PHILOSOPHICAL TRANSACTIONS.

PART I.

A

PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXI. For the Year 1781.

PART I.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

MDCCLXXXI.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

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

It

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

A

SPEECH

DELIVERED TO

THE ROYAL SOCIETY,

ON WEDNESDAY NOVEMBER 30, 1780,

BEING THEIR ANNIVERSARY.

BY JOSEPH BANKS, ESQ.
PRESIDENT.

PRINTED BY ORDER OF THE COUNCIL.

THE Emotions of Gratitude inſpired by the very Place in which, by the Munificence of our Royal Patron, we are now for the firſt Time aſſembled, render it impoſſible for me to neglect the Opportunity which this Seaſon, when ye have been uſed to hear yourſelves addreſſed from the Chair, affords me, of offering my ſmall Tribute of Acknowledgement for a Benefit ſo eminently calculated to promote the Honour and Advancement of this Society.

Eſtabliſhed originally by the Munificence of a Royal Founder; ſoftered and encouraged ſince that Time by every ſucceſſive Monarch who has ſwayed the Britiſh Sceptre, ye have ever proved yourſelves

worthy the Favor of your Royal Protectors. A NEWTON, who pruned his infant Wing under your Auſpices, when his maturer Flights ſoared to Worlds unmeaſurably diſtant, ſtill thought a Place among you an honorable Diſtinction. A NEWTON's immortal Labors, a BOYLE, a FLAMSTEAD, a HALLEY, a RAY, and many others, of whom I truſt it is needleſs to remind you, have made ample Returns for the Patronage of former Monarchs.

But bountiful as the Encouragement ye have received from former Patrons has ever been, the Favors which Science has, through your Interceſſion, received from his preſent Majeſty (whom GOD long preſerve!) have eminently outdone their moſt extenſive Ideas of Liberality. Ample Funds, by Him provided, have enabled you to reward Men of extenſive Knowledge and Ability, for ſpending whole Years in the Service of Science; obſerving twice the Tranſit of the Planet Venus over the Diſk of the Sun. At your Requeſt, the Publick defrayed the Expence of convey-

ing

ing them to the moſt diſtant Parts of the Globe we inhabit, where the purpoſes of their Miſſion, ſo important to the Science of Aſtronomy, could beſt be fulfilled; while ye alone enjoy among your Fellow Academies the Reputation of having both ſent and rewarded them.

And more; thoſe very Donations were ſo liberally planned by that Attention to Science which has ever diſtinguiſhed His preſent Majeſty's Reign, and will for ever bear Teſtimony of his enlarged Mind, and Diſpoſition favorable to the Advancement of true Knowledge, that the Surplus alone enabled you, with his Royal Approbation, to inſtitute Experiments on the Attraction of Mountains, amidſt the barren and bleak Precipices of the Highlands of Scotland, which then, for the firſt Time, beheld Inſtruments of the niceſt Conſtruction tranſported to the Summits of their pathleſs Crags, and Men, uſed to other Habitations, voluntarily reſiding in temporary Huts, eager to expreſs a grateful Senſe of their Royal Patron's Liberality,

rality, by thus promoting to the utmoſt the Cauſe of Science, in which they were, under his Protection, embarked.

Gifts like theſe, unſolicited and unconditionally beſtowed, might have ſatisfied the Impulſes even of a Princely Munificence; but not ſo with our Royal Patron. Amply informed in every Branch of real Knowledge, He reſolved to beſtow a ſtill more diſtinguiſhed Mark of his Favor on Science which he loved, and in this his laſt beſt Gift has fulfilled his Royal Reſolution.

Such a Donation, ſo ſuited to our preſent proſperous and flouriſhing Condition under his Royal Patronage and Protection, is admirably calculated to increaſe the Reſpect, great as it is, which ye have ever received from the Learned of all Europe, placing you at once, in every Point of ſplendid Accommodation, as much above all Foreign Academies, as the Labors of your learned Predeceſſors had raiſed you above them in literary Reputation.

Let

Let then Gratitude to a Sovereign, from whom ye have received ſuch conſpicuous Encouragement, engage you, by an Application to a Promotion of the Sciences ye ſeverally poſſeſs, to deſerve a Continuance of his Royal Favor; to meaſure your future Exertions by the Standard of his princely Liberality; and thus ſhew the World, that ye ſtill are, as ye always have been, worthy the Patronage of your King!

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PHILOSOPHICAL TRANSACTIONS.

I. *Natural History and Description of the* Tyger-cat *of the* Cape of Good Hope. *By* John Reinhold Forster, *LL.D. F. R. and A. S.*

Read November 9. 1780.

FEW tribes of quadrupeds have in Africa more representatives of their different species than that of the CAT. The genus of *Antelopes* may perhaps be excepted, since, to my knowledge, about twenty different *Ghazels* and *Antelopes* are to be met with in Africa; but no more than about eight or nine of the Cat tribe have hitherto been discovered on that continent. However, I know about twenty-one different species of this

great class; and, I suppose, these by no means exhaust this numerous tribe.

The greater and more numerous the different genera of animals are, the more difficult it must be to the natural historian properly to arrange the whole of such an extensive division of animals, especially if they are not equally well known. To form new genera, in order to dispose and arrange them under, is a remedy which increases the evil, instead of curing it. The best method, therefore, which can be devised, is to make great divisions in each genus, comprehending those species which, on account of some common relation or character, have a greater affinity to one another. The genus of Cat, to which the animal belongs we are going to speak of more at large, offers three very easy and natural sub-divisions. The *first* comprehends animals related to the Cat-tribe, with long hair or manes on their necks; *secondly*, such as have remarkable long tails without any marks of a mane on their necks; *lastly*, such as have a brush of hair on the tips of their ears, and shorter tails than the second sub-division. The first might be called in Latin *Feles jubatæ*; the second sub-division should be named *Ælures*; and the third and last, *Lynces*. To the first sub-division the *Lion* and the *hunting Leopard* or Indian *Chittah*, belong. The second sub-division consists of the *Tyger*, the *Panther*, the *Leopard*, the *Ounce*, the *Puma*, the *Jaguar-ete*, the *Jaguara*, the *Ocelot*, the *Gingy* of Congo, the *Marakaya*, the Tyger-cat of the Cape or the *'Nsussi* of *Congo*, the *Tibetan Tyger-cat* which I saw at Petersburg, the common *Bush-cat of the Cape*; and, lastly, the *wild Cat*, and its domestic varieties. To the third division belong the *Lynx*, the *Caracal*, the *Serval*, the *Bay Lynx*, and the *Ghaus* of Professor GULDENSTEDT.

Since

Since it is quite foreign to my purpose to speak of those species which are known already to the naturalist, I confine myself to that species only which hitherto has been imperfectly known to naturalists.

The first notice we had of the Cape Cat is, in my opinion, to be met with in LABAT's *Relation Historique de l'Ethiopie occidentale*, tom. I. p. 177. taken as is supposed from Father CARAZZI. LABAT mentions there the *'Nsussi*, a kind of wild Cat of the size of a Dog, with a coat as much striped and varied as that of a *Tyger*. Its appearance bespeaks cruelty, and its eyes fierceness; but it is cowardly, and gets its prey only by cunning and insidious arts. All these characters are perfectly applicable to the Cape Cat, and it seems the animal is found in all parts of Africa, from Congo to the Cape of Good Hope, in an extent of country of about eleven degrees of latitude. KOLBE, in his *Present State of the Cape Good Hope*, vol. II. p. 127. (of the English edition) speaks of a *Tyger Bush-cat*, which he describes as the largest of all the wild Cats of the Cape-countries, and is spotted something like a Tyger. A skin of this animal was seen by Mr. PENNANT in a furrier's shop in London, who thought it came from the Cape of Good Hope; from this skin Mr. PENNANT gave the first description which could be of any utility to a natural historian *. All the other authors mention this animal in a vague manner. When I and my son touched the second time at the Cape of Good Hope in the year 1775, an animal of this species was offered me to purchase; but I refused buying it because it had a broken leg, which made me apprehensive of losing it by death during the passage from the Cape to London. It was very gentle and tame. It was brought in a

* PENNANT's Synopsis of Quadrupeds, p. 181. first edit.

basket to my apartment, where I kept it above four and twenty hours, which gave me the opportunity of describing it, and of observing its manners and œconomy; as it did to my son that of making a very accurate drawing of it.

After a most minute examination, I found its manners and œconomy perfectly analogous to those of our domestic Cats. It ate fresh raw meat, and was very much attached to its feeders and benefactors: though it had broke the fore-leg by accident, it nevertheless was very easy. After it had been several times fed by me, it soon followed me like a tame favourite Cat. It liked to be stroked and caressed; it rubbed its head and back always against the person's cloaths who fed it, and desired to be made much of. It purred as our domestic Cats do when they are pleased. It had been taken when quite young in the woods, and was not above eight or nine months old; I can, however, positively aver, having seen many skins of full-grown Tyger-cats *, that it had already very nearly, if not quite, attained its full growth. I was told, that the Tyger-cats live in mountainous and woody tracts, and that in their wild state they are very great destroyers of Hares, Rabbits, Yerbuas, young Antelopes, Lambkins, and of all the feathered tribe.

DESCRIPTIO FELIS CAPENSIS.

Felis cauda sub-elongata, annulata; corpore fulvo, supra maculis virgatis infra orbicularibus, auriculis nigris, macula lunata alba.

* These skins, with several others of rare and non-descript animals, I bought at a very considerable expence, and deposited in the British Museum, that valuable national repository of artificial and natural curiosities.

Nsussi.

'Nsiussi. LABAT Ethiopic. occident. tom. I. p. 177.

Tyger Bosch-katten. KOLBE Cape of Good Hope, vol. II. p. 127. (Engl. edit.)

Cape-cat. PENNANT Syn. Quadrup. p. 181. (1st edit.)

Corpus magnitudine *Felis Cati sylvestris* vel paulo majus. In genere supra colore pallide fulvo, subtus e cinereo albo, maculis atris.

(*Pili* apice pallide fulvi, basi albi.)

Caput Rostro magis acuto, quam F. Cati, albo; in labio superiore prope angulum rictus macula orbiculata, nigra. *Nares* nudæ, atræ. *Mystaces* plurium ordinum in labio superiore et supra oculos validæ, albæ et fulvæ. *Oculi* figura fere trianguli sphærici, latere anteriore perpendiculari; *Irides* flavæ. *Pupilla* orbicularis, diurna (nec ut in F. Cato perpendiculari rima lemniscata). *Lingua* retrosum aculeata. *Dentes* acuti ut in cogeneribus.

Linea albida utrinque naso parallela, ad interiora oculorum latera. *Linea nigra* paullulum convergens a cantho anteriore oculorum descendit in nasum; *aliæ duæ nigræ* supra oculos infra convergentes, inque frontem ascendentes; præterea in capite *puncta et lineolæ nigræ* plures sparsæ.

Auriculæ amplæ, longitudine fere capitis, ovatæ, suberectæ, *intus* villosæ, ochroleucæ; *extus* nigræ, macula lunata, transversa alba. *Margo exterior* sacculo membranaceo nudo, lobato.

Corpus ovatum, elegans. *Lineæ atræ* longitudinales quatuor in cervice inter aurium bases orsæ, in *Dorso* interruptæ; *Superiora laterum* obtinent maculæ oblongæ, lineares, obliquæ. *Inferiora laterum* maculis rotundis sparsis. *Abdomen* e cinereo-album, maculis rotundis parvis, sparsis, nigris.

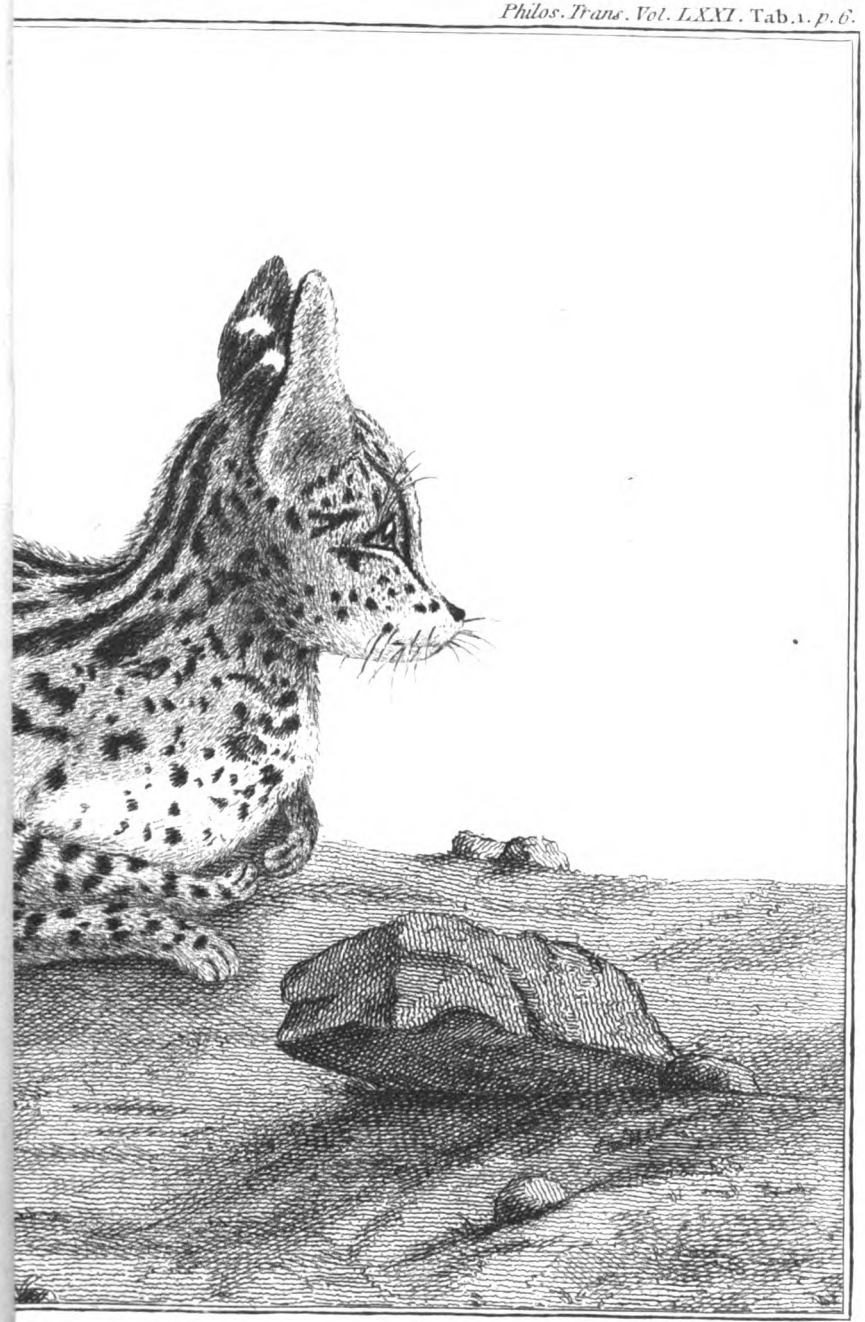
Pedes

Pedes omnes ſuperne ſubfaſciati, extremitatibus punctis numeroſis, nigris conſperſi. *Digiti* quinque felini. *Ungues* modici, retractiles, nigri.

Cauda attingit baſin tarſi, annulis circiter octo vel decem nigris cincta.

M E N S U R Æ.

Ab apice roſtri ad baſin caudæ -	18 unciæ ped. Angl.
Cauda - - -	8
Caput longum - - -	4
Auriculæ margine exteriore -	3
Pedes anteriores a cubito - -	7
Pedes poſtici (tarſi ſcilicet) -	4½



BASIRE. SC.

II. *Experiments and Obſervations on the ſpecific Gravities and attractive Powers of various ſaline Subſtances.* By Richard Kirwan, *Eſq. F. R. S.*

Read November 16, 1780.

THE doctrine of chymical affinities hath of late received great improvements from the labours of the very excellent Mr. BERGMAN of Upſal, and the ſtill later reſearches of Mr. WENTZEL; but the order of theſe attractions has hitherto been the only point attended to by theſe philoſophers, as well as by moſt preceding chymiſts: for I know of none, except Mr. MORVEAU of Dijon, who has thought of aſcertaining the various degrees of force of chemical attraction, by which one body acts on various other bodies, or even on the ſame body in various circumſtances. He has, however, ſo ably ſhewn the advantages ariſing from ſuch an inquiry, that I have made it the object of my attention, and beſtowed much pains on it for ſome time paſt; and have been thereby enabled to determine pretty exactly the proportion of the ingredients of many neutral ſalts, and the ſpecific gravity of the mineral acids in their pureſt ſtate, and free from all water. The principles on which theſe determinations are founded are the following.

1ſt. That the ſpecific gravity of bodies is as their weight, divided by the weight of an equal bulk of rain or diſtilled water, this being at preſent the ſtandard with which every other body is compared.

2dly.

2dly. That if bodies, ſpecifically heavier than water, be weighed in air and in water, they loſe in water part of the weight which they were found to have in air; and that the weight ſo loſt is juſt the ſame as that of an equal bulk of water, and conſequently that their ſpecific gravity is equal to their weight in air, or *abſolute weight*, divided by their loſs of weight in water.

3dly. That if a ſolid, ſpecifically heavier than a liquid, be weighed firſt in air, and then in that liquid, the weight it loſes is equal to the weight of an equal volume of that liquid; and conſequently if ſuch ſolid be weighed firſt in air, then in water, and afterwards in any other liquid, the ſpecific gravity of this liquid will be as the weight loſt in it by ſuch ſolid, divided by the loſs of weight of the ſame ſolid in water. This method of finding the ſpecific gravity of liquids I have found much more exact than that by the areometer, or the compariſon of weights of equal meaſures of ſuch liquids and water, both of which are ſubject to ſeveral inaccuracies.

4thly. That where the ſpecific gravity of bodies is already known, the weight of an equal bulk of water may alſo be found, it being as the quotient of their abſolute weight divided by their ſpecific gravity. This I ſhall call their *loſs of weight* in water.

Hence, where the ſpecific gravity and abſolute weight of the ingredients of any compound are known, the ſpecific gravity of ſuch compound may eaſily be calculated as it ought to be intermediate betwixt that of the lighter and that of the heavier, according to their ſeveral proportions: this I call the *mathematical* ſpecific gravity. But, in fact, the ſpecific gravity of compounds, found by actual experiment, ſeldom agrees with that found by calculation, but is often greater without any diminu-

tion of the lighter ingredient. This increase of density must then arise from a closer union of the component parts to each other than either had separately with its own integrant parts; and this more intimate union must proceed from the attraction or affinity of these parts to each other: I therefore imagined this attraction might be estimated by the increase of density or specific gravity and was proportionable to it, but was soon undeceived.

I must also premise, that the absolute weights of many sorts of air have been accurately determined by Mr. FONTANA, at whose experiments I was present, the thermometer being at 55°, and the barometer at 29¼ inches, or nearly so. Their weights were as follows:

Cubic inch of common air -	0,385
Fixed air - - -	0,570
Marine air - - -	0,654
Nitrous air - - -	0,399
Vitriolic air - - -	0,778
Alkaline air - - -	0,2
Inflammable air - -	0,03

OF SPIRIT OF SALT.

From the time I first read in Dr. PRIESTLEY's Experiments on Air (that inexhaustible source of future discoveries) of the exhibition of marine acid in the form of air, free from water; and that this air, reunited with water, formed an acid liquor in all respects the same as common spirit of salt; I conceived the possibility of discovering the exact quantity of acid in spirit of salt of any given specific gravity, and by means of

 this

this the exact proportion of acid in all other acid liquors; for if a given quantity of pure fixed alkali were saturated, first by a certain quantity of spirit of salt, and then by determined quantities of the other acids, I concluded, that each of these quantities of acid liquor must contain the same quantity of acid, and this being known, the remainder being the aqueous part, this also must be known; but this conclusion intirely rested on the supposition that the same quantity of all the acids was requisite for the saturation of a given quantity of fixed alkali; for if such given quantity of fixed alkali might be saturated by a smaller quantity of one acid than of another, the conclusion fell to the ground. This point might, indeed, be in some measure determined by weighing the neutral salts, formed by these acids, when thoroughly dry; but still a source of inaccuracy remained: for if they were exposed to a considerable heat, part of the acid would necessarily be expelled, and more of one acid than of another, and if the heat were not considerable, much of the water of crystallization would remain; so that if the weights were found to be equal, this equality could not be ascribed to equal quantities of acid, but might perhaps arise from a smaller proportion of acid in one of them, and a larger proportion of water, and in another from a larger proportion of acid and a smaller proportion of water; and if the weights were unequal, no certain conclusion could be drawn. To obviate this difficulty I used the following expedient. 1st. I supposed the quantities of nitrous and vitriolic acids, necessary to saturate a given quantity of fixed alkali, exactly the same as that of marine acid whose quantity I determined; and to prove the truth of this supposition, I observed the specific gravity of the spirit of nitre and oil of vitriol I made use of, and in which I supposed, from the trial with alkalies, a certain proportion of

acid and water; I then added to these more acid and water, and calculated what their specific gravities should be upon the above supposition, and finding the result to tally with the supposition, I concluded the latter to be exact.

The experiments made on the marine acid were as follows.

I took two bottles, which I filled nearly to the top with distilled water, of which they contained in all 1399,9 gr. and introduced them successively into two cylinders filled with marine air, which I had obtained from common salt by means of dilute oil of vitriol and heat, in a mercurial apparatus; and this process I renewed until the water had imbibed, in eighteen days, about 794 cubic inches of the marine air. The thermometer did not rise all this time above 55°, nor sink, unless perhaps at night, under 50°, and the barometer was between 29 and 30 inches. This water, or rather spirit of salt, I then found to weigh 1920 gr. that is 520,1 more than before. The quantity of marine air absorbed amounted then to 520,1 gr. I then examined the specific gravity of this spirit of salt, and found it to be 1,225. Its loss of weight in water (that is, the weight of an equal bulk of water) should then be 1567,346 gr. nearly; but it contained only, as we have seen, 1399,9 gr. of water: therefore subtracting this from 1567,346, the remainder (that is, 167,446) must be the loss of 520,1 gr. of marine acid; and consequently the specific gravity of the pure marine acid, in such a condensed state as it is in when united to water, must be $\frac{520,1}{167,446} = 3,100$. But still it might be suspected, that the density of this spirit did not intirely proceed from the mere density of the marine acid, but in part also from the attraction of this acid to water, and though the length of time requisite to make water imbibe this quantity of acid made me judge that the

attraction was not very considerable, yet the following experiment was more satisfactory.

I exposed 1440 gr. of this spirit to marine air for five days, the thermometer being at 50° or under; it then weighed 1562 gr. and consequently imbibed 122 gr. of marine air; its specific gravity was then 1,253, which agrees exactly with what it should be by calculation.

N. B. I have not repeated the whole of these experiments, as they were very tedious; but I began them over again several times before I could ascertain with any precision the quantity of marine air absorbed, as, when the whole of a cylinder full of air was absorbed, it was difficult to stop the bottles so as to prevent any mercury from falling in; and I was obliged every night to fill the cylinders with air, lest if there remained but a small quantity it might be imbibed before morning, and the mercury fall into the bottles. I also made some allowance for the common air which I could not avoid letting into the cylinder with the marine air, as will be very apparent to whoever repeats the experiment.

Being now satisfied I had discovered the proportion of acid and water in spirit of salt, I was impatient to find it in other acids also; and for that purpose I took 180 gr. of very strong oil of tartar *per deliquium*, but of whose specific gravity I can find no note, and found it to be saturated by 180 gr. of spirit of salt, whose specific gravity was 1,225. Now, by calculation it appears, that 180 gr. of this spirit contains 48,7 gr. of acid and 131,3 of water, and hence I drew up the following table.

Marine

Marine acid.	Water.	Specific Gravity.
Parts.	Parts.	
	50	1,497
	60	1,431
	70	1,381
	80	1,341
	90	1,308
	100	1,282
	110	1,259
	120	1,246
	130	1,223
	140	1,209
	150	1,196
	160	1,185
	170	1,175
	180	1,166
	190	1,158
	200	1,151
	210	1,144
	220	1,138
48,7	230	1,132
	240	1,127
	250	1,122
	260	1,118
	270	1,114
	280	1,110
	290	1,106
	300	1,103
	310	1,100
	320	1,097
	330	1,091
	340	1,089
	350	1,086
	360	1,084
	370	1,082
	380	1,080
	390	1,078
	400	1,076
	410	1,074

The specific gravity of the strongest spirit of salt, made in the usual way, is, according to Mr. BAUME, 1,187, and according to Mr. BERGMAN, 1,190; but we read in the Paris Memoirs for the year 1700, p. 191. that Mr. HOMBERG passed a spirit whose specific gravity was 1,300; and that made by Dr. PRIESTLEY (see vol. III. p. 275.) must have been about 1,500.

Hence we see, that spirit of salt, whose specific gravity is 1,261 or less, has little or no attraction with water, and therefore attracts none from air, and on that account does not heat a thermometer whose ball is dipped in it as spirit of vitriol and spirit of nitre do, as has lately been observed by the Friendly Society of Berlin.

This table is not exactly accurate, as I had not in this first experiment found the point of saturation as nicely as was requisite. However, I have not corrected it, as the error is but small, and the proportion may at any time be found by calculation; at least when the specific gravity of this spirit does not exceed 1,253. Whether the mathematical specific gravity and that by observation differ in the higher degrees of specific gravity, I have not examined; but the table is formed on the supposition that they do not.

Common

Common ſpirit of ſalt is always adulterated with vitrioſic acid, and therefore not fit for theſe trials.

Intending to determine by this experiment the proportion of acid, water, and fixed alkali in digeſtive ſalt, as it is called, I took 100 gr, of a ſolution of a tolerably pure vegetable alkali that had been three times calcined to whiteneſs, the ſpecific gravity of which ſolution was 1,097. I alſo diluted the ſpirit of ſalt with different portions of water; the ſpecific gravity of one ſort was 1,115, and of another 1,098.

I then found that the above quantity of the ſolution of a vegetable alkali required for its ſaturation 27 gr. of that ſpirit of ſalt whoſe ſpecific gravity was 1,098, and 23,35 gr. of that ſpirit of ſalt whoſe ſpecific gravity was 1,115. Now, 27 gr. of ſpirit of ſalt, whoſe ſpecific gravity is 1,098, contain 3,55 gr. of marine acid, as appears by calculation. As the principle on which this calculation, by which the proportion of ſubſtances in alloy is found, may not be generally known, I ſhall here mention them in the words of Mr. COTES.

" The *data* requiſite are the ſpecific gravities of the mixture " and of the two ingredients. . . . . Then, as the difference " of the ſpecific gravities of the mixture and the lighter ingre- " dient is to the difference of the ſpecific gravities of the mix- " ture and the heavier ingredient, ſo is the magnitude of the " heavier to the magnitude of the lighter ingredient. Then, " as the magnitude of the heavier multiplied into its ſpecific " gravity is to the magnitude of the lighter multiplied into its " ſpecific gravity, ſo is the weight of the heavier to the weight " of the lighter . . . . Then, as the ſum of theſe weights " is to the given weight of either ingredient, ſo is the weight " given to the weight of the ingredient ſought."

Thus,

Thus, in this case, 1,098 − 1,000 = ,098 is the magnitude of the heavier ingredient, *viz.* the marine acid; and, 098 × 3,100 = 0,3038 the weight of the marine acid; and, on the other hand, 3,100 − 1,098 = 2,002 the magnitude of the water, and 2,002 × 1,000 = 2,002 its weight; the sum of these weights is 2,3058: then, if 2,3058 parts of spirit of salt contain 0,3038 parts acid, 27 gr. of this spirit of salt will contain 3,55 acid.

In the same manner it will be found, that 23,35 gr. of spirit of salt, whose specific gravity was 1,115, contained 3,55 gr. acid.

The point of saturation was pretty accurately found by putting the glass cylinder which contained the alkaline solution on the scale of a very sensible balance, and at the same time weighing the acid liquor in another pair of scales, when the loss of weight indicated the escape of nearly equal quantities of the fixed air contained in the solution; then the acid was gradually added, by dipping a glass rod into it, to the top of which a small drop of acid adhered: with this the solution was stirred, and very small drops taken up and laid on bits of paper stained blue with radish juice. As soon as the paper was in the least reddened, the operation was completed so that there was always a very small excess of acid, for which half a grain was constantly allowed; but no allowance was made for the fixed air, which always remains in the solution; but as, on this account, only a small quantity of the alkaline solution was used, this proportion of fixed air must have been inconsiderable. If an ounce of the solution had been employed, this inappreciable portion of fixed air would be sufficient to cause a sensible error: for I judged of the quantity of fixed air lost by the difference betwixt the weight added to the 100 gr. and the actual weight of

of the compound. When this difference amounted to 2,2 gr. then I judged the whole of the fixed air expelled, and found it to be so, as 100 gr. of this alkaline solution, being evaporated to dryness in a heat of 300°, left a residuum which amounted to 10½ gr.; which 10½ gr. contained 2,2 gr. of fixed air, as will hereafter be seen.

Hence 8,3 gr. of pure vegetable fixed alkali, free from fixed air and water, or 10,5 of mild fixed alkali, were saturated by 3,55 gr. of pure marine acid, and consequently the resulting neutral salt should, if it contained no water, weigh 11,85 gr.; but the salts resulting from this union (the solution being evaporated to perfect dryness in a heat of 160° kept up for four hours) weighed at a medium 12,66 gr. Of this weight 11,85 gr. were acid and alkali; therefore the remainder, *viz.* 0,81 of gr. were water; therefore 100 gr. of perfectly dry, digestive salt contain 28 gr. acid, 6,55 water, 65,4 of fixed alkali.

I was then curious to compare my experiments with those made by others, but could not find any made with sufficient precision but those of Mr. HOMBERG in the Paris Memoirs for 1699. However, as to spirit of salt I did not think proper to compare them, as he mentions that his could dissolve gold, and therefore was probably impure.

OF SPIRIT OF NITRE.

The common reddish brown or greenish spirit of nitre containing, besides acid and water, a certain portion of phlogiston, and being also mixed with some portion of the acid of sea salt, I judged unfit for these trials, and therefore used only the dephlogisticated sort, which is quite colourless, and resembles pure water in its appearance. This pure acid cannot be made to exist in

in the form of air, as Dr. PRIESTLEY has shewn; for when it is deprived of water and phlogiston, and furnished with a due proportion of elementary fire, it ceases to have the properties of an acid, and becomes dephlogisticated air; I could not, therefore, determine its proportion in spirit of nitre as I had done that of the marine acid, but was obliged to use another method.

1st. To 1963,25 gr. of this spirit of nitre, whose specific gravity was 1,419, I gradually added 179,5 gr. of distilled water, and when it cooled I found the specific gravity of this mixture 1,389.

2dly. To 1984,5 gr. of this I again added 178,75 gr. of water; its specific gravity was then 1,362.

I then took 100 gr. of a solution of fixed vegetable alkali, whose specific gravity was 1,097, the same I had before used in the trials with spirit of salt, and found this quantity of alkali to be saturated by 11 gr. of the spirit of nitre, whose specific gravity was 1,419; and by 12 gr. of the spirit, whose specific gravity was 1,389; and by 13,08 of that, whose specific gravity was 1,362. The quantities here mentioned were the medium of five experiments. I found it necessary to dilute the nitrous acid with a small proportion of water, of which I kept an account. When I neglected this precaution, I found that part of the acid was phlogisticated, and went off with the fixed air. Note also, that after each affusion of acid ten minutes were allowed to the matters to unite, a precaution which I also found absolutely necessary.

Hence (upon the supposition that a given quantity of fixed vegetable alkali is saturated by the same weight of both acids) we see that 11 gr. of spirit of nitre, whose specific gravity is 1,419, contain the same quantity of acid as 27 gr. of spirit of

ſalt, whoſe ſpecific gravity is 1,098, that is, 3,55 gr.; the remainder of 11 gr. is therefore mere water, *viz.* 7,45 gr.; conſequently, if the denſity of the acid and water had not been increaſed by their union, the ſpecific gravity of the pure and mere nitrous acid ſhould be 11,8729; for the ſpecific gravity of this acid ſhould be as its abſolute weight divided by its loſs of weight in water, and this loſs ſhould be as the total loſs of theſe 11 gr. minus the loſs of the aqueous part. Now the total loſs $= \frac{11}{1,419} = 7,749$, and the loſs of the aqueous part $= 7,45$, and conſequently the loſs of the acid part is $7,749 - 7,45 = 0,299$, and therefore the ſpecific gravity of the acid part, that is, of the pure nitrous acid, is $\frac{3,55}{0,296} = 11,8729$.

But it is well known, that the denſity of the nitrous acid, as well as that of the vitriolic, is increaſed by its union with water; and therefore the loſs above found is not the *whole* of its real loſs in its natural ſtate (if it could be ſo found), but partly the loſs that ariſes from the denſity that accrues to it from its union with water: for ſince its denſity is increaſed by this union, its loſs is leſs than it would be if the nitrous acid had only its own proper denſity, and conſequently the ſpecific gravity above found is greater than its real ſpecific gravity.

To determine, therefore, the real ſpecific gravity of this acid in its natural ſtate, the quantity of *accrued* denſity muſt be found, and ſubtracted from the ſpecific gravity of the ſpirit of nitre, whoſe true mathematical ſpecific gravity will then appear. I endeavoured to effect this by mixing different portions of ſpirit of nitre and water, remarking the diminution of their joint volume below the ſum of the ſpaces occupied by their ſeparate volumes; but could never attain a ſufficient degree of preciſion.

precision. The following method, though not exactly accurate, I found more satisfactory. 12 gr. of the spirit of nitre, whose specific gravity by observation was 1,389, contained as I supposed from the former experiment 3,55 gr. of acid, and 8,45 of water; then if the specific gravity of the pure nitrous acid were 11,872, the specific gravity of this compound of acid and water should be 1,371; for the loss of 3,55 gr. acid should be 0,299, and the loss of the water 8,45; the sum of the losses 8,749. $\frac{12}{8,749} = 1,371$; but, as I already said, the specific gravity by observation was 1,389, therefore the *accrued* density in this case was at least ,018, the difference betwixt 1,389 and 1,371. I say *at least*, for as the specific gravity 11,872 was certainly too high, the loss of 3,55 gr. acid was certainly too small; and if it were greater, the mathematical specific gravity 1,371 would have been still lower. However, ,018 is certainly a near approximation to the degree of density that accrues to 3,55 gr. acid by their union to 7,45 gr. of water, and differs inconsiderably from the truth, as will appear by the sequel: therefore subtracting this quantity from 1,419 we have nearly the mathematical specific gravity of that proportion of acid and water, namely, 1,401. And since 11 gr. of this spirit of nitre contain 3,55 gr. acid and 7,45 of water, its loss of weight should be $\frac{11}{1,401} = 7,855$, and subtracting the loss of the aqueous part from this, the remainder 0,405 is the loss of the 3,55 gr. acid, and consequently the true specific gravity of the pure and mere nitrous acid is $\frac{3,55}{0,405} = 8,7654$: this being settled, the mathematical specific gravity and true increase of density of the above mixtures will be found. Thus the mathematical specific gravity of 12 gr. of that spirit of nitre, whose specific gravity by

obſervation was 1,389, muſt be 1,355, ſuppoſing it to contain 3,55 gr. acid and 8,45 of water; for the loſs of 3,55 gr. acid is $\frac{3,55}{8,763} = 0,405$, and the loſs of water 8,45; the ſum of theſe loſſes is 8,855. Then, $\frac{12}{8,855} = 1,355$, and conſequently the accrued denſity is $1,389 - 1,355 = ,034$. In the ſame manner it will be found, that the mathematical ſpecific gravity of 13,08 gr. of that ſpirit of nitre, whoſe ſpecific gravity by obſervation was 1,362, muſt be 1,315, and conſequently its accrued denſity ,047.

But the whole ſtill reſts upon the ſuppoſition that each of theſe portions of ſpirit of nitre contain 3,55 gr. of acid. To verify this ſuppoſition, I could think of no better method than that of examining the mathematical ſpecific gravities of the firſt mixture I had made of ſpirit of nitre and water in large quantities; for if the mathematical ſpecific gravities of theſe agreed exactly with thoſe of the quantities I had ſuppoſed in ſmaller portions of each, I could not but conclude, that the ſuppoſitions of ſuch proportions of acid and water, as I had determined in each, was juſt; and that this was the caſe will appear by the following calculations.

1ſt. When to 1963,25 gr. of ſpirit of nitre, whoſe ſpecific gravity was 1,419, 179,5 gr. of water were added, the quantity of acid upon the above ſuppoſition ſhould be 634,53 gr.; for ∷ 11 . 3,55 :: 1963,25 . 634,53 . the quantity of water in thoſe 1963,25 gr. of ſpirit of nitre ſhould then be 1328,72, and after adding 179,5 gr. of water, the whole quantity of acid and water ſhould be 2142,75, the loſs of acid was $\frac{634.53}{8,7654} =$ 71,24, and the ſum of the loſſes 1580,46: then the mathematical ſpecific

specific gravity should be $\frac{2142,75}{1580,46} = 1,355$, which is exactly the same as that which was found in 12 gr. of this spirit of nitre, on the supposition that they contained 3,55 gr. acid.

Again: when to 1984,5 gr. of this mixture I added 178,75 gr. of water, the whole quantity of diluted spirit of nitre was 2163,25 gr. and the quantity of acid in 1984,5 gr. was 587,081 gr. for :: 12 . 3,55 :: 1984,5 . 587,081; the loss of this quantity of acid is 66,96 gr. and the sum of the losses of acid and water is 1643,129 gr.; and consequently the mathematical specific gravity should be $\frac{2163,75}{1643,125} = 1,315$, which is the same as that determined in 13,08 gr. of the same mixture.

By continuing these mixtures until I found the mathematical specific gravity and that by observation nearly to coincide, I was enabled to draw up the following table, in which if any errors be found, I hope they will be excused, from the impossibility of avoiding them where the weights must be found with such extreme precision: the two first series were only found by analogy.

Spirit

Spirit of nitre.	Acid.	Water.	Accrued density.	Mathematical specific gravity.	Specific gravity by observation	Attract. of the acid to water.	Attract. of water to the acid.
Grs.	Grs.	Grs.					
9	– –	5,45	,000	1,537	1,537	– –	– –
10	– –	6,45	,009	1,458	1,467	,009	,054
11	– –	7.45	,018	1,401	1,419	,018	,045
12	– –	8,45	,034	1,355	1,389	,027	,036
13,08	– –	9,53	,047	1,315	1,362	,036	,027
14,15	– –	10,6	,051	1,286	1,337	,045	,018
15.23	– –	11,68	,054	1,260	1,314	,054	,009
16,305	– –	12,755	,054	1,238	1,292	,054	,009
17,38	– –	13,83	,051	1,220	1,271		
18,445	– –	14,9	,047	1,205	1,252		
19,53	– –	15,98	,044	1,191	1,235		
20,605	– –	17,055	,042	1,180	1,222		
21,68	– –	18,13	,040	1,177	1,217		
22,755	– –	19,205	,038	1,160	1,198		
23,83	– –	20,28	,036	1,152	1,188		
24,905	– –	21,45	,033	1,144	1,177		
26,17	– –	22,62	,030	1,132	1,162		
27,34	3,55	23,79	,027	1,130	1,157		
28,51	– –	24,96	,026	1,124	1,150		
29,68	– –	26,13	,024	1,114	1,138		
30,85	– –	27,30	,022	1,113	1,135		
32,02	– –	28,47	,020	1,109	1,129		
33,09	– –	29,54	,018	1,102	1,120		
34,26	– –	30,71	,016	1,101	1,117		
35,43	– –	31,88	,014	1,097	1,111		
36,60	– –	33,05	,012	1,094	1,106		
37,77	– –	34,22	,010	1,090	1,100		
38,94	– –	35,39	,008	1,088	1,096		
40,11	– –	36,56	,006	1,085	1,091		
41,28	– –	37,73	,004	1,082	1,086		
42,45	– –	38,90	,002	1,080	1.082		

The intermediate specific gravities may be found by taking an arithmetical mean between the specific gravities by observation betwixt which that sought lies, and noting how much it exceeds or falls short of such arithmetical mean; and then taking also an arithmetical mean betwixt the mathematical specific

gravities betwixt which that sought for must lie, and a proportionate excess or defect.

I have added a column of attraction of the nitrous acid to water as far as it keeps pace with the increase of density, but no farther, as I am unacquainted with the law of its further increase.

The specific gravity of the strongest spirit of nitre yet made is, according to Mr. BAUME, 1,500; and according to Mr. BERGMAN, 1,586.

I next proceeded to examine the proportion of acid, water, and fixed alkali in nitre, in the same manner as I had before done that in digestive salt, and found that 100 gr. of perfectly dry nitre contain 28,48 gr. acid, 5,2 of water, and 66,32 of fixed alkali.

I shall now compare the result of these experiments with those of Mr. HOMBERG.

The specific gravity of the spirit of nitre which Mr. HOMBERG made use of was 1,349; and of this, he says, 1 oz. 2 dr. and 36 gr. that is, 621 Troy, are requisite to saturate 1 French (oz. 472,5 Troy) of dry salt of tartar; according to my computation 613 gr. are sufficient; for this specific gravity lies between the tabular specific gravities by observation 1,362 and 1,337, and is nearly an arithmetical mean between them. The corresponding mathematical specific gravity lies betwixt the tabular quantities 1,315 and 1,286, and is nearly 1,300. Now, the proportion of acid and water in this is, 2,629 of acid, and 7,465 of water; for $\frac{8,765}{1,300} = 7,465$ water and $\frac{8,765 \times .300}{} = 2,629$ of acid; and the sum of both is 10,044. Now, since 10,5 gr. mild vegetable fixed alkali require 3,55 gr. of acid for their saturation,

472,5

472,5 will require 159,7; therefore, if 10,044 gr. of nitre contain 2,629 gr. acid, the quantity of this spirit of nitre requisite to give 159,7 will be 613,2 nearly, and hence the difference betwixt us is only about 8 gr.

2dly. Mr. HOMBERG says, he found his salt, when evaporated to dryness, to weigh 186 gr. more than before; whereas, by my experiment, it should weigh but 92,8 gr. more than at first. I shall mention the cause of this difference in treating of tartar vitriolate, for it cannot be intirely attributed to the difference of evaporation.

3dly. Mr. HOMBERG infers, that 1 oz. (that is, 472,5 Troy gr.) of this spirit of nitre contains 141 gr. Troy of real acid: by my computation it contains but 123,08 gr. of real acid. This difference evidently proceeds from his neglecting the quantity of water that certainly enters into the composition of nitre; for he proceeds on this analogy, 621 . 186,6 :: 472,5 . . 141.

The proportion of fixed alkali I have assigned to nitre is fully confirmed by a very curious experiment of Mr. FONTANA's, inserted in ROZIER's Journal for November 1778. This ingenious philosopher decomposed 2 oz. of nitre by distilling it in a strong heat for eighteen hours. After the distillation there remained in the retort a substance purely alkaline, amounting to 10 French dr. and 12 gr. Now 2 French oz. = 944 gr. Troy, and the alkaline matter amounts to 607 gr. Troy; and, according to my computation, 944 gr. of nitre should contain 625 of alkali. So small a difference may fairly be attributed to the loss in transferring from one vessel to another, weighing, filtering, evaporating, &c.

Mr. LAVOISIER, in the Paris Memoirs for the year 1776, has given us, after Dr. PRIESTLEY, the analysis of the nitrous acid.

In

In 2 oz. French measure (= 945 gr. Troy) of spirit of nitre, whose specific gravity was 1,3160, he dissolved 2 oz. and 1 dr. of mercury; the quantity of air obtained during the solution was 190 cubic inches French (= 202,55 English). This air was all nitrous. There remained a white mercurial salt, which, being distilled, afforded 12 cubic inches (= 12,785 English) of air mixed with red vapours, and which differed little from common air. There afterwards arose 224 cubic inches (= 238,56 English) of dephlogisticated air, during the production of which, the mercury was almost revivified, there remaining but a few grains of a yellow sublimate. The 12 inches of air mixed with red vapours arose, he says, from a mixture of 36 cubic inches of nitrous air (= 38,34 English) and 14 of dephlogisticated air (14,91 English); and as the mercury was almost wholly revived, he concludes, these airs arose from the nitrous acid, and formed it; and hence infers, that 16 oz. of this spirit of nitre (= 7560 gr. Troy) contained 13 oz. 7 dr. 36⅓ gr. (that is, 6589 gr. Troy) of water, and consequently only 971 gr. Troy of real acid, and therefore 2 oz. of this spirit of nitre contained but 120 gr. Troy of real acid: but, by my calculation, 2 oz. of this spirit of nitre contained 213 gr. acid; for its mathematical specific gravity is 1,265. The same weight of acid will also be found in it by computing the weight of the volumes of the different airs he himself found it consist of, or at least to afford by its decomposition; for 202,55 cubic inches of nitrous air weigh, by Mr. FONTANA's experiment, 80,8174 gr. Troy, and 238,56 inches of dephlogisticated air weigh 100,1952 gr. Troy, and adding to these the weight of 38,34 inches of nitrous air, and of 14,91 of dephlogisticated air, which made the 12 cubic inches of air mixed with red vapours, we shall find the whole weight of these airs to be 202,181 gr.;

the few grains wanting of 213 gr. may be accounted for from the absorption of the water in which he received the airs, and by allowing for that still remaining in the yellow sublimate.

OF OIL OF VITRIOL.

The oil of vitriol I made use of was not perfectly dephlogisticated; but though pale yet a little inclined to red. It contained some whitish matter, as I perceived by its growing milky on the affusion of pure distilled water. How far this may alter the result of the following experiments I have not tried; but believe it to be as pure as that which is commonly used in all experiments, and therefore the fittest for my purpose.

To 2519,75 gr. of this oil of vitriol, whose specific gravity was 1,819, I gradually added 180 gr. of distilled water, and six hours after found its specific gravity to be 1,771.

To this mixture I again added 178,75 gr. of water, and found its specific gravity, when cooled to the temperature of the atmosphere, to be 1,719; it was then milky.

I then saturated the same quantity of the oil of tartar above mentioned with each of these sorts of oil of vitriol in the manner already mentioned, and found the saturation to be effected (taking the medium of five experiments) by 6,5 gr. of that whose specific gravity was 1,819; by 6,96 gr. of that whose specific gravity was 1,771; and by 7,41 of that whose specific gravity was 1,719.

I was obliged to add a certain proportion of water to each of these sorts of oil of vitriol; for when they were not diluted, I perceived that part of the acid was phlogisticated, and went off with the fixed air; but knowing the quantity of water that was added, it was easy to find, by the rule of proportion, the quantity

tity of each sort of oil of vitriol that was taken up by the alkali.

Hence I supposed, that each of these quantities of oil of vitriol of different densities contained 3,55 gr. of acid, as they saturated the same quantity of vegetable fixed alkali as 11 gr. of spirit of nitre, which contained that quantity of acid.

I then endeavoured to find the specific gravity of the pure vitriolic acid in the same manner as I before had that of the nitrous, as it cannot be had in the shape of air unless united to such a quantity of phlogiston as quite alters its properties. The loss of 6,5 gr. of oil of vitriol, whose specific gravity is 1,819 is $\frac{6,5}{1,819} = 3,572$; but as these 6,5 gr. contained, besides 3,55 gr. acid, 2,95 of water, the loss of this must be subtracted from the intire loss, and then the remainder 0,622 is the loss of the pure *acid* part in that state of density to which it is reduced by its union with water. The specific gravity therefore of the pure vitriolic acid in this state of density is $\frac{3,55}{0,622} = 5,707$. But to find its natural specific gravity we must find how much its density is increased by its union with this quantity of water: and, in order to observe this, I proceeded as before with the nitrous acid. 6,96 gr. of oil of vitriol, whose specific gravity was 1,771, contained 3,55 gr. acid, and 3,41 of water; then its specific gravity by calculation should be 1,726, for the loss of 3,55 gr. acid is $\frac{3,55}{5,707} = 0622$; the loss of 3,41 gr. water is 3,41; the sum of the losses 4,032. Then, $\frac{6,96}{4,032} = 17,261$; therefore the *accrued* density is $1,771 - 1,726 = ,045$. Taking this therefore from 1,819, its mathematical specific gravity will be 1,774, then the loss of 6,5 gr. of oil of vitriol, whose

specific gravity by observation is 1,819, will be found to be $\frac{6,5}{1,774} = 3,664$; but of this 2,95 gr. are the loss of the water it contains, and the remainder 0,714 * are the loss of the mere acid part. Then, $\frac{3,55}{0,715} = 4,9649$ is nearly the true specific gravity of the pure vitriolic acid.

I then found the true increase of density arising from the union of the vitriolic acid and water in the foregoing mixtures, and observed, that in oil of vitriol, whose specific gravity was 1,771, it was 0,84, and in that whose specific gravity was 1,719, it was 0,100.

To obtain a synthetical proof of these deductions, I compared them with the specific gravities of the first mixtures I had made; for if these deductions were true, the mathematical specific gravities, and the *accrued* densities, added to each other, should amount to the same quantity as the specific gravities by observation; and this I found to happen very nearly; for in the first experiment, where 2519,75 gr. of oil of vitriol, whose specific gravity was 1,819, were mixed with 180 gr. of water, that oil of vitriol contained by my calculation 1376,171 gr. of acid and 1143,597 gr. of water, besides the 180 gr. of water that were added to it, the loss of the acid was $\frac{1376,171}{4,964} = 277,22$. The whole quantity of oil of vitriol was 2699,75 gr.; then the sum of the losses was 1600,81; and therefore the mathematical specific gravity $\frac{2699,75}{1600,81} = 1,686$; to which, adding ,084 the degree of accrued density, the specific gravity by observation

* By mistake, the following calculations were made on the supposition that the loss was 0,715; the difference being immaterial, the calculations were not repeated.

should be 1,770, which wants less than 1000th part in 2700 of being just.

Again: in the mixture, whose specific gravity was 1,719 the sum of the losses was 1779,549, and the weight of the whole 2878,4, the mathematical specific gravity should be $\frac{2778,400}{1779,549} = 1,617$, to which adding 0,100, the specific gravity by observation should be 1,717, which is nearly the truth.

By continuing these mixtures until the specific gravities by calculation and observation nearly coincided, I formed the following table. The extra-tabular proportions are to be sought in the manner already shewn; the two first series were formed by [illegible].

Oil and spirit of vitriol.	Acid.	Water.	Accrued density.	Mathematical specific gravity.	Specific gravity by observation	Attract. of the acid to water.	Attract. of water to the acid.
Grs.	Grs.	Grs.					
5,58	– –	2,03	,000	2,032	2,032	,005	0,140
6,04	– –	2,49	,005	1,884	1,889	,045	0,149
6,5	– –	2,95	,045	1,774	1,819	,084	0,139
6,96	– –	3,41	,084	1,687	1,771	0,100	0,137
7,41	– –	3,86	0,100	1,619	1,719	0,112	0,129
7,87	– –	4,32	0,112	1,563	1,675	0,122	0,122
8,33	– –	4,78	0,122	1,515	1,637	0,129	0,112
8,79	– –	5,24	0,129	1,476	1,605	0,137	0,100
9,25	– –	5,70	0,137	1,441	1,578	0,139	,084
9,71	– –	6,16	0,139	1,412	1,551	0,149	,045
10,17	– –	6,62	0,140	1,385	1,525	,0140	,005
10,63	– –	7,08	0,139	1,363	1,502		
11,09	– –	7,54	0,132	1,343	1,475		
11,55	– –	8,00	0,127	1,325	1,452		
12,01	– –	8,46	0,120	1,308	1,428		
12,47	– –	8,92	0,113	1,294	1,407		
12,93	– –	9,38	0,106	1,280	1,386		
13,39	– –	9,84	0,100	1,268	1,368		
13,85	– –	10,30	0,094	1,257	1,351		
14,31	– –	10,76	0,088	1,247	1,335		
14,77	– –	11,22	0,83	1,237	1,320		
15,23	– –	11,68	,078	1,228	1,306		
15,69	3,55	12,14	,074	1,220	1,294		
16,15	– –	12,60	,070	1,212	1,282		
16,61	– –	13,06	,066	1,205	1,271		
17,07	– –	13,52	,062	1,199	1,261		
17,53	– –	13,98	,059	1,191	1,250		
17,99	– –	14,44	,056	1,187	1,243		
18,45	– –	14,90	,053	1,181	1,234		
18,91	– –	15,36	,050	1,176	1,226		
19,37	– –	15,82	,047	1,167	1,214		
19,83	– –	16,28	,044	1,166	1,210		
20,29	– –	16,74	,040	1,162	1,203		
20,75	– –	17,20	,038	1,158	1,196		
21,21	– –	17,66	,035	1,154	1,189		
21,67	– –	18,12	,032	1,150	1,182		
22,13	– –	18,58	,029	1,146	1,175		
22,59	– –	19,04	,026	1,143	1,169		
23,05	– –	19,50	,023	1,140	1,163		
23,51	– –	19,96	,020	1,137	1,157		
23,97	– –	20,42	,018	1,134	1,152		
24,43	– –	20,88	,016	1,131	1,147		
24,89	– –	21,34	,014	1,128	1,142		
25,35	– –	21,80	,012	1,125	1,137		
25,81	– –	22,26	,010	1,123	1,133		
26,27	– –	22,72	,008	1,120	1,128		
26,73	– –	23,18	,006	1,118	1,124		
27,19	– –	23,64	,004	1,116	1,120		
27,65	– –	24,10	,002	1,114	1,116		

The specific gravity of the most concentrated oil of vitriol yet made is, according to Mr. BAUME and BERGMAN, 2,125.

I ascertained the proportion of acid water and fixed alkali in tartar vitriolate, as before, in nitre and digestive salt. I found the salts, resulting from the saturation of the same oil of tartar, with portions of oil of vitriol of different specific gravities, to weigh, at a medium, 12,45 gr. Of this weight only 11,85 gr. were alkali and acid, the remainder therefore was water, *viz.* 0,6 of a grain; consequently 100 gr. of perfectly dry tartar vitriolate contain 28,51 gr. acid, 4,82 of water, and 66,67 of fixed vegetable alkali. Note, in drying this salt I used a heat of 240° to expel the adhering acid more thoroughly. I kept it in that heat a quarter of an hour.

According to Mr. HOMBERG, 1 French oz: (or 472,5 gr. Troy) of dry salt of tartar required 297,5 gr. Troy of oil of vitriol, whose specific gravity was 1,674, to saturate it; but, by my calculation, this quantity of fixed alkali would require *325 gr.*: a difference which, considering our different methods of determining the specific gravity of liquids (his method, *viz.* that by mensuration, giving it always less than mine) the different desiccation of our alkalies, &c. may pass for inconsiderable.

The resulting salt weighed, according to Mr. HOMBERG, 182 gr. Troy above the original weight of the fixed alkali; but by my experiment it should weigh but 87,7 gr. more; for :: 10,5 . 12,45 :: 472,5 . 560,2. It is hard to say how Mr. HOMBERG could find this great excess of weight both in nitre and tartar vitriolate, unless he meant by the *original weight* of the salt of tartar the weight of the mere alkaline part, distinct from the fixed air it contained: and indeed one would be tempted to think, he did make this distinction; for in that

case the excess of weight will be very nearly such as he determined it: for :: 10,5 . 8,3 :: 472,5 . 373,3. Now, the whole weight of his nitre was 560,2, as I have above shewn: then 560,2 − 373,3 = 116,9, which is only 4 gr. more than he determined it.

Hence he inferred, that 1 oz. (472,5 gr. Troy) of this oil of vitriol contains 291,7 gr. of acid. By my computation it contains but 213,3; but it must be considered, he made no allowance for the water contained in tartar vitriolate, and imagined the whole of the increase of weight proceeded from the acid that is united in it to the fixed alkali. Now the aqueous part in 560 gr. of tartar vitriolate amounts to 37 gr. the remaining difference may be attributed to the different degrees of desiccation, &c.

OF THE ACETOUS ACID.

I have made no experiment on this acid; but, by calculating from the experiment of Mr. HOMBERG, I find the specific gravity of the pure acetous acid, free from superfluous water, should be 2,130. It is probable, its affinity to water is not strong enough to cause any irregular increase in its density, at least that can be expressed by three decimals; and hence its proportion of acid and water may always be calculated from its specific gravity and absolute weight.

100 parts of foliated tartar, or (as it should rather be called) acetous tartar, contain well dried 32 of fixed alkali, 19 of acid, and 49 parts of water.

The specific gravity of the strongest concentrated vinegar yet made is 1,069.

It

It is harder to find the point of saturation with the vegetable than with the mineral acids; because they contain a mucilage that prevents their immediate union with alkalies, and hence they are commonly used in too great quantity. They should be used moderately hot, and sufficient time allowed them to unite.

From these experiments it follows:

1st. That fixed vegetable alkalies take up an equal quantity of the three mineral acids, and probably of all pure acids; for we have seen, that 8,3 grains of pure vegetable alkali (that is, free from fixed air) take up 3,55 gr. of each of these acids, and consequently 100 parts of caustic fixed alkali would require 42,4 parts of acid to saturate them. Now, Mr. BERGMAN has found, that 100 parts of caustic fixed vegetable alkali take up 47 parts of the aerial acid, which, considering his alkali might contain some water, differs but little from my calculation. It should therefore seem, that alkalies have a certain determinate capacity of uniting to acids, that is, to a given weight of acids; and that this capacity is equally satiated by that given weight of any pure acid indiscriminately. This weight is about 2,35 of the weight of the vegetable alkali.

2dly. That the three mineral acids, and probably all pure acids, take up 2,253 times their own weight of pure vegetable alkali, that is, are saturated by that quantity.

3dly. That the density accruing to compound substances from the union of their component parts, and exceeding its mathematical ratio, increases from a *minimum*, when the quantity of one of them is very small in proportion to that of the other, to a *maximum*, when their quantities differ less; but that the attraction, on the contrary, of that part which is in the smallest quantity to that which is in the greater, is at its

maximum when the accrued denſity is at its *minimum*, but not reciprocally; and hence the point of ſaturation is probably the *maximum* of denſity and the *minimum* of ſenſible attraction of one of the parts. Hence no decompoſition operated by means of a ſubſtance that has a greater affinity with one part of a compound than with the other, and than theſe parts have to each other, can be complete, unleſs the *minimum* affinity of this third ſubſtance be greater than the *maximum* affinity of the parts already united. Hence few decompoſitions are complete without a double affinity intervenes; and hence the laſt portion of the ſeparated ſubſtance adheres ſo obſtinately to that to which it was firſt united, as all chemiſts have obſerved. Thus, though acids have a greater affinity to phlogiſton than the earths of the different metals have to it, yet they can never totally dephlogiſticate theſe earths but only to a certain degree; ſo though atmoſpheric air, and particularly dephlogiſticated air, attracts phlogiſton more ſtrongly than the nitrous acid does; yet not even dephlogiſticated air can deprive the nitrous acid totally of its phlogiſton, as is evident from the red colour of the nitrous acid when nitrous air and dephlogiſticated air are mixed together. Hence alſo mercury precipitated from its ſolution in any acid, even by fixed alkalies, conſtantly retains a portion of the acid to which it was originally united, as Mr. BAYEN has ſhewn; ſo alſo does the earth of allum, when precipitated in the ſame manner from its ſolution; and thus ſeveral anomalous decompoſitions may be explained. Indeed, I have reaſon to doubt, whether mercury does not attract acids more ſtrongly than alkalies attract them.

4thly. That concentrated acids are, in ſome meaſure, phlogiſticated, and evaporate by union with fixed alkalies.

5thly.

5thly. That, knowing the quantity of fixed alkali in oil of tartar, we may determine the quantity of real pure acid in any other acid substance that is difficultly decomposed, as the sedatif acid, and those of vegetables and animals; for 10,5 gr. of the mild alkali will always be saturated by 3,55 gr. of real acid: and reciprocally, the quantity of acid in any acid liquor being known, the quantity of real alkali in any vegetable alkaline liquor may be found.

OF THE SPECIFIC GRAVITY OF FIXED AIR IN ITS FIXED STATE.

Being desirous to know the specific gravity of some substances which are difficultly procured, or at least preserved for any time, free from fixed air, such as fixed and volatile alkalies, I was induced to seek the specific gravity of the former in its fixed state as of an element necessary to the calculation of the latter; it being very evident, that its density, in its fixed state, must be very different from that which it possesses in its fluid elastic state.

I therefore took a piece of white marble, of the purest kind, which weighed 440,25 gr. and weighing it in water, found it to lose 162 gr.; its specific gravity was therefore 2,7175.

Of this marble, reduced to a fine powder, I put 180 gr. into a phial, and expelling the fixed air by the dilute vitriolic acid and heat, I found its quantity amount to 105,28 cubic inches; the thermometer being at 65°, and the barometer between 29 and 30 inches, this bulk of air would, at 55° of FAHRENHEIT, occupy but 102,4 cubic inches; at which temperature, according to the experiment of Mr. FONTANA, a

cubic inch of fixed air (the barometer being at 29°½) would weigh 0,57 of a grain; therefore the weight of the whole quantity of fixed air amounted to 58,368 gr. which is nearly one third of the weight of the marble. At this rate, 100 gr. of the marble contained 32,42 of fixed air.

To determine the proportion of water and calcareous earth, and also the specific gravity of this latter, I put 3009,25 gr. of the same marble finely powdered into a crucible, loosely covered; the crucible and its contents, before calcination, weighed 8394 gr. and after remaining fourteen hours in a white heat I found it to weigh 7067,5 gr. The weight of the crucible alone was 5384,75 gr.; therefore the weight of the lime singly was 1682,75 gr. The marble then lost by calcination 1326,5 gr.; 180 gr. of the marble should then lose 79,343 gr. and 100 gr. should lose 44,08; but of these 44,08, 32,42 were fixed air, as is already seen, therefore the remainder, that is, 11,66 gr. were water, and the quantity of pure calcareous earth in 100 gr. of the marble was 55,92 gr.

I next proceeded to discover the specific gravity of the lime.

Into a brass box, which weighed 607,65 gr. and in the bottom of which a small hole was drilled, I stuffed as much as possible of the finely powdered lime, and then screwed the cover on, and weighed it both in air and water. When immersed in this latter, a considerable quantity of common air was expelled; when this ceased, I weighed it. The result of this experiment was as follows:

Weight

	Gr.
Weight of the box in air -	607,65
Its loss of weight in water -	73,75
Weight of the box and lime in air	1043,5
Weight of the lime singly in air -	435,85
Loss of weight of the box and lime in water	256,5
Loss of weight of the lime singly -	182,3

Hence, dividing the absolute weight of the lime by its loss in water, its specific gravity was found to be 2,3908.

From these *data* I deduced the specific gravity of fixed air in its fixed state; for 100 gr. of marble consist of 55,92 of earth, 32,42 of fixed air, and 11,66 of water; and the specific gravity of the marble is 2,717. Now, the specific gravity of the fixed air, in its fixed state, is as its absolute weight divided by its loss of weight in water; and its loss of weight in water is as the loss of 100 gr. of marble *minus* the losses of the pure calcareous earth and of the water.

Loss of 100 gr. of marble $= \frac{100}{2,717} = 36,8$ gr.

Loss of 55,92 gr. calcareous earth $= \frac{55,92}{2,390} = 23,39$ gr.

Loss of 11,66 gr. water $=$ 11,66

35,05

Then the loss of the fixed air $36,8 - 35,05 = 1,75$; consequently, its specific gravity is $\frac{32,42}{1,75} = 18,52$; by which it appears to be the heaviest of all acids, or even of all bodies yet known, gold and platina excepted.

OF

OF FIXED VEGETABLE ALKALI.

As the manner of conducting the experiments I made on this salt was nearly the same as that I used in the foregoing (except that to find its specific gravity I weighed it in æther instead of water), I shall content myself, to avoid the repetition of tedious calculation, with relating the result of these experiments.

1st. I found that 100 gr. of this alkali contain about 6,7 gr. of earth, which, according to Mr. BERGMAN, is siliceous: this earth passes the filter with it when the alkali is not saturated with fixed air, so that it seems to be held in solution as in *liquor silicum*.

2dly. I found, that the quantity of fixed air in oil of tartar and dry vegetable fixed alkali is various at various times and in various parcels of the same salt; but that at a medium in the purer alkalies it may be rated at 21 gr. in 100; and hence the quantity of this alkali in any solution of it may be very nearly guessed at, by adding a known weight of a dilute acid to a given weight of such solution, and then weighing it again; for as 21 is to 100, so is the weight lost to the weight of mild alkali in such solution.

The specific gravity of mild and perfectly dry four times calcined fixed alkali, free from siliceous earth, and containing 21 *per cent.* of fixed air, I found to be 5,0527.

When it contains more fixed air, its specific gravity is probably higher, except it were not perfectly dry: from whence I inferred the specific gravity of this alkali, when caustic and free from water, to be 4,234.

From the weight of the aerial acid, in its fixed state, it happens, that fixed alkalies, when united to it, are specifically heavier than when united either to the vitriolic or nitrous acids. Thus Mr. R. WATSON, in the Phil. Transf. for the year 1770, p. 337. found the specific gravity of dry salt of tartar (including siliceous earth) to be 2,761: whereas the specific gravity of tartar vitriolate was only 2,636, and that of nitre 1,933. The reason why nitre is so much lighter than tartar vitriolate, is, because it contains much more water, and its union with the alkali is less intimate.

Lastly, I have drawn up a table of the quantity of mild alkali, containing 6,7 *per cent.* of earth (which is its usual degree of purity) to be found in natural or artificial solutions of this alkali, the thermometer at 63°; and though it is not quite accurate, wanting about 1,1 *per cent.* of the truth, yet, I presume, it may be found useful, as this error is easily corrected.

Table

Table of the contents of a solution of mild vegetable alkali, according to its specific gravity.

Gr. of the solution.	Gr. of alkali.	Gr. of water.	Accrued density.	Mathematical specific gravity.	Specific gravity by observation
64,92	– –	38,67	,050	1,445	1,495
70,60	– –	44,35	,049	1,393	1,446
76,28	– –	50,03	,048	1,356	1,404
81,96	– –	55,71	,047	1,324	1,371
87,64	– –	61,39	,046	1,297	1,343
93,32	– –	67,07	,045	1,274	1,319
99,00	– –	72,75	,044	1,254	1,298
104,68	– –	78,43	,043	1,237	1,280
110,36	– –	84,11	,042	1,223	1,265
115,98	– –	89,79	,041	1,209	1,250
121,66	– –	95,4	,040	1,198	1,238
127,34	– –	101,15	,039	1,187	1,226
133,02	– –	106,83	,038	1,178	1,216
138,7	– –	112,51	,037	1,170	1,207
144,3	– –	118,19	,036	1,162	1,198
149,98	– –	123,87	,035	1,155	1,190
155,66	– –	129,55	,034	1,149	1,183
161,34	– –	135,23	,033	1,143	1,176
167,02	26,25	140,91	,032	1,138	1,170
172,70	– –	146,59	,031	1,132	1,163
178,38	– –	152,27	,030	1,128	1,158
184,06	– –	157,95	,029	1,123	1,152
189,74	– –	163,63	,028	1,119	1,147
195,42	– –	169,31	,027	1,115	1,142
201,10	– –	174,99	,026	1,112	1,138
206,78	– –	180,67	,025	1,108	1,133
212,46	– –	186,35	,024	1,105	1,129
218,14	– –	192,03	,023	1,100	1,123
223,82	– –	197,71	,022	1,099	1,121
229,50	– –	203,39	0,21	1,097	1,118
235,18	– –	209,07	,020	1,094	1,114
240,86	– –	214,75	0,19	1,092	1,111
246,54	– –	220,43	,018	1,089	1,107
252,12	– –	226,11	0,17	1,087	1,104
257,80	– –	231,79	,016	1,085	1,101
263,48	– –	237,47	,015	1,083	1,098
269,16	– –	243,15	,014	1,081	1,095

Gr.

Gr. of the solution.	Gr. of alkali.	Gr. of water.	Accrued density.	Mathematical specific gravity.	Specific gravity by observation
274,78	– –	248,83	,013	1,079	1,092
280,46	– –	254,51	,012	1,077	1,089
286,14	– –	260,19	,011	1,076	1,087
291,82	– –	265,87	,010	1,074	1,084
297,50	– –	271,55	,009	1,070	1,079
303,18	– –	277,23	,008	1,069	1,077
308,86	26,25	282,91	,007	1,068	1,075
314,54	– –	288,59	,006	1,066	1,072
319,22	– –	294,27	,005	1,065	1,070
324,90	– –	300,45	,004	1,064	1,068
330,58	– –	306,13	,003	1,063	1,066
336,26	– –	311,81	,002	1,062	1,064
341,94	– –	317,49	,001	1,061	1,062

Impure vegetable fixed alkalies such as pearl ash, pot ashes, &c. contain more fixed air, as appears by the experiments of Dr. LEWIS. Pearl ash, according to Mr. CAVENDISH, contains 28,4 or 28,7 *per cent.* of fixed air. Hence in lyes of equal specific gravity with those of a purer alkali, the quantity of saline matter will be more probably in the ratio of 28,4 or 28,7 to 21; but this surplus weight is only fixed air; and hence even in these lyes the quantity of depurated salt they will afford will be found by the above table. Much also depends on their age, the oldest containing most fixed air.

III. *Account of the violent Storm of Lightning at* East-bourn, *in* Sussex, Sept. 17, 1780; *communicated by* Owen Salusbury Brereton, *Esq. F. R. and A. S.*

Read December 14, 1780.

SIR,

I AM desired by my friend and neighbour JAMES ADAIR, Esq. of Soho Square, to communicate to you an account of the dreadful accident which happened to him and his family at East-bourn, in the county of Sussex, at nine o'clock in the morning, on Sunday the seventeenth of September last. He rented a house which stood by itself, built of various sorts of stone, three stories high, and facing the sea, which was nearly South-east of it. The morning was very stormy, with rain, thunder, and lightning; and just at nine o'clock a horrid black cloud appeared, out of which Mr. ADAIR saw several balls of fire drop into the sea successively, as he was approaching the window in a one-pair of stairs room; and very soon after, as he was standing at it with his hands clasped, and extended open against the middle of the frame, a most violent flash of fire forced his hands asunder, and threw him several yards upon the floor on his back, with both his legs upright in the air, which remained long so fixed. He was very sensible of his situation all the time, but could not open his eyes or speak; nor had he the least power of motion of any of his limbs for a long time. On

help coming in, and examining his cloaths, which were blue cloth, his right sleeve, both of coat and waistcoat, and also shirt, were all torn on the inside of the arm entirely open, as if by a dog, from the shoulder to the wrist; the right-side of the breeches was torn in the same manner, and part of each of the brass buttons melted.

He had in his fob a gold watch with a steel chain; the button which opens it and three other places of the case were melted. The pendant to which the chain is fixed was almost melted through, and much of the steel chain is incorporated with it, as is reciprocally some gold on that part of the steel which was within the fob. The going of the watch had stopped instantaneously, occasioned, as at first appeared by the small pendulum spiral steel spring having been lengthened; not that it was absolutely so, but relatively, respecting the scapement of the watch, the several inner turns being brought closer together.

His right-arm, right-side, and thigh, were miserably scorched, and the flesh torn: the foot of the stocking of his right leg and his shoe were torn in several places between the buckle and the toe-end of the shoe, and one of his toes split almost to the bone; but the buckle, which was a broad silver one, was not the least hurt or even marked, and remained buckled as before. His sleeve-button of gold, in which was platted hair covered with crystal, was broken from its link, and neither hair or crystal have been found since. A key and a pen-knife in his right-side breeches pocket have several marks of fusion upon them.

The frame of the window on which Mr. ADAIR was leaning was little damaged; but every pane of glass so completely smashed, you could scarce perceive it had ever had glass in it. The room was stuccoed and papered, and between the windows hung a large pier glass, which, with much of the stucco, was shivered to pieces, and strewed over the floor. A door opposite the window

was ſhattered to pieces, and the poſts of a bed in a room behind it, and all the bell-wires were deſtroyed.

Under the dining-room Mr. ADAIR was in, on the parlour floor, were his coachman, butler, and footman. The coachman was going to open a glaſs-door to go towards the ſea, and was ſtruck dead. His body was totally black. His cloaths, and the caul of his wig, and cravat, were much torn; but no particular fleſh wound was found. The enamelled face of his ſilver watch was broken to pieces, and the links of his ſteel chain faſtened together.

The footman was dreſſing his hair near a window, when he was thrown dead on the ground. He appeared much ſcorched, bruiſed, and black. He had a very large wound in his ſide which penetrated near his heart; but very little, if any, blood came from it. His buck-ſkin breeches were much torn, and the ſteel of a metal knee-buckle driven through them. The window ſaſh was driven into the room, and a ſtone, about eight inches ſquare, forced out of the wall into the middle of the room, not far from the body. The butler was a yard or two behind the coachman, and going out with a teleſcope in his hand, which was forced in pieces from him, his hat and wig were thrown to ſome diſtance, and he perceived a violent preſſure on his ſkull and on his back, but was no otherwiſe hurt. He had a ſilver watch with a ſilver chain, which received no damage.

In the room over Mr. ADAIR's, a young lady was dreſſing, and her maid attending. They were both driven to a diſtant part of the room, and rendered inſenſible for ſome time, but not hurt. The poſts of the bed ſhe had juſt left were all ſhivered to pieces, and the bell wires deſtroyed, and the chimney thrown down on the roof.

It is to be remarked, that though the bodies of the two ſervants lay unburied from the Sunday till Tueſday, all their limbs were as entirely flexible as thoſe of a living perſon.

Multitudes on the ſhore before the houſe ſaw the meteor dart in a right line over their heads, and break againſt the front of the houſe in different directions, and all agreed the form and flame was exactly like that of an immenſe ſky-rocket.

IV. *An Account of the* Harmattan, *a singular* African *Wind. By* Matthew Dobson, *M. D. F. R. S.; communicated by* John Fothergill, *M. D. F. R. S.*

Read December 7, 1780.

THE Harmattan is a periodical wind which blows from the interior parts of Africa towards the Atlantic Ocean, and possesses such extraordinary properties, as to merit the attention of the naturalist, making a curious and important article in the history and theory of the winds.

The first information I had on this subject was from my friend Mr. NORRIS, who has frequently visited the Coast of Africa, and is a gentleman of an excellent understanding and strict veracity. This information immediately excited my attention; and as Mr. NORRIS was preparing to make another voyage to that part of the world, I desired him to confirm the facts which he had related, by further inquiries, experiments, and observations; and it is from these materials, with which I have been obligingly furnished by Mr. NORRIS, that the following account is drawn up.

On that part of the Coast of Africa which lies between Cape Verd and Cape Lopez, an easterly wind prevails during the months of December, January, and February, which by the Fantees, a nation on the Gold Coast, is called the Harmattan. Cape Verd is in 15° N. latitude, and Cape Lopez in 1° S. latitude, and the coast between these two Capes runs, in an oblique direction,

direction, nearly ſtom W.S.W. to E.S.E. forming a range of upwards of two thouſand one hundred miles. At the Iſles de Los, which are a little to the northward of Sierra Leone, and to the Southward of Cape Verd, it blows from the E.S.E. on the Gold Coaſt from the N.E. and at Cape Lopez and the River Gabon from the N.N.E. This wind is, by the French and Portugueze who frequent the Gold Coaſt, called ſimply the N.E. wind, the quarter from which it blows. The Engliſh, who ſometimes borrow words and phraſes from the Fantee language, which is leſs guttural and more harmonious than that of their neighbours, adopt the Fantee word Harmattan.

The Harmattan comes on indiſcriminately at any hour of the day, at any time of the tide, or at any period of the Moon, and continues ſometimes only a day or two, ſometimes five or ſix days, and it has been known to laſt fifteen or ſixteen days. There are generally three or four returns of it every ſeaſon. It blows with a moderate force, not quite ſo ſtrong as the ſea breeze (which every day ſets in during the fair ſeaſon from the W. W.S.W. and S.W.); but ſomewhat ſtronger than the land wind at night from the N. and N.N.W.

1. *A fog or haze* is one of the peculiarities which always accompanies the Harmattan. The gloom occaſioned by this fog is ſo great, as ſometimes to make even near objects obſcure. The Engliſh fort at Whydah ſtands about the midway between the French and Portugueſe forts, and not quite a quarter of a mile from either, yet very often from thence neither of the other forts can be diſcovered. The ſun, concealed the greateſt part of the day, appears only about a few hours about noon, and then of a mild red, exciting no painful ſenſation on the eye.

As the particles which conſtitute the fog are depoſited on the graſs, the leaves of trees, and even on the ſkin of the negroes,

ſo as to make them appear whitiſh, I recommended to Mr. NORRIS the uſe of a good microſcope, as this might poſſibly diſcover ſomething concerning the nature of theſe particles. "I was prevented," ſays Mr. NORRIS, "by the bad ſtate of "my health from availing myſelf of the microſcope; neither "could I diſcover any thing by the taſte, or by expoſing plates "covered thinly with melaſſes, for when I had dropped an acid "or alkali into the water in which I had diſſolved the melaſſes, "nothing followed to enable me to judge of the nature "of the particles. Surely they cannot be inſects, or ani-"malculæ of inſects? for we have no appearance of any "thing produced from the myriads of them which are depoſited "on the earth. They do not flow far over the ſurface of the "ſea: at two or three miles diſtance from the ſhore the fog is "not ſo thick as on the beach; and at four or five leagues diſ-"tance it is intirely loſt, though the Harmattan itſelf is plainly "felt for ten or twelve leagues, and blows freſh enough to alter "the courſe of the current."

2. *Extreme dryneſs* makes another extraordinary property of this wind. No dew falls during the continuance of the harmattan; nor is there the leaſt appearance of moiſture in the atmoſphere. Vegetables of every kind are very much injured; all tender plants, and moſt of the productions of the garden, are deſtroyed; the graſs withers, and becomes dry like hay; the vigorous ever-greens likewiſe feel its pernicious influence; the branches of the lemon, orange, and lime trees droop, the leaves become flaccid, wither, and, if the harmattan continues to blow for ten or twelve days, are ſo parched as to be eaſily rubbed to duſt between the fingers: the fruit of theſe trees, deprived of its nouriſhment, and ſtinted in its growth, only appears to ripen, for it becomes yellow and dry, without acquiring

acquiring half the usual size. The natives take this opportunity of the extreme dryness of the grass and young trees to set fire to them, especially near their roads, not only to keep those roads open to travellers, but to destroy the shelter which long grass, and thickets of young trees, would afford to skulking parties of their enemies. A fire thus lighted flies with such rapidity as to endanger those who travel: in that situation a common method of escape is, on discovering a fire to windward, to set the grass on fire to leeward, and then follow your own fire. There are other extraordinary effects produced by the extreme dryness of the Harmattan. The covers of books, Mr. NORRIS informs me, even closely shut up in a trunk, and lying among his cloaths, were bent as if they had been exposed to the fire. Houshold furniture is also much damaged: the pannels of doors and of wainscot split, and any veneered work flies to pieces. The joints of a well-laid floor of seasoned wood open sufficiently to lay one's finger in them; but become as close as before on the ceasing of the Harmattan. The seams also in the sides and decks of ships are much injured and become very leaky, though the planks are two or three inches in thickness. Iron-bound casks require the hoops to be frequently driven tighter; and a cask of rum or brandy, with wooden hoops, can scarcely be preserved; for, unless a person attends to keep it moistened, the hoops fly off.

The parching effects of this wind are likewise evident on the external parts of the body. The eyes, nostrils, lips, and palate, are rendered dry and uneasy, and drink is often required, not so much to quench thirst, as to remove a painful aridity in the *fauces*. The lips and nose become sore, and even chapped; and though the air be cool, yet there is a troublesome sensation of prickling heat on the skin. If the Harmattan continues

four or five days, the ſcarf ſkin peels off, firſt from the hands and face, and afterwards from the other parts of the body, if it continues a day or two longer. Mr. NORRIS obſerved, that when ſweat was excited by exerciſe on thoſe parts which were covered by his cloaths from the weather, it was peculiarly acrid, and taſted, on applying his tongue to his arm, ſomething like ſpirit of hart's-horn diluted with water.

As the ſtate of ſalt of tartar placed in the open air, and the quantity evaporated from a given ſurface of water, are obvious proofs of the comparative moiſture or dryneſs of the atmoſphere, I deſired Mr. NORRIS to put the Harmattan to each of theſe teſts; and particularly to moiſten ſalt of tartar *ad deliquium*, and expoſe it to the night air during the time that the Harmattan was blowing. The following is the account of the reſult of theſe experiments. Salt of tartar will not only remain dry during the night as well as in the day; but, when liquified ſo as to run upon a tile, and expoſed to the Harmattan, becomes perfectly dry in two or three hours; and, expoſed in like manner to the night air, will be dry before morning.

With reſpect to evaporation Mr. NORRIS ſays, "I fixed the "tin veſſel, with which you favoured me, on a graſs plat "behind my houſe, upon a ſtand four feet high, and expoſed "by its ſituation moſt part of the day to the ſun, but ſhel-"tered in ſome meaſure from the wind by the houſe."

Day

Day of the Month.	Evaporation of one tenth of an inch.	Thermometer 6 Morn.	1 Noon.	6 Even.	Remarks.
		°	°	°	Light breeze and hazy.
Nov. 27	1½	–	–	–	Ditto and fair weather.
28	2	–	–	–	Hazy with regulat land and sea breezes.
29	2	–	–	–	Ditto.
30	1½	–	–	–	Ditto.
Dec. 1	1½	–	–	–	Fresh breezes and fair.
2	2	76	80	79	Harmattan began to blow moderately.
	Day. Night.				
3	2 1	74	76	75	Harmattan still blowing, but moderately, and the fog not considerable.
4	2½ 1½	75	77	76	Harmattan almost over.
5	2 1	74	76	76	Harmattan over, sea breeze as usual, hazy.
6	2	76	80	78	Light breeze and hazy.
7	2	76	80	78	Ditto.
8	2	76	80	78	

"The thermometer hung in a large warehouse near a window on which the sun never came at that season of the year, "as it had a north aspect, and where little reflected heat came, "a grass plat being before it. When removed into the next "room, which had three windows and a door opening into the "parade, the thermometer usually rose 4° higher than it did in "the warehouse; its general height in the room, from one to "three o'clock, was 84°.

"On the 14th of December, when there was no Harmattan, "the thermometer at noon, on putting it into the evaporating "vessel, rose to 88°; on taking it out, it sunk to 79°, whilst the "moisture on its surface was evaporating; but on exposing it "five minutes to the sun it rose to 102°. On the fifteenth of "December the thermometer, exposed to the wind in my

"room window, but not to the sun, stood at noon at 84°; at "88° in the evaporating vessel; sunk to 80° as the water eva-"porated from its surface; rose in the sun in six minutes to "104°; and, on putting it into the water-jar in my room, "sunk to 76°."

It appears from the preceding experiments made by Mr. NORRIS, that, if the evaporation of the whole year be supposed to go on in the same proportion with what occurred during a short and very moderate return of the Harmattan, the annual Harmattan evaporation would be 133 inches; and if the calculation was made in proportion to the evaporation which occurs during a longer visit from the Harmattan, and a more forcible breeze, the annual Harmattan evaporation would be much more considerable. If the annual evaporation be in like manner calculated, in proportion to the evaporation which took place subsequent to and preceding the Harmattan, the annual evaporation at Whydah on the Gold Coast would be 64 inches, and I have found the annual evaporation at Liverpool to be 36 inches *. These three therefore are in the following proportion; Harmattan 133 inches, Whydah 64 inches, and Liverpool 36 inches.

As the names of things are often derived from some remarkable property in the thing denoted, I desired Mr. NORRIS to inquire into the derivation of the word Harmattan. He found it to be a corruption of *Aherramantah*, the name of that season in which this wind blows. Aherramantah is compounded of *Aherraman*, which in the Fantee language signifies to blow, and *tab*, tallow or grease, with which the natives rub their skins to prevent their growing dry and rough.

* Philosophical Transactions, vol. LXVII. p. 252.

The Harmattan season is in the Dunco language called *Pepeh*, signifying a dry and rough skin.

3. *Salubrity* forms a third peculiarity of the Harmattan. Though this wind is so very prejudicial to vegetable life, and occasions such disagreeable parching effects on the human species, yet it is highly conducive to health. Those labouring under fluxes and intermitting fevers generally recover in an Harmattan. Those weakened by fevers, and sinking under evacuations for the cure of them, particularly bleeding, which is often injudiciously repeated, have their lives saved, and vigour restored, in spite of the doctor. It stops the progress of epidemics: the small-pox, remittent fevers, &c. not only disappear, but those labouring under these diseases when an Harmattan comes on, are almost certain of a speedy recovery. Infection appears not then to be easily communicated even by art. In the year 1770 there were on board the Unity, at Whydah, above 300 slaves; the small-pox broke out among them, and it was determined to inoculate; those who were inoculated before the Harmattan came on got very well through the disease. About seventy were inoculated a day or two after the Harmattan set in; but not one of them had either sickness or eruption. It was imagined, that the infection was effectually dispersed, and the ship clear of the disorder; but in a very few weeks it began to appear among those seventy. About fifty of them were inoculated the second time; the others had the disease in the natural way: an Harmattan came on, and they all recovered, except one girl, who had an ugly ulcer on the inoculated part, and died some time afterwards of a locked jaw. The great salubrity, and the power of checking epidemics, are such extraordinary properties of the Harmattan, that I desired Mr. NORRIS, on his next voyage to the Coast, to ascertain these points by further inquiries.

inquiries. "I have not much new," ſays Mr. NORRIS, "on "theſe points, ſave the general teſtimony of the natives in con- "firmation of what I have already communicated; and that I "had been very ill myſelf for nine days with a remittent fever "this voyage, of which I recovered immediately upon the Har- "mattan beginning to blow; whether from the medicines "which I had taken, or from the alteration in the ſtate of the "atmoſphere, I pretend not to determine. I now learned, for "the firſt time, that the Harmattan is noted for contributing "much to the cure of ulcers, as well as cutaneous eruptions." Mr. NORRIS is ſorry to be obliged to diſſent from ſo reſpectable an authority as that of Dr. LIND, who ſpeaks of the Harmattan as "fatal and malignant; that its noxious vapours are deſtruc- "tive to Blacks as well as Whites; and that the mortality "which it occaſions is in proportion to the denſity and duration "of the fog." The baneful effects here pointed out proceed from the periodical rains which fall in March, April, &c. and which are uſhered in by the Tornadoes, or ſtrong guſts of wind from the N.E. and E.N.E. accompanied with violent thunder and lightning, and very heavy ſhowers. The earth drenched by theſe ſhowers, and acted upon with an intenſe ſolar heat as ſoon as the ſtorm is over, ſends forth ſuch noiſome vapours as ſtrike the noſtrils with a moſt offenſive ſtench, and occaſion bilious vomitings, fluxes, and putrid fevers. Beſides theſe va- pours, which are annual, there appears to be a collection of ſtill more peſtiferous matter, confined for a longer time, and iſſuing from the earth after an interval of five, ſix, or ſeven years. "The periods," ſays Mr. NORRIS, "which I remem- "ber to have been thus marked, were in 1756, when Governor "MELVILL and moſt of the gentlemen and ſoldiers at Cape "Coaſt, died; in 1763, 1769, and 1775. The mortality "in

" in ſome of theſe years, for they were not all equally fatal, " was ſo great that, as Dr. LIND ſays, the living were ſcarce " ſufficient to remove and bury the dead."

It is to be obſerved, that there may be inſtances in which the Harmattan comes loaded with the effluvia of a putrid marſh; and if there are any ſuch ſituations, the nature of the wind may be ſo changed as to become even noxious.

Another inquiry which I deſired Mr. NORRIS to make reſpected the ſource of the Harmattan, and the nature of the ſoil over which it blows. It appears that, except a few rivers and ſome lakes, the country about and beyond Whydah is covered for four hundred miles back with verdure, open plains of graſs, clumps of trees, and ſome woods of no conſiderable extent. The ſurface is ſandy, and below that a rich reddiſh earth; it riſes with a gentle aſcent for one hundred and fifty miles from the ſea before there is the appearance of an hill, without affording a ſtone of the ſize of a walnut. Beyond theſe hills there is no account of any great ranges of mountains.

With reſpect to the origin of this wind, Mr. NORRIS ſays, " the Harmattan, according to Dr. LIND, ariſes from the con- " flux of ſeveral rivers about Benin; but when I was on a " viſit to the King of Dahomey, one hundred and twenty " miles North, or inland from the Fort at Whydah, I there " felt the Harmattan blowing from the N.E. ſtronger than I " have at any other time, though Benin then bore from me " S.E."

On this head Mr. NORRIS makes the following conjecture: " The interſection of three lines, *viz.* an eaſt line drawn from " Cape Verd, a north-eaſt one from the centre of the Gold " Coaſt, and a north line from Cape Lopez, would, I think, " point out a probable ſource of this extraordinary wind."

Three

Three lines, drawn according to the direction of Mr. NORRIS, towards the points of the compaſs from which the Harmattan blows on Cape Verd, the Gold Coaſt, and Cape Lopez, converge I find to a part of Africa about the 15th degree of N. latitude, and the 25th degree of E. longitude, which I alſo find to be that part of Africa where, according to PTOLEMY, the mountains of Caphas are ſituated. From theſe mountains, according to the ſame authority, the river Daradus aroſe, ſuppoſed by ſome to be now the river Senegal.

It may be conjectured, that the diſagreeable Levant wind of the Mediterrannean proceeds from the ſame part of the Continent of Africa; for it prevails during the ſame ſeaſon of the year, and may derive its qualities from the ſurface over which it paſſes.

The laſt article of information with which I have been favoured by Mr. NORRIS, is an account of the manner in which the Fantee nation divide their year.

Aherramantah, from the 1ſt of December to the middle of February, about 10 weeks.

Quakorah, a wind up the coaſt, from S.S.W. to S.S.E. from the middle of February to the firſt week in March, about 3 weeks.

Pempina, or Tornado ſeaſon, part of March, all April, and the greateſt part of May, about 12 weeks.

Abrenama, or the old man's and woman's children, that is, the Pleiades, the rainy ſeaſon, the latter end of May, all June, and to about the 20th of July, 8 weeks.

Atukogan, or five ſtars, that is, Orion, high wind and ſqually, the rains very heavy, to the middle of Auguſt, 3 weeks.

Worrobakorow,

Worrobakorou, or one ſtar, the ceaſing of the rains, about 3 weeks.

Mawurrah, the name of a certain ſtar; cloſe, foggy weather and no breeze, the firſt three weeks in September.

Boutch, no land breeze in this ſeaſon, the wind blows freſh down the Coaſt, about ſix weeks.

Autiophi, or the Croziers; Tornadoes and ſoutherly wind, with ſome rain, generally called the latter rains, about four weeks, to the beginning of December, when the Aherramantah ſeaſon again commences.

V. *Essay on a new Method of applying the Screw.* By Mr. William Hunter, *Surgeon; communicated by Lieutenant General* Melville, *F. R. S.*

Read December 21, 1780.

I HAVE some time ago been led to think, that the screw, which of all the mechanical powers is the most commonly employed in performing motions which require great accuracy, might be applied in a manner which would better answer many intentions than that commonly used. The plan is somewhat similar to NONIUS's division of the circle; but before I explain myself farther it may be proper to lay down a few general rules on which we may found a comparison.

The perfection of any machine consists in accomplishing the end proposed in a manner the most effectual, the most expeditious, and the least cumbersome possible. In order to attain this end the following things are required.

1. That the strength of the several parts of the engine be so adjusted to the force they are intended to exert, as that they shall not break under the weight they ought to counteract, nor yet encumber the motion by a greater quantity of matter than is necessary to give them a proper degree of strength.

2. That the increase of power, by means of the machine, be so regulated, that while the force we can exert is thereby rendered adequate to the effect, it may not be retarded in procuring it more than is absolutely necessary.

3. Th

3. That the machine be as ſimple as is conſiſtent with other conditions.

4. That it be as portable and as little troubleſome in the application as poſſible.

5. That the contrivance be ſuch that the moving power may be applied in ſuch a way as to act to the greateſt advantage; and that the motion ultimately produced may have that direction and velocity which is moſt adapted to the execution of the deſign propoſed by the whole.

6. Of two machines, equal in other reſpects, that deſerves the preference in which the friction leaſt diminiſhes the effect propoſed by the whole.

It will eaſily appear, that ſome of theſe conditions, if carried to an extreme, will be inconſiſtent with ſome of the others. Here the proper medium conſiſts in adapting them to each other in ſuch a manner, as that the reſult of the advantages of both may be the greateſt, and that of the defects the leaſt, that is poſſible.

The following method of applying the ſcrew, I think, may in certain caſes be attended with ſome of theſe advantages to a greater degree than by thoſe commonly practiſed.

Let AB (fig. 1.) be a plate of metal in which the ſcrew CD plays, having a number of threads in an inch equal to a. Within the ſcrew CD there is a female ſcrew, by which is received the ſmaller ſcrew DE of $a + 1$ threads in an inch. This ſcrew is retained from moving round along with the ſcrew CD by means of the apparatus at AFGB.

Now, if the handle CKL be turned a times round the ſcrew, CD will advance upwards an inch, and if we ſuppoſe the ſcrew DE to move round along with CD, the point E will alſo advance an inch. If we now turn the ſcrew DE a times backwards, the

point E will move downwards $\frac{a}{a+1}$ of an inch, and the result of both motions will be to lift the point E upward $(1 - \frac{a}{a+1} =)$ $\frac{1}{a+1}$ of an inch. But if, while the screw CD is turned a times round, DE be kept from moving, the effect will be the same as if it had moved a times round with CD and been a times turned back, that is, it will advance $\frac{1}{a+1}$ of an inch. At one turn therefore of the handle CKL it will move upwards $(\frac{1}{a+1} \times \frac{1}{a} =)$ $\frac{1}{a^2+a}$ of an inch. If then we suppose the handle CKL to be b inches long, the power gained by the machine will be as $\overline{a^2+a} \times 6{,}2832\, b$ to unity.

To illustrate this by a particular example, let the screw CD have 10 threads in an inch, and DE 11: then, while the handle CKL is turned 10 times round, the point D will rise one inch above its former situation. But at 10 turns it can only pass over 10 threads of the screw DE, and consequently it will advance upon that screw $\frac{10}{11}$ths of an inch. The point E therefore must rise $\frac{1}{11}$th of an inch, that the point D may have room to rise a complete inch above its former place: therefore, at one turn of the handle, the point E will rise $\frac{1}{110}$th of an inch; and if the handle be supposed half a foot long, the power, to produce an equilibrium, must be to the weight as 1 to $110 \times 6{,}2832 \times 6 = 4146{,}912$, which is the very number expressed by the general theorem, *viz.* $\overline{a^2+a} \times 6{,}2832\, b$, calling $a = 10$ and $b = 6$.

Now let us compare, according to the rules before laid down, this method of using the screw with the common one. And, first, in order to have the same power by means of the common screw that is exerted by this machine, it must have a number of threads

threads in an inch equal to $a^2 + a$, which would render it too weak to resist any considerable violence. For example, if DC have five threads in an inch, and DE six, and if the handle CKL is a foot in length, the power gained by the engine will be nearly as $(\overline{a^2 + a} \times 6\,b =)$ 2160 to 1; whereas, to have the same force by means of the common screw, it must have 30 threads in an inch, and so must yield under a resistance which the other screw would overcome without any difficulty. Upon this principle, the screw may be applied with advantage in presses of different kinds, by fixing one of the plates of the press to the end of the screw at E.

As to the second requisite, both methods may be equally adapted to it; yet other circumstances will determine us to apply the common screw where a small increase of power is necessary, and the present contrivance, when we stand in need of a greater.

This will follow from the third rule, as in the method now proposed a double number of screws is required, which makes the structure more complicated, occasions more expence, and requires a greater accuracy of construction, since, unless this is attended to, the machine will not move.

However, the machine may, in some cases, answer the fourth intention better than the common one, as the power gained by the additional screw enables us to shorten the handle which will tend to make the whole more portable.

The power is here applied in the same direction as in the common screw, so that both equally answer the first part of the fifth rule; but as to the last, the motion *ultimately* produced, it will depend on particular circumstances which of them is most fit for use in any case. Thus, if the screw DE be intended to carry an index which must turn round at the same time that it rises upwards, the common screw is preferable; for although I can

can ſee a method by which the machine before deſcribed may be made to anſwer this purpoſe, I am almoſt afraid to propoſe it. I mean, that within the ſcrew DE another ſtill ſmaller ſhould be made to play, and be connected with the ſcrew CD, ſo as to move round along with it. It muſt have $a^2 + a + 1$ threads in an inch, and they muſt be in the contrary direction to thoſe of CD, ſo that when they are both turned together, and CD moves upwards, this other one may move downwards. At one turn of the handle this will move upwards $\frac{1}{a^2+a} \times \frac{1}{a^2+a+1} = \frac{1}{a^4+2a^3+2a^2+a}$ of an inch, and at the ſame time will move round in a circular direction. For example, let CD have 5 threads ($= a$) in an inch, DE 6 ($= a1$), and a third ſcrew within DE, but connected with CD ſo as to partake of its motion, 31 ($= a^2 + a + 1$). At one turn of the handle, this ſcrew will riſe upwards $\frac{1}{5} \times \frac{1}{6} \times \frac{1}{31} = \frac{1}{930}$ of an inch; but this appears too complicated for uſe, and the leaſt inaccuracy in the conſtruction would hinder it from moving.

But, on the other hand, if while the point E riſes it is of conſequence that it be kept from going round, the machine under conſideration will beſt anſwer this purpoſe. On this principle it may be uſeful in ſeveral reſpects: fot inſtance, let A (fig. 2.) repreſent a magnifying lens, and let it be moveable upon the ſcrew BC of 16 threads in an inch, which turns within the larger ſcrew CD of 15 threads in an inch, and that again moves within the plate EF in the end of the cylinder GF *. To uſe the inſtrument, fix the object to be magnified upon the pin GL, and then turn the lens A upon the ſcrew BC, till it be

* The ſcrew BC is reſtrained from moving along with CD by the ſmall pillar HK, which ſlides backwards and forwards in a groove in the cylinder GF.

nearly

nearly at the proper diſtance from the pin, and oppoſite to it. You may then adjuſt the diſtance more accurately by turning the ſcrew DC, at each turn of which the lens will recede from, or approach to, the pin $\frac{1}{240}$th of an inch. This it will do and not turn aſide, but ſtill remain oppoſite to the pin LG. A double microſcope might be fitted on in the place of the lens A. The whole inſtrument may be furniſhed with a handle, as at M; or, if larger, it may have three feet to ſtand on a table.

On the laſt principle it muſt be owned, the common ſcrew has the advantage, as two ſcrews will produce more friction than one; and, beſides, in the compound engine there is an additional friction from the piece FG (fig. 1.) upon the pillars between which it moves.

Another caſe in which this machine may be employed is in the micrometer. Thus, let the ſcrew AB (fig. 3) of 50 threads in an inch be turned round by the index C, which moves upon the graduated circle ECD in the direction CD. Within the ſcrew AB is *the ſmaller* one AF of 51 threads in an inch, retained from moving round by the bar GFH. The piece AF is continued to K, where it forms a fine point. To uſe the inſtrument, let it be adjuſted to the teleſcope or microſcope by which you are to view a ſtar, or ſome ſmall object, and let the point K appear juſt to touch one edge of the object. Then turn the index C, and the point K will advance upwards till it appears to cover the other edge of the object, and thus you can determine its ſize. The point K will advance at each complete turn of the index $\frac{1}{2550}$ of an inch; and if the circle be divided into 80 equal parts, one of which, if it is an inch in diameter, will be very obſervable, while the index moves over one of theſe, the point K will advance $\frac{1}{102000}$ of an inch.

 Thus,

Thus, for example, ſuppoſe I am to meaſure the diameter of a nervous fibre in the medullary ſubſtance of the brain, I make the point K appear cloſe to one edge, and turn the index till the ſame point paſs over the fibre, and appear to touch the other edge: I then look on the graduated circle ECD, and perceive that the index C has paſſed over, ſuppoſe, 23,2 diviſions. Hence I conclude the diameter of the fibre to be $23,2 \times \frac{1}{504000} = \frac{1}{8750}$ of an inch, which is nearly the ſize as found by the accurate obſervations of Dr. MONRO. There ſhould be a NONIUS's ſcale on the index which will meaſure to one tenth of a diviſion.

As the index C muſt continue cloſe to the plate ECD, while at the ſame time it turns round the ſcrew AB, which is continually riſing, it muſt be made as in fig. 4. where *a*, *b*, are two ſmall pieces which play in a groove in the ſcrew AB (fig. 3.) while the groove CD (fig. 4.) in the index is filled up by a protuberance of the plate ECD (fig. 3.); the piece below the groove *cd* (fig. 4.) being ſunk into that plate. The whole machinery may be incloſed in a cylinder of braſs reaching from B to L (fig. 3.), ſo that the point of the ſcrew KL may be without it, and the ſides of the cylinder may be open at ECD.

It is farther to be obſerved, that what has been ſaid goes on the ſuppoſition that the point K, in the micrometer, is equally magnified with the object we are to meaſure. But, if this point be placed in the focus of the eye-glaſs of a double microſcope; when it moves it will paſs over, not the object itſelf, but its image, magnified by the object-glaſs. In this caſe, if the object-glaſs magnify the diameter 10 times, while the index paſſes over one diviſion, the point K will paſs over the image of an object, the diameter of which is $\frac{1}{5040000}$ of an inch. As in this mode of application the point K muſt fall between the object

object and eye-glaſs, the ſcrews may be contained within the *fulcrum* by which the microſcope is ſupported.

The machine (fig. 1.) may be applied as a jack to raiſe great weights a little way from the ground, by ſubſtituting two croſs hand-ſpikes for the handle CKL; or a vertical handle may be employed in the following manner. Let A (fig. 5.) be a pinion turned by the handle AB, which we ſuppoſe a foot in length. Let the pinion A have 4 teeth, and move the wheel CD of 16 teeth. The ſcrew EF of 4 threads in an inch is fixed in this wheel, and turns round along with it. Within it plays the ſcrew FG of 5 threads in an inch, and which we ſuppoſe prevented from following the motion of EF: it terminates in ſuch a ſhoulder as that repreſented at G, and being continued to H ends in a foot as in the figure. The whole is incloſed in a ſtrong frame. The pinion A muſt be connected in ſuch a manner with the wheel CD as to riſe within the frame along with it, which may eaſily be done by making its axis play in a piece of wood or metal, which is connected by the end to the ſcrew EF. Or, if this *ſhould* be deemed inconvenient, as the riſing of the pinion muſt raiſe the handle AB, the wheel CD may be hindered from riſing, and at the ſame time turn the ſcrew EF, by a contrivance ſimilar to that uſed with the index C (fig. 3.) in the micrometer. In either caſe, the axis of the pinion ſhould be continued through the oppoſite ſide of the frame, and armed with a heavy fly to regulate the motion. When the machine is to be applied to uſe, the bottom of the frame reſting on the ground, if the body to be lifted is already as high as the top G, that top is applied below it; but if it is cloſe to the ground, we put below it the foot H; then, if the handle AB be turned once round, the wheel CD and ſcrew EF will turn $\frac{1}{4}$ part round, and the point F will riſe ($\frac{1}{4} \times \frac{1}{4} =$) $\frac{1}{16}$th of an inch. The point G or H will

 therefore

therefore be lifted upwards ($\frac{1}{16} \times \frac{1}{5} =$) $\frac{1}{80}$th of an inch. But the end B of the handle AB has described above six feet; therefore the velocity of the point G is to that of the point B as one to ($72 \times 80 =$) 5760. Therefore, if we suppose a man to act at the handle with a force equal to 30 lbs. he may keep in equilibrio a weight of 172800 lbs. But a subduction of perhaps more than one half of this must be made, that he may raise the weight, as the friction of the engine will be considerable. Suppose it to be two-thirds, the effect still remains equal to 57600 lbs. or 25 tons 14 cwts. and 32 lbs.

