PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXIV. For the Year 1784.

PART I.

Basire sc.

LONDON,

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MDCCLXXXIV.

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 advisable, 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 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.

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THE President and Council of the Royal Society adjudged, for the last Year, 1783, Two Medals on Sir GODFREY COPLEY's Donation; One to JOHN GOODRICKE, Esq. for his Discovery of the Period of the Variation of Light in the Star Algol; and the other to THOMAS HUTCHINS, Esq. for his Experiments to ascertain the Point of Mercurial Congelation.

E R R A T A.

Page.	Line.	
117.	7.	*for* canon *read* cannon
164.	9.	*for* not *read* nor
181.	8.	*for* called *read* call
211.	7.	*for* Tweed *read* Clyde
224.	4.	*for* of *read* or

PHILOSOPHICAL

TRANSACTIONS.

I. *An Observation of the Variation of Light in the Star* Algol. *In a Letter from Sir* Henry C. Englefield, *Bart. F. R. S. and S. A. to* Joseph Planta, *Esq. Sec. R. S.*

Read November 6, 1783.

SIR, July 3, 1783.

HAVING been fortunate enough, from the fineness of the last night, to make a satisfactory observation of the variation of Algol, I lose no time in communicating it to the Society.

The last visible period was June the 10th, when Mr. AUBERT, as well as myself, observed it, though imperfectly,

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and thought the time of its greateſt diminution was about 2¾ h. in the morning; calculating from thence by Mr. GOODRICKE's period of 2 d. 20 h. 48′, the time of leaſt brightneſs was to be about one o'clock this morning.

All the following obſervations were made with an excellent night-glaſs, magnifying about eight times, with a field of 5°, in which therefore Algol and the ρ were diſtinctly viſible at once.

I firſt looked out at midnight, and readily found the ſtar, though hardly viſible to the naked eye from the vapours near the horizon. It appeared much bigger than the ρ, and full as big again as the π, alſo in the field at the ſame time.

At 12½ h. I looked again, and ſaw but little difference, as Algol was then alſo evidently much brighter than ρ. I at that time faintly perceived it with with the naked eye.

At 1 h. 10′ the ſtar was but very little bigger than ρ, the diminution having gone on moſt rapidly in the interval between the two laſt obſervations. Though higher above the horizon it was much leſs (if at all) viſible to the naked eye.

At 1 h. 35′ it was, I think, diminiſhed (though but little) ſince the former obſervation. It was ſtill, however, a very little larger than ρ, but not at all viſible to the naked eye.

At 2 h. it was ſcarce at all altered from the laſt obſervation; but, if any thing, ſeemed recovering its light.

I had meant to obſerve its progreſs ſtill further; but returning to the glaſs at half an hour after two, clouds had ſuddenly covered the whole ſky.

The fact of the diminution of Algol is, however, fully confirmed (if confirmation was wanting) by this obſervation, and the accuracy of the period fixed by Mr. GOODRICKE aſcertained,

tained, as the phænomenon was certainly within half an hour of the time fixed by Mr. Goodricke, which, divided on eight periods, gives only an error of four minutes on the length of it; and a nearer coincidence is not to be expected in a matter of this nature, where eſtimation is the only means of determining the brightneſs, and two perſons can hardly agree within a few minutes, from the difference of ſight.

I am, &c.

II. *Observations on the Obscuration of the Star* Algol, *by* Palitch, *a Farmer. Communicated in a Letter from the Count* de Bruhl, *F. R. S. to Sir* Joseph Banks, *Bart. P. R. S.*

Read November 13, 1783.

SIR,

Nov. 7, 1783.
Dover-Street,

I AVAIL myself of the permission you gave me, when I had the honour to meet you yesterday in the Drawing-room, by sending you the following short account, which was transmitted to me by Mr. CANZLER, one of the Elector's Librarians, dated Dresden the 10th of October. PALITCH, a farmer of Prolitz, a village in the neighbourhood of that Residence, saw the greatest obscuration of Algol on the 12th of September, at eight o'clock, P. M. On the 2d and 5th of October he observed the same phænomenon again. On the 5th the greatest diminution of that star's light happened some minutes before seven, when he judged it nearly of the size of a star of the fourth magnitude: it continued increasing in brightness till a quarter past ten in the evening, at which time it had entirely recovered its usual brilliancy and size. From his own observations he estimates the period of that remarkable phænomenon at 2 days 20 hours 53 minutes.

I have the honour to be, &c.

III. *Further Obſervations upon* Algol. *By the ſame.*

Read January 15, 1784.

OCT. 20th, PALITCH ſaw Algol nearly at its greateſt obſcuration, at 3 o'clock in the morning.

Oct. 22d, near 12 P. M. he obſerved it again in the ſame ſtate.

Oct. 25th, at about 9 P. M. it appeared to him like a ſtar of the third magnitude. He was prevented by clouds from making long obſervations; but as all thoſe he has had opportunities to make, indicate a period ſomewhat longer than that of 2 days 20 h. 51′ he is inclined to think that half the difference between that period and his own, *viz.* 2 d. 20 h. 52′ will come very near the truth.

IV. *Descriptions of the King's Wells at* Sheerness, Languard-Fort, *and* Harwich. *By Sir* Thomas Hyde Page, *Knt. F. R. S.; communicated by Lieut. Gen.* Rainsford, *F. R. S.*

Read November 13, 1783.

LIEUT. GEN. RAINSFORD.

SIR, St. Margaret's-Street, March 28, 1783.

I HAD the pleasure to receive your obliging letter of the 28th ult. mentioning, that it would be satisfactory to the Royal Society to have a description of the wells at Sheerness, Harwich, and Languard-Fort, which were made under my direction, whilst I commanded as Engineer at those places.

I beg to acquaint you, that it will be necessary to mention some previous circumstances that occasioned those undertakings, which will rather interfere with the descriptive part, and I fear intrude on the patience of the Society; but I shall in this respect hope for their indulgence, it being my wish to explain the nature of the different operations as fully as possible, that similar situations, where water is wanted, may receive benefit from the experiments I have had the good fortune to succeed in; and it cannot fail of affording me the highest satisfaction to have an opportunity of communicating this subject to the knowledge of the public through the Royal Society.

I have only further to request, that you will do me the honour to lay the following descriptions before Sir JOSEPH BANKS and the Society; and as you are fully acquainted with the subject, you will confer on me an additional favour by explaining the several parts (if requisite) more fully than I have done in the written account, when it is under consideration.

I am, &c.

Some circumstances respecting the garrisons of Sheerness, Landguard-Fort, *and the Town of* Harwich, *with a description of the wells which supply water for the use of the troops,* &c. *at each place.*

The Master-general of the Ordnance (Lord TOWNSHEND), in the beginning of the year 1778, recommended to his Majesty, that the fortifications upon the Eastern Coast, including Dover, Sheerness, Landguard-Fort, and some other places, should be repaired, and new works added, where they might appear necessary towards a proper state of defence, if a war with Holland, or other Northern powers, was found unavoidable. His lordship foresaw the great objection to fortifications, in the want of fresh water under the command of the guns of our garrisons; and I had directions accordingly to consider the subject, and report to his lordship and the Board of Ordnance any ideas that might be likely to remedy so great a defect.

The dock-yard and garrison at Sheerness were supplied with water from Chatham at an enormous expence, near two thousand

ſand pounds *per annum*, or occaſionally from Queenborough, neither of which ſupplies could be continued in caſe of a ſiege, which of courſe would be of ſhort duration from this circumſtance. Some attempts had been made in former times to obtain water on the ſpot, by ſinking wells, but they had failed; and ſucceſs in ſuch undertakings was at laſt conſidered as impoſſible, from the great difficulty they had met with in the vaſt quantities of ſea-water, that came by filtration through the ſands into their wells, and rendered a progreſs to any conſiderable depth impracticable. It is probable, that the courſe of the river Medway has undergone many changes, and had once an out-fall to the ſea, near the high ground of the Iſle of Shepey. The docks, garriſons, buildings, &c. for a conſiderable diſtance into the iſland, conſequently ſtand upon very looſe materials, which were found, upon ſinking the well in Fort Townſhend, to conſiſt of mud, ſea-beach, and quickſand, nearly to the preſent depth of the river Medway, and admit ſo ſtrong a filtration of ſalt-water, as muſt ever render the ſinking of wells exceedingly difficult. This was the ſituation in which I found Sheerneſs.

Landguard-Fort was not more eligible reſpecting water, as a place of ſtrength. It was, indeed, better ſupplied under any other conſideration, a pipe being laid into the place from a good ſpring about two miles diſtant, which furniſhed a plentiful quantity of water; but ſuch is the diſadvantage of ſituation that, in caſe of attack, that ſpring muſt fall into the poſſeſſion of the enemy, and our garriſon of courſe would be deprived of its uſe. This was a ſerious conſideration and objection to a great extent of fortification, however eligible in other reſpects the place might be.

Harwich was judged by the Commander in Chief (Lord AMHERST) to be a very proper ſtation for a conſiderable part of the army, in time of war with Holland, as centrical to furniſh detachments for ſuch parts of the coaſt as might be in danger, as alſo to cover a very uſeful harbour and increaſing dock-yard; but his lordſhip was ſenſible of the want of wholeſome water in that neighbourhood, and gave particular directions to eſtabliſh ſuch a ſupply for the camp to be formed there, as might appear proper for the health of the troops; and the ſubſequent orders given by General RAINSFORD, who commanded that diſtrict, perfectly anſwered every deſirable end, until good water was found within his camp. The inhabitants of the town of Harwich had chiefly depended on rains for their ſupply, the wells being in general brackiſh from the filtration of ſalt-water. The neighbourhood, to many miles diſtance, was not better furniſhed, there being only ſtagnating water in ponds or ſhallow wells, which were ſupplied from the upper ſurface of the ground; and, whether rendered bad by a mixture of copperas, or other mineral, it was not ſuch as could be given for the uſe of the troops with any degree of prudence or attention to their health, and they were, to avoid dangerous conſequences, furniſhed with water, by General RAINSFORD's order, from the oppoſite ſide of the Manningtree River, by boats employed for that purpoſe, the beginning of the firſt campaign.

I will now endeavour to deſcribe the experiments at each place, beginning with the well in Fort Townſhend at Sheerneſs, which with a reference to the plans will, I hope, render the ſubject ſufficiently intelligible.

King's Well, Fort Townshend, Sheerness.

This undertaking was at first considered as a mere experiment, the probability of success being much against it; I however thought the attempt, where a dock-yard of great consequence to the navy was established, should be made, and carried as far as it could, with a proper attention to œconomy in laying out the money of the public. Such was my opinion signified to the Master-general and the Board of Ordnance. I received an answer thereto, expressive of approbation, and full powers to employ proper persons, and proceed upon the undertaking.

These previous steps seemed highly necessary, as in all works of difficulty, great confidence is as much required as able workmen or good plans.

The favourable opinion his Majesty was graciously pleased to express publicly of the project, when he visited Sheerness, and saw the well, tended very much towards its final success; and the countenance and support of General CRAIG, governor of that garrison, greatly promoted perseverance in a work of such difficulty.

I employed a very ingenious man, Mr. COLE, engine-maker, of Lambeth, as a chief person in this business, and received every assistance I expected from his experience and judgement in mechanics; and it is but justice to him to express, that the success of the work greatly depended on his attention and the able assistants he procured from distant parts of the kingdom.

The greatest acknowledgement is also due to the ability of Lieut. HUMFRYS, of the Engineers, and Mr. MARSHALL, the Ordnance-overseer, who were constantly on the spot, and carried

ried my orders into execution with the greateſt zeal for the ſucceſs of the undertaking as well as judgement. The mentioning theſe gentlemen's names is, as well as a juſtice to their conduct, to recommend harmony and mutual exertion in any future work of this nature, as, without an equal attention in every one, I ſhould greatly doubt ſucceſs, even admitting the ſame plan to be in all other reſpects ſtrictly attended to, as there would be great difficulty and danger to the lives of the workmen if careleſsly carried on.

The work was begun the 4th of June, 1781, and finiſhed the 4th of July, 1782.

A circle of twenty-two feet diameter was firſt marked out on the ground, and the ſpace excavated to the depth of five feet; after which, pieces of wood, called ribs, upon the curve of a diameter twenty-one feet four inches, and about nine inches ſcantling, were placed, to form a complete circle within the excavated part at the bottom, above which other circles of the ſame nature were placed, and ſupported by upright pieces of ſcantlings, having ſhort boards introduced by the intervals, which afterwards preſſed upon the circles or ribs, between them and the exterior parts. Theſe, when united, formed one frame of wood from the bottom to the top, or rather higher than the excavated ſpace, and prevented the mud of the upper ſurface, which was very ſoft, from falling in upon the workmen. In proceeding deeper, care was taken to prevent the ſinking of the before-mentioned frame by its own weight, in excavating parts only under it till another circle of pieces like the firſt, called ribs, was formed, and uprights, with boards behind, introduced. The diſtance between theſe circles was in the firſt, or upper part of the work, about three feet; but as difficulties increaſed they were placed nearer, and in many parts joined each other without any boards or uprights (as will appear in the section of the plan), and continued through the whole of the wooden frame, against the weight of the mud, quick-sand, and sea-beach, to the depth of thirty-six feet.

The reason of the circular frames being nearer in some parts than in others, arose from the greater or less quantity of salt-water that came through the sands, &c. and often rendered it impossible to sink under the frame more than the thickness of one of the ribs, without danger of blowing-up, or of the sides behind the wood slipping with the streams of water, and thereby forcing into the bottom of the well, which in sinking through very wet quick-sand is much to be apprehended; and an accident of that nature would entirely destroy the work. An attention to the plan will shew at what depths the filtration of water was most dangerous, and the difficulties at different periods, may be estimated by the distance of the circles, formed of ribs, from each other, and where they appear to join, it was not without the utmost efforts of labour that the work could be carried on. At the depth of thirty-six feet the wood-work was finished, and six feet deeper a firm foundation of hard blue clay discovered. The several parts of the frame were then strengthened wherever it appeared necessary, to prevent separation, and to resist the immense pressure of soft mud, quick-sand, and loose sea-beach, which were supported by it.

It must be observed, that the salt-water, after proceeding thus far, came in very fast through all the joints of the frame, and that holes were left on purpose in certain parts to let it run into the well, that it might not be confined entirely to the bottom of the work, which, from the weight upon one part

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only, might have blown, which is ever (as has been obferved) to be guarded againft with the utmoft caution.

The frame being found of fufficient ftrength, and the workmen able, by conftant drawing with four 36-gallon buckets, to keep the bottom of the well dry enough to proceed further, the greateft difficulty feemed to be overcome. The next procefs was to cut off or ftop the falt-water out entirely: to effect which, a fmaller circle was defcribed at the bottom of the well, upon the hard clay already mentioned, of the diameter of eight feet in the clear, round which a curb, or circular frame of wood, was laid, and a brick fteening, of two bricks thick in tarris, raifed gradually towards the top of the well, whilft, as it proceeded upwards, the fpace between the back of this fteening and the wooden frame (fixed fix feet higher) was filled with good tempered clay, four feet thick, and carefully rammed. During this operation, and raifing the brick-work, with the clay behind it, the water continued to run over them into the center of the well, now reduced to eight feet diameter, and was conftantly drawn out, to leave the workmen on the fides fufficiently dry to raife their work until they had reached the top, and confequently, as it was water-tight, cut off the filtration from the fea, precautions having been taken to prevent the danger of blowing at the bottom.

The next proceeding appeared more fimple; but great care was ftill neceffary to avoid damaging the foundation of the works already done, as the leaft crack might have again introduced the falt-water. A fmaller circle than the laft was therefore defcribed, and ribs, forming circles of wood, raifed fome feet within the brick-work; and others, of the fame form, were funk to the depth of eight feet below the bottom, upon which the feveral works already defcribed refted. After this a

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course of bricks was carried up within the last mentioned ribs or circles, upon a diameter of six feet, whereby they became inclosed and joined with the first mentioned brick-work, having the clay wall and wooden frame pressing behind them upon larger diameters. In sinking lower, small curbs were at certain distances (as will appear in the section of the plan) placed to support the steening, which consisted of two stretching courses of bricks, laid separately, and keyed into the clay or back part of the brick-work by rough pieces of stone, flint, &c. to prevent a slipping or lowering of the steening by its own weight. The work was carried on from this period, without any material difficulty or difference in the clay (except the very extraordinary discovery of a piece of a tree at the depth of 300 feet from the top of the well, which is shewn in the plan) until the appearance of water at 328 feet deep, by a small mixture of sand in the clay, with oozing of water from it; and at 330 feet deep, upon boring, the whole bottom of the well blew up, and it was with difficulty the workmen escaped the torrents of water that followed them, which was mixed with a quick-sand that rose forty feet in the bottom of the well, at which height it still remains. The water rose in six hours 189 feet, and in a few days within eight feet of the top of the well. It has since been carefully analyzed by a chemist, and found perfectly good for every purpose; and, it is presumed, the quantity will be equal to every demand of public and private use at that place, as there has been, ever since it was first discovered, a constant drawing of water, and it has hitherto been found impossible to lower the well more than 200 feet, there has consequently always been a depth left in water of 130 feet. It is to be remarked, that the water is of a very soft quality, and, upon being drawn, has a degree of warmth unusual in common

common well-water. It remains yet to be determined whence that warmth proceeds; but as it proved wholeſome, the circumſtance is fortunate for the ſoldiers of the garriſon, as they will not be liable to complaints that are ſo frequent among troops (as often happens at Dover Caſtle) from imprudence in drinking great quantities of very cold well-water.

King's Wells at Landguard-Fort.

They were begun and finiſhed in the year 1782.

The peculiar ſituation of this fort made it very unlikely that ſprings of freſh-water could ever be found, there being great reaſon to think, that the out-fall of the Ipſwich and Manningtree Rivers, which unite before they reach they ſea, was formerly on the Suffolk ſide of the fort, but is now on the Eſſex ſide; and as the garriſon, in ancient writings, is deſcribed to have been built on the Andrew's Sand, there appeared little probability of any filtration of water through it, except that of the ſea. It, however, ſeemed proper to try the poſſibility of ſinking through it, to endeavour to find a hard bottom, ſimilar to that diſcovered at Sheerneſs, freſh-water being of vaſt conſequence to the defence of the place. The work was accordingly begun; but about the ſame time, in making the excavation of a ditch for one of the batteries, at a very few feet from the upper ſurface of the ſand, a ſmall quantity of freſh-water was perceived; and it was chance that led to a diſcovery of its freſhneſs, from one of the labourers happening to taſte it. The circumſtance

circumſtance was reported to me by Mr. ROBERTS, the Adjutant of the Works; and we, upon examining further, found that the quantity of water upon ſinking was conſiderable, and that it appeared perfectly freſh. I then ordered the well-ſinkers to proceed to this depth at another place, where they found a like appearance of good water; and the quantity was ſo great, as to render it very difficult to keep the bottom of the well, at twelve feet deep, dry enough to ſink further. Every exertion was notwithſtanding uſed, and with great labour a well was ſunk to the depth of low water mark at ſpring tides, about eighteen feet from the upper ſurface of the ſand; when, to the ſurprize of every perſon, the water that roſe from the bottom became, on a ſudden, entirely ſalt. This put an end to the work for a time, as it ſeemed impoſſible to penetrate deeper. I then conſidered the matter very differently with my firſt idea, and though the impoſſibility of having a deep well clearly appeared, there remained a proſpect of a ſufficient ſupply of good freſh water. It may now be neceſſary to recollect, that at a very few feet from the ſurface (eight feet) there was good water; that it continued in vaſt quantity almoſt to the ſpring tide low-water-mark, after which the ſalt-water had appeared; I therefore directed ſand to be thrown into the well, to bring it a little above what had been the *loweſt freſh-water* line (twelve feet from the upper ſurface) and then drew the water out which had mixed. After this, the filtration into the well became again perfectly freſh, and in equal quantity to the firſt appearance. This was, therefore, fixed as the greateſt depth (twelve feet) and another well ſunk at forty feet diſtance, with a horizontal brick drain, having holes left in the ſides for filtration, as deſcribed in the plan, to collect the water, and the bottoms of both wells were ſecured with hard materials; that the whole ſupply of water might be reduced to the drain, which is conſtructed to prevent as much as poſſible the mixture of ſand with the water, and is found to anſwer the deſired end. This ſucceſs aroſe from various unexpected circumſtances; but I am yet at a loſs for the cauſe of the freſh water, or whence it comes.

I conceive, that there is a certain diſtance from the ſea, upon every ſandy ſhore, to which the ſalt-water penetrates, where it is forced whilſt the tide is at its greateſt height; and that ſuch water, when ſo far preſſed into the ſands, has an action back towards the ſea again, as *the tide falls*, and continues to have it until another tide makes it revert; this may account for the filtration of ſalt-water a certain way into a country; and that further, from *probably higher ſurfaces*, there may be freſh-water in the ſame continuation of ſands, and the ſeparation diſcoverable to a degree of great accuracy; whether this action of ſalt-water in the ſand, by friction, can render it freſh, or of a leſs degree of ſalt, I will not pretend to judge. I preſume the contrary; but am even under that idea at a loſs to know how ſo much freſh water gets into the ſand at Landguard-Fort, it being ſo entirely ſeparated from the ſpring of the country. It is evident, upon a full conſideration of the ſubject, that the ſea, to the height of *low water*, will penetrate a vaſt diſtance into a ſandy country, by filtration, and to that height *only*, it having ſo far a conſtant preſſure, and no re-action; the water, therefore, being once in the ſand, can never return by the ſame paſſage, the cauſe of its entrance ſtill remaining; whereas in the higher ſurfaces, the *riſe* and *fall* of tides muſt keep it in conſtant movement, and the diſtance of filtration will bear a proportion to the duration of preſſure which gave it original motion. It is probably not ſo eaſy to account for a body of

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fresh-water being to the depth of twelve feet in the sand, and in the same line, a few feet deeper, the water should be entirely salt, and that they do not mix together. Whether the greater specific gravity of the salt-water is sufficient to prevent a mixture with the fresh upon a higher line, I cannot venture to say; but the fact of there being a separation is beyond a doubt, and the depths may be ascertained to a degree of great accuracy. However this may be accounted for, the discovery at Landguard-Fort is of very great consequence to the garrison; and there is reason to think, that in similar situations, where water is wanted, an attention to what has been already explained may be found of use.

King's Wells at Harwich.

They were begun the 6th of May, 1781, upon General Rainsford's taking the command at that camp, and finished the 29th of September following.

The wells in this neighbourhood, as has already been observed, being very shallow, and only depending on springs from the upper surfaces of the ground, have but little water in the summer, and the quality of it is very bad. The best of the old wells was in the rear of General Rainsford's camp, and was thought of at first for the use of the troops; but he prudently declined that supply. It was imagined, as the water from the upper surface was of a bad quality, that the most likely way to obtain a better spring was to sink a well from higher ground, and to endeavour to penetrate through a rock which lay a few yards under the level of the country, although the operation might be tedious, upon the chance of cutting a

spring

ſpring of better water, that might be unconnected with the land-drains. The experiment anſwered in every reſpect, as there was not a drop of water found till the rock had been entirely cut through, when, upon finding a conſiderable quantity of moiſt ſand, and boring into it, a plentiful ſpring was diſcovered, and has ſupplied the troops ever ſince with very good water. It is probable this ſupply, the ſpring being very powerful, will be found equal to every demand for public and private purpoſes, in the dryeſt ſeaſons. After this ſucceſs, as matter of curioſity, an old well was made deeper, by excavating through the rocks, where a good ſpring was alſo found; but as that well had been originally ſunk from low ground, a great deal of the bad water from the upper drains, &c. mixes with it, and gives it a diſagreeable taſte.

The plans will deſcribe the manner of making theſe wells ſufficiently. I have chiefly dwelt on the deſcriptive part, to recommend, where it is apprehended any mineral or drain from the upper ſurface of lands, by mixing in wells, may hurt the water, the ſinking from the heights, as there are few countries where very good water may not be found, by a proper attention to locality in making wells.

EXPLANATION OF THE PLATES.

Tab. I. fig. 1. Section of the King's Well in Fort Townſhend at Sheerneſs.
2. Plan of the frame and well.
3. Section of the frame AA.
4. Plan of the well.

Tab. II. X. Line of high-water mark.
Y. Line of low-water mark.
Z. Line of low-water at ſpring-tides.

V. *Extract of a Letter from* Edward Pigott, *Esq. to M.* de Magellan, *F. R. S.*; *containing the Discovery of a Comet.*

Read November 27, 1783.

SIR, York, Nov. 22, 1783.

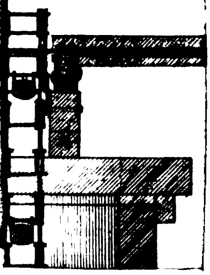
I HAVE the pleasure of informing you, that I discovered a comet on the 19th instant, and have made the following observations on it.

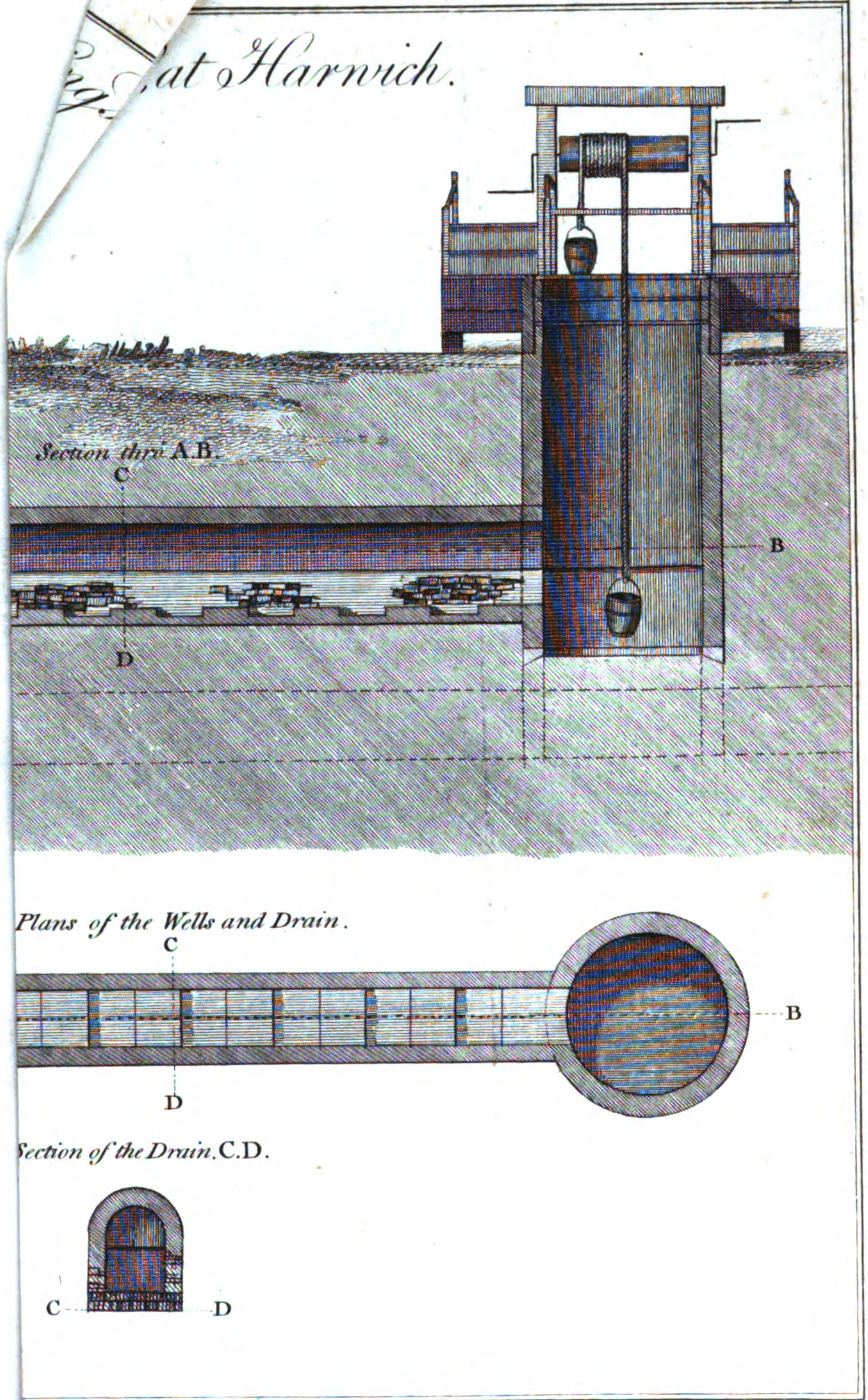
Date. 1783	h.	′		R. A. °	′		North Decl. °	′
Nov 19	11	15	-	41	0	-	3	10
20	10	54	-	40	0	-	4	32

Nov. 21. This night I saw the comet where I expected it, according to the above determinations; but could not observe it with an instrument.

The comet looks like a nebula, with a diameter of about *two minutes* of a degree. The nucleus being very faint, is seen with some difficulty, when the wires of the instrument are illuminated. It is not visible with an opera glass.

Sheern




at Harwich.
Section thro' A.B.
C
B
D
Plans of the Wells and Drain.
C
B
D
Section of the Drain. C.D.
C
D

VI. *Project for a new Division of the Quadrant.* *By* Charles Hutton, *LL.D. F. R. S.* *In a Letter to the Rev. Dr.* Maskelyne, *F. R. S. and Astronomer Royal.*

Read November 27, 1783.

Royal Mil. Acad. Woolwich,
Aug. 12, 1782.

DEAR SIR,

HAVING long since thought it would be a meritorious and useful service, to adapt the tables of sines, tangents, and secants, to equal parts of the radius instead of to those of the quadrant; and having frequently mentioned this project to you, SIR, as a proper judge and promoter of all useful improvements in science; I now beg leave to lay before you some observations I have thrown together on the subject, with a view to stimulate others, either to undertake and calculate some part of so large and painful a work, or to communicate farther hints for the improvement and easier performance of it.

I have the honour to be, &c.

A project

A project for constructing sines, tangents, secants, &c. to equal parts of the radius.

1. The arbitrary division of the quadrant of the circle into equal parts by 60ths, which has been delivered down to us from the ancients, and gradually extended by similar sub-divisions by the moderns, among various uses, serves for trigonometrical and other mathematical operations, by adapting to those divisions of the arc, certain lines expressed in equal parts of the radius, as chords, sines, tangents, &c. But among all the improvements in this useful branch of science, I have long wished to see a set of tables of sines, tangents, secants, &c. constructed to the arcs of the quadrant as divided into the like equal parts of the radius as those lines themselves. In this natural way, the arcs would not be expressed by divisions of 60ths, in degrees, minutes, &c., but by the common decimal scale of numbers; and the real lengths of the arcs, expressed in such common numbers, would then stand opposite their respective sines, tangents, &c. The uses of such an alteration would be many and great, and are too obvious and important to need pointing out or enforcing. I have therefore had for a long time a great desire to commence this arduous task; but continual interruptions have hitherto prevented me from making any considerable progress in so desirable an undertaking. But I am not without hopes that some future occasion may prove more propitious to my ardent wishes. It is not, however, to be expected, that this work can be accomplished by the labours of one person only; it will require rather the united endeavours of many. I shall therefore explain a few particulars relative to my project of this work, with a view to obtain from others, who

who may have leiſure and abilities for it, their kind aſſiſtance, either by communicating hints of improvements, or by undertaking ſome part of the computations, to which they may be excited by their zeal for the accompliſhment of ſo important a work, and by the extreme facility with which the calculations in this way are made.

2. In the firſt place then I would obſerve, that I think it will be ſufficient to print the ſines, tangents, &c. to ſeven places of figures; and that therefore it will be neceſſary to compute them to ten places, in order effectually to ſecure the truth of the ſeventh place to the neareſt unit.

3. I would aſſume the radius equal to 100000, or ſuppoſe it to be divided into 100000 equal parts. Then it is well known, that the ſemi-circumference will be 314159·26536 nearly, and conſequently the quadrant nearly 157079·63268 of the ſame equal parts, which is leſs than 157080 by ·36732, or nearly $\frac{1}{3}$ of an unit, or nearer $\frac{3}{8}$ = ·375, or nearer $\frac{4}{11}$ = ·3636, or ſtill nearer $\frac{7}{19}$ = ·3684, or ſtill nearer $\frac{11}{30}$ = ·36666 &c. And the half quadrant, or $\frac{1}{8}$ of the circle, 78539·81634 which is leſs than 78540 by only ·18366, or nearly $\frac{1}{2}$ only of any of the above-mentioned fractions.

4. The table may conſiſt of five or more columns; the firſt column to contain the regular arithmetical ſeries of arcs differing by unity, from the beginning, in this manner, 1, 2, 3, 4, 5, &c. up to half the quadrant, the next leſs whole number being 78539; then for the higher numbers, or thoſe in the latter half quadrant, beſides adding 1 continually, there muſt be at the firſt added the decimal ·63268, which will make all the numbers in this half become the exact complements of the firſt half, which conſiſts of whole numbers only; and theſe will be the lengths of the arcs. Or, in order to include the

we must also add 1″ more at every 16, which will make 3″ to be added at every 16th time, and 2″ at every other time besides; but the first time the 3″ must be added will be at the arc or number 8, to have them to the nearest second, the repetition of the fraction at the arc 8 amounting to above ½ a second; and then the 3″ must be added at every 16 afterwards, *viz.* at 24, 40, 56, 72, 88, 104, &c.

10. But besides the constant addition of 2″ every time, and of 1″ more every 16th time, there must be 1″ more added for every 6753½ time, on account of the excess of the fraction ·062648062470964 over the fraction ·0625 or $\frac{1}{16}$: for that excess is ·000148062470964 which is $= \frac{1}{6753\frac{1}{2}}$. And the easiest method of making this last addition of 1 at every 6753½, will be to make the increase of the 1 on account of the $\frac{1}{16}$ at an unit sooner for every $422\frac{3}{32}$; because 16 is $422\frac{3}{32}$ times contained in 6753½; by which means the incremental units for the $\frac{1}{16}$ will become 1 more at that number 6753½, which last unit may be considered as the increment of the former increment for the $\frac{1}{16}$, and so proceed up to the quadrant; which will complete the second column of arcs to the nearest second in each number. Or this second column may be exactly computed to as many decimals as we please, by adding continually the 2″ and decimals, *viz.* 2·062648062470964. But at the middle of the quadrant, where the numbers return again upwards by the right-hand, there will for once be to be added only the seconds and decimals answering to the arc ·63268, *viz.* 1·30499618 seconds, that number being necessary to make the numbers on the right-hand to be the exact complements of those on the left. Or it will, perhaps, be proper to make them to the nearest unit in the 6th place of decimals. And to

4 fill

fill up the second column to this degree of accuracy, add continually 2·062648 seconds, but at the 9th line add 1 more, or 2·062649, because 9 times 062470964 amounts to 56223867, or more than half a unit at that place; and after that add 1 more than 2·062648 at every 16th line, *viz.* at 25, 41, 57, 73, &c. because 16 times 062470964 amounts to ·99953542, or nearly 1, it being only ·00046458 less than 1. And this number ·00046458, thus added too much, will, in 134 times adding it, amount to more than 06223867, the excess of 56223867 above 5, or half a unit, at that place; therefore at the line or number 2153 (or $9+16\times 134$) which would be to have the 1 more added, let the 1 be there omitted, and add it at the next line or 2154, the true decimals after the first six, for 2153 being 499985, and for 2154 they are 562456. Continue thus always adding 1 more at every 16th line, except at the following numbers, where the 1 must be omitted, and added at the next following number; *viz.*

2153	10765	19377	27989	36601	45213	53825	62437	71033
4314	12926	21538	30150	38746	47358	55970	64582	73194
6459	15071	23683	32295	40907	49519	58131	66743	75339
8620	17232	25844	34456	43052	51664	60276	68888	77500

And thus proceed to the middle of the quadrant; by which means all the numbers will be to the nearest unit in the sixth or last place. Also, to have a check upon these numbers at certain intervals, it may be proper to proceed in this manner: First find every 100th number, by adding its decimal ·264806 &c. verifying them at every 10th; then find every 16th number, by adding continually ·002369 &c. which will also be checked and verified at every 25th addition by one of the former set of 100, for 25 times 16 make 400, using a proper pre-

 caution

caution to preserve each number true to the nearest unit in the 6th or last decimal.

As to the decimals of the numbers in the latter half of the quadrant, they will be the complements, to 1, of the corresponding numbers in the first half; and therefore they may be all easily found by taking each figure from 9, and the last from 10. But it will be safest to find only every 10th decimal in this way, and to fill up the intermediate nine by adding, as before, the constant decimal 062648; by which means they will be checked and verified at every 10th number.

11. To fill up the third column, or that of sines, as well as those of tangents and secants, it may first be observed, that the old tables of those lines to every minute, or even to every ten seconds of the quadrant, cannot be of so much use as it might seem at first sight; as the very near coincidence of the numbers in the new and old divisions appear very seldom to happen. I find, indeed, that our arc 1309 answers nearly to 45 minutes, that arc exceeding 45′ by only ·00632363 or $\frac{1}{158}$ part of a second nearly, and so in proportion for their equimultiples. But although this degree of coincidence may be sufficient for checking the corresponding values of the arcs in the first and second columns, we are not thereby authorised to consider the sine, tangent, or secant of 1309 as accurately equal to that of 45′ in all the seven places of figures, but differing from it by nearly the $\frac{1}{158}$ part of the difference corresponding to 1″, which is about $\frac{1}{4}$ of an unit in the sines and tangents, though next to nothing in the secants. This, therefore, although it makes no sensible difference in this particular case, will cause a difference that must not be neglected in the equi-multiples of 1309 and 45′, the sines and tangents of which will differ by half a unit or

3 more,

more, and therefore will not be expressed by the same number, but will have some small difference in the seventh or last figure. And the same will happen in almost all the other arcs; so that generally the sines, &c. which are exact for the arcs in the first column, will not be quite so for those in the second, when expressed in whole seconds only, since these will sometimes differ by the part corresponding to almost half a second. However, in this, or any other case, where the difference is exactly known, we may profitably make use of the numbers in the old tables for constructing or verifying those of the new, by taking in the proportional part of the difference. Let, therefore, all the sines, &c. of every 1309 be computed from the old tables, and entered in the new, by adding to the sine, &c. of the corresponding multiple of 45′ the like multiple of the $\frac{1}{113}$ part of the proportional difference for 1″. This will give about 120 sines, &c. to serve as a verification of the computations by the more general methods. But if the second column be exactly constructed with all its decimal places by the continual addition of 2·06264807, the old tables may be converted into the new, by allowing for the odd seconds and decimals. And for this purpose it will, perhaps, be best to use the large table of RHETICUS, which contains the sines, tangents, and seconds, to ten places of figures for every 10″, and also the differences. At least, such sines, &c. may be found in this way as have their seconds and decimals well adapted for the purpose; and for such as would be found too troublesome in this way, recourse may be had to some of the following methods.

12. Let us now examine the expressions for the sines, &c. by infinite series.

The

The radius being 1, and arc a, it is well known that the

$$\text{sine is } = a - \tfrac{1}{6}a^3 + \tfrac{1}{120}a^5 - \tfrac{1}{5040}a^7 + \tfrac{1}{362880}a^9 - \tfrac{1}{39916800}a^{11} \text{ \&c.}$$

$$\text{cosine } = 1 - \tfrac{1}{2}a^2 + \tfrac{1}{24}a^4 - \tfrac{1}{720}a^6 + \tfrac{1}{40320}a^8 - \tfrac{1}{3628800}a^{10} \text{ \&c.}$$

$$\text{tang. } = a + \tfrac{1}{3}a^3 + \tfrac{2}{15}a^5 + \tfrac{17}{315}a^7 + \tfrac{62}{2835}a^9 + \tfrac{1382}{155925}a^{11} \text{ \&c.}$$

$$\text{cotang.} = a^{-1} - \tfrac{1}{3}a - \tfrac{1}{45}a^3 - \tfrac{2}{945}a^5 - \tfrac{1}{4725}a^7 - \tfrac{2}{93555}a^9 \text{ \&c.}$$

$$\text{secant } = 1 + \tfrac{1}{2}a^2 + \tfrac{5}{24}a^4 + \tfrac{61}{720}a^6 + \tfrac{277}{8064}a^8 + \tfrac{50521}{3628800}a^{10} \text{ \&c.}$$

$$\text{cosec. } = a^{-1} + \tfrac{1}{6}a + \tfrac{7}{360}a^3 + \tfrac{31}{15120}a^5 + \tfrac{127}{604800}a^7 + \tfrac{73}{3421440}a^9 \text{ \&c.}$$

Or the same series are thus otherwise expressed:

$$\text{sine } = a - \frac{1}{2.3}a^3 + \frac{b}{4.5}a^5 - \frac{c}{6.7}a^7 + \frac{d}{8.9}a^9 - \frac{e}{10.11}a^{11} \text{ \&c.}$$

$$\text{cosine } = 1 - \frac{1}{2}a^2 + \frac{b}{3.4}a^4 - \frac{c}{5.6}a^6 + \frac{d}{7.8}a^8 - \frac{e}{9.10}a^{10} \text{ \&c.}$$

$$\text{tangent} = a + \frac{1}{1.3}a^3 + \frac{8b}{4.5}a^5 + \frac{17c}{6.7}a^7 + \frac{29\frac{3}{17}d}{8.9}a^9 + \frac{44\frac{18}{31}e}{10.11}a^{11} \text{ \&c.}$$

$$\text{cotang.} = a^{-1} - \frac{1}{3}a - \frac{b}{15}a^3 - \frac{2c}{21}a^5 - \frac{d}{10}a^7 - \frac{10e}{99}a^9 \text{ \&c.}$$

$$\text{secant } = 1 + \frac{1}{2}a^2 + \frac{5b}{12}a^4 + \frac{61c}{150}a^6 + \frac{1385d}{3416}a^8 + \frac{50521e}{124651}a^{10} \text{ \&c.}$$

$$\text{cosec. } = a^{-1} + \frac{1}{6}a + \frac{7}{60}a^3 + \frac{31c}{294}a^5 + \frac{127d}{1240}a^7 + \frac{2555e}{25146}a^9 \text{ \&c.}$$

where b, c, d, e, &c. denote the preceding co-efficients. And hence, with the help of the table of the first ten powers of the first 100 numbers, in p. 101. of my tables of powers published by order of the Board of Longitude, may be easily found the sines, &c. of all arcs up to 100, by only dividing those powers by their respective co-efficients, as also of all multiples of these arcs by 10, 100, &c. by only varying the decimal points in the several terms, as the figures will be all the same: and

and thus a number of primary ſines, &c. may be found, to check or verify the ſame when computed by other methods. By this method will be found the ſines, &c. of the arcs

1, 10, 100, 1000, 10000, 100000;
2, 20, 200, 2000, 20000;
3, 30, 300, 3000, 30000;
4, 40, 400, 4000, 40000;
&c. till
99, 990, 9900, 99000, 990000.

13. Again, it is evident, that, of the terms in the ſeries for the ſine, the firſt term a alone will give the ſine true to the neareſt unit in the ninth place in the firſt 144 ſines, or the arc and ſine will be the ſame for nine places as far as the arc 144; but they will agree to the neareſt unit in the ſeventh place as far as the arc 669; after which the ſecond term of the ſeries muſt be included.

14. When the ſecond term is taken in, theſe two terms $a - \frac{1}{6}a^3$ will give the ſines true to the neareſt unit in the ninth place till the arc becomes 3500. Now the numbers in my table of cubes (juſt publiſhed by order of the Board of Longitude) extend to 10000, and therefore all the above cubes are found in it; conſequently taking the ſixth part of thoſe cubes, and ſubtracting it from the correſponding arcs, the remainders will be the ſines of thoſe arcs, as far as till the arc be 3500: after which the third term of the ſeries may be taken in, or other methods may be uſed.

15. But ſince, for any arc a, this is a general theorem, *viz.* as radius : 2 coſ. a :: ſin. na : ſin. $\overline{n-1} \times a$ + ſin. $\overline{n+1} \times a$; taking $a = 1$, radius 10000, the ſine of a will be $1 - \cdot0000000000\frac{1}{6}$ and the coſine of a will be $100000 - \cdot000005$, and the above

above proportion will become 100000 : $\overline{200000 - \cdot 00001}$, or 1 : $\overline{2 - \cdot 0000000001}$ :: ſin. n : ſin. $\overline{n-1}$ + ſin. $\overline{n+1}$; conſequently ſin. $\overline{n-1}$ + ſin. $\overline{n+1}$ is = 2 ſin. n. − ·0000000001 ſin. n, and the ſines are in arithmetical progreſſion except only for the ſmall difference of ·0000000001 ſin. n, hence ſin. $\overline{n+1}$ is = $\overline{2 - \cdot 0000000001}$ × ſin. n − ſin. $\overline{n-1}$; and therefore taking n ſucceſſively equal to 1, 2, 3, 4, &c. the ſeries of ſines will be as follows:

ſin. 1 = 1 − ·0000000000$\frac{1}{6}$;
ſin. 2 = $\overline{2 - \cdot 0000000001}$ × ſin. 1;
ſin. 3 = $\overline{2 - \cdot 0000000001}$ × ſin. 2 − ſin. 1;
ſin. 4 = $\overline{2 - \cdot 0000000001}$ × ſin. 3 − ſin. 2;
ſin. 5 = $\overline{2 - \cdot 0000000001}$ × ſin. 4 − ſin. 3;
&c.

And by this theorem, *viz.* ſin. $\overline{n+1}$ = $\overline{2 - \cdot 0000000001}$ × ſin. n − ſin. $\overline{n-1}$, may be eaſily filled up the intervals between thoſe primary numbers mentioned in former articles.

16. In like manner, as radius : 2 coſ. a :: coſ. na : coſ. $\overline{n-1}\,.\,a$ + coſ. $\overline{n+1}\,.\,a$; and hence this theorem, coſ. $\overline{n+1}$ = $\overline{2 - \cdot 0000000001}$ × coſ. n − coſ. $\overline{n-1}$, by which the coſines will be all eaſily filled up. And theſe two theorems for the ſines and coſines are ſo eaſy and accurate, that we need not have recourſe to any other, but only to check and verify theſe at certain intervals, as at every 100th number, by a proportion from RHETICUS's canon, as mentioned at art. 11. or by any other way.

17. The ſines and coſines being compleated, the difference between the radius and coſine will be the verſed ſine; the difference between radius and ſine will be the co-verſed ſine; and the ſum of the radius and coſine will be the ſup.verſed ſine.

18. From

18. From the fines and cofines alfo, the tangents, cotangents, fecants, and cofecants, may be made by thefe known proportions, *viz.* as

1.	cofine	:	radius :: fine	:	tangent,
2.	fine	:	radius :: cofine	:	cotangent,
3.	cofine	:	radius :: radius	:	fecant,
4.	fine	:	radius :: radius	:	cofecant,
5.	radius	:	fine :: fecant	:	tangent,
6.	radius	:	cofine :: cofecant	:	cotangent,
7.	tangent	:	radius :: radius	:	cotangent.

Wherefore, the reciprocal of the cofine will be the fecant; the reciprocal of the fine, the cofecant; the quotient of the fine by the cofine, the tangent; and the quotient of the cofine by the fine, the cotangent; or the product of the fine and fecant will be the tangent, and the product of the cofine and cofecant, the cotangent; or, laftly, the reciprocal of the tangent is the cotangent; proper regard being had to the number of decimals, on account of our radius being 100000 inftead of 1 only.

And thefe are to be ufed when the application happens to be eafier than the general feries, and eafier than by proportion from RHETICUS's canon.

But there are other particular theorems, which, by a little addrefs, may be rendered more expeditious than any of the former: thus,

19. In any two arcs this is a general proportion,
As the difference of their fines :
to the fum of their fines ::
fo tangent of half the difference of the arcs :
to tangent of half their fum.

So that by taking continually the arcs, having the common difference 2, the third term of this proportion will be 1, and the fourth term will be found by dividing the fum of the fines by by their difference, which divisor or difference will never consist of more than four or five figures, *viz.* about half the number of figures that are in the divisors mentioned in the preceding article.

20. Again, As the difference of the cosines :
to the sum of the cosines ::
so tangent of half their difference :
to tangent of half their sum.

And thus the cotangents will be found by dividing the sum of the cosines of two arcs, differing by 2, by their small difference.

21. Also the secant of an arc is equal to the sum of its tangent and the tangent of half its complement; and the cosecant of an arc is equal to the sum of its cotangent and the tangent of half the arc; or half the sum of the tangent and cotangent is equal to the cosecant of the double arc. From whence the secants and cosecants will be easily made.

22. Thus I have pointed out methods by which the whole tables may be readily constructed. Should any other useful methods or improvements occur to any person, the communication of them to me will be thankfully received. I am now engaged in making some of the computations; and it is hoped, that the facility of them, with the desireableness of the tables, will induce some ingenious lovers of the mathematics to lend their aid in performing some part of the work. Should any such be so inclined, before he begins, I must request he will be pleased to signify his intention to me, that I may point out to him such parts of the work as have not before been performed or undertaken, to prevent the chance of losing his labour by re-computing any parts that may have been already executed by myself or others.

Royal Military Academy,
Woolwich, Aug. 12, 1782.

CHARLES HUTTON.

VII. *On the Means of discovering the Distance, Magnitude, &c. of the Fixed Stars, in consequence of the Diminution of the Velocity of their Light, in case such a Diminution should be found to take place in any of them, and such other Data should be procured from Observations, as would be farther necessary for that Purpose. By the Rev.* John Michell, *B. D. F. R. S. In a Letter to* Henry Cavendish, *Esq. F. R. S. and A. S.*

Read November 27, 1783.

DEAR SIR, Thornhill, May 26, 1783.

THE method, which I mentioned to you when I was last in London, by which it might perhaps be possible to find the distance, magnitude, and weight of some of the fixed stars, by means of the diminution of the velocity of their light, occurred to me soon after I wrote what is mentioned by Dr. PRIESTLEY in his History of Optics, concerning the diminution of the velocity of light in consequence of the attraction of the sun; but the extreme difficulty, and perhaps impossibility, of procuring the other data necessary for this purpose appeared to me to be such objections against the scheme, when I first thought of it, that I gave it then no farther consideration. As some late observations, however, begin to give us a little more chance of procuring some at least of these data, I thought it would not be amiss, that astronomers should be apprized of the method, I propose (which, as far as I know,

 has

has not been ſuggeſted by any one elſe) left, for want of being aware of the uſe, which may be made of them, they ſhould neglect to make the proper obſervations, when in their power; I ſhall therefore beg the favour of you to preſent the following paper on this ſubject to the Royal Society.

I am, &c.

THE very great number of ſtars that have been diſcovered to be double, triple, &c. particularly by Mr. HERSCHEL *, if we apply the doctrine of chances, as I have heretofore done in my "Enquiry into the probable Parallax, &c. of the Fixed "Stars," publiſhed in the Philoſophical Tranſactions for the year 1767, cannot leave a doubt with any one, who is properly aware of the force of thoſe arguments, that by far the greateſt part, if not all of them, are ſyſtems of ſtars ſo near to each other, as probably to be liable to be affected ſenſibly by their mutual gravitation; and it is therefore not unlikely, that the periods of the revolutions of ſome of theſe about their principals (the ſmaller ones being, upon this hypotheſis, to be conſidered as ſatellites to the others) may ſome time or other be diſcovered.

2. Now the apparent diameter of any central body, round which any other body revolves, together with their apparent diſtance from each other, and the periodical time of the revolv-

* See his Catalogue of Stars of this kind, publiſhed in the Philoſophical Tranſactions for the year 1782, which is indeed a moſt valuable preſent to the aſtronomical world. By a happy application of very high magnifyiug powers to his teleſcopes, and by a moſt perſevering induſtry in obſerving, he has made a very wonderful progreſs in this branch of aſtronomy, in which almoſt nothing of any conſequence had been done by any one before him.

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ing body being given, the density of the central body will be given likewise. See Sir ISAAC NEWTON's Prin. b. III. pr. VIII. cor. I.

3. But the density of any central body being given, and the velocity any other body would acquire by falling towards it from an infinite height, or, which is the same thing, the velocity of a comet revolving in a parabolic orbit, at its surface, being given, the quantity of matter, and consequently the real magnitude of the central body, would be given likewise.

4. Let us now suppose the particles of light to be attracted in the same manner as all other bodies with which we are acquainted; that is, by forces bearing the same proportion to their *vis inertiæ*, of which there can be no reasonable doubt, gravitation being, as far as we know, or have any reason to believe, an universal law of nature. Upon this supposition then, if any one of the fixed stars, whose density was known by the above-mentioned means, should be large enough sensibly to affect the velocity of the light issuing from it, we should have the means of knowing its real magnitude, &c.

5. It has been demonstrated by Sir ISAAC NEWTON, in the 39th proposition of the first book of his Principia, that if a right line be drawn, in the direction of which a body is urged by any forces whatsoever, and there be erected at right angles to that line perpendiculars every where proportional to the forces at the points, at which they are erected respectively, the velocity acquired by a body beginning to move from rest, in consequence of being so urged, will always be proportional to the square root of the area described by the aforesaid perpendiculars. And hence,

6. If such a body, instead of beginning to move from rest, had already some velocity in the direction of the same line, when

when it began to be urged by the aforesaid forces, its velocity would then be always proportional to the square root of the sum or difference of the aforesaid area, and another area, whose square root would be proportional to the velocity which the body had before it began to be so urged; that is, to the square root of the sum of those areas, if the motion acquired was in the same direction as the former motion, and the square root of the difference, if it was in a contrary direction. See cor. 2. to the abovesaid proposition.

7. In order to find, by the foregoing proposition, the velocity which a body would acquire by falling towards any other central body, according to the common law of gravity, let C in the figure (tab. III.) represent the centre of the central body, towards which the falling body is urged, and let CA be a line drawn from the point C, extending infinitely towards A. If then the line RD be supposed to represent the force, by which the falling body would be urged at any point D, the velocity which it would have acquired by falling from an infinite height to the place D would be the same as that which it would acquire by falling from D to C with the force RD, the area of the infinitely extended hyperbolic space ADRB, where RD is always inversely proportional to the square of DC, being equal to the rectangle RC contained between the lines RD and CD. From hence we may draw the following corollaries.

8. Cor. 1. The central body DEF remaining the same, and consequently the forces at the same distances remaining the same likewise, the areas of the rectangles RC, *r*C will always be inversely as the distances of the points D, *d* from C, their sides RD, *rd* being inversely in the duplicate ratio of the sides CD, C*d*: and therefore, because the velocity of a body falling from an infinite height towards the point C, is always in the

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sub-duplicate ratio of these rectangles, it will be in the sub-duplicate ratio of the lines CD, C*d* inversely. Accordingly the velocities of comets revolving in parabolic orbits are always in the sub-duplicate ratio of their distances from the sun inversely; and the velocities of the planets, at their mean distances (being always in a given ratio to the velocity of such comets, *viz.* in the sub-duplicate ratio of 1 to 2) must necessarily observe the same law likewise.

9. Cor. 2. The magnitude of the central body remaining the same, the velocity of a body falling towards it from an infinite height will always be, at the same distance from the point C, taken any where without the central body, in the sub-duplicate ratio of its density; for in this case the distance C*d* will remain the same, the line *rd* only being increased or diminished in the proportion of the density, and the rectangle *r*C consequently increased or diminished in the same proportion.

10. Cor. 3. The density of the central body remaining the same, the velocity of a body falling towards it from an infinite height will always be as its semi-diameter, when it arrives at the same proportional distance from the point C; for the weights, at the surfaces of different spheres of the same density are as their respective semi-diameters; and therefore the sides RD and CD, or any other sides *rd* and C*d*, which are in a given ratio to those semi-diameters, being both increased or diminished in the same proportion, the rectangles RC or *r*C will be increased or diminished in the duplicate ratio of the semi-diameter CD, and consequently the velocity in the simple ratio of CD.

11. Cor. 4. If the velocity of a body falling from an infinite height towards different central bodies is the same, when it arrives at their surfaces, the density of those central bodies must be

in the duplicate ratio of their semi-diameters inversely; for by the last cor. the density of the central body remaining the same, the rectangle RC will be in the duplicate ratio of CD; in order therefore that the rectangle RC may always remain the same, the line RD must be inversely, as CD, and consequently the density inversely, as the square of CD.

12. Cor. 5. Hence the quantity of matter contained in those bodies must be in the simple ratio of their semi-diameters directly; for the quantity of matter being always in a ratio compounded of the simple ratio of the density, and the triplicate ratio of their semi-diameters, if the density is in the inverse duplicate ratio of the semi-diameters, this will become the direct triplicate and inverse duplicate, that is, when the two are compounded together, the simple ratio of the semi-diameters.

13. The velocity a body would acquire by falling from an infinite height towards the sun, when it arrived at his surface, being, as has been said before in article 3d, the same with that of a comet revolving in a parabolic orbit in the same place, would be about 20,72 times greater than that of the earth in its orbit at its mean distance from the sun; for the mean distance of the earth from the sun, being about 214,64 of the sun's semidiameters, the velocity of such a comet would be greater at that distance than at the distance of the earth from the sun, in the sub-duplicate ratio of 214,64 to 1, and the velocity of the comet being likewise greater than that of planets, at their mean distances, in the sub-duplicate ratio of 2 to 1; these, when taken together, will make the sub-duplicate ratio of 429,28 to 1, and the square root of 429,28 is 20,72, very nearly.

14. The

14. The same result would have been obtained by taking the line RD proportional to the force of gravity at the sun's surface, and DC equal to his semi-diameter, and from thence computing a velocity, which should be proportional to the square root of the area RC when compared with the square root of another area, one of whose sides should be proportional to the force of gravity at the surface of the earth; and the other should be, for instance, equal to 16 feet, 1 inch, the space a body would fall through in one second of time, in which case it would acquire a velocity of 32 feet, 2 inches per second. The velocity thus found compared with the velocity of the earth in its orbit, when computed from the same elements, necessarily gives the same result. I have made use of this latter method of computation upon a former occasion, as may be seen in Dr. PRIESTLEY's History of Optics, p. 787, &c. but I have rather chosen to take the velocity from that of a comet, in the article above, on account of its greater simplicity, and its more immediate connexion with the subject of this paper.

15. The velocity of light, exceeding that of the earth in its orbit, when at its mean distance from the sun, in the proportion of about 10.310 to 1, if we divide 10.310 by 20,72, the quotient 497, in round numbers, will express the number of times, which the velocity of light exceeds the velocity a body could acquire by falling from an infinite height towards the sun, when it arrived at his surface; and an area whose square root should exceed the square root of the area RC, where RD is supposed to represent the force of gravity at the surface of the sun, and CD is equal to his semi-diameter, in the same proportion, must consequently exceed the area RC in the proportion of 247.009, the square of 497 to 1.

 16. Hence

16. Hence, according to article 10, if the semi-diameter of a sphære of the same density with the sun were to exceed that of the sun in the proportion of 500 to 1, a body falling from an infinite height towards it, would have acquired at its surface a greater velocity than that of light, and consequently, supposing light to be attracted by the same force in proportion to its vis inertiæ, with other bodies, all light emitted from such a body would be made to return towards it, by its own proper gravity.

17. But if the semi-diameter of a sphære, of the same density with the sun, was of any other size less than 497 times that of the sun, though the velocity of the light emitted from such a body, would never be wholly destroyed, yet would it always suffer some diminution, more or less, according to the magnitude of the said sphære; and the quantity of this diminution may be easily found in the following manner: Suppose S to represent the semi-diameter of the sun, and aS to represent the semi-diameter of the proposed sphære; then, as appears from what has been shewn before, the square root of the difference between the square of 497 S and the square of aS will be always proportional to the ultimately remaining velocity, after it has suffered all the diminution, it can possibly suffer from this cause; and consequently the difference between the whole velocity of light, and the remaining velocity, as found above, will be the diminution of its velocity. And hence the diminution of the velocity of light emitted from the sun, on account of it's gravitation towards that body, will be somewhat less than a 494.000dth part of the velocity which it would have had if no such diminution had taken place; for the square of 497 being 247.009, and the square of 1 being 1, the diminution of the velocity will be the difference between

the square root of 247.009, and the square root of 247.008, which amounts, as above, to somewhat less than one 494.000th part of the whole quantity.

18. The same effects would likewise take place, according to article 11, if the semi-diameters were different from those mentioned in the two last articles, provided the density was greater or less in the duplicate ratio of those semi-diameters inversely.

19. The better to illustrate this matter, it may not be amiss to take a particular example. Let us suppose then, that it should appear from observations made upon some one of those double stars above alluded to, that one of the two performed its revolution round the other in 64 years, and that the central one was of the same density with the sun, which it must be, if its apparent diameter, when seen from the other body, was the same as the apparent diameter of the sun would be if seen from a planet revolving round him in the same period: let us further suppose, that the velocity of the light of the central body was found to be less than that of the sun, or other stars whose magnitude was not sufficient to affect it sensibly, in the proportion of 19 to 20. In this case then, according to article 17, the square root of 247.009 SS must be to the square root of the difference between 247.009 SS and aaSS as 20 to 19. But the squares of 20 and 19 being 400 and 361, the quantity 247.009 SS must therefore be to the difference between this quantity and aaSS in the same proportion, that is as 247.009 to 222.925,62; and aaSS must consequently be equal to 24.083,38 SS, whose square root 155,2 S nearly, or, in round numbers, 155 times the diameter of the sun, will be the diameter of the central star sought.

20. As the ſquares of the periodical times of bodies, revolving round a central body, are always proportional to the cubes of their mean diſtances, the diſtance of the two bodies from each other muſt therefore, upon the foregoing ſuppoſitions, be ſixteen times greater in proportion to the diameter of the central body, than the diſtance of the earth from the ſun in proportion to his diameter; and that diameter being already found to be alſo greater than that of the ſun in the proportion of 155,2 to 1, this diſtance will conſequently be greater than that of the earth and ſun from each other in the proportion of 16 times 155,2, that is 2483,2 to 1.

21. Let us farther ſuppoſe, that from the obſervations, the greateſt diſtance of the two ſtars in queſtion appeared to be only one ſecond; we muſt then multiply the number 2483,2 by 206.264,8, the number of ſeconds in the radius of a circle, and the product 512.196.750 will ſhew the number of times which ſuch a ſtar's diſtance from us muſt exceed that of the ſun. The quantity of matter contained in ſuch a ſtar would be $\overline{155,2}^{3}$ or 3.738.308 times as much as that contained in the ſun; its light, ſuppoſing the ſun's light to take up 8′. 7″. in coming to the earth, would, with its common velocity, require 7.900 years to arrive at us, and 395 years more on account of the diminution of that velocity; and ſuppoſing ſuch a ſtar to be equally luminous with the ſun, it would ſtill be very ſufficiently viſible, I apprehend, to the naked eye, notwithſtanding its immenſe diſtance.

22. In the elements which I have employed in the above computations, I have ſuppoſed the diameter of the central ſtar to have been obſerved, in order to aſcertain its denſity, which cannot be known without it; but the diameter of ſuch a ſtar is

much too small to be observed by any telescopes yet existing, or any that it is probably in the power of human abilities to make; for the apparent diameter of the central star, if of the same density with the sun, when seen from another body, which would revolve round it in 64 years, would be only the 1717th part of the distance of those bodies from each other, as will appear from multiplying 107,32, the number of times the sun's diameter is contained in his distance from the earth, by 16, the greater proportional distance of the revolving body, corresponding to 64 years instead of 1. Now the 1717th part of a second must be magnified 309.060 times in order to give it an apparent diameter of three minutes; and three minutes, if the telescopes were mathematically perfect, and there was no want of distinctness in the air, would be but a very small matter to judge of*.

23. But

* In Mr. Herschel's Observations upon the Fixed Stars abovementioned, almost all of them are represented as appearing with a well-defined round disc. That this is not the real disc, but only an optical appearance, occasioned perhaps by the constitution of the eye, when the pencil, by which objects are seen, is so exceedingly small as those which he employed upon this occasion, is very manifest, from the observations themselves, of which indeed Mr. Herschel seems to be himself sufficiently aware: if it were not so, the intensity of the light of these stars must either be exceedingly inferior indeed to that of the sun, or they must be immensely larger, otherwise they must have a very sensible parallax; for the sun, if removed to 10.000.000 times his present distance, would still, I apprehend, be of about the brightness of the stars of the sixth magnitude; in which case he must be magnified 1.000.000 times to make his apparent disc of any sensible magnitude; or, on the other hand, if he was only removed to a thousandth part of that distance, then he must be less luminous in the proportion of 1.000.000 to 1, to make him appear no brighter than a star of the sixth magnitude. Now the sun's diameter being contained nearly 215 times in the diameter of the earth's orbit, the annual parallax therefore of such a body in that case, if it was placed in the pole of the ecliptic,

23. But though there is not the least probability that this element, so essential to be known, in order to determine with precision the exact distance and magnitude of a star, can ever be obtained, where it is in the same circumstances, or nearly the same, with those above supposed, yet the other elements, such as perhaps may be obtained, are sufficient to determine the distance, &c. with a good deal of probability, within some moderate limits; for in whatever ratio the real distance of the two stars may be greater or less than the distance supposed, the density of the central star must be greater or less in the sixth power of that ratio inversely; for the periodic time of the revolving body being given, the quantity of matter contained in the central body must be as the cube of their distance from each other. See Sir I. NEWTON's Prin. b. 3d. pr. 8th. cor 3d. But the quantity of matter in different bodies, at whose surfaces the velocity acquired by falling from an infinite height is the same, must be, according to art. 12, directly as their semi-diameters; the semi-diameters therefore of such bodies must be in the triplicate ratio of the distance of the revolving body; and consequently their densities, by art. 11, being in the inverse duplicate ratio of their semi-diameters, must be in the inverse sextuplicate ratio of the distance of the revolving body. Hence if the real distance should be greater or less than that supposed, in the proportion of two or three to one, the density of the central body must be less or greater, in the first case, in the proportion of 64, or in the latter of 729 to 1.

ecliptic, would be 215 times its apparent diameter; and as the bright star in Lyrâ appeared to Mr. HERSCHEL about a third part of a second in diameter, if this was its real disc, and it was no bigger than the sun, it would consequently have an annual parallax in the pole of the ecliptic of about 72″.

24. There

24. There is alſo another circumſtance, from which perhaps ſome little additional probability might be derived, with regard to the real diſtance of a ſtar, ſuch as that we have ſuppoſed; but upon which however, it muſt be acknowledged, that no great ſtreſs can be laid, unleſs we had ſome better analogy to go upon than we have at preſent. The circumſtance I mean is the greater ſpecific brightneſs which ſuch a ſtar muſt have, in proportion as the real diſtance is leſs than that ſuppoſed, and *vice verſâ*; ſince, in order that the ſtar may appear equally luminous, its ſpecific brightneſs muſt be as the fourth power of its diſtance inverſely; for the diameter of the central ſtar being as the cube of the diſtance between that and the revolving ſtar, and their diſtance from the earth being in the ſimple ratio of their diſtance from each other, the apparent diameter of the central ſtar muſt be as the ſquare of its real diſtance from the earth, and conſequently, the ſurface of a ſphære being as the ſquare of its diameter, the area of the apparent diſc of ſuch a ſtar muſt be as the fourth power of its diſtance from the earth; but in whatever ratio the apparent diſc of the ſtar is greater or leſs, in the ſame ratio inverſely muſt be the intenſity of its light, in order to make it appear equally luminous. Hence, if its real diſtance ſhould be greater or leſs than that ſuppoſed in the proportion of 2 or 3 to 1, the intenſity of its light muſt be leſs or greater, in the firſt caſe, in the proportion of 16, or, in the latter of 81 to 1.

25. According to Monſ. BOUGUER (ſee his Traité d'Optique) the brightneſs of the ſun exceeds that of a wax candle in no leſs a proportion than that of 8000 to 1. If therefore the brightneſs of any of the fixed ſtars ſhould not exceed that of our common candles, which, as being ſomething leſs luminous than

wax,

wax, we will ſuppoſe in round numbers to be only one 10.000dth part as bright as the ſun, ſuch a ſtar would not be viſible at more than an 100dth part of the diſtance, at which it would be viſible, if it was as bright as the ſun. Now becauſe the ſun would ſtill appear, I apprehend, as luminous, as the ſtar Sirius, when removed to 400.000 times his preſent diſtance, ſuch a body, if no brighter than our common candles, would only appear equally luminous with that ſtar at 4000 times the diſtance of the ſun, and we might then begin to be able, with the beſt teleſcopes, to diſtinguiſh ſome ſenſible apparent diameter of it; but the apparent diameters of the ſtars of the leſs magnitudes would ſtill be too ſmall to be diſtinguiſhable even with our beſt teleſcopes, unleſs they were yet a good deal leſs luminous, which may poſſibly however be the caſe with ſome of them; for, though we have indeed very ſlight grounds to go upon with regard to the ſpecific brightneſs of the fixed ſtars compared with that of the ſun at preſent, and can therefore only form very uncertain and random conjectures concerning it, yet from the infinite variety which we find in the works of the creation, it is not unreaſonable to ſuſpect, that very poſſibly ſome of the fixed ſtars may have ſo little natural brightneſs in proportion to their magnitude, as to admit of their diameters having ſome ſenſible apparent ſize, when they ſhall come to be more carefully examined, and with larger and better teleſcopes than have been hitherto in common uſe.

26. With regard to the ſun, we know that his whole ſurface is extremely luminous, a very ſmall and temporary interruption ſometimes from a few ſpots only excepted. This univerſal and exceſſive brightneſs of the whole ſurface is probably owing to an atmoſphære, which being luminous throughout,

and

and in some measure also transparent, the light, proceeding from a considerable depth of it, all arrives at the eye; in the same manner as the light of a great number of candles would do, if they were placed one behind another, and their flames were sufficiently transparent to permit the light of the more distant ones to pass through those that were nearer, without any interruption.

27. How far the same constitution may take place in the fixed stars we don't know; probably however it may do so in many; but there are some appearances with regard to a few of them, which seem to make it probable, that it does not do so universally. Now, if I am right in supposing the light of the sun to proceed from a luminous atmosphære, which must necessarily diffuse itself equally over the whole surface, and I think there can be very little doubt that this is really the case, this constitution cannot well take place in those stars, which are in some degree periodically more and less luminous, such as that in Collo Ceti, &c. It is also not very improbable, that there is some difference from that of the sun, in the constitution of those stars, which have sometimes appeared and sometimes disappeared, of which that in the constellation of Cassiopeia is a notable instance. And if those conjectures are well founded which have been formed by some philosophers concerning stars of these kinds, that they are not wholly luminous, or at least not constantly so, but that all, or by far the greatest part of their surfaces is subject to considerable changes, sometimes becoming luminous, and at other times being extinguished; it is amongst the stars of this sort, that we are most likely to meet with instances of a sensible apparent diameter, their light being much more likely not to be so great in proportion as that of the sun, which, if removed to four hundred thousand times

 his

his present distance would still appear, I apprehend, as bright as Sirius, as I have observed above; whereas it is hardly to be expected, with any telescopes whatsoever, that we should ever be able to distinguish a well defined disc of any body of the same size with the sun at much more than ten thousand times his distance.

28. Hence the greatest distance at which it would be possible to distinguish any sensible apparent diameter of a body as dense as the sun cannot well greatly exceed five hundred times ten thousand, that is, five million times the distance of the sun; for if the diameter of such a body was not less than five hundred times that of the sun, its light, as has been shewn above, in art. 16. could never arrive at us.

29. If there should really exist in nature any bodies, whose density is not less than that of the sun, and whose diameters are more than 500 times the diameter of the sun, since their light could not arrive at us; or if there should exist any other bodies of a somewhat smaller size, which are not naturally luminous; of the existence of bodies under either of these circumstances, we could have no information from sight; yet, if any other luminous bodies should happen to revolve about them, we might still perhaps from the motions of these revolving bodies infer the existence of the central ones with some degree of probability, as this might afford a clue to some of the apparent irregularities of the revolving bodies, which would not be easily explicable on any other hypothesis; but as the consequences of such a supposition are very obvious, and the consideration of them somewhat beside my present purpose, I shall not prosecute them any farther.

30. The

30. The diminution of the velocity of light, in case it should be found to take place in any of the fixed stars, is the principal phænomenon whence it is proposed to discover their distance, &c. Now the means by which we may find what this diminution amounts to, seems to be supplied by the difference which would be occasioned in consequence of it, in the refrangibility of the light, whose velocity should be so diminished. For let us suppose with Sir ISAAC NEWTON (see his Optics, prop. VI. paragr. 4 and 5) that the refraction of light is occasioned by a certain force impelling it towards the refracting medium, an hypothesis which perfectly accounts for all the appearances. Upon this hypothesis the velocity of light in any medium, in whatever direction it falls upon it, will always bear a given ratio to the velocity it had before it fell upon it, and the sines of incidence and refraction will, in consequence of this, bear the same ratio to each other with these velocities inversely. Thus, according to this hypothesis, if the sines of the angles of incidence and refraction, when light passes out of air into glass, are in the ratio of 31 to 20, the velocity of light in the glass must be to its velocity in air in the same proportion of 31 to 20. But because the areas, representing the forces generating these velocities, are as the squares of the velocities, see art. 5. and 6. these areas must be to each other as 961 to 400. And if 400 represents the area which corresponds to the force producing the original velocity of light, 561, the difference between 961 and 400, must represent the area corresponding to the additional force, by which the light was accelerated at the surface of the glass.

31. In art. 19. we supposed, by way of example, the velocity of the light of some particular star to be diminished in the

 ratio

ratio of 19 to 20, and it was there obſerved, that the area repreſenting the remaining force which would be neceſſary to generate the velocity 19, was therefore properly repreſented by $\frac{361}{400}$dth parts of the area, that ſhould repreſent the force that would be neceſſary to generate the whole velocity of light, when undiminiſhed. If then we add 561, the area repreſenting the force by which the light is accelerated at the ſurface of the glaſs, to 361, the area repreſenting the force which would have generated the diminiſhed velocity of the ſtar's light, the ſquare root of 922, their ſum, will repreſent the velocity of the light with the diminiſhed velocity, after it has entered the glaſs. And the ſquare root of 922 being 30,364, the ſines of incidence and refraction of ſuch light out of air into glaſs will conſequently be as 30,364 to 19, or what is equal to it, as 31,96 to 20 inſtead of 31 to 20, the ratio of the ſines of incidence and refraction, when the light enters the glaſs with its velocity undiminiſhed.

32. From hence a priſm, with a ſmall refracting angle, might perhaps be found to be no very inconvenient inſtrument for this purpoſe: for by ſuch a priſm, whoſe refracting angle was of one minute, for inſtance, the light with its velocity undiminiſhed would be turned out of its way 33″, and with the diminiſhed velocity 35″, 88 nearly, the difference between which being almoſt 2″. 53‴, would be the quantity by which the light, whoſe velocity was diminiſhed, would be turned out of its way more than that whoſe velocity was undiminiſhed.

33. Let us now be ſuppoſed to make uſe of ſuch a priſm to look at two ſtars, under the ſame circumſtances as the two ſtars in the example above-mentioned, the central one of which ſhould be large enough to diminiſh the velocity of its light one twentieth part, whilſt the velocity of the light of the other,

which

which was ſuppoſed to revolve about it as a ſatellite, for want of ſufficient magnitude in the body from whence it was emitted, ſhould ſuffer no ſenſible diminution at all. Placing then the line, in which the two faces of the priſm would interſect each other, at right angles to a line joining the two ſtars; if the thinner part of the priſm lay towards the ſame point of the heavens with the central ſtar, whoſe light would be moſt turned out of its way, the apparent diſtance of the ſtars would be increaſed 2″. 53‴ and conſequently become 3″. 53‴ inſtead of 1″. only, the apparent diſtance ſuppoſed above in art. 21. On the contrary, if the priſm ſhould be turned half way round, and its thinner part lye towards the ſame point of the heavens with the revolving ſtar, their diſtance muſt be diminiſhed by a like quantity, and the central ſtar therefore would appear 1″. 53‴ diſtant from the other on the oppoſite ſide of it, having been removed from its place near three times the whole diſtance between them.

34. As a priſm might be made uſe of for this purpoſe, which ſhould have a much larger refracting angle than that we have propoſed, eſpecially if it was conſtructed in the achromatic way, according to Mr. DOLLOND's principles, not only ſuch a diminution, as one part in twenty, might be made ſtill more diſtinguiſhable; but we might probably be able to diſcover conſiderably leſs diminutions in the velocity of light, as perhaps a hundredth, a two-hundredth, a five-hundredth, or even a thouſandth part of the whole, which, according to what has been ſaid above, would be occaſioned by ſphæres, whoſe diameters ſhould be to that of the ſun, provided they were of the ſame denſity, in the ſeveral proportions nearly of 70, 50, 30, and 22 to 1 reſpectively.

35. If ſuch a diminution of the velocity of light, as that above ſuppoſed, ſhould be found really to take place, in conſe-

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quence of its gravitation towards the bodies from whence it is emitted, and there ſhould be ſeveral of the fixed ſtars large enough to make it ſufficiently ſenſible, a ſet of obſervations upon this ſubject might probably give us ſome conſiderable information with regard to many circumſtances of that part of the univerſe, which is viſible to us. The quantity of matter contained in many of the fixed ſtars might from hence be judged of, with a great degree of probability, within ſome moderate limits; for though the exact quantity muſt ſtill depend upon their denſity, yet we muſt ſuppoſe the denſity moſt enormouſly different from that of the ſun, and more ſo, indeed, than one can eaſily conceive to take place in fact, to make the error of the ſuppoſed quantity of matter very wide of the truth, ſince the denſity, as has been ſhewn above in art. 11. and 12. which is neceſſary to produce the ſame diminution in the velocity of light, emitted from different bodies, is as the ſquare of the quantity of matter contained in thoſe bodies inverſely.

36. But though we might poſſibly from hence form ſome reaſonable gueſs at the quantity of matter contained in ſeveral of the fixed ſtars; yet, if they have no luminous ſatellites revolving about them, we ſhall ſtill be at a loſs to form any probable judgment of their diſtance, unleſs we had ſome analogy to go upon for their ſpecific brightneſs, or had ſome other means of diſcovering it; there is, however, a caſe that may poſſibly occur, which may tend to throw ſome light upon this matter.

37. I have ſhewn in my Enquiry into the probable Parallax, &c. of the Fixed Stars, publiſhed in the Philoſophical Tranſactions for the year 1767, the extremely great probability there is, that many of the fixed ſtars are collected together into groups; and that the Pleiades in particular conſtitute one of

theſe

these groups. Now of the stars which we there see collected together, it is highly probable, as I have observed in that paper, that there is not one in a hundred which does not belong to the group itself; and by far the greatest part, therefore, according to the same idea, must lye within a sphære, a great circle of which is of the same size with a circle, which appears to us to include the whole group. If we suppose, therefore, this circle to be about 2°. in diameter, and consequently only about a thirtieth part of the distance at which it is seen, we may conclude, with the highest degree of probability, that by far the greatest part of these stars do not differ in their distances from the sun by more than about one part in thirty, and from thence deduce a sort of scale of the proportion of the light which is produced by different stars of the same group or system in the Pleiades at least; and, by a somewhat probable analogy, we may do the same in other systems likewise. But having yet no means of knowing their real distance, or specific brightness, when compared either with the sun or with one another, we shall still want something more to form a farther judgment from.

38. If, however, it should be found, that amongst the Pleiades, or any other like system, there are some stars that are double, triple, &c. of which one is a larger central body, with one or more satellites revolving about it, and the central body should likewise be found to diminish the velocity of its light; and more especially, if there should be several such instances met with in the same system; we should then begin to have a kind of measure both of the distance of such a system of stars from the earth, and of their mutual distances from each other. And if several instances of this kind should occur in different groups or systems of stars, we might also, perhaps, begin to form

form some probable conjectures concerning the specific density and brightness of the stars themselves, especially if there should be found any general analogy between the quantity of the diminution of the light and the distance of the system deduced from it; as, for instance, if those stars, which had the greatest effect in diminishing the velocity of light should in general give a greater distance to the system, when supposed to be of the same density with the sun, we might then naturally conclude from thence, that they are less in bulk, and of greater specific density, than those stars which diminish the velocity of light less, and *vice versâ*. In like manner, if the larger stars were to give us in general a greater or less quantity of light in proportion to their bulk, this would give us a kind of analogy, from whence we might perhaps form some judgment of the specific brightness of the stars in general; but, at all adventures, we should have a pretty tolerable measure of the comparative brightness of the sun and those stars, upon which such observations should be made, if the result of them should turn out agreeable to the ideas above explained.

39. Though it is not improbable, that a few years may inform us, that some of the great number of double, triple stars, &c. which have been observed by Mr. HERSCHEL, are systems of bodies revolving about each other, especially if a few more observers, equally ingenious and industrious with himself could be found to second his labours; yet the very great distance at which it is not unlikely many of the secondary stars may be placed from their principals, and the consequently very long periods of their revolutions *, leave very little room to hope that

* If the sun, when removed to 10.000.000 times his present distance, would still appear as bright as a star of the sixth magnitude, which I apprehend to be pretty

that any very great progreſs can be made in this ſubject for many years, or perhaps ſome ages to come; the above outlines, therefore, of the uſe that may be made of the obſervations upon the double ſtars, &c. provided the particles of light ſhould be ſubject to the ſame law of gravitation with other bodies, as in all probability they are, and provided alſo that ſome of the ſtars ſhould be large enough ſenſibly to diminiſh their velocity, will, I hope, be an inducement to thoſe, who may have it in their power, to make theſe obſervations for the benefit of future generations at leaſt, how little advantage ſoever we may expect from them ourſelves; and yet very poſſibly ſome obſervations of this ſort, and ſuch as may be made in a few years, may not only be ſufficient to do ſomething, even at preſent, but alſo to ſhew, that much more may be done hereafter, when theſe obſervations ſhall become more numerous, and have been continued for a longer period of years.

pretty near the truth, any ſatellite revolving round ſuch a ſtar, provided the ſtar was not either of leſs ſpecific brightneſs, or of greater denſity than the ſun, muſt, if it appeared at its greateſt elongation, at the diſtance of one ſecond only from its principal, be between three and four hundred years in performing one revolution; and the time of the revolution of the very ſmall ſtar near α Lyræ, if it is a ſatellite to this latter, and its principal is of the ſame ſpecific brightneſs and denſity with the ſun, could hardly be leſs than eight hundred years, though 37″ the diſtance at which it is placed from it, according to Mr. HERSCHEL's obſervations, ſhould happen to be its greateſt diſtance. Theſe periodical times, however, are computed from the above diſtances, upon the ſuppoſition of the ſtar, that revolves as a ſatellite, being very much ſmaller than the central one, ſo as not to diſturb its place ſenſibly; for if the two ſtars ſhould contain equal, or nearly equal, quantities of matter, the periodical times might be ſomewhat leſs, on account of their revolving about their common centre of gravity, in circles of little more than half as great a diameter as that in which the ſatellite muſt revolve upon the other ſuppoſition.

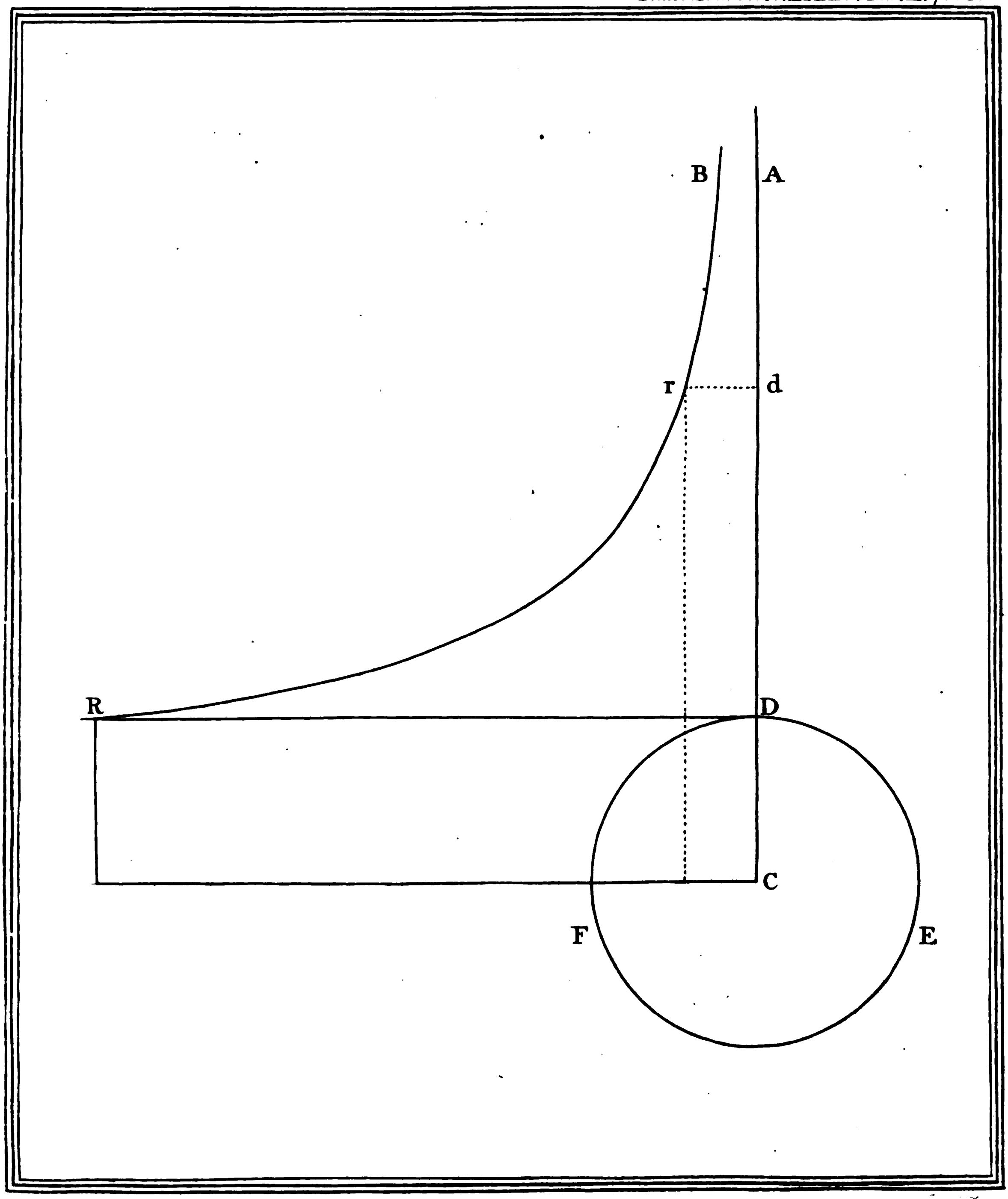
VIII. *A Meteorological Journal for the Year* 1782, *kept at* Minehead, *in* Somersetshire. *By Mr.* John Atkins; *communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read January 15, 1784.

SIR, Minehead, March 27, 1783.

THE numberless philosophical discoveries and meteorological journals which I find have been addressed to you, as being a gentleman whose great abilities have raised you to the highest pitch of grandeur in the philosophical world, and which, I find, have been treated with the greatest candour and respect, and published under your direction for the improvement of scientific knowledge, make me presume (though an unknown, and even unheard of, individual) to direct this journal to you, not boasting, but rather doubting, of its being worthy of your reception; but my having found so great a difference between the last year and several preceding years, in the variations of the atmosphere, both barometrical and thermometrical, induced me to communicate it to other observers through your approbation.

I am, &c.



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THE inſtruments are kept at a houſe about thirty feet above high water in the Briſtol Channel. The barometer is made after DE LUC's method; and to obſerve the moſt minute alteration I have divided it into the one-ſixteenth of a line, or 192 parts in an inch. The thermometer is a mercurial one, graduated according to FAHRENHEIT's ſcale, as being the moſt univerſal, though, I think, a partial one, and placed in the open air in a northern aſpect. An hygrometer I have likewiſe kept in the open air; but being an inſtrument that does not admit of, as I ever heard of, a certain baſis, whereon to fix the fundamental point between the greateſt moiſture and greateſt drought, and therefore of little uſe to diſtant obſervers, I have omitted theſe obſervations. For the eaſe of correſpondent obſervers, I have drawn two columns of the barometer; the firſt divided into 192d parts of an inch; the ſecond into 100th parts. The figures in the column of winds denote its ſtrength from 0 to 90 degrees of a quadrant. And the moſt prevailing winds are from north to weſt, being generally in thoſe directions two-thirds of the year, occaſioned, as I imagine, by the indraught of the Briſtol Channel. The barometer this year has taken a greater range than ever I found theſe ſeveral years, being 2.44 inches. The thermometer likewiſe from 21° to 81°; and the three rainy months of October, November, and December, there fell very little more rain than fell in the month of Auguſt alone, which is very uncommon in this part of the kingdom. On the ninth of February an odd phænomenon appeared to me about 10 miles from hence, on my journey to Tiverton; I obſerved an halo, exactly ſimilar to that of the ſun, the center of the arch about 15° high, and both ends terminated in a field of ſnow; but as rainbows are ſeen only with the ſun behind one's back, this, on the contrary, was between me and the ſun.

