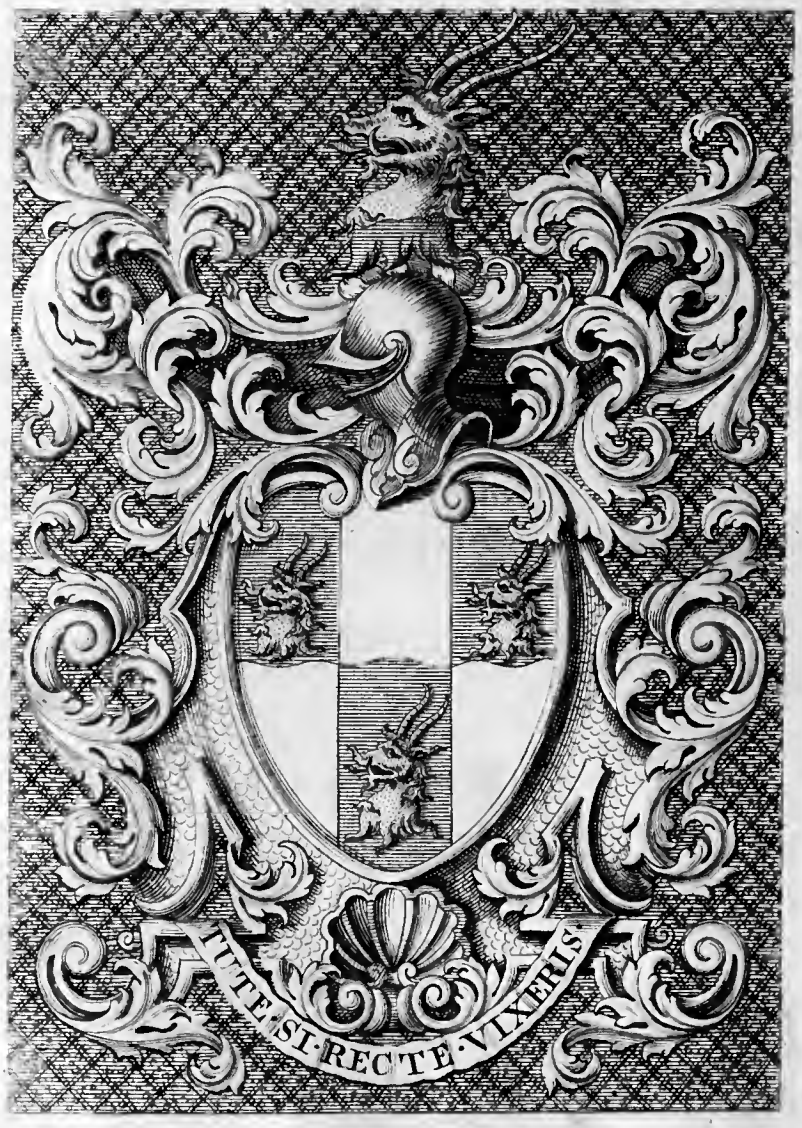
IVTE·SI·RECTE·VIXERIS

PHILOSOPHICAL TRANSACTIONS.

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, and *Labours*

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. XXXIII. For the Years 1724, 1725.

LONDON:

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TO

Dr. *RICHARD MEAD*,

VICE-PRESIDENT of the

ROYAL SOCIETY,

And FELLOW of the

Royal COLLEGE of *Physicians*,

This Thirty-Third VOLUME of

Philosophical Transactions

is Humbly Dedicated

BY

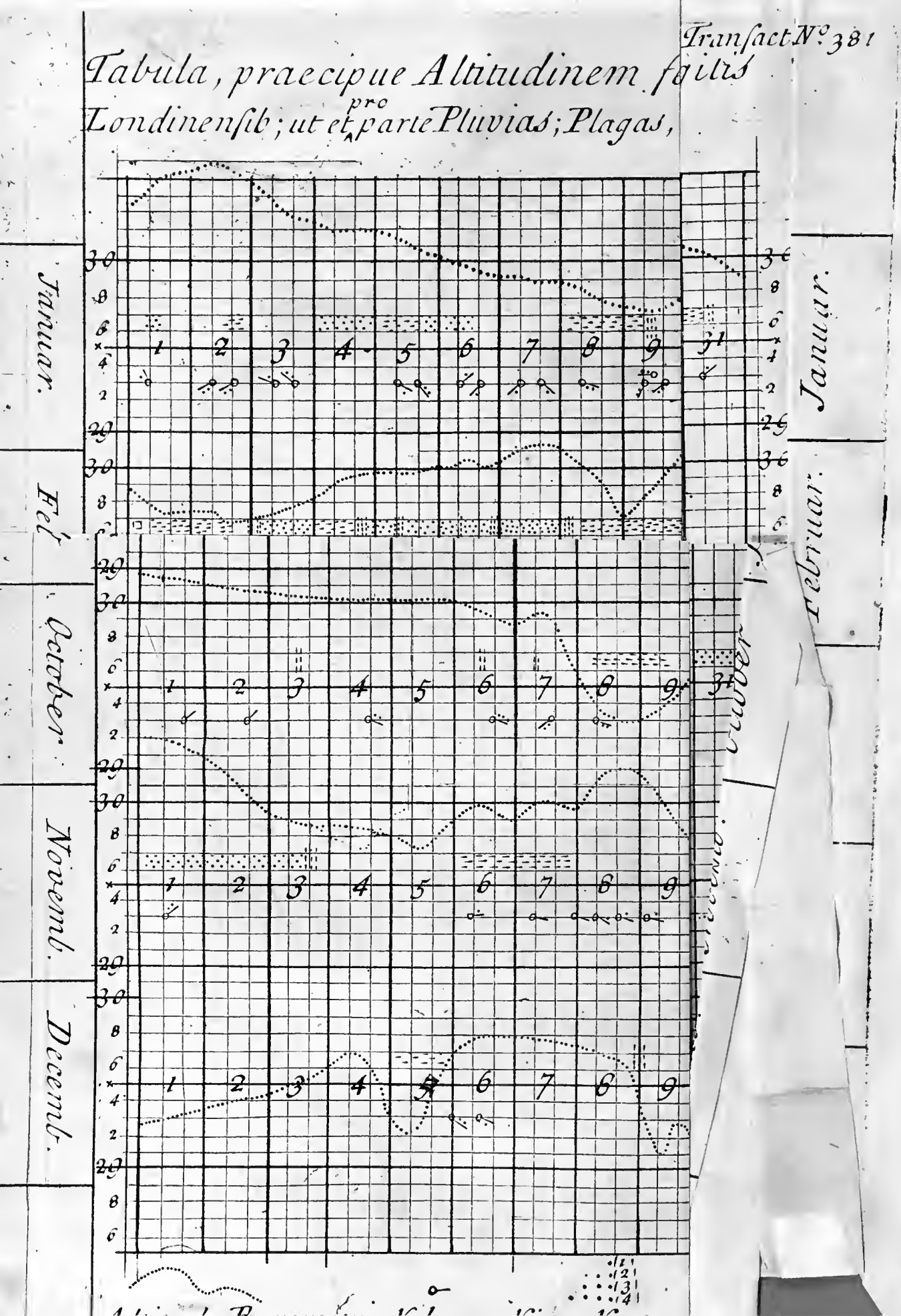
His Most Obliged, and

Most Humble Servant,

JAMES JURIN, *R. S. Sec.*

DR. RICHARD MEAD

Tabula, praecipue Altitudinem facilis pro Londinensib; ut et parte Pluvias; Plagas,



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PHILOSOPHICAL TRANSACTIONS.

For the Months of *January*, and *February*, 1724.

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I. *Experimenta circa gradum caloris liquorum nonnullorum ebullientium inſtituta.* *A* Daniele Gabr. Fahrenheit, *R. S. S.*

CUM elapſis abhinc circiter decem annis in Hiſtoriâ Scientiarum Societatis Regiæ Pariſienſis legiſſem, quod celeberrimus *Amontonius*, ope alicujus thermometri ab eo inventi, detexiſſet, aquam fixo caloris gradu ebullire; ſtatim magno accendebar deſiderio, thermometrum ejuſmodi mihimet ipſi præparare, ut pulchrum hocce naturæ phænomenon mihi oculis perluſtrare liceret, & de veritate experimenti convictus eſſem.

Quapropter thermometri ſtructuram quidem tentabam, ſed ob habitudinis ſufficientis in elaboratione illius defectum, vana erant conamina, licet ſæpius iterata; & quoniam etiam alia negotia prohibebant thermometri elaborationi magis inſiſtere, opportuniori repetitionem illius dedicabam tempori. Cum defectu virium atque temporis ardor non langueſcebat, æque avidus enim experimenti exitum videndi manebam. In mentem autem mihi veniebant ea, quæ ſolertiſſimus ille rerum naturalium ſcrutator de rectificatione barometrorum ſcripſerat; obſervaverat enim altitudinem columnæ mercurialis in barometro a vario temperamento mercurii aliquantulum (ſatis ſenſibiliter tamen) turbari. Ex his rebar, quod thermometron fortaſſe e mercurio conſtrui poſſet, cujus ſtructura non adeo difficilis foret, & cujus tamen ope experimentum maxime a me deſideratum explorare liceret.

Præparato ejuſmodi thermometro (licet in multis adhuc imperfecto) voto tamen meo eventus reſpondebat; magnâ enim animi voluptate rei veritatem contemplabar.

Tres jam erant anni elapſi, in quibus opticis aliiſque incubuiſſem laboribus, cum cupidus fierem experimentis explorare, an etiam alii liquores fixo ebullituri eſſent gradu caloris. Exitus experimentorum ſequenti continetur tabula, cujus prima columna exhibet liquores adhibitos; ſecunda illorum gravitatem ſpecificam; tertia graduin caloris, ad quem unuſquiſque liquor ebulliendo pertigit.

Liquores.	Gravitas ſpecifica Liquorum ad 48 Gr. calidorum.	Gradus ebullitione acquiſiti.
Spiritus vel Alcohol vini.	8260	176
Aqua Pluvia.	10000	212
Spiritus Nitri.	12935	242
Lixivium cineris clavellati	15634	240
Ol. Vitrioli	18775	546

Gravitatem ſpecificam cujuſcunque liquoris addendam neceſſe judicavi, ut ſi aliorum experimenta jam inſtituta, vel adhuc inſtituenda, a memoratis differrent, colligi poſſit, an e variatione gravitatis ſpecificæ, vel ex aliis differentia petenda ſit cauſis. Experimenta præterea non eodem tempore ſunt facta, & inde etiam liquores vario temperamenti vel caloris gradu erant affecti, ſed quoniam illorum gravitas diverſimode & inæqualiter turbatur, calculo illorum gravitatem ad 48 gradum (qui in thermometris meis medium tenet locum inter terminum intenſiſſimi frigoris arte commixtione aquæ, glaciei, ſaliſque Armoniaci, vel etiam maritimi, confecti, &

inter

inter terminum caloris, qui in ſanguine hominis ſani reperitur) revocavi.

Olea Volatilia aliquo gradu quidem incipiunt ebullire, ſed eorum calor ebulliendo ſemper augetur. Cujus rei cauſa fortaſſe erit, quod nempe volatiliores particulæ avolent, dum reſinoſæ majori attractione præditæ reſtant.

Olea fixa autem tanto calore afficiuntur, ut Mercurius in thermometro ſimul cum illis ebullire incipiat, & inde eorum calor memorato modo vix certe explorari poterit. Sed alium excogitavi modum, cujus in alia ſchedula coram Illuſtri Societate Regia meminiſſe me honorem habiturum eſſe ſpero.

Excepto ſpiritu vini & aqua, fortaſſe etiam gradus cœterorum liquorum hic commemoratorum variabit, precipue ſi magna ſatis quantitate adhibeantur & longius ebulliant.

II. *Observationes accuratæ captæ, anno* xvij = xxiij Lugduni Batavorum, Delphis Bataviæ, *& in pago* Rhenoburgo. *Circa mediam* BAROMETRI *altitudinem, mediam* THERMOMETRI *elevationem, tum &* HYGROMETRI *varietatem mediam, circa copiam* PLUVIÆ, *Roris, Nivis, Grandinisque, simul & circa copiam* AQUÆ *quæ exhalavit, & altitudinem putealis* AQUÆ *in* PUTEO, *unde nihil Aquæ exhaustum fuit toto anni decursu, de variatione* HOROLOGII PORTATILIS *singulis mensibus, ut certa hæc experimenta servire queant contemplatoribus* HISTORIÆ NATURALIS, *offert eadem harum deliciarum studiosis* NICOLAUS CRUQUIUS, *Geometra.* R.S.S.

BAROMETRUM.

Hic pono *Pondus* Athmosphæræ incumbentis, in planum pedis quadrati *Rhenolandici*, cujus athmos. pondus 1947 libras Amstelædamenses pendet, quando ☿ii in barometro altitudo est 27 *poll.* 7 *lin.* pendet 2094 *lb.* Amst. quando altitudo fuit ☿ in barometro 29 *poll.* 8 *lin.* hæc enim fuit maxima & minima ☿ii in barometro observata altitudo intra plures jam annos.

	lb.
Januar.	2051
Februar.	46
Martio	35
Aprili	46
Maio	57
Junio	53
Julio	2044
Augusto	46
Septemb.	54
Octobri	55
Novemb.	53
Decemb.	35
Summa	575
	12 — —
Medium	2048

THERMOM.

Hic calorem Atmosphæræ in loco observationis destinavi ex rarescente Aere ita ut summum frigus observatum sit ubi notabatur gradus 1000, aqua pura gelasceret ad grad. 1070, ebulliret vero ad grad. 1510.

	grad.
Januar.	1076
Februar.	85
Martio	102
Aprili	109
Maio	126
Junio	140
Julio	1129
Augusto	141
Septemb.	132
Octobri	121
Novemb.	104
Decemb.	96
Summa	1361
Medium	1113

HYGROMETRUM.

Mensuravi hic aquæ copiam in Athmosphærâ in loco observationis; juxta accrescens decrescensve pondus spongiæ ad bilancem appensæ, quam spongiam prius Muria Salis Ammoniaci humectaveram.

	pond.
Januar.	81
Februar.	80
Martio	80
Aprili	60
Maio	57
Junio	57
Julio	58
Augusto	60
Septemb.	61
Octobri	71
Novemb.	77
Decemb.	79
Summa	821
Medium	68

Quantitas aquæ, quæ totius hujus anni decursu mense quolibet exhalaverat, in Aere aperto, & ventis perflato. *Rhenoburgi.*

	lin.
Januar.	7
Februar.	14
Martio	33
Aprili	36
Maio	58
Junio	57
Julio	37
Augusto	39
Septemb.	24
Octobri	15
Novemb.	15
Decemb.	12
Summa	347

toto anno. 28 *pol.* 11 *lin.*

Aquæ Cœlo delapſæ, nimirum Pluviæ, Roris, Nivis, Grandiniſque Altitudo;

Cum cauta cura, ne exhalatione, aut alio modo vel minimum perierit.

Delphis. *lin. dec.*	*Rhenoburgi.* *lin. dec.*
17.9	21.2
25.1	23.8
18.8	28.-
5.5	7.5
4.2	2.7
3.2	4.8
38.6	28.-
41.9	40.2
15.1	14.8
8.2	11.3
30.7	29.7
30.6	40.-
239.8	*252*

poll. 20 (in toto anno) 21 *poll.*

Aquæ altitudo putealis, in puteo, menſurata a ſummo ejus margine, ad aquæ ſuperficiem, in fine cujuſque menſis. Fuit vero putei profunditas ima uſque ad ſabulum ſcaturiens vel currens, nihilque aquæ inde eductum toto obſervationis tempore.

ped.	*pol.*
5	4
4	11
5	7
6	10
8	1
9	3
9	7
9	7
9	9
9	8
9	3
8	2
96	—

Med. 8 *ped.*

Obſervatio accelerati, vel retardati Curſus, in Horologio portatili accuratiſſimo.

Ut pateat quot minuta, in quoque Menſe, plura vel pauciora, abſolverit. Hic (+) augmenti, (÷) decrementi nota eſt.

Ratio relata ad Solis decurſum.

Minuta.	
+ 151	Jan.
+ 21	Febr.
÷ 168	Mar.
÷ 120	Apr.
÷ 123	Maio.
÷ 130	Jun.
÷ 90	Jul.
+ 133	Aug.
+ 24	Sept.
+ 19	Oct.
+ 266	Nov.
+ 252	Dec.

+ 866, & ÷ 631
ſive + 235

in quoq; menſ. 20″ accelerat.

Observationes per aliquot annos factæ.

Ann.	Jan.	Feb.	Martio	Ap.	Mai.	Jun.	Jul.	Aug.	Sept.	Oct.	Nov.	Dec.	Sum.	Med.	Ann.
	lb.			BAROMETRUM									12	*lb.*	
1720	2034	34	38	36	38	42	45	36	50	40	38	23	454	2038	1720
1721	52	42	36	29	29	43	51	46	43	42	32	29	474	39	1721
1722	76	41	32	41	42	45	36	39	50	59	37	30	528	44	1722
1723	51	46	35	46	57	53	44	46	54	55	53	35	575	48	1723
	213	163	141	152	166	183	176	167	197	196	160	117		169	
4	2053	41	35	38	41	46	44	42	49	49	40	29		2042	
	grad.			THERMOMETRUM										*Grad.*	
1720	1087	89	86	102	126	126	145	138	121	107	91	91	309	109	1720
1721	90	74	75	112	116	137	136	142	133	110	100	84	309	109	1721
1722	79	90	97	109	122	134	139	140	135	117	101	90	353	113	1722
1723	76	85	102	109	126	140	129	141	132	121	104	96	361	113	1723
	332	338	360	432	490	537	549	561	521	455	396	361		444	
4	83	85	90	108	122	134	137	140	130	114	99	90		111	
	pond.			HYGROMETRUM										*pond.*	
1721	89	82	73	80	69	64	63	68	76	76	89	88	917	76	1721
1722	88	85	76	63	62	62	63	68	70	76	81	88	882	74	1722
1723	81	80	80	60	57	57	58	60	61	71	77	79	821	69	1723
	258	247	229	203	188	183	184	196	207	223	247	255		219	
3	86	82	76	68	63	61	61	65	69	74	82	85		73	
	lineæ			*Pluvia, &c. Delphis.*										*poll. lin.*	
1715	11	20	36	8	15	18	95	62	36	37	47	15	400	33: 4	1715
1716	19	20	14	7	17	4	48	19	55	57	21	32	313	26: 1	1716
1717	31	15	29	29	31	28	29	28	24	32	29	28	333	27: 9	1717
1718	21	18	6	30	17	17	35	27	14	46	21	25	277	23: 1	1718
1719	33	20	10	11	17	4	12	34	23	25	24	22	235	19: 7	1719
1720	36	24	21	21	15	20	23	55	47	56	25	20	363	30: 3	1720
1721	20	31	27	59	30	34	15	41	27	57	30	48	419	34:11	1721
1722	2	20	25	23	15	22	49	53	25	7	21	53	315	26: 3	1722
1723	18	25	19	5	4	3	39	42	15	8	31	31	240	20:	1723
	191	193	187	193	161	150	345	361	266	325	249	274		241: 3	
9	21	21	21	21	18	17	38	40	30	36	28	30		26:10	

Pluvia &c. Rhenoburgi

	lin.			*pol. lin.*
1720	35 16 13	24 29 14 / 20 48 54	50 39 25	367 30:7 1720
1721	21 29 28	59 35 28 / 14 47 27	51 32 46	417 34:9 1721
1722	3 20 27	29 28 27 / 60 64 26	10 21 61	376 31:4 1722
1723	21 24 28	7 3 5 / 28 40 15	11 30 40	252 21: 1723
	80 89 96	119 95 74 / 122 199 122	122 122 172	117:8
4	20 22 24	30 24 19 / 30 50 30	30 30 43	29:5

Aqua quæ exhalavit Rhenoburgi.

	lin.			*pol. lin.*
1720	10 10 10	25 44 55 / 59 56 50	30 10 12	371 30:11 1720
1721	8 9 15	54 47 60 / 48 42 27	20 22 10	362 30: 2 1721
1722	5 7 20	28 54 54 / 65 59 34	14 13 13	366 30: 6 1722
1723	7 14 33	36 58 57 / 37 39 24	15 15 12	347 28:11 1723
	30 40 78	143 203 226 / 209 196 135	79 60 47	120:6
4	8 10 20	36 51 56 / 52 49 34	20 15 12	30:2

Putealis Aquæ Altitudo.

	ped.pol.			*ped.pol.*
1720	6: 6: 3 6: 5	6: 7 6: 6 8: / 8:11 9: 8:3	6:11 5:8 5:5	84: 7: 1720
1721	5:6 5: 6 4: 6	4: 2 5: 7 7: / 8: 1 7: 9 7:7	6: 3 4:6 4:4	70:9 5:11 1721
1722	6: 5: 7 5:11	6: 1 6:10 7:11 / 8: 7:10 7:4	7: 9 7:3 4:6	81: 6: 9 1722
1723	5:4 4;11 5: 7	6:10 8: 1 9: 3 / 9: 7 9: 7 9:9	9: 8 9:3 8:2	96: 8: 1723
	22:10 22:3 22: 5	23:8 27: 32: 2 / 34:7 34:2 32:11	30: 7 26:8 22: 5	27: 8
4	5 :8 5 :7 5 :7	5:11 6:9 8: / 8:8 8:6 8:3	7:8 6:8 5:7	6:11

Horologii portatilis Cursus.

	Minuta				
1722	÷ 34 ÷ 9 ÷ 7	+ 5 ÷ 38 ÷ 55	÷ 89 ÷ 78 ÷ 84	÷ 8 + 151 + 246	+&÷ 402 1722
1723	+ 131 + 1 ÷ 187	÷ 139 ÷ 142 ÷ 149	÷ 109 + 113 + 4	÷ 1 + 246 + 232	+&÷ 727 1723
	+ 97 ÷ 8 ÷ 194	÷ 134 ÷ 180 ÷ 204	÷ 198 + 35 ÷ 80	÷ 9 + 387 + 478	+&÷ 1129
2	+ 48 ÷ 4 ÷ 97	÷ 67 ÷ 90 ÷ 102	÷ 99 + 17 ÷ 40	÷ 4 + 199 + 239	+&÷ 564
	Jan. Feb. Mar.	*Apr. Maii Jun.*	*Jul. Aug. Sept.*	*Oct. Nov. Dec.*	

III. *An Account of a Dropſy in the left Ovary of a Woman, aged* 58. *Cured by a large Inciſion made in the Side of the* Abdomen, *by Dr.* Robert Houſtoun.

AUGUST 1701. I was in the Country, with a Patient, the Lady *Anne Houſtoun*, Wife to Sir *John Houſtoun*, Baronet; in the Shire of *Renfrew*, ten Miles from *Glaſgow*, *North Britain*. This charitable Lady preſs'd me with great Earneſtneſs to viſit a Tenant's Wife, who lay bedridden, of an uncommon Diſeaſe, which no Phyſician, or Surgeon, who had ſeen her, could give any Name to, or account for. She inform'd me, the ableſt of that Country had forſaken her, and declared her incurable, ſo that I could loſe no Reputation by the Reſult of my Endeavours.

In order to oblige this worthy Lady, and in Compaſſion to the Diſtreſs of a poor Woman in ſo deplorable Condition, deſerted and given over on all ſides, I went, determined to do every thing in my Power for her Relief. She was in the 58th Year of her Age, her Name was *Margaret Millar*.

She inform'd me that her Midwife, in her laſt lying-in, at 45 Years old, having violently pull'd away the Burthen, ſhe was ſo very ſenſibly affected by a Pain, which then ſeiz'd her in the left Side, between the *Umbilicus* and Groin, that ſhe ſcarce ever had been free from it after, but that it had troubled her more, or leſs, during 13 Years together; that for two Years paſt ſhe had been extremely uneaſy, her Belly grew very large,

large, and a Difficulty of breathing increaſed continually upon her; inſomuch that for the laſt ſix Months, ſhe had ſcarce breath'd at all but with the utmoſt Difficulty. That in all that Space of Time, having quite loſt her Appetite, ſhe had ſcarce eat ſo much as would nouriſh a ſucking Child; and that for three Months together ſhe had now been forc'd to lie conſtantly on her Back, not daring to move at all, to one ſide or other.

This Tumour was grown to ſo monſtrous a Bulk, that it engroſs'd the whole left Side, from the *Umbilicus* to the *Pubes*, and ſtretch'd the Abdominal Muſcles, to ſo unequal a Degree, that I don't remember ever to have ſeen the like in the whole Courſe of my Practice. It drew towards a Point. Her being ſo long confined to lie continually on her Back, having grievouſly excoriated her, added much to her Sufferings, which, with want of Reſt and Appetite, had waſted her to Skin and Bone, as the poor Woman herſelf expreſſed it. Indeed ſhe needed not to have told me ſo, my Eyes were too faithful Witneſſes of her low and wretched Condition.

Scarce able to ſpeak out, ſhe told me, that having heard much of my Succeſs, ſhe had ſtrong Hopes of Relief, provided I would try at leaſt, and do ſomething in Pity to her Affliction.

I anſwer'd her that I was willing, but afraid, in her low State, ſhe would want Strength to undergo a large Inciſion; that in order effectually to relieve her, I muſt be oblig'd to lay open a great Part of her Belly, and remove the Cauſe of all that Swelling: ſhe ſeem'd not frightened, but heard me without Diſorder, and, as if inſpir'd with ſudden Courage, preſs'd, and urg'd me to the Operation.

I drew (I muſt confeſs) almoſt all my Confidence from her unexpected Reſolution, ſo that without loſs of Time, I prepared what the Place would allow, and

with an Impoſthume Lancet, laid open about an Inch, but finding nothing iſſue, I enlarged it two Inches, and even then nothing came forth but a little thin yellowiſh *Serum*, ſo I ventured to lay it open about two Inches more: I was not a little ſtartled, after ſo large an Aperture, to find only a glutinous Subſtance bung up this Orifice. All my Difficulty was to remove it; I try'd my Probe, I endeavour'd with my Fingers, but all was in vain; it was ſo ſlippery that it eluded every Touch, and the ſtrongeſt hold I could take.

I wanted, in this place, almoſt every thing neceſſary, but bethought myſelf of a very odd Inſtrument, yet as good as the beſt in its Conſequence, becauſe it anſwer'd the End propos'd. I took a ſtrong Firr-Splinter, ſuch as the Poor in that Country ordinarily uſe to burn inſtead of Candles; I wrapt about the End of this Splinter ſome looſe Lint, and thruſt it into the Wound, and by turning and winding it, I drew out above two Yards in Length of a Subſtance thicker than any Gellie, or rather like Glue that's freſh made and hung out to dry; the Breadth of it was above ten Inches; this was followed by nine full Quarts of ſuch Matter, as I have met with in Steatomatous and Atheromatous Tumours, with ſeveral *Hydatides*, of various Sizes, containing a yellowiſh *Serum*, the leaſt of 'em bigger than an Orange, with ſeveral large Pieces of Membranes, which ſeem'd to be Parts of the diſtended Ovary. Then I ſqueez'd out all I could, and ſtitch'd up the Wound in three Places, almoſt equi-diſtant: I was oblig'd to make uſe of *Lucatellus*'s Balſam, which was made by her Lady for the Uſe of the Poor; with this Balſam I covered a Pledget, the whole Length of the Wound, and over that laid ſeveral Compreſſes, dipp'd in warm *French* Brandy, and becauſe I judg'd that the Parts might have loſt their Spring, by ſo vaſt and ſo long a

Diſtention

Diſtention, I dipt in the ſame Brandy a large Napkin four times folded, and applied it over all the Dreſſings, and with a couple of ſtrong Towels, which were alſo dipt, I ſwathed her round the Body, and then gave her about four Ounces of the following Mixture, which I had from her Lady.

℞ *Aq. Menthæ* ℔ ſs. *Aq. Cinnamomi fort.* ℔ iſs. *Syr. Diacodii* ʒ vi. *M.*

The Cinnamon-Water was drawn off from Canary and the beſt Cinnamon; indeed it was the fineſt and moſt fragrant Cinnamon-Water I ever taſted; of this Mixture I ordered her 2 or 3 Spoonfuls 4 times a Day.

Next Morning I found her in a breathing Sweat, and ſhe informed me, with great Tokens of Joy, that ſhe had not ſlept ſo much, nor found herſelf ſo well refreſh'd, at any Time for three Months paſt. I carefully attended her once every Day, and as conſtantly dreſſed her Wound in the ſame Manner as above, for about eight Days together; I kept in the lower Part of the Wound a ſmall Tent, which diſcharged ſome Seroſities at every Dreſſing for 4 or 5 Days. But Buſineſs calling me elſewhere, I left her, having firſt inſtructed her two Daughters (both Women, who carefully attended her) how to dreſs her Wound, and told 'em what Diet I thought moſt proper, enjoining 'em ſtrictly to obſerve what I order'd.

Her chief Food was ſtrong Broth made of an old Cock, in each Porringer of which was one Spoonful of the Lady's Cinnamon-Water; this was repeated 4 times a day, and gave her new Life and Spirits.

After three Weeks Abſence, I called at her Houſe, and finding it ſhut up, was a little ſurpriz'd, but had not gone far before I was much more ſurprized, when I found her ſitting wrapt up in Blankets, giving Directions to ſome Labourers who were cutting down her Corn.

She mended apace to the Admiration of every body thereabouts, recovered ſurprizingly, and lived in perfect Health from that time, which was in *Auguſt* 1701, till *October* 1714. when ſhe died in ten Days Sickneſs.

That this Tumour, or rather Dropſy of the *Ovarium*, proceeded from the Midwife's Raſhneſs in pulling away the *Placenta*, not knowing how to ſeparate it from the *Uterus* ſkilfully, ſeems to me plain from what the Woman herſelf told me, and what fell out afterwards.

The *Placenta* adhering faſt to the *Uterus*, required more Art to bring it away than ſhe was Miſtreſs of, which probably induc'd her to uſe Violence; by which ſhe forc'd down the *Fundus Uteri*; ſo overſtrain'd the Ligaments, and all that's appended to 'em; eſpecially the *Ligamentum latum* of the left Side and its *Ovarium*, which may be reaſonably allow'd to have been hurt in the Relaxation with the reſt. Hence the Elaſticity of theſe læſed Parts was not only impair'd, but the ſmall Lymphaticks ruptur'd, ſo that the extravaſated *Lympha* ruſhing out, thicken'd, and not being able to recirculate, dilated the injur'd *Ovarium*, and thus increas'd the Tumour, and the Parts being already exceſſively diſtended, and being no longer able to reſiſt the new Influx of freſh Secretions, ruptur'd alſo, and by Degrees augmented to that huge and enormous Bulk.

Cyprianus in his Letter to Sir *Thomas Millington*, gives ſeveral Inſtances of the Miſchiefs committed by Midwives, and other ignorant Perſons. When a *Placenta* adheres, then (ſays he) they tear all before 'em, by pulling rudely, they force down the *Fundus Uteri*, conſequently do Violence to the *Ovaria* and other Parts, whence follow Inflammations, *&c.*

Foreſtus, *l.* 28. *Obſ.* 80. ſays, that an over-bold Midwife pulled out the *Placenta* too haſtily, and that the

the Woman preſently fell into a Swoon, and died immediately.

Frederick Ruyſch attributes the Cauſe of a *Placenta*'s adhering cloſe, to a central Inſertion of the Navel-String into the *Placenta*, in which Caſe it is hard to ſeparate; a Train of diſmal Accidents attend thoſe who, not knowing how to do it, venture to pull it away by Force. *Vide Obſ. Anat.* 97.

Dionis and *la Motte* have made the ſame Remark, by the latter of which we are told of innumerable Accidents, which he has known happen to Women in hard Labour, by the Raſhneſs and Ignorance of bold Pretenders, who, without Rule, Precepts, Practice, or any ſuitable Inſtructions, venture to practiſe Deliveries.

The aforeſaid *Ruyſch* has a remarkable Inſtance of the Effects of Violence in hard Labour: The Caſe is curious. *Vide Obſ. Anat.* 63.

Manget, in his *Theatrum Anat.* tells us that in all hard Labours, where the *Placenta* ſticks, the Ligaments ſuffer more or leſs by a raſh and ignorant Way of Pulling; Examples of which ſee in *Platerus*'s *Obſ. lib.* 3. *Bartholin. Cent.* 2. *Hiſt.* 91. *Cent.* 5. *Hiſt.* 19. *Marchett. Anat. cap.* 7. and others, of which *Blaſius* has given us an ample Liſt in his Commentaries on *Veſlingius*.

He likewiſe tells us, that the *Ovaria* grow to a wonderful Bulk, and contain ſuch a huge Quantity of Liquor, that they become dropſical, whereof *Skenckius* in his *Obſ.* has recorded many Examples, as hath *Riolan*, and many others.

Munnicks in his *Bibliothec. Anat.* gives us the Hiſtory of a large Dropſy of the right *Ovarium*.

Mortgagni has told us, that he has often met with large Veſicles in the *Ovaria*, and, in morbid Bodies, ſometimes full of purulent Matter.

Sylvius

Sylvius. Ex lymphaticis læſis tam Virilium, quam Muliebrium Teſticulorum, &c. Hydrops particularis excitari poteſt.

Veſlingius cap. 7. has often met with large Tumours, from Obſtructions in the *Ovaria* of Women.

Veſalius found in the right *Ovarium*, 9 or 10 *Glandules*, like Gooſe Eggs, with Matter not unlike the Whites of Eggs, or rather thicker.

Gul. Ballon, paradig. vi. found an Impoſthume of the Nature of a *Steatoma*, near the *Collum Uteri*; The Woman had a large Tumour on her Side 16 Years, and had only one *Ovarium*, which was ſhown as a wonderful thing.

At *Weſtminſter*, about two Years ago, I opened the Body of a Gentlewoman, about 60, whoſe left *Ovarium*, of a great Bulk, weigh'd vi. ℔. to which adher'd ſeveral *Hydatides* like Pullets Eggs.

Hildan mentions an *Hydropick* Tumour of the right *Ovarium*, of a prodigious bulk, ſtuff'd with Hair, *vide Gul. Fab. Hild. Cent.* 5 *Obſ.* 48. *Greg. Horſt. Tom.* 2. *l.* 4. *Ob.* 53.

Ruyſch Obſ. Anat. 17. A Dropſie (ſays he) of the *Ovarium*, or (if you pleaſe) of the Eggs, is an Affection, or Diſeaſe, well enough known to others, but I am afraid not ſo well conſider'd; Authors have agreed to call it *Hydatis*, with which name I ſhall reſt ſatisfied, provided we agree about the ſubject of the Diſtemper. I obſerve this, moſt frequently, if not always, to be the Eggs, but ſo chang'd, and ſometimes ſwell'd to ſuch a Bulk, that I have often ſeen them larger than a Child's Head.

Drelincourt has given us a very full and exact Account of a Dropſie of the left *Ovarium*, in a Lady of 35 Years. The Tumour, of an enormous Bulk, continu'd three Years. The Body of the *Ovarium*, with all contain'd in it, weigh'd, 60 ℔. It was nothing but

but a number of little Globules, cluſter'd together, that differ'd in their Roundneſs, Form, Colour, and Conſiſtence, proceeding from little ſeminary Veſicles in a Cluſter. Some had Water extremely clear and liquid, others a yellowiſh *Serum* thin, others a glutinous Matter; ſome were as big as Pullets Eggs, others bigger than one's Fiſt.

Let theſe few, out of the many Inſtances which I could produce from Authors of undoubted Reputation, ſuffice to prove, that the *Ovaria*, as well as the *Tubæ Falloppianæ*, Ligaments, and *Uterus* itſelf, are not free from Dropſies, *&c.* and that they proceed from Obſtructions, often occaſioned by rude and violent dealing with Women in difficult Labours, which generally bring on a train of diſmal Symptoms, that ſooner or later, according to the Strength of the Sufferer, after a miſerable, painful, languiſhing Life, end in Death.

The manifeſt Succeſs in this uncommon Caſe may be of uſe, and may ſhew, that we ought not to diſpair too ſoon, in Diſtempers that are ſeemingly moſt dangerous.

IV. *Præparatio Cærulei Pruſſiaci ex Germaniâ miſſa ad* Johannem Woodward, *M. D. Prof. Med. Greſh. R. S. S.*

℞ TArtari crudi & Nitri crudi Siccati ad ℥iiii. Pulveriſentur minutiſſime, & commiſceantur, deinde admoto igneo carbone detonentur, & habebis Salis Tartari extemporanei ℥iiii. Dum adhuc calidum eſt hoc Sal, pulveriſetur ſubtiliſſime, & addantur ſanguinis Bovini probe exſiccati & ſubtiliſſ. pulv. ℥iiii. Hæc,

bene mixta, indantur crucibulo, ut tertia pars vacua sit; impositio dein operculo igni committatur, & circumdetur crucibulum carbonibus, ut sensim ardescat, & materia sine præpropera accensione flammam concipiat & igneſcat. In hoc ignis gradu teneatur materia, donec flamma & accensio remittat; augeatur demum ignis, ut valde candeat materia, & parum flammæ e crucibulo amplius emineat. Remove demum ab igne crucibulum, & materiam mortario ingestam leviter contere, & ad manus habeto aquæ ferventissimæ pluvialis libras 4 ponderis civilis, cui materiam, adhuc ferventem, immittas, & per semihoræ spatium coque; decoctum per linteum coletur, & materia remanens nigra, aquæ portioni denuo affusa, igni iterum apponatur, coquatur, & percoletur; id quod eousque continuandum, donec salsedo & acrimonia omnis e materia sit elixiviata, & aqua redeat insipida. Humores omnes in linteo & materia residuos, fortiter exprime, & ubi singula in unum colligeris, igni iterum committe, & ad remanentiam 4 librarum evapora, & ulteriori usui serva, sub. No. 1.

℞. Porro Vitrioli Anglici ad albedinem leviter calcinati ʒi. solvatur in Aquæ pluvial: ʒvi. filtretur per chartam & signetur: No. 2.

℞. Denique Aluminis crudi ʒviii. Solvatur in libris 4. aquæ ferventissimæ ad omnimodam Aluminis consumptionem, hoc rite peracto, adjunge solutionem Vitrioli sub No. 2. asservatam, quæ ex igne fervens ingeratur ollæ satis magnæ & amplæ, & cum lixivio No. 1. seorsim bene fervefacto, combinetur. Fiet ex continenti magna ebullitio, & apparebit color viridis montani seu chrysocollæ; effundatur alternis vicibus, durante ebullitione, ex uno vase in aliud, qua cessante, quieti committe. Tum linteo insinuetur, ut aquositas transeat, color vero in linteo remaneat; si igitur nihil humiditatis amplius distillet, cum spathula lignea

e linteo

è linteo in ollam novam minorem remove, ſuperfunde poſtea ſpiritus ſalis comm. ʒii. vel ʒiii. & ſtatim apparebit color cæruleus pulcherrimus: quæ probe mixta per noctem quieſcant, quo facto aquæ pluvialis magna quantitas addatur, in gyrum moveatur ſpathula, & poſteaquam reſedit materia, aqua decantetur, & recens aqua ſuperfundatur, & eouſque labor reiteretur, donec omnis acrimonia ſit deſumpta, & aqua inſipida defluat; hoc pacto præcipitatum tuum ſummè cæruleum linteo expanſo ingere, ut aqua diſtillet, ſenſimq; color calore leni exſiccetur uſui.

N. B. Calcinatio magni momenti eſt in hoc opere, nam color cyaneus & cæruleus obſcurus ortum ſuum trahit a calcinatione levi, mediocri, & forti ſanguinis arefacti cum ſale Tartari, & inde diverſitas coloris.

Lixivia ferventiſſima uno eodemque feſtinatiſſimo actu ſunt confundenda.

V. *Obſervations and Experiments upon the foregoing Preparation.* By *Mr.* John Brown, *Chymiſt*, *F. R. S.*

DR. *Woodward* having lately communicated a Paper (which he receiv'd from another hand) to this Society, containing a Proceſs for making the *Pruſſian* Blue. I was willing to go thro' it exactly, according to the Proportions there preſcrib'd; and obſerv'd that by a Calcination of ʒ 4 of Blood dry'd, with ʒ 4 of *Sal Tartari*, in two Hours time that Part of the Operation was over, and a black ſpongy Subſtance remain'd in the Crucible weighing ʒ 4. a Diſſolution of which being made in boiling Water and afterwards filtred, the Remainder, when dried, weigh'd

9 Drachms, *Avoirdup.* the former having been weigh'd by the ſame kind of Weight.

The Loſs in the Diſſolution and Filtration of the Vitriol and Alum, is not worth taking Notice of, they having both been very clean before they were diſſolv'd. The Mixtures being made as preſcrib'd, with the Addition of the *Spiritus Salis*, the Product was a very fine Blue, which when well edulcorated by frequent Waſhings, and after that thoroughly dried weigh'd ℥1. or a little more, and entirely anſwer'd the Character the Author gave of it.

Among the ſeveral Experiments that were made with theſe Liquors, I mean the *Lixivium* with Blood, the Solution of Vitriol, the Solution of Alum, and the Spirit of Salt, tho' they always produced a Blue, yet that Blue differ'd in Degrees of Colour, according to the varied Proportions of the Vitriol and Alum, and the Colours produc'd from theſe ſeveral Proportions were each of them improv'd by the Addition of the *Sp. Salis.*

I ſhall mention only two of the ſeveral I tried, in one of which the Alum was entirely left out, and a pale Blue produc'd; in the other, the Proportions of Vitriol and Alum were equal, and a very deep Blue was produc'd.

Theſe Differences in Colour, ariſing from the ſeveral Proportions of the Vitriol and Alum, are only mention'd to confirm the Truth of the Author's Preſcript, as being the moſt exact and beſt-proportion'd to produce the fineſt Colour, of any I have try'd. The only Misfortune he takes notice of, as attending his Preſcript, is what may happen in the Calcination.

It would be curious to know what gave the firſt Hint for the Production of ſo fine a Colour, from a Combination of ſuch Materials; eſpecially when we come to conſider, that the Blood has the greateſt and principal

Share

Share in this ſuprizing Change. I doubt not but Blood of any kind, or Fleſh of any kind, would produce the ſame Effects, but have reaſon to believe the latter would not produce ſo beautiful a Colour as the former. I purpoſely dry'd ſome Beef freed from its Skin and Salt, and purſued the ſame Courſe as with the Blood; but there was a ſenſible Difference to be obſerv'd during the Calcination, and a very manifeſt one in the Beauty of the two Colours, when finiſh'd.

To prove the Share the Blood has in this Change, the following Experiments (ſome of which I had the Honour of ſhewing before this Society) may be convincive.

The Solution of Alum mix'd with that of the Vitriol produceth no Alteration of Colour: if to theſe you add the *Spiritus Salis*, the Appearance is the ſame; but if to the whole you put the *Lixivium* with Blood, there precipitates a Blue.

If you ſubſtitute, inſtead of the *Lixivium* with Blood, a *Lixivium* made with the ſame Salt of Tartar only, which then becomes an *Ol. Tartari*; and after the Mixture of the Solution of Alum, with that of the Vitriol, you pour on this *Ol. Tartari*, there follows indeed a Precipitation, but of no Colour; and if you add the Spirit of Salt, it ſo ſtrongly attracts what is precipitated, as to render the muddy Mixture perfectly clear.

The very ſame Effect will follow, if any Volatile Alcalious Spirit is made uſe of as a precipitant, or any Volatile Salts diſſolv'd in Water; nor can the Blood it ſelf be ſuppoſed to communicate this Change from any ſuch Properties, the Heat of Fire it undergoes in the Calcination, being ſufficient to throw them off.

In the Calcination of the dry'd Blood and Salt of Tartar it was obſerv'd, that there was a Loſs of juſt

half. It is difficult to determine exactly what Quantity of either was loſt by this Calcination, but it will eaſily be granted, that there was loſt a far larger Quantity of the Blood, than of the Salt of Tartar; and that is obvious from an Experiment, by which, when the Salt of Tartar was calcined by itſelf, with the ſame Degree of Heat, it loſt leſs than an $\frac{1}{8}$ Part, whereas, when the dry'd Blood was calcined by itſelf, it loſt more than $\frac{6}{8}$.

The Blood, in Calcination with the Salt of Tartar, communicates its tinging Quality to the Salt, or that Quality is extracted from it by the Salt, and paſſes with it in its Diſſolution in the boiling Water.

To prove this, ſome dry'd Blood was calcin'd by it ſelf, and a ſtrong Decoction was made of it in Water, and afterwards filtred: this, when mix'd with the former Solutions, produced little or no Alteration; but on the Addition of the *Spiritus Salis*, changed to an Amber Colour, without any Precipitation.

When this Liquor was mix'd with the *Ol. Tartari*, and poured to the former Solutions, it caus'd a Precipitation, but no Colour, and the *Spiritus Salis*, as in the other Experiment, made the Liquor clear again, but left this alſo of an Amber Colour.

The Change of Colour is not effected in any of the Materials, except in that of the Solution of Vitriol, ſo that the Alum ſeems only to be of uſe in fixing the Colour, as it is often us'd by the *Dyers* for that Purpoſe, and the *Spiritus Salis* gives it a deeper Dye. For if the *Lixivium* with Blood be poured to the Solution of Alum alone, there will fall a Sediment a little on the Purple, to which if you add the *Spiritus Salis*, it changes the Colour, and the Sediment is of a Brown.

So, much the ſame changes will be produced, if you pour the *Spiritus Salis* to the *Lixivium*, but not the leaſt Appearance of a Blue, whereas, as is beforemention'd,

when

when the *Lixivium* is poured to the Solution of Vitriol, there immediately follows the Blue, which is ſtill heighten'd by the Addition of the *Spiritus Salis*.

It will not be improper to take notice, that as the Author orders all the Liquors, except the *Spiritus Salis*, to be boiling hot when mix'd, ſo it is certain the Colour is thereby more immediately produc'd, and looks more beautiful; but moſt of the Experiments here mention'd were made with the Liquors cold, and the Colours came to their Beauty with a little waſhing. In one of the Experiments with the Liquors cold, after the *Lixivium* with Blood had precipitated the Blue in the Mixture of Alum and Vitriol, by pouring in a little more of the *Lixivium*, the Blue all diſappear'd, and an ugly muddy Colour was left; but the Addition of the *Spiritus Salis* ſoon diſcharg'd that, and the Blue return'd.

In calcining the Beef and Salt of Tartar, I found the Matter left in the Crucible to weigh juſt half of the whole Mixture, as in that with the Blood; but after the boiling it in Water, the *Reſiduum* in the Filtre when dry'd, was very near a third leſs in Proportion than the other. From whence may be reaſonably inferr'd, That the Salt of Tartar holds a larger Share of the Beef in the one Operation, than of the Blood in the other.

Having in the former Part of this Account of the *Pruſſian Blue* prov'd, by the Experiments there mentioned, that the Solution of Vitriol was the only Subject among thoſe Ingredients, that the *Lixivium* of Blood produc'd this Change of Colour in it; and having ſince conſider'd that the Vitriol made uſe of in this Preparation, is no more than Iron diſſolv'd by a Liquor running from the *Pyrites*, when expos'd to the Weather, which is afterwards boil'd up and ſhot into Cryſtals,

ſtals; it ſeem'd to follow as a natural Conſequence, that this Metal is the Subject on which the *Lixivium* of Blood produces the change; and this Thought gave occaſion to the following Experiments on metallick Bodies, in order to obſerve if the ſame Change of Colour could be produc'd in any of them.

To a Solution of Silver in *Aquafortis* was pour'd the *Lixivium* of Blood, which occaſion'd a *Coagulum* of a pure Fleſh Colour. The *Lixivium* made with Fleſh produc'd a whitiſh *Coagulum*, and the *Ol. Tartari* (which was continued to be us'd by way of compariſon with the other *Lixivia*) a much whiter. By the Addition of the *Spiritus Salis* to each of theſe, the Bloom of the Fleſh Colour was taken off in the firſt, but ſuffer'd no other Change. In the ſecond the *Coagulum* was a little ting'd with Blue; and in the third the White was manifeſtly improv'd. The bluiſh Tinge in the ſecond of theſe Experiments cannot entirely be aſſigned as the Effect of the *Lixivium* with Fleſh, becauſe Silver, when thus diſſolv'd, whether precipitated with Salt Water, or *Ol. Tartari*, will, after it has ſtood ſome Time, contract a bluiſh Tinge, and this from an Alloy of Copper, from which it is not entirely freed.

The ſame Liquors were made uſe of to precipitate the Mercury in the *Mercurius Sublimatus Corr.* diſſolv'd in Water; the Conſequence of which was, that the *Lixivium* with Blood produced a pure yellow; the *Lixivium* with Fleſh an Orange Colour; and the *Ol. Tartari* a dingy red. The addition of the *Spiritus Salis* to theſe, made ſome very odd Alterations; for the firſt chang'd its yellow Colour for an Orange; the ſecond, its Orange for a Blue; and the third became quite clear again without any Colour. The blue Colour in the Mixture of the *Lixivium* with Fleſh, and Solution of Sublimate, may be accounted for from the

Vitriol in the Compoſition of the Sublimate; but it will not be ſo eaſy to give a Reaſon why the ſame Colour ſhould not have been produced from the *Lixivium* with Blood, and the ſame Solution.

Copper, when diſſolv'd in *Aquafortis*, tinges the Water of a green Colour; and if to this you pour the two *Lixivia* of Blood and Fleſh, the *Coagula* are much alike, *viz.* a white ting'd with green; but when you add the *Spiritus Salis*, they both change and become of a Colour not unlike the Copper itſelf before it is diſſolv'd in the *Aquafortis*. If the *Ol. Tartari* be pour'd to a Solution of the Copper, the *Coagulum* is a pale green, which *Coagulum* the *Spiritus Salis* diſſolves, and leaves the Liquor clear, but green, as before Precipitation.

Tin-Glaſs (an imperfect Metal) diſſolv'd in *Aquafortis*, and mix'd with the *Lixivium* of Blood made a milky *Coagulum*, and by the Addition of the *Spiritus Salis*, after ſome Time ſtanding, its upper Surface chang'd to a light Blue. The *Lixivium* of Fleſh and the *Ol. Tartari* produc'd both white *Coagula*, which the *Spiritus Salis* ſcarcely alters.

Lead diſſolv'd in Spirit of Vinegar produceth much the ſame white *Coagulum*, when mix'd either with the *Lixivium* of Blood, Fleſh, or the *Ol. Tartari*, nor doth the *Spiritus Salis* make any Alteration.

By all theſe Experiments it is pretty evident, that not any of theſe metallick Bodies were affected by the *Lixivium* of Blood, ſo as to produce this fine Blue. The two Metals untried are Gold and Tin, the latter of which, when diſſolv'd in Spirit of Vinegar, has ſo near a Reſemblance to Lead diſſolv'd in the ſame *Menſtruum*, that in all Probability the Experiments would anſwer much alike in both. What may be expected from Gold, I am not yet ſo well aſſured of, as I am from Iron, which

when

when diſſolv'd in *Spiritus Vitrioli*, will anſwer all the Experiments that have been tried with the Solutions of Vitriol, and produce as fine a Colour; nor can this be owing to any Property in the Diſſolvent itſelf, which, tho' drawn from the ſame kind of Vitriol all along made uſe in theſe Experiments, yet is ſo alter'd by the violent Fire in the Production of it, as not to anſwer in any Trials to the Vitriol itſelf.

May we not therefore hence conclude, that Iron is the Metal, that is the Subject of this beautiful Colour produc'd by Means of the *Lixivium* with Blood?

VI. *The*

VI. *The Remainder of the Bills of Mortality, &c. of the ſeveral Towns of* Europe. *Extracted from the* Acta Breſlavienſia. *By Dr.* Sprengell, *F. R. S.*

A Liſt of thoſe that were Born and Buried in Breſlaw, *in the Year* 1720, i. e. *from the 25th of* December 1719 *to the 24th of* December 1720.

Buried.	
From the 25th of Decemb. to the 31ſt,	40
In January,	160
February,	107
March	139
April	192
May	158
June	120
July	131
Auguſt	182
September	189
October	130
November	132
December, only till the 24th,	127
	1816

Chriſtened.		
Males		564
Females		556
		1120
Married 460 Pair		
Among the Dead were		
Married Men		385
Married Women		186
Widows & Widowers		285
Bachelors		113
Maidens		113
Children to ten Years of Age	Boys	345
	Girls	300
Stilborn	Boys	53
	Girls	30
		1810

In the Year 1720. *In* Lignitz *were*

Married 92 Pair.
Chriſtened 283
Buried 366
Among which were 149 Children.

In Jauer.

Chriſtened of *Proteſtants* - - - - 100 Ch.
Buried 175

In Oels.

Chriſtened 180
Buried 203

In Schweidnitz.

Married 218 Pair.
Chriſtened 920
Buried 250

In Vienna.

From the 1ſt of *Jan.* to the 31ſt of *December.*

Buried 6825
Chriſtened 4126

In Lobau.

Married 58 Pair.
Born 160
Buried 355
Amongſt which were born 80 Boys and 80 Girls: Buried 95 Men and 48 Women, beſides 4 who died in Childbed: 26 Bachelors and 22 Maidens: 36 Boys and 42 Girls: 25 Chriſoms: 7 Stilborn: 55 Widows & Widowers. Likewiſe 51 Perſons between 60 and 70 Years of Age: 29 between 70 and 80: 7 betwixt 80 and 90: 1 of 90, and 1 of 99.

In Dreſden.

Married 368 Pair.
Born 1448
Among which were 719 Boys, and 641 Girls, legitimate: Illegitimate Boys 44 and Girls 44.
Buried 1733
Amongſt which were 255 married Men and 182 married Women, 189 Widowers and Widows: 88 young Men and 84 young or ſingle Women: 883 Children, *viz.* 461 Boys and 422 Girls, amongſt which were Stillborn 72, *viz.* 37 Boys & 35 Girls.

In John-George *City.*

Married 24 Pair.
Chriſtened 148

i. e.

i. e. 79 Boys and 69 Girls, among which were 2 Stilborn, and 10 Baſtards.
Buried 243
viz. 37 married Men and 18 married Women: 5 Widowers and 37 Widows, 6 Bachelors, and 18 Maidens: 64 Boys, 58 Girls, *&c.*

In St. Annaberg.
Married 18 Pair.
Born 105
i. e. 63 Boys and 42 Girls, among which were two Pair of Twins, and 4 Baſtards.
Buried 180
viz. 96 Men and 84 Women; among which were 34 married Men and 18 married Women, 1 in Childbed; 12 Widowers and 28 Widows: 12 Bachelors, and 17 Maidens: 38 Boys and 20 Girls, among which was 1 dead born.

In Schneeberg.
Married 12 Pair.
Born 89
As 44 Boys: 45 Girls; among which were 4 Stilborn, and 3 Baſtards.
Buried 157
viz. 28 married Men, and 11 married Women: 1 Childbed Woman: 5 Widowers and 25 Widows: 9 Bachelors and 10 Maidens: 68 Children, *viz.* 49 Boys and 19 Girls.

In Leipſic.
Married 314 Pair.
Born 790 *viz.* 405 Boys and 385 Girls.
Buried 1264
Among which were 233 Men and 146 Women: 86 Bachelors, and 62 Maidens: 233 Boys and 195 Girls: 9 Women in Childbed: 98 Chriſoms, *i. e.* 53 Boys and 45 Girls: 58 Stilborn, as 36 Boys and 22 Girls.

In Berlin.
Married 669 Pair.
Born 2276
Buried 2426

In Wiſmar.
Chriſtened 168
Buried 100

In Epperies.
Born 171
Buried 116 *In*

In Rawitz.

Born 134
and juſt as many Males as Females, except two Pair of Twins.
Buried 95
among which were 15 Childbed Women and 3 Stilborn.

In Dantzig.

Born 1862
Buried 1610
Married 442 Pair.
This is merely of the City, and not of the Suburbs of *Dantzig*.

A Liſt of the ſeveral Cities and Towns of the Kingdom of Pruſſia.

In the Year 1720.	*Chriſtened.*	*Pair Marr.*	*Buried.*
In Angerberg	535	88	259
Balga	638	133	395
Bartenſtein	224	63	188
Barthen	337	66	246
Brandenburg	936	213	576
Teutſch-Eulau	100	25	82
Fiſhauſen	478	103	270
Gerdauen	489	98	206
Gilgenburg	225	56	137
Inſterburg	2386	336	1398
Johanniſburg	378	68	188
Labiau	616	92	273
Liebſtadt	440	73	343
Lotzen	256	45	148
Luck	364	65	174
Marienwerder	769	145	641
Mummel	953	168	448
Neydenburg	512	104	437
Neuhauſen	243	62	141
Neuhoff	28	6	10

In

	Chri-ſtened.	Pair Marr.	Buried.
In Oletzko	688	111	261
Ortelſburg	476	82	330
Oſterode	784	92	300
Preuſs Eulau	479	112	282
—— Marck	705	130	478
—— Holland	705	125	557
Ragnit	930	93	516
Raſtenburg	610	155	452
Rhein	482	89	193
Schacken	576	96	259
Schonberg	276	51	230
Seheſten	354	62	181
Taxium	876	157	441
Tilſit	1139	187	647
the City of Koningſberg	1682	474	1402

Of the Year 1721. *In* BRESLAW.

Buried.		*Chriſtened.*	
From the 25th of Dec. to the 31ſt, 1720.	38	Males	610
January, 1721.	123	Females	585
February	129	Total	1195
March	161		
April	127	Married	405 Pair.
May	149	*Among the Dead were,*	
June	132	Married Men	301
July	86	—— Women	157
Auguſt	103	Widows & Widowers	208
September	125	Bachelors	92
October	99	Maidens	82
November	121	Children to ten Years of Age, Boys	324
December, till the 24th,	89	Children to ten Years of Age, Girls	248
		Stilborn Boys	42
		Stilborn Girls	28
Total	1482	Total	1482

In *Jauer.*

Buried 114 Chriſten'd 124.

In *Vienna.*

Buried 6490 Chriſten'd 4104

So that there died 2386 more than were chriſtened.

Among the Dead were 43 *Caſualties.*

Beſides 8 Perſons of 90 Years old.

1	- - -	91
2	- - -	92
3	- - -	93
3	- - -	94
3	- - -	96
1	- - -	98
4	- - -	99
2	- - -	100
1	- - -	105

In *Dreſden.*

Buried 1850 Chriſtened 1396 Married 404 Pair.

Among ſt the Buried were.	
Married Men	274
——— Women	206
Widowers	42
Widows	238
Bachelors	128
Maidens	93
Boys	479
Girls	400
Stilborn Males	50
Stilborn Females	28
Among the Baptiz'd were,	
Boys	701
Girls	690
Baſtards Males	40
Baſtards Females	51

In

In *LEIPZIG.*

Buried.

	Married Men.	*Married Women.*	*Bachelors.*	*Maidens.*	*Boys.*	*Girls.*	*Childbed Women.*	*Chrysoms* *Males.*	*Chrysoms* *Females.*	*Still-born.* *Males.*	*Still-born.* *Females.*	*Widowers & Wid.*	*Summa*
January	28	25	13	11	15	14	1	1	1	2	3	20	134
Febr.	17	12	15	9	16	10	2	4	3	7	2	17	114
March	28	29	13	12	14	15	0	1	3	3	3	17	138
April	31	24	9	10	17	22	2	2	3	2	0	13	135
May	25	23	10	9	23	16	3	6	5	0	0	16	136
June	28	16	17	4	15	13	2	3	6	2	2	12	120
July	7	10	7	5	8	12	2	1	4	0	3	15	74
Auguſt	14	8	6	6	19	11	3	2	3	3	2	9	86
Sept.	14	14	1	2	13	18	2	4	4	1	1	7	81
Octob.	17	10	4	7	14	20	6	7	2	7	4	5	103
Nov.	12	13	3	6	20	7	1	3	7	2	0	11	85
Dec.	16	12	2	12	19	17	10	3	3	2	1	7	94
Summ.	237	196	100	93	193	175	24	37	44	31	21	149	1300

Chriſtened.

Christened.

	Male.	Fe-males	Total	Pair marr.
Januar.	43	31	74	13
Feb.	31	23	54	30
March	34	31	65	1
April	32	38	70	
May	31	22	53	20
June	21	31	52	24
July	37	26	63	21
Auguſt	27	38	65	17
Sept.	29	46	75	30
Octob.	38	33	71	20
Nov.	37	34	71	52
Decem.	27	20	47	1
Sum	387	373	760	268

So that in this Year there died 1300. Born and Chriſtened 760. Hence there are 540 fewer born than died.

Among the Dead were
218 from 60 to 69 Years
82 — 70 — 79 old:
16 — 80 — 88 —
2 — 90 — 91 —

Beſides 1 Bachelor of 60 and 2 of 61; and 5 Maidens of 60. 62. 66. 70. and 73 Years old.

Among the Chriſtened were
8 Poſthumous:
14 Twins:
63 Baſtards:
and 2 *Jews* baptized.

In

In the *Marck.*

Chriſtened 16086 Amongſt which were 596 Baſtards.
Married 4613 Pair.
Buried - - 13511

More born 2575 than buried.
Among the Buried were 28 Perſons of 90 Years old and upwards, beſides 3 of 100, 1 of 101, 1 of 102, 2 of 104, 2 of 107, and 1 of 112 Years.

In the whole *Royal Pruſſia.*

Born 75275. Buried 58017. That there are more Born 17258 than Buried.

Next follows the Special Liſt of the Kingdom of Pruſſia.

	Chriſtened.	*Pair Marr.*	*Buried.*
In Angerberg	518	104	282
Balga	605	168	381
Bartenſtein	278	65	218
Bahrten	357	64	208
Brandenburg	944	222	695
Dutch-Elau	103	20	78
Fiſhauſen	498	112	300
Gerdauen	490	75	219
Gilgenburg	110	37	76
Inſterburg	2235	359	889
Johanniſburg	463	76	174
Labiau	569	92	277
Liebſtadt	431	94	252
Lotzen	247	68	180
Lyck	352	96	183
Marienwerder	765	162	400
Mummel	914	193	522
Neydenburg	545	121	326
Neuhauſen	234	59	138
Neuhoff	34	10	40

	Chri-ſtened.	Pair Marr.	Buried.
In Oletzko - - - - - - -	631	103	312
Ortelſburg - - - - - -	497	92	282
Oſterode - - - - - - -	369	144	183
Preuſs Eylau - - - - - -	503	113	428
—— Holland - - - - -	605	166	376
—— Marck - - - - - -	657	134	335
Rangnit - - - - - - -	791	95	336
Raſtenburg - - - - - -	738	165	537
Rhein - - - - - - - -	416	108	211
Schacken - - - - - - -	514	87	326
Schonberg - - - - - - -	255	61	147
Seheſten - - - - - - -	330	80	233
Taxium - - - - - - -	870	159	508
Tilſit - - - - - - - -	1245	185	584
the Cities of Koningſberg - -	1655	424	1776
Total	20668	4313	12406

In Ratisbon.

Chriſtened Males - - - 130
Females - - 120

Total 250

Amongſt which were 3 Pair of Twins, *viz.* 2 Boys and 4 Girls. Beſides 6 Baſtards, *i. e.* 3 Boys and 3 Girls.

Buried 220. *viz.* Married Men 41. Married Women 43. Young Men 11. Young Women 15. Children 110. *i. e.* 67 Boys and 43 Girls. Amongſt which, Widows 23. Lying-inn Women 2. Stilborn 2. Married 67 Pair.

In

In *Nurnburg.*

Buried.		*Chriſtened.*	
Married Men	234	Males	541
——Women	257	Females	543
Bachelors	48		
Maidens	73	Total	1084
Boys	232		
Girls	191	21 more born than died, amongſt the Chriſtened were 16 Pair of Twins.	
Stilborn	18		
In the Suburbs	10		
Total	1063		

In *Copenhagen.*

Born 2630. Buried 2247.

In *Amſterdam.*

The following Liſt contains 7 Years, viz. *from* 1715 *to* 1721.

Anno 1715 died 7633 Perſons.
—— —— 16 —— 7078
—— —— 17 —— 7451
—— —— 18 —— 8644
—— —— 19 —— 9726
—— —— 20 —— 7820
—— —— 21 —— 7632

In *Epperies.*

Born 214. Buried 142.

In *Dantzick.*

Born 1833. Buried 1435. Married 457 Pair.

VII. *An Account of the Diſſection of an Eye with a Cataract.* *By Mr.* John Ranby, *Surgeon.*

Sept. 21. 1723. I was conſulted by one *William Sollars*, aged Fifty, who complain'd of a Decay in his Sight; upon examining his Eyes, I found two Cataracts, that in his right Eye almoſt ripe, the other juſt forming. There being no other obvious Method of relieving him, I propos'd the Operation, but firſt advis'd him to conſult ſome others of the Profeſſion, and accordingly he advis'd with my ingenious Friend Mr. *Tanner*, who, upon viewing his Eyes, declar'd them both Cataracts, but that neither of them was as yet fit for the Operation. In the mean time the poor Man fell ill of a Fever, and died the 2d of *March*. I procur'd the Right Eye, in which the Cataract was moſt confirm'd, in order to make an exact Diſſection of it: In examining it, I found the Aqueous and Vitreous Humours in their natural State, but the Cryſtalline Humor was opake and of a foul Pearl Colour, and more ſolid Subſtance than in its natural State. The generally receiv'd Opinion, that a Cataract is a Film growing behind, or ſometimes before the Pupil, made us examine both Chambers of the Aqueous Humour, with the utmoſt Exactneſs, but all to no Purpoſe, for the Aqueous Humour had its natural Tranſparency, nor could we obſerve any thing preternatural either on the *Iris* or *Uvea*, except too great a Contraction of the Pupil. This very much ſtrengthens the Opinion of *Maitrejan*, *Briſſé*, *Heiſter*, and *Valſalva*, who have ſeverally aſſerted, that a Cataract is only an Opacity of the Cryſtalline Humour, and that it naturally proceeds from a Serous

Acid,

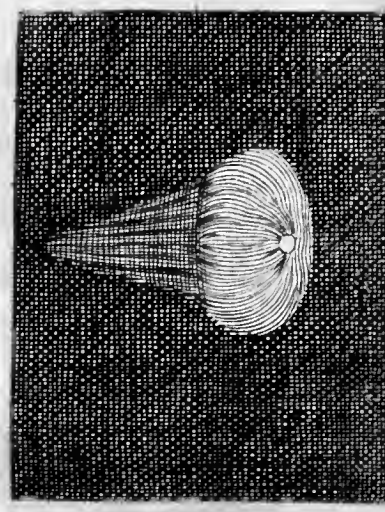
Acid, which ſo far aſtringes and corrodes its Subſtance, as to deſtroy its Tranſparency. This, *Maitrejan* confirms by an Experiment of immerging the Cryſtalline Humour in a Compoſition of three Parts Water and one of *Aquafortis*, by which he tells us it may be render'd hard and opake; but in this Point I cannot help ſiding with the learned Dr. *Pitcairn*, who has ſufficiently proved, that there is no ſuch Serous Acidity in an animal Body. To me nothing ſeems more eaſy than to deduce this Opacity of the Cryſtalline Humour from an Inflammation in the Blood, or an increas'd *Momentum* in the Fluids, with which it is ſupply'd: for in that Caſe groſſer Particles, inconſiſtent with the Tranſparency, may be impell'd into the Lymphatick Veſſels of which it is compos'd; and that there is an Inflammation is ſufficiently demonſtrated from hence; firſt, the Patient feels often a pungent Pain in the Eye, which as it is generally the forerunner of a Cataract, ſo it certainly indicates an Inflammation of the Part. Secondly, Thoſe *Maculæ*, which appear as it were ſwimming in the Air, plainly prove that there are opake Particles already enter'd the Lymphatick Veſſels, which compoſe the Vitreous Humour.

Thirdly, the *Iris*, whoſe Colour ariſes from the Blood Veſſels, as it changes from a lighter to a darker Colour, ſhews the Violence of the Inflammation, and is therefore eſteem'd a Symptom of the worſt Conſequence.

F I N I S.

L O N D O N: Printed for W. *and* J. INNYS, *Printers to the* Royal Society, *at the Weſt End of St.* Paul's *Churchyard.*

C. LEIGH and J. INNYS, at the West End of



F.3 Tuesday Oct.r 15.th at 6. a

Fig. 5.

Numb. 382.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *March* and *April*, 1724.

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I. *Obser-*

I. *Obſervations upon the Comet, that appear'd in the Months of* October, November, *and* December, 1723. *By the Reverend Mr.* Bradley, *M. A. Prof. Aſtron. Oxon. F. R. S.*

THE ſmall Comet which was ſeen in theſe Parts of *Europe*, in the Months of *October*, *November*, and *December*, 1723. was firſt obſerved in *England* by Dr. *Halley*, on *Octob.* 9. between 7 and 8 of the Clock in the Evening; it appearing then to the naked Eye not much unlike a Star of the third Magnitude. Looking at it through a Teleſcope, he ſaw ſome ſmall Teleſcopical Stars near it, whoſe Situation he noted together with the Comet's, in order to ſee which way it tended. About 9 he again viewed the Comet, and found it conſiderably moved from its former Station, having now paſſed a ſmall Star, which at the time of the firſt Obſervation was on the other ſide of it. Comparing the two Situations of the Comet together, he perceived that its apparent Motion at that time was about 8 or 9 Minutes in an Hour, in a Direction towards *Sagitta*; and that the Comet paſſed very near, if it did not wholly eclipſe the forementioned ſmall Star, whoſe place he afterwards found to be in ♒ 7° 22′ 15″ with 5° 2′ N. Latitude. From the Situation of the Comet at the time of the firſt Obſervation, he judged that it was in Conjunction with the Star at 8 *h.* 5′. equal Time. Note that the equal, and not the apparent, Time, is likewiſe made uſe of in all the following Obſervations.

The next Day he was pleaſed to communicate to me the Subſtance of what he had obſerved, whereby I was

enabled, the Night following, to ſee the Comet at *Wanſted*. The Clouds hindered me from obſerving it in the manner that I had deſigned; but I had Time enough to meaſure its Diſtance (with a Micrometer in a Teleſcope of 7 Foot) from a Star in *Aquarius*, marked ε by *Bayer*. At 6 *h*. 21′ the obſerved Diſtance between this Star and the Comet was 1° 13′ 53″, and a great Circle paſſing through the Star and Comet, made an Angle with the Vertical Circle of 60° ¼. The Comet was more ſoutherly and weſterly than the Star. By this Obſervation the Comet preceded the Star in Right Aſcenſion 1° 3′ 50″ being 39′ 5″ more ſoutherly; ſo that the Comet's Right Aſcenſion was 307° 6′ 40″ and its Declination 11° 8′ 15″ S.

The Place of ε here aſſumed is according to the *Britiſh* Catalogue, as are alſo the Places of the other Stars hereafter mentioned from which the Comet was obſerved. The Right Aſcenſions and Declinations, which are here ſet down, of ſeveral ſmall Stars that are not in that Catalogue, were determined by obſerving the Differences of Right Aſcenſion and Declination between thoſe ſmall Stars and others that were in the Catalogue, and had nearly the ſame Declinations.

The ſame Evening, at 7 *h*. 3′ a ſmall Star that was more eaſterly than the Comet, and had about the ſame Declination with it, was diſtant from it 35′ 40″. About the ſame time another ſmall Star that had nearly the ſame Right Aſcenſion with the Comet, but was more ſoutherly, was diſtant from it 39′ 58″. The Places of theſe two Stars I have not yet obſerved.

The next Night proved cloudy, ſo that I could not ſee the Comet again till *October* 12. when (the Air being very ſerene and clear) we had an Opportunity of comparing it with two or three ſmall Stars that were near it; my Uncle, the Reverend Mr. *Pound*, aſſiſting

in this and moſt of the following Nights Obſervations.

At 7 *h.* 22′ a ſmall Star, whoſe Right Aſcenſion was found 304° 40′ 23″ and its Declination 7° 8′ 22″ S. preceded the Comet in Right Aſcenſion 26′ 21″ being 10′ 42″ more Northerly. Hence the Comet's Right Aſcenſion was 305° 6′ 44″ and its Declination 7° 19′ 4″ S.

At 8 *h.* 50′ the Comet was in the ſame Parallel of Declination with another ſmall Star, whoſe Right Aſcenſion was found 305° 9′ 56″ and its Declination 7° 13′ 20″ S. and preceded the ſaid Star 6′ 20″ in Right Aſcenſion. Hence the Right Aſcenſion of the Comet was 305° 3′ 36″ and its Declination 7° 13′ 20″ S. Theſe Obſervations were made with a Teleſcope of 15 Foot furniſhed with a Micrometer, as were alſo all thoſe of the following Nights.

The next Night, *October* 13. 6 *h.* 58′ the Comet followed a ſmall Star, 4′ 10″ in Right Aſcenſion, being more Northerly than the Star 11′ 45″. The Clouds did not permit us to obſerve the Place of this Star; but its Right Aſcenſion muſt be about 304° 22′ and its Declination 6° 10′ S.

October 14. the Comet was near two Stars which are the 66th and 67th of *Aquila* and *Antinous* in the *Britiſh* Catalogue, and at 8 *h.* 57′ it followed the ſouthermoſt of them 20′ 37″ in Right Aſcenſion, being 29′ 8″ more ſoutherly. Hence the Comet's Right Aſcenſion was 303° 49′ 10″ and its Declination 4° 43′ 54″ S.

October 15. 6 *h* 35′ the Comet preceded the northermoſt of the ſaid Stars 23′ 6″ in Right Aſcenſion, being more ſoutherly than the Star 4′ 15″. Hence the Right Aſcenſion of the Comet was 303° 24′ 40″. Its Declination 3° 51′ 3″ S.

October 21. 6 *h.* 22′ a ſmall Star, whoſe Right Aſcenſion was found 301° 7′ 17″, and its Declination 0° 11′ 50″ S. preceded the Comet 41′ 6″ in Right

Aſcenſion, being 5′ 50″ more ſoutherly. Hence the Comet's Right Aſcenſion was 301° 48′ 23″ and its Declination 0° 6′ 0″ S.

October 22. 6 *h.* 24′ a ſmall Star, whoſe Right Aſcenſion was found 301° 39′ 47″ and its Declination 0° 32′ 43″ N. followed the Comet ½ a Minute in Right Aſcenſion, being 13′ 43″ more northerly. Hence the Comet's Right Aſcenſion, was 301° 39′ 17″ and its Declination 0° 19′ 0″ N.

October 24, 8 *h.* 2′ a ſmall Star whoſe Right Aſcenſion was found 301° 24′ 57″ and its Declination 1° 9′ 22″ N. preceded the Comet 0′ 37″ in Right Aſcenſion, being 5′ 12″ more Northerly. Hence the Comet's Right Aſcenſion was 301° 25′ 34″; and its Declination 1° 4′ 10″ N.

October 29, 8 *h.* 56′ a ſmall Star whoſe Right Aſcenſion was found 301° 6′ 20″ and its Declination 2° 51′ 0″ N. preceded the Comet one Minute in Right Aſcenſion, being 23′ 40″ more Northerly. Hence the Comet's Right Aſcenſion was 301° 7′ 20″ and its Declination 2° 27′ 20″ N.

October 30, 6 *h.* 20′. The ſame Star had exactly the ſame Right Aſcenſion with the Comet, being 11′ 33″ more Northerly. Hence the Comet's Right Aſcenſion was 301° 6′ 20″ and its Declination 2° 39′ 27″ N.

November 5, 5 *h.* 53′ a ſmall Star whoſe Right Aſcenſion was found 300° 35′ 00″ and its Declination 3° 45′ 30″ N. preceded the Comet 33′ 0″ in Right Aſcenſion, being 2′ 8″ more Southerly. Hence the Comet's Right Aſcenſion was 301° 8′ 0″ and its Declination 3° 47′ 38″ N.

November 8, 7 *h.* 6′ a bright Star (placed by *Hevelius* in *Roſtro Aquilæ*, but not inſerted in the *Britiſh* Catalogue) whoſe Right Aſcenſion at this time was found

found 302° 21′ 30″ and its Declination 4° 28′ 40″ N. followed the Comet 1° 7′ 40″ in Right Aſcenſion, being 13′ 3″ more Northerly. Hence the Comet's Right Aſcenſion was 301° 13′ 50″ and its Declination 4° 15′ 37″ N.

November 14, 6 *h.* 20′ a Star, whoſe Right Aſcenſion was found 301° 27′ 10″ and its Declination 4° 59′ 40″ N. preceded the Comet 5′ 35″ in Right Aſcenſion, being 5′ 50″ more Southerly. Hence the Comet's Right Aſcenſion was 301° 32′ 45″ and its Declination 5° 5′ 30″ N.

This was the laſt Time that I obſerved the Place of the Comet 'till after the Full Moon; my Affairs calling me to *Oxford*, where I had no Convenience for making ſuch Obſervations.

Dr. *Halley* and Mr. *Graham* continued to obſerve the Comet 'till *November* 20; and according to both their Obſervations that Evening at 7 *h.* 45′ the Comet followed β in *Collo Aquilæ* 6° 33′ 55″ in Right Aſcenſion, being about 4′ more Northerly than the Star. Hence the Comet's Right Aſcenſion was 301° 59′ 50″ and its Declination 5° 48′ 55″ N.

The Light of the Moon daily increaſing, prevented them from making any more Obſervations, the Comet being by this time grown ſo faint, as to become in a manner imperceptible while the Moon ſhone bright. And the faint Appearance which it made before the Moon obſtructed the Sight of it, gave little Hopes of its being to be ſeen again after the Full Moon. Notwithſtanding which on *December* 3. (being then near *Cirenceſter* in *Gloceſterſhire*) I was tempted by the Serenity of the Evening, and the Uſe of a very good Teleſcope of 10 Foot, to look for it again before the Moon roſe; and I found it among ſome ſmall Teleſcopical Stars; but it appear'd

ſo

ſo faint and dull, as made it doubtful, whether what I took for the Comet might not be a ſmall Star with a little Hazineſs about it. But this Doubt was cleared two Nights after; when I perceived that the Comet was moved from its former Situation, towards a bright Teleſcopical Star, from which I afterwards took its Difference of Right Aſcenſion and Declination, upon my Return to *Wanſted*, on *Dec.* 7. This Star's Right Aſcenſion was then found 303° 39′ 20″ and its Declination 7° 32′ 30″ N. And *Decemb.* 7. 6 *h.* 45′ the Comet followed it 3′ 15″ in Right Aſcenſion, being 14′ more Northerly than the Star. Hence the Comet's Right Aſcenſion was 303° 42′ 35″ and its Declination 7° 46′ 30″ N.

This was the laſt Night that I ſaw the Comet, tho' I belive I might have continued to have obſerved it, had not an interrupted Succeſſion of cloudy Evenings prevented ſo long, that it became uncertain where to look for it.

The forementioned Obſervations are the Principal of all that were made at *Wanſted*; and moſt of them being taken from Stars which are not in the *Britiſh* Catalogue, whoſe Places therefore are here determined, only by comparing them with ſome that were; it cannot be ſuppoſed that the Comet's Places deduced from them are altogether exact. For which Reaſon I have all along ſet down, not only the Place of the Comet and Star where it was known, but alſo the Particulars of the Obſervation, that if any hereafter ſhould be willing to examine the Tract of this Comet more nicely, they may know where to find the Stars from which it was obſerved. The Places of the Stars here ſet down are abundantly ſufficient for that Purpoſe, as will appear from the following Table, which contains the Longitudes and Latitudes of the

the Comet deduced from the foregoing Obſervations, together with the Places of the Comet calculated from the Theory of Gravity, for the Times of Obſervation on the ſeveral Days therein mentioned, as alſo the Differences between the Obſerved and Computed Places. Thoſe Differences not exceeding one Minute, ſhew that the Obſervations are not only conſonant to each other, but that the Places of the Stars are likewiſe near the Truth, ſince the Comets Places deduced from them are found all along to agree ſufficiently near with the Theory of Gravity; the Truth of which having long ſince been eſtabliſhed by its great Author Sir *Iſaac Newton*, and my worthy Collegue Dr. *Halley*, needs not the Confirmation of ſo ſhort a Series of Obſervations as was made of this Comet. But ſhort as it is, I preſume 'twill be no eaſy Matter to account for the Obſervations with the ſame Degree of Exactneſs any other way, than by that Theory, according to which the following Computations are made.

1723. Temp. Æquat.	Comet. Long. Obſervat.	Lat. Bor. Obſerv.	Comet. Long Comput.	Lat. Bor. Comput.	Differ. Long.	Differ. Latit.
D. H. ′	♒ ° ′ ″	° ′ ″	♒ ° ′ ″	° ′ ″	″	″
Octob. 9 8 5	♒ 7 22 15	5 2 0	♒ 7 21 26	5 2 47	+ 49	− 47
10 6 21	6 41 12	7 44 13	6 41 42	7 43 18	− 30	+ 55
12 7 22	5 39 58	11 55 0	5 40 19	11 54 55	− 21	+ 5
14 8 57	4 59 49	14 43 50	5 0 37	14 44 1	− 48	− 11
15 6 35	4 47 41	15 40 51	4 47 45	15 40 55	− 4	− 4
21 6 22	4 2 32	19 41 49	4 2 21	19 42 3	+ 11	− 14
22 6 24	3 59 2	20 8 12	3 59 10	20 8 17	− 8	− 5
24 8 2	3 55 29	20 55 18	3 55 11	20 55 9	+ 18	+ 9
29 8 56	3 56 17	22 20 27	3 56 42	22 20 10	− 25	+ 17
30 6 20	3 58 9	22 32 28	3 58 17	22 32 12	− 8	+ 16
Nov. 5 5 53	4 16 30	23 38 33	4 16 23	23 38 7	+ 7	+ 26
8 7 6	4 29 36	24 4 30	4 29 54	24 4 40	− 18	− 10
14 6 20	5 2 16	24 48 46	5 2 51	24 48 16	− 35	+ 30
20 7 45	5 42 20	25 24 45	5 43 13	25 25 17	− 53	− 32
Dec. 7 6 45	♒ 8 4 13	26 54 18	♒ 8 3 55	26 53 42	+ 18	+ 36

In

In order to determine the Orbit of this Comet, I ſuppoſed it to deſcribe a *Parabola* agreeable to what is delivered in the third Book of Sir *Iſaac Newton*'s *Princip. Math.* and then I found the Inclination of the Plains of the Orbit and Ecliptick 49° 59′. The Place of the Aſcending Node ♈ 14° 16′. The Place of the Perihelion ♉ 12° 52′ 20″. The Diſtance of the Perihelion from the Node 28° 36′ 20″. The Logarithm of the Perihelion diſtance 9.999414. The Logarithm of the Diurnal Motion 9.961007. The Time of the Comets being in its Perihelion, *Sept.* 16. 16^h 10′ equal Time. In its Orbit thus ſituated, the Motion of the Comet was Retrograde or contrary to the Order of the Signs.

From theſe Elements, by the Help of Dr. *Halley*'s general Table for Comets (to which they are adapted) I computed the Places in the forgoing Table; which agreeing with the obſerv'd Places as near as the Obſervations themſelves agree with one another, ſhew that it would be a vain Attempt to pretend to determine the true Ellipſe in which this Comet moves, or its Periodical Revolution, from ſo ſmall a Part of its Orbit as that was, which it deſcribed between the firſt and laſt of the forgoing Obſervations; this therefore muſt be left to Poſterity, eſpecially ſince it is certain, that this Comet is not one of thoſe of which Obſervations have hitherto been tranſmitted to us, ſufficient to determine the Situation of their Orbits.

The *Nucleus* of this Comet was very little, for it appear'd but of a ſmall Diameter when I firſt ſaw it, although it was then above three times nearer to the Earth, than the Sun is at its mean Diſtance. Its Tail was then hardly diſcernable with the naked Eye, but through a Teleſcope one might perceive a faint Light extending itſelf above a Degree from the Body.

I have not yet heard that this Comet was ſeen before *October* 6. although it was in a proper Situation to have been obſerved in the Morning, moſt part of *September*, eſpecially from the Time it was in its Perihelion, 'till near the End of that Month. For about that Time it croſſed the Milky-way between the Maſt of the Ship and the Head of the great Dog, paſſing between the bright Stars in the Body and Tail of the great Dog, towards the Head of the Dove, were it was about *September* 29. being by that time got ſo far towards the South-Pole, as not to riſe above our Horizon. From thence it paſſed under the Tail of *Xiphias* within about 15° of the South Pole of the Ecliptick; and moving on between the Head of *Hydrus* and the bright Star in *Eridanus* called *Acarnar*, it went by the Stars in the Body and Neck of the Crane about *October* 5. when it came again above our Horizon. From hence paſſing under the Tail of the Southern Fiſh, and between the Stars in the Shoulder of *Capricorn*, it croſſed the Ecliptick, *October* 8. in about 8° ½ of *Aquarius*. From thence it moved on by the Hands of *Aquarius* and *Antinous* towards the Head of the *Eagle*, according to its Courſe before deſcribed.

The Comet was in Oppoſition to the Sun *Octob.* 1. when it had near 74° Southern Latitude, and alter'd its Longitude two Signs in a Day. About *October* 3. it wasin its Perigœon, or neareſt Diſtance to the Earth, being then almoſt ten times nearer to it than the Sun is at its mean Diſtance; and its apparent Motion was then about 20° in a Day, and when I laſt ſaw it, 'twas above twice as far off as the Sun.

II. *Extract of a Letter from the Right Honourable the Lord* Paiſley, *F. R. S. to Mr.* George Graham, *F. R. S. With ſome curious Figures of the ſame Comet.*

HIS Lordſhip being at *Witham* in *Eſſex*, where he had the Advantage of a very clear Sky, firſt diſcover'd this Comet on *Friday* the 11th of *Octob.* laſt about 7 in the Evening; it then appeared not much unlike a Star of between the 4th and 5th Magnitudes, but a Hazineſs round the Head, and ſome Light ſtreaming from it on that Side that was oppoſite to the Sun, induced him immediately to look upon it as a ſmall Comet; which his Obſervation the next Evening abundantly ſatisfy'd him of. His Lordſhip was very particular in the Notice he took of its Appearance, and was pleaſed to communicate the three curious annexed Figures [*Fig.* 1, 2, 3.] of it, repreſenting it on three ſeveral Nights, *viz.* the 11th, 13th and 15th of the ſame Month; ſome time after which the Tail became ſo inconſiderable as hardly to deſerve any farther Deſcription; as will be readily judged from the Decreaſe of it between the 11th and 15th Days of the Month. The Tail was viſible on the 11th to near a Degrees Diſtance from the Body, as his Lordſhip found by comparing it with ſome known Diſtances in the Heavens; it was of a dusky Light not unlike a Cloud growing darker and darker towards its Extremity, as is expreſs'd in the firſt Figure, where, as well as in the two following, the white Speck in the Head is intended to expreſs the Bright-

neſs

neſs of a ſmall Star; from the Compariſon of which with the Tail the Brightneſs of the latter may in ſome ſort be collected: The Tail appear'd ſharper, and not ſo much ſpread in the two following Obſervations, and in the laſt did not exceed one third Part of the firſt Length; it was then of a much darker Colour, which made the Difference between that and the Head more obſervable, the Head yet appearing ſufficiently bright. For ſome following Nights his Lordſhip's Obſervations were interrupted by cloudy Weather, after which the Comet was ſo far diminiſh'd, as only to be known by its Motion, its Appearance being no ways diſtinguiſhable from that of a ſmall nebuloſe Star.

III. *Obſervatio ejuſdem Cometæ ab Illuſtriſſimo Domino* Franciſco Bianchini *habita* Albani *Menſe* Octobri, 1723. *& ab eodem Ulyſipponem miſſa* P. Joanni Baptiſtæ Carbone *Soc. JESU. Communicavit* Iſaacus Samuda, *M. D. Col. Med.* Lond. *L. S. R. S.*

DIE 17 *Octobris*, poſtquam Jovialium Comitum ſitum obſervaſſem fortè in Conſtellationem *Capricorni* oculos conieci; cumque aſtra ſingula percurrerem, in quamdam veluti nebuloſam ſtellam incidi, cæteris ſane grandiorem, quam tamen ibidem loci numquam antea obſervaram. Rei novitate perſcrutaturus, eo Teleſcopium direxi, ſtatimque Cometen eſſe deprehendi; ſiquidem tenuiſſimæ nebulæ globus apparuit, ejuſque in medio veluti lucidus nucleolus. Idem quoque nudis oculis diſcernere licuit; & præ-

ter nebulam, ſeu Cometæ atmoſphæram, brevem quoque caudam, quæ ad orientem vergebat, eratque hujuſmodi. [*Fig.* 4.]

Ne me igitur ea occaſio præteriret, conſuetas circa illum obſervationes inſtitui, ut ejus Longitudinem, Latitudinem, propriumque motum deprehenderem.

Et quidem prima nocte, die nempe 17. ſupradicta, tranſiit per Meridianum (qui penè cum Romano coincidit) circa horam ſeptimam 44′ poſt Merid. ejuſque diſtantia à Zenith, 69° 29′.

Hora 8. 11′ 30″ diſtantia Cometæ à *Fomahant Aquarii*, intercepta eſt, 20° 33′ & hora 8. 17′ 30″ diſtabat à Stella β in humero dextero *Aquarii*, 21° 8′. Proindeque verſabatur Cometes in 11° 54′. *Aquarii*, cum Latitudine Auſtrali ab Ecliptica 11° 10′ circiter.

Die 21. erat adeo proximus Stellæ ε in Lino ſupra manum ſiniſtram *Aquarij*, quàm ipſa ε eſt proxima Stellæ μ minori in eodem Lino, conſtituebatque Cometes cum utraque Stella ε, μ rectam Lineam, ſic [*Fig.* 5.] Ex hac igitur obſervatione, & ex Aſcenſionis rectæ nec non declinationis differentiâ inter Cometen, & ſupradictam Stellam ε quam diligentiſſime obſervavi, infertur locus Cometæ fuiſſe in 6° 45′. *Aquarii* cum Latitudine Boreali ab Eclipt. 8° 5′.

Hinc etiam infertur qualis Cometæ motus proprius fuerit, & quale iter ; per planum ſcilicet circuli maximi ſecantis Eclipticam in gradu 9 *Aquarii*; & conſtituentis cum eadem Ecliptica angulum 80 graduum circiter.

Reliquis diebus eadem ſemper proportione movebatur, magiſque in dies elongari à terra viſus eſt.

Paral-

Parallaxim nullam ſenſibilem, etſi pluries intentaverim, deprehendere potui; proindeque maxima ejus diſtantia à terra credenda eſt.

Hactenus Illuſtriſſimus Dominus *Franciſcus Bianchini* in Mathematicis Scientiis apprimè eruditus, & in obſervando, quoad noverim, accuratiſſimus. Ejus obſervatio à noſtra, mea ſcilicet, ac Prioris Dominici *Capaſſi* vix in uno aut altero minuto quoad latitudinem diſcrepavit, cæterùm omninò conformis. Quapropter nec illam hic arbitror apponendam.

IV. *Obſervations about* Waſps, *and the Difference of their Sexes. By the Reverend Mr.* Derham, *F. R. S. Prebendary of* Windſor.

IN the Beginning of *July* laſt 1723. having frequent occaſion to be on the Top of our *Collegiate Chapel* in *Windſor Caſtle*, I obſerved many *Waſps* flying about it, and particularly frequenting a Covering of Deal Boards, and the Pieces of Timber lying on the Leads. I took notice that moſt of theſe *Waſps* were of a larger ſort than uſual, and thought they came thither to gnaw the Wood, and to carry it away in Mouthfuls to build their Neſts; the Artifice of which I have taken notice of in my *Phyſ. Theol.* particularly *B.* 4. *ch.* 11. *note* 21. and *chap.* 13. *not.* 12. But having caught ſome of them, and amongſt the reſt a large *Queen-Waſp* (which ſort of Waſp, by the by, never engages in any Labours of the Colony) I began to be more ſtrict in my Obſervations, and on *July* 6. I obſerved a Cluſter of only three *Waſps* cloſely embracing each other; one of which was a large *Female*

male Waſp, the other two of a leſſer ſort. This more excited my Curioſity. And ſoon after, I found 8 or 10 *Waſps* cloſely hanging together, and divers other ſuch like Parcels. In the midſt of all which was conſtantly a *Queen-Waſp*, and only one; the reſt being always of a different Sort from either the Queen or the common Waſps; which gave me a Suſpicion of their being *Male* and *Female*. And therefore examining another Company of them with greater Strictneſs, I found the *Queen-Waſp*, *in coitu*, with one of the other Waſps, ſo cloſely joined Tail to Tail, that it was ſome Time before they were parted.

After this I caught all the Waſps I could, on the top of our Chapel, but could not ſee one of the *common labouring Waſps* among them; but all were for the moſt part *Male Waſps*, with now and then a *Queen*, or *Female*, among them, and ſhe generally *in coitu*.

And now from this Hiſtory of my Obſervation, it appears, That there are three ſorts of *Waſps*; The *Queens*, or *Females*; the *Kings*, or *Males*; and the *common Labouring-Waſps*; each of them very diſtinct.

The *Queen*, or *Female-Waſp* (by many called the *King-Waſp*) is much longer in the Body, and larger than any other Waſp.

The *Male Waſps* are leſſer than the *Queens*, but as much longer and larger than the *common Waſps*, as the *Queen* is longer and larger than theſe. Theſe *Males* alſo have no Stings, which the *Queens* and *common Waſps* all have. And theſe are thoſe which *Mouſet* ſaith Authors call Ἀκέντρους, and take to be *Females*, although he is of another Opinion, imagining all Waſps to have Stings; upon his examining a Waſp's

Neſt

Neſt, at *Ham*, *Anno* 1587. in which he found no Waſps without a Sting. But I wonder how that curious Enquirer miſſed of theſe ſting-leſs Male *Waſps*. Surely he was too haſty in his Examination, and not being aware of the difference, he thought the *Males* (which are but few in number to the *labouring Waſps*) were the ſame and had Stings as well as the reſt; or elſe he made his Enquiry at a Time when perhaps the Males had deſerted the Neſt, which probably they may do, as the *Male* or *Drone-Bees* are forced to do: or elſe the Year 1587. (in which *Mouſet* made his Obſervation) might produce fewer *Waſps*, at leaſt fewer *Male Waſps*, than this laſt Summer of 1723. did, in which I made my Obſervation; which was obſerved to have a greater abundance of Waſp-Neſts than hath been known in many Years. And in all the Neſts that I ſearched into, I conſtantly found *Male Waſps*, either many or few, according to the Size of the Neſt, and Number of Waſps therein. And the Part of the Neſt where theſe Males are bred, or at leaſt where I found them moſt to reſide, was chiefly the two uppermoſt Cells, or Partings, between the Combs, but one.

But to return to the Diſtinction of our *Male Waſps*. Another thing by which they may be known from other Waſps, is their *Antennæ*, or *Horns*; which are longer and larger than either thoſe of the *Queen*, or *common Waſps*; and with them they ſeem, in running, to feel more than the others do.