It will easily appear, that this method of applying the screw may have a place in many other engines, particularly where great accuracy is required; or we want a motion to be performed with great power, while at the same time it need not have any large compass. The few examples given above may serve as a specimen.

Fig. 5.

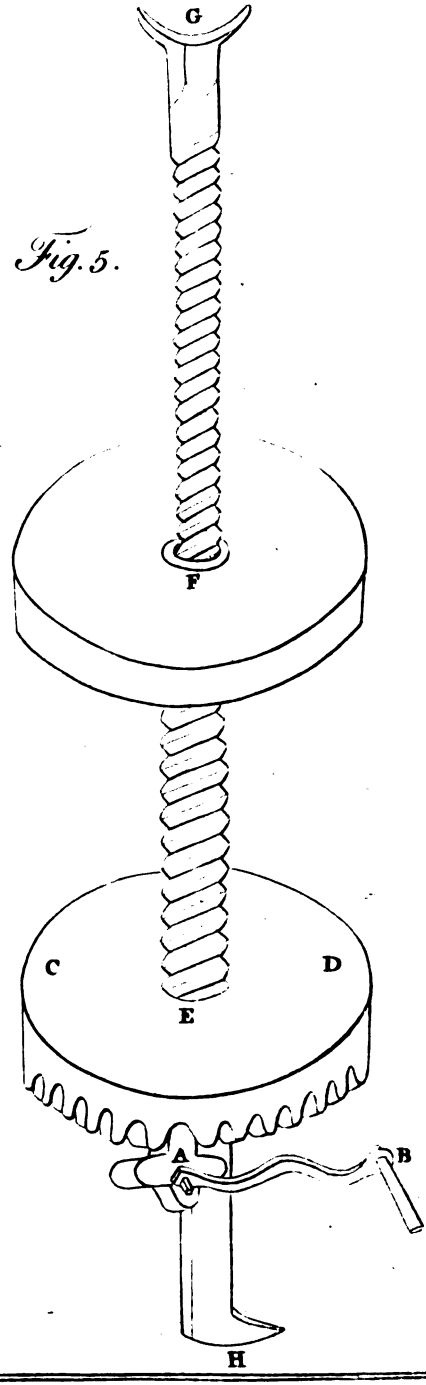
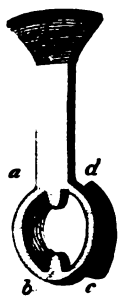


Fig. 4.



VI. *An Account of the* Turkey. *By* Thomas Pennant, *Esq. F. R. S.*; *communicated by* Joseph Banks, *Esq. P. R. S.*

Read December 21, 1781.

TURKEY. BILL convex, short and strong.

HEAD and neck covered with a naked tuberose flesh, with a long fleshy appendage hanging from the base of the upper mandible.

ON the breast a long tuft of coarse black hairs.

Wild Turkey. JOSSELYN's Voy. 99. Rarities 8. CLAYTON's Virgin: Phil. Trans. abridged, III. 590. LAWSON, 149. CATESBY Topp. XLIV.

Le coq d'Inde, BELON 248.

Gallo-pavo, GESNER Av. 481. Icon. 56.

Gallo-pavo, ALDROV. Av. 11. 18.

Gallo-pavo, the Turkey, A. 3. Gallo-pavo sylvestris Novæ Angliæ, a New England wild Turkey, RAII Synopsis Avium 51.

Meleagris Gallo-pavo. M. capite caruncula frontali gularique, maris pectore barbato, LIN. Syst. 268.

Le Dindon de BUFFON III. BRISSON. I. 158. tab. XVI. Pl. Enl. 97.

Description. T. with the characters described in the definition of the genus. The plumage, dark glossed with variable copper colour, and green. Coverts of the wings and the quill feathers barred with black and white. Tail consists of two orders. The upper or shorter very elegant, the ground colour a bright bay; the middle feather marked with numerous bars of shining black and green. The greatest part of the exterior feathers of the same ground with the others marked with three broad bands of mallard green, placed remote from each other. The two next are coloured like those of the middle; but the end is plain and crossed with a single bar, like the exterior.

Tail.

The longer or lower order are of a rusty white colour, mottled with black; and crossed with numerous narrow-waved lines of the same colour, and near the end with a broad band.

Wild Turkies preserve a sameness of colouring; the tame, as usual with domestic animals, vary. It is needless to point out the differences in so well known a bird: the black approaches nearest to the original stock. This variety I have seen nearly in a state of nature in Richmond and other parks. A most beautiful kind has of late been introduced into England of a snowy whiteness, finely contrasting with its red head. These, I think, came out of Holland, probably bred from an accidental white pair; and from them preserved pure from any dark or variegated birds.

White variety.

Size.

Size. The ſizes of the wild Turkies have been differently repreſented. Some writers aſſert, that there have been inſtances of their weighing ſixty pounds; but I find none who, ſpeaking from their own knowlege, can prove their weight to be above forty. JOSSELYN ſays, that he has eaten part of a cock, which after it was plucked, and the entrails taken out, weighed thirty *. LAWSON, whoſe authority is unqueſtionable, ſaw half a Turkey ſerve eight hungry men for two meals †; and ſays, that he had ſeen others which he believed weighed forty pounds. CATESBY tells us, that out of the many hundreds which he had handled §, very few exceeded thirty pounds; each of theſe ſpeak of their being double that ſize merely from the reports of others.

Manners. The manners of theſe birds are as ſingular as their figure. Their attitudes in the ſeaſon of courtſhip are very ſtriking. The males fling their heads and neck backwards, briſtle up their feathers, drop their wings to the ground, ſtrut and pace moſt ridiculouſly; wheel round the females with their wings ruſtling along the earth, at the ſame time emitting a ſtrange ſound through their noſtrils

Notes. not unlike the *Grurr* of a great ſpinning wheel. On being interrupted they fly into great rages, and change their notes into a loud and guttural gobble,

* New England Rarities, p. 8.

† Hiſtory of Carolina, p. 149. and 27.

§ App. XLIV. The greateſt certain weight is given by Mr. CLAYTON; who ſaw one that reached 38 lbs. Phil. Tranſ.

and

and then return to dalliance. The ſound of the female is plaintive and melancholy.

Iraſcible. The paſſions of the males are very ſtrongly expreſſed by the change of colours in the fleſhy ſubſtance of the head and neck, which alters to red, white, blue, and yellowiſh, as they happen to be affected. The ſight of any thing red excites their choler greatly.

Polygamous. They are polygamous, one cock ſerving many hens. They lay in the ſpring, and produce a great number of eggs. They will perſiſt in laying for a great while. They retire to ſome obſcure place to ſit, the cock through rage at the loſs of its mate being very apt to break the eggs. The females are very affectionate to their young, and make great moan on the loſs of them. They ſit on their eggs with ſuch perſeverance, that if they are not taken away when addle, the hens will almoſt periſh with hunger before they will quit the neſt.

Turkies greatly delight in the ſeeds of nettles; but thoſe of the purple-fox glove prove fatal to them *.

Turkies are very ſtupid birds, quarrelſome, and cowardly. It is diverting to ſee a whole flock attack the common cock, who will, for a long time, keep a great number at bay.

Swift. They are very ſwift runners in the tame as well as the wild ſtate: they are but indifferent flyers.

Perch high. They love to perch on trees, and gain the height

* DE BUFFON.

they wish by rising from bough to bough. In a wild state they get to the very summit of the loftiest trees, even so high as to be beyond the reach of the musquet *.

Gregarious. In the state of nature they go in flocks even of five hundred †, feed much on the small red acorns, and grow so fat in *March* that they cannot fly more than three or four hundred yards, and are soon ran down by a horseman. In the unfrequented parts bordering on the *Missisipi*, they are so tame as to be shot with even a pistol ‡.

Haunts. They frequent the great swamps § of their native country, and leave them at sun-rising to repair to the dry woods in search of acorns and berries; and before sun-set retire to the swamps to roost.

The flesh of the wild Turkey is said to be superior in goodness to the tame, but redder. Eggs of the former have been taken from the nest, and hatched under tame Turkies. The young will still prove wild, perch separate, yet mix and breed together in the season. The Indians sometimes use the breed produced from the wild as decoy birds to seduce those in a state of nature within their reach ||.

* LAWSON, 45.

† ADAIR's Amer. 360.

‡ LAWSON, 149.

§ It is in the swamps that the loftiest and most bulky trees are. The wet with which they are environed makes them a secure retreat.

|| LAWSON, 149.

Wild

Wild Turkies are now grown most excessively rare in the inhabited parts of *America*, and are only found in numbers in the distant and most unfrequented spots.

The Indians make a most elegant cloathing of the feathers. They twist the inner webs into a strong double thread of hemp, or inner bark of the mulberry tree, and work it like matting; it appears very rich and glossy, and as fine as a silk shag *. They also make fans of the tail; and the French of *Louisiana* were wont to make umbrellas by the junction of four of the tails †.

When disturbed, they do not take wing, but run out of sight. It is usual to chase them with dogs, when they will fly and perch on the next tree. They are so stupid or so insensible of danger, as not to fly on being shot at; but the survivors remain unmoved at the death of their companions ‡.

Place. Turkies are natives only of *America*, or the New World, and of course unknown to the ancients. Since both these positions have been denied by some of the most eminent naturalists of the sixteenth century, I beg leave to lay open, in as few words as possible, the cause of their error.

Mistaken by BELON. BELON §, the earliest of those writers who are of opinion that these birds were natives of the

* LAWSON, 18. ADAIR, 423.

† DU PRATZ, II. 85.

‡ DU PRATZ, 224.

§ Hist. des Oys. 248.

old world, founds his notion on the deſcription of the Guinea fowl, the Meleagrides of STRABO, ATHENÆUS, PLINY, and others of the ancients. I reſt the refutation on the excellent account given by ATHENÆUS, taken from CLYTUS MILESIUS, a diſciple of ARISTOTLE, which can ſuit no other than that fowl. "They want," ſays he, "natural affection towards their young; their head "is naked, and on the top is a hard round body "like a peg or nail: from their cheeks hangs "a red piece of fleſh like a beard. It has no "wattles like the common poultry. The feathers are black, ſpotted with white. They have "no ſpurs; and both ſexes are ſo like as not to be "diſtinguiſhed by the ſight." VARRO * and PLINY † take notice of the ſpotted plumage and the gibbous ſubſtance on the head. ATHENÆUS is more minute, and contradicts every character of the Turkey, whoſe females are remarkable for their natural affection, and differ materially in form from the males, whoſe heads are deſtitute of the callous ſubſtance and whoſe heels (in the males) are armed with ſpurs.

ALDROVANDUS; and GESNER.

ALDROVANDUS, who died in 1605, draws his arguments from the ſame ſource as BELON; I therefore paſs him by, and take notice of the greateſt of our naturaliſts GESNER ‡, who falls into a miſtake of another kind, and wiſhes the

* Lib. III. c. 9.
† Lib. X. c. 26.
‡ Av. 481.

Turkey to be thought a native of India. He quotes ÆLIAN for that purpose, who tells us, "That in India are very large poultry not with "combs, but with various coloured crests inter-"woven like flowers, with broad tails neither "bending nor displayed in a circular form, which "they draw along the ground as peacocks do "when they do not erect them; and that the "feathers are partly of a gold colour, partly "blue, and of an emerald colour" *.

This in all probability was the same bird with the Peacock Pheasant of Mr. EDWARDS, *Le Paon de Tibet* of M. BRISSON, and the *Pavo bicalcaratus* of LINNÆUS. I have seen this bird living. It has a crest, but not so conspicuous as that described by ÆLIAN; but it has those striking colours in form of eyes, neither does it erect its tail like the Peacock †, but trails it like the Pheasant. The *Catreus* of STRABO ‡ seems to be the same bird. He describes it as uncommonly beautiful and spotted, and very like a Peacock. The former author § gives a more minute account of this species, and under the same name. He borrows it from CLITARCHUS, an attendant of ALEXANDER the Great in all his conquests. It is evident from his description, that it was of this kind; and it is likewise probable, that it was the same with his large Indian

* De Anim. lib. XVI. c. 2.

† Edw. II. 67.

‡ Lib. XV. p. 1046

§ De Anim. lib. XVII. c. 23.

poultry

poultry before cited. He celebrates it also for its fine note; but allowance must be made for the credulity of ÆLIAN. The *Catreus*, or Peacock Pheasant, is a native of Tibet, and in all probability of the north of India, where CLITARCHUS might have observed it; for the march of ALEXANDER was through that part which borders on Tibet, and is now known by the name of *Penj-ab* or five rivers.

Not natives of Europe;

I shall now collect from authors the several parts of the world where Turkies are unknown in the state of nature. Europe has no share in the question; it being generally agreed that they are exotic in respect to that continent.

nor of Asia;

Neither are they found in any part of Asia Minor, or the Asiatic Turky, notwithstanding ignorance of their true origin first caused them to be named from that empire. About Aleppo, capital of Syria, they are only met with, domesticated like other poultry *. In Armenia they are unknown, as well as in Persia; having been brought from Venice by some Armenian merchants into that empire †, where they are still so scarce as to be preserved among other rare fowl in the royal menagery ‡.

DU HALDE acquaints us, that they are not natives of China; but were introduced there from other

* RUSSEL, 63.

† TAVERNIER, 146.

‡ BELL's Travels, I. 128.

countries. He errs from misinformation in saying that they are common in India.

I will not quote GEMELLI CARERI, to prove that they are not found in the Philippine Islands, because that gentleman with his pen travelled round the world in his easy chair, during a very long indisposition and confinement * in his native country.

But DAMPIER bears witness that none are found in Mindanao ‡.

nor Africa; The hot climate of Africa barely suffers these birds to exist in that vast continent, except under the care of mankind. Very few are found in Guinea, except in the hands of the Europeans, the negroes declining to breed any on account of the great heats §. PROSPER ALPINUS satisfies us, they are not found either in Nubia or in Egypt. He describes the Meleagrides of the ancients, and only proves that the Guinea hens were brought out of Nubia, and sold at a great price at Cairo ‖; but is totally silent about the Turkey of the moderns.

Let me in this place observe, that the Guinea hens have long been imported into Britain. They were cultivated in our farm-yards; for I discover in 1277, in the Grainge of Clifton, in the

* Sir JAMES PORTER'S Obs. Turkey, I. 1. 321.

‡ BARBOT in CHURCHILL'S Coll. V. 29.

§ BOSMAN, 229.

‖ Hist. Nat. Ægypti. I. 201.

parish of Ambrosden in Buckinghamshire, among other articles, six *Mutilones* and six *Africanæ fœminæ* *, for this fowl was familiarly known by the names of Afra Avis and Gallina Africana and Numida. It was introduced into Italy from Africa, and from Rome into our country. They were neglected here by reason of their tenderness and difficulty of rearing. We do not find them in the bills of fare of our ancient feasts †; neither do we find the Turkey: which last argument amounts to almost a certainty, that such a hardy and princely bird had not found its way to us. The other likewise was then known by its classical name; for that judicious writer Doctor CAIUS describes, in the beginning of the reign of ELIZABETH, the Guinea fowl, for the benefit of his friend GESNER, under the name of Meleagris, bestowed on it by ARISTOTLE ‡.

Having denied, on the very best authorities, that the Turkey ever existed as a native of the old world, I must now bring my proofs of its being only a native of the new, and of the period in which it first made its appearance in Europe.

but of America,

The first precise description of these birds is given by OVIEDO, who in 1525 drew up a summary of his greater work, the History of the

* KENNET's Parochial Antiq. 287.

† Neither in that of GEORGE NEVIL nor among the delicacies mentioned in the Northumberland houshold book begun in the beginning of the reign of HENRY VIII.

‡ CAII Opusc. 13. Hist. An. lib. VI, c. 2.

Indies,

Indies, for the use of his monarch CHARLES V.

the islands, This learned man had visited the West Indies and its islands in person, and payed particular regard to the natural history. It appears from him, that the Turkey was in his days an inhabitant of the greater islands, and of the main-land. He speaks of them as Peacocks; for being a new bird to him, he adopts that name from the resemblance he thought they bore to the former. "But," says he, "the neck is bare of feathers, "but covered with a skin which they change "after their phantasie into diverse colours. They "have * a horn as it were on their front, and "haires on the breast †." He describes other birds which he also calls Peacocks. They are of the gallinaceous genus, and known by the name of Curassao birds, the male of which is black, the female ferruginous.

The next who speaks of them as natives of the main-land of the warmer parts of America, is FRANCISCO FERNANDEZ, sent there by PHILIP II. to whom he was physician. This naturalist

Mexico, observed them in Mexico. We find by him, that the Indian name of the male was Huexolotl, of the female Cihuatotolin. He gives them the title of Gallus Indicus and Gallo Pavo. The Indians, as well as Spaniards, domesticated these useful birds. He speaks of the size by comparison, saying, that the wild were twice the magnitude of

* In the Spanish Peçon corto.

† In PURCHAS, III. 995.

the tame; and that they were shot with arrows or guns*. I cannot learn the time when FERNANDEZ wrote. It must be between the years 1555 and 1598, the period of PHILIP's reign.

Darien. PEDRO DE CIESA mentions Turkies on the Isthmus of Darien†. LERY, a Portuguese author, asserts, that they are found in Brazil, and gives them an Indian name‡; but since I can discover no traces of them in that diligent and excellent naturalist MARCGRAVE, who resided long in that country, I must deny my assent. But the former is confirmed by that able and honest navigator DAMPIER, who saw them frequently, as well wild as tame, in the province of Yucatan§, now reckoned part of the kingdom of Mexico.

Yucatan.

North America. In North America they were observed by the very first discoverers. When RENÉ DE LAUDONNIERE, patronized by Admiral COLIGNI, attempted to form a settlement near the place where Charlestown now stands, he met with them on his first landing in 1564, and by his historian has represented them with great fidelity in the fifth plate of the recital of his voyage‖: from his time the witnesses to their being natives of the continent are innumerable. They have been seen in flocks of hundreds in all parts from Louisiana

* Hist. Av. Nov. Hisp. 27.

† Seventeen Years Travels, 20.

‡ In DE LAET's Descr. des Indes, 491.

§ Voyages, vol. II. part II. p. 65. 85. 114.

‖ DE BRY.

even

even to Canada; but at this time are extremely rare in a wild ſtate, except in the more diſtant parts, where they are ſtill found in vaſt abundance.

When firſt introduced into Europe.

It was from Mexico or Yucatan that they were firſt introduced into Europe; for it is certain, that they were imported into England as early as the year 1524, the 15th of HENRY VIII *.

We probably received them from Spain, with which we had great intercourſe till about that time. They were moſt ſucceſsfully cultivated in our kingdom from that period; inſomuch, that they grew common in every farm-yard, and became even a diſh in our rural feaſts by the year 1585; for we may certainly depend on the word of old TUSSER, in his Account of the Chriſtmas Huſbandlie Fare †.

Beefe, Mutton, and Porke, ſhred pies of the beſt,
Pig, Veale, Gooſe, and Capon, and Turkie well dreſt,
Cheeſe, Apples, and Nuts, jolie carols to heare,
As then in the countrie, is counted good cheare.

But at this very time they were ſo rare in France, that we are told, that the very firſt which was eaten in that kingdom appeared at the nuptial feaſt of CHARLES IX. in 1570 ‡.

* BAKER'S Chr. ANDERSON'S Dict. Com. I. 354. HACKLUYT, II. 165. makes their introduction about the year 1532. BARNABY GOOGE, one of our early writers on Huſbandry, ſays, they were not ſeen here before 1530. He highly commends a Lady HALES of Kent, for her excellent management of theſe fowl, p. 166.

† Five Hundred Points of good Huſbandrie, p. 57.

‡ ANDERSON'S Dict. Comm. I. 410.

To

To this account I beg leave to lay before you the very extraordinary appearance on the thigh of a Turkey, bred in my poultry yard, and which was killed a few years ago for the table. The ſervant in plucking it was very unexpectedly wounded in the hand. On examination the cauſe appeared ſo ſingular, that the bird was brought to me. I diſcovered, that from the thigh-bone iſſued a ſhort upright proceſs, and to that grew a large and ſtrong toe, with a ſharp and crooked claw, exactly reſembling that of a rapacious bird.

VII. *Account of a* Nebula *in* Coma Berenices. *By* Edward Pigott, *Esq.* *In a Letter to* Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

Read January 11, 1781.

REV. SIR,

Frampton-house, Glamorganshire, September 3, 1779.

AS my father generally addresses to you such papers as he communicates to the Royal Society, I beg the favour of you to acquaint that learned body, that, on the 23d of March, I discovered a nebula in the constellation of Coma Berenices, hitherto, I presume, unnoticed; at least not mentioned in M. DE LA LANDE's Astronomy, nor in M. MESSIER's ample Catalogue of nebulous Stars. I have observed it in an achromatic transit instrument, three feet long, and deduced its mean R. A. by comparing it to the following stars, having made the necessary corrections for aberration and nutation, the results are:

	°	′	″
By δ Gemini - - - -	191	28	35
By γ Canis Majoris - - -	191	28	41
By ε Virginis - - - -	191	28	45
By ν Virginis - - - -	191	28	36
By δ Leonis - - - -	191	28	34
Mean R. A. of the nebula for April 20, 1779,	191	28	38

Its

Its light being exceedingly weak, I could not ſee it in the two-feet teleſcope of our quadrant, ſo was obliged to determine its declination likewiſe by the tranſit inſtrument. This determination, however, I believe, may be depended upon to two minutes: hence its declination north is 22° 53″¼. The diameter of this nebula I judged to be about two minutes of a degree.

I am, &c.

E. PIGOTT.

VIII. *Double Stars discovered in* 1779, *at* Frampton-house, Glamorganshire. *By* Nathanael Pigott, *Esq. F. R. S. Foreign Member of the Academies of* Brussels *and* Caen, *and Correspondent of the Royal Academy of Sciences at* Paris; *communicated by* Nevil Maskelyne, *D. D. F. R. S. and Astrononomer Royal.*

Read January 11, 1781.

TO THE REV. NEVIL MASKELYNE.

REV. SIR, October 27, 1779.

INCLOSED are the determinations of the places of three double stars, which I discovered this summer; at least, I presume, they have not been observed before, hecause I do not find them inserted in Dr. BRADLEY's catalogue, published in the Nautical Almanac 1773, or in the *Connoissance des Tems*, no more than in other catalogues in my possession. γ Delphini, indeed, is in M. DE LA CAILLE's catalogue; but not as a double star. The instrument he used was not, probably, powerful enough for that purpose. In the two-feet telescope of my quadrant it appears only as a single star. These stars were observed by me in a three-feet achromatic telescope of a transit instrument, with an object-glass near two inches diameter. The R. A. are nicely determined by several observations, which always agree with each to a fraction of a second in time. The declinations

declinations were deduced from the difference of altitudes between the double ſtars and the known ſtars, to which they were compared, as ſhewn by the graduated ſemi-circle of the tranſit inſtrument, which, being divided to two minutes only, cannot be ſuppoſed to give any great preciſion: however, I believe, their declinations, hence deduced, to be correct to one minute, or one minute and a half.

In obſerving the double ſtar compared to ζ Pegaſi, I found it impoſſible to illuminate the wires of the tranſit inſtrument, without nearly obliterating the ſtar. This difficulty led me to a method, which, as it completely ſucceeded, may, under ſimilar circumſtances, poſſibly be of uſe to others. A perſon illuminating the wires very faintly, ſo that I could juſt perceive the double ſtar, at a ſignal totally withdrew the light: this ſignal I made when the double ſtar was nearly as far diſtant from the firſt wire as the five wires are diſtant from each other, which, in this caſe, was 13″ by the clock. I then counted the ſeconds, and did not fail to ſee the ſtar diſappear a ſecond or two within the time expected. On its diſappearing, I made a ſignal to write down; and then beginning to count again, did the ſame at each wire. I have ſince tried this method with other ſtars, and think they may be thus obſerved, even with greater preciſion than when the wires are illuminated. Hence the troubleſome buſineſs, well known to aſtronomers, of illuminating faint objects, may be removed.

The preceding ſtar of each double ſtar was obſerved on the firſt, third and fifth wires, and the following one on the ſecond and fourth wires; and thus their difference in R. A. in time, converted into parts of a great circle, obtained. Suppoſing the apparent R. A. and App. declinations of α Delphini, β Aquarii, and ζ Pegaſi, as here aſſumed, the places of theſe double ſtars were found to be as follows.

I beg

I beg you to communicate these observations to the Royal Society; and to receive my thanks for those I received in your letter of the 7th instant.

I am, &c.

N. PIGOTT.

September 5, 1779.

App. R. A.				App. declination.		
307°	21′	5″	α Delphini, 3d mag. -	15°	8′	53″ N.
309	6	30	2d or brightest of γ Delphini 4.	15	20	40 N.
	0	9½	diff. R.A. of the 2 stars in γ Delphini.			

Note, both the stars in γ Delphini have the same, or nearly the same, declination. The 1st is of the 6th, the 2d of the 4th mag.

September 19.

319	59	27 +	β Aquarii, 3d mag. -	6	31	34 S.
318	3	21	preceding double star, 5th mag.	7	40	34 S.
	0	11	– diff. R.A. between 1st and 2d of the double star.			

Note, the 1st seemed of the 5th, the 2d of the 7th mag. The 1st is perhaps 6″ or 8″ S. of the following one.

337	36	55 +	ζ Pegasi, 3d mag. -	9	41	24 N.
346	53	36½	double star, 8th, 9th mag -	3	59	17 N.

Note, both the stars of this double star have the same, or nearly the same, R. A.; their difference in declination is 15″ or perhaps 20″.

IX. *An Account of the* Ganges *and* Burrampooter *Rivers. By* James Rennell, *Esq. F. R. S.; communicated by* Joseph Banks, *Esq. P. R. S.*

Read January 25, 1781.

THE * Ganges and † Burrampooter Rivers, together with their numerous branches and adjuncts, intersect the country of Bengal in such a variety of directions, as to form the most compleat and easy inland navigation that can be conceived. So equally and admirably diffused are those natural canals, over a country that approaches nearly to a perfect plane, that, after excepting the lands contiguous to Burdwan, Birboom, &c. (which altogether do not constitute a sixth part of Bengal) we may fairly pronounce, that every other part of the country has, even in the dry season, some navigable stream within 25 miles at farthest, and more commonly within a third part of that distance.

It is supposed, that this inland navigation gives constant employment to 30,000 boatmen. Nor will it be wondered at, when it is known, that all the salt, and a large proportion of the

* The proper name of this river in the language of Hindoostan (or Indostan) is Pudda or Padda. It is also named Burra-Gonga, or the Great River; and Gonga, the River, by way of eminence; and from this, doubtless, the European names of the river are derived.

† The orthography of this word, as given here, is according to the common pronunciation in Bengal; but it is said to be written in the Sanscrit language, Brahma-pootar, which signifies the Son of Brahma.

food consumed by ten millions of people are conveyed by water within the kingdom of Bengal and its dependencies. To these must be added, the transport of the commercial exports and imports, probably to the amount of two millions sterling *per annum*; the interchange of manufactures and products throughout the whole country; the fisheries; and the article of travelling *.

These rivers, which a late ingenious gentleman aptly termed sisters and rivals (he might have said *twin* sisters, from the contiguity of their springs), exactly resemble each other in length of course; in bulk, until they approach the sea; in the smoothness and colour of their waters; in the appearance of their borders and islands; and, finally, in the height to which their floods rise with the periodical rains. Of the two, the Burrampooter is the largest; but the difference is not obvious to the eye. They are now well known to derive their sources from the vast mountains of Thibet †; from whence they proceed in opposite directions; the Ganges seeking the plains of Hindoostan (or Indostan) by the west; and the Burrampooter by the east; both pursuing the early part of their course through rugged vallies and defiles, and seldom visiting the habitations of men. The Ganges, after wandering about 750 miles through these mountainous regions, issues forth a deity to the superstitious,

* The embarkations made use of vary in bulk from 180 tons down to the size of a wherry. Those from 30 to 50 tons are reckoned the most eligible for transporting merchandize.

† These are amongst the highest of the mountains of the old hemisphere. I was not able to determine their height; but it may in some measure be guessed, by the circumstance of their rising considerably above the horizon, when viewed from the plains of Bengal, at the distance of 150 miles.

tious, yet gladdened, inhabitant of Hindooſtan*. From Hurdwar (or Hurdoar) in latitude 30°, where it guſhes through an opening in the mountains, it flows with a ſmooth navigable ſtream through delightful plains during the remainder of its courſe to the ſea (which is about 1350 miles) diffuſing plenty immediately by means of its living productions; and ſecondarily by enriching the adjacent lands, and affording an eaſy means of tranſport for the productions of its borders. In a military view, it opens a communication between the different poſts, and ſerves in the capacity of a *military way* through the country; renders unneceſſary the forming of magazines; and infinitely ſurpaſſes the celebrated inland navigation of North America, where the *carrying places* not only obſtruct the progreſs of an army, but enable the adverſary to determine his place and mode of attack with certainty.

In its courſe through the plains, it receives eleven rivers, ſome of which are equal to the Rhine, and none ſmaller than the

* The fabulous account of the origin of the Ganges (as communicated by my learned and ingenious friend C. W. BOUGHTON ROUSE, Eſq.) is, that it flows out of the foot of Beſchan * (from whence, ſay the Bramins, it has its name Padda; that word ſignifying foot in the Sanſcrit language); and that in its courſe to the plains of Hindooſtan it paſſes through an immenſe rock ſhaped like a Cow's-head.

The allegory is highly expreſſive of the veneration which the Hindoos have for this famous ſtream; and no leſs ſo of their gratitude to the Author of Nature for beſtowing it: for it deſcribes the bleſſing as flowing purely from his bounty and goodneſs.

The rock before mentioned has, I believe, never been viſited by any European; and is even allowed by moſt of the natives to bear no reſemblance to the object from whence it is denominated. However, as the effects of ſuperſtition do often long ſurvive the illuſions that gave it birth, the rock or cavern ſtill preſerves the name of *Gowmooky*, or Cow's-head.

* Beſchan is the ſame with Viſtnou, the PRESERVING DEITY.

Thames, besides as many others of lesser note. It is owing to this vast influx of streams, that the Ganges exceeds the Nile so greatly in point of magnitude, whilst the latter exceeds it in length of course by one-third. Indeed, the Ganges is inferior in this last respect, to many of the northern rivers of Asia; though I am inclined to think that it discharges as much or more water than any of them, because those rivers do not lie within the limits of the periodical rains *.

The bed of the Ganges is, as may be supposed, very unequal in point of width. From its first arrival in the plains at Hurd-

* The proportional lengths of course of some of the most noted rivers in the world are shewn nearly by the following numbers:

European Rivers.	
Thames,	1
Rhine,	5¾
Danube,	7
Wolga,	9½
Asiatic rivers.	
Indus,	5½
Euphrates,	8½
Ganges,	9½
Burrampooter,	9½
Nou Kian, or Ava River,	9½
Jennisea,	10
Oby,	10½
Amoor,	11
Lena,	11½
Hoanho (of China),	13½
Kian Keu (of ditto),	15½
African river.	
Nile,	12½
American rivers.	
Missisipi,	8
Amazons,	15¾

war,

war, to the conflux of the Jumnah (the first river of note that joins it) its bed is generally from a mile to a mile and a quarter wide; and, compared with the latter part of its course, tolerably straight. From hence, downward, its course becomes more winding, and its bed consequently wider*, till, having alternately received the waters of the Gogra, Soane, and Gunduck, besides many smaller streams, its bed has attained its full width; although, during the remaining 600 miles of its course it receives many other principal streams. Within this space it is, in the narrowest parts of its bed, half a mile wide, and in the widest, three miles; and that, in places where no islands intervene. The stream within this bed is always either increasing or decreasing, according to the season. When at its lowest (which happens in April) the principal channel varies from 400 yards to a mile and a quarter; but is commonly about three quarters of a mile.

The Ganges is fordable in some places above the conflux of the Jumnah, but the navigation is never interrupted. Below that, the channel is of considerable depth, for the additional streams bring a greater accession of depth than width. At 500 miles from the sea, the channel is thirty feet deep when the river is at its lowest; and it continues at least this depth to the sea, where the sudden expansion of the stream deprives it of the force necessary to sweep away the bars of sand and mud thrown across it by the strong southerly winds; so that the principal branch of the Ganges cannot be entered by large vessels.

About 220 miles from the sea (but 300 reckoning the windings of the river) commences the head of the Delta of the Ganges, which is considerably more than twice the area of that of the Nile. The two westernmost branches, named the

* This will be explained when the windings of the river are treated of.

Coſſimbuzar and Jellinghy Rivers, unite and form what is afterwards named the Hoogly River, which is the port of Calcutta, and the only branch of the Ganges that is commonly navigated by ſhips *. The Coſſimbuzar River is almoſt dry from October to May; and the Jellinghy River (although a ſtream runs in it the whole year) is in ſome years unnavigable during two or three of the dryeſt months; ſo that the only ſubordinate branch of the Ganges, that is at all times navigable, is the Chundnah River, which ſeparates at Moddapour, and terminates in the Hooringotta.

That part of the Delta bordering on the ſea, is compoſed of a labyrinth of rivers and creeks, all of which are ſalt, except thoſe that immediately communicate with the principal arm of the Ganges. This tract, known by the name of the Woods, or Sunderbunds, is in extent equal to the principality of Wales; and is ſo completely enveloped in woods, and infeſted with Tygers, that if any attempts have ever been made to clear it (as is reported) they have hitherto miſcarried. Its numerous canals are ſo diſpoſed as to form a compleat inland navigation throughout and acroſs the lower part of the Delta, without either the delay of going round the head of it, or the hazard of putting to ſea. Here ſalt, in quantities equal to the whole conſumption of Bengal and its dependencies, is made

* The Hoogly River, or weſternmoſt branch of the Ganges, has a much deeper outlet to the ſea than the principal branch. Probably this may be owing to its precipitating a leſs quantity of mud than the other; the quantity of the Ganges water diſcharged here being leſs than in the other in the proportion of one to ſix. From the difficulties that occur in navigating the entrance of the Hoogly River, many are led to ſuppoſe, that the channels are ſhallow. The difficulties, however, ariſe from bringing the ſhips acroſs ſome of the ſand-banks, which project ſo far into the ſea, that the channels between them cannot eaſily be traced from without.

and tranſported with equal facility: and here alſo is found an inexhauſtible ſtore of timber for boat-building. The breadth of the lower part of this Delta is upwards of 180 miles; to which, if we add that of the two branches of the river that bound it, we ſhall have about 200 miles for the diſtance to which the Ganges expands its branches at its junction with the ſea.

It has been obſerved before, that the courſe of this river, from Hurdwar to the ſea, is through an uniform plain, or, at leaſt, what appears ſuch to the eye: for, the declivity is much too ſmall to be perceptible. A ſection of the ground, parallel to one of its branches, in length 60 miles, was taken by order of Mr. HASTINGS; and it was found to have about nine inches deſcent in each mile, reckoning in a ſtraight line, and allowance being made for the curvature of the earth. But the windings of the river were ſo great, as to reduce the declivity on which the water ran, to leſs than four inches *per* mile: and by a compariſon of the velocity of the ſtream at the place of experiment with that in other places, I have no reaſon to ſuppoſe, that its general deſcent exceeds it *.

The medium rate of motion of the Ganges is leſs than three miles an hour in the dry months. In the wet ſeaſon, and during the draining off of the waters from the inundated lands, the current runs from five to ſix miles an hour; but there are inſtances of its running ſeven, and even eight miles, in particular ſituations, and under certain circumſtances. I have an

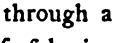
* M. DE CONDAMINE found the deſcent of the river Amazons, in a ſtraight courſe of about 1860 miles, to be about 1020 Engliſh feet, or $6\frac{1}{3}$ inches in a mile. If we allow for the windings, it comes out nearly the ſame as the Ganges (which winds about $1\frac{1}{9}$ mile in three, taking its whole courſe through the plains), namely, about 4 inches in a mile.

experiment

experiment of my own on record, in which my boat was carried 56 miles in eight hours; and that against so strong a wind, that the boat had evidently no progressive motion through the water.

When we consider, that the velocity of the stream is three miles in one season, and five or more in the other, on the same descent of four inches *per* mile; and, that the motion of the inundation is only half a mile *per* hour, on a much greater descent; no further proof is required how small the proportion of velocity is, that the descent communicates. It is then, to the impetus originating at the spring head, or at the place where adventitious waters are poured in, and successively communicated to every part of the stream, that we are principally to attribute the velocity, which is greater or lesser, according to the quantity of water poured in.

In common, there is found on one side of the river an almost perpendicular bank, more or less elevated above the stream, according to the season, and with deep water near it: and on the opposite side a bank, shelving away so gradually as to occasion shallow water at some distance from the margin. This is more particularly the case in the most winding parts of the river, because the very operation of winding produces the steep and shelving banks *: for the current is always strongest on the external side of the curve formed by the serpentine course of the river; and its continual action on the banks

* Hence it is, that the section of a river, that winds through a loose soil, approaches nearly to an obtuse angled-triangle, one of whose sides is exceedingly short and disproportioned to the other two  . But when a river perseveres in a straight course, the section becomes nearly the half of an ellipsis divided longitudinally .

either

either undermines them *, or washes them down. In places where the current is remarkably rapid, or the soil uncommonly loose, such tracts of land are swept away in the course of one season, as would astonish those who have not been eye-witnesses to the magnitude and force of the mighty streams occasioned by the periodical rains of the tropical regions. This necessarily produces a gradual change in the course of the river; what is lost on one side being gained on the other, by the mere operation of the stream: for the fallen pieces of the bank dissolve quickly into muddy sand, which is hurried away by the current along the border of the channel to the point from whence the river turns off to form the next reach, where the stream growing weak, it finds a resting place, and helps to form a shelving bank, which commences at the point, and extends downwards, along the side of the succeeding reach.

To account for the slackness of the current at the point, it is necessary to observe, that the strongest part of it, instead of turning short round the point, preserves for some time the direction given it by the last steep bank, and is accordingly thrown obliquely across the bed of the river to the bay on the opposite side, and pursues its course along it, till the intervention of another point again obliges it to change sides.

In those few parts of the river that are straight, the banks undergo the least alteration †, as the current runs parallel to

* In the dry season some of these banks are more than 30 feet high, and often fall down in pieces of many tons weight, and occasion so sudden and violent an agitation of the water, as sometimes to sink large boats that happen to be near the shore.

† It is more than probable, that the straight parts owe their existence to the tenacity of the soil of which their banks are composed. Whatever the cause may be, the effect very clearly points out such situations as the propereſt for placing towns in.

them;

them; but the leaſt inflection of courſe has the effect of throwing the current againſt the bank; and if this happens in a part where the ſoil is compoſed of looſe ſand, it produces in time a ſerpentine winding.

It is evident, that the repeated additions made to the ſhelving bank before mentioned, become in time an encroachment on the channel of the river; and this is again counter-balanced by the depredations made on the oppoſite ſteep bank, the fragments of which, either bring about a repetition of the circumſtances above recited, or form a bank or ſhallow in the midſt of the channel. Thus a ſteep and a ſhelving bank are alternately formed in the crooked parts of the river (the ſteep one being the *indented* ſide, and the ſhelving one the *projecting*); and thus, a continual fluctuation of courſe is induced in all the winding parts of the river; each meander having a perpetual tendency to deviate more and more from the line of the general courſe of the river, by eating deeper into the bays, and at the ſame time adding to the points, till either the oppoſite bays meet, or the ſtream breaks through the narrow iſthmus, and reſtores a temporary ſtraightneſs to the channel.

Several of the windings of the Ganges and its branches are faſt approaching to this ſtate; and in others, it actually exiſts at preſent. The experience of theſe changes ſhould operate againſt attempting canals of any length, in the higher parts of the country; and I much doubt, if any in the lower parts would long continue navigable. During eleven years of my reſidence in Bengal, the outlet or head of the Jellinghy River was gradually removed three quarters of a mile farther down: and by two ſurveys of a part of the adjacent bank of the Ganges, taken about the diſtance of nine years from each other, it appeared that the breadth of an Engliſh mile and a half had been

taken

taken away. This is, however, the moſt rapid change that I have noticed; a mile in ten or twelve years being the uſual rate of incroachment, in places where the current ſtrikes with the greateſt force, namely, where two adjoining reaches approach neareſt to a right angle. In ſuch ſituations it not unfrequently excavates gulfs * of conſiderable length within the bank. Theſe gulfs are in the direction of the ſtrongeſt parts of the ſtream; and are, in fact, the *young ſhoots* (if I may ſo expreſs myſelf) which in time ſtrike out and become branches of the river; for we generally find them at thoſe turnings that have the ſmalleſt angles †.

Two cauſes, widely different from each other, occaſion the meandering courſes of rivers; the one, the irregularity of the ground through which they run, which obliges them to wander in queſt of a declivity; the other, the looſeneſs of the ſoil, which yields to the friction of the border of the ſtream. The meanders in the firſt caſe, are, of courſe, as digreſſive and irregular as the ſurface they are projected on: but, in the latter, they are ſo far reducible to rule, that rivers of unequal bulk will, under ſimilar circumſtances, take a circuit to wind in, whoſe extent is in proportion to their reſpective breadths: for I have obſerved, that

* The Count DE BUFFON adviſes the digging of ſuch gulfs in the banks of ordinary rivers, with a view to divert the current, when bridges or other buildings are endangered by it.

† The courſes of theſe branches at the efflux, generally, if not always, become retrograde to the courſe of the river: for, a ſand bank accumulating at the upper point of ſeparation, gives an oblique direction upwards, to the ſtream, which would otherwiſe run out at right angles. This ſand bank being always on the increaſe, occaſions a corroſion of the oppoſite bank; and by this means all, or moſt of the outlets have a progreſſive motion downwards; as I have before remarked of the Jellinghy River, in the foregoing page.

when a branch of the Ganges is fallen ſo low as to occupy only a part of its bed, it no longer continues in the line of its old courſe; but works itſelf a new channel, which winds from ſide to ſide acroſs the former one. I have obſerved too, that in two ſtreams, of equal ſize, that which has the ſloweſt current has alſo the ſmalleſt windings: for as theſe (in the preſent caſe) are ſolely owing to the depredations made on the banks by the force of the current; ſo the extent of theſe depredations, or, in other words, the dimenſions of the windings, will be determined by the degree of force acting on the banks.

The windings of the Ganges in the plains, are, doubtleſs, owing to the looſeneſs of the ſoil: and (I think) the proof of it is, that they are perpetually changing; which thoſe, originally induced by an inequality of ſurface, can ſeldom, or never do *.

I can eaſily ſuppoſe, that if the Ganges was turned into a ſtraight canal, cut through the ground it now traverſes in the moſt winding parts of its courſe, its ſtraightneſs would be of ſhort duration. Some yielding part of the bank, or that which happened to be the moſt ſtrongly acted on, would firſt be corroded or diſſolved: thus a bay or cavity would be formed in the ſide of the bank. This begets an inflection of the current, which, falling obliquely on the ſide of the bay, corrodes it inceſſantly. When the current has paſſed the innermoſt part of the bay, it receives a new direction, and is thrown

* It has been remarked, that the courſes of rivers become more winding as they approach the ſea. This, I believe, will only hold good in ſuch as take the latter part of their courſe through a ſandy ſoil. In the Ganges, and other rivers ſubject to conſiderable variations in the bulk of their ſtreams, the beſt marks of the vicinity of the ſea are, the lowneſs of the river banks, and the increaſing muddineſs of the ſhallows in its bed.

obliquely

obliquely towards the oppoſite ſide of the canal, depoſiting in its way the matter excavated from the bay, and which begins to form a ſhallow or bank contiguous to the border of the canal. Here then is the origin of ſuch windings as owe their exiſtence to the nature of the ſoil. The bay, ſo corroded, in time becomes large enough to give a new direction to the body of the canal: and the matter excavated from the bay is ſo diſpoſed as to aſſiſt in throwing the current againſt the oppoſite bank, where a proceſs, ſimilar to that I have been deſcribing, will be begun.

The action of the current on the bank will alſo have the effect of deepening the border of the channel near it; and this again increaſes the velocity of the current in that part. Thus would the canal gradually take a new form, till it became what the river now is. Even when the windings have leſſened the deſcent one half, we ſtill find the current too powerful for the banks to withſtand it.

There are not wanting inſtances of a total change of courſe in ſome of the Bengal rivers *. The Coſa River (equal to the Rhine) once ran by Purneah, and joined the Ganges oppoſite Rajemal. Its junction is now 45 miles higher up. Gour, the ancient capital of Bengal, ſtood on the banks of the Ganges.

Appearances favour very ſtrongly the opinion, that the Ganges had its former bed in the tract now occupied by the lakes and moraſſes between Nattore and Jaffiergunge, ſtriking out of its preſent courſe at Bauleah, and paſſing by Pootyah. With an equal degree of probability (favoured by tradition) we may trace its ſuppoſed courſe by Dacca, to a junction with the Burrampooter or Megna near Fringybazar; where the accumu-

* The Mootyjyl lake is one of the windings of a former channel of the Coſſimbuzar River.

lation of two ſuch mighty ſtreams probably ſcooped out the preſent amazing bed of the Megna *.

In tracing the ſea coaſt of the Delta, we find no leſs than eight openings; each of which, without heſitation, one pronounces to have been in its time the principal mouth of the Ganges. Nor is the occaſional deviation of the principal branch, probably, the only cauſe of fluctuation in the dimenſions of the Delta. One obſerves, that the Deltas of capital rivers (the tropical ones particularly) encroach upon the ſea. Now, is not this owing to the mud and ſand brought down by the rivers, and gradually depoſited, from the remoteſt ages down to the preſent time? The rivers, we know, are loaded with mud and ſand at their entrance into the ſea; and we alſo know, that the ſea recovers its tranſparency at the diſtance of twenty leagues from the coaſt; which can only ariſe from the waters having precipitated their earthy particles within that ſpace. The ſand and mud banks at this time, extend twenty miles off ſome of the iſlands in the mouths of the Ganges and Burrampooter; and in many places riſe within a few feet of the ſurface. Some future generation will probably ſee theſe banks riſe above water, and ſucceeding ones poſſeſs and cultivate them! Next to earthquakes, perhaps the floods of the tropical rivers produce the quickeſt alterations in the face of our globe. Extenſive iſlands are formed in the channel of the Ganges, during a period far ſhort of that of a man's life; ſo that the whole proceſs lies

* Megna and Burrampooter are names belonging to the ſame river in different parts of its courſe. The Megna falls into the Burrampooter; and, though a much ſmaller river, communicates its name to the other during the reſt of its courſe.

within

within the compaſs of his obſervation *. Some of theſe iſlands, four or five miles in extent, are formed at the angular turnings of the river, and were originally large ſand banks thrown up round the points (in the manner before deſcribed) but afterwards inſulated by breaches of the river. Others are formed in the ſtraight parts of the river, and in the middle of the ſtream; and owe their origin to ſome obſtruction lurking at the bottom. Whether this be the fragments of the river bank; a large tree ſwept down from it; or a ſunken boat; it is ſufficient for a foundation: and a heap of ſand is quickly collected below it. This accumulates amazingly faſt: in the courſe of a few years it peeps above water, and having now uſurped a conſiderable portion of the channel, the river borrows on each ſide to ſupply the deficiency in its bed; and in ſuch parts of the river we always find ſteep banks on both ſides †. Each periodical flood brings an addition of matter to this growing iſland; increaſing it in height as well as extenſion, until its top is perfectly on a level with the banks that include it: and at that period of its growth it has mould enough on it for the purpoſes of cultivation, which is owing to the mud left on it when the waters ſubſide, and is indeed a part of the œconomy which nature obſerves in fertilizing the lands in general.

Whilſt the river is forming new iſlands in one part, it is ſweeping away old ones in other parts. In the progreſs of this deſtructive operation, we have opportunities of obſerving, by means of the ſections of the falling bank, the regular diſtri-

* Accordingly, the laws reſpecting alluvion are aſcertained with great preciſion.

† This evidently points out the means for preventing encroachments on a river bank in the ſtraight parts of its courſe, *viz.* to remove the ſhallows in the middle of its channel.

bution

bution of the ſeveral ſtrata of ſand and earths, lying above one another in the order in which they decreaſe in gravity. As they can only owe this diſpoſition to the agency of the ſtream that depoſited them, it would appear, that theſe ſubſtances are ſuſpended at different heights in the ſtream, according to their reſpective gravities. We never find a ſtratum of earth under one of ſand; for the muddy particles float neareſt the ſurface *. I have counted ſeven diſtinct ſtrata in a ſection of one of theſe iſlands. Indeed, not only the iſlands, but moſt of the river banks wear the ſame appearance: for as the river is always changing its preſent bed, and verging towards the ſite of ſome former one now obliterated, this muſt neceſſarily be the caſe.

As a ſtrong preſumptive proof of the wandering of the Ganges from the one ſide of the Delta to the other, I muſt obſerve, that there is no appearance of *virgin* earth between the Tiperah Hills on the eaſt, and the province of Burdwan on the weſt; nor on the north till we arrive at Dacca and Bauleah. In all the ſections of the numerous creeks and rivers in the Delta, nothing appears but ſand and black mould in regular ſtrata, till we arrive at the clay that forms the lower part of their beds. There is not any ſubſtance ſo coarſe as gravel either in the Delta or nearer the ſea than 400 miles †, where a rocky point, a part of the baſe of the neighbouring hills, projects into the river: but out of the vicinity of the great rivers the ſoil is either red, yellow, or of a deep brown.

* A glaſs of water taken out of the Ganges, when at its height, yields about one part in four of mud. No wonder then that the ſubſiding waters ſhould quickly form a ſtratum of earth; or that the Delta ſhould encroach upon the ſea!

† At Oudanulla.

I come

I come now to the particulars of the annual ſwelling and overflowing of the Ganges *.

It appears to owe its increaſe as much to the rain water that falls in the mountains contiguous to its ſource, and to the ſources of the great northern rivers that fall into it, as to that which falls in the plains of Hindooſtan; for it riſes fifteen feet and a half out of thirty-two (the ſum total of its riſing) by the latter end of June: and it is well known, that the rainy ſeaſon does not begin in moſt of the flat countries till about that time. In the mountains it begins early in † April; and by the latter end of that month, when the rain-water has reached Bengal, the rivers begin to riſe, but by very ſlow degrees; for the increaſe is only about an inch *per* day for the firſt fortnight. It then gradually augments to two and three inches before any

* An opinion has long prevailed, that the ſwelling of the Ganges, previous to the commencement of the rainy ſeaſon in the flat countries, is in a great meaſure owing to the melting of the ſnow in the mountains. I will not go ſo far as totally to diſallow the fact; but can by no means ſuppoſe, that the quantity of ſnow water bears any proportion to the increaſe of the river.

† The vaſt collection of vapours, wafted from the ſea by the ſoutherly or ſouth-weſt monſoon, are ſuddenly ſtopped by the lofty ridge of mountains that runs from eaſt to weſt through Thibet. It is obvious, that the accumulation and condenſation of theſe vapours, muſt firſt happen in the neighbourhood of the obſtacle; and ſucceſſively in places more remote, as freſh ſupplies arrive to fill the atmoſphere. Hence the priority of commencement of the rainy ſeaſon in places that lie neareſt the mountains.

All the rivers that are ſituated within the limits of the monſoons, or ſhifting trade winds, are ſubject to overflowings at annually ſtated periods, like the Ganges: and theſe periods return during the ſeaſon of the ſoutherly wind, that being the only wind which brings vapours from the ſea; and this being periodical, the falls of rain muſt neceſſarily be ſo too.

The northerly wind, which blows only over the land, is dry; for no rain (except caſual ſhowers) falls during the continuance of that monſoon.

quantity

quantity of rain falls in the flat countries; and when the rain becomes general, the increaſe on a medium is five inches *per* day. By the latter end of July all the lower parts of Bengal, contiguous to the Ganges and Burrampooter, are overflowed, and form an inundation of more than a hundred miles in width; nothing appearing but villages and trees, excepting very rarely the top of an elevated ſpot (the artificial mound of ſome deſerted village) appearing like an iſland.

The inundations in Bengal differ from thoſe in Egypt in this particular, that the Nile owes its floods *entirely* to the rain-water that falls in the mountains near its ſource; but the inundations in Bengal are as much occaſioned by the rain that falls there, as by the waters of the Ganges; and as a proof of it, the lands in general are overflowed to a conſiderable height long before the bed of the river is filled. It muſt be remarked, that the ground adjacent to the river bank, to the extent of ſome miles, is conſiderably higher than the reſt of the country*, and ſerves to ſeparate the waters of the inundation from thoſe of the river until it overflows. This high ground is in ſome ſeaſons covered a foot or more; but the height of the inundation within, varies, of courſe, according to the irregularities of the ground, and is in ſome places twelve feet.

Even when the inundation becomes general, the river ſtill ſhews itſelf, as well by the graſs and reeds on its banks, as by its rapid and muddy ſtream; for the water of the inundation acquires a blackiſh hue, by having been ſo long ſtagnant

* This property of the bank is well accounted for by Count BUFFON, who imputes it to the precipitation of mud made by the waters of the river, when it overflows. The inundation, ſays he, purifies itſelf as it flows over the plain; ſo that the precipitation muſt be greateſt on the parts neareſt to the margin of the river.

amongſt

amongst grass and other vegetables: nor does it ever lose this tinge, which is a proof of the predominancy of the rain water over that of the river; as the slow rate of motion of the inundation (which does not exceed half a mile *per* hour) is of the remarkable flatness of the country.

There are particular tracts of land, which, from the nature of their culture, and species of productions, require less moisture than others; and yet, by the lowness of their situation, would remain too long inundated, were they not guarded by dikes or dams, from so copious an inundation as would otherwise happen from the great elevation of the surface of the river above them. These dikes are kept up at an enormous expence; and yet do not always succeed, for want of tenacity in the soil of which they are composed.

During the swoln state of the river, the tide totally loses its effect of counteracting the stream; and in a great measure that of ebbing and flowing, except very near the sea. It is not uncommon for a strong wind, that blows up the river for any continuance, to swell the waters two feet above the ordinary level at that season: and such accidents have occasioned the loss of whole crops of rice *. A very tragical event happened at Luckipour † in 1763, by a strong gale of wind conspiring with a high spring tide, at a season when the periodical flood was within a foot and half of its highest pitch. It is said that the waters rose six feet above the ordinary level. Certain it is, that

* The rice I speak of is of a particular kind; for the growth of its stalk keeps pace with the increase of the flood at ordinary times, but is destroyed by a too sudden rise of the water. The harvest is often reaped in boats. There is also a kind of grass which overtops the flood in the same manner, and at a small distance has the appearance of a field of the richest verdure.

† This place is situated about fifty miles from the sea.

the inhabitants of a considerable district, with their houses and cattle, were totally swept away; and, to aggravate their distress, it happened in a part of the country which scarce produces a single tree for a drowning man to escape to.

Embarkations of every kind traverse the inundation: those bound upwards, availing themselves of a direct course and still water, at a season when every stream rushes like a torrent. The wind too, which at this season blows regularly from the south-east *, favours their progress; insomuch, that a voyage, which takes up nine or ten days by the course of the river when confined within its banks, is now effected in six. Husbandry and grazing are both suspended; and the peasant traverses in his boat, those fields which in another season he was wont to plow; happy that the elevated site of the river banks place the herbage they contain, within his reach, otherwise his cattle must perish.

The following is a table of the gradual increase of the Ganges and its branches, according to observations made at Jellinghy and Dacca.

At Jellinghy.	Ft.	In.	At Dacca. Ft.	In.
In May it rose -	6	0	2	4
June - - -	9	6	4	6
July - - -	12	6	5	6
In the first half of August	4	0	1	11
	32	0	14	3

* Although in the gulf or bay of Bengal the monsoon blows from the S.S.W. and S.W. yet in the eastern and northern parts of Bengal it blows from the S.E. or E.S.E.

These observations were made in a season, when the waters rose rather higher than usual; so that we may take 31 feet for the medium of the increase.

The inundation is nearly at a stand for some days preceding the middle of August, when it begins to run off; for although great quantities of rain fall in the flat countries, during August and September, yet, by a partial cessation of the rains in the mountains, there happens a deficiency in the supplies necessary to keep up the inundation *. The quantity of the daily decrease of the river is nearly in the following proportion: during the latter half of August, and all September, from three to four inches; from September to the end of November, it gradually lessens from three inches to an inch and a half; and from November to the latter end of April, it is only half an inch *per* day at a medium. These proportions must be understood to relate to such parts of the river as are removed from the influence of the tides; of which more will be said by and by. The decrease of the inundation does not always keep pace with that of the river, by reason of the height of the banks; but after the beginning of October, when the rain has nearly ceased, the remainder of the inundation goes off quickly by evaporation, leaving the lands highly manured, and in a state fit to receive the seed, after the simple operation of plowing.

There is a circumstance attending the increase of the Ganges, and which, I believe, is little known or attended to; because few people have made experiments on the heights to which the

* I have stated the middle of August for the period when the waters begin to run off; and in general it happens with more regularity than the vicissitudes of the seasons do. But there are exceptions to it; for in the year 1774 the rivers kept up for near a month after the usual time.

periodical flood rises in different places. The circumstance I allude to, is, the difference of the quantity of the increase (as expressed in the foregoing table) in places more or less remote from the sea. It is a fact, confirmed by repeated experiments, that from about the place where the tide commences, to the sea, the height of the periodical increase diminishes gradually, until it totally disappears at the point of confluence. Indeed, this is perfectly conformable to the known laws of fluids: the Ocean preserves the same level at all seasons (under similar circumstances of tide) and necessarily influences the level of all the waters that communicate with it, unless precipitated in the form of a cataract. Could we suppose, for a moment, that the increased column of water, of 31 feet perpendicular, was continued all the way to the sea, by some preternatural agency: whenever that agency was removed, the head of the column would diffuse itself over the Ocean, and the remaining parts would follow, from as far back as the influence of the Ocean extended; forming a slope, whose perpendicular height would be 31 feet. This is the precise state in which we find it. At the point of junction with the sea, the height is the same in both seasons at equal times of the tide. At Luckipour there is a difference of about six feet between the heights in the different seasons; at Dacca, and places adjacent, 14; and near Custee, 31 feet. Here then is a regular slope; for the distances between the places bear a proportion to the respective heights. This slope must add to the rapidity of the stream; for, supposing the descent to have been originally four inches *per* mile, this will increase it to about five and an half. Custee is about 240 miles from the sea, by the course of the river; and the surface of the river there, during the dry season, is about 80 feet

feet above the level of the sea at high water *. Thus far does the Ocean manifest its dominion in both seasons: in the one by the ebbing and flowing of its tides; and in the other by depressing the periodical flood, till the surface of it coincides as nearly with its own, as the descent of the channel of the river will admit †.

Similar circumstances take place in the Jellinghy, Hoogly, and Burrampooter Rivers; and, I suppose, in all others that are subject either to periodical or occasional swellings.

Not only does the flood diminish near the sea, but the river banks diminish in the same proportion; so that in the dry season the height of the periodical flood may be known by that of the bank.

I am aware of an objection that may be made to the above solution; which is, that the lowness of the banks in places near the sea, is the true reason why the floods do not attain so considerable a height, as in places farther removed from it, and where the banks are high; for that the river, wanting a bank to confine it, diffuses itself over the surface of the country. In

* The tides in the River Amazons are perceptible at 600 miles above its mouth; but at an elevation of only 90 feet, according to M. DE CONDAMINE. It remains to be told what the state of the river was at the time of making the experiment; because the land-floods have the effect of shortening the limits of the tide's way.

† The Count DE BUFFON has slightly mentioned this circumstance attending the swelling of rivers; but imputes it to the increased velocity of the current, as the river approaches the sea: which, says he, carries off the inundation so quick, as to abate its height. Now (with the utmost deference to so great an authority) I could never perceive, that the current, either in the Ganges, or any other river, was stronger near the sea than at a distance from it. Even if we admit an acceleration of the current during the ebb tide, the flux retards it in so considerable a degree, as at least to counter balance the effects produced by the temporary increase of velocity.

answer

answer to this, I shall observe, that it is proved by experiment, that at any given time, the quantity of the increase in different places, bears a just proportion to the sum total of the increase in each place respectively: or, in other words, that when the river has risen three feet at Dacca, where the whole rising is about 14 feet; it will have rose upwards of six feet and a half at Custee, where it rises 31 feet in all.

The quantity of water discharged by the Ganges, in one second of time, during the dry season, is 80,000 cubic feet; but in the place where the experiment was made, the river, when full, has thrice the volume of water in it; and its motion is also accelerated in the proportion of 5 to 3: so that the quantity discharged in a second at that season is 405,000 cubic feet. If we take the medium the whole year through, it will be nearly 180,000 cubic feet in a second.

THE Burrampooter, which has its source from the opposite side of the same mountains that give rise to the Ganges, first takes its course eastward (or directly opposite to that of the Ganges) through the country of Thibet, where it is named Sanpoo or Zanciu, which bears the same interpretation as the Gonga of Hindoostan: namely, the River. The course of it through Thibet, as given by Father DU HALDE, and formed into a map by Mr. D'ANVILLE, though sufficiently exact for the purposes of general geography, is not particular enough to ascertain the precise length of its course. After winding with a rapid current through Thibet, it washes the border of the territory of Lassa (in which is the residence of the grand Lama) and then deviating from an east to a south-east course, it approaches within 220 miles of Yunan, the westernmost province of

of China. Here it appears, as if undetermined whether to attempt a paſſage to the ſea by the Gulf of Siam, or by that of Bengal; but ſeemingly determining on the latter, it turns ſuddenly to the weſt through Aſſam, and enters Bengal on the north-eaſt. I have not been able to learn the exact place where it changes its name; but as the people of Aſſam call it Burrampoot, it would appear, that it takes this name on its entering Aſſam. After its entry into Bengal, it makes a circuit round the weſtern point of the Garrow Mountains; and then, altering its courſe to ſouth, it meets the Ganges about 40 miles from the ſea.

Father DU HALDE expreſſes his doubts concerning the courſe that the Sanpoo takes after leaving Thibet, and only ſuppoſes generally that it falls into the gulf of Bengal. M. D'ANVILLE, his geographer, with great reaſon ſuppoſed the Sanpoo and Ava River to be the ſame: and in this he was juſtified by the information which his materials afforded him: for the Burrampooter was repreſented to him, as one of the inferior ſtreams that contributed its waters the Ganges, and not as its equal or ſuperior; and this was ſufficient to direct his reſearches, after the mouth of the Sanpoo River, to ſome other quarter. The Ava River, as well from its bulk, as the bent of its courſe for ſome hundred miles above its mouth, appeared to him to be a continuation of the river in queſtion: and it was accordingly deſcribed as ſuch in his maps, the authority of which was juſtly eſteemed as deciſive; and, till the year 1765, the Burrampooter, as a capital river, was unknown in Europe.

On tracing this river in 1765, I was no leſs ſurprized, at finding it rather larger than the Ganges, than at its courſe previous to its entering Bengal. This I found to be from the eaſt; although all the former accounts repreſented it as from the north:

north: and this unexpected discovery soon led to enquiries, which furnished me with an account of its general course to within 100 miles of the place where DU HALDE left the Sanpoo. I could no longer doubt, that the Burrampooter and Sanpoo were one and the same river: and to this was added the positive assurances of the Assamers, "That *their* river came "from the north-west, through the Bootan mountains." And to place it beyond a doubt, that the Sanpoo River is not the same with the river of Ava, but that this last is the great Nou Kian of Yunan; I have in my possession a manuscript draught of the Ava River, to within 150 miles of the place where DU HALDE leaves the Nou Kian, in its course towards Ava; together with very authentic information that this river (named Irabattey by the people of Ava) is navigable from the city of Ava into the province of Yunan in China*.

The Burrampooter, during a course of 400 miles through Bengal, bears so intimate a resemblance to the Ganges, except in one particular, that one description may serve for both. The exception I mean is, that, during the last 60 miles before its junction with the Ganges, it forms a stream which is regularly from four to five miles wide, and but for its freshness might pass for an arm of the sea. Common description fails in an attempt to convey an adequate idea of the grandeur of this magnificent object; for,

——— Scarce the muse
Dares stretch her wing o'er this enormous mass
Of rushing water; to whose dread expanse,
Continuous depth, and wond'rous length of course,

* The courses of the Burrampooter and Ganges, as well as that of the Ava River, from Yunan to the sea, will shortly be described in a large sheet map of Hindoostan.

Our

Our floods are rills ———
Thus pouring on, it proudly ſeeks the deep,
Whoſe vanquiſh'd tide, recoiling from the ſhock,
Yields to this liquid weight ———

THOMSON's Seaſons.

I have already endeavoured to account for the ſingular breadth of the Megna, by ſuppoſing that the Ganges once joined it where the Iſſamutty now does; and that their joint waters ſcooped out its preſent bed. The preſent junction of theſe two mighty rivers below Luckipour, produces a body of running freſh water, hardly to be equalled in the old hemiſphere, and, perhaps, not exceeded in the new. It now forms a gulf interſperſed with iſlands, ſome of which rival, in ſize and fertility, our Iſle of Wight. The water at ordinary times is hardly brackiſh at the extremities of theſe iſlands; and, in the rainy ſeaſon, the ſea (or at leaſt the ſurface of it) is perfectly freſh to the diſtance of many leagues out.

The *Bore* (which is known to be a ſudden and abrupt influx of the tide into a river or narrow ſtrait) prevails in the principal branches of the Ganges, and in the Megna; but the Hoogly River, and the paſſages between the iſlands and ſands ſituated in the gulf, formed by the confluence of the Ganges and Megna, are more ſubject to it than the other rivers. This may be owing partly, to their having greater embouchures in proportion to their channels, than the others have, by which means a larger proportion of tide is forced through a paſſage comparatively ſmaller; and partly, to there being no capital openings near them, to draw off any conſiderable portion of the accumulating tide. In the Hoogly or Calcutta River, the *Bore* commences at Hoogly Point (the place where the river firſt contracts itſelf)

and is perceptible above Hoogly Town; and ſo quick is its motion, that it hardly employs four hours in travelling from one to the other, although the diſtance is near 70 miles. At Calcutta, it ſometimes occaſions an inſtantaneous riſe of five feet: and both here, and in every other part of its track, the boats, on its approach, immediately quit the ſhore, and make for ſafety to the middle of the river.

In the channels, between the iſlands in the mouth of the Megna, &c. the height of the *Bore* is ſaid to exceed twelve feet; and is ſo terrific in its appearance, and dangerous in its conſequences, that no boat will venture to paſs at ſpring tide. After the tide is fairly paſt the iſlands, no veſtige of a *Bore* is ſeen, which may be owing to the great width of the Megna, in compariſon with the paſſages between the iſlands; but the effects of it are viſible, enough by the ſudden riſing of the tides.

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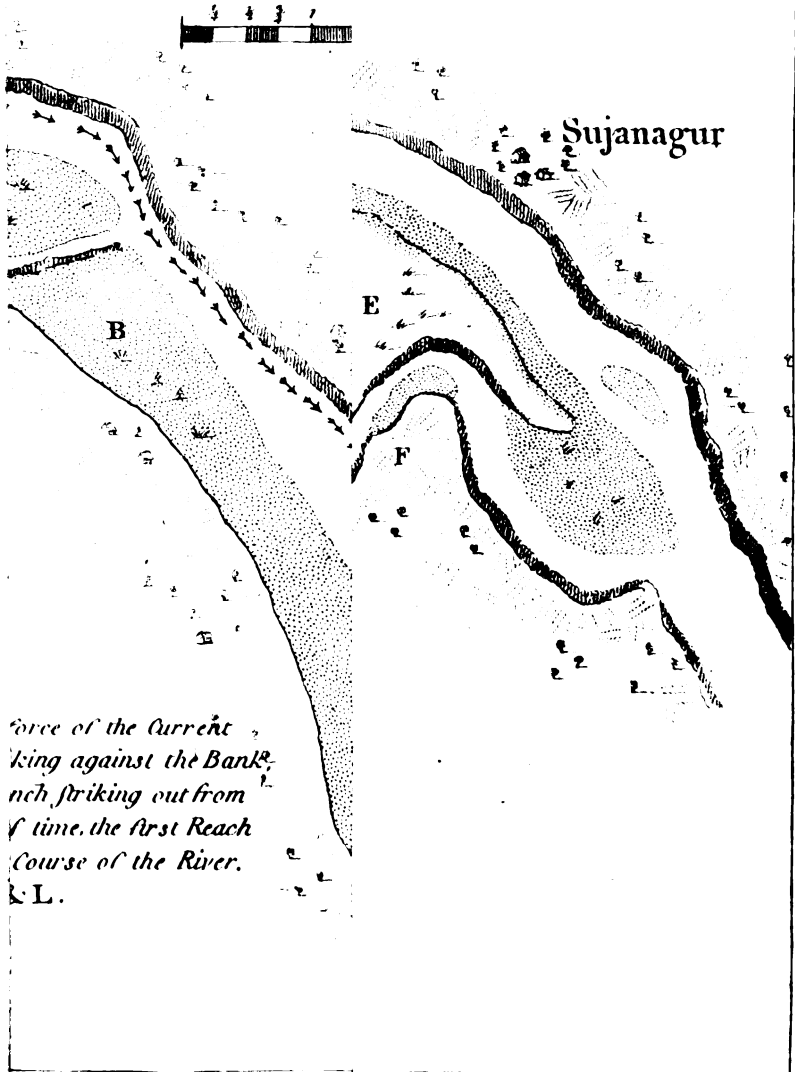

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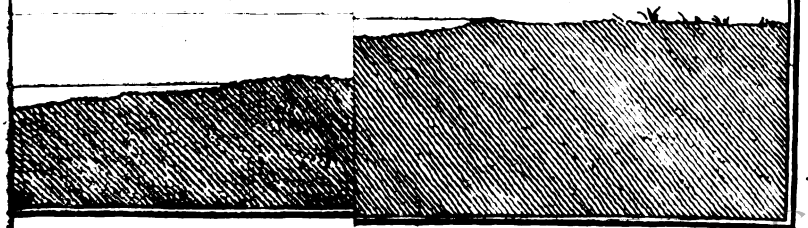
further explanation
Ganges itself, except in the a



X. *Aſtronomical Obſervations on the Rotation of the Planets round their Axes, made with a View to determine whether the Earth's diurnal Motion is perfectly equable. In a Letter from Mr.* William Herſchel *of* Bath *to* William Watſon, *M. D. F. R. S.*

Read January 11, 1781.

SIR, Bath, October 18, 1780.

THE various motions of the planet we inhabit; the annual revolution in its orbit; the diurnal rotation round its axis; the menſtrual motion round the common center of gravity of the moon and earth; the preceſſion of the equinoctial points; the diminution of the obliquity of the ecliptic; the nutation of the earth's axis: in ſhort, every one of the motions that ariſe from the actions of the ſun, moon, and planets, combined with the ſpheroidical figure of the earth, and the projectile and rotatory motions firſt impreſſed upon it, have all been conſidered by aſtronomers, and their real and apparent inequalities inveſtigated. And to the great honour of modern aſtronomers it muſt be confeſſed, that no ſcience has ever made ſuch conſiderable ſtrides towards perfection in ſo ſhort a time as aſtronomy has done ſince the invention of the teleſcope.

There is one of the motions of the earth however which, it ſeems, has hitherto eſcaped the ſcrutiny of obſervers; I mean the diurnal rotation round its axis. The principal reaſon

why this has not been looked into, is probably the difficulty of finding a proper ſtandard to meaſure it by; ſince it is itſelf uſed as the ſtandard by which we meaſure all the other motions. We have, indeed, no cauſe to ſuſpect any very material periodical irregularity, either diurnal, menſtrual, or annual; for the great perfection of our preſent time-pieces would have diſcovered any conſiderable deviation from that equability which we have hitherto aſcribed to the diurnal motion of the earth. And yet, it is not perhaps altogether impoſſible but that inequalities may exiſt in this motion which, in an age where obſervations are carried to ſuch a degree of refinement, may be of ſome conſequence.

To ſhew how far time-keepers, though ever ſo perfect, are from being a proper, or at leaſt a ſufficient, ſtandard to examine the diurnal motion of the earth by, I may aſk, whether it is probable, that any clock would have diſcovered to us the aberration of the fixed ſtars? And yet that aberration produces a change in longitude, and of conſequence in right aſcenſion, which cauſes an annual irregularity in a ſtar's coming to the meridian, which a time-piece, were it a ſufficient ſtandard, would ſoon have diſcovered, and which we might have attributed to an inequality of the earth's diurnal motion, had we not been acquainted with its real cauſe. And if we were to find out any apparent irregularity, acceleration, or retardation, ſhould we not much rather ſuſpect the clock than the diurnal motion? I may therefore venture to ſay, that the aberration of the fixed ſtars, though attended with the above mentioned conſequence, would for ever have remained a ſecret to us, if it had not been found out by other methods than time-keepers.

Now, if time-pieces do fail us in this critical caſe, where we ſtand in the greateſt need of their aſſiſtance, it is almoſt in

vain

vain to expect any help from another quarter; for what mechanical movement on earth, or motion of the heavens, is there that can measure out such equal portions of time as we require to compare the diurnal motion of the earth to? However, to proceed, since we have already great proofs that the diurnal motion of the earth is, if not perfectly equable, at least more so than any other motion we are acquainted with, it will not appear absurd to suppose the diurnal rotation of the other planets to be so likewise. This suggested to me the thoughts of estimating the diurnal motion of one planet very exactly by that of another, making each the standard of the other. In this manner we may obtain a comparative view, by which future astronomers, if they shall hereafter be inclined to pursue the subject, may be enabled to make some estimate of the general equability of the rotatory motions of the planets. For if in length of time they should perceive some small retardation in the diurnal motion of a planet occasioned by some resistance of a very subtle medium in which the heavenly bodies perhaps move; or, on the other hand, if there should be found an acceleration from some cause or other, they might then ascribe the alteration either to the diurnal motion of the earth, or to the gyration of the other planet, according as circumstances, or observed phænomena, should make one or the other of these opinions most probable.

Now, this method of comparing together different rotations of several planets, simple as it may appear, was not without some difficulties. In the first place it was evident, that the common account of their diurnal motions *, which makes that

* Venus spatio 23 horarum gyrationem circa axem ab occidente in orientem perficit. Mars similem rotationem, horis 24 min. 40 absolvit. Macularum revolutionibus sæpius observatis, Ds. CASSINI comperuit periodum Jovis circa proprium Axem esse horarum 9, minutorum 56. KEILL, Ast. Lect. V.

of

of Jupiter 9 h. 56′, of Mars 24 h. 40′, how true ſoever it may be in a general way, was much too inaccurate for this critical purpoſe. The gyration of Venus was ſtill leſs to be depended upon, being only noted to the hour without the minutes: it became, therefore, neceſſary to proceed to obſervations of a more determinate kind. From what I had already ſeen of the rotation of the planets, I concluded, that Mars on ſeveral accounts would be the moſt eligible planet for my purpoſe: for the ſpots on Jupiter change ſo often that it is not eaſy, if at all poſſible, to aſcertain the identity of the ſame appearance, for any conſiderable length of time. Nor do the dark ſpots only change their place, which may be ſuppoſed to be large black congeries of vapours and clouds ſwimming in the atmoſphere of Jupiter; but alſo the bright ſpots, though they may adhere firmly to the body of Jupiter, may undergo ſome apparent change of ſituation by being differently covered or uncovered on one ſide or the other, by alterations in the belts. It will be ſeen hereafter, that I have obſerved the revolution of a very bright ſpot, not ſuſpected of any change of ſituation, to be firſt, by one ſet of obſervations, at the rate of 9 h. 51′ 45″,6; and afterwards, by another ſet immediately following at the rate of 9 h. 50′ 48″.

As the principal belts on Jupiter are equatorial, and as we have certain conſtant winds upon our planet, eſpecially near the equator, that regularly, for certain periods, blow the ſame way *, it is eaſily ſuppoſed, that they may form equatorial belts by gathering together the vapours which ſwim in our atmoſphere, and carrying them about in the ſame direction. This will, by analogy, account for all the irregularities of Ju-

* See Acta Eruditorum, 1687. Dr. HALLEY's Account of periodical Winds.

piter's revolutions, deduced from ſpots on his diſk that may have changed their ſituation; for if we ſuppoſe the rotation of Jupiter, according to CASSINI, to be 9 h. 56′, then ſome ſpots that I have obſerved muſt have been carried through about 60° of Jupiter's equator in 22 of his revolutions or days. This would certainly be a very great velocity in the clouds, which is, however, not unparalleled by what has happened in our own atmoſphere.

But to return to my purpoſe: on the planet Mars we ſee ſpots of a different nature; their conſtant and determined ſhape, as well as remarkable colour, ſhew them to be permanent and faſtened to the body of the planet. Theſe will give the revolution of his equator to a great certainty, and by a great number of revolutions, to a very great exactneſs alſo. Suppoſing then, that, by a method I ſhall hereafter deſcribe, we can determine whether a ſpot on the diſk of Mars is, or is not, in the line which joins the center of the earth and the center of that planet, to half an hour's time with certainty (I believe ten or twelve minutes will be found ſufficient for that purpoſe), in this caſe we ſhall in 30 days have the revolution true to a minute; and, by continuing theſe obſervations for three months, we ſhall have it to 20″. When we are ſo far certain, we can eaſily arrive to a much greater degree of exactneſs; for as we now can no longer miſtake a whole revolution, if we take the time of any particular ſpot's being in the line which joins the centers of the planets during one oppoſition of Mars, and take the ſame again at or near the next oppoſition, we ſhall have an interval of about 780 days, which will give the diurnal motion of that planet true to about 2″. The next oppoſition will give it to one, and ſo forth; by which means, and by taking a proper number of ſuch periods, we may determine the rotation of

Mars

Mars to as great an exactneſs as we ſhall think neceſſary for the purpoſe of our comparative view.

Had ſuch obſervations as theſe been made two thouſand, or perhaps only ſo many hundred years ago, we might now, by repeating them, moſt probably become acquainted with ſome curious minute changes of the ſolar ſyſtem that have hitherto paſſed unnoticed.

There is a certain circumſtance which would almoſt create a ſuſpicion that there has been ſome retardation in the diurnal motion of the earth. The difference between the equatorial and polar diameters of the earth, by actual meaſurement, has been found to be about 36 Engliſh miles and 9 tenths; but, by a calculation wherein the preſent rotation is made uſe of, it will only amount to about 33 miles and 8 tenths: from which it ſhould ſeem probable, that when the earth aſſumed the preſent form, the diurnal rotation was ſomewhat quicker than it is at preſent, by which means the centrifugal force bore a greater proportion to the force of gravity to which it is contrary, and thus occaſioned a higher elevation of the equatorial parts. But I would not lay much ſtreſs upon this argument; for, in the calculation, it has been ſuppoſed, that the earth is nearly of an equal denſity at the ſurface and towards the center, which it ſeems is not agreeable to ſome late curious experiments and calculations that have been made under the conduct of the Aſtronomer Royal upon the attraction of a mountain *, the reſult of which ought now to be taken into conſideration, and the calculation repeated. If all the *data* could be exactly depended upon, it would be practicable enough from the laws of

* See Mr. HUTTON's Account of the Calculations made from the Survey and Meaſures taken at Schehallien, in order to aſcertain the mean Denſity of the Earth. Phil. Tranſ. 1778.

gravity, and the present rotation and given form of the earth, to find the centrifugal force required to produce that form, and thence to shew what must have been its diurnal motion when it assumed the same. However, these are researches that in my present situation I neither have opportunity nor perhaps ability enough to investigate properly; and which, therefore, I hope some of our excellent mathematicians will think worth while to look into.

I shall now relate my observations on Jupiter and Mars. The telescopes I used are of my own construction; and are, a twenty-feet Newtonian reflector, a ten-feet reflector of the same form, and a seven-feet reflector already mentioned in my paper on the mountains of the moon. My time I gained by equal altitudes taken with a brass quadrant of two-feet radius, carrying a telescope which magnifies about 40 times; for the correction of altitudes taken of the sun I used DE LA LANDE's tables. I kept my time by two very good pieces; one having a deal pendulum-rod, the other a compounded one of brass and iron, both having a proper contrivance not to stop when winding up. The rate of going of my clocks I determined by the transit of stars.

Observations on Jupiter in the year 1778.

February 24. Clock 1′ 10″ too soon. About 9 o'clock I saw a bright belt on one part of the disk of Jupiter, see tab. V. fig. 1.

About 10 o'clock it was advanced as far as the center, fig. 2.

11 h. The white belt still more advanced, fig. 3.

11 h. 25′. It approached towards the edge of the disk; and at 12 h. was extended all over, as in fig. 4.

February 25. 8 h. The ſame bright belt I obſerved yeſterday extends all over.

8 h. 45′. It is divided by a darkiſh ſpot, ſituated at ſome diſtance from the center, as in fig. 5.

9 h. 5′. The ſmall dark diviſion is advanced a little farther than the center, as in fig. 6.

9 h. 23′. The ſpot is viſibly advanced a conſiderable deal farther.

March 2. 8 h. 2′. The darkiſh ſpot, with ſome alteration in its ſhape, is now in the middle of the diſk, ſee fig. 7.

March 3. 10 h. 34′. The bright belt on the ſouth of the equator is now in the middle; that is to ſay, if a line be drawn perpendicular to the equatorial belt, and through the center, the end of the equatorial belt now touches it, fig. 8.

13 h. 49′. The darkiſh ſpot, in which there has been ſome alteration ſince yeſterday, ſeems now to be in the center, fig. 9.

March 14. The clock altered to true equated time; but the rate of going not changed, being well regulated.

7 h. 35′. The ſpot is now in the center, but does not ſeem quite to fill the white belt; nor is it ſo large and diſtinct as it was before, fig. 10.

April 7. 9 h. 31′. There are three dark ſpots in the equatorial belt nearly in the center, ſee fig. 11.

April 12. 7 h. 50′. The three dark ſpots are in the center. The ſourthernmoſt of the three is nearly quite vaniſhed; the other two are alſo much fainter. They are, however, diſtinct enough to be known, fig. 12.

Obſervations

Obſervations on Jupiter in 1779.

April 14. Clock 52″ too late. 8 h. 48′ A remarkable bright ſpot in the equatorial belt towards the north is in the center, ſee fig. 18.

8 h. 58′. The ſpot is a little paſt the center.

April 19. Clock true mean time. 7 h. 10′. There is a bright ſpot juſt now in the center, which, from its ſhape, I take to be the ſame that was there April 14th.

7 h. 20′. The ſpot is viſibly paſt the center.

April 23. Clock ſhews true time. 9 h. 38′. The ſame bright ſpot is in the center.

9 h. 43′. It is paſt the center. *Memorandum*, my time-piece may be depended upon to a few ſeconds.

It will not be amiſs to obſerve, that the ſpots, as well as a great many other phænomena, were watched as they came on, paſſed over the center, and went off the diſk of Jupiter; but I have only ſelected thoſe obſervations that were neceſſary to my preſent purpoſe.

Comparing together the obſervations that were made in the year 1778, February 24th and March 3d, we obtain an interval of 7 days 34 minutes, which being divided by 17 revolutions made by Jupiter on his axis, we have the time of one ſynodical revolution equal to 9 h. 54′ 56″,4.

The dark ſpot on February 25 was obſerved ſome time before, and alſo juſt after it was paſt the center; therefore I have ſuppoſed it to be in the center about 8 h. 58′: and we have,

	D.	H.	M.
February	25	8	58
March	2	8	2
Divided by 12 rev.	4	23	4

1 revolution = 9 h. 55′ 20″

February	25	8	58
March	3	13	49
15 revol.	6	4	51

1 revolution = 9 h. 55′ 24″.

February	25	8	58	0
March	14	7	36	10 allowing 1′ 10″ for the alt. of the clock.
41 revol.	16	22	38	10

1 revolution = 9 h. 55′ 4″,6.

March	2	8	2
	3	13	49
1 revol.	1	5	47

1 revolution = 9 h. 55′ 40″.

March	2	8	2	0
	14	7	36	10
29 revol.	11	23	34	10

1 revolution = 9 h. 54′ 58″,2.

March

	D.	H.	M.	S.
March	3	13	49	0
	14	7	36	10
Divided by 26 rev.	10	17	47	10

1 revolution = 9 h. 54′ 53″,4.

April	7	9	31
	12	7	50
12 revol.	4	22	29

1 revolution = 9 h. 51′ 35″.

Again, comparing together the obſervations of 1779, which were made with the utmoſt attention to time, we have,

April	14	8	48	52
April	19	7	10	0
12 revol.	4	22	21	8

1 revolution = 9 h. 51′ 45″6.

April	19	7	10
April	23	9	38
10 revol.	4	2	28

1 revolution = 9 h. 50′ 48″.

And taking both together,

April	14	8	48	52
April	23	9	38	0
22 revol.	9	0	49	8

1 revolution = 9 h, 51′ 19″,4.

Theſe

These several results are so exceedingly various, that it is evident Jupiter is not a proper planet for the critical purpose of a comparative view of the diurnal motions; nor can this great variety proceed from any inaccuracy in the observations: for, in my opinion, it is not well possible to make a mistake in the situation of a spot that shall amount to so much as five minutes of time. The observation of April 23, 1779, was made with a view to ascertain this point, when it was found that five minutes of time made a sensible difference in the situation of a spot when near the center.

If we reduce the synodical revolutions to sydereal ones, the result will be so little different from the above, that I have not thought it worth while to do it in this place. By a comparison of the different periods it appears, that a spot which is carried about in the atmosphere of Jupiter generally suffers an acceleration, or, which is the same thing, performs its revolutions by degrees in less time than it did at first; for the spot observed in 1778 moved at the following rates. From February 25. to March 2. in 9 h. 55′ 20″; to March 3. nearly the same; to March 14. in 9 h. 55′ 4″; from March 2. to March 3. in 9 h. 55′ 40″; to March 14. in 9 h. 54′ 58″; from March 3. to March 14. in 9 h. 54′ 53″. In 1779 a spot moved from April 14. to April 19. at the rate of 9 h. 51′ 45″; to April 23. in 9 h. 51′ 24″; and from April 19. to April 24. in 9 h. 50′ 48″; all which is agreeable enough to the theory of equatorial winds, since it may probably take up sometime before a spot can acquire a sufficient velocity to go as fast as those winds may blow. And, by the by, if Jupiter's spots should be observed in different parts of his year, and be found in some to be accelerated, in others to be retarded, it would almost amount to a demonstration of his monsoons and their periodical changes; but if his

axis ſhould not be inclined enough to his orbit, to occaſion ſuch a change, they may probably always blow in the ſame direction.

Obſervations on Mars in the year 1777.

Twenty-feet Newtonian reflector; power 300.

April 8. 7 h. 30′. I obſerved two ſpots upon Mars, with a bright belt or partition between them. The belt was not very well defined, ſee tab. VI. fig. 14.

9 h. 30′. The ſpots are advanced, and more ſpotted parts are viſible, fig. 15.

10 h. The revolution of Mars on his axis is now very evident, fig. 16.

April 17. Ten-feet Newtonian reflector; power about 211. 7 h. 50′. Mars appeared as in fig. 17. At *a* and *b* there were two bright ſpots, ſo luminous that they ſeemed to project beyond the diſk. At *c* and *d* there were two very dark ſpots, joined by a leſſer black line in the middle, which however was croſſed at *e* and *f* by a very faint whitiſh partition.

April 26. Ten-feet reflector; power 211.

9 h. 5′. The ſpots on the planet are very faint, and much about as in fig. 18.

April 27. Ten-feet reflector; power 324.

8 h. 40′. The evening very fine: my teleſcope in compleat order. The ſpots as in fig. 19.

Obſervations

Obſervations on Mars in the year 1779.

May 9th. Clock 15″ too faſt; by equal altitudes on the 14th of April, and by the tranſit of a ſtar, is found to loſe 1″,45 *per* day.

11 h. 1′ by the clock, I found the ſituation of the ſpots on Mars as in figure 20; there is a very remarkable dark ſpot not far from the center.

11 h. 30′. The figures are gone from the center.

May 11. Clock 12″ too faſt.

10 h. 18′. The ſame ſpot that was viſible May 9. is on the diſk, the darkeſt place being intirely ſouth-eaſt of the center, ſee fig. 21.

11 h. 43′. The darkeſt part is almoſt arrived at the center, fig. 22.

12 h. 17′. The dark ſpot is with its edge juſt near the center, as in fig. 23.

May 13. Seven-feet reflector; power 222. Clock 9″ too faſt.

11 h. 26′. Mars ſeems now to be in the ſame ſituation he was the 11th, at 10 h. 8′.

May 22. Clock 4″ too ſlow.

12 h. 5′. The figure of May 11th is not on the diſk; but ſome other fainter ſpots are viſible. The air is full of vapours.

June 6. The clock ſet by ten equal altitudes taken to-day, and by the tranſit of δ Scorpii loſes 1″,9 *per* day. What I have perhaps improperly called a tranſit is the occultation of a ſtar paſſing behind the perpendicular edge of a high building at about 40 yards diſtance, obſerved with a fixed teleſcope directed to the place where it vaniſhes.

10 h. 10′. The ſame figure is upon the diſk of Mars which was there April 8, 1777, at 7 h. 30′.

June

June 15. Clock 17″ too slow. 9 h. 45′. The same figure is upon Mars that was there May 9. at 11 h. 1′; but it is more advanced. I suppose it to be the same, and in the same situation, as April 17, 1777, at 7 h. 50′.

June 17. Clock 20″ slow.

9 h. 12′. The dark spot on Mars is rather more advanced than it was May 11th, at 10 h. 18′.

10 h. The spot is visibly advanced: I suppose it will take near an hour to come to the center.

10 h. 15′. A very thick fog obscures the sky.

11 h. 15′. The same darkness.

June 19. Clock 22″ too slow by the transit of δ Scorpii observed this evening.

8 h. 40′. The figure on the disk of Mars appears now to be as it was April 26, 1777, at 9 h. 5′, see fig. 18.

11 h. 30′. The figure of May 11. which I have been hitherto watching, is not come to the position it was then at 11 h. 43′, but cannot be far from it. I fear, as Mars approaches the horizon, I shall not be able to follow him till the figure comes to the center.

11 h. 47′. The state of the air near the horizon is very unfavourable. With much difficulty I can but just see that the figure is not quite so far advanced as it was May 11th, at 11 h. 43′, but can certainly not be above two or three minutes from it.

11 h. 51′. The undulation of the air prevents all further observation.

Let us now examine the result of the above mentioned observations: comparing together the two following short intervals of the year 1779, we have,

	D.	H.	M.	S.
From May	9	11	0	45
to May	11	12	16	48
Divided by 2 revol.	2	1	16	3

Gives 1 revolution = 24 h. 38′ 1″,5.

A second small interval.

May	11	10	17	48
	13	11	25	51
2 revol.	2	1	8	3

1 revolution = 24 h. 34′ 1″,5.

Here we have two very short intervals that agree to 4′, which is more than we could have expected in such short periods of time.

Comparing together observations that were made at a greater distance, we find,

First monthly period,

May	11	10	17	48	
June	17	9	9	20	allowing 3′ because the obs. says the spot was rather more advanced.
36 revol.	36	22	51	32	

1 revolution = 24 h. 38′ 5″,9.

Second monthly period,

May	11	11	42	48	
June	19	11	50	22	allowing 3′ for the time the spot would have taken to come to the place mentioned.
38 revol.	39	0	7	34	

1 revolution = 24 h. 38′ 5″,4.

Third

Third monthly period,

	D.	H.	M.	S.
May	13	11	25	51
June	17	9	9	20
34 revol.	34	21	43	29

1 revolution = 24 h. 38′ 20″,3

This laſt is, perhaps, as likely to be near the truth as any, ſince the ſame ſpot was here obſerved for the third time, and therefore its motion become more familiar.

Here we have three longer periods that agree to fifteen ſeconds, which is quite ſufficient for extending the interval of time to thoſe obſervations that were made in the year 1777. But as theſe are the ſynodical revolutions, it will be neceſſary firſt to reduce them to ſydereal rotations.

In figure 24. let us ſuppoſe the orbit of Mars, MABC, to be in the ſame plane with the orbit of the earth, EDFG; and the axis of Mars to be perpendicular to his orbit. Let M, E, *m*, *e*, be the ſituations of Mars and the earth on the 13th of May and 17th of June; then will the line EM, that connects the centers of Mars and the earth, point out the geocentric place of Mars on the 13th of May; and the line *em*, the geocentric place of the ſame planet on the 17th of June. Draw *er* and *ms* parallel to ER; then will *er* point out the geocentric place of Mars on the 13th of May; and the angle *sme* is equal to the angle *mer*. Now, by an ephemeris * the geocentric place of Mars, May 13. at 11 h. 26′ was 7 ſ. 20 d. 59′ 21″;

* The Nautical Almanac gives the geocentric place of Mars only to every ſixth day; for which reaſon I uſed WHITE's Ephemeris, where it is given for every day, though perhaps not with ſo much exactneſs as I could wiſh.

and on the 17th of June, at 9 h. 9′, it was 7 s. 12 d. 27′ 22″, by which we obtain the difference or angle *rem* = *ems* = 8 d. 31′ 59″.

Now a spot on Mars, situated in the direction ME, will have made a sydereal revolution when it returns to the same, or a parallel direction *ms*. From which we gather, that the spot on the 17th of June, after coming to the line *me*, where it finishes the synodical revolution, will have to go through an arch of 8 d. 31′ 59″, in order to arrive into the direction of the line *ms*, where it finishes the sydereal rotation. The time it will take to go through this arch, at the sydereal rate of 24 h. 39′ 20″ to 360 degrees, or 4″,109 *per* minute of a degree, will be 35′ 3″,8; this being divided by the numbers of revolution 34, gives 1′ 1″,8; which, added to 24 h. 38′ 20″,3, gives us 24 h. 39′ 22″,1 for the sydereal revolution of Mars, as found by the third of the monthly periods. This quantity will help us to find a proper divisor for the three following long biennial periods.

It is to be observed, that Mars has been retrograde in the above example, for which reason the measure of the angle *ems* was to be added to the synodical revolution when we wanted to find the sydereal rotation; but if he had been direct, or if his place had been more advanced in the ecliptic than that to which we compared it, as at μ, then the line $\mu\sigma$ parallel to EM would be the direction to which the spot should return, in order to accomplish a sydereal revolution, and therefore the quantity of the angle $\sigma\mu e = \mu er$, or difference of the geocentric places ought to be subtracted from the synodical revolution to obtain the sydereal one.

First

		D.	M.	S.
First biennial period,	1777, April	8	7	30
	1779, June	6	10	10
		789	2	40

The geocentric places of Mars at those times were,

S.	D.	′	″
6	6	31	26
7	13	48	30
1	7	17	4

turned into time at 4″,109 *per* minute of a degree and subtracted, because Mars is more advanced in the ecliptic, is

	789	2	40	0
	—	2	33	11,8
Divided by 768 rev.	789	0	6	48,2

1 revolution = 24 h. 39′ 23″,03.

		D.	H.	M.	S.
Second biennial period,	1777, April	17	7	50	0
	1779, June	15	9	45	17
		789	1	55	17

	S.	D.	′	″
Geocentric places	6	3	31	27
	7	12	40	23
	1	9	8	56

Turned into time	789	1	55	17
and subtracted	—	2	40	52
768 revol.	788	23	14	25

1 revolution = 24 h. 39′ 18,″94.

Third

		D.	H.	M.	S.
Third biennial period,	1777, April	26	9	5	0
	1779, June	19	8	40	22
		783	23	35	22

	S.	D.	′	″
Geocentric places	6	1	24	36
	7	12	31	48
	1	11	7	12

Turned into time	783	23	35	22
and subtracted	—	2	45	15,6
763 revol.	783	20	50	6,4

1 revolution = 24 h. 39′ 23″,04.

As these three periods are supported by observations of equal validity, I shall take a mean of them all for the nearest approximation to the true sydereal revolution of Mars on his axis, which therefore is 24 h. 39′ 21″,67.

It remains now only to see how far we may depend upon this determination of Mars's diurnal rotation as coming near the truth; and looking over those causes which may possibly produce any errors, we find, first of all, that in the long biennial periods a mistake in the number of revolutions would produce a considerable deviation from truth. Secondly, in the observations of a spot which moves so slow, we are also liable to some considerable mistake in estimating the time when it comes to a certain place; and the more so, if that place is not the center. Lastly, the time itself is liable to inaccuracy.

As to the first, it appears from the three monthly periods observed in the year 1779, when the proper allowances for the geocentric places are made, that the sydereal revolution of Mars cannot well be less than 24 h. 39′ 5″, nor more than 24 h. 39′ 22″; but if we should divide any one of the three biennial periods by a supposed number of revolutions, only one more or one less than we have done, the difference would be so considerable, that nothing but a mistake in every one of the three monthly periods, of at least one whole hour, could justify such a supposition; and that such a mistake in the situation of a spot on Mars cannot have been made in those observations, I think, is evident enough from the exactness with which they were made, and from their agreement with each other.

The second cause of error, which is the uncertainty in assigning the exact time when a spot comes to the center, is of some force. But it seems to me highly probable, from the manner in which I have seen the spots on Mars pass over the disk of that planet, that there can hardly be so great an error as 10′ in an observation of any remarkable spot's coming to the center. However, not being willing to trust more to the eye than I ought to do, I had recourse to the following experiment. I drew several circles of one inch radius, taking care to make no visible impression of a center; and placed in each a fine point at the several distances of .0424, .0636, .0848, in ten thousands of an inch from the real center; some to the right, others to the left. These measures are the sines to radius one, of 2° 26′, 3° 39′, and 4° 52′, which are the arches a spot on Mars passes over in 10, 15, 20′ minutes respectively. I exposed them to several persons unacquainted with my designs, and found, that not one of them made a single mistake in saying whether the point was, or was not, in the center of the circle,

circle, and which way it deviated from it. As the direction of the motion of a spot on Mars is known, I thought the persons who were to judge of the place of the points were intitled to be acquainted with the line in which they were placed, which for that reason was always to the right and left only. The points that answer to the excentricity of 15 and 20′ are indeed so visibly out of the center, that I believe we may safely say, that any mistake, in estimating the time of a spot on Mars coming to the center, cannot well exceed a quarter of an hour at the outside.

As for the third and last occasion of error, the time itself, I believe my manner of obtaining and keeping it in the year 1779 will appear satisfactory, and may, I think, be depended upon to a few seconds; but the observations of the year 1777, indeed, are far from having the same advantage. I was not then provided with an altitude instrument, therefore set my clock by a good sun-dial, with the equation of time contained in the Nautical Almanac, and found it to agree generally to a minute or two with the time calculated for the eclipses of Jupiter's first satellite, as I deduced it for Bath from the Nautical Almanac. However, it was certainly liable to an error of several minutes; therefore, allowing no less than 10′ for the clock in 1777, and 20′ for an error in estimating the situation of a spot in 1779, it will both amount to half an hour: then, if we take a mean of the three numbers, whereby we have divided the three biennial periods, we have 766$\frac{1}{3}$; and half an hour, divided by 766$\frac{1}{3}$, will therefore give us the quantity to which, it seems, can amount, all the uncertainty in the sydereal diurnal rotation of Mars, which is 2″,34.

A nearer approximation to truth I hope to obtain at the next opposition, which will happen about the middle of July 1781.

6 I have

I have ventured to calculate the times for that oppoſition, when the edge of the remarkable dark ſpot will be ſeen near the center, as it is in figure 23, or, which is the ſame thing, as it was the 11th of May 1779, at 12 h. 17′. The ſpot not being viſible at the time of the oppoſition, I have taken the neareſt period, before and after, in which it will paſs over the diſk. There is, however, a circumſtance which may make the appearance of the ſpot not quite ſimilar to the figure I have drawn, even though the rotations ſhould perfectly anſwer as to the times; for the poſition of the axis of Mars being ſtill in ſome meaſure unknown, I could make no allowance for a change, which a difference in the ſituation of no leſs than two ſigns may occaſion, though in all probability it will not be very conſiderable.

Thoſe who are provided with proper teleſcopes will have an opportunity to ſee how far the calculated times agree with the ſpot's appearance; and it is by this means I alſo hope to correct and improve the tables I have drawn up for this purpoſe, and further to approximate to a true theory of the gyration of this planet.

Not knowing the exact difference of meridians between Greenwich and this place, I have calculated the ſpot's appearance for the meridian of Bath. From an eclipſe or two of Jupiter's ſatellites, of which, by the favour of the Rev. Mr. HORNSBY, I have ſeen correſpondent obſervations, I ſuppoſe the difference cannot be much leſs than 9′ weſt of Greenwich; and at the ſame time I join an account of the ſolar eclipſe of the 24th of June 1778, which may be depended upon as a very compleat obſervation, and may ſerve to aſcertain the longitude of this place.

Eclipse of the sun observed at Bath.

June 24, 1778.

	H.	M.	S.
Beginning by equated or mean time,	3	30	10,7
End, - - -	5	18	7,7

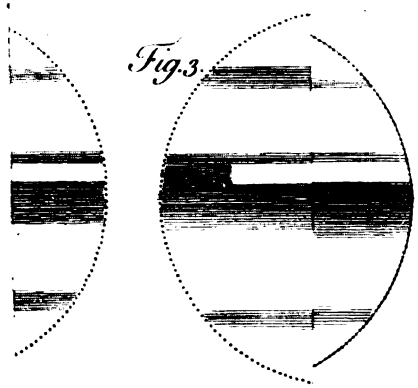
Calculations, or (as the principles on which they are founded are still established upon a few observations only, and require some time for mature confirmation) I would rather, if I might be allowed the expression, call them calculated conjectures of the times when the remarkable dark spot will be seen near the center of the disk of Mars.

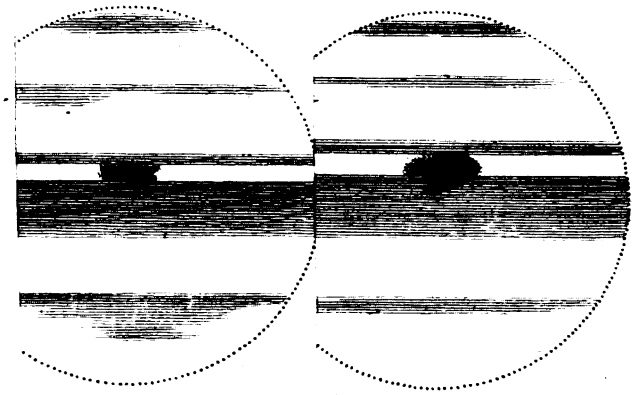
For June, July, and August, of the year 1781.

	D.	H.	M.	S.
June	28	9	48	39
	29	10	27	10
	30	11	5	42
July	1	11	44	13
	2	12	22	32
	3	13	0	51
	4	13	39	10
	5	14	17	29
	6	14	55	46
	7	15	34	4

	D.	H.	M.	S.
July	8	16	13	23
August	3	8	11	6
	4	8	49	57
	5	9	28	48
	6	10	7	39
	7	10	46	31
	8	11	25	44
	9	12	4	56
	10	12	44	8

I have the honour to be, &c.





