to C. Mortimer, M. D. Sec. R. S. serving to inclose a Relation of a Girl three Years old, who remained a Quarter of an Hour under Water without Drowning.*
- IV. *The Case of Mr. T. Cox, Surgeon at Peterborough, who fell into a Pestilential Fever, upon Tapping a Corpse lately dead of an Hydropsy, drawn up by himself, and read before the Peterborough Society, Sept. 1. 1736. communicated to the ROYAL SOCIETY by the Rev. Mr. Tim. Neve, Secretary to the Peterborough Society.*
- V. *The Variation of the Magnetic Needle, as observed in Three Voyages from London to Maryland, by Walter Hoxton, Gent.*
- VI. *Some Thoughts and Conjectures concerning the Cause of Elasticity, by J. T. Desaguliers, LL. D. F. R. S.*
- VII. *Some*

The CONTENTS.

- VII. *Some Thoughts and Experiments concerning Electricity*, by J. T. Defaguliers, LL. D. F. R. S.
- VIII. EXPERIMENTS made before the ROYAL SOCIETY, Feb. 2. 1737-8. *by the Same.*
- IX. *An Account of some Electrical Experiments made before the ROYAL SOCIETY on Thursday the 16th of February 1737-8. By the Same.*
- X. *An Account of some Electrical Experiments made at his ROYAL HIGHNESS the PRINCE of WALES's House at Cliefden, on Tuesday the 15th of April 1738. where the Electricity was conveyed 420 Feet in a strait Line. By the Same.*
- XI. *Observationes Botanicæ, Plantarum quarundam Descriptiones accuratiores exhibentes; per Dn. Paulum Henricum Gerhardum Mœhring, M. D. factæ; ad Illustrissimum Dn. Hans Sloane, Bart. R. S. Præf. missæ.*
- XII. *Observatio Anthelii Vitembergæ spectati; per J. Frid. Weidlerum Matthes. Prof. ibidem Primar. & R. S. Lond. Soc. communicata in Epistola ad C. Mortimerum, R. S. Secr.*
- XIII. *Occultatio Palilicii A. 1738. d. 23. Decembris, St. N. observata a D. Christfried Kirchio, Astronomo Regio Berolini; ex literis ad J. F. Weidlerum, datis excerpta.*
- XIV. *Eclipsis Solis observata Vitembergæ Saxonum D. IV. Aug. St. N. d. xxiv. Julii St. Vet. A. CIOIOCCXXXIX, post. merid. a Jo. Friderico Weidlero, R. S. Lond. S.*
- XV. *Part of a Letter from Mr. J. Derby to Mr. Henry Sheppard, concerning a terrible Whirl-wind, which happen'd at Corne-Abbas in Dorsetshire, Oct. 30. 1731. communicated to the ROYAL SOCIETY by Edmund Halley, LL. D. V. Pr. R. S. and Astronom. Reg.*
- XVI. *An Account of Letters found in the Middle of a Beech, by J. Theod. Klein, Secretary of Dantzick, F. R. S. Oct. 19. 1729. communicated to the ROYAL SOCIETY by Sir Hans Sloane, Bart. Pr. of the R. S. and Physician to His MAJESTY, &c. Translated from the Latin by T. S. M. D.*
- XVII. *Part of a Letter from Sir John Clark, one of the Barons of His MAJESTY's Exchequer in Scotland, and F. R. S. to Rog. Gale, Esq; Tr. R. S.* I. A







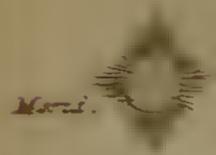
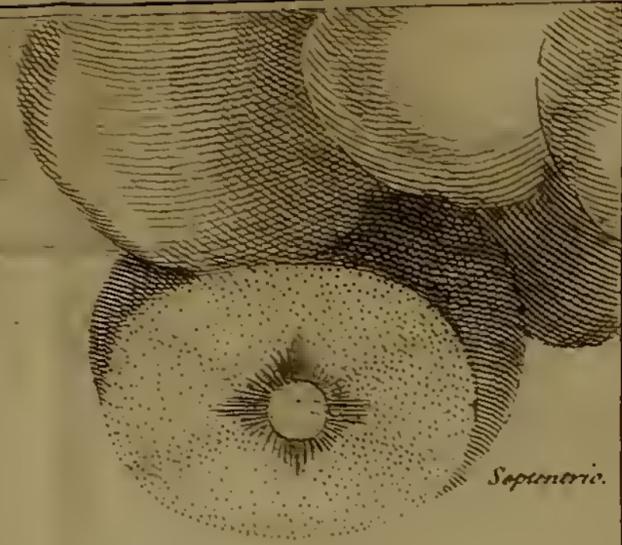


Fig. 1.



Septentrio.

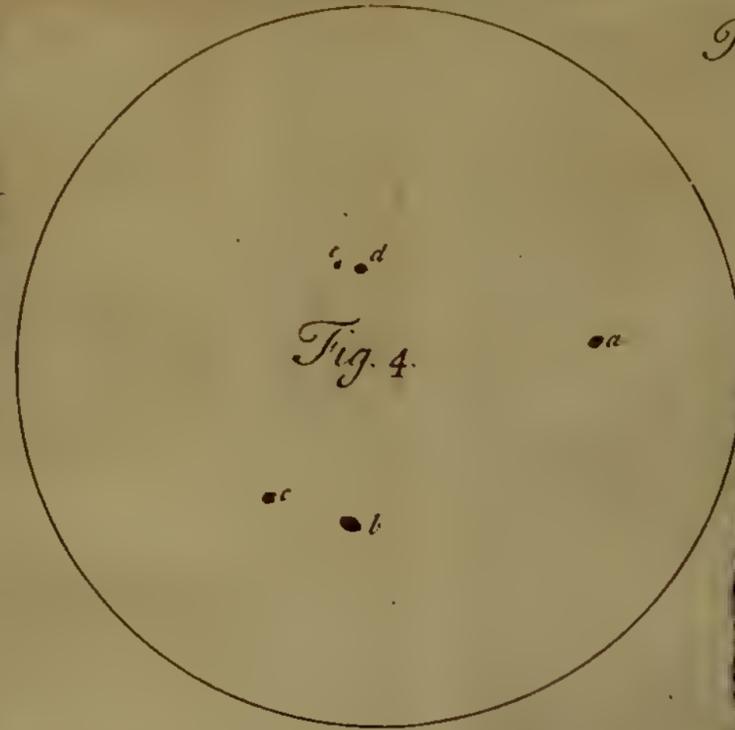


Fig. 4.

Fig. 5.

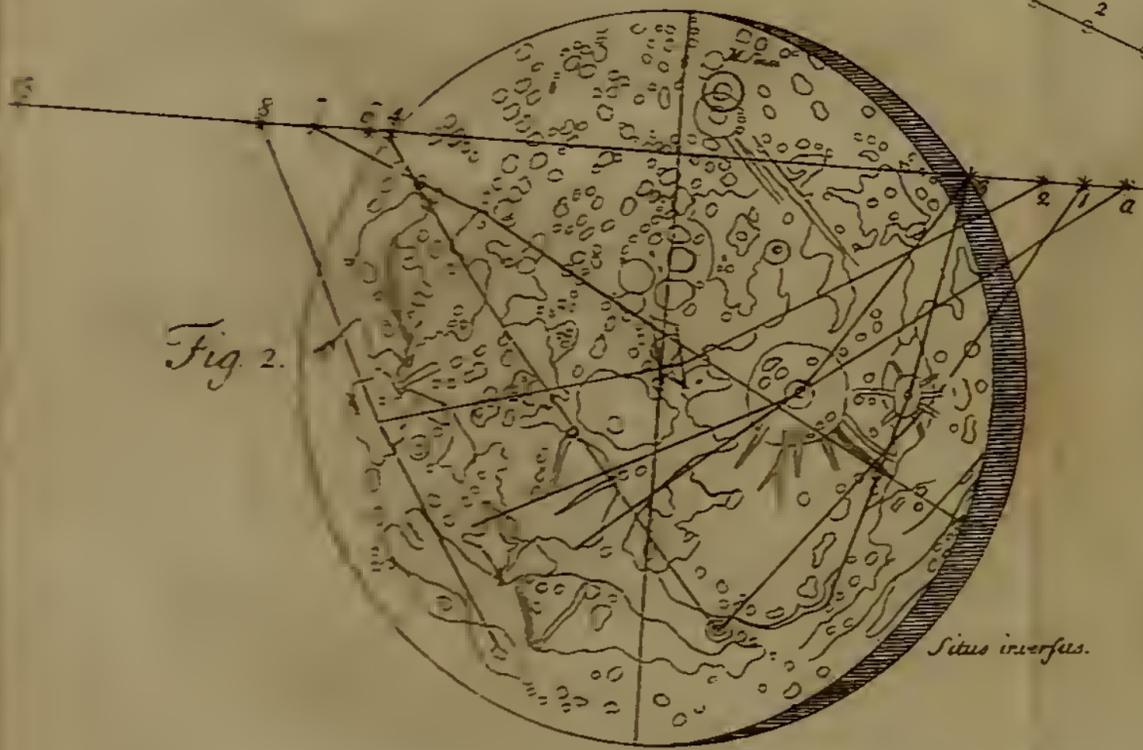
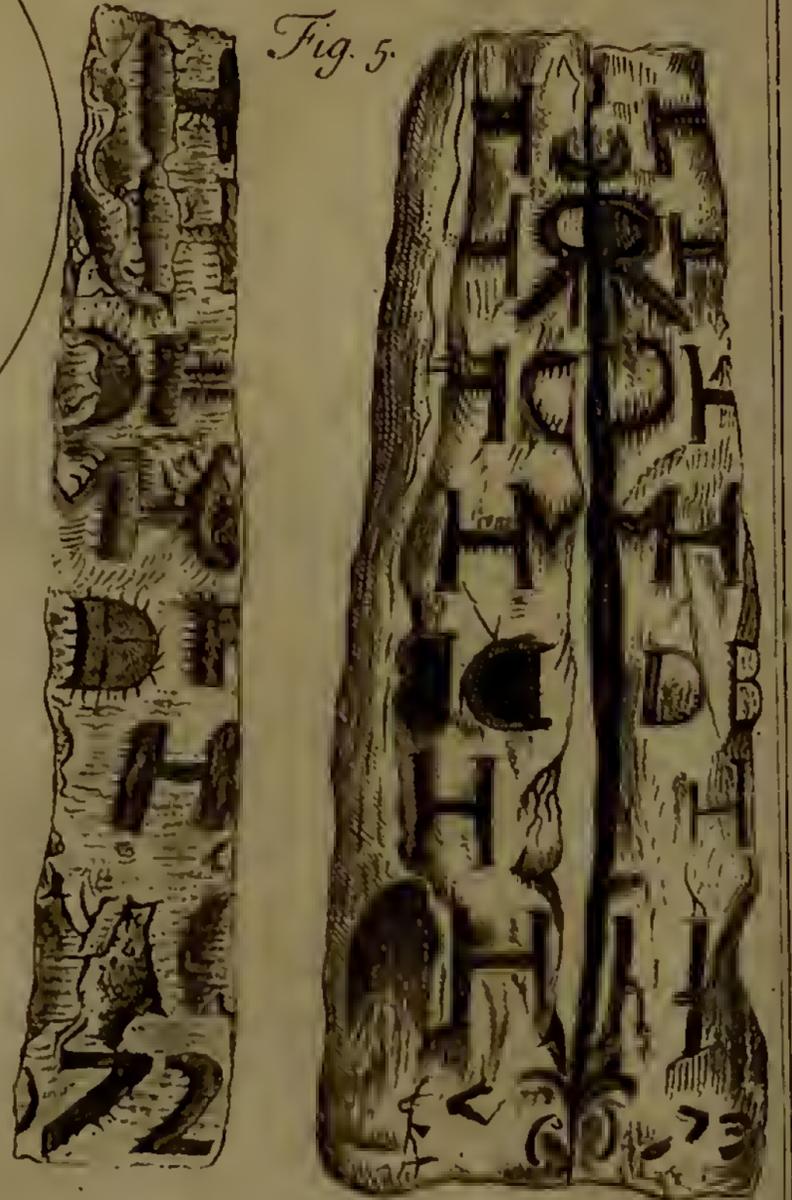


Fig. 2.

Situs interfus.

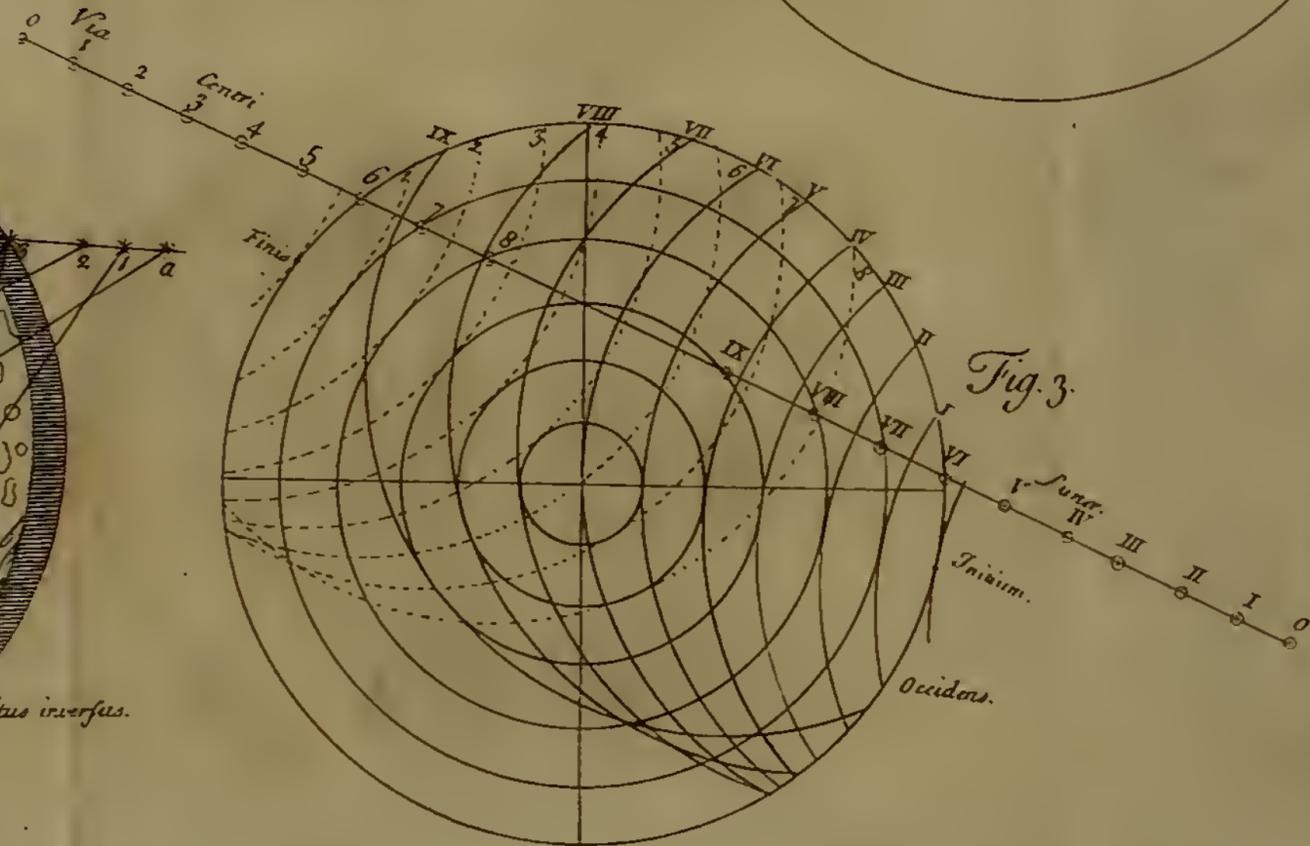


Fig. 3.

Scala Minutorum Gradus.
0 5 10 15 20

I. *A Letter from the Rev^d Mr. John Clayton, (afterwards Dean of Kildare in Ireland) to Dr. Grew, in Answer to several Queries relating to Virginia, sent to him by that learned Gentleman, A. D. 1687. * communicated by the Right Reverend Father in God Robert Lord Bishop of Corke, to John Earl of Egmont, F. R. S.*

— I Have observed many gross Mistakes in Peoples Notions of *Virginia*, when discoursing of the Natives, which have risen from the want of making a Distinction in their Expressions, when they speak of the *English* or *Whites*, born there, and so called *Natives*; and the *Aborigines* of the Country. Please therefore to take Notice, that when I speak of the Natives in general, I mean only the *Indians*.

As therefore to your first Query: Their *Wiockiff*, that is, their Priest, is generally their Physician; and is a Person of the greatest Honour and Esteem among them, next to the King, or to their great War-Captain.

2. Nature is their great Apothecary, each Physician furnishing himself, according to his Skill, with Herbs, or the Leaves, Fruit, Roots, or Barks of Trees; of which he sometimes makes use of the Juice, and sometimes reduces them into Powder, or perhaps makes a Decoction thereof.

3. Though every one, according to his Skill, is a sort of Doctor, (as many Women are in *England*)

* This may serve as a Sequel to the Accounts of *Virginia* formerly given by Mr. Clayton. See these *Transactions*, N^o 201. 205; 206. 210.

yet their Priest is peculiarly styled their Physician, to be consulted upon greater Emergencies. The Rules of the Descent hereof, as to Families, I do not know; for they are a sullen close People, and will answer very few Questions.

4. They reward their Physician with no certain Fees, but according as they bargain for *Wampaupeake* Skins, or the like. If it be to an *Englishman* they are sent for, they will agree for a Match-coat, a Gallon or two of Rum, or so-forth, according to the Nature of the Cure. Sometimes the Priest will sell his Remedy; for some of them have told me, that they have bought the Root which cures the Bite of the Rattle-snake from their *Wiochist*.

5. Their King allows no Salary, that ever I heard of; but every one that in any Nature can serve his Prince, is ready to do it, and to do it *gratis*.

6. They have no Consultations, their Practice being merely empirical. They know little of the Nature or Reason of Things. Ask them any Question about the Operation of a Remedy, and, if in good Humour, perhaps they will reply, *It cures*; otherwise, they will shrug their Shoulders, and you may ask forty Questions, and not know whether they understand either the Thing, or what it is that you say to them.

7. They pay a certain Deference of Honour to their Priest or *Wiochist*, whose Person they hold sacred; but Laws they have none (as far as I could ever learn) that binds them thereto: In general, the Will of their Prince stands for Reason and Law.

8. The Means whereby they convey their Art to Posterity, I take to be this: They lodge in their
Wiochisan

Miochisan Houses, *i. e.* their Temples, certain kinds of Reliques, such as Mens Skulls, some certain Grains or Pulse, and several Herbs, which are dedicated to their Gods; *viz.* The Skulls in Memory of their Fights and Conquests; the Pulse by way of Thank-offering for their Provisions; and the Herbs upon the same Account, for some special Cure performed thereby. For when any one is cured by any Herb, he brings Part thereof, and offers it to his God; whereby the Remembrance of this Herb and its Virtue is not only preserved, but the Priest also becomes best instructed thereby, and knowing in the Art of Medicine: For otherwise, they are mighty reserved of their Knowledge, even among themselves. Whether the Priest takes certain Persons to instruct; or teaches only his own Children, I know not. Often when they are abroad hunting in the Woods, and fall sick, or come by any Hurt, they then are forced to make use of any Herbs which are nearest at Hand, which they are not timorous in venturing upon, though they know not the Virtue or Qualities thereof. And thus, by making many Trials and Experiments, they find out the Virtues of Herbs; and by using simple Remedies, they certainly know what it is that effects the Cure.

9. They are generally most famed for curing of Wounds, and have indeed various very good Wound-herbs, as an Herb commonly called *Indian-weed*, which perhaps may be referred to the *Valerians*, and be said to be *Platani foliis*. They use also the *Gnaphalium Americanum*, commonly called there *White Plantain*. As to our *Plantain*, or the *Heptapleuron*, they call it the *Englishman's-foot*, and have a Tra-

dition, that it will only grow where they have trodden, and was never known before the *English* came into this Country. The most famous old Physician among the *Apomatick Indians*; as I was informed by a Person of a very good Understanding, used mostly an Herb which he shewed me, whose Leaf is much like *Self-heal* in Winter. I observed it was red underneath, and would at length appear tinged on the upper Side also: It makes a good Salve, only it fills a Wound too fast with Flesh. I took a Draught of this Herb, along with some others, which I have left in the North of *England*. The great Success they have in curing Wounds and Sores, I apprehend mostly to proceed from their Manner of dressing them: For they first cleanse them, by Sucking, which, though a very nasty, is, no doubt, the most effectual and best way imaginable; then they take the *biting Persicary*, and chew it in their Mouths, and thence squirt the Juice thereof into the Wound, which they will do as if it were out of a Syringe: Then they apply their Salve-herbs; either bruised or beaten into a Salve with Grease, binding it on with Bark and Silk-grass. Colonel *Spencer*, the present Secretary of State of *Virginia*, told me of a very strange and extraordinary Cure performed by an *Indian*, on one of his *Negroes*. The *Negro* was a very good Servant, wherefore his Master had valued him much; but by Degrees he grew dim-sighted, and was troubled with terrible Pains in his Eyes, so that with one he could see but a little, and none at all with the other; and as the Pain still increased, the Colonel was greatly apprehensive, lest his *Negro* would be quite blind. Several Surgeons were sent for,

for, who had tried to cure him, but in vain; when an *Indian*, coming to the House, said he would cure him; they told Mr. Secretary thereof, who sent for the *Indian*, and agreed with him for two Quarts of Rum. The *Indian* told him, that he could save one Eye, but that the *Negro* would be blind of the other. The next Morning the *Indian* went a hunting into the Woods for his Herbs, and returned with them about Noon, which he bruised, putting thereto a little Water; and having pressed forth some of the Juice, he dropped some thereof into the Eye which he said would be blind, and laid the Herbs thereon, which he would have bound fast with Bark; but the Colonel called for some Linen Rags, and had it bound up therewith. He then intimated to the Colonel, that shortly after Sun-set the *Negro* would be mad, if his Medicine took Effect, but would come to himself again before Morning; wherefore strict Orders were given, that he should be well attended, and that nothing should be altered, let what would happen. All things therefore being accordingly done as the *Indian* had directed, every thing succeeded likewise as the *Indian* had foretold. Then, about 11 o'Clock the next Day, the Binding being removed, and the Herbs taken off from the Eye, the *Indian* bid the *Negro* hold down his Head, which when he had done, out-dropped the Crystalline and aqueous Humours. The *Indian* afterwards bound it up again, and by Degrees the *Negro* was freed from his Pain, and had perfect Sight with the other Eye. What the Herbs were, the Colonel could never learn from him, though he proffered him whatever he would demand.

ro. The Distempers amongst the *English* Natives (for I cannot give so particular an Account of the Distempers most predominant among the *Indians*) are, *Scorbutical Dropsies, Cachexies, Lethargies, Seasonings*, which are an intermitting Fever, or rather a continued Fever with quotidian Paroxysms. These are now rarely sharp, but shew themselves in a lingering Sickness. The *Griping of the Guts* mostly dry, and when the *Tormina Ventrīs* cease, they generally shoot into the Limbs, and fix there, in a terrible sort of Gout, taking away the Use of the Limbs. Thus they will pine away to Skin and Bone, so that their Joints will seem dislocated, and their Hands utterly crippled. *Sore Throats*, which the last Year were very frequent, and deemed infectious, running generally through whole Families, and, unless early prevented, became a cancerous Humour, and had Effects like the *French-pox*. Likewise *Pains in the Limbs*, which I apprehend to have proceeded partly from the same Humour floating up and down the Body. These Pains are very exquisite, mostly nocturnal; for while they walk, if they have the Use of their Limbs, they feel the least Pain. The Oil of a Fish called a *Drum*, was found very effectual to cure these Pains, and restore the Limbs. I was Eye-witness when a very worthy Gentlewoman, who had lost the Use of her Limbs, was intirely recover'd by the Use of this Medicine: For her Feet (being anointed with this Oil, the Pains flew into her Head; her Head thereupon being anointed, the Pain descended again; then anointing both Head and Feet, she was recover'd. There are three sorts of Oils in that Country, whose Virtues, if fully proved, might
not

not perhaps be found despicable: The Oil of *Drums*, the Oil of *Rattle-snakes*, and the Oil of *Turkey Bustards*. The Oil of *Sassafras-leaves* may be deservedly consider'd too, for they will almost intirely dissolve into an Oil. But to return: There is another sort of Distemper, which I judge to be the *Lepra Græcorum*. And it may perhaps be no bad Conjecture, that this chiefly proceeds from their feeding so much as they do, on a delicate luscious sort of Pork. Among the *Indians* they have a Distemper which they call the *Taws*, which is nearly related to the *French-pox*; which they are said to cure with an Herb that fluxes them: But this I have only by Hear-say.

11. The *Indians* mind neither the Pulse nor Urine, only judge by the common most remarkable Symptoms; and some pretend to form a Judgment from the Countenance, and are fond of being thought Physiognomists.

12. I never could find, that they practis'd the *letting of Blood*. They purge much with several sorts of Roots of their own Country Growth, and vomit frequently with various Herbs. They sweat boldly and excessively, and after a very strange manner: For they have their Sweating-stoves always upon the Bank of some River; whence they rush forth in the Height of their Sweat, and run into the River, where they wash and bathe themselves very plentifully. They use no *Blistering-plaisters*, but are exquisite at Cupping. As the *East-Indians* use *Moxa*, so these burn with *Punk*, which is the inward Part of the Excrecence or Exuberance of an *Oak*. When they design to give a *Purge*, they make use of the following

ing

ing Herbs: *Poake-root*, i. e. *Solanum bacciferum*, a strong Purge, and by most deemed Poison. The Roots of *Tythimal*, of which I have observed two sorts; the one *Flore minimo herbaceo*, the other *Flore albo*. The Flower of this last is small, but large in Comparison with the other: They are *repentes*, and grow in old manured Grounds. They chiefly make use of the latter of these, and it is a most excellent Purge, though it sometimes vomits: It is a quick, but moderate Worker enough; and has this Peculiarity, that it opens the Body in the *Gripes*, when other much violenter Purgatives will not move it. There is another Herb, which they call the *Indian Purge*: This Plant has several woody Stalks growing near three Feet tall, and, as I remember, *perfoliat*: It bears yellow Berries round about the Joints: They only make use of the Root of this Plant. They use also the *small Fleur de Lis*, whose Virtues, I believe, are not yet half known, for it has some extraordinary Qualities: It does not grow above a Hand high, flowers in *March*, and is very fragrant. They use also some sort of the *Apocynum's*; particularly that which I think *Gerard* calls *Vincetoxicum Americanum*; for there are several sorts of *Apocynum's*, I think 13 or 14, but they are not all purgative: For having got some of the Root from an *Indian*, which he assured me was the *Rattle-snake-root*, I thought the Root of an *Apocynum* (which may well be distinguished by that of *Rosæ Mariæ foliis*) was very like it, both in Shape and Taste, considering the one dried, and the other fresh: Wherefore I got some Quantity thereof, and carrying it in my Pocket, I ventured to eat thereof, little
by

by little, till I believe I have taken a Drachm at a time, to observe if it had any peculiar Operation on the Body; but could never find, that it had.

They have likewise several sorts of Herbs, where-with they vomit; one of which is a little sort of *Squills*. They likewise take the Leaves of a certain curious odoriferous Shrub, that grows in the Swamps, which I take to be the *lesser Sassafras*; they bruise them in Water, and then express the Juice, which they drink warm. The *Indian* Interpreter, who taught me this, prized it much, as excellent Physic, and said they found it a very sovereign Remedy. It is as odoriferous as any Shrub I ever smelt at in my Life: Whoever has once taken Notice of the Smell, cannot forget it, or be deceived therein afterwards, having something peculiar in it. The Name which the *Indian* gave me hereof was *Wisochis*, which since I understand is the general Word for Physic.

13. The rest of their *Materia Medica* consists of Herbs, of which they have great Plenty, and seldom prescribe any thing else. I have collected above 300 several sorts, that were no *European* Plants; but I shall only mention those at present, whose Virtues I take to be most remarkable. And first, the *Sassafras-tree*, whose Root is well enough known. It shoots forth its Blossoms in *March*, which are yellow, and grow in little Bunches like Grape-flowers, and which, when gathered and picked from the husky Bud, make a curious Preserve. Most *Sassafras-trees* blossom, few bear Berries, but those that do are generally very thick: They are shaped much like those of *Dulcamara*, but are black of Colour, and very *aromatic*; I take them to have considerable Virtues.

The *Gum-tree*, which I refer to the Species of Plane-trees, and distinguish it by its Fig-like Leaf, only more sharply dented. Its Leaf smells much like a Lemon. Their Practice is to beat the Tree, and then pill off the Bark, and so scrape the Gum, which has Virtues like Turpentine, or rather more astringent and drying. This they usually mix with their common Turpentine, which is whiter and more Butter-like, than the *Venice* or *Chios* Turpentine. *Quere*, whether better or no? The further Method of preparing this Medicine, as I am told, is this: They expose it to the Sun on Paper, where at first it rather seems to melt, but it will afterwards grow hard; they then beat it to a Powder, and administer it. They use much the young Buds of the *Populus*, *sive Tulippa arbor*, a vast large Tree, extraordinary spacious, bearing Flowers about *April*, much like Tulips; its Leaves are large, smooth, and well-shaped, which, together with the Flowers, render the Tree exceeding beautiful to behold. It bears its Seed coniferous, and is an excellent Opener of Obstructions. The *Sorrel-tree* bears a Leaf something like a Laurel, in Taste much resembling *Lujula*. They use it in Fevers, and, as I am informed, with good Success. This Tree grows plentifully on the South-side of *James River* in *Virginia*; I cannot say I ever found it to the Northward. The *Swamp-plum-tree*, whose Wood they calcine, and make into Charcoal, which they beat to a Powder, then mix it with Grease, and make an Ointment thereof, with which they anoint the Body, and foment it very much, whereby they cure the *Dropsy*; for it opens the Pores to that Degree, that the Water runs down their Legs. Among their
 their

their Herbs, I have had 40 several Sorts, or near that Number, shewed me as great Secrets, for the *Rattle-snake-root*, or that kind of *Snake-root* which is good for curing the Bite of the *Rattle-snake*: But I have no Reason to believe, that any of them are able to effect the Cure. One Gentleman shewed me a certain Root, which was a *Smilax*, and assured me, that that was certainly the *Rattle-snake-root*. And afterwards, when I shewed Mr. Secretary *Spencer* the same Root, he said that certain *Indians* had given him of the same Root for the *Rattle-snake-root*, and that he had some Quantity to send for *England*; but this Root is by no means the same with that which I have mentioned before, in Answer to *Quære* 12. which I said was like the Root of an *Apocynum*, which I myself obtained from an *Indian*, who seemed to prize it highly, having sewed it carefully up in Leather, on the Inside of his Belt. Others have shewed me *Chrysanthemum ferulaceis foliis* for it; others *Chrysanthemum tragopyri foliis*. Again; general Report goes in favour of the *Asarum Cyclaminis foliis*, which many therefore particularly call *Rattle-snake-root*. There are strange Stories told in favour of an Herb called *Dittany*, which however is not of the *Dittany* kind, but is only a Mountain *Calaminth*. This, they say, will not only cure the Bite of a *Rattle-snake*, but that the Smell thereof will kill the *Snake*. But however, * I have some Reason to believe, that this Herb will not cure the Bite, nor that the Smell thereof will kill the

* See *Miscel. Curiosa*, Vol. III. p. 352.

Snake; for Colonel *Spencer* assured me, that he had an Opportunity of making an Experiment thereof upon a Dog which was bitten by a *Rattle-snake*, to which he gave plentifully of the Juice of his *Dittany*, as they called it; but the Dog died nevertheless a Day or two after. And Mr. *Wormley*, one of the Council of State in *Virginia*, told me, that being in Company with another Gentleman, he had an Opportunity of making the following Experiment; for seeing a *Rattle-snake* in her Coil, they went and got a Bunch of this *Dittany*, and tied it to a Pole; then putting the *Dittany* that was thereon to the Nose of the *Snake*, it seemed to offend her, whereupon she turned away her Head, which they still followed with the *Dittany*; then the *Snake* fled, and they still pursuing her, she at last stretched herself out at Length, and lay seemingly dead. Then they laid the *Dittany* upon her Head, and went into a neighbouring House to refresh themselves; for they were tired with skipping about after the *Snake*. When they had staid near half an Hour, they returned to see their supposed dead *Snake*; but, behold! the *Snake* was fled; so that they then judged, that the *Snake* had only stretched herself out, because she had been tired with their Pursuit. I look upon it probable therefore, that some Accident of the like kind may have first given Origin to this Story; the Person who had tired the *Snake* not having regularly waited for the Event, but perhaps, to secure the Conquest, may have given the *Snake* a Stroke with a Switch upon the Back (which would have killed the *Snake* without the *Dittany*). But yet nevertheless, this Plant is of more than ordinary Virtues, and might not unprofitably

ably be used by our Physicians. It may be referred to the Class of the *Calamintha montana, pulegii odore*, which has been transferred from thence into *England*, and I think is now pretty common, but is hotter, and more sudorific.

I will now mention to you an Herb, though unknown, yet worthy to be fetched from *Virginia*, yielded the Country nothing else: It is the Herb called there *Angelica*, but which I take to be *Libanotis vera latifolia Dodonæi*. It grows generally on a rich sandy Ground, on a declining Brow, that faces the rising Sun; the Root shoots deep into the Earth, sometimes near three Feet, very tender, and easily broken, of a white or rather Cream-like Colour; and being lactescent, yields a little Milk, thick and yellow as Cream; a very early Plant. It seldom flowers or seeds under five Years Growth; for I have fully and distinctly observed that Number of Years in the several Sorts of this Plant, by the Growth of those not come to Maturity to bear Seed; and it is observable, that those which do not seed, have rarely more than one Branch, which divides when it spreads, and subdivides itself still into Three. The Leaf is much like our wild *Angelica*, only thinner, and more the Colour of a Willow-green. Those that seed, have a fistulous Stalk about the Thickness of *Dill*, a white umbelliferous Plant; the Seeds are much like *Angelica-seed*, but from the Fragrancy of the Root, and its being peculiarly bearded, I undoubtedly style it a *Libanotis*. It stops the *Flux*, and cures it to a Wonder. Again; it often loosens and purges the Bodies of those that are bound, and have the Gripes, especially if it proceeds from Cold; and prevents many
unhappy

unhappy Distempers. I have Reason to speak well of it; for it is to it, under GOD, that I attribute the saving of my own Life. I have known it give 14 or 15 Stools, whereas it will not move a Child in Health. I take it to be the most sovereign Remedy the World ever knew in the Griping of the Guts, and admirable against Vapours. It is sudorific, and very aromatic, and will not be concealed; for where-ever it is mixed, it will have the predominant Scent. It is mostly called by those who know it in *Virginia*, by the Name of *Angelica*: But shewing a Piece of the Root to a great Woodsman, to see whether he knew it, and could tell me where it grew, he seemed surpris'd to see me have thereof; and told me, that he kept an *Indian* once for some Weeks with him, because he was an excellent Woodsman, and going a hunting, (*i. e.*) shooting, they came where some of this Root grew: The *Indian*, rejoicing, gather'd some of it, but was very careful to cut off the Top of the Root, and replant it: He then asked him, Why he was so careful? Whereunto the *Indian* replied, It was a very choice Plant, and very scarce; for they sometimes travelled 100 or 200 Miles without finding any of it. He then asked him, What Use it was of? To which the *Indian* answered, You shall see by-and-by. After some time, they spied four Deer at a Distance; then the *Indian*, contrary to his usual Custom, went to Windward of them, and sitting down upon an old Trunk of a Tree, began to rub the Root betwixt his Hands; at which the Deer tossed up their Heads, and snuffing with their Noses, they fed towards the Place where the *Indian* sat, till they came within easy Shot of him; whereupon he fired at them, and killed a
 large

large Buck. The Truth of this Story I no further assert, than that I was told it by a Person of seeming Seriousness, who had no Inducement to tell a Lye, or impose upon me: But I have often taken Notice, that the *Indians* smell generally strong of this Herb. And I have since learned from others, that the *Indians* call it the *Hunting-root*, which makes me more inclinable to give Credence to this Story. Another Gentleman, a white Native of that Country, when I once pulled a Piece of the Root out of my Pocket to bite thereof, (for I frequently carried some of it about me) asked me, If I loved Fishing? I required, Wherefore he asked me that Question? Because, said he, you have gotten some of the *Fishing-root*. The *Fishing-root*! replied I; pray why do you give it that Name? Because, said he, when we were Boys, we used to get some of it to lay with our Baits to invite the Fish to bite. This I can say of my own Knowledge, that having one Day got some Quantity of the Root, and likewise of the Branches, to distil, the strong Scent, as I went home, palpably put me into a breathing Sweat. In the Night I was waked by a Rat, which ran over my Face, whereas I never at any other time had the like happen to me; but will not be positive to conclude, that this Root was the Cause thereof, only the precedent Relations made me reflect thereon. There is another Root of the Species of *Hyacinths*, the Leaves whereof are grass-like, but smooth and stiff, of a willow-green Colour, and spread like a Star upon the Ground; from the Middle shoots a tall long rush-like Stem, without Leaves, near two Feet high; on one Side grow little white Bell-flowers one above another: The Root is black.

black outwardly, but brown within. It is bitter, and I take it to have much the same Virtues as *Little Centaury*. Some call it *Ague-grass*, others *Ague-root*, others *Star-grass*. I have likewise been told by several, of a Root which the *Indians* cure Bruises wonderfully with; but I apprehend it is the same Root with which the *Indian* cured the *Negro's* Eye afore-mentioned; for it operates much after the same manner, according to their Relation, making the Patients mad for some Hours, if they be recoverable. It is not to be applied where the Skin is broken. They use it thus: They chew some of the Root in their Mouths, and then squirt it forth on the bruised Part, fomenting it well with their Hands; then they give a little to the Person bruised to chew, who must swallow the Juice, but spit forth the Root again, which they bind on the Part aggrieved. If the Relations I have had of Cures performed thereby, be absolutely true, the World has not yet discovered a more wonderful Remedy. I had it described to me by Colonel *Smith*, of the *Isle of Wight* County, to be like *Langue de Bœuf*, with a yellow Flower, and rough hoary Leaf, the Root yellowish, and tasted something sweetish like Liquorice. There are several others I might name, whose Virtues are by no means despicable; such as the *Chrysanthamum platani foliis*, whose Root is very useful in *old Pains*, the *Sciatica* and *Gout*. It is a large Herb, grows betwixt five and six Feet tall. There are likewise many others, which bear some Analogy to the *European* Plants, such as *Solomon's-seal*, *Wood sage*, much better, I think, than the *English*; which the *Indians* use much for Infusions, and which they take as we do Diet-drink.

drink. *Little-Centaury*, red, white, and yellow, &c. However, I never could find above 12 or 14 Plants, which were Natives of that Country, that agreed perfectly with any of our *European* Plants, but what had some notable Differences, if they were not rather to be reckoned a distinct Genus.

13. There go Traditions of their having an Art to poison their Darts; but I could never find any solid Grounds for that Report. I have observed, that in those Countries, upon an ill Habit of Body, the least Scratch is dangerous; and that, for all the Care that can be taken to prevent it, it often turns into a very desperate ulcerous Sore. Some Herbs there are of an analogous Nature with *Hemlock*, whereof, I think, they know nothing further, than that they are to avoid them; but any Herbs, wherewith they poison their Darts, I never could hear specified. And as Persons engaged in long Marches are liable to many Accidents, which may contribute to an ill State of Health; when a slight Wound in Battle has then proved mortal; this I apprehend to have been the Cause, why the Physician has rather chosen to attribute the Death of his Patient to the Poison of the Dart, than the want of Skill in himself.

14. As to their Morals, they are simple and credulous, rather honest than otherwise, and unpractised in the *European* Art of Lying and Dissimulation; but as to the brutal Passions, they are sottish and sensual as the Beasts of the Field.

15. They are almost always either eating or sleeping, unless when they go a hunting: At all Hours of the Night, whenever they awake, they go to the *Homing-pot*, that is, Maze dressed in a manner like

our pilled Wheat; or else a Piece of Venison *barbecuted*, that is, wrapped up in Leaves, and roasted in the Embers.

16. They drink, I think, little besides *Succahannah*, that is, fair Water, unless when they can get Spirits, such as Rum, from the *English*, which they will always drink to Excess, if they can possibly get them; but do not much care for them, unless they can have enough to make them drunk; and I have heard it said, that they wonder much at the *English* for purchasing Wine at so dear a Rate, when Rum is much cheaper, and will make them sooner drunk.

17. They use *Tobacco* much, which they smoak in short Pipes of their own making, having excellent Clay, which I tried a little before I came for *England*, making Crucibles thereof, which I could not discern were inferior to the *German*. They make also neat Pots of the same Clay, which will endure the Fire for any common Uses.

18. They have no *Opium*, though in some old Fields upon *York-River*, I found Poppies perhaps of no despicable Virtue. I have been told, that in Fevers, and when their Sick cannot sleep, they apply the Flowers of *Stramonium* to the Temples, which has an Effect like *Laudanum*. I have had asserted by many, that when the Soldiers were sent over to quell the Insurrection of *Bacon*, &c. they being at *James-town*, several of them went to gather a Sallad in the Fields, and lighting in great Quantities on an Herb called *James-town-weed*, they gathered it; and by eating thereof in plenty, were rendered apish and foolish, as if they had been drunk, or were become Idiots. Dr. *Lee* likewise assured me, that the same

Accident happened once in his own Family; but that after a Night or two's Sleep, they recovered.

19. Their *Sports* are Dancing, their *Games* are playing with Straws, which as I am not perfectly acquainted with, I find it hard to describe; I can therefore only tell you how it appears to a Looker-on: They take a certain Number of Straws, and spread them in their Hands, holding them as if they were Cards; then they close them, and spread them again, and turn them very suddenly, and seem very dextrous thereat. Their *Exercise* is Hunting, that is, Shooting with a Gun, or with Bow and Arrow, wherein they excel. Their Women work, plant the Corn, and weave Baskets or Mats.

20. Several have been very old; I cannot say, that herein there is any remarkable Difference between them and the *English* Natives. If the *English* live past 33, they generally live to a good Age; but many die between 30 and 33.

21. I have been told, that one of their famous *Wiobists* prophesied, that bearded Men (for the *American Indians* have no Beards) should come and take away their Country, and that there should none of the original *Indians* be left within a certain Number of Years, I think it was an Hundred and Fifty. This is very certain, that the *Indian* Inhabitants of *Virginia* are now very inconsiderable as to their Number; and seem insensibly to decay, though they live under the *English* Protection, and have no Violence offered them. They are undoubtedly no great Breeders.

22. Though they are sluggish by Nature, and slow of Speech, yet their Method of Expression seems

vehement and emphatical, and always attended with strong Gesticulations. They are generally well proportioned, and for the most part are rather taller than the *English*. They have all either a very dark-brown Hair, that may well be called black, or a Jet-black, all lank.

II. *An Experiment to prove, that Water, when agitated by Fire, is infinitely more elastic than Air in the same Circumstances; by the late Rev^d John Clayton, Dean of Kildare in Ireland. Communicated by the same Hand as the preceding.*

—SIR *Thomas Proby* having heard of a new Digester, which I contrived, had a Desire to see it, and some Experiments made therein. I had a small one, which I designed only for an inward Cylinder; this I could easily put in my Pocket: Wherefore, going to pay him a Visit at *Elton* in *Huntingdonshire*, I took it along with me; and having softened a Bone therein in a very short Space, he was desirous to know the shortest Time it was possible to soften a Bone in: I told him, I thought I could soften the Marrow-bone of an Ox in a very few Minutes, but that that Vessel was very weak, and I feared would not endure the Pressure of so violent a Heat; yet seeming desirous to have the Experiment tried, I said I was ready to venture my Vessel: Then having fixed all things right, and included about a Pint of Water, and,

and, I believe, about 3ij of a Marrow-bone, we placed the Vessel horizontally betwixt the Bars of the iron Grate into the Fire about half way; and in three Minutes time I found it raised to a great Heat; whereupon I had a mind to have taken it out of the Fire, lest it should have burst; telling Sir *Thomas* of the Danger that I apprehended: For I remember'd, that the Screws of a Digester, made after Mr. *Papine's* Method, giving way, the Head flew one way and the Screws and Irons another, with such Violence, that the Head, having hit against a Brick, cut a Piece clearly out of it; which was one Reason and Motive to my contriving a Digester this way, that the Screws cannot possibly start, but that the Vessel would sooner break in any other Part: But in this (I added) I thought the Bottom would first burst, it being only soldered in. Scarce had I done speaking, and Sir *Thomas* thereupon moved his Chair to avoid Danger; but seeing the Heat become more raging, I stepp'd to the Side-table for the Iron wherewith I managed the Digester, in order to take it out of the Fire, when, on a sudden, it burst as if a Musquet had gone off. A Maid that was gone a milking, heard it at a considerable Distance; the Servants said it shook the House. As I had foretold, the Bottom of the Vessel, that was in the Fire, gave way; the Blast of the expanded Water blew all the Coals out of the Fire all over the Room; for the Back of the Fire-range was made just like an Oven, so that circulating therein, it brought forth all the Coals at the Mouth thereof. All the Vessel together flew in a direct Line cross the Room, and hitting the Leaf on a Table made

made of an inch Oak plank, broke it all in Pieces, and rebounded half way of the Room back again. What surprised me in this Event was, that the Noise it made at its bursting was by no means like the successive evaporating of an *Æolipile*, but like the firing off of Gunpowder. Nor could I perceive anywhere in the Room the least Sign of Water, though I looked carefully for it, and, as I said before, I had put a Pint into the Digester, save only that the Fire was quite extinguished, and every Coal belonging to it was black in an Instant.

But to confirm the Elasticity of Water, or to shew, at least, that there is a much stronger elastic Force in Water and Air, when jointly included in a Vessel, than when Air alone is inclosed therein, I made the following Experiment: I took two ζvj Phials, into the one I put about ζv of Water, or better, and so corked it as well as I possibly could; the other I corked in the same Manner, without putting any thing into it. I inclosed them both in my new Digester, Four-fifths being filled with Water; when the Heat was raised to about Five-seconds, I heard a considerable Explosion, and a jingling of Glass within the Vessel, and shortly after another Explosion, but not so loud as the former; whence I concluded, that both the Phials were broken. I then let the Digester cool leisurely, and the next Day I opened it; both the Corks were swimming on the Top of the Water, but only one of the Phials was broken, *viz.* that one into which I had not put any Water. At first, indeed, I concluded, that the Pressure or Dilatation of the Air in the empty Phial being stronger than the ambient

bient Pressure, forced forth the Cork, whereupon the Water, rushing in with Violence, might break the Phial; and therefore that this was the Cause also of the Loudness of the Explosion; whereas the other being mostly filled with Water, there being but a small Quantity of Air therein, just enough to force out the Cork, the Phial was not broken, but was preserved by the Force of the Water inclosed therein. But I have had Reason since to change my Opinion; for having had very strong Phials made, on Purpose to make some peculiar Experiments therewith, I took one of them, and having filled it about a quarter full with Water, and corked it very well, I set it in a square iron Frame, with a Screw to screw down the Cork, and keep it from flying forth. I then put it into a Digester, Four-fifths filled with Water; which being heated to a due Height, when I opened it, I found the Cork forced into the Phial, though the Cork was so very large, that it amazed several who saw it, to conceive how it was possible for so large a Cork to be forced into the Bottle. Hence it manifestly appears, that the Pressure in the Digester, wherein was proportionately more Water, and less Air, was stronger than the Pressure within the Phial, wherein was proportionately more Air, and less Water. Then I reason'd thus also of the two former Phials: That the Air in the Phial, wherein was no Water included, making not a proportionate Resistance to the ambient Pressure in the Digester, wherein was a considerable Quantity of Water, the Cork was forced inward with such Violence, that it, together with the Water, dashed the Phial in pieces; but that in the other Phial, wherein there were Five-sixths of Water, the
inward

inward Pressure in the Phial being greater than the ambient Pressure in the Digester, wherein were but Four-fifths of Water, the Cork was thereby forced outward ; and that the small Difference between the proportionate Quantity of Water and Air in the Phial and in the Digester, being only as Four-fifths to Five-sixths, was the Reason not only why the Bottle was not broken, but also of the Faintness of the Explosion.

III. *Part of a Letter from John Green, M. D. Secretary of the Gentlemens Society at Spalding in Lincolnshire, to C. Mortimer, M. D. Sec. R. S. serving to inclose a Relation of a Girl three Years old, who remained a Quarter of an Hour under Water without drowning.*

S I R,

Spalding, Feb. 18. 1737-8.

THE inclos'd is what I receiv'd this Day from a Gentleman who lives on the Spot, and what you in yours so much desir'd. The Reason of the Child's being able to abide so long under Water is pretty evident: The Child, most likely, was infirm, weak, and sickly, from the Time of her Birth, so that the *Foramen Ovale* was not grown up. I remember about three Years ago to have seen a Subject, an old Woman 80 Years old, who had the *Foramen Ovale* so large, that you might easily thrust your middle Finger through it; but she was attended with the
above-

above-mention'd Circumstance, that is, she never enjoy'd a Moment's Health in her Life.

May 16. 1737. *Rebecca Yates*, of *Billson* near *Market-Bosworth* in *Leicestershire*, had a Daughter about three Years of Age, that fell into the Mill-dam at the Head, near to the Mill-wheel; and, by the Force of the Stream, was drawn under the Water to the said Wheel, with her Legs forwards; one of her Legs went under the Mill-wheel, and by reason of the Nearness of the Wheel to the Floor of its Water-way, the Bulk of the Child's Leg stopped the said Wheel from moving at all. The sudden stopping of the Mill so much surpris'd the Miller, that he went immediately, and let down the Shuttle; but finding it would not go quite down, he came up again into the Mill, and looked both above and below, to see if he could not find out the Cause; then went and drew up the Shuttle, and let it down again; but as the Gate would not shut quite down, he could not as yet find out the Cause of his Mill standing still; for which Reason he went backwards and forwards betwixt the said Shuttle and Mill-room, as nigh as he can guess, Eight or Ten times, before he found out the Cause; but at last he drew the Shuttle quite up, by which means the Force of the Water drove the Child from under the Shuttle; then he put the Shuttle quite down, and thereby discover'd the Child with her Leg under the Wheel, and lying upon her Face. The first Word she spoke was, *Help me*, which she repeated three times; the Miller left her Arm for some other Person to hold her, whilst he endeavour'd to remove the Wheel, so as to get out her Leg; and then she said again, *For God-sake help me out, if*

you can: She spoke very briskly, after she was put to Bed. But the Mill-wheel had tore away all the Shin, Muscles, Sinews, and Tendons, of her Leg, quite to the Bone, and stript them down to her Heel; besides, the Shuttle was drawn up and let down upon the Small of her Back several times. The Child lived from *Monday* till *Friday*, and then died of her Wounds and Bruises; otherwise, in all Appearance, she might have lived to have made a fine Woman. The whole Time of her being under Water (and that at the Depth of four Feet and an half) was near 15 Minutes.

Attested by

John Bailey, *Miller.*

Rebecca Yates; *the Mother.*

*The Mark † of Grace Cooper,
the Miller's Maid.*

IV. *The Case of Mr. . . . Cox, Surgeon at Peterborough, who fell into a Pestilential Fever, upon Tapping a Corpse lately dead of an Hydropsy, drawn up by himself, and read before the Peterborough Society, Sept. 1. 1736. communicated to the ROYAL SOCIETY by the Rev^d Mr. Tim. Neve, Sec. Soc. Petroburg.*

AN elderly Gentlewoman, labouring of a Dropsy about 12 Months, underwent the Operation of Tapping four several times, by which 35 Quarts of

of Liquor were discharged; and dying at last of the same Distemper, I was desired by her Friends to let out the Water that was then contain'd in the *Abdomen*, as well to preserve the Corpse the longer from Putrefaction, as to prevent an Annoyance to the Company at the time of her Funeral.—Yet notwithstanding this was done within a few Hours after Death, the included Humours were arriv'd to such a Pitch of Putrefaction, as to discolour the external Parts with a green and livid Hue.—The Liquor itself was green, and somewhat thicker than new Milk, in Smell more fetid and offensive than what I ever met with, and so sharp and acrimonious in its Nature, as deeply to corrode a silver Canula, through which it passed.—And what shew'd it to be highly malignant, may be judged of from the following Circumstances.

The Night after the Operation, I was somewhat restless and uneasy, and the next Day afflicted with small Tremors, and an unusual Lassitude; in about three Days after, several angry Pustles arose upon my Hands and Fingers, and I believe on every Place where the least Drop of Water fell; some of which coming to Matter, went off soon; those which did not, continued painful, and remained much longer.—The Thumb of my Right Hand, and middle Finger of my other, were affected more severely than any other Part, the Pain more exquisite, the Swelling more hard and large, and of a red dusky Complexion. This was about the 6th Day of my Illness, and altho' the strongest Suppuratives were made use of, yet they fail'd of the desir'd Success, the Pains being continual.—Being persuaded from the great Pulsation

and heavy Pains I underwent, that Matter must lodge either under or upon the *Periosteum*, an Incision was made to the Bone, by which only two or three Drops of Matter were discharged.—'Twas expected this small Discharge might in some measure mitigate my Pain, but it did not; the same Evening, that Pain I at first complain'd of was changed into universal Convulsions, and the Oppression upon my Vitals so great, as to threaten immediate Death.

The Intentions of Cure (which were strictly attended to, by my ingenious Friend Dr. *Charles Balguy*) were to fortify the Heart with Cordials, to enable it to resist and throw out the Malignity, and to bring the Sores to a plentiful Digestion.

The first was treated with the highest Alexipharmacs, the latter, as at first, with strong Suppuratives: This being about the eighth Day of my Illness, and the Convulsions continuing, with an unequal and low Pulse, and as there was little Appearance of Matter, Blisters were plentifully applied, as near to the Parts affected as possible, in order to make a Revulsion from the Heart, and throw off the morbid Matter by the Wounds. In about three Days this Point was gain'd, the Convulsions began to abate, and the Wounds digest; in four more, I found a Cessation of Symptoms, except a Faintness and Lowness of Spirits, which hung upon me for a great while after, which pestilential Fevers (as this was judg'd one in an high Degree) are known always to leave behind them.

I suppose I might receive this Infection as much by Inspiration as Contact; for some of my Assistants, who were in the Room only, and never touched one Drop of the Liquor, found themselves much disorder'd,

der'd, and afterwards broke out with red and livid Eruptions; which sufficiently shew'd, that not only the Liquor itself, but the *Effluvia* too, were in the highest Degree subtle and malignant.

V. *The Variation of the Magnetic Needle, as observed in Three Voyages from London to Maryland, by Walter Hoxton.*

N. B. The Longitude is reckon'd from the *Lizard*.

The First Voyage 1732.

Latitude. North.	Longitude. West.	Variation. West.	
° ' "	° ' "	° ' "	
39 53	27 16	12	} In Sight of the Island <i>Corvo</i> . Difference of Longitude from it 35 Minutes West.
37 49	27 45	14	
35 19	39 20	13	
32 40	50 27	8	
34 40	56	6 30	
35 4	65	4 28	
36 50		4 42	} Distance from Cape Henry se- ven Leagues.
		4 58	
		4 47	} Off the Mouth of <i>Potuxon</i> Ri- ver.

Return.

Return.

Latitude. North.		Longitude. West.		Variation. West.	
o	'	o	'	o	'
36	11	56	20	9	22
34	52	53		6	17
34	33	52		6	15
34	45	51		6	5
34	36	50		6	23
36		49	30	7	37
37	20	48		9	23
38	4	48	20	10	
39	27	47	40	10	23
40	8	45	40	10	38
40	30	45		13	4
42	32	42	20	11	43
42	40	42		12	39
43	27	40	20	13	24
43	32	39	50	13	42
49	48	9		16	30

The Second Voyage 1733.

Latitude. North.		Longitude. West.		Sun's Alti- tude.	Variation. West.	
o	'	o	'		o	'
48	12	3	18	31	18	
46	7	4	30	25	16	35
44	4	7		22	16	22
42	17	9		33	16	36
40		12		37	15	38
38	5	14	20	41	14	51

Latitude. North.	Longitude. West.	Sun's Alti- tude.	Variation. West.
o ' /	o ' /	o ' /	o ' /
37 36	14 45	25	13 24
36 32	15 52	47	13 17
36 16	16 12	19	13
34 2	21 51	35	11 34
34 4	23 18	27	9 51
35 6	30 33	21	10 28
35 12	31 38	20	9 48
34 23	31 22	32	10 23
33 34	32 25	20	8 18
30 19	31 26	23	7 12
29 17	31 11	24	6 45
32 24	37 55	25	6 39
32 50	38 35	25	10 36
32 11	40 23	30	11
31 19	41 9	25	6 42
32 25	43	26	5
34 5	47 20	25	8 49
33 45	49 24	31	10 45
35 1	54 10	30	8 33
34	54 4	41	5 54
33 41	54	22	5 12
33 51	55	23	6 35
34 59	60	36	7 2
36 32	59 30	23	7 49
37 1	61 10	33	6 45
37 5	66 22	44	5 25
36 53	66 40	30	4 45
36 36		30	5

In the Soundings,
20 Leagues off
Cape Henry.

Return.

Return.

Latitude.		Longitude.		Sun's Alti- tude.	Variation.	
North.	'	West.	'		West.	'
38	9	57	40	24	11	
38	48	55	50	25	10	
42	13	43	48	29	14	
44	21	33	17	19	14	26
45	46	28	17	23	15	45
49	51	Off <i>Plymouth.</i>			13	27
50	20	Off <i>Portland.</i>			13	

The Third Voyage 1734.