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	8	Hard rain.	SW 25		29. 96	29.50	50	
1	12				29.100	29.52	52	
	9		W 20		29.104	29.54	50	0.55
	8	Rain.	W 30		29. 16	29. 9	48	
2	12				29.120	29.63	50	
	9				29.128	29.67	50	0.16
	8	Fair.	WNW 35		29.120	29.63	48	
3	12	Some showers.			29.140	29.73	50	
	9				29.152	29.79	48	
	8	Foggy, rain.	WNW 10		29.160	29.84	50	
4	12				29.140	29.73	54	
	9				29.128	29.67	49	0.17
	8	Showers.	WNW 27		29.132	29.69	50	
5	12				29.150	29.78	51	
	9				29.170	29.89	50	0.25
	8	Fair.	WNW 22		29.184	29.94	46	
6	12	Cloudy.			30. 0	30. 0	50	
	9				30. 0	30. 0	49	
	8	Hard showers.	NW 40		29.112	29.59	45	
7	12				29.120	29.63	50	
	9	Stormy, hail showers.	70		29.128	29.67	42	
	8	Fair.	WSW 20		29.158	29.83	44	
8	12	Showers.			29.150	29.78	50	
	9				29.128	29.67	48	0.50
	8	Rain.	S 40		29. 0	29. 0	50	
9	12		WNW 50		28.168	28.88	40	
	9	Very stormy.	NW 80		29. 40	29.21	38	

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
10	8	Very ſtormy.	NWbyW 80		29.128	29.67	37	
	12				29.180	29.94	40	0.49
	9	Freezing hard.			30. 48	30.25	35	
11	8	Hard froſt and fair.	NW 0		30. 48	30.25	29	
	12				30. 48	30.25	32	
	9				30. 48	30.25	35	
12	8	Foggy rain.	NW 0		30. 40	30.21	45	
	11				30. 40	30.21	48	
	9				30. 64	30.33	49	
13	8	Cloudy.	WNW 0		30. 70	30.37	47	
	12	Foggy rain.			30. 70	30.37	51	
	9				30. 67	30.35	50	
14	8	Fair.	S by W 15		30. 56	30.29	45	
	12				30. 40	30.21	50	
	9				30. 24	30.13	41	
15	8	Froſty, but foggy.	NNW 35		30. 20	30.11	40	
	12				30. 28	30.15	44	
	9				30. 16	30. 9	44	
16	8	Small rain.	WNW 10		29.128	29.67	47	
	12				29.106	29.55	50	
	9	Stormy, hail ſhowers.	NW 75		29. 80	29.42	42	
17	8	Stormy, but dry.	NW 70		29. 88	29.46	39	
	12				29. 96	29.50	43	
	9				29.112	29.59	45	
18	8	Fair.	SW 20		29.118	29.61	48	
	12	Some ſmall rain.			29.126	29.66	48	
	9				29.144	29.75	45	

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	8	Cloudy.	WNW 15		29.140	29.73	45	
	12	Fair and mild.			29.140	29.73	51	
	9	Some ſhowers.			29.130	29.68	46	
20	8	Very hard rain.	W 60		29. 90	29.47	50	
	12				29. 80	29.42	52	
	9				29. 86	29.45	48	0.66
21	8	Froſt.	WNW 10		29.140	29.73	38	
	12	Some rain, but ſnow on the hills.			29.168	29.88	45	
	9				30. 0	30. 0	48	
22	8	Flying clouds, with ſome ſhowers.	W 60		29.180	29.94	51	
	12				29 180	29.94	51	
	9	Cloudy and mild.			29.180	29.94	50	
23	8	Fair and mild.	W 60		29.184	29.96	50	
	12				29.180	29.94	51	
	9				29.176	29.92	50	
24	8	Fair, with hard winds, but very mild.	W 70		29.120	29.63	50	
	12				29.112	29.59	55	
	9	Calm, with rain.	NW 10		29.116	29.61	49	
25	8	Stormy, hail ſhowers.	NW 75		29 120	29.63	36	
	12				29.130	29.68	39	
	9	Rain.	WNW 60		29.100	29.52	44	
26	8	Fair.	NW 60		29 116	29.61	39	
	12	Hail ſhowers.			29.110	29.57	41	
	9	Stormy, hard rain.	70		29.100	29.52	40	
27	8	Stormy, hail and rain.	N by W 60		29. 48	29.25	38	0.93
	12				29. 40	29.21	42	
	9		75		29. 48	29.25	40	

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Therm.	Rain.
					Inches.	Inches.	°	Inch.
28	8	Stormy, showers.	NWbyW 80		29. 32	29.17	40	
	12				29. 16	29. 9	45	
	9	Calmer, with hail in the night.	20		29. 16	29. 9	40	0.23
29	8	Fair and cold.	W 20		28.176	28.92	33	
	12	Fair.	NW		28.160	28.84	41	
	5		SW		28.154	28.81		
	9	Cloudy.	NE	SW	29. 0	29. 0	37	
30	8	Cloudy, but snow on the distant hills.	NE 20		29. 64	29.34	37	
	12	Fair.			29.112	29.59	40	
	9	Freezing hard.			29.168	29.88	35	
31	8	Frost.	NE 15		30. 16	30. 9	33	
	12				30. 16	30. 9	37	
	9				30. 16	30. 9	32	
						Total rain		3.94

February

February 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	7	Very hard froſt.	W 0		30. 0	30. 0	27	
	12	Fair.	SW 20		29.180	29.94	34	
	9	Sleet.			29.160	29.84	37	
2	7	Rain.	SW 30		29.130	29.68	40	
	12				29.120	29.63	42	
	9	Fair.			29.112	29.59	38	
3	7	Froſt.	S by E 20	SW	29. 96	29.50	35	
	12	Sleet.	SW		29. 84	29.44	37	
	9				29. 80	29.42	37	
4	7	Fair.	W 0		29. 80	29.42	37	
	12				29. 88	29.46	38	
	9	Snow on the hills.	NE 20		29. 60	29.31	37	
5	7	Cloudy day.	NE 40		29. 12	29. 6	33	0.63
	12				29. 20	29.11	35	
	9				29. 96	29.50	35	
6	7	Fair, froſt.	NE 20		29.140	29.73	35	
	12				29.160	29.84	37	
	9		W 0		29.168	29.88	37	
7	7	Fair, hard froſt.	W 0		29.170	29.89	29	
	12			ENE	29.172	29.90	35	
	9		NE 10		29.180	29.94	35	
8	7	Fair, froſt.	NE 0		29.180	29.94	34	
	12				29.180	29.94	37	
	9				29.186	29.97	33	
9	7	Fair, very hard froſt; an extraordinary halo.	NE 10		29.182	29.95	27	
	12	Cloudy.			29.180	29.94	33	
	9				29.180	29.94	33	

February

February 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
	7	Cloudy, hard frost.	NE 15		29.180	29.94	29	
10	12	Fair.			29.180	29.94	35	
	9				29.180	29.94	36	
	7	Fair, hard frost.	ENE 20		29.184	29.96	30	
11	12				29.168	29.88	35	
	9	Cloudy.	55		29.160	29.84	34	
	7	Cloudy, hard frost.	E 50		29.170	29.89	31	
12	12	Fair.			29.186	29.97	33	
	9	Cloudy.			30. 0	30. 0	29	
	7	Fair, very hard frost.	WNW 10	NE	30. 4	30. 2	27	
13	12	Cloudy.	NW		29.184	29.96	38	
	9		N	NNE	29.184	29.96	36	
	7	Cloudy, frost.	E 20		29.176	29.92	36	
14	12	Thawing.			29.180	29.94	40	
	9				29.184	29.94	37	
	7	Cloudy.	E 10		29.184	29.96	37	
15	12	Sleet.			30. 0	30. 0	38	
	9	Fair, freezing.	60		30. 30	30.16	32	
	7	Excessive frost.	E 30		30. 40	30.21	24	
16	12	Fair.			30. 40	30.21	28	
	9	Cloudy.			30. 40	30.21	30	
	7	Cloudy and thawing.	ENE 15		30. 50	30.26	34	
17	12				30. 50	30.26	36	
	9	Fair, freezing.			30. 50	30.26	32	
	7	Fair, excessive frost.	W 5	E	3. 64	30.33	21	
18	12	Much milder.	S		30 64	30.33	37	
	9	Cloudy.	SE		30. 60	30.31	37	

February 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	7	Cloudy, a little froſt.	E 10		30. 60	30.31	35	
	12				30. 57	30.30	37	
	9				30. 48	30.25	37	
20	7	Cloudy.	W 10		30. 40	30.21	34	
	12				30. 40	30.21	39	
	9		WNW		29.180	29.94	37	
21	7	Fair.	S by W 0		29.160	29.84	37	
	12				29.144	29.75	41	
	9	Rain.			29. 32	29.17	40	
22	7	Fair.	W 30		29. 48	29.25	41	
	12				29. 40	29.21	44	
	9	Cloudy.			29. 32	29.17	40	
23	7	Hard rain and wind.	S 80		29. 0	29. 0	46	
	12				28.168	28.88	49	
	4				28.160	28.84		
	9		WNW 40		28.170	28.89	46	
24	7	Fair.	WNW 30		29. 30	29.16	45	0.67
	12				29.100	29.52	47	
	9	Rain in the night.			29.168	29.88	40	
25	7	Fair.	W by N 35		29.172	29.90	45	
	12				30. 12	30. 6	48	
	9				30. 0	30. 0	47	
26	7	Small ſhowers.	W 75		29.186	29.97	48	
	12	Fair and mild.			30. 0	30. 0	61	0.24
	9				30. 0	30. 0	57	
27	7	Cloudy.	S 5		29.180	29.94	50	
	12				29.180	29.94	52	
	9	Fair.			29.184	29.96	45	

February

February 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	7	Fair.	W 25		30. 40	30.21	45	
28	12				30. 48	30.25	48	
	9				30. 32	30.17	44	
						Total rain		1.54

March 1782

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	6	Fair.	W 10		30. 0	30. 0	42	
	12				29.186	29.97	45	
	9		40		29.174	29.91	40	
2	6	Fair.	WNW 15		29.170	29.89	38	
	12				29.176	29.92	48	
	9	Cloudy.	NW		29.180	29 94	40	
3	6	Fair, white frost.	NW 0		29.186	29.97	39	
	12				30. 0	30. 0	47	
	9		20		30. 16	30. 9	40	
4	6	Hard frost.	W 0		30. 30	30.16	32	
	12				30. 16	30. 9	37	
	9		20		30. 16	30. 9	37	
5	6	Little frost.	WSW 30		29.178	29.93	38	
	12				29.170	29.89	45	
	9				29.172	29.90	40	
6	6	Fair.	WNW 10		29.168	29.88	40	
	12				29.168	29.88	50	
	9				29.164	29.86	47	
7	6	Fair.	W 20		29.158	29.83	40	
	12				29.150	29.78	44	
	9	Showers.	75		29.144	29.75	48	
8	6	Sleet, but snow on the hills.	SSW 70		29.144	29.75	39	
	12		50		29. 32	29.17	40	
	9	Very great storm of	WNW		28.112	28.59		
	10	wind and rain.	90		28.120	28.63	37	
9	6	Fair.	WbyN 40		29 48	29.25	39	0.82
	12	Showers.			29 60	29 31	45	
	9				29.132	29.[illegible]	[illegible]	

March 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches	Inches	°	Inch.
	6	Showery day.	SE	20		29.144	29.75	43	
10	12		SW	25		29.140	29.73	50	
	9		W			29.140	29.73	48	
	6	Fair.	W	20		29.138	29.72	45	
11	12			40		29. 96	29.50	53	
	9	Very ftormy.		85		29. 48	29.25	48	
	6	Stormy.	NW.	75		29. 92	29.48	42	
12	12	Hail fhowers.				29.140	29.73	45	
	9	Fair.		80		29 184	29.96	39	
	6	Fair and froft.	NE	20	W	30. 12	30. 6	34	
13	12					30. 20	30.11	46	
	9		WSW	15		30. 10	30. 5	40	
	6	Cloudy, froft.	ENE	20		30. 8	30. 4	34	
14	12	Fair.				30. 16	30. 9	44	
	9					30. 16	30. 9	37	
	6	Fair, froft.	WNW	25		30. 24	30.13	33	
15	12					30. 26	30.14	40	
	9	Cloudy.				30. 20	30.11	40	
	6	Fair.	NNE	30		30. 17	30. 9	39	
16	12					30. 17	30. 9	41	
	9		W			30. 10	30. 5	40	
	6	Cloudy.	NNW	35		30. 0	30. 0	38	0.10
17	12	Fair.				29.188	29.98	45	
	9					29.190	29.99	40	
	6	Hazy.	WNW	10		30. 0	30. 0	37	
18	12	Somewhat fair.				29.186	29.97	45	
	9	Cloudy.				29.180	29.94	41	

March 1782

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Cloudy.	W 25		29.150	29.79	45	
	12	Some small rain.			29 136	29.71	50	
	9				29.120	29.63	42	
20	6	Hazy.	SW 15		29.140	29.73	39	
	12	Fair.			29.152	29.80	45	
	9				29.144	29.75	41	
21	6	Sleet, but snow on the distant hills.	SE 45		29 128	29.67	37	
	12				29. 96	29.50	35	
	9				29. 40	29.21	35	0.40
22	6	Ditto.	E 60		28.160	28.84	33	
	12				28.170	28.89	34	
	9		ENE		29. 0	29. 0	32	0.50
23	6	Excessive frost, with a little	E 10		29. 20	29.11	27	
	12	snow here, but five or six	N		29. 32	29.17	35	
	9	feet deep in the country.	NW 30		29. 88	29.46	37	
24	6	Some showers.	NW 50		29.100	29.52	40	
	12				29. 90	29.47	43	
	9				29.100	29.52	40	0.33
25	6	Cloudy.	W 30		29.108	29.57	40	
	12	Fair.			29.112	29.59	47	
	9				29.128	29.67	39	
26	6	Showery.	WNW 25		29.144	29.75	45	
	12				29.140	29.73	50	
	9				29.140	29.73	48	
27	6	Showery.	W 32		29.128	29.67	48	
	12				29.120	29.63	51	0.17
	9	Hard rain all night.	SW 50		29. 88	29.46	48	

March 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
28	6	Hard rain.	SSW	50		29. 32	29.17	51	
	12	Fair and mild.		30		29. 90	29.47	55	
	9	Cloudy.				29. 72	29.37	50	1.25
29	6	Hard rain.	WSW	60		29. 48	29.25	47	
	12	Fair.				29. 60	29 31	50	
	9					29. 78	29.41	49	
30	6	Fair.	WNW	50		29. 32	29.17	48	
	12					29. 64	29.34	55	
	9		NW			29. 92	29.48	50	
31	6	Very stormy.	W	60		29. 38	29.20	47	
	12	Showers.				29. 56	29.29	50	
	9			80		29. 46	29.24	46	0.34
							Total rain		3.91

April

April 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Therm.	Rain.
					Inches.	Inches.		Inch.
1	6	Hard rain showers; the bar. fell till 8 in the evening: after which it rose 1 line by 10; after which the wind rose to a great storm, but no rain.	SE 60		29. 0	29 0	42	
	12		SSW 50		28.116	28.61		
	8		W 30		28. 96	28.50	50	
	9				28. 44	28.23		
	10		WNW		28. 60	28 31	43	
2	6		NW 90		28 140	28.73	40	
	12				28.156	28.82	41	
	9		70		28.172	28.90	40	0.33
3	6	Some showers.	NW 50		29. 0	29. 0	38	
	12	Fair.			29. 16	29. 9	45	
	9				29. 48	29.25	37	
4	6	Cloudy.	WNW 40		29. 76	29.40	40	
	12	Fair.			29. 80	29.46	45	
	9		10		29. 75	29.39	38	
5	6	Hard hail showers.	E 40		29. 60	29 31	41	
	12	Fair.	NW 25	NEE	29. 72	29.38	41	
	9				29. 80	29.42	45	
6	6	Showers.	WNW 30		29. 96	29.50	40	
	12		E NW		29.108	29.57	44	
	9	Hail showers.	NE 40		29.112	29.59	40	
7	6	Cloudy.	NE 50		29.128	29.67	40	
	12				29.156	29.82	45	
	9				30. 0	30. 0	41	
8	6	Cloudy.	NE 30		30. 0	30. 0	41	
	12	Fair.			30. 0	30. 0	45	
	9				30. 0	30. 0	40	
9	6	Cloudy.	NE 20		29.184	29 96	40	
	12	Fair.			29.186	29.97	44	
	9				29.186	29.97	40	

April 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
	6	Cloudy.	NE	15		29.144	29.75	41	0.14
10	12	Fair.				29.123	29.63	48	
	9	Cloudy.	WNW	20		29. 96	29.50	42	
	6	Rain.	NW	30		29. 80	29.42	39	
11	12	Cloudy.				29. 80	29.42	44	
	9					29. 62	29.32	38	
	6	Fair, froſt.	WNW	20		29. 50	29.26	31	
12	12	Rain, but ſnow on the hills.	ENE	15		29. 50	29.26	41	
	9					29. 44	29.23	40	
	6	Cloudy.	NE	30		29. 72	29.38	40	
13	12	Showers.				29. 80	29.42	41	
	9					29. 70	29.37	41	
	6	Cloudy.	NE	10	NW	29. 80	29.42	38	
14	12	Fair.				29. 92	29.48	44	
	9	Cloudy.				29. 96	29.50	40	
	6	Cloudy.	NE	30		29. 90	29.47	40	
15	12					29. 96	29.50	44	
	9	Showers.				29.104	29.54	40	
	6	Cloudy.	NE	37		29.108	29.57	40	
16	12	Showers.				29.100	29.52	45	
	9					29.100	29.52	40	
	6	Rain.	ENE	30		29. 88	29.46	39	
17	12					29. 90	29.47	42	
	9					29. 93	29.48	41	
	6	Showers.	E	10		29. 96	29.50	40	
18	12					29. 99	29.52	41	
	9					29.104	29.54	40	0.50

April 1782

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
19	6	Fair.	ESE	25		29.112	29.59	40	
	12					29.116	29.61	44	
	9					29.116	29.61	41	
20	6	Showery.	E	15		29.128	29.67	42	
	12					29.128	29.67	45	
	9					29.140	29.73	40	
21	6	Rain.	E	20		29.130	29.68	41	
	12		SW	35		29.120	29.63	46	
	9					29.128	29.67	41	0.10
22	6	Fair.	SW	40		29.115	29.60	50	
	12	A little rain.				29.126	29.66	56	
	9	Fair.	SE	45		29.123	29.64	46	
23	6	Fair.	SW	30		29.110	29.57	46	
	12	Showers.				29.115	29.60	55	
	9					29.107	29.56	49	
24	6	Showers.	SE	25		29. 78	29.41	50	
	12					29. 66	29.34	49	
	9	Hard rain.				29. 51	29.27	50	
25	6	Fair.	W	20		29. 73	29.38	49	0.67
	12		SE		W	29. 96	29.50	50	
	9					29. 96	29.50	49	
26	6	Fair.	E	30		29.112	29.59	48	
	12					29.116	29.61	50	
	9					29.126	29.66	47	
27	6	Cloudy, black easterly wind.	E	55		29.142	29.74	45	
	12					29.142	29.74	49	
	9					29.146	29.76	45	

April

April 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
28	6	Cloudy, black easterly wind.	E	50		29.140	29.73	41	
	12					29.140	29.73	46	
	9					29.136	29.71	42	
29	6	Rain all day, but fnow on the hills.	E	45		29.138	29.72	40	
	12					29.144	29.75	38	
	9					29.160	29.84	40	
30	6	Cloudy, froft.	SE	30		29.170	29.89	34	0.5
	12	Fair.				29.176	29.92	46	
	9					29.180	29.94	40	
							Total rain		1.79

May 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	6	Fair, frost.	ESE 28		29.180	29.94	34	
	12	No leaves on the trees yet.			29.182	29.95	47	
	9	Cloudy.			29.176	29 92	48	
2	6	Fair.	E 10		29.176	29.92	40	
	12	Cloudy.			29.170	29.89	48	
	9				29.166	29.87	45	
3	6	Cloudy.	E 15		29.152	29.80	41	
	12	Sometimes fair.			29.150	29.79	49	
	9	Cloudy.			29.146	29.77	46	
4	6	Fair.	E 12		29.185	29.70	47	
	12	Cloudy.			29.128	29 67	56	
	9		NE		29.116	29.61	45	
5	6	Cloudy.	NE 20		29.130	29.68	42	
	12		E		29.130	29.68	49	
	9		NW		29.126	29.66	45	
6	6	Cloudy.	NW 20	N	29.146	29.77	45	
	12				29.146	29.77	51	
	9				29.146	29.77	45	
7	6	Cloudy.	W 30	E	29.164	29.86	48	
	12		NE		29.165	29.86	51	
	9		W		29.173	29.90	47	
8	6	Fair.	SE 15		29.169	29.89	50	
	12	Showers.			29.160	29.84	54	
	9		S by E		29.128	29.67	48	
9	6	Cloudy.	SE by S 10		29. 96	29.50	50	
	12				29. 86	29.45	50	
	9				29. 94	29.49	47	

May

May, 1781.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
	6	Cloudy.	ENE	30		29.104	29.54	47	
10	12	Mild rain.	S			29.104	29.54	58	
	9	Showers.	WNW			29.107	29.56	51	
	6	Showers.	SSW	25		29. 76	29.40	50	
11	12					29. 70	29.37	54	
	9		WSW			29. 32	29.17	48	
	6	Fair.	WSW	30		29. 80	29.42	47	
12	12	Hard showers.				29. 82	29.43	50	
	9	Fair.				29. 58	29.31	54	
	6	Fair.	W	20		29.115	29.60	50	1.19
13	12	Showers.				29.116	29.61	58	
	9		S			29.116	29.61	50	
	6	Rain.	SSW	15		29. 75	29.39	56	
14	12					29. 56	29.29	60	
	9	Cloudy.	WSW			29. 56	29.29	55	
	6	Showers.	S by W	5		29. 66	29.34	55	
15	12		SSW			29. 66	29.34	56	
	9		W by S			29. 58	29.30	53	
	6	Showers.	W by S			29. 50	29.26	50	
16	12					29. 70	29.36	59	
	9					29. 88	29.46	50	0.24
	6	Fair.	W	15		29. 21	29.12	55	
17	12					29. 15	29. 8	61	
	9	Rain.	SW by W			29. 35	29.18	53	
	6	Showers.	WSW	15		29. 38	29.20	47	
18	12	Flying clouds.				29. 79	29.42	54	
	9	Cloudy.				29. 90	29.47	49	

May 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	d	Inch.
	6	Rain.	NW 20		29.116	29.61	49	
19	12	Fair.			29.134	29.70	51	
	9				29.164	29.86	45	
	6	Fair.	S		29.161	29.84	49	
20	12		SE by E 30		29.116	29.61	59	
	9	Hard rain.			29. 76	29.40	42	
	6	Rain.	NE 45		29. 88	29.46	46	
21	12		WSW		29.104	29.54	49	
	9	Cloudy.			29.104	29.54	47	
	6	Foggy, rain.	WNW 20		29.108	29.56	50	
22	12	Fair.			29. 96	29.50	55	
	9				29. 90	29.47	48	
		Rain in the night.						
	6	Showers.	SSE 40		29. 75	29.89	47	0.93
23	12	Fair.			29. 90	29.47	50	
	9		WNW		29. 98	29.51	49	
	6	Showers.	NW 50		29.112	29.59	50	
24	12	Fair.	60		29.136	29.71	55	
	9				29.160	29.84	50	
	6	Hazy.	SW 30		29.176	29.92	50	
25	12	Fair.			29.170	29.89	54	
	9				29.146	29.77	50	
	6	Fair.	W 30		29.154	29.81	50	0.24
26	12				29.173	29.90	56	
	9	Rainy.			29.170	29.89	53	
	6	Hazy.	SW 25		29.144	29.75	48	
27	12	Showers.			29.144	29.75	62	
	9		S		29.140	29.73	55	

May 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
	6	Cloudy.	SW	0		29.140	29.73	60	
28	12	Rain.	SE	20		29.134	29.70	60	
	9	Hard rain.	SW			29.106	29.55	62	
	6	Rain.	S	30		29. 96	29.50	60	
29	12					29. 88	29.46	58	
	9	Cloudy.	W			29. 90	29.47	52	0.33
	6	Small showers.	W	25		29. 94	29.49	57	
30	12	Fair at intervals.				29.106	29.55	62	
	9					29.106	29.55	55	
	6	Cloudy.	W	0		29. 96	29.50	52	
31	12	Hard showers.				29. 96	29.50	60	
	9					29. 96	29.50	58	0.28
							Total rain		3.21

June

June 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Rain.	NE 30	W	29.128	29.67	47	
1	12	Cloudy.	NW by N		29.138	29.72	56	
	9	Rain.			29.124	29.65	49	
	6	Fair.	NNW 40		29 188	29.98	50	
2	12	Flying clouds.	NW		29 190	29 99	55	
	9				30. 4	30. 2	50	0.31
	6	Fair.	NW 50		30. 8	30. 4	50	
3	12				30. 16	30. 9	56	
	9	Cloudy.			30. 19	30.10	52	
	6	Hazy.	WNW 20		30. 18	30.10	52	
4	12	Fair.			30. 10	30. 5	60	
	9	Cloudy.			30. 0	30. 0	55	
	6	Fair.	NW 30		30. 0	30. 0	50	
5	12				30. 2	30. 1	58	
	9				30. 4	30. 2	52	
	6	Fair.	E 10		30. 6	30. 3	47	
6	12				30. 6	30. 3	60	
	9				30. 6	30. 3	52	
	6	Fair.	SE 0		30. 5	30. 3	50	
7	12	Hazy.			30. 0	30. 0	67	
	9	Cloudy.			29.184	29.96	60	
	6	Cloudy.	SSW 10		29.163	29.86	55	
8	12	Fair and hot.			29.152	29.80	70	
	9	Some rain.			29.138	29.72	57	
	6	Showers.	S by W 25		29.100	29.52	56	
9	12	Fair.			29.108	29.56	67	
	9	Cloudy.			29.112	29.59	57	0.50

June

June 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
10	6	Showers.	W	30		29. 62	29.31	52	
	12	Fair.				29. 76	29.40	62	
	9	Showers.				29. 76	29.40	60	
11	6	Showers.	W	20		29. 57	29.30	55	
	12	Fair.		50		29. 96	29.50	62	
	9					29.116	29.61	58	0.29
12	6	Showers.	W	20	SE	29. 56	29.29	55	
	12		SSE	30		29.112	29.59	63	
	9		W	75		29. 76	29.40	55	
13	6	Showers.	W	50		29. 82	29.43	51	
	12	Fair.				29.116	29.61	62	
	9	Hard rain.				29.134	29.70	55	
14	6	Showers.	W by S	75		29.144	29.75	56	0.34
	12	Fair and hot.				29.150	29.79	69	
	9					29.160	29.84	60	
15	6	Cloudy.	WSW	20		29.184	29.96	61	
	12	Fair, very hot.		50		30. 0	30. 0	75	
	9			10		30. 4	30. 2	60	
16	6	Foggy.	SE	10	W	30. 16	30. 9	60	
	12	Fair.	NW	30		30. 32	30.17	72	
	9		SW			30. 24	30 18	60	
17	6	Fair.	ESE	20		29.188	29.98	60	
	12		S by E	10		29.164	29.86	71	
	4	Exceſſive hot, ther. roſe 21°.						81	
	9					29.150	29 79	75	
18	6	Flying thunder ſhowers.	NW	10		29.154	29.81	67	
	12	Fair.	WNW			29.172	29.90	67	
	9					30. 0	30. 0	58	

June 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Fair.	WNW 10		30. 16	30. 9	58	
	12				30. 34	30.18	67	
	9				30. 32	30.17	60	
20	6	Fair.	ENE 10	W	30. 32	30.17	58	
	12		WSW		30. 38	30.20	68	
	9				30. 38	30.20	64	
21	6	Hazy.	NNW 20		30. 44	30.23	60	
	12	Hot.	SW		30. 57	30.30	70	
	9				30. 76	30.40	67	
22	6	Fair and hot.	W 25		30. 76	30.40	60	
	12				30. 76	30.40	71	
	9		SE	SW	30. 76	30 40	60	
23	6	Fair, very hot.	ENE 30		30. 72	30.38	59	
	12				30. 61	30.32	74	
	9		ESE 40		30. 48	30.25	66	
24	6	Fair, very hot.	ESE 20		30. 30	30.16	62	
	12		S by E 10		30. 22	30.12	76	
	9				30. 19	30.10	67	
25	6	Fair.	W 0		30. 8	30. 4	67	
	12	Exceffive hot.	E 22	SW	29.186	29 97	77	
	2						80	
	9		S		29.184	29.96	72	
26	6	Foggy and cooler.	WNW 25		29.184	29.96	61	
	12	Fair.			29.184	29.96	70	
	9				29.184	29.96	58	
27	6	Fair.	E 0		30. 10	30. 5	57	
	12		WNW 30		30. 10	30. 5	67	
	9				30. 0	30. 0	56	

June

June 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
	6	Fair.	NW	10		29.188	29.98	57	
28	12					30. 4	30. 2	67	
	9					30. 4	30. 2	60	
	6	Cloudy.	W	20		30. 6	30. 3	56	
29	12					30. 6	30. 3	67	
	9					30. 0	30. 0	56	
	6	A little rain.	W	10		29.176	29.92	61	
30	12	Hazy.	WNW			29.180	29.94	67	
	9	Fair.				29.180	29.94	55	
							Total rain		1.44

July 1781.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
	6	Cloudy.	WNW	20		29.180	29.94	58	
1	12					29.184	29.96	62	
	9	A hard shower.				29.172	29.90	57	
	6	Flying clouds.	NW	40		29.144	29.75	58	
2	12					29.146	29.76	62	
	9					29.152	29.79	57	0.07
	6	Hazy.	WNW	15		29.182	29.95	59	
3	12	Fair.				29.186	29.97	62	
	9					29.180	29.94	58	
	6	A little rain.	WNW	30		29.162	29.85	58	
4	12	Hazy.				29.166	29.87	62	
	9	Fair				29.170	29.89	55	
	6	Fair and cold.	WNW	10		29.158	29.83	50	
5	12	Rain.				29.140	29.73	61	
	9					29.130	29.68	58	0.28
	6	Rain.	SE	20	SW	29.122	29.64	59	
6	12		E	35		29.134	29.70	61	
	9		NE			29.150	29.79	60	0.23
	6	Foggy, rain.	NE	20		29.162	29.85	59	
7	12	Fair.				29.162	29.85	62	
	9	Cloudy.		0		29.162	29.85	60	
	6	Cloudy.	E			29.167	29.88	60	
8	12	Fair.	WNW	40		29.174	29.91	63	
	9	Cloudy.				29.174	29.91	58	
	6	Fair and hot.	WNW	45		29.156	29.82	60	0.13
9	12					29.160	29.84	70	
	9					29.170	29.89	59	

July 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
10	6	Cloudy.	WNW 20		29.168	29.88	59	
	12	Fair.			29.170	29.89	60	
	9				29.175	29.91	58	
11	6	Rainy day.	SE 30		29.154	29.81	59	
	12		SSE	W by N	29.128	29.67	60	
	9		WNW		29.100	29.52	58	
12	6	A little shower.	W 0		29.122	29.64	56	
	12	Cloudy and fair at intervals.			29.144	29.75	68	
	9	Hard showers.			29.152	29.80	58	
13	6	Rain.	S by E 30		29.164	29.86	59	
	12				29.132	29.69	61	
	9	Hard rain.			29.100	29.52	58	
14	6	Cloudy.	W 10		29.128	29.67	59	0.68
	12	Fair.			29.140	29.73	64	
	9				29.152	29.80	60	
15	6	Hazy.	W 20		29.164	29.86	60	
	12	Fair and hot.			29.174	29.91	74	
	9				29.160	29.84	65	
16	6	Fair.	E 0		29.154	29.81	59	
	12		NW 10		29.150	29.79	65	
	9	Thunder showers.	SE		29.144	29.70	60	0.40
17	6	Hard rain.	S by E 30		29.166	29.87	58	
	12				29.170	29.89	61	
	9		NW		29.186	29.97	59	
18	6	Fair and cold morning.	WNW 15		30. 0	30. 0	50	0.37
	12				30. 16	30. 9	69	
	9				30. 32	30.17	62	

July

July 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Fair and hot.	W 10		30. 40	30.21	64	
	12		E	NE	30. 48	30.25	72	
	9				30. 36	30.19	65	
20	6	Fair.	WNW 15		30. 38	30.20	62	
	12		E	ENE	30. 30	30.16	67	
	9				30. 24	30.13	59	
21	6	Fair.	ENE 10		30. 24	30.13	64	
	12	Very hot.			30. 16	30. 9	74	
	9				30. 0	30. 0	68	
22	6	Fair.	E 30		29.160	29.84	61	
	12	Exceffive hot.			29.152	29.80	80	
	2						81	
	9	Flying fhowers.	S by E		29.140	29.73	68	
23	6	Flying fhowers.	S 10		29.146	29.77	64	
	12				29.150	29.79	74	
	9	Fair.			29.154	29.81	65	
24	6	Cloudy.	SW 35	NE	29.158	29.83	63	
	12	Some fmall rain.			29.166	29.87	71	
	9	Cloudy.	W	N	29.173	29.90	60	
25	6	Fair.	SW by W 40		29.188	29.98	59	
	12			ENE	29.190	29.99	65	
	9		W		30. 0	30. 0	60	
26	6	Cloudy.	SW 20	N	29.154	29.81	58	
	12	A hard fhower.	WNW		29.162	29.85	64	
	9	Fair afternoon.	NE		29.172	29.90	59	
27	6	Rain.	NE 30	SW	29.156	29.82	61	
	12				29.150	29.79	63	
	9		NW		29.140	29.73	60	

July

July 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	6	Showers.	W 25		29.140	29.73	60	
	12				29.132	29.69	62	
	9	Cloudy.			29.138	29.72	60	
29	6	Fair.	E 10		29.150	29.79	60	
	12	Hazy.	W 35		29.154	29.81	70	
	9	A little rain.	NW		29.163	29.85	60	
30	6	Fair.	NE 20		29.170	29.89	56	0.77
	12			NE	29.172	29.90	65	
	9		WNW 40		29.174	29.91	58	
31	6	Cloudy.	WNW 15		29.160	29.84	56	
	12	Some rain.		SE	29.156	29.82	62	
	9		SW		29.144	29.75	60	
						Total rain		2.93

August

Auguſt 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.		Inch.
	6	Flying ſhowers.	NE	30	S	29.140	29.73	58	
1	12					29.128	29.67	68	
	9					29. 90	29.47	60	
	6	Fair.	NE	0	S	29. 70	29.37	58	
2	12	Showers.	WSW	30		29. 60	29.31	66	
	9	Showers.	WNW			29. 72	29.38	60	
	6	Fair.	NW	40		29. 75	29.39	60	
3	12		WNW			29.110	29.58	67	
	9					29.113	29.60	60	
	6	Fair.	WNW	35		29.122	29.64	58	
4	12					29.134	29.70	64	
	9	Showers.				29.144	29.75	60	
	6	Some rain.	W	10		29.176	29.92	57	
5	12	Fair.				29.168	29.88	65	
	9					29.160	29.84	59	0.19
	6	Showers.	W			29.126	29.66	58	
6	12	Fair.	SW			29. 96	29.50	68	
	9	Thunder ſhowers.	S			29. 48	29.25	60	0.11
	6	Hard rain.	ESE	20		28.186	28.97	57	
7	12	Very ſtormy.	NW	90		29. 19	29.10	51	
	9					29. 39	29.20	55	1.51
	6	Very ſtormy ſhowers.	NNW	90		29. 56	29.29	56	
8	12	Stormy, but fair.		80		29.100	29.52	64	
	9			75		29.125	29.65	60	0.30
	6	Cloudy.	NW	40		29.120	29.63	58	
9	12	Showers.				29.116	29.61	62	
	9					29.122	29.64	60	

August 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
	6	Small showers.	NW 50		29.135	29.71	58	
10	12	Fair at intervals.			29.140	29.73	60	
	9				29.154	29.81	57	
	6	Fair.	WNW 35		29.160	29.84	58	
11	12				29.160	29.84	61	
	9		10		29.156	29.82	52	
	6	Cloudy.	SE 30		29.134	29.70	58	
12	12	Hard rain.			29.105	29.54	55	
	9				29.100	29.52	52	
	6	Fair.	WSW 10		29.100	29.52	57	0.54
13	12	Flying showers.			29. 76	29.40	64	
	9	Hard wind and rain.	75		29. 36	29.19	60	
	6	Hard showers.	SW 75		29. 34	29.18	63	
14	12	Fair.			29. 30	29.16	68	
	9	Cloudy.			29. 24	29.13	60	
	6	Cloudy.	WNW 0		29. 80	29.42	62	
15	12	Fair.			29.100	29.52	68	
	9				29.116	29.61	60	0.23
	6	Rain.	S		29. 48	29.25	56	
16	12	Fair.	WNW 20		29. 60	29.31	64	
	9	Cloudy.			29. 86	29 45	59	
	6	Cloudy.	WNW 30		29. 86	29.45	58	
17	12	Fair.			29. 86	29.45	60	
	9	Cloudy.			29. 86	29.45	59	
	6	Fair.	NW 35		29.138	29.72	60	
18	12		WNW		29.140	29.73	64	
	9	Showers.			29.144	29.75	60	

August 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
19	6	Fair.	SE	10		29.134	29.70	59	0.17
	12					29.146	29.76	69	
	9	Cloudy.	NW			29.148	29.77	60	
20	6	Cloudy.	SW	40		29.148	29.77	58	
	12	Showers.	WNW			29.144	29.75	70	
	9					29.144	29.75	60	
21	6	Fair.	WSW	25		29.152	29.79	58	
	12		SW			29.148	29.77	71	
	9	Showers.				29.154	29.80	63	
22	6	Showery day.	SW	40		29.144	29.75	58	
	12		WSW			29.134	29.70	67	
	9					29.122	29.64	57	
23	6	Showers.	W	50		29.126	29.66	56	
	12	Fair.	NW			29.138	29.73	67	
	9	Cloudy.	SW			29.154	29.81	60	
24	6	Cloudy.	SW	20		29.150	29.79	58	
	12	Showers.				29.115	29.60	70	
	9		WNW			29. 96	29.50	60	
25	6	Fair.	WSW	25		29.106	29.56	59	
	12					29.134	29.70	67	
	9	Showers.	W			29.150	29.79	60	
26	6	Showers:	WSW	20		29.154	29.81	60	
	12					29.163	29.85	70	
	9					29.173	29.90	61	0.64
27	6	Fair.	NW	45		29.138	29.72	60	
	12					29.135	29.70	64	
	9	Hard rain.	WNW			29.106	29.55	60	

August 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Fair.	NW 50		29.122	29.64	58	
28	12	Cloudy.			29 134	29.70	63	
	9	Showers.			29.130	29.68	58	
	6	Showers.	W 40		29.111	29.58	52	
29	12	Fair.	WNW		29.118	29.62	59	
	9	Hard rain.	65		29.123	29.64	54	
	6	Showers.	NWbyW 45		29.142	29.74	55	
30	12	Fair.			29.156	29.81	59	
	9				29.182	29.95	52	
	6	Showers.	NWbyW 30		30. 4	30. 2	52	
31	12	Fair.			30. 16	30. 9	60	
	9	Rain.			30. 24	30.13	55	0.50
						Total rain		4.19

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	6	Cloudy.	WSW 20		30. 32	30.17	57	
1	12	Fair.	WNW		30. 37	30.20	60	
	9	Cloudy.			30. 40	30.21	54	0.17
	6	Cloudy.	SW 10		30. 52	30.27	60	
2	12	Fair.	NE NW		30. 66	30.34	68	
	9		SW		30. 48	30.25	58	
	6	Cloudy.	SW 5		30. 29	30.15	52	
3	12		NE SE	SW	30. 20	30.11	67	
	9	Fair.	SW		30. 5	30. 3	60	
	6	Fair.	NNE 10	SSE	29.180	29.94	55	
4	12	Hot.	SSE		29.180	29.94	71	
	9				29.184	29.96	60	
	6	Fair, cold rain.	SE 20		30. 0	30. 0	52	
5	12	Hot.			30. 5	30. 3	70	
	9				30. 10	30. 5	60	
	6	Fair.	SE 25		30. 10	30. 5	55	
6	12				30. 15	30. 8	68	
	9				30. 15	30. 8	60	
	6	Fair.	SSE 15		30. 18	30.10	60	
7	12				30. 18	30.10	70	
	9				30. 20	30.11	60	
	6	Fair.	E 10		30. 24	30.13	57	
8	12				30. 24	30.13	67	
	9				30. 20	30.11	60	
	6	Foggy.	E 10		30. 24	30.13	60	
9	12	Fair.			30. 24	30.13	68	
	9				30. 14	30. 8	62	

September

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
10	6	Foggy.	E 15		30. 16	30. 9	58	
	12	Fair.			30. 10	30. 5	67	
	9				30. 0	30. 0	61	
11	6	Fair.	ENE 30		29.184	29.96	60	
	12				29.180	29.94	67	
	9		50		29.178	29.93	60	
12	6	Cloudy.	E 50		29.179	29.93	63	
	12				30. 0	30. 0	66	
	9	Fair.			30. 12	30. 6	60	
13	6	Cloudy.	E 20		30. 12	30. 6	55	
	12	Fair.			30. 0	30. 0	65	
	9		W		29.184	29.96	58	
14	6	Cloudy.	E 25		29.170	29.89	57	
	12				29.165	29.87	64	
	9				29.144	29.75	60	
15	6	A little rain.	S 25		29. 96	29.50	58	
	12	Fair.	ESE		29. 64	29.34	63	
	9				29. 48	29.25	60	
16	6	A little rain.	S 10		29. 20	29.11	60	
	12	Hard rain.	ESE 15		29. 12	29. 7	67	
	4				29. 0	29. 0		
	9	Stormy wind and rain.	SW 75		29. 33	29.17	57	
17	6	Fair.	W 25		29. 48	29.25	55	
	12	Showers.			29. 57	29.30	63	
	9				29. 57	29.30	5[illegible]	0.61
18	6	Showers.	WNW 30		29. 48	29.25	55	
	12				29. 20	29.11	63	
	9		70		29. 57	29.30	58	

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Fair and cold.	WNW 60		29.130	29.68	51	
	12				29.170	29.89	53	
	9				29.176	29.92	52	
20	6	Showers.	WNW 30		29.150	29.79	60	
	12				29 142	29.74	64	
	9				29.110	29.58	60	0.22
21	6	Cloudy.	SW 25		29.100	29.52	58	
	12	Hard rain.			29. 96	29.50	60	
	9				29. 96	29.50	61	
22	6	Fair.	SW 28		29.100	29.52	60	
	12	Hard showers.			29.100	29.52	67	
	9				29. 90	29 47	61	0.24
23	6	Showers.	NW 60		29. 76	29.40	57	
	12				29. 80	29.42	64	
	9	Stormy.	70		29.140	29.73	60	
24	6	A little shower.	WNW 10		29.164	29.86	53	
	12	Cloudy.			29.180	29.94	63	
	9	Rain.			29.170	29.89	58	0.88
25	6	Showers.	WNW 5		29.156	29.82	53	
	12	Fair and hot.			29.164	29.80	69	
	9				29.164	29.86	60	
26	6	Showers.	W 0		29.160	29.84	55	
	12	Fair and hot.			29.160	29.84	70	
	9	Cloudy.	20		29.160	29 84	58	0.45
27	6	Rain.	NW 30		29.125	29.65	55	
	12				29.150	29.79	67	
	9	Fair.			29.182	29.95	60	

September

September 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
28	6	Cloudy and cold.	SW	10		30. 6	30. 3	49	
	12			35		30. 12	30. 6	57	
	9	Rain.				29.180	29.94	55	
29	6	Fair.	W	20		29.170	29.89	60	
	12	Showers.				29.140	29.73	67	
	9	Hard rain.				29.128	29.67	61	
30	6	Fair.	NW	60		29.116	29.61	50	
	12	Showers.				29.144	29.75	55	
	9					29.180	29.94	51	
									1.33
							Total rain		3.90

October

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	7	Fair.	NW 30		30. 8	30. 4	49	
	12				30. 10	30. 5	54	
	9				30. 0	30. 0	50	
2	7	Showers.	WNW 35		29. 80	29.42	55	
	12				29. 70	29.37	64	
	9				29. 48	29.25	57	0.41
3	7	Fair.	NE 15		29. 30	29.16	58	
	12	Cloudy.			29. 60	29.31	63	
	9				29. 80	29.42	56	
4	7	Fair and cold.	NE 20		29. 88	29.45	43	
	12				29. 90	29.47	53	
	9	Cloudy.			30. 12	30. 6	50	
5	7	Fair.	ENE 10		29.172	29.89	45	
	12				29.180	29.94	48	
	9				29.186	29.97	48	
6	7	Fair.	E 25		30. 6	30. 3	50	
	12				30. 0	30. 0	56	
	9	Cloudy.			29.184	29.96	51	
7	7	Cloudy.	WNW 10		29.175	29.91	50	
	12	A little rain.			29.175	29.91	55	
	9				29.170	29.89	51	
8	7	Moift clofe day.	NW 30		29.164	29.86	53	
	12				29.160	29.84	56	
	9				29.160	29.84	51	
9	7	Some rain.	ESE 24		29.154	29.81	53	
	12				29.140	29.73	57	
	9				29.136	29.71	54	

October

October 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
10	7	Cloudy.	E	30		29. 80	29.42	50	
	12					29. 40	29.21	56	
	9			60		29. 16	29. 9	52	
11	7	Cloudy.	E	60		29. 16	29. 9	49	
	12					29. 40	29.21	54	
	9					29. 60	29.31	51	
12	7	Cloudy.	E	50		29. 84	29.44	50	
	12					29. 88	29.46	54	
	9					29.110	29.57	51	
13	7	Fair.	E	20		29.130	29.68	48	
	12					29.154	29.81	52	
	9	Cloudy.				29.162	29.85	50	
14	7	White frost.	W	10		29.184	29.96	37	
	12					30. 0	30. 0	47	
	9		E			30. 6	30. 3	50	
15	7	Some rain.	E	20		30. 16	30. 9	50	
	12					30. 16	30. 9	54	
	9					30. 16	30. 9	51	
16	7	Cloudy.	WNW	30		30. 16	30. 9	47	
	12	Fair.				30. 18	30.10	55	
	9					30. 20	30 11	50	
17	7	Fair.	N	35		30. 24	30.13	49	
	12		NW			30. 28	30.15	55	
	9	Cloudy.				30. 36	30 19	51	
18	7	Cloudy.	WNW	50		30. 58	30.31	50	
	12	Fair.				30. 72	30.38	54	
	9	Cloudy.		70		30. 66	30.34	50	

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	7	Some little rain.	WNW 60		29.154	29.81	52	
19	12				29.160	29.84	56	
	9	Stormy wind.	80		29.165	29.86	50	0.42
	7	Fair.	NW by N 60		29.170	29.89	47	
20	12				29.172	29.90	54	
	9	Rain.			29.170	29.89	55	
	7	Rain.	NW 40		29.124	29.65	56	
21	12	Fair.			29.144	29.75	60	
	9	Cloudy.			29.144	29.75	60	
	7	Cloudy.	WNW 35		29.144	29.75	60	
22	12				29.130	29.68	62	
	9	Rainy night.			29.110	29.58	62	
	7	Fair.	WNW 20		29.144	29.75	48	
23	12				29.160	29.84	53	
	9				30. 15	30. 8	50	
	7	Fair, froſt.	WNW 0		30. 38	30.20	39	
24	12				30. 48	30.25	52	
	9		35		30. 58	30.30	50	
	7	Showers.	W 20		30. 40	30.21	47	
25	12				30. 32	30.17	54	
	9				30. 19	30.10	51	0.40
	7	Fair.	W 0		30. 16	30. 9	55	
26	12				30. 40	30.21	60	
	9	Foggy.			30. 60	30.31	55	
	7	Foggy day.	SW 0		30. 56	30.29	54	
27	12				30. 48	30.25	58	
	9				30. 32	30.17	57	

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	7	Fair.	W 5		29.184	29.96	50	
	12	Small showers.	25		29 176	29.92	55	
	9	Stormy showers.	75		29.104	29.54	54	
29	7	Excessive stormy, but dry.	WNW 90		29.136	29.72	50	
	12		NW 80		29.160	29.84	55	
	9	Shower.	10		30. 8	30. 4	51	
30	7	Rain.	WNW 0		30. 0	30. 0	40	
	12				29.140	29.73	52	
	9		30		29. 96	29.50	50	
31	7	Showers.	NW 25		29. 68	29.36.	48	
	12				29. 79	29.42	50	
	9				30. 0	30. 0	46	0.46
						Total rain		1.69

November 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
1	8	Cloudy.	SE	15		30. 0	30. 0	42	
	12	Showers.				29. 97	29.50	49	
	9					30. 6	30. 3	45	
2	8	Rain.	SE	30		29.170	29.89	45	
	12					29. 84	29.44	50	
	9					29. 76	29.40	43	
3	8	Fair.	NW	10		29. 6	29. 3	45	
	12	Showers.	W			29. 12	29. 6	49	
	9		NE			29. 24	29.13	49	
4	8	Fair.	NW	30		29. 84	29.44	40	0.73
	12					29. 90	29.47	45	
	9		NE			29. 96	29.50	44	
5	8	Rain.	NE	35		29.104	29.54	39	
	12		ENE			29.136	29.71	43	
	9		NE			29.136	29.71	43	
6	8	Fair, frost.	NW	23		30. 0	30. 0	38	
	12	Snow on the hills.				30. 12	30. 6	43	
	9					30. 50	30.26	37	
7	8	Fair, hard frost.	NNW	0		30. 56	30.30	32	
	12		NE		E	30. 64	30.33	38	
	9		NW			30. 56	30.30	32	
8	8	Fair, hard frost.	NE	20		30. 32	30.17	30	
	12		ENE			30. 24	30.13	40	
	9	Cloudy.				30. 16	30. 9	38	
9	8	Cloudy, little frost.	NW	20		30. 0	30. 0	37	
	12	Frost.				30. 0	30. 0	43	
	9					29.178	29.93	32	

November 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
10	8	Very hard frost.	NW	0		29.170	29.89	27	
	12					29.166	29.87	35	
	9					29.164	29.86	38	
11	8	Rain.	ENE	25		29.144	29.75	37	
	12					29.160	29.84	42	
	9					29.172	29.90	40	
12	8	Foggy rain.	SSE	30		30. 0	30. 0	39	
	12					30. 8	30. 4	45	
	9	Fair.				30. 20	30.11	48	0.12
13	8	Fair, frost.	NW	0		30. 80	30.42	32	
	12	N. B. The tide ebbed and	SE			30. 96	30.50	45	
	9	flowed 3 times in an hour.	W			30.120	30.63	35	
14	8	Fair, frost.	W	0		30.114	30.60	35	
	12					30.112	30.59	40	
	9	Cloudy.				30.110	30.58	40	
15	8	Cloudy.	NW	10		30.108	30.57	48	
	12					30. 90	30.47	53	
	9	Rain.		50		30. 48	30.25	50	
16	8	Cloudy.	NW	75		30. 0	30. 0	48	
	12	Fair.				30. 0	30. 0	52	
	9					30. 16	30. 9	45	
17	8	Fair, little frost.	NNW	30		30. 38	30.20	42	
	12					30. 32	30.17	45	
	9					30. 30	30.16	40	
18	8	Cloudy.	W	10		29.176	29.92	44	
	12					29.174	29.91	48	
	9	Small rain.				29.172	29.90	40	

November 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	8	Foggy, rain.	NW 20		29.180	29.94	40	
	12				29.180	29.94	47	
	9				29.180	29.94	43	
20	8	Cloudy.	E 15		29.180	29.94	40	0.10
	12	Fair.			29.180	29.94	47	
	9				29.180	29.94	41	
21	8	Fair, hard froſt.	WNW 0		29.180	29.94	31	
	12				30. 8	30. 4	37	
	9				30. 0	30. 0	30	
22	8	Very hard froſt.	S 0		29.154	29.81	29	
	12		SE		29.128	29.67	39	
	9	Cloudy.	W		29.104	29.54	38	
23	8	Cloudy.	NW 10		29. 80	29.42	38	
	12				29. 72	29.38	40	
	9	Snow.	SE		29. 70	29.37	36	
24	8	Cloudy.	S by E 15		29. 66	29.34	35	
	12		E		29. 56	29.29	39	
	9	Rain.			29. 62	29.32	38	
25	8	Hard rain.	S by E 10		29. 90	29.47	37	
	12				29. 96	29.50	38	
	9	Cloudy.			29.130	29.68	38	
26	8	Fair.	S		29.180	29.94	37	
	12				30. 0	30. 0	40	
	9	Freezing hard.	S by E 20		30. 8	30. 4	32	
27	8	Cloudy.	S by E 20		29.160	29.84	38	
	12	Rain.	SW		29.152	29.80	40	
	9				29.144	29.75	40	

November

November 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	8	Rain.	S by E 30		29.136	29.71	39	
	12		SSW		29.130	29.68	42	
	9	Cloudy.			29.112	29.59	42	0.54
29	8	Hail fhowers.	NW 70		29.134	29.70	40	
	12				29.134	29.70	43	
	9		E		29.134	29.70	42	
30	8	Rain.	E 25		29.120	29.63	40	
	12	Fair.			29.126	29.66	43	
	9				29.130	29.68	40	
						Total rain		1.40

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	8	Cloudy.	ENE 10		29.154	29.81	37	
	12				29.160	29.84	42	
	9	Fair.			29.173	29.90	39	
2	8	Exceffive hard froft.	W 0		29.153	29.80	26	
	12		SE	W	29.153	29.80	35	
	9		ESE		29.153	29.80	30	
3	8	Fair, froft.	SSE 10		30. 0	30. 0	32	
	12				30. 0	30. 0	35	
	9	Cloudy.			30. 0	30. 0	32	
4	8	Cloudy, froft.	E by S 15		30. 20	30.11	32	
	12				30. 20	30.11	34	
	9				30. 20	30.11	32	
5	8	Cloudy, froft.	E by S 25		29.176	29.92	34	
	12				29.168	29.88	34	
	9				29.154	29.81	34	
6	8	Cloudy, froft.	S by W 10		29.134	29.70	40	
	12				29 126	29.65	45	
	9	Mifling rain.	S		29.126	29.66	42	
7	8	Cloudy.	S by W 12		29.140	29.73	41	
	12	Some rain.			29.140	29.73	49	
	9				29.152	29.80	47	0.14
8	8	Foggy and cold.	E 15		30. 0	30. 0	35	
	12				30. 8	30 4	35	
	9				30. 8	30. 4	35	
9	8	Foggy.	E 20		29 154	29.81	35	
	12				29 150	29.79	37	
	9				29.150	29.79	37	

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
10	8	Foggy.	NW by W 5		29.140	29.73	40	
	12		NE		29.136	29.71	43	
	9				29.140	29.73	41	
11	8	Foggy.	E 15		29.190	29.99	36	
	12		S by E		29.190	29.99	38	
	9				29.188	29.98	37	
12	8	Foggy.	WNW 25		29.156	29.82	36	
	12				29.132	29.69	37	
	9	Rain.			29.128	29.67	37	
13	8	Rain.	WNW 20		29.112	29.59	40	
	12	Fair.			29.112	29.59	42	
	9	Hard rain.			29. 64	29.33	40	
14	8	Rain.	WNW 30		29. 96	29.50	39	
	12				29.108	29.57	41	
	9	Cloudy.			29.108	29.57	40	
15	8	Cloudy, sometimes fair.	NW 35		29.122	29.64	39	
	12				29.124	29.65	42	
	9	Cloudy.			29.140	29.73	40	
16	8	Rain.	WNW 20		29.116	29.61	41	
	12				29.110	29.58	47	
	9	Very hard rain.	60		29.110	29.58	46	
17	8	Foggy, rain.	NW 40		29.112	29.59	50	
	12	Very mild.			29.118	29 62	54	
	9				29.116	29.61	51	1.09
18	8	Cloudy.	WNW 25		29.128	29.67	48	
	12	Fair.			29.160	29.84	50	
	9.				29.190	29.99	50	

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	8	Frosty and fair.	W 0		30. 92	30.48	40	
	12		10		30.116	30.61	45	
	9	Hazy.			30.128	30.67	41	
20	8	Foggy.	W 15		30.120	30.63	39	
	12				30.120	30.63	42	
	9				30.116	30.61	40	
21	8	Cloudy.	WNW 20		30.116	30.61	39	
	12	Fair.			30.120	30.63	48	
	9		NW		30.116	30.61	41	
22	8	Frosty and fair.	E 0		30.112	30.59	39	
	12				30.106	30.55	44	
	9	Cloudy.			30.106	30.55	46	
23	8	Fair.	SW 20		30. 72	30.38	40	
	12	Cloudy.	WNW		30. 70	30.37	45	
	9				30. 57	30.30	40	
24	8	Cloudy.	NW 75		30. 50	30.26	45	
	12				30. 50	30.26	48	
	9				30. 50	30.26	45	
25	8	Foggy.	NW 25		30. 28	30.15	45	
	12				30. 40	30.21	49	
	9				30. 64	30.33	40	
26	8	Foggy.	WNW 15		30. 96	30.50	44	
	12				30. 96	30.50	48	
	9				30. 96	30.50	40	
27	8	Foggy.	WNW 25		30. 64	30.34	41	
	12				30. 60	30.32	45	
	9				30. 60	30.32	39	

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	8	Foggy.	NW 40		30. 44	30.23	38	
	12				30. 40	30.21	43	
	9	Misling rain.			30. 30	30.16	40	
29	8	Misling rain.	W byN 15		30. 30	30.16	40	
	12				30. 28	30.15	44	
	9	Fair.			30. 32	30.17	39	
30	8	Foggy.	NW 40		30. 40	30.21	39	
	12				30. 40	30.21	42	
	9				30. 44	30.23	40	
31	8	Fair.	ENE 30		30. 50	30.26	40	
	12				30. 52	30.27	44	
	9		ESE 10		30. 54	30.28	38	
							Total rain	1.23

Total of Rain from the first of January, 31.26 inches.

IX. *Description of a Meteor, observed* Aug. 18, 1783. *By Mr.* Tiberius Cavallo, *F. R. S.*

Read Jan. 15, 1784.

BEING upon the Castle Terrace at Windsor, in company with my friend Dr. JAMES LIND, Dr. LOCKMAN, Mr. T. SANDBY, and a few other persons, we observed a very extraordinary meteor in the sky, such as none of us remembered to have seen before. We stood upon the north-east corner of the terrace, where we had a perfect view of the whole phænomenon; and as every one of the company remarked some particular circumstance, the collection of all which furnished the materials for this account; it may be presumed, that this description is as true as the nature of the subject can admit of.

The weather was calm, agreeably warm, and the sky was serene, excepting very near the horizon, where an haziness just prevented the appearance of the stars. A narrow, ragged, and oblong cloud stood on the north-west side of the heavens, reaching from the extremity of the haziness, which rose as high as 18 or 20 degrees, and stretching itself for several degrees towards the east, in a direction nearly parallel to the horizon. It was a little below this cloud, and consequently in the hazy part of the atmosphere, about the N. by W. ½ W. point of the compass, compafs, that this luminous meteor was firft perceived. Some flafhes of lambent light, much like the *aurora borealis*, were firft obferved on the northern part of the heavens, which were foon perceived to proceed from a roundifh luminous body, nearly as big as the femidiameter of the moon, and almoft ftationary in the abovementioned point of the heavens (fee A in the annexed figure, tab. IV). It was then about 25 minutes after nine o'clock in the evening *. This ball, at the beginning, appeared of a faint bluifh light, perhaps from its being juft kindled, or from its appearing through the hazinefs; but it gradually increafed its light, and foon began to move, at firft afcending above the horizon in an oblique direction towards the eaft. Its courfe in this direction was very fhort, perhaps of five or fix degrees; after which it turned itfelf towards the eaft, and moving in a direction nearly parallel to the horizon, reached as far as the S. E. by E. where it finally difappeared. The whole duration of the meteor was half a minute, or rather lefs; and the altitude of its track feemed to be about 25 degrees above the horizon. A fhort time after the beginning of its motion, the luminous body paffed behind the above-mentioned fmall cloud, fo that during this paffage we obferved only the light that was caft in the heavens from behind the cloud, without actually feeing the body from which it proceeded, for about the fixth or at moft the fifth part of its track; but as foon as the meteor emerged from behind the cloud, its light was prodigious. Every object appeared very diftinct; the whole face of the country in that beautiful profpect before the terrace

* Mr. SANDBY's watch was feventeen minutes paft nine neareft; it does not mark feconds.

being

being instantly illuminated. At this moment the body of the meteor appeared of an oblong form, like that represented at B in the figure; but it presently acquired a tail, and soon after it parted into several small bodies, each having a tail, and all moving in the same direction, at a small distance from each other, and very little behind the principal body, the size of which was gradually reduced after the division (see D in the figure). In this form the whole meteor moved as far as the S. E. by E. where the light decreasing rather abruptly, the whole disappeared.

During the phænomenon no noise was heard by any of our company, excepting one person, who imagined to have heard a crackling noise, something like that which is produced by small wood when burning. But about ten minutes after the disappearance of the meteor, and when we were just going to retire from the terrace, we heard a rumbling noise, as if it were of thunder at a great distance, which, to all probability, was the report of the meteor's explosion; and it may be naturally imagined that this explosion happened when the meteor parted into small bodies, *viz.* at about the middle of its track.

Now if that noise was really the report of the explosion which happened in the abovementioned place, the distance, altitude, course, and other particulars relating to this meteor, must be very nearly as expressed in the following list; they being calculated with mathematical accuracy, upon the preceding particulars; and upon the supposition that sound travels 1150 feet per second. But if the noise we heard was not that of the meteor's explosion, then the following calculations must be considered as quite useless and erroneous.

Distance

Diſtance of the meteor from Windſor Caſtle	130 miles.
Length of the path it deſcribed in the heavens	550 miles.
Diameter of the luminous body when it came out of the clouds	1070 yards.
Its height above the ſurface of the Earth	56½ miles.

The exploſion muſt have happened perpendicularly over Lincolnſhire.

T. CAVALLO.

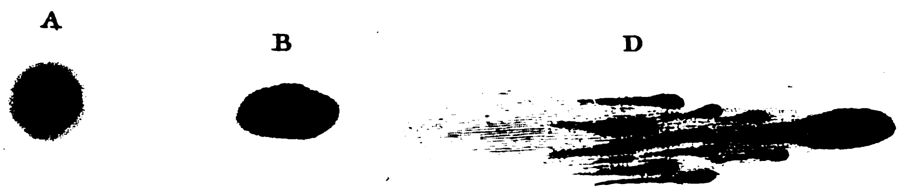
X. *An Account of the Meteors of the* 18*th of* August *and* 4*th of* October, 1783. *By* Alex. Aubert, *Esq. F. R. S. and S. A.*

Read Jan. 15, 1784.

HAVING been fortunate enough to ſee both the Meteors, of the 18th of Auguſt and of the 4th of October laſt, I think it my duty to communicate the obſervations I made upon them to the Royal Society. We are in general ſo little acquainted with theſe phænomena, that too many accounts of them cannot be collected, in order to enable us to form ſome idea of their nature, path, magnitude, and diſtance from the earth. It is not to be expected, that an obſerver, in the open air, to whom the appearance comes totally unexpected, can give a perfect account of it; but by going afterwards to the ſpot from which he ſaw it, he may, by the aſſiſtance of the objects about him, and ſome proper inſtruments, come near the truth: I have followed this method; and it is the reſult thence deduced I have the honour of communicating to the Society.

Monday the 18th of Auguſt had been a very ſultry day. At the time the meteor made its appearance, although the ſtars were bright in the upper part of the heavens, the horizon was ſurrounded with a hazineſs which did not permit any ſtars to be ſeen under an altitude of about eight degrees. I was on horſeback, returning to my Obſervatory at Loampit-hill, near Deptford, in Kent; my face was turned towards the South

Weſt,



T. Cavallo. delt.

Basire sc.

Weſt. I was at the foot of Lewiſham-bridge, when I was much ſurpriſed at perceiving ſuddenly a kind of glimmering light, reſembling faint but quickly repeated flaſhes of lightning; ſoon after which the light increaſed much towards the North Weſt; I turned directly to it, and ſaw it form into a large luminous body like electrical fire, with a tinge of blue round its edges. It roſe from the hazy part of the atmoſphere (which I have obſerved might be about 8° high), and moved at firſt almoſt in a vertical direction, changing its ſize and figure continually, having to me all the appearances of ſucceſſive inflammation, and not of a ſolid body; it was ſometimes round, at others oval and oblong, with its longeſt diameter in the line of its motion; although it had got high enough to be quite out of the hazy part of the horizon, it was ſurrounded and accompanied in its whole courſe with a kind of whitiſh miſt or light vapour. The place from which it roſe was about 38° from the north towards the weſt. After riſing a little way perpendicularly, it made its progreſs in a curve, ſo as to be at the higheſt when it had reached due eaſt, at an altitude of about 35°; after which, continuing a few degrees beyond the eaſt, and being about 30° high, it left behind it ſeveral globules of various ſhapes; the firſt which detached itſelf being very ſmall, and the others gradually larger and larger, until the laſt was nearly as large as the remaining preceding body; ſoon afterwards they all extinguiſhed gradually, like the bright ſtars of a ſky-rocket, with ſome inclination downwards, which appearance might probably ariſe from the upper parts of the ſeparate bodies extinguiſhing before the lower ones. The meteor was at the brighteſt and at the largeſt juſt before its ſeparation; I eſtimated its magnitude or area then to be equivalent to two full moons. Its light, during its whole courſe, was ſo great, that I could ſee every object diſtinctly, and when it was extinguiſhed the night appeared very dark: I could however ſee by my watch that it was ſeventeen minutes after nine: as ſoon as I got to my obſervatory, which might be about ten minutes afterwards, having compared it with my regulator, I found it about half a minute too ſlow for mean time. I think the whole appearance of the meteor, from its firſt riſing out of the hazy part of the atmoſphere to its total extinction, did not exceed ten or twelve ſeconds of time, during which it moved a ſpace correſponding to about 136° in azimuth. I recollect an appearance during its motion, which confirms me in the idea I had of its not being a ſolid body. In its progreſs it did not deſcribe a curve as regular as might have been expected from ſuch a body; but ſeemed to move in ſomewhat of a waving line. This irregularity in its courſe was probably owing to changes of its figure and ſize, occaſioned by the train of inflammation not running in an even line. I ſhould alſo mention that the meteor appeared extremely near to me, more particularly when it was at the higheſt; yet from the compariſons made already of obſervations at ſeveral diſtant places, we may reaſonably judge, that it could not be at leſs than 40 or 50 miles diſtance from the ſurface of the earth.

The meteor of Saturday the 4th of October laſt was of a much ſhorter duration and path. I was on horſeback, near the ſtones end, in Blackman-Street, Southwark; my face was turned northward. I ſaw, towards the N. N. E. a train of fire, reſembling in its motion a common meteor, vulgarly called a falling ſtar, but the colour of it was red; it originated at an altitude of about 25°, and moved quickly in a ſtrait line eaſtward, inclining gradually towards the horizon, ſo as to be, after a courſe of 15° or 20° in azimuth, about 15° above the horizon,

horizon, when it ſpread into a broader train, and growing of a lighter colour, it terminated by reſolving itſelf into a beautiful oblong body of the brighteſt fire, like electrical fire tinged blue, almoſt as large as the moon; it illuminated the ſtreet and houſes much more than any lightning I have ſeen; thoſe who had not a direct view of it, took it for a long flaſh of lightning. I think its whole courſe did not exceed 25°, nor the time of its appearance two or three ſeconds. It extinguiſhed quickly, and left behind it, in its path, a train of very dull reddiſh fire, which continued viſible to my naked eye above one minute and a half. The time of night was forty-three minutes paſt ſix; it was a fine ſtar-light evening, warmer than the preceding ones; the moon beyond the firſt quarter, and very bright; yet her light was not to be compared to the much greater light of the meteor.

I do not recollect hearing any noiſe or report, either during or after the appearance of theſe meteors.

London,
Nov. 6, 1783.

ALEXANDER AUBERT.

Since I wrote the above account, I have reaſon to think I have eſtimated the altitude of the laſt meteor rather too low; ſome of my friends in London, who had, at the time of its appearance, a very good object of compariſon for its altitude, make it nearer 30 than 20 degrees.

XI. *Obſervations on a remarkable Meteor ſeen on the* 18*th of* Auguſt, 1783, *communicated in a Letter to Sir* Joſeph Banks, *Bart. P. R. S. By* William Cooper, *D. D. F. R. S. Archdeacon of* York.

Read Jan. 15, 1784.

DEAR SIR,

Hartlepool, near Stockton, Aug. 19, 1783.

NO perſon could have a better opportunity of diſcerning this awful meteor than myſelf. The weather being, for this climate, aſtoniſhingly hot, my FAHRENHEIT's thermometer, on a north poſition, and in the open air, having for ſeveral days preceding graduated between the hours of ten o'clock in the morning and ſeven o'clock in the evening from 74° to 82°, I ſet out upon a journey to the ſea-ſide. The weather was ſultry, the atmoſphere hazy, and not a breath of air ſtirring. Towards nine o'clock at night it was ſo dark, that I could ſcarcely diſcern the hedges, road, or even the horſes heads. As we proceeded, I obſerved to my attendants, that there was ſomething ſingularly ſtriking in the appearance of the night, not merely from its ſtillneſs and darkneſs, but from the ſulphureous vapours which ſeemed to ſurround us on every ſide. In the midſt of this gloom, and on an inſtant, a brilliant tremulous light appeared to the N. W. by N. At the firſt it ſeemed ſtationary; but in a ſmall ſpace of time it burſt from its poſition, and took its courſe to the S. E. by E. It paſſed directly over our heads with a buzzing noiſe, ſeemingly at the height of ſixty yards. Its tail, as far as the eye could form any judgement, was about eight or ten yards in length. At laſt, this wonderful meteor divided into ſeveral glowing parts or balls of fire, the chief part ſtill remaining in its full ſplendor. Soon after this I heard two great exploſions, each equal to the report of a canon carrying a nine-pound ball. During its awful progreſs, the whole of the atmoſphere, as far as I could diſcern, was perfectly illuminated with the moſt beautifully vivid light I ever remember to have ſeen. The horſes on which we rode ſhrunk with fear; and ſome people whom we met upon the road declared their conſternation in the moſt expreſſive terms.

I have the honor to be, *&c.*

WILLIAM COOPER.

XII. *An Account of the Meteor of the 18th of* August, 1783. *In a Letter from* Richard Lovell Edgeworth, *Esq. F. R. S. to Sir* Joseph Banks, *Bart. P. R. S.*

Read Jan. 15, 1784.

DEAR SIR, Edgeworthstown, Mullingar, Ireland.

AT half past nine in the evening of the 18th of August, I saw the meteor which has been observed in so many different places.

Its size appeared to be about one third of the moon's diameter; and it moved from the north with an equable velocity, at an elevation of ten or twelve degrees, and in a line parallel to the horizon.

It was visible during ten or fifteen seconds, and seemed to be of a parabolic figure, with a luminous tail, twenty or five and twenty of its diameters in length.

It exhibited the most vivid colours; the foremost part being of the brightest blue, followed by different shades of red. Twice during its flight it was eclipsed or extinguished, not gradually, but at once, immerging and emerging with undiminished lustre.

I shall not venture to trouble you with any conjectures upon the nature of this phænomenon, as it is probable, that the subject has been fully discussed long before this time by your friends in London. I am, &c.

Sept. 5, 1783. RICHARD LOVELL EDGEWORTH.

XIII. *Experiments on Air. By* Henry Cavendiſh, *Eſq. F. R. S. & S. A.*

Read Jan. 15, 1784.

THE following experiments were made principally with a view to find out the cauſe of the diminution which common air is well known to ſuffer by all the various ways in which it is phlogiſticated, and to diſcover what becomes of the air thus loſt or condenſed; and as they ſeem not only to determine this point, but alſo to throw great light on the conſtitution and manner of production of dephlogiſticated air, I hope they may be not unworthy the acceptance of this ſociety.

Many gentlemen have ſuppoſed that fixed air is either generated or ſeparated from atmoſpheric air by phlogiſtication, and that the obſerved diminution is owing to this cauſe; my firſt experiments therefore were made in order to aſcertain whether any fixed air is really produced thereby. Now, it muſt be obſerved, that as all animal and vegetable ſubſtances contain fixed air, and yield it by burning, diſtillation, or putrefaction, nothing can be concluded from experiments in which the air is phlogiſticated by them. The only methods I know, which are not liable to objection, are by the calcination of metals, the burning of ſulphur or phoſphorus, the mixture of nitrous air, and the exploſion of inflammable air. Perhaps it may be ſuppoſed, that I ought to add to theſe the electric ſpark; but I think

think it much moſt likely, that the phlogiſtication of the air, and production of fixed air, in this proceſs, is owing to the burning of ſome inflammable matter in the apparatus. When the ſpark is taken from a ſolution of tournſol, the burning of the tournſol may produce this effect; when it is taken from lime-water, the burning of ſome foulneſs adhering to the tube, or perhaps of ſome inflammable matter contained in the lime, may have the ſame effect; and when quickſilver or metallic knobs are uſed, the calcination of them may contribute to the phlogiſtication of the air, though not to the production of fixed air.

There is no reaſon to think that any fixed air is produced by the firſt method of phlogiſtication. Dr. PRIESTLEY never found lime-water to become turbid by the calcination of metals over it*: Mr. LAVOISIER alſo found only a very ſlight and ſcarce perceptible turbid appearance, without any precipitation, to take place when lime-water was ſhaken in a glaſs veſſel full of the air in which lead had been calcined; and even this ſmall diminution of tranſparency in the lime-water might very likely ariſe, not from fixed air, but only from its being fouled by particles of the calcined metal, which we are told adhered in ſome places to the glaſs. This want of turbidity has been attributed to the fixed air uniting to the metallic calx, in preference to the lime; but there is no reaſon for ſuppoſing that the calx contained any fixed air; for I do not know that any one has extracted it from calces prepared in this manner; and though moſt metallic calces prepared over the fire, or by long expoſure to the atmoſphere, where they are in contact with fixed air, contain that ſubſtance, it by no means follows that they muſt

* Experiments on Air, vol. I. p. 137.

do

do so when prepared by methods in which they are not in contact with it.

Dr. PRIESTLEY also observed, that quicksilver, fouled by the addition of lead or tin, deposits a powder by agitation and exposure to the air, which consists in great measure of the calx of the imperfect metal. He found too some powder of this kind to contain fixed air *; but it is by no means clear that this air was produced by the phlogistication of the air in which the quicksilver was shaken; as the powder was not prepared on purpose, but was procured from quicksilver fouled by having been used in various experiments, and may therefore have contained other impurities besides the metallic calces.

I never heard of any fixed air being produced by the burning of sulphur or phosphorus; but it has been asserted, and commonly believed, that lime water is rendered cloudy by a mixture of common and nitrous air; which, if true, would be a convincing proof that on mixing those two substances some fixed air is either generated or separated; I therefore examined this carefully. Now it must be observed, that as common air usually contains a little fixed air, which is no essential part of it, but is easily separated by lime water; and as nitrous air may also contain fixed air, either if the metal from which it is procured be rusty, or if the water of the vessel in which it is caught contain calcareous earth, suspended by fixed air, as most waters do, it is proper first to free both airs from it by previously washing them with lime water †. Now I found, by repeated

* Exper. in Nat. Phil. vol. I. p. 144.

† Though fixed air is absorbed in considerable quantity by water, as I shewed in Phil. Transf. vol. LVI. yet it is not easy to deprive common air of all the fixed air

ed experiments, that if the lime water was clean, and the two airs were previoufly wafhed with that fubftance, not the leaft cloud was produced, either immediately on mixing them, or on fuffering them to ftand upwards of an hour, though it appeared by the thick clouds which were produced in the lime water, by breathing through it after the experiment was finifhed, that it was more than fufficient to faturate the acid formed by the decompofition of the nitrous air, and confequently that if any fixed air had been produced, it muft have become vifible. Once indeed I found a fmall cloud to be formed on the furface, after the mixture had ftood a few minutes. In this experiment the lime water was not quite clean; but whether the cloud was owing to this circumftance, or to the air's having not been properly wafhed, I cannot pretend to fay.

Neither does any fixed air feem to be produced by the explofion of the inflammable air obtained from metals, with either common or dephlogifticated air. This I tried by putting a little lime-water into a glafs globe fitted with a brafs cock, fo as to make it air tight, and an apparatus for firing air by electricity. This globe was exhaufted by an air-pump, and the two airs, which had been previoufly wafhed with lime-water, let in, and fuffered to remain fome time, to fhew whether they would affect the lime water, and then fired by electricity. The event was, that not the leaft cloud was produced in the lime-water, when the inflammable air was mixed with common air, and

air contained in it by means of water. On fhaking a mixture of ten parts of common air, and one of fixed air, with more than an equal bulk of diftilled water, not more than half of the fixed air was abforbed, and on transferring the air into frefh diftilled water only half the remainder was abforbed, as appeared by the diminution which it ftill fuffered on adding lime water.

only

only a very slight one, or rather diminution of transparency, when it was combined with dephlogisticated air. This, however, seemed not to be produced by fixed air; as it appeared instantly after the explosion, and did not increase on standing, and was spread uniformly through the liquor; whereas if it had been owing to fixed air, it would have taken up some short time before it appeared, and would have begun first at the surface, as was the case in the abovementioned experiment with nitrous air. What it was really owing to I cannot pretend to say; but if it did proceed from fixed air it would shew that only an excessively minute quantity was produced *. On the whole, though it is not improbable that fixed air may be generated in some chymical processes, yet it seems certain that it is not the general effect of phlogisticating air, and that the diminution of common air is by no means owing to the generation or separation of fixed air from it.

As there seemed great reason to think, from Dr. Priestley's experiments, that the nitrous and vitriolic acids were convertible into dephlogisticated air, I tried whether the dephlogisticated part of common air might not, by phlogistication, be changed into nitrous or vitriolic acid. For this purpose I impregnated some milk of lime with the fumes of burning sulphur, by putting a little of it into a large glass receiver, and burning sulphur therein; taking care to keep the mouth of the receiver stopt till the fumes were all absorbed; after which the air of the receiver was changed, and more sulphur burnt in it as before, and the process repeated till 122 grains of sulphur were consumed. The milk of lime was then filtered and evaporated, but it yielded no nitrous salt, nor any other substance except selenite; so that no sensible quantity of the air was changed into

* Dr. PRIESTLEY also found no fixed air to be produced by the explosion of inflammable and common air. Vol. V, p. 124.

into nitrous acid. It muſt be obſerved, that as the vitriolic acid produced by the burning ſulphur is changed by its union with the lime into ſelenite, which is very little ſoluble in water, a very ſmall quantity of nitrous ſalt, or any other ſubſtance which is ſoluble in water, would have been perceived.

I alſo tried whether any nitrous acid was produced by phlogiſticating common air with liver of ſulphur; for this purpoſe I made a ſolution of flowers of ſulphur by boiling it with lime, and put a little of it into a large receiver, and ſhook it frequently, changing now and then the air, till the yellow colour of the ſolution was quite gone; a ſign that all the ſulphur was, by the loſs of its phlogiſton, turned into vitriolic acid, and united to the lime, or precipitated; the liquor was then filtered and evaporated, but it yielded not the leaſt nitrous ſalt.

The experiment was repeated in nearly the ſame manner with dephlogiſticated air procured from red precipitate; but not the leaſt nitrous acid was obtained.

It is well known that common ſelenite is very little ſoluble in water; whereas that procured in the two laſt experiments was very ſoluble, and even cryſtallized readily, and was intenſely bitter; this however appeared to be owing merely to the acid with which it was formed being very much phlogiſticated; for on evaporating it to dryneſs, and expoſing it to the air for a few days, it became much leſs ſoluble, ſo that on adding water to it not much diſſolved; and by repeating this proceſs once or twice, it ſeemed to become not more ſoluble than ſelenite made in the common manner.

This ſolubility of the ſelenite cauſed ſome trouble in trying the experiment; for while it continued much ſoluble it would have been impoſſible to have diſtinguiſhed a ſmall mixture of nitrous ſalt; but by the abovementioned proceſs I was able to diſtinguiſh

diſtinguiſh as ſmall a proportion as if the ſelenite had been originally no more ſoluble than uſual.

The nature of the neutral ſalts made with the phlogiſticated vitriolic and nitrous acids has not been much examined by the chymiſts, though it ſeems well worth their attention; and it is likely that many beſides the foregoing may differ remarkably from thoſe made with the ſame acids in their common ſtate. Nitre formed with the phlogiſticated nitrous acid has been found to differ conſiderably from common nitre, as well as Sal Polychreſt from vitriolated tartar.

In order to try whether any vitriolic acid was produced by the phlogiſtication of air, I impregnated fifty ounces of diſtilled water with the fumes produced on mixing fifty-two ounce meaſures of nitrous air with a quantity of common air ſufficient to decompound it. This was done by filling a bottle with ſome of this water, and inverting it into a baſon of the ſame, and then, by a ſyphon, letting in as much nitrous air as filled it half-full; after which common air was added ſlowly by the ſame ſyphon, till all the nitrous air was decompounded. When this was done, the diſtilled water was further impregnated in the ſame manner till the whole of the abovementioned quantity of nitrous air was employed. This impregnated water, which was very ſenſibly acid to the taſte, was diſtilled in a glaſs retort. The firſt runnings were very acid, and ſmelt pungent, being nitrous acid much phlogiſticated; what came next had no ſenſible taſte or ſmell; but the laſt runnings were very acid, and conſiſted of nitrous acid not phlogiſticated. Scarce any ſediment was left behind. Theſe different parcels of diſtilled liquor were then exactly ſaturated with ſalt of tartar, and evaporated; they yielded 87½ grains of nitre, which, as far as I could perceive, was unmixed with vitriolated tartar or any other substance, and consequently no sensible quantity of the common air with which the nitrous air was mixed was turned into vitriolic acid.

It appears, from this experiment, that nitrous air contains as much acid as 2¾ times its weight of saltpetre; for fifty-two ounce-measures of nitrous air weigh 32 grains, and, as was before said, yield as much acid as is contained in 87¼ grains of saltpetre; so that the acid in nitrous air is in a remarkably concentrated state, and I believe more than 1½ times as much so as the strongest spirit of nitre ever prepared.

Having now mentioned the unsuccessful attempts I made to find out what becomes of the air lost by phlogistication, I proceed to some experiments, which serve really to explain the matter.

In Dr. PRIESTLEY's last volume of experiments is related an experiment of Mr. WARLTIRE's, in which it is said, that, on firing a mixture of common and inflammable air by electricity in a close copper vessel holding about three pints, a loss of weight was always perceived, on an average about two grains, though the vessel was stopped in such a manner that no air could escape by the explosion. It is also related, that on repeating the experiment in glass vessels, the inside of the glass, though clean and dry before, immediately became dewy; which confirmed an opinion he had long entertained, that common air deposits its moisture by phlogistication. As the latter experiment seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely. The first experiment also, if there was no mistake in it, would be very extraordinary and curious; but it did not succeed with me; for though the vessel I used held more than Mr. WARLTIRE's, namely, 24,000 grains of water, and though the experiment

was

was repeated several times with different proportions of common and inflammable air, I could never perceive a loss of weight of more than one-fifth of a grain, and commonly none at all. It must be observed, however, that though there were some of the experiments in which it seemed to diminish a little in weight, there were none in which it increased *.

In all the experiments, the inside of the glass globe became dewy, as observed by Mr. WARLTIRE; but not the least sooty matter could be perceived. Care was taken in all of them to find how much the air was diminished by the explosion, and to observe its test. The result is as follows; the bulk of the inflammable air being expressed in decimals of the common air,

Common air.	Inflammable air.	Diminution.	Air remaining after the explosion.	Test of this air in first method.	Standard.
	1,241	,686	1,555	,055	,0
1	1,055	,642	1,413	,063	,0
	,796	,647	1,959	,066	,0
	,423	,612	,811	,097	,03
	,331	,476	,855	,339	,27
	,206	,294	,912	,048	,58

In these experiments the inflammable air was procured from zinc, as it was in all my experiments, except where otherwise expressed: but I made two more experiments, to try whether there was any difference between the air from zinc and that from iron, the quantity of inflammable air being the same in both; namely, 0,331 of the common; but I could not find any difference to be depended on between the two kinds of air,

* Dr. PRIESTLEY, I am informed, has since found the experiment not to succeed.

either

either in the diminution which they suffered by the explosion, or the test of the burnt air.

From the fourth experiment it appears, that 423 measures of inflammable air are nearly sufficient to completely phlogisticate 1000 of common air; and that the bulk of the air remaining after the explosion is then very little more than four-fifths of the common air employed; so that as common air cannot be reduced to a much less bulk than that by any method of phlogistication, we may safely conclude, that when they are mixed in this proportion, and exploded, almost all the inflammable air, and about one-fifth part of the common air, lose their elasticity, and are condensed into the dew which lines the glass.

The better to examine the nature of this dew, 500000 grain measures of inflammable air were burnt with about $2\frac{1}{2}$ times that quantity of common air, and the burnt air made to pass through a glass cylinder eight feet long and three-quarters of an inch in diameter, in order to deposit the dew. The two airs were conveyed slowly into this cylinder by separate copper pipes, passing through a brass plate which stopped up the end of the cylinder; and as neither inflammable nor common air can burn by themselves, there was no danger of the flame spreading into the magazines from which they were conveyed. Each of these magazines consisted of a large tin vessel, inverted into another vessel just big enough to receive it. The inner vessel communicated with the copper pipe, and the air was forced out of it by pouring water into the outer vessel; and in order that the quantity of common air expelled should be $2\frac{1}{2}$ times that of the inflammable, the water was let into the outer vessels by two holes in the bottom of the same tin pan, the hole which conveyed the water into that vessel in which

which the common air was confined being 2½ times as big as the other.

In trying the experiment, the magazines being firſt filled with their reſpective airs, the glaſs cylinder was taken off, and water let, by the two holes, into the outer veſſels, till the airs began to iſſue from the ends of the copper pipes; they were then ſet on fire by a candle, and the cylinder put on again in its place. By this means upwards of 135 grains of water were condenſed in the cylinder, which had no taſte nor ſmell, and which left no ſenſible ſediment when evaporated to dryneſs; neither did it yield any pungent ſmell during the evaporation; in ſhort, it ſeemed pure water.

In my firſt experiment, the cylinder near that part where the air was fired was a little tinged with ſooty matter, but very ſlightly ſo; and that little ſeemed to proceed from the putty with which the apparatus was luted, and which was heated by the flame; for in another experiment, in which it was contrived ſo that the luting ſhould not be much heated, ſcarce any ſooty tinge could be perceived.

By the experiments with the globe it appeared, that when inflammable and common air are exploded in a proper proportion, almoſt all the inflammable air, and near one-fifth of the common air, loſe their elaſticity, and are condenſed into dew. And by this experiment it appears, that this dew is plain water, and conſequently that almoſt all the inflammable air, and about one-fifth of the common air, are turned into pure water.

In order to examine the nature of the matter condenſed on firing a mixture of dephlogiſticated and inflammable air, I took a glaſs globe, holding 8800 grain meaſures, furniſhed with a braſs cock and an apparatus for firing air by electricity. This globe was well exhauſted by an air-pump, and then filled with a mixture of inflammable and dephlogisticated air, by shutting the cock, fastening a bent glass tube to its mouth, and letting up the end of it into a glass jar inverted into water, and containing a mixture of 19500 grain measures of dephlogisticated air, and 37000 of inflammable; so that, upon opening the cock, some of this mixed air rushed through the bent tube, and filled the globe *. The cock was then shut, and the included air fired by electricity, by which means almost all of it lost its elasticity. The cock was then again opened, so as to let in more of the same air, to supply the place of that destroyed by the explosion, which was again fired, and the operation continued till almost the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the sixth part of the mixture, almost the whole of it was exploded therein, without any fresh exhaustion of the globe.

As I was desirous to try the quantity and test of this burnt air, without letting any water into the globe, which would have prevented my examining the nature of the condensed matter, I took a larger globe, furnished also with a stop cock, exhausted it by an air-pump, and screwed it on upon the cock of the former globe; upon which, by opening both cocks, the air rushed out of the smaller globe into the larger, till it became of equal density in both; then, by shutting the cock of the larger globe, unscrewing it again from the former, and opening it under water, I was enabled to find the quantity of the burnt air in it; and consequently, as the proportion which the contents of the two globes bore to each other was

* In order to prevent any water from getting into this tube, while dipped under water to let it up into the glass jar, a bit of wax was was stuck upon the end of it, which was rubbed off when raised above the surface of the water.

known,

known, could tell the quantity of burnt air in the ſmall globe before the communication was made between them. By this means the whole quantity of the burnt air was found to be 2950 grain meaſures; its ſtandard was 1,85.

The liquor condenſed in the globe, in weight about 30 grains, was ſenſibly acid to the taſte, and by ſaturation with fixed alkali, and evaporation, yielded near two grains of nitre; ſo that it conſiſted of water united to a ſmall quantity of nitrous acid. No ſooty matter was depoſited in the globe. The dephlogiſticated air uſed in this experiment was procured from red precipitate, that is, from a ſolution of quickſilver in ſpirit of nitre diſtilled till it acquires a red colour.

As it was ſuſpected, that the acid contained in the condenſed liquor was no eſſential part of the dephlogiſticated air, but was owing to ſome acid vapour which came over in making it and had not been abſorbed by the water, the experiment was repeated in the ſame manner, with ſome more of the ſame air, which had been previouſly waſhed with water, by keeping it a day or two in a bottle with ſome water, and ſhaking it frequently; whereas that uſed in the preceding experiment had never paſſed through water, except in preparing it. The condenſed liquor was ſtill acid.

The experiment was alſo repeated with dephlogiſticated air, procured from red lead by means of oil of vitriol; the liquor condenſed was acid, but by an accident I was prevented from determining the nature of the acid.

I alſo procured ſome dephlogiſticated air from the leaves of plants, in the manner of Doctors INGENHOUSZ and PRIESTLEY, and exploded it with inflammable air as before; the condenſed liquor ſtill continued acid, and of the nitrous kind.

In all these experiments the proportion of inflammable air was such, that the burnt air was not much phlogisticated; and it was observed, that the less phlogisticated it was, the more acid was the condensed liquor. I therefore made another experiment, with some more of the same air from plants, in which the proportion of inflammable air was greater, so that the burnt air was almost completely phlogisticated, its standard being $\frac{4}{10}$. The condensed liquor was then not at all acid, but seemed pure water: so that it appears, that with this kind of dephlogisticated air, the condensed liquor is not at all acid, when the two airs are mixed in such a proportion that the burnt air is almost completely phlogisticated, but is considerably so when it is not much phlogisticated.

In order to see whether the same thing would obtain with air procured from red precipitate, I made two more experiments with that kind of air, the air in both being taken from the same bottle, and the experiment tried in the same manner, except that the proportions of inflammable air were different. In the first, in which the burnt air was almost completely phlogisticated, the condensed liquor was not at all acid. In the second, in which its standard was 1,86, that is, not much phlogisticated, it was considerably acid; so that with this air, as well as with that from plants, the condensed liquor contains, or is entirely free from, acid, according as the burnt air is less or more phlogisticated; and there can be little doubt but that the same rule obtains with any other kind of dephlogisticated air.

In order to see whether the acid, formed by the explosion of dephlogisticated air obtained by means of the vitriolic acid, would also be of the nitrous kind, I procured some air from turbith mineral, and exploded it with inflammable air, the propor-

proportion being such that the burnt air was not much phlogisticated. The condensed liquor manifested an acidity, which appeared, by saturation with a solution of salt of tartar, to be of the nitrous kind; and it was found, by the addition of some terra ponderosa salita, to contain little or no vitriolic acid.

When inflammable air was exploded with common air, in such a proportion that the standard of the burnt air was about $\frac{4}{10}$, the condensed liquor was not in the least acid. There is no difference, however, in this respect between common air, and dephlogisticated air mixed with phlogisticated in such a proportion as to reduce it to the standard of common air; for some dephlogisticated air from red precipitate, being reduced to this standard by the addition of perfectly phlogisticated air, and then exploded with the same proportion of inflammable air as the common air was in the foregoing experiment, the condensed liquor was not in the least acid.

From the foregoing experiments it appears, that when a mixture of inflammable and dephlogisticated air is exploded in such proportion that the burnt air is not much phlogisticated, the condensed liquor contains a little acid, which is always of the nitrous kind, whatever substance the dephlogisticated air is procured from; but if the proportion be such that the burnt air is almost entirely phlogisticated, the condensed liquor is not at all acid, but seems pure water, without any addition whatever; and as, when they are mixed in that proportion, very little air remains after the explosion, almost the whole being condensed, it follows, that almost the whole of the inflammable and dephlogisticated air is converted into pure water. It is not easy, indeed, to determine from these experiments what proportion the burnt air, remaining after the explosions, bore to the dephlogisticated air employed, as neither the

 small

ſmall nor the large globe could be perfectly exhauſted of air, and there was no ſaying with exactneſs what quantity was left in them; but in moſt of them, after allowing for this uncertainty, the true quantity of burnt air ſeemed not more than $\frac{1}{17}$th of the dephlogiſticated air employed, or $\frac{1}{50}$th of the mixture. It ſeems, however, unneceſſary to determine this point exactly, as the quantity is ſo ſmall, that there can be little doubt but that it proceeds only from the impurities mixed with the dephlogiſticated and inflammable air, and conſequently that, if thoſe airs could be obtained perfectly pure, the whole would be condenſed.

With reſpect to common air, and dephlogiſticated air reduced by the addition of phlogiſticated air to the ſtandard of common air, the caſe is different; as the liquor condenſed in exploding them with inflammable air, I believe I may ſay in any proportion, is not at all acid; perhaps, becauſe if they are mixed in ſuch a proportion as that the burnt air is not much phlogiſticated, the exploſion is too weak, and not accompanied with ſufficient heat.

All the foregoing experiments, on the exploſion of inflammable air with common and dephlogiſticated airs, except thoſe which relate to the cauſe of the acid found in the water, were made in the ſummer of the year 1781, and were mentioned by me to Dr. PRIESTLEY, who in conſequence of it made ſome experiments of the ſame kind, as he relates in a paper printed in the preceding volume of the Tranſactions. During the laſt ſummer alſo, a friend of mine gave ſome account of them to M. LAVOISIER, as well as of the concluſion drawn from them, that dephlogiſticated air is only water deprived of phlogiſton; but at that time ſo far was M. LAVOISIER from thinking any ſuch opinion warranted, that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water. It is remarkable, that neither of these gentlemen found any acid in the water produced by the combustion; which might proceed from the latter having burnt the two airs in a different manner from what I did; and from the former having used a different kind of inflammable air, namely, that from charcoal, and perhaps having used a greater proportion of it.

Before I enter into the cause of these phænomena, it will be proper to take notice, that phlogisticated air appears to be nothing else than the nitrous acid united to phlogiston; for when nitre is deflagrated with charcoal, the acid is almost entirely converted into this kind of air. That the acid is entirely converted into air, appears from the common process for making what is called clyssus of nitre; for if the nitre and charcoal are dry, scarce any thing is found in the vessels prepared for condensing the fumes; but if they are moist a little liquor is collected, which is nothing but the water contained in the materials, impregnated with a little volatile alkali, proceeding in all probability from the imperfectly burnt charcoal, and a little fixed alkali, consisting of some of the alkalized nitre carried over by the heat and watery vapours. As far as I can perceive too, at present, the air into which much the greatest part of the acid is converted, differs in no respect from common air phlogisticated. A small part of the acid, however, is turned into nitrous air, and the whole is mixed with a good deal of fixed, and perhaps a little inflammable air, both proceeding from the charcoal.

It is well known, that the nitrous acid is also converted by phlogistication into nitrous air, in which respect there seems a considerable

considerable analogy between that and the vitriolic acid; for the vitriolic acid, when united to a smaller proportion of phlogiston, forms the volatile sulphureous acid and vitriolic acid air, both of which, by exposure to the atmosphere, lose their phlogiston, though not very fast, and are turned back into vitriolic acid; but, when united to a greater proportion of phlogiston, it forms sulphur, which shews no signs of acidity, unless a small degree of affinity to alkalies can be called so, and in which the phlogiston is more strongly adherent, so that it does not fly off when exposed to the air, unless assisted by a heat sufficient to set it on fire. In like manner the nitrous acid, united to a certain quantity of phlogiston, forms nitrous fumes and nitrous air, which readily quit their phlogiston to common air; but when united to a different, in all probability a larger quantity, it forms phlogisticated air, which shews no signs of acidity, and is still less disposed to part with its phlogiston than sulphur.

This being premised, there seem two ways by which the phænomena of the acid found in the condensed liquor may be explained; first, by supposing that dephlogisticated air contains a little nitrous acid which enters into it as one of its component parts, and that this acid, when the inflammable air is in a sufficient proportion, unites to the phlogiston, and is turned into phlogisticated air, but does not when the inflammable air is in too small a proportion; and, secondly, by supposing that there is no nitrous acid mixed with, or entering into the composition of, dephlogisticated air, but that, when this air is in a sufficient proportion, part of the phlogisticated air with which it is debased is, by the strong affinity of phlogiston to dephlogisticated air, deprived of its phlogiston and turned into nitrous acid; whereas, when the dephlogisticated air is not more than sufficient to consume the inflammable air,

none

none then remains to deprive the phlogisticated air of its phlogiston, and turn it into acid.

If the latter explanation be true, I think, we must allow that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or, in other words, that water consists of dephlogisticated air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. PRIESTLEY and Mr. KIRWAN suppose, or else water united to phlogiston *; since, according to this supposition, these two substances united together form pure water. On the other hand, if the first explanation be true, we must suppose that dephlogisticated air consists of water united to a little nitrous acid and deprived of its phlogiston; but still the nitrous acid in it must make only a very small part of the whole,

* Either of these suppositions will agree equally well with the following experiments; but the latter seems to me much the most likely. What principally makes me think so is, that common or dephlogisticated air do not absorb phlogiston from inflammable air, unless assisted by a red heat, whereas they absorb the phlogiston of nitrous air, liver of sulphur, and many other substances, without that assistance; and it seems inexplicable, that they should refuse to unite to pure phlogiston, when they are able to extract it from substances to which it has an affinity; that is, that they should overcome the affinity of phlogiston to other substances, and extract it from them, when they will not even unite to it when presented to them. On the other hand, I know no experiment which shews inflammable air to be pure phlogiston rather than an union of it with water, unless it be Dr. PRIESTLEY's experiment of expelling inflammable air from iron by heat alone. I am not sufficiently acquainted with the circumstances of that experiment to argue with certainty about it; but I think it much more likely, that the inflammable air was formed by the union of the phlogiston of the iron filings with the water dispersed among them, or contained in the retort or other vessel in which it was heated; and in all probability this was the cause of the separation of the phlogiston, as iron seems not disposed to part with its phlogiston by heat alone, without being assisted by the air or some other substance.

as it is found, that the phlogisticated air, which it is converted into, is very small in comparison of the dephlogisticated air.

I think the second of these explanations seems much the most likely; as it was found, that the acid in the condensed liquor was of the nitrous kind, not only when the dephlogisticated air was prepared from red precipitate, but also when it was procured from plants or from turbith mineral: and it seems not likely, that air procured from plants, and still less likely that air procured from a solution of mercury in oil of vitriol, should contain any nitrous acid.

Another strong argument in favour of this opinion is, that dephlogisticated air yields no nitrous acid when phlogisticated by liver of sulphur; for if this air contains nitrous acid, and yields it when phlogisticated by explosion with inflammable air, it is very extraordinary that it should not do so when phlogisticated by other means.

But what forms a stronger and, I think, almost decisive argument in favour of this explanation is, that when the dephlogisticated air is very pure, the condensed liquor is made much more strongly acid by mixing the air to be exploded with a little phlogisticated air, as appears by the following experiments.

A mixture of 18500 grain measures of inflammable air with 9750 of dephlogisticated air procured from red precipitate were exploded in the usual manner; after which, a mixture of the same quantities of the same dephlogisticated and inflammable air, with the addition of 2500 of air phlogisticated by iron filings and sulphur, was treated in the same manner. The condensed liquor, in both experiments, was acid, but that in the latter evidently more so, as appeared also by saturating each of them separately with marble powder, and precipitating

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the earth by fixed alkali, the precipitate of the second experiment weighing one-fifth of a grain, and that of the first being several times less. The standard of the burnt air in the first experiment was 1,86, and in the second only 0,9.

It must be observed, that all circumstances were the same in these two experiments, except that in the latter the air to be exploded was mixed with some phlogisticated air, and that in consequence the burnt air was more phlogisticated than in the former; and from what has been before said, it appears, that this latter circumstance ought rather to have made the condensed liquor less acid; and yet it was found to be much more so, which shews strongly that it was the phlogisticated air which furnished the acid.

As a further confirmation of this point, these two comparative experiments were repeated with a little variation, namely, in the first experiment there was first let into the globe 1500 of dephlogisticated air, and then the mixture, consisting of 12200 of dephlogisticated air and 25900 of inflammable, was let in at different times as usual. In the second experiment, besides the 1500 of dephlogisticated air first let in, there was also admitted 2500 of phlogisticated air, after which the mixture, consisting of the same quantities of dephlogisticated and inflammable air as before, was let in as usual. The condensed liquor of the second experiment was about three times as acid as that of the first, as it required 119 grains of a diluted solution of salt of tartar to saturate it, and the other only 37. The standard of the burnt air was 0,78 in the second experiment, and 1,96 in the first.

The intention of previously letting in some dephlogisticated air in the two last experiments was, that the condensed liquor was expected to become more acid thereby, as proved actually to be the case.

In the first of these two experiments, in order that the air to be exploded should be as free as possible from common air, the globe was first filled with a mixture of dephlogisticated and inflammable air, it was then exhausted, and the air to be exploded let in; by which means, though the globe was not perfectly exhausted, very little common air could be left in it. In the first set of experiments this circumstance was not attended to, and the purity of the dephlogisticated air was forgot to be examined in both sets.

From what has been said there seems the utmost reason to think, that dephlogisticated air is only water deprived of its phlogiston, and that inflammable air, as was before said, is either phlogisticated water, or else pure phlogiston; but in all probability the former.

As Mr. WATT, in a paper lately read before this Society, supposes water to consist of dephlogisticated air and phlogiston deprived of part of their latent heat, whereas I take no notice of the latter circumstance, it may be proper to mention in a few words the reason of this apparent difference betwen us. If there be any such thing as elementary heat, it must be allowed that what Mr. WATT says is true; but by the same rule we ought to say, that the diluted mineral acids consist of the concentrated acids united to water and deprived of part of their latent heat; that solutions of sal ammoniac, and most other other neutral salts, consist of the salt united to water and elementary heat; and a similar language ought to be used in speaking of almost all chemical combinations, as there are very few which are not attended with some increase or diminution of heat. Now I have chosen to avoid this form of speaking, both

both becaufe I think it more likely that there is no fuch thing as elementary heat, and becaufe faying fo in this inftance, without ufing fimilar expreffions in fpeaking of other chemical unions, would be improper, and would lead to falfe ideas; and it may even admit of doubt, whether the doing it in general would not caufe more trouble and perplexity than it is worth.

There is the utmoft reafon to think, that dephlogifticated and phlogifticated-air, as M. LAVOISIER and SCHEELE fuppofe, are quite diftinct fubftances, and not differing only in their degree of phlogiftication; and that common air is a mixture of the two; for if the dephlogifticated air is pretty pure, almoft the whole of it lofes its elafticity by phlogiftication, and, as appears by the foregoing experiments, is turned into water, inftead of being converted into phlogifticated air. In moft of the foregoing experiments, at leaft $\frac{16}{17}$ths of the whole was turned into water; and by treating fome dephlogifticated air with liver of fulphur, I have reduced it to lefs than $\frac{1}{30}$th of its original bulk, and other perfons, I believe, have reduced it to a ftill lefs bulk; fo that there feems the utmoft reafon to fuppofe, that the fmall refiduum which remains after its phlogiftication proceeds only from the impurities mixed with it.

It was juft faid, that fome dephlogifticated air was reduced by liver of fulphur to $\frac{1}{30}$th of its original bulk; the ftandard of this air was 4,8, and confequently the ftandard of perfectly pure dephlogifticated air fhould be very nearly 5; which is a confirmation of the foregoing opinion; for if the ftandard of pure dephlogifticated air is 5, common air muft, according to this opinion, contain one-fifth of it, and therefore ought to lofe one-fifth of its bulk by phlogiftication, which is what it is actually found to lofe.

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From what has been said, it follows, that instead of saying air is phlogisticated or dephlogisticated by any means, it would be more strictly just to say, it is deprived of, or receives, an addition of dephlogisticated air; but as the other expression is convenient, and can scarcely be considered as improper, I shall still frequently make use of it in the remainder of this paper.

There seemed great reason to think, from Dr. PRIESTLEY's experiments, that both the nitrous and vitriolic acids were convertible into dephlogisticated air, as that air is procured in the greatest quantity from substances containing those acids, especially the former. The foregoing experiments, however, seem to shew that no part of the acid is converted into dephlogisticated air, and that their use in preparing it is owing only to the great power which they possess of depriving bodies of their phlogiston. A strong confirmation of this is, that red precipitate, which is one of the substances yielding dephlogisticated air in the greatest quantity, and which is prepared by means of the nitrous acid, contains in reality no acid. This I found by grinding 400 grains of it with spirits of sal ammoniac, and keeping them together for some days in a bottle, taking care to shake them frequently. The red colour of the precipitate was rendered pale, but not entirely destroyed; being then washed with water and filtered, the clear liquor yielded on evaporation not the least ammoniacal salt.

It is natural to think, that if any nitrous acid had been contained in the red precipitate, it would have united to the volatile alkali and have formed ammoniacal nitre, and would have been perceived on evaporation; but in order to determine more certainly whether this would be the case, I dried some of the same solution of quicksilver from which the red precipitate was prepared with a less heat, so that it acquired only an orange colour,

colour, and treated the ſame quantity of it with volatile alkali in the ſame manner as before. It immediately cauſed an efferveſcence, changed the colour to grey, and yielded 52 grains of ammoniacal nitre. There is the utmoſt reaſon to think, therefore, that red precipitate contains no nitrous acid; and conſequently that, in procuring dephlogiſticated air from it, no acid is converted into air; and it is reaſonable to conclude, therefore, that no ſuch change is produced in procuring it from any other ſubſtance.