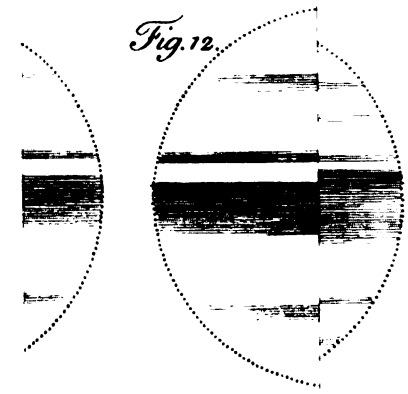
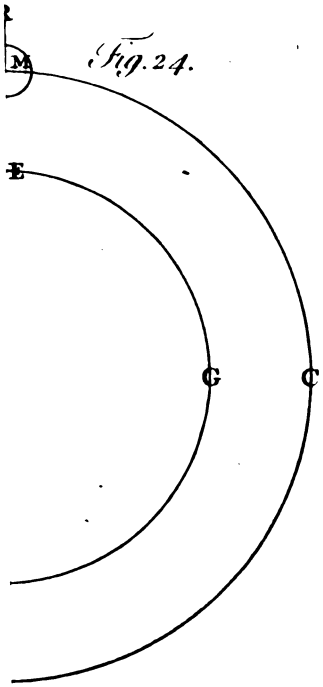


Fig. 16.

Fig. 19.

Fig. 22.

Fig. 24.



XI. *Some Account of the* Termites, *which are found in* Africa *and other hot Climates. In a Letter from Mr.* Henry Smeathman, *of* Clement's Inn, *to Sir* Joseph Banks, *Bart. P. R. S.*

Read February 15, 1781.

SIR,

Clement's Inn,
Jan. 23, 1781.

OF a great many curious parts of the creation I met with on my travels in that almost unknown district of Africa called Guinea, the TERMITES, which by most travellers have been called WHITE ANTS, seemed to me on many accounts most worthy of that exact and minute attention which I have bestowed upon them.

The amazingly great and sudden mischief they frequently do to the property of people in tropical climates, makes them well known and greatly feared by the inhabitants.

The size and figure of their buildings have attracted the notice of many travellers, and yet the world has not hitherto been furnished with a tolerable description of them, though their contrivance and execution scarce fall short of human ingenuity and prudence; but when we come to consider the wonderful oeconomy of these insects, with the good order of their subterraneous cities, they will appear foremost on the list of the wonders of the creation, as most closely imitating mankind in provident industry and regular government.

You had barely time to ſee and to admire ſome of their buildings in New Holland, and have been pleaſed to ſay, you think an accurate account of them would meet a favourable reception from the Royal Society. That which I now have the honour to preſent to you, is accurate and faithful as far as it goes. I have kept as cloſe to my ſubject as was in my power, without being obſcure, or falling ſhort of my intention; and though I have given only the heads of what I could draw from my memorandums on the ſubject, they will probably be found ſufficiently deſcriptive and hiſtorical for the bounds of a letter.

The ſagacity of theſe little inſects is ſo infinitely beyond that of any other animals I have ever heard of, that it is poſſible the accounts I have here communicated would not appear credible to many, without ſuch vouchers and ſuch corroborating teſtimony as I am fortunately able to produce, and are now before you. There are alſo many living witneſſes in England to moſt of the extraordinary relations that I have given, ſo that I hope to have full credit for ſuch remarks as no one but myſelf has probably had time and opportunities enough to make, and which are not ſuſceptible of demonſtration, except in thoſe places where the inſects are found.

Such as they are, I beg leave to lay them, with all diffidence and humility, before you and that illuſtrious Body of which you are Preſident; and if they ſhould in a ſmall degree meet with approbation, I ſhall be exceedingly ſatisfied.

Theſe inſects are known by various names. They belong to the TERMES of LINNÆUS, and other ſyſtematical naturaliſts.

By the Engliſh, { In the windward parts of Africa they are called *Bugga Bugs*.
In the Weſt Indies, *Wood Lice*, *Wood Ants*, or *White Ants*. }

By

By the French, { At Senegal, *Vague-Vagues.*
In the Weſt Indies, *Poux de Bois*, or *Fourmis Blanches.*

By the Bolms, or Sherbro people, in Africa, *Scantz.*

By the Portugueze in the Brazils, *Coupée* or *Cutters*, from their cutting things in pieces.

By this latter name and that of *Piercers* or *Eaters*, and ſimilar terms, they are diſtinguiſhed in various parts of the tropical regions.

The following are the ſpecific differences, given by Dr. SOLANDER, of ſuch inſects of this *genus* as I have obſerved and collected.

1. TERMES *bellicoſus* corpore fuſco, alis fuſceſcentibus: coſtâ ferrugineâ, ſtemmatibus ſubſuperis oculo propinquis, puncto centrali prominulo.
2. TERMES *mordax* nigricans, antennis pedibuſque teſtaceis, alis fuliginoſis: areâ marginali dilatatâ: coſtâ nigricante, ſtemmatibus inferis oculo approximatis, puncto centrali impreſſo.
3. TERMES *atrox* nigricans, ſegmentis abdominalibus margine pallidis, antennis pedibuſque teſtaceis, alis fuliginoſis: coſtâ nigrâ, ſtemmatibus inferis, puncto centrali impreſſo.
4. TERMES *deſtructor* nigricans, abdominis lineâ laterali luteâ, antennis teſtaceis, alis hyalinis: coſtâ luteſcente, ſtemmatibus ſubſuperis, puncto centrali obliterato.
5. TERMES *arborum* corpore teſtaceo, alis fuſceſcentibus: coſtâ luteſcente, capite nigricante, ſtemmatibus inferis oculo approximatis, puncto centrali impreſſo.

The Termites are repreſented by LINNÆUS as the greateſt plagues of both Indies, and are indeed every way between the

Tropics ſo deemed, from the vaſt damages they cauſe, and the loſſes which are experienced in conſequence of their eating and perforating wooden buildings, utenſils, and furniture, with all kinds of houſehold-ſtuff and merchandize, which are totally deſtroyed by them, if not timely prevented; for nothing leſs hard than metal or ſtone can eſcape their moſt deſtructive jaws.

They have been taken notice of by various travellers in different parts of the torrid zone; and indeed where numerous, as is the caſe in all equinoctial countries and iſlands that are not fully cultivated, if a perſon has not been incited by curioſity to obſerve them, he muſt have been very fortunate who, after a ſhort reſidence, has not been compelled to it for the ſafety of his property.

Theſe inſects have generally obtained the name of Ants, it may be preſumed, from the ſimilarity in their manner of living, which is, in large communities that erect very extraordinary neſts, for the moſt part on the ſurface of the ground, from whence their excurſions are made through ſubterraneous paſſages or covered galleries, which they build whenever neceſſity obliges, or plunder induces, them to march above ground, and at a great diſtance from their habitations carry on a buſineſs of depredation and deſtruction, ſcarce credible but to thoſe who have ſeen it. But notwithſtanding they live in communities, and are like the ants omnivorous; though like them at a certain period they are furniſhed with four wings, and emigrate or colonize at the ſame ſeaſon; they are by no means the ſame kind of inſects, nor does their form correſpond with that of *Ants* in any one ſtate of their exiſtence, which, like moſt other inſects, is changed ſeveral times.

The Termites reſemble the Ants alſo in their provident and diligent labour, but ſurpaſs them as well as the Bees, Waſps,

Beavers, and all other animals which I have ever heard of, in the arts of building, as much as the Europeans excel the least cultivated savages. It is more than probable they excel them as much in sagacity and the arts of government; it is certain they shew more substantial instances of their ingenuity and industry than any other animals; and do in fact lay up vast magazines of provisions and other stores; a degree of prudence which has of late years been denied, perhaps without reason, to the Ants (1).

Such however are the extraordinary circumstances attending their oeconomy and sagacity, that it is difficult to determine, whether they are more worthy of the attention of the curious and intelligent part of mankind on these accounts, or from the ruinous consequences of their depredations, which have deservedly procured them the name of *Fatalis* or *Destructor*.

As this is the case, it is a little surprising that an accurate natural history of these wonderful insects has not been attempted long since; especially as, according to BOSMAN (who wrote the beginning of this century) in his description of the Coast of Guinea, some curious cicumstances relative to them must have been known. According to that gentleman, the *King* was supposed to be as large as a Cray-fish (2). This, though a bad comparison, is pretty near the truth in respect to the size of the female, who is the *Common Mother* of the community;

(1) Though Ants have no occasion to lay up stores for winter in cold climates, they certainly must and do carry great quantities of provisions into their nests to feed the young brood; and most probably provide some before hand for fear of accidents, which might be fatal to the young ones, who, like all insects in the caterpillar state, are very voracious, and cannot bear disappointments of long duration.

(2) BOSMAN's Guinea, p. 260.

and, according to the mode we have adopted from time immemorial in speaking of Ants and Bees, the QUEEN.

These communities consist of one *male* and one *female* (who are generally the *common parents* of the whole, or greater part, of the rest), and of three orders of insects, apparently of very different species, but really the same, which together compose great commonwealths, or rather monarchies, if I may be allowed the term.

The great LINNÆUS, having seen or heard of but two of these orders, has classed the genus erroneously; for he has placed it among the *Aptera*, or insects without wings; whereas the chief order, that is to say, the insect in its perfect state, having four wings without any sting, it belongs to the *Neuroptera*; in which class it will constitute a new genus of many species (3).

The different species of this genus resemble each other in form, in their manner of living, and in their good and bad qualities: but differ as much as birds in the manner of building their habitations or nests, and in the choice of the materials of which they compose them.

There are some species which build upon the surface of the ground, or part above and part beneath, and one or two species, perhaps more, that build on the stems or branches of trees, sometimes aloft at a vast height.

(3) I have no doubt, from the account and figures given of the European Termes Pulsatorius, or Death Watch, by the illustrious BARON DE GEER, in his seventh volume of *Memoires pour servir à l' Histoire des Insectes*, that in their perfect state they have wings, and swarm or emigrate, and live in a manner analogous to those of hot climates; for they seem to have quite the external form of the exotic Termes, that is to say, of the first and third order. DE GEER, Memoires, tom. VII. p. 45. pl. IV. fig. 1, 2, 3, & 4.

Of

Of every ſpecies there are three orders; firſt, the working inſects, which, for brevity, I ſhall generally call *labourers*; next the fighting ones, or *ſoldiers*, which do no kind of labour; and, laſt of all, the winged ones, or *perfect inſects*, which are male and female, and capable of propagation. Theſe might very appoſitely be called the *nobility* or *gentry*, for they neither labour, or toil, or fight, being quite incapable of either, and almoſt of ſelf-defence. Theſe only are capable of being elected kings or queens; and nature has ſo ordered it, that they emigrate within a few weeks after they are elevated to this ſtate, and either eſtabliſh new kingdoms, or periſh within a day or two.

The *Termes bellicoſus* being the largeſt ſpecies is moſt remarkable and beſt known on the Coaſt of Africa. It erects immenſe buildings of well-tempered clay or earth, which are contrived and finiſhed with ſuch art and ingenuity, that we are at a loſs to ſay, whether they are moſt to be admired on that account, or for their enormous magnitude and ſolidity. It is from the two lower orders of this, or a ſimilar ſpecies, that LINNÆUS ſeems to have taken his deſcription of the *Termes Fatalis*; and moſt of the accounts brought home from Africa or Aſia of the white Ants are alſo taken from a ſpecies that are ſo much alike in external habit and ſize, and build ſo much in their manner, that one may almoſt venture to pronounce them mere variations of the ſame ſpecies.

The reaſon that the larger Termites have been moſt remarked is obvious; they not only build larger and more curious neſts, but are alſo more numerous, and do infinitely more miſchief to mankind. When theſe inſects attack ſuch things as we would not wiſh to have injured, we muſt conſider them as moſt pernicious; but when they are employed in deſtroying decayed trees

and ſubſtances which only incumber the ſurface of the earth, they may be juſtly ſuppoſed very uſeful, and for the reaſon tha they are in one ſenſe moſt pernicious, they are in the other moſt uſeful. In this reſpect they reſemble very much the common Flies, which are regarded by mankind in general as noxious, and at beſt as uſeleſs beings in the creation; but this is certainly for want of conſideration. There are not probably in all nature animals of more importance, and it would not be difficult to prove, that we ſhould feel the want of one or two ſpecies of large quadrupeds, much leſs than of one or two ſpecies of theſe deſpicable-looking inſects. Mankind in general are ſenſible that nothing is more diſagreeable, or more peſtiferous, than putrid ſubſtances; and it is apparent to all who have made obſervation, that thoſe little inſects contribute more to the quick diſſolution and diſperſion of putreſcent matter than any other. They are ſo neceſſary in all hot climates, that even in the open fields a dead animal or ſmall putrid ſubſtance cannot be laid upon the ground two minutes before it will be covered with Flies and their Maggots, which inſtantly entering quickly devour one part, and perforating the reſt in various directions, expoſe the whole to be much ſooner diſſipated by the elements. Thus it is with the Termites; the rapid vegetation in hot climates, of which no idea can be formed by any thing to be ſeen in this, is equalled by as great a degree of deſtruction from natural as well as accidental cauſes (4). It ſeems apparent, that when any thing whatever is arrived at its laſt degree of perfection, the Creator has decreed it ſhall

(4) The Guinea graſs, which is ſo well known and ſo much eſteemed by our planters in the Weſt Indies, grows in Africa thirteen feet high upon an average, which height it attains in about five or ſix months; and the growth of many other plants is as quick.

be

be totally destroyed as soon as possible, that the face of nature may be speedily adorned with fresh productions in the bloom of spring or the pride of summer: so when trees, and even woods, are in part destroyed by tornadoes or fire, it is wonderful to observe, how many agents are employed in hastening the total dissolution of the rest (5); but in the hot climates there are none so expert, or who do their business so expeditiously and effectually, as these insects, who in a few weeks destroy and carry away the bodies of large trees, without leaving a particle behind, thus clearing the place for other vegetables, which soon fill up every vacancy; and in places, where two or three years before there has been a populous town, if the inhabitants, as is frequently the case, have chosen to abandon it, there shall be a very thick wood, and not the vestige of a post to be seen, unless the wood has been of a species which, from its hardness, is called *iron wood.*

My general account of the Termites is taken from observations made on the *Termes bellicosus*, to which I was induced by the greater facility and certainty with which they could be made.

The nests of this species are so numerous all over the island of Bananas, and the adjacent continent of Africa, that it is scarce possible to stand upon any open place, such as a rice plantation, or other clear spot, where one of these buildings is not be seen within fifty paces, and frequently two or three are to be seen almost close to each other. In some parts near Senegal, as mentioned by Mons. ADANSON, their number, magnitude, and closeness of situation, make them appear like the villages of the natives (6): and you have yourself seen them perhaps still more numerous, though not so large, in New Holland. These

(5) See STILLINGFLEET's Tracts.

(6) "But of all the extraordinary things I observed, nothing struck me more "than certain eminences, which, by their height and regularity, made me take

These buildings are usually termed hills, by natives as well as strangers, from their outward appearance, which is that of little hills more or less conical, generally pretty much in the form of sugar loaves, and about ten or twelve feet in perpendicular height above the common surface of the ground (8) (9), tab. VII. fig. 1.

These

"them at a distance for an assemblage of negroes huts or a considerable village, "and yet they were only the nests of certain insects. They are round pyramids "from eight to ten feet high, upon nearly the same base, with a smooth surface "of rich clay, excessively hard and well built." ADANSON's Voyage to Senegal, 8vo, p. 153—337. Voyage de Senegal, 4to, p. 83 and 99.

Note. What Mr. ADANSON says of the opening which gives ingress and regress is manifestly a mistake, arising from the natural conclusion that those insects had some way out and in to their nests, without examining where it was. It will appear by this account, that they have many thousand ways out and in, but all subterraneous.

(7) JONSON, in his History of Gambia, says, "The Ant hills are remarkable "cast up in those parts by Pismires, some of them twenty foot in height, of "compasse to contayne a dozen men, with the heat of the sun baked into that "hardnesse, that we used to hide ourselves in the ragged toppes of them, when "we took up stands to shoot at deere or wild beasts." PURCHAS's Pilgrims, vol. H. p. 1570.

(8) "The Ants make nests of the earth about *twice the height of a man*." BOSMAN's Description of Guinea, p. 276—493.

(9) The labourers are not quite a quarter of an inch in length; however, for the sake of avoiding fractions, and of comparing them and their buildings with those of mankind more easily, I estimate their length or height so much, and the human standard of length or height, also to avoid fractions, at six feet, which is likewise above the height of men. If then one labourer is = to one-fourth of an inch = to six feet, four labourers are = to one inch in height = 24 feet, which multiplied by 12 inches, gives the comparative height of a foot of their building = 288 feet of the building of men, which multiplied by 10 feet, the supposed average height of one of their nests is = 2880 of our feet, which is 240 feet more than half a mile, or near five times the height of the great pyramid; and, as it is

These hills continue quite bare until they are six or eight feet high; but in time the dead barren clay, of which they are composed, becomes fertilized by the genial power of the elements in these prolific climates, and the addition of vegetable salts and other matters brought by the wind; and in the second or third year, the hillock, if not over-shaded by trees, becomes, like the rest of the earth, almost covered with grass and other plants; and in the dry season, when the herbage is burnt up by the rays of the sun, it is not much unlike a very large hay-cock (10).

Every one of these buildings consists of two distinct parts, the exterior and the interior.

The exterior is one large shell in the manner of a dome, large and strong enough to inclose and shelter the interior from the vicissitudes of the weather, and the inhabitants from the attacks of natural or accidental enemies. It is always, therefore, much stronger than the interior building, which is the habitable part divided with a wonderful kind of regularity and contrivance into an amazing number of apartments for the residence of the *king* and *queen*, and the nursing of their nu-

is proportionably wide at the base, a great many times its solid contents. If to this comparison we join that of the time in which the different buildings are erected, and consider the Termites as raising theirs in the course of three or four years, the immensity of their works sets the boasted magnitude of the antient wonders of the world in a most diminutive point of view, and gives a specimen of industry and enterprize as much beyond the pride and ambition of men as St. Paul's Cathedral exceeds an Indian hut.

(10) See a figure of one of those nests in SALMON's Universal Traveller, in the map of Gambia, where it is called a Pismire Hill: there is also a figure of one of the labouring insects; but as the hill is represented below all proportion, and the insect rather larger than life, it gives no idea of the building. I have not been able to find out from what author SALMON took this figure; and it is the only one I have met with.

merous

merous progeny; or for magazines, which are always found well filled with ſtores and proviſions.

I ſhall forbear at this time entering into a very minute account of the inſide of theſe wonderful buildings, as the bare recital might appear tedious; though I flatter myſelf, that when I have an opportunity of communicating it to the publick at large, the readers will follow me through an exact deſcription of them with pleaſure.

Theſe hills make their firſt appearance above ground by a little turret or two in the ſhape of ſugar loaves, which are run a foot high or more (11). Soon after, at ſome little diſtance, while the former are increaſing in height and ſize, they raiſe others, and ſo go on increaſing the number and widening them at the baſe, till their works below are covered with theſe turrets, which they always raiſe the higheſt and largeſt in the middle, and by filling up the intervals, between each turret, collect them as it were into one dome.

They are not very curious or exact about theſe turrets, except in making them very ſolid and ſtrong, and when by the junction of them the dome is compleated, for which purpoſe the turrets anſwer as ſcaffolds, they take away the middle ones entirely, except the tops (which joined together make the crown of the cupola) and apply the clay to the building of the works within, or to erecting freſh turrets for the purpoſe of raiſing the hillock ſtill higher; ſo that no doubt ſome part of the clay is uſed ſeveral times, like the boards and poſts of a maſon's ſcaffold.

(11) Some of theſe turrets are repreſented in the view of their hills, (tab. VII. fig. 3.). I have ſeen turrets on the ſides of theſe neſts four or five feet high (tab. VII. fig. 1. a. a. a.).

When theſe hills are at about little more than half their height, it is always the practice of the wild bulls to ſtand as centinels upon them, while the reſt of the herd is ruminating below (tab. VII.). They are ſufficiently ſtrong for that purpoſe, and at their full height anſwer excellently as places to look out. I have been with four men on the top of one of theſe hillocks. Whenever word was brought us of a veſſel in ſight, we immediately ran to ſome Bugga Bug hill, as they are called, and clambered up to get a good view, for upon the common ſurface it was ſeldom poſſible to ſee over the graſs or plants, which, in ſpite of monthly bruſhings, generally prevented all horizontal views at any diſtance.

The outward ſhell or dome is not only of uſe to protect and ſupport the interior buildings from external violence and the heavy rains; but to collect and preſerve a regular degree of genial warmth and moiſture which ſeems very neceſſary for hatching the eggs and cheriſhing the young ones.

The *royal chamber*, which I call ſo on account of its being adapted for, and occupied by, the *king* and *queen*, appears to be in the opinion of this little people of the moſt conſequence, being always ſituated as near the center of the interior building as poſſible, and generally about the height of the common ſurface of the ground, at a pace or two from the hillock. It is always nearly in the ſhape of half an egg or an obtuſe oval within, and may be ſuppoſed to repreſent a long oven (tab. VIII. fig. 1. and 2.).

In the infant ſtate of the colony, it is not above an inch or thereabout in length; but in time will be increaſed to ſix or eight inches or more in the clear, being always in proportion to the ſize of the *queen*, who, increaſing in bulk as in age, at length requires a chamber of ſuch dimenſions.

This

This ſingular part would bear a long deſcription, which I ſhall not trouble you with at preſent, and only obſerve, that its floor is perfectly horizontal; and in large hillocks, ſometimes an inch thick and upward of ſolid clay. The roof alſo, which is one ſolid and well-turned oval arch, is generally of about the ſame ſolidity, but in ſome places it is not a quarter of an inch thick, this is on the ſides where it joins the floor (tab. VIII. fig. 1. a. a.), and where the doors or entrances are made level therewith at pretty equal diſtances from each other (tab. VIII. fig. 2. and 4. b. b.)

Theſe entrances will not admit any animal larger than the ſoldiers or labourers, ſo that the *king*, and the *queen* (who is, at full ſize, a thouſand times the weight of a *king*) can never poſſibly go out.

The royal chamber, if in a large hillock, is ſurrounded by an innumerable quantity of others of different ſizes, ſhapes, and dimenſions; but all of them arched in one way or another, ſometimes circular, and ſometimes elliptical or oval.

Theſe either open into each other or communicate by paſſages as wide, and being always empty are evidently made for the ſoldiers and attendants, of whom it will ſoon appear great numbers are neceſſary, and of courſe always in waiting.

Theſe apartments are joined by the magazines and nurſeries. The former are chambers of clay, and are always well filled with proviſions, which to the naked eye ſeem to conſiſt of the raſpings of wood and plants which the Termites deſtroy, but are found in the microſcope to be principally the gums or inſpiſſated juices of plants. Theſe are thrown together in little maſſes, ſome of which are finer than others, and reſemble the ſugar about preſerved fruits, others are like tears of gum, one quite

quite transparent, another like amber, a third brown, and a fourth quite opaque, as we see often in parcels of ordinary gums.

These magazines are intermixed with the nurseries, which are buildings totally different from the rest of the apartments: for these are composed entirely of wooden materials, seemingly joined together with gums. I call them the nurseries because they are invariably occupied by the eggs, and young ones, which appear at first in the shape of labourers, but white as snow. These buildings are exceeding compact; and divided into many very small irregular-shaped chambers, not one of which is to be found of half an inch in width (tab. VIII. fig. 5.). They are placed all round the royal apartments, and as near as possible to them.

When the nest is in the infant state, the nurseries are close to the royal chamber; but as in process of time the queen enlarges, it is necessary to enlarge the chamber for her accommodation; and as she then lays a greater number of eggs, and requires a greater number of attendants, so it is necessary to enlarge and encrease the number of the adjacent apartments; for which purpose the small nurseries which are first built are taken to pieces, rebuilt a little farther off a size bigger, and the number of them encreased at the same time.

Thus they continually enlarge their apartments, pull down, repair, or rebuild, according to their wants, with a degree of sagacity, regularity, and foresight, not even imitated by any other kind of animals or insects that I have yet heard of.

There is one remarkable circumstance attending the nurseries, which I must not at this time omit. They are always found slightly overgrown with *mould* (tab. VIII. fig. 6), and plentifully sprinkled with small white globules about the size of a small pin's head. These at first I took to be the eggs; but,

on bringing them to the microſcope, they evidently appeared to be a ſpecies of muſhroom, in ſhape like our eatable muſhroom in the young ſtate in which it is pickled (tab. VIII. fig. 7.). They appear, when whole, white like ſnow a little thawed and then frozen again, and when bruiſed ſeem compoſed of an infinite number of pellucid particles, approaching to oval forms, and difficult to ſeparate; the mouldineſs ſeems likewiſe to be the ſame kind of ſubſtance [12].

The nurſeries are incloſed in chambers of clay, like thoſe which contain the proviſions, but much larger. In the early ſtate of the neſt they are not bigger than an hazel-nut, but in great hills are often as large as a child's head of a year old.

The diſpoſition of the interior parts of theſe hills is pretty much alike, except when ſome inſurmountable obſtacle prevents; for inſtance, when the *king* and *queen* have been firſt lodged near the foot of a rock or of a tree, they are certainly built out of the uſual form, otherwiſe pretty nearly according to the following plan.

The royal chamber is ſituated at about a level with the ſurface of the ground, at an equal diſtance from all the ſides of the building, and directly under the apex of the hill (tab. VII. fig. 2. A. A).

(12) Mr KONIG, who has examined theſe kind of neſts in the Eaſt Indies, in an Eſſay upon the Termites, read before the Society of Naturaliſts of Berlin, conjectures, that theſe muſhrooms are the food of the young inſects. This ſuppoſition implies, that the old ones have a method of providing for and promoting their growth; a circumſtance which, however ſtrange to thoſe unacquainted with the ſagacity of theſe Inſects, I will venture to ſay, from many other extraordinary facts I have ſeen of them, is not very improbable.

N. B. Mr. KONIG has not diſcovered the magazines of proviſions in the neſts which he opened, as far as I am informed; but I muſt obſerve here, that what I have learned of this gentleman's account was from an extempore tranſlation of the heads of it.

In

It is on all ſides, both above and below, ſurrounded by what I ſhould call the *royal apartments*, which have only labourers and ſoldiers in them, and can be intended for no other purpoſe than for theſe to wait in, either to guard or ſerve their common FATHER and MOTHER, on whoſe ſafety depends the happineſs, and, according to the negroes, even the exiſtence of the whole community.

Theſe apartments compoſe an intricate labyrinth, which extends a foot or more in diameter from the *royal chamber* on every ſide. Here the nurſeries and magazines of proviſions begin, and, being ſeparated by ſmall empty chambers and galleries, which go round them or communicate from one to the other, are continued on all ſides to the outward ſhell, and reach up within it two-thirds or three-fourths of its height, leaving an open area in the middle under the dome, which very much reſembles the nave of an old cathedral: this is ſurrounded by three or four very large Gothic-ſhaped arches, which are ſometimes two or three feet high next the front of the area, but diminiſh very rapidly as they recede from thence like the arches of aiſles in perſpectives, and are ſoon loſt among the innumerable chambers and nurſeries behind them.

All theſe chambers, and the paſſages leading to and from them, being arched, they help to ſupport one another; and while the interior large arches prevent them falling into the center, and keep the area open, the exterior building ſupports them on the outſide.

There are, comparatively ſpeaking, few openings into the great area, and they for the moſt part ſeem intended only to admit that genial warmth into the nurſeries which the dome collects.

The interior building or assemblage of nurseries, chambers, &c. has a flattish top or roof without any perforation, which would keep the apartments below dry, in case through accident the dome should receive any injury and let in water; and it is never exactly flat and uniform, because they are always adding to it by building more chambers and nurseries: so that the divisions or columns between the future arched apartments resemble the pinnacles upon the fronts of some old buildings, and demand particular notice as affording one proof that for the most part the insects project their arches, and do not make them, as I imagined for a long time, by excavation (tab. VII. fig. 2. B).

The area has also a flattish floor, which lays over the royal chamber, but sometimes a good height above it, having nurseries and magazines between (tab. VII. fig. 2. C.). It is likewise water-proof, and contrived, as far as I could guess, to let the water off, if it should get in, and run over by some short way into the subterraneous passages which run under the lowest apartments in the hill in various directions, and are of an astonishing size, being wider than the bore of a great cannon. I have a memorandum of one I measured, perfectly cylindrical, and thirteen inches in diameter (tab. VII. fig. 2. D. D.).

These subterraneous passages or galleries are lined very thick with the same kind of clay of which the hill is composed, and ascend the inside of the outward shell in a spiral manner, and winding round the whole building up to the top intersect each other at different heights, opening either immediately into the dome in various places, and into the interior building, the new turrets, &c. or communicating thereto by other galleries of different bores or diameters, either circular or oval.

From every part of these large galleries are various small pipes or galleries leading to different parts of the building.

Under

Under ground there are a great many which lead downward by ſloping deſcents three and four feet perpendicular among the gravel, from whence the labouring Termites cull the finer parts, which, being worked up in their mouths to the conſiſtence of mortar, becomes that ſolid clay or ſtone of which their hills and all their buildings, except their nurſeries, are compoſed.

Other galleries again aſcend and lead out horizontally on every ſide, and are carried under ground near to the ſurface a vaſt diſtance: for if you deſtroy all the neſts within one hundred yards of your houſe, the inhabitants of thoſe which are left unmoleſted farther off will nevertheleſs carry on their ſubterraneous galleries, and invade the goods and merchandizes contained in it by ſap and mine, and do great miſchief, if you are not very circumſpect.

But to return to the cities from whence theſe extraordinary expeditions and operations originate, it ſeems there is a degree of neceſſity for the galleries under the hills being thus large, being the great thoroughfares for all the labourers and ſoldiers going forth or returning upon any buſineſs whatever, whether fetching clay, wood, water, or proviſions; and they are certainly well calculated for the purpoſes to which they are applied, by the ſpiral ſlope which is given them; for if they were perpendicular the labourers would not be able to carry on their building with ſo much facility, as they aſcend a perpendicular with great difficulty, and the ſoldiers can ſcarce do it at all. It is on this account that ſometimes a road like a ledge is made on the perpendicular ſide of any part of the building within their hill, which is flat on the upper ſurface, and half an inch wide, and aſcends gradually like a ſtair-caſe, or like thoſe roads which are cut on the ſides of hills and mountains, that would otherwiſe

wiſe be inacceſſible: by which, and ſimilar contrivances, they travel with great facility to every interior part.

This too is probably the cauſe of their building a kind of bridge of one vaſt arch, which anſwers the purpoſe of a flight of ſtairs from the floor of the area to ſome opening on the ſide of one of the columns which ſupport the great arches, which muſt ſhorten the diſtance exceedingly to thoſe labourers who have the eggs to carry from the royal chamber to ſome of the upper nurſeries, which in ſome hills would be four or five feet in the ſtraighteſt line, and much more if carried through all the winding paſſages which lead through the inner chambers and apartments.

I have a memorandum of one of theſe bridges, half an inch broad, a quarter of an inch thick, and ten inches long, making the ſide of an elliptic arch of proportionable ſize; ſo that it is wonderful it did not fall over or break by its own weight before they got it joined to the ſide of the column above. It was ſtrengthened by a ſmall arch at the bottom, and had a hollow or groove all the length of the upper ſurface, either made purpoſely for the inhabitants to travel over with more ſafety, or elſe, which is not improbable, worn ſo by frequent treading (tab. VII. fig. 2. E. E.).

Thus I have deſcribed, as briefly as the ſubject would admit, and I truſt without exaggeration, thoſe wonderful buildings whoſe ſize and external form have often been mentioned by travellers, but whoſe interior and more curious parts are ſo little known, that I may venture to conſider my account of them as new, which is the only merit it has: for they are conſtructed upon ſo different a plan from any thing elſe upon the earth, and ſo complicated, that I cannot find words equal to

the task, and must therefore refer to the different figures, which, however extraordinary, scarce do justice to the subjects.

The nests before described are so remarkable on account of their size, that travellers have seldom, where they were to be seen, taken notice of any other; and have generally, when speaking of white Ants, described them as inhabitants of those hills. Those, however, which are built by the smaller species of those insects, are very numerous, and some of them exceedingly worth our attention; one sort in particular, which from their form I have named turret nests. These are a great deal less than the foregoing, and indeed much less in proportion to the size of the builders; but their external form is more curious, and their solidity considered they are prodigious buildings for so small an animal (13).

These buildings are upright cylinders composed of a well-tempered black earth or clay, about three quarters of a yard high, and covered with a roof of the same material in the shape of a cone, whose base extends over and hangs down three or four inches wider than the perpendicular sides of the cylinder, so that most of them resemble in shape the body of a round wind-mill; but some of the roofs have so little elevation in the middle, that they are pretty much in the shape of the top of a full-grown mushroom (tab. IX. fig. 1.)

After one of these turrets is finished, it is not altered or enlarged; but when no longer capable of containing the community, the foundation of another is laid within a few inches of it. Sometimes, though but rarely, the second is begun before the first is finished, and a third before they have completed the

(13) If their height is estimated and computed by the size of the builders, and compared with ours upon the like scale; each of them is four or five times the height of the monument, and a great many times its solid contents.

second:

ſecond: thus they will run up five or ſix of theſe turrets at the foot of a tree in the thick woods, and make a moſt ſingular group of buildings (tab. IX.).

The turrets are ſo ſtrongly built, that in caſe of violence they will much ſooner overſet from the foundation, and tear up the gravel and ſolid earth, than break in the middle; and in that caſe the inſects will frequently begin another turret and build it, as it were, through that which is fallen; for they will connect the cylinder below with the ground, and run up a new turret from its upper ſide, ſo that it will ſeem to reſt upon the horizontal cylinder only (tab. IX. fig. 5.).

I have not obſerved any thing elſe about theſe neſts that is remarkable, except the quality of the black brown clay, which is as dark coloured as rich vegetable mould, but burns to an exceeding fine and clear red brick. Within, the whole building is pretty equally divided into innumerable cells of irregular ſhapes; ſometimes they are quadrangular or cubic, and ſometimes pentagonal; but often the angles are ſo ill defined, that each half of a cell will be ſhaped like the inſide of that ſhell which is called the Sea-ear.

Each cell has two or more entrances, and as there are no pipes or galleries, no variety of apartments, no well-turned arches, wooden nurſeries, &c. &c. they do not by any means excite our admiration ſo much as the hill neſts, which are indeed collections of wonders.

There are two ſizes of theſe turret neſts, built by two different ſpecies of Termites. The larger ſpecies, the *Termes atrox*, in its perfect ſtate meaſures one inch and three-tenths from the extremities of the wings on the one ſide to the extremities on the other (tab. X. fig. 14.). The leſſer ſpecies, *Termes mordax*, meaſures

measures only eight-tenths of an inch from tip to tip (tab. X. fig. 10.

The next kind of nests, built by another species of this genus, the *Termes arborum*, have very little resemblance to the former in shape or substance. These are generally spherical or oval, and built in trees (14). Sometimes they are seated between the arms and the stems of trees, and very frequently may be seen surrounding the branch of a tree at the height of seventy or eighty feet; and (though but rarely of so large a size) as big as a very great sugar cask (15) (16).

They are composed of small particles of wood and the various gums and juices of trees, combined with, perhaps, those of the animals, and worked by those little industrious creatures into a paste, and so moulded into innumerable little cells of very different and irregular forms, which afford no amusing variety and nothing curious, but the immense quantity of inhabitants, young and old, with which they are at all times crouded; on which account they are sought for in order to feed young fowls, and especially for the rearing of Turkies. These nests are very compact, and so strongly attached to the boughs on which they are fixed, that there is no detaching them but by cutting them in pieces, or sawing off the branch; and they will sustain the force of a tornado as long as the tree on which they are fixed.

(14) The colour of these nests, like that of the roofed turrets, is black, from which, and their irregular surface and orbicular shape, they have been called *Negro Heads* by our first writers on the Carribbee Islands, and by the French, *Tetes des Negres*. See HUNTER'S EVELYN'S SILVA, p. 17.

I have never been able to discover what author Mr. EVELYN alludes to in this mention of the Negro Heads.

(15) LONG'S Jamaica, vol. III. p. 887.

(16) SLOANE'S Jamaica, vol. II. p. 221. and sequel.

This species has the external habit, size, and almost the colour, of the *Termes atrox* (tab. X. fig. 21.).

There are some nests built in those sandy plains which we call, after the Spaniards, *Savannas*, that resemble the hill nests first described. They are composed of a black mud, which is brought from a few inches below the white sand, and are built in the form of an imperfect cone, or bell-shaped, having their tops rounded. These nests are generally about four or five feet high (17). As I saw these only in passing through various Savannahs upon other pursuits, I can say very little of their interior parts. They seemed to be inhabited by nearly as large insects, differing very little except in colour, which is lighter than that of the *Termites bellicosi*.

Having given some idea of the nests, I shall beg your patient reading of a more particular account of the insects themselves, which will be exceeding necessary to a tolerable acquaintance with their oeconomy and management, their manner of building, fighting, and marching, and to a more particular account of their uses in the creation, and of the vast mischief they cause to mankind.

(17) "The nests of Ants are about four feet wide at the base, and two high, "of an hemispherical form. Though made in loose sand, they are so hard as "not to be broken without great efforts, and a laden cart could not break "through.—In October and November they add a new story.—The Cochons "de Terre (the Left Ant-eater of Mr. PENNANT) make holes in these nests eight "inches in diameter and six deep; *and having destroyed the inhabitants, the nest "is abandoned, but sometimes the Ants repair it.*" This last paragraph seems rather founded on conjecture. Voyage au Cap, par M. L'Abbé DE LA CAILLE, p. 305—356.

OVIEDO also says Ants make hillocks as high as a man.

Among

Among these you will find, I must confess, some very extraordinary relations, and many that do not admit a possibility of demonstration; such is the description of the form of an army of the *Termites viarum* marching, and the account of the regularity used by the *Termites bellicosi* in repairing a breach in their hills. But the very singular facts, of which you have the proofs before you, are sufficient I should conceive to procure me belief for the others. Should any person doubt, I would wish them to consider, that a student of nature and nature's laws, in any matter relating thereto, has no temptation to transgress the bounds of truth. I am very sensible, that the works of the creation, and the order thereof, are established in the highest wisdom; that it is as absurd to attempt to exaggerate as to detract from them; and can only serve to expose the ignorance of him who attempts it. Besides, what I have here advanced must be confirmed or contradicted in two or three years, since it will doubtless be examined into by all the curious who visit tropical regions.

I have observed before, that there are of every species of *Termites* three orders; of these orders the working insects or labourers are always the most numerous; in the *Termes bellicosus* there seems to be at the least one hundred labourers to one of the fighting insects or soldiers. They are in this state about one-fourth of an inch long, and twenty-five of them weigh about a grain; so that they are not so large as some of our ants (tab. X. fig. 6.). From their external habit and fondness for wood, they have been very expressively called *Wood Lice* by some people, and the whole genus has been known by that name, particularly among the French. They resemble them, it is true, very much at a distance, but they run as fast or faster than any

other insects of their size, and are incessantly bustling about their affairs (18).

The second order, or soldiers, have a very different form from the labourers, and have been by some authors supposed to be the males, and the former neuters; but they are, in fact, the same insects as the foregoing, only they have undergone a change of form, and approached one degree nearer to the perfect state. They are now much larger, being half an inch long, and equal in bulk to fifteen of the labourers (tab. X. fig. 8).

There is now likewise a most remarkable circumstance in the form of the head and mouth; for in the former state the mouth is evidently calculated for gnawing and holding bodies; but in this state, the jaws being shaped just like two very sharp awls a little jagged (tab. X. fig 9.), they are incapable of any thing but piercing or wounding, for which purposes they are very effectual, being as hard as a crab's claw, and placed in a strong horny head, which is of a nut-brown colour, and larger than all the rest of the body together, which seems to labour under great difficulty in carrying it: on which account perhaps the animal is incapable of climbing up perpendicular surfaces.

The third order, or the insect in its perfect state, varies its form still more than ever. The head, thorax, and abdomen, differ almost entirely from the same parts in the labourers and soldiers; and, besides this, the animal is now furnished with four fine large brownish, transparent, wings, with which it is at the time of emigration to wing its way in search of a new settle-

(18) ROCHFORT, in the History of the Carribee Islands, calls them Wood Lice, and mentions the destruction they make, &c. p. 149.

ment (19).

ment (19). In ſhort, it differs ſo much from its form and appearance in the other two ſtates, that it has never been ſuppoſed to be the ſame animal, but by thoſe who have ſeen it in the ſame neſt; and ſome of theſe have diſtruſted the evidence of their ſenſes. It was ſo long before I met with them in the neſts myſelf, that I doubted the information which was given me by the natives, that they belonged to the ſame family (tab. X. fig. 1.) Indeed we may open twenty neſts without finding one winged one, for thoſe are to be found only juſt before the commencement of the rainy ſeaſon, when they undergo the laſt change, which is preparative to their colonization. Add to this, they ſometimes abandon an outward part of their building, the community being diminiſhed by ſome accident to me unknown. Sometimes too different ſpecies of the real Ant (Formica) poſſeſs themſelves by force of a lodgement, and ſo are frequently diſlodged from the ſame neſt, and taken for the ſame kind of inſects. This I know is often the caſe with the neſts of the ſmaller ſpecies, which are frequently totally abandoned by the Termites, and completely inhabited by different ſpecies of Ants, Cockroaches, Scolopendræ, Scorpions, and other vermin, fond of obſcure retreats, that occupy different parts of their roomy buildings; which clearly accounts for your having met with the real Ants in thoſe neſts in New Holland.

(19) "There is a ſort that frequently flies, having red wings.—This flying "ſort flings up the largeſt hills, and is wonderfully nimble and induſtrious." KOLBEIN's Cape of Good Hope, 8vo, vol. II. p. 173.

DAPPER calls the Wood Ants *Acolalan*, and ſays it becomes as big as one's thumb, and then takes wing. Deſcription de l'Afrique, folio, p. 459.

Ins

In the winged ſtate they have alſo much altered their ſize as well as form. Their bodies now meaſure between ſix and ſeven tenths of an inch in length, and their wings above two inches and a half from tip to tip, and they are equal in bulk to about thirty labourers, or two ſoldiers. They are now alſo furniſhed with two large eyes placed on each ſide of the head, and very conſpicuous; if they have any before, they are not eaſily to be diſtinguiſhed. Probably in the two firſt ſtates, their eyes, if they have any, may be ſmall like thoſe of moles; for as they live like theſe animals always underground, they have as little occaſion for theſe organs, and it is not to be wondered at that we do not diſcover them; but the caſe is much altered when they arrive at the winged ſtate in which they are to roam, though but for a few hours, through the wide air, and explore new and diſtant regions. In this form the animal comes abroad during or ſoon after the firſt tornado, which at the latter end of the dry ſeaſon proclaims the approach of the enſuing rains, and ſeldom waits for a ſecond or third ſhower, if the firſt, as is generally the caſe, happens in the night, and brings much wet after it (20).

The quantities that are to be found the next morning all over the ſurface of the earth, but particularly on the waters, is aſtoniſhing; for their wings are only calculated to carry them

(20) "At night I viſited Mr. HARRISON on board the ſloop; during the time "we had a dreadful tornado, in which a ſort of large flies with long wings came "on board in ſuch prodigious numbers, that flying into the flames of the "candles, the table was ſoon covered with thoſe that burnt their wings; and "others, which were not burnt, as they walked along the table ſhed their wings, "and then were nothing but ſo many perfect large maggots." June 10, 1732. MOOR's Travels, p. 118.

a few hours, and after the rising of the sun not one in a thousand is to be found with four wings, unless the morning continues rainy, when here and there a solitary being is seen winging its way from one place to another, as if solicitous only to avoid its numerous enemies, particularly various species of Ants which are hunting on every spray, on every leaf, and in every possible place, for this unhappy race, of which probably not a pair in many millions get into a place of safety, fulfil the first law of nature, and lay the foundation of a new community.

Not only all kinds of ants, birds, and carnivorous reptiles, as well as insects, are upon the hunt for them, but the inhabitants of many countries, and particularly of that part of Africa where I was, eat them (21) (22) (23) (24) (25).

On

(21) Mr. KONIG, in an Essay upon these Insects, read before the Society of Naturalists of Berlin, says, That, in some parts of the East Indies, the queens are given alive to old men for strengthening the back, and that the natives have a method of catching the winged insects, which he calls females, before the time of emigration. They make two holes in the nest; the one to windward, and the other to leeward. At the leeward opening they place the mouth of a pot, previously rubbed within with an aromatic herb called *Bergera*, which is more valued there than the laurel in Europe. On the windward side they make a fire of stinking materials, which not only drives these insects into the pots, but frequently the hooded snakes also, on which account they are obliged to be cautious in removing them. By this method they catch great quantities, of which they make with flour a variety of pastry, which they can afford to sell very cheap to the poorer ranks of people. Mr. KONIG adds, that in seasons when this kind of food is very plentiful, the too great use of it brings on an epidemic colic and dysentery, which kills in two or three hours.

I have

On the following morning, however, as I have obſerved, they are to be ſeen running upon the ground in chace of each other;

I have not found the Africans ſo ingenious in procuring or dreſſing them. They are content with a very ſmall part of thoſe which, at the time of ſwarming, or rather of emigration, fall into the neighbouring waters, which they ſkim off with calabaſhes, bring large kettles full of them to their habitations, and parch them in iron pots over a gentle fire, ſtirring them about as is uſually done in roaſting coffee. In that ſtate, without ſauce or any other addition, they ſerve them as delicious food; and they put them by hands-full into their mouths, as we do comfits. I have eat them dreſſed this way ſeveral times, and think them both delicate, nouriſhing, and wholeſome; they are ſomething ſweeter, but not ſo fat and cloying as the caterpillar or maggot of the *Palm-tree Snout-beetle*, *Curculio Palmarum*, which is ſerved up at all the luxurious tables of Weſt Indian epicures, particularly of the French, as the greateſt dainty of the Weſtern world.

According to the Baron DE GEER, Mr. SPARRMAN ſays, that the Hottentots eat theſe inſects, and even grow fat upon them; but does not ſay what methods they take to procure or dreſs them. DE GEER, *Memoires des Inſectes*, tom. VII. p. 49.

(22) PISO, DE LAET, MARCGRAVE, and other writers, mention their being an article of diet in different parts of South America.

"Alia præterea datur grandis ſpecies *Tama-ioura* dicta digiti articulum adæquans. Quarum etiam clunes deſſecantur et friguntur pro bono alimento." PISO, Hiſt. Natural. lib. I. p. 9. lib. V. 291.

(23) MARCGR. Hiſt. Nat. 56.

(24) "Denique formicæ hic viſuntur grandiſſimæ, quas indigenæ vulgo comedunt; et in foris venales habent." DE LAET. Americæ Utriuſque Deſcriptio, p. 333.

"Formicis veſcebantur, eaſque ſtudioſe ad victum educabant. Ibid. p. 379."

(25) Sir HANS SLOANE ſays, the ſilk-cotton-tree worm is eſteemed by the Indians and negroes beyond marrow. This worm is no more than a large maggot, being the Caterpillar of a large Capricorn Beetle, or Goat Chafer: the Larva of a pretty large Cerambix (the *Lamia Tribulus* of FABRICIUS) which is alſo brought from Africa, where I have eaten thoſe worms roaſted. This inſect is moſt probably to be found in all countries where the ſilk-cotton-tree (*Bombax*) is indigenous. SLOANE's Jamaica, vol. II. p. 193.

I have

other; ſometimes with one or two wings ſtill hanging to their bodies, which are not only uſeleſs, but ſeem rather cumberſome (26).

The greater part have no wings, but they run exceeding faſt, the males after the females; I have ſometimes remarked two males after one female, contending with great eagerneſs who ſhould win the prize, regardleſs of the innumerable dangers that ſurrounded them.

They are now become from one of the moſt active, induſtrious, and rapacious, from one of the moſt fierce and implacable little animals in the world, the moſt innocent, helpleſs, and cowardly; never making the leaſt reſiſtance to the ſmalleſt Ant. The Ants are to be ſeen on every ſide in infinite numbers, of various ſpecies and ſizes, dragging theſe annual victims of the laws of nature to their different neſts. It is wonderful that a pair ſhould ever eſcape ſo many dangers, and get into a place of ſecurity. Some, however, are ſo fortunate; and being found by ſome of the labouring inſects that are continually running about the ſurface of the ground under their covered galleries, which I ſhall ſhortly deſcribe, are *elected* KINGS and QUEENS of new ſtates; all thoſe who are not ſo elected and preſerved certainly periſh, and moſt probably in the courſe of the following day. The manner in which theſe labourers protect the happy pair from their innumerable enemies, not only on the day of the

I have diſcourſed with ſeveral gentlemen upon the taſte of the white Ants; and on comparing notes we have always agreed, that they are moſt delicious and delicate eating. One gentleman compared them to ſugared marrow, another to ſugared cream and a paſte of ſweet almonds.

(26) LIGON obſerved them, but does not know what they are. LIGON's Barbadoes, p. 63.

maſſacre of almoſt all their race, but for a long time after, will I hope juſtify me in the uſe of the term ELECTION. The little induſtrious creatures immediately encloſe them in a ſmall chamber of clay ſuitable to their ſize, into which at firſt they leave but one ſmall entrance, large enough for themſelves and the ſoldiers to go in and out, but much too little for either of the royal pair to make uſe of; and when neceſſity obliges them to make more entrances, they are never larger; ſo that, of courſe, the *voluntary ſubjects* charge themſelves with the taſk of providing for the offſpring of their ſovereigns as well as to work and to fight for them until they ſhall have raiſed a progeny capable at leaſt of dividing the taſk with them.

It is not until this time, probably, that they conſummate their marriage, as I never ſaw a pair of them joined. The buſineſs of propagation, however, ſoon commences, and the labourers having conſtructed a ſmall wooden nurſery, as before deſcribed, carry the eggs and lodge them there as faſt as they can obtain them from the *queen*.

About this time a moſt extraordinary change begins to take place in the *queen*, to which I know nothing ſimilar, except in the PULEX PENETRANS of LINNÆUS, the JIGGER of the *Weſt Indies*, and in the different ſpecies of COCCUS, COCHINEAL. The abdomen of this female begins gradually to extend and enlarge to ſuch an enormous ſize, that an *old queen* will have it increaſed ſo as to be *fifteen hundred* or *two thouſand times* the bulk of the reſt of her body, and *twenty or thirty thouſand times* the bulk of a labourer, as I have found by carefully weighing and computing the different ſtates (tab. X. fig. 3.). The ſkin between the ſegments of the abdomen extends in every direction; and at laſt the ſegments are removed to half an inch

inch diſtance from each other, though at firſt the length of the whole abdomen is not half an inch. They preſerve their dark brown colour, and the upper part of the abdomen is marked with a regular ſeries of brown bars from the thorax to the poſterior part of the abdomen, while the intervals between them are covered with a thin, delicate, tranſparent ſkin, and appear of a fine cream colour, a little ſhaded by the dark colour of the inteſtines and watery fluid ſeen here and there beneath. I conjecture the animal is upward of two years old when the abdomen is increaſed to three inches in length: I have ſometimes found them of near twice that ſize. The abdomen is now of an irregular oblong ſhape, being contracted by the muſcles of every ſegment, and is become one vaſt matrix full of eggs, which make long circumvolutions through an innumerable quantity of very minute veſſels that circulate round the inſide in a ſerpentine manner, which would exerciſe the ingenuity of a ſkilful anatomiſt to diſſect and develope. This ſingular matrix is not more remarkable for its amazing extenſion and ſize than for its periſtaltic motion, which reſembles the undulating of waves, and continues inceſſantly without any apparent effort of the animal; ſo that one part or other alternately is riſing and ſinking in perpetual ſucceſſion, and the matrix ſeems never at reſt (27), but is always protruding eggs to the amount (as I have frequently counted in old queens) of ſixty in a minute (28), or eighty thouſand and upward in one day of twenty-four hours (29). Theſe

(27) "We may obſerve in a *queen*, diſtended with egg, a partition along the "back, and a continued motion from one extreme to the other, much like "that we find in ſilk-worms." Account of Engliſh Ants by GOULD, p. 22.

(28) I cannot poſitively aſſert, that the old queens yield eggs ſo plentifully at all

These eggs are instantly taken from her body by her attendants (of whom there always are, in the royal chamber and the galleries adjacent, a sufficient number in waiting) and carried to the nurseries, which in a great nest may some of them be four or five feet distant in a straight line, and consequently much farther by their winding galleries. Here, after they are hatched, the young are attended and provided with every thing necessary until they are able to shift for themselves, and take their share of the labours of the community. The foregoing, I flatter myself, is an accurate description and account of the *Termes bellicosus* or species that builds the large nests in its different states.

Those which build either the roofed turrets or the nests in the trees, seem in most instances to have a strong resemblance to them, both in their form and oeconomy, going through the same changes from the egg to the winged state. The *queens* also increase to a great size when compared with the labourers; but very short of those *queens* before described. The largest are from about an inch to an inch and a half long, and not much thicker than a common quill. There is the same kind of peristaltic motion in the abdomen, but in a much smaller de-

times, but the protruding them being the consequence of the peristaltic motion, it would seem involuntary on their parts, and the number, or nearly so, always indispensable: the astonishing multitudes of inhabitants found in their nests also countenance this opinion strongly.

(29) Since the reading of this paper, Mr. JOHN HUNTER, so celebrated for his great skill and experience in comparative anatomy, has dissected two young queens. He finds the abdomen contains two ovaria, in each of which are many hundred ova-ducts, and in each of these ova-ducts a vast many eggs; so that there seems no doubt of the fact, as the matrix of a *full-grown queen* must be calculated for the production and yielding of a prodigious number of eggs. He has also dissected the kings; The result of these dissections, with some further particulars, will be related in another paper.

gree;

gree; and, as the animal is incapable of moving from her place, the eggs no doubt are carried to the different cells by the labourers, and reared with a care similar to that which is practised in the larger nests.

It is remarkable of all these different species, that the working and the fighting insects never expose themselves to the open air; but either travel under ground, or within such trees and substances as they destroy, except, indeed, when they cannot proceed by their latent passages, and find it convenient or necessary to search for plunder above ground. In that case they make pipes of that material with which they build their nests. The larger fort use the red clay; the turret builders use the black clay; and those which build in the trees employ the same ligneous substances of which their nests are composed (30) (31) (32).

With

(30) "Small birds, fowls, Lizards, and other reptiles, search for them as the "most delicious morsels; therefore they never go abroad but under their covered "ways." DU TERTRE, quarto, vol. II. p. 345.

(31) "The earth hereabouts was all filled with a species of a white Ant, called "Vag Vague, different from that which I have elsewhere described. This, "instead of raising pyramids, continues buried under ground, and never makes "itself known but by small cylindrical galleries of the thickness of a goose quill, "which it erects against the several bodies it designs to attack. These galleries "are formed of earth with infinite delicacy of workmanship. The Vag Vagues "make use of them as of covert-ways, to work without being seen; and what-"ever they fasten themselves to, whether it be leather, cloth, linen, books, or "wood, it is surely gnawed and consumed. I should have thought myself pretty "well off, had they only attacked the reeds of my hut; but they pierced "through a trunk which stood on trestles a foot above the ground, and gnawed "most of my book." ADANSON's Voyage to Guinea, 179—337.

N. B. Mr. ADANSON is certainly mistaken when he says, "They never make "themselves known but by their covered ways, and is the only one whom I have "met

With these materials they completely line most of the roads leading from their nests into the various parts of the country, and travel out and home with the utmost security in all kinds of weather. If they meet a rock or any other obstruction, they will make their way upon the surface; and for that purpose erect a covered way or arch, still of the same materials, continuing it with many windings and ramifications through large groves; having, where it is possible, subterranean pipes running parallel with them, into which they sink and save themselves, if their galleries above ground are destroyed by any violence, or the tread of men or animals alarms them. When one chances by accident to enter any solitary grove, where the ground is pretty well covered with their arched galleries, they give the alarm by loud hissings, which we hear distinctly at every step we make; soon after which we may examine their galleries in vain for the insects, but find little holes, just large enough for them, by which they have made their escape into their subterraneous roads. These galleries are large enough for them to pass and repass so as to prevent any stoppages (though there are always numerous passengers) and shelter them equally from light and

" met with who has been attacked while living by the *white Ants*." I have some doubt, that, although the approaches of the *Termites* were carried up to his bed, the bites he received were from *real Ants*, of which there are some scarce visible which are very numerous and produce great pain; whereas the bite of the *Termes* lets out much blood, and shews not the least symptom of venom. See DU TERTRE's Antilles, vol. II. p. 344. and Descript. de l'Afrique, par LABAT, tom. III. p. 298.

(32) See SLOANE, LIGON, LINNÆUS (Termes Fatalis), FORSKAL (Termes Arda), and the various voyages to Africa and both Indies.

air, as well as from their enemies, of which the ants, being the moſt numerous, are the moſt formidable.

The *Termites*, except their heads, are exceeding ſoft, and covered with a very thin and delicate ſkin; being blind, they are no match on open ground for the ants, who can ſee, and are all of them covered with a ſtrong horny ſhell not eaſily pierced, and are of diſpoſitions bold, active, and rapacious. Whenever the Termites are diſlodged from their covered ways, the various ſpecies of the former, who probably are as numerous above ground as the latter are in their ſubterraneous paſſages, inſtantly ſeize and drag them away to their neſts, to feed the young brood (33) (34) (35). The Termites are therefore exceeding ſolicitous

(33) Sir HANS SLOANE was certainly miſtaken in his account of the Wood Ants; it is utterly improbable that they ſhould go into the neſts of the red Ants and kill them. It is moſt probable, the error has ariſen from Sir HANS's having confounded the two genera of inſects the *Formica* and *Termes* together, which made him never ſpeak of them with preciſion. The reverſe of his account is moſt likely, which is, that the *Formicæ* will follow their plunder into the neſts of the *Termites* and deſtroy them; for the latter always keep within their neſts or covered ways, avoiding all communications with other inſects and animals, and never meddling with them but when dead; whereas the *Formicæ* ramble about every where, and enter every cranny and hole that is large enough, and attack not only inſects and reptiles but even large animals. See SLOANE's Voyage to Jamaica, vol. II. p. 221, 222. tab. 238. *Hiſt. de l'Academie Royale des Sciences*, 1701, p. 16. *Fourmis de Viſite.*

(34) LIGON mentions another *ſort* of *Ants*, and deſcribes the galleries of the *Termites*. LIGON's Barbadoes, p. 64, 65.

(35) MERIAN ſays, the *Ants* make neſts above eight feet high, by which I apprehend ſhe means the neſts of the *Termites*; but in ſpeaking of the manners of the inſects ſhe certainly means ſome ſpecies of the *Formica*. Thoſe which are deſcribed as ſtripping the trees are a ſpecies called, in Tobago, *Para-ſol-Ants*, becauſe they cut out of the leaves of certain trees and plants pieces almoſt circular, and

ſolicitous about the preſerving their covered ways in good repair; and if you demoliſh one of them, for a few inches in length, it is wonderful how ſoon they rebuild it. At firſt in their hurry they get into the open part an inch or two, but ſtop ſo ſuddenly that it is very apparent they are ſurprized: for though ſome run ſtraight on, and get under the arch as ſpeedily as poſſible in the further part, moſt of them run as faſt back, and very few will venture through that part of the track which is left uncovered. In a few minutes you will perceive them rebuilding the arch, and by the next morning they will have reſtored their gallery for three or four yards in length, if ſo much has been ruined; and upon opening it again will be found as numerous as ever, under it, paſſing both ways. If you continue to deſtroy it ſeveral times, they will at length ſeem to give up the point, and build another in a different direction; but, if the old one led to ſome favourite plunder, in a few days will rebuild it again; and, unleſs you deſtroy their neſt, never totally abandon their gallery.

The *Termites arborum*, thoſe which build in trees, frequently eſtabliſh their neſts within the roofs and other parts of houſes, to which they do conſiderable damage, if not timely extirpated.

The large ſpecies are, however, not only much more deſtructive, but more difficult to be guarded againſt, ſince they make their approaches chiefly under ground, deſcending below the foundations of houſes and ſtores at ſeveral feet from the ſurface, and riſing again either in the floors, or entering at the

and are to be ſeen all the year round travelling from the plants along their road to the neſt, with each one of theſe circular pieces of leaves in their jaws, which, from their ſhape and colour, give a very good idea of people walking with paraloſs (umbrellas). MERIAN, *Inſectes de Surinam*, p. 18.

bottoms

bottoms of the posts, of which the sides of the buildings are composed, bore quite through them, following the course of the fibres to the top, or making lateral perforations and cavities here and there as they proceed.

While some are employed in gutting the posts, others ascend from them, entering a rafter or some other part of the roof. If they once find the thatch, which seems to be a favourite food, they soon bring up wet clay, and build their pipes or galleries through the roof in various directions, as long as it will support them; sometimes eating the palm-tree leaves and branches of which it is composed, and, perhaps (for variety seems very pleasing to them) the rattan or other running plant which is used as a cord to tye the various parts of the roof together, and that to the posts which support it: thus, with the assistance of the rats, who during the rainy season are apt to shelter themselves there, and to burrow through it, they very soon ruin the house by weakening the fastenings and exposing it to the wet. In the mean time the posts will be perforated in every direction as full of holes as that timber in the bottoms of ships which has been bored by the worms; the fibrous and knotty parts, which are the hardest, being left to the last (36).

They

(36) The sea worms, so pernicious to our shipping, appear to have the same office allotted them in the waters which the Termites have on the land. They will appear, on a very little consideration, to be most important beings in the great chain of creation, and pleasing demonstrations of that infinitely wise and gracious Power which formed, and still preserves, the whole in such wonderful order and beauty: for if it was not for the rapacity of these and such animals, tropical rivers, and indeed the ocean itself, would be choked with the bodies of trees which are annually carried down by the rapid torrents, as many of them would last for ages, and probably be productive of evils, of which, happily,

They ſometimes, in carrying on this buſineſs, find, I will not pretend to ſay how, that the poſt has ſome weight to ſupport, and then if it is a convenient track to the roof, or is itſelf a kind of wood agreeable to them, they bring their mortar, and fill all or moſt of the cavities, leaving the neceſſary roads through it, and as faſt as they take away the wood replace the vacancy with that material; which being worked together by them cloſer and more compactly than human ſtrength or art could ram it, when the houſe is pulled to pieces, in order to examine if any of the poſts are fit to be uſed again, thoſe of the ſofter kinds are often found reduced almoſt to a ſhell, and all or a greater part transformed from wood to clay as ſolid and as hard as many kinds of free-ſtone uſed for building in England. It is much the ſame when the *Termites bellicoſi* get into a cheſt or trunk containing cloaths and other things; if the weight

we cannot in the preſent harmonious ſtate of things form any idea *; whereas now being conſumed by theſe animals, they are more eaſily broken in pieces by the waves; and the fragments which are not devoured become ſpecifically lighter, and are conſequently more readily and more effectually thrown on ſhore, where the ſun, wind, inſects, and various other inſtruments, ſpeedily promote their entire diſſolution, and reſtore the conſtituent particles to that

——— "Mighty hand,
"Which, ever buſy, wheels the ſilent ſpheres;
"Works in the ſecret deep; ſhoots, ſteaming, thence
"The fair profuſion that o'erſpreads the ſpring:
"Flings from the ſun direct the flaming day;
"Feeds every creature; hurls the tempeſt forth;
"And, as on earth this grateful change revolves,
"With tranſport touches all the ſprings of life." THOMSON.

* That wood will endure in water an amazing number of ages, is apparent from the *oak ſtakes* which were driven into the bed of the river Thames on the invaſion of this iſland by *Julius Cæſar*, one of which is to be ſeen in Sir ASHTON LEVER's Muſeum, and likewiſe from thoſe bodies of trees which are daily found in the bogs and moraſſes of Great Britain and Ireland, which after a duration, the former of eighteen hundred, the latter of upwards of two thouſand years, are found in a perfect ſtate of preſervation.

above

above is great, or they are afraid of Ants or other enemies, and have time, they carry their pipes through, and replace a great part with clay, running their galleries in various directions. The tree Termites, indeed, when they get within a box, often make a neſt there, and being once in poſſeſſion deſtroy it at their leiſure. They did ſo to the pyramidal box which contained my compound microſcope. It was of mahogany, and I had left it in the ſtore of Governor CAMPBELL of Tobago, for a few months, while I made the tour of the Leeward Iſlands. On my return I found theſe inſects had done much miſchief in the ſtore, and, among other things, had taken poſſeſſion of the microſcope, and eaten every thing about it except the glaſs or metal, and the board on which the pedeſtal is fixed, with the drawers under it, and the things incloſed. The cells were built all round the pedeſtal and the tube, and attached to it on every ſide. All the glaſſes which were covered with the wooden ſubſtance of their neſts retained a cloud of a gummy nature upon them that was not eaſily got off, and the lacquer or burniſh with which the braſs work was covered was totally ſpoiled. Another party had taken a liking to the ſtaves of a Madeira caſk, and had let out almoſt a pipe of fine old wine. If the large ſpecies of Africa (the *Termites bellicoſi*) had been ſo long in the uninterrupted poſſeſſion of ſuch a ſtore, they would not have left twenty pounds weight of wood remaining of the whole building, and all that it contained (37).

Theſe

(37) Captain PHILLIP of the navy, who was ſome time at the Brazils in the ſervice of Portugal, gives me the following relation. " An engineer returned " from ſurveying the country, left his trunk on a table: the next morning, not " only all his cloaths were deſtroyed by *white Ants* or *Cutters*, but his papers alſo; " and the latter in ſuch a manner, that there was not a bit left of an inch ſquare.

These insects are not less expeditious in destroying the shelves, wainscotting, and other fixtures of an house, than the house itself. They are for ever piercing and boring in all directions, and sometimes go out of the broadside of one post into that of another joining to it; but they prefer and always destroy the softer substances the first, and are particularly fond of pine and fir-boards, which they excavate and carry away with wonderful dispatch and astonishing cunning: for, except a shelf has something standing upon it, as a book, or any thing else which may tempt them, they will not perforate the surface, but artfully preserve it quite whole, and eat away all the inside, except a few fibres which barely keep the two sides connected together, so that a piece of an inch-board which appears solid to the eye will not weigh much more than two sheets of pasteboard of equal dimensions, after these animals have been a little while in possession of it (38) (39) (40) (41). In short, the Termites are so

"The black lead pencils were likewise so completely destroyed, that the smallest "piece, even of the *black lead* could not be found. The cloaths were not "entirely cut to pieces and carried away, but appeared as if moth-eaten, there "being scarce a piece as large as a shilling that was free from small holes; and "it was further remarkable, that some *silver coin*, which was in the trunk, had a "number of black specks on it, caused by something so corrosive that they could "not easily be rubbed off even with sand." Queen's-square, Wednesday, Jan. 17, 1781.

(38) "The white Ants are transparent as glass, and bite so forcibly, that in "the space of one night alone they can eat their way through a thick wooden "chest of goods, and make it as full of holes, as if it had been shot through "with hail-shot." BOSMAN's Guinea, p. 276, 7. 493.

(39) MOORE's Travels, p. 221.

(40) Voyage de LABAT aux Isles, tom. II. p. 331.

(41) "The wood Ants are the most pernicious of all others, being so very "destructive to timber of most sorts, that, if not prevented, they will in a few "years

ſo inſidious in their attacks, that we cannot be too much on our guard againſt them: they will ſometimes begin and raiſe their works, eſpecially in new houſes, through the floor (42). If you deſtroy the work ſo begun, and make a fire upon the ſpot, the next night they will attempt to riſe through another part; and, if they happen to emerge under a cheſt or trunk early in the night, will pierce the bottom, and deſtroy or ſpoil every thing in it before the morning (43). On theſe accounts we are careful to ſet all our cheſts and boxes upon ſtones or bricks, ſo as to leave the bottoms of ſuch furniture ſome inches above the ground; which not only prevents theſe inſects finding them out ſo readily, but preſerves the bottoms from a corroſive damp which would ſtrike from the earth through, and rot every thing therein: a vaſt deal of vermin alſo would harbour under, ſuch as Cock-roaches, Centipedes, Millepedes, Scorpions, Ants, and various other noiſome inſects.

When the Termites attack trees and branches in the open air, they ſometimes vary their manner of doing it. If a ſtake in a hedge has not taken root and vegetated, it becomes their buſineſs to deſtroy it. If it has a good ſound bark round it, they

" years time deſtroy the whole roof of an houſe, eſpecially if it be of ſoft tim-
" ber. —— They have likewiſe cauſed great loſſes to ſhop-keepers, by boring
" through whole bales of linnen as well as woolen cloths. HUGHES's Barbadoes, p. 93.

(42) The floors are generally made of the ſtone or clay taken from the hills raiſed by theſe inſects, which, being moiſtened with water, and mixed by treading, is beaten level, ſmooth, and compact, with their feet and a kind of hand-bat or beetle.

(43) " One night, in a few hours, they pierced one foot of the table, and
" (having in that manner aſcended) carried their arch acroſs it, and then
" down through the middle of the other foot into the floor, as good luck would
" have it, without doing any damage to the papers left there." KEMPFER Hiſt. Japan, vol. II. p. 127.

will

will enter at the bottom, and eat all but the bark, which will remain, and exhibit the appearance of a ſolid ſtick (which ſome vagrant colony of Ants or other inſects often ſhelter in till the winds diſperſe it); but if they cannot truſt the bark, they cover the whole ſtick with their mortar, and it then looks as if it had been dipped into thick mud that had been dried on. Under this covering they work, leaving no more of the ſtick and bark than is barely ſufficient to ſupport it, and frequently not the ſmalleſt particle, ſo that upon a very ſmall tap with your walking ſtick, the whole ſtake, though apparently as thick as your arm, and five or ſix feet long, loſes its form, and diſappearing like a ſhadow falls in ſmall fragments at your feet. They generally enter the body of a large tree which has fallen through age or been thrown down by violence, on the ſide next the ground, and eat away at their leiſure within the bark, without giving themſelves the trouble either to cover it on the outſide, or to replace the wood which they have removed from within, being ſomehow ſenſible that there is no neceſſity for it. Theſe excavated trees have deceived me two or three times in running: for, attempting to ſtep two or three feet high, I might as well have attempted to ſtep upon a cloud, and have come down with ſuch unexpected violence, that, beſides ſhaking my teeth and bones almoſt to diſlocation, I have been precipitated, head foremoſt, among the neighbouring trees andbuſhes. Sometimes, though ſeldom, the animals are known to attack living trees; but not, I apprehend, before ſymptoms of mortification have appeared at the roots, ſince it is evident, as is before obſerved, that theſe inſects are intended in the order of nature to haſten the diſſolution of ſuch trees and vegetables as have arrived at their greateſt maturity and per-

fection, and which would, by a tedious decay, ſerve only to encumber the face of the earth. This purpoſe they anſwer ſo effectually, that nothing periſhable eſcapes them, and it is almoſt impoſſible to leave any thing penetrable upon the ground a long time in ſafety; for the odds are, that, put it where you will abroad, they will find it out before the following morning, and its deſtruction follows very ſoon of courſe. In conſequence of this diſpoſition, the woods never remain long encumbered with the fallen trunks of trees or their branches; and thus it is, as I have before obſerved, the total deſtruction of deſerted towns is ſo effectually completed, that in two or three years a thick wood fills the ſpace; and, unleſs *iron-wood* poſts have been made uſe of, not the leaſt veſtige of an houſe is to be diſcovered.

The firſt object of admiration which ſtrikes one upon opening their hills is the behaviour of the ſoldiers. If you make a breach in a ſlight part of the building, and do it quickly with a ſtrong hoe or pick-axe, in the ſpace of a few ſeconds a ſoldier will run out, and walk about the breach, as if to ſee whether the enemy is gone, or to examine what is the cauſe of the attack. He will ſometimes go in again, as if to give the alarm: but moſt frequently, in a ſhort time, is followed by two or three others, who run as faſt as they can, ſtraggling after one another, and are ſoon followed by a large body who ruſh out as faſt as the breach will permit them, and ſo they proceed, the number increaſing, as long as any one continues battering their building (44). It is not eaſy to deſcribe the rage and fury

(44) "They throw up little hills of ſeven or eight feet high, ſo very full of "holes that they rather ſeem like honey-combs than burrows. Theſe Ant hills "are of a very ſmall circumference in proportion to their height, being ſharp at top, "ſo that to judge by the looks of them one would think the wind could blow them "down.

fury they ſhew. In their hurry they frequently miſs their hold, and tumble down the ſides of the hill, but recover themſelves as quickly as poſſible; and, being blind, bite every thing they run againſt, and thus make a crackling noiſe, while ſome of them beat repeatedly with their forceps upon the building, and make a ſmall vibrating noiſe, ſomething ſhriller and quicker than the ticking of a watch: I could diſtinguiſh this noiſe at three or four feet diſtance, and it continued for a minute at a time, with ſhort intervals. While the attack proceeds they are in the moſt violent buſtle and agitation. If they get hold of any one, they will in an inſtant let out blood enough to weigh againſt their whole body; and if it is the leg they wound, you will ſee the ſtain upon the ſtocking extend an inch in width. They make their hooked jaws meet at the firſt ſtroke, and never quit their hold, but ſuffer themſelves to be pulled away leg by leg, and piece after piece, without the leaſt attempt to eſcape. On the other hand, keep out of their way, and give them no interruption, and they will in leſs than half an hour retire into the neſt, as if they ſuppoſed the wonderful monſter that damaged their caſtle to be gone beyond their reach. Before they are all got in you will ſee the labourers in motion, and haſtening in various directions toward the breach: every one with a burthen of mortar in his mouth ready tempered. This they ſtick upon the breach as faſt as they come up, and do it with ſo much diſpatch and facility, that although there are thouſands, and I may ſay millions, of them, they never ſtop or

"down. I one day attempted to knock off the top of one of them with my cane, "but the ſtroke had no other effect than to bring ſome thouſands of the animals "out of doors, to ſee what was the matter: upon which I took to my heels and "ran away as faſt as I could." SMITH's Voyage to Guinea.

embarraſs

3

embarrass one another; and you are most agreeably deceived when, after an apparent scene of hurry and confusion, a regular wall arises, gradually filling up the chasm. While they are thus employed, almost all the soldiers are retired quite out of sight, except here and there one, who saunters about among six hundred or a thousand of the labourers, but never touches the mortar either to lift or carry it; one, in particular, places himself close to the wall they are building. This soldier will turn himself leisurely on all sides, and every now and then, at intervals of a minute or two, lift up his head, and with his forceps beat upon the building, and make the vibrating noise before mentioned; on which immediately a loud hiss, which appears to come from all the labourers, issues from within side the dome and all the subterraneous caverns and passages: that it does come from the labourers is very evident, for you will see them all hasten at every such signal, redouble their pace, and work as fast again.

As the most interesting experiments become dull by repetition or continuance, so the uniformity with which this business is carried on, though so very wonderful, at last satiates the mind. A renewal of the attack, however, instantly changes the scene, and gratifies our curiosity still more. At every stroke we hear a loud hiss; and on the first the labourers run into the many pipes and galleries with which the building is perforated, which they do so quickly that they seem to vanish, for in a few seconds all are gone, and the soldiers rush out as numerous and as vindictive as before (45). On finding no enemy they return again leisurely into

(45) By the soldiers being so ready to run out upon the repetition of the attack, it appears, that they but just withdraw out of sight, to leave room for the labourers to proceed without interruption in repairing the breach, and in this instance

into the hill, and very ſoon after the labourers appear loaded as at firſt, as active and as ſedulous, with ſoldiers here and there among them, who act juſt in the ſame manner, one or other of them giving the ſignal to haſten the buſineſs. Thus the pleaſure of ſeeing them come out to fight or to work alternately may be obtained as often as curioſity excites or time permits: and it will certainly be found, that the one order never attempts to fight, or the other to work, let the emergency be ever ſo great.

We meet vaſt obſtacles in examining the interior parts of theſe tumuli. In the firſt place, the works, for inſtance, the apartments which ſurround the royal chamber and the nurſeries, and indeed the whole internal fabric, are moiſt, and conſequently the clay is very brittle: they have alſo ſo cloſe a connexion, that they can only be ſeen as it were by piece-meal; for having a kind of geometrical dependance or abutment againſt each other, the breaking of one arch pulls down two or three. To theſe obſtacles muſt be added the obſtinacy of the ſoldiers, who fight to the very laſt, diſputing every inch of ground ſo well as often to drive away the negroes who are without ſhoes, and make white people bleed plentifully through their ſtockings. Neither can we let a building ſtand ſo as to get a view of the interior parts without interruption, for while the ſoldiers are defending the

inſtance they ſhew more good ſenſe than the bulk of mankind, for, in caſe of a conflagration in a city, the number of people who aſſemble to ſtare is much greater than of thoſe who come to aſſiſt, and the former always interrupt and hinder the latter in their efforts. The ſudden retreat of the labourers, in caſe of an alarm, is alſo a wonderful inſtance of good order and diſcipline, ſeldom ſeen in populous cities, where we frequently find helpleſs people, women, and children, without any ill intention, intermixing in violent tumults and dangerous riots.

2 out-works,

out-works, the labourers keep barricadoing all the way against us, stopping up the different galleries and passages which lead to the various apartments, particularly the royal chamber, all the entrances to which they fill up so artfully as not to let it be distinguishable while it remains moist; and externally it has no other appearance than that of a shapeless lump of clay (46). It is, however, easily found from its situation with respect to the other parts of the building, and by the crouds of labourers and soldiers which surround it, who shew their loyalty and fidelity by dying under its walls. The royal chamber in a large nest is capacious enough to hold many hundreds of the attendants, besides the royal pair, and you always find it as full of them as it can hold. These faithful subjects never abandon their charge even in the last distress; for whenever I took out the royal chamber, and, as I often did, preserved it for some time in a large glass bowl, all the attendants continued running in one direction round the king and queen with the utmost solicitude, some of them stopping on every circuit at the head of the latter, as if to give her something. When they came to the extremity of the abdomen, they took the eggs from her, and carried them away, and piled them carefully together in some part of the chamber, or in the bowl under, or behind any pieces of broken clay which lay most convenient for the purpose.

(46) In tab. VIII. fig. 2. and 4. the entrances of the royal chamber, now exhibited, are represented open. They were all shut by the labourers before I had got to it, and were opened since I arrived in England. Two or three of them, however, are not quite open in the chamber itself, and that next the breach at A, and marked with a cross ⊕, is still left shut, as a specimen of the manner in which they do it. I have also more royal chambers and various specimens of the interior buildings, with several galleries and passages, shut up while we were attacking the nest.

Some of these little unhappy creatures would ramble from the chamber, as if to explore the cause of such a horrid ruin and catastrophe to their immense building, as it must appear to them; and, after fruitless endeavours to get over the side of the bowl, return and mix with the croud that continue running round their common parents to the last (tab. VIII. fig. 4. B.). Others, placing themselves along her side, get hold of the queen's vast matrix with their jaws, and pull with all their strength so as visibly to lift up the part which they fix at; but, as I never saw any effect from these attempts, I never could determine whether this pulling was with an intention to remove her body, or to stimulate her to move herself, or for any other purpose; but, after many ineffectual tugs, they would desist and join in the croud running round, or assist some of those who are cutting off clay from the external parts of the chamber or some of the fragments and moistening it with the juices of their bodies, to begin to work a thin arched shell over the body of the queen, as if to exclude the air, or to hide her from the observation of some enemy. These, if not interrupted, before the next morning, completely cover her, leaving room enough within for great numbers to run about her.

I do not mention the king in this case, because he is very small in proportion to the queen, not being bigger than thirty of the labourers, so that he generally conceals himself under one side of the abdomen, except when he goes up to the queen's head, which he does now and then, but not so frequently as the rest.

If in your attack on the hill you stop short of the royal chamber, and cut down about half of the building, and leave open some thousands of galleries and chambers, they will all be shut up with thin sheets of clay before the next morning. If even the whole is pulled down, and the different buildings are thrown

in a confused heap of ruins, provided the king and queen are not destroyed or taken away, every interstice between the ruins, at which either cold or wet can possibly enter, will be so covered as to exclude both, and, if the animals are left undisturbed, in about a year they will raise the building to near its pristine size and grandeur.

The marching Termites are not less curious in their order, as far as I have had an opportunity of observing them, than those described before. This species seems much scarcer and larger than the *Termes bellicosus*. I could get no information relative to them from the black people, from which I conjecture they are little known to them: my seeing them was very accidental. One day, having made an excursion with my gun up the river Camerankoes, on my return through the thick forest, whilst I was sauntering very silently in hopes of finding some sport, on a sudden I heard a loud hiss, which, on account of the many serpents in those countries, is a most alarming sound. The next step caused a repetition of the noise, which I soon recognized, and was rather surprised seeing no covered ways or hills. The noise, however, led me a few paces from the path, where, to my great astonishment and pleasure, I saw an army of Termites coming out of a hole in the ground, which could not be above four or five inches wide. They came out in vast numbers, moving forward as fast seemingly as it was possible for them to march. In less than a yard from this place they divided into two streams or columns, composed chiefly of the first order, which I call labourers, twelve or fifteen a-breast, and crouded as close after one another as sheep in a drove, going straight forward without deviating to the right or left. Among these, here and there, one of the soldiers was to be seen, trudging along with them, in the same manner, neither stopping

or turning; and as he carried his enormous large head with apparent difficulty, he put me in mind of a very large ox amidst a flock of sheep. While these were bustling along, a great many soldiers were to be seen spread about on both sides of the two lines of march, some a foot or two distant, standing still or sauntering about as if upon the look out least some enemy should suddenly come upon the labourers. But the most extraordinary part of this march was the conduct of some others of the soldiers, who having mounted the plants which grow thinly here and there in the thick shade, had placed themselves upon the points of the leaves, which were elevated ten or fifteen inches above the ground, and hung over the army marching below. Every now and then one or other of them beat with his forceps upon the leaf, and made the same sort of ticking noise which I had so frequently observed to be made by the soldier who acts the part of a surveyor or super-intendant when the labourers are at work repairing a breach made in one of the common hills of the *Termites bellicosi*. This signal among the marching white Ants produced a similar effect; for, whenever it was made, the whole army returned a hiss, and obeyed the signal by increasing their pace with the utmost hurry. The soldiers who had mounted aloft, and gave these signals, sat quite still during the intervals (except making now and then a slight turn of the head) and seemed as solicitous to keep their posts as regular centinels. The two columns of the army joined into one about twelve or fifteen paces from their separation, having in no part been above three yards asunder, and then descended into the earth by two or three holes. They continued marching by me for above an hour that I stood admiring them, and seemed neither to increase or diminish their numbers, the soldiers only excepted, who

quitted

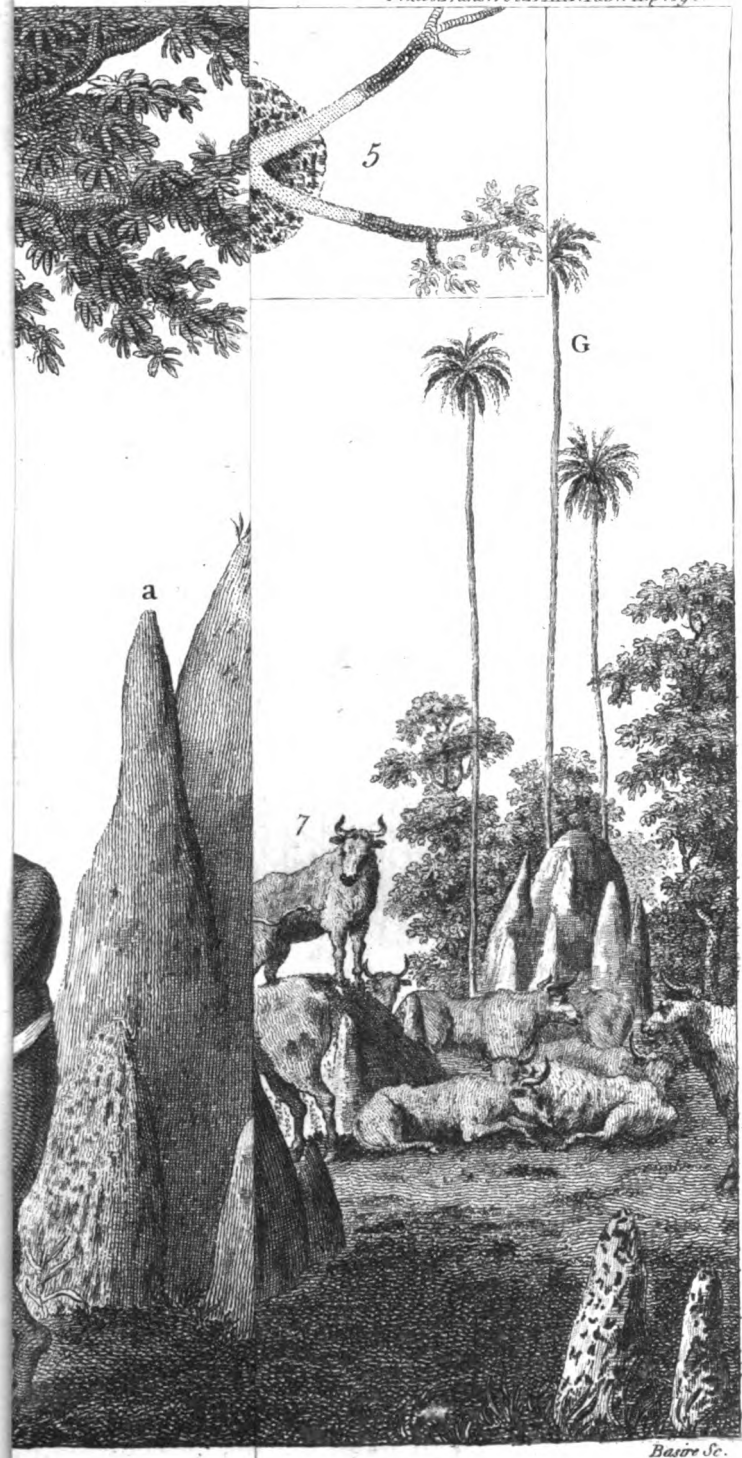
quitted the line of march, and placed themſelves at different diſtances on each ſide of the two columns; for they appeared much more numerous before I quitted the ſpot. Not expecting to ſee any change in their march, and being pinched for time, the tide being nearly up, and our departure fixed at high water, I quitted the ſcene with ſome regret, as the obſervation of a day or two might have afforded me the opportunity of exploring the reaſon and neceſſity of their marching with ſuch expedition, as well as of diſcovering their chief ſettlement, which is probably built in the ſame manner as the large hills before deſcribed. If ſo, it may be larger and more curious, as theſe inſects were at leaſt one-third larger than the other ſpecies, and conſequently their buildings muſt be more wonderful if poſſible: thus much is certain, there muſt be ſome fixed place for their king and queen, and the young ones. Of theſe ſpecies I have not ſeen the perfect inſect.

The œconomy of nature is wonderfully diſplayed in a comparative obſervation on the different ſpecies who are calculated to live under ground until they have wings, and this ſpecies which marches in great bodies in open day. The former, in the two firſt ſtates, that is, of labourers and ſoldiers, have no eyes that I could ever diſcover; but when they arrive at the winged or perfect ſtate in which they are to appear abroad, though only for a few hours, and that chiefly in the night, they are furniſhed with two conſpicuous and fine eyes: ſo the *Termes viarum*, or marching Bugga Bugs, being intended to walk in the open air and light, are even in the firſt ſtate furniſhed with eyes proportionably as fine as thoſe which are given to the winged or perfect inſects of the other ſpecies.

I am

I am afraid of encroaching upon your time, which leads me to drop the ſubject for the preſent; but, as my materials are not exhauſted, if theſe ſheets meet with your approbation, it will encourage me to give ſome further particulars, with obſervations and reflections, at a future period.

I have the honour to be, &c.



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Explanation of the plates to Mr. SMEATHMAN's Account of the Termites of Africa, &c.

TAB. VII. fig. 1. The hill-neſt raiſed by the Termites bellicoſi, deſcribed page 148.

aaa. Turrets by which their hills are raiſed and enlarged, p. 150.

Fig. 2. A ſection of fig. 1. as it would appear on being cut down through the middle from the top a foot lower than the ſurface of the ground, p. 154.

AA. An horizontal line from A on the left, and a perpendicular line from A at the bottom, will interſect each other at the royal chamber, p. 154.

The darker ſhades near it are the empty apartments and paſſages, which it ſeems are left ſo for the attendants on the king and queen, who, when old, may require near one hundred thouſand to wait on them every day.

The parts which are the leaſt ſhaded and dotted are the nurſeries, ſurrounded, like the royal chamber, by empty paſſages on all ſides for the more eaſy acceſs to them with the eggs from the queen, the proviſion for the young, &c. N. B. The magazines of proviſions are ſituated without any ſeeming order among the vacant paſſages which ſurround the nurſeries.

B. The top of the interior building, which often ſeems, from the arches carrying upward, to be adorned on the ſides with pinnacles, p. 156.

C. The floor of the area or nave, p. 156.

DDD. The large galleries which aſcend from under all the buildings ſpirally to the top, p. 156.

EE. The bridges, p. 158.

Fig. 3. The firſt appearance of an hill-neſt by two turrets, p. 150.

Fig. 4. A tree, with the neſt of the Termites arborum, and their covered way, p. 161.

FFFF. Covered ways of the Termites arborum, p. 173.

Fig. 5. A ſection of the neſt of the Termites arborum.

Fig. 6. A neſt of the Termites bellicoſi, with Europeans on it, ſeemingly obſerving a veſſel at ſea, p. 151.

Fig. 7. A bull ſtanding centinel upon one of theſe neſts, while the reſt of the herd is ruminating below, p. 151.

GGG. The African palm-trees, from the nuts of which is made the Oleum Palmæ.

Tab. VIII. fig. 1. A tranſverſe ſection of a royal chamber, p. 151.

aa. The thin ſides in which the entrances are made, p. 152.

Fig. 2. A longitudinal ſection of a royal chamber, p. 151.

b. The entrances, p. 187.

A. The door ſhut up, as left by the labourers, p. 187.

Fig. 3. A royal chamber fore-ſhortened.

Fig. 4. The ſame royal chamber repreſented as juſt opened, and diſcovering (B) the queen, and her attendants running round her, p. 188.

bb. A line drawn from *b* to *b* will run along the range of doors or entrances, p. 187.

AAA. A line run from A to AA will croſs the door, which remains cloſed as it was found. The reſt are repreſented as they appear ſince the mortar, with

with which they were stopped up, has been in part or wholly picked out with a small instrument, p. 187.

Fig. 5. A nursery, p. 153.

Fig. 6. A little nursery, with the eggs, the young ones, the mushrooms, mouldiness, &c. as just taken from the hill. p. 153.

Fig. 7. The mushrooms magnified by a strong lens, p. 154.

Tab. IX. fig. 1. and 2. The turret nests, with roofs of the Termes mordax and a Termes atrox as finished, p. 159.

Fig. 3. A turret, with the roof begun.

Fig. 4. A turret, raised only about half its height.

Fig. 5. A turret, building upon one which had been thrown down, p. 160.

Fig. 6. 6. A turret, broken in two.

Tab. X. fig. 1. A Termes bellicosus, p. 141. numb. 1. and p. 165.

Fig. 2. A KING. N. B. A king never alters his form after he loses his wings, neither does he apparently increase in bulk.

Fig. 3. A QUEEN, p. 170.

Fig. 4. The head of a perfect insect magnified.

Fig. 5. A face, with stemmata magnified, p. 141. numb. 1.

Fig. 6. A labourer, p. 163.

Fig. 7. A labourer magnified.

Fig. 8. A soldier, p. 164.

Fig. 9. A soldier's forceps and part of his head magnified, p. 164.

Fig. 10. The Termes mordax, p. 141. numb. 2. and p. 161.

Fig. 11. The face with the stemmata magnified, p. 141. numb. 2.

Fig. 12. A labourer.

Fig. 13. A soldier.

Fig. 14. The Termes atrox, p. 141. numb. 3. and p. 160.

Fig. 15. The face and stemmata magnified, p. 141. numb. 3.

Fig. 16. A labourer.

Fig. 17. A soldier.

Fig. 18. Idem.

Fig. 19. The Termes destructor, p. 141. numb. 4.

Fig. 20. The face and stemmata magnified, p. 141. numb. 4.

Fig. 21. The Termes arborum, p. 141. numb. 5. and p. 162.

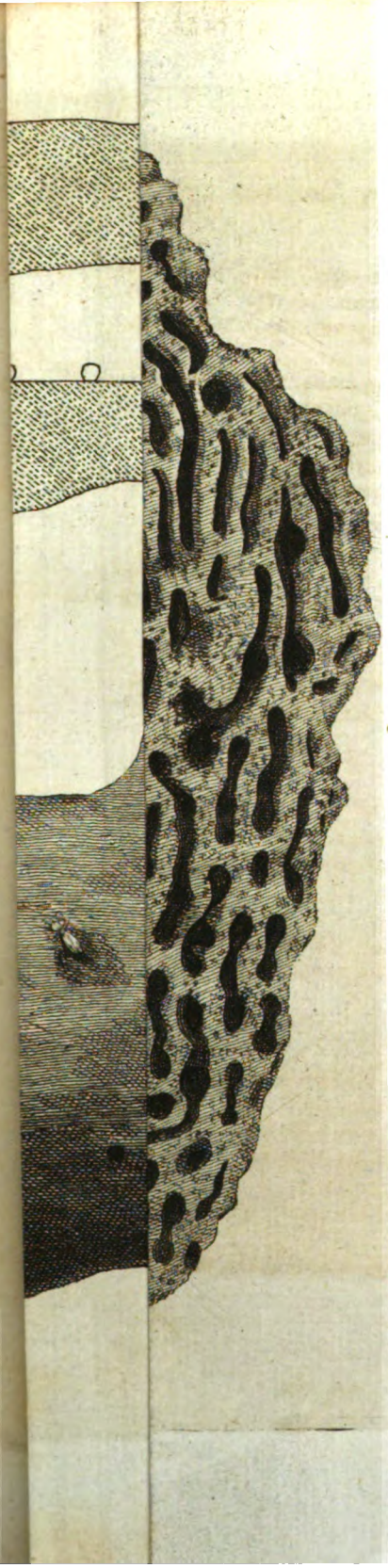
Fig. 22. The face and stemmata magnified, p. 141. numb. 5.

Fig. 23. A labourer.

Fig. 24. A soldier.

Fig. 25. A QUEEN, p. 172.

N. B. In the figures 5. 11. 15. 20. and 21. the two white spots between the edges are the stemmata.



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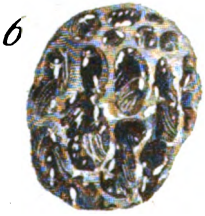

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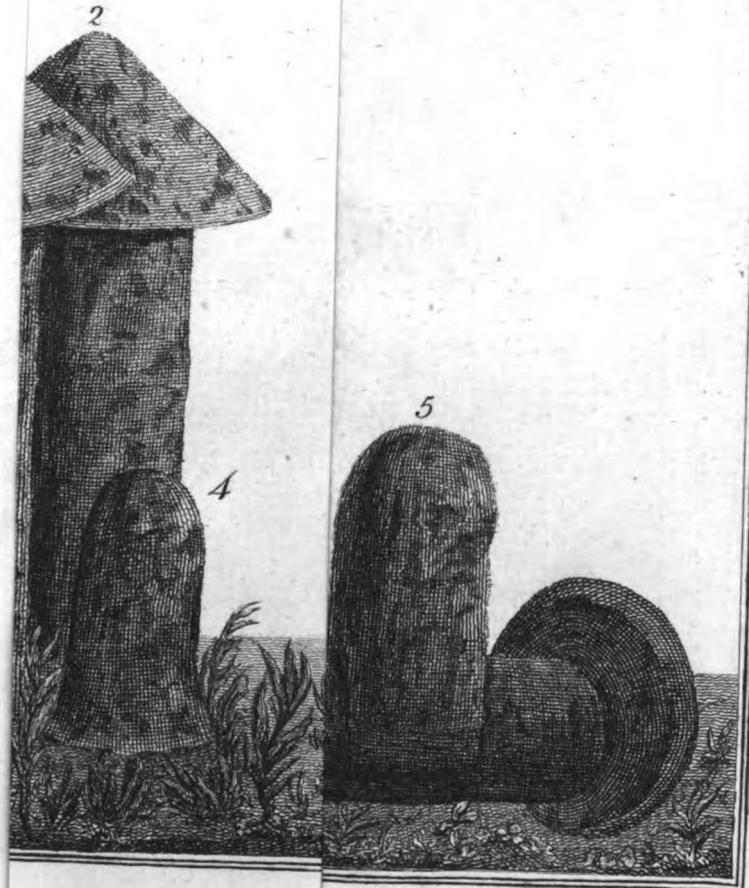

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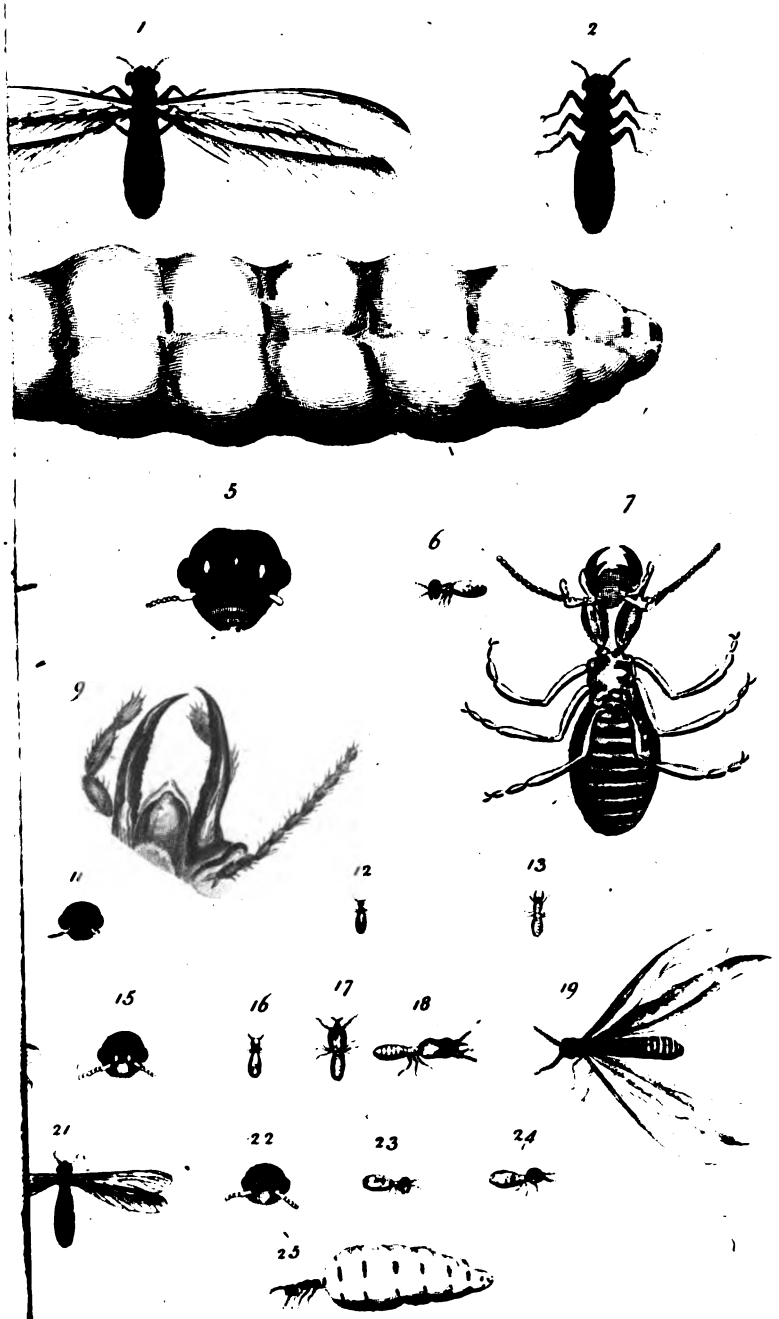

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XII. *An Account of ſeveral Earthquakes felt in* Wales. *By* Thomas Pennant, *Eſq. F. R. S. in a Letter to Sir* Joſeph Banks, *P. R. S.*

Read January 25, 1781.

DEAR SIR,

Downing,
Dec. 12, 1781.

IT is very ſingular, that in three days after my return home I ſhould be reminded of my promiſe by a repetition of the very phenomenon on which I had engaged to write to you: for on Saturday laſt, between four and five in the evening, we were alarmed with two ſhocks of an earthquake; a ſlight one, immediately followed by another very violent. It ſeemed to come from the north-eaſt, and was preceded by the uſual noiſe; at preſent I cannot trace it farther than Holywell.

The earthquake preceding this was on the 29th of Auguſt laſt, about a quarter before nine in the morning. I was fore-warned of it by a rumbling noiſe not unlike the coming of a great waggon into my court-yard. Two ſhocks immediately followed, which were ſtrong enough to terrify us. They came from the north-weſt; were felt in Angleſea, at Caernarvon, Llanrwſt, in the iſle of Clwyd ſouth of Denbigh, at this houſe, and in Holywell; but I could not diſcover that their force extended any farther.

The next in this retrograde way of enumerating theſe phenomena was on the 8th of September 1775, about a quarter before

before ten at night, the noiſe was ſuch as preceded the former; and the ſhock ſo violent as to ſhake the bottles and glaſſes on the table round which myſelf and ſome company were ſitting. This ſeemed to come from the eaſt. I ſee in the Gentleman's Magazine of that year, that this ſhock extended to Shropſhire, and quite to Bath, and to Swanſea in South Wales.

The earlieſt earthquake I remember here was on the 10th of April 1750. It has the honour of being recorded in the Philoſophical Tranſactions, therefore I ſhall not trouble you with the repetition of what I have ſaid.

Permit me to obſerve, that I live near a mineral country, in a ſituation between lead mines and coal mines; in a ſort of neutral tract, about a mile diſtant from the firſt, and half a mile from the laſt. On the ſtricteſt inquiry I cannot diſcover that the miners or colliers were ever ſenſible of the ſhocks under ground: nor have they ever perceived, when the ſhocks in queſtion have happened, any falls of the looſe and ſhattery ſtrata, in which the laſt eſpecially work; yet, at the ſame time, the earthquakes have had violence ſufficient to terrify the inhabitants of the ſurface. Neither were theſe local; for, excepting the firſt, all may be traced to very remote parts. The weather was remarkably ſtill at the time of every earthquake I have felt.

I remain, with true regard, &c.

XIII. *Extract of a Letter from the Right Honourable* Philip *Earl* Stanhope, *F. R. S. to Mr.* James Clow, *Professor of Philosophy in the University of* Glasgow. *Dated* Chevening, February 16, 1777.

Read June 10, 1780.

I HAVE lately made some curious observations concerning the roots of adfected equations, part of which have occurred to Messieurs DANIEL BERNOULLI, EULER, DE LA GRANGE, LAMBERT, and others; but some of them, I believe, are quite new. I will give you one instance of a quadratic equation, as the simplest.

Let the quadratic equation $11xx - 15x + 5 = 0$, be proposed. I say then, that if two recurring series be formed from the fractions $\frac{1+2z}{1-z-zz}$, $\frac{2+3z}{1-z-zz}$, which have a common denominator, and each series of co-efficients, continued both ways (that is, as well before, as after the first term) the fractions formed by dividing each term of the first series by the corresponding term of the second series, *viz.*

$$\&c. \frac{-11}{-14}, \frac{+7}{+9}, \frac{-4}{-5}, \frac{+3}{+4}, \frac{-1}{-1}, \frac{-3}{-4}, \frac{2}{3}, \frac{1}{2} \Big| \frac{3}{5}, \frac{4}{7}, \frac{7}{12}, \frac{11}{19}, \frac{18}{31}, \frac{29}{50} \ \&c.$$

will converge in the simplest manner possible; those before the bar, in a retrograde order to the greater root $\frac{15+\sqrt{5}}{22}$; and those after the bar, in a direct order to the smallest root $\frac{15-\sqrt{5}}{22}$; where it is to

 be

be obſerved, that the greater root is affirmative, notwithſtanding the ſign − being prefixed to ſome of the terms, becauſe in each fraction the numerator and the denominator are affected by the ſame ſign, whether + or −.