Latitude.		Longitude.		Sun's Alti- tude.	Variation.	
North.	'	West.	'		West.	'
39	53	6	37	28	14	30
37	50	6	40	21	14	
36	58	10	30	26	15	
34	56	13		30	13	40
33	33	16	10	27	12	10
33	9	17	38	27	9	13
32	44	18	6	24	9	51
31	39	20	13	34	9	49
30	55	22	53	25	9	6
30	17	25	26	36	8	39
30	1	27	14	28	7	56
30	1	27	54	27	6	48
29	55	30	20	24	7	41
29	57	33	12	32	8	
29	51	37	37	32	5	41

Latitude. North.		Longitude. West.		Sun's Alti- tude.	Variation. West.	
o	'	o	'		o	'
28	55	39	28	27	5	23
29	8	40		26	7	12
31	10	44	46	30	8	6
31	7	46	45	22	4	46
30	42	49		38	4	40
30	29	49	48	22	4	
30	31	52	10	25	4	49
30	18	53		25	4	45
30	23	55		25	4	22
30	58	57	30	22	4	52
37	9	68		38	4	50

VI. *Some Thoughts and Conjectures concerning the Cause of Elasticity*, by J. T. Desaguliers, LL. D. F. R. S.

A Ttraction and Repulsion seem to be settled by the Great CREATOR as first Principles in Nature; that is, as the first of second Causes; so that we are not solicitous about their Causes, and think it enough to deduce other Things from them. If Elasticity was admitted as a first Cause, (as it is by some) it is thought we should admit of too many principal Causes in Nature, which is contrary to the Rules of good Philosophy. Philosophers therefore have endeavour'd to deduce Elasticity from Attraction, or from Repulsion, or from both. It is observ'd, that the same Particles that repel each other strongly,

Z

will

will attract other Particles very strongly; as appears by many Chemical Dissolutions, especially by the alternate Dissolution and Precipitation of Metals in acid *Menstruums*. The Reverend and Learned Dr. *Hales* has proved this many Ways, in his *Vegetable Statics* and *Hæmastatics*. The Elasticity of Air seems to consist wholly in the repulsive Power of its Particles, which do not touch one another while the Air is in its elastic State; and if those Particles be brought nearer and nearer together, the Effect of their repulsive Force will increase, the Air's Elasticity being always proportionable to its Density by Compression, which Property will be preserv'd, though compress'd Air be kept a Year or two; notwithstanding Mr. *Hauksbee* in his *Physico-mechanical Experiments* says, that Air will lose part of its Spring by being very much compress'd. But the Air with which he try'd it, must have been fill'd with moist Vapours; and it is well known, that the Steam of Liquors will lose its Elasticity, especially where its Heat decays. I have kept several Wind-Guns, strongly charg'd, for half a Year together, in which the Air had lost none of its Elasticity: Others have found the Air as strong after a Year; and I have been told by a Person of Credit, that a Wind-Gun having been laid by and forgotten for seven Years, when it was found, discharg'd its Air as many times, and with as much Force, as it used to do. Now, though Air, compress'd by any external Force, does always increase in Elasticity, as it diminishes in Bulk; yet it may, by Fermentation, diminish its Bulk very much, without gaining any more Elasticity: For if another Fluid, whose Parts repel one another, but attract the Parts of Air, be mix'd

mix'd with Air, the Repulsion of any two Particles of Air will be diminish'd, in proportion as a Particle of the other Fluid, insinuating itself between them, attracts them towards itself on either Side. The same thing will happen to the other Fluid, in respect of the Particles of Air, which mixing with its Particles, do in the same manner destroy their Repulsion. Thus, if we allow an Attraction strong enough between the Parts of two elastic Fluids, it is possible, that by Fermentation a Solid may be made out of two elastic Fluids, which would have still continued fluid without such a Mixture. We are taught by Chemistry, to mix Fluids together, which immediately coalesce into a Solid. When Brimstone Matches are burning, the *Effluvia* of the Sulphur repel each other to great Distances, as may be known by the sulphureous Smell upon such an Occasion. Now, though these Particles repel each other, they attract the Air very strongly, as appears by the following Experiment:

Take a tall Glass Receiver clos'd at Top, holding about four Quarts of Air, and having put its open End over a Bundle of Brimstone Matches on Fire, standing up in the Middle of a large Dish with Water in it, (to keep the Air from coming in at the Bottom of the said Receiver) you will observe, that not only as soon as the Matches are burnt out, but a good while before, the Air, instead of being expanded by the Flame of the Brimstone, will retire into less Compass; the Water beginning to rise from the Dish up into the Receiver, and continuing so to do till some time after the Matches are burnt out; so that there will be in the Receiver only three Quarts of Air, instead of four (more or less, in proportion

portion to the Quantity of Brimstone burnt): And this plainly happens by some of the *Effluvia*, or little Parts of the Sulphur, attracting some of the Particles of the Air, so as to make an unelastic Compound, that precipitates into the Water. If the Elasticity of the Air is quite lost when the Repulsion of its Particles is taken off, or sufficiently counteracted, it must follow, that its Elasticity depends upon Repulsion: And that this is often the Case, appears by a great Number of Dr. *Hales's* Experiments, of which I will mention but a few. The Doctor took a Cubic Inch of Mutton-Bone, and having put it into his Gun-Barrel Retort, he distill'd out of it two or three hundred Cubic Inches of Air into a large glass Bottle, the Weight of which Air, together with the Ashes of the Bone left, weighed as much as the whole Quantity of Bone did at first. Now the Air had been confin'd in that Bone, together with many sulphureous Particles, in such manner, that the mutual Attraction of the Sulphur and the Air had alternately destroy'd each other's repulsive Force, and brought those Substances into a little Compass; but the Fire in the Distillation separated them from each other, so as to restore them, in a great measure, to their usual Elasticity. This appear'd by bringing a Candle near the Mouth of the Bottle that held this reviv'd Air; for every time the Candle was brought near, the Air took Fire, and flash'd out of the Bottle with a sulphureous Smell. The Air may be consolidated in many hard Bodies, so as to be there quite void of Elasticity, and there do the Office of a Cement, till by the Action of Fire, or some particular Fermentations, it is again restor'd to its perfectly elastic State.

This

This is the Meaning of the Doctor's Words, when he says, that some Bodies *absorb*, and others *generate* Air; and the same Bodies do sometimes *absorb*, and at other times generate Air. He found more or less Air in almost every solid Substance that he try'd; but, what was most remarkable, he found that the *Calculus humanus* (or Stone taken out of a Man's Bladder) was made up of above half its Weight of Air.

Some have endeavour'd to solve Elasticity by Attraction only; as for Example: If the String *AB* (TAB. I. Fig. 1.) be consider'd as made up of Particles lying over one another in the manner represented at *ADB*; it is plain, that if the Point *D* be forcibly brought to *C*, the Parts will be pull'd from each other; and when the Force, that stretch'd the String, ceases to act, the Attraction of Cohesion (which was hinder'd before) will take place, and bring back the String to its former Length and Situation after several Vibrations. Now, though this seems to agree pretty well with the *Phænomena* of a String in Motion, it will by no means solve the Elasticity of a Spring fasten'd at one End, and bent either way at the other, like a Knife or Sword-blade, as in Fig. 2. For if such a Spring be bent from *A* to *a*, the Particles on the Side *C*, which now becomes convex, will be farther asunder at *F*; while the Particles at *D*, carried to the concave Part *E*, will come closer together: So that the Attraction, instead of making the Spring restore itself, will keep it in the Situation in which it is, as it happens in Bodies that have no Elasticity, where perhaps only Attraction obtains. Thus a Plate of Lead, a Plate of Copper, and a Plate of soft Iron, stands bent.

But

But the most probable way of solving the Elasticity of Springs, is to consider both a repulsive and an attractive Property in the Particles, after the manner of the black Sand, which is attracted by the Loadstone, and has been shewn by the learned and ingenious Professor *Petrus van Muschenbrook*, to be nothing else but a great Number of little Loadstones.

Fig. 3. Let us suppose a Row of round Particles touching one another only in the Points c in a Line from A to B . It is plain, from what Philosophers have shewn, concerning the Attraction of Cohesion, that upon the least Shake, or Alteration of the Position of a strait Line, these Particles will run together, and form a Sphere, in which the Globules will have more Points of Contact. But if these Particles have Poles like Magnets in the opposite Places mark'd $n; s$, so that all the Poles n, n, n , &c. repel one another; and all the Poles s, s, s , &c. do likewise repel one another, the Line AB will continue strait; for if by any Force the said Line BA be put into another Position, as into the Curve ba , then the Poles nn , &c. being brought nearer together, (while the Poles s, s , &c. are farther asunder) will repel one another more strongly, and thereby hinder the Globules from running together towards the concave Part; and the Spring, left to itself, (all this while supposing one End, as b, B , or β , fix'd) will restore itself, throwing its End a back to A , and so on to α , by the first Law: Then being in the Position $\alpha \beta$, the Poles s, s , &c. are brought nearer together, whose Repulsion, thus increas'd, throws back α to A , and so on forward, the Line of Particles performing several Vibrations round B .

May

May not a Spring of Steel, or other Springs, consist of several Series of such Particles, whose Polarity and Attraction acting at the same time, will shew why such Bodies, when they have been bent, vibrate, and restore themselves?

If we take a Plate of Steel, and make it so hot till it looks white, and then immediately quench it, we thereby fix the Metal in a State very near Fluidity, so that the Particles which the Fire had almost brought to Roundness, have but a very small Contact; as appears by the Fragility of the Steel thus harden'd, which breaks like Glass, and has a short Grain. Steel, thus harden'd, is highly elastic; for what Workmen call *hard*, is the most *elastic*: as appears by the Congress of high-harden'd Steel Balls, which return, in their Rebound, the nearest to the Place we let them fall from; and, next to Glass, have the quickest Elasticity of any thing we know.

That we may not be thought to have given an imperfect Account of the Elasticity of a Steel Spring, because such an one as we have describ'd wants Toughness, and will immediately fly when bent to any Degree; we must beg Leave to consider farther the Properties of the round Particles, or little Spheres, of Steel, in which we have suppos'd a Polarity.

Let us suppose *AB* (Fig. 4.) to be two little Spheres or component Particles of Steel, in which, at first, we will suppose no Polarity, but only an Attraction of Cohesion. Then, whether the Particles have their Contact at *c, d, e, n*, or at *s, t, s*, their Cohesion will be the same; and the least Force imaginable will change their Contact from one of those Points to another; because in the rolling of these little Spheres, they

they do not come into more or less Contact in one Situation than another. But if we suppose the Point n in each Spherule to be a Pole with a Force to repel all the other Points n in any other Spherule, and likewise s another Pole, repelling the other Points s ; the Spherules will cohere best, and be at Rest in that Position where the Points c, c , are in Contact, and n and s at equal Distances on either Side. For if the Spherules be turn'd a little, so as to bring the Points d, d , into Contact, as in Fig. 5. the Poles n, n , being brought nearer, act against each other with more Force than the Points s, s , which are now farther off, and consequently drive back the Spherules to the Contact at c, c , beyond which continuing their Motion, they will go to d, d , Fig. 6. and so backwards and forwards, till at last they rest at c, c , which we may call *the Point of Equilibrium for Rest* in a Spring. Now there are, besides this, two other *Points of Equilibrium*, beyond which the Spring may break, which are the Points e, e towards n , and ϵ, ϵ towards s ; see Fig. 7. that is, when the Spherules have their Poles n, n brought very near together, the mutual Repulsion increases so, that the Attraction at the Contact is not able to hold them, and then they must fly asunder, the Spring breaking. We suppose the Points e, e , to be the Points of Contact, beyond which this must happen; but that if the Contact be ever so little short of it, as between e and d , the Spherules will return to their Contact at c , after some Vibrations beyond it, as has been already said. This is the Reason why I call e , (in one of the Spherules) and its correspondent Point ϵ on the other Side c , *the Points of Equilibrium*; for if the Spring be bent

bent towards a (Fig. 3.) so that the Spherules, like A and B , (Fig. 7.) touch beyond e , the Spring will break: Likewise if the Spring be bent the other way, till the Spherules touch beyond ε , then it will break the other way. Now when the Spherules touch at e, e , or at ε, ε , the Spring is as likely to return to its first Position as to break; for which Reason I have call'd the Points e and ε , *Points of Equilibrium*, as also having known by Experience, that a Spring left bent to a certain Degree, has, after some time, broke of itself.

From all this it appears, that Spherical Particles will never make a tough Spring; therefore the Figure of the Particles must be alter'd, in order to render it useful; and this is what is done in bringing down the Temper of the hard Steel, and *letting down a Spring*, as it is call'd. What Change ought to be made in the Particles, we shall first shew; and then consider how far that is done by those who make Springs.

If the Parts suppos'd Globules, as in Fig. 3. are now flatten'd at c , where the Contact is, so as to put on the Shape $n e d c s \varepsilon s$, (as in Fig. 8.) the Contact will be much increas'd, and reach from d to s , so that in bending the Spring there will still remain a great Contact in the Particles, and the Points of *Equilibrium* for breaking (*viz.* e, e above, and ε, ε below) will be remov'd nearer to the Poles n , or s , than when the Particles are round; the Consequence of which will be, that the Spring must be bent much farther, to be in Danger of breaking, than in the former Supposition; as may be seen in Fig. 9. where two Particles being open'd about the Point d as a
A a
Centre,

Centre, the attracting Points c, c , and d, d , have still some Force to help to bring back the Particles to their whole Contact; because in this Shape of the Particle the attracting Points c, c , d, d are remov'd but in Proportion to their Distance from the angular Point d ; whereas if the Particles had been spherical, and the Line d, d an Arc of a Circle, the attracting Points c, c , and d, d , would have remov'd from one another farther than in Proportion to twice the Square of the Distance from d , (as in Fig. 5.) and so have afforded very little Help for bringing back the Particles to their Contact. A Row of Particles in the Spring thus condition'd, is to be seen in the natural State at BA , Fig. 10. and bent at ba in the same Figure. Here it is to be observ'd, that if in this Figure of the Particles you would bend the Spring to bring the Particles to touch at their Point of *breaking Equilibrium*, you must open them so much on the contrary Side, that the Spring will be bent far beyond any Uses intended to be made of it, as appears by Fig. 11. where two Particles are brought to touch at the equilibrating Point e ; and by Fig. 12. where many Particles being put into that Condition, the Spring is brought round quite into a Circle.

Now the common Practice in making Springs is the most likely to produce this Effect requir'd in the Particles; for the hard Spring, whose Particles were round, or nearly so, is heated anew, and whilst it is cooling gently, the mutual Attraction increases the Contact, so that the Particles grow flatter in those Places where before they had but a small Contact; and lest this Contact should become too great, the Spring's Softening is stopp'd by quenching it in Water,
or

or Oil, or Grease. Another way of making Springs, is to begin and shape them in cold unelastic Steel, and then having heated them to a small Degree, for Example, to a Blood-red Heat, immediately to cool them in some proper Liquors. This also settles the Particles in their oblong Figure, through which they must pass before they become round, or nearly so, in a white Heat. That Particles of Steel are fix'd in the Figures which they have at the Instant of dipping, will not appear strange, when we consider, that dipping red-hot Steel in cold Liquors, in a particular Position, makes it magnetical. If it be ask'd, How we account for making Springs only with hammering, it is easily answer'd, That we can make Iron and Steel magnetical only with hammering; and if we can give and destroy Poles in the whole Piece, there is no Improbability to think we can give Poles to little Parts; or rather bring into a particular Situation the Poles which they have; for if the Poles that we have consider'd be plac'd quite irregularly, there will be no Elasticity at all. Agreeable to this, Springs may be made of other Metals than Iron or Steel, though not so perfect, by Hammering; for it will be sufficient for the little Particles to have Poles that attract and repel one another, driven by the Hammering into a regular Order.

N. B. This, apply'd to the Vibration of a String, will better solve its several Cases than Attraction alone; and the Elasticity of Glass is just the same as that of a very brittle Steel-Spring.

VII. *Some Thoughts and Experiments concerning Electricity*, by J. T. Desaguliers, LL. D.
F. R. S.

THE *Phænomena* of Electricity are so odd, that though we have a great many Experiments upon that Subject; we have not yet been able from their Comparison to settle such a Theory as to lead us to the Cause of that Property of Bodies, or even to judge of all its Effects, or find out what useful Influence Electricity has in Nature: Though certainly, from what we have seen of it, we may conjecture, that it must be of great Use, because it is so extensive.

Though some Persons have been too hasty in their Conjectures, and too apt to run into Hypotheses not sufficiently supported by Experiments; yet it would be of great Use to settle some general Propositions concerning Electricity from the Light we have already, and what we may further discover by future Experiments; provided we have a sufficient Number of them to settle a general Rule. For Example; I now propose some general Assertions to be consider'd, and to be rejected or allowed of as a Number of Experiments shall determine; but to stand only as *Queries* till they are settled.

I have hitherto avoided entertaining the SOCIETY upon this Subject, or pursuing it so far as I might have done, (considering that I can excite as strong an Electricity in Glass, by rubbing it with my Hand, as any body can) because I was unwilling to interfere with

with the late Mr. *Stephen Gray*, who had wholly turn'd his Thoughts that way; but was of a Temper to give it intirely over, if he imagin'd, that any thing was done in Opposition to him. But now I intend not only to go on myself in making electrical Experiments, but shall always be ready to make such as shall be propos'd by any Member of the SOCIETY. The *Queries* which I have already examin'd, are the following:

Query I. Whether all Bodies in general are not capable of receiving the Electricity which has been given to a Tube by Friction, though there be a great many Bodies, such as Metals and Vegetables, &c. in which we have not hitherto been able to excite any Electricity by Heat, or Friction, or any other Operation on the Bodies themselves?

Query II. Whether when a String is stretch'd out at Length, with a Body hanging at one End of it, to which Body we would communicate the Electricity of the Tube rubb'd at the other End, the Supporters of the String ought not to be of such Bodies as are capable of having Electricity excited in them by Friction, Heating, Beating, or Patting, or some immediate Operation on the Bodies themselves?

Query III. Whether these Supporters of the String (mention'd in the last *Query*) which stops the electrical Virtue from passing any farther, are not of such a kind as are incapable of having the electrical Virtue excited in them immediately by any Operation yet known; though they are all capable of receiving it from a rubb'd Tube, even at a great Distance, by the Communication of a String made of vegetable Substances?

Query

Query IV. Whether the Reason that some Supporters transmit the Electricity running from the rubb'd Tube along the String to Bodies beyond them, be not as follows, *viz.* That having receiv'd some of the electrical Stream, they soon become saturated with it, and so receiving no more of it, let the rest pass on without disturbing it?

Query V. Whether the Reason, that Supporters made of vegetable Substances, Metals, and such others, as stop the Electricity above-mention'd from running any farther along the String than the Place where it rests upon them, be not this? *viz.* That they are never saturated with the electrical Stream, but continually receive it, and transmit it to the next contiguous Body, provided that contiguous Body be of the same kind with themselves, and also contiguous to other Bodies of the same sort: I mean such as would stop the Electricity, if the String was supported by them. For even these Supporters will transmit the Electricity, if terminated at each End by Bodies that transmit the Electricity, when they support the String.

Query VI. Whether we may not distinguish all Bodies in general, in respect of Electricity, into such as may be excited to Electricity, and such as cannot be excited to Electricity? the two kind of Bodies receiving the Electricity from other Bodies into which it has been excited differently; the first also transmitting the Electricity, while the others do not.

These *Queries* are such as arise from a Consideration of Experiments made by others, and such as I have made myself.

As I go on in making other Experiments, other *Queries* may arise, and I shall communicate them.

Here follow the Experiments I have already made, and am ready to repeat as the SOCIETY may desire.

EXPERIMENTS relating to the First Query.

I stretch'd a Cat-gut about five Feet in Length, and fasten'd it to the Top of two Chairs in an horizontal Situation, and such another Cat-gut String to two other Chairs parallel to the first, and at the Distance of 15 or 20 Feet from the former. Then I suspended one End of a Packthread to the Middle of the first Cat-gut, and carried it on so as to lay it over the Middle of the other Cat-gut, and leave the other End of the Packthread hanging down about a Foot below the Cat-gut, with a Loop to hang several Bodies to it, successively to receive the Electricity excited by the Tube, and applied to the other End of the Packthread.

All the Bodies I tried received the Electricity communicated from the rubb'd Tube along the String, which appear'd by holding a Thread fasten'd to a Stick, the Thread being attracted towards the suspended Body.

1. A Gold Medal.
2. A Silver Medal.
3. A Copper Medal.
4. A Brass Ball.
5. A Steel Ball.
6. A Tin Ball.
7. A Leaden Ball.
8. Sulphur.
9. Sealing-Wax.
10. Pumice-Stone.
11. Bees-Wax.
12. Resin.
13. *Sal Armoniac*.
14. Ivory.
15. Human Bone.
16. Fish-Skin.
17. Loadstone.
18. Flesh.
19. Cotton.
20. Wax-Candle.
21. Tallow-Candle.
22. A Leak.
23. Celeri.
24. Tobacco.

Tobacco-Pipe. 25. A Glass-Ball. 26. A Rush rolled up.

EXPERIMENTS relating to *Query II.*

Retaining the first supporting String of Cat-gut, instead of the last Cat-gut Supporter, I made the Packthread pass over the following Substances successively, all which transmitted the Electricity to the Body suspended at the End of the Packthread; *viz.*

1. A Silk String.
2. Hair Rope.
3. Parchment.
4. A Thong of Sheep-skin, but it stopp'd the Electricity till it was dry and warm.
5. A List of Woollen Cloth.
6. A List of Flanel.
7. Cadis, or a kind of Worsted Tape.
8. Quills.
9. Whalebone.
10. A Man's Thigh-Bone.
11. A Bladder.
12. A Cat, held between two.
13. A Tallow-Candle.
14. A Wax-Candle (the String was also laid over the unburn'd Cotton Wick at the End of the Candle).
15. A Tallow-Candle and its Wick.
16. Tobacco-Pipe, with a Cat-gut or a Packthread through it, or without, that is, a Packthread String being fasten'd at each End of it.
17. A Sword-Belt.
18. A Piece of a white Hat.
19. A Piece of a black Hat.
20. A Glass Tube.
21. The same with Water in it.
22. With Spirit of Wine.
23. The same with Mercury in it.
24. Sealing-Wax.
25. Crape.

N. B. All these Substances, except the Sheep-skin, the Tobacco-Pipe, the Quills, the Candles, and the Bone, not only transmitted the Electricity, but became so far electrical, as to attract the Thread a little way on each Side of the supported Packthread.

There are more Experiments requir'd to be made, before this *Query* can be turn'd into an Assertion.

EXPERIMENTS *relating to Query III.*

Instead of the last Supporter of Cat-gut near the suspended Body, I made use of the following Substances stretch'd from Chair to Chair; and then the Thread hanging on the Stick was not at all attracted by the suspended Ivory Ball, which I made use of in all the Experiments to try the Supporters.

1. A Hempen Rope. 2. A small Packthread. 3. A drawn Sword. 4. A Sword in the Scabbard. 5. The Scabbard without the Sword. 6. A twisted Cotton Thread. 7. Tape made of Thread. 8. Bars, Tubes and Wires of Copper, Brass, Iron and Lead. 9. White Paper and brown. 10. A moist Thong of Sheep-Skin. 11. Celeri. 12. Leeks. 13. Fir-wood. 14. A Cane. 15. A Piece of black Thorn. 16. The same Rushes that had before receiv'd the Electricity when suspended. 17. A Sponge dry. 18. White Thread. 19. Hay. 20. A Marble Slab.

N. B. Such Bodies as were too short to reach from Chair to Chair, were lengthen'd out by Pieces of Packthread at each End.

EXPERIMENTS *relating to Query IV.*

The Cat-gut Supporters, and all the others mention'd in the Experiments to *Query III.* which transmitted the Electricity, attracted the Thread of the Stick near the conducting Packthread, but not so far as the Chairs to which the said Supporters were fasten'd.

EXPERIMENTS *relating to Query V.*

All the Supporters which did not transmit the Electricity, when they reach'd from Chair to Chair, were made to transmit, when they were lengthen'd out with Cat-gut at each End, and then they became electrical themselves from one End to the other, as becoming part of the suspended Body; and becoming so saturated, as not to be able to carry the Electricity on either Side any farther than the Cat-gut to which they were fasten'd.

EXPERIMENTS *relating to Query VI.*

The late Mr. *Stephen Gray* has, by rubbing, excited Electricity in several of those Bodies which I have made Supporters of to transmit the Electricity (See *Philosoph. Transf.* N^o 366.): I have done the same with several others, but not with all of them, though I shall try them all: But as it is more difficult to excite that Virtue in some than others; and all the Experiments in general succeed better in dry and cold Weather than in moist and warm, I must wait for proper Opportunities to make the Experiments, and then I shall communicate them.

EXPERIMENTS *concerning mix'd Substances.*

1. Cadis (or Woollen Tape) laid on Thread-Tape, when made a Supporter, transmitted the Electricity.
2. When the Thread-Tape was uppermost, the Electricity was stopp'd.
3. When they were twisted together, the Electricity was transmitted, but most weakly when the Pack-thread

thread going to the Ball was laid over that Part of the Twist which had the Thread-Tape.

N. B. The two Paper Supporters which did not transmit the Electricity, ought to have done it according to *Query II.* because, by Mr. *Gray's* Experiments, Electricity is to be excited in the Paper by rubbing: Therefore, perhaps, the Papers wanted to be drier or warmer, so that I shall try them again. These are the only two Experiments that do not agree with the second *Query*; but I would not omit mentioning them, because it is the Part of an impartial Philosopher to mention as well those things which favour, as those that disagree with his Hypotheses and Conjectures.

VIII. EXPERIMENTS *made before the*
ROYAL SOCIETY, Feb. 2. 1737-8. by
J. T. Desaguliers, LL. D. F. R. S.

N. B. **I**N the following Account, which is the Sequel of former Experiments, I call *Conductors* those Strings, to one End of which the rubb'd Tube is applied; and *Supporters* such horizontal Bodies as the *Conductor* rests upon.

EXPERIMENT I.

Old Packthread Supporters transmitted Electricity but weakly, though more strongly when twisted with Cat-gut; but new Packthread did better.

N. B. *Where it is not mention'd otherwise, an Ivory Ball hangs at the End of the Conductor;*

and its Electricity is tried by a Thread applied near it.

EXPERIMENT II.

A Conducting String of Cat-gut receiv'd the Electricity a little way; but did not carry it quite to the Tube.

EXPERIMENT III.

Two conducting Strings, one of Cat-gut, and one of Packthread, compar'd, the first attracted less and less, as the Distance from the Tube increas'd; and the other more and more, till it was strongest at the suspended Body: But both ceas'd immediately after the Removal of the Tube.

EXPERIMENT IV.

A Sealing-Wax Supporter transmitted the Electricity, but did receive little or none when suspended. If it was but just rubb'd with the Hand, it attracted the Thread when first suspended; and strongly, if much rubb'd; but that Virtue was soon lost, if the Tube was apply'd to the conducting String, and then it would receive no more Electricity from the Tube. If the Stick of Wax was wet, then it would strongly receive the Electricity.

A Wax Supporter wet, and Silk String wet, did not transmit the Electricity.

EXPERIMENT V.

Dried Ox-Guts did not transmit Electricity when held in Hand; but when tied to Cat-gut, transmitted it; and, when suspended, received it plentifully.

EX-

EXPERIMENT VI.

The same with a small Cord.

EXPERIMENT VII.

The same with a Rod of Iron, and Tube of Brass.

EXPERIMENT VIII.

A Glass Tube, made Conductor, receiv'd the Electricity but a little way.

EXPERIMENT IX.

Dry Sheep-Skin transmitted the Electricity, but not when wet, though it receiv'd it then when suspended.

EXPERIMENT X.

A middle Supporter of Packthread was again supported on one Side by a Glass Tube, and on the other by Sealing-Wax, and had at each End an Ivory Ball hanging. Those Balls became electrical in the same manner, and at the same time, as the Ball at the End of the conducting Spring.

EXPERIMENT XI.

When a Bar of Oak was made use of instead of the Tube, or a small Iron Bar instead of the Wax, the Electricity was stopp'd: But if the Bar was thrust a little way into a Glass Tube, the Electricity was communicated as before.

EXPERIMENTS *made at the* ROYAL
SOCIETY, Feb. 9. 1737-8.

I fixed six Iron *Radii* of twisted Iron Wire to a Brass Ring of two Foot and an half Diameter, and half an Inch wide, which had a Socket in the Centre, whereby to set it either on an upright Glass Tube, or on a wooden Pillar: Then I hung upon the End of the six *Radii*, next to the Circumference, the following Substances. 1. A Piece of Resin. 2. A Stick of Wax. 3. An Apple. 4. An Ivory Ball. 5. A Steel Ball. 6. A Glass Ball.

EXPERIMENTS I. and II.

I rubb'd the Tube, and applied it to the Centre of this Machine, as it stood on a Glass Tube; and the Electricity was communicated to all the suspended Bodies, and the Ring also; but none of them receiv'd it, when the Machine stood upon a wooden Pillar, whose Foot was on the Floor.

EXPERIMENT III.

I tied to the Ends of the six *Radii* as many Cat-gut Strings, but so long as to unite together about a Foot higher than the Centre of the Ring, where I suspended them by another Cat-gut String three Foot long, the Top of which was fasten'd to an hempen Rope. Then applying the rubb'd Tube very near the Place where all the Cat-gut Strings join'd over the Ring, (at which Ring the same Bodies were suspended

as

as before) neither the Bodies nor Ring receiv'd any Electricity.

N. B. This was done in foul Weather, when the Electricity does not extend itself far from the Tube: But in fair Weather, the Electrical Virtue, at the same Distance, reach'd the Iron *Radii* of the Ring; and consequently the Ring and Bodies suspended, though the Virtue was not propagated along the Cat-gut: For if the Tube was applied a little higher to the single Cat-gut, so as the *Effluvia*, or Vittue darted directly from the Tube, did not reach the Ring, or its Iron *Radii*, then no Virtue was communicated to the Ring, or the suspended Bodies, &c.

EXPERIMENT IV.

I suspended the Ring by six Packthreads, just in the same manner as the Cat-gut Strings before; but still all those Strings were suspended by the perpendicular Cat-gut of three Foot in Length. Then all the Bodies receiv'd the Electricity from the rubb'd Tube applied to the Top of the Pyramid of Packthreads.

EXPERIMENT V.

Instead of the perpendicular Cat-gut between the Pyramid of Packthread and the upper hempen String, I substituted a Packthread; and then no Virtue was communicated to the Ring, but all went up the hempen String, and was lost; except the Tube was held very near the Ring, and then it gave a small Degree of electrical Attraction to the Ring, and the Bodies suspended at it.

E X P E R I M E N T VI.

Having again suspended the Ring with the Bodies and Pyramid of Packthreads to the perpendicular Cat-gut, I tied a Packthread to the Ring, and carried it horizontally about 20 Feet from the Ring; and having fasten'd a Cat-gut String three Foot long to it, I gave it an Assistant to hold: Then applying the rubb'd Tube to the End joining that Cat-gut, the Electricity was communicated to the Ring, and all the suspended Bodies, as appear'd by applying the white Thread near them, which was attracted by every Part of the Ring, and all the Bodies.

E X P E R I M E N T S *made before the* ROYAL
SOCIETY, Feb. 16. 1737-8.

E X P E R I M E N T I.

I applied the rubb'd Tube to a burning Candle, and it had no manner of Effect on the Flame; but as soon as the Candle was blown out, it attracted the Smoke at four or five Inches Distance.

E X P E R I M E N T II.

An horizontal Packthread, of about 18 Feet in Length, being terminated by the Cat-gut Strings, of three Foot long each, I hung (towards one of the Ends of the Packthread) upon it a Candlestick with a lighted Candle in it; then applying the rubb'd Tube to the other End of the Packthread, the Candlestick attracted the Thread, and it was also attracted by the
Candle,

Candle, but not within two or three Inches of the Flame; but as soon as the Candle was blown out, the Thread was attracted by every Part of it; nay, even the Wick, when it was quite extinguish'd.

EXPERIMENT III.

I suspended a Wax Candle in the same manner, and the Experiment succeeded in the same manner; only the Electricity came not so near the Flame in the Wax as in the Tallow Candle.

EXPERIMENT IV.

I hung an Iron Wire 16 Foot long horizontally by two Cat-gut Strings at its Ends about three Foot long each, and bent down the Wire from the Place join'd to the Cat-gut, so as to hang down a Foot at one End; then applying the rubb'd Tube at the other End; this Conductor carried the Electricity along to the Ball; but not so well as the Packthread Conductor; but it did something better when it was wet.

The same happen'd when the Conductor was Brass Wire of the same Length.

N. B. The Packthread Conductor also carried the *Effluvia* stronger when it was wet.

IX. *An Account of some Electrical Experiments made before the ROYAL SOCIETY on Thursday the 16th of February 1737-8. By the Same.*

EXPERIMENT I.

TAB. I. Fig. I.

I Took the Glass Tube *AB* of two Inches Diameter, which had at one End *A*, a Brass Ferril with a Brim cemented to it, and at the other End *B*, a Brass Cap close at Top, the Brass-work being join'd to it, in order to exhaust it of its Air upon Occasion. When this Tube was very dry, it would become electrical by rubbing, so as to snap by passing the Ends of the Fingers near it; but that Virtue could not be excited in the Tube nearer the Brass at the Ends than from *a* to *b*, and not unless the Tube was very dry within.

The Tube being thus prepar'd, and having an Ivory Ball *C*, of about two Inches Diameter, tied to it at the End *B* by a short String, I passed the Tube through the horizontally suspended Plate *DD*, till it was stopp'd by the Brim at *A*; and as it hung perpendicularly, the Ball *C* was within a Foot and an half of the Floor. The Plate *DD* was about 10 Inches in Diameter, and suspended by three small Cat-gut Strings as *E, e*, of about two Feet in Length, all which were tied together at *E*, to an hempen String hanging from the Cieling at *F*.

By

By reason of the Distance of the Ends of the Cat-gut Strings close to the Plate at *eee*, I was able to thrust in between them one End of an open Tube *GG*, after I had rubb'd it so as to make it electrical, to see whether I could make the aforesaid suspended Tube *AB* the Conductor of Electricity to the Ball *C*; but the first Trial was in vain.

EXPERIMENT II.

Then laying horizontally over the Plate *DD* an Iron Bar a quarter of an Inch thick, and a Yard long, I hung at the Ends of it two Ivory Balls *cc*, of the same Size as *C*, by Packthreads of the same Length as the Tube *AB*.

Having again made the Tube *GG* electrical, I applied it over *A*, as before, and immediately the two Balls *cc* received the Electricity, so as to attract the Thread of Trial *T* hanging at the End of the Stick *ST*, when applied near them; though it received no Motion when applied to *C*. But if the Strings *Hc*, instead of Packthread, were Cat-gut, then the Balls *cc* received no Electricity from the Tube *GG* rubb'd and applied over *A*.

N.B. To be sure that the rubb'd Tube is made electrical, I pass my Fingers near it after rubbing, to hear whether it snaps; but always rub again before I apply it; because by snapping it loses its Electricity at the Place where it snaps.

EXPERIMENT III.

When I rubb'd the Tube *AB*, it would then attract the Thread of Trial *T* between *a* and *b*; but not at

all above *a* or below *b*, unless when I applied the Tube *GG* above *A*: Then the Thread of Trial would be attracted by the Plate *DD*, and the Top of the great Tube from *A* to *a*, but no lower. It would also be attracted by all the Bar *HH*, and only three or four Inches below *H*.

EXPERIMENT IV.

Having fill'd the Tube *AB* with Water, the Electricity of the rubb'd Tube *GG*, applied at *A*, ran strongly down the Tube *AB*, and impregnated the Ball *C*, so as to make it strongly attract the Thread of Trial, whilst the Balls *cc* received no Virtue at all. But upon wetting the Cat-gut Strings *Hc* with a Sponge, all the three Balls *cC* and *c* strongly receiv'd the electrical Virtue.

EXPERIMENT V.

I took away the Bar *HH*, and its Balls and Strings; and having well dried the Tube, I rubb'd it, and hung it up as before; so that it would snap, or attract the Thread from *a* to *b*, but no-where else.

Then putting the small Bar *HH* into the Middle of the Tube *AB* in its Axis represented by the prick'd Line, upon Application of the rubb'd Tube *GG* at *A*, the Virtue was immediately communicated to the Ball *C*. The same thing happen'd, when, instead of the Bar, a Brass Wire, a Walking-Cane, a small green Stick, or small Packthread, was placed in the Axis of the Tube.

EXPERIMENT VI.

I took a Barometer Tube empty, and very dry, and placed it in the Axis of the great Tube *AB*, but it would conduct no Electricity to the Ball *C*; though it carried it down very readily when full of Water, though quite dry on the Outside.

Another small Tube open at both Ends, which conducted no Virtue to *C* when dry, being only moisten'd a little by the Breath in blowing through it, carried down the Virtue from *A* to *C* very strongly.

N. B. All this while the Cat-gut Strings *Ee* received no electrical Virtue.

As I design to pursue these Inquiries much further, I beg Leave to be allow'd to make use of some Terms, (which I shall here define) in order to save using many Words in giving an Account of some electrical Experiments, which I have made, and shall hereafter make.

DEFINITION I.

A Body *electrical per se* is such a Body in which one may excite Electricity by Rubbing, Patting, Hammering, Melting, Warming, or any other Action on the Body itself, as Amber, Sealing-Wax, Glass, Resin, Sulphur, &c. besides many, if not all, Animal Substances.

DEFINITION II.

A *Non-electrical* is such a Body as cannot be made electrical by any Action upon the Body itself
im-

immediately; though it is capable of receiving that Virtue from an *Electrical per se*.

O B S E R V A T I O N S.

1. When the Air is full of moist Vapours, *Electricals per se* are excited to Electricity with very great Difficulty, requiring to be often warm'd, and much rubb'd; as appears in exciting that Virtue in Glass, Amber, Wax, &c.

2. In dry Weather, especially in frosty Weather, the *Electricals per se* will have their Virtue excited with very little Action upon them; as appears by warming a Glass Receiver, which, without any rubbing, will cause the Threads of a Down Feather, tied to an upright Skewer, to extend themselves as soon as it is put over the Feather. Sometimes Resin and Wax exert their Electricity by only being expos'd to the open Air.

3. *Electricals per se* retain the Virtue longest when kept near to, or inclos'd by, other *Electricals per se*. Thus the rubb'd Tube will retain its Virtue pretty long in dry Air, as appears by chasing a Feather about the Room very long without new rubbing; as also by Lumps of Resin and Sulphur, &c. which have been melted and poured into dry Drinking-glasses, keeping their Virtue long, if kept in those Glasses, and wrapp'd in dry Silk, or such sort of Paper as will become electrical by rubbing; for as often as they are expos'd to the Air, they will attract.

4. *Electricals per se* communicate their Virtue to any of the *Non-electricals*, when brought near them; in which Case the *Non-electricals* attract and repel like
like

like the *Electricals per se*. Thus an Iron Bar suspended by a silken Thread, an Hair Rope, or a dry Cat-gut, when an excited *Electric per se* is brought near it, will both attack and send out its *Effluvia* to a *Non-electric* held near it; as appears in the Dark by the Light coming out at the End of the Bar.

5. An *Electrical per se* loses its excited Virtue in communicating to the *Non-electrical*; and the sooner, the more of those Bodies are near it. Thus in moist Weather the rubb'd Tube holds its Virtue but a little while, because it acts upon the moist Vapours that float in the Air; and if the rubb'd Tube be applied to Leaf-Gold or Brass, laid upon a Stand, it will act upon it much longer, and more strongly, than if the same Quantity of Leaf-Gold is laid upon a Table, which has more *Non-electrical* Surface than the Stand.

6. When a *Non-electrical* is suspended by, or only touches an *Electrical per se*, it receives the Properties of an *Electrical per se* from a rubb'd Tube or Wax, &c. This appears by the Fire that flashes from the Fingers of a Man suspended by Hair-Ropes, or who stands upon a Cake of Resin, when he has receiv'd Virtue from the rubb'd Tube.

7. The Virtue which a *Non-electrical* receives from a rubb'd Tube, runs on to the most distant Part of the suspended Body from the Place where the Tube is applied, and seems to be collected there, from whence it flashes in the Dark, snaps, and exerts its Attraction upon the Thread of Trial; though as the Virtue runs along, it sometimes shews itself in other Parts of the suspended *Non-electrical*.

8. If a *Non-electrical*, whilst it is receiving the Virtue from the rubb'd Tube, be made to communicate with the Floor of the Room, or any other great *Non-electrical* Body by a *Non-electrical* String, how small soever, (though but a Thread) the Virtue will not shew itself, as it did before, at the Extremities, where the Flash of Light was seen.

9. If a *Non-electrical* be ever so big, when suspended, it will receive Electricity from the rubb'd Tube. And if five or six hundred Foot long, when the rubb'd Tube is applied at one End, the Bodies hanging at the other End will become electrical. This has been tried by several People as well as myself.

10. If a long *Non-electrical* String be fasten'd to an *Electrical per se*, and extended to a great Distance, being supported by *Electricals per se* to keep it from touching the Ground, all Bodies fasten'd at the End of it will become electrical when the rubb'd Tube is applied at the other End, though the Tube does not touch it, but is only brought within two or three Inches of it.

N. B. This String we have before called the *Conductor of Electricity*, and the Cat-gut or silken Strings, Glass Tubes, or whatever kept the long String from touching the Ground, *Supporters*.

If any of the *Supporters*, mention'd in the last Observation, be chang'd for a *Non-electrical* Supporter, the Virtue will there be stopp'd and taken away by that *Supporter*: But if that *Supporter* be again supported by *Electricals per se*, it will only receive so much Electricity as will impregnate it, and then the

Virtue

Virtue will go on to the End of the String, and impregnate the Bodies fasten'd to it.

12. The *Non-electricals* receive the greatest Virtue at the End of the String, and most of all, if they are wet. But the *Electricals per se*, if long Bodies, as long Sticks of Wax, and Glass Tubes, only become electrical at the End next to the String.

13. *Electricals per se* will become *Non-electricals*, if they be wet, or only moisten'd. Thus Supporters that transmit the Electricity immediately, stop it when wet with a Sponge, or when blown through, if open Tubes. And if the long *Electricals per se*, hanging at the End of the Conductor, be made wet, they will become *Non-electricals*, and strongly receptive of the Virtue given by the rubb'd Tube at the other End of the String.

N. B. All the six Experiments mention'd in the Beginning of this Paper, confirm this Observation.

14. A *Non-electrical* having been impregnated with Electricity by the rubb'd Tube, is repell'd by it, till it has lost its Electricity by communicating it to another *Non-electrical*. Then being in its first State, it is again attracted by the Tube, which holds it till it has fully impregnated it; then it repels it again. This is evident, by attracting a Down Feather by the Tube in the Air, and then repelling it; so as to make it dance backwards and forwards to and from a Finger held up at a Foot or two from the Tube. But the Thing appears more plainly from the following

EXPERIMENT VII.

Having rubb'd the Tube *Tt*, (see TAB. I. Fig. II.) and with it attracted a Feather, the Feather at *t* was repell'd from the Tube, whenever it was brought near it; but suddenly dipping the End *T* of the Tube in Water, the Feather floating in the Air came to it again, and stuck to the End of the Tube at *T* or near *F*.

N. B. In fair Weather this Experiment will not succeed, unless the Tube be thrust pretty deep into Water (a Foot at least); but in moist Weather an Inch or two will do.

P. S. Though animal Substances be generally thought to be *Electrical per se*, yet it is only when they are very dry: This is the Reason why a living Man suspended by a Hair Rope, or standing upon a Cake of Resin, to receive Electricity from the Tube, must be consider'd as a *Non-electrical*, by reason of the Fluids of his Body.

N. B. The above Observations, together with the *Queries* in my former Papers, will be further illustrated by some Experiments which I do not now mention, because I have only try'd them at home; but when I have try'd them before the ROYAL SOCIETY, I shall give an Account of them in Order.

X. *An Account of some Electrical Experiments made at his ROYAL HIGHNESS the PRINCE of WALES's House at Cliefden, on Tuesday the 15th of April 1738. where the Electricity was convey'd 420 Feet in a direct Line. By the Same.*

HAVING heard that Electricity had been carried along an hempen String five or six hundred Foot long, but having only seen it when the String was carried backwards and forwards in a Room by *Silk Supporters*, I was willing to try it with a Packthread String stretch'd out at full Length; for which Purpose having join'd a Cat-gut String of six Foot long, I fasten'd it to the Inside of a Door in the Suite of Rooms at *Cliefden*; and having also tied another Cat-gut, like the first, to the other End of the String, I tied it up to the Inside of the Door at the other End of the House; but at the Place where the Packthread was join'd to the Cat-gut, I left a Foot and an half of Packthread hanging down, and fasten'd to it a *Lignum Vitæ* Handle of a Burning-Glass. Then applying a rubb'd Tube at the other End of the String, I made the Electricity run to the *Lignum Vitæ*, but with some Difficulty, which I attributed to the Size, being an animal Substance that still stuck to the Packthread as it was new; therefore I caus'd the Packthread to be wet with a Sponge from one End to the other, to wash off the Size: Then was the Electricity from the Tube communicated very soon and very strongly; for the

Thread of Trial (mention'd in my former Papers) was drawn by the *Lignum Vitæ* at the Distance of a Foot.

Afterwards having join'd more Packthread together, I made a String of four hundred and twenty Foot long, one End of which I fasten'd (by the Interposition of Cat-gut as before) to the Iron Gates in the Garden, before the House, and the End which had the *Lignum Vitæ* Handle, to the upper Part of the Door next to the Back-side of the House in a large Drawing-Room, taking care that the String came through the middle of the open'd Doors through which it pass'd; and to prevent this String dragging upon the Ground, three Pieces of Cat-gut held across by two Men, at equal Distances from the Ends, and from each other, supported it. The String was altogether dipp'd in a Pail of Water, before the Experiment; but great Care taken, that the Cat-gut should not be wet.

Then I applied the rubb'd Tube at the End in the Garden, whilst my Assistant held the Thread of Trial near the Handle above-mention'd, which Thread was strongly attract'd, though the Wind was very high, and blow'd in the contrary Direction to that in which the Electricity ran along.

I first tried the Experiment with the Packthread dry, but then it would not do at that Distance.

N. B. The Weather was moist when I made the Experiment.

XI. Observaciones Botanicæ, *Plantarum quarundam Descriptiones accuratiores exhibentes*; per Dⁿ. Paulum Henricum Gerhardum Moehring, M. D. factæ; ad Illustrissimum Dⁿ. Hans Sloane, Bart. R. S. Præf. missæ.

I.

SALICORNIA *ramis clavatis, squamis articulorum adpressis.*

PLANTA annua, quoad omnes partes succulenta, 8. 9. digit. Rhenan. raro pedem Rhenan. alta.

RADIX e multis fibris filiformibus constat.

CAULIS ramosus, tereti-compressus parum, ad radicem lignosus & rugosus, supra succulentus, glaber, aphyllus, clavatus: *Clavis* oppositis, alterno ordine e productionibus auriculatis, squamatis, vaginantibus, arcte adpressis, caulis primarii erumpentibus, $\frac{1}{2}$, 1. ad 1 $\frac{1}{2}$ dig. Rhen. longis, suprema terminatrice reliquis longiore.

FRUCTIFICATIO e *Flosculis* ternis, figuram trianguli birectanguli sphærici, cujus basis sursum convexa est, repræsentantibus; alterno ordine sibi oppositis, constat. His fructificationibus triangularibus omnes clavæ secundum longitudinem obsitæ sunt.

Insidet cuilibet commissuræ ramulorum squamato-denticulatæ, subacutæ, *denticulo* instar receptaculi totius fructificationis serviente, ac infima stamina ocludente.

CALYX est productio scapi squamata, marcescens quocum unum planum efficit;

Flosc.

Flosculi superioris, & quasi intermedii, rhomboides, angulo inferiori rectilineo acuto; Angulo superiori curvilineo obtuso;

Flosculorum duorum inferiorum, aut, si mavis, lateralium, Trigonus, basi parum latiore.

COROLLA nulla.

STAMINA Duo, supra & infra germen opposito situ, ejusdem basi adfixa, & e rimula calycis prominula: Superius primum exsurgit; quo delapso, inferius prodit. (Unde factum est, ut omnes Botanici unum modo stamen eidem adscribant).

Filamenta filiformia, intra calycem abscondita.

Antheræ oblongæ, erectæ, extra calycem hiantes, didymæ, (inde fit, ut primo subtiliori adpectu parvum corpus tetragonum præ se ferant) extrorsum concavæ, lateribus introrsum longitudinaliter convolutis, cum filamentis æqualis longitudinis, eidem in summo utrimque adhærentes, acutèque illud obtegentes, basi sua plerumque intra foveolam calycis inclusæ. In concavitatem istam multum farinæ genitalis delabitur.

PISTILLUM, *Germen* subrotundo-acuminatum, in medio filamentorum situm, intra calycem absconditum, ejusdem cum filamentis longitudinis.

Stylus nullus. *Stigma* capillare, acutum.

PERICARPIUM *Capsula* vesicaria, inflata, ovato-acuminata, exactè referens calyptram polytrichi DILLENII, sed deorsum magis ampullatam, intra calycem latens.

SEMEN unicum, ovato-compressum, horizontaliter in capsula situm, a basi ad medium sulcatum, tenuissimi ope *pedicelli* costæ caulis adfixum.

OBSERVATIONES.

a. Crescit heic locorum ubique ad oceani Septentrionalis litus, dummodo terra pinguis, argillacea (nostratibus *Schlick* vocata) adsit, & magnis fluxibus maris aqua salina interdum conspergi possit. Simul ac vero terra aggere circumvallatur, & maris undis subtrahitur, sicque salis marini nutrimento privatur, sequenti statim anno disparet.

b. Adhibetur inter acetaria, refrigeranti sapore se commendans.

c. Floret *Augusto*, semina perficit *Septembri* & *Octobri*. Plantulæ prodeunt fine *Aprilis*, & initio *Maii*: His cotyledones tereti-oblongæ, succulentæ.

d. Ergo pertinet in *Systemate Sexuali* LINNÆI ad *Diandriam Monogyniam*.

(e.) Alia haut ita pridem species *Saltzdahlensis*, *Ducatus* scilicet *Brunsvicensis*, nobis, siccis speciminibus, missa est a D. D. FRANC. ERN. BRUCKMANNO, quæ longe diversa a nostra, adpellari posset *Salicornia*, *ramis imbricatim pyxidatis*, *squamis articularum exstantibus*, propositaque a variis auctoribus videtur.

(f.) Tandem, genus *Salicorniæ* a nullo hactenus Botanico curate satis enodatum fuisse patet, teste fragmento amicissimi LINNÆI *Charact. Gener. Plantar. & Hort. Cliffort.* sua enarrantis ex incompletis TOURNEFORTII ac MAGNOLII adumbrationibus.

II.

VERBASCUM *foliis cordatis crenatis acutis glabris: floralibus ternis.*

PLANTA biennis, secundo anno florens, caule 7, 8, pedes alto.

CAULIS.

CAULIS plerumque simplex, nonnumquam inferne ramosus, longissimus, erectus, teres, brevissimis pilis villosus, lacte viridis, inferius valde foliosus, superius ab initio florum emergentium parvis foliolis stipatus.

FOLIA *primi anni & Caulis inferiora* sessilia, semi-plexicaulia, cordata, in acumen lanceolato-lineare acutum terminata, mollia, glabra, margine crenato, dentato: denticulis inæqualibus.

Floralia terna, lanceolato-linearia, acuta, parva, medio infimo duplo triplove longiori.

FLORES plerumque quaterni ex eodem sinu: in summo caulis unicus, reliquis suffocatis.

PEDUNCULUS brevissimus, calyce triplo fere brevior, crassius, simplex.

CALYX. *Perianthium*, ut in *Linn. gen. pl.* ad basin fere quinquepartitum: *laciniis* lanceolato-linearibus, hirsutis.

COROLLA. *Petalum*, ut *Linn. gen.* 153.

STAMINA. *Filamenta* 5. subulata, corolla brevior: *tribus superioribus* reliquis brevioribus, circumcirca lanuginosis: *duobus infimis* reliquis tertiam partem longioribus, declinatis, sursum incurvatis, in medio interni lateris lanuginosis.

Antheræ trium breviorum filamentorum triangulariter incumbentes, adpressæ, planæ, sexangulares: *duorum longiorum* filamentorum rectanguli figura incumbentes, adpressæ, planæ, sexangulares.

PISTILLUM *Germen* subrotundum. *Stylus* filiformis, inclinatus, staminibus parum longior, marcescens. *Stigma*, ut *Linn. gen.*

PERICARPIUM *Capsula* subglobosa, transversim a summo ad basin bifurcata, bilocularis, &c. ut *Linn. gen.*
In-

Involucrum hujus exterius est calyx erectus, fructum amplectens.

SEMINA Numerosa, oblongo-quadrangularia, truncata, minutissime rugosa, parva. *Receptacula*, ut *Linn. gen.*

Obf. a. Ergo quoad plurimas fructificationis partes simillimum existit *Verbasco* quinto LINN. *Hort. Cliff.* p. 55. in reliquis differt.

b. Corollarum color flavus, unguibus maculatis, purpureis : filamenta interne purpurea : horum lanugo partim purpurea, partim alba.

c. Semina vere 1738. ab optumo LINNÆO titulo *Verbasci e Virginia* missa, cum plurimis aliis noviter ex *America* advectis, atque ad Illustr. Dom. GE. CLIFFORD transmissis, plantas modo descriptas largita sunt, quæ hyeme solo sub tecto conservandæ apud nos videntur.

(d.) Flos tota æstate matutinis horis apertus.

e. Ergo *Verbasco* annum, foliis oblongis sinuatis obtusis glabris. LINN. *Hort. Cliff.* 55. melius circumscribetur hoc nomine : *VERBASCUM foliis oblongis sinuato-crenatis obtusis glabris, florali unico.* Cujus differentia specifica potissimum in his consistit : *Folium florale* ovato-acuminatum, unicum. *Flos* unicus. *Pedunculus* longissimus, filiformis, calyce triplo longior. *Calyx* sub fructu plano-expansus.

III.

SENECIO *foliis pinnatifidis laciniulatis : laciniis omnibus laxis patentissimis linearibus acutis.*

Jacobæa altissima, foliis crucæ artemisiæve similibus & æmulis. Rupp. Jen. 142.

E c

Caulis

Caulis humanæ altitudinis, teres, levissime angulosus, strigosus, glaber.

Folia alterna pinnatifida, glabra, laciniis æqualibus, linearibus, patentissimis, laxe lacinulatis, apicibus acutis, rachi lineari: color saturate viridis, inferiori parte parum pallidior.

Corollæ radii flavi, magni, mox revoluti.

Calycis in basi foliola reliquis adpressæ.

Obs. a. Dixi hanc plantam, Jena missam, Senecionem, secundum characterem, a LINN. *gen. plant.* 647. datum.

b. Ergo specie diversa a LINNÆI Senecione foliis pinnato lyratis, lacinulatis. *Hort. Cliff.* 406.

IV.

ILLECEBRUM *Linn. Corollar. gen.* 947. *Rupp. Jen.* 79. *Corrigiola Dillen. Giff. Supplem. adpend.* 167.

CALYX Perianthium pentaphyllum, carnosum, foliis crassis, erectis, compressis, introrsum ad summum fere excavatis, persistentibus, seta infirma terminatis.

COROLLA. Nulla.

STAMINA. *Filamenta* quinque triangulari-subulata, fructu breviora, intra calycem. *Antheræ* rotundulæ, erectæ, simplices.

PISTILLUM. *Germen* ovato-acutum, calyce dimidio fere brevius. *Stylus* nullus. *Stigma* simplex, turgidum, obtusum.

PERICARPIUM. *Capsula* membranacea, tenuissima, ovata, utrimque acuminata, simplex, univalvis, superne dehiscens, calyce tecta.

SEMEN. Unicum, ovatum, utrimque acutum, maximum, nitidum.

Obs.

Obs. a. Ergo pertinet in *System. Sexual.* LINNÆI ad *Pentandriam Monogyniam*, commodeque post genus 94. *Achyranthis* collocabitur.

b. Observatio tribus annis repetita hæc docuit, quum plantula humifusa copiose apud nos in arenosis humidis proveniat.

V.

RUPPIA foliis linearibus obtusis.

PLANTA aquis marinis innatans, flexuosa, perennis.

RADIX ramosa, fasciculos foliorum emittens; *corpus* teres, geniculatum, repens, brunnum, solidum, crassitie pennæ circiter columbinæ, aut parum tenuius; *fibrille* capillares simplices, e geniculis tantum prodeuntes, unciam unam alteramve longæ, albicantes.

Fasciculi caulini, culmi graminei specie, singuli e singulis radicalibus ramulis prodeunt, compressi, laxè geniculati, glabri, valde infirmi, flexiles, natantes, aquave marina refluyente, argillæ incumbentes, e quolibet geniculo vaginato, alterno ordine, ramos compositos emittentes.

Folia vaginantia, alterna, linearia, verticaliter obtusa, oblonge parallelogramma, glabra, mucosa, saturate viridia, basi geniculis caulinis infixæ, ramulos cum horum foliis, narcissorum instar, includunt. Versus extremitatem cujuslibet ramuli 2, 3, 4 folia, situ parallelo, unicæ vaginæ includuntur.

Foliis vaginatis omnibus fere, ipso caule longioribus, *fructificationes* ea quidem lege insunt, ut hæ in dimidia inferiori ejus parte intra rimulam compressam abscondantur. Supra & infra fructificationis

lineam geniculum transverse sulcatum est, hanc a petiolo infra, & a reliquo folio supra distinguens.