But the grand and chief Difference, are the *Parts of Generation* of theſe *Male Waſps*, quite different from other Waſps. Which I diſſected with all Care, and ſhall give a Deſcription of, as well as I can, without Figures, which I could not get drawn, partly for want of a Deſigner, and partly from my Removals between *Windſor* and *Upminſter*: in which Time the

Parts are ſo dried up, or eaten with Mites, that they cannot be drawn 'till the Return of Waſp-time.

For the Diſcovery of theſe Parts, if the *Alvus* be preſſed, an *Horny* or *Shell-like Part* will be thruſt out, of a ſhining black Colour, which conſiſts of two Parts like Shells, ſomewhat reſembling the *Caſtagnets* uſed in Dancing; at the extreme Part of each of which grows an *Hook*, ſomewhat like thoſe of the Earwig's Tail, but much leſſer; in the Middle, between theſe Hooks, appear three Parts, the middlemoſt of which is a ſtiff brown *Tube*, very curiouſly made, with the Fore-part like a Spoon or Ladle, and the other End (within the Body) is neatly branched and braced to each Side within the two Shells I ſpake of. A little above which Branching, is a φυμάτιον or *Swelling*, like that of a Dog's Pizzle, and perhaps ſerves for the ſame Uſe, if this Tube is (as I imagine it is) the *Penis* of the Waſp.

On each Side this *Penis*, lies a ſtiff Part (in Number two) branched at the Top with ſomewhat like Hairs, giving them the Reſemblance of *Bruſhes*. At the Bottom of which are two curious black *Cells*, with an *Opening* on one Side like that of the *Concha Veneris*, with ſmall whitiſh Hairs growing on one Edge thereof. What the Uſe of theſe two Bruſh-like Members may be, I know not, unleſs it be to ſtrengthen and ſupport, or direct the *Penis in coitu*, or provoke therein.

Behind all theſe Parts, which I have deſcribed, more within the Body lies a long contorted white *Veſſel*; which at firſt I took to be the real *Penis*, penetrating the *Ladle-like Tube* I ſpake of. But upon farther Examination, I rather take it to be the *Spermatick Veſſel*.

As to the Uſe of the two little *Hooks* I mention'd at the End of the *Uropygium*, or *Shells*, I take them

to

to be, to catch hold of the *Female*'s *Podex*, and to direct and assist the Penetration of the *Penis in coitu*.

As for the *Parts of Generation* in the *Queen*, or *Female-Wasps*, nothing was to be seen so remarkable as in the *Male*; but those Parts are very like what we see in the *common Labouring-Wasps*: Indeed, with the most accurate Observations I could make with my Microscopes, I could not perceive any Difference at all. For which Reason I suppose it is that most of the Writers upon *Wasps* and *Bees*, have been very confused and wavering about the Sexes of these two Tribes of Insects. It would be endless to cite the Authors and their Opinions, especially concerning the *Bee-Tribe*. I think *Swammerdam* (*a*) (who as he was one of the first that rejected Equivocal Generation, so was one of the most judicious Writers of Insects) that his Opinion, I say, is the most just, *viz.* That of *Bees*, there are three Sorts, *viz.* 1. Rex, *aut verius* Regina, *siquidem sequioris sexûs est.* 2. Fuci, *qui Masculi proprie sunt.* 3. Apes Operariæ, *quarum Sexum distinguere non possumus, cùm in iis nec Masculas nec Fœminas partes observemus: quæ perbellè distinguntur in* Fucis *seu* Regibus, *&* Reginis, *quæ tralatitio errore* Reges *solent salutari. In* Reginis *certè invenimus* Ovarium *apud incomparabilem illum Anatomum* Joh. van Horne, *&c.*

As for what is related by *Aristotle*, *Pliny*, *Virgil*, or any other ancient Authors, or by our more numerous Moderns, concerning the Production of *Wasps* out of *Horses*, or *Bees* out of *Oxen* or *young Bullocks*; as also of their *Polity*, their *Emperours*, *Kings*, *Dukes*, and *common Subjects*, their exact *discipline and Justice*,

(*a*) Swam. Hist. Insect. *p.* 92.

their ſtrict *Temperance*, and other *Virtues*, with a great deal more of ſuch like Stuff: This is ſo very whimſical, that it is not worth while to take any farther Notice of it: But there is a Story ſeriouſly told by *Mouſet*, (*b*) that deſerves our Obſervation, *viz.* That *in the Year* 1582, *being on the higheſt Ridges of the* Cartmel-Hills, (I ſuppoſe in *Lancaſhire*) *he ſaw among the Rocks two Species of Waſps deſperately fighting: That they differed only in Magnitude; that the larger truſted to their Strength; and the leſſer to their Numbers, there being ſix of the leſſer engaged againſt only one of the larger ſize, and that the Battle was not in the Air, but among the Graſs, and laſted for ſome Hours in the hoteſt Sun, not being at an end in two or three Hours ſpace.* The Cauſe of this Engagement *Mouſet* thinks was, that the *great Waſps* are wont to rob the *leſſer* of their Honey and Young, or do them ſome other ſuch like Miſchief; and the *leſſer* being very revengeful, and naturally full of Courage, did outbrave even *Mars* himſelf in aſſaulting their Enemy. But this Engagement I take to be ſuch another, as that which I have given the Hiſtory of, namely one under the Conduct of *Venus*, not of *Mars*.

And as there is no Doubt to be made of its being ſuch, and that the Engagement ſeen by *Mouſet* was on the higheſt Tops of *Cartmel* (*in ſummis Cartmeli montium jugis*) as that I ſaw was on the very Top of our *Chapel*, it may deſerve Obſervation, whether the *Waſps* ever copulate in lower Places, obvious to Diſturbance, and every one's Eye, or only on ſuch Eminencies where they can be more out of Sight, and conſequently in greater Safety: And if at any time they ſhould be found in Copulation, they may all

(*b*) Mouſet Theat. Inſect. *l.* 1. *c.* 8.

all with Safety be ſeized with the naked Hand, provided it can be ſecured againſt the *Queen-Waſp*, which is the only one in the Company that is provided with a *Sting*.

For a Cloſe of theſe Obſervations about the Sexes of Waſps, I ſhall take Notice of *Mouſet*'s Experiment, (which I try'd) *viz. If you take a Waſp by the Feet, and ſuffer her to buz, that thoſe Waſps, which have no Stings, will fly to her, but not any that have Stings. Which ſome*, he ſaith, *uſe as an Argument to prove that ſome Waſps are Males, ſome Females.* This Experiment I was minded to try with a *Queen-Waſp*, more eſpecially, not knowing but that *Waſps*, particularly the *Males*, might be as fond of their *Queens*, as the *Bees* are of theirs, who will not forſake them, but will live and die with them. But I did not find it to ſucceed ſo among the *Waſps*. For although I put ſome *Queen-Waſps*, and others alſo, near the Entrance of ſome large *Waſp-Neſts*, yet I did not ſee any Flock near them, only now and then one of the common Waſps, for a little while, to ſee their Fellow confined. But indeed the *Queen-Waſps* which I confined were weak, and did not buz long; as alſo the time of Copulation was probably paſt, it being *Auguſt* 12. when I try'd the Experiment.

V. *Obſervationes duæ rariores, ex Literis Viri Doctiſſimi* Johannis Huxham, *M.D. ad* Jacobum Jurin, *R. S. Secr.*

OBSERVAT. I.

Ingens Omentum.

VEterani cujuſdam Militis uxor dolore colico & vomitu diu laboraverat, quibus ſupervenit ventris tumor durus, qui auctus indies magnam in molem excrevit. Jam evomuntur omnia, bilis atra, ſincera, tandem & ipſæ fœces alvinæ. Dolor vero perſæpe immanis in ſiniſtro præſertim hypochodrio, fotus, cathartica, enemata, anodyna, plurima Pharmacopoeio *Collins* adhibentur; incaſſum omnia; permanet dolor, increſſit uſque tumor, digitorum preſſui haud magis cedens quam ſi lignum fuiſſet. Ex hocce tumore plura quaſi tubera enata ſunt, quorum unum alterumve caput puerile, alia pugnum virilem, magnitudine æquabant: eminebat autem maximum in ſiniſtro hypochondrio, ubi graviſſimum perſenſit dolorem, ita ut ſæpius exclamaverit ſe voluiſſe tumorem ibidem loci fore perfoſſum. Increſcente tumore facta eſt valde dyſpnoica. Tanta tamque atrocia per menſes fere 14 perpeſſa fuerat mulier, cum tandem grata venit morborum requies, mors.

Mihi enarravit caſum Pharmacopola, rogavit inſuper ut viſcera inciſo abdomine inſpicerem, quod cum alio medico feci perlubens.

Nudato

Nudato cadavere montoſum obſervavimus ventrem; reliquum vero corpus ſumma macie confectum. Secto abdomine in conſpectum ſe dedit ingens quaſi ſebi maſſa (niſi quod colore fuit minus alba) totum ventris cavum adimplens, ita ut nec ventriculus, nec jecur, neque inteſtina apparerent ulla; adhæſerat enim omentum hoc enorme peritonæo pluribus in locis, in utroque præſertim hypochondrio, ubi & latior & firmior fuit cohærentia. Secto autem in modum crucis peritonæo, & ab omento ſeparato, conſpeximus illum in pelvim fere detruſum, omentum etiam toti jocinoris cavo annexum, ventriculum hac mole oppreſſum, uti & duodenum, colon & jejunum: cum adipoſo renum involucro (ſiniſtro præcipue) connexum & quaſi confuſum fuit; adeo ut colon, altius paulo quam in rectum deſinit, ſebo hocce concreto fuit penitus involutum, hinc fœcibus præcluſum iter, hinc illæ lachrymæ, hinc dolores illi ſæviſſimi, quibus ante mortem excruciata fuerat mulier: & ut hic obiter notem, per plures dies ante mortem nec ſua ſponte nec vel clyſmatibus acerrimis irritata alvus fœces dejecerat ullas.

His luſtratis ingens ſeparavimus omentum à jecore, ventriculo, ſchirroſo pancreate, inteſtinis, tandem & à meſenterio, & ab interna peritonæi lamina renibus inſtrata. Splen fuit hac maſſa quodam modo demerſus, contractior & quaſi coriaceus. Hunc quaſi ſebi montem exemi denique & per famulum noſtrum domum miſi: erat autem animus omentum hoc morbidum accuratius perſpiciendi, cumjam enim advesperaſceret defuit opportunitas.

Pendebat omentum hocce ℔ xvi ½ *Avoird.* nec tamen abſtulimus unam ſaltem alteramve libram partibus adhærentem. Pondus hercle maximum! Si conſideremus ex obeſiore homine omentum vix libras pendere tres, majus certe inveni antehac nunquam, quanquam

quanquam & ſecui & diſſecta vidi plurima hominum cadavera. Fateor equidem longe majus à *Gregorio Horſtio* in ſuis obſervat. memoratum fuiſſe, majus etiam in Ephemerid. *German.* ann. x. veruntamen hoc quoque noſtrum monſtris annumerari merito debet.

Hujus & in extima parte, nec non in interiore ſubſtantia, plura obſervavi vaſa ſanguinea, eaque certe maxime dilatata, quorum aliqua penna anſerina majora, aliqua vero quaſi in aneuriſmatibus terminantia. Ex horum aneuriſmatum (ſi ita dicam) maximo uncias circiter ſex nigricantis ſanguinis extraxi cum quibuſdam grumis albidioribus; anne adipis particulæ à venis epiploicis abſorptæ atque morbo congelatæ?

Ex plurimis lobis conflari mihi videbatur maſſa hæcce, ſibi invicem arcte hærentibus; aliquos tamen ſeparavi, quorum pauci pomo minori fuere magnitudine æquales, forma haud abſimiles. Media hujus pars, cætera durior, cultro haud facile cedebat.

In iſthoc cadavere ſequentia etiam fuere obſervatu digniora. Inferior hepatis pars in ſchirrum abierat. Ex veſica fellea plures exemi calculos, carbonem foſſilem colore referentes, friabiles, aquam innatantes, quos bilem haud improprie nominares ſplendidam; his quippe quum inerant plurimæ particulæ micantes, haud dubio ſalinæ.

Meſenterii glandulæ fuere ſchirroſæ, imo quædam in ſubſtantiam pene lapideam induratæ. Inteſtina tenua fuere inflammata, colon cum cæco fere totum gangrena correptum, etiam proceſſus ipſe vermicularis.

Ren uterque fuit ſanus ſatis: dexter autem emiſit ureteres duos: cum vero renem diſſecuiſſem, percepi facile, quid eſſet in cauſa, cur ureter hic fuit duplex. Renis enim pelvis ſepto quodam fuit diviſa, digitum

tranſverſum

tranſverſum craſſo, ejuſdem perfectè ſubſtantiæ cum reliquo renis parenchymate.

Hic fuit ren quaſi duplex, cui duplex pelvis, ureterque duplex.

Erant in imo abdomine feri ſubcruenti libræ quaſi duæ.

Malum hoc immedicabile penitus viſum eſt, niſi quis morbi principiis obſtitiſſet.

OBSERVAT. II.

Salivæ Color inſolitus.

DOminus *Fox*, quadragenarius, gracilis, bilioſus, per menſem unum alterumve elanguerat, nauſea, ictero & doloribus colicis vexatus: tandem ex epoto largiter vino pomaceo vehementiſſimo colices paroxyſmo correptus me conſulit, Vomitabat omnia, urinæ parum reddebat coloris lixivii, quæ ſedimenti plurimum deponebat ſubviridis.

Ego ſtatim illi potionem ex Ipecac. emeticam propinari jubeo, infuſo dein C. Bened. affatim hauſto plurima viſcoſa, bilioſa, rejecit: tum enemate Terebinth. injecto alvus bis terve reſpondit. Ex ſumpta miſtura anodyna vomitus & tormina comprimuntur; jam de languore ſummo & abdominis diſtentione conqueſtio; paulo etiam poſt recrudeſcit dolor. Bolum præſcripſi ex Jalap. ℈j. Calomelan. gr.viij. Spec. Diambr. gr. vj. Laud. ſolid. gr. j. Syrup. de Sp. Cerv. q. ſ. quam primum ſumend. cum Tinct. ſacr. ℥ij. Hinc omnia pacata. poſt horas 12 ter fundit alvus perliquida, bilioſa. Ex ſumpto dein hauſtu anodyno nox tranquilla. Mane de dolore & faucium tumore queribundus craſſa, ſubfuſca, expuebat; mox abunde effluit ſaliva colore viridiſſima, bili porraceæ quam ſimillima, niſi quod tenuior. Perduravit fluxus hic ſalivæ viridis horas

quaſi 40, quo tempore ſputavit quantum vix caperent ſextarii duo: ad flavedinem dein vergebat color fluoris eo uſque dum ad inſtar ſolutionis Gut. Gambæ evaſit: quantitas autem aucta potius quam diminuta.

Perſtitit & hicce color per horas etiam quadraginta, dein ſenſim pellucida facta eſt ſaliva, atque ſubito, uti oborta eſt, evanuit penitus ſalivatio.

Intra biennii ſpatium bis terve in icterum inciderat ante hujus morbi acceſſionem. Decem abhinc annis maxima ſalivatio ſponte erumpens hunc ipſum hominem in ſummum vitæ diſcrimen intulerat; ſalvus autem evaſit ope doctiſſimi expertiſſimique Medici, D. D. *Pyne*. Tunc vero temporis ne vel ♀ii hilum ſumpſerat, nec ullo modo fuerat colorata ſaliva.

In hac hiſtoria obſervatu haud indigna quædam occurrunt. Salivatio nempe ſponte exuſcitans, vel ex tantillo forſan Calomel. excitata. Calomel autem probe paratum novi, quippe qui plurimas ejuſdemet doſes, quarum aliquas ad ſcrupul. integrum, exhiberi juſſeram, ne vel minimo ſequente ſalivationis ſigno.

Occurrit porro, quantum ego ſaltem ſcio, ſalivæ inauditus color. Saliva viridi ita penitus tincti erant dentes & fauces quaſi ærugine obducti fuiſſent: permanſit dentium color viridis quatuordecim poſt diebus quam convaluit.

Notandum præterea fluxum hunc ſalivæ fuiſſe criticum, quo judicatus eſt & icterus & colicus morbus; ab inceptante enim ſalivatione ne vel minimus dolor ventris, & color cutis ſubviridis ſenſim evanuit, urinam etiam reddidit copioſius; ſed ſubnigram. Ille vero, qui ante hunc fluxum valde languidus fuit & jam quaſi moribundus maximum ſalivæ evacuationem εὐτέρως φέρϛ.

Serum Ictericorum tinctum eſſe bile omnibus notum eſt: nec alicui forſitan videatur magis mirandum

ſerum

ſerum flavum poſſe per glandulas ſalivares excerni quam per ſeroſa cutis vaſcula, quod tamen ictero laborantibus contigiſſe ſæpius obſervavimus, admoto prius epiſpaſtico: de urina bilioſa nil dicam. Unde vero color ſalivæ viridis haud ita facile dictu. Hujus opinor ego cauſam procatarticam eſſe pomacei vini potationem.

Bili ſi miſcueris acidum color oritur viridis. Hinc torminoſæ infantum dejectiones virides, lacte in ventriculo aceſcente. Hinc porracea bilis. Ponamus jam acidum pomaceum a venis lacteis vel & a meſaraicis forſan, abſorptum, ſeroque ſanguinis bilioſo permixtum: quid eveniret inde? Credo equidem flavi ſeri coloris in viridem permutatio. Profiteor hercle, acida quæcumque a vi corporis vitali in alcalinam naturam mutantur, etiam ipſum vitrioli acidum: langueſcentibus autem viribus, concoctione læſa, lympha effœta facta, facta & inerte bile, queis adde ſanguinem tardius circulantem, acida parum ſubacta, nec in primis viis, nec in ipſo ſanguine, acidam exuunt naturam. Hoc teſtantur acidi debilium ſudores, aciduſque in ore ſapor a febribus inflammatoriis convaleſcentium, quibus ad reſtinguendam febrem largius fuerat exhibitus acidus potus. Sæpius certe memini me vinum pomaceum largiter potâſſe brevique urinam ipſiſſimum potus odorem ſpirantem reddidiſſe. Curabam nuperrime mulierem ſumma hæmorrhagia, durante diu, correptam, cui inter cætera plurimum fuerat exhibitum Elix. Vitriol. Mynſich. ceſſavit tandem hæmorrhagia, debilis autem inde reddita mulier incidit in leves ſpaſmos, artuumque dolores, ventris tormina diarrhœa comitante, plures excrevit alvus dejectiones viridiſſimas, ſtercoris vaccini ſimillimas, taleſque per plures dies, quanquam Rhabarb. fuerat ter exhibitum ad expurgandas acidi particulas, inteſtinis

adhærentes. Hic ſal acidus ſanguini immiſtus, poſt dies plures iterum è ſanguine per inteſtina rejectus eſt, vi vitæ nullo fere modo ſubactus.

Microſcopio olim obſervavit *Leeuwenhoekius*, quod chyli ſales acidi a bile retundantur & comminuantur, quum vero in jecore ſubſiſtat obſtructa bilis, integris viribus vaſa lactea permeat acidum. In ægroto autem noſtro bilem obſtructam fuiſſe, patuit inde, quod alvus fuerat compreſſior, & fœces ante vomitum medicamento commotum reddidit albidas.

Hypotheſis fortaſſe noſtra illuſtretur magis, ſi perpendamus unde oriatur virginum χλωρῶν color herbeus (ut cum *Plauto* loquar). His enim fructus immaturi, acetum atque id genus alia, ſummæ ſunt cupidiæ, vitiatis autem concoctionis organis, parum immutata ſanguini miſcentur, bilemque & ſanguinem obruunt acido. Hinc glandulæ obſtructæ, hinc facies ex viridi pallida, tumor hinc hydropicus; atque quod non levis eſt momenti jecur ſchirroſum ex chloroſi præmortuarum ſæpe inventum fuit viride. Jam paucis reſtat dicendum unde colori ſalivæ viridi ſucceſſit color flavus, quod inde factum arbitror, quia poſt acidum a ſanguine partim ablegatum, partim & a motu ſanguinis, ob cardiaca exhibita, aucto deſtructum, ſuperfuit ſolum ſuperflua bilis ſanguine expellenda, quod & brevi feceſſit, reſeratis bilis poris, & per patulos ſalivæ ductus mirandum in modum excreta.

VI. *Part of a Letter from Mr.* Thomas Robie, *Phyſician in* New-England, *to the Reverend Mr.* Derham, *F. R. S. Concerning the Effects of Inoculation; The Eclipſe of the Sun in* November 1722; *And the Venom of Spiders.*

Salem, Tueſday June 4. 1723.

WE don't as yet ſee any ill Effects of Inoculation, but the Inoculated are as yet as well, and ſome of them a great deal better than ever; as for the ill Conſequences that have been in *England*, I can't tell what they may ariſe from, but I can hardly think they are the genuine Effects of Inoculation, but may ariſe from ſome previous Diſpoſition to ſuch Diſtempers as you mention, or for want of due Evacuations after Inoculation, and too ſoon healing the Places of Inciſion; but I dare not pretend to ſay any more, and ſo leave it to the Opinion of better Judges; but only aſſure you I don't know, as yet of any ſuch ill Effects in this Country.

As to the great Eclipſe of the Sun in *November* laſt, I will give you my Obſervations thereof, when I have told you, that at *Yale* College, in *Connecticut* Colony, I have an Account that their Obſervers ſuppoſed it to be but about 8 Digits, when by my Calculation for *Cambridge*, it was to have been above 11. *Yale* College lies about 8′ or 10′ Weſt from *Camb.* in Lat. about 41° ½ North. I never calculated it for that place, but I did for *London*, and it agrees to about

7′ or 8′ with your Obſervations, and pretty exact as to the Digits. The Gentleman that writes me word of this, tells me he calculated it and could make it ſcarce 7 *dig.* eclipſed, and that it paſſed off from *America* into the Sea, a great deal more ſouthward than I made it, for by my Calculation it was to be central at Cape *Cod*, and ſo to paſs over to the Iſles of Cape *de Verd.* But taking him to be very much miſtaken, I proceed to give you my Obſervations, as far as I was able to make them; and I think they were well and truly made.

	ſt.	*h.*	*′*	
Nov. 27. 1722.	1	7	27	morn. I ſaw the Sun riſe Eclipſed, on its ſupreme Vertex to the South, about 4 *Dig.* tho' ſome on the Top of the new College ſaw it 2′ or 3′ before. The Sun's true riſing this Morn. was 7 *h.* 30′, hence the Refraction is about 6′ and ſo I have often obſerved it. From this time, till about 8 *h.* 30′ or 40′ I ſaw no more of the Sun, but then I judge it was eclips'd 6 *dig.* or more.

d.	*h.*	*′*	*″*	
2	8	55	15	The Sun was eclipſed 4 ¼ *Dig.* neareſt.
3	9	00	16	4 *Dig.* ½.
4	9	19	45	A little Spot in the Sun emerged.
5	9	25	45	I ſaw the Moon go off the Sun.
	9	25	45	Mr. *Danforth* in a Room juſt by me ſaw the Shadow

go

h.	l	ll
9	25	20

go off the Paper about 30° from its lower Vertex to the East.

Mr. *Appleton* ſaw the Shadow go off the Paper fix'd to the College Braſs Quadrant at his Houſe.

Mr. *Owen Harris*, an ingenious Schoolmaſter in *Boſton*, ſays he obſerved the End at about 26′ *p.* 9.

By the ſecond Obſervation the Sun's Diameter was to the Moon's As 1000 to 972; by the third, As 1000 to 975. At *Boſton* the Eclipſe was obſerved, allowing for its Diſtance, as I obſerved it at the College. And at *Barnſtable*, on Cape *Cod*, there was but a little left of the Sun, and nearer the Head of the Cape there was a Ring of Light quite round the Moon.

The Teleſcope I made my Obſervations by is 24 Feet long. The Teleſcope that Mr. *Danforth* uſed, thro' which the Rays were tranſmitted, was 8 Feet, and the Braſs Quadrant the very ſame Dr. *Halley* uſed at *St. Helena*. If I have been guilty of any Miſtake pardon me, and if, with eaſe, you could tell me where the Shadow would paſs off *America*, I ſhould be glad, for I made it to be about Cape *Cod*. Taking its Latitude to be 40° North, or 40° 10′ and Eaſt from the College 10′ or 15′ I forget which.

I ſhall now give you an Account of a remarkable Accident relating to the Venom of Spiders. *Sept.* 13. 1722 one *Nat. Ware* of *Needham* was bit by a ſmall Spider, which he could not give an exact Deſcription of, cruſhing it to Pieces between his Stock-

ing and Leg; the Account he gave is this; *viz.* That getting up early in the Morning, and putting on his Stocking he presently felt something bite his left Leg a little above his Ancle, about ½ an Hour after he felt a Pain in that Leg, and about ½ an hour from his first perceiving Pain in his Leg, he felt a Pain in his Groin, and at the same Time a creeping Pain in the Calf of his left Leg; and about one Hour after it got into the Small of his Back, and then round him, and in his Stomach, and in his right Thigh, and afterwards Numbness in his Head; the Pains were not constant and fixed, but erratick and very acute. His Pulse was very low and heavy. He came to *Camb.* to a Physician there, and I was also desired to go and see him, which I did, and he gave me this Account.

Sept. 14. In the Morning the Man abovesaid came to see me, and was much better tho' he had but little Sleep in the Night. The Means the Doctor used were only *Sp. Cor. Cerv. & Sal Vol. Corn. Cerv.* with *Vinum Viperin.* and Onions or Garlick externally applied to the Place where the Wound was. These things raised his Pulse, and so, I suppose, assisted Nature to throw off the Venom.

VII. *Obſervations made in* Italy *of a Lunar Eclipſe, which happened the 8th of* Sept. 1718. *Extracted from the* Giornale di Literati *of* Venice.

I. *Obſervations made by Signor* Giovanni Poleni *and* Giovambatiſta Morgagni, *at* Padua, *in the Houſe of Signor* Pietro Bembo, *Noble of* Venice.

Tempus Apparens poſt Meridiem.			
			Sub initium Eclipſis nubes lunam obtexere.
H.	*I.*	*II.*	
6	54	25	Umbra appellit ad partem ortivam maris humorum; diſtat ab Ariſtarcho diametro ejuſdem maculæ, parique intervallo a Keplero.
7	5	5	Appellit umbra ad Copernicum.
	12	56	Umbra appellit ad Tychonem.
	18	10	Appellit ad Platonem.
	22	31	Tegit Manilium totum.
	30	55	Tegere incipit partem ortivam maris nectaris.
	41	53	Appellit ſecundam partem ortivam ſuperiorem ad mare Criſium.
	46	58	Penumbra extremum diſci attingit.
	49	4	Vix quidqam immerſionis ſupereſt.
			Toto integræ immerſionis tempore luna videri potuit commixta colore quodam obſcuro & ſubrubro. Principio poſt immerſionem lunæ pars orientem verſus erat obſcurior.

H.	′	″	
8	33	3	Obſcurior erat lunæ pars prope diſci medium: minus obſcuræ erant circumquaque partes diſci extremæ.
9	30	49	Stellula quædam, nudis oculis inconſpicua, vix decem ſecundis diſtare videbatur a lunæ diſco e regione *Lansbergii*.
	32	9	Penumbra fit clara in diſci extrema parte ortiva.
	36	4	Initium emerſionis ex ortiva plaga.
	40	39	Grimaldus jam emerſit ab umbra a qua diſtat ſui ipſius minori ſpatio.
	44	38	Stellulæ ante viſæ a luna occultatio: tamen incerta.
	49	34	Gaſſendus emergit.
	50	49	Mare humorum extra umbram totum.
10	00	3	Copernici emerſio.
	5	55	Plato emergere incipit.
	14	41	Eudoxus exit.
	19	12	Menelai emerſio.
	27	7	Mare nectaris totum emerſit.
	36	28	Umbra dividit mare criſium bifariam ſecundum ipſius majorem diametrum.
	39	12	Incipit umbra fieri tantillum rarior.
	41	2	Viſus eſt eſſe umbræ finis.
	42	57	Et finis penumbræ.

II. *Obſervations made in the Palace of the* Iſtituto delle Scienze, *at* Bologna; *by Signor* Gemininano Rondelli, Giuſeppe-Antonio Nadio, *and* Giulio-Cæſare Pariſi.

Temp. ver. p. m.

H.	′	″	
			Initium eclipſis non eſt obſervatum.
6	51	36	Mare humorum ad umbram.
	56	22	Capuanus ad umbram.

Mare

H.	′	″	
6	56	37	Mare humorum totum in umbra.
7	1	7	Bullialdus ad umbram.
	2	52	Bullialdus totus ſub umbra.
	3	37	Copernicus totus ſub umbra.
	11	22	Tycho ad umbram.
	12	52	Totus Tycho ſub umbra.
	15	37	Plato ad umbram.
	16	27	Totus Plato ſub umbra.
	19	22	Manilius ad umbram.
	19	52	Mare ſerenitatis.
	23	57	Mare tranquillitatis.
	35	8	Meſſalla ad umbram.
	36	8	Totus Meſſalla ſub umbra.
	36	38	Mare fœcunditatis ad umbram.
	37	23	Promontorium ſomni.
	39	23	Cleomedes ad umbram.
	39	53	Mare criſium.
	44	8	Mare fœcunditatis totum.
	44	43	Mare criſium totum.
	47	18	Totalis obſcuratio lunæ, juxta D. *Nadii* æſtimationem.
	47	53	Totalis obſcuratio, juxta D. *Pariſium.*
9	33	40	Initium emerſionis lunæ.
	36	35	Grimaldus totus extra umbram.
	40	54	Galilæus extra umbram.
	42	34	Sidus quoddam a luna tegitur in eodem proxime verticali cum centro lunæ.
	47	50	Mare humorum extra umbram.
	52	10	Bullialdus extra umbram.
	54	25	Centrum Tychonis extra umbram.
	55	12	Tycho totus extra umbram.
	58	46	Mare nubium extra umbram.
10	4	2	Plato ad umbræ terminum.
	5	33	Totus Plato extra umbram.

H.	′	″	
10	17	12	Insula sinus medii extra umbram.
	23	47	Messalla ad terminum umbræ; simul totum mare serenitatis extra umbram.
	27	58	Mare tranquillitatis extra umbram.
	30	12	Cleomedes extra umbram.
	32	8	Mare crisium umbræ terminum attingit.
	34	7	Mare fœcunditatis totum extra umbram.
	36	19	Mare crisium totum extra umbram.
	37	36	Finis eclipsis.

III. *Observations made in the Suburbs of* Bologna *southwards*; *by Signori* Eustachio *and* Gabbrielo Manfredi.

H.	′	″	
6	31	48	Nunc primum luna e collibus assurgere incipit, penumbra atmospheræ jam infecta.
	42	13	Initium veræ eclipsis, quantum judicare patiebatur subdubius umbræ terminus. Paulo post nubecularum atque arborum objectu lunæ tegebatur.
	52	48	Umbra per Aristarchum & Keplerum protenditur, atque una mare humorum tangere videtur.
7	2	23	Umbra per medium Bullialdi, simul tangens Copernicum.
	4	2	Umbra per medium Copernici.
	5	4	Totus Copernicus latet.
	7	58	Umbra Pitatum attingit.
	10	54	Attingit Tychonem.
	12	19	Medium Tychonis latet.
	13	9	Totus Tycho latet.
	15	34	Umbra ad Platonem.
	16	7	Ad medium Platonis.

Totus

H.	′	″	
7	16	54	Totus Plato latet.
	20	9	Manilius tegitur.
	20	34	Umbra tangit mare ſerenitatis.
	23	44	Menelaus tegitur.
	24	36	Dionyſius tegitur.
	27	34	Plinius tegitur.
	29	49	Umbra ad Catharinam, Theophilum, Cyrillum.
	30	36	Umbra tangit Fracaſtorium.
	31	44	Medium Fracoſtorii tegitur.
	32	34	Promontorium acutum umbram ſubit.
	35	15	Promontorium ſomni latet.
	37	57	Taruntius latet.
	39	39	Umbra tangit mare criſium.
	42	16	Umbra per medium mare criſium.
	44	5	Totum mare criſium in umbra conditur.
	47	50	Totalis immerſio lunæ in umbram.
			Toto tempore eclipſis luna clariſſimè in ſudo ſpectabatur, colore rubeſcenti, ea parte denſiori, qua altius in umbram immergebatur.
8	38	50	Hoc tempore, & deinceps aliquot minutis, omni ex parte æque obſcura apparebat lunæ facies, ut facile conſtaret eam prope umbræ centrum verſari.
9	27	50	E regione Grimaldi, qua parte emerſio imminebat, inſignis fulgor ſpectari cœperat.
	29	20	Dubitari cœptum de emerſionis initio.
	33	20	Proculdubio emerſio jam inceperat.
	35	21	Grimaldus ab umbra ſe ſubducere incipit.
	35	55	Centrum Grimaldi emergit, totus Ricciolus jam detectus erat.
	36	26	Totus Grimaldus exit ab umbra.

H.	′	″	
9	39	28	Galilæus exit.
	41	22	Umbra tangit mare humorum.
	42	31	Stellula quæ diu prope limbum lunæ inferiorem (qui teleſcopio ſuperior apparebat) morata fuerat, nunc demum ſub lunam conditur, circa plagam Tychonis, adhuc eclipſi laborantem. Aliæ Stellulæ lunam ſubituræ videbantur, ſed poſtquam unus vel alter digitus lunaris faciei illuſtrari cœpit, præ fulgore omnes evaneſcebant.
	43	53	Umbra per medium mare humorum.
	45	41	Ariſtarchus emergit.
	47	23	Keplerus emergit.
	52	6	Emergit Bullialdus.
	53	31	Tycho detegi incipit.
	54	9	Medium Tychonis detegitur.
	55	1	Totus Tycho detectus: quo tempore nondum ſtellula e luna ſe ſubduxerat.
	55	21	Copernicus emergere incipit.
	56	6	Medium Copernici emergit.
	57	15	Totus Copernicus.
10	0	1	Stellula quæ paulo antea ſub luna delituerat, jam ſpectabatur a lunæ limbo nonnihil diſtans, ut appareret eam ante 4 vel 5 minuta temporis emerſiſſe. Verſabatur autem e regione partis obſcuræ inferioris limbi lunæ, neque longe ab umbræ termino.
	4	51	Umbra per medium Platonis.
	5	36	Totus Plato detegitur.
	13	6	Manilius emergit.
	16	31	Dionyſius emergit.
	16	41	Menelaus emergit.

Fraca-

H.	′	″	
10	20	31	Fracaſtorius totus jam emerſerat.
	23	51	Snellius & Furnerius totaliter emergunt.
	24	5	Promontorium acutum detegitur.
	25	11	Meſſalla totus apparet.
	31	11	Proclus emergit.
	31	51	Mare criſium emergere incipit.
	34	3	Medium mare criſium emergit.
	36	7	Totum mare criſium extra umbram.
	38	51	Circa hoc tempus umbra vera lunam deſerere videtur, penumbra adhuc ad multum temporis perdurante.

In the *Ephemerides* publiſhed in the Year 1715. from *M. Caſſini*'s Tables for the Uſe of the *Iſtituto Bologneſe delle Scienze*, the Beginning of theſe Eclipſes was marked at 6*h*. 41′, the total Immerſion at 7*h*. 46′, the Beginning of the Emerſion at 9*h*. 33′, the End at 10*h*. 38′, which Times ſcarcely differ one or two Minutes from the Times obſerved.

IV. *Obſervations made by the Marquis* Antonio Ghiſilieri, *at* Bologna, *on the Obſervatory in his own Houſe.*

H.	′	″	
6	40	23	Initium eclipſis dubium.
	51	23	Mare humorum ad umbram.
	55	46	Capuanus ad umbram.
7	1	13	Bullialdus ad umbram.
	28	14	Mare nectaris totum ſub umbra.
	32	30	Promontorium acutum ad umbram.
	36	45	Promontorium ſomni ad umbram.
	38	45	Mare criſium ad umbram.
	46	37	Totalis immerſio lunæ.
9	33	50	Initium emerſionis.

Grimaldus

H.	′	″	
9	35	39	Grimaldus totus extra umbram.
	54	17	Tycho totus extra umbram.
10	15	6	Plinius totus extra umbram.
	32	39	Mare criſium emergere incipit.
	37	42	Finis eclipſis.

VIII. *Experimenta & Obſervationes de Congelatione aquæ in vacuo factæ a* D. G. Fahrenheit, R. S. S.

INter plurima admiranda Naturæ Phœnomena aquarum congelationem non minoris momenti eſſe ſemper judicavi; hinc ſæpe experiundi cupidus fui, quinam effectus frigoris futuri eſſent, ſi aqua in ſpatio ab aere vacuo clauderetur. Et quoniam dies ſecundus, tertius & quartus *Martii*, (Styli V.) Anni 1721. ejuſmodi experimentis favebat, hinc ſequentes obſervationes & experimenta a me ſunt factæ.

Antequam autem experimentorum recenſionem aggrediar, neceſſe erit, ut paucis quædam de thermometris, quæ a me conſtruuntur, eorumque ſcalæ diviſione, ut & de methodo evacuandi, qua uſus ſum, mentionem faciam. Duo potiſſimum genera thermometrorum a me conficiuntur, quorum unum ſpiritu vini & alterum argento vivo eſt repletum: Longitudo eorum varia eſt, pro uſu, cui inſervire debent: Omnia autem in eo conveniunt, quod in omnibus ſcalæ gradibus concordent, interque limites fixos variationes ſuas abſolvant. Thermometrorum ſcala, quæ meteorologicis obſervationibus ſolummodo inſerviunt, infra a Zero incipit & 96^to^ gradu finitur. Hujus ſcalæ diviſio tribus nititur terminis fixis, qui arte ſequenti

modo parari poſſunt; primus illorum in infima parte vel initio ſcalæ reperitur, & commixtione glaciei, aquæ, & ſalis Armoniaci vel etiam maritimi acquiritur; huic mixturæ ſi thermometron imponitur, fluidum ejus uſque ad gradum, qui zero notatur, deſcendit. Melius autem hyeme, quam æſtate hoc experimentum ſuccedit. Secundus terminus obtinetur, ſi aqua & glacies abſque memoratis ſalibus commiſcentur, impoſito thermometro huic mixturæ, fluidum ejus triceſimum ſecundum occupat gradum, & terminus initii congelationis a me vocatur; aquæ enim ſtagnantes tenuiſſima jam glacie obducuntur, quando hyeme liquor thermometri hunce gradum attingit. Terminus tertius in nonageſimo ſexto gradu reperitur; & ſpiritus uſque ad hunc gradum dilatatur, dum thermometrum in ore vel ſub axillis hominis in ſtatu ſano viventis tam diu tenetur donec perfectiſſime calorem corporis acquiſivit. Si vero calor hominis febri vel alio morbo fervente laborantis inveſtigandus eſt, alio thermometro utendum, cujus ſcala uſque ad 128 vel 132 gradum prolongata eſt. An autem hi gradus ferventiſſimo calori alicujus febris ſufficiant nondum expertus ſum, vix tamen credendum, quod cujuſdam febris fervor gradus memoratos excedere debeat. Thermometrorum ſcala, quorum ope ebullientium liquorum gradus caloris inveſtigatur, etiam a zero incipit & 600 continet gradus, hoc enim circiter gradu Mercurius ipſe (quo thermometron repletum eſt) incipit ebullire.

Ut autem quoque thermometra ab omnibus mutationibus caloris celeriter afficiantur, loco globulorum cylindris vitreis ſunt prædita, eo enim modo ob majoris ſuperficiei quantitatem citius a variatione caloris penetrantur.

Poſtquam

Poſtquam breviter mentionem feci de conſtructione thermometrorum meorum, adhuc deſcribendus erit modus evacuandi, quo in experimentis initio memoratis uſus ſum. Globulus vitreus (*Fig. 6.*) A tubulo BC duorum vel trium pollicum longo in extremitate C attenuato prædituſ ſupra ignem calefit, quo facto tubuli extremitas aquæ immergitur, & tam diu in aqua relinquitur donec refrigefactione aëris in globulo contenti, guttulis aliquibus aquæ ſit repletus, deinde iterum ſupra flammam latiorem lampadis cujuſdam, vel ſupra prunas ope forcipis parvæ tenetur, donec aqua in globo contenta incipit ebullire, & vapor aquæ impetu inſtar Æolipilæ erumpit: Hæc ebullitio aquæ aliquantulum continuatur, quo facto globulus ab igne removetur, & extremitati ejus flamma candelæ appropinquatur. Refrigereſcente globulo vapor ab igne rarefactus etiam ſucceſſive condenſatur, vaporumque egreſſus paulatim diminuitur, qui poſtquam plene ceſſavit, in ipſo etiam momento extremitas tubuli colliqueſcit, globuluſque hermetice ſigillatus & ab aere evacuatus redditur. An vero hoc modo probe ab aere evacuatus ſit, experire poteſt, ſi nempe extremitas tubuli ſub Mercurio diffringitur, totus enim globulus Mercurio replebitur, ſi diffractio caute ſine introitu aeris externi fuerit peracta. Diffractio extremitatis etiam ſub aqua perfici poteſt, ſed licet ſummâ curâ peragatur, globus tamen non tam perfecte aqua replebitur; dum enim aqua globum evacuatum intrat, aer, qui ſemper in aliqua quantitate aquæ commixtus eſt, ab illa in minutiſſimis bullulis ſeparatur, quæ poſtquam coiverunt ſub ſpecie bullulæ majoris in globulo apparent. Eodem modo globus ab aere evacuari poteſt, ſi tertia, dimidia vel major pars globi aqua repleta deſideratur; deſiderata enim quantitate aquæ prius impletur & deinde poſt ebullitionem aquæ hermetice

hermetice clauditur. His explicatis ad recenſionem experimentorum pervenio.

Globulum vitreum, cujus diameter uni circiter pollici æquabat, memorato modo ab aëre evacuatum, & aqua pluviatili fere ad dimidiam partem repletum, die ſecundo *Martii* Anni 1721, frigoris rigori exponebam. Aeris temperies in thermometro appoſito quindecimo gradu notabatur. Elapſo horæ ſpatio, aquam adhuc fluidam in globulo reperiebam, cujus rei cauſam eſſe arbitrabar, quod nempe aqua nondum bene a frigore eſſet penetrata, ut autem dubii omnis tolleretur ſcrupulus, globum per totam noctem aeri expoſitum relinquebam. Sequenti die tertio *Martii* ad horam matutinam quintam aquam adhuc fluidam inveniebam & liquor thermometri eundem adhuc notabat gradum cujus improviſi phœnomeni cauſam aeris abſentiæ attribuebam. Ut autem hujus conjecturæ veritas mihi innoteſceret, diffringebam tubuli extremitatem, ut ſpatium vacuum globuli iterum aere repleretur, quo facto tota aquæ maſſa celerrime tenuiſſimis glaciei lamellis permiſcebatur. Placebat mihi autem antequam experimenti repetitio fieret, alio experimento explorare, an hæ lamellæ glaciales aquæ innaturæ eſſent, quapropter globum diffringebam partemque aliquam glaciei aquæ vitreo poculo contentæ injiciebam, & illam aquæ innatare conſpiciebam.

Dum autem forte oculos admodum brevi temporis intervallo in alium quendam direxeram locum, aſpiciendo iterum poculum, totam aquam glacialibus lamellis permixtam cernebam, manente tamen adhuc in interſtitiis lamellarum plurimâ aquæ parte fluida. Thermometron huic mixturæ impoſitum, trigeſimum ſecundum notabat gradum. Attentiori autem animo & oculo hæc phœnomena contemplari cupidus, experimentum duobus aliis globulis repetere reſolvebam:

poſtquam igitur priori modo præparati erant, illos per horæ ſpatium aeri externo exponebam, liquor autem thermometri interea jam vigeſimum attigerat gradum. Elapſa hora aquam in ambobus globulis adhuc fluidam inveniebam, poſtquam autem ſpatium vacuum globuli aëre iterum repletum erat, citiſſime etiam aqua (ut in priori experimento) lamellis glacialibus permiſcebatur, illarum que generatio tam ſubitanea erat ut vix oculis aſſequi poterat. Et quoniam lamellarum generatio, quæ in poculo vitreo erat facta, obſervationi meæ effugerat, hinc adhuc maxime curioſus eram, illarum generationem paulo attentius contemplari. Priuſquam autem globulorum alterum diffringebam, aquam memorato poculo contentam a lamellis glacialibus ſeparabam quo facto globulum diffringebam, glaciem in globo generatam aquæ injiciebam. Glacies injecta quidem aquæ innatabat, ſed lamellarum generatio in poculo fruſtra a me expectabatur. Ob negotiorum quorundam neceſſitatem, experimentorum continuationem advenienti dedicabam nocti. Quæ poſtquam advenerat iterum hora undecima tres globulos ſævienti gelu exponebam. Horum duo ad dimidiam circiter partem, iterum aqua erant repleti, manente reſidua parte globulorum vacua, in tertio vero ſolummodo quarta circiter pars globuli erat vacua. Aeris temperies in thermometro appoſito vegeſimo ſexto notabatur gradu. Hora quarta matutina eundem ſtatum temperiei aëri adeſſe thermometro deprehendebam, & aquam in duobus globulis, qui tantum pro dimidia parte aqua modo erant repleti adhuc fluidam inveniebam: in tertio autem aqua congelata, atque globus diffractus erat. Glacies minutiſſimis ſed admodum paucis permixta erat bullulis, pelluciditaſque ejus maxime perturbata apparebat & confuſæ chryſtallizationi alicujus ſalis ſimillima erat. Hujus experimenti contrarium ſucceſ-

ſum inviſibili cuidam fiſſuræ attribuebam, quâ aer externus introitum invenerat atque ita congelationem aquæ procreaverat.

Quoniam autem magno adhuc flagrabar deſiderio, lamellarum generationem in poculo vitreo attente contemplari, idcirco vas vitreum e cubiculo in illam afferebam cameram ubi hæc experimenta fiebant, dum autem ſcalas paucas, quæ ad illam ducebant cameram, aſcendere volebam, deficiebam ſcalam aliquam pede bene attingere, quo facto aqua vitro contenta ruditer commovebatur, atque eo ipſo momento tota ejus maſſa plurimis permixta apparebat glacialibus lamellis. Hoc autem caſu infortuito edocebar, glaciem in aqua fatis frigida agitatione produci poſſe; curioſus inde eram experimento explorare, an congelatio aquæ etiam in ſpatio vacuo agitatione futura eſſet. Poſtquam igitur globulum aliquantum agitaveram, magna animi voluptate eundem phœnomeni eventum cernebam, ſimulque judicii errorem agnoſcebam, quod nempe abſentiæ aeris fluiditatem aquæ attribuiſſem. Interea e thermometro agnoſcebam, gelu multum langueſcere, liquor enim jam vigeſimum octavum aſcendiderat gradum, cito igitur manu diſſolvebam glaciem, globulumque unum iterum aëri exponebam (erat autem alter caſu infortuito diffractus). Relicto globo per dimidium circiter horæ ſpatium, gelu adhuc magis remittere obſervabam, thermometri enim liquor jam ad gradum trigeſimum ſecundum pervenerat. Et quoniam verebar, ne remiſſione frigoris experimenti repetitio vana futura eſſet, ſi diutius globulus aeri relinqueretur expoſitus: hinc ipſo tempore, agitatione globuli aquæ congelationem procreare tentabam; ſed licet fortiter agitaretur, non minima tamen congelationis apparebant indicia. Cum vero hoc modo omnis congelationis ſpes evanuerat, adhuc experiri volebam, an congelatio ſuc-

ceſſura eſſet, ſi ſpatium vacuum globuli iterum aere repleretur. Diffracta igitur extremitate tubuli, minutiſſimæ glaciei ſpiculæ per totam aquæ maſſam diffuſæ generabantur, quæ circumrotatione aquæ ſuperficiem petebant, amœniſſimumque ſpectaculum reflectione luminis ab earum politis ſuperficiebus præbebant. Quoniam autem hujus hyemis gelu hoc die finiebatur & cum illo experimentorum continuationi finis imponebatur, quam vero tempori opportuniori aliis excogitatis experimentis inſtituere mihi proponebam. Hyems anni 1722. ita mitis in *Hollandia* erat, ut per totum hyemem vix aquæ ſtagnantes glacie aliqua obducebantur. Et licet hyems initio anni 1723. multo ſeverior erat, attamen negotiorum copia, aliorumque experimentorum major neceſſitas continuationem eorum prohibebant. Cogitationes aliquas de horum phœnomenon cauſis quidem apponerem, ſed ob experimentorum, inſufficientem copiam illas relinquo, ſufficietque mihi experimenta & obſervationes memoratas recenſuiſſe, an forſan viri perſpicaciſſimo ingenio præditi, illas contemplationibus ſuis dignari vellent.

F I N I S.

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ERRATA.

PAG. 46. lin. 17. *read* uninterrupted.
Pag. 60. lin. 11. *read* hypochondrio.

Numb. 383

PHILOSOPHICAL TRANSACTIONS.

For the Months of *May* and *June*, 1724.

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I. *Obſer-*

I. *Part of a Letter from the Reverend Mr.* Waſſe, *Rector of* Aynho *in* Northamptonſhire, *to Dr.* Mead, *concerning the difference in the Height of a Human Body, between Morning and Night.*

I Have obſerv'd ſeveral Soldiers diſcharged for being a little under the Standard, and having experienced the difference of a Man's Height in the Morning and at Night, I mentioned it to an Officer, and thereby kept ſome Perſons from being turn'd out of the Service. Since that Time I have meaſured Sir *H. A*...., Mr. *C*....., and a great many ſedentary People, and Day-Labourers, of all Ages and Shapes, and find that Difference to be near an Inch. I try'd my ſelf, when ſitting, and found it in like manner; particularly, *Auguſt* 21. 1723. Weather warm, no Wind, at Eleven in the Morning I ſat down, and fixed an Iron Pin ſo as to touch it, and that but barely. After that, I fatigued my ſelf for half an Hour with a Garden-Roller, and the Conſequence was, that at 12 Ho. 30 Min. I could not reach the Nail ſitting, by about 5 Tenths of an Inch, or the Breadth of one of the Rods of *Hunt*'s Sliding-Rule. At 2 the ſame Day I wanted near ſix Tenths of an Inch. On the 21ſt, at 6 Ho. 30 Min. in the Morning, I touched the Nail fully; and after the above-mentioned Exerciſe for only a Quarter of an Hour, at 7 Ho. 14 Min. I fell ſhort almoſt as much as before. On the 27th, having ſat up late with ſome Friends, I was faint, and felt my ſelf heavy upon the Ground, and without any Spring, and at 9 that Morning I did not reach the Nail, though I had us'd no

Excrciſe. I rid out, but could not reach it that Day. On the 28th I rid about four Miles; and whereas at 6 that Morning I reached the Nail, I had loſt 6 tenths of an Inch by 8. *September* 19th I came from *Oxford* a little tired, and next Morning at 8 I wanted half an Inch. If I Study cloſely, though I never ſtir from my Writing-Desk, yet in 5 or 6 Hours I loſe near an Inch. All the Difference I find between Labourers and ſedentary People is, that the former are longer in loſing their Morning Height, and ſink rather leſs in the whole than the latter. I cannot perceive, that when the Height is loſt, it can be regained by any Reſt that Day, or by the Uſe of the cold Bath.

I have not informed my ſelf ſo well about Horſes; my Mare comes Seven this Graſs, is ſtrong, ſhort back'd, and well legg'd. I meaſured her before and after riding 20 Miles, *April* 30th, and could not perceive the leaſt Difference in her Height.

The Alteration in the human Stature, I imagine, proceeds from the yielding of the Cartilages between the *Vertebræ*, to the Weight of the Body in an erect Poſture.

AYNHO, *May* 16, 1724.

This curious Obſervation has been confirmed by ſeveral Members of the Royal Society, and others, who have purpoſely tried the Experiment.

II. *Some*

II. *Some Remarks upon the Obſervation mentioned in the foregoing Letter. By Mr.* William Beckett, *Surgeon, F. R. S.*

THE remarkable Difference in the Stature of human Bodies, in the Space of a few Hours time, taken Notice of in the Reverend Mr. *Waſſe*'s Letter, read before the Society this Day Sevennight, I have ſince found to be Fact, by ſeveral Experiments made with the greateſt exactneſs I have been capable of. Only this I have farther obſerved, that in thoſe Perſons who have been young, the Alteration has been more conſiderable than in thoſe that have been aged. The Trials equally ſucceeding in a ſitting as in a ſtanding Poſture, will naturally lead us to believe, that it muſt neceſſarily be from the Trunk of the Body, or ſome of its Parts, that this remarkable Alteration is brought about. Now every Body knows, that the Standard of a Perſon's Stature has been always looked upon to be determined by the whole *Compages* of the Bones, adjuſted by the Divine Architect according to the ſtricteſt Rules of Geometry.

But there is ſomething ſo wonderful in the Structure and Diſpoſition of the Spine, that nothing but ſuch a peculiar Contrivance could ſo curiouſly have fitted it for the reſpective Uſes and Purpoſes it was ordained for. The thickneſs and ſhortneſs of the Bones, with the intervening Cartilages, aſſiſted by the bony Proceſſes, diſpoſe it to a Motion peculiar to it ſelf. Whereas had the Bones been of any conſiderable length, upon bending the Body, the Articulations muſt have made a large Angle upon their inmoſt Edges, and the ſpinal Mar-

row have been continually liable to be injured; or had the Cartilages been entirely wanting, it would have been as uſeleſs as if it were but one Bone, whereby we being rendered uncapable of bending the Trunk of the Body, it muſt have always remained in an erect Poſture. But by the preſent Diſpoſition of its Parts, 'tis not only abſolutely ſecured againſt any ſuch Inconveniencies, but, although ſo ſmall a Pillar as it is, is capable of ſupporting, without Hazard, ſuch prodigious Weights, as we are not wanting in our Accounts of.

Another Particular, which beſpeaks the utmoſt Wiſdom and Deſign in the Contrivance of this Part, is the remarkable Difference there is in the thickneſs of the Cartilages, placed betwixt the Bones of the Spine; the *Vertebræ* of the Breaſt requiring but little Motion, the Cartilages are there but thin, in compariſon of thoſe of the Loins, which being very thick, the loweſt more eſpecially, the Motion is there vaſtly greater; and the Cartilages being abundantly thicker before than behind, this is the Reaſon that we bend our Bodies ſo much more forward than backward. And by this admirable Method of diſpoſing of the thicker Parts of the Cartilages forward, it is, that in all violent Exerciſes, the Parts contained in the Belly and Breaſt are in a great meaſure ſecured from any Damages they might have been liable to, becauſe by the pliableneſs and elaſticity of theſe Cartilages, they break the violent Shocks the *Viſcera* muſt otherwiſe have neceſſarily ſuſtain'd upon ſuch Occaſions.

From what I have here remarked, in relation to theſe peculiar Properties of the Cartilages placed between the Bones of the Spine, we may reaſonably ſuppoſe them to be certain compreſſible, dilatable, elaſtick Bodies, which like other Bodies, endued with the

the ſame Qualities, will naturally yield to any incumbent Weight, which is ſufficient to force the Particles of Matter of which they conſiſt, into a more ſtrict and cloſe Union, and that when this compreſſive Power is removed, they will of themſelves recover that State they enjoyed before they were obliged to give Way to that Preſſure. Now I am here particularly to obſerve, that the loweſt of all the Cartilages of the Loins, is the thickeſt, and ſo conſequently that it contains a greater Quantity of Matter than any of the reſt; by which means it becomes more diſpoſed to have its thickneſs diminiſhed, and that all of them gradually become thinner, even to the top of the Spine. Now all ſuperiour Bodies, if they come to an immediate Contact, preſſing upon their inferiour, it muſt neceſſarily follow, that the whole Weight of the Body, except the lower Limbs, muſt preſs upon and be ſuſtained by the loweſt *Vertebræ* and their Cartilages; but theſe Cartilages, as has been obſerved, being much thicker in this Part than the other, and the incumbent Weight bearing harder upon them, they muſt be unavoidably compreſs'd more than the other; and ſo, conſequently, when this Weight is removed, their Expanſion, from their natural Elaſticity, will be greater alſo.

This being the natural State and Diſpoſition of theſe Parts, during the whole ſpace of Time we are uſually employed about our neceſſary Avocations till we diſpoſe our ſelves to Reſt, the Cartilages of the Spine will, by their compreſſible and yielding Properties, become more cloſe and compact from the perpendicular Preſſure they ſuſtain, and ſo conſequently the Spine, the only ſupport of the Trunk of the Body, will become ſhorter. But when this ſuperiour Weight ſhall be entirely removed, by placing the Body in a horizontal Poſture,

ſture, as it always is when we are in Bed, the compreſs'd Cartilages will, by their natural elaſtick Power, begin gradually to enlarge themſelves, till they recover the ſame expanded State they enjoyed before they were forced to give Way, and yield to the incumbent Preſſure; and ſo conſequently it will produce a conſiderable Alteration in a Perſon's Stature, agreeable to the determined Times mentioned in the preceding Letter. For if we only conſider, that the before-mentioned compreſſive Power will leſſen the thickneſs of all the Cartilages, in proportion to the Quantity of Matter they contain; and that there are uſually reckoned about twenty four in Number, it will be no difficult Matter for us to apprehend, that their natural Expanſion being recovered by our cuſtomary Repoſe, the Aggregate of the whole of the Expanſions may amount to about an Inch. Now if the Alteration be ſo conſiderable as this, occaſioned only by the bare incumbent Weight of the ſuperiour Parts of the Body, without any additional Force applied to compreſs the Cartilages yet cloſer, how much more may we reaſonably imagine it would be, were the Experiment tried on ſuch Perſons, whoſe uſual Employment it is to carry heavy Burthens. I have only this one Particular farther to obſerve upon this Head, which is, that this Alteration is not to be expected to be the ſame in aged Perſons as in thoſe that are younger, becauſe the Cartilages, as we advance in Years, gradually grow harder and harder, till many of them arrive to the ſolidity of a Bone; that is, by Degrees they loſe their Spring or expanſive Power, and at length continue in a compreſs'd State of Reſt. And this is without doubt one principal Cauſe, why old People not only ſeem to have loſt ſomewhat of their former Height, but are actually ſhorter.

III. *A Catalogue of Fifty Plants from* Chelsea *Garden, presented to the Royal Society for the Year* 1723, *by the Company of Apothecaries of* London, *pursuant to the Direction of Sir* Hans Sloane, *Bart. President of the College of Physicians.*

51. *Abutilon Carolinianum, reptans, Alceæ foliis, gilvo flore.*
52. *Ambrosia, maritima, Artemisiæ foliis inodoris, elatior.* H. L. Bat.
53. *Alysson Veronicæ folio.* T. Inst. 217
Bursa Pastoris major loculo oblongo. C. B. prod.
54. *Aster peregrinus, Cisti folio non crenato, flore magno, luteo.* Pluk. Phyt. T. 16. f. 2.
55. *Aster montanus, Salicis glabro folio.* C. B. 266
56. *Bidens Indica, Hieracii folio, caule alato.* T. Inst. 462.
57. *Blitum monospermum, Indicum, aculeatum,* Breyn. prod. 18.
58. *Blitum quod Amaranthus Græcus, sylvestris, angustifolius.* T. Cor. 17.
59. *Calcitrapoides pumila, supina, tenuifolia, calyce turbinato.* D. Vaillant. Act. Ac. Par.
60. *Cassia decaphylla, Orobi Pannonici foliis mucronatis.*
61. *Cataria major Vulgaris.* T. Inst. 202.
62. —— *angustifolia minor.* Ib.
63. —— *quæ Nepeta Pannonica major & elatior.* H. Ox. 3. 415.

64. —*His-*

64. *Cataria Hiſpanica, Betonicæ folio anguſtiori, flore albo.* T. Inſt. 202.
65. ——— *Luſitanica, erecta, Betonicæ folio, tuberoſa radice.* T. Inſt.
66. ——— *quæ Horminum Spicatum, Lavendulæ flore & odore.* Boccon. rar. 48.
67. ——— *Orientalis, Teucrij folio, Lavendulæ odore, Verticillis florum craſſiſſimis.* T. Cor. 13.
68. *Chryſanthemum tenuifolium, flore bullato aureo.* Barrel. Ic. 450.
69. ——— *flore luteo, aphyllo minore.* Barrel. Icon. 452
70. *Cirſium humile, ramoſum, foliis integris.*
71. *Helianthemum Anglicum, vulgari ſimile, flore ſtellato.*
72 ——— *ampliore folio, flore roſeo.* D. Sherard.
73 ——— *Halimi folio, anguſtiore, acuto. An Ciſtus Halimi folio, 2.* Cluſ. Hiſt. 71.
74. *Hieracium Pyrenaicum Blattariæ folio, minus hirſutum.* Schol. Bot.
75 ——— *Caſtorei odore noſtras. Hieracium Cichorei, vel potius Staebes folio, hirſutum.* Raij Cat. Cantab.
76. *Lychnis Or. longifolia, nervoſa, flore purpuraſcente.* T. Cor. 24.
77. *Malva Sinenſis, annua, erecta flore minimo. Kitſaitſe dicta.*
78. ——— *Luſitanica, annua, flore parvo ſtriato.*
79. ——— *Sylveſtris, foliis ſinuatis minoribus, floſculis minimis, Anglica.* Boerh. Ind. 2. 268.
80. ——— *Or. erecta, major, flore vix conſpicuo.* D. Sherard.
81. ——— *Erectior, flore magno, ſuave rubente.* T. Cor. 2.

82. *Malva*

82. *Malva Syriaca, Ela Haur taunal dicta. Malva Cretica, annua, altissima, flore parvo, ad alas umbellato.* T. Cor. Ib.
83. *Mimosa Jamaicensis Zanon.* Hist. 149.
84. *Rhagadiolus alter.* Cæsalp. 511.
85. ——— *Lampsanæ foliis.* T. Cor. 36.
86. *Salvia Or. Absinthium redolens, foliis pinnatis, flore carneo, elatior.* D. Sherard.
87. *Scabiosa annua, Centauroides fistulosa.* H. Cath.
88. ——*Altissima, annua, foliis Agrimoniæ nonnihil similibus* H. L. B. 539.
89. *Stachys Cretica.* C. B. 236.
90. ——— *foliis sub verticillis latioribus, acuminatis, flore majore.*
91. *Stachys major Germanica.* C. B. 236.
92. ——— *Folio obscurè virente, flore ferrugineo.* H. Ox. 3. 382.
93. ——— *Salviæ folio, flore luteo.* D. Jussieu.
94. ——— *Elatior flore flavescente punctato.*
95. *Vicia, Craccæ majori Germanicæ similis, per omnia minor Anglica.*
96. *Vicia supina, latissimo folio, non serrato.* T. Inst. 397.
97. ——— ——— ——— *Serrato.* Ib.
98. *Sylvestris, incana, major, & præcox, Parisiensis, flore suave rubente.* T. Inst. 397.
99. ——— *Sylvatica maxima.* Phyt. Britan. 129.
——— *Multiflora maxima.* Raij. Syn. 189.
100. ——— *Multiflora, Cassubica, frutescens, Lentis siliqua.* Breyn. Prod. 52.

IV. *An Account of Observations made of the Variation of the Horizontal Needle at* London, *in the latter Part of the Year* 1722, *and beginning of* 1723. *By Mr.* George Graham, *Watch-maker, F. R. S.*

THE Figure of the three Needles, with which the Experiments were made, was prismatick; their Lengths were nearly 12,2 Inches; their Ends, which pointed to the Divisions, being filed to an Edge, which made a fine Line perpendicular to the Horizon. The Caps of two were of Chrystal, the other of Glass; they were well polished on the Inside, in that Part which touched the Pin they moved upon. The Box was Brass, and of a Breadth sufficient to admit of 20° on each Side the middle Line, and covered with a piece of ground Glass. The circular Arches at the Ends were raised so much above the Bottom of the Box, as to have their upper Surfaces, upon which the Divisions were cut, lie in the same Plane with the Needle, and at such a Distance from each other, that the Needle might play freely between them. A few of the Degrees at the North End were divided into six equal Parts, each Division being 10′. It was easy, by the help of a Convex Glass, to determine the pointing of the Needle to less than a Quarter of these Divisions, or to about 2′ of a Degree. The Pin, upon which the Needle moved, was of Steel hardned, and ground to a fine Point; and by a Spring placed in the Box, the Needle might be raised from off the Point, and let down again at Pleasure, without removing the

Glaſs, or diſturbing the Box. By this means both the ſharpneſs of the Point, and poliſh of the Cap were better preſerved from injury, when there was occaſion to move the Box. A ſmall piece of Braſs was made to ſlide upon that End of the Needle which pointed to the South, for readily bringing it to an horizontal Poſition; for according to the different ſtrength of the Touch, the North End of the Needle will dip more or leſs. The bottom Plate of Braſs was a little broader and longer than the Box, and its Edges made Lines exactly parallel to the middle Line of the Diviſions; and for the greater ſecurity of placing the Box in a right Situation, there was a Braſs Ruler of thirty Inches long, having its Edges even and parallel, except part of that Edge which was applied to the Side of the Box which was a little filed away on the Middle, that the Side of the Box near its Ends only might touch the Ruler. By this Contrivance the two Points of Contact were as far aſunder as the length of the Box would admit of, and the other Edge of the Ruler making a longer Line than the Side of the Box, afforded a better Direction for giving it the ſame Situation.

For determining the Quantity of the Variation, I got a Meridian Line ſtretched upon the top of the Houſe, between the Rails of the Leads, which were above fifteen Foot aſunder, and the Line was a little more than thirty-nine Inches above the Leads. As this Line was faſtned to two Pieces of Braſs that were fixed in the Rails, and was above fifteen Foot long, no ſenſible Error could ariſe in putting it up at any Time. The Compaſs-Box was placed upon a Wooden Stool, with three Feet, that had nothing of Iron about it, and its Top ſet level by a Plumb-Rule. But find-

ing that in the open Air the Wind gave ſome Diſturbance, I put up another Line, after the ſame manner, in a Room two pair of Stairs high ; this Line was about the ſame length with the other, and thirty-nine Inches above the Floor. Some time after I put up a third Line, of the ſame Length, in the Room over this. By the Method made uſe of in fixing theſe Lines they could not differ above 2′ of a Degree from the Meridian, or from one another. Before I had made any Trials, I imagined no other Difference would ariſe than what might be occaſioned by the Friction of the Needle upon the Point it was to move upon, and having found that conſiderable in all the Needles that I had taken notice of, I took more than ordinary Care to provide againſt it, and ſucceeded beyond my Expectation. For I have ſeveral times obſerved all the three Needles return ſo exactly to the ſame Place, that I could not perceive the leaſt Difference ; as likewiſe all three to agree very nearly about the ſame Time, when they have been placed in the ſame Box immediately one after another, the Box remaining unmoved. The firſt Needle I made, was a little above three tenths of an Inch broad, about ,06 in thickneſs, and weighed about an Ounce Troy, the Cap of Chryſtal. After ſome Trials with this Needle, it was made narrower, not to exceed half a tenth of an Inch, and it then weighed five Peny Weight and five Grains. The ſecond Needle was at firſt about three tenths of an Inch broad, and ,04 thick, the Cap of Glaſs ; and after ſeveral Trials, it was made ſo much narrower, that its Breadth was a little leſs than its Thickneſs,and it weighed two Peny Weight and five Grains. The third was nearly of the ſame Dimenſions with the ſecond, and weighed two Peny Weight and three Grains.

When

When the two firſt Needles were made narrower, care was taken that the Files made uſe of for filing the North Ends, touched not the South Ends; and after they were made lighter, I tried them both, before they were freſh touched upon the Stone, and found no ſenſible Difference in their Direction. The reaſon of making the two firſt Needles ſo heavy, was to try whether they would return more conſtantly to the ſame Situation than lighter ones. But notwithſtanding each of them would ſettle very exactly in the ſame Place, for a great Number of Trials made immediately one after another, yet I found them at different Times to differ conſiderably from their former Directions.

This occaſioned my making them narrower, fearing their Breadth had been ſome way concerned in this Irregularity. But after the Alteration, I found the ſame thing happened, though I could find nothing of it to proceed from any Friction upon the Point. This made me prefer the lighter Needles, as leſs apt to injure the Point they moved upon, and as exact in returning to the ſame Situation. After many Trials, I found all the Needles I made uſe of, would not only vary in their Direction upon different Days, but frequently at different times of the ſame Day; and this Difference would ſometimes amount to upwards of half a Degree in the ſame Day, ſometimes in a few Hours. And this Alteration I obſerved, whether the Needles were drawn aſide immediately before the Obſervation, or ſuffered to remain undiſturbed. For I have left the Box ſtanding for ſeveral Days together, without ever diſturbing the Needle, only have taken notice what it pointed at, and the Time of the Day, and I could ſometimes perceive in a few Minutes a very ſenſible Alteration. But whether it ſtood near its

greateſt

greateſt or leaſt Variation, or whether I drew the Needle to one Side with a Key a few Degrees or a greater Number, it would conſtantly return to the ſame Place it ſtood at immediately before. Sometimes I have taken the Needle out of the Box, and put it in again, and this I have repeated ſeveral times in the ſpace of an Hour. At other times I have taken down the Box from off the Stool, and put it up again, but have found no Alteration in its Direction; ſo that I found it of no Conſequence, whether the Needle was drawn aſide or let alone, the ſhaking of the Floor by walking upon it, or the trembling of the Houſe by the Coaches in the Street, was ſufficient to overcome the ſmall Friction upon the Point. When I made the Obſervations, I was very careful to have no Keys, nor Iron about me, that could affect the Needle.

The Box was placed in the Room above the Diſtance of ſix Foot from the neareſt Wall, and above thirteen Foot from the Grate in the Chimney, and no Iron could at any time be brought near it without my Knowledge. Yet, after all, I am not ſatisfied that it was out of the reach of Iron, and that the Variation ſhewn by it is the true Quantity; but I am very ſure there was no Change of Circumſtances in the Room that could affect it, for if there were any ſuch Materials in the Wall, or Floor, their Diſtances and Situations continued the ſame. But for a farther Confirmation of this Irregularity, I put one of the Needles into a Wooden Box, with a few Degrees divided as the other, and placed it at the ſame Meridian Line, at the Diſtance of three Foot and a half from the other, and found both Needles nearly agreed in their Alterations. The Needles were all touched by that excellent Loadſtone preſented to the Society by the Right Honourable

ble the Lord *Paiſley*. It may not be improper to take Notice, that the Needles were not touched upon the naked Stone, but with its Armour on, generally upon that Part of the Capping neareſt the Poles; but I could not find a Difference in the Direction, by touching upon another Part. I may add, that when I have obſerved the Needle increaſing, or decreaſing in its Variation, I have very frequently, with a Key, drawn it the contrary Way ſeveral Degrees, and then, by letting it return very gently, till it has been within a Degree, or leſs, of the Place it ſtood at immediately before, I have there ſtopt it for ſome time, by holding the Key at a proper Diſtance; and withdrawing my Hand gradually, have tried to make it ſtand ſhort of its former Place, but could never ſucceed. By this Method, and ſeveral others made uſe of, I am well aſſured theſe Changes in the Direction are owing to ſome other Cauſe than the Friction of the Needle upon the Pin; but what that Cauſe is I cannot ſay, for it ſeems to depend neither upon Heat nor Cold, a dry or moiſt Air, clear or cloudy, windy or calm Weather, nor the Height of the Barometer. The only thing that has any appearance of Regularity, is, that the Variation has been generally greateſt, for the ſame Day, between the Hours of Twelve and Four in the Afternoon, and the leaſt about ſix or ſeven in the Evening.

March

March 8. 1722.

This Day a piece of Braſs was fixed to a Wooden Box, and a few Degrees were divided into 10′ each, as in the Braſs Box, to try if both Needles would be alike affected in the ſeveral Alterations. This Wooden Box was placed at the ſame Meridian Line, and about the Diſtance of 3½ Feet from the other.

Braſs Box.	Needle 2=5.	h.	Needle 5=5
March 8.	14°= 30′—	3=00′	14°= 25′+
	14 = 20	3=15	14 = 20
	14 = 15 +	4=00	14 = 10
	14 = 20	4=15	14 = 15
	14 = 25	5=00	14 = 20
	14 = 25	5=30	14 = 20
	14 = 15	5=45	14 = 10
	14 = 00	5=57	14 —
	14 —	6= 8	13 = 55
	13 = 50	6=15	13 = 40
	14 = 20	6=38	14 = 15 +
	14 +	6=48	14 = 00
	14 = 00	6=54	14 —
	14 = 5	7= 5	14 +
	14 = 10	7=15	14 = 5
	14 +	12=00	14 +

March

Brass Box. Needle 2=5.		Needle 5=5.
March 9.		
14° = 10′	9h = 30′	14° = 10′
14 = 10 +	10 = 00	14 = 10 +
14 = 10	10 = 15	14 = 10
14 = 10 +	10 = 30	14 = 10 +
14 = 15	11 = 00	14 = 15 —
14 = 00	8 = 15	14 = 00
14 = 00	11 = 50	14 = 00
March 10.		
14° = 10′ +	10h = 00′	14° = 10′
14 = 15	11 = 00	14 = 10 +
14 = 15	12 = 00	14 = 10 +
14 = 15 +	12 = 45	14 = 10 +
14 = 15 +	1 = 00	14 = 10 +
14 = 15 +	1 = 30	14 = 10 +
14 = 15 +	1 = 45	14 = 10 +
14 = 15 +	2 = 00	14 = 10 +
14 = 15	3 = 30	14 = 10
14 = 15 +	4 = 00	14 = 10 +
14 = 15 —	5 = 30	14 = 10 —
14 = 10	6 = 00	14 = 5
14 = 00	6 = 15	14 = 00
14 —	6 = 30	14 —
14 +	7 = 30	14 +
14 = 5	7 = 45	14 +
14 +	12 = 00	14 +

March 30.

The Needle 2=5 which was in the Braſs Box, was this Day put into the Wooden Box, and a new Needle put into the Braſs Box, Weight 2=3.

No remarkable Change happened to either Needle till *April* the 5th.

Needle 2=3 in the Braſs Box.		Needle 2=5.
April 5.	$14^{\circ} = 5'$	$9^{h} = 00'$ $14^{\circ} + 00'$
	14 = 10	1 = 30 14 = 5
	14 = 10 —	5 = 30 14 +
	14 —	8 = 15 14 —
	13 = 50 +	8 = 37 13 = 45
	13 = 55 +	9 = 45 13 = 45 +
	14 —	10 = 25 13 = 50
	14 = 00	10 = 45 13 = 55
	14 +	11 = 00 14 = 00

The firſt Column ſhews the Variation of the Needle in the Braſs Box. The third the Variation of that in the Wooden Box. The ſecond Column ſhews the Time, by the Clock, when the Obſervations were made.

April

April 15. 1723.

14° = 30′ —	9h = 00′
14 = 30 —	10 = 00
14 = 30	11 = 30
14 = 30 +	12 = 30
14 = 30 +	1 = 30
14 = 30	3 = 30
14 = 30	4 = 10
14 = 30 —	5 = 30
14 = 20 —	6 = 18
14 +	7 = 8
14 = 00	7 = 50
14 = 00	8 = 15
14 = 15 +	8 = 20
14 = 15 +	8 = 40
14 = 15 +	12 = 15
14 = 00	12 = 27
14 = 00	12 = 32
14 = 00	12 = 35
14 = 00	12 = 43

Wind at S. W.

April 16.

14° = 30′ —	9h = 30′
14 = 30	11 = 00
14 = 30	12 = 00
14 = 30 +	1 = 10
14 = 30 +	1 = 40
14 = 30	2 = 45

14° = 30′	5h = 00′
14 = 30 —	6 = 00
14 = 25	6 = 20
14 = 20	6 = 30
14 = 15	6 = 35
14 = 10	6 = 40
14 = 10 —	6 = 45
14 = 5	6 = 49
14 = 00	6 = 57
14 = 00	7 = 10
14 +	7 = 20
14 = 5 +	7 = 30
14 = 10	7 = 45
14 = 15	8 = 00
14 = 15 +	8 = 20
14 = 20 —	8 = 30
14 = 25 +	9 = 00
14 = 25	12 = 12
14 = 25	12 = 21

Day warm, cloudy in the Morning, Evening clear.

April 19.

14° = 30′ —	8h = 35′
14 = 30 —	9 = 00
14 = 30 —	1 = 30
14 = 30 —	2 = 00
14 = 30	3 = 30
14 = 30	4 = 00
14 = 20	5 = 00
14 = 25	5 = 38
14 = 25 +	5 = 45

14=

14° = 30′ —	6h = 00′
14 = 30 —	6 = 45
14 = 25	7 = 00
14 = 20 —	8 = 00
14 = 20	9 = 00
14 = 20 +	10 = 00
14 = 25	11 = 00
14 = 25	11 = 15

Day warm, Wind at Eaſt, ſome Thunder in the Afternoon.

May 2.

14° = 25′	9h = 30′
14 = 30 +	10 = 30
14 = 35 —	11 = 30
14 = 35 +	1 = 00
14 = 35 +	1 = 52
14 = 40 +	2 = 30
14 = 20	3 = 30
14 = 25	3 = 45
14 = 30 —	4 = 00
14 = 25 +	6 = 45
14 = 20	7 = 00
14 = 30 —	7 = 35
14 = 20 +	12 = 50

Day cold, Wind at Eaſt.

May 3.

14° = 10′	9h = 30′
14 = 15	11 = 10
14 = 15 +	12 = 40
14 = 15 +	2 = 20
14 = 10 —	5 = 20
14 = 10	6 = 5
14 = 10	6 = 45
14 = 10 —	7 = 5
14 = 5	7 = 15
14 +	7 = 30
14 = 00	7 = 42
14 +	8 = 00
14 +	9 = 38
14 +	10 = 15
14 +	11 = 00

Day cold, Wind Eaſterly.

May 4.

14° = 5′	9h = 15′
14 = 5	9 = 30
14 = 10 +	1 = 35
14 = 10 +	3 = 17
14 = 10 +	3 = 50
14 = 10	4 = 55
14 = 10	6 = 00

14=

14°=00′	8h=15′
14 +	1 =00

Windy at Eaſt.

May 5.

14°=10′ +	9=30′
14 =15	10=45

14°=15′ +	12h=30′
14 =20	1 =57
14 =20	2 =45
14 =20	3 =25
14 =20 —	4 =35
14 =15 +	5 =30
14 =15 +	6 =10
14 =15 —	12 = 7

Day clear, Wind at Eaſt.

All theſe Obſervations are of the lighteſt of the three Needles, the Compaſs Box remaining unmoved the whole time. From *February* 6. 1722. to the 10th of *May* following, I made above a thouſand Obſervations in the ſame Place; and the greateſt Variation Weſtward, was 14°=45′, and the leaſt — — 13°=50′. It was ſeldom leſs than 14°, or greater than 14°=35′.

V. *Some*

V. *Some Observations upon Dr.* Eaton's *Styptick. By Dr.* Sprengell, *R. S. S. Coll. Med. Lond. Lic.*

THE Method of curing fresh Wounds in a few Days, without Suppuration, where neither Nerves, large Vessels, Bones, or any of the *Viscera* were concerned, has been long ago observed. *Purman*, a famous Surgeon of *Breslaw*, in his *Chirurgia Curiosa*, tells us of a Mountebank, who gave himself thirteen Wounds, by Incision, in the upper Part of his Left Arm, and thereupon applied his *Nostrum*, and with the help of a good Roller, was cured in two Days Time. Next he mentions a martial Styptick, which stopped bleeding incomparably, and healed fresh Wounds (as he says) in two Days time, especially if the Patient took withall a few Drops inwardly. This has likewise been mentioned by *Blegny* near thirty Years ago.

When I came into *France*, I found that many little Trials were made there also, with a Styptick Ball mixed with *French* Brandy, by striking a Cock thro' the Head, opening the Crural-Artery of a Dog, or chopping off a Dog's Leg, *&c.* But I found that it did not amount to any thing of Consequence; yet I have still a Ball, made above twenty Years ago, of Filings of Iron, and an equal Quantity of Tartar, mix'd well with *French* Brandy upon a Marble. This, with some Alteration, was afterwards published by the famous *Helvetius*, Physician to *Lewis* XIV. of *France*, in a Book called, *Recueil des Methodes pour la Guerison des diverses Maladies*, which was reprinted in *Holland*

Holland in the Year 1710. This Preparation was then ſold by *Pierre Rottermond*, Apothecary at the *Hague* in *Holland*.

The *Recipe* for his Medicinal Ball I have tranſlated, and is as follows:

Take four Pounds of the Filings of Steel, and eight Pounds of Tartar, well powdered; mix theſe well together, and put them in a new Earthen Pot, and pour thereupon as much *French* Brandy as will make it into a Poulteſs. Let this ſtand fermenting in a Cellar for four Days, and ſtir it between whiles. Then put it in *Baln-Mar*, and diſtill it *S. A.* with a moderate Fire, to draw off the Brandy. When you find that nothing but the Phlegm comes over, then take it from the Fire, and take out the Maſs, ſtamp it very fine, that not the leaſt Lump may remain; then mix it again as before with a ſufficient Quantity of Brandy, and put it in the Cellar to ferment, as before, and then diſtill it a ſecond Time. This Operation may be reiterated ſeven or eight times; but the laſt Time mix your Maſs well upon a Marble, and form it into two Ounce-Balls. One of theſe Balls is ſteeped in a Pint of good *French* Brandy, a little warmed, and hung only in it by a Wire, till the Brandy has received the Colour of the Ball. But if you are in haſte, then grate a ſufficient Quantity of the Ball in ſome Brandy, ſtir it well, and you may uſe it that very Inſtant.

No doubt but the Author thought by often grinding, fermenting, and diſtilling this Maſs, to comminute and ſubtilize its Particles, ſo as to make it more fit to contract the Fibres and Veſſels of a Wound, and to prevent Stagnations of the Fluids, both within and without, upon Contuſions; but the Succeſs did did not anſwer, and therefore it was laid aſide. Neither did *Helvetius* ever recommend it as an Univerſal

Styptick,

Styptick, aſtringent, or conſolidating Medicine, but meerly in freſh Wounds, and that only for a firſt Dreſſing, and where People lived at a Diſtance, and could not get immediate Aſſiſtance from a Surgeon. Beſides, he makes ſeveral Exceptions where it ſhould not be uſed; and, in general, adviſes it where Chalybeat Medicines may be made uſe of according to Experience.

But laſt Year a Balſamick Styptick was publiſhed by one Dr. *Eaton*, good to ſtop all manner of Bleeding without or within, and that without any manner of Exceptions. This made me deſirous to ſee it, and as it happened that ſoon after I had an Opportunity to examine it: I found preſently that this was the ſame old Medicine, which was got hither alſo, after other Countries had diſcarded it. But I neglected it at that Time, as not worth my notice, till I ſaw lately a Treatiſe of Conſumptions, publiſhed by Sir *Richard Blackmore*, Kt. giving Dr. *Eaton*'s Styptick the greateſt *Encomium*, that ever was given to any Invention whatſoever. For, ſays he, *Dr.* Eaton's *Balſamick Styptick bids fair for the Credit of a certain Remedy in ſtopping of Blood outwardly or inwardly, where the* Craſis *of the Blood is not entirely ruined; and will be of more Service to Mankind, than all the Diſcoveries made by Galenical Compounders of Drugs, and Syſtematical Methodiſts.*

Finding this Remedy recommended in ſo extraordinary a manner, by ſo eminent a Phyſician, I began now to think, that poſſibly I might have been miſtaken, and therefore deſired Mr. *Winterbottom*, an Apothecary in *Bow-Lane*, immediately to prepare the *Recipe*, as deſcribed by *Helvetius*. When this was ready, I ſent for a Bottle of Dr. *Eaton*'s Styptick, and tryed them both with Galls, be-

fore ſeveral Gentlemen; the Tincture was the ſame, a deep Purple. I then precipitated the Contents with old Hock, and found the precipitated Matter to be the ſame in both. Not contented with my own Enquiry, I ſent ſeveral ſmall Quantities to others, and went my ſelf to Mr. *Godfrey*, a famous Chymiſt in *Covent-Garden*. They all told me, that there was no Difference betwixt them. I tryed ſeveral ways to find out its Balſamick Quality, from whence it has its Name, but found none. Then, I confeſs, it ſurprized me, that a Man who had a mind to vend a thing as a Secret, had not done ſo much as to alter it either in Taſte, Smell, or Colour; and yet this might very well have been done, without robbing it of its Virtue in the leaſt.

My next Buſineſs was to try theſe two upon the Crural-Artery. Having got a good middle-ſiz'd Dog, Mr. *Ranby*, Surgeon, laid the Artery bare, and open'd it with a Lancet the length Way of the Artery, for near half an Inch. The old Trick uſed to be, to cut the Artery croſs ways, and then there was no neceſſity of a Styptick at all, nor indeed here neither. But at firſt *Helvetius* his Tincture was applied, and ſtopped the Bleeding; then we opened the Artery again, and tryed Dr. *Eaton*'s, with the ſame Succeſs. I then had the Artery opened in the other Thigh, and tryed it only with *French* Brandy, which, I found, did as well as the other two. I opened the Artery again, and had diſſolved in *French* Brandy a little *Sal Martis* and *Saccharum Saturni*, and applied that, and it anſwered in the ſame manner. This made me immediately conjecture, that there was but little Virtue in either of them, but only that the Brandy, by its great Heat, did meerly contract the Fibres of the Artery, which, no doubt, might be a little aſſiſted by the *Cha-*

lybs; but this could not be much. I then reflected upon the ſmallneſs of the Crural Artery in a Dog, and that it was no more to be compared to the Crural Artery of a Man, than a Cock's-Head to a Man's Head, and that a little Pledget of Lint might ſtop the Blood without more to do, as well as the Temporal Artery when opened with a Lancet, which we did, and the Pledget of Lint ſtopped it. We then untied the Dog, and ſent him going, who ran directly Home to the Crown Tavern in *Bloomſbury*, from whence we had taken him. The Miſtreſs of the Houſe tore off the Pledgets, and had the Dog well waſh'd with Butter and Beer warmed, ſhe not knowing what had been done to him, upon which the Dog fell a Bleeding again, though not much, and the Blood ſtopp'd of it ſelf: Mr. *Ranby* and I ſaw the Dog in the Evening very well, but he ran away at the Sight of us. Thus far as to its outward Uſe, and I could wiſh it were as harmleſs within. If only, according to *Helvetius*, it had been ordered to be taken inwardly, in freſh Wounds and Contuſions, a Body might have let it paſs; but when, without Exception, Dr. *Eaton* recommended it, as alſo even Sir *Richard*, in all inward Bleedings, I then thought it high Time to make ſome Animadverſions upon it. For Sir *Richard* himſelf ſays, in his Treatiſe of Conſumptions, *p.* 99 and 101, That in Spitting of Blood there is an Orgaſm, or ſtimulating Ferment: What is this but a Feveriſh Indiſpoſition? And is there any Hemorrhage without? Now, if ſo, will not Brandy and Chalybeats heighten this? Which by their Heat and *Stimuli* brace and irritate the Fibres, and accelerate the Blood's Motion. And will not then the Blood take up more Room, and preſs harder againſt the Sides of the Veſſels, and whatſoever oppoſes it?

Is

Is not this the Way to make an Orgaſm, and cauſe an Hemorrhage?

Dr. *Eaton* tells us himſelf in his Book, *p.* 57, That it did very much over-heat a Gentlewoman, and that her Bleeding ſtill continued after the taking of it, and ſhe might have periſhed if a Surgeon had not given her a cooling and aſtringent Apozeme. And but juſt before, *p.* 47, he complains of a Phyſician, that was not willing that his Patient ſhould take it, who had a Hectick Fever upon her, becauſe he was afraid that it was too hot.

Since the former Tryals, on the 10th of *June* laſt, I deſired Mr. *Ranby*, Surgeon, to open the Carotide Artery of a Dog, thinking that this Artery might give me more Satisfaction than the Crural Arteries had done, to try the Styptick Quality of *Helvetius* and *Eaton*'s Tinctures. Having laid bare the Jugular Vein, divided, and tied it, that its Bleeding might not hinder us from finding the Carotide Artery, we were obliged to cut ſome of the Muſcles through likewiſe, till, with ſome Difficulty, we found the Artery; which being opened with a Lancet, the Blood ſpouting forth, I applied to it *Helvetius*'s Tincture, upon which the Blood ſtopp'd. I took it off in leſs than a Minute, and made it bleed again, but it bled but little, and then I applied Dr. *Eaton*'s Styptick; we fill'd up the Wound with Lint, and ſtitched up the *Cutis*, then untied the Dog and let him run down Stairs, where, after ſome Time, I ſaw him again, and found he had bled a good deal, and was ſtill bleeding. I was very well ſatisfied that, the Artery being ſo very ſmall, he would not bleed to Death, and if he had had nothing but Lint upon it, it would have done as well. It muſt be obſerved, that the Carotide Arteries are largeſt in Proportion in Human Bodies, and that this Ar-

tery of the Dog was but a ſmall matter bigger than the Crural Arteries of the former Dog. This ſhews, that the Styptick Quality of theſe Tinctures is very inconſiderable; and that *Helvetius*'s Tincture is rather better than Dr. *Eaton*'s, if there is any Difference at all; though that, I believe, was owing to the Brandy, for my Brandy was ſtronger than Dr. *Eaton*'s.

I enquired for the Dog the next Morning, and found him alive and well, only hanging his Head on one Side, which proceeded from the Muſcles being cut through.

VI. *Materiarum quarundam gravitates Specificæ, diverſis temporibus ad varios ſcopos exploratæ a D. G.* Fahrenheit, *R. S. S.*

AUrum	19081
Mercurius	13575*
Plumbum	11350
Argentum	10481
Cuprum Suecicum	8834
Idem Japonenſe	8799
Ferrum	7817
Stannum provinciæ Indiæ Orientalis vulgo dictæ Malacca	7364
Stannum Anglicanum	7313
Marcaſita alba	9850
Regulus Antimonii	6622
Aurichalcum	8412
Cryſtallus de rupe	2669
Pyrites homogeneus	2584

Cenis

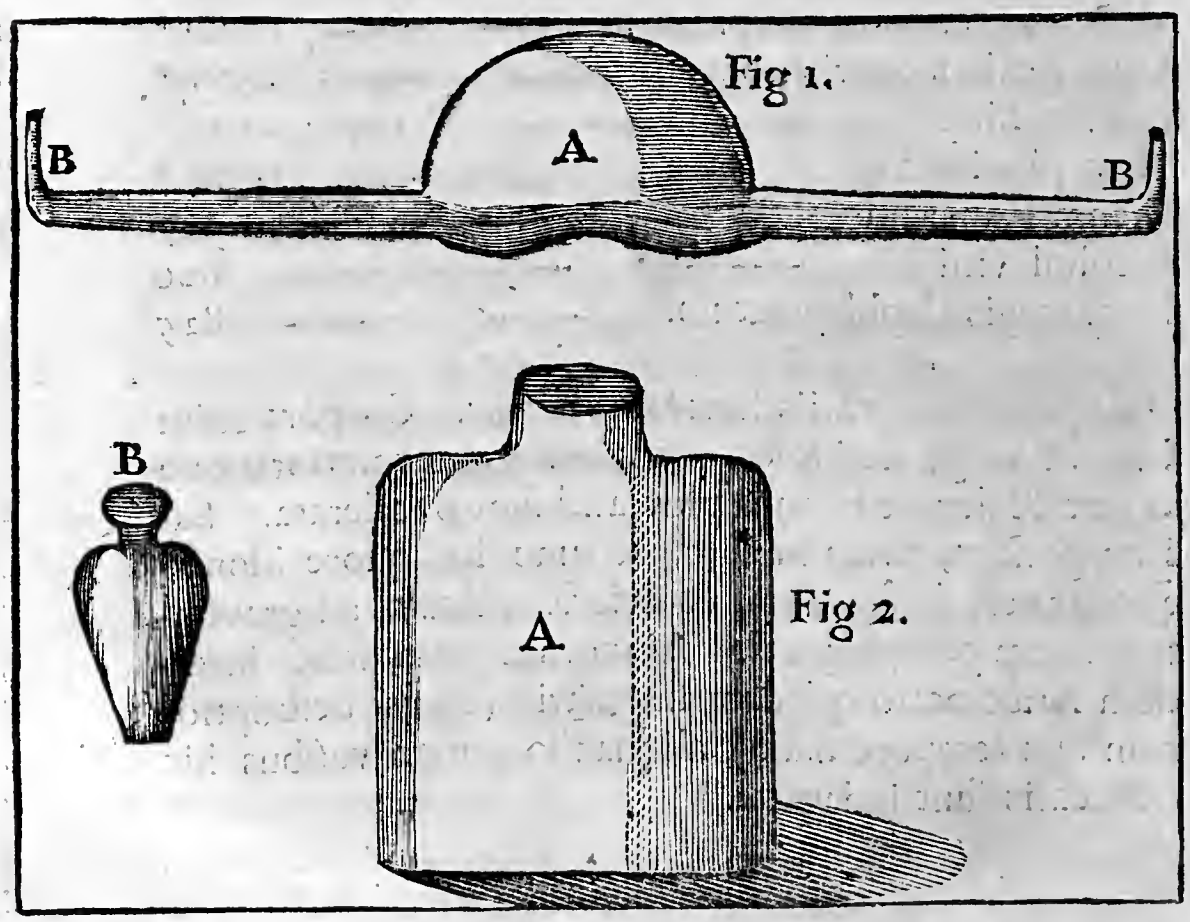
Cinis clavellatus ſordibus, ſaleq; neutro quodam (quod fere ſemper magis vel minus in cinere illo reperitur) depurgatus — — — 3112
Sal illud neutrum — — — — 2642
Sal maritimum — — — 2125
Nitrum — — — — 2150
Alumen — — — — 1738
Saccharum albiſſimum — — 1606½
Oleum Vitrioli — — — — 1877½ *
Lixivium cineris clavellati ſale quantum fieri potuit imprægnatum — — — 1563 *
Idem alio tempore præparatum — — 1571½ *
Aqua fortis melioris notæ — — 1409 *
Spiritus nitri — — — 1293½ *
Aqua pluviatilis — — 1000 *
Oleum Raparum — — — 913
Alcohol vini — — — 826
Idem magis dephlegmatum. — — 825

Experimenta variis ſunt facta modis. Corpora enim fixa, ut vulgo fieri ſolet, prius bilancis accuratioris ope in aere & deinde in aqua pluviatili ſunt ponderata. Salium pondus, prius in aere, & tunc in liquore idoneo quodam eſt exploratum, & deinde calculo ad gravitatem aquæ comparatum. Liquorum gravitates, interdum Aræometro quodam ſingulari (cujus deſcriptionem alio tempore dabo) aliquando autem vaſibus hic delineatis ſunt indagatæ.

Globus

Globus Vitreus concavus A (*Fig.* 1.) ad Lampadis flammam ſatis magnus conficitur, duobus tubulis vitreis ſibi oppoſitis B & B prædítus. Tubulorum extremitates ſunt apertæ, attenuatæ, & aliquantiſper incurvatæ, ne liquor effluere poſſit. Globulus præterea in inferiori loco aliquantulum eſt applanatus, ut eo commodius bilanci imponi poſſit.