It remains to conſider in what manner theſe acids act in producing dephlogiſticated air. The way in which the nitrous acid acts, in the production of it from red precipitate, ſeems to be as follows. On diſtilling the mixture of quickſilver and ſpirit of nitre, the acid comes over, loaded with phlogiſton, in the form of nitrous vapour, and continues to do ſo till the remaining matter acquires its full red colour, by which time all the nitrous acid is driven over, but ſome of the watery part ſtill remains behind, and adheres ſtrongly to the quickſilver; ſo that the red precipitate may be conſidered, either as quickſilver deprived of part of its phlogiſton, and united to a certain portion of water, or as quickſilver united to dephlogiſticated air*; after which, on further increaſing the heat, the water in it riſes deprived of its phlogiſton, that is, in the form of dephlogiſticated air,

* Unleſs we were much better acquainted than we are with the manner in which different ſubſtances are united together in compound bodies, it would be ridiculous to ſay, that it is the quickſilver in the red precipitate which is deprived of its phlogiſton, and not the water, or that it is the water and not the quickſilver; all that we can ſay is, that red precipitate conſiſts of quickſilver and water, one or both of which are deprived of part of their phlogiſton. In like manner, during the preparation of the red precipitate, it is certain that the acid abſorbs phlogiſton, either from the quickſilver or the water; but we are by no means authoriſed to ſay from which.

air, and at the same time the quicksilver distils over in its metallic form. It is justly remarked by Dr. PRIESTLEY, that the solution of quicksilver does not begin to yield dephlogisticated air till it acquires its red colour.

Mercurius calcinatus appears to be only quicksilver which has absorbed dephlogisticated air from the atmosphere during its preparation; accordingly, by giving it a sufficient heat, the dephlogisticated air is driven off, and the quicksilver acquires its original form. It seems therefore that mercurius calcinatus and red precipitate, though prepared in a different manner, are very nearly the same thing.

From what has been said it follows, that red precipitate and mercurius calcinatus contain as much phlogiston as the quicksilver they are prepared from; but yet, as uniting dephlogisticated air to a metal comes to the same thing as depriving it of part of its phlogiston and adding water to it, the quicksilver may still be considered as deprived of its phlogiston; but the imperfect metals seem not only to absorb dephlogisticated air during their calcination, but also to be really deprived of part of their phlogiston, as they do not acquire their metallic form by driving off the dephlogisticated air.

In procuring dephlogisticated air from nitre, the acid acts in a different manner, as, upon heating the nitre red-hot, the dephlogisticated air rises mixed with a little nitrous acid, and at the same time the acid remaining in the nitre becomes very much phlogisticated; which shews that the acid absorbs phlogiston from the water in the nitre, and becomes phlogisticated, while the water is thereby turned into dephlogisticated air. On distilling 3155 grains of nitre in an unglazed earthen retort, it yielded 256000 grain measures of dephlogisticated air*, the standard

* This is, about eighty-one grain measures from one grain of nitre; and the weight

ſtandard of different parts of which varied from 3 to 3,65, but at a medium was 3,35. The matter remaining in the retort diſſolved readily in water, and taſted alcaline and cauſtic. On adding diluted ſpirit of nitre to the ſolution, ſtrong red fumes were produced; a ſign that the acid in it was very much phlogiſticated, as no fumes whatever would have been produced on adding the ſame acid to a ſolution of common nitre; that part of the ſolution alſo which was ſuperſaturated with acid became blue; a colour which the diluted nitrous acid is known to aſſume when much phlogiſticated. The ſolution, when ſaturated with this acid, loſt its alcaline and cauſtic taſte, but yet taſted very different from true nitre, ſeeming as if it had been mixed with ſea-ſalt, and alſo required much leſs water to diſſolve it; but on expoſing it for ſome days to the air, and adding freſh acid as faſt as by the flying off of the fumes the alcali predominated, it became true nitre, unmixed, as far as I could perceive, with any other ſalt *.

It has been remarked, that the dephlogiſticated air procured from nitre is leſs pure, than that from red precipitate and many other ſubſtances, which may perhaps proceed from unglazed earthen retorts having been commonly uſed for this purpoſe, and which, conformably to Dr. PRIESTLEY's diſcovery, may poſſibly abſorb ſome common air from without, and emit it along with the dephlogiſticated air; but if it ſhould be found that the dephlogiſticated air procured from nitre in glaſs or glazed earthen veſſels is alſo impure, it would ſeem to ſhew that part

weight of the dephlogiſticated air, ſuppoſing it 800 times lighter than water, is one tenth of that of the nitre. In all probability it would have yielded a much greater quantity of air, if a greater heat had been applied.

* This phlogiſtication of the acid in nitre by heat has been obſerved by Mr. SCHEELE; ſee his experiments on air and fire, p. 45. Engliſh tranſlation.

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of the acid in the nitre is turned into phlogisticated air, by absorbing phlogiston from the watery part.

From what has been said it appears, that there is a considerable difference in the manner in which the acid acts in the production of dephlogisticated air from red precipitate and from nitre; in the former case the acid comes over first, leaving the remaining substance deprived of part of its phlogiston; in the latter the dephlogisticated air comes first, leaving the acid loaded with the phlogiston of the water from which it was formed.

On distilling a mixture of quicksilver and oil of vitriol to dryness, part of the acid comes over, loaded with phlogiston, in the form of volatile sulphureous acid and vitriolic acid air; so that the remaining white mass may be considered as consisting of quicksilver deprived of its phlogiston, and united to a certain proportion of acid and water, or of plain quicksilver united to a certain proportion of acid and dephlogisticated air. Accordingly on urging this white mass with a more violent heat, the dephlogisticated air comes over, and at the same time part of the quicksilver rises in its metallic form, and also part of the white mass, united in all probability to a greater proportion of acid than before, sublimes; so that the rationale of the production of dephlogisticated air from turbith mineral, and from red precipitate, are nearly similar.

True turbith mineral consists of the abovementioned white mass, well washed with water, by which means it acquires a yellow colour, and contains much less acid than the unwashed mass. Accordingly it seems likely, that on exposing this to heat, less of it should sublime without being decompounded, and consequently that more dephlogisticated air should be procured from it than from the unwashed mass.

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This is an inſtance, that the ſuperabundant vitriolic acid may, in ſome caſes, be better extracted from the baſe it is united to by water than by heat. Vitriolated tartar is another inſtance; for, if vitriolated tartar be mixed with oil of vitriol and expoſed even to a pretty ſtrong red heat, the maſs will be very acid; but, if this maſs is diſſolved in water, and evaporated, the cryſtals will be not ſenſibly ſo.

In all probability, the vitriolic acid acts in the ſame manner in the production of dephlogiſticated air from alum, as the nitrous does in its production from nitre; that is, the watery part comes over firſt in the form of dephlogiſticated air, leaving the acid charged with its phlogiſton. Whether this is alſo the caſe with regard to green and blue vitriol, or whether in them the acid does not rather act in the ſame manner as in turbith mineral, I cannot pretend to ſay, but I think the latter more likely.

There is another way by which dephlogiſticated air has been found to be produced in great quantities, namely, the growth of vegetables expoſed to the ſun or day-light; the rationale of which, in all probability, is, that plants, when aſſiſted by the light, deprive part of the water ſucked up by their roots of its phlogiſton, and turn it into dephlogiſticated air, while the phlogiſton uuites to, and forms part of, the ſubſtance of the plant.

There are many circumſtances which ſhew, that light has a remarkable power in enabling one body to abſorb phlogiſton from another. Mr. SENEBIER has obſerved, that the green tincture procured from the leaves of vegetables by ſpirit of wine, quickly loſes its colour when expoſed to the ſun in a bottle not more than one third part full, but does not do ſo in the dark, or if the bottle is quite full of the tincture, or if the air in it is phlogisticated; whence it is natural to conclude, that the light enables the dephlogisticated part of the air to absorb phlogiston from the tincture; and this appears to be really the case, as I find that the air in the bottle is considerably phlogisticated thereby. Dephlogisticated spirit of nitre also acquires a yellow colour, and becomes phlogisticated, by exposure to the sun's rays *; and I find on trial that the air in the bottle in which it is contained becomes dephlogisticated, or, in other words, receives an increase of dephlogisticated air, which shews that the change in the acid is not owing to the sun's rays communicating phlogiston to it, but to their enabling it to absorb phlogiston from the water contained in it, and thereby to produce dephlogisticated air. Mr. SCHEELE also found, that the dark colour acquired by luna cornea on exposure to the light, is owing to part of the silver being revived; and that gold, dissolved in aqua regia and deprived by distillation of the nitrous and superfluous marine acid, is revived by the same means; and there is the utmost reason to think, that, in both cases, the revival of the metal is owing to its absorbing phlogiston from the water.

Vegetables seem to consist almost intirely of fixed and phlogisticated air, united to a large proportion of phlogiston and some water, since by burning in the open air, in which their phlogiston unites to the dephlogisticated part of the atmosphere and forms

* If spirit of nitre is distilled with a very gentle heat, the part which comes over is high coloured and fuming, and that which remains behind is quite colourless, and fumes much less than other nitrous acid of the same strength, and the fumes are colourless. This is called dephlogisticated spirit of nitre, as it appears to be really deprived of phlogiston by the process. The manner of preparing it, as well as its property of regaining its yellow colour by exposure to the light, is mentioned by Mr. SCHEELE in the Stockholm Memoirs, 1774.

water,

water, they ſeem to be reduced almoſt intirely to water and thoſe two kinds of air. Now plants growing in water without earth, can receive nouriſhment only from the water and air, and muſt therefore in all probability abſorb their phlogiſton from the water. It is known alſo that plants growing in the dark do not thrive well, and grow in a very different manner from what they do when expoſed to the light.

From what has been ſaid, it ſeems likely that the uſe of light, in promoting the growth of plants and the production of dephlogiſticated air from them, is, that it enables them to abſorb phlogiſton from the water. To this it may perhaps be objected, that though plants do not thrive well in the dark, yet they do grow, and ſhould therefore, according to this hypotheſis, abſorb water from the atmoſphere, and yield dephlogiſticated air, which they have not been found to do. But we have no proof that they grew at all in any of thoſe caſes in which they were found not to yield dephlogiſticated air; for though they will grow in the dark, yet their vegetative powers may perhaps at firſt be intirely checked by it, eſpecially conſidering the unnatural ſituation in which they muſt be placed in ſuch experiments. Perhaps too plants growing in the dark may be able to abſorb phlogiſton from water not much impregnated with dephlogiſticated air, but not from water ſtrongly impregnated with it; and conſequently, when kept under water in the dark, may perhaps at firſt yield ſome dephlogiſticated air, which, inſtead of riſing to the ſurface, may be abſorbed by the water, and, before the water is ſo much impregnated as to ſuffer any to eſcape, the plant may ceaſe to vegetate, unleſs the water is changed. Unleſs therefore it could be ſhewn that plants growing in the dark, in water alone, will increaſe in ſize, without yielding dephlogiſticated

gisticated air, and without the water becoming more impregnated with it than before, no objection can be drawn from thence.

Mr. SENEBIER finds, that plants yield much more dephlogisticated air in distilled water impregnated with fixed air, than in plain distilled water, which is perfectly conformable to the abovementioned hypothesis; for as fixed air is a principal constituent part of vegetable substances, it is reasonable to suppose that the work of vegetation will go on better in water containing this substance, than in other water.

There are several memoirs of Mr. LAVOISIER published by the Academy of Sciences, in which he intirely discards phlogiston, and explains those phænomena which have been usually attributed to the loss or attraction of that substance, by the absorption or expulsion of dephlogisticated air; and as not only the foregoing experiments, but most other phænomena of nature, seem explicable as well, or nearly as well, upon this as upon the commonly believed principle of phlogiston, it may be proper briefly to mention in what manner I would explain them on this principle, and why I have adhered to the other. In doing this, I shall not conform strictly to his theory, but shall make such additions and alterations as seem to suit it best to the phænomena; the more so, as the foregoing experiments may, perhaps, induce tne author himself to think some such additions proper.

According to this hypothesis, we must suppose, that water consists of inflammable air united to dephlogisticated air; that nitrous air, vitriolic acid air, and the phosphoric acid, are also combinations of phlogisticated air, sulphur, and phosphorus, with dephlogisticated air; and that the two former, by a further addition of the same substance, are reduced to the common nitrous

nitrous and vitriolic acids; that the metallic calces consist of the metals themselves united to the same substance, commonly, however, with a mixture of fixed air; that on exposing the calces of the perfect metals to a sufficient heat, all the dephlogisticated air is driven off, and the calces are restored to their metallic form; but as the calces of the imperfect metals are vitrified by heat, instead of recovering the metallic form, it should seem as if all the dephlogisticated air could not be driven off from them by heat alone. In like manner, according to this hypothesis, the rationale of the production of dephlogisticated air from red precipitate is, that during the solution of the quicksilver in the acid and the subsequent calcination, the acid is decompounded, and quits part of its dephlogisticated air to the quicksilver, whereby it comes over in the form of nitrous air, and leaves the quicksilver behind united to dephlogisticated air, which, by a further increase of heat, is driven off, while the quicksilver re-assumes its metallic form. In procuring dephlogisticated air from nitre, the acid is also decompounded; but with this difference, that it suffers some of its dephlogisticated air to escape, while it remains united to the alkali itself, in the form of phlogisticated nitrous acid. As to the production of dephlogisticated air from plants, it may be said, that vegetable substances consist chiefly of various combinations of three different bases, one of which, when united to dephlogisticated air, forms water, another fixed air, and the third phlogisticated air; and that by means of vegetation each of these substances are decomposed, and yield their dephlogisticated air; and that in burning they again acquire dephlogisticated air, and are restored to their pristine form.

It seems, therefore, from what has been said, as if the phænomena of nature might be explained very well on this principle,

ciple, without the help of phlogiston; and indeed, as adding dephlogisticated air to a body comes to the same thing as depriving it of its phlogiston and adding water to it, and as there are, perhaps, no bodies entirely destitute of water, and as I know no way by which phlogiston can be transferred from one body to another, without leaving it uncertain whether water is not at the same time transferred, it will be very difficult to determine by experiment which of these opinions is the truest; but as the commonly received principle of phlogiston explains all phænomena, at least as well as Mr. LAVOISIER's, I have adhered to that. There is one circumstance also, which though it may appear to many not to have much force, I own has some weight with me; it is, that as plants seem to draw their nourishment almost intirely from water and fixed and phlogisticated air, and are restored back to those substances by burning, it seems reasonable to conclude, that notwithstanding their infinite variety they consist almost intirely of various combinations of water and fixed and phlogisticated air, united according to one of these opinions to phlogiston, and deprived according to the other of dephlogisticated air; so that, according to the latter opinion, the substance of a plant is less compounded than a mixture of those bodies into which it is resolved by burning; and it is more reasonable to look for great variety in the more compound than in the more simple substance.

Another thing which Mr. LAVOISIER endeavours to prove is, that dephlogisticated air is the acidifying principle. From what has been explained it appears, that this is no more than saying, that acids lose their acidity by uniting to phlogiston, which with regard to the nitrous, vitriolic, phosphoric, and arsenical acids, is certainly true. The same thing, I believe, may be said of the acid of sugar; and Mr. LAVOISIER's experiment is a strong

ſtrong confirmation of BERGMAN's opinion, that none of the ſpirit of nitre enters into the compoſition of the acid, but that it only ſerves to deprive the ſugar of part of its phlogiſton. But as to the marine acid and acid of tartar, it does not appear that they are capable of loſing their acidity by any union with phlogiſton. It is to be remarked alſo, that the acids of ſugar and tartar, and in all probability almoſt all the vegetable and animal acids, are by burning reduced to fixed and phlogiſticated air, and water, and therefore contain more phlogiſton, or leſs dephlogiſticated air, than thoſe three ſubſtances.

XIV. *Remarks on Mr.* Cavendish's *Experiments on Air. In a Letter from* Richard Kirwan, *Esq. F. R. S. to Sir* Joseph Banks, *Bart. P. R. S.*

Read Feb. 5, 1784.

SIR,

HAVING liftened with much attention, and derived much ufeful information from the very curious experiments of Mr. CAVENDISH, read at our laft meeting, it is with peculiar regret I feel myfelf withheld from yielding an intire affent to all he has advanced in his very ingenious paper; and it is with ftill greater that I find myfelf obliged, by reafon of the oppofition of fome of his deductions to thofe I had the honour to lay before the fociety about two years ago, to expofe the reafons of my diffent, through your mediation, before this meeting.

In the paper already mentioned, read in April, 1782, I attributed the diminution of refpirable air, obferved in common phlogiftic proceffes, to the generation and abforption of fixed air, which is now known to be an acid, and capable of being abforbed by feveral fubftances. That fixed air was fome how or other produced in phlogiftic proceffes, either by *feparation* or *compofition*, I took for granted from the numerous experiments of Dr. PRIESTLEY; and among thefe I felected, as leaft liable to objection, the Calcination of Metals, the decompofition of nitrous by mixture with respirable air, the phlogistication of respirable air by the electric spark, and, lastly, that effected by amalgamation. In each of these instances Mr. CAVENDISH is of opinion, that the diminution of respirable air is owing to the production of water, which, according to him, is formed by the union of the phlogiston, disengaged in those processes, with the dephlogisticated part of common air; and that fixed air is never produced in phlogistic processes, except some animal or vegetable substance is concerned in the operation, from whose decomposition it may arise. To which of these causes the diminution of respirable air is to be attributed, I shall now endeavour to elucidate.

Of the Calcination of Metals.

I attributed the diminution of air by the calcination of metals, to the conversion of the dephlogisticated part of common air into fixed air, by reason of its union with the phlogiston of the metal; for this plain reason, because I find it acknowledged on all hands, that the calces of all the base metals yield fixed air, when sufficiently heated. Mr. Cavendish allows the fact in general, but ascribes the fixed air found in them to their long exposure to the atmosphere, in which he says fixed air pre-exists; but that it exists in common air in any quantity worth attending to, or is extracted from it in any degree, I take the liberty of denying, grounded on the following facts. First, I have frequently agitated 18 cubic inches of common air in 2 of lime-water, and 2 of common air in 18 of lime-water, but could never perceive the slightest milkiness; and yet the thousandth part of a cubic inch of fixed air would thus be made sensible; for if a cubic inch of it be dissolved in 3 ounces of

 water,

water, a few drops of that water let into lime-water will produce a cloud. Mr. FONTANA says, he frequently agitated 1 cubic inch of Tincture of Turnsole in 7 or 800 of common air, without reddening it (23 ROZ. p. 188.); and yet, according to Mr. BERGMAN, 1 cubic inch of fixed air is sufficient to redden 50 of Tincture of Turnsole (1 BERGM. 11.); from whence I am apt to think, that 700 cubic inches of common air do not even contain $\frac{1}{10}$th of a cubic inch of fixed air. Dr. WHYTT found that 12 ounces of strong lime-water, being exposed to the open air for 19 days, still retained about 1 grain of lime, (on Lime-water, p. 32.). Now 12 ounces of strong lime-water contain at most 9,5 grains of lime, and 1 grain of lime requires only 0,56 of a cubic inch of fixed air to precipitate it, the thermometer at 55 and the barometer at 29,5, as I have found. Therefore in 19 days this lime-water did not come in contact with more than four cubic inches of fixed air; yet it is certain that a large quantity of fixed air is continually disengaged, and thrown into the atmosphere, by various processes, as putrefaction, combustion, &c. but it seems equally certain that it is either decomposed, or more probably absorbed by various bodies. Mr. FONTANA let loose 20000 cubic inches of fixed air, in a room whose windows and doors were closed, yet in half an hour after he could not discover the least trace of it (ibid.). Though fixed air perpetually oozes from the floor of the *Grotto del Cane*, yet at the distance of four or five feet from the ground none is found; animals may live, lights burn, &c. (ROZ. Ibid. Mem. Stockh. 1775.). If distilled water be exposed to the atmosphere, it is never found to absorb fixed air, but rather dephlogisticated air, according to Mr. SCHEELE's experiments, which could never happen if the atmosphere contained any sensible proportion of fixed air; nor has rain-water been ever found to contain any, which it certainly ſhould on the ſame hypotheſis; even Mr. CAVENDISH himſelf could find no fixed air in the reſiduum or products of about 1040 ounce meaſures of common air, which he burnt with inflammable air.

It is true, Dr. PRIESTLEY ſuppoſed common air to contain $\frac{1}{51}$ of its bulk of fixed air; but he drew this concluſion not from any direct experiment, but from the quantity of fixed air produced by breathing, which he at that time believed to have been barely precipitated, and not generated, an opinion which he has found reaſon to alter from his own experiments. I think I may therefore conclude, that the quantity of fixed air contained in the atmoſphere is abſolutely inappreciable.

Secondly, ſuppoſing the atmoſphere to contain a very ſmall proportion of fixed air, yet I do not think it can be inferred that metals, during their calcination, extract any, becauſe I find that lime expoſed to red heat ever ſo long extracts none, though it is formed by a calcination in open air, which laſts at leaſt as long as that of any metal; neither does precipitate *per ſe* attract any, though its calcination laſts ſeveral months; nor does this proceed from the want of affinity, for if a ſaturate ſolution of mercury in any of the acids be precipitated by a mild vegetable alkali, very little effervescence is perceived, and the precipitate weighs much more than the quantity of mercury employed, and that this increaſe of weight ariſes in part from the fixed air abſorbed will preſently be ſeen.

Since then metals may be calcined in cloſe veſſels, ſince they then abſorb one fourth part of the common air to which they are expoſed, ſince all metallic calces (except thoſe of mercury, which I ſhall preſently mention) yield fixed air, ſince common

air

air contains ſcarce any fixed air; is it not apparent that the fixed air thus found was generated by the very act of calcination, by the union of the phlogiſton of the metal with the dephlogiſticated part of the common air, ſince after the operation the metal is deprived of its phlogiſton, and the air of its dephlogiſticated part?

But Mr. CAVENDISH objects, that no one has extracted fixed air from metals calcined in cloſe veſſels. To which I anſwer, that this further proof is difficult, and no way neceſſary; it is difficult, becauſe the operation can eaſily be performed only on ſmall quantities; it is unneceſſary, becauſe it differs from the operation in open air only by the quantities of the materials employed, in every other reſpect it is exactly the ſame. Since Mr. CAVENDISH ſuſpects the reſults are different, it is incumbent on him to ſhew that difference; but until then, according to Sir ISAAC NEWTON's ſecond rule, *to natural effects of the ſame kind the ſame cauſes are to be aſſigned, as far as it may be done*, that is, until experience points out ſome other cauſe.

It may further be urged, that precipitate *per ſe* yields only dephlogiſticated air, that minium alſo yields a large proportion of it. This difficulty I have formerly anſwered by aſſerting, that theſe calces are in fact united only to fixed air, and that they yield dephlogiſticated air, merely becauſe the fixed air is decompoſed by the total or partial revivification of the metallic ſubſtances; this I think may be demonſtrated by the following experiments. Let ſublimate corroſive ſingly be treated in any manner, it will not yield dephlogiſticated air (4 Pr. 240.); but let a ſolution of ſublimate corroſive be precipitated by a mild fixed alkali, this precipitate waſhed, dried, and diſtilled in a pneumatic apparatus, will yield dephlogiſticated air, and the mer-

mercury will be revived; but, if the solution of sublimate corrosive be precipitated by lime-water, it seems no air will be produced. Here then we see, 1st, that the calx of mercury unites with fixed air; and, 2dly, that this fixed air is, during the revivification of the mercury, converted into dephlogisticated air. Again: let one ounce of red precipitate, which, according to Mr. CAVENDISH, contains no nitrous acid, be distilled with two ounces of filings of iron; this quantity of precipitate, which, if distilled by itself, would yield 60 ounce measures of dephlogisticated air, will, when distilled with this proportion of filings of iron, yield 40 ounce measures of fixed air, as Dr. PRIESTLEY has shewn in his last paper: whichever way this is explained, some or other of my opinions are confirmed; for either the mercurial calx is already combined with fixed air (which I believe to be the case), and this air passes undecomposed, because the mercury extracts phlogiston from the iron; or it contains dephlogisticated air, which is converted into fixed air by its union with the phlogiston of the iron.

If precipitate *per se* be digested in marine acid, the mercury will be revived (3 BERGM. 415.). Now this calx does not dephlogisticate the marine acid; for this acid, when dephlogisticated, dissolves mercury; how then does it revive it, if not by expelling the fixed air contained in it, which in the moment of its expulsion is decomposed, leaving its phlogiston to the mercury, which is thereby revived?

Again: if litharge be heated in a gun-barrel, it will afford more fixed and less dephlogisticated air than if heated in glass or earthen vessels. Does not this happen, because the calx of lead, receiving some phlogiston from the metal, does not dephlogisticate so great a proportion of the fixed air as it otherwise would?

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Further: there is no subſtance which yields dephlogiſticated air, but yields alſo fixed air, even precipitate *per ſe* not excepted; (3 PRIEST. 16.) and what is remarkable, they all yield fixed air firſt, and dephlogiſticated air only towards the end of the proceſs. Does not this happen becauſe metallic calces attract phlogiſton ſo much more ſtrongly, as they are more heated? Thus many calciform iron ores become magnetic by calcination, though they were not ſo before; ſo alſo do all the calces of iron when expoſed to the focus of a burning glaſs (5 Dict. Chy. 179). Thus mercury cannot be calcined but in a heat inferior to that in which it boils; thus minium cannot be formed but in a moderate heat, and if heated ſtill more it returns to the ſtate of maſſicot, in which it was before it became minium, and much of it is reduced. So if a ſolution of luna cornea in volatile alkali be triturated with mercury, the ſilver will be revived, and the marine acid unite to the mercury, which ſhews this acid has a ſtronger attraction to Mercury than to ſilver; yet if ſublimate corroſive and ſilver be diſtilled in a ſtrong heat, the mercury will be revived, and the marine acid unite to the ſilver, which ſhews that the attraction of mercury to phlogiſton increaſes with the heat applied.

Before I conclude this head, I will mention another experiment, which I think deciſive in favour of my opinion of the compoſition of fixed air. If filings of zinc be digeſted in a cauſtic fixed alkali in a gentle heat, the zinc will be diſſolved with effervefcence, and the alkali will be rendered in great meaſure mild. But if, inſtead of filings of zinc, flowers of zinc be uſed, and treated in the ſame manner, there will be no ſolution, and the alkali will remain cauſtic. In the firſt caſe the effervefcence ariſes from the production of inflammable air, which

which phlogisticates the common air contiguous to it, and produces fixed air, which is immediately absorbed by the alkali, and renders it mild. In the second case, no inflammable air is produced, the common air is not phlogisticated, and consequently the alkali remains caustic*. This experiment also proves that metallic calces attract fixed air more strongly than alkalies attract it; for the calces of zinc are known to contain fixed air, and yet alkalies digested with them remain caustic; and this accounts for the slight turbidity of lime-water when metals are calcined over it; for as soon as the phlogiston is disengaged from the metal, and before it has absorbed the whole quantity of fire requisite to throw it into the form of inflammable air, it meets with the dephlogisticated part of the common air on the surface of the metal, and there forms fixed air, which is instantly absorbed by the calx with which it is in contact, so that it is not to be wondered that it does not unite to the lime from which it is distant.

Of the Decomposition of Nitrous Air by mixture with Common Air.

AS soon as I had heard Mr. CAVENDISH's paper read, I set about trying whether lime would be precipitated from lime-water during the process, an experiment I had never made before with common air, taking it for granted that it was so, from the repeated experiments of Dr. PRIESTLEY, and indeed of all others who had treated this subject †: and, in effect,

* See Mr. LASSONE's Experiments on zinc. Mem. Par. 1777. p. 7 & 8.

† See 5 Pr. 114, 189; 2 Pr. 218. Font. Recherches Phys. p. 77. 1 Chy. Dic. 374.

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when I made the experiment with nitrous air prepared and confined by the water of my tub, I found lime-water admitted to it instantly precipitated. But after I had read Mr. CAVENDISH's paper, which he had the politeneſs to permit me, and had, according to his direction, received the nitrous air over lime-water, I did not then perceive the leaſt milkineſs after admitting common air. After 12 hours I indeed perceived a whitiſh duſt, on the bottom of the glaſs veſſel in which I made the experiment, which I cannot aſſure to be calcareous; and, on breathing into the lime-water, an evident milkineſs enſued; ſo that I little doubt but the precipitation I obſerved in the firſt experiment aroſe from the decompoſition of the aerial ſelenite contained in the water of the tub. And it is very poſſible that the precipitation of lime, which I perceived ſome years ago on mixing dephlogiſticated air and nitrous air, might have ariſen from the ſame cauſe, or from fixed air pre-contained in the dephlogiſticated, as this laſt had not been waſhed in lime-water: Yet I do not think the failure of this experiment at all concluſive againſt the ſuppoſed production of fixed air on this occaſion, becauſe the quantity of fixed air is ſo ſmall, that it may well be ſuppoſed to unite to the nitrous ſelenite formed in the lime-water. It is well known that a ſmall quantity of fixed air is capable of uniting to all neutral ſalts: thus Dr. PRIESTLEY has extracted it from tartar vitriolate and alum, (2 PR. 115, 116.) and gypſum, (2 PR. 80.); and Dr. MAC BRIDE found it in nitre and common ſalt, though in ſmall quantity. But to try whether nitrous ſelenite would attract any, I made a ſolution of chalk in nitrous acid, which, when ſaturate, weighed 381,25 grains; but, being expoſed to the air for a few hours, it weighed 382,25. I afterwards took a very dilute nitrous acid, in which an acid taſte was barely perceptible, and impregnated it with a very ſmall proportion

portion of fixed air, and then let fall a few drops of it into lime-water; not the smallest cloud was perceived, and yet when I breathed into it afterwards it became milky in a few seconds; so that this experiment is perfectly analogous to that in which nitrous and common air were mixed.

But if nitrous air and common air be mixed over dry mercury, the result is intirely adverse to the opinion of Mr. CAVENDISH, and favourable to mine; for in this case the common air is not at all diminished until water is admitted to it, and the mixture agitated a few minutes, and then the diminution is nearly the same as if the mixture were made over water. Thus when I mixed two cubic inches of common air with one of nitrous air, they occupied the space of two inches and one-eighth, and the surface of the mercury was immediately calcined; which shews that the inch of nitrous air was decomposed, and produced nitrous acid; but the common air was undiminished; and the one eighth of an inch over and above the two inches of common air, proceeded from an addition of new nitrous air, formed by the corrosion of the surface of the mercury. That the common air should remain undiminished is easily explained in my system, because fixed air is formed, which, on this occasion, must remain unabsorbed, at least for a long time, as there is nothing at hand that can immediately receive it; and hence, if water be admitted soon after the mixture of both airs, the diminution will be nearly the same as if the mixture had been originally made over water, though not exactly the same; because the nitrous air, produced by the union of the newly formed nitrous acid with the mercury, is not entirely absorbable by water. But, in Mr. CAVENDISH's hypothesis, the common air should be diminished just as much as if the mixture were made over water; for, according to him, this diminution arises from the conversion of the dephlogisticated part of the common air into water, which water should immediately unite to the nitrous salt of mercury, and leave the common air lessened in its bulk by a portion commensurate to that converted into water, or, if he will not allow the water to have immediately united to the mercurial salt, at least by the difference of the bulk of the water produced, and that of an equal weight of the common air converted into it: but neither happens; for the common air is not at all diminished; nor can he explain, consistently with his system, why the admission of water should immediately produce a diminution in the common air, as, according to him, it contains nothing that can be absorbed. Dr. Priestley has remarked, that if a mixture of both airs be suffered to stand several hours, even the admission of water will produce no diminution. This is owing to two causes; 1st, because a large quantity of nitrous air is produced, by the continued action of the concentrated nitrous acid newly formed; and, 2dly, because the fixed air, on whose absorption the diminution depends, is absorbed by the mercurial salt, as may be inferred from the experiment in 1 Lavoisier, p. 248.

Of the Diminution of Common Air by the Electric Spark.

Of all the instances of the artificial production of fixed air, by the union of phlogiston with the dephlogisticated part of common air, there is none perhaps so convincing, as that exhibited by taking the electric spark through common air, over a solution of litmus, or lime-water; for the common air is diminished one fourth, the litmus reddened, and the lime-water precipitated. Mr. Cavendish indeed attributes the redness of the

the litmus to fixed air; but he thinks it proceeds from a decomposition of some part of the vegetable juice, as all vegetable juices contain fixed air. Yet that such a decomposition does not take place, I think may be inferred from the following reasons: first, if the electric spark be taken through phlogisticated or inflammable air confined by litmus, no redness is produced, the air not being in the least diminished; and, 2dly, if the litmus were decomposed, inflammable air should be produced as well as fixed air; and then there should be an addition of bulk instead of a diminution; but what sets the origin of the fixed air from the phlogistication of the common air beyond all doubt is, that if lime-water be used instead of litmus, the diminution is the same, and the lime is precipitated. Here Mr. CAVENDISH says, the fixed air proceeds either from *some dirt in the tube*; a supposition, which, being neither necessary nor probable, is not admissible; or else *from some combustible matter in the lime*; but lime contains no combustible matter, except perhaps phlogiston, which cannot produce fixed air but by uniting to the common air, according to my supposition; but it is much more probable, that the diminution does not arise from any phlogiston in the lime, as it is exactly the same whether lime-water be used or not; and the lime does not appear to be in the least altered, and in fact contains scarce any phlogiston.

Of the diminution of Common Air, by the Amalgamation of Mercury and Lead.

I attributed this diminution to the phlogistication of the common air by the process of amalgamation, and the consequent production and absorption of fixed air. On this Mr. CAVENDISH

VENDISH observes, "that mercury, fouled by the addition of "lead or tin, deposits a powder which consists in great measure "of the calx of the metal: he found also, that some powder of "this sort contained fixed air; but it is not clear that this air "was produced by the phlogistication of the air in which the "mercury was shaken, as the powder was not prepared on "purpose, but was formed from mercury fouled by having "been used for various purposes, and may therefore contain "other impurities, besides the metallic calx." On this I remark, that Dr. PRIESTLEY did not indeed at first prepare this powder on purpose; but he afterwards did so prepare it (4 PRIEST. p. 148, 149.) and obtained a powder exactly of the same sort; and it is certain that the fixed air found in it proceeded from the common air, both because metallic calces, not formed by amalgamation, will not unite with mercury, as is well known; and because this calx cannot be formed by agitation of the mercury and lead, in phlogisticated, inflammable, or any other air which is not respirable; and the fixed air cannot proceed from any impurity, as mercury will not unite in its running form to any other but metallic substances, which it always partially dephlogisticates, like other menstruums (3 Chy. Dijon, 425.).

Of the Diminution of Respirable Air by Combustion.

Though I have no doubt but the diminution of respirable air, by the combustion of sulphur and phosphorus, proceeds also in great measure from the production and absorption of fixed air, yet I avoided mentioning this operation, as the presence of a stronger acid renders the presence of a weaker impossible to be proved, more especially, as both these acids precipitate lime from lime-water; but the great increase of weight which the

 phos-

phosphoric acid gains is a strong additional inducement to think that it absorbs fixed air. During the combustion of vegetable substances, I think it highly probable that fixed air is formed, both from my own experiments on the combustion of wax candles, and that mentioned in the first volume of Dr. PRIESTLEY's Observations, p. 136; but when inflammable air from metals and dephlogisticated air are fired, as a great diminution takes place, and yet no fixed air is found, I am nearly convinced, by Mr. CAVENDISH's experiments, that water is really produced; nor am I surprized that, in this instance, the union of phlogiston and dephlogisticated air should form a compound very different from that which it forms in other instances of phlogistication, but should rather be led to expect it *a priori*; for in this case the phlogiston is in its most rarefied known state, and unites to dephlogisticated air, the substance to which it has the greatest affinity, in circumstances the most favourable to the closest and most intimate union; for both, in the act of inflammation, are rarefied to the highest degree; both give out their specific fire, the great obstacle to their union, it being by the inflammation converted into *sensible* heat (a circumstance which, in my opinion, constitutes the very essence of flame); the resulting compound having then lost the greatest part of its specific fire, is necessarily reduced, according to Dr. BLACK's theory, into a denser state, which the present experiment shews to be water; whereas, in common cases of combustion, the phlogiston being denser and less divided, unites less intimately with the dephlogisticated part of common air, consequently expels less of its specific fire, and therefore forms less dense compounds, *viz.* fixed and phlogisticated airs; and so much the more, as a great part intirely escapes combustion; but it seems probable

probable that in very strong and bright inflammations, the union is more perfect, and water formed.

Water being then the result of the closest and most intimate union of dephlogisticated air and phlogiston, it seems to me very improbable, that it is ever decomposed by the affinity of any acid to phlogiston, as all the experiments hitherto made seem to prove, that phlogiston has a stronger affinity to dephlogisticated air than to any other substance, except hot metallic calces; and these, in my opinion, are incapable of forming any union with water, except as far as they are saline, but they never can be reduced by it. So also water is incapable of uniting with any more phlogiston, as sulphur is, both being already saturated.

Mr. CAVENDISH is inclined to think, that pure inflammable air is not pure phlogiston, because it does not immediately unite with dephlogisticated air, when both airs are simply mixed with each other; this reason seems to me of no moment, because I see several other substances, that have the strongest affinity to each other, refuse to unite suddenly, or even at all, through the very same cause that dephlogisticated and inflammable airs refuse to unite; *viz.* on account of the specific fire which they contain, and must lose, before such union can take place: thus fixed air will never unite to dry lime, though they be kept ever so long together; thus, if water be poured on the strongest oil of vitriol, they will remain several weeks in contact, without uniting, as I myself have experienced; and yet, in both cases, the specific fire need be expelled only from one of the substances, and not from both: but after a long time they will unite; so also will inflammable and dephlogisticated air, as Dr. PRIESTLEY has discovered since his last publication.

That

That phlogiſticated air ſhould conſiſt of ſuperſaturated nitrous air, I think improbable, as it retains its phlogiſton much more ſtrongly than nitrous air, which, according to the general laws of affinities, it ſhould not, if it contained an exceſs of phlogiſton; and as Dr. Priestley and Mr. Fontana repeatedly aſſure us, they have converted it into common air, by waſhing it in water, in contact with the atmoſphere. I am, *&c.*

London,
Jan. 29, 1784.

R. Kirwan.

XV. *Answer to Mr.* Kirwan's *Remarks upon the Experiments on Air. By* Henry Cavendish, *Esq. F. R. S. and S. A.*

Read March 4, 1784.

IN a paper lately read before this Society, containing many experiments on air, I gave my reasons for supposing that the diminution which respirable air suffers by phlogistication, is not owing either to the generation or separation of fixed air from it; but without any arguments of a personal nature, or which related to any one person who espouses the contrary doctrine more than to another. This being contrary to the opinion maintained by Mr. KIRWAN, he has written a paper in answer to it, which was read on the fifth of February. As I do not like troubling the Society with controversy, I shall take no notice of the arguments used by him, but shall leave them for the reader to form his own judgement of; much less will I endeavour to point out any inconsistencies or false reasonings, should any such have crept into it; but as there are two or three experiments mentioned there, which may perhaps be considered as disagreeing with my opinion, I beg leave to say a few words concerning them.

Mr. DE LASSONE found that filings of zinc, digested in a caustic fixed alkali, were partially dissolved with a small effervescence, and that the alkali was rendered in some meaſure mild. This mildneſs of the alkali Mr. KIRWAN accounts for by ſuppoſing, that the inflammable air, which is ſeparated during the ſolution, and cauſes the effervеſcence, unites to the atmoſpheric air contiguous to it, and thereby generates fixed air, which is abſorbed by the alkali. But, in reality, the only circumſtance from which Mr. DE LASSONE judged the alkali to become mild, was its making ſome effervеſcence when ſaturated with acids; and this effervеſcence is more likely to have proceeded from the expulſion of inflammable air than of fixed air, as it ſeems likely, that the zinc might be more completely deprived of its phlogiſton by the acid than by the alkali.

In the abovementioned paper I ſay, Dr. PRIESTLEY obſerved, that quickſilver fouled by the addition of lead or tin, depoſits a powder by agitation and expoſure to the air, which conſiſts in great meaſure of the calx of the imperfect metal. He found too ſome powder of this kind to contain fixed air; but it muſt be obſerved, that the powder uſed in this experiment was not prepared on purpoſe, but was procured from quickſilver fouled by having been uſed in various experiments, and may therefore have contained other impurities beſides the metallic calces. On this Mr. KIRWAN remarks, that Dr. PRIESTLEY did not at firſt prepare this powder on purpoſe, but he afterwards did ſo prepare it (4 PR. p. 148. and 149.), and obtained a powder exactly of the ſame ſort. It was natural to ſuppoſe from this remark, that Dr. PRIESTLEY muſt have obtained fixed air from the powder prepared on purpoſe, and that I had overlooked the paſſage; but, on turning to the pages referred to, I was ſurpriſed to find that it was otherwiſe, and that Dr. PRIESTLEY not ſo much as hints that he procured fixed air from the powder thus prepared.

With regard to the calcination of metals it may be proper to remark, that this operation is usually performed over the fire, by methods in which they are exposed to the fumes of the burning fuel, and which are so replete with fixed air, that it is not extraordinary, that the metallic calx should, in a short time, absorb a considerable quantity of it; and in particular red lead, which is the calx on which most experiments have been made, is always so prepared. There is another kind of calcination, however, called rusting, which is performed in the open air; but this is so slow an operation, that the rust may easily imbibe a sufficient quantity of fixed air, notwithstanding the small quantity of it usually contained in the atmosphere.

Mr. KIRWAN allows that lime-water is not rendered cloudy by the mixture of nitrous and common air; but contends that this does not prove that fixed air is not generated by the union, as he thinks it may be absorbed by the nitrous selenite produced by the union of the nitrous acid with the lime. This induced me to try how small a quantity of fixed air would be perceived in this experiment. I accordingly repeated it in the same manner as described in my paper, except that I purposely added a little fixed air to the common air, and found that when this addition was $\frac{1}{75}$th of the bulk, or $\frac{1}{50}$th of the weight of the common air, the effect on the lime-water was such as could not possibly have been overlooked in my experiments. But as those who suppose fixed air to be generated by the mixture of nitrous and common air, may object to this manner of trying the experiment, and say, that the quantity of fixed air absorbed by the lime-water was really more than $\frac{1}{75}$th of the bulk of the common air, being equal to that quantity over and

and above the air generated by the mixture, I made another experiment in a different manner; namely, I filled a bottle with lime-water, previoufly mixed with as much nitrous acid as is contained in an equal bulk of nitrous air, and having inverted it into a veffel of the fame, let up into it, in the fame manner as in the above-mentioned experiments, a mixture of common air with $\frac{1}{73}$th of its bulk of fixed air, until it was half full. The event was the fame as before; namely, the cloudinefs produced in the lime-water was fuch that I could not poffibly have overlooked. It muft be obferved, that in this experiment no fixed air could be generated, and a ftill greater proportion of the lime-water was turned into nitrous felenite than in the above-mentioned experiments; fo that we may fafely conclude, that if any fixed air is generated by the mixture of common and nitrous air, it muft be lefs than $\frac{1}{73}$th of the bulk of the common air.

As for the nitrous felenite, it feems not to make the effect of the fixed air at all lefs fenfible, as I found by filling two bottles with common air mixed with $\frac{1}{100}$dth of its bulk of fixed air, and pouring into each of them equal quantities of diluted lime-water; one of thefe portions of lime-water being previoufly diluted with an equal quantity of diftilled water, and the other with the fame quantity of a diluted folution of nitrous felenite, containing about $\frac{1}{400}$dth of its weight of calcareous earth; when I could not perceive that the latter portion of lime-water was rendered at all lefs cloudy than the former. Though the nitrous felenite, however, does not make the effect of the fixed air lefs fenfible, yet the dilution of the lime-water, in confequence of fome of the lime being abforbed by the acid, does; but, I believe, not in any remarkable degree.

There

There is an experiment mentioned by Mr. KIRWAN which, though it cannot be conſidered as an argument in favour of the generation of fixed air, as he only ſuppoſes, without any proof, that fixed air is produced in it, does yet deſerve to be taken notice of as a curious experiment. It is, that, if nitrous and common air be mixed over dry quickſilver, the common air is not at all diminiſhed, that is, the bulk of the mixture will be not leſs than that of the common air employed, until water is admitted, and the mixture agitated for a few minutes. The reaſon of this in all probability is, that part of the phlogiſticated nitrous acid, into which the nitrous air is converted, remains in the ſtate of vapour until condenſed by the addition of water. A proof that this is the real caſe is, that, in this manner of performing the experiment, the red fumes produced on mixing the airs remain viſible for ſome hours, but immediately diſappear on the addition of water and agitation.

The moſt material experiment alledged by Mr. KIRWAN is one of Dr. PRIESTLEY's, in which he obtained fixed air from a mixture of red precipitate and iron filings. This at firſt ſeems really a ſtrong argument in favour of the generation of fixed air; for though plumbago, which is known to conſiſt chiefly of that ſubſtance, has lately been found to be contained in iron, yet one would not have expected it to be decompounded by the red precipitate, eſpecially when the quantity of pure iron in the filings was much more than ſufficient to ſupply the precipitate with phlogiſton. The following experiment, however, ſhews that it was really decompounded; and that the fixed air obtained was not generated, but only ſeparated by means of this decompoſition.

500 grains of red precipitate mixed with 1000 of iron filings yielded, by the aſſiſtance of heat, 7800 grain meaſures of fixed air,

air, besides 2400 of a mixture of dephlogisticated and inflammable air, but chiefly the latter. The same quantity of iron filings, taken from the same parcel, was then dissolved in diluted oil of vitriol, so as to leave only the plumbago and other impurities. These mixed with 500 grains of the same red precipitate, and treated as before, yielded 9200 grain measures of fixed air, and 4200 of dephlogisticated air, of an indifferent quality, but without any sensible mixture of inflammable air. It appears, therefore, that less fixed air was produced when the red precipitate was mixed with the iron filings in substance, than when mixed only with the plumbago and other impurities; which shews, that its production was not owing to the iron itself, which seems to contain no fixed air, but to the plumbago, which contains a great deal. The reason, in all probability, why less fixed air was produced in the first case than the latter is, that in the former more of the plumbago escaped being decompounded by the red precipitate than in the other. It must be observed, however, that the filings used in this experiment were mixed with about $\frac{1}{13}$th of their weight of brass, which was not discovered till they were dissolved in the acid, and which makes the experiment less decisive than it would otherwise be. The quantity of fixed air obtained is also much greater than, according to Mr. BERGMAN's experiment, could be yielded by the plumbago usually contained in 1000 grains of iron; so that though the experiment seems to shew that the fixed air was only produced by the decomposition of the impurities in the filings, yet it certainly ought to be repeated in a more accurate manner.

Before I conclude this paper, it may be proper to sum up the state of the argument on this subject. There are five methods of phlogistication considered by me in my paper on air; namely,

namely, firſt, the calcination of metals, either by themſelves or when amalgamated with quickſilver; ſecondly, the burning of ſulphur or phoſphorus; thirdly, the mixture of nitrous air; fourthly, the exploſion of inflammable air; and, fifthly, the electric ſpark; and Mr. KIRWAN has not pointed out any other which he conſiders as unexceptionable. Now the laſt of theſe I by no means conſider as unexceptionable, as it ſeems much moſt likely, that the phlogiſtication of the air in that experiment is owing to the burning or calcination of ſome ſubſtance contained in the apparatus *. It is true, that I have no proof of it; but there is ſo much probability in the opinion, that till it is proved to be erroneous, no concluſion can be drawn from ſuch experiments in favour of the generation of fixed air. As to the firſt method, or the calcination of metals, there is not the leaſt proof that any fixed air is generated, though we certainly have no direct proof of the contrary; nor did I in my paper inſinuate that we had. The ſame thing may be ſaid of the burning of ſulphur and phoſphorus. As to the mixture of nitrous air, and the combuſtion of inflammable air, it is proved, that if any fixed air is generated, it is ſo ſmall as to elude the niceſt teſt we have. It is certain too, that if it had been ſo much as $\frac{1}{70}$th of the bulk of the common air employed, it would have been perceived in the firſt of theſe methods, and would have been ſenſible in the ſecond though ſtill leſs. So that out of the five methods enumerated, it has been ſhewn, that in two no ſenſible quantity is generated, and not the leaſt proof has been aſſigned that any is in two of the

* In the experiment with the litmus I attribute the fixed air to the burning of the litmus, not decompoſition, as Mr. KIRWAN repreſents it, which is a ſufficient reaſon why no fixed air ſhould be found when the experiment is tried with air in which bodies will not burn.

others; and as to the laſt, good reaſons have been aſſigned for thinking it inconcluſive; and therefore the concluſion drawn by me in the above-mentioned paper ſeems ſufficiently juſtified; namely, that though it is not impoſſible that fixed air may be generated in ſome chemical proceſſes, yet it ſeems certain, that it is not the general effect of phlogiſticating air, and that the diminution of common air by phlogiſtication is by no means owing to the generation or ſeparation of fixed air from it.

XVI. *Reply to Mr.* Cavendiſh's *Anſwer.* *By* Richard Kirwan, *Eſq. F. R. S.*

Read March 18, 1784.

I MEAN to trouble the Society but with a very few words in reply to Mr. CAVENDISH's anſwer, as I conſider the greater part of mine to him as ſtill unanſwered.

In the firſt place, he ſays, that in Mr. LASSONE's experiment the effervеſcence proceeded not from any fixed air in the alkali, but from the further action of the acid on the zinc from which inflammable air was diſengaged. But this could not have happened; for, firſt, the zinc, inſtead of being further acted on by the acid, was precipitated according to Mr. LASSONE's own account (p. 8.); and, ſecondly, the acid was only added by degrees, and undoubtedly would unite to the alkali preferably to the zinc; therefore it was from the alkali, and not from the zinc, that the effervеſcence aroſe.

2dly, With regard to the calcination of lead; though in England the ſmoke and flame may come in contact with the metal, yet in Germany red lead is formed without any communication between them, according to Mr. NOSE, who has given an ample account of this manufactory (p. 86.). Is not lime formed in contact with fuel, flame, and ſmoke? Mr. MACQUER even thinks it probable, that the contact of flame is hurtful to the production of minium (2 Dict. Chy. 639.). Mr. MONNET made minium by melting lead in a cuppel, in ſuch

ſuch a manner that it was impoſſible it could come in contact with the leaſt particle of flame or ſmoke (Mem. Turin. 1769, p. 71.).

Mr. CAVENDISH expreſſes his ſurpriſe at my aſſerting, that the black powder, which Dr. PRIESTLEY formed out of an amalgam of mercury and lead, was exactly the ſame as that out of which he had extracted fixed air; but, I think, I have aſſigned very ſufficient reaſons for my opinion: how far I was right will beſt appear by Dr. PRIESTLEY's own letter, in the hands of the Secretary, of which the following is an extract.

"I certainly imagined the two black powders you write "about to be of the ſame nature, and therefore did not at-"tempt to extract any air from the latter; but immediately "on the receipt of your favour of yeſterday, I diſſolved an "ounce of lead in mercury, and expelling it by agitation, "put the black powder, which weighed near 12 ounces, into "a coated glaſs retort; then applying heat, I got from it about "20 ounce meaſures of very pure fixed air, not $\frac{1}{10}$th of which "remained unabſorbed by water."

Fourthly, it is impoſſible to attribute the fixed air, produced by the diſtillation of red precipitate and filings of iron, to the decompoſition of the plumbago contained in the iron; for the quantity of fixed air produced in Mr. CAVENDISH's own experiment is more than *twice* the weight of the whole quantity of plumbago contained in the quantity of iron he uſed, ſuppoſing the whole of the plumbago to to conſiſt of fixed air, which is not pretended; and more than *eight* times the weight of the quantity of fixed air which plumbago really contains. For Mr. CAVENDISH employed in his experiment 1000 grains of iron and 500 grains of red precipitate, and obtained 7800 grain meaſures of fixed air, which are equal to 30 cubic inches, and weigh 17 grains. Now 100 grains of bar iron contain, according to Mr. BERGMAN, at moſt, two-tenths of a grain of plumbago; and conſequently 1000 grs. of this iron contain but two grains of plumbago; and plumbago, according to Mr. SCHEELE, contains but one-third of its weight of fixed air; ſo that here, ſuppoſing the plumbago to be decompoſed, we can have at moſt but ſeven-tenths of a grain of fixed air, or little more than one cubic inch. If we ſuppoſe the filings to be from ſteel, 1000 grains of ſteel containing eight of plumbago, we may have about 2,5 of fixed air, or about 1,5 cubic inch, and this is the ſtrongeſt ſuppoſition, and the moſt favourable to Mr. CAVENDISH. What ſhall we then ſay, if we conſider that theſe filings were mixed with copper or braſs which contain no plumbago? and, above all, that plumbago cannot be ſuppoſed decompoſable by red precipitate, ſince even the nitrous acid cannot decompoſe it?

5thly, With regard to the power which nitrous ſelenite has of abſorbing fixed air, I muſt allow the experiments of Mr. CAVENDISH to be juſt and agreeable to my own; but it only follows, that when fixed air is in its *naſcent* ſtate, it is more abſorbable. Thus many metallic calces take it from alkalies in its *naſcent* ſtate, though in other circumſtances they will take none.

Laſtly, the permanence of a mixture of nitrous and common air, made over mercury, cannot be attributed to nitrous vapour, as vapour is not elaſtic in cold; beſides, I have often made the mixture without producing any ſuch durable vapour, and this will always happen, when the nitrous air is made from nitrous acid ſufficiently diluted.

XVII. *On a Method of describing the relative Positions and Magnitudes of the Fixed Stars; together with some Astronomical Observations. By the Rev.* Francis Wollaston, *LL.B. F. R. S.*

Read February 5, 1784.

FROM some alterations which have of late years been discovered, in the relative positions and apparent magnitudes of a few of the stars we called fixed, it seems not unreasonable to conclude, that there may be many changes among others of them we little suspect. This thought has led me into a wish, that some method were adopted whereby to detect such motions. The first idea which occurred to me was, to make a proposal to astronomers in general; that each should undertake a *strict* examination of a certain district in the heavens; and, not only by a re-examination of the catalogues hitherto published, but by taking the right ascension and declination of every star in their several allotment, to frame an exact map of it, with a corresponding catalogue; and to communicate their observations to one common centre. This is what I could be glad to see begun. Every astronomer must wish it, and therefore every one should be ready to take his share in it. Such a plan, undertaken with spirit, and carried on gradually with care, would, by the joint labours and emulation of so many astronomers as are now in Europe, produce a celestial Atlas far beyond any thing that has ever yet appeared.

But this would be a work of time, and not within the compass of every one. What I mean now to propose is more immediate; and not out of the reach of any who amuse themselves with viewing the heavenly bodies.

Meridian altitudes and transits can be taken but once in 24 hours; and, though accurate, are therefore tedious. Neither can any re-examination of them be made, but with the same labour as at the first. Equatorial sectors are in the hands of few; and require great skill. Some more general method seemed wanting; to discover variations, which, when detected or only surmised, should be consigned immediately to a more strict investigation.

Turning this in my thoughts, I considered, that the noting down at the time the exact appearance of what one sees, would be far more simple, and shew any alterations in that appearance more readily, than any other method. A Drawing once made would remain, and could be consulted at any future period; and if it were drawn at first with care, a transient review would discover to one, whether any sensible change had taken place since it was last examined. Catalogues, or verbal Descriptions of any kind, could not answer that end so well.

To do this with ease and expedition was then the requisite; and a telescope with a large field, and some proper sub-divisions in it, to direct the eye and assist the judgement, seemed to bid most fair for success.

The following is the method which, after various trials, I have adopted, and think I may now venture to recommend.

To a night-glass, but of DOLLOND's improved construction, which magnifies about six times, and takes in a field of just about as many degrees of a great circle, I have added cross wires, intersecting each other at an angle of 45°. More wires

6 may

may be croſſed in other directions; but I apprehend theſe will be found ſufficient. This teleſcope I mount on a polar axis. One coarſely made, and without any diviſions on its circle of declination, will anſwer this purpoſe, ſince there is no great occaſion for accuracy in that reſpect: but as the heavenly bodies are more readily followed by an equatorial motion of the teleſcope, ſo their relative poſitions are much more eaſily diſcerned when they are looked at conſtantly as in the ſame direction. An horizontal motion, except in the meridian, would be apt to miſlead the judgement. It is ſcarcely neceſſary to add, that the wires muſt ſtand ſo as for one to deſcribe a parallel of the equator nearly. Another will then be a horary circle; and the whole area will be divided into eight equal ſectors.

Thus prepared, the teleſcope is to be pointed to a known ſtar, which is to be brought into the centre or common interſection of all the wires. The relative poſitions of ſuch other ſtars as appear within the field, are to be judged-of by the eye: whether at $\frac{1}{2}$, or $\frac{1}{3}$, or $\frac{1}{4}$ from the centre towards the circumference, or *vice verſâ*; and ſo with regard to the neareſt wire reſpectively. Theſe, as one ſees them, are to be noted down with a black lead pencil upon a large meſſage card held in the hand, upon which a circle, ſimilarly divided, is ready drawn. (One of three inches diameter ſeems moſt convenient.) The motion of the heavenly bodies in ſuch a teleſcope is ſo ſlow, and the noting down of the ſtars ſo quickly done, that there is moſt commonly full time for it without moving the teleſcope. When that is wanted, the principal ſtar is eaſily brought back again into the centre of the field at pleaſure, and the work reſumed. After a little practice, it is aſtoniſhing how near one can come to the truth in this way: and, though neither the right aſcenſions nor the declinations are laid down by by it, nor the diſtances between the ſtars meaſured; yet their *apparent* ſituations being preſerved in black and white, with the day and year, and hour if thought neceſſary, written underneath, each card becomes a regiſter of the then appearance of that ſmall portion of the heavens; which is eaſily re-examined at any time with little more than a tranſient view; and which yet will ſhew on the firſt glance, if there ſhould have happened in it any variation of conſequence. It is obvious, that very delicate obſervations are not to be made in this way.

In order to explain my meaning more fully, a card ſo marked ſhall accompany this paper (ſee tab. V. fig. 1.). What I firſt happened to pitch upon was the conſtellation of Corona Borealis, which then fronted one of my windows; and which I have ſince purſued throughout in this method; making the ſtars α, β, γ, δ, ε, ζ, ϑ, ι, $\varkappa$, π, ϱ, σ, and τ, ſucceſſively central; together with one or two belonging to Bootes, for the ſake of connecting the whole together. Theſe I have transferred ſince on a ſheet of paper, to try how well they would unite into one map; which they have done with very little alteration. A copy of that ſhall alſo be laid before this Society (fig. 2.).

My deſign was, after marking down all ſuch ſtars as are viſible with ſo ſmall a magnifier, to go over the whole again with another teleſcope of a higher power, divided in the ſame way; and after that, with a third and a fourth; ſo as to comprehend every ſtar I could diſcern. That would diſcover ſmaller changes: but it muſt be a work of time, if attempted at all. After ſuch a rough map of the conſtellation is made, the endeavouring to aſcertain the right aſcenſions and declinations of theſe, may perhaps be adviſeable in the next place, rather than ſearching for more.

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In obſerving in this way it is manifeſt, that the places of ſuch ſtars as happen to be under or very near any one of the wires, muſt be more to be depended upon, than of what are in the intermediate ſpaces, eſpecially if towards the edges of the field: ſo alſo what are neareſt to the centre, becauſe better defined, and more within the reach of one wire or another. For this reaſon, different ſtars in the ſame ſet muſt ſucceſſively be made central, or brought towards one of the wires, where any ſuſpicion ariſes of a miſtake, in order to approach nearer to a certainty: but if the ſtand of the teleſcope be tolerably well adjuſted and fixed in its place, that is ſoon done.

In ſuch a glaſs it is very ſeldom that light is wanting ſufficient to diſcern the wires. When an illuminator is required, I find, that for this purpoſe, where you wiſh to ſee every ſmall ſtar you can, a piece of card or white paſte-board, projecting on one ſide beyond the tube, and which may be brought forward occaſionally, is better than one of any other kind. By cutting acroſs a ſmall ſegment of the object-glaſs, it throws a ſufficient light down the tube, though a candle is at a great diſtance; and one may loſe ſight of that falſe glare when one pleaſes, by drawing back the head, and moving the eye a little ſide-ways, and then one ſees the ſmaller ſtars juſt as well as if no illuminator were there.

This then is the method I would recommend to the practical aſtronomer, for becoming acquainted with the appearance of the ſtars, and ſetting a watch over the heavenly motions. After a very few trials, every one would find this eaſy. And if each perſon of every rank among aſtronomers would take a conſtellation or two under his care, the numbers who could undertake it in this way would compenſate for the defects of a plan which cannot aſpire at great accuracy. The labour of

 it,

it, even at firſt, is but little. It has coſt me more time indeed than I ought commonly to allot to mere amuſement; becauſe I had my apparatus to contrive, and ſeveral different and fruitleſs ſchemes to try, before I could ſatisfy myſelf. But a quarter, or at the moſt half, an hour is generally ſufficient for the marking of one pretty full card in this way: and when once the cards are marked, and a general map of the conſtellation is formed, a little time given to it in a fine evening, to examine whether the ſtars on ſuch or ſuch a card remain in their former poſition, is little trouble indeed. Perſeverance is moſt likely to be wanting, and therefore muſt be determined upon; becauſe, after finding things time after time juſt as they were, one's hopes of diſcovering any thing new will ſlacken. But the different ſtate of the air, or of one's own eye, will frequently occaſion a freſh ſtar to become viſible, or a ſmall one which had been noted down to ſeem to have diſappeared; and ſuch a mere accident will ſerve to re-kindle the deſire of purſuing it. Beſides, if we obſerve no change after a tolerable interval of aſſiduous ſearch, we may at any time turn to another conſtellation: yet ought we never to abandon the former entirely, after having once publickly undertaken it, without giving notice of our ſo doing.

In the cards or maps, it may be obſerved, I have not marked the reſpective ſizes of the ſtars. Nor have I diſtinguiſhed them in any way, excepting a few of them with BAYER's Greek letters. It was becauſe I have not hitherto ſatisfied myſelf how to do it. Some method muſt be uſed by every one, to deſcribe to himſelf what he means; but, in laying any thing before the public, a deference ought to be paid to what has been done by others. The calling any ſtar by a new name would breed confuſion: and as I was deſirous this ſhould appear before this

Society

Society in its first rude form, that a judgement might be made from it how far such a scheme would promise success, I was unwilling to look into catalogues or capital maps for the numbers or names of the stars, lest I should be tempted to adapt the positions of what I had observed to what I there found set down by more able astronomers. Nothing, therefore, but a hemisphere of SENEX has been consulted, just for knowing how far the constellation is usually reckoned to extend, and what are BAYER's references.

Should this plan meet with approbation, I shall be happy to have proposed it; and will endeavour to forward it in any way that shall be judged proper: or should any other be preferred, which is within the abilities and leisure of one who is engaged in another profession, I shall be as happy to lend what assistance I can to it. My aim is only, to render such observations as I am capable of making, useful to science.

Before I conclude on this head, give me leave to add a few hints. Whether this method be followed, or any other, if a *general plan* be set on foot, whoever undertakes a constellation, or district, should determine to examine it with as great accuracy as he can; yet never be ashamed to let others know of his mistakes. The error of one proves a caution to another. Such a rough sketch, once made, will be found of great use to most of us, in knowing which star next to examine with greater care. He who can do no more than this, will do a useful work by going thus far: and his frequently sweeping over his district in this way, may lead him to a discovery which might escape a more regular astronomer. But whoever can, ought to do more. By degrees the exact positions of every star he has noted down may be ascertained, by the method practised by Mr. DE LA CAILLE in his Southern Hemisphere, or by any

 other

other which shall be esteemed more convenient. Every one, indeed, must use such instruments as he can procure: but assiduity can do more with indifferent ones, than will ever be accomplished with the very best without it. Whatever references are made for one's own convenience, when a map and catalogue are given to the public stock, the old letters and numbers should be retained as far as they go: though yet notice should be taken, where the magnitudes of the stars at present do not appear to correspond with the order in which they have been laid down.

To render this more complete, it were to be wished, that each should give in a copy of his original observations, with an account of the instruments he used; since they ought to be preserved as data from whence his deductions were made, which may then be re-examined at any future time. Yet must it be desired, that no one would trust himself without carrying on his calculations as fast as the observations are made: they will otherwise multiply upon his hands till the labour will dishearten him from attempting it at all. A heap of crude, undigested observations would be an unwelcome present to the public.

Having thus stated this Proposal, I shall leave it to be proceeded upon, or not, as shall be seen proper: And will now only subjoin a List of such occasional observations as I have had opportunity of making, since the last which I communicated to this Society. I find, indeed, that it is much longer than I had apprehended: but as I perceive some astronomers abroad have referred to a few of those which have been honoured with a place in our Transactions, it may be as well to follow it up. An observation retained among one's own private papers I hold to be of little use.

One

One thing let me desire Foreigners to remark: that the registers I gave of the going of my clock were meant only as the relations of a *mere fact*; that a clock, of such a construction, kept or altered its rate *so* or *so*. They seem to have understood it as an account of a capital clock, by valuing themselves upon some of theirs going better. The time-keepers in most of our Observatories are far more accurate; but, excepting those of the Royal Observatory at Greenwich, their accuracy is not made public.

Another remark it may also be proper to make; that, since my former papers, the longitude of this place has been ascertained by comparative observations on the bursting of some rockets, let off on purpose; which, on a mean of several, turns out to be 19″,02 in time E. of Greenwich Observatory; that is, it may hereafter be considered as 19″, instead of 18″,6 as I had before calculated it trigonometrically from the bearings.

Observations

Obſervations made at Chiſlehurſt, *in* Kent, *longitude* 19″ *in time Eaſt of the Royal Obſervatory at* Greenwich, and *latitude* 51° 24′ 33″ *North.*

Eclipſe of the moon, ♂ July 30, 1776: obſerved with a 3½ feet achromatic teleſcope, and a power magnifying 29 times (that is, a ſingle eye-glaſs belonging to the day-tube) the aperture of the teleſcope being reduced to 1½ inches. The night very clear and ſtill.

Apparent time. h.	′	″	
			The beginning not properly obſerved.
10	11	31	Grimaldus touched by the ſhadow.
10	12	49	———— covered.
10	14	5	Galilæus covered.
10	19	36	Ariſtarchus covered.
10	26	0	The ſpot in Kepler biſected.
10	24	25	Schikardus (but Q.) touched.
10	25	52	- - - - - - biſected.
10	27	19	- - - - - - covered.
10	28	15	Copernicus touched.
10	29	49	- - covered.
10	31	22	Helicon (but Q.) covered.
10	37	9	Plato touched.
10	37	54+	- covered.
10	38	55	Tycho touched.
10	39	39	- - biſected.
10	40	25	- - covered.

Manilius

Apparent time.			
h.	′	″	
10	43	16	Manilius covered.
10	46	51	Menelaus covered.
10	48	5	Dionysius covered.
10	55	4	Censorinus covered.
10	58	57	A point (Promontorium acutum, I believe) touched.
11	0	21	A spot between M. Fœcunditatis and M. Nectaris touched.
11	0	23	M. Crisium touched.
11	3	55	- - - covered.
11	7	57	The eclipse seemingly total.
11	11	11	The moon covers a small star near her south limb. The star hangs on the limb, before it disappears.
11	28	17	She covers another star a little south of her centre. This vanishes instantaneously.
			These occultations were observed with another power of the same telescope; which is usually reckoned 100, and which I have formerly so called; but which on an accurate examination really magnifies almost 75 times.
			The emersions of these stars were not observed.
12	43	0	I judge the beginning of the emersion to be about this time; but cannot be certain.
12	48	1	Grimaldus quitted by the shadow.
12	58	25	Aristarchus quitted.
12	59	22	Kepler bisected.
13	0	15	Tycho begins to emerge.
13	1	9	- - bisected.
13	1	53	- - emerges. Till this time I had used the whole aperture (3,6) having forgotten to reduce

Apparent time.			
h.	′	″	
			it, till the moon's brightneſs reminded me. Same power as at firſt; that is, 29.
13	6	51	Copernicus begins to emerge.
13	7	20	- - - ſeemingly biſected.
13	8	19	- - - emerges.
13	10	27	Helicon emerges.
13	15	26	Plato begins to emerge.
13	16	31	- - emerges.
13	21	30	Manilius emerges.
13	23	54	Dionyſius emerges.
13	24	57	Menelaus emerges.
13	29	47	Cenſorinus emerges.
13	31	21	The ſpot by M. Fœcunditatis emerges.
13	35	31	The point of Prom. Acutum emerges.
13	37	21 +	M. Criſium begins to emerge.
13	40	26	- - - quitted by the ſhadow.
13	42	0	The end of the eclipſe.

The air was very clear and ſtill the whole time: the ſhadow but ill defined. Indeed, it was little more than a penumbra; the principal ſpots remaining always viſible on the moon's duſky face.

Eclipſe of the ſun ☿ June 24, 1778: obſerved with a 3½ feet achromatic teleſcope magnifying 75 times. The aperture reduced to two inches, to prevent breaking the ſmoked glaſſes.

3. 41. 33,5 Beginning. I ſuſpect the minute to be miſtaken, and that it ſhould be 3 h. 40′ 33″,5. The firſt

2 impreſſion

Apparent time. h.	′	″	
			impreſſion could not be 2″, I believe not 1″, before I obſerved it.
5	25	24	End. An undulation on the ſun's limb; but the obſervation pretty good.

Eclipſe of the moon ♂ November 23, 1779: obſerved with the ſame teleſcope, magnifying 75 times. The aperture reduced to two inches. Night clear and froſty. No wind.

			The beginning not aſcertained.
6	13	19	Grimaldus touched by the ſhadow.
6	13	28	- - - covered.
6	17	29	Ariſtarchus covered.
6	20	46	Kepler biſected.
6	23	40	M. Humorum touched.
6	27	47	Helicon covered.
6	28	40	Copernicus and Timocharis both biſected.
6	29	57	M. Humorum covered.
6	33	50	Plato touched.
6	34	27	- - covered.
6	41	52	Tycho touched.
6	43	8	- - covered.
6	47	11	Plinius (but Q.) covered.
6	59	1	M. Criſium touched.
7	3	16	- - - covered.
7	7	31	The eclipſe total.
8	46	23	Moon's edge begins to emerge.
8	51	14	Grimaldus begins.
8	52	1	- - - emerges.
			A haze comes on.

Apparent time.

h.	′	″	
9	2	23	:: Kepler bisected. This not clearly seen.
9	11	41	Plato begins to emerge.
9	12	35	- - emerges.
9	13	46	Tycho emerged.
			The haze comes on again too much for the observation to be pursued any farther.

Eclipse of the sun ♂ Oct. 16, 1781: observed with the same telescope and magnifying power.

			The beginning not visible; sun too low.
20	22	13,5	The end. Good.

Eclipse of the Moon ☿ Sept. 10, 1783: observed with the same telescope, *viz.* 3½ feet achromatic, with the aperture reduced to two inches; but with a small magnifying power of 36 times, which I had made by Mr. DOLLOND for these observations, and which I found very convenient. Night a little hazy, but pretty favourable.

9	33	0	A duskiness comes on the moon.
9	45	35	The beginning of the shadow, I believe.
9	47	20	A haziness obscures the moon.
9	50	55	Aristarchus covered.
9	52	20	Kepler covered. So it is set down; but I do not recollect what I meant by this; whether it might not be only the spot in the centre, so that it might more properly be called bisected.

Gassendus

Apparent time. h.	′	″	
9	57	57	Gassendus covered. I suspect the minute here; and that it should be 56′ 57″.
9	59	41	Heraclides covered.
10	1	42	Copernicus touched.
10	3	5	- - - covered.
10	3	26	Helicon covered.
10	4	12	Bulialdus covered.
10	8	0	A haziness again.
10	8	57	Plato covered.
10	15	30	Manilius covered.
10	15	54	Tycho touched.
10	17	5 ::	- - covered. This doubtful.
10	19	10	Menelaus covered.
10	21	38	Dionysius covered.
10	22	40	Plinius covered.
			A haziness again.
10	28	25	Censorinus covered.
10	34	34	M. Crisium touched.
10	39	45	- - - covered.
10	46	34	Total darkness, as I judged it.
			At 10 h. 41′ the moon had grown reddish, and the eclipsed part become more visible than before. After some time, during the total darkness, the moon was barely to be seen. In general, about the centre, it was darker than towards the circumference, which was ill-defined. About
12	0	0	The eastern limb became more visible, and better defined.

The

Apparent time. h.	′	″	
12	14	0	The light ſpreads a great way over the moon from that ſide towards the centre, extending about two-thirds of her circumference (ſee fig. 3.)
12	23	0	The moon ſeems beginning to emerge.
12	25	0	Emerſion certainly has begun.
12	28	21	Grimaldus emerged.
12	31	40	Galileus emerged.
12	33	52	Ariſtarchus emerged.
12	37	26	Kepler (but Q. this as before).
12	39	36	Heraclides emerged.
12	42	56	Helicon emerged.
12	45	52	Copernicus emerged entirely.
12	47	22	Plato begins to emerge.
12	47	58	- - emerges.
12	48	30	Tycho begins to emerge.
12	49	58	- - emerges.
12	58	8	Manilius emerges.
13	1	40	Menelaus emerges.
13	3	18	Dionyſius emerges.
13	5	40	Plinius emerges.
13	11	22	Cenſorinus (but Q.) emerges.
13	16	35	M. Criſium begins to emerge.
13	20	53	- - - emerges.
13	25	38	The ſhadow quits the moon near Langrenus, between that and M. Criſium. The duſkineſs does not leave the moon till ſome time afterwards, but I did not wait to obſerve it.

The moon was darker during the eclipſe than uſual; but the air was not clear enough for any occultations of ſtars to be obſerved.

Tranſit

Tranſit of Mercury over the ſun's diſk ☿ Nov. 12, 1782: obſerved with the ſame teleſcope, and a power of 75 times. The aperture reduced to two inches.

Apparent time. h.	′	″	
2	51	49	Firſt impreſſion obſerved. It could not be 2″ ſooner.
2	54	57	Thread of light completed; but ſeen through clouds. The planet ſeemed to hang on the ſun's limb 30″ at leaſt.
4	6	0	Through a break in the clouds, of ſhort duration, ☿ ſeemed to have quitted the ſun; but indeed the clouds were very unfavourable the whole time.

Occultation of Saturn by the moon, ♄ February 18, 1775: obſerved with the ſame teleſcope; and, I believe, the ſame power, with the whole aperture of the object-glaſs 3,6 inches; but, I perceive, I have not ſet down theſe particulars.

9	5	39	Præc. anſa of the ring im.
9	6	9	Præc. limb of the planet im. Subſequent limb not ſet down.
9	6	48	Subſequent anſa im. The moon low at theſe immerſions, and much undulation. The emerſions loſt by looking at a wrong part of the moon's diſk, except
10	1	7	Subſequent anſa emerges. Night very clear; but the obſervation on the whole imperfect.

Occultations

Occultations of ſtars by the moon: obſerved with the ſame teleſcope, and a power of 75 times, with the whole aperture of the object-glaſs.

		Apparent time. h.	′	″	
1775.					
♂ Aug. 1.	☽ γ Virginis	7	48	17	Both ſtars viſible when a cloud covered them.
		7	49	20	A ſhort break; only one ſtar viſible.
		7	52	15	Another break; but before this the ſecond ſtar was immerged.
		8	48	58,5	Firſt ✱ em. good.
		8	49	6,5	Second ✱ em. good.
	☽ a bright ✱ N of γ Virginis	8	54	13	Im. good.
					Em. not till the moon was too low.
♂ Dec. 12.	☽ Regulus	10	5	46	Em. very good, though the moon low.
1776.					
☉ June 30.	☉ 1 ad μ ♐	9	3	49	Im good; ſome flying clouds.
		10	6	38	Em.; perhaps ſooner.
1777.					
♄ Aug. 23.	☽ μ Ceti	10	41	17	Im.: the moon low; night clear and ſtill.
		11	32	10	Em.
♄ Nov. 15.	☽ 1 ad δ Tauri				Im. not ſeen; undulation too great.
		7	22	56	Em. pretty good.
☉ Nov. 16.	☽ ζ Tauri	11	17	1,5	Im. good. } Theſe were obſerved with a power of 67 times, and an oblique ſpeculum.
		12	23	28	Em. good. }
1783.					
♀ May 16.	☽ π Scorpii	11	21	49	Im. } Night clear and ſtill; the obſervations good.
		12	31	49,5	Em. }
♃ Jul 10.	☽ π Scorpii				Im. not ſeen for clouds.
		8	43	56	Em.; it might be 1″ or 2″ ſooner; the moon's edge ill defined.
♂ Dec. 30.	☽ δ Piſcium	8	3	13	Im. dark limb, very good.
		9	8	30	Em. good. It could not be above 1″ ſooner, if that. Night very clear and ſtill; hard froſt; therm. 13°½.

Eclipses of Jupiter's satellites: observed with the same telescope and power (that is, 75 times; called usually 100) and whole aperture.

		Apparent time. h.	′	″	
1775.					
♀ Sept. 8.	1 Sat.	11	33	14	Im. flying clouds; observation doubtful.
☉ Oct. 1.	1 Sat.	11	51	1	Im. good; unless the minute be mistaken.
♃ Nov. 2.	1 Sat.	8	28	2	Im. good.
♃ 16.	2 Sat.	9	0	13	Im. pretty good; air clear, but a cold in my eyes rendered the observation not satisfactory.
☽ Dec. 18.	1 Sat.	10	45	48	Em. good.
	2 Sat.	11	2	0	Em. pretty good.
☿ 27.	1 Sat.	7	3	48	Em. good.
1776.					
☉ Nov. 17.	3 Sat.	9	38	48,5	Im.; a scintillation for some seconds before it quite disappeared.
1778.					
♃ May 21.	1 Sat.	9	9	38	Em. good.
	2 Sat.	10	10	±	Em. so near the first satellite; as scarcely to be distinguishable from it for some minutes.
♃ June 11.	4 Sat.	9	52	4	Im. good for the fourth satellite, yet visible by fits for some seconds longer.
♄ 13.	1 Sat.	9	19	6	Em. pretty good.
1779.					
♂ Mar. 9.	1 Sat.	6	59	19	Im.; that is, this was the last of my seeing it; but, though the night was clear, the satellite was too near Jupiter for the observation to be satisfactory.
♄ May 22.	2 Sat.	11	5	54	Em. good.
1781.					
♃ May 24.	1 Sat.	10	3	31	Em. very good.
♃ 31.	1 Sat.	11	57	35	Em. pretty good.
♄ June 16.	1 Sat.	10	13	13	Em.; clouds, but pretty good.
1782.					
♄ July 20.	3 Sat.	9	6	42	Em. good.
	2 Sat.	11	30	30	Em. good.

Emersion

		Apparent time. h. ′ ″	
☉ July 21.	1 Sat.	9 39 50	Emerſion; windy; but good.
♃ Aug. 29.	1 Sat.	8 20 15,5	Em.
♀ 30.	4 Sat.	8 52 19	Em.; ſatellite ſeen then, but not diſtinct for ſome time.
1783.			
♂ July 8.	1 Sat.	12 14 13	Im. pretty good.
♄ Aug. 2.	1 Sat.	9 10 31,5	Em. good.
☽ 25.	1 Sat.	9 28 54	Em.
♀ Sept. 26	1 Sat.	6 19 44	Em. pretty good, but twilight ſtrong.
♂ 30.	3 Sat.	10 3 24	Im. It was viſible only by fits for the laſt 8″. Jupiter near a tree.
♀ Oct. 3.	1 Sat.	8 18 0	Em. pretty good; but the moon below Jupiter.
☉ 26.	1 Sat.	8 39 17	Em. Jupiter low and near a tree; great undulation.

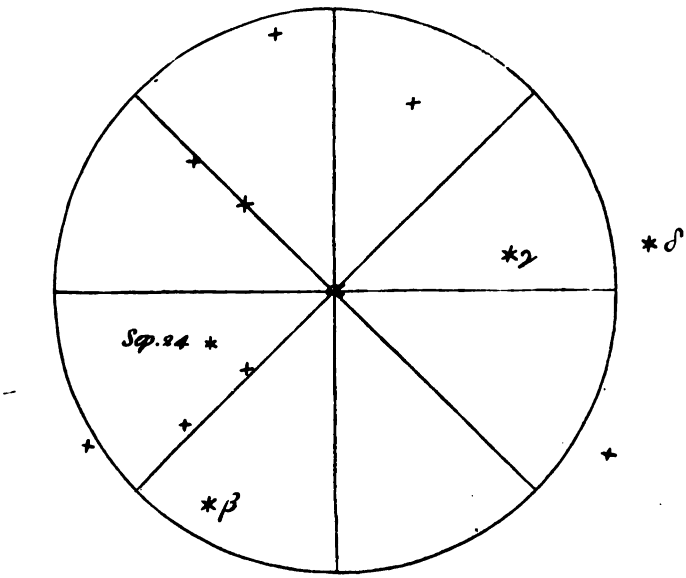
EXPLANATION OF THE FIGURES IN TAB. V.

Fig. 1. α Cor. Bor. ☿ Aug. 6, 1783, per night-glaſs. The ✻ marked Sept. 24. was not obſerved till that night, but has continued ſince, and was only overlooked at firſt.

Fig. 2. A map of 107 ſtars, beſides thoſe marked by BAYER, in the conſtellation of Corona Borealis, or the Northern Crown; together with a part of Bootes: laid down from obſervations made 1783 with a night-glaſs furniſhed with croſs-wires; as their relative poſitions were eſtimated by the eye.

Fig. 3. The moon as ſhe appeared (inverted) ☿ Sept. 10, 1783, about a quarter of an hour before ſhe began to emerge from total darkneſs.

Fig. 1.



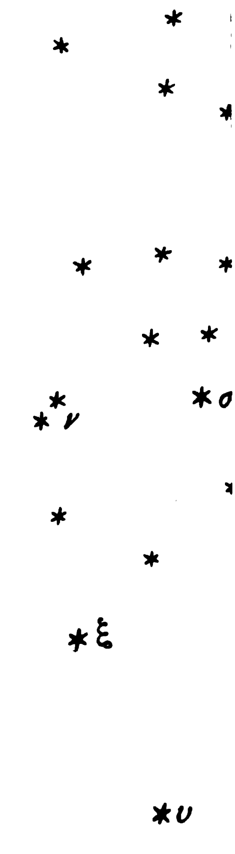
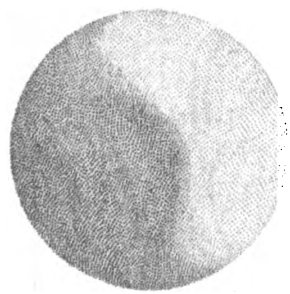


Fig. 3.



XVIII. *An Account of some late fiery Meteors; with Observations. In a Letter from* Charles Blagden, *M. D. Physician to the Army, Sec. R. S. to Sir* Joseph Banks, *Bart. P. R. S.*

Read February 19, 1784.

TO SIR JOSEPH BANKS, BART. P. R. S.

DEAR SIR,

FROM the papers you were so good as to put into my hands, together with such other information as I could procure, the following account of the two most remarkable of the late meteors is collected. I am sensible, that it is in many respects very imperfect; yet still it gives a more satisfactory idea of the phænomena than can well be acquired from the relation of any single observer, and therefore may not be disagreeable to the learned Society over which you so worthily preside, if no more perfect account shall previously have been laid before them.

These meteors were of the kind known to the ancients by the names of Λαμπαδες, Πιθοι, Bolides, Faces, Globi, &c. from particular differences in their shape and appearance, and sometimes, I believe, under the general term of Comets *; in the Philosophical

* ARISTOTLE's remark, that all the comets seen among them disappeared without setting, Ἁπαντες οἱ καθ' ἡμας ωμμενοι (Κομηται) ανευ δυσεως ηφανισθησαν εν τω υπερ του ὁριζοντος τοπω (Meteor. lib. I. c. 6.), seems scarcely applicable but to transitory meteors; and many other expressions to the same purpose occur in that author, PLINY,

Philosophical Transactions they are called indiscriminately fire-balls or fiery meteors; and names of a similar import have been applied to them in the different languages of Europe.

The most material circumstances observed of such meteors may be brought under the following heads: 1. Their general appearance. 2. Their path. 3. Their shape or figure. 4. Their light and colours. 5. Their height. 6. Their noise. 7. Their size. 8. Their duration. 9. Their velocity.

I shall begin with the first of these meteors, that which was seen on the 18th of August.

§ 1. Its general appearance in these parts of Great Britain was that of a luminous ball, which rose in the N.N.W. nearly round, became elliptical and gradually assumed a tail as it ascended, and in a certain part of its course seemed to undergo a remarkable change compared to bursting; after which it proceeded no longer as an entire mass, but was apparently divided into a great number or a cluster of balls, some larger than the others, and all carrying a tail or leaving a train behind; under this form it continued its course with a nearly equable motion, dropping or casting off sparks, and yielding a prodigious light, which illuminated all objects to a surprising degree; till having passed the east, and verging considerably to the southward, it gradually descended, and at length was lost out of sight. The

PLINY, &c. SENECA himself, though he conceived distinctly enough the difference between comets and fiery meteors, yet evidently did not know where to draw the line (compare lib. I. and VII. Quæst. Natur.). Even in modern times, these meteors have struck spectators at first as comets (Fritzes Medizinische Annalen, vol. I. p. 77.); nay, expert astronomers, as appears by a letter from NATHANIEL PIGOTT, Esq. F. R. S. lately read before the Royal Society. See also Mem. de l'Ac. des Scienc. 1771, p. 688. I have insisted the more on a subject apparently of so little consequence, in order to account for the strange opinions of the ancients respecting comets, which, I think, proceeded chiefly from confounding them with these fiery meteors.

time

time of its appearance was 9 h. 16′ P.M. mean time of the meridian of London, and it continued visible about half a minute.

§ 2. How far north the meteor may have begun I have no materials to determine with precision; but, as it was seen in Shetland, and at sea between the Lewes and Fort William, and appeared to persons at Aberdeen and Blair in Athol ascending from the northward, and to an observer in Edinburgh as rising like the planet Mars, there can be little doubt but its course commenced beyond the farthest extremity of this island, somewhere over the northern ocean. General MURRAY F. R. S. being then at Athol House, saw it pass over his head as nearly vertical as he could judge, tracing it from about 45° of elevation north-north-westward to 30° or 20° south-south-eastward, where a range of buildings intercepted it from his view. From near the zenith of Athol House, it passed on a little westward of Perth, and probably a little eastward of Edinburgh; and continuing its progress over the south of Scotland, and the western parts of Northumberland and the Bishopric of Durham, proceeded almost through the middle of Yorkshire, leaving the capital of that county somewhat to the eastward. Hitherto its path was as nearly S.S.E. as can be ascertained; but somewhere near the borders of Yorkshire, or in Lincolnshire, it appears to have gradually deviated to the eastward, and in the course of that deviation to have suffered the remarkable change already noticed under the denomination of bursting. After this division, the compact cluster of smaller meteors seems to have moved for some time almost S.E. thus traversing Cambridgeshire and perhaps the western confines of Suffolk; but gradually recovering its original direction, it proceeded over Essex and the Straits of Dover, entering the continent probably not

 far

far from Dunkirk, where, as well as at Calais and Oftend, it was thought to be vertical. Afterwards it was feen at Bruffels, Paris, and Nuits in Burgundy *, ftill holding on its courfe to the fouthward; nay, I have met with an intimation, though of doubtful authority †, that it was perceived at Rome. Our information of its progrefs over the continent is, indeed, very defective and obfcure; neverthelefs, I think, we have fufficient proof that it traverfed in all 13 or 14 degrees of latitude, defcribing a track of 1000 miles at leaft over the furface of the earth; a length of courfe far exceeding the utmoft that has been hitherto afcertained of any fimilar phænomenon.

To adduce the different accounts from which this path is determined, would not only be infufferably tedious, but contrary to the intention of this letter, which is to give a fummary view of the whole. They are contained partly in letters, and partly in the different news-papers of England and Scotland, moft of which have been perufed for this purpofe. The information derived from the news-papers, however incorrect in the detail, is brought to fome degree of certainty by the check of comparing them with one another; and their frequent publication in moft places of confequence in this ifland, procures us advantages on the occafion of fuch extraordinary phænomena, not enjoyed in former ages, nor even now, to the like extent, in any other part of the world.

It feems fcarcely more interefting to trace the path of thefe bodies with minute precifion, than it would be to mark the progrefs of a cloud's fhadow upon the ground; but it is of confequence to their theory to afcertain well the direction of their courfe; and their deviations from a ftraight line, as implying fome particular caufe, fhould be carefully noticed. I

* Journ. de Paris, Août 24, 1783.

† PARKER's General Advertifer, October 7, 1783.

have

have ventured to afcribe fuch a deviation to this meteor, from the concurrent teftimony of many obfervers, who fpeak in the plaineft terms of a manifeft change in its courfe about the time it was feen to burft; and their evidence is confirmed by drawing a line S.S.E. from that part of Scotland to which the meteor was vertical, for fuch a line is found to correfpond with its path as far as Yorkfhire, but in the fouthern parts of the kingdom falls a great deal too much to the weftward. That it afterwards refumed its former courfe is rendered probable from the teftimony of the obfervers in Kent, who almoft uniformly mention its difappearance in the S.S.E. as well as from the remarks made by feveral perfons near the metropolis, that when it attained its greateft elevation, it bore but one or two points to the northward of eaft.

§ 5. This meteor was defcribed by moft fpectators under three different forms, and is thus reprefented by Mr. SANDBY in his beautiful Drawing *; but the two firft of thofe do not imply any real variation in its fhape, depending only on a difference in the point of view. Accordingly, in the firft part of its courfe over Scotland, it was feen to have a tail, and is thus defcribed by General MURRAY when it paffed Athol Houfe. Two caufes concur in this deception; firft, the fore-fhortening, and even occultation, of the tail, when the object is feen nearly in front; and, fecondly, that the light of moft part of the tail is of fo inferior a kind, as to be difficultly perceived at a great diftance, efpecially when the eye is dazzled by the overpowering brilliancy of the body. The length and fhape of the tail, however, were perpetually varying; nor did the body continue always of the fame magnitude and figure, but was fometimes round, at other times elliptical, with a blunt or

* Since engraved. See alfo the figures tab. IV. of this volume.

pointed

pointed protuberance behind. From such changes of figure in this and other meteors it is, that they have been compared to columns or pyramids of fire, comets, barrels, bottles, flasks, paper-kites, trumpets, tadpoles, glass-drops, quoits, torches, javelins, goats, and many similar objects; whence the multifarious appellations given to them by the ancients were borrowed.

Respecting the tails of meteors, it is here necessary to distinguish between two different parts of which they consist. The brightest portion seems to be of the same nature as the body, and indeed an elongation of the matter composing it; but the other, and that commonly the largest portion, might more properly be called the train, appearing to be a matter left behind after the meteor has passed; it is far less luminous than the former part, and often only of a dull or dusky red colour. A similar train or streak is not unfrequently left by one of the common falling stars, especially of the brighter sort; and vestiges of it sometimes remain for several minutes. It often happens, that even the large fire-balls have no other tail but this train, and ours of the 18th of August appeared at times to be in that state. Its tail was likewise thought by some spectators to be spiral.

Under this changeable form, but still as a single body, it proceeded regularly till a certain period, when expanding with a great increase of light, it separated into a cluster of smaller bodies or ovals, each extended into a tail and producing a train. At the same time a great number of sparks appeared to issue from it in various directions, but mostly downward, some of which were so bright as also to leave a small train. Most fireballs have suffered a bursting or explosion of this kind; but in general they have been thought to disappear immediately afterwards.

afterwards. This, however, continued its courſe, becoming more compact, or perhaps re-uniting, and ſeems to have undergone other ſimilar exploſions before it left our iſland, and again upon the continent*. The different accounts tend to ſhew, that its firſt ſeparation or burſting happened ſomewhere over Lincolnſhire, perhaps near the commencement of the fens. Many obſervers did not get ſight of it till after this period, and therefore never deſcribe it as a ſingle ball. There appears to be ſome deception, in conſequence of which ſpectators are led to believe, that a meteor is extinguiſhed by theſe exploſions; for the ſame opinion was formed of this in ſeveral parts of its courſe, though we have ſuch deciſive evidence of its continued progreſs; whether it be that the meteors really become more dull for a time immediately after their exploſion, or merely appear ſo on account of the greater preceding light, ſince they are always deſcribed as being moſt luminous the inſtant they burſt.

It is obſervable, that the great change in this meteor correſponds with the period in which it ſuffered a deviation from its courſe, as if there was ſome connexion between thoſe two circumſtances; and there are traces of ſomething of the ſame kind having happened to other meteors. If the exploſion be any ſort of effort, we cannot wonder that the body ſhould be moved by it from a ſtraight line; but on the other hand it ſeems equally probable, that if the meteor be forced, by any cauſe, to change its direction, the conſequence ſhould be a diviſion or ſeparation of its parts.

§ 4. Nothing relative to theſe meteors ſtrikes the beholders with ſo much aſtoniſhment as the exceſſive light they afford,

* For another inſtance of repeated exploſions conſult Mem. de l'Ac. des Scienc. 1756, p. 23.

ſufficient

ſufficient to render very minute objects viſible upon the ground in the darkeſt night, and larger ones to the diſtance of many miles from the eye. The illumination is often ſo great as totally to obliterate the ſtars, to make the moon look dull, and even to affect the ſpectators like the ſun itſelf; nay, there are many inſtances in which ſuch meteors have made a ſplendid appearance in full ſun-ſhine. The colour of their light is various and changeable, but generally of a bluiſh caſt, which makes it appear remarkably white. A curious effect of this was obſerved at Bruſſels the 18th of Auguſt, that whilſt the meteor was paſſing, "the moon appeared quite red, but ſoon "recovered its natural light *." The brightneſs alone of the meteor is not ſufficient to explain this, for the moon does not appear red when ſeen by day; but it muſt have depended on the the contraſt of colour, and ſhews how large a proportion of blue rays enters into the compoſition of that light, which could make even the *ſilver* moon appear to have exceſs of red. Priſmatic colours were alſo obſerved in the body, tail, and ſparks of this meteor, variouſly by different perſons; ſome compared them to the hues of gems. The moment of its greateſt brightneſs ſeems to have been when it burſt the firſt time; but it continued long to be more luminous after that period, than it was before.

The body of the fire-ball, even before it burſt, did not appear of an uniform ſubſtance or brightneſs, but conſiſted of lucid and dull parts, which were perpetually changing their reſpective poſitions; ſo that the whole effect was to ſome eyes like an internal agitation or boiling of the matter, and to others like moving chaſms or apertures. Similar expreſſions

* From a letter of the Abbé MANN's, Director of the Academy at Bruſſels, to Sir JOSEPH BANKS, Bart. P. R. S.

have

have been uſed in the deſcription of former meteors. The luminous ſubſtance was compared to burning brimſtone or ſpirits, Chineſe fire, the ſtars of a rocket, a pellucid ball or bubble of fire, liquid pearl, lightning and electrical fire; few perſons fancied it to be ſolid, eſpecially when it came near the zenith. Different ſpectators obſerved the light of the meteor to ſuffer at times a ſudden diminution and revival, which produced an appearance as of ſucceſſive inflammation; but might, in ſome caſes at leaſt, be owing to the interpoſition of ſmall clouds in its path.

§ 5. When, in conſequence of a more accurate attention to natural philoſophy, ſuch obſervations were firſt made upon fire-balls as determined their height, the computers were with reaſon ſurpriſed to find them moving in a region ſo far above that of the clouds and other familiar meteors of our atmoſphere; eſpecially as to every uninformed ſpectator they appear extremely near, or as if burſting over his head, a natural effect of their great light when ſeen without intervening objects. Their real height is to be collected from obſervations made at diſtant ſtations, which, for the greateſt accuracy, ought to be ſo ſituated, that the line joining them may cut the path of the meteor at right-angles, and that, at its greateſt elevation, it may appear from both of them about 45° above the horizon, on oppoſite ſides of the zenith. Alſo two ſtations on the ſame ſide of its path, if the leaſt angle of elevation be not very ſmall, and the difference between that and the greateſt angle be conſiderable, are by no means to be rejected. But little reliance can be placed upon obſervations of a meteor's altitude at any ſuppoſed period of its courſe, ſuch as the moment of its burſting; becauſe thoſe changes are ſeldom ſo in-

 ſtantaneous,

ſtantaneous, or ſeen ſo much alike by different ſpectators, as to be marked with ſufficient certainty.

Even in proper ſtations it rarely happens, that the angle of elevation can be obſerved with that degree of accuracy, which is neceſſary for any certain determination of the height. An eſtimate by the eye is doubtful, not only on account of the flattened curve the ſky ſeems to deſcribe, for which the moſt experienced obſervers ſcarcely ever make a juſt allowance, but likewiſe of the emotion produced by ſuch an unexpected, magnificent, and perhaps alarming ſpectacle, which renders it almoſt impoſſible to be quite collected. Therefore, unleſs an obſervation be checked by means of a houſe, tree, or ſome fixed body, along which the meteor was found to range, it muſt be received as uncertain. By night the ſtars afford excellent marks, eſpecially if the time be known with exactneſs; the brighter meteors, indeed, render theſe faint lights inviſible for the moment, but here we derive an eminent advantage from the train, which remains after the meteor is gone, and delineates perfectly its track through the heavens. If no ſuch marks have been taken, the expedient of endeavouring to recollect the part of the ſky where it paſſed, and aſcertaining that height with a quadrant, may often be uſeful; but there are many men of ſuch a turn of mind, that the original impreſſion made upon them will be totally perverted by their own ſubſequent reflexions and the remarks of others; in which caſe ſuch an application of inſtruments is likely to give a reſult farther from the truth, than their firſt immediate judgement, however vague and hazarded.

I am ſorry to add, that moſt of the obſervations in my poſſeſſion of the meteor which appeared the 18th of Auguſt, give its altitude by eſtimation only; yet I hope their correſpondence

 with

with one another will gain them a degree of credit, to which, if single, they would not be entitled.

1. In a letter from Perth in Scotland it is said, that "a "gentleman, who has a very good eye, observed the meteor "pass about 6° to the westward of the zenith;" and a Professor in one of the Universities, being at Ardoch on the banks of the Tweed, about two miles below Dunbarton, judged it to have "at least 45° of elevation above the horizon." These altitudes would make its real height 57 statute miles.

2. At St. Andrew's in Scotland, "it was not quite vertical, "but according to some was 20° or 25° from the zenith, ac-"cording to others not so much." Taking the greatest of these distances as nearest the truth, since we are usually led to estimate altitudes greater than they really are, this observation, calculated with that of Ardoch, gives 60 miles for the height.