CALYX, *Involucrum universale, vagina spathe* vices supplens, in dimidia inferiori folii parte, introrsum rima media longitudinali, cujus latus unum ab altero aliquantum obtegitur, hians.

Spadix intra involucrum, membranaceus, albidus, compressus, obtusus, lateribus longitudinaliter parum convolutis, medio leviter carinato, dorsum involucri spectans, longitudine $\frac{3}{4}$ involucri, aut parum longior, numquam hujus finem attingens, fructificationes 9. ad 13. distice in longitudine obliqua fovens.

COROLLA Nulla.

Filamenta nulla. *Anthera* solitaria, ante florescentiam intra involucrum latentes, ovato-oblongæ, utrimque subacutæ, oblique adscendentes & distice secundum longitudinem spadici ita adhaerentes, ut nunc unam, nunc duas antheras unicum pistillum eodem in plano excipere videatur, singulis tamen antheris singula pistilla vere adsint, modo abscondita, modoque visui obvia. At explicata, (*i. e.* durante florescentia) ab alis suis lateralibus, elasticis, membranaceis, pelucidis, expansis in figuram naviculæ concavam incurvantur, carina deorsum prominente, & pistillis ita superimponuntur, ut distracta utrimque, patulaque facta involucri rima, ima sui parte fursum horizontaliter adscendant, atque navicularum concavarum specie secundum longitudinem extrorsum pateant. Corpus antherarum flavum & mucosum est, quod facile abstergi potest.

PISTILLUM *Germen* intra involucrum, oblongum, ad basin, qua spadici adfigitur, parum amplius. *Stylus* intra involucrum, simplex, filiformis, fursum recurvus,
per-

persistens, post florescentiam cum spadice parallelus, & parum curvus. *Stigmata* duo, tenuissima, capillaria, ab invicem diducta, extra involucrium horizontaliter exprorecta, cito marcescentia & evanida (ut relicta hinc macula e ferrugineo-nigricans facile alicui imponere possit, stigma modo unicum esse).

PERICARPIUM; *Cortex* tenuis, membranaceus, cylindraceus, basi obtusus, apice stylo munitus, stylo parum longior, cum spadice parallelus, basi liber; ad finem styli spadici adfixus, pedicelli ope brevissimi.

SEMEN In quolibet cortice solitarium, cylindraceum, utrimque obtusum, longitudinaliter subtilissime striatum, album.

Ergo pertinet in LINNÆI *System. Sexual.* ad *Gynandriam Monandriam*, minime vero ad *Gynandriam Polyandriam* ejusd. *ibid.* In *fragment. meth. nat.* LINN. p. 506. *Classium plantar.* ad *ordinem XLVIII.* quo & *Naias*, & *Zannichellia* referendæ videntur: In ROYENI *System.* ad *Palmas*, *Spatha bifida*. Vid. *Ejusd. Flor. Leyd. Prodr.* p. 9.

Crescit in aqua marina ad insulam *Wangeroogensem*, & reliquas ad sinum illum Oceani Septemtrionalis, *Wadt* dictum, sitas insulas. Durante aquarum adfluxu, eidem innatat; refluxo autem mari, argillæ tenaci, cui radix infigitur, incumbit.

Mirum, plantam adeo vulgarem locis maritimis, cujus folia (quæ sicca, papyri instar, alba, nigra, etiam evadunt) mensibus Augusto & Septembri a maris fluctibus ad littora abundantia copia rejiciuntur, a nullo, quantum memini, nisi forte Anglo botanico, descriptam aut depictam esse. Ruppix generi inferenda videtur, si hujus character, amicissimo LINNÆO non nisi ad siccum exemplar, ut signum †, in *generibus*

bus plantar. adjectum docet, examinatus, parum ampliatum. An malint alii novum genus?

VI.

HIPPURIS. Linn. gen. I.

Calyx, Nullus.

Corolla, Nulla.

Stamen, *Filamentum* unicum, subulatum, crassiusculum, deciduum, longitudine fere antherarum, germi-
ni insidens. *Anthera* didyma, quolibet segmento a tergo bisulcata, germine & filamento parum longior, crassa, carnosa.

Pistillum, *Germen* oblongum, infra stamen. *Stylus* unicus, filiformis, tenuis, erectus, acumine extrorsum reflexus, introrsum ad latus staminis ex apice germinis oriundus, stamine longior, marcescens. *Stigma* acutissimum.

Pericarpium Nullum.

Semen Unicum, oblongum, nudum, duriusculo tegmine abscondens nucleum parvum.

Obs. 1. Floret Jun. & Jul. fructum perficit August. Septembr. crescit abunde in fossis *Jeveranis*, prope *Emdam Frisæ Orientalis*, &c.

2. Variat *staminibus rubellis*, quæ copiosior; & aliis locis *staminibus albidis*, herbaceis, quæ rarior.

3. Ergo margines illi, a LINNÆO gen. I. sub calyce descripti, sunt veræ antheræ, nec aliud quidquam bona lente detegendum.

Vide

Vide TAB. I. Fig. A. B. C.

FIGURARUM, sub lente Musschenbrœckiana a me delineatarum, explicatio.

Fig. A. *Flos antice visus, ut filamentum adpareat.*

a. Filamentum.

b. b. Antheræ, antice visæ.

Fig. B. *Flos a tergo pictus, ut pistillum conspiciatur.*

c. c. Antheræ, postice visæ, ut sulci in conspectum prodèant.

d. d. Germen.

e. Stylus cum stigmate.

Fig. C. *Semen.*

f. Semen fere maturum.

g. Stylus marcescens, supra semen reflexus.

XII. *Observatio Anthelii Vitembergæ spectati;*
per J. Fried. Weidlerum Mathes. Prof.
ibidem Primar. & R. S. Lond. Soc. com-
municata, in Epistola ad C. Mortimerum,
R. S. Secr.

DIE $\frac{18}{7}$ Januarii A. 1738. quidam ex auditoribus meis, cum ante meridiem in campo prope *Vitembergam* agros dimetiretur, forte fortuna Anthelium, sive Solis imaginem e regione Solis veri versus septentrionem positam conspexit. Nimirum mane hora 8. coelum undique serenum, mox circa horam 9, e septentrionali plaga, nubes exoriuntur, ibique sensim

sensim & paullatim condensantur, temporis progres-
 su hæ in meridiem ulterius sese explicabant. Hora
 9. min. 30. cum nubes obscuræ fere verticem atti-
 gissent, apparuit in illis Sol Soli oppositus, pari ma-
 gnitudine, rotundus, admodum splendidus, ut oculi
 ejus lucem ferre non possent, corona sive halone
 ovali cinctus. Diameter coronæ major, Solis circiter
 diametros quinque, minor tres capiebat: Ipsa corona
 rubro flavoque coloribus exornata, parte rubra in
 Anthelium versa: Tractus nubium, reliquis intra
 coronam flavescebat, hinc inde etiam rubebat. In
 Anthelio decussatim sese secabant duæ iridis por-
 tiones, sub angulo 60 fere graduum, utrimque in
 ortum & occasum flexæ, & ad coronæ ovalis peri-
 pheriam continuatæ: Quales A. 1661. D: 6. Sept.
Hevelius quoque cum Anthelio vidit, quemadmodum
 in tractatum de Mercurio sub Sole p. 176. narrat.
 Durabat spectaculum per horæ quadrantem; cum
 enim nubes in meridiem longius extenderentur, &
 Solem verum occultarent, Anthelius evanuit. Ningit
 postea parum hora undecima: Mane lenis ventus ex
 media inter meridiem & occasum plaga spirabat.
 En figuram ab ipso spectatore mihi exhibitam, in
 TAB. II. Fig. I.

XIII. Occultatio Palilicii, A. 1738, d. 23.
 Decembris St. N. observata a D. Christ-
 fried Kirchio, Astronomo Regio, Berolini. Ex
 literis ad J. F. Weidlerum datis excerpta.

Tempus horolog. oscillar. H. M. S.		Partes microm. Tubo l. ped.	Valor partium microm. " "	Tempus correc- tum. H. M. S.	Tempus correc- tius. H. M. S.
1 56 37	* a centro <i>M. Sinai</i>	38	15 21	6 24 37	
2 58 30	* a centro <i>M. Sinai</i>	34	13 42	6 26 30	
3 7 3 54	Immersio stellæ			6 31 54	6 32 00
4 3 5 29	Emergio stellæ tub. 9 ped			7 33 29	7 33 33
5 5 35	Emergio certo facta			7 33 35	
6 6 43	* a centro <i>M. Sinai</i>	36	14 32	7 34 43	
7 15 32	* a prox. marg. Lunæ	8	3 14	7 43 32	
8 20	* a <i>M. Sinai</i>	46½	18 47	8 48	
	Diameter Lunæ	74 }	29 73		
	vel	73½ }	29 41	56	
10 29 12	<i>M. Sinai</i> a prox. marg. 2	8	3 14	7 57 12	
11 38	Diameter D sub. 9 ped.	99½	29 38	8 6	
12 42	Diameter D sub. 7 ped.	75	30	10	
13 46 55	* a <i>M. Sinai</i>	72	29 5	14 55	
14 8 48	Diameter Lunæ	73½	29 41	8 16	

Vide SCHEMA hujus Occultationis in TAB. II. Fig. 2.

Situs stellæ respectu macularum lunarium sequenti modo observatus.

I. Ante observationem (1.) h. 6. 20'. notavi stel-
 lam in linea recta a meridionali margine Insulæ
 Macræ (Posidonii) per septentrionalem partem Ponti
 Euxini, (medium maris Serenitatis) & M. Ætnam
 (Copernicum) producta: & linea a M. Sinai ad stel-
 lam fere stringebat littus sinus Sirbonidis. (M. Hu-
 morum).

II. Tempore observationis, (1.) stella erat in linea recta a lacu nigro majore (Platone) per partes orientales insulæ Cercinnæ (a Keplero versus ortum) producta.

III. Tempore observationis (2.) stella in linea per medium Paludis Mæotidis & medium M. Adriatici (per M. Crisium & S. medium) continuata.

IV. Tempore immersionis stellæ, sequentes lineam rectam ad illam coincidebant, & locum peripheriæ lunaris, ubi stella occultabatur, designabant.

1. A littore Pontus Euxini (M. Serenitatis) versus Cæciam spectante, per M. Ætnam (Copennicum).

2. A littore sinus Apollinis, per loca paludosa (sc. a littore S. Iridum, per Keplero).

3. A M. Sinai (Tychone) per littus meridionale S. Sirbonis (M. Humorum).

V. Emergio stellæ contigit e regione M. Paropamisii, (Furnerii) & in linea recta a lacu nigro majore (Platone) per Byzantium (Menelaum) producta, quæ stringebat sinum extremum Ponti (M. Nectaris).

VI. Tempore observationis (7.) M. Porphyrites, (Aristarchus) margo septentrionalis L. Thespitis (Fracastorii) & stella in linea recta.

VII. Tempore observationis (8.) Lacus Hyperboreus superior (Hermes) medium Paludis Mæotidis (M. Crisii) & stella in linea recta.

Occultatio Palilicii observata Vitembergæ Saxonum,
 d. XXIII. Dec. *St. N. A.* CIODCCCXXXVIII. a Jo.
 Friderico Weidlero, R. S. Lond. S. &c.

Immersio	6.	27.	35.
Emersio	7.	29.	20.
Duratio	I.	I.	45.

ANNOTATIONES.

1. Observatio a duobus spectatoribus simul perfecta; ego tubo astronomico IX pedum utebar, amicus & socius observationis per telescopium IV pedum lunam contuebatur.

2. Immersio & emerisio facta in instanti: uno tamen prope minuto horario citius per longiorem, quam per breviorum tubum animadversa.

3. Appulit stella ad marginem lunæ orientalem, circa gradum 163. schematis lunæ plenæ mobilis Heveliani, Selenographiæ, p. 364. emergebat circa gradum 272. ejusdem schematis. Itaque linea recta, puncta immersionis & emersionis copulans, stringit extrema maris Humorum & Nubium & inter Pitatum & Mare Nubium transit.

Cœlum tempore immersionis serenum non erat, sed tenues nubeculæ semper fere ante lunam & stellam oberrabant: ideoque figura stellæ oblonga, diu ante occultationem, per atmosphæræ vapores spectabatur.

XIV. *Eclipsis Solis observata Vitembergæ Sax-
onum, die IV. Aug. St. N. die XXIV. Julii*
St. Vet. A. CIODCCXXXIX. post merid. a
Jo. Friderico Weidlero.

Phases crescentes.

	Hor.	Min.	Sec.	<i>p. m.</i>
Initium	4.	15.	30.	
Digitus I.		22.	00.	
Digiti II.		29.	30.	
III.		35.	30.	
IV.		40.	00.	
V.		47.	30.	
VI.		55.	40.	
VII.	5.	2.	00.	
VIII.		9.	00.	
IX.		24.	40.	

Phases decrescentes.

VIII.	5.	35.	30.
VII.		43.	40.
VI.		50.	30.
V.		56.	00.
IV.	6.	2.	45.
III.		8.	40.
II.		14.	00.
I.		20.	45.
Finis		27.	20.

Vide Typum hujus Eclipsos ad Fig. 3. TAB. II.

Ad FIGURAM 4. TAB. II.

Observatio Immerfionis & Emerfionis macularum, quæ tempore eclipeos in disco folis conspicuæ fuerunt.

IMMERSIONES.		H.	M.	S.
Appulfus Lunæ ad maculam	(a)	4.	34.	35.
Tegitur tota macula	(a)	4.	34.	45.
Appulfus Lunæ ad maculam	(d)	5.	1.	30.
—————ad maculam	(e)	5.	5.	20.
—————ad maculam	(b)	5.	7.	15.
Immerfio totalis	(b)	5.	10.	00.
Appulfus Lunæ ad maculam	(c)	5.	16.	30.
Tegitur tota macula	(c)	5.	18.	00.

EMERSIONES.		H.	M.	S.
Incipit emergere macula	(b)	5.	30.	50.
Medium emerfionis	(b)		32.	30.
Emerfio totalis	(b)		34.	00.
Emerfio incipiens maculæ	(c)		39.	00.
Medium emerfionis	(c)		39.	50.
Emerfio totalis	(c)		40.	40.
Emerfio incipiens	(a)		41.	00.
Emerfio totalis	(a)		41.	40.
Emerfio	(d)	6.	4.	30.
Emerfio	(e)		6.	15.

ANNOTATIONES.

1. FIGURA 3. TAB. II. Siftit folis difcum recto fitu, qualem heliofcopium foris adfpicientibus oftendit.

2. FIGURA

2. FIGURA 4. TAB. II. Repræsentat maculas solis eo situ, quem sub principium deliquii habuere, quarum immersio & emersio durante eclipsi observata fuit.

3. Luna subiit solem circa gradum 102. a Zenith computatum: discessit a sole circa 53. gradum, ab eodem Zenith numeratum.

4. Tempore maximæ obscurationis lunæ orbis non plane niger, sed puniceo colore tinctus per telescopium apparuit. Maculæ tamen lunæ distingui non poterant.

5. Margo lunæ, parte sinistra, quæ in meridiem vergebat, circa tempus maximæ obscurationis, montium jugis distinguebatur, qui etiam in imagine, per telescopium picta, cernebantur. Reliquus margo sub sole conspicuus æqualis.

6. Durante tota eclipsi Lunæ peripheria nuda apparuit, absque nebula, vel nube, quæ in aliis quandoque eclipsibus eidem imminet. Circa finem tamen, cum unus circiter solaris discum digitus adhuc occultaretur, motus lucis solaris vehemens in margine Lunæ aspero notabatur.

7. Denique prætermittendum non duco, quod amicus, harum rerum probe gnarus, qui per Telescopium Astronomicum ix pedum solem intuebatur, circa hor. iv. 31 min. in obscuro Lunæ disco lucem aliquam, instar fulguris, celeriter huc illuc in tenebris diffusam, animadverterit: & quod idem observator circa horam v. min. 50 toti adstantium coronæ affirmaverit, a se tum ter talia fulgura subito enitentia iterum conspecta fuisse.

XV. *Part of a Letter from Mr. J. Derby to Mr. Henry Sheppard, concerning a terrible Whirlwind, which happen'd at Corne Abbas in Dorsetshire, Oct. 30. 1731. communicated to the ROYAL SOCIETY by Edmund Halley, LL. D. V. Pr. R. S. and Astronom. Reg.*

— **O**N *Saturday* the 30th of *October* last past, about a Quarter before One in the Night, there happen'd at *Corne-Abbas, Dorsetshire*, a very sudden and terrible Wind Whirl-puff, as I call it: Some say it was a Water-spout, and others a Vapour or Exhalation from the Earth; but be it of what Name it will, it began on the South-west Side of the Town, carrying a direct Line to the North-east, crossing the Middle of the Town in Breadth two hundred Yards. It stripped and uncovered tiled and thatched Houses, rooted Trees out of the Ground, broke others in the midst of at least a Foot square, and carried the Tops a considerable way. The Sign of the new Inn, a Sign of five Foot by four, was broke off six Foot in the Pole, and carried cross a Street of forty Foot Breadth, and over an opposite House, and dropp'd in the Backside thereof. It took off and threw down the Pinacles and Battlements of one Side of the Tower; by the Fall of which, the Leads and Timber of great Part of the North Alley of the Church was broke in. The Houses of all the Town were so shock'd, as to raise the Inhabitants; no hurt was done but only
across

across the Middle of the Town in a Line. No Life lost, but Three had a very providential Escape. 'Tis computed by judicious Workmen, that the Damage sustain'd by this Accident amounts to Two hundred Fifty-eight Pounds, and upwards. It is very remarkable, it only affected, as I have related: no other Parts of the Neighbourhood or Country so much as felt or heard it. It is supposed by the most Judicious, that it began and ended within the Space of two Minutes. 'Twas so remarkably calm a Quarter after Twelve, that the Exciseman walked through two Streets, and turned a Corner, with a naked lighted Candle in his Hand, unmolested and undisturbed by the Air; and as soon as over, a mighty Calm, but soon followed by a prodigious violent Rain. If this Fact be worth relating, I should be glad to know amongst the Learned, what it might be called, or, if to be known, from what Cause it might arise.

Mintern Magna,
Nov. 13. 1731.

*Your obliged Friend,
and most humble Servant,*

J. Derby.

XVI. *An Account of Letters found in the Middle of a Beech, by J. Theod. Klein, Secretary of Dantzick, F. R. S. communicated to the ROYAL SOCIETY by Sir Hans Sloane, Bart. Pr. of the R. S. and Physician to His MAJESTY, &c. Translated from the Latin by T. S. M. D.*

IN the Year 1727. a *Beech-tree* was fell'd near *Elbing*, for the domestic Use of *John Maurice Mæller*, then Post-master of *Elbing*, now Secretary of his native City. The Trunk being saw'd into Pieces, one of these, three *Dantzick* Feet six Inches long, cleft in the House on the 30th of *June*, discovered several Letters in the Wood about one Inch and a half from the Bark, and near the same Distance from the Centre of the Trunk. The Hewer, having at one Stroke unfolded such a Prodigy, and believing there was Witchcraft at the Bottom of it, ran in all possible Haste for his Master: But this Gentleman, well instructed in sound Philosophy, gave Orders to preserve the Pieces of Wood, and had them brought to my Study, at the same time communicating to me the History, and his Sentiments thereon.

Figure 5. TAB. II. exhibits the Letters conspicuous in the solid Wood, two of which, DB, shew their old Bark smooth and sound. The Wood lying between the Letters and the Bark of the Trunk, as well as that between the Letters and the Heart of the Tree, is likewise solid and sound, bearing not the least

G g

Trace

Trace of Letters. The Characters gD , being somewhat hollow, receive the Bark of the Letters DB^* .

The same Letters are seen in the Bark of the Tree, only that they are partly ill-shaped, partly almost effaced, whereas those within bear a due Proportion, as if done with a Pencil.

Now should it be ask'd after what manner these Letters reach'd the Middle of the *Beech*? and how it came to pass, that two of them, and no more, had their old dry Bark sticking to them?

Both these Queries are answer'd by the Vegetation of Plants. But as this is not a proper Place to expound it, I will suppose it known, and thus briefly complete the Affair.

'Tis an ancient Custom to cut Names, and various Characters, on the Rinds of Trees, especially on such as are smooth. That this has happen'd to our *Beech*, the mere Inspection of the Bark commands our Affirmation †.

An Incision made, the *Tubuli* conveying the nutritious Juice, and the *Utriculi* in which it is prepared, are divided and lacerated, and more of them, as the Incision was made deeper and wider: and consequently the Sap is not carried on in the Circulation, but extravasated and stopped at the Wounds. Hence the Origin of the Characters in the Bark and Wood.

*. *Daniel Barckholtz*, formerly *Cæsarean* Poet Laureat.

† The Characters, besides DB , mark the Names of a noble Family, to which the Land, whereon the Tree was fell'd, formerly belong'd: *Reginæ*, *Dorothea*, *Michael*, *Gertrude*, *Joannes*, *Helwingii*.

Now as a new Circle of *Fibres* grows yearly on the Tree between the Wood and Bark, 'tis not impossible but a Number of these should in a Process of Years more and more surround the ingraven Characters, and at length cover them. And this Number was the greater in our *Beech*, on account of better than half a Century elaps'd since the Incision, which was made in the Year 1672. as appears on the Outside of the Bark, as may be seen in the Figure. But while new Circles of *Fibres* are successively added, the *Tunicle* or Skin of the Bark is broken each Time, and the *Utriculi* extended and dilated.

Wherefore 'tis easy, from what has been said, to draw the Reason, why the Bits of Bark cut off on all Sides, in the Letters DB, had the same Fate with the Letters; why the Wood between the Bark and Letters was solid and sound; and why the Shapes of the Letters bore a just Proportion in the Middle of the Wood, and not in the Bark.

So much for our *Beech*.

Now let us see, in few Words, what Authors say of such figured Woods.

Solomon Reiselius, of *Letters found within the very cleft Trunk of a Beech*, *Eph. Nat. Cur. Dec. 1. An. 6. Obs. 4.* has at length, though with some Difficulty, guess'd the genuine Cause from frequent Examples of Incisions.

But *Joannes Meyerus*, on a *Thief hanging from a Gibbet, drawn by Nature's Pencil in a Beech*, *Eph. N. C. Dec. 3. An. 5. Obs. 29.* and *Joannes Petrus Albrechtus*, on a *certain rare Figure seen in a Beech*, *Eph. ibid.* ascribe it to a Sport of Nature, and give this Reason; because they could not discover the

least Sign of Imposture, the deep Situation of the Figures hindering them from having any Suspicion on that head.

On the contrary, *Luke Schræckius*, on figured Beech-wood, *Eph. N. C. Dec. 3. An. 7, 8. Obs. 118.* follows *Reiseli*'s Opinion; and being vers'd in *Malpighi*'s Anatomy of Plants, writes: " No wonder, " if Figures cut in a young Tree, by the Length of " Time, and the Accretion of many Barks, appear at " last about its Middle, when grown old."

John Christopher Gottwald, on a crucified Man drawn by Nature in the Middle of a Beech-trunk *, *Eph. N. C. Dec. 3. An. 9. Obs. 158.* accusing Nature's simple Violence, or a Disease of the Tree, is corrected by the most celebrated *John James Scheuchzer*, in his *Itinera Alpina*, Tom. 3. pag. 414. and in his *Herbarium Diluvianum*, p. 46. of a little Man in Beech-wood, *Tab. X.* where he makes mention of other Instances.

John Melch. Verdries is of the same Sentiment, treating of a Figure found in the Middle of a Beech, *Eph. N. C. Cent. 3 & 4. Obs. 89.*

There remains, to my Knowledge, the Figure of a Chalice with a Sword perpendicularly erect, and on its Point sustaining a Crown, found in the Heart of a Piece of Wood at the Hague; which the Authors of the Collections of *Breslaw* exhibit to us " as a " singular Phenomenon, worthy of being compared " to *Aldrovandus*'s *Guaiacum*-tree, and figured " Stones, if no Optic Fallacy, Error of Judgment,

* This Wood is kept in the Library of the Council of *Dantzick*.

“ artificial Fiffure of the Wood, or other fuch Deceit,
 “ intervene.”

In fine, the Cafe is thus: Such as were wont to be frightened with Hobgoblins from their Infancy, hear the Ruffling of Phantoms a great way off, and fee them walk at Noon-day; while others, who have learned to inquire into the Cauſes of Things, are by thoſe accounted dull of Sight and Hearing.

XVII. *Part of a Letter from Sir John Clark, one of the Barons of His MAJESTY's Exchequer in Scotland, and F. R. S. to Rog. Gale, Eſq; Tr. R. S. Nov. 6. 1731.*

— I Was lately in *Cumberland*, where I obſerved three Curioſities in *Whinfield-Park*, belonging to the Earl of *Thanet*. The firſt was a huge Oak, at leaſt ſixty Foot high, and four in Diameter, upon which the laſt great Thunder had made a very odd Impreſſion; for a Piece was cut out of the Tree about three Inches broad, and two Inches thick, in a ſtrait Line from Top to Bottom. The ſecond was, that in another Tree of the ſame Height, the Thunder had cut out a Piece of the ſame Breadth and Thickneſs, from Top to Bottom, in a ſpiral Line, making three Turns about the Tree, and entering into the Ground above ſix Foot deep. The third was the Horn of a large Deer found in the Heart of an Oak, which was diſcovered upon cutting down the
 the

the Tree. It was found fixed in the Timber with large iron Cramps; it seems therefore, that it had at first been fastened on the Outside of the Tree, which in growing afterwards had inclosed the Horn. In the same Park I saw a Tree thirteen Feet of Diameter.

Remarks by the *Publisher*.

THIS Horn of a Deer found in the Heart of an Oak, and that fastened with Iron Cramps, is one of the most remarkable Instances of this kind, it being the largest extraneous Body we have any-where recorded to have been thus buried, as it were, in the Wood of a Tree. If *Foannes Meyerus*, and *Foannes Petrus Albrechtus*, (p. 233.) had seen this; they could not have imagined the Figures seen by them in Beech-trees to have been the Sport of Nature, but must have confessed them to have been the Sport of an idle Hand. To the same Cause are to be ascribed those Figures of *Crucifix's*, *Virgin Mary's*, &c. found in the Heart of Trees; as, for Example, the Figure of a *Crucifix*, which I myself saw at *Mastricht*, in the Church of the *White Nuns* of the Order of *St. Augustin*, said to be found in the Heart of a Walnut-tree upon its being split with Lightning. And it being usual in some Countries to nail small Images of *our Saviour* on the Cross, of *Virgin Mary's*, &c. to Trees by the Road-side, in Forests and on Commons; it would be no greater a Miracle to find any of these buried in the Wood of the Tree, than it was to find the Deer's Horn so lodged.

Sir *Hans Sloane*, in his noble *Museum*, hath a Log of Wood brought by Mr. *Cunningham* from an Island in the *East-Indies*, which, upon being split, exhibited these Words in *Portuguese*, DA BOA ORA. i. e. De[us] bonam Horam.

Printed for T. WOODWARD, at the *Half-Moon*, between the *Two Temple-Gates* in *Fleetstreet*; and C. DAVIS, over-against *Gray's-Inn Gate* in *Holbourn*; PRINTERS to the ROYAL SOCIETY. M.DCC.XLII.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *November* and *December*, 1737.

The CONTENTS.

- I. *A Letter from his Excellency Nicolas-Michael d'Aragona, Prince of Cassano, and F. R. S. to the President of the ROYAL SOCIETY, containing an Account of the Eruption of Vesuvius in May 1737. Translated from the Italian by T. S. M. D. F. R. S.*
- II. *An Abstract of a Letter from an English Gentleman at Naples to his Friend in London, containing an Account of the Eruption of Mount Vesuvius, May 18. and the following Days, 1737. N. S.*
- III. *De Atmosphæra Lunari, Dissertatio Astronomica. Auctore D^{no} Johanne Paulo Grandjean de Fouchy, in suprema Gallix Rationum Curia Senatore, & Reg. Scient. Acad. Paris. Socio.*
- IV. *A*

The CONTENTS.

- IV. *A Narrative of an extraordinary Sinking down and Sliding away of some Ground at Pardines near Auvergne, sent from M. T— to a Relation in England, translated from the French, and communicated to the ROYAL SOCIETY by Phil. Henry Zollman, Esq; F. R. S.*
- V. *A Dissertation on the Worms which destroy the Piles on the Coasts of Holland and Zealand, by Job Baster, M. D. F. R. S. communicated by the President of the ROYAL SOCIETY. Translated from the Latin by T. S. M. D. F. R. S.*
- VI. *Two Observations of Explosions in the Air; one heard at Halsted in Essex, by the Rev. Mr. A. Vievar, Minister of that Place; the other by Sam. Shephard, Esq; of Springfield in the same County.*
-

Printed for T. WOODWARD, at the *Half-Moon*, between the *Two Temple-Gates* in *Fleetstreet*; and C. DAVIS, over-against *Gray's-Inn Gate* in *Holbourn*; PRINTERS to the ROYAL SOCIETY. M.DCC.XLII.



TAB. I.

Fig. 1.

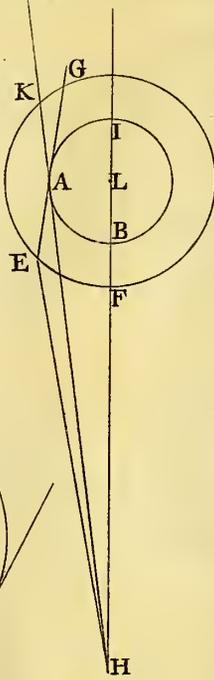


Fig. 2.

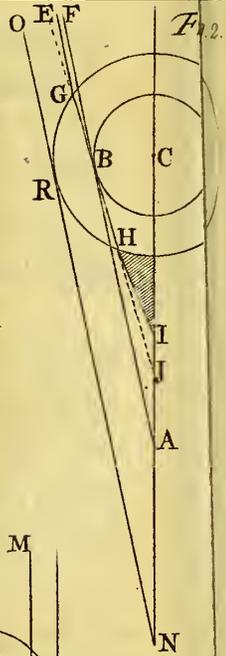


Fig. 3.

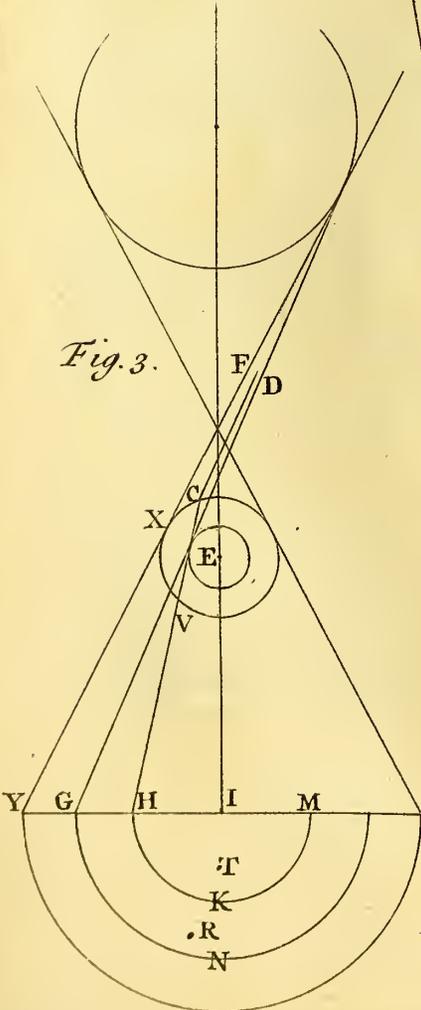


Fig. 4.

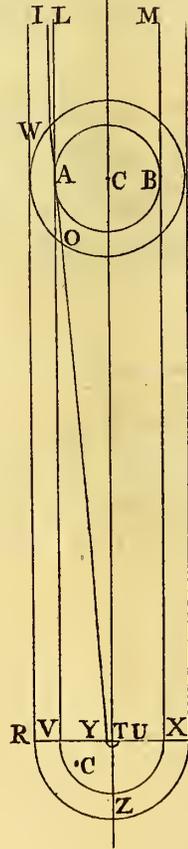


Fig. 1.



Fig. 2.

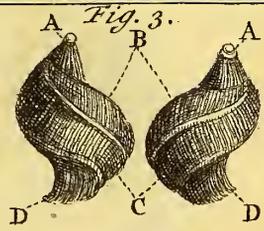


Fig. 4.

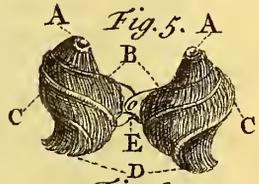


Fig. 6.

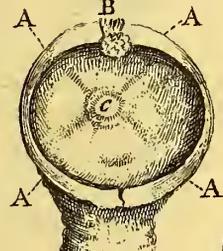


Fig. 7.

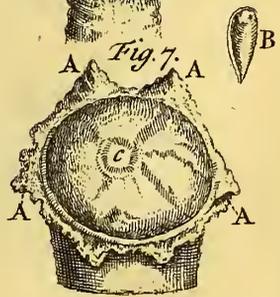
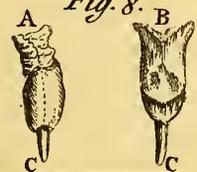


Fig. 8.



TAB. II.

1883



I. *A Letter from his Excellency Nicolas-Michael d'Aragona, Prince of Cassano, and F. R. S. to the President of the ROYAL SOCIETY, containing an Account of the Eruption of Vesuvius in May 1737. Translated from the Italian by T. S. M. D. F. R. S.*

S I R,

AS your extraordinary Talents, and excellent Taste, in a true Examination of Natural Effects, and in Discoveries relating to Experimental Philosophy, are so well known, that you have, with Justice, been elected into the most celebrated Academies of *Europe*, and to the Presidentship of the ROYAL SOCIETY of *London* in particular; I resolved, with good Reason, to offer you a short Account of the last great, dreadful and pernicious Eruption of our *Vesuvius*; to the End that, if you are pleased to investigate the Causes thereof, the Republic of Letters might reap some general Advantage, as it does daily, by means of its Members of the first Rank in Merit.

Mount *Vesuvius* is generally esteem'd about seven Miles distant from *Naples*. It rises in the Middle of a large Plain, which surrounds it on every Side. It is better than four Miles from the Sea; and the Foot of the Mountain is seen to begin from the Sea-Coast, which growing gradually higher, reaches the first Plain, to which one can easily ride on Horseback. The Figure of the Plain is almost circular, being about five Miles in Diameter, and half a Mile

H h

per-

perpendicular Height above the Level of the Sea. This is the Basis of the Mountain, out of which arises another, called by the People of the Country *Monte-vecchio*, whose perpendicular Height is about four hundred Paces, and its Top little less than two Miles in Circumference, of an irregular Figure. The said Top, before the Year 1631. was of the Form of a Basin, but all surrounded with aged Oaks, and vastly large Chestnut-trees, whose Fruit afforded Food sufficient for a Number of Cattle that fed thereon. In the Bottom a Cavern was observed, into which People descended above two hundred Paces, by difficult and interrupted Paths; and this Opening was looked upon as the antient Mouth, which for a long Space of Time had constantly cast up great Quantities of bituminous Matter, and had at the same time burnt a considerable Part of the neighbouring Country, cultivated by the Inhabitants round the Hill.

Concerning the Eruptions that have happened heretofore, they are very numerous, as well antient as modern.

Of the first, several are taken Notice of by *Berosus Chaldaeus*, *Polybius*, *Strabo* in the time of *Augustus*, *Diodorus*, and *Vitruvius*; and in *Trajan's* Reign the Name of the Mountain became more famous by the Death of *Pliny*. From that time forward, 'tis not doubted, that the Eruptions were less frequent down to the Year 1139; when, after a considerable Eruption, it began to take Rest, and continued quiet somewhat less than five Centuries; so that the horrid Remembrance of the past Ruins was pretty well obliterated out of the Minds of the
neigh-

neighbouring Inhabitants; who, vainly flattering themselves with Hopes, that the inflammable Matter was spent, planted the whole District round the Mountain, which, by its Fertility, became the Delight of these Parts. But, in Process of Time, they found themselves deceived and frustrated in their Expectations: For in the Year 1631. during six Months Space, continual Rumbings were heard, and Shocks of Earthquakes felt: And afterwards, in the Month of *December*, a dreadful fiery Eruption happen'd, which first blew up Part of the Mountain into the Air, in a terrible Manner, and then vomited out Water, Ashes, Stones and Fire; inundating almost the whole Country around, to the Sea, and for above seven Miles in Breadth, with the irreparable Loss of more than four thousand People. After which the Mountain became silent, and remain'd considerably diminish'd in its Height, from what it had been before.

It continued quiet for twenty-nine Years; but having rekindled in 1660. its Fire fill'd the whole Capacity of the immense Hollow, which remain'd since the Year 1631; whence, after several lesser Eruptions, a new Mountain appear'd in 1685.

In 1707. not only the Inhabitants of the Neighbourhood, but also the whole City of *Naples*, were put into great Terror, and not without Reason, by the Apprehensions of a Renewal of the dismal Tragedy of 1631. upon account of the frequent Noise and Shocks, the Fire seen on the Top of the Mountain, with a vast Quantity of Ashes, which issuing out with Impetuosity, were dispersed all over our Hemisphere, and darken'd the Light of the Sun for one

whole Day's Space. These were all manifest Signs of the impending Desolation : and yet (whether by a Miracle of our particular Protector St. *Jannarius*, as some were of Opinion, or by natural Causes) this dreadful Day, which had portended so much Mischief, was beyond Expectation, and to our great Astonishment, follow'd by another as pleasant as could be desired : for the Air was quite serene, and clear of the Ashes ; and on the Mountain there was no other Appearance but that of a little Smoke.

In the Year 1724. the Quantity of Ashes and Stones, thrown from the Top of the Mountain, were so heaped from the Bottom up to the Edge of the old Mountain, that the whole Space from the old Hill to the new, appeared but one continued Mountain.

In 1730. there was another Eruption of *Vesuvius*, which, though very inconsiderable in respect of the last, yet was the Occasion of much Fear.

This present Year 1737, to the Month of *May*, the Mountain was never quiet : Sometimes emitting great Quantities of Smoak, at other times red-hot Stones ; which, for want of a sufficient impelling Force, fell on the same Mountain. But in order to a clear Idea of all the Circumstances presaging the impending Eruption, 'tis requisite to know, that in the Beginning of *May*, a Smoak only was seen to issue from the open Mouth at the Top ; and from the 16th to the 19th, subterraneous rumbling Noises were heard.

On the 19th, Fire was seen to burst out in thick black Clouds ; and the same Day there were several loud Reports, returning quicker towards the Evening :

ing: And still more on *Sunday* Night, when there constantly appear'd a very great Smoak mix'd with Ashes and Stones; and the Neighbourhood felt some Shocks, like those of a weak Earthquake.

On *Monday* the 20th, at the 13th Hour, the Mountain made so loud an Explosion, that the Shock was strongly felt not only in the Neighbourhood, but also in the Cities twelve Miles round. Black Smoak, intermixt with Ashes, was seen suddenly to rise in vast curling Globes; which spread wider, as it moved farther from the Basen. The Explosions continued very loud and frequent all this Day, shooting up very large Stones through the thick Smoak and Ashes, about a Mile high, to the Horror of the Beholders, and Danger of all the neighbouring Buildings.

At the 24th Hour of the same *Monday* 20th of *May*, amidst the Noise, and dreadful Shocks, the Mountain burst on the first Plain, a Mile distant obliquely from the Summit, and there issued from the new Opening a vast large Torrent of Fire; whence, by the Quantity of Fire incessantly thrown up into the Air, at a Distance all the South Side of the Mountain seem'd in a Flame. The liquid Torrent flow'd out of the new Vent, rolling along the Plain underneath, which is above a Mile long, and near four Miles broad; and in its Way it spread very speedily near a Mile wide; and by the fourth Hour of the Night, it reached the End of the Plain, and to the Foot of the low Hills situate to the South. But as these Hills are rugged with Rocks, the greatest Part of the Torrent ran down the Declivities between these Rocks, and into two Valleys; falling
suc-

successively into the other Plain, which forms the Basis of the Mountain; and after uniting there, it divided into four lesser Torrents, one of which stopp'd in the Middle of the Road, a Mile and half distant from the *Torre del Greco*. The second flow'd into a large Valley. The third ended under the *Torre del Greco*, near the Sea; and the fourth at a small Distance from the new Mouth.

The Torrent which flowed into the Valley, ran as far as between the Church of the *Carmelites* and that of *the Souls of Purgatory*, by the 8th Hour on *Tuesday*. The Matter of the Torrent ran like melted Lead: In eight Hours it made four Miles; and consequently, it flowed half a Mile in an Hour: A new and remarkable Circumstance of this Eruption, seeing *Bulifone* thought it very strange, that in the Eruption of 1698. the Torrent had advanced sixty Paces in an Hour; whence he infers, that such great Swiftnefs proceeded from a greater Degree of Liquefaction of the Matter. The Trees, which the Torrent light on in its way, upon the first Touch took Fire, and fell under the Weight of the Matter.

The Torrent which ran behind the Convent of the *Carmelites*, after setting the little Door of the Church on Fire, entered not only therein, but also through the Windows of the Vestry, and into two other Chambers. In the Refectory, it burnt the Windows; and, what is surprising, the Glass Vessels, that stood on the Tables, were melted into a Paste by the violent Heat of the Fire. Sixteen Days afterwards, the Matter continued hot, and was very hard, but it was broke by repeated Blows.

A Piece of Glass fasten'd on the Top of a Pole (and thrust into this Matter) was in four Minutes reduced to a Paste. Under the Mass of the Torrent were heard frequent Reports, which made the Church shake, as if by an Earthquake. Along the whole Surface of the Torrent, there appear'd small Fissures, out of which issued Smoak, that stunk of Brimstone mix'd with Sea-water; yet these Exhalations are not poisonous, but rather a Remedy for some Diseases. The Stones round about these Fissures were observed to be covered with sublimed Salts, the Nature of which I shall explain hereafter.

Iron, thrust into these Fissures, was taken out moist; but upon thrusting in Paper, it was not moisten'd, but rather somewhat harden'd.

At the same time when the new Mouth open'd; that on the Summit of the Mountain vomited a vast Quantity of burning Matter, which, dividing into Torrents, and small Streams, ran partly towards the *Salvadore*, and partly towards *Ottajano*; and at the same time that this Matter issued out, red-hot Stones were seen to be cast out of the Mouth, in the midst of black Smoak, frequent Flashes of Lightning and Thunder, all produced by the same Matter.

These impetuous Expulsions of Fire continued till *Tuesday*, when the Eruption of the melted Matter, the Flashes, and thundering Noise, ceased; but a strong South-west Wind arising, the Ashes were carried in great Quantities to the utmost Boundaries of the Kingdom; in some Places very fine, in others as coarse as *Iselbian* Sand: And in the Neighbourhood they not only felt this plentiful Shower of Ashes, but

but likewise Pieces of Pumice-stones, and other large Stones.

Tuesday Night the Fury of the Mountain began to abate, so that on *Sunday* there was scarce any Flame seen to break out of the upper Mouth; and on *Monday* but little Smoak and Ashes. This Day it began to rain plentifully, which continued to *Tuesday*, and afterwards for many Days: A Circumstance which has constantly happen'd after the Eruptions of Times past.

The Damages done in the Neighbourhood by this Eruption of Fire and Ashes, are incredible. At *Ottajano*, situate between four and a half and five Miles from *Vesuvius*, the Ashes on the Ground were four Palms high. All the Trees were burnt, (or blasted) the People terribly affrighted, and many Houses crushed by the Weight of the Ashes and Stones that fell.

After the Description of this fiery Eruption, the Academy of Sciences [at *Naples*] thought proper to make an accurate Analysis of the Matter, and of the Salts, that were collected in great Plenty near the above-mentioned Fissures; and, towards the Discovery of the Truth, they effectually made the following Experiments:

EXPERIMENT I.

Some of the Stones of *Vesuvius* being pounded small, and the Loadstone applied to the Powder, some few Particles were attracted by it; and the same Powder, put into *Aqua-fortis*, caused a sensible Effervescence; whence it certainly contains no small Quantity of Iron: Which was also found upon Trial
in

in another Eruption by *Tomaso Cornelio*. But for the greater Elucidation of Truth, one of these Stones being applied to the Magnetic Needle, it turned to the Stone; and then carrying it round to the opposite End of the Needle, it immediately turned from it, in the same manner as if Iron was applied near the Compass.

EXPERIMENT II.

The Stones are not all of the same Density or Colour; but various, and of different Ponderosity. Some are composed of real Talc, others full of Marcasites: Some are almost all sulphureous, others nitrose; some of a grey Colour, others red.

EXPERIMENT III.

The Matter of the Current is spongy at Top, but very dense towards the Bottom; which is a Proof of the Fusibility thereof; whereby the heavier Bodies subsided, and the lighter remained at Top.

EXPERIMENT IV.

After growing hard, it retained part of the Heat above a Month, though unequally: For in the inward Parts, where the Air had not free Access, and the Matter was more compact, the Heat was much stronger than towards the Surface.

EXPERIMENT V.

Twenty Days after the Eruption, in divers Parts of the Mountain, from the Bottom to the Top, there were seen to arise many pernicious Damps,

[*Mofete*] especially from the Cavities, and the Fiffures of former Torrents; as also on the Plain: But none were observed in the Matter of this last Eruption. They issued out of the Fiffures under the Appearance of a cold Wind, and rose about three Palms high; then they moved along the Surface of the Ground, and, after a Progress of some Paces, disappeared. Animals, which happened to graze where these passed, were all killed thereby; and likewise a *Teresian* Frier, who inadvertently breathed the Vapour of one of these Damps.

EXPERIMENT VI.

Having placed the Barometer in the Vapour, it underwent no Change, but the Thermometer fell somewhat more or less. A lighted Torch, thrust into them at two Palms from the Ground, was soon extinguished by the Action of the Damp.

EXPERIMENT VII.

These Damps grew gradually weaker in their pernicious Effects for above three Months, even to the subsequent Autumn; as has been generally found in other former Eruptions, or when they happened to issue out of their Vents.

EXPERIMENT VIII.

Concerning the Salts which are generated in Abundance in *Vesuvius*, I have, by Order of the Academy, examined them by accurate Experiments. My Intention was to know, if besides Salt Ammoniac, there were also Sea-Salt, Vitriol, Nitre, or any other

other Salt. I thought there was no better way of proceeding in this Inquiry, than by Crystallization; because it is universally allowed, that Salts in Crystallizing constantly retain one certain and determinate Figure; Sea-Salt concreting into Cubes, Vitriolic Salt into Rhomboidal Parallelepipeds, Alom into Octædrons, and Nitre into Rectangular Prisms on Hexagonal Bases. I imagined, that if the Salt of *Vesuvius* happened to contain any Particles of the Salts above-mentioned, it would discover them after Crystallization. This way of Reasoning was confirm'd by Experiment: For the *Vesuvian* Salt, in Crystallizing, left on the Sides of the Vessels small Parcels of crystallized Salts, which, observed through a Microscope, resembled a Tree with its Branches, on the Ends of which there appeared several Pyramids of an irregular Figure, but very sharp-pointed; and between the Branches there were interspersed in some Places a Group of Prisms, in others some small Cubes: Whence I inferred, that the aforesaid Salt was Ammoniacal, and indeed a genuine and efficacious Salt Ammoniac, with insensible Portions of Nitre and Sea-Salt. Which coincides with the Sentiments of the Royal Academy of *Paris* in 1705; with those of *Thomas Cornelius* in his *Progymnasma de Sensibus*; of *Dominicus Gulielmini* in his Treatise *de Salibus*; of *Dr. Boerhaave* in his Chemistry, and many other Writers.

EXPERIMENT IX.

In order to be convinced whether this Salt was really Ammoniacal, and of the Nature of neutral Salts, I mixed it with Spirit of Vitriol, and Spirit

of Salt, without producing the least Fermentation. I afterwards put some of it into Oil of Tartar *per Deliquium*, and could not perceive any Ebullition; wherefore it is to be ranked among the neutral Salts.

EXPERIMENT X.

Thrown upon red Coals, it did not crepitate like Sea-Salt, but it boiled and swelled, and after evaporating it dried up.

EXPERIMENT XI.

It is of a very pungent Taste, strongly pricking the Tongue, and of a bituminous Smell of Brimstone, which occasions a violent Head-ach by its volatile Texture.

EXPERIMENT XII.

The Salts taken from different Stones are not all of the same Weight or Colour: For some are yellow and unctuous, as if rubbed all round with *Petro-leum*: Others are very white, others blackish, and others of other Colours, according to the Stones they adhered to.

EXPERIMENT XIII.

I have likewise found by Experience, that the Salt Ammoniac of *Vesuvius* is much more efficacious than any other Salt known at this Day, in cooling Liquors. Upon dissolving some of it in Water, it makes the Water so cold, that the Sides of the Vessel which contains it, can hardly be touched without Uneasiness, through the excessive Cold.

E X P E R I M E N T X I V .

Monf. *Geoffroy*, a celebrated Member of the Academy of Sciences, looks on it as a fingular Power of common Salt Ammoniac, that being mixed with a certain Quantity of Water, it rendered the Water fo cold, that it made the Spirit of his Thermometer, eighteen Inches high, fall thirty-three Lines. But I have fhewn to feveral Perfons, that the *Vefuvian* Salt makes the Liquor of a Thermometer, like his, fall four Inches and an half; which is equal to fifty-four Lines. Wherefore the Efficacy of this Salt, in caufing the Fall of the Liquor, exceeds the Efficacy of common Salt Ammoniac by twenty-one Lines.

E X P E R I M E N T X V .

If round a Veffel full of Water cooled with Snow, there be put fome of the Salt of *Vefuvius*, the Water freezes and grows hard in a very little time.

E X P E R I M E N T X V I .

If you put a good Quantity of the Salt of *Vefuvius* into Snow fet round a [Glaſs] Veffel full of Water, and then ſtir the Veffel, the Water contained therein becomes unfit to drink; having acquired a very disagreeable acrid ſulphureous Taſte; a manifeſt Sign, that the Salt is divided into ſmall Particles, which paſſing through the inſenſible Pores of the Glaſs, enter into and mix with the Water.

E X P E R I M E N T X V I I .

Of all kinds of Salts, this diſſolves in the greateſt Quantity in Water; and perhaps the greater or leſſer
Diſ-

Diffolubility of a Salt in Water, will be [found] proportional to its greater or lesser Effect in cooling Water.

EXPERIMENT XVIII.

Being put into Brandy, or Oil, besides that very little of it is dissolved, it occasions no Descent of the Liquor in the Thermometer.

EXPERIMENT XIX.

Being mixed with Blood lately drawn from the Vein of a Man, but coagulated after settling, the Blood was thereby dissolved, and continued in that State for the Space of twenty-four Hours.

EXPERIMENT XX.

A Solution [of this Salt] being injected into the Vein of a Dog, first occasioned Tremors, then universal Convulsions, and lastly Death: And four Hours afterwards, having opened the Dog, the Blood, which should have been coagulated, was found fluid, both in the Trunks of the Veins, and at the Ends of the Arteries.

EXPERIMENT XXI.

It has all the Properties of Salt Ammoniac to that Degree, that upon substituting this *Vesuvian* Salt, instead of common Salt Ammoniac, the strongest sort of *Aqua Regia* may be had for dissolving Gold: Which Experiment was made with Success by *Mons. Lemery*, in the Academy of *France*.

E X P E R I M E N T XXII.

If a Lump of the Mineral Matter be reduced to a fine Powder, and attentively viewed through a Microscope, it appears very like the Sand of *Ischia*, and is very proper for Writing-Sand: Whence I conjecture, that that Sand is nothing else, but the [same] Matter for a long time comminuted by the Action of the Sea.

E X P E R I M E N T XXIII.

In some of the Stones there appear some few Veins of Gold, in others of Silver, but insensible; and in others, which are very heavy, there is some Antimony.

E X P E R I M E N T XXIV.

A great Dispute arose in the Academy on the Rise of the [*Mofete*] Damps; for what Reason these should be seen only in the old *Strata* of the mineral Substances, and not in the new, where by the Action of the Fire they ought to issue: Which *Phenomenon*, if I am not mistaken, may be accounted for in this manner: As the cooling of the burning Matter began at the Surface, we may think, that the more subtle heterogeneous Particles, upon the closing of the Pores at the Surface, remained in Quantities buried in the lower Parts of the Matter; which, in Process of Time, becoming acutangular and of deleterious Figures, yet cannot offend while imprisoned: But in new Eruptions, wherein the Shocks given to the Matter produce many Fissures, the Damps, meeting with less Resistance there, issue forth: As when the Air is a
long

long time pent up in some Hollow, upon giving it Vent, it generally comes out in a pernicious Vapour.

EXPERIMENT XXV.

It was observed, that the greatest Shocks happened to such things as stood exposed to the *Volcano*; but that those things which were not thus exposed to it, received but faint Shocks: A manifest Sign, that the Vibration of the Air had a great Share in the Shocks of the Earth: Which Circumstance is taken Notice of by *Borelli* with respect to Mount *Ætna*.

II. *An Abstract of a Letter from an English Gentleman at Naples to his Friend in London, containing an Account of the Eruption of Mount Vesuvius, May 18. and the following Days, 1737. N. S.*

SIR, Dated Naples, Aug. 30. 1737. N. S.

YOU have laid a very hard Task upon me, to send you an Account of the late Eruption: **

I was lodged for some time at *Chaja*, and afterwards at *Fontina Medina*, in the Face of this surprising Neighbour [Mount *Vesuvius*], which from thence doth not appear to be above two or three Miles distant.

It gave us Strangers constant Entertainment, by shewing us what it could do, as well as great Satisfaction to the People of *Naples*, who, whilst it continues burning more or less without ceasing, are under

no Apprehension (and I believe with good Reason) of an Earthquake: But we little thought of being invited to a Sight, *quod nunquam vidimus, & nunquam videbimus*; for by all the Accounts of the Living, there has not been any Eruption in their Remembrance near so violent, nor so furious; and Authors mention none to this Degree later than above One hundred Years ago. On *Friday, May 17. 1737. N. St.* I observed, as far as I could see round, that the Mountain was covered with white Ashes a great Way down, as it hath been with Snow in the Winter, which I could not find any body here or at *Barra near Portiche*, take any Notice of; though I should be apt to think for the future, that it might be a Fore-runner; for I had never seen any thing like it. *Pliny* observes in these Words, *Præcesserat per multos dies terræmotus minus formidolosus, qui Campaniæ non solum castella, verum etiam oppida vexare solitus.* (*Plin. Lib. 6. Ep. 20.*) Other Authors say the contrary; though it may very likely be so, round and near the Foot of the Mountain; but this time I have not found any body sensible of it here; but it is certainly true, that our Windows and Doors shook all the time of the Violence of the Eruption, which I take to be from the very great Concussion of the Air upon the violent Explosions: A Door which had a Latch, to my great Surprise, opened often of itself. I cannot conceive a tremulous Motion of the Earth from the Mountain hither, unless it were thoroughly cavernous from thence, which the People here deny, and particularly the Author *Paragallo*.

On *Saturday Night, (May 18.)* this great *Phænomon* began, and increased so much on *Sunday*,

K k

that

that it brought half the People out to gaze at it, with great Variety (no doubt) of Passions and Ratiocinations. There were certainly, amongst some, great Apprehensions, by their being employed in Processions, visiting their Churches, and exposing their Images of the Virgin *Mary*; but I looked upon them as very ungrateful to their great Patron [St. *Januarius*], in having any Dread, when they even boast, that he has never failed delivering them from their greatest Distresses; but by the terrible Havock I have observed in their Country, as well as what has been made by this last Eruption, I find he hath always left them in the Lurch: However, as I had not lost a Grain of the Faith I ever had in that Saint,

I very boldly set out on *Monday* about two Hours before Sun-set. It was a melancholy Sight, to see the Road full of Numbers of poor Wretches, flying as from *Sodom*. I stopped on the Way, to observe the vast Clouds of Smoak, which was thrown up in a prodigious Column, to an Height not to be guessed at, which, by its gentle Waving and Undulation, was a most beautiful Sight; and when it had mounted so high, that it had lost the Force of the Protrusion, it was carried by the Wind a vast Way; but not too far for one to observe how its Rolls began to break, and, being dispersed and expanded, covered the Country underneath with Ashes and Darknes. There were many great Flashes of Lightning darted through this Pillar of Smoak, and frequent Discharges as of Cannon or Bombs, which were followed by falling Stars, such as we see from well-made Rockets. We turned off out of *Portiche*, to gain the North-
side

side of the Mountain, as far as we could, in Chaises, till we were forced to get upon Asses or Mules.

It was now growing dark, and the Fire began to be visible, which it was not in the Day-time, the Sun bearing no Rival. The prodigious *Bouillon* of Fire, and the extreme Force it was expelled with, as well as the vast Height it was carried up to, are not to be described or guessed at. If I should imagine an hundred *Stentors* or *Polyphemus's*, with as many of *Phalaris's* Bulls roaring all together, they could not bellow more terribly. But to have a truer Idea of this Scene, you must look into *Burnet's* most beautiful Painting of the general Conflagration. As we looked round this Northern Side, the whole Country appeared as if over-run by *Samson's* Foxes.

In a little time, by the Light of the Mountain, (though that was much obscured by the Clouds and Pillar of Smoak) and the Help of our Torches, we scrambled over very rough Roads, till we got within about a quarter of a Mile of the great *Lava* or Current; But then I ordered an Halt; for indeed the Scene on all Sides became so stupendous and terrible, that I thought I should make a very foolish Figure, if any Misfortune should happen to us.