Icones inſtrumentorum quæ inſervierunt ad facienda experimenta de gravitate ſpecificâ liquorum.



Ampulla A (*Fig.* 2.) e tenuiſſimo vitro ad lampadis flammam paratur, collo ſatis largo prædita, cujus apertura operculo B, intus concavo tam accurate, quam fieri poteſt, clauditur.

Ope hujus ampullæ, etiam ſalium gravitates ſpecificæ explorari poſſunt, & quidem hoc modo. Ampulla prius liquore quodam idoneo (in quo nempe ſal, cujus gravitas exploranda eſt, non ſolvitur) impletur, & poſtquam liquoris innotuit pondus, liquor effunditur; atque vas probe exſiccatur. Hoc facto, ſale fere totum vas impletur, & ſalis pondus inquiritur; hoc noto, interſtitia ſalis liquore replentur, ponderiſq; incrementum a liquore addito quæritur. Si hocce incrementum ponderis a pondere toto liquoris ſubtrahitur, reſiduum exprimet gravitatem liquoris a ſale deturbati.

Sal neutrum cineris clavellati in ſpiritu nitri nullam facit ebullitionem. Mercurium in ſpiritu nitri ſolutum albo colore præcipitat. Carbonibus ſuperimpoſitum, crepitu in minores partes diſrumpitur & diſpergitur.

Nitrum in tigillo ſupra ignem fuit liquefactum, ut eo modo ab omni humiditate depurgaretur, ſpatiaque nonnulla aëre alias plena nitro ipſo replerentur.

Gravitates liquorum, quæ aſteriſco notatæ ſunt, ad gradum quadrageſimum octavum meorum thermometrorum calculo ſunt revocatæ, & nonnullorum jam in* experimentis de gradu ebullitionis liquorum quorundam facta eſt mentio.

Modus ſimpliciſſimus ad inveſtigandam gravitatis differentiam, quæ a diverſo temperamento fluidorum originem ſuam trahit, eſt, ut prius liquore minus calido (cujus tamen gradus ope Thermometri notus eſſe debet) vas aliquod repleatur, & ponderetur, deinde illud vas iterum calidiori liquore impleatur, & ut prius ponderetur. Si in hoc ſecundo experimento gradus caloris iterum eſt notatus, habebitur differentia gravitatis liquoris, a calore inter hos gradus effecta, quæ deinde ope calculi facile unicuique gradui attribui poteſt.

* *Phil. Tranſ.* No. 381.

Experimenta

Experimenta in aere ſunt facta: addenda ergo erit unicuiq; numero gravitas aeris, ut habeatur materiarum gravitas in vacuo. Eſt autem aeris gravitas ſpecifica ad illam aquæ, fere ut 1 ad 1000, utpote Philoſophiæ naturalis cultoribus abunde patet.

VII. *Some Conſiderations about the Cauſe of the univerſal Deluge, laid before the Royal Society, on the 12th of* December 1694. *By Dr.* Edmond Halley, *R. S. S.*

THE Account we have of the univerſal Deluge is no where ſo expreſs as in the Holy Scriptures; and the exact Circumſtances as to point of Time, do ſhew that ſome Records had been kept thereof more particularly than is wont in thoſe things derived from remote Tradition, wherein the Hiſtorical *Minutiæ* are loſt by length of Time. But the ſame ſeem much too imperfect to be the Reſult of a full Revelation from the Author of this dreadful Execution upon Mankind, who would have ſpoke more amply as to the Manner thereof, had He thought fit to lay open the Secrets of Nature to the ſucceeding Race of Men; and I doubt not but to all that conſider the 7th Chapter of *Geneſis* impartially, it will paſs for the Remains of a much fuller Account of the *Flood* left by the Patriarchs to their Poſterity, and derived from the Revelation of *Noah* and his Sons. It muſt be granted, that there are ſome Difficulties as to the Conſtruction of the *Ark*, the Reception and Agreement of the *Animals* among themſelves, and Preſervation of it in ſo immenſe and boundleſs an Ocean, during that *Wind*

which

which God ſent to dry the Waters away, eſpecially when it firſt came on Ground: But it muſt alſo be allowed, that length of Time may have added, as well as taken away many notable Circumſtances, as in moſt other Caſes of the Story of remote Times and Actions.

This we may, however, be fully aſſured of, that ſuch a Deluge has been; and by the many Signs of marine Bodies found far from and above the Sea, 'tis evident, that thoſe Parts have been once under Water: or, either that the Sea has riſen to them, or they have been raiſed from the Sea; to explicate either of which is a Matter of no ſmall Difficulty, nor does the ſacred Scripture afford any Light thereto. All that it ſays to help us is, that all the Fountains of the great Deep, תְּהוֹם רַבָּה, were burſt, or broken up; that the Windows, or Cataracts of Heaven were opened, and that it rained inceſſantly forty Days and Nights. Now the Rain of forty Days and Nights will be found to be a very ſmall Part of the Cauſe of ſuch a Deluge; for ſuppoſing it to rain all over the Globe as much in each Day, as it is now found to do in one of the rainieſt Counties of *England* in the whole Year, *viz.* about forty Inches of Water *per Diem*, forty ſuch Days could cover the whole Earth with but about twenty two Fathom Water, which would only drown the low Lands next the Sea, but the much greater Part would eſcape. What is meant by the Fountains of the *Abyſſe* being broken up, and the opening of the Windows of Heaven, ſeems not ſo eaſy to be underſtood, but is intended to indicate the *Modus* of the Deluge, which was, according to the *Moſaic Philoſophy*, from the letting in of the Waters above the Firmament, mentioned *Geneſis* 1. 7. by the Windows

of Heaven; and the riſing up out of the Ground of the Waters under the Earth, ſpoken of in the ſecond Commandment: Or, (if you will underſtand that by the תְּהוֹם רַבָּה is meant the great Ocean) by the overflowing of the Sea, riſing upon the Land, which is expreſs'd by the breaking up of the Fountains of the great Deep. So that we may reaſonably conclude, that by the one of thoſe Expreſſions is meant an extraordinary fall of Waters from the Heavens, not as Rain, but in one great Body; as if the Firmament, ſuppoſed by *Moſes* to ſuſtain a *Supra-aerial Sea*, had been broken in, and at the ſame Time the Ocean did flow in upon the Land, ſo as to cover all with Water.

By an extraordinary Encreaſe of the Waters this could not be effected, for that at this Time there is not Water ſufficient of itſelf to cover any more of the Earth than now it doth; and to ſuppoſe a Creation and Annihilation of Water on purpoſe to deſtroy the Earth, is by much the moſt difficult Hypotheſis that can be thought of to effect it. A change of the *Center* of *Gravity*, about which Center the Sea is formed, ſeemed not an improbable Conjecture, till it appeared that this Center of Gravity was the neceſſary Reſult of the Materials of which our Globe conſiſts, and not alterable whilſt the Parts thereof remained in the ſame Poſition: And beſides this Suppoſition could not drown the whole Globe, but only that Part thereof towards which the Center of Gravity was tranſlated, leaving the other Hemiſphere all dry.

I ſhall ſay nothing of Dr. *Burnet*'s Hypotheſis, nor of the many Inſufficiencies thereof, as jarring as much with the Phyſical Principles of Nature, as with the Holy Scriptures, which he has undertaken to reconcile.

cile. Dr. *Hook*'s Solution of this Problem, as he has not fully diſcovered himſelf, I cannot undertake to judge of; but his Compreſſion of a Shell of Earth into a *prolate Spheroide*, thereby preſſing out the Waters of an Abyſs under the Earth, may very well account for drowning two extream oppoſite Zones of the Globe; but the middle Zone, being by much the greater Part of the Earth's Surface, muſt by this means be raiſed higher from the Center, and conſequently ariſe more out of the Water than before; and beſides, ſuch a Suppoſition cannot well be accounted for from Phyſical Cauſes, but require a preternatural *digitus Dei*, both to compreſs, and afterwards reſtore the Figure of the Globe.

But the Almighty generally making uſe of Natural Means to bring about his Will, I thought it not amiſs to give this Honourable Society an Account of ſome Thoughts that occurr'd to me on this Subject; wherein, if I err, I ſhall find my ſelf in very good Company.

In Num. 190. of theſe Tranſactions, I have propoſed the caſual *Choc* of a *Comet*, or other tranſient Body, as an Expedient to change inſtantly the Poles and Diurnal Rotation of the Globe; at that Time only aiming to ſhew how the *Axis* of the *Earth* being chang'd, would occaſion the Sea to recede from thoſe Parts towards which the Poles did approach, and to encreaſe upon and overflow thoſe Parts wherefrom the Poles were departed; but at that Time I did not conſider the great Agitation ſuch a *Choc* muſt neceſſarily occaſion in the Sea, ſufficient to anſwer for all thoſe ſtrange Appearances of heaping vaſt Quantities of Earth and high Cliffs upon Beds of Shells, which once were the Bottom of the Sea; and raiſing up Mountains where none were before, mixing the Elements into

ſuch a Heap as the Poets deſcribe the *old Chaos*; for ſuch a *Choc* impelling the ſolid Parts would occaſion the Waters, and all fluid Subſtances that were unconfined, as the Sea is, with one *Impetus* to run violently towards that Part of the Globe where the Blow was received; and that with Force ſufficient to rake with it the whole Bottom of the Ocean, and to carry it upon the Land; heaping up into Mountains thoſe earthy Parts it had born away with it, in thoſe Places where the oppoſite Waves balance each other, *miſcens ima ſummis*, which may account for thoſe long continued Ridges of Mountains. And again, the Recoil of this Heap of Waters would return towards the oppoſite Parts of the Earth, with a leſſer *Impetus* than the firſt, and ſo reciprocating many times, would at laſt come to ſettle in ſuch a Manner as we now obſerve in the Structure of the ſuperficial Parts of the Globe.

In this Caſe it will be much more difficult to ſhew how *Noah* and the *Animals* ſhould be preſerved, than that all things in which was the Breath of Life, ſhould hereby be deſtroyed. Such a *Choc* would alſo occaſion a differing Length of the Day and Year, and change the Axis of the Globe, according to the Obliquity of the Incidence of the Stroak, and the Direction thereof, in relation to the former Axis. That ſome ſuch thing has happened, may be gueſs'd, for that the Earth ſeems as if it were new made out of the Ruins of an old World, wherein appear ſuch Animal Bodies as were before the Deluge, but by their own Nature and Defences from the Weather, have endured ever ſince, either petrified, or elſe entire in *ſtatu naturali*. Such a *Choc* may have occaſioned that vaſt Depreſſion of the *Caſpian Sea*, and other great Lakes in the World; and 'tis not unlikely, but that extream Cold felt in the

North-West of *America*, about *Hudson's-Bay*, may be occasioned by those Parts of the World having once been much more Northerly, or nearer the Pole than now they are; whereby there are immense Quantities of Ice yet unthaw'd in those Parts, which chill the Air to that degree, that the Sun's warmth seems hardly to be felt there, and of which the Poet might justly say, *Frigus iners illic habitat pallorque tremorque —Ac jejuna fames.*

If this Speculation seem worthy to be cultivated, I shall not be wanting farther to insist on the Consequences thereof, and to shew how it may render a probable Account of the strange Catastrophe we may be sure has at least once happened to the Earth.

VIII. *Some farther Thoughts upon the same Subject, delivered on the 19th of the same Month. By the same.*

I HAVE been advised since the last Day, by a Person whose Judgment I have great Reason to respect, that what I then advanced, ought rather to be understood of those Changes which might have happen'd to the Earth in Times before the Creation, and which might possibly have reduc'd a former World to a *Chaos*, out of whose Ruins the present might be formed, than of the Deluge whereby Mankind was in a manner extinguished about 4000 Years since; that being much more gradually brought to pass, and with some Circumstances that this Hypothesis cannot admit of, which abler Pens, perhaps, may account for: What I have advanced, I desire may be taken for no more

more than the Contemplation of the Effects of such a *Choc* as might possibly, and not improbably, have befallen this Lump of Earth and Water in Times whereof we have no manner of Tradition, as being before the first Production of Man, and therefore not knowable but by Revelation, or else *a posteriori* by Induction from a convenient Number of Experiments or Observations, arguing such an Agitation once, or oftner, to have befallen the Materials of this Globe. And perhaps, in due Periods of Time, such a Catastrophe may not be unnecessary for the well-being of the future World; to bury deep from the Surface those Parts, which by length of time are indurated into stony Substances, and become unapt for vegetable Production, by which all Animals are either immediately or mediately sustained: the ponderous Matter in such a Mixture subsiding first, and the lighter and finer Mould remaining for the latter Settling, to invest the exterior Surface of the New World. This may, perhaps, be thought hard, to destroy the whole Race for the Benefit of those that are to succeed. But if we consider Death simply, and how that the Life of each Individual is but of a very small Duration, it will be found that as to those that die, it is indifferent whether they die in a Pestilence out of 100000 *per Ann.* or ordinarily out of 25000 in this great City, the Pestilence only appearing terrible to those that survive to contemplate the Danger they have escaped. Besides, as *Seneca* has it,

Vitæ est avidus quisquis non vult
Mundo secum pereunte mori.

N. B.

N.B. *The foregoing Papers having been read before the Society thirty Years since, were then depositedby their Author in their* Archives, *and not published; he being sensible that he might have adventured* ultra crepidam; *and apprehensive least by some unguarded Expression he might incur the Censure of the Sacred Order. Nor had they now been printed, but at the Desire of a late Committee of the Society, who were pleased to think them not unworthy the Press.*

Here the Reader is desired to observe, that Mr. William Whiston's *Book, entituled,* A New Theory of the Earth, *was not published till about a Year and a half after the Date hereof, and was not presented before* June 24, 1696. *to the Royal Society.*

F I N I S.

E R R A T U M.

Pag. 118. *lin.* 25, 26. *read* Relation.

A D-

ADVERTISEMENT.

ALL Persons concern'd in the Practice of *Inoculating the Small Pox*, are desir'd to send to Dr. *Jurin*, Secretary to the Royal Society, any time before the End of *February* next, an Account of the Name and Age of every Person *inoculated* by them, from the Beginning to the End of the present Year, the Place where it was done, the Manner of the Operation, the Days of Sickening and of the Eruption, what Sort of Small-Pox was produc'd, and the Event.

Where the true Small-Pox was not produced by Inoculation, it will be of use to take particular Notice, whether the Patient had any other kind of Eruption, what Symptoms preceded, or attended it, whether the Incisions inflam'd and run, and for what Time their running continued.

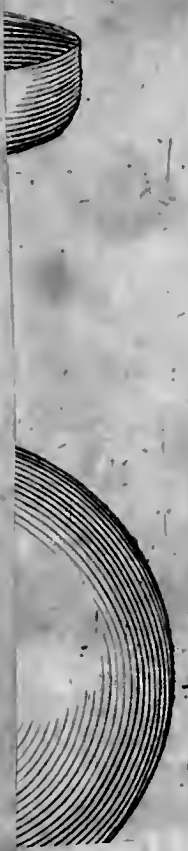
In case of any Person's dying after Inoculation, either in the Course of the Small-Pox, or after they are gone off, it is desir'd that a particular Relation of the Case may be sent, attested, if it be judged necessary, by the nearest Relations of the Party deceased, or by other credible Persons, that were Witnesses to the Fact.

Such Gentlemen as have not already sent in their Accounts for the preceding Year, are now requested to communicate them.

Lincoln's-Inn Fields,
Decemb. 7, 1724.

LONDON: Printed for W. *and* J. INNYS, *Printers to the* Royal Society, *at the West End of St.* Paul's *Church-yard.*

plate. 1.



Philosoph: Transact: N° 334. Fig. 6

Numb. 384.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *July*, *August* and *September*. 1724.

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IX. *An Account of a Book entituled,* Dominici Bottoni, de immani Trinacriæ Terræ Motu, Idea Hiſtorico-Phyſica, in qua non ſolum Telluris concuſſiones tranſactæ recenſentur, ſed noviſſimæ Anni 1717. Meſſanæ 1718. 8°. *By* J. G. Scheuchzer, *M. D. R. S. S. Coll. M. L. Lic.*

I. *An Account of ſome new Experiments, relating to the different, and ſometimes contrary Motion of the Sap in Plants and Trees, made by* Thomas Fairchild, Gardener *at* Hoxton.

I HAD the Honour, ſome Years ago, to ſhew ſome Experiments before the *Royal Society*, and they were pleaſed to allow the Experiments to be new and uſeful; which encouraged me to try further, and bring more Experiments, in order to ſhew the Courſe of the Sap; which I find, by Experience, will be ſo uſeful, that I can make barren Trees fruitful, and decaying Trees healthful, and render the Syſtem of Gardening and Planting more uſeful to the Publick.

The firſt Plant I ſhewed was the *Laureola*, grafted upon the *Mezereon*, and the *Evergreen Oak* of *Virginia* upon the common *Engliſh Oak*; both which hold their Leaves all the Winter, and are in good State and flouriſhing, though grafted on Plants that drop their Leaves in Winter; which plainly proves that the Juices riſe upwards, in Winter, in thoſe Plants that drop their Leaves, otherwiſe the *Evergreens* that are grafted on them would ſoon periſh.

I believe by grafting the Variety of foreign Oaks on the *Engliſh*, we might make the Timber more firm and laſting, than it is in its own Nature, when raiſed from foreign Acorns: For as the Crab Stock maketh the Wood of the Apple-Tree more firm and laſting than that on the Apple-Stock, and the Peaches and Almonds, budded on Plums, are more laſting than thoſe on Peach-Stocks; ſo by the contrary Rule, all firm Timber,

grafted on ſpungy Stocks, would be made worſe than it would be on its own Bottom. For Example, If that which is called the *Engliſh Elm*, ſhould be grafted on that which is called the *Dutch*, it would partake of abundance of the ſpungy Juices of the Stock, whereby the Timber would become unfit for the Purpoſes it is now uſed for.

The firſt Experiment, I have to offer to your Obſervation at preſent, is made on the *New England Cedar*, or rather *Juniper*, grafted on the *Virginia*; and what is remarkable in it, is, That the Branch, which is grafted, is left ſeveral Inches below the Grafting, which Part continues growing as well as the upper Part above the Grafting.

The ſecond Plant is the *Viburnum*, with the Top planted in the Ground, which is become Roots; and the Roots turned up, which are become Branches. But whether the ſame Veſſels, which fed the Branches, have changed their Courſe, or whether the Juices go up and down in the ſame Veſſels, I ſhall leave to better Judgments; but I find the Plant in as good State of growing, as it was in its natural State.

The third Experiment is made on a Pear-Tree, which I enarched upon two Pear-Stocks, in *March* 1721-2. which is now in a good flouriſhing State, with a Branch in Bloſſom, and receiveth no Nouriſhment but by the two enarched Branches, the Root being out of the Ground; and tho' it was done above two Years ago, it is now ſhooting Suckers out of the Root, which proveth that the Branches are as uſeful to ſupport the Roots, as the Roots the Branches; and it is therefore no Wonder that ſo many Trees miſcarry in Planting, when there are no Branches left on the Head.

The

The fourth Experiment is made on the Cedar of *Libanus*, grafted on the *Laryx*, which drops its Leaves in the Winter; yet maintains the Cedar in as flouriſhing Condition, as if it had been on a Tree that held the Leaves all the Winter; and the Part of the Graft, left below the Grafting, is in as good Health as the Part above the Grafting.

II. *An Account of an extraordinary Cure by Sweating in Hot Turff; with a Deſcription of the* Indian Hot-Houſes; *by the Honourable* Paul Dudley, *Eſq;* F. R. S.

IN the Year 1704. *Peter Coffin*, Eſq; of *Exeter* in *New England*, being then ſeventy four Years of Age, had taken a great Surfeit, as 'twas thought, by drinking cold Water, in a very hot Day, and when he had heated himſelf in the Woods. This Surfeit ſettled principally in his right Side, but gave him a racking Pain all over his Body, and particularly depriv'd him of the Uſe of his right Arm: In this Condition he kept his Houſe and Bed for nine Weeks, and his Recovery, conſidering his Age, was deſpaired of; when a Son of his, from whom I had the Relation, making a Viſit to his Father, propoſed the Sweating of him in Turff: The Father readily agreed to it, having uſed many Medicines, from other Phyſicians, without any Effect. Immediately Orders were given to cut a large Oven full of Turff; the Pieces might be to eighteen Inches ſquare. The Turff it ſelf was of *Engliſh* Graſs, and only the Swerd, or Top of the Earth, with the Graſs. Before the Turff was put into the Oven, the Doctor rubb'd

the graſſy Side of the Turff, with ſome Spirit, or Oyl, and then doubled the Graſs-ſides together, and ſo ſet them in. When they were well baked, which was in about two Hours, he took them out, and made a Bed of them upon the Floor, (the Place for the Head raiſed); as ſoon as that was done, he ordered his Father to be taken out of his Bed without his Shirt, but wrapp'd up in a Sheet, and laid upon the hot Turff, and then proceeded to cover him over with the reſt of the Turff, more eſpecially on his Side, where the Seat of his Pain was, but they laid none on his Breaſt or Head; then they cover'd him with Blankets to keep the Heat in: While the Father was in this Bath, the Son gave him warm Cordials, to prevent fainting, which he was in great Danger of; after he had lain thus about three Quarters of an Hour, which was as long as he could bear it, he was put naked into the Bed very well warm'd, where, in a few Minutes, he fell aſleep, and ſweat to that Degree, that it run, thro' his Pillow and Bed, upon the Floor. After about two Hours Sleep they dry'd him, and put him on warm Cloaths, and the old Gentleman found himſelf much eaſed and refreſhed: This was in the Morning; and before Night, he walked about the Houſe comfortably, his Pain being in a manner all gone; the next Day the Doctor repeated his Cordials, and the fourth Day he ſweat his Father a ſecond Time, in the ſame manner as above; and the next, *viz.* the fifth Day, he went abroad about his Buſineſs, and lived eleven Years afterwards in perfect Health, and free from Pain. The Doctor tells me, great Care muſt be taken that the Patient do not lie too long in the Turff, and that even a Quarter of an Hour may be ſufficient for ſome Perſons; and when e'er the Patient begins to fetch his Breath ſhort, or faint, he muſt be put to Bed immediately, and the Phyſician, or Operator, muſt by no means

means omit his Cordials. I ſhould have been glad to have made this Account yet more perfect, by acquainting you, what the Specifick was the Doctor put upon the Turff, before he ſet them into the Oven, but I could not poſſibly prevail upon him to tell me. As to the Matter of Fact, or Story of curing the old Gentleman, in this new and wonderful Manner, it was fam'd throughout the Country in the Day of it; but the Particulars I lately had an Opportunity of having from the Son, who ſo happily made the Experiment.

Houſes, to ſweat in, were common among the *Aborigines*, when the *Engliſh* firſt came into *New England*, tho' now but little uſed. A Gentleman of the Iſland of *Nantucket*, where the *Indians* ſometimes practiſe it, even at this Day, or very lately, gives me the following Relation.

The Cave was uſually four Foot high, and to eight Foot Diameter; the Roof ſupported with Sticks or Boards, covered with Earth, and they dug it in the Side of a Hill, and, as near as could be, to ſome River, Pond, or Place of Water: The Entrance into this Cave was ſmall, and the Door (when any Perſon was ſweating) was covered with a Blanket or Skin; near the Cave they make a good large Fire, and heat a Parcel of Stones, to the Quantity of five hundred Weight, and roll them in red-hot, piling them up in the middle of the Cave; when this is done, the *Indians* go in naked, and ſet round the heated Stones as many as pleaſe; as ſoon as they begin to grow faint, which may be in a Quarter of an Hour, they come out, and plunge themſelves all over in the Water for a Minute or two, and then in again, as long as they can well bear it, and ſo in the Water a ſecond Time, and then dreſs themſelves. This has been uſed with Succeſs for Colds, Surfeits, Sciatica's, and Pains fixed in the Limbs; and even the *Engliſh*

liſh have many times found Relief by it. I don't underſtand, but that it may be practis'd at any Time of the Year, without Hazard or Inconvenience. The *Indians* often uſed it before, and after long Journies, Hunting or Voyages, to ſtrengthen and refreſh themſelves.

III. *An Attempt to account for the riſing and falling of the Water of ſome Ponds near the Sea, or ebbing and flowing Rivers; where the Water is loweſt in the Pond, at the Time of high Water in the Sea or River; and the Water is higheſt in the Pond, at the Time of low Water in the Sea or River. As alſo for the increaſing or decreaſing of the Water of ſuch Pools and Brooks as are higheſt in the dry Seaſons, and loweſt in the rainy Seaſons: With an Experiment to illuſtrate the Solution of the* Phænomena. *By the Reverend* J. T. Deſaguliers, *L. L. D. and R. S. S.*

HEro *Alexandrinus*, and other Hydraulick Writers, have deſcrib'd a Cup (call'd a *Tantalus*, from its Effect) which will hold any Liquor very well, when it is not fill'd above a certain Height mark'd in the Cup; but if it be fill'd higher, not only the Liquor above the Mark will run out, but the whole Liquor that was in the Cup. This is perform'd by a Syphon in the Cup, which is ſometimes conceal'd to make the Effect the more ſurpriſing.

The

The Cup, A B (*Fig.* 1.) has a visible Syphon C E D in it; the Cup, (*Fig.* 2.) has the same, conceal'd by the Figure of a Man, to represent *Tantalus* in the Fable; and the Cup of *Fig.* 3. has its Syphon more conceal'd, as it is carried up into the Handle. Any of these Cups will hold Water very well, provided they are not fill'd up above the Line F G; for then not only the Liquor that is above F G will run out, but all the Liquor in the Cup as low as D, the Orifice of the short Leg of the Syphon.

EXPERIMENT. Fig. 4.

In the Vessel *a b c d* is plac'd an open wooden Box A B C D fill'd with Water as high as the Line L M. Another Box or Plug E F G H made tight, and containing Weights to sink it, is made to let down into the Water between the Partition I K and the End A B of the Box above mention'd; but when it is not to press the Water up to I O, (as it does when let down) it is drawn out of the Water by the Weight *m*, which pulls it up by the Bar *i k* fasten'd to a Leaver moving round the Center *l*.

When, by means of the Plug, the Water in the Space A B K I is push'd up to I O, by passing under K; it runs out thro' the Spout P Q (whose Passage is gaged by a little Sluce P *p*) and falls into the Vessel R S made of an oblong Figure like a Fish-Pond, and having a Syphon at S, so as to make it a *Tantalus*, or in the Nature of the Cups above mentioned.

Let the Weight *m* pull up the Plug E F G H, and the Water, having fill'd R S, will run down below the Orifice P to M.

The *Tantalus* R S, beginning to run out as soon as full, will for the Reasons above given, continue to run till

till it is all emptied; and as it diſcharges it ſelf into another *Tantalus* T V (whoſe Syphon is at V); this laſt *Tantalus* will alſo, when full, begin to run out, and its Water go down to *x Y o*.

If the Plug be let down gradually, as ſoon as the Water begins to run out of the laſt *Tantalus* T V, (and the firſt *Tantalus* R S be cover'd ſo as to be conceal'd from Sight) it will appear to the Lookers on, That the Cavity T V, repreſenting a Pond near an ebbing and flowing River (as I am credibly inform'd there is ſuch an one at *Greenhive* in *Kent*, between *London* and *Graveſend*) always riſes, whilſt the Water at N O (or the Tide) falls to L M; and always ſinks whilſt the Water at L M (or the Tide) riſes to O L.

EXPERIMENT II.

Let the Water in the Box A B C D not be made uſe of; only the Veſſel Z be fill'd every half Hour: It will empty it ſelf in the Space of a Quarter of an Hour, falling like Rain, and dropping alſo thro' the Leaden Platform *e f* into the hidden *Tantalus* R S, which will not begin to run till this artificial Rain is over: Then in a Quarter of an Hour more, the *Tantalus* R S will have emptied it ſelf into the viſible *Tantalus* T V, which will be filling all the Time after Z has done running; (or in the dry Seaſon) and as ſoon as T V is full, it will begin to run out thro' its Syphon V, at the End of the half Hour, when the Veſſel Z or Sieve runs again; that is, at the Return of the rainy Seaſon.

This laſt Experiment may eaſily be applied to thoſe Ponds, or thoſe Brooks, that are high in dry Weather, and low in wet Weather; of which Kind, I am told, there is a Brook at *Lambourn* in *Berkſhire*.

If it be objected, that ſuch Ponds are full for ſome time, which a *Tantalus* cannot be, becauſe it begins to run out as ſoon as full; that may be eaſily ſolv'd, by ſuppoſing the hidden *Tantalus*, (or intermediate Cavity between the River and Pond) to contain more Water than the viſible one, provided it does not contain ſo much as not to be emptied, before the Return of the Tide.

The ſame Solution will ſerve for wet and dry Seaſons, only ſuppoſing the Cavities larger.

If it be asked, where the Water of the viſible *Tantalus*, near a River, can run; it may be anſwer'd, that all this may happen, tho' the ſecond, or loweſt *Tantalus* ſhou'd have its Bottom higher than low Water-Mark in the River. And for the Syphons, which are of a particular Make in the Cup; tho' ſuch be not ſuppos'd in the Earth, yet any long Paſſage, riſing in the Middle, will anſwer the End. See *Fig.* 5. where ABCD repreſents the Channel of a River, AD high Water-Mark, and GH low Water-Mark; ZI a Paſſage from the River to the Cavity IKLMN, or firſt, or hidden *Tantalus*; LMQ the Syphon of the firſt *Tantalus*, running into the ſecond *Tantalus*, or viſible Pond OQRP, which by its Syphon RSV runs out into low Grounds that may be above the low Water-Mark GH; and the Bottom KL of the firſt *Tantalus* may be above the Top of the laſt, whoſe Level is the Line WW.

ABCDYOQRPVH is the Section of the Surface of the Earth.

IV. *Nobiliſſimi Viri* ADAMI LEYEL *Reg. Colleg. Metallici Aſſeſſoris* NARRATIO *accurata de* CADAVERE HUMANO *in fodina Cuprimontana ante duos annos reperto. Ex Actis Literariis Sueciæ. Ann.* 1722. *Trimeſt. I.*

ANimalcula quævis, inſecta, piſces, cochleas & conchylia, fructus quoſcunque, arbores, ſemina, & herbas in lapides abire, & in ſaxeam mutari duritiem, experientia quotidiana, & innumeræ res, lapidea rigiditate aſperæ, tam intra Svioniam, quam apud exteros paſſim obviæ teſtantur. Hæc petrificatio, ſ. ſolidum in lapidem transformatio, originem ducit a tenuiſſimo halitu calcario, aut alio calce gravido minerali vapore, qui ſe fumi inſtar in animalium quorumvis & vegetabilium poros ingerit, ſedem ibi figit, ac vi ſubſalſæ duritiei lapideſcere cuncta cogit. Neque enim aliunde arceſcenda eſt cauſa, cur pleræque tam indigenæ quam peregrinæ ſic dictæ petrificationes, in montibus inprimis calcariis, aut etiam terra, calcarius ubi lapis effoditur, in conſpectum prodeant, & ex iis eruantur: qua de re tam quæ in Germania & Helvetia, quam Anglia & Italia, lucem videre Lithographorum ſcripta, evolvi conſulique merentur.

Veram hancce, & ſolidam mutationem imitatur alia, ſed rudior & imperfectior, & quæ mutationem mentiatur, petrificationis ſpecies, incruſtatio dicta, quam efficit ſaxea in ſubtilem viſcum ſoluta ſubſtantia, qui occurrentium corporum ſuperficiei tenacius ubi adhæſit, lapideam iis cruſtam paulatim induit; haud ſecus ac fruges variæ, ſemina, corticeſque condiri, ſaccharoque induci ſolent. Cujus generis in Bohemia, Germania, Anglia,

aliiſque regionibus non pauca occurrunt, ſed inferiore tamen loco, quam vera petrificata, cenſenda.

Porro varia etiam ſalium genera, alumen puta, ſal culinare, & vitriolum, ſimili donata virtute deprehenduntur. Hæc corpora quidem non in ſolidum convertunt lapidem, ſalſitudine tamen ſua, & conſtringendi vi, obvia quævis ita condenſant & indurant, ut nativa poſita mollitie, in lapideam duritatem deflectant.

Vitrioli autem, inter omnia ſalium genera, vaporem proclivem maxime & idoneum eſſe ad duritiem rebus quibuſcunque hoc modo conciliandam, alia ut præteream, varia probant hominum membra, manus, pedeſque, quæ fodina Fahlunenſium cuprea, in aqua vitriolo fœta, ſubinde occurrentia præbet conſpicienda; quæque vigore ac inſita facultate vitrioli adeo induruiſſe patet, ut putredinis neſcia corruptionem omnem morentur.

Illuſtre inprimis hujus veritatis documentum dedit in laudata modo Fahlunenſium fodina repertum haud ita pridem humanum cadaver, ante 50 circiter annos infelici ruina obrutum ibi contumulatumque: ſed quod erutum integrum, ac ſive carnem, ſive cutem reſpicias, mutationis omnis expers comparuit, præterquam quod corneam contraxiſſet duritatem.

De incorrupto hocce cadavere Novellis Hafnienſium Literariis * narratiunculam ante aliquod tempus inſertam vidimus, quæ tamen quum circumſtantias nonnullas omittat, nonnulla rurſus tradat, quæ cum rei veritate minus congruant, hinc rem non ingratam facturum me ſpero, ſi poſt luſtratum rite cadaver, & collectas fide dignas loci incolarum relationes, facti hiſtoriam ſequentem in modum cum eruditis communicavero, ſchemaque adjunxero, quod non fodinæ minus faciem, quam oppreſſi ruina hominis habitum adumbret.

* Extrait des nouvelles du mois de Septembre 1720. p. 207.

Anni 1719. menſe Decembri, cum Wrediani putei cuniculus (Wred-*Schachtz Stollen*, vid. Lit. I, Fig. 6.) in eum, qui (*Mârdskins-Schachtz korgeſtaden*, Lit. D.) vocatur, agendus ducenduſque eſſet, in puteo eodem (*Mârdskins Schachtet*) 82. orgyias profundo, loco quem Lit. L. deſignat, in rupis ipſius confinio, ſub aqua & 5 orgyiarum ruina, defuncti hominis corpus in conſpectum venit. Utrumque crus, cum brachio dextro & capite, avulſa petræ moles contuderat: facies vero, corpuſque reliquum cum veſte, integra plane & intacta cernebantur; habituſque totus viri, collariæ faſciæ orâ extremâ, ſiniſtræ manus ope, os obturantis, is erat, quem Lit. K. exhibet ac demonſtrat. Crumenâ, quam gerebat, pyxis ex orichalco oblonga, pyxide vero tabaci condebatur fruſtulum, utroque illæſo & integro; ductitium autem ferrum, quo ad pyxidem annecti operculum volvique ſolet, aqua tincta edaci vitriolo totum abſumſerat. Caro hominis cutiſque, aſperæ licet & duræ palpantibus viderentur, non tamen lapidis ea erat durities, ſed corneæ aut ungulinæ, etiam ſpecie, ſuppar, quippe quæ cultro cederet ſcindique poſſet.

Poſt extractum tumulo, fodinaque corpus, diligenti examinatione quæſitum eſt, ecquis eſſet, qui agnoſcere poſſet defunctum, aut quando periiſſet, ſcire? cum *Magnus Johannis*, metallicus in *Korſgârden*, probe illum a ſe de facie, quippe quæ lineamenta omnia illibata ſervet, agnoſci, idque coram conſeſſu Metallico, profiteretur; ſubjungens etiam nomen, vocatum eum ajebat *Matthiam Iſraëlis*, alias, ob proceriorem paulo ſtaturam, *Matthiam Magnum* ſeu *Procerum*, qui in Boda Swerdſiöenſis paroeciæ pago editus, operam *Jonæ Petri* in *Dijkarebacken* locaſſet. Succurrit porro *Matthiam* hunc *Iſraëlis*, poſtquam anno 1670, & tempore quidem autumnali, ſolus capſula vectus deſcendiſſet in fodinam, deſideratum, dubioque procul ruina ſuffocatum

fuiſſe. Dictis fidem fecere idem affirmantes *Ericus Michaëlis* Prætor Metallicus, & *Ericus Petri* Reſtiarius. His accedebat vetula, quacum vivus adhuc *Matthias* ſponſalia contraxerat, quæ, veteris & jam reviviſcentis amoris jure, exanime corpus ſibi concedendum, aut terræ ſaltem mandandum poſtulabat. Aderant & alii plures, illum qui agnoſcerent, & narrationis hujus veritatem confirmarent.

Quadraginta novem adeo annorum ſpatio, ab anno videlicet 1670. ad annum 1719. ſub terra delituerat *Matthias* hicce *Procerus*, e qua in lucem protractum, in perpetuam rei memoriam, vetus ædes fodinæ publica excepit: in qua hodieque integer, & tam quod ad veſtimentum & lintea, quam carnem, cutem, capillum, & ungues, incorruptus, fœtoriſque omnis expers, oculis intuentium ſiſtitur; ſolo aquæ vitriolo abundantis miniſterio exſiccatus, & contra putredinis vim munitus.

Ex fideli hac certaque, quantum quidem ex loci ipſius incolis hauriri potuit, notitia haudquaquam petrificatum cadaver hoc, aut in lapidem mutatum, ſed aquæ tantum vitriolo ſcatentis beneficio induratum eſſe liquet. Quid quod vitrioli naturæ & ingenio magis nil, quam ejuſmodi petrificandi vis repugnat: quippe quod nil unquam in ſaxum convertat; tenuiſſimi vero vaporis vegeto motu omnia perrumpat, ſtringat, ac a putredine interituque tueatur.

V. *Aræometri novi descriptio & usus a D. G.* Fahrenheit, *R. S. S.*

NOTUM est liquorum gravitates specificas, duobus potissimum modis, investigari posse, librarum nempe, vel Aræometrorum ope. Ambo nonnullis difficultatibus sunt obnoxii, quominus exacte sæpe experimenta fieri possint. Difficultas prioris modi in eo præcipue versatur, quod cum libræ accuratissimè elaboratæ adhiberi debeant, earum vero tenor, in quibusdam solummodo punctis consistat, ea temporis tractu, vel etiam alio modo, facillime depraventur, eoque modo experimenta non eâ, qua par est, accuratione perfici possint. Aræometra autem accuratiora, quorum structura hucusque innotuit, hoc præcipue laborant incommodo, quod uno eodemque instrumento non omnium liquorum gravitates explorari possint, ita ut mutatis liquoribus, etiam aliud Aræometron adhibere oporteat. His consideratis, usumque Aræometri perpendens, difficultates memoratas structurâ sequentis instrumenti tolli posse animadverti.

Globulo, A, Fig. 7. satis magno, (quo major Globulus eo melior) tubi sibi oppositi C D & E F annectuntur, tubulo gracillimo E F receptaculum G additur, mediumque tubuli puncto a minutissimo, satis tamen visibili, denotatur. Extremitas altera tubuli C D globulo B prædita est, qui receptaculi loco ponderi inferiori (quo nempe instrumentum aggravatur) inservit. Distantia globuli B a centro globi A triplo major sit, quam distantia receptaculi G ab eodem centro. Instrumento ita præparato, globulus B tantâ mercurii quantitate repletur, ut si Aræometron liquori levissimo, exempli gratia, spiritui vini

vini bene dephlegmato, vel ſpiritui Terebinthinæ immergatur, illud in liquore fere uſque ad punctum a deſcendat; quo facto, tubulus prope E hermetice ſigillatur, & inſtrumentum bilance accuratiori ponderatur; eritque pondus inſtrumenti etiam ipſiſſimum liquoris ab inſtrumento deturbati pondus, utpote ſatis hydroſtatices peritis notum eſt. Si autem graviores inveſtigandi ſunt liquores, exempli gratia, aqua, lixivia, vel ſpiritus acidi, eorum gravitatis differentia invenitur, dum nempe inſtrumentum in receptaculo G tanto pondere oneratur, ut illud iterum ad punctum a ſubſidat. Hoc pondere gravitati inſtrumenti addito, illorum liquorum gravitates ſpecificæ (ſi pondera ſint minutiſſima) ſatis exacte habebuntur: & ſic de cæteris.

Dixi quod inſtrumentum in memoratis ſpiritibus fere ad punctum a ſubſidere debeat; melius enim erit, ut non perfecte liquor illud punctum attingat, & ut differentia parva ponderibus minutiſſimis adjuvetur: hoc enim modo, ſi forſan adhuc liquores leviores darentur, vel etiam ſi liquorum memoratorum gravitas a calore ſpecifice levio r redderetur, adhuc inſtrumento explorari poterunt, quod alias non ſuccederet, ſi illud perfecte ad punctum a in nominatis ſpiritibus ſubſideret.

Dum autem experimenta fiunt, cavendum eſt, ne ſuperficies, tam inſtrumenti, quam liquorum aliquâ pinguedine, vel aliis particulis heterogeneis ſint imbutæ; aliter enim experimenta nunquam ſatis accurate peragentur; ut optime Vir acutiſſimi ingenii, hujuſque inclytiſſimæ Societatis Membrum, inter diſſerendum de hoc inſtrumento monuit.

VI. *A Letter*

VI. *A Letter from Mr.* John Bonnet, Surgeon *at* Fowye *in* Cornwall, *to* Claudius Amyand, *Esq*; Serjeant-Surgeon *to his Majesty*, F. R. S. *concerning the preternatural Structure of the* Pudenda *in a Woman, describ'd in* Phil. Transact. N° 379.

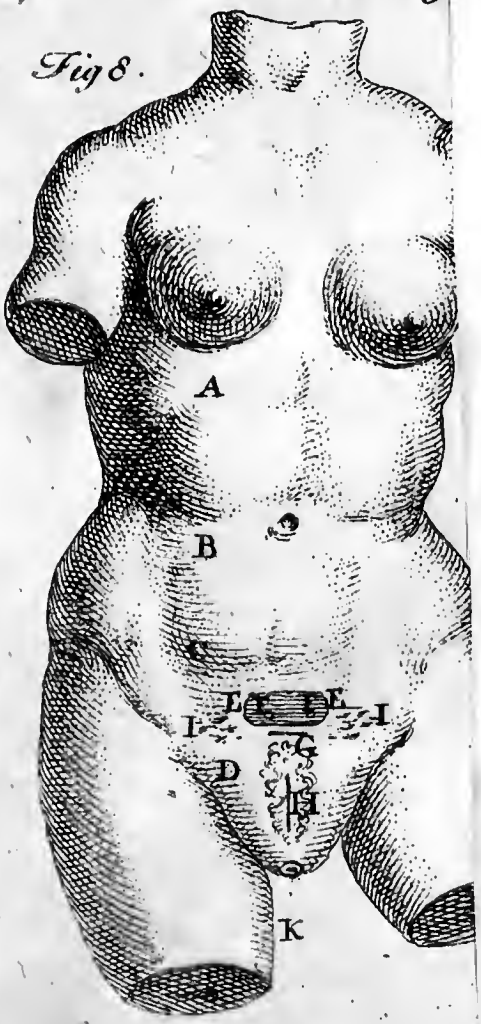
AS there appears to be some Difference in the Accounts of the remarkable Structure of the *Pudenda*, &c. of the Woman of *Lanteglass*, near *Fowye*, publish'd in the *Philosophical Transactions* N° 379. I take the Liberty of giving you a particular Account of what I observ'd, with respect to this Matter. This I am the rather embolden'd to do, as I repeatedly observ'd the wonderful Structure of the Parts, before the Birth approach'd, and before I was oblig'd to make the Incision.

This Woman was about 23 Years old when she was married, and some time after conceiv'd. As she was conscious of the preternatural Structure of the Parts, and her Mother apprehensive of the Danger that wou'd attend the Delivery under such unhappy Circumstances; they apply'd to me about the 7th Month, in order to engage my Assistance.

On viewing the *Abdomen*, I made the following Observations; There was no Sign of the *Umbilicus*; but about three Inches lower, than that is regularly plac'd in Persons naturally form'd, there was a spongy, fleshy Extuberance, nearly of the Shape and Bigness of an Hen-Egg: Not, as is said in Mr. *O* . . . .'s Account, compos'd of many Lobules invelop'd by distinct Membranes,

but

Fig 8.



but

but entirely reſembling that luxuriant Fleſh which is thrown forth in ill-digeſted Wounds, and is therefore commonly call'd proud Fleſh. This was exceeding tender, and on it ſhe cou'd not bear the leaſt Touch. On the lower Part of this Excreſence, I perceiv'd two ſmall Orifices, the one diſtant from the other about an Inch. Thro' theſe the Urine drops continually, nor is ſhe able to retain it; but by violent Efforts can make it ſpout out near a Foot. What is ſaid in Mr. *O*....'s Account of its being render'd *multis rivulis*, is certainly erroneous: The two Orifices, by which it is in deed diſcharg'd, being now very evident, and will eaſily admit a ſmall Probe.

About $\frac{1}{4}$ Inch below this Protuberance, was a tranſverſe Orifice, much reſembling the *Anus* of a Cock. Through this the *Menſtrua* regularly flow'd: By this ſhe was impregnated. 'Twas with ſome Difficulty I thruſt my Finger into this Orifice, in order to reach the *Os Tincæ*, which, however, I could by no means feel, it lay ſo deep; but I plainly felt a thick tranſverſe Membrane ſeparating this Paſſage from an Orifice, which is ſituated about two Inches below that already deſcrib'd. This lower Orifice ſeem'd to be ſituated, exactly where the *Symphyſis* of the *Oſſa Pubis* is, in Women regularly form'd, ſomewhat above the Place where the natural *Hiatus* ſhou'd have been. I cou'd but juſt enter the Tip of my Finger into this. There were a few Hairs ſcatter'd up and down irregularly about this Orifice. The *Anus* terminated as uſual, with a Sphincter, about two Inches below this lower Orifice, much more forward than uſual.

So that the upper Orifice, which may be properly call'd the Orifice of the *Vagina*, was about $\frac{1}{4}$ Inch below the umbilical Excreſcence: The lower oblong Orifice (or another Paſſage to the Womb) was about two

Inches below that of the *Vagina*: Contrary to what is ſaid in Mr. *O*....'s Account, who makes the ſuperior Orifice to be near four Inches below the Excreſcence, and the lower Orifice four Inches from the ſuperior; and then ſays, the *Anus*, which he (rightly enough) makes to uſurp the Place of the *Foſſa magna* in Women naturally conformed, was five or ſix Inches below this. According to this Proportion, the Woman muſt be really gigantick; whereas ſhe is a ſhort, little Woman: But however, Worthy Sir, you may depend upon the Account and Meaſure I have given; nor do Mr. O....'s Cuts any way repreſent the Caſe.

This Woman had no *Os*, or *Oſſa Pubis*; indeed there was an *Apophyſis* jutting out from the lower Part of each *Os Ilium*, but they were far from being join'd, as uſual, by *Synchondroſis*.

July 18. 1722. I was ſent for late at Night. I found the Woman with true Travail-Pains upon her. The Throws were exceſſive violent, and the continued Agony had almoſt quite exhauſted the poor Woman's Spirits; but the Orifice of the *Vagina* was no way ſenſibly dilated, tho' the *Anus*, thro' the Violence of the Throws, open'd extremely wide. In vain were all Endeavours to relieve her, by thruſting up the Child, and putting the Mother in a proper Poſture. Vain were her own Throws and Agonies. Convulſions now had ſeiz'd her, and Nature ſeem'd to have deny'd a longer Life to the Mother, or an Entrance into it to the Child.

I was in the utmoſt Perplexity what to do under theſe Circumſtances. On the one Hand I conſider'd, that if there was not a Paſſage made for the Child, and that by Inciſion, both Mother and Child muſt ſpeedily periſh. On the other Hand I foreſaw the Danger and Hazard of an Inciſion, and the unavoidable Cenſure of having kill'd the Woman, if ſhe ſhould die under the

Operation. At last Humanity so far prevail'd with me, as to try a doubtful Method of preserving Life, rather than none.

I told her Mother, and the other Persons in the Room, that Death was inevitable, without making the Passages wider by Incision, and so attempting a Delivery; withal apprizing them of the Uncertainty of the Event of the Operation. When they saw her (as they thought) just expiring, they deliver'd her into my Hands, to do with her what I thought fit.

I immediately thrust my Scalpel into the inferior oblong Orifice, and directly cut into the Orifice of the *Vagina*; so brought them into one: Then presently with my Scissors, snipp'd the transverse Membrane. This being done, I easily introduced my Hand, felt the Head of the Child, and with my Finger thrust into its Mouth, drew forth a Female Infant, living and well form'd, to my own Surprize and Admiration of all. Thus was this *Gordian* Knot, at last, happily cut, that cou'd not be unty'd.

Ever since she was deliver'd, in the Manner recited, she hath suffer'd a *Prolapsus uteri*, upon the least standing or walking. I propos'd to remedy this by a Suture, as is practis'd in the Case of the *Vulva* breaking into the *Anus*; but she wou'd by no means admit of it: So that she almost continually labours under a *Procidentia uteri*, and the Body of the Womb and *Vagina* are so corroded by the Acrimony of the Urine, that there are four or five Ulcers form'd upon them. Besides this Inconvenience, some of the thinner Parts of the Excrement are discharg'd at the Bottom of this large Cleft, and by introducing my Finger at the Bottom of the Cleft, I can easily thrust the Top of it thro' the *Anus*: which is an Argument that there cou'd not be 5 or 6 Inches between the oblong Orifice and the *Anus*, as is assert-

ed by Mr. O.:..., the Diſtance being ſcarce above two Inches.

Fowey, June 23,
1724.

FIGURARUM EXPLANATIO.

FIGURA Octava.

A. Epigaſtrium.
B. Regio umbilicalis.
C. Hypogaſtrium.
D. Regio pubis.
E E. Spongioſa offa.
F F. Meatus urinarii, a partu ſatis viſu faciles.
G. Orificium tranſverſale.
H. Oblongum orificium.
I. Cicatrices duæ, ex Urinæ, ut videtur, acrimonia.
K. Anus, paululum anteriùs locatus.

FIGURA Nona.

P P. Fiſſura magna quatuor pollices oblonga.
L. Procidèntia uteri.
M. Os Tincæ.

VII. *Caſus rariſſimus. Viſus objectorum partialis, communicante Domino* Abrahamo Vater, *Prof. Med.* Wittemb. *R. S. S.*

FOemina mediæ ætatis, anno ſuperiori, a retro-pulſa, per balneum, & ſubſecutam refrigerationem, Coryza, unica nocte incidit in cataractam nigram, ſeu guttam ſerenam: nam mane e ſomno experrecta, uſu oculorum orbatam ſe ſentit, nullo extrinſecus in oculis vitio apparente. Poſt uſurpata vero medicamenta varia laxantia & ſanguinem purificantia, interpoſitis ſimul veſicatoriis, una cum decenti Diæta, viſus ſenſim rediit, ita tamen ut notatu dignæ viſus viciſſitudines, ac ſingularia in eo phænomena occurrerent. Primum enim omnes ipſi obvios homines, capite quaſi truncatos vidit, ſolo corpore abſq; capite in oculos incurrente. Succeſſu temporis, ceſſante hac obſervatione, objecta quidem tota, ſed quaſi per nebulam aut per rete vidit, & hoc ceſſante, maculas tamen & flocculos oculis obverſari percepit. Tandem affectus hic in viſum partialem, quo hoc ipſo tempore adhuc laborat, degeneravit. Nunc enim, utroque oculo aperto, & verſus objectum directo, illud quidem totum conſpicit: alterutro vero eorum clauſo, pars objecti, macula aut nebula rotunda tecta, videtur, quæ macula tanto major apparet, quo longius oculus ab objecto removetur. Prout autem oculus ſive dexter, ſive ſiniſter, occluditur, ſemper quidem media objectorum pars, at diverſa, obſcuratur. Ita ſi, exempli gratia, has tres voces in charta ſcriptas (*Ego ſum cæca*) intueatur oculo ſiniſtro tantum, pupilla verſus mediam vocem *Sum* directa, hæc in viſum non incurrit, ſed tantum

tantum duæ voces *Ego* —— *cæca* conſpiciuntur. Pupilla vero verſus vocem *Ego* directa, hæc evaneſcit, ita ut tantum —— *Sum cæca* videat. Quod ſi e contrario oculum ſiniſtrum claudat, & oculo dextro inſpiciat objectum, tunc medium quidem pariter obſcuratur, ita tamen, ut tantum pars quarta viſum fugiat, tribus partibus clare conſpectis. Ita ſi, exempli gratia, oculo huic objiciat has quatuor voces, in charta ſcriptas, (*Ego opto eſſe ſana*) tunc pupilla ad medium directa, omnes voces videt, præter vocem *opto*, quæ macula itidem rotunda obſcuratur, & videt ægra ita tantum, *Ego —— eſſe ſana*. Quoniam ergo ex hoc caſu apparet, maculam illam rotundam, qua pars objecti obſcuratur, eſſe fixam quidem, ſed pro varia pupillæ directione locum mutare, dubium videtur, utrum vitium in humore cryſtallino, an in tunica retina hæreat? In tunica quidem cornea nulla obſcuritas aut macula patet. Majoris vero momenti, ac explicatu difficilius videtur antecedens phœnomenon, ubi ægra totum hominis corpus, excepto tantum capite, conſpexit. Quamobrem hunc caſum Illuſtriſſimæ Societati Regali exponendum eſſe duxi, quoniam hic viam mihi parare videtur, ut multa utilia ad viſum illuſtrandum ex eo deduci queant. Dabam Wittembergæ Saxonum D. XX. Mart. MDCCXXIV.

VIII. *Sectio Oculorum duorum Cataractâ affectorum. Epiſtola Domini* *ad* Samuelem Molyneux *Armigerum, Sereniſſimo Principi* Walliæ *a Secretis, R. S. S.*

Johannes Wright, ſenex mileſque emeritus, ſuffuſione ſeu Cataractâ utriuſque oculi laborans, viſu tandem omnino privatus eſt. Curæ ſe commiſit Domini *Caywood*, Oculiſtæ *Dublinienſis*, qui acu punctoriâ oculum utrumque perforavit ad Cryſtallinum è ſede ſuâ deprimendum. Operatione verò in oculo dextro minus feliciter ſuccedente, ſiniſtri uſum ſolummodo recuperavit. Paucis menſibus poſt operationem peractam, in hoſpitium regale militum emeritorum prope *Dublinium* receptus eſt, ubi per annos octo vel novem manſit, &, per totum iſtud tempus, viſus aciem ſatis acutam habuit, adeo per urbem ſolus ambularet, & ſodalium facies diſtinguere poſſet. Mortuus eſt tandem 5° die Aprilis 1722. ſuffocatus per caſei morſulum hærentem in gula, quam adeo arctam habuit, ut eundem mortis aditum paucis abhinc annis bis ægre evaſerit. Oculi ejus, e cranio exempti, miſſi ſunt per Noſocomii Chirurgum ad D. *Thomam Molyneux* Medicinæ Doctorem, Collegii Med. Regalis in *Hibernia* Socium, & in Academia *Dublinienſi* Medicinæ Profeſſorem. In ædibus ejus cultro Anatomico aperti ſunt, per Oculiſtam ſupradictum, neque, ut intereſſem operationi, accerſere dignatus eſt. In oculo ſiniſtro (cujus aciem recuperatam fuiſſe ſupra notavimus) ſublatâ corneâ cum portione tunicæ ſcleroticæ, ne vel minimum invenimus rudimentum pelliculæ in aqueo humore fluctuantis, aut limbo iridis adnatæ, ſicut

ſicut ſe obſervaſſe ſcribit *Wolhouſius*. Quod vero multo magis mirandum eſt, nullæ prorſus reliquiæ inventæ ſunt humoris Cryſtallini, cujus opacitati morbum adſcribit Doctor *Briſſeus*. Humor Vitreus, quoad ſitum & conſiſtentiam, nihil præternaturale oculis noſtris exhibuit; tunica choroidea & retina fuſci coloris erant tam intus quam extra. In oculo dextro eadem omnia obſervavimus; nullum nempe pelliculæ aut Cryſtallini rudimentum occurrebat. Tunica cornea rugoſa erat & flaccida, quod aqueus humor per puncturam extravaſatus, numquam renovatus ſit: adeoque iſtius oculi uſum amiſit penitus. Cryſtallini vero defectum quod attinet, res eſt prorſus inaudita, neque ab ullo Anatomico, quantum ſcio, defuiſſe obſervatus eſt, vel leviter ſuſpectus. Militem hunc Cryſtallinum habuiſſe in utroque oculo ante operationem, nullus dubito; cum vero acu a ſede ſua detruſus eſt, ligamentis ciliaribus vaſiſque, quibus nutrimentum hauſit, penitus diſruptis, eum ſenſim contabuiſſe credo, tandemque in auras diſſipatum fuiſſe. Summopere vellem hanc rem experimento ſæpius iterato confirmatam eſſe: ſuſpicor enim idem ſemper evenire ſtato tempore poſt depreſſum Cryſtallinum.

Hoc certo conſtat ex hac hiſtoria, hominem nempe abſque Cryſtallino videre poſſe, contra opinionem *Wolhouſii* Oculiſtæ *Pariſienſis*, eâque amplius confirmatur ſententia D. Doctoris *Briſſei* de Cataracta, quem conſulas quæſo ſuper hac re.

IX. *An*

IX. *An Account of a Book entituled,* Dominici Bottoni, de immani Trinacriæ Terræ Motu Idea Hiſtorico-Phyſica, in qua non ſolum Telluris concuſſiones tranſactæ recenſentur, ſed noviſſimæ Anni 1717. Meſſanæ 1718. 8°. *By* J. G. Scheuchzer, *M. D. R. S. S. Coll. M. L. Lic.*

THIS Treatiſe contains an accurate hiſtorical Account of the ſeveral violent Earth-quakes, which happen'd in the Kingdom of *Sicily*, in the Years 1693, 1694, and 1717. interſpers'd with ſome philoſophical Digreſſions concerning the Cauſes and Effects of Earth-quakes in general. I ſhall ſomething enlarge upon the former, as being the more material, as well as the more curious Part of the Book; avoiding, however, with all poſſible Care, a tedious and unneceſſary Repetition of what hath been already mention'd in another Account of thoſe two dreadful Succuſſions, which happen'd the 9th and 10th of *January*, 1693. written by *Vincentius Bonajutus*, a *Sicilian* Nobleman. This Account was communicated to the *Royal Society* by the late learned *Marcellus Malpighius*, and is inſerted in the *Philoſ. Tranſact.* N° 207. p. 2.

The Summer Seaſon, in 1692. was exceedingly hot and tempeſtuous, with frequent Thunders, Lightnings and Rains. About the middle of *September* fell ſuch profuſe Showers, that all the Rivers and Torrents increaſed to ſuch a Degree, as to overflow their Banks in ſeveral Places, and cover large Pieces of Ground with Water. This, join'd to the continual blowing of South-

erly Winds, during the *Autumn*, put the Inhabitants under great Apprehenſion of future Miſchiefs. And indeed, the diſaſtrous Fate, which befel *Sicily* about the Beginning of 1693, too manifeſtly prov'd, that this ominous Fear was no way groundleſs. For on *Friday* the 9th of *January*, about the 5th Hour (according to the *Italian* Way of counting) after a warm, ſerene and calm Day, the Earth began trembling all of a ſudden, chiefly about *Catania*, and in ſome neighbouring Places, for the Time required to ſay the Lord's Prayer. This firſt Shake was accompany'd, as generally happens in Earthquakes, by a hollow, thundering Noiſe, and and ſucceeded by another ſmall Trembling, obſerv'd only by ſome few People on *Saturday*, early in the Morning. Theſe two Succuſſions, though violent enough, were but a Prelude of the third, which happen'd the 11th of the ſame Month, by 4 of the Clock in the Afternoon, of which the Apprehenſion was ſo much the greater, becauſe all the 10th and 11th, between the firſt and third Shake, the Air was more than uſual dark and cloudy. This laſt was ſtupendous beyond human Imagination, and it would be a Task too difficult for even the ableſt Pen, to deſcribe all its dreadful Effects; the violent toſſing and dancing of the Earth; the hollow, thundering Noiſe threatening the whole Iſland with its entire Diſſolution; the fiery Eruption of the burning *Ætna* throwing out a prodigious Quantity of Flames, Stones and Aſhes; the Terror and Confuſion of the diſtracted Inhabitants running up and down the Streets, uncertain where to provide for their Safety, or how to eſcape the Fury of all the raging Elements, which ſeemed to have conſpired their Ruin. There was ſcarce one Place all over the Kingdom left without ſome particular Misfortune, *Catania*, *Syracuſa*, *Agoſta*, *Meſſina*, *Noto*, *Raguſa*, *Leontini*, *Ibla Chiarumonte*,

Chiarumonte, *Carleontino*, *Caltagirone*, *Soccino*, *Francofonte*, *Bontello*, *Militello*, *Occhiali*, *Aydono*, *Motica Maſcali*, were all, if not entirely deſtroy'd, at leaſt miſerably ſhatter'd, many Churches and ſtately Buildings, up and down the Country, violently thrown down, and above 60000 Inhabitants buried under the Ruins, of which about 16000 periſhed only at *Catania*.

In many Places the Earth gap'd prodigiouſly. Such an Opening was obſerved near *Meſſina*, in the very Bottom of the Sea; another near a Village call'd *Botto d'Aceto*, 250 Paces long, and near 8 Palms broad; another at *Caltaniſeta*, near the Jeſuits-College, 2000 Paces long and 2 Palms broad; another, at the Top of a Hill near *Leontini*, full wide enough to hold a Man; another upon the Road between *Catania* and *Leontini*, which ſwallow'd up ſome Mule-keepers, then, to their great Misfortune, happening to travel that way, along with their Mules and whole Baggage, that not the leaſt Foot-ſtep of them remain'd. Silently to paſs over a great many more, but of leſs Conſideration.

Out of all theſe Openings ſprung forth a great Quantity of Water, which drown'd the neighbouring Places. This Water was in ſome Places hot, with a ſtrong ſulphureous Smell, which laſted, even after the Earthquakes were over, and induc'd ſome of the Inhabitants, not without Succeſs, to make uſe of it in curing of Ulcers, and other cutaneous Diſeaſes, for which chiefly a hot Well near *Lazaretto* became very famous. Out of ſome of theſe Gapings of the Earth iſſued a thick Stench and Smoke, very troubleſome to all the Neighbourhood. This happen'd, amongſt other Places, upon a Mountain called *S. Theodor*, as alſo near *Mena*. Near *Agoſta* it was preceded by a ſulphureous red Flame.

Juſt at the Time of the ſecond Shock, the Sea retir'd from the Land all along the Coaſts, leaving its Bottom dry'd up for a conſiderable Diſtance, and within few Minutes return'd again with great Fury, and overflow'd the Shores. By this Accident the *Malteſe* Gallies, lying at Anchor in the Harbour of *Agoſta*, were in danger of being loſt; for the Sea ſunk down all of a ſudden, ſo that they came to ſit almoſt upon the Ground, and immediately after bubbled and ſwell'd up again with ſo great an Emotion, that they run the Hazard of having their Cables broke, and being driven away.

And it ſeemed that the Earth itſelf was in ſome Places conſiderably lower'd, and the Tops of the Mountains depreſs'd. Of this they had a remarkable Inſtance at *Paterniome*. The Hills, between this City and the Shore, hinder'd it from having any View of the Sea, which ſince the Earthquake diſcovers itſelf towards the Eaſt very plainly.

In other Places the Earth actually ſunk down, and inſtead of it appear'd great Lakes, ſome of which were large enough to become navigable. By the breaking forth of ſuch a Lake between *Noto* and *Syracuſa*, a large Piece of Ground was tranſported for about 50 Paces, where it now ſtands as firm, as if it had always ſtood there.

The Looſening and Fall of two great Rocks between *Terula* and *Caſſero* is already ſufficiently deſcrib'd in the abovementioned Account of Signor *Bonajuti*: Such Looſenings and throwing down of great Rocks happen'd every where up and down in the Country, to the great Terror of the neighbouring People. The ſame was alſo obſerv'd, according to *Kircher*, and ſeveral other Authors, in ſome Earthquakes in the Kingdom of *Naples*. Two very high Rocks near *Ibla*, with all the Trees growing thereon, were by the Violence of the Fall, quite

quite inverted, that the Top came to ſtand upon the Ground.

About two Miles off from the City of *Mena*, lies a Lake full of a bituminous, ſulphureous and combuſtible Matter, formerly famous under the Title of *Lacus Palicorum*. Upon this Lake there was obſerv'd, the very Day of the Earthquake, about a Quarter of an Hour before the ſecond Shock, a great red Flame, like a fiery Column of above three Yards perpendicular Height, which, during the Earthquake itſelf, on a ſudden diſappear'd.

At *Agoſta*, the Misfortune was ſo much the greater, becauſe, beſides all the dreadful Effects of the Earthquake itſelf, the Powder-Magazine, in the Caſtle, by ſome ill Accident, and perhaps by the violent Fall over one another of the Stones of ſome ruined Buildings (or by the breaking out of ſome ſubterraneous Flame) unluckily took Fire, by which the whole Caſtle was blown up with ſuch a Fury, that ſome of the Stones were carry'd as far as the Iſland *Thapſus*, which is near 8 *Italian* Miles diſtant from *Agoſta*. By this Accident periſh'd 1840 Inhabitants.

There remains ſtill one thing worth obſerving, and that is the very Riſe and Progreſs of this terrible Succuſſion. It aroſe in the South, and proceeded from thence towards the North. For it was firſt obſerved in the Iſland of *Malta*; then in the Southern Parts of *Sicily*; and laſt, always with ſome Difference as to the Time, in the Northern Parts of the ſame Kingdom. But the Shakes were leſs violent, the more it approach'd to the North. For the reſt, it extended itſelf ſo far, that not only the Iſland of *Malta*, but alſo *Calabria*, and ſome Parts in the Kingdom of *Naples*, participated of its Fury.

Nor

Nor was this the End of all the Miſeries which befel this noble Kingdom: for the Earth continued trembling for ſeveral Months after, during the whole Year of 1693. In the remaining Part of *January*, and from that Time to the Beginning of the Summer, the Shakes came ſtrong and thick, with hollow terrible Noiſes, and frequent Eruptions of *Ætna*. The moſt conſiderable were obſerv'd *February* the 15th in the Morning; *March* the 1ſt; *March* the 18th, by one of the Clock; *May* the 10th; *May* the 26th in the Morning. Towards the End of the Summer, the Shakes were obſerv'd to loſe a great deal of their Force, and *Ætna* to throw out Flames and Aſhes in leſs Quantity, when on the 4th of *September*, this ignivomous Mountain, having been ſome Days before very quiet, trembled and crack'd all of a ſudden, with ſo loud and thundering a Noiſe, as if ſome Thouſands of Guns were fir'd all at once. This was ſucceeded by a new Opening, about 1000 Paces diſtant from the old Mouth, out of which immediately iſſu'd a thick Stench and Smoke, follow'd by a great Flame. The ſame Mountain open'd itſelf in two other Places, with the like Noiſe, and Eruption of Smoke and Fire, the 25th of *September*, 1693, and the 1ſt of *April*, 1694. Since that Time the Shakes became viſibly weaker and weaker, and at laſt entirely waſted.

As to the Earthquake which happen'd *April* the 22d, 1717. early in the Morning, and of which the Author hath given a ſhort Account by way of Appendix; I ſhall not inſiſt upon its Effect, being much the ſame with the former, though far inferior as to the Degree of Violence.

Thus far, what I could gather out of this Treatiſe, relating to the Hiſtory of the *Sicilian* Earthquakes in 1693 and 1717. I proceed now, with all poſſible Brevity, to run over the ſeveral Obſervations, which our Author maketh

maketh about the Cauſes and Effects of Earthquakes in general: He obſerveth,

That ſuch Countries as abound much with Foſſil-coals, as *England*, ſome Parts of *Germany*, and even *Sicily* itſelf, are more ſubject to Tremblings of the Earth, than any other.

That peſtilential and other Diſeaſes, which frequently follow great Earthquakes, are rather to be aſcrib'd to noxious and infected mineral *Effluvia*, which upon ſuch an Occaſion iſſue out of the Earth through the ſeveral Openings, than to the Putrefaction of dead Bodies, which periſh'd in the Earthquake, and were buried under the Ruins of deſtroy'd Buildings.

That amongſt the Minerals hidden in the Entrails of the Earth ſome are combuſtible, ſome not; that of the combuſtible Ones, as Sulphur, Bitumen, Alum, Vitriol, Salt-Petre, *&c.* ſome take Fire quicker than others; that others, as Coals, are with greater Difficulty to be inflamed, but keep the Fire ſo much the longer; that the Heat and ſulphureous Smell of the Waters, which broke firſt through the Openings of the Earth, probably depends upon the Mixture and Fermentation of ſome of theſe Minerals.

That *Baccius*, *Kircher*, and others, which draw the Origin of the abovementioned ſulphureous Lake near *Mena*, and of ſeveral others in other Places from the ſubterraneous Cavities of the Mountain *Ætna*, are much in the wrong, there being not the leaſt Neceſſity of its being derived ſo far, ſince the Country every where abounds with combuſtible Minerals, which rather ſeem to entertain and nouriſh the fiery Eruptions of *Ætna*, than to be occaſion'd by them.

That the Sea's retiring from the Shore, and ſinking down, is the leſs to be wonder'd at, ſince there are many Examples in Hiſtories of whole Towns and Iſlands ſwal-

low'd

low'd up by it. This Misfortune befel, according to *Pliny*, *Tyndarida*, formerly a famous City in *Sicily*.

That in all probability *Sicily* itself, which, by the Testimonies of *Diodorus Siculus*, *Strabo*, *Pomponius Mela*, *Plinius*, *Solinus*, *Justinus*, *Seneca*, and several others, both antient and modern Authors, appears to have been formerly annex'd to the Continent of *Italy*, by a small Neck of Land between the Cape of *Messina* and the lower Parts of *Calabria*, was separated from it by some violent Succussion of the Earth.

That the ignivomous *Ætna*, far from being the Cause, as some imagine, of the Earthquakes, which so often desolate the Kingdom of *Sicily*, seems rather to have been created by the allwise Providence, in order to secure this Island by its fiery Eruptions from more frequent Misfortunes.

That contrary to what *Aristotle* and some others assert, solid and rocky Places receive infinitely more Damage by Earthquakes, than loose, sandy Grounds. This appears by the Ruins of *Catania*, *Leontini*, *Agosta*, *Syracusa*, and *Noto*, which were all built upon a solid stony Ground, whereas *Messina*, though every where undermin'd by subterranean Cavities, was abundantly less shaken.

The remaining Part of the Book is employ'd about examining the Opinions of all the antient and modern Philosophers about the Causes of Earthquakes, and establishing the Author's own, which is, that the Earth is shaken by the Violence of subterraneous Fires, occasion'd by the Fermentation of the combustible Minerals hid in its Entrails, and that the Effects of the Earthquakes may in all Regards be compar'd to the Effects of Mines. By the way, he observeth, that the Causes of Thunder, Lightning and Winds may be deriv'd from the same Principle.

F I N I S.

Philosoph: Transact: N.° 385.

I.

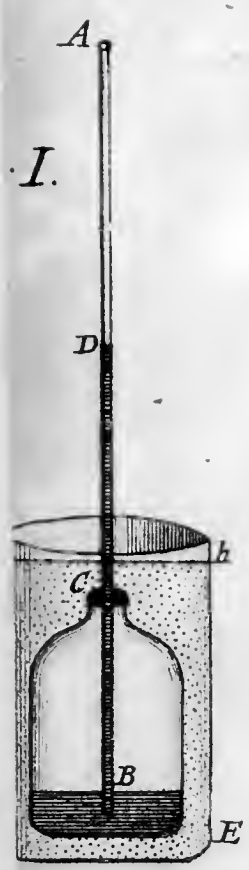
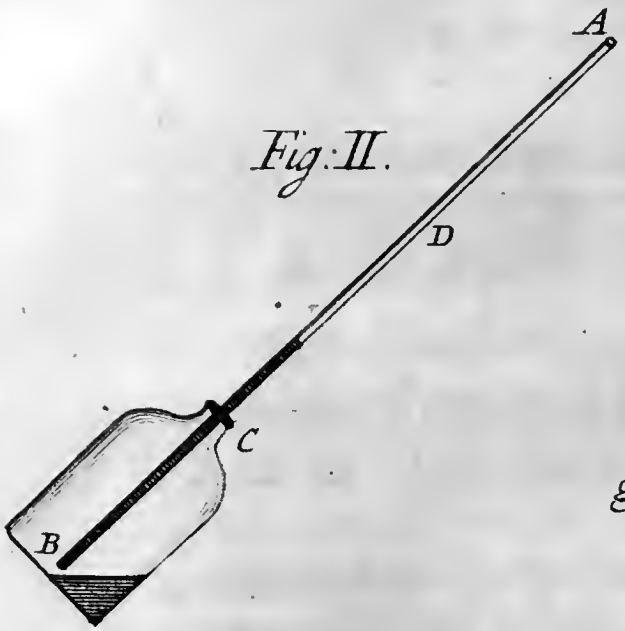


Fig: II.



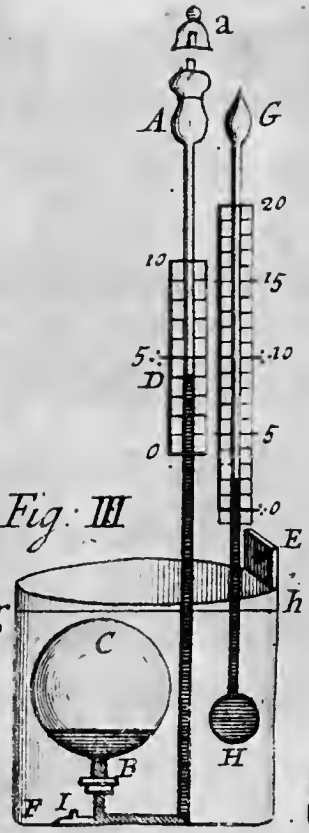


Fig: III

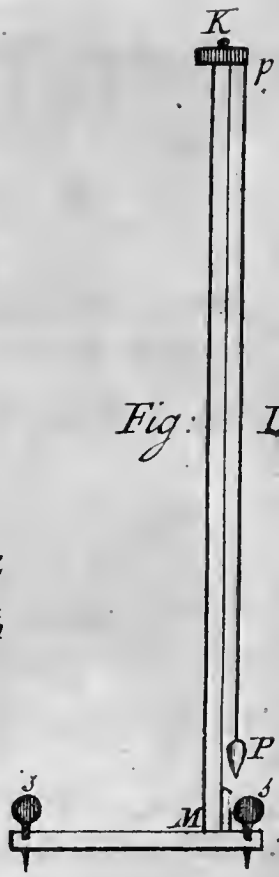
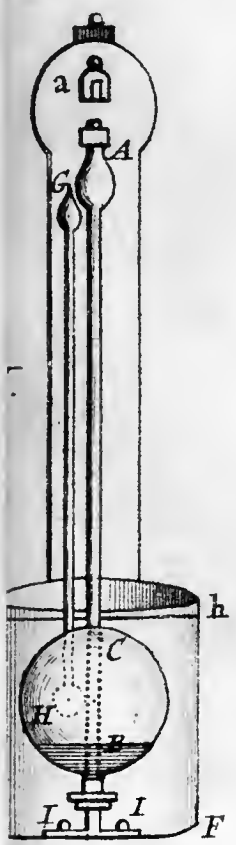


Fig: I



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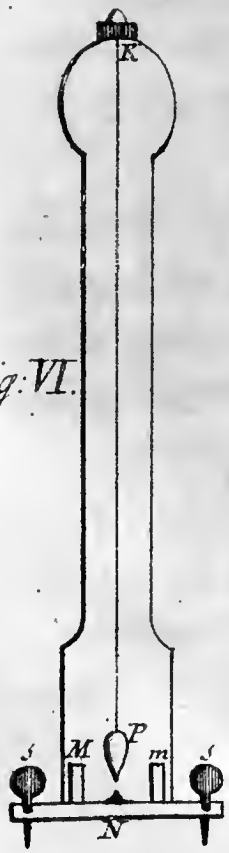
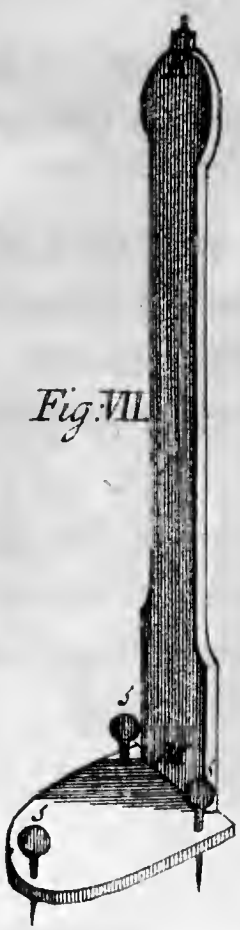


Fig: VII.



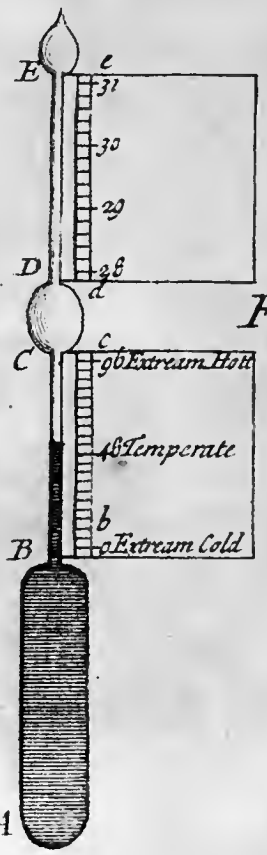


Fig: I

Numb. 385.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *October*, *November*, and *December*, 1724.

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VII. *Observatio Lunaris Eclipsis habita* Ulyssipone *in Palatio Regio Die* 1. Novembris 1724. *A PP.* Joanne Baptista Carbone, & Dominico Capasso, *Soc. Jesu. Communicante Excellentissimo Domino, Dno. de* Galvaon, *Commendatore* Villæ Meam & Francæ, *Equ. Ord. Christ. Legato Sereniss. Reg.* Portugall. *ad Sereniss. Regem* Magn. Britan. *S. R. S.*

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XI. Ambergris *found in* Whales. *Communicated by Dr.* Boylston *of* Boston *in* New-England.

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I. *An Account of the* Scarabæus Galeatus Pulſator, *or the* Death Watch, *by Mr.* Hugh Stackhouſe. *Communicated by* John Harwood, *L. L. D. R. S. S.*

AS I ſtood thoughtful in my Study, on the 16th of *May*, 1724, I happen'd to hear, what is commonly call'd the *Death Watch*, very near, as I thought, to the Place where I then ſtood: This brought into my Mind, what I had formerly read in the *Philoſophical Tranſactions*, for the Month of *October*, 1698, of the *Scarabæus Galeatus Pulſator*, found, and deſcrib'd, by Mr. *Benjamin Allen*. While I was thinking upon the Account there given, I fancied I heard the Beatings ſomewhat ſtronger than before, which encourag'd me to ſearch after it; I thereupon removed my Station to another Part of the Room, and ſoon diſcover'd, that I had paſs'd the Place where the Beatings were; upon which, I concluded, that it lay ſomewhere betwixt my two Stations; I therefore remov'd, and placed myſelf about the middle Diſtance; where, though the Beatings did not repeat ſo frequently as before, yet when they did, I found I was ſtill nearer 'em. While I ſtood in this Place, I fancied 'twas over my Head, toward the Ceiling of the Room; upon this, I got upon a Chair, that had a Sedge Bottom, and ſtood to the Wall juſt before me; but ſtanding there for ſome time, I could hear no Beatings at all. I then ſtept down from the Chair, and after I had ſtood ſtill for ſome time, the

Beatings began again; this made me conjecture, that they might be about the Chair, and that my ſtanding upon it might prevent them. Upon inclining my Head down toward the Chair, I found I was ſtill nearer and nearer to the Sound, and, at laſt, placing my Ear cloſe to the Sedge Bottom of the Chair, I diſcover'd that it was ſtill further towards the Ground: Upon that, I turn'd the Chair's Bottom upwards, but heard no Beatings for a conſiderable time after; at length, they began again, and, as I caſt a diligent Eye over the Bottom of the Chair, I happen'd, at laſt, to hit luckily upon the Place where the Inſect was beating; ſo that it diſcover'd itſelf to me by its own Pulſations. I was very much pleas'd with the Diſcovery, and not only ſtood viewing it beat for ſome time, but alſo afterwards call'd up others to ſee it beat, which they did, and that, not without Admiration. The Manner of its Beating was thus.

It lifted up itſelf upon its hinder Legs, and ſomewhat extending, or rather inclining its Neck, beat down its Face upon the Sedge, with great Force and Agility; the Sedge, upon which I found it, was bared of its outward Coat, for about the Length of half an Inch; the Inſect ſtood upon the inward bulbous Part, and beat upon the outward Coat, as if it had been working it off as it went; the Impreſſions of its Strokes were very viſible, the Coat of the Sedge being depreſs'd, where it had beaten, for about the Compaſs of a ſilver Penny; whether it beat for Exerciſe, or Food ſake, I cannot certainly ſay; but very probably it might be for the latter; and I am rather inclin'd to think ſo, becauſe there were more than one ſuch Places upon the Sedge, where it had been at work, and where, 'tis likely, it might have been a Sojourner for ſome Days.

As to what the ingenious Mr. *Derham* of *Upminſter* has obſerv'd, that the Beatings are a ſort of Prelude for Copulation, I could not diſcover that this Beetle had any other of the ſame Species near it; and therefore I am inclin'd to think, that it beat for the Preparation of its Food, at this time, at leaſt, whatſoever it might do at other times, for Pleaſure. The Deſcription Mr. *Allen* gives of the Inſect, as referr'd to above, foraſmuch as I can find by this (which I took from the Chair where I found it, into a Box) is very true: 'Tis about a Quarter of an Inch in length, of a dark, dirty Colour, having a broad *Galea*, or Helmet, over its Head; under which, when quiet, it draws up its Head; ſo that this *Galea* is, when this Inſect reſts, a very notable and providential Defence againſt ſuch Falls, as are frequent in rotten and decay'd Places, in which, it ſhould ſeem, this Inſect is very much converſant. The ſecond Day after I took it, I open'd the Box wherein it was, and ſet it in the Sun: The Inſect was ſoon very brisk, and crept backwards and forwards along the Pieces of Sedge, and rotten Wood, that I had put with it into the Box, till at length getting to the End of one of the Pieces, it immediately ſtruck out its Wings, and was juſt going to take its Farewell; but having the Lid of the Box ready in my Hand, I ſhaded it over, and it ſoon drew in its Wings, and was very quiet. I could not before perceive, though I had the Uſe of a tolerable good Glaſs, any the leaſt Sign of a Fiſſure upon its Back; and, for that Reaſon, did greatly queſtion, whether it had any Wings or not, till I ſet it, as above, in the Sun. The Head of the Inſect appears to be of a very fine Contexture, as it is ſeen, when it creeps about, and ſtretcheth it forward; but when 'tis drawn up under its *Galea*, it ſeems to be cover'd with a Membrane thick ſet with fine Hairs.

It lived with me about a Fortnight, but I could never perceive that it beat, after it was confin'd in the Box.

II. *Obſervations of the Eclipſes of the firſt* Satellite *of* Jupiter, *communicated by his Excellency* William Burnet, *Eſq; Governor of* New York, *F. R. S.*

Theſe Obſervations were made in the Fort of *New York*, for determining the Longitude of that Place by us,

William Burnet, *Cadwallader Colden*, *James Alexander*, and calculated by *Cadwallader Colden*.

The Latitude of the Fort, was formerly determin'd to be 40° 40′.

Auguſt the 9th, 1723.

	H.	′	″
TIME of Emerſion at *London*, according to Mr. *Pound*'s Tables, reduced to apparent Time -	16	09	25
Time as it was ſeen at *New York* -	11	10	43
Difference of Meridians =	4	58	42

I neglected to write down the Altitudes which were taken of the Sun, for correcting the Clock.

Auguſt

Auguſt the 25th.

Altitude of the Sun's Upper Limb.		Time by the Clock.	Time by Calculat.
	° ′ ″	H. ′ ″	H. ′ ″
Sun's Declin. 6° 55′	49 30 00	10 17 52	10 17 28
	51 13 30	10 33 10	10 32 8
Aug. 26. Sun's Declin. 6° 33′	46 24 00	9 57 40	9 56 25
	47 50 00	10 8 22	10 6 57

	H.	′	″
Time of Emerſion by Mr. *Pound*'s Tables	14	31	25
Equation of Time to be added	00	01	22
	14	32	47
Time obſerv'd by the Clock	09	35	14
The ſame corrected	09	34	14
The Difference of Meridians	04	58	33

This I look upon as the moſt diſtinct and beſt Obſervation.

September the 10th.

Altitude of the Sun's Upper Limb,		Time by the Clock.	Time by Calculat.
	° ′	H. ′ ″	H. ′ ″
Sun's Declin. 49′	33 21	09 01 00	09 00 16
	34 06	09 06 01	09 04 49
Sept. 17th Sun's Declin. 1° 54′	17 17	04 21 40	04 21 44
	15 15	04 33 05	04 32 47

Time

	H.	′	″
Time of Emerſion by the Clock *September* 10th	08	00	10
Time of Emerſion by Mr. *Pound*'s Tables	12	50	36
Equation of Time to be added	00	06	54
	12	57	30
Corrected Time at *New York*	07	59	08
Difference of Meridians	04	58	22

June 26th, 1724.

Altitude of the Sun's Upper Limb.	o ′	Time by the Clock. H. ′ ″	Time by Calculat. H. ′ ″
June 20th, Sun's Declin. 23 . 7	56 44	09 48 03	09 43 37
	60 27	10 09 40	10 05 05
June 27th. Sun's Declin. 22 . 26.	63 31	10 27 43	10 27 05
	65 21	10 40 00	10 39 27

	H.	′	″
June the 26th, Time of Immerſion by the Clock	11	41	12
Time of Immerſion by Mr. *Pound*'s Tables	16	43	02
Equation of Time to be ſubtracted	00	04	26
	16	38	36
Time at *New York* corrected	11	40	15
Difference of Meridians	04	58	21

The Mean of all theſe Obſervations is 4^h 58′ 30″ which agrees to 3″ with that Obſervation, which I thought the moſt exact, and therefore the Longitude of *New York*, is nearly 74° 57′ 30″ Weſt from *London*.

The

The Variation of the Magnetick Needle was obſerv'd, this Year, to be 7° 20′ Weſt. *Philip Wells*, Surveyor General of this Province, in the Year 1686, obſerv'd it to be 8° 45′; by which, it appears to decreaſe about 1° 25′ in 38 Years, or a little more than two Minutes in a Year.

III. *A New Contrivance for taking Levels, by the Reverend* John Theophilus Deſaguliers, *L. L. D. R. S. S.*

THAT the Air Thermometer is alſo a Barometer, has been obſerv'd long ago; and, becauſe the Liquor in it will riſe and fall, as well by the Change of the Weight of the Air, as by the Air's Rarefaction by Heat and Cold, this Inſtrument has no longer been made uſe of as a Thermometer, and, in its ſtead, Spirit of Wine Thermometers, hermetically ſeal'd, have been us'd ever ſince.

But, becauſe the Errors of the Air Thermometer (or its Difference from the Spirit Thermometer) depend only upon the Change of the Weight of the Atmoſphere from what it was, when the two Thermometers were ſet at the ſame Degree of their reſpective Scales; the late Dr. *Hook* contriv'd an Inſtrument, that he call'd a Marine Barometer, made of a Combination of the two abovemention'd Thermometers; in ſuch Manner, that a third Scale being made uſe of, to obſerve the Difference of the two Thermometers, thereby the Change of the Air's Gravity, and conſequently Storms, Rains, and fair Weather, might be foretold at Sea, where the Quickſilver Barometer becomes uſeleſs by the ſhaking of the Ship.

Dr. *Halley*, ſome Years ago, publiſh'd two Tables to ſhew, how much the *Mercury* in the Barometer wou'd ſubſide, when the Inſtrument is carried up to determinate Heights, above the Level of the Place where the firſt Obſervation was made; but as he makes only one Tenth of an Inch of Fall of *Mercury*, to correſpond with an Height of 90 Feet (which Height is rather of the leaſt) it is evident, that only very high Hills and Mountains can have their Heights determin'd by this Method. The ſame learned Profeſſor has lately, in the *Philoſophical Tranſactions*, propoſed Mr. *Patrick*'s pendent Barometer for taking the Level of diſtant Places, becauſe the *Mercury*, in the Tube of the ſaid Barometer, does ſometimes riſe and fall a Foot, or a Foot and an Half; if therefore the Motion of the *Mercury* in this Barometer, be five times more ſenſible than in the common one, a Tenth of an Inch of Fall of the *Mercury*, will anſwer to an Height of 18 Feet; and therefore ſuch an Inſtrument might be of Uſe in taking the Levels of diſtant Places. But I know by many Experiments, that this won't anſwer in Practice; becauſe as the Tube of ſuch a Barometer is of a very ſmall Bore, the Attraction of Coheſion, whereby the *Mercury* is apt to adhere to the Tube, will diſturb the Motion of the *Mercury* caus'd by the different Preſſure of the Atmosphere; ſo that ſetting up this Barometer ſeveral Times ſucceſſively in the ſame Place, it will often differ a Tenth of an Inch, or more; and if it be ſhaken, as is commonly done to ſet it right, the *Mercury* will ſometimes part, and a Drop of it fall from the reſt; ſo that it is leſs to be depended upon for this Uſe, than the common Barometer.

Mr. *Stephen Gray* has often made a very ſenſible Barometer in the following Manner. Into a Bottle C B, (Fig. I.) he fixes a Tube A B, of a very ſmall Bore, o-

pen

pen at both Ends, and cemented tight to the Neck of the Bottle at C; then having warm'd the Bottle with the Hand to drive ſome of the Air out of it, he immerges the End A into Water, ting'd with *Cochineal*; ſo that as the Air cools in the Bottle C B, ſome of the red Water is forc'd into the Bottle; then ſetting the Bottle upright again, as in the Figure, the Liquor in the Bottle will ſtand at B, (above the End of the Tube) and that in the Tube at D; but if it ſhou'd ſtand higher or lower than D, it may be brought to that Place by ſucking or blowing at A. The Inſtrument, thus prepar'd, if it be firſt ſet on the Ground, and a ſpringing Ring of fine Wire ſlipp'd on the Tube down to D, by Way of Index, and then ſet upon any Table, or other Place, ſcarce a Yard higher, one may obſerve that the Liquor is riſen ſenſibly. I have ſeen it riſe a Quarter of an Inch, when the Bottle was ſet but a Yard higher than where it ſtood before; ſo that the Column of Atmoſphere, that preſs'd down the Tube, whilſt the Machine was on the Ground, being ſhorten'd only three Feet, was ſo overbalanc'd by the Expanſion of the Air in the Bottle at B, that the Liquor roſe a Tenth of an Inch above D. There is, indeed, a great Uncertainty in this Inſtrument; for ſince it is a Thermometer, as well as a Barometer, the Warmth of the Hand that touches it, or even comes near it, will make it riſe, if the Air in the Bottle was cold before. Mr. *Gray* therefore contriv'd to put the Bottle C B, into the Veſſel F E, which he fill'd with Sand; that in raiſing the Inſtrument, and moving it up and down, the Air in C B might continue in the ſame State, and the Machine be only a Barometer during the Experiment.

This ſeems to bid fair for an Inſtrument, whereby the different Levels of Places may be taken; but upon a nice Examination, it will be liable to Error. For, in the firſt Place, tho' Sand is not alter'd in its Heat or Cold ſuddenly; yet in two or three Hours, as it is carried into a warmer or a colder Place, it will become hotter or colder, and the leaſt Degree of Heat or Cold, communicated to the Air C B, will alter the Height of the Liquor at D, when the Inſtrument is made ſo ſenſible as I have mention'd. Then if, in carrying the Inſtrument, it ſhould be accidentally inclin'd (as in Fig. II.) ſo that the Liquor in the Bottle ſhould not cover the Bottom of the Tube at B, ſome Liquor may fall out of the Tube at B, or ſome Air may get into it: each of which Accidents, will quite ſpoil the Experiment. But if this Machine be made portable, without any Inconveniency, and be ſecur'd againſt the Action of Heat and Cold (or, which is the ſame, if the Alterations by Heat and Cold be exactly allow'd for) it will be of very great Uſe and Certainty, in taking the Levels of diſtant Places, provided they be not ſo far diſtant from each other, that it requires above ſix Hours Time to carry the Inſtrument from one Place to another; nay, very diſtant Places, even at two or three Days Journey from one another, may be taken tolerably well with two Inſtruments, nicely adjuſted to each other, if they be taken notice of by two Obſervers at the ſame Hour, in fair Weather, and when there is no Wind.

Now ſuch an Inſtrument, I hope, I have contriv'd, whereby the Difference of Level of two Places, which could not be taken in leſs than four or five Days with the beſt Teleſcope Levels, may be taken in as few Hours.

To the Ball C (Fig. III.) is join'd a recurve Tube B A of a very fine Bore, with a ſmall Bubble at Top at A, whoſe

whoſe upper Part is open. It is evident from the Make of this Inſtrument, that if it be inclin'd in carrying, no Prejudice will be done to the Liquor, which will always be right, both in the Ball and the Tube, when the Inſtrument is ſet upright. If by Heat, the Air at C be ſo expanded, as to drive the Liquor to the Top of the Tube, the Cavity A will receive the Liquor, which will come down again and ſettle at D, or near it, according to the Level of the Place where the Inſtrument is, as ſoon as the Air at C returns to the ſame Tenor in reſpect to Heat and Cold. To preſerve the ſame Degree of Heat, when the different Obſervations are made, the Machine is fix'd in a Tin Veſſel F E, fill'd with Water up to *g h*, above the Ball; and a very ſenſible Thermometer has alſo its Ball under Water, that one may obſerve the Liquor at D in each Experiment, when the Thermometer ſtands at the ſame Height as before. The Water is pour'd out, when the Inſtrument is carried, which one may do conveniently by means of the wooden Frame of Fig. IV. which is ſet upright, by means of three Screws, ſuch as *s*, and a Line and Plummet *p* P. The back Part of the wooden Frame is repreſented by Fig. VI. where, from the Piece at Top K, hangs the Plummet P, over a Braſs Point at N: M *m* are Brackets to make the upright Board K N continue at Right Angles with the horizontal One at N. The VIIth Figure does likewiſe repreſent the wooden Frame and Screws. The Vth Figure repreſents the Machine ſeen in Front, ſuppoſing the Forepart of the Tin Veſſel tranſparent. And here the Braſs Socket of the recurve Tube, into which the Ball is ſcrew'd, has two Wings at H. fix'd to the Bottom, that the Ball may not break the Tube by its Endeavour to emerge, when the Water is pour'd in as high as *g h*.