The chief improvement I have made conſiſts in approximating to two roots at once, by one and the ſame ſeries, continued backwards as well as forwards. I have not time to enlarge upon this ſubject at preſent; but the little I have ſaid will be a ſpecimen of the method to be uſed in higher equations.

XIV. *Extract of Two Meteorological Journals of the Weather observed at* Nain *in* 57° *North Latitude, and at* Okak *in* 57° 20′ *North Latitude, both on the Coast of* Labradore. *Communicated by Mr.* De la Trobe.

Read March 8, 1781.

THERMOMETER AT OKAK.

1779	Highest.	Lowest.	Mean.
August	78,0	37,0	52,0
September	74,0	32,0	45,0
October	45,0	11,0	32,3
November	36,0	15,0	27,3
December	31,0	12,0	12,7
1780			
January	35,0	13,0	15,3
February	33,0	17,0	13,1
March	45,0	14,0	10,0
April	57,0	9,0	29,5
May	55,0	23,0	39,0
June	68,0	33,0	43,6
July	84,0	38,0	52,0

Ther-

	Thermometer at Nain.			Barometer at Nain.		
1778	Highest	Lowest	Mean.	Highest.	Lowest.	Mean.
September	68,0	35,0	45,3	28,4½	27,10	28,1,3
October	60,0	15,0	35,1	28,8	27,4	28,2,0
November	37,0	5,0	25,4	28,8	27,3	28,0,2
December	28,0	30,0	1,9	28,7	27,2	28,2,4
1779						
January	13,0	29,0	14,3	28,8	26,11	28,1,4
February	13,0	36,0	13,2	28,5½	27,5	27,9,9
March	29,0	20,0	5,0	28,7	27,5	28,2,8
April	51,0	5,0	27,7	28,6½	27,3½	28,1,9
May	63,0	22,0	34,9	28,7½	27,9½	28,2,0
June	71,0	32,0	42,2	28,3	27,8	28,2,5
July	74,0	36,0	48,2	28,5	27,7	28,1,9
August	78,0	38,0	52,0	28,5	27,8	28,1,5
September	74,0	35,0	45,4	28,6	27,5	28,1,4
October	47,0	14,0	33,6	28,6	27,4½	28,2,5
November	36,0	9,0	27,3	28,5	27,2	28,6,1
December	32,0	13,0	11,1	28,3½	27,1½	28,3,0
1780						
January	35,0	16,0	13,4	28,2	26,6	27,6,8
February	32,0	23,0	9,7	28,5½	26,10	27,9,5
March	39,0	18,0	10,0	28,8	27,3	28,1,9
April	45,0	4,0	28,2	28,8½	27,7	28,3,7
May	53,0	23,0	37,6	28,6½	27,9	28,3,1
June	61,0	32,0	43,1	28,3	26,11	28,0,5
July	84,0	39,0	52,2	28,1½	27,5	28,1,3
August	70,0	37,0	50,4	28,3½	27,5	28,1,0

METEOROLOGICAL JOURNAL

KEPT AT THE HOUSE OF

THE ROYAL SOCIETY

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January 1780.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 1	8	0	27,0	33,0	30,24		NE	1	Foggy and frosty.
	2	0	31,0	34,0	30,17		SE	1	Foggy.
2	8	0	32,0	33,0	30,35		SE	1	Foggy.
	2	0	36,0	35,0	30,16		SE	1	Foggy.
3	8	0	41,0	37,0	30 09		SE	1	Foggy.
	2	0	43,5	38,5	30,08		SE	1	Foggy and cloudy.
4	8	0	35,0	38,0	30,26		N by W	1	Fair.
	2	0	39,0	39,5	30,34		NE	1	Fine.
5	8	0	31,0	36,0	30,30		WNW	1	Fair.
	2	0	36,0	37,5	30,28		NW	1	Fine.
6	8	0	33,5	35,0	30,00		W	1	Cloudy.
	2	0	37,5	36,5	29,98		SW	1	Foggy and rain.
7	8	0	36,0	36,5	30,14		ENE	1	Cloudy.
	2	0	36,5	38,5	30,20		NE	1	Fine.
8	8	0	26,0	33,0	30,15		NE	1	Fair and frosty.
	2	0	30,5	35,5	30,04		NE	1	Frosty.
9	8	0	22,5	30,0	29,95		NE	1	Frosty.
	2	0	29,5	31,0	29,88		E	1	Fine.
10	8	0	29,0	30,0	29,67		N by E	1	Cloudy.
	2	0	33,0	31,5	29,59		NE	1	Cloudy and snow.
11	8	0	25,0	29,5	29,58		SW	1	Cloudy.
	2	0	32,0	31,5	29,65		NNW	1	Foggy and snow.
12	8	0	24,5	29,5	29,61		NW	1	Fair and frosty.
	2	0	29,0	30,0	29,67		NW	1	Fine.
13	8	0	20,0	27,0	29,94		N by E	1	Fair and frosty.
	2	0	27,0	28,0	29,89		NE	1	Fair and frosty.
14	8	0	20,0	25,0	29,81		NE	1	Fair and frosty.
	2	0	30,0	28,0	29,89		NE	1	Fine.
15	8	0	21,5	24,5	29,45		NE	1	Fair and frosty.
	2	0	33,0	27,5	29,27		NE	1	Foggy.
16	8	0	40,0	32,0	28,70	0,185	SW	2	Fair.
	2	0	47,0	36 5	28,59		SW	2	Rainy.

METEOROLOGICAL JOURNAL
for January 1780.

	Time. H.	Time. M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Jan. 17	8	0	40,0	37,0	28,73	0,035	SW	1	Cloudy.
	2	0	46,0	40,5	28,74		W by N	1	Fine.
18	8	0	36,5	38,0	28,92	0,042	NE	1	Cloudy.
	2	0	41,0	39,5	28,97		SW	1	Cloudy.
19	8	0	37,0	39,0	29,14	0,214	NE	1	Cloudy.
	2	0	40,0	37,5	29,28		NE	1	Cloudy.
20	8	0	34,0	38,0	29,37	0,150	N by E	1	Rain.
	2	0	35,5	39,0	29,43		NE	2	Rainy.
21	8	0	33,0	37,0	29,77	0,066	N by E	2	Fair.
	2	0	36,5	38,5	29,86		N by E	2	Fine.
22	8	0	25,0	33,0	30,00		N by W	1	Fair and frosty.
	2	0	32,0	35,0	30,00		N by E	1	Fine.
23	8	0	24,0	31,0	30,00		NE	1	Fair and frosty.
	2	0	28,0	32,5	30,05		NE	1	Foggy.
24	8	0	26,5	31,0	30,01		NW	1	Fair and frosty.
	2	0	34,0	32,5	29,99		NE	1	Fine.
25	8	0	29,0	30,0	30,12		N by E	1	Fair and frosty.
	2	0	35,5	33,5	30,06		N by E	1	Snow.
26	8	0	31,0	31,0	29,95		N by E	2	Cloudy.
	2	0	34,5	32,5	29,95		N by E	1	Cloudy and snow.
27	8	0	28,0	32,0	29,90		N	1	Cloudy with snow.
	2	0	31,0	32,5	29,92		N	1	Cloudy and snow.
28	8	0	30,0	32,5	29,92		E by N	1	Cloudy.
	2	0	32,5	34,0	29,90		E by N	1	Cloudy.
29	8	0	31,5	32,0	29,80		ENE	1	Cloudy.
	2	0	35,0	34,5	29,74		ENE	1	Cloudy.
30	8	0	29,0	31,0	29,66		NE	2	Cloudy.
	2	0	30,0	32,0	29,66		NE	2	Cloudy.
31	8	0	30,5	31,0	29,74		E by S	2	Cloudy.
	2	0	34,0	33,0	29,71		ENE	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 1	8	0	30,5	32,0	29,65		NE	2	Cloudy.
	2	0	32,0	33,0	29,62		NE	1	Cloudy.
2	8	0	28,5	31,0	29,48		N by E	1	Fair.
	2	0	33,5	34,5	29,47		NE	1	Fair.
3	8	0	31,5	32,5	29,57		NW	1	Fair.
	2	0	36,5	34,5	29,69		NW	1	Cloudy.
4	8	0	29,0	33,0	29,74		SW	1	Fair.
	2	0	38,5	35,5	29,68		S by W	1	Fine.
5	8	0	30,5	33,0	29,43		NE	1	Fine.
	2	0	35,0	35,0	29,41		SE	1	Fine.
6	8	0	33,0	34,0	29,08	0,210	NE	1	Rainy.
	2	0	36,5	35,0	29,11		WSW	1	Rainy and foggy.
7	8	0	34,0	36,0	29,48	0,034	NW	1	Fair.
	2	0	38,5	38,0	29,68		NW	1	Fine.
8	8	0	32,5	36,0	29,89	0,022	SE	1	Fair.
	2	0	41,0	48,0	29,92		SE	1	Fine.
9	8	0	43,0	39,0	30,00	0,270	S by W	1	Cloudy.
	2	0	50,5	42,0	30,08		S by E	1	Cloudy.
10	8	0	45,5	44,5	30,22	0,015	SW	1	Cloudy.
	2	0	51,5	46,5	30,24		SW	1	Cloudy.
11	8	0	40,0	45,5	30,41		NE	1	Cloudy.
	2	0	44,0	45,5	30,45		ENE	1	Fine.
12	8	0	36,5	42,0	30,47		NE	1	Cloudy.
	2	0	41,0	43,0	30,47		ENE	1	Cloudy.
13	8	0	37,0	40,5	30,48		NE	1	Fair.
	2	0	41,5	42,0	30,52		NE	1	Fair.
14	8	0	34,0	39,5	30,50		N by W	1	Cloudy.
	2	0	43,0	41,5	30,46		N by W	1	Cloudy.
15	8	0	35,5	39,5	30,40		N by W	1	Fair.
	2	0	40,0	40,0	30,39		N by W	1	Cloudy.
16	8	0	37,0	39,0	30,08		NW	1	Fair.
	2	0	45,0	42,0	30,00		NW	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 17	8	0	32,0	38,0	30,19		S by E	1	Foggy.
	2	0	38,5	39,0	30,25		SE	1	Foggy and cloudy.
18	8	0	35,0	38,5	30,06		SW	1	Cloudy.
	2	0	41,0	41,0	29,92		SW	1	Cloudy.
19	8	0	36,0	40,5	30,41	0,030	NE	1	Fine.
	2	0	43,0	42,0	29,49		W	1	Fair.
20	8	0	27,5	37,0	29,56		NW	1	Fine.
	2	0	34,0	37,0	29,57		NNW	1	Snow.
21	8	0	27,5	33,0	29,72		NW	2	Fair.
	2	0	36,0	35,5	29,78		NW	2	Fine.
22	8	0	28,0	32,5	29,86		NW	2	Fine.
	2	0	30,0	37,5	29,94		NW	1	Fine.
23	8	0	28,0	32,5	30,18		NNW	1	Fine.
	2	0	38,0	35,0	30,24		NW	1	Fine.
24	8	0	34,0	34,0	30,23		SSW	1	Cloudy.
	2	0	41,0	36,0	30,27		SW	1	Rainy.
25	8	0	41,0	38,5	30,06	0,015	SW	2	Cloudy.
	2	0	47,0	49,0	29,88		SW	2	Cloudy.
26	8	0	26,5	35,0	29,83	0,128	NW	3	Fine.
	2	0	32,5	35,5	30,08		NE	3	Fine.
27	8	0	34,0	34,5	29,99		SW	2	Snowy.
	2	0	42,0	36,5	29,82		SSW	1	Cloudy.
28	8	0	42,0	40,0	29,92	0,034	WSW	1	Cloudy.
	2	0	53,5	44,5	29,95		WSW	2	Fine.
29	8	0	46,0	46,0	29,91		W by S	1	Fair.
	2	0	50,0	41,0	30,03		SW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Mar. 1	7	0	34,5	43,0	30,10		W by S	1	Fair.
	2	0	47,0	46,0	30,09		SW	1	Fine.
2	7	0	45,0	45,5	29,78		SW	1	Cloudy.
	2	0	50,0	41,0	29,59		SE	1	Cloudy.
3	7	0	39,0	45,0	29,74		WNW	1	Cloudy.
	2	0	48,5	48,5	29,65		NW	1	Fine.
4	7	0	37,5	43,0	29,91		SW	1	Fair.
	2	0	50,5	46,0	30,05		W by N	1	Fine.
5	7	0	47,0	45,0	30,17	0,071	WSW	2	Fair.
	2	0	55,0	48,5	30,25		WSW	1	Cloudy.
6	7	0	45,0	47,5	30,37		SW	1	Fine.
	2	0	56,0	52,0	30,42		SW	2	Fine.
7	7	0	47,5	51,0	30,36		SW	1	Fair.
	2	0	57,0	53,0	30,39		SW	2	Fine.
8	7	0	48,0	51,0	30,37		SW	1	Cloudy.
	2	0	54,0	55,5	30,35		SW	1	Fair.
9	7	0	44,5	50,0	30,13		SSW	1	Cloudy.
	2	0	50,0	51,5	30,01		SW	1	Fine.
10	7	0	37,0	46,0	30,34		W by S	1	Fair.
	2	0	50,0	48,5	30,45		SW	1	Fine.
11	7	0	35,0	43,0	30,39		SE	1	Fair.
	2	0	50,0	47,0	30,25		SW	1	Fine.
12	7	0	44,0	45,0	29,78		S by W	1	Cloudy.
	2	0	52,0	48,5	29,79		SSW	1	Rainy.
13	7	0	35,0	44,0	30,16	0,072	SW	1	Fine.
	2	0	51,0	47,5	30,15		SW	1	Fine.
14	7	0	44,0	47,0	29,93		SSW	1	Fine.
	2	0	54,5	48,5	29,82		SW	2	Fine.
15	7	0	45,0	49,0	29,52	0,046	W by N	2	Rainy.
	2	0	48,5	50,0	29,58		WNW	2	Fine.
16	7	0	42,0	46,0	29,38	0,227	NW	1	Fine.
	2	0	51,0	50,0	29,67		NW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Mar. 17	7	0	38,0	36,0	29,99		NW	1	Fine.
	2	0	49,5	56,0	29,42		NW	1	Fine.
18	7	0	50,0	51,0	29,67		NW	2	Cloudy.
	2	0	48,0	51,0	29,43		NW	2	Rainy.
19	7	0	40,0	58,0	29,35	0,360	NW	1	Fine.
	2	0	54,0	50,5	29,47		NW	1	Fine.
20	7	0	47,0	49,5	29,46	0,160	NE	1	Cloudy and rain.
	2	0	59,0	54,0	29,95		NW	1	Cloudy.
21	7	0	50,0	51,0	29,82		NW	1	Cloudy.
	2	0	58,0	54,0	29,78		NW	1	Cloudy.
22	7	0	44,0	50,0	29,83		NW	1	Cloudy.
	2	0	52,0	51,5	29,92		SW	2	Fine.
23	7	0	37,0	45,0	30,28		NW	1	Fine.
	2	0	56,0	46,0	30,27		SW	1	Fine.
24	7	0	35,5	43,5	30,11		E by S	1	Fair.
	2	0	51,5	46,5	30,00		E by N	1	Fine.
25	7	0	43,0	45,5	29,82	0,112	SSE	1	Fine.
	2	0	56,0	49,0	29,97		SE	1	Fine.
26	7	0	46,0	47,0	29,94		S by E	1	Fair.
	2	0	51,5	56,5	29,94		SSE	1	Fair.
27	7	0	52,5	51,5	29,90		SSE	1	Fair.
	2	0	62,5	55,0	29,85		SW	1	Fine.
28	7	0	51,0	54,0	29,62		E by N	1	Fair.
	2	0	56,0	56,5	29,61		SW	1	Rainy.
29	7	0	47,0	51,5	29,84	0,077	S by W	2	Fair.
	2	0	55,0	54,5	29,82		S by W	2	Fair.
30	7	0	51,5	53,5	29,71		SSW	1	Fair.
	2	0	55,0	55,5	29,69		SW	1	Showery with sun-shine.
31	7	0	45,0	52,0	29,63	0,054	SSW	1	Fine.
	2	0	50,0	53,5	29,63		SW	1	Rainy.

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	Time.		Therm. without	Therm. within.	Barom	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 1	7	0	40,0	48,5	29,88	0,324	N by E	1	Fair.
	2	0	49,0	50,0	29,87		SSE	1	Fine.
2	7	0	44,0	46,0	29,51	0,014	SE	1	Cloudy.
	2	0	49,0	49,5	29,21		SSE	1	Rainy.
3	7	0	41,5	46,5	28,81	0,333	WSW	1	Cloudy.
	2	0	44,0	47,0	28,88		NNW	1	Rainy.
4	7	0	34,0	42,0	29,36	0,010	NNW	1	Fine.
	2	0	44,5	43,5	29,45		NW	2	Fine.
5	7	0	35,5	40,5	29,48	0,016	SW	1	Fine.
	2	0	35,5	41,0	29,50		NNW	1	Snow.
6	7	0	34,0	39,0	29,68	0,177	N by W	1	Fine.
	2	0	45,0	43,0	29,83		NW	1	Fine.
7	7	0	33,0	38,0	29,81	0,010	NE	1	Fair.
	2	0	46,5	42,0	29,72		SE	1	Fine.
8	7	0	34,0	39,0	29,71		NNE	1	Fine.
	2	0	46,5	42,5	29,80		N by W	1	Fair.
9	7	0	40,0	39,0	30,08	0,020	NNE	2	Fine.
	2	0	47,5	41,5	30,12		ENE	1	Fine.
10	7	0	33,5	38,0	30,17		SE	1	Fair.
	2	0	47,0	41,5	30,10		E by N	1	Fair.
11	7	0	41,0	41,0	29,77	0,050	S by E	1	Rainy.
	2	0	46,0	49,0	29,79		SE	1	Rainy.
12	7	0	42,0	43,5	29,50	0,156	NW	1	Fine.
	2	0	51,0	47,0	29,46		W by N	1	Cloudy.
13	7	0	38,0	43,0	29,57	0,250	NNE	2	Fine.
	2	0	37,0	44,5	29,69		NNE	2	Rain.
14	7	0	41,0	42,0	29,72	0,038	NW	2	Fair.
	2	0	46,5	44,5	29,75		NW	2	Fair.
15	7	0	43,5	43,5	29,75		WSW	1	Cloudy.
	2	0	51,5	46,0	29,74		W by N	1	Cloudy.
16	7	0	47,0	45,0	29,62	0,035	NW	2	Fine.
	2	0	55,0	49,0	29,67		WNW	2	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.		Points.	Str.	
Apr. 17	7	0	43,0	46,5	29,38	0,080	WNW	3	Fine.
	2	0	49,0	50,0	29,37		WNW	3	Fine.
18	7	0	41,5	46,5	29,50	0,056	SW	2	Cloudy.
	2	0	48 5	47,5	29,46		WSW	2	Showery.
19	7	0	43,0	44,0	29,72	0,050	N by E	2	Fine.
	2	0	53,0	48,0	29,76		N by E	2	Fair.
20	7	0	42,0	44,5	29,94	0,030	NW	1	Fine.
	2	0	52,0	48,0	30,01		NW	1	Fair.
21	7	0	47,5	48 0	29,91		SSE	1	Fair.
	2	0	52,0	50,0	29,83		SSW	2	Showery.
22	7	0	47,0	50,0	29,70	0,109	SW	1	Cloudy.
	2	0	58,0	53 5	29,71		SW	1	Fine.
23	7	0	47,5	50,0	29.52	0,031	S by W	1	Fine.
	2	0	56,5	54,0	29,47		SSW	2	Showery.
24	7	0	46,5	51,0	29,46	0,047	WSW	2	Fine.
	2	0	54,0	53,5	29,43		SSE	2	Showery.
25	7	0	45,0	50,5	29,51	0,053	S by E	1	Fine.
	2	0	56,5	54,0	29,46		SSW	1	Fine.
26	7	0	45,0	49,0	29,46	0,391	NW	2	Fine.
	2	0	58,0	53,5	29,74		NW	2	Fine.
27	7	0	50,0	50,5	29,50	0,210	SSW	1	Rainy.
	2	0	56,0	54,0	29,44		SSW	1	Rainy.
28	7	0	54,5	55,0	29,83	0,183	SW	1	Cloudy.
	2	0	60,5	59,5	29,85		SW	1	Rainy.
29	7	0	53,0	56,0	29,66	0,066	SW	1	Cloudy.
	2	0	65,5	60,5	29,66		SW	1	Fine.
30	7	0	55,0	58,0	29,71		SSW	1	Cloudy.
	2	0	61,0	65,0	29,73		S by W	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 1	7	0	53,0	58,0	29,76	0,067	SSW	1	Cloudy.
	2	0	65,0	62,0	29,79		SW	1	Fine.
2	7	0	52,0	56,0	29,85	0,243	N by W	1	Cloudy.
	2	0	59,5	59,0	30,04		NW	1	Fine.
3	7	0	45 5	51,0	30,26		E by S	1	Cloudy.
	2	0	57,0	55,0	30,26		SE	1	Fine.
4	7	0	48,0	52,5	30,18		WSW	1	Cloudy.
	2	0	64,0	56,5	30,16		SW	1	Fine.
5	7	0	53,0	55,0	29,92		SW	1	Cloudy.
	2	0	59,5	58,0	29,88		SW	1	Cloudy.
6	7	0	52,5	55,0	29,88		WSW	2	Fine.
	2	0	60,0	58,5	29,90		W by N	2	Cloudy.
7	7	0	53,0	55,0	29,94		W by S	1	Cloudy.
	2	0	63,5	59,5	29,94		SW	1	Fair.
8	7	0	52,0	57,0	29,72	0,032	SW	1	Rainy.
	2	0	55,0	57,5	29,58		S by W	2	Rainy.
9	7	0	54,0	55,0	29,67	0,091	SW	1	Fine.
	2	0	57,0	58,5	29,64		SW	2	Rainy.
10	7	0	53,0	54,5	29,38	0,200	SW	2	Fine.
	2	0	59,0	58,5	29,51		SW	2	Cloudy.
11	7	0	53,0	57,5	29,72		SW	2	Fine.
	2	0	58,0	59,0	29,88		SW	2	Rainy.
12	7	0	50,0	57,0	29,98		SSW	1	Fine.
	2	0	64,0	60,0	29,98		SW	1	Fine.
13	7	0	53,0	55,0	29,75	0,104	SW	1	Fair.
	2	0	63,0	60,5	29,73		SW	2	Fine.
14	7	0	51,5	57,0	29,78		W by S	1	Fine.
	2	0	57,0	58,5	29,71		SSW	1	Fine.
15	7	0	50,0	55,0	29,82	0,015	NW	2	Fine.
	2	0	33,0	68 5	29,94		NW	1	Fine.
16	7	0	54,5	56,5	30,12		NW	1	Fine.
	2	0	66,0	61,0	30,18		SW	1	Fine.

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	Time. H.	Time. M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points	Winds. Str.	Weather.
May 17	7	0	58,5	61,0	30,14		SW	1	Fair.
	2	0	68,0	63,5	30,15		SW	1	Fine.
18	7	0	54,5	60,0	30,15		NW	1	Fine.
	2	0	67,0	62,5	30,17		NW	1	Fine.
19	7	0	57,5	61,5	29,92		SW	1	Cloudy.
	2	0	66,0	62,0	29,90		SW	1	Fair.
20	7	0	46,5	61,0	30,25		NW	1	Fair.
	2	0	61,0	58,5	30,26		NW	1	Fine.
21	7	0	57,0	53,5	29,49		SE	1	Fair.
	2	0	67,0	61,0	30,01		SW	1	Fine.
22	7	0	56,0	58,0	29,61	0,027	SSW	1	Rainy.
	2	0	65,0	61,0	29,60		SW	1	Fair.
23	7	0	55,5	58,5	29,67		SW	2	Cloudy.
	2	0	63,0	65,0	29,72		SW	2	Fine.
24	7	0	53,0	58,5	29,73	0,011	NW	1	Fair.
	2	0	63,5	60,5	30,23		SE	1	Fair.
25	7	0	57,0	59,5	30,21		SW	1	Cloudy.
	2	0	66,5	61,5	30,12		SW	1	Fine.
26	7	0	58,0	61,5	30,14	0,018	SW	1	Rainy.
	2	0	70,0	65,0	30,14		SW	1	Fine.
27	7	0	60,5	64,0	30,28		SW	1	Fine.
	2	0	73,5	70,0	30,22		SW	1	Fine.
28	7	0	68,0	66,5	30,17		SSW	1	Fine.
	2	0	78,5	72,5	30,18		SW	1	Fine.
29	7	0	68,0	71,0	30,06		SE	1	Fine.
	2	0	84,5	74,5	30,00		SSW	1	Fine.
30	7	0	65,0	67,0	29,98		SW	1	Fair.
	2	0	71,5	71,0	30,01		NW	2	Fine.
31	7	0	56,0	59,0	30,06	0,024	NE	1	Fair.
	2	0	66 0	66,0	30,14		NE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 1	7	0	60,0	61,0	30,05		E by N	1	Fine.
	2	0	70,5	65,0	29,98		ENE	2	Fine.
2	7	0	66,5	67,0	29,93		SW	1	Fine.
	2	0	85,0	73,5	29,93		SW	1	Fine.
3	7	0	65,0	68,0	29,88		NE	1	Cloudy.
	2	0	71,0	72,0	29,95		NE	1	Fine.
4	7	0	59,0	66,0	30,07		NE	1	Fine.
	2	0	64,0	66,0	30,07		NE	1	Fair.
5	7	0	48,5	57,0	29,99	0,023	NE	1	Cloudy.
	2	0	55,5	59,0	29,99		NW	2	Cloudy.
6	7	0	49,0	55,0	29,99	0,010	NE	2	Cloudy.
	2	0	61,0	58,0	29,99		NE	2	Cloudy.
7	7	0	52,0	53,0	30,01		NE	1	Fair.
	2	0	60,0	58,0	30,02		NE	1	Fair.
8	7	0	48,0	50,0	29,99		NW	1	Fine.
	2	0	59,0	57,0	30,09		NW	1	Fine.
9	7	0	60,0	56,5	29,94		NW	1	Fine.
	2	0	62,5	60,0	29,94		NW	1	Cloudy.
10	7	0	61,0	59,0	29,80	0,018	SW	1	Fine.
	2	0	66,0	63,0	29,83		SW	1	Fair.
11	7	0	62,0	62,5	29,94		NW	1	Fine.
	2	0	70,0	65,0	29,90		W by N	1	Fine.
12	7	0	59,0	63,5	30,04		ENE	1	Cloudy.
	2	0	65,0	65,0	30,09		NE	1	Cloudy.
13	7	0	59,0	62,0	30,03		SW	1	Fine.
	2	0	72,0	69,5	29,98		SW	1	Fine.
14	7	0	55,5	61,5	29,81		SW	2	Fine.
	2	0	66,0	64,0	29,83		SW	2	Fine.
15	7	0	55,5	60,5	29,85	0,017	NW	1	Fine.
	2	0	63,0	64,0	29,92		NW	2	Cloudy.
16	7	0	56,0	59,0	30,01	0,020	WSW	1	Fair.
	2	0	63,0	62,0	30,04		SW	1	Fair.

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	Time. H.	Time. M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
June 17	7	0	58,5	60,5	29,91	0,030	SW	1	Fair.
	2	0	67,0	63,0	29,91		SW	1	Fair.
18	7	0	56,5	58,5	29,81	0,240	SW	1	Rainy.
	2	0	65,5	64,0	29,81		SW	1	Rainy.
19	7	0	60,0	62,5	29,79	0,152	SSW	1	Cloudy.
	2	0	72,0	67,0	29,79		SW	1	Fair.
20	7	0	60,0	61,0	29,95		W by S	1	Fine.
	2	0	66,0	69,0	29,99		SW	1	Fine.
21	7	0	57,5	62,0	30,07	0,022	W by N	1	Fine.
	2	0	68,0	64,5	30,11		SW	1	Fine.
22	7	0	58,5	62,5	30,11		SSW	1	Cloudy.
	2	0	61,0	64,0	30,07		SW	1	Rainy.
23	7	0	56,0	61,0	30,04	0,320	NW	1	Fine.
	2	0	70,0	66,0	30,18		SW	1	Fine.
24	7	0	60,0	63,5	30,10		SSW	1	Fair.
	2	0	67,5	66,5	30,07		SW	1	Fair.
25	7	0	58,0	65,0	30,08		W by S	2	Fair.
	2	0	67,0	65,0	30,12		NW	2	Fine.
26	7	0	57,5	62,0	30,24		NW	2	Fine.
	2	0	66,0	65,0	30,31		SW	1	Fine.
27	7	0	56,0	62,0	30,37		NW	1	Fine.
	2	0	69,0	64,5	30,37		SW	1	Fine.
28	7	0	62,0	64,0	30,32		NW	1	Fair.
	2	0	74,0	68,0	30,28		NNW	1	Fine.
29	7	0	66,0	67,5	30,25		SE	1	Fine.
	2	0	78,0	70,0	30,19		E	1	Fine.
30	7	0	67,0	66,0	29,99		SE	1	Fine.
	2	0	84,5	76,0	29,94		ESE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 1	7	0	64,5	70,0	29,96		SW	1	Fair.
	2	0	73,0	72,5	29,95		S by E	1	Fair.
2	7	0	63,0	68 0	29 89	0,143	SW	1	Rainy.
	2	0	73 0	72,0	29,99		SW	1	Fine.
3	7	0	61,5	68,5	29,99		SW	1	Fine.
	2	0	69,0	70,0	30,05		SW	1	Cloudy.
4	7	0	60,0	66,5	30.02		SW	1	Fair.
	2	0	70,5	68.5	30 05		NW	1	Fine.
5	7	0	61,0	67,0	30,18		SW	1	Fine.
	2	0	69,0	66,0	30,09		SW	1	Fine.
6	7	0	61,5	64,0	30 09		NW	1	Fine.
	2	0	71,0	69,0	30,08		NW	1	Fine.
7	7	0	58,5	61,0	30,06		NW	1	Fine.
	2	0	65,0	64,5	30,04		NW	1	Fine.
8	7	0	60,5	62,5	29 97		W by N	1	Fine.
	2	0	68,5	65,5	29,99		NW	1	Fine.
9	7	0	61,0	62,0	30,06		NE	1	Fair.
	2	0	68,0	66,0	30,08		WNW	1	Fine.
10	7	0	57,0	64,0	30,14	0,017	NE	1	Fine.
	2	0	67,0	66,5	30,19		NNE	1	Fine.
11	7	0	54,0	62,0	30,17		N by E	1	Fair.
	2	0	70,5	68,5	30,14		NW	1	Fine.
12	7	0	61,0	65,0	29 97		WSW	1	Cloudy.
	2	0	74,0	70,0	29,94		NNW	1	Cloudy.
13	7	0	60,0	65,5	29 98	0,116	NNW	1	Cloudy.
	2	0	68,0	68,0	30,07		NW	1	Cloudy.
14	7	0	60,0	62,5	30,16		NW	1	Fine.
	2	0	70,5	68,5	30,19		NW	1	Fine.
15	7	0	60 5	66,5	30,00		WSW	1	Cloudy.
	2	0	72,0	69,5	29,92		NW	1	Fine.
16	7	0	66,0	69,5	29,99		NW	1	Fine.
	2	0	79,5	72,0	29 98		NW	1	Fine.

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	Time.		Therm without	Therm. within	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 17	7	0	68,5	73,0	29,88		SE	1	Fine.
	2	0	82,0	78,5	29,87		SE	1	Fine.
18	7	0	70,0	75,0	29,74	0,028	SW	1	Fine.
	2	0	72,0	76,5	29,83		SW	1	Fair.
19	7	0	62,0	71,0	30,00		WSW	1	Fair.
	2	0	68,5	69,0	30,06		SW	1	Fine.
20	7	0	58,5	67,0	30,13		WSW	1	Fine.
	2	0	72,5	69,5	30,09		WSW	1	Fine.
21	7	0	60,0	67,0	29,87	0,038	SW	1	Rainy.
	2	0	67,5	68,5	29,76		WSW	1	Cloudy.
22	7	0	61,0	67,0	29,59	0,926	SE	1	Rainy.
	2	0	66,0	68,5	29,66		S by W	1	Rainy.
23	7	0	59,5	66,0	29,99	0,319	NE	1	Cloudy.
	2	0	66,0	67,0	30,01		NE	1	Fair.
24	7	0	58,5	65,0	30,18		SW	1	Fair.
	2	0	79,0	69,5	30,17		SW	1	Fine.
25	7	0	65,0	69,5	30,16	0,024	NNE	1	Cloudy.
	2	0	80,0	72,0	30,19		NNE	1	Fine.
26	7	0	62,5	63,5	30,24		NW	1	Cloudy.
	2	0	78,0	75,0	30,23		SW	1	Fine.
27	7	0	64,0	70,5	30,14		NW	1	Fair.
	2	0	71,0	71,0	30,21		NW	1	Fiue.
28	7	0	59,0	60,0	30,34		SSW	1	Fine.
	2	0	71,5	68,5	30,34		SW	1	Fine.
29	7	0	60,0	66,0	30,25		NE	1	Fine.
	2	0	71,0	68,0	30,19		E by S	1	Fine.
30	7	0	61,5	63,0	30,11		NE	1	Cloudy.
	2	0	75,0	70,0	30,11		ESE	1	Fine.
31	7	0	60,5	66,5	30,19		ENE	1	Cloudy.
	2	0	70,5	68,5	30,19		NE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Aug. 1	7	0	59,0	65,0	30,20		NE	1	Cloudy.
	2	0	78,0	74,0	30,19		NE	1	Fine.
2	7	0	62,0	68,0	30,11		SW	1	Fair.
	2	0	78,0	71,0	30,18		SW	1	Fine.
3	7	0	61,5	68,5	30,09		NE	1	Fine.
	2	0	83,0	73,5	30,13		NE	1	Fine.
4	7	0	68,0	69,0	30,10		ENE	1	Fine.
	2	0	78,0	73,0	30,14		NE	1	Fine.
5	7	0	60,5	66,5	30,11		NNE	1	Fair.
	2	0	76,5	74,0	30,11		NW	1	Fine.
6	7	0	60,0	67,0	30,07		NNE	1	Cloudy.
	2	0	78,5	73,0	30,07		N by W	1	Fine.
7	7	0	62,0	69,0	30,12		NE	1	Fair.
	2	0	80,0	74,0	30,16		NE	1	Fine.
8	7	0	59,5	67,0	30,09		NE	1	Fair.
	2	0	77,0	75,0	30,07		NE	1	Cloudy.
9	7	0	62,5	66,0	30,02		NE	1	Fair.
	2	0	82,0	75,0	30,04		NE	1	Fine.
10	7	0	64,5	71,0	30,02		NE	1	Fine.
	2	0	81,5	76,5	30,02		E by S	1	Fine.
11	7	0	66,5	73,0	30,02		ENE	1	Fine.
	2	0	79,0	76,0	29,98		ENE	1	Fine.
12	7	0	65,0	72,0	29,91		NE	1	Fine.
	2	0	79,0	73,0	29,89		S by E	1	Fine.
13	7	0	62,5	65,0	29,89		NE	1	Fine.
	2	0	76,5	75,0	29,93		NE	1	Fine.
14	7	0	60,0	62,5	29,93		NE	1	Fair.
	2	0	62,0	69,0	29,95		NE	1	Rainy.
15	7	0	61,5	66,5	29,88	0,104	NE	1	Fair.
	2	0	68,0	69,0	29,92		NE	1	Fair.
16	7	0	65,0	68,0	29,91		NE	1	Fair.
	2	0	75,0	72,0	30,03		NE	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.		Points.	Str.	
Aug. 17	7	0	65,5	67,5	30,02		SE	1	Fair.
	2	0	75,0	72,0	30,00		NE	1	Fair.
18	7	0	60,5	68,0	29,86	0,381	ENE	1	Rainy.
	2	0	71,0	71,0	29,85		ENE	1	Rainy.
19	7	0	64,0	66,5	29,92		NNE	1	Cloudy.
	2	0	74,0	72,5	29,99		NE	1	Fine.
20	7	0	63,5	66,0	30,12		ESE	1	Fine.
	2	0	74,5	71,5	30,13		SSE	1	Fine.
21	7	0	67,0	68,0	30,09		S by E	1	Fine.
	2	0	76,0	75,0	30,09		SE	1	Fine.
22	7	0	64,5	68,5	29,99		WSW	1	Fair.
	2	0	70,0	69,0	30,03		SW	1	Fine.
23	7	0	59,0	67,0	30,07		NW	1	Fair.
	2	0	72,0	70,0	30,14		SW	1	Fine.
24	7	0	58,5	63,0	30,18		ENE	1	Fair.
	2	0	74,0	72,5	30,19		NW	1	Fine.
25	7	0	59,5	62,0	30,13		NNE	1	Fine.
	2	0	75,0	73,0	30,15		NW	1	Fine.
26	7	0	60,5	64,0	30,16		NE	1	Fine.
	2	0	76,5	75,0	30,16		ENE	1	Fine.
27	7	0	61,0	63,0	30,15		E by N	1	Fine.
	2	0	73,5	69,5	30,08		E by N	1	Fine.
28	7	0	60,0	65,0	30,10		NE	1	Fair.
	2	0	68,5	68,0	30,11		NE	1	Fine.
29	7	0	61,0	65,5	30,19		NE	1	Fair.
	2	0	72,0	71,0	30,14		NE	1	Fine
30	7	0	61,0	67,0	30,10		NE	1	Cloudy.
	2	0	76,5	74,0	30,09		NE	1	Fine.
31	7	0	69,0	69,5	30,05		SSW	1	Fine.
	2	0	83,5	76,5	30,05		S by W	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 1	7	0	67,5	73,5	30,01		SSW	1	Fine.
	2	0	84,0	79,0	29,89		SW	1	Fine.
2	7	0	66,0	73,0	29,95		W by S	1	Fair.
	2	0	77,0	76,0	29,94		SW	1	Fine.
3	7	0	63,0	71,0	30,01		NE	1	Fine.
	2	0	70,0	71,0	30,05		SW	1	Fine.
4	7	0	56,0	63,5	30,01		NE	1	Fair.
	2	0	70,0	68,0	30,17		SW	1	Fair.
5	7	0	53,5	62,5	30,20		E by S	1	Fine.
	2	0	71,5	69,0	30,18		SE	1	Fine.
6	7	0	56,0	63,0	30,02		SE	1	Fine.
	2	0	71,5	69,0	29,95		SE	1	Fine.
7	7	0	57,5	62,0	29,75		ENE	1	Cloudy.
	2	0	76,0	69,0	29,73		SE	1	Cloudy.
8	7	0	65,0	68,0	29,59	0,250	SSW	1	Fair.
	2	0	72,0	70,5	29,61		SE	1	Fine.
9	7	0	58,0	67,5	29,74		S by W	1	Rainy.
	2	0	64,0	68,0	29,81		SW	1	Cloudy.
10	7	0	54,0	64,5	29,83	0 560	NNE	1	Cloudy.
	2	0	70,0	67,0	29,74		S by E	1	Rainy.
11	7	0	59,0	65,0	29,71	0,327	SSW	1	Rainy.
	2	0	67,0	67,0	29,82		SW	1	Cloudy and rain.
12	7	0	54,0	63,0	29,96	0,258	SW	1	Fine.
	2	0	67,5	66,5	29,99		NW	1	Fine.
13	7	0	61,0	62,0	30,03		S by W	1	Fine.
	2	0	69,0	65,5	29,99		SSW	1	Fair.
14	7	0	57,0	64,0	29,85	0,167	SE	1	Rainy.
	2	0	66,0	66,5	29,86		S by E	1	Cloudy.
15	7	0	57,0	63,5	29,92		S by E	1	Fine.
	2	0	62,0	73,5	29,98		SE	1	Fine.
16	7	0	62,5	65,0	29,76		SE	1	Showery.
	2	0	59,0	67,5	29,76		SE	1	Showery.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.		Points.	Str.	
Sept. 17	7	0	60,5	65,0	29,49	0,225	SE	1	Rainy.
	2	0	68,0	67,0	29,49		SE	2	Fine.
18	7	0	58,0	63,5	29,49	0,572	SSW	1	Fair.
	2	0	68,0	67,0	29,58		SW	1	Fine.
19	7	0	53,0	60,0	29,64		SSE	1	Fine.
	2	0	65,0	65,0	29,68		SE	1	Fine.
20	7	0	52,5	60,5	29,67		SE	1	Fine.
	2	0	64,0	62,5	29,66		NW	1	Fine.
21	7	0	49,5	59,5	29,81		S by W	1	Fine.
	2	0	65,0	62,0	29,88		SW	1	Fine.
22	7	0	48,0	56,0	30,02		SSW	1	Fine.
	2	0	65,0	65,0	30,11		SW	1	Fine.
23	7	0	49,5	57,0	30,09		ENE	1	Fine.
	2	0	66,0	66,0	30,05		NE	1	Fine.
24	7	0	59,0	59,5	29,84		NW	1	Fair.
	2	0	66,5	65,0	29,99		NW	1	Showery.
25	7	0	54,0	58,0	30,04	0,113	SSW	1	Fine.
	2	0	66,5	67,0	30,04		WSW	1	Cloudy.
26	7	0	59,0	61,0	29,97		SW	1	Fair.
	2	0	67,0	64,0	29,92		SW	1	Fair.
27	7	0	51,0	59,0	29,90	0,079	WSW	1	Fine.
	2	0	66,0	63,5	29,94		SW	1	Fair.
28	7	0	56,0	60,0	29,75		SW	1	Cloudy.
	2	0	64,0	64,5	29,76		SW	1	Showery and cloudy.
29	7	0	53,5	61,5	29,54	0,320	SSE	1	Rainy.
	2	0	62,0	63,0	29,64		SE	1	Fair.
30	7	0	50,5	57,0	29,34	0,032	SSW	1	Rainy.
	2	0	52,0	57,0	29,14		SW	2	Rainy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 1	7	0	45,0	52,5	29,19	0,076	SSW	1	Rainy.
	2	0	58,5	55,5	29,09		SSW	1	Rainy.
2	7	0	53,0	55,0	29,07	0,106	SW	1	Fair.
	2	0	57,0	56,5	29,44		SW	2	Fine.
3	7	0	52,5	54,0	29,57		WSW	1	Fine.
	2	0	58,5	55,5	29,64		NW	1	Fine.
4	7	0	42,0	51,5	29,79	0,050	NNE	1	Fair.
	2	0	54,0	59,5	29,87		NW	1	Fair.
5	7	0	41,5	50,5	30,16		NW	1	Fine.
	2	0	54,0	52,5	30,18		WSW	1	Fair.
6	7	0	45,5	49,0	30,04		S by E	1	Cloudy.
	2	0	57,0	53,5	29,90		S by W	1	Cloudy.
7	7	0	52,0	53,0	29,63		SSW	1	Fair.
	2	0	58,5	55,5	29,56		SW	1	Fine.
8	7	0	43,5	52,0	29,62		SSW	1	Fair.
	2	0	57,5	54,5	29,44		SE	1	Fine.
9	7	0	56,5	55,0	28,66	0,590	SSW	3	Rainy.
	2	0	57,0	56,5	28,82		SSW	3	Showery.
10	7	0	51,5	50,0	28,68		SW	1	Fair.
	2	0	58,0	55,0	28,99		SW	2	Cloudy.
11	7	0	45,5	53,5	29,51	0,020	SW	2	Fair.
	2	0	58,0	56,0	29,61		SW	2	Fair.
12	7	0	43,5	53,0	29,68	0,025	SW	1	Fair.
	2	0	55,0	54,0	29,81		W by N	1	Fine.
13	7	0	48,5	51,5	30,04		SSE	1	Cloudy.
	2	0	60,0	65,0	30,02		SE	2	Fair.
14	7	0	52,0	53,5	29,75		SE	1	Cloudy.
	2	0	65,0	59,0	29,61		SE	1	Fine.
15	7	0	58,5	59,0	29,69		SSE	1	Cloudy.
	2	0	66,0	62,0	29,69		SE	1	Cloudy.
16	7	0	53,5	56,5	29,72	0,530	SSW	1	Rainy.
	2	0	62,0	61,0	29,71		WSW	1	Cloudy.

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	Time. H.	Time. M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Oct. 17	7	0	55,0	58,0	29,87		SSW	1	Fine.
	2	0	65,0	63,5	29,91		SSW	1	Fine.
18	7	0	49,5	55,0	30,02		NNE	1	Cloudy.
	2	0	62,0	60,0	30,07		NNE	1	Fine.
19	7	0	53,0	59,0	29,79		SW	2	Fine.
	2	0	56,0	59,0	29,58		SW	2	Rainy.
20	7	0	46,5	56,0	29,48	0,071	WSW	1	Fine.
	2	0	52,0	56,0	29,44		SW	1	Cloudy.
21	7	0	47,0	50,5	29,48		NW	2	Fine.
	2	0	55,0	53,0	29,63		WNW	1	Fine.
22	7	0	46,0	50,5	29,73		SW	1	Cloudy.
	2	0	54,5	52,5	29,67		SW	1	Cloudy and rainy.
23	7	0	41,5	49,0	29,86	0,068	SW	1	Fine.
	2	0	53,0	52,0	29,88		SW	2	Cloudy.
24	7	0	50,0	51,5	29,66		SE	1	Cloudy.
	2	0	59,0	61,0	29,70		SE	1	Fair.
25	7	0	43,5	51,0	29,73		NE	1	Fine.
	2	0	51,5	52,0	29,77		NW	1	Fine.
26	7	0	37,0	48,0	30,07		SSW	1	Fine.
	2	0	52,0	50,0	30,14		NNE	1	Fine.
27	7	0	45,0	46,0	30,27		NE	1	Cloudy.
	2	0	50,0	48,0	30,29		NE	1	Fair.
28	7	0	47,0	48,0	30,14		NNE	1	Cloudy.
	2	0	55,0	51,5	30,07		NE	1	Cloudy.
29	7	0	47,5	51,0	29,85		NW	1	Rainy.
	2	0	49,5	51,5	29,82		NW	1	Rainy.
30	7	0	49,5	51,5	29,71	0,435	NNW	1	Rainy.
	2	0	50,0	52,0	29,82		NNW	1	Rainy.
31	7	0	49,5	52,0	29,88	0,375	SW	1	Rainy.
	2	0	50,5	52,5	29,89		NE	1	Rainy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 1	8	0	47,5	49,5	29,94	0,118	NNE	1	Cloudy.
	2	0	50,0	51,0	30,02		NE	2	Rainy.
2	8	0	46,0	48,5	30,08		NNE	1	Cloudy.
	2	0	50,0	51,0	30,08		NE	1	Fine.
3	8	0	46,0	48,5	29,97		NE	1	Cloudy.
	2	0	50,0	53 0	29,94		NE	1	Fair.
4	8	0	40,0	46,0	29 97		NW	1	Fine.
	2	0	46,0	47,0	30,08		NW	2	Fine.
5	8	0	47,0	47,0	29,83	0,039	SW	1	Fine.
	2	0	51,5	49,0	29,72		NW	2	Fine.
6	8	0	40,0	45,0	29 86		NW	2	Fine.
	2	0	44,0	45,0	29 87		NW	2	Fine.
7	8	0	35,0	37,5	29 72	0,039	NNE	2	Fine.
	2	0	36,0	38 0	29 82		NE	3	Snowy.
8	8	0	36,0	37,5	29,91	0,081	NNE	2	Fine.
	2	0	42,0	40,0	29 94		NE	1	Fine.
9	8	0	33,0	37,5	30 09		SW	1	Cloudy.
	2	0	41,0	40,0	30,05		SSW	1	Cloudy.
10	8	0	48,5	44,0	30 07	0,075	NE	1	Cloudy.
	2	0	52,0	47,0	30,11		NE	1	Fair.
11	8	0	46,5	45 0	30 11		NE	1	Cloudy.
	2	0	52 0	48,0	30 17		NNE	1	Cloudy.
12	8	0	47,0	49,0	30,09		WSW	1	Fine.
	2	0	51,0	49,5	30,12		WSW	1	Cloudy.
13	8	0	51,5	50,0	29,73		SW	3	Cloudy.
	2	0	52,0	51,0	29,64		SW	3	Cloudy.
14	8	0	39,5	47,0	29 82		WSW	1	Fair.
	2	0	47,0	47 5	29,84		WSW	1	Fine.
15	8	0	30,0	41,0	29,97	0,095	NW	1	Fine.
	2	0	36 0	41 0	30,02		NW	1	Fine.
16	8	0	26,0	36 0	29,93		SE	1	Foggy.
	2	0	28,5	38,0	30 16		SE	1	Foggy.

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	Time.		Therm without	Therm within.	Baron.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 17	8	0	36,5	38,5	29,73		SE	1	Foggy.
	2	0	40,0	41,0	29,63		SE	1	Rainy.
18	8	0	41,0	40,0	29,60	0,030	SE	1	Cloudy.
	2	0	45,0	42,0	29,53		SSE	1	Fair.
19	8	0	41,0	42,5	29,27	0,039	NW	1	Fair.
	2	0	43,5	44,0	29,32		NW	1	Fine.
20	8	0	43 0	43,5	28,94	0,616	W by S	1	Cloudy.
	2	0	46,0	49,0	29,02		SW.	1	Fair.
21	8	0	36,5	41,5	29,14	1,074	NW	1	Fair.
	2	0	41,5	43,0	29,31		NW	1	Cloudy.
22	8	0	36,0	41,0	29,14	0,025	SW	1	Cloudy.
	2	0	37,0	40,5	29,08		WNW	1	Fine.
23	8	0	32,0	37,0	29,39		NW	1	Fine.
	2	0	37,0	38,0	29,42		NW	1	Fair.
24	8	0	31,0	39,0	29,64		SW	1	Foggy.
	2	0	36,0	38,0	29,50		SW	1	Rainy.
25	8	0	36,0	39,0	29,89	0,236	NW	1	Cloudy.
	2	0	38,0	40,5	29 98		WSW	1	Cloudy.
26	8	0	42,0	39,5	30,10		WSW	1	Cloudy.
	2	0	48,0	42,0	30,14		NW	1	Cloudy.
27	8	0	44,0	45,0	30.27	0,038	NW	1.	Cloudy.
	2	0	47,0	45,0	30.28		NNW	1	Cloudy.
28	8	0	42,0	44,5	30,46		EN[illegible]	1	Cloudy.
	2	0	45,5	46,0	30,47		NE	1	Fine.
29	8	0	37,0	42,0	30,45		NNE	1	Fine.
	2	0	40,0	43,0	30,40		NNE	1	Fine.
30	8	0	37,5	42,5	30.32		NNE	1	Fine.
	2	0	40,5	42,0	30,32		NNE	1	Fine.

METE-

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	Time. H.	Time. M.	Therm. without	Therm. within.	Barom Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Dec. 1	8	0	38,5	40,5	30,13		NE	1	Fair.
	2	0	43,5	41,5	30,31		NE	1	Fair.
2	8	0	42,0	42,5	30,34		NE	1	Cloudy.
	2	0	44,0	46,0	30,36		NE	1	Cloudy.
3	8	0	38,0	40,0	30,47		NE	2	Cloudy.
	2	0	41,5	43,0	30,49		NE	2	Cloudy.
4	8	0	40,5	41,5	30,51		NE	1	Fine.
	2	0	45,0	44,0	30,49		NE	1	Cloudy.
5	8	0	39,5	42,0	30,45		NE	2	Cloudy.
	2	0	43,5	44,0	30,40		NE	1	Cloudy.
6	8	0	38,0	41,5	30,35		NE	1	Cloudy.
	2	0	39,0	42,5	30,33		NE	1	Cloudy.
7	8	0	33,0	39,5	30,29		NE	1	Cloudy.
	2	0	33,5	36,0	30,28		NE	1	Cloudy.
8	8	0	36,0	39,0	30,29		NE	1	Fine.
	2	0	40,0	40,0	30,31		NE	1	Fair.
9	8	0	45,0	48,0	30,53		NE	1	Fair.
	2	0	40,0	40,0	30,53		NE	1	Fair.
10	8	0	33,5	37,0	30,45		WSW	1	Fair.
	2	0	38,0	36,0	30,44		SW	1	Cloudy.
11	8	0	38,5	40,0	30,36		NW	1	Fine.
	2	0	45,0	42,0	30,34		NW	1	Fair.
12	8	0	41,0	41,5	30,45		SW	1	Foggy.
	2	0	45,5	43,0	30,41		S by E	1	Foggy.
13	8	0	40,5	43,5	30,44		NW	1	Foggy.
	2	0	44,0	44,0	30,39		WNW	1	Cloudy.
14	8	0	40,0	42,0	30,36		SW	1	Foggy.
	2	0	45,0	45,0	30,46		SW	1	Fair.
15	8	0	41,0	44,0	30,44		NW	1	Fine.
	2	0	45,0	45,0	30,46		NW	1	Fair.
16	8	0	40,0	44,5	30,55		NNE	1	Foggy.
	2	0	43,5	45,0	30,55		SE	1	Cloudy.

METE-

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for December 1780.

	Time. H.	Time. M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Dec. 17	8	0	40,0	44,0	30,51		SW	1	Foggy.
	2	0	43,5	44,0	30,48		NW	1	Fine.
18	8	0	34,5	41,0	30,46		NE	1	Foggy.
	2	0	40,0	41,5	30,46		ENE	1	Fine.
19	8	0	30,5	38,0	30,49		ENE	1	Fine.
	2	0	34,5	36,0	30,40		NE	1	Fine.
20	8	0	30,0	34,5	30,08		NE	1	Foggy.
	2	0	32,0	39,0	29,99		NE	1	Foggy.
21	8	0	28,0	33,0	29,69		S by E	1	Foggy.
	2	0	29,0	34,5	29,69		S by E	1	Foggy.
22	8	0	33,0	34,0	29,85		WSW	1	Foggy.
	2	0	36,5	35,0	29,89		NW	1	Fine.
23	8	0	24,0	32,0	30,05		SSW	1	Foggy.
	2	0	32,5	34,0	30,06		NW	1	Fine.
24	8	0	34,0	34,5	30,15		NNE	1	Foggy.
	2	0	37,0	35,5	30,15		N by E	1	Fair.
25	8	0	37,0	37,5	30,21		NNE	1	Fair.
	2	0	38,5	37,5	30,23		NNE	1	ir.
26	8	0	32,5	34,5	30,26		ENE	1	Fair.
	2	0	36,0	36,5	30,26		N by E	1	Fair.
27	8	0	29,5	32,5	30,26		NE	1	Frosty.
	2	0	32,0	34,5	30,29		NE	1	Frosty.
28	8	0	32,5	33,5	30,35	0,100	SSW	1	Foggy.
	2	0	37,0	36,5	30,34		SW	1	Cloudy.
29	8	0	40,5	37,0	30,13	0,210	SW	1	Cloudy.
	2	0	45,0	39,0	30,03		WSW	1	Fair.
30	8	0	45,0	41,0	30,01		WSW	1	Cloudy.
	2	0	50,0	44,0	30,07		SW	1	Cloudy.
31	8	0	43,5	44,0	30,06		SSW	1	Cloudy.
	2	0	50,0	46,5	30,07		WSW	1	Fair.

1780	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Inches.
Jan.	47,0	20,0	31,9	40,5	24,5	33,6	30,35	28 59	29,77	0,692
Feb.	53,5	20,0	37,8	49,0	31,0	38,4	30,52	29,08	29,91	0,858
Mar.	59,0	34,5	51,4	56,5	36,0	49,9	30,45	29,38	29,91	1,189
Apr.	65,5	33,0	46,7	65,0	38,0	48,0	30,17	28,81	29,65	2,739
May	84,5	45,0	59,7	74,5	51,0	60,4	30,28	29,38	29,94	0,822
June	84,5	48,0	62,9	76,0	50,0	63,2	30,37	29,79	30,01	0,852
July	82,0	54,0	66,4	78,5	61,9	69,4	30,34	29,59	30,05	1,602
Aug.	83,5	58,5	69,2	76,5	62,0	68,3	30,20	29,85	30,05	0,485
Sept	84,0	48,0	61,3	79,0	56,0	65,2	30,20	29,14	29,83	2,903
Oct.	66,0	41,5	52,3	65,0	46,0	53,9	30,29	28,66	29,69	2,356
Nov.	52,0	26,0	42,0	53,0	36,0	43,6	30,47	28,94	29,85	2,505
Dec.	50,0	24,0	38,5	46,5	32,0	39,8	30,55	29,69	30,29	0,310
Whole year			51,7			52,8			29,91	17,313

VARIA-

VARIATION NEEDLE.

	7 h. A. M.	12 h. M.	2 h. P.M.	10 or 11 h. P.M.	Daily Means.
	° ′	° ′	° ′	° ′	° ′
June 5	22 25	22 26	22 21	22 23	22 24
6	22 21	22 29	22 57	22 44	22 38
7	22 30	22 49	22 48	22 29	22 39
8	22 45	22 44	22 45	22 46	22 45
9	22 31	22 42	22 48	22 40	22 40
10	22 25	22 48	22 57	22 40	22 42
11	22 45	22 49	22 48	22 44	22 46
12	22 37	22 49	22 46	22 41	22 43
13	22 31	22 53	22 45	22 35	22 41
14	22 29	22 55	22 50	22 50	22 46
15	22 25	22 40	22 49	22 47	22 40
16	22 45	22 48	22 47	22 49	22 47
17	22 27	22 32	22 55	22 55	22 42
18	22 39	22 37	22 41	22 48	22 41
Means	22 32	22 43	22 47	22 42	

Mean of all 22° 41′.

DIPPING NEEDLE.

	7 h. A.M.	12 h. M.	2 h. P.M.	10 or 11 h. P.M.	Means.	
	° ′	° ′	° ′	° ′	° ′	
June 5	72 0	72 5	72 0	72 0		
6	72 5	72 5	72 10	72 10	72 3	West mark down.
7	72 0	72 0	72 10	72 0		
8	72 0	72 0	72 5	72 10		
9	72 20	72 20	72 20	72 20	72 21	East.
10	72 20	72 20	72 30	72 10		
11	72 20	72 30	72 25	72 10		
12	72 30	72 30	72 30	72 0		
13	72 5	72 15	72 10	72 10	72 13	East mark uppermost.
14	72 10	72 15	72 20	72 10		
15	72 20	72 15	72 20	72 20		
16	72 45	72 40	72 40	72 20		
17	72 40	72 30	72 25	72 30	72 32	West.
18	72 40	72 20	72 30	72 20		

THE END OF PART I. OF VOL. LXXI.

ELOCUTION AND POLITE LITERATURE.

MR. HENRY SMEATHMAN, who was many years a pupil of the late Mr. Rice, and before he went on his travels, practiſed with ſucceſs that Author's ingenious method of teaching to read, ſpeak and write, with eaſe, energy and propriety; begs leave to offer his ſervices to the public in the ſame mode of uſeful and polite inſtruction.

Gentlemen intending themſelves for the Church, the Bar or the Senate, will by oral and familiar converſation, in a few months, receive more improvement, than by a ſolitary ſtudy of many years, in an art not taught in our Schools or Univerſities, yet abſolutely neceſſary to thoſe who wiſh to excel in public or polite life, or aſpire to eminence in eloquence and literature.

By this art, a liberal education is improved and accompliſhed, claſſical learning advantageouſly diſplayed, and the want of a proficiency in the dead and other languages ſupplied, as by Mr. Smeathman's method, founded on that of Mr. RICE, the pupils may acquire a critical knowledge of the idiom and elegance of their own without them.

This recommends it peculiarly to the attention of all those who wish to have the pleasure of reading the English classic authors with grace and propriety, or to write and speak correctly; of those Gentlemen, who are intended for the Navy, the Army, the Public-office or the Counting-house; or whose juvenile studies may have been obliterated, by an early introduction into those situations; and of all those who may have been deprived of the advantages of a liberal education.

They are effectually cured of any defects in pronunciation, which do not arise from an imperfection in the organs of speech; and instead of provincial, disagreeable or absurd habits, they acquire an energetic, a persuasive and a graceful manner of speaking, reading and reciting. Above all, the younger pupils receive this singular and important advantage, that their minds are opened more early, their reason is assisted in its exertions, and directed to such objects as naturally promote the perfection, and the happiness of human beings.

This method is equally well adapted to both sexes, those advanced in life may receive instruction privately, as usual, and Ladies will be attended at their own houses.

Application may be made to Mr. Smeathman, at his Chambers, No 14. Clement's-Inn. Of whom, in a few days, may be had a short Treatise on the nature of eloquence, and the importance of learning to read, speak and write our own language, independent of all others; with a plan of his mode of teaching, and the advantages to be derived from it.

The following are detached extracts from the above-mentioned Treatise.

 "Those

" Thoſe who have not ſtudied and been exerciſed in reading, " ſpeaking or compoſing, in their own language, will not do " either in ſuch a manner as to claim or deſerve attention; " and it is the talent of performing them with eaſe, energy " and propriety, which we call eloquence.

" People are uſually very ſolicitous to have their children in- " ſtructed in all thoſe liberal arts, which are deemed requiſite to " perfect them as Gentlemen and Scholars: and ſurely as no- " thing contributes more to this intention, they would not omit " oratory, if they thought it was to be learned. They liſten " with rapture to the charms of eloquence, but are not aware " that it is in their power to procure this ineſtimable talent " for thoſe who are the objects of their inceſſant care and daily " anxiety."———

—" This can only ariſe from the erroneous idea that oratory is " not to be taught or acquired. Yet no art is more ſimple in " its Principles, or clearer in its rules, and no rules are more " eaſily reduced to practice, or more certainly practiſed with " ſucceſs."———

—" It is granted that people may improve themſelves much " by private declamation. The art of eloquence, however, " is in this reſpect like fencing, in which a man can learn " very little by puſhing againſt a wall: he may indeed " ſtrengthen his muſcles, and enable himſelf to bear the " fatigue, but it is by frequent exertion with a maſter only " that he can expect to acquire ſkill.

"It is from the friendly obſervations, the oral information, "the frequent contentions and the unreſtrained corrections, "of a ſkilful companion, that a ſtudent in oratory can derive "eſſential improvement: writing alone can no more commu- "nicate the art than it can ſpeech to the dumb.

"Thoſe therefore who educate their children for eminent "ſtations in life, with the hope that they will one day render "themſelves conſpicuous in the Senate, the Church or the "courts of Law and Juſtice, without having them grounded in "the firſt principles of eloquence, expect to reap where they "have not ſown: they muſt in all probability be diſappoint- "ed."——

—"There is ſcarce a man of a moderate age and capacity not "born with ſome invincible imperfection in the organs of "ſpeech, who may not in twelve months be enabled to com- "municate his thoughts with pleaſure to his audience, and "credit to himſelf; or ſo to read the Liturgy as to excite "Devotion in the moſt lukewarm, and to confirm it by a na- "tural, perſuaſive and convincing manner of delivering the "great and eſſential truths of Religion; and how much this "is to be wiſhed is left to the opinion of all who have heard "the diſguſting and unedifying manner in which the fineſt "compoſitions are generally delivered in our public places of "worſhip, and ſeen the awkward figures ſome of our learned "Senators make when they attempt to ſpeak in Parliament."—

—"We have hitherto treated only of the moſt obvious advan- "tages ariſing from the ſtudy of eloquence. On a cloſer "view,

" view, many other important benefits will be found to be
" derived from it. Of these, opening the mind, improving
" the heart and infusing a desire of knowledge, deserve par-
" ticular notice. Such is the tendency, effect and benefit, of
" the mode of instruction here recommended; for as it applies
" directly to the understanding without burthening the me-
" mory, or perplexing the mind, the youngest pupils recur
" to it with delight; and its uncommon success, when at-
" tempted, has arisen from the immediate conviction which
" they feel, that our path to knowledge is pleasant, and
" the possession of it highly advantageous and desirable."—

" Our youth are generally very defective in those polite
" Branches of Knowledge without which classical Learning is
" of little use. Their manners frequently continue rough and
" puerile, when they ought to be graceful and manly, their
" speech inarticulate or slovenly, dissonant or defective, dis-
" agreeable, and often unintelligible; and very few, even
" among those who are good scholars, can read or write their
" own language with ease or propriety: an accomplishment
" absolutely necessary to those who wish to derive any advan-
" tage from extensive knowledge or profound erudition."

" Young People are naturally emulous of the company of
" persons respectable either for their Rank or Abilities, and if
" they pass their time with low or ignorant people it is for
" want of being properly introduced and accustomed to mix in
" the conversation of men of polished manners and liberal ac-
" complishments."————

—" The mode of Instruction here recommended, must neces-
" sarily in its Course remove these Defects, and the prejudices
" intailed

" intailed on them; and promote the acquiſition of thoſe
" uſefull arts, on which the Succeſs of life ſo much depends."

" In the proſecution of it their Curioſity is excited and di-
" rected to proper objects, and a deſire for general knowledge
" the great foundation of excellence, implanted by making them
" acquainted with ſuch remarkable things in nature and art
" as are worthy of Attention. Inſtead of a ſlovenly or ungrace-
" full mode of ſpeaking, they are taught to deliver their ſenti-
" ments with propriety and preciſion according to times and
" circumſtances; conſequently their manner when improper
" is reformed inſenſibly, and without appearing an object of
" attention; their Minds are cultivated by frequent appeals
" to their underſtanding, and their judgements regulated by
" the conſtant habit of obſervation and reflection, exerciſed
" in a Minute enquiry into the Truth and propriety of ſuch
" thoughts as they are directed to in the courſe of their ſtudy.
" Thus being early qualified for the company of well bred and
" intelligent people they appear with becoming confidence and
" ſatisfaction on all occaſions."———

PHILOSOPHICAL

TRANSACTIONS.

XV. *New Experiments upon Gun-powder, with occasional Observations and practical Inferences; to which are added, an Account of a new Method of determining the Velocities of all Kinds of Military Projectiles, and the Description of a very accurate Eprouvette for Gun-powder.* By Benjamin Thompson, *Esq, F. R. S.*

Read March 29, 1781.

THESE experiments were undertaken principally with a view to determine the most advantageous situation for the vent in fire-arms, and to measure the velocities of bullets, and the recoil under various circumstances. I had hopes also of being able to find out the velocity of the inflammation of gun-powder, and to measure its force more accurately than had hitherto been done. They were begun in the month of July in the year

1778, at Stoneland Lodge, a country ſeat of Lord GEORGE GERMAIN's, and I was aſſiſted by the reverend Mr. BALE, rector of Withyham, who lives in the neighbourhood.

The weather proved remarkably favourable for our experiments, being ſettled and ſerene, ſo that the courſe of them was never interrupted for a whole day by rain or by any accident. The mercury in the barometer ſtood in general pretty high, and the temperature of the atmoſphere was very equal, and moderately warm for the ſeaſon. In order that each experiment might, as nearly as poſſible, be under ſimilar circumſtances, they were all made between the hours of ten in the morning and five in the afternoon: and after each diſcharge the piece was wiped out with tow till all the inſide of the bore was perfectly clean, and as bright as if it had juſt come out of the hands of the maker; and great care was taken to allow ſuch a ſpace of time to elapſe between the firings, as might render the heat of the piece nearly the ſame in every experiment.

A deſcription of the apparatus.

The barrel principally uſed in theſe experiments was made by WOGDON, one of the moſt famous gunſmiths in London; and nothing can exceed the accuracy with which it is bored, or the fineneſs of the poliſh on the inſide. It is made of the very beſt iron, and, agreeably to Mr. ROBINS's advice, I took care to have it well fortified in every part, that there might be no danger of its burſting. Its weight and dimenſions may be ſeen in the table of the weight and dimenſions of the apparatus, p. 242.

Fig. 1. Repreſents a longitudinal ſection of a part of the barrel, with the apparatus firſt made uſe of for ſhifting the vent from one part of the chamber to another, or rather for moving the

the bottom of the chamber further from, or bringing it nearer to, the vent, in order that the fire might be communicated to the powder in different parts of the charge.

a, *b*, represent the lower part of the barrel.

c, is the breech-pin, which is perforated with a hole four-tenths of an inch in diameter, the axis of which coincides with the axis of the bore.

Into this hole the screw *h*, *n*, about four inches in length, is fitted; to the end of which, *n*, that passes up into the bore, is fixed a piston *o*, *p*, which, by means of collars of oiled leather, is made to fit the bore of the piece very exactly. The end of the piston *p*, nearest the muzzle, is of brass, and forms a moveable bottom to the bore, which by turning the screw *h*, *n*, by means of the handle *m*, is brought nearer to, or removed further from, the fixed vent *v*, by which means the powder is lighted at any assignable distance from the bottom of the charge.

But the length of the bore being altered by moving the piston, which occasioned a small inaccuracy, and some inconvenience attending the apparatus, it was laid aside, and another represented by fig. 2. was substituted in the room of it.

a, *b*, is a section of part of the barrel as before, and *c* is the breech-pin, which being perforated with a small hole through its center receives the screw *f*, *g*, which is about two-tenths of an inch in diameter, and four inches long. This screw being perforated with a very small hole, serves to convey the fire into the chamber of the piece, and by screwing it further up into the bore, or drawing it backwards, the fire is communicated to different parts of the charge.

But this method being found to be not intirely free from inaccuracies and inconveniencies, a third was substituted in the

room of it, which was found to answer much better than either of the preceding.

The end of the bore was now firmly closed by a solid breech-pin *p*, fig. 3. and three vent holes *m*, *n*, and *o*, were made in the barrel; one of them, *m*, even with the bottom of the bore, and the other two at different distances from it. Any two of these vent holes, as *n* and *o* for instance, being closed up by solid screws, a perforated screw, or vent tube *v*, was screwed into the third, which served to contain the priming, and to convey the fire to the powder lodged in the bore of the piece.

Sometimes a longer vent-tube, represented by fig. 4. was made use of; which, passing through the powder in the chamber of the piece, communicated the fire immediately to that part of the charge that lay in the axis of the bore.

Another vent-tube also was used occasionally, which differs in many respects from both those that have been described. It is so constructed as to convey the fire to the charge; but, as soon as the powder in the chamber of the piece begins to kindle, and the elastic fluid to be generated, the vent is firmly closed by a valve, and no part of the generated fluid is permitted to escape. This I shall call the *valve-vent*, and it is represented by fig. 5. upon an enlarged scale, that the parts of it may appear more distinct.

a, *b*, is a longitudinal section of a small portion of the solid side of the barrel.

c, *d*, is the vent-tube, which is in all respects like the short vent-tube commonly made use of, except only that in this the end of the vent-hole (*c*) which goes into the chamber is enlarged in the form of the wide end of a trumpet or funnel.

To this enlarged aperture the valve, *v*, is accurately fitted, and by means of the small stem or tail, *t*, which is fixed to the valve,

valve, and which passes up through the vent-hole, and is connected with the spring S, the valve is pressed, or rather drawn into its place, and the vent is closed. The stem of the valve was at first made cylindrical; but, in order to make way for the priming to pass down to the valve, one-half of its substance was taken away, as is represented in the figure.

When this vent is primed, the space between the vent hole and the stem of the valve is filled with fine-grained powder, and the valve is gently opened by pressing upon the end of the stem till one or more grains of powder lodge themselves between the valve and the aperture; which preventing the valve from closing again, a small opening is left for the passage of the flame into the chamber of the piece: therefore, when the priming is lighted, the fire passing down the vent, and entering the chamber, inflames the charge, and the small grains of powder that were lodged between the valve and the aperture being destroyed by the flame in its passage through the vent, the valve immediately closes, and prevents the escape of any part of the elastic fluid generated by the inflammation of the powder in the chamber of the piece. The pressure of this fluid upon the valve assists the action of the spring, by which means the valve is more expeditiously and more effectually closed.

The valve was very accurately fitted to the aperture by grinding them together with powdered emery, and afterwards polishing them one upon the other. And it is very certain, that no part of the elastic fluid made its escape by this vent; for, upon firing the piece, there was only a simple flash from the explosion of the priming, and no stream of fire was to be seen issuing from the vent, as is always to be observed when a common vent is made use of, and in all other cases where this fluid finds a passage.

In order that every part of the apparatus employed in these experiments might be as perfect as possible, all the more delicate parts of it were executed by Mr. FRASER, mathematical instrument-maker in Duke's Court, St. Martin's Lane, and, among the rest, all the contrivances just described relative to the vent.

The velocities of the bullets were determined by means of a pendulum, according to the method invented by Mr. ROBINS.

The pendulum I made use of (fig. 6.) is composed of a circular plate of hammered iron (*a*), 13 inches in diameter, and 0,65 of an inch thick, to which is firmly fastened a bar of iron (*b*, *c*) 56,5 inches in length, 2,6 inches broad, and half an inch in thickness, by which it is suspended by means of two pivots (*d*, *e*) at the end of the bar (*c*), and at right angles to its length. These pivots being very accurately finished, and moving on polished grooves, which were kept constantly oiled to lessen the friction, the vibration of the pendulum was very free, as appeared by the great length of time its vibrations continued after it had been put in motion, and was left to itself. To the circular plate of the pendulum, targets of circular pieces of wood of different thicknesses were fixed, which in the course of the experiments were often spoiled and replaced: and, in order to mark the weight and dimensions of the pendulum in each experiment, the pendulums are numbered according to the different targets that were made use of; and the weight and dimensions of each pendulum are set down in a table at the end of the description of the apparatus.

The target of the pendulum N° 1. was made of a circular piece of elm-plank, 3½ inches thick, and equal in diameter to the iron plate of the pendulum to which it was fixed; but this target being too thin was very soon ruined.

The

The pendulum N° 2. was furnished with two targets, which were circular pieces of very tough oak-plank, near five inches thick, placed on opposite sides of the plate of the pendulum, and firmly fixed to it by screws, and to each other by iron straps. When one of these targets was ruined, the pendulum was turned about, and the other was made use of. This pendulum lasted from experiment N°9. to experiment N°39. when it was so much shattered as to be rendered unfit for further service.

The pendulum N° 3. was like the pendulum N° 2.; only, instead of oak, elm-plank near seven inches in thickness was made use of for the targets. This pendulum served from experiment N° 40. to experiment N° 101. inclusively.

But finding that targets made of planks of the toughest wood were very soon shattered to pieces by the bullets, I composed the pendulum N° 4. in a different manner. Instead of circular pieces of plank, solid cylinders of elm-timber were made use of for the targets, so that the bullets now entered the wood in the direction of its fibres. These cylinders are 13 inches in diameter, and about 5½ inches in length, hooped with iron at both their ends to prevent their splitting, and firmly fastened to the plate of the pendulum, and to each other by four iron straps. This pendulum lasted till the experiments were finished. It is still in being, and appears to be very little the worse for the service it has undergone.

Fig. 7. shews the two ends of the pendulum upon a large scale, together with the hooks or grooves by which it was suspended.

a, *b*, is the bar of the pendulum, which is seen broken off, as there is not room to shew the whole of its length.

c, *d*, are the pivots by which it was suspended.

e, is

e, is the circular plate of the pendulum, to which

f, *g*, two circular targets, are faſtened by ſcrews, and by means of the iron ſtraps, 1, 2, 3, 4, which are nailed to the edges of the targets.

h, *k*, are the hooks which ſerved inſtead of grooves to receive the pivots, *c*, *d*, of the pendulum.

The hooks were firmly fixed to the horizontal beam R. S. which ſupported the whole apparatus by means of three ſcrews *m*, *n*, *o*, which paſſed through three holes in the plate that connects the two hooks. When the hooks were faſtened to the beam, the middle ſcrew, *n*, was firſt put into its place, and the pendulum was allowed to ſettle itſelf in a poſition truly perpendicular, after which the grooves were immoveably fixed by means of the ſcrews *m*, *o*.

The chord of the arc, through which the pendulum aſcended in each experiment, was meaſured by a ribbon, according to the method invented and deſcribed by Mr. ROBINS.

The recoil was meaſured in the following manner. The barrel was ſuſpended in an horizontal poſition (and nearly in a line with the center of the target) by two ſmall pendulous rods, 64 inches in length, and 25,6 inches aſunder; which being parallel to each other, and moving freely upon poliſhed pivots about the axes of their ſuſpenſion, and upon two pair of trunnions that were fixed to the barrel, formed, together with the barrel, a compound pendulum; and from the lengths of the vibrations of this pendulum, the velocity with which the barrel began to recoil, or rather its greateſt velocity, was determined.

But in order that the velocity of the recoil might not be too great, ſo as to endanger the apparatus when large charges were made

made use of, it was found necessary to load the barrel with an additional weight of more than 40 lbs. of iron.

This additional weight of iron, which I shall call the *gun carriage*, as it was so constructed as to serve as a carriage to the barrel, is composed of a bar of hammered iron 28 inches in length, 2,6 inches broad, and half an inch in thickness, which is bent in the middle of its length in such a manner, that its two flat sides or ends are parallel to each other, and distant asunder two inches. In the middle of this bar where it is bent is a hole in the form of an oblong square, which, receiving the end of the breech-pin, supports the lower end or breech of the barrel. The other end of the barrel is supported and confined in the following manner. A ring or hoop of iron, near half an inch thick, and two inches in diameter, is placed in a vertical position between the parallel sides of the bar, and near its two ends, and firmly fixed to them by screws. The barrel passing through the middle of this ring is supported upon the ends of three screws, which passing through the ring in different parts of its circumference all point towards its center.

The carriage, together with the barrel, was suspended by the pendulous rods by means of two pair of polished trunnions that are fixed to the outside of the carriage. They are placed in an horizontal line perpendicular to, and passing through, the axis of the bore.

Fig. 8. represents the barrel fixed to the carriage.

a, *b*, *c*, is the bar of iron which forms the carriage seen edge-ways.

2, 2, 4, 4, are the trunnions by which it was suspended.

d, *e*, is the barrel in its proper place.

p, is the breech-pin, which passing through a hole in the middle of the bar, *a*, *b*, *c*, supports the end, *e*, of the barrel; and

n, is the ring that supports the end, *d*, of the barrel.

Fig. 9. represents a perpendicular section through the line 2, 2, fig. 8, and in a line perpendicular to the length of the barrel.

This figure is designed to shew the manner in which the muzzle of the piece was supported and confined in the ring *n*, fig. 8.

a, *c*, are the two ends of the bar that are seen cut off.

n, is the ring, and

p, *p*, are the screws by which it is fastened to the two parallel sides of the bar, the ends of which form the trunnions 2, 2, fig. 8.

d, is a transverse section of the barrel, and

r, *s*, *t*, are the three screws by which the barrel is supported and confined in the center of the ring.

Fig. 10. is the same as fig. 9. but upon a larger scale.

Fig. 11. represents the two ends of one of the pendulous rods, by which the barrel was suspended; and fig. 13. shews the same seen sideways.

a, *b*, is the rod which is seen broken off.

c, *d*, are the pivots by which it was suspended by a pair of hooks or grooves that were fastened to an horizontal beam, in the same manner as the pendulum for measuring the velocities of the bullets was suspended.

e, *f*, are the hooks which receive the trunnions that are fixed to the carriage.

The dimensions of every part of this apparatus may be seen in the table, p. 242.

The chord of the arc through which the barrel ascended in its recoil was measured by a ribbon, and the lengths of those chords, expressed in inches and decimal parts of an inch, are set down in the tables. The method of computing the velocity

city of the recoil from the chord of the arc through which the barrel aſcended, is too well known to require an explanation: and it is alſo well known, that the velocities are to each other as the chords of thoſe arcs. The lengths of thoſe chords, therefore, as they are ſet down in the tables, are, in all caſes, as the velocities of the recoil.

The powder made uſe of in theſe experiments was of the beſt kind, ſuch as is uſed in proving great guns at Woolwich. A cartridge, containing 12 lbs. of this powder, was given to me by the late General DESAGULIERS of the Royal Artillery, and Inſpector of Braſs and Iron Ordnance; who alſo, in the politeſt manner, offered me every other aſſiſtance in his power towards completing the experiments I had projected, or in making any others I ſhould propoſe that might be uſeful in the proſecution of my inquiries.

This powder was immediately taken out of the cartridge, and put into glaſs bottles, which were previouſly made very clean and dry; and in theſe it was kept carefully ſealed up till it was opened for uſe. When it was wanted for the experiments, it was weighed out in a very exact balance, with ſo much attention, that there could not poſſibly be an error in any inſtance greater than one quarter part of a grain. The bottles were never opened but in fine weather, and in a room that was free from damp, and no more charges of powder than were neceſſary for the experiments of the day were weighed out at a time. Each charge was carefully put up in a cartridge of very fine paper, and theſe filled cartridges were kept in a turned wooden box, that was varniſhed on the inſide as well as the outſide, to prevent its imbibing moiſture from the air.

The paper of which theſe cartridges were made was ſo fine and thin, that 1280 ſheets of it made no more than an inch in

thickness, and a cartridge capable of containing half an ounce of powder weighed but three quarters of a grain.

The cartridges were formed upon a wooden cylinder, and accurately fitted to the bore of the piece, and the edges of the paper were fastened together with paste made of flour and water.

When a cartridge was filled, the powder was gently shaken together, and its mouth was tied up and secured with a piece of fine thread; and when it was made use of it was put intire into the piece, and gently pushed down into its place with the ramrod, and afterwards it was pricked with a priming-wire thrust through the vent, and the piece was primed; so that no part of the powder of the charge was lost in the act of loading, as is always the case when the powder is put loose into the barrel: nor was any part of it expended in priming; but the whole quantity was safely lodged in the bottom of the bore or chamber of the piece, and the bullet was put down immediately upon it, without any wadding either between the cartridge and the bullet, or over the bullet.

The bullets were all cast in the same mould, and consequently could not vary in their weights above two or three grains at most, especially as I took care to bring the mould to a proper temperature as to heat before I began casting; and when leather was put about them, or other bullets than those of lead were made use of, the weight was determined very exactly before they were put into the piece.

The diameter of the bullet was determined by measurement and also by computation from its weight, and the specific gravity of the metal of which it was formed; and both these methods gave the same dimensions very nearly.

The apparatus was put up for making the experiments in a coach-house, which was found very convenient for the purpose,

as the joists upon which the floor over head was laid afforded a firm and commodious support for suspending the pendulum and the barrel, and the walls and roofs of the building served to screen the apparatus, which otherwise might have been discomposed by the wind, and injured by the rain and dews. A pair of very large doors, which formed the whole of one end of the room, were kept constantly open during the time the experiments were making, in order to preserve the purity of the air within the house, which otherwise would have been much injured by the smoke of the gun-powder; and that, in all probability, would have had a considerable effect in lessening the force of the powder, and vitiating the experiments. In order still further to guard against this evil, the barrel was placed as near as possible to the door, and the pendulum was hung up at the bottom of the room.

Fig. 12. represents the apparatus as it was put up for making the experiments.

a, *b*, is the barrel with its carriage, suspended by the pendulous rods *c*, *d*, and

R, is the ribbon which served to measure the ascending arc of its recoil.

P, is the pendulum, and

r, the ribbon that measured the arc of its vibration.

The distance from the mouth of the piece to the pendulum was just 12 feet.

A table

A table shewing the weights and dimensions of all the principal parts of the apparatus.

Of the barrel.

	Inches.
Length	44,7
Length of the bore from the muzzle to the breech-pin	43,45
Diameter of the bore	0,78
Thickness of metal at the lower vent	0,36
Thickness of metal at the muzzle	0,1

Weight of the barrel, together with the solid breech-pin, and the vent-screws and vent-tube, 6 lbs. 6 oz.

Of the gun carriage.

Length	28,4
Distance between the two pair of trunnions	25,6
Diameter of each trunnion	0,25

Weight 40 lbs. 14 oz.

Of the rods by which the carriage was suspended.

Length from the axis of suspension, or center of the pivots, to the center of the trunnions of the gun carriage, 64 inches.

Weight of each rod, 1 lb. 4 oz.

Total weight of the barrel and its carriage, together with the allowance that was made for the weight of the rods by which it was suspended, 48 lbs.

N. B. This was its weight from experiment N° 3. to experiment N° 123. inclusive.

Of

Of the bullet.

Diameter 0,75 of an inch.
Weight in lead 580 grains.

Of the pendulum.

	Inches.
Total length of the pendulum from the axis of suspension to the bottom of the circular plate	69,5
Diameter of the circular plate to which the targets were fastened	13,-
Distance between the shoulders of the pivots	3,8
Diameter of the pivots	,27

Weight of the iron part of the pendulum 47 lb. 4 oz.

Of the pendulum with the targets fixed to it, as it was prepared for making the experiments, and numbered.

	Total length to the ribbon.	Distance from the axis of suspension.		Total weight of iron and wood.
		To the center of gravity.	To the center of oscillation.	
	Inches.	Inches.	Inches.	lbs. oz.
Pendulum N° 1	69,25	50,25	58,45	57 0
——— N° 2	69,5	54,4	59,15	82 4
——— N° 3	——	55,62	60,23	100 12
——— N° 4	——	54,6	59,18	88 4

N. B. The measure is English feet and inches, and the weight is avoirdupois.

Having

Having now gone through the description of all the principal parts of the apparatus, I shall proceed to give an account of the experiments. And as it may be satisfactory to the Society to see the method of conducting these enquiries, as well as the result of them, I shall first give a table of the experiments in the exact order in which they were made, together with my original remarks; I shall then make such general observations as may occur: and afterwards I shall select, combine, and compare them, in the manner which best answers the different purposes to which I shall apply them.

General

General table of the experiments.

In the two first experiments the barrel was fixed to a carriage (that has not been described) which, together with the barrel and rods by which it was suspended, weighed only 23½.

Length of the bore of the piece 43,5 inches.

Weight of the bullet 580 grains.

The pendulum, N° 1.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	The bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
	Grs.	In.	In.	Inches.	Inches.	In.	Ft.in Sec.	
N° 1	208	1,8	0,	13,2	64,5	33,5	1267	First day.
2	. .	. .	,5	14,5	. . .	36,5	1399	

This gun carriage being found to be too light, the other, described, and represented fig. 8. was substituted in the room of it.

Order of the experiments.	The charge of powder. Weight.	Height.	Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	The bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in sec.	
N° 3	208	1,8	0	12,6	65,	17,8	1213	Second day.
4	. .	. .	,5	——	——	18,5	—	The pendulum gave way.
5	. .	. .	0	——	——	38,68	—	4 bullets were fired at once.
6	. .	. .	,5	——	——	38,48	—	Ditto.
7	. .	. .	0	——	——	6,1	—	Without any bullet.
8	416	3,6	. .	——	——	16,5	—	Ditto.
9	208	1,8	0	8,5	65,	17,69	1281	Pen. N° 2; very fair; 3d day.
10	104	,9	. .	5,2	65,25	10,18	782	
11	310	2,7	0	9,6	64,6	24,69	1459	The powder was lighted by the long vent-tube (fig. 4.).
12	. .	. .	1,22	10,1	65,	24,95	1527	
13	. .	. .	2,65	11,85	64,75	24,9	1801	
14	. .	. .	. .	10,9	65,25	. . .	1646	
15	330	2,9	2,65	10,9	61,5	26,2	1748	
16	. .	. .	. .	13,25	63,5	. . .	2060	
17	330	2,7	2,65	. . .	. . .	12,7	. .	The barrel very much heated.
18	. .	2,9	0	10,4	63,5	26,3	1619	
19	. .	. .	. .	. . .	63,	26,4	1633	
20	165	1,45	0	6,8	62,2	14,73	1084	
21	. .	. .	. .	6,85	. . .	14,2	1093	
22	. .	. .	1,32	6,7	. . .	14,8	1071	
23	. .	. .	. .	6,3	60,6	14,58	1035	The short vent-tube (*v*, fig. 3.) was made use of.
24	. .	. .	. .	7,5	61,5	14,68	1142	

In order to determine how much of the force of the powder was lost by windage and by the vent, oiled leather was fastened round the bullet, so that it now accurately fitted the bore of the piece; and in the five experiments, from N° 35. to N° 39. inclusive, the valve-vent was made use of.

Weight of the bullet, together with the leather in which it was enveloped, 603 grains.

Order

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	The bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
N°	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft.in sec.	
25	165	1,45	0	6,8	65,	14,95	1004	Fourth day.
26	. .	. .	. .	7,8	. . .	15,6	1153	
27	. .	. .	. .	8,05	. . .	16,15	1192	
28	330	2,9	. .	10,2	63,	26,	1559	
29	. .	. .	2,6	. . .	64,	28,1	1536	
30	165	3,2	. .	5,9	62,4	13,2	914	
31	. .	1,45	1,3	6,65	62,6	15,15	1027	

Finding that the blaſt of the powder always reached as far as the pendulum, when large charges were made uſe of, and ſuſpecting that this circumſtance, together with the impulſe of the unfired grains, might in a great meaſure occaſion the apparent irregularity in the velocities of the bullets; to remedy theſe inconveniences, a large ſheet of paper of a moderate thickneſs was ſtretched upon a ſquare frame of wood, and interpoſed as a ſcreen before the pendulum at the diſtance of two feet from the ſurface of the target.

Two reaſons conſpired to induce me to prefer this method of preventing the impulſe of the flame upon the pendulum to the obvious one of removing the pendulum further from the mouth of the piece; the firſt was, that I was unwilling to increaſe the diſtance between the barrel and the pendulum, leſt the reſiſtance of the air might affect the velocities of the bullets; and the ſecond, which I confeſs did not operate leſs ſtrongly than the firſt, was, that the length of the houſe did not admit of a greater

diſtance, and I was unwilling to expoſe any part of the apparatus in the open air.

But the ſcreen was found to anſwer perfectly well the purpoſe for which it was deſigned, and it was continued during the remainder of the experiments, the paper being replaced every third or fourth experiment.

The experiments continued.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
N°	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft.in ſec.	
32	165	1,45	0	5,45	63,	15,45	839	Not leathered; weight of the bullet and wad 603 grs. In exp. N° 32. no leſs than 40 large grs. of unfired powder were driven through the ſcreen.
33	. .	. .	. .	. . .	. . .	12,65	839	
34	. .	. .	. .	7,9	. . .	15,45	1217	In theſe 6 experiments the bullets were leathered, and the powder was lighted by the valve-vent.
35	. .	. .	. .	7,	60,25	15,25	1129	
36	. .	. .	. .	7,4	62,	16,3	1161	
37	. .	. .	1,3	8,	61,	17,9	1277	
38	290	2,6	2,6	9,	58,6	23,5	1497	
39	. .	. .	. .	. . .	. . .	24 8	. .	The pend. N° 2. ruined.

The bullets were now put naked into the piece, and the powder was lighted by the ſhort vent-tube (*v*, fig. 3) and ſome little improvement was made in the ſteel edges between which the ribbons paſſed that ſerved to meaſure the aſcending arcs of the pendulum and of the recoil, by which means the friction was leſſened, and the ribbon was prevented from twiſting or entangling itſelf as it was drawn out.

3. *Apparatus*

Apparatus.

The barrel with its carriage as before.
The pendulum, N° 3. and
Leaden bullets, weighing 580 grains each.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	Bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in sec.	
N°								
40	218	1,9	0	6,45	64,6	18,	1236	5th day; medium velocity in these experiments and N° 47. 1225.
41	. .	. .	. .	6,31	65,3	17,71	1197	
42	. .	. .	. .	6,45	65,	17,91	1230	
43	. .	. .	1,3	6,5	64,6	18,3	1248	Medium velocity 1276
44	. .	. .	. .	6,75	64,5	18,35	1299	
45	. .	. .	. .	6,6	64,9	. . .	1265	
46	. .	. .	. .	6,4	61,6	. . .	1293	
47	. .	. .	0	6,3	62,	18,1	1266	
48	290	2,6	0	7,2	63,5	22,58	1414	Medium velocity 1427.
49	. .	. .	. .	7,4	. . .	22,92	1455	
50	. .	. .	. .	7,3	64,6	22,38	1412	
51	290	2,6	1,3	7,4	63,	23,21	1476	Medium velocity 1493.
52	. .	. .	. .	7,6	64,	23,76	1520	
53	. .	. .	. .	7,25	61,	23,6	1483	
54	. .	. .	2,6	7,5	62,3	. . .	1502	Medium velocity 1460.
55	. .	. .	. .	7,4	64,	23,26	1450	
56	. .	. .	. .	7,1	62,2	. . .	1433	
57	. .	. .	. .	7,4	64,	23 56	1454	
58	. .	. .	. .	1,31		11,12	—	In these 4 experiments the piece was fired with powder alone, and the screen was taken away from before the pendulum.
59	. .	. .	. .	1,2		11,62	—	
60	. .	. .	0	1,16		9,62	—	
61	. .	. .	1,3	0,6		11,33	—	

Order

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	Bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in sec.	
N°								
62	330	2,9	1,3	8,	63,	26,4	1599	6th day; medium velocity 1625.
63	. .	. .	. .	8,5	65,	. . .	1652	
64	. .	. .	2,6	7,2	59,5	25,3	1562	Medium velocity 1528.
65	. .	. .	. .	7,7	65,	. . .	1495	
66	. .	. .	0	8,4	. . .	26,35	1633	Medium velocity 1594.
67	. .	. .	. .	8,	. . .	25,8	1556	
68	218	1,9	0	6,82	64,	19,56	1349	The powder was rammed very hard.
69	. .	. .	. .	6,6	64,6	18,2	1294	Ditto much harder.
70	. .	. .	. .	6,85	. . .	19,12	1345	Ditto as hard as in N° 68.
71	. .	. .	1,3	5,5	. . .	16,33	1080	Ditto, ditto.
72	. .	. .	0	—	—	8,72	—	Government powder, no bullet.
73	. .	. .	. .	—	—	8,44	—	Best double battle powder.
74	. .	. .	1,3	—	—	8,47	—	Government powder.
75	. .	. .	. .	—	—	9,3	—	Double battle powder.

The following experiments N° 78, 79, 80, and 81. were made in hopes of being able to discover a method of adding to the force of gun-powder. Twenty grains of the substances mentioned in the remarks upon each experiment were intimately mixed with the powder of the charge. In the experiment N° 82. a large wad of tow, well soaked in etherial spirit of turpentine, was put into the piece immediately upon the bullet: and in the experiment N° 83. a wad, soaked in alkohol, was put into the piece in like manner.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	Bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in sec.	
N°								
76	145	1,3	0	5,3	65,	13,25	1037	7th day; medium velocity 1040.
77	. .	. .	. .	. . .	64,6	13,25	1044	
78	. .	. .	. .	3,2	. . .	8 92		20 grs. best alkaline salt of tartar.
79	. .	. .	. .	4,35	. . .	11,68		20 grs. æthiops mineral.
80	. .	. .	. .	3,3	63,6	9,83		20 grs. sal ammon.
81	. .	. .	. .	4,2	63,4	11,45		20 grs. fine brass dust.
82	. .	. .	. .	——	——	15,25	—	The screws which held the hooks by which the pendulum was suspended gave way, and the pendulum came down.
83	. .	. .	. .	——	——	14,35	—	

In the nine following experiments, *viz.* from N° 84. to N° 92. inclusive, the valve-vent was made use of, and the bullets were made to fit the bore of the piece very exactly by means of oiled leather, which was so firmly fastened about them that in each experiment it entered the target with the bullet.

The bullet made use of in experiment N° 85. was of wood.

Those used in the experiments N° 86. and N° 87. were formed in the following manner; a small bullet was cast of plaister of Paris, which being thoroughly dried, and well heated at the fire, was fixed in the center of the mould that served for casting all the leaden bullets made use of in these experiments; and melted lead being poured into this mould, the cavity that surrounded the small plaister bullet was intirely filled up, and a bullet was produced, which to the eye had every appearance of solidity, but was as much lighter than a solid leaden bullet of the

the ſame diameter as the plaiſter bullet was lighter than a leaden bullet of the ſame ſize.

In the experiments N° 88. and N° 89. ſolid leaden bullets were made uſe of. In the experiment N° 90. two bullets were diſcharged at once; in the experiment N° 91. three; and in the experiment N° 92. four were uſed.

In each of theſe experiments a freſh ſheet of paper was made uſe of as a ſcreen to the pendulum, in order that the velocities of the bullets might be meaſured more accurately; and alſo, that the quantity of unfired powder might be eſtimated with greater preciſion.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Weight of the bullet.	Chord of the aſcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.							
	Grs.	In.	In.	Grs.	Inches.	Inches.	Inches.	Ft. in ſec.	
N°									
84	145	1,3	0	—	—	—	4,5	—	8th day; in each of theſe 4 experiments from 50 to 70 granulæ or particles of unfired powder were driven through the ſcreen.
85	. .	. .	. .	90	1,33	62,2	7,16	1763	
86	. .	. .	. .	251	2,82	63,2	9,62	1317	
87	. .	. .	. .	354	3,32	61,2	11,3	1136	
88	. .	. .	. .	600	6,5	65,4	15,22	1229	Very few unfired grains of powder ſtruck the ſcreen.
89	. .	. .	. .	603	6,3	64,6	15,13	1229	
90	. .	. .	. .	1184	10,12	65,	21,92	978	There were no marks of any unfired powder having reached the ſcreen.
91	. .	. .	. .	1754	13,65	63,4	27,18	916	
92	. .	. .	. .	2352	16,55	63,3	32,25	833	

In

In the ſeven following experiments the piece was fired with powder only.

Order of the experiments.	The charge of powder. Weight	Height.	Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	Chord of the arc of the recoil.	Remarks.
	Grs.	In.	In.	Inches.	Inches.	
N°						
93	145	1,3	0	—	4,3	
94	165	1,45	. .	—	5,5	
95	. .	. .	. .	—	5,6	
96	290	2,6	. .	—	11,70	
97	437½	3,9	. .	1,68	17,5	The ſcreen was taken away.
98	. .	. .	. .	6,7	15,88	The whole ſurface of the target was beſpattered with unfired grains of powder.
99	. .	. .	. .	—	17,9	The pendulum was not obſerved.

In the following experiments N° 100. and N° 101. the bullets were not put down into the bore, but were ſupported by three wires, which being faſtened to the end of the barrel projected beyond it, and confined the bullet in ſuch a ſituation that its center was in a line with the axis of the bore, and its hinder part was one-twentieth of an inch without or beyond the mouth of the piece.

In experiment N° 102. the bullet was juſt ſtuck into the barrel in ſuch a manner that near one-half of it was without the bore.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	Bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in sec.	
N°								
100	165	1,45	0	,65	60,5	4,9	138	In each of these experim near $\frac{1}{10}$th part of the substance of the bullet was melted and blown away by the impulse of the flame.
101	. .	. .	. .	,43	uncertain.	4,8	92	
102	. .	. .	. .	,80	63,	5,6	180	

All that part of the bullet which lay towards the bore of the piece appeared to be quite flat from the loss of substance it had sustained; and its surface was full of small indents, which probably were occasioned by the unfired grains of powder that impinged against it.

The

The following experiments were made with the pendulum N° 4. The rest of the apparatus as before.

Order of the experiments	The charge of powder. Weight.	Height.	Vent from the bottom of the charge.	Chord of the ascending arc of the pendulum.	Bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	In.	In.	In.	Inches.	Inches.	Inches.	Ft. in sec.	
								9th day.
N° 103	104	,9	0	4,51	65,	10,6	732	About 40 grains of powder were driven through the screen.
104	145	1,3	. .	5.4	. . .	12,92	877	About 40 unfired grs. of powder. Medium velocity 894.
105	. .	. .	. .	5,6	. . .	13,28	910	40 unfired grains.
106	. .	1,14	. .	6,18	65,8	14,3	990	Double proof battle powder; no unfired grains.
107	218	1,8	. .	8,48	65,	19,68	1380	Ditto, ditto.
108	290	2,6	. .	9,45	65 6	23,9	1526	Government powder; bullet leathered; weight 602 grains.
109	. .	. .	. .	8,73	65,2	22,8	1419	Bullet naked; very few unfired grains.
110	. .	. .	. .	9 3	65,6	23,4	1460	Medium velocity 1444.
111	. .	. .	. .	. . .	. . .	. . .	1462	
112	. .	. .	. .	8,85	65,5	22,94	1436	
113	. .	. .	2,6	8,65	64,	23,7	1438	Medium velocity 1413.
114	. .	. .	. .	8,5	63,6	24,1	1423	
115	. .	. .	. .	8.4	65,	23,8	1378	
116	. .	2,28	. .	9,15	64,	24,6	1525	Double proof battle powder.
117	437½	3,9	. .	10,56	64,9	33,	1738	Gov. pow. No unfired grains thro' the screen.
118	. .	. .	. .	11,	64,5	33,3	1824	Medium velocity 1764.
119	. .	. .	. .	10,5	65,	33,6	1729	
120	. .	. .	2,6	10,35	. . .	32,5	1706	Medium velocity 1751.
121	. .	. .	. .	10,65	. . .	33,2	1757	
122	. .	. .	. .	10,6	63,6	32,9	1789	
123	. .	. .	0	——	——	17,9	—	Without any bullet.

Of the method made use of for computing the velocities of the bullets.

As the method of computing the velocity of a bullet from the arc of the vibration of a pendulum into which it is fired is so well known, I shall not enlarge upon it in this place, but shall just give the theorems that have been proposed by different authors, and shall refer those who wish to see more on the subject to Mr. ROBINS's New Principles of Gunnery; to Professor EULER's Observations upon Mr. ROBINS's Book; and, lastly, to Dr. HUTTON's Paper on the initial Velocities of Cannon Balls, which is published in the Transactions of the Society for the year 1778.

If a denote the length from the axis of the pendulum to the ribbon which measures the chord of the arc of its vibration;

g, the distance of the center of gravity below the axis;
f, the distance of the center of oscillation;
h, the distance of the point struck by the bullet;
c, the chord of the ascending arc of the pendulum;
P, the weight of the pendulum;
b, the weight of the bullet, and
v, the original velocity of the bullet;

$v = \frac{c}{a} \times \overline{\frac{Pg}{bh}} + \overline{\frac{b}{f}} \times \frac{f}{\sqrt{2h}}$, is a theorem for finding the velocity upon Mr. ROBINS's principles.

*$v = \frac{c}{a} \times \overline{\frac{Pg}{bh}} + \overline{\frac{f+h}{2f}} \times \sqrt{\frac{f}{2}}$, is the theorem proposed by Professor EULER, who has corrected a small error in Mr. ROBINS's method; and

* Put the rational part $\frac{c}{a} \times \overline{\frac{Pg}{bh}} + \overline{\frac{f+h}{2f}} = n$, and express f in the thousandth parts of a Rhynland foot; then the velocity with which the ball strikes the pendulum will be $= \frac{n}{4}\sqrt{\frac{f}{2}}$ Rhynland feet in a second.

$v =$

$v = 5{,}672\, cg\sqrt{f} \times \frac{P+b}{bha}$ is Dr. HUTTON's theorem, which is sufficiently accurate, and far more simple and expeditious than either of the preceding. It is to be remembered, that g, h, and c, may be expressed in any measure; but f must be English feet, and v will be the velocity of the bullet in English feet in a second.

The velocities of the bullets in most of the foregoing experiments were first computed by EULER's method, as I had not then seen Dr. HUTTON's paper; but in going over the calculations a second time, I made use of Dr. HUTTON's theorem. Both these methods gave the same velocity very nearly, but the Doctor's method is by much the easiest in practice.