For the communication of these observations, collected by his friends, I am indebted to General MELVILL F. R. S.

At Edinburgh the meteor passed very near the zenith, in which case a deviation of a few degrees is scarcely perceptible to a common eye.

The rev. Mr. WATSON of Whitby, in a letter to Lord MULGRAVE V. P. R. S. is very confident, that the greatest altitude of the meteor, which passed to the westward of his zenith, was 60°. Mr. EDGEWORTH F. R. S. in his letter to you, Sir, states its elevation at Edgeworth's-Town near Mullingar, in Ireland, as 10° or 12° above the eastern horizon. These observations, calculated strictly from the latitudes and longitudes with the allowance for the curvature of the earth, as indeed were all the rest where the difference would be sensible, give 57 miles for the height of the meteor.

 4. In

4. In the Morning Chronicle of Sept. 19. is inferted a letter from Newton Ardes, 7 miles eaft of Belfaft, in Ireland, correfponding fo well with Mr. EDGEWORTH's in the defcription of the meteor, as to appear very good authority. The altitude is there given as 16°, whence a height of 58 miles with the obfervation at Whitby.

5. Mr. MORE, Secretary to the Society for the encouragement of Arts, Manufactures, and Commerce, faw the meteor as he was riding about three miles S.W. of Brofeley in Shropfhire, and judged it to be elevated 35°. By a perpendicular drawn from this fpot to its fuppofed path in Lincolnfhire, its height came out 59 miles.

6. The altitude of 25° determined at Windfor I take to be one of thofe on which moft reliance can be placed, becaufe the gentlemen prefent, two of them Fellows of the Royal Society, were remarkably well qualified for fuch an eftimation. The letter you received, Sir, from Profeffor ALLAMAND of Leyden, mentions that the meteor was feen there about 30° above the horizon, and the terms in which it is defcribed in the Dutch news-papers * agree with this account. Its height hence calculated appears to be 58 miles.

7. Mr. THOMAS SQUIRE, of Folkftone, obferved the meteor over his houfe, as he was in the pofture of leaning back againft a hedge; he afterwards tried "its ranging with the roof by a "quadrant, and found it 68°½ above the horizon." Reducing this obfervation to the perpendicular dropped from Windfor on the path of the meteor, its height comes out 54 or 55 miles. Mr. SQUIRE's altitude, determined by a fixed object, is confirmed by the eftimate of feveral perfons at Ramfgate.

* Amfterdamifche Courant, Aug. 28, 1783.

8. The meteor was ſeen by Mr. STEEVENS F. R. S. at Hampſtead near London, moving along over the top of a row of trees. Mr. CAVENDISH F. R. S. having taken the altitude of theſe trees with a quadrant, found that of the higheſt, as ſeen from the part of the garden-walk oppoſite to it, to be 33°; which correſponds very well with the other obſervations, and conſequently gives the ſame height for the meteor. Mr. STEEVENS kept his eye upon it conſtantly, whilſt he paſſed briſkly along the walk.

This agreement of the different altitudes is nearer than could be expected; yet I know of no contradictory obſervations of any authority, except ſome made near Plymouth and in Cornwall, where the meteor being pretty near the horizon, its altitude, as will commonly happen in ſuch caſes, is given too great. The effect of this, however, would be to ſhew, that the meteor was higher; and therefore, I think, we may ſafely conclude, that it muſt have been more than 50 miles above the ſurface of the earth, in a region where the air is at leaſt 30000 times rarer than here below.

Contrary to what has been aſſerted of moſt other fire-balls, this of the 18th of Auguſt appears by the preceding obſervations to have kept on in a parallel courſe, without any deſcent or approach toward the earth. It may be much queſtioned, whether ſuch a deſcent has been proved in any former inſtance. The meteor deſcribed by Sir JOHN PRINGLE has been cited as the moſt certain example; but any perſon who carefully examines the obſervations themſelves, as ſtated in the 51ſt volume of the Philoſophical Tranſactions, will find them totally inadequate for ſuch a concluſion; its height ſeems to me determined only in one part of its courſe, between Iſland-Bridge

and

and Ancram, and was there from 48 to 50 miles *. M. LE ROY supposes the fire-ball seen July 17, 1771, to have been 54 miles high when it began, and 27 at its explosion †; but does not give the facts on which his calculation is founded.

Every philosopher must be struck with the agreement of these meteors in their distance from the earth, just beyond the limits of our crepuscular atmosphere.

§ 6. That a report was heard some time after the meteor of the 18th of August had disappeared, is a fact which rests upon the testimony of too many witnesses to be controverted, and is, besides, conformable to what has been observed in most other instances. In general it was compared to the falling of some heavy body in a room above stairs, or to the discharge of one or more large cannon at a distance. That rattling noise, like a volley of small arms, which has been remarked after other meteors, does not seem to have been heard on this occasion. From a comparison of the different accounts, it appears as if the report was loudest in Lincolnshire and the adjacent countries, and again in the eastern parts of Kent; in the intermediate places it was so indistinct as generally not to have been noticed, and all observers of credit in Scotland deny that they heard any thing of the sort. If, therefore, this report be connected with the bursting of the meteor, I should be inclined to suppose, that sound was produced two separate times, namely at the first explosion over Lincolnshire, and again when it seemed to burst soon after entering the continent. Ingenious men have availed themselves of this sound, to calculate the distance and height of meteors; and the exactness attained by this method, in the computation of the late fire-ball from the report heard at

* Phil. Transf. vol. LI. p. 241. and 274.

† Mem. de l'Acad. des Scienc. 1771, p. 676.

Windsor,

Windsor *, is very remarkable; but in general the accounts disagreed so much, that it would have been impossible to conclude any thing from them. Perhaps too the method itself is less certain than has been thought; for as the propagation of sound, and with intensity too, in air rarefied 30000 times, presents great difficulties in theory, though it may be in some measure explicable from the vast bulk of the meteor, and the large quantity of this rare air it may therefore displace by a sudden expansion; I think it not improbable, that some hitherto unperceived circumstance comes into play, by which the whole effect may be modified: for instance, if matter belonging to the meteor itself be what conveys the sound to our lower atmosphere, it may either admit sound to be propagated through it at a different rate than through common air, or it may move much faster than sound travels, as the entire meteor certainly does, and carry on the sonorific vibrations with it. Moreover, we cannot be sure what is the velocity of sound in air so much rarer than where our experiments have been made. For these reasons, while we distrust calculations of meteors founded on the progress of sound, we should be particularly careful to note down the intervals, and all the circumstances, as they may lead to very curious discoveries. The effect of the noise is, frequently, to produce such a shaking of the doors, windows, and the whole house, as is mistaken for an earthquake.

Besides the report as of explosions which was heard *after* the meteor, another sort of sound was said to *attend* it, more doubtful in its nature, and less established by evidence; I mean, a kind of hissing, whizzing, or crackling, as it passed along. That sound should be conveyed to us in an instant from a body above 50 miles distant, appears so irreconcilable to all we know of philosophy, that perhaps we should be justified in

* See p. 111. of this volume.

imputing

imputing the whole to an affrighted imagination, or an illufion produced by the fancied analogy of fireworks. The teftimony in fupport of it is, however, fo confiderable, on the occafion of this as well as former meteors, that I cannot venture to reject it, however improbable it may be thought, but would leave it as a point to be cleared up by future obfervers.

§ 7. To determine the bulk of the fire-ball, we muft not only have calculated its diftance, but alfo know the angle under which it appeared. For this purpofe the moon is the ufual term of comparifon; but as it was thought, at very different diftances, to prefent a difk equal to that luminary's, and the fame expreffions have been applied to moft preceding fire-balls, I conceive this eftimation rather to be a general effect of the ftrong impreffion produced by fuch fplendid objects on the mind, than to convey any determinate idea of their fize. However, if we fuppofe its tranfverfe diameter to have fubtended an angle of 30′ when it paffed over the zenith, which probably is not very wide of the truth, and that it was 50 miles high, it muft have been almoft half a mile acrofs. The tail fometimes appeared 10 or 12 times longer than the body; but moft of this was train, and the real elongation behind feems feldom to have exceeded twice or thrice its tranfverfe diameter, confequently was between one and two miles long. Now if the cubical contents be confidered, for it appeared equally round and full in all directions, fuch an enormous mafs, moving with extreme velocity, affords juft matter of aftonifhment.

§ 8. The duration of the meteor is very differently ftated, partly becaufe fome obfervers had it in view a much longer time than others, and partly becaufe they formed different judgements of the time. Thofe who faw leaft of it feem to have perceived its illumination about ten feconds, and thofe who

who ſaw moſt of it about a minute: hence the various accounts may in ſome meaſure be reconciled. Mr. HERSCHEL F. R. S. at Windſor, muſt have kept it in ſight long after other obſervers had thought it extinct: for though, probably, he did not ſee the beginning, as it never appeared to him like a ſingle ball, he watched it as much as "forty or forty-five "ſeconds, the laſt twenty or twenty-five of which it remained "almoſt in one ſituation, within a few degrees of the hori-"zon." This confirms the foreign accounts of its long progreſs to the ſouthward.

As ſcarcely any one had ſufficient preſence of mind to minute the time by his watch, the periods given for its duration are moſtly by gueſs. To correct this rude conjecture, it has been propoſed, that the obſerver ſhould endeavour to paſs over the time in his own mind as well as he can by recollection, whilſt another perſon ſilently marks the ſeconds with a watch. This may do ſomething, but ſtill leaves the matter very uncertain, as the nature of the emotion felt by the ſpectator while it was paſſing will cauſe the impreſſion of a longer or ſhorter time to be left upon his mind; and the formal proceſs of recollection is ſo tedious, that I believe the duration will in this way generally be made too ſhort. Mr. HERSCHEL, at my requeſt, was ſo good as to act over his obſervation, with the poſitions and geſtures he was obliged to employ; and this ſeems likely to come nearer the truth than a ſimple effort of the mind at recollection. But the ſureſt method would be, to repeat any uniform action in which the ſpectator might have been engaged at the time; as, for inſtance, to walk over the ſame ſpace of ground that he paſſed while the meteor was in ſight.

§ 9. From the apparent motion of the meteor, compared with its height, ſome computation may be formed of its aſtoniſhing

 velocity.

velocity. As at the height of 50 miles above the surface of the earth, it might be visible from the same station for a tract of more than 1200 miles, and the longest continuance of its illumination scarcely exceeded a minute, we have hence some presumption that it moved not less than 20 miles in a second. The rev. Mr. WATSON, in his letter to Lord MULGRAVE, says, *that the arc described by it whilst in his view could not be less than* 70° *or* 80°, *and yet the time could not exceed* 4″ *or* 5″ *at most.* This, with an altitude of 60°, and height of 50 miles, gives for its velocity about 21 miles in a second. The observer at Newton Ardes estimated its motion to be 10° *in a second, at the altitude of* 16°; this would make its velocity 30 miles in a second. Mr. HERSCHEL found it describe *an arch of* 167° *during the* 40 *or* 45 *seconds he observed it*, which gives a velocity of more than 20 miles in a second. Finally, Mr. AUBERT F. R. S. thought it described *an arch of* 136° *of azimuth in* 10 *or* 12 *seconds*, which would make its velocity above 40 miles in a second. I am sensible of the objections that may be made to all these computations; undoubtedly they are too vague; and yet, all taken together, perhaps they may have some weight, especially as they correspond so well with the different phænomena of the meteor's duration, and other fire-balls have been computed to move as fast *. Stating the velocity at the lowest computation of 20 miles a second, it exceeds that of sound above 90 times, and begins to approach toward that of the earth in her annual orbit. At such a rate, it must have passed over the whole island of Great Britain in less than half a minute, and might have reached Rome within a minute

* See Mem. de l'Acad. des Scienc. 1771, p. 678. Phil. Transf. N° 341. and 360. and vol. LI. p. 263, &c.

5 afterwards,

afterwards, or in seven minutes have traversed the whole diameter of the earth!

From this calculation it will be evident, that there is little chance of determining the velocity of meteors from the times of their passing the zenith of different places; and that therefore we must principally depend on observing carefully, with a watch that shews seconds, their apparent velocity through the heavens.

THE fire-ball which appeared on the 4th of October, at 43′ past six in the evening, was much smaller than that already described, and of much shorter duration. It was first perceived to the northward as a stream of fire, like the common shooting stars, but large; and having proceeded some way under this form, it suddenly burst out into that intensely bright bluish light which is peculiar to such meteors. At this period I saw it, and can compare the colour to nothing I am acquainted with so well, as to the blue lights of India, and some of the largest electrical sparks. The illumination was very great; and on that part of its course where it had been so bright, a dusky red streak or train was left, which remained visible perhaps a minute even with a candle in the room, and was thought by some gradually to change its form. Except this train, I think the meteor had no tail, but was nearly a round body, or perhaps a little elliptical. After moving not less than 10° in this bright state, it became suddenly extinct, without any appearance of bursting or explosion.

This meteor was seen for so short a way, that it is scarcely possible to determine the direction of its course with accuracy; but as in proceeding to the eastward it very perceptibly inclined towards the horizon, it certainly moved somewhere from the north-westward to the south-eastward. Its duration was so

 short,

ſhort, that many perſons thought it paſſed in an oppoſite direction; for my own part, I found myſelf abſolutely unable to determine whether the motion was *from* or *toward* the S.E. Some ſpectators were of opinion, that it changed its courſe the moment it became bright, proceeding no longer in the ſame ſtraight line; but my information is not ſufficient to determine this queſtion.

My ſituation, Sir, was particularly fortunate for aſcertaining the height of this meteor, as I ſaw it from your Library, ranging immediately over the oppoſite roof of your houſe. Hence I find by a quadrant that its altitude, even when it became extinct, could not be leſs than 32°. The upper northernmoſt end of the train it left bore, as I judge by the compaſs, about 28° northward of true E. and the lower end about 14°. I have only one obſervation to compare with this, which was made by Mr. BOYS of Sandwich. He concludes, from the train I imagine, that "it diſappeared juſt under, and a very "little to the weſtward" (rather northward) "of, the ſtar "γ in the foot of Cepheus." At that time γ Cephei was about 57° high, and bore above 21° to the eaſtward of N. whence the height of the meteor above the ſurface of the earth, after all proper allowances are made*, muſt have been between 40 and 50 miles.

As there was no appearance of burſting at the extinction of this fire-ball, ſo no report was heard after it; nor did any ſound attend it.

Some obſervers thought *this* meteor alſo near as big as the moon, but to me it did not appear above one quarter of her diameter, which would make its breadth ſomewhat above a furlong.

* It appears from obſervations taken by Gen. ROY, F. R. S. that the bearing of Sandwich from London is not ſo much to the ſouthward of eaſt, as it is laid down in our maps.

If

If the whole of the meteor's track be included, it seems to have lasted as much as three seconds, but in the bright state its duration was less than two, I think not much above one. Supposing it described an arc of 14° in 1½ second, or, according to Mr. AUBERT's observation, of 25° in 3″, its real velocity was about 12 miles a second.

Such meteors as these, which pass like a flash of lightning, and describe so short a course, are very unfavourable for calculating the velocity, but afford great advantages for determining the height, as they must be seen nearly at the same moment and in the same place by the different observers. Other instances are found of fire-balls beginning with a dull red light like a falling star, particularly the great one of March 19, 1719, treated of so fully by Dr. HALLEY * and Mr. WHISTON †.

It is remarkable, that a similar meteor had appeared the same day, that is, Saturday the 4th of October, about three in the morning, though, on account of the early hour, it was seen by fewer spectators. They represent it as rising from the northward to a small altitude, and then becoming stationary with a vibratory motion, and an illumination like day-light; it vanished in a few moments, leaving a train behind. This sort of tremulous appearance has been noticed in other meteors, as well as their continuing stationary for some time, either before they began to shoot forward, or after their course was ended.

* Phil. Transf. vol. XXX. N° 350. p. 978.

† Account of a surprising meteor seen March 19, 1719.

I FIND

I FIND it, Sir, impoſſible to quit this ſubject, without ſome reflexions about the cauſe, that can be capable of producing ſuch appearances at an elevation above the earth, where, if the atmoſphere cannot abſolutely be ſaid to have ceaſed, it is certainly to be conſidered as next to nothing. The firſt idea which ſuggeſted itſelf, that they were burning bodies projected with ſuch a velocity, was quickly abandoned, from the want of any known power to raiſe them up to that great height, or, if there, to give them the required impetus; and the ingenuity of Dr. HALLEY ſoon furniſhed him with another hypotheſis, in which he thought both theſe difficulties obviated. He ſuppoſes there is no projection of a ſingle body in the caſe; but that a train of combuſtible vapours, accumulated in thoſe lofty regions, is ſuddenly ſet on fire, whence all the phænomena are produced by the ſucceſſive inflammation *. But Dr. HALLEY gives no juſt explanation of the nature of theſe vapours, nor of the manner in which they can be raiſed up through air ſo extremely rare; nor, ſuppoſing them ſo raiſed, does he account for their regular arrangement in a ſtraight and equable line of ſuch prodigious extent, or for their continuing to burn in ſuch highly rarefied air. Indeed, it is very difficult to conceive, how vapours could be prevented, in thoſe regions where there is in a manner no preſſure, from ſpreading out on all ſides in conſequence of their natural elaſticity, and inſtantly loſing that degree of denſity which ſeems neceſſary for inflammation. Beſides, it is to be expected, that ſuch trains would ſometimes take fire in the middle, and ſo preſent the phænomenon of two meteors at the ſame time, receding from one another in a direct line.

Theſe difficulties have induced other philoſophers to relinquiſh Dr. HALLEY's hypotheſis, and propoſe, inſtead of it,

* Phil. Tranſ. vol. XXX. N° 360.

one

one of a very opposite nature, that meteors are permanent solid bodies, not raised up from the earth, but revolving round it in very eccentric orbits; or, in other words, that they are terrestrial comets *. The objections to this opinion, however, seem to me equally great. Most observers describe the meteors, not as looking like solid bodies, but rather like a fine luminous matter, perpetually changing its shape and appearance. Of this many defenders of the opinion are so sensible, that they suppose the revolving body gets a coat or atmosphere of electricity, by means of which it becomes luminous; but, I think, whoever carefully peruses the various accounts of fire-balls, and especially ours of the 18th of August when it divided, will perceive that their phænomena do not correspond with the idea of a solid nucleus enveloped in a subtile fluid, any more than with the conjecture of another learned gentleman, that they become luminous by means of a contained fluid, which occasionally explodes through the thick solid outer shell †.

A strong objection to this hypothesis of permanent revolving bodies, is derived from the great number of them there must be to answer all the appearances. Such a regular gradation is observed, from those large meteors which strike all beholders with astonishment, and occur but rarely, down to the minute fires called shooting stars, which are seen without being regarded in great numbers every clear night, that it seems impossible to draw any line of distinction between them, or deny that they are all of the same nature. But such a crowd of revolving bodies could scarcely fail to announce their existence by some other means than merely a luminous train in the night;

* See a dissertation on this subject by Professor CLAP, of Yale College, New England.

† Phil. Transf. vol. LI. p. 267.

as,

as, for instance, by meeting or justling sometimes near the earth, or by falling to the earth in consequence of various accidents; at least we might expect they would be seen in the day-time, either with the naked eye of telescopes, by some of the numerous observers who are constantly examining the heavens. With regard to these falling stars, it were much to be wished, that observations should be made upon them by different persons in concert at distant stations, for the purpose of ascertaining their height and velocity; which would tend very much to illustrate all this part of meteorology.

Another argument of great weight against the hypothesis that fire-balls are terrestrial comets, is taken from their great velocity. A body falling from infinite space toward the earth, would have acquired a velocity of no more than 7 miles a second, when it came within 50 miles of the earth's surface; whereas these meteors seem to move at least three times faster. And this objection, if there be no mistake in regard to the velocity of the meteors, as I think there is not, absolutely oversets the whole hypothesis.

What then can these meteors be? The only agent in nature with which we are acquainted, that seems capable of producing such phænomena, is electricity. I do not mean that by what is already known of that fluid, all the difficulties relative to meteors can be solved, as the laws, by which its motions on a large scale are regulated in those regions so nearly empty of air, can scarcely, I imagine, be investigated in our small experiments with exhausted vessels *; but only that several of the facts point out a near connexion and analogy with electricity, and that none of them are irreconcilable to the discovered laws of that fluid.

* How nearly the phænomena of meteors have been represented by artificial electricity is known from a very remarkable experiment of Mr. ARDEN's. See PRIESTLEY, vol. V. p. 379.

1. Electricity moves with ſuch a prodigious velocity, as to elude all the attempts hitherto made by philoſophers to detect it; but the ſwiftneſs of meteors, ſtating it at 20 miles a ſecond, is ſuch as no experiments yet contrived could have diſcovered, and which ſeems to belong to electricity alone. This is, perhaps, the only caſe in which the courſe or direction of that fluid is rendered perceptible to our ſenſes, in conſequence of the large ſcale on which theſe fire-balls move.

2. Various electrical phænomena have been ſeen attending meteors. Lambent flames are deſcribed as ſettling upon men, horſes, and other objects*; and ſparks coming from them, or the whole meteor itſelf, it is ſaid, have damaged ſhips, houſes, &c. in the manner of lightning †. Theſe facts, I muſt own, are but obſcurely related, yet ſtill they do not ſeem to be deſtitute of foundation. If there be really any hiſſing noiſe heard while meteors are paſſing, it ſeems explicable on no other ſuppoſition than that of ſtreams of electric matter iſſuing from them, and reaching the earth with a velocity equal to that of the meteor, namely, in two or three ſeconds. Accordingly, in one of our late meteors, the hiſſing was compared to that of electricity iſſuing from a conductor ‡. The ſparks flying off ſo perpetually

* PRIESTLEY's Hiſtory of Electricity, p. 352. Mem. de l'Acad. des Scienc. 1771, p. 681, 682. See an odd fact, perhaps of this nature, in PARKER's General Advertiſer, Dec. 1, 1783.

† Mem. anc. de l'Acad. de Dijon, tom. I. Hiſt. p. 42. Phil. Tranſ. vol. XLVI. p. 366. Hiſt. de l'Acad. des Scienc. 1761, p. 28.

‡ Cheſter Weekly Courant, Auguſt 26, 1783. This and many other curious circumſtances, relative to meteors, are ſo well exemplified in the following obſervation, made ſeveral years ago by Mr. ROBINSON at Hinckley in Leiceſterſhire, that I think it worth tranſcribing here, eſpecially as it occurs in a work which few people would think of conſulting on ſuch a ſubject. "Oct. 26, "1766, at half paſt five in the evening, after a violent ſtorm of wind and rain,

perpetually from the body of fire-balls, may poſſibly have ſome connexion with theſe ſtreams*. In the ſame manner the ſound of exploſions may perhaps be brought to us quicker, than if it were propagated through the whole diſtance by air alone. Should theſe ideas be well founded, the change of direction which meteors ſeem at times to undergo, may poſſibly be influenced by the ſtate of the ſurface of the earth over which

"I obſerved a fiery meteor. Its direction was from N.W. to S.E. nearly in a "horizontal direction; it paſſed very near to me, and was of an elliptical form; "its motion about 40° in 2″ or 3″ of time. It was very bright and lucid to "appearance like the paleſt lightning, and emitted ſparks continually, which "formed a kind of tail toward the N.W. which ſeemed to be extinguiſhed at the "diſtance of 2° or 3° from the body; there was a ſmall portion that parted from "it. The coheſion of matter was ſo great, that it drew a thread of conſiderable "length from the body, before it broke from it. During the paſſage there was a "kind of *hiſſing noiſe, much like to what we hear from the electrical machine* "*when the electric matter is running away, or as when it is eſcaping from a full* "*charged jar.*" Bibliotheca Topographica Britannica, N° VII. p. 81.

* Hiſt. de l'Acad. des Scienc. 1761, p. 28. Mem. de l'Acad. des Scienc. 1771, p. 682. Extract of a letter from the Abbé MANN, Director of the Academy at Bruſſels, to Sir JOSEPH BANKS, Bart. P. R. S. "I ſhall only mention one ſingular circumſtance, which was communicated to me by a particular "friend of mine. It happened at Mariekercke, a ſmall village on the coaſt, "about half a league to the W. of Oſtend. The curate of the village was "ſitting in the duſk of the evening with a friend, when a ſudden light ſurpriſed "them, and immediately after a ſmall ball of light-coloured flame came through "a broken pane of glaſs, croſſed the room where they were ſitting, and fixed "itſelf on the chink of a door oppoſite to the window where it entered, and "there died gradually away. It appeared to be a kind of phoſphoric light, "carried along by the current of air. The curate and his friend, greatly ſurpriſed at "what they ſaw, apprehended fire in the neighbourhood; but going out, found "that the fire, which had come in through the window, had been detached from a large meteor in its paſſage."

How far theſe and ſimilar appearances may be owing ſimply to the illumination produced by meteors, ſhould be attentively conſidered in the inveſtigation of ſuch facts.

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they are paſſing, and to which the ſtreams are ſuppoſed to reach. A ſimilar cauſe may occaſion the apparent exploſion, the opening of more channels giving new vent and motion to the electric fluid. May not the deviation and exploſion which appear to have taken place in the fire-ball of the 18th of Auguſt over Lincolnſhire, have been determined by its approach toward the fens, and an attraction produced by that large body of moiſture?

3. A further argument for the electric origin of meteors is deduced from their connexion with the northern lights, and the reſemblance they bear to theſe electrical phænomena, as they are now almoſt univerſally allowed to be, in ſeveral particulars. Inſtances are recorded, where northern lights have been ſeen to join and form luminous balls, darting about with great velocity, and even leaving a train behind like the common fire-balls *. This train I take to be nothing but the rare air left in ſuch a highly electrified ſtate as to be luminous; and ſome ſtreams of the northern lights are very much like it. The *aurora borealis* appears to occupy as high, if not a higher, region above the ſurface of the earth, as may be judged from the very diſtant countries to which is has been viſible at the ſame time †; indeed the great accumulation of electric matter ſeems to lie beyond the verge of our atmoſphere, as eſtimated by the ceſſation of twilight. Alſo with the northern

* Hiſt. de l'Acad. des Scienc. 1705, p. 35. WHISTON's Account of a Meteor ſeen in the Air 171½. Phil. Tranſ. vol. XLI. p. 626; and LIII. p. 6? Alſo a moſt pointed fact in the Act. Liter. Sueciæ, 1734, p. 78.

† BERGMAN, upon a mean of 30 computations, makes the average height of the northern lights to be near 70 Swediſh, that is, about 460 Engliſh miles. Kong. Vetenſk. Acad. Handlingar, vol. XXV. p. 193. See alſo Phil. Tranſ. vol. LIV. p. 327. and M. DE MAIRAN's Traité de l'Aurore Boreale, p. 51.

 lights

lights a hiſſing noiſe is ſaid to be heard in ſome very cold climates; GMELIN ſpeaks of it in the moſt pointed terms, as frequent and very loud in the north-eaſtern parts of Siberia *; and other travellers have related ſimilar facts †.

But,

* Reiſe durch Siberien, vol. III. p. 135. As the whole paſſage is very remarkable, and has never, that I know, appeared in Engliſh, I thought the following tranſlation of it might be acceptable.

"These northern lights begin with ſingle bright pillars, riſing in the N. and " almoſt at the ſame time in the N.E. which gradually increaſing comprehend a " large ſpace of the heavens, ruſh about from place to place with incredible velo- " city, and finally almoſt cover the whole ſky up to the zenith. The ſtreams are " then ſeen meeting together in the zenith, and produce an appearance as if a " vaſt tent was expanded in the heavens, glittering with gold, rubies, and ſap- " phire. A more beautiful ſpectacle cannot be painted; but whoever ſhould ſee " ſuch a northern light for the firſt time, could not behold it without terror. " For however fine the illumination may be, it is attended, as I have learned from " the relation of many perſons, with ſuch a hiſſing, cracking, and ruſhing noiſe " throughout the air, as if the largeſt fire-works were playing off. To deſcribe " what they then hear, they make uſe of the expreſſion, *Spolochi chodjat*, that is, " the raging hoſt is paſſing. The hunters who purſue the white and blue foxes " in the confines of the Icy Sea, are often overtaken in their courſe by theſe " northern lights. Their dogs are then ſo much frightened, that they will not " move, but lie obſtinately on the ground till the noiſe has paſſed. Commonly " clear and calm weather follows this kind of northern lights. I have heard this " account, not from one perſon only, but confirmed by the uniform teſtimony " of many, who have ſpent part of ſeveral years in theſe very northern regions, " and inhabited different countries from the Yeniſei to the Lena; ſo that no " doubt of its truth can remain. This ſeems indeed to be the real birth-place " of the *aurora borealis*."

It is here to be obſerved, that GMELIN did not collect the account himſelf, but extracted it from letters or papers of M. DE L'ISLE DE LA CROYERE's, who was himſelf far to the northward of Yakutſk, without hearing theſe noiſes; probably, therefore, it is much exaggerated, though one can ſcarcely ſuppoſe the whole to be fabulous.

† Muſſchenbroeck Introduct. § 2495. Beccaria dell' Elletriciſmo artif. et nat. p. 221.

But, in my opinion, the moſt remarkable analogy of all, and that which tends moſt to elucidate the origin of theſe meteors, is the direction of their courſe, which ſeems, in the very large ones at leaſt, to be conſtantly from or toward the north or north-weſt quarter of the heavens, and indeed to approach very nearly to the preſent magnetical meridian. This is particularly obſervable in thoſe meteors of late years whoſe tracks have been aſcertained with moſt exactneſs; as that of November 26, 1758, deſcribed by Sir JOHN PRINGLE; that of July 17, 1771, treated of by M. LE ROY; and this of the 18th of laſt Auguſt. The largeſt proportion of the other accounts of meteors confirm the ſame obſervation, even thoſe of a more early period*; nay, I think, ſome traces of it are per-

p. 221. There is now working with Mr. NAIRNE F. R. S. a perſon of the name of ARNOLD, who reſided ſeven years at Hudſon's Bay, the laſt three at Fort Henley. He confirms M. GMELIN's account of the fine appearance and brilliant colours of the northern lights, and particularly of their ruſhing noiſe, which he affirms he has very frequently heard, and compares it to the ſound produced by whirling round a ſtick ſwiftly at the end of a ſtring. He adds, that on converſing about this matter with a Swediſh watch-maker of the name of LIND, that perſon aſſured him, that he had heard a ſimilar noiſe in his own country. Mr. NAIRNE too, one time, at Northampton, when the northern lights were remarkably bright, is confident he perceived a hiſſing or whizzing ſound.

This hiſſing or ruſhing noiſe, as well as that attending meteors in their paſſage, ſuppoſing it in both caſes to be real, I would attribute to ſmall ſtreams of electric matter, running off to the earth from the great maſſes or accumulations of electricity, by which I ſuppoſe both meteors and the northern lights to be produced. Compare M. DE MAIRAN's Traité de l'Aurore Boréale, p. 126.

* See Phil. Tranſ. and Mem. de l'Acad. des Sciences, &c. I have found, of an earlier or later period, accounts of more than 40 different fire-balls. Of theſe above 20 are ſo deſcribed, that it is certain their courſe was in the above-mentioned direction; only 3 or 4 ſeem to have moved the contrary way; and with regard to the remainder, it is left doubtful, from the imperfect ſtate of the relations.

perceivable in the writings of the ancients*. Whether their motion ſhall be *from* the northern quarter of the heavens or *toward* it, ſeems nearly indifferent, as the numbers of thoſe going each way are not very unequal; I conſider them, in the former caſe, as maſſes of the electric fluid repelled, or burſting from the great collected body of it in the north; and, in the latter caſe, as maſſes attracted toward that accumulation; a diſtinction, probably, much the ſame in effect, as that of poſitive and negative electricity near the ſurface of the earth.

This tendency toward the magnetic meridian, however, ſeems to hold good only with regard to the largeſt ſort of fire-balls; the ſmaller ones move more irregularly, perhaps becauſe they come further within the verge of our atmoſphere, and are thereby more expoſed to the action of extraneous cauſes. That the ſmaller ſort of meteors, ſuch as ſhooting ſtars, are really lower down in the atmoſphere, is rendered very probable by their ſwifter *apparent* motion; perhaps it is this very circumſtance which occaſions them to be ſmaller, the electric

tions. When we conſider that even the meteor of the 18th of Auguſt laſt was thought by *ſome* ſpectators to move ſouth-weſtward, it will rather appear ſurpriſing that ſo many of theſe accounts ſhould correſpond, than that a few of them ſhould differ.

* ARISTOTLE (Meteor. lib. I. c. 6.) denies that comets, with which I take meteors to be confounded, are generated *only* in the north; which ſhews it to have been then the prevalent opinion, that they appeared moſt frequently in that quarter. Αλλα μην ουδε τυτο αληθες, ότι εν τῳ προς αρκτον τοπῳ γινεται ὁ Κομητης μονον. So likewiſe PLINY (lib. II. c. 25.) Xiphias, Diſceus, Pitheus doliorum cernuntur figura, in concavo fumidæ lucis. Ceratias. Lampadias. Hippeus. Candidus Cometes. Omnes ferme *ſub ipſo ſeptentrione*, aliquâ ejus parte non certâ, ſed maxime in candidâ, quæ lactei circuli nomen accepit. And SENECA (Quæſt. Nat. lib. VII.) Placet ergo noſtris, Cometas, ſicut Tubas, Trabeſque, et alia oſtenta cœli, denſo aëre creari. Ideo *circa ſeptentrionem frequentiſſimè apparent*, quia illic plurimum eſt aëris pigri.

fluid

fluid being more divided in more resisting air. But as those masses of electricity, which move where there is scarcely any resistance, so generally affect the direction of the magnetic meridian, the ideas which have been entertained of some analogy between these two obscure powers of nature, seem not altogether without foundation *.

If the foregoing conjectures be just, distinct regions are allotted to the electrical phænomena of our atmosphere. Here below we have thunder and lightning, from the unequal distribution of the electric fluid among the clouds; in the loftier regions, whither the clouds never reach, we have the various gradations of falling stars; till beyond the limits of our crepuscular atmosphere the fluid is put into motion in sufficient

* It appears to me more rational to resolve this analogy into a power of electricity to influence magnetism, than into a supposed similarity of two fluids; as the former can be made evident by our artificial experiments, but there is no proof of the latter. When fire-balls, therefore, are said to affect the magnetic meridian, I do not mean that they are drawn in that direction because it is the line of magnetism, but rather that the magnetic poles of the earth are thrown into their present position, by the accumulation and action of that very electricity upon which the fire-balls depend. Should a change be produced by any cause in the place of this accumulation, or the state of its motion, it is not improbable, that the main polarity would be given to other portions of the earth, whence a variation in the pointing of the compass would necessarily ensue. If Dr. FRANKLIN's hypothesis be admitted, ascribing the electrical state of the polar atmosphere to the crust of ice (a bad conductor) in those regions, it follows, that should ice form or be collected in one part more than in another, the atmosphere there would become more highly electrical, and, in so far as the magnetism is given by electricity, the adjoining portion of the earth would acquire a stronger polarity. Now it is certainly worthy of remark, that since our first northern navigations, the coast of West Greenland and its surrounding seas have become gradually more and more inaccessible on account of ice, and that the magnetic needle all this time has been constantly changing its variation to the westward.

masses

masses to hold a determined course, and exhibit the different appearances of what we call fire-balls; and probably at a still greater elevation above the earth, the electricity accumulates in a lighter less condensed form, to produce the wonderfully diversified streams and corruscations of the *aurora borealis.*

I have the honour to be, with the greatest respect,

SIR,

Your most obedient humble servant,

C. BLAGDEN.

END OF PART I. OF VOL. LXXIV.

PHILOSOPHICAL

TRANSACTIONS.

PART II.

PHILOSOPHICAL TRANSACTIONS,

OF THE

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XXII.

XXIX.

PHILOSOPHICAL TRANSACTIONS.

XIX. *On the remarkable Appearances at the Polar Regions of the Planet* Mars, *the Inclination of its Axis, the Position of its Poles, and its spheroidical Figure; with a few Hints relating to its real Diameter and Atmosphere.* *By* William Herschel, *Esq. F. R. S.*

Read March 11, 1784.

WHAT I have to offer on the subject of the remarkable appearances at the polar regions of Mars, as well as what relates to the inclination of the axis, the position of the poles, and the spheroidical figure of that planet, is founded on a series of observations which I shall deliver in this paper; and after they have been given in the order they were made, it will be eafy to fhew, by a few deductions from them, that my theory of this planet is fupported by facts which will fufficiently authorife the conclufions I have drawn from them. For the fake of better order and perfpicuity, however, I fhall treat each fubject apart, and begin with the remarkable appearances about the polar regions. The obfervations on them were made with a view to the fituation and inclination of the axis of Mars; for to determine thefe we cannot conveniently ufe the fpots on its furface, in the manner which is practifed on the fun. The quantities to be meafured are fo fmall, and the obfervations of the center of Mars fo precarious, and attended with fuch difficulties (fince an error of only a few feconds would be fatal) that we muft have recourfe to other methods.

When I found that the poles of Mars were diftinguifhed with remarkable luminous fpots *, it occurred to me, that we might obtain a good theory for fettling the inclination and nodes of that planet's axis, by meafures taken of the fituation of thofe fpots. But, not to proceed upon grounds that wanted confirmation, it became neceffary to determine by obfervation, how far thefe polar fpots might be depended upon as permanent; and in what latitude of the globe of Mars they were fituated; for, if they fhould either be changeable, or not be at the very poles, we might be led into great miftakes by overlooking thefe circumftances. The following obfervations will affift us in the inveftigation of thefe preliminary points.

* A bright fpot near the fouthern pole, appearing like a polar zone, has alfo been obferved by M. MARALDI. See Dr. SMITH's Optics, § 1094.

1777,

1777, April 17. 7 h. 50′. There are two remarkable bright ſpots on Mars. In fig. 1. tab. VI. they are marked *a* and *b*. The line AB expreſſes the direction of a parallel of declination. 10 feet reflector, 9 inches aperture, power 211 *.

10 h. 20′. They are both quite gone out of the diſk.

1779, This year, in all my obſervations on Mars, there is no mention of any bright ſpots, ſo that I believe there were none remarkable enough to attract my attention. However, as my view was particularly directed to the phænomena of this planet's diurnal rotation, it is poſſible I might overlook them.

1781, March 13. 17 h. 40′. 20 feet reflector. I ſaw a very lucid ſpot on the ſouthern limb of Mars of a conſiderable extent. See fig. 2.

June 25. 11 h. 36′. 7 feet reflector, power 227. Two luminous ſpots appeared at *a* and *b*, fig. 3.; *a* is larger than *b*.

12 h. 15′. With 460. *a* is thicker than *b*, but *b* is rather longer.

13 h. 12′. *a* is grown thicker, and *b* become thinner.

June 27. 11 h. 20′. The two lucid ſpots are on Mars.

June 28. 11 h. 15′. They are both viſible; *a*, fig. 4. is much thicker than *b*.

12 h. 55′. A line joining *a* and *b* does not go through the center.

June 30. 10 h. 48′. The ſpot *a* is viſible. fig. 5.

11 h. 35′. Both ſpots are to be ſeen.

* Phil. Tranſ. vol. LXXI. p. 127. and fig. 17.

1781, July 3. 10 h. 54′. *a* seems to be larger than I have seen it, fig. 6.

11 h. 24′. *b* is not yet visible, fig. 7.

12 h. 36′. I perceive part of *b*, fig. 8.

July 4. 12 h. 9′. *a* is very full; *b* extremely thin, and barely visible.

12 h. 18′. *a* and *b* are not quite opposite each other.

12 h. 49′. *b* is increased.

July 15. 9 h. 54′. *a* is visible, fig. 9.

11 h. 35′. *b* invisible.

12 h. 12′. *b* not to be seen.

July 16. 11 h. 9′. The bright spot *a* is very large.

July 17. 11 h. 15′. No other bright spot but *a*.

July 19. 13 h. 31′. *a* visible.

July 20. 10 h. 3′. I suppose the bright spot *a* on Mars is, very nearly, the south pole; which therefore must lie in sight. There is no second bright spot *b* visible to night.

10 h. 56′ *b* not visible; the night very fine.

July 22. 11 h. 14′. At *a* and *b*, fig. 10. are bright spots; *a* is larger than *b*. Most probably the south pole is in view, and the north pole just hid from our sight. If the spots are polar, or nearly so, then *a* must, on a supposition of the south pole's being in view, appear larger than *b*; and if *b* extend a little more from the north pole one way than another, it must be subject to some change in its appearance from the revolution of Mars on its axis.

July 30. 9 h. 43′. Both spots visible.

August 8. 10 h. 4′. Only *a* visible, fig. 11.

August 17. 9 h. 21′. Only *a* in sight.

1781,

1781, August 23. 8 h. 44′. *a* as usual, and part of *b* visible, fig. 12.

Sept. 7. The white spot *a* is very large.

1783, May 20. Mars has a singular appearance. At *a*, fig. 13. is the polar spot, which is bright, and seems to project above the disk by its splendour, causing a break at *c*.

July 4. *a* is very bright.

July 23. 14 h. 45′. *a* is very lucid.

August 16. I saw the bright spot with the 20 feet reflector as usual.

Aug. 26. The lucid spot on Mars is its south pole, for it remains in the same place, while the dark equatorial spots perform their constant gyrations: it is nearly circular.

Aug. 29. The south polar spot is in the same situation.

Sept. 9. As usual.

Sept. 22. The south polar spot is of a circular shape, and very brilliant and white. I had a beautiful and distinct view of it when it was about the meridian, and measured its little diameter in the equatorial direction of Mars. With a power of 932 it gave 1″ 41‴, and I saw it very distinctly. The outward edge of the spot came just up to the upper limb; a favourable haziness, taking off every troublesome ray, gave me objects in general exceedingly well defined, especially Mars.

Sept 23. 9 h. 55. The polar spot *a*, fig. 14. as usual.

Sept. 24. The same.

1783,

1783, Sept. 25. 12 h. 30′. The bright south polar spot *a*, fig. 15. seems to be fixed in its place, and goes nearly up to the margin of the disk; it is perfectly round.

12 h. 55′. The track of the equatorial spots is incurvated, being convex towards the north, see *e*, *q*, fig. 23.: this confirms the white spot's being at the south pole. With long attention I can perceive the edge of the disk of Mars beyond the spot, extending about ¼ diameter of the spot.

Sept. 26. 12 h. 10′. The spot *a* is in a line with the center and the end of the hook, fig. 16.

Sept. 27, 28, 29. The spot as usual.

Sept. 30. 10 h. 30′. The polar spot as in fig. 17.

Oct. 1. 9 h. 55′. I am inclined to think, that the white spot has some little revolution, and therefore is not with its center exactly at the pole of Mars; it is rather probable, that the real pole, though within the spot, may lie near the circumference of it, or one-third of its diameter from one of the sides. A few days more will shew it, as I shall now fix my particular attention upon it.

10 h. 17′. The bright spot is certainly not so far upon the disk as it used to be formerly, and is either reduced or has a small motion; which of the two is the case will be seen in a few hours.

13 h. 3′. The bright spot has a little motion; for it is now come farther into the disk.

I concluded now, in general, that none of the bright spots on Mars were exactly at the poles, though they could certainly not be far from them: for what has been just related of the

4 1st,

1st, 2d, and 3d of October 1783, shews plainly, that the appearance of the southern spot *a* was a little affected by the diurnal motion of the planet; and the observations of the 3d and 4th of July 1781, shew also that the spot *b* could not be exactly at the north pole; and that, perhaps, the visible branch of the latter extended pretty far towards the equator. However, the south polar spot of the year 1783, being very small and nearly round, afforded a good opportunity for determining its polar distance, by noting the different angles of position it assumed while Mars revolved on its axis; to this end many observations were taken at different hours of the same night, which will be found among the measures of the angles of position in the next division of my subject. And since the different degrees of brilliancy, as well as the proportional apparent magnitude of the spot, would also contribute to the investigation of this point, I continued my remarks on those particulars, as follows.

1783, Oct. 2. 7 h. 59′. The bright spot near the south pole is about half visible.

Oct. 4. 8 h. 0′. The polar spot seems to project above the disk as formerly, and is very small.

Oct. 5. 11 h. 13′. The spot is very small, and seems actually to be in the circumference.

11 h. 30′. The spot is small, and seems to be with its farthest side in the circumference of the disk; or it may, perhaps, be partly beyond it, and therefore not all in sight.

11 h. 50′. I see the spot much clearer than I did before.

13 h. 15′. The white spot is more in sight, and of its usual size, but does not seem much to change its position;

sition; however, what change there is shews that it has been beyond the pole, as it appears to have been direct while the equatorial spots were retrograde.

1783, Oct. 9. 11 h. 48′. The white polar spot increases in size. At 10 h. 35′. it was as in fig. 18. but is now larger, and coming round towards that part of its orbit which is nearest to us. See fig. 24.

Oct. 10. 6 h. 20′. I see no white polar spot; but the planet is too low for any observation to be depended on.

6 h. 55′. The white spot begins to be visible; at least I see it now, the planet being higher than before, fig. 19.

9 h. 55′. With 460, the white spot is considerably increased, and shews a circular form, fig. 20.

Oct. 11. 7 h. 46′. The bright spot is very visible; the evening fine; with 278.

Oct. 16. 7 h. 7′. The spot is very luminous.

9 h. 55′. It seems rather lengthened; perhaps it may be arrived at the extreme of its parallel of declination.

Oct. 17. 7 h. 47′. The white spot *a*, fig. 21. is very bright.

13 h. 7′. It is less in appearance than it was in the beginning of the evening.

Oct. 23. 6 h. 46′. The bright spot is very large and luminous; I suppose it to be in the nearer parts of its little orbit.

7 h. 11′. It is situated as in fig. 22.

Oct. 24. 7 h. 1′. The white spot is very large.

Oct. 27. 8 h. 45′. It is very large and round.

Nov. 1. 7 h. 47′. The spot is round and bright.

1783, Nov. 11. The deficiency of light which occaſions Mars to appear gibbous, reaches over the ſouth polar ſpot towards the preceding limb, and hides it.

Nov. 14. Mars is gibbous, and the polar ſpot is thereby rendered inviſible.

Nov. 17. 6 h. 0′. The ſouth polar ſpot is under the falcated defect of light.

6 h. 30′. I do not know whether there be not a faint glimpſe of the polar ſpot left; the weather is too bad to determine it.

I have added fig. 25. (tab. X.) to ſhew the connection of the 15th, 17th, 18th, 19th, 20th, 21ſt, and 22d figures, which complete the whole equatorial circle of appearances on Mars, as they were obſerved in immediate ſucceſſion. The center of the circle marked 17 is placed on the circumference of the inner circle, by making its diſtance from the center of the circle, marked 15, anſwer to the interval of time between the two obſervations, properly calculated and reduced to ſidereal meaſure. The ſame has been done with regard to the circles marked 18, 19, &c. And it will be found, by placing any one of theſe connected circles, ſo as to have its contents in a ſimilar ſituation with the figures in the ſingle repreſentation which bears the ſame number, that there is a ſufficient reſemblance between them; but ſome allowance muſt undoubtedly be made for the unavoidable diſtortions occaſioned by this kind of projection.

In order to bring theſe obſervations on the bright ſpots into one view, I have placed them at the circumference of three circles (ſee fig. 26, 27, 28. tab. VII. VIII. IX.) divided into degrees, repreſenting the parallels of declination in which they revolved about the poles of Mars. The diviſion of the circles marked 360 is where a ſpot paſſes that meridian of the planet which is turned towards the earth, and where, conſequently, it appears to us in its greateſt luſtre. The motion of the ſpot is according to the numbers 30, 60, 90, and ſo on to 360. In calculating the daily places of the ſpots I have uſed the ſidereal period of 24 h. 39′ 21″,67 determined in my paper on the rotation of Mars*; and have alſo made proper allowances for the alterations of the geocentric longitudes calculated from the ſituations of that planet given in the Nautical Almanack; by which means the ſidereal is reduced to a proper ſynodical period.

The following three tables contain the reſult of the calculations, and ſerve to explain the arrangement of the obſervations in the circles. In the firſt column are the times when the obſervations were made. In the ſecond, the ſidereal places of the ſpot in degrees and minutes. In the third column are the geocentric longitudes of Mars at the time of the obſervations. In the fourth, the neceſſary corrections on account of theſe different longitudes. In the fifth column are the corrected or ſynodical places of the ſpots; and, according to the numbers in that column, they are marked on the circles, where conſequently each ſpot is repreſented as it muſt have appeared to be ſituated at the time of obſervation.

* Phil. Tranſ. vol. LXXI. p 134.

TABLE

TABLE I.

Time of observation.			Sider. place.		Geoc. longit.			Correction.		Synod. place.	
D.	H.	M.	D.	M.	S.	D.	M.	D.	M.	D.	M.
June 25	11	36	350	31	9	24	35	−0	0	350	31
25	12	15	0	0	9	24	35	−0	0	0	0
25	13	12	13	52	9	24	34	−0	1	13	51
27	11	20	357	28	9	24	12	−0	23	357	5
28	11	15	316	40	9	24	1	−0	34	316	6
30	10	48	290	56	9	23	38	−0	57	289	59
30	11	35	302	23	9	23	38	−0	57	301	26
July 3	10	54	263	40	9	22	55	−1	40	262	0
3	11	24	270	58	9	22	55	−1	40	269	18
3	12	10	282	9	9	22	55	−1	40	280	29
3	12	36	288	29	9	22	55	−1	41	286	48
4	12	9	272	20	9	22	40	−1	55	270	25
4	12	49	282	4	9	22	40	−1	55	280	9
15	9	54	134	7	9	19	43	−4	52	129	15
15	11	35	158	42	9	19	42	−4	53	153	49
15	12	12	167	42	9	19	42	−4	53	162	49
~~16~~	~~11~~	~~9~~	~~144~~	48	9	~~19~~	~~26~~	−5	~~9~~	137	~~39~~
17	11	15	134	40	9	19	9	−5	26	129	14
19	13	31	148	37	9	18	34	−6	1	142	36
20	10	3	88	25	9	18	21	−6	14	82	11
20	10	56	101	19	9	18	20	−6	15	95	4
22	11	14	86	32	9	17	49	−6	46	79	46
30	9	43	347	46	9	16	5	−8	30	339	16

TABLE II.

Time of observation.			Sider. place.		Geoc. longit.			Correction.		Synod. place.	
D.	H.	M.	D.	M	S.	D.	M	D.	M.	D.	M.
June 25	11	36	86	51	9	24	35	+1	40	88	31
25	12	15	96	20	9	24	35	+1	40	98	0
25	13	12	110	12	9	24	34	+1	39	111	51
28	11	15	53	0	9	24	1	+1	6	54	6
30	10	48	27	16	9	23	38	+0	43	27	59
30	11	35	38	43	9	23	38	+0	43	39	26
July 3	10	54	0	0	9	22	55	+0	0	0	0
4	12	9	8	40	9	22	40	−0	15	8	25
15	9	54	230	27	9	19	43	−3	12	227	15
15	10	12	234	50	9	19	43	−3	12	231	38
15	11	35	255	2	9	19	42	−3	13	251	49
15	12	12	264	2	9	19	42	−3	13	260	49
16	11	9	339	8	9	19	26	−3	29	235	39
19	13	31	244	57	9	18	34	−4	21	240	36
20	10	3	184	45	9	18	21	−4	34	180	11
20	10	56	197	39	9	18	20	−4	35	193	4
30	9	43	84	6	9	16	5	−6	50	77	16

TABLE

TABLE III.

Time of obſervation.			Sider. place.		Geoc. longit.			Correction.		Synod. place.	
D.	H.	M.	D.	M.	S.	D.	M.	D.	M.	D.	M.
Sept. 25	13	30	6	32	0	9	54	+6	44	13	16
Oct. 1	10	17	262	5	0	8	6	+4	56	267	1
1	13	3	302	29	0	8	5	+4	55	307	24
2	7	59	218	55	0	7	50	+4	40	223	25
4	8	0	200	0	0	7	15	+4	5	204	5
4	8	46	211	12	0	7	15	+4	5	215	17
5	11	13	237	23	0	6	55	+3	45	241	8
5	11	30	241	31	0	6	55	+3	45	245	16
5	11	50	246	23	0	6	55	+3	45	250	8
5	13	15	267	4	0	6	54	+3	44	270	48
5	14	0	278	1	0	6	53	+3	43	281	44
7	8	20	176	8	0	6	23	+3	13	179	21
7	10	5	201	41	0	6	22	+3	12	204	53
7	11	50	227	14	0	6	21	+3	11	230	25
9	11	48	207	35	0	5	49	+2	39	210	14
10	6	55	126	42	0	5	37	+2	27	129	9
10	7	50	140	5	0	5	36	+2	26	142	31
10	9	55	170	30	0	5	34	+2	24	172	54
10	12	11	203	36	0	5	33	+2	21	205	57
16	7	7	72	9	0	4	15	+1	5	73	14
16	7	46	81	39	0	4	15	+1	4	82	43
16	9	55	113	2	0	4	14	+1	4	114	6
17	7	47	72	19	0	4	3	+0	53	73	12
17	13	7	150	11	0	4	0	+0	50	151	1
23	6	46	0	0	0	3	10	−0	0	0	0
24	7	1	354	0	0	3	2	−0	8	353	52

From the appearance and diſappearance of the bright north polar ſpot in the year 1781, we collect that the circle of its motion, repreſented by fig. 26. was at ſome conſiderable diſtance from the pole. By a calculation, made according to the principles hereafter explained, its latitude muſt have been about 76° or 77° north; for I find that, to the inhabitants of Mars, the declination of the ſun, June 25. 12 h. 15′ of our time, was about 9° 56′ ſouth *; and the ſpot muſt have been at leaſt ſo

* See p. 259. and 260.

far

far removed from the north pole as to fall a few degrees within the enlightened part of the disk, to become visible to us.

The south pole of Mars could not be many degrees from the center of the large bright southern spot of the year 1781, whose course is traced in fig. 27; though the spot was of such a magnitude as to cover all the polar regions farther than the 70th or 65th degree, and in that part which was on the meridian July 3, at 10 h. 54, perhaps a little farther.

In the next division of our subject will be shewn, that the inclination and position of the axis of Mars are such, that the whole circle, fig. 28. (which will appear to be in about 81° 52' of south latitude on the globe of Mars) was in view all the time the observations on the bright south polar spot of the year 1783, which are marked upon it, were made, but in so oblique a situation as to be projected into a very narrow ellipsis. See fig. 24. where *mn* is the little ellipsis in which the spot *a* revolved about the pole. Hence then we may easily account for the observed magnitude and brightness of the spot Oct. 23, 24, and 27. when it was exposed to us in its meridian splendour. Its situations Oct. 16. and 17. on one extreme of the parallel, as well as those of Oct. 5. and Nov. 1. on the other, gave us also a bright view of it: and, when we pass over to that half of the circle which lies beyond the pole, the much greater obliquity into which the spot must there be projected will perfectly account for its being smaller at 13 h. 7' of Oct. 17. than at 7 h. 47' of the same evening. It will also explain its smallness Oct. 4. and its increase Oct. 9. We shall have occasion hereafter to recur to the same figure, so that I take no notice at present of the angles of position which are marked upon it.

Of the direction or nodes of the axis of Mars, *its inclination to the ecliptic, and the angle of that planet's equator with its own orbit.*

From the foregoing article we may gather, that the bright polar ſpots on Mars are the moſt convenient objects for determining the ſituation of the axis of this planet; I ſhall therefore collect, in one view, all the meaſures I have taken of theſe ſpots for that purpoſe. Before I conſtructed a micrometer for taking the angle of poſition, I uſed to draw a line through the figure delineated of Mars to repreſent the parallel of declination; in a few of my firſt obſervations, therefore, I can only take the ſituation of the polar ſpots from ſuch drawings, and of conſequence no great accuracy in the angles, as to the exact number of degrees, can be expected.

1777, April 17. 7 h. 50′. A line drawn through the middle of the two bright polar ſpots *a* and *b*, fig. 1. makes an angle of about 63°, with a parallel of declination AB; the ſouthern ſpot preceding and the northern following.

My reaſon for chuſing a line drawn through both the ſpots rather than through one of them and the center is, firſt, that they were not ſituated quite oppoſite each other, and therefore, unleſs other obſervations had pointed out which was moſt polar, I ſhould evidently run the greater riſk in fixing on one of them in preference to the other. In the next place, we find by the ſecond obſervation, page 235. that in two hours and a half both ſpots were intirely gone out of the diſk. This plainly denotes, that they were both in the ſame half of a ſphere orthographically projected, and divided by a plane paſſing through the axis of Mars and the eye, but that neither of them were polar. Now, a line drawn through two points not far from oppoſite each other, both in the ſame hemiſphere, and both removed from the poles of it, muſt approach more to a paralleliſm with the axis, than a line drawn through either of them and the center.

1779, May 9. There being no bright ſpots by which to judge of the poſition of the poles, it is eſtimated from a well known dark equatorial ſpot, with a line drawn through the figure to denote a parallel of declination. By very rough eſtimation it is about 42° ſouth preceding.

May 11. The ſame figure, being drawn again in another ſituation, and alſo with a line giving a parallel of declination, points out, by the ſame rough eſtimation, 62° ſouth preceding.

1781, June 25. 11 h. 35′. The poſition of the ſpots *a* and *b*, fig. 3. with regard to a parallel of declination, meaſured with a micrometer 74° 32′. The ſpot *a* was ſouth preceding, and *b* north following.

July 15. 10 h. 12′. The angle of poſition, of the center of the ſpot *a*, fig. 9. through the center of the diſk, 74° 18′ ſouth preceding.

1783, Auguſt 16. Poſition of the ſpot *a*, 64° ſouth following the center; but as the planet is not full, the center becomes dubious, and the meaſure therefore may not be quite accurate, though taken with a 20 feet reflector; power 200.

Sept.

1783, Sept. 9. Position of the supposed south pole of Mars 65° 12′ south following; 7 feet reflector; power 460.

Sept. 22. Position of the same 72° 9′ s. following; 460.

Sept. 25. 13 h. 30′. Position of the south polar spot 56° 27′. very accurately taken, by bisecting the disk of Mars through the bright spot, and supposing the planet now near enough the opposition to induce no material error. Hitherto I have taken it through a supposed center by endeavouring to allow a little for what I thought the deficiency in the disk; but not to-night.

Oct. 4. 8 h. 46′. Position of the spot 51° 21′; Mars too low and hazy to depend much on the measure with so high a power as 460.

Oct. 5. The motion of the polar spot being now strongly suspected, or rather already known, I took the following measures, by way of discovering its quantity. 11 h. 30′. Position very exactly taken 50° 6′ s. following.

12 h. 0′. Position of the spot 49° 45′.

Oct. 7. 8 h. 20′. Position 55° 12′. In order to see how far this measure might be trusted to, I set 49° 36′ in the micrometer, which was evidently too small; next I took 51° 36′, which was also too small; after this, I took a new measure, and found 55° 24′, which appeared to me very exact. 10 h. 5′. The position now was 53°. 11 h. 50′. It measured 52° 12′. As there is nothing to distinguish the center, it is extremely difficult to please one's self in bringing the spot into a line with it.

1783, Oct. 10. 7 h. 50'. Position of the polar spot 57° 12; with 460, very accurate. I tried a few parts less of the micrometer, but found the measure too little. I see pretty distinctly, but the air is tremulous.

9 h. 55'. Position 52° 42'; very distinct.

12 h. 11'. Position 46° 30'; I see not quite so well now as I could wish.

14 h. 1'. Position 44° 12'; but liable to great uncertainty, on account of tremulous air; it becomes more difficult to distinguish the center when the planet is not perfectly defined.

Oct. 16. 7 h. 7'. Position 63° 9'. By way of trial I set 59° 36'; which was too small; also 60° 24' was too small; again, 61° 24' was not large enough. Then, taking a fresh measure, I found it 62° 48', which I thought right.

9 h. 33'. I took three measures, and thought the third, which was 65° 0', the best of all, for I saw the planet and the spot remarkably well.

Oct. 27. 8 h. 45'. Position of the polar spot 59° 30'. I took three other measures, of which 60° 39' appeared to me the best; it was taken with long attendance and many changes and trials of the wires in different positions; but the gibbosity of Mars is such, that measures of the situation of the spot are now no longer to be depended on.

These positions, I believe, will be sufficient for the purpose of settling the latitude of the polar spots, and thereby obtaining a correct measure of the situation of the real pole. I have referred those of the south polar spot of the year 1783 to the same circle which contains the observations that were made on the

the apparent brightness and magnitude of that spot, that they may be compared together. (See fig. 28.) The agreement of the measures, and the phænomena attending the motion of the spot, are sufficient to point out the meridian of the circle; for which, from a due consideration of these circumstances, I have fixed on the place where the spot was Oct. 10. 6 h. 46′.

Of the angles collected in fig. 28. we find 65° 0′ the largest, and 49° 45′ the smallest; but, on account of the different situation of the earth and Mars, the angle measured 7′ less Oct. 16. than it would have done had the planets remained in the places they were in Oct. 5. when the other measure was taken. This being added, we have 65° 7′. The difference between the two positions is 15° 22′. Now, the construction of fig. 28. being admitted, we see that the angles were nearly taken at the opposite extremes of the circle in which the spot moved. However, by the 5th column of Tab. III. Oct. 5. we have the situation of the spot in the circle with respect to the meridian 281° 44′, and Oct. 16. 114° 6′: therefore the south polar distance of the center of the spot is found, by taking half the sum of the sines of these angles to radius, as 7° 41′ (half of 15° 22′) to a fourth number, which is 8° 8′; and the latitude of the circle, in which the spot moved about the pole, therefore is 81° 52′ south. This being determined, we have the following correction for the angles of position: radius is to sine of the angular distance of the spot from the meridian as 8° 8′ to the required quantity. This must be added or subtracted, according as the case requires; and thereby we shall have the position of the true pole from any one of the measures.

I shall now apply the above to determine the situation of the axis of Mars. To this end, we see that, in the first place, the meaſures muſt be corrected for the latitude of the ſpot; next, they muſt be reduced to a heliocentric obſervation, which will alſo correct them from the difference occaſioned by the different ſituation of the planets when they were taken. This being done, we may ſelect two obſervations at a proper diſtance; from which, by trigonometry, we ſhall have the node and inclination of the axis. When theſe elements are obtained, it will be eaſy to ſee how other obſervations agree with them; which will afford the means of correcting or verifying the former calculations.

Let T, fig. 29. (tab. X) be the earth; ♋ Q *q* ♑ the ecliptic as ſeen from T; P the point of the heavens towards which the north pole of the earth is directed; M the place of the orbit of Mars *μ m* M, where an obſervation of the poles of that planet has been made, which is to be reduced to its heliocentric meaſure. And, firſt, ſuppoſe it to have been made at the time of the oppoſition of that planet. Then, the place M or Q in the ecliptic being given, we have the ſides Q ♋, ♋ P; whence the angle Q, of the right-angled triangle P ♋ Q, is found. This being added to, or taken from, the obſerved angle of poſition of the axis of Mars, according to circumſtances eaſily to be determined, reduces it to its heliocentric poſition. But if this obſervation was not made at the time of an oppoſition, but at ſome other place *m*, a ſecond correction is to be applied in the following manner.

Let the angle *q*, of the triangle P ♋ *q*, be found as before, and properly applied to the poſition of the axis of Mars now at *m*; then make the angle *m* S *μ*, at the ſun S, equal to the angle S *m* T, and *μ* will be the heliocentric place, where the angle of poſition, when ſeen from S, will appear to be as it was found at *m*, after the application of the firſt correction:

4 for

for Sμ being parallel to T*m*, and supposing the axis of Mars to preserve its parallelism while it moves from *m* to μ, appearances of Mars at μ to an eye at S, must be the same as they are at *m* to an eye at T.

The following table contains the result of calculations relating to the angles of fig. 28. In the first column are the times when the observations were made. In the second, the angles as they were taken. In the third column are the quantities of the angles Q, *q*, calculated from the geocentric longitudes contained in the third column of the third table. In the fourth column are the corrections for the situation of the spot in the circle of latitude obtained from the sines of the angles in the fifth column of the third table. In the fifth are the corrections requisite on account of the change of situation of the planets, during the interval between the several days on which the measures were taken; these are obtained from the third column of this table, and I have assumed the 4th of October, as being the observation nearest the opposition, to which I have reduced the other measures. In the sixth column are the angles of the second, corrected by the quantities contained in the fourth and fifth columns, applied according to their signs.

TABLE

TABLE IV.

Time of obſervation.			Angles taken.		Angle Q.		Firſt correction.		Second correct.	Angles corrected.	
D.	H.	M.	D.	M.	D.	M.	D.	M.	M.	D.	M.
Sept. 25	13	30	56	27	+23	10	−1	52	−8	54	27
Oct. 4	8	46	51	21	+23	18	+4	42	−0	56	3
5	11	50	50	6	+23	19	+7	39	+1	57	46
5	14	0	49	45	+23	19	+7	59	+1	57	45
7	8	20	{55 55	12 24}	+23	21	−0	7	+2	{55 55	7 19
7	10	5	53	0	+23	21	+3	26	+3	56	29
7	11	50	52	12	+23	21	+6	16	+3	58	31
10	7	50	57	12	+23	22	−4	57	+4	52	19
10	9	55	52	42	+23	22	−1	7	+4	51	39
16	7	7	{63 62	9 48}	+23	25	−7	47	+7	{55 55	29 8
16	9	55	65	0	+23	25	−7	23	+7	57	45

As we have no particular reaſon to ſelect one meaſure rather than another, a mean of all the 13 will probably be neareſt the truth; ſo that by theſe obſervations, which, as we ſaid before, are reduced to the 4th of October, 1783, we find the poſition of the axis of Mars that day to have been 55° 41′ ſouth following.

From the appearances of the ſouth polar ſpot in 1781, repreſented fig. 27. we may conclude, that its center was nearly polar. We find it continued viſible all the time Mars revolved on its axis; and, to preſent us generally with a pretty equal ſhare of the luminous appearance, a ſpot which covered from 45° to 60° of a great circle on the globe of Mars could not have any conſiderable polar diſtance: however, a ſmall correction in the angle of poſition ſeems to be neceſſary, which ſhould be taken from the meaſure of the 15th of July, becauſe that branch of the ſpot which probably extended fartheſt towards the

the equator, was then in the *following* quadrant. The measure of both the spots on June the 25th, 1781, is still more to be depended on, as giving us very nearly the position of the true pole; for it appears evident from the phænomena of the bright north-polar spot in fig. 26. that that spot was in the meridian when the measure was taken, while the southern spot was in the *preceding* quadrant near its greatest limit. Now, since an angle at the circumference of a circle is but half the angle at the center, when the arches which subtend these angles are equal, the correction necessary to be applied to the measure taken through the two spots will be but one half of the correction which would have been requisite had it been taken through the center; therefore, in order to reduce this to the condition of the former, we may suppose it to have been taken through the center of Mars when the spot was only 30, or 150 degrees from the meridian. It is also necessary to add 1° 54′ to the angle of July 15, which it would have measured more had the planets remained where they were June 25. This done, we may have the polar distance of the center of the spot as before. Half the sum of the sines (of 231° 38′ and 150°) to radius, as 50′ (half the difference between 74° 32′ and 76° 12′) to a fourth number, which is 1° 18′.

I should observe here, that the measures of the angle of position would be too large before the spot came to the meridian, and too small afterwards, the axis of Mars being south preceding; whereas, in fig. 28. they would be too small before, and too large after, the meridian passage, the pole being south following.

These two observations arranged as those in the fourth table, and reduced to the time of the 25th of June, will stand as follows.

TABLE

TABLE V.

Time of obſervation.	Angles taken.	Angle Q.	Firſt correction.	Second correct.	Corrected Angle.
D. H. M.	D. M.	D. M.		D. M	D. M.
June 25 11 36	74 32	− 10 14	+ { half of 1° 18′	− 0 0	75 11
July 15 10 12	74 18	− 8 20	− 1 1	+ 1 54	75 11

I am to remark, that we have here admitted both meaſures as equally good; and that, therefore, the reſult is a mean of them both, and ſhews the axis of Mars, June 25, 1781, to have been 75° 11′ ſouth preceding.

Our next buſineſs will be to reduce theſe two geocentric obſervations to a heliocentric meaſure. This is to be done, as we have ſhewn before, by a calculation of the angle Q, fig. 29. The reſult of it ſhews, that 10° 14′ are to be ſubtracted from the mean corrected angle of poſition, reduced to June 25, 1781; and 23° 18′ to be added to the angle which is the corrected mean of 13 meaſures, reduced to Oct. 4, 1783. Hence we learn, that on thoſe days and hours, when the heliocentric places of Mars were 9 s. 24° 35′, and 0 s. 7° 15′ (which would happen about July 18, 1781, and Sept. 29, 1783) an obſerver placed in the ſun would have ſeen, on the former, the axis of Mars inclined to the ecliptic 64° 57′, the north pole being towards the left; and on the latter, he would have ſeen the ſame axis inclined to the ecliptic 78° 59′, the north pole being then towards the right.

The firſt concluſion we may draw from theſe principles is, that the north pole of Mars muſt be directed towards ſome point of the heavens between 9 s. 24° 35′ and 0 s. 7° 15′; becauſe the change of the ſituation of the pole from left to right, which

which happened in the time the planet paffed from one place to the other, is a plain indication of its having gone through the node of the axis. Next, we may alfo conclude, that the node muft be confiderably nearer the latter point of the ecliptic than the former; for, whatever be the inclination of the axis, it will be feen under equal angles at equal diftances from the node.

But, by a trigonometrical procefs of folvinga few triangles, we foon difcover both the inclination of the axis, and the place where it interfects the ecliptic at rectangles (which, for want of a better term, I have perhaps improperly called its node). Accordingly I find, by calculation, that the node is in 17° 47′ of Pifces, the north pole of Mars being directed towards that part of the heavens; and that the inclination of the axis to the ecliptic is 59° 42′.

We fhall now compare the obfervations of an earlier date with thefe principles, to fee how far they agree. Some of the particulars and calculations relating to them are as follow.

T A B L E VI.

Times of Obfervation.				Eftimations.	Geoc. longit.			Angle Q.		2d correct.
	D.	H.	M.	D.	S.	D.	M.	D.	M.	
1779, May	9	12	0	42	7	22	20	+14	45	+ 0
May	11	12	0	62	7	21	40	+15	11	+ 26
1777, Apr.	17	7	50	63	6	3	34	+23	26	

May the 9th, 1779, as we have feen, the angle of pofition was roughly eftimated at 42°, and May 11. at 62°. The great difagreement of thefe coarfe eftimations is undoubtedly owing to the very different fituation of the dark fpot from which they

 were

were taken; however, ſince we do not mean to uſe theſe obſervations in our calculations, they may ſuffice in a general way to ſhew, that the axis of Mars was actually about that time in ſuch a ſituation as our principles give it: for, reducing the two poſitions to the 9th of May, that of the 11th, from an allowance of 26′ for the ſituation of the planets, will become 62° 26′; and a mean of the two, 50° 13′ ſouth preceding; which, reduced to a heliocentric obſervation, gives 66° 30′, the north pole lying towards the left. Now, on calculating from the poſition of the node and inclination of the axis before determined, we find, that the heliocentric angle was 62° 49′, the north pole pointing towards the left; and a nearer agreement with theſe principles could hardly be expected from eſtimations ſo coarſe. If we go to the year 1777, and take the poſition of the two bright ſpots obſerved the 17th of April, we have 63° ſouth preceding; this, reduced to a heliocentric quantity, gives 86° 26′ of inclination, the north pole being to the left. By calculating we find, that that pole was then actually 81° 27′ inclined to the ecliptic, and pointed towards the left as ſeen from the ſun.

The inclination and ſituation of the node of the axis of Mars with reſpect to the ecliptic being found may thus be reduced to that planet's own orbit. Let EC, fig. 30. (tab. X.) be a part of the ecliptic; OM part of the orbit of Mars; PEO a line drawn from P, the celeſtial pole of Mars, through E, that point which has been determined to be the place of the node of the axis of Mars in the ecliptic, and continued to O where it interſects the orbit of Mars. Now, if according to Mr. DE LA LANDE we put the node of the orbit of Mars for 1783, in 1 s. 17° 58′, we have from the place of the node of the axis (that is, 11 s. 17° 47′) to the place of the node of the orbit, an arch EN of 60° 11′; in the triangle NEO, right-angled at E, there is alſo given the angle ENO, according to the ſame author, 1° 51′, which is the inclination of the orbit of Mars to the ecliptic. Hence we find the angle EON 89° 5′, and ſide ON 60° 12′. Again, when Mars is in the node of its orbit N, we have, by calculation from our principles, the angle PNE = 63° 7′, to which, adding the angle ENO = 1° 51′, we have PNO = 64° 58′; from which two angles PON and PNO with the diſtance ON, we obtain the inclination of the axis of Mars, and place of its node with reſpect to that planet's own orbit; the inclination being 61° 18′, and the place of the node of the axis 58° 31′ preceding the interſection of the ecliptic with the orbit of Mars, or in our 19° 28′ of Piſces.

Being thus acquainted with what the inhabitants of Mars will call the obliquity of their ecliptic, and the ſituation of their equinoctial and ſolſtitial points, we are furniſhed with the means of calculating the ſeaſons on Mars; and may account, in a manner which I think highly probable, for the remarkable appearances about its polar regions.

But firſt it may not be improper to give an inſtance how to reſolve any query concerning the martial ſeaſons. Thus, let it be required to compute the declination of the Sun on Mars, June 25, 1781, at midnight of our time. If ♈ ♉ ♊ ♋, &c. fig. 31. (tab. X.) repreſent the ecliptic of Mars, and ♈ ♋ ♎ ♑ the ecliptic of our planet, A*a*, *b*B, the mutual interſection of the martial and terreſtrial ecliptics, then there is given the heliocentric longitude of Mars, ♈*m* = 9 s. 10° 30′; then taking away ſix ſigns, and ♎ *b*, or ♈*a* = 1 s. 17° 58′, there remains *bm* = 1 s. 22° 32′. From this arch, with the given inclination, 1° 51′, of the orbits to each other, we have coſine of inclination to radius, as tangent of *bm* to tangent of BM = 1 s. 22° 33′. And taking away B♈ = 1 s. 1° 29′, which is the complement to ♑ B (or ♋ A, already shewn to be 1 s. 28° 31′) there will remain ♈M = 0 s. 21° 4′, the place of Mars in its own orbit*; that is, on the time abovementioned, the sun's longitude on Mars will be 6 s. 21° 4′, and the obliquity of the martial ecliptic 28° 42′ being also given, we find, by the usual method, the sun's declination 9° 56′ south.

The analogy between Mars and the earth is, perhaps, by far the greatest in the whole solar system. Their diurnal motion is nearly the same; the obliquity of their respective ecliptics, on which the seasons depend, not very different; of all the superior planets the distance of Mars from the sun is by far the nearest alike to that of the earth: nor will the length of the martial year appear very different from that which we enjoy, when compared to the surprising duration of the years of Jupiter, Saturn, and the Georgium Sidus. If, then, we find that the globe we inhabit has its polar regions frozen and covered with mountains of ice and snow, that only partly melt when alternately exposed to the sun, I may well be permitted to surmise that the same causes may probably have the same effect on the globe of Mars; that the bright polar spots are owing to the vivid reflection of light from frozen regions; and that the reduction of those spots is to be ascribed to their being exposed to the sun. In the year 1781, the south polar spot was extremely large, which we might well expect, since that pole had but lately been involved in a whole twelvemonth's darkness and absence of the sun; but in 1783 I found it considerably smaller than before, and it decreased continually

* If no very great accuracy be required, we may add 3 s. 19° 34′ to any given place of our ecliptic, which will at once reduce it to what it should be called on the orbit of Mars, and will always be true to within a minute.

from

from the 20th of May till about the middle of September, when it seemed to be at a stand. During this last period the south pole had already been above eight months enjoying the benefit of summer, and still continued to receive the sun-beams; though, towards the latter end, in such an oblique direction as to be but little benefited by them. On the other hand, in the year 1781, the north polar spot, which had then been its twelve-month in the sun-shine, and was but lately returning to darkness, appeared small, though undoubtedly increasing in size. Its not being visible in the year 1783 is no objection to these phænomena, being owing to the position of the axis, by which it was removed out of sight; most probably, in the next opposition we shall see it renewed, and of considerable extent and brightness; as, by the position of the axis of Mars, the sun's southern declination will then be no more than 6° 25′ on that planet.

Of the spheroidical figure of Mars.

That a planetary globe, such as Mars, turning on an axis, should be of a spheroidical form, will easily find admittance, when two familiar instances in Jupiter and the earth, as well as the known laws of gravitation and centrifugal force of rotatory bodies, lead the way to the reception of such doctrines. So far from creating difficulties or doubts, it will rather appear singular, that the spheroidical form of this planet, which the following observations will establish, has not already been noticed by former astronomers; and yet, reflecting on the general appearances of Mars, we soon find that opportunities for making observations on its real form cannot be very frequent: for, when it is near enough to view it to an advantage, we see it generally gibbous, and its oppoſitions are ſo ſcarce, and of ſo ſhort a duration, that in more than two years time we have not above three or four weeks for ſuch obſervations. Beſides, aſtronomers being already uſed to ſee this planet generally diſtorted, the ſpheroidical form might eaſily be overlooked.

Obſervations relating to the polar flattening of Mars.

1783, Sept 25. 9 h. 50′. I can plainly ſee that the equatorial diameter of Mars is longer than the polar. Meaſure of the equatorial diameter 21″ 53‴; of the polar diameter 21″ 15‴ *full meaſure*, that is, certainly not too ſmall. The wires were ſet as outward tangents to the diſk, and the zero, as well as the meaſures, were taken by the light of Mars.

Sept. 28. 14 h. 25′. I ſhewed the difference of the polar and equatorial diameters of Mars to Mr. WILSON, Aſſiſtant Profeſſor of Aſtronomy at Glaſgow. He ſaw it perfectly well, ſo as to be entirely convinced it was not owing to any defect or diſtortion occaſioned by the eye lens; and, becauſe I wiſhed him to be ſatisfied of the reality of the appearance, while he was obſerving, I reminded him of ſeveral well known precautions; ſuch as cauſing the planet to paſs directly through the center of the field of view, and judging of its figure at the time when it was moſt diſtinct and beſt defined, and ſo forth.

Sept. 29. I ſhewed the difference of the polar and equatorial diameters of Mars to Dr. BLAGDEN and Mr. AUBERT. Dr. BLAGDEN not only ſaw it immediately,

diately, but thought the flattening almoſt as much as that of Jupiter. Mr. AUBERT alſo ſaw it very plainly, ſo as to entertain no manner of doubt about the appearance.

As we cannot take too many opportunities of confirming our own obſervations by the eyes of other obſervers, I eſteemed it a very fortunate circumſtance to have the honour of a viſit from theſe gentlemen at ſo particular a time, Mars being this day within 37 hours of the oppoſition, and yeſterday when Mr. WILSON ſaw it, within about two days and a half.

1783, Sept. 30. 10 h. 52′. The difference in the diameters of Mars is very evident and conſiderable.

Meaſure of the equatorial diameter	22″ 9‴	with 278.
Second meaſure - -	22″ 31‴	full large.
Polar diameter very exact -	21″ 26‴.	

Oct. 1. 10 h. 50′. I took meaſures of the diameters of Mars with my 20-feet reflector. The equatorial meaſured 103 parts of the micrometer; the polar 98. The value of the diviſions in ſeconds and thirds not being well determined, on account of ſome late change in the focal length of the ſeveral 20-feet object metals I uſe, we have only from theſe meaſures the proportion of the diameters as 103 to 98.

13 h. 15′. Every circumſtance being favourable, I took the following meaſures of the diameters of Mars with my 7-feet reflector, and a diſtinct power of 625.

Equatorial diameter	22″ 12‴	narrow meaſure.
	22″ 46‴	rather full.
	22″ 35‴	exact.

Polar

Polar diameter 21″ 24‴

21″ 33‴ very exact.

I saw Mars perfectly well all the time I measured, with all its figures upon the disk appearing distinctly; and, I think, these measures may be depended upon better than any I have yet taken.

1783, Oct. 5. 14 h. 0′. The difference of the diameters is very sensible.

Oct. 7. 9 h. 43′. The flattening of the poles is very visible.

13 h. 40′. I turned my Newtonian 7-feet reflector one quarter round, so as to bring the place to look in at to the bottom; and, as well as the uneasy posture would allow, I saw the flattening of the poles the same as when I looked in at the side; power 460.

14 h. 30′. With a 3½ feet achromatic telescope and a single eye lens, I saw the difference of the polar and equatorial diameters very plainly.

Oct. 9. 8 h. 40′. I turned my reflector 90° round, so as now to look in at the upper end, but saw not the least difference in appearances; for, returning it again immediately to its usual position, in both cases the equatorial diameter appeared a little longer than the other; power 278, and the evening fine.

I turned the great speculum one quadrant in its cell, but appearances were not in the least altered; the equatorial diameter still was a little longer than the polar one.

I tried a very fine new object speculum, and found also the equatorial diameter a little longer than the polar one.

1783,

1783, Oct. 9. 10 h. 47′. The flattening at the poles very visible.

Oct. 10. 9 h. 55′. A little of the polar flattening is visible, so as to admit of no doubt; power 460, very distinct.

11 h. 32′. Mars visibly flattened, but not much; the achromatic shews it also.

11 h. 42′. The disk of Mars is visibly spheroidical.

Oct. 11. 7 h. 37′. Mars is plainly gibbous, therefore measures and estimations of the diameters must for the future be improper.

11 h. 12′. It is rather difficult to say of what shape Mars is now, for it is partly flattened and partly gibbous; but the gibbous side not being quite in the polar direction of Mars, this produces altogether an odd mixture of shapes: however, upon the whole, the polar diameter is still rather the smallest.

11 h. 13′. The *preceding* side of Mars shews the flattening of the poles, while the *following* is terminated by an elliptical arch.

Oct. 12. 11 h. 12′. The flattening upon the whole is visible.

Oct. 17. 13 h. 7′. The effect of gibbosity is scarcely equal to the flattening; or, upon the whole, the planet is still rather broader over the equator than over the poles.

Nov. 1. 7 h. 56′. The semi-disk, which is *full*, is evidently part of an oblate spheroid; but, to an eye not attentively looking for it, and knowing the shape and exact situation of the poles of Mars, this would probably not appear.

1783, Nov. 10. 9 h. 30′. The gibbosity of Mars is now such, that the polar diameter is considerably longer than the equatorial; but the deficiency not being exactly from pole to pole, makes the disk of a crooked, irregular figure, and renders precision in this estimation impossible; otherwise the phase of Mars would have made a pretty good micrometer upon the equatorial diameter, and it was with such a view I had directed my attention to this circumstance: appearances, however, are visibly in favour of the polar diameter's being the longest.

We find that the quick alterations in the visible disk of Mars, during the time it is in the best situation for us to observe it, are such, that if we were to use many measures which have been taken of its diameters, we should be obliged to have recourse to a computation of its phases, in order to make proper allowance for them. Now, since these changes are in a longitudinal direction, and the poles of Mars are not perpendicular to the ecliptic, it would bring on a calculation of small quantities, which it is always best not to run into where it can be avoided. For this reason, I shall at once settle the proportion of the equatorial to the polar diameter of this planet, from the measures which were taken on the very day of the opposition. I prefer them also on another account, which is, that they were made in a very fine, clear air, and were repeated with a very high power, and with two different instruments, of whose faithful representation of celestial objects, the many observations on very close double stars I have made with them have given me very evident proofs.

As

As we are at preſent only in queſt of the proportion of one diameter to the other, the meaſures of the 20-feet reflector, though not given in angular quantities, will equally ſuffice for the purpoſe. By them we have the equatorial diameter to the polar as 103 to 98, or as 1355 to 1289. I have turned the proportion into the latter numbers by way of comparing them the better with the meaſures of the 7-feet reflector. By that inſtrument the equator of Mars, Oct. 1. we find, was meaſured three times; but from the remarks annexed to the different reſults, I think the third meaſure ſhould be uſed. Indeed, on taking the difference of the two firſt which is 34‴, and dividing by three, we have the quotient $11\frac{1}{3}'''$; then, allotting two-thirds to the firſt, becauſe the remark ſays poſitively "narrow meaſure," it becomes 22″ $34\frac{1}{3}'''$, and taking one-third from the ſecond, which is expreſſed doubtfully, "rather too full," it becomes 22″ $35\frac{1}{3}'''$: this reflection on the two firſt meaſures gives additional validity to the third, which is 22″ 35‴, or 1355‴. The polar diameter was meaſured twice; and as no reaſon appears againſt either of the obſervations, I ſhall take the mean of both, which is 21″ 29‴, or 1289‴; ſo that by theſe meaſures the equatorial diameter of Mars is to the polar as 1355 to 1289. A leſs perfect agreement between the proportions of the diameters ariſing from the meaſures of the 20-feet reflector and thoſe which we have juſt now deduced from the 7-feet, would have been ſufficient for our purpoſe, as we might eaſily have excuſed one or two thouſandths of the whole quantity; however, we have no cauſe to be diſpleaſed with this coincidence, though it ſhould in part be owing to accident, and therefore ſhall admit the above proportion, and proceed to a farther examination of it.

 In

In the firſt place, it will be neceſſary to ſee whether any correction be required on account of the different heliocentric and geocentric ſouth latitude of Mars; which would apparently compreſs the polar diameter a little, by the defect of illumination on the north. On computation we find, that a difference ariſing from that cauſe would give the longitudinal diameter to the latitudinal as 20000 to 19987; which being much leſs than one thouſandth part of the whole, may therefore be neglected.

But next, a very conſiderable correction muſt be admitted, when we take into account the poſition of the axis of Mars. The declination of the ſun on that planet, at the time the meaſures were taken, was not leſs than 27° ſouth; ſo that the poles were not in the circumference of the diſk by all that quantity. On a ſuppoſition then, that the figure of Mars is an elliptical ſpheroid, we are now to find the real quantity of the polar diameter from the apparent one. It has been proved, that, in the ellipſis, the exceſſes of any diameters above the polar one are as the ſquares of the coſines of the latitudes *; but the diameter at rectangles to the equator of Mars, which was expoſed to our view in the late oppoſition, was not the polar one, but ſuch as muſt take place in a latitude of 63°. Putting therefore $m =$ coſine of 63°, $a = 1355$, $b = 1289$, $x =$ the polar axis, we have $1 : m^2 :: a - x : b - x$. And $\frac{b - m^2 a}{1 - m^2} = x$; which gives us 1272 nearly, for the polar diameter. The true proportion, therefore, of the equatorial to the polar diameter will be as 1355 to 1272; which, reduced to ſmaller but leſs accurate numbers, is 16 to 15 nearly.

* Aſtr. par M. DE LA LANDE, § 2680.

I ſhall

I ſhall now alſo mention ſome of the other meaſures, but with a view only to ſhew that they are very conſiſtent with the above determination. From thoſe of the 30th of September, for inſtance, we collect the proportion of the diameters of Mars as 1340 to 1286; or, reduced to our former numbers, 1355 to 1300. Now, ſince theſe meaſures were taken the night before the oppoſition, they muſt on that account be as good as the former; and, had thoſe of the day of oppoſition not been preferred, becauſe they were oftener repeated, and the ſuperior power of the 7, and great light of the 20-feet reflector, gave them additional weight, I ſhould have taken them into the account; the very ſmall difference, however, cannot but ſtrengthen the reſults of the former meaſures.

From the obſervations of the 25th of September we have the proportion of the diameters as 1313 to 1275; and if the equatorial meaſure be increaſed in the ratio of 20000 to 19953, on account of the different heliocentric and geocentric longitude, Mars not being at the full, it will give the ratio of 1316 to 1275; or, conforming to our former numbers, as 1355 to 1312. I have not been very ſtrict in the application of the correction deduced from the phaſes of Mars, ſince no other uſe was intended to be made of theſe numbers than merely to ſhew, that they do not very greatly differ from thoſe we have aſſigned before *.

It

* If more ſtrictneſs be required, let EC, fig. 32. be the ecliptic; PS its poles; *ps* the poles of Mars, and *eq* its equator. Then, the angle *pm*C being found, by calculation, we ſhall have C*m* (radius) to *cm* (coſine of the difference between the heliocentric and geocentric longitude) as *qv* (ſine of the angle *qmv* or *pm*C) to *ov*. Then, ſince with Mars C*c* can never be very great, the ſmall triangle *qno* may be taken for ſimilar to *qvm*; therefore *qm* (radius) is to *qv* (ſine of *pm*C)

It was obferved, Oct. 17, 1783, that the equatorial diameter of Mars was ftill greater than the polar, notwithftanding the depredation of the defect of light upon it. On calculating the phafes, we find, that the longitudinal diameter was, that day, to the latitudinal one as 19711 to 20000, which therefore could not be an equal balance to oppofe the fpheroidical figure fo as to render it invifible.

But, Nov. 10. the proportion of the longitudinal diameter to the latitudinal one, from a computation of the phafe of Mars, muft have been as 18762 to 20000; and accordingly it was by obfervation found to be more than fufficient to take off all appearance of the polar flattening, and leave a vifible excefs in the axis above the equator.

To obviate any doubts concerning a fallacy that might arife from the convexity of the eye-glafs, or irregular fhape of the fmall fpeculum, I need only refer, for the latter, to the experiments of the 7th and 9th of October, 1783: for fhould the fhort diameter of my fmall plane fpeculum have occafioned a compreffing of the polar diameter of Mars when expofed to it, half a turn of the telefcope muft bring the other diameter of that fpeculum into the fame fituation, and a contrary effect would have followed. With regard to the former, not only the experiments made with the achromatic, but principally the obfervation with the 20-feet reflector, where I ufed a compound eye-piece magnifying only about 300 times, will fufficiently exculpate the eye-glaffes. It is alfo well known, that in a fingle lens the diftortion of the images, if any fuch there

pmC) as qo ($=qv-vo$) to qn; which is the required correction or deficiency of the equatorial diameter eq of Mars.

Or, putting mC $=1$ and $vq=m=$ cofine of the angle Pmp; it will be $qn=m^2 \cdot e$C.

7 fhould

should be, will equally affect the wires of the micrometer, and give a true measure notwithstanding; and the compound eye-piece I used with the 20-feet reflector had likewise the same advantage, for it is constructed on the plan lately proposed by Mr. RAMSDEN in the Philosophical Transactions *, which he was so obliging as to communicate to me about a twelve-month ago, and which I immediately adapted to my large micrometers.

On the subject of the figure of Mars I ought to remark also, that perhaps the measures which were taken of its diameters during the last opposition will enable us to ascertain its real size with greater accuracy than has been done before. The micrometer which can distinguish with precision between the equatorial and polar diameters of this small planet, will certainly be admitted as an evidence of considerable consequence; and since the result of these measures is pretty different from what former observations give us, I should not omit mentioning it.

We have seen that the equatorial diameter, on the day of the opposition, measured 22″ 35‴. The distance of Mars from the earth at that time was .40457, the mean distance of the earth from the sun being 1; therefore, 22″ 35″ reduced to the same distance will be no more than 9″ 8‴.

I shall conclude this subject with a consideration relating to the atmosphere of Mars. Dr. SMITH † reports an observation of CASSINI's, where "a star in the water of Aquarius, at the "distance of six minutes from the disk of Mars, became so "faint before its occultation, that it could not be seen by the "naked eye, nor with a 3-feet telescope." It is not mentioned

* Vol. LXXIII. p. 94.

† Optics, § 1096.

tioned what was the magnitude of the ſtar; but, from the circumſtance of its becoming inviſible to the naked eye, we may conclude, that it muſt have been of the ſixth or ſeventh magnitude at leaſt. The reſult of this obſervation would indicate an atmoſphere of ſuch an extraordinary extent, ſince at the diſtance of 36 ſemi-diameters of the planet it ſhould ſtill be denſe enough to render ſo conſiderable a ſtar inviſible, that it will certainly not be amiſs to give an obſervation or two which ſeem of a very different import.

1783, Oct. 26. There are two ſmall fixed ſtars preceding Mars, of different ſizes; with 460 they appear both duſky red, and are pretty unequal; with 278 they appear conſiderably unequal. The diſtance from Mars of the neareſt, which is alſo the largeſt, with 227 meaſured 3′ 26″ 20‴. Some time after, the ſame evening, the diſtance was 3′ 8″ 55‴, Mars being retrograde. I ſaw them both very diſtinctly. I viewed the two ſtars with a new 20-feet reflector of 18,7 inches aperture, and found them, as I expected, very bright.

Oct. 27. I ſee the two ſmall ſtars again. The ſmall one is not quite ſo bright in proportion to the large one as it was laſt night, being a good deal nearer to Mars, which is now on the ſide of the ſmall ſtar; but when I draw the planet aſide, or out of view, I ſee it then as well as I did laſt night, The diſtance of the ſmall ſtar meaſured 2′ 56″ 25‴ *.

* The meaſures were accurate enough for the purpoſe, though not otherwiſe to be depended on nearer than, perhaps, ſix or eight ſeconds.

.VI. p. 272.

.X.p.272.

Basire Sc.

The largest of the two stars on which the above observations were made cannot exceed the twelfth, and the smallest the thirteeenth or fourteenth magnitude; and I have no reason to suppose that they were any otherwise affected by the approach of Mars, than what the brightness of its superior light may account for. From other phænomena it appears, however, that this planet is not without a considerable atmosphere; for, besides the permanent spots on its surface, I have often noticed occasional changes of partial bright belts, as in fig. 1. and 14.; and also once a darkish one, in a pretty high latitude, as in fig. 18. And these alterations we can hardly ascribe to any other cause than the variable disposition of clouds and vapours floating in the atmosphere of that planet.

Result of the contents of this paper.

The axis of Mars is inclined to the ecliptic 59° 42′.

The node of the axis is in 17° 47′ of Pisces.

The obliquity of the ecliptic on the globe of Mars is 28° 42′.

The point Aries on the martial ecliptic answers to our 19° 28′ of Sagittarius.

The figure of Mars is that of an oblate spheroid, whose equatorial diameter is to the polar one as 1355 to 1272, or as 16 to 15 nearly.

The equatorial diameter of Mars, reduced to the mean distance of the earth from the sun, is 9″ 8‴.

And that planet has a considerable but moderate atmosphere, so that its inhabitants probably enjoy a situation in many respects similar to ours.

Datchet, Dec. 1, 1783. W. HERSCHEL.

XX. *A Description of the Teeth of the* Anarrhichas Lupus Linnæi, *and of those of the* Chætodon nigricans *of the same Author; to which is added, an Attempt to prove that the Teeth of cartilaginous Fishes are perpetually renewed. By Mr.* William Andre, *Surgeon; communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read March 18, 1784.

THE amazing variety there is in the external form of fishes must be obvious to a common observer; and whoever examines will be convinced, that the same variety prevails in their internal structure. No parts, perhaps, afford a more convincing proof of the last assertion than the teeth of fishes. To adduce a few instances, let us only recollect the tuberculated teeth in the thorn-back; the triangular serrated teeth in the shark; the slender flexible teeth in the *chætodontes*, or angel-fishes. There is not only a difference of their form, but also in the substances of which they are composed; some being of a soft horny nature; others made up of bone; others of that substance we call enamel in the teeth of quadrupeds; and some having the apparent hardness and transparency of crystal. We may also notice their uncommon situation; many fishes having teeth not only in their jaws, but on the tongue, the palate, and about the *fauces*.

To illustrate in some degree this part of natural history, I shall describe the teeth of the *Anarrhichas Lupus*, or Sea-wolf, and those of the *Chætodon nigricans*, a species of Angel-

Angel-fifh. The former have been but imperfectly defcribed, and never reprefented diftinct from the fifh, without which it is impoffible to have any exact idea of their difpofition, number, or form, while the true fhape and compofition of the latter, from their minutenefs, have been entirely overlooked. I fhall then attempt to prove, that a continual renovation of the teeth takes place in cartilaginous fifhes.

THE SEA-WOLF is a fierce and ravenous fifh, as its name imports, found in the northern parts of the globe, where it frequently grows to the length of four feet and upwards.

The jaws of the Wolf-fifh are made up of feveral bones, to each of which a greater or lefs number of teeth are affixed; but, before I enter upon the defcription of them, I fhall take notice of the palate (marked A. tab. XI.), that being a kind of bafis or fupport to the other bones, to which they are all more or lefs connected. The palate is a thick and firm bone united above to the bones of the *cranium* and nofe, and ending below in a flat oval furface, on which are incrufted about twelve or thirteen ftrong, blunt, and rather flat teeth of the *molar* or grinder kind. The external edges of the teeth are the moft prominent; by which means a hollow is formed in the middle of the palate.

The upper jaw is compofed of three bones, two of which (BB) are placed laterally, forming the fides of the upper jaw, and the third (C) anteriorly, making the fore-part of the jaw. The third bone may be divided through its middle into two portions; but fince it has the appearance of one bone only, the connection being very firm, I fhall defcribe it agreeably to that appearance, to prevent needlefs divifions.

The fide bones of the upper jaw have nearly the fhape of an italic *f*. At their pofterior ends may be obferved a fmooth articular ſurface, for their connection with a ſimilar ſurface on the poſterior extremities of the lower jaw; and on their anterior ends there are two rows of teeth. The external row conſiſts of three or four ſharp or conical teeth; and the internal row of four or five blunt and rather flat ones. Theſe bones are connected to the palate and bones of the noſe by looſe but ſtrong ligaments.

The third bone of the upper jaw, which may be called the anterior or naſal portion, is of a triangular form, connected above to the bones of the noſe, and ending below in a flat ſurface, thick-ſet with ſharp conical teeth. The external teeth, about four in number, are large and ſtrong, and bend a little inwards; but the internal ones are ſmall, and nearly ſtraight, of which we may reckon about ten.

This bone is connected above (as I have before obſerved) to the bones of the noſe; between which a complete joint is formed, of that kind called by anatomiſts *ginglymus*, that is, where the projecting parts of one bone are received by correſponding cavities in the other. Like other articulations, it is furniſhed with a capſular ligament, and no doubt an apparatus for the ſecretion of *ſynovia*. Although a joint exiſts between this bone, and thoſe of the noſe, yet no muſcles are provided for its motion, which depends entirely upon the reſiſtance made by thoſe hard bodies which the animal takes into its mouth.