After I had contriv'd the Machine, as above mentioned, I conſider'd, that as the Tube is of a very ſmall Bore, if the Liquor ſhou'd riſe into the Ball A, in carrying the Inſtrument from one Place to another, ſome of it wou'd ſtick to the Sides of the Ball A, and that upon its Deſcent in making the Experiment, ſo much might be left behind, that the Liquor wou'd not be high enough at D, to ſhew the Difference of Level; therefore, to prevent that Inconveniency, I have contriv'd a blank Screw to ſhut up the Hole at A (Fig. IV. and V.) as ſoon as one Experiment is made, that in carrying the Engine, the Air in A may balance that in C, ſo that the Liquor ſhall not run up and down the Tube, whatever Heat and Cold may act upon the Inſtrument, in going from one Place to another.

Now, becauſe one Experiment being made in the Morning, the Water may be ſo cold, that when a ſecond Experiment is made at Noon, the Water cannot be brought to the ſame Degree of Cold that it had in the Morning; therefore in making the firſt Experiment, warm Water muſt be mix'd with the Cold; and when the Water has ſtood ſome time, before it comes to be as cold, as it is likely to be at the warmeſt Part of that Day; obſerve, and ſet down the Degree of the Thermometer, at which the Spirit ſtands; and likewiſe the Degree of the Water in the Barometer at D; then ſcrew on the Cap at A, pour out the Water, and carry the Inſtrument to the Place whoſe Level you wou'd know; there pour in your Water, and when the Thermometer is come to the ſame Degree as before, open the Screw at Top, and obſerve the Liquor in the Barometer.

My Scale, for the Barometer, is ten Inches long, and divided into Tenths, ſo that ſuch an Inſtrument will ſerve for any Heights not exceeding ten Feet, each Tenth

Tenth of an Inch anſwering to a Foot of Height. *NB.* I have not made any Allowance for the Decreaſe of Denſity in the Air, becauſe I don't propoſe this Machine for meaſuring Mountains (tho' with proper Allowance for the decreaſing Denſity of the Air, it will do very well) but for Heights to be known in Gardens, Plantations, and the Conduct of Water, where an Experiment, that anſwers to two or three Foot in a Diſtance of twenty Miles, will render this a very uſeful Inſtrument.

IV. *Inteſtinum Parturiens, ſeu Caſus oppido rarus de cranio & coſtis Fœtus per anum ejectis, deſcriptus a* Johanne Lindeſtolpe, *M. D. & Reg. Colleg. Medic. Stockholmenſis Aſſeſſore. Ex Actis Literariis Sueciæ, An. 1723. Trimeſtr. 1.*

JOhannes *Heſſelius*, M. D. & Weſtro-Gothiæ Medicus provincialis, Vir ſane doctiſſimus, & Hiſtoriæ Naturalis, ut Pliniana utar phraſi, peculiariter curioſus, ſcripſit ad me Scaris d. 20 Sept. præterlapſi anni, tranſmiſitque literas *D. Bernhardi Shiever*, Chirurgi munere fungentis in Legione Roſenſtierniana, ad Magiſtrum Equitum Nobiliſſimum *D. Cederberg* perſcriptas, in quibus refert caſum, in hiſce ſane terris rariſſimum, quod mulier quædam, poſtquam per ſeptem menſes gravida fuiſſet, evaneſcente tumore omni abdominis, per anum peperit cranii humani particulam, duaſque coſtas. Sed cum multa deſiderari viderentur, quæ lucem fœnerari poſſent tam miro phænomeno, dignum putavi, amiciſſimum *Heſſelium* per literas denuo compellare, vellet ulterius in eam rem inquirere, mihique communicare, quo poſſem tam mirandam rem ceteris meis inſe-

rere

rere obſervationibus, quas prelo paratas dudum habeo. Quod & factum eſt. Chirurgi vero relatio, ex Germanico in Latinum tranſlata, ſic ſe habet:

" Uxor *Laurentii Skallroth*, Paſtoris in *OEſtad*, " Territorii *Ahle* dicti, ditionis Elfsburgenſis, ætatis 41. " quatuor vero annos nupta, A. 1720, menſe Julio, in- " gravidatur, fructumque fert per ſeptem menſes, viſis " tamen interdum paucis menſtruis, ſed ſperans, juſto " demum tempore & poſt decimi menſis faſtidia, pulcra " prole parentem ſe futuram. Præterlapſo hoc tem- " pore, evaneſcere tumorem ſentit, remanente ſolum- " modo preſſione quadam in latere dextro. Poſt men- " ſem rurſus fit gravida, peperitque filiam, ſed mortu- " am juſtæ magnitudinis, in Decembri A. 1721. Ex " quo tempore lecto adfigitur in menſem uſque Junii " hujus anni. Menſe vero Maii, cum alvum levaret " aliquando, anus ipſi tam dolere, ut ipſum inteſtinum " rectum omnino excidere crederet: duobus ergo digi- " tis prætentans & ſe juvare volens, extrahit cranii " partem, magnitudine nummi Coronæ Svethicæ (vul- " go *dubbel Carolin*); repertis deinde in ſella duabus " coſtis, & poſt 14 dies abeuntibus eadem via reliquis " oſſibus, colore excrementitio, quæ & ipſe vidi, & " manu mea contrectavi. Maritus, uxor ipſa, & ejus " ſoror, alia non via exiiſſe hæc oſſa mihi ſancte adfir- " marunt, incredulo licet, & contra ordinem naturæ " hæc fieri non poſſe, ſtrenue defendenti. Bene nunc " valet, domeſticis apta curis, triumque mater libero- " rum, qui adhuc in vivis ſunt: menſtruis quoque na- " turaliter ſe habentibus." *Ahlingsâhs* die 28 Octobris A. 1722.

Exercuit jam dudum, & exercebit, ni fallor, hic caſus Medicorum ingenia, in diverſas forte abeuntium ſententias. Si enim fœtus in ipſo conceptus fuiſſet utero, ibique adcreviſſet, non niſi per ruptum uterum, rup-

ruptumque inteſtinum egredi potuiſſet. Sed quomodo tantum vulnus intra temporis tantillum fuiſſet ſanabile? Oſſeas aliquando fœtus particulas per apoſtema prorupiſſe, res ſatis clara eſt; interdum per umbilicum, quod vidit Albucaſis, *Chirurg. Lib.* 2. *cap.* 76. & Marcellus Donatus *Hiſt. Med. Mirab. Lib.* 4. *cap.* 22. *p. m.* 438, 439; interdum per Hypogaſtrium, ut ſcribit ad Segerum Wepferus; interdum quoque, uti nunc refertur, per inteſtinum rectum, ſicut annotavere Marſilius Cognatus, *Var. Obſ. Lib.* 4. *cap.* 9, & Joh. Langius, *Epiſt. Med. Lib.* 2. *Ep.* 39. *p. m.* 739. vir ſane doctiſſimus & candidus. *

Rumpi quoque parietes uteri, infantemque in abdomen extrudi, vel totum, vel quoad partes, teſtes fuere oculati, Hildanus *Obſ.* 64. *Cent. I.* Veſlingius *Syntagm. Anat. cap.* 7. & Joh. Caſp. Fauſius in Epitaphio Eliſabethæ Treipfenning, Petri Gilfeldii uxoris, quæ fœtum maſculum non in mundum, ſed in ſemet ipſam, rupto utero, edidit; Heidelbergæ 1657. Conf. Bohnius *Circul. Anat. Progymnaſm, I. p. m.* 20. & 21

Marquardus *Empir. Pract. p.* 304. 305. vidit molam in vaſis ſpermaticis Catharinæ Zarthin juxta uteri fundum genitam. Et Riolanus *Lib.* 2. *Antropograph. cap.* 35. quatuor producit exempla fœtuum ex Tubis Fallopianis exſectorum. In quam ſententiam multis argumentis ivit Celeberrimus Thomas Bartholinus in *Epiſtola* ad Horſtium, & Antonius Deuſingius, Profeſſor olim Academiæ Groningenſis. Hærvæus præterea de *Gener. Animal. Exerc.* 65. animantia vivipara, non in utero, ſed in cornubus concipere, ac uterum ipſis propylæum eſſe, tubas vero muliebres, cornubus aliorum

* Conferri meretur *Hiſtoire d' un Fœtus humain tiré du ventre de ſa mere par le fondement* par *Monſ. LITTRE*, inſerta Commentariis Reg. Academiæ Scientiarum Anni 1702. p. 234. ſq. edit. Pariſ.

ani-

animalium proportione, ſitu, connexione, amplitudine, ſimilitudine, perforatione & officio, reſpondere teſtatur. Ita ut nullum ſit dubium, fœtum extra uterum in tuba generari poſſe, ibique grandeſcere, hinc in abdomen delabi, & qua data porta excerni; quamvis hoc negent Guido Patinus, Conringius, aliique.

Taceo nunc Lithopædion Senonenſe, a Columba Chatry, Sartoris uxore, per 28 annos utero circumlatum, * quo nunc Regium Muſæum Hafnienſe gaudet: nominaſſe ſaltem contentus Fœtum Muſſipontanum (*Pont. a Mouſſon.*) extra uterum inventum, ſemilapideum, ab Illuſtriſſimo Angliæ Cancellario Kenelmo Digbæo apud Pharmacopæum Barbilottum, aliiſque olim viſum, & publica Diſſertatione in Academia Dolana, ſub Præſidio Stephani Willet, ventilatum; quæ omnia prolixius apud Strauſſium de *Fœtu Muſſipontano* legi poſſunt. Namque quod Pentzius, Fehrius & Academia tum putabat Altdorfina, non fœtum, ſed ſororem fuiſſe, a phantaſia matris olim gravidæ progenitam, nihil moror.

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* Non ita pridem *GEORG. FRIDER. ORTH* Ratiſp. *hiſtoriam* exhibuit *Fœtus in utero materno per XLVI annos detenti*, in Diſſert. Inaugur. ſub præſidio *Rudolfi Jacobi Camerarii* habita Tubingæ, 1720.

V. *Obſervatio circa Lumen Boreale d. 20. Sept. An. 1717. prope* Upſal. *Per* E. J. Burman. *Ex Actis Liter. Sueciæ An. 1724. Trimeſtr. 3.*

CHaſmata, uti vocantur, cœli, quæ alias horizontale lumen & aurora ſeptentrionalis audiunt, Suethice *Nord-skjen, Nord ljus, Nordblyſs, Nordbláſs, Lâtterskjen, Lyſſnor, &c.* illæ, vulgi judicio, acies, ſeu exercituum præliorumque ideæ, Meteoron in regionibus noſtris, aliiſque Polo vicinioribus, illuſtre ſane & frequens (frequentius hodie quam olim nobis ætate provectiores perſuadere volunt) juſta cum diligentia ſæpius obſervavimus. Obſervationes omnes recenſere longum; in publicum edere, citra chalcographi opem, vel impoſſibile vel fruſtra eſſet. Sed fertur alicubi Virum Mathematica doctrina atque experientia clariſſimum phænomeni hujus hiſtoriam parare: quod ſi rem ipſi gratam nos præſtituros conſtaret, ſingulas rite delineatas deſcriptaſque ipſi lubentes communicaremus. Communicabimus tamen interim unicum, quod A. 1717. d. 20. Septembris in nocturno itinere accuratius conſiderare licuit, & cujus rarior planeque ſingularis facies conjecturam de natura phænomeni paraſtatica eruditorum examini accuratiori heic ſubjiciendi anſam dedit.

Erant ſolito plures phaſmatum horuncce tractus, arcus nimirum albicantes & reliquo cœlo (ſereno utique ac tranquillo) lucidiores, quatuor ad minimum aut tres, mediocribus tenebrarum intervallis diſtincti, & unus ſupra alterum poſiti. Quod autem rariſſimum adſpectuque jucundum, dictorum arcuum diſtantiæ ſub ipſa Cynoſura maximæ (infra quam ſupremus quidem ultra viginti & ſex gradus non conſiſtebat, quippe per quem

ſtella Urſæ majoris Dubbe leviter ſubinde tranſparebat) verſus horizontem utrinque ſenſim decreſcebant, donec illi tandem in ipſis orientis occidentiſque cardinibus mutuo ſeſe interſecarent, haud ſecus quam in artificiali globo Meridiani ad Polos Æquatoris convergere cernuntur. Candidi iſti arcus ſeu ſemicirculi maximam partem ex ſtriis diſtinctis & ad horizontem normalibus conſtabant, præſertim in ſummitate, vel ſub ipſo ſeptentrione; ad latera lux debilior conſpiciebatur ac ſubobſcura.

Striarum geminus erat motus, horizontalis unus, alter perpendicularis: hic minor longe & tardior, ut vix circularem arcuum formam turbaret; ille varius ſimul atque celerrimus, ab oriente in occidentem, & retro. Quoties autem ſtriæ plures (quod ſæpius accidebat) a contrariis venientes plagis ſibi invicem occurrebant, ſive id in medio arcu, ſive alibi fieret; toties, quaſi ex illa radiorum mixtura ſeu multiplici interſectione proveniens colorum pulcherrima apparebat varietas, & quidem ordine prorſus eodem atque in priſmate vitreo, explicatis ſolique obverſis avium minorum pennis, aliiſve corporibus ſimilibus colores produci ſolent.

Hinc vero dari occaſio poterit adhuc de Opticis phænomeni rationibus, cum *Carteſio in de Meteoris Cap. VII. §.* 18. cogitandi; ſed neque tamen ideo ſubtiliori materiæ ſulphureæ in regione aëris inferiori accenſæ omnem denegari poſſe locum exiſtimamus. Ipſi enim alias ſæpius, præſertim in chaſmate A. 1716. d. 17. Martii heic longe illuſtriori quam in Anglia, Gallia, Germania, alibi, per totam noctem viſo, colores multo plures, necnon ſuſurros & ſibilos, quales excitari a focali flamma ſolent, obſervavimus. Forſan autem duo diverſa ſtatuere luminis borealis genera oportet: unum meteoron igneum ab effluviis & exhalationibus; alterum mere paraſtaticum, ex diverſimoda ſolarium radio-

rum

rum refractione & reflexione, ſive in glacialibus quibuſdam lamellis, ſtelluliſve atmoſphæræ regionem excelſiorem occupantibus facta, ſive etiam in maribus quibuſdam ad ſeptentrionem, indeque nobis ex nubibus communicata, ortum. Certe poſterius ſingulari experimento illuſtrari poſſe videtur, quod occaſione jam deſcripti phaſmatis (cui tamen ſimile vidimus A. 1716. in Februario hora veſpertina 9. ex duobus ejuſmodi arcubus ſemicircularibus, ſed minus ſtriatis & ſupra borealem horizontis plagam elevatioribus, conſtans) inventum factumque, huc redit.

Si lamina ſumatur ſtannea longitudinis latitudiniſque arbitrariæ, eademque acuto & fortiori cultro, uno ductu ſecundum longitudinem univerſam, donec tota ſtriata facta fuerit, raſa, manu ita teneatur, ut ejus planum cum lucente candela & obſcuro pariete tabulave æquales faciat angulos; deinde autem variis modis incurvetur & torqueatur, ipſam nunc concavam nunc convexam parieti vel tabulæ obvertendo, tardius ad lubitum aut celerius: phaſmata ſupra recenſitis admodum ſimilia ſpectaculo non injucundo repræſentabuntur.

Quid ſi utriuſque generis lumen forte aliquando una exiſtere, atque unum cum altero coincidere dicamus, ut neutrum alterius cauſſa ſit aut effectus, ſed ambo ad noctem illuminandam terroremque ſpectatoribus incutiendum concurrant? Quemadmodum enim ſæpiſſime quidem lumen horizontale, Zonas videlicet candentes, nunc nudas (imo per ipſum fere Zenith tranſeuntes, quas cum Galaxia, utut non parum latiori, ob ſimilitudinem vulgus confundere ſolet) nunc columnis, pyramidibus inverſis aliiſque figuris variis ſtipatas, ſed citra omne aëris, ut ita dicam, incendium; ita nec raro hoc ſine illis vel antecedentibus vel concomitantibus, apparentiis diſtincte notatis, vidimus: quamvis etiam e regione quadam cœli, nude primum candente, tandem ferven-

tiſſimas faces, ſive per ſolis radios in glacialibus Oceani partibus, ceu in ſpeculis quibuſdam cauſticis, reflexos, ſive alio quocunque modo accenſas, ad Zenith & ſuper totum nonnunquam hemiſphærium evolaſſe fatendum ſit.

Sed quia genuinas veraſque phænomeni hujus admirandi cauſſas vix cuiquam certo invenire prius licet, quam plurimarum in diverſis terræ locis unâ habitarum obſervationum rite inſtitui queat comparatio; unde ante omnia conſtet, num lumen iſtud in remotioribus etiam locis ſub eodem altitudinis angulo conſpiciatur; num quod heic horizonti parallelum, alibi verticale ſit, & id genus alia; verbo, utrum unus idemque ſit arcus qui in diverſis locis conſpicitur, an quemadmodum in Iride, ita quoque heic, quot in terra ſpectatores, tot arcus in cœlo: Enixe proinde omnes in univerſum atque ſingulos rogamus, quibus rerum naturalium in aliquo pretio eſt ſcientia, velint ubicunque terrarum, maxime vero in regionibus borealioribus, boreali huic lumini quoad omnes circumſtantias obſervando quam diligentiſſime invigilare, ſuaque obſervata quantocyus cum publico vel ſaltem nobiſcum communicare, gratiam ab erudito orbe ſane maximam merituri. Nos alia occaſione quaſdam regulas ſeu harum obſervationum normam & exemplar dabimus, parati interim & ipſi aliorum monita grato excipere animo, & quænam judicaverit quiſque potiora hujus negotii momenta, fieri certiores.

VI. *Ba-*

VI. *Barometri novi deſcriptio, a* D. G. Fahrenheit, *R. S. S.*

IN recenſione experimentorum nonnullorum circa ebullitionem quorundam liquorum a me factorum mentionem feci, gradum caloris aquæ ebullientis termino tunc memorato, 212 nempe graduum coerceri; poſtea variis obſervationibus atque experimentis edoctus ſum terminum hunc, manente eadem atmoſphæræ gravitate, ſatis fixum eſſe, ſed variante gravitate atmoſphæræ hunce terminum quoque diverſimode variari poſſe. Experimenta in hunc ſcopum jam facta nunc quidem traderem, ſed quoniam adhuc de certis circumſtantiis edoctus eſſe velim, hinc recenſionem illorum uſque in aliud tempus reponam, & interea ſolummodo thermometri alicujus mentionem faciam, quod forſitan ſi non magis, tamen æque idoneum erit ad explorandam atmoſphæræ gravitatem, quam barometrum. Hujus delineatio in Figura VIII. eſt appoſita.

Cylindro A B annectitur tubus B C, cui additur globulus oblongus C D, & huic tubulus gracillimo foramine præditus D E. Cylindrus liquore quodam, qui calorem aquæ ebullientis perferre poteſt, replebitur. In tubulo B C, gradus caloris in aere obvii menſurabuntur ope ſcalæ affixæ *b c*. Si autem thermometrum hocce aquæ bullienti imponatur, liquor thermometri non ſolum globulum C D implebit, ſed etiam uſque ad terminos varios tubuli D E aſſurget, ſecundum gradum caloris, quem aqua tempore experimenti a gravitate atmoſphæræ acquiſitura eſt. Ita, ſi, exempli gratia, tempore experimenti altitudo mercurii in barometro ſit 28 pollicum Londinenſium, liquor in hocce thermometro attinget infi-

infimum locum in tubulo D E ; Si vero gravitas atmosphæræ æquipolleat altitudini mercurii triginta & unius pollicum, liquor a calore aquæ ebullientis usque ad locum supremum tubuli D E attolletur, termini varii autem caloris aquæ ebullientis non gradibus, sed illorum loco numeris digitorum, quibus altitudo mercurii in barometris vulgo mensuratur, ope nempe scalæ additæ *de* denotabuntur.

VII. *Observatio Lunaris Eclipsis habita* Ulyssipone *in Palatio Regio Die* 1. Novembris 1724. *A PP.* Joanne Baptista Carbone, & Dominico Capasso, *Soc. Jesu. Communicante Excellentissimo Domino, Dno de* Galvaon, *Commendatore* Villæ Meam & Francæ, *Equ. Ord. Christ. Legato Sereniss. Reg.* Portugall. *ad Sereniss. Regem* Magn. Britan. *S. R. S.*

OBservavimus hanc Eclipsim Telescopiis, altero quidem Pedum Parisinorum 8. sed clarissimo, altero 10. sed minus claro : utroque tamen lunares maculæ perfectissimè discernebantur. Ad temporis dimensionem usi sumus Horologio oscillatorio, vulgo *Pendula*, satis exacto, pluribus ante diebus in ipso observationis loco firmato,& quotidiano examine per meridianam lineam, ibidem a nobis jamdiu inventam, & pluries examinatam, ad medium Solis motum quam proxime reducto. Nocte vero ipsius Eclipsis ter illud ad trutinam revocavimus, ut ejus a vero tempore discordiam deprehenderemus. Primo in transitu *Fomahantis* per Meridianum, Hor. 8. M. 17. Sec. 18. Secundo in transitu *Rigel* seu *Pedis Lucidi Orionis*, Hor. 2. M. 35. Sec. 21. Tertio

tio in tranſitu *Sirii*, Hor. 4. M. 7. Sec. 40. (Aſcenſiones rectæ deductæ ſunt ex Tabulis Hirianis.) Inventimus autem Horologium tardius incedere ſecundis tantum 7. quæ jam addita ſunt momento Obſervationis mox apponendæ.

A Solis Occaſu uſque ad Mediam Noctem Nubes ac Pluviæ Cœlum nobis identidem adimebant. Ventus tamen ſub horam 1. illud nobis ſatis clarum reſtituit, eoque uſi ſumus ad horam prope tertiam.

Temp. Ver. Hor.	′	″	
1	38	0	Penumbra incipit eſſe ſenſibilis
1	41	0	fit ſpiſſior
1	43	29	fit ſpiſſiſſima
1	47	45	Umbra incipit
1	49	25	Diſcus Lunæ apparet deficiens
2	0	16	Umbra pertingit ad *Ariſtarchum*
2	0	39	Pertingit ad *Platonem*
2	1	10	*Ariſtarchus* totus in Umbra
2	6	22	*Architas*
2	8	7	*Ariſtoteles*
2	10	29	*Pitheas*
2	11	28	*Galilæus*
2	13	22	Umbra ad littus Orientale *Maris Serenitatis*
2	15	34	*Endymion* immergitur totus
2	18	2	*Copernicus* incipit immergi
2	20	7	totus in Umbra
2	21	5	*Poſſidonius* incipit
2	22	8	totus latet
2	27	49	*Ricciolus* incipit
2	31	56	Umbra pervenit ad *Grimaldum*
2	34	37	Ad Litus Boreale *Maris Criſium*
2	37	17	*Proclus* immergitur

Hor.

Hor.	′	″	
2	40	0	*Nubes ſupervenit, quæ Lunam omnino tegit, diuque videtur duratura.*
3	25	0	*Nubes diſcedit. Jam autem ex Umbra emerſerunt* *Grimaldus,* *Ricciolus,* *Keplerus,* *Galilæus.*
3	29	2	*Ariſtarchus* emergit
3	30	30	*Copernicus* incipit emergere
3	31	34	totus extra Umbram
3	39	18	*Pitheas* emergit
3	47	46	*Timocharis*
3	54	57	*Archimedes*
3	57	18	*Plato* incipit emergere
3	58	59	emergit totaliter
4	2	0	*Nubecula iterum Lunæ aſpectum nobis adimit.*
4	6	0	*Jam Luna reſtituitur*
4	8	15	*Ariſtotelis* totalis emerſio *Nubeculis identidem Luuam occupantibus, reliquarum macularum emerſiones exacte obſervari nequeunt.*
4	20	36	Finis Eclipſis, *tardius fortaſſe viſus ob tenuem vaporem interpoſitum*
4	26	0	Deſinit Penumbra ſpiſſior
4	28	0	Deſinit Penumbra ſenſibilis.

Pecu-

Peculiari profecto cura, ac ea qua fieri potuit diligentia, Eclipſim hanc obſervare conati ſumus; non modo ut noſtri muneris partes pro modulo noſtro impleremus, quippe Matheſi, ac præcipue Aſtronomicis obſervationibus hic addicti, verum etiam, ut Sereniſſimi Regis ingenio plenius, ut par erat, indulgeremus. Valde enim Ipſe in hujuſmodi obſervationibus delectatur, ad eaſque perfecte inſtituendas, copioſam nobis Inſtrumentorum ſupellectilem, munificentia vere Regia, ſuppeditavit. Neque vero aut iiſdem obſervationibus intereſſe, aut Obſervatorem ſe nobis identidem adjungere dedignatur; illud unice ſemper ſolicitus, ne, vel minimum, a rectitudine aberretur. Hoc igitur acti ſtimulo nec diligentiæ pepercimus, nec labori, quo tanti Principis ingenio hæc noſtra obſervatio reſponderet. Acceſſit & alia qua dudum tenebamur cura, novis ac clarioribus confirmandi argumentis hujuſce Urbis Longitudinem, vel certe ejuſdem cum præcipuis Europæ Meridianis differentiam; itemque explorandi, an aliorum Tabulæ Aſtronomicæ, ad hunc Meridianum reductæ per inventas hactenus differentias, perfecte Cœlo congruerent? Ex quo enim Ulyſſiponem pervenimus, ubi jam alterum egimus annum, nulla contigerat Lunæ Eclipſis ſupra noſtrum Horizontem conſpicua. Duæ vero ſolares, altera Die 8. Decembris anni 1722. altera 22. Maii labentis anni, oppoſitis undique nubibus, obſervari nullatenus potuere. Jam autem repetitis hoc biennio obſervationibus tum Immerſionum, tum Emerſionum Intimi Jovis Satellitis, proxime deprehenderamus, ſaltem quoad minuta prima, differentiam inter hujuſce Urbis, ſeu verius hujuſce Regii Palatii, ac Regii Pariſienſis Obſervatorii Meridianos: minuta nempe horaria 48 (mox ubi receperimus obſervationes habitas in eodem Obſervatorio, deprehendere poterimus & ſecunda, quæ hic diligenter notavimus.) Hac igitur uſi differentia præſentem Eclipſim ſup-

putavimus ex Tabulis Hirianis. Tum autem mihi, tum P. *Dominico Capaſſo* eadem quantitas, idemque pene temporis momentum pro Initio, Medio, ac Fine Eclipſeos prodiit: Mihi nempe Init. Hor. 1. m. 48. ſec. 4. Medium, Hor. 3. m. 3. ſec. 59. Finis, Hor. 4. m. 19. ſec. 54. Illi vero, Init. Hor. 1. m. 48. ſec. 0. Med. Hor. 3. m. 3. ſec. 58. Fin. Hor. 4. m. 19. ſec. 56. Quantitas utrique digit. 6. min. 53. Sufficiens proinde argumentum erat, quod recte Tabulis uterque uſus eſſet; atque illud unice expectandum, ut Cœlum pariter ſupputationibus reſponderet. Quamquam ex nonnullis Ephemeridibus aliter futurum deducebatur. At revera quam proxime reſpondit (neglectis ſcilicet tantum ſecundis) ut ex ipſa patet obſervatione.

Fateor tamen nec me multum fidere hujuſmodi obſervationibus Lunarium Eclipſium, quod attinet ad ſecunda definienda. Adeo enim male terminata apparet terreſtris umbra, ut ejus ac ſpiſſioris penumbræ confinia illico diſcernere difficile admodum videatur. Hinc non abs re fore putaverim nonnullas hic ſubtexere obſervationes Immerſionum atque Emerſionum Intimi Jovis Satellitis, ex iis, quæ præſertim hoc anno habuimus; nam multas præteriti anni in promptu non habeo. Duas tantum memini, & alibi etiam adnotatas invenio, quibus ſcilicet uſus eſt Cl. V. & Illuſtriſſ. Dominus *Franc. Blanchinus*, ad determinandam differentiam inter Meridianos Ulyſſiponenſem, ac Urbinenſem. Has igitur tantum ex præterito anno ſubnectam.

Obſer-

Observationes Immersionum, ac Emersionum Intimi Jovis Satellitis, habitæ Ulyssipone in Palatio Regio, & in Collegio Divi Antonii Soc. Jesu.

Regium Palatium, ac Collegium sunt in eodem omnino Meridiano. Omnes observationes habitæ sunt Telescopio palmor. rom. 30. Josephi Campani.

1723.

Emersiones.

	H.	′	″
Die 23. Julii	7	47	00
Die 7 Septem.	8	21	48

1724.

Immersiones.

Die 8 Jun. *mane*	2	03	28
Die 15	3	56	27
Die 30	2	08	51

Emersiones.

Die 2 Sept.	9	36	57
Die 9	11	34	26
Die 25	9	59	21
Die 4 Octob.	6	26	44
Die 18	10	21	20
Die 3 Novemb.	8	42	30

VIII. *Meridianorum* Ulyſſiponenſis, Pariſienſis & Londinenſis *differentia, ex literis Clariſſimi Doctiſſimique Viri, Reverend. Patr.* Johannis Baptiſtæ Carbone *Soc. Jeſ. ad* Iſaacum Sequeyra Samuda *M. D. Coll. Med. Lond. Lic. S. R. S.*

Illud advertere juvat, differentiam inter hunc meridianum, ac Pariſienſem, minorem jam inventam, ac nos antea putabamus; non quidem novis obſervationibus hic habitis, ſed iiſdem cum Pariſienſibus, quas modo accepimus, comparatis. Nullas antehac acceperamus, unde dictam differentiam deprehendere certo poſſemus; ſed noſtras obſervationes unice contuleramus cum ſupputationibus domini *Lieutaud*, meridiano Pariſienſi accommodatis, in ſuo libello, quem *Connoiſſance des Temps* inſcribit, & quotannis, Academiæ regiæ juſſu, in lucem edit. At nimium a vero aberrare illas dignovimus, præcipue, quæ ad immerſiones, atque emerſiones intimi Jovis ſatellitis ſpectant. Ipſæ enim obſervationes habitæ in Obſervatorio regio, modo duobus, modo tribus, modo etiam 4 minutis, ab illis diſſentiunt. En tibi comparationem noſtræ obſervationis lunaris Eclipſis, cum obſervatione Pariſienſi, ab ipſo *Maraldo* obſervatore, & regio aſtronomo facta; cui etiam apponam comparationes immerſionum atque emerſionum, quæ utrobique fuerunt obſervatæ: omnia genuino idiomate, quo fuerunt ad me miſſa, Pariſiis.

Com-

Comparaiſon des Obſervations de l' Eclipſe de Lune du 1 Novembre 1724, faites a Lisbone, & a Paris.

Quoyque dans cette eclipſe, l'ombre de la Lune n' ait point paru terminée a Lisbone, ny a Paris, ce qui a rendu la determination de ces phaſes plus difficiles, cependant la plus part des obſervations s' accordent ſi bien enſemble, que nous avous cru devoir faire la comparaiſon des phaſes principales, qui paroiſſent avoir eté obſervées avec le plus d' exactitude, pour en determiner la difference des meridiens entre Lisbone, & Paris.

Hor.	′	″	
1	47	45	Commencement a Lisbone
2	33	30	a Paris
	45	45	Difference des meridiens entre Lisbone & Paris
2	0	16	L'ombre a Ariſtarque a Lisbone
2	46	15	a Paris
	45	59	Difference
2	11	28	a Lisbone l'ombre a Galilée
2	56	20	a Paris
	44	52	Difference
2	34	37	a Lisbone l'ombre au bord ſeptentrional de la mer Caſpiene
3	20	30	a Paris
	45	53	Difference
2	37	17	a Lisbone l'ombre a Proclus
3	23	30	a Paris
	46	13	Difference

Hor.

Hor.	′	″	
3	29	2	a Lisbone Ariſtarque ſort de l'ombre
4	14	30	a Paris
	45	28	Differenee
3	31	34	Tout Copernic eſt hors de l'ombre a Lisbone
4	17	50	a Paris
	46	16	Difference
3	47	46	Timocharis eſt ſorti de l'ombre a Lisbone
4	33	34	a Paris
	45	48	Difference
3	58	59	Platon eſt entierement hors de l'ombre a Lisbone
4	44	23	a Paris
	45	24	Difference
4	20	36	Fin de l' eclipſe a Lisbone
1	6	30	a Paris
	45	54	Difference

Suivant ces obſervations la durée de l' eclipſe a Liſbone a eté de $2^h\ 32'\ 51''$ plus petite ſeulement de 9 ſecondes qu'elle n'a eté obſervée a Paris, & la difference des meridiens, qui reſulte des obſervations du commencement & de la fin eſt de $45'\ 50''$ ce qui s'approche beaucoup de ce qui reſulte de la comparaiſon des autres taches obſervées a Lisbone, & a Paris.

Com-

Comparaiſon de quelques Obſervations des Satellites de Jupiter faites a Lisbone, & a Paris.

Nous avons fait a l'obſervatoire royal de Paris pluſieurs obſervations correſpondantes a celles qui nous ont eté envoyées de Lisbone, en voici la comparaiſon.

		H.	′	″	
Le 30 Juin. 1724	a	2	08	51	Immerſion a Lisbone
		2	54	41	a Paris
			45	50	Difference
Le 2 Sept. 1724	a	9	36	57	Emerſion a Lisbone
		10	22	46	a Paris
			45	49	Difference
Le 25 Sept. 1724	a	9	59	21	Emerſion a Lisbone
		10	45	05	a Paris
			45	44	Difference
Le 4 Octobre	a	6	26	44	Emerſion a Lisbone
		7	11	58	a Paris
			45	14	Difference

La plus part de ces obſervations, s' accordent a donner la difference des meridiens, entre Lisbone & Paris de 45′ 48″ d'heure, ce qui s'accorde avec toute l'exactitude que l'on peut eſperer, a celle que l'on a determinée par l'obſervation derniere de l'eclipſe de Lune, &c. Hactenus dominus *Maraldus*, cujus obſervationem lunaris eclipſeos, ſeorſim tranſcribere non vacat; pluribus enim curis ſum diſtentus.

Si vera eſt prædicta differentia, nempe 45′ 48″, erit differentia inter hunc meridianum Ulyſſiponenſem, & Londini, 36′ 7″, quam mox collatis obſervationibus in utroque meridiano faciendis, melius examinabimus, certiuſque deprehendemus.

IX. *An Excretory Duct from the* Glandula Renalis. *Extracted from the* Giornale di Literati *of* Venice, *for the Year* 1719.

THE celebrated Anatomiſt, Signor *Anton Maria Valſalva*, already known by his noble Treatiſe *De Aure humana*, has lately made a conſiderable Diſcovery. He has found the excretory Ducts of the *Glandulæ Renales*, or, *Renes Succenturiati*, which diſcharge themſelves into the Parts of Generation; that is to ſay, into the *Epididymides* in Men, and into the *Ovaria* in Women. He has read a learned Diſſertation relating to this Diſcovery, before the Academy of Sciences in *Bologna*, in which he undertakes to prove, that thoſe *Renes ſuccenturiati*, are to be reckon'd among the principal Organs of Generation. He is ſoon to publiſh it in Print, but the Publication of the Cuts belonging to it, will be retarded, becauſe the engraving of them, cannot be finiſh'd in ſo ſhort a time.

We have ſince learn'd the diſagreeable News of the Death of this very learned and inquiſitive Anatomiſt; but, as his Reputation, and known Accuracy in diſſecting, leaves no room to doubt of the reality of the abovemention'd Diſcovery, we hope it will be proſecuted by other curious Perſons.

X. *Of*

X. *Of the Currents at the Streights Mouth. By Capt. - - - - Communicated by Dr.* Hudſon.

CApe *Spartel*, and Cape *Trafalgar*, from the weſtern Ocean, are known to make the *Streights* Mouth, from whence a Current, in the middle of the Channel (which is about five Leagues broad) betwixt the *Barbary* and *Spaniſh* Land, runs, at leaſt, two Miles each Hour, as far as *Ceuta* Point; and there the two Coaſts opening about eighteen Leagues diſtant from each other, the Current does not run above one Mile an Hour, and ſo continues as far as Cape *de Gat*, which is ſeventy Leagues up the *Mediterranean*. Our Mariners obſerve a Current to ſet to the weſtern Sea, or the great Ocean from *Ceuta*, along the *Barbary* Shore; and, from *Gibraltar*, along the *Spaniſh* Shore; but that on the *Barbary* Shore is generally their common Rout, not only as being the freeſt from Rocks and leſs dangerous, but by reaſon that the Tide is much ſtronger, than it is on the other Side, which the ſooner helps the Ships out of the *Streights*, which are the narroweſt betwixt the Points of *Gibraltar* and *Ceuta*; at which laſt Place, a Neck of Land extends itſelf a conſiderable Way into the Sea; and it's my Opinion, and that of others, that whereas the Current runs, as abovesaid, two Miles an Hour againſt this Neck of Land, the Water there meets with ſo violent an Oppoſition in its Courſe, as occaſions it to rebound with ſo much Force, that Part of it returns back along the ſame Coaſt, and ſo out of the *Streights* Mouth; which, with the ſmall Tide that ſets out on the *Spaniſh* Shore, it's believ'd, may exhauſt a conſiderable Part of that Current, which continually ſets

in, to the Eaſtward, at the Rate I have already mention'd. What I look upon to be very remarkable, is that in the Year 1712, Monſieur *du L'aigle*, that fortunate and generous Commander of the Privateer call'd the *Phœnix* of *Marſeille*, giving Chaſe, near *Ceuta* Point, to a *Dutch* Ship bound for *Holland*, he came up with her in the middle of the Gut, or *Streights*, betwixt *Tariffa* and *Tangier*, and there gave her one Broad-ſide, which directly ſunk her, all her Men being ſav'd by the Means of Monſ. *du L'aigle*; and a few Days after, the ſunk Ship, with her Cargo of Brandy and Oil, aroſe on the Shore near *Tangier*, which is, at leaſt, four Leagues to the Weſtward of the Place were ſhe ſunk, and directly againſt the Strength of the Current; which has perſuaded many Men, that there is a Recurrency in the deep Water in the middle of the Gut, that ſets outwards to the grand Ocean, which, I think, this Accident very much demonſtrates; and poſſibly, a great Part of the Water, which runs into the *Streights*, does return that Way, and along the two Coaſts which I have already mention'd; otherwiſe, this Ship of Courſe, muſt have been drove towards *Ceuta*, and ſo upwards. I was at *Gibraltar* when this happen'd, where I ſaw above 100 of the Butts of that Cargo of Brandy, which were ſent thither from *Tangier*; I likewiſe ſpoke with the Captain of the *Dutch* Ship, who told the Governor, myſelf, and many others, where his Veſſel ſunk; and her riſing afterwards at *Tangier*, appear'd very unaccountable to us, as it does to me to this Day; for there's no Doubt but the Ship ſunk where the *Dutchman* told us, ſince the *Spaniards* from the Land, who ſaw it, confirm'd it to us. The Water in the Gut muſt be very deep, ſeveral of the Commanders of our Ships of War having attempted to ſound it with the longeſt Lines they could contrive, but could never find any Bottom.

XI. Am-

XI. Ambergris *found in* Whales. *Communicated by Dr.* Boylſton *of* Boſton *in* New-England.

THE moſt learned Part of Mankind are ſtill at a Loſs about many Things, even in medical Uſe; and, particularly, were ſo in what is called *Ambergris*, until our *Whale* Fiſhermen of *Nantucket*, in *New-England*, ſome three or four Years paſt, made the Diſcovery. Their Account to me is this.

Cutting up a *Spermaceti* Bull *Whale*, they found accidentally in him, about twenty Pound Weight, more or leſs, of that Drug. After which, they, and other ſuch Fiſhermen, became very curious in ſearching all ſuch Whales they kill'd; and it has been ſince found in leſſer Quantities, in ſeveral Male Whales of that Kind, and in no other, and that ſcarcely in one of an Hundred of them. They add further, that it is contained in a Cyſt, or Bag, without any inlet or outlet to it, and that they have ſometimes found the Bag empty, and yet entire.

The Bag is no where to be found, but near the Genital Parts of the Fiſh. The *Ambergris* is, when firſt taken out, moiſt, and of an exceeding ſtrong and offenſive Smell.

Whether or not (*from the Account above*) *the* Ambergris *be naturally, or accidentally produced in that Fiſh, I leave to the Learned to determine.*

XII. *Obſervations on ſome of the Plants in* New-England, *with remarkable Inſtances of the Nature and Power of Vegetation. In a Letter to the Publiſher from the Honourable* Paul Dudley, *Eſq;* F. R. S.

THE Society, without Doubt, have long ſince been acquainted, that the Plants of *England*, as well thoſe of the Fields and Orchards, as of the Garden, that have been brought over hither, ſuit mighty well with our Soil, and grow here to great Perfection; however, that the curious may better make a Compariſon, I ſhall ſingle out ſome Particulars, of my own Obſervation.

To begin with the Fruit Trees.

Our Apples are, without Doubt, as good as thoſe of *England*, and much fairer to look to, and ſo are the Pears; but we have not got of all the Sorts.

Our Peaches do rather excel thoſe of *England*, and then we have not the Trouble or Expence of Walls for them; for our Peach Trees are all Standards, and I have had, in my own Garden, ſeven or eight Hundred fine Peaches of the Rare-ripes, growing at a Time on one Tree.

Our People, of late Years, have run ſo much upon Orchards, that in a Village near *Boſton*, conſiſting of about forty Families, they made near three Thouſand Barrels of Cyder. This was in the Year 1721. And, in another Town of two Hundred Families, in the ſame Year, I am credibly inform'd, they made near ten Thouſand Barrels. Some of our Apple Trees, will make ſix, ſome have made ſeven Barrels of Cyder, but this is not common; and the Apples will yield from

ſeven,

ſeven, to nine Buſhels for a Barrel of Cyder. A good Apple Tree, with us, will meaſure from ſix to ten Foot in Girt. I have ſeen a fine Pearmain, at a Foot from the Ground, meaſure ten Feet, and four Inches round. This Tree, in one Year, has bore thirty eight Buſhels, (by Meaſure) of as fine Pearmains, as ever I ſaw in *England*. A *Kentiſh* Pippin at three Foot from the Ground, ſeven Foot in Girt; a Golden Ruſſetin ſix Foot round. The largeſt Apple Tree, that I could find, was ten Foot and ſix Inches round, but this was no Graft.

Pear Trees.

An *Orange* Pear Tree grows the largeſt, and yields the faireſt Fruit. I know one of them near forty Foot high, that meaſures ſix Foot and ſix Inches in Girt, a Yard from the Ground, and has bore thirty Buſhels at Time; and this Year I meaſur'd an *Orange* Pear, that grew in my own Orchard, of eleven Inches round the Bulge. I have a Warden Pear Tree, that meaſures five Foot ſix Inches round. One of my Neighbours has a *Bergamot* Pear Tree, that was brought from *England* in a Box, about the Year 1643, that now meaſures ſix Foot about, and has bore twenty two Buſhels of fine Pears in one Year. About twenty Years ſince, the Owner took a Cyon, and grafted it upon a common Hedge Pear, but the Fruit does not prove altogether ſo good, and the Rind, or Skin, is thicker than that of the Original.

Peach Trees.

Our Peach Trees are large and fruitful, and bear commonly in three Years from the Stone. I have one in my Garden of twelve Years Growth, that meaſures two Foot and an Inch in Girt, a Yard from the Ground,

which

which, two Years ago, bore me near a Bushel of fine Peaches. Our common Cherries are not so good as the *Kentish* Cherries of *England*, and we have no Dukes, or Heart Cherries, unless in two or three Gardens.

Trees of the Wood.

Some Years since, I measur'd a *Platanus Occidentalis*, or *Button Wood* Tree (as they are called here) of nine Yards in Girt, and it held its Bigness a great Way up. This Tree, when it was cut down, I am inform'd, made twenty two Cord of Wood. A Gentleman tells me, that in the Forest, he met with a streight Ash, that grew like a Pillar, of a great Height, and free from Limbs, that measured fourteen Feet eight Inches round, near a Yard from the Ground; and, the other Day, I met with a *Sassafras* Tree, that measur'd five Foot three Inches in Girt. I meddle not here with our noble Pines and Cedars, because I design to treat of them in a Chapter of the Evergreens of this Country. Among our Trees of quick and easy Growth, the *Button Wood* before-mentioned, and the *Locust* Tree, are the most remarkable: As to the latter, by the Description Mr. *Moore*, while in *New-England*, gave me of the *Manna* Tree, our *Locust* Tree may be called the *American* Manna. I have known a Seed of it blown off from the Tree into my Garden, that took Root of itself, and, in less than two Years, was got above six Foot high, and as big about, as a common walking Cane. The *Platanus* I have frequently propagated, by cutting off Sticks of five or six Foot long, and setting them a Foot deep into the Ground in the Spring of the Year, when the Season is wet; they thrive best in a moist Soil.

Garden.

Garden.

An Onion, ſet out for Seed, will riſe to four Foot nine Inches in Height. A Parſnip will reach to eight Foot, red *Orrice* will mount nine Foot, white *Orrice* eight. In the Paſtures, I meaſured Seed *Mullen* nine Foot two Inches in Height, and one of the common Thiſtles above eight Foot.

Vegetation.

Among the remarkable Inſtances of the Power of Vegetation, I ſhall begin with an Account of a Pumpkin Seed, which I have well atteſted, from a worthy Divine. * The Relation is as follows: That in the Year 1699, a ſingle Pumpkin Seed was accidentally dropp'd in a ſmall Paſture where Cattle had been fodder'd for ſome Time. This ſingle Seed took Root of itſelf, and without any Manner of Care or Cultivation; the Vine run along over ſeveral Fences, and ſpread over a large Piece of Ground far and wide, and continued its Progreſs till the Froſt came and kill'd it. This Seed had no more than one Stalk, but a very large one; for it meaſured eight Inches round; from this ſingle Vine, they gathered two hundred and ſixty Pumpkins; and, one with another, as big as an half Peck; enough in the Whole, to fill a large Tumbrel, beſides a conſiderable Number of ſmall and unripe Pumpkins, that they made no Account of. The *Philoſophical Tranſactions* give an Account of a ſingle Plant of Barley, that by ſteeping and watering with *Salt-Petre* diſſolv'd in Water, produced two hundred and forty nine Stalks, and eighteen thouſand Grains; but then there was Art

* *The Reverend Mr.* Edwards *of* Windſor.

Art, and even Force in that Caſe; whereas in ours, there was nothing but pure Nature and Accident.

Our *Indian* Corn is the moſt Prolifick Grain that we have, and commonly produces twelve hundred, and often two thouſand Grains from one; but the faireſt Computation is thus; ſix Quarts of this Grain will plant an Acre of Ground, and it is not unuſual for an Acre of good Ground to produce fifty Buſhels of Corn. The Mention of *Indian* Corn obliges me to take notice of an extraordinary *Phænomenon* in the Vegetation of that Grain, *viz.* the interchanging, or mixing, of Colours after the Corn is planted. For your better underſtanding this Matter, I muſt obſerve, that our *Indian* Corn is of ſeveral Colours, as blue, white, red, and yellow; and if they are planted ſeparately, or by themſelves, ſo that no other Sort be near them, they will keep to their own Colour, *i. e.* the blue, will produce blue, the white, white, *&c.* But if in the ſame Field, you plant the blue Corn in one Row of Hills (as we term them) and the white, or yellow, in the next Row, they will mix and interchange their Colours; that is, ſome of the Ears of Corn, in the blue Corn Rows, ſhall be white, or yellow; and ſome again, in the white or yellow Rows, ſhall be of a blue Colour. Our Hills of *Indian* Corn are generally about four Foot aſunder, and ſo continued in a ſtreight Line, as far as the Field will allow; and then a ſecond Line, or Row of Hills, and ſo on; and yet this mixing and interchanging of Colours has been obſerved, when the Diſtance between the Rows of Hills, has been ſeveral Yards; and a worthy Clergyman, of an Iſland in this Province, * aſſures me, that the blue Corn has thus communicated, or exchanged, even at the Diſtance of four or five Rods; and

* *The Reverend Mr.* Mayhew, *of* Martha's *Vineyard.*

and, particularly in one Place, where there was a broad Ditch of Water betwixt them. Some of our People, but eſpecially the *Ab-Origines*, have been of Opinion, that this Commixtion, and Interchange, was owing to the Roots, and ſmall Fibres reaching to and communicating with one another; but this muſt certainly be a Miſtake, conſidering the great Diſtance of the Communication, eſpecially at ſome Times, and croſs a Canal of Water; for the ſmalleſt Fibres of the Roots of our *Indian* Corn, cannot extend above four or five Foot. I am therefore humbly of Opinion, that the *Stamina*, or Principles of this wonderful Copulation, or mixing of Colours, are carried thro' the Air by the Wind; and that the Time, or Seaſon of it, is, when the Corn is in the Earing, and while the Milk is in the Grain, for at that Time, the Corn is in a Sort of Eſtuation, and emits a ſtrong Scent. One Thing, which confirms the Air's being the Medium of this Communication of Colours in the Corn, is an Obſervation of one of my Neighbours, that a cloſe, high board Fence, between two Fields of Corn that were of a different Colour, entirely prevented any Mixture or Alteration of Colour, from that they were planted with.

It has been obſerv'd by Naturaliſts, that even Nature, which gives Laws to every Thing, does not always ſtrictly obſerve her own Rules; and I think, I have, in my own Town, met with a very notable Inſtance of this in the Vegetable World.

An Apple Tree there bears a conſiderable Quantity of Apples, eſpecially every other Year, which never had a Bloſſom; I had formerly heard the Owner ſpeak of it: But for the three laſt Years, I made it my Buſineſs, in the proper Seaſon, to go and obſerve it my ſelf; and when all the reſt of the Orchard was in the Bloom, this Tree had not one Bloſſom. Not being contented with once going, I went again, and again, till I found the young

Apples perfectly formed. The laſt Year, I went early, not knowing but that it might blow ſooner than the other Trees, but I found no Bloſſoms; and the Owner, with many of his Neighbours, aſſured me, they have known the Tree theſe forty Years, and that it never had a Bloſſom. I opened ſeveral of the Apples, and obſerv'd but very few Seeds in them; and ſome of them lodged ſingle in the Side of the Apple. This Tree was no Graft, and the Fruit but ordinary for Taſte. I could not perceive, by my Obſervation, but that, in all other Reſpects, it fructify'd like other Apple Trees. But that I may not leave Nature in any Diſorder, I would juſt mention, what is frequently obſerv'd in our Gardens, as to the winding or running Vines, more eſpecially the Hop, and the *French*, or Kidney Beans; how contrary they are to one another in their climbing, and yet how ſteadily they obſerve their own Laws: The Hop Vine winding about the Pole with the Sun, and the Bean againſt the Sun; and this Courſe they keep with ſuch Obſtinacy, that though an Attempt has been made over Night, to force the Hop Vine to wind againſt the Sun; yet, in the Morning, it has got back again to its natural Courſe, and the Bean again has done the ſame in her Way. In like Manner, the *Indian* Corn, abovementioned, has always an equal Number of Rows of Grain on the Ear, as eight, twelve, *&c.*

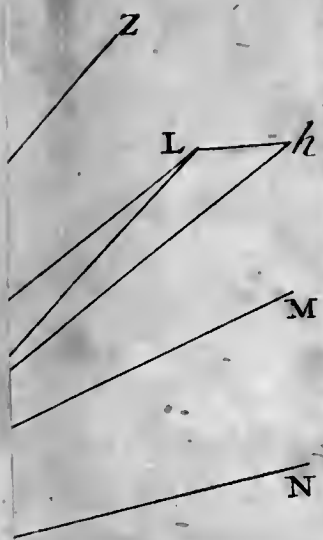
FINIS.

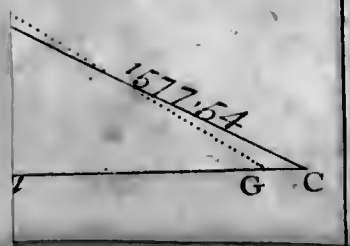
ERRATA.

Philoſ. Tranſact. N°. 383.

PAG. 94. N° 77. l. Ki—tſai—tsè. P. 95. N° 95 pro Germanicæ l. Gerardi. Ibid. N° 98. l. Vicia Sylveſtris, &c.

Plate I.





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PHILOSOPHICAL TRANSACTIONS.

For the Months of *January* and *February*, 1725.

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PHILOSOPHICAL TRANSACTIONS.

For the Months of January and February, 1752.

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III. [illegible] the Appearance of Mercury, [illegible] the Sun's Disk, on the 7th of October, 1736, determining the mean Motion, and [illegible] Node of that Planet's Orb. By Edmund Halley, LL. D. Astron. Reg. and [illegible] R. S. S.

A Diſſertation concerning the Figure of the Earth, by the Reverend John Theophilus Deſaguliers, *LL. D. F. R. S.*

THAT the Earth (that is, our Terraqueous Habitation) is of a ſpherical Figure, or nearly ſuch, has been prov'd ſo often, and by ſo many unanſwerable Arguments, that to repeat them here muſt needs be tedious to this learned Society. But, as a little Variation from a true Sphere (beſides the Irregularity of high Hills and deep Vallies) does not hinder us from calling the Earth a Globe; ſo, to determine what that Variation may be, ſince modern Philoſophers are divided about it, may be a Subject not ungrateful at this Time.

Monſieur *Caſſini* ſays: " That the Earth is an ob-
" long Spheroid, higher at the Poles than the Æquator,
" making the Axis longer than a Diameter of the Æqua-
" tor about thirteen *French* Leagues, which he de-
" duces from comparing his Father's Meaſures of the
" Meridian, from *Paris* to the *Pyrenæean* Moun-
" tains, with thoſe of Monſieur *Picard*; of which an
" Account may be ſeen in the Memoirs of the Royal
" Academy for 1713. But having afterwards con-
" tinued the Meridian, which is drawn through *France*,
" from *Paris* to *Dunkerque*, he ſtill draws Conſe-
" quences to prove the Earth an oblong Spheroid;
" but then makes the Axis exceed the Æquatorial
" Diameter 34 Leagues.

" Sir *Iſaac Newton* makes the Earth higher at the " Æquator, and, conſequently, flatted towards the " Poles, reckoning its Æquatorial Diameter 34 *Engliſh* " Miles longer than the Axis; which he proves from " the Principles of Gravity, and the Centrifugal Force " that ariſes from the Diurnal Rotation of the Earth; " and, to confirm this, mentions ſeveral Experiments " on Pendulums, which have been made ſhorter, to " ſwing Seconds, near the Æquator, than in greater " Latitudes."

Theſe are the two Opinions which have divided Philoſophers, and which we propoſe to examine here.

Monſieur *Caſſini*, taking the Meaſures above-mention'd to be exact enough, not only to determine the Magnitude of a Degree of the Earth, correſponding with a Degree of the great Circle of the Heavens, but alſo to ſhew the Difference in the Degrees of the Earth; (reckoning thoſe, that were meaſured in the South of *France*, to exceed thoſe towards the North, by a certain Number of Toiſes and Feet) demonſtrates, that if the Degrees of the Earth are longer towards the Æquator than the Poles, the Plane of the Meridian muſt be an Ellipſe, whoſe long Axis is that of the Earth. Here follows his firſt Demonſtration. [*See the* French *Memoirs for the Year* 1713]

" Let B D C R * be an Ellipſe that repreſents a " Meridian of the Earth, whoſe Poles B and C are at " Ends of the great Axis B C, and whoſe Foci E and " F are taken at Pleaſure. *Now, to divide this* " *Ellipſe into Degrees, that is, to find ſeveral* " *Points* H, I, V, *ſuch, that the Diſtance, from the* " *Pole to the Zenith, of every one of them, ſhall* " *be of any given Number of Degrees.*

* Fig. 1.

" From

" From E, one of the Foci of the Ellipſe, draw the " Line E T, ſo that it may, with the Axis B C, " make the Angle B E T equal to the Diſtance given " from the Pole to the Zenith. From the other Focus " F, with the Diſtance B C equal to the Axis, draw " an Arc, to cut the Line E T at T. I ſay, that the " Line F T, drawn from the Point T to the Focus " F, will cut the Ellipſe at the Point H; which Point " is ſuch, that the Diſtance of the Pole, from its " Zenith, contains the given Number of Degrees.

"DEMONSTRATION.

" From the Point H, raiſe H Z, perpendicular to " the Ellipſe which will paſs through the Zenith Z; " and, being produc'd inwards, will meet the Axis of " the Earth at O, and (by the Property of the Ellipſe) " divide the Angle E H F into two equal Parts. From " the Point H, draw likewiſe H P, parallel to the Axis " B C, and directed to the Pole P, ſuppos'd at an infi- " nite Diſtance. The Angle P H Z, or P O Z, mea- " ſures the Diſtance, from the Pole to the Zenith, of " an Inhabitant dwelling upon the Earth at the Point " H. F T is equal to the Axis B C, by Conſtruction; " but, by the Property of the Ellipſe, B C is equal to " E H *plus* H F; taking away from both F H, which " is common, E H will remain equal to H T. The " Angles E T H, T E H, will therefore be equal, " and, conſequently, each of them will be half of the " external Angle E H F; but the Angle E H O is " likewiſe equal to half of the Angle E H F; there- " fore the Angles T E H, E H O, will be equal to " one another; and, conſequently, the Lines E T and " H O will be parallel to one another; and the Angle " P O Z, which meaſures the Diſtance from the Pole

" to the Zenith of the Point H, will be equal to the " Angle B E T, which was, by Conſtruction, taken " equal to the given Diſtance of the Pole from the " Zenith ; *which was to be demonſtrated.*

" Now, if the Proportion of the longeſt Diameter " of the Ellipſe B C to E F, the Diſtance of the Foci, " be taken at Pleaſure, one may by Calculation " find all the Points of the Ellipſe as H, to deter- " mine the Degrees by this Analogy.

" *As F T, or B C:*

" *Is to E F : :*

" *So is the Sine of the Angle P E T (the given Di-* " *ſtance from the Pole to the Zenith) :*

" *To the Sine of the Angle E T F, or T E H:*

" whoſe Quantity will conſequently be known. This " Angle T E H being added to the Angle P E T, the " given Diſtance from the Pole to the Zenith of the " Point H, will give the Quantity of the Angle B E H, " which a Line drawn from the Focus to H, the Point " requir'd, makes with the Axis of the Ellipſe.

" Then in the Triangle E H F, whoſe Side E F is " known, as well as the Angle E H F, which is the " Double of the Angle T E H, and the Angle F E H " Supplement of the Angle B E H ; one ſhall have the " Length of the Side E H, known in Parts of the Axis " B C.

" After the ſame Manner, may be found the Angles " B E I, B E V, *&c.* and the Length of the Lines E I, " E V, to determine the Diſtance, from the Pole to the " Zenith, of all the Degrees of the Circumference of " the Earth ; and in the rectilinear Triangles H E I, " I E V, whoſe Sides H E, E I, E V, are known as " well as the Angles comprehended between the Sides " H E, E I, I E, E V, which are the Differences of " the Angles B E H, B E I, B E V, determin'd above ;

" one

" one ſhall find the Length of the Chords H I, I V,
" comprehended between each Degree.

Monſ. *Caſſini*, in the Memoirs for the Year 1718, repeats the ſame Demonſtration; except that, before it, he ſhews, that if ſeveral Points be taken upon a Terreſtrial Meridian, on the Surface of an Elliptick Earth, as G, H, I, K,* in ſuch Manner, that their reſpective Zeniths Z, L, M, N, are diſtant from one another, an equal Number of Degrees meaſur'd in a Celeſtial Meridian. The Lines Z G, L H, M I, N K (which are perpendicular to the Ellipſe) being produced, will meet in the Points O, R, and S, making equal Angles; but as thoſe angular Points are not equally diſtant from the Curve of the Ellipſe, that Elliptic Arc muſt be the longeſt whoſe angular Point is fartheſt off. Now, by the former Demonſtration, it appears, that thoſe Arcs, which are taken neareſt to the leſſer Axis, will have their angular Points farther remov'd, *&c.*

If Monſ. *Caſſini*'s Meaſures of Terreſtrial Degrees, decreaſing from the Æquator towards the Pole, were grounded on Obſervations liable to no Error, he wou'd have fully prov'd his Figure of the Earth. But ſince thoſe Meaſures (however accurately taken) are not built upon a mathematical Certainty, his Premiſes may be call'd in Queſtion, and his Concluſion, tho' mathematically drawn from theſe Premiſes, is only probable.

Now therefore, if I can ſhew from undoubted *Phænomena*, that his Concluſion will lead to an Abſurdity, his Meaſures muſt be falſe; becauſe his Reaſoning from them is juſt. This I ſhall endeavour to do firſt, which will diſprove his Figure of the Earth; and afterwards endeavour to point out ſome of the Errors which

I

* Fig. II.

I ſuppoſe to have occaſion'd the Miſtake in the Meaſures.

Monſ. *Caſſini*, as well as the *Engliſh* Aſtronomers, believes that the Earth makes one Revolution about its Axis, once in 23 Hours 56′, becauſe in that Time, the Plane of any Meridian returns to the ſame fix'd Star from which it had departed.

Let * H be taken in any Parallel of Latitude, as for Example, in the Latitude of 51 ° 46′, a Plumb Line, L H, will be perpendicular to the Curve B H, at H, and produced paſs thro' the Zenith of the Point H, if the Earth had no Diurnal Rotation; but ſince the Earth moves round its Axis, all Bodies upon its Surface, endeavour to fly from the Axis of their Motion with a Force proportionable to their Diſtance from it in a Direction along the Plane of that Parallel, in which they are. Let that Force (explain'd by Monſ. *Huygens*, and called a Centrifugal Force) be repreſented by the Line H *l*, or its Equal and Parallel L *h*; now a Plummet placed at L, if the Earth ſtood ſtill, would deſcend in the Line L H, but as it is at the ſame Time acted upon by the Force H *l* in the Direction L *h*, it will move in the Direction L *l* Diagonal of the Parallelogram H *l*, according to the known Laws of Mechanicks; and the Plumb Line L H, inſtead of being perpendicular to the Curve at H, will in the Latitude 51 ° 46′ make an Angle of 5′ with H L. This Angle will be leſs towards the Poles, till at the very Pole it quite vaniſhes, as it alſo does at the Æquator. *The Demonſtration of the Quantity of this Angle, I ſhall give before I end this Diſſertation.* Now ſince there is no ſuch Angle obſerv'd, but in all Water Levels we find the Plumb Line always perpendicular to the Line of Level, the Surface

Surface of the Earth muſt be depreſs'd towards G, and riſe farther from the Axis towards I, in order to become perpendicular (that is, to have its Tangent perpendicular) to the Line L *l*, in which we have ſhewn that the Plumb Line muſt deſcend.

If there is any Body ſo fond of Monſieur *Caſſini*'s Hypotheſis, as to deny the Diurnal Motion of the Earth for the ſake of it, I hope they will be convinc'd, when I ſhew the Meaſures, upon which it is founded, to be inſufficient for determining the different Lengths of the Degrees of a Terreſtrial Meridian.

But here I would not be thought to endeavour to leſſen the Praiſe due to the Gentlemen of the *Royal Academy*, for carrying on a Meridian the whole Length of *France*, from *Dunkerque* thro' the Royal Obſervatory at *Paris*, quite to the *Pyrenæan* Mountains on the Borders of *Spain*. Aſtronomy and Geography are doubtleſs much indebted to the Encouragement given by the *French* Government, and to the Care of their Mathematicians, who have omitted no proper Method for drawing their Meridian, and correcting it as they went on. So many Obſervations of the riſing and ſetting Sun, ſo many equal Altitudes of the ſame Stars accurately taken, ſo many Digreſſions of Stars, ſo many other Obſervations made with the Teleſcope and good Pendulum-Clocks — all compar'd together, for the true ſettling of the direct Way of this famous Meridian, leave no Doubt but that it is as perfect as the Nature of the Thing is capable of. And, certainly, by the Help of this Line, and the ſeveral Triangles made uſe of for carrying it on, a better Map of *France* is made, than has ever been of any Country before: Nay, beſides, I believe we may, at a Medium, very well receive their Number of 57060, or 57061 Toiſes, for the Meaſure of a Degree of a Meridian of the Earth,

one

one with another. But to ſay, that thoſe Gentlemen cou'd obſerve the Latitude ſo nicely, as to find a Difference in the Length of the Terreſtrial Degrees, and that only of eleven or twelve Toiſes, (when they made it the leaſt) or of thirty-one Toiſes, (when they made it the moſt) is attributing to them an Exactneſs, ſo far beyond the Nature of the Inſtruments which they made uſe of, that it wou'd be rather a Diſpraiſe than a Commendation to inſiſt upon it.

For in the firſt Place, the Inſtrument, with which they took Obſervations for the Latitude at the two Ends of their Meridian, was a ten Foot Sector (which was worſe than that which Monſ. *Picard* had made Uſe of before, becauſe the Teleſcope of his Sector was of ten Foot, whereas Monſ. *Caſſini*'s was but of three Foot, tho' applied to the ten Foot Sector) where the two hundreth Part of an Inch anſwers to eight Seconds of a Degree: Now the two hundredth Part of an Inch, being one of the leaſt viſible Parts that we can ſee in a divided Line; they could not take an Angle nearer than that; nay, their Inſtrument, according to their own Deſcription of it, was divided but to every twenty Seconds. Now they allow, that ſixteen Toiſes, upon the Surface of the Earth, anſwer to one Second in the Heavens; and they don't pretend to have taken an Obſervation nearer than to about three Seconds, which therefore cannot determine a Difference leſs than forty eight Toiſes; whereas the Degrees are only ſuppoſed to decreaſe at moſt, thirty one Toiſes each, from *Collioure* to *Dunkerque*. But an Error of eight Seconds wou'd make a Difference of one hundred and twenty eight Toiſes, on the Surface of the Earth; above ten Times greater than the Difference of Degrees in the firſt Suppoſition, and four Times greater than that Difference in the laſt. Beſides, the Latitude was not obſerv'd in the intermediate Places be-

between *Paris*, and *Collioure*, with the abovementiond Inſtrument of ten Foot Radius; but they made Uſe of a Quadrant, whoſe Radius was only thirty nine Inches, and ſometimes an Octant of three Foot Radius. Nay, they ſay themſelves, in their Account, that it is not the Obſervations made at the Ends of the Meridian, that we are to deduce the Difference of the Length of a Degree from, but the Altitudes taken at ſeveral Places between the Extreams; and, if we grant, that they can take an Angle very well, to four or five Seconds, with the great Inſtrument, they cannot come nearer than twelve or fifteen Seconds, with the Quadrant or Octant, which we muſt depend upon for the Difference of the Meaſure of Degrees: So that upon the whole, we are to determine a Length of thirty one Toiſes, by an Inſtrument which is liable to err above two hundred.

If any Conſequences of this Kind cou'd be drawn from actual meaſuring, a Degree of Latitude ſhou'd be meaſur'd at the Æquator, and a Degree of Longitude likewiſe meaſur'd there; and a Degree very northerly, as for Example, a whole Degree might be actually meaſur'd upon the *Baltick* Sea, when frozen, in the Latitude of ſixty Degrees.—— There, according to Monſ. *Caſſini*'s laſt Suppoſition, a Degree wou'd be of 56653 Toiſes, whereas, at the Æquator, it wou'd be of 58019 Toiſes, the Difference being 1364 Toiſes, about the two and fortieth Part of a Degree, which muſt be ſenſible; and likewiſe the Degree of Longitude wou'd, according to him, be of 56817 Toiſes, leſs by 1202, or the forty eighth Part, than a Degree of Latitude at the ſame Place.

But here it may be objected, that tho' the Latitude was not taken with the ten Foot Sector, in the intermediate Places between *Paris* and *Collioure*, yet the

Latitude was taken with that Inſtrument at *Dunkerque*, *Paris*, and *Collioure*, and therefore the ſouthern Part of the Meridian, containing 6° 18′ 56″ may be compar'd with the northern Part of it, which contains 2° 12′ 16″; and that the former appears to contain more Toiſes, in Proportion to the Difference of Latitude at its Extremities, than the latter. To this may be anſwer'd, that, even in this Caſe, the Obſervations made cannot be nice enough to determine the Difference of the Length of Degrees; but there is another Error, which might conſiderably miſlead the *French* Gentlemen, and make the Degrees appear longer in the South of *France*; that is, the Error in taking the true Height of ſeveral Mountains in *Auvergne*, *Languedoc*, and among the *Pyræneans*. For if they have allow'd too much for the Air's Refraction (which, by the Obſervations of Travellers, is greater towards the northern Regions, and diminiſhes as we go Southward) the Heights of thoſe Mountains will be taken too little, and their Baſes conſequently longer, which will make the Degrees appear bigger than they are. Let A B C D, * for Example, be a Mountain, as the Mountain of *Rodez*, in the Latitude of 44° 21′, whoſe Height B D is 300 Toiſes, and whoſe Sides A B and B C (ſuppos'd to make an Angle of 26° 33′, with the Horizon) are found by Trigonometry, to be of 670 8 Toiſes each; if by a Miſtake, in taking the Height, it be ſuppos'd only equal to E D, or 257 Toiſes, then the Lines A B and B C will become E F and E G; ſo that the Baſe A C, which before was of 1200 Toiſes, will become equal to F G, which will appear to be 1279,6 Toiſes, by *Eucl.* 47. 1. Now one ſuch Miſtake, in one Degree, will give a Difference above twice as great as the ſuppos'd Difference

* Fig. III.

rence of Degrees in that Latitude, which they make of 31 Toiſes. And that there was a Miſtake of this Kind in taking the Height of that Mountain, I ſhall ſhew.

The Vapours, that generally float in the Air about the Tops of high Hills, make it ſo difficult to take their Height exactly, that Experiments, made with the Barometer, will, by obſerving the Fall of the *Mercury*, ſhew the Height nearer than any Thing elſe we know of. There were, indeed, ſeveral Experiments made with the Barometer,* where the Differences of the Height of the *Mercury*, from the Heights at which it ſtood at the *Royal Obſervatory*, are ſaid to anſwer to ſo many Toiſes; but of nine Obſervations mention'd by Monſ. *Caſſini*, there are not two where the Number of Toiſes, ſaid to correſpond to the Heights of the Barometer, do agree together.

The firſt Experiment of the Barometer there mention'd, made at *Collioure*, was this, "At the Height "of $11\frac{1}{2}$ Toiſes above the Sea, the Barometer was ſet "up, and the *Mercury* ſtood $3\frac{1}{3}$ Lines higher than at "the *Royal Obſervatory* (in the Tower of the eaſtern "Hall) at the ſame Time; and therefore, ſince that "Tower is 44 Toiſes higher than the Sea, $3\frac{1}{3}$ Lines "of *Mercury* muſt anſwer to $32\frac{1}{2}$ Toiſes.

Now, reducing theſe Toiſes to Feet, and dividing by $3\frac{1}{3}$ it will appear that an Height of 58,5 Feet will anſwer to the Fall of one Line of *Mercury* in the Barometer. Let this be taken as the Standard, and the other Obſervations be compar'd with it. This may be done by the following Table, where the firſt Column ſhews the Place where the Obſervation was made; the ſecond, the Fall or Riſe of *Mercury* at each Place expreſs'd in Lines, or 12th Parts of a *French* Inch; the

* *Mem. of the* Royal Academy, *for* 1718, ch. 10.

third, the Heights or Depths anſwering to thoſe Lines of *Mercury*, which, in the Memoirs, are given in Toiſes, but here reduced to Feet; the fourth, the Number of Feet anſwering to one Line of *Mercury* in each Obſervation, which is the Quotient of the Feet in the third Column, divided by the Number of Lines in the Second.

Obſervations of the Barometer made at	*Lines of* Mercury.	*Said to correſpond with Feet.*	*The Fall of one Line of* Mercury *anſwers to Feet.*
I. Collioure - -	03 $\frac{1}{3}$	195	58,5
II. *The Tower of* Maſſane.	31	2382	76,8
III. Bugarac -	42	3636	86,5
IV. Rupeyroux -	30	2181	72,7
V. Rodez -	24	1647	68,6
VI. Rodez -	20	1425	71,25
VII. Courlande -	54	4812	89,1
VIII. Coſte -	54	4890	92,4
IX. Clermont -	03	200	66,6

A Sight of this Table will convince any one, that theſe Obſervations are not to be depended upon for determining the Height of the Mountains in the South of *France*; for the Differences are not ſmall, ſuch as might happen in making the Experiments; but ſuch as render the Obſervations uſeleſs for the Purpoſes abovemention'd. For Example, the firſt and the ſeventh differ almoſt $\frac{1}{3}$: And if 58,5 Feet were allow'd for the Fall of one Line of *Mercury* in the ſeventh Obſervation, inſtead of 944 Feet, then the Mountain of *Coſte* would be but 3085 Feet, inſtead of 4890. Nay, upon examining the Memoirs, I find that in ſeveral Obſervations the Number of Toiſes, ſaid to correſpond to a certain Height of *Mercury*, are only anſwerable to the Height of the Mountain above the Level of the Sea found by Trigonometry, from which the Height of the *Royal Obſervatory*, above the Sea, is ſubſtracted; though, by the Manner of the Expreſſion, a curſory Reader would imagine

imagine, that the Number of Toiſes nam'd, was always proportionable to the Fall of the *Mercury*, and think all the Experiments and Obſervations very accurately made, when they ſeem to agree ſo well in every Reſpect.

Now after all, I do not queſtion but that the Height of the Barometer, might be as it is ſet down in the Memoirs, and well enough obſerv'd; but it was wrong to compare the Height of the *Mercury* in the South of *France*, with the Height that the *Mercury* was at in the Barometer of the *Royal Obſervatory* at the ſame Time; for, at that great Diſtance and Difference of Latitude, the Weather (and conſequently the Preſſure of the Air and Height of the Barometer at the ſame Level) might very much vary.

Even when there is fair Weather all over *France*, it does not follow that the Barometer ſhall ſtand at the ſame Height. Let us ſuppoſe, for Example, that a North Wind blows: Where-ever the Air is check'd by a Chain of Mountains that run Eaſt and Weſt, it will be accumulated over thoſe Mountains, and conſequently preſs more as its Columns are higher; which will make the *Mercury* riſe higher than it wou'd do with the ſame Wind, if there were no Mountains, or if they ran North and South.

The Way, to have made the Experiments with the Barometer exactly, wou'd have been to have obſerv'd the Height of the *Mercury* at the Bottom and at the Top of the Mountain, and that with a Tube of a pretty large Bore (with a proportionably large Ciſtern for the ſtagnant *Mercury*) becauſe, in a ſmall Tube, the *Mercury* will often ſtick to the Sides, and riſe irregularly, as it will alſo in inclin'd Barometers. Simple Barometers are the beſt, and a magnifying Glaſs may be made uſe

of to obſerve ſmall Riſes or Falls, having two fine and well made Indices to the Tube.

Dr. *Edmund Halley*, our Royal Aſtronomer, has, ſome Years ago, given us, in the *Philoſophical Tranſactions*, the Falls of *Mercury* in the Barometer, correſponding with the Heights to which the Barometer muſt be carried to produce thoſe Falls. The firſt tenth Part of an Inch in the Fall of the *Mercury*, he makes to anſwer to an Height of 90 Feet; the next tenth, to an Height ſomething greater, and ſo in Proportion, as the Air diminiſhes in Denſity, according as we riſe in Height. The Proportion of the firſt Tenth of the *Mercury*'s Fall, he has built upon the Compariſon of the different Specifick Gravities of Air and *Mercury*; and taking *Mercury* to be $13\frac{1}{2}$ Times heavier than Water, and Water (in cold Weather) to be 800 Times heavier than Air; it follows, that $13,5 \times 800$, will give 10800; which Number, if it be taken in Feet, and divided by 120 (the Number of the 10th of an Inch in a Foot) we ſhall have 90 Feet anſwerable to the 10th of an Inch, and 75 Feet to a Line or the 12th Part of an Inch. As the Doctor's Tables may be of uſe to the Curious, I have inſerted them here.

Nor

I. *A TABLE shewing the Altitude, to given Heights of the* Mercury.

Inches.			
30		0	Feet.
29		915	
28		1862	
27		2844	
26		3863	
25		4922	
20		10947	
15		18715	
10		29662	
5		48378	
1		91831	
0 . 5		110547	
0 . 25		129262	
0 . 1	29 Miles or	154000	
0 . 01	41 Miles or	216169	
0 . 001	53 Miles or	278338	

II. *A*

II. *A TABLE shewing the Heights of the* Mercury, *at given Altitudes.*

Feet.			Inches and Tenths.
0	30	00	
1000	28	91	
2000	27	86	
3000	26	85	
4000	25	87	
5000	24	93	
1 Mile	24	67	
2	20	29	
3	16	68	
4	13	72	
5	11	28	
10	4	24	
15	1	60	
20	0	95	
25	0	23	
30	0	08	
40	0	012	

Now, as very few Mountains in the World are 3 Miles high, and, generally ſpeaking, thoſe that we look upon as high Hills, (except the *Andes*, and ſome others in *America*) are not much above a Mile high; we may, for finding the Height of Mountains, take a fix'd Number of Feet in Altitude to anſwer to every 10th or 12th of an Inch in the Fall of the *Mercury*; becauſe 90 Feet are by Dr. *Halley* only taken for the firſt Tenth, and greater Heights for other Tenths, encreaſing with the Fall of the *Mercury*. Therefore I wou'd propoſe another for a round Number, namely 96 Feet for every Tenth, and 80 Feet for every 12th of an Inch, very near the Number that I have found by my Calculation, which is as follows.

Fine

Fine *Mercury* (ſuch as is made uſe of in Barometers) is, generally ſpeaking, $13\frac{1}{3}$ times heavier than Water; and, I found ſome brought from the *Eaſt-Indies*, to be 14 times heavier. I have found Air in Summer, to be near 900 times lighter than Water; and 800 times in Winter; therefore I take 850 at a Medium. Now $850 \times 13\frac{1}{3} = 11606{,}6$, which, divided by 120, gives 96,7 Feet, for $\frac{1}{10}$ of an Inch of *Mercury*, or 80,5 Feet for $\frac{1}{12}$ of an Inch. This Number, taken invariable, will, in taking the Height of ſeveral Hills, agree pretty well with the Numbers that come out, when Dr. *Halley*'s Table is made uſe of; and with the Experiment made by the late Profeſſor, Mr. *J. Caſwell*, who, having taken the Height of *Snowdon* Hill in *Caernarvanſhire* very accurately, and finding it to be 3720 Feet above the Level of the Sea, tried how much lower the *Mercury* wou'd ſtand in the Barometer upon that Hill, than at the Level of the Sea, and obſerv'd it to ſubſide 3,9 Inches. I am ſenſible that it will be alledged, that the Air will be denſer than I may imagine on the Top of high Hills, becauſe of the great Cold, ſince they are generally cover'd with Snow; but then we are to conſider, that when we are got above a Mile higher than the Level of the Sea, the incumbent Atmoſphere has loſt almoſt a 5th Part of its Weight *; and therefore the Air at the Top of the Hill, being ſo much leſs preſs'd, will, notwithſtanding the intenſe Cold, be more rarified than at the Bottom of the Hill.

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* *If the whole Air was reduced to the ſame Denſity, as it is near the Level of the Sea, the Atmoſphere wou'd be but between five and ſix Miles high; whereas, in its preſent State of gradual Rarefaction, it is above* 50 *Miles high, as we find by ſeveral* Phænomena *of Meteors obſerv'd to float in the Air, ſo high at leaſt.*

Now if we go back to the Obſervations of the Barometer, made by the Gentlemen that drew the Meridian in *France*, we ſhall find, that on the Mountain of *Rhodez*, in the Latitude of 44°, 21′ the Barometer fell 24 Lines below the Level of that in the Obſervatory, and they allow'd only 274½ Toiſes to correſpond to that Fall; whereas, according to Dr. *Halley*'s Proportion of a Tenth of an Inch for 90 Feet, they ſhou'd have taken 300 Toiſes; and tho' the *Hypotenuſes* A B, and B C, * were taken longer than the bare Declivity of the Mountain (which wou'd make the Error leſs than the 79 Toiſes I mention'd above) yet if my Proportion be made uſe of, *viz.* of 80 Feet for each Line of *Mercury*, that will make the Mountain 320 Toiſes, which, being higher, will therefore ſhew the Baſe to be yet ſhorter, and conſequently the Error, at that Rate, will be greater.

This Error (and ſuch like, if any more were made) will encreaſe the Meaſure of the 44th Degree of Latitude on the Earth; and, by obſerving what was done in the next Degree, we ſhall find that that Degree was taken too ſhort. In the Latitude of 45°, 38′, the Mountain of *Coſte* is made 815 Toiſes high; whereas the 54 Lines of the falling *Mercury* in the Barometer, ſaid to anſwer to that Height, will give but 705,6 Toiſes (which we will call 705,5) even according to my Computation of 80 Feet to a Line, which is the greateſt Allowance. If we ſuppoſe this Mountain to riſe in an Angle of 26°, 33′, as we did that of *Rhodez*, the Sides of the Mountain, or *Hypotenuſes* A B, and B C, † will be each equal to 1577,54 Toiſes, and the whole Baſe A C, to 2822 Toiſes. Now, when the Height of this Mountain is call'd 815 Toiſes, the Baſe A D, or D C (*by*

* Fig. III. † Fig. IV.

(*by Eucl.* 47. 1.) becomes only equal to F D or D G = 1350,7 Toiſes; and its Double, or F G the whole Baſe, will be but 2701,4 Toiſes, leſs than the former by 120,6 Toiſes. This Error is ſo great (ſo much more than the Difference of 31 Toiſes for a Degree) that tho' I ſuppos'd the Lines found by Trigonometry, which terminate at the Top of the Mountain, to be much longer than the Hypotenuſe A B, yet there will be Error enough to make the 45th Degree of Latitude appear much ſhorter than it is. Suppoſing (becauſe of the Length of the Lines A B, or the great Diſtance from which the Mountain might be obſerv'd) that theſe Errors were four times leſs than I made them; yet, at that Rate, one muſt add near 20 Toiſes to the 44th Degree of Latitude, and take away above 30 from the 45th Degree, which will make the 44th of 57080 Toiſes, and the 45th, of only 57030; and this will give a Difference of 50 Toiſes; ſo that if an Angle can be taken to two or three Seconds, to which 32, or 48 Toiſes, are ſaid to anſwer upon the Surface of the Earth, ſuch a Difference might be viſible.—— And much more ſo, if other Errors of the ſame Kind ſhou'd happen to have been made the ſame Way; or if thoſe Errors were nearer my firſt Suppoſition than this laſt. Nay, tho' the 45th Degree of Latitude may be 13 Toiſes bigger than the 44th, it might by this Means appear to be conſiderably leſs.

Such a Miſtake might be the Occaſion of making the Hypotheſis of the Earth being an oblong Spheroid, eſpecially becauſe in this Hypotheſis, the Degrees differ moſt in Length from one another about the 45th Degree; and, when once an Hypotheſis is ſet on Foot, we are too apt to draw in Circumſtances to confirm it; tho', perhaps, when examin'd impartially, they may rather weaken, than ſtrengthen our Hypotheſis; otherwiſe, the Author of the Hiſtory of the *Royal Academy*, for the

1713, wou'd not have alledg'd, that *the late Monſ.* Caſſini *obſerv'd* Jupiter *to be oval*, as a Proof of young Monſ. *Caſſini*'s Hypotheſis; becauſe *Jupiter* is oval the other Way, that is, an oblate Spheroid flatted at the Poles, as the ſaid late Monſ. *Caſſini* gave the Proportion of the Axis, to the Æquatorial Diameter, to be as 15 to 16.——— And our late ingenious Aſtronomer, the Reverend Mr. *Pound*, with a Teleſcope of 123 Foot *Focus*, and an excellent Micrometer, has given thoſe Proportions as 11 to 12. If a Proof is to be drawn from Analogy, or what is obſerv'd in other Planets, this muſt deſtroy Monſ. *Caſſini*'s Hypotheſis, and confirm Sir *Iſaac Newton*'s.

The Opinion of Dr. *Burnet* (the Theoriſt) quoted in the Memoirs, for the Year 1713, is but a very weak Argument in favour of Monſ. *Caſſini*'s Hypotheſis, on Account of the Reaſon Dr. *Burnet* gave, to prove the Earth higher at the Poles, than the Æquator; for he ſays, " That the Velocity of the Parts of the Earth, " in its Diurnal Rotation, being greater at the Æqua- " tor than towards the Poles, all the Water muſt be " driven towards the Æquatorial Regions; from " whence, being repelled by the Reſiſtance of the " Air, it muſt run off again towards the Poles; and " ſo the Figure of the Water was lengthen'd out into an " oblong Spheroid, and conſequently the Cruſt of the " Earth over it did put on the ſame Figure, *&c.*

But why the Air ſhou'd reſiſt more towards the Æquator than the Poles, the Doctor did not give any Reaſon to ſhew; and, if it had been ſo, the ſame Force, that drove the Water towards the Æquator, muſt have kept it there. The Doctor, in the latter Part of his Aſſertion, forgot what he had ſaid in the former; for the Water cou'd not run off towards the Poles, whilſt the Earth continued its Rotation with the ſame Velocity.

For

For if he had conſider'd, he wou'd have found his Argument in other Words to be this. *Becauſe Bodies, that move in a Circle, always endeavour to recede from the Axis of their Motion; therefore the Water, by that Endeavour, comes nearer to the Axis of its Motion*; which is abſurd. But Dr. *Burnet*, afterwards, alter'd his Opinion, as I am credibly inform'd.

Having thus given my Reaſons for diſapproving of Monſ. *Caſſini*'s Opinion, concerning the Figure of the Earth; I come now to conſider Sir *Iſaac Newton*'s, who makes it higher at the Æquator, than at the Poles; but before I enter upon it, I beg Leave to quote a Paragraph out of the Hiſtory of the *Royal Academy* for 1713. Theſe are the Words of the Author. "Reaſonings drawn from the different Lengths of a Pendulum in different Climates, or from the Inequality of the Centrifugal Force ariſing from the Diurnal Motion of the Earth, are, perhaps, too nice to produce a certain Conviction; nay, perhaps, we are not well enough aſſured of the Principles, and the Conſequences may ſometimes be different. And therefore it is evident, that the beſt Way in this Enquiry, is only (as Monſ. *Caſſini* does) to make uſe of unqueſtion'd Obſervations, which ſerve directly to decide the Queſtion.

That Monſ. *Caſſini* has not made uſe of *unqueſtion'd Obſervations*, and the Meaſures, he mentions, are not able to decide the Queſtion, appears from what I have already ſaid. We muſt therefore ſhew, whether the Principles, from which Sir *Iſaac Newton* has deduced his Figure of the Earth, are fully prov'd or not: Whether the Concluſion drawn from them is plain and evident; and whether the Experiments on Pendulums, that confirm the Theory, are eaſy to be made, and may be depended upon.

Tho'

Tho' Sir *Iſaac Newton*, in his *Principia*, has not endeavour'd to give the Cauſe of Gravity, or to determine whether it be owing to an Impulſe or not; yet he has ſhewn what its Effects and Laws are, from plain Experiments made by others and himſelf. From the Laws of Gravity, and from the Obſervation of a Comet, * he has deduced the Annual Motion of the Earth; and it muſt have a Diurnal Motion, if it has an Annual one, otherwiſe, it will not agree with the *Phænomena*. The Laws of the Centrifugal Force, or that Force by which a Body, whirl'd round in any Circle, endeavours to recede from the Center of its Motion, have been demonſtrated by Monſ. *Huygens*.

Theſe are the Principles from which Sir *Iſaac Newton* draws his Concluſion; and tho' ſome Perſons, that will not be at the Pains to examine them, may deny them by the Lump, yet no Body has yet been able to ſhew any Flaw in the Demonſtrations that relate to them.

NB. *This Diſſertation will be continued in our next.*

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* *Princip. Lib.* 3. *Prop.* 12, 13, & 42.

II. *Some*

II. *Some Obſervations made in an* Oſtrich, *diſſected by Order of Sir* Hans Sloane, *Bart. By Mr.* John Ranby, *Surgeon. F. R. S.*

HAving ſeparated the Muſcles of the *Abdomen*, which in this Subject were only two oblique Pair, we obſerv'd, between their *Tendons*, which were very ſtrong, and the *Peritonæum*, which was exceeding thin, a thick Layer of ſevous Fat, whoſe Office, conſidering the Smallneſs of the *Epiploon*, and the few adipoſe Veſicles of the *Meſentery*, with the Thinneſs of the *Peritonæum*, might probably be to ſupply the Part both of *Epiploon* and *Meſentery* in other Animals, as to lubricating the Inteſtines.

There were, in our Subject, two diſtinct Ventricles, contrary to the Obſervation of the *Royal Academy* at *Paris*. The firſt, and in its natural Situation, the lower, which the Members of the ſaid Academy, call the Craw, and ſuppoſe to be only a Dilatation of the *Oeſophagus*, was conſiderably larger than the ſecond, and uppermoſt Muſcular One; beſides, that it had ſtrong Muſcular Fibres, both circular and longitudinal: The *Duodenum* comes immediately out of the ſecond Ventricle.

Both Ventricles were diſtended beyond their uſual Form, and fill'd up with ſo large a Quantity of Food of different Kinds, as Stones, Bones, Sticks, Grain, and other Food, that it was almoſt impoſſible for them to perform their Office of Digeſtion, which very likely was one of the chief Cauſes of the Animal's Sickneſs and Death; and, really, the Contents of both ſeem'd to have undergone but very little or no Alteration.

The

The *Epiploon* partly cover'd the firſt Ventricle, but was no Ways proportionable to the Size of the Animal.

The Spleen was faſten'd, by a Membrane, to the right Side of the ſecond Ventricle, and was very ſmall, conſidering the Size of the Animal.

The Glands of the *Meſentery* were hardly viſible, but the Veins and Arteries very conſpicuous.

The *Cæcums*, in our Subject, were near three Foot in Length, the Diameter one Inch eight Lines; they were faſten'd to the *Ileum*, and not to the *Colon*, as the Gentlemen of the *Royal Academy* aſſert.

To their Deſcription of the Kidneys, I have nothing to add, except that the two *Ureters* lay upon their Surface, as they do in other Birds, and that their different Branches, coming from all the Parts of the Kidney, of which the ſuperior was very conſpicuous, enter'd the Kidney about its Middle, and form'd there a very large *Pelvis*.

The Liver was in one Cavity with the Heart, of which it cover'd near one half; it had no Gall Bladder, and but one *Ductus Bilarius* inſerted into the *Duodenum*, about two Inches below the *Pylorus*, which ſeem'd to have an immediate Communication with the *Vena Portæ*, becauſe, by blowing into it, this latter was alſo diſtended. The Heart and Liver were ſeparated from the Inteſtines, by a membranous *Diaphragm*.

Both Heart and Liver were ſuſpended by one common *Mediaſtinum*, by the Help of its ſeveral Membranes, and eight ſtrong Muſcles on each Side, ariſing from the upper Part of the Ribs, going from thence over the Lungs, and ending in a very ſtrong Tendinous Membrane, which is inſerted into the *Spina Dorſi*.

The

The Liquor, contain'd in the *Pericardium*, was ſmall in Quantity, and perfectly tranſparent.

The Lungs lay under the *Diaphragm* and its Muſcles, in a deep Cavity, form'd by the five true Ribs. They were pretty thick about the middle, and exceeding thin and ſharp towards the Extremities.

In viewing the Eye external, it did ſomewhat reſemble the Human Eye, except that it was leſs Convex, with a free and moveable upper Eye-lid, with Eye-laſhes, as moſt Terreſtrial Animals have, beſides a *Tunica Nictitans*, as in other Birds. Beſides the ſeven Muſcles of the Eye, as they are in Brutes, it had two more, one ariſing from the fore-part of the *Sclerotica*, which ſoon form'd a ſmall *Tendon*, obliquely ſurrounding the Optic Nerve, and then join'd to another Muſcle, which ariſes oppoſite to the former, from which the *Tendon* continues its Way, and is inſerted in the *Tunica Nictitans*. The *Aqueous* Humour we found in greater Quantity, than is common. The *Cryſtalline* was of an uniform Subſtance, but leſs Convex on the inſide, then without. The *Vitreous* was ſmall in Quantity, conſidering the Largeneſs of the Eye; the *Choroides* was entirely black, without that Variety of Colours at its Bottom, which is common to moſt Brutes. The fore-part of the *Sclerotica*, where it is annexed to the *Cornea*, was bony, conſiſting of 15 bony Scales join'd one to another, ſo as to make one circular Bone round the *Cornea*.

As for a more particular Deſcription, I refer to the Anatomical Account given by the *Royal Academy* at *Paris*, in their *Natural Hiſtory of Animals*, and to *Valliſneri*, Profeſſor at *Padua*, his *Notomia del Struthio*.

References to the Figures.

Fig. V.

Shews the upper Part of the *Thorax*, the *Sternum* being remov'd, with the Heart and Liver and neighb'ring Parts, in their natural Situation.

A A. The Membranous *Diaphragm*, in which are obſerv'd ſeveral diſtinct Cavities.

a a a. The Ligament that ſuſpends the *Diaphragm.*

b b. The Ribs.

B. The Heart. C C. The two Lobes of the Liver immediately above the Heart.

c c. The Brachial Artery. *d.* The Vein.

e e. Vena Cava.

f. A *Gland*, on the Brachial Artery.

g g. Part of the *Aſpera Arteria.*

h h. Part of the *Oeſophagus.*

i i. Two Muſcles ariſing from the *Sternum*, and inſerted into the *Aſpera Arteria.*

Fig. VI.

The inferior Part of the *Thorax*, the Heart and Liver being remov'd.

A A A. The lower Part of the *Diaphragm*, immediately covering the Lungs.

B B, *&c.* Eight ſtrong fleſhy Muſcles ariſing from the Ribs, and inſerted into the *Diaphragm*, forming a Cavity for the Heart and Liver.

c c c. The Ribs. D. The deſcending Trunk of the *Aorta.*

E E. The left Lobe of the Lungs freed from the *Diaphragm.*

F. Part

F. Part of the *Aſpera Arteria.*

Fig. VII.

Part of the Globe of the Eye.

a. The *Cornea.* *b b.* The Ciliary Ligament.
c c c. The fore-part of the *Sclerotica*, compos'd of 15 bony Scales.

Fig. VIII.

The back-part of the Globe.

a a a. The back-part of the *Sclerotica.*
b b b, &c. The ſeven Muſcles.
c c c. The eighth and ninth; the *Tendon* of which (*d d*) goes round the Optic Nerve, *f*, and is inſerted into the *Tunica Nictitans.*
e e. Membrana Nictitans.

Fig. IX.

The Kidneys with their Veſſels.
A A The Kidneys.
B B. *Aorta deſcendens.*
C C. *Vena Cava.*
D D. The Emulgent Arteries.
E E. The Emulgent Vein with its Ramifications.
F F. The Ureters.
G. The Union of the ſuperior and inferior Ureter.

III. *An Account of the Appearance of* Mercury, *passing over the Sun's Disk, on the 29th of* October, 1723. *determining the mean Motion, and fixing the Nodes of that Planet's Orb.* By Edmund Halley, *LL. D. Astron. Reg. and R. S. S.*

THE Transit of the Planet *Mercury*, over the Disk of the *Sun*, being one of the most curious and uncommon Appearances that the Heavens afford, our Astronomers, both at home and abroad, made due Preparation to observe, with the utmost Exactness, that which happened on the 29th of *October*, 1723, which I had predicted in the Year 1691 (*Phil. Transf.* N° 193.) would be, in Part, visible in *England*. And the Sky proving, more than ordinary, favourable at that Time, we were enabled to observe the Ingress on the *Sun*'s Limb, with the greatest Accuracy.