In these computations care was taken to make a proper allowance for the bullets that were lodged in the pendulum, and also for the velocity lost by the bullet in passing through the screen.

The corrections necessary on account of the bullets lodged in the pendulum were made in the following manner.

b was continually added to the value of P,

$\frac{b-g}{P} \times b$ . . . to the value of g, and

$\frac{f-h}{P} \times b$ . . . to the value of f.

Of the spaces occupied by the different charges of powder.

The heights of the charges of powder, or the lengths of the spaces which they occupied in the bore, were determined by measurement; and in order that this might be done with greater accuracy, inches and tenths of inches were marked upon the ram-rod, and the charge was gently forced down till it occupied the same space in each experiment.

The following table shews the heights of the charges as they were determined by measurement, and also their heights computed

puted from the diameter of the bore of the piece, and the specific gravity of the powder that was made use of.

N. B. By an experiment I shall give an account of hereafter, I found the specific gravity of this powder shaken well together to be to that of rain water as 0,937 is to 1,000.

Weight of the powder.	Height of the charge.	
	Measured.	Computed.
Grs.	Inches.	Inches.
104	,9	0,8957
145	1,3	1,2490
165	1,45	1,4211
208	1,8	1,7914
218	1,9	1,8775
290	2,6	2,4980
310	2,7	2,6700
330	2,9	2,8422
416	3,6	3,5828
437½	3,9	3,7680

In the experiment N° 30. the powder was put into a cartridge so much smaller than the bore of the piece, that the charge, instead of occupying 1,45 inches, extended 3,2 inches. By this disposition of the powder, its action upon the bullet appears to have been very much diminished.

Of the effect that the heat which pieces acquire in firing produces upon the force of powder.

It is very probable, that the excess of the velocity of the bullet in the second experiment over that of the first was occasioned more by the heat the barrel had acquired in the first experiment than by the position of the vent, or any other circumstance; for I have since found, upon repeated trials, that the force of any given charge of powder is considerably greater when it is fired in a piece that has been previously heated by firing, or by any other means, than when the piece has not been heated. Every body that is acquainted with artillery knows, that the recoil of great guns is much more violent after the second or third discharge than it is at first; and on ship-board, where

where it is necessary to attend to the recoil of the guns, in order to prevent very dangerous accidents that might be occasioned by it, the constant practice has been in our navy, and, I believe, on board the ships of all other nations, to lessen the quantity of powder after the first four or five rounds: our 32 pounders, for instance, are commonly fired with 14 lbs. of powder at the beginning of an action, but the charge is very soon reduced to 11 lbs. and afterwards to 9 lbs. and the filled cartridges are prepared accordingly.

By the recoil it should seem, that the powder exerted a greater force also in the fourth experiment, being the second upon the second day, than it did upon the third, or the first upon that day; but the pendulum giving way, it was not possible to compare the velocities of the bullets in the manner we did in the two experiments mentioned above.

This augmentation of the force of powder, when it is fired in a piece that is warm, may be accounted for in the following manner. There is no substance we are acquainted with that does not require to be heated before it will burn; even gunpowder is not inflammable when it is cold. Great numbers of sparks or red-hot particles from the flint and steel are frequently seen to light upon the priming of a musket, without setting fire to the powder, and grains of powder may be made to pass through the flame of a candle without taking the fire; and what is still more extraordinary, if large grains of powder are let fall from the height of two or three feet upon a red-hot plate of iron, laid at an angle of about 45° with the plane of the horizon, they will rebound intire without being burnt, or in the least altered, by the experiment. In all these cases the fire is too feeble, or the duration of its action is not sufficiently

long

long to heat the powder to that degree which is necessary in order to its being rendered inflammable.

Now as gun-powder, as well as all other bodies, acquires heat by degrees, and as some space of time is taken up in this as well as in all other operations, it follows, that powder, which has been warmed by being put into a piece made hot by repeated firing, is much nearer that state in which it will burn, or, I may say, is more inflammable than powder which is cold; consequently, more of it will take fire in a given short space of time, and its action upon the bullet and upon the gun will of course be greater.

The heat of the piece will also serve to dry the air in the bore, and to clear the inside of the gun of the moisture that collects there when it has not been fired for some time, and these circumstances doubtless contribute something to the quickness of the inflammation of the powder, and consequently to its force.

As it takes a longer time to heat a large body than a small one, it follows, that meal-powder is more inflammable than that which is grained; and the smaller the particles are, the quicker they will take fire. The sailors bruise the priming after they have put it to their guns, as they find it very difficult, without this precaution, to fire them off with a match: and if those who are fond of sporting would make use of a similar artifice, and prime their pieces with meal-powder, they would miss fire less often, the springs of the lock might be made more tender, and its size considerably reduced without any risque, and the violence of the blow of the flint and steel in striking fire being lessened, the piece might be fired with greater precision.

Concluding from the result of the four experiments mentioned above, as well as from the reasons just cited, that the temperature of the piece has a considerable effect upon the force of the powder, I afterwards took care to bring the barrel to a proper degree of heat, by firing it once or oftener with powder each time I recommenced the experiments after the piece had been left to cool.

Of the manner in which pieces acquire heat in firing.

I was much surprised upon taking hold of the barrel immediately after the experiment N° 17. when it was fired with 330 grains of powder without any bullet, to find it so very hot that I could scarcely bear it in my hand; evidently much hotter than I had ever observed it before, notwithstanding the same charge of powder had been made use of in the two preceding experiments, and in both these experiments the piece was loaded with a bullet, which one would naturally imagine, by confining the flame, and prolonging the time of its action, would heat the barrel much more than when it was fired with powder alone.

I was convinced that I could not be mistaken in the fact, for it had been my constant practice to take hold of the piece to wipe it out as soon as an experiment was finished, and I never before had found any inconvenience from the heat in holding it. But in order to put the matter beyond all doubt, after letting the barrel cool down to the proper temperature, I repeated the experiment twice with the same charge of powder and a bullet; and in both these trials (experiments N° 18. and N° 19.) the heat of the piece was evidently much less than what it was in the experiment above mentioned (N° 17.).

I now regretted exceedingly the loſs of a ſmall pocket thermometer, which I had provided on purpoſe to meaſure the heat of the barrel, but it was accidentally broken by a fall the day before I began my experiments; and being ſo far from London, I had it not in my power to procure another: I was therefore obliged to content myſelf with determining the heat of the piece as well as I could by the touch.

Being much ſtruck with this accidental diſcovery of the great degree of heat that pieces acquire when they are fired with powder without any bullet, and being deſirous of finding out whether it is a circumſtance that obtains univerſally, I was very attentive to the heat of the barrel after each of the ſucceeding experiments; and I conſtantly found the heat ſenſibly greater when the piece was fired with powder only, than when the ſame charge was made to impel one or more bullets.

Though the reſult of theſe experiments was totally unexpected, and even contrary to what I ſhould have foretold if I had been aſked an opinion upon the ſubject previous to making them; yet, after mature conſideration, I am now convinced, that it is what ought to happen, and that it may be accounted for very well upon principles that are clearly admiſſible.

It is certain, that a very ſmall part only of the heat that a piece of ordnance acquires in being fired is communicated to it by the flame of the powder; for the time of its action is ſo ſhort (not being, perhaps, in general longer than about $\frac{1}{100}$th or $\frac{1}{130}$th part of a ſecond) that if its heat, inſtead of being 4 times, as Mr. ROBINS ſuppoſes, was 400 times hotter than red-hot iron, it could not ſenſibly warm ſo large a body of metal as goes to form one of our large pieces of cannon. And beſides, if the heat of the flame was ſufficiently intenſe to produce ſo great an effect in ſo ſhort a time, it would certainly be

ſufficient to burn up all inflammable bodies that it came near, and to melt the ſhot that it ſurrounded and impelled, eſpecially when they were ſmall, and were compoſed of lead or any other ſoft metal; but, on the contrary, we frequently ſee the fineſt paper come out of the mouth of a piece uninflamed, after it has ſuſtained the action of the fire through the whole length of the bore, and the ſmalleſt lead ſhot is diſcharged without being melted.

But it may be objected here, that bullets are always found to be very hot if they are taken up immediately after they come out of a gun; and that this circumſtance is a proof of the intenſity of the heat of the flame of powder, and of its great power of communicating heat to the denſeſt bodies. But to this I anſwer, I have always obſerved the ſame thing of bullets diſcharged from wind-guns and croſs-bows, eſpecially when they have impinged againſt any hard body, and are much flattened; and bullets from muſkets are always found to be hotter in proportion to the hardneſs of the body againſt which they are fired. If a muſket ball is fired into any very ſoft body, as (for inſtance) into water, it will not be found to be ſenſibly warmed; but if it is fired againſt a thick plate of iron, or any other body that it cannot penetrate, the bullet will be demoliſhed by the blow, and the pieces of it that are diſperſed about will be found to be in a ſtate very little ſhort of fuſion, as I have often found by experience. It is not by the flame therefore that bullets are heated, but by percuſſion. They may, indeed, receive ſome ſmall degree of warmth from the flame, and ſtill more perhaps by friction againſt the ſides of the bore, but it is in ſtriking againſt hard bodies, and from the reſiſtance they meet with in penetrating thoſe that are ſofter, that they acquire by far

the greater part of the heat we find in them as soon as they come to be at rest, after having been discharged from a gun.

There is another circumstance that may possibly be brought as an objection to this opinion, and that is the running of the metal in brass guns upon repeatedly firing them, by which means the vent is often so far enlarged as to render the piece intirely useless. But this, I think, proves nothing but that brass is very easily corroded, and destroyed by the flame of gun-powder; for it cannot be supposed, that in these cases the metal is ever fairly melted. The vent of a musket is very soon enlarged by firing, and after a long course of service it is found necessary to stop it up with a solid screw, through the center of which a new vent is made of the proper dimensions. This operation is called bushing, or rather bouching the piece; but in all the better kind of fowling-pieces the vent is lined, or bouched, with gold, and they are found to stand fire for any length of time without receiving the least injury. But every body knows that gold will run with a less heat than is required to melt iron: but gold is not corroded either by the spirit of nitre, or the acid spirit that is generated from sulphur, whereas iron is very easily destroyed by either; and that I take to be the only reason why a vent that is lined with gold is so much more durable than one that is made in iron. But it seems, that iron is more durable than brass; and perhaps steel, or some other cheap metal, may be found that will supply the place of gold, and by that means the great expence that attends bouching pieces with that precious metal may be spared, and this improvement may be introduced into common use.

This leads us to a very easy and effectual remedy for that defect so long complained of in all kinds of brass ordnance, *the running of the vent*; for if these pieces were bouched with iron,

iron, there is no doubt but they would ſtand fire as well as iron guns; and if ſteel, or any other metal, either ſimple or compounded, ſhould upon trial be found to anſwer for that purpoſe better than iron, it might be uſed inſtead of it; and even if gold was made uſe of for lining the vent, I imagine it might be done in ſuch a manner as that the expence would not be very conſiderable, at the ſame time that the thickneſs of the gold ſhould be ſufficient to withſtand the force of the flame for a very great length of time.

But to return to the heat acquired by guns in firing. It being pretty evident that it is not all communicated by the flame, there is but one other cauſe to which it can be attributed, and that is the motion and friction of the internal parts of the metal among themſelves, occaſioned by the ſudden and violent effort of the powder upon the inſide of the bore, and to this cauſe I imagine the heat is principally if not almoſt intirely owing. It is well known, that a very great degree of heat may be generated in any hard and denſe body in a ſhort ſpace of time by friction, and in a ſtill ſhorter time by colliſion. " For if two denſe hard elaſtic bodies be ſtruck againſt " each other with great force and velocity, all the parts of " ſuch bodies will every moment be cloſely compreſſed, and " being rigid will re-act with equal force. Hence a quick and " powerful contraction and expanſion will ariſe in every part, " reſembling that ſwift kind of vibrations obſerved in ſtretched " ſtrings; how great theſe vibrations are may be learnt from " the inſtance of a bell, when ſtruck with a ſingle blow, by " which the whole bulk, however vaſt, will for a long time " expand, and contract itſelf in infinite ellipſes. And when " the attrition above deſcribed is produced, with what force and " velocity are all the particles of the rubbed body compreſſed,

"ſhaken, and looſened to their very intimate ſubſtance*?" And in proportion to the ſwiftneſs of this vibration, and the violence of the attrition and friction, will be the heat that is produced.

A piece of iron that would ſuſtain the preſſure of any weight, however large, without being warmed, may be made quite hot by the blow of a hammer; and even ſoft and un-elaſtic bodies may be warmed by percuſſion, provided the velocity with which their parts are made to give way to the blow is ſufficiently rapid. If a leaden bullet is laid upon an anvil, or any other hard body, and in that ſituation it is ſtruck with a ſmart blow of a hammer, it will be found to be much heated; but the ſame bullet in the ſame ſituation may be much more flattened by preſſure, or by the ſtroke of a very heavy body moving with a ſmall velocity, without being ſenſibly warmed.

To generate heat therefore the action of the powder upon the inſide of the piece muſt not only be ſufficient to ſtrain the metal, and produce a motion in its parts, but this effect muſt be extremely rapid; and the heat will be much augmented, if the exertion of the force and the duration of its action are momentaneous; for in that caſe, the fibres of the metal (if I may uſe the expreſſion) that are violently ſtretched, will return with their full force and velocity, and the ſwift vibratory motion and attrition before deſcribed will be produced.

The heat generated in a piece by firing is therefore as the force by which the particles of the metal are ſtrained and compreſſed, the ſuddenneſs with which this force is exerted, and the ſhortneſs of the time of its action; that is to ſay, as the ſtrength of the powder and the quantity of the charge, the quickneſs of its inflammation, and the velocity with which the generated fluid makes its eſcape.

* Vide SHAW's tranſlation of BOERHAAVE's Chemiſtry, vol. I. p. 249.

Now

Now the effort of any given charge of powder upon the gun is very nearly the ſame, whether it be fired with a bullet or without; but the velocity with which the generated elaſtic fluid makes its eſcape, is much greater when the powder is fired alone, than when it is made to impel one or more bullets; the heat ought therefore to be greater in the former caſe than in the latter, as I found by experiment.

But to make this matter ſtill plainer, we will ſuppoſe any given quantity of powder to be confined in a ſpace that is juſt capable of containing it, and that in this ſituation it is by any means ſet on fire. Let us ſuppoſe this ſpace to be the chamber of a piece of ordnance of any kind, and that a bullet, or any other ſolid body, is ſo firmly fixed in the bore immediately upon the charge, that the whole effort of the powder ſhall not be able to remove it. As the powder goes on to be inflamed, and the elaſtic fluid is generated, the preſſure upon the inſide of the chamber will be increaſed, till at length all the powder being burnt, the ſtrain upon the metal will be at its greateſt height, and in this ſituation things will remain, the coheſion or elaſticity of the particles of metal counterbalancing the preſſure of the fluid.

Under theſe circumſtances very little heat would be generated; for the continued effort of the elaſtic fluid would approach to the nature of the preſſure of a weight; and that concuſſion, vibration, and friction, among the particles of the metal, which in the colliſion of elaſtic bodies is the cauſe of the heat that is produced, would ſcarcely take effect.

But inſtead of being firmly fixed in its place, let the bullet now be moveable, but let it give way with great difficulty, and by ſlow degrees. In this caſe, the elaſtic fluid will be generated as before, and will exert its whole force upon the chamber

of

of the piece; but as the bullet gives way to the preſſure, and moves on in the bore, the fluid will expand itſelf and grow weaker, and the particles of the metal will gradually return to their former ſituations; but the velocity with which the metal reſtores itſelf being but ſmall, the vibration that remains in the metal, after the elaſtic fluid has made its eſcape, will be very languid, as will be the heat that is generated by it.

But if, inſtead of giving way with ſo much difficulty, the bullet is much lighter, ſo as to afford but little reſiſtance to the elaſtic fluid in making its eſcape, or if the powder is fired without any bullet at all; then, there being little or nothing to oppoſe the flame in its paſſage through the bore, it will expand itſelf with an amazing velocity, and its action upon the gun will ceaſe almoſt in an inſtant, the ſtrained metal will reſtore itſelf with a very rapid motion, and a ſharp vibration will enſue, by which the piece will be much heated.

Of the effect of ramming the powder in the chamber of the piece.

The charge, conſiſting of 218 grains of powder, being put gently into the bore of the piece in a cartridge of very fine paper, without being rammed, the velocity of the bullets at a mean of the 40th, 41ſt, 42d, and 47th experiments, was at the rate of 1225 feet in a ſecond; but in the 68th, 69th, and 70th experiments, when the ſame quantity of powder was rammed down with five or ſix hard ſtrokes of the ram-rod, the mean velocity was 1329 feet in a ſecond. Now the total force or preſſure exerted by the charge upon the bullet is as the ſquare of its velocity, and $\overline{1329}^2$ is to $\overline{1225}^2$ as 1,1776 is to 1; or nearly

nearly as 6 is to 5; and in that proportion was the force of the given charge of powder increaſed by being rammed.

In the 71ſt experiment the powder was alſo rammed, but the vent, inſtead of being at the bottom of the bore, was at 1,3, and the velocity of the bullet was very conſiderably diminiſhed, being only at the rate of 1080 feet in a ſecond, inſtead of 1276 feet in a ſecond, which was the mean velocity with this charge, and with the vent in this ſituation when the powder was rammed. See the experiments N°. 43, 44, 45, and 46.

When, inſtead of ramming the powder, or preſſing it gently together in the bore, it is put into a ſpace larger than it is capable of filling, the force of the charge is thereby very ſenſibly leſſened, as Mr. ROBINS and others have found by repeated trials. In my 30th experiment the charge, conſiſting of no more than 165 grains of powder, was made to occupy 3,2 inches of the bore inſtead of 1,45 inches, which ſpace it juſt filled when it was gently puſhed into its place without being rammed; the conſequence was, the velocity of the bullet, inſtead of being 1100 feet in a ſecond or upwards, was only at the rate of 914 feet in a ſecond, and the recoil was leſſened in proportion.

And from hence we may draw this practical inference, that the powder, with which a piece of ordnance or a fire-arm is charged, ought always to be preſſed together in the bore; and if it is rammed to a certain degree, the velocity of the bullet will be ſtill farther increaſed. It is well known, that the recoil of a muſket is greater when its charge is rammed than when it is not; and there cannot be a ſtronger proof that ramming increaſes the force of the powder.

Of the relation of the velocities of bullets to the charges of powder by which they are impelled.

It appears by all the experiments that have hitherto been made upon the initial velocities of bullets, that when the weights and dimensions of the bullets are the same, and they are discharged from the same piece by different quantities of powder, the velocities are in the sub-duplicate ratio of the weights of the charges very nearly.

The following table will shew how accurately this law obtained in the foregoing experiments.

Charges.	Velocities Computed.	Velocities Actual.	Difference.	N° of exp.
437½	1764	1764	——	3
330	1533	1594	+ 61	2
310	1486	1459	− 27	1
290	1436	1436	0	7
218	1232	1225	− 7	4
208	1216	1256	+ 40	3
165	1083	1087	+ 4	2
145	1018	1040	+ 22	2
104	860	757	− 103	2

The computed velocities, as they are set down in this table, were determined from the ratio of the square root of 437½ (the weight in grains of the largest charge of powder) to the mean velocity of the bullet with that charge and the vent at 0; *viz.* 1764 feet in a second, and the square root of the other charges expressed in grains. And the *actual* velocities are means of all experiments that were made under similar circumstances.

 The

The fourth column shews the difference of the computed and actual velocities, or the number of feet in a second by which the actual velocity exceeds or falls short of the computed: and in the fifth column is set down the number of experiments with each charge, from the mean of which the actual velocity was determined.

The agreement of the computed and actual velocities will appear more striking, if we take the sum and difference of those velocities with all the charges except the first: thus,

Sum of the velocities, – 1764.			
Computed.	Actual.	Difference.	N° of exp.
9864	9854	– 10	23

So that it appears, that the difference, or the actual velocity, was smaller than the computed by $\frac{1}{983}$ part only at a mean of 23 experiments.

But as by far the greater number of the experiments were made with the following charges, *viz.* 290, 218, 208, 165, and 145 grains of powder, let us take the sum and difference of the computed and actual velocities of those charges: thus,

Sum of the velocities			
Computed.	Actual.	Difference.	N° of exp.
5985	6044	+ 59	18

Here the agreement of the theory with the experiments is so very remarkable, that we must suppose it was in some measure accidental; for the difference of the velocities in repeating the same experiment is in general much greater than the difference of the computed and actual velocities in this instance; but, I think, we may fairly conclude, from the result of all these

trials, that the velocities of like *musket* bullets, when they are discharged from the same piece by different quantities of the same kind of powder, are very nearly in the sub-duplicate ratio of the weights of the charges. Whether this law will hold good when applied to cannon balls, and bomb shells of large dimensions, I dare not at present take upon me to decide; but, for several reasons that might be mentioned, I am rather of opinion, that it will not; at least not with that degree of accuracy which obtained in these experiments.

Of the effect of placing the vent in different parts of the charge.

There have been two opinions with respect to the manner in which gun-powder takes fire. Mr. ROBINS supposes that the progress of its inflammation is so extremely rapid, "that all "the powder of the charge is fired and converted into an elastic "fluid, before the bullet is sensibly moved from its place;" while others have been of opinion, that the progress of the inflammation is much slower, and that the charge is seldom or never completely inflamed before the bullet is out of the gun.

The large quantities of powder that are frequently blown out of fire arms un-inflamed, seem to favour the opinion of the advocates for the gradual firing; but Mr. ROBINS endeavours to account for that circumstance upon different principles, and supports his opinion by shewing that every increase of the charge within the limits of practice produces a proportional increase of the velocity of the bullet, and that when the powder is confined by a great additional weight, by firing two or more bullets at a time instead of one, the velocity is not sensibly greater than it ought to be according to his theory.

If

If this were a queſtion merely ſpeculative, it might not be worth while to ſpend much time in the diſcuſſion of it; but as it is a matter upon the knowledge of which depends the determination of many important points reſpecting artillery, and from which many uſeful improvements may be derived, too much pains cannot be taken to come at the truth. Till the manner in which powder takes fire, and the velocity with which the inflammation is propagated, are known, nothing can with certainty be determined with reſpect to the beſt form for the chambers of pieces of ordnance, or the moſt advantageous ſituation for the vent; nor can the force of powder, or the ſtrength that is required in different parts of the gun, be aſcertained with any degree of preciſion.

As it would be eaſy to determine the beſt ſituation for the vent from the velocity of the inflammation of powder being known, ſo on the other hand I had hopes of being able to come at that velocity by determining the effect of placing the vent in different parts of the charge; for which purpoſe the following experiments were made.

A table

A table of experiments, shewing the effect of placing the vent in different parts of the charge.

Weight of the charge of powder.	Space occupied by the powder.	Vent from the bottom of the bore.	Velocity of the bullet at a medium.	Recoil measured upon the ribbon at a medium.	Number of experiments.
Grains.	Inches.	Inches.	Ft. in a sec.	Inches.	
165	1,45	0	1087	14,465	2
. . .	. . .	1,32	1082	14,31	3
218	1,9	0	1225	17,93	4
. . .	. . .	1,3	1276	18,34	4
290	2,6	0	1427	22,626	3
. . .	. . .	1,3	1493	23,34	3
. . .	. . .	2,6	1460	23,286	4
. . .	. . .	0	1444	23,135	4
. . .	. . .	2,6	1413	24,5	3
310	2,7	0	——	24,69	1
. . .	. . .	1,32	——	24,95	1
. . .	. . .	2,65	——	24,9	1
330	2,9	0	1594	26,075	2
. . .	. . .	1,3	1625	26,4	2
. . .	. . .	2,6	1525	25,3	2
437½	3,9	0	1764	33,3	3
. . .	. . .	2,6	1751	32,866	3

By the foregoing experiments it appears, first, that the difference in the force of any given charge of powder which arises from the particular situation of the vent is extremely small.

With 165 grains of powder, and the vent at 0, the velocity of the bullet at a mean of two experiments (*viz.* the 20th and 21st) was 1087 feet in a second; and with the same charge, and the vent at 1,32 inches, the velocity at a mean of the 22d, 23d, and 24th experiments, was 1082 feet in a second; the

the difference, equal five feet in a ſecond, is leſs than what occurred in a repetition of the ſame experiment.

With 218 grains of powder, and the vent at 0, the velocity at a mean in the 40th, 41ſt, 42d, and 47th experiments, was at the rate of 1225 feet in a ſecond; and with the ſame charge, and the vent at 1,3, the velocity was 1276 feet in a ſecond at a mean of four experiments, *viz.* the 43d, 44th, 45th, and 46th.

In the firſt ſet of experiments, with 290 grains of powder, the velocities were,

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
	1414	1476	1502
	1455	1520	1450
	1412	1483	1433
			1454
	3)4281	3)4479	4)5839
Means	1427	1493	1460

See the experiments from N° 48. to N° 57. inclusive.

In the ſecond ſet the velocities were,

	Vent at 0.	Vent at 2,6.
	1419	1438
	1460	1423
	1462	1378
	1436	
	4)5777	3)4239
Means	1444	1413

See the experiments from N° 109. to N° 115. inclusive.

And

And taking the means of all the velocities in both sets in each position of the vent it will be,

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
Mean velocity	1436	1493	1437

The mean recoils in these experiments were,

Vent at 0.	Vent at 1,3.	Vent at 2,6.
22,88	23,34	23,61

In the experiments with 310 grains of powder the velocities of the bullets were not determined with sufficient accuracy to be depended on; but the recoils, which were measured with great nicety, were as follows, *viz.*

Vent at 0.	Vent at 1,3.	Vent at 2,6
24,69	24,95	24,9

With 330 grains of powder the mean velocities and recoils were,

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
Velocities	1594	1625	1525
Recoils	26,075	26,4	25,3

In the experiments with 437½ grains (an ounce avoirdupois) of powder the velocities and recoils were,

Vent at 0.		Vent at 2,6.	
Velocity.	Recoil.	Velocity.	Recoil.
1738	33,	1707	32,5
1824	33,3	1757	33,2
1728	33,6	1789	32,9
3)5291	3)99,9	3)5253	3)98,6
Means 1764	33,3	1751	32,866

Secondly,

Secondly, From the result of all these experiments it appears, that the effect of placing the vent in different positions with respect to the bottom of the chamber is different, in different charges; thus, with 165 grains of powder the velocity of the bullet was rather diminished by removing the vent from 0, or the bottom of the bore to 1,32; but with 218 grains of powder the velocity was a little increased, as was also the recoil. With 290 grains of powder the velocity was greatest when the powder was lighted at the vent 1,3 which was near the middle of the charge, and rather greater when it was lighted at the top, or immediately behind the bullet, than when it was lighted at the bottom. And by the recoil it would seem, that the velocities of the bullets varied nearly in the same manner when the charge consisted of 310 grains of powder.

With 330 grains of powder, both the velocity and the recoil were greater when the powder was lighted at the middle of the charge, than when it was lighted at the bottom; but they were least of all when it was lighted near the top. And when *an ounce* of powder was made use of for the charge, its force was greatest when it was lighted at the bottom. But the difference in the force exerted by the powder which arose from the particular position of the vent was in all cases so inconsiderable (being, as I have before observed, less than what frequently occurred in repeating the same experiment) that no conclusion can be drawn from the experiments, except only this, that any given charge of powder exerts nearly the same force, whatever is the position of the vent.

And hence the following practical inference naturally occurs, *viz.* that in the formation of fire-arms no regard need be had to any supposed advantages that gun-smiths and others have hitherto imagined were to be derived from particular situations for

the vent, ſuch as diminiſhing the recoil, increaſing the force of the charge, &c.; but the vent may be indifferently in any part of the chamber where it will beſt anſwer upon other accounts: and there is little doubt but the ſame thing will hold good in great guns, and all kinds of heavy artillery.

Almoſt every workman who is at all curious in fire-arms has a particular fancy with regard to the beſt form for the bottom of the chamber, and the proper poſition of the vent. They in general agree, that the vent ſhould be as low or far back as poſſible, in order, as they pretend, to leſſen the recoil; but no two of them make it exactly in the ſame manner. Some make the bottom of the chamber flat, and bring the vent out even with the end of the breech-pin. Others make the vent ſlanting through the breech-pin, in ſuch a manner as to enter the bore juſt in its axis. Others again make the bottom of the chamber conical; and there are thoſe who make a little cylindric cavity in the breech-pin, of about two-tenths of an inch in diameter, and near half an inch in length, coinciding with the axis of the bore, and bring out the vent even with the bottom of this little cavity.

The objection to the firſt method is, the vent is apt to be ſtopped up by the foul matter that adheres to the piece after firing, and which is apt to accumulate, eſpecially in damp weather. The ſame inconvenience in a ſtill greater degree attends the other methods, with the addition of another, ariſing from the increaſed length of the vent; for the vent being longer it is not only more liable to be obſtructed, but it takes a longer time for the flame to paſs through it into the chamber, in conſequence of which the piece is ſlower in going off, or, as ſportſmen term it, is apt to hang fire.

The form I would recommend for the bottom of the bore is that of a hemiſphere; and the vent ſhould be brought out directly through the ſide of the barrel, in a line perpendicular to its axis, and pointing to the center of the hemiſpheric concavity of the chamber.

In this caſe the vent would be the ſhorteſt poſſible; it would be the leaſt liable to be obſtructed, and the piece would be more eaſily cleaned, than if the bottom of the bore was of any other form. All theſe advantages, and ſeveral others not leſs important, would be gained by making the bottom of the bore and vent of great guns in the ſame manner.

A new method of determining the velocities of bullets.

From the equality of *action* and *re-action* it appears, that the *momentum* of a gun muſt be preciſely equal to the momentum of its charge; or that the weight of the gun, multiplied into the velocity of its recoil, is juſt equal to the weight of the bullet and of the powder (or the elaſtic fluid that is generated from it) multiplied into their reſpective velocities: for every particle of matter, whether ſolid or fluid, that iſſues out of the mouth of a piece, muſt be impelled by the action of ſome power, which power muſt *re-act* with equal force againſt the bottom of the bore.

Even the fine inviſible elaſtic fluid that is generated from the powder in its inflammation cannot put itſelf in motion without re-acting againſt the gun at the ſame time. Thus we ſee pieces, when they are fired with powder alone, recoil as well as when their charges are made to impel a weight of ſhot, though the recoil is not in the ſame degree in both caſes.

It is easy to determine the velocity of the reccil in any given case, by suspending the gun in an horizontal position by two pendulous rods, and measuring the arc of its ascent, by means of a ribbon according to the method already described, and this will give the momentum of the gun, its weight being known, and consequently the momentum of its charge. But in order to determine the velocity of the bullet from the recoil, it will be necessary to find out how much the weight and velocity of the elastic fluid contributes to it.

That part of the recoil which arises from the expansion of this fluid is always very nearly the same, whether the powder is fired alone, or whether the charge is made to impel one or more bullets, as I have found by a great variety of experiments.

If therefore a gun, suspended according to the method prescribed, is fired with any given charge of powder, but without any bullet or wad, and the recoil is observed, and if the same piece is afterwards fired with the same quantity of powder, and a bullet of a known weight, the excess of the velocity of the recoil in the latter case, over that in the former, will be proportional to the velocity of the bullet; for the difference of these velocities, multiplied into the weight of the gun, will be equal to the weight of the bullet multiplied into its velocity.

Thus if W is put equal to the weight of the gun,

U = the velocity of its recoil, when it is fired with any given charge of powder, without any bullet,

V = the velocity of the recoil, when the same charge is made to impel a bullet,

B = the weight of the bullet, and

v = its velocity,

It will be $v = \frac{\overline{V-U} \times W}{B}$.

I Let

Let us ſee how this method of determining the velocities of bullets will anſwer in practice.

In the 94th experiment the recoil, with 165 grains of powder, without a bullet, was 5,5 inches, and in the 95th experiment, with the ſame charge, the recoil was 5,6 inches. The mean is 5,55 inches; and the length of the rods by which the barrel was ſuſpended being 64 inches, the velocity of the recoil (= U) anſwering to 5,55 inches meaſured upon the ribbon, is that of 1,1358 feet in a ſecond.

In five experiments, with the ſame charge of powder, and a bullet weighing 580 grains, the recoil was as follows, *viz.*

The 20th experiment		14,73 inches
21ſt	-	14,2
22d	-	14,8
23d	-	14,58
24th	-	14,68
		5)73, (= 14,6 inches at a mean.

And the velocity of the recoil (= V) anſwering to the length is that of 2,9880 feet in a ſecond: conſequently V – U, or 2,9880 – 1,1358 is equal to 1,8522 feet in a ſecond.

But as the velocities of recoil are known to be as the chords of the arcs through which the barrel aſcends, it is not neceſſary in order to determine the velocity of the bullet to compute the velocities V and U; but the quantity $\overline{V - U}$, or the difference of the velocities of the recoil when the given charge is fired with and without a bullet, may be computed from the value of the difference of the chords, by one operation. Thus the velocity anſwering to the chord 9,05 is that of 1,8522 feet in a ſecond, which is juſt equal to $\overline{V - U}$, as was before found.

The

The weight of the barrel, together with its carriage, was 47¾ pounds, to which three quarters of a pound is to be added on account of the weight of the rods by which it was suspended, which makes W = 48 pounds, or 336,000 grains, and the weight of the bullet was 580 grains. B is therefore to W as 580 is to 336,000, that is, as 1 is to 579,31 very nearly; and v $(= \frac{\overline{V-U} \times W}{B}$ is equal to $\overline{V-U} \times 579{,}31$.

The value of $\overline{V-U}$ answering to the experiments before mentioned was found to be 1,8522; consequently the velocity of the bullets $(= v)$ was $1{,}8522 \times 579{,}31 = 1073$ feet in a second, which is extremely near 1083 feet in a second, the mean of the velocities, as they were determined by the pendulum.

But the computation for determining the velocity of a bullet upon these principles may be rendered still more simple and easy in practice; for the velocities of the recoil being as the chords measured upon the ribbon, if

c is put equal to the chord of the recoil expressed in English inches, when the piece is fired with powder only, and

C = the chord when a bullet is discharged by the same charge,

then $C - c$ will be as $V - U$; and consequently as $\frac{\overline{V-U} \times W}{B}$, which measures the velocity of the bullet, the ratio of W to B remaining the same.

If therefore we suppose a case in which $C - c$ is equal to one inch, and the velocity of the bullet is computed from that chord, the velocity in any other case, wherein $C - c$ is greater or less than one inch, will be found by multiplying the difference of the chords C and c by the velocity that answers to a difference of one inch.

The

The length of the parallel rods by which the barrel was suspended being 64 inches, the velocity of the recoil answering to $C - c = 1$ inch measured upon the ribbon is 0,204655 parts of a foot in a second; and this is also, in this case the value of $V - U$; the velocity of the bullet is therefore $v = 0{,}204655 \times 579{,}31 = 118{,}35$ feet in a second.

Consequently the velocity of the bullet expressed in feet *per* second may in all cases be found by multiplying the difference of the chords C, and c, by 118,35, the weight of the barrel, the length of the rods by which it is suspended, and the weight of the bullet remaining the same, and this whatever the charge of powder may be that is made use of, and however it may differ in strength or goodness.

According to this rule the velocities of the bullets in the following experiments have been computed from the recoil; and by comparing them with the velocities shewn by the pendulum, we shall be enabled to judge of the accuracy of this new method of determining the velocities of bullets.

In the 76th and 77th experiments, when the piece was fired with 145 grains of powder and a bullet, the recoil was 13,25 and 13,15, or 13,2 at a mean; and with the same charge of powder, without a bullet, the recoil was 4,5 and 4,3, or 4,4 at a mean (see the 84th and 93d experiments). $C - c$ is therefore $13{,}2 - 4{,}4 = 8{,}8$ inches, and the velocity of the bullets $= 8{,}8 \times 118{,}35 = 1045$ feet in a second. The mean of the velocities as they were determined by the pendulum is that of 1040 feet in a second. In the 104th and 105th experiments the recoil was 12.92 and 13,28, and the velocity computed from the mean of those chords is 1030 feet in a second; but the velocity shewn by the pendulum was no more than about 900 feet in a second. As the recoil was so nearly equal to what

what it was in the 76th and 77th experiments before mentiond, when the velocities ſhewn by the recoil and by the pendulum were almoſt exactly the ſame, I am inclined to believe, that there muſt have been ſome miſtake in determining the velocities by the pendulum in theſe laſt experiments, and that the velocity ſhewn by the recoil is moſt to be depended on.

With 290 grains, or half the weight of the bullet in powder, in the 48th, 49th, and 50th experiments, the recoil was 22,58, 22,92, and 22,38; and the recoil, with the ſame charge of powder, without a bullet, at a mean of the 60th and 99th experiments, was 10,66. The mean of the velocities of the bullets, computed from the recoil, is therefore 1416 feet in a ſecond, and the velocity ſhewn by the pendulum was 1427 feet in a ſecond: the difference is not conſiderable. The mean of the velocities in the 109th, 110th, 111th, and 112th experiments is by the recoil 1464, and by the pendulum 1444 feet in a ſecond.

With 330 grains of powder the velocities of the bullets appear to have been as follows, *viz.*

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
By the recoil	1543	1620	1610
By the pendulum	1594	1625	1528

See the 62d, 63d, 64th, 65th, 66th, 67th, and 17th experiments.

The uniformity of the recoil was in all caſes very remarkable. Thus, in the firſt ſet of experiments with 290 grains of powder (from the 48th to the 57th experiment incluſive), the recoil was,

Vent

	Vent at 0.	Vent at 1,3.		Vent at 2,6.
	22,58	23,21		23,06
	22,92	23,76		23,26
	22,38	23,06		23,26
				23,56
	3)67,88	3)70,03		4)93,14
Means =	22,626	23,343	and	23,285

If now we take a mean of the 60th and 99th experiments, and call the recoil, without a bullet, 10,66 as before, the velocities will turn out,

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
By the recoil - -	1416	1501	1494
And by the pendulum they were	1427	1493	1494
The difference is only .	− 11	+ 8	+ 34

The recoil was equally regular in the 117th and five succeeding experiments, when the charge was no less than 437½ grs. = 1 ounce avoirdupois in powder; and the velocities of the bullets determined from the recoil are very nearly the same as they were shewn by the pendulum. Thus, in the 117th, 118th, and 119th experiments the mean recoil was 33,3; and in the 120th, 121st and 122d experiments it was 32,866. And if the recoil without a bullet is called 17,9, as it was determined by the 123d experiment, which was made immediately after the experiments before mentioned, then will the velocities be,

	Vent at 0.	Vent at 2,6
By the recoil - -	1822	1771
And by the pendulum they were	1764	1751
The difference is only -	+58 and	+20 feet

in a ſecond, which is leſs than what frequently occurs in repeating the ſame experiment.

In the 11th, 12th, 13th, and 14th experiments, when the piece was fired with 310 grains of powder and a bullet, the recoil was 24,69, 24,95, 24,9, and 24,9: and in the 15th, 16th, 18th, and 19th experimentss with 330 grains of powder, the recoil was 26,2, 26,2, 26,3, and 26,4. The regularity of theſe numbers is very ſtriking; and though we cannot compare the velocities of the bullets determined by the two methods as we have done in other caſes (as there are reaſons to believe, that the velocities, as they are ſet down in the tables, are not much to be depended on, and as the recoil, with the given charge of 310 grains of powder without a bullet, is not known) yet the regularity of the recoil in theſe experiments affords good grounds to conclude, that the method of determining the velocities of bullets founded upon it muſt be very accurate.

But of all the experiments thoſe numbered from 84 to 92 incluſive afford the ſtrongeſt proof of the accuracy of this method. In thoſe every poſſible precaution was taken to prevent errors ariſing from adventitious circumſtances, and the weights of the bullets and their velocities were ſo various, that the uniform agreement of the two methods of determining the velocities in thoſe trials amounts almoſt to a demonſtration of the truth of the principles upon which this new method is founded.

By

By the following table the reſult of theſe experiments may be ſeen at one view.

The experiments.	Weight of the bullets.	The barrel heavier than the bullet.	The recoil.	Velocity of the bullet. By the recoil.	Velocity of the bullet. By the pendulum.	Difference.
	Grs.	$\frac{W}{B}=$	$C=$	$v=$	$v=$	
84th and 93d	——	——	$c=$ 4,4	——	——	——
85th	90	3733,3	7,16	2109	1763	+ 346
86th	251	1338,6	9,62	1430	1317	+ 113
87th	354	949,15	11,03	1288	1136	+ 152
88th	600	560,	15,22	1240	1229	+ 11
89th	603	557,22	15,13	1224	1229	+ 5
90th	1184	283,78	21,92	1017	978	− 39
91ſt	1754	191,56	27,18	893	916	− 23
92d	2352	141,86	32,25	812	833	− 21

The charge of powder conſiſted of 145 grains in weight in each experiment.

In order to ſhew, in a more ſtriking manner, the reſult of theſe experiments, and the compariſon of the two methods of aſcertaining the velocities of bullets, I have drawn the fig. 16. where the numbers that are marked upon the line AB are taken from A towards B, in proportion to the weights of the bullets; while the lines drawn from thoſe numbers perpendicular from AB (as *w*, *v*, for inſtance, at the number 2352) and ending at the curve *c*, *d*, expreſs their velocities, as ſhewn by the pendulum. The continuation of thoſe lines on the oppoſite ſide of the line AB ſhew the recoil, and alſo the velocities of the bullets as determined from it; thus *w*, *r*, and the (dotted) lines

 parallel

parallel to it, which end at the line *g*, *f*, expreſs the recoil; and the portion of each of thoſe lines that is comprehended between the line AB and the curve *m*, *n* (as *w*, *u*) is as the velocity of the bullet in the ſeveral experiments. The line A, *e*, denotes the weight of the charge of powder; and the line A, *m*, the velocity with which the elaſtic fluid eſcapes out of the piece, when the powder is fired without any bullet.

Upon an inſpection of this figure, as well as from an examination of the foregoing table, it appears, that the velocities determined by the two methods agree with great nicety in all the experiments after the 87th; but in the 87th experiment, and alſo in the 86th, but particularly in the 85th, the difference in the reſult of theſe different methods is very conſiderable: and it is remarkable, that in thoſe experiments where they diſagree moſt, the velocities of the bullets, as determined by the pendulum, are extremely irregular; while, on the other hand, the gradual increaſe of the recoil as the bullets were heavier, and the great regularity of the correſponding velocities, afford good grounds to conclude, that this diſagreement is not owing to any inaccuracy in the new method of aſcertaining the velocities, but to ſome other cauſe that remains to be inveſtigated.

But before we proceed in this inquiry, let us ſeparate the five laſt experiments in the foregoing table; and, ſumming up the velocities determined by the two methods, we ſhall ſee by their difference how thoſe methods agreed upon the whole, in this inſtance.

Expe-

Experiments.	Weight of the bullets.	Velocity. By the recoil.	Velocity. By the pendulum.	Difference.
	Grs.			
88th	600	1240	1229	+ 11
89th	603	1224	1229	− 5
90th	1184	1017	978	+ 39
91ſt	1754	893	916	− 23
92d	2352	812	833	− 21
Sums and diff. of the velocities		5186	5185	+ 1

Here the difference in the reſult of the two methods does not amount to $\frac{1}{5000}$th part of the whole velocity; but I lay no ſtreſs upon this extraordinary argument. I am ſenſible that it muſt in ſome degree have been accidental; but as the difference in the velocities, computed by theſe different methods, was in no inſtance conſiderable, not being in any caſe ſo great as what frequently occurred in the moſt careful repetition of the ſame experiment, and as the velocities, as determined by the recoil, were much more regular than thoſe ſhewn by the pendulum, as appears by comparing the curves *g*, *f*, and *m*, *n*, (fig. 16.) with the crooked line *c*, *d*, I think we may fairly conclude, that this new method may with ſafety be relied on in practice.

The greateſt difference in the velocities, as aſcertained by the two methods, appears, in the inſtance of the 85th experiment, where the velocity determined from the recoil exceeds that ſhewn by the pendulum by 346 feet in a ſecond, the former velocity being that of 2109 feet in a ſecond, the latter only 1763 feet in a ſecond; and in the two ſucceeding experiments, the velocities ſhewn by the pendulum are likewiſe deficient, though not in ſo great a degree.

This

This apparent deficiency remains now to be accounted for; and, first, it cannot be supposed, that it arose from any imperfection in Mr. ROBINS's method of determining the velocities of bullets; for that method is founded upon such principles as leave no room to doubt of its accuracy; and the practical errors that occur in making the experiments, and which cannot be intirely prevented, or exactly compensated, are in general so small, that the difference of the velocities in question cannot be attributed to them. It is true, the effect of those errors is more likely to appear in experiments made under such circumstances as those under which the experiments we are now speaking of were made, than in any other case; for the bullets being very light, the arc of the ascent of the pendulum was but small, and a small mistake in measuring the chord upon the ribbon would have produced a very considerable error in computing the velocity of the bullet: thus, a difference of one tenth of an inch, more or less, upon the ribbon in the 85th experiment, would have made a difference in the velocity of more than 120 feet in a second. But independent of the pains that were taken to prevent mistakes, the striking agreement of the velocities determined by the two methods in the experiments which immediately follow, as also in all other cases where they could be compared, affords abundant reason to conclude, that the errors arising from those causes were in no instance very considerable.

But if both methods of ascertaining the velocities of bullets are to be relied on, then the difference of the velocities, as determined by them in these experiments, can only be accounted for by supposing that it arose from their having been diminished by the resistance of the air in the passage of the bullets from the mouth of the piece to the pendulum; and this suspicion will

will be much ſtrengthened when we conſider how great the reſiſtance is that the air oppoſes to bodies that move very ſwiftly in it, and that the bullets in theſe experiments were not only projected with great velocities, but were alſo very light, and conſequently more liable to be retarded by the reſiſtance on that account.

To put the matter beyond all doubt, let us ſee what the reſiſtance was that theſe bullets met with, and how much their velocities were diminiſhed by it. The weight of the bullet (in the 85th experiment) was 90 grains; its diameter was 0,78 of an inch, and it was projected with a velocity of 2109 feet in a ſecond.

If now a computation be made according to the method laid down by Sir ISAAC NEWTON for compreſſed fluids, it will be found, that the reſiſtance to this bullet was not leſs than 8½ lbs. avoirdupois, which is ſomething more than 660 times its weight. But Mr. ROBINS has ſhewn, by experiment, that the reſiſtance of the air to bodies moving in it with very great velocity is near three times greater than Sir ISAAC has determined it, and as the velocity with which this bullet was impelled is conſiderably greater than any in Mr. ROBINS's experiments, it is highly probable, that the reſiſtance in this inſtance was at leaſt 2000 times greater than the weight of the bullet.

The diſtance from the mouth of the piece to the pendulum, as we have before obſerved, was 12 feet; but, as there is reaſon to think, that the blaſt of the powder, which always follows the bullet, continues to act upon it for ſome ſenſible ſpace of time after it is out of the bore, and by urging it on counter-balances, or at leaſt counter-acts in a great meaſure the reſiſtance of the air, we will ſuppoſe, that the reſiſtance does

not begin, or rather that the motion of the bullet does not begin to be retarded, till it has got to the diſtance of two feet from the muzzle. The diſtance, therefore, between the barrel and the pendulum, inſtead of 12 feet, is to be eſtimated at 10 feet; and as the bullet took up about $\frac{1}{192}$ part of a ſecond in running over that ſpace, it muſt in that time have loſt a velocity of about 335 feet in a ſecond, as will appear upon making the computation, and this will very exactly account for the apparent diminution of the velocity in the experiment; for the difference of the velocities, as determined by the recoil and by the pendulum, $= 2109 - 1763 = 346$ feet in a ſecond, is extremely near 335 feet in a ſecond, the diminution of the velocity by the reſiſtance as here determined.

If the diminution of the velocities of the bullets in the two ſubſequent experiments be computed in like manner, it will turn out in the 86th experiment $= 65$ feet in a ſecond, and in the 87th experiment $= 33$ feet in a ſecond; and making theſe corrections, the compariſon of the two methods of aſcertaining the velocities will ſtand thus:

	85th exp.	86th exp.	87th exp.
Velocities ſhewn by the pendulum,	1763	1317	1136
Add the diminution of the velocity by the reſiſtance of the air,	335	65	33
	2098	1382	1169
Velocity by the recoil, -	2109	1430	1288
The difference =	+11	+48	+119

So that it appears, notwithſtanding theſe corrections, that the velocities in the 86th and 87th experiments, and particularly in the laſt, as they were determined by the pendulum, are ſtill

ſtill conſiderably deficient. But the manifeſt irregularity of the velocities in thoſe inſtances affords abundant reaſon to conclude, that it muſt have ariſen from ſome extraordinary accidental cauſe, and therefore, that little dependance is to be put upon the reſult of thoſe experiments. I cannot take upon me to determine poſitively what the cauſe was which produced this irregularity; but I ſtrongly ſuſpect, that it aroſe from the breaking of the bullets in the barrel by the force of the exploſion: for theſe bullets, as has already been mentioned, were formed of lead, incloſing leſſer bullets of plaſter of Paris; and I well remember to have obſerved at the time ſeveral ſmall fragments of the plaſter which had fallen down by the ſide of the pendulum. I confeſs, I did not then pay much attention to this circumſtance, as I naturally concluded, that it aroſe from the breaking of the bullet in penetrating the target of the pendulum, and that the ſmall pieces of plaſter I ſaw upon the ground had fallen out of the hole by which the bullet entered. But if the bullets were not abſolutely broken in pieces in firing, yet, if they were conſiderably bruiſed, and the plaſter or a part of it were ſeparated from the lead, ſuch a change in their form might produce a great increaſe of the reſiſtance, and even their initial velocities might be affected by it, for their form being changed from that of a globe to ſome other figure, they might not fit the bore, and a part of the force of the charge might be loſt by the windage.

That this actually happened in the 87th experiment ſeems very probable, as the velocity with which the bullet was projected, as it was determined by the recoil, is conſiderably leſs in proportion in that experiment than in either of thoſe that precede it in that ſet, or in thoſe which follow it, as will ap-

pear upon inſpecting the curvature of the line *m*, *n*, fig. 16. But I forbear to inſiſt further upon this matter.

As I have made an allowance for the reſiſtance of the air in theſe experiments, it may be expected that I ſhould do it in all other caſes; but, I think, it will appear upon enquiry, that the diminution of the velocities of the bullets on that account was in general ſo inconſiderable that it might ſafely be neglected: thus, for inſtance, in the experiments with an ounce of powder, when the velocity of the bullet was more than 1750 feet in a ſecond, the diminution turns out no more than about 25 or 30 feet in a ſecond, though we ſuppoſe the full reſiſtance to have begun ſo near as two feet from the mouth of the piece; and in all caſes where the velocities were leſs, the effect of the reſiſtance was leſs in a much greater proportion: and even in this inſtance there is reaſon to think, that the diminution of the velocity as we have determined it is too great; for the flame of gun-powder expands with ſuch an amazing rapidity, that it is ſcarcely to be ſuppoſed but that it follows the bullet, and continues to act upon it more than two feet, or even four feet, from the gun, and when the velocity of the bullet is leſs, its action upon it muſt be ſenſible at a ſtill greater diſtance.

With 218 grains of powder the recoil appears to have been very uniform; but if the velocities of the bullets are determined from the recoil in the 40th and ſeven following experiments, when this charge was made uſe of, and from the recoil without a bullet in the 72d and 73d experiments, the velocities will turn out conſiderably too ſmall, as we ſhall ſee by making the computation.

The

	Vent at 0.				Vent at 1,3.
The recoil in the	40th exp. was 18,		and in the 43d exp.		it was 18,3
	41st	- 17,71	-	44th	- 18,35
	42d	- 17,91	-	45th	- 18,35
	47th	- 8,1	-	46th	- 18,35
		4)71,72			4)73,35
	Means =	17,93	and		18,34

And in the 72d and 73d experiments the recoil, with the ſame charge without a bullet, was 8,72, and 8,47 = 8,595 at a medium, the velocities therefore turn out,

	Vent at 0.		Vent at 1,3.	
By the recoil	1105	-	1153	
inſtead of	1225	and	1276	as they were ſhewn by the pend.

The difference 120 and 123 feet in a ſecond amounts to near one twelfth part of the whole velocity.

This difference is undoubtedly owing to the recoil without a bullet being taken too great; for it is not only greater than it ought to be, in order that the velocities of the bullets may come out right; but it is conſiderably greater in proportion than the recoil with any other charge.

Thus, with 145	grains of powder	the recoil was		4,4
with 165	grains	- -	it was	5,55
290	grains	- - -		10,66
330	grains	- - -		12,7
and with 437½	grains	- - -	it was	17,9

And if the recoil with 218 grains is determined from theſe numbers by interpolation, it comes out 7,5; and with that

value for C the velocities of the bullets in the before mentioned experiments appear to be,

	Vent at 0.		Vent at 1,3.
	1243	and	1283 by the recoil
which is extremely near	1225	and	1276 the velocities

shewn by the pendulum.

It is to be remembered, that the 72d and 73d experiments, from which we before determined the recoil with the given charge of powder without a bullet, were not made upon the same day with the experiments before mentioned; and it is well known, that the force of powder is different upon different days. And it is worthy of remark, that in those two experiments the strength of government powder appeared to be considerably the greatest. I mention these circumstances to shew the probability there is, that the recoil in those experiments, from some unknown cause, was greater than it ought to have been, or rather than it would have been, had the experiments been made at the same time when the experiments with the bullets were made; or at any other time under the same circumstances.

As this method of determining the velocities of the bullets did not occur to me till after I had finished the course of my experiments, and had taken down my apparatus, I have not had an opportunity of ascertaining the recoil, with and without a bullet, with that degree of precision that I could wish. If I had thought of it sooner, or if I had recollected that passage in Mr. ROBINS's new Principles of Gunnery, where he says, "The part of the recoil, arising from the expansion of "the powder alone, is found to be no greater when it impels "a leaden bullet before it, than when the same quantity is fired "without any wad to confine it:" I say, if that passage had occurred

occurred to me before it had been too late, I certainly ſhould have taken ſome pains to have aſcertained the fact; but as it is, I think, enough has been done to ſhew, that there is the greateſt probability that the velocities of bullets may in all caſes be determined by the recoil with great accuracy; and I hope ſoon to have it in my power to put the matter out of all doubt, and to verify this new method by a courſe of concluſive experiments which I am preparing for that purpoſe.

In the mean time I would juſt obſerve, that if this method ſhould be found to anſwer, when applied to muſket bullets, it cannot fail to anſwer equally well when it is applied to cannon balls and bomb ſhells of the largeſt dimenſions; and it is apprehended, that it will be much preferable to any method hitherto made public; not only as it may be applied indifferently to all kinds of military projectiles, and that with very little trouble or expence in making the experiment; but alſo becauſe by it the velocities with which bullets are *actually projected* are determined; whereas by the pendulum their velocities can only be aſcertained at ſome diſtance from the gun, and after they have loſt a part of their initial velocities by the reſiſtance of the air through which they are obliged to paſs to arrive at the pendulum.

At the trifling expence of ten or fifteen pounds an apparatus might be conſtructed that would anſwer for making the experiments with all the different kinds of ordnance in the Britiſh ſervice. The advantages that might be derived from ſuch a ſet of experiments are too obvious to require being mentioned.

Of

Of a very accurate method of proving gun-powder.

All the *eprouvettes*, or powder-triers, in common uſe are defective in many reſpects. Neither the abſolute force of gun-powder can be determined by means of them, nor the comparative force of different kinds of it, but under circumſtances very different from thoſe in which the powder is made uſe of in ſervice.

As the force of powder ariſes from the action of an elaſtic fluid that is generated from it in its inflammation, the quicker the charge takes fire, the more of this fluid will be generated in any given ſhort ſpace of time, and the greater of courſe will be its effect upon the bullet. But in the common method of proving gun-powder, the weight by which the powder is confined is ſo great in proportion to the quantity of the charge, that there is time quite ſufficient for the charge to be all inflamed, even when the powder is of the ſloweſt compoſition, before the body to be put in motion can be ſenſibly removed from its place. The experiment, therefore, may ſhew which of two kinds of powder is the ſtrongeſt, when equal quantities of both are confined in equal ſpaces, and completely inflamed; but the degree of the inflammability, which is a property eſſential to the goodneſs of the powder, cannot by theſe means be aſcertained.

Hence it appears, how powder may anſwer to the proof, ſuch as is commonly required, and may nevertheleſs turn out very indifferent when it comes to be uſed in ſervice. And this, I believe, frequently happens; at leaſt I know complaints from officers of the badneſs of our powder are very common; and I would ſuppoſe that no powder is ever received by the

Board of Ordnance but ſuch as has gone through the eſtabliſhed examination, and has anſwered to the uſual teſt of its being of the ſtandard degree of ſtrength.

But though the common powder triers may ſhew powder to be better than it really is, they never can make it appear to be worſe than it is; it will therefore always be the intereſt of thoſe who manufacture that commodity to adhere to the old method of proving it. But the purchaſer will find his account in having it examined in a method by which its goodneſs may be aſcertained with greater preciſion.

The method I would recommend is as follows. A quantity of powder being provided, which, from any previous examination or trial, is known to be of a proper degree of ſtrength to ſerve as a ſtandard for the proof of other powder, a given charge of it is to be fired, with a fit bullet, in a barrel ſuſpended by two pendulous rods, according to the method before deſcribed, and the recoil is to be carefully meaſured upon the ribbon. And this experiment being repeated three or four times; or oftener if there is any difference in the recoil, the mean and the extremes of the chords may be marked upon the ribbon by black lines drawn acroſs it, and the word *proof* may be written upon the middle line; or if the recoil is uniform (which it will be to a ſufficient degree of accuracy, if care is taken to make the experiments under the ſame circumſtances) then the *proof mark* is to be made in that part of the ribbon to which it was conſtantly drawn out by the recoil in the different trials.

The recoil, with a known charge of ſtandard powder, being thus aſcertained and marked upon the ribbon, let an equal quantity of any other powder (that is to be proved) be fired in the ſame barrel, with a bullet of the ſame weight, and every other circumſtance alike, and if the ribbon is drawn out as far

far or farther than the proof mark, the powder is as good or better than the ſtandard; but if it falls ſhort of that diſtance, it is worſe than the ſtandard, and is to be rejected.

For the greater the velocity is with which the bullet is impelled, the greater will be the recoil; and when the recoil is the ſame, the velocities of the bullets are equal, and the powder is of the ſame degree of ſtrength, if the quantity of the charge is the ſame. And if care is taken in proportioning the charge to the weight of the bullet, to come as near as poſſible to the medium proportion that obtains in practice, the determination of the goodneſs of gun-powder from the reſult of this experiment cannot fail to hold good in actual ſervice.

Fig. 14. repreſents the propoſed apparatus, drawn to a ſcale of one foot to the inch. *a*, *b*, is the barrel ſuſpended by the pendulous rods *c*, *d*; and *r* is the ribbon for meaſuring the recoil.

The length of the bore is 30 inches, and its diameter is one inch, conſequently it is juſt 30 calibres in length, and will carry a leaden bullet of about 3 ounces.

The barrel may be made of gun metal, or of caſt iron as that is a cheaper commodity; but great care muſt be taken in boring it, to make the cylinder perfectly ſtrait and ſmooth, as well as to preſerve the proper dimenſions. Of whatever metal the barrel is made, it ought to weigh at leaſt 50 lbs. in order that the velocity of the recoil may not be too great; and the rods by which it is ſuſpended ſhould be five feet in length. The vent may be about one twentieth of an inch in diameter; and it ſhould be *bouched* or lined with gold, in the ſame manner as the touch-hole is made in the better kind of fowling pieces, in order that its dimenſions may not be increaſed by repeated firing.

The bullets ſhould be made to fit the bore with very little windage; and it would be better if they were all caſt in the ſame mould, and of the ſame parcel of lead, as in that caſe their weights and dimenſions would be more accurately the ſame, and the experiments would of courſe be more concluſive.

The ſtated charge of powder may be half an ounce, and it ſhould always be put up in a cartridge of very fine paper; and after the piece is loaded it ſhould be primed with other powder, firſt taking care to prick the cartridge by thruſting a priming wire down the vent.

As it appears, from ſeveral experiments made on purpoſe to aſcertain the fact, that ramming the powder more or leſs has a very ſenſible effect to increaſe or diminiſh the force of the charge; to prevent any inaccuracies that might ariſe from that cauſe, a ram-rod, ſuch as is repreſented fig. 15. may be made uſe of. It is to be made of a cylindric piece of wood in the ſame manner as ram-rods in general are made, but with the addition of a ring C, about one inch and a half, or two inches in diameter, which, being placed at a proper diſtance from the end (*a*) of the ram-rod that goes up into the bore, will prevent its being thruſt up too far. This ring may be made of wood, or of any kind of metal as ſhall be found moſt convenient. The other end of the ram-rod (*b*) may be 31 or 32 inches in length from the ring, and the extremity of it being covered with a proper ſubſtance, it may be made uſe of for wiping out the barrel after each experiment.

The machine (*f*) for the tape to ſlide through may be the ſame as is deſcribed by Dr. HUTTON in his account of his experiments on the initial velocities of cannon balls; as his method is much better calculated to anſwer the purpoſe than that propoſed and made uſe of by Mr. ROBINS. It will alſo be

 better

better for the axis of the pendulous rods to reſt upon level pieces of wood or iron, than for them to move in circular grooves: only care muſt be taken to confine them by ſtaples or ſome other contrivance, to prevent their ſlipping out of their places.

The trunnions, by means of which the barrel is connected with the pendulous rods, and upon which it is ſupported, ſhould be as ſmall as poſſible, in order to leſſen the friction; and for the ſame reaſon they ſhould be well poliſhed, as well as the grooves that receive them. They need not be caſt upon the barrel, but may be ſcrewed into it after it is finiſhed.

In making the experiments, regard muſt be had to the heat of the barrel, as well as to the temperature of the atmoſphere; for heat and cold, dryneſs and moiſture, have a very ſenſible effect upon gun-powder to increaſe or diminiſh its force. If therefore a very great degree of accuracy is at any time required, it will be beſt to begin by firing the piece two or three times merely to warm it; after which three or four experiments may be made with ſtandard powder, to determine anew the proof mark (for the ſtrength of the ſame powder is different upon different days); and when this is done, the experiments with the powder that is to be proved are to be made, taking care to preſerve the ſame interval of time between the firings, that the heat of the piece may be the ſame in each trial.

If all theſe precautions are taken, and if the bullets are of the ſame weight and dimenſions, powder may be proved by this method with much greater accuracy than has hitherto been done by any of the common methods made uſe of for that purpoſe.

Of

Of the comparative goodneſs, or value, of powder of different degrees of ſtrength.

Let V denote the velocity of the bullet with the ſtronger powder, and put v equal to the velocity with the weaker, when the charges are equal, and the weight and dimenſions of the bullets are the ſame, and when they are diſcharged from the ſame piece. If the charge is augmented when the weaker powder is made uſe of, till the velocity of the bullet is increaſed from v to V, or becomes equal to the velocity with the given charge of the ſtronger powder, the *value* of the charges may then be ſaid to be equal; and conſequently the weaker powder is as much worſe than the ſtronger, or is of leſs value in proportion as the quantity of it required by the pound, to produce the given effect is greater.

But it is well known, that the velocities, with different quantities of the ſame kind of powder, are in the ſub-duplicate ratio of the weights of the charges. The charges, therefore, muſt be as the ſquares of the velocities, and conſequently the charge of the weaker powder muſt be to that of the ſtronger, when the velocities are equal, as VV is to vv. The weaker powder is therefore as much worſe than the ſtronger as VV is greater than vv; or the comparative goodneſs of powder, of different degrees of ſtrength, is as the ſquares of the velocities of the bullets when the charges are equal.

The mean velocity of the bullets, as ſhewn by the pendulum in the 104th and 105th experiments, when the piece was fired with 145 grains of government powder, was 894 feet

in a ſecond; and with the ſame quantity of double proof * battle powder (experiment N° 106) the velocity was 990 feet in a ſecond. Now the ſquares of theſe velocities, which, as we juſt obſerved, meaſure the goodneſs of the powder, are to each other as 1 is to 1,2263, or nearly as 5 is to 6.

With 218 grains of government powder, the mean velocity in four experiments (*viz.* the 40th, 41ſt, 42d, and 43d) was 1225 feet in a ſecond; and in the experiment N° 107. when the ſame quantity of double proof battle powder was made uſe of, the velocity was 1380 in a ſecond; and $\overline{1225}^2$ is to $\overline{1380}^2$ as 1 is to 1,2691.

With 290 grains, or half the weight of the bullet in government powder in the 109th, 110th, 111th, and 112th experiments, the mean velocity of the bullet was 1444 feet in a ſecond; but with the ſame quantity of the battle powder (experiment N° 116.) the velocity was 1525 feet in a ſecond; $\overline{1444}^2$ is to $\overline{1525}^2$, as 1 is to 1,1153.

By taking a medium of theſe trials it appears, that double proof battle powder is better than government powder in proportion as 1,2036 is to 1, or nearly as 6 is to 5.

But if, inſtead of weighing the powder, we eſtimate the quantity of the charge by meaſurement, or the ſpace it occupies in the bore of the piece, the comparative ſtrength of battle powder will appear to be conſiderably greater, or its ſtrength will be to that of government powder nearly as 4 is to 3; for the grains of this better kind of powder being more compact and nearly of a ſpherical form, a greater weight of it will lie in any

* This is called *battle* powder, not becauſe it is uſed in battle or in war; but from *Battle*, the name of a village in Kent, where that kind of powder is made.

given ſpace than of government powder, which is formed more looſely, and of various and of very irregular figures.

Now the common price of double proof battle powder, as it is ſold by the wholeſale dealers in that commodity, is at the rate of £. 10 *per* cwt. net, which is juſt two ſhillings by the pound; while government powder is ſold at £. 5 5*s.* *per* hundred, or one ſhilling and $\frac{6}{10}$th of a penny *per* pound; but battle powder is better than government powder only in the proportion of 1,2036 to 1, or of one ſhilling and two pence to one ſhilling and $\frac{6}{10}$th of a penny; battle powder is therefore ſold at the rate of ten pence by the pound, or $41\frac{2}{3}$ *per cent.* dearer than it ought to be; or thoſe, who make uſe of it in preference to government powder, do it at a certain loſs of $41\frac{2}{3}$ *per cent.* of the money that the powder coſts them.

Of the relation of the velocities of bullets to their weights.

According to Mr. ROBINS's theory, when bullets of the ſame diameter, but different weights, are diſcharged from the ſame piece by the ſame quantity of powder, their velocities ſhould be in the reciprocal ſub-duplicate ratio of their weights; but as this theory is founded upon a ſuppoſition that the action of the elaſtic fluid, generated from the powder, is always the ſame in any and every given part of the bore when the charge is the ſame, whatever may be the weight of the bullet; and as no allowance is made for the expenditure of force required to put the fluid itſelf in motion, or for the loſs of it by the vent; it is plain that the theory is defective. It is true, Dr. HUTTON in his experiments found this law to obtain without any great error, and poſſibly it may hold good with ſufficient accuracy in many caſes; for it ſometimes happens that a number of errors or actions,

actions, whose operations have a contrary tendency, so compensate each other, that their effects when united are not sensible. But when this is the case, if any one of the causes of error is removed, those which remain will be detected.

When any given charge is loaded with a heavy bullet, more of the powder is inflamed in any very short space of time than when the bullet is lighter, and the action of the powder ought of course to be greater on that account; but then a heavy bullet takes up more time in passing through the bore than a light one, and consequently more of the elastic fluid, generated from the powder, escapes by the vent and by windage. It may happen, that the augmentation of the force, on account of one of these circumstances, may exactly counterbalance the diminution of it arising from the other; and if it should be found upon trial that this is the case in general, in pieces as they are now constructed, and with all the variety of shot that are made use of in practice, it would be of great use to know the fact: and possibly it might answer as well, as far as it relates to the art of gunnery, as if we were perfectly acquainted with, and were able to appreciate, the effect of each varying circumstance under which an experiment can be made. But when, concluding too hastily from the result of a partial experiment, we suppose with Mr. ROBINS, that because the sum total of the action or pressure of the elastic fluid upon the bullet, during the time of its passage through the bore, happens to be the same when bullets of different weights are made use of (which collective pressure is in all cases proportional to, and is accurately measured by, the velocity, or rather motion, communicated to the bullet) that therefore the pressure in any given part is always exactly the same, when the quantity of powder is the same with which the piece is fired; and from thence endeavour to

prove,

prove, that the inflammation of gun-powder is instantaneous, or that the whole charge is in all cases inflamed, and "con-"verted into an elastic fluid before the bullet is sensibly moved "from its place;" such reasonings and conclusions may lead to very dangerous errors.

It is undoubtedly true, that if the principles assumed by Mr. ROBINS with respect to the manner in which gun-powder takes fire, and the relation of the elasticity of the generated fluid to its density, or the intensity of its pressure upon the bullet as it expands in the barrel, were just, and if the loss of force by the vent and windage was in all cases inconsiderable, or if it was prevented, the deductions from the theory respecting the velocities of bullets of different weights would always hold good. But if, on the contrary, it should be found upon making the experiments carefully, and in such a manner as intirely to prevent inaccuracies arising from adventitious circumstances, that the velocities observe a law different from that which the theory supposes, we may fairly conclude, that the principles upon which the theory is founded are erroneous.

Let us now see how far these experiments differ from the theory. Those numbered from 84 to 92 inclusive were made in such a manner that no part of the force of the powder was lost by the vent, or by windage, as has already been mentioned, and all possible attention was paid to every circumstance that could contribute to render them perfect and conclusive.

A particular account of them with the means used for forming the bullets, and making them fit for the bore, and the contrivance for preventing the escape of the elastic fluid by the vent, &c. may be seen in the general table, p. 245. The following table shews the result of them.

N. B. The charge of powder was the ſame in each experiment, and conſiſted of 145 grains in weight.

	Weight of the bullet.	Velocity of the bullet. Actual.	Computed.	Difference.
85th exp.	90 grs.	2109	2109	——
86th	251	1430	1262	+168
87th	354	1288	1063	+225
88th	600	1240	817	+423
89th	603	1224	815	+409
90th	1184	1017	581	+436
91ſt	1754	893	478	+415
92d	2352	812	413	+399

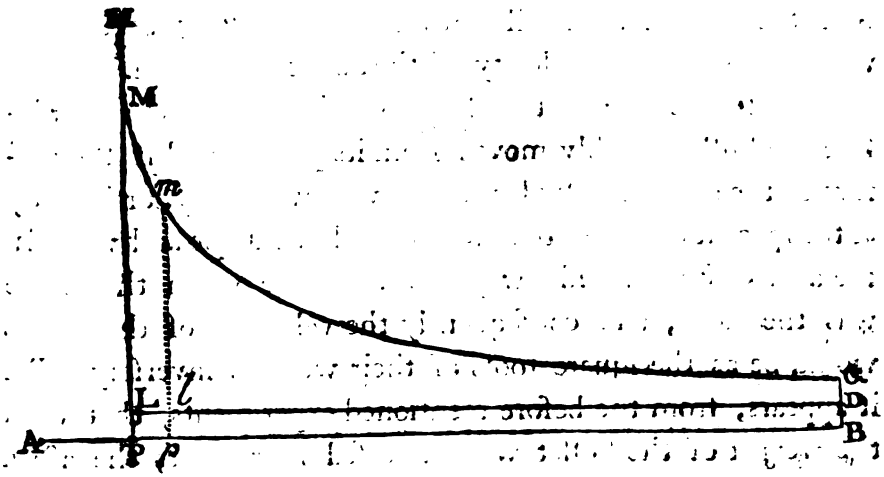
The computed velocities, as they are ſet down in this table, were determined from the actual velocity of the bullet, as determined by the recoil in the 85th experiment; and the reciprocal ſub-duplicate ratio of its weight to the weight of the bullet in each ſubſequent experiment; and in the laſt column is marked the difference between the experiment and the theory, or the number of feet in a ſecond, by which the actual velocity exceeds the computed.

But in order that we may ſee this matter in different points of view, let the order of the experiments be now inverted, and let the computed velocities be determined from the actual velocity in the 92d experiment; and aſſuming the total or collective preſſure exerted by the powder upon the bullet in that experiment equal to unity, let the collective preſſure in the other experiments be computed from the ratio of the actual to the computed velocities, and the table will ſtand thus:

92d

	Weight of the bullet.	Velocity of the bullet Actual.	Computed.	Difference.	Collective pressure.
92d exp.	2352 grs.	812	812	——	1,0000
91st	1754	893	940	− 47	0,9020
90th	1184	1017	1145	− 128	0,7897
89th	603	1224	1604	− 380	0,5825
88th	600	1240	1608	− 368	0,5949
87th	354	1288	2093	− 805	0,3778
86th	251	1430	2486	− 1056	0,3310
85th	90	2109	4151	− 2042	0,2581

In the following figure let AB represent the axis of the piece, and AP the length of the space filled with powder; and at the



point P let the perpendicular PH be erected, upon which let PL and PM be taken from P towards H of such magnitudes that while PL expounds the uniform force of gravity, or the weight of the bullet, PM shall be as the force exerted by the powder

upon the bullet at the moment of the explosion. If now we suppose, that while the bullet moves on from P towards B, the line PM or *pm*, goes along with it, and that the point *m* is always taken in such a manner that the line *pm*, shall be to *pl*, or PL, as the force acting upon the bullet in the point *p*, is to its weight, till *pm*, coincides with QB, then will the area PMQB be to the area PLDB in the duplicate proportion of the velocities which the bullet would acquire when acted on by its own gravity through the space PB, and when impelled through the same space by the force of the powder, as may be seen demonstrated by Sir ISAAC NEWTON, in his Mathematical Principles of Natural Philosophy, book I. prop. 39.

Now what I call the collective pressure, or sum total of the action of the powder upon the bullet, is the measure of the area PMQB; and it is plain, from what has been said above, that its measures are in all cases to be accurately determined, when the weight and velocity of the bullet are known.

If all the powder of the charge was inflamed at once, or before the bullet sensibly moved from its place; and if the pressure of the generated fluid was always as its density, or inversely as the space it occupies, then would the line MQ be an hyberbola, the area PMQB would always be the same when the charge was the same, and consequently the velocities of the bullets would be as the square roots of their weights inversely. But it appears, from the before mentioned experiments, that when the weight of the bullet was increased four times, the action of the powder, or area PMQB, was nearly doubled; for in the 92d experiment, when four bullets were discharged at once, the collective pressure was as 1; but in the 89th experiment, when a single bullet was made use of, the collective

pressure

pressure was only as 0,5825; and in the 85th, 86th, and 87th experiments, when the bullets were much lighter, the action of the charge was still less.

But though we can determine with great certainty, from these experiments, the ratio in which the action of the powder upon the bullet was increased or diminished, by making use of bullets of greater or less weight; yet we cannot from thence ascertain the relation of the elasticity of the generated fluid to its density, nor the quantity of powder that is inflamed at different periods before and after the bullet begins to move in the bore.

But assuming Mr ROBINS's principles as far as relates to the elasticity of the fluid, and supposing that in all the experiments, except the 92d, a part only of the charge took fire, and that that part was inflamed and converted into an elastic fluid before the bullet began to move; upon that supposition we can determine the quantity of powder that took fire in each experiment; for the quantity of powder in that case would be as the collective pressure.

Thus, if the whole charge, = 145 grains in weight, is supposed to have been inflamed in the 92d experiment, the quantity inflamed in each of the other experiments will appear to have been as follows; *viz.*

	Weight of the bullet.	Velocity of the bullet.	Collective pressure.	Powder inflamed.
85th exp.	90 grs.	2109	0,2581	37 grs.
86th	251	1430	0,3310	48
87th	354	1288	0,3778	55
88th	600	1240	0,5949	86
89th	603	1224	0,5825	84
90th	1184	1017	0,7897	114
91st	1754	893	0,9020	131

But there are many reasons to suppose, that the diminution of the action of the powder upon the bullet, when it is lighter, is not so much owing to the smallness of the quantity of powder that takes fire in that case as to the *vis inertiæ* of the generated fluid. It is true, that a greater portion of the charge takes fire when the bullet is heavy than when it is light, as I found in the very experiments of which I am now speaking; but then the quantity of unfired powder in any case was much too small to account for the apparent diminution of the force when light bullets were made use of.

If the elastic fluid, in the action of which the force of powder consists, were infinitely fine, or if its weight bore no proportion to that of the powder that generated it; and if the gross matter, or *caput mortuum*, of the powder remained in the bottom of the bore after the explosion, then, and upon no other supposition, would the pressure upon the bullet be inversely as the space occupied by the fluid: but it is evident that this can never be the case.

A curious subject for speculation here occurs: how far would it be advantageous, were it possible, to diminish the specific gravity of gun-powder, and the fluid generated from it, with-

out lessening its elastic force? It would certainly act upon very light bullets with greater force; but when heavy ones came to be made use of, there is reason to think, that, except extraordinary precaution was taken to prevent it, the greatest part of the force would be lost by the vent and by windage.

The velocity with which elastic fluids rush into a void space is as the elasticity of the fluid directly, and inversely as its density; if, therefore, the density of the fluid generated from powder was four times less than it is, its elasticity remaining the same, it would issue out at the vent, and escape by the side of the bullet in the bore, with nearly four times as great a velocity as it does at present; but we know from experiment that the loss of force on those accounts is now very considerable.

In the experiments N° 76. and 77. when the piece was fired with 145 grains of powder, the velocity of the bullets at a medium was 1040 feet in a second; but in the 88th and 89th experiments, when the bullets were even heavier, and the piece was fired with the same quantity of powder, the mean velocity was 1232 feet in a second. The difference = 192 feet in a second, answers to a difference of force greater in the last experiments than in the first in the proportion of 14 to 10.

I know of no way to account for this difference, but by supposing that it was owing intirely to the escape of the elastic fluid by the vent, and by windage, in those experiments where the vent was open, and the bullets were put naked into the piece.

An elastic bow, made of very light wood, will throw an arrow, and especially a light one, with greater velocity than a bow of steel of the same degree of stiffness: but, for practice, I think it is plain, that gun-powder may *be supposed* to be so light

light as to be rendered intirely uselefs: and for fome purpofes it feems probable, that it would not be the worfe for being even heavier than it is now made. Vents are abfolutely neceffary in fire-arms, and in large pieces the windage muft be confiderable, in order that the bullets, which are not always fo round as they fhould be, may not ftick in the bore; and thofe who have been prefent at the firing of heavy artillery and large mortars with fhot and fhells, muft have obferved, that there is a fenfible fpace of time elapfes between the lighting of the prime and the explofion; and that, during that interval, the flame is continually iffuing out at the vent with a hiffing noife, and with a prodigious velocity, as appears by the height to which the ftream of fire mounts up in the air. It is plain, that this lofs muft be greater in proportion as the fhot that is difcharged is heavier; and I have often fancied, that I perceived a fenfible difference in the time that elapfed between the firing of the prime and the explofion, when bullets were difcharged, and when the piece has been fired with powder only; the time being apparently longer in the former cafe than the latter.

Almoft all the writers upon gun-powder, and particularly thofe of the laft century, gave different recipes for powder that is defigned for different ufes. Thus the French authors mention *poudre à mousquet*, *poudre ordinaire de guerre*, *poudre de chasse*, and *poudre d'artifice*; all of which are compofed of falt petre, fulphur, and charcoal, taken in different proportions. Is it not probable, that this variety in the compofition of powder was originally introduced in confequence of obfervation that one kind of powder was better adapted for particular purpofes than another, or from experiments made on purpofe to afcertain the fact? There is one circumftance that would lead us to fuppofe that that was the cafe.— That kind of pow-

der

der which was designed for great guns and mortars was weaker than those which were intended to be used in smaller pieces: for if there is any foundation for these conjectures, it is certain, that the weakest powder, or the heaviest in proportion to its elastic force, ought to be used to impel the heaviest bullets, and particularly in guns that are imperfectly formed, where the vent is large, and the windage very great.

I am perfectly aware, that an objection may here be made, viz. that the elastic fluid, which is generated from gun-powder, must be supposed to have the same properties very nearly, whatever may be the proportion of the several ingredients, and that therefore the only difference there can be in powder is, that one kind may generate more of this fluid, and another less; and that when it is generated, it acts in the same manner, and will alike escape, and with the same velocity, by any passage it can find. But to this I answer, though the fluid may be the same, as undoubtedly it is, and though its density and elasticity may be the same in all cases at the instant of its generation, yet in the explosion, the elastic and un-elastic parts are so mixed and blended, that I imagine the fluid cannot expand without taking the gross matter along with it, and the velocity with which the flame issues out at the vent is to be computed from the elasticity of the fluid, and the density or weight of the fluid and the gross matter taken together, and not simply from the elasticity and density of the fluid. If antimony in an impalpable powder, or any other heavy body, was intimately mixed with water in a vessel of any kind, and kept in suspension by shaking or stirring them about; and if a hole was opened in the side or bottom of the vessel, the water would not run out without taking the particles of the solid body along with it. And in the same manner I conceive the solid particles that remain

remain after the explosion of gun-powder are carried forward with the generated elastic fluid, and being carried forward retard its motion.— But to return from this digression.

As it appears from these experiments, that the relation of the velocities of bullets to their weights is different from that which Mr. ROBINS's theory supposes, it remains to inquire what the law is which actually obtains. And, first, as the velocities bear a greater proportion to each other than the reciprocal sub-duplicate ratio of the weights of the bullets, let us see how near they come to the reciprocal sub-triplicate ratio of their weights.

	Weight of the bullet.	Velocity of the bullet: Computed. Recip. sub-dup. ratio.	Computed. Error of the theory.	Actual.	Actual. Error of the theory.	Computed. Recip. sub-trip. ratio.
92d	2352	812	——	812	——	812
91st	1754	940	+ 47	893	+ 2	895
90th	1184	1143	+ 128	1017	+ 4	1021
89th	603	1604	+ 380	1224	+ 54	1278
88th	600	1608	+ 368	1240	+ 40	1280
87th	354	2093	+ 805	1288	+ 239	1527
86th	251	2486	+ 1056	1430	+ 282	1712
85th	90	4151	+ 2042	2109	+ 301	2410

Here the velocities computed upon the last supposition appear to agree much better with the experiments than those computed upon Mr. ROBINS's principles; but still there is a considerable difference between the actual and the computed velocities in the three last experiments in the table.

As the powder itself is heavy, it may be considered as a weight that is put in motion along with the bullet; and if we suppose

suppose the density of the generated fluid is always uniform from the bullet to the breech, the velocity of the center of gravity of the powder or (which amounts to the same thing) of the elastic fluid, and the gross matter generated from it will be just half as great as the velocity of the bullet. If therefore we put P to denote the weight of the powder, B the weight of the bullet, and v its initial velocity; then $Bv + \frac{1}{2}Pv = \overline{B + \frac{1}{2}P} \times v$ will express the *momentum* of the charge at the instant when the bullet quits the bore.

If now, instead of ascertaining the relation of the velocities to the weights of the bullets, we add half the weight of the powder to the weight of the bullet, and compute the velocities from the reciprocal sub-triplicate ratio of the quantity $\overline{B + \frac{1}{2}P}$ in each experiment, the table will stand thus:

	Weight of the bullet and half the powder.	Velocity of the bullet. Actual.	Computed.	Error of the theory.
	$\overline{B + \frac{1}{2}P} =$			
92d exp.	$\overline{2352 + 72\frac{1}{2}}$	812	812	——
91st	$\overline{1754 + 72\frac{1}{2}}$	893	892	− 1
90th	$\overline{1184 + 72\frac{1}{2}}$	1017	1011	− 6
89th	$\overline{603 + 72\frac{1}{2}}$	1224	1243	+ 19
88th	$\overline{600 + 72\frac{1}{2}}$	1240	1245	+ 5
87th	$\overline{354 + 72\frac{1}{2}}$	1288	1449	+ 161
86th	$\overline{251 + 72\frac{1}{2}}$	1430	1589	+ 159
85th	$\overline{90 + 72\frac{1}{2}}$	2109	1999	− 110

The agreement between the actual and computed velocities is here very remarkable, and particularly in the five first experiments, which are certainly those upon which the greatest dependence may be placed.

And hence we are enabled to determine the natures of the *mn*, and *gf* (fig. 16); for since B (which expresses the weight of the bullet) is as the length taken from A towards B in the figure in the several experiments; and as the velocities are as the lines drawn perpendicular to the line AB from the places where those lengths terminate, as *w*, *u*, &c. ending at the curve *m*, *n*; if we put $a = \frac{1}{2}P$, $x = B$, and $y = wu$; then will the relation of x and y be defined by this equation $\frac{1}{\sqrt{a+x^2}} = y$. And if z be put to denote the line *wr*, and *b*, the recoil when the given charge is fired without any bullet, it will be $\frac{x}{\sqrt{a+x^2}} + b = z$ in the curve *gf*, x being the abscissa, and z the corresponding ordinate to the curve.

In the 92d experiment half the weight of the powder ($=a$) was 72¼ grains; the weight of the bullet was 2352 grains ($=x$); the recoil ($=z$) was 32,25 inches, and with the given charge without any bullet the recoil ($=b$) was 4,4 inches; if now from these *data*, and the known weight of the bullet in each of the other experiments in this set, the recoil be computed by means of the theorem $\frac{x}{\sqrt{a+x^2}} + b = z$, we shall see how the result of those experiments agrees with this theory; thus,

92d

	Weight of the bullet.	Recoil Actual.	Recoil Computed.	Difference.
92d exp.	2352	32,25	32,25	——
91st	1754	27,18	27,22	+0,04
90th	1184	21,92	21,85	−0,07
89th	603	15,13	15,33	+0,2
88th	600	15,22	15,29	+0,07
87th	354	11,03	11,87	+0,84
86th	251	9,62	10,21	+0,59
85th	90	7,16	7,02	−0,14
84th and 93d	0	4,4	4,4	——

Here the agreement of the actual and computed recoils is as remarkable as that of the actual and computed velocities in the foregoing table.

By the figure 17. may be seen at one view the result of all these experiments and computations. The numbers upon the line AB (as in the fig. 16.) represent the weights of the bullets, while the lines drawn from those numbers perpendicular to AB on each side, and ending at the curves *m*, *n*, are as the velocities of the bullets in the several experiments; the line AB being the axis of the curves, the lengths taken from A to the different numbers towards B ($=x$) the abscissa, and the perpendiculars ($=y$) the corresponding ordinates. The ordinates to the the curve *hn*, are as the velocities computed from the theorem $\frac{1}{\sqrt{a+x^2}}=y$, and the ordinates to the curve *p*, *n* (which is the logarithmic curve, as it is $\frac{1}{\sqrt{x}}=y$) shew the velocities computed upon Mr. ROBINS's principles. The curve *gf* is drawn from

the theorem $\frac{x}{\sqrt{a+x^2}}+b=z$; and the actual recoil is marked upon the ordinates to this curve by large round dots, which in all the experiments, except the 86th and 87th, very nearly coincide with the curve.

In the fig. 18. the numbers upon the line AB, taken from A, denote the different charges of powder used in the course of the experiments, while the ordinates to the curve *cd*, express the velocities of the bullets, with the vent at o. The lines drawn perpendicular from the line AB to the line *ef*, represent the recoil with the several charges of powder, and a leaden bullet; and the portion of those lines that is comprehended between the line AB and the line *gh*, denotes the recoil when the given charge was fired without any bullet.

Having now shewn by experiment the relation of the velocities of bullets to their weights, when care is taken to prevent intirely the loss of force by the escape of the elastic fluid through the vent and by the windage, I shall leave it to mathematicians to determine from those *data* the properties of that fluid.

But, before I take my leave of this subject, I would just observe, that Mr. ROBINS is not only mistaken in the principle he assumes, respecting the relation of the elasticity of the fluid generated from gun-powder to its density, or rather the law of its action upon the bullet as it expands in the bore; but his determination of the force of gun-powder is also erroneous, even upon his own principles: for he determines its force to be 1600 times greater than the mean pressure of the atmosphere; whereas it appears, from the result of the 92d experiment, that its force is at least 1308 times greater than the

mean

mean pressure of the atmosphere, as will be evident to those who will take the trouble to make the computation.

Of an attempt to determine the explosive force of aurum fulminans, *or a comparison between its force and that of gun-powder.*

Having provided myself with a small quantity of this wonderful powder, upon the goodness of which I could depend, I endeavoured to ascertain its explosive force by making use of it instead of gun-powder for discharging a bullet, and measuring, by means of the pendulum, the velocity which the bullet acquired; and concluding, from the tremendous report with which this substance explodes, that its elastic force was vastly greater than that of gun-powder, I took care to have a barrel provided of uncommon strength, on purpose for the experiment. Its length in the bore is 13,25 inches, the diameter of the bore is 0,55 of an inch, and its weight 7 lbs. 2 oz. It is of the best iron, and was made by WOGDON; and the accuracy with which it is finished does credit to the workman.

This barrel being charged with one sixteenth of an ounce (= 27,34 grains) of *aurum fulminans* and two leaden bullets, which, together with the leather that was put about them to make them fit the bore without windage, weighed 427 grains; it was laid upon a chaffing-dish of live coals, at the distance of about 10 feet from the pendulum, and against the center of the target of the pendulum the piece was directed.

Having secured the barrel in such a manner that its direction should not alter, I retired to a little distance, in order to be out of danger in case of an accident, where I waited in anxious expectation the event of the explosion.

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I had remained in this situation some minutes, and almost despaired of the experiment's succeeding, when the powder exploded, but with a report infinitely less than what I expected, the noise not greatly exceeding the report of a well-charged wind gun; and it was not till I saw the pendulum in motion that I could be persuaded that the bullets had been discharged. I found, however, upon examination, that nothing was left in the barrel, and from the great number of small particles of revived metal that were dispersed about, I had reason to think that all the powder had exploded.

The bullets struck the pendulum nearly in the center of the target, and both of them remained in the wood: and I found, upon making the calculation, that they had impinged against it with a velocity of 428 feet in a second.

If we now suppose that the force of *aurum fulminans* arises from the action of an elastic fluid that is generated from it in the moment of its explosion, and that the elasticity of this fluid, or rather the force it exerts upon the bullet as it goes on to expand, is always as its density, or inversely as the space it occupies; then, from the known dimensions of the barrel, the length of the space occupied by the charge (which in this experiment was 0,47 of an inch), and the weight and velocity of the bullets, the elastic force of this fluid at the instant of its generation may be determined: and I find, upon making the calculation upon these principles, that its force turns out 307 times greater than the mean elastic force of common air.

According to Mr. ROBINS's theory, the elastic force of the fluid generated from gun-powder in its inflammation is 1,000 times greater than the mean pressure of the atmosphere; the force of *aurum fulminans*, therefore, appears to be to that of gun-powder as 307 is to 1,000, or as 4 is to 13 very nearly.

Of the specific gravity of gun-powder.

To determine the specific gravity of gun-powder I made use of the following method. A large glass bucket, with a narrow mouth, being suspended to one of the arms of a very nice balance, and exactly counter-poised by weights put in the opposite scale, it was filled first with government powder poured in lightly, then with the same powder shaken well together, afterwards with powder and water together, and lastly with water alone, and in each case the contents of the bucket were very exactly weighed.

The specific gravity of gun-powder, as determined from these experiments, is as follows:

Specific gravity of rain water - -	1,000
Government powder, as it lies light in a heap, mixed with air - - - -	0,836
Government powder well shaken together -	0,937
The solid substance of the powder - -	1,745

Hence it appears, that a cubic inch of government powder shaken well together weighs just 243 grains; that a cubic inch of solid powder would weigh 442 grains; and, consequently, that the interstices between the particles of the powder, as it is granted for use, are nearly as great as the spaces which these particles occupy.

MIS-

MISCELLANEOUS EXPERIMENTS.

Of some unsuccessful attempts to increase the force of gun-powder.

It has been supposed by many, that the force of steam is even greater than that of gun-powder; and that if a quantity of water, confined in the chamber of a gun, could at once be rarified into steam, it would impel a bullet with prodigious velocity. Several attempts have been made to shoot bullets in this manner; but I know of none that have succeeded; at least so far as to render it probable that water can ever be substituted in the room of gun-powder for military purposes, as some have imagined.

The great difficulty that attends making these experiments lies in finding out a method by which the water can at once be rarified, and converted into elastic steam; and it occurred to me, that possibly that might be effected by means of gun-powder, by confining a small quantity of water in some very thin substance, and surrounding and inclosing it with powder, and afterwards setting fire to the charge. The method I took to do this was as follows. Having procured a number of air bladders of very small fishes, I put different quantities of water into them from the size of a small pea to that of a small pistol bullet, and tying them up close with some very fine thread, I hung up these little globules in the open air till they were quite dry on the outside. I then provided a number of cartridges made of fine paper, and filled them with a known quantity of powder, equal to the customary charge for a common horseman's pistol; and having loaded such a pistol with one of them

and

and a fit bullet, I laid it down upon the ground, and directing it against an oaken plank that was placed about six feet from the muzzle, I fired it off by a train, and carefully observed the recoil, and also the penetration of the bullet. I then took several of the filled cartridges that remained, and pouring out part of the powder, I put one or more of the little bladders filled with water in the center of the cartridge, and afterwards pouring back the remaining part of the charge, confined the water in the midst of the powder.

With these cartridges and a fit bullet, the pistol was successively loaded, and being placed upon the ground as before, and fired by a train, the recoil, and the penetration of the bullets were observed; and I constantly found, that the force of the charge was very sensibly diminished by the addition of the globule of water, and the larger the quantity of water was that was thus confined, the less was the effect of the charge; neither the recoil of the pistol, nor the penetration of the bullet, being near equal to what they were when the given quantity of powder was fired without the water; and the report of the explosion appeared to be lessened in a still greater proportion than the recoil or penetration.

Concluding that this diminution of the force of the charge arose from the bursting of the little bladder, and the dispersion of the water among the powder before it was all inflamed, by which a great part of it was prevented from taking fire, I repeated the experiments with highly rectified spirits of wine instead of water; but the result was nearly the same as before: the force of the charge was constantly and very sensibly diminished. I afterwards made use of etherial oil of turpentine, and then of small quantities of quicksilver; but still with no

better ſucceſs. Every thing I mixed with the powder, inſtead of increaſing, ſerved to leſſen the force of the charge.

Theſe trials were all made ſeveral months before I began the courſe of my experiments upon gun-powder, which I have already given an account of; and though they were altogether unſucceſsful, yet I reſumed the inquiry at that time, and made ſeveral new experiments, with a view to find out ſomething that ſhould be ſtronger than powder, or which, when mixed with it, ſhould increaſe its force.

It is well known, that the elaſtic force of quickſilver converted into vapour is very great; this ſubſtance I made uſe of in my former trials, as I have juſt obſerved, but without ſucceſs. I thought, however, that the failure of that attempt might poſſibly be owing to the quickſilver being too much in a body, by which means the fire could not act upon it to the greateſt advantage; but that if it could be divided into exceeding ſmall particles, and ſo ordered that each particle might be completely ſurrounded by, and expoſed to, the action of the flame of the powder, it would be very ſoon heated, and poſſibly might be converted into an elaſtic ſteam or vapour, before the bullet could be ſenſibly removed from its place. To determine this point I mixed 20 grains of æthiops mineral very intimately with 145 grains of powder, and charging the piece with this compound, it was loaded with a fit bullet and fired; but the force of the charge was leſs than that which the powder alone would have exerted, as appears by comparing the 76th and 77th experiments with the 79th.

Common *pulvis fulminans* is made of one part of ſulphur, two parts of ſalt of tartar, and three parts of nitre; and if we may judge by the report of the exploſion, the elaſtic force of this compound is conſiderably greater than that of gun-powder.

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I was willing to ſee the effect of mixing ſalt of tartar with gun-powder, and accordingly having provided ſome of this alkaline ſalt in its pureſt ſtate, thoroughly dry, and in a fine powder, I mixed 20 grains of it with 145 grains of gun-powder; and upon diſcharging a bullet with the mixture, I found that the alkaline ſalt had conſiderably leſſened the force of the powder. See experiment N° 78.

I next made uſe of *ſal ammoniacum.* That ſalt has been found to produce a very large quantity of elaſtic air, or vapour, when expoſed to heat under certain circumſtances; but when 20 grains of it were mixed with a charge of gun-powder, inſtead of adding to its force, it diminiſhed it very ſenſibly. See the 80th experiment.

Moſt, if not all, the metals, are thought to produce large quantities of air when they are diſſolved in proper *menſtrua*, and particularly braſs, when it is diſſolved in ſpirit of nitre. Deſirous of ſeeing if this could be done by the flame, or acid vapour of fired powder, I mixed 20 grains of braſs in a very fine powder, commonly called braſs duſt (being the ſmall particles of this metal that fly off from the wheel in ſharpening pins), with 145 grains of powder, and with this compound and a fit bullet I loaded my barrel and diſcharged it; but the experiment (N° 81.) ſhewed, that the force of the powder was not increaſed by the addition of the braſs duſt, but the contrary.

It ſeems probable, however, that neither braſs duſt nor æthiops mineral are of themſelves capable of diminiſhing the force of gun-powder in any conſiderable degree, otherwiſe than, by filling up the interſtices between the grains, and obſtructing the paſſage of the flame, and ſo impeding the progreſs of the inflammation. And hence it appears, how earthy particles and

impurities of all kinds are ſo very detrimental to gun-powder. It is not that they deſtroy or alter the properties of any of the bodies of which the powder is compoſed, but ſimply, that by obſtructing the progreſs of the inflammation, they leſſen its force, and render it of little or no value. Too much care, therefore, cannot be taken in manufacturing powder to free the materials from all heterogeneous matter.

Of an attempt to ſhoot flame inſtead of bullets.

Having often obſerved paper and other light bodies to come out of great guns and ſmall arms inflamed, I was led to try if other inflammable bodies might not be ſet on fire in like manner, and particularly inflammable fluids; and I thought if this could be effected, it might be poſſible to project ſuch ignited bodies by the force of the exploiion, and by that means communicate the fire to other bodies at ſome conſiderable diſtance; but in this attempt I failed totally. I never could ſet dry tow on fire at the diſtance of five yards from the muzzle of my barrel. I repeatedly diſcharged large wads of tow and paper, thoroughly ſoaked in the moſt inflammable fluids, ſuch as *alkohol*, *ætherial ſpirit of turpentine*, *balſam of ſulphur*, &c.; but none of them were ever ſet on fire by the exploſion. Sometimes I diſcharged three or four ſpoonfuls of the inflammable fluid, by interpoſing a very thin wad of cork over the powder, and another over the fluid; but ſtill with no better ſucceſs. The fluid was projected againſt the wall as before, and left a mark where it hit; but it never could be made to take fire; ſo I gave up the attempt. If it had ſucceeded, probably it would have turned out one of the moſt important diſcoveries in the art of war that have been made ſince the invention of gun-powder.

Fig. 1.

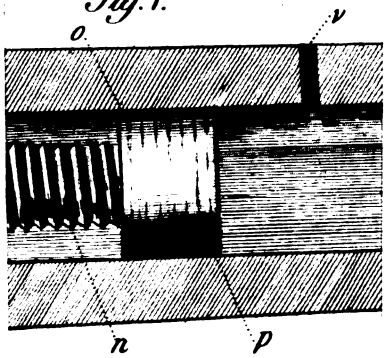


Fig. 4.

Fig. 2.

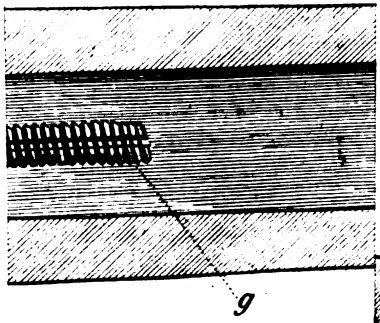
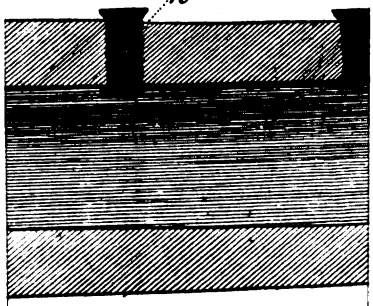


Fig. 3.



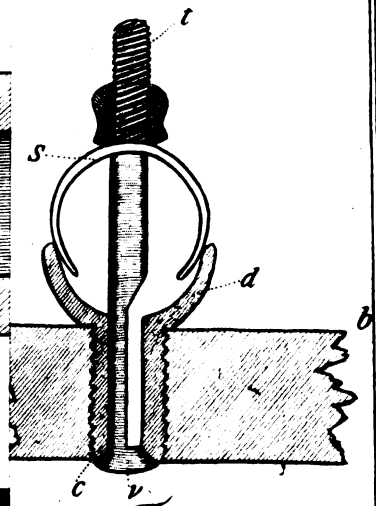


Fig. 5.