The lower jaw (D) conſiſts of two bones, united at their fore-parts by a ſtrong ligament, which allows of ſome motion. On their anterior extremities are placed ſix large and as many ſmall ſharp and conical teeth; the large teeth are placed externally, and their points are bent a little inwards; while the ſmall ones, which ſtand within them, are nearly ſtraight. Behind theſe are two or three rows of grinder teeth. The external teeth ſtand nearly upright; but the internal ones are placed obliquely, inclining towards each other.

The teeth are formed of a hard bony matter, not covered with enamel as in ſome animals; nor is there an equal diſtribution of enamel and bone as in ſome others. They are not fixed in ſockets, but are faſtened to the jaws in the ſame manner as the *epiphyſes* are united to the bodies of the bones in young animals.

From the foregoing deſcription it will appear, that the anterior ſharp teeth of the Sea-wolf are admirably calculated for ſeizing its prey, while the poſterior grinding teeth ſerve to break down the hard ſhells of lobſters, crabs, muſcles, ſcollops, &c. which this animal is known to feed upon. The external teeth on the ſides of the upper and lower jaw being higher than thoſe placed within them, a hollow is formed above and below, in which the convex ſhells of cruſtaceous animals, &c. are confined during their compreſſion between the jaws, which is effected by the action of ſtrong muſcles placed on the ſides of the head. The jaws being made up of a number of pieces, and connected by looſe ligaments, a freedom of motion is allowed, and the colliſion or ſhock ariſing from the comminution of hard bodies is ſo much the leſs by being divided among a number of bones.

MERRET informs us *, the *lapis buſonites* are the flat grinder teeth of this fiſh petrified. But certainly theſe foſſils are not the production of the Sea-wolf alone, ſince they may originate from all thoſe fiſhes which have flat teeth in their palate or jaws; a ſtructure which the French naturaliſts diſtinguiſh by the appellation of *palais pavé*.

* Pinax Rerum Naturalium Britannicarum.

OF

OF THE CHÆTODON NIGRICANS.

The individual which furnished the following account was brought from the West Indies, and measured about five inches in length *. Its teeth (the only parts I mean to describe) were so small as to require the assistance of a microscope to discover their real shape. There were fourteen teeth in each jaw, seven of which from the upper one are represented tab. XII. They consist of a cylindrical body fixed in the jaw, above which they spread out into a broad and rather flat surface, on the edges of which are twelve or thirteen *denticuli*, making an uncommon appearance, and totally different from the teeth of any other animal. Another singularity is their being transparent, unless viewed with a deep magnifier, when a few opaque lines may be perceived, which point out the cellular part of the tooth through which the blood vessels ramify, which are destined for its growth and nourishment. They are not all of the same length. Those in the anterior parts of the jaws are the longest, from whence they gradually diminish in length as they approach the angles of the mouth.

From the foregoing description of the teeth of the *Chætodon nigricans*, this fish seems to be misplaced in the *Systema Naturæ* of LINNÆUS; since one generic distinction of the *Chætodontes* is to have numerous, slender, and flexible teeth; whereas the teeth of the *Chætodon nigricans* are few in number, placed in one row, and of a crystalline hardness.

* This fish is well represented in DU HAMEL Traité général des Pêsches, tom. III. seconde partie, section IV. planche xii. under the name of *Chirurgien ou Porte Lancette*.

OF THE TEETH OF CARTILAGINOUS FISHES.

When STENO examined the teeth of the ſhark, he was ſurpriſed to find a great number of them placed on the inſide of each jaw, lying cloſe to the bone, and many of them buried in a looſe ſpongy fleſh; concluding that theſe internal teeth could be of little or no uſe to the animal. Mr. HERISSANT * afterwards ſhewed the uſe of theſe internal or poſterior teeth, by proving, that as the anterior teeth of each row are broken off, drop out, or wear away, the poſterior ones come forward to ſupply their places †.

But though it be certain that the anterior teeth, when loſt, are replaced by the poſterior ones, neither of the above naturaliſts, or any other that I know of, have attempted to aſcertain how often this circumſtance happens. Whether the renovation be perpetual during life; or whether that operation be ſuſpended after a limited number of teeth have been ſupplied.

From a ſingular circumſtance, which I met with ſome time ago, I am inclined to think the former is the fact; or, that in cartilaginous fiſhes, ſuch as ſharks, rays, &c. there is a perpetual renovation of the teeth.

Being engaged in diſſecting the jaws of a very large ſhark, I was ſurpriſed to find a portion of that ſharp, bearded bone found in the tail of the fire-ſtaire, or ſting-ray ‡, driven quite

* BOMARE Dictlonaire d'Hiſtoire Naturelle, article Requien.

† It may not be improper on this occaſion to point out a miſtake which ſome naturaliſts have fallen into, in allowing a ſet of muſcles for raiſing the numerous teeth placed in the jaws of ſharks. I have frequently diſſected the jaws of thoſe animals, and am certain no ſuch muſcles exiſt, nor are they indeed at all neceſſary.

‡ *Raia Paſtinaca* LINNÆI. The French naturaliſts, on account of the bone in the tail, call this fiſh *Raie baionnette*.

through

through the lower jaw among the posterior teeth, and fixed almost immoveably. How this happened must be obvious to every one. (See the figure, tab. XIII.)

Before I proceed, it will be necessary to observe, first, that the posterior teeth of cartilaginous fishes are always found in a soft, membranous state, and but imperfectly formed; notwithstanding this, they have the whiteness of teeth from a small quantity of calcareous earth already deposited within their substance. Their hardness and perfect form is acquired as they advance towards the anterior parts of the jaws. Secondly, that of the three angles in each tooth of the shark, one is placed towards the right, another towards the left, and the other, which is in the middle, and the most acute angle, is directed inwardly towards the tongue or *fauces*. They are placed then in such a manner as that the angles of the teeth on the left-side in one row, approach the angles of the teeth on the right-side in the next row. Those teeth which stand on a line from without inwards, I call a row; not those which are placed nearly in a parallel line from one side of the mouth to the other.

The sharp bone of the sting-ray was fixed in the lower jaw between two rows of teeth, and at their posterior part, where the first rudiments of the future teeth are formed, and it will be clear to every one, particularly those who are conversant in such matters, that this could not have happened without producing a great deal of pain, swelling, and disorder in the part where it was fixed. It is unnecessary to enumerate the different kinds of mischief this might occasion. Let it suffice to observe, that on account of the space taken up by this extraneous body, the teeth on each side of it, for want of room, could never after be perfectly formed. The teeth on the left-side

wanting

wanting their angles to the right, and the teeth on the right side being destitute of their angles to the left.

As it is certain, that the anterior teeth were formerly posterior ones, and as the teeth in each row were all deficient in one angle, it follows, that they must have been formed posterior to the insertion of this extraneous body. Again, if we allow that before the accident the animal was in possession of perfect teeth, it follows also, that they were consumed and replaced by imperfect ones.

There were six teeth in each row, and fifty-two rows, making together about 312 teeth. Now allowing the consumption to have been equal in all parts of the jaws, it follows, that the animal had already consumed 312 teeth, and was in possession of a like number for future consumption.

The teeth of sharks, rays, &c. may be divided into active and passive. The active teeth are the anterior ones of each row, standing with their points upwards. The passive teeth are the remaining ones, lying one upon another, like the tiles upon a house (imbricated), with their points downwards. It appears from the foregoing account, that the anterior or active teeth had been replaced six times; and that they might have been renewed six times more, making in all twelve times. From which, I think, we may reasonably conclude, that this does not happen any precise number of times; but that the renovation is perpetual during the life of the animal.

The longevity of fishes is a fact pretty well established. In addition to this part of natural knowledge, I have endeavoured to prove, that a part of the inhabitants of the great deep retain, in the article of teeth, a perpetual juvenility, being apparently utter strangers to edentulous old age.

EXPLANATION OF THE PLATES.

Tab. XI. The jaws of the Wolf-fish.

A. The palate.

BB. The side bones of the upper jaw.

C. The anterior or nasal portion of the same.

D. The lower jaw.

Tab. XII. The teeth of the *Chætodon nigricans* magnified.

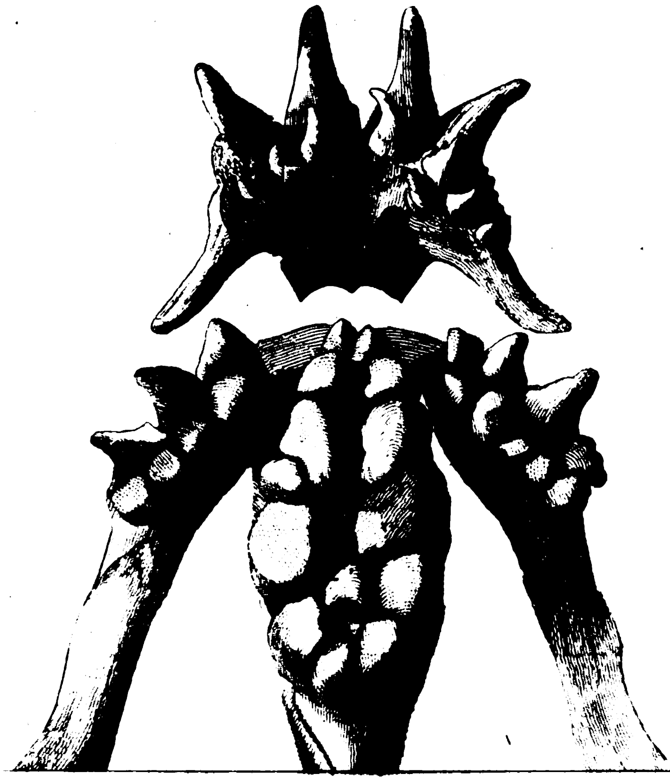
Tab. XIII. Part of the lower jaw of a large shark.

AA. Two rows of perfect teeth.

BB. Two rows of imperfect ones.

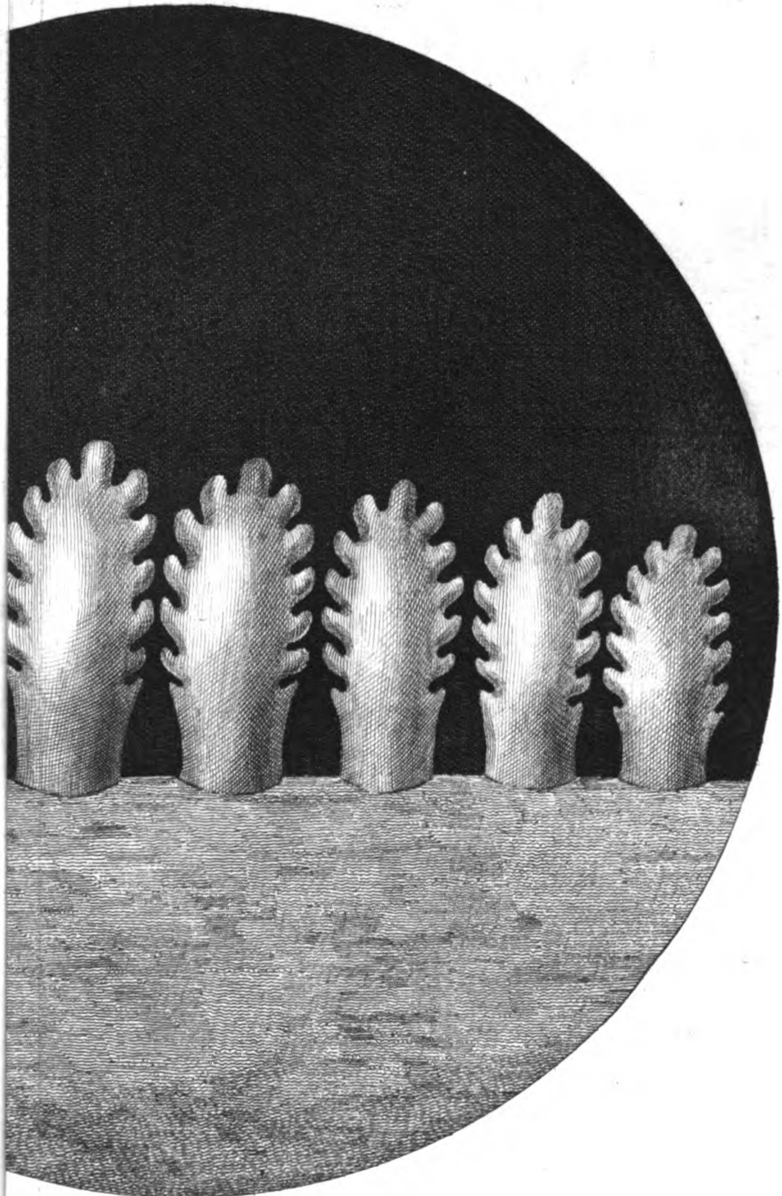
C. The bone of the sting-ray.

C



B

B



TODON nigricans *LINN.*


B
B
A
C

XXI. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1783. *By* Thomas Barker, *Esq.; communicated by* Thomas White, *Esq. F. R. S.*

Read March 18, 1784.

		Barometer.			Thermometer.						Rain.		
					In the House.			Abroad.			Lyndon	Selbourn, Hampsh.	South Lambeth, Surrey.
		Highest	Lowest.	Mean.	High.	Low.	Mean	High.	Low.	Mean			
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.
Jan.	Morn.	29,87	28,38	29,04	47	33½	41	46½	18½	35	1,805	4,43	1,51
	Aftern.				48	35	42	50	27½	40			
Feb.	Morn.	30,12	28,08	29,28	48	36	43	49	17½	36½	2,313	5,54	2,98
	Aftern.				49½	37	43½	53½	30	43			
Mar.	Morn.	30,01	27,88	29,28	47½	33½	40	44½	21½	33	1,604	2,16	,93
	Aftern.				49½	33½	41½	55	33	43			
Apr.	Morn.	30,14	29,15	29,70	58	46	51	52	32½	43	0,558	,88	,59
	Aftern.				62½	47½	53	67½	41½	55½			
May	Morn.	29,82	29,13	29,48	63½	47	53	55½	34½	44	4,218	2,84	2,36
	Aftern.				66	48½	55	72½	44	56			
June	Morn.	29,85	28,80	29,47	65½	55½	60	63½	46	55	3,033	2,82	4,00
	Aftern.				68½	56½	61½	79	55	67			
July	Morn.	29,89	29,16	29,55	72	61½	66	68	55	61½	2,663	1,45	,78
	Aftern.				75	67	68½	85	67	74			
Aug	Morn.	29,83	29,17	29,49	67	57½	62	67½	46	57	1,102	2.24	2,23
	Aftern.				75	58½	64½	84	57	67			
Sept.	Morn.	29,87	28,77	29,36	62½	54	57½	59	40	50½	1,440	5,53	4,30
	Aftern.				64	56	59	68	55	60			
Oct.	Morn.	29,88	28,99	29,48	60½	46½	53	58	34	44½	0,658	1,71	,72
	Aftern.				61½	48	54	64½	44	54			
Nov.	Morn.	29,96	28,42	29,45	51	42	46½	52½	30½	40	1,783	3,01	1,63
	Aftern.				52	42½	47	54½	37½	45½			
Dec.	Morn.	29,99	28,49	29,29	46	28	40½	43½	8½	32½	1,602	1,10	1,22
	Aftern.				46½	28	40	45½	19	37			
										Inches	22,779	33,71	23,25

The

The year began with a ſhort dry froſt, then ſhowery, intermixed with froſt. The end of January, and near half of February, ſtormy and wet, and after ten days fine and mild; a ſevere ſeaſon for ſnow, wet, wind, and froſt. The end of February and beginning of March cut the graſs, corn, and ſtock, more than all the winter before. From March 10. to May 27. was a very dry ſeaſon and fine ſeed-time; but ſo dry at laſt the late ſown corn could not come up. The ſpring was pleaſant; but almoſt conſtant froſty mornings till April, and frequent afterward, kept things backward; and though there was ſome fine warm weather the middle of April, yet later in May the drought and N.E. winds ſtopped the growth of things; and two ſharp froſty nights, May 25. and 26. the rime was ſo particularly cutting in the meadows, that the young ſhoots on many oak and aſh trees in the vallies were entirely killed, while thoſe on the hills were unhurt, and ſome of the tops of the trees eſcaped, though the bottoms were blaſted.

May 27. to 30. in a continued three days rain there fell 3¼ inches, which is, I believe, the moſt that has come in one continued unceaſing rain ſince July 1736, when, in about the ſame time, there came five inches; but the rain this May was not alike in all places, for there was not a quarter ſo much in Hampſhire. This rain was of vaſt ſervice to bring up the late ſown corn, and make the graſs grow well; but this and ſome other haſty rains afterwards hurt the meadow graſs, by flooding it three times. Hot weather ſucceeding, it was a very growing time, and ten days together, in the middle of June, were all wet.

During the ſhowery time an uncommon hazineſs began, which was very remarkable all the reſt of the ſummer: the air was all thick both below the clouds and above them, the hills

hills looked blue, and at a distance could not be seen; the sun shone very red through the haze, and sometimes could not be seen when near setting. There was more or less of this haze almost constantly for a month, and very frequently to the end of the summer, and it did not cease till Michaelmas; and neither rain nor fair, wind nor calm, east nor west winds, took it away; and it was as extensive as common, for it was the same all over Europe, and even to the top of the Alps. This haze was very like VIRGIL's description of the summer after J. CÆSAR's death, which was probably the same case,

Cum caput obscurâ nitidum ferrugine texit,

for rusty iron is a very good description of the colour the sun shone. But by PLUTARCH's account, near the end of C. CÆSAR, that summer was very different from this in other respects for, he says, the sun gave very little heat, the air was cloudy and heavy, and the fruits not ripened, which was not the case this year; for this was a dry haze, the summer in general hot and dry, and in some countries very much so.

I think I never knew more mischief done by thunder than there was in different places this year, from the beginning of July, and very seldom more or hotter weather; yet where they had not those thunder-showers they suffered by being burnt up. Here we never wanted grass after May, and the hay and harvest were both well got in; but in Surrey, Hampshire, and Dorset, they were very much burnt up, had little hay, and as they had a good deal of showery weather in harvest, their barley suffered twice, from not coming up in time, and again in getting it in. As the rain this year was chiefly in showers or sudden rains, it fell very uncertainly, as appears by comparing

comparing what fell here with that in Hampſhire. The latter part of Auguſt and firſt half of September was ſhowery; but in this country not ſo much as to hurt the harveſt, a great part of which was in firſt. The crop of grain was in general pretty good, but did not yield enough to make up the defect of the laſt year's crop, every body was ſo much out of all ſorts, as the corn laſt year was both ſcarce and bad; grain, therefore, continued dear this year, eſpecially barley.

The ſummer 1782 had been ſo cold and wet, that the flower buds on many trees were very ſmall and not perfected, ſo that this ſpring there was a great want of bloſſoms on the wall fruit and apples, and exceeding few indeed on aſh-trees and hawthorn. I do not know of any aſhkeys at all, nor any bunches of haws, only a few ſcattered ſingle ones; but cherries and plumbs bloſſomed well, and there was no want of fruit; plenty of currants, and vaſt quantities of gooſeberries.

Auguſt 18. a remarkable ball of fire was ſeen between nine and ten at night all over England, and even in foreign countries. It ſeemed to move from north to ſouth or ſouth-eaſt. There was another October 4. but not ſo much obſerved, and ſome ſay another afterward, but little ſeen; but there were very few northern lights this autumn.

The autumn was a very fine one; calm, fair, and mild, but rather too dry for the ſowing of wheat, which, however, in general came up well, and what lay dry was brought up very finely by ten days wet the middle of November; after which it was dry and fine again, an open mild time, with few froſty mornings; but a good deal of dark or miſty weather in December, yet mild till the laſt week, when there came a great ſnow, very ſevere froſt, and cutting ſtrong wind, which ended the year.

XXII. *On the Periods of the Changes of Light in the Star* Algol. *In a Letter from* John Goodricke, *Esq. to the Rev.* Anthony Shepherd, *D. D. F. R. S. Professor of Astronomy at* Cambridge.

Read April 1, 1784.

S I R, York, Dec. 8, 1783.

AS I am now able, by collating some of my late observations on Algol with those I sent you last May, to determine with greater precision the periodical return of its changes, I wish to add this as a kind of supplement to that account.

The method I have here pursued is by taking the intervals between accurate observations of Algol's least brightness or greatest diminution of light made at long distances of time from each other, and dividing those intervals by a certain number of revolutions, as will be best understood by the table below. The reason of my chusing long intervals is, that the number of revolutions being greater, the errors of observation are thereby diminished: all error cannot, however, as yet be excluded, but I think the period is now, by the following calculation, ascertained within ten or fifteen seconds.

 Mean

Mean times of Algol's least brightness. 1783 h. ′				d.	h.	′	″
Jan. 14 9 25 Oct. 25 6 39	an interval of	99	revolutions, each of	2	20	49	14
Jan. 14 9 25 Nov. 14 8 17	Ditto	106	Ditto	2	20	49	10
Jan. 14 9 25 Nov. 17 4 52	Ditto	107	Ditto	2	20	49	2
Feb. 6 8 15 Oct. 25 6 39	Ditto	91	Ditto	2	20	49	3
Feb. 6 8 15 Nov. 14 8 17	Ditto	98	Ditto	2	20	48	59
Feb. 6 8 15 Nov. 17 4 52	Ditto	99	Ditto	2	20	48	51
Feb. 26 9 43 Oct. 25 6 39	Ditto	84	Ditto	2	20	49	14
Feb. 26 9 43 Nov. 14 8 17	Ditto	91	Ditto	2	20	49	9
Feb. 26 9 43 Nov. 17 4 52	Ditto	92	Ditto	2	20	49	0
Jan. 31 14 29 Nov. 14 8 17	Ditto	100	Ditto	2	20	49	4
Mar. 21 8 36 Nov. 17 4 52	Ditto	84	Ditto	2	20	48	46
Hence the period of Algol's variation is, on a mean,				2	20	49	3

I could have added several more comparisons of the like kind; but these are, I think, sufficient. It is to be remembered, that all the observations contained in the above table are reduced to mean time.

It appears to me now, that the duration of the variation is about eight hours; but, as it is difficult to hit exactly the beginning and end of the variation, this may occasion different observers to differ in this respect. Before I conclude, I beg leave to mention a circumstance deserving of notice; which is, that

that FLAMSTEAD has alſo amongſt other ſtars obſerved Algol, and in two places has marked it of leſs magnitude than at other times, *viz.* of the third magnitude, 1696, January 16. 6 h. 24′, and 1711, December 5. 9 h. 13′, both mean time and old ſtile *. Suſpecting theſe might probably be days of Algol's variation, I computed the interval between them, but could not find a period anſwerable to that which I have above determined. Upon examining more cloſely the obſervations, I find, in that of 1696, he marked at the ſame time the magnitude of ρ Perſei; which, conſidering eſpecially the nearneſs of ρ Perſei to Algol, makes this obſervation to be relied on for its juſtneſs, and leſs liable to any miſtake of judgement; whereas the other obſervation of December 5, 1711, is more liable to error or doubtfulneſs, becauſe he did not then mark the magnitude of ρ Perſei, or of any ſtar of the ſame magnitude near enough to Algol. Preſuming, therefore, on the juſtneſs of FLAMSTEAD's obſervation of 1696, to think that it probably was made at a time when Algol varied, I compared it with one of mine, *viz.* October 25. 6 h. 39′, 1783, and I find there is, in the interval between thoſe obſervations, either 11,176 periods, each of 2 d. 20 h. 49′ 18″; or 11,177, each of 2 d. 20 h. 48′ 56″. The laſt, as it approaches neareſt to the reſults of my beſt obſervations, I think, is the exacteſt determination of the period. This, however, all proceeds upon the ſuppoſition that Algol varied at the time of FLAMSTEAD's obſervation, and alſo that the period is regular.

* Hiſtoria Cœleſtis, vol. II. edit 1725, p. 284. and 534.

 The

The following is a ſhort abſtract of my late obſervations on Algol, when its leaſt magnitude was accurately determined.

August 17, 1783.

App. time.
h. ′
10 52 About equal to ρ Perſei, though Algol ſeemed to be rather brighter.
11 7 Evidently leſs than ρ Perſei.
11 22 Ditto; but rather difficult to diſtinguiſh them from each other.
11 30 Rather brighter than ρ, and not ſo bright as δ Perſei.
12 0 About the brightneſs of δ Perſei, and rather leſs than β Trianguli.
12 30 Brighter than δ Perſei, and rather not ſo bright as β Trianguli.

From thoſe obſervations, by taking a mean between 11 h. 7′ and 11 h. 22′, it appears, that its leaſt brightneſs happened at 11 h. 14′; true, I think, to 5′.

October 25.

6 40 It was conſiderably leſs than ρ Perſei.
7 5 Ditto.
7 20 Equal to ρ Perſei, though Algol ſeemed rather leſs.
7 35 About equal to ρ Perſei.
7 50 Brighter than ρ, and alſo than δ Perſei.
8 25 About the third magnitude, and equal to β Trianguli.
9 35 Between the ſecond and third magnitude; brighter than β Arietis, and rather leſs than α Pegaſi.
10 10 About the ſecond magnitude; rather brighter than α Pegaſi, rather leſs than β Caſſiopeæ, and not ſo bright as α and γ Caſſiopeæ.

Rather

App. time.
h. ′
10 40 Rather brighter than β Caſſiopeæ, but leſs than α and γ.
11 0 Nearly equal to, if not rather brighter than, γ Caſſiopeæ, and leſs than α Caſſiopeæ*.
In 20′ afterwards it was of the ſame brightneſs; hence we may conclude, that the variation has ended at 11 h. 0′.
Its leaſt brightneſs from the obſervations appears to have happened at 6 h. 55′; true, I think, to 10′.

November 11.

10 5 Third magnitude; not much different from ι Perſei and β Trianguli.
10 45 Between the third and fourth magnitude; believe equal to δ Perſei.
11 14 Leſs than ρ Perſei.
11 48 Ditto; but think it rather increaſed.
Its leaſt brightneſs from thoſe obſervations appears to have happened at 11 h. 31′; true, I believe, to a quarter of an hour. The weather was rather hazy.

November 14.

5 0 Between the ſecond and third magnitude, and leſs than β Caſſiopeæ.
5 45 A little brighter than β Arietis.
6 50 Not ſo bright as β Arietis, and rather brighter than β Trianguli.
8 10 A little brighter than ρ Perſei, and believe equal to δ Perſei.

* Algol's uſual and greateſt brightneſs, by my later and more accurate obſervations, is thus: a little leſs than α Caſſiopeæ, brighter than β Caſſiopeæ and α Pegaſi, and rather a little brighter than γ Caſſiopeæ.

App. time.

h.	′	
8	25	Leſs than ρ Perſei.
8	40	Ditto.
9	0	Equal to ρ, though Algol appeared rather brighter.
9	15	A little brighter than δ and ρ Perſei.

By taking a mean between 8 h. 25′ and 8 h 40′, it appears, its leaſt brightneſs happened at 8 h. 32′; true to 10 minutes. The weather was rather hazy during ſome part of this obſervation.

November 17.

4	58	A little leſs than ρ Perſei.
5	15	Ditto.
5	35	Rather brighter than ρ Perſei.
5	50	A little brighter than ρ Perſei, but leſs than δ Perſei.
6	5	Rather brighter than δ Perſei.
6	40	Equal to β Trianguli, and brighter than ϵ and ζ Perſei.
7	20	A little brighter than β Arietis.
8	30	Between the ſecond and third magnitude, and equal to β Caſſiopeæ, but leſs than α and γ.
8	50	Second magnitude, and equal to γ Caſſiopeæ.
9	25	Nearly the ſame, if not rather brighter.

The variation has therefore ended at 9 h. 0′ nearly, and its leaſt brightneſs by taking a mean between 4 h. 58′ and 5 h. 15′, happened at 5 h. 7′; true, I believe, to 10 minutes. The weather was fine.

I have ſeveral more obſervations on Algol, where I have not been able to aſcertain its leaſt brightneſs, which all happened agreeable to the period as above determined; *viz.* May 20. July 5. and 22. Auguſt 14. September 6. 9. 12. and 26. October 2. 5. 19. and 22. and December 7.

XXIII. *Experiments and Observations on the* Terra Ponderosa, &c. *By* William Withering, *M. D.; communicated by* Richard Kirwan, *Esq. F. R. S.*

Read April 22, 1784.

SECTION I.

Terra ponderosa aërata.

THIS substance was got out of a lead-mine at Alston-Moor, in Cumberland. I first saw it in the valuable collection of my worthy and ingenious friend MATTHEW BOULTON, Esq. at Soho; who, when he picked it up, conjectured from its weight that it contained something metallic. About two years ago I saw it in his possession; and partly from its appearance, being different from that of any calcareous spar I had seen, and partly from its great weight, I suspected it to be the spatum ponderosum.

A few experiments made at the moment confirmed my suspicions, at least so far as to shew that it contained a large proportion of the terra ponderosa united to fixed air; but I did not then flatter myself that it would prove so pure as I afterwards found it to be.

Professor

Professor BERGMAN, in his Sciagraphia Regni Mineralis, published last year at Leipsic, conjectures (§ 58.), with his usual sagacity, "Terra ponderosa *nitrata* forte alicubi nativa "occurrit, a nemine tamen adhuc inventa, quod etiam valet "de *aërata*."

I was much delighted by the detection of a substance which promises to be of very considerable utility in chemical inquiries, and more so when I found it to be a native of this country; for it is not improbable, that it may be met with in many other mines, besides that at Alston-Moor.

Mr. BOULTON, with his usual benevolence, presented me with a piece of it, part of which accompanies this paper, for the inspection of the Members of the Royal Society.

More obvious Properties.

Its general appearance is not much unlike that of a lump of alum; but, upon closer inspection, it seems to be composed of slender spiculæ in close contact, but more or less diverging. It may be cut with a knife. Its specific gravity is from 4,300 to 4,338.

It effervesces with acids, and melts under the blow-pipe, though not very readily. Placed in a covered crucible, in a hot parlour fire, it lost its transparency.

After exposure to a moderate heat in a melting furnace, it adhered to the crucible, and exhibited signs of fusion; but was not diminished in weight, did not feel caustic when applied to the tongue, nor had it lost its property of effervescing with acids.

Hence

Hence it is probable, that its loss of transparency was rather occasioned by numerous small cracks, than by any escape of the water of crystallization, or of its aërial acid.

EXPERIMENTS.

A. 500 grains, dissolved in muriatic acid, in such a manner that nothing but elastic fluid could escape, lost in solution 104 grains, and there remained an insoluble residuum of nearly 3 grains.

2. In another experiment 100 grains lost in solution 21 grains, and there remained 0,6 of a grain of insoluble matter.

B. 100 grains dissolving in dilute muriatic acid, gave out 25 ounce measures of air. This air was received in quicksilver, and when the spar was wholly dissolved, the solution was boiled, in order to drive out what air might be lodged in it.

2. This air was heavier than atmospheric air; it was readily absorbed by agitation in water, it precipitated lime from lime-water, and it extinguished flame. The water which had absorbed it changed the blue colour of litmus slowly * to a red; so that this elastic fluid was undoubtedly fixed air.

C. The solution (B) by the addition of mild fossil fixed alkaly, afforded a precipitate which, after proper washing and drying, weighed 100 grains.

2. This precipitate, upon being again dissolved in marine acid, yielded only 20 ounce measures of fixed air.

* Other acids turn the blue of litmus instantly to a red, whilst water, impregnated with fixed air, does not change the litmus immediately; but, after some seconds, the red colour begins to appear, and then gradually grows more distinct

D. To a ſaturated ſolution in marine acid mild fixed vegetable alkaly was added; the earth was precipitated, and a quantity of fixed air eſcaped.

2. The ſame thing happened when mild foſſil alkaly was added.

3. When cauſtic vegetable alkaly was uſed, the precipitation took place, but without any appearance of efferveſcence.

4. 50 parts diſſolved in marine acid loſt, during the ſolution, nearly 10,5. This ſolution, upon the addition of cauſtic vegetable alkaly, let fall a precipitate which, when waſhed and dried, weighed 45,5.

5. Phlogiſticated alkaly precipitated the whole of the earth from part of the ſolution D; for mild fixed alkaly afterwards added to the filtered liquor occaſioned no further precipitation.

E. Part of the precipitates D. 1. 2. after expoſure to a ſtrong heat in a crucible, was thrown into water. Next morning the water was completely covered with an ice-like cruſt, and had the acrid taſte of lime-water in a very high degree.

2. The ſmalleſt portion of vitriolic acid added to this water occaſioned an immediate and copious precipitation; and when this acrid water was diluted with 200 times its bulk of pure water, the precipitation upon the addition of vitriolic acid was yet ſufficiently obvious.

3. A ſingle drop of this acrid water, added to ſolutions of tartar of vitriol, Glauber's ſalt, vitriolic ammoniac, alum, Epſom ſalt, ſelenite, occaſioned an immediate precipitation in all of them.

F. The precipitate thrown down by the cauſtic vegetable alkaly (D. 3.) was put into water, in expectation that it would make lime-water; but neither upon ſtanding, nor after boiling, did this water exhibit any precipitation when concentrated vitriolic

vitriolic acid was dropped in it; nor had it any acrimonious or other peculiar taſte.

G. Concentrated vitriolic acid was added to one portion of the precipitate D. 3.; concentrated nitrous acid to a ſecond portion; and marine acid to a third portion. No efferveſcence could be obſerved, nor was there any appearance of ſolution. After ſtanding one hour water was added; and the acids, thus diluted, were ſuffered to remain upon the portions of the precipitate for another hour. They were then decanted, and ſaturated with mild foſſil fixed alkaly, but without any appearance of precipitation.

H. The part precipitated by the phlogiſticated alkaly, when mixed with nitre and borax, and fluxed by a blow-pipe upon charcoal, formed a black glaſs; upon flint-glaſs, a white; and upon a tobacco-pipe an opaque yellowiſh white one.

2. Another portion melted with ſoap and borax in a crucible, formed a black glaſs, but without any metallic appearance.

I. The inſoluble reſiduum (A.) was boiled in water, the water decanted, and mild fixed alkaly added, but without any ſubſequent precipitation.

2. This inſoluble powder was not attacked by the nitrous or marine acids; but being put into vitriolic acid, and boiled a conſiderable time until the acid became highly concentrated, it diſſolved entirely, and ſeparated again upon the addition of water. It will appear in the ſequel, that the ſame thing happens to marmor metallicum, when diſſolved by boiling in the acid of vitriol.

CONCLUSIONS.

Hence it appears, that 100 parts of this ſpar contain

Terra ponderoſa pura	-	78,6
Marmor metallicum	-	,6
Fixed air - -	-	20,8
		100

OBSERVATIONS.

1ſt, The quantity of mild fixed alkaly neceſſary to ſaturate an acid, previouſly united to the terra ponderoſa, contains more fixed air than is neceſſary to ſaturate that quantity of terra ponderoſa D. 1. 2.

2dly, The terra ponderoſa, when precipitated from an acid by means of a mild fixed alkaly (D. 1. 2.), readily burns to lime; and this lime-water proves a very nice teſt of the preſence of vitriolic acid. E. 2. 3.

3dly, It is very remarkable, that the terra ponderoſa ſpar, in its native ſtate, will not burn to lime. In the lower degrees of heat it ſuffers no change, as was before obſerved, beſides the loſs of its tranſparency. When urged with a ſtronger fire, it melts and unites to the crucible, but does not become cauſtic.

I buried it in charcoal-duſt in a covered crucible, and then expoſed it to a pretty ſtrong heat; but it did not part with its air.

May we not conjecture, then, that as cauſtic lime cannot unite to fixed air without the intervention of moiſture, and as this ſpar ſeems to contain no water in its compoſition, that it

is

is the want of water which prevents the fixed air assuming its elastic aërial state? This supposition becomes still more probable, if we observe that when the solution of the spar in an acid is precipitated by a mild alkaly, C. 1. 2. some water enters into the composition of the precipitate, for it weighs the same as before it was dissolved, and yet contains only 20 ounce measures of fixed air, whilst the native spar contained 25 ounce measures; so that there is an addition of weight equal to that of 5 ounce measures of fixable air, or 3¾ grains to be accounted for, which can only arise from the water; and this precipitate, thus united to water, readily loses its aërial acid in the fire, E. 1.

4thly, Professor BERGMAN supposes the terra ponderosa to be a metallic earth *; its entire separation from an acid by means of the phlogisticated alkaly (D. 5.) certainly favours such a supposition; but, if it be so, it is evident from experiments H. 1. 2. that other means than those commonly employed must be used to effect its reduction.

5thly, The precipitate made by the caustic vegetable alkaly D. 4. taking some of the alkaly down with it, and thus forming a substance neither soluble in water nor in acids, is a very curious phænomenon.

I afterwards varied the experiment by adding the terra ponderosa lime-water (E.) to caustic vegetable and caustic fossil alkaly. In both cases this insoluble compound was immediately formed; but not so when caustic volatile alkaly was used. This composition of an alkaly and an earth certainly deserves more attention than I am at present able to bestow upon it.

6thly, As it appears from experiments D. 1. 2. 3. 4. that

* See preface to his Sciagraphia Regni Mineralis.

fixed alkalies, both mild and caustic, separated the terra ponderosa from marine acid, I was at a loss to know why Professor BERGMAN, in his admirable table of simple elective attractions, placed the terra ponderosa caustica immediately under the vitriolic, nitrous, and marine acids, and consequently above the caustic alkalies. I was interested in the reality of the facts, because I had so seldom seen reason to doubt the observations of that very excellent chemist, and therefore made the following experiments.

To different portions of terra ponderosa salita and terra ponderosa nitrata I added, drop by drop, caustic vegetable, caustic fossil, and caustic volatile alkalies. In every case the EARTH was thrown down; and I have so often repeated these experiments to satisfy myself and others, that I am persuaded the terra ponderosa caustica ought to be placed below the alkalies, except in the column appropriated to the vitriolic acid; and it may be separated even from that acid, by the vegetable fixed alkaly, if the alkaly be applied *via sicca*, as will appear in the next section.

7thly, The necessity for using pure acids upon many occasions, and the difficulty of obtaining them pure, are sufficiently obvious. The VITRIOLIC ACID, as made in the large way, is generally pure enough for most purposes. It is apt to get coloured by inflammable matter; but this is seldom an inconvenience; and, when it would be so, it is easy to drive it off by boiling the acid in a Florence flask over a common fire. But there is another cause of impurity in this acid, which appears upon diluting it with water; for then it becomes milky, and in a short time a powder subsides.*

The

* About two years ago I examined this powdery matter; both that which was thrown

The acid may be freed from this powder either by diftillation in glafs veffels, which is a tedious and dangerous procefs, or by the affufion of water; and, after the powder has fubfided, a gentle evaporation will drive off moft of the fuperfluous water.

NITROUS ACID may be freed from vitriolic and marine acids, by folution of filver in the acid of nitre, as is daily practifed; but the MARINE ACID has not, to my knowledge, been purified by any other method than the laborious one of re-diftilling it from common falt. It is generally mixed with vitriolic acid, and often in large proportion. There is no temptation, and fcarcely an opportunity, for it to be contaminated by nitrous acid. From the vitriolic acid then it may be readily purified by the addition of terra ponderofa cauftica diffolved in water, or by the terra ponderofa falita. If the latter be ufed, a fmall

thrown down by dilution with water, and alfo fome which Dr. PRIESTLEY gave me, being the refiduum of vitriolic acid diftilled to drynefs in a flint-glafs retort.

1ft, Repeated boiling in water, reduced 6½ grains to 2 grains.

2dly, This folution, by gentle evaporation, afforded 5 grains of cryftals, as hard and as taftelefs as felenite.

3dly, To thefe cryftals, re-diffolved in water, mild foffil alkaly was added, and a white powder precipitated.

4thly, This powder, after expofure to a pretty fharp heat, was thrown into water; part of it diffolved, and the water got the tafte and other properties of lime-water.

5thly, The infoluble part (1.) fuffered no change by boiling in nitrous acid; one-half of it mixed with borax, and expofed to the blow-pipe upon charcoal, vitrified; the other half, mixed with borax and charcoal-duft, likewife vitrified.

CONCLUSIONS. It appears, then, that the greater part of this fubftance was calx vitriolata, or felenite; the remainder a vitrifiable earth.

I had before found, that the terra ponderofa vitriolata, or heavy gypfum, would diffolve in concentrated vitriolic acid; but always feparated in a powdery form upon the affufion of water; and now it appears, that calx vitriolata, or felenite, does the fame.

portion

portion of the acid muſt firſt be tried in a diluted ſtate, from whence we muſt judge how much of the terra ponderoſa ſalita the whole will require; or elſe the whole of the acid muſt be diluted with water. Whether we uſe the terra ponderoſa diſſolved in water or in marine acid; in either caſe the acid of vitriol immediately ſeizes upon it, and ſubſides with it in form of an inſoluble powder.

As there are reaſons for preferring the marine acid in ſeveral of the nicer and more important enquiries of chemiſtry, this ready method of purifying it cannot but prove acceptable.

SECTION II.

Terra ponderoſa vitriolata. BERGMAN's Sciagraphia, §§ 58. 89.

Variety, Heavy Gypſum. Ponderous Spar.

Marmor Metallicum. CRONSTEDT Min. § 18. 2. 19. C.

From Kilpatrick-hills near Glaſgow. A ſort, with ſmaller cryſtals, amongſt the iron ore about Ketley in Shropſhire. In the lead mines at Alſton-Moor.

More obvious Properties.

White; nearly tranſparent, but has not the property of double refraction; compoſed of laminæ of rhomboidal cryſtals; decrepitates in the fire. Specific gravity from 4,402 to 4,440.

EXPE-

EXPERIMENTS.

A. 100 grains exposed to a red heat for one hour, in a black lead crucible, lost five grains in weight; but as a sulphureous smell was perceptible, I suspected that a decomposition had taken place, and therefore exposed another portion to a similar heat for the same space of time in a tobacco-pipe. This had no smell of sulphur, nor was it diminished in weight.

2. It is barely fusible under the blow-pipe; but with borax fluxes readily into a white opake glass.

B. 100 grains, ground in a mortar, and washed over extremely fine by repeated additions of water, were boiled in the same water, and, after settling, the water was poured off. The powder, when dried, had not sensibly lost weight.

2. To separate portions of the washing water, were added mild vegetable and mild fossil alkaly; but without any appearance of precipitation. Nitre of mercury gave a very slight brownish cloud, barely discernible; and nitre of silver an extremely slight bluish appearance.

3. The same powder, boiled again in fresh water, did not affect the water at all; for it stood the test of nitre of silver without any change.

C. Portions of the powder B. were boiled in vitriolic, nitrous, and muriatic acids, of the usual strength, for several minutes. The acids were then saturated with vegetable fixed alkaly, but without any appearance of precipitation, nor had the portions of powder lost any weight.

2. But when boiled in vitriolic acid, until that acid became very much concentrated and nearly red-hot, the whole of it dissolved; but, separated again upon the addition of water, was

not altered in its weight, was not acted upon by acids of the ufual ftrength, and had, under the blow-pipe, the properties mentioned at A. 2.

3. Some of the folution in the concentrated vitriolic acid was left expofed to the atmofphere, that the acid might flowly attract water. After fome days, beautiful cryftals appeared in the fhapes of ftars, fafciæ, and other radiated forms.

4. To another portion of this folution mild fixed vegetable alkaly was added; but the precipitate appeared to be the marmor metallicum unchanged.

D. One ounce of this marmor metallicum in fine powder was fluxed in a crucible with two ounces of falt of tartar, until it ran thin. This fubftance, boiled with water in a Florence flafk, left a refiduum of fix drams.

E. This refiduum was thrown into water, and pure nitrous acid added, until there was no more effervefcence. The undiffolved part weighed 52 grains.

F. This undiffolved part appeared to be the original fubftance no ways changed; for it did not diffolve in nitrous or marine acids, but did wholly diffolve in the greatly concentrated and boiling vitriolic acid, from which it was again feparated by the addition of water. (C. 2.)

G. The folution D. was faturated with diftilled vinegar, and then evaporated to drynefs, but with lefs than a boiling heat. The fal diureticus, thus formed, was wafhed away with alcohol. The remaining falt weighed 5 drams nearly.

2. This falt had the appearance and the tafte of vitriolated tartar; it decrepitated in the fire; roafted with charcoal-duft, it formed a hepar fulphuris; and with muria calcarea gave a precipitation of felenite.

5 H. The

H. The salt, formed with the nitrous acid (E), shot readily into beautiful permanent crystals, of a rough bitterish taste.

2. Some of this salt was deflagrated with nitre and charcoal, and the alkaly afterwards washed away.

3. The residuum, being the earth of the marmor metallicum, was very white, burnt to lime, and again formed an insoluble compound with acid of vitriol.

I. 100 grains of terra ponderosa aërata were dissolved in diluted marine acid. Vitriolic acid was dropped into this solution, until no more precipitation ensued. The precipitate, after very careful washing and drying, was exposed to a red heat in a covered tobacco-pipe for half an hour: when cool, it weighed 117 grains.

2. 50 grains of terra ponderosa aërata in a lump were put into diluted vitriolic acid; but the action of the acid upon it was hardly sensible, even when made hot.

Marine acid was then added, and after some time an effervescence appeared. The terra ponderosa vitriolata, thus formed, after proper washing and drying, was exposed to a red heat for an hour: it then weighed 58,4 grains.

CONCLUSIONS.

1st, It appears that the marmor metallicum is composed of vitriolic acid and terra ponderosa, D. E. F. G. H.

2dly, That this compound, though probably soluble in water, has so little solubility as almost to escape detection by the nicest chemical tests, B. 1. 2. 3.

3dly, That it is not soluble in acids of the usual strength; but that it perfectly and entirely dissolves in highly concentrated

trated vitriolic acid, from which it again separates entire and unchanged upon the affusion of water, C. 1. 2.

4thly, That it cannot be decomposed (*via humida*) by mild fixed alkaly, C. 4.

5thly, That it may be decomposed (*via sicca*) by the vegetable fixed alkaly, D. E. G. H.

6thly, That it may be decomposed by inflammable matter, uniting to its acid, and forming sulphur; but that it cannot be decomposed by heat alone, A. 1.

7thly, From experiments I. 1. 2. it appears, that 100 parts of this substance contain

Vitriolic acid pure -	32,8
Terra ponderosa pure	67,2
	100

For the 100 parts of terra ponderosa aërata made use of in the experiment I. 1. would lose during the solution 20,8 of fixed air (§ 1st, A.); then, deducting 0,6 for the marmor metallicum contained in the terra ponderosa aërata (§ 1st. A. 1. 2.), there remains 78,6 of pure terra ponderosa. This, when saturated with vitriolic acid, and made perfectly dry, weighed 117; consequently it had taken 38,4 of vitriolic acid.

OBSERVATIONS.

The apparent insolubility of terra ponderosa aërata in the diluted vitriolic acid (I. 2.) can be accounted for by remarking, that the moment the surface of the lump was acted upon by the acid, an insoluble coat of marmor metallicum was formed upon it, which effectually precluded any further operation of the acid.

Professor

Profeſſor BERGMAN, in order to obtain the earth from the terra ponderoſa vitriolata, directs the latter to be roaſted with fixed alkaly, and the duſt of charcoal; but I have always done it by charcoal duſt alone, though probably this method may require a greater degree of heat.

It has been remarked, that terra ponderoſa and calx of lead reſemble each other in many reſpects; and I muſt add, that the vitriol of lead diſſolves in the highly concentrated vitriolic acid much in the ſame manner that the marmor metallicum does, and like this too ſeparates upon the affuſion of water; but I never obſerved it diſpoſed to cryſtallize.

The marmor metallicum may probably be uſeful in ſome caſes where a powerful flux is wanted; for I once mixed ſome of it with the black flux, and expoſing it to a pretty ſharp heat, it entirely ran through the crucible. May not, therefore, ſome of the more common varieties of it be uſed advantageouſly as a flux to ſome of the more refractory metallic ores?

SECTION III.

Terra ponderoſa Vitriolata.
Variety, Calk or Cauk.
Marmor Metallicum, CRONSTEDT Min. § 18. B?

Plentiful in the Mines in Derbyſhire.

More obvious Properties.

Of a white or reddiſh colour; cryſtallized in rhomboidal laminæ, but theſe very much intermixed and confuſed. Loſes little or nothing of its weight by being made red-hot. Specific gravity 4,330.

EXPERIMENTS.

A. Ground in a mortar, and washed over, the washing water, when decanted, gave no precipitation with mild vegetable alkaly; but with nitre of silver and nitre of mercury the slightest cloud imaginable.

B. 100 grains boiled in marine acid weighed, after proper washing and drying, 99,5.

C. The acid solution B let fall a Prussian blue upon the addition of a single drop of phlogisticated fixed alkaly; and, when saturated with mild fossil alkaly, afforded an ochry-coloured precipitate.

D. This precipitate, collected and washed, weighed half a grain. It was roasted with tallow, and then was wholly attracted by a magnet.

E. A quantity of the cauk, finely powdered, was mixed with charcoal-dust, and roasted in a crucible at a white heat, for five hours, fresh charcoal-dust being occasionally added. It gave out a strong smell of sulphur.

F. To this roasted cauk nitrous acid was added, which dissolved the greater part of it; producing, during the solution, some effervescence, and a strong smell of hepar sulphuris.

G. Some of this solution, after proper evaporation, afforded beautiful crystals, not deliquescent, exactly resembling those obtained from the marmor metallicum, (§ II. H.).

H. To other portions of the solution F, were added fixed vegetable and fossil alkalies, and likewise volatile alkaly, each of which precipitated the earth from the acid.

I. This

I. This earth, after exposure to a white heat for one hour, became caustic, and made lime-water, similar in properties to to that mentioned at § Ist. E.

K. Some of the part not acted upon by the nitrous acid F, dissolved entirely by boiling in highly concentrated vitriolic acid, and wholly separated again by the affusion of water. More water was added, and the whole was boiled again; but the filtered liquor gave no signs of precipitation upon the most liberal addition of mild fixed vegetable alkaly.

CONCLUSIONS.

It appears, therefore, that 100 parts of Derbyshire cauk contain

Marmor metallicum	-	99,5
Calciform iron	- -	,5
		100

And it is probable, that the redder pieces contain a little more iron.

SECTION IV.

Terra ponderosa vitriolata.

Variety, radiated Cauk.

Gypsum crystallisatum capillare. CRONSTEDT Min. § 19. B.

From Pennely by the Bog, near Minsterley, in Shropshire, fifteen miles from Salop, on the road to Montgomery.

More

More obvious Properties.

Somewhat gloffy like fatin; yellowifh-white, opake; compofed of flender fpiculæ fet clofe together, and pointing from a center.

In fome pieces there are concentric circles of a femi-tranfparent horn like appearance. It is not very brittle; may be fhaved with a knife; lofes little or nothing of its weight by being made red-hot. Its fpecific gravity 4,000; but after foaking one night in the water 4,200, or more.

EXPERIMENTS.

When treated in the fame manner that the Derbyfhire cauk was, in the preceding fection, 100 parts of it appeared to contain

Marmor metallicum -	97,7
Calciform iron - -	2,3
	100

Sufpecting that the prefence of fo fmall a proportion of iron could hardly occafion the whole of the apparent differences betwixt the Shropfhire and Derbyfhire cauks and the marmor metallicum; and thinking it not improbable, that they might contain lead; I mixed fome of them with charcoal-duft and borax, but could not by means of the blow-pipe produce any metallic appearance, although vitriol of lead, treated in the fame manner, was readily reduced.

I then mixed four parts of cauk with one part of vitriol of lead; the lead could ftill be reduced, though not fo readily as before.

GENERAL

GENERAL OBSERVATIONS.

The terra ponderofa feems to claim a place betwixt the earths and the metallic calces. Like the former, it cannot be made to affume a metallic form; but, like the latter, it may be precipitated from an acid, by means of phlogifticated alkaly. In many of its properties it much refembles the calx of lead; and in others, the common calcareous earth, but ftill feems fufficiently different from that to conftitute a new genus, as will appear from a little attention to the following circumftances.

Terra ponderofa,	Terra calcarea,
When diffolved in water, precipitates upon the addition of the fmalleft portion of vitriolic acid.	Diffolved in water, does not precipitate upon the addition of vitriolic acid.
Its gypfum, therefore, is infoluble.	Its gypfum, therefore, is foluble.
With the nitrous and marine acid, forms cryftals which do not deliquefce.	With nitrous and marine acids forms falts fo deliquefcent that they cannot be kept in a cryftallized form.
Decompofes vitriolic falts *via humida*.	Does not decompofe vitriolic falts.

It has been called terra ponderofa, or heavy earth, upon account of the great fpecific gravity of its gypfum; its fpar is likewife heavy enough to countenance fuch an appellation; but the earth itfelf does not appear to be a heavy fubftance, and I imagine the great weight of its compounds with the vitriolic and aërial acids is owing to the abfence of water.

Birmingham, Nov. 1783.

XXIV. *Observations du Passage de* Mercure *sur le Disque du Soleil le 12 Novembre, 1782, faites à l'Observatoire Royal de* Paris, *avec des réflexions sur un effet qui se fait sentir dans ces mêmes Observations semblable à celui d'une Réfraction dans l'Atmosphere de* Mercure. *Par* Johann Wilhelm Wallot, *Membre de l'Académie Électorale de Sciences et Belles Lettres de* Manheim, *&c. Communicated by* Joseph Planta, *Esq. Sec. R. S.*

Read April 29, 1784.

1. LES passages de Mercure sur le disque du soleil sont d'autant plus intéressans pour les astronomes, qu'ils donnent principalement le moyen de déterminer avec plus d'exactitude la position des nœuds de son orbite, et que la difficulté de voir cette planète dans ses autres aspects avec le soleil en rend les observations plus précieuses.

2. Deux circonstances assez désavantageuses qui devaient accompagner particulierement le passage dont il s'agit ici, savoir la proximité du soleil de l'horizon, et Mercure passant trop près du bord de cet astre, semblaient par leur nature offrir trop d'inconvéniens pour en espérer des observations bien exactes; cependant l'encouragement qu'a donné le beau tems qu'il fit toute la journée du 12 Novembre, nous ayant fait apporter une plus grande attention aux observations, nous autorise maintenant à en avoir une meilleure opinion. Je crois pouvoir assurer sans ostentation d'y avoir réussi assez pour être satisfait des miennes, et pour oser les garantir autant que la nature des

 choses

choses peut le permettre. Si je puis me flatter d'avoir obtenu de ce passage une observation très exacte et peut-être la plus complette, je ne dissimulerai pas que je dois en grande partie cet avantage à M. DE CASSINI qui, m'ayant laissé la meilleure lunette * qu'il y ait à l'Observatoire Royal, m'avait mis par là dans le cas d'employer la plus grande vigilance pour mériter par l'exactitude de mes opérations la confiance qu'on me témoignait dans une occasion aussi importante.

3. Nous avons fait (M. DE CASSINI et moi) toutes les observations nécessaires pour constater avec la plus grande exactitude l'état de notre pendule; et, en réduisant mes observations au tems vrai, je n'ai pas même négligé les dixièmes de seconde. Cette précision scrupuleuse paraitra peut-être superflue dans de pareilles observations, mais on verra par la suite de ce Mémoire les raisons qui m'y ont déterminé. Voici mes observations dans le même ordre où elles se sont faites, et réduites au tems vrai de la méridienne de l'Observatoire Royal de Paris.

	Tems vrai. h. ′ ″	
Entrée	à 2 56 28,8	Je soupçonne la planète. Contact extérieur de l'entrée.
	à 2 58 28,8	J'estime Mercure entré à moitié. Centre de ☿ sur le bord du ☉.
	à 3 2 3,8	Contact intérieur de l'entrée.
	à 3 3 45,8	Mercure absolument détaché du soleil.

En mésurant le diamètre de Mercure sur le disque du soleil je l'ai trouvé par deux fois exactement de la même quantité, savoir de 9 parties du micromètre objectif, qui valent 9″,535 de degrés du grand cercle.

Sortie	à 4 17 18,4	Contact intérieur de la sortie.
	à 4 20 36,4	Le centre de Mercure sur le bord du soleil.
	à 4 22 53,4	Contact extérieur de la sortie. Mercure totalement perdu de vue.

* Une excellente lunette achromatique de DOLLOND de 3 pieds.

Le bord du soleil était si ondoyant que Mercure, aux approches de sa sortie totale, ressemblait exactement à un corps flottant sur les vagues d'une eau fortement agitée, et qui tantôt disparait entièrement, tantôt élevé par les vagues se montre en partie et quelquefois tout entier. Ces vagues ou ondulations allaient toujours dans le même sens du N. Ouest au Sud Est. Leur mouvement était assez rapide, et c'est précisément la rapidité de ce mouvement qui m'a singulièrement favorisé l'observation du contact extérieur de la sortie de Mercure, parceque je ne la perdais jamais de vue qu'un instant.

Je terminerai le détail de mes observations par assurer, que je n'ai pas apperçu la moindre apparence d'une atmosphère ou nébulosité autour de Mercure pendant toute la durée de son passage, quoique la lunette me représentât tous les objets très distinctement. J'ai toujours vu le disque de Mercure bien noir, et également bien terminé dans toute sa circonférence qui me paraissait toujours tranchée nette, surtout dans le commencement où les ondulations étaient moins fortes jusques vers le milieu du passage. Mais cela ne m'empêchera pas d'être très persuadé de l'existence d'une atmosphère autour de Mercure, comme autour de tous les corps célestes, et qu'on peut fort bien l'avoir apperçue dans ce passage sous un ciel plus pur et plus beau que celui de Paris.

Résultats du calcul des observations précédentes selon leurs différentes combinaisons.

4. La methode que j'ai suivie pour réduire les observations de ce passage au centre de la terre, m'est en quelque sorte particulière; mais comme elle n'est pas entièrement nouvelle puisqu'elle

qu'elle ne différe de toutes les methodes connues qu'en ce que je l'ai ſimplifiée en la rendant abſolument directe, je me contenterai d'en donner une idée générale. Je n'ai employé dans mes calculs que ce qui eſt donné directement par obſervation, ou bien des quantités plus exactement données par les tables, telles que le diamètre du ſoleil, ſon mouvement horaire et celui de Mercure. Mais ce qui caractériſe eſſentiellement cette méthode, c'eſt qu'en combinant les obſervations toujours enſemble deux à deux, on a la durée ou le tems écoulé d'une obſervation à l'autre qui eſt une des principales données du probléme, et la plus exacte qu'on puiſſe ſe procurer par obſervation. Or, quand l'obſervation nous fournit directement des données exactes, je ne vois abſolument pas la néceſſité d'en aller chercher de moins exactes pour les faire entrer dans le calcul. C'eſt pourtant ce que font quelques aſtronomes modernes *, qui, en recommendant dans leur Traité d'Aſtronomie de calculer les obſervations ſéparément afin, diſent-ils, de multiplier les réſultats et d'en déduire plus exactement par un milieu la quantité qu'on cherche, ſont obligés pour cet effet de ſuppoſer à peu près connu le milieu du paſſage et la plus courte diſtance des centres †. Ce raiſonnement, auſſi éloigné des principes de la géometrie, que des regles de l'analyſe, me parait encore illuſoire quant à l'exactitude qu'on eſpère obtenir de la multiplicité des réſultats ainſi déterminés ; voici pourquoi.

5. Je ſuppoſe pour un inſtant qu'on prenne au hazard deux obſervations, et qu'on les calcule ſeparément chacun ſuivant ce précepte ; il eſt certain que ſi l'on ne ſuppoſe pas le milieu du paſſage et la plus courte diſtance des centres tels que les donneraient

* Principalement M. De la Lande dans ſon Traité d'Aſtronomie, edition de 1771, livre XI. art. 2152.

† Ibid. art. 2062 et 2063.

neraient directement ces deux observations combinées ensemble, on doit trouver, pour la quantité qu'on cherche, deux résultats différens, et qui différeront d'autant plus que la supposition qu'on aura faite sera plus éloignée de la véritable. On prend donc alors un milieu entre les deux résultats et l'on s'imagine avoir trouvé la vérite; mais il me semble qu'il est très permis d'en douter, car, outre qu'il y a bien des cas où l'on ne peut pas regarder le résultat moyen comme le véritable, ici ce n'est pas même admissible, puisque le milieu du passage et la plus courte distance des centres sont deux quantités qui dependent l'une de l'autre, et qu'il est impossible de les supposer telles précisément qu'elles se conviennent relativement à deux observations déterminées, à moins que ce ne soit un effet du hazard. Or si je suppose maintenant qu'on prenne les deux mêmes observations, et qu'on les combine ensemble, il est clair qu'on ne trouvera qu'un seul résultat pour la quantité cherchée, mais ce sera précisément la même qu'on aurait eue par un milieu entre les deux résultats trouvés suivant l'autre manière si l'on y avait fait une supposition qui s'écartât peu de celle qu'il convenait de faire. Il s'en suit donc qu'on serait arrivé au même but par les deux methodes, mais avec cette différence que les quantités déterminées d'après la méthode des combinaisons sont dans tous les cas de vrais résultats tels que les donne véritablement l'observation, tandisque d'après l'autre ce ne sont que des résultats fictifs ou approchés. Le calcul devient à la vérité plus long, lorsqu'il y a plus de trois observations; parceque alors le nombre des combinaisons qu'on en peut faire deux à deux, conséquemment aussi le nombre des résultats qui en proviennent, surpassera toujours celui des observations. Or si pour déterminer une quantité quelconque d'après une méthode on risque de trouver des résultats inexacts, et que d'après une autre méthode

thode on peut déterminer la même quantité sans courir ce danger, il est incontestable que celle-ci est préférable à l'autre. Lorsqu'on ne peut avoir que des observations isolées, il faut bien alors se résoudre à les calculer séparément, mais encore avec la restriction que les quantités qu'on supposera connues soient données par d'autres observations, qui étant dans le cas d'être combinées deux à deux, soient elles-mêmes très exactes. Il est donc aisé de conclure de tout ce que je viens de dire que la manière de calculer séparément chaque observation, non seulement ne procure pas les avantages qu'on en attend pour la multiplicité des résultats, mais elle est encore moins exacte que celle de combiner deux à deux les observations, ainsi que l'enseignent les plus célèbres astronomes. Je ne me suis permis d'entrer dans ces détails que pour prouver à la Société Royale que je ne me sers jamais avec confiance d'aucune méthode sans l'avoir examinée auparavant en la créant, pour ainsi dire, une seconde fois.

6. J'ai calculé le lieu du soleil et de Mercure par les tables de HALLEY pour 2½ h. 3½ h. et 4½ h. espace de tems qui comprend à peu près par son milieu toute la durée du passage, et j'ai trouvé,

	à 2h30' tems vrai				à 3 h. 30' t. v.				à 4 h. 30' t. v.			
	S.	°	'	"	S.	h.	'	"	S.	°	'	"
La longitude du soleil de	7	20	22	43,6	7	20	25	14,8	7	20	27	45,9
Son ascension droite	7	17	55	55,3	7	17	58	28,4	7	18	1	1,5
Sa déclinaison - - australe		17	51	49,6		17	52	29,9		17	53	10,1
La longit. géocentrique de Mercure	7	20	32	2,9	7	20	28	40,8	7	20	25	18,4
Sa latitude - - - boréale		0	14	31,0		0	15	22,6		0	16	13,8

Ce qui me donne	entre 2½ h. et 3½ h	entre 3½ h. et 4½ h
	° ' "	° ' "
Le mouvem. horaire relatif Merc. sur l'écliptique de	5 33, 3	5 53, 5
L'inclinaison de l'orbite relative sur l'écliptique de	8 18 33, 8	8 14 28, 5
Le mouvem. horaire relatif de Merc. dans son orbite	5 57,05	5 57,19

Je

Je me suis servi de l'inclinaison et du mouvement horaire qui avait lieu entre 2½ h. et 3½ h. dans le calcul des observations du commencement, et l'inclinaison avec le mouvement horaire qui avait lieu entre 3½ h. et 4½ h. m'a servi pour la fin du passage. Quant aux autres élémens, j'ai employé le diamètre du soleil de 32′ 24″,5; celui de Mercure de 9″,535 comme je l'ai mésuré sur le disque du soleil pendant le passage, et la parallaxe horizontale du soleil dans ses moyennes distances de 8″,7 telle que je l'ai établie dans mon Mémoire sur le passage de Venus en 1769. D'où j'ai conclu la différence des parallaxes horizontales du soleil et de Mercure pour le jour du passage 12 Novembre de 4″,088.

7. Avec ces éléments j'ai calculé les observations des contacts en ne négligeant pas même les millièmes de seconde dans certains cas; je n'ai mis cette scrupuleuse exactitude dans tous mes calculs que parceque je voulais m'assurer dans le cas où je viendrais à trouver des différences entre les résultats de même dénomination que je n'eusse à les attribuer uniquement qu'aux observations. La table suivante renferme les résultats les plus importans de ces calculs.

Table

Table des résultats du calcul des observations des contacts et du centre de Mercure.

	Contacts intérieurs	Contacts extér.	Centre de ☿.
	h. ′ ″	h. ′ ″	h. ′ ″
Heure vraie de l'observation { entrée	3 2 3, 8	2 56 28, 8	2 58 28,8
Heure vraie de l'observation { sortie	4 17 18, 4	4 22 53, 4	4 20 36,4
Durée donnée directement par l'obs.	1 15 14, 6	1 26 24, 6	1 22 7,6
Plus courte distance des centres vue à la surface de la terre	15 41, 2	15 42, 5	15 41,0
Heure vraie du milieu du passage pour le centre de la terre	3 39 47, 4	3 39 47, 1	3 39 38,7
Plus courte distance des centres vue du centre de la terre	15 45, 1	15 46, 4	15 44,9
Reduction de l'observation au centre de la terre { entrée	+ 2 59,45	+ 2 34,38	+ 2 42,9
Reduction de l'observation au centre de la terre { sortie	— 2 46,70	— 2 22,27	— 2 30,7
Heure vr. de l'observation arrivée pour le centre ☿ { entrée	3 5 3,25	2 59 3,11	3 1 11,7
Heure vr. de l'observation arrivée pour le centre ☿ { sortie	4 14 31,70	4 20 31,13	4 18 5,7
Durée du passage pour le centre de la terre	1 9 28,45	1 21 28,02	1 16 54,0
Heure vraie de la conjonction de Mercure et du soleil	4 2 53, 2	4 2 54, 8	4 2 44,1
	S. ° ′ ″	S. ° ′ ″	S. ° ′ ″
Latitude de ☿ en conjonction donnée par observation	15 55,1	15 56,4	15 54,8
Longitude du soleil ou de Mercure en conjonction	7 20 26 37,6	7 20 26 37,7	7 20 26 37,2
Longitude de Mercure en conjonction donnée par les tables eu égard à l'aberration	7 20 27 8,4	7 20 27 8,3	7 20 27 8,9
Latitude de ☿ en ☌ donnée par les tables . . . boréale	15 50,7	15 50,7	15 50,5
Erreur des tables { en longitude	— 30,8	— 30,6	— 31,7
Erreur des tables { en latitude	+ 4,4	+ 5,7	+ 4,4

En adoptant la latitude de ☿ au moment de la ☌ donnée par les contacts intérieurs de 15′ 55″,1 je trouve le lieu du ☊ de ☿ à

	S. ° ′ ″		° ′ ″	
	1 15 45 22,8	en supposant l'inclinaison de l'orbite	7 0 0	avec M. Cassini.
	1 15 44 55,7	en supposant l'inclinaison de l'orbite	6 59 20	avec Halley.

8. L'on voit par cette table que les contacts intérieurs donnent l'heure du milieu du passage à 3 dixièmes de seconde près la même que les contacts extérieurs; l'heure de la conjonction à 1″,6 près la même*, et la plus courte distance des centres ainsi que la latitude de Mercure en conjonction de 1″,3 plus petite. Quant aux deux observations du centre de Mercure sur les bords du soleil, elles donnent le milieu du passage de 8″,7 plutôt que les contacts intérieurs, et la plus courte distance des centres ainsi que la latitude de Mercure en conjonction de 2 dixièmes de seconde seulement plus petite. Cette différence dans l'heure du milieu du passage ne peut venir que de la manière dont j'ai estimé le centre de Mercure; car il y a d'abord seconde pour seconde le même intervalle de tems entre les deux contacts de l'entrée qu'entre ceux de la sortie, c'est à dire l'un et l'autre de 5′ 35″. Ensuite je trouve qu'il s'était écoulé 2′ 0″ depuis le contact extérieur de l'entrée jusqu'au moment où j'ai estimé le centre de Mercure sur le bord du soleil, au lieu de 2′ 17″ qu'il y a entre les pareilles observations de la sortie; mais cet intervalle de tems devant être le même pour l'entrée et pour la sortie, la différence 17″ fait voir que j'ai estimé le centre de Mercure plus près du contact extérieur à l'entrée qu'à la sortie, ce qui devait aussi avancer l'instant du milieu du passage; or la moitié de ces 17″ fait précisément les 8″½ dont le milieu du passage est arrivé plutôt selon cette observation que selon celle des contacts intérieurs (puisque l'erreur de l'une des deux observations n'est que la moitié sur le milieu du passage). J'ai donc marqué l'instant de l'observation du centre à l'entrée plutôt qu'il

* L'instant de la conjonction diffère de 1″,6 quoique celui du milieu du passage ne diffère que de 0″,3, parceque la portion de l'orbite relative comprise entre le milieu du passage et la conjonction est plus grande pour une plus grande distance des centres.

ne

ne fallait; car je pencherai toujours à croire plutôt que c'eſt ſur celle de l'entrée que doit tomber l'erreur, parceque n'ayant pas encore vu Mercure ſur le diſque du ſoleil, je ne pouvais pas juger de ſa grandeur auſſi bien qu'à la fin après l'avoir vu pendant toute la durée de ſon paſſage. C'eſt auſſi en partie par cette même raiſon, jointe à celle qu'on ne peut pas eſtimer avec quelque préciſion le centre d'un corps qu'on ne voit pas entièrement, que je puis avoir obſervé le centre de Mercure ſur le bord du ſoleil trop tôt à l'entrée, et trop tard à la ſortie relativement aux obſervations des contacts. Cette diſcuſſion, en apparence d'ailleurs peu importante, devient ici d'une grande néceſſité, parcequ'il s'agit de montrer les défauts de deux obſervations que je ne rejette qu'avec beaucoup de regrets; car l'obſervation du centre de la planète ſur le bord du ſoleil n'étant pas affectée de l'effet de pluſieurs élémens (le diamètre de la planète et l'effet d'une atmoſphère qui l'envelopperait) que nous connaiſſons ſouvent mal, ou que nous ignorons abſolument, offrirait des avantages réels, ſi elle pouvait ſe faire avec une certaine préciſion.

9. Quoique les réſultats de mes calculs s'accordent aſſez pour inſpirer quelque confiance, je n'ai cependant pas été trop ſatiſfait de trouver la plus courte diſtance des centres de 1″,3 plus grande par les contacts extérieurs que par les contacts intérieurs. Cette différence annonce une erreur dans les durées. Ou la durée du paſſage entre les deux contacts extérieurs eſt trop petite, ou celle des contacts intérieurs eſt trop grande. Mais je me ſuis impoſé la loi de ne jamais faire aucune correction à mes obſervations lorſque je ne les ai accompagnées d'aucune marque qui me faſſe douter de leur bonté; je ne trouve donc aucune raiſon qui m'autoriſe à changer la durée des contacts intérieurs, et quand je voudrais m'écarter ici un moment de mes

 prin-

principes pour augmenter la durée des contacts extérieurs, je ne le pourrais faire qu'en considération de l'incertitude avec laquelle on peut estimer le contact extérieur de l'entrée trop tard, et celui de la sortie trop tôt, ce qui est toujours probable ; mais je ne la pourrais augmenter que tout au plus de 5 à 6 secondes de tems, puisqu'on a vu dans l'article précédent qu'il n'y a que 17″ d'incertitude sur l'estime des deux observations du centre de Mercure sur les bords du soleil qui comparativement entre elles-mêmes se font beaucoup moins exactement. Or ces 5 ou 6 secondes d'augmentation sur la durée extérieure ne suffisent pas à beaucoup près (car il en faudrait 106″) pour réduire à zéro la différence qui se trouve entre les deux valeurs de la plus courte distance des centres. Il faut donc chercher ailleurs que dans les observations la cause de cette différence. C'est ce que je crois pouvoir trouver dans l'effet d'une atmosphère supposée autour de Mercure, ou d'une cause semblable.

10. D'après les recherches que j'ai faites sur l'atmosphère de Venus à l'occasion de son passage en 1769, et dont j'ai établi et démontré les principes dans un petit Traité complet sur les passages de Venus et de Mercure, j'étais prévenu que la circonstance caractéristique de ce passage de Mercure qui était si desavantageuse à l'égard de l'utilité qu'on en retire pour perfectionner les tables, devait être extrêmement favorable à la détermination de l'effet d'une atmosphère qui environnerait Mercure, puisque la planète passant fort près du bord du soleil, son mouvement se faisait très obliquement à ce bord, et agrandissait beaucoup l'effet d'une atmosphère. En conséquence je me suis singulièrement appliqué à observer ce passage et principalement les quatre contacts avec la plus grande attention, afin de me procurer des observations suffisamment exactes pour pouvoir m'en servir avec avantage à determiner l'effet de cette atmosphère,

ſphère, ou du moins à m'aſſurer de ſon exiſtence. Je puis dire maintenant que les réſultats de mes calculs, de quelque manière que je les combine, en ſuppoſant l'obſervation et le diamètre de Mercure employé dans mes calculs rigoureuſement exacts, m'indiquent la préſence d'un effet ſemblable à celui d'une réfraction ou inflexion que ſouffriraient les rayons ſolaires dans leur paſſage auprès du globe de Mercure. Voici comment.

11. J'ai démontré dans le petit Traité que je viens de citer que la combinaiſon des deux obſervations des contacts extérieurs doit donner le même inſtant pour celui du milieu du paſſage que la combinaiſon des deux contacts intérieurs, et que cet inſtant du milieu du paſſage déduit de l'une et de l'autre combinaiſon reſtera toujours abſolument le même, qu'on ſuppoſe la planete entourée d'une atmoſphère ou non. Il eſt évident qu'à plus forte raiſon le milieu du paſſage déduit de la combinaiſon des deux obſervations du centre de la planète ſur la bord du ſoleil ne ſera point altéré par l'effet d'une atmoſphère, puiſqu'elle n'influe pas même ſur chacune de ces deux obſervations ſéparément. Enſuite j'ai encore fait voir que dans la ſuppoſition d'une atmoſphère autour de la planète qui paſſe ſur le diſque du ſoleil, le milieu du paſſage déduit de la combinaiſon de l'obſervation du contact extérieur de l'entrée avec celle du contact intérieur de la ſortie, doit arriver plus tard; et le milieu du paſſage donné par la combinaiſon du contact intérieur de l'entrée avec le contact exterieur de la ſortie, doit arriver préciſément de la même quantité plutôt que le milieu du paſſage conclu par la combinaiſon des deux contacts intérieurs, ou par celle des deux contacts extérieurs, ou, ce qui revient encore au même, que le milieu du paſſage que donneraient indiſtinctement toutes les obſervations des contacts combinées comme on voudra, ſi la planète n'avait point d'atmoſphère. La différence

ou la quantité, dont le milieu du paſſage eſt trouvé plus tard ou plutôt, ſera l'effet de l'atmoſphère de la planète ſur le milieu du paſſage.

12. En conſéquence de ces principes j'ai donc fait encore deux combinaiſons pour en déduire le milieu du paſſage, et j'ai trouvé que la combinaiſon du contact extérieur de l'entrée avec le contact intérieur de la ſortie donne cet inſtant à 3h. 40′ 13″,6; celle du contact interieur de l'entrée avec le contact extérieur de la ſortie le donne à 3 h. 39′ 20″,8. Or on a vu (art. 7.) que le milieu du paſſage, ſelon la combinaiſon des deux contacts intérieurs et celle des deux contacts extérieurs eſt arrivé à 3 h. 39′ 47″,2, quantité qui ſe trouve entre les deux précédentes et exactement à égales diſtances de l'une et de l'autre, ſavoir de 26″,4. Il eſt donc évident que l'effet de l'atmoſphère de Mercure dans ce paſſage-ci a été 26″,4 de tems ſur le milieu du paſſage, en faiſant abſtraction de toute autre cauſe qui peut avoir quelque influence ſur les obſervations des contacts.

13. Mais ces 26″,4 ne peuvent provenir que de trois cauſes: ou de l'inexactitude des obſervations, ou d'une erreur ſur les diamètres du ſoleil et de Mercure employés dans les calculs, ou de la réfraction des rayons ſolaires dans l'atmoſphère de Mercure; ainſi que je l'ai démontré dans mon petit Traité ſur les Paſſages de Venus et de Mercure, et où j'arrive, après un examen rigoureux de toutes les hypothèſes poſſibles, à cette équation générale $A = \alpha \pm \beta \pm \gamma + \varepsilon$; dans laquelle A eſt la quantité déterminée par les combinaiſons des obſervations, comme ici les 26″,4, et par conſéquent connue; α, β, γ, ε, la part qui en appartient reſpectivement à l'atmoſphère de la planète, à l'erreur de ſon diamètre, à celle du ſoleil et à l'erreur de l'obſervation. Je ferai remarquer ſeulement au ſujet de cette formule qu'il n'y a que l'erreur ſur le diamètre de la planète dont l'effet β pourrait quelquefois entrer dans

dans le valeur de A comme quantité négative, mais alors, loin de nuire à l'opinion d'attribuer cet effet, qui eſt ici de 26″,4, à l'atmoſphère de la planète, elle la favoriſerait plutôt. Quant à l'erreur ſur le diamètre du ſoleil, ſon influence peut être regardée comme nulle dans tous les cas, c'eſt à dire γ peut toujours être regardé comme zéro, à moins que l'erreur ſur le diamètre du ſoleil ne ſoit très conſidérable, et c'eſt un des avantages de ma méthode pour déterminer la valeur de A. Or la probabilité ſerait en faveur des obſervations, puiſqu'elles donnent, ce qui eſt conforme à la théorie, le même intervalle de tems entre les deux contacts de l'entrée qu'entre les deux contacts de la ſortie, ainſi l'on aurait ici $\varepsilon = 0$. Quant aux deux autres cauſes, il n'en eſt pas de même, puiſqu'il eſt évident par la formule générale qu'une même quantité conſidérée comme erreur ſur les diamètres, ou comme réfraction des rayons ſolaires dans l'atmoſphère de Mercure, eſt capable de produire exactement le même effet. Mais comme il eſt très probable que les trois cauſes à la fois peuvent avoir concouru à produire ces 26″,4 = A, et qu'il eſt abſolument impoſſible, d'après ma méthode comme d'après toute autre, de démêler les effets pour aſſigner à chaque cauſe la part qui lui appartient dans la valeur de A, le problème reſtera indéterminé à cet égard, par conſéquent ſi l'on ne veut admettre qu'une ſeule cauſe, on ſera libre de ſe décider pour l'une ou pour l'autre; or la queſtion n'étant plus alors qu'une affaire d'opinion, le choix doit tomber néceſſairement ſur la cauſe qui eſt la moins connue, et dont nous ne pouvons pas raiſonnablement conteſter l'exiſtence. On peut donc fort bien attribuer cet effet à l'atmoſphère de Mercure ſans craindre de ſe tromper beaucoup. Il s'en ſuit donc qu'en regardant ces 26″,4 ſimplement comme effet de l'atmoſphere de Mercure, la quantité, qui en réſulterait pour l'inflexion ou la

réfraction

réfraction réelle de cette atmosphere, nous assurerait au moins d'une espece de limite qu'elle ne surpasserait jamais ou du moins très-rarement, puisque l'inflexion des rayons solaires, à elle-seule, ne peut égaler la somme des trois causes dont elle fait partie, que dans l'hypothèse particulière des deux autres égales à zéro. Cette manière d'envisager le problème me donnera du moins une connaissance approchée de la valeur de la réfraction de l'atmosphère de Mercure, dont je n'aurais sans cette recherche absolument aucune idée. Or il me semble qu'il vaut mieux acquérir une connaissance imparfaite que de rester dans l'ignorance absolue.

14. La quantité de cet effet, quel qu'il soit, étant donc connue, j'ai cherché à concilier les deux valeurs de la plus courte distance des centres trouvées par les contacts intérieurs et extérieurs, et pour cet effet je me suis proposé ce problème qui doit s'en suivre naturellement, puisque la valeur de y peut toujours être regardée comme zéro: *Déterminer le diamètre du soleil, celui de Mercure étant connu par observation, tel que la durée donnée par les contacts extérieurs et la durée des contacts intérieurs fassent trouver, l'une et l'autre, la même quantité pour valeur de la plus courte distance des centres.* Ce problème étant resolu en nommant a, la demi-durée entre les contacts exterieurs, b celle des contacts intérieurs, δ le diametre de Mercure, x la différence des demi-diamètres de Mercure et du soleil, y la plus courte distance des centres cherchée, je trouve $x = \frac{a^2 - b^2 - \delta^2}{2\delta}$ et $y = \sqrt{\left(\frac{a^2 - b^2 - \delta^2}{2\delta}\right)^2 - b^2}$; formules qui étant évaluées après avoir convenablement corrigé des 26,″4 chaque observation des quatre contacts, et augmenté la durée des contacts extérieurs de ces 6″ dont j'ai parlé ci-devant, me donnent $x = 967'',04$ valeur plus

 petite

petite de $0'',44$ que celle que j'avais ſuppoſée dans mes calculs, et $y = 15'45'',24$ plus grande de $0'',1$ que celle qui m'a été donnée par la combinaiſon des deux contacts intérieurs; ainſi tous les réſultats trouvés par cette combinaiſon n'auront beſoin d'aucune correction, et je les adopterai, comme étant les meilleurs, tels qu'ils ſont rapportés dans la table ci-deſſus art. 7.

15. En ſuppoſant donc que les $26'',4$ ſoient produites par l'atmoſphère de Mercure, je trouve $0''276$ pour la réfraction horizontale de cette atmoſphère. Les obſervations du paſſage de Venus en 1769 m'ayant annoncé un effet ſemblable d'environ $8''$ à $9''$ de tems, je trouve ſa réfraction horizontale d'environ $0''205$ qui n'eſt qu'à peu près les deux tiers de celle de Mercure.

CONCLUSION.

16. Quelque peu de confiance que j'attache à ces réſultats, et quelque ſoit l'opinion que j'adopte pour choiſir entre les cauſes qui peuvent produire l'effet en queſtion, je crois du moins pouvoir conclure avec certitude, ce que je m'étais principalement propoſé de prouver dans ce Mémoire, que les obſervations dont il s'agit ici, malgré le degré d'incertitude qu'on puiſſe leur ſuppoſer, indiquent clairement l'exiſtence d'un effet ſemblable à celui d'une atmoſphère qui environnerait la planète; et que cet effet, ſoit qu'il provienne effectivement de cette atmoſphère, ou d'une erreur ſur le diamètre de la planète, ou d'une erreur dans les obſervations, ou qu'il ſoit le réſultat de l'action ſimultanée des trois cauſes réunies, il ſe fait ſentir evidemment dans toutes les obſervations des paſſages de Venus et de Mercure, du moins dans toutes celles que j'ai calculées. Par conſéquent l'influence de ces cauſes qui altèrent les obſervations d'une maniere ſi ſenſible me parait, ſous tous les points de vue, mériter l'attention des aſtronomes; et je ſuis très perſuadé que faute d'y avoir eu égard

égard dans la comparaison des observations du passage de Venus pour en déduire la parallaxe du soleil, bien des astronomes seraient dans le cas de recommencer leur calcul. Heureusement je n'ai pas ce reproche à me faire; car j'ai constamment évité avec le plus grand soin l'effet d'une atmosphère autour de Venus en choisissant les observations pour en faire la comparaison de manière que l'effet de cette atmosphère, qu'il ait existé ou non, se trouvait toujours réduit à zéro. C'est ainsi que dans mon Mémoire sur le passage de Venus en 1769 j'ai fixé à 8″,7 la parallaxe horizontale du soleil dans ses moyennes distances à la terre.

XXV. *Thoughts on the conſtituent Parts of Water and of Dephlogiſticated Air; with an Account of ſome Experiments on that Subject. In a Letter from Mr.* James Watt, *Engineer, to Mr.* De Luc, *F. R. S.*

Read April 29, 1784.

DEAR SIR, Birmingham, November 26, 1784.

IN compliance with your deſire, I ſend you an account of the hypotheſis I have ventured to form on the probable cauſes of the production of water from the deflagration of a mixture of dephlogiſticated and inflammable airs, in ſome of our friend Dr. PRIESTLEY's experiments.

I feel much reluctance to lay my thoughts on theſe ſubjects before the public in their preſent indigeſted ſtate, and without having been able to bring them to the teſt of ſuch experiments as would confirm or refute them; and ſhould, therefore, have delayed the publication of them until theſe experiments had been made, if you, Sir, and ſome other of my philoſophical friends, had not thought them as plauſible as any other conjectures which have been formed on the ſubject; and that though they ſhould not be verified by further experiments, or approved of by men of ſcience in general, they may perhaps merit a diſcuſſion, and give riſe to experiments which may throw light on ſo important a ſubject.

I firſt thought of this way of ſolving the phænomena in endeavouring to account for an experiment of Dr. PRIEST-

LEY's, wherein water appeared to be converted into air; and I communicated my sentiments in a letter addressed to him, dated April 26, 1783 *, with a request that he would do me the honour to lay them before the Royal Society; but as, before he had an opportunity of doing me that favour, he found, in the prosecution of his experiments, that the apparent conversion of water into air, by exposing it to heat in porous earthen vessels, was not a real transmutation, but an exchange of the elastic fluid for the liquid, in some manner not yet accounted for; therefore, as my theory was no ways applicable to the explaining these experiments, I thought proper to delay its publication, that I might examine the subject more deliberately, which my other avocations have prevented me from doing to this time.

II. It has been known for some time, that inflammable air contained much phlogiston; and Dr. PRIESTLEY has found, by some experiments made lately, that it "is either wholly "pure phlogiston, or at least that it contains no apparent mix-"ture of any other matter." (In my opinion, however, it contains a small quantity of water and much elementary heat.)

* This letter Dr. PRIESTLEY received at London; and, after shewing it to several Members of the Royal Society, he delivered it to Sir JOSEPH BANKS, the President, with a request that it might be read at some of the public meetings of the Society; but before that could be complied with, the author, having heard of Dr. PRIESTLEY's new experiments, begged that the reading might be delayed. The letter, therefore, was reserved until the 22d of April last; when, at the author's request, it was read before the Society. It has been judged unnecessary to print that letter, as the essential parts of it are repeated, almost *verbatim*, in this letter to M. DE LUC; but, to authenticate the date of the author's ideas, the parts of it which are contained in the present letter are marked with double commas.

heat *.) " He found, that by expoſing the calces of metals " to the ſolar rays, concentrated by a lens, in a veſſel contain- " ing inflammable air only, the calces of the ſofter metals " were reduced to their metallic ſtate;" and that the inflammable air was abſorbed in proportion as they became phlogiſticated; and, by continually ſupplying the veſſel with inflammable air, as it was abſorbed, he found, that out of 101 ounce meaſures, which he had put into the veſſel, 99 ounce meaſures were abſorbed by the calces, and only two ounce meaſures remained, which, upon examination, he found to be nearly of the ſame quality the whole quantity had been of before the experiment, and to be ſtill capable of deflagrating in conjunction with atmoſpheric or with dephlogiſticated air. *Therefore, as ſo great a quantity of inflammable air had been abſorbed by the metallic calces; the effect of reducing them to their metallic ſtate had been produced; and the ſmall remaining portion was ſtill unchanged, at leaſt had ſuffered no change which might not be attributed to its original want of purity; it was reaſonable to conclude, that inflammable air muſt be the pure phlogiſton, or the matter which reduced the calces to metals.*

2. " The ſame ingenious philoſopher mixed together cer- " tain proportions of pure dry dephlogiſticated air and of pure " dry inflammable air in a ſtrong glaſs veſſel, cloſely ſhut, " and then ſet them on fire by means of the electric ſpark," in the ſame manner as is done in the inflammable air piſtol. " The firſt effect was the appearance of red heat or inflamma-

* Previous to Dr. PRIESTLEY's making theſe experiments, M. KIRWAN had proved, by very ingenious deductions from other facts, that inflammable air was, in all probability, the real phlogiſton, in an aerial form. Theſe arguments were perfectly convincing to me; but it ſeems more proper to reſt that part of the preſent hypotheſis on the direct experiment.

"tion in the airs, which was soon followed by the glass vessel "becoming hot. The heat gradually pervaded the glass, and "was dissipated in the circumambient air, and as the glass "grew cool, a mist or visible vapour appeared in it, which "was condensed on the glass in the form of moisture or dew*. "When the glass was cooled to the temperature of the atmo-"sphere, if the vessel was opened with its mouth immersed in "water or mercury, so much of these liquids entered, as was "sufficient to fill the glass within about $\frac{1}{100}$dth part of its "whole contents; and this small residuum may safely be con-"cluded to have been occasioned by some impurity in one or "both kinds of air. The moisture adhering to the glass, after "these deflagrations, being wiped off, or sucked up, by a "small piece of sponge paper, first carefully weighed, was "found to be exactly, or very nearly, equal in weight to the "airs employed."

"In some experiments, but not in all, a small quantity of "a sooty-like matter was found adhering to the inside of the "glass," the origin of which is not yet investigated; but Dr. PRIESTLEY thinks, that it arises from some minute grains of the mercury that was used in order to fill the glass with the air, which being super-phlogisticated by the inflammable air, assumed that appearance; but, from whatever cause it proceeded, "the whole quantity of sooty-like matter was too "small to be an object of consideration, particularly as it did "not occur in all the experiments."

I am obliged to your friendship for the account of the experiments which have been lately made at Paris on this subject,

* I believe that Mr. CAVENDISH was the first who discovered that the combustion of dephlogisticated and inflammable air produced moisture on the sides of the glass in which they were fired.

with

with large quantities of these two kinds of air, by which the essential point seems to be clearly proved, that the deflagration or union of dephlogisticated and inflammable air, by means of ignition, produces a quantity of water equal in weight to the airs; and that the water, thus produced, appeared, by every test, to be pure water. As I am not furnished with any particulars of the manner of making the experiment, I can make no observations on it, only that, from the character you give me of the gentlemen who made it, there is no reason to doubt of its being made with all necessary precautions and accuracy, which was farther secured by the large quantities of the two airs consumed.

3. "Let us now consider what obviously happens in the "case of the deflagration of the inflammable and dephlogisti- "cated air. These two kinds of air unite with violence, they "become red-hot, and upon cooling totally disappear. When "the vessel is cooled, a quantity of water is found in it equal "to the weight of the air employed. This water is then the "only remaining product of the process, and *water*, *light*, and "*heat*, are all the products," unless there be some other mat- ter set free which escapes our senses.

"*Are we not then authorised to conclude, that water is com- "posed of dephlogisticated air and phlogiston, deprived of part of "their latent or elementary heat; that dephlogisticated or pure air "is composed of water deprived of its phlogiston, and united to "elementary heat and light; and that the latter are contained in "it in a latent state, so as not to be sensible to the thermometer or "to the eye; and if light be only a modification of heat, or a cir- "cumstance attending it, or a component part of the inflammable "air, then pure or dephlogisticated air is composed of water de- "prived of its phlogiston and united to elementary heat?*"

4. "It

4. "It appears, that dephlogiſticated water," or, which may be a better name for the baſis of water and air, the element you call *humor*, "has a more powerful attraction for "phlogiſton than it has for latent heat, but that it cannot "unite with it, at leaſt not to the point of ſaturation, or to "the total expulſion of the heat, unleſs it be firſt made red-"hot," or nearly ſo. "The electric ſpark heats a portion o "it red-hot, the attraction between the humor and the phlo-"giſton takes place, and the heat which is let looſe from this "firſt portion heats a ſecond, which operates in a like manner "on the adjoining particles, and ſo continually until the whole "is heated red-hot and decompoſed." Why this attraction does not take place to the ſame degree in the common temperature of the atmoſphere, is a queſtion I am not yet able to ſolve; but it appears, that, in ſome circumſtances, "dephlo-"giſticated air can unite, in certain degrees, with phlogiſton "without being changed into water." Thus Dr. PRIESTLEY has found, that by taking clean filings of iron, which, alone, produce only inflammable air of the pureſt kind, and *mercurius calcinatus per ſe*, which gives only the pureſt dephlogiſticated air, and expoſing them to heat, in the ſame veſſel, he obtained neither dephlogiſticated nor inflammable air, "but in their "place fixed air." Yet it is well known, that a mixture of dephlogiſticated and inflammable air will remain for years in cloſe veſſels in the common heat of the atmoſphere, without ſuffering any change, the mixture being as capable of deflagration at the end of that time as it was when firſt ſhut up. Theſe facts the Doctor accounts for, by ſuppoſing that the two kinds of air, when formed at the ſame time in the ſame veſſel, can unite in their *naſcent* ſtate; but that, when fully formed, they are incapable of acting upon one another, unleſs they are firſt

first set in motion by external heat. "Phlogisticated air seems "also to be another composition of phlogiston and dephlogisti-"cated air;" but in what proportions they are united, or by what means, is still unknown. It appears to me to be very probable, that fixed air contains a greater quantity of phlogiston than phlogisticated air does, because it has a greater specific gravity, and because it has more affinity with water.

5. "For many years I have entertained an opinion, that air "was a modification of water, which was originally founded "on the facts that in most cases, wherein air was actually "made," which should be distinguished from those wherein it is only extricated from substances containing it in their pores, or otherwise united to them in the state of air, "the sub-"stances were such as were known to contain water as one of "their constituent parts, yet no water was obtained in the "processes," except what was known to be only loosely connected with them, such as the water of the crystallization of salts. "This opinion arose from a discovery," that the latent heat contained in steam diminished in proportion as the sensible heat of the water from which it was produced increased; or, in other words, "that the latent heat of steam was less "when it was produced under a greater pressure, or in a more "dense state, and greater when it was produced under a less "pressure, or in a less dense state; which led me to conclude, "that when a very great degree of heat was necessary for the "production of the steam, the latent heat would be wholly "changed into sensible heat; and that in such cases, the "steam itself might suffer some remarkable change. I now "abandon this opinion in so far as relates to the change of "water into air, as I think that may be accounted for on better "principles."

6. "In every case, wherein dephlogisticated air has been "produced, substances have been employed, some of whose "constituent parts have a strong attraction for phlogiston, and, "as it would appear, a stronger attraction for that substance "than *humor* has; they should, therefore, dephlogisticate the "water" or fixed air, and the *humor* thus set free should unite to the matter of fire and light and become pure air. Dephlogisticated air is produced in great abundance from melted nitre. "The acid of nitre has a greater attraction for phlogiston than "any other substance is known to have; and it is also certain, "that nitre, besides its water of crystallization, contains a "quantity of water as one of its elementary parts, which "water adheres to the other parts of the nitre with a force "sufficient to enable it to sustain a red heat. When the nitre "is melted, or made red-hot, the acid acts upon the water and "dephlogisticates it; and the fire supplies the *humor* with the "due quantity of heat to constitute it air, under which form "it immediately issues. It is not easy to tell what becomes of "the acid of nitre and phlogiston, which are supposed to be "united," as they seem to be lost in the process. Dr. PRIESTLEY has lately made some experiments, with a view to ascertain this point. He distilled dephlogisticated air from pure nitre, in an earthen retort glazed within and without. He employed 2 oz. = 960 grains of nitre: the retort was placed in an air furnace, and, by means of an intense heat, he obtained from the nitre in one experiment 787, and in another experiment 800 ounce measures of dephlogisticated air; and he found that, upon weighing the retort and nitre before and after the process, they had suffered a loss of weight equal to the weight of the air, and to the water of crystallization of the nitre, but nothing more. He remarked, that the air had a pungent smell,

ſmell, which he could not diveſt it of by waſhing; and that the water in which the air was received had become ſlightly acid. I examined a portion of this water, which he was ſo kind as to ſend me, and found by it that the whole of the receiving water had contained the acid belonging to 2 drams = 120 grains of nitre. I alſo examined the reſiduum and the retort in which the diſtillation had been performed, and found the reſiduum highly alkaline, yet containing a minute quantity of phlogiſticated nitrous acid. It had acted conſiderably upon the retort, and had diſſolved a part of it, which was depoſited in the form of a browniſh powder, when the ſaline part was diſſolved in water. This earthy powder I have not yet thoroughly examined, but have no doubt that it principally conſiſts of the earth of the retort. This experiment, and all others tried in earthen veſſels, leave us ſtill at a loſs to determine what becomes of the acid and phlogiſton. They ſeem either to remain mixed with the air, in the form of an incoercible gas; or to unite with the alkali, or with the earth of the retort, in ſome manner ſo as not to be eaſily ſeparated from them; or elſe they are imbibed by the retorts themſelves, which are ſufficiently porous to admit of ſuch a ſuppoſition.

All that appears to be concluſive from this experiment is, that above one half of the weight of the nitre was obtained in the form of dephlogiſticated air; and that the reſiduum ſtill contained ſome nitrous acid united to phlogiſton.

7. Finding that the action of the nitre on the retort tended to prevent any accurate examination of the products, I had recourſe to combinations of the nitrous acid with earths from which the dephlogiſticated air is obtained with leſs heat than from nitre itſelf. As theſe proceſſes have been particularly deſcribed by Dr. PRIESTLEY, by Mr. SCHEELE, and others, I

 ſhall

ſhall not enter into any detail of them; but ſhall mention the general phænomena which I obſerved, and which relate to the preſent ſubject.