Accordingly, the same Day, *Octob.* 29. *styl. vet.* at *Greenwich* in the *Royal Observatory*, I first perceiv'd, with my 24 Foot Tube, the *Planet* making a small Notch in the *Sun*'s Limb at $2^h\ 41'\ 23''$ *T. app.* And at $2^h\ 42'\ 26''$ he was wholly enter'd, making an interiour Contact, the Light of the *Sun*'s Limb just beginning to appear behind his dark Body; which, notwithstanding the Slowness of the Motion, was, in a Manner, instantaneous. Then, applying the *Micrometer* to the said 24 Foot Tube, I open'd it so as to take in $16'\ 15''$ equal to the *Sun*'s Semidiameter at that time; and causing the northern Edge of the *Sun*, to move exactly along one of the Pointers, I waited till the Center of

Mer-

Mercury came to move along the other, as I found it to do at $3^h\ 1'\ 16''$ *T. app.* But *Refraction* contracting this Difference of Declination about 5 Seconds (the *Sun* being then but about 11° high) I concluded that the Centers of the *Sun* and *Mercury*, were truly in the ſame Parallel of Declination at $3^h\ 3'$ *T. app. proxime.*

At *Wanſted* in *Eſſex*, my worthy Collegue, the Rev. Mr. *James Bradly*, *Savilian* Profeſſor of *Aſtronomy*, obſerv'd with the *Hugenian Teleſcope*, of above 120 Foot long, the total Immerſion, or interiour Contact of the Limbs, at $2^h\ 26'\ 45''$ *T. æq.* that is $2^h\ 42'\ 38''$ *T. app.* twelve Seconds later than I found it at *Greenwich*; moſt of this Difference being due to the Difference of our Meridians. And applying the *Micrometer* to that vaſt *Radius*, he meaſured the Diameter of the *Planet* $10''\ 45'''$. At $2^h\ 48'\ 57''$ he found the Difference of Declination between the ſouthern Limbs of the *Sun* and *Planet* by the *Micrometer*, in a fifteen Foot Tube, to be $15'\ 19''$. Wherefore, allowing the obſerv'd Semidiameter of the *Planet*, and the *Refraction*, the ſaid Difference was neareſt $15'\ 30''$, and conſequently, *Mercury* more ſoutherly than the *Sun*'s Center in reſpect of Declination $0'\ 45''$.

Mr. *George Graham*, in *Fleet-ſtreet*, *London*, obſerv'd the firſt Impreſſion on the *Sun*'s Limb at $2^h\ 41'\ 9''$ *T. app.* and at $2^h\ 42'\ 19''$ *Mercury* was intirely within the Disk. At $3^h\ 6'\ 41''$ he meaſur'd with a *Micrometer*, in a twelve Foot Tube, the Diſtance of his Center from the neareſt Limb of the *Sun* $2'\ 13''$. And again, at $3^h\ 25'\ 24''$ their Diſtance was found $3'\ 57''$. At $3^h\ 34'\ 43''$ he meaſur'd the Difference of Declination, from the northern Limb of the *Sun* $14'\ 57''$, which, corrected by *Refraction*, becomes $15'\ 4''$, that is, $1'\ 11''$ more northerly than the *Sun*'s Center.

In

In the *Observatory* at *Paris*, *Signor Maraldi* observed the first Appearance of *Mercury* on the *Sun's* Limb at $2^h\ 50'\ 13''$ *T. app.* and the interiour Contact at $2^h\ 51'\ 48''$. And Mr. *de Lisle*, observing a-part, concluded the same at $2^h\ 51'\ 37''$, but suspects it might have been some few Seconds later. This Gentleman has communicated his Observation at large, from whence we shall only borrow the following observed Latitudes.

h	′	″		′	″
At 2	56	20	*Latitudo Borea Mercurii*	3	36
3	00	40		3	42
3	10	20		3	46
3	16	12		3	55

At *Bononia*, in *Italy*, Signor *Manfredi* observed *Mercury* indenting the *Sun*'s Limb at $3^h\ 26'\ 22''$; and that he was gotten entirely within, at $3^h\ 27'\ 45''$. And these are the Observations most to be depended on, that we have receiv'd from abroad.

In order to deduce from this *Phænomenon*, so accurately observ'd, what may contribute to the Perfectioning of the Theory of *Mercury*'s Motion, which (as appears by the near Agreement of our Numbers with this and many other Observations of him) seems to need but very little Correction; I carefully computed, from our Tables, the Motion of the Planet in five Hours, and found his apparent Motion on the *Sun*, to be in Longitude $29'\ 21''$ Retrograde, and that his Latitude encreas'd northerly $4'\ 17\frac{1}{2}''$ in the same time; whence the Horary Motion in Longitude $5'\ 52''$, and in Latitude $0'\ 51\frac{1}{2}''$, and thence the Angle of the visible Way with the Ecliptick $8^\circ\ 19'$, and the Horary Motion in that Way $5'\ 56''$. Again, the Angle of the Ecliptick with

with the Meridian, being in this Place 73° 24′, the viſible Way of *Mercury*, made an Angle of 65° 51′ with the Meridian paſſing through the Center of the *Sun*, whence the Horary Change of Declination becomes exactly 2′ 30″.

Theſe *Data* I chooſe rather to take from the Theory, than from immediate Obſervation; becauſe there is always an unavoidable, tho' ſmall Uncertainty, in what we obſerve, yet greater than there can be in the Computation for ſo ſmall a Space of Time, eſpecially now the Theory is, as I ſaid before, ſo very near the Truth.

This premiſed, let us now enquire the true Time of the central Ingreſs, and the Latitude of the Planet at that Time. And firſt, by my own Account, *Mercury* was gotten into the Parallel of the *Sun*'s Center, 21½ Minutes after the central Ingreſs, in which Time he aſcended to the *Northward* 0′ 54″, and ſo much, therefore, was he more *Southerly* than the *Sun*'s Center at his Ingreſs. Mr. *Bradly*, 7½ Minutes after the ſaid Ingreſs, in which the Planet aſcended 0′ 19″, found his Declination 0′ 45″ South, and therefore at the Ingreſs, his Declination was 1′ 4″ South. And by by Mr. *Graham*'s Obſervation, *Mercury* was more northerly than the *Sun*'s Center 1′ 11″, 53′ 20″ after the central Ingreſs; but in that Time, *Mercury* aſcended 2′ 13″, wherefore, according to him, at the Ingreſs the Planet had 1′ 2″ South Declination. We ſhall not therefore err above a Semidiameter of *Mercury*, if we aſſume his Declination, at that Time, to have been preciſely one Minute.

Now the *Sun*'s Semidiameter being then 16′ 15″, one Minute is the *Sine* of 3° 32′ in the Arch of the *Sun*'s Limb; and conſequently, the Point of this Ingreſs was 13° 4′ more northerly than the Ecliptick; whence the Latitude of *Mercury* was then 3′ 40″ North, and

Dif-

Difference of Longitude 15′ 50″, by how much he, at that Time, follow'd the *Sun*'s Center.

If therefore, to the Arch of 13° 4′, we add the Double of 8° 19′, or of the Angle which the viſible Way made with the Ecliptick, we ſhall have 29° 42′ for the Point on the *Sun*'s weſtern Limb, at which the Planet made his *Exit*, likewiſe to the North of the *Ecliptick*. Hence the Chord, deſcrib'd in the whole Tranſit, was of 137° 14′, and the Chord itſelf 30′ 16″; and the neareſt Diſtance to the *Sun*'s Center 5′ 56″. Now the Horary Motion in this Chord, being 5′ 56″, the whole Duration of this *Mercurial Eclipſe* becomes 5^h 6′ in reſpect of the Center of the *Planet*; and therefore the neareſt Approach of their Centers was at 5^h 14′ 30″ at *Greenwich*, and the *Exit* at 7^h 47′½ both viſible in our *American Plantations*, had there been any curious Perſon there qualified to obſerve them.

It follows likewiſe, by the obſerv'd Diameter of *Mercury*, 10″ 45‴ that he was very little leſs than two Minutes of Time in paſſing the Limb; and, by the given neareſt Diſtance to the *Sun*'s Center, it is concluded that he was in Conjunction, in Point of Longitude, at 5_h 23′ 15″ having then preciſely 6′ 00″ North Latitude. Nor can it be doubted, but that all this would have been found exceeding near to Truth, had not the too early ſetting of the *Sun* deprived all *Europe* of the deſirable Sight.

There being a very remarkable *Period* of the Motion of *Mercury* in 46 Years, in which Time, he makes 191 Revolutions about the *Sun*; this Tranſit of ours is found to have been preceded by two others at that Interval: The firſt, in the Year 1631, when *Gaſſendus* at *Paris*, on the 28th Day of *October*, *ſtyl. vet.* was the firſt that ever obſerv'd this Appearance of *Mercury* within the *Sun*'s Disk, and found him to paſs off

at

at 10^h 28′ *mane*. The ſecond was, *Octob.* 28° 1677, when myſelf had the good Fortune to obſerve both the Ingreſs and Egreſs of the Planet in the Iſland of *St. Helena*; the middle Time, when he was neareſt to the *Sun*'s Center, being there but 3′ 50″ paſt Noon, and the viſible Duration of the Tranſit of the Center of the Planet 5^h 14′ 20″; which was ſome ſmall matter contracted by Parallax, and moſt likely might have been 5^h 15′ 00″ without it. Now in 5^h 15′, *Mercury* deſcrib'd the Chord of 146° 52′ in the *Sun*'s Limb, being 31′ 9″, and conſequently the neareſt Diſtance to the Center was 4′ 38″, or the Sine of 16° 34′ the *Sun*'s Semidiameter being Radius; that is, 1′ 18″ leſs than we found it in 1723. Hence alſo it follows, that the true Conjunction in Longitude was 7 *min.* of Time later than the neareſt Approach of the Centers, *viz.* at 0^h 10′ 50″ at St. *Helena*, or at 0^h 35′ paſt Noon at *Greenwich*: and, that the North Latitude of the Planet, at that Time, was 4′ 41″.

Suppoſing, therefore, the neareſt Diſtance of the Centers in the Tranſit of 1631, to have been 3′ 20″, that is, 1′ 18″ leſs than in 1677, we ſhall find that *Mercury* then deſcrib'd a Chord of 156° 20′, traverſing the Disk of the *Sun* in 5^h 21′ 30″; ſo that ſuppoſing his *Exit* at 10^h 28′ at *Paris*, that is 10^h 18′ 40″ at *Greenwich*, he enter'd on the *Sun* at 4^h 57′ 10″ in the Morning; and was neareſt his Center at 7^h 38′ *T. app.* but in the ſame Longitude with him at 7^h 43′, or *Octob.* 27° 19^h 43′ *T. app.* having then 3′ 22″ North Latitude.

And here, I think I may, without Vanity, advertiſe the Reader, that above thirty Years ſince, *viz.* in *Philoſoph. Tranſ.* N° 193, for the Month of *March*, *&c.* 169$\frac{9}{1}$, I predicted, by Help of the two former, this laſt Tranſit, with a ſurpriſing Exactneſs,

even beyond my Hopes, making the Time of the middle, or neareſt Approach of the Centers of the *Sun* and *Mercury*, *Anno* 1723, *Octob.* 29^{d} 5^{h} 19′ *T. app.* which we found by Obſervation at 5^{h} 14′$\frac{1}{2}$, only 4′$\frac{1}{2}$ Minutes ſooner; and, in Latitude, *Mercury* was but ſix Seconds more ſoutherly than I then had computed it; the Error, in Longitude, being little more than two Diameters of this exceeding ſmall Planet; and, in Latitude, but a ſingle Semidiameter thereof. So, that for the Future, Aſtronomers may truſt my Table of theſe Tranſits, in *Tranſact.* N^{o} 193, to a few Minutes of Time, and not wait with the Uncertainty of Hours, nay Days, as has lately been done.

But, in order to obtain a yet further Degree of Exactneſs by Help of this Obſervation, it may be moſt expedient to compare with it the Ingreſs I obſerv'd at St. *Helena*; becauſe, in that, as well as in this, the Latitudes of the Planet being very ſmall, a little Error in them will not ſo much affect the Longitudes. Suppoſing therefore, that *Anno* 1677, *Octob.* 27^{o} 21^{h} 26′ 15″ at St. *Helena*, or 21^{h} 50′ 15″ *T. app.* at *Greenwich*, the Center of *Mercury* entered on the *Sun*, and that, at that Time, he was 8′$\frac{1}{4}$ Degrees on the *Sun*'s Limb, to the North of the Ecliptick (according to what is above concluded) it follows, that he had then 2′ 20″ North Latitude, and 16′ 5″ greater Longitude than the *Sun*'s Center; as in this preſent Tranſit, *Octob.* 29^{o} 2^{h} 41′ 30″ *T. app.* at *Greenwich*, he had 3′ 40″ North Latitude, and 15′ 50″ more Longitude.

Now the apparent Geocentrick Differences of Longitude, are to the real Heliocentrick Differences, as the Planet's true Diſtance from the *Sun*, to his Diſtance from the *Earth*; that is, in both Caſes, as 313 to 676; wherefore, in 1677, *Mercury* wanted 34′ 45″ of the Conjunction with the *Sun*; and, in 1723, but 34″ 13′,

at

at the Times of his apparent Ingreſs on the Disk. And, equating the Times, I find, that the *Sun*, *Anno* 1677, *Octob.* $27^d\ 21^h\ 34'\ 20''$ *T. æq.* was, in ♏ $15^\circ\ 36'\ 55''$ and, conſequently *Mercury*'s Heliocentrick Place ♉ $15^\circ\ 2'\ 10''$: and, *Anno* 1723, *Octob.* $29^d\ 2^h\ 25'\ 30''$ *T. æq.* the *Sun* was in ♏ $16^\circ\ 39'\ 43''$, and therefore *Mercury*, at that Time, in ♉ $16^\circ\ 5'\ 30''$.

Mercury therefore, in 46 Years with 11 Intercalations, and beſides $1^d\ 4^h\ 51'\ 10''$, has made 191 Revolutions to the Equinoctial Points, and over and above $1^\circ\ 3'\ 20''$. But, by the *Scholion* to *Prop.* XIV. *Lib.* III. *Natur. Philoſoph. Principia Math.* the Motion of the *Aphelion* of *Mercury*, from the Equinox in that Time, is $40'\ 18''$; ſo that there remains $23'\ 2''$ of *True Anomaly* to be reduced to the *Mean*: Now the Mean Anomaly of *Mercury*, in both Caſes, being 5 *ſig.* 12°, $23'\ 2''$ of True Anomaly gives $15'\ 24''$ Mean Anomaly; which added to $40'\ 18''$ becomes $55'\ 42''$, for the Mean Motion above ſo many Revolutions: and this is to be encreaſed by $8''$ to reduce it to the Plane of *Mercury*'s Orb, in all $55'\ 50''$.

Hence, doubling the Interval, in 92 *Julian* Years $1^d\ 9^h\ 42'\ 20''$, the Mean Motion of *Mercury* from the Equinox is $0^s\ 1^\circ\ 51'\ 40''$, from which, taking $5^\circ\ 44'\ 50''$ the Motion in $1^d\ 9^h\ 42'\ 20''$, we have his Motion in 92 *Julian* Years $11^s\ 26^\circ\ 6'\ 50''$, and in 100 Years, $2^s\ 14^\circ\ 2'\ 13''$, which is but $20''$ more than I had ſome Years ſince printed it, in my Aſtronomical Tables ſhortly to be publiſhed, and differs but one Hour's Motion therefrom in 3000 Years.

The forementioned Proportion of the Diſtances, *viz.* 313 to 676, is alſo between the Latitudes ſeen from the *Earth* and the Inclinations, or Heliocentrick Latitudes of the Planet: ſo that $2'\ 20''$, at the Ingreſs of 1677, gives $5'\ 2''$; and $3'\ 40''$ in 1723, becomes $7'\ 55''$

for the Latitudes at the *Sun*. And the Inclination of the Orb of *Mercury* to the Plane of the Ecliptick (determined by accurate Obſervations near his northern Limit) being 6° 59′ 20″, we compute the Diſtance of the Planet from his Node, in the former 0° 41′ 7″, and, in the latter, 1° 4′ 37″; which, being deducted from his Heliocentrick Places reſpectively, leave the Place of the aſcending Node, in 1677, ♉ 14° 21′ 3″; and, in 1723, ♉ 15° 0′ 53″: So, that in 46 Years the Node is found 39′ 50″ forwarder in the Ecliptick; which is but 1′ 30″ more than the Præceſſion of the Equinox in the ſame Time. We may therefore ſafely aſſume the Plane of the Orb of *Mercury* to be immoveable in the Sphere of fix'd Stars, and its aſcending Node to be ♋ 15° 41′ from the *firſt Star of* Aries. Nor can ſo very ſlow a Motion (ſuppoſing ſuch to be) be fully defined, but by the utmoſt Care and Diligence of future Aſtronomers, after the Obſervation of many Ages.

As to the reſt of the Theory of this Planet's Motion, I make his mean Diſtance from the *Sun*, 38710 ſuch Parts as the mean Diſtance of the *Sun* and *Earth* is 100000; and his greateſt Equation 23° 42′ 37″. The *Epocha* of his middle Motion, *ineunte Anno* 1723, *ſtyl. vet.* from the Equinoctial Point, I make ♐ 19° 9′ 31″; and that of his Aphelion to the ſame Time ♐ 13° 3′ 34″: the Aphelion moving *ſecundum Seriem Signorum*, ſeven Minutes in eight Years. And theſe Numbers I preſume, may repreſent the Motion of *Mercury*, with an Exactneſs equal to that of any of the other Planets; perhaps as near as the *Sun*'s Place by any Tables, or thoſe of the fixed Stars by any Catalogue yet extant.

It were to be wiſhed, that ſome good Obſervation, like this, had been made of the like Tranſit of *Mercury* at his other Node in *April*, where he was ſeen indeed

deed *April* 23° 1661, but ſo imperfectly, that neither Ingreſs nor Egreſs was any where obſerv'd; and, though it be certain, that he traverſed the *Sun* on *April* 26°, 1674; and again *April* 24, 1707, yet we were ſo unfortunate, that the Conjunction in both happened ſo near Midnight, that he eſcaped unſeen by all the Aſtronomers of *Europe*, excepting ſingly Mr. *Roemer* at *Copenhagen*, whoſe Obſervation I have lately received by the Favour of Mr. *De l' Iſle* the Aſtronomer, communicated in the Words of the Manuſcript Journal of Obſervations of the ſaid Mr. *Roemer*. " *Hodie ſexto Maii* (*Anno* 1707) *hora matutina* " 4^h 19', *ſpectabatur Mercurius in extremo margine* " *Solis jamjam exiturus; altus ſupra imum ſolis* " *marginem* ¼ *diametri ſolaris, & ad ſiniſtram in* " *Tubo* (*ſc. invertente*) *Accuratius hæc determi-* " *nare non licuit ob moram nimis brevem.*" It was great Pity, that he did not, at leaſt, eſtimate, how many Diameters of his Body he was diſtant from the Limb of the *Sun*, or what Part of a Diameter, if ſo near: But having examined this Obſervation, I find that the *Sun*, at that Time, was but juſt riſen, or rather riſing, and ſoon after entered into a Cloud, ſo that the Limb of the *Sun* could not be diſtinctly ſeen, it always undulating and ſparkling much, when ſo near the Horizon; in which Circumſtance, a juſt Obſervation could hardly be made.

Let us now ſee how our Numbers, corrected as above, will repreſent this Obſervation. *Anno* 1707, *April* 24° 16^h 19' at *Copenhagen* is 15^h 28' at *Greenwich*, but 15^h 24 20" *T. æq.* To this Time, I find the *Sun*'s true Place ♉ 14° 50' 1", and his Diſtance from the *Earth* 101005. The correct *Epocha* of *Mercury*'s mean Motion, for the Year 1707, is 3^s 13° 18' 45", to which adding, for the reſt of the Time, 3^s 19° 9' 28",

we

we ſhall have his middle Motion at the Time of the Obſervation ♏ 2° 28′ 13″, and, taking his *Aphelion* in ♐ 12° 49′ 49″ therefrom, we have his mean Anomaly 10ˢ 19° 38′ 24″, and thereby the Equation to be added 12° 39′ 41″, and thence the Place of *Mercury* in his Orb ♏ 15° 7′ 54″. But the correct Place of the deſcending Node is ♏ 14° 46′ 25″, and therefore *Mercury*, being 21′ 29″ paſt the Node, had 2′ 36″ South Latitude at the *Sun*; and his Place, reduced to the Ecliptick, was ♏ 15° 7′ 45″, that is, 17′ 44″ paſt the Conjunction of the *Sun*, which diminiſhed in the Proportion of 5567 to 4533, or of the Diſtance of the Planet from the *Earth* to his Diſtance from the *Sun*, becomes 14′ 27″; and by ſo much was he paſt the Conjunction as viewed from the *Earth*. Again, by the ſame Proportion, his Geocentrick Latitude, at that Time, was 2′ 7″ South; and therefore, his apparent Diſtance from the *Sun*'s Center, was 14′ 37″; that is, but 1′ 18″ from his weſtern Limb; ſo that he might well be ſaid to be, *jamjam exiturus*.

But, that *Mercury* ſhould at that Time be ſo far northerly, as Mr. *Roemer*'s Words import, was abſolutely impoſſible; and, I am apt to believe, that ſo acute an Aſtronomer as Mr. *Roemer* was, could not himſelf be the Obſerver, but ſome Perſon leſs acquainted with theſe Matters; which the Words *ſpectabatur Mercurius*, inſtead of *Mercurium vidi*, ſeem to import. If he had then had North Latitude, he muſt needs have been ſeen in the *Sun* in *April* 1720, which we are aſſured he was not.

Laſtly, it may not be amiſs to advertiſe, that on the laſt Day of *October* 1736, *Mercury* will again traverſe the northern Part of the *Sun's* Diſk, both Ingreſs and Egreſs being viſible to all *Europe*.

F I N I S.


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PHILOSOPHICAL TRANSACTIONS.

For the Months of *March* and *April*, 1725.

The CONTENTS.

ICAL
SITIONS.

I. *The* * *Diſſertation concerning the Figure of the Earth continued; by the Reverend* J. T. Deſaguliers, *LL. D. F. R. S.*

HOW the Figure of the Earth is deduc'd from the Laws of Gravity and Centrifugal Force, is very well ſhewn by the late *Savilian* Profeſſor of Aſtronomy, Dr. *John Keill*, in a Book that he wrote in the Year 1698, againſt Dr. *Burnet*'s Theory of the Earth; and therefore I ſhall tranſcribe from him what he has ſaid upon that Subject; becauſe, otherwiſe, I ſhou'd only ſay the ſame Thing in other Words.

I own indeed that he has made a Miſtake in that Book concerning the Meaſure of the Degrees of an Ellipſe; but I find that all that relates to the oblate Spheroidical Figure of the Earth is right; and the little Difference of taking 15 *Paris* Feet for the Space that a Body falls thro' in a Second, inſtead of 15 Feet 1 Inch and 2 Lines, and a Number of Feet, a little leſs than true, for the Diameter of the Earth (which was not ſo well known at that Time) will no way invalidate his Demonſtration and Proof. Here follow his Words.

" To prove the Earth to be higher at the Æquator " than at the Poles, I will ſuppoſe firſt, that, at the Be- " ginning of the World, the Earth was fluid and ſpheri- " cal; but afterwards God Almighty having given it a " Motion round its own Axis, all Bodies upon the Earth " wou'd deſcribe either the Æquator or Circles, paral- " lel to the Æquator, and, by conſequence, all wou'd " endeavour to recede from the Center of their Motion.

* *V.* Philoſ. Tranſact. N. 386.

" It is to be here obſerv'd, that if a Body doth freely " revolve in a Circle about a Center, as the Planets do " about the Sun, that its Centripetal Force (or that Force " by which it is drawn towards the Center) is always " equal to its Force, by which it doth endeavour to re- " cede from the Center; for the Force, which detains a " Body in its Orbit, muſt be equal to the Force by " which it endeavours to recede from its Orbit, and fly " off in the Tangent. This may be clear by the Ex- " ample of a Body turn'd round a Center by the Help " of a Thread, which detains the Body in its Orbit; " the Thread, being ſtretch'd by the Motion of the Body, " will endeavour to contract itſelf equally towards both " Ends, by which it will pull the Center as much to- " wards the Body, as it doth the Body towards the " Center.

" Now this Centrifugal Force is always proportional " to the Periphery, which each Body deſcribes in its " diurnal Motion by the firſt *Theorem* of *Hugenius de* " *Vi Centrifugâ:* So that under the Æquator, which " is the biggeſt Circle, the Centrifugal Force wou'd be " greateſt, and ſtill grow leſs as we approach the Pole " where it quite vaniſheth, there being there no diurnal " Rotation. And without doubt, all Bodies having this " Centrifugal Force, by which they endeavour to re- " cede from the Center of their Motion, wou'd fly off " from the Earth, if they were not kept in their Orbit " by their Gravity, or that Force by which they are " preſs'd towards the Center of the Earth, which is " much ſtronger upon our Earth than the Centrifugal " Force; and becauſe the Gravity upon the Surface of " the Earth is always the ſame; but the Centrifugal " Force alters and grows leſs, the nearer we come to " the Poles, it is plain that the Gravity under the Æ- " quator, having a greater Force to oppoſe it, than that

which

" which is near the Poles, will not act so strongly in " the one Place as in the other, and consequently Bo- " dies will not be so heavy under the Æquator as at " the Poles. ——— If the Circle * Æ P Q P represent " the Earth, Æ Q the Æquator, and P P the Poles, if " C be a Body in the Æquator, it is evident that it will " be pull'd by two contrary Forces; namely, that of " its Gravity, which pulls it towards the Center, and " that of its Centrifugal Force, which pulls it from it. " Now, if both these Forces were equal, it is evident " it wou'd go neither of these Ways; but if one were " stronger than the other, it wou'd move where the " strongest Force pulls it, but only with a Velocity " which is proportional to the Differences of these two " Forces, and therefore it wou'd not descend so fast as " if there were no Centrifugal Force, pulling against it; " that is, a Body in the Æquator, does press less towards " the Center, than at the Pole, where there is no Cen- " trifugal Force to lessen its Gravity. Bodies therefore, " of the same Density, are not so heavy in one Place as " in the other.

" Now in a spherical Fluid, all whose Parts gravitate " towards the Center, I think it is evident from the " Principles of Hydrostaticks and Fluidity, that all those " Bodies, which are equally distant from the Center, " must be equally press'd with the Weight of the incum- " bent Fluid, and if one Part come to be more press'd " than another, that which is most press'd will thrust " that out of its Place which is least, till all the Parts " come to an *Æquilibrium* one with another; and this " is known by a common and easy Experiment, if you " take a recurv'd Tube, † and fill it with Water or any " other Fluid, it will rise equally in both Legs of the

* Fig. I.

† Fig II.

" Tube, ſo that the Surfaces C E and F I are equally
" preſs'd by the incumbent Columns B C E D, and
" G F I H, but if one of the Legs of this Tube ſhou'd
" be fill'd with Oil, or ſome other lighter Fluid, and
" the other with Water, the lighter Fluid will riſe
" higher than the other, for otherwiſe, theſe Surfaces,
" which are equally diſtant from the Center, wou'd not
" be equally preſs'd.

" Juſt ſo if P Æ M P S, * repreſents a fluid Sphere,
" which we may imagine compoſed of a great many
" communicating Canals or Tubes, the Fluid in every
" one of which preſſes upon the Center; now if the
" Fluid, in every one of theſe Tubes, was of equal
" Weight or Gravity, it is plain, that, by that means,
" they wou'd be alſo of an equal Height from the Cen-
" ter; for by that means only, wou'd the Center be e-
" qually preſs'd by the Weight of all the Tubes; but
" if the Fluid, in the Canal Æ O M, were lighter than
" the Fluid in the Canal P O S, it is plain, that in this
" Caſe, the Fluid P O S, preſſing more on the Center,
" than the Fluid in the Canal Æ O M, the Surface of
" the Fluid Æ O M, will riſe to a greater Height or
" Diſtance from the Center; ſo that by its greater
" Height, which recompenſes its leſſer Gravitation, it
" will preſs equally upon the Center with the Fluid in
" the Canal P O S. After the ſame manner, † if the
" Fluid in the Canal G O H, were heavier than the
" Fluid in the Canal Æ O M, but lighter than that
" which is in P O S, then wou'd the Canal G O H be
" ſhorter than Æ O M, but longer than P O S, and the
" Figure compoſed of all theſe Tubes, wou'd be in the
" Form of a Spheroid which is generated by the Cir-
" cumrotation of a Semi-ellipſis round its Axis; but as I

Fig. III. Fig. IV.

" have

" have already shew'd, that if Æ O M represent the Se-
" midiameter of the Æquator, that all Bodies in it are
" lighter than in P O S, the Axis of the Æquator
" (we take the Diameter and Axis here, not as pure
" Mathematical Lines, but as small Canals or Tubes;)
" and just so those Bodies which are in the Tube G O H,
" I have prov'd to be lighter than those in P O S, but
" but heavier than the Bodies which are in Æ O M,
" the Centrifugal Force in G H being less than that
" which is in Æ M, and there is no Centrifugal Force
" in the Poles P S. It is plain, therefore, that the Tube
" Æ O M will be longer than G O H, and G O H will
" be longer than P O S, that is, the Diameter of the
" Æquator, will be longer than the Axis of the Earth,
" and consequently the Figure of the Earth will be af-
" ter the Fashion of a broad Spheroid, which is gene-
" rated by the Rotation of a Semi-ellipsis round its les-
" ser Axis. This, I hope, will be sufficient to convince
" *Theorist* of the Falseness of his own Assertion, since
" it is plain Demonstration, than an Earth, form'd from
" a Chaos, must have a very different Figure from what
" he supposes it had.

" But I will now proceed farther, and inquire how
" much the Gravity is diminish'd at the Æquator, or
" any other Parallel by the Centrifugal Force, which
" all Bodies acquire by being turn'd round the Earth's
" Axis, that from thence we may endeavour to deter-
" mine, what Proportion the Diameter of the Earth's
" Æquator has to its Axis; to calculate which, I will
" first suppose, that the mean Semidiameter of the
" Earth is 19615800 *Paris* Feet, according to the late
" Observations of the *French* Mathematicians, and since
" the Earth turns round its Axis in the Space of 23
" Hours, 56', for in that Time, the same Meridian re-
" turns to the same immoveable Point of the Heaven

" again

" again (but the Sun, in the mean time, ſeeming to be
" mov'd a Degree, according to the Series of the Signs,
" is the Cauſe why there are four Minutes more requir'd
" before the Meridian can overtake him) from thence
" it follows, that a Body, under the Æquator, moves
" through 1426,88 Feet, in the Space of one Second of
" Time. Now, according to the Theorem given us by
" Sir *Iſaac Newton* in his *Philoſophiæ Naturalis*
" *Principia Mathematica*, Schol. Prop. 4. Lib 1. The
" Centrifugal Force of any Body has the ſame Propor-
" tion to the Force of Gravity, that the Square of the
" Arch, which a Body deſcribes in a given Time, di-
" vided by its Diameter, has to the Space, through which
" a heavy Body moves, in falling from a Place in which
" it was at reſt in the ſame Time; and ſuppoſing a heavy
" Body falls 15 Foot in a Second of Time, by Calcula-
" tion, it will from thence follow, that the Force of
" Gravity has the ſame Proportion to the Centrifugal
" Force at the Æquator, that 289 has to Unity; and
" therefore by this Centrifugal Force which ariſes from
" the Diurnal Rotation of the Earth round its Axis;
" any Body, placed in the Æquator, loſes $\frac{1}{289}$ Part of its
" Gravity, which it wou'd have were the Earth at reſt,
" or which is the ſame Thing, a heavy Body plac'd at
" either of the Poles (where there is no Diurnal Rota-
" tion, and conſequently no Centrifugal Force) which
" weighs 289 Pounds, if it were brought to the Æquator,
" wou'd weigh only 288 Pounds.

" Having thus determin'd the Proportion of the Cen-
" trifugal Force, at the Æquator, to the Force of Gravity,
" it will be eaſy from thence to ſhew their Proportions
" in any Parallel; for it is compounded of the Pro-
" portion of One to 289, and of the Co-ſine of the La-
" titude to the Radius; for if two Bodies deſcribe diffe-
" rent Peripheries in the ſame Time, their Centrifugal

" Forces

" Forces are proportional to their Peripheries, or to the " Semi-diameters of these Peripheries, as is determin'd " by Monſ. *Hugens*, in his *Theoremata de Vi Centri-* " *fuga & Motu circulari*; but the Periphery which " a Body in the Æquator deſcribes, has its Semi-diame- " ter equal to the Radius or Semi-diameter of the Earth, " and in any other Place, the Parallels, in which Bodies " move, have the Co-ſines of their Latitude for their " Semi-diameters, and therefore it will follow, that the " Force of Gravity is to the Centrifugal Force in a Pro- " portion, compounded of the Radius to the Co-ſine of " the Latitude, and of 289 to 1. and therefore at the " Latitude of 51° 46′ (for Example) it will be as " 466 to 1.

" But we muſt obſerve, that it does not from thence " follow, that a Body in that Latitude loſes $\frac{1}{466}$ Part of " its abſolute Gravity, which it wou'd have, were the " Earth at reſt. For that cou'd not be, unleſs the Cen- " trifugal Force acted directly contrary to the Force of " Gravity, which it doth no where but at the Æquator; " for let the Circle * Q P E repreſent the Earth, Q E " the Diameter of the Æquator, O its Center, and let " B repreſent a Body which we ſuppoſe to hang by the " Thread A B, and to be placed any where between the " Pole P and the Æquator Q, and let B D be drawn per- " pendicular to the Axis. It is plain, that if the Earth had " had no Diurnal Rotation, the Body B wou'd draw the " Thread A B into the Poſition of A C, ſince by that " means it deſcends as near as it can to the Center, and " there it wou'd ſtretch the Thread with all the Force " of its Gravity; or if we will ſuppoſe, that the Centri- " fugal Force acted according to the ſame Direction A C, " it wou'd then directly oppoſe the Force of Gravity,

* Fig. V.

" and

" and the Thread wou'd remain in the ſame Poſition,
" but it wou'd be ſtretch'd with a Force proportional
" to the Differences of theſe two Forces.

" But becauſe the Body B turns round the Center D,
" it will endeavour to recede from it according to the
" Line C B, in which Direction the Centrifugal Force
" acting, it will not directly oppoſe the Force of Gra-
" vity, but it will draw the Thread from the Poſition
" A C into the Poſition A B, let B G be drawn perpen-
" dicular to A C; if B C repreſent the Centrifugal
" Force, acting according to the Direction B C, it is equi-
" valent (as is commonly known) to two Forces, one
" of which is as G C, and acts according to the Dire-
" ction C G, which is contrary to that by which it de-
" ſcends to O, the other is as G B, and acts according
" to the Direction G B, which is no way contrary to
" the Force of Gravity. If therefore B C repreſent the
" total Centrifugal Force of the Body B, that Part of it,
" which directly oppoſes the Force of Gravity, will be
" G C; from whence it follows, that the Decreaſe of
" Gravity, in going from the Pole to the Æquator, is al-
" ways as the Square of the Co-ſine of the Latitude;
" for draw B H parallel to the Axis P P, and becauſe
" the Triangles H C B, C D O are Equi-angular, there-
" fore H C is to C B as C O is to C D, or as Q O is to
" C D, but Q O is to C D as the Decreaſe of Gravity
" at Q is to the Centrifugal Force at C. And there-
" fore H C is to C B, as the Decreaſe of Gravity at Q,
" is to the Centrifugal Force at C. But if C B repre-
" ſent the Centrifugal Force at C, G C will repreſent
" that Part of it which acts directly againſt the Force
" of Gravity, and conſequently the Decreaſe of Gra-
" vity at the Æquator is to the Decreaſe of Gravity at C,
" as H C is to G C; now H C is to G C, in duplicate
" Proportion of H C to C B, or of C O or O Q to C D

" by

" by the 8th of the 6th of *Euclid*, and therefore the " Decreaſe of Gravity at Q is to the Decreaſe of Gra- " vity at C, as the Square of C O is to the Square of " C D, which was to be demonſtrated.

" From whence, it is plain, that if H C repreſent the " Decreaſe of Gravity at the Æquator, and G C its De- " creaſe at C, then will G H repreſent the Difference " of theſe two Diminutions, or the Difference between " the Gravity at Q, and the Gravity at C, but H C is " to H G in duplicate Proportion of H C to H B, or " of O C to D O; that is, the Decreaſe of Gravity at " the Æquator is to its encreaſe at C, as the Square of the " Radius is to the Square of the Sine of the Lati- " tude.

" By this alſo it will appear, that the Direction of " heavy Bodies is not to the Center of the Earth, as has " been always ſuppoſed; for if we take a heavy Body " and hang it by a Thread, the Thread produced will " not paſs through the Center any where but at the " Poles and the Æquator, for in the Figure the Thread " is carry'd by the Centrifugal Force of the Body B, " from the Poſition A C into the Poſition A B, where " it will reſt.

" Now to determine the Angle C A B, which the " Line of Direction of the Body makes with the Line " A C, let A N be drawn parallel to B C, and pro- " duce O B till it meet with it in N, and let us conſider " the Body B as drawn by three Powers, according to " three different Directions B O, B L, and A B, the " Power which pulls it, according to B O, is its Gravity, " that which draws it, according to the Direction B L, " is its Centrifugal Force, and that which acts accord- " ing to A B, is the Strength of the Thread, by which " the Body is hinder'd to move according to either of the " two other Directions, and therefore it is an *Æquili-*

" *brium* with the other two Powers; but by a Theorem
" which is demonstrated by several of the Writers of
" Mechanics, but particularly by Monſ. *Huygens* in his
" ſmall Treatiſe *De Potentiis per Fila trahentibus:*
" If a Body be pull'd by three different Powers which
" are in *Æquilibrio* with one another, according to
" three different Directions, A B, B L and B O, theſe
" three Powers will be as the three Sides of the Tri-
" angle A B N, *viz.* as A B, A N and B N reſpectively;
" or as A B, B C and A C; B N being very near pa-
" rallel, and conſequently equal to A C, ſince they do
" not meet but at a great Diſtance. From hence it fol-
" lows, that the Force of Gravity is to the Centrifugal
" Force, as A C to B C. But a Method has been al-
" ready ſhown, how the Proportion of the Force of
" Gravity to the Centrifugal Force may be determined,
" and therefore the Proportion of A C to B C may be
" be alſo determined, which at the Latitude of 51° 46'
" is as 446 to 1. Therefore in the Triangle A B C, the
" Proportion of A C to B C is known, and the Angle
" A C B being equal to the Angle C O Q, which is ſub-
" tended by the Arch C Q, the Latitude of the Place,
" from thence by the Tables of Sines and Tangents,
" the Angle B A C may be known, which in the above-
" mentioned Latitude is about 5 Minutes.

" From hence alſo it will appear, that it is not the
" Line A C, which being produced paſſes through the
" Center, but the Line A B that is perpendicular to the
" Curve P Q, for all the Particles of the Fluid will ſettle
" themſelves in ſuch a Poſition, that their Lines of Di-
" rection downwards, muſt be perpendicular to the Sur-
" face of the Body which they compoſe, for otherwiſe the
" Parts of the Fluid wou'd not be in *Æquilibrium* one
" with another, and therefore altho' the Lines of Di-
" rection of heavy Bodies do not paſs through the

" Center

" Center of the Earth, yet are they ſtill perpendicular " to their Horizons; and, upon this Account, there " cou'd ariſe no Error in levelling of Lines, and in find- " ing the Riſings and Fallings of the Ground.

" Upon this account alſo it will appear, that the " Surface of the Earth is not ſpherical, for if it were, " then wou'd all Lines, drawn from the Center, be per- " pendicular to the Surface of the Earth, ſince it is the " known Property of a Sphere that they muſt be ſo; " but I have already ſhew'd, that it is not ſo in the " Earth, and therefore it is plain, that the Earth is not " a Sphere. That therefore I may inquire more parti- " cularly into the Figure of the Earth, I will reſume " my former Hypotheſis, that the Earth is compoſed of " an infinite Number of Canals, which communicate " with one another at the Center, and are all equipon- " derant, of which we will conſider two, as O Q and " O C, and let O Q be $= r$, O D $= X$ and D C $= y$, let " the abſolute Gravity be call'd p, and the Centrifugal " Force at the Æquator n. O C is equal to $\sqrt{x^2 \times y^2}$ " the Weight of the Canal O Q is equal to the abſolute " Gravity of the whole Canal *minus* the Centrifugal " Force of each Particle contain'd in it, and becauſe the " Centrifugal Force of each Particle is as its Diſtance " from the Center, and therefore it encreaſes in an A- " rithmetical Progreſſion, the greateſt of which is n, " conſequently the Sum of all the Centrifugal Force is " equal to $\frac{1}{2} n r$, but upon the Hypotheſis, that Gravity " is the ſame at all Diſtances from the Center, the ab- " ſolute Gravity of the Canal O Q is $p r$, and therefore " its real Weight upon the Center O Q is $p r - \frac{1}{2} n r$, " after the ſame Manner, the abſolute Gravity of the " Canal O C is $p \times \sqrt{x^2 + y^2}$; but the Sum of all " the Centrifugal Forces of all the Fluids in the Canal " O C, is equal to the Centrifugal Force of the Fluid in

" C D (as may be eaſily prov'd from the Conſideration
" of inclin'd Planes) but the Centrifugal Force at C,
" being to the Centrifugal Force at Q, as C D is to O Q
" (that is, as y is to r) the Centrifugal Force at C will
" be equal to $\frac{ny}{r}$, and becauſe the Centrifugal Force of
" each Particle is as its Diſtance from the Point D,
" which is the Center of the Circle that the Fluid in
" the Canal C D deſcribes, and therefore the Centrifugal
" Forces, in counting from the Point D, muſt encreaſe
" in an Arithmetical Progreſſion, the greateſt of which
" is $\frac{ny}{r}$, and therefore the Sum of all the Centrifugal
" Forces in C D muſt be equal to $\frac{nyy}{2r}$, therefore the
" Weight of the Canal O C is $= p\sqrt{x^2+y^2} - \frac{1}{2}$
" $\frac{nyy}{r} = pr - \frac{1}{2}nr$, which Equation expreſſes the
" Nature of the Curve that is made by the Section of
" the Earth with a Plane through its Poles, and by this
" the Proportion of the Axis of the Earth, to the Dia-
" meter of the Æquator, may be eaſily determin'd; for
" when C O coincides with O P, then C D or y be-
" comes equal to nothing, and the Equation is $p\sqrt{x^2}$
" $= pr - \frac{1}{2}nr$ or $px = pr - \frac{1}{2}nr$, and therefore
" by the 16th of the 6th, p has the ſame Proportion to
" $p - \frac{1}{2}n$ that r has to x, or O Q to O D, but p is to
" $p - \frac{1}{2}n$ as 289 is to $288\frac{1}{2}$, or as 578 is to 577, which
" therefore is the Proportion of the greateſt Diameter of
" the Earth to the leaſt; but this is upon Suppoſition,
" that Gravity is the ſame at all Diſtances from the
" Center; but if we will ſuppoſe, that the Gravity of
" Bodies without the Earth is in a Proportion reciprocal
" to the Squares of their Diſtances from the Center, the

" Gravity

" Gravity of thoſe Bodies, which are within the Earth,
" will be directly as their Diſtance, both which do beſt
" agree with the obſerv'd Phænomena of Nature; then
" will the Gravity at the Æquator be to the Gravity
" at the Poles as 689 to 692, which Numbers, in this
" Hypotheſis, do alſo expreſs the Proportion of the Dia-
" meter of the Earth, drawn through its Poles, to its
" Diameter drawn in the Plane of the Æquator.

" It is upon the Account of this Diminution of Gra-
" vity, according as we approach the Æquator, that
" Pendulums of the ſame Lengths in different Latitudes
" take different Times to perform their Vibrations;
" for becauſe the accelerating Force of Gravity is leſs
" at the Æquator than under any Parallel, and under
" any Parallel it is ſtill leſs than under another which
" is nearer the Poles; it does plainly from thence fol-
" low, that a Body plac'd in the Æquator, or in any
" other Parallel, will take a longer Time to deſcend thro'
" an Arch of a given Circle, than it wou'd do at the
" Poles, and the farther a Body is remov'd from the
" Poles, the longer Time it will take to deſcend through
" any given Space.

" From hence it follows, that the Length of Pendu-
" lums, which perform their Vibrations in equal Times
" in different Latitudes, are directly as the accelerating
" Forces of their Gravities; for the Time a Body takes
" to deſcend through an Arch of a Cycloid, is to the
" Time it will take to fall through the Axis of the Cy-
" cloid always in a given Proportion, *viz.* as the Semi-
" periphery of a Circle is to its Diameter by the 25th
" Prop. of *Huygen*'s *Horologium Oſcillatorium*; and
" therefore when the Times in which a Body deſcends
" through the Axes of two different Cycloids are equal,
" the Times of the Deſcent through the Cycloids will
" be alſo equal; but when the Times of the Deſcent

" through

" through the Axes are unequal, these Axes, and conse-
" quently the Lengths of the Pendulum which vibrates
" in these Cycloids, are proportional to the accelerating
" Forces of their Gravities.

" By this if we know the Length of a Pendulum
" which performs its Vibrations in a given Time, in
" any one Part of the Earth, it is easy to determine the
" Length of a Pendulum, which performs its Vibra-
" tions in the same Time in any other Part of the Earth;
" as for Example, the Length of a Pendulum, which vi-
" brates Seconds at *Paris*, is three Foot eight Lines
" and a Half, let it be requir'd to find the Length of a
" Pendulum, which vibrates Seconds at the Æquator.
" Because the Gravity at the Poles is to the Gravity at
" the Æquator, as 692 is to 689; therefore the De-
" crease of Gravity at the Æquator is $\frac{1}{578}$ Parts of the
" whole Gravity; but, as I have before demonstrated, the
" Decrease of Gravity at the Æquator is to its Encrease
" in any other Latitude, as the Square of the Radius is
" to the Square of the Sine of the Latitude, now the
" Latitude of *Paris* being 48° 45′, its Sine is 75.183,
" and therefore the Square of the Radius is to the
" Square of the Sine of the Latitude as 1000000 to
" 565248, but as 1000000 is to 565248, so is 3.000
" the Number which represents the Decrease of Gra-
" vity at the Æquator to 1.695, the Number which
" represents its Encrease at *Paris*, which added to 689
" the Gravity at the Æquator, makes 690.695, the Num-
" ber which will represent the Gravity at *Paris*. But
" I have already shew'd, that as the Gravity at *Paris*
" is to the Gravity at the Æquator, so is the Length of
" a Pendulum which vibrates Seconds at *Paris*, to the
" Length of a Pendulum that vibrates Seconds at the
" Æquator, that is as 690, 695 to 689, so is 36,708 the
" Length of a Pendulum at *Paris*, which performs its

" Vibra-

" Vibration in a Second to 36,616, which therefore is " the Length of a Pendulum which performs its Vibra- " tions in a Second at the Æquator; ſo that the Diffe- " rence between theſe two Pendulums is $\frac{22}{100}$ Parts of an " Inch, which comes pretty near the Obſervations of " Monſ. *Richer*, who at the Iſland of *Cayenne*, whoſe " Latitude is 5° 00′ found that a Pendulum, which vi- " brates Seconds there, was a tenth Part of an Inch " ſhorter than a Pendulum, which vibrates Seconds at " *Paris*.

" Thus we ſee that the Principles and Hypotheſis, " and withal their Conſequences, upon which the broad " ſpheroidical Figure of the Earth is founded, do exactly " agree with Obſervations, and therefore there is no " Doubt to be made, but that the Earth is really of ſuch " a Figure, and that the Hypotheſis upon which this " Figure is grounded (*viz.* the diurnal Rotation of the " Earth, and by conſequence the Centrifugal Force of " all Bodies upon it) muſt be admitted for a true one; " ſince the different Vibrations of Pendulums of the " ſame Length, in different Latitudes, can depend upon " no other Cauſe; for the Change of Air is not able to " produce any ſuch Effect, for if the Air made really " any Alterations in the Vibrations of a Pendulum, it " wou'd produce a quite contrary Effect than what is " obſerv'd; for Pendulums near the Æquator wou'd " move faſter than they wou'd do in Places of greater " Latitude, the Air in the one Place, being more rari- " fied, is much thinner and finer than it is in the other, " and therefore gives leſs Reſiſtance to Bodies that move " in it.

" In this Reaſoning, we have ſuppos'd the Earth to " have been at firſt fluid, as the *Theoriſt* has done be- " fore us, but if we will put the Caſe, that the Earth " was firſt partly fluid and partly dry, as it is at preſent,

" yet

" yet becauſe we find that the Land is very near of the " ſame Figure with the Sea (only rais'd a little higher " that it might not be overflow'd) compoſing with it " the ſame Solid, and I have already ſhew'd that the " Surface of the Ocean is ſpheroidical and not ſpherical, " there is no doubt to be made, but that the Land was " form'd into the ſame Figure by its wiſe Creator at the " Beginning of the World, for if it were otherwiſe, then " wou'd the Land towards the Æquator have been over- " flow'd with Water, which, as I have already prov'd, " muſt have been higher at the Æquator than at the " Poles; and therefore the Sea wou'd riſe there and " ſpread itſelf like an Inundation upon all the Land."

To make an End of this long Diſſertation, let us in a few Words compare the Experiments and Obſervations made uſe of to confirm each of the Opinions above-mentioned.

To prove Monſ. Caſſini's Figure of the Earth, we muſt take the Altitude of a Star nearer than to 2 Seconds; becauſe 2 Seconds anſwer to 32 Toiſes on the Surface of the Earth, and the Difference of the Length of Degrees is but 31. And what is more, we muſt take this Angle with an Inſtrument of 39 Inches Radius; becauſe the 10 Foot Sector was only us'd at the Ends of the two Parts of the Meridian.

To diſprove Monſ. Caſſini's Hypotheſis, we need only obſerve whether a Plumb-Line makes an Angle of of 5 Minutes with a Perpendicular to the Surface of ſtagnant Waters, or Lines of Level.

To prove Monſ. Caſſini's Opinion, the Height of a great many Mountains muſt be accurately meaſur'd by Trigonometry, which Mathematicians have always found very difficult.

To

To prove Sir Iſaac Newton's Opinion, we are only to meaſure about one Tenth of an Inch in a Rod of 39,129 Inches; and to know what to allow for the lengthening of the ſame Rod by the Summer Heat, when it is ſhut up in a Caſe, and carried towards the Æquator. For though the Experiments on Pendulums, made by ſeveral Perſons that travell'd Southward, differ among themſelves, yet they all agree in this, that the Obſervers were oblig'd to ſhorten their Pendulums, in order to make them ſwing Seconds, as they went towards the Æquator. And when we come to compare them together, in order to have the exact Proportion of Length in different Latitudes; we muſt rely on the moſt exact Experimenter, which we may very well do on Monſ. *Richer*: becauſe when he found a Difference, he was ſo careful to find out how much it was, that he caus'd a ſimple Pendulum to ſwing, and compar'd it with a good Pendulum Clock, which he did ſeveral Times every Week for 10 Months together; and when he return'd to *France*, he compar'd it with the Length of the Pendulum at *Paris*; which is of 3 Feet 8 $\frac{1}{5}$ Lines (or 39,129 *Engliſh* Inches) and found it to be ſhorter by 1$\frac{1}{4}$ Line.

II. *An Eſſay upon the Natural Hiſtory of Whales, with a particular Account of the Ambergris found in the* Sperma Ceti *Whale. In a Letter to the Publiſher, from the Honourable* Paul Dudley, *Eſq; F. R. S.*

SIR,

I AM very ſenſible that the World has long ſince, and often been entertained with various Relations of this wonderful Creature: But as I ſhall endeavour to avoid any vain Repetition, ſo I flatter myſelf with the Hopes of preſenting the Royal Society with ſome Particulars not yet publiſhed, both of the Whale in general, and more eſpecially of that Species or Kind of Whale, which our People call the *Sperma Ceti*, but, in my Opinion, much rather deſerves the Name of the Ambergris Whale. But here I would have it noted, that the following Account reſpects only ſuch Whales, as are found on the Coaſt of *New England.*

And of theſe there are divers Sorts or Kinds.

* As firſt, The Right, or Whalebone Whale is a large Fiſh, meaſuring ſixty or ſeventy Feet in Length, and very bulky, having no Scales, but a ſoft fine ſmooth Skin, no Fins, but only one on each Side, from five to eight Feet long, which they are not obſerved to uſe, but only in turning themſelves, unleſs while young, and carried by the Dam on the Flukes of their Tails; when with thoſe Fins they claſp about her Small, and ſo hold them-

* *The ſeveral Species of Whales.*

ſelves

ſelves on. This Fiſh, when firſt brought forth, is about twenty Feet long, and of little Worth, but then the Dam is very fat. At a Year old, when they are called Short-heads, they are very fat, and yield to fifty Barrels of Oil, but by that Time the Dam is very poor, and term'd a Dry-skin, and won't yield more than thirty Barrels of Oil, tho' of large Bulk. At two Years old, they are called Stunts, being ſtunted after weaning, and will then yield generally from twenty four to twenty eight Barrels. After this, they are term'd Scull-fiſh, their Age not being known, but only gueſs'd at by the Length of the Bone in their Mouths. The Whale-bone, ſo called, grows in the upper Jaw on each Side, and is ſometimes ſix or ſeven Feet in Length. A good large Whale has yielded a thouſand Weight of Bone. 'Tis thought by ſome, that the hairy Part of the Whale-bone, and which is next to the Tongue, ſerves in the Nature of a Strainer of their Food.

The Eye of a Whale is about the Bigneſs of an Ox's Eye, and they are ſituated in the After-part of the Head on each Side, and where the Whale is broadeſt; for his Head tapers away forward from his Eyes, and his Body tapers away backward; his Eyes are more than half way his Depth, or neareſt his Under-part; juſt under his Eyes are his two Fins before-mentioned; he carries his Tail horizontally, and with that he ſculls himſelf along.

The Entrails of this Whale are made and ſituated much like thoſe of an Ox, and their Scalps are ſometimes found covered with Thouſands of Sea-lice. One of theſe Whales has yielded one hundred and thirty Barrels of Oil, and near twenty out of the Tongue. The Whale-bone Whale is the moſt valuable, except the *Sperma Ceti* Whale.

The Scrag Whale is near a-kin to the Fin-back, but, inſtead of a Fin upon his Back, the Ridge of the After-part of his Back is ſcragged with half a Dozen Knobs or Nuckles; he is neareſt the right Whale in Figure and for Quantity of Oil; his Bone is white, but won't ſplit.

The Finback Whale is diſtinguiſhed from the right Whale, by having a great Fin on his Back from two Feet and an Half, to four Feet long, which gives him the Name; he has alſo two ſide Fins, as the Whale-bone Whale, but much longer, meaſuring ſix or ſeven Feet. This Fiſh is ſomewhat longer than the other, but not ſo bulky, much ſwifter, and very furious when ſtruck, and very difficultly held; their Oil is not near ſo much, as that of the right Whale, and the Bone of little Profit, being ſhort and knobby. The Belly of this Whale is white.

The Bunch or humpback Whale, is diſtinguiſhed from the right Whale, by having a Bunch ſtanding in the Place where the Fin does in the Finback. This Bunch is as big as a Man's Head, and a Foot high, ſhaped like a Plug pointing backwards. The Bone of this Whale is not worth much, tho' ſomewhat better than the Finback's. His Fins are ſometimes eighteen Feet long, and very white; his Oil much as that of the Finback. Both the Finbacks and Humpbacks are ſhaped in Reeves longitudinal from Head to Tail on their Bellies and their Sides, as far as their Fins, which are about half way up their Sides.

* The *Sperma Ceti* Whale. This Fiſh is much of the ſame Dimenſion with the other, but of a greyiſh Colour, whereas the others are black; he has a Bunch on his Back like the Humpback, but then he is diſtin-

* *The* Sperma Ceti *Whale.*

guiſhed by not having any Whale-bone in the Mouth; but inſtead of that, there are Rows of fine ivory Teeth in each Jaw, about five or ſix Inches long. One of theſe Teeth I have done myſelf the Honour to ſend the Society. The Man, who gave it me, ſays, the Whale was forty nine Foot long, and his Head made twelve Barrels of *Sperma Ceti* Oil. They are a more gentle Fiſh than the other Whales, and ſeldom fight with their Tails; but when ſtruck, uſually turn upon their Backs, and fight with their Mouths. The Oil, which is made of the Body of this Fiſh, is much clearer and ſweeter than that of the other Whales.

* The *Sperma Ceti* Oil, ſo called, lies in a great Trunk about four or five Feet deep, and ten or twelve Feet long, near the whole Depth, Breadth, and Length of the Head, in the Place of the Brains, and ſeems to be the ſame, and diſpoſed in ſeveral membranous Cells, and covered not with a Bone, but a thick griſly Subſtance below the Skin, through which they dig a Hole, and lade out the clear Oil. Not but that the Head, and other Glandulous Parts of this Fiſh, will make the *Sperma Ceti* Oil; but the beſt, and that which is prepared by Nature, is in the Trunk aforeſaid: And an ingenious Man, who has himſelf kill'd many of theſe Whales, aſſures me, that only the Trunk will afford from ten to twenty Barrels. Beſides the *Sperma Ceti* Oil, this Fiſh will yield from twenty to fifty Barrels of common Oil.

One of our Country Doctors tells me, that the Tooth of this Fiſh, ſhaved or powdered, and ſo infuſed in Liquor, equals the Harts-horn, and has been uſed in the Small-Pox, and given to Lying-in-Women, in Caſe of Sickneſs, with Succeſs. The Quantity is as much as

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* Sperma Ceti *Oil.*

will

will lie upon an *Engliſh* Shilling. I meddle not here with the precious Ambergris found in this Whale, becauſe I deſign to cloſe the Whole with that Diſcovery.

I go on therefore, in the ſecond Place, to give an Account of the Manner of the Propagation of Whales. They generate much like to our neat Cattle, and therefore they are termed Bull, Cow, and Calf. The *Latins* called the Whale *Bellua Marina.* They bring forth but one at a Time, and but every other Year. When the Cow takes Bull, ſhe throws herſelf upon her Back, ſinking her Tail, and ſo the Bull ſlides up, and, when the Bull is ſlid up, ſhe claſps him with her Fins. A Whale's Piſſel is ſix Feet long, and at the Root is ſeven or eight Inches Diameter, and tapers away till it comes to about an Inch Diameter: his Stones would fill half a Barrel, but his Genitals are not open or viſible, like thoſe of the true Bull. The Calf, or young Whale, has been found perfectly form'd in the Cow, when not above ſeventeen Inches long, and white; yet, when brought forth, is uſually twenty Feet, but of a black Colour; it is ſuppoſed they go with their Young about nine or ten Months, and are very fat in that Time, eſpecially when they bring forth. When the Female ſuckles her Young, ſhe turns herſelf almoſt upon her Back, upon the Rim of the Water, ſhe has two Teats of ſix or eight Inches long, and ten or twelve Inches round. The Milk is white, like that of a Cow; and upon opening a young ſucking Whale, the Milk was found curdled in his Bag, juſt like that of a Calf.

* 3. The Care of their Young is very remarkable, they not only carrying them on their Tails, and ſuckling them, but often riſing with them for the Benefit of the Air;

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* σύργη.

and

and however they are chaſed or wounded, yet as long as they have Senſe, and perceive Life in their Young, they will never leave them, nor will they then ſtrike with their Tail, and if, in their running, the young one loſes his Hold and drops off, the Dam comes about, and paſſing underneath, takes it on again. And therefore Care is taken by thoſe who kill theſe Mate Fiſh (as they are called) only to faſten the Calf, but not to kill her, till they have firſt ſecured the Cow. For ſo ſoon as ever the Calf is dead, the Cow perceives it, and grows ſo violent, that there is no managing her.

4. The Whales are very gregarious, being ſometimes found a Hundred in a Scull, and are great Travellers. In the Fall of the Year, the right or Whale-bone Whales go Weſtward, and in the Spring they are headed Eaſtward. But here it muſt be noted, that the ſeveral Kinds of Whales don't mix with one another, but keep by themſelves.

5. Reſpiration. Their Way of Breathing is by two Spout-Holes in the Top of the Head. The *Sperma Ceti* has but one, and that on the left Side of the Head. Once in a Quarter of an Hour, when not diſturbed, they are obſerved to riſe and blow, ſpouting out Water and Wind, and to draw in freſh Air: but, when purſued, they will ſometimes keep under Water half an Hour or more; tho' 'tis obſerved when any Cow has her Calf on her Tail, ſhe riſes much oftener (through a natural Inſtinct) for the young one to breath, without breathing herſelf. Out of their breathing Holes they ſpout great Quantities of Blood, when they have receiv'd their Death Wound.

6. Food or Suſtentation. For the firſt Year, as has been already obſerv'd, they all ſuck the Dam. After they are weaned, the right Whales, as is generally ſuppoſed, live upon ſome ouzy Matter, which they ſuck up from the

Bottom

Bottom of the Sea. The Triers, that open them when dead, acquaint me, that they never obſerved any Graſs, Fiſh, or any other Sort of Food in the right or Whale-bone Whale, but only a greyiſh ſoft Clay, which the People call *Bole Armoniac*; and yet an experienced Whale-man tells me, that he has ſeen this Whale in ſtill Weather, skimming on the Surface of the Water, to take in a Sort of reddiſh Spawn or Brett, as ſome call it, that at ſome Times will lie upon the Top of the Water, for a Mile together. Here alſo it may be obſerv'd, that tho' the Body of this Whale is ſo very bulky, and ſo exceeding fat; yet when cut open, they are ſeldom found to have much more Draught than that of an Ox, and they dung much as neat Cattle do. Their Swallow is not much bigger than an Ox's; but the Finback Whale has a larger Swallow: for he lives upon the ſmaller Fiſh, as Mackarel, Herring, *&c.* great Sculls of which they run thro', and, with a ſhort Turn, cauſe an Eddy or Whirlpool, by the Force of which, the ſmall Fiſh are brought into a Cluſter; ſo that this Fiſh, with open Mouth, will take in ſome Hundreds of them at a Time. The *Sperma Ceti* Whale, beſides other Fiſh, feeds much upon a ſmall Fiſh that has a Bill; our Fiſhermen call them Squid Fiſh. The ſmall Peices of theſe Squid Bills are plainly to be diſcerned in the Ambergris, and may be pick'd out of it; they appear glazy, and like little Pieces of broken Shells.

7. The Way and Manner of killing Whales.

Mr. *Harris* in his *Bibliotheca Navigantium*, &c. has given a very particular Account of the Method of taking Whales at *Greenland*, and though our Way in *New England* differs very much from that, yet I ſhall wave it, as not ſo ſtrictly appertaining to Philoſophy. Only I would take notice of the Boats our Whale-men uſe in going from the Shoar after the Whale: They are made of Cedar Clapboards, and ſo very light, that two

Men

Men can conveniently carry them, and yet they are twenty Feet long, and carry ſix Men, *viz.* the Harpooneer in the Fore-part of the Boat, four Oar-men, and the Steerſ-man. Theſe Boats run very ſwift, and by reaſon of their Lightneſs can be brought on and off, and ſo kept out of Danger. The Whale is ſometimes killed with a ſingle Stroke, and yet at other Times ſhe will hold the Whale-men in Play, near half a Day together, with their Lances, and ſometimes they will get away after they have been lanced and ſpouted blood, with Irons in them, and Drugs faſtened to them, which are thick Boards about fourteen Inches ſquare. Our People formerly uſed to kill the Whale near the Shore; but now they go off to Sea in Sloops and Whale-boats, in the Months of *May*, *June*, and *July*, between *Cape-Cod* and *Bermudas*; where they lie by in the Night, and ſail to and again in the Day, and ſeldom miſs of them; they bring home the Blubber in their Sloops. The true Seaſon, for taking the right or Whale-bone Whale, is from the Beginning of *June*, to the End of *May*; for the *Sperma Ceti* Whale, from the Beginning of *June*, to the End of *Auguſt*. And it has been obſerved by our Fiſhermen, that when a *Sperma Ceti* Whale is ſtruck, he uſually, if not always, throws the Excrements out of the *Anus*.

8. The wonderful, and even prodigious Strength of this Creature, which lies principally in their Tails, that being both their offenſive and defenſive Weapon. Many Inſtances of this Kind I have had from credible Perſons, who were Eye-witneſſes; I will mention but a few. A Boat has been cut down from Top to Bottom with the Tail of a Whale, as if cut with a Saw, the Clap-boards ſcarce ſplinter'd, tho' the Gunnel upon the Top is of tough Wood. Another has had the Stem, or Stern-poſt of about three Inches through, and of the

tougheſt Wood that can be found, into which the Ends of the Cedar Clap-boards are nailed, cut off ſmooth above the Cuddee, without ſo much as ſhattering the Boat, or drawing the Nails of the Clap-boards. An Oar has been cut off with a Stroke upwards, and yet not ſo much as lifted up out of the Thole-Pin. One Perſon had an Oar cut off, while in his Hand, and yet never felt any Jarring.

A few Years ſince, one of the Finback Whales came into an Harbour near *Cape-Cod*, and tow'd away a Sloop of near forty Tun, out of the Harbour into the Sea. This Accident happened thus: It is thought the Whale was rubbing herſelf upon the Fluke of the Anchor, or going near the Bottom, got the Fluke into her Niſket, or the Orifice of the *Uterus*, and, finding herſelf caught, tore away with ſuch Violence, and tow'd the Sloop out of the Harbour, as faſt as if ſhe had been under Sail with a good Gale of Wind, to the Aſtoniſhment of the People on Shore, for there was nobody on board. When the Whale came into deep Water, ſhe went under, and had like to have carried the Sloop with her, but the Cable gave Way, and ſo the Boats that were out after her, recover'd it. This Whale was found dead ſome Days after on that Shore, with the Anchor ſticking in her Belly.

After a Whale is dead, it has been obſerved, that the ſame way the Head lies, ſo the Head will lie if not forcibly turned, and let the Wind blow which Way it will, that Way they will ſcull a Head, tho' right in the Eye of the Wind, and they are much eaſier tow'd to the Shore, if they die that Way with their Head, than any other.

9. The Enemies of the Whale, or the Fiſh that prey upon the Whales, and often kill the young ones, for they won't venture upon an old one, unleſs much wounded.

Our

Our Whale-men have given this Fiſh the Name of Killers. Theſe Killers are from twenty to thirty Feet long, and have Teeth in both Jaws that lock one within another. He has a Fin, near the Middle of his Back, four or five Foot long. They go in Company by Dozens, and ſet upon a young Whale, and will bait him like ſo many Bull-dogs; ſome will lay hold of his Tail to keep him from threſhing, while others lay hold of his Head, and bite and threſh him, till the poor Creature, being thus heated, lolls out his Tongue, and then ſome of the Killers catch hold of his Lips, and if poſſible of his Tongue; and after they have killed him, they chiefly feed upon the Tongue and Head, but when he begins to putrify, they leave him. This Killer is without doubt the *Orca*, that Dr. *Frangius* deſcribes in his Treatiſe of Animals. His Words are theſe, *Quando Orca inſequitur Balænam, ipſa Balæna horibilem edit Mugitum, non aliter quam cum Taurus mordetur a Cane.* Theſe Killers are of ſuch invincible Strength, that when ſeveral Boats together have been towing a dead Whale, one of them has come and faſtened his Teeth in her, and carried her away down to the Bottom in an Inſtant. And ſometimes again, they have bit out a Piece of Blubber of about two Foot ſquare, which is of that Toughneſs, that an Iron, with little Beards being ſtruck into it, will hold it till it draws the Boat under Water. The Killers are ſometimes taken, and make good Oil, but have no Whale-bone. The Carcaſes, or Bodies of dead Whales in the Sea, ſerve for Food for Gulls, and other Sea-Fowl, as well as Sharks, for they are not very nice.

* I come now to what I firſt and principally propoſed, in this Eſſay on the Natural Hiſtory of Whales, to give

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* *Of Ambergris.*

an Account of the Ambergris. Many and various have been the Opinions even of the learned World, as to the Origin and Nature of this precious Perfume; ſome have reckoned it a *Bitumen*, and to iſſue from the Entrails of the Earth; others, that it was produced from ſome Inſect, as Honey, Silk, *&c.* The famous Mr. *Boyle*, as I find it, in the Second Volume of *Lowthorp*'s *Abridgment of the Philoſophical Tranſactions*, communicates an Account of Ambergris, from a *Dutch* Merchant, who firſt denies it to be the Scum or Excrement of a Whale, and then gives it, as his Opinion, that it is a fat Gum that iſſues from the Root of a Tree, and that you may raiſe it in Quantities by planting thoſe Trees by the Shore, and ſo the Stream will caſt it up to great Advantage. But Truth is the Daughter of Time; it is now at Length found out, that this *Occultum Naturæ* is an Animal Production, and bred in the Body of the *Sperma Ceti* Whale, analogous to what is found in ſome Animals of the Land, as the Muſk Hog or *Taiacu*, the Muſk Deer, the Bezoar Sheep, and ſome Amphibious Animals, as the Muſquaſh, *&c.* who have their valuable Scent in a particular *Cyſtis* or Bag. I am apt to think, that which firſt gave Occaſion to the Notion of Ambergris being the Production of the Whale, was becauſe it was found in conſiderable Quantities on the Shores of the Summer Iſlands, and among the *Bahama's*, where the dead Whales are frequently wreck'd and broke up with the Sea, and the Ambergris found floating, or on the Shore; but here again, the Ingenious, until very lately, were at a Loſs, and divided in Opinion, for tho' they agreed it to come from the Whale, yet ſome took it to be the true and proper *Semen*, being found only in the Bull, at the Root of the *Penis*, near the Teſticles; others again thought it was the Ordure or Excrement of the Whale.

The

The beſt and moſt exact Account of Ambergris, that I have been able to procure (and I may truly ſay, I have taken a great deal of Pains for it) I very lately received from one Mr. *Atkins*, now an Inhabitant at *Boſton* in *New England*, who uſed the Whale Fiſhery, for ten or twelve Years together, and was one of the firſt that went out a fiſhing for the *Sperma Ceti* Whales, about the Year 1720, and then began to diſcover the Ambergris; and being a ſober ingenious Man, what he ſays may ſafely be depended on; tho', for Subſtance, I have had it from ſeveral of the Whale-men.

His Relation, which was taken a few Days ſince from his own Mouth, is as follows:

" The Ambergris is found only in the *Sperma Ceti* " Whales, and conſiſts of Balls or globular Bodies, of va- " rious Sizes, from about three Inches to twelve Inches " Diameter, and will weigh from a Pound and an Half " to twenty two Pounds, lying looſe in a large oval " Bag or Bladder of three or four Foot long, and two " or three Foot deep and wide, almoſt in the Form of " an Ox's Bladder, only the Ends more acute, or like " a Blackſmith's long Bellows, with a Spout running " tapering into and thro' the Length of the *Penis*, and " a Duct, or Canal, opening into the other End of the " Bag, and coming from towards the Kidneys; this Bag " lies juſt over the Teſticles, which are above a Foot " long, and is placed lengthways at the Root of the " *Penis*, about four or five Foot below the Navel, and " three or four above the *Anus*. This Bag or Bladder " is almoſt full of a deep Orange colour'd Liquor, not " quite ſo thick as Oil, and ſmelling ſtrong, or rather " ſtronger of the ſame Scent with the Balls of Amber- " gris, which float and ſwim looſe in it; the Inſide of " the Bag is very deeply tinged with the ſame Colour " as the Liquor, which may alſo be found in the Canal

" of

" of the *Penis*; the Balls ſeem to be pretty hard while " the Whale is alive, inaſmuch as there are many Times " found, upon opening the Bag, large concave Shelves, " of the ſame Subſtance and Conſiſtence, that have ſcaled " off from them, and the Balls themſelves ſeem to be " compoſed of ſeveral diſtinct Coats incloſing one ano- " ther, ſomething like the Coats of an Onion.

As to the Number of Balls, Mr. *Atkins* never found above four Balls in a Bag, and in the Bag where he found one that weighed twenty one Pounds, which was the largeſt he ever ſaw, there was no other in the Bag but that.

He further ſays, " That to one *Sperma Ceti* Whale " that has any of theſe Balls, there are two, that have " nothing but the deep Orange-colour'd Liquor afore- " ſaid in their Bags." This Remark confirms what a- nother Whale-man told me, " that the Ambergris was " found only in ſuch *Sperma Ceti* Whales as are old and " well grown." It is the general Opinion of the Whale- men, that the Ambergris is produced only by the Male or the Bull *Sperma Ceti* Whale. As to this Particular, Mr. *Atkins* ſays, " He never ſaw, nor certainly heard " of a *Sperma Ceti* Female taken in his Life, the Cows " of that Species of Whales being much more timorous " than the Males, and almoſt impoſſible to be come at, " unleſs when haply found aſleep on the Water, or de- " tained by their Calves." This is certain, the Boats can never come near them, when they are awake, they are ſo very ſhy and fearful.

Mr. *Atkins*'s Method of getting the Ambergris out of the Whale was thus; after the Fiſh is killed, he turns the Belly upwards, and fixes a Tackle to the *Penis*, then cuts a Hole round the Root of the *Penis*, through the Rim of the Belly, till he comes to the Entrails, and then ſearching for the Duct or Canal at the further End

of

of the Bag, he ties it pretty near to the Bag, and cuts the Duct off beyond it, upon which he draws forth the *Penis* by the Tail, and the Ambergris Bag, entirely follows it, and comes clean and whole out of the Belly.

* A very worthy Divine, and one of my intimate Acquaintance, in a neighbouring Town, who took the preceding Relation from Mr. *Atkins*, apprehends the Bag aforesaid to be the urinary Bladder, and the Ambergris Ball to be a certain Concretion, formed out of the greasy odoriferous Substance of the Liquor aforesaid contained within it. As for my own Part, I dare not pretend to give any Opinion upon the Point, but content myself with relating Matter of Fact.

I doubt not but that, in Process of Time, some farther Particulars may be procured with respect to Ambergris, and I shall be proud to transmit them; in the mean Time I hope the Society will accept of this first Essay, and allow my poor Country the Honour of discovering, or at least ascertaining the Origin, and Nature of Ambergris.

I cannot conclude this History of Whales, without acknowledging that I have been much assisted herein by the Reverend Mr. *Greenleafe* of *Yarmouth*, near *Cape-Cod*, and Mr. *J. Coffin*, sometime of the Island of *Nantucket*, both of them Places famous for the Whale-Fishery.

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* *The Reverend Mr.* Prince *of* Boston.

III. *An Enquiry into a Diſcovery, ſaid to have been made by Signor* Valſalva *of* Bologna, *of an Excretory Duct from the* Glandula Renalis *to the* Epididymis. *By Mr.* John Ranby, *Surgeon, F. R. S.*

THE late learned Signor *Valſalva* having ſome Time ago diſcover'd a Veſſel, which he took to be an Excretory Duct going from the *Glandula Renalis*, to the *Epididymis* in Men, and the *Ovarium* in Women; an Account whereof was publiſh'd in the *Giorn. de Litterati*, 1719. and afterwards inſerted into the *Philoſophical Tranſactions* N° 385. I lately procur'd a human Body, in order to ſearch for it, which I did with all the Diligence I was capable of. I was not ſo happy as to diſcover any Duct of this Kind, but, having injected the *Aorta*, I found the Arteries going to the *Glandulæ Renales* diſpos'd, as hath been repreſented according to the Life, in the annexed Figure. Now whether that Branch of the Artery, which from the *Glandulæ Renales* goes down on both Sides towards the Teſticles, without ſupplying any of the neighbouring Parts, might not have been miſtaken by the abovementioned learned Anatomiſt for an Excretory Duct, all Arteries being generally in dead Bodies free from Blood, and of a whitiſh Colour, I will not preſume at preſent to determine, for Want of farther Experiments.

IV. *An*

EXPLANATION.

Fig. VI.

A A The right Kidney.
B B The left Kidney.
C C The deſcending Trunk of the *Aorta.*
D The right emulgent Artery.
E. The left emulgent Artery.
F The right *Glandula Renalis.*
G G The right ſpermatick Artery.
H The left *Glandula Renalis.*
I I The left ſpermatick Artery.
K K The Ureters.

a a A ſmall Artery ariſing out of the deſcending Trunk of the *Aorta,* a little above the right emulgent Artery. It ſends two Branches *b b* upwards to ſupply the right *Glandula Renalis.* A third Branch *c c* goes downwards towards the right ſpermatick Artery G G, and then farther in Company with the ſame to the right Teſticle.

d d A ſmall Artery ariſing out of the left emulgent Artery, near the deſcending Trunk of the *Aorta,* and going directly upwards to the left *Glandula Renalis* H.

e e A ſmall Artery ariſing out of the deſcending Trunk of the *Aorta,* a little below the left emulgent Artery. It divides into two Branches, one whereof *f f* goes upwards between the emulgent Artery and Vein to the left *Glandula Renalis*, the other *g g* downwards, towards the left ſpermatick Artery, and in Company with the ſame to the left Teſticle.

IV. *An Account of a Book entituled,* Prodromus Cryſtallographiæ. De Cryſtallis impropriè ſic dictis Commentarium. A Mauritio Antonio Cappeler, M. D. & Centumviro Lucernenſi. Lucernæ 1723. 4o. *By* J. G. Scheuchzer, *M. D. Coll. Med. Lond. Lic. R. S. S.*

THE Author of this Treatiſe takes notice in the Preface, that it is only a ſmall Part of a larger Work, which he promis'd the Publick ſome Years ago, under the Title of *Cryſtallographia*, and hath now almoſt ready for the Preſs. This greater Work, as he intimates at the Cloſe of this Diſcourſe, is to conſiſt of three Parts, the firſt of which contains the Definition of Cryſtal, with the Synonyms given it by ſeveral Authors both ancient and modern, and an Account of its Properties, Figure, Pellucidity, ſpecifick Gravity, and Bigneſs; as alſo the Place of its Growth, chiefly in regard to *Swiſſerland*, the moſt plentiful Country in *Europe*, as to this Sort of natural Productions; the Signs, whereby hidden Cryſtal Mines may be diſcover'd, and the Way of working them. In the ſecond Part will be examined the Opinions of ſeveral Natural Hiſtorians, about the Origin of Cryſtal, and the Author's own eſtabliſh'd and prov'd. In the third Part will be ſhewn the Uſes of Chryſtal, both Phyſical and Mechanical, and ſome few Hints given, relating to the juſt Value the World has at all Times put upon this beautiful Production of Nature.

The Author divides this preſent Eſſay into two Parts, *viz.* a ſhort Commentary upon the *Cryſtallos*, as he calls them, *impropriè ſic dictos*, in the firſt; and an Account of ſuch as he found mentioned in ſeveral Authors, with a Reduction of them under certain Heads, in the ſecond. *Cryſtalli impropriè ſic dicti*, according to the Author's Definition, are ſuch Bodies, either Stones, Metals, or Salts, as have any Reſemblance with the true Cryſtal; either, as to their multangular, regular, or irregular Figure, or as to their Pellucidity, or any other of its eſſential Properties. As the Number of theſe Bodies is very extenſive, ſo an exact Enquiry into them cannot but be both uſeful and agreeable. I juſt now obſerved, that the *Cryſtalli impropriè ſic dicti* are taken out of Stones, Metals, and Salts. To the firſt belong amongſt other Things ſuch precious Stones, as, in all Probability, have their certain, determined Cryſtal like Figures, as Diamonds, Amethyſts, *&c.* To the ſecond belong all Sorts of *Pyritæ*, as alſo the Growth of Silver, and other Metals, in Form of Trees, or other Things. By the third are underſtood all Chymical Preparations of Salts, and ſaline Bodies, the Figure of which is, generally ſpeaking, more accurate, than in any of the two former. Several Authors of Note have endeavour'd to explain how, Cryſtalliſation is perform'd, or how it comes, that certain Subſtances ſhoot into Cryſtals. Dr. *Cappeler* mentions the Hypotheſis of the learned *Gulielminus*, and that of a late *Swediſh* Author, *Swedenbergh*; and though he ſeems more to favour the latter, than he doth the former; yet he thinks, that they are both liable to Exceptions. But whatever the Cauſe or Method of Cryſtalliſation be, our Author takes it for granted, that three Things are abſolutely requir'd for it, *viz. Salt*, which muſt al-

ways be an Acid, as is evident by Chymical Experiments, and the very Taſte of ſaline Cryſtals, *Water* and *Earth*. Cryſtalliſation, as far as can be gueſs'd by Chymical Obſervations, is perform'd thus: Particles of certain determined Shapes, ſwimming in a Fluid of a certain Conſiſtence, are, either by the inteſtine Motion of this Fluid, or by the Motion of the Air, ſuppoſed to circulate perpetually through its Pores, or by ſome other Cauſe, brought together, ſo as to form larger Bodies of a Figure proportionable both to the Degree of Impulſion, and the primitive Shape of the conſtituent Parts, or determined by theſe two Things. This Act of Cryſtalliſation, though uniform, as to the Union of Particles conſider'd in itſelf, is yet obſerved to be very different, and to have different Effects, with reſpect to the different Nature of the Fluid, in which Cryſtals are formed, and the Degree of Perfection, to which they are brought. Our Author mentions ſix different Kinds of Cryſtalliſation, each of which, he intends to explain more fully in his larger Work. The firſt Sort of Cryſtalliſation, which hath been examin'd with a great deal of Accuracy by *Gulielminus*, is performed in an aqueous Fluid, wherein ſaline Particles have been diſſolved, boil'd to a certain Degree of Conſiſtence, commonly that of a thin *Pellicula* covering its Surface. This aqueous Fluid muſt be afterwards repos'd in a cool Place, that the ſaline Particles contain'd in, it may form themſelves into Cryſtals, which is done in more or leſs Time, according to the different Nature both of the Fluid and Salts. All Chymical Preparations of Salts, the Origin of precious Stones, and of the Cryſtal itſelf, belong to this firſt Kind. Our Author obſerved, that in the *Spiritus aperitivus Glauberi*, (which is a Preparation of *Ciner. Clavell.* p. ii. & *Sal Armon.*

Armon. p. i.) after a Year's ſtanding, form'd themſelves artificial Cryſtals, in Figure and Pellucidity exactly like the true ſexangular Cryſtal, and pointed on both Sides. The ſecond Kind of Cryſtalliſation differs from the firſt only in this, that it is performed not in a thin, pellucid, aqueous Fluid, but between thick, mineral, or metallical Mixtures, corroded by acid Salts in the Bowels of the Earth. The third Sort is of a middle Kind between the firſt, and a Coagulation, *viz.* when the Fluid, in which Salts have been diſſolved, is, by degrees, entirely evaporated. This Way of Cryſtalliſation is more proper for diſcovering the primitive Shapes of ſaline Particles. Our Author hath examined ſeveral Salts, both Mineral and Vegetable, and ſeveral Chymical Preparations after this Manner, and hath given us their Figures, as they appear'd to him under a good Microſcope, in two Tables annexed to this Treatiſe. The fourth Sort of Cryſtalliſation is perform'd in a ſtill thinner Fluid than Water is, or in the Air; the Sublimations of the Chymiſts, the Diſtillation of volatile Salts; Snow, whoſe wonderful Figure hath been thought worthy the Amuſement of ſeveral eminent Men; Hail, which is again of very different Shapes; Froſt, and that admirable Variety of Trees, Landſkips, and other inimitable Figures, which, in very froſty Weather, appear upon Glaſs-Windows, or other pellucid Bodies, muſt be all reduced to this fourth Sort. The fifth Sort of Cryſtalliſation is perform'd upon the Surface of a thicker Fluid, as Water, between that and a thinner one, as Air; of this Kind is chiefly Ice. The ſixth and laſt Sort of Cryſtalliſation, mention'd by our Author, differs from all the former, in that it is perform'd neither by the Riſe of Vapours, nor by the repoſing of any Fluid, but on the contrary in a Fluid, which is in a perpetual Motion. That Sort of icy Concretions, which

is

is obſerv'd near ſwift running Waters, and is commonly very porous, not unlike the Tophus, and the ſtony Concretions in ſubterranean Cavities, call'd *Stalactitæ*, belong to this laſt Sort. Thus far what is contained in the firſt Part,

The ſecond gives an Account of ſuch *Cryſtalli improprie ſic dicti*, or Cryſtalline Bodies, which are not properly Cryſtals, as have been mentioned by ſeveral Natural Hiſtorians both ancient and modern. The Author diſtributed them into the following Claſſes, each of which comprehends Stones, Metals and Salts.

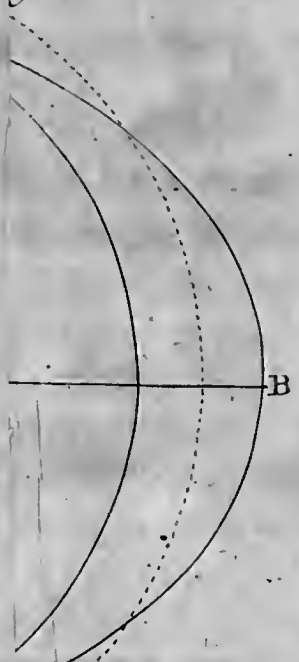
Corpora Cryſtalliſata, quæ improprie Cryſtalli vocantur.

Claſſ. I. Globoſa, rotundata & ſphæroidea.
——— II. Conica, Conoidea & fuſiformia.
——— III. Cylindrica, ſolida aut tubulata.
——— IV. Pyramidalia & cuneiformia.
——— V. Priſmatica, Parallelepipeda, Rhomboidea, Trapezoidea.
——— VI. Polyedrica, & Polygona, regularia & minus regularia.
——— VII. Racemoſa, Arbuſculorum in modum, & filamentoſa, filorum, aut capillorum inſtar naſcentia & ſtriatim contexta.
——— VIII. Cruſtis, ſquamis, lamelliſque contexta, ſine, vel cum figura propria.
——— IX. Corpora, quorum cum Cryſtallo affinitas in pelluciditate potiſſimum conſiſtit, figurâ eorum nativâ, vel incertâ vel nondum perſpectâ.

F I N I S.

2.

Fig. 4.



Numb. 388.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *May* and *June*, 1725.

The CONTENTS.

Numb. 388.

PHILOSOPHICAL TRANSACTIONS.

For the Months of May and June, 1742.

The CONTENTS.

I. *A Diſſertation concerning the Figure of the Earth. Part the Second. By the Reverend* J. T. Deſaguliers, *LL. D. F. R. S.*

SINCE my * Paper concerning the Figure of the Earth was read before the Royal Society, I met with a Diſſertation of Monſ. *Mairan* (in the Memoirs of the Royal Academy of *Paris*, for the Year 1720.) wherein the learned and ingenious Author has taken a great deal of Pains to reconcile the Obſervations made on Pendulums, (found to be ſhorter at the Æquator than at *Paris*, when they ſwing Seconds) with the oblong ſpheroidical Figure of the Earth, deduced from Monſ. *Caſſini*'s Meaſures. And tho' upon a ſtrict Examination of his Conjectures, and what he gives for Demonſtrations, I do not find Reaſon to alter my Opinion concerning the oblate or flatted Spheroid, which Sir *Iſaac Newton* has ſhewn to be the Figure of the Earth; yet ſince it might be thought by ſome, who have read Monſ. *Mairan*'s Treatiſe, and afterwards may read mine, that I have not conſider'd all the Circumſtances that He has done, and that I have not been exact enough in the Mathematical Part of my Diſſertation, becauſe I have drawn ſome Concluſions from ſuppoſing the Figure of the Earth ſpherical, when I ſhould have ſuppos'd it an oblong Spheroid; I beg Leave to ſhew here, wherein I think Monſ. *Mairan* is miſtaken, and to give thoſe additional Proofs of my Aſſertions, which I promis'd the Society when I gave in my laſt Paper.

* *Vid.* Philoſ. Tranſ. N°. 386, 387.

First then I begin with the Conjectures.

Monſ. *Mairan* ſays, that it is as reaſonable to ſuppoſe the Earth (if it was once fluid) to have been an oblong Spheroid at firſt, as a Sphere; and that, in ſuch a Caſe, the Centrifugal Force of the ſeveral Parts of the Earth, ariſing from its Revolution about its Axis, which might convert a Sphere into an oblate Spheroid, wou'd only change an oblong Spheroid into one leſs oblong.

If the Earth was at firſt a Fluid, (ſuppoſed homogeneous, and of any given Form,) and left to thoſe Laws, which we find to obtain at preſent; it muſt put on a ſpherical Figure, for the ſame Reaſon that Drops of Mercury, of Water, and other Fluids, put on ſuch a Figure. And to ſuppoſe any Change made in that Figure from the Preſſure of an external Fluid, filling up all Space, is contrary to what has been demonſtrated by Sir *Iſaac Newton* in his *Principia Lib.* 2. *Prop.* 19. where he ſhews, *That if any Portion of a Fluid be compreſs'd by the ſame or any other homogeneous Fluid, that Portion will not have its Figure alter'd by that Preſſure.*

And indeed we ſee, that in the Receiver of the Air-Pump, Lumps of Butter, coagulated Oil, or Honey, Drops of Quickſilver or Water, *&c.* have the ſame Figure, whether the Preſſure of the Air acts upon them, or be taken off by exhauſting the Receiver.

That a fluid Subſtance, of any Figure, will by the Gravity of its Parts become ſpherical, is plain by the following

DEMONSTRATION. *Fig.* I,

Let ABCDE be a Portion of an homogeneous Fluid, whoſe Parts tend towards one another, and whoſe Figure is

is not ſpherical. If in ſuch a Fluid we ſuppoſe a Syphon as ACE (or which is the ſame thing, if all the Fluid ſhou'd be frozen, except the Canal ACE) whoſe Legs AC and CE are unequal, and meet at C, the Center of the Fluid, towards which there is the greateſt Tendency; the Fluid will run out at A in the Leg AC, till it be come down as far as *g* in the Leg CE, ſuppoſing C*g* equal to AC. But if the Leg AC be lengthened as far as *c*, then the Fluid will only come down as far as *e* in the Leg CE, and at the ſame time riſe up to *a* in the Leg C*a*, C*a* being equal to C*e*.

If ſuch another Canal or Syphon be ſuppos'd at BCD, the Fluid in it will come down from D to *d*, and riſe from B to *b*. And ſince ſuch Syphons may be ſuppos'd all over the Fluid ABDE; that Fluid, by the mutual Tendency of its Parts towards one another, muſt be reduc'd to the ſpherical Figure *abde*. *Which was to be demonſtrated.*

Now, without conſidering the Unreaſonableneſs of the Suppoſition, let us imagine the Earth to have been an oblong Spheroid at firſt, and then to have a diurnal Revolution given to it, which ſhou'd by Degrees ſhorten its Axis, to bring it to what Meſſieurs *Caſſini* and *Mairan* ſuppoſe it at preſent to be. If in ſuch a Caſe the Earth be ſuppos'd fluid enough to change its Figure, by the Revolution about its Axis, why ſhould it ſtop when the Æquatorial Diameter comes to want juſt one 96th Part of the Length of the Axis? ſince two Powers act upon it to ſhorten its Axis, *viz.* Gravity, and the Centrifugal Force; the firſt of which has already been ſhewn capable to reduce it to a Sphere, and the Centrifugal Force is acknowledged by Monſ. *Mairan* to be (as Sir *Iſaac Newton* has prov'd it) at the Æquator equal to $\frac{1}{289}$ Part of the Gravity there. Certainly the Alteration of Figure wou'd not have ſtopp'd, before the

Earth came to be a Sphere; nay, and it must have risen at the Æquator; and how much, I have already shewn in my former Paper.

Again, if we suppose the Earth of an heterogeneous Fluid, before the diurnal Revolution, the heaviest Parts wou'd go towards the Center, and the lighter towards the Surface; and that Way the Terraqueous Globe wou'd also become a Sphere. Then if, when the Central Parts are fix'd, and the superficial *Strata* are still fluid, the Earth receives a diurnal Motion; it will rise at the Æquatorial Parts, and that to a greater Height than what I have shewn in my former Paper, where I suppos'd the Earth of uniform Matter. And that something like this must be the Case, appears from what Sir *Isaac Newton* has said upon this Subject. For after having shewn, from supposing the Earth of uniform Matter, that the Centrifugal Force of all its Parts wou'd bring it to be $17\frac{1}{2}$ *English* Miles higher at the Æquator than at the Poles, and after having given a Table of the proportionable Decrease of the Length of the Degrees of a Meridian of the Earth, going from the Poles to the Æquator, in such a Figure of the Earth, with the Lengths that Pendulums must have to swing Seconds in several Latitudes; from a Comparison of the Lengths of Pendulums (observ'd by different Persons to be shorter towards the Æquator, than in greater Latitudes (when they swing Seconds) he shews that the Earth must be $31\frac{7}{12}$ Miles higher at the Æquator than at the Poles; and therefore that it must be denser towards the Central than the Superficial Parts to produce a flatted Spheroid, where the Æquatorial Diameter must exceed the Axis so much more; that is, be longer something more than $\frac{1}{125}$ Part.

Lastly, let us suppose the Earth, at its first Creation, to have been made of Land and Water, the first as solid, and

and the laſt as fluid as it is now, but of Monſ. *Caſſini*'s Figure, and examine the Conſequence. Since in that Figure the Axis is $\frac{1}{578}$ Part longer than the Æquatorial Diameter; the Gravity will be ſo much greater at the Æquator than at the Poles, that the Waters will all flow to the Æquatorial, and leave the Polar Regions; which will happen ſtill more by the Centrifugal Force, which the Earth in its diurnal Motion will give to the Fluid; and therefore the Sea wou'd be $43\frac{85}{100}$ Miles (reckoning 5000 Feet to a Mile) higher at the Æquator than at the Poles, which muſt overflow all the Torrid Zone, and leave the Polar Regions dry.

I am very well aware, that it may be objected by ſuch as have read Monſ. *Mairan*'s Diſſertation, and have not read Sir *Iſaac Newton*'s *Principia*, or have not read that Book with due Attention — "That I have not argued fairly in drawing Conſequences from a greater "Gravity at the Æquator than at the Poles, in an oblong Spheroid; becauſe Monſ. *Mairan* has ſhewn, "that, in ſuch a Figure of the Earth, the Gravity is greater "at the Poles than at the Æquator; and that I ſhou'd "have drawn my Conſequences from theſe Principles." To which I anſwer, that his Demonſtrations about Gravity are built upon wrong Suppoſitions, as I ſhall ſhew by and by. Nevertheleſs, ſuppoſing that Gravity was greater towards the Poles than towards the Æquator, in the Proportion that he aſſigns, namely of the Ray of Curvature drawn into the Perpendicular to the Curve, terminated at the Axis; let us conſider what will follow from his Principles.