We returned to *Portiche*, where we supped, and got home, much fatigued, by Two in the Morning. The Fury of this Eruption was at its Height this Night, as to burning; but the next Day (*Tuesday*) the Columns and *Bouillons* of Smoak were as great, and thrown out with as much Violence, which, as the Wind sat, carried its Destruction, not of the large massy metallic Bodies, but of infinite Quantities of Ashes and Cinders, all that Day, and Part of the Night.

Through the Columns of Smoak was a continued Lightning, the most beautiful Sight imaginable.

The following Day (*Wednesday*) we set out again to view the West-side of the Mountain at *Torre del Grejo*, Eight Miles from hence; where we heard, that the great *Lava* had stopped at the Church of the *Carmelites*, but not without carrying Part of it away: Yet the People were superstitious enough to think this Stop miraculous; though it made a great Breach on one Side, broke down and quite demolished their Sacrify, besides cracking the Roof. This *Lava* had from the Declivity taken the Water-course, which was the Preservation of the Country from being drowned (and the People had best look to themselves, unless they make another). This Hollow, which was for some Miles between Thirty and Forty Feet deep, and as many wide, was not only filled up, but the Matter rose as many Feet above the Surface of the Land about it: We walked to view it on one Side, but the Heat was so intense, and the sulphureous Stench so suffocating, that we were obliged to keep at a good Distance; and I was well informed by several, that it continued very hot a Month or five Weeks after; so long in cooling is that great Quantity of bituminous and metallic Matter, with which this *Vomes* is loaded.

As the Fury of the Expulsion and Explosion was much abated on *Tuesday* Morning, the Stop here was about Four o'Clock that Day in the Afternoon; which might be the more easily conceived, when no more of this vast metallic Matter was discharged, and the Motion of all the rest was relented, for want of more Protrusion, and the Bitumen growing a little cooler.

cooler. As this Stop was made at the Church, Part of the *Lava* took a Turn into the great large Road to *Salerno*, to a great Height; which Part is choaked up for ever, the Expence being immense to remove it. *N. B. Giulio Cesare Reputio*, one of the Authors who describes the Eruption in 1631. says, one of the massy metallic Bodies was in his Time weighed, and the Weight amounted to Five hundred *Cantaras*; a *Cantara* being nearly Two hundred Weight. They have since made the Road passable, by laying Earth upon the *Lava*, and so have added to the Hills of their Country. There are some who pretend to say, that the Matter discharged this time in the different Currents or *Lava's* round about, would make a Mountain as big as their *Sire*. The *Carmelites* here soon fled, and were not come back ten Days afterwards, when we returned that Way, to visit the South-east Side, to view the great Devastation which was made about *Ottajano*, eighteen Miles from hence; for though the great Discharge of the metallic Body ceased on *Tuesday*, (*N. B.* they did not let their Patron budge till the next Morning) a vast Destruction of the Country followed for a long time after; for as the Force of the Explosion was very great, it continued to throw out vast Showers of Cinders and Ashes. The Lands indeed, where the *Lava's* fall, are annihilated to the Owners; but the other Materials destroy all the Fruit and Produce of the Earth where they fall, which doth not recover for a long time; and in this unhappy District, his Majesty hath, with great Goodness, taken off all Taxes for Ten Years.

As we turned on the Left from *Torre del Grejo* towards *Ottajano*, we passed all the Way through their *Masserias* [Farms]; and the Mountain, having the Weather-gage of us for three or four Miles, rained Ashes plentifully upon us, and we lost our Smell of every thing but Brimstone. All the Trees, Vines; and Hedges, bent under the Weight of these Ashes, several Arms, and even Bodies of Trees, were broken with the Weight; so that in some narrow Roads we had Difficulty to pass. Within a Mile or two of the Prince of *Ottajano's* Palace (a very honest worthy Gentleman, who has suffered a Loss of above 100,000 Ducats, or 50,000 *l.* some say more) one can scarce frame to one's self a Sight of greater Desolation; Ten successive Northern Winters could not have left it in a worse Condition: Not a Leaf on a Tree, Vine, or Hedge, to be seen all the Way we went, and some Miles farther, as we were informed: Here, and at the Town, they had a new Earth, about two Feet deep, some said more, by the Account of the miserable Inhabitants, who were a dismal Spectacle, though they had recovered their Fright, and seemed to be got into a new Heaven. The Storm fell so thick and heavy for that time, that they almost all fled, and many Houses were beaten down. In one Convent, two or three Nuns were buried in the Ruins. At *Somma*, on the North-east Side, it has made great Havock; a Monastery of Nuns was destroyed. After a long Day's Work, we returned at Six o'Clock.

Thus, Sir, I have given you our Journal, and an Account of what I observed of this *Monstrum horrendum ingens!* I fear you expect some Reflections upon the
the

the *Phænomena*: A few Thoughts, though very common ones, I offer for your Correction.

The Earth of this Country is, no doubt, greatly compounded of Sulphur and Nitre, from whence Dr. *Burnet* hath fixed it for the Beginning of the general Conflagration; though he has, out of a particular Spite to the People of *Rome*, laid the Commencement of it there. The great Quantities of Sulphur and Nitre are, to be sure, the Operators of these great Explosions, Lightnings, Bombs, Bellowings, and Expulsions of all this Matter; and Nature can certainly make much stronger and more elastic Gunpowder, than Mankind; else those great massy Bodies of Metals could not be thrown up with that vast Force, to that great Height. The Bodies are compounded of various Metals, and, as it were, incorporated with the Bitumen: They pretend to find some Silver, but I question whether the Gains will pay the Costs. They have spoken of the *Lava's*, as if their Motion was quick; but I observed otherwise, that it is slow, and the Progression rather like a vermicular one: And besides trusting to my Sight, I am rather apt to think it must be so, because, though in a great Declivity, these great Masses must be much retarded in their Motion, by their large unequal Points or Angles; besides, the Glewyness of the Bitumen as it cooled, would very much impede a quick Motion; which Bitumen is that Matter that flames, smokes, and is so very suffocating.

By some of the Antients, these burning Mountains have been looked upon as Divinities, and that they lived there: To confirm which, there was a Marble found at *Capua* (as *Paragallo* affirms) with this In-
 scription,

scription, *Jovi Vesuvio sacrum D. D.* The Greeks made use of them in their Mythology, by clapping the Rebel Giants under them.

I must not conclude without saying something of the *Mofete*, upon which much hath been written, particularly by *Leonardo de Capua*; but all might be reduced into a narrow Compass. I mention it now, because it hath given great Terror to the Neighbourhood of this Mountain; Four or Five Persons near *Torre del Grejo*, *Portiche*, and some other Place, having been killed by going into their Caves or Cellars: And it is particularly remarked to have been thus destructive all round the Hill, after the great Eruptions; upon which the great Agitation and Rarefaction of this inflammable Earth, composed of such active Particles, even *Sal Ammoniac*, must send out vast and strong *Effluvia*, (or what in such close Places may properly be termed Exsudations) pernicious, no doubt, when confined under Ground, and hindered from expanding and mixing with fresh Air: And, no doubt, all round the Mountain they abound; but the open Air is a Specific against their ill Effects; as we see it is an immediate Cure to the poor Dog at the *Grotto del Cane*, and not any particular Quality of the neighbouring Lake, which Throwing him into, I should rather think, would kill him, till he had recovered his Respiration and Spirits. *Virgil, Æn.* VII. 84. mentions these Damps:

— *Nemorum quæ maxima sacro*

Fonte sonat, sævamque exhalat opaca mephitim.

And to these Stenches *Persius*, Satire III. l. 99. resembles the Scent of a stinking Breath:

Gutturè sulphureas lente exhalante mephitès.

The

The following Authors have given very ample Accounts of the Eruption of Mount *Vesuvius*, on Dec. 16. 1631.

Giulio Cesare Braccini; Dell' Incendio fattosi nel *Vesuvio*, a 16 Dec. 1631. *Neapoli*, 1632. 4^{to}.

Don *Juan de Quinones*; El Monte *Vesuvio*, *Madrid*. 1632. 4^{to}.

Julius Caesar Recupitus; de Incendio *Vesuviano* Nuncius, *Neapol.* 1632-3. 8^{vo}.

Joh. Bapt. Musculus; de Incendio *Vesuvii*. *Neapol.* 1633. 4^{to}.

Gaspar Paragallo; Ragionamento de' *Tremuoti*. *Napoli*, 1689. 4^{to}.

III. *De Atmosphæra Lunari, Dissertatio Astronomica.* Auctore D^{no} Johanne Paulo Grandjean de Fouchy, in suprema Gallix Rationum Curia Senatore, & Reg. Scient. Acad. Paris. Socio.

CELEBERRIMA est apud astronomiæ physicæ cultores de atmosphære lunaris existentia quæstio. Multi, iique præstantissimi viri, ipsam diversis rationibus physicis solvendam, sed vanis hucusque conatibus, susceperunt. Tot igitur & tantorum artificum irritis laboribus edoctus, aliam mihi viam tenendam statui; ipsiusque atmosphære lunaris existentiam ex meris observationibus inquirendam judicavi, non quidem ex ipsius atmosphære corporis directo intuitu, quippe quod oculorum aciem facillime effugere posset, sed ex phænomenis ipsius existentiam necessario comitantibus, puta refractione radiorum ipsam, luna pleno

orbe fulgente, & in eclipsibus solis & fixarum a luna, pertranseuntium; idque eo libentius, quod ista radiorum refraçtio minime ab atmospheræ altitudine pendeat, quin e contra ipsi quodammodo reciproçetur; eadem enim atmospheræ virtute refringente supposita, eo major erit refraçtio, quo minor atmospheræ altitudo, radio luminis in superficiem ipsius obliquius incidente.

Atmospheræ nomine intelligitur quædam materiæ diaphanæ congeries planetam involvens, quæ radios luminis ipsam pertranseuntés a recta linea deflectere potest; sive hæc materia in aëre nostro simili, sive separatim ab ipso existat, quicquid sit, hic de sola materiæ refringente agitur, idque tantum in hujus operis decursu probandum suscipio, nullam circa lunam dari materiæ, quæ radios luminis a recto tramite sensibiliter deflectere valeat. Hoc unum lectorem admonitum volo, me hic atmospheram ut fluidum homogœneum concipere superficie spherica obductum, ejusdemque ubique densitatis, quæ decrecentium densitatum in reali atmospheræ existentium summæ æqualis sit, ommissa expresse partium ejus densitatis differentia, quæ nullatenus nostras demonstrationes turbare potest. His itaque præmissis, ad rem ipsam properandum est: Et primo, quid variationis in diametris lunaribus ipsius, si existat, atmospheræ afferre possit, discutiendum.

TAB. I. FIG. 1. Si luna atmospheræ cingitur, major observari debet ipsius diameter, quam in planeta nudo observaretur: ut autem ejus incrementi quantitas innotescat, sit *AIB* ipsius lunæ corpus, *GEF* ipsius atmospheræ, erit angulus *AHL* ipsissima lunæ diameter; & angulus *EHL* axe *LH*, & radio *AEH*, in *E* refracto comprehensus, erit diameter lunæ observata.

fervata. Angulus igitur EHA erit incrementum diametri lunæ ob ipsius atmosphæram: Sed angulus EHA opponitur lateri EA trianguli EHA ; & angulus AEH supplementum ad 180° refractionis horizontalis in atmosphæra lunari opponitur lateri AH distantiæ lunæ a terra. Porro latus EA medietas est chordæ atmosphære lunaris ipsius lunæ corpus in A tangentis. Sinus ergo incrementi EAH diametri lunæ ob ipsius atmosphæram erit ad sinum supplementi refractionis horizontalis AEH , ut medietas AE chordæ atmosphære corpus Lunæ tangentis ad distantiam AH lunæ a terra.

Hinc sequitur plane insensibile evadere illud diametri lunaris incrementum: Si enim ad $2''\frac{1}{2}$ assurgeret, supposita refractione horizontali $5'$, id est, triginta saltem vicibus majori, quam supponi potest, ut ex infra dicendis patebit; semichorda EA 276 leucas Gallicas æquaret, atmosphæreque terrestris chordam similem longe superaret. Luna igitur atmosphæra cingatur, necne, eadem semper observabitur ejus diameter; nulloque modo diametri lunaris observatio solvendæ quæstioni par esse potest.

FIG. 2. Majorem decidendi dubii ansam præbent eclipses solis a luna: Radii enim extremi conum umbræ lunaris terminantes, utpote qui corpus lunæ stringunt, & ejus atmosphæram pertranscunt, necessario versus conum axem inflectentur; ideoque brevior & obtusior conus evadet: ut autem illius variationis quantitas innotescat, notandum est radium FA , seu ipsi parallelum EG , qui, si nulla existeret atmosphæra, umbræ lunaris FAC terminus esset, in ipso atmosphære ingressu G , & in egressu H , versus axem CA , refringi: unde semiangulus conum umbræ lunaris

quantitate duplæ refractionis horizontalis in atmosphæra lunari augebitur.

Hinc sequitur, posita atmosphæra lunari, totalem solis eclipsin serius incipere, & citius absolvi, quam illa sublata; quin etiam in certis casibus nullam fore eclipsin totalem; quam tamen diametri lunares & solares in eodem anomalix gradu observatæ poscerent: in his enim casibus conus umbræ lunaris ob atmosphæram contrahitur, & ita contrahi posset, ut ne quidem discum telluris mucrone tangeret.

FIG. 3. Eodem plane modo minuerentur eclipsium partialium duratio & quantitas: eclipsis enim partialis initium observatur, cum penumbræ conus *GDI* habitationem observatoris supergreditur: supposita autem duplici in lunæ atmosphæra refractione *FCE*, *EVH*, semiangulus conus penumbræ minuitur, semidiameterque basis *GI* in *IH* contrahitur: ut igitur in loco dato initium eclipseos observetur, centro *I* basis penumbræ spatium æquale *GH* percurrendum erit: idem de emersione dicendum. Partialis ergo eclipsis serius incipiet, & citius absolvetur, supposita atmosphæra lunari, quam luna nuda; quin etiam & minor observabitur: habitatio enim *T*, in penumbram quantitate *TN* immersa, posita atmosphæra lunari, ipsam distantia tantum *TK* ingreditur. Fieri etiam poterit, ut nulla eo loci observetur eclipsis, ubi nulla supposita circa lunam atmosphæra observaretur: disco enim penumbræ imminuto, locus *R*, qui luna nuda in eam immergeretur, quantitate *RN* immunis ab ipsa pertransibit. Qui autem in spatio *TH* radium inter directum *XY* atmosphæram radentem, & radium refractum *EH* penumbram terminantem, comprehenso degens

dēgens solem ab ipso quidem lunæ corpore expertem, sed tamen ab ejus atmosphæra obscuratum, videbunt; ideoque penumbra quædam pallida, quæ quidem ex prædemonstratis, ubi de altitudine chordæ atmosphære lunaris, & diametri ejus incremento, tertiam aut quartam ad minimum diametri lunaris partem lata observabitur, discum lunæ prægredi & subsequi debet: quin etiã hæc obscuratio absque omni eclipsi observari poterit, quotiescumque circa terminos eclipticos luminarium synodus celebrabitur.

Hæc quidem præcipua phænomena in solaribus eclipsibus observari debent, si circa lunam existat atmosphæra: quid autem revera observetur, nunc videndum est.

Primo, Cum axis umbræ lunaris ad 55 semidiametros terrestres, cum maximus, & $52\frac{1}{2}$, cum minimus est, extendatur, & præterea minima lunæ a terra distantia sit 54 semidiametros terrestres, si atmosphæra lunaris refractionis horizontalis $8''$ capax esset, semiangulus conii umbrosi quantitate dupla, id est, $16''$, ex prædemonstratis augebitur; ideoque $16' 41''$, cum apertissimus, & $16' 5''$, cum angustissimus, æquabit. Porro minimo semiangulo conii supposito æquali $16' 5''$; minor erit ejus axis minima lunæ a terra distantia 54 semidiametrorum terrestrium, proindeque mucro umbræ lunaris nunquam ad terram usque pertinet. Si igitur circa lunam existat atmosphæra, in qua refractionis horizontalis sit $8''$, nulla dabitur solis eclipsi totalis in terra. Nulla ergo existit circa lunam atmosphæra; aut, si existat, refractionem horizontalem $8''$ minorem producit.

Quin & solares defectus totales cum duratione totalis obscurationis observantur. In eclipsi, v. g. anni

1724. duratio obscurationis totalis ad $2' 16''$ assurgebat. Luna tunc temporis motu horario $1' 15''$ percurrerat, umbraque ejus sibi semper parallela in disco terræ gradibus spatium 54 vicibus majus, id est, æquale $1^{\circ} 7' 30''$, perambulabat: unde, si auferatur motus diurnus habitationis æqualis $20'$, quippe qui durationem eclipsos prolongare potest, diameter umbræ habebitur æqualis $47' 30''$, seu 45173 hexapedis, seu tandem 22 leucis Parisiensibus. Unde instituto calculo eruitur axis conii umbræ lunaris uno saltem terræ diametro major distantia lunæ a terra, quæ tunc minima erat, luna circa perigæum versante. Porro ex datis luminarium diametris in eodem anomalix gradu observatis axis conii umbræ lunaris elicitor 55 semidiametros, ad minimum, æquale: unde sequitur maculam umbræ lunaris in disco terræ, & axem conii, eisdem præcise reperiri, quos distantix lunæ & luminarium diametri observatæ postulare videntur. Nulla igitur circa lunam existit atmosphæra, aut nullam, si existat, refractionem sensibilem producere valet. Sed ut nullus dubitationi locus relinquatur, eorum reddenda est ratio phænomenon, quæ in eclipsibus solaribus observata atmosphære lunari excogitandæ locum dedere.

Primo quidem, exigua illa, quæ in defectibus totalibus observatur, lucula nullam in fluido lunam ambiente refractionem arguit: experimentis enim D. *Maraldy* a meipso summa cura, & eodem successu, repetitis, constat corporum nulla certe atmosphæra vestitorum, si soli exponantur, umbram circa conii axem clarescere; idque eo magis, quo longius ab ipso corpore receditur. Porro habitatio observatoris in defectu totali circa axem conii umbræ lunaris, & in
ejus

ejus mucronis vicinia, versatur. Mirum igitur esse non debet medium umbræ luce quadam maligna suffundi, quæ aliunde augeri potest radiis ab aëre illuminato umbram ambiente versus umbræ medium reflexis.

Secundo, annulus lucidus lunam in defectibus totalibus ambiens atmosphæræ Lunaris existentiam nullo modo probat, cum cuius solem pila lignea, vel alicujus materiæ opacæ sibi occultanti appareat. Unde atmosphæræ solari, non lunari, tribuendus est; ut abunde probavit Cl. D. *De Mairan* in tract. de Aurora Boreali, Sect. I. Cap. I. pag. 14.

Tertio, imminutio diametri lunaris, quæ in eclipsibus solaribus 30'' circiter minor observatur quam luna in eodem anomalix gradu pleno orbe fulgenti; hæc, inquam, imminutio atmosphæram lunarem minime probat, etiamsi observentur in disci lunæ circumferentia quædam montium inæqualitates, quæ in luna plena penitus evanescent: objecta enim lucida fibras oculi tam valide concutiunt, ut motus ipsarum fibrarum vicinis communicetur, & ita corporis lucidi imago ultra debitam quantitatem augetur, quod experientia pervulgata notum fiet: si enim palus seu fustis lunam inter & oculum subjiciatur, diameter pali e regione lunæ imminuta videbitur; quod si tunc temporis nubes aliqua planetam subeat, minor videtur pali deminutio; nulla est, si planetam nubes ab oculis furripiat; ac tandem pro varia lucis lunaris intensitate varia observatur.

Quod ad montium inæqualitates attinet, eadem ratione in luna potius sitiente, quam in luna plena, observari debent: montes enim lunares per se obscuro, in solis fulgentissimo orbe conspecti, longe minus

minus oculorum aciem fugiunt, quam ubi luna plena clarescentes vicino lunæ splendore extinguuntur; cum præsertim ea sit lucis lunaris intensitas, ut in ejus vicinia ægre admodum stella tertiæ magnitudinis discerni possit. Sed ut nodus hac in re penitus præscindatur, si lunæ soli oppositæ limbus ejus atmosphære non ipsissimi corporis planetæ terminus esset, telescopiis longioribus, arctioribusque aperturis objectivis, montes in lunari peripheria numquam observarentur: porro tubo optico 36 ped. Paris. & apertura objectiva unius pollicis, multoties plurimas montium inæqualitates in lunæ plenæ disco observavi; unde sequitur lunæ plenæ discum ejus corporis, non atmosphære peripheria terminari.

Quarto, nunc demum de mira illa Anni 1715 observatione fulgurum lunarium *Londini* facta * a Dno. *Delouville*, præsentibus multis REGIÆ SOCIETATIS astronomis, paucis est differendum; paucis quidem; quid enim de re ante & post hanc eclipsin inobservata dicendum? Si tamen aliquid in medium de tam insolito phænomeno proferræ liceat, supponemus limbum lunæ visibilem ex insitis montium cacuminibus componi; quæ quidem in eclipsi totali solem observatori eodem modo occultant, quo sylvarum majorum arbores visui officiant. Unde si in lunæ superficie quædam montium series liberum in rectam lineam transitum radiis solaribus præbuerint, fulgurum quorundam speciem mentiri debuerunt, eodem modo ac si in camera obscurata radius solis ope speculi subito

* Observatio hæc facta fuit revera a Cl. D. *Edmundo Halleio*, præfente quidem D. *Delouville*, uti videre est in *Transact.* N^o 343, p. 249.

intromittatur, & subtrahatur objectorum externorum pictura in foco lentis exarata, tractibus luminis fulguri simillimis illustrabitur: quod quidem eo libentius admittendum censeo, quod ista subitanea fulgura in limbi lunæ vicinia semper observata fuerint; ut ex ipso hujus eclipseos schemate * ab illustrissima D. *Sara Sloane* exarato patet.

Quod ad pallidam illam fasciam limbum lunæ in hac eclipsi comitantem, cum in eclipsibus solaribus hucusque observatis nil simile nec mihi nec cuiquam astronomo apparuerit; quod tamen in hypothese atmosphæræ lunaris semper & ubique observari debet; nullam ejus mentionem hic faciemus.

Ex his omnibus manifestum est nihil in eclipsibus solis observari atmosphæræ lunari consonum. Nunc de fixarum & planetarum eclipsibus a luna differendum.

FIG. 4. Si luna atmosphæra cingitur, planetæ & stellæ fixæ serius pone lunam occultari, & citius ab ejus disco egredi, observatori in terræ superficie constituto videbuntur, quam luna absque atmosphæra supposita; quin & in quibusdam locis, in quibus videri debuisset planetæ vel fixæ a luna eclipsis, nulla observabitur: quod ut manifestum fiat, sit corpus lunæ ABC , & stella in S distantia quasi infinita posita; radii paralleli LV , MX , lunare corpus undique contingentes, superficiem cylindricam constituunt, cujus quidem cylindri basis VZX in disco telluris cunctas habitationes, in quibus stella seu planeta a luna tegitur, suo ambitu comprehendit. Observator itaque in V initium eclipseos, & finem in X , conspiciet, durationem-

* In Museo celeberrimi patris ejus Cl. Dn. *Hans Sloane*, Bart. R. S. Pr.

que metietur temporis, per quod luna diametrum suam, seu potius spatium huic æquale percurrere poterit. Si autem lunæ atmosphæra supponatur radius IW , axi cylindri parallelus non remanebit, & cylindrus ipse conus evadet, cujus sectio YTU habitationes, quibus debebitur eclipsis, designabit. Porro angustata basi YTU , punctum Y serius habitationem supergreditur, quam punctum V ; limesque U citius ipsam deseret quam X : serius ergo incipiet stellæ vel planetæ a luna eclipsis, & citius absolvetur, atmosphæra circa lunam supposita, quam luna nuda: quin & nulla observabitur eclipsis eo loci, ubi absque atmosphæra observari debuisset; locus enim C , circumferentia VZX prioris cylindri involutus, a sectione conici YTU immunis erit. Porro supposita refractione horizontali in lunæ atmosphæra æquali $8''$, VI , 1384 hexapedas, id est, leucæ Parisiensis dodrantem æquabit: unde sequitur nullam in locis calculo indicatis eclipsin observari debuisse, quotiescumque in cylindricam aream non magis leucæ dodrante immerguntur.

Aliud etiam phænomenon ex atmosphæaræ lunaris suppositione oritur: in cylindri parte TR stella quidem semper videbitur, sed interposita atmosphæra lunari: ideoque motum & colorem a genuino diversum induet; idque in omnibus omnino eclipsibus, sive stella sit maxima sive minima.

Porro, eclipsium fixarum & planetarum a luna duratio nullo modo imminuta videtur, sed semper diametro lunæ, & motui ejus, præcise consona observatur. Quod ad illas attinet observationes, in quibus stella post contactum, in disco lunæ paululum progredi ante occultationem videtur, ipsarum causam in
auctam

auctam lunæ & stellæ diametrum penitus refundemus: si enim hujus apparentiæ causa esset atmosphæra lunaris, semper in omnibus stellis, & cum quibuslibet objectivorum aperturis, eadem observaretur. Porro nullam hæcenus stellam in disco lunæ progredientem observavi, nisi primæ, aut ad minimum secundæ magnitudinis, idque dimidia ad maximum sui parte; & diameter vera fixarum, ut cuilibet observanti satis constat, insensibilis evadit, nec nisi a radiis spurii augetur: unde radii adventitii tam stellæ quam lunæ in fundo oculi miscentur ante veram corporum stellæ & lunæ copulam: aliunde, si limbus lunæ visibilis atmosphære non ipsius corporis terminus esset, majoribus tubis, & angustioribus aperturis objectivis, nulli in peripheria montes observarentur; qui tamen, ut supra dictum est, satis clare conspiciuntur.

Ex his igitur omnibus manifestum erit lunam nulla atmosphæra refringente vestiri, cujus refractioni pateat: fieri enim posset, ut circa lunam existeret atmosphæra, in qua refractionis horizontalis ad 1" vel 2" assurgeret: huic enim opinioni suffragari videntur majores in luna maculæ, quæ nullo modo sylvæ dici possunt, ut Clar. *Hartsoëker* & aliquibus aliis visum fuerat. Umbræ enim marginum semper limbo lunæ claro viciniore observantur; unde recte concluditur eas cavitates esse non sylvas, quæ ex altero latere umbram projicerent. Porro supponi potest in ipsis fluidum quoddam, quo quidem casu halitus ex ipsis a sole extolli valde physicæ consonum esset; quorum congeries speciem quamdam atmosphære circa lunam mentiretur; quæ quidem atmosphæra nec densa admodum, quippe quæ ex Cl. *Newtoni* demonstratis vix, cæteris paribus, tertiam halituum

terrestrium densitatis partem adæquaret, nec sibi ipsi diversis temporibus similis observaretur, vaporibus istis omni alio adminiculo destitutis.

Multa sunt & alia ratiocinia physica, quibus nostra de atmosphæra lunari opinio fulciri posset: sed astronomice tantum, non physice, rem discutiendam suscepimus. Lectorem fatis in hujus operis decursu distinuisse mihi visus sum: huic itaque finem impono, illustrissimæ SOCIETATIS in re tam ancipiti judicium propississima rei veritate libentissime amplexurus.

IV. *A Narrative of an extraordinary Sinking down and Sliding away of some Ground at Pardines near Auvergne, sent from M. T— to a Relation in England, translated from the French, and communicated to the ROYAL SOCIETY by Phil. Henry Zollman, Esq; F. R. S.*

THE Parish of *Pardines*, in the District of *Issoire* (in *Auvergne*) is situate about a League from the Town of *Issoire* on the Road to *Clermont*, almost on the Top of a pretty steep Hill.

This Parish consists of two Villages or Hamlets distant from each other about 200 Paces; the one, which is called *Le Fort*, in which is the Parish Church, and Part of the Houses of the Inhabitants, stands upon a Rock; there appear the Remains of an antient Fortification, with which some Houses were surrounded in the time of the Wars.

The

The other Village, which is properly called *Pardines*, was composed of the greater Part of the Houses of the Inhabitants to the Number of 46 Buildings; the Ground whercon this Village was built, as well as that of the whole Hill, is a good and light Earth, mixed with a little white Clay: There are also found in it some Stones and Rocks of a middling Size. This Land was very well cultivated, and very fruitful, consisting of Fields sowed with Corn, of Orchards, and for the greater Part of Vineyards; the whole Ground was overspread with Fruit-trees, particularly Walnut-trees.

This Earth used to dry soon and chap from the Heat; they even observed in it long since Clefts of a considerable Depth, which sometimes growing wider and wider, formed several Gullies.

On the 23d of *June* 1733. about Nine in the Evening, the Inhabitants of the Village of *Pardines* saw the Walls of their Houses shake sensibly; whereupon they all retired out of them, and saw that the Hill visibly melted away, as it were, the greater Part of the Land sliding along towards the Vale; others subsided sensibly; in some Places the Earth, opening itself, formed new Gulls, and those that were observed there before, grew much wider; sometimes the Ground which slid along in great Pieces, stopt and tumbled one Piece over the other; and the Rocks, which broke loose from that rolling Earth, precipitated themselves into the Valley, which at present is quite filled up with them, as well as with the Earth which rolled down, whereby the Road from *Issoire* to *Clermont* is become impassable.

All this was done, not with any impetuous Motion, but very gently, and even sometimes almost imperceptibly; a sensible Motion was observed during the Space of three or four Days at different times; there was even a House which did not fall till the 10th of the present Month of *July*. During all that Time no Noise was heard, any otherwise than what proceeded from the Rocks falling into the Valley, and from some large Clods of Earth, which loosening themselves from the steeper Parts, fell down with Precipitation.

By this Rolling were carried away 26 Buildings, large or small, some of which subsided with the Ground, and, being shaken at their Foundations, tumbled on a Heap; the Remains of some others appear, as yet, on those Pieces of Ground that rolled down into the Valley.

It is computed, that the Lands which slid away, or were lost by being buried under the Rubbish of the others, amount to the Number of 466 *Oeuvres* of Vineyards, 40 *Septerées* of arable Land, and 56 *Oeuvres* of Grass-fields, which all together may make up 150 Acres of *Paris* Measure. It is observable, that in this Number were comprised several Orchards, besides that the whole Ground was covered with Trees, either Walnut-trees on the Hill, or Willows and Poplars in the Valley, of which they reckon 4000 in all.

If one may conjecture what was the Cause of so dismal an Accident, it seems it proceeded from the Situation of the Ground, and the Nature of the Soil. The first Surface of the Hill about four or five Foot deep, was a pretty light Earth, easily dried by the

the Heat of the Sun; under this first Layer there was a *Stratum* of fat Clay, which at present lies open in several Places, and which is very moist, so that one even sees the Water bubbling out of it in some Places.

The great Rains that fell in the Beginning of the Spring, soaked through and diluted this *Stratum* of Clay, which retained and gathered all the Waters of the Hill running between the two Layers; the Heat of the Summer ensued, which dried up the upper Surface, and formed it into a sort of solid Crust, which Crust resting itself upon a fat and moist Clay, and by its steep Situation being inclined to slide towards the Valley, the whole Surface of it loosened itself by great Pieces, and breaking in several Places, slid along towards the Place whither its Declivity would naturally carry it. There are some Parts which moved almost insensibly, and only sunk or subsided, either because the Rolling of the neighbouring Soils made room, that what was under this Surface might slide off, or perhaps because the Parts under this Surface had been hollowed a long while since, by the Waters which passed between this Surface and the *Stratum* of fat Clay. Other Parts, which were much more in Number, rolled all together towards the Valley, and one sees yet whole Pieces of Vineyards, with the Props remaining upright; which may easily be conceived: There are again other Parts, which in tumbling were overturned in different manners.

I am to add here, that this Accident is not without Example in the Province of *Auvergne*; we have not indeed seen so considerable a one till now, yet

it has often happened, that Pieces of Earth of a Quarter or Half an Acre, have separated themselves all in one Piece, from the Top of a Hill, and slided down visibly on the Lands lying below.

How considerable soever this Accident may be in regard to the poor People who suffered by it, yet it was to be wished it was the only one that has befallen this Province. The Overflowings of the River *Allier*, and of the Rivers and Brooks that run into it, and the Hail that fell almost continually since, have intirely ruined above One hundred Parishes, in which they will have no Harvest this Year as for Corn and Hemp, nor any Vintage at all.

V. *A Dissertation on the Worms which destroy the Piles on the Coasts of Holland and Zealand, by Job Baster, M. D. F. R. S. communicated by the President of the ROYAL SOCIETY. Translated* from the Latin by T. S. M. D. F. R. S.*

SECTION I.

IN the Year 1730. the Persons appointed to take care of the Dikes on our Coasts, observed that the Piles made of the hardest Oak, defending the

* This is here inserted in *English*, because a very ample Treatise hath been published in *Latin* on the same Subject, intituled, *Godofredi Sellii, J. U. D. R. S. Lond. S. Hist. Nat. Terebinis seu Xylophagi marini. Tubulo-conchoidis*, Traj. ad Rhen. 1733. 4to.

Coasts of the *Netherlands* against the Sea, were eat through in a few Months, so as to be broken by the least external Force. Surprised at this uncommon and dangerous *Phænomenon*, they inquired into its Cause, and saw that a sort of Worms, before that time very scarce, but now increased to an incredible Number, had in so short a time eat into those Piles between the highest and lowest Water-marks, and threatened very great Damage to the Inhabitants of these Countries.

The superstitious Populace immediately persuaded themselves, that this new Genus of Animals was created by the divine Wrath for punishing the Sins of Mankind: But prying Experience has taught, that those Worms, like other Insects, were created in the Beginning; but now multiplied to an incredible degree from some unknown Cause.

S E C T. II.

If a Pile of the hardest Oak has stood six Months on the Shore, and be taken out in Summer or Autumn, there appears Mud and Filth sticking to its outward Surface; which being scraped off with a Knife, discovers a vast Number of Holes, scarcely as large as Pins Heads.

S E C T. III.

If you view this Mud (SECT. II.) through a Microscope, you will see,

1. A Number of whitish Points, not bigger than Grains of Sand.
2. Some very small Worms.

The whitish Points seem to be the Eggs of this Insect, and the Worms to be such as are already hatched from them; and these Worms gradually perforating the outward Surface of the Wood, rendered soft by lying in the Water, made the aforesaid Holes, (SECT. II.) and through them worked their way into the Substance of the Wood.

SECT. IV.

A small Style of Whalebone or Lead, thrust into these small Holes, runs strait into them for three or four Lines, so that its outer End always makes a right Angle with the Pile: But afterwards, if the Style be gently pushed forward, it does not continue in the strait Line, but runs either way, generally upward.

SECT. V.

But if one of these Piles (SECT. II.) be split lengthwise with a Hatchet or Wedge, it is found full of Passages, or hollow cylindrical Ducts, each of which contains a Worm, surrounded with a thin testaceous Substance, exactly filling the Duct, and forming its *Involucrum* or Sheath, in which Sheath it can move with Freedom. See SECT. XIX.

These Ducts, (SECT. IV.) beginning at the outward Surface by a narrow Hole, grow gradually wider, and run either strait, oblique, upward or downward. But what is most surprising is, that these Ducts never run into one another, nor communicate; but each of them continues separate for every single Worm. Over the Worm's Head there are found two or three Drops of a salt Liquor, thicker than Water, but not

not the least Appearance of the Dust of the corroded Wood.

S E C T. VI.

Whence it appears, that all the Wood, which had before filled up the Place of the Duct, in which the Worm with its Covering is now found, was eaten and consumed by the Worm: And as it seems quite incredible, that an Animal, which appears soft, and almost as fluid as the White of an Egg, should be able to eat through such hard Wood; I offer the Description of this *Xylophagous Worm* to the ROYAL SOCIETY, in order to give them some Knowledge of this Water-Insect, which has done so many Millions Damage to these Countries.

S E C T. VII.

They are found of various Sizes and Thickness. There are some of the younger ones not above an Inch or two in Length; some of a middle Size, such as we have represented in our first and second Figures; see TAB. II. and some thirteen or fourteen Inches long.

S E C T. VIII.

But in order to a more accurate Description, we will divide the Animal into Head, Body and Tail.

The Head is of a most wonderful Structure, being covered with two hard . . . (I know not which to call them, Shells or *Hemicrania*) of a Substance neither testaceous nor ossicous, securing their softer Contents: And being viewed through a Microscope, they appear as in Figure 3. as well as I could have them drawn.

S E C T. IX.

These *Hemicrania* are two white Bodies, much harder than the Substance which forms the testaceous Covering; the inner Surface hollow and smooth; the outer, convex and rough, with three Fibres running different ways; and both together perfectly represent a double Bit, of that kind of Borer, we call an Augar.

S E C T. X.

The upper Part of the external convex Surface (Fig. 3. *A.*) has a very sharp Edge, in which the first Series of Fibres begins from one Point; which Fibres gradually dilating, and running lengthwise, end about the middle Part of it; and this middle Part makes a right Angle with the upper Part. In this Part the Fibres being elevated, run cross-wise (Fig. 3. *B.*). The lower Part is thicker than the upper, but softer and less compact. In this Part the Fibres are raised up and rough, first curve, then strait, and, like the others, run length-wise to the lower Edge of this Part, which is strongly fastened to the Head by various Ligaments (Fig. 3. *C, D.*).

S E C T. XI.

The concave or inner Part of these *Hemicrania*, (S E C T. IX.) which contains the softer Parts of the Head, is very smooth; but almost in the Middle has a very small and tender Eminence or Procefs, (in Shape much like Dr. *Raw's* Procefs in the Organ of Hearing) fixed at one End, and loose at the other, running almost
the

the whole Width, and doubtless destin'd for supporting some of the inward Parts of the Head (*See* Fig. 4. *A.*).

S E C T. XII.

These two *Hemicrania*, connected together by strong Ligaments, and as it were by a small Hinge, (by means whereof they can dilate without separating) besides their defending the soft Head from external Injuries, are the Instruments wherewith the Animal gets its Food. For whatever way it turns its Head, the raised and rough Fibres, running either length-wise or cross-wise, always rub off some of the Wood.

S E C T. XIII.

These *Hemicrania* carefully removed, the contained Parts (Fig. 6) are laid open to View; but they are so soft, and of so wonderful a Structure, that the Eye, though armed with a Microscope, can neither discern their true Make or Use. First, indeed, there appears a Membrane enveloping the whole Head; in the middle and anterior Part, which is not covered by the said *Hemicrania*, it appears as if raised by a *Tubercle*, (Fig. 6. *c.*) and in that Place it is of a red Colour; but the lower ligamentous Edge firmly adheres both to the small Process (S E C T. XI.) and to the lower Edge of the *Hemicranium*.

S E C T. XIV.

This Membrane carefully separated and removed, (Fig. 7. *A. A.*) in the middle of the subjacent Pulp you will find a small Pear-like Body, perfectly pellucid, somewhat protuberant above the other Parts, which

which made the *Tubercle* in the Membrane (SECT. XII.). It is much harder than the other contained Parts of the Head and Body; so that it will bear cutting with the Scalpel. It is of a red Colour, as perfectly pellucid as a Drop of Water; of the Shape of a Pear, from a larger Basis terminating in a Point. I cannot better resemble it to any thing than to the Crystalline Lens of the Eye: Yet in Spirit of Wine it preserved its Transparency, but its Bulk was diminished (Fig. 7. B.).

I cannot guess its Use: It does not seem to me, as it does to some, to be the Organ of Sight; for the Worm seems to have no Occasion for an Eye, as spending its Life in perfect Darkness; besides that the investing Membrane is not transparent, and therefore would obstruct the Sight.

SECT. XV.

At the Sides, where the lower Edges of the *Hemicrania* do not touch one another, there is a sort of Cavity; and in these Sides the harder Fibres may be distinguished, disposed in such a manner, as perfectly to resemble the Gills of Fish; and through them the Worm seems to breathe.

SECT. XVI.

The extreme Softness of the other Parts of the Head prevents our coming at the Knowledge of the Use of the Membranes furnished with *Fibres* of different Tendencies, or inquiring by what Organs the Worm takes the Wood shaved off by the *Hemicrania*, or rough Shells; whether it does this by Suction, or not; by what Muscles, or how acting,
this

this wonderful Head is moved. 'Tis probable, indeed, that its Motion consists in the opening and closing these Shells (SECT. VIII.) that shave off the Wood; and that the inner Parts have a Power to move on all Sides, as the Ball does in the Socket of the Eye; and perhaps to come forth of these Shells, and re-enter after taking their Food. But of these things there can be no Certainty, because the Parts dissolve between the Fingers.

SECT. XVII.

The Body, viewed forward, (Fig. 2.) is of a reddish Colour. In the middle appears a Line, often dark-brown, often blackish, sometimes not visible, sometimes running near half the Length. The rest of the Animal is of a whitish or grey Colour.

1. If you intend to dissect it, and examine the Inside, you must first remove a thin Membrane surrounding the whole Body, which for that Reason may be called the *Cutis* or *Cuticula*. When this is removed, there appears an oblong Vessel placed in the Middle, (Fig. 2.) of a reddish Colour, from the shaved Wood, of which it is full: Hence it seems to be the Stomach, or at least the first Organ of Digestion.

2. In the lower Part you will find another Vessel, appearing like a dark-brown Line, which contains the Excrements, of which it is often found full, and discharges them at the End of the Tail.

3. At the Sides of the reddish Vessel or Stomach (SECT. XVII. 1.) is placed a white, clammy, fat Substance, sticking to the Fingers, and perhaps constituting the Flesh of the Animal.

SECT.

S E C T. XVIII.

Where the Body ends, the Tail begins, thicker than the Body, and rendered stronger by circular *Fibres*. At its End it has two small hard Bodies, containing and defending the tender Extremities of the Tail.

This Tail thicker than the Body terminates in two Ends, the thickest of which certainly serves for the Discharge of the Excrements, the slenderest doubtless for Generation: And this it can stretch out to an incredible Length, so that in Worms that seemed to be in Copulation, it appeared above an Inch out of the Pile.

The two small Bodies, that contain these Ends of the Tail, are of a harder Substance than even the *Hemicrania*. The outer Part is gibbous, the inner hollowed. The lower End is bifid; whence I conjecture, that they serve the Animal for Feet, when it is mounting upright, or corroding the Wood; by leaning on them as on a Prop (Fig. 8.).

S E C T. XIX.

The above-described Worm dwells now very securely in a testaceous Tube of a white Colour, which it exactly fills, yet so as to be able to move with Freedom. That Tube, like the Coverings of Snails, &c. daily grows with the Animal, from the Matter which perspires from its Body; whence it is sometimes found strait, sometimes bent, according to the Course which the Worm steered in corroding.

S E C T.

S E C T. XX.

As to their Generation, it is probable enough, that, analogous to that of other Insects, it is performed by Copulation of Male and Female: For they can so lengthen one End of their Tail, and thrust it out of the Pile, that they may copulate by that means. Then they lay their Eggs in the Water close to the Piles, to which they flick by their clammy viscid Matter, (such, for Example, as Frog's Spawn) and afterwards, by the Heat of the Sun, hatch the Worm, which immediately endeavours to get into the Pile (See SECT. II. and III.).

I could not observe the Difference of Sex, either with my Eye, or a Microscope. Some think them Hermaphrodites, as Snails, and that they copulate in the same Manner: But these Conjectures are not very probable.

S E C T. XXI.

Many Remedies and Secrets for destroying these dangerous Enemies were immediately boasted of, which for the most part were Preparations of *Arsenic* or *Mercury*, and are not worth enumerating: I will only give the Receipt of one, which is the best and surest of all.

Take an Iron Plate of an oblong Figure, and of the Width of the Pile, with a strong Handle at each End. One End of this Plate must be armed with thick Nails half an Inch long, and about an Inch asunder. The Nails of this Plate must be driven into a Pile of any slight Wood, with a Hammer, and then the Plate pulled off by means of its Handles.

.. O O

And

And this is to be so often repeated until the Pile is perforated every-where with small Holes: Then it must be dawbed over with *Varnish* in the hottest Sun (the *Varnish* is imbibed by the soft Wood with so many Holes in it); and while the *Varnish* is yet hot, let it be strewed over with *Brick-dust*: And this is to be repeated three or four times, after the preceding *Varnish* is thorough dry, till the Pile is intirely surrounded with a stony Crust, which will be impenetrable to all Insects, and last many Years.

But the Divine Clemency has already so far destroyed these pernicious Insects, which multiplied so prodigiously for eight or nine Years past, that there is great room for Hope, that our Country will in a short time be intirely freed from them.

An Explanation of T A B. II.

Fig. 1. The Pile-worm of its natural Size, lying on its Belly.

Fig. 2. The same lying on its Back.

See SECTION XVII.

A. The Stomach.

B. The Duct full of Excrements.

C. The Tail, with its Defences *dd*, and its Point *e*, which it can stretch out.

The Six following Figures are represented much larger than Life.

SECTION X.

Fig. 3. *AA*. The first Series of Fibres running strait down.

BB.

BB. The second Series running transversely.

CC. The third taking a different Course.

DD. The lower Edge, which is infixed to the Head.

S E C T. XI.

Fig. 4. The Shell or *Hemicranium* seen on the Inside with the Process running cross it, one End of which *A* is fix'd, the other *a* is moveable.

S E C T. XII.

Fig. 5. *A.B.C.D.* The same as in Fig. 3.

E. The Hinge, whereby these are connected, and may easily dilate or open.

S E C T. XIII.

Fig. 6. *AA.* The Membrane covering the Head freed from the *Hemicrania*, which were attach'd to this Membrane.

B. The Place, where the *Hemicrania* were connected.

c. The middle anterior Part, in which the *Tubercle* was prominent.

S E C T. XIV.

Fig. 7. *AA.* The Membrane of Fig. 6. separated and turned back.

B. The pellucid pyriform Body lying in the middle of the Head, and which formed.

c. The *Tubercle*.

S E C T.

S E C T. XVIII.

Fig. 8. The two Defences of the Tail, of which the exterior Part *A.* is gibbous, the other or interior *B.* is, as it were, hollowed: These Extremities are bifid. *C.* By this Part they are joined to the Tail.

VI. *Two Observations of Explosions in the Air; one heard at Halsted in Essex, by the Rev. Mr. A. Vievar, Minister of that Place; the other by Sam. Shepherd, Esq; of Springfield in the same County.*

ON *Sunday* the 12th of *March* 1731-2. between One and Two o'Clock in the Afternoon, walking in my Garden by the Side of a Canal, I heard as it had been a large Clap of Thunder from the North-East, being a very clear Day, and no Clouds appearing. While I was looking into the Air, the Noise was repeated very loud, but seemed more like the violent Fall of a House, insomuch that I expected every Moment an Out-cry from the Town: But I was soon undeceived, when it began again, and I found it made towards me, with a different Noise from what I had heard, that is, like the Grinding of Flint-stones, but very loud: The Dimensions of it seem'd to be about three Foot wide. I found it sink in the Air, and as it seem'd to point directly at my Head, I laid myself down upon a Grass-slope, to let it pass over me. However, at the upper End
of

of the Walk I found it fell to the Ground, and came rolling down the Grass-walk ; and I can compare it to nothing better than to that of a violent Grinding of Flint-stones, or a Coach and Six upon the full Speed upon a Causeway of loose Stones. I lay attentive, expecting to see something, and saw a Piece of Wood came running before it. When the *Phenomenon* came to the Water-side, it twisted up a large Stake that stood in its way, and tossed it towards me with much Violence, and immediately fell into the Water with the Violence and the Noise of a red-hot Mill-stone. I have seen the Seas break against a Rock in a Storm, but never saw a greater Ferment caused by the boiling of the Waters. It staid about a Quarter of a Minute in the Water, and then mounted again into the Air, and went rattling away, but with much less Violence : I heard it for about a Quarter of a Mile, and lost it.—*N. B.* It came against the Wind, and not faster than a Man may walk. The Froth and Foam upon the Water remained thirty Hours after, when I shewed it to some Friends.

Halsted in Essex,
1731-2.

A. Vievar.

*Part of a Letter to W^m. Woodford, M. D.
Fellow of the College of Physicians, London.*

— **U**PON *Tuesday* the 15th Instant, between Eleven and Twelve, the Sun shining very bright and hot, without the least Cloud, the Wind so calm, that the Water was as smooth as Glass, I

was dressing in my little Room next the Garden, about forty Yards from the Canal, when I heard a very surprizing Noise of Fire, resembling, as I told you at *London*, as if a very large Quantity of Oil had been thrown into a great Bonfire, burning in its greatest Rage. I stepp'd immediately to the Window which was open, where I saw the Middle of the Canal, which this dry Season has sunk about six Inches, in extreme Agitation, as rough as the *Thames* in a Storm, foaming and smoaking, and forced up, to my Appearance, full two Foot above the Surface, but it might be much more, my Window being greatly higher than the Canal; and the Fellow who was at Work, whom I examined again this Morning, protests he saw the Water, like the Spray of the Sea, above the Dwarf-Trees, which must necessarily be five or six Foot. I wish I had seen the Beginning of this uncommon *Phænomenon*, the Duration of which, I think, might be half a Minute, and made the House stink, as if a Gun had been fired in it.

My Canal bears East and West, and the Fellow says he heard it coming from the West, bringing the Leaves of some tall Trees from an adjacent Field in its Passage; but could not discover any material or substantial Body to fall in the Water, where the Hissing, as I observed above, was very loud and violent; neither was there any Lightning or Thunder before or after, but the Day remain'd bright, still, and hot. I forgot to say, the Space of the Canal that was affected by it, might be twelve or fifteen Yards.

Springfield in Essex,
Aug. 22. 1732.

Sam. Shephard.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *January, February, March, April,*
May, and June, 1740.

The CONTENTS.

- I. *A Catalogue of the FIFTY PLANTS from Chelsea Garden, presented to the ROYAL SOCIETY by the Company of Apothecaries, for the Year 1738. pursuant to the Direction of Sir Hans Sloanes, Bart. Med. Reg. & Soc. Reg. Præf. By Isaac Rand, Apothecary, F. R. S. Hort. Chel. Præf. ac Prælec. Botan.*
- II. *Some Reflections on Generation, and on Monsters, with a Description of some particular Monsters: By Daniel de Superville, Privy-Counsellor and chief Physician to his most Serene Highness the Margrave of Brandenburg-Bareith, President of the College of Physicians, Director of the Mines and of all Medicinal Affairs in the Margravite, Member of the Imperial Academy Naturæ Curioforum, and of the Royal Society of Berlin.*
- III. *Offis Bregmatis Gigantæ Magnitudinis Icon, TAB. I. Fig. 2. & TAB. II. Fig. 1. cum Problemate de Gigantis Statura determinanda secundum Regulas Artis Delineatoriæ: quæ ad Illustr. REGALIS SOCIETATIS Præsidentem Dum Hans Sloane, Bart. transmisset Jac. Theodor. Klein Reipubl. Gedan. a Secretis, & Reg. Soc. Lond. Soc.*
- IV. *An Account by the Rev. Zachary Pearce, D. D. F. R. S. of a Book intituled, Reflexions Critiques sur les Histoires des Anciens Peuples, &c. Paris, 1735. 4^o. in 2 Vol.*
- V. *A Query proposed to such curious Persons as use the Greenland Trade, occasioned by the annexed Letter from Mr. David Nicolson, Surgeon, to Dr. Mortimer, Sec. R. S. concerning Scurvy-grafs.*
- VI. *A Letter from Edmund Stone, F. R. S. to ——— concerning two Species of Lines of the Third Order, not mentioned by Sir Isaac Newton, nor Mr. Sterling.*
- VII. *An Account of an Improvement on the Terrestrial Globe, by Joseph Harris, Gent.*
- VIII. *A new Method of improving and perfecting Catadioptrical Telescopes, by forming the Speculums of Glafs instead of Metal. By Caleb Smith.*
- IX. *Extract of a Letter from the Honble Henry Temple, Esq; to his Father the Right Honble the Lord Viscount Palmerston, concerning an Earthquake at Naples 1732. communicated to the Royal Society by Claudius Amyand, Esq; F. R. S. & Sergt. Surgeon to his MAJESTY.*

X. *A*

The C O N T E N T S.

- X. *A Letter from Mr. Timothy Sheldrake to Sir Hans Sloane, Bart. Pr. R. S. &c. concerning a Monstrous Child born of a Woman under Sentence of Transportation.*
- XI. *A Paper concerning the Mola SALVIANI, or Sun-fish, and a Glue made of it; communicated by the Rev. Mr. William Barlow to the President of the Royal Society.*
- XII. *An Account of the Discovery of the Remains of a City underground, near Naples; communicated to the ROYAL SOCIETY by William Sloane, Esq; F. R. S.*
- XIII. *An Account of a Meteor seen in the Air in the Day-time, on Dec. 8. 1733; communicated by Mr. Crocker to Sir Hans Sloane, Bart. Pr. R. S. &c.*
- XIV. *An Account of a Luminous Appearance in the Sky, seen at London on Thursday March 13. 1734-5. by John Bevis, M. D.*
- XV. *An Account of the Case of a Calculus making its Way through an old Cicatrix in the Perinæum, by David Hartley, M. A. F. R. S.*
- XVI. *An Account of a Stone, or Calculus, making its Way out through the Scrotum; communicated by Mr. John Sisley, Surgeon, to Sir Hans Sloane, Bart. Pr. R. S. &c.*
- XVII. *A Letter from Mr. Moreton Gilks, F. R. S. to Dr. Mortimer, Sec. R. S. giving some Account of the Petrefactions near Matlock Baths in Derbyshire; with his Conjectures concerning Petrefaction in General.*
- XVIII. *Part of a Letter from the Abbé Pluche to Dr. Mortimer, Sec. R. S. concerning the Smut of Corn.*
- XIX. *A Letter from John Bartram, M. D. to Peter Collinson, F. R. S. concerning a Cluster of small Teeth observed by him at the Root of each Fang or great Tooth in the Head of a Rattle-Snake, upon dissecting it.*
- XX. *Notices of some Meteors observed at Philadelphia in North-America by Joseph Breintnall, extracted out of a Letter from him to the Same.*
- XXI. *A Description of the Caye of Kilcorny in the Barony of Burren in Ireland, contained in a Letter from Mr. Charles Lucas, Apothecary at Dublin, to Sir Hans Sloane, Bart. Pr. R. S. &c.*
- XXII. *The Case of Grace Lowdell, aged about Sixty Years, who had an extraordinary Tumour on her Thigh; communicated by John Chandler, F. R. S. from Mr. Mizael Malfalguerat, Surgeon, at St. Edmund's-bury.*
- XXIII. *An Extract of a Letter from Mr. James Short, of the College at Edinburgh, to Mr. Rich. Graham, F. R. S. of an Aurora Borealis.*
- XXIV. *A Letter from Mr. John Freke, F. R. S. Surgeon to St. Bartholomew's Hospital, to the ROYAL SOCIETY, relating a Case of extraordinary Exostoses on the Back of a Boy.*

I. A

E R R A T A.

Pag. 308. l. 11. lege TAB. I. Fig. 2. & TAB. II. Fig. 2,
 12. pro adjuncta, l. adpicta.
 14. dele (TAB. II.)



Fig. 2.



J. Mynde Sculp.

I. *A Catalogue of the FIFTY PLANTS from Chelsea Garden, presented to the ROYAL SOCIETY by the Company of Apothecaries, for the Year 1738. pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. & Soc. Reg. Præs. By Isaac Rand, Apothecary, F. R. S. Hort. Chel. Præs. ac Prælec. Botan.*

801. **A** DONIS; Hellebori radice; Buphthalmi
flore. H. L. Bat.
802. Anonis Alopecuroides, mitis, annua
purpurascens. H. Elt. 28.
803. Asteriscus; Coronæ Solis flore & facie. H.
Elt. 42.
804. Carduus galactites. J. B. III. 54.
805. Caryophyllata Alpina, lutea. C. B. 322.
806. Chamælæa tricoccus. C. B. 462.
807. Chenopodium Ambrosioides; folio sinuato.
T. 506.
808. Chenopodium Ambrosioides, Mexicanum.
T. 506.
809. Clinopodium Americanum; Salicariæ foliis
perforatis; Pulegii odore.
810. Clinopodium; Menthæ folio, incanum &
odoratum. H. Elt. Tab. 74.
811. Cnicus Hispanicus, arboreus foetidissimus. T.
451.
812. Crocus, vernus, latifolius, flavus. C. B. 66.

813. Crocus Vernus latifolius; flore albo purpuro-violacea basi. C. B. 66.
814. Crocus albus variegatus; fundo floris flavescente.
815. Delphinium Platani folio,
Staphysagria dictum. T. 428.
816. Doronicum Plantaginis folio. C. B. 184.
817. Eryngium coeruleum, albis maculis notatum, H. Ox. III. 165.
818. Eryngium montanum, Amethystinum. C. B. 386.
819. Hyoscyamus albus, major, vel *tertius* Dioscoridis, & *quartus* Plinii. C. B. 169.
820. Hyoscyamus major, albo similis; Umbilico floris atro-purpureo. T. Cor. 5.
821. Hyoscyamus luteus, minor frutescens.
822. Jasminum humilius, magno flore. C. B. 398.
823. Iris tuberosa; folio anguloso. C. B. 40.
824. Laserpitium angustifolium majus; segmentis longioribus & indivisis. H. Ox. III. 321.
825. Laserpitium foliis latioribus; semine crispo, & verrucoso. H. Ox. III. 320.
826. Laserpitium humilius; Paludarii folio; flore albo. T. 325.
827. Laserpitium; lobis angustioribus, & dilute virentibus, conjugatim positis. H. Ox. III. 321.
828. Laserpitium; lobis angustioribus, longioribus & dilute virentibus, plurifariam divisis. Pluk. Phyt. Tab. 198. F. 6.
829. Laserpitium, lobis minimis trifidis; seminum alis fere planis.

An *Laserpitium angustiore folio, umbella
 concava & contracta.* Pluk. Phyt.
 Tab. 199.