The earths I uſed were magneſia alba, calcareous earth, and minium or the red calx of lead. I diſſolved them in the reſpective experiments in nitrous acid dephlogiſticated by boiling, and diluted with proper proportions of water. I made uſe of glaſs retorts, coated with clay; and I received the air in glaſs veſſels, whoſe mouths were immerſed in a glazed earthen baſon, containing the ſmalleſt quantity of water that could be uſed for the purpoſe. As ſoon as the retort was heated a little above the heat of boiling water, the ſolutions began to diſtil watery vapours containing nitrous acid. Soon after theſe vapours ceaſed, yellow fumes, and in ſome of the caſes dark red fumes, began to appear in the neck of the retort; and at the ſame time there was a production of dephlogiſticated air, which was greater in quantity from ſome of theſe mixtures than from others, but continued in all of them until the ſubſtances were reduced to dryneſs. I found, in the receiving water &c. very nearly the whole of the nitrous acid uſed for their ſolution, but highly phlogiſticated, ſo as to emit nitrous air by the application of heat; and there is reaſon to believe, that with more precaution the whole might have been obtained.

8. As the quantity of dephlogiſticated air produced by theſe proceſſes did not form a ſufficient part of the whole weight, to enable me to judge whether any of the real acid entered into the compoſition of the air obtained, I ceaſed to purſue them further, having learned from them the fact, *that however much the acid and the earths were dephlogiſticated before the ſolution, the acid always became highly phlogiſticated in the proceſs.*

In

In order to examine whether this phlogiston was furnished by the earths, some dephlogisticated nitrous acid was distilled from minium till no more acid or air came over. More of the same acid was added to the minium as soon as it was cold, and the distillation repeated, which produced the same appearance of red fumes and dephlogisticated air. This operation was repeated a third time on the same minium, without any sensible variation in the phænomena. The process should have been still farther repeated, but the retort broke about the end of the third distillation. The quantity of minium used was 120 grains, and the quantity of nitrous acid added each time was 240 grains, of such strength that it could dissolve half its weight of mercury, by means of heat.

It appears from this experiment, that unless minium be supposed to consist principally of phlogiston, the source of the phlogiston, thus obtained, was either the nitrous acid itself, or the water with which it was diluted; or else that it came through the retort with the light, for the retort was in this case red-hot before any air was produced; yet this latter conclusion does not appear very satisfactory, when it is considered, that in the process wherein the earth made use of was magnesia, the retort was not red-hot, or very obscurely so, in any part of the process; and by no means luminous, when the yellow and red fumes first made their appearance.

9. As the principal point in view was to determine whether any part of the acid entered into the composition of the air, I resolved to employ some substance which would part with the acid in a moderate heat, and also give larger quantities of air than had been obtained in the former processes. Mercury was thought a proper substance for this purpose. 240 grains of mercury were put into a glass retort with 480 grains

of diluted dephlogisticated nitrous acid, which was the quantity necessary to dissolve the whole of the mercury, a gentle heat was applied, and as soon as the common air contained in the retort was dissipated, a vessel was placed to receive the nitrous air proceeding from the solution, which was 16 ounce measures. When it had ceased to give nitrous air, the neck of the retort became hot from the watery steams of the acid. The air receiver was taken away, and a common receiver was luted on, with a little water in it, to condense the vapours, and a quantity of dilute, but highly phlogisticated, acid was caught in the receiver. When the watery vapours had nearly come over, and yellow fumes appeared in the neck of the retort, the common receiver was removed, and the air receiver replaced; about four ounces of very strong nitrous air passed up immediately, the fumes in the retort became red, and dephlogisticated air passed up, which, uniting with the nitrous air in the receiver, produced red fumes in the receiver; and the two kinds of air acting upon one another, their bulk was reduced to half of an ounce measure. At this period the fumes in the retort were of a dark red colour, and dephlogisticated air was produced very fast. After a short time, some orange-coloured sublimate appeared in the upper part of the retort, and extended a little way along its neck, the red colour of the fumes gradually disappeared, and the neck of the retort became quite clear. At the same time that this happened, small globules of mercury appeared in the neck of the retort, and accumulated there until they ran down in drops. The production of the air was now very rapid, and accompanied with much of the white cloud or powdery matter, which passed up with the air into the receiver, and mixed with the water, but did not dissolve in it. After giving about 36 ounce measures of dephlogisticated air,

it

it suddenly ceased to give any more; and the retort being cooled, the bulb was found to be quite empty, excepting a small quantity of black powder, which, on being rubbed on the hand, proved to be mostly running mercury. The orange-coloured sublimate was washed out of the neck of the retort, and what running mercury was in it was separated, and added to that which had run down into the bason among the water. The whole fluid mercury, when dried, weighed 218 grains; therefore 22 grains remained in the form of sublimate, which, I believe, would also have been reduced if I could have applied heat in a proper manner to the neck of the retort, as some of it, to which heat could be applied, disappeared.

10. The 16 ounce measures of nitrous air, which had been produced in the solution of the mercury, and had remained confined by water in the receiver, was converted into nitrous acid by the gradual admission of common air, and was taken up by the water; this water was added to that in the bason, which had served to receive the dephlogisticated air. The whole quantity was about two quarts, was very acid to the taste, and sparkling with nitrous air. It was immediately put into bottles, and well corked, until it had lost the heat gained in the operation. In order to determine the quantity of acid in the receiving water and in the sublimate, I dissolved, first, alkali of tartar in water, and filtered the solution. 352 grains of this alkaline solution saturated 120 grains of the nitrous acid I had employed to dissolve the mercury, and 1395 grains of the same alkaline solution saturated the orange-coloured precipitate, and all the acid liquor obtained from the process: therefore we have the proportion as 352 : 120 :: 1395 : 475, from which it appears, that all the acid employed was recovered again in the form of acid, excepting only five grains; a smaller

a ſmaller quantity than what might reaſonably be ſuppoſed to be loſt in the proceſs by the extreme volatility of the nitrous air. In order to aſcertain the exact point of ſaturation, ſlips of paper, ſtained by the juice of the petals of the ſcarlet roſe, were employed, which were the niceſt teſt I could procure, as litmus will not ſhew the point of ſaturation of any liquor containing much phlogiſticated nitrous acid, or even fixed air, but will turn red, and ſhew it to be acid, when the teſt of thoſe leaves, violets, and ſome other of the like kind, will turn green in the ſame liquor, and ſhew it to be alkaline. But the exact point of ſaturation of ſo dilute a liquor is ſo very difficult to aſcertain, than an error might eaſily be committed, notwithſtanding the attention beſtowed upon it. Suppoſing this experiment to be unexceptionable, the concluſions which may be drawn from it are very favourable to the hypotheſis I endeavour to ſupport. *Thirty-ſix ounce meaſures of dephlogiſticated air were obtained, and only five grains of a weak nitrous acid were loſt in the proceſs. Two hundred and eighteen grains of mercury out of two hundred and forty were revived, and all the dephlogiſticated nitrous acid employed is found to be highly phlogiſticated in the proceſs. It appears, that the nitrous acid does not enter into the compoſition of dephlogiſticated air; it ſeems only to ſerve to abſorb phlogiſton from the watery part of the mercurial nitre.*

11. As this laſt proceſs proved very tedious and complicated on account of the neceſſity of aſcertaining the quantity of acid in the receiving water, by means of an alkali which afforded a double ſource of error in the point of ſaturation, I reſolved to try the diſtillation of dephlogiſticated air from cubic nitre in a glaſs veſſel, and to draw from it only ſuch a quantity of air as it would yield without acting much upon the retort, which latter

latter circumstance is essentially necessary to be attended to. An ounce of the crystals of mineral alkali were dissolved in nitrous acid, and the mixture brought to an exact saturation by the test of litmus; 30 ounce measures of air were distilled from it, which, during the latter part of the process, was accompanied with slightly yellow fumes; the receiving water was found to be acid, and the residuum alkaline. The residuum being dissolved in the receiving water, the solution was neutral, or very nearly so, by every test; for in this case litmus might be used, as the acid was very slightly phlogisticated. On adding a few drops of a very dilute nitrous acid, the tests shewed the liquor to be acid.

12. Encouraged by the success of this experiment, I took an ounce = 480 grains of pure common nitre, and put it into a flint-glass retort, coated, which was placed in a furnace. It began to give air about the time it became red-hot, and during the latter part of the process this air was accompanied with yellowish fumes. I stopped the process when it had produced 50 ounce measures of air. The receiving water, and particularly the air, had a strong but peculiar smell of nitrous acid. The air was well washed with the receiving water, but was not freed from the smell. The receiving water, which was 50 ounces, was slightly acid, and the residuum alkaline. I dissolved the latter in the former, and found the mixture alkaline. 10 grains of weak nitrous acid were added to it, which saturated it, and 105 grains of this spirit of nitre was found to contain the acid of 60 grains of nitre; therefore the 10 grains contained the acid of 5,7 grains of nitre, which, by Mr. KIRWAN's experiments is equal to two grains of real nitrous acid. *We have, therefore, 34 grains weight of dephlogisticated air produced, and only two grains of real acid missing*; and it is not certain

certain that this quantity was destroyed, because some portion of the glass of the retort was dissolved by the nitre, and some part of the materials employed in making the glass being alkali, we may conclude, that the alkali of the nitre would be augmented by the alkali of that part of the glass it had dissolved. As the glass cracked into small pieces on cooling, and some part of the coating adhered firmly to it, the quantity of the glass that was dissolved could not be ascertained. *From this experiment it appears, that if any of the acid of the nitre enters into the composition of the dephlogisticated air, it is a very small part; and it rather seems, that the acid, or part of it, unites itself so firmly to the phlogiston as to lose its attraction for water.*

13. "The vitriolic salts also yield dephlogisticated air by "heat; and in these cases the dephlogisticated air is always "attended with a large quantity of vitriolic acid air or sul-"phureous vapour," even when the salts used are not known to contain any phlogistic matter. Mr. SCHEELE mentions his having obtained dephlogisticated air from manganese dissolved in acid of phosphorus, and also from the arsenical acid: from whence it appears, that these acids, or perhaps any acid which can bear a red heat, can concur to the production of dephlogisticated air. *It is necessary to remark, that no experiments have been yet published shewing that dephlogisticated air can be produced from salts formed by the muriatic acid. The acids which produce salts suitable for this purpose, have all a strong affinity with phlogiston; and the marine acid has either a very small affinity with it, or else is already saturated with it, at least so far saturated as not to be able to attract it from the* humor.

14. "The dephlogisticated air obtained from the pure calces "of metals may be attributed to the calces themselves, attract-"ing the phlogiston from water which they have imbibed from

" the atmosphere, or from dephlogisticating the fixed air which " they are known to contain."

It is very probable, that the dephlogisticated air extruded from growing vegetables may be owing to their dephlogisticating the water they grow in; but it appears more probable, that the plants have a power of dephlogisticating the fixed, or phlogisticated, air of the atmosphere.

" When dephlogisticated and nitrous air are mixed, the de-" phlogisticated air seizes part of the phlogiston of the nitrous " air." The water contained in the nitrous air, and the other part of the phlogiston, unite with the nitrous acid, which then assumes a liquid form, or at least that of a dense vapour; " and " that part of the latent heat of the two airs not essential to the " new combination is set at liberty *."

In the combustion of sulphur the same thing happens, but in a greater degree; for the vitriolic acid, having a much weaker attraction for phlogiston than air has, abandons it almost entirely to the latter, which is thereby converted into water, and in that form attracts the vitriolic acid, and reduces it to a liquid state. The same reasoning may be applied to the combustion of phosphorus, which is attended with similar effects.

* I cannot take upon me to determine, from any facts which have come to my knowledge, whether any part of the dephlogisticated air employed in this experiment is turned into fixed air; but I am rather inclined to think that some part is, because the quantity of heat, which is separated by the union of the two airs, does not seem to be so great as that which is separated when the dephlogisticated air is wholly changed into water: yet some water appears to be formed, because when the mixture is made over mercury, the solution of the mercury in the nitrous acid assumes a crystallized form, which, however, may be due to the watery part of the nitrous air.

15. I shall not make, at present, any further deductions from what I myself consider still in the light of a conjectural hypothesis, which I have perhaps dwelt upon too long already. I shall only beg your attention to some general reasoning on the subject; which, however, may possibly serve more to shew the uncertainty of other systems on the constituent parts of air, than the certainty of this. Some of those systems suppose dephlogisticated air to be composed of an acid and something else, some say phlogiston. If an acid enters into the composition of it, why does not that acid appear again when the air is decomposed, by means of inflammable air and heat? And why is the water which is the product of this process pure water? And if an acid forms one of its constituent parts, why has nobody been able to detect any difference in the dephlogisticated air, made by the help of different acids, when compared with one another, or with the air extruded by vegetables? These airs, of such different origins, appear to be exactly the same. And if phlogiston be a constituent part of air, why does it attract phlogiston with such avidity? Some have, on the other hand, contended that air is composed of earth, united to acids or phlogiston, or to both, or to some other matter. Here we must ask, what earth it is which is one of the component parts of air? All earths which will unite with the nitrous or vitriolic acids, and with some others, such as the phosphoric and the arsenical acids, will serve as bases for the formation of air, and the air produced from all of them appears by every test to be the same, when freed from accidental impurities. To this argument it is answered, that it is not any particular species of earth which is the basis of air, but elementary or simple earth, which is contained in all of them. If this were the

matter

matter of fact, would not that earth be found after the decomposition of the air?

Mr. SCHEELE has formed an hypothesis on this subject, in which he supposes heat to be composed of dephlogisticated air united to phlogiston, and that this combination is sufficiently subtile to pass through glass vessels. He affirms, that the nitrous and other acids, when in an ignited state, attract the phlogiston from the heat, and set the dephlogisticated air at liberty; but he does not seem to have been more successful than myself in explaining what becomes of the acid of nitre and phlogiston in the case of the decomposition of nitre by heat. And since we know, from the late experiments, that water is a composition of air, or more properly, *humor* and phlogiston, his whole theory must fall to the ground, unless that fact be otherwise accounted for, which it does not seem easy to do.

16. To return to the experiment of the deflagration of dephlogisticated and inflammable air, "it appears from the "two airs becoming red-hot on their union, that the quantity "of heat contained in one or both of them, is much greater "than that contained in steam; because, for the first moments "after the explosion, the water deposited by the air remains in "the form of steam, and consequently retains the latent heat "due to that modification of water. This matter may be easily "examined by firing the mixture of dephlogisticated and inflam-"mable air in a vessel immersed in another vessel containing a "given quantity of water of a known heat, and after the vessel "in which the deflagration is performed is come to the same "temperature with the water in which it is immersed, by ex-"amining how much heat that water has gained, which being "divided by the quantity of water produced by the decom-"position of the airs, will give the whole quantity of elemen-

" tary or latent heat which that water had contained, both as " air and as steam; and if from that quantity we deduct the " latent heat of the steam, the remainder will be the latent or " elementary heat contained more in air than in steam." This experiment may be made more compleatly by means of the excellent apparatus which Mess. LAVOISIER and DE LA PLACE have contrived for similar purposes.

Until direct experiments are made, we may conclude, from those which have been made by the gentlemen just named, on the decompositions of air by burning phosphorus and charcoal, that the heat extricated during the combustion of inflammable and dephlogisticated air is much greater than it appears to be; for they found that one Paris ounce (= 576 Parisian grains) of dephlogisticated air, when decomposed by burning phosphorus, melted 68,634 ounces of ice; and as, according to another of their experiments, ice, upon being melted, absorbs 135° of heat, by FAHRENHEIT's scale, each ounce of air gave out 68,634 × 135° = 9265°,590; that is to say, a quantity of heat which would have heated an ounce of water, or any other matter which has the same capacity for receiving heat as water has, from 32° to 9265½°: a surprising quantity! (It is to be understood, that all the latent heats mentioned herein are compared with the capacity of water). And when an ounce of dephlogisticated air was changed into fixed air, by burning charcoal, or by the breathing of animals, it melted 29,547 oz. of ice; consequently we have 29,547 × 135° = 3988°,845. the quantity of heat which an ounce of dephlogisticated air loses when it is changed into fixed air. By the heat extricated during the detonation of one ounce of nitre with one ounce of sulphur, 32 ounces of ice were melted; and, by the experiment I have mentioned of Dr. PRIESTLEY's (6), it appears that

 nitre

nitre can produce one half of its weight of dephlogisticated air. When the nitre and sulphur are kindled, the dephlogisticated air of the nitre unites with the phlogiston of the sulphur, and sets its acid free, which immediately unites to the alkali of the nitre, and produces vitriolated tartar. The dephlogisticated air, united to the phlogiston, is turned into water, part of which is absorbed by the vitriolated tartar, and part is dissipated in the form of vapours, or unites to the nitrous air, or other air, produced in the process.

As half an ounce of dephlogisticated air is, in this process, united by inflammation to a quantity of phlogiston sufficient to saturate it, and no fixed air is produced, it should melt a quantity of ice equal to the half of what was melted by the combination of an ounce of air with phlogiston in burning phosphorus; that is, it should melt 34,917 ounces of ice; and we find, by Mess. LAVOISIER and DE LA PLACE's experiment, that it actually melted 32 ounces of ice: the small difference may be accounted for by supposing, that the heat produced by the combustion might not be quite so great as that Dr. PRIESTLEY employed in his experiment; or, that the nitre might be less pure, and consequently not so much air formed. The two facts, however, agree near enough to permit us to conclude, *that dephlogisticated air, in uniting to the phlogiston of sulphur, produces as much heat as it does in uniting with the phlogiston of phosphorus.*

17. According to Dr. PRIESTLEY's experiments, dephlogisticated air unites compleatly with about twice its bulk of the inflammable air from metals. The inflammable air being supposed to be wholly phlogiston, and being $\frac{1}{9,6}$ of the weight of an equal bulk of dephlogisticated air, and being double in quantity, will be $\frac{1}{4,8}$ of the weight of the dephlogisticated air

it

it unites with. Therefore one ounce (576 grains) of dephlogisticated air, will require 120 grains of inflammable air, or phlogiston, to convert it into water. And supposing the heat extricated by the union of dephlogisticated and inflammable air to be equal to that extricated by the burning of phosphorus, we shall find, that the union of 120 grains of inflammable air with 576 grains of dephlogisticated air, extricates 9265° of heat.

18. In the experiment on the deflagration of nitre with charcoal, by Mess. LAVOISIER and DE LA PLACE, an ounce of nitre and one third of an ounce of charcoal melted twelve ounces of ice. Supposing the ounce of nitre to have produced half an ounce of dephlogisticated air, it ought to have consumed 0,1507 ounces of charcoal, and should have melted 14,773 ounces of ice; and I suppose it fell short of its effect by the heat not being sufficiently intense to decompose the nitre perfectly.

19. By the above gentlemen's experiment an ounce of charcoal required for its combustion 3,3167 ounces of dephlogisticated air, and produced 3,6715 ounces of fixed air; therefore there was united to each ounce of air, when changed into fixed air, 61,5 grains of phlogiston, and 3988° of heat were extracted. *It appears by these facts, that the union of phlogiston, in different proportions, with dephlogisticated air, does not extricate proportional quantities of heat.* For the addition of 61,5 grains produces 3988°. and the union of 120 grains produces 9265°. This difference may arise from a mistake in supposing the specific gravity of the inflammable air Dr. PRIESTLEY employed to have been only $\frac{1}{9,6}$ of that of dephlogisticated air; for if it be supposed that its specific gravity was a little more than $\frac{1}{7}$ of that of the dephlogisticated air, then equal additions of phlogiston would

have

have produced equal quantities of heat *: this matter should therefore be put to the test of experiment, by deflagrating dephlogisticated air with inflammable air of a known specific gravity, or by finding how much dephlogisticated air is necessary for the combustion of an ounce of sulphur, the quantity of phlogiston in which has been accurately determined by Mr. KIRWAN; or by finding the quantity of phlogiston in phosphorus, the quantity of dephlogisticated air necessary for its decomposition being known from Mess. LAVOISIER and DE LA PLACE's experiments.

On considering these latter gentlemen's experiments on the combustion of charcoal, a difficulty arises, to know what became of the remainder of the ounce of charcoal; for the dephlogisticated air, in becoming fixed air, gained only the weight of 0,3548, or about $\frac{1}{3}$ of an ounce; about $\frac{2}{3}$ of an ounce are therefore unaccounted for. The weight of the ashes of an ounce of charcoal is very inconsiderable; and, by some experiments of Dr. PRIESTLEY's, charcoal, when freed from fixed air, and other air which it imbibes from the atmosphere, is almost wholly convertible into phlogiston. The cause of this apparent loss of matter, I doubt not, these gentlemen can explain satisfactorily, and very probably in such a manner as will throw other lights on the subject.

* Or it may arise from my being mistaken, in supposing that the same quantity of heat is disengaged by the union of dephlogisticated air with phlogiston, in the form of inflammable air, as is by its union with the phlogiston of phosphorus or, sulphur; and there appears to be some reason why there should not; because in these latter cases the water, being united to the acids, cannot retain so much elementary heat as it can do when left in the form of pure water, which is the case when the inflammable air is used.

It

It is also worthy of enquiry, whether all the amazing quantity of heat let loose in these experiments was contained in the dephlogisticated air; or whether the greatest portion of it was not contained in the phlogiston or inflammable air. If it was all contained in the dephlogisticated air, "*the general rule is not "fact, that elastic fluids are enlarged in their dimensions in propor-"tion to the quantity of heat they contain*;" because then, inflammable air, which is ten times the bulk of dephlogisticated air, must be supposed to contain no heat at all; "and it is known, "from some experiments of my friend Dr. BLACK's, and some "of my own, that the steam of boiling water, whose latent "and sensible heat are only 1100°, reckoning from 60°, or tem-"perate, is more than twice the bulk of an equal weight of "dephlogisticated air." It seems, however, reasonable to suppose, that the greater quantity of heat should be contained in the rarer fluid.

It may be alledged, that in proportion to the quantity of phlogiston that is contained in any fluid, the quantity of heat is lessened. But if we reason by analogy, the attraction of the particles of matter to one another in other cases is increased by phlogiston, and "bodies are thereby rendered specifically "heavier;" and we know of no other substance besides heat which can be supposed to separate the particles of inflammable air, and to endow it with so very great an elastic power, and so small a specific gravity. On the other hand, if a great quantity of elementary heat be allowed to be contained in inflammable air, on account of its bulk, the same reasoning cannot hold good in respect to the phlogiston of phosphorus, sulphur, charcoal, &c. But all these substances contain other matters besides phlogiston and heat. The acids in the sulphur and

and phofphorus, and the alkali and earth in charcoal, may attract the phlogifton fo powerfully that the heat they contain may not be able to overcome the adhefion of their particles, until, by the effect of external heat, they are once removed to fuch a diftance from one another as to be out of the fphere of that kind of attraction *.

If it be found to be a conftant fact, that equal additions of phlogifton to dephlogifticated air do not extricate equal quantities of heat, that may afford the means of finding the quantities of heat contained in phlogifton and dephlogifticated air refpectively, and folve the problem.

Many other ideas on thefe fubjects prefent themfelves; but I am not bold enough to trouble you, or the public, with any fpeculations, but fuch as I think are fupported by uncontroverted facts.

I muft therefore bring this long letter to a conclufion, and leave to others the future profecution of a fubject which, however engaging, my neceffary avocations prevent me from purfuing. I cannot however conclude, without acknowledging my obligations to Dr. PRIESTLEY, who has given me every information and affiftance in his power, in the courfe of my enquiries, with that candour and liberality of fentiment which diftinguifh his character.

I return you my thanks for the obliging attention you have paid to this hypothefis; and remain, with much efteem, &c.

JAMES WATT.

* On the whole, this queftion feems to involve fo many difficulties, that it cannot be cleared up without many new experiments.

XXVI. *Sequel to the Thoughts on the constituent Parts of Water and Dephlogisticated Air. In a subsequent Letter from Mr.* James Watt, *Engineer, to Mr.* De Luc, *F. R. S.*

Read May 6, 1784.

DEAR SIR, Birmingham, April 30, 1784.

ON re-considering the subject of my letter to you of the 26th of November last, I think it necessary to resume the subject, in order to mention some necessary cautions to those who may chuse to repeat the experiments mentioned there, and to point out some circumstances that may cause variations in the results.

In experiments where the dephlogisticated air is to be distilled from common or cubic nitre, these salts should be purified as perfectly as possible, both from other salts and from phlogistic matter of any kind; otherwise they will produce some nitrous air, or yellow fumes, which will lessen the quantity, and, perhaps, debase the quality of the dephlogisticated air. If the nitre is perfectly pure, no yellow fumes are perceptible, until the alkaline part begins to act upon the glass of the retort, and even then they are very slightly yellow.

When earthen retorts are used, and a large quantity of air is drawn from the nitre, it acts very much upon the retort, dissolves a great part of it, and becomes very alkaline, retaining only a small part of its acid, at least only a small part which can be made appear in any of the known forms of that acid; and unlefs retorts can be obtained of a true apyrous and compact porcelain, I fhould prefer glafs retorts, properly coated, for making experiments for the prefent purpofe.

In fome of my experiments the nitre was left in the retort placed in a furnace, fo that it took an hour or more to cool. In thefe cafes there was always a deficience of the acid part, which feemed, from fome appearances on the coating, either to have penetrated the hot and foft glafs, by paffing from particle to particle, or to have efcaped by fmall cracks which happened in the retort during the cooling. There was the leaft deficience of the acid when the diftillation was performed as quickly as was practicable, and the retort was removed from the fire immediately after the operation was finifhed. In order to fhorten the duration of the experiment, and confequently to leffen the action of the nitre on the retort, it is advifable not to diftil above 50 ounce meafures of dephlogifticated air from an ounce of nitre. The experiment has fucceeded beft when the retort was placed in a charcoal fire in a chafing-difh or open furnace; becaufe it is eafy in that cafe to ftop the operation, and to withdraw the retort at the proper period.

When the dephlogifticated air is diftilled from the nitre of mercury, the folution fhould be performed in the retort itfelf, and the nitrous air produced by the folution fhould be caught in a proper receiver, and decompofed by the gradual admiffion of common air through water; and the water, which thus becomes impregnated with the acid of the nitrous air, fhould be added after the procefs to the water through which the dephlogifticated air has paffed. When the folution ceafes to give any more nitrous air, the point of the tube of the retort fhould be raifed out of the water; otherwife, by the condenfation of the watery and acid vapours which follow, a partial exhauftion will take place, and the receiving water will rife up into the retort and break it, or at leaft fpoil the experiment. A common receiver, fuch as is ufed in diftilling fpirit of nitre, fhould be applied, with a little water in it, to receive the acid fteam; and it fhould be kept as cool as can conveniently be done, as thefe fumes are very volatile. This receiver fhould remain as long as the fumes are colourlefs; but when they appear, in the neck of the retort, of a yellow colour, it is a mark that the mercurial nitre will immediately produce dephlogifticated air; the receiver fhould then be withdrawn, and an apparatus placed to receive the air. The reft of the procefs has been fufficiently explained in my former letter.

The phlogifticated nitrous acid, faturated by an alkali, will not cryftallize; and, if expofed to evaporation, even in the heat of the air, will become alkaline again, which fhews the weaknefs of its affinity with alkalies when diffolved in water *; a farther proof of which is, that it is expelled from them by all the acids, even by vinegar (which fact has been obferved by Mr. SCHEELE). I have obferved, that litmus is no teft of the faturation of this acid by alkalies; for the infufion of litmus added to fuch a mixture will turn red, when the liquor appears to be highly alkaline, by its turning the infufions of violets, rofe leaves, and moft other red juices, green. This does not proceed from the infufion of litmus being more fenfible to the prefence of acids than other tefts; for I have lately difcovered a teft liquor (the preparation of which I mean to publifh foon) which is more fenfible to the prefence of acids than

* You have informed me, that Mr. CAVENDISH has alfo obferved this fact; and that he has mentioned it in a paper lately read before the Royal Society; but I had obferved the fact previous to my knowledge of his paper.

than litmus is; but which turns green in the same solution of phlogisticated nitre that turns litmus red.

The unavoidable little accidents which have attended these experiments, and which tend to render their results dubious, have prevented me from relying on them as *full* proofs of the position that no acid enters into the composition of dephlogisticated air; though they give great probability to the supposition. I have, therefore, explained the whole of the hypothesis and experiments with the diffidence which ought to accompany every attempt to account for the phænomena of nature on other principles than those which are commonly received by philosophers in general. And in pursuance of the same motives it is proper to mention, that the alkali employed to saturate the phlogisticated nitrous acid, was always that of tartar which is partly mild; and I have not examined whether highly phlogisticated nitrous acid can perfectly expel fixed air from an alkali, though I know no fact which proves the contrary. It should also be examined, whether the same quantity of real nitrous acid is requisite to saturate a given quantity of alkali, when the acid is phlogisticated, as is necessary when it is dephlogisticated.

As I am informed that you have done me the honour to communicate my former letter on this subject to the Royal Society, I shall be obliged to you to do me the same favour in respect to the present letter, if you judge that it merits it.

I remain, &c.

JAMES WATT.

XXVII. *An Attempt to compare and connect the Thermometer for strong Fire, described in Vol. LXXII. of the Philosophical Transactions, with the common Mercurial Ones. By Mr.* Josiah Wedgwood, *F. R. S. Potter to Her Majesty.*

Read May 13, 1784.

THIS thermometer, which I had the honour of laying before the Royal Society in May 1782, has now been found, from extensive experience, both in my manufactories and experimental enquiries, to answer the expectations I had conceived of it as a measure of all degrees of common fire above ignition: but at present it stands in a detached state, not connected with any other, as it does not begin to take place till the heat is too great to be measured or supported by mercurial ones.

What is now therefore wanting, to give us clear ideas of the value of its degrees, is, to connect it with one which long use has rendered familiar to us; so that if the scale of the common thermometer be continued indefinitely upwards as a standard, the divisions of mine may be reduced to that scale, and we may thus have the whole range of the degrees of heat brought into one uniform series, expressed in one language, and comparable in every part, from the lowest that have hitherto been produced by any artificial freezing mixtures, up to the highest that can be obtained in our furnaces, or that the materials of our furnaces and vessels can support.

The

The hope of attaining this desirable and important object gave rise to the experiments which I have now the honour of communicating. How far I may have succeeded, or whether the means employed were adequate to the end proposed, is, with all deference, submitted to this illustrious Society.

This attempt is founded upon the construction and application of an intermediate measure, which takes in both the heats that are measurable by the mercurial thermometer, and a sufficient number of those that come within the province of mine to connect the two together; the manner of doing which will be apparent from the three first figures (tab. XIV.); wherein F represents FAHRENHEIT'S thermometer, with a continuation of the scale; W my thermometer; and M the intermediate measure divided into any number of equal parts at pleasure.

For if the heat of boiling water, or 212 degrees of FAHRENHEIT, be communicated to M, and its measure upon M marked, as at *a*; and if the heat of boiling mercury, or 600° of FAHRENHEIT, be also communicated to M, and marked as at *b*; it is plain, that the number of degrees upon M between *a* and *b* will be equal to the interval between 212 and 600, that is, to 388° upon FAHRENHEIT.

In like manner, upon exposing M to two different heats above ignition along with my thermometer pieces, if a certain degree of my scale be found to correspond with the point *d*, and another degree of mine with the point *c*; then the interval between those two degrees upon mine must be equal to the interval *dc*; and how many of FAHRENHEIT'S that interval is equivalent to will be known from the preceding comparison. Thus we can find the number of FAHRENHEIT'S degrees contained in any given extent of mine, and the degree of FAHRENHEIT'S with which a given point of mine coincides; whence

whence either ſcale is eaſily reducible to the other through their whole range, whether we ſuppoſe FAHRENHEIT's continued upwards, or mine downwards.

For obtaining the intermediate thermometer different means were thought of; but the only principle which, upon attentive conſideration, afforded any proſpect of ſucceſs, was the *expanſion of metals*. This therefore was adopted, and among different methods of meaſuring that expanſion, which either occurred to myſelf, or which I can find to have been practiſed by others, there is no one which promiſes either ſo great accuracy, or convenience in uſe, as a gage like that by which the thermometer pieces are meaſured: the utility of this gage had now been confirmed to me by experience, and the machines and long rods, which have been employed for meaſuring expanſions on other occaſions, were abſolutely inadmiſſible here, on account of the inſuperable difficulties of performing nice operations of this kind in a red heat, and of communicating a perfectly equal heat through any conſiderable extent.

To give a clearer idea of this ſpecies of gage, which, ſimple as it is, I am informed has been miſunderſtood by ſome of the readers of my former paper, a repreſentation of one uſed on the preſent occaſion is annexed in fig. 4. where ABCD is a ſmooth flat plate; and EF and GH two rulers or flat pieces, a quarter of an inch thick, fixed flat upon the plate, with the ſides that are towards one another made perfectly true, a little further aſunder at one end EG than at the other end FH; thus they include between them a long converging canal, which is divided on one ſide into a number of ſmall equal parts, and which may be conſidered as performing the offices both of the tube and ſcale of the common thermometer. It is obvious, that if a body, ſo adjuſted as to fit exactly at the wider end of this

this canal, be afterwards diminiſhed in its bulk by fire, as the thermometer pieces are, it will then paſs further in the canal, and more and more ſo according as the diminution is greater; and converſely, that if a body, ſo adjuſted as to paſs on to the narrow end, be afterwards expanded by fire, as is the caſe with metals, and applied in that expanded ſtate to the ſcale, it will not paſs ſo far; and that the diviſions on the ſide will be the meaſures of the expanſions of the one, as of the contractions of the other, reckoning in both caſes from that point to which the body was adjuſted at firſt.

I is the body whoſe alteration of bulk is thus to be meaſured, which, in the preſent inſtance, is a piece of fine ſilver: this is to be gently puſhed or ſlid along, towards the end FH, till it is ſtopped by the converging ſides of the canal.

K is a little veſſel formed in the gage for this particular ſeries of experiments, the uſe of which will appear hereafter.

The *contraction*, which the thermometer pieces receive from fire, is a permanent effect, not variable by an abatement of the heat, and which accordingly is meaſured commodiouſly and at leiſure, when the pieces are grown cold. But the *expanſion* of bodies is only temporary, continuing no longer than the heat does that produced it; and therefore its quantity, at any particular degree of heat, muſt be meaſured in the moment while that heat ſubſiſts. And further, if the heated piece was applied to the cold gage, the piece would be deprived of a part of its heat on the firſt contact; and as the gage receives ſome degree of expanſion from heat as well as the piece, it is plain that in this caſe the piece would be diminiſhed in its bulk, and the gage enlarged, before the meaſurement could be taken. It is therefore neceſſary that both of them be heated to an exact equality; and in that ſtate we can meaſure, not indeed the *true expanſion of* *either*, but the *excess* of the expansion of one above that of the other, which is sufficient for the present purpose, as we want only an uniform and graduated effect of fire, and it is totally immaterial whether that effect be the absolute expansion of one or the other body, or the difference of the two, provided only that its *quantity* be sufficient to admit of nice measurement.

Some difficulties occurred with respect to the choice of a proper *matter* for the gage; the essential requisites of which are, to have but little expansibility, and to bear the necessary fires without injury. All the metals, except gold and silver, would calcine in the fire: those two are indeed free from that objection, and accordingly it is of the most expansible of them that the piece is made; but if the gage also was made of the same, the measure itself would expand just as much as the body to be measured, and no expansion at all would be sensible; and though the gage was made of one of those metals, and the piece of the other, the difference between their expansions would be too small to give any satisfactory results, as more than two-thirds of the real expansion of either would be lost or taken off by the other.

For these reasons I had recourse to earthy compositions, which expand by heat much less than metallic bodies, and bear the necessary degrees of fire without the least injury. I made choice of tobacco-pipe clay, mixed with charcoal in fine powder, in the proportion of three parts of the charcoal to five of the clay by weight. By a free access of air, in the burning by which the gage is prepared for use, the charcoal is consumed, and leaves the clay extremely light and porous; from which circumstance it bears sudden alternations of cold and heat, often requisite in these operations, much better than the clay alone. Another and more important motive for the use of charcoal

was,

was, that in consequence of the remarkable porosity which it produces in the clay, it would probably diminish the expansibility, by occasioning the mass to contain, under an equal surface, a much less quantity of solid or expansible matter. It may be objected to this idea, that the expansions of metals, in Mr. ELLICOTT's * and Mr. SMEATON's † experiments, do not appear to have any connection at all with their densities: but the cases are by no means parallel; for there the comparison lies between different species of matter; but here, between one and the same matter in different states of compactness. If a metal could be treated as clay is in this instance, that is, if a large bulk of any foreign matter could be blended with it, and this matter afterwards burnt out, so as to leave the metallic particles at the same distances to which they had been separated by the mixture of it, we may presume that the metal thus enlarged would not expand so much as an equal volume of the solid metal. Such at least were the ideas which determined my choice to a composition of clay and charcoal powder; and being afterwards desirous of satisfying myself whether they had any foundation in fact, I have, since the experiments were made, prepared some pieces of clay with and without charcoal, and having burnt them in the same fire, I ground them at the sides, to make them both fit exactly to the same division near the narrow end of the gage; then, examining their expansions by equal heats, I found the piece with charcoal to expand only one-third part so much as that without; and thus was fully satisfied with the composition of the gage.

To ascertain a fixed point on the scale for the divisions to be counted from, the silver piece and gage were laid together for

* Phil. Transact. vol. XLVII. p. 485.

† Ibid. vol. XLVIII. p. 612.

some time in spring water, of the temperature of 50° of FAHRENHEIT: the point which the piece went to in this cold state is that marked o near the narrow end of the gage. The adjustment is re-examined at the beginning and end of every succeeding experiment, lest the repeated attrition, in sliding the piece backwards and forwards, should wear off so much from the surface of this soft metal as to occasion an error in the minute quantities here measured.

The apparatus is then exposed successively to different degrees of heat, with the piece lying always in a part of the canal at least as wide as it is expected to fill when expanded, otherwise the sides of the gage would be burst asunder by its expansion, as I experienced in some of my first trials. When the whole has received any particular degree of heat desired, the piece is cautiously and equably pushed along, till it is stopped by the convergency of the sides, of which I always find notice given me by the gage itself (which is small and light) beginning to move upon the continuance of the impulse. A flat slip of iron, a little narrower than the piece, bent down to a right-angle at one end, and fixed in a long handle at the other, makes a convenient instrument for pushing the piece forward, or drawing it back again, whilst red-hot: this instrument, at every time of using, is heated to the same degree as the piece itself.

The heat of boiling water is taken without difficulty, by keeping the apparatus in boiling water itself during a sufficient space of time for the full heat to be communicated to it. The water I made use of was a very fine spring water, which on chemical trials appeared very nearly equal in purity to that of rain or snow; and I had previously satisfied myself, by trials in the cold, that the gage and piece being wet, or under water, made no difference in the measurement. The expansion of the

3 silver

silver by this heat, that is, by an increase of the heat from 50° to 212°, or a period containing 162° of FAHRENHEIT, was just 8° of the gage or intermediate thermometer M; whence one of these degrees, according to this experiment, contains just 20°¼ of FAHRENHEIT's. The operation was many times repeated, and the result was always precisely the same.

For the boiling heat of mercury, it was necessary to proceed in a different manner; not to convey the heat from the mercury to the instrument, but to convey it equally to them both from another body. I made a small vessel for holding the mercury in the gage itself, seen at K fig. 4. and more distinctly in fig. 5. which is a transverse section of the gage through this vessel. The plate CD, which forms the bottom of the canal, serves also for the bottom of the vessel, which is situated close to the side of the canal, and as near as could be to that part of it, in which both the silver piece, and the divisions required for this particular experiment, are contained. By this arrangement it is presumed, that all the parts concerned in the operation will receive very nearly an equal heat.

The gage, with some mercury in the vessel, was laid upon a smooth and level bed of sand, on the bottom of an iron muffle kept open at one end; the fire increased very gradually till the mercury boiled, and then continued steady, so as just to keep it boiling, for a considerable time. The boiling heat of mercury was thus found to be 27°½ of the intermediate thermometer, which answering to an interval of 550° of FAHRENHEIT, makes one degree of this equal to just 20° of his; a result corresponding even beyond my expectations with that which boiling water had given.

These standard heats of FAHRENHEIT's thermometer are obtained with little difficulty on a common fire; but it is far otherwise

otherwise with the higher ones in which mine begins to apply; and all the precautions I could take, by using a close muffle, surrounding it as equally as possible with the fuel, varying its position with respect to the draught of air, &c. proved insufficient for securing the necessary equality of heat even through the small space concerned in these experiments. Nor had I any idea, before the discovery of this thermometer, of the extreme difficulty, not to say impracticability, of obtaining, in common fires, or in common furnaces, an uniform heat through the extent even of a few inches. Incredible as this may appear at first sight, whoever will follow me in the operations I have gone through, placing accurate measures of the heat in different parts of one and the same vessel, will soon be convinced of its truth, and that he can no otherwise expect to communicate with certainty an equal heat to different pieces, than by using a fire of such magnitude as to exceed perhaps some hundreds of times the bulk of the matters required to be heated.

To such large body of fire, therefore, after many fruitless attempts in small furnaces, not a little discouraging by the irregularity of their results, I at length had recourse, fitting up for this purpose an iron oven, used for the burning-on of enamel colours upon earthen ware, about four feet long, by two and a half wide, and three feet high, which is heated by the flame of wood conducted all round it. An iron muffle, four inches wide, two inches and three quarters high, and ten inches long, containing the gage and piece, was placed in the middle of this oven, and the vacancy between them filled up with earthen ware, to increase the quantity of ignited matter, and thereby communicate the heat more equably from the oven to the muffle. In such a situation of the muffle, in the center

center of an oven more than five hundred times its own capacity, it could not well fail of being heated pretty uniformly, at least through the small space which these experiments required; nor have I found any reason to suspect that it was not so.

The gage being laid flat upon the bottom of the muffle, with the silver piece in the canal as before, some of the clay thermometer pieces were set on end upon the silver piece, with that end of each downwards which is marked to go foremost in measuring it; that is, they were in contact with the silver in that part of their surface by which their measure is afterwards ascertained. I was led to this precaution by an experiment I had made upon another occasion, in which a number of thermometer pieces having been set upright upon an earthen-ware plate, over a small fire, till the plate became red-hot, all the pieces were found diminished, some of them more than two degrees, at the lower ends which rested upon the plate, whilst the upper ends were as much enlarged, not having yet passed the stage of extension which, as observed in the former paper, always precedes the thermometric diminution: thus we see how punctually every part of the piece obeys the heat that acts upon it.

The fire about the oven was slowly increased for some hours, and kept as even and steady as possible, by an experienced fireman, under my own inspection. Upon opening a small door, which had been made for introducing the apparatus, and looking in from time to time, it was observed, that the muffle, with the adjacent parts of the oven and ware, acquired a visible redness at the same time; and in the progress of the operation, the eye could not distinguish the least dissimilarity in the aspect of the different parts; whereas in small fires, the difference not only between the two ends of the muffle, but in much less distances, is such as to strike the eye at once.

When

When the muffle appeared of a low red heat, such as was judged to come fully within the province of my thermometer, it was drawn forward, towards the door of the oven; and its own door being then nimbly opened by an assistant, I immediately pushed the silver piece as far as it would go. But as the division which it went to could not be distinguished in that ignited state, the muffle was lifted out, by means of an iron rod passed through two rings made for that purpose, with care to keep it steady, and avoid any shake that might endanger the displacing of the silver piece.

When grown sufficiently cold to be examined, I noted the degree of expansion which the silver piece stood at, and the degree of heat shewn by the thermometer pieces measured in their own gage; then returned the whole into the oven as before, and repeated the operation with a stronger heat, to obtain another point of correspondence on the two scales.

The first was at 2°¼ of my thermometer, which coincided with 66° of the intermediate one; and as each of these last has been before found to contain 20 of FAHRENHEIT's, the 66 will contain 1320; to which add 50, the degree of his scale to which the 0 of the intermediate thermometer was adjusted, and the sum, 1370, will be the degree of FAHRENHEIT's corresponding to my 2°¼.

The second point of coincidence was at 6°¼ of mine, and 92° of the intermediate; which 92 being, according to the above proportion, equivalent to 1840 of FAHRENHEIT, add 50 as before to this number, and my 6°¼ is found to fall upon the 1890th degree of FAHRENHEIT.

It appears from hence, that an interval of 4 degrees upon mine is equivalent to an interval of 520° upon his; consequently 1 of mine to 130 degrees of his; and that the 0 of mine corresponds

to

to his 1677°½. Several other trials were made, which gave results so nearly alike, that I have little apprehension of any material error.

From these data it is easy to reduce either scale to the other through their whole range; and from such reduction it will appear, that an interval of near 480° remains between them, which the intermediate thermometer serves as a measure for; that mine includes an extent of about 32000 of FAHRENHEIT's degrees, or about 54 times as much as that between the freezing and boiling points of mercury, by which mercurial ones are naturally limited; that if the scale of mine be produced downwards, in the same manner as we have supposed FAHRENHEIT's to be produced upwards, for an ideal standard, the freezing point of water would fall nearly on 8° below 0 of mine, and the freezing point of mercury a little below 8°½; and that, therefore, of the extent of now measurable heat, there are about $\frac{5}{10}$ths of a degree of my scale from the freezing of mercury to the freezing of water; 8° from the freezing of water to full ignition; and 160° above this to the highest degree I have hitherto attained.

As we are now enabled to compare not only the higher degrees among themselves, and the lower among themselves, upon their respective scales, but likewise the higher and lower with each other in every stage, it may be proper to take a general view of the whole range of measurable heat, as expressed both in FAHRENHEIT's denominations and in mine; and for this purpose I have drawn up a little table of a few of the principal points that have been ascertained, to shew their mutual relations or proportions to each other: any other points that have been, or hereafter may be, observed, by these or any other known thermometers, may be inserted at pleasure.

 Extremity

	FAHR.	WEDG.
Extremity of the scale of my thermometer	32277°	240°
Greatest heat of my small air-furnace -	21877	160
Cast iron melts - - - -	17977	130
Greatest heat of a common smith's forge	17327	125
Welding heat of iron, greatest -	13427	95
———————— least - -	12777	90
Fine gold melts - - -	5237	32
Fine silver melts - - -	4717	28
Swedish copper melts - -	4587	27
Brass melts - - -	3807	21
Heat by which my enamel colours are burnt on	1857	6
Red-heat fully visible in day-light -	1077	0
Red-heat fully visible in the dark -	947	1
Mercury boils - - -	600	$3\frac{673}{1000}$
Water boils - - -	212	$6\frac{658}{1000}$
Vital heat - - -	97	$7\frac{542}{1000}$
Water freezes - - -	32	$8\frac{42}{1000}$
Proof spirit freezes - - -	0	$8\frac{218}{1000}$
The point at which mercury congeals, consequently the limit of mercurial thermometers, -	about 40	$8\frac{506}{1000}$

To assist our conceptions of this subject, it may be proper to view it in another light, and endeavour to present it to the eye; for *numbers*, on a high scale, are with difficulty estimated and compared by the mind. I have therefore completed the scales of which a part is represented in fig. 1. and 3. by continuing the same equal divisions, both upwards and downwards, as far as the utmost limits of heat that have hitherto been attained and measured *.

* Mr. WEDGWOOD presented this, in the form of a very long roll, to the Society.

In

In a ſcale of heat drawn up in this manner, the comparative extents of the different departments of this grand and univerſal agent are rendered conſpicuous at a ſingle glance of the eye. We ſee at once, for inſtance, how ſmall a portion of it is concerned in animal and vegetable life, and in the ordinary operations of nature. From freezing to vital heat is barely a five-hundredth part of the ſcale; a quantity ſo inconſiderable, relatively to the whole, that in the higher ſtages of ignition, ten times as much might be added or taken away, without the leaſt difference being diſcernible in any of the appearances from which the intenſity of fire has hitherto been judged of. From hence, at the ſame time, we may be convinced of the utility and importance of a phyſical meaſure for theſe higher degrees of heat, and the utter inſufficiency of the common means of diſcriminating and eſtimating their force. I have too often found differences, aſtoniſhing when conſidered as a part of this ſcale, in the heats of my own kilns and ovens, without being perceivable by the workmen at the time, or till the ware was taken out of the kiln.

SINCE the foregoing experiments were made, I have ſeen a very curious Memoir by Meſſ. LAVOISIER and DE LA PLACE, containing a method of meaſuring heat by the quantity of ice which the heated body is capable of liquefying. The application of this important diſcovery, as an intermediate ſtandard meaſure between FAHRENHEIT's thermometer and mine, could not eſcape me, and I immediately ſet about preparing an apparatus, and making the experiments neceſſary for that pur-

 poſe;

pofe; in hopes either of attaining by this method a greater degree of accuracy than I could expect from any other means, or of having what I had already done confirmed by a feries of experiments upon a different principle.

But in the profecution of thefe experiments I have, to my great mortification, hitherto failed of fuccefs; and I fhould have contented myfelf for the prefent with faying little more than this, if fome phænomena had not occurred, which appear to me not unworthy of farther inveftigation.

The authors obferve, that if ice, cooled to whatever degree below the freezing point, be expofed to a warmer atmofphere, it will be brought up to the freezing point through its whole mafs before any part of its furface begins to liquefy; and that confequently ice, beginning to melt on the furface, will be always exactly of the fame temperature, *viz.* at the freezing point; and that if a heated body be inclofed in a hollow fphere of fuch ice, the whole of its heat will be taken up in liquefying the ice; fo that if the ice be defended from external warmth, by furrounding it with other ice in a feparate veffel, the weight of the water produced from it will be exactly proportional to the heat which the heated body has loft; or, in other words, will be a true phyfical meafure of the heat.

For applying thefe principles in practice, they employ a tin veffel, divided, by upright concentric partitions, into three compartments, one within another. The innermoft compartment is a wire cage, for receiving the heated body. The fecond, furrounding this cage, is filled with pounded ice, to be melted by the heat; and the outermoft is filled alfo with pounded ice, to defend the former from the warmth of the atmofphere. The firft of thefe ice compartments terminates at bottom in a ftem like a funnel, through which the water is conveyed off; and the other ice compartment terminates in a feparate canal, for discharging the water into which *that* ice is reduced. As soon as the heated body is dropped into the cage, a cover is put on, which goes over both that and the first ice compartment; which cover is itself a kind of shallow vessel, filled with pounded ice, with holes in the bottom for permitting the water from this ice to pass into the second compartment; all the liquefaction that happens here, as well as there, being the effect of the heated body only. Over the whole is placed another cover with pounded ice, as a defence from external warmth.

As soon as this discovery came to my knowledge, on the 23d of February, a thaw having begun three days before, after a frost which had continued with very little intermission from the 24th of December, I collected a quantity of ice, and stored it up in a large cask in a cellar.

I thought it necessary to satisfy myself in the first place, by actual experiment, that ice, how cold soever it may be, comes up to the freezing point through its whole mass before it begins to liquefy on the surface. For this purpose I cooled a large fragment of ice, by a freezing mixture, to 17° of FAHRENHEIT's thermometer, and then hung it up in a room whose temperature was 50°. When it began to drop, it was broken, and some of the internal part nimbly pounded and applied to the bulb of a thermometer that was cooled by a freezing mixture below 30°. The thermometer rose to, and continued at, 32°; being then taken out, and raised by warmth to 40°, some more of the same ice, applied as before to the bulb, sunk it again to 32°; so that no doubt could remain on this subject.

Apprehensive that pounded ice, directed by the authors, might imbibe and retain more or less of the water by capillary attraction, according to circumstances, and thereby occasion some error in the results, I thought it necessary to satisfy myself in this respect also.

alſo by experiment. I therefore pounded ſome ice, and laid it in a conical heap on a plate; and having at hand ſome water, coloured with cochineal, I poured it gently into the plate, at ſome diſtance from the heap: as ſoon as it came in contact with the ice, it roſe haſtily up to the top; and on lifting up the lump, I found that it held the water, ſo taken up, as a ſponge does, and did not drop any part of it till the heat of my hand, as I ſuppoſe, began to liquefy the maſs. On further trials I found, that in pounded ice preſſed into a conical heap, the coloured water roſe, in the ſpace of three minutes, to the height of two inches and a half; and by weighing the water employed, and what remained upon the plate unabſorbed, it appeared, that four ounces of ice had thus taken up, and retained, one ounce of water.

To further aſcertain this abſorbing power, in different circumſtances, more analogous to thoſe of the proceſs itſelf, I preſſed ſix ounces of pounded ice pretty hard into the funnel, having firſt introduced a wooden core in order to leave a proper cavity in the middle: then, taking out the core, and pouring an ounce of water upon the ice, I left the whole for half an hour; at the end of which time the quantity that ran off was only 12 pennyweights and 4 grains, ſo that the ice had retained 7 pennyweights and 20 grains, which is nearly one-twelfth of its own weight, and two-fifths of the weight of the water.

Theſe previous trials determined me, inſtead of uſing pounded ice, to fill a proper veſſel with a ſolid maſs of ice, by means of a freezing mixture, as the froſt was now gone, and then expoſe it to the atmoſphere till the ſurface began to liquefy. The apparatus I fitted up for this purpoſe was made of earthen ware well glazed, and is repreſented in fig. 6. (tab. XV.).

A, is

A, is a large funnel, filled with a solid mass of ice. B, a cavity in the middle of this ice, formed, part of the way, by scraping with a knife, and for the remaining part, by boring with a hot iron wire. C, one of my thermometer pieces, which serves for the heated body, and rests upon a coil of brass wire: it had previously been burnt with strong fire, that there might be no danger of its suffering any further diminution of its bulk by being heated again for these experiments. D, a cork stopper in the orifice of the funnel. E, the exterior vessel, having the space between its sides and the included funnel A, filled with pounded ice, as a defence to the ice in the funnel. F, a cover for this exterior vessel, filled with pounded ice for the same purpose. G, a cover for the funnel, filled also with pounded ice, with perforations in the bottom for allowing the water from this ice to pass down into the funnel.

The thermometer piece was heated in boiling water, taken up with a pair of small tongs equally heated, dropped instantly into the cavity B, and the covers put on as expeditiously as possible; the bottom of the funnel being previously corked, that the water might be detained till it should part with all its heat, and likewise to prevent the water from the other ice, which ran down on the outside of the funnel, from mingling with it.

After standing about ten minutes, the funnel was taken out, wiped dry, and uncorked over a weighed cup: the water that ran out weighed 22 grains. Thinking this quantity too small, as the piece weighed 72 grains, I repeated the experiment, and kept the piece longer in the funnel; but the water this time weighed only 12 grains. Being much dissatisfied with this result, I made a third trial, continuing the piece much longer in the cavity; but the quantity of water was now still less, not amounting to quite three drops; and, to my great surprise, I found the piece frozen to the ice, so as not to be easily got off, though all the ice employed was, at the beginning of the experiment, in a thawing state.

I had prepared the apparatus for taking the boiling heat of mercury; but being entirely discouraged by these very unequal results, I gave that up, for the present at least, and heating the piece to 6° of my thermometer, turned it nimbly out of the case in which it was heated into the cavity, throwing some fragments of ice over it. In about half an hour, I drew off the water, which amounted to 11 pennyweights; then stopping the funnel again, and replacing the covers, I left the whole about seven hours.

At the end of that time, I found a considerable quantity of water in the funnel: the melting of the ice had produced a cavity between it and the sides, great part of the way down, which, as well as that in the middle, was nearly full. The water nevertheless ran out so slowly, that I apprehended something had stopped the narrow end of the funnel, but the true cause became afterwards apparent upon examining the state of the ice. The fragments which I had thrown over the thermometer piece were frozen entirely together, and in such a form as they could not have assumed without fresh water superadded and frozen upon them, for the cavities between them were partly filled with new ice. I endeavoured to take the ice out with my fingers, but in vain; and it was with some difficulty I could force it asunder even with a pointed knife, to get at the thermometer piece. When that was got out, great part of the coiled wire was found enveloped in new ice. The passage through the ice to the stem of the funnel, which I had made pretty wide with a thick iron wire red-hot, was so nearly closed

up,

up, that the slow draining off of the water was now sufficiently accounted for, and indeed this draining was the only apparent mark of any passage at all. On taking the ice out of the funnel, and breaking it to examine this canal, I found it almost entirely filled up with ice projecting from the solid mass in crystalline forms, similar in appearance to the crystals we often meet with in the cavities of flints and quarzose stones.

If, after all these circumstances, any doubt could have remained of the ice in question being a new production, a fact which I now observed must have removed all suspicion. I found a coating of ice, of considerable extent and perfectly transparent, about a tenth of an inch in thickness, upon the outside of the funnel, and on a part of it which was not in contact with the surrounding ice, for that was melted to the distance of an inch from it.

Some of the ice being scraped off from the inside of the funnel, and applied to the bulb of the thermometer, the mercury sunk from 50° to 32°, and continued at that point till the ice was melted; after which, the water being poured off, it rose in a little time to 47°.

Astonished at these appearances, of the water freezing after it had been melted, though surrounded with ice in a melting state, and in an atmosphere about 50°, where no part of the apparatus or materials could be supposed to be lower than the freezing point, I suspected at first that some of the salt of the freezing mixture might have got into the water, and that this, in dissolving, might perhaps absorb, from the parts contiguous to it, a greater proportion of heat than the ice of pure water does. But the water betrayed nothing saline to the taste, and I had applied the freezing mixture with my own hands with great care, to prevent any of it being mixed with the water.

 To

To remove all doubts, however, upon this point, I purposed repeating the experiment with some pieces of the ice I had stored up in the cellar, to see if this would congeal, after thawing, in the same manner. But going to fetch the ice, and examining it in the cask in which it was kept, I was perfectly satisfied with the appearances I found there; for though much of it was melted, yet the fragments were frozen together, so that it was with difficulty I could break or get out any pieces of it with an iron spade; and, when so broken, it had the appearance of *breccia* marble or plum-pudding stone, for the fragments had been broken and rammed into the cask with an iron mall.

A porcelain cup being laid upon some of this ice about half an hour, in a room whose temperature was 50°, it was found pretty firmly adhering, and when pulled off, the ice exhibited an exact impression of the fluted part of the cup which it had been in contact with; so that the ice must necessarily have liquefied first, and afterwards congealed again. This was repeated several times, with the same event. Fragments of the ice were likewise applied to one another, to sponges, to pieces of flannel and of linen cloth, both moist and dry: all these, in a few seconds, began to cohere, and in about a minute were frozen so as to require some force to separate them. After standing an hour, the cohesion was so firm, that on pulling away the fragments of ice from the woollen and sponge, they tore off with them that part of the surface which they were in contact with, though at the same time both the sponge and flannel were filled with water which that very ice had produced.

To make some estimate of the force of the congelation, which was stronger on the two bodies last mentioned than on linen, I applied a piece of ice to a piece of dry flannel which weighed two pennyweights and a half, and ſurrounded them with other ice. After lying together three quarters of an hour, taking the piece of ice in my hand and hooking the flannel to a ſcale, I found a weight of five ounces to be neceſſary for pulling it off, and yet ſo much of the ice had liquefied as to increaſe the weight of the flannel above 12 pennyweights. I then weighed the piece of ice, put them together again, and four hours after found them frozen ſo firmly as to require 78 ounces for their ſeparation, although, from 42 pennyweights of the ice, 15 more had melted off: the ſurface of contact was at this time nearly a ſquare inch. I continued them again together for ſeven hours; but they now bore only 62 ounces, the ice being diminiſhed to 14 pennyweights, and the ſurface of contact reduced to about ſix-tenths of a ſquare inch.

Having ſeen before that pounded ice abſorbs water in very conſiderable quantity, I ſuſpected that ſomething of the ſame kind might take place even with entire maſſes; and experiment ſoon convinced me, that even apparently ſolid pieces of ice will imbibe water, ſlower or quicker according to its ſtage of decay. I have repeatedly heated ſome of my thermometer pieces, and laid them upon ice, in which they made cavities of conſiderable depth, but the water was always abſorbed, ſometimes as faſt as it was produced, leaving both the piece and the cavity dry.

Thus, though I cannot ſufficiently expreſs how much I admire the diſcovery that gave riſe to theſe experiments, I have nevertheleſs to lament my not being able to avail myſelf of it at preſent for the purpoſe I wiſhed to apply it to.

That in my experiments the two ſeemingly oppoſite proceſſes of nature, congelation and liquefaction, went on together, at the ſame inſtant, in the ſame veſſel, and even in the ſame fragment of ice, is a fact of which I have the fulleſt evidence that my ſenſes can give me; and I ſhall take the liberty of ſuggeſting a few hints, which may tend perhaps to elucidate their cauſe, and to ſhew that they are not ſo incompatible as at firſt ſight they appear to be.

It occurred to me at firſt, that water highly attenuated and divided, as when reduced into vapour, may freeze with a leſs degree of cold than water in its aggregate or groſſer form; hence hoar-froſt is obſerved upon graſs, trees, &c. at times when there is no appearance of ice upon water, and when the thermometer is above the freezing point*. BOERHAAVE, I find, in his elaborate theory of fire, aſſigns 33° as the freezing point of vapour, and even of water when divided only by being imbibed in a linen cloth.

* I am aware, that experiments and obſervations of this kind are not fully deciſive; that the atmoſphere may, in certain circumſtances, be much warmer or colder than the earth and waters, which, in virtue of their denſity, are far more retentive of the temperature they have once received, and leſs ſuſceptible of tranſient impreſſions; that even inſenſible undulations of water, from the ſlighteſt motion of the air, by bringing up warmer ſurfaces from below, may prove a further impediment to the freezing; and, therefore, that the degree of cold, which is ſufficient to produce hoar-froſt, may poſſibly, if continued long enough, be ſufficient alſo to produce ice. I am not acquainted with any ſatisfactory experiments or obſervations yet made upon the ſubject; nor do I advance the principle as a certain, but as a probable one, which occurred to me at the moment, which is countenanced by general obſervation, and conſentaneous to many known facts; for there are numerous inſtances of bodies, in an extreme ſtate of diviſion, yielding eaſily to chemical agents which, before ſuch diviſion, they entirely reſiſt: thus ſome precipitates, in the very ſubtile ſtate in which they are at firſt extricated from their diſſolvents, are re-diſſolved by other menſtrua, which, after their concretion into ſenſible moleculæ, have no action upon them at all.

Now,

Now, as the atmoſphere abounds with watery vapour, or water diſſolved and chemically combined, and muſt be particularly loaded with it in the neighbourhood of melting ice; as the heated body introduced into the funnel muſt neceſſarily convert a portion of the ice or water there into vapour; and as ice is known to melt as ſoon as the heat begins to exceed 32°, or nearly one degree lower than the freezing point of vapour; I think we may from hence deduce, pretty ſatisfactorily, all the phænomena I have obſerved. For it naturally follows from theſe principles, that vapour may freeze where ice is melting; that the vapour may congeal even upon the ſurface of the melting ice itſelf; and that the heat which (agreeably to the ingenious theory of Dr. BLACK) it emits in freezing, may contribute to the further liquefaction of that very ice upon which the new congelation is formed.

I would further obſerve, that the freezing of water is attended with plentiful evaporation in a cloſe as well as an open veſſel, the vapour in the former condenſing into drops on the under ſide of the cover, which either continue in the form of water, or aſſume that of ice or a kind of ſnow, according to circumſtances*; which evaporation may perhaps be attributed to the heat that was combined with the water, at this moment rapidly making its eſcape, and carrying part of the aqueous fluid off with it. We are hence furniſhed with a freſh and continual ſource of vapour as well as of heat; ſo that the proceſſes of liquefaction and congelation may go on uninterruptedly together, and even neceſſarily accompany one another, although, as the freezing muſt be in an under proportion to the melting, the whole of the ice muſt ultimately be conſumed.

* See Mr. BARON's paper on this ſubject, in the Memoires of the Academy of Sciences at Paris for the year 1753.

In the remarkable inftance of the coating of ice on the outfide of the throat of the funnel, there are fome other circumftances which it may be proper to take notice of. Neither the cover of the outer veffel, nor the aperture in its bottom which the ftem of the funnel paffed through, were air-tight, and the melting of the furrounding ice had left a vacancy of about an inch round that part of the funnel on which the cruft had formed. As there was, therefore, a paffage for air through the veffel, a circulation of it would probably take place: the cold and denfe air in the veffel would defcend into the rarer air of the room then about 50°, and be replaced by air from above. The effect of this circulation and fudden refrigeration of the air will be a condenfation of part of the moifture it contains upon the bodies it is in contact with: the throat of the funnel, being one of thofe bodies, muft receive its fhare; and the degree of cold in which the ice thaws being fuppofed fufficient for the freezing of this moift vapour, the contact, condenfation, and freezing, may happen at the fame inftant.

The fame principles apply to every inftance of congelation that took place in thefe experiments; and a recollection of particulars which paffed under my own eye convinces me, that the congelation was ftrongeft in thofe circumftances where vapour was moft abundant, and on thofe bodies which, from their natural or mechanic ftructure, were capacious of the greateft quantity of it; ftronger, for inftance, on fponge than on woollen, ftronger on this than on the clofer texture of linen, and far ftronger on all thefe than on the compact furface of porcelain.

If, neverthelefs, the principle I have affumed (that water highly attenuated will congeal with a lefs degree of cold than water in the mafs) fhould not be admitted; another has above

been

been hinted at, which experiments have decidedly established, from which the phænomena may perhaps be equally accounted for; and which, even though the other also is received, must be supposed to concur for some part of the effect; I mean, that *evaporation produces cold*; both vapour and steam carrying off some proportion of heat from the body which produces them. If, therefore, evaporation be made to take place upon the surface of ice, the contiguous ice will thereby be rendered colder; and as it is already at the freezing point, the smallest increase of cold will be sufficient for fresh congelation. It seems to be on this principle that the formation of ice is effected in the East Indies, by exposing water to a serene air, at the coldest season of the year, in shallow porous earthen vessels: part of the water transudes through the vessel, and evaporating from the outside, the remainder in the vessel becomes cold enough to freeze; the warmth of the earth being at the same time intercepted by the vessels being placed upon bodies little disposed to conduct heat *. If ice is thus producible in a climate where natural ice is never seen, we need not wonder that congelation should take place where the same principle operates amidst actual ice.

It has been observed above, that the heat emitted by the congealing vapour probably unites with and liquefies contiguous portions of ice; but whether the whole, either of the heat so emitted, or of that originally introduced into the funnel, is thus taken up; how often it may unite with other portions of ice, and be driven out from other new congelations; whether there exists any difference in its chemical affinity or

* See a description of this process in the Philosophical Transactions, vol. LXV. p. 253.

elective

elective attraction to water in different ſtates and the contiguous bodies; whether part of it may not ultimately eſcape, without performing the office expected from it upon the ice; and to what diſtance from the evaporating ſurface the refrigerating effect of the evaporation may extend; muſt be left for further experiments to determine.

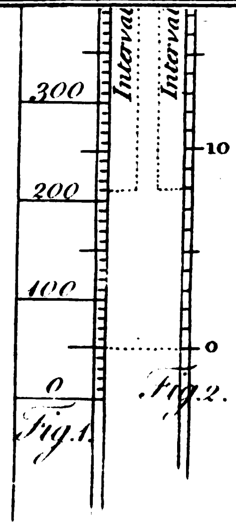


Fig. 1. Fig. 2.

Basire S.

XXVIII. *On the Summation of Series, whose general Term is a determinate Function of* z *the Distance from the first Term of the Series. By* Edward Waring, *M. D.* Lucasian *Professor of the Mathematics at* Cambridge, *and Fellow of the Societies of* London *and* Bononia.

Read May 20, 1784.

PROBLEM.

THE sum S *being given, to find a series of which it is the sum.*

1. Reduce the sum S into a converging series, proceeding according to the dimensions of any small quantities, and it is done. For example: let any algebraical function S of an unknown or small quantity x be assumed, reduce it into a converging series proceeding according to the dimensions of x, and there results a series whose sum is S. 2. Let A, B, C, &c. be algebraical functions of x; reduce the $\int A\dot{x}, \int B\dot{x}, \int C\dot{x}$, &c. into a converging series, proceeding according to the dimensions of x, and the problem is done.

It is always necessary to find the values of x, between which the abovementioned serieses converge. Reduce the algebraical function S in the first example, and the algebraical functions A, B, C, &c. in the second into their lowest terms; and in such a manner, that the quantities contained in the numerator and denominator may have no denominator: make the deno-

 minator

minator in the first example, and the denominator in the second, and every distinct irrational quantity contained in them respectively $=0$; and also every distinct irrational quantity contained in the numerators $=0$. Suppose α the least root affirmative or negative (but not $=0$) of the abovementioned resulting equations; then a series ascending according to the dimensions of x will always converge, if the value of x is contained between α and $-\alpha$; but if x be greater than α or $-\alpha$, the abovementioned series will diverge. Let π be the greatest root of the abovementioned resulting equations; then a series descending according to the reciprocal dimensions of x will converge, if x be greater than $\pm\pi$; but, if less, not. When impossible roots $a \pm b\sqrt{-1}$ are contained in the equations, an ascending series will converge, if x be less than the least root $\pm\alpha$, and $\pm(a-b)$ and $\pm(a+b)$; or more generally, if x be less than the least root $\pm\alpha$, and x^{n+1} at an infinite distance n, be infinitely less than

$$\frac{2a^n - 2 \cdot n \cdot \frac{n-1}{2} a^{n-2} b^2 + 2 \cdot n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} a^{n-4} b^4 - \&c.}{(a^2+b^2)^n}:$$

a descending series will always converge, when x is greater than the greatest root of the resulting equations; and x^{n-1}, when n is infinite, is infinitely greater than $(a+b)^n$ and $(a-b)^n$; or more generally than $2a^n - 2n \cdot \frac{n-1}{2} a^{n-2} b^2 + 2n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} a^{n-4} b^4 - \&c.$

This follows from Caput 3. of the Meditationes Algebraicæ.

Cor. It appears from hence, that, if the ascending series converges, the descending will diverge; and, *vice versâ*, if the descending converges, the ascending will diverge, unless all the roots of the above-mentioned resulting equations may be deemed of of equal magnitude, as $+\alpha$ and α, $\alpha\sqrt[n]{-1}$, &c. and $x=\alpha$; in which caſe ſometimes both ſerieſes may become the ſame converging ſeries, &c.

When x, in the preceding caſes, is equal to the leaſt or greateſt root, the ſeries will ſometimes converge, and ſometimes not, as is ſhewn in the above-mentioned chapter. Whether the ſum of a ſeries, whoſe general term is given, can be found or not, will in many caſes appear from the law of the multinomial and other more general ſerieſes.

2. There are ſerieſes which always converge, whatever may be the value of x; as, for example, the ſeries $1+\frac{1}{2\cdot3}x+\frac{1}{2\cdot3\cdot4\cdot5}x^2+$&c. or $1+\frac{x}{2^2}+\frac{x^2}{3^3}+\frac{x^3}{4^4}+$&c. &c. always converge, whatever may be the value of x; but it may be obſerved, that theſe ſerieſes never ariſe from the expanſion of algebraical functions of x, or the before-mentioned fluents; but, in a few caſes, they may from fluxional equations. There are alſo ſerieſes which never converge as $1+1\cdot2x+1\cdot2\cdot3x^2+1\cdot2\cdot3\cdot4x^3+$&c. to which the preceding remark may be applied.

3. In the year 1757 ſome papers, which contained the firſt edition of my Meditationes Algebraicæ, were ſent to the Royal Society, in which was contained the following rule, *viz.* let S be a given function of the quantity x, which expand into a ſeries ($a+bx^m+cx^{2m}+$&c.) proceeding according to the dimenſions of x; in the quantity S, for x^m write αx^m, βx^m, γx^m, &c. where α, β, γ, &c. are roots of the equation $x^n-1=0$; and let the reſulting quantities be A, B, C, D, &c. then will $\frac{A+B+C+D+\&c.}{n}$ be equal to the ſum of the firſt, $2n+1$, $3n+1$, &c. terms *in infinitum.* This method, in the preface to the

last edition of the Meditationes Algebraicæ, is rendered more correct and general.

4. Let the sum of a series required be expressed by a function of a quantity z, the distance from the first term of the series, then will the general term be the difference between the two successive sums generally expressed.

5. Let the general term be an algebraical function of z:

1st, let it be $\frac{az^m + bz^{m-1} + cz^{m-2} + \&c.}{z+e \cdot z+e+1 \cdot z+e+2 \ldots z+e+n-1} = T$, where m and n are whole numbers; and m (if the sum of an infinite series of terms is required) less than n by two or more:

then the general term $\frac{az^m + bz^{m-1} + \&c.}{z+e \cdot z+e+1 \cdot z+e+2 \ldots z+e+n-1} =$

$$\frac{\gamma}{z+e \cdot z+e+1} + \frac{\delta}{z+e \cdot z+e+1 \cdot z+e+2} +$$

$$\frac{\varepsilon}{z+e \cdot z+e+1 \cdot z+e+2 \cdot z+e+3} + \&c. \ldots \frac{\theta}{z+e \cdot z+e+1 \ldots z+e+n-1};$$

whence if

$z+e+2 \cdot z+e+3 \cdot z+e+4 \ldots z+e+n-1 = z^{n-2} + Az^{n-3} + Bz^{n-4} + \&c.;$

$z+e+3 \cdot z+e+4 \cdot z+e+5 \ldots z+e+n-1 = z^{n-3} + A'z^{n-4} + B'z^{n-5} + \&c.;$

$z+e+4 \cdot z+e+5 \ldots z+e+n-1 = z^{n-4} + A''z^{n-5} + B''z^{n-6} + \&c.$ and so on; then, if $m = n-2$, will $\gamma = a$, $\delta = b - \gamma A$, $\varepsilon = c - \delta A' - \gamma B$, $\zeta = d - \varepsilon A'' - \delta B' - \gamma C$, &c.; whence the integral *in infinitum*, or sum of the infinite series, will be $\frac{\gamma}{z+e} + \frac{\delta}{2 \cdot z+e \cdot z+e+1} +$

$$\frac{\varepsilon}{3 \cdot z+e \cdot z+e+1 \cdot z+e+2} + \&c.$$

The reduction of the general term T into quantities of the before given formulæ was published in the Meditationes, printed in the year 1774. It was before reduced into formulæ of the same kind nearly by Mr. NICHOLE in the Paris Acts.

2d, Let the general term be $T' =$

$$\frac{az^b + bz^{b-1} + cz^{b-2} + \&c.}{z+e \cdot z+e+1 \cdot z+e+2 \ldots z+e+n-1 \times z+f \cdot z+f+1 \ldots z+f+m-1 \times z+g \cdot z+g+1 \ldots z+g+l-1 \times \&c.}$$

where

where h is a whole number less than $n+m+l+$ &c. (if it be greater, then the fraction can easily be reduced into a rational quantity $az^{h-n-m-l-\&c.}+$ &c. and a fraction of the before-mentioned kind); then will $T'=\left(\frac{\alpha}{z+e}+\frac{\alpha'}{z+f}+\frac{\alpha''}{z+g}+\&c.\right)+$

$$\left(\frac{\beta}{z+e\,.\,z+e+1}+\frac{\beta'}{z+f\,.\,z+f+1}+\frac{\beta''}{z+g\,.\,z+g+1}+\&c.\right)+$$

$$\left(\frac{\gamma}{z+e\,.\,z+e+1\,.\,z+e+2}+\frac{\gamma'}{z+f\,.\,z+f+1\,.\,z+f+2}+\frac{\gamma''}{z+g\,.\,z+g+1\,.\,z+g+2}\right.$$

$$\left.+\&c.\right)\ldots\left(\frac{X}{z+e\,.\,z+e+1\,.\,.\,z+e+n-1}+\frac{X'}{z+f\,.\,z+f+1\,.\,.\,z+f+m-1}\right.$$

$\left.+\frac{X''}{z+g\,.\,z+g+1\,.\,.\,z+g+l-1}+\&c.\right)$; whence its integral *in infinitum*, that is, the sum of the infinite series can be found, when $\alpha=0$, $\alpha'=0$, $\alpha''=0$, &c.; and consequently h not greater than $n+m+l+$ &c. -2; otherwise not. If h is not greater than $n+m+l+$ &c. -2, then will $\alpha+\alpha'+\alpha''+$ &c. $=0$, for else the sum would be infinite.

Let the number of quantities (e, f, g, &c.) be r, then from r independent integrals of a series, whose term is T'; or from $(r-1)$ independent sums of infinite serieses, whose term is T'; that is, where h is not greater than $n+m+l+$ &c. -2; can be deduced the sum of all infinite serieses of the before-mentioned formulæ, whose general term is T'.

If any factors are deficient in the denominator, as suppose the term to be $z+e\times z+e+3\times z+e+n-1$; multiply the numerator and denominator by the deficient factors, *viz.* by $z+e+1\,.\,z+e+2\times z+e+4\,.\,z+e+5\,.\,.\,z+e+n-2$, and it acquires the preceding formula; and so in the following examples.

3d, Let the denominator be $\overline{x+e}^{\pi}\times\overline{x+e+1}^{\pi}\times\overline{x+e+2}^{\pi}\ldots\overline{x+e+n-1}^{\pi}\times\overline{x+e+\mu}^{\pi'}\times\overline{x+e+\mu+1}^{\pi'}\times$

$x+e$

$\overline{x+e+\mu+2}^{\pi'} \times$ &c. $\times \overline{x+f}^{\varrho} \times \overline{x+f+1}^{\varrho} \times \overline{x+f+2}^{\varrho} \ldots \times \overline{x+f+m-1}^{\varrho} \times$ &c. $=\text{D}$, where π, π', ϱ, &c.; μ, &c. are whole numbers; and the general term is $\frac{az^b+bz^{b-1}+cz^{b-2}+\&c.}{\text{D}}$ $=\text{T}''$; then, if the dimenſions of z in the numerator be leſs than its dimenſions in the denominator, will $\text{T}''=$

$$\left(\frac{\alpha}{z+e}+\frac{\alpha'}{(z+e)^2}+\frac{\alpha''}{(z+e)^3}\ldots\frac{\alpha'^{\pi+\pi'-1}}{(z+e)^{\pi+\pi'}}+\frac{\beta}{z+f}+\frac{\beta'}{(z+f)^2}+\frac{\beta''}{(z+f)^3}\ldots\frac{\beta'^{\varrho-1}}{(z+f)^{\varrho}}+\&c.\right)+\left(\frac{\gamma}{z+e\,.\,z+e+1}+\frac{\theta}{z+f\,.\,z+f+1}+\&c.\right)+\&c.;$$

and in general there will be included all terms of the formulæ,

$$\frac{\text{A}\left(\overline{z+e+i}^{p}-\overline{z+e}^{p}\right)}{(z+e)^{p}\,.\,(z+e+1)^{p}\ldots(z+e+i)^{p}},$$

$$\frac{\text{B}\left((z+f+i')^{p'}-(z+f)^{p'}\right)}{(z+f)^{p'}\,.\,(z+f+1)^{p'}\ldots(z+f+i')^{p'}}, \&c.$$

$$\frac{\text{C}\left((z+e+\mu+i'')^{p''}-(z+e+\mu)^{p''}\right)}{(z+e+\mu)^{p''}\,.\,(z+e+\mu+1)^{p''}\ldots(z+e+\mu+i'')^{p''}}, \&c.$$

where A, B, C, &c. α, α', &c. β, β', &c. γ, θ, &c. denote invariable quantities; and p, p', p'', &c. are whole numbers not greater than π, ϱ, π', &c. reſpectively; and i, i', i'', &c. are whole numbers not greater than $n-1$, $m-1$, &c.

If all the quantities α, α', α'', &c. β, β', β'', &c. &c. are $=0$, the ſum of the ſeries can be expreſſed in finite terms of the quantity z, otherwiſe not; and alſo if b be leſs than the dimenſions of z in the denominator by two or more, then will $\alpha+\beta+\&c.=0$, otherwiſe the ſum would be infinite.

From $\pi+\pi'+\varrho+\&c.-1$ independent ſums of infinite ſerieſes of this kind can be deduced the ſums of all infinite ſerieſes of the ſame kind.

This method may be extended to infinite series, in which exponentials as e^{z} are contained, which will easily be seen from some subsequent propositions; but in my opinion the subsequent method of finding the sum of serieses is to be preferred to the preceding one, both for its generality and facility.

6. 1. Let the general term be $(az^{b}+bz^{b-1}+cz^{b-2}+\&c.)\times$ $(z+e)^{-1}.(z+e+1)^{-1}.(z+e+2)^{-1}\ldots(z+e+n-1)^{-1}$; where b is a whole number less than n by two or more, when the sum of an infinite series is required.

Assume for the sum the quantity $(z+e)^{-1}.(z+e+1)^{-1}.$ $(z+e+2)^{-1}\ldots(z+e+n-2)^{-1}\times(\alpha z^{b'}+\beta z^{b'-1}+\gamma z^{b'-2}+\&c.)$; find the difference between this sum and its successive one $(z+e+1)^{-1}.(z+e+2)^{-1}.(z+e+3)^{-1}\ldots(z+e+n-1)^{-1}$ $\times(\alpha\overline{z+1}^{\,b'}+\beta\overline{z+1}^{\,b'-1}+\&c.)$, which will be $-(z+e)^{-1}.$ $(z+e+1)^{-1}.(z+e+2)^{-1}\ldots(z+e+n-1)^{-1}\times(\overline{z+e}\times$ $(\alpha\overline{z+1}^{\,b'}+\beta\overline{z+1}^{\,b'-1}+\&c.)-\overline{z+e+n-1}\times(\alpha z^{b'}+\beta z^{b'-1}+\&c.)$ $=\overline{b'-n+1}\,\alpha z^{b'}+\&c.)$; then make the terms of this difference equal to the correspondent terms of the given quantity $az^{b}+bz^{b-1}+\&c.$ and there result $b'=b$, $-\overline{b-n+1}\times\alpha=a$, and consequently $\alpha=\frac{-a}{b-n+1}$, &c.

2. Let the general term be $(z+e)^{-1}.(z+e+1)^{-1}.$ $(z+e+2)^{-1}\ldots(z+e+n-1)^{-1}\times(z+f)^{-1}.(z+f+1)^{-1}.$ $(z+f+2)^{-1}\ldots(z+f+m-1)^{-1}\times(az^{b}+bz^{b-1}+cz^{b-2}+\&c.)$. Assume the quantity $(z+e)^{-1}.(z+e+1)^{-1}\ldots(z+e+n-2)^{-1}$ $\times(z+f)^{-1}.(z+f+1)^{-1}.(z+f+2)^{-1}\ldots(z+f+m-2)^{-1}$ $\times(\alpha z^{b'}+\beta z^{b'-1}+\gamma z^{b'-2}+\&c.)$ for the sum of the series sought; and thence deduce the general term, which suppose equal to the given general term, and from equating their corresponding parts easily can be deduced the index b' and co-efficients α, β, γ, &c. and consequently the sum of the series sought.

3. Le

3. Let the general term reduced to its lowest dimensions be $\overline{z+e}^{\pi} \times \overline{z+e+1}^{\pi} \ldots \overline{z+e+n-1}^{\pi} \times \overline{rz+f}^{-\rho} \times \overline{rz+f+r}^{-\rho} \times \overline{rz+f+2r}^{-\rho} \ldots \overline{rz+f+m-1r}^{-\rho} \times \overline{z+g}^{-\sigma} \times \overline{z+g+1}^{-\sigma} \times \ldots \overline{z+g+l-1}^{-\sigma} \times$ &c. $\times (az^{b} + bz^{b-1} + cz^{b-2} + \text{\&c.})$. If it be required to reduce the term $\overline{rz+f}^{-\rho}$, &c. to a conformity with the rest, for $\overline{rz+f}^{-\rho}$, &c. substitute $\overline{z+\frac{f}{r}}^{-\rho} \times r^{-\rho}$, &c. and it is done. Assume for the integral or sum the quantity $S = \overline{z+e}^{\pi} . . \overline{z+e+1}^{\pi} . . \overline{z+e+n-2}^{\pi} \times \overline{rz+f}^{-\rho} . \overline{rz+r+f}^{-\rho} . . \overline{rz+m-2r+f}^{-\rho} \times \overline{z+g}^{-\sigma} \times \overline{z+g+1}^{-\sigma} \ldots \overline{z+g+l-2}^{-\sigma} \times$ &c. $\times (\alpha z^{b'} + \beta z^{b'-1} + \text{\&c.}) = S$, find its successive sum by writing $z+1$ for z in the sum S, and let the quantity resulting be S'; then will the general term be $S - S'$, which equate to the given general term, that is, their correspondent quantities; and thence may be deduced the index b' and co-efficients α, β, &c.; and consequently the sum sought. If the series does not terminate, then the sum will be expressed by a series proceeding *in infinitum*, according to the reciprocal dimensions of z.

From $\pi + \rho + \sigma +$ &c. $- 1$ independent integrals of the above-mentioned kind can be deduced the integrals of all quantities of the same kind; that is, where b is any whole affirmative number whatever, and the co-efficients a, b, c, &c. are any how varied.

If any factor $z+g$ in the denominator, &c. has no other $z+g+l-1$, which differs from it by a whole number $l-1$; or the factor $rz+f$ has no correspondent factor $rz+f+mr$, where m is a whole number; then the integral of the above-mentioned series cannot be expressed in finite terms of the quantity z. In like manner, if the dimensions of z in the numerator are less than

than its dimensions in the denominator by unity, then the integral of the general term cannot be expressed by a finite algebraical function of z. If the number of terms to be added be infinite, it is well known that the sum in this case will be infinite.

It may be observed, that in finding the sum of a series, whose general term is given, all common divisors of the numerator and denominator must be rejected, otherwise serieses may appear difficult to be summed, which are very easy: for example, let the series be $\frac{1}{1.2.3.4.5}+\frac{4}{4.5.6.7.8}+\frac{9}{7.8.9.10.11}$ $+\&c.=\frac{1}{3}\left(\frac{1}{1.2.4.5}+\frac{2}{4.5.7.8}+\frac{3}{7.8.10.11}+\&c.\right)$, whose general term is $\frac{z+1}{3z+1.3z+4\times 3z+2.3z+5}$; and by assuming, as before taught, $\overline{3z+1}^{-1}\times\overline{3z+2}^{-1}\times\alpha$ for the sum sought; and finding its general term $\overline{3z+1}^{-1}\times\overline{3z+4}^{-1}\times\overline{3z+2}^{-1}\times\overline{3z+5}^{-1}\times 18\,\overline{z+1}\times\alpha$, which equating to the general term given, there results $18\alpha=1$, and the sum sought $=\frac{1}{18}\times\frac{1}{3z+1.3z+2}$.

Ex. 2. Let the series be $\frac{1}{1.2.3.4.5}+\frac{14}{5.6.7.8.9}+\frac{55}{9.10.11.12.13}+\frac{140}{13.14.15.16.17}+\&c.=\frac{1}{24}\left(\frac{1}{1.5}+\frac{1}{5.9}+\frac{1}{9.13}+\frac{1}{13.17}+\&c.\right)$, of which the general term is $\frac{1}{24}\times\frac{1}{4z+1.4z+5}$; and consequently the sum deduced is $\frac{1}{24}\times\frac{1}{4}\times\frac{1}{4z+1}$.

These are serieses given by Mr. De Moivre, and esteemed by Dr. Taylor *altioris indaginis*.

 Some

Some other writers have made ſome ſerieſes to appear more difficult to be ſummed, by not reducing them to their loweſt terms.

7. Having given the principles of a general method of finding the ſum of a ſeries, when its general term can be expreſſed by algebraical, and not exponential, functions of z, the diſtance from the firſt term of the ſeries; it remains to perform the ſame when exponentials are included.

1. Let S the ſum be any algebraical function of z multiplied into $e^z = x^z$; then will the general term be $Se^z - eS'e^z = (S - eS')\,e^z$; whence, from the general term Te^z being given, aſſume quantities in the ſame manner (with the ſame denominator, &c.) as when no exponential was involved, which multiplied into e^z, ſuppoſe to be the ſum; from the ſum find its general term, and equate it to the given one by equating their correſpondent co-efficients, and it is done.

Ex. Let the general term be $\frac{z+2}{2z+1\,.\,2z+3} \times e^{z+1}$: aſſume for the ſum ſought $\frac{\alpha}{2z+1} \times e^{z+1}$, whence the general term is $\left(\frac{\alpha}{2z+1} - \frac{\alpha e}{2z+3}\right) e^{z+1} = \frac{2\alpha(1-e)z + 3\alpha - \alpha e}{2z+1\,.\,2z+3} \times e^{z+1}$; equate it to the given term, and there reſults $2\alpha(1-e) = 1$ and $3\alpha - \alpha e = 2$, and conſequently $e = \frac{1}{3}$ and $\alpha = \frac{3}{4}$, if the ſeries can be ſummed.

The ſame obſervation, *viz.* that if any factor in the denominator or irrational quantity have no other correſpondent to it; for example, if the factor be $z+g$, and there is no correſpondent one $x+g+n$, where n is a whole number, then its integral cannot be expreſſed by a finite algebraical function of z.

In the ſame manner may the ſums be found, when the terms are exponentials of ſuperior orders; for the exponential, irra-

6 tional,

tional, &c. quantities in the denominators of the sums may be easily deduced from the preceding principles; and thence, by proceeding as is before taught, the sum required.

The principles of all these cases have been given in the Meditationes.

8. Mr. JAMES BERNOULLI found summable serieses by assuming a series V, whose terms at an infinite distance are infinitely little, and subtracting the series diminished by any number (l) of terms from the series itself, &c.

It is observed in the Meditationes, that if T (m), T $(m+n)$, T $(m+n+n')$, T $(m+n+n'+n'')$, &c. be the terms at m, $m+n$, $m+n+n'$, $m+n+n'+n''$, &c. distances from the first, and $a\mathrm{T}(m)+b\mathrm{T}(m+n)+c\mathrm{T}(m+n+n')+d\mathrm{T}(m+n+n'+n'')+$&c. be the general term, it will be summable, when $a+b+c+d+$&c. $=0$; the sum of the series will be $a(\mathrm{T}(m)+\mathrm{T}(m+1)+\mathrm{T}(m+2)+\ldots+\mathrm{T}(m+n+n'+n''+\&c.-1))+b(\mathrm{T}(m+n)+\mathrm{T}(m+n+1)+\mathrm{T}(m+n+2)+\ldots+\mathrm{T}(m+n+n'+n''+\&c.-1))+c(\mathrm{T}(m+n+n')+\mathrm{T}(m+n+n'+1)+\ldots(\mathrm{T}(m+n+n'+n''+\&c.-1))+\&c.=\mathrm{H}$. If the sum $a+b+c+d+$&c. be not $=0$, and the series $\mathrm{T}(m)+\mathrm{T}(m+1)+\mathrm{T}(m+2)+$&c. *in infinitum* be a converging one $=\mathrm{S}$, then will the sum of the resulting series be $(a+b+c+d+\&c.)\mathrm{S}-(b+c+d+\&c.)(\mathrm{T}^{m}\ldots+\mathrm{T}^{m+n-1})-(c+d+\&c.)(\mathrm{T}^{m+n}\ldots\mathrm{T}^{m+n+n'-1})-(d+\&c.)(\mathrm{T}^{m+n+n'}+\ldots\mathrm{T}^{m+n+n'+n''-1})-$&c.

8. 2. Let the series V consist of terms, which have only one factor in the denominator, and its numerator $=1$; that is, let the general term be $\frac{1}{rz+e}$, and the series consequently $\frac{1}{e}+\frac{1}{r+e}+\frac{1}{2r+e}+$&c. $=\mathrm{V}$; from the before-mentioned addition or subtraction there follows $\frac{a}{rz+e}+\frac{b}{rz+r+e}+\frac{c}{rz+2r+e}+$ &c. $=$

$\frac{\alpha z^m+\beta z^{m-1}+\gamma z^{m-2}+\&c.}{rz+e\,.\,rz+r+e\,.\,rz+2r+e\,.\,\&c.}$; where m is not greater than the number (N) of factors in the denominator diminished by unity. From α, β, γ, &c. r and e being given, easily can be acquired by simple equations, or known theorems, the required co-efficients a, b, c, &c. If $m=N-1$ and α and $a+b+c+d+\&c.=0$, then the sum of the series resulting will be finite.

8. 3. If the terms of the series assumed $\frac{1}{e}-\frac{1}{r+e}+\frac{1}{2r+e}-\frac{1}{3r+e}+\&c.$ be alternately affirmative and negative; then by the preceding case find $\frac{\alpha z^m+\beta z^{m-1}+\gamma z^{m-2}+\&c.}{rz+e\,.\,rz+r+e\,.\,rz+2r+e+\&c.}=\frac{a}{rz+e}+\frac{b}{rz+r+e}+\frac{c}{rz+2r+e}+\&c.$ Where the terms of the resulting series are alternately affirmative and negative, let the two subsequent terms be supposed $\frac{\alpha z^m+\beta z^{m-1}+\gamma z^{m-2}+\&c.}{rz+e\,.\,rz+r+e\,\ldots\,rz+\overline{n-1}r+e}=\frac{a}{rz+e}+\frac{b}{rz+r+e}+\&c.$ and $\frac{\alpha\overline{z+1}^m+\beta\overline{z+1}^{m-1}+\gamma\overline{z+1}^{m-2}+\&c.}{rz+r+e\,.\,rz+2r+e\,\ldots\,rz+nr+e}=\frac{a}{rz+r+e}+\frac{b}{rz+2r+e}+\&c.$ of which the one is affirmative and the other negative: reduce the resulting series to an affirmative one by subtracting the subsequent term from its preceding, and it becomes $\frac{(rz+nr+e)\cdot(\alpha z^m+\beta z^{m-1}+\&c.)-(rz+e)\,(\alpha\overline{z+1}^m+\beta\overline{z+1}^{m-1}+\&c.)}{rz+e\,.\,rz+r+e\,.\,rz+2r+e\,\ldots\,rz+rn+e}$ $=\frac{\overline{n-m}r\alpha z^m+\&c.}{rz+e\,.\,rz+r+e\,\ldots\,rz+rn+e}=\frac{a}{rz+e}+\frac{b-a}{rz+r+e}+\&c$ In this case, since two terms are added into one, the distance from the first term of the series will be $\frac{z}{2}$, which suppose $=w$; and write $2w$ for z in the above-mentioned term, and there results $\frac{\overline{n-m}r\alpha z^m+\&c.}{rz+e\,.\,rz+r+e\,\ldots\,rz+nr+e}=\frac{\overline{n-m}r\alpha\times 2^m w^m+\&c.}{2rw+e\,.\,2rw+r+e\,\ldots\,2rw+nr+e}=$

$\frac{a}{2rw+e}$

$\frac{a}{2rw+e}+\frac{b-a}{2rw+r+e}+$ &c.; whence the ſum of any ſeries, whoſe general term is $\frac{a'w^m+b'w^{m-1}+\&c.}{2rw+e\,.\,2rw+r+e\,...\,2rw+nr+e}$, where m is a whole number leſs than n by two or more, and w the diſtance from the firſt term of the ſeries can be found from the ſum of the ſeries $\frac{1}{e}-\frac{1}{r+e}+\frac{1}{2r+e}-\frac{1}{3r+e}+$ &c.

9. Let there be two ſerieſes $\frac{1}{e}+\frac{1}{e+r}+\frac{1}{e+2r}+$ &c. $=S$ and $\frac{1}{f}+\frac{1}{f+r}+\frac{1}{f+2r}+\frac{1}{f+3r}+$&c.$=S'$, whoſe general terms are reſpectively $+\frac{1}{e+rz}$ and $+\frac{1}{f+rz}$; then from the ſum of theſe two ſerieſes can be collected the ſum of any ſeries, whoſe general term is

$$\frac{\alpha z^m+\beta z^{m-1}+\&c.}{rz+e\,.\,rz+e+r\,.\,rz+e+2r\,...\,rz+\overline{n-1}r+e\times rz+f\,.\,rz+r+f\,...\,rz+f+\overline{m-1}r}$$

$$=\frac{a}{rz+e}+\frac{b}{rz+e+r}+\frac{c}{rz+e+2r}\,...+\frac{\lambda}{rz+\overline{n-1}r+e}+\frac{a'}{rz+f}+\frac{b'}{rz+r+f}$$

$+\frac{c'}{rz+2r+f}\,...+\frac{\mu'}{rz+\overline{m-1}r+f}$; where $e-f$ is not a whole number. Let $a+b+c\,..+\lambda=0$, and $a'+b'+c'\,...+\mu'=0$, then the ſum will be $a\left(\frac{1}{rz+e}+\frac{1}{rz+r+e}\,...+\frac{1}{rz+\overline{n-2}r+e}\right)+b\left(\frac{1}{rz+e+r}\right.$ $\left.+\frac{1}{rz+e+2r}\,...+\frac{1}{rz+\overline{n-2}r+e}\right)+c\left(\frac{1}{rz+e+2r}+\frac{1}{rz+e+3r}+\,..+\right.$ $\left.\frac{1}{rz+\overline{n-2}r+e}\right)+$ &c. $+a'\left(\frac{1}{rz+f}+\frac{1}{rz+r+f}\,...\frac{1}{rz+\overline{m-2}r+f}\right)+b'$ $\left(\frac{1}{rz+r+f}+\,...+\frac{1}{rz+\overline{m-2}r+f}\right)+$ &c.

2. If the ſerieſes are $\frac{1}{e}-\frac{1}{e+r}+\frac{1}{e+2r}-$ &c. and $\frac{1}{f}-\frac{1}{f+r}+\frac{1}{f+2r}$ $-$ &c.; then from the ſum of theſe two ſerieſes can be collected by the principles given above the ſum of any ſeries, whoſe general term is

$$\frac{\alpha z^{m}+\beta z^{m-1}+\gamma z^{m-2}+\&c.}{2rz+e\,.\,2rz+r+e\,.\,2rz+2r+e\ldots 2rz+\overline{n-1}r+e\times 2rz+f\,.\,2rz+r+f\,.\,2rz+2r+f\ldots 2rz+\overline{m-1}r+f}.$$

The ſame principle may be applied to find the ſum of any ſeries of the abovementioned ſort, in whoſe denominator are contained other factors, $rz+g$, $rz+g+r$, &c. &c.; or $2rz+g$, $2rz+g+r$, $2rz+g+2r$, &c. Like propoſitions may be deduced from ſerieſes, in which r and r', &c. and the factors $rz+e$ and $r'z+g$, &c. denote different quantities.

10. An apparently more general method may be given from aſſuming a ſeries or ſerieſes as before; and adding every two, three, four, &c. (n) ſucceſſive terms together for terms of a new ſeries beginning from the firſt, ſecond, third, &c. n^{th} term; and in general adding together two, three, &c. n ſucceſſive general terms; and in their ſum writing for z the diſtance from the firſt term of the ſeries $2z+a$, $3z+a$, &c. $nz+a$; there will reſult the general term of a ſeries not to be found from the abovementioned addition.