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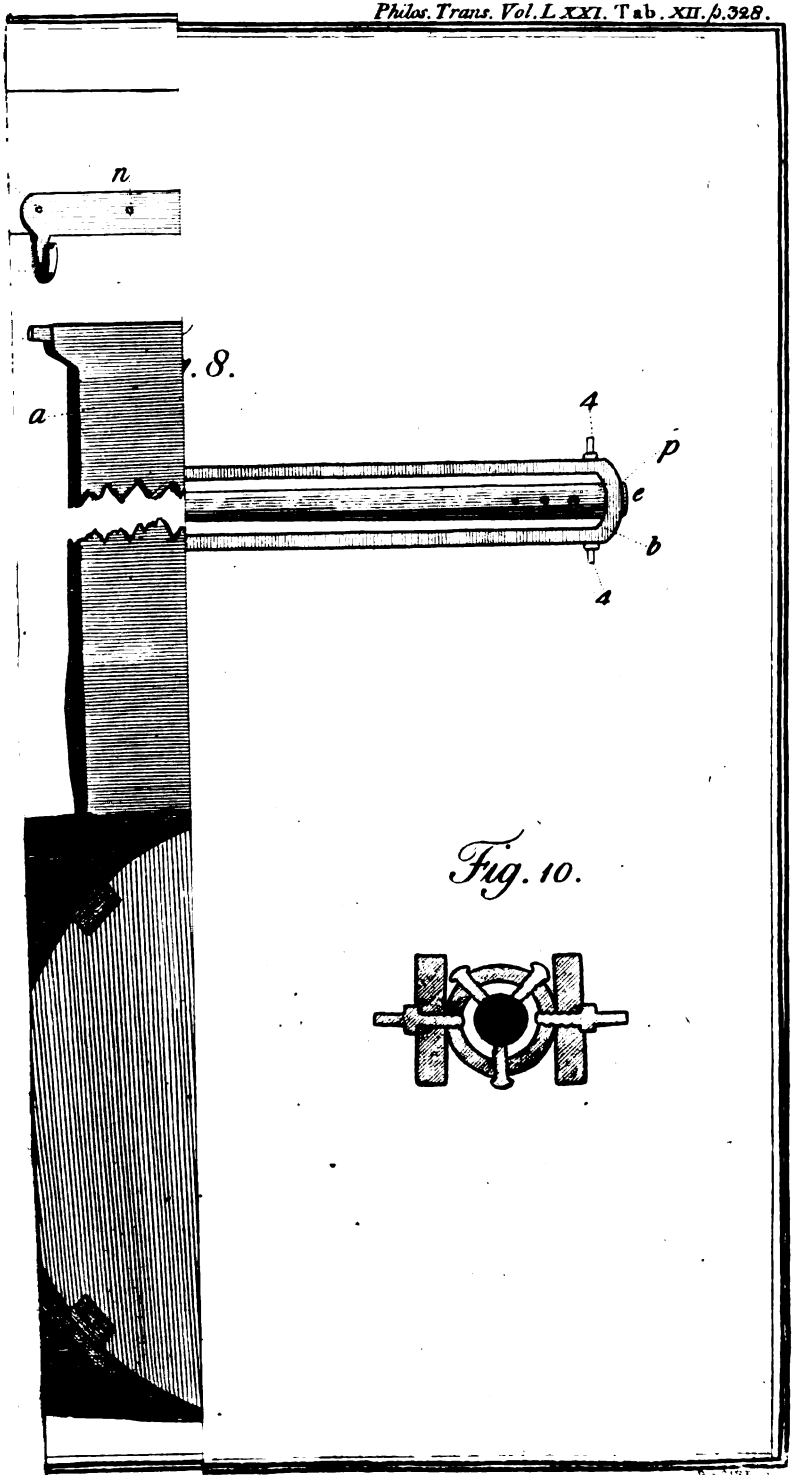
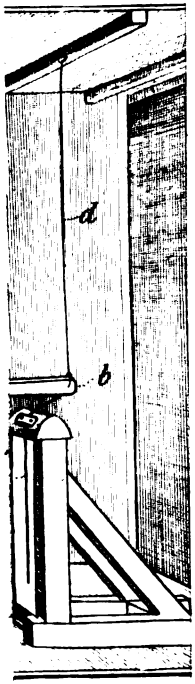
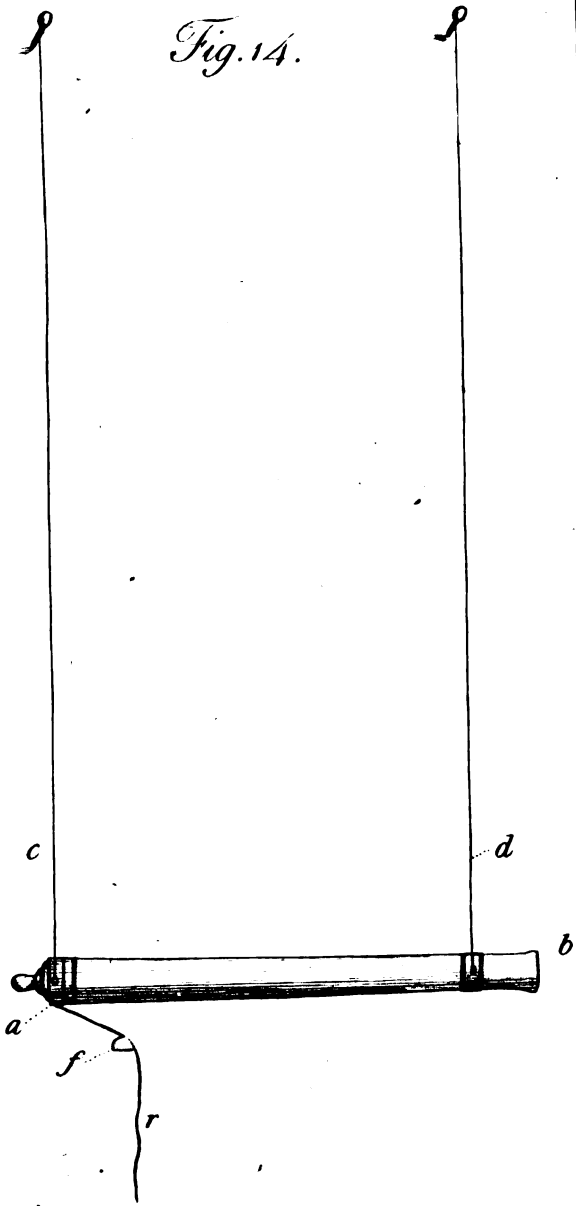


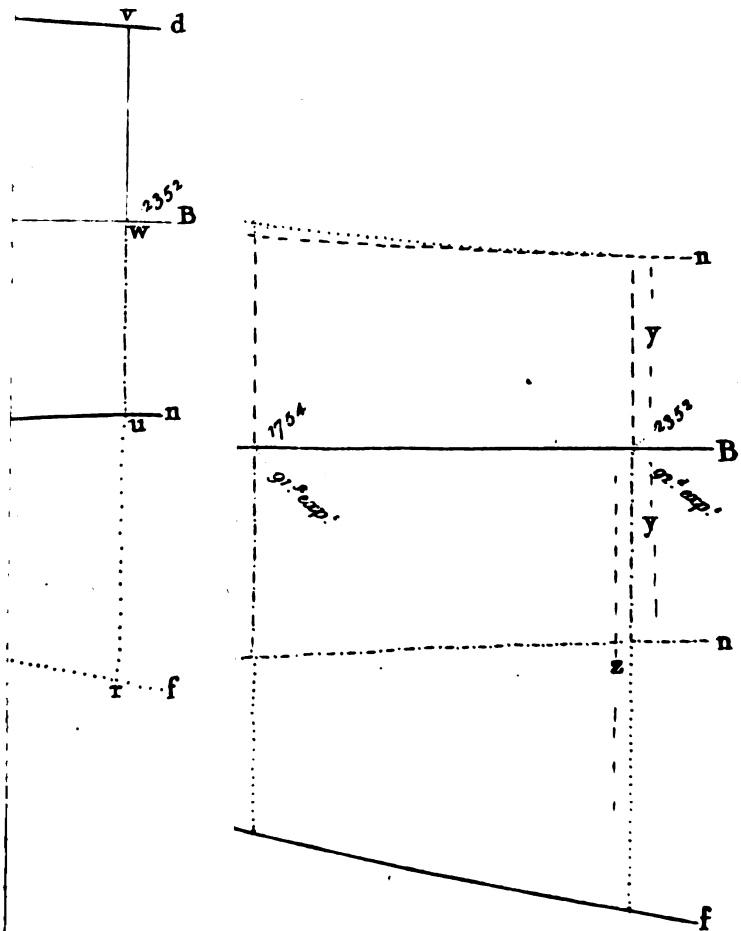
Fig. 8.

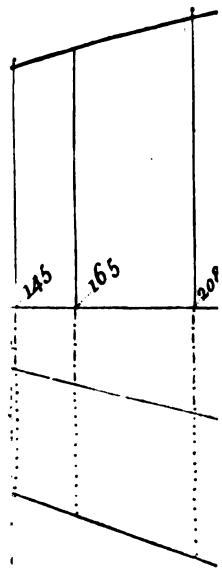
Fig. 10.

Fig. 14.









XVI. *Account of a luminous Appearance in the Heavens. By Mr.* Tiberius Cavallo, *F. R. S. in a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read April 5, 1781.

I TAKE the liberty to ſend you an account of a luminous appearance obſerved laſt night in the heavens, which ſeems to be very ſingular in its nature, and quite diſtinct from the *aurora borealis.*

At about half an hour paſt nine yeſterday evening, being the 27th of March, a white light began to be ſeen in the ſky, which became gradually more and more denſe till ten o'clock, at which time it formed a compleat luminous arch from eaſt to weſt. Of this I have been informed by others; but at a quarter paſt ten I went out of the houſe and obſerved it myſelf. At that time it appeared to be an arch of about ſeven or eight degrees in breadth extended from eaſt to weſt, or, as ſome of my friends imagined, in the direction of eaſt by north to weſt by ſouth. Its weſtern part quite reached the horizon; but the eaſtern part of the arch ſeemed to begin at about 50° or 60° above the horizon. It did not paſs through the zenith but at about 8° or 10° ſouthward of it, and it was nearly perpendicular to the horizon.

The whiteneſs of this arch was much denſer than that any *aurora borealis* I ever obſerved, though it did not caſt ſo much light upon the terreſtrial objects. Towards the middle it was ſo denſe, that the ſtars over which it paſſed were

 eclipſed;

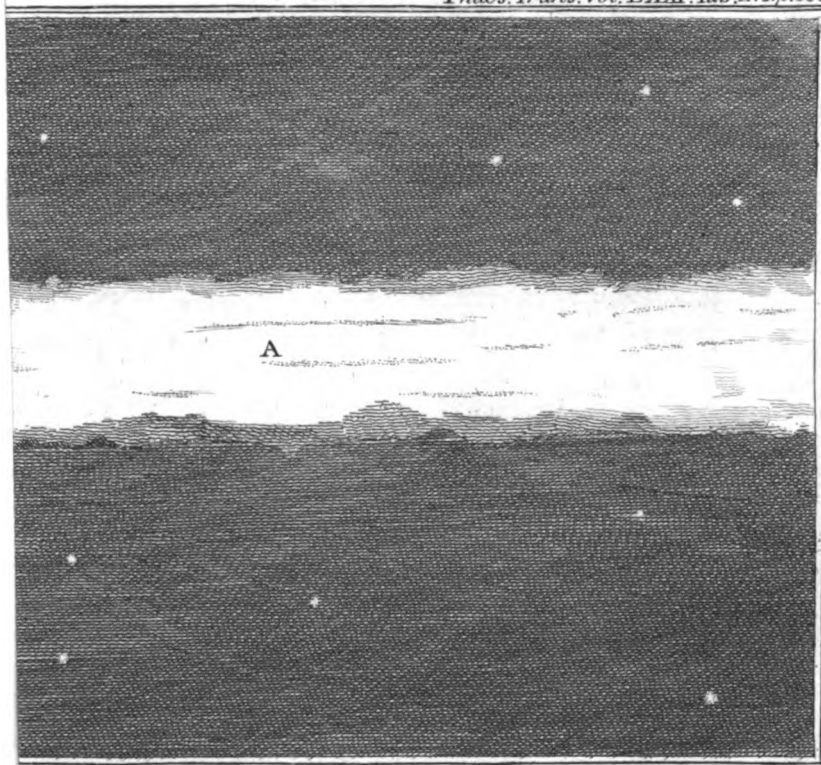
eclipſed; but the ſides of this luminous arch were more faint and tranſparent.

The incloſed drawing ſhews its appearance at about half an hour paſt ten o'clock. At about three quarters paſt ten it began to loſe its brightneſs, firſt at A, and then vaniſhed gradually, ſo that at eleven o'clock none of it could be perceived. As ſoon as any part of this arch loſt its denſe whiteneſs, the ſtars appeared through it quite diſtinct, ſo that it could not be a cloud. The light alſo ſeemed to vaniſh without change of place; for it did not appear to be diſperſed through the ſky, or to be driven in any direction.

This extraordinary appearance to me ſeemed quite diſtinct from the *aurora borealis* for the following reaſons, *viz.* becauſe it eclipſed the ſtars over which it paſſed; becauſe its light, or rather its white appearance, was ſtationary and not lambent; and becauſe its direction was from eaſt to weſt.

The atmoſphere was in other reſpects very ſerene, the ſtars ſhining very bright, and no cloud appearing. The northern light was exceedingly faint, and very low about the northern point of the horizon. The wind was nearly north-eaſt, and it could be juſt perceived in the ſtreets.

I am, &c,



Baire Sc.

XVII. *Account of an Earthquake at* Hafodunos *near* Denbigh. *By* John Lloyd, *Esq. F. R. S. in a Letter to Sir* Joseph Banks, *Bart. P. R. S.*

Read April 5, 1781.

Hafodunos near Denbigh,
Dec. 31, 1781.

DEAR SIR,

UPON the 29th day of last August, at 8 h. 37′ 30″, I was sitting on my * bed-side, and heard a rumbling noise, as if at a distance: the sound seemed to approach me, and when it was greatest the bed rocked and shook so much that I could scarcely keep my seat. I could have no doubt of its being an earthquake, and instantly looked at my barometer, which is of Mr. DE LUC's construction; it stood at 29.57 inches. Attached thermometer 65°. The barometer had been stationary nearly for the three preceding days, and did not seem to be affected with the shock. The morning was remarkably fine, and not a single cloud to be seen. Two of my sisters and a gentleman were walking upon the terrace in the garden by the side of a wall: they all perceived the noise, at first as if at a great distance; but when it was greatest they perceived the wall to shake, though they did not observe any agitation under their feet. As they were walking, and observed the spot when

* This house is built upon the side of a rock; and my bed-chamber, though up two pair of stairs, is on a ground-floor, the floor is not more than one foot from the solid rock in my bed-chamber.

they

they first heard the sound, and the spot they came to when it ceased, I was enabled to ascertain its duration pretty exactly, and find it to have been from fifteen to eighteen seconds. Its course was nearly from south-east to north-west. Some other persons in our house perceived a double shock; and this has been observed by many who felt it in other places. It was felt at Flint by Mrs. SEAMAN and her daughter, who observed the cups rattled upon the saucers as they sat at breakfast. Mr. PENNANT's family, at Downing, fancied that an empty waggon was coming into the back-court, which is paved. It was strongly felt at Llonrwst by the whole town, and part of a stone wall was flung down. At Carnarvon (which is in the same parallel of latitude as this place, 53° 10′) the shock was very slight. It was perceived in many places about Conway; but not at all by any one in town. Sir HUGH WILLIAMS felt it very strongly at his house near Beaumaris. At our friend Mr. DAVIES's, in that town, a door clapped backwards and forwards several times; and at Lord BULKELEY's seat, Boron-hill, the family were much alarmed, it was so violent. It was strongly felt at Holyhead, and at an eminent solicitor's in the island of Anglesey, the desks before several clerks in his office shook so that they could not write. It was strongly perceived at Mr. FITZMAURICE's, at Lleweny-hall in the Vale of Clwyd, and in several other places in that Vale. All the peninsula in Carnarvonshire called Llun, surrounded by St. George's Channel, was shook very much. There have been two shocks since this I have been describing. Mr. PENNANT felt one; but I was not sensible of either. The times it was felt at differ very much on account of the variations in the several dials from whence the clocks are regulated; but I am very exact as to my own time, having the day preceding the earthquake, and that very day, ascertained

aſcertained my time by equal altitudes, taken with one of Mr. BIRD's aſtronomical quadrants of one foot radius. As every phenomenon of this kind is intereſting, you may, perhaps, wiſh to communicate this account to the Royal Society; which you are welcome to do, if you think it worth the attention of ſo illuſtrious a body.

I have the honour to be, &c.

XVIII. *On the Heat of the Water in the Gulf-ſtream.* By Charles Blagden, *M. D. Phyſician to the Army, F. R. S.*

Read April 26, 1781.

ONE of the moſt remarkable facts obſerved in navigating the ocean, is that conſtant and rapid current which ſits along the coaſt of North America to the northward and eaſtward, and is commonly known to ſeamen by the name of the Gulf-ſtream. It ſeems juſtly attributed to the effect of the trade-winds, which blowing from the eaſtern quarter into the great Gulf of Mexico, cauſe there an accumulation of the water above the common level of the ſea; in conſequence of which, it is conſtantly running out by the channel where it finds leaſt reſiſtance, that is, through the Gulf of Florida, with ſuch force as to continue a diſtinct ſtream to a very great diſtance. Since all ſhips going from Europe to any of the ſouthern provinces of North America muſt croſs this current, and are materially affected by it in their courſe, every circumſtance of its motion becomes an object highly intereſting to the ſeaman, as well as of great curioſity to the philoſopher. An obſervation which occurred to me on the ſpot ſuggeſts a new method of inveſtigating a matter that appears ſo worthy of attention.

During a voyage to America in the ſpring of the year 1776, I uſed frequently to examine the heat of ſea-water newly drawn, in order to compare it with that of the air. We made our paſſage

far to the southward. In this situation, the greatest heat of the water which I observed was such as raised the quicksilver in FAHRENHEIT's thermometer to 77°½. This happened twice; the first time on the 10th of April, in latitude 21° 10′ N. and longitude, by our reckoning, 52° W; and the second time, three days afterwards, in latitude 22° 7′ and longitude 55°; but in general the heat of the sea near the tropic of Cancer about the middle of April was from 76° to 77°.

The rendezvous appointed for the fleet being off Cape Fear, our course, on approaching the American coast, became northwestward. On the 23d * of April the heat of the sea was 74°, our latitude at noon 28° 7′ N. Next day the heat was only 71°; we were then in latitude 29° 12′; the heat of the water, therefore, was now lessening very fast in proportion to the change of latitude. The 25th our latitude was 31° 3′; but though we had thus gone almost 2° farther to the northward, the heat of the sea was this day rather increased, it being 72° in the morning, and 72°½ in the evening. Next day, the 26th of April, at half after eight in the morning, I again plunged the thermometer into sea-water, and was greatly surprised to see the quicksilver rise to 78°, higher than I had ever observed it, even within the tropic. As the difference was too great to be imputed to any accidental variation, I immediately conceived that we must have come into the Gulf-stream, the water of which still retained great part of the heat that it had acquired in the torrid zone. This idea was confirmed by the subsequent regular and quick diminution of the heat: the ship's run for a quarter of an hour had lessened it 2°; the thermometer,

* From the difference between civil and nautical time, it becomes necessary to observe, that the former is always meant in this paper.

at three quarters after eight, being raiſed by ſea-water freſh drawn only to 76°; by nine the heat was reduced to 73°, and in a quarter of an hour more, to 71° nearly: all this time the wind blew freſh, and we were going ſeven knots an hour on a north-weſtern courſe. The water now began to loſe the fine tranſparent blue colour of the Ocean, and to aſſume ſomething of a greeniſh olive tinge, a well-known indication of ſoundings. Accordingly, between four and five in the afternoon ground was ſtruck with the lead at the depth of eighty fathom, the heat of the ſea being then reduced to 69°. In the courſe of the following night and next day, as we came into ſhallower water and nearer the land, the temperature of the ſea gradually ſunk to 65°, which was nearly that of the air at the time.

Unfortunately bad weather on the 26th prevented us from taking an obſervation of the ſun; but on the 27th, though it was then cloudy at noon, we calculated the latitude from two altitudes, and found it to be 33° 26′ N. The difference of this latitude from that which we had obſerved on the 25th, being 2° 23′, was ſo much greater than could be deduced from the ſhip's run marked in the log-book, as to convince the ſeamen that we had been ſet many miles to the northward by the current.

On the 25th at noon, the longitude by our reckoning was 74° W. and I believe the computation to have been pretty juſt; but the ſoundings, together with the latitude, will determine the ſpot where theſe obſervations were made better than any reckoning from the eaſtward. The ſhip's run on the 26th, from nine in the forenoon to four in the afternoon, was about ten leagues on a north-weſt by north courſe; ſoon afterwards

we

we hove-to in order to ſound, and, finding bottom, we went very ſlowly all night, and till noon the next day.

From theſe obſervations, I think, it may be concluded, that the Gulf-ſtream, about the 33d degree of north latitude, and the 76th degree of longitude weſt of Greenwich, is, in the month of April, at leaſt ſix degrees hotter than the water of the ſea through which it runs. As the heat of the ſea-water evidently began to increaſe in the evening of the 25th, and as the obſervations ſhew that we were getting out of the current when I firſt tried the heat in the morning of the 26th, it is moſt probable, that the ſhip's run during the night is nearly the breadth of the ſtream meaſured obliquely acroſs; that, as it blew a freſh breeze, could not be much leſs than twenty-five leagues in fifteen hours, the diſtance of time between the two obſervations of the heat, and hence the breadth of the ſtream may be eſtimated at twenty leagues. The breadth of the Gulf of Florida, which evidently bounds the ſtream at its origin, appears by the charts to be two or three miles leſs than this, excluding the rocks and ſand-banks which ſurround the Bahama Iſlands, and the ſhallow water that extends to a conſiderable diſtance from the Coaſt of Florida; and the correſpondence of theſe meaſures is very remarkable, ſince the ſtream, from well-known principles of hydraulics, muſt gradually become wider as it gets to a greater diſtance from the channel by which it iſſues.

If the heat of the Gulf of Mexico was known, many curious calculations might be formed by comparing it with that of the current. The mean heat of Spaniſh-town and Kingſton in Jamaica ſeems not to exceed 81°*; that of St. Domingo on

* Hiſtory of Jamaica, London, 1774, vol. III. p. 652, 653. The different obſervations of the heat recorded in that work do not agree together; but thoſe adopted here are taken from that ſeries which appeared to me the moſt correct.

the

the sea-coast may be estimated at the same from Mons. GODIN's observations *; but as the coast of the continent which bounds the gulf to the westward and southward is probably warmer, perhaps a degree or two may be allowed for the mean temperature of the climate over the whole bay: let it be stated at 82° or 83°. Now there seems to be great probability in the supposition that the sea, at a certain comparatively small distance below its surface, agrees in heat pretty nearly with the average temperature of the air during the whole year in that part; and hence it may be conjectured, that the general heat of the water, as it issues out of the bay to form the stream, is about 82° †, the small variations of temperature on the surface not being sufficient to affect materially that of the general mass. At the tropic of Cancer I found the heat to be 77°; the stream, therefore, in its whole course from the gulf of Florida, may be supposed to have been constantly running through water from 4° to 6° colder than itself, and yet it had lost only 4° of

* Mons. GODIN's experiments upon the pendulum were made at the Petit Goave. They continued from the 24th of August to the 4th of September, and the average heat during that time was such as is indicated by 25° of Mons. DE REAUMUR's thermometer (see Mem. Acad. Scienc. 1735, p. 517.). According to Mons. DE LUC's calculation (see Modifications de l'Atmospere, vol. I. p. 378.) the 25th degree of Mons. DE REAUMUR's *true* thermometer answers to about the 85th of FAHRENHEIT's; but the average heat in Jamaica during the months of August and September is also 85°; hence we may conclude, that the mean heat for the whole year is nearly the same on the sea-coasts in both islands.

† The lowest calculation of the mean temperature of the gulf is preferred on this occasion, because of the constant influx of new water from the Atlantic Ocean produced by the trade-winds; which water not having been near any land must, I think, be sensibly cooler than that which has remained some time inclosed in the bay. On this subject the observations made by ALEXANDER DALRYMPLE, Esq. relative to the heat of the sea near the Coast of Guinea, ought to be consulted (see Phil. Trans. vol. LXVIII. p. 394, &c.).

heat,

heat, though the ſurrounding water where I obſerved it was 10° below the ſuppoſed original temperature of the water which forms the current. From this ſmall diminution of the heat, in a diſtance, probably of 300 miles, ſome idea may be acquired of the vaſt body of fluid which ſets out of the gulf of Mexico, and of the great velocity of its motion. Numerous obſervations on the temperature of this ſtream, in every part of it, and at different ſeaſons of the year, compared with the heat of the water in the ſurrounding ſeas, both within and without the tropic, would, I apprehend, be the beſt means of aſcertaining its nature, and determining every material circumſtance of its movement, eſpecially if the effect of the current in puſhing ſhips to the northward is carefully attended to, at the ſame time with the obſervations upon its heat.

On the 25th of September 1777, as the ſhips which had tranſported Sir WILLIAM HOWE's army up Cheſapeak Bay were returning toward the Delaware, with the ſick and ſtores, they were overtaken, between Cape Charles and Cape Hinlopen, by a violent gale of wind; which, after ſome variation, fixed ultimately at N.N.E. and continued five days without intermiſſion. It blew ſo hard that we were conſtantly loſing ground, and driving to the ſouthward: we alſo purpoſely made ſome *eaſting* to keep clear of the dangerous ſhoals which lie off Cape Hatteras.

The 28th at noon our latitude was 36° 40′ N. and the heat of the ſea all day about 65°. On the 29th our latitude was 36° 2′; we had, therefore, in the courſe of theſe twenty-four hours, been driven by the wind 38 nautical miles to the ſouth-

ward:

ward: the temperature of the ſea continued nearly at 65°. Next day, the 30th, our latitude at noon was 35° 44′, only 18 miles farther to the ſouthward, though in the opinion of the ſeamen aboard, as well as my own, it had blown at leaſt as hard on this as any of the preceding days, and we had not been able to carry more ſail; conſequently it may be concluded, that ſome current had ſet the ſhip 20 miles to the northward. To know whether this was the Gulf-ſtream, let us conſult the thermometer. At half after nine in the forenoon of this day the heat of this water was 76°, no leſs than *eleven* degrees above the temperature of the ſea before we came into the current!

Towards evening the wind fell, and we ſtood N.W. by N. cloſe-hauled. As the ſea ſtill ran very high, and the ſhip ſcarcely went above two knots an hour, we did not make leſs than three points of lee-way on this tack; the courſe we made good, therefore was W.N.W. which, on the diſtance run by noon next day, gave us about ſixteen miles of *northing*; but that day, the 1ſt of October, our latitude was 36° 22′, 38 miles farther to the north than we had been the day before; the difference, 22 miles, muſt be attributed to the Gulf-ſtream. This, however, is only part of the effect which the current would have produced upon the ſhip if we had continued in it the whole four and twenty hours; for, though we were ſtill in the ſtream at five in the afternoon of the 30th, as appeared by the heat of the water being then above 75°, and at eight in the evening the heat being ſtill 74°, yet by ſeven next morning we were certainly got clear of it, the heat of the ſea being then reduced to its former ſtandard of 65°. On this occaſion, therefore, we did not croſs the ſtream, but having fallen-in with it obliquely

obliquely on the weſtern ſide, we puſhed out again on the ſame ſide as ſoon as the gale abated.

Theſe obſervations having been made three degrees to the northward of my former ones, it is curious to obſerve, that the heat of the Gulf-ſtream was about 2° leſs. The ſeaſons of the year, indeed, were very different; but, perhaps, under ſuch circumſtances that their effects were nearly balanced. In the latter obſervations the meridian altitude of the ſun was leſs; but then a hot ſummer preceded them: whereas in the former, though the ſun's power was become very great, yet the winter had been paſt but a ſhort time. Calculating upon this proportion we may be led to ſuſpect, that about the 27th degree of latitude, which is as ſoon as the ſtream has got clear of the gulf of Florida, it begins ſenſibly to loſe its heat from 82°, the ſuppoſed temperature of the gulf of Mexico, and continues to loſe it at the rate of about 2° of FAHRENHEIT's ſcale to every 3° of latitude, with ſome variation, probably as the ſurrounding ſea, and the air, are warmer or colder at different ſeaſons of the year.

The preceding facts had made me very deſirous of obſerving the heat of the Gulf-ſtream on my paſſage homeward; but a violent gale of wind, which came on two days after we had ſailed from Sandy Hook, diſabled every perſon aboard, who knew how to handle a thermometer, from keeping the deck. The maſter of the ſhip, however, an intelligent man, to whom I had communicated my views, aſſured me, that on the ſecond day of the gale the water felt to him remarkably warm; we were then near the 70th degree of weſt longitude. This agrees very well with the common remark of ſeamen, who alledge, that they are frequently ſenſible of the Gulf-ſtream off Nan-

tucket ſhoals, a diſtance of more than 1000 miles from the gulf of Florida! According to the calculation I have before adopted of a loſs of two degrees of heat for every 3° of latitude, the temperature of the Gulf-ſtream here would be nearly 73°; the difference of which from 59°, the heat that I obſerved in the ſea-water both before and after the gale, might eaſily be perceived by the maſter of the veſſel. This was in the winter ſeaſon, at the end of December.

An opinion prevails among ſeamen, that there is ſomething peculiar in the weather about the Gulf-ſtream. As far as I could judge, the heat of the air was conſiderably increaſed by it, as might be expected; but whether to a degree or extent ſufficient for producing any material changes in the atmoſphere muſt be determined by future obſervations.

Perhaps other currents may be found which, iſſuing from places warmer or colder than the ſurrounding ſea, differ from it in their temperature ſo much as to be diſcovered by the thermometer. Should there be many ſuch, this inſtrument will come to be ranked among the moſt valuable at ſea; as the difficulty of aſcertaining currents is well known to be one of the greateſt defects in the preſent art of navigation.

In the mean time, I hope the obſervations which have been here related are ſufficient to prove, that in croſſing the Gulf-ſtream very eſſential advantages may be derived from the uſe of the thermometer: for if the maſter of a ſhip, bound to any of the ſouthern provinces of North America, will be careful to try the heat of the ſea frequently, he muſt diſcover very accurately his entrance into the Gulf-ſtream, by the ſudden increaſe of the heat; and a continuance of the ſame experiments will ſhew him, with equal exactneſs, how long he remains in it. Hence he will always be able to make a proper allowance

for the number of miles that the ſhip is ſet to the northward, by multiplying the time into the velocity of the current. Though this velocity is hitherto very imperfectly known, for want of ſome method of determining how long the current acted upon the ſhips, yet all uncertainty ariſing from thence muſt ſoon ceaſe, as a few experiments upon the heat of the ſtream, compared with the ſhip's run checked by obſervations of the latitude, will aſcertain its motion with ſufficient preciſion. From differences in the wind, and perhaps other circumſtances, it is probable, that there may be ſome variations in the velocity of the current; and it will be curious to obſerve, whether theſe variations may not frequently be pointed out by a difference in its temperature; as the quicker the current moves, the leſs heat is likely to be loſt, and conſequently the hotter will the water be. In this obſervation, however, the ſeaſon of the year muſt always be conſidered; partly, becauſe it may, perhaps, in ſome degree affect the original temperature of the water in the gulf of Mexico; but principally, becauſe the actual heat of the ſtream muſt be greater or leſs in proportion as the tract of the ſea through which it has flown was warmer or colder. In winter, I ſhould ſuppoſe, that the heat of the ſtream itſelf would be rather leſs than in ſummer; but that the difference between it and the ſurrounding ſea would be much greater; and I can conceive that, in the middle of ſummer, though the ſtream had loſt very little of its original heat, yet the ſea might, in ſome parts, acquire ſo nearly the ſame temperature, as to render it ſcarcely poſſible to diſtinguiſh by the thermometer when a ſhip entered into the current.

Beſides the convenience of correcting a ſhip's courſe, by knowing how to make a proper allowance for the diſtance ſhe is ſet to the northward by the current, a method of determining

with certainty when ſhe enters into the Gulf-ſtream is attended with the further ineſtimable advantage of ſhewing her place upon the ocean in the moſt critical ſituation: for, as the current ſets along the coaſt of America at no great diſtance from ſoundings, the mariner, when he finds this ſudden increaſe of heat in the ſea, will be warned of his approach to the coaſt, and will thus have timely notice to take the neceſſary precautions for the ſecurity of his veſſel. As the courſe of the Gulf-ſtream comes more to be accurately known, from repeated obſervations of the heat and latitudes, this method of determining the ſhip's place will be proportionably more applicable to uſe. And it derives additional importance from the peculiar circumſtances of the American coaſt, which, from the mouth of the Delaware to the ſouthernmoſt point of Florida, is every where low, and beſet with frequent ſhoals, running out ſo far into the ſea that a veſſel may be aground in many places where the ſhore is not to be diſtinguiſhed even from the maſt-head. The gulf-ſtream, therefore, which has hitherto ſerved only to increaſe the perplexities of ſeamen, will now, if theſe obſervations are found to be juſt in practice, become one of the chief means of their preſervation upon that dangerous coaſt.

XIX. *Account of the Appearance of the Soil at opening a Well at* Hanby *in* Lincolnſhire. *In a Letter from Sir* Henry C. Engleſield, *Bart. F. R. and A. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 3, 1781.

DEAR SIR,

THE appearance of the ſoil which fell under my own inſpection, on opening a well at Hanby, the ſeat of Sir C. BUCK, in Lenton pariſh, Lincolnſhire, being, as far as I can recollect, quite ſingular, I hope you will not think this account of it unworthy the attention of the Society.

The ſpot on which the well was ſunk is nearly on a level with Lincoln Heath, and of courſe high ground compared with the fen, which is diſtant from it above ſix miles. The ſoil was uniformly a blue clay, in parts rather inclining to a ſhaly ſtructure, and contained many caſts of tellinæ, a very little pyrites, and ſome few ſmall, but very elegant, belemnites. Theſe are all the uſual foſſils of clay; but what I think without example is, that through the whole maſs of clay were interſperſed nodules of pure chalk, evidently rounded by long attrition, and of all ſizes from that of a pea to a child's head.

They lay in no ſort of order that I could find. How deep this appearance might have continued I cannot determine, but no water having been found at the depth of thirty feet, the

trial was given up, as the expence would have exceeded the advantage propoſed. A ſpecimen of the chalk is herewith exhibited to the Society.

I muſt add, that in all the environs there is not the leaſt trace of chalk in any form whatever that I could diſcover or hear of.

I am, &c.

XX. *Astronomical Observations made by* Nathaniel Pigott, *Esq. F. R. S. Foreign Member of the Academies of* Brussels *and* Caen, *and Correspondent of the Academy of Sciences at* Paris. *Communicated by Sir* Henry C. Englefield, *Bart. F. R. and A. S.*

Read May 3, 1781.

HAVING given in the Philosophical Transactions, vol. LXVI. for the year 1776, and vol. LXVIII. part II. for 1778, an ample description of the excellent astronomical instruments in my possession, it will be needless to say any thing here on that subject, further than that the following observations were made with them.

During the summer of 1777, part of which I spent with Lady WIDDRINGTON, at her house named Wickhill, about a mile from Stow on the Would, Gloucestershire, I determined, by six observations of ♃'s satellites, compared to correspondent ones made on the same days, Wickhill W. of Greenwich 1° 29′ 45″. It is proper to add, this is likewise the longitude of Stow, it being under the same meridian, or very nearly so, as Wickhill.

In 1778 and 1779 I observed in Glamorganshire; and by thirty-five meridian observations of the sun and stars, all agreeing within 12″ from the mean, I determined the latitude of my observatory at Frampton House 51° 25′ 1″ N.

Frampton

Frampton-houſe lies between Cowbridge and Lantwit; about four miles ſouth of the former, and one mile north of the latter, and about two miles from the Briſtol channel; is nearly under the ſame meridian as Watchet, a market town in Somerſetſhire.

The rocks on the Welch coaſt, which run obliquely ſlanting into the Briſtol channel, render the navigation ſo dangerous, that each year affords the horrid ſpectacle of ſhips wrecked; and here I am ſorry to add, that the barbarous cuſtom of plundering theſe unfortunate veſſels ſtill ſubſiſts in all its inhumanity; at the ſame time it would be injuſtice to the gentlemen of the country to paſs under ſilence their repeated endeavours to check this enormity; but hitherto their efforts have not been attended with much ſucceſs: it is due to humanity to make ſuch bad practices public, in hopes of exciting an inquiry, which juſtice and the honour of the nation loudly call for.

The little that has been ſaid ſuffices to ſhew the expediency of correct maps of this channel. The univerſal opinion of the country is, that to the Somerſetſhire oppoſite coaſt, about Watchet, Purlock, &c. the breadth of the channel is twenty or twenty-one miles. A ſingle glance of the eye ſeems ſufficient to contradict this notion; however, as upon inſpecting my maps I found the diſtances ſet down not greatly different from what report had made them, we meaſured them geometrically, and the reſult gave the channel, not twenty, but little more than thirteen miles broad at the abovementioned places.

Upon the whole it is to be wiſhed, that aſtronomical obſervations, ſufficiently correct, were made on the Somerſetſhire ſide, which might be compared with thoſe I have made on the oppoſite ſhore. It may poſſibly be found, that the towns on the

Engliſh

English coast are placed in the maps too much south, and those in Wales too much north; and hence, perhaps, the too great breadth given to the Bristol channel. This is, at least, the case with the town of Lantwit, which, as I have said, is to the southward of Frampton-house, the latitude of which is 51° 25′ 1″; nevertheless the best and most extensive map * I have been able to procure gives the latitude of Lantwit 51° 29′ 40″, that is to say 4′ 39″ N. of Frampton-house. There may very possibly be particular charts of the Bristol channel more exact; but it is not less true, that the common maps ought to be cleared of such enormous errors.

I determined the difference of meridians between Frampton-house and Greenwich by comparing four immersions and fourteen emersions of ♃'s first and second satellites to corresponding ones made in other observatories. The result is as follows:

App. time.		Immersions.
′	″	
14	11	By an observ. at Greenwich.
13	52	——— at Upsal.
13	54	——— at Paris.
14	6	——— at Oxford.
14	1	the mean.

App. time.		Emersions.
′	″	
13	43	By an observ. at Oxford.
14	2	——— at Greenwich.
13	50	——— N. Almanack.
13	45	——— at Greenwich.
13	46	——— at Oxford.
13	44	——— at Paris.
14	12	——— at Paris.
13	50	——— at Oxford.
14	11	——— at Berlin.
13	42	——— at Oxford.
13	50	——— at Greenwich,
13	59	——— at Oxford.
14	3	——— at Greenwich.
14	7	——— at Greenwich.
13	55	Mean of emersions.
14	1	Mean of immersions.
13	58	by a mean of the means

Frampton-house W. of Greenwich in time, or 3° 29′ 30″ by the equator.

* By JOHN ADAMS. The scale is to minutes of a degree.

Occultations of fixed stars observed at Frampton-house in 1777 and 1778.

App. time. October 21, 1777.

18° 16′ 20″ Imm. δ Pollux; doubtful to 2″ or 3″.

November 15.

6 19 3 Imm. 1st, δ Tauri into light part of ☽: doubtful to 3″.
6 54 22 Imm. 2d, δ Tauri.
7 8 42 Emers. 1st, δ Tauri: dark part of ☽.

November 16.

10 59 27 Imm. ζ Tauri: very good.

July 5, 1778.

9 2 15½ Imm. telescopic star: instantaneous.
9 3 28½ Imm. *vm*: instantaneous.
10 27 8½ Emers. *vm*, from light limb of ☽: sure to 1″ or 2″.

Declination of the needle.

In the beginning of 1778 the declination west of a magnetic needle of four inches made by Mr. DOLLOND, appeared to be 22° 11′.

II. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1780. *By* Thomas Barker, *Esquire.*

Read May 3, 1781.

	Barometer.			Thermometer.						Rain.	Mean rain.	
				In the House.			Abroad.				10 yrs.	44 yrs.
	Highest	Lowest.	Mean.	High.	Low.	Mean	High.	Low.	Mean		71—80	45 yrs. 36—80
Morn.	29,89	28,16	29,39	39	28	34	36½	15½	27	1,013	1,677	1,573
Aftern.				40	29	35	43	22½	32			
Morn.	30,06	28,72	29,49	44	33	37	41	22	31	1,572	1,871	1,378
Aftern.				44½	34	38	46	30½	39			
Morn.	29,94	28,99	29,47	51½	41½	46	49	30	40	1,175	1,355	1,315
Aftern.				53½	43½	47½	61	45	51			
Morn.	29,77	28,40	29,21	53	40	46	52	29½	38	2,727	1,314	1,465
Aftern.				55	41½	46	60	38	48			
Morn.	29,83	28,80	29,48	67	51	55	65	40½	50½	1,201	2,081	1,610
Aftern.				72	52½	57	80½	53	61			
Morn.	29,89	29,26	29,55	65½	52	57½	62½	43	53½	1,920	2,374	2,249
Aftern.				71½	52½	59	80	51	64			
Morn.	29,87	29,21	29,61	69½	58½	63	65	51½	57½	1,566	2,507	2,516
Aftern.				71½	59½	64	83	61	69½			
Morn.	29,81	29,43	29,65	68	61½	64	63	52	58	0,432	2,468	2,247
Aftern.				72	63	66	81½	63	70			
Morn.	29,79	28,70	29,38	70	53	61	61	43	52	3,427	3,142	2,016
Aftern.				73½	53½	63	82	53½	65			
Morn.	29,87	28,20	29,23	58½	46½	51	56	33½	44	3,080	2,975	2,158
Aftern.				59	48	52	64	45½	53			
Morn.	30,00	28,62	29,41	50	38½	43	48½	19½	36	1,461	2,372	1,943
Aftern.				50½	38½	43½	48	31	40			
Morn.	30,08	29,22	29,83	45	35	40	46½	26	35	0,534	1,889	1,740
Aftern.				45½	35	40½	50	30½	38½			
										20,108	26,024	22.210

The three drieſt ſeaſons from one month to twelve are the ſame as in vol. LXI. except the following.

Months						
2			Feb. and Mar. 79	0,370	Jan. and Feb. 79	0,451
3	Jan.—Mar. 79	0,582	Dec. 42—Feb. 43	0,934	Jan.—Mar. 40	0,942
7					Oct. 39—Apr. 40	0,543
10			Oct. 40—July 41	10,174		
12	Sept. 40—Aug. 41	13,427				

The three wetteſt ſeaſons from one month to twelve are entirely different from thoſe in vol. LXI.

Months.						
1	Sept. 74.	8,000	Nov. 70	7,818	May 73	6,843
2	Aug. and Sept. 74	11,910	Oct. and Nov. 70	10,932	Aug. and Sept. 75	10,430
3	July—Sept. 74	15,137	July—Sept. 75	14,508	Oct.—Dec. 70	13,545
4	July—Oct. 75	17,988	June—Sept. 74	17,620	Oct. 70—Jan. 71	14,958
5	July—Nov. 75	21,558	May—Sept. 74	20,762	Aug.—Dec. 70	17,038
6	July—Dec. 75	22,654	April—Sept. 74	22,285	Jun.—Nov. 70	18,978
7	July 75—Jan. 76	25,165	Mar.—Sept. 74	25,013	July 63—Jan. 64	22,902
8	July 75—Feb. 76	28,360	Feb.—Sept. 74	26,959	May—Dec. 73	25,623
9	Jan.—Sept. 74	30,267	July 75—Mar. 76	29,878	May 73—Jan. 74	28,931
10	Dec. 73—Sept. 74	33,164	May 73—Feb. 74	30,877	July 75—April 76	30,765
11	Nov. 73—Sept. 74	36,769	May 73—Mar. 74	33,605	July 75—May 76	32,392
12	Oct. 73—Sept. 74	39,390	May 73—April 74	35,128	May 75—April 76	34,877

The year began with froſt, and was perhaps the ſevereſt winter ſince 1740, but there was not a great deal of ſnow, and in general it was calm. The froſt was not ſo ſteady as it was that winter, there being ſeveral breaks in it; but was very ſharp, and the ice was never intirely gone for nine or ten weeks together from December 22, till near the end of February, when it went away without wet, leaving the ground remarkably light and fine, and the weather grew mild, and continued ſo moſt part of March; but the coldneſs of the ground hindered the graſs from growing greatly till toward the end of the time. The ſeed-

time

time was fine and good, and the grain came up very well, but the firſt three weeks of April were cold, backening, and often froſty. Toward the end it was more ſhowery, warmer and growing, and from that time the ſpring continued to come on, and there were ſo few N.E. winds that ſhips found a difficulty in getting down the channel, which is very unuſual at that time of year, and all the ſpring from the end of February till toward the end of June was very windy, chiefly N.W. and S.W.

In the former part of ſummer there were at times very hot days; but the ſeaſon was oftener cool; many little ſhowers, which in ſome countries were ſo ſmall there was want of rain and graſs, here we did pretty well. The hay-time was fine, but the crop ſmall. The harveſt was exceedingly well got: the barley and oats good, and ſome of the wheat; but the late-ſown was thin through the ſevere winter, and in ſeveral places the wheat was mildewed, which could not be by wet ſuch a year as this; but by this means wheat became three times the price of barley, being 50 and 52 ſhillings a quarter, and barley 16 or 17 ſhillings. From the latter part of July to the beginning of September it was very dry, hot, and burning; much ſcorching ſun, the ground very much burnt up, and great want of water; but the N.E. winds, which came at this time of year inſtead of the ſpring, were ſometimes freſh and cool.

The beginning of September the rains began, and for above two months there was a good deal, with ſuch fine and warm weather, that there was good graſs, a pleaſant autumn, and very few froſty mornings, and the ground, which before was ſo dry, did not get much dirty with it. The wheat ſeed-time was fine, and the weather mild till the middle of November, when

when a hard froſt, with ſnow, made people think of a hard winter; but it grew mild again, was chiefly dark and cloudy, but little rain, and drying a good deal of December; remarkably calm, but near a week's froſt about Chriſtmas.

The ſickly ſeaſons, which began in Auguſt 1779, continued more or leſs all the year; and about the ſame time of the year increaſed again. There were great numbers of fevers and agues, eſpecially in and near the fens, which were very obſtinate, and did not yield to the uſual medicines, but frequently returned again, and hung very long on the patients.

XXII. *Some Calculations of the Number of Accidents or Deaths which happen in consequence of Parturition; and of the Proportion of Male to Female Children, as well as of Twins, monstrous Productions, and Children that are dead-born; taken from the Midwifery Reports of the* Westminster General Dispensary: *with an Attempt to ascertain the Chance of Life at different Periods, from Infancy to Twenty-six Years of Age; and likewise the Proportion of Natives to the rest of the Inhabitants of* London. *In a Letter from* Robert Bland, *M. D. Physician-Man-Midwife to the* Westminster General Dispensary, *to* Samuel Foart Simmons, *M. D. F. R. S.*

Read May 10, 178:.

DEAR SIR,

THE great advantage of hospitals and other similar institutions, in improving and disseminating medical knowledge, is generally acknowledged; but there are other purposes they seem equally calculated to answer, which, though subordinate to the former, may yet deserve attention, as they may throw light upon, and perhaps finally determine, certain political questions, about which various opinions are at present entertained. Thus, though it is known that this city contains persons from various countries, and that a very small portion of its inhabitants are natives; yet the proportion which the latter bear to the aliens can at present only be guessed at.

But

But this queſtion might be reſolved, with a tolerable degree of accuracy, if to the regiſter of the names of the perſons admitted to the ſeveral charities were added the places of their birth. Again, the great mortality of the human ſpecies, particularly in infancy, and the ſmall chance a child has to attain to years of maturity, have been calculated from pariſh-regiſters, bills of mortality, &c. But I do not know, that an attempt has hitherto been made to aſcertain them, by noting the number of children a promiſcuous multitude of women had borne, together with the number they had been able to preſerve.

This, however, is what I have here done; and from it I have attempted to form a table, ſhewing the chance of life at different periods. I am far from pretending that by this mode clear and certain intelligence will be obtained; but in a matter of ſuch moment, I preſume, that any aſſiſtance will be acceptable. Dr. SMELLIE * has curſorily mentioned, for the encouragement of his pupils, the ſmall proportion of the unnatural and laborious births to the natural; but he did not carry his views farther, or point out the proportionate number of conſequent accidents, which might occur to retard or prevent the recovery of the woman, although this is not leſs neceſſary to be known than the former. With a view to theſe, and other uſeful purpoſes, the following regiſter has been kept of the moſt material circumſtances concerning the patients admitted to the midwifery-department of the Weſtminſter General Diſpenſary, from its firſt inſtitution, in the year 1774, to the preſent time; *viz.*

1. The ages of the ſeveral women.
2. The number of children they had borne.
3. The ſexes of the children,

* See SMELLIE's Midwifry, 8vo, p. 195.

4. The number of children they had been able to preſerve.

5. The place or country where they and their huſbands were born.

And after the delivery of the patient I have conſtantly noted,

1. The accidents that attended, or were the conſequences of parturition.

2. The ſexes of the children delivered.

3. The number of twins or triplets.

4. The number of the children that were deficient or monſtrous.

5. The number of the children that were dead-born: and, as the women were enjoined to return their letters as ſoon as they were able to go abroad, I farther intended to have added the proportion of the children who died under four or five weeks; but many of the women neglecting this duty, prevented my information under this head from being ſo compleat as I could have wiſhed. Of thoſe, however, who came, or of whom certain account could be obtained, the number is ſet down.

From the above mentioned regiſter the following tables and accounts have been compoſed; and as the greateſt care and exactneſs were uſed in recording the ſeveral circumſtances, the ſame punctuality has been obſerved in collecting and digeſting them. And that they might be kept as free from error as poſſible, tables for each year were firſt compoſed and compared together; but finding no material variation, I did not think it neceſſary to produce them in that form. My firſt intention was to have given the tables ſimply, and without any explanatory obſervations; but finding I could not introduce all the circumſtances I had noted in my regiſter, as was particularly the caſe with regard to the firſt table, and imagining that

in ſome places they were not perfectly intelligible, without ſome explanation, as for inſtance in the table of the chance of life at different periods, I have ventured to add ſuch occaſional remarks as I think will tend to illuſtrate the ſubject.

As my firſt view was to find the proportion of difficult labours, and of the accidents or deaths that happen in conſequence of child-birth, I ſhall begin with the following TABLE.

Of 1897 women delivered under the care of the Diſpenſary,

63 or 1 in 30 had unnatural labours: in

18 of theſe, or 1 in 105, the children preſented by their feet; in

36 or 1 in 52, the breech preſented; in

8 the arms preſented; and in
1 the funis. } 9*, or 1 in 210.

63

17 women, or 1 in 111, had laborious labours: in

†8 of theſe, or 1 in 236, the heads of the children were leſſened; in

4 a ſingle blade of a forceps was uſed; and in the remaining

5 in which the faces of the children were turned to the pubes, the delivery was at length accompliſhed by the pains.

80 17

80

* In all theſe nine caſes the children were turned.

† Two of theſe women have ſince been delivered of full-ſized healthy children. A third bore a very ſmall and weakly child, who died in two or three days. A fourth

80

1 woman had convulſions about the ſeventh month of her pregnancy, and was delivered a month after of a dead child, and recovered.

1 woman had convulſions during labour; brought forth a live child, and recovered.

*9 women, or 1 in 210, had uterine hæmorrhage before and during labour.

Of theſe 1 died undelivered;
1 died a few hours, and
1 ten days, after delivery, and
6 recovered.
—
9

5 women had the puerperal fever, of whom four died. In one of theſe the placenta was undelivered, and continued ſo to her death.

2 women were ſeized with mania, but recovered in about three months. In

1 woman a ſuppuration took place, ſoon after labour, from the vagina into the bladder and rectum. This patient recovered, but the urine and ſtools continue to paſs through the wounds. Of

—
99
—

fourth was delivered of a ſeven-months child, without mutilating it, which died in its paſſage. The number of women, therefore, who from error in their conformation were incapable of bearing live children appears to be very inconſiderable. Of the remaining four I have not been able to get any intelligence.

* In theſe nine caſes only one child was ſaved.

99

1 woman the perinæum was lacerated to the ſphincter ani. A ſuture was attempted, but without effect; ſhe recovered, but is troubled with prolapſus uteri.

5 had large and painful ſwellings of the legs and thighs, but recovered.

105 therefore of theſe, or 1 in 18, had preternatural or laborious births, or ſuffered in conſequence of labour. Of this number of caſes 43, or 1 in 44, were attended with particular difficulty or danger; and 7 only, or 1 in 270, died. The remaining 62 were delivered and recovered with little more than the common aſſiſtance: and

1792 had natural labours, not attended with any particular accidents.

1897

Of two women the uterus was retroverted in the third or fourth month of their pregnancy; but in both the uterus was replaced, and the women went to their full time, and brought forth live children.

Beſides the accidents above enumerated, it ſeems right to obſerve, that many of the women were afflicted with ſevere after-pains, or had what is called the milk-fever; but as theſe complaints were generally relieved in three or four days, and did not ſeem to have any influence in retarding their recovery, or to affect their future healths, no notice is taken of them. Some women alſo had ſymptoms of incipient prolapſus uteri, who had not before been troubled with that complaint; but as

 I ſeldom

I ſeldom ſaw them above once or twice, I cannot give the reſult. But as few of them were able to indulge themſelves with reſt, or to comply with the rules neceſſary for their cure, it is to be feared, that in many of them the complaint would gradually increaſe; and that in a courſe of years, the uterus would make its appearance externally, when, finding it an impediment to their activity, they would apply and ſubmit to wear a peſſary, or to ſuch palliatives as in that ſtate can only be adminiſtered. Excepting this accident, and the fluor albus, to which many of them are ſubject after child-bearing, but which, I think, does not often materially affect their healths until late in life, I am inclined to believe, that the lower ſort of people recover more certainly after parturition than perſons in higher ſtations of life: at leaſt, they are leſs ſubject to the puerperal fever, which is ſo fatal, if not checked on its firſt attack; and which, if not cauſed, is certainly nouriſhed, and its malignancy increaſed, by great fires, cloſe rooms, warm ſeptic diet and coſtiveneſs. But the apartments of the poor are generally ſo crazy, that without opening doors or windows, to which they are ſufficiently averſe, the air pours in upon them from all ſides. To this circumſtance, added to their inability to keep great fires, or to indulge themſelves with animal food, and to the care that is taken very early to empty their bowels, I have been induced to attribute their ſo generally eſcaping this fatal diſeaſe; and by adopting rules in my private practice conſonant to this idea, I have the ſatisfaction to be able to ſay, that I have not ſeen the puerperal fever among my private patients for more than three years.

TABLE

TABLE of the proportion of male to female children, of the number of twins, and of the children that were deficient or monſtrous, and of thoſe that were dead-born.

1897 women were delivered of 1923 children; 972 boys and 951 girls, or as 46 boys to 45 girls.

23 of the women, or 1 in 80, were delivered of twins, 16 of whom were boys and 30 girls.

1 woman was delivered of 3 girls.

Of the twins and triplets, therefore, the males were only half the number of the females.

8 of the children, or 1 in 241, were deficient or monſtrous.

Of theſe 1 was web-fingered;
1 had a hare-lip;
1 had a dropſical head and diſtorted ſpine;
1 a dropſical head;
in 1 a part of the palate;
and in 2 a conſiderable portion of the cranium * was wanting;
and 1 had two heads †, ſee fig. 1.
8

One

* One of theſe lived an hour after it was born.

† This was the child of ELIZABETH wife of —— BROMFIELD, Peruke-maker, Jees-court, Oxford-ſtreet. It had two heads and necks, four hands and arms, two ſpines, uniting at the ſacrum, and terminating in one pelvis, from whence the lower extremities proceeded ſingle: there was one navel-ſtring, and one male organ of generation. On opening the body there were found, two thoracic cavities, the right more compleat than the left: the heart alſo, and the lungs on the right ſide, were more perfect than thoſe on the left, which latter were very ſmall.

3 There

One woman was delivered of a twin *, ſee fig. 2.

84 of the children, or 1 in 23 of the whole number, were dead-born †. Of theſe, 49, or nearly five-eighths, were boys, and 35 were girls.

Of 1400 women who returned their letters, or of whom a certain account could be obtained, 85, or nearly 1 in 16, had

There were two ſtomachs, two ſets of inteſtines, which, at length uniting, terminated in one rectum and anus. There was but one urinary bladder. The drawing that accompanies this will give a more juſt idea of its external figure; and Dr. HUNTER, who diſſected it, will probably one day oblige the world with an exact anatomical deſcription of it.

* Of this ſingular production, to which I have not ventured to give a name, the following is the hiſtory and deſcription. The woman who produced it is about twenty-ſeven years of age; this was her firſt pregnancy. She was, after a labour, delivered of a female fœtus, and its placenta, in which nothing uncommon was obſerved; and although the uterus remained of an unuſual ſize, yet the pains not recommencing, there was no ſuſpicion entertained but that its bulk was occaſioned by coagulated blood. On the third day the pains became violent, and this monſter was born. Its ſhape was ſpherical, but ſomewhat flattened. It meaſured in its largeſt diameter eight inches, and weighed about eighteen ounces. It received its nouriſhment by an umbilical chord, to which was attached a portion of membranes, and although no placenta was found, it is probable it had a ſmall one, and that it was incloſed in its own involucrum. It was completely covered with a cuticula, and a little above the part, where the navel-ſtring terminated, there was a hairy ſcalp covering a bony prominence, ſomewhat reſembling the arch of the cranium. On diſſection it was found to be plentifully ſupplied with blood veſſels, proceeding from the navel-ſtring, and branching through every part of it. It had a ſmall brain and medulla ſpinalis continued into a bony theca, with nerves paſſing from thence through the foramina of the bones; but no reſemblance of any thoracic or abdominal viſcera. The reſt of its bulk was made up of fat.

† By dead-born children I mean thoſe that die after they have been perceived to move, that is, generally after four months. Abortions, or deaths before that period, may reaſonably be eſtimated at double this number; ſo that, perhaps, 1 child in 8 dies in the womb, or in the act of coming into the world.

buried

buried their children before the end of two months. Of this number 53, or 5 in 8, were boys, and 32 girls.

This ſingular circumſtance of there being a greater number of males than females among the ſtill-born children, and of a greater number of male children dying in infancy than of females, has been remarked by Dr. PRICE and other writers on calculations; and Dr. HAYGARTH * has ſhewn that at Cheſter more huſbands die in a given period than wives. This naturally ſuggeſts an enquiry, whether the lives of males are at all ages more precarious than thoſe of females.

To be enabled to aſſiſt in anſwering this queſtion, I have added the following article to my regiſter, *viz.* of the children that ſhall be living at the time the women apply for their letters, how many will be boys, and how many girls?

* Obſervations on the bills of mortality in Cheſter for the year 1772.

TABLE

TABLE of the ages at which women begin and ceaſe to be capable of bearing children, and of the intermediate periods at which they are moſt ſo.

Of 2102 * pregnant women

		Years of age.	
† 36	or 1 in 58 were from -	15 to 19	85, or 1 in 25, from 15 to 20 inclusive.
49	or 1 in 43 were -	20	
578	or 5 in 19 were from -	21 to 25	1684, or four-fifths were from 21 to 35 inclusive.
699	nearly 1 in 3 were from	26 to 30	
407	nearly 1 in 5 were from	31 to 35	
291	or 3 in 22 were from -	36 to 40	
36	or 1 in 58 were from -	41 to 45	42, or 1 in 50, from 41 to 49.
6	or 1 in 350 were from	46 to 49	
2102			

* Although 2102 women, the number here mentioned, obtained letters, entitling them to the aſſiſtance of the midwives, 1897 only were delivered by them: the remainder either removed out of the bounds of the Diſpenſary, or, from ſome alteration in their circumſtances, were obliged to go to an hoſpital or workhouſe.

† 1 of theſe women was between 15 and 16 years of age.
1 between 16 and 17;
3 between 17 and 18;
10 between 18 and 19; and
21 between 19 and 20.
36

TABLES of the number of children borne by 1389 * women, with the number that were living at the time of their applying to the Dispensary.

Women.	N° of children borne by each woman.	Total of children born.	Total of children living.	N° of women who had preserved their children.	N° of children preserved by each woman.	Total of children preserved.
1	24	24	5	—	—	—
1	17	17	3	—	—	—
3	16	48	5	—	—	—
2	14	28	11	—	—	—
11	13	155	46	—	—	—
14	12	168	44	—	—	—
15	11	165	45	1	11	11
22	10	220	84	—	—	—
33	9	297	93	—	—	—
56	8	448	151	4	8	32
74	7	518	213	3	7	21
89	6	534	214	11	6	66
138	5	690	288	32	5	160
169	4	676	293	84	4	336
208	3	624	299	174	3	522
254	2	508	259	306	2	612
299	1	299	171	464	1	464
1389		†5419	2224	1079		2224
and 370 were in their first pregnancy,				and 310 had lost all their childr.		
1389				1389		

* In order to account for the difference between the number of the women in these and the preceding tables, it is proper to mention, that this account was not begun until some months after the former one. In these also care has been taken that no woman is reckoned more than once, although many of them had been assisted by the midwives to the Dispensary two, three, or four times. 370, as noted in the table, were in their first pregnancy.

† Of these 5419 children 2747 were boys, and 2672 girls, or nearly as 36 boys to 35 girls. This proportion of the boys to the girls will be found a little different from what is given in the table p. 362.

I have

I have placed theſe two tables together, that we might have an opportunity of obſerving how exceedingly fertile the women of the poorer claſſes in this country are; and at the ſame time how unable to rear any conſiderable number of children; for, although 321 of the women had borne ſix children and upwards each, and were all again pregnant, 19 only of them had been able to rear ſix or more children; and, although 102 of the women had borne nine children and upwards each, only one of them had been able to preſerve that number living.

I am inclined to believe, that this great mortality amongſt the children does not ariſe from any natural imbecillity or a conſtitution vitiated from the birth, many of thoſe victims being born with all the appearances of health and vigour; but that we ought rather to ſearch for the cauſe of it in the poverty of the parents, which prevents their taking the neceſſary care of, or even affording ſufficient cloathing and nouriſhment to their offſpring. Whether this great check to population is in its nature irremediable; and whether an abatement in the pariſh rates and taxes, but particularly the former, to perſons rearing more than a certain number of children, or any other mode of relief and encouragement, would contribute to reſtrain ſo melancholy an evil, are inquiries well deſerving the attention of government. In order to determine how well my conjectures on this ſubject are founded, it might be uſeful, perhaps, to learn what the proportion of deaths is in more opulent families, where the cauſe juſt now mentioned can have but little influence. But this muſt be the reſult of the united obſervations of different practitioners.

I ſhall now from theſe tables attempt to collect what the chance of life is at different periods, from infancy to twenty-ſix years of age; but, that I may be underſtood, it will be

neceſſary to premiſe ſome account of the method I have followed.

I have ſuppoſed each of the women to bear a child every two years; this, from the account of thoſe who returned to the Diſpenſary a ſecond, third, or fourth time, appearing to be the mean term. Upon this principle, when I find that a woman applied at the Diſpenſary who had had one child before, I conclude, that that child would be two years old, if living; but if the woman had borne two children, I ſuppoſe that the firſt would be four, the ſecond two years old, and ſo on. And finding, that of 299 children borne by as many women, who were now advanced in their ſecond pregnancy, 171, or ſeven-twelfths only were living, I conclude, that on an average 5 out of 12 die under two years of age: and obſerving that of 508 children borne by 254 women, who were now advanced in their third pregnancy, 259 only were living, I firſt deduct 210, which is five-twelfths of the whole number, who died under two years of age; and then find that 39, which is nearly one-twelfth of the whole number, or one-ſeventh of the ſurvivors, died between two and four years of age.

TABLE

TABLE of the chance of life from infancy to 26 years of age.

Age.	Perſons living.	Decreaſe of life.	
0	5400	2250	5 in 12.
2	3150	450	6 in 12, or 1 in 7 of the ſurvivors.
4	2700	180	8 in 15, or 1 in 15 of the ſurvivors.
6	2520	204	4 in 7, or 1 in 12 and ½ of the ſurvivors.
8	2313	156	6 in 10, or 1 in 15 of the ſurvivors.
18	2160	540	7 in 10, or 1 in 4 of the ſurvivors.
26	1620		

3780 or ſeven-tenths would die.
1620 or three-tenths would be living at the end of 26 years.

5400

Whether this mode of calculating the chance of life will be admitted by gentlemen who have made ſpeculations of this kind their peculiar ſtudy, I know not. I confeſs, that when I firſt thought of it, I expected it would have proved more certain and accurate than upon examination I have found it to be: for, although in the firſt ſeries of years, where the deaths are numerous, the proportions agree tolerably well with the tables of M. BUFFON and others, yet as we advance we find ourſelves obliged to take longer periods than two years. Thus, for inſtance, we may obſerve, that although from 2 to 4, from 4 to 6, and from 6 to 8 years of age, the decreaſe continues to go on; yet ſo far is this from being the caſe between the ages of 8 and 10, or even 12, that there then appears to be ſome trifling increaſe. But as the proportion of deaths from 8 to 10 or 12 is probably inconſiderable, a very ſmall deduction from the deaths in the earlier years would rectify this difference. A deviation of this kind, I preſume, might be occaſioned by the ſmall-pox,

or

or ſome other epidemic, prevailing amongſt children during two or three years of the time I was making this collection, which would occaſion the decreaſe in the firſt and ſecond ſeries to be greater than uſual. If this ſhould prove to be the caſe, it is probable, that in a courſe of years, by comparing this with a variety of ſimilar tables, the true medium may be found.

A COMPARATIVE TABLE of the population of London, with a view to ſhew the proportion of natives to perſons born in the different counties of England and Wales, in Scotland, Ireland, or foreign countries.

Of 3236 married perſons

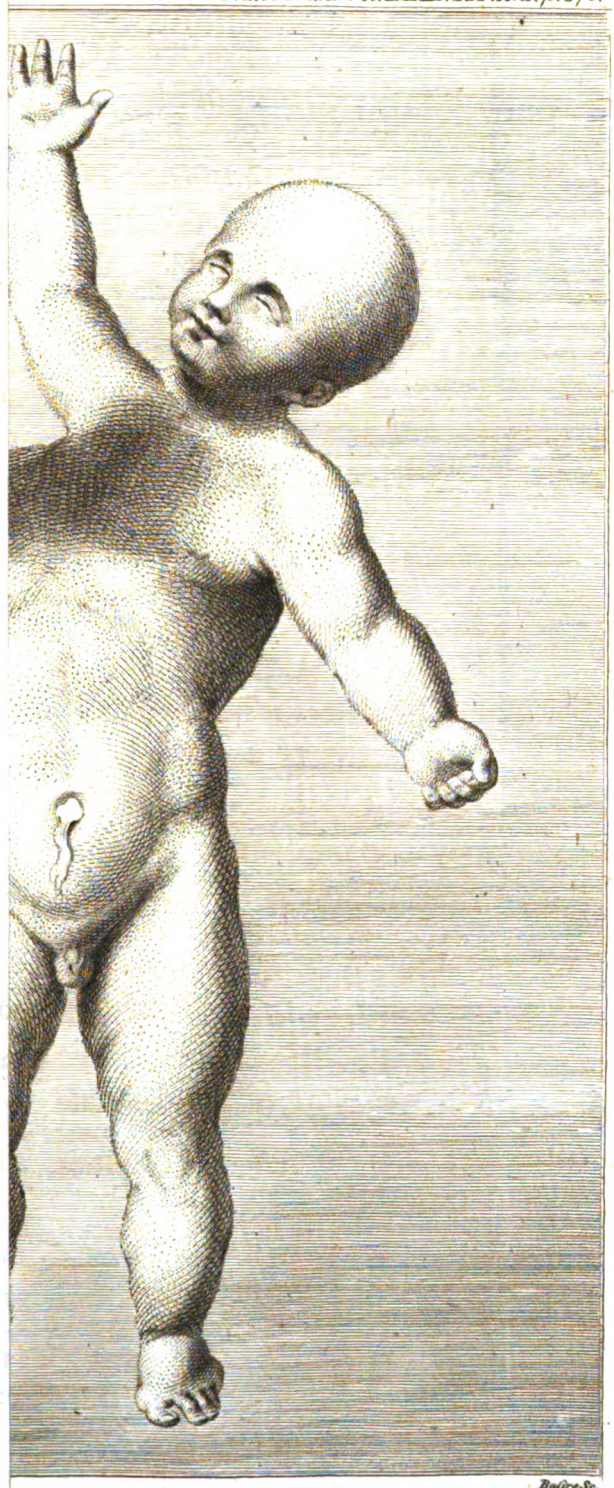
824 or one-fourth were born in London.
1870 or four-ſevenths in the different counties of England and Wales.
209 or 1 in 15 in Scotland.
280 or 1 in 11 in Ireland.
53 or 1 in 60 were foreigners.

3236

Of the above number the males and females were in the following proportions.

Men.		Women.	
329	were born in London, and	495	or 166 more than men.
952	—— in different counties	917	or 35 fewer than men.
135	—— in Scotland,	74	or 61 fewer than men.
162	—— in Ireland, —	119	or 43 fewer than men.
40	—— were foreigners,	13	or 27 fewer than men.
1618		1618	166

Thus,



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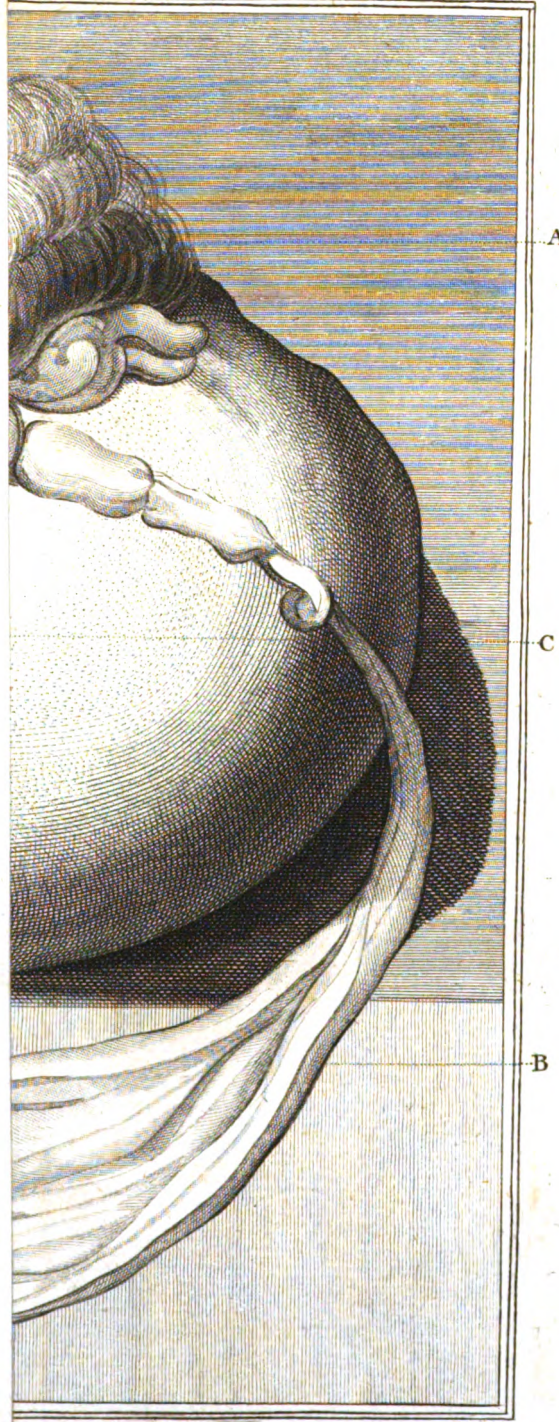

Basire Sc.

Thus, of 824 married perſons born in London, there were one-fifth more women than men. This may be accounted for either by ſuppoſing a greater number of males to die or to migrate before they attain a marriageable age than women. It is alſo to be obſerved, that of the Scotch and of the foreigners the women are in proportion to the men as about 1 to 3; but of the Iriſh they are as 3 to 7.

By this table we find at how great an expence to the country this city is maintained; and as we may ſuppoſe that the bulk of the Scotch, Iriſh, and foreigners, who come into the kingdom, reſide in the metropolis, we hence may alſo learn in what proportion they contribute to repair the waſte which is incurred by its exceſſive populouſneſs. A more compleat knowledge of theſe facts may give riſe to regulations which, if the calculations of Dr. PRICE ſhall be found to be juſt, are but too neceſſary; but I fear I have already intruded upon your patience, and extended this paper beyond its due bounds. I ſhall only add, that if theſe inquiries ſhould be favourably received by the illuſtrious body to whom you have ſo obligingly undertaken to preſent them, they will be continued, and their value of courſe increaſed by the additional number of objects which each year will ſupply.

I am, &c.

St. Alban's Street,
March 26, 1781.

XXIII. *Account of a Child who had the Small-pox in the Womb. In a Letter from* William Wright, *M. D. F. R. S. to* John Hunter, *Esq. F. R. S.*

Read May 21, 1781.

Southampton-buildings, Holborn,
Feb. 27, 1781.

SIR,

I HAVE read with much pleasure and information Mrs. FORD's case, which you published in Phil. Transf. vol. LXX. p. 128. From the facts you have adduced it amounts to a certainty, that her fœtus had received the variolous infection in the womb.

This induces me to lay before you a singular case, that fell under my care some years ago. I am sorry I cannot be more particular, having unfortunately lost all my books and my notes of practice of this case and several others, by the capture of the convoy on the 9th of last August.

In 1768 the small-pox was so general in Jamaica that very few people escaped the contagion. About the middle of June Mr. PETERKIN, merchant at Martha-brae, in the parish of Trelawney, got about fifty new negroes out of a ship: soon after they landed, several were taken ill of a fever, and the small-pox appeared; the others were immediately inoculated. Amongst the number of those who had the disease in the natural way, was a woman of about twenty-two years of age, and big with child. The eruptive fever was slight, and the small-pox

ſmall-pox had appeared before I ſaw her. They were few, diſtinct and large, and ſhe went through the diſeaſe with very little trouble, till on the fourteenth day from the eruption ſhe was attacked with the fever, which laſted only a few hours. She was, however, the ſame day taken in labour, and delivered of a female child with the ſmall-pox on her whole body, head, and extremities. They were diſtinct and very large, ſuch as they commonly appear on the eighth or ninth day in favourable caſes. The child was ſmall and weakly; ſhe could ſuck but little; a wet nurſe was procured, and every poſſible care taken of this infant, but ſhe died the third day after ſhe was born. The mother recovered, and is now the property of ALEXANDER PETERKIN, Eſq. in St. James's Pariſh.

In the courſe of many years practice in Jamaica, I have remarked, that where pregnant women had been ſeized with the natural ſmall-pox, or been by miſtake inoculated, that they generally miſcarried in the time of, or ſoon after, the eruptive fever; but I never ſaw any ſigns of ſmall-pox on any of their bodies, except on the child's above mentioned.

I am, &c.

XXIV. *Natural History of the Insect which produces the Gum Lacca. By Mr.* James Kerr, *of* Patna; *communicated by Sir* Joseph Banks, *P. R. S.*

Read May 24, 1781.

COCCUS LACCA.

Head and trunk.

THE head and trunk form one uniform, oval, compressed, red body, of the shape and magnitude of a very small louse, consisting of twelve transverse rings. The back is carinate; the belly flat; the antennæ half the length of the body, filiform, truncated, and diverging, sending off two, often three, delicate, diverging hairs, longer than the antennæ. The mouth and eyes could not be seen with the naked eye.

Tail.

The tail is a little white point, sending off two horizontal hairs as long as the body.

Feet.

It has three pair of limbs, half the length of the insect.

I have often observed the birth of these insects, but never could see any with wings; nor could I find any distinction of sexes, nor observe their connubial rites: nature and analogy seem to point out a deficiency in my observations, possibly

 owing

owing to the minuteness of the object, and want of proper glasses.

Change. This insect is described in that state in which it sallies forth from the womb of the parent in the months of November and December. They traverse the branches of the trees upon which they were produced for some time, and then fix themselves upon the succulent extremities of the young branches. By the middle of January they are all fixed in their proper situations, they appear as plump as before, but shew no other marks of life. The limbs, antennæ, and setæ of the tail are no longer to be seen. Around their edges they are environed with a spissid subpellucid liquid, which seems to glue them to the branch: it is the gradual accumulation of this liquid, which forms a compleat cell for each insect, and is what is called Gum Lacca. About the middle of March the cells are completely formed, and the insect is in appearance an oval, smooth, red bag, without life, about the size of a small cuchanical insect, emarginated at the obtuse end, full of a beautiful red liquid. In October and November we find about twenty or thirty oval eggs, or rather young grubs, within the red fluid of the mother. When this fluid is all expended, the young insects pierce a hole through the back of their mother, and walk off one by one, leaving their exuviæ behind, which is that white membraneous substance found in the empty cells of the Stick Lac.

Place. The insects are the inhabitants of four trees.

1. Ficus Religiosa, LINNÆI. In Hindostan, Pipul. Banyan Tree.

2. Ficus Indica, LINNÆI. In Hindostan, Bhur. Banyan Tree.

3. Plaso Hortus Malabarici. By the natives, Praso.

4. Rhamnus Jujuba, LINNÆI. In Hindostanick, Beyr.

The insects generally fix themselves so close together, and in such numbers, that I imagine only one in six can have room to compleat her cell: the others die, and are eat up by various insects. The extreme branches appear as if they were covered with a red dust, and their sap is so much exhausted, that they wither and produce no fruit, the leaves drop off, or turn to a dirty black colour. These insects are transplanted by birds: if they perch upon these branches, they must carry off a number of the insects upon their feet to the next tree they rest upon. It is worth observing, that these fig trees when wounded drop a milky juice, which instantly coagulates into a viscid ropey substance, which, hardened in the open air, is similar to the cell of the Coccus Lacca. The natives boil this milk with oils into a bird-lime, which will catch peacocks or the largest birds.

A red medicinal gum is procured by incision from the Plaso Tree, so similar to the Gum Lacca that it may readily be taken for the same substance.

substance. Hence it is probable, that those insects have little trouble in animalizing the sap of these trees in the formation of their cells. The Gum Lacca is rarely seen upon the Rhamnus Jujuba; and it is inferior to what is found upon the other trees. The Gum Lacca of this country is principally found upon the uncultivated mountains on both sides the Ganges, where bountiful nature has produced it in such abundance, that was the consumption ten times greater the markets might be supplied by this minute insect. The only trouble in procuring the Lac is in breaking down the branches, and carrying them to market. The present price in Dacca is about twelve shillings the hundred pounds weight, although it is brought from the distant country of Assam. The best Lac is of a deep red colour. If it is pale, and pierced at top, the value diminishes, because the insects have left their cells, and consequently they can be of no use as a dye or colour, but probably they are better for varnishes.

This insect and its cell has gone under the various names of Gum Lacca, Lack, Loc Tree. In Bengal, La; and by the English it is distinguished into four kinds.

1st. Stick Lac, which is the natural state from which all the others are formed.

2. Seed Lac is the cells separated from the sticks.

3d. Lump

3d. Lump Lac is Seed Lac liquified by fire, and formed into cakes.

4th. Shell lac is the cells liquified, strained, and formed into thin transparent laminæ in the following manner. Separate the cells from the branches, break them into small pieces, throw them into a tub of water for one day, wash off the red water and dry the cells, and with them fill a cylindrical tube of cotton cloth, two feet long, and one or two inches in diameter; tie both ends, turn the bag above a charcoal fire; as the Lac liquifies twist the bag, and when a sufficient quantity has transuded the pores of the cloth, lay it upon a smooth junk of the Plantain tree (Musa Paradisiaca, LINNÆI), and with a strip of the Plantain leaf draw it into a thin lamella; take it off while flexible, for in a minute it will be hard and brittle. The value of Shell Lac is according to its transparency.

Use to the natives.

This is one of the most useful insects yet discovered.

Ornaments for the ladies.

The natives consume a great quantity of Shell Lac in making ornamental rings, painted and gilded in various tastes, to decorate the arms of the ladies; and it is formed into beads, spiral and linked chains for necklaces, and other female ornaments.

Sealing wax.

Take a stick, and heat one end of it upon a charcoal fire; put upon it a few leaves of the Shell Lac softened above the fire; keep alternately heating and adding more Shell Lac, until you

you have got a mass of three or four pounds of liquified Shell Lac upon the end of your stick *. Knead this upon a wetted board with three ounces of levigated cinnabar, form it into cylindrical pieces; and, to give them a polish, rub them while hot with a cotton cloth.

Japanning. Take a lump of Shell Lac, prepared in the manner of sealing-wax, with whatever colour you please, fix it upon the end of a stick, heat the polished wood over a charcoal fire, and rub it over with the half-melted Lac, and polish, by rubbing it even with a piece of folded Plantain leaf held in the hand; heating the lacquer, and adding more Lac as occasion requires. Their figures are formed by Lac, charged with various colours in the same manner.

Varnish. In ornamenting their images and religious houses, &c. they make use of very thin beat lead, which they cover with various varnishes, made of Lac charged with colours. The preparation of them is kept a secret. The leaf of lead is laid upon a smooth iron heated by fire below, while they spread the varnish upon it.

Grindstones. Take of river sand three parts, of Seed Lac washed one part, mix them over the fire in a pot, and form the mass into the shape of a grindstone, having a square hole in the center, fix it on an axis with liquified Lac, heat the stone moderately, and by turning the axis it may easily be formed into an exact orbicular shape. Polish-

* In this manner Lump Lac is formed from Seed Lac.

ing

ing grindſtones are made only of ſuch ſand as will paſs eaſily through fine muſlin, in the proportion of two parts ſand to one of Lac. This ſand is found at Ragimaul. It is compoſed of ſmall angular cryſtalline particles, tinged red with iron, two parts to one of black magnetic ſand.

The ſtone-cutters, inſtead of ſand, uſe the powder of a very hard granite called Corune.

Theſe grindſtones cut very faſt. When they want to increaſe their power they throw ſand upon them, or let them occaſionally touch the edge of a vitrified brick. The ſame compoſition is formed upon ſticks, for cutting ſtones, ſhells, &c. by the hand.

Painting. Take one gallon of the red liquid from the firſt waſhing for Shell Lac, ſtrain it through a cloth, and let it boil for a ſhort time, then add half an ounce of ſoap earth (foſſil alkali); boil an hour more, and add three ounces of powdered load (bark of a tree); boil a ſhort time, let it ſtand all night, and ſtrain next day. Evaporate three quarts of milk, without cream, to two quarts, upon a ſlow fire, curdle it with ſour milk, and let it ſtand for a day or two; then mix it with the red liquid above mentioned; ſtrain them through a cloth, add to the mixture one ounce and an half of allum, and the juice of eight or ten lemons: mix the whole, and throw it into a cloth-bag ſtrainer. The blood of the inſect forms a coagulum with the caſeous part of the milk, and remains in the bag, while a limpid acid water

water drains from it. The coagulum is dried in the shade, and is used as a red colour in painting and colouring.

Dying. Take one gallon of the red liquid prepared as before without milk, to which add three ounces of allum. Boil three or four ounces of tamarinds in a gallon of water, and strain the liquor. Mix equal parts of the red liquid and tamarind water over a brisk fire. In this mixture dip and wring the silk alternately until it has received a proper quantity of the dye. To increase the colour, increase the proportion of the red liquid, and let the silk boil a few minutes in the mixture. To make the silk hold the colour, they boil a handful of the bark called Load in water, strain the decoction, and add cold water to it; dip the dried silk into this liquor several times, and then dry it. Cotton cloths are dyed in this manner; but the dye is not so lasting as in silk.

Spanish wool. The Lac colour is preserved by the natives upon flakes of cotton dipped repeatedly into a strong solution of the Lac Insect in water, and then dried.

Use to the Europeans. See European authors.

Explanation of the figures.

a The Coccus Lacca at its birth, } natural size.
b Ditto, big with young, }

γ The embrio before birth inclosed in its membrane, } magnified.
δ The Coccus, with two hairs from each antenna, }
ε Ditto, with three hairs from each antenna, }

XXV. *Account of a Phenomenon observed upon the Island of* Sumatra. *By* William Marsden, *Esq.; communicated by Sir* Joseph Banks, *P. R. S.*

Read May 24, 1781.

SIR,

Caroline-street, Feb. 24, 1781.

DURING my residence on the island of Sumatra in the East Indies, I had occasion to observe a phenomenon singular, I believe, in its kind, an account of which may not perhaps be uninteresting to the curious.

In the year 1775 the S.E. or dry monsoon, set in about the middle of June, and continued with very little intermission till the month of March in the following year. So long and severe a drought had not been experienced then in the memory the oldest man. The verdure of the ground was burnt up, the trees were stripped of their leaves, the springs of water failed, and the earth every where gaped in fissures. For some time a copious dew falling in the night supplied the deficiency of rain; but this did not last long: yet a thick fog, which rendered the neighbouring hills invisible for months together, and nearly obscured the sun, never ceased to hang over the land, and add a gloom to the prospect already but too melancholy. The Europeans on the coast suffered extremely by sickness; about a fourth part of the whole number being carried off by fevers

 and

and other bilious diſtempers, the depreſſion of ſpirits which they laboured under not a little contributing to haſten the fatal effects. The natives alſo died in great numbers.

In the month of November 1775, the dry ſeaſon having then exceeded its uſual period, and the S.E. winds continuing with unremitting violence, the ſea was obſerved to be covered, to the diſtance of a mile, and in ſome places a league from ſhore, with *fiſh* floating on the ſurface. Great quantities of them were at the ſame time driven on the beach or left there by the tide, ſome quite alive, others dying, but the greateſt part quite dead. The fiſh thus found were not of one but various ſpecies, both large and ſmall, flat and round, the Cat-fiſh and Mullet being generally the moſt prevalent. The numbers were prodigious, and overſpread the ſhore to the extent of ſome degrees; of this I had ocular proof or certain information, and probably they extended a conſiderable way farther than I had opportunity of making enquiry. Their firſt appearance was ſudden; but though the numbers diminiſhed, they continued to be thrown up, in ſome parts of the coaſt, for at leaſt a month, furniſhing the inhabitants with food, which, though attended with no immediate ill conſequence, probably contributed to the unhealthineſs ſo ſeverely felt. No alteration in the weather had been remarked for many days previous to their appearance. The thermometer ſtood as uſual at the time of year at about 85°.

Various were the conjectures formed as to the cauſe of this extraordinary phenomenon, and almoſt as various and contradictory were the conſequences deduced by the natives from an omen ſo portentous; ſome inferring the continuance, and others, with equal plauſibility, a relief from the drought. With reſpect to the cauſe, I muſt confeſs myſelf much at a

loſs to account for it ſatisfactorily. If I might hazard a conjecture, and it is not offered as any thing more, I would ſuppoſe, that the ſea requires the mixture of a due proportion of freſh water to temper its ſaline quality, and enable certain ſpecies of fiſh to ſubſiſt in it. Of this ſalubrious correction it was deprived for an unuſual ſpace of time, not only by the want of rain, but by the ceaſing of many rivers to flow into it, whoſe ſources were dried up. I rode acroſs the mouths of ſeveral perfectly dry, which I had often before paſſed in boats. The fiſh no longer experiencing this refreſhment, neceſſary as it would ſeem to their exiſtence, ſickened and periſhed as in a corrupted element.

If any thing ſimilar to what I have above deſcribed has been noticed in other parts of the world, I ſhould be happy by a compariſon of the attendant circumſtances, to inveſtigate and aſcertain the true cauſes of ſo extraordinary an effect. In communicating to you the obſervations I have made, I purſue the moſt likely means of obtaining this ſatisfaction.

I have the honour to be, &c.

XXVI. *Farther Experiments on Cold, made at the* Macfarlane *Obſervatory belonging to* Glaſgow College. *In a Letter from* Patrick Wilſon, *M. A. to the Rev.* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read May 24, 1781.

DEAR SIR, Glaſgow College, Feb. 9, 1781.

SOME days of very cold weather, which we had lately in this country, having afforded an opportunity of proſecuting a little farther the experiments and obſervations begun in the courſe of laſt year, I now do myſelf the pleaſure of communicating to you the following particulars, which perhaps may be conſidered as not unworthy of notice.

The froſt ſet in on Sunday the 21ſt of January, after a conſiderable fall of ſnow on the preceding evening, and about midnight the thermometers were expoſed near to the Obſervatory in the ſituations mentioned in my former letter. The following regiſter ſhews the difference of temperature between the ſnow and the air, till eight o'clock on Monday morning, to which are ſubjoined ſome facts which prove very conſonant to thoſe deſcribed in the former paper.

The ſign – prefixed denotes degrees below 0. The ſign + degrees above 0 of FAHRENHEIT's thermometer.

Monday

Monday morning, January 22, 1781.

h.	m.		Thermometer in air.		Thermometer on snow.
1		-	0	-	− 12
1	30	-	+ 2	-	− 12
1	45	-	0	-	− 8
2		-	− 2	-	− 7
3		-	0	-	− 4
3	45	-	0	-	− 8
4		-	− 1	-	− 12
4	30	-	− 3	-	− 8
5		-	− 2	-	− 12
6	15	-	− 3	-	− 13
6	45	-	− 2	-	− 10
7		-	− 3	-	− 13
7	30	-	− 2	-	− 10
8		-	− 4	-	− 11
8	30	-	− 2	-	− 10

From one o'clock till three in the morning the thermometer in air at the baluſtrade of the eaſt wing of the Obſervatory pointed from + 4 to + 6, and on the ſnow there from − 2 to 0. At half an hour after one the thermometer in air, twenty-four feet from the ground, and to the windward of the houſe, pointed to + 7, and at eight o'clock to + 1. At three o'clock the ſnow in the park, three inches below the ſurface, raiſed the thermometer to + 14, and at ſix inches below, near the ground, to + 24. The barometer ſtood at 29.8 inches, and there was a perceptible motion of the air from the eaſt and one point ſouth. This night was a very general and lively aurora borealis, moſt part

part of it of a bright red, which formed a crown near to the zenith; but it mostly vanished about three o'clock, after which time the air became more still. During the whole of this night, as well as of the succeeding times of observing, the air was not nearly so much disposed to give out hoar-frost as it was last year.

On Monday evening the difference of temperature was found to be as in the following register.

Monday evening,

h.	m.		Therm. in air.	Therm. in snow.
8		— — — —	+ 16	+7
8	30	— — — —	+ 14	+3
9		— — — —	+ 8	+1
9	30	— — — —	+ 7	+1
10		— — — —	+ 7	+3
10	30	— — — —	+ 6	+0
11		Ball of therm. ¼ an inch above the surface of the snow,	+ 5	+3
12		Ditto, — — — —	+ 5	+3
		Tuesday morning.		
1		Ball of therm. as formerly half immersed in the snow,	+ 6	+3
2		— — — —	+ 8	+5
2	30	— — — —	+ 10	+6

No aurora this evening; the air very still and serene till about two o'clock Tuesday morning, when the wind rose remarkably, and clouds formed in the north-east.

On

On Thurſday, January 25, the difference of temperature was found to be as here ſet down.

Thurſday morning.

h.	m.		Thermometer in air.		Thermometer in ſnow.
9	45	-	+ 10	-	+ 3
10		-	+ 10	-	+ 3
10	30	-	+ 14	-	+ 4
11		-	+ 14	-	+ 8
11	30	-	+ 17	-	+ 9
12		-	+ 20	-	+ 12
12	30	-	+ 22	-	+ 20
1 afternoon.		-	+ 25	-	+ 26
1	30	-	+ 27	-	+ 27

From ten till eleven o'clock this forenoon the thermometer on the baluſtrade in air, ſix inches above the ſnow, pointed to + 14, and when tried upon the ſnow to + 10. About noon this day ſome clouds were formed, which became quite general by one o'clock.

During the two laſt times of obſerving, three experiments were made with a view of diſcovering whether the ſnow without doors was gaining any thing from the air; or if any of it was carried off in the way of evaporation? For this purpoſe, a ſhallow diſh, made of ſheet braſs, four inches in diameter, was exactly filled with ſnow, and carefully weighed. In order to defend the outſide of the diſh from the air, that no hoar-froſt might attach itſelf to the metal, a circular hole was cut in the lid of a paſte-board box, ſo wide as juſt to let in the diſh

to the very brim, ſo that nothing communicated with the external air but the ſnow itſelf. The apparatus, in this ſtate, was ſet without doors for three hours each time, and then brought in to the lobby of the Obſervatory, where the diſh was again weighed: but in none of theſe trials did it ever appear, that any weight was loſt. On the contrary, at the firſt weighing, which was on Monday night, twelve o'clock, it had gained five grains. In the other two trials the increaſe of weight was ſcarce perceivable.

The temperature of the air in the weſt room of the Obſervatory remaining very conſtantly for near two days at +27°, a diſh of ſnow, ſimilar to the other expoſed there, was found to loſe weight very ſenſibly, and for the moſt part at the rate of two grains in an hour. Notwithſtanding this, the ſnow thus waſting or evaporating had no power of ſinking the thermometer below +27, the temperature of the ſurrounding air; though at one time it was fanned for four minutes by a piece of paper faſtened to the end of a long ſtick. Not to diſturb the uniform temperature of this room during theſe experiments, care was taken to ſtay in it a very ſhort time at every viſit, and to keep the door and the window-ſhutters cloſe.

On Chriſtmas-day we had a froſt, which in the morning made the thermometer in air point to +21; and during the preceding night there had been a profuſe depoſition of hoar-froſt. A pound of this was collected, and its capacity for heat compared to that of ice, and found equal as nearly as could be judged. Before making the two mixtures neceſſary for this experiment, the ice was reduced to a powder, and ſpread out on a paper beſide the hoar-froſt till both had acquired the ſame temperature.

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On Monday night, January 22, about twelve o'clock, having occaſion to take up a little ſnow, there was obſerved a coheſion among its parts rather greater than what might have been expected in a ſubſtance, at that time, ſo much frozen. This circumſtance was farther examined by the following experiment. A pane of glaſs was laid on the ſurface of the ſnow till it had acquired the temperature of + 3, after which, with a bit of parchment equally cold, ſome ſnow was ſcraped from the very ſurface, and ſhaken all over the pane, ſo as to cover it in moſt parts lightly. Upon now lifting the pane, and holding it with the ſnow undermoſt, the whole of it adhered, and it required ſome ſmart raps before the greater part fell away. What remained cleaved to the glaſs with ſtill a greater adheſion.

The experiments related above afford further reaſons againſt the opinion of the difference of temperature betwixt the ſnow or hoar-froſt and the air depending upon evaporation. It would moreover appear, that this phenomenon depends not either upon the depoſition of hoar-froſt. What renders this the more probable is, that on laſt year there was a much more copious depoſition at times when the difference of temperature was not more remarkable. But allowing that a depoſition had been found a neceſſary circumſtance, and always in proportion to that difference, the experiments on the capacities of hoar-froſt and ice ſeem to ſhew, that the ſenſible heat which diſappears enters not into the compoſition of the hoar-froſt; otherwiſe the capacity of this ſubſtance for heat, compared to that of ice or common ſnow, ſhould be very different. It muſt be confeſſed, however, that the above mentioned experiment would have been more applicable to this reaſoning, had it been made with hoar-froſt given out in colder ſtates of the air.

If the air, at low temperatures, had any power of acting upon the ſnow or hoar-froſt, ſo as to produce a gradual melting, this circumſtance, according to the known laws of heat, might occaſion the difference of temperature under conſideration. And what renders this idea not altogether improbable, is the peculiar coheſion among the parts of the ſnow above deſcribed. Perhaps a gentle melting might take place without much altering the appearance of the ſnow or hoar-froſt at the ſurface, as the parts, when diſſolved, might be gradually ſucked downwards, and be afterwards diſtributed through the whole drier maſs. It may alſo be worthy of an experimental inquiry to determine, how far that ſort of concretion, obſervable all over the ſurface of ſnow which has been long frozen, bears any marks of a ſlow proceſs of this kind. From a hill, a little way to the N.E. of the town, and which was to windward during the froſt, there were gathered two portions of ſnow, the one from the ſurface, and the other ſeven inches below it. The water produced from the two kinds is preſerved in very clean phials, in order to be compared together by ſome chemical trials, which, perhaps, may throw ſome light upon the whole of this matter.

At preſent I ſhall conclude this letter, perhaps already much too long, with juſt mentioning one other fact which was new to me; namely, the power of ardent ſpirits of diſſolving ſnow, and conſequently of producing with it a freezing mixture. The alcohol and ſnow ſeparately were at eight degrees below the freezing point, and when mixed ſuddenly and intimately, the temperature became in the ſpace of twenty ſeconds 28° below 0. This is a cold only 12° ſhort of that which FAHRENHEIT firſt produced by uſing ſpirit of nitre for the experiment; and

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it is not improbable, had the present experiment been tried with more precaution and address, that the result would have been still more remarkable. There was employed only about a pint of alcohol, but the proportion of snow was not then attended to, and the thaw coming soon afterwards prevented a repetition of the experiment.

I am, &c.

POSTSCRIPT.

I beg leave to add, that the water mentioned as produced from the superficial snow has been examined by several chymical trials, with a view of discovering if it differed in any respect from the water obtained from snow gathered at considerable depths, and near the ground. Had the atmosphere, when the thermometers pointed so low, been disposed to furnish any saline principle, the union of such an ingredient with the snow would have tended to produce an excess of cold at the surface, similar to what was then observed. Or if the snow at these low temperatures had acquired any remarkable power of dephlogisticating the air in contact with it, a cooling process at and near the confines of the snow and air might thereby have been maintained. In either of these cases, some very sensible indications of a saline or of a phlogistic principle might be expected on the water given by the

the ſnow collected from the ſurface. But in oppoſition to both theſe views it remains now to be mentioned, that nothing of this kind did appear in the courſe of the experiments, which indeed were contrived chiefly to detect ſuch circumſtances.

If therefore the arguments produced in both papers upon this ſubject will not allow us to account for ſo remarkable a cooling proceſs by an evaporation at the ſurface of the ſnow, it would appear, that there remains ſtill ſomething unknown with reſpect to the cauſe. A proper inveſtigation of this matter, in climates favourable to ſuch experiments, may poſſibly unfold ſome farther properties of heat with which at preſent we may be wholly unacquainted.

XXVII. *A general Theory for the Menſuration of the Angle ſub-tended by Two Objects, of which One is obſerved by Rays after Two Reflections from plane Surfaces, and the other by Rays coming directly to the Spectator's Eye.* By George Atwood, *M. A. F. R. S.*

Read June 21, 1781.

THE actual determination of an angle implies two obſervations, one taken at each extremity of the arc by which that angle is meaſured. When fixed aſtronomical quadrants or other ſectors are uſed for the practical eſtimation of angles, one of theſe obſervations is previouſly made by directing the axis of the teleſcope or line of collimation to ſome fixed point in the heavens, the index being then coincident with the initial point on the arc of the ſector: after this adjuſtment one obſervation only is neceſſary to aſcertain the angular diſtance

diſtance between that point and any other celeſtial object in the plane of the ſector. This method, however, is evidently impracticable, unleſs the inſtrument can be ſteadily fixed; for which reaſon aſtronomical quadrants become uſeleſs at ſea; and from the difficulties which attend placing them in their due poſition and adjuſtment on firm ground, they are almoſt wholly confined to regular obſervatories.

Mr. HADLEY *, by an ingenious application of optical principles, contrived to bring both extremities of the arc meaſured into the field of the ſpectators's view at the ſame time; by which improvement, angles are taken at ſea, as well as on land with an unfixed inſtrument, to a degree of accuracy ſufficient for nautical and other purpoſes, when the utmoſt exactneſs is not required.

Mr. HADLEY's invention is a particular caſe of a very extenſive theory, as yet but little attended to. According to his method, which is well known, the two reflecting ſurfaces uſed in the obſervation are perpendicular to the plane of motion; the direction of the teleſcope, and of the rays paſſing between the reflectors being parallel to that plane; whereas the inclination of the teleſcope, and of the intermediate rays, as well as of the reflectors themſelves to the plane of motion, admits of unlimited variety. A general theory to determine the angle obſerved by two reflections from the data on which its magnitude depends, without limitation or reſtriction, ſeems applicable to ſeveral uſeful purpoſes in practical aſtronomy. Having never ſeen any geometrical conſtruction or analyſis of this curious problem, I was induced to beſtow ſome conſideration

* Phil. Tranſ. N° 420. See alſo a tract, intituled, The Theory of HADLEY's quadrant, by the rev. W. LUDLAM.

on

on the subject, and shall be happy if the result of my inquiries appears to merit the attention of the Royal Society.

Art. 1. The manner of taking an observation by two reflections unconfined to any particular case may be described thus. Let C, B (fig. 1.) represent two plane reflecting surfaces, inclined to a plane OPA at any given angle. Through any point of the reflecting surface C draw a line perpendicular to the plane OPA, and with the point where the line meets the plane as a centre (which must here be represented by C) and any distance CP, describe a circle OPA. The reflecting plane B always continuing fixed, let the reflector C be moveable along with the radius CP as it revolves in the plane OPA round the centre C: the angular motion of the speculum C, referred to the circumference OPA, will be measured by the arc which the radius CP describes, the inclination of the plane C to the plane of motion OPA being always the same, and equal to that of the fixed speculum B.

2. The two plane reflectors, B and C, being equally inclined to the plane OPA, it follows, that during the motion of C there must be some point O in the circumference OAP, at which when CP arrives, the reflector C will be parallel to the fixed reflector B.

3. When the moveable radius which carries round the plane C is at any other position CP, let a ray flowing from a distant object T impinge on the speculum C; let it be reflected from thence in the direction CB, and being again reflected at B in the direction BG, let it be observed by a spectator's eye at G; the image of T will appear somewhere in the line GBS; suppose that a ray flows from a distant object S situated in the line GB produced, and that this ray SG comes directly to the spectator's

eye at G: the object S ſeen by direct rays, and the image of the point T ſeen by rays after two reflections, will appear to coincide in the line GBS. This is an obſervation by two reflections, from which, together with ſuch data as limit the problem, the true angle ſubtended by the objects T and S is to be inferred.

4. The data which limit this problem, being neceſſary for the determination of the angle ſubtended by T and S are in number four, which are next to be conſidered. 1ſt. One of theſe data is the arc PO, being the angular diſtance of the moveable radius CP, meaſured on the circumference of the circle OPA, from that poſition CO, at which the two reflectors are parallel; the ſituation of this arc OP in reſpect of the point O being ſuppoſed known, that is, it being known on which ſide of that point, OP is ſituated in reſpect of the ray BG. 2dly, The common inclination of the reflecting planes B and C to the plane of motion is another of theſe data. The third and fourth of the conditions muſt be mentioned rather more particularly. The ray BG is always underſtood to be given in poſition in reſpect of the plane of motion OPA (conſidered as immoveable) being either coincident with the line of collimation of a teleſcope, or directed by ſights ſo as to be invariably fixed: the ſpeculum B alſo being unmoved, the line or ray BC will never change its poſition, from the known principles of reflection. The angle CBG, therefore, and the half of that angle being the angle of incidence at which CB impinges on B, will be always of the ſame magnitude; whereas the half of the angle BCT, or the angle of incidence on the moveable ſpeculum C, is continually changing, while C is carried round in the plane of motion: this conſtant angle of incidence or reflection at the fixed ſpeculum B will be another of the data neceſſary to determine the problem.

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The rays GB, BC, and the speculum B, being fixed in respect of each other, and of the plane OAP, the plane CBG will also be given in position; that is, its inclination to the plane of motion, or to any other fixed plane, will constantly be the same: whereas the inclination of the plane BCT to the plane of motion, or other fixed plane, will be continually changing while the reflector C revolves with the radius CP. The position of the plane GBC constitutes the fourth and last of the data, and it will be immaterial to what fixed plane it is referred. In the ensuing solution the situation of this plane will be defined by its inclination to the fixed secondary of the plane of motion which passes through the point O.

5. The enumeration of these data leads to the construction of the problem, a few observations being previously inserted to prevent repetitions and unnecessary references. 1st, The objects observed are understood to be lucid or illumined points, and so distant, that the rays which flow from either of them may be esteemed parallel without error as far regards these observations: such objects are the fixed stars, any given points in the disks of the sun or planets, &c. 2dly, As in measuring the angular positions of objects which lie in the same plane, these objects are referred to the circumference of a circle, the centre of which is coincident with the spectator's eye; so in estimating the positions of objects which lie in different planes, and of the inclinations of these planes to each other, the objects, &c. are referred to the circumference of a sphere, of which the centre coincides with the centre of the spectator's view: applying this to the present case, since the lines CT, CB, SG (fig. 1.) are situated in different planes; in order to estimate their positions, any point may be assumed as the centre of a sphere, and through that point lines are to be drawn parallel to the given lines CT, CB, SG, the points in

 which

which the lines interſect the ſpheres ſurface will give their relative ſituations by the rules of trigonomety. 3dly, There will be no neceſſity to repreſent the reflecting planes in the general conſtruction, ſince the poſitions of the perpendiculars to the planes will give the ſituations of the planes themſelves.

6. To determine by conſtruction the angle ſubtended by the objects T, S, from the data which have been deſcribed, let APOCQ (fig. 2.) repreſent a great circle of the ſphere to the ſurface of which the objects obſerved, and the poſitions of the incident and reflected rays, &c. are referred; C being the center, CK the axis, and K the pole of this great circle; through K draw any ſecondary KO, and from the pole K, at the diſtance of the arc KF, = the meaſure of the given inclination of the reflecting planes to the plane of motion, deſcribe a parallel or leſſer circle FIM: with the pole F, and at a diſtance equal to a quadrant, deſcribe an arc of a great circle interſecting the ſecondary KO produced in the point X, and in this arc from X take XY = the meaſure of the given inclination of the fixed plane of reflection at the ſpeculum B to the ſecondary which paſſes through the point O; and draw the quadrant YF, which produce in the direction YF: from F on either ſide of F ſet off FD equal to the meaſure of the given conſtant angle of incidence at the ſpeculum B, and make FB (taken on that ſide of F which is oppoſite to D) equal to FD. Draw the radius CO: from O ſet off an arc OP in the circumference OPA equal to the meaſure of the angular diſtance deſcribed by the moveable radius CP from that poſition at which the reflectors are parallel; obſerving that the arc OP be on that ſide of the point O which * correſponds with the conditions of the problem (art. 4.): through P deſcribe the ſecondary KP interſecting the parallel FIM in the point I: through B and I deſcribe

* It is ſuppoſed to be known, whether CP beginning its motion from the poſition CO approaches towards the viſual ray BG or recedes from it.