830. *Lavandula angustifolia.* C. B. 216.

Lavendula Officinarum.

831. *Lavandula latifolia.* C. B. 216.

Spica Nardus Germanica. Trag.

832. *Leonurus minor*; *Capitis Bonæ Spei*, vulgo.
 Boerh. 180.

833. *Limonium minimum*; flagellis tortuosis,
 nostras.

834. *Linaria annua, angustifolia*; flosculis albis,
 longius caudatis. Triumph.

835. *Matricaria vulgaris*, seu *fativa*; caulibus ruben-
 tibus. H. L. Bat.

836. *Menispermum Canadense*, scandens; umbi-
 licato folio. Ac. R. Sc. 1705.

837. *Menispermum*; umbilicato folio, mucronato,
 ad basin non sinuato.

838. *Menispermum*; *Hederaceo folio.*

839. *Pinus, Halepensis*; foliis tenuibus late viri-
 dibus.

840. *Polium erectum minus angustifolium.*

An *Polium Hispanicum tenuifolium, flore
 albo, capitulo breviori.* T. 207.

841. *Populus nigra*; folio maximo; gemmis Bal-
 samum odoratissimum fundentibus. Cates.
 Hist. Tab. 34.

842. *Saxifraga, rotundifolia, alba.* C. B. 309.

843. *Saxifraga, verna, annua, humilior.* T. 252.

Sedum tridactylites tectorum. C. B.

844. *Serratula Marilandica*; foliis glaucis *Cirsii*
 instar denticulatis. H. Elt. 354.

845. Stramonium; foliis subrotundis sinuatis & denticulatis.
846. Thapsia latifolia, villosa. C. B. 348.
847. Verbena Bonariensis, altissima; Lavendulæ Canariensis spica multiplici. H. Elt.
848. Virga aurea Marilandica, cæsia, glabra. H. Elt. 414.
849. Virga aurea Novæ Angliæ, lato, rigidoque folio. Par. Bat.
850. Virga aurea, vulgaris, latifolia. J. B. II. 1062.

II. *Some Reflections on Generation, and on Monsters, with a Description of some particular Monsters: By Daniel de Superville, Privy Counsellor and chief Physician to his most Serene Highness the Margrave of Brandenburg-Bareith, President of the College of Physicians, Director of the Mines and of all Medicinal Affairs in the Margravite, Member of the Imperial Academy Naturæ Curiosorum, and of the Royal Society of Berlin. Translated from the French by Phil. Hen. Zollman, F. R. S.*

IT cannot be denied, that since the middle of last Century to this Time, very important Discoveries have been made in Natural History: However, those Discoveries are very insignificant, in comparison to what is still concealed from us. We have some Know-

Knowledge of the coarser sort of Nature's Operations, but the Niceties, the Particulars of them, escape us. If we endeavour to push our Knowledge so far, we find ourselves surrounded with Clouds, we grope in the dark, and it is very difficult, if not impossible, to catch Nature in the Fact. It even seems, we have had better Success in determining what Nature does not do, or cannot do, than in specifying what she actually does.

The Human Body is a Compound of Springs, which produce very regular Motions: Yet these Springs themselves we do not know but very superficially, and are far from knowing how those Motions are produced. We know, that we are born; that we exist; but how came we to this Existence? How were we produced? The Generation of Mankind and of Animals is one of those *Phænomena*, where innumerable Experiments have not been of so great Use, as they are else in other *Phænomena* of Natural Philosophy, for discovering their most secret Springs.

It is still a Dispute, whether the Male or the Female contributes most towards Generation. It is certain, that for the Generation of Mankind there must be a Male and a Female, and it is the same thing with regard to that of Brutes. There is all the Reason in the World to believe, that what is written about Hermaphrodites, and about those Animals which, being endued with the Advantage of the two Sexes, produce alone their Like, has not been examined with all the necessary Attention and Exactness.

The *Semen* of Man, which is certainly a most necessary Agent for Generation, because it has been

observed, that those who have none, or do not eject it according to certain requisite Conditions, are not fit for multiplying their own Species: This *Semen*, I say, is a Liquid full of small Worms. It would be absurd to deny it: All exact Observators have taken Notice of them, and offered to shew them to the incredulous. I have observed these *Animalcula* in human *Semen*, in that of several Quadrupedes, and in that of some Birds. I have observed, that the Figure of these *Animalcula*, as to Birds, was different from that of other Animals. I have preserved *Animalcula* in a proportionable Warmth alive for several Hours; I have observed their Strength and Liveliness to lessen by Degrees, and at last intirely to cease; and I have observed them dead, not swimming any longer, but always sinking to the Bottom. I have observed in the *Semen* of Men, who had a virulent *Gonorrhœa* upon them, those *Animalcula* to be without Motion, and like dead. I might enlarge upon the Particulars of a greater Number of Observations; they all prove the real and constant Existence of *Animalcula* in the *Semen* of Males.

These Worms, according to some Natural Philosophers, are true *Embryoes*. As soon as an *Animalculum* has entered into an Egg, the Female who carries the Egg in her Body, has conceived; she harbours it, nourishes it, and contributes towards the shaping of it, until it becomes an Animal, too big to be any longer contained in so small a Place, and strong enough to bear the Air.

According to other Natural Philosophers, the Eggs that are in the *Ovaria* of the Females, contain the Image, the Type, the Picture of the *Embryo*; and the
subtile

subtile Vapour of the Male *Semen*, or rather the occult Quality of that Seed, impregnating one of those Eggs, immediately fixes that Image, and makes a real *Embryo* of it.

These latter intirely deny the Existence of *Animalcula* in the Seed, because they have not seen them; and if they are shewn them, they maintain that they are foreign Beings; or, that they are a particular sort of Worms, who form a separate Class among those Insects: That God created them to exist in the Seminal Liquid, that they keep in it as in their Element, that they multiply there, and that they continue there and die, such as we observe them by the Microscope.

I do not pretend to decide, that the former are intirely in the Right; they maintain an Hypothesis founded on some Probabilities. Alas! who can hope upon so dark and hidden a Subject to find a demonstrated System? The second Opinion seems to me unwarrantable: It is founded upon Words which have no Reality. How can one form to himself the Idea of a Vapour extremely subtile and active, that shall have the Faculty of giving Life and Motion to an Image, to a Type, in short, to a thing that was not real? The Pre-existence of the *Embryo* in the Egg can by no means be demonstrated: Even by the Help of the best Microscopes, there is never any thing found in those Eggs, but a clear and limpid Liquid.

I keep among my Curiosities six ossified Eggs, which I found in the *Ovaria* of a Woman who died at the Age of Sixty. They are not all of the same Bigness. I broke two of them, and examined their

their internal Structure with all the Attention imaginable; but found nothing there except officous Fibres, issuing from the Centre towards the Superficies; there was not the least Appearance of an *Embryo*, nor of its Image.

One must have an Imagination extremely prepossessed to persuade one's self, that there is an organized Body in the Liquid contained in those Eggs: Or, it requires a very particular Natural Philosophy, to pretend to demonstrate, that a bare Vapour (more subtile than any the most spirituous Vapours we know of) could, by its simple Touch or Friction, produce an organized Body, where there was none.

The Generation of Mankind as well as of Brutes by the means of the *Animalcula*, which are observed in the *Semen* of Males, seems more analogous to all that we see Nature do for the Production and Multiplication of the Vegetables. There needs no Imagination for forming to one's self an Idea of it. Each *Animalculum* is an *Embryo*, is a small Animal of the same Species with that which harbours it: As soon as it finds itself disengaged from the Confinement in which it was, and in a Place where it meets with a Humour proper for its Vegetation and Expansion, it takes Root there, it swells like a Corn newly put into the Earth, it spreads itself, its Members shape themselves, and by degrees take more Strength and Consistence, its Parts grow longer, and disentangle themselves, as it were, from all those Plaits and Folds in which they were confined before, and the *Embryo* becomes a *Fætus*.

I own, that the immense Number of *Animalcula*, which are observed in the Seminal Liquid of Man, seems

seems to oblige one to reject this Hypothesis, and particularly this Opinion, that every *Animalculum* is an *Embryo*. For it is certain, that in every Man there would be enough of them to people a vast Country, and of all that immense Quantity there are but a few that come to any thing. And so, there you have Millions of little Men, created never to exist; which seems directly contrary to the wise Intentions of the CREATOR, who, in all Likelihood, made nothing in vain. But *Teleology* is one of those Parts of Philosophy, in which there has been but little Progress made, wherein one reasons only by Conjecture, nor can demonstrate any thing otherwise than *à posteriori*. Who dares presume so far as to pretend to penetrate into all the Designs of the ALMIGHTY, and into the divers Ends He has proposed to Himself in the Creation of the Universe? Besides, it is certain, that half of Mankind perish, before they come to the Age of one Year, that is to say, before they can know themselves, before they can answer the Ends GOD proposed to Himself when He created them. Would one say therefore, that their Existence was useless? But moreover, this seemingly useless Quantity of *Animalcula* equally affords an Argument against the Hypothesis of those, who believe the *Embryo* is in the Egg. One cannot maintain, that all the Eggs in the *Ovaria* are fruitful. And so there we have equally an immense Quantity of Types of *Embryos* created for nothing, and absolutely useless; and it will follow from both Hypotheses, that God might have saved Himself the Trouble of creating so prodigious a Quantity of Creatures in order to precipitate them into nothing. But who

dares say, that the creating so many Millions of Creatures more has cost Him any more Pains? And by what could one prove, that all those *Animalcula*, which do not come to the State of a *Fœtus*, are annihilated?

The Hypothesis of the Generation by *Animalcula* in the Seminal Liquid of Man, appears supported and confirmed by several Experiments. *Leeuwenhoek* has already observed, that a wild Male Rabbet, and a tame and white Female, produce young ones intirely resembling the Father; and that it is a Cheat very common in *Holland* to sell that sort of Rabbets, for wild ones, and that it is only by the Taste one can find out the Truth. There is among domestic Animals a sort of Poultry without Tails, and another sort with the Feathers turned upwards; if a Cock without a Tail is put amongst ordinary Hens, or a Cock with the Feathers upwards, all the Chicks will prove like the Cock: The same Experiment may be made with Pigeons, with Canary-Birds, &c.— A Mule sprung from an Ass and a Mare, resembles more to the Ass than to the Mare, whereas a Mule coming from a Horse and a She-Ass, has more of the Horse's Nature. All this proves in some measure, that the Male furnishes the most essential Part in the Generation, *viz.* the *Embryo*.

By the same Hypothesis some *Phænomena* observed in Generation, may be more easily accounted for. *Hippocrates* believed that the Difference of the Sexes depended on certain Dispositions in the Seeds of the Male and the Female; that when the Male was the most vigorous in the Copulation, they begot Males; but if the Seed of the Female prevailed, they produced

duced only Females. This Opinion, absurd as it is, has been followed and maintained by several celebrated Physicians. How can one believe, that a little more of I do not know what, (for they do not determine wherein the more or less of the Virtue in the Seed must consist) a little more Activity, a little more Spirituousness, should compose, should determine any Organization? It is more natural to believe, that every *Animalculum* has already the Sex it is to have when it comes into the World. It has been disputed Tooth and Nail, to determine the Time when the *Fætus* becomes animated, and to know from whence and how its Soul enters into its Body. According to the most general Opinion, there must be at every Conception a new Creation of a Soul: Or, according to others, there is always a Legion of created Souls fluttering about in the Air, and watching the Minute for entering into a fruitful Egg as soon as it is impregnated. What an Extravagance is this! Would it be as absurd to believe, that every *Animalculum* has already its Soul, which waits only for the little Machine's unfolding itself in order to exert its Function?

According to the Hypothesis of *Animalcula*, one may easily account for those monstrous Births, when two *Fætuses* are joined together, or Children and Animals are double, in the Whole or in Part. I keep in my Collection a Pig, that has eight Feet; the two Bodies, that are separated, reunite themselves by the *Spina Dorfi* below the *Diaphragma*, and have but one visible Neck supporting a Head, bigger than it should be, on which there appear four Ears, three Eyes, and the Snout seems double. I have also

the Head of a Foal, which is double, and has three Eyes. I have a *Turkish* Duck, which is double, the two Bodies are joined by the Breast; each Body has two Wings, and two Legs; but they have only one Neck with one Head. I keep a Chicken, which has a second Rump fixed to its Breast, with the two Legs, and two Paws. I even have a Frog, which besides its four Paws, has a Fifth as well formed as the others, which comes out at the Right Shoulder. The Production of all these Monsters that are double, or have superfluous Members, may very well be occasioned by two *Animalcula* entering into the same Egg; they touch, they close, they unite, they crowd each other: The Parts of the weakest, being too much crowded, cannot extend nor display themselves; so they vanish, as it were, so much the easier as they are extremely tender, and without any sensible Consistency.

It is not more difficult to find plausible Reasons for imperfect Monsters, or that have an odd Conformity, as to the Whole, or as to some of the Members. I have the *Fætus* of a Sheep which has no Nose; the Part where the Nostrils should be, seems to be flayed, and the two Eyes are there one by the Side of the other. On the Forehead there is a small Trunk of about an Inch and a half long, and pierced at the End by two Nostrils. I have another, which has but one Eye in the Middle of the Forehead. I have a human *Fætus* of about seven Months, which has no Mark of the Sex, and instead of the Legs there is a Bag that runs to a Point, the Extremity of which is cartilagineous: In that Bag there is a Bone three Inches long, covered with a muscular Flesh; it

it is articulated with the *Os Sacrum*; the *Ossa innominata* are wanting, and below the *Anus*, which is upon the Middle of the *Os Sacrum*, there is a small Tail like that of a Pig.

When I was at *Stetin* in *Pomerania*, about 12 or 14 Years ago, a Midwife came to tell me, that a Sergeant's Wife was delivered of three dead Children, one of which had no Head. I immediately went, and observed, that these *Fætuses* had died at different times. One began already to corrupt, and the *Epidermis* fevered itself at the least Touch. The Monster without a Head was also already quite flabby, and the third seemed to have died but a few Hours before. I examined the Monster; there was no Appearance of any Head, and instead of the Navel there was a small Lump of spongy Flesh of the Bigness of a large Strawberry. About the *Secundines* I found but two *Placentas*, and two Coats; so that this Monster must absolutely have been in one of those Coats with another *Fætus*. The Midwife was not skilful enough to give me an Account of the Delivery: I put Questions to the Mother, who assured me she felt one Child dying three Weeks before, and that the last died the Evening before. I offered a good Sum of Money to have all she was delivered of, but they would not let me have it. I still offered Money to have only Permission to dissect the Monster, but the impertinent Superstition of the Parents deprived me of that Satisfaction.

I still preserve in my Collection a monstrous *Fætus*, which deserves particular Attention. It is of eight Months, without Head or Arms: The Figure [See TAB. I. Fig. 1.] is here annexed, which outwardly

wardly seems to be nothing else but the *Abdomen* with the Legs; these are well-shaped and proportioned, with the Toes, and the Beginning of the Nails; the Right Foot however is, as it were, crooked, and bending inwards. Having opened it, I found indeed but one Cavity, which in the upper Part contains a small Bladder. There is not in all the Cavity any thing besides a Bit of Intestine, the two Kidneys, the Bladder, and the Right Testicle, which lay upon the Ring. The Flesh was hard, and, as it were, *carcinomatose*. The Navel-string went in a little higher than naturally, and a little towards the Right Side, entering into the Intestine. There is a slender Intestine of about 14 Lines in Length, proceeding from the same Place, where the Navel entered into the Cavity; next comes the *Cæcum* with its vermicular Appendix, the *Colon* and the *Rectum*, the Whole together of the Length of about two Feet. These Intestines go from above to below in *Zic-Zac*, and are attached to the *Spina Dorsi*. There is no Footstep of the Heart, the Lungs, the Stomach, the Liver, the Spleen, the Pancreas, the Mesentery; all that is wanting. The small Bladder I mentioned was fleshy, and contained some Serosity; it is attached to the first of the *Vertebras* of the Neck. This Beginning of the *Spina* is bent forwards like a Bow, and forms the Monster's Roundness from above. The bended Extremity kept the little Bladder, as it were, under, and shut up in the Cavity closed up by the Ribs. This Cavity was to form the *Thorax*, but the *Sternum* was wanting as well as the *Diaphragm*.

Des-

Descartes and *Lancisci* would in vain have looked out here for the Seat of the Soul, and the *Punctum Saliens* would prove very hard to be determined in this *Fœtus*. But I do not now intend to inlarge upon it. The Business is to find some plausible Reasons about the Origin of those sorts of Monsters I have now described.

The Opinions of most of the Natural Philosophers on this head may, upon the main, be reduced to these two Hypotheses: 1. That Monsters are original, that is to say, that even in Conception the Monster is conceived. 2. That they are not produced but by Accident. One may conclude from what I have said about double Monsters, that I believed them accidental; and I believe, rigorously speaking, they are so; whatever they be: For supposing every *Animalculum* to be an *Embryo* created, I cannot imagine them to be created imperfect. Their Imperfection, their Deformity, may proceed from a thousand Accidents, either in the *Reservoirs* where they are contained, or in the different Routs they are obliged to take going from Father to Son. In this Case it may easily happen, that they are Monsters, even in the Moment of Conception, though they be such by Accident. To how many Accidents are they not subject afterwards in the Venter of the Females? A Fall of the Mother, a strong Pressure, a Contusion, &c. may disorder the nice and tender Structure of that little Creature so far, that a great many of its Parts do not unfold themselves any longer, are destroyed, or have their Order and natural Situation intirely changed.

The

The disturbed and disordered Imagination of the Females ought also to be ranged among the accidental Causes of Monsters. I have seen in a Sow just slaughtered seven Pigs, which all had the bloody Mark of the Knife about their Necks. About some twenty Years ago, a Cloth-shearer in *Holland* had the Misfortune to fall into the Hands of some drunken young Fellows, who murdered him, and stabbed him with more than twenty Wounds with their Swords. He was to be married that very Week: His Sweat-heart saw his Corpse naked with all those Wounds, and was two Days after delivered of a dead Child, which had the Marks of the Wounds in the same Places of its Body, where the Mother had observed them on her dead Lover.

I very well know, that these sorts of Instances, of which one might alledge some Hundreds, will not go down with certain People, who deny the Effect of the Mother's Imagination on the *Fætus*. They lay Stress on two principal Reasons: 1st, It is pretended, that the *Fætus* has no immediate Connexion with the Mother who carries it. But this is ridiculous; for one cannot deny, that the *Secundines* are closely united to the *Matrix*, and receive from the Mother a Humour, or a Liquid, which by the Navel-string it remits to the *Fætus*. It is by that way it receives its Nourishment, that is to say, the Matter necessary for its Increase. Accordingly one may say, that the *Fætus* owes part of its Being to the Mother; and that the Liquid which runs in the Vessels of the Mother, runs likewise in the Vessels of the *Fætus*. 2^{dly}, It is said, that it is incomprehensible how the Soul of the Mother can have an Effect on
the

the Child. I own I do not comprehend it neither. It does not follow from thence, that we ought to reject as false all that our Reason cannot penetrate into. When once the Existence and the Nature of the Soul has been demonstrated, when once we have a perfect Knowledge of the Manner how an immaterial Being acts upon Matter, we shall then reason in Consequence about what the Soul can do, and cannot do. Daily Observations demonstrate to us, that the disordered and disturbed Imagination of Women often hurts the Infants. And this is a Reason, which I add to all the others, to think I have good Grounds to conjecture, that all Monsters are accidental; and to believe, that by the Hypothesis of *Animalcula* one may better explain the *Phænomena* which are observed in Generation, than by any other Hypothesis known as yet.

III. *Offis Bregmatis Giganteæ Magnitudinis Icon; cum Problemate de Gigantis Statura determinanda secundum Regulas Artis Delineatoriae: quæ ad Illustr. REGALIS SOCIETATIS Præsidem D^{um} Hans Sloane, Bart. transmisit Jac. Theodor. Klein Reipubl. Gedan. a Secretis & Reg. Soc. Lond. Soc.*

EX celebri musæo, quod Amstelodami fuit, Wittseniano, os bregmatis giganteum *, cujus altitudo 9 poll. Angl. latitudo 7 (TAB. I.) una cum descriptione & adjuncta figura †, altitudinem capitis a mento ad verticem 20 poll. latitudinem ad tempora 12 poll. (TAB. II.) representante, necnon aliud ejusmodi os, cujus altitudo $5\frac{6}{8}$ poll. Angl. latitudo 5 poll. sed sine figura & relatione ad caput anno 1728 nactus, facile invenire potui, assumtis secundum regulas artis delineatoriae octo capitis altitudinibus, staturam gigantis fuisse 13 ped. 4 poll. alterius vero ossis bregmatis, quod insolitum quoque simulabat caput, justam rationem & quidem juxta regulas mathematicas scire cupidus, viro excellentissimo *Henrico Kühn*, J. U. D. & Athenæi *Dantiscani* mathes. P. P. meritissimo, sequens proposui Problema :

* *Vid. Catal. Mus. Wittsen. p. 82.*

† A pie defuncto *Ruyshio* comparatis.

Si in duobus corporibus humanis diversæ staturæ fuerit,

In Priore,

Ossis bregmatis altitudo 9 poll. Angli.

—————latitudo 7.

Integri capitis altitudo 20.

—————latitudo 12.

In Posteriore,

Ossis bregmatis altitudo $5\frac{6}{8}$ seu $\frac{46}{8}$ poll. Angli.

—————latitudo 5.

Integri capitis altitudo ignota.

—————latitudo ignota.

Determinare ignotum, & rationem staturæ prioris ad posteriorem definire.

CUJUS TRIPLEX RESOLUTIO, hæc:

Si corpora ista forent similia, res brevissime expediri posset, inferendo; ut in priore corpore, ossis bregmatis altitudo ad integri capitis, vel etiam integri corporis, (tanquam illius octuplam) altitudinem, ita in posteriore corpore, ossis bregmatis altitudo vel latitudo ad integri capitis, vel etiam integri corporis, altitudinem quæsitam; quoniam vero 9 ad 7, & $\frac{46}{8}$ ad 5 sunt rationes dissimiles, corpora ista similia non sunt. Quamobrem opus est, ut in calculo & altitudinum & latitudinum ossium bregmatis ratio habeatur; uti ex triplici methodo subjecta apparebit.

METHODUS PRIMA.

(Fiat α .) $9'' : 20'' = \frac{46''}{8} : \left\{ \begin{array}{l} \text{altitud. capitis quæsitam} \\ \frac{20 \cdot 46''}{9 \cdot 8} \end{array} \right.$
 erit hujus octuplum $= \frac{8 \cdot 20 \cdot 46''}{9 \cdot 8} = \frac{20 \cdot 46''}{9} = \frac{920''}{9}$

$= 102 \frac{2}{9} \left\{ \begin{array}{l} = 8 \text{ ped. } 6 \frac{2}{9} \text{ poll.} \\ = \text{staturæ corporis secundi.} \end{array} \right.$

(Fiat β .) $7'' : 20'' = 5'' : \left\{ \begin{array}{l} \text{altitud. capitis quæsitam;} \\ \frac{100''}{7} \end{array} \right.$

erit hujus octuplum $= \frac{800''}{7} = 114 \frac{2}{7} = 9 \text{ ped. } 6 \frac{2}{7} \text{ poll.}$

$=$ staturæ corporis secundi.

(Fiat γ .) Staturarum inventarum additio, & summæ bisectio, pro obtinendo medio arithmetico, erit $\frac{8', 6'' \frac{2}{9} + 9', 6'' \frac{2}{7}}{2} = \frac{17', 12'' \frac{1}{2}}{2}$ quam prox.

$= \frac{18' + \frac{1}{2}''}{2} \left\{ \begin{array}{l} = 9 \text{ ped. } \frac{1}{4} \text{ poll.} \\ = \text{structuræ corporis secundi} \\ \text{proxime veræ.} \end{array} \right.$

METHODUS SECUNDA.

(Fiat) $9'' \div 7'' : \frac{46''}{8} \div 5'' = 20'' : \text{altitud. capitis}$
[posterioris.]

h. e. $16 : \frac{46 \div 40}{8} = 20'' : \dots$

h. e. $16 : \frac{86}{8} = 20'' : \left\{ \begin{array}{l} \text{altitud. capitis posterioris} \\ \frac{20 \cdot 86''}{16 \cdot 8} = \frac{5 \cdot 86''}{4 \cdot 8} = \frac{430''}{32} = \frac{215''}{16} \end{array} \right.$

erit hujus octuplum $= \frac{8 \cdot 215''}{16} = \frac{215''}{2} = 107'' \frac{1}{2}$
 $\left\{ \begin{array}{l} = 8 \text{ ped. } 11 \frac{1}{2} \text{ poll.} \\ = \text{staturæ corporis se-} \\ = \text{cundi proxime veræ.} \end{array} \right.$

Id quod cum priori calculo sic satis consentit, cum differentia $\frac{3}{4}$ pollicis non excedat.

METHODUS TERTIA.

Quoniam in diversorum corporum partibus cognominibus (e. g. in duobus ossibus bregmatis) superficies partium sunt inter se, ut quadrata altitudinum corporum integrorum; nec minus superficies istæ sint inter se, ut facta ex altitudinibus partium in latitudines: erunt etiam facta ex altitudinibus partium in suas latitudines inter se, ut quadrata altitudinum corporum integrorum. Quare, cum altitudo corporis prioris sit $20''$, adeoque ejus octuplum, seu altitudo

in,

integri corporis prioris, fit 160'', cujus quadratum est 25600''; fiat

altit. bregmat. prioris.	in	ejus latitud.	altitud. bregm. posterioris.	in	ejus latitud.	quadratum alti- tudinis, prioris corporis integri.	quadratum alti- tudinis, poste- rioris corporis integri.
9	•	7	: $\frac{46}{8}$	•	5	= 25600''	:
h. c. 63 : $\frac{230}{8} = 25600''$: $\frac{230 \cdot 25600''}{63 \cdot 8} = \frac{230 \cdot 3200''}{63}$							

Cum itaque sit $\frac{230 \cdot 3200''}{63} = \frac{736000''}{63} = 11682'' \frac{1}{2}$

quam proxime, = quadrato staturæ corporis secundi ;
erit (radice quadrata ex 11682 extracta) ipsa corporis
secundi statura quam proxime vera = 108 = 9 ped. 0
poll. Angl. Quæ ipsa statura, cum sit inter supra in-
ventas media, pro accuratissima est habenda.

Denique, cum octo capitis altitudines pro statura
corporis humani integri satis recte assumere sole-
amus, & capitis gigantei prioris altitudo sit 20 poll.
Angl. erit statura gigantis prioris = 8 . 20'' = 160'' =
13 ped. 4 poll. Angl. Consequenter statura gigantis
prioris est ad staturam posterioris, ut 13 *pedes*, 4
pollices ad 9 *pedes* in mensura Anglicana, seu ut 160''
ad 180'' seu ut 40 ad 27.

IV. *An Account by the Rev. Zachary Pearce, D. D. F. R. S. of a Book intituled, Reflexions Critiques sur les Histoires des Anciens Peuples, &c. Paris 1735. 4^o. in 2 Vol.*

MONS. *Fourmont* is well known to the learned World for some curious Pieces which he has already published, and for very many others in almost all Languages, which he has prepared for the Press, and the Titles of which he has given us in a Catalogue of his Works printed at *Amsterdam* 1731. in 8vo.

This Work of his, which now lies before the Society, is intituled, *Reflexions Critiques sur les Histoires des Anciens Peuples, &c.* lately printed at *Paris*, in 2 Vols. in 4to, at the Expence of some *French* Gentlemen of his Acquaintance, as he tells us in the *Advertisement* placed before his *Preface*.

His general Design is to set right the History of the most antient Nations, particularly the *Chaldeans, Hebrews, Phœnicians, Egyptians, Greeks, &c.* down to the Time of *Cyrus* the Founder of the *Persian* Empire.

The Work consists of three Books.

In the First of which he gives us at Length the famous Fragment of *Sanconiathon* the *Phœnician*, as translated by *Philo Byblius*, and preserved by *Eusebius* in his *Præparatio Evangelica*, Lib. I. cap. 9.

With this Fragment he has published a *French* Version of it, in which he endeavours to distinguish between the Account given by *Sanconiathon* the
Au-

Author, and what he supposes to be the Additions of *Philo* the Greek Translator.

After this he examines into the Reasons brought by several of the Learned *for* and *against* the Genuine-ness of the Fragment, and determines in favour of it with as much Weight of Argument as the Question will admit. He then takes Notice of a Treatise, written on the same Subject as his own, by our learned Countryman Bishop *Cumberland*; and having examined and declared his Dislike of the Bishop's Scheme in the main, he prepares his Reader to expect full Satisfaction from his own, which makes the Subject of his second Book.

In his second Book, he undertakes to reconcile the Generations of Men set forth in *Sanchoniathon's* Fragment, with those which are recorded by *Moses* of the Patriarchs before and for some time after the Flood.

By the Help of *Hebrew*, *Phœnician* and *Egyptian* Etymologies, he often makes the Names, which at first Sight are almost all quite unlike, to be the same in Sound, or at least in Sense. And by this Application of his Skill in the antient Languages, he readily finds out a Coincidence between *Moses's* and *Sanchoniathon's* earliest Generations.

But his main Work, and what he appears most pleased with, is his Discovery of *Abraham* and his Family among the later Generations recorded by *Sanchoniathon*. Having laid down (upon good Grounds, as he assures us) that *Ouranos* is *Terah*, the Father of *Abraham*, he undertakes to prove, that *Abraham* is the *Cronus* of *Sanchoniathon* and the *Saturnus* of the *Latins*; that *Sarah* (his Wife) is
the

the same with the Goddess *Rhea*; that *Ishmael* (*Abraham's* Son) is the *Múth* of *Sanchoniathon*, and the *Dis* or *Pluto* of the *Greeks* and *Romans*: That *Isaac* (*Abraham's* other Son) is the same with the *Sadid* of *Sanchoniathon*, with *Jupiter* among the *Latins*, and *Zeús* among the *Greeks*, his Wife *Rebecca* being *Juno*; that *Esau* (*Isaac's* eldest Son) is *Osiris* and *Bacchus*, and that *Jacob* (the youngest) is *Typhon*. And, in like manner, he finds a very great Part of the *Grecian* Theology in *Abraham's* Family.

In the mean while his Readers will, perhaps, make two very material Observations on this extraordinary Discovery of his: The one, that *Cronus's* Character in *Sanchoniathon's* Fragment, is the most immoral and tyrannous of any recorded there: And how to reconcile this with the Character given in Scripture to *Abraham*, as *the Friend of God*, the *Father of the Faithful*, &c. is no easy Task: It requires (to be sure) more than a Resemblance of two or three Circumstances, common to *Cronus* and *Abraham*, when their Historians in Fifty other Circumstances make their Characters essentially different. The other Consideration, which occurs, when we read this Treatise, is, that *Abraham* had ill Luck indeed, if, when he left his native Country because of the Rise of Idolatry there, all the grosser Idolatry of the Heathen Nations after his Time took its Rise from him and his Family: The very Crime which he took Pains to avoid, he was the accidental Occasion of, if he and his are to be thus placed at the Head of the Heathen Theology.

The Author, having finished this remarkable Part of his Work, enters into a very learned Detail of the particular Gods of the several Heathen Nations, who are most celebrated in History; and he has shewed a great Compass of Reading upon this Occasion. Hardly any Writer has been more copious on the Subject, or has given better Hints for clearing up many Passages of sacred and profane Story.

In his third Book he has treated at large about the Dynasties of *Egypt*, and the Shepherd-Kings who reigned there: Both of them, perhaps, the darkest Spots in the whole Face of Antiquity. He has taken great Pains to fix the Epochs of the Kings of *Sicyon*, *Sidon* and *Tyre*, of *Arabia*, *Assyria*, *Lydia*, of the *Medes* and *Babylonians*; concerning all which he has laid together the most remarkable Testimonies of the Antients. At length he comes to his favourite Point, the *Chinese* History, and gives us (as he says) a complete List of their Kings, from the Flood down to the present Monarch of that Empire, and shews that the Chronology of the *Chinese* may be made pretty nearly consistent with the true Chronology of the *Old Testament*.

And for this Part of the Work the Author seems well fitted, being skill'd (as he tells us in his *Preface*) in the learned Characters of that Country, which he has studied for near twenty Years, and has for some time taught in the Royal College at *Paris*; and having composed five Dictionaries, and a Grammar of that Language, together with a Translation, almost intire, of the Geography of *Tamim*, which contains no less than the whole History of that Empire: On which Occasion he applies to himself, and the

the Progress which he has made in the *Chinese Learning*, those expressive Verses of *Virgil* in his Sixth Book of the *Aeneid*:

——— *Pauci, quos æquus amavit
Jupiter, aut ardens evexit ad æthera virtus,
Diis geniti, potuere.*

V. *A Query proposed to such curious Persons as use the Greenland Trade, occasioned by the annexed Letter from Mr. David Nicolson, Surgeon, to Dr. Mortimer, Sec. R. S.*

“ **W**Hether the *Scurvy-grass* of *Greenland* be the
“ same Species, as to its external Appearance,
“ with the common *Scurvy-grass* of *England*?
“ And, having no acrid Taste while growing in
“ *Greenland*, doth it, being brought growing in
“ Earth from *Greenland*, gradually acquire an acrid
“ Taste, as it is brought into a warmer Climate?”

S I R,

London, Dec. 16. 1730.

I Communicate this as Matter of Truth, and not Hypothetic, *viz.* That the *Scurvy-grass* in *Greenland*, equally the same with ours in *England*, as to the Figure of the Plant, and all its Appearance to the Eye, changes its Nature much, as it approaches the Sun; for in that Climate, its principal Quality, the volatile Salt, is neither pungent nor perceivable; but to the Taste, the whole Plant is intirely as insipid as the Colwort or Beet. So by my Endeavour,

I preserved some Plants with their natural Earth, and brought them to *London* alive; and I observed the remarkable Change produced by the Sun's Heat on them; for the saline Matter in *Greenland*, which certainly was analogous to a fix'd Salt, became, in a Month's time, almost to the same Volatility as that which naturally grows in *England*.

This I make mention of, in case other Gentlemen, who have had the same Opportunity, have been remiss in their Curiosity.

David Nicolson.

VI. *A Letter from Edmund Stone, F. R. S. to ——— concerning two Species of Lines of the Third Order, not mentioned by Sir Isaac Newton, nor Mr. Sterling.*

S I R,

July 31. 1736.

HAVING for some time past been reading and considering the little Treatise of Sir *Isaac Newton*, intituled, *Enumeratio Linearum tertii Ordinis*, as also the ingenious Piece of Mr. *Sterling*, called, *Illustratio Tractatus Domini Newtoni Linearum tertii Ordinis*; I have observed, that they have neither of them taken Notice of the two following Species of Lines of the Third Order; and venture to affirm, that the Seventy-two Species mentioned by Sir *Isaac*, together with the Four more of Mr. *Sterling*, and these Two, making in all Seventy-eight, is the exact Number of the different Species
of

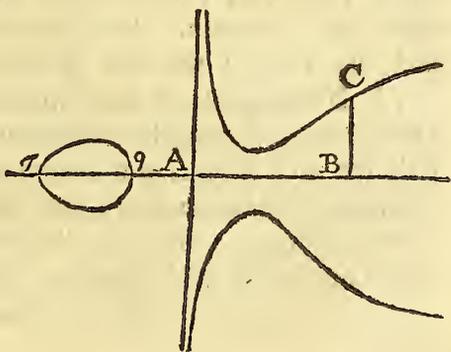
of the Lines of the Third Order, according to what Sir *Isaac* has thought fit to constitute a different Species.

The two Species I mean, are to be reckoned amongst the Hyperbolo-parabolical Curves, having one Diameter, and one Asymptote, at No. 8. of *Newton's* Treatise, or Page 104. of Mr. *Sterling's*; whose Equation is $xyy = \pm bx^2 \pm cx \pm d$; which will give, not Four, as in these Authors, but Six Species of these Curves: For,

I. If the Equation $bx^2 \pm cx + d = 0$, has two impossible Roots, the Equation $xyy = bx^2 \pm cx + d$, will (as they say) give two Hyperbolo-parabolical Figures equally distant on each side the Diameter *AB*. See the 57th Figure in *Newton's* Treatise, and this is his 53d Species, and *Sterling's* 57th.

II. If the Equation $bx^2 - cx + d = 0$, has two equal Roots both with the Sign $+$; the Equation $xyy = bx^2 - cx + d$, will (as they say) give two Hyperbolo-parabolical Curves crossing each other at the Point τ in the Diameter. See Fig. the 58th in *Newton*; and this is his 54th Species, and *Sterling's* 58th.

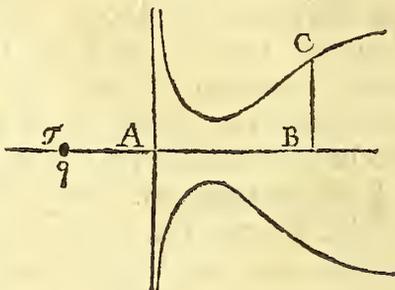
III. But if the Equation $bx^2 + cx + d = 0$, has two possible unequal negative Roots $A\rho$ and $A\tau$, the Curve given by the Equation $xyy = \pm bx^2 + cx + d$, will consist of two Hyperbolo-pa-



rabolical

rabolical Parts, as also of an Oval on the contrary Side the Asymptote or principal Abscifs. And this is one of the Species omitted by Sir *Isaac* and Mr: *Sterling*, which is really the 59th Species.

IV. Also if the Equation $bx^2 + cx + d = 0$, has two equal negative Roots $A\rho$ and $A\tau$; the Curve given by the Equation $xyy = +bx + cx + d$, will consist of two Hyperbolo-parabolical Parts, and also of a Conjugate



Point on the contrary Side the Asymptote or principal Ordinate: And this is the other Species of these Curves omitted by Sir *Isaac* and Mr: *Sterling*, which is really the 60th Species.

V. If the Roots of the Equation $bx^2 - cx + d = 0$, are real, and unequal, having both the Sign $+$; the Curve given by the Equation $xyy = bx^2 - cx + d$, will (as they say) consist of a conchoidal Hyperbola and a Parabola, on the same Side the Asymptote or principal Ordinate. See Fig. the 59th in *Newton*; and this is really the 61st Species.

VI. If the Roots of the Equation $bx^2 + cx - d = 0$, have contrary Signs, the Equation $xyy = bx^2 + cx - d$, will (as they say) give a conchoidal *Hyperbola* with a *Parabola* on the contrary Side the Asymptote or principal Ordinate. See Fig. the 60th in *Newton*; and this is really the 62d Species. I remain

Your humble Servant,

Edmund Stone.

VII. *An*

VII. *An Account of an Improvement on the Terrestrial Globe, by Joseph Harris, Gent.*

THE Globe is justly reckoned very useful and instructive, both as a general Map, and also for explaining the first Principles of Geography, and the spherical Doctrine of Astronomy. By this Instrument it is easy to find the Length of the Days, and their Increase and Decrease, in all Places, and at all Times of the Year. But this is not usually performed in such a manner as at the same time to explain how these *Phænomena* arise from the Motion of the Earth, which is the principal thing Beginners especially should have in View: Nor can this be remedied, at least but in few Cases, as Globes are commonly fitted up; for the Axis and the horary Circle prevent the Brass Meridian from being moveable quite round in the Horizon, which it ought to be, and so indeed prevent the Globe from being universally useful, even in the common way of considering it.

It is now about Six Years since I removed this Impediment, by placing two horary Circles under the Meridian, one at each Pole. These Circles are fixed tight between two Brass Collars placed about the Axis, but so that they may be easily turned by the Hand when the Globe is at Rest; and when the Globe is turned, they are carried round with it, the Meridian serving as an Index to cut the horary Divisions. The Globe, being thus fitted, serves readily for solving of Problems in South as well as in North Latitudes,

as also in Places near the Equator. But the chief Advantage gained by this Alteration, is, that the Globe is now adapted for solving of Problems upon the Principles of the *Pythagorean* System, or to shew how the Vicissitudes of Days and Nights, and the Alterations of their Lengths, are really made by the Motions of the Earth. To expedite this, I had the Brass Meridian at one of the Poles divided into Months and Days, according to the Sun's Declination, reckoning from the Pole. This being done, if we bring the Day of the Month to the Horizon, and rectify the Globe according to the Time of the Day, the Horizon will represent the Circle separating Light and Darkness, and the upper Half of the Globe, the illuminated Hemisphere, the Sun being in the Zenith.

While we view the Globe in this Position, we see the Situations of all Places in the illuminated Hemisphere, with respect to the Horizon, Meridian, &c. and by observing the Angles which the Meridians, cutting any Parallels of Latitude in the Horizon, make with the Brass Meridian, we have the Semidiurnal Arches of these Parallels respectively: And at the same time (if the Sun be not in the Equator) we see why the Diurnal Arches of the Parallels continually decrease from the Neighbourhood of the elevated Pole, till we come to the opposite Part of the Horizon. If we turn the Globe Easterly round its Axis, we shall see how all Places change their Positions with respect to the general Horizon, the Meridian, &c. by the Motion of the Earth round her Axis.

It yet remains to be shewed, how the annual Motion of the Earth in her Orbit, causes the Change of the Sun's Declination: This cannot be done by the Globe simply taken, but is very well shewed by the Instruments called *Orreries*: But to these their Costliness is an Objection, not mentioning others from a want of due Proportion in the things they exhibit. I had therefore an Instrument made, which consisted only of a round Trencher of Wood, a Circle of Brass upon the Face of it, and between these three Wheels of the same Dimensions and Number of Teeth: The innermost Wheel was fixed to the Wood in the Centre, the third had its Axis come through the brass Plate, round which was a brass Circle having a Socket making an Angle with it of $66\frac{1}{2}$ Degrees; in this Socket was fixed the Axis of a little Globe, having an Horizon about it, to represent the Circle separating Light from Darkness, the Sun being supposed to be in the Middle of the Instrument. While the brass Plate is turned round through the Scale of Months and Days expressed on the under Plate, the Axis of the *Terrella* is kept all the while parallel to itself, by means of the second Wheel placed between the two above-mentioned; and so the Change of the Sun's Declination, or rather, which comes to the same Purpose, the different Position of the Equatorial Axis with respect to the Circle separating Light and Darkness, is exhibited all the while the Earth is going round in her Orbit. By placing the Axis of an ivory Ball having one half blacked, upright in the middle of the Circle which carries the *Terrella*, this little Instrument will serve to explain the *Phænomena* of the Moon's *Phases*.

Having thus learned the Cause of the Sun's Change of Declination, we may now have recourse to the larger Globe, and moving it according to the different Seasons, we may observe the *Phænomena* thence arising more distinctly.

For a graduated Meridian, I had a flexible Slip of Brass divided into Degrees, which I could fix occasionally in the two Hour Circles; and upon such another Slip I had a Scale of Months, answering to the Sun's Declination, reckoning both ways from the Equator. By means of this graduated Meridian, the Globe being rectified according to the Sun's Declination, if we gently turn it round its Axis, we may presently find the Time of the Sun's rising or setting in all Places, by observing the Hour Circle, when the several Degrees of Latitudes respectively come to the Horizon.

After the same manner, if the Globe be elevated to any particular Latitude, and the Meridian having the Scale of Months be fixed in its Place, we may soon find the Time of the Sun's rising or setting in that Latitude throughout the Year, by observing the Hour Circle when the respective Days come to the Horizon. This Method is not only useful on the Account of its being expeditious, but also because it intimates, why at the same time the Days are of different Lengths in different Latitudes, and in the same Latitude at different Times of the Year.

The Globe-makers might save us the Trouble and Expence of having these graduated Slips of Brass, by dividing some Meridian, which goes over the least Land, into Degrees, which might be marked with round Dots, and every Tenth numbered. The Scale
of

of Months might be engraved upon some other Meridian. It would be of Use likewise, if the Parallels and Meridians of every Degree between the Tropics be drawn in faint Lines, which I think might be done without obscuring the Map.

Parallel to the Horizon, and 18 Degrees below it, I had a Circle fixed for shewing the Limits of the Twilights: This is useful, as it shews at one View the State of the Twilights, and also why they do not lengthen or shorten, as the Days do. The Semi-circle of Position is a thin narrow Plate of Brass as usual, but made so that its Axis is moveable quite round the Horizon. I had also a narrow flexible Slip of Brass, which might be girt round the Globe in any Position, and so be made to represent any great Circle whatsoever: This occasional Circle may be instructive to Beginners on several Occasions.

If the principal Horizon be of Wood, or made so as to obscure the Globe below it, the Twilight Horizon had best have small Feet of a proper Length fixed so that it might stand in its proper Place upon the other, occasionally; then inverting the Position of the Globe, the same thing will be shewed as before.

The farther Use and Application of these Contrivances to different Projections of the Sphere, &c. will be obvious to those who are acquainted with these things; and without dwelling any longer upon this Subject, it may seem, that I have already said more than was needful in this Place. But the Globe being in every-body's Hands, and in reality a very useful, entertaining and instructive Instrument; I

thought an Attempt to render it more so, would not be altogether useless, or yet unworthy the Notice of the Curious.

VIII. *A new Method of improving and perfecting Catadioptrical Telescopes, by forming the Speculums of Glass instead of Metal.*
By Caleb Smith.

THE Telescope is deservedly reckoned one of the most excellent of all the Inventions of the Moderns; such noble and useful Discoveries have been made by means of this admirable Instrument, and are still to be expected from its further Improvement, that many of the most eminent Mathematicians have employed their utmost Skill and Industry to bring it to Perfection.

The Imperfections of Telescopes are attributed to two Causes; to wit, The Unfitness of the Spherical Figure to which the Glasses are usually ground, and the different Refrangibility of the Rays of Light.

The first of these Defects only, was known to the Writers of Dioptrics, before Sir *Isaac Newton*; for which Reason (as he informs us himself, *Opt. Lect.* 1, 2.) they “ imagined, that Optical Instruments
“ might be brought to any Degree of Perfection,
“ provided they were able to communicate to the
“ Glasses, in grinding, what Geometrical Figure they
“ pleased; to which Purpose various Mechanical
“ Contrivances were thought of, whereby Glasses
“ might be ground into Hyperbolical, or even Para-
“ bolical,

“ bolical, Figures; yet nobody succeeded in the
 “ exact Description of such Figures; and had their
 “ Success been answerable to their Wishes, yet their
 “ Labour would have been lost (continues this
 “ incomparable Mathematician); for the Perfection
 “ of Telescopes is limited, not so much for want of
 “ Glasses truly figured, according to the Prescriptions
 “ of Optic Authors, (which all Men have hitherto
 “ imagined) as because that Light itself is an hetero-
 “ geneous Mixture of differently refrangible Rays;
 “ so that were a Glass so exactly figured as to collect
 “ any one sort of Rays into one Point, it could not
 “ collect those also into the same Point, which
 “ having the same Incidence upon the same Medium,
 “ are apt to suffer a different Refraction” (*Phil.*
Trans. No. 80.). And again, — “ *Diversa diver-*
 “ *sorum Radium Refrangibilitas Impedimento est,*
 “ *quo minus Optica, per Figuras, vel sphaericas, vel*
 “ *alias, perfici possint; nisi corrigi possint Errores*
 “ *illinc oriundi, Labor omnis in caeteris corrigendis*
 “ *imperite collocabitur*” (*Principia, &c. Scholium*
ad finem Libri Primi).

Now, for this principal and last-mentioned Defect,
 no one, that we know of, has proposed any Remedy;
 apprehending, perhaps, the Difficulty of attaining such
 to be insuperable; inasmuch as the great Author of
 this Discovery, himself, had not shewed us any Me-
 thod whereby to correct those Errors which arise
 from this Inequality of Refraction; but rather dis-
 couraged any such Attempts, by declaring, “ that
 “ on this Account he laid aside his Glass-works,”
 (*Phil. Trans.* No. 80.) “ and looked upon the
 “ Improvement of Telescopes, of given Lengths,
 “ by

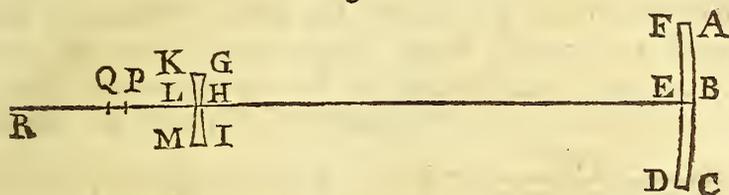
“ by Refraction, as desperate” (*Optics*, 2d Edit. p. 91.).

However, as it has been proved by incontestable Experiments, that this Dissipation of the Rays of Light, from whatever Cause it proceeds, in passing out of one Medium into another, is not accidental and irregular; but that every sort of homogeneal Rays, whether more or less refrangible, considered apart, are refracted according to some constant uniform and certain Law; and as the Removal of so great an Impediment as this of unequal Refraction in the Rays of Light, is of great Importance to the Science of Dioptrics, and absolutely necessary to its further Advancement; we have thought it worthy of a careful Examination, whether, in some Cases at least, it might not be possible for contrary Refractions so to correct each other's Inequalities, as to make their Difference regular; and if this could be conveniently effected, Sir *Isaac Newton* has acknowledged, “ there would be no farther Difficulty” (*Phil. Transf.* No. 88).

Now, upon a due Consideration of this Subject, we have found it possible, by proper Methods and Expedients, to rectify those Errors which proceed from the different Degrees of Refrangibility in different Rays, passing from one Medium into another; admitting only this well-known and established Principle, upon which we ground our Reasoning, *viz.* “ That the Sines of Refraction of Rays differently refrangible, are one to another in a given Proportion, when their Sines of Incidence are equal” (*Optics*, 2d Edit. p. 66.). And our present Design is, to shew what Advantage this will yield towards
im-

improving and perfecting Catadioptrical Telescopes, by making the Speculums of Glass, instead of Metal, in the following Manner :

Fig. 1.



Let the Figure *ABCDEF* represent the Section of a concavo-convex Speculum, whose two Surfaces are Segments of unequal Spheres; call the Radius of the Sphere, to which the concave Side is ground, *a*; and the Radius of the convex Surface, which must be quicksilver'd over, *e*; let *BR* be the Axis of the Speculum, or a Line perpendicular to both the Surfaces; and therein let *P* be the principal Focus, or Point where parallel Rays of the most refrangible Kind are collected, by this Speculum; and *Q* the Focus, or Point of Concourse, of such Rays as are least refrangible; to wit, after they have suffer'd two Refractions, at entering into, and passing out of, the concave Surface *DEF*, and also one Reflection from the convex Surface *ABC*: If the Radius of Concavity be greater than the Radius of Convexity, as we will in the first Place suppose, then *P* will fall nearer the Vertex of the Speculum than the Point *Q*; and the Interval *QP* will be the greatest Aberration, or Error, occasioned by the Separation, or unequal Refraction, of the greatest and least refrangible Rays, after their Emergence from

from the concave Surface FED . Call the common Sine of Incidence, n ; the Sine of Refraction of the least refrangible Rays out of a dense Medium into a rarer, m ; and, of the most refrangible, μ ; then, according to the known and received Laws of Refraction and Reflection, the Focal Distance of the most refrangible Rays, from the Vertex of the Speculum, (neglecting its Thickness, as of little or no Moment in the present Case) will be found

$$= \frac{nae}{(a-e)2\mu + 2ne} = PB.$$

And the Quantity of the greatest Aberration, occasioned by the different Refrangibility of the most and least refrangible Rays, PQ , will be to the focal Distance just mentioned, PB , as $(a-e)(\mu-m)$ to $(a-e)m + en$; which Quantity, or Error, thus obtained, (to abbreviate the Calculation) call ε ; and now let it be required to form a Lens, if possible, which, placed at some given Point in the Axis between the Focus of the most refrangible Rays P , and the Vertex of the Speculum (as H), shall refract not only the Rays of the most refrangible Kind tending to the Point P , but also the Rays of the least refrangible Kind tending to Q , in such a Manner, that both Sorts shall concur, after such Refraction, in some other Point of the Axis R ; let HP the given Distance of the Point in the Axis H , from the Focal Point P , be called d ; and then if the Point H has been assumed, so that the said given Quantity, or Distance, d , is greater than $\frac{(\mu-n)\varepsilon}{\mu-m}$, but less than $\frac{m\varepsilon}{\mu-m}$, I say the refracting Superficies GHI , that shall perform what was required, will be part of a concave Sphere, whose

whose Radius is $= \frac{(dd + d\epsilon) \times (\mu - m)}{m\epsilon - (\mu - m)d}$; and HR , the

Distance of the given Point H , from R , the Point to which all the Rays will tend, after Refraction at the said concave Surface, (whose Radius being found, as

above, we call v) will be $= \frac{\mu d v}{(d + v)n - \mu d}$. Lastly,

upon the Point R thus obtained, as a Centre, with an Interval a little less than HR , describe the Circumference KLM , and the Figure $GHIMLK$ will denote the Section of a double concave Lens, which, placed at the given Point in the Axis H , (taken nevertheless within the Limits above-mentioned) will collect all Sorts of Rays proceeding from the Speculum, into one and the same Focus, or Point of the Axis, R , as was required; for the Surface GHI , which first receives those Rays, will refract the most refrangible Sort converging to the Point P , and also the least refrangible converging towards Q , so that both Sorts, after such Refraction, will concur in the Point R ; but the Rays tending to R , 'tis manifest, will suffer no Refraction at their Emergence from the Superficies KLM , because R is the Centre thereof, by Construction; which Point, R , where a perfect Image of an Object infinitely distant will be formed, we call the Focus of the Telescope, to distinguish it from the Point, P , which we have before called the Focus of the Speculum.

In this manner a Lens, (or instead thereof a triangular Prism with two of its Sides ground concave, and the third plain, if that be found as practicable) may be formed and situated, so as to correct the Errors of the Speculum arising from the different

Refrangibility of the Rays of Light. But, in order to render this kind of Telescopes absolutely perfect in their Construction, the Errors also that result from the spherical Figure, must be rectified; and with regard to this, we assert, that it is possible to assume a Point in the Axis, between the Focus of the Speculum and its Vertex, (as we have taken the Point *H*, in the following Example, see Fig. 2. p. 337.) at which, if a refracting Superficies, or Lens, be constituted, according to the Method already delivered, it will not only correct the Errors occasioned by the unequal Refraction of the Rays of Light, but also rectify such as proceed from the spherical Figure of this Speculum, to a much greater Degree of Exactness than is requisite for any Physical Purpose (meaning always the Errors of those Rays which respect the Axis). Now to find or determine this Point, affords a Problem not easy to be solved; and we recommend it, as worthy of the Consideration of Geometricians.

Seeing therefore it is possible, and we believe also practicable, to remedy the Imperfections of this kind of Speculums, (from whatsoever Cause they arise) by the Method we have here proposed; it seems to follow, that Catadioptrical Telescopes may be carried, by this means, to as great a Degree of Perfection, as they are capable of receiving; provided spherical Figures can be truly communicated, with an exquisite Polish, to Glasses of a large Aperture, and a Foil of Quicksilver made also to retain that Figure accurately, and without any Inequality; for the Object-glass or Speculum being rendered perfect, so as that all sorts of Rays, proceeding from one lucid Point in its
Axis,

Axis, shall be collected by means of the Lens exactly in another Point, its Aperture may then be extended to its furthest Limits; and that is, till the whole Pupil of the Eye (or the whole Portion of the Eye-glass to be used, when that becomes necessarily less than the Pupil) be filled with Rays proceeding from the Speculum, and flowing from one Point of the Object, but no farther; because this is a Limitation made by Nature in the Structure of the Eye itself: And in Telescopes whose Construction is such as we have now described, the largest Aperture of the Speculum that can ever be of Use, will be to the Diameter of the Pupil of the Eye, very nearly, in a *Ratio* compounded of the *Ratio's* of the Focal Length of the Speculum to the Distance of that Focus from the Lens, and of the Distance of the Lens from the Focus of the Telescope, to Unity: That is, of *BP* to *PH*, and of *RH* to 1; which Proportion holds, whatever be the Charge or the Power of Magnifying.

But if Inquiry be made as to the Charge most proper and convenient, that will be determined best by Experience, in these, as well as in all other sorts of Telescopes: However, on Supposition that one of a given Length has its Aperture and Charge rightly ordered and proportioned, the Rule for preserving the same Degree of Brightness and Distinctness, in all others of a like Construction, will be, to make the Apertures, and magnifying Powers, directly as the Focal Lengths of the Speculums; which shews the vast Advantage and Perfection of these Telescopes, above the common reflecting ones; where, according to Sir *Isaac Newton's* Rule, the Apertures, and

Powers of Magnifying, must be as the Biquadrate Roots of the Cubes of their Lengths (See his *Optics*, 2d Edition, p. 97.).

It is likewise a considerable Advantage in this Construction, that the Reflection from the concave Side of the Speculum will do no sensible Prejudice; because the Image of any Object made thereby, is removed to so vast a Distance from the principal Image, formed by the convex Surface, as to create no manner of Confusion or Disturbance in the Vision; which necessarily happens, in some Degree, from the Vicinity of those Images, when the Glass is ground concave on one Side, and as much convex on the other; according to the Method propounded by Sir *Isaac Newton*, in his most excellent Book of *Optics*.

It may be imagined, perhaps, at first View, that (if our Reasoning is just) the Errors of refracting Telescopes, occasioned by the different Refrangibility of Light, may be corrected by a like Artifice: But the Aberration of the Rays from the principal Focus is there so great, and bears so considerable a Proportion to the Focal Length of the Telescope, that the Error cannot be rectified by the Interposition of any Lens, until the Rays are, by a contrary Refraction, collected again at an infinite Distance, which renders this Expedient quite useless; however, there is no need to despair of accomplishing even this, by other Methods: And, by the way, we may observe, if it were worth while to seek a Remedy for the Errors occasioned by the spherical Figure of the Object-glass only, in Dioptrical Telescopes; that might be obtained by the proper Application of a
suitable

suitable Lens, between the Focus and the Vertex of the Object-glass; which is much more easy and practicable, than the grinding of Glasses to Hyperbolic or Elliptical Figures.