* Let us then ſuppoſe the Earth at firſt in a fluid State; A A the Axis, *d*Æ the Æquatorial Diameter, *a b* a Ray of Curvature, *d n* another, *a c* and *d*C two Lines

* Fig. II.

of

of Tendency or Perpendiculars to the Curve, intercepted by the Axis at *c* and C; and *d* C, A C, two Tubes or Canals of the Fluid, gravitating towards, and communicating at C. I ſay that, according to Monſ. *Mairan*'s Principles of Gravity, the Earth cannot preſerve its oblong ſpheroidical Figure. For ſince the Gravity at *a* : Is to the Gravity at *d* :: As $dn \times dC$: to $ab \times ac$, it will follow (from the Nature of the Ellipſe) that the Gravity at A : will be to the Gravity at *d* :: As AC^4 : to dC^4: and therefore the Forces, with which the Columns of Fluid A C and *d* C tend towards C, will be as their Maſſes drawn into the Forces driving towards C, that is, as $AC \times AC^4$ to $dC \times dC^4$. Now by the Principles of Hydroſtaticks, it is evident that the Fluid, in the Canal A C, will cauſe the Fluid in the Canal *d* C to run out at *d* as long as $AC \times AC^4$ is greater than $dC \times dC^4$: And if the Canal C *d* be continued quite to δ, the Surface of the Fluid in A C will ſink to α, whilſt the Surface of the Fluid in *d* C riſes up to δ, in which Caſe as $\alpha C = C\delta$, the Point A will come to α, and the Point *d* to δ, and the Curve A *d* being chang'd into $\alpha\delta$, the oblong Spheroid will be chang'd into a Sphere, the only Figure conſiſtent with the *Æquilibrium* of the fluid Parts, according to Monſ. *Mairan*'s own Principles; becauſe then you will have $AC^4 = dC^4$, and $AC \times AC^4 = dC \times dC^4$. If we make uſe of Sir *Iſaac Newton*'s Principles in this Reaſoning, we ſhall alſo ſhew, that an oblong, ſpheroidical, fluid Earth will be chang'd into a Sphere; but not ſo faſt as it does by Monſ. *Mairan*'s Laws; for, according to Sir *Iſaac Newton*, the Gravity at A : Is to the Gravity at *d* :: As $\sqrt[5]{Cd}$: $\sqrt[5]{AC}$. *Q. E. D.*

---

* Fig. II.

NB. *Here*

NB. *Here we have ſuppos'd no diurnal Revolution, for as ſoon as that begins, the Centrifugal Force will raiſe the Æquatorial Parts, and change the Sphere into a flatted Spheroid, as has been before ſhewn, and is allow'd by Monſ.* Mairan.

Now if we ſuppoſe the ſame Figure of the Earth, but the Land (at its firſt Creation) as firm as it is now; it will in that Caſe follow from Monſ. *Mairan*'s Principles, that the Sea muſt riſe and overflow all the Æquatorial Regions, tho' the Earth had no diurnal Revolution; and much more ſo, when the Centrifugal Force, ariſing from the diurnal Motion, helps to carry the Water the ſame Way.

DEMONSTRATION.

Let P *æ* P Æ * repreſent the Plane of a Meridian, P P the Axis of the Earth (ſuppos'd an oblong Spheroid) *æ* Æ the Diameter of the Æquator, *d e a æ* Part of the Surface of the Earth, *æ* A and *e* B two Perpendiculars to the Surface of the Earth (which are here two Rays of Curvature) *f c* the Surface of the Sea, and *f d e g*, *b a æ c* two Cylinders of Sea-Water of equal Baſes and equal Heights.

Since Gravity acts on the two equal Columns of Water *b a c æ*, *f d e g* in the reciprocal Ratio of the Ray of Curvature (at the reſpective Places of the Columns) drawn into that Part of it which Monſ. *Mairan* calls the Line of Tendency, (that is, in the Ratio of *e* B × *e* Z to *æ* A × *æ* C) the Weight of *f e* : will be to the Weight of *b æ* : : As *æ* A × *æ* C : to *e* B × *e* Z. Therefore if there be a Communication between the fluid Co-

* Fig. III.

lumns

lumns *f e* and *b æ*, there cannot be an *Æquilibrium*, till the Quantity of Matter in *f e*, becomes to the Quantity of Matter in *b æ*, reciprocally as the Gravity at the Place *æ* is to the Gravity at *e*; and in that Case the Height *g e* will be reduc'd to *k e*, if *k e* : *c æ* :: *e* B × *e* Z : *æ* A × *æ* C. And consequently the Surface of the Sea will go thro' the Points *i k b c*, where *b c* under the Æquator is higher than *i k* towards the Poles. *Q. E. D.*

NB. *That the Centrifugal Force will still add to the Height of the Sea at* b c, *is plain from what we have said before. And if we apply these Principles to determine the different Lengths of Pendulums, swinging Seconds at* Paris *and at the Æquator; from the Gravity at* Paris, *compar'd to the Gravity at the Æquator (in this Supposition of the Action of Gravity and Figure of the Earth) a Pendulum must be shorter at the Æquator by more than* 10 *Lines, without considering the Centrifugal Force; and if the Centrifugal Force be taken into Consideration, the Pendulums must be shortened near a whole Inch. But this being about five Times more than agrees with Observation; what proves too much, proves nothing at all.*

Having thus shewn, that Monsr. *Mairan*'s Account of the Action of Gravity, on several Places upon the Earth's Surface, can be of no Service for reconciling the Experiments made on Pendulums, with the Figure of the Earth deduc'd from Monsr. *Cassini*'s Measures: I proceed to shew that his Demonstrations are founded upon wrong Principles. And first, in Relation to Gravity.

This Gentleman has follow'd Sir *Isaac Newton*, in saying, that Gravity increases in a duplicate reciprocal

Proportion of the diminiſh'd Diſtance from the Center of the Force, and ſo *vice versâ*; but he has follow'd Sir *Iſaac Newton* no farther than ſerv'd his preſent Purpoſe; otherwiſe he wou'd have known.—That in reſpect to a Central Body (as a Planet) towards which others are (*attracted* or) impell'd by Gravity, this Law obtains only, as Bodies attracted, are remov'd from the Surface of the Planet, to greater Diſtances from the Center compar'd with that Diſtance; or as from greater Diſtances they approach nearer to the Planet. — That the greateſt Action of Gravity is at the Surface of the Planet.—That afterwards in advancing towards the Center, the Force of Gravity, on the Body attracted, continually grows leſs, decreaſing directly as the Diſtance; and that this holds true in a Spheroid as well as a Sphere. — That on different Parts of the Surface of the Earth (in the Condition it is now) the Gravity on Bodies is reciprocally as their Diſtance from the Center of the Earth. — That though at a conſiderable Diſtance we look upon the Earth, or any Planet, or even the Sun, as a Point (in the Center of the Forces tending towards it) endued with an abſolute Force, proportional to its Quantity of Matter; yet when we come ſo near the Body as to conſider the Space it takes up, we are to take notice, that the whole Attraction or Gravity of the Body, is made up of the Sum of the Attraction of all its Parts properly combin'd; and therefore, that when a Corpuſcle, or Body attracted, comes to be within the Planet, or Body attracting, the Matter above it draws it back in ſuch a Manner, that it leaves it only a Force to go on towards the Center, which is directly as the Diſtance, as we have already ſaid; juſt as if a Body concentric to the Planet (whether ſpherical or ſpheroidical) had its Surface juſt where the Corpuſcle is, and all the exterior Cruſt or Shell was annihilated.

I do not doubt but Monſ. *Mairan* will be of this Opinion, when he has carefully and impartially examin'd the 12th and 13th *Sections* of the Firſt Book of Sir *Iſaac Newton*'s *Principia*, and the 18th, 19th, and 20th *Prop.* of the Third Book. And if he will be at the Pains to compare the 38th and 39th *Propoſition* of the Third Book with the 66th of the Firſt, he will find that the Preceſſion of the Æquinoxes is owing to the broad ſpheroidical Figure of the Earth; and that if it had Monſ. *Caſſini*'s Figure, the Æquinoctial Points wou'd move *in Conſequentia* faſter than they do now *in Antecedentia.*

Further, Monſ. *Mairan* demonſtrates, that in an oblong Spheroid, the Diminution of Gravity, by the Centrifugal Force, encreaſes faſter in going from the Poles to the Æquator, than it wou'd do in a Sphere, and faſter in a Sphere than it wou'd do in a broad Spheroid; and therefore wou'd ſhew, "That notwithſtanding the Surface of the Earth is nearer to the Center in Monſ. *Caſſini*'s Figure than in Sir *Iſaac Newton*'s, yet the Centrifugal Force will diminiſh the Gravity ſo faſt in going from *Paris* to the Æquator, that the ſhortening of Pendulums, to make them ſwing Seconds at the Æquator, may very well be accounted for that Way."

Now let us examine into this Matter, to ſee whether the Cauſe is adequate to the Effect.

If the Diſtance from the Surface of the Earth at the Pole to the Center be 96, and the Diſtance of the Surface at the Æquator be 95, the Diſtance of the Surface at *Paris*, in the Latitude of 48° 50′, will be 95,562, *&c.* by the Property of the Ellipſe. Now ſince the Force of Gravity, in different Places on the Earth's Surface, is reciprocally as the Diſtance from the Center, and the Lengths of Pendulums, that perform their Vibrations in the ſame Time, are directly as the Force of Gravity; therefore

therefore the Length of Pendulums at *Paris*, will be to their Length at the Æquator, as 95 to 95,562, &c. that is, as 440,555, &c. to 443,165, &c. and conſequently they muſt be lengthen'd 2,61 &c. Lines. But as from Monſ. *Mairan*'s Principles, the Diminution of Gravity by the Centrifugal Force, is greater at the Æquator than at *Paris*, hardly $\frac{1}{440}$ Part of the whole Gravity at the Æquator, the Pendulums muſt be ſhortened in that Proportion; ſo that then the Length of a Second-Pendulum, will be 440,555 + 2,61 — 1 Lines. But as that Quantity is greater than 440,555, &c. therefore the Pendulums upon the Whole muſt be lengthen'd: Nay, though we ſhou'd allow a ſhortening of two Lines; ſince by Obſervation Pendulums are found to be about two Lines ſhorter at the Æquator, the oblong ſpheroidical Figure of the Earth cannot be conſiſtent with the Experiments on Pendulums.

I beg Leave to ſet down Monſ. *Mairan*'s aforeſaid Demonſtration here; that we may ſee whether he has aſſum'd true Principles.

PROPOSITION V.

" * XI. *The Centrifugal Force at any Degree of*
" *Latitude, taken upon the oblong Spheroid, between*
" *the Æquator and the Pole, is leſs in Compariſon*
" *to the Centrifugal Force at the Æquator, than it*
" *wou'd be at the ſame Degree of Latitude taken upon*
" *a Sphere; or, which is the ſame thing, the Centri-*
" *fugal Force encreaſes more, going from the Poles*
" *towards the Æquator, upon an oblong Spheroid,*
" *than upon a perfect Sphere; and conſequently Gra-*
" *vity diminiſhes more, and a Pendulum muſt be more*

* *See Monſ.* Mairan's *Diſſertation, Article* XI. &c.

" *ſhortened under the Æquator, in the Hypotheſis of* " *the oblong Spheroid, than in that of a perfect* " *Sphere.*

' Having deſcrib'd an oval Curve of any Kind, as " for Example, the Ellipſe * ADBE abovementioned, " and inſcrib'd the Circle DHE, whoſe Radius is DC " = half the ſhorter Axis DE; upon AD take any " Point as R, between the Æquator and the Pole, and " from that Point to the *Evoluta* OTX draw the *Ray* " *of Curvature* RT, which gives the *Line of Tendency* " RP (*Art.* IV.) Draw likewiſe from the common Cen- " ter C, to the Circumference of the Circle DH, a Ra- " dius CV, parallel to PR, and meeting the Circle at V; " then from the Points R, V, draw the Lines RN, VZ, " perpendicular to the Axis AB.

" It muſt be obſerv'd, *Firſt*, That as the Ellipſe AD " repreſents a Meridian of the oblong Spheroid, the " Circle DH repreſents a Meridian of a Sphere in the " ſame Plane.

" *Secondly*, That the Point V, on the Circular Me- " ridian, anſwers to the ſame Degree of Latitude as " the Point R, upon the elliptical Meridian; becauſe " the Lines PR, CV, being parallel to each other, and " perpendicular, the one to the Ellipſe and the other " to the Circle (*by Conſtruction*) the touching Planes, " or Horizons of the Points R, V, will alſo be pa- " rallel.

" *Thirdly*, Whence it follows that the Diminution of " the Centrifugal Force (acting againſt Gravity) on ac- " count of its Obliquity to the Horizon (*Art.* X.) of

* Fig. IV.

" the

" the ſame Degree of Latitude on the Elliptical and on " the Circular Meridian, is the ſame in both Caſes, and " in the ſame Ratio as the abſolute Centrifugal Forces " repreſented by the Perpendiculars R N, V Z, (*Art.* IX.) " Therefore to know whether the Centrifugal Force " (whether abſolute or relative) of the Point R, upon the " oblong Spheroid A D B E, be leſs or greater in reſpect " to the Centrifugal Force under the common Æquator " D E, than the Centrifugal Force (whether abſolute or " relative) of the correſpondent Point V upon the Sphere; " nothing more is requir'd than to ſee which is the " longeſt of the two Perpendiculars, namely, R N in " the oblong Spheroid, or V Z in the Sphere; ſince " theſe two Lines expreſs the Radii of the Circles of " Revolution, and conſequently the abſolute Quantity " of the Centrifugal Forces.

" *4thly and laſtly*, That the Ratio of the Centrifugal " Forces of two correſpondent Points upon the oblong " Spheroid A D B E, and the inſcrib'd Sphere D H E, to the " Centrifugal Force of their Æquators is the ſame, ſuppo- " ſing the Sphere of any other Bigneſs; and that it has " been determin'd here of the Diameter D E, only to " render the Demonſtration eaſier, by giving the ſame " Conſequent to the Antecedents R N and V Z. For " if about the Center C and with the Radius C *d*, the " Circle *d h e* be deſcrib'd equal (for Example) to a " Meridian of a Sphere of the ſame Solidity as the ob- " long Spheroid A D B E; and the Radius C V be pro- " duc'd till it meet the Circle *d h* at the Point *u*, and " *u z* be let fall perpendicular to the common Axis of " Revolution, and parallel to V Z: It is plain, that " we ſhall always have V Z : D C :: *u z* : *d* C, or " $\frac{VZ}{DC} = \frac{uz}{dC}$, and conſequently $\frac{RN}{DC}$ will have the

" ſame

" ſame Ratio to $\frac{VZ}{DC}$ as to $\frac{uz}{dC}$.

" Therefore, in order to demonſtrate that the Centri-
" fugal Force of a Point, taken in any Latitude upon
" the oblong Spheroid, is leſs when compar'd to the Cen-
" trifugal Force of the like Point, taken upon a Sphere
" in reſpect to the Centrifugal Force at the Æquator;
" there is nothing more requir'd than to ſhew that RN
" $\prec$ VZ, becauſe by that means we ſhall have $\frac{RN}{DC}$
" $\prec \frac{VZ}{DC}$.

" This being obſerv'd; from the Point R, draw the
" Line RI, parallel to the Axis AB, and meeting the
" Circle DH at K, and the Diameter DE of the Æqua-
" tor at the Point I. From the Point K having let fall
" the Perpendicular KL = RN, upon the Axis AB,
" and drawn KC to the Center C; the Queſtion will be
" brought to this, *viz.* To know whether the Point V
" coincides with the Point K; or whether it is above it
" towards D, or below towards H.

" But CK = CV = CD $\succ$ PR (*Art.* VIII.) there-
" fore CK and PR being both between the Parallels
" AC, RI, the greateſt CK is more inclin'd to them
" than the leaſt PR, and the Angle KCA is leſs than
" the Angle RPA = VCA. And ſince theſe two
" Angles have each of them one of their Sides coinciding
" with the Line AC, namely, the Side AP of the Angle
" RPA, and the Side AC of the Angle KCA, it fol-
" lows that the Side VC of the Angle VCA = RPA
" $\succ$ KCA, will go above CK between CK and CD,
" and meet the Line RI at the Point G, between K and
" J, and the Circle DH at the Point V, which conſe-
" quently

" quently will be above RI, between K and D. There-
" fore CV = CG + GV is = PR + GV, and con-
" sequently VZ, which meets RI at the Point F, is
" = ZF + FV = RN + FV; and therefore RN
" = VZ — FV. Therefore RN ≺ VZ.

" And because the same Thing may be demonstrated
" in respect of any other Point, taken between the Æ-
" quator and the Pole; and that Gravity, and conse-
" quently the Length of a Pendulum diminishes, as the
" Centrifugal Force encreases. Therefore *&c. Q. E. D.*

COROLLARY.

" XII. From what has been demonstrated, and from
" *Prop.* 3. *Art.* VIII. it follows, that the Perpendicular
" which is drawn from any Point of an oval Meridian
" to the Axis, will be so much shorter, in Comparison to
" the Perpendicular drawn from the correspondent Point
" of an inscrib'd circular Meridian, as the Latitude is
" greater; and consequently (by *Art.* XI. *Num.* 3.)
" the Centrifugal Force will be so much the less, and
" Gravity so much the greater, upon the oblong Sphe-
" roid, in respect to the Centrifugal Force, and the Gra-
" vity under the Æquator.

" For as the Line RP does always decrease, as the
" Point R is taken nearer to the Pole A, it is evident,
" that the Angle VCK will continually encrease, in
" respect to the Angles VCA, KCA, as it is their Dif-
" ference, and consequently that the Perpendicular VZ
" will be so much greater than the Perpendicular
" KL = RN.

I pass over the Demonstration of the latter Part of his Proposition above-mentioned, which he deduces justly from his Construction, if what he says (*Num.* 2.) be right; because in such a Case it cannot be call'd in Que-

stion;

ſtion; and proceed to an Obſervation that he makes afterwards, *viz.* "*We muſt take care to obſerve in the* "*foregoing Propoſitions and Corollaries, that the* "*Compariſon is always made between two ſimilar* "*Points of Latitude, taken upon the two Spheroids,* "*or upon one of the Spheroids and the Sphere,* "*between the Æquator and the Poles, in reſpect to* "*the Centrifugal Force upon the Æquator of any* "*one of theſe Spheroids, or of the Sphere. For if* "*we only compar'd abſolutely the Centrifugal Force* "*of a Point of the Æquator of the one, to the Cen-* "*trifugal Force of a correſpondent Point of the Æ-* "*quator of the other, it is plain that it wou'd be* "*greater upon a flatted Spheroid, than upon a Sphere,* "*or than upon an oblong Spheroid of the ſame Soli-* "*dity, in the Ratio of the great Axis of the gene-* "*rating Ellipſe of the flatted Spheroid, to the Di-* "*ameter of the Sphere, or to the ſhorter Axis of the* "*generating Ellipſe of the oblong Spheroid. And in* "*all Likelihood, this muſt be the Reaſon that has* "*made others, who have treated of this Subject, to* "*imagine the very contrary of what I have demon-* "*ſtrated.*

As Monſ. *Mairan* conſiders the Earth at reſt, in the Conſtruction for his Demonſtration above quoted, and afterwards obſerves what Effect the Centrifugal Force will have upon Bodies on its Surface, to diminiſh the Gravity, with which they endeavour to deſcend in their Line of Tendency R P: He ſhou'd not only have taken notice (as he has done) that the whole Centrifugal Force N R is not to be ſubſtracted from the Gravity at R, as the whole Centrifugal Force C D is to be ſubſtracted from the whole Gravity at D, becauſe of the Obliquity of R N to P R; but he ſhould have obſerv'd alſo, that the Obliquity of the Plane of the Parallel N R, in which

the Centrifugal Force acts, muſt alter the Line of Tendency R P, and change the Direction R P into R W, ſomewhere between the Point P and the Center C; for if there be a heavy Body as a Plummet, hanging by a Thread in the Line S R, or S P, the Line of Tendency which has been ſuppos'd perpendicular to the Curve A R D, without taking in the Effect of the Centrifugal Force; as ſoon as the Spheroid revolves about its Axis, the Body which wou'd fall in the Line S R, acted upon only by one Force, namely, that of Gravity, will now be acted upon by another Force, at the ſame Time puſhing it in the Line S *s* (which is the ſame as R *r*) and conſequently will move in the Line S *r*, diagonal of the Parallelogram *s* S R *r*; or, which is all one, a Body plac'd at R will have its Line of Tendency in R W, as I have already ſhewn in my firſt Diſſertation on this Subject; only I did not ſuppoſe the Earth a Spheroid before the diurnal Motion, and therefore made uſe of the Line Z V inſtead of the Line N R; ſo that it may be objected that the Angle *r* S R will not be ſo great in a Spheroid as in a Sphere, becauſe the Centrifugal Force which acts with the ſame Obliquity (ſince N R P = Z V C) is as much leſs in the Spheroid as N R is leſs than Z V: But I was aware of that, and therefore made the Angle R S *r* only of 5 Minutes, when it really appears to be of almoſt ſix Minutes, when the Earth is ſuppos'd ſpherical; and therefore, without coming to give the exact Quantity of the ſaid Angle, one may eaſily perceive, that Monſ. *Caſſini*'s Difference of the Axis and Æquatorial Diameter will produce a Figure, in which the Angle R S *r*, will not be leſs than of 5 Minutes.

Such an Obliquity, caus'd in the Direction of Gravity, will render the oblong ſpheroidical Figure of the Earth impoſſible, becauſe then Fluids wou'd not have the Lines of their Gravity perpendicular to the Horizons of the

Places where they are, (ſuppoſing the Horizons of Places to be Planes touching the Curve of the Earth in thoſe Places) and Plumb Lines wou'd be ſo far out of the Perpendicular to Lines of Level, as to make an Angle eaſy to be obſerved, as I have ſhewn in my former Paper.

But if the ſame Cauſe be ſuppos'd to act upon the Sea to make it level, as makes heavy Bodies to fall (which certainly muſt) then indeed Lines of Level will be perpendicular to Plumb Lines, and the Level of the Sea, taken always for the Horizon of a Place, will not be a Plane touching the Earth, but cutting it towards the Poles, and conſequently the Water will be carried towards the Æquator, as was before ſhewn.

Beſides, the Difference of the Action of the Centrifugal Force wou'd not be ſo great between correſpondent Points of the ſame Latitude in the Spheroid and in the Sphere; for when the Line of Tendency R P is by the Centrifugal Force chang'd into R W, the Point R upon the Spheroid does no longer correſpond in Latitude with the Point V upon the Sphere, but muſt be taken nearer to V, ſo that the Line R W may become parallel to V C, and R W A = V C A.

If it be alledged here, that Monſ. *Mairan* ſuppoſes the Earth in Motion, and takes in the Effect of the Centrifugal Force, when he makes the Line of Tendency to be R P; I anſwer, that if he had conſider'd the Earth as revolving upon its Axis, he wou'd not have made V C the Line of Tendency of a ſpherical Earth in Motion, ſince it is the Line of Tendency of ſuch an Earth at reſt.

In Monſ. *Mairan*'s Obſervation above-mentioned, he ſays, " that we are not to compare the Centrifugal Force at the Æquator of an oblong Spheroid, with the Centrifugal Force at the Æquator of a Sphere, or at the

Æquator

" Æquator of a flatted Spheroid of the ſame Solidity ;
" allowing that then it wou'd be greater in the Sphere,
" and ſtill greater in the flatted Spheroid : but only the
" Centrifugal Forces in ſeveral Latitudes upon the ſame
" Figure." —But I beg Leave to differ from him for the following Reaſons.

Firſt, Becauſe the Force of Gravity is not the ſame at the Æquator of the flatted Spheroid, as it is at the Æquator of the Sphere, or as it is at the Æquator of the oblong Spheroid.

Secondly, Becauſe it is not the ſame in different Latitudes, in either of the Spheroids. (See Sir *Iſaac Newton* Lib. 3. Prop. 19 and 20.) And Monſ. *Mairan*'s Way of arguing will only ſerve, in Caſe the Gravity ſhou'd be the ſame in all the Points of the Surface of the Earth in his Figure, and alſo in the two other Figures.

For Example, let the uniform Gravity be call'd g; and *Firſt*, let the Centrifugal Force at the Æquator of the flatted Spheroid be call'd $c + 2$; and the Centrifugal Force in any Latitude, as for Example, the Latitude of *Paris* (as it is diminiſhed on Account of a ſhorter Co-ſine of Latitude, and likewiſe on Account of its Obliquity to the Line of Tendency,) be call'd $c + 2 - l$; the Difference of the Diminution of Gravity at *Paris*, and at the Æquator will be $\overline{g - \overline{c + 2}} - \overline{g - \overline{c + 2 - l}} = l$.

Secondly, Let the Centrifugal Force at the Æquator of the Sphere be call'd $c + 1$, and the Centrifugal Force at the Latitude of *Paris* be call'd $c + 1 - \overline{l + m}$; the Difference of the Diminution of Gravity at

Paris and at the Æquator in a ſpherical Earth, will be $\overline{g - \overline{c + 1}} - \overline{g - \overline{c + 1 - \overline{l + m}}} = l + m$.

Thirdly, Let the Centrifugal Force at the Æquator of the oblong Spheroid be call'd c, and the Centrifugal Force at *Paris* be call'd $c - \overline{l + m + n}$; the Difference of the Diminution of Gravity at *Paris*, and at the Æquator, in an oblong ſpheroidical Earth, will be $g - c - \overline{g - \overline{c - \overline{l + m + n}}} = l + m + n$.

Now, if Gravity ſhou'd in every Caſe be equal to g, it is evident, that the ſhortening of Pendulums, at the Æquator, wou'd be greater in the oblong Spheroid, than in the Sphere, or in the flatted Spheroid; becauſe as the Lengths of Pendulums diminiſh with the Gravity, thoſe Lengths will be at *Paris* and at the Æquator, when compar'd, as $g - \overline{c + 2 - l}$ to $g - \overline{c + 2}$ in the flatted Spheroid; as $g - \overline{c + 1 - \overline{l + m}}$ to $g - \overline{c + 1}$ in the Sphere, and as $g - \overline{c - l + m + n}$ to $g - c$ in the oblong Spheroid; and conſequently, from what M. *Mairan* has demonſtrated this Ratio of $g - \overline{c - \overline{l + m + n}}$ to $g - c$, being greater than either of the others, the Pendulums muſt be ſhortened in the oblong Spheroid.

But as the Force of Gravity is leſs at the Æquator of the flatted Spheroid, than at the Æquator of the Sphere, or of the oblong Spheroid of the ſame Solidity: let us expreſs its Quantity in the three Caſes by $g - s$, g, and $g + s$, and we ſhall then find the Lengths of the Pendulums, at the Æquator of the three Solids, as $g - s - \overline{c + 2}$, $g - \overline{c + 1}$, and $g + s - c$; conſequently the Lengths of

Pendulums

Pendulums will be greateſt at the Æquator of the oblong Spheroid, becauſe $g + s - c$ is the greateſt Quantity.

Laſtly, To compare the Lengths of Pendulums at the Æquator of the oblong Spheroid, thus found; with their Lengths at the Latitude of *Paris* upon the ſaid Spheroid — Let us expreſs the Exceſs of Gravity at the Æquator, whereby it is greater than at *Paris* (becauſe in this Figure, *Paris* is farther from the Center of the Earth, than the Æquator, by $\frac{1}{230}$ Part) by the Letter s, and the Exceſs of the Centrifugal Force at the Æquator, above that Part of it which acts directly againſt Gravity at *Paris*, by $l + m + n$, the Gravity at *Paris* by g, and the Centrifugal Force at the Æquator by c; then $g + s - c$ will ſtill repreſent the diminiſh'd Gravity, and anſwer to the Length of Pendulums at the Æquator, whilſt $g - \overbrace{c - l + m + n}$ or $g - c + l + m + n$ repreſents the diminiſh'd Gravity, and conſequently the Length of Pendulums at *Paris*. If s be equal to $l + m + n$, Pendulums will be as long at the Æquator as at *Paris*; and if s be greater than $l + m + n$, Pendulums will be longer at the Æquator. But making all poſſible Allowance, in Favour of Monſ. *Mairan*'s Hypotheſis, no Calculation will bring $l + m + n$ to be greater than, or ever equal to s. Therefore Monſ. *Mairan*'s Demonſtrations, above-mentioned, are of no Force to prove the Earth to be an oblong Spheroid.

And now, I think, I have anſwer'd all that relates to the Figure of the Earth in Monſ. Mairan's *Diſſertation; in ſhewing, That his Conjectures can neither be ſupported by thoſe Phyſical Principles which Sir* Iſaac Newton *has Mathematically deduc'd from unqueſtioned*

queſtioned Obſervations and Experiments accurately made; nor even by thoſe Principles which He (*M.* Mairan) *has aſſum'd to ſerve his intended Purpoſe — That his Demonſtrations relating to the Difference of the Action of the Centrifugal Force, are of no Service to him, for reconciling the Experiments made on Pendulums, with Monſ.* Caſſini's *Meaſures; — becauſe, when applied to Sir* Iſaac Newton's *Principles, they will make Pendulums longer at the Æquator than at* Paris, *and when applied to Monſ.* Mairan's *own Principles, they will make them a whole Inch ſhorter at the Æquator than at* Paris, *contrary to all Obſervations, which, at a Medium, make Pendulums but about two Lines or* $\frac{166}{1000}$ *of an Inch longer at the Æquator than at* Paris. — *That he has built his Demonſtrations upon a wrong Notion of Gravity — And that he has not conſider'd what is moſt material in the Effect of the Centrifugal Force, acting on Bodies deſcending by their Gravity, between the Æquator and the Poles, namely, the Alteration of their Line of Direction, which wou'd make them fall out of the Perpendicular towards the Æquator.*

I ſhall add one more Philoſophical Argument, given me by a Friend, to whom I communicated my Thoughts on this Subject; becauſe it is wholly independent on thoſe Principles of Philoſophy, concerning which, ſome of the Gentlemen that believe the oblong ſpheroidical Figure of the Earth, and the *Engliſh* Philoſophers, are not yet agreed; and it is this.

If the Earth was of an oblong ſpheroidical Figure, higher at the Poles than the Æquator; the Axis of its Revolution, wou'd either go thro' one of its ſhort Diameters, or be continually changing, unleſs the ſaid Axis did exactly coincide with the Axis of the Figure.

DEMONSTRATION.

Suppoſe ſuch an oblong Figure as A *a* * fix'd to the Axis P *p* at the Center C, but capable of moving freely round it towards P or towards *p*, yet ſo as to be oblig'd to move with the Axis, when it is turned round. Suppoſe now the Poles P and *p* to be fix'd, and the Body, thus conſtituted, to be turn'd ſwiftly round the Axis P *p*; then if the Angle A C P be oblique, and the Figure A D *a* E be oblong, the Parts A C and C *a* will acquire a Centrifugal Force, which will enlarge the Angle *p* C A, till it comes to be a right one. Beſides this, a Velocity will be generated in the Motion, while A is going towards the Perpendicular α C, which will make it go farther on towards P, as to B, with a Motion which will after that be retarded, till the Centrifugal Force has Strength enough to ſend it back again the contrary Way; and ſo it will move continually with a reciprocal Motion, like the Oſcillation of a Pendulum; and if a little of this Motion be loſt at every Oſcillation, then the oblong Figure A D *a* E will at laſt move quietly about its leſſer Axis D E coinciding with P *p*.

If A a *did not at firſt exactly coincide with* P p, *the Centrifugal Force will have the above-mentioned Effect; and that this is not the Caſe in the Earth is more than probable, becauſe the unequal Diſtribution of Sea and Land, beſides the* Phænomena *of the Tides muſt make the Axis of its Gravity, and conſequently the Axis of its Revolution, to differ from the Axis of the oblong Spheroid, if the Earth had ſuch a Figure; without conſidering that every Earth-quake wou'd alter ſo nice an* Æquilibrium, *which once loſt, wou'd never be recover'd again.*

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* Fig. V.

To

To leave nothing unexamin'd, relating to the Controverſy, I have again conſider'd the Meaſures and Obſervations, mentioned in the Account of the Meridian drawn thro' *France*, in the Memoirs of the *Royal Academy*, for the Year 1720; and I find them to want a great deal of the Accuracy requir'd in ſo nice a Point, as determining the different Lengths of Degrees upon the Surface of the Earth. To prove my Aſſertion, I beg that the Reader will examine the following Tables, whereby it appears, that if any thing certain can be deduc'd from the ſaid Obſervations and Meaſures, (either taken as they are, or reduc'd to the Level of the Sea, by the Rules given by Monſ. *Caſſini* *) it will be in Favour of Sir *Iſaac Newton*'s Figure of the Earth, rather than theirs.

In the following Table, the firſt Column gives the Names of Places; the ſecond, the Diſtances from *Paris*, according to the Meaſures taken by the *French* Gentlemen; the third, the Latitudes obſerv'd by the ſame; the fourth, the Latitudes, ſuch as the meaſur'd Diſtances will give them, ſuppoſing the Earth ſpherical; the fifth, the Differences between theſe and the Latitudes obſerv'd, expreſs'd in Seconds of a Degree, where when the Latitude computed, exceeds the Latitude obſerv'd, the Letter N (North) ſhews that Difference to be in Favour of Monſ. *Caſſini*'s Figure, and the contrary Difference mark'd by the Letter S (South) is in Favour of Sir *Iſaac Newton*'s Figure.

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* *Memoirs for the Year* 1720. Vol. I. P. 1. Ch. 13.

Names of Places.	Distances from *Paris* measur'd.	Latitudes obſerv'd.			Latitudes in a ſpherical Earth computed from the meaſur'd Diſtances.			Differences in Seconds.	
I.	II. Toiſes.	III.			IV.			V.	
Dunkirk.	125552	51°	2′	25″½	51°	2′	25″½	0″	
Amiens.	60370	49	53	48	49	53	48	0″	
Sourdon.	49970½	49	42	42	49	42	52,1	10,1	N
Paris.		48	50	10	48	50	20,3	10,3	N
Malvoiſine.	18838	48	30	47	48	30	32,1	14,9	S
Vouſon.	67962	47	39	17	47	38	53,6	23,4	S
Bourges.	100192	47	4	31	47	04	58,7	27,7	N
S. Sauvier.	139934	46	23	24	46	23	12	12,0	S
Croc.	169540	45	51	43	45	52	4,6	21,6	N
Bort.	196484	45	23	27	45	23	45,	18,2	N
Aurillac.	223606	44	55	13	44	55	14,5	1,5	N
Rodès.	256575	44	20	54	44	20	35,	19,9	S
Alby.	280612	43	55	32	43	55	19	13,0	S
Carcaſſone.	321430	43	12	55	43	12	24,	31,5	S
Collioure.	360604	42	31	13⅝	42	31	13,0	0.	

In this Table it is to be obſerv'd that there is an equal Number of Differences mark'd N (North) and S (South) and if the Differences on each Side be added together, there will be 89″,4 on the North Side, and 114″7 on the South: This laſt agrees beſt with Sir *Iſaac Newton*'s Figure, which muſt be ſuppoſed for the Correction of ſo great a Difference.

In the next Table, the firſt Column gives the Names of Places; the ſecond, the Latitudes obſerv'd; the third, the Diſtances in the Meridian from *Paris*, reduc'd to the Level of the Sea; the fourth, the Differences of the ſecond Column expreſs'd in Seconds of a Degree; the fifth, the Differences of the Numbers in the third Column; and the ſixth, the Meaſure of a Degree by the fourth and fifth Columns compar'd.

I. Names of Places.	II.			III. Toiſes.	IV. Seconds of a Degree.	V. Toiſes.	VI. Toiſes.
Dunkirk.	51°	2′	19″	125454			
Amiens.	49	53	56	60444	4103″	65010	57040
Clermont.	49	22	57	31028	1859	29416	56965
The R. Obſervatory.	48	50	10	0	1967	31028	56787
Vouſon.	47	39	17	67959	4253	67959	57525
& S. Sauvier.	46	23	24	139937	4553	71978	56912
Croc.	45	51	43	169539	1901	29602	56058
Bort.	45	23	46	196480	1677	26941	57834
Aurillac.	44	55	13	223616	1713	27136	57028
Rodés.	44	20	53	256474	2060	32858	57422
Alby.	43	55	32	280612	1521	24138	57131
Carcaſſone.	43	12	55	321430	2557	40818	57468
Collioure.	42	31	13	360614	2502	39184	56380

In this Table in the third Column, over-againſt *St. Sauvier*, the Number which was 139944 is corrected to make it 139937, to the Advantage of the oblong Figure. In the ſixth Column, the Numbers appear ſo irregular, as to be unfit to decide this Controverſy. Then if a Compariſon be made between *Dunkirk*, *St. Sauvier* (which is very near the Middle of *France*, and almoſt in the Meridian of *Paris*) and *Collioure*, the Meaſurement is abſolutely in Favour of Sir *Iſaac Newton*'s Theory; the mean Degree between *Dunkirk* and *St. Sauvier* being larger by about 64 Toiſes, than between *S. Sauvier* and *Collioure*; and to reduce them even to an Equality, there muſt be a greater Alteration made in the Situation of thoſe three Places, than it is reaſonable to ſuppoſe their Obſervations to be capable of admitting. Here follows the Compariſon.

Dunkirk and *Collioure*	A mean Degree is	57061
Dunkirk and *Paris*		56960
Paris and *Collioure*		57097
Dunkirk and *S. Sauvier*		57090,4
S. Sauvier and *Collioure*		57026,5
According to Monſ. *Picard*,		57060

To conclude, I will propoſe a Method of obſerving the Figure of the Shadow of the Earth in Lunar Eclipſes, whereby the Difference between the Diameters in the oblong ſpheroidical Figure, if there be ſuch an one as Monſ. *Caſſini* affirms (*viz.* of 96 to 95) may be diſcover'd.

Let P Æ P Æ * repreſent the Earth, ſeen from the Sun at the Time of the Summer Solſtice; it is evident, that the ſame Figure will expreſs the Section of the Earth's Shadow at the Moon's Diſtance, as ſeen from the Earth. If E E repreſents the Ecliptick, Æ Æ will be the ſhorteſt Diameter of the Section; and if L L be taken for the Moon's Way, in a total and central Eclipſe of the Moon, by obſerving the Time which is ſpent in the Paſſage of the Center of the Moon, thro' the Shadow, and reducing that Time to Seconds of a Degree of a great Circle of the Heavens, we ſhall have the leaſt Diameter of the Shadow.

Again, let the ſame Letters † repreſent the ſame Things, only here the Section of the Shadow is ſuch, as the Earth will caſt at the Æquinox, and the Eclipſe of the Moon is here ſuppoſed partial, its Center juſt touching the Shadow. When the Moon's Center is got to *c*, if the Latitude of its Center or its Diſtance from the Ecliptick be obſerv'd, we ſhall have the Length *c* C nearly equal to the longeſt Semi-diameter of the Shadow.

Now, comparing *c* C * in this Figure to L C * in the former (the Difference between *c* C and C P (*Fig.* 7.) and between C L and C Æ (*Fig.* 6.) not being worth notice) they ought to be to one another, as 96 to 95, which in ſuch a Shadow will give a Difference of about 25″ at a Medium, ſenſible enough to be obſerved, notwithſtanding the *Penumbra*. If therefore thoſe A-

* Fig. VI. † Fig. VII. * Fig. VII. * Fig. VI.

ſtronomers who have Inſtruments nice enough, and ſufficient Skill in the Management of them, to take Angles to 3 or 4 Seconds of a Degree, will obſerve what I have been mentioning in total and partial Eclipſes of the Moon; by ſuch Obſervations they will eaſily convince us, that the Figure of the Earth is ſuch as Monſ. *Caſſini* ſuppoſes it, or convince him that he has been miſtaken.

The Semi-diameter of the Earth's Shadow, when the Earth is in Perihelio, *and the Moon in* Apogæo *is* 38′, *or* 2280″, *without conſidering the Encreaſe of the Shadow, on account of the Atmoſphere of the Earth, which wou'd make it* 39′ or 2340″ (*allowing one Second for a Mile*;) *and the Semi-diameter of the Shadow, when the Earth is in* Aphelio, *and the Moon in* Perigæo *is* 46′, 20″, *or* 2780″, *which encreaſed on account of the Atmoſphere of the Earth, will bring it to* 47′, 20″ *or* 2840″. *Now if the Proportion of* 95 *to* 96 *be taken in both Caſes, you will have theſe Analogies,* $\left\{\begin{array}{l} 95 : 96 :: 2340'' : 2364''.6 \\ 95 : 96 :: 2840'' : 2869'',8 \end{array}\right\}$ *So that* 2364″,6 — 2340″ = 24″,6 *will be the Difference of the Semi-diameters, when the Section of the Shadow is the leaſt, and* 2869″,8 — 2840″ = 29″,8 *will be the Differenee of Semi-diameters, when the Section of the Shadow is the greateſt; the Sum of thoſe Differences* 24″,6 + 29″,8 *halved, will give the Difference, when the Section of the Shadow is at a Medium* = 27″,4; *from which if we take* 2″,4 *becauſe in* Fig. 7. C c *is a little leſs than* CP, *and in* Fig. 6. L C *is ſomething greater than* Æ C, *we ſhall have* C *c in* Fig. 7. *to compare with* L C *in* Fig. 6. *which will exceed it by* 25″, *if Monſ.* Caſſini's *Figure of the Earth be the true one.*

II. *A Catalogue of the* 50 *Plants, from* Chelsea *Garden, presented to the* Royal Society, *for the Year* 1724. *by the Company of Apothecaries, pursuant to the Direction of Sir* Hans Sloane *Bart. Pr. Coll. Med. S. P. V. Pr. By Mr.* Isaac Rand, *Apothecary, F. R. S.*

101. GEranium lucidum, saxat ile. C.B. 318.
102. Geranium folio Malvæ rotundo. C. B. Ib.
Majus, foliis in breviores & latas lacinias divisis.
103. — Columbinum, vulgare, petalis florum bifidis, purpureis.
— Secundum *Camer. Epit.* 600.
104. — — Petalis florum bifidis albis.
An Ger. Malacoides, flore albo. *H. Reg. Blæs.*
105. — — Humile, flore cæruleo, minimo. *Raii Syn. Ed.* 3. p. 359.
— — Tenuius laciniatum. C. B. 318. *Prod.* 138.
106. — — Majus, flore minore, cæruleo. *Raii H.* 1058.
107. — — Maximum, dissectis foliis. *Plot. Hist. Ox.*
108. — Orientale, columbinum, flore maximo, Asphodeli radice. *T. Cor.* 20.
109. — Sanguineum, maximo flore. C. B. 318.
110. — — Cauliculis erectis, flore minore. *H. L. Bat.* 286.
111. — Hæmatodes, foliis majoribus, pallidioribus & altiùs incisis, D. *Dale. Raii Syn.* 219.

112. Geranium hæmatodes Lancaſtrenſe, flore eleganter ſtriato. *Raii Hiſt.* 1061.
113. —— Batrachoides. Gratia Dei, Germanorum. C. B. 318.
114. —— —— Montanum noſtras. *Raii Hiſt.* 1062.
115. —— —— Odoratum. C. B. 318.
116. —— Annuum minus Batrachoides, Bohemicum, purpureo-violaceum. *H. Ox.* p. 2. 511.
117. —— Phæum, flore reflexo I. B T. 3. 477.
118. —— Magnum, folio trifido I. B. T. 3. 478.
119. —— Romanum verſicolor, ſive ſtriatum. *Park. Parad.* 229.
120. —— Robertianum primum. C B. 319.
121. —— —— Rubens, flore albo.
122. —— Lucidum, ſaxatile, foliis Geranii Robertiani. *D. Sherard. Raii Syn.* 218.
123. —— Folio Althææ. C. B. 318.
124. —— —— —— Floribus ſaturatiùs rubentibus. *H. L. Bat.* 290.
125. —— Chium Vernum, Caryophyllatæ folio. *T. Cor.* 20.
126. —— Folio Alceæ, tenuiter laciniato; flore pentapetalo; purpuraſcente, ſemine tenui. *Boerh. Ind. alt.* 266.
127. —— Cicutæ folio, moſchatum. C. B. 319.
128. —— —— ——, Minus & ſupinum C. B. 319.
129. —— Arvenſe, album. *Tabern. Ic.* 57.
130. —— Myrrhinum, tenuifolium, amplo flore purpureo. *Barrel.* ob. 55.
131. —— Latifolium, longiſſimâ acu. C. B. 319.
132. —— Cicutæ folio, acu longiſſimâ. C. B. ib.
133. —— Acu longiſſimâ, Cicutæ folio tenuiùs laciniato.
—— Cicutæ folio, viſcoſum, acu longiſſima. D. *Juſ. ſieu.*

134. — Triſte *Cornuti.* 109.
135. — Æthiopicum, noctu olens, radice tuberoſa, foliis Myrrhidis, anguſtioribus. *Breyn. Cent.*
136. — Africanum, Coriandri folio, floribus incarnatis, minus. *H. L. Bat.* 279.
137. — Africanum fruteſcens, Chelidonii folio, petalis florum anguſtis, albidis; carnoſo caudice.
138. — — Noctu olens, tuberoſum & nodoſum, Aquilegii foliis. *H. L. Bat.* 284.
139. — — Betonicæ folio, procumbens, floribus parvis, eleganter variegatis. *Pluk. Alm.* 169.
140. — — Foliis inferioribus Aſari, ſuperioribus Staphydis agriæ, maculatis, ſplendentibus, & Acetoſæ ſapore. *Com. Præl.* 52.
141. — Africanum fruteſcens, folio craſſo & glauco, Acetoſæ ſapore. *Com. Præl.* 54.
142. — — Alchimillæ hirſuto folio, floribus albidis. *H. L. Bat.* 282.
143. — — Folio Malvæ craſſo, molli, odoratiſſimo, floſculo pentapetalo, albo. *Boerh. Ind. alt.* 263.
144. — — Fruteſcens, Malvæ folio laciniato, odorato. *H. L. Bat.* 277.
145. — — — — — — Inſtar Meliſſæ, flore purpuraſcente. *Boerh. Ind.* 110.
146. — — Arboreſcens, Malvæ folio mucronato, petalis florum inferioribus vix conſpicuis.
147. — — — Alchimillæ hirſuto folio, floribus rubicundis. Com. præl. 51.
148. — — — Folio Malvæ plano, lucido, glabro; flore Kermeſino, Di van Leur. *Boerh. Ind.* 109.
149. — — — — Ibiſci folio rotundo; Carlinæ odore. *H. L. Bat.* 274.
150. — — — —, — — — Anguloſo; floribus amplis purpureis.
— — Fruteſcens, Alchimillæ folio, hirſuto, cum fimbriis purpureis. *Boerh. Ind.* 110.

III. *Obſer-*

III. *Obſervations concerning the Height of the Barometer, at different Elevations above the Surface of the Earth, in a Letter to the Publiſher from the Learned Dr.* Nettleton.

SIR,

BEING curious to learn by Obſervation, how far the Mercury will deſcend in the Tube at any given Elevation, for which there is ſufficient Opportunity hereabouts, I propoſed to take the Altitude of ſome of our higheſt Hills; but, when we attempted it, we found our Obſervations ſo diſturbed by Refractions, that we cou'd come to no Certainty. Having meaſur'd one Hill of a conſiderable Height, in a clear Day, and obſerved the Mercury at the Bottom and at the Top, we found, according to that Eſtimation, that about 90 Feet, or upwards, were required to make the Mercury fall one Tenth of an Inch; but coming afterwards to repeat the Experiment on a cloudy Day, when the Air was ſomewhat groſs and hazy, we found the ſmall Angles ſo much augmented by Refraction, as to make the Hill much higher than before, tho' they were taken carefully with very good Inſtruments, both at that Time and before. I afterwards frequently obſerv'd at home, by pointing the Quadrant to the Tops of ſome of our neighbouring Mountains, that they wou'd appear higher in the Morning before Sun-riſe, and alſo late in the Evening, than at Noon, in a clear Day, by ſeveral Minutes: Particularly, one Morning in *December* laſt, when the Vapours lay condens'd in the Vallies, and the Air above was very pure, the Top of a Mountain, at ſome Diſtance from

from hence, appear'd more elevated, by above 30 Minutes, than it had done in the Beginning of *September* about Noon, on a very clear Day. From whence it appears, that the Refraction is at ſome times greater than at others; but probably 'tis always very conſiderable, and, as there is no certain Rule to make Allowance for it, it ſeems likely, that all Obſervations made on very high Hills, eſpecially when view'd at a Diſtance, and under ſmall Angles, as they commonly are, are uncertain, and ſcarce to be depended on, generally erring in making the Heights greater than they really are.

I then proceeded to obſerve, as near as I was able, the Alteration of the Mercury in ſome ſmaller perpendicular Elevations, which we cou'd meaſure with a Line, and alſo on the Tops of ſome Hills of a moderate Height, whoſe Altitude we cou'd obſerve moſt commodiouſly, and, by taking the Angles large, avoid the Danger of any conſiderable Refraction.

At the Bottom of the Tower of *Halifax* Church, the Mercury ſtood at 29 *Inch.* 78 *Dec.* At the Top it ſubſided to 29. 66. The Height of the Place, where the Obſervation was made, was found to be 102 Feet.

At the Bottom of a Coal-Mine, near this Place, the Mercury ſtood at 29. 48. At the Top, it fell to 29. 32. The Depth of the Mine, being meaſured, was found to be 140 Feet.

At the Bottom of another Mine, the Mercury was obſerved to ſtand at 29. 50. At the Top, it fell to 29. 23. The Depth of this Mine was 236 Feet.

At the Foot of a ſmall Hill, whoſe Height we cou'd meaſure very exactly, the Mercury ſtood at 29. 81. At the Top it fell to 29. 45. The Height of the Hill was 312 Feet.

At the Bottom of *Halifax* Hill, commonly call'd the Bank, the Mercury was obſerved to ſtand at 30. 00.

At the Top, it fell to 29. 41. The Height of this Hill was found to be 507 Feet.

Our Mathematicians do demonstrate, that the Density of the Air decreases in a Geometrical Progression, as the Elevation encreases in an Arithmetical one, and consequently, that the Logarithms of the Densities are as the Elevations reciprocally. But the Weight of the Air being as its Density, and the Height of the Mercury in the Barometer being always proportional to the Air's Weight, it follows, that the Logarithms of the Heights of the Mercury are, reciprocally, as the Elevations: Whence having found by Observation, what Elevation is requir'd to make the Mercury stand at any given Height, it will be easy to determine, how much is requisite to reduce it to any other Height propos'd. If we make 30 Inches the Standard Height of the Mercury, equal to Unity, and suppose an Elevation of 85 Feet be requir'd to make it fall one Tenth of an Inch from that Height, as by these Observations it is very nearly; then as the Logarithm of $\frac{30,0}{29,9}$ is to 85, so is the Log. $\frac{30,0}{29,5}$ to the Number of Feet requir'd to make it fall Half an Inch, and so of the rest. When the Mercury stands above 30 Inches, the Numbers will be negative, and shew the Spaces descending; by which Method I computed the following Tables.

The latter, which contains the Differences of the Numbers in the former, was of very great Use to me, when, in these Experiments, the Mercury stood at any other Height in the Tube, besides 30 Inches, and fell any Number of Tenths, or Parts of a Tenth, by adding the Numbers answering thereto, or proportionable Parts of them, to find the Elevation requir'd in the Table, to make the Mercury fall so much, and thereby readily

to

to compare the Heights found by Obſervation therewith. And though ſome ſmall Errors, in the Obſervations, do make them vary a little from each other, yet in the main, they agree as near as poſſible with the Numbers of the Table; as did alſo ſeveral other Experiments too long to mention, which makes me believe thoſe Numbers are not far from the Truth; but of that you will be beſt able to judge, by comparing theſe Experiments with others of the ſame Kind.

That the Air is colder, as well as more light and rare, in Places that are ſituated high, than it is in the Vallies and low Grounds, is generally known; and in order to learn, how much it might be ſo, I got a Friend of mine, who lives higher than we do here, to obſerve the portable Barometer and Thermometer, at his Houſe, for ſome Days, being plac'd as near as poſſible in the ſame Circumſtances with mine; and we found his Barometer ſtood at a Medium for 20 Days, 3 Tenths lower than mine, and the Thermometer 3. *deg.* 2. lower; allowing for the Difference of the Inſtruments, which had been obſerved before.

At another Place the Barometer, at a Medium for 14 Days, ſtood lower by 4. 46. and the Thermometer was lower by 4. *deg.* 4. At another Place, which was very high upon the Moors, the Barometer, at a Medium for ten Days, ſtood lower by 0. 65. and the Thermometer fell 7°.

A Table ſhewing the Number of Feet aſcending, requir'd to make the Mercury fall to any given Height in the Tube, from 30 to 26 Inches. As alſo the Number of Feet deſcending, requir'd to make the Mercury riſe, from 30 to 31 Inches.

In.	Dec.	Feet	Dec.	In.	Dec.	Feet	Dec.
31	0	834	79	27	9	1847	55
30	9	752	53	27	8	1938	97
30	8	670	01	27	7	2030	72
30	7	587	21	27	6	2122	80
30	6	504	15	27	5	2215	21
30	5	420	82	27	4	2307	95
30	4	337	21	27	3	2401	02
30	3	253	32	27	2	2494	44
30	2	169	10	27	1	2588	20
30	1	84	72	27	0	2682	33
30	0	00	00	26	9	2776	80
29	9	85	00	26	8	2871	62
29	8	170	29	26	7	2966	79
29	7	255	87	26	6	3062	32
29	6	341	73	26	5	3158	21
29	5	427	89	26	4	3254	46
29	4	514	34	26	3	3351	07
29	3	601	08	26	2	3448	05
29	2	688	11	26	1	3545	41
29	1	775	44	26	0	3643	14
29	0	863	08				
28	9	951	01				
28	8	1039	25				
28	7	1127	80				
28	6	1216	66				
28	5	1305	83				
28	4	1395	32				
28	3	1485	13				
28	2	1575	26				
28	1	1665	70				
28	0	1756	47				

A Table ſhewing the Number of Feet requir'd to make the Mercury fall one Tenth of an Inch from any given Height in the Tube, from 31 to 26 Inches.

In.	Dec.	Feet	Dec.	In.	Dec.	Feet	Dec.
31	0	82	26	27	9	91	42
30	9	82	53	27	8	91	75
30	8	82	79	27	7	92	08
30	7	83	06	27	6	92	41
30	6	83	33	27	5	92	74
30	5	83	61	27	4	93	07
30	4	83	89	27	3	93	41
30	3	84	16	27	2	93	76
30	2	84	44	27	1	94	12
30	1	84	72	27	0	94	47
30	0	85	00	26	9	94	82
29	9	85	29	26	8	95	17
29	8	85	58	26	7	95	53
29	7	85	86	26	6	95	89
29	6	86	16	26	5	96	25
29	5	86	45	26	4	96	61
29	4	86	74	26	3	96	98
29	3	87	03	26	2	97	36
29	2	87	33	26	1	97	73
29	1	87	63	26	0	98	10
29	0	87	93				
28	9	88	24				
28	8	88	55				
28	7	88	86				
28	6	89	17				
28	5	89	49				
28	4	89	81				
28	3	90	13				
28	2	90	45				
28	1	90	76				
28	0	91	09				

IV. Experimentum, in Argenti-Fodina Salana, circa aſcenſum Mercurii in Barometro, inſtitutum. Per *And. Celſium.* Ex Actis Literariis Sueciæ Ann. 1724. Trimeſtr. IV.

AD variationem columnæ mercurialis obſervandam, pro diverſa altitudine regionum atmoſphæræ, fodinæ noſtræ profundiores in primis idoneæ cenſeri debent. Harum enim profunditatem non modo omni exactitudine metiri; ſed & brevi temporis ſpatio obſervationem totam abſolvere licet. Quo ſane commodo haud raro deſtituuntur, quibus circa altiores montes hæc talia experiri animus eſt. Si itaque quamplurima in variis fodinis inſtituerentur experimenta; nulli dubitamus, quin vera progreſſio, quâ denſitates aëris decreſcant, tandem ſua ſponte ſe proderet.

In *magna fodina Cuprimontana* argenti vivi aſcenſum a viro ampliſſimo *Georgio Vallerio* dudum obſervatum eſſe, ex litteris ipſius ad Cl. *De La Hire* d. 15 Julii, A. 1711. Fahlunæ datis, certiores reddimur. *

In *argenti* vero *fodina Salana*, ſeptem fere milliaribus ab Upſalia verſus occidentem diſſita, hujus rei periculum a me factum eſt die 28 Aug. currentis anni. Scilicet juxta limen *putei Reginæ Chriſtinæ*, (*Drottning Chriſtinæ Schatcht*,) hydrargyri altitudinem 30 digit. & 38 centeſ. ſeu $\frac{3038}{1200}$ pedis Suecani obſervavi. Cum Barometro deinde in tonna, quæ funi adpenſa machina hydraulica trahitur, ad profunditatem 636 pedum me demiſi; ubi mercurius ad 30 dig. 98 centeſ. aſcendiſſe de-

* *Vid.* Memoires de l'Acad. R. des Sc. l'ann. 1712. p. 108.

prehenſus eſt. Inde iterum evectus ad orificium putei, in eadem ac antea altitudine, nempe 30 dig. 38. cent. columnam mercurialem notavi. Adeo ut hydrargyrum 636 ped. in aëre elatum 6 lineas ſeu $\frac{6}{110}$ ped. deſcenderit; & ſic conſequenter, ſi aër æque denſus ubique ſupponeretur, unius lineæ in cylindro mercuriali variatio, 106 ped. altitudini perpendiculari correſponderet. Horæ illius intervallo, quo integra perficiebatur obſervatio, cœlum erat pluvium nonnihil & ventoſum; nulla tamen ſenſibilis mutatio, columnæ mercurialis in alio Barometro ſupra fodinam parieti affixo, iſto tempore videri potuit.

Poſtero die, aëre ſereno & tranquillo, ad baſin templi urbis *Salæ*, haud procul a fodina diſtantis, argentum vivum 30 dig. 36 cent. altum hæſit, ad altitudinem vero 145 ped. in turri ejuſdem templi ſcandens, mercurium ad 30 dig. 23 cent. ſubſtitiſſe deprehendi, ut unius lineæ in Barometro deſcenſui, altitudo $111\frac{7}{13}$ ped. reſpondeat. Baſis templi 60 fere pedes infra ſuperficiem fodinæ deprimitur. Ipſius autem fodinæ elevationem ſupra mare Balticum explorare nondum licuit.

Ut hæc noſtra obſervatio cum exterorum hujus generis experimentis rite conferri queat, notandum eſt inter pedem Suecanum & Pariſinum Regium eam rationem intercedere, quæ eſt inter 1000 & 1096, ſeu 125 & 137 proxime; quam ex pede Gallico orichalceo, inſignis artificis *Chapotot* manu inſculpto, cum pede *Stiernhielmiano*, qui in Bibliotheca publica Upſal. ſervatur, collato, exactiſſime obſervavi.

F I N I S.

Fig. 1.

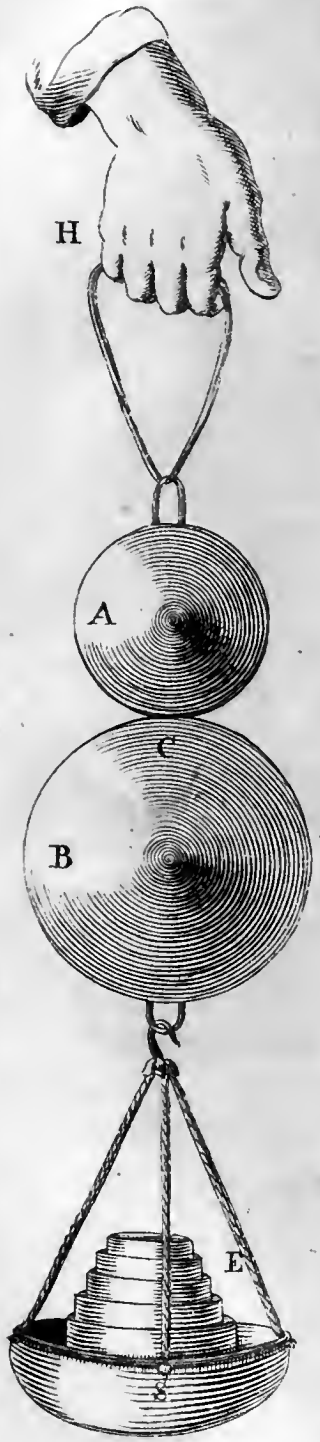
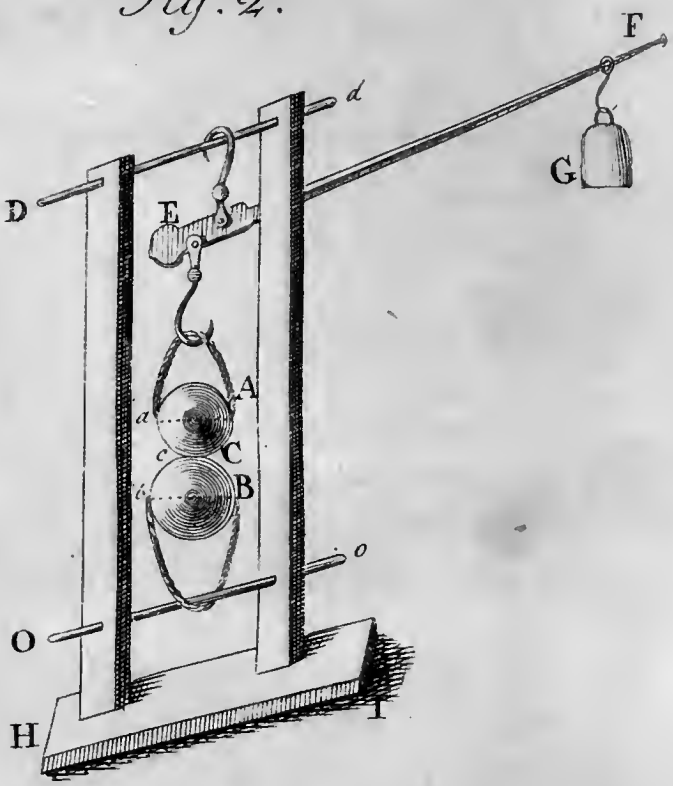


Fig. 2.



S. Parker Sculp

Numb. 389.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *July* and *August*, 1725.

The CONTENTS.

VI. *Some Experiments concerning the* Coheſion *of* Lead, *by the ſame.*

VII. *Extract of ſeveral Letters and Certificates ſent to his Majeſty the King of* Great Britain, *concerning a very particular* Nævus Maternus, *or* Mole, *Communicated by Dr.* Steigertahl, *Phyſician to his Majeſty,* F. R. S.

VIII. *An Account of a Book, entitled,* Hiſtoria Cœleſtis Britannica, tribus Voluminibus contenta, Authore *Joanne Flamſteedio* Aſtronomo Regio.

ERRATA.

No. 387. Page 263. line 20. for *June*, read *February*. p. 268. l. 3. for *Shelves*, r. *Shells*. p. 269. l. 3. for *Tail*, r. *Tackle*. No. 389. p. 318. l. 7. r. *Degrees*. p. 324. l. 30. r. *in ſatis magna*.

I. *Remarks upon the Obſervations made upon a Chronological Index of* Sir Iſaac Newton, *tranſlated into* French *by the Obſervator, and publiſh'd at* Paris.

ON *Thurſday November* 11, 1725. A ſmall Tract in print was deliver'd to me, as a Preſent, from Mr. *William Cavelier*, Jun. a Bookſeller at *Paris*, a Perſon unknown to me, entituled, *Abregé de Chronologie de M. le Chevalier Newton, fait per lui même, & traduit ſur le Manuſcript Anglois.* And the Bookſeller hath premiſed an Advertiſement, in which he endeavoureth to defend himſelf for printing it without my Leave, ſaying, that he had writ three Letters to me for my Leave, and in the Third had told me, that he would take my Silence for a Conſent; and that he had alſo charged one of his Friends in *London* to ſpeak to me, and procure my expreſs Anſwer; and that having long expected my Anſwer, he thought that he might take my Silence for a Sort of Conſent, and ſo procured a Privilege, and printed it, and then received my Anſwer from his Friend, which was as follows.

I *Remember that I wrote a Chronological Index for a particular Friend, on condition that it ſhould not be communicated. As I have not ſeen the Manuſcript which you have under my Name, I know not whether it be the ſame. That which I wrote was not at all done with Deſign to publiſh it. I intend not to meddle*

with that which hath been given you under my Name, nor to give any Consent to the publishing of it,

I am,

Your very humble Servant,

ISAAC NEWTON.

London, May 27, 1725, St. Vet.

The Privilege was granted *May* 21, and registerd *May* 25 Old Style, my Letter was dated *May* 27, and the Chronological Index, or Abridgement as he calls it, was printed before the Arrival of my Letter, and kept ever since to be publish'd at a convenient Time. The Bookseller knew that I had not seen the Translation of the Abridgement, and without seeing it could not in reason give my Consent to the Impression. He knew that the Translator was unknown to me, and was against me; and therefore he knew that it was not fit that I should give my Consent, nor be ask'd to do it. He knew that the Translator had written a Confutation of the Paper translated, and that this Confutation under the Title of *Observations*, was to be printed at the End of it, and he told me nothing of all this, nor so much as the Name of the Observator, and yet asked my Consent to the publishing; as if any Man could be so fooliſh as to consent to the publishing of an unseen Translation of his Papers, made by an unknown Person, with a Confutation annexed, and unanswered at their first Appearance in Publick.

After the Recital of my Letter, he adds, that the Author of the Translation, and of the Observations upon it, pretends to have an entire Certainty that this Index, or Abridgement of Chronology, is the same

ſame with the Writing own'd by me in my Letter, and is perſuaded that the Manuſcript, which hath been communicated to him, hath been copied from that of this Friend, that is, from that of the particular Friend above-mention'd in my Letter. And therefore the Manuſcript, which hath been communicated to him, is that of *Abbè Conti*, a noble *Venetian* now at *Paris*. He, being about ſeven Years ago in *England*, gave me notice, that the Friend above-mention'd deſir'd to ſpeak with me. And this Friend then deſir'd a Copy of what I had written about Chronology. I replied that it was imperfect and confus'd, but in a few Days I could draw up an Abſtract thereof, if it might be kept ſecret. And ſome Time after I had done this, and preſented it, this Friend deſir'd that Signor *Conti* might have a Copy of it. He was the only Perſon who had a Copy, and he knew that it was a Secret, and that it was at the Deſire of this Friend, and by my Leave, that he had a Copy, and he kept it ſecret, while he ſtaid in *England*; and yet, without either this Friend's Leave or mine, he diſpers'd Copies of it in *France*, and got an Antiquary to tranſlate it into *French*, and to confute it; and the Antiquary hath got a Printer to print the Tranſlation and the Confutation; and the Printer hath endeavour'd to get my Leave to print the Tranſlation, without ſending me a Copy thereof to be perus'd, or telling me the Name of the Tranſlator, or letting me know that his Deſign was to print it with a Confutation unanſwer'd and unknown to me.

The Tranſlator near the End of his Obſervations (Page 90) ſaith, I believe that I have ſaid enough concerning the *Epocha* of the *Argonauts*, and the Length of *Generations* to make People cautious about the reſt. For theſe are the two Foundations of all this new

Syſtem of Chronology. What he ſaith, concerning the *Epocha* of the *Argonauts*, is founded on the Suppoſition that I place the Equinox in the Time of the *Argonautic* Expedition, fifteen Degrees from the firſt Star of *Aries*, Pag. 75, 79. I place it in the Middle of the Conſtellation, and the Middle is not fifteen Dergees from the firſt Star of *Aries*. The Obſervator grants that the Conſtellations were formed by *Chiron* (Pap. 70, 71, 79.) and that the Solſtices and Equinoxes were then in the Middle of the Conſtellations (Pag. 65, 69, 75.) and that *Eudoxus*, in his *Enoptron* or *Speculum* cited by *Hipparchus*, followed this Opinion, Pag. 62, 63, 65, 69, 79. And * *Hipparchus* names the Stars, through which the Colures paſſed in this old Sphere, according to *Eudoxus*, and ſaith expreſly that *Eudoxus* drew one of theſe Colures through the Middle of *Cancer* and the Middle of *Capricorn*, and the other through the Middle of *Chelæ* and the Back of *Aries*. And the Colurus, paſſing through the Back of *Aries*, paſſes through the Middle of *Aries*, and is but eight Degrees from the firſt Star of *Aries*. I follow *Eudoxus*, and, by doing ſo, place the Equinoctial Colure about 7*gr*. 36′ from the firſt Star of *Aries*. But the Obſervator repreſents, that I place it fifteen Degrees from the firſt Star of *Aries*, and thence deduces that I ſhould have made the *Argonautic* Expedition 532 Years earlier than I do. Let him rectify his Miſtake, and the *Argonautic* Expedition will be where I place it.

As for the Length of *Generations*, the Obſervator ſaith, that I reckon them one with another at 18 or 20 Years a piece (Pag. 52, 55.) which is another

* *See* Hipparchus *publiſh'd by* Petavius, Vol. 3. Pag. 116, 117, 119, 120.

Miſtake.

Miſtake. I agree with the Antients in reckoning three Generations, at about an hundred Years. But the Reigns of Kings I do not equal to Generations, as the antient *Greeks* and *Egyptians* did; but I reckon them only at about 18 or 20 Years a piece one with another, when ten or twelve Kings, or more, are taken in continual Succeſſion. So the firſt 24 Kings of *France* (*Pharamond*, &c.) reigned 458 Years, which is one with another 19 Years a piece. The next 24 Kings of *France* (*Ludovicus Balbus*, &c.) reigned 451 Years, which is one with another 18¾ Years a piece. The next 15 Kings (*Philippus Valeſius*, &c.) reigned 315 Years, which is one with another 21 Years a piece. And all the 63 Kings of *France* reigned 1224 Years, which is 19¼ Years a piece. And, if the long Reign of *Lewis* XIV. be added, the 64 Kings of *France* will reign but 20 Years a piece. And they, that examine the Matter, will find it ſo in other Kingdoms: And I ſhorten the Duration of the antient Kingdoms of *Greece*, in the ſame Proportion that I ſhorten the Reigns of their Kings, and thereby place the *Argonautic* Expedition about 44 Years, and the Taking of *Troy* about 76 Years after the Death of *Solomon*, and find *Seſoſtris* contemporary to *Seſac*.

So then the Obſervator hath miſtaken my Meaning, in the two main Arguments on which the Whole is founded, and hath undertaken to tranſlate and to confute a Paper which he did not underſtand, and been zealous to print it without my Conſent; tho' he thought it good for nothing, but to get himſelf a little Credit, by tranſlating it to be confuted, and confuting his own Tranſlation.

The Obſervator ſaith, that I ſuppoſe that the *Egyptians* began, about 900 Years before Chriſt, to form their Religion, and deify Men for their inventing of

Arts,

Arts, notwithſtanding that it appears by the Scriptures, that their Idolatry and Arts were as old as the Days of *Moſes* and *Jacob*, Pag. 82, 83. But he is again miſtaken. I deny not that the Kingdom of the lower *Egypt*, called *Mizraim*, had a Religion of their own, till they were invaded and ſubdued by the Shepherds, who were of another Religion: but I ſay, that, when the *Thebans* expelled the Shepherds, they ſet up the Worſhip of their own Kings and Princes. I ſay alſo, that Arts were brought into *Europe* principally by the *Phœnicians* and *Curetes*, in the Time of *Cadmus* and *David*, about 1041 Years before Chriſt; and do not deny that they were in *Phœnicia*, *Egypt*, and *Idumea*, before they came into *Europe*.

The Obſervator ſaith alſo, that, 884 Years before Chriſt, I place the Beg nning of the canicular Cycle of the *Egyptians* upon the vernal Equinox, although that Cycle never begins in Spring, Pag. 84, 85. But he is again miſtaken. I meddle not with that Cycle, but ſpeak of the *Egyptian* Year of 365 Days.

The Obſervator repreſents, that I have a great Work to come out: but I never told him ſo. When I lived at *Cambridge*, I us'd ſometimes to refreſh myſelf with Hiſtory and Chronology for a While, when I was weary with other Studies: but I never told him, that I was preparing a Work of this Kind for the Preſs.

Abbè Conti came into *England* in Spring 1715, and, while he ſtaid in *England*, he pretended to be my Friend, but aſſiſted Mr. *Leibnitz* in engaging me in new Diſputes, and hath ſince acted in the ſame Manner in *France*. The Part he acted here may be underſtood by the Character given of him in the *Acta Eruditorum* for the Year 1721, Pag. 90. where the Editor, excuſing himſelf from repeating ſome Diſ-

putes

putes which had been publiſh'd in thoſe *Acta*, ſubjoins: *Suffecerit itaque annotaſſe Abbatem quendam Italum de Conti nobilem Venetum (de quo admiratione digna ſibi præſcripta eſſe ab Hermanno fatetur Leibnitius) cum ex Gallia in Angliam trajeciſſet, mediatoris vices in ſe ſuſcipere voluiſſe, atque literas Newtoni ad Leibnitium deferri curaſſe, Leibnitianas cum Newtono communicaſſe.* And how Mr. *Leibnitz* by this Mediation, endeavour'd to engage me, againſt my Will, in new Diſputes, about occult Qualities, univerſal Gravity, the Senſorium of God, Space, Time, Vacuum, Atoms, the Perfection of the World, Supramundane Intelligence, and Mathematical Problems, is mention'd in the Preface to the ſecond Edition of the *Commercium Epiſtolicum*. And what he hath been doing in *Italy*, may be underſtood by the Diſputes raiſed there by one of his Friends, who denies many of my Optical Experiments, though they have been all tried in *France* with Succeſs. But I hope that theſe Things, and the perpetual Motion, will be the laſt Efforts of this Kind.

II. De Camphora. Auctore *Dno. Carolo Neuman*, Chym. Prof. Reg. *Berolin.* S. R. S.

CUM jam plures, quorum aliqui ipſas quoque Indias peragrarunt, de Camphora tam copioſe ſcripſerint, ut nihil eorum, quæ ad integram hiſtoriam Camphoræ dilucidandam pertinere videri poterant, intermiſerint, ſiquidem & originem vocis explicarunt, & ſynonyma cuncta collegerunt, arborem, unde petitur Camphora, & loca, in quibus arbores hujuſmodi proveniunt, indagarunt, modum quo Cam-

Camphora ibi colligitur, & in Europa depuratur, denique & uſus, in quos adhiberi poteſt Camphora, docuerunt; ſupervacaneum plane foret, iſta omnia hic repetere, chartaſque meris citatis complere. Cuivis etiam ſuam de Camphora opinionem & ſententiam liberam eſſe volo, ſive eam pro ſucco nativo, ſive pro gummi, pro reſina, ſale volatili, bitumine, ſive pro ſuccini ſpecie, ſive pro quocunque quivis eam reputet. Etenim nihil jam aliud in propoſitis habeo, quam peculiarem quandam obſervationem recenſere &, quid de ea ſentiam, explicare, denique novas quasdam ex tali meo judicio deductas doctrinas communicare, ſperans fore, ut alii veriorem ex inde ſententiam de Camphora, quid ea proprie ſit, dicant, alii vero occaſionem capiant, in rem inſtitutis deſuper experimentis convenientibus inquirere, eamque dilucidare.

Traditum equidem eſt in ſcriptis de Camphora editis, poſſe etiam præter ordinariam, quam copioſam ſatis habere poſſumus, aliam ex radice Cinnamomi, Zedoaria Ceylonica, Roremarino Camphorato, Abrotano aliiſque vegetabilibus aromaticis parari, quod certo reſpectu negari nequit, & hac ipſa obſervatione mea confirmatur; nihilo tamen minus confiteri me oportet ante omnia, me nuſquam locorum, quæ peregrinando adivi, talem ex aliis & jam dictis vegetabilibus paratam, licet diligenter perquiſitam, ne in minima quidem quantitate vidiſſe Camphoram, quæ in ſolida & vere ſicca conſiſtentia cum orientali conveniret; ſed quicquid hujuſmodi mihi monſtratum eſt, Amſtelodami fuit illud oleum, quod in India ex radice Cinnamomi deſtillatum dicitur & Camphoram non minus quam Cinnamomum redolet, quamvis dubius adhuc hæreo, genuinum ne illud deſtillatum fuerit, an oleum Cinnamomi ordinarium copioſa Camphora imprægnatum? Si quidem multa experientia comprobatum

batum habeo, ſolere vulgo & imprimis in loco jam dicto, varias merces, potiſſimum ad rem medicam pertinentes, fuco inducto effingi, et per miſcelam adulterari: ſed quoquo modo ſe res cum iſto oleo habeat, poſito etiam, nihil fuci ſubfuiſſe, & oſtendi poſſe Camphoram, etiam in ſolida conſiſtentia, ex radice, & ex Zedoaria Ceylonica, aliiſque orientalibus vegetabilibus elicitam, nemo tamen, quoad ſcio, ſolidum quid Camphoræ æmulum ex vegetabilibus Europæis unquam produxit; licet de Roremarino multi multa dixerint, & de aliis quoque plantis noſtratibus conjectura præſumſerint; imo licet etiam *Dn. Hofmannus* Profeſſor *Hallenſis* Celebr. & Eques *Angl. Boyle* memoriæ prodiderint, quod ille ſimile quid ex floribus Roſarum & Ligno Aloes * hic vero ex oleo Aniſi impetraverit, † penitus tamen ego ſum perſuaſus, quicquid Camphoræ ſimile ex his tribus productum fuerit, molle quid & unctuoſum, vel ad ſummum butyraceæ conſiſtentiæ, minime vero ſiccum, cryſtallinum, fragile, & Camphoræ duritie æmulum fuiſſe, id quod etiam ex ipſorum deſcriptionibus quivis facile poteſt colligere. Taceo, quod in vulgus notum eſt, oleum Aniſi in frigore butyri inſtar condenſari, minime vero in Camphoram eo ipſo abire. Præter hæc, quæ in ſcriptis commemorata legimus, fuerunt aliqui, & in his certus quidem Pharmacopoeus, cæteroquin rerum ſuarum ſatagens, qui ore tenus mihi affirmarunt, poſſe ſe ex variis plantis indigenis Camphoram parare, cujus modi tamen itidem ne tantillum quidem unquam videre licuit; interim aſſentior illis & lubens credo, potuiſſe hos eadem facilitate, qua ego meam, impetrare Camphoram.

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* *Vid.* Ejus Diſſertat. de Camphora & Obſervat. Phyſico-Chym.
†† *Vid.* Ejus Tr. de Firmitate & Fluiditate.

Placuit igitur ad Inclytam Reg. Societatem referre, acquiſiviſſe me ex noſtrati Thymo vulgari, verum, denſum, cryſtalliformem & in omnibus qualitatibus conſpirantem, nec niſi odore differentem Camphoram, quæ cuncta experimenta huc uſque cognita, & pro juſto examine excogitata poſſibilia, æque ac Orientalis ſuſtinet, atque ita cum hoc in omnibus, ſolo odore excepto, convenit.

Deſtillaveram *Anno* MDCCXIX. ex dicta herba olei copiam bene multam, inſtitutaque hujus ab aqua ſeparatione cum goſſypio, cernebam tam ad orificium vitri, quam in goſſypio oleo imbuto parvos figuratos cryſtallulos hærentes, qui ipſam deſtillationem alias ſatis celerem, magis magiſque remorabantur, adeo, ut neceſſarium eſſet goſſypium, contra morem, aliquoties renovare. Mirabar quid rei novæ & extraordinariæ contigerit, variaſque cogitationes agitabam, parum tamen vel nihil certi ea de re unde evenerit, & quid eſſet, poteram colligere; lagenam igitur oleo plenam, bene munitam, reponebam. Interim ſemel excitatæ & conceptæ de rei novitate cogitationes inquietum me tenebant; quare ſciendi cupiditate ductus, poſt paucos dies, reſumo lagenam olei, viſurus, an denuo cryſtallini quid ad orificium vitri hæreat, & quantulumcunque demum id fuerit, per experimenta tamen parvula probaturus, cujus indolis illud ſit, ſed circa orificium nihil ejuſmodi reperiebam, circa fundum vitri vero, quod me tanto magis attentum, ne dicam attonitum habuit, conſediſſe vidi cryſtallos in ſi ſatis magna quantitate, diverſæ magnitudinis, partim avellanas exæquantes, pleraſque cubiformes, ſaccharo cando ſimiles. Nondum ſuſpicabar Camphoram, ſed potius ſal volatile, quod pro copia herbæ ſucceſſive ſimul tranſcenderit, & in parte aquoſa ſolutum, poſtquam hæc fundum petiiſſet, purumque

oleum

oleum ſupernataret, tandem ita coaluerit. Effundo igitur oleum, ut ſal, quod eſſe putabam, impetrarem, & judicii mei certior fierem. Sed denuo in admirationem rapiebar, quod ne guttulam quidem aquæ in fundo invenirem, ubi conſultius nihil erat quam magma ſalinum a fundo liberare, & ab oleo ſeparatum ſervare. Quo me igitur ex dubio extricarem, vel potius, ut de ſale, quod judicando præſumſeram, certior eſſem, tentabam experimentum quo cuncta ſalia, cujuſcunque indolis & nominis fuerint, probantur. Magnam ſcilicet quantitatem aquæ deſtillatæ infundebam ſuper materiam cryſtallinam, ſat diu ſimul agitabam, hinc per aliquot horas quieti dabam, ſolutionem cryſtallini per omnes horæ quadrantes cupide, ſed fruſtra expectans, neque enim ſolutionem, neque ullam mutationem ejus animadvertebam, præter quod minimæ particulæ in ſuperficiem elevarentur, majores vero quantumvis bene interagitatæ fundum ſemper repeterent, & aqua particulis oleoſis cryſtallinæ materiæ adhærentibus, ſed agitatione liberatis, imprægnata eſſet. Transfundo aquam, & nova adfuſa, denuo tentabam cryſtallorum pro indole ſalium ſolutionem, poſtquam hi ab adhærente oleo, quod forſan ſolutionem ejus in aqua impediret, liberati eſſent, ſed pertinaciter ſolutioni renitebantur, quam neque adhuc dum, ſæpius licet tentatam, admiſerunt. Dehinc, præoccupatione de ſalino concremento plane abjecta, conjecturare cepi, quod plane quid aliud, ſcilicet mixtum oleoſum, vel quaſi reſinoſum volatile ex oleo condenſatum eſſe debeat. Hoc mecum perpendens, ſub examen revoco varia, alias vere exiſtentia, mihique nota ſubjecta, ſi quod eorum cum novo hoc comparari queat, & mox ob ſimilitudinem cogitabam Camphoram. Sine mora igitur nova quædam, per quæ me magis certum de re fore

ſperabam, adornavi, & ecce rei certus eram, ſi quidem omnia quæ modo feci experimenta, non alia quam quæ communis Camphora ſuppeditat, Phænomena exhibuerunt, unde tandem, utpote veritatis convictum, nullo plane dubio relicto, concludere me oportuit, corpus hoc cryſtalliforme aliud nihil quam veram eſſe Camphoram, quæ ab Orientali non niſi in eo differat, quod hæc Thymum, illa vero aliud vegetabile redoleat, quod, quia Camphoram copioſam ſuppeditat, huc uſque nullo alio quam arboris Camphoriferæ nomine indigitatum fuit.

Ex his omnibus ſequentia mihi concepi dogmata, quæ ulteriori cujuſvis indagationi, cum libertate ſentiendi, proponam.

I. Nomen hoc Camphora, vel Caphura, non ſpeciale, ſeu proprium eſt, quod uni ſoli ex India venienti Camphoræ ſemper competat, ſed pro generali, ſeu appellativo potius haberi debet (uti *v. g.* nomen ſalis, olei, aquæ, ſpiritus, &c.) ita ut pro diſtinctione ſpeciei Camphoræ nomen illius vegetabilis addatur, ex quo parata eſt, ſicuti mea Camphora jure meritoque Camphora Thymi dicenda venit.

II. Sub hoc nomine Camphoræ concepta ceſſat omnis controverſia inter autores hactenus agitata de ſpecie, ad quam Camphora pertineat, ſi quidem neque reſinis, neque ſalibus volatilibus, neque oleis & ſuccis accenſenda eſt, multo minus ad gummi vel bitumina referenda, quod cum his omnibus per omnia non conveniat, ſed jam in hac, jam in alia qualitate, jam plus, jam minus præcipui, vel tamen plane diverſi quid habeat.

1. Reſina dici nequit, quia, quod etiam jam Dom. Prof. *Hofmannus* adnotavit.

(a) *Omnes*

(a) *Omnes reſinæ poſt combuſtionem, cineres, vel aliud terrei aliquid relinquunt, Camphora e contra plane nihil, ſed tota deflagrat & in auras avolat.*

(b) *Nullæ reſinæ in vaſe clauſo penitus & in totum elevantur ab igne, ſeu ſublimantur, quod tamen cum Camphora ſuccedit.*

(c) *Reſinæ, ubi deſtillantur, phlegma præbent, & oleum, partim etiam ſpiritum ſic dictum, Camphora vero horum nihil.*

(d) *Nulla plane reſina eſt, quæ ad inſtar Camphoræ in ſpiritu nitri ſolvi queat.*

(e) *Nec ulla datur, quæ æque cito & in tanta copia in ſpiritu vini colliqueſcat, neque*

(f) *Talis, quæ in aqua calida Camphoræ inſtar avolet.*

2. Multo minus Camphora ſal volatile dici poteſt, quia in aqua non ſolvitur, nec cum aqua intime, quin videri poſſit, miſcetur, quam tamen ſolutionem inviſibilem cuncta ſalia admittere debent neceſſario, uti ſupra jam commemoratum eſt.

3. Quamvis autem Camphora oleoſum mixtum, vel ex oleo condenſatum corpus ſit, ideo tamen ſimpliciter oleum vocari nequit, æque ac alia mixta ſolida oleo prægnantia, ſiquidem olei nomen liquidis præcipue, vel ad ſummum tenuiter unguinoſis, minime vero ſolidis & ſiccis ſubjectis tribuitur & convenit; neque ullum oleum in ſicca ſubſtantia, ceu Camphora, ſublimari poteſt.

4. Succi nomen Camphoræ non convenit, quia ſucci proprie dicti mixtionem,

(a) *Vel gummoſam, quæ in aqua facile totaliter liqueſcit,*

(b) *Vel*

(b) *Vel gummi reſinoſam, quæ partim in aquoſo, partim in oleoſo, inflammabili, ſpirituoſo & ſimili ſolvitur,*

(c) *Vel reſinoſam obtinuerunt, qui poſteriores reſinæ propriæ dicuntur, de quibus jam ſub numero* 1. *dictum eſt, quarum proprietatum nulla Camphoræ convenit. Nam ſi omnia ea, quæ ex arboribus aliiſque vegetabilibus partim ſponte proveniunt, vel a natura & ſole, partim etiam per artem parantur, quamvis in tota planta ejuſque ſucco & partibus aliis diſperſa & commixta fuerint, ſuccos vocare quis vellet, quantam non confuſionem ineptam & conceptu impoſſibilem pareret in deſcribendis rerum eſſentiis? Sic enim omnis aquæ, ſucci, gummi, olei, balſami, reſinæ ſimiliumque diverſitas plane periret, verbaque hæc diſtinctiva nullius plane uſus forent.*

5. Camphoram etiam gummi vel bitumen non eſſe, ex prioribus jam ſatis apparet, quicquid enim gummi vocatur, penitus in aqua colliqueſcit, ſicuti bitumina maximam partem in ea ſolvuntur, ambo vero ſublimationi refragantur, quam Camphora tamen admittit.

Cum igitur nihil in orbe terrarum cognitum ſit, quod cum Camphora comparari poſſit, æquum etiam eſt, ut ſuo quoque nomine ſingulari & ſpecifico inſigniatur, per quod, ſi res quædam dicatur Camphora, ſtatim intelligatur, eam neque oleum, neque gummi, ſal volatile, reſinam, ſpiritum, neque bitumen, ſed plane aliam & a dictis his diverſam eſſe materiam, mixtum nempe ex ingredientibus, Camphoram veram conſtituentibus, concinnatum, indolis ſpecialiſſimæ. Unde quæſtio, an ſal, vel gummi, reſina, vel ſuccus ſit, æque inconveniens & ſupervacanea judicabitur, ac ſi quis quærere velit, an ſal ſit oleum, vel an gummi ſit Camphora.

III. Tertium

III. Tertium dogma, quod ex prioribus deduco, hoc eſt, quod omnis Camphora certum ſuum ſpecificum odorem, & quidem oleum illius plantæ, ex qua parata eſt, conſtanter retineat, quodque odor iſte ſine deſtructione totius Camphoræ ſeparari ab ea nequeat.

IV. Quarto conſequitur, quod experimentum noſtrum de Camphora Thymi orientalem Camphoræ fabricationem ſatis ſuperque confirmet, nempe quod deſtillando paretur, & acquiratur, in quo negotio vix alia intercedit diverſitas, quam quod ex ſic dicta arbore Camphorifera ibidem in magna copia & ita longe plus Camphoræ quam olei haberi queat, cum e contrario vegetabilia *Europæorum* plurimum olei, parum vero Camphoræ largiantur.

V. Quintum dogma eſt, quod certo credere poſſimus, alias quoque plantas *Europæas*, copioſo oleo eſſentiali prægnantes, æque ac Thymum, Camphoram daturas eſſe, quamvis nullam adhuc viderim, neque pro ea elaboranda juſtam occaſionem nactus fuerim, vel otio abundarim.

VI. Tandem ſi qui fuerint, qui plane ſingularem vel peculiarem Camphoræ indolem diſtincte nequeunt concipere, & ex quibus denique conſtet, ſcire velint, in horum gratiam pro mea quidem qualicunque experientia, amice mentem meam ita declarabo; quod Camphora ſit ſiccum quid, album, pellucidum & fragile, valde acute olens, non ſolum in igne, ſed etiam in aere ſubcalido prorſus volatile corpus, quod ratione mixtionis, ex copia principii inflammabilis rarefacti, pauca aquoſitate, & ſumme attenuata vel ſubtiliſſima terra, intime inter ſe unitis conſtat.

1. Conſtat Camphora rarefacto Phlogiſto, quia non ſolum cito & tenuiter, imo etiam in aqua & nive ardet, ſed etiam cum aliis de eodem principio copioſe parti-

cipantibus celerrime unitur, ex quibus ſpiritus vini commemoratione, nitri vero ſpiritus meditatione dignißimus eſt, ſiquidem de poſteriore pauciſſima ſunt cognita.

2. Aquoſi aliquid continet Camphora, cum alias principium inflammabile, licet copioſius, flammam exhibere non poſſet.

3. Terram ſubtilem nemo negabit, quia extra hanc ſiccum corpus cryſtallinum non ſiſteret.

4. Aeque minus in dubium vocabitur, terram hanc ſubtiliſſimam, & cum coeteris intime, perque minimas, quas vel cogitando concipere poſſumus, partes unitam eſſe, ſi perpendimus, quod Camphora in ſolo aere, aliquantulum tamen ſubcalido in tota mixtura ſua, plane nihil poſt ſe relinquendo, avolet.

Definitio hæc per ea ipſa, quæ hactenus de Camphora Thymi diximus, confirmatur, conſiderando, quod ea ſolummodo ex ſubtili oleo eſſentiali ortum duxerit, in quo oleo itidem non alia, quam ſuperius dicta tria conſtituentia, phlogiſton, aquà & terra demonſtrari poſſunt, quamvis ſub notabili hoc diſcrimine, quod

1. Camphoræ mixtura multo ſubtilior quam olei, quinimo (ſi ita loqui liceat) quinta eſſentia & extractum olei ſit.

2. Camphora parum ſaltem aquei principii in ſui mixturam recepit, oleum vero intuitu hujus longe plus.

3. Terra Camphoræ ſumme attenuata, & adeo ſubtilis eſt, ut ſponte ſimul cum coeteris conſtitutivis avolet in auras, cum contra terra olei multo eſt craſſior, id quod etiam, ut alia taceam, in rectificatione vel exhalatione olei videre eſt, ubi primo

reſinæ

reſinæ vel bituminis inſtar, tandem etiam craſſiori ſpecie apparet.

Quod ſupereſt, diſſimulare non poſſum, hæc aliaque experimenta de Camphora facta dubium me habere, an etiam ullo alio quam deſtillationis modo Camphora acquiratur. Præ cœteris, de veritate ſuſpecta mihi videntur, quæ de collectione Camphoræ in Inſula *Borneo* aſſeruntur; dicunt vero quod ibi locorum ſponte Camphora inſtar gummi, vel reſinæ, ex arbore exſudet, atque ita ab incolis colligatur, multo pretioſior quam Japonica, quæque præ raritate tam cara habeatur, ut incolæ illius inſulæ nihil ejus concedant exteris. Sed quoquo modo res cum hac traditione ſeſe habeat, juſtas tamen de ea dubitandi cauſas habeo, & in his potiſſimum Camphoræ volatilitatem, naturæ ejus penitius inſitam; namque ſi ex arboribus exſudaret, non adeo diu ſub ſole fervidiore, quo terræ illius climatis incaleſcunt, perſiſteret, ſed ſat cito in aere libere exhalando fugeret, idque tanto facilius, quia cum exſudaret, multo liquidior eſſe deberet, nec conſiſtentiam Camphoræ duriorem habere poſſet, quæ vero liquiditas ad tanto celeriorem exhalationem diſponeret, ſi quidem per ſe Camphora in molli vel liquida conſiſtentia retineri plane non poteſt, cum accepto calore, & ubi colliqueſcit, mox avolat, qua de cauſa etiam cum reſina comparari nequit. Quod vero Camphora nativa, ad inſtar deſtillatæ, in oleo ſuo deſtillato quaſi ſoluta ex arbore profluat, & dein ad arboris corticem Cryſtallorum in modum concreſcat, tanto minus perſuadeor, cum pauca vel nulla potius exempla comperta ſint, quod natura olea deſtillata eſſentialia, aliaque arte facta, inſtrumentis debitis quippe deſtituta, produxerit. Et parum abeſt, quin credam totam traditionem de tali

collectione Camphoræ non niſi ex auditu commemorari, & viros cœteroquin eruditos *Ten Rheine*, *Breynium*, *Hermannum* cœteroſque, alium ex alio tranſcripſiſſe, nullum vero, quantum credere par eſt, rem oculis in arbore ipſa conſpexiſſe, quæ quidem fallacia in Hiſtoria naturali prima non foret. Hinc denique concludo, eſſe omnino hanc ſpeciem Camphoræ tam raram, ut eam oculis cernere nemini liceat, nec forſan unquam contingere queat.

C. NEUMAN.

III. *Obſervations of the* Dipping *Needle, made at* London, *in the Beginning of the Year* 1723. *By Mr.* George Graham, Watchmaker, F. R. S.