Ex. Let the ſeries aſſumed be $\frac{1}{1}+\frac{1}{2}+\frac{1}{3}+\frac{1}{4}+$ &c. *in infinitum*, of which the general term beginning from the firſt is $\frac{1}{z+1}$; add three ſucceſſive general terms $\frac{1}{z+1}+\frac{1}{z+2}+\frac{1}{z+3}$ $=\frac{3z^2+12z+11}{z+1\,.\,z+2\,.\,z+3}$; in this term for z write $3z$, and there reſults $\frac{27z^2+36z+11}{3z+1\,.\,3z+2\,.\,3z+3}$. In the ſame manner, if the beginning is inſtituted from the ſecond or third term of the given ſeries, the terms reſulting will be $\frac{3z^2+18z+26}{z+2\,.\,z+3\,.\,z+4}$ and $\frac{3z^2+24z+47}{z+3\,.\,z+4\,.\,z+5}$. In theſe terms for z write $3z$, and there reſult $\frac{27z^2+54z+26}{3z+2\,.\,3z+3\,.\,3z+4}$ and $\frac{27z^2+72z+47}{3z+3\,.\,3z+4\,.\,3z+5}$.

If

If the terms of the given feries are alternately affirmative and negative, the terms of the refulting feries will be alternately affirmative and negative, if n be an odd number; otherwife its terms will be all affirmative. The fum of this feries will be finite or infinite, as the fum of the feries $1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} +$ &c. is finite or infinite; but from it, by the preceding method of addition or fubtraction of Mr. BERNOULLI's, or a like method applied to more feriefes, may be found the fums of different finite feriefes.

It may be obferved, that from Mr. BERNOULLI's addition or fubtraction can never be deduced the feriefes which arife from this method; for, by his method, the denominator can never have any factors but what are contained in the denominators of the given feries, *viz.* (in the feries $\frac{1}{1} + \frac{1}{2} + \frac{1}{3} +$ &c.), $z + l$, where l is a whole number; but by this method are introduced into the denominator the factors $2z + l$, $3z + l$, &c. and $nz + l$, or which may be reduced to the fame $\left(z + \frac{l}{n}\right) \times n$.

If n fucceffive general terms of the feriefes arifing from Mr. BERNOULLI's addition or fubtraction be added together, and in the quantity thence arifing for z the diftance from the firft term of the feries be fubftituted nz, there will be produced feriefes of the above-mentioned formula.

11. Multiply two converging feriefes $a + bx + cx^2 + dx^3 +$ &c. $= S$ and $\alpha + \beta x + \gamma x^2 +$ &c. $= V$, or find any rational and integral function of them, and the feries refulting will be finite and $= S \times V$, &c. Let $\alpha + \beta x + \gamma x^2 +$ &c. $x^m = V$ be finite, and the refulting feries will be finite and $= S \times V$, &c. If S be a feries converging or not, whofe ultimate terms are lefs than any finite quantity, then will the feries $(a + bx + cx^2 +$ &c.$) \times (\alpha + \beta x + \gamma x^2 +$ &c. $x^m) = V \times S$ be a converging one, if $\alpha + \beta x + \gamma x^2 + \ldots$ &c. $x^m = 0$; which cafe was given by Mr. DE MOIVRE.

Mr.

Mr. BERNOULLI's addition, &c. can be applied to serieses of this kind. For example, let the given series be $\frac{1}{e}+\frac{1}{e+1}x+\frac{1}{e+2}x^2+$ &c. $=S$. From this series subtract the same series diminished by m terms, *viz.* $\frac{1}{e+m}x^m+\frac{1}{e+m+1}x^{m+1}+\frac{1}{e+m+2}x^{m+2}$ + &c. and there remains $\frac{e+m-ex^m}{e\cdot m+e}+\frac{e+m+1-\overline{e+1}x^m}{e+1\cdot e+m+1}x+\frac{e+m+2-\overline{e+2}x^m}{e+2\cdot e+m+2}x^2+\frac{e+m+3-\overline{e+3}x^m}{e+3\cdot e+m+3}\times x^3+$ &c.; for x^m write A, then will the series become $\frac{m-eA}{e\cdot m+e}+\frac{e+m+1-\overline{e+1}A}{e+1\cdot e+m+1}x+\frac{e+m+2-\overline{e+2}A}{e+2\cdot e+m+2}x^2+\frac{e+m+3-\overline{e+3}A}{e+3\cdot e+m+3}x^3+$ &c. $=\frac{1}{e}+\frac{1}{e+1}x+\frac{1}{e+2}x^2\ldots\ldots\frac{1}{e+m-1}x^{m-1}$.

Let the general term be $\frac{az^m+bz^{m-1}+cz^{m-2}+\&c.}{z+e\cdot z+e+1\cdot z+e+2\ldots z+e+n-1}\times x^z$ $=\left(\frac{\alpha}{z+e}+\frac{\beta}{z+e+1}+\frac{\gamma}{z+e+2}\ldots\frac{X}{z+e+n-1}\right)x^z$. Suppose $\beta=\beta'x$, $\gamma=\gamma'x^2$, $\delta=\delta'x^3$, .. $X=X'x^{n-1}$; then will the sum of the above-mentioned series be $(\alpha+\beta'+\gamma'+\delta'+\&c.)\times S-\frac{1}{e}(\beta'+\gamma'+\delta'+\&c.)-\frac{1}{e+1}(\gamma'+\delta'+\&c.)-\frac{1}{e+2}(\delta'+\&c.)-\&c.$

From the sum of the series $\frac{1}{e}-\frac{x}{e+1}+\frac{x^2}{e+2}-$ &c. by these and the principles before delivered can be deduced the sum of any series, whose general term is

$$\frac{az^m+bz^{m-1}+\&c.}{2z+e\cdot 2z+e+1\cdot 2z+e+2\cdot 2z+e+3\times\&c.}x^z.$$

In like manner from the sum of the serieses $\frac{x}{e}+\frac{x^2}{e+1}+\frac{x^3}{e+2}$ + &c. $\frac{x}{f}+\frac{x^2}{f+1}+\frac{x^3}{f+2}+$ &c. $\frac{x}{g}+\frac{x^2}{g+1}+\frac{x^3}{g+2}+$ &c. &c. can be deduced

deduced the ſum of any ſeries, whoſe general term is

$$\frac{az^{m}+bz^{m-1}+\&c.}{z+e.z+e+1.z+e+2\times\&c.\times z+f.z+f+1.z+f+2.\&c.\times z+g.z+g+1.\&c.}\times x^{z}.$$

And alſo from the ſum of the ſerieſes $\frac{1}{e}-\frac{1}{e+1}x+\frac{x^2}{e+2}-\&c.$ $\frac{1}{f}-\frac{x}{f+1}+\frac{x^2}{f+2}-\&c.$ $\frac{1}{g}-\frac{x}{g+1}+\frac{x^2}{g+2}-\&c.$ &c. can be deduced the ſum of any ſeries, whoſe general term is

$$\frac{az^{m}+bz^{m-1}+\&c.}{2z+e.2z+e+1.\&c.\times 2z+f.2z+f+1.\&c.2z+g.2z+g+1.\&c.}\times x^{z}.$$

The method of adding more terms of a given ſeries together, as before taught, may be applied to theſe and all other ſerieſes. For example: let the given ſeries be $1+\frac{1}{2}x+\frac{1}{3}x^2+\frac{1}{4}x^3+\&c.$; add two terms conſtantly together, and it becomes $1+\frac{1}{2}x+\&c.=\frac{2+x}{2}+\frac{4+3x}{3\cdot 4}x^2+\frac{6+5x}{5\cdot 6}x^4+\&c.=\frac{2+A}{2}+\frac{4+3A}{3\cdot 4}x^2+\frac{6+5A}{5\cdot 6}x^4+\&c.$ whence the general term is $\frac{2z+2+(2z+1)}{2z+2}\frac{x}{2z+1}x^{2z}$. From the methods before given of addition, ſubtraction, and multiplication; and the ſerieſes found by this method, can be derived ſerieſes, whoſe ſums are known.

12. Suppoſe a given ſeries $ax^{n}+bx^{n\pm s}+cx^{n\pm 2s}+dx^{n\pm 3s}+\&c.$ whoſe ſum p is either an algebraical, exponential, or fluential fluxion of x; multiply the equation $p=ax^{n}+bx^{n\pm s}+cx^{n\pm 2s}+dx^{n\pm 3s}+\&c.$ into $x^{\pm r-n}$, and there reſults $ax^{\pm r-n}p=ax^{\pm r}+bx^{\pm r\pm s}+cx^{\pm r\pm 2s}+\&c.$; find the fluxion of this equation, and there follows $\frac{1}{\dot{x}}$ multiplied into the fluxion of the quantity $(x^{\pm r-n}p)$ $=\pm rax^{\pm r-1}+(\pm r\pm s)\,bx^{\pm r\pm s-1}+(\pm r\pm 2s)\,cx^{\pm r\pm 2s-1}+\&c.$ of which the general term is $(\pm r\pm zs)\times t$, where z denotes the diſtance from the firſt term of the ſeries, and t is

is the term in the given series, whose distance from the first is z. In the same manner may be deduced the sum of a series whose general term is $t' \times \overline{\pm r \pm zs} \times \overline{\pm r' \pm z \pm ns}$, or by repeated operations $t' \times \overline{ez^2 + fz + g}$, where t' is a term of the given equation, whose distance from the first term is z. And in general, from the sum of a given series, whose fluxion can be found, and whose general term is t', can be deduced by continued multiplication, and finding the fluxion, the sum of a series or quantity, of which the general term is At', where A is any function of the following kind $a'z^m + b'z^{m-1} + c'z^{m-2} + \&c.$ in which z denotes the distance from the first term of the series, and m a whole number. It is to be observed, that if the given series converges in a ratio, which is at least equal to the ratio of the convergency of some geometrical series, the resulting equation will always converge. But if in a less ratio, then it will sometimes converge, sometimes not; according to the ratio which the successive terms of the resulting series have to each other at an infinite distance.

Corollary. $\frac{p \cdot p+1 \cdot p+2 \cdot p+3 \cdot\cdot p+z}{r \cdot r+1 \cdot r+2 \cdot r+3 \cdot\cdot r+z} = \frac{p+z \cdot p+z-1 \cdot p+z-2 \cdot p+z-3 \cdot\cdot z+r+1}{r \cdot r+1 \cdot r+2 \cdot r+3 \cdot\cdot\cdot\cdot\cdot p-1}$, if $p-r$ be a whole affirmative number; but this latter quantity has the formula above-mentioned $az^m + bz^{m-2} + cz^{m-3} + \&c.$; and consequently, if the sum of the series $a + bx^s + cx^{2s} + dx^{3s} + \&c. = p$ be known, by this method can be deduced the sum of the series $a + \frac{p}{r} bx^s + \frac{p \cdot p+1}{r \cdot r+1} cx^{2s} + \frac{p \cdot p+1 \cdot p+2}{r \cdot r+1 \cdot r+2} dx^{3s} + \&c.$

Ex. 1. Since $\overline{a+x}^{\frac{m}{n}} = a^{\frac{m}{n}} \left(1 + \frac{m}{n} \times \frac{x}{a} + \frac{m}{n} \times \frac{m-n}{2n} a^{-2}x^2 + \frac{m}{n} \cdot \frac{m-n}{2n} \cdot \frac{m-2n}{3n} a^{-3}x^3 + \&c.\right)$; multiply the successive terms of this series into

into the terms of the ſeries $1, \frac{p}{r}, \frac{p \cdot p+1}{r \cdot r+1}$, &c. and a ſeries is deduced $a^{\frac{m}{n}} + \frac{p \cdot m}{r \cdot n} a^{+\frac{m}{n}-1} x + \frac{p \cdot p+1 \times m \cdot m-n}{r \cdot r+1 \cdot n \cdot 2n} x^{+\frac{m}{n}-2} +$ &c. whoſe ſum is known, if the ſum of the ſeries $= \overline{a+x}^{+\frac{m}{n}}$ is known.

Ex. 2. If the ſeries begins from the $l+1^{\text{th}}$ term of the above-mentioned binomial theorem $a^{\frac{m}{n}} + \frac{m}{n} a^{+\frac{m}{n}-1} x +$ &c. *viz.* the ſeries be $\mathrm{H} \times \overline{1 + \frac{m-\overline{l+1}n}{l+2n} \frac{x}{a} + \frac{m-\overline{l+2}n}{l+3n} \frac{x^2}{a^2} + \frac{m-\overline{l+3}n}{l+4n} \frac{x^3}{a^3} + \text{\&c.}}$ of which let the reſpective terms be multiplied into 1, $\frac{p}{r}, \frac{p \cdot p+1}{r \cdot r+1}$, &c. there will reſult a ſeries whoſe ſum is known.

Ex. 3. From the rule firſt given by me for finding the ſum of the terms at b diſtances from each other, the ſum of the ſeries $1 + \frac{m-\overline{l+1}n}{l+2 \cdot n} \times \frac{m-\overline{l+2}n}{l+3\,n} \ldots \frac{m-\overline{l+b}n}{l+b+1n} \times \frac{x^b}{a^b} + \mathrm{P} \times \frac{m-\overline{l+b+1}n}{l+b+2n} \times \frac{m-\overline{l+b+2}n}{l+b+3n} \ldots \frac{m-\overline{l+2b}n}{l+2b+1n} \frac{x^{2b}}{a^{2b}} +$ &c. where P denotes the co-efficient of the preceding term, can be deduced; and conſequently the ſum of the ſeries deduced from multiplying the ſucceſſive terms of this ſeries into the quantities $1, \frac{p}{r}, \frac{p \cdot p+1}{r \cdot r+1}$, &c. reſpectively.

The general principles of this caſe were firſt delivered by Mr. Bernoulli, Mr. De Moivre, Mr. Euler, &c.

12. Aſſume the ſeries $a + bx^n + cx^{2n} +$ &c. $= p$, multiply it into $x^{\alpha-1}\dot{x}$, and find the fluent, then will $\frac{1}{\alpha} x^{\alpha} p - \frac{1}{\alpha} \int x^{\alpha} \dot{p} = \frac{1}{\alpha} a x^{\alpha} +$

$\frac{1}{\alpha+n} bx^{\alpha+n} + \frac{1}{\alpha+2n} cx^{\alpha+2n} +$ &c.; multiply this equation into $x^{\beta-\alpha-1}\dot{x}$, and find the fluent of the equation resulting, which will be $\frac{1}{\beta} \times \frac{1}{\alpha} x^{\beta} p - \frac{1}{\alpha} \cdot \frac{1}{\beta} \int x^{\beta}\dot{p} - \frac{1}{\alpha} \times \frac{1}{\beta-\alpha} x^{\beta-\alpha} \int x^{\alpha}\dot{p} + \frac{1}{\alpha} \cdot \frac{1}{\beta-\alpha} \int x^{\beta}\dot{p} = \frac{1}{\alpha} \cdot \frac{1}{\beta} ax^{\beta} + \frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} bx^{\beta+n} + \frac{1}{\alpha+2n} \cdot \frac{1}{\beta+2n} cx^{\beta+2n} +$ &c.; divide by x^{β}, and there results $\frac{1}{\beta} \cdot \frac{1}{\alpha} p + \frac{1}{\alpha} \cdot \frac{1}{\alpha-\beta} x^{-\alpha} \int x^{\alpha}\dot{p} + \frac{1}{\beta} \cdot \frac{1}{\beta-\alpha} x^{-\beta} \int x^{\beta}\dot{p} = \frac{1}{\alpha} \cdot \frac{1}{\beta} a + \frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} bx^{n} +$ &c.: and in general $\frac{1}{\alpha} \cdot \frac{1}{\beta} \cdot \frac{1}{\gamma} \cdot$ &c. $p + \frac{1}{\alpha} \cdot \frac{1}{\alpha-\beta} \cdot \frac{1}{\alpha-\gamma} x^{-\alpha} \int x^{\alpha}\dot{p} + \frac{1}{\beta} \cdot \frac{1}{\beta-\alpha} \cdot \frac{1}{\beta-\gamma}$, &c. $x^{-\beta} \int x^{\beta}\dot{p} + \frac{1}{\gamma} \cdot \frac{1}{\gamma-\alpha} \cdot \frac{1}{\gamma-\beta}$, &c. $x^{-\gamma} \int x^{\gamma}\dot{p} = \frac{1}{\alpha} \cdot \frac{1}{\beta} \cdot \frac{1}{\gamma}$ &c. $a + \frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} \cdot \frac{1}{\gamma+n}$ &c. $bx^{n} + \frac{1}{\alpha+2n} \cdot \frac{1}{\beta+2n} \cdot \frac{1}{\gamma+2n}$, &c. $cx^{2n} +$ &c. whence the law of continuation is immediately manifest.

Hence, if no two quantities α, β, γ, δ, &c. be equal to each other; and the successive terms a, b, c, d, &c. of any series $a + bx^{n} + cx^{2n} +$ &c. $= p$ be divided by $\alpha . \beta . \gamma . \delta .$ &c.; $\overline{\alpha+n} . \overline{\beta+n} . \overline{\gamma+n} . \overline{\delta+n}$. &c.; $\alpha+2n . \beta+2n . \gamma+2n . \delta+2n$. &c. &c.; and in general by $\alpha+nz . \beta+nz . \gamma+nz . \delta+nz$. &c. &c.; then can the sum of the series be found from the fluents of the fluxions $x^{\alpha}\dot{p}$, $x^{\beta}\dot{p}$, $x^{\gamma}\dot{p}$, $x^{\delta}\dot{p}$, &c. as has been observed in the Meditationes. If two are equal, *viz.* $\alpha = \beta$, then also the fluent of the fluxion $\frac{\dot{x}}{x} \int x^{\alpha}\dot{p}$ is required. If three are equal, *viz.* $\alpha = \beta = \gamma$; then it is necessary to find the fluent of the fluxion $\frac{\dot{x}}{x} \int \frac{\dot{x}}{x} \int x^{\alpha}\dot{p}$; and so on.

1. Let $\dot{p} = \frac{\dot{x}}{1 \pm x^{n}}$; and if the differences of the quantities α, β, γ, δ, &c. are divisible by n, from the fluent of the fluxion

fluxion $x^{\alpha}\dot{p}$ can be deduced the fluents of all the other fluxions $x^{\beta}\dot{p}$, $x^{\gamma}\dot{p}$, &c.; and in general, if $\alpha-\beta$ is divisible by n, then from the fluent of the fluxion $x^{\gamma}\dot{p}$ can be deduced the fluent of the fluxion $x^{\delta}\dot{p}$.

2. Suppose $p=$ the terms of the binomial theorem expanded according to the dimensions of x, *viz.* $(a+bx^{n})^{\frac{r}{s}}=$

$a^{\frac{r}{s}}+\frac{r}{s}a^{\frac{r}{s}-1}bx^{n}+$&c. beginning from the first or any other terms; then, if α, β, &c. divided by n give whole affirmative numbers, will all the fluxions $x^{\alpha}\dot{p}$, $x^{\beta}\dot{p}$, $x^{\gamma}\dot{p}$, &c. be integrable; and if the differences of the quantities α, β, γ, δ, &c. are divisible by n, from the fluent of the fluxion $x^{\alpha}\dot{p}$ can be deduced the fluents of the fluxions $x^{\beta}\dot{p}$, $x^{\gamma}\dot{p}$, &c.

If p denotes the sum of the alternate or terms whose distance from each other are m, of the binomial theorem, the same may be applied.

3. If $p=\overline{a+bx^{n}+cx^{2n}}^{\frac{r}{s}}$; and α, β, γ, δ, &c. divided by n give whole affirmative numbers, then from $\int x^{\alpha}\dot{p}$ can be deduced all the remainder $\int x^{\beta}\dot{p}$, $\int x^{\gamma}\dot{p}$, &c.: and in general from two can be deduced all the remainder.

To find when the sum of any series of this kind can be found, add together each of the fluents, which can be found from each other, and not otherwise, and suppose their sum $=0$; and so of any other similar fluent, and from the resulting equations can be discovered when the series can be integrated.

13. If the general term of a series contains in it more variable quantities, z, v, w, &c.; then find the sum of the series, first, from the hypothesis that one of them (z) is only variable,

ble, which, properly corrected, let be A; in the quantity A ſuppoſe all the quantities invariable but ſome other v, and find the ſum of the ſeries thence reſulting, which let be B, and ſo on; and the ſum of the ſeries will be deduced.

Ex. Let the term be $\frac{1}{z \cdot z+1 \times v \cdot v+1 \cdot v+2}$; the dimenſions of z and v, &c. in the denominator muſt be at leaſt greater than its dimenſions in the numerator by a quantity greater than the number of the quantities z, v, &c. which proceed *in infinitum* increaſed by unity. Firſt, ſuppoſe z only variable, and the ſum of the infinite ſeries reſulting will be $\frac{1}{z \cdot v \cdot v+1 \cdot v+2} = A$; then ſuppoſe v only variable, and the ſum reſulting will be $\frac{1}{2z \cdot v \cdot v+1} = B$, which is the ſum required.

If it be ſuppoſed, that the quantities z and v, &c. in the ſame term ſhall never have the ſame values, then ſuppoſe z and v always to have the ſame values, and the general term $\frac{1}{z \cdot z+1 \cdot v \cdot v+1 \cdot v+2}$ becomes $\frac{1}{z^2 \cdot \overline{z+1}^2 \cdot z+2}$, of which let the ſum be V, then will $B - V$ be the ſum required.

On this and ſome other ſubjects more have been given in the Meditationes.

14. If the ſum of the ſeries cannot be found in finite terms, and it is neceſſary to recur to infinite ſeries; it is obſerved in the Meditationes to be generally neceſſary to add ſo many terms together, that the diſtance from the firſt term of the ſeries may conſiderably exceed the greateſt root of an equation reſulting from the general term made $= 0$; and afterwards a ſeries more converging may commonly be deduced from the fluents of fluxions reſulting from neglecting all but the greateſt quantities in the general terms reſulting; and by other

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different methods. Mr. NICHOLAS BERNOULLI and Mr. MOMMORT investigated the sum of the series (P) $A+Br+Cr^2+$ &c. by a series (Q) $\frac{A}{1-r}+\frac{d'r}{(1-r)^2}+\frac{d''r^2}{(1-r)^3}+\frac{d'''r^3}{(1-r)^4}+$ &c.; where d', d'', d''', &c. denote the successive differences of the terms A, B, C, D, &c. If r be negative, the denominators become $1+r$, $(1+r)^2$, $(1+r)^3$, &c.

It has been observed, in the Meditationes, that in swift converging series the series P will converge more swiftly than the series Q; in series converging according to a geometrical ratio, sometimes the one will converge more swift, and sometimes the other. In other series, which converge more slow, where most commonly r nearly $=1$, it cannot in general be said, which of the serieses will converge the swiftest. The preceding remark, *viz.* the addition of the first terms of the series, is necessary in most cases of finding the sums by serieses of this kind.

It is not unworthy of observation, that in almost all cases of infinite series, the convergency depends on the roots of the given equations, which remark was first published in the Meditationes. For example: in finding approximates to the roots of given equations the convergency depends on how much the approximates given are more near to one root than to any other; and consequently, when two or more roots or values of an unknown quantity are nearly equal, different rules are to be applied, which are improvements of the rule of false. This rule, and the above-mentioned observations were first given in the Meditationes Algebraicæ et Analyticæ, with several other additions on similar subjects.

Many

Many more things concerning the ſummation of ſeries, which depend on fluxional, &c. equations, might be added; but I ſhall conclude this paper with congratulating myſelf, that ſome algebraical inventions publiſhed by me have been ſince thought not unworthy of being publiſhed by ſome of the greateſt mathematicians of this or any other age.

1ſt, In the year 1757, I ſent to the Royal Society the firſt edition of my Meditationes Algebraicæ: they were printed and publiſhed in the years 1760 and 1762, with Properties of Curve Lines, under the title of Miſcellanea Analytica, and a copy of them ſent to Mr. EULER in the beginning of the year 1763, in which was contained a reſolution of algebraical equations, not inferior, on account of its generality and facility, to any yet publiſhed (*viz.* $y = a\sqrt[n]{p} + b\sqrt[n]{p^2} + c\sqrt[n]{p^3} + \ldots \sqrt[n]{p^{n-1}}$). This reſolution was publiſhed by Mr. EULER in the Peterſburg Acts for the year 1764. Whether Mr. EULER ever received my book, I cannot pretend to ſay; nor is it material: for the fact is, that it was publiſhed by me in the year 1760 and 1762, and firſt by Mr. EULER in the year 1764. Mr. DE LA GRANGE and Mr. BEZOUT have aſcribed this reſolution to Mr. EULER, as firſt publiſhed in the year 1764, not having ſeen (I ſuppoſe) my Miſcell. Analyt. Mr. BEZOUT found from it ſome new equations, of which the reſolution is known, and applied it to the reduction of equations: more new equations are given, and the reſolution rendered more eaſy by me in the Philoſophical Tranſactions. 2d, In the above-mentioned Miſcell. Analyt. an equation is transformed into another, of which the roots are the ſquares of the differences of the roots of the given equation; and it is aſſerted in that book, that if the co-efficients co-efficients of the terms of the resulting equations change continually from + to − and − to +, the roots of the given equation are all possible, otherwise not; and in a paper, inserted by me in the Philosophical Transactions for the year 1764, in which is found from this transformation, when there are none, two or four impossible roots contained in an algebraical equation of four or five dimensions; it is observed, that there will be none or four, &c. impossible roots contained in the given equation, if the last term be + or −; and two, &c. on the contrary, if the last term be − or +. These observations and transformation have been since published and explained in the Berlin Acts for the years 1767 and 1768, by Mr. DE LA GRANGE. 3d. In the Miscell. Anal. an equation is transformed into another, whose roots are the squares, &c. of the roots of a given equation; and it is asserted, that there are at least so many impossible roots contained in the given equation, as there are continual progresses in the resulting equation from + to + and − to −. It is afterwards remarked, that these rules sometimes find impossible roots when Sir ISAAC NEWTON's, and such like rules, fail; and that Sir ISAAC NEWTON's, &c. will find them, when this rule fails. This rule may somewhat further be promoted by first changing the given equation, whose root is x, into another whose root is $\sqrt{-1}x$; but, in my opinion, the rule of HARRIOT's, which only finds whether there are impossible roots contained in a cubic equation or not, is to be preferred to these rules, which, in equations of any dimensions, of which the impossible roots cannot generally be found from the rules, seldom find the true number. 4th, It is remarked, that rules which discover the true number of impossible roots require immense calculations, since they must necessarily find, when the

the roots become equal. In order to this, in the Miſcell. Anal. there is found an equation, whoſe roots are the reciprocals of the differences of any two roots of the given equation; and from finding a quantity (π) greater than the greateſt root of the given, and $\left(\frac{1}{A}\right)$ greater than the greateſt root of the reſulting equation, and ſubſtituting π, $\pi - A$, $\pi - 2A$, &c. for x in the given equation; will always be found the true number of impoſſible roots. 5th, In the ſame book are aſſumed two equations ($nx^{n-1} - \overline{n-1}px^{n-2} + \overline{n-2}qx^{n-3} - \&c. = 0$ and $x^n - px^{n-1} + \&c. = w$), and thence deduced an equation, whoſe root is w, from which, in ſome caſes, can be found the number of impoſſible roots.

6. In the Miſcell. Anal. is given the law of a ſeries, and its demonſtration, which finds the ſum of the powers of the roots of a given equation from its co-efficients. Mr. EULER has ſince publiſhed the ſame in the Peterſburg Acts, Mr. DE LA GRANGE printed a property of this ſeries, alſo printed by me about the ſame time; *viz.* that if the ſeries was continued *in infinitum*, the powers would obſerve the ſame law as the roots, which indeed immediately follows from the ſeries itſelf; but from thence with the greateſt ſagacity he deduces the law of the reverſion of the ſeries ($y = a + bx + cx^2 + dx^3 + \&c.$): it has ſince been given in a different manner from ſimilar principles in the Medit. Analyt. 7. In the Miſcell. Analyt. the law of a ſeries is given for finding the ſum of all quantities of this kind ($\alpha^m \times \beta^n \times \gamma^r \times \delta^s \times \&c. + \&c.$) where α, β, γ, δ, &c. denote the roots of a given equation, from the powers of the roots of the given equation. This law, with a different notation, has been ſince publiſhed in the Paris Acts by Mr. VANDERMONDE; who indeed mentions that he

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had heard, that a ſeries for that purpoſe was contained in my book, but had not ſeen it. In the ſame book is given a method of finding the aggregates of any algebraical functions of each of the roots of given equations, which is ſomewhat improved in the latter editions. 8. In the ſame book are aſſumed $\frac{az^n+bz^{n-1}+\&c.}{pz^m+qz^{m-1}+\&c.}$ and $\frac{Az^{n'}+Bz^{n'-1}+\&c.}{pz^m+qz^{m-1}+\&c.}$, where z is any rational quantity whatever for x and y, the unknown quantities of a given equation of two or more dimenſions. 9. In the Miſcell. Analyt. a biquadratic ($x^4+2px^3=qx^2+rx+s$, of which no term is deſtroyed) is reduced to a quadratic ($x^2+px+n=\sqrt{p^2+2n+q}\,x+\sqrt{s+n^2}$;) and in the ſecond edition of it, printed in the years 1767, 1768, 1769, and publiſhed in the beginning of the year 1770, the values of n are found $\frac{\alpha\beta+\gamma\delta}{2}$, $\frac{\alpha\gamma+\beta\delta}{2}$, and $\frac{\alpha\delta+\beta\gamma}{2}$; and the ſix values of $\sqrt{p^2+2n+q}$ reſpectively $\frac{\alpha+\beta-\gamma-\delta}{2}$, $\frac{\alpha+\gamma-\beta-\delta}{2}$, $\frac{\alpha+\delta-\beta-\gamma}{2}$, and their negatives; and the ſix values of $\sqrt{s+n^2}$ reſpectively $\frac{\alpha\beta-\gamma\delta}{2}$, $\frac{\alpha\gamma-\beta\delta}{2}$, $\frac{\alpha\delta-\beta\gamma}{2}$, and their negatives. 10. From a given biquadratic ($y^4+qy^2+ry+s=0$) by aſſuming $y^2+ay+b=v$ and a and b ſuch quantities as to make the ſecond and fourth terms of the reſulting equations to vaniſh, there reſults an equation ($v^4+Av^2+B=0$) of the formula of a quadratic. Mr. DE LA GRANGE has aſcribed this reſolution to Mr. TSCHIRNHAUSEN; but in the Leipſic Acts the reſolution of a cubic is given by Mr. TSCHIRNHAUSEN, but not of a biquadratic: his general deſign ſeems to be the extermination of all the terms.

11. Mr.

11. Mr. EULER or Mr. DE LA GRANGE found, that if α be a root of the equation $x^n - 1 = 0$, where n is a prime number, $\alpha, \alpha^2, \alpha^3, \ldots \alpha^{n-1}, 1$ will be (n) roots of it. More on a similar subject has been added in the last edition of the Medit. Algebr. 12. It is observed in the Miscell. Analyt. that CARDAN's or SCIPIO FERREUS's resolution of a cubic is a resolution of three different cubic equations; and in the Medit. Algeb. 1770, the three cubics are given, and the rationale of the resolution (for example: if α, β, and γ, be the roots of the cubic equation $x^3 + qx - r = 0$, then is given the function of the above roots, which are the roots of the reducing equation $z^6 - rz^3 = q^3$); and also the rationale of the common resolution of biquadratics. 13. It is asserted in the Miscell. that if the terms ($My^n + by^{n-1}x + cy^{n-2}x^2 +$ &c. and $Ny^m + By^{m-1}x + Cy^{m-2}x^2 +$ &c.) of two equations of n and m dimensions, which contain the greatest dimensions of x and y have a common divisor, the equation whose root is x or y, will not ascend to $n \times m$ dimensions; and if the equation, whose root is x or y, ascends to $n \times m$ dimensions, the sum of its roots depends on the terms of n and $n-1$ dimensions in the one, and m and $m-1$ dimensions in the other equation, &c. It is also asserted, in the Miscell. that if three algebraical equations of n, m, and r dimensions contain three unknown quantities x, y, and z, the equation, whose root is x or y or z, cannot ascend to more than $n \cdot m \cdot r$ dimensions. 14. Mr. BEZOUT has given two very elegant propositions for finding the dimensions of the equation whose root is x or y, &c.; where x, y, &c. are unknown quantities contained in two or more (b) algebraical equations of π, ρ, σ, &c. dimensions, and in which some of the unknown quantities do not ascend to the above π, ρ, σ, &c. dimensions

dimensions respectively. In demonstrating these propositions he uses one (amongst others) before given by me (*viz.* if an equation of n dimensions contains m unknown quantities, the number of different terms which may be contained in it will be $\overline{n+1} \cdot \frac{n+2}{2} \cdot \frac{n+3}{3} \cdot \cdot \frac{n+m}{m}$). In the Medit. 1770 there is given a method of finding in many cases the dimensions of the equation, whose root is x or y, &c.; from which one, if not both, of the above-mentioned cases may more easily be deduced, and others added. 15. In the Medit. 1770. is observed, that if there be n equations containing m unknown quantities, where n is greater than m, there will be $n-m$ equations of conditions, &c. 16. In the Miscell. is given and demonstrated the subsequent proposition; *viz.* if two equations contain two unknown quantities x and y, in which x and y are similarly involved; the equation, whose root is x or y will have twice the number of roots which the equation, whose root is $x+y$, x^2+y^2, &c. has. In the Medit. 1770 the same reasoning is applied to equations, which have two, three, four, &c, quantities similarly involved. 17. Mr. DE LA GRANGE has done me the honour to demonstrate my method of finding the number of affirmative and negative roots contained in a biquadratic equation. A demonstration of my rule for finding the number of affirmative, negative, and impossible roots contained in the equation $x^n + Ax^m + B = 0$ is also omitted, on account of its ease and length. From the Medit. the investigation of finding the true number of affirmative and negative roots appears to be as difficult a problem as the finding the true number of impossible roots; and it further appears, that the common methods in both cases can seldom be depended on. But their faults lie on different sides, the one generally finds too many, the other too few. 18. In the Medit. 1770, from the number of impossible roots in a given equation ($x^n - px^{n-1} + \&c. = 0$) is found the number of impossible roots in an equation, whose roots (v) have any assignable relation to the roots of a given equation; and examples are given in the relation ($nx^{n-1} - \overline{n-1}px^{n-2} + \&c. = v$); and in an equation, whose roots are the squares of the differences of the roots of the given equation. 19. It is observed in the Medit. 1770, that in two or more equations, having two or more unknown quantities, the same irrationality will be contained in the correspondent values of each of the unknown quantities, unless two or more values of one of them are equal, &c. The same observation is also applied to the coefficients of an equation deduced from a given equation. 20. In the Miscell. was published a new method of exterminating, from a given equation, irrational quantities, by finding the the multipliers, which, multiplied into it, give a rational product. 21. In the Medit. 1770, are given the different resolutions of a certain quantity $(a^2 + rb^2)^{2m+1}$ and $(a^2 + rb^2)^{2m+2}$ into quantities of the same kind. 22. Mr. DE LA GRANGE has very elegantly demonstrated Mr. WILSON's celebrated property of prime numbers contained in my book. In the last edition of the Medit. the same property is demonstrated, and some similar ones added. 23. In the Miscell. is given a method of finding all the integral correspondent values of the unknown quantities of a given simple equation, having two or more unknown quantities; and, in the Medit. 1770, are given methods of reducing simple and other algebraical equations into one, so that some unknown quantities may be exterminated; and if the unknown quantities of the resulting equations be integral or rational,

rational, the unknown quantities exterminated may alſo be integral or rational. 24. In the Medit. are given rules for finding the different and correſpondent roots of an equation, whoſe reſolution is given. 25. Mr. DE LA GRANGE has recommended my new transformation of equations, publiſhed in the Miſcell. which perhaps is not leſs general nor elegant than any yet publiſhed; and in the Meditat. 1770 is given a method very uſeful in finding the co-efficients.

If either here, or in the preface to the Medit. Algebraicæ, I have aſcribed to myſelf any algebraical, or in the properties of curve lines any geometrical, or in the Medit. Analyt. any analytical invention, which has been before publiſhed by any other perſon, I can only plead ignorance of it, and ſhall on the very firſt conviction acknowledge it.

I muſt further add, that I have been able to carry my algebraical improvements into geometry; for from them, with ſome geometrical principles added, I have (unleſs I am deceived) deduced as many new properties of conic ſections and curve lines as have been publiſhed by any one ſince the great geometrician APOLLONIUS.

XXIX. *An Account of a remarkable Frost on the 23d of* June, 1783. *In a Letter from the Rev. Sir* John Cullum, *Bart. F. R. S. and S. A. to Sir* Joseph Banks, *Bart. P. R. S.*

Read May 27, 1784.

DEAR SIR, Hardwick-house, Nov. 10, 1783.

WHEN I had the pleasure of seeing you in London, in the autumn, and mentioned a frost that happened in my neighbourhood on the 23d of last June, you expressed a desire of receiving some particulars about it. I therefore now send you some memorandums which I made at the time.

About six o'clock, that morning, I observed the air very much condensed in my chamber-window; and, upon getting up, was informed by a tenant, who lives close to my house, that finding himself cold in bed, about three o'clock in the morning, he looked out at his window, and to his great surprise saw the ground covered with a white frost: and I was afterwards assured, upon indubitable authority, that two men at Barton, about three miles off, saw between three and four o'clock that morning, in some shallow tubs, ice of the thickness of a crown-piece, and which was not melted before six.

This unseasonable frost produced some remarkable effects. The aristæ of the barley, which was coming into ear, became brown and withered at their extremities, as did the leaves of the oats; the rye had the appearance of being mildewed; so that

that the farmers were alarmed for thofe crops. The wheat was not much affected. The larch, Weymouth pine, and hardy Scotch fir, had the tips of their leaves withered; the firft was particularly damaged, and made a fhabby appearance the reft of the fummer. The leaves of fome afhes, very much fheltered in my garden, fuffered greatly. A walnut-tree received a fecond fhock (the firft was from a fevere froft on the 26th of May) which completed the ruin of its crop. Cherry-trees, a ftandard peach-tree, filbert and hafel-nut trees, fhed their leaves plentifully, and littered the walks as in autumn. The barberry-bufh was extremely pinched, as well as the hypericum perforatum and hirfutum: as the two laft are folftitial, and rather delicate plants, I wondered the lefs at their fenfibility; but was much furprifed to find, that the vernal black-thorn and fweet violet, the leaves of which one would have thought muft have acquired a perfect firmnefs and ftrength, were injured full as much. All thefe vegetables appeared exactly as if a fire had been lighted near them, that had fhrivelled and difcoloured their leaves;

—— *penetrabile frigus adurit.*

At the time this havock was made among fome of our hardy natives, the exotic mulberry-tree was very little affected; a fig-tree, againft a north-weft wall, remained unhurt, as well as the vine, on the other fide, though juft coming into bloffom. I fpeak of my own garden, which is high; for in the low ones about Bury, that is but a mile off, the fig-trees, in particular, were very much cut: and, in general, all thofe gardens fuffer more by froft than mine.

Some weather, that was cold for the time of year, had preceded this froft. On the 21ft the thermometer had, at no time of the day, rifen to 60°; on the 22d, at ten at night, it

 had

had ſunk to 50°. On the laſt day, and on the 23d, diſappeared that dry haze, which had taken place ſome days before, and continued to blot out the face of the ſun for ſo long a time afterwards. After ſun-ſet on the 24th it appeared again, and the next day the leaves of many vegetables were covered with a clammy ſweetneſs.

The above ſlight notes were taken in my garden and its environs; and I wiſh they may afford you the ſmalleſt entertainment. If you ſhould think them worth the attention of the Royal Society, diſpoſe of them accordingly. So ſevere a froſt, at ſo advanced a ſeaſon, is certainly not one of the leaſt remarkable among the atmoſpherical phænomena of this year.

I remain, dear Sir,

Your much obliged and faithful ſervant,

JOHN CULLUM.

XXX. *On a new Method of preparing a Test Liquor to shew the Presence of Acids and Alkalies in chemical Mixtures. By Mr.* James Watt, *Engineer; communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read May 27, 1784.

THE syrop of violets was formerly the test of the point of saturation of mixtures of acids and alkalies, which was principally used; but since the late improvements in chemistry it has been found not to be sufficiently accurate, and the infusion of tournesol, or of an artificial preparation called litmus, have been substituted in the place of it.

The infusion of litmus is blue, and becomes red with acids. It is sensible to the presence of one grain of common oil of vitriol, though it be mixed with 100000 grains of water; but as this infusion does not change its colour on being mixed with alkaline liquors, in order to discover whether a liquor be neutral or alkaline, it is necessary to add some vinegar to the litmus, so as just to turn the infusion red, which will then be restored to its blue colour, by being mixed with any alkaline liquor. The blue infusion of litmus is also a test of the presence of fixed air in water, with which it turns red, as it does with other acids.

The great degree of sensibility of this test would leave very little reason to search for any other, were there reason to believe that it is always a test of the exact point of saturation of acids and alkalies, which the following fact seems to call in question.

I have observed, that a mixture of phlogisticated nitrous acid with an alkali will appear to be acid, by the test of litmus, when other tests, such as the infusion of the petals of the scarlet rose, of the blue iris, of violets, and of other flowers, will shew the same liquor to be alkaline, by turning green so very evidently as to leave no doubt.

At the time I made this discovery, the scarlet roses and several other flowers, whose petals change their colour by acids and alkalies, were in flower. I stained paper with their juices, and found that it was not affected by the phlogisticated nitrous acid, except in so far as it acted the part of a neutralizing acid; but I found also, that paper, stained in this manner, was by no means so easily affected by acids of any kind as litmus was, and that in a short time it lost much of that degree of sensibility it possessed. Having occasion in winter to repeat some experiments, in which the phlogisticated nitrous acid was concerned, I found my stained paper almost useless. I was, therefore, obliged to search for some substitute among the few vegetables which then existed in a growing state; of these I found the red cabbage (*brassica rubra*) to furnish the best test, and in its fresh state to have more sensibility both to acids and alkalies than litmus, and to afford a more decisive test, from its being naturally blue, turning green with alkalies, and red with acids; to which is joined the advantage of its not being affected by phlogisticated nitrous acid any farther than it acts as a real acid.

To extract the colouring matter, take those leaves of the cabbage, which are freshest, and have most colour; cut out the larger stems, and mince the thin parts of the leaves very small; then digest them in water, about the heat of 120 degrees, for

 a few

a few hours, and they will yield a blue liquor, which, if used immediately as a test, will be found to possess great sensibility. But, as this liquor is very subject to turn acid and putrid, and to lose its sensibility, when it is wanted to be preserved for future use the following processes succeed the best.

1. After having minced the leaves, spread them on paper, and dry them in a gentle heat; when perfectly dry, put them up in glass bottles well corked; and when you want to use them, acidulate some water with vitriolic acid, and digest, or infuse, the dry leaves in it until they give out their colour; then strain the liquor through a cloth, and add to it a quantity of fine whiting or chalk, stirring it frequently until it becomes of a true blue colour, neither inclining to green nor purple; as soon as you perceive that it has acquired this colour, filter it immediately, otherwise it will become greenish by longer standing on the whiting.

This liquor will deposite a small quantity of gypsum, and by the addition of a little spirit of wine will keep good for some days, after which it will become a little putrid and reddish. If too much spirit is added, it destroys the colour. If the liquor is wanted to be kept longer, it may be neutralized by means of a fixed alkali instead of chalk.

2. But as none of these means will preserve the liquor long without requiring to be neutralized afresh, just before it is used; and as the putrid and acid fermentation which it undergoes, and perhaps the alkalies or spirit of wine mixed with it, seem to lessen its sensibility; in order to preserve its virtues while it is kept in a liquid state, some fresh leaves of the cabbage, minced as has been directed, may be infused in a mixture of vitriolic acid and water, of about the degree of acidity of vinegar; and it may be neutralized, as it is wanted, either by means of chalk,

on

or of the fixed or volatile alkali. But it is necessary to observe, that if the liquor has an excess of alkali, it will soon lose its colour, and become yellow, from which state it cannot be restored; therefore care should be taken to bring it very exactly to a blue, and not to let it verge towards a green *.

3. By the same process I have made a red infusion of violets, which, on being neutralized, forms at present a very sensible test; but how long it will preserve its properties I have not yet determined. Probably the coloured infusions of other flowers may be preserved in the same manner, by the antiseptic power of the vitriolic acid, so as to lose little of their original sensibility. Paper, fresh stained with these tests in their neutral state, has sufficient sensibility for many experiments; but the alum and glue which enter into the preparation of writing-paper seem in some degree to fix the colour; and paper which is not sized becomes somewhat transparent, when wetted, which renders small changes of colour imperceptible; so that where accuracy is required, the test should be used in a liquid state †.

* Since writing the above, I have found, that the infusions of red cabbages and of various flowers in water acidulated by means of vitriolic acid, are apt to turn mouldy in the summer season, and also that the moulding is prevented by the addition of spirits of wine. The quantity of spirit which is necessary for this purpose I have not been able to ascertain; but I add it by little at a time, until the progress of the moulding is prevented.

† I have found, that the petals of the scarlet rose, and those of the pink-coloured lychnis, treated in this manner, afford very sensible tests.

XXXI. *An Account of a new Plant, of the Order of* Fungi. *By* Thomas Woodward, *Esq*; *communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read June 10, 1784.

Plantæ novæ Deſcriptio — an Genus novum?

Radices paucæ; tenues; albidæ.

Volva ovata; duplex, mucilagine interpoſita; ſubalbida.

Stipes, e volva interiore ſurgens, ſublignoſus; cavus; cortice lacerato veſtitus; ſubfuſcus.

Capitulum, ſtipitis ſummitati inſidens, reflexum; ſubtus campanulatum, glabrum; ſuperne pulverulentum, et, e pulveris craſſitie, globiforme; volvæ ruptæ ſummitatem, minime adhærentem, in ſe gerens.

Pulvis ſphæricus; ſemipellucidus; luteo-fuſcus.

THIS extraordinary vegetable production ariſes from a volva, which is buried ſix or eight inches deep in dry ſandy banks; and, conſequently, it is extremely difficult to detect it in its earlieſt ſtate. At its firſt appearance above ground, the powdery head is covered with a looſe campanulated cap, which does not adhere by any the ſmalleſt filaments; and which, I ſuppoſe, to be the upper part of the volva, as both always appear ragged when taken up. When the plant is taken up immediately on its appearing above ground, the ſtem is about ſix or eight inches long; and, as well as the volva, replete with

with mucilage, making it much heavier than when it has attained its full growth. This is the ſtate to which the deſcription given above refers. The duſt is now perfectly formed, and is diſperſed by the ſlighteſt touch, or by the wind. A great alteration ſoon takes place, as it now proceeds very rapidly, and in a few days attains the ſummit of its growth, which is from nine to fifteen inches, more than half being generally buried in the ground. The ſtem becomes woody, though hollow, the bark ſtill more ragged, and the whole plant much lighter, both volva and ſtem being now quite dry, and free from mucilage. The wind and ſhowers ſoon diſperſe the greateſt part of the duſt; and at length the ſtalk appears with a naked, coriaceous, campanulated pileus, and conſiderably bleached, in colour and appearance not unlike a dry ſtalk of hemp. In this ſtate ſome of them are now to be found (Aug. 28, 1783) with plants of this year riſing near them.

Mr. HUMPHREYS, of Norwich, who firſt found this very extraordinary plant, met with it only in the ſtate laſt deſcribed, and without diſcovering the volva; ſo that no judgement of it could be formed. It has been taken by ſome perſons for a decayed or abortive agaric; but that opinion could not be maintained by any one who had ſeen it in its recent ſtate.

I firſt met with it in February or March 1783 in its dry and withered ſtate; but as it was ſuſpected, though with little appearance of reaſon, to be a decayed Agaricus procerus, I wiſhed to examine the root carefully, in order to obſerve whether it was bulbous. The bulb of the Agaricus procerus is ſcarcely hidden under the ſurface, and I was much ſurpriſed at the depth to which I was obliged to ſearch for the root of

this

this plant; at length, however, removing the earth carefully to the depth of feven or eight inches, I met with it, and to my great pleafure and furprife, on raifing the plant, I difcovered the volva, which was fo unlike the fugitive one of the agaric, that I was immediately convinced it muft be fomething new.

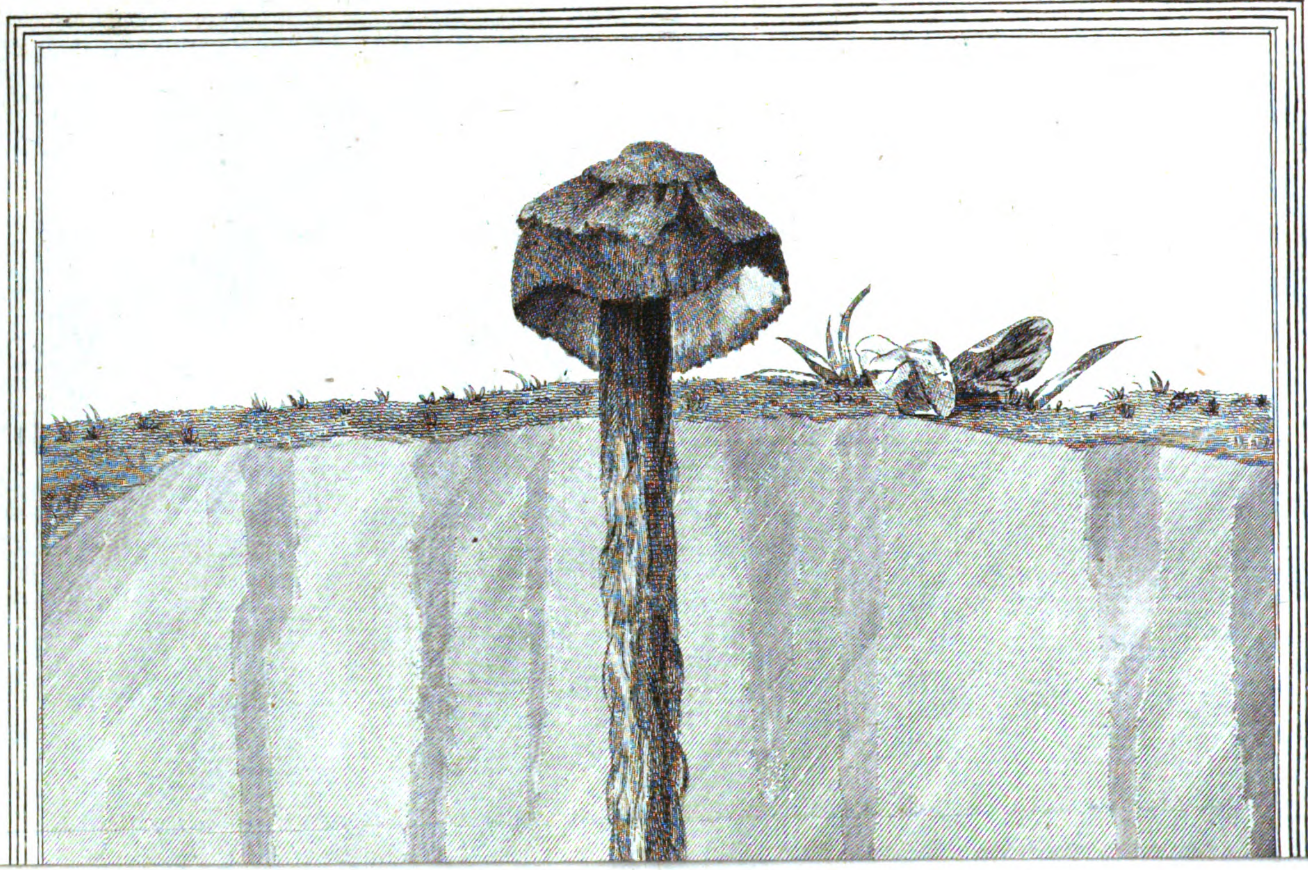
An account of this was directly fent to Mr. DICKSON, of Covent-Garden, an able botanift, and diligent enquirer after the clafs Cryptogamia. Mr. DICKSON, who had before feen it in the ftate in which it was found by Mr. HUMPHREYS, but could make nothing of it, though thoroughly convinced it was no agaric, immediately requefted that I would watch the fpot, and endeavour to detect the plant in its earlieft appearance. I communicated this to my neighbour Mr. STONE, a moft diligent and fkilful botanift, who firft reftored the Lycoperdon coliforme; and we determined to examine the fpot carefully together, from the month of Auguft downward. About the middle of Auguft we firft difcovered a plant juft arifen, which was fent to Mr. DICKSON, and a full defcription of which is before given; but though we have daily vifited the fpot fince, we have never been able to find it again in fo young a ftate; for fo rapid appears to be its growth, that we have found plants of two or three inches height above the ground, the ftems of which had loft part of their mucilage, where the day before none had been vifible. We have three or four times attempted to difcover the volva in its earlieft ftate, by removing the earth carefully near the old ftems of the preceding year; but this has been without fuccefs: and there is little hope of fucceeding in it, as the volva lies very deep in the ground, and the plant arifes at fuch various times.

This plant agrees with the genus Phallus in its volva, which has a double coat replete with mucilage; and its ſtipes crowned with a reflexed pileus. But it more nearly approaches the genus Lycoperdon, by its head covered with a thick duſt, contained in a ſubſtance of a ſpongy appearance, and by the form of the duſt, which agrees perfectly with that of moſt of the true lycoperdons, when examined in the microſcope. To this genus it muſt at preſent probably be referred, though the total want of an exterior coat prevents its agreeing with it ſo perfectly as it ought.

The Mucor * ſepticus of HUDSON and LIGHTFOOT (Mucor ovatus of SCHÆFFER); the Mucor * butyraceus of SCHÆFFER (194.), not taken notice of by either HUDSON or LIGHTFOOT, but which I have often found here; and the Lycoperdon * epidendrum of LIGHTFOOT, which I ſuppoſe to be what HUDSON calls Lycoperdon epiphyllum, as he has referred to the ſame plate of SCHÆFFER (193. Mucor fragiformis); have all ſome affinity with the fructification of this plant; and the more ſo, if we ſuppoſe the head to be at firſt covered with a mucilage, which afterwards turns to a duſt; but this will hardly be admitted, as the plant ſent to Mr. DICKSON had the duſt perfectly formed, though the volva and ſtem were both replete with mucilage. But we cannot admit it to agree with any of theſe laſt mentioned plants, as they have all an exterior coat, though very fugitive, of which this ſeems entirely deſtitute. We may add, that they are all very fugitive productions;

* I cannot help obſerving that, in my opinion, HALLER has done more rightly in making theſe into a new genus (Filago), than our botaniſts, who have jumbled them with the genera Lycoperdon and Mucor, to which they have no great affinity any more than the Sphæria of HALLER, likewiſe very improperly ranked with the Lycoperdons and Clavariæ.

7 whereas



whereas this, though ſoon arriving at maturity, is of a woody and permanent ſtructure.

P. S. In a letter Mr. Dickson received from Mr. Woodward, Feb. 12, 1784, he informs him, that he is quite convinced by ſome late obſervations, that the above-mentioned plant frequently comes to a ſtate of perfection before it reaches the ſurface. The only difference to be obſerved is, that the duſt in that caſe is of a darker colour, which he ſuppoſes is owing to its not being expoſed to the air.

XXXII. *Experiments to investigate the Variation of Local Heat. By* James Six, *Esq.*; *communicated by the Rev.* Francis Wollaston, *LL.B. F. R. S.*

Read June 10, 1784.

BEING desirous of investigating the variation of local heat, I made the following experiments.

On the 4th of September, 1783, I placed thermometers in three different stations; one on the top of the high tower of Canterbury Cathedral, about 220 feet from the ground; another at the bottom of the same tower, at about 110 feet; and a third in my own garden *, not more than six feet from the ground. They were all carefully exposed to the open air in a shady northern aspect; the lowest was as little liable to be affected by the reflection of the sun's rays as the elevation would permit, the second still less, and the highest not at all. They continued unremoved in their several places, where I visited them daily for the space of three weeks, and minuted down the greatest degree of heat and cold that happened each day and night in their respective stations †.

* This garden is situate not far from the Cathedral, at the extremity of the buildings on the north side of the city.

† The thermometers here made use of were constructed to shew the greatest degree of heat and cold which happened in the observer's absence (described Phil. Trans. vol. LXXII. part I.), which rendered them particularly convenient on this occasion. They had hung together for some time, and seldom differed half a degree from each other.

By

By these observations it appears, see Table I. that, notwithstanding some irregularities, the heat of the days at the lowest station always exceeded that at the middle, and still more the heat at the upper station. As in many instances the higher regions of the atmosphere have been found to be colder than the lower, and the thermometer in the garden was more liable to be heated by the reflection of the sun's rays from the earth than the upper ones, a difference of this kind might have been expected. But I was greatly surprised to find the cold of the night at the lowest, not only equal to, but, very frequently, exceeding the cold at the higher stations. As I wished to know, whether these variations would continue the same in the winter, when the weather was colder; and whether a thermometer, placed at some distance from the city, having an elevation equal to that on the top of the Cathedral tower, would agree with it; on the 19th of December, 1783, I disposed the three thermometers in the following manner: one in my garden; one on the top of the high tower, as before; and the third on the top of St. Thomas's Hill, about a mile distant from the city, where, at fifteen feet from the ground, it was nearly level with that on the Cathedral tower. Table II. contains the observations that were then made *. The weather at this time proving cold, favoured the experiment; and I now found the several thermometers nearly agreeing with each other in the day-time: but in the night, the cold at the lower station exceeded the cold at the higher ones rather more than it did in the month of September, when the weather was warmer.

* The few omissions in this Table were occasioned by the severity of the cold preventing my attending at a proper time the thermometers, which were at a considerable distance from each other.

At

At the time of taking these thermometrical observations, I likewise noted the different dispositions of the atmosphere in other respects: such as the pressure, moisture, and dryness of the air; force and direction of the winds; quantity of rain; whether the appearances of the sky were clear or cloudy, &c. as I apprehended the local variation of the thermometers might, in a certain degree, correspond with some particular change in the state of the atmosphere.

The event answered my expectation in a singular manner in respect to the nocturnal variation; for it generally happened, that when the sky was dark and cloudy, whatever was the condition of the atmosphere with relation to the other particulars above enumerated, the thermometers agreed pretty nearly with each other; but, on the contrary, whenever the sky became clear, the cold of the night at the lowest station in the garden constantly exceeded the cold at the top of the Cathedral tower, where the instrument was placed 220 feet from the ground, entirely exposed to the open air, wind, dews, and rain, in a shady northern aspect.

The local variations in the day-time seemed to be regulated by the general degree of heat only, without being affected by any other particular disposition of the atmosphere, or the clearness or cloudyness of the sky, as the nocturnal variations were. In the month of September, when the glasses rose from 60° to 70°, the heat at the lower station constantly exceeded the heat at the upper station; and in some measure proportionally, as the weather was hotter *. In December and January, when

* As the heat at the lower station exceeded the heat at the upper ones, when the weather was hot; and equally so, whenever the sky was cloudy, as well as when it was clear; it appears, that the glass at the lower station was not materially affected by the reflection of the sun's rays from the earth, as at first I apprehended it would be.

from

from below 30° they ſeldom roſe to 40°, the local variation in the day-time nearly ceaſed, or was found in very ſmall degrees inclining ſometimes one way, ſometimes the other.

That the clearneſs of the ſky ſhould contribute to the coolneſs of the air in the night, is not at all ſurpriſing; but that, whenever the ſky becomes clear, the cold ſhould ſeem to ariſe from the earth, and be found in the greateſt degree, as long as it continues clear, in the loweſt ſituation, ſeems a little extraordinary: this, however, appeared to be the caſe, both in the warmer as well as in the colder weather, during the whole time theſe obſervations were taken, and remarkably ſo on the following days. On the firſt of January the weather was cold, the ſky cloudy, the glaſſes in the night were at 20°, and in the day at 34°: the wind which had been at S.E. the day before, changed in the evening to S. and brought on a thaw. On the ſecond of January clouds and miſty rain darkened the ſky all day; the wind blew briſkly at S.W.; the glaſſes in the night were at 32°½, in the day at 40°. On the third of January the clouds and rain continued, the weather growing ſtill warmer; wind at S.W. by S.; the glaſſes in the night were at 36°, in the day at 45°½. Theſe three days the weather gradually became warmer; and, while the ſky remained darkened by clouds, all the glaſſes in their ſeveral ſtations nearly agreed with each other. About noon, on the third of January, the ſky becoming clear, the air grew cooler; and going into my garden, about eight o'clock in the evening, I perceived the ſurface of the ground, which had been wet by the rain in the forenoon, began to be frozen. Looking immediately at the thermometer, I ſaw the mercury at 33°½; and obſerving a piece of wet linen hanging near the glaſs, not five feet from the ground, I took it into my hand, and found it not in the leaſt frozen; by which it appeared, that

that the degree of cold which had frozen the furface of the ground, had not then afcended to the glafs, nor to the linen, and confequently had not been communicated to the air five or fix feet above the earth. The next day I found, as I expected, a confiderable local variation; the index for the cold of the night in the garden being at 32°, that on the hill being at 35°½, and that on the top of the tower at 37°½ *. Probably the weather did not continue clear the whole night; if it had, it is likely the degrees of cold would have been found proportionally greater at every ftation. On the morning of the 4th there fell a mifty rain, which continued only till noon, when the fky became clear again, and continued fo till the 7th; during which time the nocturnal heights of the thermometers differed confiderably from each other; but on the fky's becoming cloudy, the local variation ceafed.

Thermometrical obfervations, made under the fame circumftances in refpect to the feafon of the year, place, and fituation †, may probably be liable to fimilar local varia-

* It is remarkable, that the thermometer on St. Thomas's hill did not vary fo much from that in the garden, as that did which was on the Cathedral tower, although thefe two elevated glaffes were within three feet of a perfect level with each other; the variations, however, as often as they happened, inclined the fame way. The reafon of this might probably be, that although the glafs on the hill was at an equal altitude with that on the tower, in refpect to the ground on which the Cathedral ftands; yet the former was only 15 feet, while the latter was 220 feet from the ground.

† Situation in regard to hill or valley. The valley in which Canterbury ftands is at that place about a mile in breadth, opening to the N.E.; the hills on either fide do not rife very fudden, nor very high; the river Stour, divided into branches, paffes through the city, and, about fourteen miles below, empties itfelf into the fea, which wafhes the coaft from the NN.W. round by the E. to the S.; diftant from the city at different places from fix to fixteen miles.

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tions: to thofe who make them, the refult of thefe experiments may be of fome ufe. If convenient opportunity offered, I fhould be glad, by the affiftance of friends, to try the local difference of heat and cold in more diftant, as well as more elevated, fituations.

By experiments of this kind it may poffibly in fome meafure be found, how far evaporations from the earth, at certain times, or vapours afcending, defcending, or meeting, in different parts of the atmofphere, may increafe or diminifh the heat of the air in thofe places: or whether different degrees of heat and cold (fubject however to change) may not be found in different ftrata of air, or vapour, floating in different parts of the atmofphere; or in what degree and proportion, the cold increafes at different altitudes and in different feafons of the year: whether the cold, which is known to be very intenfe in the fummer time on the tops of high mountains, receives a proportional increafe, or be not lefs fubject to variety by the return of winter and fummer, night and day, than what we experience in the plains below.

March 10, 1784. JAMES SIX.

TABLE I.

The greatest daily variation of heat and cold in the atmosphere, fror
1783, taken from three different stations, and compared togethe
tower in Canterbury, 120 feet from the ground; another at the bot
third in a garden, about six feet from the ground. N. B. The noct
night immediately preceding the day to the date of which they are

	Greatest degree of cold in the night.					Greatest degree of heat in the day.				
	Thermometer in the garden.	Thermometer at the bottom of the tower.	Thermometer on the top of the tower.	Difference of garden from bottom of tower.	Difference of garden from top of tower.	Thermometer in the garden.	Thermometer at the bottom of the tower.	Thermometer on the top of the tower.	Difference of garden from bottom of tower.	Difference of garden from top of tower.
Sept.	°	°	°	°	°	°	°	°	°	°
4	50½	—	51	—	−0½	66	61	61	+5	+5
5	48	47¾	47½	+0¼	+0½	62½	61½	61½	+1	+1
6	48½	50	50½	−1½	−2	66½	65¼	64½	+1¼	+2
7	48	49½	49½	−1½	−1½	63½	62½	63	+1	+0½
8	50	51	51	−1	−1	66	62½	62	+3½	+4
9	55½	55½	55½	—	—	65	63	62½	+2	+2½
10	45	47	47½	2	−2½	63¾	59¾	59¾	+4	+4
11	42	45	45½	−3	−3½	63½	62	60½	+1½	+3
12	52½	53½	54	−1	−1½	69	66½	65	+2½	+4
13	45	48	48½	−3	−3½	65	62	62	+3	+3

n the 4th to the 24th of September, 1783, to the
er. One thermometer placed on a e thermometer
tom of the same tower, 110; and a istant, but on
urnal degrees of cold belong to the . N. B. The
placed.

Morning still and foggy; wind began to blow in the forenoon at S.W.; clouds and rain in the afternoon and night; bar. 29.3.
Morning cloudy; heavy rain; clear in the afternoon; wind high at W.N.W.; bar. 29.3.
Morning rained a little; wind very high at S.W. most part of the day; bar. 29.5.
Morning clear; continued so most part of the day; wind very high at W.; bar. 29.8.
Sometimes clear, sometimes cloudy; wind very high at W.; bar. 29.9.
Morning close and cloudy; clear at noon; wind brisk at S.W.; bar. 29.5.
Morning and great part of the day clear; wind high at S.W.; evening clear; bar. 29.6.
Morning clear; cloudy about noon; brisk wind at S.; evening still; bar. 29.8.
Morning cloudy; wind high at S.; evening nd clear; bar. 29.4.
lear; a little rain at noon; cloudy ; wind brisk at S.; bar. 29.0.

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14	57¾	57
15	57	57¼
16	52¾	53
17	51¾	51¾
18	57	57
19	53	54¾
20	56	56
21	44¾	47¾
22	56	57
23	50	49
24	43¾	46

XXXIII. *Account of some Observations tending to investigate the Construction of the Heavens.* *By* William Herschel, *Esq. F. R. S.*

Read June 17, 1784.

IN a former paper I mentioned, that a more powerful instrument was preparing for continuing my reviews of the heavens. The telescope I have lately completed, though far inferior in size to the one I had undertaken to construct when that paper was written, is of the Newtonian form, the object speculum being of 20 feet focal length, and its aperture $18\frac{7}{10}$ inches. The apparatus on which it is mounted is contrived so as at present to confine the instrument to a meridional situation, and by its motions to give the right-ascension and declination of a celestial object in a coarse way; which, however, is sufficiently accurate to point out the place of the object, so that it may be found again. It will not be necessary to enter into a more particular description of the apparatus, since the account I have now the honour of communicating to the Royal Society regards rather the performance of the telescope than its construction.

It would, perhaps, have been more eligible to have waited longer, in order to complete the discoveries that seem to lie within the reach of this instrument, and are already, in some respects, pointed out to me by it. By taking more time I could undoubtedly be enabled to speak more confidently of the *interior construction* of the heavens, and its various *nebulous and sidereal strata* (to borrow a term from the natural historian) of which this paper can as yet only give a few outlines, or rather hints. As an apology, however, for this prematurity, it may be said, that the end of all discoveries being communication, we can never be too ready in giving facts and observations, whatever we may be in reasoning upon them.

Hitherto the sidereal heavens have, not inadequately for the purpose designed, been represented by the concave surface of a sphere, in the center of which the eye of an observer might be supposed to be placed. It is true, the various magnitudes of the fixed stars even then plainly suggested to us, and would have better suited the idea of an expanded firmament of three dimensions; but the observations upon which I am now going to enter still farther illustrate and enforce the necessity of considering the heavens in this point of view. In future, therefore, we shall look upon those regions into which we may now penetrate by means of such large telescopes, as a naturalist regards a rich extent of ground or chain of mountains, containing strata variously inclined and directed, as well as consisting of very different materials. A surface of a globe or map, therefore, will but ill delineate the interior parts of the heavens.

It may well be expected, that the great advantage of a large aperture would be most sensibly perceived with all those objects that require much light, such as the very small and immensely distant fixed stars, the very faint nebulæ, the close and compressed clusters of stars, and the remote planets.

On applying the telescope to a part of the *via lactea*, I found that it completely resolved the whole whitish appearance into small stars, which my former telescopes had not light enough to effect. The portion of this extensive tract, which it has hitherto been convenient for me to observe, is that immediately about the hand and club of Orion. The glorious multitude of stars of all possible sizes that presented themselves here to my view was truly astonishing; but, as the dazzling brightness of glittering stars may easily mislead us so far as to estimate their number greater than it really is, I endeavoured to ascertain this point by counting many fields, and computing, from a mean of them, what a certain given portion of the milky way might contain. Among many trials of this sort I found, last January the 18th, that six fields, promiscuously taken, contained 110, 60, 70, 90, 70, and 74 stars each. I then tried to pick out the most vacant place that was to be found in that neighbourhood, and counted 63 stars. A mean of the first six gives 79 stars for each field. Hence, by allowing 15 minutes of a great circle for the diameter of my field of view, we gather, that a belt of 15 degrees long and two broad, or the quantity which I have often seen pass through the field of my telescope in one hour's time, could not well contain less than fifty thousand stars, that were large enough to be distinctly numbered. But, besides these, I suspected at least twice as many more, which, for want of light, I could only see now and then by faint glittering and interrupted glimpses.

The excellent collection of nebulæ and clusters of stars which has lately been given in the *Connoissance des Temps* for 1783 and 1784, leads me next to a subject which, indeed, must open a new view of the heavens. As soon as the first of these volumes came to my hands, I applied my former 20-feet reflector of 12 inches aperture to them; and saw, with the greatest pleasure, that most of the nebulæ, which I had an opportunity of examining in proper situations, yielded to the force of my light and power, and were resolved into stars. For instance, the 2d, 5, 9, 10, 12, 13, 14, 15, 16, 19, 22, 24, 28, 30, 31, 37, 51, 52, 53, 55, 56, 62, 65, 66, 67, 71, 72, 74, 92, all which are said to be nebulæ without stars, have either plainly appeared to be nothing but stars, or at least to contain stars, and to shew every other indication of consisting of them entirely. I have examined them with a careful scrutiny of various powers and light, and generally in the meridian. I should mention, that five of the above, *viz.* the 16th, 24, 37, 52, 67, are called clusters of stars containing nebulosity; but my instrument resolving also that portion of them which is called nebulous into stars of a much smaller size, I have placed them into the above number. To these may be added the 1st, 3d, 27, 33, 57, 79, 81, 82, 101, which in my 7, 10, and 20-feet reflectors shewed a mottled kind of nebulosity, which I shall call resolvable; so that I expect my present telescope will, perhaps, render the stars visible of which I suppose them to be composed. Here I might point out many precautions necessary to be taken with the very best instruments, in order to succeed in the resolution of the most difficult of them; but reserving this at present too extensive subject for a future opportunity, I proceed to speak of the effects of my last instrument with regard to nebulæ.

My present pursuits, as I observed before, requiring this telescope to act as a fixed instrument, I found it not convenient to apply it to any other of the nebulæ in the *Connoissance des Temps* but such as came in turn; nor, indeed, was it necessary to take any particular pains to look for them, it being utterly impossible that any one of them should escape my observation when it passed the field of view of my telescope. The few which I have already had an opportunity of examining, shew plainly that those most excellent French astronomers, Mess. MESSIER and MECHAIN, saw only the more luminous part of their nebulæ; the feeble shape of the remainder, for want of light, escaping their notice. The difference will appear when we compare my observation of the 98th nebula with that in the *Connoissance des Temps* for 1784, which runs thus: "Nébuleuse sans étoile, "d'une lumière extrêmement foible, au dessus de l'aile boréale "de la Vierge, sur le parallèle et près de l'étoile N° 6. cin-"quième grandeur, de la chevelure de Bérénice, suivant "FLAMSTEED. M. MECHAIN la vit le 15 Mars, 1781." My observation of the 30th of December, 1783, is thus: A large, extended, fine nebula. Its situation shews it to be M. MESSIER's 98th; but from the description it appears, that that gentleman has not seen the whole of it, for its feeble branches extend above a quarter of a degree, of which no notice is taken. Near the middle of it are a few stars visible, and more suspected My field of view will not quite take in the whole nebula. See fig. 1. tab. XVII. Again, N° 53. "Nébuleuse sans étoiles, "découverte au-dessous et près de la chevelure de Bérénice, à "peu de distance de l'étoile quarante-deuxieme de cette constel-"lation, suivant FLAMSTEED. Cette nébuleuse est ronde et "apparente, &c." My observation of the 170th Sweep runs thus: A cluster of very close stars; one of the most beautiful objects I remember to have seen in the heavens. The cluster appears under the form of a solid ball, consisting of small stars, quite compressed into one blaze of light, with a great number of loose ones surrounding it, and distinctly visible in the general mass. See fig. 2.

When I began my present series of observations, I surmised, that several nebulæ might yet remain undiscovered, for want of sufficient light to detect them; and was, therefore, in hopes of of making a valuable addition to the clufters of ftars and nebulæ already collected and given us in the work before referred to, which amount to 103. The event has plainly proved that my expectations were well founded; for I have already found 466 new nebulæ and clufters of ftars, none of which, to my prefent knowledge, have been feen before by any perfon; moft of them, indeed, are not within the reach of the beft common telefcopes now in ufe. In all probability many more are ftill in referve; and as I am purfuing this track, I fhall make them up into feparate catalogues, of about two or three hundred at a time, and have the honour of prefenting them in that form to the Royal Society.

A very remarkable circumftance attending the nebulæ and clufters of ftars is, that they are arranged into ftrata, which feem to run on to a great length; and fome of them I have already been able to purfue, fo as to guefs pretty well at their form and direction. It is probable enough, that they may furround the whole apparent fphere of the heavens, not unlike the milky way, which undoubtedly is nothing but a ftratum of fixed ftars. And as this latter immenfe ftarry bed is not of equal breadth or luftre in every part, nor runs on in one ftraight direction, but is curved and even divided into two ftreams along a very confiderable portion of it; we may likewife expect the greateft variety in the ftrata of the clufters of ftars and nebulæ. One of thefe nebulous beds is fo rich, that, in paffing through a fection of it, in the time of only 36 minutes, I detected no lefs than 31 nebulæ, all diftinctly vifible upon a fine blue fky. Their fituation and fhape, as well as condition, feems to denote the greateft variety imaginable. In another ftratum, or perhaps a different branch of the former, I have feen double and treble nebulæ, varioufly arranged; large ones with

with small, seeming attendants; narrow but much extended, lucid nebulæ or bright dashes; some of the shape of a fan, resembling an electric brush, issuing from a lucid point; others of the cometic shape, with a seeming nucleus in the center; or like cloudy stars, surrounded with a nebulous atmosphere; a different sort again contain a nebulosity of the milky kind, like that wonderful, inexplicable phænomenon about θ Orionis; while others shine with a fainter, mottled kind of light, which denotes their being resolvable into stars. See fig. 3. &c. But it would be too extensive at present to enter more minutely into such circumstances, therefore I proceed with the subject of nebulous and sidereal strata.

It is very probable, that the great stratum, called the milky way, is that in which the sun is placed, though perhaps not in the very center of its thickness. We gather this from the appearance of the Galaxy, which seems to encompass the whole heavens, as it certainly must do if the sun is within the same. For, suppose a number of stars arranged between two parallel planes, indefinitely extended every way, but at a given considerable distance from each other; and, calling this a sidereal stratum, an eye placed somewhere within it will see all the stars in the direction of the planes of the stratum projected into a great circle, which will appear lucid on account of the accumulation of the stars; while the rest of the heavens, at the sides, will only seem to be scattered over with constellations, more or less crowded, according to the distance of the planes or number of stars contained in the thickness or sides of the stratum.

Thus, in fig. 16. (tab. XVIII.) an eye at S within the stratum *a b*, will see the stars in the direction of its length *a b*, or height *c d*, with all those in the intermediate situations, projected into the

 lucid

lucid circle ACBD; while those in the sides *m v*, *n w*, will be seen scattered over the remaining part of the heavens at MVNW.

If the eye were placed somewhere without the stratum, at no very great distance, the appearance of the stars within it would assume the form of one of the less circles of the sphere, which would be more or less contracted to the distance of the eye; and if this distance were exceedingly increased, the whole stratum might at last be drawn together into a lucid spot of any shape, according to the position, length, and height of the stratum.

Let us now suppose, that a branch, or smaller stratum, should run out from the former, in a certain direction, and let it also be contained between two parallel planes extended indefinitely onwards, but so that the eye may be placed in the great stratum somewhere before the separation, and not far from the place where the strata are still united. Then will this second stratum not be projected into a bright circle like the former, but will be seen as a lucid branch proceeding from the first, and returning to it again at a certain distance less than a semi-circle.

Thus, in the same figure, the stars in the small stratum *p q* will be projected into a bright arch at PRRP, which, after its separation from the circle CBD, unites with it again at P.

What has been instanced in parallel planes may easily be applied to strata irregularly bounded, and running in various directions; for their projections will of consequence vary according to the quantities of the variations in the strata and the distance of the eye from the same. And thus any kind of curvatures, as well as various different degrees of brightness, may be produced in the projections.

From appearances then, as I obſerved before, we may infer, that the ſun is moſt likely placed in one of the great ſtrata of the fixed ſtars, and very probably not far from the place where ſome ſmaller ſtratum branches out from it. Such a ſuppoſition will ſatisfactorily, and with great ſimplicity, account for all the phænomena of the milky way, which, according to this hypotheſis, is no other than the appearance of the projection of the ſtars contained in this ſtratum and its ſecondary branch. As a farther inducement to look on the Galaxy in this point of view, let it be conſidered, that we can no longer doubt of its whitiſh appearance ariſing from the mixed luſtre of the numberleſs ſtars that compoſe it. Now, ſhould we imagine it to be an irregular ring of ſtars, in the center nearly of which we muſt then ſuppoſe the ſun to be placed, it will appear not a little extraordinary, that the ſun, being a fixed ſtar like thoſe which compoſe this imagined ring, ſhould juſt be in the center of ſuch a multitude of celeſtial bodies, without any apparent reaſon for this ſingular diſtinction; whereas, on our ſuppoſition, every ſtar in this ſtratum, not very near the termination of its length or height, will be ſo placed as alſo to have its own Galaxy, with only ſuch variations in the form and luſtre of it, as may ariſe from the particular ſituation of each ſtar.

Various methods may be purſued to come to a full knowledge of the ſun's place in the ſidereal ſtratum, of which I ſhall only mention one as the moſt general and moſt proper for determining this important point, and which I have already begun to put in practice. I call it *Gaging the Heavens*, or the *Star-Gage*. It conſiſts in repeatedly taking the number of ſtars in ten fields of view of my reflector very near each other, and by adding their ſums, and cutting off one decimal on the right, a mean of the contents of the heavens, in all the parts which re thus gaged, is obtained. By way of example, I have joined a ſhort table, extracted from the gages contained in my journal, by which it appears, that the number of ſtars increaſes very faſt as we approach the Via Lactea.

N. P. D. 92 to 94.	
R. A.	Gage.
15 10	9.4
15 22	10.6
15 47	10.6
16 8	12.1
16 25	13.6
16 37	18.6

N. P. D. 78 to 80.	
R. A.	Gage.
11 16	3.1
12 31	3.4
12 44	4.6
12 49	3.9
13 5	3.8
14 30	3.6

Thus, in the parallel from 92 to 94 degrees north polar diſtance, and R. A. 15 h. 10′, the ſtar-gage runs up from 9.4 ſtars in the field to 18.6 in about an hour and a half; whereas in the parallel from 78° to 80° north polar diſtance, and R. A. 11, 12, 13, and 14 hours, it very ſeldom riſes above 4. We are, however, to remember, that with different inſtruments the account of the gages will be very different, eſpecially on our ſuppoſition of the ſituation of the ſun in a ſtratum of ſtars. For, let *a b*, fig. 17. be the ſtratum, and ſuppoſe the ſmall circle *g h l k* to repreſent the ſpace into which, by the light and power of a given teleſcope, we may penetrate; and let GHLK be the extent of another portion, which we are enabled to viſit by means of a larger aperture and power; it is evident, that the gages with the latter inſtrument will differ very much in their account of ſtars contained at MN, and at KG or LH; when with the former they will hardly be affected by the change from *m n* to *k g* or *l h*. And this accounts for what a celebrated author ſays concerning the effects of teleſcopes,

scopes, by which we must understand the best of those that are in common use *.

It would not be safe to enter into an application of these, and such other gages as I have already taken, till they are sufficiently continued and carried all over the heavens. I shall, therefore, content myself with just mentioning that the situation of the sun will be obtained, from considering in what manner the star-gage agrees with the length of a ray revolving in several directions about an assumed point, and cut off by the bounds of the stratum. Thus, in fig. 18. let S be the place of an observer; S r r r, S r r r, lines in the planes r S r, r S r, drawn from S within the stratum to one of the boundaries, here represented by the plane AB. Then, since neither the situation of S, nor the form of the limiting surface AB, is given, we are to assume a point, and apply to it lines proportional to the several gages that have been obtained, and at such angles from each other as they may point out; then will the termination of these lines delineate the boundary of the stratum, and consequently manifest the situation of the sun within the same. But to proceed.

If the sun should be placed in the great sidereal stratum of the milky way, and, as we have surmised above, not far from

* On voit avec les télescopes des étoiles dans toutes les parties du ciel, à peu près comme dans la voie lactée, ou dans les nébuleuses. On ne sauroit douter qu'une partie de l'éclat et de la blancheur de la voie lactée, ne provienne de la lumière des petites étoiles qui s'y trouvent en effet par millions; cependant, avec les plus grands télescopes, on n'en distingue pas assés, et elles n'y sont pas assés rapprochées les unes des autres pour qu'on puisse attribuer à celles qu'on distingue la blancheur de la voie lactée, si sensible à la vue simple. L'on ne sauroit donc prononcer que les étoiles soient la seule cause de cette blancheur, quoique nous ne connoissions aucune manière satisfaisante de l'expliquer. Ast. M. De la Lande, § 833.

the

the branching out of a ſecondary ſtratum, it will very naturally lead us to gueſs at the cauſe of the probable motion of the ſolar ſyſtem: for the very bright, great node of the Via Lactis, or union of the two ſtrata about Cepheus and Caſſiopeia, and the Scorpion and Sagittarius, points out a conflux of ſtars manifeſtly quite ſufficient to occaſion a tendency towards that node in any ſtar ſituated at no very great diſtance; and the ſecondary branch of the Galaxy not being much leſs than a ſemi-circle ſeems to indicate ſuch a ſituation of our ſolar ſyſtem in the great undivided ſtratum as the moſt probable.

What has been ſaid in a former paper on the ſubject of the ſolar motion ſeems alſo to ſupport this ſuppoſed ſituation of the ſun; for the apex there aſſigned lies nearly in the direction of a motion of the ſun towards the node of the ſtrata. Beſides, the joining ſtratum making a pretty large angle at the junction with the primary one, it may eaſily be admitted, that the motion of a ſtar in the great ſtratum, eſpecially if ſituated conſiderably towards the ſide fartheſt from the ſmall ſtratum, will be turned ſufficiently out of the ſtraight direction of the great ſtratum towards the ſecondary one. But I find myſelf inſenſibly led to ſay more on this ſubject than I am as yet authoriſed to do; I will, therefore, return to thoſe obſervations which have ſuggeſted the idea of celeſtial ſtrata.

In my late obſervations on nebulæ I ſoon found, that I generally detected them in certain directions rather than in others; that the ſpaces preceding them were generally quite deprived of their ſtars, ſo as often to afford many fields without a ſingle ſtar in it; that the nebulæ generally appeared ſome time after among ſtars of a certain conſiderable ſize, and but ſeldom among very ſmall ſtars; that when I came to one nebula, I generally found ſeveral more in the neighbourhood; that afterwards

wards a considerable time passed before I came to another parcel; and these events being often repeated in different altitudes of my instrument, and some of them at a considerable distance from each other, it occurred to me, that the intermediate spaces between the sweeps might also contain nebulæ; and finding this to hold good more than once, I ventured to give notice to my assistant at the clock, "to prepare, since I expected in a few minutes to come at a stratum of the nebulæ, "finding myself already" (as I then figuratively expressed it) "on nebulous ground." In this I succeeded immediately; so that I now can venture to point out several not far distant places, where I shall soon carry my telescope, in expectation of meeting with many nebulæ. But how far these circumstances of vacant places preceding and following the nebulous strata, and their being as it were contained in a bed of stars, sparingly scattered between them, may hold good in more distant portions of the heavens, and which I have not yet been able to visit in any regular manner, I ought by no means to hazard a conjecture. The subject is new, and we must attend to observations, and be guided by them, before we form general opinions.

Before I conclude, I may, however, venture to add a few particulars about the direction of some of the capital strata or their branches. The well known nebula of Cancer, visible to the naked eye, is probably one belonging to a certain stratum, in which I suppose it to be so placed as to lie nearest to us. This stratum I shall call that of Cancer. It runs from ε Cancri towards the south over the 67 nebula of the *Connoissance des Temps*, which is a very beautiful and pretty much compressed cluster of stars, easily to be seen by any good telescope, and in which I have observed above 200 stars at once in the field of view.

view of my great reflector, with a power of 157. This cluster appearing so plainly, with any good, common telescope, and being so near to the one which may be seen by the naked eye, denotes it to be probably the next in distance to that within the quartile formed by γ, δ, η, θ; from the 67th nebula the stratum of Cancer proceeds towards the head of Hydra; but I have not yet had time to trace it farther than the equator.

Another stratum, which perhaps approaches nearer to the solar system than any of the rest, and whose situation is nearly at rectangles to the great sidereal stratum in which the sun is placed, is that of Coma Berenices, as I shall call it. I suppose the Coma itself to be one of the clusters in it, and that, on account of its nearness, it appears to be so scattered. It has many capital nebulæ very near it; and in all probability this stratum runs on a very considerable way. It may, perhaps, even make the circuit of the heavens, though very likely not in one of the great circles of the sphere: for, unless it should chance to intersect the great sidereal stratum of the milky way before-mentioned, in the very place in which the sun is stationed, such an appearance could hardly be produced. However, if the stratum of Coma Berenices should extend so far as (by taking in the assistance of M. MESSIER's and M. MECHAIN's excellent observations of scattered nebulæ, and some detached former observations of my own) I apprehend it may, the direction of it towards the north lies probably, with some windings, through the great Bear onwards to Cassiopeia; thence through the girdle of Andromeda and the northern Fish, proceeding towards Cetus; while towards the south it passes through the Virgin, probably on to the tail of Hydra and the head of Centaurus. But, notwithstanding I have already fully ascertained the existence and direction of this stratum for more

 than

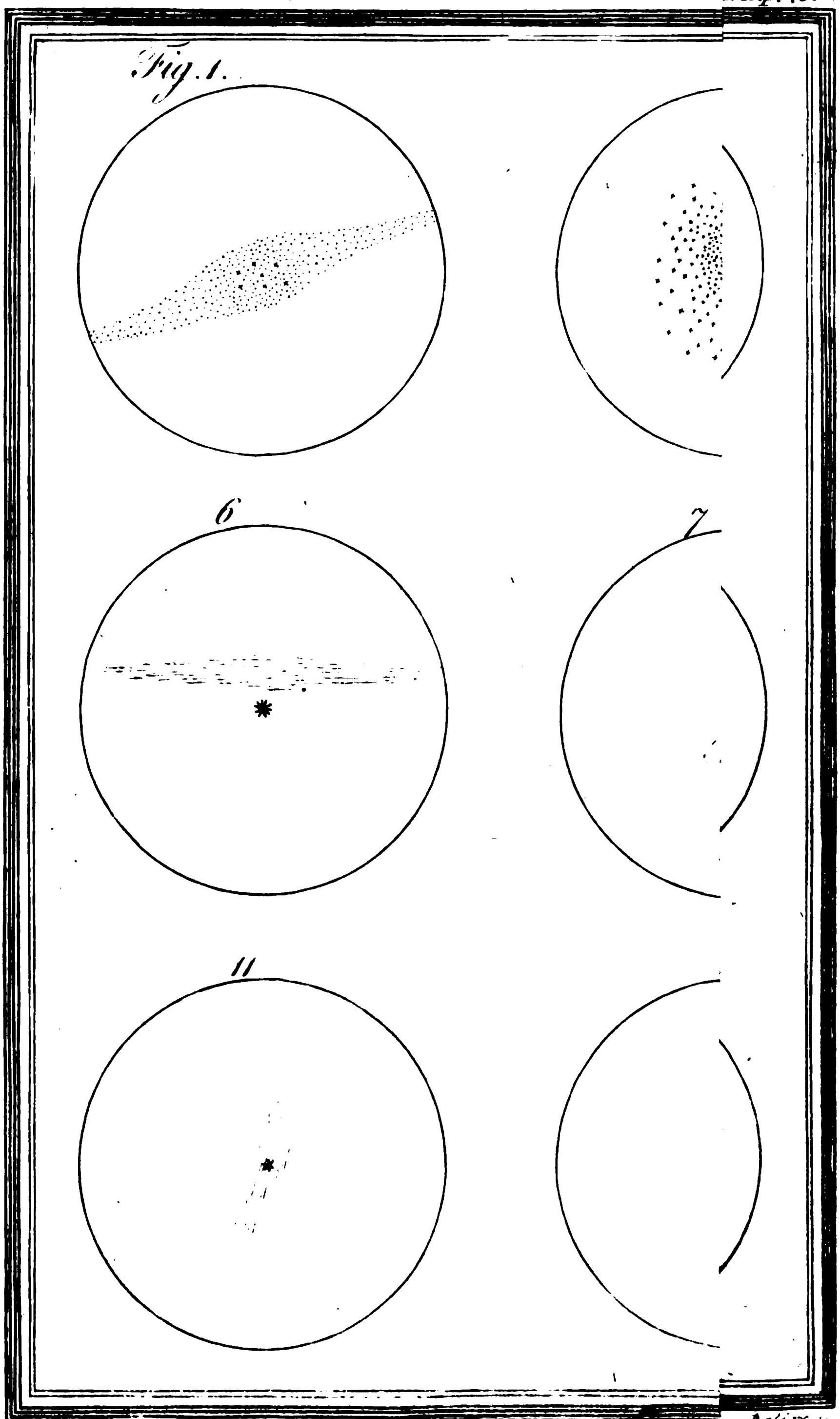


Fig. 1.
6
7
11

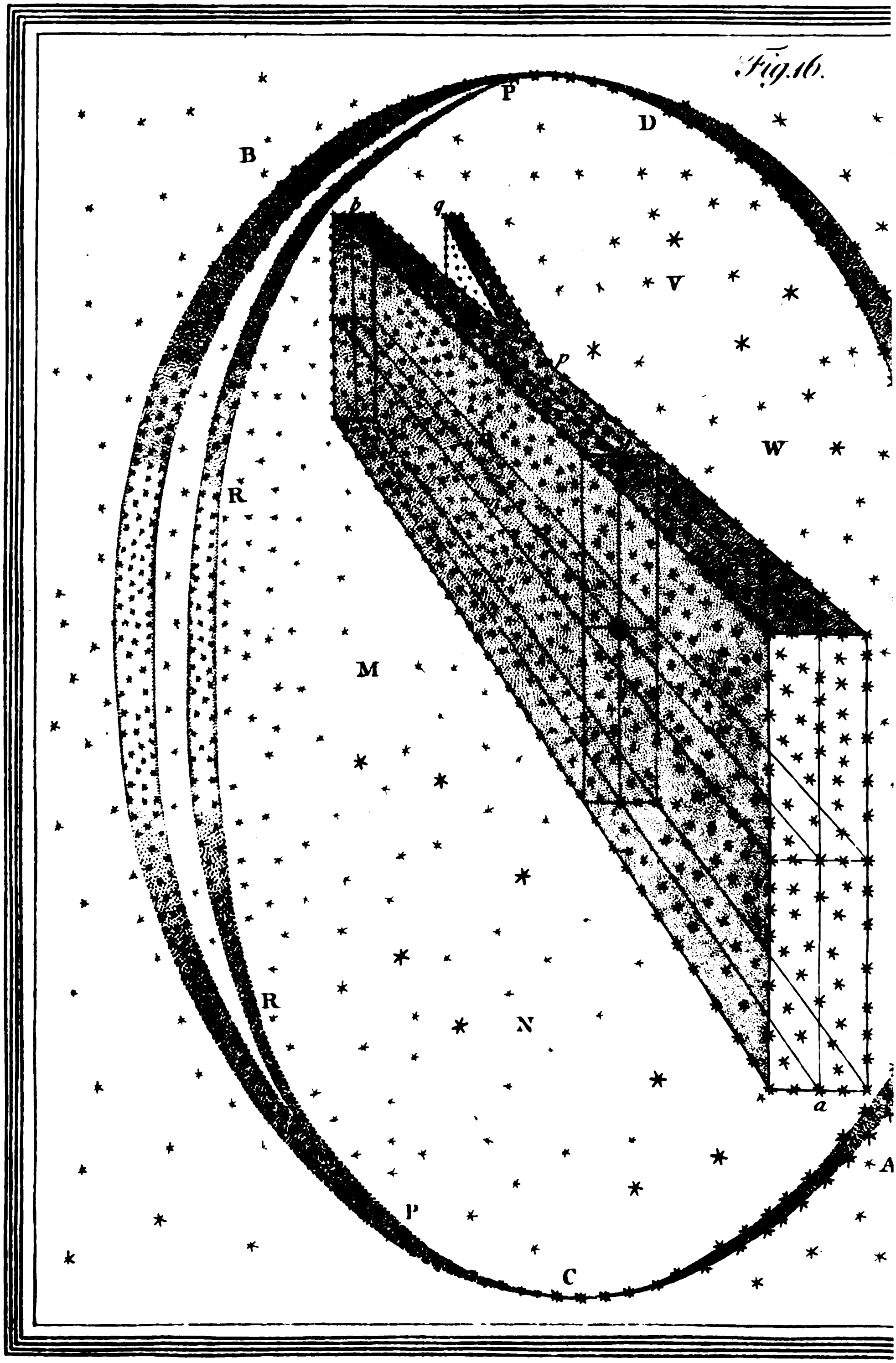


Fig. 16.

than 30 degrees of a great circle, and found it almoſt every where equally rich in fine nebulæ, it ſtill might be dangerous to proceed in more extenſive conjectures, that have as yet no more than a precarious foundation. I ſhall therefore wait till the obſervations in which I am at preſent engaged ſhall furniſh me with proper materials for the diſquiſition of ſo new a ſubject. And though my ſingle endeavours ſhould not ſucceed in a work that ſeems to require the joint effort of every aſtronomer, yet ſo much we may venture to hope, that, by applying ourſelves with all our powers to the improvement of teleſcopes, which I look upon as yet in their infant ſtate, and turning them with aſſiduity to the ſtudy of the heavens, we ſhall in time obtain ſome faint knowledge of, and perhaps be able partly to delineate, *the Interior Conſtruction of the Univerſe.*

Datchet near Windſor,
April, 1784.

WILLIAM HERSCHEL.

XXXIV. *An Account of a new Species of the* Bark-Tree, *found in the Island of St. Lucia. By Mr.* George Davidson; *communicated by* Donald Monro, *M. D. Physician to the Army, F. R. S.*

Read June 24, 1784.

DR. DONALD MONRO.

SIR,

HAVING received from my correspondent Mr. DAVIDSON, surgeon, in the island of St. Lucia, some Bark, the product of that island, which is said to possess the virtues of the Jesuit's Bark, and in a much smaller dose, I shall esteem it a favour if you will lay before the Royal Society the specimen which I have sent to you with this letter, together with Mr. DAVIDSON's account of it, if you think they merit that honour.

I have examined the dried specimens very carefully. They are not so well preserved as I could wish; but I have since seen much finer in the possession of Sir JOSEPH BANKS, who has done me the honour to favour me with the following character, as most distinctive of it from the other species of Cinchona already described, which he gave me an opportunity of examining.

It

It is undoubtedly a Cinchona, but not the Cinchona officinalis of Linnæus; for it differs from it effentially in its bark in feveral particulars. It has an emetic quality not common to the true bark, breaks more woody and fplintery, and is far more naufeous to the tafte. Its decoction is of a dull Burgundy colour; and its extract refembles more the bitter of Gentian than that of the Quinquina. I have procured four ounces of it from half a pound of the Bark boiled in water, and herewith fend to you a fmall fpecimen.

The drawings, which accompany this letter, are exact copies of the fpecimens which I received; I therefore hope they will not be thought unworthy the acceptance of the Royal Society.

I have the honour to be, &c.

Henrietta-ftreet, Nov. 6, 1783. G. WILSON.

Botanic character of the Bark-Tree of St. Lucia.

" Cinchona floribus paniculatis, glabris; laciniis linearibus,
" tubo longioribus; ftaminibus exfertis; foliis ellipticis, glabris."

Extract of a Letter from Mr. George Davidson, *dated* St. Lucia, July 15, 1783.

IT is now about four years fince Mr. Alexander Anderson difcovered in the woods, near the Grand Cul de Sac, fome trees refembling, in the botanical characters, the true Quinquina of LINNÆUS. He brought the bark, flowers, and ſeeds, to Dr. YOUNG of the General Hoſpital, and trial was made of it there; but not being ſufficiently dried, its ſtrong emetic and purgative qualities prevented its exhibition.

The publication of Dr. SAUNDERS, which I received about two months ago, mentioning the introduction of a ſpecies of bark of a redder colour, and poſſeſſing greater powers than the bark formerly in uſe, induced us here to try the bark of this country. Dr. YOUNG had by him ſome that was collected in General GRANT's time: on account of the length of time it had been kept, and its being ſufficiently dried, he has met with all the ſucceſs he could wiſh.

It is manifeſtly more aſtringent than the bark, and the bitter is likewiſe more durable on the palate.