For a further Illustration of what is gone before, it may be proper to exhibit the several Parts and Proportions of a Telescope in Numbers computed according to the Theorems already delivered; and in Practice we judge it will be most convenient, that the *Radii* of the Spheres to which the concave and convex Sides of the Speculum are ground, be nearly in the Ratio of 6 to 5; as in the following Example; where (see Fig. 2. p. 337.)

ABCDEF, represents the great Speculum of Glass, ground concave on one Side, and convex on the other; quicksilver'd over the convex Side, and of an equal Thickness all round its Circumference.

The *Radius* of Concavity $= a = 48$ Inches.

The *Radius* of Convexity $= e = 40$ Inches.

Then putting n , the Sine of Incidence $= 100$; m , the Sign of Refraction of the least refrangible Rays, out of Glass into Air, $= 154$; and μ , the Sine of Refraction of the most refrangible Rays, $= 156$; as Sir *Isaac Newton* found them by Experiments; we shall have,

PB, the Focal Length of the Speculum with regard to the most refrangible Rays $= 18.2926 \dagger$, which will be somewhat increased by the Thickness of the Glass, when that is considerable.

PQ, the greatest Aberration of the Rays, occasioned by their different Degrees of Refrangibility, $= .05594 \dagger$, which Quantity, in Practice, should be

be a very little augmented, rather than otherwise ;
wherefore we put it here $= .056 = \varepsilon$.

The *Radius* of the concave Surface of the Lens,
turn'd towards the Speculum, *viz.* of GHI , $= v$
 $= 2.8$ Inches.

The *Radius* of the concave Surface of the Lens,
turn'd from the Speculum, *viz.* of KLM , $= 6.7$
Inches.

The Thickness of the Lens at the Vertex $LH = \frac{1}{10}$
of an Inch.

The Aperture of the Lens must be about $\frac{1}{2}$ of the
Aperture of the Speculum.

HP , the Distance of the Focal Point \mathcal{P} from the
Point H , where the abovesaid Lens is to be placed,
so as to correct the Errors arising from the different
Refrangibility of the Rays, and also the Errors of
the spherical Figure, $= 2.\frac{2}{7}\frac{4}{3}$ Inches.

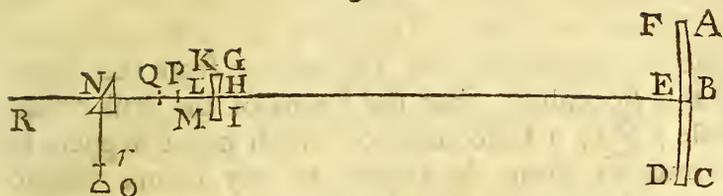
HR , the Distance of H the Vertex of the Lens from
 R the Focus of the Telescope, $= 6.8$ Inches.

And if we suppose the Diameter of the Pupil of the
Eye to be $\frac{3}{8}$ of an Inch, (though it has not one
certain Measure) then the Diameter of the greatest
Aperture of the Speculum, that can ever be of Use,
will be $6\frac{2}{3}$ Inches, nearly.

The small plano-convex Eye-glass O must always
have one common Focus with the Telescope, to
wit, the Point R translated to r , by Reflection from
the Base of the Prism N ; for which Reason it must
retain, at all times, an equal and invariable Distance
from the Lens $GHIKLM$; which Distance will be
the Focal Length of the said Eye-Glass more HR
($= HN + Nr$) the Distance of the Lens from the
Focus of the Telescope R .

The

Fig. 2.



The Form and Position of the Prism *N*, and the Contrivance of the other Parts necessary, will be much the same as in the *Newtonian* Telescope.

If the Focal Length of the Eye-glass be $\frac{1}{4}$ of an Inch, the Telescope will magnify about 200 times.

This Telescope may be contrived in the *Gregorian* way, by using, instead of a Lens and Prism, a small Speculum spherically concave on one Side, and convex on the other; but we think it not worth while to attempt this Construction, as an Investigation of the Proportion between the two Surfaces necessary, in this small Speculum, to unite the Rays proceeding from the great one, into one Point, would be intricate, and the Practice also very difficult; by reason that a little Inaccuracy will, in this Case, occasion Errors much more considerable than a like Imperfection in the refracting Lens.

We have hitherto supposed the *Radius* of the Concavity greater than that of the Convexity; as being most convenient and useful, on several Accounts, in forming this kind of Telescopes; however, it may be proper to remark, that the same Method may be used for correcting the Errors of the Speculum, when the *Radius* of its Concavity is less than that of the Convexity; only the refracting Superficies of the Lens, placed between its Vertex and Focus, will be

be convex, and not concave, as in the former Case. And there is another thing worthy of Remark, that the Focus, or Point (*P*), where the most refrangible Rays are collected, will fall farther from the Vertex of this Speculum, than the Focus of the least refrangible (*Q*); a Circumstance which never happens by Refraction alone, in Glasses of any Figure whatsoever, or howsoever they be disposed.

Fig. 3.



Now all things being put as before, and making (Fig. 3.) $HQ = d$, I say the convex Superficies *GHI* of a Lens placed at *H*, that shall correct the Errors arising from the different Refrangibility of Rays, in this kind of Speculum, will be part of a Sphere, whose *Radius* is $= \frac{(\mu - m) \times (dd + d\epsilon)}{(\mu - m)d + n\epsilon} = v$. And *HR*, the Distance of the Point *R*, where the Rays of all sorts will unite, after this Refraction, from *H* the given Point in the Axis, will be $= \frac{\mu dv}{(\mu - n)d + nv}$; which Point *R* being taken as a Centre, describe thereon the Arch *KLM*, and the Figure *GHIMLK* will represent the Section of a Meniscus-glass, or Lens, which, placed at the Point *H*, assumed between the Vertex and Focus of the Speculum, will collect all sorts of Rays proceeding therefrom into one and the same Point, or Focus, *R*. We might also shew, how

how this Error may be rectified by one or more Glasses, placed in the Axis, at a Distance farther from the Vertex than the Focal Point *P*; but the former Speculum is so much preferable to this, for the constructing of Telescopes, that we think it not worth while to prosecute this Matter farther. To conclude this Essay;

Whoever shall think fit to put the Method here proposed in Execution, we dare venture (from a Trial that has been made) to assure him of Success; provided the same Diligence, Care, and Accuracy, be applied, in choosing, figuring, polishing and foiling, the Glass, that has of late been employed for the forming Speculums of Metal; and let none be discouraged, though the first and second Attempt should fail; for that must be expected, if the ordinary way of grinding and polishing be used: Greater Exactness is here required, than is usually thought sufficient for the Object-glasses of refracting Telescopes: Let it be also considered how many Essays, for a long Term of Years, were made by Mr. *Gregory*, Sir *Isaac Newton*, and others, to reduce their Constructions of the reflecting Telescope into Practice, without answering, in any tolerable Degree, what their Theories promised: The Workmen they employed were chiefly Optical Instrument-makers, and had it been left to such Persons only to perform by themselves, we have reason to think, that it would have been pronounced impracticable to this Day, to make a reflecting Telescope that should equal or excel refracting ones of Ten times its Length; though we now see, that most of these Artificers are

capable of making them to such a Degree of Perfection as was formerly despaired of.

April 5. 1739.

IX. *Extract of a Letter from the Hon^{ble} Henry Temple, Esq; to his Father the Right Hon^{ble} the Lord Viscount Palmerston, concerning an Earthquake at Naples; communicated to the Royal Society by Claudius Amyand, Esq; F. R. S. and Sergeant Surgeon to His MAJESTY.*

Naples, Dec. 12. N. S. 1732.

— **T**HEY tell me, the last Earthquake here has made a great Crack in the Side of Mount *Vesuvius*, above 30 Yards long. I am not sure if this be true or not; though I think it very possible; but I made another Observation upon it, which I think much more extraordinary; which is, that the second Shock, which was a very slight one, had a great Effect upon the Nerves: I and all the Company where I was, as soon as the Shock was over, were seized with a Shaking, just as if we all had the Palsy, our Teeth chattering in our Heads to such a degree, that we could hardly speak; and I find, that half the Town felt the same Effect from it. It would be natural to imagine, that this Shaking was caused by the Fright, but it is easy to prove the contrary; because, in the first place, the first Shock, which was much more terrifying, had not that Effect: Secondly,

Secondly, many People who were not sensible of the Earthquake, found themselves seized in the same Manner: Thirdly, Mr.—who used to be troubled with convulsive Fits, and had got quite cured of them here, was immediately seized with them again, after the Earthquake; and, Fourthly, every body, more or less, complained of Head-achs for some Days after.

X. *A Letter from Mr. Timothy Sheldrake to Sir Hans Sloane, Bart. Pr. R. S. &c. concerning a Monstrous Child born of a Woman under Sentence of Transportation.*

S I R, *Norwich, Jan. 8. 1734-5.*

I Herewith send you both a Draught and Description of a monstrous Birth, which I believe the ROYAL SOCIETY have had as yet no Account of; which gives me Hopes, that what I here send will prove the more acceptable to you.

Elizabeth Spencer, being tried at our Assizes for the City and County of *Norwich*, for Shop-lifting, and being found guilty of the Crime, received Sentence for Transportation; for respiting of which Sentence she pleaded her Belly, which Plea, as she was a married Woman, appearing what was very probable, she was favour'd by the Mayor and the other Magistrates, by being allowed the full Time that she said she had to go; at the Expiration of which she was delivered of a Child, which I saw a few Hours after

it was born, and was exactly, in every Part, according to the following Account, and as I have here delineated it [See TAB. II. Fig. 2.]. The Head had a Rising on the Top of it, and the Nose was as if one Nose was on the Top of another, but only two Nostrils, and those at the Bottom of the lower Nose. The Arms were without the Elbow-Joint; the two Bones, which make the lower Joint of the Arm, in common, were in this extended to the Shoulder. Just under the Ribs, and above the Hips, was a deep Place, as if a Cord had been tied very streight, so as to sink down below the Reach of the Eye: This girding-in of the Body, I believe might go almost round: I did not turn it, to see whether it did or not, but it was continued as far about the Body as I could see, without turning it. By this girding-in of the Body, the lower Part of it was almost round, it being without either Legs or Thighs; but had two Feet joined unto the lower Part of the Body, the Heels inward, the Toes (of which it had not the full Number) pointing towards the Sides, as is described in the Drawing here inclosed. As to Sex, this Creature was a Female, and born alive. It was the Opinion of the Women about her, that the Midwife had injured the Head in the Birth, by which the Rising in the Head was produced; and this surprizing Creature that was born alive, was thereby soon deprived thereof. This Woman, who had been the Mother of several Children, before this strange Production, and all in perfect Form, was by some free-speaking Persons charged with having been guilty of some Practices both unnatural and unlawful, which she very positively always denied; and said that she knew nothing that could

could give any Change to the natural Form of this Creature, but the strange Apprehensions that her Sentence had put her under, from the uncommon Creatures the Country to which she was sentenced might bring in her Sight. These odd Ideas that she had formed to herself, was all and the only thing, that had occasioned so great a Change from the natural Form the Child might otherwise have had, as she often asserted. I am,

S I R,

Your very humble Servant,

Tim. Shel Drake.

XI. A Paper concerning the Mola SALU. or Sun-fish, and a Glue made of it; communicated by the Rev. Mr. William Barlow to the President of the Royal Society.

*Stoke-Dock near Plymouth, Devon,
June 29. 1734.*

THERE was brought to this Place, struck the Day before in our River, a *Sun-fish* weighing about 500 Pound-Weight. The Form of it nearly answers that given by Mr. *Willoughby*, except that the Tail of this was scollop'd.

This Fish differed very much in one thing from that described by Mr. *Willoughby*, whose Flesh, he says, was very soft: On the contrary, the Flesh of this was hard and firm, rather a gristly Substance than soft Flesh.

A Gentleman of my Acquaintance, Commander of a Vessel, tells me, his People took a *Sun-fish*, South of *Newfoundland*, which, by his Description, was considerably larger than that brought hither. They made no Use of the Flesh; but he remembers it was a gristly Substance, hard and firm.

A Piece of the Flesh boiled, to try how it would look and taste, to our Surprise, was all turned into a Jelly. Being soft and tender, it could not be taken out of the Saucepan with a Fork, but only with a Spoon; in Colour and Consistence nearly resembling boiled Starch when cold. It had little or nothing of the Fishy, but a grateful and pleasant Taste.

By the sticking together of my Lips, and from what I observed by touching it with my Fingers, I took Notice, that this boiled Flesh was clammy and glutinous; which brought to my Mind, that what the Antients made use of to serve the Purposes of Glue, was made from Fish. I then tried it upon Paper and Leather, and found it to answer the Use of Paste very well: And it was owing in part to Neglect, and partly to Accident, that it was not also tried upon Wood.

From this Discovery of the glutinous Nature of the Flesh of the *Sun-fish*, I would recommend it to those who have Opportunity to make farther Experiments upon it; and probably something useful, or curious at least, may be a satisfactory Reward for the Trouble they shall give themselves on that Account.

From the Descriptions given us of the *Ichthyocolla* by *Dioscorides* and *Pliny*, the Glue-fish seems not

to be the same as our *Sun-fish*. Whether the Fish from which our *Isinglass* is made, be the same as the *Ichthyocolla* of the forementioned Authors, as the Name usually given to it seems to import, I cannot tell: But neither the *Ichthyocolla* of *Rondeletius* or *Bellonius*, nor the *Huso* taken in the *Danube*, from the Bladder of which Fish-glué is made, can, by the Descriptions given of them, be the same as the *Sun-fish*.

XII. *An Account of the Discovery of the Remains of a City under-ground, near Naples; communicated to the ROYAL SOCIETY by William Sloane, Esq; F. R. S.*

AT *Resina*, about four Miles from *Naples*, under the Mountain, within half a Mile of the Seaside, there is a Well in a poor Man's Yard, down which about 30 Yards there is a Hole, which some People have the Curiosity to creep into; and may afterwards creep a good way under-ground, and with Lights find Foundations of Houses and Streets, which, by some it is said, was in the Time of the *Romans* a City called *Aretina*; others say *Port Hercules*, where the *Romans* usually embarked from for *Africa*. I have seen the Well, which is deep, and a good Depth of Water at the Bottom, that I never cared to venture down, being heavy, and the Ropes bad. This City, it is thought, was overwhelmed by an Eruption

Eruption of the Mountain *Vesuvius*, not sunk by Earthquakes, as were *Cuma*, *Baia*, *Trepergola*, &c.
Naples, March 7. 1731-2.

Signed,

William Hammond.

SIR,

In pursuance to your Desire, the above Account is transmitted me by my Partner, about the City you mentioned under-ground near *Naples*.

I am very respectfully,

SIR,

Your most humble Servant,

John Green.

XIII. *An Account of a Meteor seen in the Air in the Day-time, on Dec. 8. 1733; communicated by Mr. Crocker to Sir Hans Sloane, Bart. Pr. R. S. &c.*

SIR, *Fleet, Dorsetshire, Dec. 23. 1733.*

ON Saturday the 8th of this Instant, the Sun shining bright, the Weather warm, and Wind at South-East, some small Clouds passing, I saw something (between Eleven and Twelve) in the Sky, which resembled a Boy's Paper Kite, which appeared towards the North, and soon vanished from my Sight, being intercepted by the Trees which were near the Valley where I was standing. The Colour
of

of it was of a pale Brightness, like that of burnished or new-washed Silver. It darted out of my Sight with a seeming Coruscation, like that of Star-shooting in the Night; but had a Body much larger, and a Train much longer, than any thing of that Kind I had ever seen before. At my coming home, one *Brown* assured me, he had seen the same thing, for the Continuance of a Minute; and that the Body and Train appeared to him to be about twenty Foot long, and seemed to him to fall to the Ground somewhere about the Kennel-garden, whither I accompanied him in Expectation of finding some of those Jellies which are supposed to owe their Beings to such Meteors: But we might have searched long enough, as I understood the next Day, when Mr. *Edgcombe* informed me, that he and another Gentleman had seen the same Appearance at the same time about fifteen Miles from us, steering the same Course from East to the West, and vanished from them between *Walkhampton* and *Oakhampton*: They gave the same Account of its Figure, Length and Colour.

XIV. *An Account of a Luminous Appearance in the Sky, seen at London on Thursday March 13. 1734-5. by John Bevis, M. D.*

AS I was observing *Mars* near a small fixt Star, then in the West, on the Top of my House in *Buckingham-street*, about five Minutes after Eight, equal Time; happening to turn my Face Southward, I was surpris'd with an uncommon bright Glade of

Y y

Light.

Light. It was strait, about $2\frac{1}{2}$ Degrees broad, and 110, or 120 Degrees long, ill defined at either End, but pretty well at the Sides, that is, much as the common Rainbow, or one of those Pyramids which are used to dart up from the Horizon in an *Aurora Borealis*, which Light it resembled in all respects, except in its Place and Position, and that this was steady, and altogether without that tremulous kind of Motion, which usually accompanies that. Besides *Saturn*, *Mars*, *Venus*, and the fixt Stars, there was then no other Light in the Sky, nor the least Cloud, nor any of that horizontal Blackness which we see Northward in the *Aurora*. The Stars were as discernible through it, as if nothing had been there. A Gentleman who was with me, fancied it to be the Tail of a Comet; but as neither he nor myself had ever seen one, I gave but little Heed to that Conjecture: However, I carefully directed a 17 Foot Glass to all Parts of its western Extremity, but could discern nothing like a *Nucleus*. When I first saw it, it extended itself from about the Mid-way between *Aldebaran*, and *Orion's* Left Shoulder, through *Gemini* a little under β , and so on through *Cancer* and *Leo*, just above *Cauda Leonis*, till it arrived between *Vindematrix* and *Coma Berenices*, where it ended very dilutedly. In about half an Hour it grew dim about the Middle; where in a short time it separated in two, or rather became quite dark there; but then methought the disjointed Parts were more luminous than before; but they too in a little while after grew dimmer, and shortened away, on to their remote Extremities, which remained visible the longest; the Western one about Nine o'Clock, the
Time

Time of its Extinction, being near *Orion's* Right Shoulder, and the other near the Left Knee of *Boötes*; so that this Meteor seems pretty nearly to have accompanied the Earth in its diurnal Motion, and to have had little or no Motion besides. I have looked for this Light since, but could find nothing like it.

The Day was exceeding fine, and by my Journal I find, that,

At Noon, the Barometer was	29.98.
Thermometer	57.
Wind	East.
Decl. of the Needle	14° 10'.
At 10 at Night, Barometer	29.86.
Thermometer	57.5.
Decl. of the Needle	13° 50'.

XV. *An Account of the Case of a Calculus making its Way through an old Cicatrix in the Perinæum, by David Hartley, M. A. F. R. S.*

Jan. 12. 1740.

William Jarman, of the Parish of *Bayton* in *Suffolk*, was cut for the Stone about 15 Years ago, and a large Stone taken from him.

The said *William Jarman* says, that he continued easy for about four Years after he was cut; that the Wound was quite healed up, and that he made Water in the natural Way, without any Leakage at the Wound.

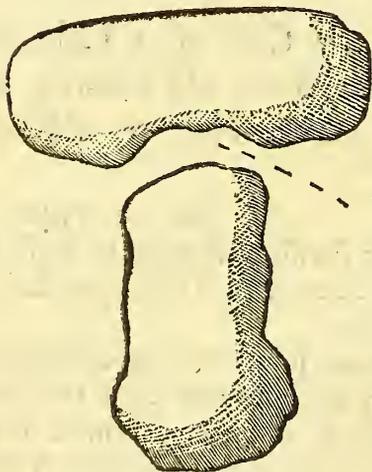
In *July* last, he says, he felt great Pain at the Place where he was cut, and that it was much swelled.

It looked black, and a little Hole broke open there, out of which the Water came; and a Stone appearing, the Hole grew wider by the Force of the Water, and his frequently touching it, till at last the Stone came away whole. It was broken afterwards by a Fall.

As soon as the Stone was come away he says that he grew easy, and the Swelling abated. The Wound is now reduced to a small Compass, and the Water still comes away through the Wound, and but very little Water comes out the natural Way.

William Jarman is about 30 Years of Age. He says, that the great End of the Stone came away first, which he suffered to lie at the Mouth of the Wound near a Fortnight, but he applied to no Surgeon.

An Addition by the Publisher to the foregoing Case.



There they articulated or rubbed against each other, while in the Bladder; one having a round Head, the other a Cavity.

Figures of Stones which made their Way through the *Perinaeum* of a Man at *Leyden*, Anno 1724. seen there by *C. Mortimer*.

XVI. *An Account of a Stone, or Calculus, making its Way out through the Scrotum; communicated by Mr. John Sisley, Surgeon, to Sir Hans Sloane, Bart. Pr. R. S. &c.*

ONE Robert Swann, of East-Malling, Kent, a hard working Man in the Woods, sent for me one Day to see him. When I came to him, I found him with a large Swelling on his *Testicles*; on the upper Part of the *Scrotum*, I found a small Hole or two, and he told me, his Urine oused out sometimes. I passed the Probe in, and found a hard Substance, which seemed to be large: I told him, he had a large Stone lodged there, at which the poor Man was much surpris'd. I told him, I would make Incision and take it out; he refused to be cut. I dilated it in another Manner, made the Orifice pretty large: The Swelling of his *Testicles* abated, he goes to work, as usual; about a Week's time after, coming home at Night with a large Bundle of Wood at his Back, found himself more in Pain than ordinary; as soon as he got home, complain'd to his Wife, and told her he was very much in Pain, went to-bed, desired me to be sent for immediately; but before I could get to him, the Stone forced its Way out; and as soon as I came to him, the poor Man seemed much rejoic'd, and told me, (as he expressed himself) the *Swan* had laid an Egg: Its Weight at first was $\bar{z}v$ and $\bar{z}ij$, now almost four Ounces and three Quarters. This Man lived about seven Years after this, in a good State of Health, and lived to the Age of Threescore
or

or upwards. He told me, he believed the Stone had been growing there for near thirty Years; but never apprehended it to be a Stone, but used to complain of a Weight, as if it were half a Pound, carried between his Legs.

John Sisley,
Surgeon and Apothecary,
Town-Malling, Kent.

XVII. *A Letter from Mr. Moreton Gilks, F. R. S. to Dr. Mortimer, Sec. R. S. giving some Account of the Petrefactions near Matlock Baths in Derbyshire; with his Conjectures concerning Petrefaction in General.*

S I R,

IN the mountainous Part of *Derbyshire*, about *Cromford*, is a Valley of at least a Mile and half long, walled on each Side with high craggy Rocks; the East Side clifty, the West more reclining, but extremely rough and difficult of Ascent; being composed of large loose Pieces of the Lime-stone Rock, of five, ten, or twenty Ton Weight; that seem at some Distance of Time to have broken off from the Top of the Cliffs, and fallen down into the Valleys.—At the Bottom of the Valley, which seems to be a great gaping Fissure of the Rock, runs the River *Derwent* harshly along its rocky Bottom. About the Middle of the Valley, at near 50 Foot perpendicular Height from the River, issue forth several
Ri-

Rivulets of a luke-warm Water, that pour themselves into the *Derwent* below. Some of this Water, being collected in a Reservoir, on account of its agreeable Warmth, hath of late Years been much used for bathing, and is called *Matlock-Bath*. Now for about the Compass of 500 or 600 Yards, near to where this Water gushes out, the Stone appears of a very different Texture and Complexion; and proves, upon Examination, to be a perfect Incrustation, formed upon the original Rock; composing a facitious Stone, of Earth, Vegetables, &c. of various Kinds, such as usually grow in rocky Places, as Polybody, Tricomanes, and other Species of the capillary Tribe, Mosses, Brambles, Ivy, Hazle, &c.— There are several large Grotto's at about 15 Foot above the Level of the River, lined most curiously with the *Stalactitæ*, *Lapides Stillatitii*, &c. Some of them nearly resemble large Bunches of Grapes, and other Clusters of Fruit, very beautiful to look upon. Specimens of most of them I think I have sent.—I found upon Examination, that the farther you penetrate into this Mountain, the closer and more compact the Stone appears; the Interstices in the petrified Matter being at the Depth of 15 or 16 Foot, almost filled up, and nearly as solid as the Lime-stone, of which the original Rock is composed; and even within four or five Foot of the Surface, though very open and porous; yet is it so hard as to be used in the Building about the Bath; and I imagine it may be equally durable, though easier to work with the Saw, than the close Lime-stone.

The Mountain in several Places jets out almost over the Brink of the River; under these Protuberances
are

are the Grotto's, very dangerous and difficult to get at. It was here the Specimens I send you were collected †, but it is impossible to give you an Idea of the natural Beauty of the Place. The Frost-work, and incruſted Plants, are ſome of them ſo very delicate and tender, as to make it impracticable to bring them away with half their Beauty, by the moſt careful Conveyance. In one Place there is an Ivy creeping along the Rock, part of it intirely petrified, another part only incruſted, and a third ſtill vegetating. In another Place is a Hazle-tree, the Root whereof compoſes a Part of this petrified Mountain, the Branches ſome petrified, and ſome tenderly incruſted. As theſe are changed, others ſpring up, and in Time will undergo the ſame Fate. In ſhort, nothing in Nature can give a more clear Idea, or more beautiful Representation, of the whole Buſineſs of Petrefaction, than a curious Obſerver will ſee, and frame in his Mind from this Mountain. He will ſee, that not only the Water, as it diſtills out of the Rocks, is capable of incruſting and petrifying the Bodies it meets with in its Paſſage, but that even the Steams and Exhalations *, being highly ſaturated with theſe mineral Particles, will work the ſame Effect; as is evident in the Place under Conſideration, and will generally beſt account for the Supply of petrifying Matter, brought to fill up the Vacuities that are left by the Decay and Waſte of Vegetables incruſted over; and which, he will ſee, are in Courſe of Time conſtantly filled therewith. For although the Water of ſome

† They are depoſited in the *Muſeum* of the ROYAL SOCIETY.

* Vide *Woodward's Natural Hiſtory*, p. 136. 209.

Springs may be so loaded with mineral Matter, as, perhaps by penetrating the Pores of Wood and other lax Bodies, to increase greatly their specific Gravities; yet surely it is contrary to the Laws of Matter, and absurd to say, there is any hidden Property in such Waters capable of changing the Parts of one Body into another Body specifically different. It may in time, no doubt, lose its Texture and Coherency, by the Admittance of heterogeneous Particles of different Attractions; but the Cause of Coherency in the Parts of the original Body must intirely cease, and be dissolved, before it can be said to become a Part of any other Body whatever. Afterwards, indeed, the Space that was possessed by the Parts of the original Body, may be supplied by those of the new one, so as to make in time a uniform Stone in the Shape of the original Plant: But if this petrified Plant be still kept in the Place where the same petrifying Quality continues to act upon it, it will lose even that Shape, and become a Part of the Body it is contiguous to; and so a great many of these petrified Plants, and other Bodies united together, will compose large Masses and whole *Strata* of Stone. This is clearly the Case in the Instance now before us, and perhaps it might be carried so far as to strengthen our Conception about the general Formation of the *Strata* of Lime-stone or Marble; that appearing to be every-where, (notwithstanding Dr. *Woodward* dispatches them much more expeditiously*) but especially in the *Peak* of *Derbyshire*, such a Petrefaction as I

* *Woodward's* Natural History, Part II Conf. 4.

have been describing, quite finished. I could urge many Reasons for my Supposition, but I will not trouble you with them here, the Compass of this Letter not permitting me; nor do I know how far such Conjectures are capable of being used, with regard to the received Opinion of the World's Age; but if we had as good Authority to suppose it 60,000 Years old, as we have 6000, it would be worth the while to trace the Origin and Source of these petrifying Exhalations a little deeper than seems to have been done by Dr. *Woodward*; and might either perfect his History, or produce a more rational System of the Earth than has yet appeared.

You will find, amongst the things I send, some Land-coral found in a Lime-pit, where is a great Quantity of it, between Two *Strata* of Lime-stone of at least Three Foot thick. You will find also some few Pieces of *Pseudo-sapphirus*, and other kinds of Spar; they are such as I picked out of the Fissures of the Rocks I have been describing. There is a vast Variety of these things in the *Peak*, much greater than hath been taken Notice of by any one; as I shall convince the ROYAL SOCIETY, when I am able to present them with a complete Collection of *Derbysbire* Fossils, in which I have already made some Progress: Who am,

S I R,

Burton, Nov. 26. 1735.

Your most humble Servant,

Moreton Gilks.

XVIII. *Part of a Letter from the Abbe Pluche to Dr. Mortimer, Sec. R. S. concerning the Smut of Corn. Translated from the French by T. S. M. D. F. R. S.*

S I R,

Paris, Oct. 24. 1736. N. S.

I Have lately passed some Months in the Country, where I have had the Satisfaction to read in the great Book (Nature), which far exceeds all our Libraries; and I made several small Observations, among which are the following:

1. Having with the Assistance of the Microscope viewed the *Smut of Corn*, I observed the Stalks were all spotted and pricked with small Burnings: Now as the *Smut* happens after a fine Rain followed by a bright Sun-shine, the Cause of this Evil is, that the Focus of those very small Drops is just near them, and on the Stalk that supports them: Wherefore the Sun's Rays, collected in this Point, must there burn; which dries up the Stalk, and prevents the Ear from graining.

The second Remark is on the Corn that grows up into Ears, the Grains of which are for the most part full of Meal quite black. With the Microscope I saw, all round or above these black Grains, small long Bodies, rolled up, and having each a Pedicle; which I found to be the Flowers, that could not reach their due Form, or come forth and ripen; so that the Grain, being deprived of this Help, could not develop its Germ, and produced only a black Meal, for want of the unfolding of certain Vessels.

The third Remark is, the Reason that invites *Thrushes* or *Starlings* under the Legs of black Cattle grazing in a Pasture. Not being able to get near them, I observed them at a Distance with a good Glass. I saw all these Birds thrust their Head and half their Body down into the Grass, in such manner that their Tails remained erect in the Air, as that of a *Duck* upon diving; which makes me think, that those Birds seek after Worms in the Earth; and that they gather about the Cattle, because as they are large Animals, upon trampling on the Ground, they oblige such Worms to come forth, as happen to be press'd under the Weight of their Hoofs.—I shall be glad of every Opportunity of convincing you how much I respect you, and am,

*Your most humble,
and most obedient Servant,*

Pluche.

XIX. *A Letter from John Bartram, M. D. to Peter Collinson, F. R. S. concerning a Cluster of small Teeth observed by him at the Root of each Fang or great Tooth in the Head of a Rattle-Snake, upon dissecting it.*

Mr. Collinson, Philadelphia, July 17. 1734.
NEAR *German-town*, about six Miles from this City, we found a *Rattle-snake*, which is now become a Rarity so near our Settlements. I took it home,

home, and dissected it: In the Head I met with what has not been observed before by any, that I can remember — that is, a Cluster of Teeth on each Side the upper Jaw, at the Root of the great Fangs, through which the Poison is ejected. — I observed, in the same Case, that the two main Teeth were sheathed in; lay four others at the Root of each Tooth, in a Cluster together, of the same Shape and Figure with the great ones — and I am apt to think for the same Use and Purposes, if by Accident the main Teeth happen to be broken — as was the Fellow to this that I send you. — May not these Cluster of Teeth be placed to supply such a Defect successively — for the Support and Defence of this Creature?

I am not certain whether this is an uncommon Case; perhaps others have not dissected the Head of this Animal with the Care that I have done. —

I am yours,

John Bartram.

XX. *Notices of some Meteors observed at Philadelphia in North-America by Joseph Breintnall, extracted out of a Letter from him to Peter Collinson, F. R. S.*

Philadelphia, May 9. 1738.

— THE remarkable *Aurora Borealis*, that was seen in *Europe* the Beginning of last *December*, was not seen here.

But:

But we had a visible *Aurora Borealis* the 29th of *December* 1736. The Day was clear, with a brisk cold Wind North-west, the Evening calm and serene, and about Seven we had a red *Aurora Borealis*.

On the 19th of *November* 1737. about Sun-set, many People in this Town saw a fiery *Meteor* in the Air, large and bright; it seemed in the *Zenith*, and so it seemed to them some Miles from Town; it was observed to be higher than the lower Clouds.

On the 7th of *December* 1737. a Minute or Two before Eleven at Night, we had two Shocks of an Earthquake, greater than ever felt here before. The second Evening after, and for several Evenings in this Month, a red Vapour appeared to the Southward and South-westward, like the *Aurora Borealis*.

XXI. *A Description of the Cave of Kilcorny in the Barony of Burren in Ireland, contained in a Letter from Mr. Charles Lucas, Apothecary at Dublin, to Sir Hans Sloane, Bart. Pr. R. S. &c.*

S I R,

BEFORE I give a particular Description of this Cave, it may be proper to give a short Sketch of that Part of the Country in which it is situate, being mostly neglected, or deemed unworthy of the Notice or Observation of any Historian hitherto.

That Part of *Ireland* called *Burren*, is a small Barony in the North-west Part of the County of
Clare,

Clare, and bounded on the North Side by the Bay of *Galway*. It is from one End to the other a Continuation of very high, rocky, Lime stone Hills, there being little or no plain Land throughout the Whole. It is that Part of which it is reported, that *Oliver Cromwell* said, (when he came to storm a few Castles in it) That he could neither see Water enough to drown a Man, Wood enough to hang a Man, or Earth enough to bury a Man in: Notwithstanding it is most fertile, and produces immense Quantities of Juniper, and some Yew; besides great Variety of the Capillary Herbs, *Virga Aurea*, *Verbena*, and some other common Plants, I have found the *Teucrium Alpinum magno flore*, of *Caspar Bauhin*, and a large shrubby *Cinquefoil*, answering the Description Mr. *Morison* gives, in his Second Volume of *Hist. Oxon.* of his *Pentaphylloides rectum fruticosum Eboracense*.

The Inhabitants are but few, and they mostly poor Cottagers, whose chief Stock is a Parcel of Goats. They are courteous and good-natured to Strangers, though very wild and unpolished; weak, blind, superstitious Zealots to the Church of *Rome*, and (like some more polite People in the World) led and enslaved by a Set of mean, ignorant and illiterate Priests.

The Place where this Cave lies, is called *Kilcorny*: It is a pretty low Valley; in Comparison to the Hills that surround it: The Entrance is into the East End of it, (for it lies East and West) about Midway. There are the Ruins of an old Church, and, a little Westward of it, an even Plain of about an Acre of Ground; on the North Side of which, under a steep rugged Cliff, lies the Cave.

The

The Mouth of it is level with the Plain, about Three Feet Diameter: It has been much larger, but was blocked up with Lime and Stone, which plainly appears still, but to what Purpose is not known. Some conjecture it was an Attempt to restrain the great Flux of Water; but the fabulous Natives, who tell numberless romantic Tales of it, say, it is a Passage to the *Antipodes*; and that a Stud of fine Horses have been seen coming out of it very often, to eat the Corn sown in the Valley: They further add, that many Stratagems have been tried to catch some of them; but, with the Loss of some Mens Lives, they could catch but one Stone-horse, the Breed of which, being very valuable, they say is kept to this Day by *O Loghlen*, which with them is a kind of titular King that they pay great Respect to. But to return to the Cave:

When you pass this narrow Entrance, it grows much wider and loftier. The Floor is a pretty even Rock, from Two to Four or Five Yards broad: The Sides and Top are rugged and unequal, from Six to Twelve or Fourteen Feet high.

About Forty Yards from the Door, there is a pretty deep Pit, Seven or Eight Yards over; but, when passed, the Floor is plain and even, as before, for about Two hundred Yards, which is the farthest that any one known has ventured into it: For my part, I did not pass this Pit, but have seen several that did, whose Veracity I can depend upon. Most People that have gone into it, went by a Thread or Clue; others have carried a Bundle of Straw, and dropped it by the Way, to guide their Return; which seems altogether unnecessary, there being no Windings or Chambers throughout

throughout of any Extent. It is all over, even in the Depth of Winter, as dry as any Place of the kind under-ground can be; and what seems very prodigious, is, that it often pours forth such a Deluge as covers the adjacent Plain; sometimes with above twenty Feet Depth of Water.

The Times of its overflowing are uncertain and irregular; sometimes it does not happen above once in a Year or two, but most commonly Three or Four times a Year: It is sometimes observed to succeed great Rains and Storms, though it often happens without either.

The neighbouring Inhabitants are alarmed at its Approach, by a great Noise, as of many falling Waters at a Distance; which continues for some Hours before, and generally all the time of the Flood.

The Water comes forth with extreme Rapidity from the Mouth of the Cave, and likewise from some smaller Holes in the low Ground, attended with a surprising Noise: It flows for a Day or two, and always returns into the same Cave, and partly into the small Holes, from whence it was observed to come before, but with a more slow and tardy Course. The Water is of a putrid Quality, like stagnated Pond-water, insipid as Spring-water. It always leaves a filthy muddy Scum upon the Ground it covered, which greatly enriches the Soil.

It has been known sometimes (though rarely) to overflow and ebb in six or eight Hours time, but in a much less Quantity.

There is neither River or Lake any-where in that Part of the Country, and it is above Six Miles from the Sea. There are very near it several much lower
A a a Valleys,

Valleys, in which there is no Appearance of Water, unless a little Rain-water collected in a Pit, in the Fissure of a Rock, or the like.

If there be any Queries about further Circumstances relating to it, any way material to be asked, I shall be always ready to answer them, having spent a good deal of Time upon the Spot, examining of it myself; so that I can aver this whole Narrative for Truth, from my own actual Observations.

I thought it a pity so remarkable and wonderful a *Phænomenon* of Nature should lie hid from the Learned; and though conscious of my own Insufficiency, and Incapacity of writing or stating the Case methodically, have at last resolved, at all Adventures, to send you a true, simple, and naked Description of it.

I have been very cautious in saying more than any Man may see there.

S I R,

*Your most obedient,
and faithful humble Servant.*

Dublin,
Nov. 11. 1736.

Charles Lucas.

XXII. *The Case of Grace Lowdell, aged about Sixty Years, who had an extraordinary Tumour on her Thigh; communicated by John Chandler, F. R. S. from Mr. Mizael Malfalguerat, Surgeon, at St. Edmund's-bury.*

GRACE Lowdell, a poor old Woman of the Parish of *St. James, in Bury St. Edmund's, Suffolk*, being naturally of a gross, fat, and relaxed Constitution, and constantly given to the drinking of strong Liquors; and consequently labouring for many Years under an ill Habit of Body, such as the Rheumatism, which had caused Contraction of some of her Fingers; with some nervous Affections in her Head, often causing some little Fits of Vertigo, &c. And though she had formerly some Child-bearing Weakness, *viz. a Procidencia Uteri*, yet there could not be found any other scrophulous Symptoms, than that she observed, when about Thirty Years of Age, soon after her Delivery of a Son, a little hard Swelling on the Muscle *Biceps*, and posterior, inferior and external lateral Part of the Thigh, a little above the Ham, without her knowing any manifest Occasion for it; which at first went on slowly, but after proceeding more quickly, and the older it grew, it still came on the faster, until it increased to the Bulk of near a Foot in Circumference, being somewhat of a globulous and a little longish Figure from its Basis, which was lax, like a Peduncle, or Stalk, and about half the Circumference of the Tumour, like a Neck to the Head of a Child hanging down.

From the first Appearance of this Tumour to the Excision of it, there were more than Thirty Years: She had excessive Pains and Uneasiness in it, and at last its Bulk and Weight had in some measure intercepted the Nourishment to it, so that an Ulcer had affected the inferior Part of it, very putrid and sinuous, of about Six Months standing.

This Excrecence was of the natural Colour of the Skin, and was for the most part of a pinquedinous Substance; the Centre and Basis being an *Atheroma*, but more scirrhus than common.

This Excrecence, having grown so big, was not contained in a manifest *Cystis*, but had some large Circumvolutions of Fat adherent to its common Teguments, as was observed after the Excision of it, when it was soon conveyed away; so that, through Inadvertency, we did not weigh it.

My Design in this Case was to have made a total Extirpation of this Excrecence; but by reason of its lying with large Vessels, and amongst the Tendons of the Muscles, I was content (as Dr. *Turner* advises in such Cases) “ To level it, the best we can, by
 “ Escharotics, still repeated as the Sloughs throw off;
 “ till we have consumed as much of the Gland or
 “ Substance, and gone as deep, as may be safely ad-
 “ ventured; when probably some powerful Desic-
 “ cative may induce a *Cicatrix*, which may so tie
 “ the Remains, as to create no farther Trouble.”

This Tumour had been shewn to most of the Physicians and Surgeons herabouts, some of no less Skill than Note, who seemed to approve of the Operation: Therefore, *July* the 7th 1735. I made a Ligature about the Basis of it, with a Slip-knot, by
 which

which I gradually constringed it once or twice a Day, as the Patient could suffer it, without causing any ill Symptoms, till the 17th of the same Month, when she was taken with strong Convulsions, a slow Fever, Syncope, her Teeth set in her Head, and a Loss of her Senses, which lasted that whole Day, and the Night following; from which time I did no more constringe the Tumour, prescribed Cordials, volatile Drops, a purging *Enema*, and a paregoric Draught at Night, which had so good an Effect, that by the next Day she was much recovered, and came to her Senses. The Ligature began to make a Separation in the Neck of this preternatural sprouting Excrecence; and on the 20th, in the Presence only of one in the Profession, having all my Apparatus before me, I extirpated the whole outward Tumour without any great *Hæmorrhage*. I was induced to use the Ligature, in order to prevent the too great Effusion of Blood; which might otherwise happen; thinking it not very safe to make a Ligature of the Body of so large an Artery as is in the Ham, for fear of intercepting afterwards the Nourishment to the Leg, as happens often after the Operation of the *Aneurisma*.

The Remains, though sordid at first, by a peculiar Method of Dressing, and proper Applications of strong Digestives, Detergives, &c. cleansed, and the Ulcer soon digested, the Substance came even to the Skin, and, *September* the 21st, it was all perfectly cured, without any Hardness, or any Inconvenience to her walking, and is like to remain always so.

XXIII. *An Extract of a Letter from Mr. James Short, of the College at Edinburgh, to Mr. Richard Graham, F. R. S.*

S I R,

Edinburgh, Nov. 18. 1736.

I Came here on *Saturday* last: That Evening, about Six o'Clock, there was one of the most remarkable *Aurora Boreales* that ever I saw. At first there appeared the ordinary luminous Arch, the Vertex of which was about 30° above the Horizon, and had its Centre somewhere in the Meridian Circle. After this was perfectly well formed, there appeared little or none of the purple and red Colours which are usually in that Arch; but immediately there broke out, from the most Western Extremity, a great deal of that Northern Light which formed this Arch, and, rushing along with Rays directed to the *Zenith*, formed another *Aurora Borealis* above the first, the Centre of which was to the East of the Meridian: After this was formed, there followed from the same Extremity, a great deal of purple and red-coloured Light, quivering and shaking towards the *Zenith*, with a flapping Noise in rushing along, till it formed a third *Aurora Borealis*, above the second, the Centre of which was somewhere on the East-side of the Meridian. When I was pleasing myself with this remarkable *Phenomenon*, looking again to the Western Source of these Arches, I perceived, as it were, a huge Pillar of a dull red-coloured Light, rising out of the same Place whence the Arches took their Beginning, extending itself in a Direction towards the *Zenith*,

Zenith, till it rose almost 60° high. These Arches and Pillar lasted very near an Hour; the two uppermost Arches were continually quivering and shaking, and the Pillar always turning to a paler Red.

I forgot to tell you, that the Night before the *Aurora Borealis*, there was a prodigious Hurricane of Wind, which lasted till the *Saturday* Morning; but all that Day it continued to blow, though not so hard. The Arch from whence the Wind blew, was from the North-west, the same Quarter from whence the Arches took their Rise. To this Day, ever since the Hurricane of Wind, there has been a most intense Frost: It froze so hard, that in less than 24 Hours after it began, the Lake on the North side of this City was so strong as to bear People on it. Just now the Wind has changed, so that we expect a Thaw.

XXIV. *A Letter from Mr. John Freke, F.R.S. Surgeon to St. Bartholomew's Hospital to the ROYAL SOCIETY, relating a Case of extraordinary Exostoses on the Back of a Boy.*

GENTLEMEN,

I Would not have troubled you with this Account of a Case which came to my Inspection Yesterday at *St. Bartholomew's Hospital*, had I ever seen the same before in my Practice. I know it may be said to come under the Denomination of an *Exostosis*, but as all others that I have seen, which have been very many, arose upon some particular Parts, and have not been

been found to proceed from a general Dissolution of the Bones, as this hath, I think fit to submit it to your Consideration. The Case is as follows: Yesterday there came a Boy of a healthy Look, and about Fourteen Years old, to ask of us at the Hospital, what should be done to cure him of many large Swellings on his Back, which began about Three Years since, and have continued to grow as large on many Parts as a Penny-loaf, particularly on the Left Side: They arise from all the *Vertebrae* of the Neck, and reach down to the *Os Sacrum*; they likewise arise from every Rib of his Body, and joining together in all Parts of his Back, as the Ramifications of Coral do, they make, as it were, a fixed bony Pair of Bodice. If this be found worthy your Thoughts, it will afford a Pleasure to,

GENTLEMEN,

Your most humble Servant,

*Salisbury-Court,
April 15. 1736.*

John Freke.

It is to be observed, that he had no other Symptom of the Rickets on any Joint of his Limbs.

Printed for T. WOODWARD, at the *Half-Moon*, between the *Two Temple-Gates* in *Fleetstreet*; and C. DAVIS, over-against *Gray's-Inn-Gate* in *Holbourn*; PRINTERS to the ROYAL SOCIETY. M.DCC.XLII.

Numb. 457.

PHILOSOPHICAL
TRANSACTIONS.

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

L O N D O N :

Printed for T. WOODWARD, and C. DAVIS, over-
against *Gray's-Inn-Gate* in *Holbourn*; PRINTERS
to the ROYAL SOCIETY. 1743.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *July* and *August*, 1740.

The CONTENTS.

- I. *An Account by Mr. John Eames, F. R. S. of a Dissertation, containing Remarks upon the Observations made in France, in order to ascertain the Figure of the Earth, by Mr. Celsius, intituled, De observationibus pro Figura Telluris determinanda, in Gallia habitis, Disquisitio. Auctore Andrea Celso, in Acad. Upsal. Astronom. Prof. Regio, &c. Upsalix, 1738. 4^{to}.* pag. 371.
- The Continuation of the Remarks made by Mr. Celsius on the Observations taken in France, in order to ascertain the true Figure of the Earth.* 378.
- II. *A Letter from Mr. J. Alexander to Peter Collinson, F. R. S. concerning a Place in New-York for measuring a Degree of Latitude.* 383.
- III. *Ex Veterum Prussorum RE ANTIQUARIA Schediasma a D. Jac. Theodoro Klein Reipubl. Gedan. a Secretis, R. S. S. cum D. Hans Sloane, Bart. R. S. Pr. communicatum.* 384.
- IV. *Observations and Experiments on Madder-root, which has the Faculty of tinging the Bones of living Animals of a red Colour, by M. Du Hamel du*

The CONTENTS.

- du Monceau, F. R. S. *communicated in a Letter to Sir Hans Sloane, Bart. Pr. R. S. Translated from the French by T. S. M. D. F. R. S.* 390.
- V. *A Catalogue of the FIFTY PLANTS from Chelsea Garden, presented to the ROYAL SOCIETY by the Company of Apothecaries, for the Year 1739. pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. & Soc. Reg. Præs. by Isaac Rand, Apothecary, F. R. S. Hort. Chel. Præs. ac Prælec. Botan.* 406.
- VI. *A Physico-mathematical Demonstration of the Impossibility and Insufficiency of Vortices: By M. de Sigorgne. Translated from the French by T. S. M. D. F. R. S.* 409.
- VII. *An Account by David Hartley, M. B. F. R. S. of Dr. Trew's Dissertation concerning the Differences of a Human Body before and after Birth, intitled, Dissertatio epistolica de differentiis quibusdam inter hominem natum & nascendum intervenientibus, deque vestigiis Divini Numinis inde colligendis; Jo. Georgio Kramero inscripta cum Tab. Æn. Auctore Christoph. Jacobo Trew, Noribergæ, 1736. 4^{to}.* 436.
- VIII. *Some curious Experiments and Observations on a Beetle, that lived Three Years without Food: Communicated to the ROYAL SOCIETY in a Letter from Mr. Henry Baker to Alexander Stuart, M. D. F. R. S.* 441.
- IX. *The Discovery of a perfect Plant in Semine; c. gr. of the Gramen tremulum. By the Same.* 448.



... .. 43/

TAB. I.

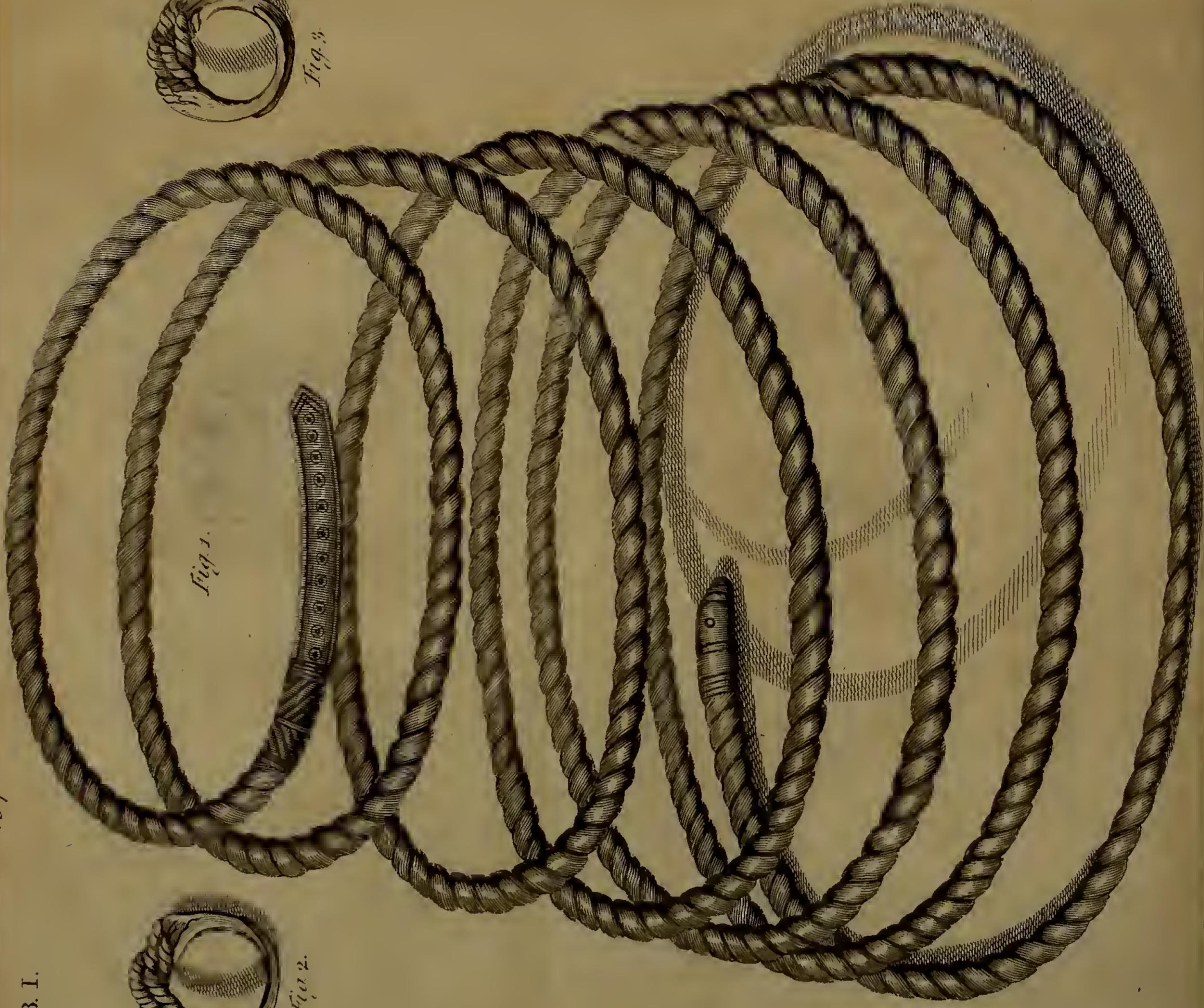


Fig. 1.

Fig. 3.

Fig. 2.



Fig. 1.

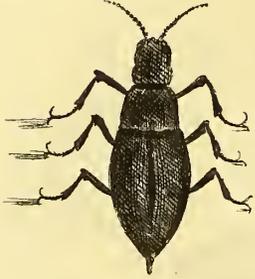


Fig. 2.*

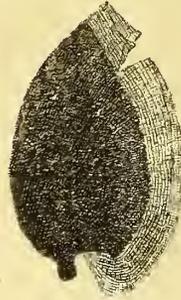


Fig. 2.



A

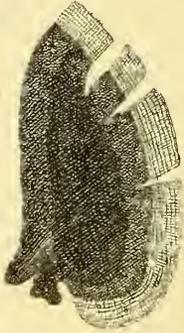
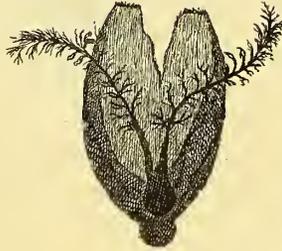


Fig. 3.

B



C



A

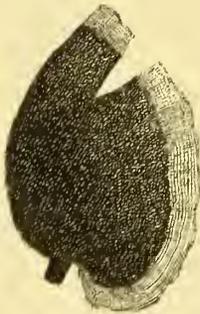
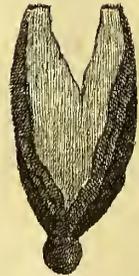


Fig. 4.

B



C



I. *An Account* by John Eames, F. R. S. of a *Dissertation, containing Remarks upon the Observations made in France, in order to ascertain the Figure of the Earth, by Mr. Celsius, intituled, De observationibus pro Figura Telluris determinanda, in Gallia habitis, Disquisitio. Auctore Andrea Celso, in Acad. Upsal. Astronom. Prof. Regio, &c. Upsaliæ, 1738. 4^{to}.*

THAT the Figure of the Earth is Spheroidal is agreed upon by all: But whether it be an oblong or oblate Spheroid, *i. e.* whether the Axis be longer or shorter than a Diameter at the Equator, has been for some time a matter of Doubt. Three several Methods have been proposed to determine this Controversy by Experiments; as by the different Lengths of Pendulums vibrating Seconds, in different Latitudes; the Figure of the Earth's Shadow in Lunar Eclipses; and by the actual Measurement of the Lengths of a Degree on the Meridian in different Latitudes.

It is certain, if the Lengths of the Degrees of Latitude decrease as we go from the Equator toward the Poles, then the Axis is greater, and the Figure an oblong Spheroid; but, on the contrary, if these Lengths increase as you remove towards the Poles, the Axis is less than a Diameter at the Equator, and consequently an oblate Spheroid.

B b b

Mr.

Mr. *Cassini* and others, judge the Earth to be of an oblong Spheroidal Figure; and the Observations made in *France*, if intirely to be depended upon, prove this *Hypothesis* to be a Matter of Fact. Our late illustrious President, Sir ISAAC NEWTON, Mr. *Huygens*, and others, make the Earth to be an oblate Spheroid, higher at the Equator than at the Poles; and this Figure of the Earth is undoubtedly the true one, if the Observations lately made near the Arctic Circle be admitted as certain and exact. So that since both Sets of Observations have been taken by Persons of known Skill, Dexterity, and Integrity, it is now become absolutely necessary to inquire into this Matter, in order to find out the Occasion of so great a Difference in their Conclusions.

Mr. *Celsius*, in the Treatise before us, proposes to consider this Matter more closely, and begins with a Defence of the Observations made at *Tornea*, near the North Polar Circle; and then takes Notice of some things, proper to be considered, relating to the Instruments, Astronomical Observations, and Trigonometrical Operations, performed in *France*; which, in his Judgment, render the Observations uncertain; at least so far as not to be accurate enough to be depended upon in determining the Matter in Question.

To begin with the Defence of the Observations made at *Tornea*: Perhaps it may not be improper to premise a short Account of them. They were undertaken at the Charge of the King of *France*, by Five skilful Gentlemen; Three of them Members of the *Royal Academy* at *Paris*, who were joined by Mr. *Celsius*, and the Abbé *Authier*. The Trigonometrical
Part

Part of the Work was performed near the River of *Tornea*, whose Direction is the same with the Meridian of *Tornea*; the Coasts of the Gulph of *Bothnia* being found very inconvenient for that Purpose. By the favourable Situation of Five Mountains they formed Eight Triangles, which took in Space enough for their Design. All the Five Gentlemen observed, one after another, each Angle of these Triangles, setting them down in writing separately.

They afterwards determined the Distance between *Tornea* and Mount *Kittis*, under the same Meridian, by a Basis, measured on the River when frozen over, whose Length was 7406 Toises 5 Feet, by the first Measurement; and when measured again, was barely Four Inches over. This Distance between them they found to be 55,234 Toises.