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the arc of a great circle BIE, and in it take EI equal to IB: through D and E draw the arc of a great circle DE: the arc DE will be the measure of the true angle subtended by the objects observed, according to the data of the problem.

Previous to the demonstration of this construction, the application of it to the method of observation by two reflections should be described. Join CP, CI, and CF. To the extremity C of the radius CP let a plane speculum be affixed, CI being always perpendicular to this plane: as PC revolves in the plane of motion, the perpendicular CI will describe the parallel or lesser circle FIM, and when CP coincides with CO, CI will coincide with CF. Through B draw BR parallel to CF, and let a plane speculum be fixed at B perpendicular to BR; CF and BR being parallel when the perpendicular CI coincides with CF, the reflectors at C and B will then be parallel.

Join CD, and produce it to a very distant point S, and through B draw GBS parallel to CDS; the reflectors C and B being parallel, and their perpendiculars coinciding with CF and BR, let a ray SC impinge on the reflector C: because FC is the perpendicular to the speculum C and the arc DF = FB by construction, these arcs being in the plane of the same great circle DBQ, it follows, that the ray SC will be reflected from C in the direction CB, impinging on the speculum B at the angle of incidence CBR; and since DC and BG are parallel by construction, and the parallel lines FC BR fall on them, the angles RBG, FCD, will be equal, and FCB or CBR = RBG. CB therefore being the ray incident on the speculum B will be reflected in the direction BG parallel to SC; and a ray SG coming directly from S will be seen coincident with the reflected ray BG. Here we observe, that the planes of reflection at C and B, that is, the planes DCB and CBG coincide, the reflectors being parallel.

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Let the radius CP move from the poſition CO, carrying with it the ſpeculum C and its perpendicular CI: then, EI being equal to IB by conſtruction, a ray impinging on C in the direction TEC will be reflected in the plane ECB, and becauſe ECI = ICB, the reflected ray will coincide with the line CB, and after reflection at B will * proceed in the direction BG, being coincident with the ray SG which comes directly from S. When the perpendicular CI leaves CF, the plane of reflection ICB becomes inclined to the plane of reflection DCBG with which it before coincided; but the poſition of the rays CB, BG, and of the perpendicular BR, remains unaltered; for which reaſon the plane GBCFD correſponds to the fixed plane of reflection deſcribed among the conditions (art. 4.). When CI was coincident with CF, the radius CP was coincident with CO, O being the initial point of the arc OP, deſcribed by the radius CP, denoting that when CP coincides with O, the reflectors being then parallel, the inclination of the ray SC obſerved after two reflections, and SG obſerved by direct rays parallel to SC, is nothing: the great circle KO, therefore, which paſſes through O and F, will be the fixed or primitive ſecondary to which the inclination of the fixed plane of reflection at the ſpeculum B is referred.

The demonſtration of the conſtruction will conſiſt of two parts. It muſt be firſt ſhewn, that the conditions or data of the problem are obſerved in the conſtruction. 2dly, That the magnitude of the arc ED, which meaſures the angle ſubtended by the obſerved objects is limited or determined by them.

Suppoſing the angle TCS to be of any unknown quantity, it has appeared, that according to the conſtruction, the rays which come from T, and are ſeen after two reflections at C and B, will be obſerved to coincide with the rays which come

* Supra.

directly from S. That the conditions of the problem are fulfilled in the construction is demonstrated thus:

1st, The inclination of the reflectors B and C to the plane of motion was constructed of the magnitude which is measured by the arc KF. KC is perpendicular to the plane of motion. CF is perpendicular to the reflector C, and the inclination of these two lines CK, CF, is measured by the arc KF; but the inclination of any two planes is the same as the inclination of two lines which are perpendicular to them; the inclination therefore of the reflector C to the plane of motion is measured by the arc KF, and the speculum B is equally inclined to the plane of motion with C by the construction, the perpendiculars CF and CI being parallel when both are situated in the plane of the same great circle DBQ.

2dly, KO being the secondary to which the position of the fixed plane of reflection DFB at the speculum B was referred, that given inclination will be equal to the angle OFB, which is measured by the arc XY according to the construction, FY being a quadrant.

3dly, Moreover, FD = FB, was constructed equal to the constant angle of incidence at the fixed speculum; CBR is the angle of incidence at the fixed speculum B, and it is equal to the angle BCF, because CF and BR are parallel by construction, and CB falls on them; FB, or its equal FD therefore is truly constructed the measure of the given constant angle of incidence at the fixed speculum B.

4thly, Because it has been shewn that CO is the position of the radius CP, when the reflectors are parallel, the arc OP is rightly constructed the measure of the angular distance of the radius CP from that position.

It remains only to demonstrate that these four given quantities, KF, OP, XY, and DF, limit the magnitude of the arc ED:

ED: through I and F draw the arc IF: then the given arc KF or KI, and the angle IKF, measured by the given arc PO, define the triangle IKF, and in it, therefore, the side IF and and the angle IFK are determined. If from IFK, the given angle DFK, measured by the arc XY, be subtracted, the remainder IFD, and IFB its supplement to 180° will be defined: the given arc FB, with the angle IFB and the arc IF, determine the angle IBF, and the arc IB, or its double BE: and the given arc BD, the arc BE, with the contained angle DBE before determined, define the arc ED, which is therefore the true measure of the angle subtended by the objects observed under the conditions fulfilled in the construction.

7. The computation of the observed angle DCE being for the present omitted, some consequences which follow from the construction may be inserted in this place, being either corollaries, or such truths as admit of easy geometrical deduction from the general proposition. The line DC will always be the position of the visual ray or line of collimation of the telescope used in the observation, and the inclination of it to the plane of motion will be measured by the complement of the arc DK to a quadrant. The line BC will be the position of the ray which passes between the reflectors B and C, and the inclination of it to the plane of motion will be measured by the complement of the arc BK to a quadrant. These arcs are left out of the figure, that the more material parts of the construction might not be confused by them.

8. Every thing else remaining, let the parallel FIM (fig. 3.) be projected on the plane of motion QOP. Through the points F and I draw the arc of a great circle NIFR. The observed * objects T and S, or, which is the same thing, the points of intersection at the sphere's surface E and D will be at equal

* Compare fig. 2.

perpen-

perpendicular distances from this arc, which may be demonstrated thus. Through the points E, D, and B, draw the arcs EN, DL, and BR, perpendicular to NIFR: then the triangles DFL, FBR, being equal, DL will be equal to BR; moreover, the triangles ENI, IRB, being equal, the arcs EN, RB, will be equal: from whence it follows, that EN = DL, or the perpendicular distances of the points E and D from the arc of a great circle which passes through the points I and F, are equal. It appears also, from the same construction, that the arc NL, intercepted between the two perpendiculars EN, DL, is equal to twice IF: for because the triangles EIN, RIB, are equal, as are the triangles DLF, RFB, it follows, that NI is equal to IR, and LF to FR, wherefore 2IR = NR, and 2RF = LR: whence, by subtracting equals from equals, 2RI − 2RF = NR − LR, or 2IF = NL, which was the equality to be demonstrated.

9. From this last construction and demonstration the following proportion is inferred. As radius : cosine of DL or EN, so is the sine of IF to the sine of half the arc ED, or of half the observed angle: for if the arcs NE, LD (fig. 3.), be continued until they meet in the pole H, the arcs NH, LH, will be quadrants, and the triangle EHD isosceles, which, from a property of spherics too obvious to need demonstrating, gives this proportion: as the chord of NL to the chord of ED, so is radius to the sine of DH, or cosine of DL; but the chord of NL is equal to the chord of 2IF from art. 8. We have, therefore, as radius : cosine DL, so is the chord of 2FI to the chord of ED, or, which is the same proportion, as radius : cosine DL, so is the sine of IF to the sine of half ED.

10. From the last article it appears, that the sine of half the angle between the observed objects, or the sine of half ED,

is proportional to the ſine of FI and the coſine of DL jointly; conſequently the ſine of FI being the ſame, ſin. ½ ED is proportional to the coſine of DL; this will lead to the reaſon why in enumerating (art. 4.) the * conditions which limit the magnitude of the obſerved arc ED, the poſition of the ſecondary KP, in reſpect of the point of interſection Q and of the fixed ſecondary KO, was annexed: for it will appear, that every thing elſe being the ſame, the magnitude of the arc ED will depend on the poſition of the ſecondary KP, whether it be on one ſide of the fixed ſecondary KO, or on the other, the angles PKO, *p*KO, being equal. Having ſet off O*p* = OP draw the ſecondary K*p* interſecting the parallel FIMU in the point U; and through B and U draw the arc of a great circle BUW; take UW = BU; and through D and W draw the arc of a great circle DW: then by the conſtruction and demonſtration in art. 6. the angle ſubtended by the obſerved objects will be meaſured by the arc DW, and it will be eaſy to ſhew, that DW is not equal to DE, except in two extreme caſes; that is, when the fixed plane of reflection DFB is either coincident with the primitive ſecondary KO or perpendicular to it. Through the points F and U draw the arc of a great circle VFU, and from D draw the arc DV perpendicular to VFU: ſince † the ſines of half the arcs DE, DW, are in a proportion compounded of the proportions of the ſine of IF to the ſine of FU, and of the coſine of DL to the coſine of DV, the ſines of IF, FU, being equal by the conſtruction, the ſines of half the arcs ED, DW, will be in the ſame proportion with the coſines of DL and DV, which are evidently unequal; conſequently, the ſines of half the arcs DE, DW, and therefore the arcs themſelves, muſt be unequal.

11. The angles PKO, OK*p*, remaining equal, when the fixed plane of reflection BFD (fig. 4.) is coincident with the

* Compare fig. 2. † Supra.

ſecondary

secondary KO, or at right angles to it, the perpendiculars DL, DV, become equal in both cases, which is obvious from the equality of the triangles DVF, DLF; it follows, therefore, (art. 10.) that the sines of FI and FU, and the cosines of DL, DV, being equal, the arcs DE, DW, will be equal in these two extreme cases, but in no other.

12. Since the angle subtended by the observed objects (art. 10.) depends only on the sine of IF and the cosine of DL, it is plain, that if the points D and B be interchanged, (fig. 2, 3, 4.) the angle observed will not be altered, every thing else remaining the same; because neither the sine of IF, nor the cosine of DL, is affected by this change. For this reason in any construction for measuring angles by two reflections, the position of the * visual ray may be altered into that of the ray BC passing between the reflectors, which will become in that case the situation of the visual ray, this alteration noways affecting the observed angles.

13. While the perpendicular CI (fig. 2. and 5.) describes the parallel FIM, the angle of incidence on the moveable speculum C, that is, the angle ECI or ICB, measured by the arc BI, continually increases until it arrives at a certain limit. This limit is determined by drawing through the points B and K the arc of a great circle BKM. When the perpendicular CI arrives at M, the arc BM is the greatest possible, which will therefore be the measure of the greatest angle of incidence on the moveable speculum, according to this construction, the radius CP having then described from O an arc which is the measure of the angle FKM. Now it is plain, that if the arc MB should be greater than a quadrant, there can be no vision by two reflections, when the perpendicular CI coincides with M (supposing the moveable speculum to reflect on one side only) because the angles of incidence and reflection on any speculum

* The position of the ray DC is the same with that of the ray BG parallel to it, when referred to distant objects.

muſt be leſs than 90°. If BM be leſs than a quadrant, an obſervation by two reflections may be taken when the radius CP is directed to any point in the circumference of the plane of motion. When the arc BM is greater than a quadrant, two other limits will be produced in the circumference of the plane OCP; while the radius CP is between theſe limits, no obſervation by two reflections can be taken: theſe limits are conſtructed thus (fig. 6.). BM being greater than a quadrant, with the pole B and diſtance BI equal to a quadrant, deſcribe the arc of a great circle I*i* intersecting the parallel FIM in the points I and *i*: through I and *i* draw the ſecondaries KY, KZ: while the radius CP is between Z, and Y no obſervation can be taken by two reflections. If BIE, B*i*E, be drawn equal to a ſemi-circle, and DE joined, then DE will be the meaſure of the limiting angle which can be obſerved by this conſtruction, either on one ſide of KO or on the other; and becauſe, by the principles of trigonometry, the arcs BD and DE are in the ſame great circle, BDE being a ſemi-circle, we ſhall derive from the conſtruction this concluſion: the difference between 180° and double the angle of incidence on the fixed ſpeculum, will be a limit which terminates the angle obſerved by two reflections in every caſe, when the arc BM is greater than a quadrant *.

14. In any given example formed on the principles which have been demonſtrated (fig. 2.) for the eſtimation of angles by two reflections, three of the four quantities neceſſary to determine the reſult muſt conſtantly be the ſame, while the fourth, that is, the arc OP, varies with the magnitude of the angle ſubtended by the objects obſerved: the different magnitudes of theſe three given quantities will cauſe a great variety of properties in conſtructions which depend on the general

* This termination of the angle which can be obſerved by two reflections may happen while the obſerved angle is increaſing or decreaſing during the revolution of the index in the plane of motion.

theory. If the angle DFK (fig. 2.), being the inclination of the fixed plane of reflection to the primitive secondary be $= 90°$, and the arc KF, or the inclination of the reflectors to the plane of motion, be $= 90°$ also, the construction will become that of HADLEY's instrument (fig. 7.), whatever be the magnitude of the arc DF, that is, of the angle of incidence on the fixed speculum B: in this case the points F and O, and the points I and P, coincide. Here IF or PO measures the inclination of the reflectors to each other; and because BF = FD, and BI = IE, by construction, it follows, that DE = 2PO, that is, the angle subtended by the observed objects is double to the angle at which the reflectors are inclined to each other. This is a known property of HADLEY's instrument, in which the visual ray, and the ray intermediate between the reflectors, are in the plane of motion, which is also expressed in the construction, DC and BC coinciding with the plane POC.

15. Bisect KO in F; then will KF = 45° (fig. 8.). The visual ray CD being coincident with the plane of motion, let the inclination of the reflectors to that plane be equal to 45°: moreover, let the angle * DFK = 180°; so shall D coincide with O, and B with K: this will afford a good example to the general theory. Let the radius CP move into any given position, carrying with it the speculum C and its perpendicular CI: here the observed object E and the point B are always equi-distant from I; and because BI is half a quadrant, by construction, it follows, that IE will be of the same magnitude, BE therefore will be a quadrant, and consequently E will coincide with P, being always in the plane of motion. The following properties are also derived from this construction. 1st, The arc DE subtended by the observed objects is equal to the arc described by the index or moveable radius CP from O;

* Compare fig. 2.

differing

differing in this from Mr. HADLEY's construction, in which the angle observed is equal to double the angle described by the moveable radius from the initial point of the arc O. While therefore the moveable speculum C is carried round by the radius CP in the plane of motion according to the new construction just described, the image of E moves with an angular velocity just equal to that of the radius, the motion of the image being, according to Mr. HADLEY's invention, always greater than that of the radius in the proportion of 2 to 1. 2dly, The angles of incidence and reflection on both surfaces are constantly the same, being equal to 45°. 3dly, BI (art. 13.) being always less than 90°, observations by two reflections may be taken all round the circle, that is, angles of any magnitude may be measured by this construction. It will not be difficult in practice to regulate the inclination of the plane reflectors to the plane of motion, with the other given quantities to their true magnitude. Let the reflectors B and C be brought parallel when the index or radius CP is directed to O, being the initial point of the arc OP: in order to examine whether the fixed plane of reflection BFD be coincident with the primitive secondary KO, it is only necessary to observe the angle subtended by two given objects when the index CP is on the different sides of the initial point O: if the index be directed to unequal distances from that point at the times of observation, a correction is required (art. 11.). To examine whether the inclination of the reflectors to the plane of motion be exactly 45°, let the index CP be directed to 180°: if the inclination of the reflectors to each other be not then = 90°, a correction must be applied. It will be known whether the inclination of the reflectors to each other be = 90°, by observing the two opposite horizons at sea, and at land by various obvious methods. These examinations

are

are wholly independent of the inclination of the telescope to the plane of motion, which is regulated to its true situation parallel to the plane OPA, by making the plane OPA fixed in regard to distant objects, and by observing if the images of objects E, seen after two reflections of the rays, describe the arc of a great circle while C is carried round the plane of motion. Any three fixed objects, at a sufficient distance, and situated in the same plane with the observer's eye, will be sufficient for making this adjustment.

Fig. 9. 10. and 11. represent the progress of the rays, and the position of the reflectors according to this construction. TC is a ray issuing from any object T in a direction parallel to the plane of the motion, and impinging on the speculum C, which is inclined to that plane at an angle of 45°: from hence it is reflected in the direction CB perpendicular to the plane OCA, and being there reflected by the speculum B proceeds in the given or constant direction BG parallel to the plane of motion OPA.

16. There is another construction which follows from the general theory, the description of which should not be omitted. This will require some little explanation. As before (fig. 12.) let OPA represent the plane of motion, K its pole, FIM a parallel or lesser circle projected on it, the distance of this parallel from the pole K being measured by the arc FK; let KO be the primitive secondary, and BFD the fixed plane of reflection on the speculum B coincident with it. The other parts of the construction * remaining, it has been demonstrated (art. 10.), that the sine of half the observed angle, that is, the sine of half ED, is proportional to the sine of IF, and the cosine of DL jointly. Every thing else being the same, it is manifest, that the sine of half ED will be proportional to the sine of IF: as therefore the arc KF, that is, the inclination of the reflecting planes to the plane of motion, is decreased,

* Compare fig. 2. and 3.

the

the angle measured or arc ED will become smaller at the same time, because FI decreases with KF, the angle IKF remaining. This property seems applicable to good purpose in measuring small angles, not only from the great extent of scale, which is here obtained, but from various advantageous circumstances, which will appear in the subsequent article, and from the computations annexed in those which follow.

In this construction the fixed plane of reflection is made coincident with the primitive secondary for various reasons: there are only two positions of that fixed plane which admit of easy and exact adjustments; these are when the fixed plane of reflection is either perpendicular (art. 11.) to the primitive secondary or coincident with it. The latter position is preferred exclusive of the advantages it possesses in common with the other, because it affords means for a very precise adjustment of the inclination of the reflecting planes to the plane of motion, that is, of the arc KF; for if the primitive secondary OKMD (fig. 12.) be produced, and in it DG be taken equal to four times KF, it is manifest, that when the perpendicular CI coincides with M, or, which is the same thing, when the radius CP is directed to 180°, the object E observed by two reflections will coincide with G, because BF = FD and BM = MG by construction. If then two given objects be observed when the index points to 180°, the inclination of the plane reflectors to the plane of motion will be one fourth part of the angle subtended by these objects.

Concerning the magnitude of the arc FB, being the measure of the angle of incidence on the fixed speculum, and of KF = the inclination of the reflectors to the plane of motion, it will appear, by the computations*, that the smaller they are both taken, every thing else being the same, the more exact will be the result of the observation; but both are limited by circumstances which

* Infra.

should

should next be described; these will be more obvious if an outline be annexed, representing this particular case of the theory adapted to the mensuration of small angles when reduced to practice. OAPC (fig. 13.) is the plane of motion, C the moveable speculum carried round in the plane of motion by the radius CP; a ray coming from any distant object T impinges on the speculum C, and being reflected in the direction CB, is there again reflected in the direction BG, passing along the axis of a telescope. A ray coming from another distant object S, inclined to the ray TC at a small angle enters the telescope parallel to the direction of its axis, which is coincident with BG, and consequently the images of the two objects S and T will be seen to coincide in the middle of the field of the telescope, the angle subtended by them being BGT, which must be determined by the subsequent computations.

17. By this figure (fig. 13.), without further argument, it is plain, that the magnitude of the reflecting speculum C limits the constant angle of incidence on B; for were that angle $= o$, the lines CB, BG, would coincide, by which means the ray BG and others adjacent to it would be intercepted from entering the telescope. The magnitude of the reflecting plane depends on the quantity of light required; if a circle of about 1 2 inches diameter be sufficient, and the perpendicular distance of the reflecting planes be made equal to five inches, the least angle of incidence, consistent with these conditions, will be about 7°. It is however to be remembered, that the magnitude of the reflectors should be adapted to the aperture of the telescope used in the observation. As the area of the speculum increases, the light admitted into the same telescope decreases, and these areas should be so proportioned as to afford equal quantities of light, so that the objects seen by two reflections, and by direct rays, may be nearly of equal brightness; but for the

ſake of conſtructing an example to this theory, the magnitude of the reflecting planes and the angle of incidence on the fixed ſpeculum B depending on it may be aſſumed of the value mentioned in this article.

18. The magnitude of the arc KF, or of the inclination of the reflecting planes to the plane of motion is limited by the angles which the obſerved objects ſubtend (art. 16.). Fig. 12. Becauſe the greateſt angle obſervable will be meaſured by four times the arc KF, it follows, that the arc KF muſt not be leſs than one fourth part of the greateſt angle intended to be obſerved by this conſtruction; if the inclination denoted by the arc KF be fixed at 10′, four times that angle being 40′ will be greater than the apparent diameters of the ſun or any of the planets.

19. It remains to infer from the preceding conſtruction (fig. 2), the actual meaſure of the angle ſubtended by the objects obſerved. This muſt be effected by computation, which will not only ſerve as an illuſtration of the theory, but afford means of eſtimating and comparing the errors in the angle deduced, occaſioned by the unavoidable errors in obſervation and practical conſtruction; an examination extremely uſeful in aſtronomical ſubjects: next to removing errors entirely from obſervations, which is ſcarcely to be hoped for, the leſſening, circumſcribing, and reducing them within known limits is an object of principal conſequence.

20. The conſtruction of fig. 2. remaining, through the points F and I (fig. 14.) draw the great circle FI. Biſect FI in Q, and through the points K and Q draw the arc KQ, which will be perpendicular to IF. To determine by computation the arc ED which meaſures the angle ſubtended by the obſerved objects, three ſpherical triangles, KQF or KIF, IFB, and DBE, muſt be ſolved, for which the data are evidently ſufficient;

or the value of ED may be obtained from the solution of two triangles KQF and DFL, with the proportion demonstrated in art. 9.

21. To proceed with the computation, through D draw the arc DL perpendicular to FI, and let the sine of QKF $= p$, being the sine of half the arc OP, the measure of IKF: put the sine of KF $= s$, the sine of DF $= m$, the sine of DFK $= n$, radius $= 1$. In the right-angled spherical triangle KQF, the properties of spherics give this proportion: as radius to the sine of KF so is the sine of QKF to sine of QF; wherefore sin. QF $= sp$; cos. QF $= \sqrt{1 - s^2p^2}$; and sin. FI (FI being double to QF) $= 2sp \times \sqrt{1 - s^2p^2}$. Moreover, because as rad. to cos. QK so is cos. QF to cos. KF, we have cos. KQ $= \frac{\sqrt{1-s^2}}{\sqrt{1-p^2s^2}}$; and sin. KQ $= \frac{\sqrt{s^2-s^2p^2}}{\sqrt{1-s^2p^2}}$. And since as rad. : sin. QFK so is sin. KF to sin. KQ; this proportion gives sin. QFK $= \frac{\sqrt{1-p^2}}{\sqrt{1-s^2p^2}}$; and because the sine of the angle LFD is the sine of the difference (or sum) of the angles QFK, DFK, of which the sines are, sin. QFK $= \frac{\sqrt{1-p^2}}{\sqrt{1-s^2p^2}}$ just found, and sin. DFK $= n$ by the data, we have from the rules of trigonometry,

$$\text{sin. * DFL} = \frac{\sqrt{1-p^2} \times \sqrt{1-n^2} \mp \sqrt{p^2n^2 - n^2s^2p^2}}{\sqrt{1-s^2p^2}},$$

and since in the right-angled triangle LDF, as rad. : sin. DF :: sin. DFL : sin. DL, and by the problem sin. DF $= m$ it appears, that

* If the points P and Q be on different * sides of the point O as they are represented in the construction, the last term will be affected with the sign $-$: if P and Q be on the same side of O, the sign of the last term will be $+$. It may be here observed, concerning the geometrical construction (fig. 2. and 3.) that when P and Q are on different sides of O, the angle observed ED will be greater than when those points are on the same side of the initial point O, the arcs OP, Op, being equal.

* Compare fig. 2.

$$\text{ſin. DL} = \frac{\sqrt{1-p^2} \times \sqrt{m^2 - m^2n^2} \mp \sqrt{p^2m^2n^2 - p^2n^2m^2s^2}}{\sqrt{1-s^2p^2}}, \text{ and}$$

$$\overline{\text{ſin. DL}}^2 = \frac{m^2 - m^2n^2 - p^2m^2 + 2p^2m^2n^2 - m^2n^2p^2s^2 \mp 2m^2pn \times \sqrt{1-p^2} \times \sqrt{1-s^2} \times \sqrt{1-n^2}}{1-s^2p^2}$$

and the ſquare of the coſine of DL

$$= \frac{1 - s^2p^2 - m^2 + m^2n^2 + p^2m^2 - 2p^2m^2n^2 + m^2n^2p^2s^2 \pm 2m^2pn \times \sqrt{1-p^2} \times \sqrt{1-s^2} \times \sqrt{1-n^2}}{1-s^2p^2}$$

The ſine of IF was ſhewn to be $2sp \times \sqrt{1-s^2p^2}$, and its ſquare $= 4s^2p^2 \times \overline{1-s^2p^2}$: moreover, it was demonſtrated in art. 9. that as $\overline{\text{rad.}}^2 : \overline{\text{coſ. DL}}^2 :: \overline{\text{ſin. IF}}^2 : \overline{\text{ſin. } \tfrac{1}{2}\text{ED}}^2$ which gives, by ſubſtituting the values of $\overline{\text{coſ. DL}}^2$ and $\overline{\text{ſin. IF}}^2$, and multiplying the $\overline{\text{coſ. DL}}^2$ into $\overline{\text{ſin. IF}}^2$, $\overline{\text{ſin. } \tfrac{1}{2}\text{ED}}^2 =$

$$4s^2p^2 \times \overline{1 - s^2p^2 - m^2 + m^2n^2 + p^2m^2 - 2p^2m^2n^2 + m^2n^2s^2p^2 \pm 2m^2pn \times \sqrt{1-s^2} \times \sqrt{1-p^2} \times \sqrt{1-n^2}}$$

and the coſine of $\tfrac{1}{2}\text{ED}^2 =$

$$1 - 4s^2p^2 \times \overline{1 - s^2p^2 - m^2 + m^2n^2 + p^2m^2 - 2p^2m^2n^2 + m^2n^2s^2p^2 \pm 2m^2pn \times \sqrt{1-s^2} \times \sqrt{1-p^2} \times \sqrt{1-n^2}}$$

finally the coſine of ED is therefore $=$

$$1 - 8s^2p^2 \times \overline{1 - s^2p^2 - m^2 + m^2n^2 + p^2m^2 - 2p^2m^2n^2 + m^2n^2s^2p^2 \pm 2m^2pn \times \sqrt{1-s^2} \times \sqrt{1-p^2} \times \sqrt{1-n^2}}.$$

22. The particular caſes inferred from the geometrical conſtruction may be compared with this analytical value of the coſine of ED, or of the angle ſubtended by the obſerved objects. If $s = 1$ and $n = 1$, by ſubſtituting 1 for s and n in the expreſſion juſt found, we ſhall have the coſine of ED $= 1 - 8p^2 + 8p^4$, which is the coſine of an arc four times greater than that of which the ſine $= p$. This anſwers to the properties of HADLEY'S inſtrument, in which KF or the inclination of the reflecting planes to the plane of motion is 90°, and its ſine $= 1 = s$: moreover, in HADLEY'S inſtrument, the fixed plane of reflection at the unmoved ſpeculum is parallel to the plane of motion, and therefore perpendicular to any ſecondary of that plane; its inclination to any ſecondary

condary therefore will be 90°, and the sine of this inclination $= 1 = n$ by the problem. And since p is the sign of half the inclination of the reflectors, the angle of which the cosine is $1 - 8p^2 + 8p^4$ will be twice the inclination of the reflecting planes, which is a property of HADLEY's instrument. In the analytical value of the cosine of ED, the last term is affected by two signs; these depend on the position of the secondary KP and the intersection Q in respect of the point O. If the secondary KP or the index CP be on the same side of O with the intersection Q (fig. 2.), the sign of the last term is negative: if CP and Q be on opposite sides of O, the sign of the last term will be positive; and when DFK $= o$ or 180°, the whole term vanishes, because in that case $n = o$. Also, if $m = 0$, $n = 1$, $s = 1$, or if $p = 1$, the last term vanishes. When $m = s = \frac{1}{\sqrt{2}}$, KF $= 45°$: in this case, if $n = o$ the construction will be that described in art. 15. and the cosine of the observed angle ED will equal $1 - 2p^2$, the other terms vanishing: and because $1 - 2p^2$ is the cosine of an arc double to that of which the sine $= p$, it follows, that the angle observed will be equal to the arc described by the index from o, of which the sine of one half is by the problem $= p$. In every case, when $n = o$, that is, when the fixed plane of reflection at the unmoved speculum coincides with the primitive secondary KO (fig. 2. and 12.), the cosine of ED $= 1 - 8s^2p^2 \times \overline{1 - s^2p^2 - m^2 + p^2m^2}$.

23. The sine of ED will be necessary (art. 27.) to ascertain the variation of ED from the truth occasioned by errors in the data; to obtain sin. ED let

$$1 - s^2p^2 - m^2 + m^2n^2 + p^2m^2 - 2p^2m^2n^2 + m^2n^2s^2p^2 \pm 2m^2np \times \sqrt{1-s^2} \times \sqrt{1-p^2} \times \sqrt{1-n^2}$$

$= d$: then (art. 21.) from the value of $\overline{\text{cos. } \tfrac{1}{2}\text{ED}}^2$ we have sin. ED $= 4sp \times \sqrt{d} \times \sqrt{1 - 4s^2p^2d}$. When s is very small, and

and $n=o$, $d=1-m^2+m^2p^2$ nearly, which gives

$$\text{ſin. ED}=4sp\times\sqrt{1-m^2+m^2p^2}\text{ nearly.}$$

24. The coſine of the obſerved angle repreſented by ED (fig. 2. and 14.) in the conſtruction, being computed from the four given quantities p, s, m, and n, if either of theſe ſhould deviate from its true value, the angle deduced will be erroneous: and from the general expreſſion for the coſine of ED, an eſtimation of this error will be obtained. In the inveſtigation, however, it muſt be obſerved, that although the ſmall increments or decrements of arcs or ſines are aſſumed proportional to the fluxions of theſe quantities, which is ſtrictly true only in the naſcent ſtate of the increments or decrements, yet when the given variations are in a practical ſenſe very ſmall, the eſtimation of correſponding variations will be in general ſufficiently exact for practical purpoſes.

25. Small increments and decrements, that is, ſmall variations, being aſſumed proportional to the fluxions of arcs and of their ſines and coſines, if the variation of the ſine or coſine of any given arc be known, the cotemporary variation of the arc will be for the moſt part inferred from the following proportions: as ſin. : rad. :: $-\dot{\overline{\text{coſ.}}}$: $\dot{\overline{\text{arc}}}$; and as coſ. : rad. :: $\dot{\overline{\text{ſin.}}}$: $\dot{\overline{\text{arc.}}}$ But theſe proportions muſt be uſed under reſtrictions very neceſſary to be inſerted in this place, being true when applied to the intermediate parts of the quadrant only and failing at the extremities; for example, at the very beginning of the quadrant, or at the very end of the ſemi-circle, the variation of the coſine is the verſed ſine of the arcs increment or decrement, which gives the proportion as ſin. : 2 × rad. :: $-\dot{\overline{\text{coſ.}}}$: $\dot{\overline{\text{arc}}}$, being wholly different from the former: in like manner, at the very extremity of the quadrant,

drant, the increment of the sine becomes the versed sine of the arcs last increment, which gives this proportion: as cos. : 2 × rad. :: $\dot{\overline{\text{sin.}}}$: $\dot{\overline{\text{arc.}}}$ And since in this case $\dot{\overline{\text{arc}}}$ = cosine, we shall have, $\dot{\overline{\text{arc}}} = \sqrt{2 \times \dot{\overline{\text{sin.}}}}$ radius being = 1. In the other parts of the quadrant which are not very near its extremity, $\dot{\overline{\text{arc}}} = \frac{\dot{\overline{\text{sin.}}}}{\text{cos.}}$; having given, therefore, the variation of the sine or cosine of any arc, the sine or cosine being known, the cotemporary variation of the arc itself may be obtained, when it is either at the very extremities of the quadrant, or at some distance from those extremities. The difficulty lies in ascertaining in what part of the quadrant the value of the $\dot{\overline{\text{arc}}} = \frac{\dot{\overline{\text{sin.}}}}{\text{cos.}} = \frac{-\dot{\overline{\text{cos.}}}}{\text{sin.}}$ begin to fail, and the value expressed by $\dot{\overline{\text{arc}}} = \sqrt{2 \times \dot{\overline{\text{sin.}}}}$ or $-\sqrt{2 \times \dot{\overline{\text{cos.}}}}$ to take place. This leads to a general proposition comprehending both these values for the arc's variation, extended to every part of the quadrant.

The proposition is this: the difference of the cosines is to the chord of the difference of any two arcs, as the sine of an arithmetical mean between them to radius; and the difference of the sines is to the chord of the difference, as the cosine of the same arithmetical mean to radius. Let AB, AF (fig. 15.) be the given arcs; BF their difference; BL, FH, the sines; CL, CH, the cosines of the arcs AB, AF, respectively; join CA, CB, CF, and FB; FB will be the chord of the difference of the arcs AF, AB. Through B draw BG parallel to CA; then HL = BG will be the difference of the cosines, and FG the difference of the sines. Bisect FB in D, so shall DA be an arithmetical mean between the arcs FA, BA; join DC, which will intersect FB at right angles in E: through D and E draw DK, EI, perpendicular

to

to CA: DK will be the ſine, and CK the coſine of the mean arithmetical DA: the ſimilar triangles CEI, CDK, FGB, give the following proportions:

HL or GB : FB :: DK : DC, and

GF : FB :: CK : DC, which was the propoſition to be demonſtrated *.

* When FB (fig. 15) is ſo ſmall in compariſon of FA, that FG ſhall be evaneſcent in compariſon of FH, FH and BL will be in the ratio of equality, and conſequently the ratio FH : FC equal to the ratio BL : BC, or to the ratio DK : DC; for this reaſon, and becauſe it has been proved, that as HL : FB :: DK : DC, it follows, that as HL : FB :: FH or BL : BC, that is, as the variation of the coſine is to cotemporary variation of the arc, ſo is the ſine of the varying arc to radius; and, for ſimilar reaſons, as the variation of the ſine is to the cotemporary variation of the arc, ſo is the coſine to radius.

If BA be ſo diminiſhed that FG ſhall bear a finite proportion to FH, and too great to be neglected, BL will not be either to FH or to DK in a ratio of equality: conſequently, FH or BL muſt no longer be ſubſtituted for DK: as BA becomes leſs, FB being ſtill ſuppoſed evaneſcent, DK approximates to the ſine of ½FB to which it is ultimately equal when B and F arecoinciding with A (fig.16.). In which caſe the proportion will become as HL or HA : FB or FA :: ½FA : to CA, that is, as the verſed ſine of FA is to the arc FA ſo is half the arc FA to radius, or ſo is the arc FA to diameter.

The propoſitions which have been demonſtrated, comprehend the variation of the arc expreſſed in terms of the cotemporary variation of the ſine or coſine in every part of the quadrant without limitation, it being only allowed to ſubſtitute the arc FB inſtead of its chord, theſe quantities approximating the more nearly to equality as FB is ſmaller, and being ultimately equal in their evaneſcent ſtate. Moreover, it will be eaſy from what has preceded to conſtruct a plane right-lined triangle, which ſhall be ſimilar to the mixtilinear triangle contained under an arc, its ſine and verſed ſine when they are diminiſhed ſine limite. Let FA (fig. 16.) be any arc, FA the chord, FH the ſine, CH the coſine of the arc FA. Biſect FA in D, join CD, and draw the right ſine DK: then will the plane right-lined triangle KDC continually approximate to ſimilarity with the mixtilinear triangle FDAH as FA becomes ſmaller, and the two triangles will be ultimately ſimilar when FA is vaniſhing.

3 By

From these geometrical proportions, having given any arc and the variation of its sine or cosine, the cotemporary variation of the arc may be estimated by computation in general for any part of the quadrant. Let the sine of any arc be s, the cosine $= c$, the chord of the arc's variation $= x$, the given variation of the cosine $= d$, or the given variation of the sine $= b$, radius $= 1$; then if the cosine of the arc increases by the difference d, the chord of the cotemporary decrease of the arc, or

$$-x = \sqrt{2s^2 - 2dc \mp \sqrt{2s^2 - 2dc^2 - 4d^2}}$$

and if the sine of the given arc increases by the difference b

$$+x = \sqrt{2c^2 - 2bs \mp \sqrt{2c^2 - 2bs^2 - 4b^2}},$$

which are the mathematically true values of the chord FB, and will approximate to the magnitude of the arc FB as that arc is continually diminished. The following expressions for the chord of the variation x are more compendious, and will be sufficiently near the truth when FB is very small.

$$-x = \frac{s}{c} - \sqrt{\frac{s^2}{c^2} - \frac{2d}{c}}$$

$$+x = \frac{c}{s} - \sqrt{\frac{c^2}{s^2} - \frac{2b}{s}}.$$

In these four expressions it must be observed, that the sine and cosine are supposed to vary by increase: should the variation be a decrement, the sign of x and of b or d must be changed.

26. Let the quantities p, s, m, n, vary by small increments $\dot{p}$, $\dot{s}$, $\dot{m}$, $\dot{n}$, respectively, then to obtain the cotemporary variation of cos. ED, because (art. 21.)

$$\text{Cos. ED} = 1 - 8s^2p^2 \times \overline{1 - s^2p^2 - m^2 + m^2n^2 + m^2p^2 - 2p^2m^2n^2 + m^2n^2s^2p^2 \pm 2m^2np \times \sqrt{1-p^2} \times \sqrt{1-n^2} \times \sqrt{1-s^2}},$$

by taking the fluxion of the equation we have

$$\overline{\text{Col. ED}} = -16s^2p\dot{p} \times \overline{1 - 2s^2p^2 - m^2 + n^2m + 2m^2p^2 - 4m^2n^2p^2 + 2n^2m^2s^2p^2}$$
$$\pm \frac{\overline{mnp \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times 3 - 4p^2}}{\sqrt{1-p^2}}$$
$$-16p^2s\dot{s} \times \overline{1 - 2s^2p^2 - m^2 + nm^2 + p^2m^2 - 2p^2m^2n^2 + 2p^2m^2n^2s^2}$$
$$\pm \frac{\overline{m^2np \times \sqrt{1-n} \times \sqrt{1-p^2} \times 2 - 3^2}}{\sqrt{1-s^2}}$$
$$+16s^2p^2m\dot{m} \times \overline{1 - n^2 - p^2 + 2p^2n^2 - s^2p^2n^2 \mp 2pn \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times \sqrt{1-p^2}}$$
$$+16s^2p^2m^2\dot{n} \times \overline{-n + 2p^2n - s^2p^2n \mp \frac{\sqrt{1-s^2} \times \sqrt{1-p^2} \times p \times 1 - 2n^2}{\sqrt{1-n^2}}}.$$

27. This value of $\overline{\text{col. ED}}$ is expressed in terms of the variation of the sines of the given quantities: if it be necessary to express $\overline{\text{col. ED}}$ in terms of the variation of the arcs themselves, it must first be considered to what part of the quadrant they belong: for example, if s be a sine of an arc b not very near the extremity of the quadrant, and the variation be $\dot{s}$, the cotemporary variation of the arc $\dot{b}$ will be $\frac{\dot{s}}{\sqrt{1-s^2}}$; but if the variable arc be nearly $= 90°$, and becomes exactly equal to it ultimately having varied by a small arc $\dot{b}$ of which the versed sine $= v$; then will $-\dot{s} =$ the versed sine of $\dot{b}$ and $-\dot{b} = \sqrt{2v}$. Lastly, if the variable angle approximates to 90°, but is not equal to it, and the variation of its sine should be $= \dot{s}$, the cotemporary variation of the arc must be obtained from the general theorem in art 25. When either of the two latter cases happen, the variation of the arc must be determined for each particular case; but it will be necessary to give a general expression for $\overline{\text{col. ED}}$ in terms of the variations of the given arcs, of which p, s, m, n, are the respective sines when these arcs are at some distance from 90°; this is contained in the next article.

28. Let the angle QKF $= a$ (fig. 14.); the arc KF $= b$; the arc DF $= c$, and the angle DFK $= d$; their respective increments being $\dot{a}$, $\dot{b}$, $\dot{c}$, and $\dot{d}$, their sines p, s, m, and n, and the cotemporary increments of their sines $\dot{p}$, $\dot{s}$, $\dot{m}$, and $\dot{n}$: from the proportion contained in art. 24. we shall have $\dot{p} = \dot{a} \times \sqrt{1 - p^2}$, $\dot{s} = \dot{b} \times \sqrt{1 - s^2}$, $\dot{m} = \dot{c} \times \sqrt{1 - m^2}$, and $\dot{n} = \dot{d} \times \sqrt{1 - n^2}$, which being substituted in the value of $\overline{\text{cos. ED}}$ last found will give

$$\overline{\text{Cos. ED}} = -16s^2 \times \sqrt{1-p^2} \times p\dot{a} \times \overline{1 - 2s^2p^2 - m^2 + n^2m^2 + 2m^2p^2 - 4m^2n^2p^2 + 2s^2m^2n^2p^2 \pm \frac{m^2np \times \sqrt{1-s^2} \times \sqrt{1-n^2} \times \overline{3-4p^2}}{\sqrt{1-p^2}}}$$

$$-16p^2 \times \sqrt{1-s^2} \times s\dot{b} \times \overline{1 - 2s^2p^2 - m^2 + n^2m^2 + p^2m^2 - 2p^2m^2n^2 + 2s^2m^2n^2p^2 \pm \frac{m^2np \times \sqrt{1-n^2} \times \sqrt{1-p^2} \times \overline{2-3s^2}}{\sqrt{1-s^2}}}$$

$$+16s^2p^2 \times \sqrt{1-m^2}\,m\dot{c} \times \overline{1 - n^2 - p^2 + 2p^2n^2 - s^2p^2n^2 \mp 2pn \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times \sqrt{1-p^2}}$$

$$+16s^2p^2m^2 \times \sqrt{1-n^2} \times \dot{d} \times \overline{-n + 2p^2n - s^2p^2n \mp \frac{p \times \sqrt{1-s^2} \times \sqrt{1-p^2} \times \overline{1-2n^2}}{\sqrt{1-n^2}}}.$$

This quantity (art. 23.) being divided by the sine of the observed angle, the variation of that angle or $\overline{\text{ED}}$ will be the quotient.

29. In the expression for $\overline{\text{cos. ED}}$ contained in art. 26. the variations $\dot{p}$, $\dot{s}$, $\dot{m}$, and $\dot{n}$, are arbitrary, as are $\dot{a}$, $\dot{b}$, $\dot{c}$, and $\dot{d}$, in the last article. If a condition be annexed to the variation of any of them, two or more may become dependant on each other; and their relation must be determined by the nature of the case. Moreover, if one or more of the given arcs and their sines should be correct, the variations corresponding and all the terms multiplied into them will vanish. To give an example of the use of these expressions before they are applied to the immediate purpose of examining the new constructions

described in art. 15. and 16. let it be required to assign what error is occasioned in observing a given angle with a HADLEY's sectant, in which the telescope is parallel to the plane of motion, but the two reflectors deviate from their perpendicular to that plane by a small angle $\dot{b}$. Suppose the error of half the arc pointed to by the index to be $\dot{a}$, and consequently the error of the sine of half that * arc $= \dot{a} \times \sqrt{1 - p^2} = \dot{p}$: in this case, because the inclination of the reflectors to the plane of motion is nearly $= 90°$, the variation of the sine will be equal to the versed sine of the small arc $\dot{b}$, by which the inclination deviates from 90°; let v be the versed sine of $\dot{b}$, then will $-\dot{s} = v$ (s varying by a decrement of v). Moreover, because a condition is annexed, which is, that the line of observation is parallel to the plane of motion, the variations $\dot{s}$, $\dot{m}$, and $\dot{n}$, will be dependent on each other. To investigate their relation let FO $= \dot{b}$ (fig. 7.) be the small arc which measures the deviation of the reflectors from the perpendicular to the plane of motion: then, because sin. DO $= m$, and sin. DOF $= n = 1$ by the problem, when F from having been coincident with O has moved through the arc OF, it is plain, that $\dot{n} =$ sin. DFO $-$ sin. DOF $=$ ($\dot{n}$ being a decrement); but, by the † properties of spherics, cos. DFO $= \frac{\sqrt{2v} \times \text{cos. DF}}{\text{sin. DF}} = \frac{\sqrt{2v} \times \sqrt{1 - m^2}}{m}$: and FO being very small, the

* p here, as in the general solution, denotes the sine of half the arc to which the index on the plane of motion is directed, that is, $p =$ the sine of one-fourth of the angle observed in Mr. HADLEY's construction.

† Fig. 7. as rad. : cotang. DF :: tang. FO : cos. DFO, that is, FO being very small, and therefore $\overline{\text{FO}}^2 = 2 \times$ versed sine of FO, as rad. : cotang. DF :: $\sqrt{2v}$: cos. DFO : by the problem sin. DF $= m$, and cos. DF $= \sqrt{1 - m^2}$, wherefore cot. DF $= \frac{\sqrt{1 - m^2}}{m}$, which gives cos. DFO $= \sqrt{2v} \times \frac{\sqrt{1 - m^2}}{m}$.

sine

sine of DFO $= 1 - \frac{v \times \overline{1-m^2}}{m^2}$, from which $1 =$ sin. DOF being subtracted leaves $\dot{n} = \frac{-v \times \overline{1-m^2}}{m^2}$; or because $\dot{s} = -v$, $\dot{n} = \frac{+\dot{s} \times \overline{1-m^2}}{m^2}$. Moreover, the* quantity $\frac{1-s^2}{1-n^2}$ in the nascent state of $\overline{1-s^2}$ and $\overline{1-n^2} = \frac{m^2}{1-m^2}$: and $\sqrt{\frac{1-s^2}{1-n^2}} = \frac{m}{\sqrt{1-m^2}}$. Making therefore in the general expression contained in art. 26. $\frac{-v \times \overline{1-m^2}}{m^2} = \dot{n}$, $-v = \dot{s}$, $\dot{a} \times \sqrt{1-p^2} = \dot{p}$, and $\frac{m}{\sqrt{1-m^2}} = \sqrt{\frac{1-s^2}{1-n^2}}$, $n = s = 1$, we shall have

$$\dot{\overline{\text{Cos.ED}}} = -16p\dot{a} \times \sqrt{1-p^2} \times \overline{1-2p^2} + 16p^2 v \times \overline{1-2p^2 + p^2m^2 \mp mp \times \sqrt{1-m^2} \times \sqrt{1-p^2}}$$
$$+ 16p^2 v \times \overline{1-p^2-m^2+m^2p^2 \mp mp \times \sqrt{1-m^2} \times \sqrt{1-p^2}} = -16p\dot{a} \times \sqrt{1-p^2} \times \overline{1-2p^2} +$$
$$16p^2 v \times \overline{2-3p^2-m^2+2p^2m^2 \mp 2mp \times \sqrt{1-m^2} \times \sqrt{1-p^2}}:$$

and because the sine of the observed angle is $4p \times \sqrt{1-p^2} \times \overline{1-2p^2}$, the error of the observation itself, that is,

$$\dot{\overline{\text{ED}}} = 4\dot{a} - \frac{4pv \times \overline{2-3p^2-m^2+2p^2m^2 \mp 2mp \times \sqrt{1-m^2} \times \sqrt{1-p^2}}}{\sqrt{1-p^2} \times \overline{1-2p^2}}.$$

In this example the position of the telescope has been supposed exactly in the plane of motion; should it be inclined to that plane at a small angle, of which the versed sine $= v$, the position of the reflectors and the arc pointed to by the index being correct, the general value of $\dot{\overline{\text{cos. ED}}}$ will give the error of the observation, or $\dot{\overline{\text{ED}}} = \frac{-4pv \times \sqrt{1-p^2}}{\overline{1-2p^2}}$ †.

30. To

* The nascent value of $\frac{1-s^2}{1-n^2} = \frac{-2s\dot{s}}{-2n\dot{n}}$; but $\dot{n} = \frac{-v \times \overline{1-m^2}}{m^2} = \frac{+\dot{s} \times \overline{1-m^2}}{m^2}$: wherefore $\frac{1-s^2}{1-n^2} = \frac{m^2}{1-m^2}$, when s and n are nearly $= 1$.

† When the position of the telescope only is erroneous, the points F and O coincide:

30. To examine in what degree an obſervation taken by the new conſtruction deſcribed in art. 15. is affected by known errors in the given quantities, let the reflectors B and C deviate by exceſs from their true angle of inclination to the plane of motion by a ſmall angle $+\dot{b}$: let the angle of incidence on the fixed ſpeculum be too great by the increment $\dot{c}$: let the fixed plane of reflection deviate from the ſecondary KO with which it ſhould coincide by a ſmall angle $\dot{d}$; and laſtly, let the error of the arc pointed to by the index be $= 2\dot{a}$; then theſe variations are arbitrary, no condition being annexed. Moreover, by the conſtruction $m = s = \frac{1}{\sqrt{2}}$, and $n = o$, which values being ſubſtituted in the general expreſſion contained in art. 18. we ſhall have.

$$\overline{\text{Coſ.}\dot{\text{ED}}} = -4\dot{a}p \times \sqrt{1-p^2} - 4p^2\dot{b} \times \overline{1-p^2} + 4p^2\dot{c} \times \overline{1-p^2} \mp 2\sqrt{2}p^3\dot{d} \times \sqrt{1-p^2};$$

and becauſe the ſine of the angle meaſured $= 2p \times \sqrt{1-p^2}$, the error of the obſervation required, or

coincide (fig. 7.) let the inclination of the teleſcope to the plane of motion with which it ſhould coincide, be meaſured by the ſmall arc Dd; then the correſponding variation of the angle DOK will be DOd. Let D$d = e$, and its verſed ſine $= v$; ſince the ſine of DO $= m$, and the ſine of DOK $= 1 = n$ by the problem, $\dot{n} =$ the verſed ſine of DOd; but DO$d = \frac{e}{m}$, and the verſed ſine of DO$d = \frac{e^2}{2m^2} = \frac{v}{m^2}$: wherefore $\dot{n} = \frac{-v}{m^2}$. This being premiſed, it appears from art. 26. when $\dot{p}$, and $\dot{s}$, are $= 0$, that

$$\overline{\text{coſ.}\dot{\text{ED}}} = +16s^2p^2m^2\dot{n} \times \overline{-n + 2p^2n - s^2p^2n}, \pm \frac{\sqrt{1-s^2} \times \sqrt{1-p^2} \times \sqrt{1-2n^2}}{\sqrt{1-n^2}}$$

$$+16s^2p^2m\dot{m} \times \overline{1 - n^2 - p^2 + 2p^2n^2 - s^2p^2n^2 \mp 2pn \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times \sqrt{1-p^2}}$$

in which quantity, ſubſtituting 1 for s, 1 for n, and $\frac{-v}{m^2}$ for $\dot{n}$, we ſhall have $\overline{\text{coſ.}\dot{\text{ED}}} = +16p^2v \times \overline{1-p^2}$, which being divided by the ſine of the obſerved angle $= 4p \times \sqrt{1-p^2} \times \overline{1-p}$, the quotient will be the variation of that angle or $\overline{\dot{\text{ED}}} = \frac{-4vp \times \sqrt{1-p^2}}{1-2p^2}$.

ED =

$$\dot{\overline{ED}} = 2\dot{a} + 2p\dot{b} \times \sqrt{1-p^2} - 2p\dot{c} \times \sqrt{1-p^2} \pm \sqrt{2}p^2\dot{d}.$$

It appears from the first term $2\dot{a}$, that an error in the arc pointed to by the index, causes an equal error in the observed angle; whereas a double error is caused by it in HADLEY's sextant, which gives the new construction considerable advantages: to counter-balance these the errors in HADLEY's construction, caused by a wrong position of the reflecting planes, &c. are almost evanescent; whereas the three last terms in the value of $\dot{\overline{ED}}$ just found, may become considerable, unless great care be taken in making the adjustments: the separation also of the images, when in contact, caused by any unsteadiness of the instrument will be greater than in Mr. HADLEY's construction *; but

* The variation of the observed angle $\dot{\overline{ED}}$ will shew how much the images seen in contact in the field of the telescope will appear to divaricate on any motion of the entire construction. For example, while the images of the observed objects are coincident in the field of the telescope, suppose that Mr. HADLEY's instrument were turned round in its own plane through a small angle: here s, n, and p, not being affected by this motion, it follows, that $\dot{s}$, $\dot{n}$, and $\dot{p} = 0$, $m =$ the sine of the angle of incidence on the fixed speculum, being the only quantity which suffers alteration: let its variation $= \dot{m}$, which will give from art. 26. the corresponding variation in the cosine of the observed angle, or

$\dot{\overline{\text{Cos. ED}}} = +16s^2p^2m\dot{m} \times \overline{1-n^2-p^2+2p^2n^2-s^2p^2n^2 \mp 2pn \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times \sqrt{1-p^2}}$ $= 0$, because $n = s = 1$. Wherefore any motion of the images in the plane of the instrument will not cause the least separation of them. Now suppose the whole to be turned on an axis situated in the plane of motion, and perpendicular to the telescope's axis: if the angular motion be measured by an arc of which the versed sine $= v$, the points in contact will be separated through an angle $= \frac{4vp \times \sqrt{1-p^2}}{1-2p^2}$, p being the sine of one quarter of the angle observed; but the quantity v being very small, when the angular motion does not exceed 30′, the divarication of the images will be inconsiderable. All oblique motions of the telescope's axis, and consequently of the image seen by direct rays, may be resolved into those that have

but in meaſuring the ſmaller angles, this ſeparation of the images, as well as the errors expreſſed by the three laſt terms will be greatly diminiſhed while that which is denoted by $2\dot{a}$ contained in the firſt term is not increaſed. On the whole, from the properties which have been demonſtrated to belong to this conſtruction deſcribed in art. 15. it may ſeem worthy of attention in practice, for ſome aſtronomical as well as other uſes.

31. By the ſame way of examination it may be judged, whether the method of obſerving by two reflections from plane ſurfaces be applicable to the menſuration of ſmall angles, according to the conſtruction deſcribed in art. 16. Let the errors of the four given quantities (as in the laſt article) be $2\dot{a}$ = the error of the arc

have been conſidered, which are perpendicular to each other: and from hence the reaſon appears, why the motion of a ſhip at ſea does not much diſturb the obſervation of angles by Mr. HADLEY's inſtrument.

The new conſtruction deſcribed in art. 15. is not ſo well adapted for obſervation where it cannot be ſteadily fixed. When the images are in contact, if the inſtrument be turned in its own plane through a ſmall angle e'' the ſeparation of the images will be $= 2e''p^2$ (becauſe $d'' = \sqrt{2}\,e''$, vid. p. 426.) p ſignifying the ſine of half the obſerved angle: this it is evident will moſt affect the obſervation of the larger angles; but in meaſuring thoſe that are ſmall, the divarication will become inconſiderable. Moreover, if the angular motion of the inſtrument be e'', when it turns round an axis in the plane of motion, and perpendicular to the teleſcope's axis, the ſeparation of the images will be $= 2e''p \times \sqrt{1-p^2}$, p. 426. which it is plain will moſt diſturb the obſervations of angles about 90°, but will ſcarcely alter the contact of the images, when the angles meaſured are very ſmall, or near 180°.

The objects obſerved and their images are here underſtood to be phyſical points: thus, when the two images of the ſun are ſeen by direct and reflected rays, and the limbs appear preciſely in contact, if by any motion of the inſtrument the contact is diſturbed, the points which before touched, being the obſerved objects, are ſaid to be ſeparated, whether the centres of the ſolar images approach or recede from each other, the ſeparation being eſtimated in the direction of an arc which paſſes through the centers of the two ſolar images.

Experience muſt determine in what degree this ſeparation of the images will diſturb obſervations taken at ſea with the new conſtruction.

3 pointed

pointed to by the index; $\dot{b}$ = the error of the inclination of the reflectors to the plane of motion; $\dot{c}$ = the error in the angle of incidence on the fixed speculum; and $\dot{d}$ = the inclination of the fixed plane of reflection to the primitive secondary with which it should coincide. Referring to the general value of $\overline{\text{cos. ED}}$, (art. 28.) and making $n = o$ we shall have

$$\overline{\text{Cos.}\dot{\text{ED}}} = -16 \times \sqrt{1-p^2}\, p s^2 \dot{a} \times \overline{1-2s^2p^2-m^2+2m^2p^2} - 16p^2 s \dot{b} \times \sqrt{1-s^2} \times \overline{1-2p^2s^2-m^2+m^2p^2}$$
$$+ 16s^2p^2\dot{c}m \times \sqrt{1-m^2} \times \overline{1-p^2} \mp 16m^2p^3s^2\dot{d} \times \sqrt{1-s^2} \times \sqrt{1-p^2},$$

and because s being very small, the sine of ED (art 23.) approximates to $4sp \times \sqrt{1-m^2+p^2m^2}$, the error of the observation itself, or

$$\overline{\dot{\text{ED}}} = \frac{4\dot{a}s \times \sqrt{1-p^2} \times \overline{1-2p^2s^2-m^2+2p^2m^2}}{\sqrt{1-m^2+p^2m^2}}$$
$$+ \frac{4p\dot{b} \times \sqrt{1-s^2} \times \overline{1-2p^2s^2-m^2+m^2p^2}}{\sqrt{1-m^2+p^2m^2}}$$
$$- \frac{4spm\dot{c} \times \sqrt{1-m^2} \times \overline{1-p^2}}{\sqrt{1-m^2+p^2m^2}}$$
$$\pm \frac{4m^2p^2s\dot{d} \times \sqrt{1-s^2} \times \sqrt{1-p^2}}{\sqrt{1-m^2+p^2m^2}}.$$

The first term of this expression gives the relation between any small variation in the arc pointed to by the index, and the corresponding alteration in the angle observed; if therefore the variation on the divided arc be any small angle $\pm 2\dot{a}$, + or − the first term will express the variation by which the observed angle is increased or diminished. According to the magnitude of m and s assumed for this construction m being the sine of 7°, and s = the sine of 10′, it appears, that at the very beginning of the scale one second of a degree in the angle observed corresponds to somewhat less than three minutes on the divided arc OP; that is, when $2\dot{a}$ = about 173″, $\overline{\dot{\text{ED}}} = 1''$, $\dot{c}$, $\dot{b}$, and $\dot{d}$, not being here considered. When $p = \frac{1}{2}$, the index then pointing to 60°, one second in the observed angle cor-

reſponds to about 199″ in the divided arc OP: when p is nearly $= 1$, the index being then directed to almoſt 180°, it muſt deſcribe above 2 degrees to make an alteration of 1″ in the obſerved angle. The ſecond term expreſſes the variation in the obſervation occaſioned by an error $\dot{b}$ in adjuſting the inclination of the reflecting planes to the plane of motion; but $\dot{b}$ (art. 18.) cannot exceed $\frac{1}{4}$ of the leaſt angle viſible in the teleſcope, conſequently the utmoſt value of the ſecond term cannot be ſo great as that leaſt angle, being at its limit when $p = 1$: it is manifeſt when p is ſmall, that the ſecond term is ſo much diminiſhed, as to be in a phyſical ſenſe evaneſcent. The ſame may be ſaid of the fourth term, containing the error of the optical adjuſtment $\dot{d}$, which beſides is multiplied into s the ſine of 10′. The third term is occaſioned by the error $\dot{c}$, for which, conſiderable latitude muſt be allowed, ſuppoſe 3′: to eſtimate the effect of this error on the obſervation, let a caſe be aſſumed: let the index be directed to 90° when an obſervation is taken for determining the angle ſubtended between two objects: then will $p = \frac{1}{\sqrt{2}}$; by ſubſtituting $\frac{1}{\sqrt{2}}$ for p, the ſine of 7° for m, the ſine of 10′ for s, and 180″ for $\dot{c}$ in the third term, we ſhall have, by computation, the value of that term, or the error in the obſervation occaſioned by this deviation of the angle of incidence from its true magnitude $= .''090$ not the tenth part of a ſecond. This is rather an unfavourable caſe, the variation being not much leſs than at its maximum when $p = \frac{1}{\sqrt{2}}$: if p is ſmall, or nearly $= 1$, the * variation will be wholly inſenſible.

32.

* The variation is a maximum when $p =$ the ſine of 35° 12′; and conſequently the arc pointed to by the index $= 70°\ 24'$. Subſtituting therefore the ſine of 35° 12′ for

32. From contemplating the value of the error $= \overline{ED}$, as expressed by the four terms just referred to, some further observations are suggested. It appears, that setting aside the error of the scale expressed by the first term (the angles measured being here supposed less than 22′ 56″ 33‴) all the others become smaller as the quantity p, and consequently the angle measured is diminished, p being multiplied into each of the three last terms. This is a material circumstance: for the same given error would affect the mensuration of the smaller angles in a greater proportion than the larger. Moreover, if $p = 1$, or approximates to that quantity, the index then pointing to an angle nearly = to 180°, the first term and the two last terms become almost evanescent, not only from the circumstances that have been considered, but from the quantities $\overline{1-p^2}$, $\sqrt{\overline{1-p^2}}$, which are multiplied into them. This also the observer may avail himself of: for supposing he should know, that the angle of incidence on the fixed speculum, and the fixed plane of reflection, are imperfectly adjusted, and even that the divided arc is incorrect, he may almost wholly avoid the errors which they would occasion, by so adjusting the inclination of the reflectors to the plane of motion, in respect of the angle measured, that

for p in the third term, (p. 429.) every thing else remaining, the maximum of variation in the observed angle on account of 3′ difference in the angle of incidence on the fixed speculum will appear to be = ″.098.

The error of an observation occasioned by a variation $\dot{c}$ in the angle of incidence on the fixed speculum, can never exceed the $\frac{1}{1637}$ part of $\dot{c}$, which is the maximum of error, the angle measured being = 22′ 56″ 33‴, the index then pointing to 70° 24′. The error, caused by a small angle $\dot{d}$, at which the fixed plane of reflection is inclined to the primitive secondary with which it should coincide, cannot exceed the $\frac{1}{14996}$ part of $\dot{d}$, which is its maximum when the angle measured is 32′ 33″ 3‴, the index then being directed to 109° 20′.

the index ſhall point to ſome degree near to 180°, which is done by making that inclination very little more than one-fourth of the angle to be obſerved.

33. The tables I. and II. are calculated for taking the diameters of the ſun and planets: the conſtruction being formed on the principles which have been explained (art. 16. 17. 18. 31. 32. fig. 12. 13.) The fixed plane of reflection is coincident with the primitive ſecondary, and conſequently $n = 0$: the common inclination of the reflectors to the plane of motion = 10′; and the conſtant angle of incidence on the fixed ſpeculum = 7°.

TABLE I.

Arc pointed to by the index.	Obſerved angle.			Diff.	
	′	″	‴	″	‴
1°	0	20	47	20	47
2°	0	41	34	20	47
3°	1	2	21	20	47
4°	1	23	8	20	46
5°	1	43	54	20	46
6°	2	4	40	20	45
7°	2	25	25	20	45
8°	2	46	10	20	44
9°	3	6	54	20	43
10°	3	27	37		

TABLE II.

Arc pointed to by the index.	Obſerved angle.			Diff.	
	′	″	‴	″	‴
100°	30	32	51	13	28
101°	30	46	19	13	20
102°	30	59	39	13	12
103°	31	12	51	13	3
104°	31	25	54	12	54
105°	31	38	48	12	46
106°	31	51	34	12	37
107°	32	4	11	12	28
108°	32	16	39	12	19
109°	32	28	58	12	11
110°	32	41	9		

The

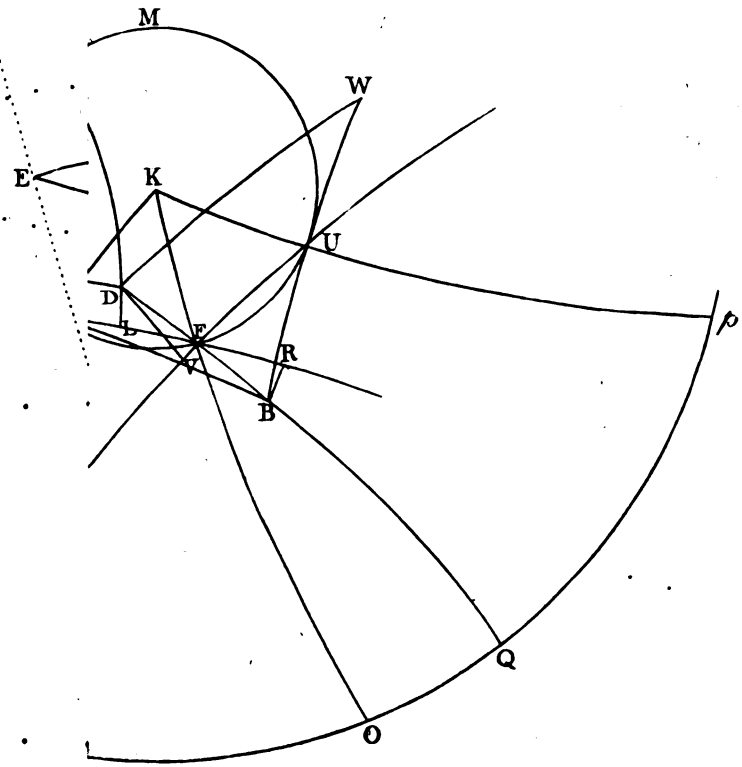
The divarication of the images while they traverse the field of the telescope during the time of an observation, and the errors of the observed angle in consequence of any change in the quantities m * and n (p. 429) should they appear to be of sensible magnitude, may be diminished until they are in physical sense evanescent, by altering the values of s and m (art. 31.): for this purpose it will be requisite to use two separate constructions; the one for observing very small angles, those, for example, which do not exceed 2′: and the other, for measuring such angles as are subtended by the sun and moon. In the former of these s may be assumed = the sine of 30″, m = sin. 7°, and n = 0. From these data tab. III. is calculated. In the other construction, because very small reflecting surfaces are necessary to observe the sun by two reflections (art. 17.) m may be assumed = sin. 1°, s being = sin. 10′, and n = 0: from which conditions the fourth table is calculated. These tables may be easily extended, by calculating from the value of cos. ED or $\overline{\text{sin. } \tfrac{1}{2}\text{ED}}^2$ contained in art. 21. or 22,

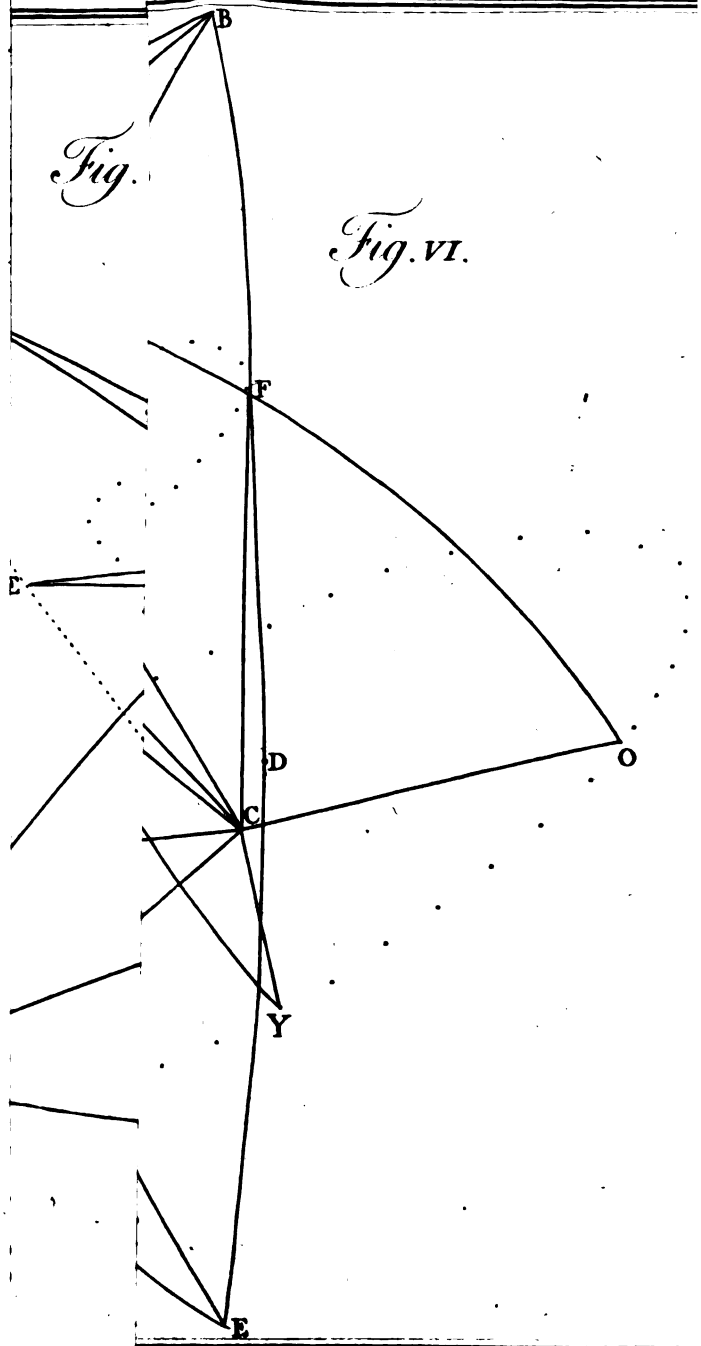
* Let the telescope with the entire construction be steadily fixed: then if the objects observed be in contact while the touching points occupy the center of the field, the angle of incidence on the fixed speculum will be of its true magnitude; that is, its sine or m will = sin. 7° in tables I. II. and III.; and m = sin. 1° in table IV. The fixed plane of reflection also will be coincident with the primitive secondary in all the constructions corresponding to tables I. II. III. and IV. The telescope continuing unmoved, the diurnal motion of the heavens will cause the points in contact to leave the center of the field; this will occasion no alteration in the quantities s and p, but will affect m and n only: therefore, $\dot{s}$ and $\dot{p} = 0$. To estimate the effects of this change in the values of m and n, let $\dot{m} = \dot{c} \times \sqrt{1 - m^2}$, and $\dot{n} = \dot{d} \times \sqrt{1 - n^2}$, as in art. 28. and suppose a line to be drawn through the centre of the field in a plane, perpendicular to the plane of motion: this line will be in the fixed plane of reflection; and any deviation of the points in contact through a small angular space $\dot{c}$, from the center of the field will cause an equal variation $\dot{c}$ in the

the angle of incidence on the fixed ſpeculum; the correſponding ſeparation of the images will be $= \frac{4spm\dot{c} \times \sqrt{1-m^2} \times \overline{1-p^2}}{\sqrt{1-m^2+m^2p^2}}$ (p. 429.) which is the greateſt poſſible when $p = \sqrt{\frac{\sqrt{9-10m^2+m^4}-3-3m^2}{4m^2}}$ = ſine of 35° 11′ 47″ (if m = ſin. 7°, and s = ſin. 10′, as in tab. I. and II.) being then $= \frac{\dot{c}}{1837}$; but if s = ſin. 30″, as in tab. III. the greateſt ſeparation will be only $\frac{\dot{c}}{36740}$, correſponding to leſs than one-third of a degree for the images motion through 10′ in the field of the teleſcope. In like manner let a line be drawn through the center of the field perpendicular to the line before deſcribed; any deviation of the points in contact through a ſmall angle $\dot{e}$ in the direction of this line will cauſe the plane of reflection at the fixed ſpeculum to be inclined to the primitive ſecondary with which it ſhould coincide at an angle $= \frac{\dot{e}}{m}$; this will occaſion a ſeparation of the images $= \frac{4mp^2s\dot{e} \times \sqrt{1-s^2} \times \sqrt{1-p^2}}{\sqrt{1-m^2+m^2p^2}}$ (page 429. $\frac{\dot{e}}{m}$ being there ſubſtituted for $\dot{d}$) which is the greateſt poſſible when $p = \sqrt{\frac{\sqrt{9-8m^2}-3-4m^2}{4m^2}}$. If m = ſin. 7° and s = ſin. 10′, as in tab. I. and II. the greateſt ſeparation of the images will be $= \frac{\dot{e}}{1828} = \frac{\dot{d}}{14996}$: but if s = ſin. 30″ and m = ſin. 7°, as in tab. III. it will only $= \frac{\dot{e}}{36560} = \frac{\dot{d}}{299920}$, which anſwers to a divarication of leſs than 1‴ for the images motion through 10′ in the field of the teleſcope, in the direction of the line above deſcribed, which is drawn through the center of the field, and perpendicular to the fixed plane of reflection. It muſt be remembered, that the ſeparations of the images here eſtimated are greater than can poſſibly happen in theſe conſtructions, when the index is directed to any other points of the circumference of the plane of motion (the diſtance of the images from the center the field not exceeding 10′) and are, even in this caſe, phyſically ſpeaking, evaneſcent.

TABLE

Fig. III.





BASIRE.

BASIRE Sc.

Fig. XV.

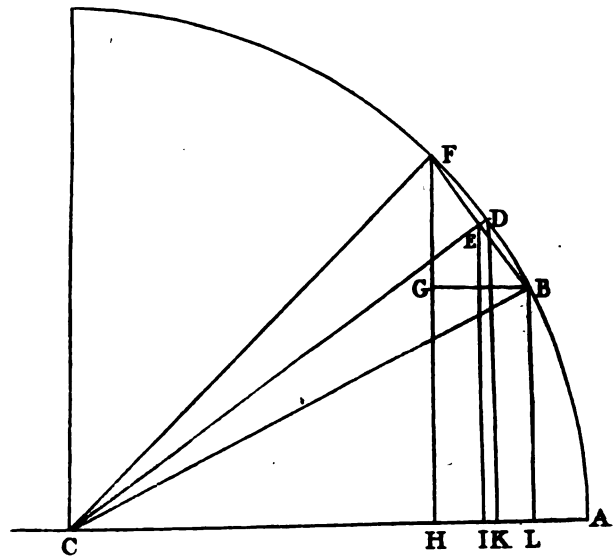


Fig. XVI.

TABLE III.

Arc pointed to by the index.	Observed angle.			Diff.
	′	″	‴	‴
100°	1	31	39	40
101°	1	32	19	40
102°	1	32	59	40
103°	1	33	39	39
104°	1	34	18	38
105°	1	34	56	38
106°	1	35	34	38
107°	1	36	12	38
108°	1	36	50	37
109°	1	37	27	36
110°	1	38	3	

TABLE IV.

Arc pointed to by the index.	Observed angle.			Diff.	
	′	″	‴	″	‴
100°	30	38	23	13	24
101°	30	51	47	13	15
102°	31	5	2	13	7
103°	31	18	9	12	58
104°	31	31	7	12	49
105°	31	43	56	12	41
106°	31	56	37	12	32
107°	32	9	9	12	23
108°	32	21	32	12	14
109°	32	33	46	12	6
110°	32	45	52		

XXVII. *An Account of the* Ophidium barbatum LINNEI. *By* P. M. Auguſtus Brouſſonet, *M. D.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read July 5, 1781.

THIS ſpecies of fiſh ſeems not to have been unknown to the ancients, though probably they confounded it with the *Conger*, to which it bears ſome reſemblance. Perhaps the early Greek and Latin writers on natural hiſtory have mentioned it under the name of *Tragus*, or *Callarias*; but for want of deſcriptions, they left us much in the dark concerning it. PLINY indeed ſpeaks of a fiſh which appears to be of this ſpecies: he calls it *Ophidion* *, and as that is the name given to it by all the modern writers, we are obliged to accept his ſynonymy without further inquiry.

The firſt author to whom we are indebted for a deſcription and figure of the *Ophidium* is BELLONIUS; yet it appears, that he was not certain of the name of this fiſh, ſince he calls it *Gryllus, falſo congrus, tragus, aſelli ſpecies*: nor was he leſs doubtful of the claſs to which he ſhould refer it, and therefore placed it among the *Aſelli*, or *Gadi*, though very different from the ſpecies of that family. RONDELETIUS, who wrote ſoon after BELLONIUS, has given us a better deſcription, and a more accurate figure of this fiſh, which he calls *Ophidion*,

* Ophidion or Ophidium, though the beſt is Ophidium, is not a good generic name, ſince it is a diminutive from Οφις.

 with

with a reference to PLINY. In the figure of BELLONIUS the cirri are very ill represented, and the whole fish appears without any spots, whereas in the plate of RONDELETIUS it is covered with oblong spots. This remarkable difference between the figures of these authors was sufficient to determine GESNERUS, and others who have written since their time, and who are to be considered rather as compilers than authors, to take the fish described by BELLONIUS to be a different species from that of RONDELETIUS.

WILLOUGHBY, who is the first ichthyologist who has given us any good description of fish, treats largely of the *Ophidium*; and in his account describes the scales, which are, as we shall hereafter explain, oblong, distinct, and disposed without any regular order. This description was sufficient to ascertain, that the difference between the figures arose from RONDELETIUS having drawn the scales omitted by BELLONIUS: yet the authors who wrote immediately after WILLOUGHBY, and particularly RAY in his Synopsis, follow GESNERUS, in maintaining two different species of *cirrata Ophidia*, one with, the other without, spots.

ARTEDI did not take notice of the spots; he describes the fish in a genus to which he gives the name of *Ophidion*, and places that genus among the *Malacopterygii*. After him KLEINIUS once more took notice of the spots; but at the same time introduced another confusion concerning this fish, arising from RONDELETIUS having said, that it has two cirri, while WILLOUGHBY asserts it has four; but it is easy to reconcile these authors, for though the *Ophidium* has only two cirri, yet each of these being divided in two, they appear as four; so that WILLOUGHBY might justly say, that it is quadri-cirratus. The same author places the *Ophidium* in a genus which he calls

Enchelyopus, which is indeed not a good family, since it comprehends the genera of *Gymnotus*, *Anarrhichas*, *Cepola*, *Blennius*, *Cobitis*, &c.

LINNE in his description of the *Ophidium barbatum* says, that its whole body is covered with oblong spots, without any regular direction. Dr. GOUAN, in his description of the genus of the *Ophidium*, does not mention the scales; but gives the spots as a generic character. The last author who has mentioned these spots, and given a description of this fish, is Mr. BRUNNICHE in his *Ichthyologia Massiliensis*.

Having adduced the various opinions of the writers on the *Ophidium*, and endeavoured to reconcile their sentiments, we now proceed to give the description of this fish, which is so very remarkable for its singularities. The genus of *Ophidium* has the following principal characters, *viz.* the body long; the fins of the back, tail, and anus, confounded in one; no fin on the under part of the body; and the eyes covered by the common skin. There are besides many other characters which it is needless to observe here, since I intend not to describe all the species of the genus, but only to mention them. The first species (which is the species of which we are treating) is distinguished by its cirri. The second differs from the former not only by the absence of the cirri, but also by many other marks. ARTEDI, in his account of this species, has adopted the synonymy of SCHONEVELDE, who describes a fish under the name of *Ophidion imberbe flavum*; but this fish, which is the *Blennius gunnellus* LINN. is certainly very different from the *Ophidion imberbe*, LINN. the rays of its dorsal fin being prickly; which circumstance perhaps induced LINNE to place the *Ophidium* among the *Acanthopterygii* in the first editions of his Systema, in which he followed the classification of ARTEDI. Perhaps, for the same

ſame reaſon, that author has placed among the *Jugulares* in his *Fauna Suecica*, the ſpecies of which we are now ſpeaking. This fiſh is alſo very different from the *Sea ſnail* deſcribed by WILLOUGHBY and PETIVER, which is the *Cyclopterus lipparis* LINN. though the ſynonymy of PETIVER has been accepted by GRONOVIUS as being the *Ophidium imberbe* *.

LINNE, in the tenth edition of his Syſtema, mentions a third ſpecies of *Ophidium*, which he calls *Macrophthalmum*; but afterwards diſcovered that this ſpecies belonged to the genus of *Cepola*. It is another ſpecies of this genus which OTHO FABRICIUS has lately deſcribed in his *Fauna Grœnlandica*, under the denomination of *Ophidium viride*. There are two other fiſhes, of which GRONOVIUS made a new genus, aſſigning to it the name of *Maſtacembelus*, which comes near to the genus of *Ophidium*, though I am perſuaded it conſtitutes a different one by the preſence of the aculei on the back. I ſhall now proceed to give a deſcription of that ſpecies of which I am particularly treating, which I have written in Latin for the ſake of perſpicuity.

DESCRIPTIO
OPHIDII BARBATI LINN.

B. 7. D. 124. P. 20. V. 0. A. 115. C. — —.

CAPUT compreſſum, ſub-acutum, nudum, cute communi laxe tectum.

Rictus amplus.

Mandibula ſuperior duplicata, inferiore paulo longior.

* The fiſh mentioned by Mr. PENNANT, in the Britiſh Zoology, tom. III. append. p. 346. and engraved in the tab. 93. tom. II. under the name of *Ophidium imberbe* LINN. is a ſpecies of *Muræna*.

Labia cutacea, tenuia.

Dentes in utriusque maxillæ margine, in aream angustam antice latiusculam dispositi, minuti, acuti, conferti, anteriores paululum majores.

Lingua subobtusa, glabra.

Palatum medio glabrum, antice areis tribus denticulorum exasperatum, quarum duæ laterales lineares, intermedia subtriangularis.

Fauces superne exasperatæ duabus areis longitudinalibus denticulorum.

Narium foramina utrinque bina, ante oculos posita, distantia; foramine anteriore minore, posteriore nudo.

Oculi supremi, vicini, magni, cute communi tecti: *iris* argentea; *pupilla* lutescens.

Opercula branchiarum cute communi tecta, rotundata polyphylla, mollia.

Membrana branchiostega, cute communi tecta, radiis subæqualibus.

Branchiæ quaternæ; tres interiores parte concava gerunt tubercula aspera, separata, in duas series disposita; quarta cæteris longior, latere interno gerit tubercula aspera, externo radios uno latere læviter serratos.

Cirri duo ad apicem maxillæ inferioris, bipartiti; *lacinia* altera longiore.

Tuberculum subacutum, seu apophysis ossea tecta, recumbens, ante oculos in fronte posita.

CORPUS. Compressum, versus caudam attenuatum.

Linea lateralis suprema, lævis, dorso parallela, vitta argentea subtus ornata.

Squamæ obovatæ, tectæ, umbonatæ, separatæ.

Pinna

Pinna dorsalis longa * anali longior sed angustior, cum caudali continuata, basi sordide albescens, margine nigra e punctis plurimis nigris: *Radii* simplices, flexiles, molles.

Pinnæ pectorales obovatæ, pellucidæ, membrana punctis minutissimis irrorata.

Pinna analis caudali unita, basi albescens, margine nigra, radiis simplicibus.

Pinna caudalis nigra, apice obtusa.

Color capitis et corporis carneo-argenteus.

Lc. : A :: 85 : 35 Lc. : PI :: 85 : 15
Lc. : DI :: 85 : 27 Lc. : PF :: 85 : 22
Lc. : DFCFAF :: 85 : 87 Lc. : AI :: 85 : 37
R. $5\frac{2}{3}$. P. 2. D. 2. 3. 4. 5. 6. A. 3. 4. 5. 6.

Before I proceed, I think it necessary to explain the proportional measures which I employed in the foregoing description. ARTEDI considered the measurement of fishes as necessary to render the description of them perfect; and therefore in the species which he described most completely, we find, he did not fail to express, with the greatest accuracy, the distances between the several parts. He has been followed in this by the best authors who have written upon fishes, as GRONOVIUS and PALLAS. The like practice has been followed by some writers in describing animals of other classes; and particularly by MC. DAUBENTON, who has, with extraordinary exactness, expressed the measurements of quadrupeds. The use of this measurement, however, can only ascertain that such or such individual is larger or less than another described by an author: for it is impossible by such means to determine a species, and therefore a single measurement, taken from the head to the tail, or in

* Pinna mihi audit longa, cujus basis longitudine excedit radium altiorem.

short

ſhort from any one part to another, would anſwer the ſame purpoſe; but if you take the terms of the meaſures, and compare them one with another, or all of them to a ſingle one, a method may be deduced for determining the ſpecies, and deſcribing each with accuracy, and that is the method which I uſed.

There is no doubt that fiſhes have a regular growth, and that all their parts have a proportional increaſe: it is therefore of no conſequence whether the meaſure be taken from an old or a young ſubject, it will be always of the ſame uſe for determining the ſpecies. The term to which I refer all the reſt, is taken from the diſtance between the apex of the upper jaw, and the baſis of the fin of the tail. The extremity of the upper jaw is the point from which I take the diſtance to all the other parts. To determine with more accuracy the junction of the fin to the tail, I incline the fin ſo as to form an angle with the tail; I take the diſtances with a pair of compaſſes, to avoid the irregularities of the ſurface of the body, which are infinitely various. When I have taken the diſtance with the compaſſes, I make a reference to a rule, which is divided in Engliſh inches, each of them ſub-divided in tenths. To exemplify my practice in a fiſh of the ſuppoſed length of 40 lines from the upper jaw to the tail, and from the ſame point to the anus of 20 lines, I ſay, that the diſtance from the head to the tail compared with the diſtance from the head to the anus is in this ſpecies as 40 to 20, which I expreſs in this manner: Lc. : A :: 40 : 20, which I thus reduce, Lc. : A :: 10 : 5. I then take the length from the upper jaw to the beginning of the dorſal * fin, and to its end: next from

* I meaſure the dorſal and anal fins at their baſis, never in their margin.

the

the ſame point to the baſis of the pectoral, to its extremity; and in the like manner for the ventral, anal, and tail fins; all which meaſures I refer to the common term 40. Admitting then the dorſal fin is diſtant from the upper jaw as ten, I write as before LC : DI :: 40 : 10, which I reduce, if I chuſe ſo, Lc. : DI :: 20 : 5.

I take the meaſures in length only, and never in breadth or depth, becauſe ſuch meaſures, on many accounts, for inſtance, from the quantity of ſpawn, from that of food in the ventricle, &c. would be uncertain; beſides there are terms enough afforded by the ſituation of the anus and the fins. I pay no regard to the meaſure of the noſtrils, of the eyes, &c. as theſe parts lie too near the extremity of the upper jaw to admit of a ſufficient accuracy in their meaſure. All theſe diſtances are written in the ſhorteſt manner poſſible; I therefore do not go into the inquiry what claſs of the LINNÆAN ſyſtem the fiſh deſcribed may belong to; or whether the anal fin is before or behind the region of the dorſal; whether the pectoral are extended beyond the ventral; or the tail is forked or not; and many other particulars which could not be expreſſed but by a very long deſcription, tedious both to the writer and reader.

To make an application of the proportional meaſurements, I ſuppoſe a fiſh, of a ſpecies deſcribed, to be in length 40 lines, and from the upper jaw to the anus 10: I take a ſpecimen which I ſuſpect to be of the ſame ſpecies, but which I wiſh to determine with certainty: the length of it is 20 lines; then I ſay 40 is to 10 as 20 is to another term, which I diſcover by multiplying 20 by 10, and dividing the product by 40, which will produce 5, the number of the lines comprehended between the upper jaw and the anus, if the fiſh be of the ſpecies it:

it is thought to be. I proceed in the like manner, in comparing the terms of the measures of the other parts; but the utility of these measures is very apparent in the distinguishing of the species of some genera which are so natural as that of *Cyprinus*, *Clupea*, and many others, of which the species could not be otherwise easily distinguished. I shall instance a species of *Perca* described in the *Commentarii Petropolitani* under the name of *Perca acerina*, by Mr. GUELDENSTAEDT, which could not have been distinguished from the *Perca cernua* but by the proportional measures; since the *Cernua* has the body about three times longer than the head, whereas the *Acerina* has it but twice as long, though by the other characters they are almost indistinguishable.

To express the position of the fins briefly, and with all possible accuracy, which, however, I think may be very well understood by the described measures, I take the distance from the upper jaw to the basis of the pectoral fin, and then see into how many equal parts the whole body may be divided, and to these parts I apply the name of regions; I measure them to the extremity of the middle of the fin of the tail, and I express the position of the fins as follow: D. 3. 4. A. 3. V. 2. P. 2. the letters being the initials of the fins, and the numbers of the regions, the first being from the head to the pectoral fin *.

The scales of the *Ophidium*, which have been figured by RONDELETIUS, but overlooked by many other writers, have

* I beg pardon for the digression; but I thought it would not be improper in this place to observe, that the utility of this method of measurement will appear not only in distinguishing fishes, but also animals of other classes, and particularly snakes, which cannot be well determined otherwise. Besides, I do not know any author on subjects of natural history, who has adopted that method.

been mentioned by WILLOUGHBY, but without any particular description. They are very different from those on the skin of the *Ophidium imberbe*, which are shortly described by GRONOVIUS. Their position, as may be seen in the figure, is irregular. They are dispersed over the whole body. Their form is sometimes round, sometimes nearly oval. They are larger near the head, and in the lower part of the body; but are hardly to be distinguished near the tail. They adhere to the body by means of a particular transparent skin, which is in general very thin, but somewhat thicker near the neck, and extended loosely over the whole head: this skin is very easily destroyed, after which the scales falling, the body appears spotted (fig. 1.). These scales are of the same sort as those that LEEUWENHOEK has described on the eel, like those I have seen on the *Anarrhichas lupus*, the *Blennius viviparus*, and many other fishes, which are commonly thought to be without scales. When you look at them with the naked eye (fig. 2.) they appear as covered with very small grains; but viewed through a microscope (fig. 3.) the middle of them appears more elevated than the margin; and from the center to the margin, close by each other, there are many lines or rays, formed by small scales placed one upon another, like tiles upon a roof, the superior being always the nearer to the center. This sort of scales, which may be called *umbonatæ*, are fastened to the body by very small vessels which are inserted in their middle; they are to be seen on the body only, not on the head nor the fins.

I shall now proceed to the anatomy of this fish, which certainly comprehends some very remarkable circumstances, which, I believe, have not yet been observed in any other species. When we have drawn off the skin there appears a thin membrane of a silver colour, which covers the muscles. The

muſcles being removed, we find the peritoneum, which lines the abdominal cavity, and is adherent to the ſwimming bladder by ſome elongations. It is of a ſilver hue, with ſome very ſmall black points. The ventricle is not to be diſtinguiſhed from the inteſtines by any other mark but by its ſize: its form is oblong; it is extended almoſt to the anus, from whence the inteſtinal duct has a retrograde courſe, and then deſcends again, having a little dilatation near the anus. On the vertebræ next the anus on the outſide of the peritoneum is a kind of cavity of an oblong form, containing a reddiſh viſcus, which I take to be the kidney.

The firſt vertebra from the head has nothing very remarkable in its ſtructure. The ſecond has on each ſide an elongated and ſharp apophyſis, to the apex of which is annexed a ſmall ligament. The third is very flat, and has on each ſide a kind of triangular and ſharp apophyſis, to which adheres a ligament as to the ſecond. The fourth is remarkable in having a ſharp apophyſis on each ſide, articulated with the body of the vertebra, and under each of them, is another articulated apophyſis, flattiſh, thick, roundiſh at its extremities, and forked at its baſis (fig. 5.). The fifth, which is ſtrongly adherent to the former, has in its middle a bifid proceſs. The ſixth has in its middle a flattiſh elevation, ſharp on each ſide. Between the extremity of the larger apophyſis of the fourth vertebra, is a bone, or rather a hard cartilage, which bears the figure of a kidney (fig. 6.) its convexity being turned towards the body of the vertebra: its poſition is parallel to the bodies of the vertebræ; its motion is half circular; one of its parts, *viz.* the loweſt, being in the cavity of the ſwimming bladder, to which it adheres by a thin membrane, ſo that no air can eſcape at that part. It is covered by membranes, which adhere ſtrongly to its middle, in this part are faſtened the two ligaments

ments of the apophysis of the second and third vertebræ, of which we spoke before, and which are of a great tenuity. In the same point are fastened also two ligaments each of which belongs to an oblong muscle parallel to each other, and fixed to the bones of the lowest and posterior part of the head (fig. 4.).

All this apparatus is certainly subservient to the purpose of swimming, I suppose, by the cavity of the bladder being made larger or less by the motions of the cartilagineous bone; but it is very remarkable, that if these parts are necessary to some animal function, they should not be found in all the individuals; for I have seen two, of which the vertebræ were not different from the vertebræ of the other species: which difference depends, perhaps, on the difference of sex. I am inclined to believe so; but the generation in this fish seems to be no less mysterious than that of the eel: I could never distinguish a male from a female in this species. I do not know if the other species of *Ophidium* have the same structure; I could not perceive it in some specimens of *Mastacembelus*. WILLOUGHBY mentions that singular structure, but without any particular description.

This fish commonly grows to the size of eight or nine inches. It is to be found in all the Mediterranean Sea, and in great plenty in the Adriatic. It is taken by nets in Provence and Languedoc, together with many other small species, which are not esteemed, that is, what they call *Ravaillā*. It is often confounded with the *Cepola* by the fishermen, though they have different names for each species. In Languedoc the *Ophidium* is called *Donzellā*, and the *Cepola*, *flammā*. In Provence the former has the name of *Corrudgiaö*, and the latter that of *Rougeollā*. But the name of *Donzellā*, very common on all the coast of the Mediterranean, is also applied to the *Cepola*, and the *Sparus julis*

LINN. which, however, is commonly called *girellā*. In ſummer the *ophidium* is more common: its fleſh is not of a good taſte, rather coarſe, as that of all the ſpecies of fiſhes, which having no ventral fins, are obliged to make great efforts in ſwimming, and have conſequently the muſcles harder. The want of ventral fins induces me to believe, that it is not a migratory ſpecies. It feeds upon ſmall crabs and fiſhes.

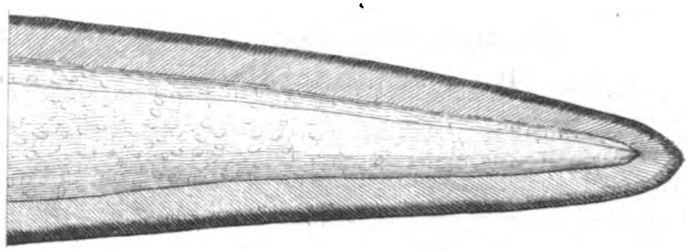


Fig. 5.

Fig. 6.

Fig. 3.

Fig. 2.

barbatum Linn.

XXIX. *A further Account of the Usefulness of washing the Stems of Trees.* By Mr. Robert Marsham, *of* Stratton, *F. R. S.*

Read May 31, 1781.

THE following account is a kind of postscript to my letter to Dr. Moss, Lord Bishop of Bath and Wells, in 1775, which the Royal Society did me the honour to publish in the Philosophical Transactions in 1777. In that I shewed how much a Beech increased upon its stem being cleaned and washed*; and in this I shall shew, that the benefit of cleaning the stem continues several years: for the Beech which I washed in 1775 has increased in the five years since the washing eight inches and six-tenths, or above an inch and seven-tenths yearly; and the aggregate of nine unwashed Beeches of the same age does not amount to one inch and three-tenths yearly to each tree. In 1776 I washed another Beech (of the same age, *viz.* seed in 1741); and the increase in four years since the washing is nine inches and two-tenths, or two inches and three-tenths yearly, when the aggregate of nine unwashed Beeches amounted to but one inch and three-tenths and a half. In 1776 I washed an Oak which I planted in 1720, which has increased in the four years since washing seven inches and two-tenths, and the

* Vide Phil. Transf. vol. LXVII. for the year 1777, part I. p. 12.

aggregate of three Oaks planted the same year (*viz.* all I measured) amounted to but one inch yearly to each tree. In 1779 I washed another Beech of the same age, and the increase in 1780 was three inches, when the aggregate of fifteen unwashed Beeches was not full fifteen inches and six-tenths, or not one inch and half a tenth to each tree; yet most of these trees grew on better land than that which was washed. But I apprehend the whole of the extraordinary increase in the two last experiments should not be attributed to washing: for in the autumn of 1778 I had greasy pond-mud spread round some favourite trees, as far as I supposed their roots extended, and although some trees did not show to have received any benefit from the mud, yet others did, that is, an Oak increased half an inch, and a Beech three-tenths, above their ordinary growth. Now though the Beech gained but three-tenths, yet, perhaps, that may not be enough to allow for the mud; for the summer of 1779 was the most ungenial to the growth of trees of any since I have measured them, some not gaining half their ordinary growth, and the aggregate increase of all the unwashed and unmudded trees that I measured (ninety-three in number of various kinds) was in 1779 but six feet five inches and seven tenths, or seventy-seven inches and seven-tenths, which gives but eight-tenths and about one-third to each tree; when in 1778 (a very dry summer in Norfolk) they increased seven feet and nine-tenths, or near eighty-five inches, which gives above nine-tenths to each tree: and this summer of 1780 being also very dry, yet the aggregate increase was above half an inch more than in 1778. But the best increase of these three years is low, as there are but twenty of the ninety-three trees that were not planted by me, and greater increase is reasonably expected in young than old trees;

yet

yet I have an Oak now two hundred years old * (1780) which is sixteen feet and five inches in circumference, or one hundred and ninety-seven inches in two hundred years. But this Oak cannot properly be called old. The annual increase of very old trees is hardly measurable with a string, as the slightest change of the air will affect the string more than a year's growth. The largest trees that I have measured are so far from me, that I have had no opportunity of measuring them a second time, except the Oak near the honourable Mr. LEGGE's Lodge in Holt Forest, which does not show to be hollow. In 1759 I found it was at seven feet (for a large swelling rendered it unfair to measure at five or six feet) a trifle above thirty-four feet in circumference, and in 1778 I found it had not increased above half an inch in nineteen years. This more entire remain of longevity merits some regard from the lovers of trees, as well as the hollow Oak at Cowthorp in Yorkshire, which Dr. HUNTER gives an account of in his edition of EVELYN's Silva, and calls it forty-eight feet round at three feet. I did not measure it so low; but in 1768 I found it at four feet, forty feet and six inches; and at five feet, thirty-six feet and six inches; and at six feet, thirty-two feet and one inch. Now, although this Oak is larger near the earth than that in Hampshire, yet it diminishes much more suddenly in girt, *viz.* eight feet and five inches in two feet of height (I reckon by my own measures as I took pains to be exact). Suppose the diminution continues about this rate (for I did not measure so high) then at seven feet it will be about twenty-eight feet in circumference, and the bottom

* I cannot mistake in the age of this Oak, as I have the deed between my ancestor ROBERT MARSHAM and the Copyhold Tenants of his Manor of Stratton, dated May 20, 1580, upon his then inclosing some of his waste; and the abuttal [illegible].

fourteen feet contain ſix hundred and eighty-ſix feet round or buyers meaſure, or ſeventeen ton and ſix feet; and fourteen feet length of the Hampſhire Oak is one thouſand and ſeven feet, or twenty-five ton and ſeven feet, that is, three hundred and twenty-one feet more than the Yorkſhire Oak, though that is ſuppoſed by many people the greateſt Oak in England.

I am unwilling to conclude this account of waſhing the ſtems of trees without obſerving, that all the ingredients of vegetation united, which are received from the roots, ſtem, branches, and leaves of a moſſy and dirty tree, do not produce half the increaſe that another gains whoſe ſtem is clean to the head only, and that not ten feet in height. Is it not clear that this greater ſhare of nouriſhment cannot come from rain? for the dirty ſtem will retain the moiſture longer than when clean, and the nouriſhment drawn from the roots, and imbibed by the branches and leaves, muſt be the ſame to both trees. Then muſt not the great ſhare of vegetative ingredients be conveyed in dew? May not the moſs and dirt abſorb the fineſt parts of the dew? and may they not act as a kind of ſcreen, and deprive the tree of that ſhare of air and ſun which it requires? To develope this myſterious operation of nature would be an honor to the moſt ingenious, and the plain fact may afford pleaſure to the owners of young trees; for if their growth may be increaſed by cleaning their ſtems once in five or ſix years (and perhaps they will not require it ſo often) if the increaſe is but half an inch yearly above the ordinary growth, it will greatly over-pay for the trouble, beſides the pleaſure of ſeeing the tree more flouriſhing. Although the extra increaſe of my firſt waſhed Beech was but four-tenths of an inch, the ſecond was nine-tenths and a half, and the third near two inches, ſo the aggregate extra increaſe is above one inch and one-

one tenth yearly; and the increase of the oak is eight-tenths. But calling it only half an inch, then six years will produce above five cubic feet of timber, as the oak is eight feet round, and above twenty feet long, and six pence will pay for the washing, so there remains nine shillings and six pence clear gain in six years.

Stratton,
Oct. 29, 1780.

XXX. *Hints relating to the Use which may be made of the Tables of natural and logarithmic Sines, Tangents, &c. in the numerical Resolution of adfected Equations. By* William Wales, *F. R. S. and Master of the Royal Mathematical School in* Christ's Hospital.

Read June 14, 1781.

THE first intimation that I can meet with relating to the use which may be made of the tables of sines, tangents, and secants, in resolving adfected equations, is in the latter part of the second volume of Professor SAUNDERSON's Elements of Algebra, printed in 1741, after his decease. The professor there shews how to resolve those two cases which make the first and second of the following examples, by means of the tables; but it appears, from many circumstances, he was not aware that the third case could be resolved in the same manner. All the three forms were, however, resolved by the late Mr. ANTHONY THACKER, a very ingenious man, who died in the beginning of the year 1744, by the help of a set of tables, of his own invention; different from, but in some measure analogous to, the tables of sines and tangents. These tables were finished and published, together with several papers concerning them, after his death, by a Mr. BROWN of Cleobury. In these papers, beside explaining fully the use of the tables in resolving cubic equations, Mr. THACKER shews that his method comprehends the resolution of all biquadratic equations, if they be

first reduced to cubic ones in the manner which has been explained by DESCARTES and others, and the second term then taken away.

Since that time M. MAUDUIT has shewn how to find the roots of all the three forms of cubic equations, by means of the tables of sines, &c. in his excellent Treatise of Trigonometry. But none of these authors have attempted to resolve equations of more dimensions than three, by these means, without first reducing them to that number; nor even these, before the second term, or that which involves the square of the unknown quantity, is taken away: whereas such reductions will generally take up more time than is required to bring out the value of the unknown quantity by the following method; and, after all, frequently serve no other purpose but that of rendering the operation more intricate and troublesome.

The truly ingenious Mr. LANDEN, in his lucubrations, published in 1755, has given a general method of resolving that case of cubic equations, by means of the tables of sines, where all the roots are real, without the trouble of taking away the second term of the equation: and Mr. SIMPSON has shewn how to resolve equations of any dimensions, by the same means, provided those equations involve only the odd powers of the unknown quantity, and that the co-efficients observe such a law as will restrain the equation to that form which is expressive of the cosine of the multiple of an arc, of which the unknown quantity is the cosine. This was first done, I believe, by JOHN BERNOULLI, and afterwards by Mr. EULER, in his *Introduct. ad Analyt. Infinit.* and Mr. DE MOIVRE, in his *Miscell. Analyt.*; but the resolution of all equations of this form, as well as many others, is comprehended in the first of the following observations.