Hitherto I have generally uſed the cold infuſion, either in lime or ſimple water, in the proportion of one ounce to three pints of the water. I have likewiſe given it in ſubſtance from twenty to thirty grains; but never exceeded the laſt quantity, for I never found the ſtomach able to retain more than twenty grains.

Joined with the Canella alba, it forms in ſpirits an agreeable and elegant tincture. I have made a tincture from the ſeeds, which are infinitely ſtronger in taſte than the bark itſelf.

(Signed) GEO. DAVIDSON.

Mr.

Mr. George Davidſon's *account of the* Bark-Tree *of the iſland of* St. Lucia.

THE Bark-Tree of this iſland is nearly about the ſize of the cherry-tree, ſeldom thicker than the thigh, and tolerably ſtraight; the wood is light and porous, without any of the bitterneſs and aſtringency of the bark itſelf.

It delights in a ſhady ſituation, the north-weſt aſpect of hills, under larger trees; and is generally to be found about the middle of an hill, near ſome running water.

The leaves are large, oblong, oppoſite, and plain, preſerving (as well as the flowers and ſeeds) the bitter taſte of the bark.

In the beginning of the rainy ſeaſon (June), the tree puts forth its flowers in ſmall tufts; at firſt they are white, but afterwards turn purpliſh. The ſtamina are five in number, with a ſingle ſtyle. The germen is oblong, bilocular, and furrowed on each ſide. The ſeeds are many, and of the winged kind. The corolla is monopetalous, with its mouth divided into five long ſegments.

The ſoil in general where it grows is a ſtiff red clay. The bark itſelf is of a lighter red than that ſent out here to the hoſpital under the name of *red bark.* It inclines more to the colour of cinnamon. The bitterneſs and aſtringency appear to be greater than in either of the other barks.

I apprehend, the proper ſeaſon for obtaining it is about the month of March, before the flowers come out: after-experience will beſt determine this.

Infuſed in cold water, in which form, or in lime-water, I generally uſe it, it forms a very red tincture, poſſeſſing the bitterneſs and aſtringency of the bark very ſtrongly. A few drops of the Tinctura florum martialium give it a very black colour, and occaſion a copious depoſition of a black ſediment. It does the ſame with the ſpirituous tincture.

With ſpirits it forms a beautiful red tincture.

Explanation of the references tab. XIX.

A. A branch of the Cinchona of St Lucia, with the flowers not yet opened.

B. The entire ſeed-veſſels.

C. A ſeed-veſſel ſplit.

D. One of the ſeeds, of its natural ſize.

E. The ſame magnified.

p. 456.

Basire sc.

XXXV. *An Account of an Obſervation of the Meteor of* Auguſt 18, 1783, *made on* Hewit Common *near* York. *In a Letter from* Nathaniel Pigott, *Eſq. F. R. S. to the Reverend* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read June 24, 1784.

REVEREND SIR, York, Oct. 18, 1783.

ON the 19th of laſt Auguſt I communicated to you an account of the remarkably fine meteor, which I had ſeen under circumſtances peculiarly favourable the preceding night. I was then preparing myſelf for a journey into the Eaſt Riding; and, on that account, obliged to poſtpone the verifications, mentioned hereafter, till my return.

On the 18th of Auguſt, about ten o'clock P.M. after a hot day, the weather a little hazy, but not ſo as to obliterate the ſtars, and no wind, being on horſeback, in company with two other gentlemen, on Hewit Common, about three miles from York, my attention was attracted towards the W.N.W. by ſeveral faint flaſhes of lightning, ſuch as are often ſeen near the horizon, or which may be ſtill better compared to flaſhes of an aurora borealis. Soon after which I perceived ſome luminous matter in motion, and collecting together from ſeveral directions, fig. 1. (tab. XX.) which immediately taking fire preſented itſelf under the form of a ball, of ſo vivid a brightneſs, that the whole horizon was illuminated, ſo that the ſmalleſt object might have been ſeen on the ground. This ball, when formed, began to move, with an eaſy ſliding motion, from W.N.W. towards the S.S.E. It ſuggeſted the idea of a highly brilliant comet, emitting a train or tail, but of a different colour from the ball itſelf, this laſt being of a moſt brilliant bluiſh white, and the tail of a duſky red, the length of which appeared to extend over fifteen or more degrees of the heavens, fig. 2. The apparent diameter of the nucleus ſeemed one-third or one-fourth of the full moon's diameter. The greateſt difficulty in this eſtimation hence ariſes, that I cannot, notwithſtanding all my endeavours, repreſent in my mind the moon otherwiſe than as a plane or diſk; nor the meteor, than as a ſpherical body. The altitude of it, when it formed in the W.N.W. was about 30°; and about 19° or 20° above the horizon, when it became extinct in the S.S.E. a few ſparks of the tail, neareſt the nucleus, ſcattering themſelves much in the ſame manner as thoſe of a ſky-rocket when burnt out, fig. 3.

It has been ſaid, that the ball divided itſelf into three or four parts before its extinction. To me it appeared to vaniſh or gently die away: what confirms me in the opinion, that it did not divide, is, that the three or four ſcattering parts above-mentioned were not of the bright colour of the ball itſelf, but of the duſky red which the tail invariably ſhewed. The interval of time from the meteor's formation to its extinction was nearly twenty ſeconds, perhaps two or three ſeconds leſs. The long habit I have of counting ſeconds in aſtronomical obſervations induces me to think this quantity may be relied on; and this I mention, becauſe ſome have eſtimated it more, ſome leſs. Nine or ten minutes after its diſſipation, I heard a noiſe, much reſembling the report of a cannon at a very great diſtance; but I would not wiſh to have it underſtood, that I ſpeak

ſpeak to this laſt interval with the ſame certainty as to the other; if, however, it be exact, and ſuppoſing ſound to move 1106 feet in one ſecond of time, and the ſame in the upper regions of the atmoſphere as here below, which, however, may be very different, its diſtance from me, at its extinction, muſt have been about 120 miles, and its perpendicular altitude above the earth's ſurface about 40 miles.

I have added a ſcheme and a ſmall ſketch, preſuming by that means to convey a clearer idea of what I ſaw. The altitudes, azimuths, &c. are not merely from eſtimation. After my return from the Eaſt Riding, I went to the very ſpot, where I had ſeen the meteor on the 18th of Auguſt. The road, as in the ſcheme, being exactly ſtraight from my ſtation, both *towards* and *from* York, no miſtake can ariſe in that reſpect. With all the circumſtances clearly and forcibly impreſſed on my mind, I watched till ſome remarkable ſpot in the ſky preſented itſelf at the ſame place in which I had ſeen the meteor itſelf form, croſs the road, vaniſh, &c.: then, with a theodolite, I took the ſeveral bearings, which may be the more relied on, as I repeated the operations three different times, on different ſpots, which agree ſurpriſingly well for meaſures where no minute exactneſs can be expected. I have marked minutes in the ſcheme, becauſe the reſults gave them, without any pretenſion to ſuch nicety.

I am, &c.

NATH. PIGOTT.

XXXVI. *Observations of the Comet of* 1783. *In a Letter from* Edward Pigott, *Esq. to the Rev.* Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

Read June 24, 1784.

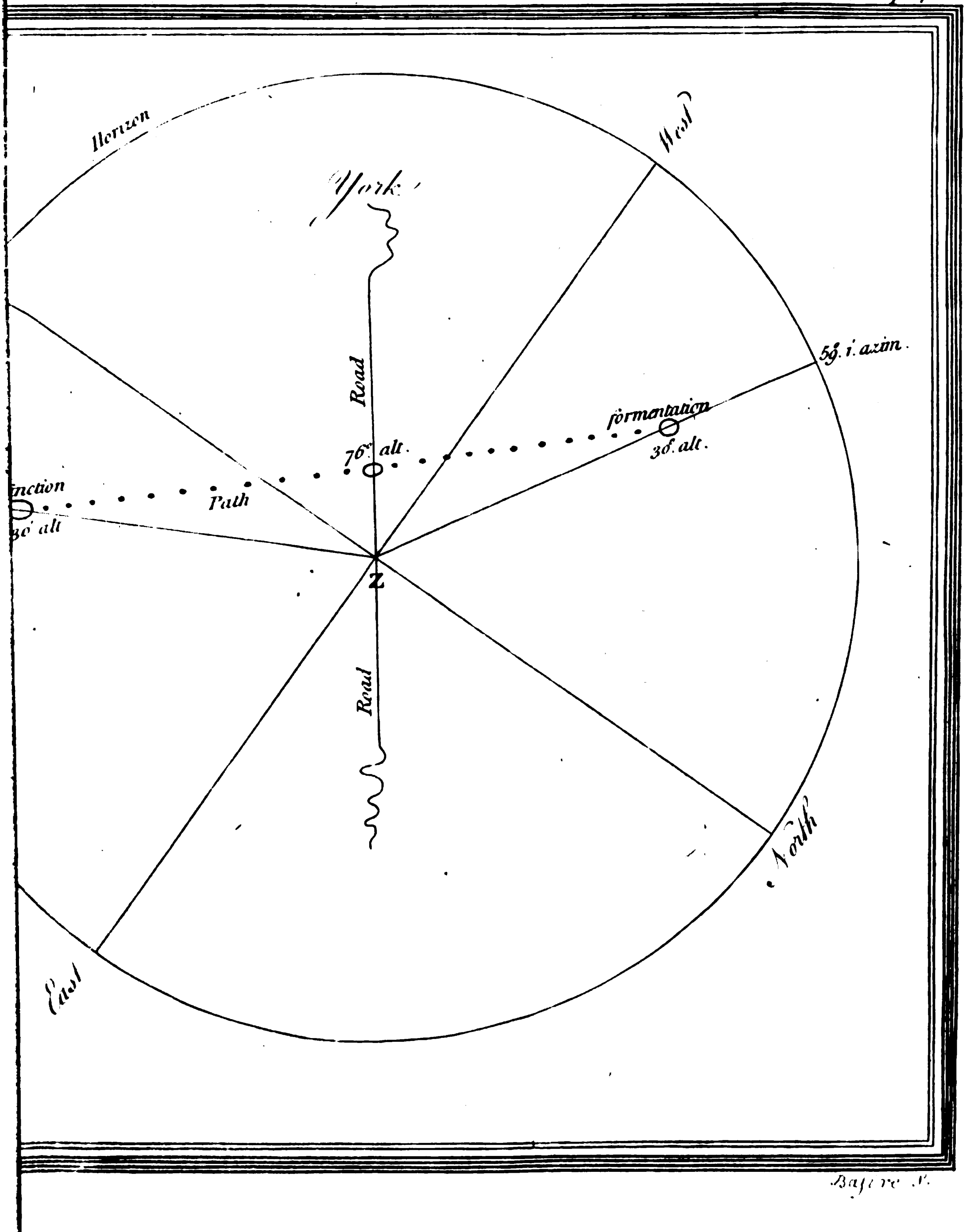
REVEREND SIR, York, Dec 11, 1783.

HAVING compleated my observations of the comet I discovered on the 19th of November last, I take the liberty of desiring you to present them to the Royal Society. The faintness of the comet's light, and the unfavourable sky you have had in the south, induce me to believe, that few observations of it have been made besides the following.

Dates.	Apparent time.	R. A.	North declination.	Greatest error of each R. A.	Longitude.	Latitude.
1783	h. ′	° ′ ″	° ′ ″	′ ″	S. ° ′	° ′
Novem. 19	11 4	41 1 30	3 11 ½	3 00	1 9 37	12 4½ S
20	10 55−	40 0 3	4 32 ¾	0 22	1 9 2⅔	10 31
22	6 52+	38 21 10	6 50	0 30	1 8 11½	7 50
24	10 24−	36 29 28	9 36 ¼	0 15	1 7 19	4 52¼
26	10 9−	34 49 20	12 3 ¼	0 15	1 6 33⅓	2 6
Decem. 3	15 54	29 21 59	20 15 ½	0 40	1 4 24	7 42½ N

The R. A's of November 20th, 24th, and 26th, were deduced from observations made at the transit instrument: the others, except the first, were determined with an excellent 2½ feet night-glass, made by DOLLOND, magnifying 20 times, having cross wires at right angles in its focus, which were

3 visible


Horizon
West
York
Road
59. 1. azim.
formentation
76°. alt.
30. alt.
inction
Path
30. alt
Z
Road
North
East

Basire Sc.

visible without being illuminated. With this instrument the comet, by the common method, was compared to stars in the field of the telescope, and within four minutes on the same parallel. The places of those stars were afterwards settled with the meridian instruments. As sometimes several stars were observed, I easily found to what degree of certainty those observations might be depended on, which I have marked with the above results. The declinations, I think, cannot err two minutes, being compared to stars within four minutes on the same parallel. The three of November 20th, 24th, and 26th, were taken with the transit instrument by comparing the comet to the nearest stars. I was much chagrined in not being able to see the comet in our equatorial when the wires were illuminated.

The comet had exactly the appearance of a nebula: its light was so faint that it could not be seen in a good opera glass. In the night-telescope the nucleus was scarcely visible, and the diameter of the surrounding coma was about three minutes of a degree. Between the 19th and 26th of November, I thought it had rather diminished in brightness. December the 1st and 3d it was very difficult to be seen, occasioned perhaps by its little elevation above the horizon. Between December the 3d and 10th, the comet was entirely effaced by the increased light of the Moon. On the 10th, the moon being in the horizon did not obliterate stars of the eighth or ninth magnitude; but I could not find the comet. The following observations were made by my friend Mr. JOHN GOODRICKE.

Dates.	Apparent time.	R. A.	North declination.	Longitude.	Latitude.
1783	h. ′	° ′ ″	° ′	S. ° ′	° ′
Novem. 24	8 16	36 32 57	9 30¾	1 7 21	1 42½S
28	6 8½	33 20 0	14 16½	1 5 55½	0 52½N

I am, &c.

EDW. PIGOTT.

P. S. This morning I received a letter from M. DE MECHAIN, in which he informs me, that he discovered the comet on the 26th of November seven days after my first observation. He has made several observations on it.

XXXVII. *Experiments on mixing Gold with Tin. In a Letter from Mr.* Stanefby Alchorne, *of his Majefty's Mint, to* Peter Woulfe, *Efq. F. R. S.*

Read June 24, 1784.

DEAR SIR,

Tower of London, March 25, 1784.

YOU know it is a generally received opinion among metallurgifts, that tin has a property of deftroying the ductility of gold, on being melted with it, even in very fmall quantities. Our late ingenious countryman Dr. LEWIS, in his Philofophical Commerce of Arts, p. 85. has well expreffed the fenfe of moft writers on this fubject, in the following words: "The moft minute proportion of tin and lead," fays he, "and even the vapours which rife from them in the fire, "though not fufficient to add to the gold any weight fenfible "in the tendereft balance, make it fo brittle that it flies in "pieces under the hammer."

Divers circumftances, neverthelefs, long fince induced me to difbelieve the fact; but thefe, having chiefly arifen from fmall experiments, did not feem to warrant any general conclufion. A late public occafion, however, which led me to various trials of mixing thefe metals together, in different proportions, and in fufficiently large quantities, has put the matter out of difpute; and fhewn me, that tin, in fmall quantity at leaft, may be added to gold, either pure or alloyed, without producing any other effect than what might eafily be conceived,

ceived, *à priori*, from the different texture of the two metals. In confirmation of which, I beg leave to lay ſome of the experiments before you.

EXPERIMENT I.

Sixty Troy grains of pure tin were ſtirred into twelve ounces of refined gold, in fuſion; and the mixture was then caſt into a mould of ſand, producing a flat bar, one inch wide, and one-eighth of an inch thick. The bar appeared ſound and good, ſuffered flatting under the hammer, drawing ſeveral times between a large pair of ſteel rollers, and cutting into circular pieces, of near an inch diameter, which bore ſtamping in the money-preſs, by the uſual ſtroke, without ſhewing the leaſt ſign of brittleneſs; or rather with much the ſame ductility as pure gold.

EXPERIMENT II.

Ninety grains of like tin were added to twelve ounces of fine gold, ſtirred, and caſt as above. The bar produced was ſcarcely diſtinguiſhable from the former, and bore all the operations, as before-mentioned, quite as well.

EXPERIMENT III.

One hundred and twenty grains of fine tin were mixed with twelve ounces of fine gold, and being caſt like the foregoing, produced a bar rather paler and harder than the preceding, but which ſuffered the like operations very well; except that, on drawing between rollers, the outer edges were diſpoſed to crack a little.

EXPE-

EXPERIMENT IV.

One hundred and forty grains, or half an ounce, of the like grained tin, were mixed, as before, with twelve ounces of fine gold; and the bar resulting from this mixture was completely found and good; evidently paler and harder, however, than any of the foregoing, and cracking rather more than the last on passing between the rollers; but bearing every other operation, even stamping under the press, by the common force, without any apparent injury.

EXPERIMENT V.

One ounce of tin was next stirred into twelve ounces of the like refined gold, and then cast as before; but the bar produced, though seemingly solid and good, was bad coloured, brittle in texture, and, on the first passing between the rollers, split into several pieces, so that no farther trials were made with it.

EXPERIMENT VI.

To inquire how far the fumes of tin, brought into contact with the gold, would do more than mixing the metal in substance, a small crucible, filled with twelve ounces of standard gold, $\frac{11}{12}$ fine, was placed in a larger crucible, having one ounce of melted tin in it, and kept there in fusion, the whole being covered by another large inverted crucible, for about half an hour. In this time a full quarter part of the tin was calcined; but the gold remained unaltered, and equally capable of being manufactured as another portion of the same gold melted in the common manner.

It

It may well be aſked, whether the tin, or part of it, in every trial, might not be deſtroyed, and thus render the concluſions fallacious? But as, in any of theſe experiments, not more than ſix or eight grains of the original weight were miſſing after the caſting, and as even fine gold can ſcarcely be melted without ſome loſs in the operation, ſo we may reaſonably ſuppoſe, that our ſmall loſſes, in the foregoing trials, do not deſerve conſideration.

The above experiments then ſeem to ſhew, that tin is not ſo miſchievous to gold as hath been generally repreſented. But it would be unfair to infer, that the original author of this doctrine (from whom ſo many have implicitly tranſcribed) had no foundation for the aſſertion. Gold and Tin, indeed, are ſubſtances pretty well known; but it is eaſy to imagine, that coins or trinkets may have been uſed for one, and impure tin, or pewter, perhaps, for the other; and it is difficult to gueſs what might be the reſult of ſuch uncertain combinations. To inquire farther, therefore, the experiments were continued as follows.

EXPERIMENT VII.

To determine whether the two metals might be more intimately combined, and the maſs rendered brittle, by additional heat; the mixture of gold and tin, produced in the firſt of theſe experiments, was re-melted in a ſtronger fire than before, and thus kept in fuſion full half an hour. By this operation ſix grains only were loſt in the weight; and the bar obtained was no leſs manufacturable than at firſt.

EXPE-

EXPERIMENTS VIII. AND IX.

The mixtures of gold and tin, from the ſecond and fourth experiments, were re-melted ſeparately, and one ounce of copper added to each. Being both well ſtirred, they were caſt as uſual; and the bars, though ſenſibly harder, bore all the operations of manufacturing as before. The laſt bar cracked a little at the edges, on drawing through the rollers, as it had done without the copper, but not materially, and bore cutting rather better than in its former ſtate.

EXPERIMENTS X. AND XI.

A quarter of an ounce of the laſt mixture (being tin half an ounce, and copper one ounce, with gold twelve ounces), and as much of the bar from experiment the third (being tin one hundred and twenty grains with gold twelve ounces), were each melted by a Jeweller, in the moſt ordinary manner, with a common ſea-coal fire, into ſmall buttons, without any loſs of weight. Theſe buttons were forged by him into ſmall bars, nealing them often by the flame of a lamp, and afterwards drawn each about twenty times through the apertures of a ſteel plate, into fine wire, with as much eaſe as coarſe gold commonly paſſes the like operation.

EXPERIMENT XII.

To enquire whether the adding of tin to gold, already alloyed, would cauſe any difference, ſixty grains of tin were ſtirred into twelve ounces of ſtandard gold, $\frac{11}{12}$ fine; and the reſult paſſed every operation before deſcribed, without ſhewing the leaſt alteration from the tin.

For greater certainty, ſeveral other trials were made, of different mixtures of copper, tin, and ſilver, with gold, even ſo low as two ounces and a half of copper, with half an ounce of tin, to twelve ounces of gold. But these are not worth particularizing; for they all bore hammering, and flatting by rollers, to the thinness of stiff paper, and afterwards working into watch-cases, cane-heads, &c. with great ease. They all, indeed, grew more hard and harsh, in proportion to the quantity of alloy; but not one of them had the appearance of what all workmen well know by the name of brittle gold. Whence it should seem, that neither tin in substance, or the fumes of it, tend much to render gold unmanufacturable.

Whenever, therefore, brittleness has followed the adding small quantities of tin to fine gold, it must be supposed to have arisen from some unfriendly mixture in the tin, probably from Arsenic; for other experiments have shewn me, that twelve grains of regulus of arsenic, injected into as many ounces of fine gold, will render it totally unmalleable.

From the foregoing experiments, I presume, we may fairly conclude, that though tin, like other inferior metals, will contaminate gold, in proportion to the quantity mixed with it, yet there does not appear any thing in it specifically inimical to this precious metal. And this being contrary to the doctrine of most chemical writers, I submit to your better judgement, whether it may not be useful to publish these experiments, by laying them before the Royal Society.

I am, &c.

S. ALCHORNE.

XXXVIII. *Sur un moyen de donner la Direction aux Machines Aerostatiques. Par M. Le Comte* De Galvez. *Communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read July 1, 1784.

NOUS soussignés certifions, que M. le Comte DE GALVEZ nous ayant communiqué ses idées sur le moyen de pouvoir donner la direction aux machines aërostatiques, pour faire route à volonté et par un rumb certain et assuré dans l'air, principalement fondé sur differentes observations qu'il avoit faites sur l'usage que les oiseaux font de leurs aîles quand ils volent, et sur celui que font les poissons de leurs nageoires et de leur queue quand ils nagent:

Nous nous sommes transportés, l'apres-midi du prémier Mars de cette année 1784, au canal de Manzanarês, où on avoit preparée une chaloupe de vingt-cinq pieds de long sur quatre et demi de large, avec une machine qu'il avoit inventée pour démontrer ses idées. Cette machine *, qui consistoit en un chevalet qui alloit de poupe à proue à la hauteur de cinq pieds, étoit croisée en rectangles par trois vergues de bois élastique, de dix-huits pieds de long chacune, avec une aîle à chaque bout, composée de baguettes de baleine, couvertes d'un morceau de taffetas de cinq pieds de long, et trois de large, laquelle étoit jointe par un de ses quatre côtés à la vergue, de façon que l'aîle restoit horisontale. Le mouvement se communiquoit à chaque vergue, et par conséquent à ses deux aîles, par un seul homme, qui tirant avec vitesse des cordes attachées aux bouts de chaque vergue, les agitoit verticalement, d'où resultoit que

* See tab. XXI. fig. 1.

quand elles se plioient, les aîles prenoient à leurs extrémités une inclination de quarante-cinq degrés de l'horison. Ce mouvement et celui de la réaction produisoient dans la chaloupe, où il y avoit six hommes, une impulsion qui la faisoit marcher contre le courant du canal et le peu d'air qu'il faisoit, cent cinquante pieds par minute, outre soixante pieds qu'elle parcouroit avant de s'arrêter depuis l'instant qu'on cessoit de mouvoir les aîles: elle parcouroit deux cents quarante-trois pieds par minute, allant avec le courant et l'air, par le même mouvement continu des aîles.

Nous fûmes tous très-etonnés de l'effet que produisit cette expérience; car, quoique le desir qu'avoit l'inventeur de mettre ses idées en pratique au plutôt, fut cause qu'il se servit d'une chaloupe lourde et mal construite, avec laquelle les aîles n'avoient point de proportion; nous sommes persuadés que la situation des aîles et leur mouvement vertical, qui formoient lors qu'on les battoient un plan incliné, imitant en cela les oiseaux et les poissons, fournissent un principe sûr et certain pour donner une direction par quelque rumb que se soit, à toute espèce de corps qui nagent dans un fluide, et par conséquent très-applicable aux nouvelles machines aërostatiques.

Cette invention nous paroît digne de l'approbation et de l'eloge des physiciens qui, sans doute, employeront leurs efforts pour lui donner toute la perfection dont elle est susceptible dans l'exécution de son mécanisme.

Et pour constater que la dite expérience a été faite de la manière qu'on vient d'exposer, nous avons signé la présente certification, ainsi qu'un dessein de la dite machine, à Madrid le deux Mars, mil sept cent quatre-vingt-quatre. D. JOSEF DE VIEXA, D. AGUSTIN BETANCOURT Y MOLINA, D. RICARDO WORSLEY, RAIM DE S. LAURENT, CASIMIRE ORTEGA.

XXXIX. *An extraordinary Case of a Dropsy of the Ovarium, with some Remarks. By Mr.* Philip Meadows Martineau, *Surgeon to the* Norfolk *and* Norwich *Hospital*; *communicated by* John Hunter, *Esq. F. R. S.*

Read July 1, 1784.

SARAH KIPPUS, a pauper in the city of Norwich, was, for many years, a patient of my father's, and, at his decease, was under the care of Mr. SCOTT, as city surgeon, who obliged me many times by taking me to the poor woman, from whom I received the account of the early part of her disease.

Her complaints came on first after a miscarriage at the age of 27. She had never been pregnant before; and her discharges at that time were so great as to bring her into a very weak condition. She soon perceived some uneasiness, attended with a swelling, on one side, which, after a few months, became too large to distinguish whether it was greater on one side or the other. As the swelling was found to arise from water, it was drawn off, which was in the year 1757. She was never afterwards pregnant; but the catamenia continued regularly till the usual period of their cessation. When I first saw her, which was in the year 1780, she had been many times tapped, and she was then full of water. Her appearance was truly deplorable, not to say shocking. She was rather a low woman, and her body so large as almost wholly to obscure her face, as well as every other part of her: with all she was tole-

 rably

rably chearful, and ſeldom regarded the operation. I ſaw her juſt before we took away 106 pints of water, and I begged leave to take a meaſure of her. She was ſixty-ſeven inches and a half in circumference, and from the cartilago enſiformis to the os pubis thirty-four inches. Her legs were now greatly ſwelled; but this, and every other ſymptom of which ſhe complained, evidently aroſe from the quantity and weight of water. She neither ate nor drank much, and made but a ſmall quantity of urine.

The operation of drawing off the water was generally performed on a Sunday, as the moſt convenient day for her neighbours to aſſiſt her, and before the latter end of the week ſhe was able to walk very well. She was firſt tapped in the year 1757, and died in Auguſt 1783. Thus ſhe lived full twenty-five years with ſome intervals of eaſe, having eighty times undergone the operation, and in all had taken from her 6631 pints of water, or upwards of thirteen hogſheads.

I will ſubjoin the account of the dates, and the quantity drawn off at each time, as given me by Mr. SCOTT, obſerving that till 1769 no exact memorandum was kept, except of the *number* of times, although the quantity of water drawn off was always meaſured. By my father ſhe was tapped twenty-ſix times, averaged at 70 pints each time: by Mr. DONNE once, 73 pints, which makes 1683 pints from ſome part of the year 1757 to 1769. By Mr. SCOTT as follows:

1769.

1769.	Pints.
Mar. 16.	70
July 17.	72
Nov. 20.	78
Dec. 31.	70
	290

1770.	
April 15.	70
Aug. 11.	73
Dec. 4.	76
	219

1771.	
Mar. 22.	74
July 14.	78
Nov. 3.	79
	231

1772.	
Feb. 22.	79
June 6.	73
Sept. 12.	74
Dec. 12.	82
	308

1773.	
March 7.	78
May 29.	71
Aug. 29.	79
Dec. 5.	81
	309

1774.	Pints.
Mar. 13.	77
June 26.	89
Oct. 23.	92
	258

1775.	
Jan. 24.	94
May 28.	91
Sept. 13.	72
Dec. 16.	80
	337

1776.	
April 9.	84
July 28.	82
Nov. 27.	85
	251

1777.	
Mar. 16.	89
July 27.	90
Nov. 9.	98
	277

1778.	
March 8.	96
July 5.	99
Nov. 5.	105
	300

1779.	Pints.
Feb. 28.	106
June 13.	108
Aug. 17.	92
Oct. 24.	99
Dec. 10.	90
	495

1780.	
Feb. 6.	73
Apr. 23.	102
July 24.	106
Sept. 10.	95
Nov. 12.	98
	474

1781.	
Jan. 1.	100
Mar. 11.	94
June 25.	100
Oct. 14.	100
	394

1782.	
Jan. 13.	99
Mar. 18.	64
June 2.	74
Aug. 25.	98
Nov. 17.	90
	425

7 1783.

1783.	Pints.
Feb. 14.	104
May 11.	100
July 20.	98
Aug. 11 on opening	78
	380

Total 6631 pints.

In looking over this account it appears, that 108 pints was the greatest quantity ever taken away at any one time; that she was never tapped more than five times in any one year; and the largest quantity in a year was 495 pints. The most collected in the shortest space of time was 95 pints in seven weeks, from July 24th to September 10th in 1780, which is very nearly two pints a day. It appears also, that in the last 14 years of her life, when a regular account was kept, she increased faster in the winter than in the summer months. If the six summer months from April to September inclusive are reckoned, she lost in the 14 years in 23 operations 1972 pints, and in the winter months from October to March inclusive, by 30 tappings, 2596 pints; and it will be found, that 30 is to 2596 rather more than 23 to 1972, so that seven more tappings were at least necessary in the winter than in the summer. In the months of March and November she oftener underwent the operation than in any other. In these calculations the three months in 1783 are not included, as the year was not finished.

If we compare the famous case of Lady PAGE, related by Dr. MEAD, the quantity of water taken from her ladyship appears small when opposed to the number of pints drawn from

from SARAH KIPPUS. The one loſt 1920, the other 6631. It muſt be confeſſed, however, that Lady PAGE collected faſter than the poor woman whoſe caſe I have related.

I come now to ſpeak of the diſſection, and to make ſome obſervations on the whole. On the 10th of Auguſt 1783, the poor woman died; and the following day Dr. DACK, an eminent phyſician of this place, accompanied me to open the body. I firſt drew off 78 pints of clear water: ſuppoſing, therefore, all the water to have been taken away at the laſt operation, then in three weeks ſhe had collected 78 pints, which is more than three pints and a half in each day: a quantity far exceeding what ſhe had taken. I then opened into the cavity from which the water came, and ſeparated the ſac from the peritoneum, and found the ſac had ariſen in the ovarium of the left ſide. After this, I diſſected out the uterus, with the right ovarium in a natural ſtate, and thus obtained every part neceſſary to ſhow the diſeaſe, *viz.* the uterus, the right ovarium ſound, and the left enlarged into an immenſe pouch. The cyſt itſelf was not very thick, but lined in almoſt every part of it, but more eſpecially in the fore part, with ſmall oſſifications. The peritoneum was *prodigiouſly* thickened, and thus, by its additional ſtrength, became the chief ſupport of the water. There was ſomething ſingular in the ſac itſelf, for it was rather two than one, from there being an opening in the ſide of what appeared at firſt the only cavity, which led to another cavity, almoſt equally large with the firſt, ſo that if all the water in any operation had not been evacuated, it muſt probably have been owing to a difficulty in its paſſage from the ſecond into the firſt or more external cyſt. From the ſize, however, of the poor woman after each operation, it is evident, that in her there being two ſacs did not prevent the total drawing off of the water. The other viſcera appeared all in a natural ſtate. The inteſtines were quite empty, and puſhed up under the ribs, ſo as to have left but very little room for the expanſion of the lungs within the thorax. The bladder was contracted, or rather I ſhould ſay appeared leſſened. The kidneys were healthy, and both ureters in a natural ſtate. The ſac is in the collection of JOHN HUNTER, eſq.

In reflecting upon this caſe, an obvious queſtion ariſes; from whence proceeded this immenſe collection of water? At different periods of this poor woman's life the quantity drawn off, without conſidering the urine ſhe made, was much greater than the fluids ſhe drank, which appeared from meaſuring whatever ſhe took. It appears then pretty certain, that this ſuperabundant quantity muſt have been taken into the body by abſorption; and if we allow the bodies of animals to have this power of abſorbing, which we very well know vegetables are poſſeſſed of, it will account for many appearances in the animal œconomy. This poor woman collected faſter in the wet moiſt months of winter, than in ſummer.

From all, this happy concluſion may be drawn, that although human art is at preſent inſufficient to the perfect cure of diſeaſes ſimilar to the poor woman's caſe I have related, yet nature is continually defending herſelf from ſudden death; and ſuch relief *may* be granted as to protract life a long time without much pain, and often with intervals of great eaſe and comfort.

XL. *Methodus inveniendi Lineas Curvas ex proprietatibus Variationis Curvaturæ. Auctore* Nicolao Landerbeck, *Mathes. Profess. in Acad. Upsaliensi Adjuncto. Communicated by* Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

Read July 1, 1784.

PARS SECUNDA*.

CURVAS, ex proprietate variationis curvaturæ invenire, indice per functionem coordinatarum cujusdam expresso, problema etsi indeterminatum est; juvat tamen ad curvas cognoscendas, quum facile et sponte sese offerunt conditiones determinantes qui rei conveniunt et quæ in casu quovis examini subjecto locum habent. Quo consilio et qua arte calculum inire oporteat, ut et hæc et his affinia peragenda sint, quæ ad curvas ex curvaturæ variatione cognoscendas pertineant, per theoremata quæ sequuntur, exponere conabor.

THEOREMA I. (Vide tab. XXI. fig. 2.)

Si curvæ cujusdam LC index variationis curvaturæ sit T, radius curvedinis R, sinus anguli BCD p, posito sinu toto 1, arcus curvæ LC z coordinatæ perpendiculares x et y earumque fluxiones dp, dz, dx, et dy dicantur, erit $\frac{dz}{\int Tdz} = -\frac{dp}{\sqrt{1-p^2}}$.

Quoniam $dx = -Rdp$ et $dz = -\frac{dx}{\sqrt{1-p^2}}$ habetur $\frac{dz}{R} = -\frac{dp}{\sqrt{1-p^2}}$

* See Vol. LXXIII. p. 456.

 et

et quum $d\mathrm{R} = \mathrm{T}dz$ erit $\mathrm{R} = \int \mathrm{T}dz$ et ſubſtitutione $\frac{dz}{\int \mathrm{T}dz} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. 1. Hinc obtinetur $\frac{dx}{\mathrm{R}} = -dp$, $\frac{dy}{\mathrm{R}} = -\frac{pdp}{\sqrt{1-p^2}}$ et $\frac{dz}{\mathrm{R}} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. 2. Si Tangens anguli BCD per r, Secans per s deſignentur habetur $\frac{dz}{\int \mathrm{T}dz} = -\frac{dr}{1+r^2}$ et $\frac{dz}{\int \mathrm{T}dz} = -\frac{ds}{s\sqrt{s^2-1}}$.

Schol. 1. Ex hoc theoremate facilis deducitur methodus generaliter calculandi variationem curvaturæ curvæ cujuſcumque. Nam $\int \mathrm{T}dz = -\frac{dz\sqrt{1-p^2}}{dp}$, quantitas vero $\frac{dz\sqrt{1-p^2}}{dp}$ datur, data inter x et y relatione. Sit valor quantitatis $-\frac{dz\sqrt{1-p^2}}{dp} = \mathrm{Z}$ functioni curvæ z, $\int \mathrm{T}dz = \mathrm{Z}$ et ſumtis fluxionibus $\mathrm{T}dz = \dot{\mathrm{Z}}dz$ qua $\mathrm{T} = \dot{\mathrm{Z}}$ functioni ipſius z. Si valor quantitatis $-\frac{dz\sqrt{1-p^2}}{dp} = \mathrm{P}$ per p expreſſus, erit $\int \mathrm{T}dz = \mathrm{P}$ ſumtiſque fluxionibus $\mathrm{T}dz = \dot{\mathrm{P}}dp$ et $\mathrm{T} = \frac{\dot{\mathrm{P}}dp}{dz}$, quæ functio eſt quantitatis p, in poteſtate ſemper eſt $\frac{dp}{dz}$ per p exprimere.

Schol. 2. Hujus etiam theorematis ſubſidio inveniri poſſunt curvæ ex data relatione inter T et z, R et z, R et y, et R et p. Si enim ſit $\mathrm{T} = \mathrm{Z}$ functioni quantitatis z, erit $\int \mathrm{T}dz = \int \mathrm{Z}dz + \mathrm{A}$, vi theorematis $\frac{dz}{\int \mathrm{Z}dz + \mathrm{A}} \left(= \frac{dz}{\int \mathrm{T}dz}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{dz}{\int \mathrm{Z}dz + \mathrm{A}} + \mathrm{C} = -\frac{dp}{\sqrt{1-p^2}}$. Poſita $\int \frac{dz}{\int \mathrm{Z}.z + \mathrm{A}} + \mathrm{C} = b$ et N numerus

merus cujus logarithmus hyperbolicus 1 habetur $\sqrt{1-p^2}=\frac{N^{b\sqrt{-1}}-N^{-b\sqrt{-1}}}{2\sqrt{-1}}$ et $p=\frac{N^{b\sqrt{-1}}+N^{-b\sqrt{-1}}}{2}$, quæ functiones sunt quantitatis z, quibus positis $\dot{Z}$ et $\sqrt{1-\dot{Z}^2}$ respective proveniunt $x\left(=\int dz\sqrt{1-p^2}\right)=\int \dot{Z}dz$ et $y\left(=\int pdz\right)=\int dz\sqrt{1-\dot{Z}^2}$ quarum alterutra curvarum indoles innotescit.

Si $R=X$ functioni abscissæ x provenit $\frac{dx}{X}\left(=\frac{dx}{R}\right)=-dp$ et integratione $\dot{X}\left(=C-\int\frac{dx}{R}\right)=p$ unde $\sqrt{1-p^2}=\sqrt{1-\dot{X}^2}$ et $y\left(=\int\frac{pdx}{\sqrt{1-p^2}}\right)=\int\frac{\dot{X}dx}{\sqrt{1-\dot{X}^2}}$ æquatio curvæ indolem exprimens.

Et si $R=Y$ functioni ordinatæ y, habetur $\frac{dy}{Y}\left(=\frac{dy}{R}\right)=-\frac{pdp}{\sqrt{1-p^2}}$ et integratione $\dot{Y}\left(=\int\frac{dy}{Y}+C\right)=\sqrt{1-p^2}$, unde $p=\sqrt{1-\dot{Y}^2}$ et $x\left(=\int\frac{dy\sqrt{1-p^2}}{p}\right)=\int\frac{\dot{Y}dy}{\sqrt{1-\dot{Y}^2}}$ quæ exprimit naturam curvæ.

Hinc colligitur quod quoties $\dot{Z}dz$ perfecte integretur et $\int\frac{dz}{\int \dot{Z}dz+A}$ obtineatur per arcus circulares dum aut $\int \dot{Z}dz$ aut $\int dz\sqrt{1-\dot{Z}^2}$ absolutam admittat integrationem, curvæ erunt rectificabiles, et algebraicæ, si relatio inter x et z vel inter y et z in relationem algebraicam inter x et y permutari possit.

Evidens etiam est quod si X functio est algebraica quantitatis x vel Y quantitatis y, et non solum $\frac{dx}{X}$ vel $\frac{dy}{Y}$ sed etiam $\frac{\dot{X}dx}{\sqrt{1-\dot{X}^2}}$ vel $\frac{\dot{Y}dy}{\sqrt{1-\dot{Y}^2}}$ quantitates perfecte integrabiles, curvæ evadunt algebraicæ, alias transcendentes.

Exempl.

Exempl. 1. Invenienda sit curva ubi variatio curvaturæ $T = \frac{3 \cdot \overline{\overline{8a+27z}|^{\frac{2}{3}} - za^{\frac{2}{3}}}}{a^{\frac{1}{3}} \sqrt{\overline{8a+27z}|^{\frac{2}{3}} - 4a^{\frac{2}{3}}}}$. Ut simplicior reddatur calculus ponatur $\overline{8a+27z}|^{\frac{2}{3}} = u$ et $a^{\frac{2}{3}} = b$ erit $z = \frac{u^{\frac{3}{2}} - 8b^{\frac{3}{2}}}{27}$, $dz = \frac{du\sqrt{u}}{18}$, $T = \frac{3u-2b}{\sqrt{b}\sqrt{u-4b}}$ et $\int T dz = \frac{u\sqrt{u}\sqrt{u-4b}}{18\sqrt{b}} + A$; sit constans hæc $A = a$, quod accidit evanescente $\int T dz u = b$, habetur per theorema $\frac{du\sqrt{b}}{u\sqrt{u-4b}} \left(= \frac{dz}{\int T az}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{du\sqrt{b}}{u\sqrt{u-4b}} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, cujus æquationis termini quum sint arcus circulares quorum sinus $\sqrt{1-p^2} = \frac{\sqrt{u-4b}}{\sqrt{u}}$ et cosinus $p = \frac{2\sqrt{b}}{\sqrt{u}}$, posito arcu constanti $C = 0$, obtinetur $y \left(= \int p dz\right) = \int \frac{du\sqrt{b}}{9} + B) = \frac{\sqrt{u-4b}\sqrt{b}}{9}$ nam $B = \frac{4b\sqrt{b}}{9}$, posita $y = 0$ et $u = 4b$, atque $x (= \int dz\sqrt{1-p^2}) = \int \frac{du\sqrt{u-4b}}{18} = \frac{\overline{u-4b}|^{\frac{3}{2}}}{27}$ quibus æquationibus exterminata u et substituta a habetur $y^3 = ax^2$ æquatio pro parabola cubica.

Exempl. 2. Si sit variatio curvaturæ $T = \frac{2z}{a}$ erit $\int T dz \left(= \int \frac{2zdz}{a}\right) = \frac{z^2}{a} + A$ et si $Z = 0$ $\int T dz = a$ erit constans $A = a$, atque vi theorematis $\frac{adz}{a^2+z^2} \left(= \frac{dz}{\int T dz}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{adz}{a^2+z^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$; posito arcu constanti $C = 0$ cæteri sunt æquales eorumque sinus et cosinus, unde $\sqrt{1-p^2} = \frac{z}{\sqrt{a^2+z^2}}$, $p = \frac{a}{\sqrt{a^2+z^2}}$ et $dx \left(= dz\sqrt{1-p^2}\right) = \frac{zdz}{\sqrt{a^2+z^2}}$ et dy (= pdz)

$pdz) = \frac{adz}{\sqrt{a^2+z^2}}$, quibus conſtat curvam eſſe catenariam.

Exempl. 3. Sit variatio curvaturæ $T = \frac{a-z}{\sqrt{2az-z^2}}$, evadit $\int Tdz = \sqrt{2az-z^2}$, per theorema $\frac{dz}{\sqrt{2az-z^2}} \left(= \frac{dz}{\int Tdz}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et per integrationem $\int \frac{dz}{\sqrt{2az-z^2}} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, ſi arcus ille conſtans $C = 0$, cæteri ſunt æquales eorumque ſinus et coſinus, quo $\sqrt{1-p^2} = \frac{\sqrt{2ax-x^2}}{a}$, $p = \frac{a-z}{a}$ et $y \left(= \int pdz\right) = \int \frac{a-zdz}{a}\Big) = \frac{2az-z^2}{a}$ æquatio pro cycloide ordinaria.

THEOREMA II.

Manentibus antea adhibitis denominationibus erit $\frac{dx}{y+\int Tdx} = -\frac{dp}{\sqrt{1-p^2}}$.

Quoniam $\frac{dx}{R} = -dp$, erit dividendo per $\sqrt{1-p^2}$, $\frac{dx}{R\sqrt{1-p^2}} = -\frac{dp}{\sqrt{1-p^2}}$. Propter $1 : \sqrt{1-p^2} :: CD(R) : CF = R\sqrt{1-p^2}$, ſed $dz : dx :: Tdz : Tdx$, quæ fluxio eſt ipſius DE, quare $DE = \int Tdx$, unde $CF = y + \int Tdx$ qua pro $R\sqrt{1-p^2}$ ſubſtituta, prodit $\frac{dx}{y+\int Tdx} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. 1. Quantitas $dy + Tdx$ ſemper eſt perfecte integrabilis. Nam $Tdx = -\frac{ddx\sqrt{1-p^2}}{dp}$ et $dy = \frac{pdx}{\sqrt{1-p^2}}$ unde $dy + Tdx = \frac{pdx}{\sqrt{1-p^2}} - \frac{ddx\sqrt{1-p^2}}{dp}$ et integratione $y + \int Tdx = -\frac{dx\sqrt{1-p^2}}{dp}$.

Cor.

Cor. 2. Dicatur ſemichorda curvaturæ CF F, obtinetur $\frac{dx}{F} = -\frac{dp}{\sqrt{1-p^2}}$, $\frac{dy}{F} = -\frac{pdp}{1-p^2}$ et $\frac{dz}{F} = -\frac{dp}{1-p^2}$.

Cor. 3. Si Tangens anguli BCD per r, Secans per s deſignentur habetur $\frac{dx}{y+\int Tdx} = -\frac{dr}{1+r^2}$ et $\frac{dx}{y+\int Tdx} = -\frac{ds}{s\sqrt{s^2-1}}$.

Schol. 1. Per hoc theorema via etiam patet calculandi generaliter variationem curvaturæ. Eſt enim $y+\int Tdx = -\frac{dx\sqrt{1-p^2}}{dp}$, quantitas vero $\frac{dx\sqrt{1-p^2}}{dp}$ datur data inter x et p relatione. Sit valor quantitatis $-\frac{dx\sqrt{1-p^2}}{dp} = X$ functioni abſciſſæ x æquatione ad curvam inventus, erit $\int T dx = X - y$ et ſumtis fluxionibus $Tdx = \dot{X}dx - dy$, qua $T = \dot{X} - \frac{dy}{dx}$ ubi tam $\dot{X}$ quam $\frac{dy}{dx}$ ſunt functiones abſciſſæ x. Si valor quantitatis $-\frac{dx\sqrt{1-p^2}}{dp} = P$ per p expreſſus, erit $\int Tdx = P - y$ ſumtiſque fluxionibus $Tdx = \dot{P}dp - dy$, qua $T = \frac{\dot{P}dp}{dx} - \frac{p}{\sqrt{1-p^2}}$ ubi $\frac{\dot{P}dp}{dx}$ functio eſt quantitatis p, nam $\frac{dp}{dx}$ per p exprimi poteſt.

Schol. 2. Hoc adhibito theoremate inveniri etiam poſſunt curvæ, ex data relatione inter T et x, F et x, F et y, F et z, et F et p. Poſita enim T functione quantitatis x, patet per curvarum quadraturas, aut perfectam aut imperfectam quantitatis Tdx obtineri integrationem. Sit $\int Tdx = \dot{X} + \int \dot{X}dx$ functioni vel algebraicæ vel ex parte tranſcendenti ipſius x, cujus terminis homogeneus valor ipſius $y = \int \ddot{X}dx$ capiatur, iſque ejus indolis ut $\int \overline{\dot{X} + \ddot{X}}dx$, vel quod idem eſt $y + \int Tdx =$

 X +

$\mathrm{X}+\int \overline{\overset{\text{I}}{\mathrm{X}}+\overset{\text{I}}{\mathrm{X}}}\,dx$ integratione abſoluta habeatur, permanente $\mathrm{T}dz=\mathrm{T}dx\sqrt{1-\overset{\text{II}}{\mathrm{X}}{}^2}$ perfecte integrabili. Per theorema deinde habetur $\dfrac{dx}{\mathrm{X}+\int \overline{\mathrm{X}+\overset{\text{I}}{\mathrm{X}}dx}}\left(=\dfrac{dx}{y+\int \mathrm{T}dx}\right)=-\dfrac{dp}{\sqrt{1-p^2}}$, et per integrationem $\displaystyle\int\frac{dx}{\mathrm{X}+\int \mathrm{X}+\overset{\text{I}}{\mathrm{X}}dx}+\mathrm{C}=-\int\frac{dp}{\sqrt{1-p^2}}$, ſi ponatur $\displaystyle\int\frac{dx}{\mathrm{X}+\int\overline{\overset{\text{I}}{\mathrm{X}}+\overset{\text{II}}{\mathrm{X}}dx}}+\mathrm{C}=k$ et N baſi logarithmorum hyperbolicorum, erit $\sqrt{1-p^2}=\dfrac{\mathrm{N}^{k\sqrt{-1}}-\mathrm{N}^{-k\sqrt{-1}}}{2\sqrt{-1}}$ et $p=\dfrac{\mathrm{N}^{k\sqrt{-1}}+\mathrm{N}^{-k\sqrt{-1}}}{2}$, $\sqrt{1-p^2}$ et p igitur ſunt functiones ipſius x, quæ ſi ponantur $\sqrt{1-\overset{\text{I}}{\mathrm{X}}{}^2}$ et $\overset{\text{III}}{\mathrm{X}}$, habetur $y\left(=\displaystyle\int\frac{p\,dx}{\sqrt{1-p^2}}\right)=\displaystyle\int\frac{\overset{\text{III}}{\mathrm{X}}dx}{\sqrt{1-\overset{\text{I}}{\mathrm{X}}{}^2}}$, æquatio qua curvæ internoſcuntur.

Si ſit $\mathrm{F}=\mathrm{Y}$ functioni quantitatis y erit per Cor. 2. $\dfrac{dy}{\mathrm{Y}}\left(=\dfrac{dy}{\mathrm{F}}\right)=-\dfrac{dp}{1-p^2}$ et integratione $\displaystyle\int\frac{dy}{\mathrm{Y}}+\log.\mathrm{C}=\log.\sqrt{1-p^2}$, ponatur $\displaystyle\int\frac{dy}{\mathrm{Y}}=k$ et N logarithmorum baſi, erit facto ad quantitates abſolutas tranſitu $\mathrm{CN}^k=\sqrt{1-p^2}$, $p=\sqrt{1-\mathrm{C}^2\mathrm{N}^{2k}}$ et $x\left(=\displaystyle\int\frac{dy\sqrt{1-p^2}}{p}\right)=\displaystyle\int\frac{\mathrm{CN}^k dy}{\sqrt{1-\mathrm{C}^2\mathrm{N}^{2k}}}$, æquatio quæ indolem curvæ indigitat.

Si $\mathrm{F}=\mathrm{Z}$ functioni ipſius z erit $\dfrac{dz}{\mathrm{Z}}\left(=\dfrac{dz}{\mathrm{F}}\right)=-\dfrac{dp}{1-p^2}$ et integratione $\displaystyle\int\frac{dz}{\mathrm{Z}}+\log.\mathrm{C}=\log.\sqrt{\frac{1-p}{1+p}}$, et ſi $\displaystyle\int\frac{dz}{\mathrm{Z}}=k$ et N baſi logarithmica habetur $p=\dfrac{1-\mathrm{C}^2\mathrm{N}^{2k}}{1+\mathrm{C}^2\mathrm{N}^{2k}}$ et $y=\int p\,dz)=\displaystyle\int\frac{1-\mathrm{C}^2\mathrm{N}^{2k}}{1+\mathrm{C}^2\mathrm{N}^{2k}}dz$ qua curvæ cognoſcuntur.

 Conſtat

Conſtat hinc quod quoties $X+\int \overset{\prime}{X}dx$ perfecta integratione habeatur $\int \frac{dx}{X+\int X+Xdx}$ per arcus circulares dum $\frac{\overset{\prime\prime\prime}{X}dx}{\sqrt{1-\overset{\prime\prime}{X}^2}}$ abſolutam admittat integrationem curva ſit algebraica, ſi vero aliter evenerit tranſcendens.

Quoties $\frac{dy}{Y}$ ſit integrale logarithmicum et $\frac{CN^k dy}{\sqrt{1-C^2N^{2k}}}$ abſolutam admittat integrationem curva eſt algebraica, in aliis caſibus tranſcendens.

Et quoties $\int\frac{dz}{Z}$ per logarithmos inveniatur, $\frac{1-C^2N^{2k}dz}{1+C^2N^{2k}}$ abſolute ſit integrabilis pariter ac $\frac{2CN^k dz}{1+C^2N^{2k}}$ curva eſt algebraica, alias tranſcendens.

Exempl. 1. Si ſit variatio curvaturæ $T=\frac{3\cdot\overline{b^2-a^2}x\sqrt{a^2-x^2}}{a^3b}$ erit $\int Tdx\ (=\frac{\overline{a^2-b^2}\cdot\overline{a^2-x^2}\sqrt{a^2-x^2}}{a^3b})=\frac{a\sqrt{a^2-x^2}}{b}-\frac{x^2\sqrt{a^2-x^2}}{ab}-\frac{b\sqrt{a^2-x^2}}{a}+\frac{bx^2\sqrt{a^2-x^2}}{a^3}$, ſi ponatur $y=\frac{b\sqrt{a^2-x^2}}{a}$ habetur $y+\int Tdx=\frac{\overline{a^4+b^2-a^2x^2}\sqrt{a^2-x^2}}{a^3b}$, adhibendo theorema $\frac{a^3bdx}{\overline{a^4+b^2-a^2x^2}\sqrt{a^2-x^2}}\ (=\frac{dx}{y+\int Tdx})=-\frac{dp}{\sqrt{1-p^2}}$ et integrando $\int\frac{a^3bdx}{\overline{a^4+b^2-a^2x^2}\sqrt{a^2-x^2}}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, cujus termini ſunt arcus circulares quorum ſinus $\sqrt{1-p^2}=\frac{a\sqrt{a^2-x^2}}{\sqrt{a^4+b^2-a^2x^2}}$ et coſinus $p=\frac{bx}{\sqrt{a^4+b^2-a^2x^2}}$ evaneſcente arcu conſtanti C, quare $y\ (=\int\frac{pdx}{\sqrt{1-p^2}})=\int\frac{bdx}{a\sqrt{a^2-x^2}})=\frac{b\sqrt{a^2-x^2}}{a}$ et in hoc caſu curva eſt ellipſis.

Exempl. 2. Sit jam variatio curvaturæ $T = \frac{2\sqrt{2ax+x^2}}{a}$ erit $\int T dx = \frac{x\sqrt{2ax+x^2}}{a} + \int \frac{xdx}{\sqrt{2ax+x^2}}$ et poſita $y = \int \frac{adx}{\sqrt{2ax+x^2}}$ perfecta integratione habetur $y + \int T dx = \frac{\overline{a+x}\sqrt{2ax+x^2}}{a}$. Theorematis itaque auxilio erit $\frac{adx}{\overline{a+x}\sqrt{2ax+x^2}} \left(= \frac{dx}{y + \int T dx} = -\frac{dp}{\sqrt{1-p^2}}\right)$, et integratione $\int \frac{adx}{\overline{a+x}\sqrt{2ax+x^2}} = C = -\int \frac{dp}{\sqrt{1-p^2}}$, ſi vero arcus ille conſtans $C = 0$ cæteri ſunt æquales eorumque ſinus et coſinus, unde $\sqrt{1-p^2} = \frac{\sqrt{2ax+x^2}}{a+x}$, $p = \frac{a}{a+x}$ et $y \left(= \int \frac{pdx}{\sqrt{1-p^2}}\right) = \int \frac{adx}{\sqrt{2ax+x^2}}$, æquatio indicans curvam eſſe catenariam.

THEOREMA III.

Dicatur coſinus anguli BCD q, poſito radio 1, cæteriſque manentibus denominationibus erit $\frac{dy}{\int T dy - x} = \frac{dq}{\sqrt{1-q^2}}$.

Eſt enim $\frac{dy}{R} = dq$, qua per $\sqrt{1-q^2}$ diviſa, dat $\frac{dy}{R\sqrt{1-q^2}} = \frac{dq}{\sqrt{1-q^2}}$; et ob $1 : \sqrt{1-q^2} :: CD\,(R) : CG = R\sqrt{1-q^2}$, ſed $dz : dy :: Tdz : Tdy$ cujus integrale eſt $AE = \int T dy$, unde CG ($= AE - AB$) $= \int T dy - x$, qua pro $R\sqrt{1-q^2}$ ſubſtituta, prodit $\frac{dy}{\int T dy - x} = \frac{dq}{\sqrt{1-q^2}}$.

Cor. 1. Semper $Tdy - dx$ admittit perfectam integrationem. Etenim $Tdy = \frac{ddy\sqrt{1-q^2}}{dq}$ et $dx = \frac{qdy}{\sqrt{1-q^2}}$, quibus $Tdy - dx = \frac{ddy\sqrt{1-q^2}}{dq} - \frac{qdy}{\sqrt{1-q^2}}$ et integratione $\int T dy - x = \frac{dy\sqrt{1-q^2}}{dq}$.

Cor. 2. Dicatur femichorda curvaturæ CG G, habetur $\frac{dy}{G} = \frac{dq}{\sqrt{1-q^2}}$, $\frac{dx}{G} = \frac{qdq}{1-q^2}$ et $\frac{dz}{G} = \frac{dq}{1-q^2}$.

Cor. 3. Dicatur cotangens anguli BCD t, et cofecans v erit $\frac{dy}{\int Tdy - x} = \frac{dt}{1+t^2}$ et $\frac{dy}{\int Tdy - x} = \frac{dv}{v\sqrt{v^2-1}}$.

Schol. 1. Quoniam $\int Tdy - x = \frac{dy\sqrt{1-q^2}}{dq}$ datur ex data relatione inter y et q, fit $\frac{dy\sqrt{1-q^2}}{dq} = Y$ functioni ordinatæ y erit $\int Tdy = Y - x$ fumtifque fluxionibus $Tdy = \dot{Y}dy - dx$ qua $T = \dot{Y} - \frac{dx}{dy}$ functioni ipfius y. Si autem $\frac{dy\sqrt{1-q^2}}{dq} = Q$ functioni ipfius q erit $\int Tdy = Q - x$ et fumtis fluxionibus $Tdy = \dot{Q}dq - dx$, qua habetur $T = \frac{\dot{Q}dq}{dy} - \frac{q}{\sqrt{1-q^2}}$ per q.

Schol. 2. Hujus theorematis auxilio elicere licet curvas data relatione inter T et y, G et y, G et x, G et z, et G et q. Si enim fit T functio ipfius y generaliter $\int Tdy = Y + \int \dot{Y}dy + A$, quæ functio eft algebraica ipfius y quoties $\int \dot{Y}dy$ abfolute fumi poffit. Affumatur $x = \int \ddot{Y}dy$, tali ipfius y functioni ut non folum $\int Tdy - x = Y + \int \overline{\dot{Y} + \ddot{Y}}dy$ fed etiam $\int Tdz = \int Tdy \sqrt{1 - \dot{Y}^2}$ abfoluta integratione habeantur, provenit vi theorematis $\frac{dy}{Y + \int \overline{\dot{Y} + \ddot{Y}}dy + A} \left(= \frac{dy}{\int Tdy - x}\right) = \frac{dq}{\sqrt{1-q^2}}$ et integratione $\int \frac{dy}{Y + \int \overline{\dot{Y} + \ddot{Y}}dy + A} + C = \int \frac{dq}{\sqrt{1-q^2}}$. Pofita $\frac{dy}{Y + \int \overline{\dot{Y} + \ddot{Y}}dy + A} + C = l$ et N bafi logarithmica erit $q = \frac{N^{l\sqrt{-1}} - N^{-l\sqrt{-1}}}{2\sqrt{-1}}$ et $\sqrt{1-q^2}$

$= \frac{N^{l\sqrt{-1}} + N^{-l\sqrt{-1}}}{2}$ quæ functiones sunt quantitatis y, quibus positis $\overset{\text{III}}{Y}$ et $\sqrt{1-\overset{\text{I}}{Y}^2}$ prodit $x \left(= \int \frac{qdv}{\sqrt{1-q^2}}\right) = \int \frac{\overset{\text{III}}{Y}dy}{\sqrt{1-\overset{\text{I}}{Y}^2}}$ æquatio quæ indolem curvarum indicat.

Si $G = X$ functioni ipsius x erit per Cor. 2. $\frac{dx}{X} \left(= \frac{dx}{G}\right) = \frac{qdq}{1-q^2}$, et integratione log. $CN^l \left(= \int \frac{dx}{X} + \text{log. } C\right) = \text{log. } \frac{1}{\sqrt{1-q^2}}$ si $\int \frac{dx}{X} = l$, exinde $\sqrt{1-q^2} = \frac{1}{CN^l}$, $q = \frac{\sqrt{C^2N^{2l}-1}}{CN^l}$ et $y \left(= \int \frac{dx\sqrt{1-q^2}}{q}\right) = \int \frac{dx}{\sqrt{C^2N^{2l}-1}}$, quæ curvæ naturam indigitat.

Si $G = Z$ functioni ipsius z erit $\frac{dz}{Z} \left(= \frac{dz}{G}\right) = \frac{dq}{1-q^2}$, et integratione log. $CN^l \left(= \frac{dz}{Z} + C\right) = \text{log. } \sqrt{\frac{1+q}{1-q}}$ si $\int \frac{dz}{Z} = l$, unde $q = \frac{C^2N^{2l}}{1+C^2N^{2l}}$ $\sqrt{1-q^2} = \frac{2CN^l}{1+C^2N^{2l}}$ $x \left(= \int qdz\right) = \int \frac{\overline{C^2N^{2l}-1}dz}{1+C^2N^{2l}}$ et y $\left(= \int dz\sqrt{-1q^2}\right) = \int \frac{2CN^l dz}{1+C^2N^{2l}}$ quibus curvæ cognoscuntur.

Patet hinc quod quando $Y + \int \overset{\text{I}}{Y}dy$ algebraice habeatur $\int \frac{dy}{Y + \int \overset{\text{I}}{Y} + \overset{\text{II}}{Y}dy + A}$ per quadraturam circuli, et $\int \frac{\overset{\text{III}}{Y}dy}{\sqrt{1-\overset{\text{III}}{Y}^2}}$ etiam obtineatur algebraice, curvæ evadunt algebraicæ, secus vero transcendentes.

Quando $\int \frac{dx}{X}$ vel $\int \frac{dz}{Z}$ obtineatur per logarithmos, et $\int \frac{dx}{\sqrt{C^2N^{2l}-1}}$, vel tam $\int \frac{\overline{C^2N^{2l}-1}dz}{1+C^2N^{2l}}$ quam $\int \frac{2CN^l dz}{1+C^2N^{2l}}$ absoluta integratione, curvæ erunt algebraicæ.

Exempl.

Exempl. 1. Sit index variationis curvaturæ $\mathrm{T} = \frac{6y}{a}$ erit $\int \mathrm{T}dy = \frac{3y^2}{a} + \mathrm{A}$, si quantitas illa constans $\mathrm{A} = \frac{a}{2}$ quod evenit quum $\int \mathrm{T}dy = \frac{a}{2}$ et $y = 0$; sumatur $x = \frac{y^2}{a}$ erit vi theorematis $\frac{2ady}{a^2 + 4y^2}$ $\left(= \frac{dy}{\int \mathrm{T}dy - x}\right) = \frac{dq}{\sqrt{1 - q^2}}$ et integratione $\int \frac{2ady}{a^2 + 4y^2} + \mathrm{C} = \int \frac{dq}{\sqrt{1 - q^2}}$, cujus æquationis termini quoniam sint arcus circulares quorum sinus $q = \frac{2y}{\sqrt{a^2 + 4y^2}}$ et cosinus $\sqrt{1 - q^2} = \frac{a}{\sqrt{a^2 + 4y^2}}$, arcu constanti $\mathrm{C} = 0$, obtinetur $x \left(= \int \frac{qdy}{\sqrt{1 - q^2}}\right) = \frac{y^2}{a}$ æquatio pro parabola Apolloniana.

Exempl. 2. Si sit $\mathrm{T} = \frac{a^2}{y\sqrt{a^2 - y^2}}$ habetur $\int \mathrm{T}dy = \int \frac{dy\sqrt{a^2 - y^2}}{y} - \sqrt{a^2 - y^2} + \mathrm{A}$, si quantitas illa constans $\mathrm{A} = 0$ quod evenit quum $\int \mathrm{T}dy = 0$ et $y = a$, et assumatur $x = \int \frac{dy\sqrt{a^2 - y^2}}{y}$, evadit per theorema $-\frac{dy}{\sqrt{a^2 - y^2}} \left(= \frac{dy}{\int \mathrm{T}dy - x}\right) = \frac{dq}{\sqrt{1 - q^2}}$, et per integrationem $-\int \frac{dy}{\sqrt{a^2 - y^2}} + \mathrm{C} = \int \frac{dq}{\sqrt{1 - q^2}}$, quorum arcuum sinus $q = \frac{\sqrt{a^2 - y^2}}{a}$ et cosinus $\sqrt{1 - q^2} = \frac{y}{a}$ si constans ille $\mathrm{C} = 0$, atque inde $dx \left(\frac{qdy}{\sqrt{1 - q^2}}\right) = \frac{dy\sqrt{1 - y^2}}{y}$ qua patet curvam esse tractoriam.

THEOREMA IV.

Dicatur summa tangentium angulorum HCD et BCD H, et differentia tangentium angulorum HCD et CKB K, retentis reliquis denominationibus erit $\frac{dx}{\int \mathrm{H}dx} = -\frac{dp}{\sqrt{1 - p^2}}$ et $\frac{dy}{\int \mathrm{K}dy} = \frac{dq}{\sqrt{1 - q^2}}$.

Quoniam

Quoniam $dy = \frac{p\,dx}{\sqrt{1-p^2}}$ erit $dy + T\,dx = T + \frac{p}{\sqrt{1-p^2}}\,dx$ et quum $H = T + \frac{p}{\sqrt{1-p^2}}$ habetur $dy + T\,dx = H\,dx$. Eodem modo quum $dx = \frac{q\,dy}{\sqrt{1-q^2}}$ erit $\int T\,dy - dx = T - \frac{q}{\sqrt{1-q^2}}\,dy$, sed $K = T - \frac{q}{\sqrt{1-q^2}}$, unde $\int T\,dy - x = K\,dy$. Per theorema igitur 2 et 3 provenit $\frac{dx}{\int H\,dx} = -\frac{dp}{\sqrt{1-p^2}}$ et $\frac{dy}{\int K\,dy} = \frac{dq}{\sqrt{1-q^2}}$.

Cor. Si sit ut antea tangens anguli BCD r, cotangens t, secans s, et cosecans v, erit $\frac{dx}{\int H\,dx} = -\frac{dr}{r+r^2}$ et $\frac{dx}{\int H\,dx} = -\frac{ds}{s\sqrt{s^2-1}}$, $\frac{dy}{\int K\,dy} = \frac{dt}{1+t^2}$ et $\frac{dy}{\int K\,dy} = \frac{dv}{v\sqrt{v^2-1}}$.

Schol. Ope hujus theorematis invenire licet curvas, data relatione inter H et x atque K et y. Itaque sit $H = X$ functioni ipsius x erit $\int H\,dx = \int X\,dx + A$, vi theorematis $\frac{dx}{\int X\,dx + A}$ $\left(= \frac{dx}{\int H\,dx}\right) = -\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int \frac{dx}{\int X\,dx + A} + C = -\int \frac{dp}{\sqrt{1-p^2}}$. Posita $\int \frac{dx}{\int X\,dx + A} + C = m$, et N logarithmorum basi prodit $\sqrt{1-p^2} = \frac{N^{m\sqrt{-1}} - N^{-m\sqrt{-1}}}{2\sqrt{-1}}$ et $p = \frac{N^{m\sqrt{-1}} + N^{-m\sqrt{-1}}}{2}$, quibus functionibus quantitatis x positis $\sqrt{1-\acute{X}^2}$ et $\acute{X}$ provenit æquatio $y \left(= \int \frac{p\,dx}{\sqrt{1-p^2}}\right) = \frac{\acute{X}\,dx}{\sqrt{1-\acute{X}^2}}$ naturam curvarum exprimens.

Si $K = Y$ functioni quantitatis y, eadem calculandi ratione habetur $x \left(= \int \frac{q\,dy}{\sqrt{1-q^2}}\right) = \frac{\acute{Y}\,dy}{\sqrt{1-\acute{Y}^2}}$ æquatio qua curvæ cognoscuntur.

Quando

Quando $\int X dx$ vel $\int Y dy$ absoluta integratione, $\int \frac{dx}{\int X dx + A}$ vel $\int \frac{dy}{\int Y dy + A}$ per rectificationem circuli, et $\int \frac{X dx}{\sqrt{1 - X^2}}$ vel $\int \frac{Y dy}{\sqrt{1 - Y^2}}$ integratione perfecta obtineantur, curva est algebraica.

Exempl. 1. Si sit $H = \frac{a + 12x}{2\sqrt{a}\sqrt{x}}$ erit $\int H dx = \frac{\overline{a + 4x}\sqrt{x}}{\sqrt{a}} + A$, et posita $A = 0$ habetur per theorema $\frac{dx\sqrt{a}}{a + 4x\sqrt{x}} \left(= \frac{dx}{\int H dx}\right) = -\frac{dp}{\sqrt{1 - p^2}}$ et per integrationem $\int \frac{dx\sqrt{a}}{a + 4x\sqrt{x}} + C = \int \frac{dp}{\sqrt{1 - p^2}}$, cujus termini quum sint arcus circulares quorum sinus $\sqrt{1 - p^2} = \frac{2\sqrt{x}}{\sqrt{a + 4x}}$ et cosinus $p = \frac{\sqrt{a}}{\sqrt{a + 4x}}$, posita $C = 0$, obtinetur $y \left(= \int \frac{p dx}{\sqrt{1 - p^2}}\right) = \sqrt{ax}$, quæ parabolam Apolloniam exprimit.

Exempl. 2. Sit $H = \frac{2a^4 - x^4}{ax^2\sqrt{a^2 - x^2}}$ erit $\int H dx = \frac{\overline{x^2 - 2a^2}\sqrt{a^2 - x^2}}{ax} + A$, et si $A = 0$, per theorema $\frac{ax dx}{x^2 + 2a^2\sqrt{a^2 - x^2}} \left(= \frac{dx}{\int H dx}\right) = -\frac{dp}{\sqrt{1 - p^2}}$ et per integrationem $\int \frac{ax dx}{x^2 - 2a^2\sqrt{a^2 - x^2}} + C = -\int \frac{dp}{\sqrt{1 - p^2}}$, et si $C = 0$, $\sqrt{1 - p^2} = \frac{\sqrt{a^2 - x^2}}{\sqrt{2a^2 - x^2}}$ $p = \frac{a}{\sqrt{2a^2 - x^2}}$ et $y \left(= \int \frac{p dx}{\sqrt{1 - p^2}}\right) = \int \frac{a dx}{\sqrt{a^2 - x^2}}$ æquatio pro curva sinuum.

Exempl. 3. Si sit $K = \frac{\overline{5a^2 + 6y^2} \cdot y}{a\sqrt{a^2 + y^2}}$ erit $\int K dy = \frac{\overline{a^2 + 2y^2}\sqrt{a^2 + y^2}}{a^2} + A$, si $A = 0$ habetur per theorema $\frac{a^2 dy}{a^2 + 2y^2\sqrt{a^2 + y^2}} \left(= \frac{dy}{\int K dy}\right) = \frac{dq}{\sqrt{1 - q^2}}$ et integratione $\int \frac{a^2 dy}{a^2 + 2y^2\sqrt{a^2 + y^2}} + C = -\int \frac{dp}{\sqrt{1 - p^2}}$, qua $q = \frac{y}{\sqrt{a^2 + 2y^2}}$, $\sqrt{1 - q^2} = \frac{\sqrt{a^2 + y^2}}{\sqrt{a^2 + 2y^2}}$, si $C = 0$, unde $x \left(= \int \frac{q dy}{\sqrt{1 - q^2}}\right) = \sqrt{a^2 + y^2}$ æquatio pro hyperbola æquilatera.

Exempl.

Exempl. 4. Sit $K=\frac{y}{\sqrt{a^2-y^2}}$ erit $\int K dy = A-\sqrt{a^2-y^2}$ et si $A=0$, per theorema $-\frac{dy}{\sqrt{a^2-y^2}}\left(=\frac{dy}{\int K dy}\right)=\frac{dq}{\sqrt{1-q^2}}$ et per integrationem $-\int\frac{dy}{\sqrt{a^2-y^2}}+C=\int\frac{dq}{\sqrt{1-q^2}}$ qua $q=\frac{\sqrt{a^2-y^2}}{a}$, $\sqrt{1-q^2}=\frac{y}{a}$ et $dx\left(=\frac{qdy}{\sqrt{1-q^2}}\right)=\frac{dy\sqrt{a^2-y^2}}{y}$ quæ Tractoriam exprimit.

THEOREMA V.

Designetur productum tangentium angulorum HCD et BCD per U, et angulorum HCD et CKB per V cæteris manentibus erit $\frac{dx}{\int U dx - x} = -\frac{dp}{p}$ et $\frac{dy}{Y+\int V dy}=\frac{dq}{q}$.

Quoniam $dy=\frac{pdx}{\sqrt{1-p^2}}$ et $U=\frac{Tp}{\sqrt{1-p^2}}$ erit $T dy \left(=\frac{Tpdx}{\sqrt{1-p^2}}\right)=$ $U dx$, et integratione $\int T dy=\int U dx$ qua $\int T dy - x=\int U dx - x$. Et quoniam $dx=\frac{qdy}{\sqrt{1-q^2}}$ et $V=\frac{Tq}{\sqrt{1-q^2}}$ erit $T dx \left(=\frac{Tqdy}{\sqrt{1-q^2}}\right)=$ $V dy$, $\int T dx=\int V dy$ et $y+\int T dx=y+\int V dy$. Theoremate 2. et 3. prodit $\frac{dx}{\int U dx - x}=-\frac{dp}{p}$ et $\frac{dy}{y+\int V dy}=\frac{dq}{q}$.

Cor. Si anguli BCD tangens, cotangens, &c. designentur ut antea, habetur $\frac{dx}{\int U dx - x}=-\frac{dr}{r\cdot 1+r^2}$, $\frac{dy}{y+\int V dy}=-\frac{dt}{t\cdot 1+t^2}$, &c.

Schol. Per hoc theorema curvæ inveniuntur ex data relatione inter U et x, atque inter V et y. Si enim sit $U=X$ functioni ipsius x erit $\int U dx=\int X dx + A$, per theorema $\frac{dx}{\int X dx - x + A}$ $\left(=\frac{dx}{\int U dx - x}\right)=-\frac{dp}{p}$, et per integrationem $\int\frac{dx}{\int X dx - x + A}+\log. C=$

log. $\frac{1}{p}$. Ponatur $\int \frac{dx}{\int Xdx - x + A} = n$ et N basi logarithmica, erit $\frac{1}{p} = CN^n$, $p = \frac{1}{CN^n}$, $\sqrt{1-p^2} = \frac{\sqrt{C^2N^{2n}-1}}{CN^n}$ et y $\left(= \frac{pdx}{\sqrt{1-p^2}}\right) =$ $\int \frac{dx}{\sqrt{C^2N^{2n}-1}}$ qua æquatione curvarum indoles innotescit.

Si V = Y functioni ipsius y, eadem calculandi ratione proveniet $x \left(= \int \frac{qdy}{\sqrt{1-q^2}}\right) = \int \frac{CN^n dy}{\sqrt{1-C^2N^{2n}}}$ qua curvæ cognoscuntur.

Evidens hinc est quod quoties $\int Xdx$ vel $\int Ydy$ algebraice $\int \frac{dx}{\int Xdx - x + A}$ vel $\int \frac{dy}{y + \int Ydy + A}$ per logarithmos, atque $\int \frac{dx}{\sqrt{C^2N^{2n}-1}}$ vel $\int \frac{CN^n dy}{\sqrt{1-C^2N^{2n}}}$ integratione absoluta, obtineantur, curva est algebraica.

Exempl. 1. Si sit U = 3 erit $\int Udx = 3x + A$, si vero $\int Udx = \frac{a}{2}$ quando $x = 0$ erit $A = \frac{a}{2}$ et $\int Udx - x = \frac{a+4x}{2}$. Per theorema igitur $\frac{2dx}{a+4x} \left(= \frac{dx}{\int Udx - x}\right) = - \frac{dp}{p}$ et per integrationem log. $\sqrt{a+4x}$ + log. C = log. $\frac{1}{p}$, posita $p = 1$ dum $x = 0$ log. C = − log. $\sqrt{a}$, unde facto a logarithmis transitu $\frac{\sqrt{a+4x}}{\sqrt{a}} = \frac{1}{p}$, qua $p = \frac{\sqrt{a}}{\sqrt{a+4x}}$, $\sqrt{1-p^2} = \frac{2\sqrt{x}}{\sqrt{a+4x}}$ et y $\left(= \int \frac{pdx}{\sqrt{1-p^2}}\right) = \int \frac{dx\sqrt{a}}{2\sqrt{x}}\Big) = \sqrt{ax}$ æquatio pro Parabola Apolloniana.

Exempl. 2. Sit $U = \frac{x^3 - 4a^3}{x\sqrt{x}}$ erit $\int Udx = \frac{x^3 - 2a^3}{3x^2} + A$, si autem $\int Udx = 0$ et $x = a\sqrt[3]{2}$, erit $A = 0$ et $\int Udx - x = \frac{2 \cdot a^3 - x^3}{3x^2}$. Vi igitur theorematis erit $\frac{3x^2dx}{2a^3 - x^3} \left(= \frac{dx}{\int Udx - x}\right) = - \frac{dp}{p}$, et integratione

 log.

$\log. \frac{a\sqrt{a}}{\sqrt{a^3-x^3}} + \log. C = \log. \frac{1}{p}$ qua $p = \frac{\sqrt{a^3-x^3}}{a\sqrt{a}}$; $\sqrt{1-p^2} = \frac{x\sqrt{x}}{a\sqrt{a}}$ et $y \left(= \int \frac{pdx}{\sqrt{1-p^2}}\right) = \frac{dx\sqrt{a^3-x^3}}{x\sqrt{x}}$ æquatio ad curvam quæsitam.

Exempl. 3. Si $V = -\frac{1}{2}$ erit $\int Vdy = A - \frac{y}{2}$, posita $\int Vdy = 0$ et $y = 0$ erit $A = 0$ et $y + \int Vdy = \frac{y}{2}$. Per theorema obtinetur $\frac{2dy}{y} \left(= \frac{dy}{y+\int Vdy}\right) = \frac{dq}{q}$ et per integrationem $\log. y^2 + \log. C = \log. q$, si $q = 1$ et $y = a$ erit $\log. C = -\log. a^2$, unde $q = \frac{y^2}{a^2}$, $\sqrt{1-q^2} = \frac{\sqrt{a^4-y^4}}{a^2}$ atque $dx \left(= \frac{qdy}{\sqrt{1-q^2}}\right) = \frac{y^2dy}{\sqrt{a^4-y^4}}$, curva ergo est Elastica,

Exempl. 4. Sit $V = \frac{a^2-2y^2}{y^2}$ erit $\int Vdy = A - \frac{a^2+2y^2}{y}$, si $\int Vdy = -3a$ et $y = a$ erit $A = 0$, indeque $y + \int Vdy = -\frac{a^2+y^2}{y}$. Theorematis ope habetur $-\frac{ydy}{a^2+y^2} \left(= \frac{dy}{y+\int Vdy}\right) = \frac{dq}{q}$ et integratione $\log. \frac{1}{\sqrt{a^2+y^2}} + \log. C = \log. q$, si $q = 1$ et $y = 0$ erit $\log. C = \log. a$ et exinde $q = \frac{a}{\sqrt{a^2+y^2}}$, $\sqrt{1-q^2} = \frac{y}{\sqrt{a^2+y^2}}$ et $dx \left(= \frac{qdy}{\sqrt{1-q^2}}\right) = \frac{ady}{y}$ æquatio pro Logarithmica.

THEOREMA VI.

Dicatur ED L, et AE M, retentis præterea adhibitis denominationibus erit $\frac{dL}{T} = dx$ et $\frac{dM}{T} = dy$.

Quoniam $dz : dx :: Tdz\ (dR) : Tdx$ habetur $dL = Tdx$ et

$\frac{dL}{T} = dx$. Et quoniam $dz : dy :: Tdz\ (dR) : Tdy$ obtinetur $dM = Tdy$ et $\frac{dM}{T} = dy$.

Cor. Quum $Tdy = Udx$ et $Tdx = Vdy$, erit substitutione $\frac{dM}{U} = dx$ et $\frac{dL}{V} = dy$.

Schol. Hoc adhibito theoremate inveniri possunt curvæ data relatione inter T et L, T et M, atque inter U et M et V et L. Ponatur $L = \dot{T}$ functioni quantitatis T habetur per theorema $\frac{d\dot{T}}{T}\ (= \frac{dL}{T}) = dx$ et integratione $\int \frac{d\dot{T}}{T} + C = x$ qua T per x datur. Curvæ deinde per theorema 2. elici possunt.

Si $M = \dot{T}$ ipsius T functioni, habetur eodem modo T per y. Si $M = \dot{U}$ functioni ipsius U, obtinetur U per x, et si $L = \dot{V}$ functioni quantitatis V, datur V per y. Per theorema deinde 3. et 5. curvæ inveniuntur.

Evidens quidem est quod curvæ esse non possunt algebraicæ nisi $\int \frac{dL}{T}$, $\int \frac{dM}{T}$, $\int \frac{dM}{U}$ vel $\int \frac{dL}{V}$, obtineantur integratione absoluta.

Exempl. 1. Si sit $L = \frac{aT^3}{54}$ erit $dL = \frac{aT^2dT}{18}$, et per hoc theorema $\frac{aTdT}{18}\ (= \frac{dL}{T}) = dx$ et integratione $\frac{aT^2}{36} + C = x$ qua $T = \frac{6\sqrt{x}}{\sqrt{a}}$, si $C = 0$. Per theorema 2. reperitur $y = \sqrt{ax}$, æquatio pro Parabola Apolloniana.

Exempl. 2. Si sit $M = -\int \frac{aT^2dT}{2 \cdot \overline{1+T^2}|^2}$ erit $dM = -\frac{aT^2dT}{2 \cdot \overline{1+T}|^2}$ et ope theorematis $-\frac{aTdT}{2 \cdot \overline{1+T^2}|^2}\ (= \frac{dM}{T}) = dy$, et integratione $\frac{a}{4 \cdot \overline{1+T^2}} + C = y$, qua si $C = 0$, $T = \frac{\sqrt{a-4y}}{2\sqrt{y}}$. Per theorema 3. habetur

$dx =$

$dx = \frac{2dy\sqrt{y}}{\sqrt{a-4y}}$, æquatio pro Cycloide ordinaria.

Exempl. 3. Sit $L = -a\sqrt{V}$ erit $dL = -\frac{adV}{2\sqrt{V}}$ et per theorema $-\frac{adV}{2V\sqrt{V}}\ (= \frac{dL}{V}) = dy$ et integratione $\frac{a}{\sqrt{V}} + C = y$, et si $C = 0$, habetur $V = \frac{a^2}{y^2}$ et deinde per theorema 5. $dx = \frac{dy\sqrt{a^2-y^2}}{y}$, qua constat curvam esse Tractoriam.

THEOREMA VII.

Dicatur ut antea CF F et CG G, et summa tangentium angulorum HCD et BCD, H, et differentia tangentium angulorum HCD et CKB, K, erit $\frac{dF}{H} = dx$ et $\frac{dG}{K} = dy$.

Quoniam $dF\ (= dy + Tdx) = Hdx$ et $dG\ (= \int T dy - x) = Kdy$ provenit $\frac{dF}{H} = dx$ et $\frac{dG}{K} = dy$.

Cor. Quum $F = -\frac{dx\sqrt{1-p^2}}{dp}$ et $G = \frac{dy\sqrt{1-q^2}}{dp}$ provenit divisione $\frac{dF}{FH} = -\frac{dp}{\sqrt{1-p^2}}$ atque $\frac{dG}{GK} = \frac{dq}{\sqrt{1-q^2}}$.

Schol. Auxilio hujus theorematis inveniuntur curvæ ex data relatione inter F et H, G et K, H et p atque K et q. Nam si sit $F = \overset{\prime}{H}$ functioni ipsius H, vel $G = \overset{\prime}{K}$ functioni ipsius K, habetur per theorema $\frac{d\overset{\prime}{H}}{H}\ (= \frac{dF}{H}) = dx$ et integratione $\int \frac{d\overset{\prime}{H}}{H} + C = x$ qua H per x datur. Eodem modo $\frac{d\overset{\prime}{K}}{K}\ (= \frac{dG}{K}) = dy$ et integratione $\int \frac{d\overset{\prime}{K}}{K} + C = y$ qua K per y obtinetur. Theorema 4. ulterius progredienti viam monstrat ad curvas inveniendas.

Patet

Patet quod curva non ſit algebraica niſi $\int\frac{dH}{H}$ vel $\int\frac{dK}{K}$ obtineantur perfecta integratione.

Exempl. 1. Si ſit $F=\frac{a}{\sqrt{1+H^2}}$ habetur per theorema — $\frac{adH}{\overline{1-H^2}|^{\frac{3}{2}}}\left(=\frac{dF}{H}\right)=dx$, et integratione $\frac{aH}{\sqrt{1-H^2}}+C=-x$ qua $H=-\frac{x}{\sqrt{a^2-x^2}}$, poſita $C=0$. Per theorema deinde 4. provenit $y=\sqrt{a^2-x^2}$ æquatio pro circulo.

Exempl. 2. Sit $F=\frac{a\,.\,\overline{H^3+H^2+6\sqrt{H^2-12}}}{108}$, erit per theorema $\frac{a\,.\,\overline{H^2-6+H\sqrt{H^2-12}}\,.\,dH}{36\sqrt{H^2-12}}\left(=\frac{dF}{H}\right)=dx$ et integratione facta $\frac{a\,.\,\overline{H^2-6+H\sqrt{H^2-12}}}{72}+C=x$, et poſita $C=0$ habetur $H=\frac{a+12x}{2\sqrt{a}\sqrt{x}}$, unde per theorema 4. prodit $y=\sqrt{ax}$ æquatio pro Parabola Apolloniana.

Exempl. 3. Sit $G=-\frac{a\,.\,\overline{4+K^2}}{4}$ erit per theorema $\frac{adK}{2}\left(=\frac{dG}{K}\right)=dy$, et integratione $\frac{aK}{2}+C=y$, et ſi $C=0$ $K=\frac{2y}{a}$ unde per theorema 4. $dx=\frac{ady}{y}$, qua conſtat curvam eſſe Logarithmicam.

THEOREMA VIII.

Dicatur ut antea productum tangentium angulorum HCD et BCD U, et productum tangentium angulorum HCD et CKB V manentibus reliquis denominationibus erit $\frac{dG}{U-1}=dx$ et $\frac{dF}{1+V}=dy$.

Quoniam

Quoniam $G = \int T dy - x$ erit $dG = T dy - dx$, ſed $T dy = U dx$, unde $dG = \overline{U-1}\, dx$ et $\frac{dG}{U-1} = dx$. Eeodem modo quum $F = y + \int T dx$ erit $dF = dy + T dx$, ſed $T dx = V dy$ quare $dF = \overline{1+V}\, dy$ et $\frac{dF}{1+V} = dy$.

Cor. Quoniam $G = \frac{dy\sqrt{1-q^2}}{dq}$ et $F = -\frac{dx\sqrt{1-p^2}}{ap}$, habetur ſubſtitutione debita $\frac{dG}{G \cdot \overline{U-1}} = -\frac{dp}{p}$ et $\frac{dF}{F^2\, \overline{1+V}} = \frac{dq}{q}$.

Schol. Ope hujus theorematis indagantur curvæ data relatione inter G et U vel inter F et V. Nam ſi ſit $G = \acute{U}$ functioni quantitatis U vel $F = \acute{V}$ functioni quantitatis V obtinetur per theorema in caſu priori $\frac{d\acute{U}}{U-1}\ \left(= \frac{dG}{U-1}\right) = dx$ et integratione $\int \frac{d\acute{U}}{U-1} + C = x$, qua U per x habetur; in poſteriori $\frac{d\acute{V}}{1+V}\ \left(= \frac{dF}{1+V}\right) = dy$ et integratione $\int \frac{d\acute{V}}{1+V} + C = y$, qua V habetur per y. Per theorema deinde 5. curvæ cognoſcuntur.

Datur etiam per Cor. U in p, et V in q, et conſequenter T in p vel q, nam $U = \frac{Tp}{\sqrt{1-p^2}}$ et $V = \frac{Tq}{\sqrt{1-q^2}}$.

Conſtat hinc quod curvæ non ſint algebraicæ niſi $\int \frac{d\acute{U}}{U-1}$ vel $\int \frac{d\acute{V}}{1+V}$ obtineantur integratione abſoluta.

Exempl. 1. Si ſit $G = \frac{a \cdot \overline{U-3}}{2}$ erit per theorema $\frac{a\, dU}{2\, \overline{U-1}}\ \left(= \frac{dG}{U-1}\right) = dx$ et integratione $\log. \overline{1-U} + \log. C = \frac{2x}{a}$ et ſi $C = \frac{a^2}{2}$ log.

log. $\frac{a^2 . \overline{1-U}}{2} = \frac{2x}{a}$ et $\frac{a . \overline{1-U}}{2} = N^{\frac{2x}{a}}$ qua $U = \frac{a^2 - 2N^{\frac{2x}{a}}}{a^2}$. Per theorema deinde 5. habetur $dy = \frac{dxN^{\frac{x}{a}}}{a}$ qua constat curvam est Logarithmicam.

Exempl. 2. Si sit $T = \frac{a . \overline{V-1}\sqrt{V+2}}{3\sqrt{3}}$ erit per theorema $\frac{adV}{2\sqrt{3}\sqrt{V+2}} \left(= \frac{dF}{1+V}\right) = dy$ et per integrationem $\frac{a\sqrt{V+2}}{\sqrt{3}} = y$ qua $V = \frac{3y^2 - 2a^2}{a^2}$; et per theorema 5. $dx = \frac{dy\sqrt{y^2-a^2}}{a}$, æquatio ad curvam cujus constructio a quadratura hyperbolæ dependet.

THEOREMA IX.

Sint LC et lc duæ curvæ eandem habentes Evolutam QD, dicatur radiorum osculi CD cD constans differentia cC b, curvæ lc variatio curvaturæ S, ceterisque ut antea manentibus erit $\frac{dR}{R-bS} = -\frac{dp}{\sqrt{1-p^2}}$.

Quoniam radius curvaturæ DH evolutæ sit $RT = R - bS$, erit $\frac{1}{R-bS} = \frac{1}{RT}$, quæ per dR $(= Tdz) = -\frac{RTdp}{\sqrt{1-p^2}}$ multiplicata, mostrat esse $\frac{dR}{R-bS} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. Si sint ut antea tangens anguli BCD r et secans s, habetur $\frac{dR}{R-bS} = -\frac{dr}{1+r^2}$ et $\frac{dR}{R-bS} = -\frac{ds}{s\sqrt{s^2-1}}$.

Schol. Subsidio hujus theorematis invenire licet curvas, data relatione inter S et R vel inter S et T nam $\frac{S}{T} = \frac{R}{R-b}$. Itaque si

ponatur

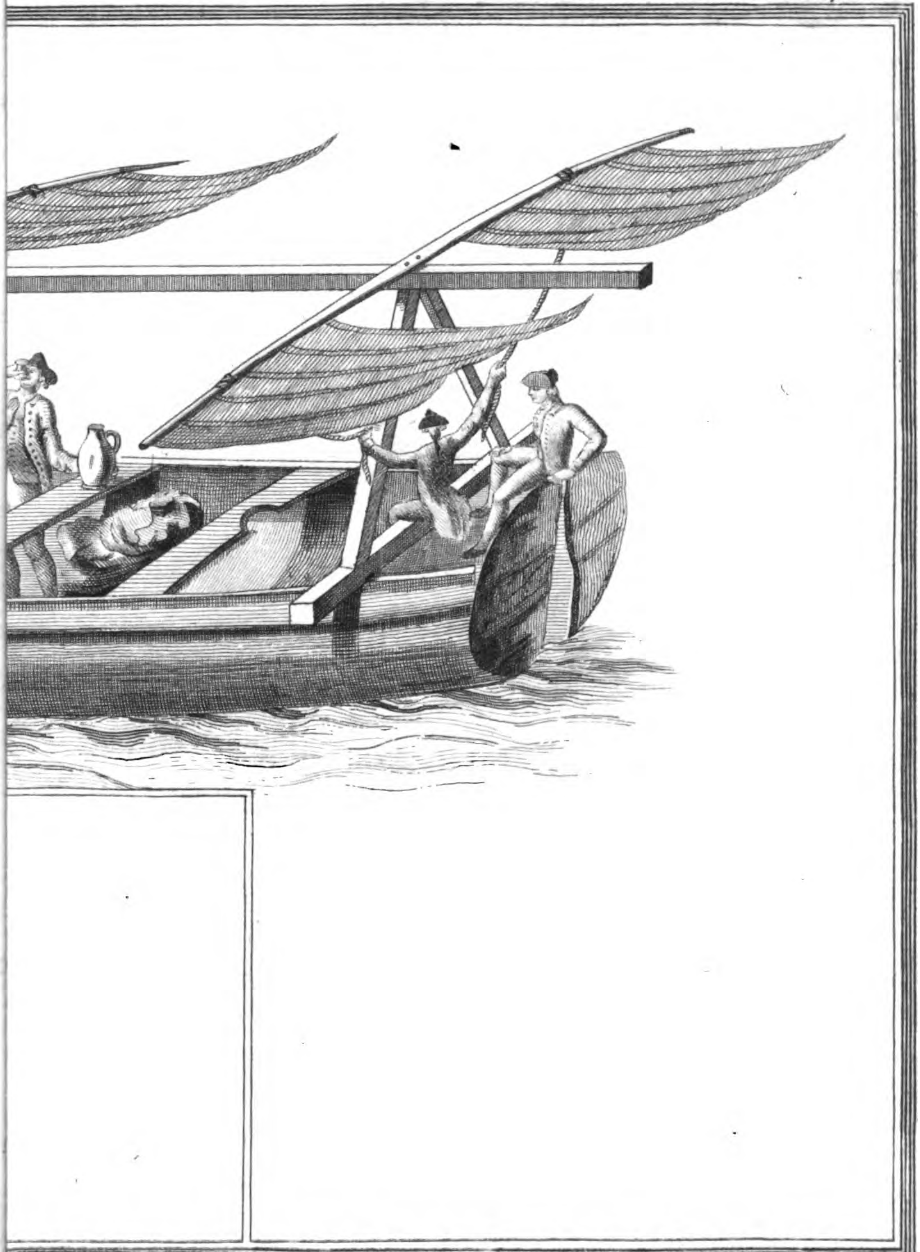
ponatur $S=\acute{R}$ functioni radii curvedinis R, erit $\frac{dR}{R-b\acute{R}}\left(=\frac{dR}{R-bS}\right)$ $=-\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int\frac{dR}{R-b\acute{R}}+C=-\int\frac{dp}{\sqrt{1-p^2}}$. Sit $\int\frac{dR}{R-b\acute{R}}+C=f$ et N logarithmorum basi habetur $\sqrt{1-p^2}=$ $\frac{N^{f\sqrt{-1}}-N^{-f\sqrt{-1}}}{2\sqrt{-1}}$ et $p=\frac{N^{f\sqrt{-1}}+N^{-f\sqrt{-1}}}{2}$ functionibus quantitatis R, quibus R per p exprimi potest. Per theorema igitur 1. curvas internoscere valemus.

Si $R=\acute{S}$ functioni quantitatis S habetur $\frac{d\acute{S}}{\acute{S}-bS}\left(=\frac{dR}{R-bS}\right)$ $=-\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int\frac{d\acute{S}}{\acute{S}-bS}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, posita $\int\frac{d\acute{S}}{\acute{S}-bS}+C=g$, erit $\sqrt{1-p^2}=\frac{N^{g\sqrt{-1}}-N^{-g\sqrt{-1}}}{2-\sqrt{1}}$ et $p=\frac{N^{g\sqrt{-1}}+N^{-g\sqrt{-1}}}{2}$ quibus S per p datur. Per theoremata Partis I. invenire licet curvas omnes eandem evolutam habentes.

Hinc videtur, quod curvæ non sint algebraicæ nisi $\int\frac{dR}{R-b\acute{R}}$ vel $\int\frac{d\acute{S}}{\acute{S}-bS}$ per circuli rectificationem obtineatur.

Exempl. 1. Si sit $S=\frac{2R}{\sqrt{a}\,.\,\sqrt{R-a}}$ supposita $b=a$, erit per theorema $\frac{dR\sqrt{a}}{2R\sqrt{R-a}}\left(=\frac{dR}{R-bS}\right)=-\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int\frac{dR\sqrt{a}}{2R\sqrt{R-a}}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, si vero arcus ille constans $C=0$ erit $\sqrt{1-p^2}=\frac{\sqrt{R-a}}{\sqrt{R}}$ qua $R=ap^2$, et per Cor. 1. Theor. 1. habetur $dy=\frac{adx}{\sqrt{x^2-a^2}}$, æquatio pro Catenaria.

Exempl. 2. Sit $S=\frac{5a^2+R^2}{a\cdot\overline{a-5R}}$, posita $b=\frac{a}{5}$ erit per theorema $-\frac{adR}{a^2+R^2}\left(=\frac{dR}{R-bS}\right)=-\frac{dp}{\sqrt{1-p^2}}$ et facta integratione $-\int\frac{adR}{a^2+R^2}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, qua si $C=0$, habetur $\sqrt{1-p^2}=\frac{R}{\sqrt{a^2+R^2}}$ et $R=\frac{a\sqrt{1-p^2}}{p}$. Per theorema 1. $dx=\frac{dy\sqrt{a^2-y^2}}{y}$ qua constat curvam esse Tractoriam.



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V O L. LXXIII.

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V O L. LXXIV. P A R T II.

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365. 11. *for* I *read* K

In plate XIV. fig. 2. the letter *a* fhould be inferted at the bottom, and the letter *b* at the top, of the dotted *interval* 19½, in the fame manner as the letters *c* and *d* at the two extremes of the dotted *intervals* in fig. 3.

In plate XXI. fig. 2. at the lower angle of the diagram, in fome copies, the letter K ftands by miftake inftead of H.