The first Part of their Work being thus finished, the next was to find the Difference of Latitude of these two Places: This they did by the Help of a Telescope, fixed to a Sector of Nine Foot, made at *London*, by the Care and Direction of Mr. *George Graham*, to whom the Lovers of Astronomy are indebted for the curious and well-contrived Instruments he has supplied them withal. The Star they observed at *Tornea* was *α Draconis*: They repeated their Observations three times, and the greatest Difference between them was but Two Seconds: Removing to Mount *Kittis*, they took the same Number of Observations, of the same Star, without finding more than One Second Difference. The Result was, that the Amplitude of the Arch, in the Heavens, between *Tornea* and Mount *Kittis*, (allowing for the Precession of the Equinox, and the Time elapsed between

the Two Observations, according to Mr. *Bradley's* Theory) was 57 Minutes 26 Seconds. Hence the Magnitude of a Degree, on the Earth, intersecting the Polar Circle, was found to be greater than a mean Degree of *France* 377 Toises; and to differ 900 Toises from what it should have been, according to Mr. *Cassini's* Hypothesis: And if the Correction, according to Mr. *Bradley's* Theory, were omitted, the Difference would have amounted to above a Thousand Toises: The Consequence of which, say the curious Observers, is, That the Earth is not only flatted towards the Poles, but that it is much more so than Sir *Isaac Newton* or Monsieur *Huygens* thought it. This unexpected Difference being so very great, made them resolve upon a careful as well as new kind of Verification of the Whole. In the first Place, they repeated their Astronomical Observations Three several times, at *Tornea* and *Kittis*, with the same Instrument, but on another Star, *viz.* δ *Draconis*: The Difference of Latitude between the Two Places was found to be the same, within Three Seconds and an half, with the First. They then not only examined the Truth of their Meridian Line, the Exactness of the Sector, in the different Divisions upon the Limb, chiefly in the Two Degrees employed in observing α & δ *Draconis*, but supposed that, in their Trigonometrical Operations, they had erred in each Triangle, by Twenty Seconds in each of the Two Angles, and Forty Seconds in the Third; and that all these Errors tended to diminish the Length of the Arch; the Calculation, upon this Supposition, gives but $44 \frac{1}{25}$ Toises for the greatest Error that could be committed.

When

When a particular Relation of all these Observations was read before the *Royal Academy of Sciences* at *Paris*, and inquired into; the main Exception taken to them was, That the Observers, omitting to make a Proof of the Line of Collimation, by means of double Observations, with the Face of their Instrument turned contrary Ways, have thereby not duly ascertained the Truth of their Observations. But this Objection was fully answered by Monsieur *Maupertuis*, as Mr. *Celsius* hopes and believes, to the intire Satisfaction of Monsieur *Cassini*, who made it. He allows M. *Cassini* had very good Reason to mention this, as a thing proper to be done in Instruments of common Use, for this Purpose, which generally stand in need of such a Method of Verification: But it was not at all necessary in the Instrument used at *Tornea* and Mount *Kittis*: The very Make of it was such, that no Alteration could easily be made in it, so as to create any perceptible Error in the Observations. The whole Apparatus of the Telescope and Sector is all framed together; the Object-glass and Cross-wires, as well as the Limb, so firmly fixed to the Tube, as not to be dislocated without great Violence. Notwithstanding all this, the utmost Care was taken in transporting it from one Place to another; being placed in a Chest, that the *Laplanders*, to use his own Words, *in illa cista idolum quoddam servari facile sibi persuaderent*. He adds, the same Objection may be made to Monsieur *Picard's* Observations, who does not seem to have used this Precaution, as Monsieur *Cassini* himself acknowledges, who nevertheless approves and extols his Observations for their Accuracy: So that those at the Arctic Circle

Circle may be very good, notwithstanding the want of this, supposed necessary, Operation. And indeed, that they were so, sufficiently appears from this Fact. The Difference of Latitude between *Tornea* and Mount *Kittis*, found in *September*, was observed again in *March* following, by the Help of the same Star δ *Draconis*, and did not differ from the former above $3\frac{1}{2}$ Seconds, though the Instrument had been twice carried from one Place to the other. This is a Degree of Exactness not easy to be met with; no not in Monsieur *Cassini's* Observations, made on different Stars, which differ sometimes 40 Seconds, in determining the Amplitude of an Arc in the Heavens, though their Instrument was carefully examined in the way above-mentioned.

The Author then proceeds, in his Turn, to inquire into the Accuracy and Certainty of the two Sets of Observations made in the North and South Parts of *France*, in respect of the Royal Observatory at *Paris*.

As to the Measures of the Degrees in the Northern Parts of *France*, between *Paris* and *Dunkirk*, he owns they cannot be much out of the way; being in some measure confirmed by Monsieur *De la Hire*, in the Year 1683. and Monsieur *Cassini* himself. Yet Mr. *Celsius* observes, that the Basis on the sandy plain Shore, near *Dunkirk*, when measured again, differed Three Feet from the former Measurement; which is a much greater Difference than that Mr. *Celsius* and the other Gentlemen found, in measuring a much longer Line twice over, which was but Four Inches.

As to the Astronomical Observations taken by the Six Foot Sector, whose Limb of 12 Degrees was divided only at every 20 Seconds; it is true, Monsieur
Cassini

Cassini examined the Instrument several ways, at *Paris*, after his Return thither: but that a Correction, owing to the Change of Centre, might be safely applied to the Observations at *Dunkirk*, the Examen of the Centre should also have been taken at *Dunkirk*; it being uncertain, whether this Alteration or Aberration of the Centre was caused by the Journey to or from *Dunkirk*.

The Difference of 41 Seconds between the Observations taken to settle the true Measure of the Arc of the Heavens, seems to be enormous. Perhaps the Stars were not lucid enough to be well observed by the Three Foot Tube; but might they not, for a due Degree of Accuracy, have been viewed through the Nine or Ten Foot Telescope?

Our Author prefers the Observations of 1719, made after the Return to *Paris*, to those made before; because made at the same time of the Year with those of *Dunkirk*, and so not standing in need of Mr. *Bradley's* Correction: Though this Caution, perhaps, may be thought not necessary here, where the Errors of the Observations are greater than the Correction itself. Mr. *Celsius* remarks farther, if the Difference of Latitude between *Dunkirk* and *Paris* be supposed to be Two Degrees 12 Minutes 12 Seconds and an half, which is a Mean between Four others he mentions, the Length of a Degree will amount to but 56,395 Toises. And if the Observations at *Malvoisine* and *Amiens*, be counted according to Mr. *Bradley's* Theory, for the Interval of a Month between the Observations, the Length of a Degree will come out to be 56,926 Toises; which is 135 Toises less than the Length of a Degree, found
by

by measuring the whole Length of *France*; and 134 less than that of Mr. *Picard*, so highly approved of by Mr. *Cassini*, as confirming his own.

The Reflections Mr. Celsius makes upon the Observations, &c. taken in the South Parts of France, shall be referred to another Meeting.

The Continuation of the Remarks made by Mr. Celsius on the Observations taken in France, in order to ascertain the true Figure of the Earth.

MR. *Celsius* having finished his Remarks upon the Observations made in the North Part of *France*, extending from *Paris* to *Dunkirk*, proceeds to examine those taken in the South, from *Paris* to *Collioure*, near the Borders of *Spain*, and the *Pyrenean* Mountains. By the former, a mean Degree was found to consist of 56,960 Toises, by the latter 57,097; and consequently the Earth is an oblong Spheroid.

Mr. *Celsius*, in examining these Observations, which were taken under the Conduct and Direction of the late M. *Cassini* in 1700, first considers the Structure and Goodness of the Instruments used; then the Accuracy of the Astronomical Observations for finding the Difference of Latitude; and, in the last place, the Trigonometrical Operations for determining the Distances of Places; especially the two Extremes under the same Meridian,

The

The principal Instrument M. *Cassini* carried with him, was, a Limb of 12 Degrees, whose Radius was indeed 10 Foot, but divided only into Degrees and Minutes; the other Parts were added to it at *Perpignan*. Here Mr. *Celsius* observes, that the finding the true Centre of this Limb was and still is a very difficult and troublesome Problem to a good Artist; that no mention is made, whether the Position or Place of this Centre, and the Divisions of the Limb, were ever examined at *Paris* or *Collioure*, though the Carriage of the Instrument through so long and rough a Way, could not but make some Alteration in the Place of the Centre.

It is true, the Zenith Distance of *Capella*, taken by it at *Paris*, was confirmed to be right by another Instrument; but it cannot be concluded, that the Zenith Distance of the same Star, taken at *Collioure* by this Instrument, and not confirmed there by another Instrument, must be true also. For the Point of Division, answering to this Distance in the Limb, was not examined; and a Centre wrong placed may by Accident give the true Zenith Distance, *viz.* when the true and erroneous Centre happen to lie in the same Perpendicular to the Horizon.

The Exceptions taken to the Astronomical Observations for finding the Difference of Latitude between *Paris* and *Collioure*, are, in the first place, That though Five Stars were observed at *Collioure* and *Paris*, yet One only was made use of, *viz.* *Capella*: That the Difference of Latitude by *Capella* is $6^{\circ} 18' 57''$: If *Lucida Lyra* had been used, the Difference would have been but $6^{\circ} 17' 7''$; but by the Right Shoulder of *Auriga*, $6^{\circ} 19' 25''$: Hence arises the

Uncertainty or Difference of $2' 18''$ between the greatest and least of their Observations: That the late Mr. *Cassini* makes the Difference $57''$ less than Mr. *Cassini*, who accounts for this Difference from the Observations being taken by an ordinary Instrument; but the Instrument is the same which was used to take the Altitude of the Pole of *Amiens*, which was very near that found by Mr. *Picard*.

As to the Trigonometrical Operations for finding the Distance of Places, Mr. *Celsius* thinks they labour under considerable Uncertainties; not only on the Account of the many Difficulties they met withal, *viz.* mountainous Countries, want of proper Signals, &c. so that convenient Triangles could not be formed; but add to all these, several of the Triangles had but Two Angles observed, and some of these Angles too acute; whence, as Mr. *Cassini* himself very justly observes, in his Examination of *Snel-lius* and *Riccioli's* Observations, great Errors may arise. Mr. *Picard* thinks all Angles less than 20 Degrees ought to be avoided; as also that the Triangles should be contrived so as to have Sides of a due Length, neither too great nor too small: Then follow 16 Triangles, wherein one or more of these Inconveniencies are to be found.

It may be said, the Whole of these Observations and Measures of Monsieur *Cassini* seem to be sufficiently confirmed, if not ascertained; since the principal Base in *Roussillon* was found, when computed, to differ but Three Toises from the same as it was actually measured; and that, after some due Corrections, it was made to agree with the greatest Exactness. Mr. *Celsius* replies, Why are we not told what

what those Corrections were, that we may see whether they were really necessary or no? Why were they not taken notice of in the Calculations of each Triangle? Besides, the real Length of the Base, or the fundamental Line, in *Roussillon*, is not fully ascertained, it not being measured more than once; whereas that at *Dunkirk* and that of Mr. *Picard* were measured twice; and there was more Reason for doing so here than at *Dunkirk*, on account of the uneven and almost ever changing Shore in *Roussillon*, from the restless overflowing Sea.

The great Number of the Triangles, joined with the numerous small Errors of the Angles, is another Ground of Uncertainty; for the Errors in the Angles, though small, may make the Distance of the Parallels of the Two extreme Places greater than it ought to be; and yet the principal Sides, that is, those that are made Bases to the following Triangles, continue the same. This made it necessary to verify the Sides, at least at every second Degree, by measuring the principal Base twice over with due Care; which might have been done, and therefore should have been done, in a Matter of so much Nicety as an Attempt to find the Difference between Two Degrees so near one another, under the same Meridian.

To shew what bad Consequences may arise from small Errors committed in observing the Angles of several Triangles, Mr. *Olavus Hiorter*, a curious and ingenious Friend of Mr. *Celsius*, has taken the Pains to form the Triangles of Mr. *Cassini* between *Bourges* and *Collioure*; so that the Distance between their Parallels shall be considerably lessened; and yet the Base in *Roussillon*, found by Computation, shall not, after due Correction, differ sensibly, if at all, from

the same actually measured. In consequence of this, Mr. *Celsius* concludes with observing, that the Distance between the Royal Observatory and the Perpendicular to the Meridian of *Collioure*, deduced from the Triangles of *Cassini*, corrected after Mr. *Hortor's* Method, &c. will amount to but 358,980 Toises. This, divided by the mean Difference of their Latitudes, $6^{\circ} 19' 11''$, will give 56,803 Toises, for the Length of a Degree, one with another, between *Paris* and *Collioure*, which is less than the Length of a mean Degree found by Mr. *Picard*, and pretty near the Truth: So that the Degrees decrease as you go towards the Equator; and consequently the Earth is higher at the Equator than at the Poles, as Sir *Isaac Newton* and Mr. *Huygens* believed.

The Distance of the Parallels of *Paris* and *Collioure* by this Method is indeed less than that computed by Mr. *Cassini*; but this cannot reasonably be complained of, since these computed Measures of Mr. *Cassini* seem very capable of being lessened; and it is no more than what Mr. *Cassini* himself hath done to the Measures published by his Father, which he has shortened by $325\frac{1}{2}$ Toises. But however that Matter be, whether this particular Correction of Mr. *Cassini's* Distance, and, consequently, Length of a mean Degree, be admitted or no, Mr. *Celsius* is fully persuaded, upon the Whole, that he hath made it plain to every unprejudiced Reader, that these Two Sets of Observations in *France* are not taken with such a Degree of Exactness as to be depended upon, in determining so nice a Matter, in Dispute for 50 Years, as the true Figure of the Earth; which was the thing proposed to be done by them.

II. *A Letter from Mr. J. Alexander to Peter Collinson, F. R. S. concerning a Place in New-York for measuring a Degree of Latitude.*

S I R, *New-York, July 21. 1740.*

THE Mention of the *French* Endeavours to discover the Figure of the Earth by Observation, puts me in Mind—That a very exact Observation for that Purpose might be made here, because *Hudson's* River here is frozen over from *New-York* up to *Albany*, and its Course is very strait, almost true North, and the Distance between *New-York* and *Albany* is above One hundred and Fifty Miles; *New-York* is in Latitude of $40^{\circ} 40'$, nearly; so that the Length of above Two Degrees of Latitude on the Earth might be measured here, with much more Exactness than it was possible in *England* or *France*, because of the Ascents and Descents, and curved Lines, which, I think, they would continually be obliged to make Allowances for.

From all which Difficulties the Mensuration here on the Ice would be intirely clear.

Yours,

J. Alexander.

III. *Ex-*

III. *Ex Veterum Prufforum RE ANTIQUA-
RIA Schediasma, a D. Jac. Theodoro Klein
Reipubl. Gedan. a Secretis, R. S. S. cum D.
Hans Sloane, Bar^o. R. S. Pr. communicatum.*

INTER varia Veterum Prufforum & Utensilium
& Luxus sui vel Ornamenti monumenta, olim
circa funerationes terræ concreta, rursus e terra
aliquando eruuntur ex *Ære* ductili affabre tornatæ
complicatæque triplices lineæ, quæ vel laxatum Cin-
gulum ex unico aut duplici triplicive circulo constan-
tem effingunt, vel Coni figuram e basi sua per Spi-
ram assurgentis, apice truncato repræsentant, cujus
Spiræ ex ære nobiliori tam egregia elasticitas, ut facile
comprimi & facilius in figuram conoidalem remitti
queat, ita quidem, ut plus quam quatuor horæ mi-
nuta requirat, antequam ab impulsu requiescat.

Prioris Generis de Historia naturali optime meritus
*Helwingius Præpos. Diæco. Algerb. Lithograph. I.
p. 91. §. 2.* Funiculos metallicos vocat, eosque Cin-
guli loco destinatos fuisse recte judicat; illos, qui in
Conum truncatum assurgunt, **CORONAS SEPUL-
CRALES** (*ibid. §. 4.*) appellat, in Sepulcretis ob-
vias, sed rarissime integras.

SIMILEM, ex omni parte integram & liberam ab
Ærugine, (*vid. TAB. I. Fig. 1.*) cujus Diameter in basi
 $6\frac{1}{2}$ pollices Rhinland. adæquat, anno 1726. in Museum
nostrum deposuit vir admodum Reverendus, simulque
Sententiam suam in literis ad me datis exposuit;
nimirum, hujusmodi Conum spiralem fuisse Simula-
crum Serpentis ænei Israelitarum, cum quibus veteres
Prussi

Prussi multa circa Cultus suos Idololaticos communia habuerint, quod Simulacrum demum loco Amuleti in Sepulcra deposuerint; ut Serpentes, quos Prussi veteres pro Diis coluerunt, post obitum quoque reverenter habiti, Cadaveribus non nocerent.

Proculdubio Vir Doctissimus hujus gratia secutus est Castra eorum, qui Virgines Veterum Prufforum ab Israelitis, quos *Salmanassar* in captivitatem abduxit, deducere satagunt; quorum tamen opinionem *Christoph. Hartznoch, Dissert. III. p. 48. §. v.* labefactavit.

Ast, licet verum sit, Prussos Ethnicos præter **DIOSTRES** majores, *Percunum, Pikollum, & Potrimpum*, imo præter Solem, Lunam, Stellam, Lucos, Fontes, Alcem, Bufones, aliasque Bestias, etiam Serpentes veneratos, eosdemque & cultu & litamine ex lacte parato, in Quercubus præcipue cavis, quæ inaudita plerumque magnitudinis & crassitiei, (*vid. Hartknoch Dissert. VI. de locis Divino cultui dicatis*) persecutos esse*; licet quoque egomet ipse in Lithuania Prussiarum orientali subjecta sæpius observaverim, Serpentes infantibus adeo familiares esse; ut iisdem cochlearia lacte plena circumverterint, quorum esurientium ejulatu Parentes accersiti nihil contra indiscretos hospites ausi sunt, nisi quod eosdem pariter ac volatilia domestica a prandiis liberorum fugaverint: nullatenus tamen exinde inferri poterit, Prussos antiquos hac ærea Spira respectum habuisse ad Serpentem æneum **MOÏSIS**; nisi nobis etiam persuadere velimus, quod

* Qua ratione, quibusve Cæremoniis epula Serpentibus sint parata, vid. *Hartkn. in Ant. & Nova Prussia, p. 63. Conf. Dissert. ejus VIII.*

respectu Afflictionum Ægyptiacarum, nimirum 2^{dæ}, 3^{tiæ}, 4^{tæ} & 8^{væ}, Bufones, Ranas, & plurima Insecta singulari cultu sint profecuti, eorundemque simulacra sibi comparasse. Neque verisimile est, ex veneratione erga Serpentes, quos, forsan ultra numerum, vivos in Ollis conservarunt, iisdemque valde familiares fuerunt, eorum insuper Idola, & multo minus Amuleta Serpentina fabricasse, ut Cadaveribus sepultis de Serpentibus caverent, cum extra dubium est, quod veteres Prussi mortuos non aliter quam crematos terræ mandaverint, cujusmodi funerationes temporibus Ducis *Alberti* necdum aboliri potuerunt, hinc Cadavera Amuletis contra injuriam Serpentum non indignisse. Cum, quod reliquum est, notissimum sit, veteres Prussos per Secula rudissimum fuisse populum, (*vid. Henneberger, in libr. de vet. Pruss. fol. 5.*) qui nec lanas ducere, nec ferrum, multo minus ænea vel utensilia vel luxui inservientia vasa parare didicerint, quin potius pro Hastis & Ensisibus Clavos horribiles, & pro malleis cuneisque ferreis lapides ad usus & domesticos & bellicos (qui diu sub nomine Cerauniarum venditati sunt) adaptaverint, nihilominus tamen non sine omni industria.

Summa itaque præmissorum hæc: Ceimelium nostrum nullam relationem ad simulacrum Serpentis ænei Moïsis habuisse, multo minus Probabilitatem Amuleti contra morsus Serpentum involvere; neque ex antiquissimis veterum Prussorum temporibus, sed ex medio potius eorum ævo originem trahere.

Unde autem Clarissimus *Helwingius* in *Alleg. §. 4.* adductus fuerit, ut vetus hoc monumentum titulo Coronæ Sepulcralis insigniverit, hariolari non possumus; quippe neque figura ullo modo Coronam
æmu-

æmulatur, nisi dicere velis, Philosophiam Prussorum, occasione funerationis, quæ causa quandoque maxime lugubris, prout mors homini naturali omnium terribilissimum est, Coronæ loco ejusmodi Spiram tremulam excogitasse; neque apud ullum Auctorem vestigium apparet, veteres Prussos mortuos suos cum ejusmodi Cæremoniis, quibus nos utimur, ad funerationes exportasse, ita ut Virginis vel Juvenis loculum Corona ornaverint; cum neque ullibi Coronæ Sepulcralis mentio sit facta, quæ in honorem Defuncti vel ad latus ejusdem, vel ad caput, vel etiam ad pedes, prout apud nos viris, qui in Coelibatu supremum obierunt diem, in Sepulchro deposita fuerit.

Sufficient autem sequentia, quæ Summam rituum funerandi Defunctos exhibent ex fide nostri *Hartknochii Dissert. XIII de Funeribus Vet. Pruss. p. 193. seq.* præter reliqua: “ Cum Defunctus erat tumulo
 “ inferendus, primo pyræ impositus comburebatur,
 “ deinde optimæ quoque vestes rogo injiciebantur, ut
 “ & canes venatici, equi, arma, aliaque defuncto,
 “ dum viveret, acceptissima.— Injiciebant quoque
 “ Annulos æreos, ut & Armillas æreas, præsertim si
 “ defunctus Christianorum sacra susceperat.” Et paulo post ex *Erasmo Stella*: “ Exutos spiritu, ar-
 “ matos vestitosque, ac magna Supellectilis parte
 “ circumposita, humarunt.”— Demum ex *Jacobi Leodiniensis* Privilegio Prussis dato: “ Promiserunt
 “ (Prussi) quod ipse & Hæredes eorum in mortuis
 “ comburendis vel subterrands cum equis, sive homi-
 “ nibus, vel cum armis seu vestibus, vel quibus-
 “ cunque aliis rebus pretiosis, vel etiam aliis quibus-
 “ cunque ritus gentilium de cætero non servabunt,
 “ sed

“ sed mortuos suos juxta morem Christianorum in
 “ Cœmeteriis sepelient.”

His omnibus rite perpensis, confidimus jure affirmare posse, Monumentum, de quo nobis sermo est, ad Supellectilem veterum Prussorum, & quidem Nobilioris Ordinis spectasse, & non nisi ARMILLAM, qua defunctus vel defuncta, sive in signum optime de Patria meritorum vel Nobilitatis, sive singularis ornatus luxusque gratia usi sunt, fuisse, quæ cum reliqua Supellectilis parte circum Cadaver posita tali modo in terram devenerit, donec nostris temporibus aliquando in lucem rursus extrahatur.

Confirmavit sententiam nostram Doctissimus *Thomas Bartholinus*, qui in *Schedio* suo de *Armillis Veterum*, pag. 48. figuram exhibet Armillæ Brachialis pluribus Circulis inter se connexis ex Museo Antiquario *Olai Wormii*, nunquam ab aliis annotatæ; quam pag. 49. stupendæ Antiquitatis monumentum, dignum posterorum memoria appellat. Quæ si cum nostro Ceimelio debite conferatur, nescio, quid obstarer, quo minus Nostram ARMILLAM BRACHIALEM veterum Prussorum, & quidem pari jure cum *Wormii* Armilla, stupendæ Antiquitatis nostræ monumentum, nunquam ab aliis annotatum dignumque Posterorum memoria salutaremus.

Structura ejus laxior insuper magisque fluens, quam *Wormii* Armillâ, est, ita ut super manica Brachii applicata Humerum Cubitumque simul commode stringere possit. Neque pro Plebeia, quod ærea est, censerî credas; cum aureas vel argenteas apud nos unquam effossas non audiverim. Ast Plebeiarum vestigia in hunc usque diem supersunt circa Ornatus
 utri-

utriusque Sexus in Lithuania Prussiæ orientalis, qui veri Prussorum veterum Descendentes, & Curlandiæ, qui diebus in primis solennibus Ossa Cubiti eadem plane figura, qua Armilla ærea apparet, arctius licet, vel ex serico & lana contextis fasciis (si ditiores sunt) vel Fimbriis variegatis cujuscunque Panni (si pauperiores) constringunt. Moris iisdem adhuc quoque est, Abdomine minimum duobus & quidem similibus cingulis vincto, superbire.

Propter affinitatem cum Armilla hac Brachiali veterum Prussorum, libet eidem adjungere Annulum ragentem, (*vid.* TAB. I. Fig. 2 & 3.) quem ante annum, & quod superfluxit temporis spatium, vir prænob. Dⁿ. *Andreas Lilienthal*, S. R. Maj. Boruss. a Consiliis Belli & Redituum, Cameræ Regalis, in Urna Prussica effossa inventum mihi tradidit, similibus lineis convolutis pro Gemma contextis, reliquo in duas extremitates sibi non conjunctas, sed invicem vicinas, circumque formantes, excurrentem, ita ut digitum plus minusve crassum æqualiter ambire possit.

Expedito hoc Schediasmate a Viro Consulari Gravissimo Dⁿ. *C. G. Ehlero* ex Diario, quod 1734. & 1735. Petropoli præfens per Filium suum conscribi curavit, & edocti sumus, Toreumata non nisi Aurea varii generis, ad nostrum Ceimelium æreum proxime accedentia in ditissimo totius Russiæ Autocraticis Antiquitatum Thesauro, ubi imprimis Sinensia asservantur, observata: Quæ Bibliothecarius Sacræ Imperialis Majestatis ex Sepulcretis, nescitur tamen cujus regionis, effossa esse & pro Armillis haberi affirmaverat.

IV. *Observations and Experiments with Madder-root, which has the Faculty of tinging the Bones of living Animals of a red Colour, by M. Du Hamel du Monceau, F. R. S. &c. communicated in a Letter to Sir Hans Sloane, Bart. Pr. R. S. Translated from the French by T. S. M. D. F. R. S.*

IN the Month of *February* 1737. (N. S.) Monsieur *Geoffroy* communicated to the *Royal Academy of Sciences at Paris*, the following Observation, extracted from a Letter of *Sir Hans Sloane*, Bart. President of the ROYAL SOCIETY of *London*, to him.

“ Mr. *Belchier*, a Surgeon, and Member of that
 “ SOCIETY, dining one Day with a Callicoe-printer,
 “ remarked that in a Leg of fresh Pork, the Flesh of
 “ which was well-tasted, the Bones were red. He
 “ asked the Cause of so singular a thing, and was
 “ told, that these Callicoe-printers make use of the
 “ *Rubia Tinctorum*, or *Madder-root*, to fix the Co-
 “ lours printed on the Cloth. Some of these Colours
 “ are made with Preparations from Iron, others with
 “ a Mixture of Alum and Sugar of Lead. The Parts
 “ printed with the Preparation of Iron, produce
 “ Black and Purple; those printed with the Mixture
 “ of Alum, Red of different Degrees. These Cal-
 “ licoes are afterwards boiled in a Copper with Bran,
 “ in order to clean them from a dirty red Colour
 “ occasioned by an Infusion of *Madder-root*. In
 “ fine, that this Bran should not be lost, though
 “ charged

“ charged with a red Colour, it is mixed with the
 “ usual Food of these Hogs; and this is what pro-
 “ duces this Effect on their Bones, without causing
 “ the least Alteration either in the Flefh, Membranes,
 “ Cartilages, or any other Parts of the Body. Mr.
 “ *Belchier*, whose Observation here related has been
 “ communicated to the Public in N^o 442. and 443.
 “ of the *Philosophical Transactions*, desiring to be
 “ assured whether the *Madder* alone, or all the In-
 “ gredients blended together, produced this Colour
 “ in the Bones, made some other Experiments.

“ He mixed some of the *Madder-root* with the
 “ Food with which he intended to feed a Cock.
 “ The Cock dying within 16 Days after his first
 “ feeding on the *Madder*, he dissected him, and was
 “ surpris’d to find, that the Root had produced its
 “ Effect in so small a time; for he found the Bones
 “ universally of a red Colour. Whence he con-
 “ cludes, that the *Madder* alone causes this Altera-
 “ tion; as he had not mixed either the Iron, Alum,
 “ or any of the other Ingredients of the Dyer, with
 “ the Cock’s Food. He remarked, that the red Co-
 “ lour penetrated into the internal Parts of the Bones,
 “ and that the hardest Bones took more of this Co-
 “ lour than the softer ones; excepting the enameled
 “ Part of the Teeth, which in the Hog retained its
 “ Whiteness.”

Mr. *Belchier* promises, at the Close of his Obser-
 vation, to try further Experiments, in order to know
 with Certainty, why this Change of Colour takes
 place only in the Bones. But as he has not published
 any thing more than what I have above related, I
 think myself at Liberty to communicate to the Public
 the

the Experiments I began to make in the Country, soon after the Observation came to my Knowledge. They are a Confirmation of it, and diminish nothing of the Merit of the first Discovery.

It is proper to observe, that *Mizaldus*, in a Work published in 1566. with this Title, *Memorabilium, utilium ac jucundorum Centuriæ novem*, (Cent. 7. No.) has these Words: “ Erythrodanum, vulgo
“ *Rubia tinctorum* dictum, ossa pecudum rubenti
“ & sandycino colore imbuat, si dies aliquot illud
“ depastæ sint oves, etiam intacta radice, quæ rutila
“ existit, &c.”

First, I took Four strong Pullets, which I shut up in Coops. I fed them with a Paste made of Wheat-meal and Powder of *Madder-root*; and gave them an Infusion of the same Root to drink, which I was in hopes they would have no Dislike to. The first Days they eat their Paste pretty well; but I found, that the Addition of the *Madder* rendered it much less agreeable to them than that made of the Meal alone, on which they fell with much greater Eagerness than on the other, when, to try their Relish, I now-and-then gave them some of it. As to the Infusion of the *Rubia Tinctorum*, they never would drink it, and I was obliged to give them pure Water, which they drank plentifully; for this Root made them thirsty. In short, at the End of some Days they could not relish the Mixture, of which they eat but very little, and wasted away visibly.

On the 10th Day, one of them died; and another Two Days after: and both of them had their Bones tinged of a Rose-colour. In order to prolong the Lives of the other Two, I diminished the Dose of the
the

the *Madder*, and from time to time I gave them the Paste without it. The Root had already produced its Effect; for notwithstanding the new Regiment, they continued to waste; which obliged me to kill the Third Five Days after the Death of the first Two. The Colour of its Bones was not different from that of the Two, who died Five Days before. As to the Fourth Pullet, which seemed not quite so sick, I marked it on the Leg with a Bit of Cloth tied round, and set it at Liberty. It recovered by degrees, by choosing Food to its Taste in the Yard. But at the same time the Tincture its Bones had received, went off gradually, and almost intirely disappeared in a Month's time. For I took care to observe the Change every second or third Day, by looking at the Bones on the Under-side of the Wing, which have no other Covering than a thin Skin.

From this Experiment, as from that of Mr. *Belchier's* Cock, it appears, that the *Madder-root* is alone sufficient to tinge the Bones of Animals red, which eat it. The Bones of my Pullets had taken no more than a Rose-colour; because these Creatures, being disgusted with their Food, never eat of it, but when urged by extreme Hunger: And I had never been able to tinge them of a fine red Colour, had I not repeated the Experiment on such Animals as I could feed with the Paste, and had it in my Power to make them swallow *Madder* in large Quantities.

For that Purpose I chose young Pigeons, the strongest of a whole Pigeon-house. Two of these had no other Food given them but Wheat-meal, others were fed with the Meal and *Madder* mixed
and

and made into Pellets of a convenient Size, given them Three times a Day till their Crops were full. I endeavoured to make the young Pigeons drink of the Infusion of *Madder*, which were fed with the Root and Meal; but I could never succeed, and was obliged to give them Water alone, as to the Pullets of the First Experiment. The Two young Pigeons fed with the Meal alone were lively and fat, digested their Food, and throve as well as if fed by the old ones. But on the contrary, those that were fed with the Paste of Meal and *Madder*, took this Food only by Force, digested ill, were dull and very thirsty. And though Care was taken to keep their Crop constantly full, as well as the others, yet they grew leaner daily. They were always shivering, and endeavouring to get into the Sun, or near the Fire, to warm themselves: And the strongest of them was very sick by the 10th Day. I got the Two killed, that had fed on the Meal alone, as well as the others that had the *Madder* given them; and I preserved but Two, which appeared to me to have better borne the Effect of it than the rest, and had the Bones of the Wings already tinged red.

One of the Two was intended to be recoverd by a simple Diet, in order to see, if, by prolonging its Life, the Colour, which was already very visible in the Wing-bones, would wear off: But in Three Days time it was killed accidentally. However, I thought I perceived the Colour weaker than before the Change of Diet: And the same Experiment, repeated some time after, confirmed me (in the Notion) that the Change of Food makes the Colour disappear by degrees. I continued to feed the other remaining
young

young Pigeon with *Madder*, but in small Quantities, for fear of killing it too soon. It lived Eight Days longer without any Appearance of the Bones being deeper coloured than the first that were killed.

All these Creatures, that had been fed with the Mixture, were dissected; and I made the following Observations on them.

Neither the Feathers, the Horn of the Bill, nor Claws, had changed their Colour, even where they are inserted into the Skin. The Skin of the whole Body had preserved its natural Colour. The Brain, Nerves, Muscles, Tendons, Cartilages, *Epiphyses*, and Membranes, afforded nothing to the Sight contrary to the usual State of these Parts. But the long bony Tendons, that run along the great Bone, which is improperly called the Leg of Fowls, were red about the Middle of their Length, which is their hardest Part. All the true Bones, even to the very thinnest of them, were as red as Carmine; and in some Places this Red was so deep, that they appeared almost black.

In these young Birds, all the Bones do not take the red Tinge alike. The hardest are generally more coloured than those that are tenderer. A Difference of this kind is perceivable even in the same Bone; for the Middle, which has more Solidity than the Ends, is almost always the reddest. Not but there are sometimes found little pale Spots in the Part where the Red is deepest; and sometimes Spots of a very deep Red in those Parts which have taken but a Carnation Tinge.

I have always found, that the great Bone of the Foot, which is commonly called the Bone of the

Leg, was visibly less red than the others. I have found the little Bones of the *Larynx* and of the *Apophyses* tinged of a fine Red, though these are as small as a Thread in young Pigeons. The Rings of the *Trachea*, which are intirely cartilaginous, had not taken the least Tinge; but the Ring nearest the Division of the *Trachea* was red in these Pigeons; and even the First Ring of each Branch of the Bifurcation had in several taken the Tincture, in the Middle at least of its Outside.

The other Parts of the Thorax, *viz.* The Heart, Lungs, *Mediastinum*, *Pleura*, and Diaphragm, remained of their natural Colour. There was nothing remarkable in the Liver, Spleen, Kidneys, nor on the Outside of the Gizzard; but the inner Membrane of the Crop and Intestines, especially the large ones, appeared red. Having washed Pieces of these Crops and Intestines, I found that their outer Membrane continued white, and that the inner, or *Tunica Villosa*, only was tinged by the *Madder*. At first Sight it appeared to me as if injected; but upon examining it with a Glass, I saw distinctly, that it was not a coloured Liquor that was contained in Vessels, as in Parts injected; but that it was only a sort of *Facula* detained in the villose Part of these Membranes. It is doubtless the Adhesion of these tinging Particles of the Root to the small *Villi* of the inner Membranes of the Organs of Digestion, that is the Source of all the Distempers with which these Creatures appeared to be seized, while I fed them with the *Madder*. Their Crop especially was relaxed and flabby, as if it had been macerated several Months in Water; it was easily torn; and its inner or villose Membrane ad-

adhered so little to the others, that it was detached from them in Pieces. It is very probable, that the coloured *Fæcula* detached from the *Madder*, that is, the Part of the Root which gives the Tincture, had obstructed the small Vessels and Glands of the Stomach, which might possibly occasion a *Sphacelus* therein. However that be, a certain Quantity of this *Fæcula*, being accumulated there, retarded Digestion, and those Animals died hectic, though with a full Stomach.

The Eyes of these Animals, while alive, seemed as red as those of some Parrots. I thought, after having dissected them, that no other Part was coloured but the *Capsula* of the Crystalline: But Monsieur *Morand*, to whom I had sent a Turkey fed with the *Madder*, observed that the vitreous *Capsula* was of a crimson Red, though neither the vitreous Humour nor the Crystalline were dyed: The Eye of this Turkey being larger than those of the Pigeons, the Hand that dissected it much more dexterous than mine, and the Anatomist more knowing, I willingly come into his Opinion. This then is the only soft Part, that is really tinged in these Animals; for I do not look on those Parts as such, which appear so only by their immediate Contact with those Parts that are charged with the Colour: Monsieur *Morand* having, in the Notes he sent me of his Observations, confirmed all that I had before observed, there ought to remain no Doubt of what I have here related.

I come to the Examination of the Skeletons, and of all the coloured osseous Parts of my Pigeons; in order to compare them with the Skeletons of the Two Pigeons fed with Wheat-meal alone without

the *Madder*. The Bones of the first were, as I have said above, of a very lively Carmine-red, in some Places of a Crimson; and I have some of them of the Colour of yellow Okre; but whence this Difference arose, I could not discover. These tinged Bones being broke, while fresh, or before drying in the Air, seemed to me somewhat bigger and fuller of Marrow; but also more spongy, or of a looser Texture, and easier to break, than the white Bones of the Pigeons fed with Meal only. The Parts of these Bones that had the least Degree of Hardness, broke between the Fingers, which remained coloured from them: And this Tincture does not come from the Marrow, which continues in its natural State, like all the other soft Parts. The same Parts in the white Bones were not to be broke in this manner.

If we recollect, that the Pigeons fed with the Mixture of Meal and *Madder* are always in a languishing Condition, in a continual Decay; it will be easy to judge, that this is the only Reason why the red Bones must be not so well formed, nor so hard, as the white Bones of the Pigeons fed with good Aliments. But why are they bigger, and, as it were, puffed up? It is hard to suppose any other Cause of this, but the Interposition of the colouring *Facula* of the *Madder* between the *Lamellæ* of the Bones. These heterogeneous Particles hinder the immediate Contact of these *Lamellæ*; and thence proceeds the preternatural Increase of their Size, and their little Solidity. Upon viewing these Bones with a good Glass, their smoothest Surface appears bored with a vast Number of small Holes, in which the colouring *Facula* is perceived. And with a Microscope that mag-

magnifies still more, there appears a sort of Net-work of Fibres, which divide, and reunite, to form this Net. Under the first Order of this Net-work, which appears white, another is seen somewhat red, and under this a Third and a Fourth, still deeper coloured: In fine, the Ground under all these reticular *Strata* is of a very deep Red; and the Whole may be justly enough compared to a Piece of Wood stripped of its Bark. It is probable, that this sort of Injection, made by the way of Digestion, might lead an able Anatomist to some very useful Discoveries on the Nature and Formation of the Bones. Nay, I think I have already found out something new on this Head; but, as I have still some Scruple remaining on my Observations, I will not venture to communicate the Consequences drawn from them.

In order that the *Madder* should produce the above related Effect on the Bones, its Tincture must have such a Degree of Fixity, (according to the Dyers Term) as not to be changed by the dissolving Action of the *Saliva*, of the Juice of the Stomach, of the pancreatic Juice, of the Bile, &c. nor by the peristaltic Motion of the Stomach and Intestines; and yet these Juices act so powerfully on common Aliments, that after Digestion they are not to be known either by their Smell, Taste or Colour. This is not all: These colouring Particles must be small enough to pass with the Chyle into the Blood, and circulate with it through a great Number of Strainers or Vessels, without being separated, and without being deposited either in the Liver, Spleen, or Pancreas.

I strongly suspect that Portion of the Lymph of the Blood, which is fit for nourishing the Bones, might
be

be the true Dissolyent of the Tincture of the *Madder*, and might convey it to the Place whither it carries Nourishment to the solid Parts of the Body of these Animals. In consequence of this Conjecture, which I shall resume in the Sequel, I thought that the Skeletons of young Animals ought to take a stronger and quicker Tincture, than those of full-grown Animals; because the Bones of young Animals are in a State of Growth, which requires a greater Quantity of osseous Juice. It is likewise true, as above said, that it is the hardest Bones of young Animals, that imbibe most of the Colour. All these Considerations gave Rise to a Difficulty, which was to be cleared up.

Wherefore, in the Beginning of last *October*, I chose Two Turkeys of the Year, the strongest I could find, and young Pigeons in their first Hair or Down. I could wish to have made the Experiment on Animals of the same Species; but it was impossible to find young Turkeys in their first Down at that Time of the Year: And besides, these Animals being extremely tender during the first Months, their Stomach would never have been able to bear the Effect of the *Madder*. As to old Pigeons, I had no tame ones: The wild are difficult to be fed with the Paste; and if they were suffered to feed at Discretion, they would not have been sufficiently *maddered*, if I may be allowed this Expression. However, the Bones of my Two Turkeys were very hard, in comparison of those of the young Pigeons: And thus I had in these Animals, though of different Kinds, all that was of Importance for my Experiment.

My

My young Pigeons, fed with the Paste mixed with *Madder*, died the Third Day; yet all that had the Consistence of Bone in their Skeletons, was become as red as Scarlet. Mr. *Belchier* was surpris'd to see the Bones of his Cock tinged red in Sixteen Days, and here are Bones so coloured in Three Days. But all that should in Course of Time have turned to Bone in One of my young Pigeons, and as yet was but Cartilage, as the *Epiphyses*, the great *Apo-physis* of the *Sternum*, &c. had not taken the least Colour. In the other, there were some Spots of a very weak red on the Cartilage of the *Sternum*, which probably began to ossify. Other Experiments, since tried, have taught me with greater Certainty, that the Cartilages in general are not tinged red by the *Madder*, but when they begin to acquire the Consistence of Bone.

If, as I suspect, it is the lymphatic Part of the Blood that is the *Menstruum* of the colouring Particles of the *Madder*; if this Lymph contains the nutritious Juice of the Cartilages and Bones; why does it not, in carrying with it the colouring Particles it has extracted from the Root, why does it not, I say, tinge the Cartilages as well as the Bones? In my Opinion this Difficulty cannot be solv'd but by the Difference of the Pores. In the Cartilages they are too large, the colouring Matter pass'es through them too easily, and finding no oss'eous *Laminae* yet formed, for want of a Surface sufficiently extended to retain it, it pass'es with the superabundant Lymph through the Pores of the Cartilages. When these Cartilages begin to take a proper Consistence, where there are *Strata* of oss'eous *Laminae* already formed, the Ob-
 stacle

stacle exists, the colouring *Fæcula* is detained and deposited there. When the ossifying Juice is no longer necessary for repairing a daily Loss of Substance, as in Animals arrived at their full Growth; besides that probably this Juice is then much less abundant, and consequently, in proportion, less charged with the colouring Parts of the Root; it must necessarily result thence, that the Bones of an adult Animal will be much weaker coloured. And this is what happened to my Two Turkeys, which, though fed for Fifteen Days with the Paste of Meal and *Madder*, had their Bones tinged but of a Rose Colour, which appeared to me somewhat deeper towards the Ends than the Middle, which, having too much Consistence, could not admit or retain the same Quantity of the colouring *Fæcula* as the tender Bones of the young Pigeons. Therefore the Bones of Animals that are still growing, are dyed better and quicker than those of full-grown Animals; and, in my Opinion, for the Reasons already given. My Two Turkeys had the same Ailments with the Pullets of the First Experiment, they fell into a Decay like those, and I was obliged to have them killed in Fifteen Days time.

Here we see young Pigeons, whose Bones were dyed of a fine Carmine-red in three Days; which is nearly the Time they must have for acquiring this Degree of Tincture. By other Experiments on young Pigeons of the same Age, I have found, that in Thirty-six Hours their Bones were of a lively Rose-colour, and in Twenty-four Hours they were at least of a Flesh-colour.

These

These last Experiments prove with what Expedition the Distribution of the nutritious Juice is performed in Animals of this kind, which acquire all their Growth in a few Months; and how rapid the Distribution is, even in those Parts where the Blood's Circulation meets with the greatest Obstacle, as in the Substance of the Bones.

As one ought likewise to infer from these Experiments, that there are vegetable Medicines whose chief Tendency is to the Bones, and which consequently might remedy many of their Distempers, I looked on myself obliged to employ the *Madder* with this View; but not having it in my Power to raise Diseases of different kinds in the Bones of my Animals, I confined myself to the Examination of what Effect it would have in a Fracture.

I chose Four very vigorous young Pigeons: A Thigh-bone of each of them was broke; the Reduction was immediately performed, and secured by a proper Bandage. Two of these Pigeons were fed with the Meal and *Madder*, and the other Two with the Meal alone. These last, notwithstanding the Pain the Fracture must have given them, had always a good Appetite, and in Eight Days they began to walk with their Dressing, which was a little loosened. The others fell into the Accidents already mentioned, and died, one on the Tenth, the other on the Fourteenth Day. The Two Pigeons that had recovered were killed, in order to compare the *Callus*.

That of the Pigeons which had not taken any *Madder*, was little, close, and very even: That of those fed with this Root, was large, spongy, and uneven: There shot out of it a sort of Vegetation: It

F f f

broke

broke between the Fingers, and crumbled into small Grains. It is true, that the State of Suffering of these Animals, occasioned by the Hurt, and increased by a Food improper for them, might retard the perfect Reunion of their Bones; yet I think, it certainly results from this Experiment, and others which I suppress, because they prove nothing more, that the *Rubia Tinctorum*, taken inwardly, is rather prejudicial than beneficial in the Case of Fractures; and it is not without its Use to know what is to be avoided.

The *Rubia*, probably, is not the only vegetable Substance that can change the Colour of the Bones; and yet I have tried the Log-wood, the *Anchusa* and *Curcuma*, without Success. In all Likelihood, it must be a Substance less susceptible of Alteration; and it is well known, that the *Rubia* is of that sort, seeing the Cloths dyed with this Root bear very well the Action of the Air, and that of boiling.

I have put the coloured Bones of my Animals to several Proofs: First, as Mr. *Belchier*, to that of boiling Water, and of Spirit of Wine, without the least Change of Colour. It also resisted Soap-suds. A strong Lixivium of Salt of Tartar discharged a little of the Colour, and made it look brighter. Vinegar made it take a yellowish brown and obscure Tinge. In fine, Alum-water discharged the Colour pretty considerably, and the Water remained somewhat vinous. Thus these Bones perfectly well resist the same Boilings as the Cloths dyed with the same Root; but the Air acts upon them much sooner than on these Cloths: For the Bones of the Pullets in the First Experiment, those of the Turkeys in the Third, and those of the young Pigeons, that had eat of the
Mad-

Madder but Three Days, became intirely white in less than a Year; and the reddest Bones lost much of their Colour. And I am of Opinion, that the Dew, to which I have exposed some of them for a few Days, will finish the Whitening of them.

As there is a sort of Analogy between the Nutrition of Animals and that of Vegetables, I have not neglected trying, if the Tincture of the *Rubia* would introduce itself into the Vessels of some Plants; which would, perhaps, contribute much to lay open their Organization.

For the First Experiment, in which indeed I had no Hopes of Success, I planted Two Bulbs of Tuberoses in Earth, with which I had mixed a good Quantity of *Madder*: But I found nothing, either in the Leaves, Stalk, or Flowers, but what was in the common and natural State. And this must have been so: For since it is only the Bones that take the Tincture in Animals, the Tuberoſe, having all its Parts soft, is in the State of an Animal without Bones: Such as a Leach, an Earth-worm, a Lamprey, which would probably continue in their natural State, whatever Quantity of *Madder* were given them, supposing it could possibly be done.

Wherefore I resolved to try the Experiment on a Tree. I planted a Paradise Apple-tree in a Box, which I had filled with Earth mixed with a great deal of *Madder*; and I covered the Upper Surface of the Earth with a Layer of *Madder* Two Inches thick. This Layer was renewed several times for near Two Years that my Tree is under the Experiment; but I have not as yet been able to examine if its Wood is coloured by this Root. In case the Experiment

does not succeed, as it is very likely it will not, it will serve at least to expose the Vanity and Uselessness of all those Receipts and Processes of Vegetation; that are to be found printed in *Mizaldus*, *Porta*, and other more modern foreign Compilers.

V. *A Catalogue of the FIFTY PLANTS from Chelsea Garden, presented to the ROYAL SOCIETY by the Company of Apothecaries, for the Year 1739. pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. & Soc. Reg. Præs. By Isaac Rand, Apothecary, F. R. S. Hort. Chel. Præs. ac Prælec. Botan.*

851. **A** Carna major; caule folioso. C. B. 379.
 852. **A**stragalus, perennis hirsutus, Alopecuroides; Galegæ foliis; floribus luteis.
 853. Bauhinia non aculeata; folio subrotundo, bicorni; floribus albis. D. Houston.
 854. Bidens Americana; flore albo radiato; foliis inferioribus trilobatis; superioribus subrotundis, acute crenatis.
 855. Bidens latifolia, hirsutior; semine angustiore, radiato. H. Elt. P. 51.
 856. Bidens scabra; flore niveo; folio Panduræformi. Ibid. P. 54.
 857. Bignonia Americana; Fraxini folio; flore amplo phœnicco. T. 164.

858. *Bignonia Americana*; *Fraxini folio*, minor; flore coccineo.
859. *Calendula*, minor, *arvensis*. H. L. Bat.
860. *Camara*, *Americana*; *Urticæ foliis*, latioribus, spinosa; floribus miniatis.
861. *Camara*, *Americana*; *Urticæ folio*; floribus miniatis.
862. *Camara*, *Americana*; *Urticæ foliis minoribus*; flore vario.
863. *Camara Americana*; foliis parvis subrotundis; floribus, foliolis interceptis.
864. *Camara Americana*; *salviæ foliis*, mucronatis; floribus luteis.
865. *Caprifolium Germanicum*. Dod. 411.
866. *Chenopodio-morus media*; foliis argute dentatis.
867. *Chenopodium*; *Botryos folio*, subtus candicante.
Atriplex sylvestris. II. Tab. Icon. 407.
868. *Chenopodium*; folio laciniato majus.
Pes Anserinus. Fuschii.
869. *Chenopodium*; folio sinuato, candicante. T. 506.
Atriplex sylvestris. Tab. Icon. 406.
870. *Chondrilla*; *Sonchi folio*; flore luteco-pallescente. T. 475.
871. *Cirsium*, *arvense repens*; folio vix sinuato, in aculeum abeunte.
872. *Cruciata nova*, *Romana*, minima, muralis. Col. Ecph. 295.
873. *Cynoglossum*, *Virginianum*; flore minimo, albo. Banisteri. Pluk. Alm. 126.
874. *Eryngium maritimum*. C. B. 386.

875. *Eryngium vulgare*. Ibid.
Eryngium campestre. Dod. 730.
876. *Jacobæa*; foliis ferulaceis.
877. *Lappa*; sive *Bardana major*; flore albo. H.
 Ox. III. 147.
878. *Lappa major*, ex omni parte minor; capitulis
 parvis eleganter reticulatis. Pluck. Alm.
879. *Lilac*; *Ligustri folio*. T. 62.
880. *Lyfimachia annua*, minima; *Polygoni folio*.
 T. 142.
881. *Mentha*, *angustifolia*, *spicata*. C. B. 227.
882. *Mentha*, *angustifolia altera*, *rugosior*; *spica hir-*
suta.
883. *Mentha*, *verticillata*; *longiori acuminato folio*;
odore aromatico.
884. *Oldenlandia*, *humilis*, *Hyssopifolia*. Plum.
 N. G. 42.
885. *Oxys*, *lutea*, *Americana*, *procumbens*.
886. *Pervinca vulgaris*, *angustifolia*. T. 120.
887. *Petasites major*, & *vulgaris*. C. B. 197.
888. *Petasites major*; *floribus albis*; *spica bipedali*.
 D. Bobart.
Petasites major; *floribus pediculis longis*
insidentibus. R. Syn. III. 179.
889. *Petasites minor*; *folio Tuffilaginis*. Mor. H.
 Reg. Bles.
890. *Phlomis*; *acuminato*, *viridi*; *splendente folio*;
flore purpurascente; *caulibus villosis*.
891. *Phlomis*, *Hispanica*, *fruticosa*, *candidissima*;
flore ferrugineo. T. 178.
892. *Phlomis Narbonensis*; *Hormini folio*; *flore*
purpurascente. Ibid.

893. *Phytolacca*; fructu monopyreno, majore; folio longiore, glabro.
894. *Pilofella*, major, umbellifera, macrocaulos. *Floribus est flosculosis*. Col. Ecph. 248.
895. *Rosa*; *Pimpinellæ* folio, Scotica; flore eleganter variegato.
Rosa Ciphiana. Sibald. Scot. Illust.
896. *Rosa sylvestris*, *Virginienfis*.
897. *Senecio Americanus*; folio hastato, nitide serrato.
898. *Sium umbellatum*, repens. Ger. Emac. 256.
899. *Solanum*, fruticosum, Africanum; Lauri foliis.
900. *Vulneraria erecta annua*; folio subrotundo leviter crenato.
Loto affinis, Coryli folio. Dod. Mem.

VI. *A Physico-mathematical Demonstration of the Impossibility and Insufficiency of Vortices: By M. de Sigorgne. Translated from the French by T. S. M. D. F. R. S.*

THAT natural Philosophers of an inferior Class, who consider only the Outside of Things, are obstinate in the Defence of *Vortices*, is, in my Opinion, not to be wondered at: The Idea of them strikes the Mind very agreeably at first, and even seems to promise the true Mechanism. But that Persons versed in the most profound Geometry, and in the most sublime Calculations, able Academicians, who incessantly apply themselves to the Study of
Na-

Nature, should plunge headlong into these Notions, and sustain the *Vortices pro aris & focis*, is to me Matter of unaccountable Surprize.

It appears to me, that a *Vortex* is as shocking upon mature Consideration, as its Idea is satisfactory upon the first mentioning it. And *Vortices*, in my Opinion, are like smart Sayings (*bons mots*), which charm the Mind the first, or, perhaps, the second time, but by a Repetition become quite flat and insipid. What Man, indeed, (if free from Prejudice, and that the Spirit of Party has not depraved his Judgment) would not be astonish'd to see brought on the Scene, not only *Vortices*, but such as are compos'd of an infinite Number of smaller *Vortices*, each of which still contains an innumerable Number of others subordinate to them? For Example: What is this Air, this Water, this Oil, &c. which Monsieur *de Moliere* takes pains to introduce? A Sport of the Imagination, or of the Mind, if you please; but in reality a Paper-building. It has been long since said, that according as *Vortices* shall be multiplied, they will degenerate into Littleness and Puerility: And these are the Sentiments even of the good *Cartesians* of our Days. But might it not be said, that the great *Vortices* having the same Origin with the little, the latter shew the Meanness of Extraction of the former? As Matter is divisible *in infinitum*; as to Vorticity, there is no Difference between the Great and the Small: And consequently, we have a Right to reject the large *Vortices*, since *Cartesians* proscribe the small.

It is on this Consideration that I am resolv'd to attack the *Vortices*: For I must own, to the Shame
of

of our Nation, that the Spirit of Party is so predominant therein, that several Persons, who by a close Study have found the Insufficiency of *Vortices* for explaining the *Phænomena* of the Heavens, yet have not dared to publish their Notions on that Subject.

But as at present the System of small *Vortices* is freely attacked, I think, as already said, that I have a Right to attack the large; and to this Purpose I hope to prove,

I. That the mechanical Formation of a *Vortex* is impossible.

II. That the *Vortex*, were it formed, cannot be of long Duration.

III. In fine, that it is not sufficient for explaining the *Phænomena*.

FIRST PART.

The mechanical Generation of the Vortex is impossible.

DEMONSTRATION.

In the Hypothesis of a perfect *Plenum*, GOD at first created Matter indefinite, uniform, homogeneous, and at Rest. This is allowed by all *Cartesians*, and follows in their Principles from this alone, that Matter was created at Rest. Now, from this perfect Homogeneity of Matter it evidently results, in my Opinion, that the *Vortex* cannot be mechanically formed. Suppose, say the *Cartesians*, that while Matter is as yet at Rest, GOD imprints a Motion in a strait Line on one of its Particles: This Particle will every Instant meet with Obstacles to the recti-

G g g

linear

linear Motion in the encompassing Matter; this Motion must therefore be turned aside, and will by this means become circular.

But why should the encompassing Matter, which is at Rest, be an Obstacle to the rectilinear Motion? Because, say they, it happens to be in the Line described by the Particle, on which Motion is supposed to be imprinted. But this very Reason would also prove, that the Body supposed to be in Motion could not circulate round a Centre at a Distance from it; because it would constantly meet with Matter at Rest in the Sides of the Polygon which it was to have described.

In a Word, it is a received Principle, that a Body which moves in a homogeneous Medium, never quits the Line of its first Direction: It does not refract, or deviate on one Side or the other of this Direction, except when it passes from an easier into a more difficult Medium, or from a denser into a less dense Medium: and even then its Direction must be oblique on the Surface of this Medium.

Now, the Body in Question would move in a Medium intirely homogeneous; seeing all the created Matter is supposed to be so, and that all but one Particle of this Matter is at Rest. It is moreover evident, that as all the Matter is uniform, every Direction, of what kind soever, of a Body which moves in the midst of this Matter, will be perpendicular to the Surface which corresponds to it; as is demonstrated in Mechanics. The supposed *Mobile* will therefore always move in the Line of its first Direction, until it has communicated all its Force; or rather it will
remain

remain at Rest after the least Shock, if Regard be had to nothing more than what I have hitherto said.

But there still remains a very important Remark to be made on this Subject, to wit, that as it is universally agreed at this Day, that Rest is not a Force, all this Matter created at Rest will be infinitely soft: Its Parts will have no Tenacity, no Connexion, no Viscosity; they will be but contiguous, and will not have more Adhesion to one another, than Two Globes which would touch out of the Bounds of the World without any reciprocal Attraction; since Tenacity, Viscosity, &c. are in the *Cartesian* System but the Effects of Compression every Way. Wherefore these Parts will be divided at the least Shock, in the same manner as if Quicksilver be thrown against a Wall, it is instantly seen to be divided into a Million of Parts, to be reflected on every Side, and be again divided as soon as it falls on the Floor. I know my Comparison is not exact, but the Advantage is on my Side; because Quicksilver is not without Viscosity, or a certain Tenacity between its Parts; whether it proceeds from Attraction, which is my Opinion, or that it be the Effect of the Pressure of the ambient Fluid. Therefore the *Cartesian* Matter will have more Facility to divide than Quicksilver, and will not be susceptible of any regular Motion; which alone demonstrates, that the mechanical Generation of the *Vortex* is impossible.