ABOUT the Time I was obſerving the Variation of the Horizontal Needle, I made likewiſe ſome Experiments with the Dipping Needle, to try, if the Dip and Vibrations were conſtant and regular. The Needle I made for this Purpoſe was 12 Inches and one Tenth long, half an Inch broad in the Middle, but not above one Tenth near the Ends; the Ends themſelves being filed to fine Edges; and in Thickneſs it was about one Third of a Tenth. The Ends of the Axis, upon which the Needle turn'd, were very ſmooth, and not bigger than was neceſſary for the Support of the Needle, which weighed nine Pennyweights twenty one Grains, or about half an Ounce *Troy*. The Ends of the Axis were placed upon the Edges of two thin Plates of Steel, that were hard and well poliſh'd, and parallel to the Horizon, that the Needle, when vibrating, might roll, and not ſlide upon the Edges of the Plates, to avoid the Friction

Friction they would have been ſubject to, by moving in Holes. A Braſs Semicircle was provided, and from the loweſt Point graduated each Way, and a few of the Degrees, about that Part of it which anſwer'd to the Dip, were divided into ſix equal Parts. By the Help of Screws, the Semicircle could be brought to a due Situation; and by two ſpirit Levels, placed at right Angles to each other, any Change of Situation was eaſily perceiv'd, and by the Screws it could be readily reſtor'd to its former Poſition; all was inclos'd with Glaſs to ſecure the Needle from being diſturb'd by the Motion of the Air. I muſt here take Notice of the great Difficulty there is of poiſing the Needle ſo exactly, before it is touch'd with the Loadſtone, as to take any Poſition indifferently: for, when it is pretty near the Truth, it is extremely troubleſome to place it at reſt in the Poſition deſir'd, in order to try which Way it is inclin'd to move. It cannot be done in the open Air; for the leaſt Motion of it will diſturb the Needle, and when it is ſhut up, it is no eaſy Matter to ſettle it in the Place intended. And that there will be a ſenſible Difference of the Dip, upon ſhifting the Sides of the Needle, whatever Pains be taken to prevent it, I am fully ſatisfied from the following Experiments.

March 20, 1722.

EXPERIMENT I.

I Touch'd both Sides of that End of the Needle, which I deſign'd to point South, upon the North-pole of a ſmall *Terrella*; after which I cauſed it to vibrate in an Arch of ten Degrees, and counted the Time by a Pendulum Clock, ſhewing Seconds, till the Needle had performed 50 Vibrations.

			′	″
It perform'd the firſt 25 Vibrations in			2	58
The next 25 Vibrations in			2	27
The 50 in			5	25
Which gives for each Vibration at a Medium				6, 5
The Needle dipp'd	73°	15′		

EXPERIMENT II.

Then I ſhifted the Needle ſo as that Side, which before reſpected the Eaſt, was now turn'd Weſt, and cauſing it to vibrate in the ſame Arch, as before, it perform'd

			′	″
The firſt 25 Vibrations in			2	49
The next 25 in			2	39
The 50 Vibrations in			5	28
That is, each Vibration in				6, 56
The Dip	73°	50′		

EXPERIMENT III.

I now touch'd the ſame End of the Needle, a ſecond Time, on both Sides, upon the ſame Stone, and ſuffering it to vibrate, as before,

It perform'd 25 Vibrations in			2′	49″
That is, one Vibration in				6, 76
The Dip	73°	20′		

EXPE-

EXPERIMENT IV.

The Needle was now ſhifted, and ſtood as in the ſecond Experiment.

It perform'd 25 Vibrations in		2′	41″
That is, one Vibration in			6, 44
Dip	73°	45′	

EXPERIMENT V.

The ſame End of the Needle being now touch'd twice on each Side, with the Loadſtone preſented by Right Honourable the Lord *Paiſley* to the Royal Society, in the Armour,

			′	″
It perform'd the firſt 25 Vibrations in			1	58
The next 25 in			1	46
The 50 Vibrations in			3	44
That is each Vibration in				4, 48
The Dip	73°	55′		

EXPERIMENT VI.

The Needle being turn'd, and ſtanding as in the ſecond and fourth Experiments, it perform'd

			′	″
The firſt 25 Vibrations in			2	00
The next 25 in			1	57
The 50 Vibrations in			3	57
That is, each Vibration in				4, 74
The Dip	74°	10′		

EXPE-

EXPERIMENT VII.

I now touch'd the Needle at both Ends with the ſame Stone, with which it was touch'd in the fifth Experiment, after which it perform'd

			′	″
The firſt 25 Vibrations in			1	35
The next 25 in			1	34
The 50 in			3	9
That is, each Vibration in				3, 78
The Dip	74°	20′		
The Dip repeated with the Needle taken off and replaced	74	20+		

EXPERIMENT VIII.

Upon ſhifting the Needle, it perform'd

			′	″
The firſt 25 Vibrations in			1	33
The next 25 in			1	34
The 50 in			3	7
The Dip	74°	25′		
The Dip repeated	74	30—		

NB. *The Needle had the ſame Side to the Eaſt in the firſt, third, fifth, and ſeventh Experiments; and had that Side turn'd Weſtward in the ſecond, fourth, ſixth, and eighth; and I began to count the Vibrations, when I obſerv'd it to vibrate juſt* 10 *Degrees, as near as I could gueſs. All theſe Experiments were made with ſufficient Care in every Particular, excepting the Quantity of the Dip, which requires the Diviſions of the Semicircle to be very equal,*

equal, and the 90th Degree to be perpendicularly under the Axis of the Needle; this laſt I found was a little faulty, the Dip being in Reality greater than the Semicircle ſhewed it. After I had rectified this Error, and new touch'd the Needle, upon that Part of the Armour to which Iron is applied, when it is to be lifted by the Stone, it perform'd the ſame Number of Vibrations in leſs Time than in any of the former Trials. I now determin'd to obſerve, for ſome Space of Time, both the Dip and Vibrations, without freſh touching the Needle.

The Obſervations follow, by which it appears there is a very conſiderable Difference, both in the Quantity of the Dip, and in the Quickneſs of the Vibrations.

N.B. *In all theſe Experiments, the Needle was placed, ſo as to vibrate exactly in the Plane of the Magnetick Meridian; and ſufficiently diſtant from all Iron that could affect it, as far as I could perceive, till I had Occaſion to put up a very large Iron Rod in the Room above it, which immediately alter'd the Dip of the Needle, and thereby put an End to theſe Trials.*

Dip

1723.	Dip ° ′	Time of the Day. h ′
Mar. 29.	75=00	at 10=00.
	74=53	4=15
30	74=55+	1=00
	74=50—	4=00
31.	74=50—	10=00
	74=50—	12=30
	74=50—	2=15
April 1.	74=25	6=45
	74=25—	7=15
	74=20+	9=00
2.	74=20+	7=30. A.M.
	74=20+	7=30. P.M.
3.	74=20+	9=30
	74=20+	12=30
	74=50	4=15
4.	74=55+	10=00
	74=50+	11=15
	74=40	12=45
	74=35	7=30
5	74=40	9=15
	74=40	1=45
	74=40+	5=30
	74=30+	8=15
6.	74=35	10=00
	74=35	12=00
7.	74=35+	10=20
	74=35+	12=30
	74=35	4=00
	74=35	6=30
8.	74=40—	12=15
	74=40—	3=30
9.	74=40—	10=00
	74=40—	4=15
10.	74=40—	10=00
	74—30+	8=00

	Dip ° ′	Time of the Day. h ′
Apr. 11.	74=35+	10=00. A.M.
13.	74=40—	
14.	74=40—	10=45
	74 40—	11=15
	74=35	5=10
	74=35	8—17
15.	74=35	9=10. A.M.
16.	74=35	11=00
	74=30+	8=45
17.	74=45	12=25
18.	74=40+	9=15
	74=45	5=00
19.	74=45	9=00. A.M.
20.	74=45	
21.	74=50	10=30
23.	74=50	12=00
26.	74=50+	2=30
27.	74=55	
28.	75=00	1=00. P.M.
	75=00	3=15
	74=58	5=15
30.	74=40	3=15
May 1.	74=45	1=30
2.	74=45	12=00
	74=45+	1=00
	74=40+	3=50

The Weight of the dipping Needle 9pt. 21gr. *Troy*.

N.B. The Mark + ſignifies ſomething more than is here ſet down, and — ſignifies ſomething leſs, but the Difference could ſcarce amount to more than two Minutes.

Experi-

Experiments of the Vibrations of the Dipping Needle, beginning with an Arch of 10 Degrees, with the Times in which 100 Vibrations were perform'd,

1723 h '
April 1. about 7=15 Afternoon.

' "
First 50 in 3= 2
Last 50 in 2=45

The 100 in 5=47. Dip 74°=25'—

April 2. in the Evening
First 50 in 3= 3
Last 50 in 2=43

The 100 in 5=46. Dip 74=20+

April 3. about 4 in the Afternoon.
First 50 in 2=52
Last 50 in 2=39

The 100 in 5=31. Dip 74=50

Repeated about an Hour after.
First 50 in 2=53
Last 50 in 2=35

The 100 in 5=28. Dip 74=50+

April 4. about 11=15 in the Morn.
First 50 in 2=54
Last 50 in 2=30

The 100 in 5=24. Dip 74=50+

April 28. about 5=15 Afternoon.
First 50 in 2=48
Last 50 in 2=16

The 100 in 5= 4. Dip 74=58

Repeated
First 50 in 2=47
Last 50 in 2=16

The 100 in 5= 3. Dip 74=58

May 20. ' "
First 50 in 3=11
Last 50 in 3= 1

The 100 in 6=12

Repeated the Needle being new touch'd.
First 50 in 2=38
Last 50 in 2=23

The 100 in 5= 1. Dip 74=35

Repeated again about an Hour after
First 50 in 2=38
Last 50 in 2=20

The 100 in 4=58. Dip 74=30+

May 21. about Noon
First 50 in 2=41
Last 50 in 2=28

The 100 in 5= 9. Dip 74=30

May 23. about 12=45
First 50 in 2=40
Last 50 in 2=27

The 100 in 5= 7. Dip 74=40

May 25. about 3=30
First 50 in 2=41
Last 50 in 2=30

The 100 in 5=11. Dip 74=40+

May 27. about 6=30 Afternoon.
First 50 in 2=41
Last 50 in 2=28

The 100 in 5= 9. Dip 74=50

An extraordinary Caſe in Surgery, communicated by Mr. Joſeph Atkinſon *Senior, Surgeon.*

A Maiden, about 20 Years of Age, was brought to me about *Chriſtmas* 1723; who had a Tumour on the inſide of her right Thigh, extending from the Groin to the Knee, which was ſo large, that I judg'd it contained at leaſt the Quantity of a Gallon; the *Cutis* was exceedingly diſtended, but of the natural Colour, only, the capillary Veins appear'd varicous, and very numerous; ſhe had alſo a large Tumour on the Buttock of the ſame Side, of the Bigneſs of a Quartern Loaf; but when the Tumour on the Thigh was preſſed, the Tumour above very much encreas'd, which ſhew'd a Communication, and prov'd afterwards ſo to be: She had alſo another Tumour on her right Side, ſtretching from the left Side of the *Vertebræ* of the Back to the *Hypochondrium*, about the Bigneſs of a Penny Loaf, her Body was very much emaciated, and ſhe could hardly breathe, and the little Victuals ſhe eat very difficultly paſſed out of the Stomach. She had had the *Menſes* but twice or thrice, about 12 Months before the Beginning of thoſe Tumours, and it is to be marked, that the Tumour of her Thigh began firſt, and increaſed to near the Magnitude I firſt ſaw it, before the Tumour of the Buttock and Hip began; after that, the Tumour of her Back began, which, as it increas'd, brought on great Difficulty in Breathing. She had been with ſeveral other Perſons, who adviſed againſt opening the Tumour of her Thigh, moſt of them being of the Opinion it was from Blood, and that her Caſe was incurable: I confeſs,

confeſs, I was of a contrary Opinion; but being told what ſo many others had ſaid, I declin'd meddling with it at that Time, though her Parents and herſelf were willing to it: At this Time I diſmiſſed her, ſaying, that, if ſhe liv'd, a little Time wou'd diſcover more of her Caſe. About two Months after I was deſired to viſit her again, which I did, but the Tumours were ſo monſtrouſly increaſed, and her Body ſo waſted, that I wonder'd ſhe could live under ſuch Circumſtances; the Tumour of the Thigh was every Way yielding to the Preſſure of the Finger, nor was there the leaſt Hardneſs about the Extremities of it, ſo that it might be eaſily miſtaken for an *Aneuriſma*, had it not wanted the grand Characteriſtick, Pulſation, which ſome ſay, is not to be felt when thoſe are very large, the Middle of this then look'd a little red, and ſhining, and ſeem'd to point a little; I told them I believed it would break with a ſmall Orifice, and ſhew what was contain'd, willing them to notify it to me if ſuch a Thing happen'd: Three Days after they call'd me in haſte to this Patient, ſaying, the Swelling of her Thigh was broke; when I came, I found there had been diſcharg'd a ſmall Quantity of matteriſh Subſtance, much like what is contain'd in a *Meliceris*, but the Opening was ſo ſmall or cloſed, that I could not enter my Probe; however, though ſhe ſeem'd to me (and to Mr. *Wall*, an Apothecary then preſent) to be ready to expire, yet, at the Deſire of her Parents, I open'd this Tumour with my Launcet, making an Inciſion about an Inch and an half long, through which poured three Pint Baſons full of Matter, beſides ſeveral ſmaller, which together contain'd about five Quarts, it was very fœtid and bloody towards the latter End of this Diſcharge; upon this, the Tumour wholly ſubſided, in-

ſomuch that the Thigh inſtantly became as ſmall as the other; I put my Finger into the Wound, and found the *Faſcia lata* quite conſum'd, the Muſcles lay all looſe, ſo that I fairly touch'd the Thigh-Bone between them, as I would.

But I ſhould acquaint you, that, immediately upon the Di'charge of this Humour, the Tumour on the Buttock was conſiderably abated, but there followed about two or three Spoonfuls of florid Blood; I dreſs'd it up for this Time with a proper Digeſtive, and a ſuitable Bandage; the Day following I viſited her again, and found ſhe had ſlept pretty well, and was much refreſh'd, and not the leaſt Faintneſs had attended her, during my Abſence, which ſhews the imaginary *Synocope*, that is feared to follow ſuch, Evacuations, to be groundleſs; the Day following, taking off my Dreſſings, I found the Limb, in its Figure, bigger than the other; at the third Dreſſing there appear'd a ſmall hard Swelling a little below the Orifice, which was made by ſome grumous Blood that lay there, which I turn'd out with my Finger, in Quantity about four Ounces, this was follow'd by a florid Blood, which much ſurpriſed me; I then judg'd this proceeded from ſome Hypogaſtrick Veſſel that ſupplied this Tumour, and I deſpair'd of Succeſs, unleſs I could meet with this Spring; ſo, in order to it, I laid open the Sinews to the Groin, and, though I could not diſcover the Veſſel, yet I ſo ſucceſsfully applied my Aſtringent, that from that Time it bled no more; however, there was, for a Week, a great Flowing of a ſerous Matter, which wholly ſunk the Tumour of the Buttock and Hip, and, by boulſtering and compreſſing with ſuitable Bandage, the ſo long ſeparated *Cutis* clos'd with the Muſcles,

Muſcles, and all Things, in about a Fortnight ſeem'd to be in a fair Way of healing, yet, it was near three Months, before this Cure was compleated.

But ſtill (that is, a Fortnight after the Opening of the Thigh) the Tumour on her Back continu'd, and ſhe much ſtraitened for Breath; ſaying, if that was opened, ſhe ſhould be preſently reliev'd; this Tumour I then open'd (which was, I ſaid, as big as a Penny white Loaf, and there iſſued out about two Quarts of Matter, or rather more: I was again greatly ſurpriſed that ſuch a Quantity, ſo much more than what poſſibly this viſible Tumour could contain, ſhould run out of this Orifice; I enter'd my Probe, and found it penetrate into the Cavity of the *Thorax*, between the ſecond and third ſpurious Rib, reckoning from beneath, on which ſhe reſpired with all Freedom, but there was a *Halitus* at this Wound; I continu'd to dreſs this and believe, before this tended towards Healing, not leſs (at Times) than a Gallon, or rather five Quarts of Matter, was diſcharg'd; but, when I thought all was over, it filled again, the external Tumour became almoſt as big as before, and her Breathing as difficult as ever, ſo that I now thought all my Labour had been in vain; yet I open'd it again with a larger Orifice, and from that Time dreſſed it ſucceſsfully, to the perfect Healing; the *Menſes* are return'd, the Patient continuing well to this Day, and in a good State of Health. This Patient has been viewed by Mr. *John Douglaſs*, and he has had an ample Account of this Affair.

V. *An*

V. *An Experiment to illuſtrate what has been ſaid in the Philoſophical Tranſactions,* N°. 386, 387, 388, *concerning the Figure of the Earth, by the Rev.* J. T. Deſaguliers, L. L. D. F. R. S.

UPON an Axis of Iron, that cou'd be made to turn ſwiftly (by means of a Wheel, whoſe String went round a Pulley fix'd to the ſaid Axis) I ſlipp'd on two Iron Hoops, whoſe Planes interſected at right Angles, repreſenting two Colures, which, being of a ſpring Temper, ſprung in ſuch Manner as to be $\frac{1}{356}$ Part longer in that Diameter that coincided with the Axis, than in their Æquatorial Diameter; this Proportion being the ſame that Mr. *Caſſini* ſuppoſes to be between the Axis and Æquatorial Diameter of the Earth. Two circular Plates, to which the ſaid Hoops were riveted, had ſquare Holes, thro' which the Axis paſs'd, ſo that the two Poles of the oblong Spheroid, which the Hoops deſcribe in their Revolution, might approach together in ſuch Manner, as to let them put on the Form of a true Sphere, when, by the Whirling, the Æquatorial Diameter of the Machine ſwell'd and over-power'd the Elaſticity of the Hoops. A greater Degree of Swiftneſs turn'd the Sphere into an oblate Spheroid of Sir *Iſaac Newton*'s Figure. A Velocity ſtill greater makes the Diſproportion of the Diameters, ſuch as thoſe of *Jupiter*; and ſtill the Æquatorial Diameter encreaſes with the centrifugal Force.

Another Hoop with a Catch, repreſenting the Æquator, ſhews (in the Experiment) the Increaſe of the Æquatorial Circumference, and an Index applied to the Frame, ſhews the Increaſe of the Diameter.

As

As ſoon as the Revolution of the Machine ceaſes, the Colures, Meridians or Hoops return to their Elliptical Figure, whoſe longeſt Diameter is the Axis of Revolution.

If the Force, by which the Hoops endeavour to keep their Figure, be conſider'd, as the Gravity that keeps together the Parts of the Earth; from this Experiment compar'd with what has been ſaid in the Tranſlations above-mention'd, it will appear that the Earth cannot preſerve its Figure, unleſs it be an oblate Spheroid.

VI. *Some Experiments concerning the* Coheſion *of* Lead, *by the ſame.*

HAVING, on *Thurſday* the 29*th* of *April* laſt, made mention of ſome Experiments, concerning the Coheſion of two Balls of Lead, applied together in a ſmall Part of their Surface (ſo as to require a Weight far greater than what will overcome the Preſſure of the Atmoſphere, to ſeparate them) made by Mr. *Trievall*, at *Newcaſtle*, and at *Edinburgh*; I made the following Experiment to the ſame Purpoſe, before the *Royal Society*.

I took the Leaden Balls A and B (*Fig.* 1.) the firſt weighing one Pound, and the other two Pounds; and having from each of them cut off a Segment of about $\frac{1}{4}$ Inch in Diameter, I preſs'd them together with my Hand, with a little Twiſt, to bring the flat Parts to touch as well as I cou'd. The Balls ſtuck ſo faſt, that when the Hand H, by means of a String, ſuſtain'd the upper Ball A, the lower one B (by reaſon of its Contact at C) was ſuſtain'd, tho' loaded with the

Scale

Scale S, and Weights E, which amounted to 16 Pounds. A little more Weight added ſeparated them, and, upon viewing the touching Surfaces, it appear'd that they did not exceed a Circle of $\frac{1}{10}$ Inch Diameter; but this Surface can hardly be meaſur'd exactly, on account of its Irregularity. The Experiment was repeated ſeveral Times, and the Coheſion of the Balls was different every Time.

On *Thurſday* the 6th of this Month, I made the Experiment another Way, at the Meeting of the *Society*, and juſt after their Riſing, before ſeveral of the Members.

On the upper Pin or Bar of the Wooden Frame D *d* I H, I ſuſpended the Steelyard E F, whoſe Hook held up a leaden Ball A of two Inches in Diameter, having an Hole thro' it, at A, to receive a String; the lower Ball B equal to, and prepar'd in the ſame Manner as the firſt, receiv'd the Pin *O o* thro' its String, ſo that G, the Weight of the Steelyard, was made Uſe of to ſeparate the Balls, which happen'd when it was applied at the Number 20, in the firſt Experiment; but, in the three following Experiments, the Balls were not ſeparated till the Weight was remov'd to the Numbers 25, 37, and 45, expreſſing Pounds on the Steelyard.

Laſtly, the Balls being applied together as before, ſtill cleaning the Surface of Contact with my Knife, and never making a Contact ſenſibly greater than what I mention'd before; the Weight G remov'd quite to the End F, where it weigh'd 47 Pounds, was not able to ſeparate the Balls, ſo that I was oblig'd to make Uſe of another Steelyard; but as I intended to begin my Trials with the Weight, at the Number 47 upon this laſt Steelyard, by Miſtake, I applied a wrong Weight inſtead of G, which, being of

7 Pounds

7 Pounds, inſtead of 4, ſeparated the Balls. Having found the right Weight afterwards, I made ſeveral Trials, but cou'd not again bring the Force of the Contact to be equal to 47 Pounds. How much greater than 47 Pounds the Force of the Contact was in the 5th Experiment, I cou'd not determine, by reaſon of the above-mention'd Accident; but the Surface was much as before. I ſhall make ſome more Experiments of this Kind, and if I can come to any Certainty in the Meaſure of the touching Surfaces, and utmoſt Force of the Coheſion; I will communicate the ſame, and repeat them before the Society.

VII. *Extract of ſeveral Letters and Certificates ſent to his Majeſty the King of* Great Britain, *concerning a very particular* Nævus Maternus, *or* Mole. *Communicated by Dr.* Steigertahl, *Phyſician to his Majeſty,* F. R. S.

JEREMIAS *Rudolph von Walthauſen*, a Captain of the Garriſon at *Danneberg*, near *Lunebourg*, was born *October* 24, 1680, with a very ſingular Mole upon his right Arm, Shoulder, and Hind-part of his Side, not unlike the Branch of a Vine, with its Leaves and Grapes. It hath been affirm'd and atteſted both by the Deceas'd himſelf, and ſeveral of his Relations and Friends, that his Mother, when big with Child, had an earneſt Deſire for Grapes, and impatient to ſtay, till they were full ripe, went down into the Garden to pull off ſome of thoſe unripe; whereupon it happen'd, that a whole Branch with its Leaves and Grapes ſuddenly fell down upon

her right Arm, which Accident much frighten'd her. Some Time after ſhe was brought to Bed, and the Child was obſerv'd to have ſeveral reddiſh or bluiſh Spots, beginning from behind his Shoulder, and from thence running over the ſame, down the right Arm to the Fingers. The Captain's whole right Side was bigger than the Left by an Inch and upwards, and ſo continu'd to his Death. The Veins of the right Arm were much rais'd, lying almoſt immediately under the *Cuticula*, which made them very conſpicuous, they were, beſides, very much diſtended, chiefly between the Elbow and Hand, where they were almoſt as big as a Man's Thumb. On the Inſide of the fore Finger the Vein was extended into a ſmall Tumour, of a reddiſh, or purple blue Colour, about the bigneſs of a Nutmeg, corrugated with ſome lenticular Protuberances, which made it in ſome meaſure reſemble a Grape. The like Tumours, but not ſo big, were obſerv'd in ſeveral other Parts of the Arm, in the Spring Time; and as they thought, when the Sap began to enter the Vines, as alſo when the Vines flower'd, and in *Autumn*, when the Wine was fermenting, the Captain was taken ill, with violent and itching Pains in the affected Arm for ſome Days. The whole right Side then ſwell'd more than uſual, and the Veins and Tumours above-mention'd were ſo diſtended with Blood, that at laſt, a ſerous Matter was forc'd out of the Pores of the ſaid Tumours, which as it gave the Patient ſome Relief, ſo he promoted it, by ſcraping the Tumours with the Edge of a Penknife. If the Captain held up his affected Arm, the Running of the Blood backwards in the diſtended Veins was very viſible. If he held his Arm down again, the Blood return'd with ſome Noiſe, and ſenſibly fill'd up again the Veſſels, which by the preceding

ceding Action had been emptied. For this Reaſon, the Captain, when he was a Bed, was oblig'd to lay his Arm upwards. In *February* laſt, the Captain was again taken ill with violent Pains, and a ſtrong ſenſible Puſhing of the Blood into the affected Arm; for both which, they being not only more violent, but continuing longer than uſual, he was advis'd to be let Blood on his left Arm; as this did not immediately relieve him, they applied at his Deſire externally *Epithemata* of Plants boil'd in Wine; this eas'd his Pains, and made him ſomewhat more quiet, ſo that he could walk about the Houſe, but he nevertheleſs continu'd reſtleſs for ſome Days, and was, on *Saturday* the 17th of *February*, S. N. early in the Morning, ſeiz'd with a violent Oppreſſion of his Breaſt, which in a few Minutes made an End of his Life. The Surgeon, then preſent, did not bleed him, but gave him only ſome Spoonfuls of a ſtrong Cordial Mixture, which he juſt happen'd to have about him, and of which the Patient, with much Difficulty, could ſwallow but a ſmall Part. He was, when he died, 44 Years, 3 Months, and 24 Days old; he was otherways a ſtrong robuſt Man, much given to Fatigue, and diſcharg'd with great Bravery all the Duties of a good Soldier, as appear'd by his Conduct throughout the laſt War.

VIII. *An*

VIII. *An Account of a Book, entitled,* Hiſtoria Cœleſtis Britannica, tribus Voluminibus contenta, Authore *Joanne Flamſteedio* Aſtronomo Regio.

THE firſt Volume contains the Obſervations of Mr. *William Gaſcoigne* (the firſt Inventor of the Way of meaſuring Angles in a Teleſcope, by the Help of Screws; and the firſt that applied Teleſcopical Sights to Aſtronomical Inſtruments) taken at *Middleton*, near *Leeds*, in *Yorkſhire*, betwixt the Years 1638 and 1643, excerpted from his Letters to Mr. *Crabtree*; with ſome of Mr. *Crabtree*'s Obſervations of the ſame Years; as alſo Obſervations of the Sun's and Moon's Diameters; Configurations and Elongations of *Jupiter*'s Satellites from him, ſmall Diſtances of fixed Stars, with Appulſes of the Moon and Planets to them, obſerv'd with a Teleſcope and Micrometer at *Derby*, by Mr. *Flamſteed*, betwixt the Years 1670 and 1675; with the larger intermutual Diſtances of fixed Stars, and of the Planets from them; Eclipſes of the Sun, Moon, and *Jupiter*'s Satellites, Spots on the Sun, Comets and Refractions, taken with a Sextant of near ſeven Foot *Radius*, a voluble Quadrant, and the above-mention'd Inſtruments, betwixt the Years 1675 and 1689, at his Majeſty's Obſervatory, rank'd under proper Heads, with the Places of the *Moon*, *Saturn* *Jupiter*, *Mars*, *Venus*, and *Mercury*, deduced from the Obſervations, and alſo neceſſary Tables to be us'd with them.

The ſecond Volume contains his Obſervations (made with a mural Arch of near ſeven Foot *Radius*, and 140 Degrees on the Limb) of the meridional Zenith

Zenith Diſtances of the fix'd Stars, Sun, Moon, and other Planets, with the Time of their Tranſits over the Meridian, together with Obſervations of the Sun's and Moon's Diameters, Eclipſes of the Sun, Moon, and *Jupiter*'s Satellites, Variations of the Compaſs from 1689 to the End of the Year 1719, *&c.*

Tables ſhewing to render the Calculation of the Stars and Planets Places, from the Obſervations, eaſy and expeditious; to which are added the Places of the Moon, (at the Oppoſitions, Quadratures and on her Limits, *&c.*) and the Places of *Saturn*, *Jupiter*, *Mars*, *Venus* and *Mercury*, derived from the above-mentioned Obſervations.

The third Volume contains a Catalogue of the right Aſcenſions, Diſtances from the Pole, Longitudes, and Magnitudes of near 3000 fixed Stars, with Variations of their right Aſcenſions and Diſtances from the Pole, whilſt they change their Longitudes one Degree, whereby their right Aſcenſions and Diſtances from the Pole may be determin'd for 200 Years paſt, or to come, ſufficiently exact. Large Tables, by which the right Aſcenſions, and Diſtances from the Pole of the Stars and Planets, being given, their Longitudes and Latitudes may be found by Inſpection. To this Volume is prefix'd a very large Preface; containing, an Account of all the Aſtronomical Obſervations made before his own Time, with a Deſcription of the Inſtruments made Uſe of; as alſo an Account of his own Obſervations and Inſtruments, together with a new *Latin* Verſion of *Ptolomy*'s Catalogue of 1026 fixed Stars, from the *Greek*, and *Uleg-beig*'s Places annexed on the *Latin* Page, with the Corrections; ſmall Catalogues of the *Arabs*, *Tycho Brahé*'s of about 780 fixed Stars, in a proper

proper Order; the *Landtgrave* of *Heſſe*'s of 386; *Hevelius*'s of 1534, in a proper Order. A Catalogue of ſome of the Southern fixed Stars, not viſible in our Hemiſphere, reduc'd to right Aſcenſion, Diſtance from the Pole, Longitude, and Latitude, with Variations of right Aſcenſions, and Diſtances from the Pole; calculated from Obſervations made by Dr. *Halley*, at St. *Helena*, and Mr. *Flamſteed*'s Star's Places, and fitted to the Year 1726.

FINIS.

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Philos Trans Nº 390.

Agnus Scythicus vegetabilis,
Borametz dictus;
ex
Musco Breyniano.

Numb. 390.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *September* and *October*, 1725.

The CONTENTS.

ERRATA.

NO. 389. Page 341. line 31, 32. read *three quart Basons*
p. 342. l. 15. r. *Syncope.* l. 18. r. *but little bigger.*
l. 27. r. *Sinus.*

I. *Dissertatiuncula de Agno Vegetabili Scythico*, Boranetz *vulgo dicto. Auctore D^no* Joh. Phil. Breynio, *M. D.* Dantisc. *R. S. S.*

I.

NON fingendum, sed inveniendum, quid Natura faciat, aut ferat. Aurea hæc verba utinam continuo animo obversarentur illorum, qui reconditos Naturæ thesauros quærunt & ejus arcana rimantur. Hoc si factum fuisset olim, certe Historiam Naturæ per se ampliſſimam, tot ac tantis non haberemus inquinatam anilibus fabulis & opinionum commentis. Cujus rei exemplum nobis inpræsentiarum præbet Agnus dictus Vegetabilis Scythicus, Barbaro nomine *Baranetz*, *Boronetz* vel *Boranetz* inter Historiæ Nat. Scriptores notus.

II.

DE hoc Agno Vegetabili imprimis egerunt Athanasius Kircherus in Opere de Arte Magnetica, (*a*) qui citat Sigismundum L. B. ab Herberstein, Haytonem Armenum, Surium & Jul. Cæs. Scaligerum, Franciscus Baco de Verulamio, (*b*) Fortunius Licetus, (*c*) Andreas Libavius, (*d*) Eusebius Nierenbergius, (*e*) Adamus Olearius (*f*) & Olaus Wormius, (*g*) ut cæteros, quos inter multi Botanici, qui eandem fere canunt cantilenam, nunc taceam.

(*a*) *p.* 504, & 505. (*b*) Histor. Natur. Cent. 7. N°. 609. (*c*) de Spontaneo Viventium Ortu. C. 45. (*d*) Historia Agni Scythiæ. (*e*) Histor. Natural. *p.* 34. (*f*) de Itinere Persico. *p. m.* 155. (*g*) Mus. *p.* 190.

III.

HUNC Julius Cæſ. Scaliger (*b*) titulo Agni Scythici, Borametz, ſequentem in modum deſcribit: " Superiora ludum putes, prout eſt admirabilis Tartaricus frutex. Tartarorum horda primaria Zauolha eſt, vetuſtiſſimæ nobilitatis commendatione. In eo agro ſerunt ſemen feminis Melonis ſimillimum, ſed minus oblongum. Ex eo ſatu plantam exire, quam Borametz, id eſt, agnum vocant. Creſcit enim agni figura ad pedum fere ternum altitudinem: quem pedibus, ungulis, auribus, toto capite, præterquam cornibus, repræſentat. Pro cornibus pilos gerit, ſingularis cornu ſpecie. Obducitur corio tenuiſſimo: cujus detracti uſus ad capitum tegmina incolis. Ferunt internam pulpam Gammari referre carnes. Cæterum è vulnere quoq; ſanguinem manare. Dulcore eſſe admirabili. Radicem humo exertam ſurrigere ad umbilicum uſq; Illud miraculi fovet magnitudinem. Quandiu vicinis obſidetur herbulis, tamdiu vivere, quaſi agnum in læto paſcuo. Abſumptis illis, tabeſcere, atq; interire. Idque non ſolum vel caſu, vel tractu temporis, ſed etiam experiundi gratia, ſubtractis, atq; ablatis evenire. Quin illud auget admirationem: appeti à Lupis eam, non item ab aliis beſtiis, quæ carne veſcantur. Hoc quaſi condimentum, atq; intritum, ad fabulæ, & agni alluſionem. Illud ſcire velim: Ab uno ſtipite, quatuor diſſita crura cum ſuis pedibus qui poſſint provenire, atque produci.

" Hæc, quod non ignorarem haud ingrata fore tibi, cæteriſq; Philologiæ ſtudioſis, enarravi partim à nobi-

" liſſimis,

(*b*) de Subtilitate Contra Cardanum Exerc. 181. *Sect.* 29, & 30.

" liſſimis, partim ab exercitatiſſimis in rerum Natura
" viris non ſolum ſecta, ſed etiam audita : quibus in-
" genii tui ampliſſima ſpatia implere aliqua ex parte
" poſſes."

Pari ratione cæteri eum deſcribunt, vel potius ex Scaligero exſcribunt Authores, quorum tamen non nulli in quibuſdam circumſtantiis variant, & Athanaſius Kircherus ejuſdem addit, vel ut rectius dicam, fingit figuram. Imo in nonnullis Rerum Nat. Muſæis, ut in Wormiano, Swammerdammiano, *&c.* ejus, uti volunt, quondam demonſtrabatur detracta pellis.

IV.

ANTONIUS DEUSINGIUS (*i*) rem accuratiori rationis trutina examinans, fabuloſa eſſe, quæ de Agno hoc traduntur, ſubolfecit, & ipſum Julium Cæſ. Scaligerum, qui inter primos Authores, ut ſupra dictum, ejus meminit, eum ut fabulam tractâſſe evincere conatur: Quemadmodum & alii, minus credduli, eundum in dubium vocârunt.

V.

ET revera totam hanc de hoc Agno Hiſtoriam, ſi ab animo præjudiciis vacuo accurate examinetur, fabulam ſapere, imo eſſe, & Deuſingium recte judicâſſe, ſequentes evincunt rationes.

1°. Quia à nullo fide digno Authore Agnus ejuſmodi Vegetabilis unquam viſus. Quæ enim Olaus Wormius (*k*) narrat ex relatione D. Eovaldi de Kleiſs, Electoris Brandenburgici Legati, (ipſi ſcilicet in confinibus Tartariæ degenti à Tartaro quodam fuiſſe oblatam ejuſmodi plantam exſiccatam, foliis Tabaci, cu-

(*i*) De Agno Vegetabili, quod extat in Faſciculo ejus Diſſertationum ſelectarum, *p.* 598. & *ſeq.* (*k*) *l. c.*

jus cauli adhærebat fructus, agnellum figura plane referens, magnitudine pedali, vellere crispo tectus) suspecta sunt, cum Vir ille Nobilissimus, ab astuto quodam Tartaro, ipsi fucum faciente facile seduci potuit. Quid verò de pellibus, quæ hoc nomine in Musæis demonstrantur, sentiendum sit, ex infra dicendis apparebit.

2°. Quia à Doctiss. & Rerum Nat. Scientissimo Engelberto Kæmpfero, M. D. (*l*) in supposita hujus Agni patria anxie licet quæsitus, tamen nihil huic simile inventum; hæc enim ejus sunt verba: "Quia de "exist entia Zoophyti gramina pascentis, nullibi Tar- "tariæ apud vulgum, vel Botanicæ peritos extat "notitia & memoria, prout ipse ad risum & nauseam "exploravi, neq; ulla res Borometz dicta, præterquam "ovinum pecus ibidem potest reperiri, meram esse, "quicquid de hac planta proditur, fictionem & fa- "bulam afferimus."

3°. Denique quia tota de hoc Agno relatio fabulæ adeò videtur similis, ac ovum ovo.

VI.

FABULÆ autem hujus originem optime detexit diligentissimus Naturæ in Orientalibus Regionibus Scrutator, jam jam laudatus Kæmpferus loc. cit. ubi (præmissis de Etymologia vocis Borometz, quod corruptum ait ex Moscovitico Boranetz, Polonicè Baranek, quod diminutivum est vocis Baran Sclavonicæ originis, Polonis Russisq; ovem significantis) ait, in quibusdam Provinciis circa Caspium Mare esse quoddam ovium genus, præter vulgare nobis cognitum, quod ab eo in multis discrepat, imprimis commendabile

(*l*) Observat: de Agno Scythico s. fructu Borometz qui; exstat in Dissert. sua inaugurali, nec non in Amoenitatum Exoticarum Fasciculo III. Obs. 1.

bile pellium elegantia, quas deſcribit, & qua ratione ad uſus pro veſtimentis adornandis Tartaris Perſiſq; familiares præparentur, docet, & addit: " Magnatum " divitumq; faſtum ſupra vulgi ſortem amiciri cupi- " entem, pullorum pelliculas expetere, qui annoſis " multo teneriores & eo quidem cariores, quo juniores " fuerunt agnelli, quibus detractæ ſunt; horum enim " pili à pellionibus cogi patiuntur in ſubtiliores & " denſiores cirros, qui toti pelliculæ pretium & venu- " ſtatem tribuunt. Inde fit, ut teneritatis & lucri " inſidiatores non vereantur ipſam anticipare, crudeli " laniena, nativitatem, & matribus uterum gerentibus, " diſſecto ventre partum ante partum eximere, ſolius " gratia pelliculæ obtinendæ. Hæc rite elaborata, " tam dubiam prorſus & delicatiſſimam teneritatem " exhibet, ut reſectis extremitatibus, vix agninæ cu- " tis retineat ſimilitudinem, ſed ignaram credulita- " tem, ſpecie quadam membranæ cucurbitinæ lanugi- " noſæ poſſit decipere. His ſubjungit: Pretium pel- " liculæ, pro bonitatis opinione, exſurgit ad aureos " tres, vel amplius; ſervit ea pro duplicatione mitra- " rum, nec rarò, ornatus gratia, togarum & amicu- " lorum limbum conſtituit. Tandem concludit: Sive " fabula hæc natales debeat conjecturæ alicujus con- " templatoris Philoſophi, ſive inſcitiæ tribuenda ſit pri- " mi relatoris, qui per linguæ ignorantiam, vel incu- " riam, in parergo rem auditam perperam intel- " lexiſſe potuit, ſive cuicunq; acceptam referamus " occaſioni alii, qua per longe diſſitas regiones paſ- " ſim provecta, ambiguæ teneritatis pellicula, integri- " tatem hiſtoriæ, ac nominis ſui amiſerit: donec tan- " dem appulſa nobis cum prodigii lepore, in Virum " illuſtrem, curioſum & hujus peregrinæ lanugi*n*is " admiratorem incidens, vegetabili vultu ſuo fidem " prodigii, ut ſolent mirabilia omnia, nullo negotio " fecerit.

" fecerit. Sic authoritate illuſtratus error, mox ſcrip-
" tis quoq; firmatus, ſapientiſſimorum ingenia & vul-
" gi opinionem ita occupavit, ut hodie pro Zoophiti
" ſpecie inter rariora Muſæorum oſtentari ſoleat, quæ
" veriſſima Cæſarei foetus pellicula eſt." Hactenus ille.

Ex citatis patet pelles illas agninas Perſicas, quæ à Pellionibus noſtris Perſianiſche Baranken ſalutantur, hujus eſſe generis, quamvis non optimæ notæ, illorum ſcilicet agnorum, qui Cæſarea ſectione ex Matrum uteris exſcinduntur, cum earum pretium, teſte Kæmpfero ad aureos tres, vel amplius in ipſa eorum patria exſurgat, noſtræ verò pretio longe minori apud nos, unico ſcilicet aureo, ad ſummum veneant.

VII.

Interim ante triennium circiter Vir quidam eruditus & curioſus ex Moſcovia iter faciens & Urbem noſtram tranſiens, Muſæolum meum, inter alia quædam Naturalia, Agno ejuſmodi, ut vocabat, Scythico, quod pro genuino Borometz, ceu magnum Κειμήλιον venditabat, ditavit. Hic ſex pollicum præter propter erat longitudinis, capite cum auribus & quatuor cruribus inſtructus, coloris ferruginei & totus lanugine quadam inſtar panni holoſerici villoſi Gammet vulgo dicti, tectus, auribus & cruribus, quæ glabra erant, coloris ad fuſcum magis vergentis, exceptis. (*) Ad examen vocatum cognovi, non eſſe animalis naturæ, neq; fructum alicujus plantæ, ſed radicem cujuſdam vegetabilis craſſam, reptantem & villoſam, vel potius caulem ſcandentem plantæ alicujus, qui arte obſtetricante figuram aliqualem animalis quadrupedis acquiſiverat. Crura enim quatuor erant tot reliquiæ caulium,

(*) *Vid.* Figuram naturali dimenſione expreſſam.

lium, vel ſi mavis pediculorum abſciſſorum, qui *folia* ſuſtentaverant, quemadmodum & aures, qui tamen cornibus ſimiliores; præterea hinc inde emergentes fibræ, per quas more cæterarum nutrimentum radix craſſa, vel potius caulis & per hunc planta ceperat, nullum amplius relinquebant dubium. Quartum quoq; crus anterius, non ut cætera corpori continua, ſed arte erat intruſum, quemadmodum & ipſum cum collo caput ſubtili modo annexum, accuratiori examine deprehendi. Ut adeò hic Agnus eodem artificio ex radice hac vel caule formatus fuerit, quo Homunciones ex Mandragoræ & Bryoniæ radicibus, qui Alraunen vulgo dicuntur ab Agyrtis. Remanſit mihi tamen dubium, ex qua planta hoc ludibrium artis & naturæ efformatum ſit, quanquam mox ſubiit cogitatio, illud plantæ cuidam ex capillarium genere vulgo dicto adſcribendum eſſe, cujus varia habebam indicia, ab analogia mihi cognitarum quarundam exoticarum ſpecierum, nonnullarumq; ab Illuſtri D° Hans Sloane & Rev. P. Carolo Plumier in ſplendidiſſimis ſuis Operibus deſcriptarum & delineatarum; utpote, quarum diverſæ caules ſcandentes ejuſmodi lanugine ferruginea *ſ.* rufeſcente muſco, ut vocant, tectos emittunt. Interim tamen, ex qua ſpecie præciſe, determinare non valui. Peculiarem itaq; forte nondum deſcriptam ſpeciem eſſe, quæ in Tartaria reperitur, arbitror, donec cum tempore certiora edoctus fuero.

VIII.

Hanc meam ſententiam confirmant, quæ poſtmodum legi in Tranſactionibus Philoſophicis Anglicanis (*m*) ubi Illuſtris D^s. Hans Sloane, Collegii Med. Lond.

(*m*) N° 287. *p.* 861. quod etiam reperitur *in the Philoſophical Tranſactions abridged by* John Lowthorp', *Vol.* 2. *p.* 646. *f.* 160.

Lond. nunc Præſes & Regiæ Societatis Vice-Præſes digniſſimus ſimilem Agnum Scythicum fictum ex India Orientali nactus, deſcripſit & delineavit, qui tamen longe minus Agni figuram refert, quàm meus memoratus. De ejus origine eandem modo laudatus Vir mecum fovet ſententiam; cæterùm ait, lanuginem ejus in uſu eſſe contra Hæmoptyſin, ad ſex grana pro doſi ter exhibitam, præterea ejuſdem lanuginis mentionem fieri à Merretto (*n*) titulo POCO SEMPIE, *ſ.* **a Golden Moſs,** ubi Virtus ipſi cordialis adſcribitur. Porro ſibi certò relatum, lanuginem hanc in China adhiberi pro ſiſtendo ſanguine in vulneribus recentibus, ut apud nos telæ araneorum, ibidemq; in tanta æſtimatione haberi, ut paucæ familiæ hac careant; dum ipſe ſæpius in ſputo ſanguinis uſurpatam meminerit, ubi tamen non infallibile remedium hoc, licet innocens, expertus eſt.

IX.

CREDO autem ejuſmodi Agnos ex certis radicibus vel caulibus in Moſcovia & Tartaria effingi, ut aliquo modo Hiſtoriæ Agni Scythici Vegetabilis adſtruatur Veritas. Quis autem non videt hunc ab eo, qui a ſupra citatis Authoribus deſcribitur, plane diverſum, nec tanta admiratione dignum eſſe, cum etiam hic ex variis radicibus varia portenta, quæ rerum naturalium quarundam ſimilitudinem aliqualem habeant, effingi poſſint; quemadmodum ſupra de Mandragoræ radicibus retuli, dum interim hic non magis agnus dicendus, ſed radix vel caulis revera eſt & manet plantæ cujuſdam, ac Homunciones **Alraunen,** Belgice PISSE DIEFJES (*o*) dicti, radices Mandragoræ.

II. *De*

(*n*) In Appendice ad Nehemiæ Grævii Muſ. Soc. Reg. *p.* 386.

(*o*) De quibus Vid. Deuſingius de Mandragoræ Mangoniis, quod extat in Ejuſdem Faſciculo Diſſertationum, *p.* 586. nec non Miſcellan. Natur. Curioſ. D. 1. A. 1. Obſ. 48.

II. *De Camphora. Auctore Dno* Joh. Brown, *Chym. R. S. S.*

IN tractatu nuper à Domino *Neuman*, Germano, in Angliam miſſo, cui Titulus de Camphora, quique coram hâc Societate lectus fuit, inter alia Vir Doctus hæc aſſerit. " Acquiſivi ex noſtrati Thymo " Vulgari verum, denſum, cryſtalliformem, & in " omnibus qualitatibus conſpirantem, nec niſi odore " differentem Camphoram ; quæ cuncta experimenta " huc uſque cognita, & pro juſto examine excogita- " ta poſſibilia, æque ac orientalis ſuſtinet, atque ita " cum hoc in omnibus, ſolo odore excepto, convenit." .... Quibus verbis paulo poſt ſubjungit, " ſe hanc " Camphoram ex Oleo Thymi deſtillato obtinuiſſe."... Hæc ille : de quibus non inutile erit pauca obſervare.

Primo mihi animum ſubit, Quoddam apud nos eſſe eodem modo è noſtrati Thymo deſtillatum, quod hactenus improprio Salis Thymi Volatilis nomine dictum fuit : non tamen video quomodo Camphora Thymi verius dici poteſt. Equidem non in dubium voco quod ex propria experientia aſſeruit Vir Doctus de eo, quicquid demum ſit, quod è Thymo extraxit, quodque appellat Camphoram : Liceat tamen mihi, pace viri eruditi, paucis, notare differentias, quas ipſe obſervavi inter Camphoram proprie dictam, & illud quod è Thymo apud nos deſtillatum habemus.

Præparatio noſtra eſt hujus modi : Oleum Thymi coagulatum ſeu condenſatum, pellucidum, cryſtalli formâ indutum, variæ magnitudinis, variæque figuræ, poſt primam ſeparationem ſaccharo cando albo, mox vero rubro, ſimile ; valde Thymum ſpirans, pun-

gentis vel etiam cauſtici ſaporis, non fragile ut Camphora. Hoc in quibuſdam experimentis à vulgari Camphorâ, qualis in officinis venditur, non multum differre fateor.

Eadem ſpecifica gravitate videntur eſſe.

Neutrum horum in aquâ ſolvi poteſt. In Spiritu Vini ambo facile ſolvuntur.

In aliis vero Phænomena adeo ſunt diverſa ut mea ſententia nomen Camphoræ utrique competere non poſſit.

(1.) Camphorâ in Spiritu Vini ſolutâ & poſt in aquam projectâ, ſubito coagulum album ſupernatat, quod reverâ apparet eſſe Camphora, odore, ſapore, & ſubſtantiâ. Quod ſi eadem in oleo noſtro coagulato experiri velis, videbis in aquæ ſuperficie guttas parvas olei liquidi innatare, quæ ſenſim in cryſtallos planos (cryſtallis Lunæ non multum abſimiles) formantur, etiamque ſaporem & odorem Thymi retinent. Diverſitas adhuc magis è ſequenti experimento comparuit. Utramque mixturam ſupradictam tota nocte in aere frigido expoſui: Mane coagulum, quod prius Camphoræ in Spiritu Vini ſolutæ innataſſe dixi, prorſus evanuerat, aquâ Camphoræ odorem adhuc retinente ; (forte propter ſpiritum vini admixtum :) In altero vero, nempe Oleo Thymi coagulato, cryſtalli aquam ſupernatantes, in eodem ſtatu, quo ante fuerant, remanebant, nec quidquam erant deminuti.

(2.) Camphoram ſolidam per vim ſpiritus nitri in liquorem pellucidum mutatum iri, quis in re chymicâ vel minime verſatus non novit ? At oleum noſtrum coagulatum, ſi eidem menſtruo adjiciatur, priſtinam quidem formam cryſtallinam penitus amittit, & ſubſtantiæ reſinoſæ ſeu gummoſæ formam induit, neque, quantumvis vaſculum, in quo continetur, validâ manu & ſæpe concutias, unquam in fluidum evadet.

vadet. Preterea, ſi liquorem illum pellucidum, in quem Camphora redacta erat, in aquam poſtea effundas, redintegratur Camphora: contra, oleo noſtro coagulato eadem ſubſtantia reſinoſa, ſeu gummoſa, adhuc manet, & ſapor Thymi adhæret cum notabili amaritudine adjecta.

(3.) Oleum Vitrioli (ut docuit noſter Boyleus) Camphoram in totum diſſolvit: in Oleum vero Thymi coagulatum, quod ſupra deſcripſimus, nullam aliam mutationem inducit, niſi quod vi manûs concutientis cryſtalli in minores partes dividantur; & hoc modo cryſtalli olei, ſi microſcopio obſerventur, fragmentis Camphoræ ſe magis aſſimilant.

Inſuper Camphora Oleo Vitrioli ſoluta ſe ab hoc menſtruo recuperat, ſi poſtea in aquam fundatur: noſtrum vero eandem formam cryſtallorum, quam in Oleo Vitrioli habuit, etiamſi in aquam effundas, una cum ſapore & odore Thymi, adhuc retinet.

(4.) Scribit Dnus *Neuman Camphoram conſtare rarefacto Phlogiſto* (hæc ſunt ejus verba) at minime quidem noſtrum: nam ſi igni admoveatur, extemplò colliqueſcit, & oleum liquidum fit. Fatendum quidem eſt, ſi goſſipium immergas, idque deinde igni admoveas, flammam ſtatim concipit: ſed hoc goſſipio deberi clarum eſt, idemque oleis omnibus commune.

(5.) Ignis in duo iſta corpora diverſos effectus habet. Nam qualiſcunque ignis gradus idoneus eſt ad ſublimandam Camphoram ſine ulla liquefactione, idem corpus hoc è thymo deſtillatum & condenſatum prorſus liquefaciet, & liquefactum tenebit cum nulla vel minimâ ſublimatione. Hactenus de differentiis.

Quibus non abs re fuerit teſtimonia quædam de oleis coagulatis, ſeu ſalibus ex oleis natis, huc pertinentia, ſubjungere.

Quidam ex noſtris ſociis, rei chymicæ admodum peritus, Dom. *Slare*, in Philoſoſophicis Actis, ab hinc pluſquam triginta annis editis, oleum noſtrum hiſce verbis deſcribit, Ex ſatis magnæ copiæ Thymi deſtillatione, ſeparatum eſt ſal volatile indolis ſingularis, (quod Peritiſſimus noſter Chymicus D. *Moult* primum mihi exhibuit) hoc eſt pellucidum, vel cryſtallinum, quoad gravitatem ſuam ſpecificam paulo aqua ponderoſius, idemque & ſal & oleum in uno corpore coagulatum eſſe videtur: in aqua non ſolvitur, ſed calefactum facile avolat.

Idem Vir Doctus ſe Oleum Cinnamomi viginti annos aſſervaſſe dicit, & inta ſex annos poſtremos, ſingulis annis quoddam ſalis præcipitatum fuiſſe, & hoc ſine ullo additamento aut arte ad ſalis formam inducendam. Sed quoad hujus ſalis naturam, an ſimile fuerit ei, quod ex Thymo deſtillatum oſtendit D. *Moult*, ſilet.

Helmontius equidem de ſale ex eodem Oleo, arte facto ſic locutus eſt. "At quando Oleum Cinnamomi ſuo ſali Alkali miſcetur, abſque omni aqua, "trium menſium artificioſa occultaque circulatione, "totum in ſalem volatilem commutatum eſt;" etiamque in alio loco modum, quo Oleum Cedri ſalis naturam aſſumat, vi menſtrui ſui Alkaheſt, docet, additque hæc verba, "ſeſe permiſcebit aquæ.

Cl. *Boerhavius* (ſi modo Chemiæ inſtitutiones ejus nomine nuper editæ, genuinæ ſunt) ſic ſcribit, "Diu "aſſervata olea pretioſiſſima aromatum orientalium, "vaſis bene clauſis, mutantur in ſalem, ſeu ſaponem; "ſed experimentum non facile imitari poſſumus, nam "docente *Hombergio*, demum fit per multos annos, "& dicuntur tunc aquæ miſceri."

In

In alio loco cum Dom. *Neuman* meritò nunc in Societatem noſtram admiſſo, conſentire videtur *Boerhavius.* " Camphora eſt nil niſi Oleum per deſtillatio-
" nem concretum in glebam pellucidam ; nam non
" eſt progenies ſolius arboris Camphoræ, ſed omnes
" plantæ aromaticæ Camphoram ſui generis produ-
" cere poſſunt ; " & paulo poſt " Oleum quod in
" Inſula Ceilon & alibi ex ſemine Cardamomi fit, fe-
" re totum in Camphoram mutatur, quod idem verum
" eſt de pluribus Oleis ibi nalis." At vero ante ſex vel quatuor dies Olea varia in India deſtillata, qualia ſunt Oleum ex cortice Caryophill. & Calam. Aromat. & radicibus Caſſumun. & ſimilia perluſtravi ; atqui nullam in his materiam condenſatam ſive coagulatam reperire potui, quamvis pluſquam decem annis hic recondita fuiſſent. Neque dicere poſſum cum D. *Geoffroy* jun. (*Memoires de L'Academie Royale* 1721.) me unquam obſervaſſe Cryſtallos ex Ol. Terebinth. vel Majoranæ, parietibus vaſium, in quibus aſſervata fuerant, adhærentes, ſimiles Cryſtallis Camphoræ ; quamvis vaſa multos annos his uſibus applicata diligenter inſpexi.

Quod ad hoc Sal, ſeu Oleum coagulatum Thymi attinet, idem aliis in hac urbe, in re chymica verſatis notum eſt ; nam hujus parvam quantitatem (niſi quod colore ſit candidiore) noſtro ſimilem vidi : at in nullis aliis Oleis aliquid hujus generis me vidiſſe memini, niſi forte mihi perſuadere liceat, ineſſe hoc tempore in ſuis principiis, (ſi ita dicam) in Oleo Macis, à nobis ab hinc jam quindecim menſibus deſtillato, cujus olei eliciendi hæc eſt narratio.

Satis magnam quantitatem Macis pro una operatione recepi, totoque die deſtillavi ; at minore olei quantitate elicita, quam expectabam, altero die, & etiam tertio, pro re nata, deſtillationem reiterare inſtitui : Quantum memini, quarto die, poſteaquam Oleum aquam

ſupernatans ſeparaveram, viſus eſt mihi alius liquor, cum aqua non permiſtus, vaſis ſeparatorii fundum petiiſſe; hoc etiam educto, inveni eſſe Oleum Macis, aqua ponderoſius: Ita ut ex eadem planta, eademque operatione, Oleum diſpari gravitate extraxerim. In ſuperiore parte hujus Olei, aliquid cryſtallinæ formæ hodie fluctuare videtur, attamen cujus generis ſit ſubſtantia, ſive Camphora, ſive non, tempus docebit.

P. S.

Poſtquam differentias inter noſtrum Oleum Thymi coagulatum & Camphoram orientalem ſupra deſcripſeram, Dominus *Johannes Manel*, Chymicus, oſtendit mihi Oleum Majoranæ coagulatum, quod nuper, & inopinato in vaſe, (in quo Oleum Majoranæ quinque annos aſſervatum fuerat) invenit.

Hoc Oleum coagulatum Majoranæ etiam eadem experimenta, cum Oleo noſtro Thymi coagulato, ſubire volui, quod & factum eſt, & multum inter ſe convenire reperiebam, ita ut hoc nihilo magis, quam noſtrum, nomen Camphoræ mereri, ſatis ſciam.

III. *Two Letters on the Effects of* Lightning, *from the Reverend Mr.* Joſ. Waſſe, *Rector of* Aynho *in* Northamptonſhire, *to Dr.* Mead.

WE are told by Mr. *Jeſſop*, in the Tranſactions, that what the common People call *Fairy Circles*, are occaſioned by Lightning; but I think it has not yet been obſerved, that they continue viſible 50 Years, and that no Compoſition of Uſe in Fire-works will produce near ſo laſting an Effect, as I have experienc'd. There ſeems to be ſomething here, which

Sulphur

Sulphur and Nitre will hardly account for. Does it depend upon the great Quantity of the Matter diſcharged, or the Violence with which it is impell'd? The Ground is no way tore up, and the Graſs is only a little blaſted; which would make one think its Force well nigh ſpent: Whereas, when the Burſt is near us, the Effect is like that of a Petard, as appears from the following Inſtance.

On *Saturday July* 3, at *Mixbury*, three Miles Eaſt of this Place, about two in the Afternoon, *William Hall*, aged above Sixty, was found dead in a hard gravelly Field, together with five Sheep, which lay round him about 30 Yards Diſtance: Of the five, that only, which lay neareſt him, had a viſible Wound through the Head. The Shepherd lay partly upon his Side; the upper Part of his Head was terribly fractur'd, and his right Knee was out of joint: He had a Wound in the Sole of his Foot, towards the Heel; his right Ear was cut off, and beaten into his Skull, and Blood flow'd out of that Part upon the Ground. He is ſuppoſed to have been driving thoſe Sheep. All his Cloaths and Shirt were torn into ſmall Pieces, and hung about him; but from the Girdle downwards were carried away intirely, and ſcattered up and down the Field: Particularly, the Soles of a new ſtrong Pair of Shoes were rent off. His Hat was driven to Pieces: I have a Hand breadth of it full of irregular Slits, and, in ſome few Places, cut as with a very ſharp Pen-knife, and a little ſinged in the upper Part. His Beard, and the Hair of his Head were, for the moſt Part, cloſe burnt off. The Iron Buckle of his Belt was thrown 40 Yards off, and a Knife in the right Side Pocket of his Breeches was broken in Pieces, not melted, and the Haft ſplit. Near each Foot appear two round Holes about a Yard

deep, and five Inches Diameter, which ſhews the Force of the Blow. I have ſeen an Iron Ball ſhot out of a Mortar almoſt perpendicular, which, upon a like gravelly Soil, made not a greater Impreſſion. About the Time this Accident happen'd, a Tradeſman of the ſame Town obſerved a Sort of Fire-ball, as large as a Man's Head, to burſt in four Pieces near the Church. The Storm began here at 1^h 30′, and laſted, with Interminſſions, to 2^h 30′, and we ſaw the Lightning towards *Ailesbury* all the Evening. Two Perſons at *Aynho* were a little hurt at the ſame Time, and one of them ſtruck down to the Ground, and ſays, he thought he was felled with a Beetle. I myſelf heard the Hiſs of a Ball of Fire, almoſt as big as the Moon, which flew over my Garden, from S. *E.* to *N. W.*

I Thought I had been impertinently circumſtantial in the Account of the late Storm; but there ſtill remains a Particular or two to complete it. I order'd my Nephew, a Student of *Merton*, a pretty good Philoſopher, to ſearch the Holes made by the Blaſt, which the Owner of the Ground ſaid, his Conſcience permitted him not to do. He prevailed upon him, at laſt, with Money, Ale, and other *rural* Arguments, to trace the Path-way of it. Both the Holes, at firſt, were almoſt perpendicular for half a Yard, and after that grew narrower; in both of them, the Matter divided into two Parts, and form'd horizontal Cavities about three Inches Diameter. In one he found a very hard glazed Stone, of about 10 Inches long, 6 wide, and 4 in Thickneſs, crack'd in two: Others it could not pierce, but was turned here and there out of its Courſe, but left not the leaſt Blackneſs, or other Diſcolouring any where. As to the Knife, it was not the Blade, but the Haft, and the Hinge

Hinge that goes into it, which was ſhiver'd in Pieces. Near the Sheep that was wounded, the Ground was tore up near two Yards round. It was very ſurprizing, that the Man's Body was not beat to Pieces, or Bones broke at leaſt. Had *Statius* obſerv'd a human Tabernacle withſtanding a Shock ſo terrible, when the very Stones, lodg'd in the Ground, gave Way, he would, no doubt, have put him upon a Level with *Capaneus*;

—— —— —— toto Jove fulmen adactum
Corripuit: primæ fugere in nubila criſtæ
Et clypei niger umbo cadit; jamq; omnia lucent
Membra viri.

Exuiturq; animus; paulum ſi tardius artus
Ceſſiſſent, potuit fulmen meruiſſe ſecundum.

To make a groſs Eſtimate of the Force, I took a *Cohorn* charg'd with three Quarters of a Pound of very good Powder, wadded with thick Paper, and fired it againſt a Stone of the ſame Dimenſions, but not ſo hard, which it ſhatter'd to Pieces at half an Inch Diſtance: But, in the other Blow, we have above treble the Effect, without any diſcoverable Particles at all; and yet it ſeems to fly like ſmall Shot; pierces only here and there, and leaves a good many Places quite untouch'd, as is evident from the Hat which I have by me. To confirm this, *James Marſhal* of this Town aſſures me, that in the Middle of the ſame Storm, he received a Blow upon his Hat, which rattled like Shot through the Branches of a Tree: It beat in the Crown a little without penetrating it: He ſtagger'd and was giddy for two Days afterward. Two of his Sons were, at the ſame Inſtant, both knock'd down to the Ground, and ſtunn'd a little, but preſently came to themſelves, and have no Wound:

They are about 20, and 23 Years old. *Qu.* whether this may not be accounted for, by ſuppoſing the Flame to rarify the Air, and make a Sort of *Vacuum* about one; into which when it returns again, it gives the Likeneſs of a Stroke with a Beetle, as he expreſſes it. I fancy a Wind-Gun, with compreſs'd Air, wou'd have the ſame Effect, and might eaſily be try'd upon a Dog, or ſuch like Animal.

IV. *De Viribus Magneticis. Epiſtola Viri Doctiſſimi* P. Muſchenbroek, *M. D. ad Rev^m.* J. Th. Deſaguliers, *LL. D. R. S. S.*

QUum motus, quos vocamus Attractiones, frequentiſſimi videantur in Natura, & plurimi effectus ab hiſce pendeant, ad eos animum advertere cæpi, ut intelligerem quales eſſent, quænam eorum cauſa, & quibuſnam proportionibus operarentur: cum vero in magnetibus videantur hi effectus fortiſſimi, ſuſpicari cæpi an ex iis non aliquantum plus lucis fænerarer, quam ex aliis corporibus; & an, quod in majori gradu obtinet inter magnetes, non obtineret in minori forſan inter quælibet alia corpora; ad experimenta cum magnetibus igitur veni, plura diverſa inſtitui, quæ infra deſcribam, ſed poſt plurimos labores videor illud tantum colligere poſſe; magnetes non agere in ſe, aut in ferrum, per effluvia quædam corporea, ſed cauſam eorum eſſe penitus ignotam, & forte non corpoream. Conjecturis non detinebo te, nec pretioſiſſimum, quod tibi ſupereſt à gravioribus negotiis, furripiam hiſce vanis cogitationibus tempus; credo enim nihil eſſe magis noxium promovendæ ſcientiæ, quam ſi conjecturas pro demonſtrationibus obtrudamus.

Primum

Primum igitur volui experiri an magnetes in se operarentur juxta certam proportionem in diversis distantiis, vidique in Actis Britannicis, N°. 335. *pag.* 506. simile venisse in mentem experiuntissimo *Hauksbejo*, sed ipsum instituisse experimenta cum magnete & acu modo tali, qui omnibus non satisfaceret accuratis rerum scrutatoribus, unde concludit tamen his verbis. *I see no Reason to doubt, but the Proportions of this Power will be regular, and agreeable to the several Distances.* Quæ verba non adeo placuisse omnibus Eruditis colligo, cum Nobilissimus *Taylor* eadem experimenta repetiit, N°. 344. *pag.* 294. & alia reliquit observata.

Rem eandem aggressus fui methodo prorsus diversa, sic meditatus, si sumerem duos magnetes, & unum Suspenderem ex filo supra alium, ad diversas distantias à se invicem, sique fili extremum annecterem bilanci, me ponderare posse quantitatem virium, quibus magnetes in se agebant; neque successu caruit meditatio. Sumpsi accuratissimam bilancem, qua melior nullibi forte exstitit, & uni brachio annexui lancem, alteri filum longissimum plurimorum pedum, cujus parti inferiori adhærebat magnes nudus; filum longissimum feci, ne actio magnetis ulla in ferream libram turbaret experimentum; ideoq; selegi locum in quo ferri tam parum, ac in ædibus unquam seligi potest. Sumpsi præstantissimos magnetes perfecte sphæricos, terrellas vester *Gilbertus* vocavit, horum poli erant accurate in extremo utroq; axeos sphæræ, ita accuratissime distantias amborum polorum mensurare poteram. Gravitatem magnetis primo reduxi ope ponderis in altera lance in æquilibrium; dein ambos magnetes posui infra se, & quia libra erat, ope funis, mobilis supra trochleam, eam demittebam ad diversas distantias pro lubitu, & cum magnes suspensus ageretur

deorſum attractus vi magnetis inferioris, ſemper imponebam tantundem ponderis alteri lanci, donec vis magnetis cum pondere faceret æquilibrium: hæ tamen diſtantiæ menſurari non poſſunt, niſi interponendo corpus cupreum tantæ longitudinis ac eſt diſtantia inter ambos magnetes, ob oſcillationes libræ, & quia in majoribus diſtantiis magnetes minus operantur, quam in minoribus diſtantiis, æquilibrium libræ obtineri non poteſt, niſi eo artificio. Ecce nunc tabulam continentem experimenta in diverſis diſtantiis pollicum & linearum, columna remotior continet grana quæ æquiponderant cum attractionibus in iis diſtantiis.

Diſtantia Poll.	Lin.	Grana attractionis.		Lin.	Gran.
13 —	6 —	0		8 —	106.
12 —	0 —	0 $\frac{1}{20}$.		7 —	114.
11 —	0 —	0 $\frac{1}{8}$.		6 —	131.
10 —	0 —	0 $\frac{1}{4}$.		5 —	146.
9 —	0 —	0 $\frac{1}{2}$.		4 —	172.
7 —	6 —	1 $\frac{1}{2}$.		3 —	190.
7 —	0 —	2 $\frac{1}{2}$.		2 —	215.
	12 —	70 $\frac{1}{2}$.		1 —	250.
	11 —	78 $\frac{1}{2}$.		$\frac{1}{2}$ —	290.
	10 —	87.	In ipſo contactu, ſive	0 —	340.
	9 —	94.			

Pollices ſumſi Rhenolandicos, & grana ſunt pondera noſtra medica, quæ etiam ſumuia acribeia prius examinavi, ut eſſent vera & æque gravia.

Hæc experimenta inſtitueram die 24 *Decemb.* 1724. & animo adeo ad omnia attento ne hallucinarer, ut vix ſperaverim melius fieri poſſe.

Sed

Sed an ex his colligere poſſumus ullo modo dari proportionem inter vires & diſtantias? ego non video; tu vero vir, qui oculatiſſimus es, & cui tantum confiderem in judicio ferendo de rebus phyſicis, imprimis de experimentis, quantum ulli mortalium, perpende an quidem aliquid regularis adſit, excute omnes proportiones, invenies nullas; evolve curvas cognitas, nec felicior eruendo eris.

Poſtquam eo uſque perrexeram, ſuſpicabar an non forte ſuſpenſus magnes eſſet heterogeneus utcunque, & an alius ejus ſubſtitutus loco, eventum quoq; non daret magis proſperum, ſaltem ex quo plus lucis caperem, tædioſa enim nimis fuerant hæc experimenta quam ut inde tam parum emolumenti colligerem; ſed ecce quid cum alio magnete parvo, admodum præſtanti, obſervare datum fuit, dum magnes rotundus alius inferior idem maneret, firmiter in menſa poſitus: eodem autem modo experimenta inſtituta fuerunt.

Diſtantia Poll.	Lin.	Grana attractionis.	Lin.	Gran.
5 —	10 —	1 ¼.	7 —	33.
4 —	6 —	2 ¼.	6 —	38 ½.
3 —	9 —	3.	5 —	43 ½.
2 —	4 —	9.	4 —	50 ½.
1 —	9 —	12.	3 —	62.
1 —	0 —	23.	2 —	79.
	11 —	23 ½.	1 —	140.
	10 —	26 ¼.	½ —	186.
	9 —	29.	0 —	340.
	8 —	30 ¾.		

Sed irregularitates hic iterum adſunt maximæ, ex quibus concludi poteſt nihil: id ſolum eſt mirandum, quod dum magnes pro hoc ſecundo experimento fuerat minor,

minor, quam qui primo inſerviverat, tamen in mutuo contactu viribus æqualibus attrahebatur, nempe 340 granorum; dum in aliis diſtantiis longe minus tamen attracta fuit, uti ex comparatis ambabus tabulis patet: ſed præterea hic minor magnes ſecundi experimenti multo generoſior fuit & præſtantior ad elevandum ferrum, quam magnes primi experimenti.

Hæc experimenta repetii cum aliis magnetibus & imprimis cum aliquo, cujus tanta vis ut acum magneticam inflectat, quæ diſtat ab ipſo 14 pedibus Rhenolandicis; neſcio an ſimilis deſcriptus ullibi habeatur: ſed ex omnibus id modo concludere poſſum experimentis, proportionem inter vires & diſtantias dari nullam.

Quum tam declinatio, quam inclinatio acus magneticæ variat ſingulis annis fere, ſubiit quoque deſiderium videndi, an vis magnetis omni die eſſet eadem, an minor vel major æſtate quam hyeme; ſed vim eſſe minorem æſtate quam hyeme, me docuerunt experimenta multa, ſaltem de hac æſtate loquor, an futuro anno idem obtinebit, explorandum erit.

Sumſi igitur magnetes binos, qui primo experimento inſervierant, & eodem prorſus modo inſtitui experimenta cum iis, ac ante, dies vero fuit 11 *Julii* 1725. cum Baroſcopium eſſet elevatum ad 29 $\frac{5}{24}$ pollic. Thermoſcopium *Fahrenheytii* ad 62 grad. & Ventus *Noordten Weſten* ſive Septentrionalis verſus Occaſum, cælum ſiccum, ſerenum, & in eodem loco mearum ædium.

Diſtantia Poll.	Lin.	Grana æqualia attractioni.	Lin.	Gran.
12 —	0 —	0	7 —	106.
9 —	0 —	1 $\frac{1}{8}$.	6 —	111.
8 —	0 —	1 $\frac{1}{2}$.	5 —	132.
7 —	6 —	2.	4 —	149.
7 —	0 —	2 $\frac{1}{2}$.	3 —	173.
	12 —	70 $\frac{1}{2}$.	2 —	205.
	11 —	75 $\frac{1}{2}$.	1 —	240.
	10 —	85.	$\frac{1}{2}$ —	270.
	9 —	92.	0 —	300.
	8 —	100.		

Conſtat quidem inter Philoſophos, magnetis utriuſque polos non agere æque fortiter, ſed polos boreales eſſe fortiores viribus quam auſtrales, ſed hoc aſſertum quidem, demonſtratum accurate fuit nullibi; quia vero noſtra methodus ponderandi vires magnetis ſatis facilis erat, at accurate ejus ope hoc determinari poſſe videbam, converti modo ambos polos amborum magnetum ita, ut iterum duo poli amici ſibi eſſent obverſi, & in magnetibus hujus ultimi experimenti hæc obſervavi.

Ad diſtant. Lin.	Grana æqual. attractioni.	Lin.	Gran.
12 —	57.	5 —	101.
11 —	63.	4 —	113.
10 —	66.	3 —	124.
9 —	70.	2 —	148.
8 —	79.	1 —	168.
7 —	83.	0 —	228.
6 —	90.		

Ex his patet manifeſto non ambos polos magnetis agere viribus iiſdem, quanta autem intercedat differentia, ex comparatione ambarum tabularum videre poteris: brevis eſſe cogor, qui tibi epiſtolam, non tractatum tranſmitto, & jam ante verebar, ne prolixo ſermone tædium creaverim.

Quum ab ineunte ætate, qua primum operam Philoſophiæ dare inceperam, in hunc uſque annum mihi perſuaſeram actionem magnetum ab effluviis, vel aliquo ſaltem fluido pellente extus magnetem, pendere; neque videram præſtantiſſimos viros aliter ſenſiſſe; experiri volui an ullo experimento hanc meam opinionem confirmare poſſem, affirmare enim effluvia vel aliquod fluidum premens extus, & non demonſtrare, mihi viſum fuit nimis temere conjecturis dare operam. Dum igitur experimenta priora cum magnetibus inſtituebam ad varias diſtantias à ſe mutuo, interpoſui fruſta craſſiſſima plumbi, ſtanni, argenti, cupri, mercurii maſſam inſignem, viſurus an hæc effluvia magnetica non impedirentur, & ſi non omnino, ſaltem aliquo modo; vitrum pellucidum eſt, lucem tranſmittit, tamen non adeo copioſam, quam ſi nullum vitrum adfuiſſet; eodem credidi modo effluvia magnetica, ſi non prohiberentur omnino, ſaltem impediri aliquo modo ne magnetes tam fortiter ad ſe traherent, ſi plumbi fruſtrum 1 pedis cubici interpoſitum foret, vel ſi plumbum craſſitiei 2 digitorum, & ſtannum ejuſdem craſſitiei, tum cuprum, tum maſſam magnam mercurii interponerem; ſed vidi, quæcunque interpoſueram corpora, ſemper vires magneticas eſſe eaſdem, ac ſi nullum corpus interlocaretur; hoc profecto mirandum exiſtimo, neque intellectum credo ab ullo mortalium: non enim fingere licet hæc corpora eſſe adeo poroſa ut nihil ſolidi in ſe habeant; quod ſi igitur ſolidum habent, ut habent plurimum, an hæ

partes non impedient quominus fluidum extraneum adveniat, vel ex magnete exeat; non dico quod omne fluidum impedient, ſed ſaltem aliquid, experimenta tamen omnia docent, vires magneticas impediri nullo modo: vel an hæc effluvia erunt multo ſubtiliora luce? præterquam quod hæc eſſet iterum hypotheſis, difficultas ſuperior non tollitur; Ignis impeditur à corporibus, lux non penetrat illico per omnia corpora, & ita ſe habent fluida omnia ut à ſolidis reſiſtentiam experiantur, ſed effluvia magnetica ita ſe non habebunt, immunia erunt reſiſtentiæ à ſolido corporeo; hoc eſt, ad quod mens noſtra plane hebeſcit.

Sed argumentum fortiſſimum ex viribus repellentibus magnetum depromam, ſunt hæ longe debiliores, quam vires attrahentes, uti mox experimentis confirmabo, adeoque oportebit ut fluidum accedat ab exterioribus verſus magnetem, quod dum occurrit alteri magneti, unum pellit ad aliud, quodque magnetem ingreditur, & quia attractio magnetum eſt longe fortior quam repulſio, copioſius fluidum ingredietur magnetem, quam egreditur: unde fieri non poteſt quin brevi magnes adimpleatur hoc fluido, ut non amplius poroſus maneat; nec ſtatui poteſt quaſi undiquaque ex magnete fluidum hoc exiret, nam fit attractio in omni puncto magnetis, & fit tantum repulſio in locis polorum. Ut vero demonſtrem repulſionem magnetum eſſe minorem attractione eorundem, ecce tabulam continentem experimenta cum memoratis ultimis magnetibus facta.

Distantia Poll.	Lin.	Grana æqualia repulsioni.		Distantia Poll.	Lin.	Grana æqualia repulsioni.
13 —	0 —	0.		1 —	11 —	16.
11 —	11 —	$\frac{1}{2}$.		1 —	10 —	17.
10 —	9 —	$\frac{3}{4}$.		1 —	4 —	17.
9 —	9 —	1.		1 —	0 —	24.
9 —	0 —	1.			10 —	24.
8 —	0 —	$1\frac{1}{4}$.			7 —	25.
7 —	0 —	$1\frac{3}{4}$.			6 —	$25\frac{1}{2}$.
6 —	1 —	2.			5 —	$27\frac{1}{2}$.
5 —	1 —	$3\frac{1}{2}$.			4 —	29.
4 —	0 —	$6\frac{1}{2}$.			1 —	34.
2 —	9	$11\frac{1}{2}$.	In ipso contactu.		0 —	44.
2 —	3	13.				

Ex his experimentis circa repulsum magnetum iterum videbis non posse deduci ullam proportionem, sed profecto magnetes esse corpora admodum miranda, de quibus huc usque pauca scimus, ignoramus plurima. Longe alia diversaque institui cum his corporibus tentamina, sed quæ nimis prolixa forent, quam ut nunc adderem.

Trajecti ad *Rhenum*,
20 *Julii* 1725.

V. *A ſhort Account of the* Anomalous Epidemic Small-Pox, *beginning at* Plymouth *in* Auguſt 1724, *and continuing to the Month of* June 1725. *By the Learned and Ingenious Dr.* Huxham, *Phyſician at* Plymouth.

THE *Small-Pox* were preceded by the uſual Symptoms of that Diſtemper; but the Pains of the Limbs, and Back, were generally more ſevere than common, as were likewiſe the *Nauſea* and Vomiting. Abundance were ſeized with violent Colick Pains, which would leave them upon the Eruption, or after a Clyſter or two, with a gentle Anodyne: The Stools were commonly bilious.

It ſometimes happen'd, that the Symptoms would not ſeem very ſevere before, and at the Eruption; and yet the Pox would prove very confluent and fatal at the State.

The *Puſtules* were very ſmall, and did not regularly fill; but, in a Day or two after the Eruption, would flat and be depreſs'd in the Middle. I obſerv'd this even in the diſtinct Kind. In ſome Perſons they appeared in leſs than 24 Hours from the Seizure: When they broke out ſo very ſoon, they were always of the Flux Kind, as is commonly obſerved. The Eruption was attended with prodigious Sneezing, eſpecially in Children. I ſaw one * Child about five Years old, that ſneezed inceſſantly for more than 30 Hours, nor could it be allay'd, but by Anodynes. This Child had the *Confluent Pox*, and dy'd the 13th Day. In ſome, at, and after the Appearance of the

* Mr. *Cockey's* Child.

Puſtules, they would itch moſt intolerably: This happen'd alſo to the Child now mentioned, and was generally a bad Symptom; as it was an Argument of the great Acrimony of the morbific Matter.

In ſome few, a Day or two after the Eruption ſeem'd to be compleated there would appear in the Interſtices of the Pox ſeveral miliary Puſtules, ſome of a dark Red, others filled with a limpid *Serum*: Theſe never came to Suppuration, as the ſecondary Crop of *Small-Pox*, which I have now and then obſerv'd, ſometimes do; nor were they as large. Tho' this is an ill Symptom in general, yet, in a * Girl of ſeven Years old, I perceiv'd her Fever and *Delirium* go totally off upon this Eruption, and the Urine immediately ſettled.

Some had abundance of Purple *Petechiæ* appear among the Pox at the Eruption, and the Puſtules would look of a lurid Hue: In others, the Purples would not diſcover themſelves, till the Maturation. I knew but † one Perſon that had theſe Spots, during this Conſtitution, that ſurviv'd the Diſtemper; but ſome dy'd the 5th or 6th Day, ſome dwindled on till the 10th or 11th.

During the Suppuration, the Pox would become very ſeſſile, and the coherent Kind would enlarge their Baſes exceedingly; ſo that, though they ſeem'd for ſome Time after the Eruption to be very diſtinct, they would now flux together. A Purple Speck would often appear in the Center of the Puſtules, which would ſpread and grow blacker and blacker by degrees. The Interſtices would alſo ſometimes turn pale, ſometimes livid. Symptoms of very ill Omen! The Puſtules, that had not the Purple Speck, did not incruſt

* Miſs *Spurrel*. † Mr. *Lang*'s Son.

incruſt yellow, but appear'd of a dead, aſh Colour, and by degrees grew into a dark black Cruſt.

The Salivation, which conſtantly ought to accompany the Maturation in the *Confluent Small-Pox*, was in ſeveral very inconſiderable, in ſome none at all, ſaving a very ſmall Quantity of extremely viſcid Matter, which was got off by ſyringing. I had two adult Perſons, and ſome Children, labouring under the confluent Sort, who neither ſalivated, nor purged, except when ſome lenient Catharticks were given them; and yet they got over the Diſtemper. Indeed, it was very rare, that we ſhould find Children have that gentle *Diarrhœa* which *Sydenham*, and others juſtly reckon, ſupplies the Salivation in Perſons of more advanced Age. Some very young Children, on the contrary, drivell'd exceedingly through the Courſe of the Diſtemper. In * two Children, one of five, the other of ſeven Years old, no Salivation came on till after the 13th Day, and then it was ſo profuſe, and continued ſo long, that it was with Difficulty I put a Stop to it by Purges firſt, and then by the Bark, Aſtringents, *&c.* To the younger of theſe, indeed, I had given *Calomel*, Gr. iv, but it was ſoon purged off.

Where the Swelling of the Face and Throat was very hard, painful, and tenſe, with a ſtrong Vibration of the carotid Arteries, and little or no Salivation, the Patients generally grew delirous at the State. Theſe Symptoms frequently prov'd fatal. The maxillary and parotid Glands, of thoſe that recover'd, would remain ſwoln and indurated for a conſiderable Time after the intire Deſquammation of the Pox, (though that was very ſlow) nor would theſe Tumors go off, but after repeated Purging, and that with *Calomel*, &c.

* Mr. *Wallis*'s little Boy, Mr. *Collier*'s Daughter.

Thoſe Tumors were undoubtedly the Conſequence of a very viſcid Matter obſtructing thoſe Glands, which harden'd the Swelling of the Face, hinder'd the Salivation, and in ſome Meaſure, the Circulation through the external Carotids; by which Means, more Blood being forc'd through the Internal, an Inflammation of the Brain, and a *Delirium* might be partly brought on; and this happening too, when the Blood was fraught with acrimonious Matter abſorb'd from the Puſtules, render'd the *Delirium*, at that *Stadium* of the Diſeaſe, vaſtly more to be dreaded than in the *Apparatus*, when it happen'd almoſt of courſe. Under theſe Circumſtances, Bleeding, emollient Clyſters, Eccoproticks, plentiful Dilution, were abſolutely neceſſary.

On this Occaſion it may be ask'd, whether or no, the Salivation being very viſcid and defective, the Tumor of the Face hard and tenſe, ſome Mercurial (as a duly prepar'd *Calomel*) might not be given with Advantage, even in the State of Maturation? (I have frequently given *Cinnabar* to good Purpoſe). There are ſome Inſtances that would ſeem to juſtify ſuch a Practice; and I know but one material Objection to it, and that is, that the Weight of the Mercury would, by encreaſing the Moment of Motion of the Blood, augment the Fever; but ſurely we have given *Calomel* after the Incruſtation, when the ſecondary Fever hath ſubſiſted, without any Manner of ill Conſequence, I might ſay, with great Succeſs.

Nothing ſo certainly fuſeth viſcous tough Humors, being join'd with plentiful diluting Liquors, as this, and ſo prepares them to be diſcharg'd by proper Outlets. As to *Oxymel Scillit.* Syringing, and the like, in a defective Salivation; the former, indeed, by Puking, ſometimes irritates the Glands of the *Membrana*

brana Schneideriana to discharge their Contents; Syringing barely deterges the Mouths of the *Ductus Salivares*: Either have little certain Effect further; whereas the viscous obstructing Matter is lodg'd in the inmost Glands, and even in the Blood itself.

This Method seems peculiarly adapted to such an *Epidemic Small-Pox*, as I am now describing, in which we had all the Indications imaginable of a very viscid State of Humors. The Blood, when drawn, was always excessively viscous, especially at the State of the Disease: Frequently there was little or no Salivation; generally it was extremely glutinous; so that the Nurses were many times obliged to pull the Matter out of the Patient's Mouth with their Fingers; and without drinking very plentifully, it would soon cease. A *Diarrhœa* very seldom happen'd to Children. The Blisters soon dry'd up. I heard of no one, during this Constitution, that made bloody Urine. Where that dreadful Symptom happens, the *Crasis* of the Blood seems to be dissolv'd, (as *Lyster* well observes); on the contrary, the recited Symptoms argued a too compact and viscous *Diathesis* of the Blood.

This State of the Humors, during this Constitution, might, in part, at least, depend on the extraordinary Driness of the Season, and the almost constant Northerly and Easterly Winds, which we had in the Months of *October*, *November*, *February* and *March* last. From the Middle of *January* to the Middle of *April*, was a drier Season than ever was known in this Country, where we have certainly, in general, more continu'd Rain than in most Places in *England*, *Plymouth* being infamous for wet Weather.

This

This remarkable Change of the Temperature of the Air muſt undoubtedly have ſome conſiderable Effect on human Bodies: A very cold Wind ſuffering only the thinner Part of the Blood to paſs of by Perſpiration: Nor, in ſuch Seaſons, doth the Body imbibe ſo much of a diluting Humidity from the Air (as *Keil* obſerves). Hence the Neceſſity of drinking plentifully of thin diluting Liquors, which, as it is always proper in this Diſtemper, ſo, when it happens in ſuch a Seaſon, is highly neceſſary. And I am of Opinion, Monſ. *Andry*'s Method of bathing in warm Water and Milk, or warm Milk, before the Eruption, may, upon many Accounts, be proper in ſuch a Conſtitution of the Air. There can be no Objection againſt it, but its not being in Faſhion.

I took particular Notice, that while, and juſt after the Eaſterly Winds blew exceſſively ſtrong for ſeven or eight Days together in the Months of *October* and *November*, the Patients, I then ſaw in the *Small-Pox*, ſcarce ſalivated at all. Then particularly, * an *adult* Perſon, who had the *confluent Pox* very ſeverely, did not ſpit the leaſt through the whole Courſe of the Diſeaſe; She was ſeized with a violent *Pleuriſy* the 18th Day, but was reliev'd by Bleeding. The Blood was the moſt viſcid that ever I ſaw. 'Tis remark'd by *Lanciſi*, that People expectorate very little in Diſorders of the Breaſt, when cold, dry, Eaſterly Winds blow; and 'tis what I have frequently obſerved: And this may be one Reaſon, why ſome *Aſthmaticks* generally ſuffer a Paroxyſm at ſuch Seaſons.

The Swelling of the Hands did not ſo regularly ſucceed the *Detumeſcence* of the Face, during this Conſtitution, as I have obſerv'd in other *Epidemic Small-Pox.*

* *Betty Boddy.*

Pox. Some had very ſmall, or rather no Tumors at all. 'Twas very rare the Legs and Feet ſwell'd, till after the Patients ſat up, and then they had much Pain in the Parts.

I have often reflected upon it, whether the Succeſſion of the Tumors of the Hands, to that of the Face, might not partly depend on the later Inflammation and Suppuration of the Puſtules of thoſe Parts: The Pain and Inflammation being a *Stimulus* determining the Humors to the pained Part: And 'tis particularly to be obſerv'd, that the greateſt Pain of the Hands and Arms commonly happens at the Time, when the Salivation begins to ceaſe: So that the Tumor of the Hands may, in ſome Meaſure, prove a *Succedaneum* to the Spitting. It is the common Obſervation, that the Puſtules of the Arms and Hands inflame and maturate a Day or two later than thoſe of the Face, and thoſe of the Legs and Feet lateſt, which may alſo be the Reaſon, that the Tumor of the Legs ſucceeds that of the Hands. I have been the rather inclin'd to this Opinion; inaſmuch as I have ſometimes obſerv'd a conſiderable Swelling of the Hands (the Puſtules being very painful and inflam'd) and that too in the diſtinct Kind, when there hath been little or none in the Face. Generally the more painful a Boil is, the greater the Tumor around it; and by conſequence, the Tumor of a Part is in proportion to the Painfulneſs of the Boils, and their Number.

From this, I would enforce the Uſe of Epiſpaſticks apply'd above the Wriſts, a little before the Time we expect the Tumor of the Hands ſhould ariſe, (eſpecially when Symptoms are threat'ning) as they are *ſtimuli* to be depended on, not only attenuating and deriving the Humors to the Parts, but alſo diſcharging them, and ſo proving a convenient Outlet to the morbifick

Matter, which before was thrown off by the (now partly ſuppreſs'd) Salivation.

Bliſters apply'd to the Neck frequently relieve the extreme Pain of the Throat, and Difficulty of ſwallowing which are ſometimes exceeding troubleſome to the Patient in the third *Stadium* of the *Small-Pox*, by drawing the Humors another Way. Nay, in ſome, where Veſicatories have been early apply'd, and continu'd to run extreamly, there hath been leſs Swelling, and leſs Salivation, than ſeem'd proportionate to the Vehemence of the Diſtemper, but without any Diſadvantage to the Patients; the Running of the Bliſter ſupplying the Defect of the Spitting. It ſeems then but reaſonable, when we expect the Tranſlation of the noxious Humor to the Hands, which is what Nature itſelf affects, to endeavour to promote its Flux thither, and give it Vent.

How advantageous Diſcharges of this Nature may be, I had Occaſion ſome Years ſince to obſerve in the Caſe of a Lady, for whom I was concern'd, together with that worthy and ingenious Phyſician Dr. *Seymour* of this Town; where, through the prodigious Diſcharge of Bliſters apply'd to her Neck, Ears, and Arms; as likewiſe a plentiful Flux of Urine, ſhe neither ſwell'd, nor ſalivated, through the whole Courſe of a very dangerous, *confluent Small Pox*, and yet recover'd.

Any Perſon, that hath been converſant in Practice, cannot but have obſerv'd Tranſlations of the morbid Matter from one Part to another ſometimes of the greateſt Service, eſpecially where it hath had a Diſcharge. (Indeed, all critical Evacuations are of this Nature). But I mean, how often hath a Boil, an Impoſthume, or Swelling of the Limbs, been the evident Means of terminating a Fever? This I experienc'd particularly

particularly in myſelf, ſeveral Years ſince at *Paris*; when labouring under a violent, inflammatory Fever with *Delirium*, the 9th Day towards Night, I was ſeiz'd with exceſſive Pain in my Arms and Hands, upon which I bath'd my Hands a long Time in warm Water, by Perſuaſion of two worthy Gentlemen of the Faculty, now living, who were then my Fellow Students, and watched by me. In a little Time my Hands began to ſwell, and in 4 or 5 Hours my *Delirium* and Fever went off intirely, tho' my Hands remain'd ſwoln and pain'd for ſome Time.

If Nature, therefore, in ſome Caſes, take ſuch extraordinary Methods to free herſelf from Diſeaſes, how intent ought we to be in promoting her Operations, in a Diſtemper, where the *Metaſtaſis* of the morbific Matter to the Hands and Feet is generally regular and ſalutary. 'Tis, undoubtedly, upon this View, that *Baglivi* orders Sponges ſoak'd in a warm emollient Decoction, to be apply'd to the Hands and Feet in the *Small-Pox*: And this, he ſaith, he hath done with great Succeſs. I have ſeen no leſs from Bliſters maturely apply'd to the Arms and Legs; but then I order'd the Patients to drink plentifully of a thin Whey, or the like, which takes off, in great Meaſure, the Acrimony of the *Cantharides*.

Before I conclude this Paragraph, I cannot but obſerve, that the *Delirium*, attending the Eruption of the *Small-Pox*, is very much alleviated by the Application of emollient Cataplaſms to the Feet, in Children eſpecially. 'Tis, indeed, what I do commonly of courſe apply, when I am conſulted at the Beginning of the Diſtemper, and I think I have had Reaſon to imagine, in many Caſes, that it hath been a Means of deriving the variolous Matter that Way; and, by making the Erup-

tions more copious in the lower Parts, the Face and Breaſt have ſuffer'd leſs than, I had Reaſon to fear, otherwiſe might have happen'd. The great Tenderneſs of the Feet, which happens after their Application, is a trifling Diſadvantage, in Compariſon of the Benefit may be receiv'd by them; and ſo are thoſe ſhooting Pains, which often affect the Legs on the Uſe of thoſe Cataplaſms: Not to ſay, that theſe are rather an Argument of the Benefit ariſing from their Uſe.

In the *confluent* Kind, generally a *Micturition* and *Dyſury* came on about the 12th, or 13th Day; and that when there were no Bilſters apply'd. If a large Quantity of turbid Urine follow'd, it was ſoon ſucceeded by Urine, which depoſed a very large Sediment; but if it prov'd thin and limpid, and in ſmall Quantity, a *Delirium*, *Tremor*, *Subſultus Tendinum*, and other convulſive Symptoms, ſoon follow'd.

There were no Symptoms ſo certainly fatal at the Turn of theſe *Small-Pox*, as a *Delirium*, and (what is, I think, of conſtant ill Omen in all Kinds of eruptive Fevers at the State) a *Dyſpnœa*, or the *Anheloſa Reſpiratio*: If we bled upon the firſt Appearance of them, we frequently ſav'd our Patient; the Omiſſion of which a few Hours made the Caſe irrecoverable.

It was very common in Perſons afflicted with theſe *Pox*, that 8, or 10 Puſtules would run together, and form a large Veſication full of a limpid, crude Matter, which would continue ſo ſeveral Days after the Incruſtation. * In one that dy'd, I ſaw Mortifications under theſe Bladders. I thought it neceſſary to let out this Matter with a Lancet, or Needle, as ſoon as poſſible, leſt it ſhould, (as it did when left to itſelf) cauſe an Ulceration;

* Miſs *Booſe*.

ceration; and I could not but believe it proper, inaſmuch as the Thinneſs of the Matter render'd it capable of being abſorb'd into the Maſs of Blood, and that the longer it lay, the more acrimonious it would grow, it not admitting of Concoction.