The first thought of extending the use of the tables of sines, tangents, and secants, farther than to the cases which have been already mentioned, occurred to me while I was considering the problem which produced the equation given in this paper as the fourth example. And it is remarkable, that the very same thought occurred to Dr. HUTTON about the same time, and in the resolution of the same problem; and we were not a little surprized, on comparing our solutions together, to find that our ideas had taken so exactly the same turn; and that both should have stumbled on a thought, which, as far as either of us knew, had never presented itself to any one before. Having since examined farther into the matter, I have the satisfaction to find, that the principle is very extensive, and that a great number of equations, especially such as arise in the practice of geometry, astronomy, and optics, may be resolved by it with great ease and expedition.

But beside the facility with which the value of the unknown quantity is brought out by means of the tables of sines, tangents, and secants, this method of resolution has another considerable advantage over most others which have been proposed, inasmuch as the first state of the equation, without any previous reduction, is generally the best it can be in for resolution; and from which it may most readily be discovered, how to separate it into such parts as express the sine, or the tangent, or the secant of the arc of a circle; or into the sine, tangent, or secant of some multiple of that arc, or of a part of it; and in the doing of which consists the principal part of the business in question. It will also be of some advantage to preserve the original substitutions as distinct as possible, by using only the signs of the several operations which it may be necessary to go through

through in bringing the ſolution of a problem to an equation, inſtead of performing the operations themſelves.

Beſides the advantages which this method of procedure affords to the mode of reſolution now more particularly under conſideration, it has ſo many others over that which is commonly made uſe of, that I am much ſurprized the latter ſhould ever have been adopted. By preſerving thus the original ſubſtitutions diſtinct, all the way through an operation, every expreſſion, even to the final equation, will exhibit the whole proceſs up to that ſtep; and it will appear as clearly, how every expreſſion has been derived, as it does in that mode of analyſis which was uſed by the ancient geometricians: whereas, when the ſeveral original expreſſions are melted down into one maſs by the multitude of actual additions, ſubtractions, multiplications, and diviſions, which they generally undergo, in a long algebraical proceſs, conducted in the uſual manner, it is impoſſible to trace the ſmalleſt veſtige of the original quantities in the final equation, except ſuch as are repreſented by a ſingle letter. Of courſe, however obvious the ſeveral ſteps might be at the time when they were taken, every idea of them muſt be totally loſt in the reſult; and it will be utterly impoſſible to trace them back again, in the manner they are done in the compoſition of a problem, the ſolution of which has been inveſtigated by the geometrical analyſis*. Let me add, that it is to this cauſe

* This ſubject, if ever I am bleſſed with more leiſure than is at preſent my lot, ſhall be purſued farther in another paper: in which I ſhall endeavour to ſhew, that, notwithſtanding the great difference which there appears to be between algebra and geometry, they are really but one ſcience, differently treated; and that the operations of the former may be rendered as clear and perſpicuous as thoſe of the latter are allowed to be. A diſquiſition of this nature will at leaſt have the merit of reſcuing a very uſeful and expeditious mode of inveſtigation from

cause we must attribute all that obscurity which the algebraic mode of investigation has been so frequently charged with.

I shall endeavour to verify this doctrine, in some measure, by the expressions which are put down in the following tables for the sines, cosines, and tangents of arcs of circles, and of the multiples of those arcs; which tables will be found very useful in the prosecution of the design which I am now upon, and are absolutely necessary in the explanation of it.

from an unmerited stigma: and if I never be happy enough to have an opportunity of doing it myself, what I have here said may be the means of putting some other person, who has, upon it.

TABLE

	Tangent.
	$\frac{rx}{\sqrt{r+x \cdot r-x}}$
2	$\frac{2}{r+x \cdot r-x-x^2} \sqrt{r+x \cdot r-x}$
3	$\frac{3 \cdot r+x \cdot r-x-x^2}{r+x \cdot r-x-3x^2} \times \frac{rx}{\sqrt{r+x \cdot r-x}}$
4	$\frac{4 \cdot r+x \cdot r-x-4x^2}{r+x^2 \cdot r-x^2-6x^2 \cdot r+x \cdot r-x+x^4} \sqrt{r+x \cdot r-x}$
5	$\frac{5 \cdot r+x^2 \cdot r-x^2-10x^2 \cdot r+x \cdot r-x+x^4}{r+x^2 \cdot r-x^2-10x^2 \cdot r+x \cdot r-x+5x^4} \times \frac{rx}{\sqrt{r+x \cdot r-x}}$
	$\frac{\cdot r+x^2 \cdot r-x^2-20x^2 \cdot r+x \cdot r-x+6x^4}{\cdot r-x^3-15x^2 \cdot r+x^2 \cdot r-x^2+15x^4 \cdot r+x \cdot r-x} \cdot \sqrt{r+x \cdot r-x}$

	Tangent.
	x
2	$\frac{2r^2x}{r+x \cdot r-x}$
3	$\frac{x \cdot r+x \cdot r-x+2r^2x}{r+x \cdot r-x-2x^2}$
4	$\frac{4r^2x \cdot r+x \cdot r-x}{r+x^2 \cdot r-x^2-4r^2x^2}$
5	$\frac{x \cdot r+x^2 \cdot r-x^2+4r^2x \cdot r+x \cdot r-x-4r^2x^3}{r+x^2 \cdot r-x^2-4x^2 \cdot r+x \cdot r-x-4r^2x^2}$
6	$r^2 \cdot \frac{6x \cdot r+x^2 \cdot r-x^2-8r^2x^3}{r+x^3 \cdot r-x^3-12r^2x^2 \cdot r+x \cdot r-x}$

Tangent.
$\sqrt{x+r \cdot x-r}$
$\frac{2r^2 \cdot \sqrt{x+r \cdot x-r}}{r^2-x+r \cdot x-r}$
$\frac{3r^2-x+r \cdot x-r}{r-3 \cdot x+r \cdot x-r}\sqrt{x+r \cdot x-r}$
$\frac{4r^4-4r^2 \cdot x+r \cdot x-r}{r^4-6r^2 \cdot x+r \cdot x-r+x+r|^2 \cdot x-r|^2}\sqrt{x+r \cdot x-r}$
$\frac{5r^4-10r^2 \cdot x+r \cdot x-r+x+r|^2 \cdot x-r|^2}{r^4-10r^2 \cdot x+r \cdot x-r+5 \cdot x+r|^2 \cdot x-r|^2}\sqrt{x+r \cdot x-r}$
$\frac{6r^6-20r^4 \cdot x+r \cdot x-r+6r^2 \cdot x+r|^2 \cdot x-r|^2}{r^6-15r^4 \cdot x+r \cdot x-r+15r^2 \cdot x+r|^2 \cdot x-r|^2-x+r|^3 \cdot x-r|^3}\sqrt{x+r \cdot x-r}$

Tangent.
$\frac{r}{r-x}\sqrt{r^2-r-x|^2}$
$\frac{2r \cdot r-x}{2 \cdot r-x|^2-r^2}\sqrt{r^2-r-x|^2}$
$\frac{4r \cdot r-x|^2-r^3}{4 \cdot r-x|^2-3r^2 \cdot r-x}\sqrt{r^2-r-x|^2}$
$\frac{8r \cdot r-x|^3-4r^3 \cdot r-x}{8 \cdot r-x|^4-8r^2 \cdot r-x|^2+r^4}\sqrt{r^2-r-x|^2}$
$\frac{16r \cdot r-x|^4-12r^3 \cdot r-x|^2+r^5}{16 \cdot r-x|^5-20r^2 \cdot r-x|^3+5r^4 \cdot r-x}\sqrt{r^2-r-x|^3}$
$\frac{32r \cdot r-x|^5-32r^3 \cdot r-x|^3+6r^5 \cdot r-x}{32 \cdot r+x|^6-48r^2 \cdot r-x|^4+18r^4 \cdot r+x|^2-r^6}\sqrt{r^2-r-x|^2}$

Observations on the foregoing tables.

EACH of the formulæ in these tables may be considered as one side of an equation, involving the unknown quantity x to different dimensions. In some of the formulæ the odd powers of x are only found, in others the even ones alone, and in others both; but they are all equally useful in finding the value of the unknown quantity in adfected equations which contain all the powers of that quantity, as will plainly appear from the following considerations.

I. If, on bringing the solution of any problem to an equation with some known quantity, it be found to correspond with any of the formula in these tables; or, if by any means it can be reduced to any of them, it is manifest, that nothing remains to be done but to divide the known side of the equation by the value of the quantity which is here denoted by r, and to seek for the quotient in the tables of sines, cosines, or tangents, as the case may require, and the value of the unknown quantity will be the sine, tangent, secant, or versed sine, of a given part of that arc (according as the expression is found in the first, second, third, or fourth table) multiplied by the value of r.

II. If, as will more frequently happen, the final equation of an operation be found equivalent to the sum, difference, product, or quotient, of some two or more of these formula; or to the sum, difference, product, or quotient, of some two or more of them multiplied or divided, increased or lessened, by some known quantity or quantities; then, having taken away

the known quantities by the common algebraic rules, observe the following ones.

1st. When the equation is found to correspond with the sum or difference of two formula in these tables, which are the sine and tangent, sine and cosine, or cosine and tangent, of the same arc, by running the eye along the tables of natural sines and tangents, find these two arcs, immediately following one another, the sum or difference of the sine and tangent, sine and cosine, or cosine and tangent, of which are one of them greater, and the other less than the number which constitutes the known side of the equation. Take the excess of one of these sums or differences above, and what the other sum or difference wants of the said given number, add these two errors together, and say, as the sum of them is to 60″, so is that error which belongs to the less arc to a number of seconds; which being added to the less arc will give one, the sum or difference of whose sine and tangent, sine and cosine, or cosine and tangent, is exactly equal to the number which constitutes the known side of the equation. Of the arc, thus found, let such a part be taken as the table in which the formula are found directs, and the natural sine, tangent, secant, or versed sine (as the case may require) of this part, being multiplied by the value of r, if r be found in the equation, will be the value of x sought.

2d. When the equation happens to be the product or quotient of two formulæ which express the sine and cosine, sine and tangent, or cosine and tangent, of the same arc, take the logarithm of the number which constitutes the known side of the equation, and then follow exactly the directions given in the first case, using the tables of logarithmic sines and tangents instead of the tables of natural ones.

3d. If the equation, finally resulting from the resolution of any problem, present itself in an expression which is composed of the sum or difference of the sine, cosine, or tangent, of an arc, of which the unknown quantity is the sine, cosine, tangent, or versed sine, and the sine, cosine, or tangent, of some multiple of that arc, it will then be convenient to have two tables of sines and tangents; and in running the eye along them to find the two arcs immediately following one another, of which the sum or difference of the sine, cosine, or tangent, of one of them, and the sine, cosine, or tangent, of some multiple of it, may be less, and the sum or difference of the sine, cosine, or tangent, of the other, and the sine, cosine, or tangent, of the same multiple of it, may be greater than the number which constitutes the known side of the equation, for every minute of a degree that the finger is moved over in one, it must be moved over a number of minutes in the other, which is equal to the number of times that the single arc is contained in the multiple one. When these two arcs are found, the operation will not differ so materially from that which is pointed out in the first rule as to merit repetition.

4th. If, instead of the sum or difference of the sine, cosine, or tangent, of an arc, and the sine, cosine, or tangent, of some multiple of it, the form of the equation be such as to be constituted of the product of them, or the quotient of one divided by the other, the last rule will still hold good, using only the logarithmic sines and tangents instead of the natural ones, and comparing the sum or difference of them, according as the equation is composed of the product or quotient of the two factors, with the logarithm of the number which constitutes the known side of the equation, instead of that number itself.

5th. Sometimes the final equation will come out in expressions which are constituted of the sum, difference, product, or quotient, of the sine, cosine, or tangent, of some multiple of an arc, of which the unknown quantity is the sine, tangent, secant, or versed sine, and the sine, cosine, or tangent, of some other multiple of the same arc. And in any of these cases it is manifest, that the method of proceeding, in order to obtain one of the multiple arcs, and from thence the single one, of which the unknown quantity is the sine, tangent, &c. will not be greatly different from those which have been described in the third and fourth rules. The most material difference consists in this, that instead of proceeding minute by minute, according to the directions in the third rule to find the single arc, it will now be most convenient to proceed in each table by as many minutes at each step as are equal in number to the number of times which the single arc is contained in the multiple ones respectively.

6th. Equations will frequently make their appearance in formulæ which express the square, cube, &c. of the sine, cosine, or tangent, of the multiple of some arc, of which the unknown quantity is the sine, tangent, secant, or versed sine; or in formulæ which are expressive of the sum, difference, product, &c. of the sine, cosine, or tangent, of an arc, and some power of the sine, cosine, or tangent, of the same arc; or of some multiple of it, the unknown quantity being some other trigonometrical line belonging to that arc. Or the equation may be compounded of the sum, difference, product, &c. of the same, or different powers of the sines, tangents, or cosines, of different multiples of an arc, the unknown quantity being the sine, tangent, secant, or versed sine, of that arc. In every one of these cases the tables will give the value of the unknown quantity,

quantity, and in most of them with great ease and expedition. The method which is to be pursued in each case will readily present itself to a skilful analyst, who attends carefully to what has been already said, and to the examples which follow.

IV. The formula in the four preceding tables may be greatly varied by supposing x, the unknown quantity, to be some part or parts of the sine, tangent, &c. as $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{3}$, $\frac{3}{4}$, &c. or some multiple of it, as twice, thrice, &c. Or x may be the square, or the square root, or any other power of the sine, tangent, secant, or versed sine, of an arc; in every one of which cases the formula will put on different appearances, either with respect to the powers or co-efficients of the unknown quantity, and yet admit of the same kind of application.

V. The tables may be rendered yet more extensively useful by inserting expressions for the sines, cosines, and tangents, of half the arc which has x for its sine, tangent, secant, or versed sine; and also for the sines, cosines, and tangents, of the odd multiples of this half arc, which expressions, together with those already inserted, may be considered as the sines, cosines, and tangents, of the multiples of an arc, the unknown quantity, being the sine, tangent, &c. of twice that arc. And this consideration may sometimes be applied to very useful purposes.

VI. In order to render the formula in the tables more general, I have put r for the radius of the circle; whereas it will frequently happen, that the equation, finally resulting from the resolution of a problem, especially those which relate to the doctrine of the sphere, will present itself in a form where the radius must be taken equal to unity: what these forms are will readily appear by substituting unity for r and its powers every where in the expression.

It would be endless were I to undertake to enumerate all the various circumstances and cases in which this method of bringing out the unknown quantity may be applied with success: what has already been said will be sufficient to explain the nature of it, and to enable the analyst to apply it in other instances as they occur to him, I shall therefore only add a few examples to illustrate it.

EXAMPLE I.

Let it be required to find the value of x in an equation of the form $x^3 - r^2x = a$.

If r^2 be expounded by 50, and a by 120 (see Phil. Transf. vol. LXVIII. p. 937.) the equation may be reduced to $\sqrt{x} \times \sqrt{x^2 - 50} = \sqrt{120}$; and, consequently, by tab. III. if x be considered as the secant of an arc, of which the radius is $\sqrt{50}$, $\sqrt{x^2 - 50}$ will be the tangent of it, and we shall have to find an arc, such that the tangent multiplied by the square root of the secant may be equal to $\sqrt{120}$; or, which amounts to the same thing, such an arc that the log. tang. together with half the log. secant may be equal to half the log. of 120. But because the tangent and secant, here required, are to the radius of the $\sqrt{50}$, the log. tangents and secants in the tables must be increased by the logarithm of that number, and therefore log. tang. $+ \frac{1}{2}$ log. 50 $+ \frac{1}{2}$ log. secant $+ \frac{1}{4}$ log. 50 $= \frac{1}{2}$ log. 120: or log. tang. $+ \frac{1}{2}$ log. secant $= \frac{1}{2}$ log. 120 $- \frac{3}{4}$ log. of 50. Hence, having taken $\frac{3}{4}$ the log. of 50 from $\frac{1}{2}$ the log. of 120, run the eye along the tables of logarithmic tangents and secants until an arc be found of which the sum of the log. tangent and half the log. secant is equal to 19.7653631, the remainder.

In

In this manner it will be readily found, that the ſum of the log. tangent and half the log. ſecant of 28° 37′ is leſs than that difference by 2012, and that the ſum of the log. tangent and half the log. ſecant of 28° 38′ is greater than it by 1337: therefore 3349 (2012 + 1337) : 60″ :: 2012 : 36″. The exact arc, therefore, of which the ſum of the log. tangent and half the log. ſecant is equal to 19.7653631 is 28° 37′ 36″, and the log. ſecant of it is 10.0566242, which being increaſed by 0.8494850, the log. of $\sqrt{50}$ gives 0.9061092, which is the logarithm of 8.055810, the value of x ſought, and which is true to ſeven places of figures.

EXAMPLE II.

To find the value of x in an equation of the form $x^3 - r^2x = -a$.

If r be expounded by 3, and a by 10, as they are in the example, given at p. 433. of the Phil. Tranſ. vol. LXX. the equation will be $x^3 - 9x = -10$, and may be transformed to $\sqrt{x} \times \sqrt{9 - x^2} = \sqrt{10}$; and, therefore, by tab. I. the ſquare root of the ſine into the coſine of an arc, of which the radius is 3, is equal to the ſquare root of 10. Conſequently, an arc muſt be found, ſuch that the ſum of the log. coſine and half the log. tangent is equal to half the log. of 10. But becauſe the radius of this arc muſt be 3, the log. ſines and coſines muſt be increaſed by the log. of 3; and, therefore, log. coſ. + log. of 3 + ½ log. ſine + ½ log. of 3 muſt be equal to half the log. of 10; or, an arc muſt be found of which the ſum of the tabular log. coſine and half the log. ſine is equal to the difference between half the log. of 10 and 1½ the log. of 3. Hence, having ſubtracted 1½ log. of 3 from half the log. of 10, run the eye

along GARDINER's tables of logarithmic sines, by which means it will be readily found, that the sum of the log. cosine and half the log. sine of 28° 53′ 30″ is less than 19.7843181, the excess of half the log. of 10 above 1½ log. 3, by 15, and that the sum of the log. cosine and half the log. sine of 28° 53′ 40″ is greater than that difference by 60. Consequently 75 (15+60) : 10″ :: 15 : 2″. The exact arc, therefore, of which the sum of the log. cosine and half the log. sine is equal to 19.7843181, is 28° 53′ 32″; and the log. sine of this arc, increased by the log. of 3, is 0.1612153, the logarithm of 1.44949, the value of x required, true to the last place.

But many equations of this form, and this example among the rest, admit of two positive values of the unknown quantity; and by carrying the eye farther along the tables it will be found also, that the sum of the log. cosine and half the log. sine of 41° 48′ 30″ is greater than 19.7843181 by 50, and that the sum of the log. cosine and half the log. sine of 41° 48′ 40″ is too little by 21. Consequently, 71 (50+21) : 10″ :: 50 : 7″: of course, 41° 48′ 37″ is another arc, of which the sum of the log. cosine and half the log. sine is equal to 19.7843181, and the log. sine of this arc, increased by the log. of 3, is the logarithm of 1.999999, another value of x, and which errs but by unity in the seventh place.

The third root, as it is generally called, of this equation, which is necessarily negative, and equal to the sum of the other two, belongs properly to the equation which is given as the first example, of which it is the affirmative root, and may be found by the directions which are there given.

EXAMPLE

EXAMPLE III.

To find the value of x in an equation of the form $x^3 + r^2x = a$.

Let us take as examples of this equation $x^3 + 3x = .04$, $x^3 + 3x = .08$, and $x^3 + 3x = .12$, which are three of the inſtances given by Dr. HALLEY, in his Synopſis of the Aſtronomy of Comets, to illuſtrate the mode of computation that he purſued in conſtructing his general table for calculating the place of a comet in a parabolic orbit: and it is obvious, a being put for the known ſide of the equation, that it may be transformed to $\sqrt{x} \times \sqrt{3 + x^2} = \sqrt{a}$: where, if x be conſidered as the tangent of an arc, the radius of which is $\sqrt{3}$, $\sqrt{3 + x^2}$ will be the ſecant of that arc; and, conſequently, by what is ſhewn in the firſt example, an arc muſt be found, ſuch, that the ſum of the tabular log. ſecant and half the tabular log. tangent may be equal to the exceſs of half the log. of a above $\frac{3}{4}$ of the log. of 3. In the firſt of the above three inſtances this exceſs will be found, 18.9431891, in the ſecond 19.0937041, and in the third 19.1817497; and by running the eye along GARDINER's Tables of Logarithmic Sines and Tangents, it will be found, that the firſt falls between 0° 26′ 20″ and 0° 26′ 30″, the ſecond between 0° 52′ 50″ and 0° 53′ 0″, and the third between 1° 19′ 20″ and 1° 19′ 30″; and, by purſuing the mode which has been deſcribed in the two former examples, the exact arcs will be found 0° 26′ 27″,7, 0° 54′ 51″,7, and 1° 19′ 20″,1, and their reſpective tangents, to the radius $\sqrt{3}$, .01333248, .0266611, and .0399787, the three values of x ſought. And in this manner Dr. HALLEY's table may be extended to any length with the utmoſt eaſe, expedition, and accuracy.

3 Thus

Thus far this matter has been carried by former writers; but those who may be at the trouble of consulting them will find that I have not copied their methods: on the contrary, these which are given here are more plain and obvious than theirs are, and the operations considerably shorter. What follows has not, I believe, been adverted to by any before me.

EXAMPLE IV.

Let the equation arising from the proportion $a : b+x \,.\, \overline{1-c^2} :: c\sqrt{1-x^2} : c^2x$ be taken, which is the result of an inquiry into the situation of that place on the surface of the earth, considered as a spheroid, which is at the greatest distance from a given one, suppose London. In this inquiry a and b were put to represent the sine and cosine of the latitude of the given place (in the spheroid); c for $\frac{229}{230}$, the ratio of the axes; and x for the sine of the distance of the required place from the opposite pole (in the spheroid also). The equation, which is of four dimensions with all the terms, is manifestly $acx = \overline{b+x} \,.\, \overline{1-c^2} \times \sqrt{1-x^2}$, or $\frac{x}{\sqrt{1-x^2}} - x \,.\, \frac{1-c^2}{ac} = \frac{b}{ac}$; in which it is evident from tab. I. that the difference between the tangent and the product of the sine into a given quantity is known. In order, therefore, to find the value of x, compute $\frac{b}{ac}$, and $\frac{1-c^2}{ac}$, and find the logarithm of the latter. Now, because the elliptic meridian differs but little from a circle, the place sought will not be far from the antipodes of the given one, and its distance from the opposite pole may therefore be estimated at

39° 5′;

39° 5′; and, having taken out the natural tangent, and logarithmic ſine of this arc, add the logarithm of $\frac{1-c^2}{ac}$ to the latter, and find the number correſponding to the ſum, which will be leſs than the natural tangent of 39° 5′ by 2869. As this aſſumption is ſo near, take 39′ 6″ for the next, repeat the operation, and the reſult will be 1935 too great. Then 4804 (2869 + 1935) : 60″ :: 2869 : 36″; which being added to 39° 5′, gives 39° 5′ 36″, for the co-latitude of the place ſought, and the natural ſine of this arc, or .6305856 is the value of x in this equation.

EXAMPLE V.

Let the equation $x^3 + \frac{b^2-2a^2}{4a}x^2 + \frac{2a^2-b^2}{2}x - \frac{ab^2}{4} = 0$, be taken, which reſults from a ſolution of one caſe of the problem *de inclinationibus* of APOLLONIUS; but which, as it naturally riſes to a ſolid problem, was not, I conceive, conſidered by that celebrated author. The reſult of the analyſis, before any reduction takes place, is this proportion, $x+a : x-a :: 2\sqrt{ax} : b$; and hence, $\frac{x-a}{x+a}\sqrt{ax} = \frac{1}{2}b$. But it is here manifeſt, that if a be taken for the tangent of an arc, of which the radius is $\sqrt{ax}$, x will be the cotangent of it, and $\frac{x-a}{x+a}\sqrt{ax}(=\frac{1}{2}b)$ the coſine of twice that arc. Conſequently, we have to find an arc, the tangent of which is to the coſine of twice that arc as a is to $\frac{1}{2}b$; and this being done, the natural co-tangent of that arc, to the proper radius, will be the value of x.

Thus, let a be 10, and b 24; and the difference of the logarithms of a and $\frac{1}{2}b$ will be 0.0791812. Now, by running the eye along GARDINER's Tables of logarithmic Sines and Tangents, it will be readily seen, that the log. tangent of 26° 33′ 50″, when increased by 0.0791812, is less than the cosine of 53° 7′ 40″ twice that arc; and that the log. tangent of 26° 34′ 0″, when increased by the same quantity, is too great. And, by actually taking out the logarithms, and making the additions, the former will be found too small by 455, and the latter too great by 632. Then, 1087 (455 + 632) : 10″ :: 455 : 4″; which being added to 26° 33′ 50″ gives 26° 33′ 54″ for the arc of which x is the co-tangent. And if to twice the log. co-tangent of this arc the logarithm of a (10) be added, the sum (1.6020600) is the log. of 40, the value of x sought.

EXAMPLE VI.

The equation resulting from a solution of the famous problem of ALHAZEN may be given as another example of the use of this method. Many solutions of this celebrated problem, by HUYGENS, SLUSIUS, and others, may be met with in the Philosophical Transactions. Solutions to it may also be found at the end of Dr. ROBRRT SIMPSON's Conic Sections, in Dr. SMITH's Optics, Mr. ROBIN's Mathematical Tracts, and other places; but the most direct and obvious method is, perhaps, that which follows.

Put $a = DC$, $b = dC$, $r = CI$; $x = CB$ and $y = CE$, the cosines of the arcs IA, IH, to the radius r: then will the sines of those arcs, BA, EH, be expressed by $\sqrt{r^2 - x^2}$ and $\sqrt{r^2 - y^2}$; and,

and, becauſe of the ſimilar triangles ABC and BFC, HEC and dGC, $r : x :: a : \frac{ax}{r} = \text{CE}$, $r : \sqrt{r^2 - x^2} :: a : \frac{a\sqrt{r^2 - x^2}}{r} = \text{DF}$; $r : y :: b : \frac{by}{r} = \text{CG}$, and $r : \sqrt{r^2 - y^2} :: b : \frac{b\sqrt{r^2 - y^2}}{r} = d\text{G}$; conſequently, $\frac{ax}{r} - r = \text{FI}$, $\frac{bx}{r} - r = \text{GI}$; and becauſe the angles DIF, dIG, are equal by the nature of the problem, and the angles DFI and dGI both right angles, the triangles DFI and dGI are alſo ſimilar, and conſequently $\frac{a\sqrt{r^2 - x^2}}{r} : \frac{ax}{r} - r :: \frac{b\sqrt{r^2 - y^2}}{r} : \frac{by}{r} - r$; and $\frac{x}{\sqrt{r^2 - x^2}} - \frac{r^2}{a\sqrt{r^2 - x^2}} = \frac{y}{\sqrt{r^2 - y^2}} - \frac{r^2}{b\sqrt{r^2 - y^2}}$; or $\frac{r^2}{b\sqrt{r^2 - y^2}} - \frac{r^2}{a\sqrt{r^2 - x^2}} = \frac{y}{\sqrt{r^2 - y^2}} - \frac{x}{\sqrt{r^2 - x^2}}$: or, the co-ſecant of the arc HI $\div b$ – co-ſecant of AI $\div a$ = the co-tangent HI $\div r$ – co-tangent AI $\div r$; or, laſtly, the co-tangent of HI – co-tangent of AI = co-ſecant of HI $\times \frac{r}{b}$ – co-ſecant AI $\times \frac{r}{a}$. Conſequently, we have to find two arcs, the ſum of which is given, and ſuch that the difference of their co-tangents may be equal to the difference of the products of their co-ſecants into given quantities.

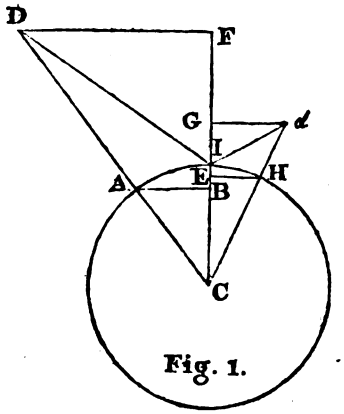


Fig. 1.

To do this aſſume the angle DCF as near as poſſible; and, becauſe the ſum of the two angles is given, the angle dCG will be known alſo. Take the difference of the logarithms of r and a, r and b, which will be conſtant, alſo the difference of the co-tangents of the two aſſumed arcs, and having taken out the log. co-ſecants, add to them reſpectively the two logarithmic

differences. Find the numbers correſponding to theſe two ſums, and if the difference of theſe two numbers be equal to the difference of the co-tangents, the angle DCF was rightly aſſumed; but as that will ſeldom happen, take the difference, or error; aſſume the angle DCF again, repeat the operation, and find the error as before. Then, as the ſum of the errors, if one of them was too great, and the other too little, or their difference, if both were too great, or both too little, is to the difference of theſe aſſumptions, ſo is the leſs error to a number of minutes and ſeconds, which muſt be added to that aſſumption to which the leaſt error belongs, if that aſſumption was too ſmall; or ſubtracted from it, if the aſſumption was too great: and, unleſs the firſt aſſumption was made very wide of the truth, which may always be avoided, the two angles will generally be obtained within a few ſeconds of the truth, and, by repeating the operation once more, to the utmoſt exactneſs.

Suppoſe DC (a) be taken equal to 72, dC (b) = 48, and the radius CI (r) = 40, the angle DCd being 82° 45′: then the whole operation will ſtand as follows:

r = 40 log.	11.6020600	- -	11.6020600
a = 72 log.	1.8573325	b = 48 log.	1.6812412
Conſtant log.	9.7446275	Conſtant log.	9.9208188

Now, in the two triangles DCI, dCI, the angles DIC and dIC being equal, and CI common, but dC conſiderably leſs than DC, it is manifeſt, that the angle dCI will be conſiderably leſs than the angle DCI: let them be aſſumed in the proportion that DC bears to its exceſs above dC; in which caſe the angle dCI will be 27° 35′ and DCI 55° 10′. The co-tangent of the former will be 1.9141795, of the latter .6958813; and the difference of them 1.2182982. The log. co-ſecants of thoſe two

two angles are 10.3343832 and 10.0857536, which being respectively increased by 9.9208188 and 9.7446275, the two constant logarithms, make 0.2552020 and 9.8304811, which are the logarithms of 1.7997079 and .6768323; and the difference of these two numbers is 1.1228756, which is less than the difference of the log. co-tangents by .0954226.

I next assume the angles 30° and 52° 45′; and by pursuing the same steps which have been described above, I find the difference of their co-tangents exceeds the difference of the products by .0028987. Then, as 925239 (the difference of the errors) is to 145′ (the difference of suppositions), so is the latter error 28987 to 4′ 33″, which being added to 30°, gives 30° 4′ 33″ for the next assumption of the angle *d*CI; but for ease in the computation I shall take 30° 5′; in which case the angle DCI will be 52° 40′; and by repeating the operation the difference of the co-tangents will be found less than the difference of the products by .0002425. And 31412 (the sum of the two last errors) is to 5′ (the difference of the suppositions) as 2425 (the last error) is to 23″; which being taken from 30° 5′, the last supposition, because it was too great, leaves 30° 4′ 37″ for the exact value of the angle *d*CI.

This equation, like that in the fourth example, when the value of *y* is properly substituted, and the equation reduced in the usual manner, will rise to four dimensions with all the inferior ones; and it does not appear, that either HUYGENS, SLUSIUS, Mr. ROBINS, Dr. WILLSON, or Professor SIMSON, with all their artifice, have been able to depress it: but by this method of resolution the point of reflection is found, with the greatest exactness, in much less time than this substitution and reduction can be made. And this example farther suggests to us, that when the answer is sought by the method now under

2 consi-

consideration, it is not always necessary to exterminate all the unknown quantities but one.

EXAMPLE VII.

Suppose the equation to be resolved were $a = .375 = 16y^5 \mp 4y^4 - 20y^3 \pm 4y^2 + 5y$: and, first, let the upper signs have place, and it is manifest, that the latter side of the equation may be divided into two parts; namely, $4y^2 - 4y^4 = 4y^2 . \overline{1+y} . \overline{1-y}$, and $16y^5 - 20y^3 + 5y = y^5 - 10y^3 . \overline{1+y} . \overline{1-y} + 5y . \overline{1+y}^2 . \overline{1-y}^2$. But the former part is (by tab. *I.*) the square of the sine of twice the arc which has y for its sine (radius being $= 1$) and the latter part the sine of five times the same arc. Hence, therefore, the given quantity a ($= .375$) is equal to the sum of the sine of five times the arc (A) which has y for its sine, and the square of the sine of twice the same arc. Now, as the square of the sine of twice the arc (A) must necessarily in this instance be very small in comparison of the sine of five times the same arc (A), it is manifest, that the sine of five times the arc which has y for its sine will be very little less than .375, and of course that arc (5A) can be but very little less than 22° 2′, the sine of which is next greater than that number. Assume it 21°, and the fifth part of it, or that arc which has y for its sine, will be 4° 12′, the double of which is 8° 24′. Now the log. sine of 8° 24′ is 9.1645998, which being doubled is 8.3291996, the logarithm of .0213403, and this number being taken from .375 leaves .3536597, which ought to have been .3583679, the sine of 21°, and of course is too small by .0047082: the arc has, therefore, been assumed too great.

Let

Let 20° 45′ be next assumed; the fifth part of which is 4° 9′, and twice this last number is 8° 18′, of which the log. sine is 9.1594354; and this being doubled is 8.3188708, the log. of .0208387; and this being taken from .375 will leave .3541613: less still than the sine of 20° 45′ by .0001297.

Take now 20° 44′, the fifth of which is 4° 8′ 48″, and two-fifths is 8° 17′ 36″; and the log. sine of this is 9.1590889, which being doubled gives 8.3181778, the logarithm of .0208055; and this being taken from .375, leaves .3541945; more than the sine 20° 44′ by .0000795. Now 2092 (the sum of the last two last errors) is to 60″ as 795 (the last error) is to 23″. Which being added to 20° 44′, the last assumption, gives 20° 44′ 23″ for five times the arc of which y is the sine: y is therefore the sine of 4° 8′ 52″.6, or .07233202.

When the lower signs in the equation have place, the given quantity a will be equal to the excess of the sine of five times an arc above the square of the sine of twice that arc: and the operation, after assuming, from the circumstances of the question, or equation, an arc which is nearly five times that having y for its sine, is this. Find the logarithmic sine of two-fifths of that arc, double it, find the number corresponding to this logarithm, and add to it the value of a, which should then be equal to the sine of the arc first assumed; and if it is not, to repeat the operation until an error is obtained on each side, and not very distant from the truth, as is done above, and which may always be done with three assumptions.

A multitude of examples might be added from the writings of different authors, who have either left their conclusions

unexhibited in numbers, for want of ſome ſuch eaſy method as this, or have done it by means of a long and laborious ſeries of difficult computations; which, beſide the labour attending them, are always ſubject to a variety of errors, which cannot be detected, in many caſes, without repeating the operation.

XXXI. *Experiments on the Power that Animals, when placed in certain Circumſtances, poſſeſs of producing Cold.* By Adair Crawford, *M. D.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 14, 1781.

IN the following paper I ſhall lay before the Society the reſult of ſome experiments, which I made in the courſe of the laſt ſummer, on the power that animals, when placed in certain circumſtances, poſſeſs, of producing cold, having premiſed a few remarks on the progreſſive improvements which have been made in the knowledge of heat in general.

The opinions of the ancients, reſpecting the nature and properties of fire, conſiſted of bold conjectures, which ſeem rather to have been the offſpring of a lively and vigorous imagination, than of a juſt and correct judgement: their ideas on this ſubject being evidently derived, not ſo much from an accurate obſervation of facts, as from thoſe ſentiments of admiration and awe which many of the phenomena of fire are calculated to excite. Thus, this element was ſuppoſed, on the original formation of the univerſe, to have aſcended to the higheſt place, and to have occupied the region of the heavens: it was conceived to be the principle which firſt communicated life and activity to the animal kingdom: it was conſidered as conſtituting the eſſence of inferior intellectual beings; and, by many of the ancient nations, it was reverenced as the ſupreme Deity. Indeed the profound veneration with which the element of

fire was contemplated, for a long ſucceſſion of ages, by a great part of mankind, appears to be one of the moſt curious circumſtances in the hiſtory of antient opinions. To account for this we may obſerve, that there is no principle in nature, obvious to the ſenſes, which produces ſuch important effects in the material ſyſtem, and which, at the ſame time, in the mode of its operation, is ſo obſcure and incomprehenſible.

It appears to be accumulated in an immenſe quantity in the ſun and fixed ſtars, from whence its beneficial influence ſeems to be continually diffuſed over the univerſe: it is the great inſtrument by means of which the changes of the ſeaſons are effected; the diverſity of climates is chiefly owing to the various proportions in which it is diſtributed throughout the earth. If we add to this the mighty alterations which have been produced in human affairs by the introduction of artificial fire, by its employment in the ſeparation of metals from their ores, and in the various arts which are ſubſervient to the comfort, the ornament, and the preſervation, of the ſpecies, it will not appear ſurprizing, that in a rude and ignorant age, this wonderful principle ſhould have been conſidered as endued with life and intelligence, and that it ſhould have become the object of religious veneration.

In the dark ages the alchymiſts regarded pure fire as the reſidence of the Deity: they conceived it to be uncreated and immenſe, and attributed to its influence moſt of the phenomena of nature. Indeed, it is not wonderful that they ſhould have aſſigned it a high rank in the ſcale of being, as it was the great agent which they employed in the chymical analyſis of bodies, and was the inſtrument of thoſe diſcoveries that attracted ſuch univerſal admiration, and that enabled them ſo ſucceſſfully to impoſe upon the ignorance and credulity of the times.

Upon

Upon the revival of literature, the importance of this branch of science began very soon to engage the attention of philosophers. It could not escape the general observation, in a penetrating and inquisitive age, when the powers of the human mind were employed with so much ardour and success in exploring the operations of nature, that the element of fire acts a principal part in the system of the world; that by the influence of this element those motions are begun and supported in the animal and vegetable kingdoms, which are essential to the production and preservation of life; and that it is the great agent in those successive combinations and decompositions, by which all things on the surface of the earth, and probably throughout the universe, are kept in a continual fluctuation.

But though the utility of this branch of science was perceived, yet the progress that was made in the cultivation of it did not keep pace with the opinion which men entertained of its importance. Our senses inform us, that heat has a real existence, but they give us no direct information with regard to its nature and properties: it is endowed with such infinite subtilty, that it has been called, by a very eminent philosopher, an occult quality: by some it has even been considered as an immaterial being. It is, therefore, with great difficulty that it can be made the subject of philosophical investigation; and hence the opinions of men concerning it have been fluctuating and various, and the words which express it vague and ambiguous.

The first step that was taken with a view to the cultivation of this branch of science was the construction of a machine for measuring the variations of sensible heat; observing, that heat has the power of expanding bodies, and considering

the degree of expansion as proportional to the increase of heat, philosophers have endeavoured by means of the former to render the latter obvious to the senses.

To this important invention, the author of which cannot be distinctly traced, we are indebted for all the succeeding improvements in the philosophy of heat. By means of it men were enabled to establish a variety of interesting facts, and to bring some of the most obscure and intricate phenomena of nature to the test of experiment. The opinion, that the heats inherent in various heterogeneous substances differed from each other in kind, as well as in degree, was now exploded, since all were found to produce similar effects upon the thermometer. The increase and diminution of temperature in the different seasons and climates, the laws which nature observes in the heating and cooling of bodies, the melting, the vaporific, and shining points, and the degrees of heat in the animal, the mineral, and the vegetable kingdoms, were accurately determined. In consequence of the attention that was paid to this subject, many curious questions arose, which have long exercised the ingenuity of philosophers. That property of heat by which it is capable of expanding the densest and hardest bodies; its power in producing fluidity; its tendency to an equilibrium; and the causes of its various distribution throughout the different substances in nature, have become the objects of philosophical enquiry. It was observed, that some bodies on exposure to heat, become red and luminous, but are incapable of producing flame, or of maintaining fire: that, on the contrary, others, by the application of fire, and the contact of fresh air, kindle into flame, and continue to emit light and heat, apparently from a source within themselves, till they are consumed. Hence arose the questions concerning the pabulum

ſum of fire, the uſe of the air in inflammation, and the diſtinction of bodies into combuſtible and incombuſtible.

From the firſt dawnings of philoſophy it muſt have been perceived, that moſt animals have a higher temperature than the medium in which they live; and that a conſtant ſucceſſion of freſh air is neceſſary to the ſupport of animal life. The cauſes of theſe phenomena have afforded matter for much ſpeculation in ancient as well as modern times: but the diſcovery that animals have, in certain circumſtances, the power of keeping themſelves at a lower temperature than the ſurrounding medium, was reſerved for the induſtry of the preſent age.

This diſcovery ſeems originally to have ariſen from obſervations on the heat of the human body in warm climates. It was mentioned by Governor ELLIS in 1758; it was taught by Dr. CULLEN before the year 1765; and at length it was completely eſtabliſhed by the experiments of Dr. FORDYCE in heated rooms, which were laid before the Society in 1774.

In the courſe of theſe experiments the doctor remained in a moiſt air heated to 130° for the ſpace of fifteen minutes, during which time the thermometer under his tongue ſtood at 100°, his pulſe made 139 beats in a minute, his reſpiration was but little affected, and ſtreams of water ran down over his whole body, proceeding from the condenſation of vapour, as evidently appeared from a ſimilar condenſation on the ſide of a Florentine flaſk that had been filled with water at 100°.

He found, however, that he could bear a much greater degree of heat when the air was dry. In this ſituation, he frequently ſupported, naked, for a conſiderable time, without much inconvenience, the heat of 260°, his body preſerving very nearly its proper temperature, being never raiſed more than 2° above the natural ſtandard.

Various

Various opinions have been entertained with regard to the causes of the facts which were established by these experiments. Some have attributed the cold solely to evaporation, and have conceived that the same degree of refrigeration would have been produced by an equal mass of dead matter, containing an equal quantity of moisture. Others have affirmed, that the cold did not arise solely from this cause; but have maintained, that it depended partly upon the energy of the vital principle, being greater than what would have been produced by an equal mass of inanimate matter.

The ingenious Dr. MUNRO, of Edinburgh, ascribes the cold in the above mentioned experiments to the circulation of the blood, in consequence of which the warmer fluids are continually propelled from the surface towards the center, where they are mixed with blood at a lower temperature, and hence the animal is slowly heated, in the same manner as the water in a deep lake, during the winter, is slowly cooled, and not without a long continuance of frost congealed, no part of it becoming solid till the whole is brought down to the freezing point.

The following experiments were made with a view to determine with greater certainty the causes of the refrigeration in the above instances.

To discover whether the cold produced by a living animal, placed in air hotter than its body, be not greater than what would be produced by an equal mass of inanimate matter, I took a living and a dead frog, equally moist, and of nearly the same bulk, the former of which was at 67°, the latter at 68°, and laid them upon flannel in air which had been raised to 106°. In the course of twenty-five minutes the order of heating was as follows *.

* In the two following experiments the thermometers were placed in contact with the skin of the animals under the axillæ.

In

	Air.	Dead frog.	Living frog.
In 1′	—	70½°	67½°
2	102	72	68
3	100	72½	69½
4	100	73	70
25	95	81¼	78¼

The thermometer being introduced into the stomach, the internal heat of the animals was found to be the same with that at the surface.

From hence it appears, that the living frog acquired heat more slowly than the dead one. Its vital powers must, therefore, have been active in the generation of cold.

To determine whether the cold produced in this instance depended solely upon the evaporation from the surface, increased by the energy of the vital principle, a living and dead frog were taken at 75°, and were immersed in water at 93°, the living frog being placed in such a situation as not to interrupt respiration.

	Dead frog.	Living frog.
In 1′	85°	81°
2	88½	85
3	90½	87
5	91½	89
6	91½	89
8	91½	89

These experiments prove, that living frogs have the faculty of resisting heat, or producing cold, when immersed in warm water: and the experiments of Dr. FORDYCE prove, that the human body has the same power in a moist as well as in a dry

* In the above experiment the water, by the cold frogs and by the agitation which it suffered during their immersion, was reduced nearly to 91°½.

air:

air: it is therefore highly probable, that this power does not depend solely upon evaporation.

It may not be improper here to observe, that healthy frogs, in an atmosphere above 70°, keep themselves at a lower temperature than the external air, but are warmer internally than at the surface of their bodies: for when the air was 77°, a frog was found to be 68°, the thermometer being placed in contact with the skin; but when the thermometer was introduced into the stomach, it rose to 70°½.

It may likewise be proper to mention, that an animal of the same species placed in water at 61°, was found to be nearly 61°¼ at the surface, and internally it was 66°½. These observations are meant to extend only to frogs living in air or water at the common temperature of the atmosphere in summer. They do not hold with respect to those animals, when plunged suddenly into a warm medium, as in the preceding experiments.

To determine whether other animals also have the power of producing cold, when surrounded with water above the standard of their natural heat, a dog at 102° was immersed in water at 114°, the thermometer being closely applied to the skin under the axilla, and so much of his head being uncovered as to allow him a free respiration.

In 5 minutes the dog was			108°,	water	112°
6	-	-	109	-	112
11	-	-	108	-	112 the respiration

having become very rapid.

In thirteen minutes the dog was 108°, water 112°, the respiration being still more rapid.

In about half an hour the dog was 109°, water 112°, the animal was then in a very languid state.

Small

Small quantities of blood being drawn from the femoral artery, and from a contiguous vein, the temperature did not seem to be much increased above the natural standard, and the sensible heat of the former appeared to be nearly the same with that of the latter.

In this experiment a remarkable change was produced in the appearance of the venous blood: for it is well known, that in the natural state, the colour of the venous blood is a dark red, that of the arterial being light and florid; but after the animal, in the experiment in question, had been immersed in warm water for half an hour, the venous blood assumed very nearly the hue of the arterial, and resembled it so much in appearance, that it was difficult to distinguish between them. It is proper to observe, that the animal which was the subject of this experiment, had been previously weakened by losing a considerable quantity of blood a few days before. When the experiment was repeated with dogs which had not suffered a similar evacuation, the change in the colour of the venous blood was more gradual; but in every instance in which the trial was made, and it was repeated six times, the alteration was so remarkable, that the blood which was taken in the warm bath could readily be distinguished from that which had been taken from the same vein before immersion, by those who were unacquainted with the motives or circumstances of the experiment.

To discover whether a similar change would be produced in the colour of the venous blood in hot air, a dog at 102° was placed in air at 134°.

In ten minutes the temperature of the dog was 104°½, that of the air being 130. In fifteen minutes the dog was 106°, the air 130°. A small quantity of blood was then taken from the

jugular vein, the colour of which was ſenſibly altered, being much lighter than in the natural ſtate.

The effect which is produced by external heat upon the colour of the venous blood, ſeems to confirm the following opinion, which was firſt ſuggeſted by my worthy and ingenious friend Mr. WILSON, of Glaſgow. Admitting that the ſenſible heat of animals depends upon the ſeparation of abſolute heat from the blood by means of its union with the phlogiſtic principle in the minute veſſels, may there not be a certain temperature at which that fluid is no longer capable of combining with phlogiſton, and at which it muſt of courſe ceaſe to give off heat? It was partly with a view to inveſtigate the truth of this opinion that I was led to make the experiments recited above.

I ſhall now endeavour, from the preceding facts, to explain what appear to me to be the true cauſes of the cold produced by animals when placed in a medium, the temperature of which is above the ſtandard of their natural heat.

In a work which I ſome time ago laid before the public, having attempted to prove, that animal heat depends upon the ſeparation of elementary fire from the air in the proceſs of reſpiration, I obſerved, that when an animal is placed in a warm medium, if the evaporation from the lungs be increaſed to a certain degree, the whole of the heat ſeparated from the air will be abſorbed by the aqueous vapour.

From the experiments on venous and arterial blood, recited in the third ſection of that work, it appears, that the capacity of the blood for containing heat is ſo much augmented in the lungs, that, if its temperature were not ſupported by the heat which is ſeparated from the air, in the proceſs of reſpiration, it would ſink 30°. Hence, if the evaporation from the lungs be ſo much increaſed as to carry off the whole of the heat that is detached

detached from the air, the arterial blood when it returns by the pulmonary vein will have its sensible heat greatly diminished, and will consequently absorb heat from the vessels which are in contact with it, and from the parts adjacent. The heat which is thus absorbed in the greater vessels will again be extricated in the capillaries, where the blood receives a fresh addition of phlogiston. If, in these circumstances, the blood during each revolution were to be equally impregnated with this latter principle, it is manifest, that the whole effect of the above process would be to cool the system at the center, and to heat it at the surface; or to convey the heat to that part of the body where it is capable of being instantly carried off by evaporation. But it appears, from the experiments which have been last recited, that, when an animal is placed in a heated medium, the sanguineous mass, during each revolution, is *less* impregnated with phlogiston; for we have seen, that the venous blood, in these circumstances, becomes gradually paler and paler in its colour till at length it acquires very nearly the appearance of the arterial: and it is rendered highly probable by the experiments of Dr. PRIESTLEY, that the dark and livid colour of the blood in the veins depends upon its combination with phlogiston in the minute vessels. Since, therefore, in a heated medium, this fluid does not assume the same livid hue, we may conclude, that it does not attract an equal quantity of the phlogistic principle *.

* It is of no consequence in the above argument, whether we suppose, with Dr. PRIESTLEY, that the alteration of colour in the blood depends upon its combination with phlogiston in the capillary arteries, or maintain with some other philosophers that this alteration arises from a change produced in the blood itself by the action of the vessels; it is sufficient for our purpose to assume it as a fact, which, I think, has been proved by direct experiment, that, in the natural state of the animal, the blood undergoes a change in the capillaries, by which its capacity for containing heat diminished; and that in a heated medium it does not undergo a similar change.

It follows, that the quantity of heat given off by the blood in the capillaries will not be equal to that which it had absorbed in the greater vessels, or positive cold will be produced. If the blood, for example, in its passage to the capillaries, absorb from the greater vessels a quantity of heat as 30°, and if in consequence of its receiving a less impregnation of phlogiston than formerly, it give off at the extreme vessels a quantity of heat only as 20°, it is manifest, that upon the whole a degree of refrigeration will be produced as 10°, and this cause of refrigeration will continue to act while the venous blood is gradually assuming the hue of the arterial, till the difference between them is obliterated; after which it will cease to operate. Thus it appears, that when animals are placed in a warm medium, the same process which formerly supplied them with heat becomes for a time the instrument of producing cold, and probably preserves them from such rapid alterations of temperature as might be fatal to life.

Upon the whole, the increased evaporation, the diminution of that power by which the blood in the natural state is impregnated with phlogiston, and the constant reflux of the heated fluids towards the internal parts, seem to be the great causes upon which the refrigeration depends. Having found that the attraction of the blood to phlogiston was diminished by heat, it appeared probable, on the other hand, that it would be increased by cold. To determine this, a dog at 100° was immersed in water nearly at 45°. In about a quarter of an hour a small quantity of blood was taken from the jugular vein, which was evidently much deeper in its colour than that which had been taken in the warm bath, and appeared to me, as well as to several other gentlemen, to be the darkest venous blood we had ever seen.

From this experiment, compared with thoſe which have been recited above, we may perceive the reaſon why animals preſerve an equal temperature, notwithſtanding the great variations in the heat of the atmoſphere, ariſing from the viciſſitudes of the weather, and the difference of ſeaſon and climate: for, as ſoon as by expoſure to external cold, an unuſual diſſipation of the vital heat is produced, the blood, in the courſe of the circulation, begins to be more deeply impregnated with the phlogiſtic principle. It will therefore furniſh a more copious ſupply of this principle to the air in the lungs, and will imbibe a greater quantity of fire in return.

In ſummer, on the contrary, the reverſe of this will take place, leſs phlogiſton will be attracted in the minute veſſels, and leſs fire will be abſorbed from the air.

And hence the power of generating heat is in all caſes proportioned to the demand. It is increaſed by the winter colds, diminiſhed by the ſummer heats: it is totally ſuſpended or converted into a contrary power, according as the exigencies of the animal may require.

From the changes which are produced in the colour of the venous blood by heat and cold, we may likewiſe perceive the reaſon why the temperature of the body is frequently increaſed by plunging ſuddenly into cold water, and why the warm bath has ſuch powerful effects in cooling the ſyſtem, and in removing a general or partial tendency to inflammation.

XXXII. *Account of a Comet. By Mr.* Herſchel, *F. R. S.; communicated by Dr.* Watſon, *Jun. of* Bath, *F. R. S.*

Read April 26, 1781.

ON Tueſday the 13th of March, between ten and eleven in the evening, while I was examining the ſmall ſtars in the neighbourhood of H Geminorum, I perceived one that appeared viſibly larger than the reſt: being ſtruck with its uncommon magnitude, I compared it to H Geminorum and the ſmall ſtar in the quartile between Auriga and Gemini, and finding it ſo much larger than either of them, ſuſpected it to be a comet.

I was then engaged in a ſeries of obſervations on the parallax of the fixed ſtars, which I hope ſoon to have the honour of laying before the Royal Society; and thoſe obſervations requiring very high powers, I had ready at hand the ſeveral magnifiers of 227, 460, 932, 1536, 2010, &c. all which I have ſucceſsfully uſed upon that occaſion. The power I had on when I firſt ſaw the comet was 227. From experience I knew that the diameters of the fixed ſtars are not proportionally magnified with higher powers, as the planets are; therefore I now put on the powers of 460 and 932, and found the diameter of the comet increaſed in proportion to the power, as it ought to be, on a ſuppoſition of its not being a fixed ſtar, while the diameters of the ſtars to which I compared it were not increaſed in

in the ſame ratio. Moreover, the comet being magnified much beyond what its light would admit of, appeared hazy and ill-defined with theſe great powers, while the ſtars preſerved that luſtre and diſtinctneſs which from many thouſand obſervations I knew they would retain. The ſequel has ſhewn that my ſurmiſes were well founded, this proving to be the Comet we have lately obſerved.

I have reduced all my obſervations upon this Comet to the following tables. The firſt contains the meaſures of the gradual increaſe of the Comet's diameter. The micrometers I uſed, when every circumſtance is favourable, will meaſure extremely ſmall angles, ſuch as do not exceed a few ſeconds, true to 6, 8, or 10 thirds at moſt; and in the worſt ſituations true to 20 or 30 thirds: I have therefore given the meaſures of the Comet's diameter in ſeconds and thirds. And the parts of my micrometer being thus reduced, I have alſo given all the reſt of the meaſures in the ſame manner; though in large diſtances, ſuch as one, two, or three minutes, ſo great an exactneſs, for ſeveral reaſons, is not pretended to.

TABLE

TABLE I. Measures of the Comet's diameter *.

Days.	″ ‴	Powers.
March 17	2 53	932. 460.
19	2 59	932. 460.
21	3 38	460.
28	4 7	932 } these measures agree to 9‴.
—	3 58	227 }
29	4 7	227 rather too small a measure.
—	4 25	227 seems right.
April 2	4 25	227
6	4 53	227
15	5 11	227
—	5 20	227 very good; not liable to half a second of error.
18	5 2	227 true to 12‴ or 18‴ at most.

Having measured the diameter of the Comet with such high power as 932 and 460, it may not be amiss to make one observation on this subject, lest it should be misapprehended that I pretend to a distinct power of such magnitude upon all celestial objects in general. By experience I have found, that the aberration or indistinctness occasioned by magnifying much, provided the object be still left sufficiently distinct, is rather to be put up with, than the power to be reduced, when the angles to be measured are extremely small. The reason of this may, perhaps, be that a small error of judgement, to which we are always liable, is of great consequence with a low power, as bearing a considerable proportion to the diameter of the object;

* There are several optical deceptions which may affect the measures of objects that subtend extremely small angles. Thus I have found, by experience, that a very small object will appear something less in a telescope when we see it first than when we become familiar with it. There is also a deflection of light upon the wires when they are nearly shut; but as none of these deceptions are well enough understood to apply a correction, I leave them affected with them.

whereas

whereas with a higher power the proportion of this error to the whole becomes much less, and the measure more exact, even after we have made allowance for a small additional error occasioned by the want of that perfect distinctness which is required for other purposes. However, to enter deeply into an explanation of this would lead me to speak of the causes of the aberration of rays in the focus of an object speculum, of which there are some that are seldom taken into consideration by opticians, and indeed are such as cannot be calculated; but this not being my present purpose, suffice it to observe, that the method is justified by experience.

When the diameter of the Comet was increased to about 4″, I thought it advisable to lessen the power with which I measured; and, as I made use of two different micrometers, as well as eye-glasses, I took a measure with both of them. The agreement of the micrometers to 9‴ is no small proof of the goodness of the observations of the 28th of March, and very properly connects the measures of the high powers with those that were made with 227.

TABLE II. Distance of the Comet from certain telescopic fixed stars which I have marked α, β, γ, δ, ϵ, ζ.

D.	H.	M.		′	″	‴	
Mar. 13	10	30	from α,	2	48	0	by pretty exact estimation true to 20″.
17	11	0	fig. 1*.	0	41	58	by the micrometer and power 227.
18	7	20		1	0	35	
—	9	16		1	6	59	
—	10	55		1	10	40	
19	7	4		1	46	40	
—	10	42		1	51	23	
21	10	0		3	39	46	
24	8	12	from β,	2	55	39	true to 4 or 5″, an indifferent observation.
—	10	58	fig. 2.	2	53	4	true to 4 or 5″.
25	7	24		2	12	46	true to 2 or 3″.
—	9	47		2	14	18	
26	10	43		1	48	3	true to 2 or 3″.
28	7	46		2	55	49	true to 4 or 5″.
29	8	50	from γ.	2	20	51	true to 2″.
30	7	55	fig. 3.	1	28	48	true to 2 or 3″.
Apr. 1	7	45		2	39	20	
6	8	50	from δ. fig. 4.	2	51	23	
15	10	18	from ϵ.	4	27	57	estimated by the field, true to 5 or 6″.
16	7	50	fig. 5.	3	9	14	by the micrometer, true to 3 or 4″.
—	10	47		2	50	56	true to 3 or 4″.
18	8	18		3	18	4	mean 3′ 17″, true to 1″ or 1½.
—	—	—		3	15	57	
—	8	50	from ζ,	2	24	57	
19	8	38	fig. 6.	3	2	5	true to 3 or 4″.

* The figures are drawn upon a scale of 80 seconds to one inch.

TABLE

TABLE III. Angle of position of the Comet with regard to the parallel of declination of the same telescopic fixed stars measured by a micrometer, of which I have given the description, and a magnifying power of 278. See fig. 1. 2. 3. 4. 5. 6.

D.	H.	M.		°	′	
Mar. 13	10	30	B α Comet,	0	0	by superficial estimation, liable to an error of 10 or 12 degrees.
17	11	0	A α Comet,	89	56	by the micrometer.
18	8	20	fig. 1 *.	56	39	
—	9	24		41	33	true to 1°.
19	7	23		29	47	true to 1°.
21	10	10		11	46	true to 4 or 5°.
—	11	48		12	14	
24	8	23	B β Comet,	38	39	true to 2 or 3°.
—	11	4	fig. 2.	36	14	true to 3 or 4°, air very tremulous.
25	7	33		53	18	
—	9	55		56	32	liable to a considerable error.
26	10	55	A β Comet.	87	0	true to 2 or 3°.
28	7	58		28	51	true to 3 or 4°.
29	9	25	B γ Comet,	32	19	true to 1 or 2°.
30	8	25	fig. 3.	72	14	true to 3 or 4°.
Apr. 1	7	55	A γ Comet,	28	51	well taken, } 27° 46′, true to 1°.
—	—	—		27	14	more exact,
6	8	28	B δ Comet, fig. 4.	84	42	true to less than 2°
15	10	27	B ε Comet,	29	9	true to 2 or 3°.
16	8	1	fig. 5.	49	11	true to 1°.
—	10	55		50	47	true to 1½ or 2°½.
18	8	31	A ε Comet,	47	9	very well taken, } 47°, true to less than 1°.
—	—	—		46	35	pretty well,
—	9	8	B ζ Comet,	82	39	
19	8	56	A ζ Comet,	48	18	} 49° 3′, true to 1°.
—	—	—	fig. 6,	49	48	
—	10	45		47	30	true to 2 or 3°.

* The angles are drawn true to the measure, without allowing for errors.

Miscellaneous observations and remarks.

March 19. The Comet's apparent motion is at present 2¼ seconds *per* hour. It moves according to the order of the signs, and its orbit declines but very little from the ecliptic.

March 25. The apparent motion of the Comet is accelerating, and its apparent diameter seems to be increasing.

March 28. The diameter is certainly increased, from which we may conclude that the Comet approaches to us.

April 2. This evening at 8 h. 15′ the Comet was a little above the line drawn from η to θ in fig. 7. This figure is only delineated by the eye, so that no very great exactness in the distances of the stars is to be expected; but I shall take the first opportunity of measuring their respective situations by the micrometer.

April 6. With a magnifying power of 278 times the Comet appeared perfectly sharp upon the edges, and extremely well defined, without the least appearance of any beard or tail.

April 16. Fig. 8. represents the situation of the Comet this evening about nine o'clock, and is only an eye-draught of the telescopic stars.

Remarks on the path of the Comet.

We may observe, that the method of tracing out the path of a celestial body by taking its distance from certain stars, and the angle of position, with regard to them, cannot be expected to give us a compleatly just representation of the tract it describes, since even the most careful observations are liable to little errors, both from the remaining imperfections of instruments, though they

they ſhould be the moſt accurate that can be had, and from the difficulty of taking angles and poſitions of objects in motion. Add to this a third cauſe of error, namely, the obſcurity of very ſmall teleſcopic ſtars that will not permit the field of view ſo well to be enlightened as we could wiſh, in order to ſee the threads of the micrometer perfectly diſtinct.

This will account for the apparent diſtortions to be obſerved in my figures of the Comet's path. Some little irregularity therein may alſo proceed from different refractions, as they have not been taken into account, though the obſervations have been made at very different altitudes, where conſequently the refractions muſt have been very different. But though this method may be liable to great inconveniences, the principal of which is, that many parts of the heavens are not ſufficiently ſtored with ſmall ſtars to give us an opportunity to meaſure from them, yet the advantages are not leſs remarkable. Thus we ſee that it enabled me to diſtinguiſh the quantity and direction of the motion of this Comet in a ſingle day (from the 18th to the 19th of March) to a much greater degree of exactneſs than could have been done in ſo ſhort a time by a ſector or tranſit inſtrument; nay even an hour or two, we ſee, were intervals long enough to ſhew that it was a moving body, and conſequently, had its ſize not pointed it out as a Comet, the change of place, though ſo trifling as 2¼ ſeconds *per* hour, would have been ſufficient to occaſion the diſcovery. A gentleman very well known for his remarkable ſucceſs in detecting Comets * ſeems to be well aware of the difficulty to diſcover a motion in a heavenly body by the common methods when it is ſo very ſmall; for in a letter he favoured me with, ſpeaking of the Comet, he ſays: "Rien n'etoit plus difficile que de la "reconnoître et je ne puis pas concevoir comment vous avés pu

* Monſ. MESSIER.

"revenir

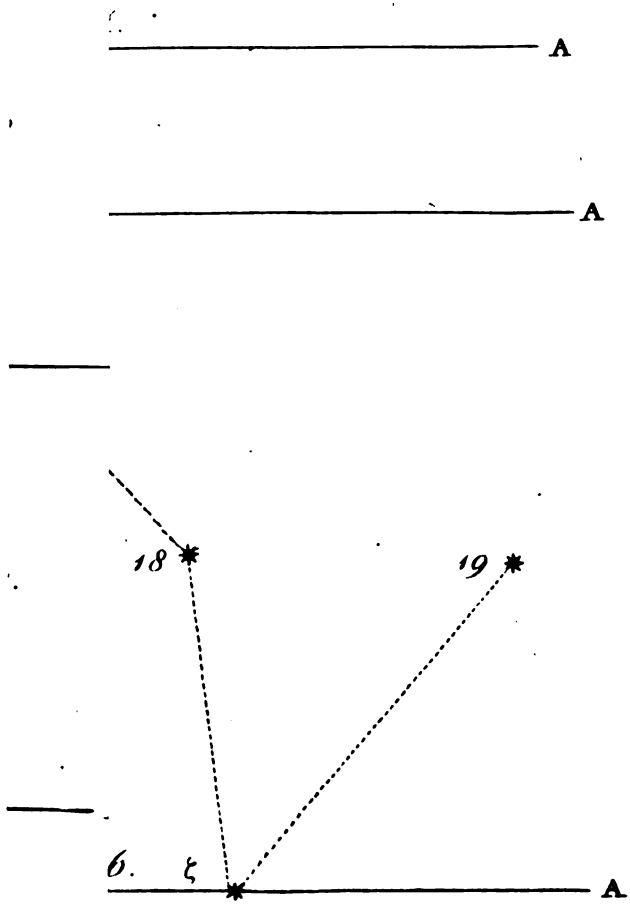
" revenir plusieurs fois sur cette étoile ou Comête; car absolu-
" ment il a fallu l'observer plusieurs jours de suite pour s'ap-
" perçevoir qu'elle avoit un mouvement."

I need not say that I merely point this out as a temporary advantage in the method I have taken; for as soon as we can have regular, constant, and long continued observations by fixed instruments, the excellence of them is too well known to say any thing upon that subject: for which reason I failed not to give immediate notice of this moving star, and was happy to surrender it to the care of the Astronomer Royal and others, as soon as I found they had begun their observations upon it.

Description of a micrometer for taking the angle of position.

FIG. I. Represents the micrometer inclosed in a turned case of wood, as it is put together, ready to be used with the telescope. A is a little box which holds the eye-glass. B is the piece which covers the inside work, and the box A is screwed into it. C is the body of the micrometer containing the brass work, shewing the index plate *a* projecting at one side, where the case is cut away to receive it. D is a piece, having a screw *b* at the bottom, by means of which the micrometer is fastened to the telescope. To the piece C is given a circular motion, in the manner the horizontal motion is generally given to Gregorian reflectors, by the lower part going through the piece D, where it is held by the screw E, which keeps the two pieces C and D together, but leaves them at liberty to turn upon each other.

Fig. II. Is a section of the case containing the brass work, where may be observed the piece B hollowed out to receive the



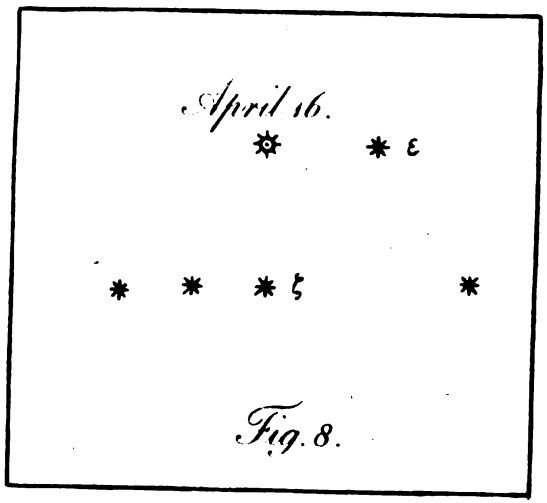


Fig. 8.

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Fig. II.

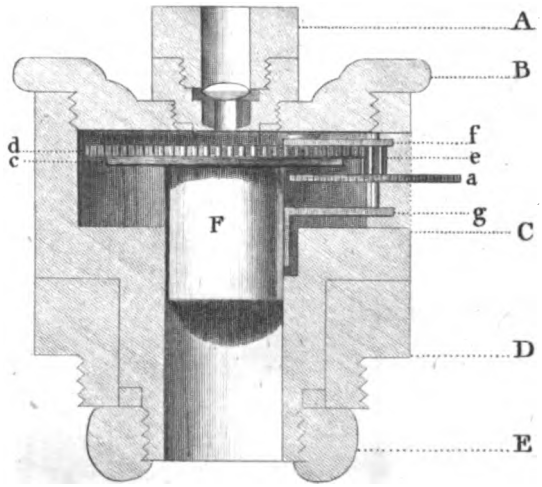
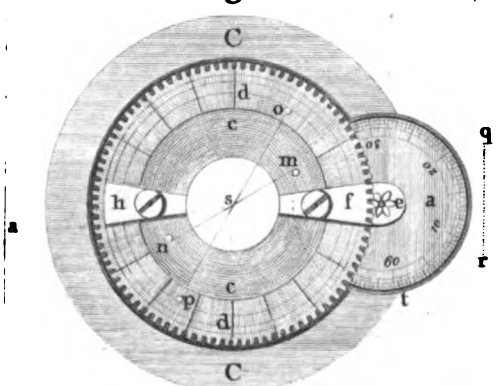


Fig. IV.



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box A, which consists of two parts inclosing the eye lens. This figure also shews how the piece C passes through D, and is held by the ring E: the brass work, consisting of a hollow cylinder, a wheel and pinion, and index plate, is there represented in its place. F is the body of the brass work, being a hollow cylinder with a broad rim *c* at the upper end; this rim is partly turned away to make a bed for the wheel *d*. The pinion *e* turns the wheel *d*, and carries the index plate *a*. One of its pivots moves in the arm *f*, screwed upon the upper part of *c*, which arm serves also to confine the wheel *d* to its place upon *c*. The other pivot is held by the arm *g* fastened to F.

Fig. III. Is a plan of the brass work. The wheel *d*, which is in the form of a ring, is laid upon the upper part of F or *c*, and held by two small arms *f* tnd *h*, screwed down to *e* with the screws *i*, *i*.

Fig. IV. Is a plan of the brass work. *d*, *d*, is the wheel placed upon the bed or socket of the rim of the cylinder *c*, *c*, and is held down by the two pieces *f*, *h*, which are screwed upon *c*, *e*. The piece *f* projects over the center of the index plate to receive the upper pivot of the pinion *m*, *n*, is the fixed wire fastened to *c*, *c*. *o*, *p*, the moveable wire fastened to the annular wheel *d*, *d*. The index plate *a* is divided into 60 parts, each sub-divided into two, and milled upon the edge. When the finger is drawn over the milled edge of the index plate from *q* towards *r*, the angle *m*, *s*, *o*, will open, and if drawn from *r* towards *q*, it will shut again. The case C, C, must have a sharp corner *t*, which serves as a hand to poin out the division on the index plate.

XXXIII. *A Letter from* Joseph Willard *to the Rev. Dr.* Maskelyne, *Astronomer Royal, concerning the Longitude of* Cambridge *in* New England.

Read July 5, 1781.

REV. SIR,

THE difference of meridians between Greenwich and Cambridge has been generally reckoned 4 h. 44′. This was what the late Dr. WINTHROP made use of; but I do not find that he determined it by actual observations, made by him at Cambridge, compared with corresponding ones, made at the Royal Observatory at Greenwich. It appears, that in 1769, at the time of the transit of Venus, the doctor was not entirely certain of the longitude of Cambridge. He mentioned 4 h. 44′ as near the truth; but for better fixing it, he gave several of his observations of the eclipses of Jupiter's satellites to be compared with those made at Greenwich; but there were too few corresponding ones to determine the point with precision; and as modern astronomers do not make absolute dependence upon the difference of meridians deduced from the eclipses of Jupiter's satellites, unless there has been a series of observations, both of immersions and emersions, I have wished to find some observations of solar eclipses and occultations of fixed stars by the moon, made at Cambridge, of which corresponding ones were made at Greenwich. I have met with no observations of occultations made by Dr. WINTHROP; but a solar

eclipse was observed by him and several other gentlemen, at his house, August 5, 1766, at which I was present and assisting, being then a resident graduate at Harvard College: this eclipse, Sir, I find that you observed at Greenwich. By your observations in the printed volume, a copy of which the Royal Society was so generous as to send to Harvard College, which was received the last spring, and for which the College is very grateful, I find the beginning of the eclipse was seen by you at 5 h. 29′ 56″ P.M. and the end at 7 h. 11′ 27″ P.M. apparent time. At Dr. WINTHROP's house at Cambridge, lat. 42° 25′ N. the beginning of this eclipse was observed at 11 h. 39′ 23″ A.M. and the end at 2 h. 45′ 9″ P.M. apparent time. Allowing for the spheroidal figure of the earth, and going through the parallactic calculations and deductions, I find the difference of meridians between Greenwich and Cambridge, by the observations of this eclipse, to be 4 h. 44′ 22″.

In the transit of Venus, in 1769, the internal contact was observed by Dr. WINTHROP at 2 h, 47′ 30″ apparent time, and by Mr. HITCHINS, at the Royal Observatory, at 7 h. 28′ 57″ apparent time. Allowing the sun's parallax on the day of the transit to be 8″,38, I find by calculation from these observations, that the difference of meridians between Greenwich and Cambridge is 4 h. 44′ 12″. The reason of my taking Mr. HITCHINS's observation is, your saying, that the telescope which he used was much superior to all the others which were made use of at that day at the Observatory; and to its greater excellence and distinctness you attribute the difference of 26″ by which Mr. HITCHINS saw the internal contact before you. There seems to be the greater propriety, when comparing the American observations of that phenomenon with those made at Greenwich, to take that where the observer was peculiarly

aided by the diſtinctneſs of his teleſcope, becauſe the ſun was very near the horizon with you, while with us the altitude was great, and the atmoſphere exceeding clear. Taking the mean between the deduction made from the obſervations of the internal contact of Venus, and of the beginning and ending of the above ſolar eclipſe, the difference of meridians between Greenwich and Cambridge is 4 h. 44′ 17″.

I find, Sir, in a letter from you to Dr. SMITH of Philadelphia, Dec. 26, 1769, that by the obſervations of the eclipſes of Jupiter's ſatellites made at Norriton you determined the difference of meridians between Greenwich and Norriton to be 5 h. 1′ 0′ 35″. If we ſubtract 52″, the difference of meridians between Philadelphia and Norriton, gotten, agreeably to your requeſt, by terreſtrial meaſurement, we find the difference of meridians between Greenwich and Philadelphia to be 5 h. 0′ 43″, which is the ſame that it appears to be by the immerſions and emerſions of Jupiter's firſt ſatellite obſerved at Philadelphia, corrected in the ſame manner you corrected the obſervations for Norriton, which is 8″ more than Dr. EWING's determination. By obſervations of the tranſit of Mercury in 1769, made at Cambridge and Philadelphia, the difference of meridians between thoſe two places appears by the external contact to be 16′ 02″, by the internal 16′ 28″; the mean 16′ 30″ ſubtracted from 5 h. 0′ 43″ leaves 4 h. 44′ 13″ for the difference of meridians between Greenwich and Cambridge deduced in this way, which, though not direct, may yet be conſidered as an evidence of ſome weight to prove, that the difference is more than 4 h. 44′, and that 4 h. 44′ 17″ may be very near the truth. This is the difference that I at preſent take, when I make uſe of tables fitted to the meridian of Greenwich; but I ſhould be ſtill glad of more

 corresponding

corresponding observations to ascertain this point. June 24, 1778, there was a solar eclipse, visible both at Greenwich and Cambridge. The beginning of this eclipse was observed at Cambridge by the late Dr. WINTHROP at 9 h. 6′ 20″ A.M. and the end at 11 h. 37′ 22″ A.M. apparent time. If the atmosphere favoured your observing it at Greenwich, I should be extremely obliged to you, if you would communicate to me your observations. I shall also be happy to know the time of the beginning of the solar eclipse of the 23d of next April at Greenwich.

I now, Sir, beg leave to communicate to you some observations of the solar eclipse of the 27th of last October. At Beverly, lat. 42° 36′ N. I carefully ascertained the going of my clock, by equal altitudes of the sun's upper and lower limb, for several days preceding the eclipse, and on the day when it happened, constantly applying the equation for the change of declination. October 25th, when the sun's center passed the meridian, it was by the clock 11 h. 59′ 11″; on the 26th, 11 h. 58′ 15″; and on the 27th, 11 h. 57′ 18″.

Two gentlemen observed with me; the rev. Mr. CUTLER of Ipswich, and the rev. Mr. PRINCE of Salem. Mr. CUTLER and I were each furnished with a reflecting telescope made by JAMES MANN of London, one magnifying 34, and the other 45 times. Mr. PRINCE had an achromatic refractor magnifying 43 times. The times of observation are as follow:

		By the clock.			Reduced to app. time.			
		h.	′	″	h.	′	″	
Beginning of eclipse observed by	Mr. CUTLER,	10	59	2	11	1	42	A.M.
	Mr. PRINCE,	10	59	6	11	1	46	
	Myself,	10	59	8	11	1	48	
End of eclipse observed by	Mr. CUTLER,	1	38	37	1	41	23	P.M.
	Mr. PRINCE,	1	38	43	1	41	29	
	Myself,	1	38	40	1	41	26	

 I had

I had no micrometer to measure the lucid part of the sun in its least state, and thereby determine the error of MAYER's tables in latitude at the middle of this eclipse. But according to the observations of Professor WILLIAMS, Dr. WINTHROP's successor, the error was not great. He and several assistants observed this eclipse at Long Island in Penobscot Bay. The latitude of the place of observation he found to be 44° 17′ 7″ N. He observed the beginning of the eclipse at 11 h. 11′ 8″ A.M. and the end at 1 h. 50′ 25″ P.M. apparent time. He was furnished with an excellent DOLLOND's micrometer, with which he measured the sun's diameter on the morning of the eclipse, and the lucid parts many times during its continuance. By his observations, compared with those made at Beverly, I find the difference of meridians between Beverly and Long Island to 8′ 4″ in time. The time of the greatest obscuration was at 12 h. 30′ 22″, when the lucid part of the sun was 8″ on the lower limb. The sun's semi-diameter according to observation was 16′ 8″,7; the moon's horizontal semi-diameter, according to the tables, 16′ 23″,8; the augmentation of her semi-diameter, agreeably to her altitude, 9″,5; the sum of the visible semi-diameters of the sun and moon therefore 32′ 42″. The lucid part of the sun 8″ being subtracted from 32′ 17″,4, the sun's diameter, leaves 32′ 9″,4 for the eclipsed part of the sun, which subtracted from 32′ 42″ leaves 32″,6 for the least distance of the centers of the sun and moon. The visible angle of the moon with the sun was, I find, 15° 54′ 54″, and her motion from the sun in her visible orbit 24″,5 in one minute; the visible ecliptic conjunction therefore was at 12 h. 29′ 57″, and the distance of the centers of the sun and moon 34″. The moon's parallax in latitude from the sun was then 49′ 15″,8 S. which added to 34″ gives 49′ 49″,8 N. equal to the moon's lati-

tude by obſervation. By MAYER's tables her latitude was then 49′ 39″,4, by which it appears, that the error of theſe tables in latitude, at the middle of the eclipſe, by the Penobſcot obſervation, was – 10″4. The error in longitude, taking the mean between that of the beginning and end, I find to be – 9″. The determination of theſe errors is upon the ſuppoſition that Beverly is 4 h. 42′ 59″ W. of Greenwich, which, I believe, very near the truth. Beverly appears by obſervation to be 1′ 18″ in time E. of Cambridge, which ſubtracted from 4 h. 44′ 17″ leaves 4 h. 42′ 59″.

I hope, Sir, no umbrage will be taken at my writing to you, on account of the political light in which America is now viewed by Great Britain. I think political diſputes ſhould not prevent communications in matters of mere ſcience; nor can I ſee how any one can be injured by ſuch an intercourſe.

I am, &c.

Beverly in Maſſachuſetts,
February 16, 1781.

AN

ACCOUNT

OF SOME

THERMOMETRICAL EXPERIMENTS;

CONTAINING,

I. Experiments relating to the cold produced by the evaporation of various fluids, with a method of purifying ether.
II. Experiments relating to the expanſion of mercury.
III. Deſcription of a thermometrical barometer.

By TIBERIUS CAVALLO, F. R. S.

Who was nominated by the Preſident and Council to proſecute Diſcoveries in Natural Hiſtory, purſuant to the Will of the late HENRY BAKER, Eſq. F. R. S.

Read at the ROYAL SOCIETY, June 28, 1781,

AN ACCOUNT, &c.

IT is at present well known, that by the evaporation of various fluids a sensible degree of cold is produced; and that by the evaporation of ether, which is the most volatile fluid we are acquainted with, water may be congealed, and the thermometer may be brought several degrees below the freezing point. But as various thermometrical experiments, which I lately made, have exhibited some new phenomena, and as I have contrived an easy and pleasing method of freezing a small quantity of water in a short time, and in every climate; I think it not improper to give an account of these things in the first part of this lecture.

My first experiments were intended to discover, if possible, a fluid cheaper than ether, by the evaporation of which a degree of cold sufficient for some useful purpose might be generated. But in this my expectation was disappointed, as I found that ether was incomparably superior to any other fluid, as the cold it produced was several degrees greater than that occasioned by any other of the most volatile fluids whatever. Being, therefore, obliged to use ether, I endeavoured to contrive a method, by which the least possible quantity of it might be wasted in the production of a degree of cold sufficient to freeze water, and in this I met with success. But before we come to the description of this method, I

ſhall briefly relate ſome obſervations made on the cold produced by the evaporation of other fluids beſides ether.

In a room, the temperature of which was 64° according to FAHRENHEIT's thermometer, and in which the air was gently ventilated, I obſerved the effects produced by various fluids when thrown upon the ball of a thermometer. The ball of this thermometer was quite detached from the ivory piece upon which the ſcale was engraved. The various fluids were thrown upon the thermometer through the capillary aperture of a ſmall glaſs veſſel, ſhaped like a funnel, and care was taken to throw them ſo ſlowly upon the bulb of the thermometer, that a drop might now and then fall from the under part of it; except when thoſe fluids were uſed, which evaporate very ſlowly, in which caſe it was ſufficient to keep the ball of the thermometer only moiſt, without any drop falling from it. During the experiment the thermometer was kept turning very gently round its axis, in order that the fluid uſed might fall upon every part of its bulb. This method I find to anſwer much better than that of dipping the ball of the thermometer into the fluid and removing it immediately after, or that of wetting the thermometer with a feather. The evaporation, and conſequently the cold produced by it, may be increaſed by ventilation. *viz.* by blowing with a pair of bellows upon the thermometer; but this was not uſed in the following experiments, becauſe it is not eaſily performed by one perſon, and alſo becauſe it occaſions very uncertain reſults.

With the above deſcribed method I began to examine the effects of water, and found, that the thermometer was brought down to 56°, *viz.* 8° below the temperature of the room in which the experiment was made, and of the water employed.

This

This effect was produced in about two minutes time, after which a larger continuation did not bring the mercury lower.

By means of ſpirit of wine the thermometer was brought down to 48°, which is only 16° below the temperature of the room, and of the ſpirit employed. When the ſpirit of wine is highly rectified, the cold produced by its evaporation is certainly greater than when it is of the common ſort; but the difference is not ſo great as one, who never tried the experiment, might expect. The purer ſpirit produces the effect much quicker.

Uſing various other fluids, which were either compounds of water and ſpiritous ſubſtances, or pure eſſences, I found that the cold produced by their evaporation was generally in ſome intermediate degree between the cold produced by the water and that produced by the ſpirit of wine.

Spirit of turpentine brought the thermometer only 3° lower than the temperature of the room; but olive oil and other oils, which evaporate either very ſlowly or not at all, did not ſenſibly affect the thermometer.

Willing to obſerve how much electrization could increaſe the evaporation of ſpirit of wine, and conſequently the cold produced by it, I put the tube containing the ſpirit into an inſulating handle, and connected it with the conductor of an electrical machine, which was kept in action whilſt the experiment was performed; by theſe means the thermometer was brought down to 47°. Having tried the three mineral acids I found, that inſtead of cooling they heated the thermometer, which effect I expected; ſince it is well known, that thoſe acids attract the water from the atmoſphere, and that heat is produced by the combination of water and any of them. The vitriolic acid, which was very ſtrong and tranſparent, raiſed the

thermometer to 102°; the ſmoaking nitrous acid raiſed it to 72°; and the marine acid raiſed it to 66°; the temperature of the room, as well as of the acids, being 64°, as mentioned above.

The apparatus which I contrived for the purpoſe of uſing the leaſt poſſible quantity of ether in freezing water, &c. conſiſts in a glaſs tube, terminating in a capillary aperture, which tube is to be fixed upon the bottle that contains the ether. Fig. 1. of the annexed drawing exhibits ſuch a tube, round the lower part of which, *viz.* at A ſome thread is wound, in order to let it fit the neck of the bottle. When the experiment is to be made, the ſtopper of the bottle containing the ether is removed, and the above mentioned tube is fixed upon it. The thread round this tube ſhould be moiſtened a little with water or ſpittle before it is fixed on the bottle, in order to prevent more effectually any eſcape of ether between the neck of the bottle and the tube. Then holding the bottle by its bottom FG (fig. 2.) and keeping it inclined as is ſhewn in the figure, the ſmall ſtream of ether iſſuing out of the aperture D of the tube DE, is directed upon the ball of the thermometer, or upon a tube containing water or other liquor that is required to be congealed.

Ether being very volatile, and having the remarkable property of increaſing the bulk of air, does not require any aperture, through which the air might enter the bottle, in proportion as the ether goes out: the heat of the hand is more than ſufficient to force the ether in a ſtream from the aperture D.

After this manner, throwing the ſtream of ether upon the ball of a thermometer in ſuch quantity as that a drop of ether might now and then, for inſtance every 10 ſeconds, fall from the under part of the thermometer, I have brought the mercury down to 3°, *viz.* 29° below the freezing point, when the atmoſphere

atmosphere was somewhat hotter than temperate, and that without blowing upon the thermometer.

When the ether is very good, *viz.* is capable of dissolving elastic gum, and the thermometer has a small bulb, not above twenty drops of ether are required to produce this effect, and about two minutes of time; but when the ether is of the common sort, a greater quantity of it, and a longer time, is necessary to be employed, though at last the thermometer is brought down very nearly as low by this as by the best sort of ether.

In order to freeze water by the evaporation of ether, I take a thin glass tube about four inches long and about one-fifth of an inch in diameter, hermetically closed at one end, and put a little water in it, so as to fill about half an inch length of it, as is shewn at CB in the figure. Into this tube a slender wire H is also introduced, the lower extremity of which is twisted in a spiral manner, and serves to draw up the ice, when formed. Things being thus prepared, I hold the glass tube by its upper part A with the fingers of the left hand, and keep it continually and gently turning round its axis, first one way, and then the contrary; whilst with the right hand I hold the phial containing the ether in such a manner as to direct the stream of ether on the outside of the tube, and a little above the surface of the water in it. The capillary aperture D should be kept almost in contact with the surface of the tube that contains the water. Continuing this operation for two or three minutes, the water will be froze as it were in an instant; since it will appear to become opaque at the bottom B, and the opacity will ascend at C in less than half a second of time, which exhibits a beautiful appearance. This congelation, however, is only superficial, and in order to congeal the whole quantity of water,

ter, the operation muſt be continued one or two minutes longer; after which the wire H will be found to be kept very tight by the ice. Now the bottle with the ether is left upon a table or other place, and to the outſide of the glaſs tube the hand muſt be applied for a moment, in order to ſoften the ſurface of the ice, which adheres very firmly to the glaſs, and then pulling the wire H out of the tube, a ſolid and hard piece of ice will come out, faſtened to its ſpiral extremity.

Inſtead of the wire H ſometimes I put a ſmall thermometer into this tube ſo as to have its bulb immerſed in the water. With this thermometer I have obſerved a very remarkable phenomenon, which ſeems to be not explicable in the preſent ſtate of knowledge concerning heat and cold. This is, that water will freeze in the winter with a leſs degree of cold than it will in the ſummer, or when the weather is hotter: for inſtance, in the winter the water in the tube AB will freeze when the thermometer is about 30°; but in the ſummer, or even when the temperature of the atmoſphere is about 60°, the quickſilver in the thermomer muſt be brought ten or fifteen, or even more, degrees below the freezing point, before the water which ſurrounds the ſaid thermometer will be converted into ice, even ſuperficially; hence it appears, that in the ſummer time a greater quantity of ether and longer time is required to freez a given quantity of water than in the winter, not only becauſe then a greater degree of heat is to be overcome, but principally becauſe in the ſummer a much greater degree of cold muſt be actually produced before the water that is kept in it will aſſume a ſolid form. When the temperature of the atmoſphere has been about 40°, I have froze a quantity of water with an equal weight of good ether, but at preſent,

being ſummer, between two and three times the quantity of the ſame ether muſt be uſed to produce the ſameeffect.

There ſeems to be ſomething in the air which, beſides heat, interferes with the freezing of water, and perhaps of all fluids, though I cannot ſay from experience whether the above mentioned difference between the freezing of water in winter and ſummer, takes place with other fluids, as milk and other animal fluids, oils, wines, &c.

The proportion between the quantity of the ether and of the water that may be frozen by it, ſeems to vary according to the quantity of water; for a larger quantity of water ſeems to require a proportionably leſs quantity of ether than a ſmaller quantity of water, ſuppoſing that the water is contained in cylindrical glaſs veſſels; for I have not tried whether a metal veſſel inſtead of a glaſs one, and whether ſome other ſhape beſides the cylindrical, might not facilitate the congelation. In the beginning of the ſpring I froze about a quarter of an ounce of water with nearly half an ounce weight of ether, the apparatus being larger, though ſimilar to that deſcribed above.

Now as the price of ether, ſufficiently good for the purpoſe, is generally between eighteen pence and two ſhillings *per* ounce, it is plain, that with leſs than two ſhillings a quarter of an ounce of ice, or ice cream, may be made in every climate, and at any time, which may afford great ſatisfaction to thoſe perſons, who living in places where no natural ice is to be had, never ſaw or taſted any ſuch delicious refreſhments.

When a ſmall piece of ice, for inſtance, of about ten grains in weight, is wanted, the neceſſary apparatus is very ſmall, and the expence of the ether not worth mentioning. I have a ſmall box, which is four inches and a half long, two inches broad, and one inch and a half deep, which contains all the apparatus.

ratus necessary for this purpose, *viz.* a bottle capable of containing about one ounce of ether, two pointed tubes (in case that one should break) a tube in which the water is to be frozen, and the wire. With the quantity of ether contained in this small and very portable apparatus, the experiment, when carefully performed, may be repeated about ten times. A person who wishes to perform such experiments in hot climates, and in places where ice is not easily procured, requires only a large bottle of ether, besides the small apparatus described above.

It is a known fact, that the moment a quantity of water becomes ice, a thermometer kept immersed in it, *rises a few* degrees, and accordingly this is observed in our experiment, *viz.* the mercury of the thermometer, which is immersed in the water of the tube AB, will suddenly rise, sometimes as much as ten degrees, when the water becomes first opaque. Electrization increases very little the degree of cold produced by the evaporation of ether. Having thrown the electrified, and also the unelectrified, stream of ether upon the bulb of a thermometer, the mercury in it was brought down two degrees lower in the former than in the latter case.

As various persons may, perhaps, be induced by this paper to repeat such experiments, and as ether is a fluid which can with difficulty be preserved, it may be useful to mention, that a cork confines ether in a glass bottle much better than a glass stopple, which it is almost impossible to grind so well as intirely to prevent the evaporation of ether. When a stopple, made very nicely out of a uniform and close piece of cork, which goes rather tight, is put upon a bottle of ether, the smell of that fluid cannot be perceived through it; but I never saw a glass stopple that could produce the same effect. By open-

ing

ing the bottle very often, or by long keeping, the cork becomes looſe, in which caſe it muſt be changed; and thus ether, ſpirit of wine, or any fluid, excepting thoſe which corrode cork, may be preſerved.

I ſhall now deſcribe a method of purifying vitriolic ether, which is very eaſy and expeditious, though not very profitable: this method I learned of Mr. WINCH, Chemiſt, in the Haymarket. Fill about a quarter of a ſtrong bottle with common ether, and upon it pour about twice as much water, then ſtop the bottle, and give it a ſhake, ſo as to mix for a time the ether with the water. This done, keep the bottle without motion, and with the mouth downwards, till the ether is ſeparated from the water and ſwims over it, which requires not above three or four minutes of time; then open the bottle, and keeping it ſtill inverted, let the greateſt part of the water come out very gently; after this the bottle being turned with the mouth upwards, more water muſt be poured in it, and in ſhort the ſame operation muſt be repeated three or four times. Laſtly, all the water being ſeparated from the ether by decanting it with dexterity, the ether will be found to be exceedingly pure. By this means I have purified common vitriolic ether, which could not affect elaſtic gum, and have reduced it into ſuch a ſtate as that elaſtic gum was eaſily diſſolved by it. Indeed this purified ether appeared by every trial to be purer than I ever ſaw it, even when made after the beſt uſual method, and in the moſt careful manner. The only inconvenience attending this proceſs is, that a vaſt quantity of ether is loſt. Not above three or four ounces of a pound of common ether remain after the purification. As the greateſt part of the ether is certainly mixed with the water that is uſed in the proceſs, it may perhaps be worth while to put that water into a retort, and

to distil the ether from it, which must come sufficiently pure for common use.

It is commonly believed, that water combines with the purest part of ether, when those two fluids are kept together; whereas, by the above described process, the contrary is established: perhaps when ether is kept in contact with water for a long time, the purest part of it may appear to be lost, because the ether may be combined with, and may retain some water in itself, at the same time that the water combines with and retains some ether, whereas the case may be different when the ether is quickly washed in water, and is immediately after separated from it: but in respect to this I have yet not made any experiments, so as to be able to decide the matter.

II. *Experiments relating to the expansion of mercury.*

THE difficulty and uncertainty attending the various methods hitherto proposed for investigating the expansion of quicksilver, or its increase of bulk when rarified by a given degree of heat, determined me to contrive some method by which this purpose might be effected with more certainty and precision. After various experiments I hit upon the following method, which to me seems both new and capable of great accuracy, though in this I may be deceived.

First, having blown a ball to a capillary tube, such as are commonly used for thermometers, I weighed it, and found that this empty thermometer was equal to 79,25 grains. This empty glass previous to its being weighed was rendered as per-

fectly

fectly clean as poſſible, which is a neceſſary precaution in this experiment, which depends upon a very great accuracy of weight. Then I introduced ſome mercury into the ſtem of this thermometer, taking care that none of it entered the ball, and, by adapting a ſcale of inches to the tube, obſerved that 4,3 inches length of the tube was filled with the mercury. The thermometer was now weighed again, and from this weight, the weight of the glaſs found before being ſubtracted, the remainder, *viz.* 0,24 gr. ſhewed the weight of ſo much quickſilver as filled 4,3 inches of the tube. Now the ball of the thermometer, and alſo part of the tube, were intirely filled with quickſilver: then, in order to find out the weight of the mercury contained in it, the thermometer was weighed for the laſt time, and from this weight the weight of the glaſs being ſubtracted, the remainder, *viz.* 3205 grs. ſhewed the weight of the whole quantity of quickſilver contained in the thermometer.

By compariſon with a graduated thermometer in hot and cold water, I made a ſcale to the new thermometer according to FAHRENHEIT's, and by applying a ſcale of inches found, that the length of 20° in this ſcale was equal to 1,33 inch. But 0,24 gr. was the weight of ſo much mercury as filled 4,3 inches length of the tube; therefore, by the rule of proportion it will be found, that the weight of ſo much quickſilver as fills 1,33 inch of the tube, *viz.* the length of 20° is equal to 0,0742 gr. nearly, and that the weight of ſo much quickſilver as fills the length of the tube that is equivalent to one degree, is equal to 0,00371 gr. Now it is clear, that the weight of the whole quantity of quickſilver contained in the thermometer is to the weight of ſo much quickſilver as fills the length of one degree in the tube, as the bulk of the whole quantity of quick-

ſilver in a given degree of heat, to the increaſe of bulk, that the ſame whole quantity of quickſilver acquires when heated of but 1°; *viz.* 32,05 grs. is to 0,00371 gr. as 1 is to 0,00011 +; ſo that by this experiment it appears, that 1° of FAHRENHEIT's thermometer increaſes the bulk of mercury not above $\frac{11}{100000}$ths parts. In this proceſs a ſmall deviation from mathematical exactneſs is occaſioned by the ſmall difference of weight between the quickſilver of the tube when firſt weighed and when it is afterwards heated to 1°; but by an eaſy calculation it will be found, that this difference is ſo exceedingly ſmall as not to be perceived by our exacteſt weighing and meaſuring inſtruments.

For clearneſs ſake I ſhall ſubjoin the calculation of the above related experiments, diſencumbered from words. Here the decimals are not computed to a very large number, that being unneceſſary for this purpoſe.

Weight of the glaſs, - - - -	79,25 grs.
Weight of ſo much quickſilver as filled 4,3 inches length of the tube, - - -	0,24 grs.
Weight of the whole quantity of quickſilver contained in the thermometer, - -	32,05 grs.
Length of the tube equal to 20°, - -	1,33 inch.

4,3 : 0,24 :: 1,33 : 0,0742 = 20°

20° : 0,0742 :: 1 : 0,00371

32,05 : 0,00371 :: 1 : 0,00011 + = to the expanſion occaſioned by one degree of heat.

Having repeated this experiment with other thermometers, and by ſimilar calculations, each proceſs gave a reſult little different from the others, which irregularity is certainly owing to the imperfection of my ſcales, which are not of the niceſt ſort: but taking a mean of various experiments it appears, that

that 1° of heat, according to FAHRENHEIT's thermometer, increases the bulk of a quantity of quicksilver of $\frac{9}{100000}$th parts, *viz.* if the bulk of a quantity of quicksilver in the temperature of 50° is equal to 100,000 cubic inches, the bulk of the same quantity of quicksilver in the temperature of 51° will be equal to 100,009 cubic inches.

It is almost superfluous to mention, that the cavity of the tubes employed for these experiments, must be perfectly uniform throughout. The scales to be used for this method should be so exact as to be turned by the hundredth part of a grain when charged with about half an ounce weight.

From these observations the method of graduating, or of determining the length of a degree in a new thermometer, is easily deduced, the only requisites for the calculation being the weight of a quantity of quicksilver, which fills a known length of the tube, and the weight of the whole quantity of quicksilver contained in the thermometer when filled. Suppose, for instance, that in making a new thermometer it be found, that the weight of so much quicksilver as fills five inches length of the tube is equal to ten grains, and that the weight of the whole quantity of quicksilver contained in the thermometer weighs 300 grains. It is plain, that if the whole quantity of quicksilver weighs 300 grs. $\frac{9}{100000}$th parts of it must weigh 0,027 gr. But the weight of so much mercury as fills five inches of the tube is equal to 10 grains; therefore, 0,027 gr. weight of quicksilver must fill 0,0133 inch of the tube, and this is equal to the length of 1°, or the double, treble, &c. of it is equal to two, three, &c. degrees.

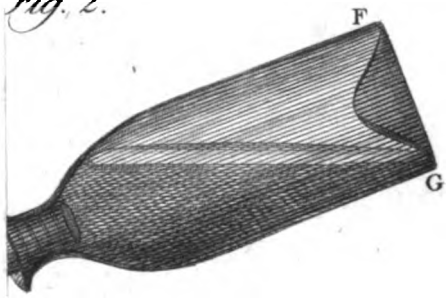
By this means the scale may be made; that is, it may be divided into degrees, but the numbers cannot be added to them without finding which of those degrees corresponds with the

freezing point or boiling point. Either the point of boiling or freezing may be found by experiment, or any other point may be ascertained by comparison with another thermometer, and then the other degrees are nominated accordingly.

III. *Description of a thermometrical barometer.*

THE determination of the various degrees of heat shewn by boiling water under different pressures of the atmosphere, has been attempted by various persons, but it was lately completed by the accurate and numerous experiments of Sir GEORGE SHUCKBURGH, member of this Society. His valuable paper is inserted in the LXIXth vol. of the Phil. Trans. Upon considering this paper, I thought it possible to construct a thermometer with proper apparatus, which, by means of boiling water, might indicate the various gravity of the atmosphere, *viz.* the height of the barometer. This thermometer, together with the suitable apparatus, might, I thought, be packed into a small and very portable box, and I even flattered myself, that with such an instrument the heights of mountains, &c. might perhaps be determined with greater facility than with the common portable barometer. My expectations are far from having been disappointed, and although the instrument which I have hitherto constructed has various defects, I have, however, thought of some expedients which will undoubtedly render it much more perfect; I shall then present to this Society a more particular account of it, and also of the experiments which I intend to make with it. The instrument in its present state consists of a cylindrical tin vessel, about two inches in diameter and five inches high, in which vessel the water is con-

Fig. 2.



Ryder Sc.

contained, which may be made to boil by the flame of a large wax candle. The thermometer is faſtened to the tin veſſel in ſuch a manner as that its bulb may be about one inch above the bottom. The ſcale of this thermometer, which is of braſs, exhibits on one ſide of the glaſs tube a few degrees of FAHRENHEIT's ſcale, *viz.* from 200° to 216°. On the other ſide of the tube are marked the various barometrical heights, at which the boiling water ſhews thoſe particular degrees of heat which are ſet down in Sir G. SHUCKBURGH's table. With this inſtrument the barometrical height is ſhewn within one-tenth of an inch. The degrees of this thermometer are ſomewhat longer than one-ninth of an inch, and conſequently may be ſubdivided into many parts, eſpecially if a nonius is uſed. But the greateſt imperfection of this inſtrument ariſes from the ſmallneſs of the tin veſſel, which does not admit a ſufficient quantity of water: and I find, that when a thermometer is kept in a ſmall quantity of boiling water, the quickſilver in its ſtem does not ſtand very ſteady, ſometimes riſing or falling even half a degree; but when the quantity of water is ſufficiently large, for inſtance is ten or twelve ounces, and is kept boiling in a proper veſſel, its degree of heat under the ſame preſſure of the atmoſphere is very ſettled.

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V O L. LXIX.

Page.	Line.
16.	5. *for* Goan *read* Jean
—	15. *for* ℥iſſ. *read* ʒiſſ.
17.	1. *for* of vitriol *read* of tartar of vitriol
18.	7. *for* erica, formed, *read* erica-formed
—	19. *for* wetted *read* melted
21.	13. *for* clays *read* clay
23.	14. *for* ℥vij. *read* ʒvij.
24.	1. *for* ℥v. *read* ʒv.
26.	6. *for* dale *read* deal
28.	12. *for* ſubſtance *read* ſubſtances
30.	20. *for* tin *read* fire
33.	laſt line, *omit the word* than
34.	23. *after* iron *inſert* ore.

V O L. LXX.

478.	1. Journal of the Weather at Senegambia, &c. *ſhould be* Journal of the Weather at Senegal, [For Senegambia is the whole province, the river Gambia included, in which river the weather muſt have been quite different at this period from what it was at Senegal, becauſe the diſorder did not rage there.
483.	laſt line, *read* that the wind in the ſame month in the river Gambia, &c.
490.	6. *for* blank *read* planked

E R R A T A.

V O L. LXXI.

Page.	Line.	
334.	5.	*for* ſits *read* ſets
343.	20.	*for* flown *read* flowed
363.	3.	of the note *, *for* after a labour *read* after a natural labour.
—	9.	———— *for* eight inches *read* five inches.
383.	13.	*for* then *read* there
417.	1.	art. 23. *for* (art. 27.) *read* (art. 28.)
423.	5.	art. 28. *for* (art. 24.) *read* (art. 23.)
425.		laſt line but two, *for* $\overline{1-p}$ *read* $1-2p^2$

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FROM THE PRESS OF J. NICHOLS.

MDCCLXXXII.