There is however this Difference between the *Vortex* imagined by *Descartes*, composed of hard Globules; and that of the infinitely soft Matter of Father *Malebranche*, whose System is revived by his Disciple Monsieur de *Molieres*; that if the *Cartesians*

admitted Gravity as a Principle; besides that it would give the true Cause of Hardness, its Combination with the strait or projectile Motion would produce a Motion in a Curve; as Sir *Isaac Newton* has demonstrated. But until they will return to this Idea of primitive Gravity, and further while they will make use of no other Matter than one infinitely soft, and really unintelligible, it will not be possible to conceive a single *Vortex* formed; far from having this infinite Number, which, by-the-bye, ought to be dissipated as Waves raised in the Water, upon account of their perfect Homogeneity.

The famous *Cartesians*, always refusing to allow this primitive Gravity, and at the same time plainly seeing, that this first Manner of forming the *Vortex* was impossible, have had recourse, in order to its Formation, to the Motion of Rotation of a solid Sphere at the Centre of a small Particle of Matter at Rest, &c. and they have pretended, that this Sphere in its Circulation ought to carry along with it the circumambient Matter.

But this Notion is certainly as unsustainable as the First. For,

1st, They must explain to us the mechanical Formation of this Sphere; they must account for its Solidity: But all this manifestly supposes the *Vortex* already formed; all this supposes a Pressure equal on every Side, uniform and concentric.

2^{dly}, This Sphere would never imprint an equal Velocity on all the Points of the concave Surface which touches and incloses it, seeing itself has not an equal Velocity in every Point of its last Surface; and therefore the *Vortex* would not have as much
Force

Force to defend itself towards the Poles, as towards the Equator; as we shall shew hereafter.

3dly, This Sphere, in striking against the ambient Matter, would but divide it *ad infinitum*; because it is infinitely soft, and that its Parts have no Adherence with each other.

4thly, It is not sufficient, that a Sphere turns round its Centre, to draw into its Circulation the ambient Matter: It is moreover requisite, that to press on this Matter in a Direction from the Centre to the Circumference, (which a solid Globe either cannot do, or can hardly be conceived possible for it to do) and further still, it is necessary there should be Unevennesses on this Sphere, and on the concave Surface of the ambient Matter; because otherwise, though the Sphere should press this Surface by its centrifugal Force, it would only raise it up, or tend to raise it, and it would slide along the Surface without dragging it away with it: On which Head there is this Particularity to be remarked, that, for the uniform Circulation and Conservation of the *Vortex*, and still more for the preserving of *Kepler's* Laws, the Spheres and Surfaces must be strictly Mathematical, as we shall soon see; and for its Formation they must be rough, and full of Unevennesses: But what can be more whimsical? And further, though these Surfaces were full of Prickles, yet could not the *Vortex* be formed in the Hypothesis of Father *Malebranche's* soft Matter; because the Parts which would form these Eminences and Unevennesses on the concave Surface of the Matter surrounding the Sphere, not being connected with the other Parts of the same Matter, would be carried off without Difficulty by the Rotation of
the

the Sphere; and the rest of the Matter would remain at Rest. And those who would pretend, that these Unevennesses, these Parts which form the Hillocks we are speaking of, could not, in consequence of GOD'S Decree, loose themselves from the other Parts of the Matter, would evidently abandon Mechanism, without reaping any Advantage: Because, supposing it true, that by this Means the ambient Matter would be compelled to circulate, yet could it not form a fluid *Vortex*, wherein *Kepler's* Laws could be observed; because both the Sphere and these Surfaces being by these Unevennesses wedged into each other by solid hard and inflexible Parts, they would necessarily move all of a Piece, as the Parts of a Sphere do.

stbly, By means of this Sphere one could have but a great *Vortex* formed; and not that infinite Multitude of small *Vortices*, with which the great ones are at this Day supposed to be filled, and in the Centre of all, or most Part of which, People will not allow that there are hard Globules, and so of the rest: For I am persuaded, that the Reader, by a little Meditation on this Subject, will find almost as many Reasons against this System, as there are small *Vortices* supposed to exist.

It may be objected, that we do not pretend to form a *Vortex*: We suppose that GOD formed it in the Beginning, and in Consequence hereof we account for its Properties and Conservation.

But, besides that the Impossibility of the mechanical Generation of a *Vortex* is a strong Prejudice against its Conservation; I pretend, in the Principles
of

of our Adversaries, GOD could not form a single *Vortex*.

I desire Attention may be given, that a circular Motion is a redoubled and forced Motion; and not, as Mr. *Perault* thought, a natural Motion. Now the rectilinear Motion cannot be redoubled thus, as against its Nature, in order to become circular, but upon a Supposition that it meets in the ambient Matter invincible Obstacles to its Direction; or that by a primitive Law it is carried towards a Centre by a Motion of Gravitation, at the same time that it receives a Motion in a strait Line. Therefore, since on one hand this universal and primordial Gravity is obstinately rejected; and on the other, as it is solidly proved above, that the ambient Matter is no Obstacle to the rectilinear Motion; it remains certain, that the Formation of the *Vortex* is impossible. Q. E. D.

SECOND and THIRD PART.

The Vortex, though once formed, cannot last, and it is not sufficient for explaining the celestial Phænomena.

POSTULATUM.

The cylindric *Vortex* cannot long subsist, and is not sufficient for explaining the celestial *Phænomena*: This Principle is allowed by all *Cartesians* in both its Parts. It cannot subsist; because not having Force to defend itself towards the Poles, if it happened to hit on that Side against another cylindric *Vortex*, that presented its Equator, it would soon be broke into,
and

and burst to its very Centre. If, on the contrary, its same Side touched another cylindric *Vortex* by the Poles, they would both mix together, and would compose but one *Vortex*.

It is not sufficient for explaining the celestial *Phænomena*; because it is allowed, that the translative Velocities of its Points cannot be in an inverted *Ratio* to the Roots of the Distances, and that its centrifugal Force does not diminish in the inverted *Ratio* of the Squares of these Distances, &c.

C O R O L L A R Y.

Therefore the spherical *Vortex*, in order to be of Use, must have other Properties than the cylindric: That is to say, it must have a relative Force to one and the same Centre; for it is by this Force alone that it can be different from the cylindric *Vortex*.

This Force, moreover, must be equal in all the Points of the same spherical Superficies; because otherwise it might be burst and broke into in its weak Parts, as well as the cylindric, &c.

T H E O R E M I.

Even in the spherical *Vortex* there is no relative Force to one and the same Centre: That is to say, that it has properly but an axifugal Force.

D E M O N S T R A T I O N.

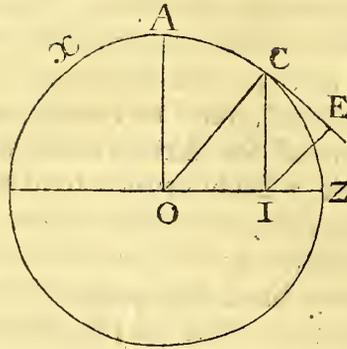
The spherical *Vortex* is composed, as well as the cylindrical, of several parallel Circles, but with this Difference, that in the spherical *Vortex* the *Radii* of the parallel Circles are not all equal, but on the contrary

trary diminish according as they recede from the Equator, and approach the Poles. Now it is manifest, that all the parallel Circles circulating round different Points of the Axis in the spherical *Vortex*, as well as in the cylindrical, tend to recede only from these different Points of the Axis, round which they circulate; because a Body cannot tend to recede from any Centre but that of its Circulation. In a Word, in order to make a *Vortex* spherical, which was cylindrical, they have but proportionally shortened the parallel Circles. But let the *Radius* of a Circle be ever so much shortened or lengthened, that will not change the Direction of its dilatative Effort. I am mistaken! an imaginary Line is going to change the Direction of the axifugal Force. This Force, as all agree, has for its Direction the *Radius* IC, in the Circumference whereof it is the *Radius*; but the Direction IC is oblique to CE the Tangent to the Sphere; therefore it changes, according to the general Law of an oblique Shock, into the Determination IE or OC relative to the Centre O.

But if Lines may be imagined, and that nothing more is requisite to realize them, than Points that correspond to them; we shall have some of all sorts in the *Vortex*: We shall have oblique Lines on the *Radius* OA, a perpendicular one, and some more or less oblique, on the *Radius* IC, and by that means we shall be able to determine nothing. Let us grant however, that there is a Tangent to the Sphere CE, at the Point C, and let us see if it will be a sufficient Reason for decomposing the centrifugal Force IC into a central Force IE or OC. For that Purpose I ask, What are the Points that compose this Tangent?

H h h

gent?



gent? It is evident, that it can only be the Globules of the upper *Stratum* that answer thereto. The Line CE is therefore composed only of a certain Number of Points separate one from the other, and which consequently can move one without the other. Therefore if the Line IC is perpendicular to the Globule that occupies the Point C, and that it passes through its Centre; there will be no Decomposure, and the Force IC will not change into a Force that has the *Radius* OC for its Direction.

Now it is infinitely probable, that the *Radius* IC passes through the Centre of the Globule C; and it is easy to demonstrate, that it is actually so even in the Principles of Monsieur *Saurin*, who first invented this central Decomposition. For what has been the Cause of the Decomposition of the circular Velocity into the centrifugal Force IC? It seems plain to me, that no other Cause can be assigned than the Point or Globule C; seeing there is but that one at the Point where it happened. The Line IC passes then through the Centre of the Globule C; since the
De-

Decomposition is always made in a perpendicular Line to the Point that caused it.

And indeed, either the *Radius* IC passes through the Centre of the Globule C, or the Centre of this Globule is on one Side or the other of this *Radius*, but so as that this *Radius* cuts the Point C; or else, it is a Space intercepted between Two Globules, which directly answers to the Point C. In the First Case, there is no Decomposition: In the Second, and in the Hypothesis, that the Centre of the Globule C happens to be between the *Radius* IC and the Equator, there will be a Decomposition; but it is manifest, that it will not be a central one: It will, on the contrary, be relative either to the very Pole, or to one of the polar Circles. In the Third Case, wherein it is supposed, that it is a Space intercepted between Two Globules, which answers to the Point C; there may be a Decomposition, but it will be double, the one relative to the Centre O, and the other relative to the Pole Z.

Now the *Cartesians* can never draw from this Decomposition the Advantage they propose; because there will not be more Reason for heavy Bodies precipitating to the Centre of the Sphere by means of the central Force, than to the very Pole by the Assistance of the polifugal Force; or rather, the Complication of these Two Forces will compel the *Mobile* to precipitate to the Centre I of the Parallel it happens to be in.

Wherefore, in order to defend the spherical *Vortex*, they must say, that the Centre of the Globule C is comprehended between the Poles and the *Radius* IC. But on what Foundation will they assure it?

What are the Proofs they will give for it? One must certainly be a very bold Gamester, to hazard this Point; because besides the Appearance of Truth, the Adversaries of *Vortexes* may wager Three to One, that it does not so happen. But in case it be allowed, will they ever find in the soft Matter of Father *Malebranche* and Monsieur *de Moliere*, a sufficient Cause of the Decomposition? There must be a Resistance to produce a Decomposition, and an infinitely soft Matter does not resist. And further, in the Hypothesis of the Decomposition of IC or OC, the *Vortex* would not be in Safety; because there would be a Remainder of the centrifugal Force IC, that would be parallel to the Tangent CE, and would evidently spread Confusion in the *Vortex*, by driving all the parallel Circles towards the Equator.

This seems to me sufficient to discredit, in the Minds of rational People free from Prejudice, this central Force, which is attempted by all means to be introduced. But let us not be tired of examining this Point thoroughly: It is of Consequence, and the *Cartesians* well deserve the Trouble of an abundant Refutation. Wherefore let us suppose, that GOD forms a *Vortex* cylindrical and fluid; it is a received and evident Principle, that its Points will have but an axifugal Force. And if a Sphere be conceived to be inscribed in this Cylinder, the Points that compose it, will not in like manner have any central centrifugal Force, according to the Axiom: *Nostrum intelligere nihil ponit in re*. Now let us realize this spherical *Vortex*, which before we had but conceived; that is, let us suppose, that GOD has destroyed the translative Velocity of the Points that form the

angular Spaces intercepted between the last Surface of the inscribed Sphere and that of the Cylinder; it is manifest, that no Change will happen in the Velocity and axifugal Force of the rest of the Points, which are not included in these; for this Reason, that the Points which fill the Two kinds of Basins that mark the Excess of the Cylinder above the inscribed Sphere, remain in the same Order, Disposition, and Direction, with regard to the inferior Points, which they were in at the time of their Motion. And there is no other Difference to be perceived herein, except that at present it is the same Point that constantly corresponds to the same Place; and that before this Place was successively occupied by Points intirely resembling each other, and that which remains or is supposed constantly to remain therein.

Now whether this Place be constantly occupied by one and the same Point, or successively by Points intirely alike and in the same Order, is what ought not to produce any Variation in the Effect which we are examining: And this appears to me at least as clear as Noon-day.

Wherefore, since these inferior Points had then but an axifugal Force, it follows that even now they have no other Tendency than to recede from the Centres of their Circulations, without having any Force relative to the Centre of the *Vortex*.

This is all that pure Reason dictates to me on this Point of the Nature of the *Vortex*, whether spherical or cylindrical: And I dare flatter myself, that whosoever will attentively examine my Reasonings, will find

find them as demonstrative as can be desired in Natural Philosophy.

In Effect, Experience agrees here with Reason. If a glass Globe filled with Water be rapidly turned on its Axis, one sees little Foulnesses; the small Atoms which it never fails to contain, gather together along the Axis, and form a little Cylinder round it.—Which very plainly shews, that in this spherical *Vortex* of Water there is but an axifugal Force.
Q. E. D.

C O R O L L A R Y.

Therefore Gravity is inexplicable in the *Vortex*, and it has not Strength to defend itself towards the Poles.

T H E O R E M II.

Supposing there was in the spherical *Vortex* a central Force according to the *Radius* OC , it could not by Reaction be changed into a centripetal Force according to the *Radius* CO .

This Proposition is well known to all who are somewhat conversant in Mechanics.

It is therein demonstrated, that if the *Radius* IC , for Example, forms with the Tangent CE an Angle of 45 Degrees, the Line of Reflexion will be parallel to the Axis; and that from the Point C to the Pole Z , the Lines of Reflexion will be divergent to the Axis; and, in fine, that from the Point C to the Equator, these same Lines of Reflexion will be indeed convergent to the Axis, but will never terminate at the Centre O : In a word, that because the Angle of Reflexion is always equal to the Angle
of

of Incidence, it is only at the Equator that the centrifugal Force can be changed into a centripetal Force.

Q. E. D.

C O R O L L A R Y I.

Therefore the modern *Cartesians* are strangely mistaken, when they pretend to account for Gravity by the Reverse of the central centrifugal Force.

C O R O L L A R Y II.

And they can never, *à fortiori*, in their Principles, explain the Figure of the Earth and of *Jupiter*, which are flatted Spheroids made by the Conversion of an Ellipsis upon its small Axis.

L E M M A I.

If the centrifugal Force represented by IC (see p. 420.) be decomposed on the spherical Tangent into a Force, that for its Direction has the Centre of the Sphere; the central Force, which results from this Decomposition, will be to the centrifugal Force, as the *Radius* IC to the *Radius* OC.

For the centrifugal Force IC, being decomposed into C on the Tangent of the Sphere, will strike this Tangent with a Force that will be represented by IE. But on account of the simular Triangles IEC, IOC; IE. IC :: IC. OC.

L E M M A II.

A Body which describes a Curve, strikes this Curve every time it passes from one Side to the other, with an infinitely small Force of the first kind with regard to its Velocity.

To

To the best of my Remembrance, this Proposition is demonstrated in Dr. *Clarke's* Notes on *Robault's Physica*, and in Monsieur *de Moliere's* Lectures: And it is evident from this alone, that it can only be by a Force represented by the Sine of the Angle of Contact that this moveable Body strikes the Tangent of its Curve.

T H E O R E M III.

Let us put Complaisance on the Stretch, and grant that *Vortexes* have a central and centripetal Force relative to one Centre O: I say, that the spherical *Vortex* will not have as much of this central Force, to defend itself towards the Poles, as towards the Equator.

C O N S T R U C T I O N.

Let us take, in the same Superficies X (see the Fig. p. 420.) Two Points at Pleasure, the Point A in the Circumference of the Equator, and the Point C in the Circumference of a subduple parallel Circle; we will give in the Demonstration an equal Velocity to the Globules which circulate in these Two Circumferences; which is the most favourable Concession imaginable for the Patrons of *Vortexes*.

D E M O N S T R A T I O N.

It is manifest, that if the Point A is in an equal Space of Time struck an equal Number of Times as the Point C, and that each Stroke against the Point A be double each Stroke against the Point C; it is manifest, I say, that there is more Force at the
Equator

Equator than at the parallel Circle. Now the Supposition is very certain in both its Parts: For,

1. Since the Circumference of the Equator is the double of that of the parallel Circle, and that being at an equal Distance from the Centre O, (see Fig. p. 420.) the Globules they contain are equal to each other; if there be a Thousand Globules in the Circumference of the Parallel, there will be Two thousand in the Circumference of the Equator. And as these Globules are supposed to have in both an equal Velocity, they will make (but) One Revolution in the Equator, while those of the subduple Circumference will make Two. Therefore, in both, there will be Two thousand Strokes employed in the same Space of Time, against the Points A and C.

2. Each central Stroke is double at the Equator: Because, as there is in both an equal Velocity, and that (LEM. II.) each centrifugal Stroke in every Circumference is a Fluxion of the first kind, with regard to the Velocity of the Globule which is in Motion; it follows that the centrifugal Strokes both in the Equator, and in the parallel Circle, are equal to each other. But the central Effort (which is the only one by which a *Vortex* can defend itself towards the Poles) is at the Point C (LEM. I.) but half the centrifugal Effort, since it is represented by IE subduple of IC; whereas at the Equator the central Effort is the same with the centrifugal Effort, because the *Radius* OA is perpendicular on the spirical Tangent, which corresponds to it. Therefore, &c.
Q. E. D.

COROLLARY I.

Therefore if a *Vortex* be in *Æquilibrium* with another *Vortex*, and that the Equator of one happens to answer to the Poles or Tropics of the other; the latter will be burst and penetrated to the Centre: And I do not think, that the *Cartesians* can find their Account in this Consequence.

COROLLARY II.

Therefore if the *Vortex* was the mechanical Cause of Gravity, Gravity ought to be greater at the Equator than at the Poles; and the Earth would be an oblong Spheroid; which is contrary to Observations.

REMARK.

I have said, that it was making a large Concession to the *Cartesians*, to suppose that the Globules of both the Circumferences have an equal Velocity. For if a Sphere full of Water be made to turn on its Centre, Experience teaches, that the Velocity is greater at the Equator than in the parallel Circles; since it is observed, that the Times of their periodical Revolutions are equal. Whence it follows, that I have, in my Demonstration, made the most favourable Supposition for the *Cartesians* that was possible.

THEOREM IV.

In order to determine the Tendency of a Layer towards the Upper Part of the *Vortex*, regard must be had not only to that which results from its own Circulation, but also to that which it receives from
the

the other lower Layers, unless it be the Layer next the Centre.

DEMONSTRATION.

While a Layer is in Circulation, it visibly makes a continual Effort towards dilating itself, by reason of the centrifugal Force, with which all its Parts endeavour to recede from the Centre of Circulation: But its actual Dilatation being impeded by the Layer next above it, this last will be naturally pressed by it. And thus it is that the first or lowest Layer, being put into Circulation, presses the Second; and the Second, assisted by the First, presses the Third; this, assisted by the Two preceding, presses the Fourth; and so on from Layer to Layer, through the whole Extent of the *Vortex*. Whence it follows, that in order to estimate the Quantity of Force with which a Layer tends towards the Surface of the *Vortex*, one must take the centrifugal Force proper to this Layer and that, which all the Matter of the Fluid contained under it acquires by Circulation. *Q. E. D.*

COROLLARY I.

Therefore the dilatative Effort of the Layers increases with the Layers in a greater Proportion than these Layers.

COROLLARY II.

Therefore it is impossible to explain in the *Vortex*, how Gravity decreases in an inverted *Ratio* of the Squares of the Distances; and consequently there will be nothing found in the *Vortex* to answer to

Attraction, whose Existence Sir *Isaac Newton* has so demonstratively established.

COROLLARY III.

Thus we have re-established in its full Light the Difficulty, which Monsieur *de Fontenelle* proposed to Monsieur *Villemot* in the Memoirs of the Academy for the Year 1705 *. This learned Academician pretends, that as in the *Vortex* the lower Points ought to move faster than the upper, in order to preserve *Kepler's* Astronomical Law; they ought also to have a greater centrifugal Force, and consequently compel them to descend, particularly in proportion to their Fluidity. The Objection made a great Noise, and the only Method found of getting rid of it, was by saying, that although each lower Point had more centrifugal Force than each upper; yet as the *Vortex* was in *Æquilibrium*, and the Sums of the Force of each of the Two Layers were equal, there was no Reason why the lower *Stratum* should get the better of the upper; because this was as prevalent by the Number of its Points, as that was by the Force of each of its own.

But it is manifest, after what has been demonstrated above, that the second Layer, being assisted by the first, must have a greater Force than the third, and consequently compel it to descend, pursuant to the Principle then granted to Monsieur *Fontenelle*.

But if it be asked, How could the upper Layer descend, seeing Matter is impenetrable?

* He afterwards published a Book, intituled, *Nouveau Systeme, ou nouvelle Explication du Mouvement des Planetes, par M. Philippe Villemot, Pretre, Docteur en Theologie, &c.* Lyon, 1707. in 12°.

I shall ask in my turn, How, in an intire *Plenum*, do heavy Bodies fall to the Centre? And I reason on the Principle granted to Monsieur *Fontenelle*.

But yet, because what is allowed by one *Cartesian* is not always allowed by all; let us suppose, that the upper Layer cannot descend; this, at least, will follow from my Demonstration, that, according to the Principles of all these Gentlemen, an upper Layer being pressed by all the under ones, it must hasten its Circulation, as long as it is slower than that of these under Layers; by reason that the Excess of their Velocities will act upon it, as if it had been at Rest.

C O R O L L A R Y IV.

Therefore the Layers of a *Vortex* will move, all of a Piece, as do those of a solid Sphere; and *Kepler's* Law cannot possibly be preserved. We shall now give other Proofs upon other Principles.

T H E O R E M V.

The Motion of the Points of the Equator is absolutely independent of the Motion of the parallel Circles; and consequently, in order to determine the *Equilibrium* of the Points of the Equator, we must attend to nothing but its Motion.

D E M O N S T R A T I O N.

The Plane of the Equator is parallel to the Planes of the other parallel Circles, that turn round the same Axe with it: Its centrifugal Force is perpendicular to the Tangent to the Sphere, which answers to it: It has not then any lateral Tendency towards these
parallel

parallel Circles, and by a necessary Consequence its Motion is absolutely independent of theirs.

And indeed, if it be supposed, that the Motion of the other parallel Circles stops, there is still some Motion conceived in the Equator, just as in the Case of the cylindrical *Vortex*: It is likewise conceivable, that the Velocity may be greater at the Equator than in the parallel Circles, as the Experiment already cited shews us: And if no Regard be had to the lateral Frictions, as the *Cartesians* would have it, who suppose them none or insensible, and as indeed they are obliged to say, that the *Vortex*, by the lateral Friction of the Equator, may not become cylindrical; this Equator will always continue to circulate uniformly, without communicating any of its Velocity to the Points that laterally surround it. Therefore, &c. *Q. E. D.*

C O R O L L A R Y I.

Therefore for the *Equilibrium* of the Points of the Equator, it is necessary, at least, that an upper Circumference should have as much Tendency towards the Superficies of the *Vortex*, as another under concentric Circumference; because, if it had less, there would be no *Equilibrium*, even in the Principles of the *Cartesians*; and the under Circumference, pressing the upper, would either make it descend, or communicate to it a Force equal to its own. Wherefore, calling *F* the proper centrifugal Force of a Point of the upper Circumference, and *f* that of a Point of the under one; if *S, s* mark the different Sums of the Points contained in these Two Circumferences, we shall have $FS = fs$.

C o-

COROLLARY II.

Therefore the centrifugal Force does not diminish in the Plane of the Equator in the inverted *Ratio* of the Squares of the Distances from the Centre; for since $FS = fs$; $F.f :: s.S$. But the Points being supposed equal on both Sides, their Sums are as the Circumferences, and one has $s.S :: d.D$, which gives $F.f :: d.D$. instead of $:: d d. D D$.

COROLLARY III.

Therefore *Kepler's* Rules cannot be observed in the *Vortex*, or at least in the Plane of its Equator; for since $F.f :: d.D$; by putting in the Place of F, f , their Values, we shall have $\frac{VV}{D} \cdot \frac{uu}{d} :: d.D$, and therefore $V = u$ and $D^2.d^2 :: TT.tt$. whereas we ought to have $V.u :: \sqrt[2]{d} \cdot \sqrt[2]{D}$ and $D^3.d^3 :: TT.tt$.

REMARK.

There is here a *Finesse* of the *Cartesians* to be observed. These Gentlemen consider only the *Equilibrium* of the spherical Layers of the *Vortex*, and from the Equality of their central Forces they deduce *Kepler's* Laws, as well as they can.

But it is manifest, that whatever becomes of the Equality of Force in different spherical Superficies of the *Vortex*, there must be an *Equilibrium* in the Plane of the Equator; because it is in this Plane that the Planets move; and if there had not actually been an *Equilibrium* between its Points, they would soon place themselves there, by reason that Fluids
always

always tend to the Side where they are less pressed; and it is by an actual *Æquilibrium* alone that they are kept in their Places; which intirely overturns the Theory of these Gentlemen.

Let us however grant to the *Cartesians*, that the Sums of the Forces of the Two spherical Surfaces are equal; I cannot see, that they can thence infer, as they do, that the central Force diminishes in a reciprocal *Ratio* of the Square of the Distance from the Centre. Let us examine their Argument:

$FS = fs$, say they; therefore $F.f :: s.S$; but s, S mark the Sums of the Points contained in the Two Surfaces; therefore they are as these Surfaces, which, being as the Squares of their Distances, give, $F.f :: d.d. D D$.

But it must be remarked, that the Surfaces of the *Vortex* are not Mathematical, they are Surfaces which have some Thickness: They cannot then be proportional to the Squares of their Distances from the Centre, except in the Case when their Thickness is equal. Now, according to the *Cartesians*, the Points or Globules, which compose the *Vortex*, increase in Bulk according as they recede from the Centre; and, besides, they are homogeneous, or of an equal specific Density, at least in their common System. And consequently it is certain, that the different natural or real *Strata* of the *Vortex* are not of an equal Thickness, and that the Matter contained therein is not proportionate to the Squares of the *Radii* of these Surfaces, but only to the Squares of these *Radii* multiplied by the Thickness of the *Strata*. Therefore, &c. *Q. E. D.*

COROLLARY IV.

Therefore, even allowing the *Cartesians*, what one has a Right (COROL. I. THEOR. IV.) to refuse them, they will never be able to explain *Kepler's* Rules in the *Vortex*; for it is only by the Proportion, which I have just now annulled, that they pretend to do it. See *M. de Moliere's Leçons de Physique*.

And if it be objected, that I have not, in the preceding Corollaries, had any regard to the Thickness of the Circumferences; I answer, that it was by way of pure Concession that I have not done it; and if any Person will be at the Pains of doing it, he will easily find, that *Kepler's* Rules will only be the more disturbed thereby.

CONCLUSION.

Therefore the *Vortex* is every way impossible, and insufficient in Natural Philosophy. Its mechanical Generation is impossible (PART I.); it has only an axifugal Force, and not a centrifugal and centripetal Force, as it should have (THEOR. I. and II.); and even if it had, it cannot (THEOR. III.) defend itself equally on all Sides. It is not sufficient for explaining Gravity, and its Properties; it destroys *Kepler's* Astronomical Laws (COROL. III. THEOR. IV. and V.). What more can be desired, in order to conclude with *Sir Isaac Newton*? “Itaque hypothesis Vorticum “ (est impossibile &) cum phænomenis astronomicis “ omnino pugnat, & non tam ad explicandos quam “ ad perturbandos motus cœlestes conducit.” *Q. E. D.*

VII. *An Account by David Hartley, M. B. F. R. S. of Dr. Trew's Dissertation concerning the Differences of a Human Body before and after Birth, intituled, Diss. epistolica de differentiis quibusdam inter hominem natum & nascendum intervenientibus, deque vestigiis Divini Numinis inde colligendis. Jo. Georgio Kramero inscripta. Cum Tab. Æn. Autore Christoph. Jacobo Trew, Noribergiae, 1736. 4^{to}.*

THERE are, according to Dr. *Trew*, Two remarkable Observations, which Animal Bodies suggest, *1st*, That the same general Ends are accomplished in different Animals by all the possible Varieties of Means. *2^{dly}*, That Animal Bodies are Machines, which produce in themselves all those Changes, that are necessary for their Preservation and Well-being. Thus the same general Ends of Chylification, Circulation, Secretion of Bile, &c. are accomplished in different Animals by Organs that differ considerably from each other; and in the same Animal the Body of the *Fetus* is very different in its Structure from that of the Adult, at the same time that this Difference is effected by the Body itself, each subsequent Variation, the natural and mechanical Consequence of that which immediately preceded, and the Whole conducted in the best possible manner for the Welfare and Happiness of the Animal.

The

The Author's Design in this Dissertation is to consider those Differences of a Human Body before and after Birth, which affect the Circulation of the Blood. And for this Purpose he has given us 78 very curious and accurate Figures of the Parts relating thereto, such as the Heart, and Trunks of the great Blood-vessels, the Liver, the *Vena Portarum*, the Umbilical Chord, &c. subjoining to them a very minute and precise Explanation of each. Some of these Figures represent the Parts as they appeared immediately upon Dissection, others as inflated and dried, others again as injected with Wax; and lastly, others as having been first injected, and well dried, then cleared of the Injection, and laid open, in order to shew the several Cavities and Valves in their natural Dimensions and Positions. This last Method he prefers to all the rest, and observes, with relation to it, that the Injection must not be thrown in too hot, and that the internal Parts of the Preparation must be perfectly dry before we attempt to evacuate it; inasmuch as a Neglect of either of these Cautions would make the Valves, and their Membranes, shrivel up and contract themselves from their natural Sizes and Positions. The Manner of doing it is to suspend the Preparation in a proper Vessel placed in a gentle Heat, having first made an Aperture in the most depending Part, for the Injection to run out at.

From these Figures, with their Explanations, our Author draws the following Anatomical and Physiological Conclusions.

1st, That, contrary to *Casseri*'s Figure, the Umbilical Vein enters the Liver towards the Left Part of it.

2dly, That the Sulcus of the Liver, through which the Umbilical Vein passes, is not always the same. In some Subjects it surrounds the Vein along its whole Passage, in others only in Part of its Passage, and in others it is an imperfect Chanel, which merely receives the Vein.

3dly, There is but one Umbilical Vein, it empties itself into the Left Extremity of the *Sinus Venæ Portarum*, and sends no Branches to the Liver.

4thly, The Communication between the Umbilical Vein, and the *Sinus Venæ Portarum*, is so free, that the Blood has no Obstacle in passing either Way. Our Author asks therefore, What is the Cause of the Blood's Motion from the Umbilical Vein into the Liver, and whether the Pulsation of the Umbilical Arteries be one sufficient to produce this Effect?

5thly, The *Venæ Portarum* sends no Branches to the Liver, but opens into a particular *Sinus*, called *Sinus Venæ Portarum*; and this Opening is nearer to the Right Extremity of the *Sinus* than to the Left.

6thly, The Diameter of the *Venæ Portarum* is much less than that of the Umbilical Vein. The Diameter of the Left Part of the *Sinus Venæ Portarum* is generally larger than both these together, never much less than that of the Umbilical Vein; and the Diameter of the *Canalis Venosus* is least of all. The Blood therefore of the *Venæ Portarum* mixes with that of the Umbilical Vein in the *Sinus*. And since the Blood of the Umbilical Vein, which abounds with chylous Particles, does thus mix with that of the *Venæ Portarum* in the *Fœtus*, it may be asked, Whether in Adults the Branches of the *Venæ Portarum*, which arise from the Stomach and Intestines,

restines, do not suck up some chylous Parts from the Aliment? And whether both in the *Fœtus*, and in the Adult, Chyle be not a necessary Ingredient in the Composition of Bile? It is certain, that the Chyle passes into the *Vena Portarum* in Birds.

7thly, The *Canalis Venosus* empties itself into the *Cava Inferior*, where the Three Veins arising from the Liver empty themselves.

8thly, The *Valves* which are placed at the Two Extremities of the *Canalis Venosus*, facilitate the Ascent of the Blood in it, and also contribute to close it after Birth.

9thly, The *Valve* of the Coronary Vein is nothing else but its external Coat, something elongated within the Cavity of the Right *Auricle*; and its Use is to close the Orifice of this Vein when the *Auricle* is distended with Blood, just as the nervous Coat of the Bladder closes the Orifices of the Ureters when the Bladder is distended with Urine.

10thly, *Eustachius's Valve* is found both in the *Fœtus*, and in the Adult; and its Use seems to be, to direct the Blood's Motion variously, according to the various Circumstances of the Right *Auricle*, during its *Diastole* and *Systole*; and principally to hinder the Regress of the Blood into the *Cava Inferior*, when the *Auricle* is contracted.

11thly, The Use of the *Foramen Ovale*, and *Canalis Arteriosus*, seems to be, to intercept Part of the venal Blood, and transmit it to the Left *Auricle* and *Aorta*, that so the Whole be not forced upon the Lungs during their State of Inactivity in the *Fœtus*; of the Membrane, which is placed before the *Foramen Ovale*, to direct the Communication of the
Auricles

Auricles before Birth, and prevent it afterwards; and lastly, of the valvulous Productions at the Two Extremities of the *Canalis Arteriosus*, in like manner to direct the Blood in its Motion through this Canal before Birth, and to exclude it afterwards. Here our Author enters into a very minute Examination of Monsieur *Mery's* Hypothesis, but does not agree to it; affirming, that the Membrane of the *Foramen Ovale* is so placed as to permit the Blood to pass freely from the Right *Auricle* to the Left, during the *Diastole* of the *Auricles*, but never from the Left *Auricle* to the Right.

12thly, The Use of the *Urachus* in the human *Fœtus* is not yet discovered. Our Author here supposes, according to the Determination of the best Anatomists, that the human *Fœtus* has no *Allantois*.

13thly, The Situation of the Stomach in the *Fœtus* is such, as makes up for the want of Action in the *Diaphragm*, as far as relates to Digestion. For as in the Adult, the Action of the *Diaphragm* facilitates the Descent of the Aliment, so in the *Fœtus* the *Cardia* is made to rise above the *Pylorus* more than in the Adult, from its Connexion with the *Diaphragm*, for the same Purpose.

14thly, The Smallness of the Stomach in new-born Children shews, that it ought not to be oppressed either with much Aliment at once, or with such as is gross.

15thly, The Descent of the *Testicles* into the *Scrotum* does not always happen at the same time.

16thly, The recurrent Nerve seems to be some way subservient to the *Canalis Arteriosus*. This our Author conjectures from its passing round the *Aorta* just

just where this receives the *Canalis Arteriosus*; but observes, that the Knowledge of the Use and Action of the Nervous System is much more imperfect than any other Branch of the Animal Oeconomy.

There is a short Dissertation (with Four Figures of the Tongue, its Vessels, Glands, Muscles, and Nerves annexed) by the same Author; whose principal Intent is to shew, that the Vessels called salival Duets by Coschwitzius, are not salival Duets, but Veins.

VIII. *Some curious Experiments and Observations on a Beetle, that lived Three Years without Food: Communicated to the ROYAL SOCIETY in a Letter from Mr. Henry Baker to Alexander Stuart, M. D. F. R. S.*

S I R,

WHEN I had the Honour of waiting on you lately, and was mentioning, in Conversation, the uncommon and surprizing Strength of Life bestowed by Providence on a certain *English* Insect, called by *Petiver*, *Scarabæus impennis tardipes*, the slow-legged Beetle. *Moff.* 139. *Fig. id. Angl.* 999. *Gaz.* Decad. 3^e, Tab. xxiv. 7. (whose Natural History, as to that Particular, has never, I believe, been touched on) you was pleased to think it so extraordinary, as to desire I would write down the Observations I had made, since the Accidents that led me into them may perhaps never again occur: And you seemed of Opinion, that their being known may

may open a Way to more amazing Discoveries, and tend considerably towards enlarging our Notions of Animal Life in general. — In Obedience therefore to your Commands, I presume to lay before you a Series of plain Facts, without troubling you with my own Reflections, or endeavouring to set them off with any other Ornament than Truth.

In the Middle of the Month of *June* 1737. I happened to be at a Relation's House at *Tottenham* in the County of *Middlesex*; and whilst I was there, a large Cistern of Lead, that was placed in the Coach-house-yard, to receive by Pipes the Rain-water from some Out-buildings, fell down, through the Failure of a wooden Frame whereon it stood. My Curiosity led me to examine into this Cistern; and at the Bottom of it, I observed several black *Beetles*, plunging in a muddy slimy Sediment, which the Water had left. Taking out Two or Three of them, I found them of a middling Size, somewhat above an Inch in Length, having Six pretty long Legs, with Two little Hooks at the Extremity of each, in the Manner of the common *Beetles*: They were all over of a rusty black Colour, with *Antenna* long and jointed; a Body covered with one strong Shell, forming an Appearance of Case-wings, but undivided, and without any filmy Wings underneath, and a Tail turning up a little: In short, they resemble very much a Sort of *Beetle* that is sometimes seen in Houses, but were of a stronger and much more firm Contexture.

As I have preserved most of our *English* Insects, (after a Manner I shall not here take up your Time in describing) I chose one of the largest of these *Beetles*, and threw it into a Cup full of common *Lamp-spirits*,
(that

(that being the Way of killing and preparing them for my Purpose) and in a few Minutes it appeared to be quite dead: Whereupon I shut it up in a round Pill-box of about an Inch and half Diameter, and carried it in my Pocket next Day to *London*, where I tossed it into a Drawer, and thought no more of it for above Two Months after; when, opening the Box, I found it, to my great Surprize, alive and vigorous; though it had nothing to eat for all that Time, nor received any more Air than what could be met with in so small a Box, whose Cover shut very close. Having, however, no Intention of keeping it alive, I again plunged it into *Spirit of Wine*, and let it lie considerably longer than the First time, till supposing it dead beyond any Possibility of Recovery, I put it into the said Box again, and lockèd it in my Drawer, without looking any more at it for a Month at least, when I found it again alive.——And now I began to imagine there must be somewhat extraordinary in this Creature, since it could survive the Force of *Spirit of Wine*, which soon kills most other Insects, and live for Three Months, without taking in any Sustenance.

A few Days before this, a Friend had sent me Three or Four *Cock-Roches*, or as *Merian* calls them, *Kakkerlacæ*, brought alive from the *West-Indies*: These I had placed under a large Glàs of Six or Seven Inches Diameter, made on purpose to observe the Transformation of *Caterpillers*: And now I put my *Beetle* amongst them, that he might enjoy a greater Share of Liberty than he had done for Three Months before. I fed them with green *Ginger* moistened in *Water*, and they eat it greedily; but I

could not find, nor do I believe, that the *Beetle* ever tasted it during the whole Five Weeks they lived under the Glass together. I often took notice, that the *Cock-Roches* would avoid the *Beetle*, and seem frightened at his Approach; but never observed any Tokens of his Liking or Dislike of them, for he usually stalked along, without regarding whether they came in his Way or not. Perceiving the *Cock-Roches* begin to decline in Vigour, I was afraid they would lose much of their Beauty, if I permitted them to die of Sickness, and would become unfit to be preserved as I proposed: Wherefore I put them into *Spirit of Wine*, and the *Beetle* their Companion with them. They appeared dead in a few Minutes, and I believe were really so: The *Beetle* seemed likewise in the same Condition: Whereupon, after they had lain in *Spirits* about an Hour, I took them out, and whelmed the Glass over them, till I should have Leisure to dispose of them as I intended. This was about Ten o'Clock in the Morning, and I saw them no more till Evening, but found the *Beetle* then creeping about as strong and vigorous as ever: And therefore I resolved to put him to a Trial I imagined he could not possible survive, which was to let him remain a whole Night in *Spirits*; but here too I found myself mistaken, for after he had been taken out a Day, he appeared as lively as if nothing had happened to him.

Since that time I have put him no more in *Spirits*, but have kept him under the Glass afore-mentioned, where he is alive at present: Though during the Two Years and half he has been in my Possession, I have never been able to discover, that he has drank or eaten any thing.

I must not conceal, however, that, by way of Experiment, I have put under his Glass, at different times, Water, Bread, Fruits, &c. but I never found them in the least diminished or touched by him. These Trials too were always made at many Months asunder, and I am pretty certain, there has been at least a Year together, during some Part of the aforesaid Time, wherein nothing has been offered him either to eat or drink.

The Question will then be, How this Creature has been wonderfully kept alive for Two Years and an half, without taking any visible Food? —And, Sir, your Supposition, that it finds its Nourishment in the Air, carries with it the highest Probability: Since, as you was pleased to observe, there are Particles in the Air which evidently supply a Growth to Plants of some particular Kinds, such as the *Sempervive*, *Orpine*, *House-leek*, &c. And the same or some other Particles therein contained may possibly be likewise able to afford a Nourishment to Animals of some certain Kinds.—There is a farther Reason also to believe, that something like this must be; for, in the amazing Plan of Nature, the *Animal*, *Vegetable*, and *Mineral* Kingdoms are not separated each from other by wide Distances, or broken off by sudden Starts, but differ from each other (near their Boundaries) by such minute and insensible Degrees, that it is impossible to find out certainly where the one begins, or where the other ends.—As the Air, therefore, yields Nourishment to some Kinds of Plants, it may probably do the same to some Kinds of Animals; for otherwise a Link would seem wanting in the mighty Chain of Beings.—And that *Chameleons*, *Lizards*, *Snakes*,

&c. can live for Months together without any visible Sustenance, is a Fact generally allowed to be true; the Cause of it too has been attributed to an exceeding slow Digestion, Circulation, and Distribution of Nourishment, in those Creatures; but as their Agility seems to imply a brisk Motion of their *animal Spirits*, I am inclined to think the Circulation of their other Fluids cannot be so sluggish as commonly is supposed: And, perhaps, it may not be unreasonable to believe, that their being able to live so long without visible Food, is rather owing to some other Nourishment they receive from the Air, which supplies the want of more substantial Diet.

I have met with no Instance I could give Credit to, of any Creature's living without Food for so long a time as the *Beetle* I have been mentioning; and yet I doubt not, (though it may have been kept alive by Air only) but that, in its natural State, it eats more solid Food; after somewhat the same manner as the Plants before-named thrive best when set in a little Earth, notwithstanding they may flourish a long while, and send forth Branches and Flowers, when they are suspended in the Air, and receive no Nourishment but from the Humidity or some other Qualities thereof.

We have not, indeed, as yet, many Instances of this Sort in Animals; nor is it probable any of the larger Kinds can live long without Supplies of Food: But there may be several Insects capable of subsisting on minute Particles carried about every-where with the Air, though, for want of sufficient Experiments, we are not acquainted with them.

Its reviving so often after being seemingly killed by *Spirit of Wine*, shews a Strength of Life I never found in any other Insect: Some Kinds, indeed, will come to Life again, if taken out as soon as they appear dead; and the *Ear-wig*, in particular, after continuing so some Minutes: But half an Hour in *Spirits* puts a final End to the Life of all the *Insects* I ever tried, except this *Beetle*.

It walks not much about under the Glass that covers it, but is usually found with its Nose thrust close down to the Bottom thereof, perhaps to suck in the Air. On removing the Glass, it appears robust and vigorous, and would willingly run away. A strong aromatic kind of Smell issues from it, agreeable enough when there is not too much of it; and the same Scent hangs about the Fingers a long while after touching it.—Since the Weather has been so excessive cold, it is grown somewhat torpid; but till now has always appeared as lively in cold as in hot Weather, and I have observed its Smell to be stronger in Winter than in Summer. In the exhausted Receiver, where I have kept it sometimes for half an Hour, it seems perfectly unconcerned, walking about *in Vacuo* as briskly as in the open Air; but, upon Admission of the Air, it shrinks its Legs together, and appears in a Surprise for near a Minute.

We know the *Egyptians* had a high Veneration for the *Beetle*, by their many Images thereof, which are still preserved in the Cabinets of the Curious, and Historians tell us it was one of their Deities: But, as the *Egyptians* were a wise and learned People, we cannot imagine they would shew so much Regard to a Creature of such a mean Appearance, without some extraordinary Reason for so doing:

And

And is it not possible they might have discovered its being able to subsist a very long time without any visible Sustainance, and therefore have made it a Symbol of the Deity? In the same Manner as it is probable the *Onion* was held sacred by them, for representing the *Orbits* of the *Planets*.—But these Conjectures may seem impertinent to one so used to curious Disquisitions, and therefore I shall not dare intrude on your Time any longer, than to profess myself, with the greatest Respect,

S I R,

Your most humble Servant,

Strand, Jan. 2. 1739-40.

H. Baker.

P. S. This *Beetle* (after being kept half a Year longer) was permitted to get away, by the Carelessness of a Servant, who took down the Glass to wipe it.

See the Figure of this Insect, in TAB. II. Fig. I.

IX. *The Discovery of a perfect Plant in Semine; by Mr. Henry Baker.*

SINCE the ancient Supposition of *equivocal Generation* has been rejected, for a more reasonable Belief, that every Thing proceeds from Parents of its own Kind, Numbers of curious People have busied themselves in Search of Experiments, whereby to demonstrate the Truth of the latter, and consequently the Falsity of the former Opinion. For this Purpose the *Animal* and *Vegetable* Worlds have been examined, and such Analogy found between them, as proves convincingly, that their *Generation* and *Increase* are brought

brought about in a Manner pretty much alike. The animal and vegetable *Semina* are found to be alike the *Rudiments* of their future Offspring; and both alike require only a proper Repository to preserve them from Injuries, and proper Juices to advance their Growth, and bring them to Perfection.

Glasses (which the Moderns have so much improved) are the Means whereby these Secrets in Nature are discovered to us. The Eye, assisted by a good Microscope, can distinguish plainly, in the *Semen masculinum* of Animals, Myriads of *Animalcules* alive and vigorous, though so exceedingly minute, that it is computed three thousand Millions of them are not equal to a Grain of Sand, whose Diameter is but the one hundredth Part of an Inch: And the same Instrument will inform us beyond all Doubt, that the *Farinae* of Vegetables are nothing else but a Congeries of minute *Granula*, whose Shapes are constant and uniform as the Plants they are taken from. And as the Seeds of Plants are found by repeated Experiments to be unprolific, if the *Farinae* be not permitted to shed, it has been supposed, that all its *Granula* contain *Seminal Plants* of their own Kind.

The *Growth* of Animals and Vegetables seems to be nothing else but a gradual Unfolding and Expansion of their Vessels by a slow and progressive Insinuation of Fluids adapted to their Diameters, until, being stretched to the utmost Bounds allotted them by Providence at their Formation, they reach their State of Perfection, or, in other Words, arrive at their full Growth.—If this be granted, the Consequence must be, that all the Members of a perfect Animal *exist* really in every *Animalcule* of the *Semen animale*.

animale masculinum, and all the Parts of a perfect Plant in every *little Grain* of the *Farina Plantarum*, however minute either of them may be.

According to this *Theory*, it is supposed by some, that, in *Animals*, the *Semen* of the Male being received into the *Matrix* of the Female, some of the *Animalcules* it contains in such Abundance, find an Entrance into the *Ovaria*, and lodge themselves in some of the *Ova* placed there by Providence as a proper *Nidus* for them. An *Ovum*, becoming thus inhabited by an *Animalcule*, gets loosened in due time from its *Ovarium*, and passes into the *Matrix* through one of the *Fallopian Tubes*. The Veins and Arteries that fastened it to the *Ovary*, and were broken when it dropped from thence, unite with the Vessels it finds here, and compose the *Placenta*: The Coats of the *Ovum*, being swelled and dilated by the Juices of the *Matrix*, form the *Chorion* and the *Amnion*, Integuments needful to the Preservation of the *little Animal*, which, receiving continually a kindly Nourishment from the same Juices, gradually stretches and enlarges its Dimensions, becoming then quickly visible with all the Parts peculiar to its Species, and is called a *Fætus*.

In *Plants*, say they, (which are incapable of removing from Place to Place, as *Animals* can) it was requisite a *Repository* for their *Farina* should be near at hand to prevent its being lost; and accordingly we find, that almost every Flower, producing a *Farina*, has likewise in itself a proper *Ovary* for its Reception; where the *Ova* thereby impregnated are expanded by the Juices of the *Parent Plant*. to a certain Form and Bulk, and then, becoming what we call

call *ripe Seeds*, they fall to the *Earth*, which is a natural *Matrix* for them.

According to the above Supposition, a *ripe Seed*, falling to the *Earth*, is in the Condition of the *Ovum* of an Animal getting loose from its *Ovary*, and dropping into the *Uterus*: And, to go on with the Analogy, the Juices of the *Earth* swell and extend the Vessels of the *Seed*, as the Juices of the *Uterus* do those of the *Ovum*, till the *Seminal Leaves* unfold, and perform the Office of a *Placenta* to the Infant included Plant; which, imbibing suitable and sufficient Moisture, gradually extends its Parts, fixes its own Root, shoots above the Ground, and may be said to be born.

Others disapprove of this *Hypothesis*, and insist that no *Animalcule* can possibly enter the *Ovum animale*, nor any Particle of the *Farina* get into the *Embryo* of a Seed: But, say they, in Animals, either the finest Part of the *Semen* is taken in by the Vessels of the *Vagina* and *Uterus*, circulated with the Fluids, and carried into the *Ovaria*, and even into the *Ova*, by the Vessels that run thither; or else, Fecundation is occasioned by a *subtile Spirit* in the *Semen masculinum*, which passes the *Uterus*, enters the *Ovaria*, pervades the Female *Ova*, actuates and enlivens the *seminal Matter* in them contained, and produces all the various Symptoms of Conception:—In Plants too, say they, the same is effected by penetrating *Effluvia* from the *male Semen* or *Farina*.

This Account of animal and vegetable Generation is intended to introduce a Discovery, which may possibly some way lead to a greater Certainty about it.

Amongst numberless Inquirers, whom the Opinion, that every Seed includes a real Plant, has set at work to open all Kinds of Seeds, and try by Glasses to find evident Proofs thereof, I have not been the least industrious: But after repeated *Experiments*, in every Manner I could think of, and with the utmost Nicety in my Power, I began to despair of ever attaining an ocular Demonstration of it. If by moistening the Seed it began to vegetate, I could indeed discern the *seminal Leaves*, and the *Germen* or *Bud*, whence the future Plant should arise; but was able to go no further, unless I waited till the Moisture gradually extending its Vessels made the little *Root* shoot down, the *Stalk* rise up, and the *minute Leaves* expand, and bring themselves to View. This, however, was not the Thing I sought for: But, some Days ago, mere Accident, when I thought nothing about it, favoured me with a Discovery I had so often searched after to no purpose.

Endeavouring with a fine Lancet to dissect a Seed of the *Gramen tremulum*, (a Seed whereof is placed in the Hole, marked Fig. 1. of the Ivory Slider herewith presented, and as it appears in TAB. II. Fig. 2. prefixed to this *Transaction*) with Intention to examine the several Parts of it with a Microscope, imagining I might find somewhat curious in the Contexture of its Husk, the Edges of which I observed to be transparent, I opened it the long Way exactly in the Middle, and took notice of something exceeding small between the Two Sides, which I had separated. I stuck the Point of the Lancet into it, with no other Design than to take it up, and place it in the Microscope to see what it might be; which I had no sooner done, than,

than, to my great Surprize and Joy, I found the Lancet had fortunately opened a membranous Case, that included a *perfect Plant*, arising from a double Root in the Basis of its said Case, with Two Stems of an equal Height, each whereof had many Leaves upon it, like the Grass from whence it was produced. This was a Sight I little expected to meet with; and being aware how much Imagination has frequently had to do with microscopical Observations, I distrusted my own Eyes, and examined it every way I could contrive, to prevent being deceived; but in all Positions I found it a Reality. Wherefore having secured it between Two Pieces of *Isinglass*, together with the Cases that inclosed it, (as in the Hole of the same Slider, Fig. 2. (see TAB. II. Fig. 3.) I afterwards cut open a great many Seeds of the same sort, in hopes I might be able to separate one of these *minute Plants* intirely from its *Theca*; which at last I successfully effected, and placed as in the Hole (see TAB. II. Fig. 3, 4).

Having never met with any Experiment, that so plainly proved the Existence of a real and perfect Plant *in Semine*, I imagined an Account thereof might be acceptable to this most learned SOCIETY, which encourages every Endeavour for the Advancement of true Knowledge: And I beg Leave, with all Submission, to recommend to your Consideration, how far this Discovery may conduce towards ascertaining the Manner of Generation.

A Dozen Lines, in a little Poem called *The Universe*, are so adapted to the present Subject, that, if a Quotation from myself may be excused, I shall conclude with them.

*Each Seed includes a Plant: that Plant, again,
Has other Seeds, which other Plants contain:
Those other Plants have All their Seeds, and Those
More Plants again, successively, inclose.*

*Thus, ev'ry single Berry that we find,
Has, really, in itself whole Forests of its Kind,
Empire and Wealth one Acorn may dispense,
By Fleets to sail a thousand Ages hence.
Each Myrtle Seed includes a thousand Groves,
Where future Bards may warble forth their Loves.
Thus Adam's Loins contain'd his large Posterity,
All People that have been, and All that e'er shall be.*

Henry Baker.

An Explanation of the Figures. See TAB. II.

Figure 1.

The Beetle, mentioned in the preceding Paper.

Figure 2.

*A Seed of the Gramen tremulum intire, of the
natural Size.*

Figure 2 *.

The same magnified.

Figure 3:

A Seed of the Gramen tremulum dissected, viz.

A. *One Lobe or Side of the Seed.*

B. *A membranous Case containing a minute Plant,
which arises with Two Stems bearing many
Leaves*

Leaves from its Radicle in the Basis of the said Case: This Case lies between the Two Lobes, and, before it is opened, seems to be the Germen of the Seed.

C. A Piece of the Case cut off in opening.

Figure 4.

A. A Lobe of the Seed.

B. The minute Plant extracted from its Case, that its Root and Branches may be seen to better Advantage.

C. The Case whence the minute Plant was taken.

Printed for T. WOODWARD, and C. DAVIS, over-
against *Gray's-Inn-Gate* in *Holbourn*; PRINTERS
to the ROYAL SOCIETY.

BOOKS lately Published.

I. **T**HE Elements of the Art of Assaying Metals. In Two Parts. The first containing the Theory, the second the Practice, of the said Art. The Whole deduced from the true Properties and Nature of Fossils; confirm'd by the most accurate and unquestionable Experiments, explain'd in the natural Order, and with the utmost Clearness. By *John Andrew Cramer*, M. D. Translated from the *Latin*, illustrated with Copper Plates. To which are added, Several Notes and Observations, not in the Original, particularly useful to the *English* Reader. With an Appendix, containing a List of the chief Authors that have been published in *English*, upon Minerals and Metals.

II. A Treatise of Diseases in General; wherein the true Causes, Natures, and Essences, of all the principal Diseases incident to the Human Body, are mechanically accounted for and explained, and their respective Intentions of Cure assign'd upon the same Principles. To which is subjoined, A System of Practice, applied to each Disease, and constituted upon the same most legitimate and solid Principles of Mechanical Reasoning. The Prescriptions, in *English*, all render'd familiar to every Capacity; and digested (for Method's sake) into Seven Books. With an Appendix, containing a Philosophical Essay on the Nature, Properties, Action, Use and Abuse of Quicksilver. Comprehending likewise a cursory View of the wonderful Virtues and Properties of Antimony and Steel. In Two Volumes. By *Charles Perry*, M. D.

III. A Treatise on the Small-Pox. In Two Parts. Part I. Containing a Description both of the Distinct and Confluent Kind, with Directions for the Management of various Patients, as to Diet and Medicines, in each Period of the Distemper. Also an Account of the incidental Symptoms, as to their Causes, curative Indications, and proper Remedies, in Reference to each of them. Likewise Instructions for managing Infants and Children: Together with a Method of external Remedies, for those who will not take internal Medicines. And some Considerations, shewing the Probability of curing the Small-Pox in the febrile State, so as to prevent the Eruption of Pustles, and the subsequent Period; with a Method likely to effect it. Part II. Containing Fifty Histories, in which this Distemper, and its various Symptoms, are exemplified. To which are added, Practical Aphorisms deduced from them. The Second Edition, corrected, with large Additions, and accommodated for Usefulness in Families. By *Theophilus Lobb*, M. D. Fellow of the *Royal Society* of London, and Member of the *Royal College of Physicians* in London.

IV. The Theory of the Working of Ships, applied to Practice. Containing the Principles and Rules for Sailing with the greatest Advantage possible. By *Monf. Pitot*, of the *Royal Academy of Sciences* at Paris. Translated from the *French* by *Edmund Stone*, F. R. S.