In two Patients, I ſaw ſeveral of the Puſtules fill'd with a bloody *Sanies*: I was ſurpriz'd to find * one of them get ſo eaſily over the Diſtemper, tho' ſhe labour'd alſo under the Flux Kind.

The Deſquammation was very ſlow, the black Cruſts adhering ſeveral Days, nay Weeks, after the Turn, while abundance of purulent Matter gleeted from under them. Theſe left very ugly *Cicatrices*. No Application ſeem'd to me to have a better Effect, in this Caſe, than frequently fomenting the Parts with warm Milk, or Milk and Water; this diluted the acrid Salts, waſh'd them off, and ſoften'd the Skin: Oily Liniments, by ſtopping the Pores, are frequently hurtful.

In a Caſe or two I obſerv'd a Repullulation of Puſtules under the Cruſts in the Face and Hands, when thrown off. This particularly in the Boy, that recover'd with Purple Spots. The latter were diſtinct, tho' the former were in the greateſt Degree confluent.

Nothing ſo certainly abated, and took off the ſecondary Fever after Bleeding, (if indicated) as gentle Cathartics; ſuch as *Rhubarb*, *Manna*, *Tartar*, *Infuſ. Senn.* and the like. The hot, ſcammoniate, aloetic Purgers ſeem not ſo proper, at leaſt, to begin with. Theſe I gave the 10th, 11th, 12th, or 13th Days, if I found the Patient have a quick Pulſe, feveriſh Heat, dry Tongue, Head-ach, reſtleſs Anxiety, and other Symptoms of the putrid Fever. Some one, or other of theſe, being once or twice repeated, I gave *Calomel*, aud purg'd it off. This was our general Method, and the moſt ſucceſsful. I can-

I cannot but think the World highly oblig'd to the moſt learned Dr. *Friend*, and the other noble Ornaments of our Faculty, that have introduc'd, and wrote in Favour of, this Method. In the Beginning of my Practice, relying upon the Authority of *Morton*, I gave the *Cortex* to check the ſecondary Fever; eſpecially when I found it (as is very frequent) evidently intermit; but I cannot ſay with a Succeſs any way anſwering my Expectation: Not but that after due Purging, the *Bark* is very proper to extinguiſh the hectical Diſpoſition of the Blood, which is frequently the Conſequence of the *Small-Pox*, to which, if a cool Regimen, and Aſſes Milk (where no *Idioſyncracy* forbids it) be ſubjoin'd, we have done, perhaps, as much as lies in the Power of Phyſick.

By this Method of early Purging, in the *coherent* and *confluent Pox*, I have ſeen ſeveral reſcued from the moſt imminent Danger, whoſe Recovery look'd rather like a Reſurrection.

This, ſurely, is the only Way of cleanſing the *Primæ Viæ* ſtuff'd with a Load of fœtid, acrid Impurities thrown off by the Glands of the Guts, which cannot be ſuppos'd to ceaſe from their Office, during the Courſe of this Diſtemper: And foraſmuch as the Pores of the Skin are at this Time very much conſtipated by the incruſted Puſtules, 'tis reaſonable to believe, the Glands of the Guts rather ſeparate more than uſual; it being an allow'd Maxim in Phyſick, That the Leſſening one Evacuation is the Encreaſe of another; eſpecially where there is ſuch a peculiar Conſent, as between the Skin and the Guts.

If ſo, the Excrement, being retain'd for a Week, or more, by its Weight preſſing on the great Artery, hinders the Blood from paſſing freely to the lower Parts, and ſo deluges the Brain. Hence thoſe *Deli-*

riums,

riums, *Coma*'s, *&c.* ſo frequently threatening at this *Stadium* of the Diſeaſe. Further, can we imagine, that the putrid Recrement of the now putrid Blood, join'd, perhaps, with the *Pus* of the internal *Pox*, and having alſo the Addition of ſome Part of the morbifick Matter ſeparated by the Glands of the *Fauces*, which is accidentally ſwallowed, muſt not be greatly hurtful, by remaining in the Inteſtines? where growing more and more acrimonious, (as is the Nature even of our moſt balſamic Juices, when they are *extra Aream Circulationis*, and expos'd to the conſtant Heat of the Body) it contaminates the Chyle, or Liquors, that are drunk, is re-abſorb'd into the Maſs of Blood, and becomes a *Pabulum* to the very Fever, which Nature endeavours, even this Way, partly, at leaſt, to throw off.

And, in very Deed, what horribly offenſive, fœtid, large Stools do we obſerve in this Diſtemper on the Uſe of Clyſters, and more eſpecially after a Purgative? I mean at the State, or in the Declination: So that this very putrid Matter lying long in the Guts, and growing more and more ſo, becomes at laſt ſo virulent, as to corrode them, and brings on that very *Diarrhœa*, or Dyſentery, which, 'tis ſo vainly fear'd, would ariſe from a gentle Cathartic; ſeeing we are always eaſily able to check its Force (if Need be) with an Opiate, *&c.*

How often do we find feveriſh, hectic Heats proceed from a *Cacochymy* of the firſt Paſſages, in Children eſpecially? In which Caſe, a little Rhubarb, or a few gentle ſtomachick Purgers, ſhall do more to remove the feveriſh Diſpoſition than a Pound of the *Cortex*. Indeed, it's common enough to find the *Bark* itſelf purge gently upon its firſt Adminiſtration: And I'm confident, in ſome Caſes, it hath the better Effect. Have

not we ſeen ſome Intermittents cur'd by one ſeaſonable Vomit? And that not barely by the Shock and Agitation given to the Blood-Veſſels, *Genus Nervoſum, &c.* and ſo acting as an Attenuant; but by throwing off the *Saburra* from the Stomach, which fed the feveriſh *Paroxyſms:* And this is more particularly evident in the Fever frequently ſupervening a Surfeit. Hence it is, that a ſmall Quantity of the *Bark* ſometimes does more after a Vomit, than a much greater cou'd before it. Why, therefore, when there is a Lodgment of putrid Matter in the *Primæ Viæ*, which, in part, at leaſt, feeds the ſecondary Fever, ſhould not we attempt to carry it off by either gentle Vomiting or Purging, as may be judg'd moſt convenient?

It hath, and may be objected to this Practice, that it tends to draw the noxious Humors from the Circumference to the Center; but to this it hath been anſwer'd, that the Purging is more eſpecially pleaded for, when the Incruſtation is begun, and the Matter too thick to be abſorb'd.

If Nature, neither by her own Effort, nor the Help of Art, is capable of keeping the morbific Matter from falling on the more vital Parts; but, by an unfortunate Tranſlation of it, is like to ſink under its Weight: As upon a ſudden Retroceſſion of the Tumor of the Face and Hands; a premature Suppreſſion of the Salivation, or the like: Doth it not ſeem neceſſary to endeavour to carry off the offending Matter by ſome other Outlet? As, in the preſent Caſe, by the Guts, which are much more eaſily ſolicited to a Diſcharge, than either the Pores of the Skin, the urinary Paſſes, or the ſalivary Ducts.

Indeed, when the Salivation of courſe ceaſes, (in my Opinion) it ſeems neceſſary to promote ſome other Evacuation in its Room. At the ſame Time, Cardiack, or

Alexipharmick Medicines are in no wiſe contraindicated by this Method (if judg'd needful). This Way Nature affects in Children, to whom a gentle *Diarrhœa* is commonly of the greateſt Service, as proving a happy Subſtitute to the Salivation in elder Perſons.

The following Hiſtory will evince, how neceſſary it may be ſometimes to evacuate an offending acrimonious Matter lodg'd in the Guts, and that too, even in the Midſt of the Suppuration.

About three Months ſince, I had under my Care a * Perſon about 30, ill of the *Small-Pox*. It happen'd, that the 4th Day from the Eruption, he was ſeiz'd with a violent bilious Colick, to which he had been formerly ſubject: This threw him into the utmoſt Agony. His Pox flatted and grew pale, as likewiſe the *Interſtices*: His Pulſe was extreme languid, and he had a prodigious *Tremor*, with clammy Sweats. I order'd him two Clyſters to be thrown up; one aſſoon as the other was render'd: Theſe gave him five large bilious Stools: After the third Stool, he was tolerably eaſy: However, I order'd him *Laudan. Solid.* Gr. 1 ß. *Croc. Anglic.* Gr. iv. *Theriac. Andromach* 3 ß. 4*tis*, *vel* 6*tis horis*, to be waſh'd down with a teſtaceous Julep. He took the *Laudan.* three Times, and ſlept ſound all Night. The next Morning, I found the Puſtules round, florid and turgid. The Man got over the Diſtemper, tho' he relapſed into his Colick ſome Days after the Turn, which upon purging with *Calomel*, *&c.* and the Uſe of *Opiates*, ſoon left him. This Perſon, before, and at the Eruption, complain'd of a great Difficulty of Breathing, with a ſhort importunate Cough, and a violent Pain under his *Sternum*; for which Reaſon I order'd ℥xvi of Blood to be drawn, which was very ſizy.

* *Tho. Hodge.*

The major Part of the *adult* Perſons, that had the Unhappineſs to be ſeiz'd with this Diſtemper, dy'd; among whom fell an old Gentlewoman of 72; a very uncommon Exit for a Perſon of her Years!

It was a remarkable Inſtance of the extraordinary Virulence of theſe *Small-Pox*, that the Women (tho' they had had the *Small-Pox* before, and ſome very ſeverely too) who conſtantly attended thoſe ill of the confluent Kind, whether Children, or grown Perſons, had generally ſeveral Puſtules broke out on their Face, Hands, and Breaſt, exactly reſembling the pocky Puſtules, which undoubtedly aroſe from the Matter of the cruſh'd Pox infecting the Skin in thoſe Parts. Thoſe Puſtules aroſe, maturated, and ſcabb'd off, intirely like the true Pox. † I knew one Woman, that had more than forty on one Side of her Face and Breaſt; the Child ſhe attended, frequently leaning on thoſe Parts on that Side. I obſerv'd, that thoſe, which had the tendereſt Skins, and who attended thoſe ill of the worſt Sort, had moſt of theſe Eruptions. We had abundance of Inſtances of this Nature.

In the Beginning of this Conſtitution, the *Small-Pox* were much more malignant than they have been for this Month, or two, laſt paſt. Indeed, when they raged moſt ſeverely, ſome Children had them very favourably, and requir'd no other Phyſick than to be duly purg'd after them.

† Mr. *Hicks*'s Nurſe.

F I N I S.

LONDON, Printed for W. and J. INNYS, Printers to the *Royal Society*, at the Weſt End of *St. Paul*'s *Church-Yard*.

Plate. II.

Fig. 3.

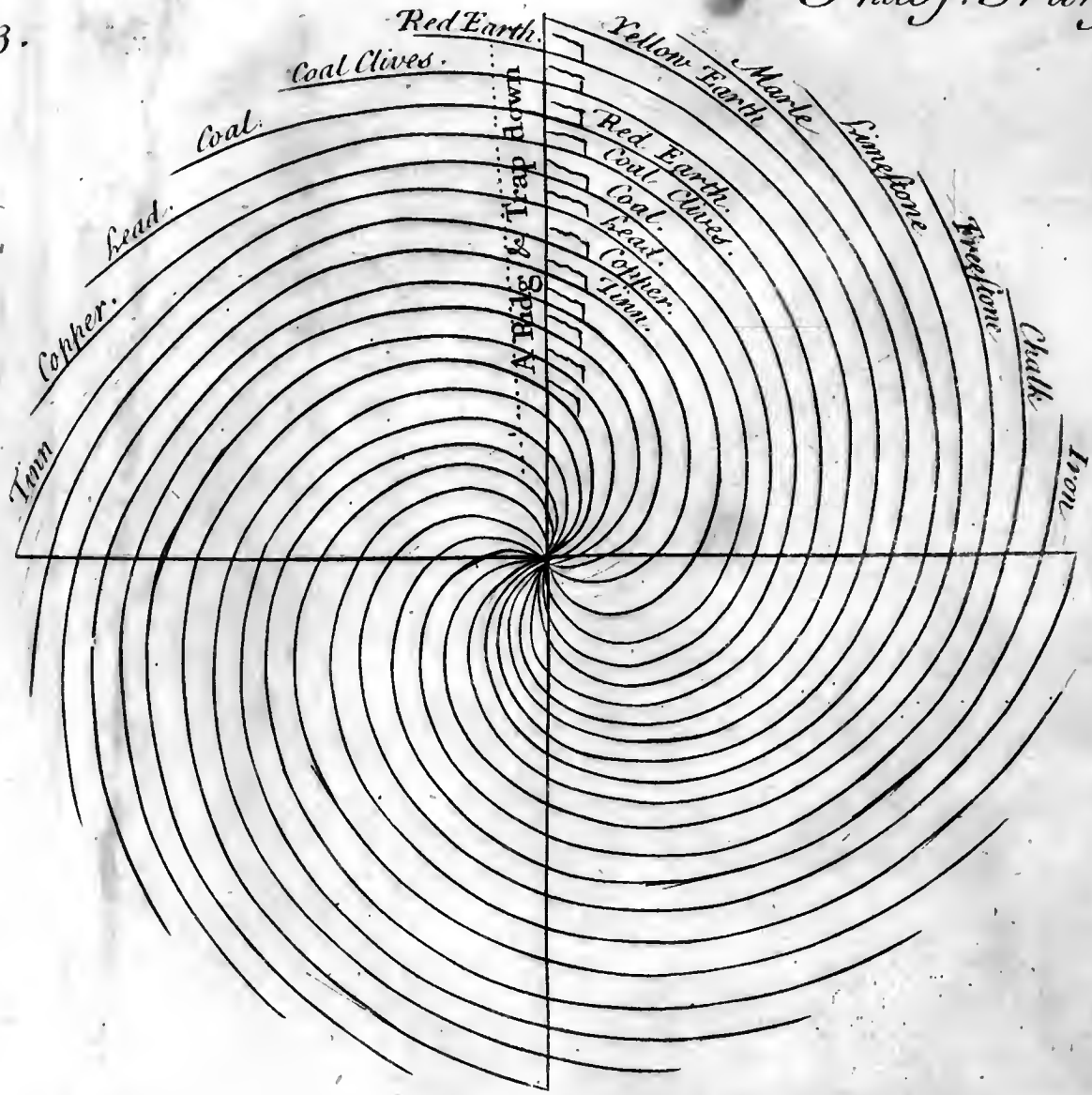

Fig. 4.

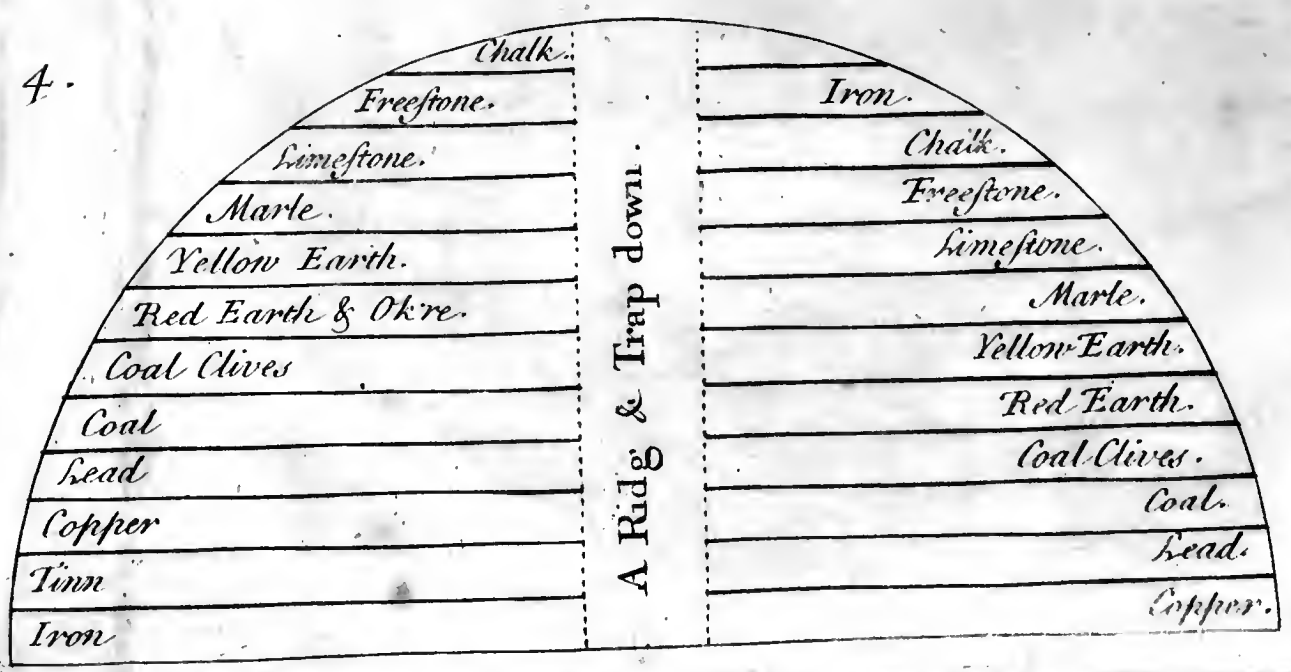

Fig. 6.

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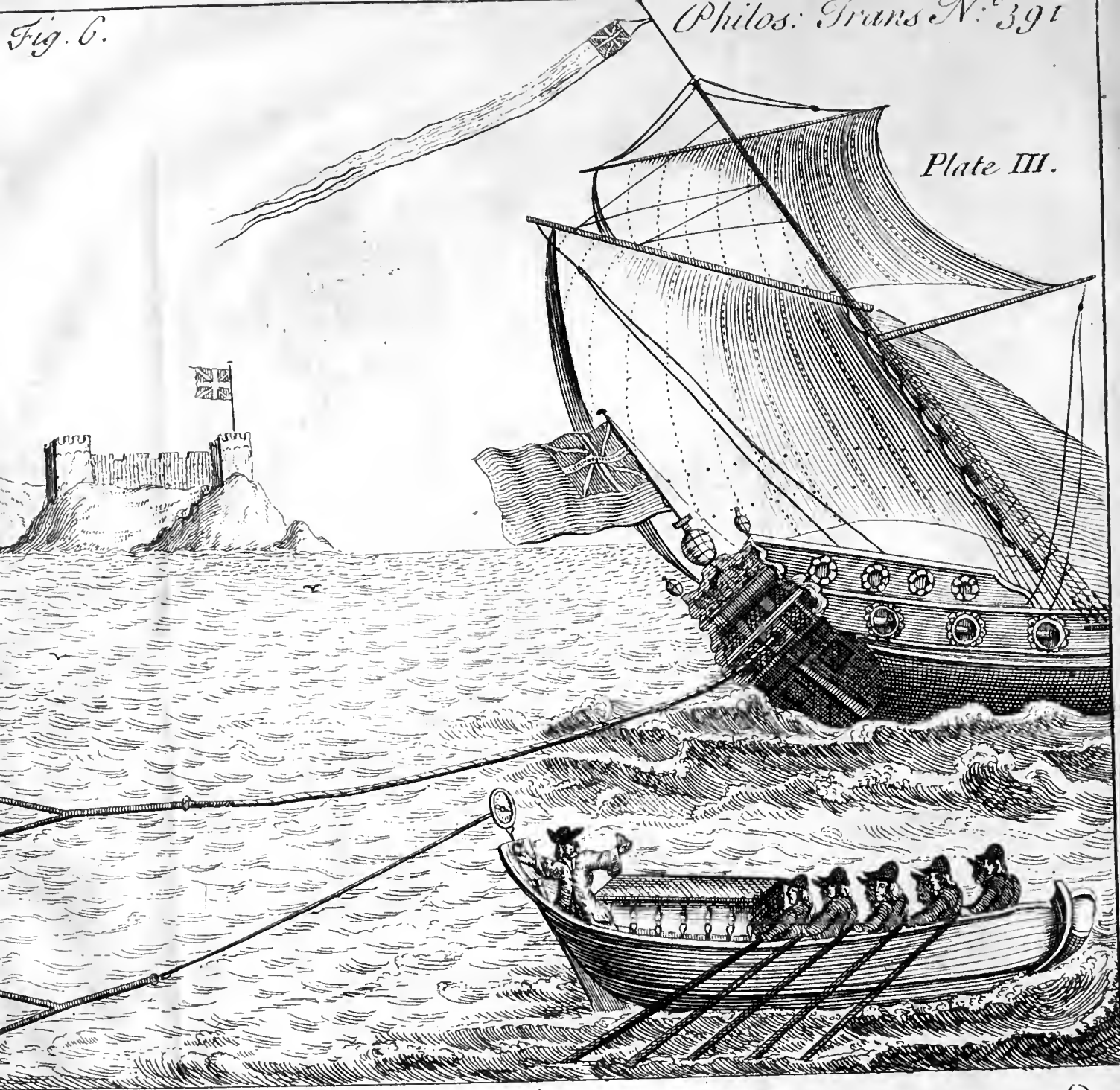
Plate III.



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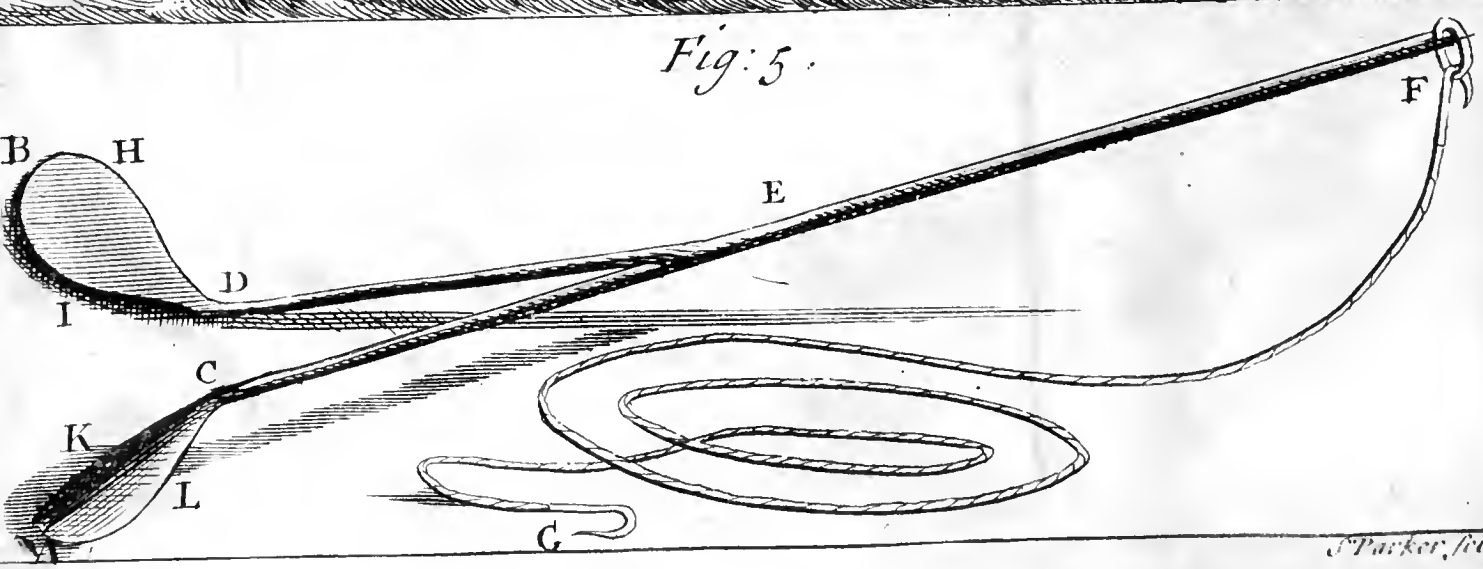

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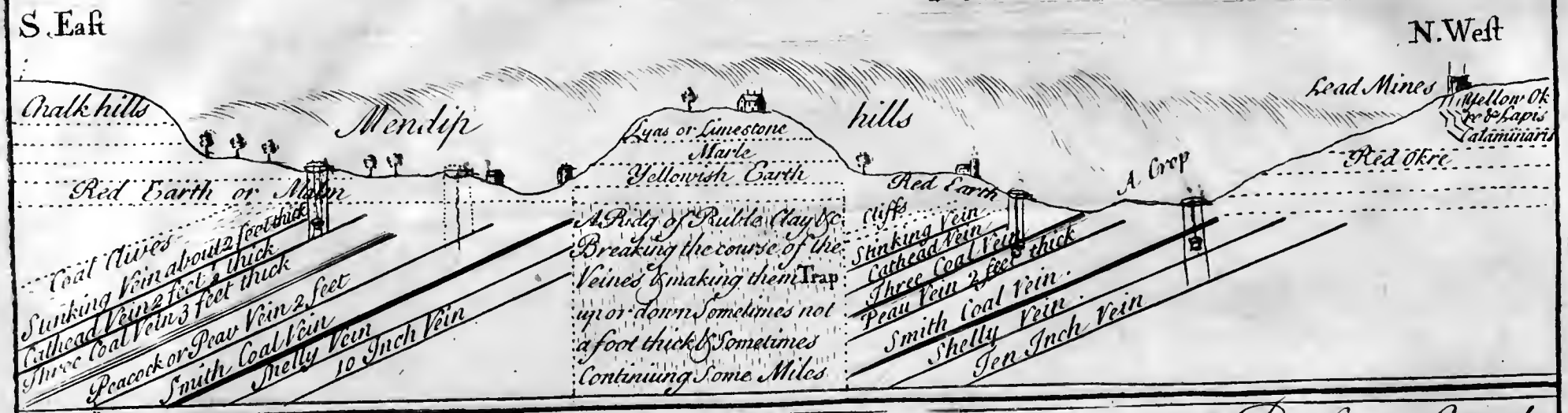
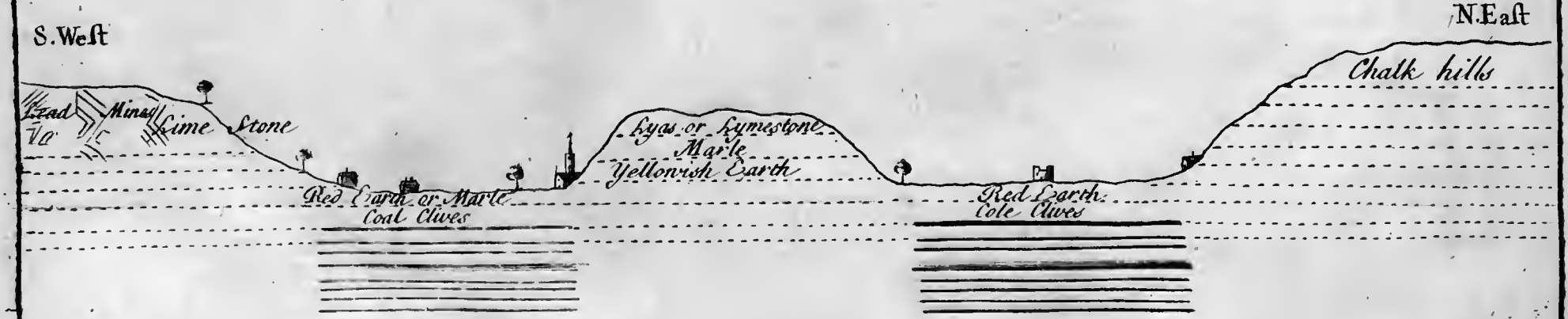
Fig: 1.

Plate I.



A Section of the same Country from North East to S. W. on y^e Drift or Level of the Coal & at right Angles with y^e former

Fig. 2



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PHILOSOPHICAL TRANSACTIONS.

For the Months of *November* and *December*, 1725.

The CONTENTS.

by the Log, *or any other Method hitherto uſed for that Purpoſe; together with ſeveral Teſtimonials, ſetting forth the Uſefulneſs of this Invention. By Mr.* Henry de Saumarez *of the Iſland of* Guernſey.

ERRATUM.

N° 390. *p.* 366. *l.* 12. *for* Manel *r.* Maud.

I. *An Account of the* Strata *in Coal-Mines*, &c. *By* John Strachey, *Esq*; *F. R. S.*

IT was some Time since, that in a Letter to one of the Members of this Society, I gave an Account of the several *Strata* of Earths and Minerals, found in some of the Coal-Works in *Somersetshire*, which was printed in *Phil. Transactions*, N° 360. But there is one great Error in the Print; for whereas I said, that in those Parts they never meet with *Freestone* over the Coal; the Printer, by mistake, calls it *Firestone*; whereas *Firestone* is always found in those Mines, contrary to the Works in *Staffordshire*, *Newcastle*, and *Scotland*, where *Freestone* does, indeed, lie over the Coal. I have farther observed the *Strata* of Stone, Clay, and Marle, of the interjacent Hills, where, under the black Marle, lies a spongy yellowish Earth; all this lies above the red Soil, which I have said is generally the Surface of the Vallies, where the Coal is found. And as this red Mould on the Surface degenerates into Marle or Loom, so, towards the North-West, beyond or without the Veins of Coal, about *Winford*, in the same County, it turns to Ruddle, or Red-Okre, used chiefly for marking of Sheep, and for ground Colours or Priming, instead of *Spanish* Brown; and often counterfeits Bole Armoniack.

But as I never heard any Coal was found to the West or South of *Mendip-hills*; so *Cotswold*, to the North-East, and the Chalk-Hills of *Marlborough-Downs* and *Salisbury Plains*, seem to set Bounds to the Coal Country, to the East and South-East of which *Fig* 1.

may be ſuppoſed a Section from South-Eaſt to North-Weſt, *viz.* from the Dip to the Riſe; and *Fig.* 2. at right Angles, from South-Weſt to North-Eaſt, on the Drift or Level.

I mention this by way of Correction and Addition to my former Obſervations of the Coal-Works in *Somerſetſhire*. I have ſince had Opportunities to be underground, and view ſeveral Coal-Works in *Scotland* and *Northumberland*, and to obſerve the ſeveral *Strata* there. At *Widdrington* they have four Fathom Clay, then a Seam of Coal, about ſix Inches thick, not worth working; then a white Freeſtone; then an hard Stone, which they call a Whin; then two Fathom of Clay; then a white ſoft Stone; and under that a Vein of Coal three Feet nine Inches thick. This is a ſmall Coal of the ſame Nature, but not ſo good as the *Newcaſtle-Coal* which comes to *London* Market. Theſe Veins dip to the South-Eaſt, one Yard in twenty. Near *Tranent*, in *Eaſt-Lothian* in *Scotland*, the Coal dips alſo to the South-Eaſt, in the ſame Proportion; but at *Baldoe*, in the Pariſh of *Campſy*, three Miles from *Kylſith*, it dips to the North-Eaſt; and at *Madeſtone*, near *Falkirk*, to the ſame Point, and in the ſame Proportion. The *Strata* of Earths and Minerals, at theſe Places, agree very near: They have, as the Ground riſes or falls, one, two, or three Fathom of Clay; then eleven Fathom of Slate, or Coal-Clives; one Fathom of Limeſtone; under that two Fathom of Slate, Earth and Stone; and then Coal. And all theſe agree in this; that the Pits generally need no Timber, and have a good Roof, which is ſupported by Pillars of Coal, which they leave in the working. At *Baldoe*, the Coal is commonly forty-five Inches thick; and all along, for ſome Miles Eaſtward thence, on the Sides of the Hills,

Hills, are Crops of Coal and Limeſtone; and oftentimes the Tenants ſpit up as much as will ſerve their Turn for a Winter's burning, juſt under the Surface; for there wants a Market, and it is ſcarce worth working for Sale. And to the North-Weſt and North, in the Drift of the Coal in higher Ground, and, conſequently, lying over it, there appear, in the Sides of the Hills, Seams of Spar and Lead, the Drift of whch is North-Eaſt, and lies almoſt perpendicular; but what Obliquity there is, pitches to the South-Eaſt. At *Auchenclaugh*, ſix Miles Eaſt from *Kylſith*, there is a Coal eighteen Feet thick; this dips one Foot in three, and is not purſued by reaſon of Water; and, for want of a Market, will not quit the Coſt of draining. At *Madeſtone*, the Coal is four Feet and a half thick, above three Fathom and a half deep: They land it (as at many Coalhews in the Country) on Girls Backs. Near *Tranent* are three different Veins wrought; the undermoſt is about eighteen Fathom from the Surface, call'd the *Splenty Coal*, four Feet and a half thick; it's a hard but not large Coal; makes a clear and ſtrong Fire; lies ten Fathom under the *main Coal*, which is nine or ten Feet thick, and comes out very large. Its Roof is of Freeſtone, under which I walked backward and forward two Hours; but had no Opportunity to make any other Obſervations on the upper Vein, than that it is about four Feet thick, and neither ſo hard or large as the other.

As I have, *Fig.* 1. and 2. drawn the different *Strata* (which have come to my Obſervation) on a ſuppoſed Plane, as they there lie; in *Fig.* 3. and 4. I protract the ſame in a globular Projection, ſuppoſing the Maſs of the Terraqueous Globe to conſiſt of the foregoing, or, perhaps, of ten thouſand other different Minerals, all originally, whilſt in a ſoft and fluid State, tending

towards the Center. It muſt mechanically, and almoſt neceſſarily follow, by the continual Revolution of the crude Maſs from Weſt to Eaſt, like the winding up of a Jack, or rolling up the Leaves of a Paper-Book, that every one of theſe *Strata*, tho' they each reach the Center, muſt, in ſome Place or other, appear to the Day; in which Caſe there needs no ſpecifick Gravitation to cauſe the lighteſt to be uppermoſt, *&c.* for every one in its Turn, in ſome Place of the Globe or other, will be uppermoſt; and, were it practicable to ſink to the Center of the Earth, all the *Strata*, that are, would be found in every Part, and according to the Poet, *Ponderibus librata ſuis.* Add to this, that in all Places within my Knowledge, the Obſervation of * another Member of the Society has held good, that the Precipices of all Hills are to the Weſtward, whereas the Aſcent to the Eaſt is more gradual. The farther Enquiry into which I offer to the Curious, who have better Opportunity.

II. *De Experimento probandi Spiritum Vini Gallici, perquam uſitato, ſed revera falſo et fallaci. Auctore* C. Neuman, *Chym. Prof. Reg.* Berolin. *R. S. S.*

SI qua rerum naturalium eſt ſcientia, è qua multæ erroneæ opiniones, præjudicia, ſpeculationes, & inde ſecutæ falſæ hypotheſes atque experimenta extiterunt, Chymia in primis eſt; quæ tamen in ſe conſiderata, non niſi certis, in externos ſenſus incurrentibus

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* Dr. *Stukely.*

rentibus, proinde facillimis, & tuto concludentibus rebus occupata eſt; quo magis mirandum eſt, quod quam plurimi ea de re conſcripti libelli pauciſſima vera in ſe contineant, multo minus neceſſariæ ad rem experientiæ reſpondeant. Hinc etiam aliquantulum modo rei expertus non moratur magnificos & ingenioſe excogitatos librorum titulos, apparationes Chymicas, vanaſque arcanorum oſtentationes, dum protinus, quam fallacibus & incertis opinionibus fundata ſint, perſpicit. Hos flexus Labyrintheos pervadere, eorumque varias aberrationes attingere, non eſt inſtituti noſtri; ſed impræſentiarum tantummodo de experimento quodam Chymico, fortaſſe jam multis abhinc annis frequentato, imaginario, & reapſe falſo ac fallaci, quod tamen quaſi peculiare aliquod arcanum & ingenioſum inventum, adhuc dum occultatur, ſententiam exponemus, eademque opera demonſtrabimus, quod qui tali experimento fidunt, manifeſto decipiantur, & quod ad id negotii requiſita præparatio Chymica nullum prorſus ſit myſterium, uti venditatur, ſed res rudem ſimplicitatem redolens, admodum vulgaris & perquam notiſſima.

Quantum ad rem propoſitam: Nonnulli negotiatores in *Hollandia*, *Anglia*, *Hamburgi*, *Dantiſci*, &c. qui maxime in coemendo & divendendo ſpiritu vini Gallici occupati ſunt, certum aliquod experimentum probatorium crepant, idq; pro magno artificio ac ſingulari arcano venditant, firmiter perſuaſi, ſe ope hujus experimenti non ſolum ſpiritum vini Gallici a ſpiritu frumenti, ſed etiam genuinum ab adulterato diſcernere, atque ita in coemendo genuino ſpiritu vini Gallici, nequaquam defraudari poſſe. Hinc etiam tanquam indubitatum & infallibile probamentum, imo magnum aliquod arcanum, quod non omnes calleant, adhuc exiſtimatur, adeo, ut nemo hunc liquorem probatorium, nec olfaciendum, neque guſtandum

ſtandum mihi concedere unquam voluerit, tametſi fide interpoſita proteſtabar, me illorum commercio nec nociturum, nec artem hanc cum aliquo communicaturum. Quid quod nec ego ipſe, nec illi prænoſcere quibant, utrum ego nudo olfactu vel guſtu conjectando illico aſſecuturus eſſem, quidnam rei ſit, & qua ratione paranda eſſet?

Subjectum liquor quidam eſt ſubfuſco-flaveſcens, quo adhibito mercatores experimentum iſtud ita peragunt. Complent primo ſcyphum vitreum ſpiritu vini Gallici probando, deque liquore iſto in ſcyphum illum vel unicam, vel duas treſve guttas, pro quantitate ſpiritus vini infuſi, inſtillant. Quod ſi bonus fuerit & genuinus ſpiritus vini Gallici, ſtatim in fundo vitri impleti exiſtit admodum cæruleus color pulcherrimus, qui ſi caule mundiore agitatur, cum reliquo ſpiritu permiſtus, totum vitrum colore cyaneo tingit; ſin autem ſpiritus frumenti ſit, ne quicquam cyanei coloris in vitro conſpicitur, ſed ſpiritus ille colorem ſuum primigenium retinet, etiamſi de liquore dicto vel vigecuplo plures guttæ ſcypho impleto inſtillentur. Sicuti igitur hic modus probandi generatim, putum frumenti ſpiritum a puto puro ſpiritu vini Gallici, ex opinione Mercatorum diſcernit: ita hi conſequenter hac hypotheſi fidentes de utriuſque ſpiritus vini & frumenti adulterinæ commiſcelæ gradibus diverſis judicant. Norunt enim ex conſpecto colore cæruleo, niſi hic obſcure talis, ſed ſubcæruleus modo, cæſius, glaucus, aut ex cyaneo virideſcens appareat, dijudicare, ſpiritum vini non ſolum adulteratum, ſed etiam quoad gradus tot pluſve minuſve partibus ſpiritus frumenti mixtum eſſe.

Equidem lubens fateor, hanc rationem probandi ſpiritus, ubi primum eam animadvertebam, admiratione quadam & delectatione me affeciſſe, ita ut eam omnino certam & falſi neſciam primo adſpectu crederem

derem, quo etiam magis nunc talibus Mercatoribus & Oenopolis, rerum Chymicarum ignaris, inſcrutabilem ſibi errorem condonare poſſum. Quum autem animo mecum ſubinde agitarem, quod nullum adhuc, quantum ſciam, demonſtrativum experimentum proſtat, cujus ope peculiares puri ſpiritus vini Gallici & quidem rectificatiſſimi partes conſtitutivas a ſpiritu frumenti æque rectificatiſſimo diſtinctas oſtendere vel ſaltem ſuſpicari poſſimus, ſed quod uterque ſpirituum ex iiſdem partibus eſſentialibus conſtet, in quam communem qualitatem ſimpliciter neceſſaria fermentatione rediguntur, ideo argumentatus quoque mecum ſum, quod ſi qua notabilis diverſitas in dictis ſpiritibus eveniret, ea non a mixto intrinſeco tanquam mixto ſpiritus vini formante, vel a vini indole, ex quo ſpiritus deſtillatus fuit, ſed neceſſario ab additamento quodam heterogeneo, ad conſtituendum ſpiritum vini, qua talem, nullo modo pertinente, ortum ducat, ſive ſub fermentatione aut deſtillatione peregrinum quid addatur, ſive tingendo, extrahendo, vel ipſa commiſcela aliorum liquidorum aut ſolubilium contingat.

Meum non eſt in ſolis ejuſmodi cogitatis, præcipitatis ut plurimum & fallacibus, acquieſcere; quare varia, ad rem pertinentia, experimenta inſtitui, quibus etiam facile impetravi conjecturæ meæ confirmationem, quod ſcilicet in iſta per dictum experimentum apparenter proveniente diverſitate, nulla ſit ratione ſpiritus ardentis vera & eſſentialis diſtinctio, ſed quod productio coloris cærulei, ex quo differentia judicatur, heterogeneo cuidam, & ad ſpecialem conſtitutionem ſpiritus vini plane non requiſiti additamento debeatur, atque ita totum experimentum, quacunque demum veriſimilitudine ſeſe commendans, falſum ſit, fallax, & inutile, id quod jam ulterius comprobabo.

Præ-

Præmonendum autem mihi eſt quod, cum de additamento hoc ſpiritus vini, liquore ad experimentum pertinente deſtitutus, certior fieri non æque poſſem, operam dederim ſedulam, dictum modo liquorem, utpote ipſummet artificium laudatum, adinveniendi, quamvis ab initio parum in id concitatus, quod eum nunquam, ceu dixi, neque guſtu, neque odoratu ſentire, nec niſi ſub inſtitutis experimentis, eminus intueri licuerit, neque etiam difficultate ſua caruiſſent eum explorandi tentamina (quamvis, ut apparebit, ſimplex ille ſit & ſatis notus) niſi ab amico, qui *Gedani* eum quondam guſtaverat, percepiſſem, quod ſtyptico ſapore ſe exhibeat, quorſum cum omnia mea experimenta dirigerem, tandem reperi, quod arcanum illud tanto in pretio habitum aliud nihil ſit, quam mera ſolutio Martis in acido vitriolico, ſive ex Marte cum ſpiritu vitrioli ſoluto, & cum aqua diluto, ſive ex vitriolo Anglico, ſive ex vitriolo quodam Venereo-Martiali, præcipitando parato, ſive ex mineræ cujuſdam Martis, qualis Haſſiaca eſt, extractione conſtet, quamvis color cæruleus omnium elegantiſſimus cum ultima, nempe cum ſic dicto liquore terræ Martis ſolaris appareat: quo ſaturatior ſolutio eſt, eo minus ejus pro experimento faciendo requiritur.

Quamvis autem liquoris hujus, utpote jam dudum cogniti, nova non ſit inventio, quouſque tamen pro experimento illo probatorio adhibetur, res mihi nova omnino fuit, & perquam grata, tum quod antea cogitando in eam nunquam ſponte inciderim, tum quod in ſecretis haberetur a Mercatoribus, & pro arcano ſollicite ſervaretur, tum etiam & quidem potiſſimum ideo, quod per experimenta mea acceſſorium illud, ſpiritui vini, qua tali, peregrinum & minus eſſentiale, colorem vero cæruleum proprie cum liquore dicto producens, quid ſit, & quanta fallacia, imo falſitate manifeſta

manifeſta laboret experimentum probatorium detegere & propalare potuerim, quæ præcipua mihi cauſa fuit, ob quam obſervationem hanc inclytæ Societati exhibere geſtiam.

Additamentum autem illud flavedinem primum ſpiritui vini Gallici concilians, hinc & ſub experimento cæruleum colorem provocans, lignum quercinum eſt, ſive ſegmenta aut ramenta ejus cum ſpiritu vini infundantur, ſive in dolio quercino novo ſervetur ſpiritus, uſque dum flavedinem ex ligno extraxerit; quo magis autem flaveſcit ille, eo magis cæruleus quoque cum liquore martiali evadit ſub experimento, niſi croco vel alia quadam re flava tinctus fuerit.

Quod autem totum negotium non niſi ligno quercino debeatur, experimento ſequenti in contrarium facto, confirmo: Aſſumo nempe ſpiritum frumenti, eundem, qui cum liquore neque cæruleum colorem exhibet, neque ullam aliam mutationem ſubit, atque ideo a mercatoribus pro puro frumenti ſpiritu judicatur, huic indo ramenta quercina, & infuſa relinquo, donec ſpiritus frumenti flavedinem ſpiritus vini Gallici fere contraxerit, filtrato huic ſpiritui dum liquorem vitriolicum, ſicuti ſpiritui vini Francici, inſtillo, eundem colorem cæruleum & æque elegantem acquiro, nulla ne leviſſima quidem diverſitate, aut diſſimilitudinem indicante phænomeno interveniente; quod abunde teſtatur, omnem mutationem coloris ex ligno quercino originem ducere.

Poſſumus quoque ſpiritum frumenti cum alio quodam, analogia ad lignum quercinum accedente, cum gallis, *v. g* infundere, ſequente ſimiliter experimento. Succedit quoque res quodammodo cum cortice granati & aliis vegetabilibus adſtringentibus, omnium tamen optime cum ligno quercino, cui cortices granatorum longe ſunt inferiores, violaceum potius, quam

cæruleum colorem, & cum agitatur, viroris aliquid trahentem, exhibentes.

Notatu dignum eſt, quod perparum liquoris ex ligno quercino extracti ſufficiat ad qualitatem ſpiritui frumenti bene multo inducendam, pro cæruleo colore, cum dicto ſæpius liquore, exhibendo, ſiquidem cum gutta unica infuſi ligni quercini dimidiam ſpiritus frumenti unciam pro experimento ſubeundo aptam reddidi.

Liquorem ſive ſolutionem ex puro vitriolo Martiali, nullatenus Venereo, conſtare & paratam eſſe debere, exinde apparet, quod experimentum 1. cum Goſlariienſe, Ungarico, Dantiſcano, omnibuſque ſimiliter mixtis vitriolis, quæ qualitercunque venerea vel cuprea ſunt, pro copia cupri commixti plus minus male ſuccedat, & colorem valde dilute cœruleum, ſeu cæſium producat. 2. Quod cum puro vitriolo Veneris plane non ſuccedat, nec ullum colorem cæruleum provocet, ſicuti id variis experimentis, infra in ejus teſtimonium allatis, tentando didici.

Dicendum adhuc reſtaret ut, quibus de cauſis & quomodo color iſte cæruleus oriatur, explicarem; quia vero rem pro incipiente, vel diluto, atramentorio ſcriptorio habeo, cujus ingredientia, colorem ſuppeditantia, præcipua ex iiſdem, nempe ex vitriolo Martis & adſtringente vegetabili conſtant, de quibus *D. Lemery* ſcite & copioſe diſſeruit (*v.* Hiſtoire de l'Academie des Sciences l'annee 1707.) hunc potius allegare volui illis, qui talibus delectantur, quam obſervationem hanc meam allegatis hujuſmodi complere & extendere. Qui opticis aliiſque Philoſophicis rationibus capiuntur, D. *Boyle*, D. *Newton*, & alios legant, qui de coloribus ſcripſerunt.

Sub finem diſcurſus mei paucis commemorabo, quod inter Gallici vini & frumenti ſpiritum, purum nempe

nempe hunc & cum cura deſtillatum, non alia quam peculiaris odoris & ſaporis in Francici ſpiritu vini a vinaceis, petiolis & acinis uvarum oriundi differentia indagari debeat, licet idem quoque odor & ſapor variis modis ſpiritui frumenti conciliari, atque hic ita in Francicum adulterari queat, ut eam vel peritiſſimus pro genuino Francico aut ad minimum pro frumenti ſpiritu plane non habuerit, unde apparet, quod uti primo allegatum, ita & alia experimenta ſpirituum ardentium probatoria nullius, vel ſaltem inſufficientis uſus ſint.

ELENCHUS.

Experimentorum in rem præſentem factorum.

I. Primum octuplas paravi ſolutiones vitriolicas, pro qualibet drachmas duas vitrioli & unciam unam cum dimidia aquæ communis deſtillatæ recipiendo. Habui autem ſolutiones vitrioli. 1. Goſlarienſis, 2. Gedanenſis, 3. Hungarici, 4. De Cypro, 5. Anglici, 6. Martis cum oleo vitrioli parati, 7. Martis ex vitriolo venereo martiali per præcipitationem facti, & 8. ſolutionem mineræ Martis Haſſiacæ, vel ſic dictæ ſolaris, cujus itidem binas tantum drachmas ad dictam aquæ quantitatem aſſumpſi.

II. Deinde triplices feci adſtringentium vegetabilium extractiones per infuſionem, ad quamvis unciam vegetabilis libram unam medicam ſpiritus frumenti, qui à liquore probatorio antea inſtillato nihil plane cœrulei oſtenderat, accipiendo; fuerunt autem extractiones 1. Ligni quercini, 2. Gallar. Turcic, 3. Granatorum corticum.

III. Tertio: Cum tribus hiſce admodum ſaturatis extractionibus, triplicem quoque Gallici vini ſpiritum adulterinum, colorem ſaltem & flavedinem imitando, ex

frumenti ſpiritu effinxi, & quibuſvis octo ſpiritus frumenti unciis unam extractionis unciam ſingulatim commiſcui.

IV. Denique: Ordinarium ſumpſi ſpiritum vini Gallicum flaveſcentem, atque etiam ordinarium recens deſtillatum ſpiritum frumenti, & de utroque, ſecundum ordinem modo dictum cum ſolutionibus vitriolicis, experimenta cepi ac ſucceſſive inſtitui.

V. Infudi nempe quavis vice unciam dimidiam ſpiritus vini Francici in vitrum mundum infra acuminatum, & ſolutionis vitriolicæ unciam, vel ſi non ſufficeret ad cæruleum colorem proferendum, duas, tres uſque ad quatuor guttas inſtillavi, vidique diligenter attendendo, quod ſpiritus vini Gallici dictus 1. cum ſolutione vitrioli Goſlarienſis pallide cæruleum, 2. cum ſolutione vitrioli Gedanenſis, & 3. vitrioli Hungarici itidem ſubcæruleum, 4. cum ſolutione vitrioli de Cypro autem nullum plane cæruleum, ſed ſubvirideſcentem colorem induerit, 5. cum ſolutione vitrioli Anglici, atque 6. cum ſolutione vitrioli Martis, cum oleo vitrioli parati, itidemque 7. cum ſolutione vitrioli Martis per præcipitationem facti, valde elegantem, ſed cum liquore Martis ſolari ſic dicto, ſeu ſolutione mineræ Martis Haſſiacæ, amœniſſimum colorem cæruleum acquiſiverit. Cauſam diverſitatis hujus ſupra jam dedimus.

VI. Cepi porro pro quovis experimento probatorio dimidiam itidem ſpiritus frumenti unciam, & cujuſvis ſolutionis vitriolicæ unam, duas, tres, imo decem & plures guttas inſtillavi, ſed à nullius neque etiam ipſius liquoris ſæpius dicti commiſcela, quamvis plus ad hunc, quam ad Gallici vini ſpiritum additum fuerit, ullam ne leviſſimam quidem livedinem cœrulei coloris obſervare licuit.

VII.

VII. Poſtmodum aſſumpſi dicti ſub numero III. & de eodem ſpiritu frumenti, triplici modo imitando effecti ſpiritus vini Gallici unciam dimidiam pro quovis experimento, eandemque cum ordinario ſpiritu vini Francici factam, ac ſub numero V. commemoratam inſtillationem cujuſvis ſolutionis vitriolicæ inſtitui, & ecce conveniebant experimenta cum genuino, & cum adulterato hoc ſpiritu vini Gallici facta, per omnia phænomena; niſi quod propter combinatas variorum vegetabilium extractiones aliqua hinc inde, quamvis admodum levis differentia interveniret, uti ex ſequentibus apparebit.

VIII. Spiritus frumenti cum extractione gallarum ab inſtillata ſolutione vitrioli 1. Goſſarienſis, itidemque 2. Gedanenſis nigreſcebat quodammodo, 3. cum ſolutione vitrioli Hungarici ab initio, ſub inſtillatione, dabat quidem aliquid cærulei, ſed inter commiſcendum perdebat penitus, 4. cum ſolutione vitrioli de Cypro plane non tingebatur, 5. cum ſolutione vitrioli Anglici eleganter cæruleſcebat, & 6. cum ſolutione Martis utroque modo parati, ab initio quidem cæruleo, ſed commiſcela vero & interagitatione cum caule pennæ, violaceo colore tingebatur; denique 7. cum ſolutione mineræ Martis, & quidem una ſaltem ejus gutta, mixtus hic ſpiritus frumenti egregium acquiſivit colorem cæruleum.

IX. Spiritus frumenti extractione corticum granatorum, ceu dictum, imbutus, cum ſolutionibus vitriolicis, juxta ordinem præcedentem confuſus, parum vel nihil cærulei provocavit, & circa priora quatuor puncta, eodem fere modo, ſe exhibuit, atque ſupradictus frumenti ſpiritus gallarum extractione imprægnatus, in poſterioribus quoque experimentis mox viridiuſculi quid, mox atramento æmulum oſtendebat.

X. E contrario ſpiritus frumenti extractione ligni quercini in ſimilitudinem quandam Gallici infucatus,

cum

cum ſolutionibus vitriolorum pure martialium, & præ cæteris cum liquore Martis ſolari, omnium pulcherrimo colore cæruleo inficiebatur. Cum primis quatuor ſolutionibus vitriolicis vero eodem ſe modo habuit, ac cœteri ſpiritus frumenti fucati, imo non ſecus ac ipſe ſpiritus vini Gallici.

XI. Tandem cum ſatis eſſem confirmatus, rem cum ramentis ligni quercini optime ſuccedere, experiri libuit, quantum infuſi vel extractionis ligni quercini, ad minimum pro aliquali ſaltem cœruleato colore producendo requireretur.

Varias ergo quantitates minuendo ſemper parciores, à dimidia drachma ad viginti, quindecim & pauciores guttas deſcendendo, probavi, adeo ut unica gutta ſufficeret totam ſemiunciam ſpiritus frumenti qualitate imprægnándo, quæ ad colorem cœruleum, licet non adeo ſaturatum, unica ſaltem gutta liquoris mineræ martis accedente, producendum requiritur.

III. *An Account of a Fork put up the* Anus, *that was afterwards drawn out through the Buttock; communicated in a Letter to the Publiſher, by Mr.* Robert Payne, *Surgeon at* Loweſtofft.

James Biſhop, an Apprentice to a Ship-Carpenter in *Great-Yarmouth*, about 19 Years of Age, had violent Pains in the lower Part of the *Abdomen*, for 6 or 7 Months; it did not appear to be any Species of the Colick; he ſometimes made bloody Urine, which induced me to believe it might be a Stone in the Bladder. He was very little relieved by Phyſick; at length a hard Tumor appeared in the Left Buttock, on or near the *Glutæus Maximus*, 2 or 3 Inches from

the Verge of the *Anus*, a little ſloping upwards. A ſhort time after he voided purulent Matter by way of the *Anus*, every Day for ſome Time; the Tumor broke; I ſuſpected a Fiſtula *in Ano*, but could not get the Probe, by the Orifice of the Sore, into the *Rectum*. Shortly after the Prongs of a Fork appeared through the Orifice of the Sore, above half an Inch beyond the Skin. As ſoon as the Prongs appeared, his violent Pains ceaſed; I divided the Fleſh between the Prongs, according to the beſt of my Judgment; and after that made a circular Inciſion about the Prongs, and ſo with a ſtrong Pair of Pincers extracted it, not without great Difficulty, Handle and all entire; the End of the Handle was beſmeared with the Excrement, when I drew it out; it is 6 Inches and a half long, a large Pocket-Fork; the Handle is Ivory, but is dyed of a very dark-brown Colour; the Iron Part is very black and ſmooth, but not ruſty. He is likely to do well in a ſhort Time, the Operation being performed the 11th of *October* laſt; he would not own how it came, till a Relation of his, a Gentleman in this Neighbourhood, who ſent him to me to be under my Care, the Reverend Mr. *Gregory Clark*, Rector of *Blundeſton*, on whom, in a great Meaſure, his Dependence is, threatened never to look upon him more, unleſs he would give him an Account how it came; and he told him, that, being coſtive, he put the ſaid Fork up his Fundament, thinking by that Means to help himſelf, but unfortunately it ſlipt up ſo far, that he could not recover it again.

Loweſtofft, 7 *Miles from* Great-Yarmouth, *5th* Novemb. 1725.

Rob^t Payne.

P. S. He ſays he had no Trouble or Pain, till a Month, or more, after it was put up.

IV. *Two*

IV. *Two Caſes of Inſects voided by the urinary Paſſage, communicated in a Letter to the Publiſher, by Dr.* Daniel Turner, *Coll. Med. Lond. Lic.*

Dec. 6. 1725.

ON the 24th of *November* laſt, a poor Woman, in my Neighbourhood, came to crave my Advice for her Grand-child, an Infant of about 16 Months old, bringing with her a Worm, which ſhe told me the Apothecary had juſt then drawn out of the Child's *Penis*, who had for ſeveral Days before labour'd under great Uneaſineſs; which (as uſually term'd by theſe People) ſhe call'd Convulſions of the Bowels. The Infant was continually drawing up the lower Limbs, and ſtraining at both Sphincters; the Urine ſeemed to paſs with Difficulty for ſome Days, till at laſt there came on a total Suppreſſion, and the Worm, advancing, ſhew'd itſelf at the Extremity of the *Urethra*, when the Apothecary was called up to help them. I examined the Inſect, and found it meaſur'd above 4 Inches, reſembling the Worms uſually excern'd *per Podicem*, of the Earth-worm Kind, but whiter; which made me think at firſt they had been miſtaken, and that the ſame had been that Way excreted. Having directed what I thought proper, I went to the Apothecary, who has the Worm now in his Cuſtody, to be better inform'd. He aſſures me, that, when he came to the Child, he ſaw a preternatural Body, which at firſt he knew not what to make of, hanging half an Inch out of the *Glans*, and lying double in the Paſſage: Perceiving it farther advancing, he took hold of it, and with little Difficulty drew forth the ſame.

The Summer before the laſt, a Woman ſhow'd me an Inſect of the Maggot Species, with a cruſty red *Galea* over the Snout, and a creſcent or forked Tail, which ſhe had juſt then voided by the urinary Paſſage.

V. *An Account of a new Machine, called the* Marine Surveyor, *contrived for the Menſuration of the Way of a Ship in the Sea, more correctly than by the* Log, *or any other Method hitherto uſed for that Purpoſe; together with ſeveral Teſtimonials, ſetting forth the Uſefulneſs of this Invention. By Mr.* Henry de Saumarez *of the Iſland of* Guernſey.

HAving for ſeveral Years applied my Studies to the Improvement of Navigation, I have fallen on various Projects, as well for the better clearing a Ship off a Lee-ſhore, as for her Steering, Tacking, or Waring, *&c.* which I may probably ſoon publiſh to the World: But what has moſt of all employ'd my Thoughts, has been to contrive ſomething to aſcertain the Way of a Ship in the Sea, more correctly than by the Log, (at preſent in Uſe in our Navigation;) the Errors of which are ſuch, that I am fully perſuaded the unhappy Fate of the brave Sir *Cloudſley Shovell*, was more owing thereto, than to any Errors in Judgment. The melancholy Reflections I made on that national Loſs, was what ſpurr'd me on to find out ſomething that would not only have a regular Motion under Water, but might communicate the ſame to a Dial, or Piece of Clock-Work, within the Ship.

After Variety of Experiments, I was at laſt ſo happy as to anſwer my Purpoſe in ſome Meaſure; and as in *Guernſey* we are at a Loſs for curious Workmen, in the Year 1715. I came to *London*, and furniſhed my ſelf with a Boat, and all the Materials neceſſary for my Deſign. Daily was I on the River *Thames* making Experiments, and was frequently honoured with the Company of ſeveral of the *Literati*, who were greatly pleaſed with my Invention; but, not being fully ſatisfied of the Certainty thereof, they adviſed me to try it on a Standing Water. According to their Deſire I did ſo, for I obtained a Liberty ſeveral Times of having a Boat on the Canal in St. *James*'s *Park*, where I demonſtrated, to ſuch curious Perſons as favoured me with their Company, the Uſefulneſs of ſeveral Inſtruments; for we there fixed two Poles for Marks, and notwithſtanding I many Times alter'd the Motion of my Boat, by ſometimes rowing faſt, and at other Times ſlow, yet had my Machine under Water the ſame Number of Revolutions between the Marks. It may, perhaps, be ask'd, how I came to be aſſured that the Revolutions of the Engine under Water are regular, let the Motion be ſwift or ſlow, and that they anſwer the ſame Diſtance? I will not yet ſay, that this admits of ſo clear a mathematical Proof, as any Propoſition in *Euclid*; however, I have mechanically found it ſo in a great Variety of Experiments; and that every Turn or Revolution of the Engine under Water juſt meaſures 10 Feet, which I call the decimal hydraulical Circumference thereof.

The *Primum Mobile*, or Soul of this Machine, is in the Form of the Letter Y, and is made in Iron, or any other Metal: At each End of the Lines, which conſtitute the Angle, or upper Part of that Letter, are two Pallets not much unlike the Figure of the Log;

Log; one of which falls in the ſame Proportion as the other riſes. The falling or pendent Pallet meeting a Reſiſtance from the Water, as the Ship moves, has, by that Means, a circular Motion under Water, which is faſter or ſlower, according as the Veſſel moves. This Motion is communicated to a Dial within the Ship (which is fix'd either in the Maſter's Cabbin, or any other proper Place) by means of a Rope (of any convenient Length) faſten'd to the Tail of the Y, and carried to the Dial. The Motion being thus communicated to this Dial, which has a Bell in it, it ſtrikes exactly the geometrical Paces, Miles, or Leagues, which the Ship has run. Thus is the Ship's Diſtance attained; and with equal Eaſe may the Forces of Tides and Currents be diſcovered by this Inſtrument.

The Figures adjoin'd more fully explain the firſt Movement of my Machine.

In Figure 5. AKCL and BHDI are the Pallets, which are work'd from the Legs DE and CE into the Form they appear, to a Breadth of about 4 Inches and a half. The Length of the Pallets (BD and AC) are 8 Inches. The Branches or Legs, DE and CE, are each 15 Inches and a half long, and 2 in Circumference, the Diameter of which is about two Thirds of an Inch; and the Angle CED, which is contained between them, is 45 Degrees.

The Shank EF is of the ſame Thickneſs or Circumference with CE and DE, and is 27 Inches long. At the Point F there is a Ring, where one End of the Rope FG is hook'd to the Machine, the other End G being fixed to the Dial within the Ship or Veſſel. This Rope may be about 5 Fathoms, more or leſs, according as the Dial is fixed high or low, in reſpect to the Surface of the Water.

In the Figure afore-mentioned, this Machine has but two Branches; however, it may be form'd of three, if not four, and adjuſted to the ſame Standard or Meaſure: But as three or four Branches would be more ſubject to entangle themſelves in Sea-Weeds, and thereby prevent the regular Motion of the Inſtrument, if not in ſome Meaſure impede the Ship's Way, I cannot but recommend their being made only of two Branches, in the Manner I have laid down; for, in my own Experiment at Sea, I have obſerv'd thoſe made in this Form have been ſo far from being choak'd by Weeds, that if they encountered any at any Time, they have always cleared themſelves of them, without the Trouble of hauling the Engine into the Ship to do it.

To regulate this Inſtrument, it may be done ſeveral Ways; as firſt, by opening or cloſing the Angle CED; ſecondly, by lengthening or ſhortening the Branches, or turning or bending more or leſs the Pallets AKCL and BHDI; and ſo in this Manner the Machine is brought to what Standard or Meaſure you pleaſe, to make the hydraulical Revolution to anſwer either to a geometrical Pace of 5 Feet, or to 10, 12, 14 Feet, *&c.*

The Machines of this Kind, which I have tried at Sea in all Sorts of Weather, did weigh ſome 4, others 5, and others 6 Pounds; the Weight of them not at all affecting the peculiar Property of the Inſtrument, or hindering the Regulation thereof according to the Methods I have laid down.

Theſe Machines may be made of Tin as well as Iron, and ſo light as not to weigh above two or three Pounds, which may ſerve for any Boat, Wherry, Barge, *&c.* without any Hindrance to their Rowing or Sailing. The Manner of fixing them to a Ship, or Boat, is repreſented in *Fig.* 6.

I come now to the Explanation of three ſeveral Dials, any one of which may be uſed with this Machine.

The firſt Dial had three Indexes, one of which mark'd 10 Revolutions of the Engine, each Revolution 10 Feet; ſo that of conſequence the whole Round of the Circle was 100 Feet. As five of theſe Revolutions make 50 Feet, which I reckon to be (or at leaſt ſhould be) the Diſtance marked between each Knot on the Log-Line now in Uſe at Sea; by holding the Half-minute Glaſs in one's Hand (which is always uſed with the Log-Line) one may, by Inſpection, ſee how many Times 50 Feet ſhe runs in half a Minute, and of courſe how many Miles in an Hour, without the Trouble of employing four or five Hands, as there generally is, in heaving the Log. My ſecond Index on this Dial marked 100 Revolutions, which makes 1000 Feet, as the third Index did 1000 Revolutions, which is equal to 10,000 Feet; and then a little Bell ſtruck, ſignifying when the Ship had ſailed that Diſtance, which may be alſo fitted to ſtrike to any other Meaſure.

My ſecond Dial had the Circle on its Plate divided into twelve Parts, ſo that as the Index paſt each Diviſion, the Ship had run one Mile, and conſequently twelve Miles, when it had meaſured the Circumference. On one Side of this Dial, I had fixed another Plate, which was graduated in ſuch Manner, that by the Half-minute Glaſs I could alſo, by Inſpection, tell what the Veſſel run in that Space of Time, *&c.*

On my third Dial I had three Circles; the firſt was ſo divided, as to ſhew when the Ship had run 60 Leagues; the ſecond was ſo contrived, as to ſhew when the Ship had run the ſame Diſtance in Miles; and on the third was mark'd 120 Knots; ſo that, computing

puting each Knot at 50 Feet, the Circumference was 6000 Feet, which I take to be the Standard of an *Engliſh* Maritime Mile, or the $\frac{1}{60}$ Part of a Degree upon the Equator; in running which Length, my Inſtrument has juſt 600 Revolutions; to which Diſtance a little Bell ſtrikes to give Notice, to the Man at the Helm, of the Diſtance ſailed in that Time.

Beſides the ſeveral Circles on this Dial (graduated as I have mentioned) I had alſo two Plates on each Side, having two Circles; one divided into 100 Leagues, and the other into 300 Miles; ſo that, without hearing the Bell ſtrike to every Mile or League, one might at any Time ſee by them, what Number of Miles or Leagues the Ship had run, from the Time ſhe had left her Port.

As to the Materials within the Dial, there is little more than common Clock-work.

As by this Machine I undertake to correct the Errors of the Log, I flatter my ſelf that a Compariſon between that Inſtrument, and my Invention, will not be unacceptable to the Curious; I therefore preſent you with

A Comparative Diſcourſe between the Log *and my Inſtrument, which I chuſe to call the* Marine Surveyor.

1*ſt*. "THE firſt Error I chuſe to touch on, in re-
" lation to the *Log*, is in the half and quar-
" ter Minute Glaſſes; I think I may well affirm, that
" they are ſeldom or never true, in regard it rarely
" happens that we can find two to finiſh their Courſe
" in the ſame Space of Time; yet, if they did run
" their Sand out equally, it is no Demonſtration of
" their Truth, ſince two, that are falſe, may do the

" ſame

" ſame, as well as two that are true. But, admitting
" they were never ſo truly made, they are notwith-
" ſtanding ſubject to Error, ſince it is but too well
" known, that dry and wet Weather have a great In-
" fluence on them. Should the Half-minute Glaſs
" lack but two Seconds, or be two Seconds too long,
" it makes an Error of ſome Miles in 24 Hours. If
" the *Log* be hove by Quarter-minute Glaſſes, in like
" manner defective, (which is the general Practice,
" when the Ship has great Way) in doubling the
" Knots, the Error is alſo doubled. Beſides, when the
" Ship runs after the Rate of 8 or 9 Miles an Hour,
" (and the Line is left to run off of the Reel) it
" rarely happens but ſome Fathoms are out, before
" the Line can be ſtopp'd; though this may be ſmall
" in the Courſe of 24 Hours, and therefore diſregard-
" ed; yet in a long Voyage it will make a great
" Addition to the many Errors in the Diſtance (which
" we gain by the *Log*) which, added to thoſe of our
" Judgment, occaſions ſo many that keep Journals
" at Sea, to be a Shore, when they have reck-
" oned themſelves 50, 60, or more Leagues from
" the Land; and others to be as many Leagues
" from their Port, at the Time when they have
" expected to make it.

" In the *Marine-Surveyor* it is not ſo; for this In-
" ſtrument requires no Glaſſes of any Kind: Let the
" Ship run faſt or ſlow, it is the ſame, for it works in
" Proportion, and the Bell ſtrikes to every Mile ac-
" cordingly. To evidence the Truth of this, I take
" Leave to mention an Inſtance, *viz.* When I was
" making my Experiments on the Canal, the Reverend
" Dr. *Deſaguliers*, one of the Members of this honou-
" rable Society, and ſeveral other Mathematicians,
" at Times, were with me, and we meaſur'd out a

" certain

" certain Diſtance there; upon which I fitted my " Machine to ſtrike to that Diſtance, and accordingly " it did ſo. We then alter'd the Motion of the Boat, " and row'd much faſter to the Mark than we had " done before; however, the Bell ſtruck, when we " came up to it, to the greateſt Exactneſs: And ſuch " is the Property of this Inſtrument, that it may be " fitted to ſtrike to Miles, Leagues, *&c.* as ſhall be " thought proper. This Machine is made of Materi- " als ſo durable, that one of them ſhall laſt 50 or 60 " Years; and ſuch is the Price, that they will prove " as cheap or cheaper to the Government, than the " *Log*, which is attended with an Expence of ſo ma- " ny Lines, Glaſſes, *&c.* As for the making a Trial " of this Inſtrument, it may be as fully done in the " Channel, as in an *Eaſt-India* Voyage; for if it an- " ſwers to 20, 30, or 40 Leagues, the Reaſon holds " good for as many Thouſand.

2*d.* " The chief Property of the *Log* is to have it " ſwim upright, or perpendicular to the Plane of the " Horizon. This is too often wanting in *Logs*, be- " cauſe but few Seamen examine whether it is ſo or " no, and generally take it upon Truſt, being ſatis- " fied, if it weigh a little more at the Stern than the " Head. What erroneous Reckonings flow from hence " is but too evident; for if the *Log* does not ſwim " upright, it will not hold Water, neither remain " ſteady in the Place where it is heav'd, ſince the " leaſt Check of the Hand, in veering the Line, will " make it come up ſeveral Feet. This repeated, the " Errors become Fathoms, and perhaps Knots, which, " how inſignificant ſoever they may ſeem, are Miles " and Parts of Miles, and amount to much in a long " Voyage.

" In

" In anſwer to this, the *Marine-Surveyor* is of ſuch " a Property, that there is no Neceſſity to take Care " about its ſwimming; and it is a conſtant Truth, " peculiar to this Inſtrument, that be the Ship's Moti- " on on the Water what it will, whether ſhe runs one " Mile faſter or ſlower than another, yet all ſhe runs, is " exactly mark'd on the ſaid Inſtrument, as appears " plainly from ſome Tables of Experiments made by " me in the River *Thames*, for obtaining the gradu- " al Increaſe and Decreaſe of both Ebb and Flood.

3. " The ſtretching and ſhrinking of the *Log-Line*, " is another great Error in the Uſe of the *Log*; for " when a new Line is firſt us'd, let it be ever ſo well " ſtretched upon Deck, and meaſured as true as poſ- " ſible, it ſhrinks after wetting conſiderably; and " therefore if we rely on the Line run out for the " Ship's Diſtance, we ought to meaſure and alter the " Knots on it every Hour before we uſe it; but I " am well aſſured that this is ſeldom done oftner than " once a Week, and ſometimes not above once or " twice in a Voyage. What great Dependance then is " there on a Reckoning kept by the *Log*? Since in " this Caſe the Line will ſhrink ſo, as to add Miles to " the other Miſtakes of every 24 Hours. Again, " when the Line is meaſured to its greateſt Degree " of Shrinking, it is generally left there; and when, " by much Uſe, it comes to ſtretch again, it is ſel- " dom or never mended, although it will ſtretch be- " yond what it firſt ſhrunk. In ſhort, ſuch are the " Errors incident to the *Log*, that I don't wonder at " our Neighbours the *Dutch* for preferring their " *Chips* or an irregular Pulſe to it; which conjec- " tural Reckoning of theirs is obtained after the fol- " lowing Manner. They fix two Marks on the " Side of the Ship at a certain Diſtance, when an ex-

" perienced Perſon, ſtanding at the foremoſt Mark,
" throws a Chip over-board, and counts the ſeveral
" Beats of his Pulſe, during the Chip's Paſſage from
" one Mark to the other; and from thence it is they
" compute the Number of Miles that the Ship runs
" in an Hour.

" As for the *Marine-Surveyor*, it is not hove with
" a Line, but is tow'd a Stern by a Rope; and let
" that Rope ſtretch or ſhrink (be long or ſhort) it is
" all one, for the Inſtrument will have the ſame true
" Revolutions. Should it be objected, that it holds
" Water, I affirm, from my own Experiments of it,
" that the *Log* haul'd in from 5 or 6 Knots, is much
" heavier upon the Hand; and that the faſter the
" Ship runs, the leſs Water this Inſtrument of mine
" holds, becauſe it gives Way to the Water and turns
" quicker; nay, I can venture to ſay, that it is ſo far
" from being any conſiderable Impediment to the
" Ship's Way, that ſhe does not loſe one Mile in an
" hundred by it. But ſhould this Inſtrument be intro-
" duced into the Navy, in caſe of chaſing an Enemy,
" or the like, it may be taken in at any Time, and
" let down again at Pleaſure.

4. " I appeal to all Seamen, if in a moderate Gale,
" when the Ship runs 5 or 6 Knots, two diffe-
" rent Perſons (every way qualified) were to heave
" the *Log* immediately after one another, whether
" they would exactly agree. Surely no. Since 'tis
" but Chance if they do ſo, and is what may not
" happen in an hundred Trials. I therefore affirm
" the *Log* to be very erroneous on this Account, and
" that the Error frequently increaſes with the Wind;
" for in a ſtiff Gale, when a Ship has run about 8 or
" 9 Knots before the Wind, it has been known that
" two expert Seamen have hove the *Log* in this Man-

" ner, and on their comparing Notes, they have found " a Knot Difference ; ſometimes it has been more, and " at others leſs, which muſt certainly make a ſtrange " Confuſion in the Reckoning. Under this Head I " take leave to obſerve, that when the *Log* is hove, " it is ſometimes in ſo ſtrong a Gale, that the Ship " runs 9 Knots; but before it is hove again, there " may be ſuch a Decreaſe of the Wind, that for half " of the Hour ſhe may not run above 5 Knots. Her " true Diſtance ſailed then, is the Mean between the " Extremes of 9 and 5 ; but this has been ſo far from " being conſider'd by ſome *Chalkers of the Log-board*, " that it is but too well known, the Extremes have " been put for the Mean, and the contrary. Were " there Truth in the *Log*, two Ships in Company " would nearly have the ſame Account; but it is " otherwiſe; for we too often find many Leagues Dif- " ference in Reckonings, even on board the ſame " Ship. In a word, ſuch Errors have been found in " the *Log* by ſome of my Acquaintance, that when " they have ſailed between a Meridian and a Paral- " lel, the whole Difference on the *Log-board* has not " prov'd *Difference of Latitude* enough to agree with " their *Obſervation*, although each Day they had a " good obſerv'd Latitude, and no Currents.

" In the *Marine-Surveyor* we are ſo aſſured of the " Ship's Diſtance, that all Ships ſhall agree which are " in Company, as to their Reckonings, ſave that ſome " Allowance be made for Difference of Judgment in " the ſeveral Perſons who keep Journals.

There are ſeveral other Caſes equally, if not more momentous than what I offer here, wherein the *Marine Surveyor* will be found to have the Preference of the *Log*; but I forbear to mention them, chuſing rather to entertain you with undeniable Proofs of the

Uſefulneſs of my Invention, which I perſwade my ſelf will be found to be of ſingular Advantage to the Trade and Navigation of my Country.

The following are the Subſtance of two Affidavit, taken under the Seal of the Royal Court at *Guernſey*, by ſome expect Seamen, who have had Trial of my Inſtrument, *viz.*

"KNOW ALL MEN BY THESE PRESENTS, that "on the 30th of *November* 1720, there perſonally "appeared before *William Le Marchant* Eſq. (Judge "Delegate in the Iſland of *Guernſey*, &c.) Meſſieurs "*Jean Andros*, and *Eleazar Le Marchant* (Jurats of "the Royal Court of the ſaid Iſland)

"*William Ahier*, aged about 40 Years, who com- "manded ſeveral Privateers in the late War, (and "particularly that call'd *La Chaſſe*, of about 150 "Tuns, 16 Guns, and 140 Men) and is now Maſter "of the Ship call'd the *Eagle*, of which Veſſel he is "the only Proprietor, who voluntarily makes Oath, "that on *Sunday* the 9th of *October* 1720, he parted "from *Southampton* with ſeveral Gentlemen Paſſen- "gers on board for *Guernſey*; that he had fix'd at the "Stern of his Ship a new Invention call'd the *Ma-* "*rine-Surveyor*, projected, to the beſt of his Know- "lege, by Mr. *Henry de Saumarez*, a Gentleman of "the Iſland of *Guernſey*, for correcting the *Log*, &c. "That after they had left the *Needles*, they had a "ſtiff Gale of Wind, attended with a rolling Sea, "notwithſtanding which, the Machine work'd as re- "gularly as if it had been ſmooth Water, the little "Bell of it ſtriking to every Mile the Ship run with "great Exactneſs. And this Deponent further de- "clares, that having thoroughly view'd and examined "the Experiment of this new Invention, he finds it "to be not only practicable, but preferable to the

"common

" common Methods us'd at Sea for attaining the " Ship's Diſtance ſail'd; that therefore, for the pub- " lick Good, he doth atteſt the Truth of the above- " mentioned Particulars. In witneſs whereof, the " Seal of the Royal Court of *Guernſey* is hereunto " affix'd by us the under-written,

William Le Marchant, Judge Delegate.
Jean Andros, } Jurats.
Eleazar Le Marchant, }

The other Affidavits runs as follow, *viz.*

" KNOW ALL MEN BY THESE PRESENTS, That " on the 30th of *November* 1720, there perſonally " appear'd before *William Le Marchant* Eſq; Judge " Delegate in the Iſland of *Guernſey*, &c. Meſſieurs " *Jean Andros* and *Eleazar Le Marchant*, Jurats of " the Royal Court of the ſaid Iſland.

The following Perſons, *viz.*

" *Abraham Le Meſurier*, of about 48 Years of Age, " formerly Captain of ſeveral Ships,

" *Peter Bonamy*, of about 58 Years of Age, for- " merly Captain of ſeveral Ships, and who has uſed " the Sea above 40 Years,

" *John Hardy*, of about 38 Years of Age, formerly Cap- " tain of ſeveral Ships, *William Ahier*, about 40 Years " of Age, and formerly Captain of ſeveral Ships; and " *James Hubert*, of about 27 Years of Age, who has " alſo been Maſter of ſeveral Veſſels, who voluntа- " rily make Oath, that on the 19th of *October* 1720, " they ſet Sail in the Morning out of *Guernſey Pier*, " with a freſh Gale of Wind, in a Sloop call'd the " *Dolphin*, in Company with ſeveral Gentlemen of

" the ſaid Iſland, in order to make an Experiment at
" Sea of a Machine call'd the *Marine-Surveyor*, pro-
" jected, to the beſt of their Knowledge, by Mr.
" *Henry de Saumarez* of *Guernſey*; which Invention
" is intended to correct the many Errors of the *Log*,
" *&c.* And they further declare, that they have not
" only throughly view'd, conſider'd, and examin'd the
" ſaid Machine, but have alſo made ſeveral Experi-
" ments of it in a rough Sea, ſometimes ſailing right
" before the Wind, then quartering; at other Times
" turning to Windward, and then lying by to know
" the Drift of the Ship both with and againſt the
" Tide: That having tried the ſame Invention all
" Manner of Ways, they find it much preferable to
" the *Log*, or any of the Methods in uſe for ob-
" taining the Ship's Diſtance run, having nothing to
" object againſt it, as to its being a *Clog* or Hindrance
" to the ſailing of the Ship *&c.* That being fully
" ſatisfied of the great Uſefulneſs of this Invention
" for the Improvement of Navigation, and the Service
" it may be of to all the Maritime Powers, they
" publickly atteſt the Truth of the above-mentioned
" Particulars, to the End the Author thereof may
" make ſuch Uſe of it, as he ſhall think moſt proper.
" In witneſs whereof, the Seal of the Royal Court of
" *Guernſey* is hereunto affix'd by us the underwritten,

" Signed by the Judge Delegate and Jurats, as
" above-mentioned.

Here you have ſome Proof of the Uſefulneſs of this new Invention, and that from Seamen of long Standing and Practice: But, notwithſtanding theſe Teſtimonials, I was yet determined to have it tried further: Accordingly I made a Preſent of one of my Machines to a Friend of mine, Captain *John Thoumes*, who be-

ſides

ſides his Knowledge in the Theory and Practice of Navigation, was the better qualified to make Trial of it in regard he had ſometimes accompanied me in my Experiment on the Canal in St. *James*'s *Park*, and in the River *Thames*. As he was then going a Voyage, I intreated him to act impartially with me, and to loſe no Opportunity in letting me know how far, and with what Certainty, my Invention might be depended on. Agreeable to my Requeſt, he wrote twice to me on this Occaſion: His firſt Letter was dated at *Nantes* the 20th of *October* 1724, and the following is an Extract of it, *viz.*

" According to my Promiſe, I am to acquaint you, " that I have had as favourable an Opportunity as I " could have wiſh'd for, to try your *Marine-Surveyor*; " for ſome Part of my Voyage being from St. *George*'s " Channel to the Bay of *Biſcay*, I paſs'd cloſe to the " Land's-End of *England*, with a moderate Gale of " Wind at North, our Courſe *S.* by *E.* When I had the " Land's-End Eaſt of me about 3 Miles, I began to " reckon, and the next Morning, when *Uſhant* bore " Weſt, about 5 Miles Diſtance, the *Surveyor* had " made juſt 37 Leagues. Theſe two noted Headlands, " which are very near under the ſame Meridian, dif- " fer in Latitude about 33 or 34 Leagues. As for " the Tides, we croſs'd them, having in this Run two " Floods and two Ebbs; and as the Wind blew croſs " the Channel, one Tide was no more influenced by " it than the other, nor could the Current be any " Impediment to the Trial. Now as to our having " 3 or 4 Leagues more than the true Diſtance, the " Reaſon is very plain, ſince it cannot be expected " but that a Ship before the Wind will deviate from " her true Courſe, ſometimes one Way, ſometimes " another, in her *Yaws* and *Sheers*. Of this all Sea-

" men

" men are ſenſible. What I would remark from hence " is, that the *Surveyor* meaſures all the little Traver- " ſes exactly; 'tis therefore the Buſineſs of the Navi- " gator to allow for this, when he works the Ship's " Run. But I cannot help obſerving here, that a good " Effect is produced from theſe little Traverſes being " ſo meaſur'd; for ſhould we be running boldly on " the Land in a dark Night, it forewarns us to look " out in time, by marking ſomewhat more than the " true Diſtance ſail'd upon a ſtreight Line.

" Many are the Advantages which accrue to Navi- " gation by this Invention, which I ſhall not take " upon me to enumerate: In ſhort, the Sailors are in " love with it, and when at the Helm, they value " themſelves on chalking more Miles than thoſe who " went before them. For my own Part, I am ſo " pleas'd with it, that I have done with the *Log*. " One excellent Quality I obſerve in it, which I " cannot omit mentioning, *viz.* That in plying to " Windward along Shore in a dark Night, our uſual " Way, by the *Log*, is to ſtand two or three Hours " out, and ſo many in; and here we may be a ſhore " before we are aware, becauſe in running out we " may not have had ſo much Wind as in running in; " or we may have reef'd Topſails, ſhorten'd Sail, " hanker'd in the Wind, or have met with many " other Impediments, which, by being drouſy in the " Night, a Man may ſometimes not take Notice of; " but it is otherwiſe with the *Surveyor*; for if the " Ship is hindered in her Way, it will not mark more " Miles than ſhe has run.

" I have ſhew'd it to ſome curious Perſons at " *Nantes*, who are greatly delighted with it. They " wanted to ſee the Movement within, but I ſhall " never grant that to a Stranger. I have been offer'd

" fifty

" fifty Piſtoles for it, and might have had more, would
" I have parted with it; but I value the worthy Do-
" nor of it too much, to do any ſuch Thing.

P. S. " When I ſaid my Courſe from the *Lands-*
" *End* to *Uſhant* was *S. b E.* it muſt be underſtood
" that I did not go on the Outſide, but paſs'd within,
" between *Uſhant* and the Main: For in the other
" Caſe, to paſs to the Weſtward, the Courſe had been
" about *S. b W.* to go clear of all.

The ſecond Letter, which I receiv'd from Captain *Thoumes*, in relation to my Inſtrument, was dated at *Guernſey* the 2d of *September* 1725; and what follows is the Subſtance of it, ſo far as it relates to the *Marine-Surveyor*, viz.

" I am now fully confirm'd of the Uſefulneſs of
" your *Marine-Surveyor*, having tried it, this laſt
" Voyage to *Marſeilles* and *Toulon*, ſufficiently to
" perſuade me, that it is greatly preferable to the *Log*.

" Having in two former Voyages in the Bay of
" *Biſcay* been apprized, that the Ship's Diſtance ſail'd,
" as obtain'd by the *Marine-Surveyor*, was really true,
" yet I was oblig'd every 24 Hours to ſhorten the
" Diſtance by a certain Proportion, that I gueſt to be
" near one ſeventh Part of the Whole; which, from
" the Bearings of Headlands, *&c.* I found conſtantly
" ſo. However, to be better ſatisfied of this Allow-
" ance, I wanted a long Run, near, or upon a Meri-
" dian, with good Obſervations, which could not be
" had in the Bay or our Channels; therefore, when
" I ſail'd for the *Mediterranean*, which was in *January*
" laſt, I continued to make the ſame Allowance, and
" caution'd my Mate to make it alſo. It happen'd,
" that for the firſt eight Days, we had hard Gales of
" ſoutherly Winds, attended with violent Squalls of
" Rain, and a diſtracted Sea, inſomuch that we try'd

" under a double reef'd Main-ſail, great Part of the " Time, and drove to the Weſtward, without the Be- " nefit of celeſtial Obſervations; yet all the While the " *Marine-Surveyor* ſtruck the Miles of our Drift, " which are to be ſeen upon our Journals for every " Hour; and ſo far did I depend on it, that I did not " order the *Log* to be once hove.

" After the bad Weather, the Wind chang'd with " the new Moon, to *N. N. E.* and *N. E.* with a brisk " Gale, which gave us a fair Run for five Days, near " 50 Leagues every 24 Hours. We had daily Obſer- " vations, and our Courſe was near South. Here it " was, that I found the one ſeventh of the Ship's Di- " ſtance was to be deducted from the whole, and that " it was for *Yaws* and *Sheers*, which the *Marine Sur-* " *veyor* marks exactly. After this Allowance was " made, ſo well did my Reckoning agree with my " Obſervation, that when there was 2 or 3 Miles dif- " ference, I rather imputed it to the Want of Exact- " neſs in my obſerving, or a Fault in the Quadrant, " than to the *Marine-Surveyor*, in regard my Mate " alſo found it to agree to a ſurprizing Exactneſs.

" Three Weeks after our Departure, I had the " Misfortune to loſe the Fork of the Machine, and " therefore was afterwards without the Help of the " *Surveyor*, till our Arrival at *Toulon*; which Place " being one of the chief Nurſeries for Navigators " that ſerve the *French* King, I was the more con- " cerned for my Loſs; but I in ſome meaſure repaired " it, by ordering a Smith to make two ſuch Forks, of " nearly the ſame Dimenſions and Turns in the Fins, " as I could remember the other had, which ſerv'd " there ſo well, as to gain the Admiration of all who " ſaw me try it. My Merchant was ſo taken with it, " that he deſir'd me to ſhew it to a Friend of his, a " noted

" noted Profeſſor of the Mathematicks in the College " of Jeſuits there. He was all Surprize at the regu- " lar Motion of the Machine under Water, and more " that it ſhould ſo nicely determine the Diſtance " fail'd of any Ship or Boat. I ſhould ſwell my Let- " ter to too great a Bulk, ſhould I repeat the Conver- " ſation I had with this Jeſuit, who importun'd me " much to ſee the Inſide of the Clock-Work, offer- " ing me what I pleas'd for a Sight of it. In a word, " I was deaf to him, and many other Gentlemen of " the Town, who crouded to me every Day on the " ſame Account, and who were all greatly pleaſed " with the Invention.

" The Machine made by my Directions at *Toulon*, " I us'd in my Way home, and found it to anſwer " very well in the Ocean; from whence ariſes this " Remark, which ſufficiently ſhews the Uſefulneſs of " your Invention, *viz.* That even rough ones, made " by a meer Cobler of a Smith, and turn'd by the " Directions of a ſhort Memory, which I dare not " truſt in many Things, are capable of anſwering the " End for which you invented them.

" It muſt be noted, that though I allow one ſeventh " of the Ship's Diſtance for her Deviation from her " Courſe, yet ſome Ships are ſo built, that they will " ſteer much truer, and others worſe than ours did; " and in this Caſe the *Marine-Surveyor* ſhews its " Worth; for if two Ships are in Company, the one " ſteering well, the other ill, the Latter ſhall have " more Miles than the Former on comparing their " Run, although they ſet out from the ſame Port, and " never part Company.

I ſhould be wanting to my ſelf, if I did not produce the moſt convincing Proof of the Uſefulneſs of this new Invention; I ſhall therefore add to the foregoing

Teſtimonials, ſome other Certificates whereby it will further appear, that the *Marine Surveyor* has the Preference of the *Log*, *viz.*

" We the underwritten Maſters of Ships, *&c.* do " certify all whom it may concern, That this 21ſt of " *October* 1725, we accompanied Mr. *Henry de Sau-* " *marez* on board the *Richard* Yatch, in order to " make an Experiment of an Inſtrument invented by " him, call'd the *Marine-Surveyor*; and as by it he " propos'd to aſcertain the Way of a Ship in the Sea, " much more correctly than by any Thing hitherto " invented for that Purpoſe, we tried it between *Lon-* " *don* and *Graveſend* with the *Log* (which we hove " ſeveral Times) to which it appears to us to have " the Preference; for by its conſtant and regular Mo- " tion, the Ship's Diſtance ſail'd muſt be more exactly " attain'd than by the *Log*; which being hove but " once in an Hour or two, cannot be ſo correct, in " regard the Wind may increaſe or leſſen ſoon af- " ter the *Log* is hove, in ſuch Manner, that it entire- " ly depends on him who chalks the *Log-board* to " allow for it. As therefore very conſiderable Errors " muſt ariſe from thence, if a proper Allowance is " not made for an Increaſe and Decreaſe of Wind; and " as the *Marine-Surveyor* is not ſubject to this, but " keeps a regular Motion, according as the Wind is " more or leſs: We are therefore of Opinion, that " this new Invention is not only an ingenious Con- " trivance in its Kind, but is exactly calculated for " the Ends propoſed. As witneſs our Hands this 21ſt " of *October* 1725.

" Sign'd in the Original { *Michael Hales*, *Benjamin Hutchinſon*, *Joſiah Hales*, *Peter Perchard*, *Robert Gamble*. }

" Theſe

" These are to certify all whom it may concern, " that I *John Harris*, who have us'd the Sea for thir- " ty Years past and who was lately Mate of the " *William* and *Thomas*, bound from *London* to *Canso* " in *America*, was present, when Mr. *Henry de Sau-* " *marez* came on board our Vessel and fix'd an In- " strument at the Stern of her, call'd the *Marine-* " *Surveyor*, invented by him for ascertaining the " Way of a Ship in the Sea, much more correctly " that by the *Log*, or any Method hitherto in Use " for that Purpose: And as he desir'd us to try it " with the *Log*, and to make an impartial Report " whether we found it preferable to the *Log*, or not; " I do hereby, in Justice to that Gentleman, certify, " That we kept our Reckoning both by the *Log* and " this Instrument, and do find it much preferable to " the *Log*, or any Thing that has yet appeared to me " for attaining the Ship's Distance sail'd; the Truth " of which I am ready to testify on Oath, if call'd on " to do it. In witness whereof, I have hereunto set " my Hand this 15th of *November*, 1725.

" Sign'd in the } *Robert Gamble*,
" Presence of } *Elisha Dobree*, *John Harris.*

It may perhaps be asked, how I came to produce a Certificate from the Mate, and not from the Captain of the *William* and *Thomas*? To which I answer, that the Mate left the Ship at *Plymouth*, and came to Town, so that I had an Opportunity of obtaining his Opinion of it, without the Captain's, who, soon after his Arrival in *England*, made the best of his Way to the Island of *Guernsey*: However, as I had desir'd him to try my Instrument with the *Log*, and impartially report to me, whether he found it preferable, or not, to that Method of obtaining the Ship's Distance sail'd;

he

he favour'd me with a Letter from thence: His Name is *Thomas Picot*, and his Letter bears Date the 16th of *November* 1725; it is in *French*, and the Substance of it in *English*, is as follows, *viz.*

" That he had made use of the *Marine-Surveyor* in " his Voyage to *Canso* in *America*, and had been more " than ordinarily careful therein, in order to make a " just Report of it; that he had tried it upon a *Me-* " *ridian* with good Observations, and found it to an- " swer his Expectation, and to be preferable to the " *Log*, particularly in rough and stormy Weather; " that it had been much admir'd by several Masters " of Ships, and particularly by Captain *St. Loe*, of his " Majesty's Ship the *Ludlow-Castle*, who express'd a " great Liking to it. He concludes his Letter with " wishing I had an Opportunity to peruse his Journ- " als, whereby it would fully appear how much my " Invention is preferable to the *Log*.

Being inform'd, that Captain *Henry Daniell* had come over as a Passenger from *Canso* in *America*, to *England*, in the aforesaid Vessel *William* and *Thomas*; and being willing to obviate every Objection that might be brought against the *Marine-Surveyor*, I applied my self to that Gentleman for his Opinion of it, who was pleased to send me the following Certificate.

" *THESE are to certify all whom it may concern, that I* Henry Daniell, *who* " *have been at Sea upwards of twelve Years, first as a Voluntier, and af-* " *terwards as a Midshipman, did lately come over as a Passenger in the* William " *and* Thomas, *from* Canso *to* Plymouth, *in which Vessel there was an In-* " *strument fix'd at the Stern of her, call'd the* Marine-Surveyor, *invented by* " *Mr.* Henry de Saumarez, *for ascertaining the Way of a Ship in the Sea; and* " *as that Gentleman has applied to me for my Opinion of it, I do hereby certify,* " *that we found it much more correct than the* Log; *and that in a Gale of* " *Wind, our Reckoning by it agreed with our Observation, which the Reckoning* " *by the* Log *seldom did. And I must, in Justice to that Gentleman, say, that* " *we kept our Reckoning both by his Instrument and the* Log, *and found it* " *much preferable thereto, or to any other Method for obtaining the Ship's Di-* " *stance. In witness whereof, I have hereunto set my Hand this 4th of* De- " cember 1725. H. Daniell.

F I N I S.

AN

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